processes. They may be allowed to diffuse freely into one another, or may be separated by a porous partition. In the latter case a considerable difference of pressure may be produced between different parts of the space containing them, and this difference of pressure can be used to do work. The final condition in this case is, of course, cooler than if the gases did no external work. In the same way a solution diffusing into water may do so without doing external work, or it may do so by a reversible process, through a semi-permeable diaphragm, producing considerable differences of pressure, which may be used to do work. The final condition in this latter case would, of course, be cooler than in the former case of inter-diffusion without doing external work. Now whenever convection currents are produced, these are to some extent reversible. We might put vanes into the liquid to be moved by the currents and to do work outside the liquid, and by reversing this we would reverse the convection currents. Hence any method of mixing in which convection currents are produced, which do work or produce heat outside the liquid, will necessarily produce less heat in the liquid than a method of mixing in which there are either no convection currents, or these produce heat by viscous flow inside the liquid.

That we can, at pleasure, either use the diffusion of two gases into one another to do external work or not, is really not different from the case of a single gas expanding into a larger volume. We may do work by this expansion and cool the gas, or we may allow the gas, as in Joule's experiments, to expand into a larger volume without doing external work, and in this case there is only a very small change of temperature.

In these cases it is a question of change of entropy in the system, which can either be effected by an irreversible process in which no work is done, or by a variety of other processes, more or less reversible, in which the more reversible they are the more work can be done. In the case of producing convection currents, or, in general, of diffusion of a heavy fluid upwards into a lighter one, the amount of heat produced would not be exactly the same as if gravity were not acting: the centre of gravity of the system is raised by diffusion. Now in Mr. Griffiths's case, and in the case of diffusion currents generally, this raising of the centre of gravity takes place throughout part of the space considered by diffusion, and the centre of gravity is continually falling down again in the convection currents. Hence the work that can be done by the convection currents is part of the work that was done by diffusion against gravity. In the case of diffusion without convection currents, we might use the whole of this work done against gravity, by which the centre of gravity of the system has been raised, to do external work. If, for example, the containing vessel were supported at its centre of gravity, in the unmixed condition, the centre of gravity would, after diffusion, be above the point of support, and the vessel and its contents might be arranged to turn round the support doing work during the fall of the centre of gravity to its original level. Another way of utilising the rise is to allow the fluid to flow into another broader vessel until its centre of gravity has returned to the original level. The thing to be specially observed is, that the amount by which the centre of gravity is raised depends entirely upon the shape of the vessel. If it be tall, the centre of gravity will be raised a great deal; while if it be low, the centre of gravity will be only slightly raised. By causing diffusion to take place in a tall thin vessel, the final temperature will be lower than in a broad low one, not on account of any superficial tensions, but on account of the work done against gravity. In Mr. Griffiths's methods diffusion is continually taking place along tall thin vessels, and convection currents lowering the centre of gravity again by flow into broad ones. GEO. FRAS. FITZGERALD.

THE EXPECTED METEORIC SHOWER.

THE imminent return of the Leonids once more attracts us to prepare for their observation and discuss their phenomena. The circumstances this year will be much more favourable, all round, than they were in 1897, but our prospects of witnessing a really brilliant return appear to be somewhat slender. No doubt, on the morning of November 15, meteors will appear in sufficient abundance to gratify moderate expectation, but the conditions scarcely warrant the influence that we are to have a grand display. We must wait until 1899 or 1900 to see the shower at its best. In 1832 it is true Dawes saw many astonishingly fine meteors; and well he might, for the parent comet of the Leonids was very near that section of the orbit which the earth intersected in the year named. In 1865 we passed through a region of the stream some way in advance of the comet, for the latter arrived at its descending node about two months after the earth had crossed the point. There was nothing deserving the title of a great meteoric shower on that occasion. But there was certainly an unusual number of fine shooting-stars, the majority of the objects observed being as bright as, or brighter than, stars of the first magnitude. At Greenwich it was estimated that more than 1000 meteors must have been visible on the morning of November 13. Mr. Knott, observing at Cuckfield in Sussex, estimated the number as more than one per minute for two observers. According to some other accounts the richness of the display far exceeded this, for a captain of a British ship, near the West Indies, wrote to say that the heavens were in a blaze with shooting-stars from 8 p.m. on November 12 to 5 next morning. But accounts of the latter description are often exaggerated, and it is always unsafe to draw any definite conclusions from them.

At the approaching return the earth crosses the meteoric orbit still further in front of the comet than it did in 1865. In fact the comet will have five or six months' journey to run at its highest rate of speed before it reaches its descending node. This is not allowing for any perturbations which the comet has experienced since 1866, and there is no doubt that some serious disturbances have been introduced, particularly, by Saturn and Jupiter.

It seems that in July 1895, the comet approached to within 45 millions of miles of Saturn, and though the former has not passed so near as this to Jupiter, both planets have exercised a very appreciable influence both on the comet and its associated meteoric stream. Dr. Berberich gives these conclusions in an important paper published in *Ast. Nach.*, 3526, and states as a result of his investigation that the meteor shower will appear 21 hours late in 1898 and 26 hours behind time in 1899. The comet of Tempel (1866 I.) is not, according to Dr. Berberich, likely to be observed at the ensuing return to perihelion, as it will present itself under unfavourable conditions. Dr. Berberich's results are interesting as showing the necessity for expecting the meteors on the mornings of the 15th and 16th, rather than on earlier dates. His conclusions seem strengthened by the fact that last year a pretty strong shower of Leonids was witnessed just before sunrise on the morning of the 15th, whereas very few were seen on the previous morning.

Under all the circumstances a very rich shower can hardly be expected. Our historical records do not warrant the assumption that the section of the orbit in the van of the comet is thickly strewn with meteoric particles. In the comet's wake, for an enormous distance, the material appears to be densely distributed. This was sufficiently attested by the succession of three brilliant displays of 1866, 1867 and 1868.

Meteoric and cometary phenomena are, however, somewhat unstable in character, and certainly variable in their manifestations. They are quite capable of giving surprises. More meteors may now precede the cometary nucleus than the number there a generation ago, though the period is a comparatively short one, and comprises only one revolution of the swarm. There is one highly favouring circumstance this year, and that is the absence of moonlight. If the atmosphere is also free from cloud, the nights following the 14th and 15th will afford a splendid opportunity both for the visual observer and the photographic manipulator. I believe the night of the 14th will turn out the most productive, and especially the latter part of it forming the few hours before sunrise on the 15th.

Ordinary observers, while watching the meteors, will be usefully employed in determining, as accurately as possible, the time when the maximum in point of numbers is reached. The meteors should be counted at short intervals, and the horary rates of apparition during the night ascertained. The position of the radiant point is already well known; a mean of seventy values places it at R.A. 149° 28′, Dec. 22° 52′+, so that it is centrally within the curve of the "Sickle" of Leo, and close to the star *x* Leonis (Mag. 5·7) of Bode or Piazzi IX. 230.

It is especially to be hoped that attempts to obtain determinations of the radiant point by photography will be successful. The want of success in previous efforts has been very disappointing. Thus Mr. W. H. Pickering writes in *Popular Astronomy*, that on November 13, 1897, though he exposed eighty-one plates, only two meteor trails were secured. No doubt there are difficulties to be overcome; but as soon as the photographic method can be successfully utilised on a great meteoric shower, and a sufficient number of trails obtained to indicate a really good radiant, the visual method will have to be abandoned in its favour. It will be a long time hence, if ever, that the photographic plate will supersede the eye in ordinary meteoric observation; but in the case of a display such as the Leonids can furnish, the new method seems to promise well as regards the great accuracy of its records, though hitherto the latter have been exceedingly meagre.

W. F. DENNING.

MR. LATIMER CLARK, F.R.S.

ON Sunday, October 30, Mr. Latimer Clark, F.R.S., died very suddenly at his residence at Kensington, in his seventy-sixth year. His loss will be keenly felt by the various learned societies of which he was a member; especially by the Institution of Electrical Engineers, who claimed him as a founder and past-president. The name of Latimer Clark is familiar to all who during the past half-century have watched the various phases of progress in the science and practice of electrical engineering. Submarine cable engineers associate it with inventions that relate to every branch of their profession, from the process of sheathing the "core," to the last refinements of testing; and the constructors of land-lines still recognise the "Latimer Clark" double-bell insulator as a type universally accepted. His book, written in conjunction with the late Robert Sabine, on "Electrical Tables and Formulae," is to be found in every electrician's library, and in every cable-factory and telegraph testing-station in the world; his "approximate method" of fault-testing on submarine cables, by applying two successive potential differences, was an important step in the development of the modern empirical but nevertheless remarkably exact system of testing by two applications of different battery power; and his test of the electrical condition of "joints" in cable core is, under the name of "the accumulation method," still in daily use at cable works and on board ship. Another of his valuable contributions to telegraph progress is his study of the errors due to the inductive action of a galvanometer-needle upon its own coil when using shunts of different values, in a series of comparative "discharges." To this must be added his important modification of Poggendorff's method of comparing electro-motive forces, and the introduction, with this test, of the well-known potentiometer that bears his name. This instrument is perhaps associated in our minds rather with the laboratory than with the cable-testing room; and, moreover, it is here in the physical laboratory that we discover what is undoubtedly the best-known of Mr. Latimer Clark's inventions: the zinc-mercury standard cell. The vast amount of work that has been done, the modifications suggested, and the pages written in regard to this small apparatus, might well lead the uninitiated to suppose that it contains some potent *talia azman* to which electricians are for ever looking for revelation and mysteries. It happens to be merely the electricians' practical standard of potential-difference; but to those who care to study such things, it is still full of the mystery of the origin and meaning of contact electro-motive force.

The written and legendary history of the early days of electric telegraphs, over land and under sea, shows how closely Mr. Latimer Clark was associated with this work, both at home and abroad. Success did not always reward the efforts of the telegraph engineer, even in those times; for although commercial competition did not then exist to its present extent, there were all the difficulties of inexperience to be fought against. Success as regards the technical details of construction and working, came sooner than financial success. Estimating the cost of land-lines was beset with the almost insurmountable difficulties of transport and commissariat in countries savage and unexplored. Mr. Latimer Clark, in those pioneer days, was one upon whom the brunt of these reverses at first fell somewhat heavily. All honour to him and to his comrades; they fought for the greatest achievement in the world's history.

R. A.

THE TREASURERSHIP OF THE ROYAL SOCIETY.

IN the list of the proposed Council of the Royal Society for the ensuing year will be noticed a change in the Treasurership. Sir John Evans, K.C.B., retires, and the Council proposes to replace him by Mr. Kempe. Concerning this proposal the following letter has appeared in the *Times*:—

Sir,—The list of officers of the Royal Society proposed for election at the general meeting at the end of this month, published in the *Times* of Friday last, will not surprise any Fellow who is acquainted with the inner history of the society during the past few years, but in the change of *personnel* of the treasurership suggested it will astonish the great body of Fellows and may well arouse misgiving, if not anxiety, in the mind of the public—misgiving not to be lessened by the veiled *communiqué*, intended, apparently, to allay apprehension, which appeared in a certain section of the London press on Saturday.

The treasurer of the society is, like the two secretaries, a permanent officer, and these three officers have, therefore, a dominant influence in the affairs of the society, the treasurer having place by custom, at any rate next to the president.

Outside the society, too, in those responsible relationships with the public which the position of the society, as representative of science, engenders these permanent officials have a voice, consultative or executive, for the society. The choice, then, of treasurer is a matter of immediate moment to a wider circle than the Fellows of the society, and the nomination to the office by the present officers and council may therefore be fairly submitted for criticism in the *Times*. It is an open secret that an influential protest failed to arrest it.

Assuredly the roll of the society furnishes in abundance names of Fellows well tried in its work and veterans in the cause of science from which, as heretofore, a selection of treasurer could be made which would not only safeguard the interests of the society but also be a guarantee to the public that the best blood of the society was being devoted to the