The Fuchs Case

Klaus Fuchs. The Man Who Stole the Atomic Bomb. NORMAN MOSS. Grafton (Collins), London, and St. Martin's, New York, 1987. 216 pp. + plates. $15.95.

Klaus Fuchs, Atom Spy. ROBERT CHADWELL WILLIAMS. Harvard University Press, Cambridge, MA, 1987. x, 267 pp., illus., + plates. $25.

At the end of January 1950, a senior British atomic scientist appended his signature to an unusual document. It was a confession that from early 1942 until the spring of 1949 he had conveyed information about atomic research to agents of the Soviet Union. On 2 February the man, Klaus Fuchs, was arrested on the direct orders of the British Attorney-General, charged with violating the Official Secrets Act, tried, and, in view of his confession, quickly sentenced to 14 years in prison.

To put it mildly, the case created a scandal. It did more. It terminated negotiations for an Anglo-American weapons enterprise. In the United States it left a residue of suspicion of British security methods and affected scientific relations between the two countries for years. For Fuchs had been the genuine article: a top-level scientist with access to the innermost secrets of the Manhattan Project during the war and with useful knowledge of the prospects for the hydrogen bomb, not to mention the development of British weaponry after 1945. All this he had turned over to the Russians.

The subject has attracted attention in virtually every book dealing with Los Alamos, including a sober and sensible summary by the British official historian of atomic energy, Margaret Gowing, in her *Independence and Deterrence*; but the lapse of time and official secrecy, as well as the inherent drama of the subject, made it inevitable that Fuchs would become the focus of a book. The present publishing season has produced two.

Much of the material in Norman Moss's and Robert Chadwell Williams's books is the same. Both books work over Fuch's conversion from social democracy to communism in the face of the rise of Hitler; his left-leaning family; his adventures in exile in the 1930s in Britain, where he came into contact with agents of Soviet intelligence; his internment as an enemy alien in 1940; and his subsequent release back into his laboratory from a Canadian internment camp. The books even highlight the same things: the drama of Fuchs's confession and the confession itself, which both books reprint as an appendix. This is not to say that the books are interchangeable. They start from different premises and use different techniques to answer what is, essentially, the same question: how did Fuchs come to commit his acts of betrayal, and why, since he had been a known member of the Communist Party of Germany (KPD) before he fled Hitler in 1933, was he not detected earlier?

Moss takes the psychological line, whereas Williams prefers the broad sweep of historical context (and it would seem that he is well equipped to provide it, with a massive bibliography and documentary research in three countries). Moss does less well on context, relying extensively on interviews he conducted while working up a BBC radio program on the Fuchs case. Moss is the better writer, as one would expect of a well-published journalist, and he has the advantage of greater familiarity with British life and nomenclature. To take one minor example, he correctly identifies Harold Laski as "chairman" of the Labour Party, rather than as "leader," as does Williams (p. 59)—a slip that would have surprised Clement Attlee (correctly identified as leader in a photo caption elsewhere in the Williams book). He does better on Fuchs's arraignment as well, adding the prosecutor's assurance to the magistrate that Fuchs on his salary could well afford counsel—another detail, but one that alters the impression left by Williams. The two books contradict one another as to whether Fuchs had a privately retained or a court-appointed lawyer at his preliminary hearing; I am inclined to accept Moss's version.

It is too bad that Williams, who has accumulated so much information, much of it new, should commit so many minor errors. There is, for example, no such newspaper as the *Ottawa Gazette* (p. 233; *The Gazette* lives in Montreal). Hindenburg was a field marshal, not a general, when he was elected president of Germany (p. 15); Frederick Hoyer Millar was not British "ambassador" to Washington in 1950, but minister (p. 136); Lewis Strauss was not AEC chairman in 1948 (p. 97); *The Ensign* hardly counts as "a leading Canadian Catholic weekly journal of opinion" unless one applies very odd categories (p. 146). One begins to wonder what the editors at Harvard University Press do when they vet a book for publication.

There are also problems in both books with the hypothetical reconstruction of Fuchs's state of mind. Moss speculates that the scientist's "dry, racking coughs" were "a price exacted by his unconscious for his betrayal" (p. 124), and Williams conjectures that Fuchs did not know the difference between "right" and "*Recht*" (pp. 133–34). Maybe; but again maybe not. Historians use psychology at their peril, especially when ideology is blended in. The result is frequently unconvincing, as it is in these two books.

The Williams book is certainly scholarly, but the author does not convince the reader that he has done well to devote whole sections of it to Philby and the four or five other men who populate current British spy literature. A footnote, or at most a couple of paragraphs, would have sufficed. Nor, ultimately, does he persuade the reader that Fuchs belongs in the pantheon of victims of Stalinism in eastern Europe, Traicho Kostov, László Rajk, and their ilk. When Williams argues that "in 1949 Fuchs could have faced prison or worse in East Germany" (p. 174) the mind reluctantly boggles. It is much more likely that he would have shared a privileged exile in Russia with his fellow atomic scientist Bruno Pontecorvo, who defected soon after the Fuchs affair.

Readers searching for a straightforward and on the whole persuasive account of the Fuchs case should prefer Moss; those searching for the "big explanations" that characterize so much of the literature of espionage (and, alas, of modern historiography) should direct themselves to Williams.

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Models of Food-Finding

Foraging Theory. DAVID W. STEPHENS and JOHN R. KREBS. Princeton University Press, Princeton, NJ, 1987. xiv, 247 pp., illus. $40; paper, $14.50. Monographs in Behavior and Ecology.

Foraging theory (or optimal foraging theory, as it is commonly called) attempts to understand and predict the ways in which animals obtain food. It does this through first considering the function of the foraging process (to obtain energy while avoiding predators) and then hypothesizing that the animal will maximize or minimize some outcome of this process (for example maximize the net rate of energy gain during foraging). This book shows how the result-

ing mathematical models are developed and considers how successful the approach has been.

Animal foraging involves many different kinds of decisions. An animal must, for example, decide whether to eat or ignore food items when encountered, whether to continue foraging in the same patch or move to another, which patch to visit, and what direction to move in. Most studies of foraging consider one or more of these kinds of decision. This book focuses on the first three. The lack of attention to decisions concerning directionality of movement detracts, I think, from an otherwise excellent treatment because a large number of studies have indicated that animal movement patterns and decisions involving them need to be integrated with other decision processes. A nectar-feeding animal may, for example, choose its next plant or inflorescence on the basis of possible directions of movement, and an understanding of pollen movement brought about by animals may require an understanding of the animals' movement patterns and what influences them.

The book consists essentially of four sections: discussion of philosophy, underlying assumptions, and general methodology of foraging models; development of simple models; development of more complex models; and discussions of the testing of foraging models and of criticisms of the optimal foraging approach. In the first section (chapter 1) the authors show how a foraging process can be modeled on the basis of a combination of decision assumptions, currency assumptions, and constraint assumptions—that is, assumptions about the nature of the choice the forager makes, about the criterion used to evaluate its outcome, and about the factors (environmental or endogenous) that limit the relationship between the currency and the decision variables.

In the second section (chapter 2) are developed the simplest models of prey choice and patch departure. In the first model the animal is assumed to encounter food items of types that differ in terms of energy yield and handling time and, because search and handling are mutually exclusive, the animal may choose either to eat an encountered food item or to continue searching for food. It is also assumed the prey types are encountered at different rates, that they are recognized instantly and with complete accuracy, and that the animal encounters only one food item at a time. In the second model the animal is assumed to obtain energy in patches at a rate that decreases as time spent in a patch increases and to decide at each point in time whether to continue foraging in the same patch or to search for another. It is also assumed that an animal cannot simultaneously search for patches and feed within a patch and that only one patch is encountered at a time. In both models it is also assumed that the animal adopts the decision rule that maximizes the long-term net rate of energy gain while foraging and that the animal has "complete information" in the sense that it knows all the parameters of the model and does not modify its behavior as a result of its experience while foraging. The authors present the simple predictions of the two models and show that these predictions are mostly preserved when the two models are combined.

In the third section (chapters 3 to 8) the authors develop more complex and more realistic models by relaxing some of the assumptions of the simple models. They consider, for example, foraging situations in which there are simultaneous encounters with more than one food item, food search and handling can occur at the same time, certain nutrient requirements must be satisfied, prey recognition is not instantaneous or is not completely accurate, the animal returns to a central place with collected food (central place foraging), the animal does not know the quality of the patch it is in, but its experience in the patch provides information about its likely quality (incomplete information), the currency to be maximized is the probability of obtaining enough energy to avoid starvation (risk-sensitive foraging), and the animal's decision rules can change through time and depend on the state of the animal (dynamic optimization). Stephens and Krebs show how with these and other variations to the assumptions of the simple models the equally simple predictions no longer hold.

In the fourth section (chapters 9 and 10) Stephens and Krebs consider how foraging models should be tested, how well they have performed, and criticisms of the optimal foraging approach. This is likely to be the most controversial section of the book for the following reasons. First Stephens and Krebs argue that foraging theory "has often been verified, and this lays the groundwork for many future developments." Their argument is, however, based on what appears to me to be a somewhat subjective assessment of whether particular studies do or do not support the optimal foraging approach. Two studies of mine, for example, are considered to be "partially or qualitatively consistent with model." However, in one of these studies there is no model, only a verbal argument that bumblebees that are visiting flowers on vertical inflorescences should commence at the lowest flower (which has on average the highest nectar volume). The bumblebees tend to begin with the lowest flower but do not always. In the other study I attempted to test the earliest and simplest patch departure model in a situation in which the assumptions are not satisfied and in which, it is now known, adopting more realistic assumptions alters the predictions. I would not now claim that these studies provide support for optimal foraging theory but would argue instead that they should be viewed as neutral. It seems to me that the success of the optimal foraging approach in explaining and predicting foraging behavior remains essentially undetermined because there are virtually no studies that meet the obvious criteria for inclusion in such an assessment (for example, that the model is appropriate to the foraging situation; that its assumptions are realistic and justified; that the mathematics is correct; and that confidence intervals are determined for both predictions and observations. Gray, in another recent book on the subject (*Foraging Behavior*, A. C. Kamil, J. R. Krebs, and H. R. Pulliam, Eds.; Plenum, 1986), takes a more negative view of the optimality approach and argues that foraging theory is largely at odds with the published data.

The last section of this book will also be controversial because it deals with criticisms that have been leveled at the optimality approach in general, and optimal foraging in particular. It has, for example, been suggested that optimal foraging theory is untestable because post hoc rationalization can explain any discrepancy between theory and observation. Stephens and Krebs counter this claim by pointing out that the generally accepted scientific process typically involves post hoc rationalization followed by testing of the new theories that result. If this process leads to theories that are generally successful without repeated modification the scientific process has also been successful and the resulting theories can be used as foundations for other kinds of studies. Stephens and Krebs deal well with this and other criticisms of optimal foraging theory and point out that optimality models are more prone to attract the refutability criticism because they are more overt and specific in formulating testable hypotheses than are most others in evolutionary biology. The mathematical nature of the models also tends to make any assumptions clear and explicit.

This book should be read by anyone with an interest in understanding foraging behavior or in applying the optimality approach to other aspects of evolutionary biology. It is clearly written and generally easy to follow, though some readers may find the mathematics difficult, especially in the chapter on dynamic optimization. The structure of the

book should make it excellent as a textbook. Its integration of chapters, overview of the subject, and, in some cases, novel developments should also make it most useful to established researchers of animal foraging.

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Sign Language and the Brain

What the Hands Reveal about the Brain. HOWARD POIZNER, EDWARD S. KLIMA, and URSULA BELLUGI. MIT Press, Cambridge, MA, 1987. xx, 236 pp., illus. $25. MIT Press Series on Issues in the Biology of Language and Cognition. A Bradford Book.

One approach that has been frequently used by researchers in neurolinguistics is to examine the linguistic functioning of individuals who have suffered various kinds of brain damage. This is the approach taken by the authors of this book, but the six subjects they chose for their investigation of brain-language relations are special: they are users of sign language, not oral language.

Although the linguistic functioning of brain-damaged deaf signers has been studied before, no previous study has so carefully selected subjects, so thoroughly documented the site and extent of their brain lesions, or so extensively examined their language functioning. Three of these subjects had left- and three had right-hemisphere lesions. All had used American Sign Language (ASL) for most of their lives and were fluent in it before they suffered brain damage. They had deaf spouses and associated with other deaf persons in the community. Their acquisition and use of language were therefore similar to those of oral-language users who suffer brain damage.

The central issue addressed by the research is whether or not language is organized in the brains of signers in the same way as it is for oral-language users. The authors begin by describing in detail the similarities and differences between the visual-motor system used by signers and oral-language systems. Signs are made up of the features of shape, movement, and place, which are roughly comparable to the phonetic features of sounds in words. Like oral language, sign language has a morphological system that marks, for example, word class, aspect, and tense; but, say the authors, whereas oral language marks such notions sequentially, "ASL tends to transmit structural information in a simultaneously layered fashion" (p. 107). Word combinations in sign are primarily spatially organized, whereas in oral language they are temporally organized. (One might argue about the extent to which oral language is temporally sequential given co-articulation effects, intonation, and multiple representations for words that must be processed by the listener, but this does not take away from the essential argument.)

The data from these six subjects make clear that sign language, despite its visual and spatial nature, is, like oral language for most people, represented in the left hemisphere: the three subjects with left-hemisphere damage all have linguistic problems, whereas those with right-hemisphere damage do not, though they have severe visual problems. A further finding is that those with left-hemisphere damage suffer particular difficulties depending on the site and extent of the lesion: one subject is agrammatical and nonfluent, similar to a Broca's aphasic, another has difficulty with the features of signed words and with pronominal referencing, and the third has difficulty, in particular, with morphology. Not all of these difficulties are identical to those suffered by speaking aphasic subjects with lesions in corresponding sites. The authors conclude that as deaf individuals acquire and use sign they use the potential structures in the left hemisphere to organize and represent aspects of their language, just as hearing persons do. However, the modality of the language may determine which structures are recruited in that hemisphere.

What the Hands Reveal about the Brain is sometimes repetitive, perhaps because some of the chapters are based on previously published papers. However, the book holds one's attention. Its authors pose important and fascinating questions and pursue the answers in a manner that indicates a deep knowledge of language.

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Zooarcheology

The Archaeology of Animals. SIMON J. M. DAVIS. Yale University Press, New Haven, CT, 1987. 224 pp., illus. $27.50.

Zooarcheology, the study of animal remains from archeological sites, is a relatively new field, and the development of a consistent methodology is still under way. This is partly because animal remains come from many different kinds of sites, which present the zooarcheologist with varied problems, such as the separation of scavenging from hunting in early sites and of herding from hunting in much later ones. The quality of preservation also varies widely and requires methodological accommodation. Individual zooarcheologists tend to develop methodologies based on their own orientation and experience, and this is reflected in the divergent perspectives of many recent books on faunal analysis. In this book, Davis attempts first to explain some of the methods he thinks are important in zooarcheology and second to summarize the history of human-animal interactions, from the Plio-Pleistocene to historic times.

The text closely reflects Davis's own interests and background, especially his extensive research on late Pleistocene and Holocene faunas from the Near East. Understandably, it is strongest where his experience is greatest. However, it also draws upon examples from other parts of the world, supported by numerous fine illustrations. The emphasis is mainly on mammal bones, again reflecting Davis's own expertise, but mollusc and fish remains are also considered, particularly in a chapter on seasonality of site occupation.

Perhaps the book's weakest aspect is its treatment of quantification, a subject that concerns all zooarcheologists. The use of bone measurements to distinguish domestic animals from their wild precursors, to construct age profiles and sex ratios, and to infer mean individual size is adequately covered, but the more complex question of measuring taxonomic abundance is hardly addressed. Davis mentions some of the more important abundance measures, such as the number of specimens identified to a species (the NISP) and the minimum number of individuals (the MNI), but he does not discuss their respective merits and deficiencies. This is a serious shortcoming in a book designed to introduce readers to modern zooarcheology.

The second part of the book, a survey of human-animal relationships through time, is more successful. To summarize "our hunting past," Davis focuses on several major topics, such as hunting versus scavenging in very early sites, Quaternary extinctions, and the peopling of new worlds, including Australia, the Americas, Madagascar, and New Zealand. Partly on the basis of his own analyses of fossil faunas from Cyprus, he provides a particularly fascinating account of how Neolithic colonization of the Mediterranean islands probably contributed to the extinction of many endemic species, including a small antelope (the "mouse-goat"), pygmy elephants and hippopotami, dwarf deer, and giant mice and dormice.

The high point of the book is probably its treatment of animal domestication. Davis recognizes three major domestication cen-