



Against the Tide. An Autobiographical Account of a Professional Outsider, *L. C. Woods*, Institute of Physics Publishing, 2000, pp: 319, ISBN: 0750306094 (hc); Price: US\$40

The publisher's "Review" summarizes the bald facts fairly concisely, so they are simply repeated here, rather than merely paraphrased. "Leslie Woods, born in 1922, recalls his life – from a fisherman's son in New Zealand to Professor of Plasma Physics at Balliol College, Oxford. As a boy, Leslie Woods won a scholarship to university and later joined the RNZAF, becoming a fighter pilot in the Pacific during WW2. After the war, he won a scholarship to Merton College, Oxford and, after some years of research in aerodynamics, was appointed Professor of Engineering at the University of New South Wales. He was elected a Fellow of Balliol College Oxford in 1961, and researched the theory of magnetically-confined hot plasmas with a consultancy at Culham Laboratory. He became Professor of Plasma theory in 1970, but became disillusioned with the fusion energy project, which he believes survive on exaggerated claims of progress – the author explains why, in his view, magnetic fusion has not succeeded and outlines his own philosophy of science – he remains 'against the tide'."

Leslie Woods was clearly always a maverick, and through most of his life this trait did him little harm and much good. Not many non-mavericks would have the drive and the nerve to go from a D. Phil. in engineering, to a first-class BA in mathematics (both at Oxford), to go from England back to success in academia in Australia (not New Zealand!) and then to return again to England to become head of the Oxford Institute for Industrial and Applied Mathematics. He had considerable success in aerodynamics and early on in his plasma physics career. He brought his engineering skepticism and search for direct mechanisms supported by his undoubted skill in applied mathematics to gain a considerable reputation in subsonic aerodynamics, neutron transport, shock-front studies and plasma kinetics. He was characteristically quite vociferous in his disputes with what he regarded as the unrealistic views of entropy and thermodynamics of the "Truesdell school", but in these battles, he was either right or at least had an arguable case, if a bit overstated. However, in the end, his reliance on his intuition led him astray in spectacular fashion, and this is the focus of the rest of this review.

Since this review is for physicists, it is not out of place to set the scene and to indicate the problem, if only to compensate for the naturally one-sided version given by Woods (pp. 247-252, 264-266, 272-281). Woods says that for a gas one can usefully speak of a fluid when the gas constituents' mean free path for collisions are much smaller than the inhomogeneity length(s), otherwise one has a Knudsen gas. This is certainly the case for fluids dominated by neutrals and also when magnetic fields are not important. With charged particles and a magnetic field strong enough to have charge gyro-orbits far smaller than the inhomogeneity lengths one can have a gyro-fluid — at least perpendicular to the field lines. Calculations of collisional cross-field transport across fairly straight field lines — the so-called "classical" transport — can be readily made, and these proved to give confinement times which were something like thousands of times better than practice. However, more subtle effects associated with the actual fields and magnetic-mirror effects in magnetic "doughnuts" led to an improvement in the formulation, so-called "neo-classical" transport. However, this

transport result was still too optimistic, but now by a factor of something like only a hundred or less. The actual transport much in excess of this was declared to be “anomalous”. (Empirical versions of transport could of course be constructed, but it was far from clear what scaling laws should be applied for the design of bigger machines. For a successful large scale-up, understanding is essential.) As Woods observes (p. 252) the blame for the anomalous transport was then (and it is still the case now) put on some form of turbulence. In fact, after many years of painful effort, in both models and computer development, one can now say that the extensive modeling of the plasma microturbulence (now in its ITG (Ion Temperature Gradient) form), so disdained by Woods in favour of his own “cure”, is proving more and more effective in understanding transport in tokamaks. (Understanding so-called “transport barriers” still presents some problems, however.)

Returning to Woods: disdaining the turbulence explanation, Woods galloped to the rescue of the effort to understand what was going on by insisting on a Draconian cure involving major surgery of the fundamental equations. The Woods cure would have killed the patient, in that it would have apparently ruled out any possibility of magnetically confined fusion, an unpalatable conclusion for the establishment. However, Woods’ intuition had led him astray in the important plasma case with strong magnetic field (relevant to magnetic confinement fusion) when the collision frequencies are low (and neutral particles are absent). Because the negative opinion of various referees blocked his access to the peer-reviewed journals, Woods chose to “go public” via a monograph *Principles of Magnetoplasma Dynamics*. In the outline for the book, Woods says, “One of the paradoxes arising in kinetic theory concerns the fluid pressure. Collisions are necessary for a fluid force to result, yet standard kinetic theory does not entail this, being satisfied to bypass Newton's equations by defining pressure as a momentum flux. This omission usually has no adverse consequences, but with increasing Knudsen number, it leads to errors. This text pays particular attention to pressure, explaining the importance of allowing for its collisional nature from the outset in developing kinetic theory.” The most authoritative and strongly negative verdict (“good parts” are interlaced with statements which “are drastically wrong”) on the work was delivered by Alan Ware in a review published in *Nuclear Fusion* (vol. 28, pp. 1141-2, 1988) (followed by an attempted rebuttal by the author pp. 1142-4). The Woods recipe is, “... when collisions are thought to be unimportant [the pressure gradient] should be set to zero.” and “an extra force the pressure gradient divided by the particle number must be added to the usual electric and magnetic forces and the guiding drift contains the extra term [a term containing the pressure gradient].”

In fact the Woods recipe was completely wrong. The equations used for basic tokamak theory (including neo-classical theory) are derived from fundamental kinetic equations by very careful approximation, and there is no justification for inserting such *ad hoc* (more properly *ad hominem*) forces derived from one man’s intuition. In spite of the comments by Ware and by others, Woods’ head is bloody but unbowed. His opinions have apparently not changed, being repeated relatively recently, first in a letter to the editor of the US journal *Fusion Science and Technology* in November 2001 (vol. 40, p. 284) and then in another letter in March 2003 (vol. 43, p. 256) — a voice crying in the wilderness.

It is a pity that, since Woods' efforts to publish in more detail have failed at the referee level, it is only through abbreviated discussions in book reviews and the like are we able to glimpse the core of this disagreement. Perhaps the field would have been better served by, say, a provisional publication, together with a critique, and perhaps a rebuttal, with the judgement being left to the reader. But it is difficult for a peer-reviewed journal to do such a radical thing as to publish (albeit with a warning message) something their referees say is actually wrong.

Years ago an interesting analysis of the playwright Christopher Marlowe was written by Harry Levin, which was titled "The Overreacher", in which he portrayed Marlowe as an "overreacher" who couldn't stop from overreaching. It could be said that "Les" Woods showed himself to be such a man, larger than life, and one who could well have gone out a winner. He raced long enough, however, to have a professionally very bad fall at the very last fence of a long and exciting race for this "professional outsider". However, if he had not been such a man and such an overreacher, he probably wouldn't have come near to reaching that last fence.

His adventures on the wild ride that was his life are well worth reading. His flaming personality and energy clearly imposed a fierce pressure on his surroundings (undoubtedly a short mean-free-path effect of the kind he liked, not to mention the presence of a short fuse on his side). On another note, the reactions we can infer of those around him do shed much light on the academic climate in which he worked and on his distance from (and incomprehension of) the pen-pushers, the bean-counters, the academic politicians and the other little men. "He was a man, take him all in all, we shall not look upon his like again."

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