the researchers and research institutions that produce it.

Most participants in the debate agree that science has nothing to fear from a closer study of its social features. Indeed, they accept that it is only by understanding the creative and critical practices by which scientists cooperate and compete that we can fully appreciate the status of scientific knowledge — the extraordinary reliability of some parts of it and the horrible uncertainty of much of the rest. How come, then, that sociology is widely perceived as 'anti-science'?

This conversation doesn't resolve the puzzle. In the book, the so-called 'sociology of scientific knowledge', or SSK, fills the stage. But despite their self-proclaimed relativism, its proponents repeatedly insist that they too are faithful supporters of the scientific enterprise. In any case, the innocent reader should not accept their claim to be speaking for the whole field of 'science studies'. As the admirably broad-minded contributions of Michael Lynch and Steven Shapin show, SSK is actually only one of the many guerilla bands that operate in this wide-open academic territory.

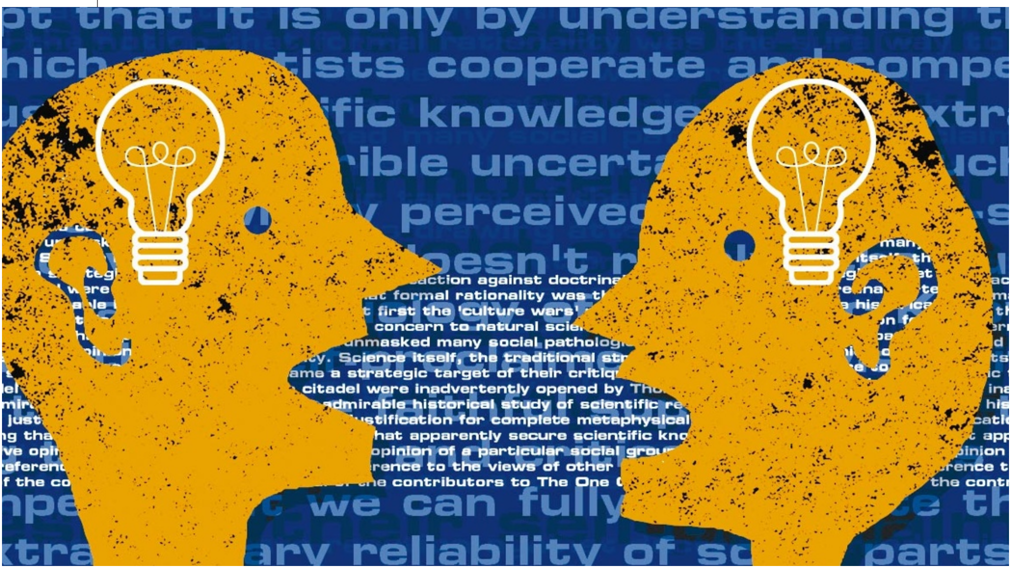
What the SSKers say again and again in this rambling conversation is that their relativism is strictly "methodological". As their avowed leader Harry Collins puts it, they are carrying out an "amoral analysis", in which they "leave the scientists to decide on the truth" while "recording the argument without taking sides". The practices and beliefs associated with a research project in physics should be treated "symmetrically" with those associated with, say, the casting of a horoscope or the detection of a witch among the Azande of Sudan. Indeed, the adherents of SSK say that they are just ethnographers, like Edward Evans-Pritchard, who adopted a similar stance in order to demonstrate the innate rationality of pre-scientific cultural systems.

Unfortunately, as most social scientists now acknowledge, this highly desirable objectivity is nowhere near attainable. The conversation does not include any sociologists or ethnographers outside the narrow speciality of the sociobiology of science, so it is never pointed out that outsiders don't necessarily have a clearer view of a culture than the people who live inside it. In practice, methodological relativism shades into rhetorical affectation, dangerously compromising the sincerity of the 'science studier' in the eyes of the 'studied scientist'. Thus, to preserve a semblance of consistency, SSK overbalances into full metaphysical relativism, where its pro-science protestations sound very thin.

What has SSK actually taught us? Principally, it has shown us that scientific research is 'really' just like any other human activity. Big deal! In fact, this unsurprising outcome was written into the initial script. SSK aims to tell everyone what they should know about science, but aborts its own mission by perversely bracketing out the social peculiarities that differentiate science from other modes of knowledge production. Thus, it has nothing to say about the practices that seem to make scientific knowledge so remarkably reliable for certain purposes.

SSK is just too conceptually limited to answer the questioning title of this muddled, muddling book. The 'pro-science' conversationalists here are all physical scientists, so they fail to see that the natural world has many different aspects, each inspiring a somewhat different scientific culture. Or should one say that the natural and human sciences, including SSK itself, are all subcultures of modernism, all challenged by the post-modern critique, and all really on the same side in the science wars?

John Ziman is at 27 Little London Green, Oakley, Aylesbury HP18 9QL, UK. His most recent book is Real Science: What It Is and What It Means *(Cambridge University Press).*



A life of good taste

Fine Wines and Fish Oils: The Life of Hugh Macdonald Sinclair
by Jeannette Ewin
Oxford University Press: 2001. 240 pp. £25

Tom Sanders

The nutritionist Hugh Sinclair (1910–90) was the archetypal Oxford don: erudite, urbane but disorganized, and an accomplished and witty speaker with an astonishing repertoire of nutritional anecdotes. This biography, however, which borders on being a hagiography, focuses on Sinclair's work, particularly his Oxford Nutrition Survey, his role in stimulating research on essential fatty acids and his unsuccessful struggle to establish a department of nutrition at Oxford University.

Sinclair's father was an elderly army colonel, who died when Sinclair was at boarding-school, and his mother, a member of the wealthy Scottish aristocracy, lived with him until her death in 1969. Sinclair was proud of his ancestral heritage and was an inveterate snob. Educated at Winchester and Oxford, he cultivated a taste for entertaining and fine wines while at Magdalen College. Having purchased life membership of the Middlesex Cricket Club aged nineteen, and inveigled himself into membership of the Athenaeum Club, Sinclair had a sound base from which to network with the influential scientists and magnates of his day. He read widely, travelled extensively and was able to meet some of the world's great nutritional scientists. Indeed, it could be said that he spent more time travelling and attending conferences than doing research.

Sinclair was director of the Oxford Nutrition Survey, which aimed to assess the prevalence of malnutrition among the British population. As it evolved, the survey grew more complex and became embellished by numerous studies of dietary interventions, generating enormous amounts of social, clinical and biochemical data. In 1943, during a critical phase of the survey, Sinclair absented himself for three months to attend scientific meetings and visit colleagues in the United States and Canada. He failed to work out how the accumulating data could be analysed, and the survey remained unanalysed and unpublished in his lifetime.

In 1945, Sinclair was given the rank of brigadier and sent to Holland and Germany to work as part of a team assessing the nutritional status of the Dutch and German populations. For this work he received the Medal of Freedom with Silver Palm from the United States.

While touring Canada in 1943, Sinclair met Group-Captain Tisdale of the Royal Canadian Air Force, who invited him the following year on a short expedition to observe whether riboflavin deficiency was related to snow blindness among the Canadian Inuit. It is reported that he failed to keep any written record in his diaries but embellished the tale in later life to suggest that he had joined the expedition because he was interested in the fact that the Inuit diet was high in fat, rich in essential fatty acids, and yet the Inuit were free from heart disease. During this period of his life, Sinclair's work was concerned with thiamine and diseases of the nervous system, and there was no evidence of his having any interest in cardiovascular disease and dietary fat. His epic letter to the *Lancet* in 1956, in which he suggested that cardiovascular disease was caused by a deficiency of essential fatty acids, was an important stimulus to future research.

But Sinclair was blinkered by the deficiency paradigm. And the book perpetuates the myth that he was responsible for drawing attention to the cardioprotective properties of omega-3 fatty acids. What he failed to note was that the balance of omega-6 to omega-3 fatty acids was important to health. Indeed, for many years he promoted the consumption of a diet high in omega-6 fatty acids. The major impetus for cardiovascular research on the omega-3 fatty acids arose from the work of Salvador Moncada and John Vane on prostacyclin, and Philip Needleman on thromboxane, published in 1976, three years before Sinclair embarked on his Eskimo diet to demonstrate the effects of omega-3 fatty acids from fish oils on haemostasis.

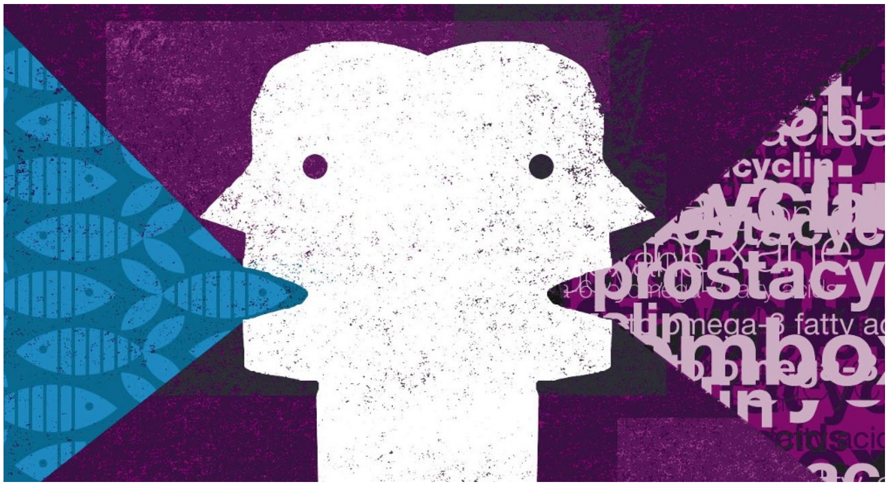
Sinclair's life-long ambition was to establish a department of nutrition at Oxford. He was appointed reader in human nutrition there in 1951. There is little doubt that he was an able scholar, but his ability as a research scientist is questionable because of his lack of attention to detail and failure to publish his results in peer-reviewed journals. He was a prolific letter-writer and collector of manuscripts (including a collection of erotica) and, following his death, these sold for more than £85,000 (US$124,000).

Like a few other famous nutritionists, such as Boyd Orr and Robert McCarrison, Sinclair liked to dabble in the politics of food and influence national policy. But his outpourings tended to be based on belief and theory rather than evidence and he was openly contemptuous of the work of his contemporaries, such as John Yudkin and Elsie Widdowson. But to his credit, Sinclair truly understood the complexity of the relationship between diet and health and recognized the need for a multidisciplinary approach.

As a scientist, he came to be regarded as a dilettante; his research lacked focus and was unsystematic. This, coupled with his failure to complete projects and produce peer-reviewed publications, and his sniping at influential contemporaries, eventually resulted in his ejection from Oxford's Department of Biochemistry in 1956 by Sir Hans Krebs, and his readership was not renewed in 1958. For the rest of his working life, Sinclair remained in the wilderness of his self-styled National Institute of Nutrition, which was situated in the grounds of his home at Lady Place in Sutton Courtenay. On his death, he bequeathed his estate to establish a chair in nutrition at Oxford which the university declined. The offer was eventually taken up by the University of Reading, where the Hugh Sinclair Nutrition Unit thrives under Christine Williams.

This is no detective story: there are no elegantly designed experiments or startling discoveries. It is a salutary warning to nutritionists that scientific progress is made by good experimental design and meticulous attention to detail and not by travelling the world on lecture tours. ■

Tom Sanders is in the Department of Nutrition & Dietetics, King's College London, Franklin-Wilkins Building, Stamford Street, London SE1 9NN, UK.



In for the count

Mathematical Mountaintops: The Five Most Famous Problems of All Time
by John Casti
Oxford University Press: 2001. 288 pp. £19.95, $25

Simon Singh

The recent boom in mathematics bestsellers has contributed a great deal towards raising the public profile of the subject. But such books ignore a significant section of potential readers, namely those who have more of a mathematical background than the general reader but who are not professional mathematicians. Such mathematical enthusiasts have no doubt enjoyed some of the popular books, but would really prefer a more technical treatment. This is exactly what John Casti provides in *Mathematical Mountaintops*. It is neither a textbook nor a pop maths book, rather it is a serious in-depth look at the great problems of mathematics.

Casti has picked "the five most famous problems of all time", and spends 30 to 40 pages describing each one. The problems are Hilbert's tenth problem, the four-colour problem, the continuum hypothesis, the Kepler conjecture and Fermat's last theorem. Each of these has now been solved, so, in addition to outlining the problem, the author is able to explain the solution and recount the story behind it. Four of the problems have been written about extensively elsewhere, but perhaps not with Casti's balance of technical explanation and background narrative.

Casti's remaining problem, the Kepler conjecture, has (to my knowledge) not been written about since the recent announcement that it has been proved, and provides perhaps the most interesting chapter. The problem dates back to 1606, when Johannes Kepler posed a question in a paper for his patron Johann Matthäus Wacker of Wackenfels, Knight Bachelor. Kepler asked, what is the most efficient way to stack spheres so as to minimize the spaces between them? Alternatively, what is the best way to pack oranges in an infinite box? Kepler proposed that the best arrangement was the face-centred cubic lattice, in which every sphere in the first layer is surrounded by six others, and each subsequent layer is built by putting spheres in the dimples of the layer below. This arrangement has a packing efficiency of 74.048%. Grocers, who traditionally stack oranges in this way, suspected that Kepler was right, but it took mathematicians almost four centuries to prove it.

There were some notable milestones along the way. In 1694, Isaac Newton and the Scottish astronomer James Gregory argued about the sphere-kissing problem: what is the maximum number of spheres you can place simultaneously in contact with a central sphere? Newton said that the answer was twelve, which is easily achievable, but Gregory was convinced that it was possible to squeeze in a thirteenth sphere. Newton turned out to be right, but this took 180 years to prove.

The Kepler conjecture was eventually proved in 1998 by Thomas Hales of the