LETTERS TO THE EDITOR.

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Geological Survey of Canada.

IN the issue of NATURE of June 21 is a letter from Mr. A. P. Low. This communication is liable to be misleading, and I shall be greatly obliged if you will allow me to correct the misstatement which it contains, namely, that "at the same time, Dr. R. Bell simply returned to his former position of assistant-director and chief geologist, to which he had been appointed in 1892."

I was not appointed chief geologist in 1892. This office did not then exist. It was created on March 27 last, and I was appointed to it by a formal Order-in-Council on that date, a large increase being made to my salary at the same time.

ROBERT BELL.

Office of the Canadian High Commissioner, London,
July 9.

I HAVE taken some trouble to inquire into the extraordinary appointment to the Geological Survey of Canada concerning which you published a paragraph on April 26 (vol. lxxiii., p. 613) and a letter on June 21 (p. 175). I send you my results in case you would care to continue the correspondence.

Report states that the Premier informed Dr. Bell that the Government, for its own reasons, was going to make certain changes in the administration of the department, but that wishing Dr. Bell to be quite contented with these changes, he asked him to state the conditions which would be agreeable to him. I have also learned that the Premier transferred Dr. Bell's letter for action to the Minister of the Interior, who is at the head of the Geological Survey Department. Owing to the great pressure of the business of the session of Parliament, the matter has not yet been considered, and further changes are probable, but for the present Dr. Bell has been promoted to be chief geologist of Canada, and allowed to devote his time entirely to scientific matters. He attains at least equal rank, and receives a substantial addition to his salary, with a promise of further increase in the near future. In connection with the above change, Sir Wilfrid Laurier spoke in Parliament in the highest terms of Dr. Bell's ability and of the great scientific services he had already rendered the Dominion.

If these are facts, then Mr. Low's letter (p. 175) appears to be inaccurate. The office of chief geologist was, it seems, newly created for Dr. Bell last March, and he was not appointed to it, as Mr. Low asserts, in 1892.

Mr. Low, I find, is quite unknown in the geological world, whereas Dr. Robert Bell's name has long been familiar throughout Europe and America. He is now in his fiftieth year of service to the Government of Canada in connection with its Geological Survey, and as practical head of that department for the last five years he has maintained its high reputation and administered all its affairs with credit. He is a Fellow of the Royal Society of London, a Doctor of Science of Cambridge, a Doctor of Medicine of McGill, a Doctor of Laws, &c., and has been honoured by the King with the companionship of the Imperial Service Order.

During his administration of the business affairs of the Canadian Survey, it is generally recognised that he has improved its efficiency in many ways, and has increased the number of its officers, the extent of its operations, the Government grant, the library, the extent of its premises, &c. He has sent to the field an average of more than thirty parties every year, as compared with less than half that number in the time of his various predecessors. Surely this is a good record, for the sooner a country is surveyed the better it is for all economic purposes.

The above matters and many others are clearly described by Dr. Bell in his annual summary reports of the survey for the past five years. He had previously caused

to be carried on very extensive topographical surveys in all sections of the vast Dominion, taking the leading part himself in this work. It was for these valuable services to geography that the Royal Geographical Society this year awarded him the patron's gold medal, with the approval of the King.

It is clear, I think, that although the interests of science have not been wholly sacrificed, party politics and not geology have been in question in regard to Mr. Low's appointment.

F. R. S.

July 7.

Osmotic Pressure.

THE gravamen of our criticism of Prof. Kahlenberg's paper is directed against his statement that "indirect measurements of osmotic pressures involve the assumption that the gas laws hold for solutions." In vol. lxxvii., Proc. Roy. Soc., we deduce a relation between the osmotic and vapour pressures of a solution which is independent of the gas laws "holding for solutions." Prof. Kahlenberg, in his recent letter, does not attempt to show that this relation is unsound; we may therefore take it that he accepts the theory, but is dissatisfied with the experimental evidence which we adduced to corroborate it. Perhaps the following will help to convince him.

In a paper read before the Royal Society, June 7, we give the results of the direct and indirect measurements of the osmotic pressures of some aqueous solutions of cane sugar.

Concentration	Direct O.P. at 0° C.	Indirect O.P. (from V.P. at 0° C.)
540 grs. per lit. sol. ...	67.51 ...	69.4
660 " " ...	100.78 ...	101.9
750 " " ...	133.74 ...	136.0

Since reading this paper we have found that aqueous solutions of dextrose and galactose give similarly concordant results. As regards the last paragraph of Prof. Kahlenberg's letter (p. 222), we would point out that he gives no experimental evidence for the assumption that the sugar that had passed through the rubber membrane was sugar from which, so to speak, the solvent had been filtered off. Until such evidence is forthcoming, it seems to us that the criticism we levelled at his work is legitimate, and suggests a simple explanation of the low results he obtained.

BERKELEY.

Foxcombe, near Oxford.

E. G. J. HARTLEY.

Family Diseases and Temperaments.

MAY I appeal through your columns to those of your readers who are interested in the tendency of certain diseases and temperaments to run in particular families to aid me in an investigation I am at present making? The schedules now being issued contain space for a great deal of information, but it is rare for any single recorder to be able to supply all of it. What is wanted is a perfectly frank statement of what the recorder knows or can find out without much trouble. The only request made is that if the recorder feels unable to state certain facts not to the family credit, as well as those which indicate a sound, successful stock, no attempt should be made to fill in the schedule. At the same time, *no names are required*, the recorder may select any family he pleases for record, and the name of the recorder is only required in case it is needful to ask for explanation of any entry, and as a general sign of good faith.

I am fully aware of the labour involved in giving a fairly full family record, and my gratitude for aid in the matter is very great. At the same time, it is, I think, not unjustifiable to hope that among the readers of NATURE there will be some ready to help in an inquiry which if completed will be of considerable scientific value. There exists at present no ample data from which we can determine the inter-relationship of disease, temperament, and success in life. We know comparatively little the extent to which these factors are associated together or persist in certain families. After some considerable labour, about 200 records have been obtained, some of them very full and excellent, and the majority of considerable value. But the

number must be extended, if possible to 1000, before the work of reduction is begun. May I ask for further aid in the matter? I shall be glad to send two or more schedules to anyone who will help to get a faithful record.

KARL PEARSON.

University College, London, W.C.

Thermodynamics of Diffusion.

IN applying the principles of thermodynamics to diffusion of gases, several pitfalls have to be guarded against.

In the first place, if we adopt the old definition of entropy in terms of integrals of the form $\int dQ/T$, we are almost certain to go wrong when we come to deal with diffusion. If we imagine diffusion to take place between two of the ideal "perfect gases" of our text-books at constant pressure, volume and temperature, and without gain or loss of heat, no quantity of the nature of dQ appears to be associated with the phenomenon, and it is easy to rush to the conclusion that no change of entropy takes place. This danger is avoided if we adopt Mr. Swinburne's plan of defining entropy in terms of "waste" or unavailable energy relative to an assumed auxiliary medium. By "auxiliary medium" is here meant a medium at uniform temperature T_0 which can be used indefinitely as a refrigerator in thermodynamic operations, and any change in the amount of *unavailable* energy under such conditions, when divided by the temperature T_0 , gives the corresponding change of entropy.

If this definition is adopted we see that the phenomenon of mixing the gases does not in itself suffice to determine the changes of entropy associated with it. The matter can only be decided by an appeal to experience as to the means whereby the gases can be separated or mixed reversibly. The case of an ideal "perfect gas" forms no exception to this statement.

The proper inference is, not that the diffusion involves no change of entropy, but that the change of entropy, if it exist, cannot be expressed as a sum of differentials of the form dQ/T .

The second pitfall occurs when we take the well-known expression for the entropy of a perfect gas in terms of pressure (or volume) and temperature, and try by this means to connect the entropy of the mixture with the entropies of the components. Where we are likely to get into trouble is by ignoring the integration constants in the expressions for the entropy. There is no evidence from mere thermodynamic reasoning that the constant does not change in the process of diffusion. All we can infer is that the change of entropy associated with the mixing of gases at uniform pressure and temperature is constant, *i.e.* independent of pressure and temperature.

To sum up, then, even when we have defined an ideal perfect gas in the ordinary way, and assumed the property that two such gases can mix in a closed vessel without change of pressure and temperature, thermodynamical considerations still give us no information whatever as to the change of entropy accompanying diffusion, and on this point a further appeal to experience is necessary.

This amounts to saying that our definition of perfect gases is still incomplete. What further property shall we assume in order to complete it? If we regard a "perfect gas" as a mere invention on paper, the most *useful* plan is to take some simple property which is approximately satisfied in the case of actual gases and assume that this property is accurately satisfied by our perfect gases. Now, actual gases may be separated and re-mixed either by diffusion through a membrane or by liquefying, or, if preferred, freezing one of the constituents.

Taking either of these processes, and making suitable assumptions which would render that process perfectly reversible, we are led to the inference that the whole entropy of a mixture of perfect gases should be taken to be equal to the sum of the whole entropies of its components at the same temperature and partial pressure, *i.e.* if each component occupied the same volume as the final mixture.

According to this view, when diffusion takes place at

constant temperature and *pressure*, there is a gain of entropy and a loss of available energy equal in amount to that which would be incurred if each of the constituents were to expand by rushing into a vacuum until it occupied the same volume as the final mixture.

There is another way of partially separating the constituents of a gas mixture. If the mixture be introduced into a field of force such as that due to the earth's attraction, or if we imagine it to be whirled in a centrifuge, the denser gases will predominate in the lower parts of the atmosphere or where the potential is greatest, and the lighter gases will predominate in the upper regions or where the potential is least. In this case the partial separation is effected at the expense of work done by the field of force.

This note does not purport to deal in full detail with the thermodynamics of diffusion, but merely to direct attention to certain points which are easily overlooked. One of the most important of these points is that the possibility of producing mechanical work by the diffusion of gases through a membrane at constant temperature is not necessarily inconsistent with the principles of thermodynamics or the ordinary definitions of a perfect gas.

If any physicist should claim to have discovered Maxwell's demons in connection with the diffusion of gases, the first questions we should ask him are:—

(1) Can he, without the performance of external work, separate the gases in a mixture in such a way that the temperature is the same at the end as at the beginning, and the separated constituents each occupy volumes smaller than that of the original mixture?

(2) Can he obtain external work by the mixing of two gases without change of temperature if the initial volume of each gas is not less than the final volume of the mixture?

(3) Are his claims based on new experimental evidence?

G. H. BRYAN.

Early Meteors of the Perseid Shower.

THE moon being new on July 21 this year renders the conditions favourable for observing the earlier members of the great Perseid display. A few of these are usually visible on July 15, and probably just before that night, and it would be interesting if multiple observations of supposed Perseids could be obtained so that their radiants might be definitely assigned without the risk of error.

A single record of a meteor-flight only permits an assumption to be made as to the apparent radiant, and mistakes frequently result. For example, if a streak-leaving meteor, seen at the July–August epoch, happens to be directed from the northern part of Perseus it will certainly be attributed to the Perseid swarm, though it may quite possibly have had its origin in a different shower from Cassiopeia, Andromeda, Aries, Camelopardus or Auriga. To avoid such errors of allocation it is proposed to maintain simultaneous watches this year between July 15 and 28 from 10 to 12 p.m., and the writer would be glad to hear particulars of any observations for comparison with similar results obtained at Bristol.

The mean height of the Perseid meteors has already been satisfactorily deduced, but it seems desirable further to investigate the position and motion of the radiant, especially during the last half of July. Such meteors as appear amongst the stars of Perseus or bordering constellations are the best for indicating the exact place of the radiant, and bright meteors should always be carefully registered, as they are very likely to have been noticed elsewhere. The centre of radiation travels from near ϕ Andromedæ at the middle of July to a few degrees south of the star-cluster at χ Persei at the end, the ephemeris places (*Monthly Notices*, lxii., 169) being as under:—

Date			R.A.	Dec.	Date			R.A.	Dec.
July 15	...	...	15°3'	+48°9'	July 25	...	...	24°9'	+52°5'
" 17	...	...	17°1'	+49°7'	" 27	...	...	27°1'	+53°2'
" 19	...	...	18°0'	+50°5'	" 29	...	...	29°3'	+53°8'
" 21	...	...	20°8'	+51°1'	" 31	...	...	31°6'	+54°4'
" 23	...	...	22°8'	+51°8'	Aug. 2	...	...	33°9'	+55°0'

Bishopston, Bristol.

W. F. DENNING.