ART. XXII.—*On the Velocity of Transmission of Earth Waves ;*
by General H. L. ABBOT, Corps of Engineers.

ADVANTAGE was taken of the explosion of 50,000 pounds of dynamite at Hallet's Point, on September 24th, 1876, to measure the velocity with which the shock was transmitted through the ground, both across Long Island and along the south bank of East River. The results were embodied in a paper read by me before the National Academy of Sciences, on October 18th, 1876, and subsequently again read and printed as one of the papers of the Essayons Club of the Corps of Engineers.

In the number of the London, Edinburgh and Dublin Philosophical Magazine, for October, 1877, appeared a short review of this paper from the pen of Mr. Robert Mallet, F.R.S., a gentleman well known for his numerous and able contributions to seismology. In this article, he suggested reasons which led him to doubt the value and accuracy of the Hallet's Point results.

Even if I had felt disposed to enter into a controversy upon the subject, I should have been quite disarmed by the concluding sentence of this article, which expresses views so just and liberal that it may well be quoted as an exemplar of the manner in which scientific questions should be considered. He writes :

"In these objections I wish to be clearly understood as having no *a priori* difficulty in accepting a higher velocity of wave transit than the highest attained experimentally by myself. It is highly probable that such may be elicited by future experiment. But should such cases arise, their results like all great physical truths, should only be credited upon unexceptionable observations or experimental evidence. While feeling justified in making these objections, I wish to disclaim all controversial spirit or intention ; loss of sight, indeed, and diminished energy would prevent my engaging in any scientific controversy, were any called for."

Believing, at the date of my first paper, that the data secured at the Hallet's Point explosion demonstrated the necessity for more exact and comprehensive knowledge of the subject, I have, during the past season, taken advantage of the facilities offered by large sub-aqueous explosions at the School of Submarine Mining at Willet's Point, to continue the investigation ; and, on October 23d, 1877, I read a second paper before the National Academy of Sciences, giving the results thus obtained. As only a brief abstract of this paper has appeared in print, I propose now to give a summary of the conclusions suggested by the whole series of experiments and, incidentally, to explain

my reasons for believing that Mr. Mallet has not quite understood the parts of my first paper to which he has taken exception.

Limited space forbids any detailed explanation here of the method adopted for measuring the time of transmission of the shocks; especially as this is fully given in my printed paper, together with the notes of the observers in full. Suffice it to say that the instant of explosion and the time of arrival of the tremors, were electrically recorded on the same moving paper with extreme precision. The following table exhibits the data, of which only the first six observations were known to Mr. Mallet when he wrote his article.

No. of observations.	Date.	Observer.	Cause of shock.	Distance to station.	Type of seismometer.	Tremor of mercury.		Velocity of transmission.
						Arrived in	Last- ed for	
				miles.		secs.	secs.	ft. per sec.
1	Aug. 18, '76.	Capt. Livermore	200 lbs. dynm.	5 ±	B.	5 ±		5280 ±
2	Sept. 24, '76.	Lieut. Young	Hallet's Pt. ex.	5·134	A.	7 ±	63 ±	3873 ±
3	" " "	Lieut. Griffin	" " "	8·330	B.	5·3	72·3	8300
4	" " "	Lieut. Kingman	" " "	9·333	A.	10·9	23·5	4521
5	" " "	Lieut. Leach	" " "	12·769	B.	12·7	19·0	5309
6	Oct. 10, '76.	Lieut. Kingman	70 lbs. powder.	1·360	A.	5·8	inst.	1240
7	Sept. 6, '77.	Lieut. Kingman	400 lbs. dynm.	1·169	A.	1·8	7·8	3428
8	" " "	Lieut. Leach	" " "	1·169	B.	0·7	17·8	8814
9	Sept. 12, '77.	Lieut. Griffin	200 lbs. dynm.	1·340	A.	1·05	8·8	6730
10	" " "	Lieut. Leach	" " "	1·340	B.	0·81	17·1	8730
11	" " "	Lieut. Griffin	70 lbs. powder.	1·340	A.	1·27	4·8	5559
12	" " "	Lieut. Leach	" " "	1·340	B.	0·84	15·1	8415

Mr. Mallet's results were reported many years ago to the Royal Society, and unquestionably are to be accepted as exhibiting the velocities due to his initial shocks and given by the method which he employed. Unfortunately, no full description of this method, and especially no record of the optical power of the instrument, is contained in his paper to the Royal Society; and my results, as will soon appear, prove the latter to be of unexpected importance. The following are his figures:

Velocity in ft. per second in sand	825 ft.
" " " in discontinuous and much shattered granite	1306 ft.
" " " in more solid granite	1665 ft.
" " " in quarries at Holyhead (mean)	1320 ft.

The extraordinary differences between these rates and those measured at the Hallet's Point explosion, and the apparent discrepancies of the latter among themselves, led me to so plan the new observations as to throw light upon two points: 1st, Does a telescope of high power detect a tremor in the mercury in advance of the one first revealed by a lower power? 2d, Is

there a difference in the rate of transmission of the shocks, due to differences in the intensity of the initial explosion?

The method adopted was to station two observers near each other at a carefully selected inland position; each observing a mercury seismometer, and holding in his hand the key of an accurate Morse register to record the instant of arrival and the duration of the tremor. These seismometers were the same instruments used at the Hallet's Point explosion. They differed from each other only in the optical power of the telescope, that designated A in the table having a magnifying power of 6, and that marked B of 12.

A fuse in the electrical circuit which fired the torpedo, was bedded in the cartridge of a field gun directed toward the observers; and, as a very powerful battery was used, the two explosions were absolutely simultaneous. A third observer, holding in his hand a key which closed the circuit of both registers, observed the muzzle of this gun with a good telescope, and recorded the flash electrically with precision. The observers were all officers of the Corps of Engineers accustomed to delicate observations. The distance traversed by the shock was measured by triangulation.

The observations of September 6th and September 12th, 1877, marked 7 to 12, and sufficiently explained in the foregoing table, answered the query relative to optical power decisively in the affirmative; and, as may be seen from the table, the earlier observations singly and collectively suggest the same conclusion. The mean velocity given by the six observations with type A is 4225 feet per second, while that given by the same number with type B is 7475 feet per second. There can, therefore, be no doubt that the first tremor of the mercury is of too feeble intensity to be detected with a magnifying power of 6. Hence, as the first tremor determines the true rate of transmission of the shock, the rejection of all the observations made with type A follows as a matter of course. They are valuable as exhibiting the rate of advance of waves having a certain intensity, but they do not reveal the velocity of the leading tremor. Indeed it is not impossible that a power above 12 might have detected a still earlier tremor; or, in other words, have indicated velocities of transmission exceeding those in the table.

These conclusions are so novel and important that certain additional evidence will be given, furnished by the notes of the observers recorded at the time and without consultation.

Lieut. Leach, using a power of 12, recorded of the second observation of September 12th (No. 12), "The gunpowder wave was peculiar, in having a much more gradual increase than has been observed in dynamite shocks. I should say it was at least two seconds in attaining a maximum, whereas the dynam-

ite usually reaches its maximum in a very small fraction of a second."

Lieut. Griffin, who used a power of 6, recorded of the same shock (No. 11), "The gunpowder disturbance remained constant for a perceptible interval, instead of immediately decreasing as is the case with dynamite."

It will be noticed that the shock of October 10th, 1876 (No. 6), was caused by the explosion of seventy pounds of gunpowder—the same charge which was fired on September 12th, 1877, and to which these records refer—but that the duration of the tremor was much less. The reason is, that the initial earth waves were far more violent in the latter case, when the torpedo lay on the bottom in thirty feet of water, than in the former, when the charge was only submerged five feet in water thirteen feet deep, and thus expended much of its energy in throwing a huge jet of water 330 feet into the air.

Thus it will be seen that these records, and the velocities observed on the two days, all tend to confirm the idea that a slow-burning explosive, like gunpowder, generates a series of gradually increasing tremors which, at a distance of a mile, are at first quite invisible with the less sensitive seismometer; and are only detected by it when near their maximum intensity. If Lieut. Leach's estimate of time for the arrival of the maximum wave be accepted, we have, therefore, for the first mile:

Deep torpedo (70 lbs.)	{	Power of 12 gives 8415 feet per second.					
		"	"	6	"	5559	" " "
Shallow torpedo; (70 lbs.)	{	Estimated minimum power gives					
		actual minimum power gives					
						2489	" " "
						1240	" " "

Without wishing to attach undue weight to these records, they appear to me to suggest a possible explanation of the discrepancies between the results obtained by Mr. Mallet and myself, provided his magnifying power was low. It would be very interesting to know the actual power employed.

As to the second query—Whether the rate varies with the intensity of the initial shock, the data, although less decisive, certainly suggest an affirmative answer. The evidence afforded by No. 6 and No. 11 has been already pointed out. For a power of 12, the table shows that for the first mile:

400 lbs. of dynamite	give	8814 feet per second.
200 " " " "	"	8730 " " "
70 " " powder (deep)	"	8415 " " "

If it be admitted also that the velocity of the wave diminishes with its advance, all the data become accordant. Thus:

200 lbs. of dynamite give for 1 mile, 8730 ft. per second.									
"	"	"	"	"	5	"	5280	"	"
50,000	"	"	"	"	8	"	8300	"	"
"	"	"	"	"	13½	"	5300	"	"

In fine, I believe that the following general conclusions are suggested by these measurements: 1st, A high magnifying power of telescope is essential in seismometric observations. 2d, The more violent the initial shock the higher is the velocity of transmission. 3d, This velocity diminishes as the general wave advances. 4th, The movements of the earth's crust are complex, consisting of many short waves first increasing and then decreasing in amplitude; and, with a detonating explosive, the interval between the first wave and the maximum wave, at any station, is shorter than with a slow-burning explosive.

A summary of the objections suggested by Mr. Mallet to the Willet's Point observations will now be given; although, in the new light thrown upon the subject by these subsequent measurements, perhaps he might not desire to press them.

Mr. Mallet considers that the explosion at Hallet's Point furnished data ill suited to determine the delicate physical problem of the rate of progression of a wave of disturbance through the earth's crust, for the reason that the initial impulse was due to the combined effect of many small charges, and not of one single mass of dynamite; hence, as he states:

"I conceive it, therefore, impossible with sufficient exactitude to assign the instant at which the wave of shock started in this instance; and to assume the appearance of disturbed water above the seat of explosion, or the shock felt from the ground closely adjacent to it, as marking that instant seems to me, in either choice, to neglect many sources of error."

Being unable to read my paper personally, he has not fully understood the manner in which the instant of the starting of the wave was fixed. The great mine was fired by a mechanical drop, which closed simultaneously all the twenty-three electrical circuits. One extra pin and mercury cup in this drop was provided by General Newton for my use, and the signal was thus automatically sent over the telegraph line to the several stations. Human judgment had no part to perform. The absolute instant of closing the battery circuits was electrically recorded on all the register papers. It is impossible that this could have *followed* the explosion; and if it preceded it, by a differential of time, the effect upon the records would be to indicate *too slow* and not *too fast* a rate, and thus to bring my results more nearly into accordance with those of Mr. Mallet.

Mr. Mallet next argues that the discrepancies of the four records among themselves invalidate their accuracy. I have just shown how these apparent discrepancies are explained by

the later observations at Willet's Point; and have thus answered this argument which, when the results were first collated, suggested itself to my own mind.

Mr. Mallet next mentions certain objections that show him not quite to understand the geography and geology of the region separating Hallet's Point from the four stations; which, in the absence of a map to illustrate the paper, is very natural. The straight line to Willet's Point follows the shore and waters of East River, offering both a land and water route to the wave of disturbance. The other three stations lie in the interior of Long Island, with no water between them and Hallet's Point. The intermediate country consists of low rolling hills formed of deposits of the clay, sand and boulders characteristic of drift, the geological formation to which they belong. The stations were carefully selected to detect, if possible, what influence is exerted upon the rate by intermediate sheets of water; and the fact that the wave reached Willet's Point with a higher velocity than the inland stations is, perhaps, confirmatory of the idea that water is a better conductor than land.

Mr. Mallet refers, as causes of uncertainty, to three sets of waves generated: (1) by the explosion, (2) by the fall of the rock, (3) by the fall of the water; and also to the wave-diffusion experienced in traversing long distances. He argues, therefore, that the distances were too great for satisfactory observations. Now the records show both the beginning and end of the mercury vibration, as well as the automatic signal sent by the explosion. The first and last determine the velocity of the advanced wave, which is that given in the table; the second and last approximately fix the time of arrival of the last wave, and enable its velocity to be roughly estimated. The physical phenomena following so exceptionally large an initial shock are thus made a matter of record. As to the element of distance, it seems to me that the reduced probable error in the measurement of the time of transit, should give the preference to long over short routes, when the velocity is so high as that under consideration.

As to the precision with which these distances were determined, Mr. Mallet expresses doubt; but since they depend upon the exact triangulation of the Coast Survey, supplemented by careful railroad surveys, no scientific man who is familiar with the geodesy of this region will hesitate for an instant to give them full credence. The probable error due to length of route, as compared with that due to any possible measurement of so short a time, is practically zero.

Mr. Mallet suggests that a measurement of the personal equation of the observers should have been made. This was not done, because absolute, not differential times were to be ob-

served ; and no absolute personal equation machine was available. The officers were trained observers ; and their distances from Hallet's Point were so considerable, that a small personal equation would probably exert no material influence on the relative velocity over the different routes.

In fine, my own opinion is that in this, as in all physical investigations, the more thoroughly the ground is covered the greater will be the number of important facts developed ; and that complex rather than simple phenomena should be expected. It is quite impossible to ascribe the discrepancy between Mr. Mallet's results and those here described to inaccuracy of observation. Differences in the material traversed by the waves, and in the method of observing, may possibly explain them. If Mr. Mallet's health will permit him to publish the details of his mode of observing, and to give the whole subject a general discussion, the paper will certainly find interested readers.

Willet's Point, N. Y. Harbor, Jan. 14, 1878.