

Semiotix Course 2008, The epistemology of Pleistocene archaeology

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## Lecture No. 4. Mistakes in Pleistocene archaeology

In Lectures 1 and 2 we have briefly touched upon the question of mistakes that have been made in archaeology historically—but we have considered these only in the context of specific issues at hand, such as the treatment of dissenters who were eventually shown to have been right. Here we will consider more carefully the generics of this problem: are there definable patterns in the epistemology of mistakes? Can we identify common or systematic factors, or do blunders in archaeology occur in random patterns? Since the purpose of this exercise is not to discredit individuals, but rather to strengthen through constructive critique, I will provide no names of those who became victims of their own overconfidence (except where the actors have long passed away). I will also focus on case histories in which not a small group or an individual failed, but where most or even all of the discipline shared responsibility for the mistakes. The objective here is to examine what features or currents of archaeology are conducive to blunders of wide-ranging effects, and particularly to explore what specific aspects tend to facilitate the development and entrenchment of the trends and fads archaeology appears to be subject to.

### Piltdown man

One of the most celebrated frauds in the history of archaeology (although perhaps not its most consequential) concerns the find of nine fragments of a brain case and



Figure 1. The Piltdown hoax, comprising a modern human skull and an ape mandible.

a right lower mandible with two molars, found in a gravel pit in Sussex and unveiled to the ‘scientific’ world in December 1912. The Piltdown discovery consisted of an ape-like jaw with a seemingly modern human cranium (Fig. 1), and since archaeology was actively looking for a

‘missing evolutionary link’ at the time of the early 20th century, the find was fairly readily accepted. A few scholars were skeptical from the start, and during the 1940s, ‘Piltdown man’ became increasingly anomalous. Thanks to the detective work of Kenneth Oakley, a non-archaeologist, its fraudulent nature was ascertained in 1953. In the meantime, this false evidence had had a great impact on the study of human origins. For instance when Raymond Dart reported his Taung australopithecine hominin in 1925 it was greeted with contempt in Britain.

The culprit(s) of the Piltdown fraud was (were) never established, although Charles Dawson (the co-discoverer) was long suspected. After all, he was an amateur archaeologist, which already renders him suspect in the eyes of ‘real’ archaeologists. There are probably as many theories about who perpetrated the fraud and for what purpose as there have been prominent commentators. Among the principal suspects, apart from Dawson and Arthur Smith Woodward (the other co-discoverer), were Sir Arthur Keith, who was conservator of the museum at the Royal College of Surgeons and had spirited debates with Smith Woodward over how the fragments should be assembled; Australian Sir Grafton Elliot Smith, who was favored by Ronald Millar in his book, *The Piltdown men*; Professor William Sollas, a geologist from Oxford; Teilhard de Chardin, because he was a cleric; and even Sir Arthur Conan Doyle (the creator of Sherlock Holmes), who happened to live nearby and had been to the site. In Australia, Ian Langham favored Elliot Smith as the likely culprit, perhaps together with paleontologist Dr Smith Woodward as his accomplice. An interesting variation to a common theme was that Dawson, a lawyer, initiated the fraud, which was spotted by Hinton who sought to protect his superior, Smith Woodward, from ridicule, and tried to expose Dawson by subsequently planting more outrageous objects, such as a bone shaped like a cricket bat. The clumsiness of the fraud seems to be intentional, and appears to exclude Dawson as the perpetrator: if the fraud had been intended to remain undetected, it would have been executed much better. Rather than a fraud this appears to be a hoax, with Smith Woodward the intended victim (Fig. 2).

In the 1990s British researchers Brian Gardiner and Andrew Currant reported the discovery of fossil elephant and



Figure 2. Portrait painted in 1915 of the examination of the Piltdown find. Standing at the right are the discoverers, Dowson (left) and Smith Woodward.

hippopotamus teeth and various bones in a trunk stored in the Natural History Museum. The remains had been stained in the same way as the Piltdown material, and the trunk was marked with the initials of Martin A. C. Hinton, who had been curator of zoology at the time. He had already at the age of sixteen published a paper on how to impregnate bones with iron and manganese oxides to make them appear ancient. The relative proportions of iron and manganese were the same in the Piltdown finds and the objects found by Currant in the trunk, and both also contained chromium. The staining, it was argued, represents the evidence of a practice run, and it is claimed that Hinton wanted to dupe Smith Woodward because he bore a grudge against his colleague and superior who worked in the same institute.

If Hinton (Fig. 3) were indeed the originator, as appears rather likely on the basis of this information, the fake evidence would not have been intended to fool anyone but Smith Woodward himself. But it seems the hoax went terribly wrong: Hinton had not reckoned with the gullibility of the physical anthropologists, who ignored even the most



Figure 3. Hinton (left) and Dowson at the Piltdown site.

ridiculous additional material planted (and the cricket bat would surely imply a sense of humor on Hinton's part), and by the time he realized that his practical joke was accepted as real evidence by the discipline, instead of being exposed by it, he had no choice but to remain mute. So instead of damaging Smith Woodward's reputation and exposing his ignorance he ended up enhancing his status. One can only assume that

it taught him a lesson: never underestimate the gullibility of 'scholars'.

Seen in a historical perspective we can be grateful to Hinton (if indeed he was the perpetrator), who has shown us that a healthy disrespect for the pomp and ignorance of academia is most requisite. But whatever the true answer to the Piltdown riddle is, most commentators would agree on one point: with a very few exceptions, all those hoodwinked by the hoax were genuinely duped, they really did believe in the find's authenticity.

### Glozel

One of the most controversial archaeological sites is Glozel, in a small farming community near Vichy, central France. Discovered by a teenager, Émile Fradin, and his grandfather while plowing a field called Duranthon on 1 March 1924, it comprises tombs or underground chambers, and has yielded many thousands of finds. These are mainly inscribed ceramic tablets, engraved stones, urns with faces, flint axes, apparent idols, fine bone sculptures, glass (including high-potassium glass typical of the Middle Ages) and skeletal remains. Systematic excavations were initiated by an amateur archaeologist, Antonin Morlet, in the following year. He published a report later in 1925, naming the young discoverer as his co-author and defining the site as Neolithic. It was widely rejected by French archaeologists, no doubt because of the author's amateur status, but both Salomon Reinach, curator of the National Museum, and the famous abbé Henri Breuil authenticated the finds. Breuil, after publishing two papers in 1926 declaring his support, then changed his mind and in October 1927, claimed most of the finds were fakes, the exception being the ceramics. This occurred after some very curious developments. A close colleague of Breuil, André Vayson de Pradenne, approached Fradin under a false name and proposed to buy the collection. Fradin angered him by refusing and the archaeologist promised to "destroy the site". Indeed, he published a paper accusing Fradin of having salted the trench, after he obtained permission from Morlet to dig there (under his true name this time). Breuil, probably troubled by the fact that the reindeer, depicted on several items, had disappeared from France with the end of the Pleistocene, had identified an apparent reindeer engraving on a pebble as a 'cervid'. Morlet secured the judgment of the zoological director of a Norwegian museum that the engraving was of a reindeer, thus directly contradicting the 'Pope of Prehistory', which is when Breuil began rejecting Glozel.

The curator of the Louvre, René Dussaud, also accused the young Fradin of fraud, who responded by filing for defamation in early 1927 (having the pro bono support of a prominent attorney who was intrigued by the spectacular case of a 'peasant boy against the Louvre curator'). The Glozel inscriptions disproved Dussaud's life work, which was based on the assumption that our alphabet is derived from the Phoenician. A commission excavating at the site late that year for three days pronounced all finds as fakes, with the exception of a few flints. By that time the issue had already descended into a farce. During this investigation,



Figure 4. Morlet accusingly confronts Garrod, who hides behind several men and looks away with guilt; Glozel site, November 1927.

on 8 November 1927, Morlet observed three commission members slip under the site's barbed wire fence early in the morning, and then one of them, Dorothy Garrod, made a hole with her hand in the excavated section. Morlet immediately confronted her, she first denied the accusation, then admitted her action as there were several other witnesses, including an attorney and a science journalist (both of whom confirmed in writing having observed the incident). It is to be noted that Garrod, a distinguished archaeologist, had studied under Breuil, and like most members of this 'commission' was convinced the site was a fraud. Her indiscretion would not have come to light had the subsequent heated discussion not been photographed (Fig. 4). The principal author of the commission's report, Count Bégouen, even had to admit having falsified part of it: one of its members, Professor Mendes Corrêa, had supported the authenticity of Glozel, so Bégouen made up his commentary.

Next, the president of the French Prehistoric Society, Felix Regnault, filed a police complaint of fraud, leading to the confiscation of three cases of artifacts. During the police raid of his home, Fradin was beaten when he objected to the removal of his little brother's schoolbooks. Further excavations by a group of relatively neutral archaeologists took place in 1928, and they confirmed that the site was authentic. Nevertheless, in 1929 Fradin was indicted for fraud, but the ruling was quashed on appeal two years later. In March 1932, by which time the young farmer was 25, Dussard was found guilty of defamation and had to pay all court costs.

Much later scientific work has detected no evidence of fraud. Moreover, several sites with similar material are now known, including Chez Guerrier and Puyravel in the same region, and even as far away as in Portugal or Romania. The ages of bones from Glozel range from the 5th to the 20th century, while the ceramics fall into three groups, from 300 B.C.E. to recent times. The flints are from all accounts Neolithic. The most controversial components of the substantial collection from Glozel are the approximately one hundred ceramic tablets bearing an unknown script. It

has not been conclusively deciphered but appears to be of a Gaul dialect, dating apparently from the final Iron Age to the Roman period. It may derive from the Etruscan alphabet and vaguely resembles Phoenician script.

The inscriptions may well be of considerable value to the understanding of Gallo-Roman cultural connections, and their investigation seems to be more comprehensive than the archaeology of the site. Thermoluminescence analysis of many of the tablets places the majority of them between 300 B.C.E. and 100 C.E. It has been suggested that the characters resemble alphabets of the late Iron Age and the Gallo-Roman period, particularly the Lepontic script of northern Italy. The Lepontians spoke a Celtic language related to Gaulish. Gallic immigrants, who according to the Roman historian Livy migrated about 400 B.C.E. to the Po basin, are thought to have adapted the Lepontic script, itself derived from a northern Etruscan alphabet. It is thought that they developed a Gaulish-Cisalpine alphabet, which in turn was imported into Transalpine Gaul. This is thought to be the source of the Glozel script (Fig. 5).

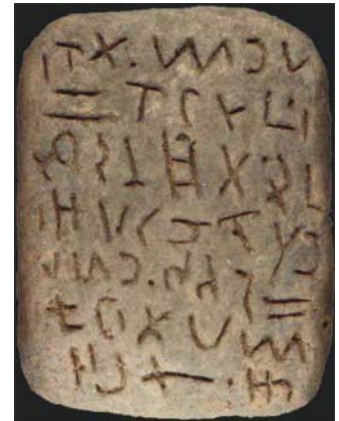


Figure 5. One of the approximately one hundred ceramic tablets covered with writing; Glozel, considered authentic.

The most likely interpretation of the substantial corpus of archaeological material from Glozel is therefore that it contained material of various phases, beginning with Neolithic flints, but mostly dating from the last two millennia. The bulk of the finds, including the tombs, may be of medieval age. The issue remains unresolved because of the highly controversial status of the assemblage, which has prompted a lack of published data until recent years. It is quite possible that the material from Glozel includes fakes, because many of the players had to lose a great deal of prestige had its authenticity been proved. And many of them had the opportunity to salt the site—in fact one of them, Garrod, was caught attempting to do just that. She confessed many years later that her interference was to preserve “the honor of the discipline—allowing Glozel to be recognized formally would have damaged too many careers and reputations”.

More interesting than the character failings of many of the archaeologists involved are the legal and practical aspects of this affair. First of all, it should have always been obvious that it is absurd to accuse a teenager of rural background of perpetrating what would have been an extremely sophisticated scientific fraud. How could the young Fradin possibly have concocted such an elaborate scam, constructed underground chambers and salted the site with thousands of finds? How could he have invented an appropriate script, made the authentic tablets and the flints? Or erected the subsequently discovered megalithic structures?



Figure 6. Engraving of what appears to be a reindeer, with Glouzel writing. This may possibly be a fake planted by archaeologists.

The proposition seems absurd, yet the archaeological establishment sought to resolve the issue by legal sanction, intimidation and surreptitious means. Surely Fradin was not the one to be blamed for the inadequate excavations of the site that apparently lumped together materials of widely different antiquities. Surely the fault here lies with the archaeological establishment, with its confrontational approach and its pompous self-importance. An innocent young man who had accidentally stumbled upon the site had to fight for eight years to clear his name, while people of considerable authority used their weight trying to crush and destroy him. Similarly, Morlet, who continued his efforts to secure scientific authentication for Glouzel until his death in 1965, died without having achieved it. The inability to untangle the mysteries of this site in several decades does not inspire great confidence in the many archaeologists who have contributed to this controversy.

Since it is absurd to attribute the alleged fake to Fradin, some commentators have even suggested that perhaps the site was salted well before its discovery, but this is again highly unlikely. If it had not been for the foot of Fradin's cow that broke into the first tomb, the Glouzel site may still remain undiscovered. Moreover, it is hardly realistic that the tombs could have been constructed by unknown hoaxers. Retrospectively it is difficult to see how so many leading archaeologists could have erred so profoundly. There was never any indication of fraud, except that which archaeologists themselves probably planted (Fig. 6). The whole story defeats every purpose of archaeology, and seems to illustrate what happens when a discipline gives way to self-interest.

### Côa

Now I would like to turn my attention to a much more recent and more serious controversy. A long-standing practice in Portugal of archaeological complicity in the destruction of rock art sites in that country was successfully challenged by Mila Simões de Abreu, who in December 1994 took on the archaeological establishment of Portugal. The destruction of cultural heritage is a worldwide phenomenon, but until that time it had never been challenged in any really consequential fashion. Archaeologists had long facilitated



Figure 7. Part of the massive Côa dam construction site in June 1995, at the time it was abandoned.

and often actually conducted the destruction of rock art and other immovable sites, instead of implementing the protective laws applicable around the world. Powerful developers and governments paid well for the clandestine eradication of such monuments that were in the way of development. With the support of the local and international media, and that of rock art researchers around the world, Abreu succeeded in stopping the construction of the Côa dam and the toppling of the Portuguese government in October 1995 (Fig. 7).

But this was not, as one would have been entitled to assume, the end of the Côa controversy. A university professor was appointed to head the restructuring of the discredited public archaeology of Portugal. Under his authority, an ardent effort to find evidence of Paleolithic human presence in the Côa valley was commenced, particularly in the vicinity of the rock art panels so as to 'prove' their Paleolithic antiquity. Sediments were churned up everywhere in this search and the rock art panels were systematically cleaned of their microflora. The wholesale removal of lichens with hard tools also removed fragile mineral accretions and rock flakes, and once it became known, Abreu and her supporters rightly voiced their criticism of these practices, labeling them as site vandalism. In the ensuing public brawl in the Portuguese mass media, the professor conceded that he had ordered the removal of the lichens. His admission was published in the national newspaper *O Independente* of 6 September 1996, under the title '*Broncôa. Zilhão admite limpeza das gravuras*' (*bronco* means stupid in Portuguese). He explained his actions as having been necessary due to 'political expediency', and he offered to resign if a commission of four scholars, *nominated by himself*, would find the accusation of professional vandalism justified. It was well known that the professor was fervently committed to proving the Côa petroglyphs to be Paleolithic, i.e. of the Solutrean. He vigorously and often abusively opposed the "fraudulent science" of archaeometry "imposters" and their "accomplices" who found the rock art to be quite young. That included me, I was one of three rock art dating scientists who were requested by the Portuguese government to test his age claims. When the discovery of the rock art was initially announced I had shared the view that it seemed to be of the Pleistocene, but after seeing detailed photographs



Figure 8. Photograph published of a Barca petroglyph, Côa valley, Portugal, before the lichens were removed in 1996. The dissection of lichen thalli by engraved grooves is visible.

and receiving a sample of the schist on which it occurred I counseled caution.

The need to remove lichens from the Côa sites is surprising, because lichen growth was not luxuriant, and there were no cases where lichens actually obscured petroglyphs. Lichen thalli in engraved grooves were mostly of microscopic size only. Sites where they were much better developed or had not been scrubbed before 1995 were never examined by the dating specialists, which is rather unfortunate. They would have offered numerous examples where large thalli were actually dissected by engraved or pounded lines, which shows that the petroglyphs must be younger than the lichens. Some of the greatest lichen concentrations had occurred at the site Barca, where two distinctive species formed discrete areas of growth. The engraved animal figures, where they cross lichen thalli, always postdate these zones, but some small thalli have developed in their grooves locally, postdating the petroglyphs (Fig. 8). By sampling the two sets of lichens for radiocarbon dating, those older and those younger than the petroglyphs, scientists could have acquired dating information bracketing the art with precision and reliability. By selective sampling of the dead lichen tissue they could have probably achieved dating of the art to within some tens of years.

Once the professor, whose views were shared by practically all the world's archaeologists who claimed to be rock art specialists, had taken up his position as director of Portuguese archaeology, he must have also discovered the remaining lichen occurrences that would disprove his hypothesis of Paleolithic age. Soon access to the Barca sites was limited to his own staff and associates, and numerous requests for access by other researchers were rejected. An American rock art conservation specialist, Jane Kolber, was forcibly ejected by a security guard from the neighboring site Penascosa on 29 July 1996. Why all the secrecy, and why ban observers from excavations?

There can be no doubt whatsoever that the rock art could have been dated through the lichens at five panels of the two

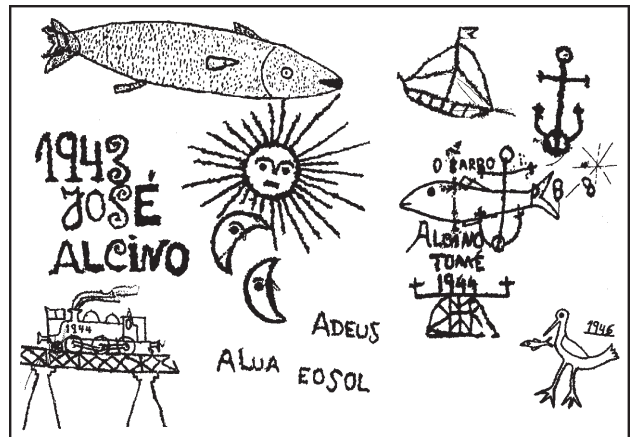


Figure 9. Typical examples of Côa petroglyphs.

Barca sites. Their physical and chronological relationship to the petroglyphs is obvious even to the layperson. The professor claimed that he had the lichens systematically removed because of some misguided site management policy, and because they might pose a threat to the rock art. He also claimed that he believed the rock art is well in excess of 20,000 years old. The Côa petroglyphs are exceptionally well preserved, so why did the removal of the lichens become so important after 20,000 years?

The professor's systematic scrubbing of the lichens with "wooden tools and river water", as he described it himself, has destroyed the possibility of determining the precise age of the petroglyphs via the related lichen thalli, and thus any testing of his stylistic dating of the art to the Solutrean may not be possible. These extraordinary developments in the Côa valley seem to be without precedent in the history of archaeology. Nevertheless, the mythology of the Paleolithic age of the Côa petroglyphs was supported by every commenting archaeologist in the world, and was (and continues to be) virulently defended by many of them. The true age of the overwhelming majority of the valley's engravings and other petroglyphs is in the order of one to four centuries, although a very few are older, and some may even date from the Neolithic. In addition to numerous clearly recent images (of locomotives, bridges, fish, trees, clocks, crucifixion scenes and the like; see Fig. 9) there are hundreds of inscriptions, many of them with dates. It is obvious even to

the casual observer that these dates, mostly of the 18th to 20th centuries, are often much more weathered than the adjacent animal drawings of Spanish fighting bulls, horses (one wearing a bridle, see Fig. 10) and ibexes the world's archaeologists contended to be of the



Figure 10. Horse image at Fariseu, Côa valley, claimed to be about 26,000 years old; note the depiction of a bridle (horses were domesticated about 5000 years ago).

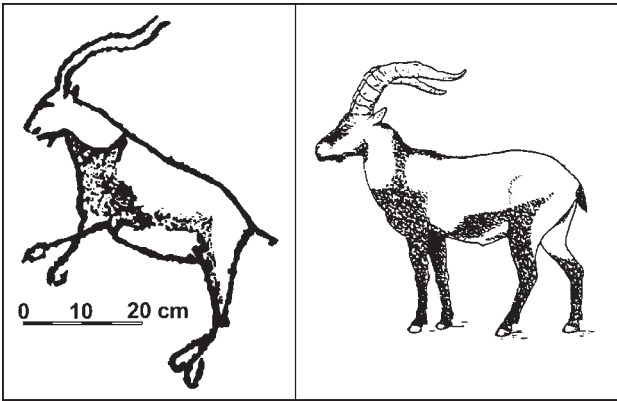


Figure 11. C $\hat{o}$ a depiction of a caprid (left), falsely claimed to be extinct in the region since the Ice Ages, compared with *Capra ibex victoriae*, an extant subspecies found in the wider region.

Pleistocene. The professor also claimed falsely that ibex had not existed in the region since the Ice Ages, and that a colluvial sediment covering some petroglyphs dated from that period. Both times he showed his ignorance: ibex still survive in the region, and in recent centuries were quite plentiful and a favorite target of hunters (Fig. 11); and a colluvium is always younger than any of its components. In the particular case the sediment purported to be 26,000 years old was in fact less than 17 years old! And a series of stone tools the professor had placed in the Upper Paleolithic were Neolithic microliths, which co-occurred with ceramics in practically all cases. Finally, scientific dating of some of the petroglyphs by several archaeometrists, conducted as a blind test under government supervision (to exclude the possibility of collusion between the three analysts), yielded essentially identical results: the rock art was relatively recent.

Nevertheless, the archaeologist in the center of this issue was not the victim of some hoax; rather he became the victim of a fervent desire to prove the art's very great age, and in this quest he ignored all evidence conflicting with his view, no matter how persuasive it was. The lichen thalli truncated by the engraved grooves clearly rendered a Paleolithic age impossible, and he probably realized that the lichens refuted his views. There are several lessons to be learnt from this controversy, the first relating to the possibility that some personal convictions may disqualify a person from exercising executive control over a rock art site. In the present context of exploring the epistemological reasons for the C $\hat{o}$ a controversy, however, it is more important to review two other issues: why did a presumably experienced Pleistocene archaeologist convince himself that 18th century images are of the Gravettian, and why did an entire discipline believe him? Being able to answer these questions would bring us a step closer to understanding what it is that renders archaeology, particularly Pleistocene archaeology, so accident prone.

It may be difficult to answer the first question, as we can never fathom the motivations of the individual, but the professor's objection to the "interference" of rock

art scientists, called "interlopers" by him, seems to have initially led to his stance, and his inability to retract hasty pronouncements, having been made very publicly, perhaps prompted his intractability on the issue. This may therefore be a case similar to that of Cartailhac, which we visited in the first lecture: relying on the often not refutable nature of archaeological propositions and on a personal hunch, a scholar finds himself in an academic cul-de-sac and, instead of backing out early, opts for seeking to 'preserve his reputation' by intransigence. This is then not necessarily attributable to the individuals' character, but rather to the academic conventions within which he operates. The Western system of academe encourages—even expects—the individual to defend a chosen position against all challenges. To admit that one was wrong is considered an academic defeat, and we have certainly seen very few such concessions in archaeology.

The systematic acceptance of the C $\hat{o}$ a claims by the entire discipline is perhaps easier to explain, on the basis of the available public commentary. These claims were accepted prematurely (before any details of the find had become available) by several key scholars of Paleolithic rock art, and authority and consensus are certainly the most powerful forces in the discipline. As we have seen from previous historical examples, influential scholars determine dogma, and the discipline then amplifies these views, especially if they come to the attention of the mass media. Whenever challenges arise, they are strongly contested, and most particularly so when they derive from 'outsiders': practitioners from other disciplines, and especially as the "impertinent transgressions of amateurs".

### The Jinnium blues

While we are on the subject of very old rock art: 'much older' petroglyphs have been reported in northern Australia, from the Jinnium site in the eastern Kimberley region, Western Australia. The considerable media hype around the world that followed the announcement in 1996 of this "oldest rock art in the world" could have easily led to a stalemate similar to the Portuguese example we just considered. Here, however, the controversy developed very differently, and this was purely the result of the particular mode of involvement of the press.

Jinnium is a small sandstone shelter whose wall carries a large number of cupules. These are hemispherical depressions hammered into the rock surface, a form of rock art that occurs globally and tends to be among the earliest manifestations of non-utilitarian or symbolic activities. Therefore the Jinnium rock art might have been a good contender for great antiquity. The sediment deposit in the shelter was excavated (Fig. 12), thermoluminescence 'dates' were secured from it, and an exfoliated rock fragment with cupules was found at a level corresponding to TL dates of 58,000 to 75,000 years ago. Based on the TL data, it was also claimed that the site's earliest human occupation occurred about 176,000 years ago—three times as long ago as the hitherto accepted first colonization of Australia. Instead of presenting these extraordinary claims for peer review in the academic literature, as should properly have been done,

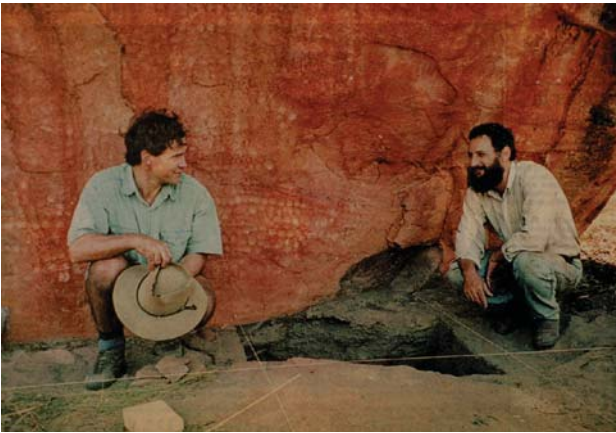


Figure 12. The cupules of the Jinmium shelter in the far north of Western Australia.

they were announced in an exclusive newspaper report in *The Sydney Morning Herald* on 21 September 1996.

This form of public announcement had several effects. First of all, other newspapers cried foul, saying that the research had been publicly funded and the announcement of its results should therefore have been made to all media, not just to selected newspapers. Secondly, the premature public announcement deprived the project of the safety net of open peer review. The ‘scientific’ article reporting the Jinmium claims was to appear two months later in the British journal *Antiquity*. So I suggested to the editor of that journal that he was entitled to cancel publication of the paper, because of the premature media hyperbole the material had been subjected to in the mass media. The editor responded by telling me that “he was proud to be a member of the research team” and would run the article. My subsequent suggestion that he publish with the paper also an opposing view to provide balanced coverage for what was becoming a very controversial theme was similarly rejected, a mistake that eventually cost the editor his job. The newspapers who had been slighted by the selective announcement began to test the claims by the Jinmium team, which rightly would have been the responsibility of the discipline, through refereeing the paper. So here we have the trial of a spectacular archaeological claim by the media, instead of the discipline, because the latter had not conducted such testing. Most of the media circus following the premature announcement of the Jinmium findings consisted of recitals of archaeological platitudes and countless errors of fact. For instance the most spectacular specific claim was that the Jinmium cupules are the oldest rock art in the world. The oldest *known* rock art in the world is in Auditorium Cave and Daraki-Chattan, India, and is safely attributed to the Lower Paleolithic (well over 170,000 years old; see Fig. 13).

A team of archaeometrists eventually conducted scientific dating and reported its results in May 1998. The problem with the TL method is that it seeks to determine the time mineral grains were last exposed to light, which would correspond to the time they became concealed by further sediment deposition. However, where the sediment is derived from the quartz grains of decomposing sandstone fragments exfoliated from a rock exposure above, only the

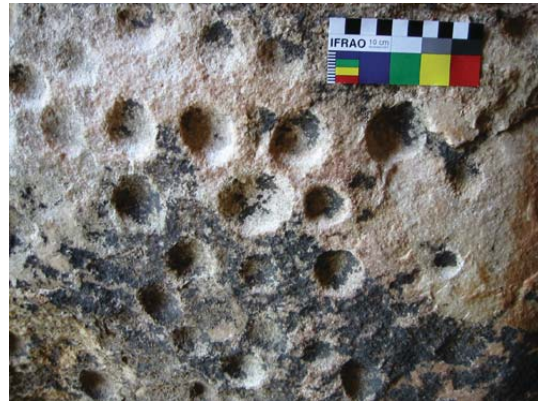


Figure 13. Cupules of the Lower Palaeolithic of India, at Daraki-Chattan.

grains on the surface of each fragment could have been exposed to light. As these clasts disintegrate in the soil the grains in their interior are freed, but the TL results from them suggest extremely high ‘ages’. The way to overcome this problem is to determine the last exposure time of only those grains that were actually subjected to light. This is done by analysing each sample grain individually, and then ignoring those values that are clearly much too high. This method is called optically stimulated luminescence method (OSL) and it yielded in the case of the Jinmium site perfectly sensible results, matching carbon isotope results. The sediment was entirely of Holocene age, i.e. less than 10,500 years old, and the oldest human occupation was estimated to be in the order of 6000 years ago. The exfoliated rock art fell to the ground around 4000 years ago. The entire issue was the result of misuse of a dating method for an application it was not intended for. It was a great embarrassment for Australian archaeology, and it could have been avoided easily. Fortunately it did not lead to a protracted controversy—at least partly because of positive press involvement.

#### ‘African Eve’: a gene fetish

A classical example of an archaeological blunder that held the discipline enthralled for well over twenty years, and that at its zenith held sway for almost all practitioners (although never quite managing to silence the last few dissenters), is the following highly improbable scenario. A new human species, miraculously evolving separate from any other populations and thus becoming unable to produce fertile offspring with other humans, arose in sub-Saharan Africa, and then expanded throughout the occupied world, replacing or exterminating all other humans in their wake. These ‘Moderns’, as they were called, could all trace their genetic lineage back to one single female, which the mass media facetiously dubbed the ‘African Eve’. This strange tale is called the ‘Replacement Theory’ in archaeology, and one of its key postulates is that the ‘African Moderns’ invaded Europe around 35,000 or so years ago and replaced (either exterminated or out-competed) the resident ‘Neanderthal people’. Indeed, the African *Übermenschen*, our ancestors, are said to have exterminated all other people of the world. Importantly, there is no archaeological evidence in favor of this model and it is based purely on the views

of some geneticists, rejected by other geneticists. Its foundations are fictional DNA base-pair substitution rates, unknown population sizes and incorrect assumptions about unique colonization events; therefore it does not appear to have any demographic justification. Despite these significant shortcomings, this model has for the past two decades provided the dogma for the origins of ‘anatomically modern humans’, especially in the globally dominant Anglo-American version of archaeology (see Lecture 3 concerning the regional fragmentation of the discipline).

Before this genocidal model’s weaknesses are considered, its obvious shortcoming is the way in which it addresses human modernity. It links it to specific skeletal differences (mostly concerning crania) and controversial genetics, when in fact it should be obvious that human modernity is primarily an issue of cognition, intelligence and culture. What renders hominins so different from all other primates are these factors, and not appearance or skeletal characteristics. So the model is initially couched in questionable premises, before we even consider its actual merits or problems. In a scientific sense, the question of ‘anatomically modern humans’ is an anthropocentric triviality; we do not search with the same enthusiasm for the origins of the anatomically modern fruit fly, because the first consideration of biology are taxonomic issues of separating species.

But when I began to examine the Replacement Theory more closely it became also evident that it was based on a previous model by a German palaeoanthropologist, G. Bräuer, called the Afro-European *sapiens* hypothesis. This, in turn was largely derived from a series of radiocarbon datings of human remains from Europe. In 2003 it was discovered that most of these datings were false. Several had been provided by Professor Reiner Protsch ‘von Zieten’ and were as bogus as that man’s aristocratic title and second doctorate (he was heavily fined by a German court for the second imposture). Protsch had supplied the discipline with false ages of skeletal remains for about 30 years before it was discovered that he could not operate the analytical equipment, and that he had simply guessed all of his results. They turned out to be spectacularly wrong, for instance the Paderborn-Sande skull fragment, dated by him at about 27,400 years, turned out to be only 238 years old. Although this affair is just as damaging to the discipline as the Piltdown affair was, it remains almost unknown outside of Germany; it was essentially hushed up by the discipline.

Moreover, this development coincided with the announcement that several further human remains attributed to the crucial period from about 35,000 to 28,000 years ago—the latter date marking the time when the ‘Neanderthals’ were said to have finally disappeared—were also false. These fossils, too, were younger than the dogma of human evolution had demanded. In fact by 2006 it became obvious that there are no fully anatomically modern human fossils of more than 27,700 available in Europe, i.e. from the entire first half of the Upper Paleolithic period—when the replacement advocates had consistently claimed that this technological change was introduced in Europe by the

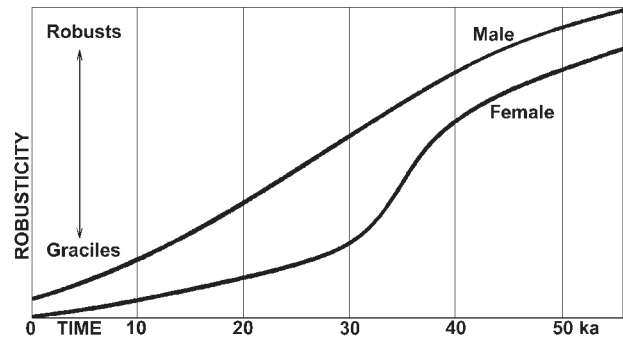


Figure 14. Schematic depiction of male and female relative cranial gracility in Europe through time, showing that the decline in robusticity is gradual in males, but accelerated in females between 40,000 and 30,000 years ago.

African invaders. In contrast, human remains defined as ‘Neanderthals’ were being found from this period, and in 2007 I proposed that even the rock art and portable art of a period around 32,000 years ago (called the Aurignacian) is either the work of Neanderthaloid people, or of their immediate descendents. European humans experienced a gradual change from very robust cranial and other skeletal features to ‘gracile’ features, which began roughly 50,000 years ago and is still continuing today. For each ten millennia we proceed from the present time into the past, Europeans were about 10% more robust than they are today, and there is in fact no indication of any sudden change in their morphology, at any point in time: Europeans descend from the ‘Neanderthals’ (Fig. 14). Similarly, there is no sudden appearance of a new technology, of totally new types of stone tools or any such indication of a material culture introduced from outside. All of the cultural traditions we distinguish in the first half of the European Upper Paleolithic (c. 45,000 to 27,000 years BP), of which we distinguish about fifteen, developed locally and had no precedents in Africa. Similarly, art-like products such as beads and engravings had been in use for hundreds of thousands of years, and were not introduced from Africa a mere 35,000 years ago.

Thus the entire Replacement Theory dissolved in a puff of smoke, and was even replaced with another, far more plausible model that has the support of the archaeological evidence. This is the ‘domestication hypothesis’, which takes note of the fact that the gracilization of robust humans is not a specifically European phenomenon, but it occurs in all four continents then occupied by humans, and at about the same time. This surely demands a systematic reason for such developments. The archaeology of the last 40,000 years recognizes a rapid acceleration in cultural and technological development, and I have cited extensive evidence suggesting that cultural imperatives were becoming so strong that they began to impact, at a small scale, on mating patterns. Because gracilization of humans begins in females, who still today are many millennia ahead of males in this development, I suggested that a cultural (conditioned) preference for neotenus fe-



males led to a gradual reduction in robust features. In other words, humans domesticated (i.e. influenced their own genetic makeup) themselves, albeit of course unintentionally. Biologically, the two most outstanding features of today's humans are their state of encephalization (the phenomenal development of the brain) and their pronounced neoteny: adult humans resemble fetal chimps much more than these resemble adult chimps: we are essentially neotenous apes.

Irrespective of this new hypothesis, the notion of an invasion of, first, the rest of Africa, then of Europe, Asia and Australia by a 'super-race' of genocidal people from sub-Saharan Africa is now fully superseded. It has no empirical foundation, and the idea that genes in modern populations indicate the mass movement of people in the past is absurd. Today's distribution patterns of genes are much easier to explain as the outcome of reticulate introgression, genetic drift and generational mating site distance. The important question that remains is to determine why the discipline has so enthusiastically embraced a rather cynical theory that seems to explain genocide and emphasizes extreme competition among groups of humans. Actually, this was again much more pronounced in the Anglo-American school than elsewhere. Most notably the Chinese tradition of hominin research has steadfastly rejected the Replacement Theory all along, and several other schools were at best lukewarm in their support. It would then seem that the British school is particularly prone to fads in Pleistocene archaeology. This is apparent from several developments, such as the old chestnut of whether there is any Pleistocene rock art in Britain. Perhaps there is, but the issue is firstly not particularly important, and secondly the documentation of such claims has always been inadequate.

So why was the African Eve so enthusiastically welcomed, especially in Britain? The answer is to be found in the books of her most ardent supporters. They were unaware of much if not all the contradictory evidence, and they mistakenly conflated the issues of skeletal morphology with technological and other changes: according to them, 'modern' anatomical appearance equals modern cognition and more sophisticated tools. But none of the developments in technology or art production seems to coincide with the purported appearance of the 'Moderns' from Africa. For instance it has been known since 1979 that the 'Neanderthals' of a tradition called the Châtelperronian used art objects, so this was explained away by the absurd proposition that they must have scavenged these symbolic objects, such as beads, from the 'Moderns'. Which begs the question: why would creatures that have no concept of symbolism scavenge symbolic artifacts? To eat them? This shows the accommodative nature of such mythical constructs of some archaeologists, but it also shows that other practitioners seem unable to see through such thin arguments.

### The Hobbit myth

One of the most recent examples of a significant blunder has not yet been exposed, so what I present here cannot be final. This is a race still being run, and it may be some decades before a clear outcome can be determined. But here

is the story so far, followed by a prediction of what it may lead to. One of the key attributes of a scientific proposition is that it must be testable; it must be open to processes of falsification. My proposition about the outcome of this controversy is testable: if we are patient enough and still alive in thirty or forty years, we may see its closure one day. From Lecture 1 it will have become evident to the reader that it takes that long to resolve any major controversy in palaeoanthropology.

During the early to mid-1990s, in the course of examining the epistemology of the Replacement Theory, I realized that archaeologists were generally unaware of much of the relevant information. One example of this is the fact that Theodor Verhoeven, a non-archaeologist who had investigated the past of the island of Flores in the 1950s and 1960s, had demonstrated that humans occupied this island during an early phase of the Pleistocene. Just like Boucher de Perthes a good century earlier (see Lecture 1), he had excavated Lower Paleolithic stone tools occurring together with the remains of extinct fauna, in his case dominated by stegodonts. Since Flores was never connected to the Asian landmass, this meant that the hominins concerned must have crossed the sea, an ability the replacement advocates could not credit these early people with. After writing a couple of rather critical papers on this topic, I prompted a colleague, Dr X, to reopen Verhoeven's excavations and test his claims. He found them fully justified and fine-tuned the dating of the finds to over 800,000 years. *Homo erectus* had obviously reached Flores, and no doubt other Indonesian islands. In 1997 I visited Dr X at his excavations and sensed that, despite his success he was looking for more than stone tools; he was trying to secure skeletal remains of the people concerned. I explained to him that for this, he needed to dig in a very different site: one with high-pH sediments and in a well-sheltered location, rather than an open site with acidic soils. He asked what I recommended, and I suggested he look for a very spacious limestone cave with a very deep sediment. He did precisely that and ended up excavating in Liang Bua ('Cold Cave'), a cave Verhoeven had investigated much earlier.

In 2004 Dr X and his colleagues reported a new hominin species from the cave, which they named *Homo floresiensis*. Called the 'Hobbit' because of its exceedingly small size, at 1.06 m or so height, it became an immediate world sensation (Fig. 15). With a brain size little greater than that of chimps, the little people from Flores were claimed to be so different from any other known hominin that they represented a separate species, if not genus. They were reported to have lived on Flores in the last ten millennia of the Pleistocene, i.e. at a



Figure 15. The skull of the first Flores 'Hobbit' reported, on left, besides a modern skull.

time when the rest of the world was entirely occupied by 'modern humans', *Homo sapiens sapiens*. This announcement was soon followed by numerous opposing interpretations, and within a year or two, these included practically every possibility from a gibbon-like creature (according to Gert van den Bergh) to a modern but microcephalic human. Some commentators saw the creature as a dwarf *H. erectus*, others as deriving from *H. dmanisi* or *H. habilis*. Others again suggested it was an Asian australopithecine, but the strongest argued alternative case was for a severely dwarfed and perhaps pathological modern human. Together with a



Figure 16. Teuku Jacob in 1992, pointing out where the skull of the Mojokerto child was supposedly found. Photograph by C. C. Swisher.

number of fragmentary specimens, stone tools had also been excavated, and there was no doubt that these were similar to the lithics modern humans made elsewhere about that time. The controversy has raged ever since. The elder statesman of Indonesian paleoanthropology, the late Teuku Jacob (Fig. 16), pronounced the fossils as those of insular dwarfs, at least one of whom suffered from primary microcephaly, microcrany, microencephaly and micrognathia, which caused mental retardation, a disruption in brain growth especially in the forehead and cerebellum. This, he said, resulted in a passing resemblance to *H. erectus* and *H. ergaster* skulls.

A bitter confrontation between him and the discoverers of the remains ensued, with many others throughout the world taking sides. The range of views expressed suggests that the discipline is incapable of conclusively deciding whether this is an ape or a modern human, or any possible contender intermediate between these two extremes. In a new twist physical anthropologist Maciej Henneberg announced in April 2008 that the first lower left molar of one of the fossils showed signs of dental work. Henneberg detected the presence of material he suggests might be a tooth filling of a type used in the region around the 1930s. The specimen has been claimed to be about 18,000 years old, and it is rather difficult to see (though not entirely impossible) how it could have ended up several metres below ground in less than a century.

This only served to deepen the academic chasm between those experts who see the 'Hobbit' as a distinctive species of very ancient features, and those who see it as a result of severe congenital or genetic defects caused by insular isolation of a tiny gene pool (Fig. 17). As early as 18 February 2005 I commented publicly that "until the [Australian] team considers in its publications ALL Asian pygmy remains of the

Pleistocene in a balanced and scientific fashion, their inadequate knowledge of the issue remains a problem. It appears that [the] team needs to improve its knowledge of the subject." Most importantly the team had not known about the occurrence of other very small humans in the Asia-Pacific region, which is often the result of isolation of small founder populations. For instance they were unaware of the Liang Toge skull, also from Flores,



Figure 17. Artist's rather fanciful reconstruction of the Flores 'Hobbit'.

which Verhoeven had considered to be of a proto-Negrito, and of extant Flores pygmy populations (e.g. Rampasasa village). They were unfamiliar with other such specimens, most importantly with the long-established presence of very small people, about the same size as their 'Hobbit', in central India up to 200,000 years ago. Announcing to the world a new, 'radically different' species of human in the absence of such relevant knowledge was rather foolhardy. Among the insular pigmoid populations of the wider region are those found on the Andamans, whose art I have studied (Fig. 18), and on Micronesian islands such as the Palau Ar-



Figure 18. Pygmoid Jarawa man, Andaman Islands.

chipelago. Lee R. Berger, a South African, excavated with his team (Steven E. Churchill, Bonita De Klerk, Rhonda L. Quinn) Ucheliungs and Omedokel Caves (Chelechol ra Orrak) on the Rock Islands, where at least ten burial caves are known, and the remains of dozens of tiny human skeletons have been found. These are about the same size as the first Flores specimen, with adult body weight estimated to be as low as 28 kg, and they, too, exhibit distinctive traits often interpreted as primitive. These include reduction of the absolute size of the face, pronounced supraorbital tori, non-projecting chins, relative megadontia, expansion of the occlusal surface of the premolars, rotation of teeth within the

maxilla and mandible, and dental agenesis. The brain size is not as low as that of the first Flores specimen, but resembles that of *H. erectus*. The Palauen pygmies date from between 2900 and 1400 years ago, after which time they appear to have been replaced by larger people. Berger's team has no hesitation defining them as fully modern *H. sapiens sapiens*, subjected to rapid reduction in body and craniofacial size, echoing my first comment: "Apprehending the full nature of regional variation in Austroriparian and Pacific Island populations is essential to interpreting the taxonomic status and phylogenetic history of *H. floresiensis*." Therefore the 'Hobbit' is very probably also the result of a combination of founder effects, genetic isolation and a high inbreeding coefficient, perhaps manifested as microcephalic osteodysplastic primordial dwarfism. Laron Syndrome is a congenital deficiency of insulin-like growth factor (IGF-I), resulting from inbred genetic defects of the growth hormone (GH) receptor gene. Such patients manifest a complete block of the GH-Rs, resulting in a shorter stature than pygmies, who have only a partial defect in the GH receptors. Laron Syndrome yields even lower body heights in adult females than that of the 'Hobbit', the lowest recorded being 95 cm, and in practically all other respects matches its skeletal morphology.

Rapid body size reduction is a common response in endemic island populations, and is certainly not limited to humans: elephants, rhinos and hippos may become as small as pigs, and deer may assume of the size of hares (Sardinia). In humans, morphological features considered primitive for the genus (e.g. small brain size, enlarged supraorbital tori, absence of chins, relative megadontia) may be developmental correlates of small body size in pygmoid populations. Even if further specimens of such features were reported from Liang Bua it would not confirm the taxonomic validity of *H. floresiensis*. It is my prediction that in thirty or forty years from now, that 'species' will be included on that ever-growing list of blunders in archaeology. And this is a falsifiable proposition.

### Conclusions

The above six examples of major mistakes in archaeology were selected on the basis of providing the greatest potential variety of different scenarios as possible in the smallest number of examples, and these can be extended by the additional examples I have described in the first lecture. Thousands upon thousands of mistakes are made by archaeology every year, but most of them are of very minor consequences. Those of interest in the present context are the major controversies that can illuminate the epistemology of the discipline, by showing why and how things went wrong on a spectacular scale.

Therefore each of the examples cited presents us with a different set of circumstances and consequences. It is, however, also evident, on close examination, that a number of consistent themes soon become apparent. I draw attention to the following:

1. The mass media is manipulated by archaeologists presenting false claims, but sometimes it actually helps exposing bungles, as in the Jinmium case (although out

of self-interest). As a discipline that produces nothing of economic worth, archaeology needs the patronage of the public, and thus the support of the mass media. Moreover, media coverage affects academic funding of individuals, teams and projects. However, competition for media space is fierce. As a result, stories tend to be embellished for media consumption and researchers can become trapped in the creations they conjured up to draw public attention to their work. This can easily become a factor in having to defend untenable positions.

2. Archaeology, as we have seen in Lecture 3, has a political role, and as a discipline it is also ruled by political considerations. These include loyalty or opposition to colleagues on principle, formation of cliques, exclusion of amateurs, and energetic suppression of dissidents. Political dimensions may even include jingoism, as evident in the 'Hobbit' case and others.
3. A generic observation is that many archaeologists have an inadequate comprehension or knowledge of their chosen discipline, and that this does not necessarily prevent them from rising within its hierarchy. This runs like a red thread through even the most diverse examples: we see the incompetence of much of the entire discipline in the Piltdown, Coa and African Eve controversies (and in all those listed in Lecture 1). Even the remaining cases involve easily identifiable shortcomings in archaeological knowledge, rendering this factor universal to the issue.
4. This leads to a related observation. For better or for worse, English has become the international language of research, and it is widely but falsely assumed that all important archaeological information is available in that language, and has been published prominently. Since all other material is neglected there is a scramble to get one's work published in one of the foremost British or American journals. Yet these journals and their referees tend to adhere to narrow views, resulting in 'academic inbreeding' and an exclusion of alternative views or information not published in English. However, more than 80% of all available archaeological information has never appeared in English, and much of what is available in English is not covered in the favored venues.
5. Placing the reputation of the discipline or of individual high-ranking academics above veracity, as we observed in the Glozel and Coa cases—but which is also present in more latent forms in all others—is a key factor in archaeological blunders. That is to be expected, and it is a serious shortcoming of a discipline comprising unfalsifiable propositions to rely on that factor, the lack of refutability, to deliberately uphold capricious notions.

The fear of damage to the discipline through exposing the mistakes of senior academics is perhaps directly related to the weak epistemology of archaeology. To compensate for its lack of refutability, the discipline is very conservative, and it relies heavily on authority. That authority resides primarily in its high-ranking leaders, and any damage to their standing tends to be regarded as damage to the discipline. In other words, the strength of the discipline is perhaps grounded in the 'infallibility' of its el-

ders, a defiance of which may be perceived as challenging the discipline's authority. If one adds to this state the tradition of Western academe, of encouraging the individual to vigorously oppose any contradiction, to defend one's hypotheses at all costs, and recalls that archaeological hypotheses are not falsifiable by archaeological means, two effects are likely to be encountered. Firstly, unfalsifiable propositions are easy to defend by one simple principle: never concede anything. So there is already an unhealthy epistemology inherent at the best of times. Secondly, if one adds to this the perception that the discipline's public

prestige would suffer from exposure of mistakes, or from the disclosure that its leaders have erred, the epistemological issue becomes obvious. It is that the discipline would rather preserve its position than concede an error; it prefers dogma to veracity.

At that point it ceases to be a scholarly pursuit, i.e. one existing purely for the sake of finding truth. It has become a belief system.

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