undergone the dynamic action of an artificial whirlpool in all points comparable with the dynamic action of a torrential current of water, present all the characteristics of the ancient river-gravels; it is easy to find among them, after a few minutes' search, all the most characteristic forms of eoliths, such as are given as typical. My colleagues and I have been able to make a collection of flints admirably *retouchés*, identical with the forms called by M. Rutot hammer-stones, planes, notched flints, &c. We have also collected flints showing the cone of percussion, which is generally regarded as an infallible mark of intentional fashioning.

THE BRITISH ASSOCIATION.

INAUGURAL ADDRESS BY PROF. G. H. DARWIN, M.A., LL.D., PH.D., F.R.S., PRESIDENT OF THE ASSOCIATION.

PART II.[1]

THUS far we have been concerned with the almost inconceivably minute, and I now propose to show that similar conditions prevail on a larger scale.

Many geological problems might well be discussed from my present point of view, yet I shall pass them by, and shall proceed at once to Astronomy, beginning with the smallest cosmical scale of magnitude, and considering afterwards the larger celestial phenomena.

The problems of cosmical evolution are so complicated that it is well to conduct the attack in various ways at the same time. Although the several theories may seem to some extent discordant with one another, yet, as I have already said, we ought not to scruple to carry each to its logical conclusion. We may be confident that in time the false will be eliminated from each theory, and when the true alone remains the reconciliation of apparent disagreements will have become obvious.

The German astronomer Bode long ago propounded a simple empirical law concerning the distances at which the several planets move about the sun. It is true that the planet Neptune, discovered subsequently, was found to be considerably out of the place which would be assigned to it by Bode's law, yet his formula embraces so large a number of cases with accuracy that we are compelled to believe that it arises in some manner from the primitive conditions of the planetary system.

The explanation of the causes which have led to this simple law as to the planetary distances presents an interesting problem, and, although it is still unsolved, we may obtain some insight into its meaning by considering what I have called a working model of ideal simplicity.

Imagine then a sun round which there moves in a circle a single large planet. I will call this planet Jove, because it may be taken as a representative of our largest planet, Jupiter. Suppose next that a meteoric stone or small planet is projected in any perfectly arbitrary manner in the same plane in which Jove is moving; then we ask how this third body will move. The conditions imposed may seem simple, yet the problem has so far overtaxed the powers of the mathematician that nothing approaching a general answer to our question has yet been given. We know, however, that under the combined attractions of the sun and Jove the meteoric stone will in general describe an orbit of extraordinary complexity, at one time moving slowly at a great distance from both the sun and Jove, at other times rushing close past one or other of them. As it grazes past Jove or the sun it may often but just escape a catastrophe, but a time will come at length when it runs its chances too fine and comes into actual collision. The individual career of the stone is then ended by absorption, and of course by far the greater chance is that it will find its Nirvana by absorption in the sun.

Next let us suppose that instead of one wandering meteoric stone or minor planet there are hundreds of them, moving initially in all conceivable directions. Since they are all supposed to be very small, their mutual attractions will be insignificant, and they will each move almost as though they were influenced only by the sun and Jove. Most of these stones will be absorbed by the sun, and the minority will collide with Jove.

When we inquire how long the career of a stone may be, we find that it depends on the direction and speed with which it is started, and that by proper adjustment the delay of the final catastrophe may be made as long as we please. Thus by making the delay indefinitely long we reach the conception of a meteoric stone which moves so as never to come into collision with either body.

There are, therefore, certain perpetual orbits in which a meteoric stone or minor planet may move for ever without collision. But when such an immortal career has been discovered for our minor planet, it still remains to discover whether the slightest possible departure from the prescribed orbit will become greater and greater and ultimately lead to a collision with the sun or Jove, or whether the body will travel so as to cross and re-cross the exact perpetual orbit, always remaining close to it. If the slightest departure inevitably increases as time goes on, the orbit is unstable; if, on the other hand, it only leads to a slight waviness in the path described, it is stable.

We thus arrive at another distinction: there are perpetual orbits, but some, and indeed most, are unstable, and these do not offer an immortal career for a meteoric stone; and there are other perpetual orbits which are stable or persistent. The unstable ones are those which succumb in the struggle for life, and the stable ones are the species adapted to their environment.

If, then, we are given a system of a sun and large planet, together with a swarm of small bodies moving in all sorts of ways, the sun and planet will grow by accretion, gradually sweeping up the dust and rubbish of the system, and there will survive a number of small planets and satellites moving in certain definite paths. The final outcome will be an orderly planetary system in which the various orbits are arranged according to some definite law.

But the problem presented even by a system of such ideal simplicity is still far from having received a complete solution. No general plan for determining perpetual orbits has yet been discovered, and the task of discriminating the stable from the unstable is arduous. But a beginning has been made in the determination of some of the zones surrounding the sun and Jove in which stable orbits are possible, and others in which they are impossible. There is hardly room for doubt that if a complete solution for our solar system were attainable, we should find that the orbits of the existing planets and satellites are numbered amongst the stable perpetual orbits, and should thus obtain a rigorous mechanical explanation of Bode's law concerning the planetary distances.

It is impossible not to be struck by the general similarity between the problem presented by the corpuscles moving in orbits in the atom, and that of the planets and satellites moving in a planetary system. It may not, perhaps, be fanciful to imagine that some general mathematical method devised for solving a problem of cosmical evolution may find another application to miniature atomic systems, and may thus lead onward to vast developments of industrial mechanics. Science, however diverse its aims, is a whole, and men of science do well to impress on the captains of industry that they should not look askance on those branches of investigation which may seem for the moment far beyond any possibility of practical utility.

You will remember that I discussed the question as to whether the atomic communities of corpuscles could be regarded as absolutely eternal, and that I said that the analogy of other moving systems pointed to their ultimate mortality. Now the chief analogy which I had in my mind was that of a planetary system.

The orbits of which I have spoken are only perpetual when the bodies are infinitesimal in mass, and meet with no resistance as they move. Now the infinitesimal body does not exist, and both Lord Kelvin and Poincaré concur in holding that disturbance will ultimately creep in to any system of bodies moving even in so-called stable orbits; and this is so even apart from the resistance offered to the moving bodies by any residual gas there may be scattered through space. The stability is therefore only relative, and a planetary system contains the seeds of its own destruction. But this ultimate fate need not disturb

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[1] Delivered at Johannesburg on August 30. The first part of the Address, delivered at Cape Town on August 15, appeared in NATURE of August 17.

us either practically or theoretically, for the solar system contains in itself other seeds of decay which will probably bear fruit long before the occurrence of any serious disturbance of the kind of which I speak.

Before passing on to a new topic I wish to pay a tribute to the men to whom we owe the recent great advances in theoretical dynamical astronomy. As treated by the master-hands of Lagrange and Laplace and their successors, this branch of science hardly seemed to afford scope for any great new departure. But that there is always room for discovery, even in the most frequented paths of knowledge, was illustrated when, nearly thirty years ago, Hill of Washington proposed a new method of treating the theory of the moon's motion in a series of papers which have become classical. I have not time to speak of the enormous labour and great skill involved in the completion of Hill's Lunar Theory, by Ernest Brown, whom I am glad to number amongst my pupils and friends; for I must confine myself to other aspects of Hill's work.

The title of Hill's most fundamental paper, namely, "On Part of the Motion of the Lunar Perigee," is almost comic in its modesty, for who would suspect that it contains the essential points involved in the determination of perpetual orbits and their stability? Probably Hill himself did not fully realise at the time the full importance of what he had done. Fortunately he was followed by Poincaré, who not only saw its full meaning but devoted his incomparable mathematical powers to the full theoretical development of the point of view I have been laying before you.

Other mathematicians have also made contributions to this line of investigation, amongst whom I may number my friend Mr. Hough, chief assistant at the Royal Observatory of Cape Town, and myself. But without the work of our two great forerunners we should still be in utter darkness, and it would have been impossible to give even this slight sketch of a great subject.

The theory which I have now explained points to the origin of the sun and planets from gradual accretions of meteoric stones, and it makes no claim to carry the story back behind the time when there was already a central condensation or sun about which there circled another condensation or planet. But more than a century ago an attempt had already been made to re-construct the history back to a yet remoter past, and, as we shall see, this attempt was based upon quite a different supposition as to the constitution of the primitive solar system. I myself believe that the theory I have just explained, as well as that to which I am coming, contains essential elements of truth, and that the apparent discordances will some day be reconciled. The theory of which I speak is the celebrated nebular hypothesis, first suggested by the German philosopher Kant, and later re-stated independently and in better form by the French mathematician Laplace.

Laplace traced the origin of the solar system to a nebula or cloud of rarefied gas congregated round a central condensation which was ultimately to form the sun. The whole was slowly rotating about an axis through its centre, and, under the combined influences of rotation and of the mutual attraction of the gas, it assumed a globular form, slightly flattened at the poles. The justifiability of this supposition is confirmed by the observations of astronomers, for they find in the heavens many nebulæ, while the spectroscope proves that their light at any rate is derived from gas. The primeval globular nebula is undoubtedly a stable or persistent figure, and thus Laplace's hypothesis conforms to the general laws which I have attempted to lay down.

The nebula must have gradually cooled by radiation into space, and as it did so the gas must necessarily have lost some of its spring or elasticity. This loss of power of resistance then permitted the gas to crowd more closely towards the central condensation, so that the nebula contracted. The contraction led to two results, both inevitable according to the laws of mechanics: first, the central condensation became hotter; and, secondly, the speed of its rotation became faster. The accelerated rotation led to an increase in the amount of polar flattening, and the nebula at length assumed the form of a lens, or of a disc thicker in the middle than at the edges. Assuming the existence of the primitive nebula, the hypothesis may be accepted thus far as practically certain.

From this point, however, doubt and difficulty enter into the argument. It is supposed that the nebula became so much flattened that it could not subsist as a continuous aggregation of gas, and a ring of matter detached itself from the equatorial regions. The central portions of the nebula, when relieved of the excrescence, resumed the more rounded shape formerly possessed by the whole. As the cooling continued the central portion in its turn became excessively flattened through the influence of its increased rotation; another equatorial ring then detached itself, and the whole process was repeated as before. In this way the whole nebula was fissured into a number of rings surrounding the central condensation, the temperature of which must by then have reached incandescence.

Each ring then aggregated itself round some nucleus which happened to exist in its circumference, and so formed a subordinate nebula. Passing through a series of transformations, like its parent, this nebula was finally replaced by a planet with attendant satellites.

The whole process forms a majestic picture of the history of our system. But the mechanical conditions of a rotating nebula are too complex to admit, as yet, of complete mathematical treatment; and thus, in discussing this theory, the physicist is compelled in great measure to adopt the qualitative methods of the biologist, rather than the quantitative ones which he would prefer.

The telescope seems to confirm the general correctness of Laplace's hypothesis. Thus, for example, the great nebula in Andromeda presents a grand illustration of what we may take to be a planetary system in course of formation. In it we see the central condensation surrounded by a more or less ring-like nebulosity, and in one of the rings there appears to be a subordinate condensation.

Nevertheless it is hardly too much to say that every stage in the supposed process presents to us some difficulty or impossibility. Thus we ask whether a mass of gas of almost inconceivable tenuity can really rotate all in one piece, and whether it is not more probable that there would be a central whirlpool surrounded by more slowly-moving parts. Again, is there any sufficient reason to suppose that a series of intermittent efforts would lead to the detachment of distinct rings, and is not a continuous outflow of gas from the equator more probable?

The ring of Saturn seems to have suggested the theory to Laplace; but to take it as a model leads us straight to a quite fundamental difficulty. If a ring of matter ever concentrates under the influence of its mutual attraction, it can only do so round the centre of gravity of the whole ring. Therefore the matter forming an approximately uniform ring, if it concentrates at all, can only fall in on the parent planet and be re-absorbed. Some external force other than the mutual attraction of the matter forming the ring, and therefore not provided by the theory, seems necessary to effect the supposed concentration. The only way of avoiding this difficulty is to suppose the ring to be ill-balanced or lop-sided; in this case, provided the want of balance is pronounced enough, concentration will take place round a point inside the ring but outside the planet. Many writers assume that the present distances of the planets preserve the dimensions of the primitive rings; but the argument that a ring can only aggregate about its centre of gravity, which I do not recollect to have seen before, shows that such cannot be the case.

The concentration of an ill-balanced or broken ring on an interior point would necessarily generate a planet with direct rotation—that is to say, rotating in the same direction as the earth. But several writers, and notably Faye, endeavour to show—erroneously as I think—that a retrograde rotation should be normal, and they are therefore driven to make various complicated suppositions to explain the observed facts. But I do not claim to have removed the difficulty, only to have shifted it; for the satellites of Neptune, and presumably the planet itself, have retrograde rotations; and, lastly, the astonishing discovery has just been made by William Pickering of a ninth retrograde satellite of Saturn, while the rotations of the eight other satellites, of the ring and of the planet itself are direct Finally, I express a doubt as to whether the telescope does really exactly confirm the hypothesis of Laplace, for I imagine that what we see indicates a spiral rather than a ring-like division of nebulæ.[1]

This is not the time to pursue these considerations further, but enough has been said to show that the nebular hypothesis cannot be considered as a connected intelligible whole, however much of truth it may contain.

In the first theory which I sketched as to the origin of the sun and planets, we supposed them to grow by the accretions of meteoric wanderers in space, and this hypothesis is apparently in fundamental disagreement with the conception of Laplace, who considered the transformations of a continuous gaseous nebula. Some years ago a method occurred to me by which these two discordant schemes of origin might perhaps be reconciled. A gas is not really continuous, but it consists of a vast number of molecules moving in all directions with great speed and frequently coming into collision with one another. Now I have ventured to suggest that a swarm of meteorites would, by frequent collisions, form a medium endowed with so much of the mechanical properties of a gas as would satisfy Laplace's conditions. If this is so, a nebula may be regarded as a quasi-gas, the molecules of which are meteorites. The gaseous luminosity which undoubtedly is sent out by nebulæ would then be due only to incandescent gas generated by the clash of meteorites, while the dark bodies themselves would remain invisible. Sir Norman Lockyer finds spectroscopic evidence which led him long ago to some such view as this, and it is certainly of interest to find in his views a possible means of reconciling two apparently totally discordant theories.[2] However, I do not desire to lay much stress on my suggestion, for without doubt a swarm of meteors could only maintain the mechanical properties of a gas for a limited time, and, as pointed out by Prof. Chamberlin, it is difficult to understand how a swarm of meteorites moving indiscriminately in every direction could ever have come into existence. But my paper may have served to some extent to suggest to Chamberlin his recent modification of the nebular hypothesis, in which he seeks to reconcile Laplace's view with a meteoritic origin of the planetary system.[3]

We have seen that, in order to explain the genesis of planets according to Laplace's theory, the rings must be ill-balanced or even broken. If the ring were so far from being complete as only to cover a small segment of the whole circumference, the true features of the occurrences in the births of planets and satellites might be better represented by conceiving the detached portion of matter to have been more or less globular from the first, rather than ring-shaped. Now this idea introduces us to a group of researches whereby mathematicians have sought to explain the birth of planets and satellites in a way which might appear, at first sight, to be fundamentally different from that of Laplace.

The solution of the problem of evolution involves the search for those persistent or stable forms which biologists would call species. The species of which I am now going to speak may be grouped in a family, which comprises all those various forms which a mass of rotating liquid is capable of assuming under the conjoint influences of gravitation and rotation. If the earth were formed throughout of a liquid of the same density, it would be one of the species of this family; and indeed these researches date back to the time of Newton, who was the first to explain the figures of planets.

The ideal liquid planets we are to consider must be regarded as working models of actuality, and inasmuch as the liquid is supposed to be incompressible, the conditions depart somewhat widely from those of reality. Hence, when the problem has been solved, much uncertainty remains as to the extent to which our conclusions will be applicable to actual celestial bodies.

We begin, then, with a rotating liquid planet like the earth, which is the first stable species of our family. We next impart in imagination more rotation to this planet, and find by mathematical calculation that its power of resistance to any sort of disturbance is less than it was. In other words, its stability declines with increased rotation, and at length we reach a stage at which the stability just vanishes. At this point the shape is a transitional one, for it is the beginning of a new species with different characteristics from the first, and with a very feeble degree of stability or power of persistence. As a still further amount of rotation is imparted, the stability of the new species increases to a maximum and then declines until a new transitional shape is reached and a new species comes into existence. In this way we pass from species to species with an ever-increasing amount of rotation.

The first or planetary species has a circular equator like the earth; the second species has an oval equator, so that it is something like an egg spinning on its side on a table; in the third species we find that one of the two ends of the egg begins to swell, and that the swelling gradually becomes a well-marked protrusion or filament. Finally the filamentous protrusion becomes bulbous at its end, and is only joined to the main mass of liquid by a gradually thinning neck. The neck at length breaks, and we are left with two separated masses which may be called planet and satellite. It is fair to state that the actual rupture into two bodies is to some extent speculative, since mathematicians have hitherto failed to follow the whole process to the end.

In this ideal problem the successive transmutations of species are brought about by gradual additions to the amount of rotation with which the mass of liquid is endowed. It might seem as if this continuous addition to the amount of rotation were purely arbitrary and could have no counterpart in nature. But real bodies cool and contract in cooling, and, since the scale of magnitude on which our planet is built is immaterial, contraction will produce exactly the same effect on shape as augmented rotation. I must ask you, then, to believe that the effects of an apparently arbitrary increase of rotation may be produced by cooling.

The figures which I succeeded in drawing, by means of rigorous calculation, of the later stages of this course of evolution, are so curious as to remind one of some such phenomenon as the protrusion of a filament of protoplasm from a mass of living matter, and I suggest that we may see in this almost life-like process the counterpart of at least one form of the birth of double stars, planets, and satellites.

As I have already said, Newton determined the first of these figures; Jacobi found the second, and Poincaré indicated the existence of the third, in a paper which is universally regarded as one of the masterpieces of applied mathematics; finally I myself succeeded in determining the exact form of Poincaré's figure, and in proving that it is a true stable shape.

My Cambridge colleague Jeans has also made an interesting contribution to the subject by discussing a closely analogous problem, and he has besides attacked the far more difficult case where the rotating fluid is a compressible gas. In this case also he finds a family of types, but the conception of compressibility introduced a new set of considerations in the transitions from species to species. The problem is, however, of such difficulty that he had to rest content with results which were rather qualitative than strictly quantitative.

This group of investigations brings before us the process of the birth of satellites in a more convincing form than was possible by means of the general considerations adduced by Laplace. It cannot be doubted that the supposed Laplacian sequence of events possesses a considerable element of truth, yet these latter schemes of transformation can be followed in closer detail. It seems, then, probable that both processes furnish us with crude models of reality, and that in some cases the first and in others the second is the better representative.

The moon's mass is one-eightieth of that of the earth, whereas the mass of Titan, the largest satellite in the solar system, is 1/4600 of that of Saturn. On the ground of this great difference between the relative magnitudes of all other satellites and of the moon, it is not unreasonable to suppose that the mode of separation of the moon from the earth may also have been widely different. The

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[1] Prof. Chamberlin, of Chicago, has recently proposed a modified form of the nebular hypothesis, in which he contends that the spiral form is normal. See "Year Book," No. 3, for 1904, of the Carnegie Institution of Washington, pp. 195–258.

[2] Newcomb considers the objections to Lockyer's theory insuperable. See p. 190 of "The Stars." (London: John Murray, 1904.)

[3] See preceding reference to Chamberlin's paper.

theory of which I shall have next to speak claims to trace the gradual departure of the moon from an original position not far removed from the present surface of the earth. If this view is correct, we may suppose that the detachment of the moon from the earth occurred as a single portion of matter, and not as a concentration of a Laplacian ring.

If a planet is covered with oceans of water and air, or if it is formed of plastic molten rock, tidal oscillations must be generated in its mobile parts by the attractions of its satellites and of the sun. Such movements must be subject to frictional resistance, and the planet's rotation will be slowly retarded by tidal friction in much the same way that a fly-wheel is gradually stopped by any external cause of friction. Since action and reaction are equal and opposite, the action of the satellites on the planet, which causes the tidal friction of which I speak, must correspond to a reaction of the planet on the motion of the satellites.

At any moment of time we may regard the system composed of the rotating planet with its attendant satellite as a stable species of motion, but the friction of the tides introduces forces which produce a continuous, although slow, transformation in the configuration. It is, then, clearly of interest to trace backwards in time the changes produced by such a continuously acting cause, and to determine the initial condition from which the system of planet and satellite must have been slowly degrading. We may also look forward, and discover whither the transformation tends.

Let us consider, then, the motion of the earth and moon revolving in company round the sun, on the supposition that the friction of the tides in the earth is the only effective cause of change. We are, in fact, to discuss a working model of the system, analogous to those of which I have so often spoken before.

This is not the time to attempt a complete exposition of the manner in which tidal friction gives rise to the action and reaction between planet and satellite, nor shall I discuss in detail the effects of various kinds which are produced by this cause. It must suffice to set forth the results in their main outlines, and, as in connection with the topic of evolution retrospect is perhaps of greater interest than prophecy, I shall begin with the consideration of the past.

At the present time the moon, moving at a distance of 240,000 miles from the earth, completes her circuit in twenty-seven days. Since a day is the time of one rotation of the earth on its axis, the angular motion of the earth is twenty-seven times as rapid as that of the moon.

Tidal friction acts as a brake on the earth, and therefore we look back in retrospect to times when the day was successively twenty-three, twenty-two, twenty-one of our present hours in length, and so on backward to still shorter days. But during all this time the reaction on the moon was at work, and it appears that its effect must have been such that the moon also revolved round the earth in a shorter period than it does now; thus the month also was shorter in absolute time than it now is. These conclusions are absolutely certain, although the effects on the motions of the earth and of the moon are so gradual that they can only doubtfully be detected by the most refined astronomical measurements.

We take the "day," regarding it as a period of variable length, to mean the time occupied by a single rotation of the earth on its axis; and the "month," likewise variable in absolute length, to mean the time occupied by the moon in a single revolution round the earth. Then, although there are now twenty-seven days in a month, and although both day and month were shorter in the past, yet there is, so far, nothing to tell us whether there were more or fewer days in the month in the past. For if the day is now being prolonged more rapidly than the month, the number of days in the month was greater in the past than it now is; and if the converse were true, the number of days in the month was less.

Now it appears from mathematical calculation that the day must now be suffering a greater degree of prolongation than the month, and accordingly in retrospect we look back to a time when there were more days in the month than at present. That number was once twenty-nine, in place of the present twenty-seven; but the epoch of twenty-nine days in the month is a sort of crisis in the history of moon and earth, for yet earlier the day was shortening less rapidly than the month. Hence, earlier than the time when there were twenty-nine days in the month, there was a time when there was a reversion to the present smaller number of days.

We thus arrive at the curious conclusion that there is a certain number of days to the month, namely twenty-nine, which can never have been exceeded, and we find that this crisis was passed through by the earth and moon recently; but, of course, a recent event in such a long history may be one which happened some millions of years ago.

Continuing our retrospect beyond this crisis, both day and month are found continuously shortening, and the number of days in the month continues to fall. No change in conditions which we need pause to consider now supervenes, and we may ask at once, what is the initial stage to which the gradual transformation points? I say, then, that on following the argument to its end the system may be traced back to a time when the day and month were identical in length, and were both only about four or five of our present hours. The identity of day and month means that the moon was always opposite to the same side of the earth; thus at the beginning the earth always presented the same face to the moon, just as the moon now always shows the same face to us. Moreover, when the month was only some four or five of our present hours in length the moon must have been only a few thousand miles from the earth's surface—a great contrast with the present distance of 240,000 miles.

It might well be argued from this conclusion alone that the moon separated from the earth more or less as a single portion of matter at a time immediately antecedent to the initial stage to which she has been traced. But there exists a yet more weighty argument favourable to this view, for it appears that the initial stage is one in which the stability of the species of motion is tottering, so that the system presents the characteristic of a transitional form, which we have seen to denote a change of type or species in a previous case.

In discussing the transformations of a liquid planet we saw the tendency of the single mass to divide into two portions, although we failed to extend the rigorous argument back to the actual moment of separation; and now we seem to reach a similar crisis from the opposite end, when in retrospect we trace back the system to two masses of unequal size in close proximity with one another. The argument almost carries conviction with it, but I have necessarily been compelled to pass over various doubtful points.

Time is wanting to consider other subjects worthy of notice which arise out of this problem, yet I wish to point out that the earth's axis must once have been less tilted over with reference to the sun than it is now, so that the obliquity of the ecliptic receives at least a partial explanation. Again, the inclination of the moon's orbit may be in great measure explained; and, lastly, the moon must once have moved in a nearly circular path. The fact that tidal friction is competent to explain the eccentricity of an orbit has been applied in a manner to which I shall have occasion to return hereafter.

In my paper on this subject I summed up the discussion in the following words, which I still see no reason to retract:—

"The argument reposes on the imperfect rigidity of solids, and on the internal friction of semi-solids and fluids; these are *verae causae*. Thus changes of the kind here discussed must be going on, and must have gone on in the past. And for this history of the earth and moon to be true throughout it is only necessary to postulate a sufficient lapse of time, and that there is not enough matter diffused through space materially to resist the motions of the moon and earth in perhaps several hundred million years.

"It hardly seems too much to say that granting these two postulates and the existence of a primeval planet, such as that above described, then a system would necessarily be developed which would bear a strong resemblance to our own.

"A theory, reposing on *verae causae*, which brings into quantitative correlation the lengths of the present day and month, the obliquity of the ecliptic, and the inclination and eccentricity of the lunar orbit, must, I think, have strong claims to acceptance."[1]

We have pursued the changes into the past, and I will refer but shortly to the future. The day and month are both now lengthening, but the day changes more quickly than the month. Thus the two periods tend again to become equal to one another, and it appears that when that goal is reached both day and month will be as long as fifty-five of our present days. The earth will then always show the same face to the moon, just as it did in the remotest past. But there is a great contrast between the ultimate and initial conditions, for the ultimate stage, with day and month both equal to fifty-five of our present days, is one of great stability in contradistinction to the vanishing stability which we found in the initial stage.

Since the relationship between the moon and earth is a mutual one, the earth may be regarded as a satellite of the moon, and if the moon rotated rapidly on her axis, as was probably once the case, the earth must at that time have produced tides in the moon. The mass of the moon is relatively small, and the tides produced by the earth would be large; accordingly the moon would pass through the several stages of her history much more rapidly than the earth. Hence it is that the moon has already advanced to that condition which we foresee as the future fate of the earth, and now always shows to us the same face.

If the earth and moon were the only bodies in existence, this ultimate stage when the day and month were again identical in length would be one of absolute stability, and therefore eternal; but the presence of the sun introduces a cause for yet further changes. I do not, however, propose to pursue the history to this yet remoter futurity, because our system must contain other seeds of decay which will probably bear fruit before these further transformations could take effect.

If, as has been argued, tidal friction has played so important a part in the history of the earth and moon, it might be expected that the like should be true of the other planets and satellites, and of the planets themselves in their relationship to the sun. But numerical examination of the several cases proves conclusively that this cannot have been the case. The relationship of the moon to the earth is in fact quite exceptional in the solar system, and we have still to rely on such theories as that of Laplace for the explanation of the main outlines of the solar system.

I have as yet only barely mentioned the time occupied by the sequence of events sketched out in the various schemes of cosmogony, and the question of cosmical time is a thorny and controversial one.

Our ideas are absolutely blank as to the time requisite for the evolution according to Laplace's nebular hypothesis. And again, if we adopt the meteoritic theory, no estimate can be formed of the time required even for an ideal sun, with its attendant planet Jove, to sweep up the wanderers in space. We do know, indeed, that there is a continuous gradation from stable to unstable orbits, so that some meteoric stones may make thousands or millions of revolutions before meeting their fate by collision. Accordingly, not only would a complete absorption of all the wanderers occupy an infinite time, but also the amount of the refuse of the solar system still remaining scattered in planetary space is unknown. And, indeed, it is certain that the process of clearance is still going on, for the earth is constantly meeting meteoric stones, which, penetrating the atmosphere, become luminous through the effects of the frictional resistance with which they meet.

All we can assert of such theories is that they demand enormous intervals of time as estimated in years.

The theory of tidal friction stands alone amongst these evolutionary speculations in that we can establish an exact but merely relative time-scale for every stage of the process. It is true that the value in years of the unit of time remains unknown, and it may be conjectured that the unit has varied to some extent as the physical condition of the earth has gradually changed.

It is, however, possible to determine a period in years which must be shorter than that in which the whole history is comprised. If at every moment since the birth of the moon tidal friction had always been at work in such a way as to produce the greatest possible effect, then we should find that sixty million years would be consumed in this portion of evolutionary history. The true period must be much greater, and it does not seem extravagant to suppose that 500 to 1000 million years may have elapsed since the birth of the moon.

Such an estimate would not seem extravagant to geologists who have, in various ways, made exceedingly rough determinations of geological periods. One such determination is derived from measures of the thickness of deposited strata, and the rate of the denudation of continents by rain and rivers. I will not attempt to make any precise statement on this head, but I imagine that the sort of unit with which the geologist deals is 100 million years, and that he would not consider any estimate involving from one to twenty of such units as unreasonable.

Mellard Reade has attempted to determine geological time by certain arguments as to the rate of denudation of limestone rocks, and arrives at the conclusion that geological history is comprised in something less than 600 million years.[1] The uncertainty of this estimate is wide, and I imagine that geologists in general would not lay much stress on it.

Joly has employed a somewhat similar, but probably less risky, method of determination.[2] When the earth was still hot, all the water of the globe must have existed in the form of steam, and when the surface cooled that steam must have condensed as fresh water. Rain then washed the continents and carried down detritus and soluble matter to the seas. Common salt is the most widely diffused of all such soluble matter, and its transit to the sea is an irreversible process, because the evaporation of the sea only carries back to the land fresh water in the form of rain. It seems certain, then, that the saltness of the sea is due to the washing of the land throughout geological time.

Rough estimates may be formed of the amount of river water which reaches the sea in a year, and the measured saltness of rivers furnishes a knowledge of the amount of salt which is thus carried to the sea. A closer estimate may be formed of the total amount of salt in the sea. On dividing the total amount of salt by the annual transport Joly arrives at the quotient of about 100 millions, and thence concludes that geological history has occupied 100 million years. I will not pause to consider the several doubts and difficulties which arise in the working out of this theory. The uncertainties involved must clearly be considerable, yet it seems the best of all the purely geological arguments whence we derive numerical estimates of geological time. On the whole I should say that pure geology points to some period intermediate between 50 and 1000 millions of years, but the upper limit is more doubtful than the lower. Thus far we do not find anything which renders the tidal theory of evolution untenable.

But the physicists have formed estimates in other ways which, until recently, seemed to demand in the most imperative manner a far lower scale of time. According to all theories of cosmogony, the sun is a star which became heated in the process of its condensation from a condition of wide dispersion. When a meteoric stone falls into the sun the arrest of its previous motion gives rise to heat, just as the blow of a horse's shoe on a stone makes a spark. The fall of countless meteoric stones, or the condensation of a rarefied gas, was supposed to be the sole cause of the sun's high temperature.

Since the mass of the sun is known, the total amount of the heat generated in it, in whatever mode it was formed, can be estimated with a considerable amount of precision. The heat received at the earth from the sun can also be measured with some accuracy, and hence it is a mere matter of calculation to determine how much heat the sun sends out in a year. The total heat which can have been generated in the sun divided by the annual

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[1] *Phil. Trans.*, pt. ii., 1880, p. 883.

[1] "Chemical Denudation in Relation to Geological Time," Bogue, London, 1879; or Roy. Soc., January 23. 1879.

[2] "An Estimate of the Geological Age of the Earth," *Trans. Roy. Dub. Soc.*, vol. vii. series iii., 1902, pp. 23-66.

output gives a quotient of about 20 millions. Hence it seemed to be imperatively necessary that the whole history of the solar system should be comprised within some 20 millions of years.

This argument, which is due to Helmholtz, appeared to be absolutely crushing, and for the last forty years the physicists have been accustomed to tell the geologists that they must moderate their claims. But for myself I have always believed that the geologists were more nearly correct than the physicists, notwithstanding the fact that appearances were so strongly against them.

And now, at length, relief has come to the strained relations between the two parties, for the recent marvellous discoveries in physics show that concentration of matter is not the only source from which the sun may draw its heat.

Radium is a substance which is perhaps millions of times more powerful than dynamite. Thus it is estimated that an ounce of radium would contain enough power to raise 10,000 tons a mile above the earth's surface. Another way of stating the same estimate is this: the energy needed to tow a ship of 12,000 tons a distance of six thousand sea miles at 15 knots is contained in 22 ounces of radium. The *Saxon* probably burns five or six thousand tons of coal on a voyage of approximately the same length. Again, M. and Mme. Curie have proved that radium actually gives out heat,[1] and it has been calculated that a small proportion of radium in the sun would suffice to explain its present radiation. Other lines of argument tend in the same direction.[2]

Now we know that the earth contains radio-active materials, and it is safe to assume that it forms in some degree a sample of the materials of the solar system. Hence it is almost certain that the sun is radio-active also; and besides it is not improbable that an element with so heavy an atom as radium would gravitate more abundantly to the central condensation than to the outlying planets. In this case the sun should contain a larger proportion of radio-active material than the earth.

This branch of science is as yet but in its infancy, but we already see how unsafe it is to dogmatise on the potentialities of matter.

It appears, then, that the physical argument is not susceptible of a greater degree of certainty than that of the geologists, and the scale of geological time remains in great measure unknown.

I have now ended my discussion of the solar system, and must pass on to the wider fields of the stellar universe.

Only a few thousand stars are visible with the unaided eye, but photography has revealed an inconceivably vast multitude of stars and nebulæ, and every improvement in that art seems to disclose yet more and more. About twenty years ago the number of photographic objects in the heavens was roughly estimated at about 170 millions, and some ten years later it had increased to about 400 millions. Although Newcomb, in his recent book on "The Stars," refrains even from conjecturing any definite number, yet I suppose that the enormous number of 400 million must now be far below the mark, and photography still grows better year by year. It seems useless to consider whether the number of stars has any limit, for infinite number, space, and time transcend our powers of comprehension. We must then make a virtue of necessity, and confine our attention to such more limited views as seem within our powers.

A celestial photograph looks at first like a dark sheet of paper splashed with whitewash, but further examination shows that there is some degree of method in the arrangement of the white spots. It may be observed that the stars in many places are arranged in lines and sweeping trains, and chains of stars, arranged in roughly parallel curves, seem to be drawn round some centre. A surface splashed at hazard might present apparent evidence of system in a few instances, but the frequency of the occurrence in the heavens renders the hypothesis of mere chance altogether incredible.

Thus there is order of some sort in the heavens, and, although no reason can be assigned for the observed arrangement in any particular case, yet it is possible to obtain general ideas as to the succession of events in stellar evolution.

Besides the stars there are numerous streaks, wisps, and agglomerations of nebulosity, the light of which we know to emanate from gas. Spots of intenser light are observed in less brilliant regions; clusters of stars are sometimes imbedded in nebulosity, while in other cases each individual star of a cluster stands out clear by itself. These and other observations force on us the conviction that the wispy clouds represent the earliest stage of development, the more condensed nebulæ a later stage, and the stars themselves the last stage. This view is in agreement with the nebular hypothesis of Laplace, and we may fairly conjecture that the chains and lines of stars represent pre-existing streaks of nebulosity.

As a star cools it must change, and the changes which it undergoes constitute its life-history, hence the history of a star presents an analogy with the life of an individual animal. Now, the object which I have had in view has been to trace types or species in the physical world through their transformations into other types. Accordingly it falls somewhat outside the scope of this address to consider the constitution and history of an individual star, interesting although those questions are. I may, however, mention that the constitution of gaseous stars was first discussed from the theoretical side by Lane, and subsequently more completely by Ritter. On the observational side the spectroscope has proved to be a powerful instrument in analysing the constitutions of the stars, and in assigning to them their respective stages of development.

If we are correct in believing that stars are condensations of matter originally more widely spread, a certain space surrounding each star must have been cleared of nebulosity in the course of its formation. Much thought has been devoted to the determination of the distribution of the stars in space, and although the results are lacking in precision, yet it has been found possible to arrive at a rough determination of the average distance from star to star. It has been concluded, from investigations into which I cannot enter, that if we draw a sphere round the sun with a radius of twenty million millions of miles,[1] it will contain no other star; if the radius were twice as great the sphere might perhaps contain one other star; a sphere with a radius of sixty million millions of miles will contain about four stars. This serves to give some idea of the extraordinary sparseness of the average stellar population; but there are probably in the heavens urban and rural districts, as on earth, where the stars may be either more or less crowded. The stars are moving relatively to one another with speeds which are enormous, as estimated by terrestrial standards, but the distances which separate us from them are so immense that it needs refined observation to detect and measure the movements.

Change is obviously in progress everywhere, as well in each individual nebula and star as in the positions of these bodies relatively to one another. But we are unable even to form conjectures as to the tendency of the evolution which is going on. This being so, we cannot expect, by considering the distribution of stars and nebulæ, to find many illustrations of the general laws of evolution which I have attempted to explain; accordingly I must confine myself to the few cases where we at least fancy ourselves able to form ideas as to the stages by which the present conditions have been reached.

Up to a few years ago there was no evidence that the law of gravitation extended to the stars, and even now there is nothing to prove the transmission of gravity from star to star. But in the neighbourhood of many stars the existence of gravity is now as clearly demonstrated as within the solar system itself. The telescope has disclosed the double character of a large number of stars, and the relative motions of the pairs of companions have been observed with the same assiduity as that of the planets. When the relative orbit of a pair of binary or double stars is examined, it is found that the motion conforms exactly to those laws of Kepler which prove that the planets circle

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[1] Lord Kelvin has estimated the age of the earth from the rate of increase of temperature underground. But the force of his argument seems to be entirely destroyed by this result.

[2] See W. E. Wilson, NATURE, July 9, 1903; and G. H. Darwin, NATURE, September 24, 1903.

[1] This is the distance at which the earth's distance from the sun would appear to be 1″.

round the sun under the action of solar gravitation. The success of the hypothesis of stellar gravitation has been so complete that astronomers have not hesitated to explain the anomalous motion of a seemingly single star by the existence of a dark companion; and it is interesting to know that the more powerful telescopes of recent times have disclosed, in at least two cases, a faintly luminous companion in the position which had been assigned to it by theory.

By an extension of the same argument, certain variations in the spectra of a considerable number of stars have been pronounced to prove them each to be really double, although in general the pair may be so distant that they will probably always remain single to our sight. Lastly, the variability in the light of other apparently single stars has proved them to be really double. A pair of stars may partially or wholly cover one another as they revolve in their orbit, and the light of the seemingly single star will then be eclipsed, just as a lighthouse winks when the light is periodically hidden by a revolving shutter. Exact measurements of the character of the variability in the light have rendered it possible not only to determine the nature of the orbit described, but even to discover the figures and densities of the two components which are fused together by the enormous distance of our point of view. This is a branch of astronomy to which much careful observation and skilful analysis has been devoted; and I am glad to mention that Alexander Roberts, one of the most eminent of the astronomers who have considered the nature of variable stars, is a resident in South Africa.

I must not, however, allow you to suppose that the theory of eclipses will serve to explain the variability of all stars, for there are undoubtedly others the periodicity of which must be explained by something in their internal constitution.

The periods of double stars are extremely various, and naturally those of short period have been the first noted; in times to come others with longer and longer periods will certainly be discovered. A leading characteristic of all these double stars is that the two companions do not differ enormously in mass from one another. In this respect these systems present a strongly marked contrast with that of the sun, attended as it is by relatively insignificant planets.

In the earlier part of my address I showed how theory indicates that a rotating fluid body will as it cools separate into two detached masses. Mathematicians have not yet been able to carry their analysis far enough to determine the relative magnitudes of the two parts, but so far as we can see the results point to the birth of a satellite the mass of which is a considerable fraction of that of its parent. Accordingly See (who devotes his attention largely to the astronomy of double stars), Roberts, and others consider that what they have observed in the heavens is in agreement with the indications of theory. It thus appears that there is reason to hold that double stars have been generated by the division of primitive and more diffused single stars.

But if this theory is correct we should expect the orbit of a double star to be approximately circular; yet this is so far from being the case that the eccentricity of the orbits of many double stars exceeds by far any of the eccentricities in the solar system. Now See has pointed out that when two bodies of not very unequal masses revolve round one another in close proximity the conditions are such as to make tidal friction as efficient as possible in transforming the orbit. Hence we seem to see in tidal friction a cause which may have sufficed not only to separate the two component stars from one another, but also to render the orbit eccentric.

I have thought it best to deal very briefly with stellar astronomy, in spite of the importance of the subject, because the direction of the changes in progress is in general too vague to admit of the formation of profitable theories.

We have seen that it is possible to trace the solar system back to a primitive nebula with some degree of confidence, and that there is reason to believe that the stars in general have originated in the same manner. But such primitive nebulæ stand in as much need of explanation as their stellar offspring. Thus, even if we grant the exact truth of these theories, the advance towards an explanation of the universe remains miserably slight. Man is but a microscopic being relatively to astronomical space, and he lives on a puny planet circling round a star of inferior rank. Does it not then seem as futile to imagine that he can discover the origin and tendency of the universe as to expect a housefly to instruct us as to the theory of the motions of the planets? And yet, so long as he shall last, he will pursue his search, and will no doubt discover many wonderful things which are still hidden. We may indeed be amazed at all that man has been able to find out, but the immeasurable magnitude of the undiscovered will throughout all time remain to humble his pride. Our children's children will still be gazing and marvelling at the starry heavens, but the riddle will never be read.

SECTION E.

GEOGRAPHY.

OPENING ADDRESS BY REAR-ADMIRAL SIR W. J. L. WHARTON, K.C.B., F.R.S., PRESIDENT OF THE SECTION.

It is sometimes denied to Geography that she has any right to consider herself as a science, the objection being apparently founded on the view that it is a subject that can be learnt by heart, but not studied on any systematic line or reduced to principles which enable advance to be made, as in the more exact sciences, by continual investigation by means of laws discovered in the course of such investigation. This, it appears to me, is a misapprehension due to an incomplete recognition of what Science is, and of what Geography is.

Science is, in its simplest interpretation, "knowledge," such knowledge as comes from an intimate acquaintance with and study of any subject duly coordinated and arranged. The subjects which the advancing education and civilisation of the world have caused to be minutely studied are very many, and as knowledge has increased specialisation has become a necessity, until the list of sciences is very long.

Science may be broadly divided into several categories.

Pure or Exact Science, such as Mathematics; Natural or Physical Science, which rests on observations of Nature; Moral Science, which treats of all mental phenomena.

Some Sciences are of ancient foundation, some have arisen from new inquiries and needs of man, or from fissure in subjects too wide for convenient treatment as one.

Many of them are capable of exact definition, and their boundaries and limits can be well marked.

To others no very distinct limitations can be assigned. From their nature they overlap and are overlapped by other subjects, and it is impracticable to confine them by a strict line.

Geography is one of the latter.

Geography is one of the most ancient subjects studied with the view of coordinating facts. A desire for exact knowledge of, first, the bearings and distances of one place from another for the purposes of intercommunication must have arisen as soon as men became collected into groups whose growing civilisation and needs required travel to obtain what could not be obtained in the community. This was the earliest form of Geography, and it is an aspect which still remains, and to some is, in the modern shape of maps, the principal, if not the sole, end of Geography.

From the earliest times, however, geographical information included other than topographical data.

It was soon found that for the traveller and statesman, whether in peace or war, more was wanted to enable Geography to supply requirements.

The nature of a country, the supply of food and water, the characters of the rivers, the manners and customs of the inhabitants, their language and affinities, the climate, and other matters, were all of much moment, and Geography dealt with them all, being, as its name denotes, in the broadest sense a "description of the earth."

After the first crude guesses of relative positions, founded on times occupied on journeys, other knowledge was enlisted in the cause.

Astronomy was soon recognised as the only means by which to ascertain the distances of places far apart and separated by seas, but for many centuries this could only be applied to latitude. Still the scientific geographer had to study and use the astronomical and geodetic methods known.

As knowledge increased, the subjects became too wide to be strictly considered as one study, and many have become the objects of special research under different titles.

Geodesy deals with the precise form of the earth and its dimensions.

Geology studies the nature of the materials forming the earth's crust, and the changes in it in past ages.

Ethnology and *Anthropology* treat of the different races of mankind.

The study of *Economics* takes note of the conditions of communities and nations, their laws and systems of government.

Botany and *Zoology* now concern themselves with the details of vegetable and animal life.

Archæology investigates the remains of past civilisations which cover the earth.

Meteorology strives to unravel and reduce to law the complicated conditions of the atmosphere, its continual movements, and the results which have such varying effect on our daily life.

Oceanography, the study of the phenomena of the sea as distinct from the dry land, is still regarded as an integral part of Geography, but is rapidly becoming a subject by itself.

Of all these subjects Geography may be considered to be the parent; and though the family be large and has gone off on many separate lines, it is necessary when taking a large and comprehensive view of the united results of knowledge thus gained, especially from the point of view of Distribution, to return to that parent and consider them on a general or geographical basis.

I cannot pretend to define Geographical Science in a clearer or shorter form than that in which it has been already put by General Sir Richard Strachey, and I will quote his words:—

"To investigate and delineate the various features of the earth, to study the distribution of land and sea, and their effects upon climate, the configuration and relief of the surface, positions on the globe, and so forth, facts which determine the existent conditions of various parts of the earth, or which indicate former conditions, and to ascertain the relations that exist between those features and all that is observed on the earth."

Strabo, in the opening words of his introduction to his great Geography, puts it thus:—

"If the scientific investigation of any subject be the proper avocation of the philosopher, Geography, the science of which we propose to treat, is certainly entitled to a high place. In addition to its vast importance in regard to social life and the art of government, Geography unfolds to us the celestial phenomena, acquaints us with the occupants of the land and ocean, and the vegetation, fruits, and peculiarities of the various quarters of the earth."

This was written when Geography included all natural science, and before it gave birth to so many separate subjects; but it sets forth so admirably the aims which the geographer still pursues that it is worthy of remembrance.

It is not advocated, nor is it in any way necessary, that all should study Geography in the extended sense thus indicated; but it cannot be too strongly pointed out that an educated man—and education is now essential to the successful conduct of affairs—must have a considerable knowledge of the elementary facts of Geography.

These elementary facts are, it is true, of the nature of a lesson, and must be learnt, so to speak, by heart by the aid of maps and books; but this is nothing more than making use of the labours of others without which no advance is possible in any subject, and is common to all studies.

We must, in fact, distinguish between the science of Geography, which consists in ascertaining and coordinating new facts, and putting them into a shape for the use of others, which is the work of comparatively few; and the practical Geography which consists of making use of that work, and, as in many other branches of science, is within the reach of all who choose to devote time to it.

It is the object and business of the British Association to try to interest their fellow-countrymen in all branches of knowledge, to gain if possible more workers in science, but at any rate to induce all educated persons to take advantage of the solid work done by others towards the elucidation of the details of the glorious Nature which surrounds us on all sides, and in so many forms, and without which ignorance and superstition, those primary bars to the advancement of mankind, can never be banished.

It is impossible to have a clear comprehension of history, whether past or current, without calling in the aid of Geography; but unfortunately much history has been written and taught without such aid.

To read the daily paper requires either geographical knowledge or constant reference to maps; and if readers would only make a practice of such reference on every occasion when they are at fault, they would soon find themselves acquiring knowledge of the greatest use to them in the easiest and most interesting manner, and with the smallest expenditure of time.

The mistakes made even by those responsible for the conduct of public affairs, by reason of the want of this essential but elementary knowledge, are innumerable, and to this day there are many who consider themselves highly educated and capable men who cannot even rightly understand a map.

As I have before indicated, good maps are the foundation of all sound geographical knowledge, and these maps must be founded on good surveys.

Now a good survey is a comparatively modern operation, and the parts of the world that have been subjected to it are small indeed.

It is true that we now have general maps of the larger parts of the world, which more or less convey a fair representation of the configuration of land and sea when large areas are considered, but details are sadly lacking almost everywhere.

It is not astonishing, for to make the necessary surveys requires an enormous expenditure of both time and money, and the latter is hard to get until the necessity for its expenditure is patent to the smallest intelligence. Thus many countries long settled and in a high state of civilisation are still without any organised system of survey or maps, and even in the United Kingdom it is only from the year 1784 that a proper survey was established of the British Isles, though no maps were published from it until 1801; and it has proceeded so slowly that it has only recently been in one sense completed, while its revision, badly wanted on account of changes, is still in active prosecution, and must be continued *ad infinitum*.

Such indifference is, however, giving way to experience of the results of absence of proper maps, and all who wish well to the progress of South Africa must be pleased at finding that their provision has been taken in hand on such an admirably scientific basis as is provided by the Trigonometrical Survey, now far advanced, and the successful progress of which is, I believe, greatly due to the inexhaustible energy of my friend Sir David Gill, who seems to find time to promote and aid all branches of knowledge, and that steps are now being taken to prosecute the detailed topographical survey and provide good maps.

To many people one map is as good as another. They do not pause to consider on what it is based, or what degree of accuracy it probably possesses, but so long as there is a map they are satisfied.

A vast number of existing maps are compiled from the roughest materials: in partly occupied countries, from drawings of small areas placed together as can best be done, by means of places here and there the relative positions of which are fairly known by distances along roads, with perhaps in some cases angles and astronomical positions; in less civilised parts by routes of travellers laid down by estimation of the distance traversed and direction of march, checked perhaps by a few astronomical observations of more or less value as the traveller possesses or does not possess the necessary skill.

The compilers of such a map have a difficult task. Discrepancies are, of course, multitudinous. Nothing agrees, and one has to accept, reject, and adjust as best he can on his own responsibility and with what knowledge he can procure of the respective trustworthiness of each author.

Happy is he if he has even a few positions in his map which have been properly determined, as between them he is saved from the constantly increasing errors of adding one little area to another, which if carried on indefinitely culminates in great errors.

Of course such maps are of no practical use, save as giving a very general idea of a country, and when required by the administrator or traveller lead to endless mistakes and annoyances.

The feature of our globe which is now, broadly speaking, most accurately laid down is the coast-line. The safety of navigation has caused general marine surveys to be carried on all over the world during the nineteenth century, which have finally determined the position and shape of the boundaries of the sea.

These surveys, executed for the most part by skilled naval officers with proper instrumental outfit, and supplied especially with trustworthy chronometers, and based upon frequent carefully determined astronomical positions, have resulted in this boundary line being delineated with an accuracy, so far as its absolute position is concerned, far in advance of any other main feature in maps.

Here I may perhaps explain to those unversed in these matters why this is so.

The position of any spot on the earth's surface can be ascertained in two ways: either by careful measurement by means of an accurate system of triangles from another spot already fixed, or by independent observations of the heavenly bodies and calculations from them, which give the precise latitude and longitude of the place. The former is suitable for positions inland, but entails much time and labour, and is only adopted when a perfect map is to be made, for which it is the indispensable foundation. The latter can be carried on from a ship, and in most circumstances only from a ship, because of the limitations of the methods of determinating longitudes.

Longitude can now be satisfactorily and rapidly ascertained in two ways: by the electric telegraph or by use of chronometers.

The places served by the electric telegraph are still few, and its use is therefore restricted; but the chronometer has been in working use for more than a hundred years.

This instrument, which is merely a watch of especial construction, will only keep a steady rate when it is undisturbed by irregular shocks or motions.

No means have yet been found for transporting a chronometer on land without upsetting its regularity, and therefore rendering it useless; but on board a ship it can be so suspended and stowed as to prevent its being disturbed by any ordinary movements of or in the ship. The accurate time of any place departed from, ascertained by astronomical observations, can therefore be carried about on board ship for considerable periods, and by comparison with the local time, also determined by sextant observations of the heavenly bodies, at any required spot on the coast, the difference of longitude is at once obtained with very small limits of error when a number of chronometers are employed. These two simple yet marvellous instruments, the sextant and the chronometer, have thus placed in the hands of sailors ready means of fixing with great exactitude and celerity the position of selected points on coasts all over the world; and it will be seen that, while the detail of the line of coast between such fixed positions will depend upon the degree of accuracy of the survey or sketch, the general line cannot get far out, as it is constantly checked at the selected points.

It is not claiming too much to say that at the present time very few salient points on the coast-lines of the world are as much as two miles in doubt.

It should be a source of great satisfaction to the Briton to know that both these instruments were devised by Englishmen, John Hadley producing the sextant in 1730, in the form still used, on the basis of ideas formulated by Newton fifty years before; and John Harrison the chronometer in 1736. The latter instrument has undergone modifications in detail, but the principle remains the same. It required seventy years before its value was fully recognised and it came into general use.

It is a still further satisfaction to think that it is British naval officers who have made by far the greatest use of them in mapping the coasts of the whole world. Since the time of the great Captain Cook British surveying vessels have been constantly employed in this work, not only in British colonies, but in all parts, aiding and often paving the way for British commerce, and for the men-of-war that protect it.

It is difficult to find coasts of any extent that have not been laid down by British marine surveyors. The whole of Africa has been their work. By far the greater part of America, all the south and east coasts of Asia, Australia, and most of the innumerable islands in all oceans have been fixed and laid down by them. Even in the Mediterranean, until very lately, the charts were mostly founded on British surveys, and the improvements now being carried out by other nations on their own coasts in details required for modern navigation do not materially modify the main shapes and positions formerly determined by the British.

It has been, and is, a great work, and I hope I may be pardoned for dwelling on it with pride as the result of the wise administration of the Admiralty for many years, and of the immediate labours of my predecessors as Hydrographer, and as a very great contribution to geographical knowledge, more especially as I do not think that it is generally realised that this great advance in geographic accuracy is due to marine surveyors.

To give an idea of the comparative accuracy of the chronometer method, I may mention that on taking at hazard eleven places distributed all over the world at great distances from England, the longitudes of which have been recently determined by means of the electric telegraph and elaborate series of observations, I find that the average difference between the chronometer and the telegraph positions is 700 yards. The shapes of the different continents and the positions of islands as at present on our maps and charts will never be altered except in insignificant degree, and the framework is ready for many years' work of land mapping.

It is not to be inferred from what I say that marine surveys are approaching their close. It is far otherwise. The time given to these enormous extents of coasts and seas, and the necessarily small scales on which the surveys have been carried on, have caused them to be very imperfect in all details. Hundreds of rocks and shoals, both stretching from the land and isolated in the sea, have been missed in the course of them, and loss of ships and life on these unknown dangers still continues. With the increase of shipping, increased number of ships of heavy draught, the closeness of navigation due to steam, and the desire to make quick passages, smaller inaccuracies of the charts become yearly of greater importance.

As an illustration of the condition of affairs I may mention that in Hamoaze, the inner harbour of Plymouth, one of the headquarters of the British fleet for more than 300 years, a small but dangerous pinnacle of rock was only discovered five years ago; whilst numerous other dangers of a similar character have been yearly revealed in close surveys of other harbours in the United Kingdom, supposed to be well examined and charted in the last century.

There never was a greater need for close marine surveys of places frequented by ships than now.

It is interesting to look back and see the gradual progress of the delineation of the world and to mark how very recent any approach to accuracy is.

The very earliest maps of any extent of country are unfortunately lost to us. The first man who made a map of which any historical record exists is Anaximander of Miletus, about 600 B.C., but we know nothing of it. A map is mentioned by Herodotus as having been taken in 500 B.C. by Aristagoras of Miletus in the shape of an engraved bronze plate whereon the whole circuit of the earth was engraved, with all its seas and rivers, to influence Cleomenes, King of Sparta, to aid the Ionians against Persia. This was probably the work of Hecatæus, to whom early Geography owed much. His works are also only known to us by quotation; but they are especially interesting as containing an early idea of the limits of Africa, which he represents as entirely surrounded by the sea—a circumstance apparently either forgotten or disbelieved in later years.

Erotosthenes, 250 B.C., and Hipparchus, 150 B.C., made great advances, and the former made the first attempt to measure the size of the earth by the difference of latitudes between Assouan and Alexandria in Egypt, an attempt which, considering the great imperfection of his means, was remarkably successful, as, assuming that we are right in the length of the stadium he used, he made the circumference of the globe 25,000 geographical miles, whereas it should be 21,600.

He also devised the system of meridians and parallels as we now have them; but the terms "latitude" and "longitude," to denote positions on those circles, were introduced by Ptolemy.

The maps of Ptolemy, the great Alexandrian astronomer and geographer of A.D. 150, are the earliest we possess. He drew, besides a general map of the whole known world from the southern part of the Baltic to the Gulf of Guinea, north and south, and from the Canary Islands to the China Sea, east and west, a series of twenty-six maps of the different parts.

Ptolemy's maps and his method of representing the spherical globe on a flat surface had a great influence on Geography for many years. After his time the Greek civilisation waned, and the general decline of the Roman Empire, followed by its disruption by the invasion of barbarians, closed the course of discovery in all branches of research for centuries. It is not too much to say that for 1300 years no advance was made, and until the commencement of exploration by sea, which accompanied the general revival of learning in the fifteenth century, Ptolemy's maps represented the knowledge of the world.

As might be expected, the further he got from the Mediterranean, the greater were his errors; and his representations of Eastern Asia and North-Western Europe are somewhat grotesque, though quite recognisable in the main.

Of Africa south of the Equator he knows nothing, and his map of it terminates with the border.

This is somewhat remarkable, as I am one of those who firmly believe in the circumnavigation of Africa by the Phœnicians sent by Necho, King of Egypt, in 600 B.C. from the head of the Red Sea. As described by Herodotus, the voyage has all the impress of veracity. My personal faith in Herodotus was much strengthened by finding when I surveyed the Dardanelles in 1872 that his dimensions of that strait were nearer the truth than those of other and later authorities, even down to the time at which I was at work, as well as by other geographical tests I was able to apply. When, therefore, he records that the Phœnicians declared that in their voyage they had the sun on their right hand, and says he does not believe it, he registers an item of information which goes far to prove the story correct. Influenced by Hecatæus, who though surrounding Africa by the sea cut it far short of the Equator, Herodotus could not conceive that the travellers had passed to the south of the sun when it was in the southern tropic.

No historical incident has been more discussed than this voyage, commentators varying much in their opinions of its truth. But we have to-day some new facts. No one who has followed the exploration of the ancient buildings in Rhodesia, and considered the information we possess on the early inhabitants of Southern Arabia, whether we call them Sabæans or Himyarites, can doubt that the former were mainly the work of men coming from Arabia at a very early date, while the period of time necessary to carry out gold-mining operations over the large areas now found to have been exploited must have been very great.

It seems strange that no record of the constant voyages to this El Dorado should remain, but the very natural desire to keep lucrative information to themselves is not an unknown thing amongst traders of the present day, while the conditions of society and the absence of written records of South Arabia would make concealment easy.

The Phœnicians, an allied race, and the great seafaring trading nation of the Mediterranean, succeeded in keeping the majority of their marts secret, and we have incidents recorded showing their determination not to allow others to follow their steps, while to this day we are very doubtful of the limits of their voyages.

It may be considered certain that while we naturally quote Greek historians and geographers as the early authorities for the growth of geographical knowledge, and that the scientific basis for proper maps of large areas was really provided by them, the seafaring nations, Arabians, Phœnicians, and Chinese, knew a very great deal practically of the coasts of various parts of the Old World that were absolutely unknown to the Greeks.

The favourable conditions afforded by those remarkable periodic winds, the monsoons, would in the China Sea, Bay of Bengal, and the Arabian Sea naturally facilitate any attempts at extensive sea voyages, and would lead to such attempts under conditions that in the regions of variable winds would be considered too dangerous and uncertain. The fact that the monsoons in nearly every case blow practically parallel to the coasts in opposite directions is a most important factor in considering early navigation. The direction of the wind itself in such cases roughly guides a vessel without a compass, and the periods of cyclones and unsettled weather between the monsoons would soon be noted and avoided, as they are to this day by the Arabs and Chinese, whose vessels, I have very little doubt, have remained practically the same for thousands of years.

The unknown Greek author of that unique and most interesting document, the "Periplus of the Erythræan Sea," probably of the first century A.D., describes vessels built without nails, the planks of which were bound together by cords, in precisely the same way as many Arab dhows now navigating the Indian Ocean. His personal knowledge of Africa evidently ceased at Cape Guardafui, though he gives information gained from others on the East Coast as far as Zanzibar, which—or, rather, a part on the mainland near—he describes as the limit of trade to the south. We know that Arabs had penetrated further, but no doubt they kept their knowledge to themselves.

These early navigators very probably had charts. When Vasco da Gama first passed along the eastern coast of Africa he found that the Arab dhows had charts. Unfortunately none of them has come down to us, or it would have been interesting to compare them with those of the West Coast used by the Portuguese at the time, and which were of the crudest description.

I claim for sailors of all ages that they would be the first to make practical maps of the shape of the coasts. Their safety and convenience demanded it, while it is a far easier task to compile such a picture of the earth from successive voyages along coasts over the sea, where average distances from known rates of sailing and courses from the sun and stars can be more accurately ascertained, than from long and generally tortuous land journeys in directions governed by natural features, towns, and so forth. A navigator *must* be a bit of an astronomer. A landsman to this day seldom knows one star from another.

It was the sea-charts, or *portolani*, of the Middle Ages that on the revival of learning first gave respectable representations of the shape of the coasts, at a time when the learned monks and others were drawing the most fantastic and absurd pictures which they called maps.

At the same time it must be remembered that in all ages and down to the present day pilots, who within a hundred years were usually carried by all ships, even for sea voyages, jealously keep their knowledge largely in their heads, and look upon good charts as contrivances to destroy their profession, and that such charts or notes as they had they would keep religiously to their fraternity.

The Egyptians were no sailors, but we know that they habitually employed Phœnicians for sea expeditions, while we have the historical record of the Old Testament for their employment by David and Solomon for a like purpose in the Red Sea, and probably far to the south. It is, therefore, almost impossible to doubt that the Phœnicians were also acquainted with the navigation of the Red Sea and east coast of Africa. Such a voyage as that recorded by Herodotus would in these circumstances be far from improbable.

The varying monsoons which had led the Arabians centuries before to get so intimate a knowledge of the east coast as to enable them to find and work the goldfields would be well known to the Phœnicians, and the hardy seamen who braved the tempestuous regions lying between

Cadiz and Great Britain would make little of the difficulties of the African seas.

The limit of easy navigation from and to the Red Sea is Sofala. I do not think that it is too great a use of imagination to suppose that it would be from information received in what is now North Rhodesia that it was learnt that to the westward lay the sea again, and that this led to the attempt to reach it by the south.

Once started from the neighbourhood of Sofala, they would find themselves in that great oceanic stream, the Agulhas Current, which would carry them rapidly to the southern extremity of Africa.

I, as a sailor, can also even conceive that finding themselves in that strong current they would be alarmed and attempt to turn back, and that after struggling in vain against it they would have accepted the inevitable and gone with it, and that without the Agulhas Current no such complete voyage of circumnavigation would have been made.

As Major Rennell in the last century pointed out, once past the Cape of Good Hope, the periodic winds, and over a great part of their journey the currents, would help them up the West African coast; and the general conditions of navigation are favourable the whole way to the Straits of Gibraltar, the ships keeping, as they would do, near the land; but we can well understand that, as recorded, the voyage occupied nearly three years, and that they halted from time to time to sow and reap crops. I should say that it is highly probable that either Simon's Bay or Table Bay was selected as one of these stopping-places.

No reference to this voyage has been found amongst the hieroglyphic records, and, indeed, so far few such records of Necho, whose reign was not for long, are known; but that it was regarded at the time as historical is evident, for Xerxes, a hundred years later, sent an expedition to repeat it in the contrary direction.

This, however, failed, and the unfortunate leader, Sataspes, was impaled on his unsuccessful return.

This attempt shows that the greater difficulty of the circumnavigation from west to east, as compared with that from east to west, was not realised, and points to the concealment of any details of the successful voyage.

Of Hanno's voyage from the Straits of Gibraltar to about Sierra Leone, the date of which is uncertain, but from 500 to 600 B.C., we should know little had not good fortune preserved the record deposited in a Carthaginian temple.

But the well-known secrecy of the Phœnicians in all matters connected with their foreign trade and voyages would explain why so little was known of Necho's voyage, and our present knowledge of the extensive ancient gold workings of Rhodesia shows how much went on in those times of which we are wholly ignorant.

I have dwelt perhaps too long on this subject, but it has to me a great interest; and as it has not, so far as I know, been dealt with by a seaman who is personally well acquainted with the ways of seamen in sailing ships and with the navigation of the coasts in question, I hope I may be excused for putting my views on record.

There are several references in Greek and Latin historians to other circumnavigations, but none of them can be trusted, and apart from Necho's voyage we hear nothing of the east and south coasts of Africa until the arrival of the Portuguese at the end of the fifteenth century. But they found a thriving civilisation along the coast from Sofala northward, Shirazi, Arab, and Indian.

Ruins exist in many places which have not yet been properly investigated, and we are quite unable to say from what date we are to place the earliest foreign settlements, nor how many breaks existed in the continuity of the gold-mining, which apparently was proceeding at or very shortly before the Portuguese visit.

After the recommencement of exploration by sea in the fifteenth century, seamen slowly gathered enough information to draw the lines of the coasts they passed along, and in time—that is, by the middle of the eighteenth century—most lands were shown with approximately their right shapes. But of true accuracy there was none, for the reason I have before mentioned, that there was no exact method of obtaining longitude.

If we look at a general world chart of A.D. 1755—and to get the best of that period we must consult a French chart—we shall find on this small scale that the shape of the continents is fairly representative of the truth. But when we examine details we soon see how crude it all is.

I have compared with their true positions the positions of thirty-one of what may be taken as the fundamental points in the world as given in the larger scaled French charts of 1755, from which the general one is drawn, and I find that on an average they are forty-eight miles in error. The errors vary from 160 miles to two miles. If the delineation of the coast-lines between be considered the inaccuracies are very much greater.

Very shortly after this date more accurate determinations began to be made. The method of lunar distances was perfected and facilitated by tables published in the various astronomical "ephemerides," and seamen and explorers commenced to make use of it. Still the observation required constant practice, and the calculation, unless constantly made, was laborious, and it was used with complete success by the few. The great Captain Cook, who may be looked upon as the father of modern methods of surveying, did much to show the value of this method; but the chronometer came into use shortly after, and the principal advance in exact mapping was made by its aid, as I have already stated.

There is a vast amount yet to be done for Geography. Until we possess publications to which we can turn for full information on all geographical aspects of things on this globe of ours, there is work to be done. Seeing that our present publications are only now beginning to be worthy of being considered trustworthy for the very small amount of knowledge that we already possess, geographical work in all its branches is practically never-ending.

But of exploration pure and simple very little remains to be done. The charm of travelling through and describing an entirely new country which may be practically serviceable to civilised man has been taken from us by our predecessors, though limited regions still remain in Central Asia and South America of which we know little in detail.

I must except the Polar regions, which are in a somewhat special category, as their opening-up affords few attractions to many people. But a knowledge of the past history of our globe—fit study for human thought—can only be gained by study of the portions still under glacial conditions.

What is there round the South Pole—a continent or a group of large islands? What is going on there? What thickness does ice attain? Have these regions always been glaciated; and if not, why not? Can we get any nearer the mystery of magnetism and its constant changes by study at or near the magnetic poles? All these and many other scientific questions can only be solved by general geographical research in these regions, and all interested in such questions have been delighted at the recent attempts to gain more knowledge.

The object of these expeditions was frankly and purely scientific. All hope of remunerative whale or seal fisheries had been dispelled by the visit of the Norwegian whalers in 1892 to the region south of Cape Horn, and the known general condition of the land forbade any expectation of other profitable industries, unless indeed gold and other valuable minerals should be found, which is always possible. Beyond the fact that exploring expeditions of this character keep alive the spirit of enterprise and bring out the finest characteristics of a race—which is a point by no means to be despised—no immediate practical benefit was to be expected.

Progress under the conditions must be slow, but I think that Great Britain may well be satisfied with the information collected in the Antarctic by Captain R. F. Scott and his gallant companions. The unfortunate detention of the *Discovery* by an unfavourable summer prevented the further coastal exploration which was part of the programme, but gave opportunity for further detailed examination of the inland conditions, which was carried out in defiance of the severest atmospheric and topographical difficulties, and with the greatest zeal and intelligence; and it may be doubted whether Science in the end has not gained more than she lost by the unexpected diversion of energy. The healthy conditions which prevailed throughout are a standing proof both of Captain Scott's eminent capacity as a leader and of the cheery spirit which animated the whole expedition.

The full results of the scientific observations are not yet worked out, and in many cases for a complete appreciation of their bearing they must be compared and correlated with those of the other Antarctic expeditions, but many highly suggestive points have already been revealed.

For the first time Antarctic continental land has been travelled over for long distances, and though the actual area of new discovery looks small on a map of the world, the distances covered can only be described as extraordinary, and far exceeding the most sanguine anticipations.

Few who considered the mountainous coast-line of Victoria Land and its complete glaciation, as reported by Sir James Ross from his distant view, thought that it would prove practicable not only to ascend those mountains, but to reach to heights much surpassing them behind.

The reason that it proved feasible is that, while there are occasional heavy snowstorms, the annual snowfall is small, and the surface, therefore, is generally unencumbered with soft deep snow.

And what did Captain Scott find after his memorable struggle up the glacier through the mountains?

An enormous plateau at an elevation of about 9000 feet, nearly level, smooth, and featureless, over which he travelled directly inland for more than 200 miles, seeing no sign at his furthest point of any termination or alteration in character. So far as could be seen from other journeys, glacial discharge from this great ice-sheet is very small, and practically it appears to be dead. Its accretion by fresh snowfall is insignificant, while on all sides along the flanks of the coastal mountains there are signs of diminution in the mass of ice.

The great ice-barrier east of Ross Island tells the same tale. This magnificent feature presents to the sea a face of perpendicular ice-cliffs varying from 60 to 240 feet in height and 450 sea-miles long. Sir J. Ross mapped its position in 1841, and Captain Scott finds that it has retreated on an average fifteen miles, varying much in different parts.

Should this rate of retreat continue the whole of this ice mass, so far as Captain Scott saw it, will have vanished in 1000 years.

As the motion of the ice mass is also about fifteen miles to the north in the same time, icebergs covering collectively an area of 450 miles by 30 have been discharged from it in sixty years.

Captain Scott travelled over it nearly due south to a point 300 miles from its face, and then saw no sign of its end.

It is bordered on its western side by a mountainous coast-line, rising in places to 15,000 feet He found the ice practically flat and wholly unfissured, except at the side, where its northerly motion, found to be about 130 feet in the month, caused shearing and vast crevasses. All that is known of its eastern edge is that it is bordered, where it meets the sea, by land from 2000 to 3000 feet high, suspected by Ross and verified by Captain Scott. This may be an island, or more probably the eastern side of the great fiord or bay now filled by the barrier.

Captain Scott is of opinion that this great ice-sheet is afloat throughout, and I entirely agree with this conclusion. It is unexpected, but everything points to it.

From soundings obtained along the face it undoubtedly has about 600 feet of water under it.

It is difficult to believe that this enormous weight of ice, 450 miles by at least 360, and perhaps very much more, with no fall to help it along by gravity, can have behind it a sufficient force in true land glacier to overcome the stupendous friction and put it in motion if it be resting on the bottom. It is sufficiently astonishing that there is force enough even to overcome the cohesion at the side, which must be very great.

The flat nature of the bottom of the Ross Sea and the analogies of many geographical details in other parts of the world make it most probable that the water under the whole barrier is deep.

A point on which I have seen no comment is the difference in the appearance of the slopes of Mount Terror. Captain Scott found the bare land showing over large areas, but during the two summers of Ross's visit it was wholly snow-clad. Sir Joseph Hooker, the sole survivor of Ross's expedition, when questioned had no doubt on the subject, and produced many sketches in support.

This may be due to temporary causes, but all the information collected by the expedition points without doubt to steadily diminishing glaciation in recent times. We have, therefore, this interesting fact, that both in Arctic and Antarctic regions, as indeed all over the world, ice conditions are simultaneously ameliorating, and theories of alternate northern and southern maximum glaciations seem so far disproved.

But this does not mean that climatic conditions in the Antarctic are now less severe—probably the contrary. It has been pointed out by many that land glaciation may arise from varied primary causes, but one obvious necessity is that the snowfall should exceed melting and evaporation. It need not be heavy; but if it is, it may produce glaciation under somewhat unexpected conditions. This would entail a vapour-laden air more or less continuously impinging upon the land at a temperature which will enable it when cooled, either by passing over chilled land or when raised to higher regions by the interposition of mountains, to give up its moisture freely. This condition is not fulfilled when the air as it arrives from the sea is already at a very low temperature.

It was my fortune to spend two long seasons in the Straits of Magellan, and I was daily more impressed by what I saw.

There you have a mountainous ridge of no great height —very few peaks rising more than 4000 feet—opposed to the almost continuous westerly winds pouring in from the Pacific at a very moderate temperature and charged with much moisture.

The result is that in the latitude of Yorkshire every mountain mass over 3000 feet high is covered with eternal snow, and sends glaciers down to the sea.

I was convinced by what was going on under my eyes that it only required an upheaval of the land of 2000 feet or so to cover the whole of Patagonia with ice. But then the climate would still not be very severe. The temperature of the wind from the sea would be the same, and such part of it as blew along the channels and on the lower land would moderate the cold caused by the ice-covered slopes.

The shores of the whole of Western Southern Patagonia, deeply indented with long and deep fiords, indicate, according to all received views of the origin of such formations, that the land was formerly higher, while signs of glaciation are everywhere present.

The results of geographical research show us that in many parts of the world climate must have greatly changed in comparatively recent times.

In the now arid regions of Northern Africa, Central North America, and in parts of Asia there is ample evidence that the climate was in times past more humid. In a remarkable paper on the causes of changes of climate, contributed by Mr. F. W. Harmer to the Geological Society in 1901, and which has not obtained the notice it deserves, it is pointed out how changes in the distribution of the prevalent winds would vastly alter climatic conditions. Like everything else in Nature, and especially in the department of meteorology, these questions are exceedingly complex, and similar results may be brought about in different ways, but there can be no doubt that the climate of South Africa would be greatly modified, and more rainfall would occur, if only the cyclonic storms which now chase each other to the eastward in the ocean south of the Cape of Good Hope could be prevailed upon to pursue a slightly more northerly line, and many obstacles to the agricultural prospects of South Africa now existing would be removed. This is, however, beyond the powers of man to effect; but, as I have just said, there are other ways of attaining the object, and it is earnestly to be hoped that the attention now being paid to afforestation may result in vigorous efforts to bring about by this means the improvement in humidity so much required in many parts of the country.

The other recent event in geographical exploration is the result of the expedition to Lhasa. It was an unexpected solution of this long-desired knowledge that it should come from political necessities and by means of a Government mission. The many ardent travellers who have dreamed of one day making their way in by stealth have thus been disappointed, but our knowledge is now fuller than could otherwise have been gathered.

The most important fact is the revelation of the fertility of a large part of Southern Tibet. Much has been added to topographical knowledge, but the route maps of the secret Indian native surveyors already had given us a rough knowledge of the country on the road to Lhasa. It was not, however, realised how great was the difference between the aridity of the vast regions of the north, known to us from the travels of men of various nationalities, and the better-watered area in the south, though from the great height of the plateau—some 12,000 feet—the climate is very severe. The upper course of the Brahmaputra has been traced by Captain Ryder, but, unfortunately, a political veto was placed on the project to solve the interesting problem of how this great river finds its way to the Indian plains, and this still remains for the future to unravel.

Of the ocean, which has been my own particular study for many years, and on which alone I feel any special qualification to speak, I have said but little, for the reason that when presiding over this Section on a former occasion I took it for my theme, but there are a few points regarding it which I should like to bring to your notice.

It is of the ocean, more than of any other physical feature of our globe, that our knowledge has increased of late years. Forty years ago we were profoundly ignorant even of its depth, with the exception of a few lines of soundings then recently taken for the first submarine telegraph cables, and consequently we knew nothing of its real vast bulk. As to the life in it, and the laws which govern the distribution of such life, we were similarly ignorant, as of many other details.

The *Challenger* expedition changed all this, and gave an impetus to oceanographic research which has in the hands of all nations borne much fruit.

Soundings have been obtained over all parts of the seas, even in the two polar seas; and though much remains to be done, we can now form a very close approximation to the amount of water on our earth, whilst the term "unfathomable ocean" has been shown to have been based on an entire misconception. Biological research has also revealed a whole world of living forms at all depths of the existence of which nothing was known before.

In my former Address, eleven years ago, I gave many details about the sea, of which I will only repeat one—which is a fact that everyone should know—and that is, that the bulk of the ocean is about fourteen times as great as that of the dry land above water, and that if the whole of that land were thrown into the Atlantic Ocean it would only fill one-third of it.

Eleven years ago the greatest depth known was 4700 fathoms, or 28,000 feet. We have since found several places in the Pacific where the depth is nearly 5170 fathoms, or 31,000 feet, or somewhat higher than Mount Everest, which has been lately definitely shown to be the culminating point of the Himalayas. These very deep parts of the ocean are invariably near land, and are apparently in the shape of troughs, and are probably due to the original crumpling of the earth's surface under slow contraction.

The enormous area of the sea has a great effect upon climate, but not so much in the direct way formerly believed. While a mass of warm or cold water off a coast must to some extent modify temperature, a greater direct cause is the winds, which, however, are in many parts the effect of the distribution of warm and cold water in the ocean perhaps thousands of miles away. Take the United Kingdom, notoriously warm and damp for its position in latitude. This is due mainly to the prevalence of westerly winds. These winds, again, are part of cyclonic systems principally engendered off the coasts of Eastern North America and Newfoundland, where hot and cold sea-currents, impinging on one another, give rise to great variations of temperature and movements of the atmosphere which start cyclonic systems travelling eastwards.

The centre of the majority of these systems passes north of Great Britain. Hence the warm and damp parts of them strike the country with westerly winds, which have also pushed the warm water left by the dying-out current of the Gulf Stream off Newfoundland across the Atlantic, and raises the temperature of the sea off Britain.

When the cyclonic systems pass south of England, as they occasionally do, cold north-east and north winds are the result, chilling the country despite the warm water surrounding the islands.

It only requires a rearrangement of the direction of the main Atlantic currents wholly to change the climate of Western Europe. Such an arrangement would be effected by the submergence of the Isthmus of Panama and adjacent country, allowing the Equatorial Current to pass into the Pacific. The gale factory of the Western Atlantic would then be greatly reduced.

The area south of the Cape of Good Hope is another birthplace of great cyclonic systems, the warm Agulhas Current meeting colder water moving up from the Polar regions; but in the Southern Ocean the conditions of the distribution of land are different, and these systems sweep round and round the world, only catching and affecting the south part of Tasmania, New Zealand, and Patagonia.

In 1894 I spoke of the movements of the lower strata of water in the sea as a subject on which we were only beginning to get a little light. Since that year we have learnt a little more. It is a common idea that at the bottom of the sea all is still; but this is a mistake, even for the deepest parts, for the tidal influence reaches to the bottom and keeps every particle in motion, though such motion is quiet and slow.

Near the shore, however, though still in deep water, the movement may be considerably increased. Cases have occurred in late years where submarine cables have broken several hundred fathoms deep, and when picked up for repair it has been found that the iron wire covering has been literally rubbed away as by a file. This can only be the result of an undercurrent along the bottom moving the cable to and fro. Such a current might be caused by a submarine spring, for there is no doubt that much fresh water finds its way into the ocean in this fashion, but it is more probably generally an effect of acceleration of the tidal movement due to the rising slope of the continent.

In connection with this, further facts have come to light in the course of recent marine surveys.

Many isolated shoal spots in the great oceans have figured in our charts, the results of reports by passing sailors who have said they have seen breakers in fine weather.

Such places are the terror of seamen, and it is part of the duty of surveying ships to verify or disprove them. Very much has been done in the last eighteen years, with the result that the majority of them have, as dangers, disappeared. In many cases, however, a bank has been found, deep in the ordinary acceptation of the word, but must less deep than the surrounding sea—solitary ridges, in fact, rising from the ocean floor. Frequently, in examining these banks in search of shoaler spots, breakers have been reported and recognised as such on board the surveying ship from a distance, but on approach they have proved to be small overcurls caused by tide ripplings, and the depth of water has proved to be several hundred fathoms. These ripplings are clearly caused by the small tidal motion in the deep water, generally in these cases of more than 2000 fathoms, meeting the slope of the submerged mountain range, being concentrated and accelerated until the water finally flows up the top of the slope as a definite current, and taking the line of least resistance, that to the surface, makes itself visible in the shape which we are accustomed to associate with comparatively shallow water.

These cases form remarkable instances of the manner in which extensive motion of water may arise from very small beginnings.

An observation I was anxious to make in 1894 has been successfully carried out since. This was to ascertain whether there was any permanent undercurrent in the Straits of Bab-el-Mandeb due to more water being forced through the strait on the surface by the persistent S.E. wind of winter than could be evaporated in the closed Red Sea.

Such return undercurrents have in somewhat similar circumstances been shown to exist in the Dardanelles, Strait of Gibraltar, and in the Suez Canal.

The observation at Bab-el-Mandeb was difficult. The wind is strong and the disturbance of the sea is considerable, while the water is 120 fathoms or 700 feet deep. But a surveying vessel maintained herself at anchor there during four days, and, by the aid of an ingenious apparatus sent from England for the purpose, clearly proved the existence of a current of 1½ knot flowing steadily at depths below 70 fathoms out of the Red Sea, whilst in the upper strata there was a similar current flowing in. In such ways is interchange of water provided for by Nature in places where tidal action does not suffice.

In what I fear is a very discursive Address I have not mentioned the interior of Africa. In the first place, it is a subject of itself; and as we shall have, I hope, many papers on African subjects I have thought it better to deal mainly with generalities.

Still I cannot refrain from a few words to express the astonishment I always feel when I hear people complain that Africa goes slow. When I look at what has been effected in my own lifetime, it appears to me that, on the contrary, it has been rushed. The maps I learnt from as a boy showed the whole interior as a blank. There are now no parts that are not more or less known. The great lakes have all been revealed; the great rivers have all been traced; Europeans are now firmly fixed with decent governments in parts formerly a prey to tribal wars and the atrocities of the inland slave traffic. Railways are running over regions unknown forty years ago, and one of the most astonishing things to me is that I should be able to hope now to visit in comfort and luxury the great Victoria Falls which my old friend Sir John Kirk—whom I left the other day hale and hearty—was, with the exception of Livingstone, the first white man to see, after a long and laborious journey in his company in 1860.

I could not help being amused as well as interested at seeing a short time ago a proclamation by the Government of Northern Rhodesia, dated not far from Lake Bangweolo, calling on all concerned to observe neutrality during the present war between Russia and Japan. I think that if anyone had prophesied to Livingstone, as he lay in 1873 lonely and dying by the shores of that newly discovered lake, that such an edict would be issued in thirty years he would have expressed a doubt as to its fulfilment.

To Southern Africa Nature has denied two of the features that facilitate rapid progress—good harbours and sufficient rainfall—but the energy of man has done wonders to provide the former where possible, and will doubtless do more; whilst I believe that the lack of the latter will also be overcome in the same way. The coordinated—or, in other words, the scientific—observations made in many other countries have pointed out a possible solution. On the other hand, the height of the inland plateaux makes it possible for the white man to live and work in latitudes which would under other conditions be tropical.

South Africa must have a great future before it; and while some present circumstances may delay development of its natural advantages, I am inclined to think that in the long run prosperity may be more solid and material for being reached in the face of difficulties, as has so often occurred in the history of the world.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 21.—M. Bouquet de la Grye in the chair.—On the laws of sliding friction: Paul **Painlevé.** A discussion of a problem suggested by M. de Sparre in a recent paper, and of the conditions necessary for a solution without ambiguity.—The cause of the presence of abnormal quantities of starch in bruised apples: G. **Warcollier.** It is shown that tannin from galls prevents all action of amylase on starch, and it is supposed that the accumulation of starch in bruised apples is due to a similar action.

CALCUTTA.

Asiatic Society of Bengal, August 2.—Additions to the collection of Oriental snakes in the Indian Museum, part iii.: N. **Annandale.** Four new species and a new genus are described, two of the former coming from the Malay Archipelago, one from N.E. India, and one from Gilgit. Notes on other species from different parts of the Oriental region are given. This paper completes the series for the present, the collection now being worked out and arranged.—Sal-ammoniac: a study in primitive chemistry: H. E. **Stapleton.** An attempt to carry back the history of sal-ammoniac through Mohammedan times, and to throw light on the primitive conceptions of nature which led to its introduction as an alchemical drug. Although little used by the Greek school of Alexandria, it was in high repute as one of the alchemical "stones" of the Arabs, and through their agency the substance passed into European alchemy. Authorities are given for the belief that the salt owed its reputation partly to its magical qualities, which were due to its connection with human hair and other animal substances, and partly to its strictly chemical qualities. A suggestion is finally made that the salt was originally introduced into Western Asia through Persia from China.—Alchemical equipment in the eleventh century, A.D.: H. E. **Stapleton** and R. F. **Azo.** This paper is an annotated analysis of an Arabic treatise on alchemy lately discovered in the library of His Highness the Nawab of Rampur. The treatise was written in Baghdad in the year 426 A.H. (1034 A.D.), and though now in a somewhat mutilated state, it affords a welcome addition to our knowledge of alchemical methods and equipment in the eleventh century. Special attention is directed to (1) the great importance attached to weights in chemical operations 700 years before the time of Black and Lavoisier; and (2) the drawings and description of the *Vthāl* (Aludel), which furnish, for the first time from Arabic sources, a clear conception of this instrument.

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