US space programme was grievously misconceived from the start and that all the projects designed to assert the nation's technological virility — putting a few people into low orbits or retrieving fragments of Mars — were pointless extravagances, which did nothing to enlarge human knowledge or the limits of useful technology.

White makes some curious judgements. He accepts without reservation the (to me) unconvincing evidence that Werner Heisenberg held back the German atomic bomb project for reasons of conscience; he does not reveal Heisenberg's famous mistake in estimating the critical mass of fissile material. And he exculpates Wernher von Braun, whose activities led to the deaths of some 3,000 Londoners and many more labourers at the Peenemünde missile site; von Braun, he tells us, was "a scientist first and a weapons designer second", was no overt supporter of the Nazi party and was only doing his humble best to help his country win the war. (So that's all right, then.)

There is no dearth of mistakes, small and not so small. Max Perutz did not "unravel haemoglobin" in the year in which the DNA structure was revealed; Aaron Klug did not win his Nobel prize for genetics; Maurice Wilkins was not an X-ray crystallographer, nor did he scorn model building; Francis Crick never worked on "the effect of magnetic fields on development of cells"; the Allies did not use the Enigma machine to send misleading signals to the Germans; Martin Ryle was not the originator of the Big Bang theory and Enrico Fermi was no Hungarian physicist; Silesia is not in Czechoslovakia; and, for that matter, Eisenhower did not lose a presidential election to Kennedy.

There are, moreover, some inscrutable passages of explanation. Why are we instructed that proteins "form enzymes ... and antibodies and are integral to many huge molecules such as DNA and RNA"? And what is one to make of Maurice Wilkins's gift to Rosalind Franklin of DNA "in the *beta* [sic] or *hydrated* form", which is "colloidal, and in some respects more troublesome to work with". There is a wise maxim which states that a little inaccuracy can save a world of explanation, but here the principle has surely been carried to excess.

But I do not want to end on a disobliging note. Mistakes will no doubt be put right in the paperback edition, and *Rivals* is agreeably written, and seldom less than absorbing. You do not have to swallow the thesis on which it uneasily rests to take pleasure in these unusual, often lurid and sometimes scandalous episodes in the history of science; and you may come away with some useful tips on how to knee your competitors in the groin. ■

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Statistical conundrum: did the tea go into the cup before or after the milk?

Still waiting for the revolution

The Lady Tasting Tea: How Statistics Revolutionized Science in the Twentieth Century
by David Salsburg
W. H. Freeman: 2001. 340 pp. $23.95

David Colquhoun

This is the fun side of statistics. David Salsburg's popular account of some of the great statisticians is a great read, full of anecdotes and unusual personal information. It starts around the turn of the last century with Francis Galton, Karl Pearson, W. S. Gossett ('Student' of the *t*-test) and, of course, R. A. Fisher, and continues with Egon Pearson, Jerzy Neyman and Florence Nightingale David.

Thus far, the action is all at University College London and Cambridge, but the book then spreads out worldwide. There are accounts of the life and work of many twentieth-century statisticians, from A. N. Kolmogorov's work on the axioms of probability, through I. J. Good's work on cryptography at Bletchley Park and subsequently on bayesian methods, to the many achievements of John Tukey, not least the fast Fourier transform. Along the way there are descriptions of the great (and still unresolved) wrangles between statisticians about inverse probability and the foundations of inductive inference.

The title of the book refers to a famous chapter in Fisher's *The Design of Experiments* (1935). In this chapter Fisher illustrates the principles of experimental design by discussing the hypothetical problem of how to test the claim that it is possible to tell whether the tea is put into the cup before or after the milk. The first riveting fact I discovered from this book is that the problem is not entirely hypothetical. It is based on an actual incident at a tea party in Cambridge in the late 1920s (the author knows someone who was there).

The book is crammed with such personal anecdotes, both amusing and sometimes (thanks to Hitler and Stalin) harrowing. For example, I discovered that I. J. Good, who worked for much of his life in the United States, is the son of a Polish immigrant to London's East End who owned a well-known antique jewellery shop (Cameo Corner) near the British Museum (I bought a ring there). Salsburg does not, however, mention Florence David's predilection for cigars of churchillian proportions. I have often wondered what impression they must have made when she eventually moved to California; these days she would probably be arrested.

Salsburg's account is entirely non-mathematical, and he makes a creditable attempt at verbal descriptions of some of the great work. Nevertheless, the difficulty of conveying the ideas in words was brought home to me by the modest amount that I felt I understood in those areas I did not know about already, such as martingales (I trust his mathematical definition is more accurate than his nautical definition of this word).

When it comes to the science in the book — as opposed to history and anecdote —

Salsburg's views will not gain universal agreement. One of his recurring themes is that the old, deterministic clockwork Universe was swept aside by the "statistical revolution ... The 'things' of science are not the observables but the mathematical distribution functions that describe the probabilities associated with observations."

My problem here is that Salsburg makes little distinction between the unavoidable variability that results from the random behaviour of atoms, and avoidable variability due to errors of observation. At the level of individual molecules, atoms and subatomic particles, random behaviour is part of the physics of the system. In subatomic physics and in my own field of single-ion channels, it is quite true that what we observe are distributions. But nevertheless we can, and do, regard the (true) means of such distributions as deterministic constants. We can measure the duration of random lifetimes with high accuracy relative to the real variability of the system.

But these are specialist areas of research. In most areas, the variability is seldom of this unavoidable single-molecule sort. Most people would think it entirely reasonable that when you measure, for example, the equilibrium constant for a well-defined reaction, there *is* a true value. Of course there will be experimental errors in measuring this value, but the problem is essentially deterministic.

Crucial though statistical ideas are in many areas, I cannot help thinking that Salsburg exaggerates when he talks of statistics as "probably the single most important tool of biological science". What about the genome, the electron microscope and the patch clamp? If you read the great papers in my own area, for example those of Hodgkin and Huxley, of Bernard Katz, or of Neher and Sakmann, you will find very little statistics (at least of the sort that refers to measurement errors), and there was no need for any. In other areas, such as clinical trials and psychology, the use of statistics is critically important. However, one is reminded of the dangers of excessive enthusiasm for numbers when the author unblushingly tells us that "psychology developed techniques of measuring intelligence".

Most practical scientists who are dealing with large numbers of molecules that behave in an essentially deterministic way remain largely untouched by the "statistical revolution". The lady tasting tea is famous among statisticians, but none of my colleagues recognized the allusion, and very few are interested in the basis of scientific inference. This may show a lack of intellectual curiosity — or may merely reflect the amount of time they have to spend on bureaucratic activities imposed by governments and universities — but most of the time it does not hinder their science very much. It would be nice to think that the book would be read by many biologists, and indeed by the general public, but I suspect the main audience will be statisticians — amateur and professional.

Fisher's lady tasting tea was intended to illustrate the crucial importance of randomization in experimental design. This lesson has been learned well in areas such as the design of clinical trials, but is still largely ignored in laboratory sciences. Perhaps Salsburg does have a point. Studies on transgenic animals are a mainstay of research in the post-genomic era, and they can give useful results. But it is impossible to randomize gene-knockout experiments. And nobody seems to worry that this makes it impossible to do a valid significance test on the results of their experiments. Perhaps molecular biologists should read about Fisher's lady. ■

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Breaking down the barriers

The Ape and the Sushi Master: Cultural Reflections by a Primatologist
by Frans de Waal
Basic Books: 2001. 433 pp. $26, £14

Christophe Boesch

Christian theology places animals on the other side of an insurmountable barrier from humans, whereas a Buddhist believes that humans are reincarnated as animals and vice versa. Christians are trained to do their utmost to preserve a large philosophical and psychological gap between animals and humans. Buddhist and similar traditions, on the other hand, attribute intentions, feelings and a mind to all living animals, including humans. As Frans de Waal puts it in *The Ape and the Sushi Master*, "inspired by the pervasive human-animal dualism of the Judeo-Christian tradition, the anthropodenial, that is the *a priori* rejection of shared characteristics between human and animals, has no parallel in other religions and cultures".

Does animal culture exist? And how do scientists approach this question? The author's approach is to contrast Japanese and occidental attitudes to it. He reveals how much the philosophical background of a society affects its view of human culture. Occidental scientists believe firmly that imitation and teaching are the basis of human culture. The Japanese sushi master, however, neither teaches nor instructs his apprentice — for at least three years, the apprentice watches his master at work, without ever being allowed to practise. After this, he will prepare his first sushi, usually with remarkable dexterity. So much for teaching. We therefore need a more open definition of culture to take account of the variability of human culture. Culture, defined by de Waal, is "a way of life shared by the members of one group but not necessarily with the members of other groups of the same species ... The way individuals learn from each other is secondary, but that they learn from others is a requirement."

To illustrate his proposition that animals can have 'minds' and intention, de Waal describes Georgia, a female troublemaker in the chimpanzee colony at Yerkes Regional Primate Research Center in Atlanta, Georgia. She would take a mouthful of water as visitors were arriving, then casually mingle with the rest of the colony and wait with closed lips until the visitors came near. She would then suddenly spray them, to the accompaniment of shrieks and laughter. About this example, de Waal observes that "if Georgia the chimpanzee acts in a way that in any human would be considered deliberately deceitful, we need compelling evidence to the contrary before we say that, in fact, she was guided by different intentions, or worse, that apes have no

MILES BARTON/BBC WILD

High culture: potato-washing by the macaque colony on Koshima Island.