Book Review Section

Compiled by R. P. Robertson*

Gwyn Macfarlane, *Howard Florey: The Making of a Great Scientist* **(Oxford Univ. Press, 1979), 396 pp. ($26.00).**

This book by a distinguished biologist deals with the emotional, intellectual and social development of its subject. It derives its authority from personal letters, especially those between Florey and his far distant *belle ideale*, published scientific material, the lay press and interviews with appropriate people around the world. There is no doubt about any of the recorded facts which are all authenticated by a satisfactory bibliography and index.

It deals fully with only the first twenty-five years of Florey's forty-six years of active scientific life, i.e. to the time when Florey had led the team at Oxford through enormous difficulties of technical innovation and finance to the complete purification of penicillin, the proof of its structure and properties and of its epoch making curative properties.

It starts with a historical introduction which sketches the development of bacteriology, antisepsis, immunology and chemotherapy. His account of the development of biological research and teaching at Oxford sets the scene for much of Florey's work as post-graduate student and later, as professor there.

The author, a haematologist, then deals with the social mix of Florey's family and its contacts in Adelaide. Florey's father was a bootmaker who went from clogs to affluence to bankruptcy in one generation. It is indicated that this instability of the economic system was the basis of Florey's 'will to win', but he showed it all his life, even as a boy when they were well off, with great advantage to his scientific projects but considerable reaction from the Colonel Blimps of the scientific hierarchy. The author points less directly to the background of these gentlemen. On p. 336 Edward Mellanby, Secretary of the Medical Research Council, is reported to have rejected violently the proposal by Chain that patents should be taken out on penicillin. Twenty years earlier Mellanby had missed discovering the antirachitic effect of irradiating food stuffs by ultraviolet light because he did not remove the glass filter from the UV source which he had borrowed and used. Shortly afterwards Steenbock took out the highly profitable US patent #1,680,818. Was Mellanby's reaction generated from medical ethics or personal experience or a possible combination of both? Sir Henry Dale, President of the Royal Society, took the same view as Mellanby, despite his long experience of the commercial firm, Wellcome. Eight years earlier while with the assistance of Feldberg he was developing the proof of chemical transmission of nerve impulse to muscle, an intern (Mary B. Walker, *Lancet* 9 (1934) 1209) showed the clinical effectiveness of eserine and prostigmine in myasthenia gravis, thereby opening up a whole new field for the pharmaceutical manufacturers.

That Florey was capable of due appreciation of ideals and the worth of people is borne out by many instances reported in the book. But, curiously, fashionable expressions which seem to go counter to such motivation are taken seriously by the author. In Canberra in 1967 Florey stated 'People sometimes think that I and others worked on penicillin because we were interested in suffering humanity — I don't think it ever crossed our minds about suffering humanity.' This was spoken in Canberra when a number of ingenuous academics were pressing to bleed some of the funds from the scientific departments in order to set up a clinical department. This was the reverse of what happened in Oxford when the Nuffield Institute for Medical Research was established. While the proponents of this were seeking the approval of the University, words were spoken which appeared to promise substantial funds for the basic science departments. Florey had a low opinion of the value of clinical research and he combated strongly the proposal to direct money away from basic science to suffering humanity. In this instance quoted he conveniently overlooked his expressions of despair at clinical impotence when an intern and writing to his future wife while she was an intern.

Another instance of where the attitudes of people involved need to be taken into account is the varied statements about when Florey first became aware of Fleming's paper on penicillin. In the June 1979 issue of *Trends in Biomedical Sciences* Chain states: 'Before writing up this paper (on lysozyme) we (i.e. Epstein and Chain) made the usual survey of older literature for bacteriolytic substances. In this way we came across Fleming's paper on penicillin'. In the first issue for 1979 of the same journal Chain states: 'I told Florey about my finding in the literature of penicillin. Though he never mentioned the word penicillin to me during our frequent conversations, he appeared to be familiar with the sub-

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stance and asked me whether I was aware that in 1933 a group of well known biochemists, Raistrick and two of his colleagues, had worked on it but could not make any progress because of its instability . . .'. This is vintage Chain — Florey did not mention the word penicillin but obviously knew the literature on it! Perhaps a better example is Heatley's development of the back extraction system of purification of penicillin. In the same article Chain states 'a preparation which was extracted at acid pH after cooling with ether and back into water and obtained in a dry state of lyophilisation, a *method worked out by Heatley and myself*.' Heatley, quoted by Bickel in *Rise up to Life* (p. 91), reports that when he proposed back extraction into water, Chain said: 'Then, if you think it will work, why don't you do it yourself? That will surely be the best and quickest way to show that you are wrong.' Heatley proved it to be the key to overcoming the instability that had defeated Raistrick and was frustrating Chain. The author appears disinclined to use direct speech in dealing with Chain's appropriations and Florey's long toleration of them until the final break. It was typical of Florey that he never published any correction of Chain. Those who knew him well, know the hurt it would have given him, and even that tears might have been shed.

The accounts of Florey's progress in the study of microcirculations, lymph systems, white cell reactions and so on are clear but too short to do justice to his work. In his summing up the author indicates that Florey had only one original idea — the study of secretion of mucus (p. 362). The question arises as to the definition of original. If it must be completely *de novo*, as the author suggests, there has probably never been an original idea. If from considering many observations on an organized set of circumstances, or the reports of such, a person perceives a new possible relationship between components one could contend that that is an original hypothesis to be tested by the strict rules of the field. In the case of penicillin his hypothesis stemmed from it being lethal to pathogenic bacteria *in vitro* and on animal surfaces and not toxic when given systemically. The hypothesis naturally arises that penicillin may be lethal to pathogens *in vivo* if given systemically and not toxic to the host and therefore curative. The whole of Florey's efforts in the antibiotic field were based on this proposition and the experiments went to prove it, and Epstein (see *J. Am. Med. Assoc.* 124 (1944) 1219) and I attest his interest long before Chain spoke to him of therapy. While there may be extralogical leaps, as with Kekulé's carbon ring hypothesis, the criteria of testing of causative relationships as set out by Mill should not be discarded in favour of instinctive action as proposed on p. 319. It provides a checklist which impetuous instinct may need.

This notion of instinctual capacity comes up in the discussion of Florey's 'uncanny' knack of picking fruitful fields for his researches. The author refers to his skill in manipulation, to his search around the world for instruction in techniques, but does not bring to notice his insatiable reading. In any field in which he was interested his reading went back to the earliest text, be it in Oxford, Cambridge, the Royal Society of Medicine or the British Museum. Consequently any proposition that Chain in 1938 introduced him to Fleming's 1929 paper on penicillin is not credible, as instanced above. A thorough knowledge of antecedents saves a great deal of time and is fruitful of ideas. It would appear probable that when Chain did at last read Fleming's paper, being unwarrantably self-confident, he persuaded Florey that, despite Raistrick's failure, 'penicillin can be easily prepared in large quantities' (p. 299). Against the background of Chain and Epstein's success with lysozyme Florey believed him, and he launched the penicillin program. Four years later, with the essential intervention of Heatley and Abraham, and American finance and technology, this was achieved. The author's understatements leave the reader a lot to fill in as to the character and social relationships of these principals in the performance. It was true to type C.P. Snow.

But the author's record of the development of Mellanby from a congenial helpful colleague when a fellow professor at Sheffield to a domineering skinflint when he became Secretary of the Medical Research Council is clear enough. The quotations and comments on pp. 297, 300, 301, 322 and 323 show the deterioration of attitude. It is an example of which any body which places one man in a financially dominant position over his intellectual peers or superiors for a long time should take notice.

The author also clearly sets out the facts about private industry's legendary voracious appetite for innovatory 'progress'. On pp. 319, 324 and 340-3 he tells the sorry tale of industry without insight being brought eventually to action only by government edict and guaranteed costs. And the treachery of Moyer (p. 342) does not improve the after taste. The inclusion of an

account of Florey and Abraham's work on cephalosporin and the cephalosporin trust would have improved the finish.

This is a book which everybody who wishes to have insight into the highly complex world of the intellectually competitive A grade players should read. There is a great deal to be learned from it about the method of biological experiments. The author has gone to enormous pains to authenticate his every statement of fact. The availability of Florey's private correspondence with his first wife is made the vehicle for recounting events and building a personality analysis. Perhaps all Florey's communications should have been read keeping in mind the true statement on p. 301: 'He was, by nature and experience, anxious to keep his major ideas to himself until he had worked them out'. Allowing for secrecy and adaptability as mentioned above, and Florey's statement (p. 304), 'we all know that when we compose a paper setting out discoveries we write it in such a way that the planning and unfolding of the experiments appear to be a beautiful and logical sequence but the facts are that we usually blunder from one lot of dubious observations to another and only at the end do we see how we should have set about our problem', the author has evoked a fairly true image of his subject and his environment. It is in the class of Eve Curie's biography of her mother, Marie, and Vallery Radot's biography of Pasteur. The pity is that it stops halfway.

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S. Cockburn and D. Ellyard, *Oliphant: The Life and Times of Sir Mark Oliphant* **(Axiom Books, Adelaide, 1981), 334 pp. ($19.95).**

Mark Oliphant was one of the small group of researchers to work with Ernest Rutherford in the Cavendish Laboratory, Cambridge, where the new discipline of nuclear physics was established in the late 1920's and early 30's. He left Cambridge for Birmingham University in 1937. There he became his own master as Professor of Physics and Head of the Physics Department and so able to choose his own colleagues and select his own lines of research. At the time I was a student completing an Honours course in that Department, and had the benefit of the first lectures from the new Professor on topics in nuclear physics.

On completion of my examinations I was awarded a research scholarship and became Mark Oliphant's first research student in Birmingham. So began what was to become a lifelong association with this rather special Australian, an association which has been very close and, indeed, continues to this day.

When the authors contacted me during the preparation of this book for information and discussion I really did wonder what kind of a job they could make of it. For Oliphant, more than most men, is a very complex personality and prone to be misunderstood. But let me say at the outset that my fears were unfounded, for the authors have done a remarkably good job in capturing the complexities of personality, and motivation, of their subject. His life in physics has touched most of the developments of the last sixty years, which led to the technological revolution, which, in turn, has given us the highest standard of living mankind has yet enjoyed. They consulted widely with contemporary physicists and other personalities who had interacted with Sir Mark in Britain and the USA as well as here in Australia. They have built up a picture with thoroughness and completeness which allows them to illuminate the many facets of Mark Oliphant's being and the environment in which he has lived and worked. Characteristically Sir Mark, himself, gave them great help, insisting that the account be a full representation 'warts and all'. And it is that reality which brings this book alive and makes it informative and enjoyable reading.

The book is not intended for physicists, indeed many will find its potted versions of scientific developments irritating, and sometimes superficial, and even patronizing. It is aimed at the intelligent layman and meets that requirement admirably.

The early chapters of the work, describing family antecedents and the early years of Sir Mark's life in Adelaide are amongst the most interesting in the book, and throw much light on life in South Australia at that time. They set a high standard for what is to follow.

The real story, however, begins with Oliphant, newly married, winning an 1851 Exhibition and sailing for England with his young bride to undertake post-graduate research under Rutherford in Cambridge. Those early years in the Cavendish Laboratory left an indelible stamp on Oliphant, and established him

as one of the early founders of what was to become the most important discipline of the physical sciences, affecting all of our lives deeply. The rapid development of understanding of the nucleus and nuclear reaction physics in the light elements was exciting and established the Cavendish, at that time, as the leading research centre in the world. It culminated in the discovery (not in the Cavendish as it turned out) of fission and the dawning of the nuclear age, just before World War II.

By this time Oliphant had moved to Birmingham where he initiated research in nuclear physics and was building a large cyclotron for nuclear work. But the war broke out and this work had to be postponed until it was over. In the meantime all British scientific effort switched to military research and Oliphant accepted a contract from the Admiralty for work on radio direction finding — later to be called radar.

In the laboratory we went to work designing new centimetre-wave transmitters of high power. We started with the recently developed klystron but a major breakthrough led to development of the cavity magnetron and this produced the highest pulsed powers a cm-wave oscillator had ever generated. The discovery of 'strapping', to compel the device to operate at a single wavelength, meant that the Birmingham group had given to the armed services the basis of long range, high precision radar. This was to play vital roles in the Battle for Britain and the Battle of the Atlantic.

Towards the end of that development, under Oliphant's encouragement, I moved to join Robert Frisch in early experiments with uranium to elucidate the possibility that a nuclear bomb might be technically achievable. Oliphant by now was on the MAUD Committee, the group of most senior British scientists, charged with overseeing research into the uranium possibilities. Much effort in the Birmingham laboratory was swung into this field of research. His own personal energies went into the possible conversion of the partly completed cyclotron to separate the isotopes of uranium. Before this could be achieved Churchill and Roosevelt, meeting in Ottawa, decided that all uranium research in Britain should be moved to North America, out of range of German bombs. Oliphant joined one of his closest friends, Ernest Lawrence, at Berkeley where the technology of isotope separation by magnetic means was perfected and put into large scale operation at Oak Ridge.

For technical reasons I was assigned to the new laboratory at Los Alamos and so, for the remainder of the war, he and I were separated. But in correspondence, toward the end of the war, he told me of new ideas he had for a high energy accelerating system using a variable magnetic guide field and a varying frequency to match the particle bunches in orbit. This is the basis of the modern synchrotron and there is little doubt that Oliphant was one of the earliest, if not the earliest, to conceive of this idea. He did not publish it, however, and two people — McMillan in the USA and Veksler in the USSR — who had similar ideas independently, and at about the same time, did. They are usually credited with the invention of this machine and Oliphant seldom gets a mention. It seemed to many, who knew the details, somewhat unfair that Oliphant got no credit for his part in the development. But he accepted the position gracefully and without complaint. He proceeded to complete the cyclotron, started before the war in Birmingham, and commenced erection of a 1000 MeV synchrotron there to allow the team to enter the new research field of high-energy physics. At about this time however, here in Australia, the idea of a National University to be built in Canberra was under discussion. Oliphant, with two other distinguished Australians, Hancock and Florey, and a New Zealander, Firth, all in England were asked to act as UK Academic Advisers to the project. And eventually two of them, Hancock and Oliphant, came out to Australia to act as Directors of the Research Schools of Social Sciences and Physical Sciences respectively. Oliphant thus came 'home' in 1950 and, in subsequent years, he became very well known throughout Australia. Today his name is a household word.

The second half of the book details his career at the ANU and, after retirement from the laboratory, his term as Governor of his native State of South Australia.

No thumb-nail sketch in a review such as this can bring out Olly's (as he was affectionately known by his 'boys') most characteristic quality. He has always spoken out firmly and fearlessly on matters about which he feels strongly. And he has never cared who liked, or who disliked, what he said. This laid him open to exploitation by the media, who have often been guilty of overplaying his views, with judicious doctoring of his statements. This situation led the authors to

what I believe was an error of judgment, towards the end of the book.

During their work they gained access to a personal and confidential letter from Sir Mark to the then Premier of South Australia, Mr Dunstan. It expressed reservations about the appointment of an Aborigine as Governor of the State — very sensible and thoughtful reservations which most of us would endorse. This confidential letter was printed in its entirety in an Appendix to the book.

It was, of course, picked up by the media and presented, quite incorrectly, as a racist statement. This is just the kind of thing I meant when I said, earlier, that Mark Oliphant is prone to be misunderstood and misrepresented.

But in spite of this 'wart' the book is well done and portrays one of Australia's outstanding sons in a realistic and highly readable account.

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P. Goodman (Ed.), ***Fifty Years of Electron Diffraction*** **(D. Reidel, Dordrecht, 1981), 440 pp. ($57.00).**

The subject of electron diffraction was born in 1927. The fifty year anniversary was marked in various ways but most notably by a conference in London in 1977 organized by Professor M. Blackman of Imperial College. At that meeting the members of the Commission on Electron Diffraction of the International Union of Crystallography (IUCr) met and the idea of the present volume was discussed. Stimulation came from the highly successful volume on *Fifty Years of X-ray Diffraction* edited by P.P. Ewald and published in 1962 for the International Union. Again, for this volume, the IUCr sponsorship was sought and received and the Australian member of the Commission, Peter Goodman of the CSIRO Division of Chemical Physics, became the Editor. He, with a panel of regional and special subject editors, has produced a volume which in no way suffers by comparison with its predecessor.

During its fifty years of history, electron diffraction has passed from prominence as a major event in the growth of ideas in physics, to relative obscurity as a minor and scarcely reputable branch of crystallography and back to be an important component of the present explosive growth of the technology of solid state science. Each of these phases of its history is the subject of a section of this book. It is in the transition from the second to the third of these phases that the Australian contribution has been significant.

The justification for a review of the book in this Journal is partly the significance of the Australian contribution. However, it is probably more appropriate in the present context to address the question of why this subject, in particular, should have grown in Australia and what were the factors which nurtured its growth.

The beginnings of the subject were remote from Australia and Australian science. The initial observations on the diffraction of electrons by the periodic arrays of atoms in crystals were hailed as the first clear proof of the ideas of wave mechanics proposed by de Broglie in 1923. While the accounts of the experiments by Davisson and Germer at Bell Laboratories and by Thomson and Reed in Aberdeen are well described by science historians in the opening chapters, in the absence of any survivor from that fine group of experimentalists, it is fascinating to find a short article by de Broglie himself, still alive and still deploring the fact that his original ideas have been perverted. Schrödinger, he suggests, made the fundamental mistake of abandoning the concept of the electron as a localized particle carried along by a wave.

Whether based on a misconception or not, the idea that electrons acted like waves which could be diffracted by crystals captured the imagination of many researchers and a few of these persisted in seeking applications where the special characteristics of electron diffraction by crystals could provide unique advantages over the better established X-ray diffraction methods. Almost immediately the subject split into three branches. Low energy electron diffraction (LEED), following the initial work of Davisson and Germer, almost died because of serious experimental difficulties until revived by Germer and others in 1960. Diffraction by gas molecules started in Germany but developed rapidly in the USA. The diffraction of higher energy electrons (20-50 keV) by thin films of solids and solid surfaces sprang to life particularly in England, Japan and Sweden but faltered for lack of control of the experimental variables and lack of any apparent correlation of experimental

observations with an accessible body of theory.

It was in the post-war period that the great developments of electronics and electron optics (and electron microscopy in particular) allowed the techniques of electron diffraction to be refined to the stage where definitive experiments could be conducted, stimulating a development of the theory in a form adequate for direct description of the experimental results.

It is interesting to speculate why these developments should have been initiated in the early post-war years mostly in isolated corners of the scientific world; in Japan, in the USSR and in Australia.

Partly, the reason was economic. In those countries the electron diffraction cameras costing a few hundred dollars, or entirely hand built in the laboratories, were accessible, whereas the accelerators and reactors which dominated the thinking in the physical sciences in the USA and Europe were not. Scientists of high calibre turned their attention to the available equipment.

In Australia, the start was made with such lowly equipment. Then it was very greatly due to the scientific intuition and foresight of A.L.G. (Lloyd) Rees that the new techniques of chemical physics, including electron diffraction along with electron microscopy, X-ray diffraction and spectroscopy, were introduced into what was then the CSIR Division of Industrial Chemistry. The equipment he introduced was the best available in the world although still very cheap compared with the particle accelerators. It provided a group of young scientists with the opportunity to compete effectively in a limited part of the world scientific scene.

Until the late 1950's all of these isolated electron diffraction groups developed almost independently. Then as international travel and international meetings became more common and accessible, the interactions were vigorous and highly productive after the initial shocks and adjustments. There were several key meetings for electron diffraction. The first major international electron diffraction meeting was held with the IUCr Congress in Montreal in 1957. Lloyd Rees helped initiate the idea and got me the job of program chairman. A graphic account of this occasion is provided in the article by Lorenzo Sturkey. Then, as Alex Moodie recounts in his article on current diffraction theory, the level of understanding of the whole field was enormously advanced by the remarkable meeting in Kyoto in 1961 when many of the loose threads of divergent ideas were brought together.

The theme of productive international collaboration recurs again and again in those chapters of this book which make up its largest section, describing the developments of the many research groups during the last 30 years or so. When the Japanese people began to travel in the late 1950's, to Australia, to Europe and later to the USA, their impact was clear and striking. For the gas diffraction work, repeated references emphasize the value of the interactions between the USA, Norway and, more recently, the USSR and Japan.

Gradually in the 1960's electron diffractionists came to mingle freely, like everyone else, as part of the international jet set of the scientific world. However, the legacy of the earlier days persists. The Australian groups are still prominent, as evidenced by the contributions to the final section of this book, forming a group of articles summarizing the present state of the subject. The strong Japanese contributions in electron diffraction provided a basis, at least in part, for their more recent dominance in many areas of electron optics including the manufacture and application of electron microscopes. The USA is still strong in gas diffraction and in LEED which was revived there with powerful new techniques in the 1960's, but is still struggling to lift its standards in high energy electron diffraction.

There are many such implications which can be read into this book by those in the field. There are many revelations of personal biases and many expressions of idiosyncrasies and individual attitudes and style which are fascinating to those who know the many authors. For a student entering the subject there is a wealth of information to broaden the outlook. For the science historian there is a careful and comprehensive documentation of the development of one small area of science. Our thanks are due to all the many contributors to this volume, but principally to Peter Goodman who, to my personal knowledge, took enormous pains and spent a great deal of time and effort to produce this valuable compilation and who is, incidentally, one of the few prominent electron diffractionists not included in the fine collection of photographs in the book.

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R. T. M. Pescott, ***The Royal Botanic Gardens Melbourne: A History from 1845 to 1970*** **(Oxford Univ. Press, 1982), 212 pp. ($29.95).**

Of the multitudes who throng Melbourne's beautiful Royal Botanic Gardens few can be aware of the convoluted history of its creation in the 19th century, traced in this book. It began with use of Pleasant or Batman's Hill as a 'favourite resort of the public in the early days' of the township and ends with changes made to the Gardens in the early 1970's. This is the first book to deal extensively with the history of the Gardens. In Sir Frank Clarke's book from 1924, *In the Botanic Gardens: Their History, Art and Design, with Stories of the Trees*, the history occupied a mere three of the 52 pages. Crosbie Morrison's *Melbourne's Garden* in 1946 followed the same pattern, devoting only 23 of its 158 pages to history, the rest to description and eulogy of the Gardens itself and its inhabitants, humans and birds. In 1974, Pescott published his *W.R. Guilfoyle*; the second half of it, devoted to Guilfoyle's involvement with the creation of the Gardens, is re-worked as Chapter 4 of this book.

The Introduction contains somewhat conflicting definitions of the purpose of a Botanic Garden, but 'all agree on the role of the collections of plants for scientific study and educational purposes'. Sir George Taylor, then Director of Kew, added the phrase 'sources of aesthetic and intellectual delight . . . making the science of botany the handmaiden of horticulture'. Much of the conflict which developed between Mueller, the first Government Botanist of Victoria, charged in 1857 with (honorary) Directorship of the 'Botanic Garden', and the Government, which led to his dismissal from that post in 1873, might have been avoided had agreement been reached at an early stage on a definition, acceptable to both parties, of the purposes and aims of a Botanic Garden. Mueller was, of course, much better equipped to be a botanist than a gardener. Brought up in the bleak flatness of Schleswig-Holstein, he would have gained little knowledge of the wider sweep of horticulture either there or at university in Kiel (where, *pace* Pescott, he trained as a pharmacist, not a chemist). He would have been familiar with the formal layout of the 'system gardens' of the time, in which plants were grown in beds arranged in taxonomic order, and of 'gardens of simples', in which herbs and medicinal plants were displayed, also in flat beds. He would have acquired no first-hand knowledge of the great traditions of landscape gardening which developed during the 18th and 19th centuries, particularly in Britain, into an acceptable art form, of which Melbourne's Gardens is a supreme example. Not that Germany itself lacked great gardens, such as those at Schloss Nymphenburg, Heidelberg and Schönbrunn, but they, like those in France and Britain, were private domains, the appurtenances of great houses. Melbourne followed the example of Kew in developing a great garden for the public on the grand scale hitherto reserved for the wealthy occupants of an adjoining great house and their friends. Melbourne had the advantage over Kew in its aspect and climate and its rolling terrain. But the purpose of a superbly landscaped garden is hardly to be equated with that of a Botanic Garden unless its art is accompanied by at least an equivalent scientific intent and effort. How far does the Royal Botanic Gardens in Melbourne fulfil the function implied by its name? As the book shows, the history of its development moved its function further and further away from Mueller's original intention until, by 1896, the last vestiges of his work on it (except for the oak lawn plantings and a few pines) had disappeared.

With Mueller's dismissal in 1873 and the appointment of Guilfoyle, the real creator of the garden landscape as we know it, there was a sudden change in emphasis from science to practical horticulture, from the educational to the aesthetic. Guilfoyle's father, Irish born, came to Australia from England with a reputation not only as one of that country's leading nurserymen but as an outstanding landscape gardener; he had trained under Knight at Chelsea and he passed on his training to his son, William Robert, who was brought up in Redfern and Double Bay, Sydney. The boy grew up surrounded by the wealth of plants in his father's 'Exotic Nursery', a 'surprisingly fine collection of palms and ferns' (plants he later used with great success in landscaping), 'trees and shrubs of all kinds' including 'one hundred and thirty two species of Conifers and Taxads'. But the pride of the nursery were the Jacarandas and Camellias, plants introduced by his father. W.R. received botanical advice and instruction from William Macleay, John McGillivray and the Reverend William Woolls and became a proficient field botanist. He ex-

perienced the lushness of the tropics and made collections of plants there as a participant in the five month punitive expedition of the gunship HMS *Challenger* to the rebellious South Sea Islands. He thus brought to the Gardens a wealth of experience in cultivating introduced plants, of the dramatic effects to be produced by tropical plants in hot-houses, and of the creation of landscape by judicious selection and planting of trees and shrubs. Especially important were his sweeping lawns of drought-resisting buffalo (*Stenotaphrum*, not *Hemitaphrum* as in the book) and Indian couch grass which, thanks to the development of the lawn mower, invented in 1830 by E. B. Budding, enabled Guilfoyle to economize on labour and eliminate 'untidiness', two of the bases of the friction caused by Mueller's management policies.

It is clear from Pescott's narrative that, although Charles Joseph La Trobe, the first Governor of Victoria had intended Melbourne to have a Botanic Garden *per se*, not, like Sydney, 'such only in name . . . a public promenade or place of recreation', by 1871 the Government was being advised by its Board of Enquiry 'that the gardens be not only 'Botanic' but also horticultural in their character . . . to produce pleasing and congruous effects . . . with a view to the general harmony of the whole'. That recommended change reflected, no doubt, the explosive growth of the population of the city, creating a demand for a recreational resort for its citizens. While illustrating some of the social events — such as the Eight-Hours fête — held in the gardens, the book does not explore the demographic and social pressures of the period on the plan for the Gardens, but deals with the deliberations of committees which were the instruments of those pressures; the history of the Gardens is not put into the context of the history of the State and the changing concepts of urban living standards and leisure in the 19th century. Nor is there mention of the reciprocal effects of the Gardens themselves on the development of social concepts in the 'Garden State'.

Mueller's views on art have not come down to us, but his concepts of the Gardens as a place of recreation embody much that is traditionally German, of delight in gloomy pine forests and the contrast between the orderliness of rectilinear beds and paths and the wildness and chaos of the 'Urwald' (as exemplified in the view of the 'unkempt' lagoon in his time, p. 50). Guilfoyle's traditions were, in contrast fully English: the uplift of expanding vistas, the pleasure of Hogarth's curvacious 'Line of Beauty', the restfulness of manicured lawns and the stateliness of great specimen trees.

With Mueller's relegation to control of the herbarium, its work became almost the sole scientific aspect of the Gardens, a situation which has continued to the present time. Mueller's botany was analytical rather than synthetic; he dealt more happily with the smaller units of taxonomy than with the larger. He made no contribution such as those of the de Candolles, Bentham, Hooker and Gray to the construction of the systems of natural classification which, in the 19th century, replaced the old Linnean system, thus setting a tradition in Australian taxonomy which still prevails. Mueller was happy to adopt Lindley's *Vegetable Kingdom* as the basis for the lay-out of his system beds in the Gardens. The scientific work of the Gardens receives scant mention in Pescott's book, restricted to passages in its last two chapters. The Gardens produced no one of the calibre of William Aiton, the incomparable cataloguer of Kew. Nor has the herbarium produced the string of first-rate botanists that Kew has produced (Ewart's FRS was for his plant physiology not his *Flora of Victoria*), nor emulated its experimental Jodrell Laboratories. The work of the herbarium goes on in much the same way as in Mueller's day. Devoid of a full range of microscopical instruments, including those for electron microscopy, of chemical and physical analytical instruments and a computer, all of which are now viewed as essential to the progress of taxonomical investigation, the work of the herbarium will become increasingly archaic, as it will in other State herbaria in Australia. No evidence is offered by Pescott that the Botanic Gardens presently fulfils any of the more specific functions of the definitions in the Introduction — the 'trial, selection and hybridization and distribution into horticultural commerce' of new plants, 'the acclimatization of economically valuable plants' or 'comparative study of the plants' grown in the Gardens. In his day as Director, Mueller, who regarded the Gardens as a trial ground for commercially valuable animals as well as plants, evidently did his best to have the Gardens serve those functions.

From 1957 to 1970, Pescott was Director of the Gardens, and thus in a privileged position to be its historian. The book is lavishly illustrated and the various plans, many in colour (but often lacking explanation), and the panoramic photo-

graphs taken in 1876 and 1901 are invaluable. But the text, particularly that dealing with the period from 1909 (the date of Guilfoyle's retirement) onwards, reads in part like a typical public servant's report of his Department's progress, compiled from Committee minutes. The pages are filled with details such as the date of closure of the advertisement of a post, quotations from Board of Works letters concerning water supply and resolutions of the Advisory Committee such as those on the adoption of notices: 'Keep your Gardens Tidy', and 'Save our Birds' (presumably including the blackbirds which, despite Pescott's assertion to the contrary, are not recorded as keeping down slugs and snails). Perhaps these humdrum events are all there is to report. Except for the abortive plan for an Arboretum, the act of creation was complete in 1909; since then the changes to the Gardens have been cosmetic and its history mundane, its Directors lacking the calibre of Mueller and Guilfoyle. The heady days when Guilfoyle was transplanting trees 6 m high (832 in one season, December to March, with a loss of only six) with the aid of a 'two-wheel dray, two horses and manpower' and completing the job of converting the old lagoon into the flood-protected and landscaped lake were long since gone.

The Gardens retain their beauty and many of its trees are now fine, mature or even overmature specimens. But pleasing as they are in their majesty, botanically they are almost inaccessible — their scientific usefulness is past, except possibly as seed sources. As scientific resources, the potential of much younger Botanic Gardens, such as those of Perth and Canberra is now incomparably greater than that of the older gardens in Melbourne and Sydney. The younger gardens are also investing heavily in research on the newer horticultural techniques involving aseptic culture of embryos and tissues. Pescott's book does not deal in such comparisons; it parades the trivia of the history of the Gardens in Melbourne uncritically along with the great moments of triumph. The history of the Gardens is treated in isolation as though, apart from having to deal with various Boards over services such as roads and water mains, and the ponderous deliberations of Committees, it has little connection with the community it serves and even less with the scientists and educationists for whom La Trobe intended it. As an illustrated chronicle of events we can recommend the book to the scholar in search of sources and dates, although he will find its index quite inadequate. For the general reader the illustrations and the chapters on Mueller and Guilfoyle are recommended, but not much else.

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Edward Kynaston, *A Man on Edge: A Life of Baron Sir Ferdinand von Mueller* **(Allen Lane, Melbourne, 1981), 389 pp. ($24.95).**

Reading the impressive list of people and institutions acknowledged at the front of this book I approached the lengthy text with an anticipatory awe and reverence for the scholarship that I hoped would be devoted therein to a long-awaited life of one of Australia's leading and most productive men of science of the nineteenth century. For Mueller was undoubtedly the Joseph Banks of his time and place. No matter where he looked, Kynaston tells us breathlessly in the Introduction, Mueller turned up 'as an explorer, botanist, geologist, biologist, geographer, forester, agriculturalist, chemist, pharmacist, taxonomist . . . He seemed to have been an encyclopaedist of the natural sciences, something of a Teutonic pedant who was saved from his pedantry by his intense practicality' (p. 1). Somehow, this writer of sometimes 'several thousand letters a year as well as books, learned papers, treatises and pamphlets' had got 'irretrievably concealed behind all the pomp and pretence and platitudes and hypocrisy' of his Victorian age (p. 2). And, after all, with the superb, critical, measured scholarship of Elsie M. Webster (*Whirlwinds on the Plains, Ludwig Leichhardt — Friends, Foes and History*) so recently behind us, did we not have a right to expect in Australian scientific and exploration biography the highest possible standards? No longer is the 19th century Australian scientific experience, even of Germans and 'foreigners', masked by the prejudice, shallowness, arrogance and ignorance of former years. Where Marcel Aurousseau had led with *The Letters of F.W. Ludwig Leichhardt* in 1968, others have followed. Consequently those 'universal geniuses' of Lady Jane Franklin, the German and German-speaking Middle-European scientists and explorers who so thickly and

productively (and, admittedly, sometimes with great eccentricity) enrich the pages of our scientific heritage from Captain Cook onwards, are being unravelled and exposed. Even the ravages of time and psychohistory have not blunted forever our growing perception of the real contribution of Poles, Austrians and Germans to Australian and Pacific science. But Edward Kynaston, I fear, has temporarily — and for Mueller most of all — retarded our hard-won progress. Measured by almost any yardstick of modern scholarship (the author had grants from the Literature Board of the Australia Council to initiate the research on Mueller's life in Germany, and to bring the writing to a conclusion) this book is, mildly put, grossly disappointing.

The salient facts of Mueller's life are already well known from the earlier —although now since overhauled — biography by Margaret Willis (*By Their Fruits: A Life of Ferdinand von Mueller, Botanist and Explorer*) and the botanico-historical writings of, for example, J.H. Willis, J.M. Powell and Lionel Gilbert. There are, too, C. Daley's very useful biographical article (*Victorian Historical Magazine* 10 (1924) 23-75) and his printing of a large portion of the *Flora Australiensis* correspondence and controversy between Mueller, the Hookers and Bentham in the *Victorian Naturalist* (44 (1927)). Deirdre Morris in her biographical entry for the *Australian Dictionary of Biography* has adequately distilled the facts of this many-faceted public, yet intensely private figure. What then has Kynaston added?

In essence very little beyond some fulsome writing. Despite the promise of many letters, archives and original and secondary works consulted, *no* documentary sources are listed, although many are cited in the text. There is no critical evaluation of the scholarship, good, bad or indifferent, preceding Kynaston's 'discovery' of *Terra von Mueller* (as he terms the object of his biographical researches). Mueller's life, we are told, 'is obscured a good deal by tens of thousands of facts, far too many facts . . . The truth of human existence is always subjective, internal, delicate as mist' (pp. 369-70). True, but is it not the biographer's art — a difficult one albeit — to penetrate or, at least, to lift some of the mist? This biographer, lacking historical objectivity and mastery of the period, either in general or scientific terms, and harbouring still, I suspect, a certain ambivalence towards his complex subject, fails to penetrate convincingly the facts, the times or the life. In consequence the appreciation of Mueller's many scientific, ecological and exploration achievements also suffers. Kynaston is honest (or bland) enough to admit at the close (p. 375): 'The facts need disturb no-one's ignorance. The mystery of the man remains'! Indeed it does.

This book is a mixture of historical romance, unconvincing psychohistory and exceedingly bad historical scholarship. Kynaston sometimes writes well but mostly, it seems, for effect. Leichhardt, we are told, 'was a desert dust storm blowing in Mueller's mind' (p. iii). Mueller never returned to Europe because it represented for him 'the focus of death, disease and insecurity' (p. 136). The German became involved in 'the almost Goon-show-like fantastic comedy of the acclimatisation of the species, manipulated and manoeuvred most likely by the wily, theoretical Professor McCoy . . . an amiable ass' (pp. 143-4). Kynaston's chapter on 'Darwinism and Dogma' (pp. 170-82), itself a complex and well-researched topic, is shallow and mindlessly vituperative, wandering cavalierly between the scientific ethics of 19th century biological controversy and 20th century physics. Throughout this book there is also discernible an apparent unease between the author's adopted country and its countrymen's perceived foibles. It abounds, under the guise of historical biography, with contemporary social commentary. Take, for instance, his random remarks on Australian egalitarianism (pp. 271-2), Colonial Victoria ('a very small pimple on the backside of the world', p. 273) and the (modern and historical) alleged dislike by Australians of real meritorious achievement and their preference for the 'tradesmen's greed'. Mueller was dismissed as Director of the Melbourne Botanic Gardens in 1872 after a concerted campaign in 'the silly season for Australian newspapers', during the summer holidays (pp. 293-4).

Biography, like history, requires conviction and commitment. But, even if we dubiously allow Kynaston his prejudices, there is no excuse for repeated misidentifications, non-identification of key scientific figures in colonial Australia and, above all, for the perverse hiding of his historical sources and tools, to which later scholars will rightly claim access. It is known, for instance, that many of Mueller's papers at the Botanic Gardens were destroyed. Where then can we go for our Muelleriana? Kynaston is silent. For some sources we can consult the 'Mueller Bibliography' by Churchill, Muir and Sinkora in *Muelleria* (Vol. 4, July 1978). In

Kynaston's book, which does not venture beyond generalities in assessing Mueller's science, there is not even a list of his major monograph or serial publications. The 'Short Bibliography' lives up to its title (pp. 377-9); there appear no citations of any recent monograph or thesis work on Mueller and his period, and references to only a few articles. Such, however, do exist.

For romantic, sometimes racy reinterpretations of the Burke and Wills episode; Mueller's abortive courtship with Ephemia Henderson; his alleged obsessions with Leichhardt and personal disease; the *Flora Australiensis* controversy; his devotion to Victoria, botany, exploration, and to his beloved Gardens and his unfair dismissal as their director, the book is readable if factually irritating. It is, indeed, another 'A Life' of Ferdinand von Mueller. We still, alas, await 'The Life'.

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