Fraud By Numbers:

Quantitative Rhetoric in the Piltdown Forgery Discovery

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This paper is a contribution to two projects: the sociological analysis of debunking and "fraud-busting" in science and the discursive analysis of quantification rhetoric.[1] It attempts a partial deconstruction of the strong factual status accorded to the result of an exemplary piece of debunking work - the discovery, in 1953-4, of the fraudulence of "Piltdown Man". (For those who are unaware of what John Ziman has famously described as "the only well-known case" of "deliberate, conscious fraud" in academic science, Piltdown Man was for the forty years before this date a very famous, though increasingly problematic, paleontological find of some pieces of cranium and jaw which were taken to represent a significantly ancient humanoid; and for the forty years since, the "same" bones have represented an even more famous, and entirely *un*problematic case of forgery.[2]) I concentrate here on just one aspect of this debunking work: the ways in which the numerical results of a series of chemical dating techniques (most notably, the "fluorine test") were used to cast doubt on the authenticity of the assemblage. My strategy is to pick as many holes in these results as I can find with the object of weakening (just a little) the unchallengable character of the Piltdown forgery. *Your* best strategy is to be patient - and to concentrate. The materials I am working through here are extremely detailed. The rhetorical effect I am after, if you will so oblige me, is similar to that worked up in my protagonists' texts - the overwhelming of resistance through the cumulative effect of "a thousand blows." They were luckier than I, however. Their detailed labors were built into a story with a very dramatic outcome; so dramatic that the details could soon be safely ignored, as indeed many of them have

been. So I will own up. There is little drama here. The odd factoid questioned, one or two little puzzles set up. But no denouement, no revelation. Sorry.

[BREAK]

Are you still there?

[BREAK]

> The great lesson of the Piltdown business for me is that it is unwise to accept current scientific decisions and "proofs" as final, irrevocable, and conclusive, no matter how authoritative they may sound or look. Always keep in mind the possibility, no matter how small it may presently appear, that future evidence and improved scientific techniques may alter that proof, conclusion, or decision.[3]

> All the collateral lines of evidence appeared to be mutually confirmatory and in complete harmony with each other.... So much so, indeed, that ... none of the experts concerned were led to examine their own evidence as critically as otherwise they would have done.[4]

These two statements, as post-forgery accounts, intend to refer to, comment on, criticise and draw moral lessons from, the blindness of those who, prior to the discovery of forgery, accepted, wrongly, the genuineness of the Piltdown fossils. In short, they are accounts of error.[5] My aim is to make these statements refer, not to the career of Piltdown Man from 1912-1953, but to the career of the Piltdown Forgery from 1953 to the present. Doing this involves going against the advice of a *Nature* editorial on the occasion of the first announcement of the full extent of the forgery: "The Piltdown objects have lately been reinvestigated in such detail, by so many

specialists using so many techniques, that there can scarcely be factual profit from further work upon them".[6] But then, the skeptical sociologist of Science has a duty to go against Nature.

The heroes of the story of the discovery of the Piltdown forgery are Joseph Weiner, from the Department of Anatomy at Oxford University; Kenneth Oakley, from the British Museum, a pioneer of fluoride relative dating techniques; and Wilfred Le Gros Clark, a senior colleague of Weiner's. Their respective roles in the event were played along these lines: Weiner was the hypothesizer of the fraud, the initiator of the investigation and the revisionist historian; Oakley was the British Museum representative and chief technical tester; and Le Gros Clark was the establishment figure.[7]

Throughout their public life, the Piltdown bones were subject to two major interpretations. This interpretative flexibility was occasioned, in part, by their anatomical make-up (and incompleteness). The first and most important set of bones, known as "Piltdown 1" (the set from the first site, a gravel pit at Barkham Manor, Piltdown, Sussex; see Table 1 for a complete list), comprised some pieces of cranium, half a mandible with two molars attached and an isolated canine tooth found together with several other, seemingly prehistoric artifacts and faunal remains. The first interpretation of these finds was that they all belonged to a single individual, popularly dubbed "Piltdown Man." This monistic, single-creature theory was the official line supported by the British Museum and the scientists closest to the original discovery (and discoverer, Charles Dawson), such as Arthur Smith Woodward and Arthur Keith. It was this version that was celebrated by giving the bones a (single) new genus name - *Eoanthropus dawsoni* (Dawson's dawn man). This theory was reinforced by the 1915 finding of more

fragments from a second site (said to be Sheffield Park, about three kilometers from Piltdown) which were taken to represent another individual of the new genus: "Piltdown 2." The original significance of the monistic interpretation was that the bones could thereby represent a possible version of the long-sought-after, and Darwinianly predicted, "missing link" between ape and man. As such, this candidate had a humanlike cranium and an apelike jaw (though with rather humanoid teeth), thus providing potential evidence for human evolution as "brain-led". Archaeological and geological evidence of provenance led to an initial dating of well over 100,000 years old - thus making Piltdown Man not only the "Earliest Englishman," as it was jingoistically called,[8] but one of the very earliest "human" remains known at that time (1912-20). However, from the beginning there were critics (such as David Waterston and Gerrit Miller) who held to the alternative dualistic, or two-creature, theory that the cranium was that of a "human" and the jaw that of an "ape."[9] One feature of the artefacts which legitimised this version, was the damage to the jaw which made its physical articulation with the cranium fragments impossible.

Neither theory fit well with later finds from the 1920s and 1930s, all of which suggested that very early man had more simian cranial features than either version of Piltdown Man, whether represented by the cranial fragments plus the jaw, or by the pieces of cranium alone. Thus by the Second World War, Piltdown Man had been shunted into a marginal line in human evolution. All it did was exist; it was no longer significant; it was just embarrassing. And it became even more puzzling as a result of Oakley's (first) fluorine dating tests carried out in 1949 and published in 1950 which considerably reduced the probable age of the bones from (very roughly) 500,000 to (very approximately) 50,000 years.[10] Crucially, *neither* of the two competing and

unresolved theories about the Piltdown remains seemed to make sense given these results. The monistic interpretation suffered if one took the view, as most authorities did, that "no man could have possessed such an ape-like form [of jaw] at so late a stage in human evolution."[11] The dualistic theory also faced a severe problem: If the jaw did not belong with the skull, what did it belong with? "The great apes are totally absent from the fossil record of Britain and Europe and are highly unlikely to have inhabited the region during the upheavals of the Ice Age."[12]

Then Weiner came up with his Idea (reputedly on the evening of 30 July 1953).[13] According to his own account, Weiner produced the fraud hypothesis (at this point, that only the Piltdown 1 *mandible* - and the isolated canine tooth - were fake, planted to make the genuine fossil cranium seem older and more significant) as the only conceivable resolution for these chronic interpretative problems.[14] However, alternative explanations of the puzzle were then available, although, post-forgery, they have almost vanished from the record. For example, as Oakley wrote to Keith, "[Robert] Broom recently suggested that *Eoanthropus* may represent an isolated sideline in evolution, in which the brain became large as in *Homo sapiens*, while the jaws evolved in parallel with modern apes. In this case, the later [the] date of *Eoanthropus*, the more "modern-apelike" one might expect its jaws to be."[15] The contemporary availability of accounts such as this, which save the coherence of the assemblage even after the first fluorine tests reduced its age, suggests that the forgery hypothesis is somewhat less "necessary" than is usually claimed, most prominently, by Weiner himself.[16]

Another feature of the 1949 fluorine test which would become important only after the forgery hypothesis was in place was the similarity of the results for all of the bones tested (see Table 1,

cols. 1 and 3). As post-forgery accounts tend to put it, the results "failed" to differentiate between the age of the cranium and that of the mandible.[17] This was then, a crucial datum which counted against the idea that the mandible was a modern fake. If Weiner's hypothesis was valid, Oakley's test must be wrong. It is sometimes claimed that Weiner already had doubts about the test prior to getting his Idea: "In 1949, when Oakley had presented his [fluorine test] results ... Weiner broached the idea of the mandible being much younger than had been supposed" but "Oakley believed that it was unlikely that a modern specimen could have such a high level of fluorine."[18] However, with the help of Le Gros Clark, Weiner managed to recruit Oakley, who as a dating expert as well as a British Museum insider, was vital to the success of the project on both a technical and a political level. In putting their case to Oakley, then, Weiner and Le Gros Clark had to persuade him that, among other things, "the first fluorine dates must be incorrect and that all fluorine tests would have to be redone."[19] They were; and the results of this second set of fluorine tests fit well with Weiner's expectations. They showed not only that the bones had generally less fluorine than the first tests had indicated, but also that the mandible and teeth had considerably less fluorine than the cranial fragments (see Table 1, col. 2).

[TABLE 1 ABOUT HERE]

A whole battery of further tests and experiments were carried out which eventually led not merely to the 1953 confirmation of Weiner's original hypothesis that the jaw and teeth had been faked,[20] but to the comprehensive declaration, published in 1955, that every last piece of the Piltdown finds had been forged and planted:

> The mandible has been shown by further anatomical and X-ray evidence to be almost certainly that of an immature orang-utan; that it is entirely Recent has been confirmed by a number of microchemical tests, as well as by the electron-microscope demonstration of organic (collagen) fibres; the black coating on the canine tooth ... is a paint (probably Vandyke brown); the so-called turbinal bone is shown by its texture not to be a turbinal bone at all, but thin fragments of probably non-human limb-bone; all the associated flint implements have been artificially iron-stained; the bone implement was shaped by a steel knife; the whole of the associated fauna must have been "planted", and it is concluded from radioactivity tests and fluorine analysis that some of the specimens are of foreign origin. The human skull fragments and some of the fossil animal bones are partly replaced by gypsum, the result of their treatment with iron sulphate to produce a colour matching that of the gravel. Not one of the Piltdown finds genuinely came from Piltdown.[21]

These conclusions were instantly and universally accepted (with one exception: the dissenting dentist, Alvan Marston[22]), and the Piltdown forgery has been the hardest of hard facts ever since.

[BREAK]

> The development of chemical dating methods makes it possible to settle some of the problems which up to now have been matters of personal opinion.... the more such problems can be settled by methods which are *independent of intellectual traditions* the more rapidly our understanding of human evolution will progress.[23]

"Fluorine testing" is a chemical dating technique which, in the 1940s, was developed as a practical means for the relative dating of anthropological remains from the "neglected" and much earlier work of Middleton and Carnot.[24] Kenneth Oakley, who is usually credited with this development, first used the technique successfully to assess the ages of two other contentious British "early-Men": the Galley Hill skeleton, and the Swanscombe skull (or alternatively, to "test the validity of the fluorine methodology").[25] The tests showed that whereas the Galley Hill remains were much younger than had been thought (by their proponents) the Swanscombe specimen was genuinely ancient. Oakley's next use of the fluorine test was on the Piltdown assemblage.

The test relies on the idea that bones will adsorb fluorine from the environment at a steady rate over time. Thus, the amount of fluorine present (in the form of fluorapatite) can serve as an indication of age: the more fluorine, the older. However, the method is one of *relative* dating only, as the amount of fluorine adsorbed depends on the amount in the deposit, which varies from place to place. Therefore, it is best used to assess the relative ages of findings from a single location and thus, for example, to determine if certain bones are later intrusions (the conclusion from the test of the Galley Hill remains). Oakley frequently makes much of the danger of overinterpreting the test's results. Here is an early example of his cautious approach:

> Oakley [in 1943] ... said the progressive increase in the fluorine content of bones with increasing geological age was directly concerned with the amount of fluorine present in the deposit in which they were found. It was a statistical law, he said, and not applicable to individual specimens.... Although *a negative result would not be proof that a bone*

> *was recent*, a high fluorine content would be strong evidence of antiquity "in case of doubt arising."[26]

Oakley used this kind of technical argument to correct the "mistake" of directly comparing the numerical results of studies of artifacts from different sites. For example, Marston claimed that because Oakley's fluorine dating of the Galley Hill skeleton and of the Piltdown remains showed them to have similar amounts of fluorine, the latter should be interpreted as of a similar age to the former, which had been estimated to be from the Holocene period (i.e. geologically Recent).[27] Oakley did not agree:

> It is a mistake to suppose that the fluorine content of a fossil bone provides a direct indication of its geological ... age. In the case of the Piltdown material, fluorine analysis simply showed that ... the mandible and cranial fragments could not be separated ... and are contemporaneous with the latest fossils in the gravel.... [T]here can be no reason to doubt that *Eoanthropus* is Pleistocene [i.e. earlier than Holocene].[28]

And when commenting on the later radiocarbon dating results (see below), Oakley is also cautious about the utility and validity of fluorine dating: "Unless one knows the source of a bone, precise relative dating by fluorine content is impossible."[29]

In fact, as we shall see, only when discussing the results of the second Piltdown tests, done under the influence of the forgery hypothesis, does Oakley's technical caution vanish.

[BREAK]

> Analytical results of the Oakley-Hoskins 1950 determinations of the Piltdown materials which differ from those of 1953 are presumably to be taken as errors. They do not imply a lack of reliability of the fluorine method for deriving relative ages of bones in the same bed.[30]

It is clearly not sufficient, rhetorically, for immediate post-forgery accounts of the differences between the two sets of fluorine tests to cite the Forgery Hypothesis alone as the reason for the inadequacies of the first set, or indeed as the sole reason for their repetition. Weiner's account that we have already noted - "the first fluorine dates must be incorrect and ... all fluorine tests would have to be redone" (see note 19) - was published thirty years later, and posthumously, in an effort to establish that it was Weiner (rather than Oakley) who played the major role in the Discovery. Such bald statements of the test results' incompatibility with the assumption of forgery did not appear at the time. Instead, contemporary accounts of (1) why the second set of fluorine tests had to be done at all, and (2) why their results were judged superior, tend to emphasise *technical* issues. These fall into two camps: improvements in the analytic methodology, and the use of larger and/or heavier samples for analysis.

The notion of *improvement* is a very common trope in scientific discourse; indeed, the famed "progressiveness" of science is in large part textually constituted through its use. (There is a noticeable tension here between evolutionary and revolutionary change: between improvement brought about through continuous modification or through novelty and rupture. How this tension shows itself in this case will be discussed more fully below.) With respect to the second

fluorine tests, “improvement” is articulated in the post-forgery literature in two main ways, both of which amount to related yet distinct claims for the “increased accuracy” of the method.[31] The first of these involves the ability to measure smaller amounts of fluorine. This accuracy-claim is used as the main technical justification for having the tests redone:

> Improvements in technique have since [1949] led to greater accuracy in estimating small amounts of fluorine, and it therefore seemed worthwhile submitting further samples ... for analysis.[32]
>
> In 1953, new samples ... were submitted to the Department of the Government Chemist, where Mr. C.F.M. Fryd had devised a technique for estimating smaller amounts of fluorine than could be measured in 1949.[33]

But this notion of “increased accuracy” is ambiguous. The ability to measure “smaller” quantities could mean one (or both) of two quite different things. If we think of the space of measurement as a scale, it could mean the ability to distinguish more points on the scale (increased discrimination). Alternatively, it could mean the ability to detect points below the scale’s previous lower limit (reduced threshold). It appears that both of these versions of greater accuracy may be warranted if we compare the fluorine content (%F) figures from each test (see Table 1, cols. 1 and 2). Increased discrimination could be represented by the change from the one-decimal-place figures of 1950 to the two-decimal-place figures of 1953/55, while the decrease from the lowest 1950 figure (<0.l) to the lowest 1953/55 figure (<0.01) could represent a reduced threshold. Which of these versions is “meant” is unclear from the post hoc accounts, but I would argue that stressing the reduced threshold threatens to undermine the sense of objectivity achieved through the use of a discourse of technical progress. This is because it suggests a prejudgement of the outcome, namely, that there were only very small amounts of

fluorine to be found. On the other hand, the increased discrimination version does a great deal of useful rhetorical work. Apart from permitting the crucial possibility of discriminating between the jaw and the cranium (the main aim of the second set of tests), this version's textual representation (the use of figures with two decimal places) manages to *show* the much-vaunted methodological improvement "unarguably."[34] It should be noted, however, that at one point in the report of the earlier tests, a figure of the "later" order of accuracy is used: "0.05% fluorine".[35] This anomaly apart, it does seem that much of the warrant for the claim of increased accuracy is rhetorically achieved by the striking absence of (the *very* small) hundredths-of-one-per-cent figures from this text in contrast to their notable presence in the later ones.[36]

The second kind of technical improvement in fluorine testing claimed in the immediate post-forgery texts is the reduction of the method's margin of error by as much as an order of magnitude (from ± 0.2% to ±0.02%).[37] This form of improvement is used to explain the failure of the earlier test to distinguish between the Piltdown parts: in 1949, "no appreciable difference was found in the fluorine content of the skull and teeth of Piltdown man; this was due to the margin allowed for experimental error .... [which] was to have very significant results, though it was not suspected at the time."[38] Spencer, drawing on Weiner's own account, suggests that the margin of error (±0.2%) attributed to the 1949 test allowed the hypothesis of a modern faked jaw to surmount the obstacle of its published fluorine content (0.2%).[39] Given such a margin, the *real* amount "might well be less than 0.1 per cent - a figure more in line with that of recent bone."[40]

Margins of error are closely linked with the other technical issue mentioned above: sample size and/or weight. Their quantitative relationship appears to be a simple inverse correlation: the larger the sample, the smaller the margin of error. Thus it would appear that the claimed reduction in the 1953 test's margin of error implies a corresponding increase in the size of the samples analysed; and indeed this is exactly what is claimed. But, first, we should note that the size of the sample is not the only relevant variable effecting the size of the margin of error. As the forgery's discoverers themselves put it in 1955, "The experimental error in the determination of fluorine obviously depends on the size of the sample *and the amount of fluorine it contains.*"[41] Unfortunately, "the amount of fluorine" contained in a sample is precisely what is unknown prior to its determination by the test. This circularity puts all estimations of experimental error radically in doubt (see Table 2).[42]

[TABLE 2 ABOUT HERE]

So what were the margins of error for the two sets of fluorine tests? As can be seen from Table 2, accounts of this issue, like accounts of the related issue of sample size/weight are highly variable. This variation, however, is significantly patterned. In the case of the 1949 tests, the carefulness and subtlety with which the original published estimates of margins of error are described is not manifested in later texts. Moreover, the later accounts are marked by the virtual disappearance of the lower of the two 1950 estimates and the dominance of the higher estimate. As far as almost all post-forgery accounts are concerned, the 1949 fluorine test had a margin of error of ±0.2 percent.[43] I have set out (in Figure 1) the complete account[44] of the link between

the weights of the samples tested and the corresponding experimental errors, exactly as published in 1950, together with my interpretation and conclusions.

[FIGURE 1 EXACTLY HERE]

My main conclusion from this analysis, then, is that the *highest* margin of error that can legitimately be ascribed to *the majority* of samples is ± 1.0 percent. The naturalisation of the figure of ± 2.0 percent - which in the original text functioned as the absolute ceiling - as *the* margin of error for these tests, clearly serves the rather obvious rhetorical function of widening the gap between the degrees of accuracy of the two sets of tests. The worse (i.e. the less accurate) the earlier one can be made to look, the greater the perceived accuracy of the later one. As shown in Table 2, the 1949 threshold of accuracy in terms of minimum sample weight suffers a similar fate. In 1950 it is five milligrams; by 1953 (the first announcement of fraud), it has doubled to ten: "The method of analysis used in 1949 was accurate only within rather wide limits when applied to samples weighing less than 10 milligrams ...."[45] Later texts no longer include any specification of sample weights, apart from occasional comments on their inadequacy: "The amounts we were dealing with [in 1949] were too small to be measured except approximately."[46]

There is created, then, a general impression that the shortcomings of the first fluorine tests were due to the margin of error's being too high because the weight of the samples used was too low. However, there are several problems with this explanation of inadequacy. First, it is far too general. Such an explanation does not account for the fact that, according to post-forgery texts, the first test was only *differentially* inadequate. In particular, the 1949 result for the mandible

was taken to be far more wrong than those for the Piltdown 1 cranial bones. It is this problem that is resolved by de Vries and Oakley's otherwise puzzling decision to split the 1949 (and 1953) test results, together with their associated errors, into those for the cranium and those for the mandible. With respect to the 1949 test, the fluorine-content percentages are given as 0.2 ± 0.1 for the cranium and 0.2 ± 0.2 for the mandible.[47] In terms of Oakley's (own) account from 1950 (see Figure 1), this appears to suggest that the cranium samples weighed "5 mgm. and upwards" while the mandible samples weighed "less than 5 mgm." The use of only a small sample from the mandible in 1949 is again suggested in Weiner and Oakley's 1954 review of the evidence for fraud: "The fluorine content of the mandible ... was based on analysis of only a few milligrams of material."[48] But this is odd because it is reported in the 1950 text that five determinations were made on mandible samples (see Table 1, col. 1). If this means, as it appears to, that five separate samples were taken from the mandible, then, even if each sample was only around 5 milligrams, the total amount of material collected should have been enough to reduce the margin of error to an acceptable level. [49]

A second problem with the blanket "small sample/large error" explanation for the flaws of the first set of fluorine tests is that it leads to difficulties in explaining variations between the results of the first and second tests that do not lie within the range of even the most pessimistic margin of error ascribed to the first. An obvious example of this problem was noticed by Sherwood Washburn in 1953: "The fluorine content of the molar of Piltdown 2 [*E.* 648; see Table 1] was given as 0.4% in 1950 and now appears as <0.01."[50] This difference entails a margin of error that is nearly double the highest 1950 percentage of ± 0.2 for the smallest samples. Thus it cannot have been due to any recognised experimental error for this test; hence there was an

unknown and greater error in 1950, in 1953, or in both. Or perhaps it was just an anomaly - like another problematic datum for the standard account of error we are discussing. How else could supporters of this account explain a case where both the bad and the good tests yielded exactly the same result (see Table 1: Piltdown 2, *E.* 646, R. frontal[51])?

The final problem I will raise is this one: if the 1949 tests are to be treated as inaccurate due to the imprecision of the method as developed at that time, then all other uses of the method up to around 1953 are also put in doubt; including Oakley's own earlier dating of the Galley Hill and Swanscombe remains as well as his 1951 analysis of the Fontéchavade specimens.[52] But, you will not be surprised to hear, such doubts have not been raised.

The rhetorical process of widening the gap between the two sets of tests is also carried out "from the other end," so to speak. While in the first report of forgery, no margin of error is specified at all for the new fluorine tests, Weiner et al. do state that "the new estimations [were] based mainly on larger samples."[53] In a popular account published later, Oakley and Weiner again claim that larger samples were used, although this time the claim is made without the qualification of "mainly."[54] In the second, 1955 forgery report, we *are* given a margin-of-error figure - ± 0.02 percent, which contrasts very neatly with the ± 0.2 percent attributed here to the old tests - but we are not informed of the weights of the samples used. In de Vries and Oakley's 1959 text the margin of error is suddenly, and without explanation, reduced by half ("0.1 ± 0.01"); but, continuing their practice of splitting the sample types, this figure is applied only to the cranium measurements.[55] The fluorine content figure they quote for the mandible ("<0.03") is, I believe we are being asked to believe, so low that no meaningful margin of error can even be

computed. If the inverse relation between the sizes of samples and of errors holds here, then one wonders how much of the mandible could have been left after *this* determination!

As I have indicated in Table 2, it is frequently claimed that the samples used in 1953 were larger than the miserable "few milligrams of material" used in 1949.[56] Interestingly, the only contrasting account I have found is at least as authoritative as any of the others. In his own brief contribution to the 1955 report, the government chemist who carried out the tests, C. F. M. Fryd, describes the size of the available samples as "very small .... of a very few milligrams in weight."[57] Fryd's account also casts doubt on another standard claim that we have already noted: the suggestion that the analytical technique used in 1953 was both different and superior to the one used in 1950. Far from being a radical departure, the 1953 measurements were done "by adaptation of published methods," according to Fryd, who cites Oakley's two earliest studies, *including the 1950 Piltdown work*, as (the only) cases in point![58] Moreover, both sets of tests were conducted in the Department of the Government Chemist, where (the-new-and-improved) Fryd and (the-old-and-outmoded) Hoskins both worked - closely enough, indeed, to have coauthored an article on fluoride dating techniques [ADD TO BOOK VERSION: applied to "Piltdown and related fossils", post-forgery, in 1955].[59] It is in the difference between the discoverers' and the technician's accounts of the new tests that the tension between the evolutionary and revolutionary versions of scientific progress, mentioned above, is most clearly felt: where Weiner and Oakley stress novelty, Fryd emphasises modification.

[BREAK]

As can be seen from Table 1, two different measures of the amount of fluorine present were used in both 1949 and 1953. The first measurement, and the only one mentioned outside of the technical literature, is the "straight" fluorine content (%F) determination we have been discussing so far. The second one was a fluorine/phosphate ratio (%F ÷ %P_2O_5 [x 100]). Oakley describes the purpose of this measure in his 1953 review of fossil dating procedures: "It is now a routine procedure in fluorine dating to check the phosphate content of each sample analysed and to use the percentage fluorine/phosphate ratio as one basis of comparison, particularly where there has been some alteration or mineral contamination of the bone."[60] One result of the forgery investigations was to establish that most components of the Piltdown assemblage had indeed suffered extensive "alteration or mineral contamination". The investigators took such changes to be further evidence of fakery, and were not led by them to question the validity of the percentage-fluorine content results. Yet according to Oakley's own account of the method used in the 1949 tests, this was precisely what they should have done:

> In the case of coarsely porous bone, it is sometimes difficult to obtain a sample which is completely free from silt contamination. The fluorine content of a contaminated portion of a bone will obviously be *misleadingly low*. It was therefore decided to determine the phosphate content of all samples, and to express the fluorine value of each sample as the percentage ratio of fluorine to phosphate (as P_2O_5). This procedure facilitates comparison of the fluorine contents of bones in which there has been variable contamination.[61]

So, one possible explanation for what at the time seemed the surprisingly low fluorine readings presents itself: if any of the bones were found to be contaminated with silt (which does not seem impossible in either a pre- or a post-forgery examination), then their straight fluorine content figures would be "misleadingly low," leading to an interpretation of their ages as misleadingly modern.

In contrast to the 1950 account, the fluorine/phosphate ratio measure is not discussed in either of the two forgery reports, though its results are displayed in their respective tables.[62] And none of the later commentaries that I have read sees fit to mention it.[63] We can ask, then, why the measure that was acknowledged to be problematic in terms of technical adequacy would nevertheless have been the one overwhelmingly used in practice. A potentially plausible justification for the evident preference of all concerned to discuss these findings in terms of fluorine-content percentages alone would be that this measure is the "simpler" of the two and thus more readily understood. A more analytically interesting explanation would stress the rhetorical advantage to be gained, post-forgery, from the use of the percentage-fluorine figures rather than the fluorine/phosphate ratios. As can be seen from Table 1, the former are much *lower* than the latter. And because the forgery theory is helped by the bones' appearing to evince the greatest possible degree of youthfulness, the lower the numbers the better.

[BREAK]

In 1953, the new attempts to date the Piltdown remains by fluorine-content analyses were supplemented by tests for nitrogen content. "Whereas a fluorine assay reflects the gradual

accumulation in bone [and teeth] of an exogenous element, the nitrogen content indicates the progressive loss of organic matter from the bone itself. Accordingly, in fresh or recent bone the nitrogen content is very high while its fluorine content is very low; but with the passage of time this situation is reversed."[64] As is evident in Table 1 (cols. 2, 4, and 5), the nitrogen test results tended to confirm the differential ages of the cranial fragments and of the jaw and teeth, as assessed by the new fluorine measurements. However, one striking anomaly was soon noticed by Robert F. Heizer and Sherburne F. Cook (who are usually credited with developing a reliable method of nitrogen dating): the results for the occipital bone from Piltdown 2 (*E*.647).[65] In responding to Sherwood Washburn's summary of the first announcement of forgery,[66] Heizer and Cook claim that his assignment of this bone to the "modern" category, along with the mandible and all of the teeth, ignores the contradictory evidence provided by the percentage-fluorine (0.03) and the percentage-nitrogen (0.6) results. While the fluorine content of the Piltdown 2 occipital bone was far lower than all other pieces of cranium in the collection, and therefore indicated a relatively young age, its nitrogen content was also the lowest of all the pieces sampled, including the "modern" jaw and teeth. And this, of course, indicated a relatively ancient date. As Heizer and Cook comment, this bone could therefore belong to any one of three categories: "(1) a modern bone introduced as a hoax; (2) an archaeological specimen removed from its original site elsewhere and introduced to the Piltdown locality, and (3) an indigenous bone in the Piltdown gravels."[67] In their 1954 review of the evidence for forgery, Weiner and Oakley also noted the anomaly and attempted to resolve it by explaining away the indication of ancientness: "In a bone which may have been exposed on the surface for some time this [low nitrogen content] is no proof of antiquity".[68]

Interestingly, in a later discussion of their findings, Oakley and Weiner chose to make the opposite argument when presented with another apparent contradiction between the fluorine and nitrogen measurements of one of the associated faunal remains: “The ... hippopotamus molar tooth from Piltdown ... contains very little fluorine. On the other hand it has lost almost all trace of organic matter which indicates it is *certainly* ancient”.[69] Here, then, the lack of nitrogen is cited as evidence for this specimen’s being “certainly ancient,” while the contrary indication of its lack of fluorine is allowed to pass.

As I have indicated in Table 1, there are two other cases of apparent disagreement between the fluorine and nitrogen test results as reported in 1955. The fluorine test figures for the nasal and the turbinal bones (*E*.610a and b) are very high (indeed, the highest by far of the whole collection), thus indicating their comparatively great age, while the same bones also yielded relatively high levels of nitrogen which would indicate their youthfulness. Finally, the set of nitrogen test results for the Piltdown 1 cranium shows considerable variation while the corresponding sets of fluorine tests do not.

[BREAK]

> In 1953-55, the possibility of dating the Piltdown bones absolutely by the radiocarbon method was not seriously considered because it would have involved total destruction of the specimens to provide the minimum quantity of carbon (2 gm.) then demanded by radiocarbon laboratories for a single determination. During the last four years,

improvement of technique has made it possible to attempt radiocarbon dating on the basis of much smaller quantities.[70]

Another reason for the failure to employ radiocarbon dating at any time much before this period could be that the greatest age this technique can indicate is approximately 40,000 years. And only in the post-forgery world would it have seemed worthwhile to consider the possibility that the specimens were quite so young. When radiocarbon dating was finally applied in 1959 (after the obligatory "improvement of technique" had occurred) the results were given as follows:

mandible	500 ± 100 years
cranium (R. parietal, Pl)	620 ± 100 years.[71]

These "absolute" ages are interestingly at variance with the earlier fraud-finding fluorine/nitrogen relative datings. While the latter made the mandible modern and the cranium "fairly ancient",[72] the radiocarbon dates for the mandible and cranium were so similar as to have a possible eighty-year overlap. As it was precisely the ability of the second set of fluorine tests to *differentiate* the parts of the skull that gave it such high credibility compared with the first set, it has to be asked, in light of these near-identical radiocarbon results, whether the first fluorine tests were not, after all, in this respect at least, "better" than the second?

Since 1959, the Piltdown material has twice been redated by the radiocarbon method.[73] The first of these tests, done in 1964, reportedly yielded exactly the same numbers as were obtained in 1959.[74] The second, and much more recent, test (and therefore, surely, that much more

"improved") did not.[75] This 1989 test was done to assess whether the cranial fragments of Piltdown 2 could belong to Piltdown 1, as was strongly suggested by the 1955 report.[76] It involved testing a Piltdown 2 bone (for the first time) alongside the Piltdown 1 mandible, which was retested. (I do not know why the mandible rather than the cranium of Piltdown 1 was tested, given the stated rationale. But let that pass.) The results were as follows:

Piltdown 2 cranium	970 ± 140 years
Piltdown 1 mandible	90 ± 120 years.

Spencer notes that these results imply that the cranial fragments of Piltdown 1 and 2 "might well belong to two quite *distinct* individuals" after all; but what he does *not* comment on is the new, younger age (so young, indeed, that it may even be a pre-conception) of the forever-younger mandible.[77] Nor does he explain why these later (and therefore better) tests had a higher (and more variable) margin of error than the earlier ones.

[BREAK]

And that's it. That's all I've got. "Of course, I've only scratched the surface" (but us post-classical theorists [theorist? where's the theory here?] will tell you that the surface, the appearance, is all).

[BREAK]

So okay, I imagine you're asking yourselves, what do all these details add up to? Well, I warned you. "No denouement, no revelation." This text performs what it is about, that's all. Detail is detail.

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[1] On the first project see Malcolm Ashmore, "The Theatre of the Blind," *Social Studies of Science* 23 (1993): 67-106. See also Jan Sapp, *Where the Truth Lies: Franz Moewus and the Origins of Molecular Biology* (Cambridge, 1990). On the second, see Malcolm Ashmore, Michael Mulkay, and Trevor Pinch, *Health and Efficiency: A Sociology of Health Economics*, (Milton Keynes, Bucks., 1989), Chapter 7. See also, Jonathan Potter, Margaret Wetherell, and Andrew Chitty, "Quantification rhetoric - cancer on television," *Discourse and Society* 2 (1991): 333-365.

[2] John Ziman, "Some Pathologies of the Scientific Life," *Nature* 227 (1970): 996. Cited, inter alia, in William Broad and Nicholas Wade, *Betrayers of the Truth: Fraud and Deceit in Science* (Oxford, 1982), 83. For representative works on the first career of the bones, see Arthur Keith, *The Antiquity of Man*, (London, 1929 [1915], 2nd Ed., 7th Imp., 2 Vols.); Arthur Smith Woodward, *The Earliest Englishman*, (London, 1948). On their second career, see Charles Blinderman, *The Piltdown Inquest*, (Buffalo, NY,1986); Ronald Millar, *The Piltdown Men*, (St. Albans, Herts., 1972); Frank Spencer, *Piltdown: A Scientific Forgery*, (London, Oxford & New York, 1990); Joseph Weiner, *The Piltdown Forgery*, (Chicago, 1980, 2nd Ed. [Oxford, 1955]).

[3] E.A. Hooton, "Comments on the Piltdown Affair," *American Anthropologist*, 56 (1954), 289.

[4] Wilfred Le Gros Clark; quoted in Roger Lewin, *Bones of Contention*, (New York, 1987), 75.

[5] See Michael Mulkay and Nigel Gilbert, "Accounting for Error," *Sociology*, 16 (1982): 165-183.

[6] "The Piltdown Bones and 'Implements'," *Nature*, (10 July, 1954): 61.

[7] A rather less laudatory account of these figures is given in some private correspondence of the time: "...there was no Piltdown forgery .... With ... [Oakley] as the mouth-piece ... Le Gros Clark the windbag and Weiner as the garbage collector ... I have got them "holed" and am biding my time..." (Alvan Marston, letter to A. Gunner, 17 May 1955; quoted in Spencer, *Piltdown*, 229 n. 20).

[8] Woodward, *Earliest Englishman.*

[9] See David Waterston, "The Piltdown Mandible," *Nature* 92 (1913): 319; Gerrit S. Miller, "The Jaw of the Piltdown Man," *Smithsonian Miscellaneous Collections*, 65 (1915), 1-31.

[10] See Kenneth Oakley and C. Randall Hoskins, "New Evidence on the Antiquity of Piltdown Man," *Nature* 165 (11 March, 1950): 379-382. Pre-fluorine test estimates of the bones' age varied from 200,000 to 1,000,000 years. See Kenneth Oakley, "Dating Fossil Human Remains," in *Anthroplogy Today*, ed. A.L. Kroeber (Chicago, 1953), 47.

[11] John Reader, *Missing Links: The Hunt for Earliest Man*, (London, 1988), 76.

[12] Ibid. See also, William L. Straus, Jr., "The Great Piltdown Hoax," *Science*, 119 (26 February, 1954): 266.

[13] See Spencer, *Piltdown*, 133.

[14] Weiner, *Piltdown Forgery*, 30. See also G. Ainsworth Harrison, "J.S. Weiner and the Exposure of the Piltdown Forgery," *Antiquity* 57 (March 1983): 46-48.

[15] Letter, Oakley to Keith, 3 January 1950; quoted in Frank Spencer, *The Piltdown Papers 1908-1955*, (London, Oxford & New York, 1990), 189.

[16] Weiner, *Piltdown Forgery*, 26-35; Joseph Weiner, Wilfred Le Gros Clark, Kenneth Oakley, G.F. Claringbull and M.H. Hey, F.H. Edmunds, S.H.U. Bowie and C.F. Davidson, C.F.M. Fryd, A.D. Baynes-Cope, and A.E.A. Werner and R.J. Plesters, "Further Contributions to the Solution of the Piltdown Problem," *Bulletin of theBritish Museum of Natural History (Geology)*, 2 (6) (1955), 233.

[17] See, for example, Kenneth Oakley and Joseph Weiner, "Piltdown Man," *American Scientist*, 4 (October, 1955): 577.

[18] Spencer, *Piltdown*, 137. See also Harrison, "J.S. Weiner," 46-47.

[19] Harrison, "J.S. Weiner," (quoting Weiner), 47.

[20] Joseph Weiner, Kenneth Oakley and Wilfred Le Gros Clark, "The Solution of the Piltdown Problem," *Bulletin of the British Museum of Natural History (Geology)*, 2 (3) (1953): 141-46.

[21] Weiner et al., "Further Contributions....." 228.

[22] See Alvan T. Marston, "Missing Link - But He Wasn't a Fake, Says Alvan T. Marston FDS," *Picture Post* (19 December, 1953): 41-431; Alvan T. Marston, "Comments on 'The solution of the Piltdown problem'," *Proceedings of the Roval Society of Medicine (Section of Odontology)* 47 (1954): 100-102. I am preparing another paper on Piltdown which, among other things, deals in more detail with the arguments of this lone dissenter.

[23] Sherwood Washburn, "An Old Theory Is Supported By New Evidence and New Methods," *American Anthropologist,* 56 (1954): 437; his emphasis.

[24] A. Carnot, "Recherches sur la composition générale et la teneur en fluor des os modernes et des os fossiles de différents âges," *Annales des Mines (Paris)* 3 (1893): 155-95; and J. Middleton, "On fluorine in bones, its source, and its application to the determination of the geological age of fossil bones," *Proceedings of the Geological Society of London 4* (1844): 431-33. For some of the accounting strategies associated with "neglect" and "rediscovery," see Augustine Brannigan, *The Social Basis of Scientific Discoveries,* (Cambridge, 1981), chapter 6. For accounts of fluorine dating and its Piltdown connections, see Reader, *Missing Links,* 73-77; Blinderman, *Piltdown Inquest,* 65-69; Spencer, *Piltdown,* 128-29; and Sonia Cole, *Counterfeit,* (London, 1955), 152-56.

[25] According to Spencer, in *Piltdown,* Oakley's first use of the test, on the contentious Kanam-Kanjera remains discovered in Kenya by Leakey, was *not* successful "because of excessive amounts of background fluorine in the ... material" (129). On the Galley Hill skeleton, see Kenneth Oakley and M.F.A. Montague, "A Re-Consideration of the Galley Hill Skeleton," *Bulletin of the British Museum of Natural History (Geology),* 1 (2) (1949), 25-46. The uncertainty and circularity of whether an experiment tests a phenomenon (or theory) or tests "the validity of [a] methodology" (Spencer, *Piltdown,* 129) or the competence of an experimenter or theorist has been stressed by Harry Collins, *Changing Order: Replication and Induction in Scientific Practice* (London, 1985); and by Trevor Pinch, "Theory Testing In Science - the Case of Solar Neutrinos: Do Crucial Experiments Test Theories or Theorists?" *Philosophy of the Social Sciences,* 15 (1985): 167-187.

[26] Millar, *Piltdown Men,* 194; my emphases.

[27] See Alvan T. Marston, "The Relative Ages of the Swanscombe and Piltdown Skulls, with Special Reference to the Results of the Flourine Estimation Tests," *British Dental Journal,* 88 (1950): 299. For the Galley Hill dating, see Oakley and Montague, "A Re-Consideration."

[28] Oakley, "Dating Fossil Human Remains," 46, 47.

[29] H. de Vries and Kenneth Oakley, "Radiocarbon Dating of the Piltdown Skull and Jaw," *Nature* 184 (25 July, 1959): 225. It could be argued that it was only *before* 1953 that Oakley can be said to have *known* the source of the Piltdown bones: namely, the Piltdown gravels. Once the forgery hypothesis was in place, their source became radically unknown. The dating carried out in 1953 thus cannot, by this argument, be taken to be "precise."

[30] Robert F. Heizer and Sherburne F. Cook, "Comments on the Piltdown Remains," *American Anthropologist,* 56 (1954): 93.

[31] Cole, in *Counterfeit,* hints at a third claim for improvement: the use, in 1953, of a "blind" analysis. The samples "were contained in little glass tubes, each with a number. The chemists who did the tests had little idea what the samples were or where they came from" (156). Though she does not say it in so many words, the implication is that the 1949 test was not carried out in this rigorous fashion.

[32] Weiner, Oakley and Clark, "The Solution," 143.

[33] Weiner et al., "Further Contributions," 256.

[34] For an analysis of the rhetorical utility of other forms of "undeniabilty device," see Derek Edwards, Malcolm Ashmore, and Jonathan Potter, "Death and Furniture: On the Rhetoric, Politics and Theology of Bottom-Line Arguments Against Relativism," *History of the Human Sciences,* 8 (1995): 25-49.

[35] Oakley and Hoskins, "New Evidence," 381. For some reason, this figure was given in the text but not in the table.

[36] This rhetorical effect is spoilt somewhat by the use of considerably more "accurate" figures in a 1935 text on the measurement of fluorine content in (modern) human teeth (never otherwise cited in this literature); see Marston, "Comments," 101, citing P.J. Brekhus and W.D. Armstrong, "A Method for the Separation of Enamel, Dentine and Cementum," *Journal of Dental Research* 15 (1935): 23. (Their figures go to *four* decimal places. Very impressive for such an "early" work!)

[37] Weiner et al., "Further Contributions," 256.

[38] Cole, *Counterfeit,* 154, 153.

[39] See Weiner, *Piltdown Forgery,* 30.

[40] Spencer, *Piltdown,* 137.

[41] Weiner et al., "Further Contributions," 256; my emphases.

[42] A similar form of circularity, known as "the experimenters' regress," which is endemic to the experimental determination of novel phenomena, is beautifully explored in Collins, *Changing Order,* chapters 4 and 5.

[43] The single exception, as far as I know, appears in the 1959 account of the first radiocarbon dating: see de Vries and Oakley, "Radiocarbon Dating," 224. Here, the two 1950 values (0.1% and 0.2%) were

ascribed, re(tro)spectively, to the cranium and the mandible. (A possible rhetorical explanation for this is explored below.)

[44] Oakley and Hoskins, "New Evidence," 380; my emphases.

[45] Weiner, Oakley and Clark, "The Solution," 143.

[46] Oakley and Weiner, "Piltdown Man," 577. Note that "amounts" is ambiguous here: it could refer either to sampled material or to fluorine. For the purposes of my argument, I am treating it as the former.

[47] De Vries and Oakley, "Radiocarbon Dating," 224.

[48] See Joseph Weiner and Kenneth Oakley, "The Piltdown Fraud: Available Evidence Reviewed," *American Journal of Physical Anthroplogy* 12 (1954): 2.

[49] In 1953, Oakley stated that seventeen samples of *Eoanthropus* had been analysed; see his "Dating Fossil Human Remains," 52. Cf. Table 1, col. 1 and note *e*.

[50] Washburn, "Piltdown Hoax," 760.

[51] Interestingly, this bone has been interpreted as a fifth piece of the same skull used for the Piltdown 1 assemblage; see Oakley and Weiner, "Piltdown Man," 583. Recent radiometric dating results have cast doubt on this interpretation; see Spencer, *Piltdown*, 230 n.30.

[52] Oakley and Montague, "A Reconsideration"; and Kenneth Oakley, C. Randall Hoskins and G. Henri-Martin, "Application du test de la fluorine aux crânes de Fontéchavade" *L'Anthropologie* 55 (1951): 239-42. See also Oakley, "Dating Fossil Human Remains," 45-46; and Robert W. Ehrich and Gerald M. Henderson, "Concerning the Piltdown Hoax and the Rise of a New Dogmatism" *American Anthropologist* 56 (1954): 433-35. Note that Ehrich and Henderson do not themselves question the validity of these datings; indeed, they describe them as "soundly dated" (435).

[53] Weiner, Oakley and Clark, "The Solution," 143.

[54] See Oakley and Weiner, "Piltdown Man," 577. Elsewhere, an amusing account of how and why these supposedly larger samples were now available was offered: "Since Piltdown man had shed his aura of extreme antiquity after [1949], drilling could this time be carried out more boldly"; see Cole, *Counterfeit*, 155.

[55] De Vries and Oakley, "Radiocarbon Dating," 224.

[56] Weiner and Oakley, "Piltdown Fraud," 2.

[57] C.F.M. Fryd, "Chemical Changes in Bones: A Note on the Analyses," in Weiner et al., "Further Contributions," 266.

[58] Fryd, "Chemical Changes," 266.

[59] C.R. Hoskins and C.F.M. Fryd, "The Determination of Fluorine in Piltdown and Related Fossils," *Journal of Applied Chemistry* 5 (1955): 86-87.

[60] Oakley, "Dating Fossil Human Remains," 52.

[61] Oakley and Hoskins, "New Evidence," 380; my emphasis.

[62] Weiner, Oakley and Clark, "The Solution," 143; Weiner et al., "Further Contributions," 260, 262-65.

[63] Specifically, there is no mention of fluorine/phosphate ratios in Blinderman, *Piltdown Inquest*; Cole, *Counterfeit*; Reader, *Missing Links*; or Spencer, *Piltdown*, all of which discuss fluorine-content dating at length.

[64] Spencer, *Piltdown*, 139.

[65] See Heizer and Cook, "Comments," 93.

[66] Washburn, "Piltdown Hoax."

[67] Heizer and Cook, "Comments," 93.

[68] Weiner and Oakley, "Piltdown Fraud," 4.

[69] Oakley and Weiner, "Piltdown Man," 582, my emphasis.

[70] De Vries and Oakley, "Radiocarbon Dating," 224.

[71] Ibid. See also W.M. Krogman, "The Planned Planting of Piltdown: Who? Why?" in *Human Evolution: Biosocial Perspectives,* ed. S.L. Washburn and E.R. McCown (Menlo Park, CA, 1978), 238-51.

[72] Weiner et al., "Further Contributions," 257.

[73] See Spencer, *Piltdown*, 229-30 n.30.

[74] See J. C. Vogel and H. T. Waterbolk, "Gronïngen Radiocarbon Dates [Piltdown Series]" *Radiocarbon* 6 (1964):368. See also Spencer, *Piltdown Papers*, 198 n. 6.

[75] See R. E. M. Hedges, R. A. Housley, I. A. Law and C. R. Bronk, "Radiocarbon Dates from the Oxford AMS System: Archaeometry Datelist 9" *Archaeometry* 31 (1989): 207-234 (contribution of Frank Spencer and C. Stringer).

[76] Weiner et al., "Further Contributions," 260.

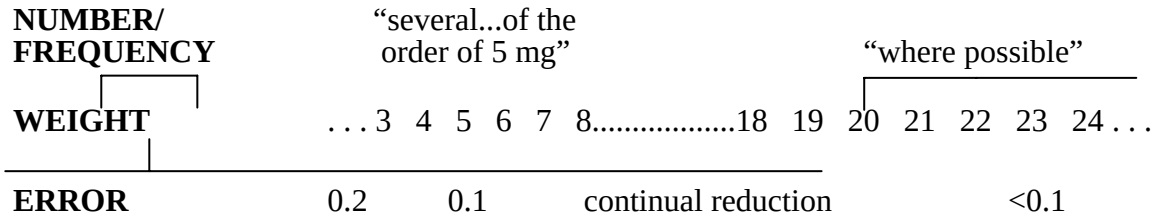
[77] Spencer, *Piltdown*, 230 n. 30.

Figure 1

Interpreting the 1950 account of weights and errors

The Account. "Where possible, *at least 20 mgm*. of bone was used for fluorine determination; but *in several cases* it was necessary to rely on samples *of the order of 5 mgm*. The errors of analysis naturally increase as the weight of sample decreases, but it is believed that with sample weights of *5 mgm. and upwards* the error in the adopted values *is not greater than ± 0.1* per cent of fluorine. For sample weights *less than 5 mgm*. the error *may be ± 0.2* per cent of fluorine."

The Interpretation. In at least some cases, sample weights were greater than 20 mg.; in some proportion of "several cases", sample weights were smaller than 5 mg.; *therefore (though speculatively*), sample weights varied from c. 4 mg. to c. 25 mg., with the range of "several cases" varying from c. 4mg. to c. 6 mg., *as shown (diagrammatically)* below:



The Conclusions. Some sampling errors were *possibly as great as* ± 0.2 percent; some sampling errors were *less than* ± 0.1 percent; and most sampling errors were *no greater than* ± 0.1 percent.

Table 2

Margins of Error and Sample Weights in the 1949 and 1953 Fluorine Content Tests

	Text	Margin of error (± %)	Sample type and weight (mg)
1949 Test			
	1950	"not greater than" 0.1	>5
		"may be [as much as]" 0.2	<5
	1953	"accurate only within rather wide limits"	<10
	1955a	0.2	---
	1955b	---	"too small"
	1959	0.1	cranium
		0.2	mandible
	Cole	0.2	---
	Spencer	0.2	---
1953 Test			
	1953	---	"mainly larger samples"
	1955a	0.02	---
	1955b	---	"larger samples"
	1959	0.01	cranium
		---[too small to measure?]	mandible
	Cole	---	larger sample
	Spencer	---	---

Sources:

Kenneth Oakley and C. Randall Hoskins, "New Evidence on the Antiquity of Piltdown Man," *Nature* 165 (11 March, 1950): 381 (1950); Joseph Weiner, Kenneth Oakley and Wilfred Le Gros Clark, "The Solution of the Piltdown Problem," *Bulletin of the British Museum of Natural History (Geology),* 2, 3 (1953): 143-44 (1953); Joseph Weiner, et al., "Further Contributions to the Solution of the Piltdown Problem," *Bulletin of the British Museum of Natural History (Geology),* 2 (6) (1955), 262-65 (1955a); Kenneth Oakley and Joseph Weiner, "Piltdown Man," *American Scientist*, 4 (October, 1955): 577 (1955b); H. de Vries and Kenneth Oakley, "Radiocarbon Dating of the Piltdown Skull and Jaw," *Nature* 184 (25 July, 1959): 224 (1959); Sonia Cole, *Counterfeit*, (London, 1955), 153-56 (Cole); and Frank Spencer, *Piltdown: A Scientific Forgery,* (London, Oxford & New York, 1990), 137-39 (Spencer).

Note:

A dash indicates that no quantification was given.

Table 1

Fluorine Contents (%F), Fluorine/Phosphate Ratios ($100F/P_2O_5$) and Nitrogen Contents (%N), as Reported in 1950, 1953, and 1955

British Museum registration number and description	%F		$100F/P_2O_5$		%N
	1950	1953/55	1950	1953/55	1953/55
Piltdown 1					
Cranium					
Given averages[a]	0.2[b]	0.1	---	0.8	1.4
Calculated averages[c]	0.25	0.15	1.3	0.8	0.95
*E.*590 L.parietal-frontal[d]	0.1(2)[e]	---	0.5	---	---
*E.*590a L.parietal	---	---	---	---	*1.9*
*E.*590b L.frontal	---	*0.15*	---	*0.8*	*0.3*
*E.*591 L.temporal	0.4	*0.18*	2.2	*0.8*	*0.2*
*E.*592 R.parietal	0.3	*0.15*	1.8	*0.8*	*1.4*
*E.*593 Occipital	0.2	---	0.7	---	*0.3*
Additional fragment[f]	---	*0.14*	---	*0.7*	*1.6*
Jaw, other bones and teeth					
*E.*594 Mandible (jaw)	0.2(5)	<0.03	1.0	<0.2	3.9
*E.*594 Molars (in jaw)	<0.04	0.4	<0.2	4.3	<0.1
*E.*610a Nasal bones[g]	---	*0.21*	---	*1.5*	*3.8*
*E.*610b Turbinal bone[h]	---	*0.28*	---	*1.7*	*1.7*
*E.*611 Canine tooth	<0.1	<0.03	0.4	<0.2	5.1
Piltdown 2					
Cranium:					
*E.*646 R.frontal	0.1	0.1	0.8	0.8	1.1
*E.*646 (1955)		*0.11*			
*E.*647 Occipital	0.1(2)	0.03	0.6	0.2	0.6
Tooth:					
*E.*648 Molar	0.4(2)	<0.01	1.3	<0.1	4.2
Controls					
Fresh bone[i]	<0.1	*0.03*	0.3	*0.1*	4.1
U.Pleistocene bones[j]	---	0.1	---	0.4	0.7
U.Pleistocene bones 1955 (range of values)[k]		*0.14 to 1.3*		*0.9 to 4.7*	*0.03 to 3.4*
Modern chimp molar	---	<0.06	---	<0.3	3.2

Sources: Compiled from tables in Kenneth Oakley and C. Randall Hoskins, "New Evidence on the Antiquity of Piltdown Man," *Nature,* 11 March 1950, 381 (1950); Joseph Weiner, Kenneth Oakley, and Wilfred Le Gros Clark, "The Solution of the Piltdown Problem," *Bulletin of the British Museum of Natural History (Geology)* 2, 3 (1953): 143-44 (1953); and Joseph Weiner et al., "Further Contributions to the Solution of the Piltdown Problem," *Bulletin of the British Museum of Natural History (Geology)* 2, 6 (1955): 262, 264-65 (1955).

Note: Roughly speaking, the *higher* the number in cols. 1-4, the older the sample; in col. 5, the *lower* the number, the older the sample. *Numbers in italics* are those from 1955 that do not appear in 1953, both sources having been reports of the same tests undertaken in 1953. Underlined numbers (in cols. 2 and 5) indicate apparent disagreements between the (%N) and (%F) results.

a In 1953, the test results for the bones comprising the cranium of Piltdown 1 (E. 590 to E. 593) were labeled "cranium" and not given individually. Thus these figures are averages, although in the case of both the fluorine (%F) and the nitrogen (%N) content figures, the given values are significantly at variance with my own calculations, based on the results for the constituent bones presented in 1955 (see n. c). The significance is that in both cases the given average, when compared to the calculated average, makes the cranium appear younger.

b This figure is the average given for the 1950 cranium results in H. de Vries and Kenneth Oakley, "Radiocarbon Dating of the Piltdown Skull and Jaw," *Nature*, 25 July 1959, 224. Note its variance from my calculation (see n. *c*).

c These figures are my own calculations of the average (mean) values for the (4?; see n. *d*) constituent cranium bones for which such values were not given in the 1950 and 1955 sources.

d Bone *E*. 590, described in the 1950 source as the "left parieto-frontal," is described in terms of two constituent parts in the 1955 source: *E*. 590a, the left parietal, and *E*. 590b, the left frontal. Note that *E*. 590a was tested only for nitrogen, while *E*. 590b got the full treatment, with no explanation offered either for this disparity or for the large difference between the (%N) results for these two parts of the "same" bone. Finally, on both types of fluorine measurement, the results for bone *E*. 590, however it may be divided up, were actually *higher* in the later test than in the earlier one; this bone, that is, looked older in 1955 than it did in 1950. Needless to say, this difference is *not* pointed out in the post-forgery literature.

e In col. 1, numbers in parentheses indicate the number of determinations made, if more than one; in these cases, the recorded figure is an average. (See discussion in text of an implication of the five measurements made on the mandible.) There is no mention anywhere of the number of determinations made in the 1953 analyses.

f Both types of fluorine test, as reported in the 1955 source, appear to have been done only on this apparently unregistered "additional fragment" and not, as reportedly was the case in 1950, on the occipital bone itself. When comparisons are made between the tests, this difference is ignored, yet to treat these two pieces as the same looks problematic, given the large difference between their (%N) scores.

g No mention is made of tests on the nasal bones in either the 1950 or the 1953 source. Note also the contrasting (%F) and (%N) results (see further discussion in text).

h No mention is made of tests on the turbinal bone in either the 1950 or the 1953 source. Note also the contrasting (%F) and (%N) results (see further discussion in text).

i In the 1950 source, this control sample is described as "fragment of fresh bone from soil"; in the 1953 source, only (%N) results for a sample labeled "fresh bone" are given; and, in the 1955 source, all three test results are reported for a sample labeled "recent; surface, Transvaal."

j In the 1953 source, both fluorine test results are described as "minimum F-content of local U. Pleistocene bones," while the nitrogen result is reported for "U. Pleistocene bone (London)."

k In the 1955 source, among the list of 14 "bones used for comparison" were 5, described as Upper Pleistocene, which had the range of values given here for each of the 3 tests. Oddly, not one of these 15 (5 x 3) values coincides with any of the 3 values reported in the 1953 source; and, in the case of both fluorine measurements, the figures given in the 1953 source are outside, and lower than, the range reported in the 1955 source. The significance of this is that, between the first announcement of fraud (when the cranium was not yet under suspicion) and the second (when it was), the "fossil barrier," as represented by this control measurement, had been raised. That is, in the 1955 report, "even" the cranium seemed more borderline than it had seemed (even) in 1953.