every 200 metres of height of the mountains. The heat thus gained is liberated in the condensation of the vapour. I believe this is satisfactory as regards the *Foehn*.

But this will not account for a rise of the temperature of Southern Greenland from its mean December temperature, which, according to Dove's map, is below freezing, to 14° C. A rise of 14° C. would require, according to the above law, a mountain chain 2,800 metres, or about 8,000 feet in height, and there is none in Greenland approaching this.

I used to think that great rises of temperature in the Arctic winter were due to the wind tearing up the frozen surface of the sea, and liberating the heat of the water below; but this will not account for an increase of temperature above freezing. I have no explanation to offer. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, August 13

Does Sunshine Extinguish Fire?

IT is a popular belief that a fire will not burn if exposed to the sun, and, from all I have observed, it seems well founded. Can any of your readers favour me with an explanation of the phenomenon, if true; or is it a mere superstition?

Schwarzwald, August 11 CHARLES WATSON

OUR ASTRONOMICAL COLUMN

THE OPPOSITION OF MARS, 1877.—The present opposition of the planet Mars offers conditions so nearly analogous to those of the opposition in 1862, that the many fine drawings made in that year, a number of which are contained in vol. xxxii. of the "Memoirs" of the Royal Astronomical Society, become available for comparison with such as may be made during the actual opposition. The same hemisphere of the planet is presented to the earth, and our depression from the martial equator is sensibly the same; thus, at opposition in 1862 the angle of position of the visible (southern) pole was 145°·3 and the earth's depression −22°·7, while at the opposition of 1877 the figures are respectively 160°·3 and −22°·5. The least distance of Mars from the earth in 1862 was 0·406, while in 1877 it is 0·377.

Notwithstanding Secchi observing in 1858 found the features upon the disc of Mars irreconcilable with those delineated in Mädler's drawings made under similar circumstances in 1830, it was sufficiently evident at the opposition of 1862 that these differences are to be attributed to the temporary conditions occasioned by clouds of varying density, form, and extent, in the atmosphere of the planet itself, heightened perhaps in some degree by the state of our own atmosphere at the times of the observations. A striking instance in support of this conclusion was afforded by Mr. Lockyer's observations on September 25, 1862. At 10h. 44m., when his drawing No. 14 was completed, the well-known spot *a* of Mädler was *quite invisible*, while when No. 15 was made shortly afterwards, this spot was "among the most prominent features upon the planet's disc."

There would appear now to be little doubt that the green and red portions of the disc do really represent seas and continents, and are not due to the effect of contrast, another explanation which has been suggested. During the actual favourable appearance of the planet, we may expect that measures will be made which will admit of a closer determination of the position of the axis of rotation than any yet obtained. The results at present upon record are (1) Sir W. Herschel's, which assigns for the longitude of the ascending node of the equator of Mars upon his orbit, 79° 27′ for 1872, and for the obliquity of his ecliptic 28° 42′. The reduction by Oudemann's of Herschel's measures, make these figures 79° 18′ and 20° 53′ respectively; (2) Schroeter's, as given by M. Terby, which places the south pole in 172° 54′·7, with latitude 60° 33′·2, whence we find for 1798, longitude of ascending node of equator on orbit, 84° 54′, obliquity of ecliptic 27° 57′ for 1798; and (3) Oudemann's reduction of Bessel's measures with the Königsberg heliometer, made September 28, 1830, January 21, 1835, and February 11, 1837, giving for the place of the ascending node 80° 50′, and for the obliquity 27° 17′ for 1834. With the last values which have been generally adopted, we have for the ascending node of the equator of Mars upon the earth's equator (N), and its inclination thereto (I):—

$$N = 47^{\circ}\,42' + 0'{\cdot}50\,(t - 1850)$$
$$I = 39\;52 - 0{\cdot}25\,(t - 1850).$$

The following table showing the angle of position of the visible pole of Mars, and the elevation of the earth above the plane of his equator, at the oppositions between 1850 and 1880, has been calculated from the above elements, and may be of interest to some readers; the least distance of Mars from the earth is added:—

Date of opposition.	Position of visible pole.	Elevation of earth.	Least distance of Mars.
	° ′	° ′	
1852, Jan. 24	23 16	9 8 N.	0·660
1854, Feb. 26	343 46	22 6 N.	0·675
1856, April 2	35 2	23 30 N.	0·625
1858, May 15	40 45	12 4 N.	0·514
1860, July 17	192 34	10 47 S.	0·391
1862, Oct. 5	145 17	22 42 S.	0·406
1864, Nov. 30	142 37	6 29 S.	0·534
1867, Jan. 10	343 54	10 24 N.	0·636
1869, Feb. 13	7 37	21 40 N.	0·677
1871, March 19	28 35	24 53 N.	0·636
1873, Feb. 13	41 2	17 59 N.	0·563
1875, June 19	209 7	0 51 S.	0·433
1877, Sept. 5	160 14	22 31 S.	0·377
1879, Nov. 12	138 48	13 54 S.	0·482

A glance at this table exhibits a well-known condition that when Mars is nearest to the earth and when we have consequently the best opportunities of studying the features upon his disc, his southern hemisphere is always directed to the earth, and hence we are likely to be better acquainted with that hemisphere than with the northern one, which is turned towards the earth only at the greater distances of Mars.

THE SATELLITES OF SATURN.—A series of observations of all the eight satellites of Saturn by Prof. Asaph Hall, dated from Washington in December, 1876, has at last made its appearance in No. 2,145 of the *Astronomische Nachrichten*.

SATELLITES OF MARS.—A telegram from the Smithsonian Institution to M. Leverrier, received August 19, notifies the extraordinary discovery of two satellites of Mars by Prof. Asaph Hall, of the U.S. Naval Observatory at Washington. The telegram runs thus: "Two satellites of Mars by Hall at Washington, first elongation west, August 18, eleven hours, Washington distance, eighty seconds, period thirty hours, distance of second, fifty seconds."

THE BRITISH ASSOCIATION

PLYMOUTH, *Tuesday*

THE Plymouth Meeting of the British Association has not realised, as far as numbers are concerned, the success which we anticipated last week, and which was indicated by the business done on the days immediately preceding the opening of the meeting.

The attendance of the regular members from a distance has been very good indeed, and can compare favourably with meetings that have gone before it, but the visit of the British Association, while opening wide the gates of hospitality of the people of Plymouth, does not seem to have awakened the scientific interest of the community sufficiently to cause many to enlist in its ranks. It is seldom that so small a number of local members have been added to the list of members and associates at the annual meeting of the British Association. Yet Plymouth and its surroundings have a well-known scientific reputation, and the falling off in numbers of the present meeting must rather be looked for to the fact that one of the two sister-towns to Plymouth has, as a body, held aloof from the Association, than to any want of scientific interest pervading the great port of the west of England.

At the opening meeting on the evening of Wednesday last, there was a fair attendance, but the noble Guildhall was not filled. It is estimated that about 1,300 persons were present, including the members of the General Committee, who, as usual, were accommodated with seats on the platform. The business commenced by the Mayor of Plymouth introducing to the meeting the President, Prof. Allen Thomson. It was, we believe, intended as a graceful compliment to its host, the town of Plymouth, that the British Association requested the mayor to take the place of the retiring president, Prof. Andrews, who is absent through illness. The meeting went off very well and the presidential address was, notwithstanding its difficult and abstruse nature, listened to with profound attention, and commanded the respect which must be paid to anything coming from so high an authority upon embryology as Prof. Allen Thomson.

On the next morning the sectional addresses were delivered, some of which appeared in our last issue and others we give in the present number. In Section A the address by Prof. Carey Foster, F.R.S., was warmly received and elicited great applause when he spoke of the radiometer and of the great value of the researches of Mr. Crookes, notwithstanding the recent utterances of an influential mover in scientific circles which might have a tendency to depreciate the value of those researches. Prof. Haughton, of Dublin, read two papers, the first upon a method of calculating the absolute duration of geological periods, and the second, a reply to Prof. Newcomb upon the co-efficient of acceleration of the moon's mean motion as illustrated by the original account of the solar eclipse of Agathocles. Upon both papers an interesting discussion took place.

At the same section Prof. Osborne Reynolds read a paper (which we publish to-day) upon the rate of progression of groups of waves and the rate at which energy is transmitted by waves. The paper was illustrated by a very beautiful model in which the progression of wave-groups was made visible to the audience by means of a long series of light pendulums connected elastically with one another.

Friday was a great day in Section A as far as attendance was concerned. It had been announced the day before that Mr. Preece would read a paper on the telephone and show it at work, and that Sir William Thomson would make a communication to the section on the possibility of life on a meteoric stone falling on the earth. In anticipation of these two papers the room of Section A was crowded from an early hour, and although there were several long mathematical papers on the list, the non-mathematical visitors waited in the most patient manner for the papers they had come to hear. It so happened, however, that the papers that appeared so dry to those waiting for something else were of the very highest interest and value to mathematicians and astronomers; especially that by Prof. J. C. Adams on his discovery of original papers by Newton which proved that that great philosopher had solved some of the most important lunar problems, the solution of which has been till now attributed to a much later date, and proving most conclusively that Newton had never fallen into the error which for years had been attributed to him.

Sir Wm. Thomson's paper on the possibility of a meteorite becoming the vehicle of animal life to this earth from another planet, or heavenly body, was evidently listened to with much enjoyment. Sir William sees no difficulty in the assumption that animals or germs might without injury be conveyed to this earth by meteorites if protected in the crevices of the meteoric mass, and much amusement was caused by his saying that though the outside shell of a meteoric stone might be incandescent from the friction caused by its flight through the terrestrial atmosphere, yet within a crevice of that stone might be concealed a Colorado beetle, which, falling on the earth, might become the father of a large and prosperous family. The amusement caused by this quaint idea was increased to roars of laughter when Prof. Haughton, with his well-known wit, ridiculed the idea of the transmission of living animals by meteorites, and said that if Sir William Thomson had spoken of a Colorado beetle arriving by a meteoric stone becoming the mother of a large number of baby Colorado beetles he might have felt some sort of alarm, but he didn't care how many papa beetles came, so long as they left the mamma Colorado beetles at home.

Mr. Preece followed with his paper on the telephone. The room was crowded to excess and the paper was of the highest possible interest, and not only illustrated by diagrams and the instruments themselves, but the latter were connected by wires with the post-office at Plymouth, about a quarter of a mile off and with that at Exeter some fifty miles away. By means of Bell's articulating telephone the human voice was distinctly conveyed and conversations were carried on between the two stations. Owing to induction from the parallel wires between Plymouth and Exeter, there was a confused roar as from hail pattering on a window pane, and no words could be heard, but in his lecture in the Guildhall on the following evening, the traffic was stopped for ten minutes, and a conversation was carried on by the human voice between Plymouth and Exeter. The Guildhall on the occasion of Mr. Preece's lecture, was crammed almost to suffocation, and the discourse lasted two hours and a half; the lecture was upon telegraphy, but the telephone was undoubtedly the chief attraction.

The instrument was again described in Section G this afternoon by the inventor, Dr. Graham Bell, who arrived from Liverpool yesterday; he was received with enthusiastic applause, and a most interesting series of experiments were shown in illustration of his paper.

At the General Committee Meeting yesterday, a letter from the city of York was read by the secretary, inviting the British Association to celebrate their jubilee or fiftieth anniversary in that city in consideration of the fact that the first meeting of the Association was held at York in 1831. The invitation was unanimously accepted.

After some quiet discussion the resolution that the Association should visit Nottingham in 1879, and Swansea in 1880, was carried unanimously; whereupon Prof. Haughton rose, and, amidst great laughter, expressed his regret at the proceedings terminating so peacefully, for, as an Irishman, he never liked to see a good fight stopped. Mr. Spottiswoode is to be president at the Dublin meeting.

Some wholesome resolutions have been approved of by the Council on the regulations as to the admission of papers to be read in the various sections. With regard to the discontinuance of Section F the Council ask the General Committee to report more fully on the reasons which have induced them to recommend this step.

PLYMOUTH, *Wednesday*

[*By Telegraph*].

The following grants were passed at the meeting of the General Committee held at the Royal Hotel to-day. The names of the members who would be entitled to call on the general treasurer for the respective grants are prefixed:—

Mathematics, and Physics.	£
Cayley, Prof.—Continuation of Borckhardt's Tables ...	100
Foster, Prof. Carey.—Observation of Atmospheric Electricity at Madeira	15

	£
Glaisher, Mr. J.—Luminous Meteors	10
Joule, Dr.—Determination of the Mechanical Equivalent of Heat (renewed)	65
Thomson, Sir W.—Measurement of the Lunar Disturbance of Gravity (renewed)	50

Chemistry.

Brown, Prof. Crum.—Quantitative Estimation of Atmospheric Ozone	10
Roberts, Mr. Chandler.—Chemical Composition and Structure of some of the less-known Alkaloids	25

Geology.

Evans, Mr. J.—Kent's Cavern Exploration	50
Evans, Mr. J.—Record of the Progress of Geology	100
Godwin Austen, Mr.—Kentish Boring Exploration	100
Harkness, Prof.—North-West Highlands Fossils	10
Haughton, Rev. Dr.—Fermanagh Caves Exploration	30
Herschel, Prof. A.—Thermal Conductivity of Rocks	10
Hull, Prof.—Circulation of Underground Waters	15
Lubbock, Sir J., Bart.—Victoria Cave, Settle, Exploration	100

Biology.

Dew Smith, Mr.—Table at the Zoological Station, Naples	75
Fox, Col. Lane.—Exploration of Ancient Earthworks	25
McKendrick, Dr.—Investigation of Pulse Phenomena by Thomson's Siphon Recorder	10
Rolleston, Prof.—Examination of two Caves and Tumuli near Tenby	25
Stainton, Mr.—Record of Zoological Literature	100
Thomson, Dr. Allen.—Transmission of Electrical Impulses through Nerve Structure	30

Statistics and Economic Science.

Farr, Dr.—Anthropometric Committee (renewed)	66

Mechanics.

Froude, Mr. W.—Instruments for Measuring the Speed of Ships (renewed)	50
Thomson, Sir W.—Datum Level of the Ordnance Survey	10
	£1,081

SECTION A.—MATHEMATICAL AND PHYSICAL.

On the Rate of Progression of Groups of Waves and the Rate at which Energy is Transmitted by Waves, by Prof. Osborne Reynolds, F.R.S.—When several waves forming a discontinuous group travel over the surface of deep water, the rate of progression of the group is always much less than the rate at which the individual waves which compose the group are propagated. As the waves approach the front of the group they gradually dwindle down and die out, while fresh waves are continually arising in the rear of the others. This, which is a well-known phenomenon, presents itself to our notice in various ways.

When a stone is thrown on to the surface of a pond, the series of rings which it causes gradually expands so as finally to embrace the entire surface of the water; but if careful notice be taken it is seen that the waves travel outwards at a considerably greater rate than that at which the disturbance spreads.

Or, when viewing a rough sea, if we endeavour to follow with the eye any wave which is larger than its neighbours, we find, after following it in its course for a short distance, that it has lost its extra size, while on looking back we see that this has been acquired by the succeeding wave.

But perhaps the most striking manifestation of the phenomenon is in the waves which spring from the bows of a rapid boat, and attend it on its course. A wave from either bow extends backwards in a slanting direction for some distance and then disappears; but immediately behind it has come into existence another wave parallel to the first, beyond which it extends for some distance when it also dies out, but not before it is followed by a third which extends still farther, and so on, each wave overlapping the others rather more than its predecessor. Although not obvious, very little consideration serves to show that the stepped form of these columns of waves is a result of the continual dying out of the waves in front of the group, and the formation of fresh waves behind. For as each wave cuts slantwise through the column formed by the group, one end is on the advancing side or front of the group, and this is continually dying while the other is in the rear and is always growing.

So far as I am aware, no general explanation of these phenomena has as yet been given It has been shown, and I believe first by Prof. Stokes, that if two series of parallel waves of equal magnitude, but differing slightly in length, move simultaneously in the same direction over the same water so as to form a series of groups of waves separated by bands of interference, that these groups will advance with half the velocity of the individual waves. This is doubtless an example of the same phenomenon, and shows that the theory of wave motion is capable of explaining the phenomena; but it appears to leave something to be desired,—for instance, why should the bands of interference only progress with half the velocity of propagation in a deep sea, whereas in sound the corresponding bands of interference which constitute the beats move at the same velocity as the waves.

My object in this paper is to point out a fact in connection with wave transmission which appears to have hitherto passed unnoticed at all events in connection with the phenomena described above, of which it affords a clear and complete explanation. One of the several functions performed by waves progressing through a medium is the transmission of energy. Thus the energy which we receive from the sun is brought to us in the waves of light and heat; so in the case of sound the work done by the arm of the drummer is transmitted to our ears by the waves of sound. It is possible however to have waves which travel through a medium without conveying energy; such are the waves caused by the wind on a field of corn. This kind of wave may be well understood by suspending a series of small balls by threads, so that the balls all hang in a row, and the threads are all of the same length. If we then run the finger along, so as to set the balls oscillating in succession, the motion will be such as to give the idea of a series of waves propagated from one end to the other; but in reality there is no propagation, each pendulum swings independently of its neighbours, there is no communication of energy, the waves being merely the result of the general arrangement of the motion.

In this case there is no communication of energy, neither is there any propagation of disturbance. Any one ball may be set swinging without in the least disturbing the others; and what is indicated here is a general law that wherever a disturbance is transmitted through a medium by waves there must always be communication of energy. The rate at which energy is transmitted in different media, or by different systems of waves, is very different. This may be illustrated at once by experiment. If the balls just described are all connected by an elastic thread, then they can no longer swing independently. If one be set in motion then, by virtue of the connecting thread, it will communicate its motion to its neighbours until they swing with it, so that now waves would be propagated through the balls. The rate at which a ball would impart its motion, *i.e.* its energy, to its neighbours would clearly depend on the tension of the connecting thread. If this was very slight compared with the weight of the balls it would stretch, and the ball might accomplish several swings before it had set its neighbours in full motion, so that of the initial energy of disturbance a very small portion is communicated at each swing. But if the tension of the thread be great compared with the weight of the balls, one ball cannot be disturbed without causing a similar disturbance in its neighbours, and then the whole energy will be communicated. This is simply illustrated by laying a rope or chain on the ground, and fastening down one end; if then the loose end be shaken up and down the wriggle caused will travel to the other end, leaving the rope perfectly straight and quiet on the ground behind it, so that in this case it is at once seen that the wave carries forward with it the whole energy of disturbance.

The straight cord and the pendulous balls represent media in which the waves are at the opposite limits—in one case none of the energy of disturbance is transmitted, and in the other case the whole is transmitted. Between these two limits we may have waves of infinite variety, in which any degree of energy from all to nothing is transmitted. Now the waves of sound belong to the class of the cord in which all the energy is transmitted; but what I want particularly to make clear is that the waves on water are between the limits they are analogous to the waves in the balls suspended when connected by an elastic string And I have so to show that according to the accepted theory of wave motion the waves on deep water only carry forward half the energy of disturbance.

In regular trochoidal waves the particles move in vertical circles with a constant velocity and are always subject to the same pressure. Of the energy of disturbance half goes to give motion to the particles and half to raise them from their initial position to the mean height which they occupy during the passage of the wave.

Now the mean horizontal positions of the particles remain unaltered by the waves, hence, since their velocities are constant, none of their energy of motion is transmitted; nor since the pressure on each particle is constant can any energy be transmitted by pressure. The only energy therefore which remains to be transmitted is the energy due to elevation, and that this is transmitted is obvious since the particles are moving forward when above their mean position, and backward when below it. This energy constitutes half the energy of disturbance, and this is therefore the amount transmitted.

For a definite mathematical proof that—

In waves on deep water the rate at which the energy is carried forward is ½ the energy of disturbance per unit of length × by the rate of propagation.

Let h_0 be the initial height occupied by a particle supposed to be of unit weight, h_1 the height of the centre of the circle in which it moves as the wave passes, r the radius of the orbit, and θ the angle the radius vector makes with the horizontal diameter, then the height of the particle above its initial position is $h_1 - h_0 + r \sin \theta$, adding to this the height due to its velocity we have the whole energy of disturbance—

$$= 2(h_1 - h_0) + r \sin \theta.$$

The velocity of the particle is—

$$= \sqrt{2g(h_1 - h_0)},$$

and the horizontal component of this is—

$$= \sqrt{2g(h_1 - h_0)} . \sin \theta.$$

Therefore the rate at which energy is being transmitted by the particle—

$$= \left\{2(h_1 - h_0) + r \sin \theta\right\} \sqrt{2g(h_1 - h_0)} . \sin \theta.$$

and the mean of this—

$$= \frac{1}{2\pi} \int_0^{2\pi} \left\{2(h_1 - h_0) + r \sin \theta\right\} \sqrt{2g(h_1 - h_0)} \sin \theta \, d\theta$$

$$= \tfrac{1}{2} r \sqrt{2g(h_1 - h_0)},$$

and if λ be the length of the wave and $n\lambda$ the rate of propagation—

$$h_1 - h_0 = \frac{\pi r^2}{\lambda} \text{ and } \frac{2g}{\lambda} = 4\pi r^2,$$

∴ the mean rate at which energy is transmitted by this particle

$$= n\lambda(h_1 - h_0),$$

or the rate of propagation multiplied by half the energy of disturbance. Q.E.D.

It now remains to come back to the speed of the groups of waves and to show that *if the rate at which energy is transmitted is equal to the rate of propagation multiplied by half the energy of disturbance, then the velocity of a group of waves will be ½ that of the individual waves.*

Let P_1, P_2, P_3, P_4 be points similarly situated in a series of waves which gradually diminish in size and energy of disturbance from P_3 to P_1, in which direction they are moving. Let E be the energy of disturbance between P_1 and P_2 at time t, $E + a$ the energy between P_2 and P_3, $E + 2a$ between P_3 and P_4, and so on.

Then at the time $t + n$ after the wave has moved through one wave-length it follows that the energy between P_1 and P_2 will be—

$$= \frac{E + E + a}{2} = E + \frac{a}{2},$$

and between P_2 and P_3 will

$$= \frac{E + a + E + 2a}{2} = E + \frac{3a}{2},$$

and again after another interval, n, the energies between P_1 and P_2, P_2 and P_3 will be respectively—

$$= \frac{E + \frac{a}{2} + E + \frac{3a}{2}}{2} = E + a,$$

$$= \frac{E + \frac{3a}{2} + E + \frac{5a}{2}}{2} = E + 2a.$$

So that after the waves have advanced through two wave-lengths the distribution of the energy will have advanced one, or the speed of the groups is ½ that of the waves. Q.E.D.

Of course this reasoning applies equally to the waves on the suspended balls, when connected by an elastic string, as to water; and in this case the conclusions may be verified for, as on water, the groups of waves travel at a slower rate than the waves. This experiment tends to throw light on the manner in which the result is brought about. When a ball is disturbed, the disturbance is partly communicated to the adjacent ball by the connecting string, and part retained in the form of pendulous oscillation; that part which is propagated forward is constantly reduced in imparting oscillations to the successive balls and soon dies out, while the motion retained by the swinging pendulum constantly gives rise to succeeding waves until it is all absorbed. If the tightness of the cord be adjusted to the length of the suspending threads, waves may be made to travel along in a manner closely resembling the way in which they travel on water, the speed of the group being ½ the speed of the individual waves.

Although the progression of a group has hitherto been spoken of as if the form of the group was unaltered, this is by no means the case as a rule.

In the mathematical investigation it was assumed that the motion of the particles is circular; this, however, cannot be the case when the succeeding waves differ in size by a sensible quantity, and hence in this case the form of the group cannot be permanent. And it may be further shown that as a small group proceeds, the number of waves which compose it will continually increase, until the gradation becomes indefinitely small; and this is exactly what is observed, whether on water or on the strings.

So far as we have considered deep water, when the water is shallow compared with the length of the waves, the results are modified, but in this case the results as observed are strictly in accordance with the theory.

According to this, as waves enter shallow water the motion of the particles becomes elliptical, the eccentricity depending on the shallowness of the water; and it may be shown that under these circumstances the rate at which energy is transmitted is increased, until when the elliptic paths approach to straight lines the whole energy is transmitted, and consequently it follows that the rates of the speed of the groups to the speed of the waves will increase as the water becomes shallower, until they are sensibly the same. In which case only the groups of waves are permanent, and Mr. Scott Russell's solitary wave is possible. Besides the explanation thus given of these various phenomena, it appears that we have here a means of making some important verifications of the assumptions on which the wave theory is based; for the relative speed of the groups and the waves which compose them affords a criterion as to whether or not the particles move in circles.

SECTION D.—BIOLOGY.

Department of Anthropology.

ADDRESS BY FRANCIS GALTON, F.R.S.

PERMIT me to say a few words of personal explanation to account for the form of the address I am about to offer. It has been the custom of my predecessors to give an account of recent proceedings in anthropology, and to touch on many branches of that wide subject. But I am at this moment unprepared to follow their example with the completeness I should desire and you have a right to expect, owing to the suddenness with which I have been called upon to occupy this chair. I had indeed the honour of being nominated to the post last spring, but circumstances arising which made it highly probable that I should be prevented from attending this meeting, I was compelled to ask to be superseded. New arrangements were then made by the Council, and I thought no more about the matter. However, at the last moment, the accomplished ethnologist who otherwise would have presided over you was himself debarred by illness from attending, and the original plan had to be reverted to.

Under these circumstances I thought it best to depart somewhat from the usual form of addresses, and to confine myself to certain topics with which I happen to have been recently engaged, even at the risk of incurring the charge of submitting to you a memoir rather than an address.

I propose to speak of the study of those groups of men who are sufficiently similar in their mental characters or in their physiognomy, or in both, to admit of classification; and I especially desire to show that many methods exist of pursuing the inquiry in a strictly scientific manner, although it has hitherto been too often conducted with extreme laxity.

The types of character of which I speak are such as those described by Theophrastus, La Bruyère, and others, or such as may be read of in ordinary literature and are universally recognised as being exceedingly true to nature. There are no worthier professors of this branch of anthropology than the writers of the higher works of fiction, who are ever on the watch to discriminate varieties of character, and who have the art of describing them. It would, I think, be a valuable service to anthropology if some person well versed in literature were to compile a volume of extracts from novels and plays that should illustrate the prevalent types of human character and temperament. What, however, I especially wish to point out is, that it has of late years become possible to pursue an inquiry into certain fundamental qualities of the mind by the aid of exact measurements. Most of you are aware of the recent progress of what has been termed psycho-physics, or the science of subjecting mental processes to physical measurements and to physical laws. I do not now propose to speak of the laws that have been deduced, such as that which is known by the name of Fechner, and its numerous offshoots, including the law of fatigue, but I will briefly allude to a few instances of measurement of mental processes, merely to recall them to your memory. They will show what I desire to lay stress upon, that the very foundations of the differences between the mental qualities of man and man admit of being gauged by a scale of inches and a clock.

Take, for example, the rate at which a sensation or a volition travels along the nerves, which has been the subject of numerous beautiful experiments. We now know that it is far from instantaneous, having indeed no higher velocity than that of a railway express train. This slowness of pace, speaking relatively to the requirements that the nerves have to fulfil, is quite sufficient to account for the fact that very small animals are quicker than very large ones in evading rapid blows, and for the other fact that the eye and the ear are situated in almost all animals in the head, in order that as little time as possible should be lost on the road, in transmitting their impressions to the brain. Now the velocity of the complete process of to and fro nerve transmission in persons of different temperaments has not been yet ascertained with the desired precision. Such difference as there may be is obviously a fundamental characteristic and one that well deserves careful examination. I may take this opportunity of suggesting a simple inquiry that would throw much light on the degree in which its velocity varies in different persons, and how far it is correlated with temperament and external physical characteristics. Before I describe the inquiry I suggest, and towards which I have already collected a few data, it is necessary that I should explain the meaning of a term in common use among astronomers, namely, "personal equation." It is a well known fact that different observers make different estimates of the exact moment of the occurrence of any event. There is a common astronomical observation, in which the moment has to be recorded at which a star that is travelling athwart the field of view of a fixed telescope, crosses the fine vertical wire by which that field of view is intersected. In making this observation it is found that some observers are over sanguine and anticipate the event, while others are sluggish and allow the event to pass by before they succeed in noting it. This is by no means the effect of inexperience or maladroitness, but it is a persistent characteristic of each individual, however practised in the art of making observations or however attentive he may be. The difference between the time of a man's noting the event and that of its actual occurrence is called his personal equation. It remains curiously constant in every case for successive years, it is carefully ascertained for every assistant in every observatory, it is published along with his observations, and is applied to them just as a correction would be applied to measurements made by a foot-rule, that was known to be too long or too short by some definite amount. Therefore the magnitude of a man's personal equation indicates a very fundamental peculiarity of his constitution; and the inquiry I would suggest, is to make a comparison of the age, height, weight, colour of hair and eyes, and temperament (so far as it may admit of definition) in each observer in the various observatories at home and abroad, with the amount of his personal equation. We should thus learn how far the more obvious physical characteristics may be correlated with certain mental ones, and we should perhaps obtain a more precise scale of temperaments than we have at present.

Another subject of exact measurement is the time occupied in forming an elementary judgment. If a simple signal be suddenly shown, and if the observer presses a stop as quickly as he can when he sees it, some little time will certainly be lost, owing to delay in nerve transmission and to the sluggishness of the mechanical apparatus. In making experiments on the rate of judgment, the amount of this interval is first ascertained. Then the observer prepares himself for the exhibition of a signal that may be either black or white, but he is left ignorant which of the two it will be. He is to press a stop with his right hand in the first event, and another stop with his left hand in the second one. The trial is then made, and a much longer interval is found to have elapsed between the exhibition of the alternative signal, and the record of it, than had elapsed when a simple signal was used. There has been hesitation and delay: in short, the simplest act of judgment is found to consume a definite time. It is obvious that here, again, we have means of ascertaining differences in the rapidity of forming elementary judgments and of classifying individuals accordingly.

It would be easy to pursue the subject of the measurement of mental qualities to considerable length, by describing other kinds of experiment, for they are numerous and varied. Among these is the plan of Prof. Jevons, of suddenly exhibiting an unknown number of beans in a box, and requiring an estimate of their number to be immediately called out. A comparison of the estimate with the fact, in a large number of trials, brought out a very interesting scale of the accuracy of such estimates, which would of course vary in different individuals, and might be used as a means of classification. I can imagine few greater services to anthropology than the collection of the various experiments that have been imagined to reduce the faculties of the mind to exact measurement. They have engaged the attention of the highest philosophers, but have never, so far as I am aware, been brought compendiously together, and have certainly not been introduced, as they deserve, to general notice.

Wherever we are able to perceive differences by inter-comparison, we may reasonably hope that we may at some future time succeed in submitting those differences to measurement. The history of science is the history of such triumphs. I will ask your attention to a very notable instance of this, namely, that of the establishment of the scale of the thermometer. You are aware that the possibility of making a standard thermometric scale wholly depends upon that of determining two fixed points of temperature, the interval between them being graduated into a scale of equal parts. These points are, I need hardly say, the temperatures of freezing and of boiling water respectively. On this basis we are able to record temperature with minute accuracy, and the power of doing so has been one of the most important aids to physics and chemistry as well as to other branches of investigation. We have been so accustomed, from our childhood, to hear of degrees of temperature, and our scientific knowledge is so largely based upon exact thermometric measurement, that we cannot easily realise the state of science when the thermometer, as we now use it, was unknown. Yet such was the condition of affairs so recently as two hundred years ago, or thereabouts. The invention of the thermometer, in its present complete form, was largely due to Boyle, and I find in his "Memoirs" (London, 1772, vol. vi. p. 403) a letter that cannot fail to interest us, since it well expresses the need of exact measurement that was then felt in a particular case, where it was soon eminently well supplied, and therefore encourages hope that our present needs as anthropologists may hereafter, in some way or other, be equally well satisfied. The letter is from Dr. John Beale, a great friend and correspondent of Boyle, and is dated February, 1663. He says in it:—

"I see by several of my own thermometers that the glassmen are by you so well instructed to make the stems in equal proportions, that if we could name some degrees, ... we might by the proportions of the glass make our discourses intelligible in mentioning what degrees of cold our greatest frosts do produce. . . . . If we can discourse of heat and cold in their several degrees, so as we may signify the same intelligibly, ... it is more than our forefathers have taught us to do hitherto."

The principal experiments by which the mental faculties may be measured require, unfortunately for us, rather costly and delicate apparatus, and until physiological laboratories are more numerous than at present, we can hardly expect that they will be pursued by many persons.

Let us now suppose that, by one or more of the methods I have described or alluded to, we have succeeded in obtaining a group of persons resembling one another in some mental quality, and that we desire to determine the external physical characteristics and features most commonly associated with it. I have nothing new to say as regards the usual anthropometric measurements, but I wish to speak of the great convenience of photographs in conveying those subtle but clearly visible peculiarities of outline which almost elude measurement. It is strange that no use is made of photography to obtain careful studies of the head and features. No single view can possibly exhibit the whole of a solid, but we require for that purpose views to be taken from three points at right angles to one another. Just as the architect requires to know the elevation, side view, and plan of a house, so the anthropologist ought to have the full face, profile, and view of the head from above of the individual whose features he is studying.

It might be a great convenience, when numerous portraits have to be rapidly and inexpensively taken for the purpose of anthropological studies, to arrange a solid framework supporting three mirrors, that shall afford the views of which I have been speaking, by reflection, at the same moment that the direct picture of the sitter is taken. He would present a three-quarter face to the camera for the direct picture, one adjacent mirror would reflect his profile towards it, another on the opposite side would reflect his full face, and a third sloping over him would reflect the head as seen from above. All the reflected images would lie at the same optical distance from the camera, and would, therefore, be on the same scale, but they would be on a somewhat smaller scale than the picture taken directly. The result would be an ordinary photographic picture of the sitter surrounded by three different views of his head. Scales of inches attached to the framework would appear in the picture and give the means of exact measurement.

Having obtained drawings or photographs of several persons alike in most respects, but differing in minor details, what sure method is there of extracting the typical characteristics from them? I may mention a plan which had occurred both to Mr. Herbert Spencer and myself, the principle of which is to superimpose optically the various drawings and to accept the aggregate result. Mr. Spencer suggested to me in conversation that the drawings reduced to the same scale might be traced on separate pieces of transparent paper and secured one upon another, and then held between the eye and the light. I have attempted this with some success. My own idea was to throw faint images of the several portraits, in succession, upon the same sensitised photographic plate. I may add that it is perfectly easy to superimpose optically two portraits by means of a stereoscope, and that a person who is used to handle instruments will find a common double eye-glass fitted with stereoscopic lenses to be almost as effectual and far handier than the boxes sold in shops.

In illustration of what I have said about photographic portraits, I will allude to some recent experiences of my own in a subject that I have still under consideration. In previous publications I have treated of men who have been the glory of mankind, I would now call your attention to those who are its disgrace. The particular group of men I have in view are the criminals of England, who have been condemned to long terms of penal servitude for various heinous offences.

It is needless to enlarge on the obvious fact that many persons have become convicts who, if they had been afforded the average chances of doing well, would have lived up to a fair standard of virtue. Neither need I enlarge on the other equally obvious fact, that a very large number of men escape criminal punishment, who in reality deserve it quite as much as an average convict. Making every allowance for these two elements of uncertainty, no reasonable man can entertain a doubt that the convict class includes a large proportion of consummate scoundrels, and that we are entitled to expect to find in any large body of convicts a prevalence of the truly criminal charactistics, whatever these may be.

Criminality, though not very various in its development, is extremely complex in its origin: nevertheless, certain general conclusions are arrived at by the best writers on the subject, among whom I would certainly rank Prosper Despine. The ideal criminal has three peculiarities of character; his conscience is almost deficient, his instincts are vicious, and his power of self-control is very weak. As a consequence of all this, he usually detests continuous labour. This statement applies to the criminal classes generally, the special conditions that determine the description of crime being the character of the instincts; and the fact of the absence of self-control being due to ungovernable temper, or to passion, or to mere imbecility.

The deficiency of conscience in criminals, as shown by the absence of genuine remorse for their guilt, appears to astonish all who first become familiar with the details of prison life. Scenes of heartrending despair are hardly ever witnessed among prisoners; their sleep is broken by no uneasy dreams—on the contrary, it is easy and sound; they have also excellent appetites. But hypocrisy is a very common vice; and all my information agrees in one particular, as to the utter untruthfulness of criminals, however plausible their statements may appear to be.

The subject of vicious instincts is a very large one; we must guard ourselves against looking upon them as perversions, inasmuch as they may be strictly in accordance with the healthy nature of the man, and, being transmissible by inheritance, may become the normal characteristics of a healthy race, just as the sheep-dog, the retriever, the pointer, and the bull-dog have their several instincts. There can be no greater popular error than the supposition that natural instinct is a perfectly trustworthy guide, for there are striking contradictions to such an opinion in individuals of every description of animal. All that we are entitled to say is, that the prevalent instincts of each race are trustworthy, not those of every individual. A man who is counted as an atrocious criminal by society, and is punished as such by the law, may nevertheless have acted in strict accordance with his instincts. The ideal criminal is deficient in qualities that oppose his vicious instincts; he has neither the natural regard for others which lies at the base of conscience, nor has he sufficient self-control to enable him to consider his own selfish interests in the long run. He cannot be preserved from criminal misadventure, either by altruistic or by intelligently egoistic sentiments.

It becomes an interesting question to know how far these peculiarities may be correlated with physical characteristics and features. Through the cordial and ready assistance of Sir Edmund Du Cane, the Surveyor-General of Prisons, who has himself contributed a valuable memoir to the Social Science Congress on the subject, I was enabled to examine the many thousand photographs of criminals that are preserved for purposes of identification at the Home Office, to visit prisons and confer with the authorities, and lastly to procure for my own private statistical inquiries a large number of copies of photographs of heinous criminals. I may as well say, that I begged that the photographs should be furnished me without any names attached to them, but simply classified in three groups according to the nature of the crime. The first group included murder, manslaughter, and burglary; the second group included felony and forgery; and the third group referred to sexual crimes. The photographs were of criminals who had been sentenced to long terms of penal servitude.

By familiarising myself with the collection, and continually sorting the photographs in tentative ways, certain natural classes began to appear, some of which are exceedingly well marked. It was also very evident that the three groups of criminals contributed in very different proportions to the different physiognomic classes.

This is not the place to go further into details: indeed my inquiry is far from complete. I merely quote my experiences in order to show the way in which questions of character, physiognomy, and temperament admit of being scientifically approached, and to give an instance of the helpfulness of photography. If I had had the profiles and the shape of the head as seen from above, my results would have been much more instructive. Thus, to take a single instance, I have seen many pencil studies in outline of selected criminal faces drawn by Dr. Clarke, the accomplished and zealous medical officer of Pentonville Prison; and in these sketches a certain very characteristic profile seemed to me conspicuously prevalent. I should have been very glad of photographs to corroborate this. So, again, if I had had photographic views of the head taken from above, I could have tested, among other matters, the truth of Prof. Benedict's assertion about the abnormally small size of the back of the head in criminals.

I have thus far spoken of the characters and physiognomy of well-marked varieties of men: the anthropologist has next to consider the life history of those varieties, and especially their tendency to perpetuate themselves, whether to displace other varieties and to spread, or else to die out. In illustration of this, I will proceed with what appears to be the history of the criminal class. Its perpetuation by heredity is a question that deserves more careful investigation than it has received, but it is on many accounts more difficult to grapple with than it may at first sight appear to be. The vagrant habits of the criminal classes, their illegitimate unions and extreme untruthfulness, are among the difficulties. It is, however, easy to show that the criminal nature tends to be inherited while, on the other hand, it is impossible that women who spend a large portion of the best years of their lives in prison can contribute many children to the population. The true state of the case appears to be that the criminal population receives steady accessions from classes who, without having strongly marked criminal natures, do nevertheless belong to a type of humanity that is exceedingly ill-suited to play a respectable part in our modern civilisation, though they are well-suited to flourish under half-savage conditions, being naturally both healthy and prolific. These persons are apt to go to the bad; their daughters consort with criminals and become the parents of criminals. An extraordinary example of this is given by the history of the infamous Jukes family in America, whose pedigree has been made out with extraordinary care, during no less than seven generations, and is the subject of an elaborate memoir printed in the thirty-first annual report of the Prison Association of New York, 1876. It includes no less than 540 individuals of Jukes blood, among whom the number of persons who degraded into criminality, pauperism, or disease, is frightful to contemplate.

It is difficult to summarise the results in a few plain figures, but I will state those respecting the fifth generation, through the eldest of the five prolific daughters of the man who is the common ancestor of the race. The total number of these was 103, of whom thirty-eight came through an illegitimate granddaughter, and eighty-five through legitimate grandchildren. Out of the thirty-eight, sixteen have been in gaol, six of them for heinous offences, one of these having been commited no less than nine times; eleven others were paupers or led openly disreputable lives; four were notoriously intemperate; the history of three had not been traced, and only four were known to have done well. The great majority of the women consorted with criminals. As to the 85 legitimate descendants, they were less flagrantly bad, for only five of them had been in gaol and only thirteen others had been paupers. Now the ancestor of all this mischief, who was born about the year 1730, is described as having been a hunter and a fisher, a jolly companionable man, averse to steady labour, working hard and idling by turns, and who had numerous illegitimate children, whose issue has not been traced. He was, in fact, a somewhat good specimen of a half-savage, without any seriously criminal instincts. The girls were apparently attractive, marrying early and sometimes not badly; but the gipsy-like character of the race was unsuited to success in a civilised country. So the descendants went to the bad, and the hereditary moral weaknesses they may have had rose to the surface and worked their mischief without a check. Cohabiting with criminals and being extremely prolific, the result was the production of a stock exceeding 500 in number, of a prevalent criminal type. Through disease and intemperance the breed is now rapidly diminishing; the infant mortality has of late been horrible among them, but fortunately the women of the present generation bear usually but few children, and many of them are altogether childless.

This is not the place to go further into details. I have alluded to the Jukes family in order to show what extremely important topics lie open to inquiry in a single branch of anthropological research and to stimulate others to follow it out. There can be no more interesting subject to us than the quality of the stock of our countrymen and of the human race generally, and there can be no more worthy inquiry than that which leads to an explanation of the conditions under which it deteriorates or improves.

SECTION G.—MECHANICAL SCIENCE.

THE following is an abstract of the address of the president, Mr. E. Woods, C.E:—The president selected the question of railway brakes as his topic. He said that the provision of adequate brake power to control trains was a subject which had latterly much engaged the attention of railway companies and of the Government. In the summer of 1874 a Royal Commission was appointed to inquire into the causes of accidents on railways, and the possibility of removing them by further legislation. One branch of the inquiry naturally led to the consideration of accidents caused by collision; and it appeared from the evidence taken before the Commissioners that trains were generally provided with insufficient controlling power, and that the distance within which, when running at high speed, they could be stopped by the brake ordinarily in use had not been ascertained with any approach to accuracy. It was under these circumstances that the Commissioners applied to the railway companies to institute a definite series of experiments to test the value of hand-brakes, and the effect of various systems of continuous brakes. In conjunction with Col. Inglis, R.E., he was intrusted by the Commissioners with the supervision of the experiments, to the satisfactory conduct of which the railway companies contributed in the most liberal manner. With few exceptions, and up to a comparatively recent period, the companies had remained content with the brake appliances which were common forty years ago. These, no doubt, were sufficient to control the trains in those early days, few as they were in number, and limited in weight and speed. The brakes were applied separately, and by hand-power, always to the tender, and usually to some few of the carriages and to the guard's van or vans, if such accompanied the train. As long ago as 1858 the Board of Trade called the special attention of the railway companies to the fact that the amount of brake power then habitually applied was insufficient to prevent frequent accidents occurring from collisions, many of which they considered might be averted. Particular reference was made to two systems which had come into daily use on the East Lancashire and the Lancashire and Yorkshire railways, namely, the brakes of Newall and of Fay, by means of which trains of ninety to 100 tons weight, running fifty miles an hour, could be effectually controlled by driver or guard, even when proceeding down steep inclines, and brought up within a moderate distance. It was certainly matter for surprise, seeing the advantage of continuous brakes, that the railway companies should have so long tolerated the old system, and been so slow to adopt a method which, instead of being dependent for its due action on the attention of several persons, was effectually placed under the control of one. This lethargy prevailed, too, throughout a period when increased speed had come to be demanded, when augmenting traffic required heavier trains, and when, consequently, more ponderous and powerful engines had to be used—circumstances which ought to have induced the companies to effect simultaneously a readjustment of their brake appliances. After the year 1850 many attempts were made to supersede the ordinary type of brake, some of the brakes introduced being self-acting and put into operation by the momentum of the train, while others acted as sledges or shoes. None, however, proved successful. The continuous breaks of Newall and Fay simply involved a wider distribution of power over the different vehicles of the train, and gave the means of applying that power by one, or, at most, two attendants. It was in that direction that the ingenuity of inventors had recently been turned, and there were now several systems of continuous brakes in successful working on the leading railways, each claiming some special advantages over its rivals, whether as more simple in construction, less expensive in application, or effecting more complete control of the train. The Royal Commissioners desired that attention should be primarily directed to the following points:—1. The distances within which trains running at various speeds could be stopped by the system of brakes in ordinary use on the different lines of the United Kingdom; (2) what results could be obtained by the additional application of brake power; and (3) how far a very large amount of brake power could be suddenly resorted to with safety in heavy trains running at high speeds. For the purpose of the experiments a portion of the Nottingham and Lincoln branch of the Midland Railway was selected as offering a piece of line comparatively level and free from any sharp curves. Six companies furnished eight complete trains, which represented as many systems of continuous brakes comprehended in four classes, namely, (1) Clarke's and Webb's and Fay's brakes, applied by ordinary mechanical gear; (2) Smith's and Westinghouse's vacuum brakes, actuated by atmospheric pressure produced by exhaustion of air; (3) Westinghouse's and Steel McInnes's air brakes; and (4) Clarke's and Barker's hydraulic brakes. The experiments extended over a week, and comprised several series. It was demonstrated that the friction of a complete train, in which the weight of the engine and tender constituted, say one-fourth of the gross weight, inclusive of the atmospheric resistance it encountered in its course, was 42-100ths per cent., or about 9½ lbs. per ton. This result confirmed what long experience had led them to anticipate. It was discovered further that, on a level line, a train running at the rate of forty-five miles an hour could be stopped by hand brakes within 1,000 yards, or, if at the rate of sixty miles, within 1,700 yards. The necessity for some greater control over fast passenger trains was thus rendered obvious. Through the want of a larger amount of brake power much time was lost on a journey, when the stoppages were frequent, the drivers being compelled to slacken speed at long distances from the stopping-places. It seemed, indeed, scarcely to admit of question that a system which was deemed necessary in special cases might be advantageously applicable in all cases; that to render the control of a train complete, brakes should be applied to all, or nearly all, the wheels; and that, at least, the driver, if not the guards, should possess the power of promptly bringing the whole into action. The truth of the principle was now very generally admitted by the leading companies, some of whom had already adopted continuous brakes, while others were preparing to do so. Rather startling disparities were disclosed during the experiments. Some of the disparities were attributable to the contrivances being of comparatively recent origin, but others were clearly owing to the principle upon which the action of the brake was founded. As between the air-pressure and the vacuum brakes there was a loss of 6¼ seconds, which in a train running sixty miles an hour was equivalent to 180 yards additional space traversed in the stop. Three of the experiments involved the application of all available power for stopping. Sand was used, and was found to add sensibly to the stopping power. On an average it made an addition of 1·30 per cent. to the retarding force otherwise brought into play. The trials proved in a very striking manner the great advantage of continuous brakes, for even in their least effective form they afforded more than double the stopping power of the usual hand brakes, whilst in their most effective form the power was quadrupled. He was of opinion that no system could be considered satisfactory which did not produce a retarding power of at least 8 to 10 per cent. of the entire weight of the train, in other words, a power by which fast trains could be stopped in from one-third to one-fourth less time than at present. Obviously the stopping distance was primarily influenced by two considerations:—(1) The length of the interval which elapsed between the brake being put into operation and its taking an effective grip on the wheels; and (2) the amount of pressure brought to bear on each wheel, and the constancy or otherwise of the action after the blocks had gripped the wheels. The unpleasant sensation often experienced during quick stoppages was produced by intermittent and fitful action. After the brakes had been made to bite the wheels their hold became relaxed, a slip took place, followed by successive bites and slips, the latter giving rise to sudden accelerations of speed. The action of a perfect brake should exactly resemble that which gravity would cause if an ascending incline of uniform gradient could be suddenly placed in front of the train to prevent its motion. Under such conditions no inconvenience or danger need be apprehended from the stoppage being accomplished within even a shorter distance than any that was effected during the experiments. A valuable addition of power, under the immediate control of the driver, would be afforded by the fitting of brakes to the engine, and he was glad to find that the recommendation of the Royal Commissioners in this respect had met with prompt attention at the hands of the railway companies. The question of the best material for brake blocks had of late received a good deal of consideration, and it would seem that cast-iron, and even steel, was fast superseding wood. It generally happened that wheels did not become skidded until the speed of the train had been materially reduced. It seemed desirable, therefore, that for ordinary stops the brake pressure should be applied so as to act just short of skidding the wheels, the full skidding power being only used in cases of imminent danger. The general adoption of an effective system of continuous brakes on carriages which had to run from one line to another would be productive of much advantage, for then, in breaking-up and re-making a train at any junction station, the carriages would be found ready-fitted with the requisite appliances for working. If allied companies could only agree to adopt the same system, brake improvements would proceed with far greater rapidity than at present, and public convenience would thereby be promoted. The time had arrived not only when each system should be scrutinised and tested in the most complete manner, but when the companies should clearly set before themselves the conditions which a good continuous brake ought to supply. A study of the different methods which came under his (the lecturer's) notice during the experiments pointed to the following considerations as necessary in view of the provision of perfect brake power for heavy fast trains:—1. The brake power should be applied to all the wheels of all the vehicles throughout the train. 2. The power by which the blocks were forced upon the wheels should be adequate for skidding the wheels on the speed becoming moderately reduced. 3. The driver should have the whole of the brake power completely under his command, and be able to apply it at a moment's notice, as he was the first person likely to discover any obstruction ahead, and was primarily responsible for the regard of the danger signals. He could thus stop the train at once, and no time would be lost by his having to signal danger to the guard. 4. The guards should individually possess the like means of applying the continuous brake, so that they might be able to stop the train without reference to the driver, on an emergency which might manifest itself to them but not to him, such, for instance, as a broken axle, or a carriage getting off the line. 5. The power in hand should be so susceptible of modification that the driver should be able to apply a moderate amount only for effecting ordinary stops, while he kept in reserve a proper excess to be used only on emergencies, or on slippery rails. 6. Full brake application should not require more than a very moderate effort on the part of either driver or guard. 7. The pressure should be steady, and distributed as equally as possible over all the wheels, and, with the intervention of some elastic medium, should act upon the wheels in such a way as to prevent too sudden stopping or the snapping of chains, which produced discomfort and inconvenience to the public. 8. The machinery should be of simple construction, not likely soon to get out of order, and admitting of being easily repaired. 9. Indication should be constantly afforded to driver and guards that the brakes were in proper condition to work or otherwise. 10. The power of working the tender brakes and the van brakes by hand might be advantageously retained. 11. The brakes should be self-acting in case of the severance of the train. 12. Automatic action being provided, means should be furnished the brake attendants for modifying that action instantaneously, according to the circumstances in which the train might be placed after an accident. 13. It would be dangerous, and therefore unadvisable, to give to passengers any power over the brakes. Such seemed to be the principal conditions necessary for realising the conception of a perfect brake—a brake which would constitute an invaluable instrument in contingencies of almost daily occurrence at some place or another in the great railway network of the country.

REMARKABLE PLANTS

III.—The Sensitive Plant (*Mimosa pudica*).

In our ordinary popular conception of the difference between the two kingdoms into which the organic world is divided, we are apt to attribute to one a power of spontaneous motion dependent on the possession of a certain internal mental faculty to which we apply the term voluntary power; while a similar property is not considered to be inherent in the members of the other kingdom. The most recent researches throw, to say the least, considerable doubts on the universal applicability of this test to distinguish animals from plants. Now that the Desmidieæ and the Oscillatorieæ are, by universal consent, relegated to the vegetable kingdom, and that many bodies described by Ehrenberg as animals are found to be particular stages in the life-history of certain vegetable organisms, this character seems but to follow in the wake of others which have one by one been abandoned as absolute discriminating tests between the members of the two kingdoms. Among the more commonly-occurring and familiar movements of vegetable tissues, the dependence of which on external mechanical causes is at present but imperfectly understood, are those motions of the leaves and other parts of plants which are comprised under the common designation of Movements of Sensitiveness or Irritability. It has been well shown by Sachs[1] that these movements are of three different kinds, viz.:—

1. Those periodic movements which are produced entirely by internal causes, without the co-operation of any considerable external impulse of any kind. Such movements may be termed *automatic* or *spontaneous*,

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[1] "Text-Book of Botany," English edition, Book III., chap. 5.