ted, as his has been essentially a scientific career. For a number of years past he has held the office of President of the Andersonian University, in Glasgow. Surrounded by the members of his own family and by those of his lamented friend Livingstone—for he has really been *in loco parentis* to the children of the African traveler—Mr. Young, for whose portrait we are indebted to the *Practical Magazine*, now spends the great bulk of his time at his beautiful estate of Kelly, near Greenock, Scotland, or at his no less fine and romantic estate of Durris in Aberdeenshire. But he also mixes to some little extent in public life, contributing liberally to all movements of a patriotic or charitable character, and aiding by every means within his power the progress of scientific knowledge.

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VELOCITY OF NERVOUS IMPULSES.

In his suggestive lecture on the sun, our English visitor, Mr. R. A. Proctor, makes use of several striking illustrations to give an idea of the immense distance between us and our great luminary. One of these supposes an infant with an arm of the inconvenient length of ninety-one millions of miles, who should stretch forth his hand and touch the sun. Naturally the darling would have his finger burnt; but, so slow is the transmission of feeling, he would have to wait until he was a hundred and thirty-five years old before he could be conscious of the fact. In this estimate Mr. Proctor evidently adopts the rate of nerve motion obtained some twenty years ago by the observations of Dr. Hirsch—that is, about one hundred and eleven feet a second. The later and more elaborate researches of Dr. Schleske show a rapidity of conduction by the sensory nerves of about ninety-seven feet a second, which would require our sunburnt infant to wait some years longer before discovering his indiscretion. If he trusted his sight in the matter, he might become aware of the danger of his distant member in the short space of eight minutes, so much more rapid is the speed of light than the movement of feeling along the nerves. The passage of volition along the motor nerves appears to be still slower; so that upwards of a century and a half, perhaps, might elapse before the mental order to withdraw the finger could be carried out.

However slow the rate of nervous movement may be, as compared with the velocity of light or the still fleeter motion of electricity, it is nevertheless so rapid that until quite recently it was thought to be immeasurable, within the limited range in which our observation of it is possible. The most widely separated points in the course of any nerve allow but a few feet of difference at best for timing the periods of sensation or volition; and the nervous impulse travels so quickly that such small distances would seem to be wholly annihilated. To our consciousness a prick on the great toe is discovered as promptly as one on the cheek; and it is only by the intervention of the most delicate and ingenious of mechanical contrivances that the difference in time is made apparent.

The first step toward making the solution of this interesting problem possible was taken in the antiphysiological art of gunnery. In the development of that art, it became necessary to measure the speed of projectiles, both in the gun and during the several stages of their flight. For this purpose Pouillet's chronoscope was devised, by means of which an electric current was made to indicate the duration of the most rapidly transient processes. Thus the passage of a bullet along the barrel of a gun was found to occupy the hundred and fiftieth part of a second. It quickly occurred to Helmholtz that here, possibly, was a means of measuring the speed of nervous action. His application of the method was too complex for description in this place; it was, however, so trustworthy as to leave no doubt of the practical accuracy of its results. His object was to measure the intervals of time, if there were any, between the excitation of a nerve at two different points and the corresponding contractions of the muscle. The difference between such intervals would, of course, be the time required for the passage of the nervous impulse over the space between the two points of excitation. Experimenting with the leg of a frog, two sets of observations were obtained, differing from each other by a small but constant quantity. For the more distant point of excitation, a measurable fraction of a second longer was uniformly required to make the muscle contract. The difference of distance being exactly measured, the rate of propagation of the nervous impulse was easily calculated. Instead of rivaling the velocity of electricity, as had hitherto been supposed, the rapidity of conduction in the motor nerves of the frog was found to be no more than eighty five feet a second. All this was as early as 1851. To test the accuracy of the result thus obtained, Professor Helmholtz devised another and more simple apparatus, which he called a myographicon. In this the contracting muscle was made to directly register the beginning and successive stages of the contraction by means of a style working against a rotating cylinder. This confirmed the general correctness of the estimate obtained with Pouillet's apparatus, the rate demonstrated being a little over 89 feet a second.

Various improvements of the myographicon were soon suggested by Du Bois Raymond and others, whose observations, while differing slightly in result, were not conflicting with previous results, due allowance being made for temperature and other disturbing conditions. The maximum rate obtained by the last named observer was 30 meters a second, or 98½ feet. This was the estimate on which he based his widely quoted illustration of the harpooned whale. If one of these sea monsters, a hundred feet long, were struck in the tail, he said it would take a full second before the sense of pain could reach the victim's brain; and, omitting the time necessary for perception and volition, another second must pass before an order could be telegraphed to the tail to retaliate by upsetting the harpooner's boat.

In all the experiments on motor nerves thus far, the leg of a frog had been used. In 1867, Baxt and Helmholtz applied the test to man, using an improvement of the myographicon suggested by Du Bois Raymond. The result gave the rate of conduction for the motor nerves of man, corresponding to that already obtained by Hirsch for the sensory nerves. A very careful series of experiments by the same observers, in the summer of 1869, showed a mean rapidity for the motor nerves in man very much greater, or about 254 feet a second. But this by no means invalidated the result already obtained, since, as Helmholtz had shown, the rate varies greatly with temperature, being not more than one tenth as great at 32° as at 60° or 70°.

More recently it has been established by Dr. Munck that the velocity of nervous impulses is different in different nerves, and in different parts of the same nerves, the rapidity increasing as the termination of the nerve is approached, and by Marey's observation, that fatigue of the muscles has the effect of seriously reducing the rate of nervous conduction; while Wittich has found that the rate is in some degree dependent on the mode of excitation, being greater when electricity is used than when the stimulus is mechanical. The same observer also reports a considerable difference between the rates of motor and sensory nerves, the latter excelling by at least a third.

The measurement of the rate at which the nervous impulse travels brainward necessarily involves a process very different from any employed in the study of the motor nerves. The problem was first attacked by the Swiss astronomer Dr. Hirsch. Soon after Helmholtz took up the other branch of the investigation, and his solution of it was as ingenious as it was successful. It involved the measurement, with the delicate chronometric instruments employed by astronomers, of the difference in time between the appreciation of impressions made at a distance from the brain, say on the great toe, and others nearer, as on the cheek. Roughly described, the plan adopted was substantially this: The observer sat with his finger on a signal key, with which he announced the perception of an electric shock as soon as possible after feeling it, thus closing an electric circuit which had been broken by the shock. The minute interval between the breaking and closing of the circuit measured the time taken by the transmission of the shock to the brain, the time required for the perception of the sensation, time for willing the movement of the signal key, time for the transmission of this volition to the proper muscles, time for the contraction of the muscles, and finally the time lost in the physical process of signaling. Obviously all these parts, except the first, must be substantially the same in all experiments by the same person, using the same finger for making the signal. Any difference in the whole time must therefore be owing to the greater or smaller distance of the particular point of impression from the brain. This difference being measured with tolerable exactness, it is possible to calculate pretty closely the rate at which the nervous impulse is transmitted. The estimate first made by Dr. Hirsch was, as already noted, 111 feet a second. More recent determinations give averages ranging from 97 feet, by Dr. Schleske, to 136 feet, Wittich's estimate for a nervous impulse excited by electricity. With a mechanical stimulus, he found an average velocity of 124 feet. These figures, of course, are to be taken relatively. The rate varies in different individuals, and, doubtless, in the same individual, with varying conditions of health, temperature, and so on, the general average being about that of a high wind, a race horse, or a locomotive. Light excels it about ten million times, and electricity more than fifteen million times.

But, it may be asked, what is the use of all these investigations? Of what account is a delay of the hundredth part of a second, more or less, in the perception of a sensation or the transmission of a volition, so long as we are not conscious of it? In astronomy, it has proved to be of material account; and it is more than probable that the knowledge of the normal rate of nervous impulses thus obtained may some day be of the greatest help in the diagnosis of nervous diseases.

With the nicest appliances for observing and timing phenomena, there still remain discrepancies between the reports of different observers, however skillful. Time is required for the act of perception, for willing the pre-determined signal, and yet more for executing the volition, all of which directly affect the accuracy of the observation; and since these intervals differ with different observers, the exact moment of an occurrence cannot be fixed without knowing and allowing for them.

THE AUTOPSY OF PROFESSOR AGASSIZ.

Dr. Morrill Wyman, of Cambridge, Mass., has published a report on the autopsy recently made upon the body of Professor Agassiz, from which it may be deduced that the disease to which the great naturalist succumbed was one of long standing. The arteries at the base of the brain showed evidence of extensive chronic disease of their lining membrane, and also several important changes which were fatal. In the left ventricle at the lower third, a firm, organized clot, of the size of a peach stone, attached to the wall at the anterior portion near the septum, was found, and around this clot a more recent one had formed, its center softened and granular. From this, probably some small portions had been carried by the blood to the arteries at the base of the brain, doing their part in obstructing them and causing the fatal alterations above noted. The lungs showed evidence of old inflammation. The entire weight of the brain was 53·4 ounces avoirdupois, and its greatest weight, between the ages of 35 and 40 years, was estimated at 56·5 ounces.

Without entering into the technical details of the investigation, the result shows that the trouble began with inflammation of the lining membrane of the lungs, and that the morbid processes, carried by the blood from heart to brain, there disorganized and checked the circulation. The malady was too deeply situated to have admitted of surgical aid, nor could any effort of human skill have averted death from its effects. The autopsy was made in the interests of science and in deference to the expressed wishes of Professor Agassiz, long since placed on record.

MICROSCOPIC CRYSTALS IN PLANTS.

Besides the familiar bundles of needle-shaped crystals, called raphides, dispersed throughout the cellular structure of certain plants, there are in the seed covers and leaves of several orders of plants, and in the pods of the bean family, multitudes of prismatic crystals of extreme minuteness, which have hitherto escaped detection. In the horned poppy, these crystals are as small as the 8,000th of an inch in diameter. In the gooseberry and elm, they are $\frac{1}{3000}$ of an inch; in the black currant, about half as large; in the black bryony, they are about $\frac{1}{1500}$ of an inch in diameter, thickly set at regular distances throughout the seed covers. In the gooseberry, they are so distinctly and regularly placed in the outer skin—each crystal in a separate cell—that they present the appearance of crystaline tissues. In plants of the bean family, the size is variable, the average being about $\frac{1}{3000}$ of an inch. In the garden pea, they are much larger These crystals appear to consist chiefly of oxalate of lime, sometimes carbonate. Raphides are mainly phosphate of lime.

Plants most relished by animals are found to be especially rich in these microscopic crystals. In a piece of the midrib of a clover leaflet, $\frac{1}{70}$ of an inch in length, Mr. Gulliver, who has added more than any other to our knowledge of these minute but important products of vegetable action, has counted 10 chains of crystals with 25 in a chain, making 250 in all, or no less than 17,500 to the inch. In like manner 21,000 crystals were reckoned for one inch of the sutral margin of a single valve of a pea pod. The pod had four such margins, each three inches in length; so that in a single pod there must have been as many as 250,000 crystals. In view of the marvelous number of these crystals, as well as their regularity and constancy, Mr. Gulliver believes it no longer possible for physiologists to maintain that such structures are accidental freaks of nature, of no relation to or value in the life and use of the species.

THE FIRELESS LOCOMOTIVE.

Mr. Richard H. Buel, a well known consulting engineer in this city, has recently published in the *Railroad Gazette* an account of a trial trip with one of the engines of the Fireless Locomotive Company. This article is interesting as being the first in which the theory of the action has been fully set forth. We have, on several occasions, made mention in our columns of the fireless locomotive, and have pointed out the advantages it possesses in many cases, such as greater comparative safety, less need of skilled attendants, and the absence of smoke and other products of combustion. Mr. Buel, in the article referred to, demonstrates that the locomotive can be operated successfully, if properly designed and managed; and he points out such improvements as seem to be desirable. We give a brief summary of the principal statements, omitting all mathematical work: