SCIENCE.—SUPPLEMENT.

FRIDAY, MAY 20, 1887.

ABSTRACT OF THE RESULTS OF THE INVESTIGATION OF THE CHARLESTON EARTHQUAKE.[1]

THE amount of information now in possession of the U. S. geological survey, relating to the Charleston earthquake, is very much larger than any of similar nature ever before collected relating to any one earthquake. The number of localities reported exceeds sixteen hundred. The sources of information are as follows: 1°, we are deeply indebted to the U.S. signal service for furnishing us the reports of their observers; and, 2°, equally so to the Lighthouse board, which has obtained and forwarded to us the reports of keepers of all lighthouses from Massachusetts to Louisiana and upon the Great Lakes; 3°, to the Western union telegraph company, which instructed its division superintendents to collate and transmit many valuable reports; 4°, to the Associated press, which has given us access to the full despatches (with transcripts thereof) which were sent over the wires centring at Washington during the week following the earthquake; 5°, to geologists and weather-bureaus of several states, who have kindly exerted themselves in this matter, and collected much important information; 6°, to a considerable number of scientific gentlemen who have distributed for us our circular letters of inquiry in special districts, notably, Profs. W. M. Davis, C. G. Rockwood, J. P. Lesley, T. C. Mendenhall, and Messrs. W. R. Barnes of Kentucky and Earle Sloan of South Carolina; 7°, to a large number of postmasters in the eastern, central, and southern states; and, finally, to hundreds of miscellaneous correspondents throughout the country.

In collecting this information, a printed list of questions was prepared. This practice has been resorted to in Europe and in Japan with considerable success, and the questions which have been devised for distribution in those countries have been prepared with great skill by some of the ablest investigators of earthquakes. Prof. C. G. Rockwood of Princeton has also been in the habit of distributing formal questions of this character in this country whenever apprised by the newspapers of a notable shock. Availing ourselves of his advice and assistance, questions prepared by him were printed and widely distributed. They were much fewer and more simple than those employed in Europe, because European investigators depend almost wholly upon the educated classes to answer them, while in this country the uneducated but intelligent and practical classes of the people must be the main reliance. These questions were designed to elicit information, 1°, as to whether the earthquake was felt; 2°, the time of its occurrence; 3°, how long it continued; 4°, whether accompanied by sounds; 5°, the number of shocks; 6°, general characteristics which would serve as a measure of its intensity, and indicate the kind and direction of motion.

It is to be observed that the only information to be hoped for, which can have even a roughly approximate accuracy, is the time of transit of the shock. The degree of approximation in the time data actually obtained will be adverted to later. Special effort was made to obtain information as to the relative intensity of the shocks in all parts of the country. At the very outset a serious difficulty presents itself. In the estimates of intensities there is no absolute measure. What is really desired is some reliable indication which shall serve as a measure of the amount of energy in any given portion of the wave of disturbance as it passes each locality. The means of reaching even a provisional judgment are very indirect, and qualified by a considerable amount of uncertainty. To estimate the force of a shock, we have no better means than by examining its effects upon buildings, upon the soil, upon all kinds of loose objects, and upon the fears, actions, and sensations of people who feel it. In view of the precise methods which modern science brings to bear upon other lines of physical research, all this seems crude and barbarous to the last degree. But we have no other resource. Even if it were possible to obtain strictly comparative results from such facts, and decide with confidence the relative measure of intensity which should be assigned to each locality, we should have gained measures only of a series of local surface intensities, and not of the real energy of the deeply seated wave which is the proximate cause of the surface phenomena. Notwithstanding the indirect bearing of the facts upon the real quantities we seek to ascertain, and their apparently confused and distantly related character, they give better results than might have been supposed. When taken in

[1] Read before the National academy of sciences at Washington, April 19, 1887.

large groups, they give some broad indications of a highly suggestive character; and though affected with great inequalities, which for the time being seem to be anomalous, these anomalies are as instructive as the main facts themselves.

We have given the preliminary plotting of the intensities in the map before you. The first point to which we shall invite attention is the magnitude of the area affected by the shocks. It was sensibly felt in Boston, which is the most distant point on the Atlantic coast from which affirmative reports have been received. From Maine the answers are all negative. Most of those from New Hampshire are negative, but two or three positive ones show clearly that it was felt in sensitive spots. In Vermont, affirmative reports come from St. Johnsbury and Burlington on Lake Champlain. No positive reports come from the Province of Quebec. In New York state it was felt in the vicinity of Lake George, and at Lake Placid and Blue Mountain Lake in the Adirondacks. In Ontario it was quite noticeable in several localities, though the great majority of reports from that place are negative. In Michigan it was noted in several places; and at Manistee lighthouse, on Lake Michigan, the trembling was strongly marked. In Wisconsin, though many of the reports are negative, it was felt quite strongly at Milwaukee, and was also noticed at Green Bay and at La Crosse on the Mississippi, 967 miles from Charleston, — the remotest point within the United States which has given a positive report. In central Iowa and central Missouri it was unmistakably felt. In Arkansas the eastern portion of the state from sixty to seventy-five miles west of the Mississippi gives numerous positive reports. In Louisiana the reports are mostly negative, but numerous persons in New Orleans felt the shocks, and recognized their nature. In Florida it was universally felt, and in the northern part of the state was severe and alarming. From the Everglade region, of course, no reports have been received, as it is uninhabited; but in some of the Florida Keys it was felt in notable force. From Cuba a few reports have come; and the most distant point in that island which was shaken was Sagua la Grande, where the vibration was very decided. Lastly, a report comes from Bermuda, a thousand miles distant from Charleston, which leaves little doubt that the tremors were sensible there.

The area within which the motion was sufficient to attract the attention of the unexpectant observer would be somewhat more than circumscribed by a circle of a thousand miles radius; and the area of markedly sensible shaking, would, including the oceanic area, be somewhere between two and one-half and three million square miles. In this estimate, however, only well-defined seismic movement of notable force is considered. There are reasons for believing, that, by proper instrumental observation, the movement could have been detected over a much greater area. In the first place, it is to be noted that the peripheral portions of the observed area lie in districts which are rather thinly populated, sometimes also in districts which, from the nature of the ground, do not disclose forcibly the passing shock. Furthermore, the passing wave in the outer portions of the area was almost everywhere of an undulatory character and of great wave-length, and, while still retaining a large amount of energy, did not often dissipate itself into those smaller and shorter tremors which are very much more likely to attract attention, though really possessing very much less energy. Six hundred miles from the origin the long swaying motion was felt, and was often sufficient to produce seasickness, yet was unaccompanied by sound or by the tremulous motion due to short waves.

It will be observed upon the map that there are several large tracts which show a comparatively feeble intensity, while completely surrounding them is the general area of greater intensity. The most conspicuous of these areas of silence is the Appalachian region. The facts here are extremely interesting and suggestive. It has been generally supposed that a mountain-range serves as a barrier to the propagation of earthquakes, not from any known relation of cause and effect, but merely as the result of observation. In Japan it is universal testimony that the central range of the island marks the dividing-line between earthquake and no earthquake. The shocks, so frequent there, are seldom noticed beyond the mountains. A similar conclusion has been drawn from South American earthquakes, and also from those which have visited southern Italy. As soon as the data in the earlier stages of the inquiry began to indicate insulated areas of minimum action, they were completely investigated, and every effort has been made to secure full data from them. The result has been to show satisfactorily that such was the case. The Appalachian belt south of middle Pennsylvania disclosed a few spots where the shaking was considerable; but in the main it was but lightly affected until we reach the extreme southern portion of this range, where the shocks begin to be somewhat vigorous, even in the mountains. West and northwest of the range, however, the force of the undulations resumes even more than its normal vigor. In eastern Kentucky and south-eastern Ohio the force of the shocks was very considerable causing general alarm. Chimneys and bricks were shaken down, and the oscillation of the houses was strongly felt. In south-eastern Ohio nearly every theatre, lodge, and prayer-meeting was broken up in confusion. It does not appear that the Appalachians offered any sensible barrier to the progress of the deeper waves, but it does appear that they affected in a conspicuous degree the manner in which the energy of the waves was dissipated at the surface. Another minimum area was found in southern Indiana and Illinois, and also in southern Alabama and Mississippi. There is a curious circumstance connected with the minimum area in Indiana and Illinois. On the 6th of last February an earthquake of notable force occurred in just this locality. Circulars were sent out at once, and, on plotting the isoseismals, they showed a singular coincidence in almost exactly filling the vacancy or defects of intensity of the Charleston earthquake. At present there is nothing to indicate whether this coincidence is accidental, or whether there is some hidden relation.

Where the waves passed into the newer delta region of the lower Mississippi, the surface intensity of the shocks rapidly declined. This is indicated in the map by the compression of the isoseismals in those localities. We incline to the opinion that this sudden diminution of the intensity is due to the dissipation of the energy of the waves in a very great thickness of feebly elastic, imperfectly consolidated, superficial deposits. It is a matter of common observation in all great earthquakes, that the passage of the principal shocks from rigid and firm rocks into gravels, sands, and clays, is, under certain circumstances, attended with a local increase in the amplitudes of the oscillations and in the apparent local intensity and destructiveness; and the reason for it is intelligible. But, where such looser materials are of very great thickness and great horizontal extent, the reverse should be expected: for, when a wave passes from a solid and highly elastic medium into a less solid and imperfectly elastic one, the amplitude may be suddenly increased at the instant of entering; but so rapid is the extinction, that, if the new medium be very extensive, the impulse is soon dissipated.

Many reports throughout the central states indicate localities of silence which are not expressed upon the map. The reason for omitting them is, that it has been impracticable to secure a sufficient density of observation (i.e., a sufficient number of reports per unit area) to enable us to mark out and define these smaller areas with very great precision. To do this for the whole country would require some tens of thousands of observations and the expenditure of tens of thousands of dollars to systematize and discuss the data. A map shaded to show the varying intensity by varying the depth of the shading would have a mottled appearance, in which the mottling would be most pronounced in the areas of a little below the mean intensity, say, between the isoseismals 3 and 5. This fact is of great importance in the interpretation of the isoseismals, for the omission to consider it results in giving to the middle isoseismals too high a value. In any isoseismal zone, what we should like to ascertain is the mean intensity of the whole area included within that zone. As a matter of fact, the data we possess consist more largely of maximum than of minimum or average intensities, and therefore tend to considerably augment the mean derived intensity above the true mean. This will become apparent by an inspection of the map where the zones of 5, 6, and 7 intensity are disproportionately broad, while those of 3 and 4 are disproportionately narrow. We have not attempted to allow for this source of error, though fully aware of it, because we had no means of determining what allowance to make. We have drawn the lines wholly upon the face of the returns, and the investigators who may attempt to utilize our results must grapple with the corrections as best they may.

Throughout the states of North Carolina, South Carolina, Georgia, and north-eastern Florida, and, in general, anywhere within about two hundred and fifty miles of the centre, the energy of the shocks was very great. At Columbia, Augusta, Raleigh, Atlanta, and Savannah the consternation of all people was universal. The negroes and many of the poor whites were for a week or two, not exactly 'demoralized,' but intensely moralized, giving themselves to religious exercises of a highly emotional character; the stronger and deeper natures among them being impressed with a feeling of awe, the weaker natures with a feeling of terror. And this was general throughout the large region just specified. In all of the large towns within two hundred miles of Charleston, more or less damage was suffered by houses and other structures. Walls were cracked to such an extent as to necessitate important repairs, dams were broken, chimneys were overthrown, plastering shaken from ceilings, lamps overturned, water thrown out of tanks, cars set in motion on side-tracks, animals filled with terror, fowls shaken from their roosts, loose objects thrown from mantels, chairs and beds moved horizontally upon the floor, pictures banged against the walls, trees visibly swayed and their leaves agitated and rustled as if by a wind. These occurrences were general, and were more strongly marked, until they became terrifying and disastrous as the centre of the disturbance was approached. At Augusta, 110 miles distant from the epicentrum, the damage to buildings was considerable, and at the arsenal in that place the commanding officer's residence was so badly cracked and shattered as to necessitate practical reconstruction. In Columbia, 100 miles distant, the shock was very injurious to buildings, and appalling to the people, but no substantial structures were actually shaken down. In Atlanta, 250 miles distant, there was no worse injury than falling chimneys and some slight cracks in the walls; but the houses were instantly abandoned in great alarm and confusion by their occupants, and many preferred passing the night in the streets to re-entering their dwellings. At Asheville, N.C., 230 miles distant, and at Raleigh, 215 miles distant, the shocks were quite as vigorous as at Atlanta.

Coming nearer the seismic centre, we find the intensity increasing on all sides. The region immediately about the epicentrum in a great earthquake always discloses phenomena strikingly different from those at a distance from it; and the differences are not merely in degree, but also in kind. The phenomena characteristic of the epicentral area cease with something like abruptness as we radiate away from the epicentrum. The central phenomena are those produced by shocks in which the principal component of the motion of the earth is vertical. Proceeding outwards, these predominating vertical motions pass, by a very rapid transition, into movements of which the horizontal component is the greater, and in which the undulatory motion becomes pronounced. The epicentrum, and the zone immediately surrounding it, is the portion of the disturbed tract which merits the closest attention; for it is here that we may find the greatest amount of information concerning the origin and nature of the earthquake. To appreciate this, we will venture to offer some theoretical considerations.

Allusion has already been made to the indefinite character of the data used for estimating the intensity of the shock. There is no unit of intensity which is at present available. In selecting certain effects of an earthquake to characterize varying degrees of intensity, the most that can be hoped for is a means for discriminating whether the relative energy of a shock is greater or less in one locality than in another. But how much greater and how much less — in conformity with what law — is a problem which remains to be solved. An earthquake impulse, however, is a form of energy transmitted as an elastic wave through the deeply seated rocks, and its propagation and varying intensity are subject to the laws of wave-motion. There must be, therefore, some typical law governing the rate at which such a wave diminishes the intensity of its effects as it moves onward. To anticipate the objection that this typical law would apply only to a medium which is perfectly elastic, homogeneous, and isotropic, while the rocks are far from being so, we reply that we have investigated the objection, and are satisfied, that, while it has some validity, the effect of these inequalities is not great enough to seriously impair the applicability of the law, nor to vitiate greatly the results to be deduced from it. The analysis we offer is a novel one. We attach considerable importance to it, and the consequences which flow from it are somewhat remarkable.

Let us suppose an elastic wave to originate at a point C (fig. 1), situated at the depth q below the surface. Let the intensity of the shock (amount of energy per unit area of wave-front) at the distance unity from C be denoted by a. Since the intensity is inversely proportional to the square of the distance, the intensity at the epicentrum would be $\frac{a}{q^2}$. Take any other point on the surface of the earth at the distance x from the epicentre, and connect it with C by the line Cx. The intensity at any such point will obviously be equal to $\frac{a}{r^2}$. If we denote the intensity by y, we shall then have the equation,

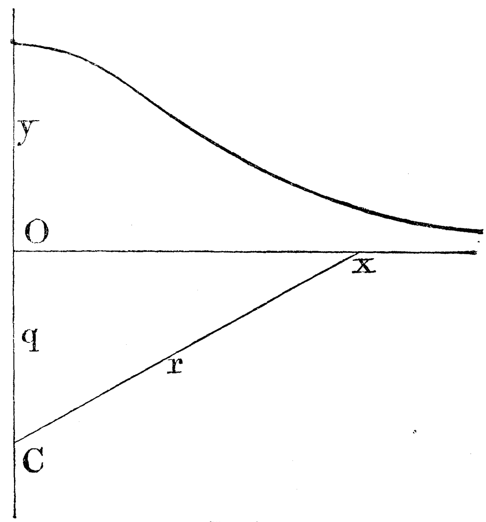


Fig. 1.

$$y = \frac{a}{r^2} = \frac{a}{q^2+x^2}$$

This equation expresses a curve which will serve as a graphic representation of the way in which the surface intensity varies along a line radiating from the epicentre.

The first noteworthy feature of this curve is the contrast between the rapidity with which the intensity diminishes near the epicentre, and the slowness with which it diminishes at remote distances. Thus, at a distance from the epicentre equal to the depth of the focus, the intensity has fallen to one-half, at twice this distance it has fallen to one-fifth, and at three times the distance to one-tenth, of the intensity at the epicentre. This suggests at once the possibility of making an approximate estimate of the depth of the focus, based upon the rate at which the intensity of the shock at the surface diminishes in the neighborhood of the epicentre. If we were able to construct upon any arbitrary scale whatever a series of isoseismal curves around the central parts of the earthquake with any approach to accuracy, this depth would follow at once from the relations of these isoseismals to each other. In the case of a very powerful earthquake in a region which is so flat and uniform in its features as the vicinity of Charleston, this can be done with a rough approach to accuracy.

To appreciate more fully the validity of this mode of reasoning, let us take a series of these intensity curves, and vary the values of the constants. And first let us suppose the total energy of the shock, measured by the constant a, remains the same, while the depth of the focus varies. The first series of curves (fig. 2) will enable us to make a comparison of the effect of two or more shocks of the same total energy, but originating at different depths. The intensity at the epicentre being inversely proportional to the square of the depth, the shallower shock would be much more energetic than the deeper one; while at a great distance from the epicentre the two would be approximately equal in their effects. The rate of diminution of intensity would be correspondingly varied, and we might commit large errors in estimating these ratios on the ground, while the error of the depth deduced for the focus would be less than our errors of estimate. In short, the method is not sensitive to small or moderate errors of observation.

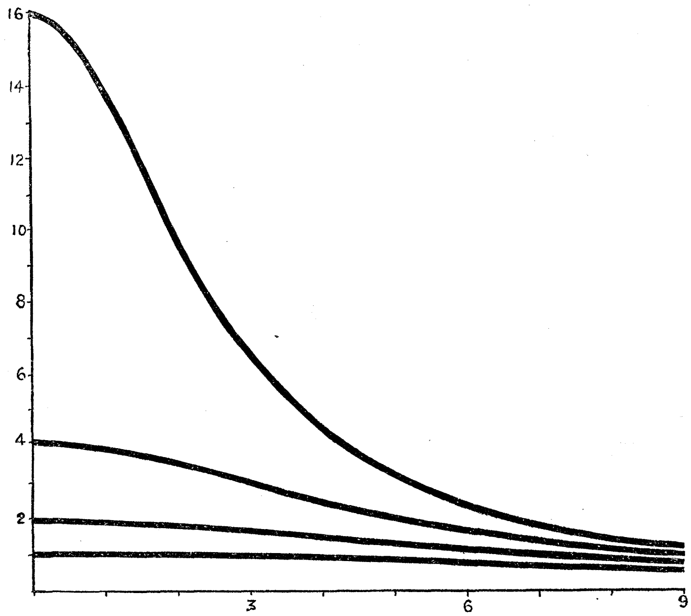


FIG. 2.—ENERGY CONSTANT, DEPTH VARYING IN RATIOS 1, 2, 3, AND 4.

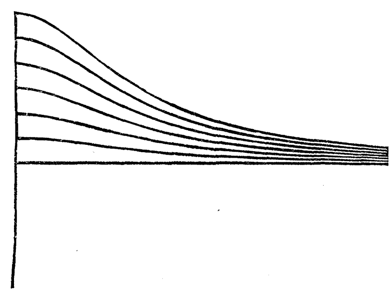


FIG. 3.—DEPTH CONSTANT, ENERGY VARYING IN RATIOS 1 TO 6.

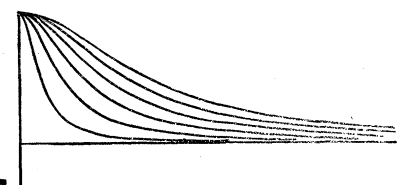


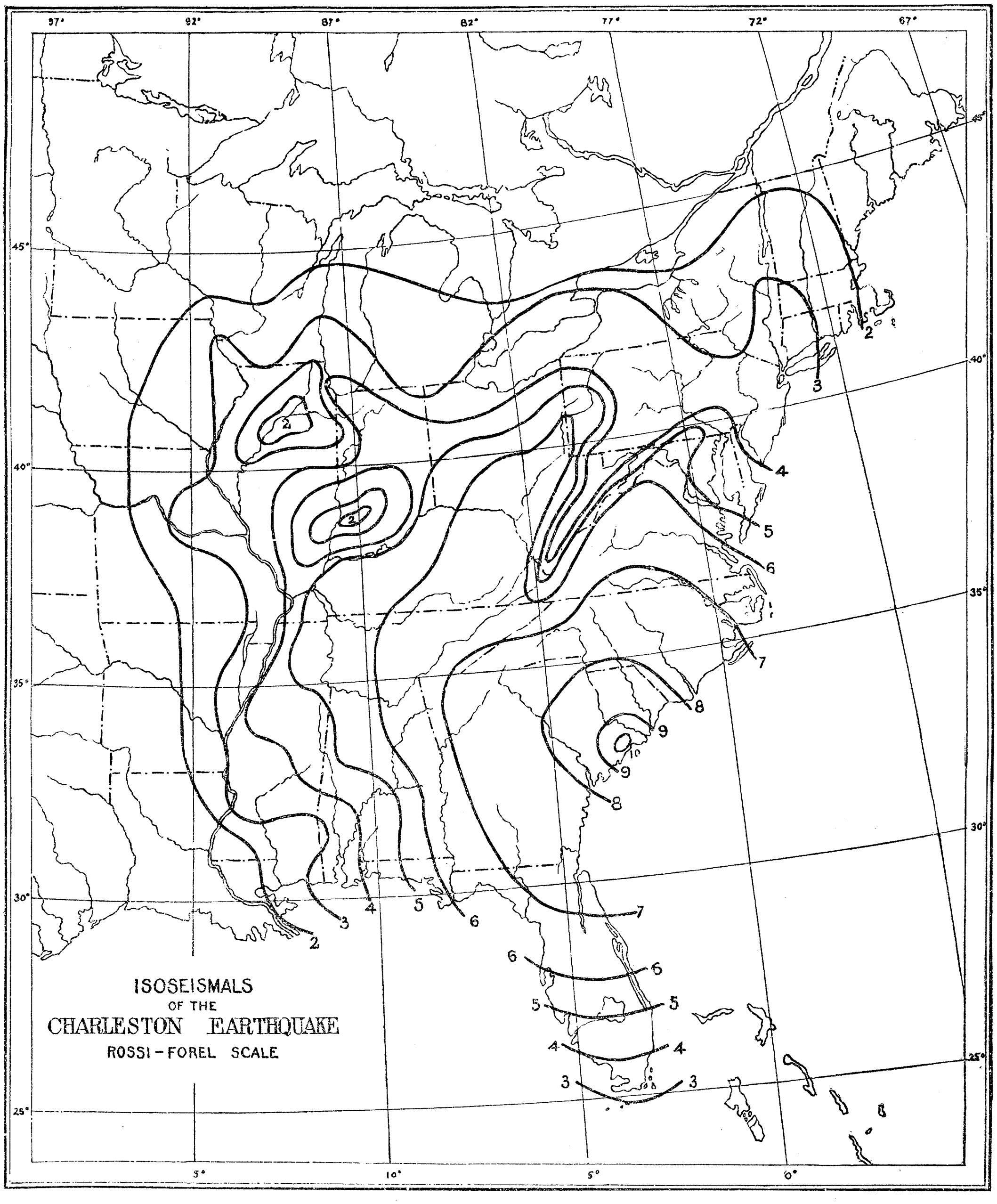
FIG. 4.—DEPTH AND ENERGY BOTH VARIABLE, BUT WITH CONSTANT INTENSITY AT THE EPICENTRE.

The second series of curves (fig. 3) is conditioned upon the assumption that the depth remains constant, while the energy of the shock varies. In these curves, the ordinates corresponding to any abscissa are proportional to each other in a simple ratio. In the first series they are proportional to each other in a duplicate ratio.

The third series (fig. 4) represents the effect of varying both the energy and the depth in such a way that the intensity at the epicentre is constant.

It will appear, therefore, that every shock must have some characteristic intensity curve, depending upon the total energy and the depth below the surface. The intensity at any point on the surface will therefore depend upon these two quantities, — energy and depth. It still remains to find some means of discriminating whether the intensity at any point is due to a more energetic shock deeply seated, or to a less energetic one nearer the surface. The criterion is soon given.

It is obvious that in any shock there is some point at some particular distance from the epicentre at which the rate of diminution of surface intensity has a maximum value. As we leave the



epicentre and proceed outwards in any direction, the intensity diminishes, at first more and more rapidly, but farther on diminishes less and less rapidly. We wish to find the point at which the rate of decline changes from an increasing to a decreasing rate. In the curve, this point is represented at the point of inflexion, where the curve ceases to be concave towards the earth, and begins to be convex towards it. To find the co-ordinates of this point, we differentiate the equation of the curve twice, and equate the value of the second differential coefficient to zero, and deduce the corresponding value of the abscissa x,

$$\frac{d^2y}{dx^2}=\frac{8ax^2-2a\ (q^2+x^2)}{(q^2+x^2)^3}=0,$$

which equation is satisfied when

$$8ax^2=2a\ (q^2+x^2),$$

whence

$$\pm x=\frac{q}{\sqrt{3}}.$$

In this value of x it is seen that the constant a has disappeared ; and the abscissa of the point of inflexion is therefore independent of the energy of the shock, and dependent upon the depth alone. The meaning of this is, that the distance from the epicentre to the point where the rate of decline of the intensity is greatest is simply proportional to the depth of the focus, and is the same whether the energy be greater or less. This property of the intensity curves makes us independent of any absolute standard of measurement for the intensity, and all that we require is to find with reasonable approximation the points where the intensity falls off most rapidly. The depth of the focus follows at once.

The determination of the epicentral tract is chiefly the work of Mr. Earle Sloan of Charleston, a young civil engineer, who, immediately after the disaster, made an extensive series of observations. In the brief time at his disposal he accumulated a surprisingly large amount of detailed information, and in searching for it exercised a discrimination and sagacity which would have been highly creditable to the most experienced and learned observer. It is to be regretted that his business engagements prevented him from continuing the work. As it is, he has located with considerable precision the epicentral tract, and has furnished data which show well the variation of intensity along several lines radiating from it.

The summary obtained from the examination of Mr. Sloan's data is as follows : the tract which includes the most forcible action of the earthquake is an elliptical area about twenty-six miles in length, and with a maximum width of about eighteen miles. The major axis of this area is not a straight line, but a curve, which is concave towards Charleston, and is situated from fourteen to sixteen miles west and north-west of that city. Along this line there are three points, each of which has all the characters of an epicentrum, determined by as many distinct shocks, each having a focus of its own. Much the most powerful shock centres in the northernmost focus, though the other two were of sufficient energy to have occasioned great havoc if either of them had occurred alone. The southernmost was also considerably more energetic than the middle one. The distance between the northern and southern epicentrum was about twelve miles. Within this tract, except near the edges of it, the motion was most conspicuously of subsultory character ; i.e., motion in which the vertical component predominated over the horizontal. The marginal portions of this area, where the character of the movement changes, and where the intensity falls off most rapidly, seem to be very well indicated. The positions where the intensity most rapidly declines may be located with an error not exceeding one or two miles on both sides of the epicentres. The South Carolina railroad crosses the tract in a straight line very near the most forcible seismic vertical. The first point where the intensity falls off with greatest rapidity is near the nine-mile post, measuring from the railway depot in Charleston ; and so well marked upon the ground are the indications of this change, that it seems very improbable that this point is more than a mile distant either way from the precise point we seek to locate. Passing north-westward through Summerville to the opposite side of the tract, we find the corresponding point of most rapid decline in the vicinity of the twenty-third-mile post. This gives us a base-line with which to measure the depth of the focus of the principal shock. The computed depth is twelve miles, with a probable error of one or two miles. The computed depths of the other foci are about the same, but the probable errors are somewhat larger.

In speaking of a focal point of a shock, it must be understood as referring to the centre of all the forces, considered with reference both to amount and direction, which constitute a great seismic impulse. The presumption is, that this impulse originates in a large subterranean tract of which this ideal focus is merely the central point, or nearly so. The form of the subterranean tract may be any thing, and, within limits, may have its three dimensions (length, breadth, and thickness) of any magnitude, and bearing any ratios to each other. The form and dimensions of it, we cannot, of course, determine, though it may be possible to obtain some notion of its most general features if the data are sufficient.

This method of computing the depth of a seismic focus is here proposed for the first time. The method employed by Mallet, which consists in finding the angle of emergence of a wave-front from the earth by studying the configuration of cracks in buildings, is believed to be pretty nearly valueless by all seismologists. There is no definite angle of emergence, of the nature he contemplates, disclosed at the surface. Certainly in Charleston there was nothing of the kind to be found. The method employed by Seebach is sound in theory, but it requires such extreme accuracy of time-determinations that very small errors of time give very large errors in the result. Our own method consists in finding two points on opposite sides of the seismic vertical, at which the changes in seismic action along a given line are most strongly marked. These points ought to be indicated in powerful earthquakes with a fair approach to precision, and the probable errors of determination should not usually exceed one or two tenths of the distance between the two points. The feebler the shock, however, the less is the degree of precision to be expected. Whatever may be the errors in the estimate of this distance, the resulting error in the computed depth is smaller than the error of observation in the ratio of the square root of three to two. How much the estimate may be vitiated by want of homogeneity in the superficial strata, we have no means of determining; but we do not believe that it would be so affected to any great extent in such a region as South Carolina. Being independent of any absolute measures either of the surface intensity or of the total energy of the shock, the greatest difficulty of all is at once eliminated. Our own opinion of this method is, that it is incapable alike of very great precision and of very great errors.

Probably the first thought occurring to any one examining this method will be that the determination of the two required points would be liable to very large errors. But, if he will examine the varying values of the ordinates of the curve corresponding to varying values of the abscissas and of the depth, we think he will be satisfied that the limits within which each of the two points of inflexion must fall cannot be wide apart, and that an error in the determination of the base-line greater than two-tenths of its estimated length would in such a country as Carolina be very improbable. It will appear that the relations of these variables are such as to restrict the locus within which the desired points are to be found to a very narrow annulus around the epicentrum. We believe the method will improve upon acquaintance.

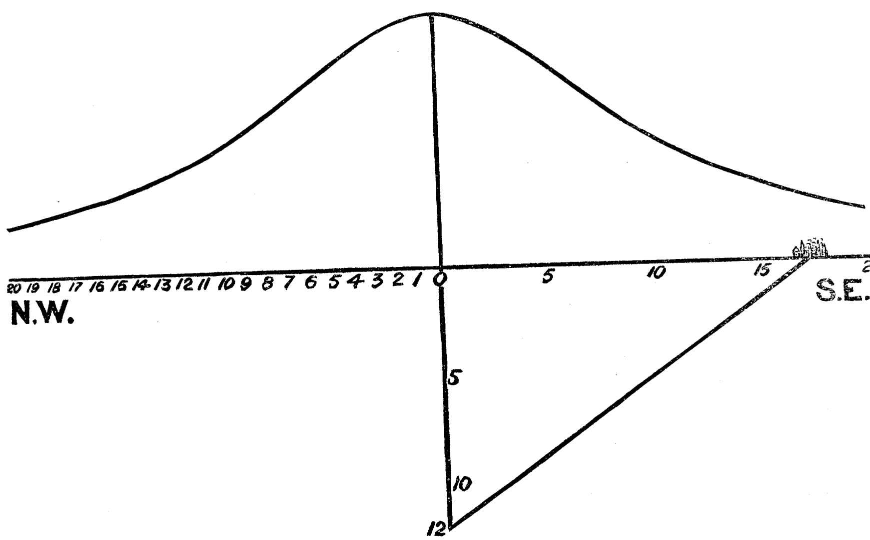


FIG. 5.—INTENSITY CURVE OF CHARLESTON EARTHQUAKE.

We have endeavored to apply our method of computing the depth of the focus to other earthquakes, but have found difficulty in obtaining any thing more than very general results, such as the following. The depth of the Charleston earthquake was relatively great; and we find reason for believing, that among those great earthquakes of the last hundred and fifty years, of whose effects we possess any considerable knowledge, none have originated from a much greater depth, and few from a depth so great. Our reasoning is this: very few earthquakes have been felt at a distance from the origin so great as a thousand miles; but the greatest distance at which the tremors are felt is the best measure of the total energy of the shock. On the other hand, the intensity of the Charleston earthquake in the epicentral tract was relatively low in comparison with other great earthquakes. If, then, any shock is more intense at the epicentre without extending to a greater distance than that of the Charleston earthquake, it is certain that its focus was nearer the surface. This is true of the vast majority of recent earthquakes which have been sufficiently investigated. It is suggested that all estimates of the depth of earthquake foci much exceeding twelve miles are in need of re-examination.

The city of Charleston is situated from eight to ten miles outside of the area of maximum intensity, and did not experience its most destructive power. Following the law which we have laid down, the intensity of the shock at Charleston was only three-tenths what it must have been at the epicentrum, and about one-third the intensity at Summerville. The diagram (fig. 5) showing the long intensity curve stretching from Charleston to a point forty miles north-west of it, will illustrate the position of the city with reference to the varying force of the shock.

Had the seismic centre been ten miles nearer to Charleston, the calamity would have been incomparably greater than it was, and the loss of life would probably have been appalling. Another circumstance greatly broke the force of the shocks. All of the coastal region of the Carolinas consists of a series of clays and quicksands, which have been penetrated by artesian borings to a depth of two thousand feet, and which are believed to have a much greater thickness. These beds of loose material, no doubt, absorbed and extinguished a considerable portion of the energy of the shocks. We have already remarked that a wave passing from firmer and more elastic material into material less firm and elastic, produces at first an increased amplitude of wave-motion which is liable to be more destructive or injurious to buildings. But, if the mass of less consistent strata be very great, the reverse result is produced, by reason of the rapid extinction of the energy in passing through a considerable length or thickness of very imperfectly elastic material. We cannot but think that Charleston owes in some measure its escape from a still greater calamity to the quicksands beneath the city.

Another aspect of the same fact, if such it be, is found a hundred miles west and north-west of Charleston. Here the loosely aggregated sediments of tertiary and cretaceous age which cover the Carolina coastal plain have thinned out, and the crystalline rocks appear at the surface, thinly covered with soil and alluvium. All along the junction of these loose strata and superficial material with the metamorphics, the intensity of the shocks was conspicuously greater than to the eastward and southward. The loose covering of these firm rocks is just thick enough to give full effect to the increased amplitude of vibration which occurs when the wave passes from very solid and elastic rocks to those which are less so.

We have also endeavored to reach some trustworthy estimate of the amplitude of movement at the surface, but the results are meagre and far from satisfactory. The 'amplitude of the earth particle' in any earthquake is a question of great practical importance, and it is much to be regretted that no better facilities for determining it can be obtained. There were, however, many occurrences at Charleston bearing upon this question, which are extremely difficult to explain upon any valuation of the amplitude less than ten inches to a foot. Such amplitudes, however, were most probably limited to spots here and there, while in other spots it was probably much less. That within a small area the amplitude of movement in the surface soil varies between very wide limits, seems to be a practically certain conclusion from the observations. In Charleston it appears to have been greatest in the 'made ground,' where ravines and sloughs were filled up in the early years of the city's history. The structures on higher ground, though severely shaken, did not suffer so much injury.

With regard to the time data from which the speed of propagation must be computed, we are not yet in a position to give final results, but can only state how the problem stands at present. The time reports have been placed in the hands of Professors Rockwood and Newcomb, with the request that they would scrutinize and discuss them. But neither has been able to finish, as yet, the task he has so courteously undertaken. Probably the greatest difficulty in the way of determining the speed of propagation arises from the ill-defined character of the disturbance at considerable distances from the origin, and from the very considerable duration of it. Wherever a time observation seems to be well authenticated, there still remains, in most cases, the difficulty of deciding to what particular phase of the earthquake the record refers; and this difficulty is a very serious one. At Summerville the first shock came almost like an explosion. Before people had time to think, they were pitched about like ten-pins. At Charleston there was a perceptible interval, estimated at from five to eight seconds, from the first note of warning to the maximum of the great shock. At Savannah (90 miles distant) the interval from the beginning to the first maximum was considerably longer, probably ten to twelve seconds; at Augusta (115 miles) the interval was still greater; and, generally speaking, the greater the distance, the more the phenomena were 'long drawn out.' The duration of the earthquake at Charleston will probably never be known with accuracy, but the general testimony ranges between fifty and ninety seconds. At Washington (450 miles) Professor Newcomb, with his watch in his hand, observed a duration of perceptible tremors with two maxima lasting about five and one-half minutes. Professor Carpmael's magnetographs recorded the disturbance, and he interprets their photographic traces as showing a duration of about four minutes. Mr. G. W. Holstein of Belvidere, N.J., gives five minutes very nearly as the observed duration. From other localities come well-attested observations showing durations of several minutes, though few of these pretend to give the whole time with any accuracy. This progressive lengthening of the shocks is a well-marked feature of the testimony. The explanation suggests itself at once. The elastic modulus of compression being greater than that of distortion, the speed of the normal waves is the greater, while the waves of distortion lag behind.

It is obvious that the phase which it is desired to observe should be the arrival of the first impulses, but the great duration of the tremors has left much doubt on this point. Stopped clocks were plentiful all over the country, but at what phase of the earthquake did they stop? So great, indeed, are the uncertainties on this point, that the observations of intelligent men, with watches in their hands, measuring a part of the shock and estimating the beginning, are in most cases to be preferred to stopped clocks, even if we knew with certainty that the clocks had been accurate to the second. It matters little how we twist and turn the time data: the smallest estimate we can put upon the speed of propagation must prove to be a great surprise to seismologists.

The time at Charleston of the occurrence of the main shock has been fixed at 9.51.10 P.M., 75th meridian, or eastern standard time (all times in this paper, unless otherwise specified, are reduced to that meridian). The uncertainty does not exceed ten seconds. The beginning of the first tremors at Charleston was from six to eight seconds earlier. The time at Summerville was probably less than four seconds earlier than Charleston. For all localities within two hundred miles, the time observations are of little value. So swiftly did the waves travel, that a small error in the time record gives a very large uncertainty in the resulting speed.

The nearest point which yields a valuable record is Wytheville, Va. (286 miles).[1] Mr. Howard Shriver was sitting at a transit instrument, waiting for the passage of a star, and at once noted the time at 9.52.37 (reduced to 75th meridian), giving a speed of about 3.3 miles (5,300 metres) per second. There is some slight uncertainty about the precise phase of the shock corresponding to the observation.

The signal service observer at Chattanooga (332 miles) gives only the nearest minute for the principal shock at 9.53, corresponding to a speed of 3.02 miles per second, or 4,860 metres.

The best observation in our possession is that of Prof. Simon Newcomb himself, at Washington (450 miles), who gives the time of the beginning of the shock at 9.53.20, with an uncertainty not greatly exceeding ten seconds. The resulting speed is 3.46 miles per second, or 5,570 metres.

From Baltimore (486 miles) Mr. Richard Randolph, C.E., reports a very intelligent and carefully verified observation of 9.53.20 as the beginning of the shock, — exactly Professor Newcomb's time for Washington, giving a speed of 3.74 miles, or 6,000 metres, per second.

At Atlantic City, N.J. (552 miles), a large pendulum-clock in the Fothergill House stopped at 9.54 very nearly. If this may be taken to be the beginning of the shock, the speed would be 3.26 miles per second, or 5,250 metres.

George Wolf Holstein, Belvidere, N.J. (622 miles), gives 9.54 for the beginning of the shock, and 9.59 for the end, and compared his watch next morning with the time of the Pennsylvania railroad. The gradual and uncertain character of the beginning and end would not admit of precise determination to seconds. The speed, taking 9.54 for the beginning, would be 3.66 miles, or 5,900 metres.

From New York City (645 miles) and its suburban towns and cities come many reports, all of which give either 9.54 or 9.55 as the nearest minutes. If we take as a mean 9.54.25 at New York and Brooklyn for the beginning of the shock, the speed would be 3.31 miles, or 5,330 metres.

[1] The distances are measured somewhat hastily with a scale upon the war department map of the United States, taking the greater epicentrum 16½ miles north-west of Charleston as the starting-point.

At distances greater than six hundred miles, the difficulty of associating the time records with particular phases of the shocks becomes very great. In most cases the motion was the swaying movement, with only faint tremors of the rapid kind; and those who felt them were slow in recognizing their character. Readers must form their own opinions as to the degree of approximation to the time of the earliest movements, from the following records. We give them only as we received them, without attempting any discussion.

J. O. Jacot, watchmaker and jeweller at Stockbridge, Mass. (772 miles), was sitting by his regulator-clock, distinctly recognized the nature of the movement, and noted the time as 9.56. The phase of the shock is uncertain.

At Albany, N.Y. (772 miles), Mr. J. M. Clarke, of the New York state museum of natural history, heard the mortar falling down the chimney and the creaking and straining of the building. As soon as he appreciated the character of the disturbance, he noted the time by his watch at 9.56.30. He did not ascertain the error of his watch. In the same city, Dr. Willis G. Tucker says he instantly looked at his watch, and after comparing it next morning with the time of the Dudley observatory, and making correction of the error, gave 9.55 very nearly, with an error probably not exceeding twenty seconds.

From Fonda, N.Y. (780 miles), Francis L. Yates reports 9.55 (no particulars).

At Ithaca, N.Y. (695 miles), the regulator clock on the wall of the railway-depot stopped at 9.55 'exactly.'

At Gowanda, N.Y. (666 miles), where the shocks were faintly felt, W. R. Smallwood, watchmaker and jeweller, noted the end of the perceptible shocks at 9.55.30 by his regulator-clock.

At Toronto (753 miles) the earthquake was recorded automatically upon the magnetographic traces in the observatory of Prof. Charles Carpmael, superintendent of the Meteorological service of Canada. In his letter of Sept. 14 he says, "I may state that at 9.55 P.M. all our magnetic needles were set in motion by earth-tremors. The vibrations of the magnets continued for about four minutes. I would say, that, from later and more careful measurements from our magnetic curves, I make the time of the earth-tremor at Toronto to be 9.54.50 P.M. standard: this time, I should say, would not be astray more than a few seconds." As this record was automatic, and gave not only the time but the phases, it has been thoroughly investigated by Professors Newcomb and Carpmael, assisted by Mr. C. A. Schott of the U. S. coast survey. The final result of this reexamination is to change Professor Carpmael's computation to 9.56.18 for the beginning of the tremors, with a probable error of fully one minute. This large probable error is due to the very small scale upon which the magnetograph records time intervals (one-tenth of a millimetre corresponding to twenty seconds), and to want of sharpness in the photographed traces. This time gives 2.66 miles per second, or 4,250 metres, with a probable error of one or two tenths the amount.

The clock in the Western union telegraph office at Pittsburgh (523 miles) was stopped at 9.54.

From Cincinnati and suburban towns (500 miles) come many reports. In this city, local mean time is largely used, owing to the fact that it is nearly midway between the 75th and 90th meridians, where the only inconvenience of standard time is at a maximum. The correction to the 75th meridian is 37m. 40s. The Western union telegraph office gives 9.54. The *Times-star* newspaper gives from the clock in its own office 9.16 'exactly' (9.53.40 standard); at the *Commercial gazette* office, 9.17.45 local, 9.55.25 standard (probably noted after the shocks were over). At the fire-tower after the principal shock, 9.16.17 was noted; clock error twenty-three seconds slow, giving 9.54.20 standard. Two other observers noting by watches give 9.16, and one notes an advanced stage of the shocks at 9.17, but they give no means of estimating their errors. At Covington, Ky., across the Ohio River, I. J. Evans, watchmaker and jeweller, reports his regulator-clock stopped at 9.17.20, Cincinnati local mean time; phase of shock unknown.

From Crawfordsville, Ind. (622 miles), E. C. Simpson, C.E., reports through Prof. J. M. Coulter of Wabash college, "Suddenly felt my chair move, jumped up, and said, 'We are having an earthquake.' At once pulling out my watch, I found it was 8.54 P.M., standard time (central). Professor Coulter adds, that the watch was exactly with railroad time as shown at the railroad-station and also by the town-clock.

From Dyersburg, Tenn. (569 miles), Louis Hughes writes, "My time-piece was an English patent lever watch of Charles Taylor & Son, London, which from business necessity I keep closely with railroad time at the station, which receives the time at ten o'clock every morning. The railroad uses central time. My first thought was that the shaking was caused by the children in the next room, but in the next moment, recognizing the peculiar sensation, I dropped the newspaper and observed the time, which was probably from four to six seconds after 8.54, and from that approximated it in even minutes." Speed 3.25 miles, or 5,230 metres.

At Memphis, Tenn. (590 miles), the signal-service observer reports a considerable number of stopped clocks, one at 9.54, and the others at 9.55. For some unaccountable reason the seconds were not noted. The phase is unknown.

The foregoing comprise those time reports which seem to justify the presumption that the errors do not exceed one minute. There are others which are obviously rude approximations, giving exact hours, quarter-hours, or tens of minutes. There are also some which look at first like good observations, but which surely involve some large unexplained error.

As the discussion of the time data is now progressing, no further comment will be offered here, beyond the remark that there can be no doubt that the speed of propagation exceeded 3 miles, or 5,000 metres, per second. The only questions are, how much this speed was exceeded, and whether the speed along any given line was constant. As regards the latter question, the data are not yet precise enough to justify an opinion. This matter will be inquired into.

The high rate of propagation will probably prove unexpected to European seismologists. We propose, however, to follow it up with the suggestion that it is about the normal speed with which such waves ought to be expected to travel, and that all determinations of the rate of propagation in any former great earthquakes, which are much less than 5,000 metres per second, for normal waves at least, are probably erroneous in proportion as they fall short of the Charleston earthquake. Finding, as the time reports accumulated, that a speed in excess of 5,000 metres was indicated, and this presumption having become a conviction, we were led to inquire whether there were not some speed deducible from the theory of wave-motion in an elastic solid to which all great earthquakes ought to approximate.

In a homogeneous and perfectly elastic solid, the rate of propagation is, according to theory, dependent upon two properties of the medium, — elasticity and density. There are two coefficients of elasticity in solid bodies, one of which measures their resistance to changes of volume, the other to changes of form. Absolute experimental determinations of the values of these coefficients have never been made. If, however, we knew the ratios of these coefficients in one substance to the homologous coefficients in any other substance, and if we also knew the rate of propagation in either of them, the rate in the other would be at once deducible. The rate in steel bars has been the subject of much experimentation, and is given by Wertheim, whose researches have been as careful as any, at 16,800 feet per second. But, as the waves in a steel bar are essentially waves of distortion, he multiplies this result by $\sqrt{\frac{3}{2}}$ or $\frac{5}{4}$ for the normal wave, giving a speed of 21,000 feet per second. The elastic modulus of steel for engineering purposes is usually taken to be 29,000,000. The corresponding modulus for such rocks as granite and basalt in a very compact state is about 8,000,000. If we may assume that these moduli are proportional to the two elasticities of the two substances respectively, we can compute the rate of propagation in rock. This assumption may or may not be true; but we assume it to be so. Let V_s be the rate of propagation in steel, and V_r the rate of propagation in rock, and let e_s and e_r be their true elasticities of volume, and let D_s and D_r be their respective densities. Our assumption is, that $29 : 8 :: e_s : e_r$, from which we may form the equation,

$$\frac{V_s}{V_r} = \sqrt{\frac{e_s}{D_s} \times \frac{D_r}{e_r}}$$

Taking the density of steel at 7.84, and of deeply buried rocks in their most compact state at 2.85,

$$\frac{V_s}{V_r} = \sqrt{\frac{29}{7.84} \times \frac{2.85}{8}} = 1.15 \text{ nearly.}$$

Taking the rate of compressional waves in steel to be 6,400 metres per second, gives 5,570 metres for similar waves in very compact and dense rock. The corresponding rate for waves of distortion would be 4,450 metres. These results are so near to those deduced for the Charleston earthquake that they seem to be worthy of consideration.

The experimental measurements of the rate of impulses obtained by Milne and Fouqué seem to us inapplicable. The elasticity of the surface soil, we think, is no more to be compared with that of the profound rocks which transmit the great waves of an earthquake, than the elasticity of a heap of iron filings is to be compared with that of an indefinitely extended mass of solid steel. The difference is *toto coelo*. But the rate of propagation is a question of elasticity and density chiefly. The effect of temperature we have not considered. Perhaps the most striking experiment ever made with an artificial earthquake was at the Flood Rock explosion in Hell Gate, near New York, where General Abbott found a speed of propagation approaching very closely to that of the Charleston earthquake.

The question which is undoubtedly of deepest interest in this connection is whether the Charleston earthquake throws any new light upon the origin of such events. While we are not prepared to say that absolutely nothing will be added to our information on this question, we are forced to admit that we expect very little new light. Hitherto our efforts have been devoted to bringing together the facts, and to arranging and comparing them, and we have as yet given but little consideration to this final question. It will, however, shortly engage our attention; and, in anticipation of this, we prefer to remain silent for the present, fearing that if we commit ourselves here to any preference for a particular view, we may find ourselves encumbered with a bias arising from the intensely human propensity to defend, through thick and thin, utterances which have once been formally given.

C. E. DUTTON.
EVERETT HAYDEN.

WAGNER'S ANNUAL REPORT ON THE PROGRESS OF GEOGRAPHY.

IT is always with some impatience that we expect the publication of Wagner's report on the progress of geography (*Geographisches Jahrbuch*), because we know that we shall find there a full report of the work done in the field and in the study, and that we shall have a never-failing book of reference. We do not know of any similar publication, — except the fragmentary notes published by the Smithsonian institution and in the journals of many societies, — and therefore it is indispensable to the geographer. Though *Petermann's Mittheilungen*, the leading German geographical journal, contains regular reports on recent publications, their character is different from those in the *Jahrbuch*, the reports in the journal giving a more detailed review of the single publications, and being more disconnected. The list of reviewed books is consequently not so full as that of the annual report. The latter gives a comprehensive account of the work done during the last two years. The present volume is the eleventh of the series. The editor, Prof. H. Wagner of Göttingen, has preferred to divide the material, and to publish alternating volumes, one containing the various branches of geography, the other the progress of explorations, methods and teaching of geography, etc. Through this division, the book has increased in volume and the report has become more exhaustive. The present volume contains the special part, geophysics, geognosy, oceanography, climatology, geography of plants and animals, and ethnology. The place of the late Professor Zöppritz is taken by Dr. Hergesell and Dr. Rudolph; the former report on deep-sea explorations has been enlarged so as to cover all problems of oceanography, and is given by Professor Krümmel; F. Toula reports on geognosy; the other parts are in the hands of the same specialists who gave the valuable reports of former years.

In looking at the long series of reports, we find that each number served more satisfactorily the purpose of being a reliable book of reference to all interested in geography. At the present time there are few branches of geographical study which are not embraced in the book. The steady development of the plan, by dropping unnecessary parts, including in one part what belongs together, and adding new departments which had developed into important branches of science, encourages us to hope that within a few years the whole domain of geography will be represented in it. We should wish, for instance, to have an additional report on the history of geography. That on terrestrial magnetism is promised for next year. Among the important additions in the volume of 1887 is the first report of the geography of ancient Greece and the neighboring countries. The ancient geography of other countries, except that of the birthplace of our culture, is so little studied, that the contents of a biennial report would be very meagre. We hope, with the development of these studies, which are principally carried on by ethnographers, we shall find an account of these also. In 1882 Egli's reports on the study of geographical names, and S. Günther's on the theory of map-projections, were added to the book. We consider it a waste of time and work, that the Physical society of Berlin continues its reports on physical geography in the way they were given before the *Jahrbuch* had attained its present importance. It is true that they contain some material not included in the *Jahrbuch*, for instance, measurements of heights, etc.; however, these would far better find their place in the latter publication than in the reports on the progress of physics.

The rapid development of the *Jahrbuch* and the fact that every department is intrusted to the care of a specialist, make it an extremely reliable and useful book, which is a valuable help to the student of geography.

Geographisches Jahrbuch. Vol. xi. 1887. Ed. by HERMANN WAGNER. Gotha, Justus Perthes, 1887.

THE MECHANICS OF MACHINERY.

PROFESSOR KENNEDY is well known as one of the ablest among British workers in this field, and this volume contains a series of lectures delivered by him to his classes during the period of his connection with the University college, on a subject with which he is especially familiar. As was to be expected, the work is one of exceptional value.

The mechanics of machinery. By ALEX. B. W. KENNEDY. London, Macmillan. 16°.