development in South-eastern Russia. Not only are there rich deposits known to exist in immediate proximity to the sea in the Transcaspian province, but an immense area of petroleum-producing strata extends from the Crimea to the Taman peninsula, and thence across the northern boundary of the Caucasian range to Petrovsk upon the Caspian, and many centres of production in these districts are now being opened up, which must shortly come into keen competition with the Baku industry. Already during many years oil has been extracted from borings in the Kouban district, whence by means of pipe-lines it is transported to a refinery at Novorossisk; but these will

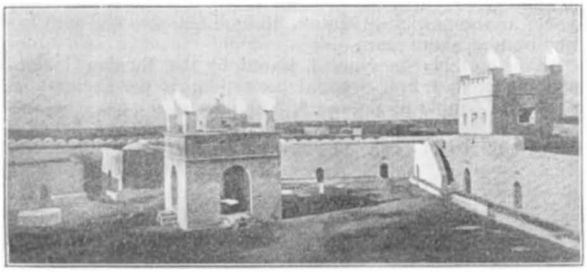

FIG. 3.—The Temple of Zoroaster.

be insignificant when (should all the reports be confirmed) the wells at Groznaia and its neighbourhood are tapped, it being considered that they will rival, if they do not surpass, Baku in productiveness. It would appear that the beds are almost identical in age to those of the Balachani-Sabountchi areas, and it would be an interesting subject for future study to ascertain if the line of petroleum productiveness to the north of the Caucasus follows that of the depression which in a former period connected the waters of the Caspian with those of the Azoff, the Black Sea, and the Mediterranean.

The accompanying illustrations are reproduced from an excellent series in *Globus*. W. F. HUME.

A SEISMIC SURVEY OF THE WORLD.

THE principal object of a seismic survey of the world is to measure the velocity with which earthquake motion is propagated through its crust, and possibly through its interior, and from the resulting figures to give to astronomers and physicists additional data respecting its effective rigidity.

It is the converse of the answer to a problem which in 1889 was incidentally worked out by Lord Kelvin, who, assuming a certain rigidity for our earth, determined the rate at which vibrations were likely to be transmitted through the same, the object of the calculation being to compare the result with that obtained from observations on an earthquake which in that year, originating in Japan, had been noted at many stations in Europe. The feasibility of the proposed undertaking and the probability of its yielding satisfactory results are based upon the existence of observations of the following nature.

For many years past astronomers, and those in charge of self-recording magnetographs, have observed disturbances in their instruments at varying intervals after the occurrence of an earthquake in some remote locality. In 1867, about seven minutes after an earthquake in Malta, M. Wagner observed at Pulkova an oscillation of 3″ in a level. One hour and fourteen minutes after the great earthquake of Iquique on May 10, 1877 (effects due to which were observed by the writer in Japan), at the same observatory M. Nyren noted oscillations in the bulb of a level of 2″ which had periods of 20 seconds. The late Dr. E. von Rebeur-Paschwitz repeatedly observed and obtained records of earthquakes which had their origin at distances equal to or more than one quarter of the earth's circumference from his observing stations. Vicentini, Agamennone, and others provided with instruments sensible to slight movements of the earth's crust, have made similar records; whilst the writer has not only shared in contributing to this class of observations, but on one occasion at least has obtained satisfactory photograms of a disturbance originating at his antipodes.

The conclusion which may be taken as well established by these observations is that suitable apparatus placed in any part of the globe will record the movements due to severe earthquakes originating in any other portion of our globe, and therefore there is nothing unreasonable in saying that every observatory throughout the world, if it were equipped with proper instruments, would be in a position to contribute to the knowledge of changes which are continually taking place, not only beneath the land, but also beneath the ocean.

For a person or a community to imagine that they reside in a locality free from earthquakes, is one of the greatest of modern fallacies. Although movements may not be felt, all places are disturbed in a manner capable of being recorded very many times per year. In addition to earthquakes the focus of which may have been some thousands of miles distant, to the recording of which the present note is intended to draw special attention, unfelt disturbances of a local origin may be recorded. Even at places where shocks are unknown, excepting as rare events recorded in ancient history, these may sometimes average two a day. Other movements taking place beneath our feet are slow diurnal tiltings, annual variations in the vertical, tremors of probably two distinct characters, earth pulsations and elastic vibrations.

To designate all these movements, which vary in their periods between the fraction of a second and twelve months, as "earth tremors," and an instrument to record them, a seismograph or a tromometer, are evidently misnomers. Although a single instrument may be obtained which will give information about each of these movements, experience has shown that it is better to have a particular instrument for a particular purpose. To record rapidly recurring vibrations the most sensitive arrangement that is self-recording is, perhaps, a Perry tromometer, which will detect the disturbance produced by a moving train at the distance of a mile. To record slight changes of level in a district, such, for instance, as may accompany changes in barometric pressure, a bifilar or horizontal pendulum, which is nearly as insensible to elastic tremors as a Perry tromometer is to change of level, would be best.

What is wanted for a seismic survey of the world is an instrument that is sensible to the preliminary elastic tremors of an earthquake, and then to the slowly recurring quasi-elastic gravitational waves by which these are followed. For this purpose it appears that our choice rests between some form of ordinary pendulum apparatus, like that of Agamennone or Vicentini, or some form of horizontal pendulum. Whatever form is selected, each instrument must be similar and similarly adjusted. If this is not the case, then at each station different instruments may commence to move with different phases of motion, and the records for purposes of comparison are without value. For example, an earthquake may originate at a known locality, at a known time, and be recorded at twenty different observatories in Europe, at each of which good time is kept, but at each of which the recording instruments are different in character.

The result of the calculations based on these observations have shown, in one instance at least, that the velocities of propagation of motion from the origin to each of these stations have varied between 2 and 20 km. per second.

The cause of this apparent discrepancy lies in the fact, that at different stations, during a disturbance having a duration of perhaps several hours, the different instruments have commenced to move with different phases of motion. This is a source of error which has been thoroughly recognised by observers in Japan for the last twenty years, and by timing the rate at which a particular vibration has travelled between given stations, the apparently conflicting results to which we are otherwise led have been greatly reduced.

When an earthquake is observed at stations far distant from each other, it is no longer possible to identify a particular vibration at these stations; but what can be done, is to note the time at which the preliminary vibrations commence and the interval which follows before the undulatory motions appear. So far as observations have gone, the velocity of propagation of the latter movements varies between 2 and 3 km. per second, which is about the rate we should anticipate to be found for motion passing through the materials constituting the earth's crust. The velocity for the former, however, appears to vary between wide limits, 10 or 12 km. per second being about the average. Because this rate of transmission is greater than that at which motion could pass through glass or steel, the inference is that it may possibly pass *through* our earth, and because it is variable the idea suggested is, that the rate of transmission varies with the depth of the wave-path. Should this be so, the next suggestion is, that as a wave proceeds downwards refraction may take place, and that a focal concentration of energy may be found at the antipodes of a seismic centre.

From these remarks it is clear that amongst the most important work with which the seismologist has now before him, is to measure the speed at which the preliminary vibrations of an earthquake are transmitted, and because this is high, the definition on the recording surface must be clear, the rate at which this is moved must be such that time intervals may be measured to within 10 seconds, and the observing stations, if they are limited in number, should be widely separated.

In the choice of stations, at all of which there must be the means of keeping fairly accurate time, the plan originally suggested by the author was to choose these relatively to districts where large earthquakes are frequent. The districts selected were the South American coast, Japan, and the Philippines, Himalaya and Central Asia. By a system of trials it was found that fifteen stations could be chosen, nearly all of which happen to be in the United States or British colonies, about ten of which would form a series approximately 2000 miles and multiples of 2000 miles distant from any of the three districts.

With a series of this description, data of a fairly complete nature respecting the rate at which motion may be transmitted round and, possibly, through our earth at varying depth should be obtainable. Any addition to this series would naturally render our information more certain, and add to the value of records obtained from centres other than those specified.

The cost of installation at each observatory would be approximately £50.

The proposal here made is similar to one published by the writer in January 1895, and does not materially differ from the one put forward by that distinguished investigator, the late Dr. E. von Rebeur-Paschwitz, and now being so warmly advocated by Dr. G. Gerland of Strassburg (see pp. 135, 136).

J. M.

THE SPECIFIC GRAVITY OF THE WATERS OF THE SEA.

IN continuation of his paper on oceanic circulation, in the concluding volume of the *Challenger* Reports, which chiefly dealt with the distribution of temperature, Dr. Buchan has published in the *Transactions* of the Royal Society of Edinburgh a series of maps showing, so far as the present state of knowledge permits, the specific gravity of the waters of the great oceans at various depths; and accompanying the maps is an extended discussion of some of the points treated in the previous memoir.

In the paper just published, Dr. Buchan has departed from the mode of representing salinities and specific gravities employed in the *Challenger* Report, and instead of charting the actual values, gives the departures above and below an average assumed to be a mean for all the oceans. It is difficult to see that anything is gained by this method; and it has the undoubted disadvantage that any future change in the assumed means will involve the reconstruction of all the maps. Even at the surface there are considerable portions of the sea of which we can only guess at the mean temperature and salinity, and the general average given by Dr. Buchan may therefore undergo modification, notwithstanding the attempts to apply a process of integration. Below the surface, the general average is simply the mean of existing observations; and while an inspection of the map shows that these are by no means perfect, the fact that there is only a single line of observations in the North Pacific, one in the Southern Ocean, none in the Atlantic north of 40°, and none in the Indian Ocean, indicates that the general averages must be mere approximations. Another unsatisfactory effect of the adoption of this method is due to the fact that values above and below the general average are thrown into strong contrast by being printed in different colours on the maps, thereby frequently exaggerating their apparent difference. In the case of the North Pacific, for example, Dr. Buchan lays great stress on the low specific gravity of the waters of this ocean at all depths. Undoubtedly the observations show that they are lighter than the Atlantic by a quantity amounting below the surface to about 0·0008; but the fact remains that a change of, say, ·0003 in the mean for the globe at 300 fathoms would throw the whole of the North Pacific *above* the average, while the observations within that area themselves show inconsistencies amounting to double that quantity. We draw special attention to this point, because it seems to lie at the root of a certain weakness in the line of argument taken up by Dr. Buchan, leading to a confusion of what we may call the statical and dynamical problems of ocean circulation, somewhat analogous to that involved in Ferrel's theory of cyclones. In drawing up any general scheme of the movements of oceanic waters, it is necessary to keep clearly in mind certain "conditions of continuity"; if the surface salinity is anywhere reduced by copious rainfall, it must somewhere else be correspondingly increased by evaporation; if reduced by melting of field ice, a corresponding quantity of salt must have been added to the deeper waters where the ice was formed; if up-welling is produced by an off-shore wind, the same force must be competent to cause a down-draught somewhere else. These considerations seem to suggest several simplifications in the scheme of circulation proposed by Dr. Buchan.

Comparing the Atlantic and Pacific Oceans, we find in the former a limited area subject to atmospheric systems of considerable intensity, the air over a considerable proportion of the surface being relatively dry. Over the Atlantic there is accordingly a relatively great amount of evaporation, producing high surface salinity, and the water carried off is distributed over an area nearly four times as large as in the case of the Pacific, allowing of its gradual return. In the Pacific, on the other hand, the winds are not so strong, the rate of evaporation is slower, and the redistribution of the moisture more local. It may therefore be possible to account for a considerable part of the low salinity of the Pacific without assuming that the high rainfall of the East Indian region produces effects so markedly in excess of those due to the immense discharges of fresh water into the Atlantic or the Indian Ocean. Dr. Schott's observations in East Indian waters support this view, indicating that the great freshening due to heavy rainfall is here, as elsewhere, largely restricted to the immediate neighbourhood of the land.

As an example of the converse of the foregoing, we may take the case of the undercurrent flowing from the Mediterranean to the Atlantic through the Straits of Gibraltar. From the observations of temperature and salinity Dr. Buchan regards it as placed "beyond dispute" that the warming effect of this outflowing water "becomes strikingly apparent at about 500 fathoms," and "beyond this depth its influence is felt over nearly the whole breadth of the Atlantic to at least about 1000 fathoms." Now at the Straits of Gibraltar the depth is something under 200 fathoms, and the extreme width at the surface a little greater than the Straits of Dover; and it is known that the loss by evaporation from the surface of the Mediterranean is not nearly compensated for by the fresh water additions from the rainfall and the rivers which empty themselves into its basin. The amount of water issuing into the Atlantic must therefore be greatly less than the amount entering the Mediterranean, and a comparison of the volumes and temperatures of the two bodies of water shows that it is almost impossible to give the outflow from the Mediterranean credit for such widespread action.

The two cases we have quoted, perhaps the strongest of several suggested by Dr. Buchan's papers, seem to support the results of a number of recent investigations, indicating that the effect of differences of specific gravity, while by no means a negligible quantity, is in general small compared to the dynamical effects due to the momentum of the surface currents, even at great depths.

From this point of view, we at once obtain help from the researches of Pettersson and Krümmel, noting specially their results as to the tendency of surface currents to induce *reaction* currents under them, and to divide on changing direction, and bearing in mind the deflecting influence of the earth's rotation at all depths. In the Atlantic, the water driven northward along the American coast is blocked by the land, and is partly drained off by the easterly drift currents, partly sent downwards in a column separated into two parts, at least in certain seasons, by a bulging out of the cold Labrador current. Crossing towards the west coast of Europe, the easterly drift divides, a part escaping northward under regulation of the polar streams from the east of Greenland and Iceland, and a part banking up against the French and Spanish coasts and the north-west of Africa, as is shown by the "bottle charts" of the Prince of Monaco and M. Hautreux. The shape of the coast prevents all the water escaping laterally, and a part descends, carrying with it the efflux from the Mediterranean.

In the Pacific the effect is similar, subject to the difference that while the circulation is less energetic, it is also less inter-