

author justifies the comprehensive nature of the treatment given by the argument that some readers will need to refer back to past literature on engineering subjects where, unfortunately, one finds a medley of units used in the different texts.

Units and their definitions as well as their symbology undoubtedly form a kind of language, and once one grasps this concept the absurdity of using so many different systems becomes clear.

There is so much detail given in this book that the text, although by no means difficult or complicated, is heavy going. It is inherently a book for reference and guidance, many aspects of the subject obscured or lost in past discussions being explained and clarified. In particular the author stresses that any system of units needs to be coherent, implying, that is, that the system of base units must be well chosen so that the constants of proportionality in the corresponding equations are consistent among themselves.

Another topic, well covered and documented, is how one converts between the various systems and the associated equations even when the constants of proportionality change. Are such conversions really essential? Surely it is far simpler to refer the measured quantities in each system to the fundamentals and convert these. It may be a longer method but, without the aid of a reference book like this, far safer. The lack of a proper terminology has undoubtedly in the past been a severe hardship and an unnecessary burden on teachers and students alike.

One can only hope that the present drive and effort to bring home to all workers the vital need to use only SI units in their future work will be effective. This new book will unquestionably aid this crusade.

P. F. SOPER

## TWO VIEWS ON RELATIVITY

### Relativity, Time and Reality

A Critical Investigation of the Einstein Theory of Relativity from a Logical Point of View. By Harald Nordenson. Pp. 215. (Allen and Unwin: London, August 1969.) 60s.

### The ABC of Relativity

By Bertrand Russell. Third revised edition, edited by Felix Pirani. Pp. 139. (Allen and Unwin: London, August 1969.) 21s.

Of all scientific theories relativity seems to have to defend itself most often against the forces of superstition deeply entrenched in human nature. Nordenson's book is one more attack, and this time of an exceedingly subtle kind. The author describes his critical investigation as being from a logical point of view and it is indeed the philosopher and logician who will have most sympathy with his arguments. His most telling objection to special relativity refers to Einstein's presentation of the theory, in which what has now come to be known as the radar method is used for synchronizing distant clocks. To this Nordenson raises (pages 39-42) the superficially telling objection that the rule given by Einstein: "assign to a distant event a time which is the average of the times of emission of a light signal to the event and the reception back of the reflected signal" relies on tracing the path of the light in order to identify the reflected ray. This tracing out of the path presupposes some causal relation, and this causality, according to the author, already presupposes in turn a universal (classical) time, and so the very distant simultaneity that Einstein is at pains to deny. Certainly this argument makes it clear that too simple minded a formulation of the radar method may run into trouble; in particular, more is assumed than is sometimes admitted about the identification of signals, and about the existence of a "local region" in which certain approximations are valid. But if the argument really proved what it claims,

then it would also prove that real radar would not work; or at least that the pilot would need an impossibly accurate pre-set chronometer to use it. The author also claims to have a positive theory to put forward, but the essence of this can best be judged from pages 54-55 where the expression  $V-v$  ( $V$  the velocity of light) in a certain equation is interpreted as a relative velocity and so replaced by the value that the Einstein velocity relation would give it (that is,  $V$ ); whereas the occurrence of  $V-v$  in the equation in question is not for a relative velocity reason at all, but simply as a result of certain algebraic manipulations.

In contrast to Nordenson's polemic, it is a pleasure to welcome a new edition of Bertrand Russell's lucid volume. This third edition is a slight modification, and brings up to date the revised edition of 1958 which was also edited by Felix Pirani. It affords an ideal introduction to the theories of special and general relativity for those with very little mathematics.

C. W. KILMISTER

## Correspondence

### Radiobiological Terminology

SIR,—Some twenty-five radiobiologists met on September 8 and 9 at the Strahlencentrum of the Justus-Liebig University in Giessen, to confer on special problems of recovery from radiation damage. Some time was given to discussing terminology, which it was agreed is currently of a nature leading to confusion in thought as well as communication.

The word "recovery", in particular, is used with numerous different meanings not only in the whole of radiobiology, but also by radiation chemists. Even when the author has defined his terms, work describing "recovery processes" is frequently quoted in papers in which the same word is used in quite a different context, and to which the citation is therefore irrelevant. Confusion has increased with the great interest shown during the past decade in phenomena associated with dose fractionation. Cells exposed to two doses of radiation separated in time often show higher survival than if a single equal total dose is given, and, since Elkind and Sutton<sup>1</sup> reported quantitative observations on this phenomenon in mammalian cells, it has become known as recovery "from sub-lethal damage" (or "sub-lethal injury"), a phrase they introduced. Radiobiologists investigating the phenomenon nevertheless do not always qualify the word "recovery", and the ambiguous phrase "intra-cellular recovery" (or "repair") is sometimes used.

"Recovery from sub-lethal damage" is observable only in cells which have retained their ability to proliferate after a first dose of radiation, and then only if they are exposed to one or more subsequent doses. The phenomenon is thus easily resolved from others in which post-irradiation treatments may cause more cells to survive a single dose. Confusion has undoubtedly arisen from the use of the word "recovery" in both contexts. The participants in the Giessen meeting therefore agreed that greater clarity would be achieved if the word "sparing" were reserved for describing the effects of dose-fractionation. It was thought to be appropriate that this word is already in use in the same context by radiotherapists, for much of the interest of radiobiologists in the phenomenon stems from its bearing on radiotherapeutic practice. Advantages of the term "sparing" are that it can be used also to describe the reduction in the effects of radiation often observed when this is delivered at a lower dose-rate, a phenomenon associated with the sparing effect of fractionation; and that it can be used to describe the effects of dose-fractionation when end-points other than cell death are under observation: for example, dose fractionation is

sparing also for the induction by radiation of mutations<sup>2</sup> and chromosome aberrations<sup>3</sup>, and for the erythematous reaction of human skin<sup>4</sup>. An important reason for eschewing the use of the word "recovery" is that this carries implications about mechanism which are not at present warranted by experimental observation, and which may well block a new approach.

It was agreed at the meeting that the same criticism could be levelled at the prevalent use of the word "repair", currently applied in any situation in which one set of post-irradiation conditions brings less damage to light than another, or in which effects on radiosensitive and radio-resistant variants of a cell line are compared. The positive implication of the word "repair" might, it was thought, influence the approach towards, and thus impede, the elucidation of the mechanisms at work. This word, like "recovery", should therefore be avoided unless there were evidence which justified its use. Processes at the biochemical level which result in a diminution of the effects of radiation would be better described by a word carrying less implication about mechanism, for example "rescue".

Yours faithfully,

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<sup>1</sup> Elkind, M. M., and Sutton, H., *Nature*, **184**, 1293 (1959).

<sup>2</sup> Abrahamson, S., *Genetics*, **44**, 173 (1959).

<sup>3</sup> Wolff, S., *Radiat. Res.*, Suppl. 1, 453 (1959).

<sup>4</sup> MacComb, W. S., and Quimby, E. H., *Radiology*, **27**, 196 (1936).

### "Anomalous" Water

SIR,—The possible danger of nucleating the environment with "anomalous" water, mentioned in a recent letter to *Nature* (**224**, 198; 1969), assumes that failure to find this material in nature is due to a lack of polymer nuclei. This explanation appears unlikely given the variety of surfaces and conditions found on the Earth's surface and the widespread occurrence of water and water vapour.

Physical chemists rarely work under sterile conditions or in the total absence of protein contamination. An enzyme as ubiquitous as catalase and as heat stable as ribonuclease may well exist which rapidly depolymerizes polywater. Contamination may explain the variability observed between different capillaries during attempts to produce the material, and enzymatic breakdown would also explain failure to find polywater in natural waters. An extension of this view would suggest that polymeric water may be the natural state of all planetary water in the absence of life or of the early stages of organic chemical evolution.

The experimental problem is therefore the complete elimination of contaminants of biological origin from the experimental polywater production systems to see if this proposal is correct.

Yours faithfully,

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### International Conferences

SIR,—The letter by Dr F. H. C. Crick and his colleagues concerning international congresses (*Nature*, **224**, 93; 1969) discusses an important dilemma which, we fear, will face the international scientific community for some time to come. Because the issues to be considered by the individual scientist who has to decide whether to accept an invitation to a meeting or an honour bestowed by a foreign government are in principle often similar to those which concern the organizers or sponsors of international meetings, your readers may be interested in recent decisions by the council of the Federation of European Biochemical Societies concerning future meetings of FEBS.

The sixth FEBS meeting was held in Madrid in April of this year in the face of objections that had been made because of the declaration of a "state of exception" in Spain and the closure of some of the universities earlier in the year. Following a visit to Madrid by us, the executive committee of the federation decided that the Madrid meeting should be held as planned. This decision was endorsed subsequently by the council of FEBS at the meeting in Madrid on April 7, 1969, which was attended by delegates from all 24 constituent societies. At this meeting, there was a full discussion not only of the situation that had arisen in connexion with the Madrid meeting, but also of the principles that should be applied in considering similar problems in the future. It was agreed unanimously that the following four criteria should be a necessary and sufficient condition for holding an international meeting: (1) Freedom for all foreign participants to enter and leave the country concerned. This requires recognition of valid passports and the issue of visas without difficulty. (2) Complete freedom of speech on scientific matters at the meeting, both in the official sessions and in private discussions. As a tradition, political, religious or racial questions should not be discussed at official sessions. (3) Freedom of movement in the city where the meeting is being held. (4) As far as citizens of the host country are concerned at least, conditions (2) and (3) should apply.

These guidelines, if generally accepted, would provide a rational basis on which to judge whether a particular international meeting should be supported. They differ in several respects from those suggested by Dr Crick and his colleagues and we believe them to be more realistic. Scientific meetings take a long time to organize and the financial cost of the preparations is not negligible. Unfortunately, also, there are not many political regimes which place no restriction of any kind on the freedom of communication between scientists or on academic freedom in general. Conditions for meetings may therefore not always be perfect, yet cancellation should be a last resort. The criteria evolved by FEBS have been found useful in practice and we therefore commend them to other international scientific bodies and organizers of meetings.

In the last analysis, it is right that individual scientists must remain free to follow the dictates of their conscience, but international scientific organizations have to operate within a consensus of a wide spectrum of opinions. They should act in a way that preserves both their integrity and the opportunity to carry out their function. We hope that this contribution to the discussion will help to clarify the issues.

Yours faithfully,

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