is "not proven," as he *may* have been the vehicle ridden by a guilty Other, convictions elsewhere for dishonest antiquarian practices not being admissible as evidence in this prosecution. And perhaps the charge should be the misdemeanor of "common mischief" rather than the felony of "scientific fraud"—the cricket bat is an absurdity, and Oakley was in the habit of calling Piltdown not a fraud but a hoax.

The field method at Piltdown was simple. A laborer (quaintly named Venus Hargreaves) dug the gravel and spread it to be washed by the rain. Then Dawson and colleagues raked and sieved through the heaps, looking for curiosities, and then left it to be washed and raked again. The second Piltdown industry has come to resemble the first. The principals are long dead, and so now are the first generation of investigators and the many witnesses they talked to. Their work dug out a large body of papers and information that successive workers rake over for conjunctions of events and for significances passed over by the previous rakes. Each rake sees a new pattern, finds some little new curiosities and unnoticed anomalies. New characters enter, but they are not large ones. The white goose who appears in some of the photographs is known to be "Chipper," but the dog is not yet identified (*1*). The body of material under the rake has not changed materially, and the obvious goodies were taken out long ago. Introducing Spencer's work, the former Keeper of Palaeontology at the Natural History Museum remarks that there will be no end to the affair until "an unequivocal, signed and detailed confession comes to light." He is right. The chances of a confession coming now, from decades beyond the grave, are minute. And even a confession is not enough: like a celebrated murder, the Piltdown case is so notorious, Spencer reports, that the Natural History Museum files now contain several confessions sent in by various eager persons.

The companion volume, *The Piltdown Papers 1908–1955*, offers a digest of the collected sources as a thorough annotated catalogue of the documents in the Natural History Museum and elsewhere, with extensive quotations, notes, and indications of significance. It enables the Piltdown buff to rake from a distance, to find new patterns, and perhaps to name a new Other. The modern Piltdown industry begins more to resemble a parlor game than intellectual history. One may have one's own opinion as to whether the parlor game is in good taste.

This is a pity. The circumstances of contemporary scientific fraud, much in the public eye at present, are far removed from those of Piltdown. But there is a fascinating aspect, of enduring interest even to those of us who care not much for the identity of the goose, the dog, or even the Other. Piltdown was created in relation to an existing (and small) body of knowledge and of ideas about early hominids. It was plausible because it fitted sufficiently with some of the shape of that body. It became impossible as that shape grew and changed in directions that left it behind. Spencer gives proper attention to this aspect, of how science works as a fluid set of understandings shared among researchers in the field, and I find this much more instructive than yet more raking of old gravel. Who still cares, in the year 1990, who dunit?

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Note

1. The known Piltdown dog is Juno, who is buried 830 meters south of the Piltdown site. The dates on her headstone are 1918–1931, so she cannot be the Other dog in the excavation photograph. Her mother or father may have been there and told her all about it. An earlier Piltdown dog is recorded by a painting signed and dated to 1886. (I thank Denis Kenward for this new intelligence.) Notice that the doggy record is blank for the years material to the mystery!

Another Discrediting

Where the Truth Lies. Franz Moewus and the Origins of Molecular Biology. JAN SAPP. Cambridge University Press, New York, 1990. x, 340 pp., illus. $59.50; paper, $18.95

Hard-boiled historians have shown that some of the greatest scientists were secretive, fierce competitors who could cook and trim results with gusto in order to buttress their arguments. Yet journalists and congressional investigators perpetuate a naive idealization of norms of scientific behavior in their searches for "betrayers of the truth." The very eminence of men like Newton and Millikan seems to limit the ability to use their behavior to understand contemporary fraud controversies.

Jan Sapp proposes to bridge the gap between geniuses and crooks with the case of the German geneticist Franz Moewus (1908–1959). Between 1938 and 1940, Moewus, assisted by his wife, Liselotte, outlined the biochemical genetics of a microorganism (the alga *Chlamydomonas*) and presented a precise account of the means by which genes and enzymes controlled the activities of cells. World War II interrupted professional response to this work and pushed Moewus to take up the more practical subject of plant growth hormones, where he made little headway, intellectually or professionally. Yet his aging papers remained significant into the 1950s because the influential Indiana University geneticist Tracy Sonneborn considered them fundamental contributions to biochemical genetics. Sonneborn gave them priority—both chronological and conceptual—over Beadle and Tatum's *Neurospora* experiments and seemingly simple-minded model of gene expression. This interest ultimately drew Moewus to the United States in 1954 to replicate his experiments. At both Woods Hole and Columbia University he soon found himself accused of rigging results. A discreet announcement in *Science* (**122**, 470 [1955]) signaled the consensus that he was not credible and effectively ended his career.

Sapp's largest ambition is to use the Moewus case to show that labels such as "fraud" should be viewed as the periodic yet contingent outcomes of the ordinary competitive interactions among scientists, and not as the result of deviant individuals penetrating an otherwise upright community. I did not find this argument wholly successful. The narrative contains so many dangling, unexploited "clues" that a reader can plausibly conjecture that more detective work would lead to a classic whodunit solution. The most notable example here is Sapp's heavy and seemingly naive reliance on his interviews with Liselotte Moewus. More vigorous interrogation of this prime suspect—concerning the division of labor in their long collaboration, the procedures in their early experiments, and the "nervous breakdown" she suffered on learning they were coming to America—might reveal the location of some pertinent truths.

Still, Sapp provides a valuable critical review of the literature on scientific fraud and uses this perspective to recreate the murkiness of early molecular biology, thereby countering the triumphalism implicit in the image of a "path to the double helix." The most remarkable element of his story is the near unanimity of American biologists in willfully ignoring Moewus's papers for 15 years, in spite of Sonneborn's articulate demand that conscientious geneticists must take such significant claims seriously. The exception that proves this rule was the ambitious graduate student James D. Watson, who wrote a sympathetic seminar paper for Sonneborn on Moewus (but then ignored him after that). When Moewus was physically on the scene at Woods Hole and thus unavoidable, those working in the same field did and said the bare minimum necessary to discredit him, resenting that they had been forced to do so. This lack of engagement with Moewus's papers illustrates the importance of scientific "taste" in distinguishing

work that might matter from the amorphous mass of experimental reports. For good or ill, by the middle of this century American biologists had developed unusually discriminating standards.

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Decades of Turtles

Life History and Ecology of the Slider Turtle. J. WHITFIELD GIBBONS, with contributions by Harold W. Avery *et al.* Smithsonian Institution Press, Washington, DC, 1990. xiv, 368 pp., illus. $60.

Outside of the atypical case presented by our own species, the demography and population biology of long-lived vertebrates is poorly known. Long-lived vertebrates include many of the "high profile" endangered species that serve as the rallying points for conservation efforts, and often we don't know enough about their population biology to understand why they are in decline. This embarrassing dearth of information probably reflects the reluctance of funding agencies to support (or of researchers to undertake) descriptive studies having a long duration and an uncertain payoff. As a result, the *sine qua non* of population biology, life tables of age-specific rates of birth and death, are unavailable for most long-lived organisms.

Few long-lived vertebrates are as amenable to quantitative demographic study as turtles. Adults can live for many years, and during their lives they can reproduce many times. Each individual carries in its shell annuli a record of its growth and age. The shell also provides a convenient substrate for marking, so that the fate of known individuals can be followed, and a wealth of methods exist for capturing turtles and monitoring their reproductive activity and population dynamics.

In this volume, J. Whitfield Gibbons has assembled the results of such a study of the slider turtle, *Trachemys scripta*. Most of the information concerns the turtles studied at the Savannah River Ecology Laboratory, where the population biology of this strongly defended shelled organism has been elucidated as a decidedly nontoxic by-product of the manufacture of another kind of defense, nuclear weaponry. The slider turtle is common in the lakes used to cool production reactors at the Savannah River plant, although it is more readily studied in smaller nearby ponds. The study was originally supported by the Atomic Energy Commission to help understand the impact of thermal effluent on aquatic ecosystems and has been continued for over 20 years through the enlightened patronage of the managers of the plant. This book thus summarizes the results of a turtle population study of unprecedented duration, and these are supplemented with information about the turtle in other geographic areas.

Gibbons is an author or coauthor of 11 of the book's 24 chapters. Other chapters are authored by colleagues at the Savannah River Lab or by individuals working with the slider in other portions of its range. Apart from introductory and concluding chapters providing background information about the study, an overview of current theory regarding life history evolution in turtles, and recommendations for further research, the chapters fall into thematic groups: taxonomy and population genetics, reproduction and growth, population structure and demography, and bioenergetics.

Two broader themes emerge in the book's preface: the value of long-term descriptive studies, and the value of collecting data for their own sake in the course of research that is not hypothesis-directed. The suggestion that fruitful research need not be hypothesis-directed struck this reviewer as unfortunate. The literature in herpetology and population biology is littered with studies that would have benefitted from the clear articulation of testable hypotheses before the first datum was collected. A few of the chapters in this volume also fall into that category. Other chapters are far more successful.

One outstanding chapter presents a series of estimates for life table statistics for the best-studied population of slider turtles at the Savannah River Lab. Depending on the assumptions used, the population is predicted to be declining, in average and worst case scenarios, or approximately stable, in the best case. Mark-recapture data for the same population are in agreement with a precipitous population decline during eight years of the study. The conclusion is that this population, like the very few others that have been rigorously studied, is in a steep decline. Reasons for the decline are uncertain; possibilities include long-term climatic changes and habitat deterioration.

Other chapters contain a wealth of information on reproduction, growth, sex ratios, movements, feeding, and thermoregulation. There is much here to attract the interest of both herpetologists and ecologists. An extensive bibliography will also be useful to anyone with an interest in the biology of turtles. Consisting as it does of relatively independent chapters, the book provides a somewhat limited synthesis of the available information, however. Where the synthesis succeeds, as in the chapter on life tables, the results are impressive. In other cases, one is left wondering whether information on feeding, movements, or bioenergetics might be used to identify reasons for the decline of the population.

Despite the unusual duration of the central study represented in this book, the turtle population still contains individuals that were born before the study began. This observation underscores the fascination and difficulty of studying long-lived organisms. The suites of adaptations that we call life histories may be sculpted by events operating on very long time scales, and short-term studies may badly mislead us about the frequency and selective importance of such events. Indeed, one wonders whether the decline exhibited by the Savannah River population is only a relatively short-term setback for a population that might regularly weather such events. The problem offers a compelling reason for even longer studies of long-lived organisms.

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