which this result is stated in "Thermodynamics" (§§ 124, 126, 156)?

It cannot be deduced from the laws of thermodynamics or the definitions of a perfect gas (§ 124). These leave the change of entropy in the form of an undetermined constant.

It must necessarily be based entirely on experimental evidence (§§ 126, 156). It is in all probability *approximately* true for actual gases, but of this the experimental physicist is the only competent judge. As applied to "perfect gases" it should be regarded, in common with Boyle's law, as one of the "definitions of a perfect gas," a definition selected partly on account of its simplicity and partly on account of its approximate agreement with the properties of actual gases (§ 156).

An irreversible transformation does not, *ipso facto*, imply a gain of entropy. Unless a compensating transformation exists (§§ 50, 51), and unless the final result involves nothing more than a loss of available energy, we have no justification for applying the methods of thermodynamic analysis. If diffused gases could never be separated, we should have an instance in point; but do such exceptions exist?

Mr. Burbury asks why should different gases behave differently from different portions of the same gas? This question must be decided by the experimental physicist, subject to some further condition, *e.g.* that the gases are in the presence of a liquid which dissolves one of them or of a membrane which is permeable to one of them only. In other words, the matter resolves itself into the question, Why should the conditions of equilibrium of a gas in such circumstances depend on its *partial* pressure instead of on the total pressure of the mixture?

If the experimental physicist had told me that the total pressure, and not the partial pressure, was the determining factor, I should have asserted that no entropy was gained by diffusion, and should have written zero as the value of my constant C.

But then we should have no vapour of water in our atmosphere unless the temperature rose above the boiling point of water. These, generally speaking, are the views which the book was intended to convey; but may I direct attention to the large number of open questions in thermodynamics that have hitherto only received scanty attention in the hands of mathematical physicists?

G. H. BRYAN.

IN the passage in my review to which Prof. Bryan takes exception I had in my mind his definition of available energy at p. 35 and p. 43:—"The available energy of a system *under given conditions* is the quantity of energy which *under these conditions* can be converted into work"; and in the same passage the conditions are also spoken of as "external" conditions. Let the system consist of two gases occupying equal halves of a cylinder, both at the same temperature and at pressure p, separated by a piston impervious to either, and the whole surrounded by air at the same pressure p. It seems to me to be impossible under those conditions to convert any of the energy of the system into work; but if it can be done, it must be possible to explain how. The context of p. 125 does not seem to me to explain it.

S. H. BURBURY.

The Nomenclature of Radio-activity.

THE name "ionium" which Dr. Boltwood proposes for the new radio-active element, of which he announces the discovery in NATURE of October 10, is open to serious objections. I do not mean merely linguistic objections—it is too late to consider them; beside such a hybrid as "ionisation" the philological barbarity suggested by Dr. Boltwood is insignificant; but it is a first principle of scientific nomenclature that a name should connote some of the distinctive properties of the thing named. A thoroughly satisfactory system for naming radio-active elements has not been put forward, but that adopted by Prof. Rutherford in designating the members of the series descended from radium is at least better than none.

According to this system, the products arising successively from the disintegration of a radio-active element are denoted by the name of that element followed by the letters X, A, B, C, &c. The principle of this plan has been adopted by universal consent in the nomenclature of the products of radium, thorium, and actinium, but for historical reasons slight divergences from the simplest form of the system have been permitted. Only one disintegration product of uranium (other than the radium series) has been known hitherto; its name, uranium X, is in accordance with Prof. Rutherford's nomenclature. Dr. Boltwood now announces the discovery of a descendant of uranium subsequent to uranium X; it appears to me desirable that this product should be known as uranium A, and should not be given any purely fanciful and meaningless name such as its discoverer suggests.

NORMAN R. CAMPBELL.

Trinity College, Cambridge, October 12.

On Correlation and the Methods of Modern Statistics.

IN my last letter (October 3, p. 566) I ventured to express the modest hope that "an astronomer may be permitted to dissent from these applications of modern statistical methods." Prof. Pearson refuses the desired permission with such warmth of language and wealth of argument that I find it difficult to make a suitable renewal of the request. Perhaps I may be allowed to confine my reply to the point of most general interest.

With regard to the supposed relation between magnitude and colour, Prof. Pearson wishes "to say a strong word" about my criticism of a conclusion respecting the bulk of the lucid stars, which I said was based on a record in which the white stars had no frequency. I have re-read Miss Gibson's paper, and am unable to see that my criticism in any way misrepresents the facts.

In section (3) of her paper Miss Gibson discusses the relation between magnitude and colour, basing her results upon the Cape list of 159 stars, from which all stars less coloured than deep yellow are excluded.

In section (4) she fits various types of frequency curve to statistics of counts of the lucid stars. In this part of the paper there is no mention whatever of colour or spectral type.

At the end of section (4) we have the conclusion, to which I ventured to take exception:—"Thus we have the suggestion, even if it be only of the vaguest kind, that the bulk of the lucid stars may belong to a separate universe within which magnitude is not mainly determined by parallax or distance, but is more closely associated with colour, and thus probably with chemical or physical condition." The phrase "*but is more closely associated with colour*" is undoubtedly there. If it does not arise from section (3), its origin is "wrop up in mystery"; if it does arise from section (3) my criticism was not so unjustifiable as Prof. Pearson would with strong words call upon the reader to believe.

ARTHUR R. HINKS.

Cambridge Observatory, October 18.

New Zealand Birds.

DURING the past twenty-five or thirty years many reports have been published in regard to the extinction of New Zealand birds, and an impression has gone abroad that our avifauna, with its striking peculiarities and its wealth of interest to ornithologists, will soon be lost. Some time ago, when I was inquiring into the results of the acclimatisation of English birds, I had thousands of circulars distributed in all parts of the colony, and on those circulars I placed questions dealing with the position of the native birds. When the circulars were returned to me I found that every native bird was accounted for, in some cases in many different districts.

I feel, therefore, that I am able to sound a brighter note than has been sounded by most writers on New Zealand ornithology. From personal observations, I can say that several species the extinction of which was announced twenty years ago are fairly plentiful, and are increasing. I may mention specially the stitch-bird (*Pogonornis cincta*), the bell-bird (*Anthornis melanura*), the North Island robin (*Miro australis*), and the tui (*Prosthemadera novae zealandiae*).

I do not know of a single New Zealand bird which we can say with any degree of certainty has become extinct since European occupation of the country, except perhaps