

Correspondence

Apartheid for Whom?

SIR.—Your review (*Nature*, 227, 5; 1970) of my two papers in the *South African Journal of Science* (65, 329; 1969, and 66, 12; 1970) gives a false impression both as to the extent of duplication of research in South Africa and its cause. The question asked in the survey was, "Have you ever had the experience of discovering after you had completed a piece of research that someone else had already discovered and published substantially the same facts?" Although 17.5 per cent of the responses were affirmative, one is not justified in concluding that "nearly one South African scientist in five is duplicating research being carried out in other laboratories", or that this is a "feature of the isolation of South Africa". It is interesting to note that my figure for South African research workers is remarkably close to those obtained in similar surveys by Martyn¹ and Flowers² in the UK, Törnudd³ in Scandinavia and Glass and Norwood⁴ in the USA. Likewise the reasons for the failure of scientists to learn of other work in their fields in time to avoid duplication of research are much the same in those countries and are related rather to the literature searching habits of the scientists themselves than the political systems under which they work, as seems to be implied in your choice of headline.

Owing to a delay in the publication of my paper by the *S.A. Journal of Science*, the figures for the remuneration of research scientists are grossly out of date. I am happy to be able to report that currently the financial rewards of scientific research in South Africa are some 15 to 20 per cent better than those listed for various groups in my paper.

Yours faithfully,
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¹ Martyn, J., *New Scientist*, 21, 338 (1964).

² Advisory Council on Scientific Policy, *J. Doc.*, 21, 83 (1965).

³ Törnudd, E., *Proc. Intern. Conf. Scientific Information*, Washington DC, 19 (National Academy of Sciences/National Research Council, 1959).

⁴ Glass, B., and Norwood, S. H., *Proc. Intern. Conf. Scientific Information*, Washington DC, 195 (National Academy of Sciences/National Research Council, 1959).

Phytopathology in Brazil

SIR.—I read with great interest the short report on the occurrence of coffee rust in Brazil (*Nature*, 226, 997; 1970). I was also interested in the fact that many of the author's conclusions were based on observations made by Professor F. L. Wellman on the occasion of his recent visit to the rust-infected area.

I have known Professor Wellman for many years and also met him during his recent visit to Brazil, so I am sure he would be the first to agree with me about the need to add more details to the report and to give credit to a larger number of phytopathologists. He did not fail to give generous credit to others in another recent report¹.

The occurrence of the disease was observed by Brazilian phytopathologists, who also made a correct diagnosis of the causal agent. This diagnosis was later confirmed by other Brazilian colleagues. Among the Brazilian phytopathologists concerned with the problem I should mention Mr Arnaldo Gomes Medeiros (Centro de Pesquisas do Cacáo, Itabuna), Dr A. A. Bitancourt, and Miss Victoria Rossetti (Instituto Biológico, São Paulo) and Professor Charles F. Robbs (Universidade Federal Rural, Rio de Janeiro).

There is a relatively large number of phytopathologists working in Brazil, and they joined a few years ago to form the Brazilian Society of Phytopathology (Sociedade

Brasileira de Fitopatologia.) In this connexion it should be mentioned that the first foreign phytopathologist approached by the Brazilian authorities to take part in the planning of control measures was Professor A. Branquinho D'Oliveira (Centro de Pesquisas sobre ferrugem do Café, Oeiras, Lisboa, Portugal). Some phytopathologists from other countries, such as Dr E. Schieber from Guatemala, were also given the opportunity to study the problem *in loco*.

Yours faithfully,
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¹ Wellman, F. L., *Phytopathol. News*, 4 (6) (1970).

The Definition of Aggression

SIR.—In his article in *Nature* (227, 1006; 1970) reviewing trends in neuroscience, F. O. Schmitt considers the role which neurophysiological investigations may play in the understanding and control of social behaviour, and of aggression in particular. I do not wish to challenge the methodology nor the interpretation of studies on the implantation of electrodes, etc, nor do I wish here to question the ethics of such research on human subjects. What is of concern, however, is the apparent failure to define the behaviour which is being modified.

Schmitt states, for example, that "violent and aggressive behaviour is an all too prevalent manifestation of social imbalance in many parts of the world", and he later refers to "aggressive and other aberrant behaviour" (italics added). It is debatable to what extent any scientist can evaluate or initiate research in the context of the bias inherent in the view that aggressive behaviour is an aberrant form of behaviour and that aggression is too prevalent in the world.

Aggression is a term which can be used to cover a wide variety of forms of behaviour and to be of any value in a scientific context it must be defined in as neutral and precise a manner as possible. Any investigation of social behaviour has to identify the structure of the behaviour and the structure of the situation in which that behaviour is displayed. It may be that a definition of aggression will include reference to injury or harm to another person, but an analysis of other components in the sequence of behaviour is necessary and account must also be taken of the situation in which the behaviour is manifested. Schmitt's view that aggression is a "manifestation of social imbalance" has prejudged and loosely defined the determinants of the behaviour and has stated a hypothesis in the context of a set of values which prevents rather than facilitates the understanding of the behaviour and its causes. In social science research it seems important to make explicit those value premises which may bias the form of research and the interpretation and utilization of the results of research.

Yours faithfully,
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Theories of Electromagnetism

SIR.—McCaig gives a quite unwarranted impression of confusion in electromagnetism (*Nature*, 227, 935; 1970). It is not the case that the Kennelly and the Sommerfeld formulations lead to self-contradictory results. McCaig has misinterpreted the basic claims of the two theories. The field of a physical magnet depends on its shape, so the statements that the H-field of a magnetic dipole is inversely proportional to μ_r in the one system, and inde-