The Borderland of Astronomy and Geology.[1]

By Prof. A. S. EDDINGTON, F.R.S.

THE region in which geology and astronomy most conspicuously overlap is in the theories of the origin of our planet. We have, in fact, two main theories—one due originally to an astronomer, Laplace, and the other to a geologist, Chamberlin.

In the last century the evolution of a star seems often to have been regarded as something quite detached from the evolution of the stellar universe. Just as the birth and death of a man is an incident which can occur at any time in the rise and decline of the human race, so it was thought that the birth and extinction of a particular star formed merely a detached incident in the course of progress of the stellar universe —if, indeed, the universe was progressing in any particular direction. Thus it was a natural belief that the stars died out and were re-formed by collisions of extinct stars; and that the matter which now forms the sun had undergone many alternations of incandescence and extinction since things first began. But this view is quite at variance with the general tendency of sidereal astronomy in the present century. We have come to recognise that the stellar system is one great organisation, and that the stars which are shining now are more or less coeval with one another. Everyone would admit that Mars and Jupiter were formed as parts of one process of evolution—not necessarily at the same moment, but each formed as the process reached the appropriate stage; and similarly we now believe that it was one process of evolution sweeping across the primordial matter which caused it to form itself into stars; and these original stars are the actual stars which we see shining now. No doubt the evolution did not develop at the same rate in all parts of the universe, and there are probably places where stars are still being formed; but you will see that this view is entirely different from the other view that stars were being formed individually by haphazard collisions of dark stars, so that each was an independent formation, having no time-connexion with other stars.

This view has been forced on us partly by direct evidence of organisation among the stars, pointing to a common origin for large groups of stars. We notice scattered groups such as the Hyades, which have almost exactly equal and parallel motions. Clearly it would be impossible to form such a group if each star were the product of an accidental collision. The only way in which a common motion like this can arise is by associated development from some nebula or other diffuse distribution of matter. The connexion is clearly a connexion of common origin. Again, practically all the bright stars of Orion form a similar group, having common motion; and, moreover, they have all reached a similar stage of evolution. They are connected with the great Orion nebula, the faint extensions of which fill up nearly the whole constellation. It is obvious that here we have to deal with a single evolutionary development. But another point which militates against a collision theory is the extreme rarity of collisions and close approaches. The distances separating the stars are enormous compared with their own dimensions. Sir Frank Dyson once used the illustration of twenty tennis-balls, distributed at random throughout the whole interior of the earth, to give a model of the density of distribution of the stars. It has sometimes been objected that we do not know how many extinct stars may be wandering about and colliding. Dyson's twenty tennis-balls represent only the *luminous* stars; there may, for all we know, be millions of *dark* bodies ready to be fired into incandescence by collision. I think, however, that there is now good evidence, based on the dynamics of stellar motions, that the dark stars cannot greatly outnumber the luminous stars—probably not ten times and certainly not a hundred times. (If they were more numerous than that, the average velocities of stars would, owing to the gravitational attraction, be much higher than is observed.) That argument, then, is no longer valid. Taking a very liberal view of the kind of approach that can be held to constitute a collision, it is estimated that a star would only suffer collision once in 10^{14} years.

Thus the astronomer is not predisposed to look favourably on a hypothesis of the origin of the solar system which postulates anything of the nature of a collision. He has the conception of an orderly development of the stars crystallising out of the primordial material, and, unless perhaps in exceptional cases, following an undisturbed course of development. We hope for a theory that will show us the star after its first isolation from surrounding material spontaneously developing the system of planets.

It now appears almost certain that, whether the original matter was gaseous or whether it was composed of meteors, it must at an early stage in the star's history have been completely volatilised into gas. This was while the star was extremely diffuse, and, for example, before the planets separated from it. This means that the material now forming a planet has at one time passed through the furnace, and has cooled down from a gaseous stage. How far that has a direct bearing on geology I cannot say, since I have nothing to guide me as to the course of its subsequent chequered history. I do not say that the earth was a gaseous body when it first became recognisable as an independent planet, but I am convinced that its material was at one time merged in a completely gaseous sun.

It may be of interest to indicate why it seems so probable that a star in its early diffuse state is gaseous and not meteoric. The stars are known to be of closely similar mass. There are occasional exceptions, but probably 90 per cent. of them are between one-half and five times the sun's mass. We have no explanation of this uniformity if they are initially merely aggregations of solid meteors; but we have a very exact explanation if they are gaseous. In fact this critical mass round which the actual masses of the stars cluster so closely is predicted by the theory of equilibrium of spheres of gas, using only well-known physical constants determined in the laboratory. The crucial factor is radiation-pressure, which is inappreciable in smaller masses, and almost suddenly takes control between one-half and five times the sun's mass.

[1] A lecture delivered before the Geological Society of London on November 21.

There can be little doubt that large radiation-pressure, tending to overcome gravity, conduces to instability, so that larger masses have small chance of survival. Somewhere about one-half the sun's mass the radiation-pressure no longer counts seriously, so that there is no tendency for the primitive material to break into smaller units.

The existence of radioactive minerals on the earth seems to supply another reason for believing that its material was originally subjected to high temperature or to physical conditions of a different order from those now prevailing. In radioactivity we see a mechanism running down which must at some time have been wound up. Without entering into any details, it would seem clear that the winding-up process must have occurred under physical conditions vastly different from those in which we now observe only a running-down. The only possible guess seems to be that the winding-up is part of the general brewing of material which occurs under the intense heat in the interior of the stars.

The trend of this argument has been against the Chamberlin-Moulton hypothesis and in favour of some form of nebular origin of the solar system. It is, of course, accepted that the details of the original nebular hypothesis of Laplace require modification. Also the word nebula is meant to signify diffuse gaseous material in general, and has no immediate connexion with those objects which we see in the sky, and call nebulæ more particularly. There is still controversy as to what process of evolution is represented by the spiral nebulæ which are seen in such numbers—what they will ultimately turn into; but the controversy is whether the spiral nebula will give rise to a cluster of a few hundred stars, or whether it will turn into a stellar universe on the same scale as the great system of some thousands of millions of stars which forms our galactic system. There is now no suggestion that it has anything to do with the formation of so insignificant a system as the solar system. But in preferring the nebular hypothesis to that of Chamberlin and Moulton, it is necessary to make a certain reservation. We have hitherto taken it for granted that the formation of a system of planets is a normal feature of the evolution of a star. Most of my arguments have referred to the development of stars in general, and would become irrelevant if it could be admitted that the solar system were an exceptional formation violating ordinary expectation.

We know that at least a third of the stars are double stars, and I do not think there is any reason to think that planetary systems would be formed when the evolution takes that course; but until recently it was taken for granted that the remaining single stars would generally (or at least frequently) be the rulers of systems of planets. Jeans has recently pitched a bombshell into the camp, suggesting that the solar system is a freak system—the result of a rare accident, which could only happen to one star out of a very large number. He found no way of accounting for it as a normal process. I have not the specialist knowledge necessary to criticise the details of the working of the nebular or of the planetismal theory of development, but before regarding Jeans's argument as conclusive (he himself makes reservations) I should be more satisfied if the effect of radiation-pressure had been taken into account. It is fairly clear that radiation-pressure plays a great part in the separation of nebulous matter into stars, and although I have no definite reason to think that it can account for the separation of planets from the sun, I do not feel satisfied that we have got at the whole truth until that point has been duly examined.

Supposing, however, that we are forced to accept Jeans's suggestion that the solar system is a freak system, some of my objections to the Chamberlin-Moulton hypothesis are removed. I cannot admit that the conditions of collision which that hypothesis requires are normal features in the formation of stars; but they might have happened occasionally in the history of the universe, and produced the solar system, the sun being thus as an exceptional star born out of due time. But if my arguments against Chamberlin's hypothesis fall to the ground, there are probably other astronomers prepared to attack it in other directions.

The new views as to the age of the earth are now pretty well known to geologists. I may sum them up briefly in the statement that Lord Kelvin's estimate of the extent of geological time need not now be taken any more seriously than Archbishop Ussher's, and that the geologist may claim anything up to 10,000 million years without provoking a murmur from astronomers. Although there may still be some difficulties about the exact source from which the vast heat-energy the stars pour out into space is derived, it is now clear that the Helmholtz contraction-theory is inadequate to give the necessary supply. The astronomer has no such precise means of measuring geological time as the physicist has now discovered by the analysis of radioactive minerals; but he can add his contributory evidence that the sun, and presumably therefore the earth, is much older than Lord Kelvin allowed. In the Cepheid variable stars it seems possible to measure the actual rate at which evolution is proceeding—the rate at which the star is condensing from a diffused state to a denser state. The star is believed to be pulsating, and as it expands and contracts the light varies in quantity and character. In a pulsating gravitating mass the period is proportional to the inverse square root of the density, so that by observing the rate at which the period is changing we can deduce the rate at which the density is changing. I may add that the law that the period depends on the inverse square root of the density is very closely confirmed by comparing the values for the various Cepheids. In this way we find that for the best observed of these stars, δ Cephei, the density is changing 500 times slower than the contraction hypothesis assumes. It would, of course, be risky to assume that the same proportion holds at all stages of the evolution of a star; but it suggests that Lord Kelvin's estimate of 20 million years for the age of the sun might well be multiplied by 500 to give 10,000 million years. At any rate, the Cepheid observations show that the stars must have some other source of energy besides contraction.

I suppose it must be a matter of interest to geologists whether the intensity of the sun's heat has been constant or whether it was at one time hotter than now. I think we can say fairly definitely that the sun was formerly much hotter. There must have been a time when the sun's heat was from 20 to 50 times more intense than it is now. That would no doubt have made a great difference to many geological processes. Unfortunately, I cannot say whether it occurred in known geological epochs. It must have occurred after the earth had begun to exist as a separate planet; but whether it was before or after the sequence of geological strata began to be laid down I have no idea. It would not be unreasonable, however, to expect that in the early geological times the sun was several times hotter than it is at present.

After the evolution of the solar system, we naturally turn to consider the evolution of the earth-moon system. My impression is that nothing in recent progress suggests any doubt that the beautiful theory of Sir George Darwin is substantially correct. The main features are that the moon at one time formed part of the earth, and broke away. At that time the rotation period of the earth was between 3 and 4 hours, and the cause of the fracture was that the solar tidal force synchronised with a free period of natural vibration of the earth; owing to resonance the tidal deformation of the earth continually increased until rupture occurred. The earth's period of rotation has since lengthened to 24 hours, owing to frictional dissipation of energy by lunar and solar tides; and the back-reaction of the lunar tides on the moon has caused the moon to recede to its present considerable distance. All this has well stood the test of searching criticism, and must be considered as extremely probable. Modern research has added two contributions; it enables us to calculate the magnitude of this tidal friction at the present time, and it enables us to locate more exactly the region where the frictional dissipation is occurring.

I believe it was Darwin's view that the tides most potent in wasting energy were not water-tides but tides in the solid earth; that is to say, we have to do with deformations of the whole earth under the tide-raising force of the moon's attraction. Undoubtedly these deformations of the earth occur, but everything turns on whether the process of deformation is attended with serious friction. H. Jeffreys has pointed out that the phenomenon of latitude variation is accompanied by similar deformations of the earth; and in this case it is clear that the friction is inconsiderable, for otherwise the deviations of the pole from the symmetrical position would be damped out almost at once. It seems, therefore, very unlikely that the solid tides can have had much effect in the process of tidal evolution of the earth-moon system. Ocean tides are likewise of small effect as Darwin himself had seen. The modern conclusion is a very curious one; it is in the land-locked shallow seas that nearly all the mischief occurs. This was discovered by G. I. Taylor, who found that the Irish Sea alone is responsible for $\frac{1}{50}$ of the whole amount required by observation. The remaining land-locked basins on the earth are probably capable of making up the necessary total.

The actual rate at which the earth's rotation is being slowed down at the present era can probably be deduced with fair accuracy from the records of ancient eclipses. The day is lengthening about one-thousandth of a second per century or 1 minute in 6,000,000 years. At this rate we should have to go back more than 10,000 million years to the time when the day was between 3 and 4 hours and the moon was born. Since the rate depends on the accidental circumstance of occurrence of shallow seas no definite prediction can be made; but allowing for the much greater effect of the tides when the moon was nearer to us, it is difficult to date the birth later than 1000 million years ago.

Had the earth a solid crust at the time the cataclysm happened? I cannot tell at all. But if it suits geological theories I can see no objection whatever to the hypothesis that the earth had a solid crust at the time. No cohesion of the crust would seriously resist the enormous forces involved when the resonant vibration got started. It would not be appreciably more difficult than the disruption of a molten earth. The view that the Pacific Ocean is the hollow left at the place where the moon broke off seems tenable unless geologists find objection to it; and in that case we may suppose that the water now collected in the hollow formerly covered the earth—or most of it. This change of condition of the earth may (or may not) have happened within geological times. When the earth was covered with water there would be no land-locked seas and no appreciable tidal friction from the sun (the moon being not yet born), so that we can allow a long previous history during which the length of day was nearly constant at 3 or 4 hours. That rather helps to make the whole theory self-consistent.

These speculations stand very much as they did when Darwin put forward his theory. But I am tempted to add further speculations arising out of the location of the frictional dissipation. (I am taking advantage of the great opportunity for speculation which this address affords. Ordinarily I am restrained, because people would ask, What facts can you produce in support of your speculations? But here I am asking the question, Have you any facts which seem to support them? If not, by all means let them drop.) The frictional dissipation acts as a brake on the earth's rotation, and we now feel confident that the brake is a surface-brake applied at certain points on the earth's surface where the favourable conditions exist. The retarding force is transmitted to the earth's interior, and so delays the rotation as a whole; but unless the material is entirely non-plastic there will be a tendency for the outer layers to slip on the inner layers. I do not know how much the material a few hundred miles below the surface would be expected to give under the strain; it may be inappreciable, but I will assume that though small it has some effect.

We have then the whole crust slipping from east to west over the main part of the interior. Probably it would go very stickily, sometimes arrested by a jamming which would hinder it for a time and then going on more easily. That is helpful in explaining certain astronomical observations. There are irregularities in the motions of heavenly bodies, noticed particularly in the swift-moving moon but shown also on a smaller scale in the sun and planets, which appear to indicate that our standard timekeeper, the earth, is a little irregular. Now, of course, it is the rotation of the *surface* of the earth which determines our standard time. I find it difficult to believe that there can be irregular variations in the angular velocity of the earth as a whole; but it seems less difficult if the variations are merely superficial, due to the crust sliding non-uniformly on the interior. I have even entertained the wild idea that the motion of the magnetic poles might be due to this cause; the magnetism being constant in the interior but with the axis emerging at changing points of the crust as the crust slips over the inner magnet. Unfortunately, so little seems to be known about the motion of the magnetic poles that I have not even been able to make out whether the motion is from west to east as this theory definitely requires.

What interests the geologist more nearly is that the brake is applied only at certain areas on the surface, so that there would be a tendency to crumple the crust more particularly to the west of these areas. It is unfortunate that shallow seas are necessarily the least permanent features of the earth; otherwise I would have asked whether the geologists had evidence of special crumpling in such areas.

I have regarded the crust as fairly mobile from east to west. I suppose the geologists would also like it mobile from north to south in order to have glacial periods in those portions which are now near the equator. It is not possible to hold out much encouragement for such an idea, because we cannot imagine any force acting from north to south. Still if the crust, which is being urged by the east-west force of tidal friction, is resisted by obstacles it may be deflected, finding that say a south-west track offers less resistance. In a long enough time almost any displacement may have happened, granting my hypothesis that the connexion of the crust to the interior is reasonably plastic. So I cannot forbid this possible interpretation of glacial periods in the earlier geological times.

I am sure that it will not be supposed that, in presenting the astronomical side of these questions which belong both to geology and astronomy, I have any intention of laying down the law. The time has gone by when the physicist prescribed dictatorially what theories the geologist might be permitted to consider. You have your own clues to follow out to elucidate these problems, and your clues may be better than ours for leading towards the truth. We both recognise that we are adventuring in regions of extreme uncertainty where future discoveries will probably lead to various modifications of ideas. Where, as in the new views of the age of the earth, physics, biology, geology, astronomy, all seem to be leading in the same direction, and producing evidence for a greatly extended time-scale, we may feel more confidence that a permanent advance is being made. Where our clues seem to be opposed, it is not for one of us to dictate to the other, but to accept with thankfulness the warning from a neighbouring science that all may not be so certain and straightforward as our own one-sided view seemed to indicate.

Nature and Reproduction of Speech Sounds.[1]

By Sir RICHARD PAGET, Bart.

ALL the characteristics of English speech—the vowels and diphthongs and consonant sounds—can be produced—as breathed or whispered speech—without using the larynx at all; so that in the use of the English language (at least) it may be said that the larynx is not an essential organ of speech. The function of the larynx is to give carrying power and inflexion to speech, and melody to song—it has nothing to do with the essential characteristics of speech.

If any one with a normal "ear for music" will whisper the words "Noah's rather at sea"—thinking of the sounds rather than of the sense—they will hear

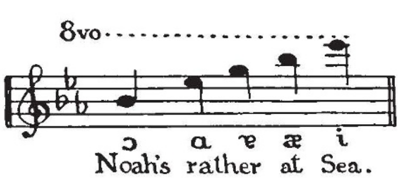

FIG. 1.

an ascending arpeggio something like the phrase shown in Fig. 1. The exact notes heard in each case will depend on *how* the individual person pronounces the vowel sounds in question.

These whispered or breathed notes are formed, as is well known, by the resonance of the cavity of the mouth, and they are varied for each different vowel sound by altering the size of the cavity and the opening of the mouth, mainly through the operation of the tongue and lips. With many of the vowel sounds, namely, **i** (eat), **ei** (hay), **e** (men), **æ** (hat), **o** (not), and in some types of **ɑ** (calm), two simultaneous resonant notes have been heard by many investigators, but the remaining principal vowel sounds, **ɔ** (all), **ou** (no), and **u** (who), have been generally supposed to be characterised by a single resonance.

Some observations made by me at the beginning of this year, using my own breathed vowel sounds, indicated that in *every* case the mouth—or rather the oral cavity as a whole, from the larynx to the lips—actually gives *two* simultaneous resonances for *each* vowel sound. It appeared that these pairs of resonant notes are not fixed in pitch for any one vowel sound, but might vary over three or four semitones—and sometimes even more—without a very appreciable change in the character of the vowel.

The resonances heard in the use of my own voice are set out in the accompanying chart, in which the vertical scale represents semitones of the equal temperament scale, and the vowel sounds are represented in the notation of the International Phonetic Association (Fig. 2).

It will be seen that **i** (eat), **I** (it), **ei** (hay), **e** (men), **æ** (hat), **ɐ** (earth), **ə** (sofa), **ʌ** (up), and **ɑ** (calm) form very nearly a converging series—the upper resonances falling by steps of 1 to 3 semitones, while the lower resonances are more active and take larger jumps—

[1] Substance of a lecture delivered at University College, University of London, on October 18.