

its development; else, in the sparse atmosphere of 1926 when the Commonwealth Council for Scientific and Industrial Research (CSIR) was born, it would not have survived.

Rivett was a puritan, fanatical, single-minded. Without him, there would have been no highly successful CSIR. He did not spare himself. In the days before air travel was a commonplace, he spent scores of uncomfortable days in trains getting to know local conditions and people. His family life was minimal; yet there was an essential solidity and sincerity of affection with his wife and children without which he could not have survived.

What were his achievements? Without doubt, to have created an atmosphere and an organization in which science in the service of the nation—and funded by Federal Government—could take roots. He preferred Australians with experience abroad, to whom he gave complete support in their research—at the expense of a comfortable HQ office. This high regard for the man at the bench, and his lesser attitude to the administrator, never changed. It was well expressed in his briefing to a plant pathologist, B. T. Dickson, who agreed to leave McGill University in 1927 to become CSIR's chief mycologist and second chief of a division. He explained there was an abundance of problems and practically no organization. "This Council has been in existence for only a year and to a very great extent its plans have yet to be made. But briefly the object at which we are aiming is to get together a band of enthusiastic leaders in different branches of science (chiefly those associated with the primary industries) and to afford them every opportunity for initiating and developing organizations for attack upon national problems."

The Second World War made it urgent to concentrate scientific effort in the secondary industrial field, to concentrate on specific problems. Rivett aided in this, of course. But he felt that CSIR—as he wrote in 1946—was turning "into a mob of testers for industry and I hate it." He did not want to handle the routine everyday problems of industry. He was not against the building up of secondary industries, but he believed passionately that only through basic research would come real strength.

He succeeded Julius as chairman of CSIR, but the postwar "cold war" years with the emphasis on security made him unhappy. He did not accept that secrecy and integrity in science could flourish together, and he resigned in 1949 when the CSIR was placed under the public service, at a time when charges of being weak on security and of allowing Communists to work on security projects were made and debated in the House of Representatives.

When W. L. Morrison, the Minister for Science, dissolved the Advisory Committee on Science and Technology last February, he insisted that science and technology were no longer polarized, and that there should be no rigid divisions between the natural and the social sciences. He was announcing—although he may not have understood he was—a new generation of science policy, which would have been impossible without the sound foundations laid by David Rivett.

MAURICE GOLDSMITH

Leibniz

Leibniz and Dynamics: The Texts of 1692. By Pierre Costabel. Translated by Dr R. E. W. Maddison. Pp. 141. (Hermann: Paris; Methuen: London; Cornell University Press: New York; 1973.) £3.

In 1960 Pierre Costabel published two previously unknown texts by Leibniz on mechanics, adding a commentary and an account of their origin amounting in all to a short book. The subject of the present review is a recent translation of this work into English. The first of the Leibniz texts, the "Essay de Dynamique", has been provided with a translation; the second, entitled "Règle générale de la Composition des mouvements", is given, for no clear reason, only in the original French.

Costabel's work comes struggling towards the reader through a translation of very poor quality, in which infelicities of style and vocabulary abound, and downright mistakes are not infrequent. We hear of being "unaware a complexity", of something being unescapable, of a battle's being waged, of an "amazing perpetual secretary", of someone "urged by the collector's passion". Someone's doubts are "raised" when what is meant is that they are assuaged; the English version of Definition I of the *Essay on Dynamics* does not make sense (though fortunately in this case one has the perfectly clear French version on the facing page). This sort of thing tends to sap one's confidence—one begins to wonder just how much of what one is reading is actually due to the author; or for that matter, to the translator, for stretches of the book are so clumsy that one suspects that some unnamed non-English editor must be partially responsible for the outcome.

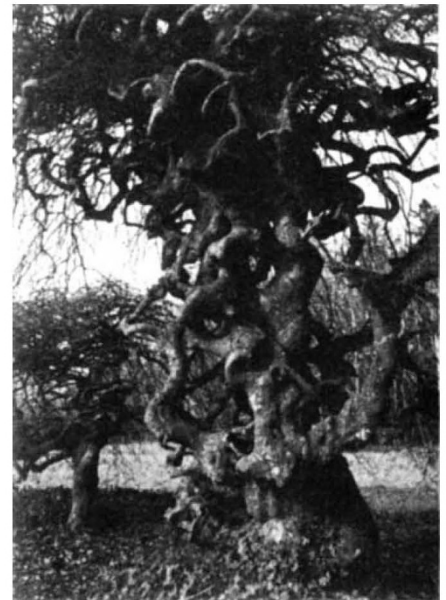
The central theme is the controversy between Leibniz, on the one hand, and the Cartesian scientists of the Paris Academy of Sciences on the other, concerning the nature of the principles of conservation to be used in mechanics. Chapter 1, rather pretentiously called "From the History of a Discovery to the Discovery of History", introduces this controversy as background to the two texts, and tells of their discovery and

the identification of their copyist. One of the difficulties I had with the book is to do with the fact that the exact nature of the debate is rather slow to emerge. The Cartesians believe in the conservation of "force", measured as "quantity of motion"; and as the latter is the product of mass and velocity it seems that they are asserting, and Leibniz denying, a principle of conservation of momentum. But one discovers (and this is, I feel, the sort of thing which a good exposition would make clear much earlier) that it is Leibniz who is asserting this principle, since it turns out that the Cartesians regard "velocity" as scalar, whereas Leibniz takes it to be a vector. Again, I can't help thinking that everything would be much more readily comprehensible to the modern reader if "energy" had been used for "force" in most contexts throughout the book.

Problems of comprehension held me up many times during the reading of this book. They may be the product of the author's far greater familiarity with the subject matter, but they are nevertheless a defect. I do not think that the interested layman is going to enjoy *Leibniz and Dynamics* very much. Still less, at £3, is he going to wish to buy it. The professional historian of physics will be grateful to Fr. Costabel for the discovery and publication of the manuscripts. But he may well feel that the task of unmistakably charting the course of the controversy to which they contribute is one that remains to be done.

E. J. CRAIG

Tortured Tree



One of the rare Tortuosa beeches, whose malformations may have been caused by radioactivity from meteors, is illustrated in *The International Book of Trees* by Hugh Johnson (Beazley: London, October 1973, £9.95).