important when the free path is no longer small compared with the other lengths involved. Cercignani prefers analytical methods of solving the integral equations involved; he lists problems which have been discussed numerically, but gives little detail.

The book has been attractively prepared. It reads easily, though this is in part because of the suppression of (not always unimportant) ancillary matter. It attempts to give exact proofs where possible; to achieve this it has sometimes to make non-physical assumptions which limit the range of validity of the proofs. One is not always sure that the book is concerned with actual gases; however, as a mathematical exposition it possesses refreshing novelty.

T. G. COWLING

Obituaries

Professor P. W. Robertson

PHILLIP WILFRED ROBERTSON, who died in London on May 7, was born in Auckland in 1885. From Wellington College he proceeded to Victoria University College, graduating MA with first class honours in chemistry in 1905. In recognition of his academic competence and his ability in sport, especially hockey and tennis, he became the first of Victoria's Rhodes Scholars (1905). At Trinity College, Oxford, he took MA with first class honours in chemistry and subsequently at Leipzig he was awarded a PhD. He became successively professor of chemistry at Rangoon College, Burma (1909–11), lecturer at Imperial College, London (1911–19), and professor of chemistry at Victoria University College, Wellington (1920–49).

As an undergraduate, Robertson had an unusual aptitude for research. Even at seventeen it was obvious that he had in mind one of his principal objectives—to investigate the relationship between chemical constitution and physical properties. By the age of nineteen he had published in the *Journal of the Chemical Society*, London, and also in the *Transactions of the New Zealand Institute*, eight scientific papers entirely on his own. His earlier contributions touched on analytical chemistry including the well known method for halogen estimation, and on the mechanism of aromatic substitution. In extensive researches on cryoscopy, Robertson discovered the tendency of fatty acids to form double molecules which explained *inter alia* the two inflexions in the melting point and solidification curves of mixtures of adjacent even numbered fatty acids and assisted in the development of the theory that the component acids in the double molecules were held together by hydrogen bonding.

Much of Robertson's later work was concerned with the mechanism of organic chemical reactions, especially in connexion with electrophilic addition and substitution of halogens. Several of his papers on the importance of hyperconjugation in chemical substitution reaction appeared after his retirement in 1949, the last paper being published in the *Journal of the Chemical Society* in 1956.

Robertson's contributions to chemistry were paralleled by his literary achievements. They comprised two books, a number of short stories and a few poems. The earliest work published in 1920 under the title *The Soul's Progress: Mezzotints in Prose* has been compared by Richard Le Gallienne with Walter Pater's *Imaginary Portraits*. Its thread is continued in the autobiographical *Life and Beauty*, a testimony to the author's ability to interpret foreign cultures, literatures and writers. His short story *Odyssey in Wellington Harbour* has been described as a masterpiece of contemporary New Zealand prose.

His last publication appeared in *Biology and Human*

Affairs in 1967 in the form of a profound treatise on colour words and colour vision.

In the preface to his first book he wrote: "There have been some who imagine that natural sciences will eventually solve the riddle of the universe, while others have insisted, and perhaps rightly, that art being pure intuition, is a more certain key to a problem, one at least of whose factors is infinity". In his unassuming way, Robertson endeavoured to approach the problem from both sides.

Correspondence

Will there be Catastrophe?

SIR,—I am deeply saddened by the comfortable conservatism and complacency of your leading article "Will there be Catastrophe?" (*Nature*, 223, 550; 1969).

The basic issues involved seem to have been avoided. Only minor occasions in which technological progress has clashed with social values were considered. But why does society have a say (and then usually too late) merely in the side effects of technology? Why are not the principal choices determined socially (i.e., selecting which of the opportunities presented by scientific research are to be developed)?

The reason seems to be that the public is mostly unaware of the choices available and scientists are too uninterested to inform them of the range of technological opportunities. The answer to this dilemma lies, largely, in an education system which instils in scientists an awareness of society and of their own special social responsibilities.

Passive acceptance of the *status quo* and a complacent view of science are the antithesis of such a policy and just the type of attitude that suggests there will be a catastrophe.

Yours faithfully,

DAVID BIGGINS

ARC Unit of Nitrogen Fixation,
University of Sussex,
Brighton BN1 9QJ,
Sussex.

Putting the Moon where it Belongs

SIR,—In the article "Putting the Moon where it Belongs" (*Nature*, 223, 333; 1969), where you are discussing the scientific objectives of the Apollo flight, you say: "Where basic science is concerned, for example, it would probably have been possible to avoid some of the mistakes of the past few years if NASA had been more closely in relationship with the scientific community. Some of the Biosatellite experiments would have had a poor showing if they had been put through the kind of intellectual mill to which academic research proposals are usually subjected, and is it really sensible to go looking for magnetic monopoles in samples of Moon dust recovered from the Moon this week?"

Since I am the principal investigator who proposed the monopole search to NASA, it would appear that *Nature's* question (if it is not rhetorical) should be answered by me. I have just re-read my 1967 proposal, which is unfortunately too long to be reproduced with this letter, and I find the arguments presented at that time still valid. The opening sentence of my proposal reads: "Modern experimental physicists are generally agreed that the discovery of any one of the following three objects would constitute a major breakthrough in our understanding of the physical universe: (1) free magnetic