A PARAGRAPH recently went the round of English and foreign papers and geographical journals, purporting to give the population of Japan according to a census taken in 1878. We have the best authority for stating that no census has been taken in Japan since 1875, and that the numbers given as for 1878 were really those of 1875.

NORDENSKJÖLD'S ARCTIC EXPEDITION

LETTERS have just come to hand from the Swedish North-east Passage Expedition in the neighbourhood of Behring Straits. The latest date is February 20, when all was as well as possible. We take the following details from Prof. Nordenskjöld's report, addressed to Mr. Oscar Dickson, of Gothenburg. The *Vega* and the *Lena* parted company on August 27 at the mouth of the River Lena, the former shaping her course for the New Siberian Islands. The air was calm, but for the most part overcast; the temperature as high as 4° C., and the sea free from ice. On the 28th Semenoffskj or Stolbovoj, the most western of the New Siberian Islands, was sighted, and on the 30th Liakhoff's Island, but a landing was not effected on account of the shallowness of the water in its vicinity. On the 31st Svjatoi Nos was passed without difficulty, the weather being fine, and the land in the neighbourhood free of snow. The water was slightly salt, and had a temperature rising to 4° C. The weather continued fine until September 1, the wind being southerly, and the temperature of the air in the shade 5·6° C. On the night before the second the wind became northerly, and the temperature fell to – 1° C. The following night there was a large fall of snow. Next day the Bear Islands were reached. Tschaun Bay was passed on the night before September 6, and Cape Schelagskoj reached by 4 A.M. The nights now began to be so dark, and the sea so filled with ice, that the *Vega* had to lie-to at night, generally anchored to a large ground ice. Two boats resembling the *umiaks* of the Eskimo were now seen filled with natives, the first that had been encountered since the expedition left Chabarova at Jugor Schar. They were received in a friendly way, but none of them could speak Russian or any other language intelligible to the Swedes. A boy could, however, count ten in English, showing that the intercourse with American whalers was greater than with Russian merchants. On September 6 and 7 the *Vega* steamed slowly along in a narrow open and ice-free channel along the coast. On the 8th a landing was effected near a Tchuktch encampment, where the Swedes were received in a very hospitable manner. They found in one tent reindeer flesh boiling in a large pot of cast iron. Another start was made on September 6, but a fog compelled the Swedes to lie-to till the 10th. Many excursions were made on land. The strand was formed of sand which, immediately above high water-mark, was covered with luxuriant turf. Farther inland, a range of very high hills was visible, and beyond that, at a considerable distance from the coast, snow-covered mountain-tops. The low land consists of sand and clay beds, evidently raised above the level of the sea very recently. No erratic blocks were to be seen, from the absence of which Nordenskjöld concludes that there is not at present to the north of this any such glacial land as Greenland. The rocks here were non-fossiliferous. Few land plants could be collected on account of the advanced season of the year, and in the sea Dr. Kjellman dredged for algæ in vain. On land many graves with burned bones were found. On the night before September 10 the sea was covered with a very thick crust of newly-frozen ice, but the *Vega* continued her course. On the 12th, after passing Irkaipi, or the North Cape, the vessel had to be anchored to a block of ice, where she lay till the 18th, when another advance was made. After lying-to from September 24 to 26, the *Vega* reached Cape Onman, and on the 27th Koljutschin Bay. The following day the cape to the east of this bay was passed, and the *Vega* lay-to, anchored to a ground ice, waiting for a favourable change, but no such change took place. Northerly winds heaped greater and greater masses of drift ice along the coast, and soon extinguished all hope of getting free before the summer of this year.

SIR THOMAS MACLEAR, F.R.S.

THE last Cape mail brought intelligence of the death of Sir Thomas Maclear, which took place at his residence, Mowbray, near Capetown, on July 14.

Sir Thomas Maclear was a son of the late Mr. James Maclear, of the County of Tyrone, and was educated at Winchester. He was originally destined for the medical profession, but, after settling at Biggleswade, we find him occupying himself in astronomical pursuits. He joined the Astronomical Society in 1828, and erected a small observatory at Biggleswade, which contained the Wollaston telescope, lent by the Society, with which he observed many occultations and other phenomena. He also engaged upon astronomical calculations, chiefly for the prediction of occultations. In conjunction with Henderson he computed the circumstances of the occultations of Aldebaran for ten European observatories in 1829–31, and himself calculated such of the occultations in 1833, about 100 in number, as were visible at Greenwich, for the supplement to the *Nautical Almanac* of that year. On Henderson's retirement from the direction of the Royal Observatory at the Cape of Good Hope, Maclear was appointed his successor, and entered upon the office in January 1834. Of the great number of observations made during his superintendence a portion only have as yet been published. He entered upon an undertaking of the importance of which there cannot be two opinions—the verification of Lacaille's arc of the meridian, but it was allowed to disorganise the regular work of the observatory to a serious extent. The observations by Maclear and his assistant in 1834 were speedily reduced and published, and various series of observations of comets when beyond reach at the northern observatories, have appeared in the *Memoirs* of the Royal Astronomical Society, where also have been published his determinations of the parallax of α and β Centauri, the latter of which had not been previously investigated, and there are memoirs on other subjects. The field work for the re-measurement of Lacaille's arc was completed in 1847, but from various delays the results were not published until 1866, when they appeared in two quarto volumes, under the editorship of Sir George Airy. The time occupied upon this work prevented the reduction and publication of the meridian observations; so that on Mr. Stone's arrival at the Cape in 1870 (as successor to Maclear on his retirement) he states he found himself "confronted with the results of thirty-six years of miscellaneous observing, in all stages of reduction."

Acting upon his official instructions Mr. Stone completed the reductions and published in several volumes the results of the observations with the new transit-circle from 1856 to 1860 inclusive; there remain still unpublished the observations from 1834 to 1855 with the old instruments, and those from 1861 to 1869 with the new one. Of the large number accumulated in the former period, the places of southern stars will still be of value for proper motions, but Mr. Stone has expressed a doubt whether "the immense number of observations of well-known stars" made with the old instruments would now repay the labour of reduction.

Maclear was knighted in 1860. He had been a Fellow of the Royal Society since 1831, and was elected a Correspondent of the Institute of France in 1863 in place of the American astronomer Bond; in 1867 the Lalande medal was awarded him by the Academy of Sciences, and in 1869 he received one of the Royal medals annually adjudged by the Royal Society.

We are informed that at Sir Thomas Maclear's funeral, on July 16, all the principal residents in the colony were present. The Cape Parliament has passed a resolution or memorandum acknowledging the work he did for the colony.

A POINT AFFECTING THE DIFFUSION OF THE GASES OF THE ATMOSPHERE IN RELATION TO HEALTH

THE great importance in relation to health of the part played by the internal motion of gases, as indicated by the now established and admirably simple kinetic theory, would seem scarcely to receive adequate appreciation. The old and vaguely developed statical idea of a stagnant atmosphere with molecules at rest, has given place to the opposite view of a high activity of motion, even when the atmosphere appears to the senses to be still. By this motion noxious vapours or gases, instead of remaining stagnant, are rapidly scattered by diffusion, and thereby rendered harmless. The part apparently played here by inequality of molecular *velocity* (dependent on inequality of molecular mass) in contributing to this end, would seem scarcely to have received the attention it appears to deserve. In Prof. Tait's work, "Lectures on some Recent Advances in Physical Science" (p. 237, second edition), reference is made to the diffusion of the gases of the atmosphere under the kinetic theory, and here it would seem as if the influence of the *inequality* of the normal velocity of the molecules of the different gases of the atmosphere (dependent on inequality of molecular mass) had not been taken into account, and hence it would appear as if the gases in their mutual diffusion were regarded as subject to the pure contingencies of chance, as they would be if the velocities of the molecules were equal (or their masses equal); this necessarily leading to some rather startling conclusions, which make the continuance of life and health (as dependent on the equable mixture of the constituents of the atmosphere) a matter more or less dependent on contingency or accident. The passage in question runs as follows:—

"There is another extremely important point of this statistical question as to the particles of gases which I must carefully explain; and it is this, how it happens that in the enormous bulk of the whole atmosphere of the earth these particles of oxygen and nitrogen, moving about amongst one another, should not by chance, at some place or other, operate on one another in such a way that in some particular cubic inch the particles of nitrogen might for a moment expel from it all the particles of oxygen, so that in virtue of the great extent of the earth's atmosphere, compared with the size of a particle of gas, there might be at some definite instant a region filled mainly with nitrogen, and other regions filled mainly with oxygen. Now the beauty of this statistical method is that it explains to us how such an event, though perfectly possible, can never occur. It is a thing which is itself absolutely possible, but it never can occur in practice, because the probability of its occurrence is so exceedingly small. There is a probability (numerically measurable) for everything which is possible, but if that probability (reckoned in numbers) is as small as the probability of the accident we are considering, we never expect to find it occur. And not only do we never expect to find it at any time, but we can say boldly from experience that it is never met with at all, however long our observations are conducted, or through however great an extent of space we conduct them. If you had originally in a box divided into two equal parts, nitrogen in the one part and oxygen in the other, and then allowed them to mix with one another, the probability that in any assigned time you could find all the nitrogen back again in the space where it was originally, and all the oxygen back again in the space where it was originally, is certainly one which can be measured, but it is one which is so infinitesimally small that we know perfectly by experience that it can never be realised."

The above appears a somewhat unsatisfactory conclusion to contemplate, and there would seem to be something scarcely consistent in the inference that an event which is itself absolutely possible never can occur in practice, "because the probability of its occurrence is so exceedingly small." For we know from the doctrine of probabilities that an event of chance (if possible at all) *must* occur, if the range of time be not restricted, or at least its probability approaches with *indefinite* closeness to absolute certainty in that case. That the probability, for example, of suffocation in a room [taking the above illustration of a box on a large scale] within a given range of time, by the oxygen separating itself sufficiently from the nitrogen, could be rigidly calculated, seems scarcely pleasant to contemplate, however remote the contingency might be, and it is hardly satisfactory to think that the contingency of such an event approaches with indefinite nearness to absolute certainty if an adequate *time* be conceded. The very fact that considering the vast extent of the atmosphere and the range of historic time, no record whatever exists of any irregularity having been detected in the constitution of the atmosphere, would surely be strong argument for the existence of some physical cause tending to prevent such irregularity from occurring, and removing it from the pure contingencies of chance. The above quotation that—"we can boldly say from experience that it [*i.e.* the irregularity] is never met with at all, however long our observations are conducted"—would surely tend to prove that some preventive means existed.

If the molecules of nitrogen and oxygen of a mass of air confined in a room were supposed subject to the pure contingencies of chance in their mutual actions in diffusion, they would be comparable to a number of equal perfectly elastic black and white balls imagined to be moving and colliding freely among themselves, or left to their own dynamics in an analogous manner. In this case there would evidently be practically an infinite number of chances against the molecules of the two gases (represented by the two differently coloured sets of balls) from becoming uniformly diffused through the room; indeed the probability of this event would be exactly the same as the probability of the oxygen being all separated in one part of the room and the nitrogen in the other (or in analogy all the black balls separated from the white); for we know that, according to the doctrine of probabilities, every *assigned* arrangement for all the balls is equally probable.

I venture to suggest that the inequality in the *mean velocity* of the molecules of the two gases (dependent on the inequality of the masses of the molecules) plays an important part here. If this particular point has been considered elsewhere (without my knowledge), I may still perhaps give an elementary analysis of the problem, as it has occurred to me. It may be remarked that on account of the simplicity of the kinetic theory, its problems frequently admit of elementary treatment, and it will at least be admitted that wherever this is practicable, perspicuity does not lose thereby. We will imagine for illustration a portion (say spherical shaped) of pure oxygen gas to be at a given instant of time surrounded by an atmosphere of hydrogen. [We may neglect the existence of gravity, for simplicity, as it does not affect the point with which we have to deal.] Then diffusion at once commences. The molecules of hydrogen which have one-sixteenth less mass, are known to possess a normal velocity four times that of the molecules of oxygen. The molecules of hydrogen by their own normal motion will therefore rush into this spherical space occupied by the oxygen, four times as fast as the molecules of oxygen can move out by their natural motion. Owing to this inequality in the rate of exchange of places of the two gases, the mass of gas occupying the spherical space will begin to increase in density, and (for