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The origins of angiosperms are still debated, despite many years of work by scientists from differing disciplines. The progress made toward resolving the problem is reviewed in this book. The author suggests that the only fruitful method of study is the total integrated use of the fossil record, particularly dispersed palynomorphs. This includes the use of electron microscopy and refined data handling to record the occurrence of microscopic fossils, rather than the extensive use of morphology and cladistics. The methods advocated in this book could result in a rethink of the current classification of living plants. The ideas presented will initiate discussion between professionals and students of paleontology and plant science on the wider possibilities that may clarify the enigmatic origins of the dominant flowering plant groups.

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DEDICATED IN MEMORY OF

Hugh Hamshaw Thomas F.R.S. 1885–1962

of the Botany School, Cambridge, and of Downing College,
who essentially solved a significant part of
the problem in the context of knowledge
seventy years ago,
but who was ahead of his time

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Preface

This book is intended as a sequel to my previous book published in 1976 (*Paleobiology of angiosperm origins*, CUP), but is not in any sense a second edition. The still unsolved problem of angiosperm origins has moved on in terms of interest and of general attention, but remains in some respects as wide open as ever. It is currently possible to publish serious papers proposing origins scattered over 150 million years of Triassic to Cretaceous time, a duration longer than the probable subsequent period of existence of this centrally important group of plants. Having studied Mesozoic fossils relevant to the problem over the last forty years, and having thus been personally involved along with many others in the failure to solve it, I sincerely believe that this failure is not due primarily to lack of application on the part of any one of us, and that consequently there is a significant error of method concealed somewhere.

The current divide appears to be between the relatively few who maintain that reliable evidence can only come from the fossil record, and perhaps the majority who feel that all the weight of scientific effort committed over many years to comparative morphology, to interpretation of chromosome numbers, to DNA or RNA signatures and the like should somehow prevail. To a believer in the ultimate efficacy, when properly employed, of the whole integrated fossil record, despite its shortcomings, the 'comparative morphology' method can offer only approximations with no time-sequence basis and thus no penetration beyond a fog of enthusiasm that is not meaningfully susceptible to statistical manipulation. Compromise has already been tried for perhaps two decades now without any noticeable success, and will doubtless persist, but the strong suspicion that the significant error of method is indeed to be found in this neontologic procedure suggests that compromise is not any longer of value here.

Unfortunately the issues very easily become emotionally charged. This is a measure of the great investment in the existing shored-up part-solution of the problem; the very central classification of extant angiosperms, leading on to good order in botanical science and even to public esteem, appears to hang on the maintenance of this uneasy position. Because the consequences of any change of approach in the direction proposed might reach into the understanding of evolution, into the classification of all land plants affected by concepts of primitive and advanced characters, into taxonomy and the nomenclature codes, and into recording methods in the whole of paleontology, there is great difficulty in avoiding the giving of unintended offence.

No unusual rectitude is claimed for the ideas presented here, and no rapid solutions are offered, but I hope that the challenge is serious enough to encourage exploration for new lines both of evidence and of argument.

Although I have trawled ideas both positive and negative from very many paleobotanical colleagues past and present throughout the world, most of whose names appear at some stage in the book, none of them should be held to account even by association for any disasters of mine. Particularly helpful have been some of the botanical brigade of free-thinkers of the unexpected such as Tom M. Harris, E. J. H. Corner, Jack Douglas, William G. Chaloner, Val A. Krassilov, A. D. J. Meeuse, James A. Doyle, John M. Anderson and others who have unknowingly quashed for me various wilder aberrations. Research students and associates who have suffered and thus helped, knowingly or otherwise, include the late R. Ashley Couper, Frank R. Gnauck, Kathleen I. M. Chesters, Margaret G. Mortimer, Geoffrey Playford, Mary E. Dettmann, Jenny K. Friend, Geoffrey Norris, Keith C. Allen, Judy C. Moody-Stuart, Elizabeth M. Truswell, David J. Batten, Catherine A. Croxton, David G. Smith, John F. Laing, Timothy C. B. Oldham, Gillian E. Drewry, the late Timothy H. Jefferson, Jenny L. Chapman, Babagide Salami, Hamish J. Campbell, Audrey G. McDougall, Ihsan I. Aslam, James H. J. Penny and Ian C. Harding. I have also been very grateful for long periods over the years for much technical assistance in the Department of Earth Sciences from the late Albert Barlow, Paul Hensher, Stan Curtis and David Newling, and from many others at particular times. In the last few months I have been specially appreciative of help from Robert A. Spicer, Alan G. Smith, Gillian E. Drewry, Catherine Flack and Sandi Irvine of CUP, Audrey McDougall, Christine Few, Jacqueline Hodgkinson and Hilary Alberti.

Norman F. Hughes

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