Piltdown: Who Dunit? Who Cares?

Piltdown. A Scientific Forgery. FRANK SPENCER. Natural History Museum Publications and Oxford University Press, New York, 1990. xxvi, 272 pp., illus. $24.95.

The Piltdown Papers 1908–1955. The Correspondence and Other Documents Relating to the Piltdown Forgery. FRANK SPENCER. Natural History Museum Publications and Oxford University Press, New York, 1990. xii, 282 pp., illus. $65.

Piltdown is the most famous forgery in anthropology and perhaps even in science. Of the making of theories about who did it there seems no end.

The essentials are these. In 1912, Charles Dawson, lawyer and amateur archeologist, and Arthur Smith Woodward, senior British Museum geologist, announced fragments of the skull and the jaw of a new and early hominid, recovered from gravels at Piltdown, in the Sussex Weald of southern England. There followed, until Dawson's death in 1916, further finds from the pit and its neighborhood until Piltdown amounted to sufficient essentials of the ape-man or woman (opinion favored the skull being female)—parts of braincase and mandible, teeth, several tools in stone and one in bone, an associated fauna of elephants and hippopotami. He or she caused a great stir, at least in the British anthropological community, as scholars endeavored to relate this "earliest Englishman" to hominids from Java and Germany. The remains being fragmentary and those relationships obscure, dispute raged over rival reconstructions and the cubic capacity of the braincase. Nor did Piltdown mesh with new finds from the interwar years—the Taung baby, the first australopithecine fossil from South Africa, published 1925, and the large series of hominids from Choukoutien (China), published 1927. Confidence faded, and in 1950 Piltdown was unmasked as a fraud by Kenneth Oakley. The skull fragments come from a modern human skull of unusual thickness. The jaw comes from an orangutan, also of no great age. The bones had been stained to look old, the teeth filed to flatten their profile. The lithics come from elsewhere and the animal bones from as far away as north Africa. That was the end of Piltdown's career in science of just 38 years as *Eoanthropus dawsoni*.

With hindsight, fraudulent objects appear self-evidently fraudulent. The Piltdown assemblage had an honored place last year in a first-rate British Museum exhibition, *Fake!*, and fake it most certainly now looks. The braincase looks like a modern human braincase, the jaw like an ape jaw. How could anyone be fooled? The bone artifact—identified by Oakley as cut with a metal knife from fossil elephant bone—was nicknamed the "cricket bat," and that is just what it resembles. By what absurdity of the guiding unconscious or through what sense of surreal mischief did the forger choose to arm the Piltdown person with the broken bat of a player in that ancient national sport of England? (Although cricket in its modern form is not reliably reported before A.D. 1711.)

Since Piltdown's career in science ceased, a new industry has provided him, her, or it with a new vocation in a continuing mystery story, "Who faked Piltdown, and why?" The second career, after 40 years already longer than the first, continues in the pair of books under review. It began with Joseph Weiner's valuable explorations as to who was placed, by access, by knowledge, and by motive, to be the villain, published in his *Piltdown Forgery* (1955). Charles Dawson, it became clear, was in it up to the neck. Alongside Piltdown, the *Fake!* exhibit showed some of his other "discoveries," which run from forged Roman bricks to a toad, "perfectly" mummified and "found" sealed inside a hollow flint nodule. Access, motive—and experience in deception—Dawson had in full measure. Whether he had sufficient knowledge is doubted, so the detectives have thought in terms of Dawson and an Other—Dawson and Other in collaboration; or Dawson used by Other as a vehicle for Other's deception; or perhaps both these circumstances. Dawson's co-worker Smith Woodward is one candidate to be the Other. Further identifications have been made by industrious workers after their labors of sieving through the Piltdown deposits, especially the main spread of papers archived in the Natural History Museum, London: charges have been brought against the Jesuit paleontologist Teilhard de Chardin (most recently by Stephen Jay Gould, 1980); against William Butterfield, curator of the Hastings museum (by van Esbroeck, 1972); against the novelist and creator of Sherlock Holmes, Sir Arthur Conan Doyle (by Winslow, 1983); against the prehistorian William Sollas (by Douglas, around 1977); against the anatomist Sir Grafton Elliot Smith (by Millar, 1972); against the geologist Lewis Abbott; against the zoologist Martin Hinton; against the chemist Samuel Woodhead (by Costello, 1985); against the chemist Hewitt (by report of Daniel, 1986). In some opinion, there is more than one Other to be found.

Ian Langham, historian of science at the University of Sydney, died in 1984 while excavating in the Piltdown archive; he had come to suspect the anatomist Sir Arthur Keith, another principal in the events, and one of the few not yet named as the Other. Frank Spencer, professor of anthropology at the City University of New York, has brought Langham's work to conclusion in these two books.

The narrative history, *Piltdown: A Scientific Forgery*, has a straightforward and sensible structure. First, a valuable sketch of the search for early man at the turn of the century—the intellectual context for the making of the forgery and for its reception. Then, the detailed story of the finds and the controversies that followed. Then a chapter on the proof of fraud, and one on the roles of Dawson and of the several Others previously named. A final chapter, "Beyond a reasonable doubt?," makes Langham and Spencer's prosecution case against their new Other, Keith. Wisely, Spencer has separated this entirely from the main account, which reports events as they seemed at the time. Keith therefore appears in the main narrative as another honest researcher, and his further role is offered in the later chapter.

Spencer's narrative book is fuller than its predecessors and benefits from previous laborers in the industry. Here is the old tale, told better than before. As the decades pass, the color of the characters fades; the friendships and rivalries and spites that so concern the second Piltdown industry have long lost immediacy.

Keith was a Scot, born to a farm near Aberdeen; if he must be tried, it should be under Scots law. As well as the usual verdicts of "guilty" and "not guilty," Scottish law provides for a jury to return "not proven" when a case has been established but reasonable doubt persists. This juror would hesitate between "not guilty" and "not proven"—and that before hearing the case for Keith's defense. The new evidence, that Keith wrote for the *British Medical Journal* about Piltdown *before* the find was announced, does not look to me like a smoking gun. Even the stronger case against Dawson

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