following. A few years ago Wolff showed that if the spores from the *Æcidia* of *Peridermium Pini* (var. *acicola*) are sown on the leaf of *Senecio*, the germinal hyphæ which grow out from the spores *enter the stomata of the Senecio leaf, and there develop into the fungus called Coleosporium Senecionis.* In other words, the fungus growing in the cortex of the pine, and that parasitic on the leaves of the groundsel and its allies, are one and the same: it spends part of its life on the tree and the other part on the herb.

If I left the matter stated only in this bald manner, it is probable that few of my readers would believe the wonder. But, as a matter of fact, this phenomenon, on the one hand, is by no means a solitary instance, for we know many of these fungi which require two host-plants in order to complete their life-history; and, on the other hand, several observers of the highest rank have repeated Wolff's experiment and found his results correct. Hartig, for instance, to whose indefatigable and ingenious researches we owe most that is known of the disease caused by the *Peridermium*, has confirmed Wolff's results.

It was to the brilliant researches of the late Prof. De Bary that we owe the first recognition of this remarkable phenomenon of *heterœcism*—*i.e.* the inhabiting

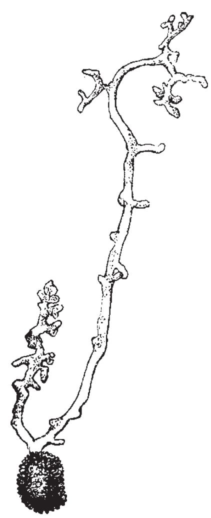

FIG. 35.—A spore of *Peridermium Pini* germinating. It puts forth the long, branched germinal hyphæ on the damp surface of a leaf of *Senecio*, and one of the branches enters a stoma, and forms a mycelium in the leaf: after some time, the mycelium gives rise to the uredospores and teleutospores of *Coleosporium Senecionis.* (After Tulasne: highly magnified.)

more than one host—of the fungi. De Bary proved that the old idea of the farmer, that the rust is very apt to appear on wheat growing in the neighbourhood of berberry-bushes, was no fable; but, on the contrary, that the yellow *Æcidium* on the berberry is a phase in the life-history of the fungus causing the wheat-rust. Many other cases are now known, *e.g.* the *Æcidium abietinum*, on the spruce firs in the Alps, passes the other part of its life on the Rhododendrons of the same region. Another well-known example is that of the fungus *Gymnosporangium*, which injures the wood of junipers: Oersted first proved that the other part of its life is spent on the leaves of certain Rosaceæ, and his discovery has been repeatedly confirmed. I have myself observed the following confirmation of this. The stems of the junipers so common in the neighbourhood of Silverdale (near Morecambe Bay) used to be distorted with *Gymnosporangium*, and covered with the *teleutospores* of this fungus every spring: in July all the hawthorn hedges in the neighbourhood had their leaves covered with the Æcidium form (formerly called *Rœstelia*), and it was quite easy to show that the fungus on the hawthorn leaves was produced by sowing the *Gymnosporangium* spores on them. Many other well-established cases of similar heterœcism could be quoted.

But we must return to the *Peridermium Pini*. It will be remembered that I expressed myself somewhat cautiously regarding the *Peridermium* on the leaves (var. *acicola*). It appears that there is need for further investigations into the life-history of this form, for it has been thought more than probable that it is not a mere variety of the other, but a totally different species.

Only so lately as 1883, however, Wolff succeeded in infecting the leaves of *Senecio* with the spores of *Peridermium Pini* (*acicola*), and developing the *Coleosporium*, thus showing that both the varieties belong to the same fungus.

It will be seen from the foregoing that in the study of the biological relationships between any one plant which we happen to value because it produces timber, and any other which grows in the neighbourhood there may be (and there usually is) a series of problems fraught with interest so deep scientifically, and so important economically, that one would suppose no efforts would be spared to investigate them: no doubt it will be seen as time progresses that what occasionally looks like apathy with regard to these matters is in reality only apparent indifference due to want of information.

Returning once more to the particular case in question, it is obvious that our new knowledge points to the desirability of keeping the seed-beds and nurseries especially clean from groundsel and weeds of that description: on the one hand, such weeds are noxious in themselves, and on the other they harbour the *Coleosporium* form of the fungus *Peridermium* under the best conditions for infection. It may be added that it is known that the fungus can go on being reproduced by the *uredospores* on the groundsel-plants which live through the winter.

H. MARSHALL WARD.

(*To be continued.*)

EARTHQUAKES AND HOW TO MEASURE THEM.[1]

PROF. EWING explained that the study of earthquakes had two aspects, one geological and the other mechanical, and it was of the latter alone that his lecture was to treat. The mechanical student of earthquakes concerned himself with the character of the motion that was experienced at any point on the earth's crust, and with the means by which an earthquake spread from point to point by elastic vibration of rock and soil. The first problem in seismometry was to determine exactly how the ground moved during an earthquake, to find the amount and direction of every displacement, and the velocity and rate of acceleration at every instant while the shaking went on. He was to deal with the solution of that problem, and to describe some of the results which had been obtained in the measurement of earthquakes in Japan, where earthquakes happened with a frequency sufficient to satisfy the most enthusiastic seismologist. Most early attempts to reduce the observing of earthquakes to an exact science had failed because they were based on a false notion of what earthquake motion was. It had been supposed that an earthquake consisted of a single or at least a prominent jerk, or a few jerks, easily distinguishable from any minor oscillations that might occur at the same time. The old column seismometer, for instance, recommended in the Admiralty Manual of Scientific Inquiry, attempted to measure what was called the intensity of the shock by means of a number of circular columns of various diameters which were set to stand upright like ninepins on a level base. It was expected that the shock

[1] Abstract of a Lecture delivered at the Royal Institution on Friday evening, June 1, by Prof. J. A. Ewing, F.R.S.

would overthrow the narrower columns, the broadest that fell serving to measure its severity, and that the columns would fall in a direction which would point to the place of origin of the disturbance. In fact, however, such columns fell most capriciously when they fell at all, and it was impossible to learn anything positive from their behaviour in an earthquake. The reason was that there was no single outstanding impulse: an earthquake consisted of a confused multitudinous jumble of irregular oscillations, which shifted their direction with such rapidity that a point on the earth's surface wriggled through a path like the form a loose coil of string might take if it were ravelled into a state of the utmost confusion. The mechanical problem in seismometry was to find a steady-point—to suspend a body so that some point in it, at least, should not move while this complicated wriggling was going on. The steady-point would then serve as a datum with respect to which the movement of the ground might be recorded and measured. The simple pendulum had often been suggested as a steady-point seismometer, but in the protracted series of oscillations which made up an earthquake the bob of a pendulum might, and often did, acquire so much oscillation that, far from remaining at rest, it moved much more than the ground itself. The lecturer illustrated this by showing the cumulative effect of a succession of small impulses on a pendulum when these happened to agree in period with the pendulum's swing. The fault of the pendulum, from the seismometric point of view, was its too great stability, and its consequently short period of free oscillation. To prevent the body whose inertia was to furnish a steady-point from acquiring independent oscillation, the body must be suspended or supported astatically; in other words, its equilibrium must be very nearly neutral. Methods of astatic suspension which had been used in seismometry were described and illustrated by diagrams and models, in particular the ball and block seismometer of Dr. Verbeck, the horizontal pendulum, and a method of suspension by crossed cords based on the Tchebicheff straight-line link-work.

The complete analysis of the ground's motion was effected by a seismograph which resolved it into three components, two horizontal and one vertical, and recorded each of these separately, with respect to an appropriate steady-point, by means of a multiplying lever, on a sheet of smoked glass which was caused to revolve at a uniform rate by clock-work. The clock was started into motion by the action of the earliest tremors of the earthquake on a very delicate electric seismoscope, the construction of which was shown by a diagram. In this way a record was deposited upon the revolving plate which gave every possible particular regarding the character of the earth's motion at the observing-station. A complete set of the instruments as now manufactured by the Cambridge Scientific Instrument Company was shown in action. Prof. Ewing also described his duplex pendulum seismograph, which draws on a fixed plate of smoked glass a magnified picture of the horizontal motion of the ground during an earthquake. Apparatus was shown for testing the accuracy of the seismographs by means of imitation earthquakes, which shook the stand of the instrument, and drew two diagrams side by side upon the glass plate—one the record given by the seismograph itself, and the other the record derived from a fixed piece which was held fast in an independent support. The agreement of the two records with one another proved how very nearly motionless the "steady-point" of the seismograph remained during even a prolonged shaking resembling an earthquake. This test was applied to the instruments on the table, and the close agreement of the two diagrams was exhibited by projecting them on the lantern-screen. A large number of autographic records of Japanese earthquakes were thrown on the screen, including several which have been already reproduced in this journal (NATURE, vol. xxx. p. 174, vol. xxxi. p. 581, vol. xxxvi. p. 107); and particulars were given of the extent of the motion, and the velocity and rate of acceleration, in some representative examples. To determine the rate of acceleration was of special interest, because it measured the destructive tendency of the shock. The lecturer explained that some of the seismograms exhibited on the screen had been obtained since he had left Japan by his former assistant, Mr. Sekiya, who now held the unique position of Professor of Seismology in the Imperial Japanese University. Prof. Sekiya had recently taken the pains to construct a model representing, by means of a long coil of copper wire carefully bent into the proper form, the actual path pursued by a point on the earth's surface during a prolonged and rather severe shaking. This model of an earthquake had been made by combining the three components of each successive displacement as these were recorded by a set of seismographs like those upon the lecture-table. The appearance of Prof. Sekiya's model (a description of which will be found in NATURE, vol. xxxvii. p. 297) was shown to the audience by means of the lantern.

Prof. Ewing drew attention to the small tremors of high frequency which characterized the beginnings of earthquake motion, and which were apparent in a number of the diagrams he exhibited. These generally disappeared at a comparatively early stage in the disturbance. In the early portion they were generally found at first alone, preceding the larger and and slower principal motions; and then when the principal motions began, small tremors might still be seen for some time, superposed upon them. In all probability these quick-period tremors were normal vibrations, while the larger motions were transverse vibrations; and a reference to the theory of the transmission of vibrations in elastic solids served to explain why the quick-period tremors were the first to be felt. The whole disturbance went on for several minutes, with irregular fluctuations in the amplitude of the motion, and with a protracted dying out of the oscillations, the period of which usually lengthened towards the close. The record of a single earthquake comprised some hundreds of successive movements, to and fro, round fantastic loops. Each single movement usually occupied from half a second to two seconds. Earthquakes were quite perceptible in which the greatest extent of motion was no more than 1/100 of an inch. In one case, on the other hand, Prof. Sekiya had obtained a record in which the motion was as much as an inch and three-quarters. Even that was in an earthquake which did comparatively little damage, and there was therefore reason to expect that in a severely destructive shock (such as had not occurred since the present system of seismometry was developed) the motion might be considerably greater.

Prof. Ewing concluded his lecture by pointing out that seismographs might find practical application in measuring the stiffness of engineering structures. He exhibited, by the lantern, seismographic records he had recently taken on the new Tay Bridge, to examine the shaking of the bridge during the passage of trains. The instrument had been placed on one of the great girders, two-thirds of a mile from the Fife end, at a place where there was reason to expect the vibration would be a maximum. The extent of motion was remarkably small. It was less than an eighth of an inch, even while the train was passing the seismograph—a fact which spoke well for the stiffness of the structure. Nevertheless, by watching the index of the seismograph he had been able to tell whenever a train came on at the Dundee end of the bridge, a distance of $1\frac{1}{3}$ mile from the place where the instrument was standing. One could then detect a vibratory motion, the extent of which was probably not more than 1/500 of an inch. This began in the longitudinal direction, and for some time longitudinal vibration only could be seen. As the train came nearer, lateral vibration also began, and the amplitude of course increased. It reached a maximum when the train was close to the seismograph, and continued visible until the train had passed off the bridge at the other end.

DOES PRECIPITATION INFLUENCE THE MOVEMENT OF CYCLONES?

IN Prof. Elias Loomis's first "Contribution to Meteorology," in the *American Journal of Arts and Science*, he examined the distribution of rain around 152 storms (cyclones) in the United States, in order to determine whether there exists any relation between the velocity of a storm's progress and the extent of the accompanying rain area. He found that "the average extent of the rain area on the east side of the storm's centre is 500 miles; and when the rain area extends more than 500 miles, the storm advances with a velocity greater than the mean; but when the extent of the rain area is less than 500 miles, the storm advances with a velocity less than the mean." In his twelfth "Contribution" he examined 39 storms which moved with exceptional velocity (1000 miles or more per day) and found that "the rain area generally extended a great distance in advance of the storm centre, the average distance being 667 miles." Finally, Loomis examined 29 cases of those abnormal cyclones in the United States which moved toward the west. He says:—"In nearly every case we find a fall of rain or snow in the region toward which the low centre advanced, and in most of the cases the rainfall was unusually great. . . . It may be inferred from these comparisons that the fall of rain or snow is one of the most important causes which determine the abnormal movements of areas of low pressure" (ninth memoir, p. 44). Ley and Abercromby state that in Great Britain the relation of the weather to the cyclone centre is the same whatever the path of the cyclone; thus when storms advance toward the west the greatest cloud development and rainfall is to the west of the cyclone centre. In the Proceedings of the Royal Meteorological Society, vol. xliii., Abercromby gives a table showing the relation between the intensity of "trough phenomena" and the velocity of cyclones. This table indicates very clearly that the greater the velocity of the cyclone the more marked the "trough phenomena." Hence, according to Abercromby's definition of "trough phenomena" the heaviest rain and cloud areas are massed toward the front of rapidly advancing cyclones, while immediately after the passage of the line of minimum pressure the sky begins to show signs of clearing. This is especially well marked in cyclones passing off the north-east coast of the United States. When the cyclones are moving with unusual rapidity, not only all the rain, but almost all of the cloud area is confined to the front half of the cyclone.

Loomis suggested that the excess of rain in front of rapidly advancing cyclones was one of the causes of the rapid advance; but when investigating heavy rainfalls in the United States he concludes that "the forces which impart that movement to the air which is requisite to an abundant precipitation of vapour, instead of deriving increased strength from the great volume of rain, rapidly expend themselves and become exhausted;" and after examining certain cyclones which were accompanied by no rain he adds: "So that it seems safe to conclude that rainfall is not essential to the formation of areas of low barometer, and is not the principal cause of their formation or of their progressive movement." Hann arrives at similar conclusions from investigations in Europe. After investigating an especially heavy rainfall which occurred in Austria and vicinity in August 1880, he concludes thus:—"The appearance of a barometric minimum in Hungary occasioned abnormal and extended precipitation on the west and north-west side of this barometric depression. The reaction of this precipitation on the position of the centre of the depression is scarcely perceptible. . . . We find, therefore, through the investigation of the relative lowest barometer reading in its behaviour to rainfall, that our former conclusions are confirmed" (lxxxii. *Bunde d. Wiss.* ii. Ab., November 1880). This investigation does not necessarily prove that precipitation does not appreciably influence the movements of cyclones in general, but at least suggests that in the first cases mentioned above the unequal distribution of rain around rapidly moving cyclones was not the cause, but the result of the cyclone's advance. In cyclones which move very slowly, as do tropical cyclones, the air ascends almost uniformly around the centre; but when cyclones have a more rapid progressive motion, the air in the rear, which has not only to enter, but to follow the cyclone, is more retarded by friction than the air in front, and hence does not enter the cyclone so freely, so that the formation of cloud and rain in the rear is retarded; while, on the other hand, a larger volume of new air enters the progressing cyclone in front, and increases the amount of precipitation. Thus, between February 12 and 14, a cyclone passed across the American continent with the exceptionally high velocity of 58 miles per hour. During its passage the highest wind velocity reported on any of the United States Signal Service morning weather maps was 40 miles per hour, occurring immediately in the rear of the cyclone at Father Point, Can., on the morning of the 14th. At none of the other 130 stations did the maps show a wind velocity exceeding 30 miles per hour during the passage of the cyclone. This is an example of many similar cases which show that in rapidly moving cyclones the air in the rear near the earth's surface does not move as rapidly as the cyclone itself. Hence, it seems evident that the air near the surface immediately in the rear of these cyclones is not air which has followed the cyclone near the surface, but air which has descended from above. Espy showed many years ago that, on account of mechanical heating by compression, no descending air can be accompanied by precipitation; and an explanation is thus afforded why there is none, or but little cloud and precipitation in the rear of rapidly moving cyclones. On the other hand, in order that a cyclone may advance rapidly, there must be a rapid decrease in pressure, and consequently a rapid removal of the air, in front of the advancing depression. Since, according to the normal circulation of a cyclone, there is an inward movement near the earth's surface and an upward and outward movement near the top, this upward and outward movement is necessarily increased in unusually rapid-moving cyclones, and hence also the cloudiness and precipitation are increased.

Hourly observations of cloud movements made during the day hours for nearly two years at Blue Hill Observatory indicate that the velocity of storm movement, and especially the variability of the weather, are intimately connected with the velocity of movement of the general atmosphere.

The writer is hence led to believe that the main cause of rapid cyclone progression is an unusually rapid drifting of the atmosphere over large regions; and the unequal distribution of rain around the cyclone is due to the rapid progress of the cyclone.

H. HELM CLAYTON.

Blue Hill Observatory, Boston, June 18.

NOTES.

MR. JOHN WHITEHEAD returned to Labuan in safety from his second expedition to Kina Balu, and is daily expected in England. He ascended the mountain to its summit, and attained to an altitude of 13,500 feet. His collection will contain many novelties, the small portion sent by him in advance to Mr. Bowdler Sharpe exhibiting many curious features. The new species will be described by Mr. Sharpe in the forthcoming