

social wilderness of Shanghai in the 1930s he persuaded employers to provide better rice for their workers. This all embracing grasp of the causes of nutritional disease and their relationship to other diseases and social conditions marked him to the end of his days.

Joining the scientific staff of the MRC in 1939, as a senior member of the Central Organization for Coordination of Nutritional Research in the Colonial Empire, he played a major part in the preparation of the report on nutrition in the Colonial Empire published in 1939. This was a key document at the United Nations Conference at Hot Springs in 1943, at which the Food and Agriculture Organization was conceived, and he was a United Kingdom delegate to the First FAO Conference at Quebec in 1945. He directed nutrition surveys in Nyasaland and Gambia, and in his report to the Colonial Office on nutrition in the British West Indies, published in 1946, he gave a model exposition of the technique of rapid clinical nutritional surveys.

At the Human Nutrition Research Unit he directed the experimental investigation and measurement of the protein-calorie value of typical diets that he had seen to be consumed by under-developed communities. He reproduced the pathological features of kwashiorkor in experimental animals and, by means of precise assays, measured the protein-calorie values of diets as they are consumed, showing the effect on these values of such variable factors as infection, calorie restriction and intoxication.

Many of his students are now leaders in nutritional work throughout the world, but Platt was just as interested in the nutritional education of the health visitor in the village or the social worker in the city slum, and he took great pains to understand their difficulties. His reward has been their great respect and affection for a man who, showing deep understanding of their problems, would always point the way to their solution.

Correspondence

New Look for MRC

SIR,—In your note on the Medical Research Council's annual report for 1968-69 and, in particular, concerning the report of the Radiobiology Committee under the chairmanship of Professor Paton (*Nature*, 223, 437; 1969), you make the statement that "radiological protection is not the pressing topic it used to be".

We are concerned lest your statement be misleading if it is read without reference to the full report of the Paton Committee. It might be correct if radiological protection were solely concerned with environmental contamination from nuclear weapons fallout, at any rate now that the present levels of radiation from this source are low and assuming they decrease further in the future. Nevertheless, it would be prudent to keep under review the problem of environmental contamination from all sources rather than have to do this in the emotional atmosphere which is likely to result if any future substantial release of radioactivity into the environment occurs.

Our main point, however, is that radiological protection is concerned with a far wider field than fallout from nuclear weapons testing. This larger field has included in the past the safe use of radiation sources in medicine, industry, research and teaching. The need for continued supervision of these and other sources of radiation was foreshadowed by the Paton Committee in its reference to the forthcoming National Radiation Protection Board of the United Kingdom.

The use of radiation sources is increasing throughout the world and, together with expanding nuclear power programmes, these will continue to require effective radio-

logical protection organizations, both at national and international level, if we are to strike a proper balance between the risks and the benefits from these sources and if we are to prevent a repeat of the runaway pollution problems which have beset so many of man's advances. It would be unfortunate, therefore, if your remark were to undermine the important role that radiological protection must increasingly fulfil.

Yours faithfully,

E. E. SMITH
J. VENNART

Radiological Protection Service.

F. D. SOWBY

International Commission on
Radiological Protection,
Clifton Avenue,
Belmont, Sutton,
Surrey.

An Unhonoured Biologist

SIR,—I am sorry that Dr N. Dewey (*Nature*, 221, 394; 1969) derives so little benefit from Burton's great work, but I take particular exception to his view that Burton's work was "patching together the pros and the cons of an argument". Burton himself shows his impatience with such work—"a thing of mere industry, a collection without wit or invention, a very toy".

I do not observe any "question and tentative judgment" about Burton's definite conclusion which I quoted, "But this diversity of air . . .". Here he is drawing the same conclusion as Charles Darwin drew—some 230 years later, be it noted—from the same class of facts. "If we compare, for instance, certain parts of South America with parts of South Africa or Australia, we see countries closely similar in all their physical conditions, with their inhabitants utterly dissimilar."¹ When Dr Dewey writes of "lacking scientific method, his answers were a product of his imagination only", I presume he is implicitly comparing Burton with later writers on evolution. Let me point to what L. T. Hogben wrote: "The contribution of Darwin and Wallace first gained general acceptance for the doctrine of descent. But the success of the latter cannot be said in the light of modern knowledge to have been due to the greater measure of experimental (as opposed to circumstantial) data on which their conception of the process was based: for, like Lamarckism, Darwinism was an essentially dialectical construction."² Besides, as a product of the imagination only, what better example is there than Darwin's imaginary description of how the eye possibly evolved?³

I consider the sweeping statements in the last paragraph of the letter to be valueless. In my book I presented very extensive evidence. If Dr Dewey has counter-evidence, I hope he will publish it.

Yours faithfully,

A. BROWNLEE

509 Lanark Road,
Juniper Green,
Midlothian.

¹ Darwin, C., *Origin of Species*, 334 (Murray, 1885).

² Hogben, L. T., *Principles of Evolutionary Biology*, 105 (Juta and Co.).

³ Darwin, C., *Origin of Species*, 146 (Murray, 1885).

Complementarity and Philosophy

SIR,—It is somewhat disarming to read, in the article "Complementarity and Philosophy" by T. Bergstein (*Nature*, 222, 1033; 1969), a philosophical appreciation of the doctrine of complementarity at a time when physicists themselves have expressed reservations about that doctrine as a permanent element of theory. I am not here concerned with discussing the value of complementarity in disposing of traditional philosophy (although

a doctrine whose working hypothesis is a chameleon-like ability to avoid refutation should appeal as much to a metaphysician as to an anti-metaphysician). What I dispute is that modern physics necessarily supports such a view or can progress from it.

The first point to be made is that indeterminism in quantum physics does not, of itself, call for a new epistemology. Bergstein seems to imply that it does, for, although he develops the idea of complementarity of language as an independent philosophical principle, he then adds "... (philosophers) have not yet realized how profoundly the discoveries of modern physics affect the whole of epistemology". To say this is to imply that the uncertainty principle has a meaning which transcends quantum physics. But this is precisely what is in dispute. What the uncertainty principle tells us is that our knowledge of nature is limited by the existing language of physics. It does not limit all that we can ever know unless the existing language is the only possible one. What Bergstein argues is that the existing language—"ordinary" language—is so inextricably bound up with our experience that any other language would be unthinkable, even meaningless. But this is an independent assertion which cannot be inferred from quantum physics.

The notion that "ordinary language is the ultimate source of the unambiguosness of physical description" seems to me to be a very restrictive hypothesis which is not evinced by scientific history. Relativity, for example, was opposed by philosophers on the ground that Euclidean space was part of everyday experience and therefore Riemannian space was unthinkable. Of course, one can argue (with hindsight) that the example is not a good one because both theories employ the same variables; but it would be absurd to suggest that the everyday meaning of time has not been modified.

Although Bergstein regards ordinary language (and complementarity in it) as unavoidable, he does not, however, exclude the possibility of physical theory beyond quantum mechanics. He is prepared (as many who are otherwise his co-idologists are not) for "a more fundamental theory of matter and radiation which will contain quantum mechanics as an inherent sub-section". If by this he means "a more detailed theory", then he is being inconsistent. For, as Bohm has pointed out, such a theory is equivalent to an assumption of hidden variables. And, since these variables have not yet been observed, they will not be part of ordinary language. If he means "a logical extension of quantum theory" he should say so. But it is not clear what kind of extension can be meaningful in the face of an irreducible complementarity.

Perhaps the way out of the difficulty is to relegate complementarity from the level of a fundamental principle to that of a heuristic description, as Landé has suggested. After all, the discoveries of modern physics were not revealed by complementarity in language, but by experiments designed as if there is an external world independent of what we think. It is this ontological view which has motivated science from Galileo to Einstein. But then motivation is also "no problem for the paper philosopher".

Yours faithfully,

M. P. MELROSE

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King's College,
Strand, London WC2.

CORRIGENDUM. In the article "Multiple Forms of Rat Brain Monoamine Oxidase" by Youdim, Collins and Sadler (*Nature*, 223, 626; 1969), the cathodes and anodes should be interchanged in Fig. 1, and lines 3-4 of the legend should read "MAO₁₋₃ migrated from cathode to anode and MAO₄ from anode to cathode".

International Meetings

September 1-4, **Group Conflict and Mutual Acceptance**, San Francisco (Professor H. A. Enzer, Hofstra University, Hempstead, New York 11550).

September 1-5, **British Pharmaceutical Conference**, Belfast (Pharmaceutical Society of Great Britain, 17 Bloomsbury Square, London WC1).

September 2-6, **International Congress of Cybernetics**, London (Dr J. Rose, International Congress of Cybernetics, c/o Blackburn College of Technology and Design, Blackburn BB2 1LH, Lancashire, UK).

September 4-5, **British Pharmacological Society Meeting**, Manchester (Dr G. E. Mawer, Department of Pharmacology, Medical School, Oxford Road, Manchester 13, M13 9PL, UK).

September 15-17, **Social Demography and Medical Responsibility**, Budapest (Regional Secretary, International Planned Parenthood Federation, Europe and Near East Region, 64 Sloane Street, London SW1).

September 19-20, **Application of Microchemical Techniques in the Petrochemical and Allied Industries**, Portsmouth (Society for Analytical Chemistry, 9-10 Savile Row, London W1X 1AF).

September 24-27, **Diamond Conference**, Reading (Professor C. W. McCombie, Department of Physics, The University, Whiteknights, Reading, Berkshire, UK).

September 25-26, **Optimum Population for Britain**, London (Institute of Biology, 41 Queen's Gate, London SW7).

September 25-26, **Biochemical Society Meeting**, Cardiff (Biochemical Society, 7 Warwick Court, Holborn, London WC1).

September 30-October 3, **Non-Silver Photographic Processes**, Oxford (Mr C. Roberts, Kodak Research Laboratories, Wealdstone, Harrow, Middlesex, UK).

October 2-3, **Autoimmunity Phenomena**, Paris (Société Française d'Immunologie, Institut Pasteur, 28 Rue du Docteur Roux, Paris 15e, France).

Sabbatical Itinerants

In the hope of providing some practical assistance in the good cause of mobility between laboratories, *Nature* advertises the needs for housing of families about to take up periods of sabbatical leave. To begin with, no charge will be made for advertisements like this. It is hoped that a period of experiment will show what form these advertisements could most usefully take and whether they are effective.

Wanted: Furnished accommodation for 12 months from October 1969, within reasonable distance of Oxford, for English speaking young Swiss family of 3. Please contact Dr Max Dobler, Laboratorium für Organische Chemie, Eidg. Technische Hochschule, Universitätsstrasse 6, 8006 Zürich, Switzerland.

Wanted: Furnished, centrally heated house in the country in the south of England for entomologist and family of 2 school-age children. Returning for 1 year in February 1970 for writing-up. Please contact R. A. Farrow, OICMA (International African Migratory Locust Organization), Kara, Macina, Mali.

Vacant: Spacious furnished apartment with 4 bedrooms, central heating and garden, in central Edinburgh, for 1 year from October 1969. Please contact N. Stebbing, Department of Zoology, West Mains Road, University of Edinburgh, Scotland.