This is only common prudence, in order to secure the full return from the large sums of public money already allocated to schemes of scientific research and development; but although there is welcome evidence in this report that the importance of conditions of service and of securing contented teams of skilled staff are more widely appreciated than when the Barlow Committee reported, there is evidence also of waste through neglect of this consideration. The Committee refers, for example, to the probability that the proposed transfer of the Fighting Vehicles Design Establishment to a fresh site will involve not only a large waste of public money but also the break-up of a valuable team of scientific experts.

The Select Committee fully recognizes the difficulties which may sometimes be encountered in avoiding costly decisions of this type. The risk, too, that expensive constructional work may be rendered out of date by scientific discoveries made before the work has even been completed cannot always be excluded. Apart from the secrecy issue already mentioned, the Select Committee's report is on the whole reassuring, and even where the secrecy curtain was most baffling, it appears to be satisfied that the money spent on the atomic energy project is justified by the results so far achieved. Nevertheless, the warnings given in this report are plain enough, and sufficient material should now be available for scientific men to make sure by their professional action that the position does not deteriorate.

Some other points in this report merit notice. Evidence was submitted of the need for greater attention to the function of the technical colleges in providing technicians—a function commonly ignored in discussions on technological education. It was estimated, however, that only some 1,750 technicians would be required for research and development in the rearmament programme, instead of the 3,000 estimated by the Hankey Committee.

Sir John Cockcroft's evidence before the Committee is of special interest. He estimated that about one-third of the effort at Harwell could be broadly described as research and the remainder as development, including about 15 per cent on the production of isotopes. The demand for these is rapidly increasing, and it has since been announced that Great Britain is now the world's largest exporter. Sir John also expressed the opinion that the numbers and quality of staff recruited for atomic energy work are satisfactory, although it is the younger men rather than the older who are being attracted. He was not disturbed at a wastage of scientific staff running to ten per cent. What is disturbing, however, is the reference elsewhere in the evidence before the Committee to a drift of highly qualified men of science from Britain to America, attracted by the much higher salaries offered there.

This loss is not great at the moment: it was put at about fifty last year; but the loss of even that number of our best brains is nevertheless a serious tax on our limited resources of scientific and technical man-power, and the evidence of the Controller of Atomic Energy on the disparity of salaries in Britain and overseas is not reassuring in this connexion. The Ministry of Supply's evidence on scientific staffing admitted that the scientific man entering the government service is probably underpaid in comparison with industry—as is also the scientist at the top of an establishment; it is contended, nevertheless, that the men in the middle range compare favourably in salary and conditions with those elsewhere. Sir Frederick Morgan, however, was prepared to argue a case for special scales for those on atomic energy work. He maintained that the man who is capable of original thought and of reducing it in a disciplined way into something which is handed over later to the production side is invariably on a scale of wages which bears no relation to the value of the work he is doing.

This report clearly emphasizes the danger of failing to get full returns from Britain's huge expenditure on research equipment through parsimony in the conditions offered to the men who use that equipment; nevertheless, it will fully justify the Committee's careful inquiries. No less important, however, is its warning of the dangers which also attend the use of the cloak of secrecy to hamper Parliamentary scrutiny of expenditure on research and development. Public scrutiny remains the surest safeguard against either extravagance or duplication of effort, and other evidence of Sir Frederick Morgan rather offsets Sir Henry Tizard's confidence as to the effectiveness of the Defence Research Policy Committee in this regard. His evidence led the Select Committee to correct its earlier impression that that Committee and the Advisory Council on Scientific Policy covered the whole field of research and development. In a strictly formal sense, atomic energy stands apart from the field covered by those two committees, and the complicated position disclosed by the evidence set forth in this report suggests that the position is not entirely satisfactory. Something more appears to be required in the way of co-ordination, even informally, before the organization of Britain's research and development effort can be regarded as adequate. The experience which Sir John Cockcroft brings from the atomic energy field to his new responsibilities as chairman of the Defence Research Policy Committee and of the Advisory Council on Scientific Policy should be a real help; but the need for fresh constructive thought here is as plain as is the need for limiting the use of the security curtain to the minimum consistent with national safety.

## LOGICAL FOUNDATIONS OF PROBABILITY

### Logical Foundations of Probability

By Rudolf Carnap. Pp. xvii+607. (London: Routledge and Kegan Paul, Ltd., 1951.) 42*s*. net.

PROF. R. CARNAP is one of the leaders in the modern school of mathematical logic. Perhaps his most striking contribution is his emphasis on the distinction between language and semantics (the theory of the meanings of the expressions in language). For example, Whitehead and Russell are said to have

defined the number of a class as the class of all classes cardinally similar to the given class. (At least, this is the usual description of what they did; their actual procedure was more elaborate.) This is one interpretation of number. If it is supposed to be the only one, an ordinary mathematician appears to be precluded from using 'number' to mean anything else, even though other things may appear to follow the same rules. The modern method would be to consider a language containing the signs for the numbers—rules for manipulating the signs are stated in a further language called the metalanguage—and in this way a formal development of mathematics is created. The legitimacy of the interpretation of the signs and operations in a particular way becomes a separate problem. This idea can be applied to logical systems at the most elementary levels, and has led Carnap and others to considerable clarification of the foundations of logic.

The main theme of the present book is the application of this method to probability theory. Carnap distinguishes between two kinds of probability, which he denotes by probability$_1$ and probability$_2$. Both satisfy the same rules of manipulation. Probability$_1$ is regarded as a logical relation between pairs of statements expressible in a given language; probability$_2$ as a limiting relative frequency in an infinite series of experiments. An adequate account of scientific inference must include both. The discussions are very full on both the symbolic and the interpretative levels.

One difficulty of the subject is the distinction between 'subjective' and 'objective'. I have gone so far myself as to say that both words have been used in so many different senses that they should be abandoned as *nomina confusa*. Carnap's method enables him to say that both kinds of probability are objective. For probability$_1$, his argument permits him to avoid the term 'degree of rational belief', on grounds similar to those given by Russell for deductive logic. Russell argued that $2 + 2 = 4$ is a logical relation between sets of classes and has nothing to do with the existence of a mind, rational or not, to appreciate it. The ability of a mind to understand and use it belongs to the interpretative level. Similarly, Carnap would regard probability$_1$ as a ratio of measures of sets of sentences and call it a 'degree of confirmation'; this is *used* in rational belief but is not its whole content. This seems to add some clarity. However, as he himself points out, the measures can be taken in many ways; his theory gives rules that must be satisfied by any system of probability$_1$, but many different numerical assessments would satisfy the rules, and the decisions between them need further discussion on the interpretative level. Most of the argument is quite general, and many controversial questions are well discussed.

Carnap discusses one particular assessment, which appears to be equivalent to Laplace's principle of indifference, and remarks that in some circumstances this leads to contradictions. A second volume is in preparation and will apparently produce more specific results. Some of these are described in an appendix, which states a modification of the principle of indifference to avoid contradictions. This seems similar to the rule that I have stated for the sampling of a population with respect to more than two exclusive properties. I doubt, however, whether this can cover the ground. Carnap does not appear to mind the conclusion (given by Laplace's form) that the initial probability of a general law, applied to an infinite class, is zero; and there is something to be said for that view. But Laplace's form also leads to the conclusion that if an event has succeeded in $l$ trials and failed in none, the probability that it will succeed in all the next $l + 1$ trials is $\frac{1}{2}$, and this seems definitely too low to correspond to scientific practice. The modified rule appears to intensify this difficulty. I hope that Carnap will attend to this point, because I know of nobody better equipped to produce a satisfactory solution.

The book has not much to say about probability$_2$, except that values of probability$_1$ are often estimates of probability$_2$. I am inclined myself to think that the idea of an intrinsic probability is useful, though I think that the attempt to define it in terms of a limiting frequency creates more difficulties than it solves, and I cannot see that anything in the book depends on the definition. It might be argued that the only cases of probability$_2$ are in quantum physics; one, for example, would be the probability that a radium atom will disintegrate in the next minute. In coin-tossing, we might make a definition in terms of a limiting frequency (assuming it to exist) for a sequence of throws with one coin (assuming that the coin never wears out), or in terms of one throw each for an infinite set of coins (again assuming the limit to exist). But there is no way of proving that the two definitions would give the same assessment, without piling up further assumptions that appear no more plausible than the notion of an intrinsic probability seems to begin with.

The book is far the best analysis of the logic of probability that has yet appeared, and will be indispensable to students of the subject. The specific applications announced for the second volume will be awaited with interest. Harold Jeffreys

## ORGANIC CHEMISTS' VADE-MECUM

### Chemistry of Carbon Compounds

A Modern Comprehensive Treatise. Edited by Dr. E. H. Rodd. Vol. 1, Part *A*: General Introduction and Aliphatic Compounds. Pp. xxiii+778. (New York and Amsterdam: Elsevier Publishing Co., Inc.; London: Cleaver-Hume Press, Ltd., 1951.) £7.

THE hiatus existing between the necessarily highly selective and simplified text-book treatment of organic chemistry and the comprehensive scope of compendia such as "Beilstein" has for long been filled by Richter's "Chemistry of Carbon Compounds". The last edition in English of this work appeared thirteen years ago, and even this was a patchwork overhauling of an earlier version. The need for a successor has been markedly apparent, and the onus of this task has been accepted by the Elsevier Publishing Co. in consultation with an advisory committee of six distinguished British organic chemists headed by Sir Robert Robinson; the herculean task of editorship is being shouldered by Dr. E. H. Rodd. The present book represents the first stage in the compilation of a five-volume work to take the place of "Richter" in providing a modern comprehensive treatise on organic chemistry.

The book opens with an editorial historical survey and sections on the classification, nomenclature and