Audra J. Wolfe, *Competing with the Soviets: Science, Technology, and the State in Cold War America*. Baltimore: Johns Hopkins University Press, 2013. Pp viii+166. ISBN 978-1-4214-0771-5. £10.50 (paperback).

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The British Journal for the History of Science / Volume 46 / Issue 04 / December 2013, pp 729 - 731
DOI: 10.1017/S0007087413000800, Published online: 13 November 2013

original that this paper should also be published. Church reviewed the paper and confirmed the editor's assessment; furthermore it convinced him that Turing should come to Princeton, where – not least due to the exodus of scientists from Germany after January 1933 – the elite of mathematics, logics and physics had gathered.

After a stay in Princeton beginning in September 1936, Church persuaded Turing that he should write a PhD thesis under his supervision. Turing came back to England in the summer of 1937 and managed to get funding for this venture, returning in the autumn of the same year. He took Gödel's incompleteness theorem as the point of departure and essentially devised an 'iterative' construction of logical systems. Starting with a basic set of 'ordinal' formulae L1, which need to be checked by intuition, one would then add on top of that another logic L2, and so on, arriving at a logical system that at every step would be more complete than the previous one. Turing's own findings in his 1936 paper basically reaffirmed Gödel's position that there will always be at least one statement that cannot be proven. His PhD work then introduced an approach to successively narrow down the set of provable logical systems to as few unprovable statements as possible by iteratively joining axioms 'seen intuitively to be correct' (p. 91). He thus tried to mitigate his own findings on the limits of computability by offering an approach to compute as much as possible, hopefully leaving only the matter of self-referential statements as uncomputable (or rather, undecidable). In his PhD thesis Turing also presented the concept of an 'oracle – or just 'oracle' (pp. 52 ff.) – a black box kind of machine that can solve any kind of problem, even undecidable ones. Whenever a given machine fails to find a solution to a problem it can consult a more powerful machine (the oracle) which will produce an answer. The oracle functions as a placeholder, i.e. as a partial problem to be solved at a later stage. The more ordinal formulae have been joined, the less often the oracle needs to be consulted, ideally leaving only the undecidable problems for the oracle to solve.

As mentioned, little has been published on Turing's PhD thesis beside some internalistic articles in computer-science journals. Turing's thesis could not only serve as an important episode in the history of computer science, it could also help in understanding the philosophy of it. However, Appel's introductory chapter gives only a glimpse of this, and by itself the facsimile of the thesis will only be accessible to readers with a good knowledge of mathematical logics. The book's usefulness for the wider history-of-science community will thus be rather small. It must also be asked how much sense the reproduction of the thesis makes, given that the submitted typescript is not so very different from the printed version in the *Proceedings of the London Mathematical Society* (the changes requested by the editors were rather minimal), copies of this journal are available in many libraries, and it has been reprinted in the volume of Turing's *Collected Works* (2001) devoted to mathematical logic, to which Feferman also provided an introduction. It would have been much more interesting had Appel or Feferman discussed more of Turing's interactions with the other scientists present in Princeton. Most of what is presented in the book is available elsewhere, making one wonder if the purchase of the book is really worthwhile.

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There is a huge amount of material out there on Cold War science in general, and Cold War American science in particular, which can be rather daunting for someone approaching this subject for the first time. This book, a concise and highly readable account of science and technology in the

Cold War USA, appearing as part of a series of introductory studies in the history of science, is therefore a very welcome addition to the literature.

Following a short introduction, which sets out the author's aims, the book begins with a chapter on that most potent symbol of Cold War science: the atom bomb. Here we explore for the first time one of the themes that runs throughout the rest of the book: the tension between scientists on the one hand, and military and political figures on the other, concerning, for example, the matter of secrecy versus openness, and the question who should have the final say in state applications of science. This leads into a chapter on the US military–industrial complex, in which we learn about the funding structures of post-war American science, and the blurring of academic/military/industrial divisions. This breaking down of boundaries was due, in large measure, to the massive military investment in science, both in so-called basic research – investigations 'driven by disciplinary rather than practical concerns' (p. 38) – and also in defence applications. Needless to say, investment in the latter was considerably greater. This chapter also touches briefly upon the experiences of US scientific leaders during the years of the anticommunist witch-hunts, before giving way to Chapter 3, which deals with so-called 'big science': the industrial scale of Cold War American science, wich was characterized by, amongst other things, the stupendous injection of cash into science and technology (particularly after the Soviet launch of Sputnik), the focus on large-scale expensive equipment, and the churning out (indeed, overproduction) of legions of qualified physicists and engineers.

Chapter 4 turns to ideological matters and considers the role that science (including now the social sciences) had to play in the Cold War campaign for hearts and minds, both at home and abroad. Offers by the USA of scientific and technological aid to those countries in its sphere of influence (particularly in the Third World) are contrasted with the similar efforts of the USSR. Back in the United States, however, the general public was becoming rather disillusioned with their national science, as Chapter 5 relates: American faith in science and scientists remained reasonably firm until the early 1960s, in light of the affluence that science was perceived to have brought to US society, but it then began to wane as it was realized that science had failed to deliver the wonders that had been promised of it in the years immediately following the Second World War. This chapter also deals with the application to society of science developed by the military; this includes, for example, the role of psychologists in changing racist attitudes, and the employment of military systems analysts in the organization of welfare reforms. The results were mixed, however: science developed in the defence context did not always prove to be applicable in civilian life.

In Chapter 6, we come to another of the more conspicuous examples of Cold War scientific achievement: the space race. In contrast to the material discussed in the preceding chapter, the US space programme was an unparalleled success, especially for propaganda purposes, not least because of its ostensibly (and carefully cultivated) peaceful, civilian image, in contrast to the rather more military nature of the Soviet space programme.

The scientific disillusionment mentioned in Chapter 5 is picked up again in Chapter 7, which considers the renewed worries of the 1970s concerning the potential dangers of science, with the continued nuclear threat, and the application of military science in the Vietnam War. This decade signalled the end of the widespread military–academic ties that had existed since the end of the Second World War; protests on university campuses across the United States resulted in the relocation of many defence-related research facilities. Indeed, it was not only the general American public that had lost faith in scientists: scientific advisers to the country's political leaders were finding themselves increasingly ignored, a trend that continued into the 'new Cold War' of the 1980s, and is perhaps exemplified best by the Reagan administration's plans for the so-called 'Star Wars' Strategic Defence Initiative – plans that were roundly dismissed as science fiction by the majority of American scientists. This disconnect between US scientists and political leaders is the subject of the final chapter of the book, which also looks at the increasingly globalized and commercialized nature of science in the 1980s. The book concludes with an epilogue which muses upon the place of American science in the post-Cold War world.

This is an interesting book, written in an authoritative tone. It provides just enough detail to tell the story, but not too much for an introductory account (a bibliographical essay at the end of the book provides ample suggestions for further reading). At the end of each chapter, we find a useful summary of the main points of that chapter, which also serves to lead us very smoothly into the next. A small number of photographs and illustrations with informative captions provide added richness along the way. All of these factors, together with the author's very engaging style, combine to produce a book that is particularly easy to read, and hence one that I strongly recommend to anyone with a burgeoning interest in the study of Cold War science.

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JEFFREY A. BARRETT and PETER BYRNE (eds.), **The Everett Interpretation of Quantum Mechanics: Collected Works 1955–1980 with Commentary**. Princeton and Oxford: Princeton University Press, 2012, Pp. xii + 389. ISBN 978-0-691-14507-5. £52.00 (hardback).
doi:10.1017/S0007087413000812

The so-called many-worlds interpretation of quantum mechanics is one of the main contenders in current approaches to solve the quantum-measurement problem. Due to the demands it imposes on our imagination and its oftentimes bizarre ramifications, if not to say its – well – craziness, the approach also features prominently in the popular perception of the debates on the foundations of quantum mechanics.

The measurement problem of quantum mechanics arises from the worrisome dualism of the linear, deterministic dynamics of the Schrödinger equation and the non-linear, stochastic dynamics of the wave-function collapse. The Everett interpretation responds to the problem by dropping the projection postulate from the theory altogether. Without a measurement-induced wave-function collapse, it introduces a universal wave function and describes measurement as induced correlations between subsystems of this universal function. Observers are conceived in information-theoretic terms as subsystems that record histories of correlated eigenvalues relative to object systems. Crucially, observations preserve in some sense the object system's superposition of states. This feature was later described in terms of a splitting of the actual world into many different ones, each with equally real observers who subjectively record determinate measurement outcomes.

This interpretation goes back to the doctoral work of Hugh Everett III (1930–1982), who obtained his PhD in physics in April 1957 from Princeton University under the supervision of John A. Wheeler with a thesis entitled 'On the foundations of quantum mechanics'. Everett's short thesis of some twenty pages was published in the same year, under the title '"Relative state" formulation of quantum mechanics', in the issue of *Reviews of Modern Physics* which contains the proceedings of the 1957 Chapel Hill conference on quantum gravity where his thesis had been discussed. Next to Everett's piece, Wheeler published a companion paper in the same issue, commending his student's work as a conceptual turning point comparable to the revolutions initiated by Newton, Maxwell and Einstein.

Hugh Everett III never published anything else on his ideas about quantum mechanics. In fact, he had not even attended the Chapel Hill conference himself, but instead had already left the field for good in favour of a job in military operations research with the Pentagon's top secret Weapons System Evaluation Group (WSEG). The early publications of Everett's interpretation therefore reveal little, if anything, about the genesis of his ideas or about the considerable efforts that Wheeler undertook to convince his own mentor, Niels Bohr, as well as his colleagues in the physics department at Princeton, of the soundness and value of Everett's approach. It was only in 1973