twenty-five years for man's cessation of growth is therefore only an average one, in introducing that number as a factor of the curve, I thought that it would be manifestly an error to take examples of exceptionally long lives, when striking an average for length of life. In the curve as published in NATURE (which, although published somewhat before my communication to the *Lancet*, is really a revised curve), I reduced the age of man from eighty to seventy-five years from considerations such as I have just mentioned. Even seventy-five years is somewhat greater than the expectation of life given in Bourne's "Manual," for those who have completed the first half-century of their existence. The age given by Buffon, and quoted by Mr. Bell, is undoubtedly far too long. Similar considerations from some fresh data induced me to reduce the mean lifetime of the domestic mouse from four and a half years to four years, as noticed by Mr. Bell. W. AINSLIE HOLLIS.

Hove.

Barnes' "Plant Life."

In re centrosomes:

Poor misguided Prof. Zacharias! With absolutely no provocation, he now departs from "the almost universal consensus of opinion among good botanists" by saying of centrosomes (*Bot. Zeit.*, 572: 6, 1899):

"However, on an unprejudiced consideration of the literature involved, one may consider it not impossible that, on renewed search, the centrosomes will finally be again discovered where, for the present ('mark, Jew!'), they have been missed."

And Guignard! What a stupid he is to repeat in greater detail the blunder of figuring and describing those "discredited" centrosomes when all *good* botanists (who swear by Strasburger and his young American students) know that there are no such things! And to think of his calling them "Les centres cinétiques chez les végétaux" (see *Annales des Sci. Naturelles, Bot.*, viii. 5, 177–220, 1898), as though they were common! How "amazingly behind the times"!

But there must be more reason than assigned for designating "Plant Life" as "amazingly behind the times." Prof. Barnes would really be under obligations to the reviewer if he could find time to indicate by number of page or paragraph (doubtless marked as the book was yawned over) the statements to which he considers this phrase applicable. This request is made in all sincerity, and in the hope that the number of these passages will not be so great as to make it presumptuous in its demands upon the reviewer's time. C. R. BARNES.

THE reviewer cannot help regretting the evident pain which his remarks (vol. lviii. p. 519) have caused Prof. Barnes, though the latter can scarcely seriously believe that his arguments and assertions meet the original objections to which he has taken exception.

Prof. Barnes appears to be particularly aggrieved at the reference made to the figures and account of centrosomes, but his own explanations merely serve to give force to the reviewer's contention that they ought not to have found a place in an elementary book at all.

If the best final reply he can make is to quote the opinion of Zacharias to the effect that "it is not impossible that on renewed search the centrosomes will finally again be discovered," he should see that his case is parlous indeed. He has, in fact, cast a far greater slur on his own critical judgment than the reviewer would have ventured to do. His further quotation of Guignard's recent work might perhaps be regarded as somewhat *ex parte*, even had that investigator reiterated the old statements on which Prof. Barnes' account was based. As a matter of fact he does not do so, and his silence tells against our author.

The somewhat contemptuous reference to Strasburger (who is nevertheless *facile princeps* amongst botanical cytologists) and to those younger American botanists whose reputations, *pacè* Prof. Barnes, are largely founded on the splendid results achieved by them at Bonn, are scarcely calculated to increase one's regard for Prof. Barnes' power of discrimination.

Prof. Barnes appears to be quite unable to realise the degree of mental confusion which would be the inevitable lot of a student endeavouring to deal with the account given by him of the movement of water in plants. In one place (§ 204) root-pressure is spoken of as the force which causes the movement from the root to the evaporating surfaces of the leaves; but in § 207 the author rightly remarks that root-pressure is practically inoperative at the time when transpiration is most active. But he goes on to add that "recent experiments" indicate that the negative pressure of the gas-bubbles in the tracheids may be "a very important, or even the chief factor in lifting the water." After this one ceases to be surprised that no mention is made of the conclusions reached by Dixon and Joly, or by Askenasy!

But Prof. Barnes asks for further evidence for the reviewer's unfavourable opinion of the book. Only a few instances need be mentioned here, for if "this request is made in all sincerity," the author's own friends will easily supply more.

In a work of this kind, it is astonishing to find no mention of the occurrence of motile antherozoids amongst the lower phanerogams, which is perhaps the most important of all recent botanical discoveries—important for the student as clearly showing the connection between the higher and lower plants.

The account given in § 143 of annual rings is so preposterous as to call for no further comment.

The respiratory quotient of the ordinary plant is still given as unity, when, as a matter of fact, it is nearly always other than 1 in growing plants.

The statement that "true geotropic curvatures are brought about by the acceleration of the growth of the irritable cells" is, as it stands, absurd, for it involves no necessary *curvature* at all.

A student reading the account given in § 230 would naturally fall into the error of concluding that all the rays of light absorbed by chlorophyll are equally active in promoting assimilation.

In view of the evidence here adduced, at Prof. Barnes' own request, the reviewer considers that his judgment of the book was by no means unduly harsh or severe.

THE REVIEWER.

Optical Experiment.

BEING driven past a row of trees, I noticed that their intermittent shadow on the closed eye-lids gave rise to a vivid chess-board pattern of red and black squares arranged horizontally and vertically. These were perfectly regular, each being equal to about one-twelfth of an inch at ten inches distance. Waving the open fingers in front of the closed eye-lids exposed to the sun gave the pattern fairly well, but better by flashing the sun's rays across the lids by means of a vibrating hand-mirror. I see about seven or eight squares each way, the outer ones not well defined; but a younger man, who was not told what to expect, described them as more numerous.

What structure in the eye gives rise to the phenomenon? It is not caused by the eye-lids, because a piece of tissue-paper can be substituted, the eyes then being open. If the paper is white the squares are white and black. The pattern occupies the centre of the field of each eye. THOM. D. SMEATON.

Adelaide, South Australia, February 6.

A SEISMOLOGICAL OBSERVATORY AND ITS OBJECTS.

TEN years ago seismologists practically confined their attention to the movements of the ground which could be felt. In Italy and Japan, where these were frequent and sometimes violent, they attracted serious attention; whilst in Britain, where earth tremors were comparatively unknown, any suggestion that this country should establish a seismological observatory might only have cast doubts upon the mental balance of its author. At that time it was popularly supposed that in our islands earthquakes were of such rare occurrence that a special establishment for seismological investigations was unnecessary. Seismology, however, like several other sciences, has in a comparatively short period advanced with strides, and now stands as foster-mother not only to a Romulus and Remus, but also to a number of other children all filled with promise.

Now we know that in England, or in any other non-seismic region on the surface of the globe, at least seventy unfelt earthquakes, each of which have durations varying between twenty minutes and several hours, may be recorded yearly. The probability is that these movements are transmitted from their origins as compressional and distortional vibrations through our globe, and the rate of transmission of the former is closely connected with the average depth of the path along which they have travelled. When our observations on these movements are more exact and numerous, we shall then know more than we do at present about certain physical characteristics of the planet on which we live.

At Utrecht, Potsdam and Wilhelmshaven, these unfelt movements frequently correspond in time with well-marked perturbations of magnetic needles, but inasmuch as similar needles are not disturbed at other stations, we are not quite certain that the observed irregularities in magnetograms are altogether the result of mechanical disturbance.

Then, again, we have the curious observation that at certain magnetic observatories prior to great earthquakes originating in their vicinity there have been uneasy movements in magnetic needles. When considering whether these observations are merely accidental coincidences, we must remember that the initial impulse or impulses of these disturbances have been sufficient to cause our world to palpitate from pole to pole, that they have sometimes been accompanied by bodily displacements of material sufficiently large to set the Pacific Ocean in a state of oscillation for many hours, and that the displaced material is in every probability highly charged with magnetite. We do not know the nature of the changes which were taking place in this material before its rupture, but we see in the final movement a possibility of sudden local magnetic disturbance.

Other possible connections between the movements of magnetic needles and those of horizontal pendulums lie in the facts that each have diurnal movements, and each may exhibit continuous or nearly continuous movement. The diurnal movements of horizontal pendulums are closely connected with effects accompanying solar radiation. The late Dr. Reinhold Ehlert, of Strassburg, saw in some of these a world distortion; whilst in others, my own observation leads to the supposition that their explanation is to be found in changes of surface-load brought about by evaporation, condensation, precipitation, and transpiration of moisture. To account for the almost unbroken continuity of earth tremors we are at a loss; but when we remember that in the world there is upon the average an earthquake occurring every half-hour which might be recorded, that probably there are many taking place beneath the sea and deep in our earth which are never felt, it does not seem unreasonable to look for their explanation in the vibrations accompanying these frequent adjustments in operation within the earth. If it is admitted that these continually moving materials are magnetic, not only do they suggest an explanation for the minute sinuosities on magnetograms, but they also indicate a possible relationship between hypogenic geological activity and secular magnetic changes.

In Japan, built up as it is round a core of rocks saturated with magnetite, which in their deeper portions are intensely hot, and therefore probably possess a reduced magnetic susceptibility, these earth tremors are apparently, and as we should expect them to be, more pronounced than they are in England or Europe. As to whether the *frétillements* on the magnetograms from that country are more frequent and distinct than those taken under similar conditions in non-volcanic countries, I leave to be answered by those who have the means of making the necessary comparisons.

That the records from a seismological observatory throw light upon sudden movements of magnetic needles at certain observatories is an established fact; but whether the bond connecting magnetic observations and those obtained by the seismologist is closer than is usually admitted, is apparently a matter worthy of consideration.

From a series of seismograms obtained from different stations we should be in a position to locate the site of sub-oceanic changes, and determine positions to be avoided by the cable engineer. A single seismogram may often set our minds at rest as to the cause leading to cable interruption, a matter of special importance to isolated Colonies, whilst it has repeatedly been the means of extending, confirming, or disproving ordinary telegraphic information. Another class of observations to which the seismologist devotes his attention are those indicating secular, seasonal and irregular changes in the vertical, which are of importance to the astronomer, earth pulsations and a variety of instrumental movements, the cause of which is not yet clear.

Although all the above-mentioned investigations can be carried out at a single observatory, the results to which they lead are by no means of equal value. For example, should we wish to know the velocities with which the vibrations of a given earthquake have been propagated along various paths through our earth, it is evidently necessary to have the means of making comparisons between seismograms obtained from similar instruments at widely separated stations. It is a pleasure to state that the necessary co-operation here indicated has been obtained, and in response to an invitation issued by a Committee of the British Association, the directors of observatories at the following places have kindly undertaken to make the necessary observations: Kew, Paisley, San Fernando (Cadiz), Cairo, Beirut, Cape of Good Hope, Mauritius, Madras, Calcutta, Bombay, Batavia, Tokio, two in New Zealand, Cordova (Argentina), Honolulu, Victoria (B.C.), Toronto, Philadelphia, Arequipa, Mexico, Trinidad. Certain of these are already sending in records.

What is now required in Great Britain is not simply a central office where these records can be examined, but also a station at which a variety of seismological observations can be made which will be comparable with the records from corresponding instruments similarly installed in other localities. As illustrative of this, although it would be extremely interesting to note the varying effects of barometrical pressure upon the plains of Lincolnshire, and to compare the magnitude and period of earthquake waves as recorded there with those recorded on a rocky surface, it is extremely probable that the records of diurnal waves from such surfaces, or in fact from any two, but different alluvial or soft foundations, would only yield results of local value. Any haphazard selection of a site for a laboratory might take us to a place where we might find an apparent diurnal or other variation in gravity, and where the same gold bead upon an assayer's balance rapidly changing its zero, might appear to have a different weight at different times. Then, again, if we wish to study the continuous trembling of our earth, we require to be on solid materials at least half a mile from a railway, and some distance from any source of artificially produced vibration. If in addition to this it should ever be found desirable to obtain a highly magnified record of the movements of a magnetic needle, it is obvious that we must be far removed from the possibility of electrical disturbance.

What is required, and especially for earthquake recording, is a platform or foundation which continues downwards as a uniform mass into the interior of the earth. Such conditions may probably be met with upon certain granite bosses, but it is likely that the greatest continuity would be found upon an old volcanic neck, of which we have very many illustrations in these islands. Although these are mere *coup d'épingles* in the crust of our earth, it is not unlikely, especially when their lateral dimensions increase with depth, that they convey more vibrational energy to the surface than is conveyed through discontinuous sedimentary strata.

If in connection with diurnal waves we wish to record sunshine, or to note the rise and fall of stars or distant objects as seen through a telescope at the time of large earth waves, the station should command, especially in an east and west direction, a fairly extensive horizon.

Inasmuch as an observer may, as a means by which "air tremors" can be destroyed, require in one of his rooms a copious ventilation with a minimum of dampness, a precaution of some importance is not to ignore the hygrometric conditions of a locality.

A good site having been found, the remaining requirements for a seismological observatory are small. All that is necessary is a small one-storied structure. It should contain one or two large rooms in which to place some half-dozen instruments, and three small rooms to be used respectively as an office, a workshop and a dark room.

In Italy there are fifteen observatories of this order, and a very large portion of the work is to record movements of the earth's crust, which can be equally well recorded in England. At Strassburg, which is as free from earthquakes as any town in England, a seismological observatory, costing 3500*l*., with an annual grant for maintenance of 275*l*., is being erected. Austria and Germany are establishing stations, whilst the great work which for years past has been carried out in Japan is too well known to require restating.

In conclusion, when we consider that the observations made at a seismological laboratory are connected with those made by the meteorologist, the geologist and the astronomer, that they suggest problems to the elastician, shed light upon perturbations of magnetic needles, are of direct importance to the cable engineer, and in the interpretation of certain telegrams, and that in many other directions they are of value both scientifically and practically, it seems strange, especially in the face of the hearty co-operation we have received from abroad, that this country is yet without a definite centre at which these observations can be carried on.

JOHN MILNE.

SATURN'S NINTH SATELLITE.

ON Saturday last, March 18, the astronomical world, somewhat recovering from the excitement incident to the discovery of the remarkable asteroid now named Eros, was again pleasantly surprised by the news of another "find," distributed by telegram from the Central Astronomical Bureau at Kiel. This time it is the planet Saturn which supplies the feature of interest, in that an addition to its already numerous family of attendant satellites has been discovered by Prof. William H. Pickering, assistant astronomer at Lowell Observatory, Flagstaff, Arizona. The name of this station will be familiar to all in connection with the many notable observations of the planet Mars which have been made there by Mr. Lowell, its director, with the 24-inch refractor. Most of this work is so delicate as to need the best conditions for seeing, and it is only the extremely favourable situation of this observatory which has rendered them possible. This is probably to be attributed to the extreme transparency of the air consequent on the high altitude above the sea-level.

The new satellite has been run to earth, as it were, by photography. On examination of four photographs of Saturn, Prof. Pickering found traces on each of a very faint object, the behaviour of which led him to consider it to be a satellite of the planet. The little stranger is estimated to be of the 15th magnitude, so that it is unlikely that it would ever have been discovered by visual observation, even in the huge instruments now at the disposal of our leading astronomers. Measurements of the coordinates of its position from the four plates have furnished the data for computing its period or time of revolution round the parent planet, and this is found to be about *seventeen* months. This indicates that it will take its place as the outermost of the nine satellites, the period of Japetus, the furthest from Saturn of the known ones, being only about $79\frac{1}{3}$ days. While the distance of Japetus is 2,225,000 miles, that of the new moon will therefore be about 7,500,000 miles, and this, combined with its extremely slow motion, all tended to diminish the chances of its detection by the usual method of tracking non-stellar objects by the elongated trails they leave on the photographic plate, the stars being shown as symmetrical round dots.

It is interesting to note how the gradual discovery of the attendants of the various planets has influenced the compounding of the "laws" which from time to time have been found to approximately represent the positions of these bodies in the solar system. From the first discovery of Jupiter's four satellites by Galileo in 1610 to the recognition of the already known eight of Saturn by Huyghens, Cassini, and Sir W. Herschel, no regular relationship was perceived. When, however, in August 1877, Prof. Asaph Hall discovered the two moons of Mars, Deimos and Phobos, with the newly-erected 26-inch refractor of the United States Naval Observatory at Washington, it was seen that all the then known satellites were grouped in a geometrical progression, reckoning outwards from the Earth. Thus the Earth had one, Mars two, Jupiter four, and Saturn eight. This seeming regularity was broken by the discovery on September 9, 1892, of a fifth satellite to Jupiter by Prof. E. E. Barnard at the Lick Observatory. This last discovery of a ninth satellite for Saturn will furnish a reason for a new series being formed, as counting from the Earth outward from the Sun, the numbers of satellites to the planets Earth, Mars, Jupiter, and Saturn are now 1, 2, 5 and 9 respectively, and these numbers are very nearly proportional to the distances of those planets from the Sun.

No information is yet to hand as to the diameter of this newly-found member of the solar system. From its brightness it may be from 100-200 miles, but its measurement will be extremely difficult.

The importance of photography in astronomical research is very well illustrated in the case of this event. Although it might be possible to see the satellite under good conditions, it is easy to understand how many times such an insignificant object might be passed over among so many more prominent ones. Once it has impressed its image on a photographic plate, however, it is caught, and its detection is sure, sooner or later, on complete examination of the negative. Then the possibility of duplication removes all doubt of personal error of any kind. Another advantage of the photographic plate over the eye is that the longer it is exposed, so much fainter objects will it record; while, on the other hand the eye only becomes more fatigued the longer it is used in the search.

It should be instructive to notice how most of the astronomical discoveries of late years hail from across the Atlantic. Whether it is that the love of science is more generally developed there, or that the liberal endowment of a scientific institution is considered the most serviceable way of handing one's name down to posterity, it is certain that in the establishment of the Harvard College, Lick and Yerkes Observatories the American people have placed themselves ahead in astronomical matters; and there is little doubt that they are well satisfied with the results obtained by means of their liberality.

A later telegram to the *Standard* states that the discovery was made with the Catherine-Bruce telescope, an instrument of large aperture and short focal length.

C. P. BUTLER.