Science and Psychical Research.

MY departure for New Zealand having been delayed for a month, perhaps I may be allowed a short reply to Sir Bryan Donkin's remarks in NATURE of Nov. 6. Sir Bryan and myself have come much closer in our views, apparently, but he still misunderstands some of the main points both in my original article and following letter. The term 'supernormal phenomena' certainly includes both 'physical' and 'mental' phenomena of the type under discussion; on that we are agreed. But Sir Bryan goes on: "In the mental part, however, are included practically all the various 'phenomena' known generally under the term 'spiritualistic,'" or, later in his letter, 'ghostly.' It is here that I disagree. The genuine psychical researcher does not allow that these may be termed either, since both words connote a hypothetical explanation of the phenomena which we hold is not yet proven. Sir Bryan keeps trying to tie me down to an acceptance of the spiritistic hypothesis, whereas the whole of what I have written shows clearly that I am studying the evidence with an absolutely open mind. Another remark of his, "Seeing that the present discussion has been mainly concerned with these [*i.e.* the mental] phenomena," suggests that he cannot really have read carefully what I have written. Throughout, I have emphasised the importance of the *physical* phenomena, not the mental, and it was Sir Bryan himself who, by his narrowing of the field to the mental phenomena, attempted to deprive me of my chief argument.

As regards the subject of *trance*, Sir Bryan and I are alike in not knowing what it is. The only difference in our attitudes, I take it, is that I consider it a phenomenon worthy of scientific study, whereas Sir Bryan does not. I wonder whether Sir Bryan considers the phenomenon of *sleep* to be worthy of scientific study, or whether he would take the stand of the physicists who (mostly, but not all) maintain that the only phenomena which science may properly take account of are those which can be always repeated accurately under given experimental conditions. If I put Sir Bryan to bed and tell him to go to sleep, can he always do so? Is sleep any the less a fit subject for scientific study because it cannot be produced to order in an experiment? Trance, if anything, is more amenable to such procedure than is sleep. It is therefore undoubtedly a fit subject for scientific study. My complaint is that those who have the necessary knowledge and training refuse to become interested in these things; they seem to think that the year 1926 marks the culmination of human knowledge in some way, and that there is nothing more to learn about life except on the purely mechanistic side. My whole article was really a protest against this view.

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Active Nitrogen.

FURTHER experiments have tended to confirm the work recently published by Dr. Rideal and myself upon the energy and nature of active nitrogen.

A study of the effect of the admission to the afterglow of a number of gases, elementary, compound, and mixed, shows that in no case so far examined does any chemical action resulting in the formation of definite compounds occur when the critical increment of the gas introduced exceeds *c.* 45,000-50,000 cal./gm. mol.

Experiments upon the action of active nitrogen upon the metals in the form of fine filaments show that catalytic decay takes place at their surfaces and in some cases (*e.g.* copper) is very marked. The efficiencies of the metals in this respect depend upon the stability of their nitrides. Calculations from the data obtained here again show that the energy of active nitrogen is *c.* 45,000 cal./gm. mol.

The decay of the afterglow has been measured by optical methods, and it has been found that the process is in all probability bimolecular with respect to the active nitrogen, but termolecular in reality since the total pressure appears to exert a marked influence, as suspected by Lord Rayleigh.

The line of reasoning adopted by Dr. Ludlam and Mr. Easson is sensibly the same as that of Saha and Sur. It is quite probable that an unstable compound such as N_2I_2 or a quasi-molecule of sorts is first formed when the iodine vapour is admitted to the afterglow, but is then broken up by collision with another molecule of active nitrogen, the energy liberated going to produce the line they mention. The nitrogen halides are notoriously unstable compounds and hence probably highly endothermic.

An alternative explanation is suggested by the presence of the β and γ bands in the spectrum of the afterglow. While the weight of evidence appears to be against their being really part of the true nitrogen afterglow bands, they invariably appear when the gas is purified in the usual manner. Since they extend at least so far as $\lambda 2154$, collisions between molecules of iodine and those of nitrogen or nitric oxide at this level may result in the appearance of the line in question.

This explanation could easily be tested by increasing the intensity of the β and γ bands, by the addition of oxygen, as in the recent experiments of Johnson and Jenkins (*Phil. Mag.* Sept. 1926); an increase in the strength of this iodine line should then follow. It is surely not at all legitimate to attempt an evaluation of the energy of active nitrogen from the spectral phenomena to be seen in the presence of such factors of totally unknown magnitude.

E. J. B. WILLEY.

Laboratory of Physical Chemistry,
Cambridge, October 27.

Liveing's Fire-damp Indicator.

SOME years ago I made frequent use of Mr. Liveing's very ingenious instrument, referred to in NATURE of October 30, p. 626, and I found its indications to be both accurate and excellent. The only drawback to its use in those days was the shaking which accompanied the turning of the handle to produce the glow, as this made accurate observation of the percentage of fire-damp somewhat difficult.

W. GALLOWAY.

17 Park Place, Cardiff,
October 30.

The Imaginary Roots of Equations.

PROF. C. RUNGE has informed me that the method I gave under the above title in NATURE of October 30, p 627, has already been given by him in the "Encyclopädie der mathematischen Wissenschaften," vol. I. 1, p. 431. I need scarcely add that I was not aware that I had been anticipated when I sent my letter to NATURE.

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