photographer; this can be done in ten seconds by using strong solutions. It is handed over to a computer, who reads the time intervals and plots the result, and the location of the battery is telephoned to all interested. The time taken to work out a result is generally from four to ten minutes after the battery fires. The location is first telephoned to the artillery, in order that immediate action may be taken if desired. The neighbouring sections and other units engaged in location are then informed in order that results may be compared. At the end of the day the section sends in a full report of the day's work, and this is used by the compilation staff employed in estimating the positions and strength of the enemy artillery in any particular sector.

In 1917 and 1918 there were about thirty sections on the Western front, each section having four officers and forty men. The average number of locations obtained per day by each section was about five, though on a day when conditions were particularly favourable it was not uncommon for a section to get thirty, forty, or even more locations. Long spells of westerly weather were responsible for keeping the average number so low, because it was found impossible to "sound-range" in a wind blowing from the base towards the enemy guns. The sound is deflected upwards in the well-known manner, and fails to be recorded by the most sensitive microphone.

The accuracy of the results was tested in many ways. After a successful advance it was possible to examine the positions the enemy batteries had occupied and compare them with the locations. When this could not be done, an examination of aeroplane photographs generally revealed the gun pits, when sound ranging or other methods of location had indicated the approximate battery position. The average error of location, at a range of 10,000 yards, was about fifty yards, though naturally the conditions under which the section was working affected the accuracy greatly. Whenever possible, the aeroplane photograph was relied on to give the exact battery position. The sound-ranging results were especially valuable, however, in that they gave not only the approximate location of the battery, but also its calibre and the target at which it was firing. The shell-burst was recorded as well as the gun report, and so the time of flight of the shell could be found. The character of the report was a clue to the calibre of the piece. The area shelled could be examined to find the shell fragments, and there were other clues to the calibre which made it possible for sound ranging to give very full information about any battery recorded, and this greatly enhanced the value of the locations.

The most serious of the difficulties encountered by the sections were: Confusion between the gun report and the shell-wave which precedes it in the case of a high-velocity shell; inaccuracy caused by ignorance as to the effect of wind and temperature on the sound-wave; interruption by the noise of our own artillery and the enemy batteries; cutting of the lines by shell fire and traffic or by enthusiasts of other units collecting cable of a very useful type; the difficulty of survey of the microphone positions in a country where all landmarks were destroyed; and in the final stages of the war the problem of transporting and installing the section quickly when the line moved every few days. Experience solved these difficulties one by one, and towards the end of the war the sections reached a high state of efficiency, though the limit of development had by no means been attained, and it is certain that they might have played an even greater part than they did in the final struggle.

The British system of sound ranging, founded on the Bull recording apparatus, was developed entirely by officers of sound-ranging sections working at the front. The original experimental section was installed on Kemmel Hill, south of Ypres, and its researches were carried out there. Later, when there were sections along the whole front, it was arranged that an officer from each section should attend a conference which was held every two months. At the conference, proposed improvements were gone into, the equipment was discussed, results were compared, and the report of the discussion was submitted to General Headquarters. This informal conference did more than anything else to improve the work of the sections—it stimulated rivalry and ensured that all proposed alterations in the existing methods were subjected to severest criticism by men who had first-hand experience before they were adopted or turned down. The officers were for the greater part university men who had had a scientific training, and it would not be possible to imagine a more keen and enthusiastic body of men. They were sorely tried in the early days of sound ranging, when they worked under great difficulties, and had yet to prove that reliance could be placed on their results; but they were amply repaid when sound ranging came to its own at the end of the war, and was recognised as one of our most valuable means of locating the enemy's batteries.

RESULTS OF THE TOTAL SOLAR ECLIPSE OF MAY 29 AND THE RELATIVITY THEORY.

THE results obtained at the total solar eclipse of May 29 last were reported at a joint meeting of the Royal and the Royal Astronomical Societies, held on November 6. The stations occupied were Sobral, in North Brazil, and Principe Island. Two cameras were employed at Sobral, the 13-in. objective of the Greenwich astrographic equatorial, and a 4-in. lens, of 19-ft. focus, lent, together with an 8-in. cœlostat, by the Royal Irish Academy. It was realised, before the expedition started, that the cœlostat was scarcely suitable for observations of such extreme precision as were required to detect and measure the small shift in the places of the stars that might be produced by the sun's attraction. War conditions, however, made it impossible to construct a suitable equatorial mounting, though it is hoped that this may be done before the eclipse of 1922.

The results, to some extent, but, fortunately, not entirely, justified these apprehensions. The eclipse plates taken with the 13-in. (stopped down to 8 in.) are out of focus. Since the focus was good on photographs taken at night a few hours earlier, and also on the check plates taken before sunrise in July, the explanation appears to be a change of figure of the cœlostat mirror, due to the heat of the sun. These plates were compared with the July check plates by using a duplex micrometer. They show an undoubted gravitational shift, the amount at the sun's limb being 0·93″ or 0·99″, according to two different methods of treatment. The probable error, as estimated by the individual discordances, is about 0·3″, but there is reason to suspect systematic error, owing to the very different character of the star-images on the eclipse and check plates. This instrument supports the Newtonian shift, the amount of which is 0·87″ at the limb. There is one mode of treatment by which the result comes out in better accord with those of the other instruments. Making the assumption that the bad focus did not alter the scale, and deducing this from the July plates, the value of the shift becomes 1·52″.

The results with the 4-in. lens are much more satisfactory. The star-images are well defined, and their character is the same on the eclipse and check plates. As the duplex micrometer would not fit these plates, a key-plate, on which the film was placed away from the lens, was taken in July, and all the plates in turn were placed in contact with this plate and compared with it. The resulting shift at the limb is 1·98″, with a probable error of 0·12″. The values from the separate stars are in good accord, and they support the fact of the shift varying inversely as the distance from the sun's centre; they are thus unfavourable to its being due to refraction, as was suggested by Prof. Newall at the meeting. Moreover, Prof. Lindemann pointed out that the comets of 1880 and 1882 had traversed this region without giving the slightest evidence of having encountered resistance; as their speed was about 300 miles per second, a vivid idea is given of the extreme tenuity of any medium that they encountered.

The Principe expedition was less fortunate in the matter of weather, but a few plates showed five stars. Since no check plates of the eclipse field could be taken there, another field near Arcturus was photographed, and both it and the eclipse plates were compared with plates of the same fields taken at Oxford with the same object-glass. It was, moreover, necessary to assume that the scale of the eclipse plates was the same as that of the check plate. This is justified by the fact that the diurnal variation of temperature in Principe is only some 4° F., and that there had been no bright sunshine on the mirror before totality. The measures indicate a shift at the limb of 1·60″, with a probable error of 0·3″.

It will be seen that the mean of this result and that with the 4-in. at Sobral agrees very closely with Einstein's predicted value 1·75″. It was generally acknowledged at the meeting that this agreement, combined with the explanation of the motion of the perihelion of Mercury, went far to establish his theory as an objective reality. Sir J. J. Thomson, who presided, spoke of the verification as epoch-making; he suggested that it would probably have a bearing on electrical theory, but he regretted the very complicated form in which Einstein expressed his theory, and hoped that it might be possible to put it into a form in which it would be more generally comprehensible and useful.

Dr. Silberstein laid great stress on the failure to confirm Einstein's third prediction, that of the displacement of lines in the sun's spectrum towards the red, to the amount of 1/20 Ångström unit; this had not been verified, in spite of the careful search made by Dr. St. John and Mr. Evershed. As the probable error of their measures was much less than the quantity predicted, he looked on this result as final; some people had suggested that the shift might be veiled by a systematic outward movement of the photosphere, but as Dr. St. John made measures both at the sun's centre and limbs, that suggestion was not tenable. Prof. Eddington admitted that the failure threw doubt on the validity of some of the steps which led Einstein to his gravitational result; but he contended that the two other successes indicated that the result was right, even if reached by a wrong method.

There was some discussion on Prof. Lindemann's method of photographing stars in daylight by the use of red screens. However, the eclipse method seems more trustworthy, and the Astronomer Royal expressed the hope that the eclipse of 1922 might be observed with equatorials. The star-field is not so rich as in the late eclipse, but with longer exposure much fainter stars could be recorded. The eclipse-track crosses the Maldive Islands and Australia, and is therefore fairly accessible.

A. C. D. CROMMELIN.

THE JUBILEE OF "NATURE": CONGRATULATORY MESSAGES.

IT is with a certain amount of diffidence that we give here a number of cordial messages which have reached us upon the attainment of the fiftieth anniversary of the foundation of NATURE. We believe, however, that many readers will be interested not only in the friendly greetings expressed in these messages, but also in the references to the work of science, and its expanding field of usefulness. To the official representatives of scientific societies and university institutions, and to the other men of light and leading who have honoured us with their congratulations, we offer our sincerest thanks. Such appreciation of past efforts affords the strongest stimulus to future endeavour.

While NATURE has the advantage of the active