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 Martyn[1] and Flowers[2] in the UK, Törnudd[3] in Scandinavia and Glass and Norwood[4] 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of my paper by the *S.A. Journal of Science*, the figures for the remuneration of research scientists are grossly out of date.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 report[1].

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 sôbre ferrugem do Café, Oeiras, Lisbôa, 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 an 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 independent of μ_r in the other, are to be understood in reference to a particular shape. The early treatments of magnetism were based on the formulae for isolated poles, so when the same formulae are applied to magnets the shape assumed is that for which the magnet approximates to an ideal dipole, namely bar-shaped. The formulae must then only be used within their range of applicability. (For a magnet which is infinitesimally thin, the field at all points at a finite distance is indistinguishable from the field of a pair of isolated poles.) Kennelly is in this respect an MKS version of the traditional theory of electromagnetism, and his views presuppose the bar shape; Sommerfeld is explicit on the question of shape[1]. Thus it is incorrect to apply the relations uncritically to an ellipsoidal shape.

Both the traditional and the Sommerfeld systems are self-consistent, in magnetic media as well as vacuum. The two theories, however, make different statements about observable results; therefore they are not both consistent with the facts. The difference between them is of fact, not of arbitrary convention. The difference is not shown simply in the position of μ_r, for this difference can be accommodated by a different interpretation of magnetic moment. The difference in factual content comes when one asserts what magnetic quantities are constant. I gave the references previously[2].

An experiment has been performed[3], which decided in favour of the traditional view, and against the view now associated with the name of Sommerfeld; but it was not satisfactory and should be repeated. But in the meanwhile one can readily show that the factual falsity of Sommerfeld follows from the basic equations of magnetostatics, namely, div $\mathbf{B}=0$, curl $\mathbf{H}=0$, $\mathbf{B}=(\mathbf{H}+\mathbf{M})/\mu_0$, if one supposes that an ideal "hard" magnet is one in which magnetization $\mathbf{M}$ is unchanging. I propose publishing a fuller discussion on another occasion.

The clear understanding of electromagnetism is difficult; it is therefore important to eliminate mistakes as quickly as possible.

Yours faithfully,

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[1] Sommerfeld, A., *Lectures on Theoretical Physics: III, Electrodynamics*, 41, 87 (Academic Press, New York, 1952).
[2] Stopes-Roe, H. V., *Nature*, **224**, 579 (1969).
[3] Sargant, E. B., *Phil. Mag.*, **14**, 395 (1882).

Disputed Pronoun

SIR,—It is simple to make the disputed sentence make sense by re-arranging the clauses in their syntactical order of importance—a sequence used by every competent journalist in this country. Thus: "John was surprised to learn (that) he had won the race." Mis-related participles are unacceptable because of their lack of precision.

I must confess surprise that the medical and scientific communities, who put so much stress on publication, are unaware of this basic rule, which is, I admit, related in practice to the degree of concentration available for sentence construction.

What does genuinely worry me is that people who can not set down their thoughts clearly may not be capable of thus assembling them. I trust, to preserve our belief in the omniscience of the communities concerned, I am forthwith proved wrong.

Yours faithfully,

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Obituary

Professor W. O. Kermack

WILLIAM OGILVY KERMACK, who died on July 20, 1970, made distinguished contributions to chemistry, biochemistry and statistics. He was born in Kirriemuir, Angus, on April 26, 1898, attended Webster's Seminary, Kirriemuir, and graduated at the University of Aberdeen in 1918. The following year he joined Professor W. H. Perkin, jun., at the Dyson Perrins Laboratory, Oxford, and succeeded in synthesizing heterocyclic compounds related to the alkaloid harmaline. In 1921 he became head of the chemical research laboratory, Royal College of Physicians, Edinburgh, but a laboratory accident in June 1924 rendered him totally blind. Such a catastrophe, which would have ended most research careers, presented a challenge to him to seek a way of resuming his scientific life. This he was able to achieve by the careful design of experiments which were undertaken by others under his direction. He kept abreast of the literature by having selected articles read to him, sometimes by colleagues, but especially by his wife without whose help he could not have continued. By 1930, in addition to his work in synthetic organic chemistry, he had studied the mechanism of flocculation of colloidal solutions and investigated certain aspects of glyconeogenesis.

After 1930, his organic chemical interests led him to look for new antimalarials. Many new quinoline and acridine derivatives were synthesized and, later, some substituted pyridoacridines and phenanthrolines, a few of which had great chemotherapeutic potency. The decade 1930–1940 was also the period of his greatest activity in the sphere of medical statistics, where he made important contributions to the mathematical theory of epidemics. At the same time, he began to write review articles on recent advances in biochemistry. Over a period of twenty years, few aspects escaped his notice. In 1938, he collaborated with Dr Philip Eggleton in writing *The Stuff We're Made Of*. This layman's guide to the basic sciences on which biochemistry is founded is still an entertaining book.

In 1949 he was appointed to the Macleod Smith chair of biological chemistry in the University of Aberdeen and characteristically accepted the task of creating a new department. His teaching commitments, University administrative duties, committee work in connexion with local research institutes, and plans for a School of Biochemistry left little time for research, yet he managed to study several biochemical problems principally concerned with amino-acids and enzymology. By the time of his retirement in 1968, he had established an active and effective department of high reputation.

During his career, many honours came his way, including Fellowship of the Royal Society of Edinburgh (1924), honorary LlD of St Andrews University (1937) and Fellowship of the Royal Society (1944). His scientific knowledge was broadly based and the clarity of his thinking, the logic of his arguments and his understanding of first principles were widely acknowledged. His advice was always generously given to colleagues who brought their problems to him. He enjoyed discussion within a small group, and had a good memory for scientific anecdotes and a sense of fun. His achievements would have been remarkable for anyone, but for a man blinded so early in life they could well be unique.