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Edited by Michael Heap

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EDITORIAL

Michael Heap

This issue of the *Skeptical Intelligencer* is devoted to unusual theories and beliefs in the fields of history, archaeology, anthropology and palaeontology. All are largely unsupported by mainstream knowledge in these disciplines and run counter to existing evidence and acceptable standards of scholarship and practice. Some of the ideas, such as those of Mr. Graham Hancock, have received considerable public attention and if they have any validity they would have astonishing consequences for our

understanding of the evolution of early civilisations. Yet, as always, extraordinary ideas require extraordinary evidence, and such evidence appears not to have been forthcoming.

I am very grateful to Michael Brass, Lee Keener, Mark Newbrook, Sarah Thomason, John Wall and Doug Weller for their contributions. Much of the material in this issue was presented at the 11th European Skeptics Congress in September 2003, organised by ASKE.

ARTICLES

AN AMATEUR LOOKS AT EGYPTIAN PSEUDOHISTORY

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1. Introduction

Some years ago I was browsing in a bookstore and chanced upon a large new book by Graham Hancock. I found it interesting because it discussed a topic about which I had an avocational interest: the history of Ancient Egypt. A few minutes' examination convinced me that I was not dealing with what might be called 'conventional scholarship'. Included were some of the old arguments about the presence of certain mathematical constants in the dimensions of the Great Pyramid, arguments that I have always considered unconvincing. I speculated about the degree to which my response was conditioned by what others had written on the topics rather than on my own objective investigation and analysis. From this time, I found myself engaged in an informal project to conduct that investigation and analysis. I set a few rules for myself: I would not personally consult other experts, I would use only information available in my personal library, the library of my university, or on the web, and I would strive to examine any evidence presented in a dispassionate and unprejudiced way.

In the title to this article I have described myself as an amateur. I have very little formal training in astronomy. I am not entirely ignorant of the history of Ancient Egypt;

but this knowledge helped me mostly in determining what resources to consult. And though I am a professional mathematician, the mathematics required for my analysis was typically of the secondary school sort. Perhaps I am what might be referred to as the 'educated reader'. The nature of my background was an important aspect of the project. I was anxious to see what someone with this background could conclude when presented with the evidence that will be described below.

To be more specific, this article will examine skeptically what for convenience I will call the **early civilisation theory**. The theory has been developed in a series of books: *The Orion Mystery* by Robert Bauval and Adrian Gilbert (1994), *Fingerprints of the Gods* by Graham Hancock (1995), and *The Message of the Sphinx* by Bauval and Hancock (1996). Although John West is not a co-author of any of these books, he plays a major part in their theories and is frequently quoted. Hancock continues to publish books in this vein. While the cited books are fairly recent, they may be considered an extension of ideas put forward on Atlantis by Ignatius Donnelly (1882) in the nineteenth century; by Charles Hapgood on 'earth-crust displacement' in a series of books (1958; 1966; 1970); by Arthur Posnansky (1945) on the South American site of

Tiahuanaco in 1945; and by a number of other writers. This theory also has both an historical and an astronomical aspect, and indeed there are similarities to some of Velikovsky's work.

Briefly, the idea is that there existed some time prior to 10,000 BCE, an accomplished 'protocivilisation' (my term) possibly situated in Antarctica, which was itself located in more temperate climes than currently. The protocivilisation influenced the subsequent development of descendant civilisations around the world but has left little direct evidence of its existence. However, the Great Sphinx of Giza is an exception and was built around 10,450 BCE¹. In fact, the entire complex on the Giza Plateau (including, in particular, the pyramids of Khufu, Khafre, and Menkaure) was laid out in very early times and constructed over an extremely lengthy period. The Great Pyramid is not that of Khufu, the Sphinx was not built by Khafre, and the Egyptologists (who are generally absurdly conservative and immensely incompetent) have most of their dates muddled.

The astronomy component comes in a series of rather complicated arguments that purport to show that certain structural alignments are consistent with the proposed datings of Hancock et al. Some of the arguments are related to the precession of the Earth's axis of rotation. Owing to gyroscopic effects, the axis 'wobbles' about a line perpendicular to the plane of the Earth's orbit. A full cycle takes about 26,000 years. This has the effect of changing the position of all the celestial bodies with respect to the celestial north pole. We will look at some of these arguments in more detail.

The so-called 'Sleeping Prophet', Edgar Cayce, plays an odd role in the theory. He claimed many years ago (he died in 1945) that there was what he called a 'Hall of Records' buried under the paws of the Sphinx, and that it would likely be discovered in 1998. (It wasn't.)

2. Expert Witnesses

As we will see below, the age of the sphinx plays an important role in the early civilisation theory. To estimate the age of this monument from its current condition, a geologist from Boston University, Robert Schoch, was invited by John West, an amateur Egyptologist and proponent of the theory, to examine the Great Sphinx to assess the extent to which it showed signs of rainwater erosion. This was important to the early civilisation theory because it is well known that rainfall has been extremely limited since the putative time of construction of the Sphinx, around 2500 BCE, but more abundant prior to that time (Hoffman, 1991). Schoch determined that the Sphinx showed clear signs of extensive water erosion through rainfall and that this was dramatically inconsistent with a dating of 2500 BCE². But Mark Lehner³,

Director of the Giza Mapping Project, and certainly an expert on the Sphinx, believes that the monument shows signs of erosion by ground water, not rainwater, with consequences that are consistent with the usual dating. He is not a geologist. But his interpretation has substantial support from the geology community. Schoch debated his theory at the AAAS meeting in Chicago in early 1992. Elizabeth Culotta reporting on that debate in *Science* (Culotta, 1992) said: 'When it was over, it was clear that Schoch hasn't convinced many archaeologists or geologists that they've been wrong all these years'. K. Lal Gauri of the University of Louisville, a geologist who worked in the vicinity of the Sphinx for ten years stated at that meeting (Culotta, 1992): 'Neither the subsurface evidence nor the weathering evidence indicates anything as far as age is concerned. It's just not relevant.' Gauri, J. J. Sinai, and J. K. Bandyopadhyay subsequently published their findings, which contradict Schoch, in the journal *Geoarchaeology* (Gauri, Sinai, & Bandyopadhyay, 1995). To my knowledge, Schoch has never published his theory in a peer-reviewed professional (i.e. geological) journal. His reasoning is not indicated in the books by Bauval and Hancock.

3. Pi and the Age of the Great Pyramid

There has been a huge amount written about the Great Pyramid, including much nonsense. The consensus among Egyptologists is that it was constructed a little before 2500 BCE by Khufu (Cheops in the Greek version) for his tomb. It is very closely aligned to the cardinal directions; that is, the base on the north side runs almost exactly east-west, and so on. This is surely not accidental. The deviation from exact alignment varies from a minimum of 1' 57" to a maximum of 5' 30" (Edwards, 1985: p. 99). Its dimensions are difficult to measure exactly since the pyramid was once covered by a casing of smooth limestone which is now gone. However, a 1925 survey by Cole (1925) extrapolated the original perimeter of the base to be 3023.16 feet, with a height of 481.4 feet. The difference between the longest and shortest side is 7.9 inches. The degree of accuracy in Cole's measurements seems somewhat suspect to me. There have been other measurements, but let us accept Cole's. I have visited the Great Pyramid and agree with Randi's simple description of it as 'a heap of roughly squared blocks' (Randi, 1987).

With this information at our disposal, let us hear what Hancock has to say about the structure. I should point out that he is anxious to demonstrate that the pyramid

BCE and 5000 BCE. Upon reflection, this should appear very odd. On the basis of the best available physical evidence, he should make a point estimate of age. If he wishes to be conservative, he then should provide a large confidence interval centred around the point estimate. To do anything else is to pervert the data.

³Mark Lehner plays a bizarre role in the whole history of the early civilization theory. Early in his career he received funding from the Edgar Cayce Foundation and was sympathetic towards the Cayce movement. Subsequently he moved into the mainstream and is certainly now an opponent of the theory. The reader is encouraged to examine Appendix 2 of *The Message of the Sphinx* for more information.

¹The idea that the Sphinx was built in a remote time, and considerably predates the pharaonic period, is by no means new. It goes back at least to Le Plongeon (1896) in the late nineteenth century.

²According to West, Schoch deliberately made a very conservative (i.e. minimal age) estimate that the Sphinx was built sometime between 7000

incorporated information and required technologies of construction that were beyond the knowledge and capabilities of Fourth Dynasty Egypt. He says: 'Thus, if we take the pyramid's height and multiply by 2π we get an accurate read-out of the monument's perimeter (481.3949 feet $2 [sic] \times 3.14 = 3023.16$ feet)' [18; 178]⁴. This innocent looking statement is very revealing. We are of course meant to be amazed at the astonishing accuracy of the Egyptian construction, demonstrating simultaneously the knowledge of the value of π and the possession of extremely precise building techniques. However . . .

The figure for the height, which has suspiciously many digits, is said, in a footnote, to be taken from Edwards's classic *The Pyramids of Egypt* (1985). But the figure there is 481.4 as quoted above. We have a small misstatement⁵. Next, the figure taken for π is of course not exact. Why take 3.14? Why not $22/7$ or 3.14159 (which is the correct value to the places displayed)? The answers to both problems appear to be the same - the choices make the equation work out! But we can make a further observation. The discrepancies in the individual measurements of the sides of the base very forcefully argue that the Egyptians, while displaying impressive accuracy, were not able to achieve tolerances of less than a few inches in a measurement of the magnitude of the pyramid perimeter (about 3000 feet). This degree of accuracy is unquestionably achievable with fairly primitive techniques. And the accuracy in the alignment of the corners is similarly achievable (Krupp, 1983). One can get an idea of the size of five and a half minutes of arc (the maximum observed discrepancy from the cardinal directions) by noting that this is a little bit more than one sixth of the arc subtended by the moon in the sky. Impressive? Yes. Unbelievable? Hardly. Yet Hancock calls this 'an almost supernatural feat' (1998: p. 276)⁶.

⁴The perimeter figure used here is the sum of the lengths of the four sides as given in Edwards's *The Pyramids of Egypt*. I do not have access to the original survey figures of Cole. If Hancock's figures given on page 278 in *Fingerprints of the Gods*, said to be taken from Cole, are correct, then the latter must claim accuracy to 1/10,000 of an inch. For a monument in the condition of the Great Pyramid, this is patently absurd. Edwards states (1985: p. 98) that the original Cole figures were given in meters and decimal fractions of meters so the possibility exists that the extra precision was introduced in an inappropriate conversion to English units. Of course the Hancock quote is then not from Cole, but from Cole interpreted by Hancock. In any event, given the missing casing stones and the weathering and damage over several millenia, I would expect a margin of error of at least a foot or two! It would be interesting to know how the Cole survey was conducted.

⁵The figure used may be from Cole's original report.

⁶Hancock implies that Edwards supports the argument that methods of achieving the accuracies observed are not explainable in terms of the Egyptians' known methods of construction and expertise. This is simply false. See (Edwards, 1985: Chapter 8). In *The Message of the Sphinx* (Bauval and Hancock, 1997: p. 39) we have the following passage, offered as an analogy to the construction of the Great Pyramid: 'However, if your requirement is for a wall that is straight within 1 arc minute per 100 meters and directed *exactly* due north, then you are going to need a laser theodolite, an ordnance survey map accurate to 10 meters, and a highly qualified team of professionals including an expert setting-out engineer, an astronomer, a surveyor, several master-masons and a week or so to ensure that the precision you are aiming for has in fact been achieved.'

But what about the value of π said to be implicit in the dimensions? This has been noted by many people. Piazza Smyth, the 'Great Pyramidiot' thought that this was deeply significant and found many other supposedly meaningful values from simple calculations using various pyramid dimensions (Smyth, 1864). Many Egyptologists accept that the presence of π in the ratio considered above is not accidental. I am inclined to disagree, and view the case as not proved. Here is another possibility, suggested by comments of Boyer (1968: pp. 19 - 20): If the slopes of the four faces of the pyramid are 14/11 (i.e. the face rises 14 units for every 11 horizontal units, an easily measurable slope choice), then the observed ratio of perimeter to height is very nearly achieved⁷. In fact, using the figures of Cole, an increase of about three inches for each side of the base is consistent with this hypothesis. That such an approach may have been used in pyramid construction is hinted at by Problem 56 of the famous Rhind papyrus (Gillings, 1982: pp. 185 - 187). The Egyptians even had a name for the reciprocal of the slope. It was called the *seqt*. It has also been suggested (in a Nova television production) that the value of π was introduced through the use of (circular) wheels for measuring distances, though I am skeptical that this could be a very accurate method of measurement.

In any event, what is the significance of the presence of π in the dimensional ratio? Not much. The concept of π as a certain geometrical constant is not a deep one and it is easy to determine fairly good values for π through direct experiment. Referring to formulas for calculating circumference from diameters or radii, Hancock states that their discovery 'is thought to have been made late in human history' (1998: p. 177) and then cites Archimedes's determination of π at 3.14 in the third century BCE. And in *The Message of the Sphinx*, Hancock and Bauval say (1997: p. 37): '... the mathematical value π (3.14) is not supposed to have been calculated by any civilisation until the Greeks stumbled upon it in the third century BCE'. But he has not researched this well enough. In fact the Rhind papyrus shows clearly that at the time the papyrus was written, around 1800 BCE, the Egyptians had a formula for computing the area of a circle (a harder problem) with the value of π of $256/81$ implicitly used. A formula for computing the circumference of a circle was also of course

This statement has many problems. For example, the accuracy he is asking for in the analogy is not found in the Great Pyramid; a maximum error in excess of 5 minutes of arc indicates that the Egyptians' error margin is at least that much. And what does the qualification 'per 100 meters' mean in the context of angular error? What is the ordnance map for? But most important, the errors observed in the Great Pyramid alignments are based on Cole's measurements taken in 1925, with angles given to the **second**. I guess Cole must have had all of the listed resources including a laser theodolite. Of course the laser was not invented until 1958.

⁷Bauval and Hancock (1997: pp. 233 - 236) actually observe that this ratio is used in the Great Pyramid in the course of an incoherent numerological discussion involving the number 11. They are clearly unaware that they cannot have it both ways. The 14/11 ratio (to a close approximation) results from the π ratio. They are observing a property of the numbers involved, not something that is of human origin. In other words, if the π ratio is purposely built into the dimensions of the Great Pyramid, the 14/11 ratio is produced automatically, and conversely.

known. This fraction 256/81 is about 3.16. Exactly when these formulas were first developed is not known, though they may go back to Imhotep (27th century BCE) (Boyer, 1968: p. 12; Gillings, 1982: p. 142 and p. 197).

What about the early civilisation theorists' doubts that the Great Pyramid was constructed by Khufu? The main difficulty facing Egyptologists is the lack of any inscription on the tomb. This situation is in stark contrast to, say, the New Kingdom tombs in the Valley of the Kings.

Let us review some of the evidence that the pyramid is that of Khufu, or at least that it was built at about the time of his reign.

1. Herodotus reports in the fifth century BCE (1987: pp. 185 - 187), that it was then thought by the Egyptians to have been built by Khufu. Thus at the very least, the identification is not an inference made by modern Egyptologists. If there is a misidentification, it was made more than 24 centuries ago.

2. The complex of the three major pyramids at Giza strongly suggests, both by similarity of construction and proximity, that all were built at approximately the same time. But the two smaller pyramids are pretty firmly identified with Khafre and with Menkaure, who succeeded Khufu during the Fourth Dynasty⁸.

3. Col. H. Vyse found stone blocks inside the pyramid that displayed quarry marks indicating the blocks were for the pyramid of Khufu. Hancock (1998: pp. 302 - 304; see also Bauval & Hancock, 1997: pp. 101 - 104) has questioned Vyse's honesty and suggested that he forged these quarry marks. He has 'expert witnesses' to back him up. The issues, which are essentially epigraphic, are far outside of whatever expertise I may have. However, essentially all mainstream Egyptologists accept the marks as genuine. It is hard to find much motivation for such fraud on Vyse's part, but easy to attack the integrity of a dead man.

4. All of the Giza pyramids indicate a better understanding of pyramid construction techniques than those displayed in other pyramids conventionally dated before Khufu, such as the Bent pyramid, the pyramid at Meidum, and the pyramid of Djoser. In fact, this last pyramid is clearly identified with that Third Dynasty king and the other two mentioned, with less confidence, with Snefru, Khufu's immediate predecessor (Edwards, 1985). The implication is of course that the Giza pyramids were built later.

5. There are several shafts built in to the Great Pyramid that appear to have astronomical significance (Krupp, 1983: pp. 102 - 105). These are the north and south shafts of the King's Chamber and the north and south shafts of the Queen's Chamber. The shafts point to the position in the sky, as it appeared in about 2500 BCE, of the meridian

transits of Alpha Draconis (the pole star in that era, also called Thuban), Orion's belt, Kochab⁹, and Sirius respectively. Assuming that these shafts were designed to point to the stars in question, and there are persuasive arguments from the writings of the period that these stars were important, one can then, by calculation, date the construction of the shafts to about the time of the Fourth Dynasty, since the precession of the Earth's axis alters over time the altitude of meridian transit of a given star.

6. Although the Great Pyramid itself is without inscription, there are many inscriptions on the subsidiary tombs and other structures to the immediate east and west of it, and references to Khufu and his pyramid are abundant in these inscriptions¹⁰.

7. Numerous examples of Fourth Dynasty pottery and other artifacts have been found throughout the Giza Plateau region.

8. No alternative location for Khufu's tomb is known. There may be other arguments that can be advanced by experts in the area. But what arguments do Hancock and John West advance?

Actually, it is hard to get a clear idea of exactly what they believe. West and Hancock are unable to ignore the evidence of the alignment of shafts presented above. West has offered two theories to account for this while still preserving the early civilisation (in 10,450 BCE) argument. One possibility he advances is that the pyramids (I guess all three) were built in 10,450 BCE but designed (because the protocivilisation had excellent knowledge of astronomy and especially precession) so that the alignments would be accurate in about 2450 BCE since that future date is somehow special. This explanation reminds me of the invisible demons in a dialogue of Eric Rogers (1960) on friction, which are endowed with stranger and stranger properties just to justify their existence. Such an *ad hoc* explanation is, if you will forgive me, absolutely preposterous. So it is comforting to discover that West personally favours the alternative explanation, which is that the entire Giza Plateau complex was laid out in 10,450 BCE but completed over the next 8,000 years, with the shaft alignments actually performed about 2450 BCE [18; 449]. I will call this the *layout hypothesis* for convenience. I guess by comparison, the second explanation is marginally more believable. But perhaps I do not do justice to this possibility. We shall see below what additional evidence is advanced for the layout hypothesis. The reader can judge how compelling it is. Incidentally, it is a mystery to me why Hancock spends a great deal of time calling into doubt Khufu's connection to the Great Pyramid when the layout hypothesis has it completed during his reign (or at least during the Fourth Dynasty). But a characteristic of pseudoscience, which is exceedingly common, is this: Be

⁸They did not directly succeed him. The eight-year reign of Ra'dejedef intervened between that of Khufu and that of Khafre. His tomb is at Abu Roash, away from Giza. We will comment on Ra'dejedef again when discussing the likeness on the Sphinx.

⁹This identification may be due to Bauval. It is true that the altitude of Kochab at the meridian is the 39 degrees he indicates.

¹⁰See Zahi Hawass's comments on this and the following item at <<http://www.pbs.org/wgbh/nova/pyramid/explore/howold.html>>.

as vague as possible. Raise a lot of questions and blow a lot of smoke, but whatever you do, don't stick your neck out with a clear and falsifiable hypothesis.

Before we look at the proffered evidence for the layout hypothesis, let us examine another curious feature of Hancock's and Bauval's arguments that question the conventional dating of the Great Pyramid. This is the affair of the iron plate. In 1837, a certain J. R. Hill, working under the aforementioned Col. Vyse, discovered a flat iron plate located in the masonry of the Great Pyramid. Its provenance strongly suggested that it dated to the time of the construction of the monument. Only thing is, smelted iron, as this apparently was, was unknown during the Fourth Dynasty and only introduced to Egypt much later. If you think this argues for a later date for the Great Pyramid, you have a lot to learn before you can call yourself a pseudoscientist. Hancock and Bauval, in *The Message of the Sphinx*, take this as evidence for an **earlier** date. That the plate is indeed smelted iron from ancient times seems very likely, although testing of the item by metallurgists M. P. Jones and Sayed El Gayer has been looked upon skeptically by the British Museum, for reasons that are unclear. If one believes all that Hancock and Bauval tell us about the affair, there does seem to be a mystery here, though I do not think its resolution is to be found in their thesis. Is there a ready explanation? Nothing entirely satisfactory presents itself. Smelting was introduced in western Asia earlier than in Egypt. A date of 2000 BCE is not impossible for the first such activity (Scarre, 1988: p. 120). Egypt could have acquired smelted iron by trade, though there is still about 500 years that have to be explained away. Still, 500 years is a less of a challenge than 8,000 years.

There is a little more to the iron story. Hancock and Bauval indicate that there are apparent references in Old Kingdom inscriptions to iron. Their point is, I guess, that knowledge of iron had been inherited from the protocivilisation. They say that the word used is 'B'ja - the divine metal' (Bauval & Hancock, 1997: pp. 108 - 109; Hancock; 1998: pp. 369 - 370). But is this correct? A standard working reference to the ancient Egyptian language is Faulkner's *A Concise Dictionary of Middle Egyptian*. But an immediate difficulty in even looking up the word is that neither 'j' nor 'a' is used in the standard transliteration of ancient Egyptian, so one must guess what Hancock and Bauval mean. I believe they are referring to 'bi'. Faulkner (1962: p. 80) defines this as 'bronze (?)'. The eminent authority Sir Alan Gardiner, defined it as 'copper' (1957: p. 564). Both bronze, and of course then copper, were known and used in the Old Kingdom. I am happy to be corrected by experts in the language, but it does look as though Hancock and Bauval are indulging in a bit of wishful thinking.

4. The Layout Hypothesis

I would judge the two major components of the early civilisation theory to be the redating of the Sphinx and the

layout hypothesis. We have touched on both already but there is much more to say about each.

Robert Bauval is the champion of the layout hypothesis. It is developed in *The Orion Mystery* and later in *The Message of the Sphinx*. Essentially, the argument is that the positions of the various monuments at Giza (and beyond, but I will confine the discussion to Giza) have an astronomical significance, but primarily with regard to the appearance of the heavens about 10,450 BCE.

4.1 Giza and Orion compared

One argument advanced is based on the putative similarity between the positions on the Giza Plateau of the three major pyramids and the three stars in the belt of the constellation Orion. For future reference, let us note that the names of the stars, looking from left to right when Orion crosses the meridian, are Al Nitak, Al Nilam, and Mintaka. Bauval identifies these stars with the pyramids of Khufu, Khafre, and Menkaure respectively. Note also that the head and foot stars of the constellation are Betelgeuse and Rigel respectively, two of the brightest stars in the sky and both far brighter than any of the stars in the belt. A figure from *Fingerprints of the Gods* (Hancock, 1998: p. 355) summarises the evidence presented for the identification of the ground plan with the sky plan. (The discussion that follows becomes rather technical. It is useful in reading this section and the next, to have copies of the referenced books available.) In this figure, the stars are configured as they appear today, when the constellation is at the meridian. Once we accept the identification of the Giza plan with its celestial counterpart, we are asked to believe that the Giza plan was developed in about 10,450 BCE, because the ground orientation agrees with the celestial orientation at that time. To be more precise, we can define this to mean that the angle made by the arc joining Al Nitak and Al Nilam with the east-west great circle through the Al Nitak declination, is the same as the angle between the line joining the apexes of the two larger pyramids and a line running strictly east-west through the Great Pyramid. (See Baines & Málek, 1980: p. 158, for a good map of the area on the Giza Plateau around the Great Pyramid.)

At first glance, the case for identifying the ground plan with the sky plan, and ignoring for the moment the orientations, seems impressive, even ironclad. However, there are problems beneath the surface. First, the correspondence in the horizontal scales between the portion of the celestial sphere shown and the Giza layout is induced by Hancock's choices of scales to use on the paper. This is one parameter in the similarity that must be ignored. Next, the angles made between the configurations and the horizontal are equal only because Hancock has chosen to orient the ground plan so as to make that happen. So this is a second parameter of similarity that must be ignored. We are left with only two real parameters: the angle between the lines connecting the pyramid centres (and the

corresponding stars), and the relative distances between the three objects. In fact, while there is reasonable agreement in each of these, the error is at least several percent. Also, after careful measurement, I have detected further errors in both the site plan of Giza¹¹ and the horizontal to vertical scale ratios in the sky map. These errors are small, in the range of 1 - 2%. So we see that the almost 'supernatural' skill of the protocivilisation has strangely vanished¹². In fact, what we have in each case are three fairly equally spaced objects that are not quite collinear. Hancock makes the further point (1998, p. 356) that the sizes of the pyramids provide '... some indication of their [*i.e. the stars*] individual magnitudes'. There is partial truth to this in that the magnitudes of Al Nitak and Al Nilam are significantly greater than that of Mintaka, and the first two stars of course correspond to the two larger pyramids. However, Al Nilam is the brightest star of the three yet it does not correspond to the Great Pyramid. It is interesting to note that Bauval and Gilbert tell us in *The Orion Mystery* (1994) that Al Nitak is the brightest of the three stars in Orion's belt. Since Al Nitak is supposed to correspond to the Great Pyramid, this is a desirable state of affairs. Too bad it isn't true¹³. While the similarity in the configurations is certainly noticeable, that is not the same as being planned. I do not believe the case is proven, though I would take it to be plausible. Fagan and Hale (2001) do not concede even this much in their paper on the topic. The reader must judge independently.

But let us give Hancock and Bauval the benefit of the doubt and accept that the pyramid configuration is a representation of Orion's belt¹⁴. It is still far from clear that the designers would have felt compelled to include the orientation with the heavens as part of the design. Or even if they did, how did they define when the constellation is at the meridian? By the choice of a star in the constellation? Which star?

And how close are the orientations of the sky and land maps? If you refer to another key figure in *Fingerprints of the Gods* (Hancock, 1998: p. 447), you will notice without effort that the orientations are clearly different. By eye, I would have guessed they were off by 8 - 10 degrees. I have in fact calculated the angle Orion made with the east-west great circle through Al Nitak in 10,450 BCE. It is a straightforward, if tedious, exercise in spherical trigonometry. By comparing this with the ground plan of Giza, I found the error to be about 11 degrees. I am at a loss to explain the various diagrams in *Fingerprints of the*

Gods and *The Message of the Sphinx*. The angle of the belt stars given on page 447 of the former does not seem to match my calculations, but the scale chosen is so small that it is hard to determine the angles with any accuracy. In the latter, we have for example on page 72 several orientations of Orion at different times in history, but they are not rotations of each other, yet should be since none of the stars in Orion displays significant proper motion over the time periods considered. Furthermore, in that same diagram, the belt stars at 10,450 BCE and at 2000 CE are at quite different angles, yet simple geometric considerations show that they should be close to the same. The illustration on page 253 is severely in error. I have also run (a newer version of) Skyglobe¹⁵, the program used by Bauval to obtain his orientation results. I do not obtain any of the orientations displayed in *The Message of the Sphinx*. Bauval and Hancock never give us the actual angle of orientation of the belt at 10,450 BCE¹⁶, yet this is the crucial information needed to demonstrate that the orientations are the same.

Of course the important issue is not how the Bauval and Hancock pictures appear, but how the actual Giza orientation compares with the Orion belt orientation in 10,450 BCE. The error in orientation angle, both from my own calculations and from a manual measurement of a Skyglobe printout, is about 10 - 11 degrees.

What Bauval apparently did can be summarised as follows. During the precessional cycle¹⁷, the constellation Orion rotates in orientation, and its altitude at meridian transit oscillates between roughly 11 degrees and 58 degrees. It is at the lowest point roughly at 10,450 BCE, but it is not oriented correctly at that time. Still, Bauval latched on to this date and claims of the supposed similarity: '... it's a perfect match - faultless' (Hancock, 1998: p. 444). Bauval must use a different definition of 'faultless' than I do. We can only conclude that while the identification of the Giza plan with Orion may be correct, the identification of its date of layout to 10,450 BCE, at least on the basis of the argument described above, is unquestionably spurious. Of course it is only the latter which supports the early civilisation theory. I should add that there are times earlier than 10,450 BCE (for example 11,820 BCE) when the match is much better, as is easily discovered by experimenting with Skyglobe. But these

¹¹As compared to that given in *Atlas of Ancient Egypt* (Baines and Málek, 1980: p. 158).

¹²The similarity is nevertheless called 'unbelievably precise' by Hancock (1998: p. 356).

¹³Al Nitak has magnitude 1.8 and Al Nilam magnitude 1.7 (larger numbers correspond to lesser magnitudes), so they are close. These figures are taken from (Mitton, 1998: p. 411).

¹⁴Of course one could also ask 'why choose Orion?' This is a fair question, but the importance of Orion to the Egyptians and its connection to the Egyptian pantheon is well attested (Krupp, 1983: p. 105).

¹⁵Skyglobe 4.0. Bauval used version 3.6. Skyglobe, produced at Villanova University, is a wonderful program, available as shareware which can be downloaded from the web <<http://astro4.ast.vill.edu/skyglobe.htm>>.

¹⁶In my calculation mentioned above, I obtained 34.59 degrees for the angle, against an angle slightly in excess of 45 degrees for the Giza complex.

¹⁷At several points Bauval and Hancock state that the cycle takes 25,920 years. This is incorrect. The actual figure is a little less than 25,800 years (Bowditch, 1958: p. 373 and p. 955). This is especially peculiar given that Hancock uses elsewhere the correct value (1998: p. 235) He then makes an argument, on the basis of work done by a certain Jane Sellers, that the ancients (*i.e.* the protocivilization?) used the figure 25,920 years for the precession period. The argument is so confused that I will not try to repeat it. See Hancock (1998): pp. 256 - 260.

dates are not considered, possibly because they do not fit with the rest of the theory.

4.2 The Pyramids as pointers

A second argument offered for the layout hypothesis, due originally to Robin Cook (1992), is presented in *The Message of the Sphinx* (Bauval & Hancock, 1997: p. 251). There are three small 'satellite' pyramids near the pyramid of Menkaure. I believe the idea is that the three satellite pyramids are meant to correspond to the belt stars of Orion. The line that joins the pyramid of Khafre and the leftmost of the satellite pyramids of Menkaure (looking south) points at Al Nitak when it is on the horizon to the west of the meridian. But this is for the sky as it appeared in 10,450 BCE. Wow! But wait. A careful examination shows that the line does not quite pass through the apex of the smaller pyramid. Also, while the first line works, sort of, for Al Nitak, things fail miserably (against expectation) for the other two using the remaining satellite pyramids as is easily checked. But the real problem with this picture is the multitude of choices available. There are ten pyramids in the picture. (Notice the small pyramid immediately to the south of Khafre's.) The number of lines of distinct direction that can be formed by choosing any two of these ten pyramids turns out to be 40. (Some lines are determined by more than one pair of pyramids.) The number of belt stars is three. Of course one could add Betelgeuse and Rigel, both far brighter than the belt stars. And why are we looking at the western horizon? How about the eastern horizon? That some combination of pyramids produced a line that came close to pointing at some star in Orion when the constellation was in some standard position in the sky was to be expected. It is only surprising that they couldn't do better.¹⁸

4.3 Leo and the Sphinx

There is one further major argument put forward for the layout hypothesis. It goes as follows. The Great Sphinx is aligned so that it faces due east. (Hard to get much out of that it would seem.) When the sun is at the vernal equinox, it rises exactly to the east. In 2500 BCE, the sun was in the constellation Taurus at the equinox, so the Sphinx was gazing at Taurus when the sun arose. But in 10,450 BCE, due to precession, the sun was in the constellation Leo. Leo is the Lion. So the Sphinx was gazing at the Lion at sunrise. The Sphinx is a hybrid creature with the face of a man and the body of a lion. Do you get it now? The argument is elaborated at length (Bauval & Hancock, 1997: Chapters 9 - 10), but this is the basic idea. I guess we are to deduce that if the Sphinx were constructed in 2500 BCE, it would have been a hybrid of a bull and a man. The reader may already have detected a nasty little difficulty with this reasoning.

¹⁸The choice of the pyramid of Khafre here is not natural since the line does not point to its putative celestial counterpart, Al Nilam. The choice of the second (satellite) pyramid can only be considered arbitrary.

I discovered when I was quite young (as did most of you I am sure) that the constellation names are virtually arbitrary¹⁹. The star arrangements do not really resemble the objects after which they are named. As an experiment, the reader might wish to examine a star chart that includes the constellation Leo to try to find a constellation that looks like a lion. I used the *American Practical Navigator* (Bowditch, 1958: p. 583). Even with the help given by an identifying overlay it is hard to see the lion. Without the lines on the overlay and the grouping given by the lines, it could be a goat or even a bull!²⁰ So for the argument to make sense, we must suppose that the Zodiac concept, at least a part of it that includes Leo, was known and used in 10,450 BCE. The evidence that such was the case is non-existent. This is clearly an absolutely crucial component of the argument, yet Bauval and Hancock almost ignore it. The only reference to this problem that I can find in their argument is the following (Bauval & Hancock, 1997: p. 154): '... Herodotus (the so-called 'Father of History') displayed great reverence for the priests of Egypt and attributed to them the discovery of the solar year and the invention of the twelve signs of the Zodiac - which he says the Greeks later borrowed.'²¹In fact, the history of the Zodiac is quite murky. It is most likely of Mesopotamian origin, dating to 2,000 BCE or before, later to be adopted and adapted by the Greeks and then passed on to the Egyptians. Baines and Málek state in *Atlas of Ancient Egypt* (1980: p. 227) that 'The Babylonian and Greek signs of the Zodiac were introduced into Egypt in the Greco-Roman period, 'translated' into Egyptian representational forms, and used in the decoration of astronomical ceilings of tombs and temples, and on coffin lids.' So with no support for this crucial component, the rest of the argument crumbles. I need not trouble the reader with the remaining details.

4.4 Khafre and the age of the Sphinx

Bauval and Hancock, as we have seen, question the age of the Sphinx. Of course, if it was built in 10,450 BCE, it couldn't display the face of Khafre (unless it was recarved) and couldn't have been built by Khafre, an Old Kingdom Pharaoh whose dates are fairly well known. There is some confusion in the argument presented since they can't make up their minds whether it is now a likeness of Khafre and had been recarved or it is not (and I guess then it could still have been recarved). As evidence for recarving, they point to the fact that the upper part of the monument is not quite

¹⁹The 'Big Dipper' must be considered an exception; it really does look like a dipper. But even here there is a problem since the constellation is also called Ursa Major, the Great Bear. I don't see the bear, but perhaps I lack imagination.

²⁰Bauval and Hancock provide a citation where the author claims it does look like a lion (Hathaway, 1994). Perhaps the reader will agree with me in considering 'expert' testimony to be of no special importance in this case.

²¹Bauval and Hancock should not believe Herodotus. After all, he reported that the Great Pyramid was built by Khufu.

aligned with the lower part. I do not see why this is relevant. Why would recarvers work to different standards than the original carvers? The other piece of evidence is the apparently abnormally small face compared to the body. Of course this is in contrast to later sphinxes. One could hypothesise, as does Mark Lehner, that this was a prototype and the proportions were subsequently improved upon later. But is it Khafre? It may not be. Christine Hobson (1987) notes the similarity of the face (or what is left of it) to Khafre's predecessor, Ra'djedef. I see the resemblance, but the reader may wish to judge independently by viewing the appropriate figures in Edwards (1985: Plates 30, 31) and Bunson (1991: p. 220).

There is some positive evidence to associate the Sphinx with Khafre. There was a stela erected in later times that apparently identified the Sphinx as built by Khafre²². Bauval and Hancock doubt its veracity (perhaps with good reason in this case). The Sphinx is located immediately beside the Valley Temple that has associations with Khafre. There is abundant archaeological evidence excavated all over the Giza Plateau that dates the site to the Fourth Dynasty. In any event, none of this proves much with regard to the early civilisation theory.

4.5 Miscellaneous arguments

There are a number of other arguments advanced by Bauval and Hancock to support the early civilisation theory. These are minor arguments compared to the ones just considered and can be stated and critiqued fairly briefly.

One such argument is that it is not accidental that Giza was picked for the great layout of monuments. Rather it was chosen because it is located almost exactly at latitude 30 degrees north, one third of the way from the equator to the pole (Bauval & Hancock, 1997: p. 40). Such a choice shows astronomical sophistication. A response to this might point out that Giza is not coincidentally adjacent to the Old Kingdom capital of Memphis which itself is located, surely for reasons of convenience, at the southernmost point of the Nile Delta. Edwards provides a number of important criteria for choice of location of pyramid construction that Giza satisfies admirably (Edwards, 1985: p. 241). Hancock actually comments on this (1998: p. 432) but doesn't seem to recognise the full implication. One could further point out that the 30th parallel also passes through downtown New Orleans or that the 40th parallel passes through downtown Philadelphia (and very close to Ankara, the capital of Turkey). I would presume that these are not observations of cosmic significance.

Another observation (Hancock, 1998: p. 432) is that the number of degrees of latitude separating the official northern and southern borders of the Old Kingdom (the

parallel at latitude 31°06' and Aswan) is almost exactly seven. But Aswan's importance is geographically induced. It is the location of the first cataract and a barrier to navigation! The northern boundary is that claimed by Tompkins (1972: p. 299) and may be suspect. It is in any event also defined on the basis of the geographical features of the Nile delta. The case is further weakened by the lack of evidence that angles were measured in degrees in Old Kingdom times, let alone thousands of years before.

Then there is another observation (Hancock, 1998: pp. 434 - 436) that the ratio of the earth's polar radius to the height of the Great Pyramid is very close to 43,200. Why is this important? Surely the reader must see that this is related to the precession of the equinoxes. It is ten times the number of years it takes for the equinoxes to advance through two complete Zodiacal constellations. Or twenty times the number of years to pass through one constellation. Well actually it isn't. The latter figure is closer to 43,000. (Recall that Bauval and Hancock use the wrong number of years for the period of a precessional cycle.) And why multiply by twenty? Apparently it is to make the argument work. If you use the right numbers, you discover that the ratio of the polar radius of the Earth to the Great Pyramid's height is about 20.14 times the number of years for the equinoxes to pass through one Zodiacal constellation. Not a very interesting observation.

As our final example, let us shift our focus to a new structure, the Osireion, an unusual building in Abydos. It is probably a New Kingdom monument as the eminent Henri Frankfort (1933) argued. But one of the early excavators, Margaret Murray (1904) believed it, with some reason, to date from the Old Kingdom. Enter Hancock and West, who deduce, as best as I can follow their argument, that since there is a dispute over dating, anything goes and it might very well date back before 10,000 BCE (Hancock, 1998: pp. 399 - 407, pp. 424 - 425). The only additional evidence they offer, as far as I can see, is that the building is quite different than anything else in Ancient Egypt. Not compelling.

5. Conclusion

The arguments supporting the early civilisation theory, at least those presented in the books referenced in this article, are far from strong. It is not necessary for a reader to be an expert to detect many of the problems inherent in these arguments. What is required is a skeptical attitude, access to a reasonably good library, and a willingness carefully to scrutinise claims that appear outrageous, rather than be charmed by the mystery and romance in these claims. The wise reader will adopt an independent standard for evaluating evidence rather than use the standard employed (often implicitly) by the presenter of the evidence.

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²²James Henry Breasted doubted this identification because the name of Khafre, which was only partially readable, was not contained in a cartouche.

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SPIN DOCTORING HISTORY

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Introduction

The current 'king' of mass-market pseudohistorians is Graham Hancock. He and others in the genre describe themselves as *alternative* historians but this is just 'spin' to try and conceal the true nature of their work.

Hancock's 'big idea' is that the various ancient cultures around the planet inherited a 'legacy' from a civilisation that was destroyed without trace in c. 10,500BC. It is essentially

von Däniken's 'Ancient Astronauts' but substituting refugees from a 'Lost Civilisation' as the 'Bringers of Civilisation'.

An Open Mind or an Empty Mind?

Pseudohistorians frequently claim that their 'opponents', the orthodoxy, have 'closed minds'. With the possible exceptions of consumer electronics or the entertainment

industry anything 'new' or 'radical' is usually greeted cautiously and Max Planck's dictum, *A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it*, is sometimes appropriate.

But it can work the other way as Feder, writing about Piltdown Man, noted:

The lesson.....is clear.....many were convinced by what appears to be, in hindsight, an inelegant fake. It shows quite clearly that scientists, though striving to be objective observers and explainers of the world around them, are, in the end, human. Many accepted the Piltdown evidence because they wished to...Nevertheless, the Piltdown story, rather than being a black mark against science, instead shows how well it ultimately works. (Feder, 2002: p. 81)

This demonstrates an important difference between 'real' history and pseudohistory; real history is, albeit sometimes slowly, self-correcting.

What did the Lost Civilisation do for us?

Scholars are unanimous that Hancock is wrong, as he is peddling an antiquated idea which was found wanting and abandoned decades ago: pseudohistory is not self-correcting.

Diffusion of ideas, technology, religious beliefs, etc. between neighbouring cultures is well established but archaeologists have found worldwide evidence of independent developmental sequences making a 'legacy' from a common source (Hancock's *Lost Civilisation*), *hyperdiffusion*, unnecessary. (Trigger, 1989: p. 152-4)

There is no fundamental 'mystery' regarding the origins of civilisation other than, perhaps, that of motivation and many of its characteristics can be traced deep into prehistory. The works of, for example, Richard Rudgley (1998, 2000) present this in an accessible form. Hancock has to reduce the established view, derived from a mountain of evidence, to 'opinions' so that he can substitute his own, unevidenced, 'opinions'.

Any new hypothesis must either explain the existing evidence better or encompass additional data, but hyperdiffusion can only be made plausible by ruthless 'Observational Selection' - counting the 'hits' but not the 'misses'. On his website -

<http://www.grahamhancock.com/features/trenches-p3.htm>

- Hancock is open about this:

If you want a slavishly 'balanced' and objective account of 'both sides of the argument' then I'm the wrong author for you!

and:

A parallel for what I do is to be found in the work of an attorney defending a client in a court of law. My 'client' is a lost civilisation and it is my responsibility to persuade the jury - the public - that this civilisation did exist.

and:

So it is certainly true, as many of my critics have pointed out, that I am selective with the evidence I present. Of course I'm selective! It isn't my job to show my client in a bad light! Another criticism is that I use innuendo to make my case. Of course I do - innuendo and anything else that works. I don't care about the 'rules of the game' here - because it isn't a game and there are no rules.

If there was a *Pseudospeak Dictionary*, 'Open Minded' would be defined as *ignoring or suppressing the mass of information that is contradictory to the desired answer*. It is clear who has the 'closed' mind here!

Politically INcorrect?

In the South American section of *Fingerprints of the Gods* is:

Our route took us through the towns of Puccarini and Laha, populated by stolid Aymara Indians who walked slowly in the narrow cobbled streets and sat placidly in the little sunlit plazas.

Were these people the descendants of the builders of Tiahuanaco, as the scholars insisted? Or were the legends right? Had the ancient city been the work of foreigners with godlike powers who had settled here, long ages ago? (Hancock, 1995: p. 74)

It is sad, but indicative of the weakness of his 'case', that he has to stoop so low.

Pyramid Marketing

Most pseudohistorians have toyed with the Giza pyramids' being built much earlier but, as the evidence for ancient Egyptian construction is incontrovertible, a date of *c.* 2500BC is now accepted by all except a few diehards.

In 1994's *The Orion Mystery* Hancock's future co-conspirator Robert Bauval (with Adrian Gilbert) proposed that the Giza pyramids had been positioned to represent the 'belt stars' in the constellation of Orion¹. Citing the phenomenon of precession, by which the 'wobble' of the earth causes the position of stars to change over a period of some 26,000 years, Bauval used astronomical software on a personal computer to *wind the clock back*. He claimed a 'match' at 10,450/10,500BC, not 2500BC, the time when the 'belt stars' were at their lowest position in the sky and the psychic Edgar Cayce's date for the destruction of Atlantis (Bauval & Gilbert, 1994: p. 192-6). In *Fingerprints of the Gods* Hancock 'spun' this:

...the three pyramids were an unbelievably precise terrestrial map of the three stars of Orion's belt, accurately reflecting the angles between each of them and even (by means of their respective sizes) providing some indication of their individual magnitudes. Moreover, this map extended outwards to the north and south to encompass

¹See also the article by Lee Keener in this issue.

several other structures on the Giza plateau - once again with faultless precision (Hancock 1995: p. 375).

and:

At 10450BC - and at that date only - we find that the pattern of the pyramids on the ground provides a perfect reflection of the pattern of the stars in the sky. (Bauval quoted in Hancock, 1995: p. 469-70)

If something sounds too good to be true it probably is. The *unbelievably precise terrestrial map* was rather less precise than suggested, the *several other structures* were literally miles away (Legon, 1995: p. 48), and the *perfect reflection* was many centuries different (Fairall, 1999) from the supposed confirmation of Edgar Cayce's date. It should also be noted that without an *unbelievably precise terrestrial map* there are several possible 'matches'.

In 1999 when the BBC's *Horizon* examined Hancock's work he claimed that:

...the Ancient Egyptians were making a pleasing, symbolic resemblance to what they saw in the sky on the ground...

and conceded:

No, they're not absolutely correct and I don't care (Hancock, 2001: p. 686).

Hancock and Bauval assert that the pyramid builders were 'master astronomers' (Hancock & Faiia, 1998: p. 55) and endow them with precessional knowledge so that they could 'encode' Edgar Cayce's date. Surely 'master astronomers' would have done a better job of representing Orion's Belt and got a better 'match'? Perhaps the pyramid builders did not intend Giza to represent Orion's Belt and did not 'encode' any date?

Eugenics?

Assuming, and it is probably the least likely scenario, that there was an intention to 'encode' 10,450/10,500BC at Giza, the question arises as to how this was perpetuated for eight millennia? Their answer is a colony of 'refugees':

We also suggest that this shadowy brotherhood, whose members were said to have carried the 'knowledge of divine origin' (which they would later use to 'unify the country'), may have interrelated with the more primitive inhabitants of the Nile Valley in the prehistoric and Predynastic periods, interbreeding with chosen women and recruiting new generations from amongst the brightest and the best of their offspring - but leaving little or no trace of their presence in the archaeological record. (Bauval & Hancock 1996: p. 226-7)

One wonders where their 'inspiration' came from?

Angkor's Awry

Heaven's Mirror (Hancock and Faiia, 1998) claimed that some of the Cambodian temples at Angkor 'mirrored' the constellation of Draco (p. 126-8). This was on a grand scale, ten temples 'mapping' stars and only 'matching' at 10,500BC (p. 133) Again it was too good to be true. The

relationship is vague, all the temples in question have clearly defined reasons (such as a battle site) for their location and 50+ other temples are excluded. When questioned on his website's message board in May 2001, Hancock's answer included this wonderful circularity: *What sets them apart, as a group, is their obvious resemblance to Draco!*

Hancock and Bauval's stars and monuments only align convincingly on TV with the help of computer graphics.

The Smoking Gun

The supposedly astronomically-derived 10,500BC is so inaccurate it seems contrived and this was confirmed (with a couple of scribal errors) by the co-author of *The Orion Mystery*, Adrian Gilbert (2000: p. 257):

Following the line of the famous American clairvoyant Edgar Cayce we suggested that this was c. 10,500 BC.

and:

...but if we go back a further 330 years to c. 10,880(BC), then the alignment is exact.

Without considering the 'big picture' the detail in the so-called 'Lost Civilisation Hypothesis' fails time after time. But by 'adjusting' the astronomy to confirm the predictions of a long-dead psychic, Hancock and Bauval are trying to take us to Carl Sagan's *new Dark Age of irrationality and superstition*.

A Manufactured 'Mystery'

Having failed to 'find' his 'Lost Civilisation' on land Hancock got his feet wet, resulting in a 700+ page tome *Underworld* (Hancock, 2002) accompanied by a TV series, *Flooded Kingdoms of the Ice Age*.

To appear 'scientific' Hancock employs a computerised sea level rise model. Many atlases, however, show continental shelf contours and there are numerous sites post-dating the end of the ice's melting whose elevation has changed owing to factors such as tectonics or erosion.

He tells us that since the Last Glacial Maximum (LGM) sea level rise has flooded 25 million square kilometres of land. This sounds impressive until you realise he is pulling the same trick as a Chancellor of the Exchequer, stressing how much spending is increasing but without mentioning that this is insufficient to keep up with inflation.

Was this land a 'continent' in the Atlantic (*Atlantis*) or Pacific (*Lemuria*)? No, it was largely distributed around existing coastlines or connecting island chains, occasionally hundreds of kilometres wide but mainly long, narrow strips. He even provides a map (Hancock, 2002: p. 54-5) showing the difference between the land currently exposed and that exposed at the LGM which quickly demonstrates the futility of his 'quest'.

As the ice melted, the water rose about 1m per century and those living on the continental shelf moved progressively inland; this is generally accepted by scholars as being the basis of the various and diverse flood myths.

There would have been occasional localised catastrophic floods as 'build ups' were released, which were also incorporated in the myths.

Something is required to wipe out these coastal 'kingdoms' before they could retreat inland and Hancock cites Professor Shaw of the University of Alberta, describing him as *The author of an impressive list of peer-reviewed scientific papers* (Hancock, 2002: p. 67) It is difficult not to warm to someone subsequently described as *An aggressive little bugger from Yorkshire* (Hancock, 2002: p. 75) – perhaps the Geoffrey Boycott of geology! – but it does start the sceptical antennae twitching. Shaw proposes, with little support from other geologists – hence Hancock's 'spinning' – 'abrupt steps' in sea level rise. If correct, these mean that any population would just move inland faster.

In essence, in *Underworld*, Hancock is searching for the remains of 'Lost Civilisation(s)' which he contends mapped the world, were entirely resident on the mainly narrow coastal plain, and left no traces inland!

To Boldly Go....

Pseudohistorians often try and portray themselves as 'pioneers'. Hancock frequently claims that archaeologists are generally not looking for, or ignoring, the possibility of, potential evidence of settlement habitation on the continental shelf. (Looking closely, he says that most resources are allocated to wrecks (Hancock, 2002: p. 58) and thus creates a vacuum into which he can place his unevicenced speculations).

Archaeology is expensive: underwater archaeology is *extremely* expensive. Whether on land or under water, archaeologists rarely know what's there and there are considerably more wrecks than any hypothetical 'Flooded Kingdoms'. The work Hancock describes as 'infinitesimal' (Hancock, 2002: p. 58) has located submerged sites showing human habitation from all periods, complementing what has been found on land, but none show any evidence of 'Flooded Kingdoms'. Did cultures at different stages of development coexist in the same locations without interacting? Douglas Adams once noted that: *In an infinite universe anything.....is possible* and this is all Hancock can do: invoke 'possibilities' to try and excuse the inconsistencies in his work. But no amount of 'possibilities' can make, or replace, a single fact.

The Cowboy and the Indians

In summer 2001, reports from the Gulf of Cambay (north-west India) suggested 'structures' deep beneath the water's surface. Sonar indicated regular 'features' and dredging recovered 'artifacts', wood and indications of a 'script'. With an age of at least 7000 years from the sea level rise model and comparable radiocarbon dates from the wood, the Hancock hype started. Newspaper articles and his TV

series featured a computer graphics reconstruction of this 'city' claiming it to be the size of Manhattan.

The sonar images are largely seen as 'Ghosts' in the machine or natural features and geologists quickly recognised the 'artifacts' as 'geofacts' (natural objects that frequently fool non-specialists) and the 'script' as the artificial-looking 'hieroglyph fossil'. Considering the very strong currents in the Gulf, which is fed by several sizeable rivers, it is clear that even if the 'artifacts' are manmade they probably originated elsewhere. There may have been humans in this area prior to submergence, but a large sophisticated 'city' would have had a massive 'influence' on the surrounding (still dry) area, for which there is no evidence. Consider Roman London (*Londinium*): the surviving traces are minimal and buried deep below the modern city. However, projecting the routes of the main Roman roads in southern England would show that they converged on the Thames; a river crossing at the intersection of major roads would be strategically important and result in a large settlement.

Proving the Academics Right?

Another misrepresentation is Hancock's frequent claim that academics (sometimes western academics) don't take 'Flood Myths' seriously, but a literature search soon disproves this.

Mahabalipuram, south-east India, was identified by Hancock as a location with flood myths, 'lost' temples, and relevant fisherman's tales. He dived there in April 2002; 'ruins' were found, seemingly vindicating him, and the sea level rise model gave an age of more than 6000 years. He claimed to *...have proved the myths right and the academics wrong.* (Hancock, 2002a)

Indologists and those from the area were not surprised as the temples were historically known (7th Century AD); Hancock had 'mythologised' history!

Within days the thoroughness of Hancock's 'research' was exposed. The *Geological Survey of India* and the *Archaeological Survey of India* had scientifically studied (although not dived) the area in 1996-7 (Mohapatra & Prasad, 1999). He admitted that he was unaware of this. What else did he miss?

This work concluded that the 'ruins' were probably 'associated' with the surviving temple and that erosion was the most likely cause of submergence.

Some Lead, Others Follow

But hadn't Hancock *proved* that there *are* submerged ruins? It was soon revealed that the *Underwater Archaeology Wing* of the *Archaeological Survey of India* had beaten him to it, dived as early as November 2001 (Mukul 2002), and noted, 'The remnants are well carved and look like mouldings and pillars of temple. They are similar to the carvings in the existing temples of Mahabalipuram'.

Underwhelmed

The book and TV series also include submerged artificial-looking, but undoubtedly natural, formations such as the Caribbean *Bimini Road*, features near Malta, and some interesting, but useless for practical purposes, regular-shaped rocks near Japan. There is nothing that will 'revolutionise' our understanding of the past and *Underworld's* 'success' depends upon Hancock's ability to keep real underwater archaeology from his audience and substitute his evidence-free speculations.

The big 'mystery' is how he gets away with it but as Abraham Lincoln noted, *You can fool all the people some of the time and some of the people all the time, but you can not fool all the people all of the time.*

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THE ANTIQUITY OF MAN: REVIEWING HINDU CREATIONISM

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Introduction

What convinced me to take up this task was twofold: firstly, Michael Cremo and Richard Thompson's books have been phenomenally successful, selling briskly around the world. They have a simplistic yet expressive writing style that is highly effective. Their Hindu creationist views are presented in such a way as to appear scientific when

they are in fact being *scientistic* (using scientific terminology in order to expropriate scientific authority). Secondly, considering its widespread commercial success, it was clear that their book has reached many readers who neither possess the background knowledge or the training nor have easy access to university facilities and the technical journals, in order to do their own critiques.

Furthermore, the literature of human evolution is vast and extremely complex, and, I felt it needed to be synthesised within a framework accessible so that a general readership can apply it to the evaluation of Cremo and Thompson's work.

Cremo is a prominent staff member of a New York Hindu Institute, the research branch of the International Society for Krishna Consciousness. Cremo and Thompson used the Institute's basic religious concepts based on Vedic literature from India, and expressed them in the bestseller *Forbidden Archeology* and its shortened version for the general public, *The Hidden History of the Human Race*. On his website (<http://www.mcremo.com/cremo>) Cremo states:

With Dr. Richard L. Thompson, a founding member of the Bhaktivedanta Institute, I began a series of books aimed at both scholarly and popular audiences. The first to be published was Forbidden Archeology. ... This book shows that archaeologists and anthropologists, over the past one hundred and fifty years, have accumulated vast amounts of evidence showing that humans like ourselves have existed on this planet for tens of millions of years. We show how this evidence has been suppressed, ignored, and forgotten because it contradicts generally-held ideas about human evolution. In lecture presentations on Forbidden Archeology to scientific and lay audiences around the world I see a new consciousness emerging that integrates science and religion into a cohesive paradigm of reality.

In light of the book's foundation in the Hindu creationist narrative, it may be somewhat surprising to encounter the ringing endorsement given to their book by one of the most prominent Christian creationists, Phillip Johnson, on the back cover of *The Hidden History of the Human Race*: 'A stunning description of some of the evidence that was once known to science, but which has disappeared from view due to the "knowledge filter" that protects the ruling paradigm.'

The Anatomical Evidence

The anatomical evidence, both fossil and contemporary, demonstrates that australopithecines and chimpanzees share a geologically recent common ancestor and that *Homo sapiens* are descendants of the evolutionary branch that began with the divergence of the australopithecines. Nowhere does *The Hidden History of the Human Race* discuss these anatomical characteristics in detail.

The anatomical characteristics that link the australopithecines to *Homo*, and show their intermediate form between modern humans and the last common ancestor between humans and chimpanzees, include the following:

- The canines of the australopithecines do not project much further forward in relation to the other teeth than they do in *Homo*;

- Australopithecine canines also show a decrease in sexual size differences over time; the more recent forms are more like the condition of modern humans;
- Tooth enamel progresses to a more *Homo*-like thickness over time;
- Wear patterns on australopithecine teeth suggest a 'crushing' action, similar to that of *Homo*;
- The cranial capacity of the australopithecines increases to a capacity range approaching that of early *Homo*;
- The australopithecine foramen magnum, which allows the spinal cord to connect with the base of the brain, is located more toward the base of the skull than in apes, yet not completely under the skull, as in *Homo*, except in the robust australopithecines (also known as *Paranthropus*) where it was just as in *Homo*; and
- The features of the tibiae (orientation angle, thickness and internal structure) shared by australopithecines and *Homo* reflect the demands placed on their bodies by bipedalism.

The anatomical similarities between chimpanzees and anatomically modern humans (*Homo sapiens*) can be summarised as follows:

- In both species, the rib cage is broad from side to side and shallow from front to back; the rib cage extends back beyond the vertebral column;
- Both have a dorsally-placed scapula and shoulder joints facing outward to the side, giving humans a mobile shoulder joint; a hangover from our arboreal ancestry; and
- The positioning and angle of the humeral shaft and humeral head and other joints in the forelimb are the same in both species.

It is within this context that paleoanthropologists evaluate the taxonomic significance of hominin fossil remains. The anatomy of upright walking should show great differences between hominins and their nearest ape relatives, while other features that show only slight variations between modern chimps and humans ought to be less distinctive in the fossil record. For example, in 1965 Patterson and Howells (1967) discovered a humerus at Kanapoi that has now been attributed to *Australopithecus anamensis*. Although Cremo and Thompson do not mention *A. anamensis* by name, they discuss this find and attribute it to *Homo sapiens* by stating:

Patterson and Howells found that the Kanapoi humerus was different from the humeri of gorillas, chimpanzees, and australopithecines but similar to those of humans. They noted that 'there are individuals in our sample of man on whom measurements...of Kanapoi Hominoid I can be duplicated almost exactly'. Patterson and Howells would not have dreamed of suggesting that the Kanapoi humerus belonged to an anatomically modern human. Nevertheless, if an anatomically modern human had died at Kanapoi 4.0–4.5 million years ago, he or she might have left a

Modern Chimpanzees	Australopithecines	Modern Humans
Canines larger and project out from tooth row	Canines slightly larger, but non-projecting	Canines of similar size to other teeth and non-projecting
Extension canine size dimorphism by sex	Moderate canine size dimorphism by sex	Minimal canine size dimorphism by sex
Tooth enamel thin	Tooth enamel moderate	Tooth enamel thick
Dental wear pattern shows grinding action	Dental wear pattern shows crushing action	Dental wear pattern shows crushing action
Cranial capacity average 400 cc	Cranial capacity 350 – 540 cc	Cranial Capacity \geq 1000 cc
<i>Foramen magnum</i> opens toward rear of skull.	<i>Foramen magnum</i> opens between rear and base of skull.	<i>Foramen magnum</i> opens at base of skull.
Tibiae thin and angled	Tibiae thicker and straighter	Tibiae thick and straight
Rib cage broad and extends past vertebral column	Rib cage broad and extends past vertebral column	Rib cage broad and extends past vertebral column
Scapulae on the back, shoulder joints oriented to the sides	Scapulae on the back, shoulder joints oriented to the sides	Scapulae on the back, shoulder joints oriented to the sides

Table 1

Anatomical characteristics of modern chimpanzees, australopithecines and modern humans

humerus exactly like the one they found. Further confirmation of the humanlike morphology the humanlike morphology of the Kanapoi humerus came from anthropologists Henry M. McHenry and Robert S. Corruccini of the University of California. They concluded that 'the Kanapoi humerus is barely distinguishable from modern *Homo*' and 'shows the early emergence of a *Homo*-like elbow in every subtle detail'. In a 1975 study, physical anthropologist C.E. Oxnard agreed with this analysis. He stated: 'we can confirm clearly that the fossil from Kanapoi is very humanlike.' This led Oxnard to suggest, as did Louis Leakey, that the australopithecines were not in the main line of human evolution. Keeping *Australopithecus* as a human ancestor would result in a very unlikely progression from the humanlike Kanapoi humerus, to the markedly less humanlike humerus of *Australopithecus*, and then to one more humanlike again (Cremo and Thompson 1999: 215–22).

It should be made clear that it is the claim of Cremo and Thompson, not Patterson and Howells, that modern humans would have left behind a 'humerus exactly like the one they found'. The important question for paleoanthropologists is not whether the humerus proves *Homo sapiens* was present 4.2 million years ago but whether the humeri of other australopithecines really are distinct from that of Kanapoi.

To begin to answer this question, we should look to see if more recent examinations have been undertaken on the Kanapoi humerus than McHenry and Corruccini's 1975 study. When the humerus was first uncovered, its date could not be established with any degree of certainty. Shortly after the discovery of *Australopithecus anamensis* and the application of advanced dating techniques to the area, it became possible to address some of the important issues in understanding the place of this specimen. By the early 1980s, the discovery of *Australopithecus afarensis* at

Hadar, Ethiopia, and further *Australopithecus anamensis* finds at Kanapoi allowed a more careful analysis. Consequently, the findings of these new investigations, which include more material, better dating, and new techniques for quantitative analysis, should bear more weight than the earliest reports on these specimens in determining the place of their in human history. In other words, Cremo and Thompson appear to have selectively ignored the most recently scientific research and analyses work that would have resolved many of the issues and questions their book raises in favour of 25-year-old publications that can only be regarded as preliminary.

Modern studies show great similarities between the Kanapoi humerus and those of *A. afarensis*. However, apart from its many resemblances to the Hadar remains, the Kanapoi humerus lacks an important diagnostic trait of the genus *Homo*. Modern human distal humeri contain an internal cavity of spongy bone near the elbow - a derived trait that is lacking in the Kanapoi find (Wolpoff 1999).

Ancestors and Intermediates

No creationist story would be complete without disputing that the australopithecines are a part of our ancestral lineage. It is an essentially part of their myth, in which they try to impress ordinary people by citing two palaeoanthropologists in particular: Sir Solly Zuckerman and Charles Oxnard. Standard creationist procedure also dictates that those direct rebuttals to Zuckerman and Oxnard's articles or subsequent re-evaluations of earlier work are overlooked. *The Hidden History of the Human Race* follows the dictate to the letter, but with a few added extras by stating: 'Louis Leakey held that *Australopithecus* was an early and very apelike offshoot from the main line of human evolution. Later, his son Richard Leakey took much the same stance' (Cremo and Thompson 1999: 257). What Cremo and Thompson do not mention is that Richard

Leakey has subsequently abandoned this stance (Leakey and Lewin 1993: 141).

After giving the impression that Richard Leakey still supports his father's idea, thereby lending an additional 'air of authority' to their claims, Cremo and Thompson head back on the normal creationist track, insisting that Zuckerman and Oxnard have disproved any ancestral relationship between *Homo* and the australopithecines. This approach is mandatory, for if one of the australopithecine species were ancestral to *Homo* it would effectively destroy any factual basis to their religious paradigm that humans are a separate creation and that there are no intermediates between humans and other primates. Zuckerman and Oxnard lost the debate in the 1950s and 1970s respectively; paleoanthropologists do consider *Homo* to have emerged from australopithecine ancestral stock. It should also be noted that while Oxnard removed the australopithecines from our line of *direct* ancestry, he agreed that *Homo* and the australopithecines shared a common ancestor.

Out of the muddle that Cremo and Thompson make of the hominin fossil record, it is fair to ask what they expect intermediary ancestors, along the path from the time of the chimpanzee-hominin split to the advent of *Homo sapiens*, to look like. The further back in time we go, surely the greater the anatomical similarities between chimpanzees and hominins become. Those very characteristics, which are cited as supposed proof that the australopithecines were different from us and therefore do not belong in our lineage, are indeed proof positive of the transition from the more ape-like ancestors to the more modern human forms that appear in the fossil record over the past 5 million years. But how do Cremo and Thompson evaluate this evidence?

In 1913 the German geologist Hans Reck was searching at Olduvai Gorge when his helpers came across an anatomically modern skeleton in Bed II, the second oldest of five beds (Morell 1995). Bed II is dated at 1.2 million years ago (MYA). Louis Leakey visited the site in 1931 with Reck and agreed that the skeleton was in the bed where it was originally preserved and not an intrusive burial dug down into the older bed from a younger level. Later however, after soil samples were tested from Bed II and the skeleton, they published their revised conclusions, in the prestigious journal *Nature*: the skeleton was indeed an intrusive grave filling from Bed V - where modern human skeletons ought to be found. Leakey also stated, in *Stone Age Races of Kenya* (1935), that although at first this specimen appeared to be of great antiquity, subsequent closer scientific investigations revealed its age to match its recent modern morphology.

Cremo and Thompson (1999: 236) reject the revised conclusion on the invalid basis that 'perhaps Reck was simply tired of fighting an old battle against odds that seemed more and more overwhelming'. Further details are given by Louis Leakey's biographer, Virginia Morell

(1995), that go unmentioned by Cremo and Thompson (1999):

Meanwhile, in England the death knell was sounding for Olduvai Man. Several independent geological tests had been run on the skeleton and soil samples. These showed that the body had been buried in Bed II in comparatively recent times, when a fault exposed that horizon. Sometime after the burial, Beds III and IV eroded away; then Bed II had been covered over by the deposits of Bed V. Reck had mistaken the soil of Bed V for that of Bed III - an easy enough error to make as both are a deep red in color (Morell 1995: 66).

Cremo and Thompson (1999: 237) point out that Bed V has an age of 400 000 BP, so the skeleton 'still gives a potentially anomalous age for the fully human skeleton'. However, Bed V is divided up into a number of component beds. While the lower Ndutu Bed began accumulating around 400 000 BP and ended c. 75 000 BP, the most recent Naisiusiu Beds are dated between 22 000 - 15 000 BP. Anatomically modern humans appeared before the end of the time period encompassed by the Ndutu Bed.

Reiner Protsch (1974) published the results of radiocarbon tests in 1974, which dated the skeleton to 16,920 years ago, well within the upper limits of the most recent age of the Naisiusiu Bed and much *younger* than the original estimates based on the burial horizon. These estimates are challenged as untrustworthy by Cremo and Thompson (1999) on the grounds of:

- Uncertainty over whether the sample tested came from the original skeleton;
- The sample (224 grams) being a third smaller than the normal test size;
- Possible contamination, if the sample was from the skeleton, by more recent carbon through exposure to bacteria and preservation with Sapon, an organic preservative, which *may* not have been taken into account during the testing process; and
- The amino acids not being dated individually (for the technique called 'Accelerator Mass Spectrometry' (AMS) had not yet been developed) therefore placing doubt on the reliability of the resulting age.

In the 1970s the sample requirement of many laboratories was 1 gram of carbon. Some laboratories had smaller counters. Fresh bone contains 20% collagen. The carbon content of collagen is 40%, which means that 224 grams of bone could potentially give 18 grams of carbon. Even if the bone was poorly preserved and had only 1% of carbon in it, the tests could still have been carried out reliably.

The procedure of extracting collagen with weak acid and getting pseudomorphs (whereby one mineral chemically replaces another mineral without changing the external form of the original mineral) eliminates the confounding effects of bacterial contaminants.

The requirement for dating each individual amino acid is an artifact of AMS dating procedures; it is not necessary

in most other radiocarbon analyses. In part, it is necessary when using extremely small samples, which can confound the age estimates in the presence of even very small amounts of contaminant. In the case of the skeleton Reck studied, this technique is irrelevant and do not affect the validity of the procedures followed by Reck's laboratory.

Cremona and Thompson make an additional elementary mistake. In the extremely unlikely event that there were errors with the dating, they would be in exactly the opposite direction from what Cremona and Thompson predict: an intrusive burial would only have contamination from material much older than the skeleton. This contamination would make the age estimates falsely older, not younger, than the true age of the skeleton.

The Tool Man

The spread of stone tools is another line of evidence that supports the conventional views of human evolution, the rise of *Homo sapiens* (Foley and Lahr 1997), and the rise of modern behaviour (Kaufman 1999, McBrearty & Brooks 2000). *The Hidden History of the Human Race* postulates that some eoliths (literally, 'ancient stones') resemble Upper Pleistocene and Holocene (Late Stone Age) tools, despite the fact that none of the 'tools' they use as examples exist today except as drawings whose accuracy cannot be determined. Furthermore, none is available for modern analyses with more powerful tools and techniques that have been developed in the last several decades.

According to Cremona and Thompson, anatomically modern humans manufactured Holocene-type tools for tens of millions of years ago, side-by-side with Middle Stone Age, Acheulian and Oldowan-type industries. In that case it is very revealing that *H. heidelbergensis*, who has clear anatomical ancestry to *Homo sapiens*, is found with the Acheulian (1.6 mya – 250 kya) and Middle Stone Age (250 kya – 20 kya) industries; the first clear *Homo sapiens* fossils are in context with Middle Stone Age industries. Cremona and Thompson's hypothesis would require an association between hominin species and ancient tools which simply does not exist.

Cremona and Thompson do little better with New World tool technologies. Even taking into account the fierce debate raging within New World palaeoanthropology regarding the age of the first populations in the Americas (Fiedel, 2000), the date can be fixed in a broad range from about 14 000 - 40 000 BP. Cremona and Thompson (1999) seek to extend the time-frame back to beyond 200 000 BP and one of the sites they focus upon is Calico in California, North America.

Their use of the specimens from Calico is a good example of their brand of 'scientific scholarship'. This is a prime example of a setting for eoliths, some dated to 200 000 BP by uranium isotope methods (Morell 1995). The site is in an alluvial fan, the geological signature of an active transport system. The site has clearly, therefore, been

disturbed by a significant movement of rocks and boulders that have been removed, re-deposited, and modified by erosion, abrasion, and impact. Cobblestones, often used for the most primitive stone tools, were broken as a consequence of this movement. These factors make any deduction that these fractured pebbles were stone tools a haphazard guess, at best. On the other hand, the geological processes that most likely created the Calico eoliths are detailed by Haynes (1973).

Conclusions

Cremona and Thompson can reach their conclusions only with a selective use of scientific data and ignoring the progress that paleoanthropology has made both in the number of specimens and the techniques available for analysis. Their conclusions regarding the nature of the hominin fossil and stone-tool record are internally contradictory, often demonstrating precisely the opposite of what they propose as their 'alternative' model of human history. As with other creationist models, this inconsistent and contradictory use of the scientific evidence is caused by a prior commitment to a pre-scientific religious model. They commit the very sins of which they accuse the scientific establishment: in their book, 'evidence has been suppressed, ignored, and forgotten because it contradicts' their prior commitment to a different view of human history.

Both the hominin artifactual and fossil remains supports current models of human evolution: Hominins diverged from a common ancestor with chimpanzees between 5-8 million years ago, first mastering upright walking and basic tool making in a succession of australopithecine species, later dramatically increasing geographic range, brain size, and cultural complexity, including tool technologies. Despite the variety of hominin species to precede us and pioneer a number of evolutionary innovations, we - modern *Homo sapiens* - remain the sole surviving member of the hominin branching tree.

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THE NEWPORT TOWER AND THE PLOWDEN PETITION

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Introduction

Before 1492 at least one group of Europeans, probably two, visited North America. The first is the Norse, whose visits we know about through evidence of trade, the Sagas, and the remains of one short-term settlement. The second group is less certain, but it is very likely that sailors from Bristol were fishing off North American coasts before 1492. The historian Kirsten Seaver believes that English sailors were visiting Greenland during much of the 15th century and that 'by 1480 at the latest, Bristol men in search of less troublesome fishing opportunities had learned to navigate directly from western Ireland to the Newfoundland-Labrador Banks ('the Isle of Brazil') without using the familiar outward route by way of Iceland or Greenland' (Seaver 2004 p. 84). Besides evidence of trade in the form of artefacts (Barrett 2004), the only other widely accepted archaeological evidence is the short-term settlement at L'Anse aux Meadows in Newfoundland.

This article focuses on a widely disputed claim for an early European structure in North America, namely the Newport Tower in Newport, Rhode Island, and in particular the claim that its existence before any known settlement is proven by certain documents.

Rhode Island

Rhode Island is the smallest state in America. It is bordered to the north and east by Massachusetts, to the west by

Connecticut, and to the south by the Atlantic Ocean. One of the original Thirteen Colonies, it was founded in 1636 by Roger Williams. The earliest known contact between Europeans and its Native American inhabitants was by Giovanni da Verrazano. His 1524 report to the King of France describes a visit to an area which was probably in the vicinity of Newport. In his Report to Francis I, July 8, 1524, 'The History of the Dauphine and Its Voyage' (a 16th-Century copy of this, in Italian, is held by the Pierpont Morgan Library, New York.) he writes:

(On entering the Bay) we founde about 20 small boates of the people which with divers cries and wonderings came about our shippe, comming no nearer than 50 paces toward us, they stayed and behelde the artificialnesse of our stripe, our shape & apparel, I than they al made a loud showte together declaring that they rejoiced. when we had some-thing animated them using their geasters, they came so neers us that wee cast them certaine bells and grasses and many tones, which when they had received them they lookte on them I with Laughlin & came without feare aborde our ship.... This is the goodliest people and of I the fairest conditions that wee haue found in this our voyage. They exceed us in bignes, they I are of the colour of brasses (also translated as bronze), some of them encline more to whitnes; others are of yellowe (also translated as tawny) colour, of comely visage with long & black haire.... wee were oftentime within the lande 5. or 6. leagues, which we

found as pleasant as is possible to declare very apt for any kinds of husbandry of corn, wine and oyle: for that there are plaines 25. or 30. leagues broad, open and without any impediment of trees and such fruitfulness, that any seede being sowne therein, will bringforth most excellent fruite. We entred afterwards into the woods which wee found so great and thicke, that any armie were it never so great might have hid it selfe therein.... We saw their houses made in circular or rounde forme, 10 or 12 foote in compasse, made with half circles of timber, separate one from another without any order of buldIng, covered with mattes of strange wrought cunningly together which sane them from the wince and raise.... I say that the south of the haven lyeth open to the South halfe a league broads, and being entered within betweene the East and the North, It stretcheth twelve leagues. where It weareth broder and broder, and maketh a gulf about 20 leagues in compasse, wherein are five small islands very fruitful and pleasant, full of hie and broads trees, among the which Illandes, any great Nauie may rude safe without any feere of tempest or other daunger.

Many claims are made of what Verrazano wrote and found, but anyone who actually reads a translation of his report will see clearly that nowhere does he mention white or Norse Indians, European architecture, or indeed any signs of Europeans having been in any of the areas he visits. A few years later his brother produced a world map which illustrated his voyage. I have found a number of places on the Internet where people have confused the two Verrazanos and made various unfounded claims of what they wrote and drew. The map does indeed have various symbols on it, one of which might be a tower near but not at Newport, and a variety of French names to which little significance can be attributed.

Rhode Island wasn't settled by Europeans until over a century later, when in 1636 Roger Williams and a group of followers left the Massachusetts Bay Colony in search of freedom to worship and established a settlement at Providence. Newport was settled in 1639.

The Tower

The Newport Tower itself is a round stone tower supported by eight cylindrical columns forming stone arches, and is now about twenty eight feet high. It is located at 41' 27 minutes north latitude on a hill in Touro Park, Newport. Its origin is uncertain and claims range from Celts, Irish, Templars, Norse, Portuguese, and Chinese to, of course, the early settlers of Newport.

The windmill is mentioned in the 1677 will of the Governor of Rhode Island, Benedict Arnold, who wrote of his 'stonebuilt-windmill.' John Hull, a contemporary of Arnold, observed the Arnold windmill also in 1665 (Diaries of John Hull, 1847, Transactions of the American Antiquarian Society, pp 208,213,218, cited in Kuhlmann, 1929:5). Hull goes on to state that Arnold built the tower to

resemble a windmill he had known back home in England. This is the Chesterton Windmill in Warwickshire, which although different in style is a stone windmill built on columns, and although the columns are square and have capitals, they look very much alike. It used to be argued that it was originally built as an observatory, but recent work on the Chesterton estate records has confirmed it as a windmill (Wise 1994).

The proponents of an earlier construction have many arguments that I will not attempt to deal with in this short article - architectural features, the fireplace in the 2nd floor (although there are plenty of examples of early windmills with fireplaces), measurements, etc. Those arguing for its being of colonial origin rest their arguments partially on the documentary evidence but also on archaeological evidence and radio carbon dating (both of course disputed by the first group).

This article, however, concentrates on the specific claim that the tower is mentioned in a 1632 document, in other words that it was already in existence seven years before Newport was settled. The claim made by Frederick J Pohl (1945; not the science fiction writer) was that a petition and land grant made in 1632 referenced a round stone tower supposedly located at Newport, Rhode Island.

The Petition

Some time ago I had to go to Heathrow to pick up my daughter and found time to visit the Public Records Office in Kew, where I read and copied the two documents in question. The first is a petition by Sir John Lawrence and others to King Charles, asking for a grant of land. This is referred to often as the 'Plowden Petition' as it was made on behalf of Sir Edmund Plowden, an English Catholic with an Irish knighthood (and later an Irish peerage and an Earl Palatine). The second document is much longer and harder to read, and is called the 'Commodities of the Island called Manati or Long Island within the Continent of Virginia'. This describes resources to be found in the land described, proposals for their use, proposals for the types and numbers of settlers, etc.

The only mention of the tower is in the 'commodities' document, which states:

27: So that 30 idle men as soldiers or gent be resident in a rownd stone towre and by tornes to trade with the savages and to keep their ordinance and arms neat.

There are two issues here. Is this a statement about an existing tower, and, more importantly, what is the location in question?

Boland (Boland) writes about Pohl's article, adding that 'as Pohl has noted, if it were a "commodity" to be built in the future, it would not be listed in the 29 paragraphs at all. These paragraphs list only existing "commodities" - advantages to be had immediately on settling in New Albion.'

However, Para 28 starts, 'The partners are willing to maintain (?) Governor (?) and 2 men'; the rest is difficult to read but is clearly about maintaining 25 soldiers and 25 mariners to trade with the Indians. In other words, it is not about existing 'commodities'. Other 'commodities' statements refer to arrangements for a governor, others to the possible earnings of fishermen, others to the numbers of types of settlers to be 'provided' (some were to be slaves, for instance). In addition it makes specific proposals for financing housing and fortifications in the first year and general proposals for boats, mills and shipbuilding. The document also mentions 'a race of buffaloes which will be ridden and brought to plowe and be milked.' It thus mentions existing, future, and fanciful 'commodities'.

Thus it is, in my opinion, impossible to say whether when it says 'a rownd stone towre' that it is referring to an existing tower or one to be built; of course, if it had said 'the rownd stone towre' it would be clear what was meant.

The next question is the location of the land requested. A transcription of the first part of the petition was provided for me by Brian Scott, who notes:

'Here's a transcription. Square brackets indicate superscriptions. Angle brackets, which I believe occur only once, indicate expansion of a standard scribal abbreviation. I've separated the lines for ease in matching the transcription to the original. I've omitted a couple of scribal marks -- horizontal strokes over letters, and arcs with tick marks -- that probably don't have any significance beyond ornamentation. I did this in some haste, and there might be one or two errors, but this is a very straightforward hand, so I don't expect many. (It's a mixture of secretary and italic rather characteristic of the period, and I've read quite a bit of it at one time or another.)' (*Editor's note: the line breaks coincide with the original text.*)

'To the Kings most Excellent Ma:[tie]
The humble petiton of S:[r] John Lawrence K:[t] C
Barronett, Sir
Edmund Plowden K:[t] S:[r] Boyer Worsley K:[t] John
Trusler Roger
Pack William Inwood, Tho: Ryebread Charles Barret, &
~~~~  
George Noble, Adventurers./  
Sheweth  
That whereas there is a remoate place w[th]in the Confines  
of Virginia some  
150 myles Northwards from the Savages, & James Citty  
w[th]out the bay of Chisapeak  
and a conuenient Isle there to be inhabited called Manitie,  
or long Isle in 39  
degrees of lattitude, and within the bounds of Virginia  
abutting on the Ocean  
Eastwards 18 myles and of the Continent Westwards neere  
Dellaware Baye.  
and not formerly granted, And are willing now att their  
only coste, and

chardges to adventure, plant, and settle there 300  
inhabitants for the ma  
king of Wine, Saulte, and Iron, ffishing of Sturgeon, &  
Mullet, and for  
Cattle, and Corne for the Coloney and for the Yearely  
building of Shipping  
there with all Materialls for Yo[r] Ma:[ties] service, All  
[th]is is to be done w[th]out any  
chardge to Yo[r] Ma:[tie]/ att the only costs of the  
adventurers, and w[th] the haszard  
of Yo[r] said subiets liues and fortunes: Who humbly  
request only Yo[r] Ma:[ties]  
Royall Proteccion w[th] a fitting Pattent and power to  
enable them to gouerne  
and order their Planters and seruaunts and with a supply of  
Victualls fro~  
Ireland being much nearer, Cheaper, and safer sailing then  
from hence.  
Humbly beseeching Yo[r] most Excellent Ma:[tie]/ to com~aund the S[d]: Chaun  
celor of Ireland to make to Yo[r] subiects y[e] Adventurers,  
a Pattent und[r]  
Yo[r] Seale of Ireland of the saied Isle and 30 myles square  
Coste next adioyneing to be erected into a County Palatine  
called  
Syon to be held of yo[r] Ma:[ties] Crown & of Ireland  
w[th]out appeale or sub  
ieccion to the Governour, or Company of Virginia, and  
reserving y[e]  
5:[th] of all Royall Mynes, and w[th] the like title, dignity  
and priuiledges  
to S[r] Edmund Plowden there as was graunted to S:[r]  
George  
Caluert K:[t] in Newfound land by Yo[r] Ma:[ties] Royall  
father : And with  
y[e] vsuall grants and priuiledges to other Coloneys, and  
w[th] power for y[e]  
supplyinge of the saied Coloney by licence of the Lord  
Deputy Presi  
dent or Lords Justices to transp<or>te thence Corne, Cattle,  
and Victu  
alls, and condemnd, or reprimed Malefactors, and  
Vacabonds at Yo[r]  
Ma:[ties] price, and Wages for necessary Artificers././.  
And Yo[r] Subiects shall euer pray for yo[r] Ma:[tie]/./'.

And there's the problem with the Newport Tower claim. The petition says that this 'county palatine' is to include the island in question and some of the adjoining continent, and is at 39 degrees latitude and 150 miles north of James City. Newport is at 41.27 degrees latitude and much more than 150 miles north of James City. The area in the petition is the area of the Delaware/Maryland peninsula, and the tower in the 'commodities' document, whether proposed or existing, is not in Rhode Island.

Shortly after this, Plowden revised his petition, renaming the island 'Isle Plowden', increasing the number of prospective settlers, changing the name of the colony to New Albion and asking for a grant 120 miles to a side to include all other small islands between 39 and 40 degrees latitude. A patent was issued on July 24, 1632, although the charter was not forthcoming until 1634. This was a feudal charter held by Plowden by knight service of Charles I's Irish crown (which meant the king could profit from it), allowing Plowden to grant titles, etc. which he liberally did (creating one of his sons 'High Admiral and Baron of Roymount').

However, nothing came of it. Plowden did not leave for New Albion until 1642, spending his time in lawsuits with tenants, creditors, debtors and his wife while Swedes were busy colonising along the Delaware River. Rather than go to New Albion, in fact, he went to Virginia. An attempt to go to New Albion was then met with mutiny, and that was the end of Plowden's only known attempt to settle the land he had been granted. He spent the rest of his life (he died in 1859) engaged in lawsuits and in conflict with his family.

A copy of the original petition can be found at <http://www.ramtops.co.uk/petition.html>

And excerpts from an article on the charter (Carter and Lewis 1959) at

<http://www.ramtops.co.uk/charter.html>

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## Further Reading

For a review of the history and archaeology of the Newport Tower, see:

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Hertz, J. (1997) Round church or windmill? New light on the Newport Tower. *Newport History: Journal of the Newport Historical Society*, **68** (part 2, no. 235), 55-91. (Pages 92-97 include an appendix: 'Carbon-14 dating of the Newport Tower'.)

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# LINGUISTIC RECONSTRUCTION AND REVISIONIST ACCOUNTS OF ANCIENT HISTORY

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## Introduction

This article deals with recent novel non-standard (often 'fringe') claims made about ancient languages, and with the writers' approaches to historical linguistics, which display a considerable degree of uniformity. Claims in this area are sometimes made for their own sake, by non-linguists with a specific interest in language(s) or more usually one particular language, frequently their own. More often, however, they are invoked in support of revisionist positions on early human history – often,

again, by authors with an interest in one specific culture, frequently their own (see below for more on this aspect of the matter).

Revisionist historical claims of this kind typically assert that some geographically (and in some cases temporally) separated cultures and the associated languages, normally thought of as unconnected, were in fact closely linked. There are two main ways in which this is said to have occurred. One involves initial cultural diffusion from an



earlier common source civilisation, which is itself typically unknown to mainstream scholarship. (Sometimes this civilisation is said to have been destroyed in a catastrophe, e.g. Atlantis).

In many cases this civilisation is said to have been an ultimate ancestor civilisation of humanity. Its language is thus often identified as the ultimate ancestor language of humanity, the *Ursprache* or, in current linguistic terminology, Proto-World. The language identified as Proto-World may be (a) a known language (usually an ancient rather than a modern one, for obvious reasons); (b) a language reconstructed by linguists as an ancestor of a known family of 'genetically' related languages (e.g., Proto-Indo-European, the preliterate and thus unattested ancestor language of the Indo-European language family; this strategy is actually rather rare); or (c) a language reconstructed/invented by the author in question.

In this context, two points should be noted: (a) it is not regarded as certain that there ever was one single Proto-World: humanity may have developed language more than once; (b) with only a few exceptions – some now dated and all marginal to the mainstream at best (e.g. Swadesh, Landsberg, Ruhlen, Ryan, Diamond, Willmore, even the respected McWhorter) – it is generally agreed by linguists that Proto-World or multiple ancestor languages must have been spoken so long ago (at least 70,000 years ago, probably more like 150,000) that (given the observed range of rates of linguistic change) it/they cannot possibly be reconstructed in any detail. (Ryan is unusual in accepting the dating but still claiming that Proto-World can be largely reconstructed.)

However, not all such theories of unrecognised common 'genetic' origin involve claims about a primordial common ancestor civilisation or Proto-World. Some simply involve alleged more recent (but still ancient) 'genetic' links between specific cultures and languages normally deemed unconnected.

Obviously, in many such cases (and in all such cases involving a single ancestral civilisation and an *Ursprache*), the linguistic, archaeological, textual and other evidence offered is currently judged inadequate by mainstream scholars to demonstrate the reality of the proposed events and situations.

The other way in which unacknowledged links between languages may have occurred involves later (but again still ancient) diffusion by way of contact; e.g., members of one culture completed long voyages and arrived in the territory of the other, and the cultures and languages influenced each other. (Claims of this kind clearly do not directly involve an *Ursprache*.) Some such events are of course recognised by mainstream scholarship as at least plausible, but there are many more such scenarios which are regarded in the mainstream as implausible, or at best as feasible but altogether undemonstrated. In these cases, the evidence offered is again judged inadequate to demonstrate the

reality of the alleged events, as in the case of Heyerdahl's theories of transoceanic contact (his successes showed only that such voyages were possible).

As intimated, the alleged empirical evidence for the actuality of such diffusion or contact is varied. It includes folk-historical narratives, legends and myths, interpreted as referring to the events in question, and also archaeological evidence in the form of surviving material artefacts which are held to display significant similarities which would be unexpected in the absence of contact or common origin. However, linguistic forms (spoken and/or written) are very commonly invoked here – whether or not they themselves are of interest to the writers in question. One can readily see why. Linguistic forms (with their meanings) appear much more specific and much more easily identifiable than most other cultural traits, and the probability of chance similarity seems much lower. The non-specialist author therefore believes that a case for a significant connection between cultures can be supported especially well through language data. For instance, it is observed that the male name *Madoc* is common in Welsh and that the male name *Modoc* is common in Mandan (an Amerindian language). It is held that the two forms are so similar that they are very probably etymologically related; and on the basis of a limited number of individual cases of this kind it is deduced that the Welsh and the Mandans had a common ancestor culture or else experienced influential contact (not recognised by contemporary mainstream scholarship) in remote times. (In this particular case the associated historical claims usually involve Prince Madoc and his supposed voyage to America.)

Such cases are in fact much more complex and uncertain. In the early years of historical linguistics, this kind of approach was common in mainstream scholarship; but we have now been studying language change and the reconstruction of unrecorded ancestor languages for over 200 years, and we have learned that one cannot rely on unsystematic superficial resemblances of form, even when accompanied by similarity of meaning, in establishing such etymologies. The methods adopted by these writers are now confined to the amateur fringe. Using such methods, one can in fact 'prove' (spuriously) that almost any two languages share large amounts of vocabulary (as has been demonstrated in spoof papers).

In fact, in the absence of continuous textual evidence it can be established that words are **cognates** – that they descend from a common ancestor word/root in a common ancestor language, or that one of them is that common ancestor word – only if they display systematic correspondences in their phonology (the structural sound-units that make them up), repeated over large numbers of word-sets. This is because language change is largely systematic and regular, at least at the level of phonology and phonetics. Within historical linguistics, the application



of this observation to the reconstruction of unattested ancestor languages and the tracing of historical relationships and changes involving entire languages and specific cognate sets of words and other linguistic items is called the **comparative method**.

In fact, some demonstrable cognates do not even resemble each other any more, because one or more of them have undergone major, separate changes. In one extreme case, the Latin sequence *du-*, as in *duo* 'two', regularly and systematically corresponds with *erk-* in Armenian. An ancestral Proto-Indo-European sequence reconstructed as close to [du] has gradually undergone a series of major changes in Armenian, to the point where the Armenian and Latin sequences, though demonstrably (and surprisingly) cognate, are now totally dissimilar in phonetic terms.

'Fringe' writers (who typically know nothing of the requirement of systematicity) often work cheerfully with examples involving only very approximate (and unsystematic) phonetic similarities; but many of them seem to think that **very close** similarity (again involving isolated pairs or sets of words in different languages) materially strengthens their case for an unrecognised connection. In fact, however, it is actually unreasonable to expect cognate forms to show exact or near-exact similarity where a long period of time has elapsed since they had a known or reconstructed ancestor form. Even clearly related cognate forms are normally distinguished by the results of a discernible degree of (largely systematic) change and (where several descendant languages are involved) divergence, e.g. French *chien* 'dog', *pain* 'bread' versus the equivalent Italian words *cane* and *pane*. Systematically very close similarity across a range of forms suggests very recent divergence in well-documented historic times (or else a fairly unusual degree of linguistic conservatism). On the other hand, **unsystematic** close similarities between the members of isolated pairs/sets of words are likely to be accidental.

In those rare cases where very similar forms exist and are known to be genuinely related, but where the time-depth separating them is great, the very close fit between the forms is itself demonstrably accidental. For example, Romanian *nuput* 'nephew' is very similar indeed to the ancestral Proto-Indo-European word as reconstructed; but in the intervening period of several thousand years it has clearly passed through a range of other forms, notably Latin *nepot-* (dictionary form *nepos*). It has come to have a very similar form again only through new changes specific to Romanian. (This has not prevented some non-linguists from citing this very example, apparently imagining that the word remained as *nuput* for the entire period.)

Another kind of case involves 'borrowing'; these are cases where an individual word from one language is taken over by another language in a contact situation (e.g., English *restaurant*, from French). Partly because these

involve interaction between two sound-systems, the changes are less predictable here; but they are still fairly systematic, not merely haphazard, and any claim that a word has the shape it has because it has been borrowed must be supported. Such claims are obviously common where influential contact rather than common 'genetic' origin is proposed.

In deeper-time cases involving possible ancestor languages behind two or more of the recognised language families, the data are typically too scanty and too dependent upon reconstruction to permit the use of the comparative method as just outlined above. In such cases there are more overtly statistical methods which enable the researcher to assess the probability that sets of similar words with similar meanings across a wide range of languages are genuinely connected (as cognates or by borrowing), or that particular pairs or sets of known language families are ultimately related in deeper time. During the 1990s, Ringe and other linguists refined these methods, and though there is much debate about the specifics the general approach appears robust. In this context, it should be observed that there has been **no** case so far in which the comparative method cannot be applied because of the time-depth and in which these statistical methods clearly suggest a genuine relationship (unless one follows the outdated methods of Swadesh or accepts the maverick ideas of Ruhlen; see below). In the view of most analysts, even the near-mainstream theory of Nostratic (see below) does not find more than very marginal support in respect of these considerations.

The precise likelihood of accidental (unsystematic) similarity depends upon a number of factors: degrees of phonological and semantic similarity required between words, lengths of the words, the phonological and semantic systems of the relevant languages, etc.. However, in any given case it is easily shown to be much greater than most non-linguists (including 'fringe' writers) imagine. As noted earlier, if one relies on superficial isolated similarities, one can 'prove' that almost any two languages are connected; but these 'proofs' are easily overturned by careful attention to statistics and a suitable injection of linguistic sophistication.

Thus, if we ignore all these principles and constraints, we are liable to make many errors. Most obviously, we may imagine that superficially similar words are cognates when they are in fact unrelated. For instance, we may imagine that Latin *habere* and German *haben* are cognates; after all, they are very similar, they both mean 'have', and in this case we know independently that the languages themselves are related. But in fact these words are **not** cognate, they are unrelated, and their similarity is accidental. German *haben* does have a Latin cognate, but this is *capere* 'take', 'capture'; German(ic) words beginning with *h-* normally have Latin/Romance cognates with *c-*, as in *Hund* and *canis* 'dog', *hundert* and *centum*

‘hundred’, not with *h-* (because of different changes within the two language subfamilies as they diverged from Proto-Indo-European). Another example, again involving languages which themselves are known to be related, involves Spanish *mucho* (‘much’) and English *much*, which are again unconnected and only accidentally similar.

We know that these cases involve ‘false cognates’ because the forms and the entire languages are recent and well documented. Such cases show that, where we do not have adequate information about the relevant linguistic (and historical) changes (e.g. where very ancient and/or unwritten languages are involved), we cannot assume that similar forms are genuinely cognate (or related by borrowing) – unless we find systematic correspondences.

In the opposite direction, we are very likely to ignore genuine cognates which no longer resemble each other and which can be discovered, if at all, only by very careful analysis. We have already seen one dramatic case of this, involving Armenian and Latin. But there are many others. For instance, in the absence of the detailed evidence which we possess we would almost certainly ignore English *cow* and *beef*, which are demonstrably cognate (one form is Germanic, one Romance) but which have long ceased to resemble each other and share only an approximate meaning.

We must also be careful not to place too much reliance upon **approximate** similarities of meaning between superficially similar forms. Mere relatedness of meaning is no more than an indication of **possible** cognate status. Cases such as that of *habere/haben*, where the meanings are in fact virtually identical, illustrate this well. Unsupported speculation based on loose semantic links (or links which are valid only given certain contentious assumptions) is of little value here.

All this indicates that it is phonological systems which are decisive, not superficially similar words *per se*. In addition, ‘genetic’ relationships between languages are often shown quite accurately by specific grammatical similarities. But few revisionist writers know enough linguistics to deal adequately with phonology or grammar. Indeed, the vast bulk of the argumentation associated with non-standard amateur claims involves vocabulary, which is replete with superficial similarities and which requires much less understanding of linguistic theory or the techniques needed for describing and explaining linguistic systems.

It is true that some linguists on the fringes of mainstream thought have endorsed somewhat looser standards of evidence even in recent times. Swadesh and others developed the theory of ‘glottochronology’, which purported to allow reconstructions (and quite precise estimates of date) on the basis of large numbers of superficially similar potential cognates. The theory was rapidly undermined by contrary data, but Swadesh persisted, and towards the end of his life his proposals

become truly wild. Some years later Landsberg had rather similar ideas. (See below on Marr, who was even more extreme.)

More recently Ruhlen has argued that careful reconstruction is not needed in the initial task of establishing relationships and that mass comparison of such forms can take us back all the way to Proto-World. Ruhlen dates Proto-World more recently than most, implausibly indeed given accepted dates for e.g. human arrival in Australia. Many ‘fringe’ linguists could invoke Ruhlen as a supporter, but by no means all seem aware of him (displaying some writers’ utter indifference to the literature; his ideas are available in a non-specialist book). Ruhlen’s work has met with highly critical reviews and his ideas have not been accepted in the scholarly mainstream. Still more recently, McWhorter has made more modest proposals along similar lines; these too are regarded as at best dubious.

Somewhat less ‘fringe’ but still highly controversial is the work of Greenberg and the Nostraticists, who use a revived glottochronological approach and believe they can reconstruct a deep-time ancestor language, dated at around 10,000 years ago, as the common source of Indo-European, Semitic, Uralian, etc.. Most historical linguists view the Nostratic ‘paradigm’ with suspicion. Specifically, Ringe has argued statistically that an alarmingly high proportion of the Nostraticists’ ‘cognates’ could well involve chance similarity; only to a very small extent do their ideas hold up (see above). Some of the more sophisticated ‘fringe’ writers on language are aware of the Nostraticists and try to draw support from their more liberal ideas about what counts as good evidence for a claim in this area. Because the Nostraticists typically hold academic positions, ‘fringe’ writers can misconstrue (or misrepresent) their ideas as constituting the latest mainstream consensus.

Where mainstream linguists do come to comment on non-standard theories of this kind, they very largely agree that they are unconvincing for the reasons given here. But their opinions are seldom sought and, if encountered, they are often rejected.

Salient examples of the revisionist strategy are listed below (this list is far from exhaustive!). Some additional points arise here:

1. As will be seen, a number of these proposals are partly motivated by nationalistic feelings, including the conviction that one’s own language and culture are somehow pre-eminent and very ‘old’. This is especially common where the language and/or the culture in question is regarded as historically very significant or distinguished but as now lacking in power; an obvious example is Greek. It is also common in the cases of ‘genetically’ isolated and thus mysterious languages such as Basque or Burushaski (a language considered ‘genetically’ isolated is one for which no ‘genetically’ related languages are currently known). Again, it is common in the cases of languages of disputed

‘genetic’ affiliation (thus again mysterious) such as Hungarian. In some cases, the traditional religious associations of a language are also a factor (see below for examples).

2. Some of the claims discussed are even more suspect than most, in that they repeatedly fly in the faces of **known** etymologies, which are often very well supported with historical and linguistic evidence. Other claims deal mainly with the remote past where the actual etymologies are uncertain, and the point here is not that those offered are known to be wrong but that there is no particular reason to accept them.

3. In addition, the writers mostly pay no attention to the positions of the various languages in their respective families with their well-established histories. This renders many of the etymologies offered even more implausible or indeed impossible.

4. In some cases, **multiple** etymologies with different sources are posited for the very same word. For obvious reasons, such claims are most unlikely to be correct.

5. There is an important contrast between (a) proposals which involve normal unplanned linguistic change and (b) a special group of proposals which involve the deliberate concoction of known languages out of other known languages or reconstructed (or invented) languages (often by churches and other bodies with an alleged interest in deceiving humanity). For cases of type (b), the relevant statistical considerations are much more difficult, since these assume normal unplanned change; those theories, although they are typically both implausible and indemonstrable, are thus almost immune to effective disproof along these lines. (However unsystematic and/or otherwise implausible a set of changes might be, it **could** occur if it was deliberately planned as part of a project of language concoction!) By way of an additional feature, some cases of type (b) and a few of type (a) involve (re)analysis of linguistic forms (especially of the alleged ancestral forms) into monophonemic, monosyllabic or other very short morphemes.

## Examples

### Asante, Bekerie, Diop and other ‘Afrocentrists’

On these undeservedly influential writers, see my article in *The Skeptic* (Australia) 19:2. Their main linguistic ‘evidence’ involves their allegations that words (and loosely similar sounds) from Ancient Egyptian, Ge’ez and other widely-distributed and apparently unrelated African languages have common origins. The intention is to argue that all African languages are really one family, possibly descended from Ancient Egyptian. Following up on the work of nineteenth century precursors, some Afrocentrists also attribute many forms in European languages to African sources (see also below on Bernal). Winters and others go further, ‘deciphering’ the genuinely mysterious Indus Valley script as Dravidian (Southern India) and linking

Dravidian generally, Sumerian and even Chinese with African languages held to have been widely diffused by an early African diaspora. The level of the Afrocentrist writers’ knowledge of linguistics itself is often very poor indeed, and they frequently ignore or reject (nonchalantly) the results of mainstream scholarship. Bernal’s claims to the effect that Greek borrowed very heavily indeed from Egyptian as part of an Egyptian cultural ‘invasion’ of Greece are set in a more scholarly context, but these too have been generally rejected by classical scholars and Egyptologists following justifiably sharp critical reactions. Compare also Hallett on the pygmies as discussed below.

### More on Sumer

Sumer (see above) arises repeatedly in this kind of context; it is popular because it is the earliest known civilisation and because its language – which can now be read – is ‘genetically’ isolated. Sitchin and other advocates of early extraterrestrial contact have advanced novel interpretations of Sumerian to suit their theses, but there is no reason to accept these. (See below on Temple’s similar ideas.) Allegro traces important parts of Aramaic and other languages involved in New Testament studies to Sumerian, again unpersuasively. (See later on other non-standard theories involving the Bible.) Recently Rohl in turn has offered his own non-standard, unconvincingly loose version of Sumerian philology in support of aspects of his ‘New Chronology’. Yet another non-standard interpretation of Sumerian and its cuneiform script has been proposed by Linaker; this is more sophisticated than the rest but it is still very strange. Other cases involving Sumer are mentioned below.

### Fell and the ‘American Epigraphists’

Fell is the most prominent recent exponent of the hyper-diffusionist claim that transatlantic and/or transpacific voyages brought representatives of many cultures to the Americas before the firmly established Norse settlements of around ten centuries ago (and over and above the occasional return journeys by e.g. Polynesians or even Romans which are generally regarded as at least arguable). He and his many and varied associates (some of them professional academics) claim that the Norse penetrated deep into North America in mediaeval times, and, more importantly, they also identify the cultural and linguistic influence of Shang Chinese and later Chinese groups, Japanese, Indians, ‘Celts’ from Ireland or Wales, Egyptians, Phoenicians, other Semitic-speakers, etc., etc., and also that of Africans (some of the Afrocentrists are again involved here, notably Van Sertima). (There are parallels here with similar claims for Australasia; see my recent articles in the ASKE newsletter, 2004.) In most cases these claims involve the contentious decipherment of what are alleged to be inscriptions (on rocks etc.) as being in known Old World scripts (or variants thereof) and in

known Old World languages. However, many of the alleged inscriptions appear natural or else forged; and the specific scripts and linguistic forms identified are sometimes so unfamiliar that their provenance is partly a matter of speculative reconstruction.

Some of these writers go beyond epigraphics and make philological claims, suggesting e.g. that Mayan or other Amerindian languages have common ancestors with, e.g., Semitic languages. Van Sertima and others also allege that there are borrowings into Olmec, Mayan etc. from West African languages. Others have asserted that some Amerindian languages are in fact varieties of Japanese associated with permanent settlement in remote times, or at least closely associated with Japanese; see especially Davis on Zuni. (Jack found an eccentric – himself Japanese, almost inevitably – who argued on a broader front that Japanese retains many features of an *Ursprache*. In reverse, Smithana proposes that various Amerindian languages are the sources of many Japanese words, through early, unrecognised voyages between Japan and North America.) Still other claims surround allegedly mysterious scripts such as the ‘hieroglyphs’ used to write Micmac.

One of the most bizarre of the epigraphists is White, who uses the usual amateur methods to trace all languages back to an *Ursprache* called ‘Earth Mother Sacred Language’. This language had very short morphemes, many of them consisting of a single phoneme (see above on type (a) and (b) proposals; compare some of the type (b) proposals). But there are in fact solid general linguistic reasons why the morphemes of a language cannot be predominantly monophonemic. In addition this claim renders White’s proposal more difficult to refute. Very many longer words of known languages will contain each given sound, and it is not difficult to concoct accounts deriving the meanings of these words from those allegedly associated with each sound. Where the etymologies do lend themselves to serious examination, the resemblances are often very approximate indeed, and the derivations are typically far-fetched and naturally in conflict with those generally accepted.

White is supported by Covey, an archaeologically and historically trained follower of Fell who is known for his own inadequately argued historical linguistic proposals and for his advocacy of the philological views of other Fellians such as Sten and Stewart. Stewart proposes close links between the Amerindian Na-Dene languages and languages of Turkistan; she believes the relevant Amerindians migrated to North America only in mediaeval times. None of this appears to be supported by adequate evidence. The case of Covey shows how even vast sophistication in one area of scholarship is no guarantee of expertise in another, even where the fields in question are closer than in the case of Fell (who was a professional biologist).

There is a vast literature on all this. Some of it is much more scholarly than Fell’s material and a few cases could

conceivably have some validity. On the other hand, a great deal of the work is quite amateurish, and the evidence for most of these claims is flimsy to say the least. The main academic linguist associated with this tradition, Gordon, is generally agreed to have abandoned scholarly caution late in his life, as arguably have the very few other linguists who accept any of this (notably Key).

### **The ‘Saturnists’ and other neo-Velikovskians**

There is an entire small world of non-mainstream scholarship based on the works of Velikovsky. Velikovsky was a self-taught writer on astronomy, mythology and history; his main claims (some of them reminiscent of nineteenth/early-twentieth claims about Atlantis etc.; see below) involved recent major-planet catastrophes in the inner Solar System (during recorded history) and associated major revisions to the accepted chronology of the ancient Near/Middle East. The most prominent contemporary manifestation of neo-Velikovskianism is ‘Saturnism’, the view that Earth and other rocky planets orbited Saturn before catastrophic restructuring of the system a few millennia ago. The main Saturnist journal is *Aeon*, but they have their own ‘lunatic fringe’ with even wilder ideas (Holden, etc.)!

Some Saturnists, notably Talbot (the author of the key book in this tradition), place much emphasis upon the similarity and alleged common origin of words in many apparently unrelated languages, which in their view relate to myths and motifs associated with cultural ‘memories’ of the earlier configuration and the ensuing cataclysm. Most of the Saturnists understandably show little detailed knowledge of linguistics, but one of the *Aeon* committee, now deceased, was in fact a retired academic linguist (a Nostraticist), and other linguists have been involved.

### **Temple on the Dogon and similar claims about extraterrestrial contact**

See the review article by Newbrook & Groves in *The Skeptic* 19:4. Temple incorporates linguistic arguments into his theories of the early diffusion of ideas introduced by aliens from the Sirius system. (Compare Sitchin; see above.) He adopts the usual amateur approach and simply does not know enough linguistics – though he clearly thinks that he does; he advances a novel theory on a quite technical issue, displaying his utter confusion in the process!

### **Atlantis and other ‘lost’ continents/civilisations**

Many of the less scholarly advocates of Atlantis and other ‘lost’ continents or local civilisations also attempt to link various apparently unrelated languages as part of a diffusionist account of the aftermath of a catastrophe. In a few cases (e.g., that of the scientists Ryan & Pittman, advocating the possible sudden flooding of the Black Sea a few thousand years ago) there is a stronger overall case for

a catastrophe. Indeed, academic linguists helped Ryan & Pittman on the specifically linguistic side of their case; but their use of the information provided is dubious and in places simply confused. And more typically the level of linguistic scholarship applied in this kind of study is very low and the usual eighteenth century methods are adopted. However, there are more sophisticated treatments. One involves the scientist dos Santos' claim that Guanche (Canary Is.) is related to Dravidian; this claim is linked to a fairly traditional Atlantic model for Atlantis. Even this material, however, does not appear to meet the criteria used in mainstream historical linguistics. When apprised of this, dos Santos sought to defend his position. He disputes the mainstream use of statistics in this area, holding that accidental similarities are much less likely than is thought. The disagreement centres upon his handling of the linguistic data, which appears too simplistic. On Atlantis in the Atlantic, compare also Simon as discussed below.

Another case (potentially associated with dos Santos') involves Oppenheimer's respected and not implausible claims about a recently sunken continent and a lost civilisation in what became South-East Asia. The linguistic aspects of this case involve Dravidian and (again) Sumerian. But Oppenheimer's linguistics is one of his weaker points, and even his linguist ally Manansala does not appear sufficiently familiar with historical linguistics; the arguments are mostly of the usual dated/amateur kind (and some are naïvely typological in character; see later).

A rival to Atlantis is 'Atland', located in the North Sea; the history of this 'lost' land is given in the *Oera Linda Book*, a Frisian work which appears to be a fairly recent nationalistically motivated forgery. Supportive commentators on this work identify many 'cognates' involving Frisian and various languages of the ancient world.

In some cases there is inscriptional 'evidence' of the 'lost' civilisation. This sometimes appears to have been forged; e.g., the tablets found at what some still regard as a genuinely mysterious site at Glozel in France. This site was in fact very probably 'salted' for nationalistic and other reasons, and the markings on the tablets do not pattern like genuine texts in a natural language. In other cases no authoritative decipherment exists; e.g., the 'Old European Script' which Gimbutas and others associate with a 'lost' Stone Age civilisation, possibly a matriarchy; again, it is not even clear that these markings really represent a script as such. Gimbutas and followers such as Rudgley also engage in loose philology of the usual type; see the review article by Hiscock & Newbrook in *The Skeptic* 20:1.

In another vein, the Flem-Aths, who believe that Atlantis was in Antarctica and that the Atlantean refugees arrived first in South America, endorse some extreme and unjustifiable claims about the structure of the Andean language Aymara which are associated with the idea that it is of Atlantean origin.

### **Other cases involving mysterious scripts**

In *The Skeptic* 20:3 I comment on the various 'decipherments' of the mysterious but surely linguistic material on the famous Phaistos Disk of Crete (one of which came from Fell; he read the disk in a mixture of Anatolian and Polynesian!). Note also the undeciphered Indus Valley script (the subject of a vast literature but of no accepted interpretation; the unidentified language represented could be Indo-European, Dravidian or something else again), Linear A, the Eastern Island tablets, the Voynich Manuscript, etc..

### **Hallet on the pygmies**

One of the theories of Hallet, the maverick Belgian explorer of Africa, was that the Ituri pygmies of the Congo region, notably the speakers of Efe, represent the original human population. This is reflected in their lore, which includes all the basic motifs of myth and religion, and in the Efe language, from which large elements of Egyptian, Hebrew and many Indo-European languages can be derived. The methodology is again the same: comparison of isolated forms which are superficially similar. (Others, such as King, have made other wild claims about the pygmies.)

### **Relocation of traditional stories**

Some writers have claimed that the scene of key events in ancient history or legend was not in fact the obvious location as normally interpreted but some other quite distant location. For instance, Wilkens, Mertz and others have sought to relocate the events of the Trojan War in the Atlantic. Some of these writers again invoke linguistic evidence. For instance, Wilkens thinks that the main action involved the Netherlands (he is Dutch) and the British Isles, and equates many of the relevant Greek, Dutch and British place-names.

In a similar vein, Salibi, Daunt and others made such claims regarding the central narratives of the Old Testament. Salibi placed the Biblical events in Arabia (he is an Arab), while Daunt located them further east, especially in India, equating Biblical characters with figures from the history and myth of that region. He supported these claims with linguistic equations of the usual kind, involving superficial similarities between isolated words. Another maverick scholar, Beaumont, actually argued that the events of the Old Testament occurred mainly in Scotland.

Another set of claims in this vein involves Jesus, who is said to have spent some of his 'missing' years outside Palestine in various countries and/or to have survived his crucifixion and then to have relocated to Kashmir or Japan, eventually dying there. There is a linguistic aspect to the version involving Japan, centring on a temple chant at Herai in northern Honshu where the 'Grave of Jesus' is exhibited. Bergman claims that this chant is in fact in Hebrew, modified to fit Japanese phonology. It is also

claimed that a document dating from around 100 CE and written in the kana syllabary (several hundred years before kana are known to have existed) exists in the area; this text allegedly shows that Jesus is indeed buried in Herai, and contains his will. But Bergman's reading of the chant can be made to seem **plausible** only by very special pleading. In 20 minutes I devised a Latin reading which is closer to the Japanese phonetics than Bergman's Hebrew is and also fits the situation better ('Dark Age' missionaries in Japan). The most plausible analysis is still that this is a normal Japanese folk-chant with some sequences that display accidental rather approximate similarities to Hebrew words. And the key document is probably a modern forgery.

### More on Hebrew

There are, of course, fundamentalist Christians and Jews who accept the literal truth of *Genesis* – including the story of the Tower of Babel – and the rest of the Old Testament. Surprisingly, some of these modern believers in the Tower of Babel are trained in linguistics. Around the world there are various branches of the Summer Institute of Linguistics (SIL). SIL trains linguists in fieldwork methods, so that they can analyse unwritten languages around the world, develop writing systems, prepare dictionaries and grammars – and translate the Bible into each such language, for this otherwise worthy enterprise is linked with Wycliffe Bible Translators, an arm of fundamentalist Christianity! Indeed, some of its qualified linguists and instructors are creationists. One of these linguists is May, who published a piece in *Creation Ex Nihilo* which essentially upholds the Babel story. For a qualified linguist, May is remarkably ill-informed on historical linguistics and his summaries of orthodox views are wildly outdated. (This is of course often true of scientifically trained creationists.) Naturally, there are also rank amateurs with similar views.

At a time when *Genesis* was generally interpreted as historical (which was before the development of historical linguistics), it was often assumed that the single pre-Babel language was Hebrew, the language of the Pentateuch. This idea is in fact far from dead. One current manifestation of it is the work of the Jewish creationist writer Mozeson. Mozeson claims that virtually all the words of all languages derive from 'Edenic', which is basically early Hebrew with some (Proto-)Semitic roots not attested in Hebrew itself. (Hebrew is a member of the Semitic language family.) But Mozeson's proposal again involves the usual outdated methods of comparative linguistics. In fact, it is clear from a range of major errors that he simply does not understand historical linguistics.

Another writer who regards Hebrew as close to the *Ursprache* is one 'Dave', who runs various groups on Yahoo. On the basis of some reading in the discipline (not always with full comprehension), he espouses an array of implausible and at times self-confounding non-standard ideas about historical linguistics. Some of these involve

Biblical Hebrew and Ancient Egyptian. According to 'Dave', both languages have been badly misanalysed by mainstream scholars: he rejects the accepted decipherment of Egyptian, and he believes that later scholars have failed to recognise major changes in the use of Hebrew script and have thereby missed major changes in the language itself. In fact, he says, the two languages were much closer than is normally thought, and Biblical Hebrew in particular was close to a rather recent *Ursprache* (compare Swadesh etc. as discussed above). In addition, Hebrew had monophonemic morphemes, as per the rider to my discussion above of type (a) and (b) proposals (compare White), although 'Dave' believes that this has been missed by way of error rather than conspiracy. His arguments are, however, confused and weak.

Yet another such writer, Benner, argues (implausibly) that Hebrew script, which was clearly partly pictographic in origin, kept its pictographic function even after it became alphabetic and that the Hebrew language and its script must have appeared simultaneously when God created Adam with a mature knowledge of the spoken and written language (another creationist). Another fairly similar project is that of 'Britam', a British-Israelite-like group led by Davidy; but this group (naturally) focuses on alleged linguistic parallels between Hebrew and the Celtic languages specifically. The parallels presented again lack conviction, for the same reasons and also because of reliance on outdated sources. In the same way, the British Israelites proper implausibly proclaim linguistic connections between Hebrew on the one hand, and both English and Welsh on the other. British Israelite philology is especially bizarre. (In recent times, the professional linguist Vennemann and some relatively well-informed amateurs have actually argued seriously for early Semitic influence on Celtic, with better evidence; but even Vennemann's case is regarded as dubious.)

Another, perhaps more arguable proposal is that of Blodgett, a lecturer on German and Hebrew, who argues that Hebrew exerted major influence on Germanic in antiquity through the dispersion of the 'Lost Tribes' of Israel into central Europe. He knows some historical linguistics (although there are several undergraduate-level errors which are quite damaging). But even his case for this relatively modest revision of history is simply not strong enough, in linguistic terms at any rate.

There is a long non-mainstream tradition of re-interpreting the events related in *Exodus*. Notably, Akhenaten and Moses are often linked or even identified as the same person or as closely related. Akhenaten's dynastic (non-immediate) successor Tutankhamun is also involved. The main relevant work with a linguistic element is by Sabbah & Sabbah, who argue that the 'chosen people' were in fact the Egyptians, who were conquered by the Hebrews and suffered under the re-writing of history by the victors (a common tale and not an untrue one, albeit overused by

postmodernists and fringers). In this version, Moses was not Akhenaten but another pharaoh, Ramesses I (while Akhenaten was Abraham!). Predictably, there are many problems here; but the linguistics is especially weak. The Sabbahs write as if the origin and development of the Hebrew abjad (consonantal alphabet) and other related Semitic scripts were only very sketchily known, with large gaps waiting to be filled by researchers such as them. They derive the abjad from key parts of hieroglyphs, each retaining much of its Egyptian pictographic significance (compare Benner above).

However, by dynastic times the relevant Egyptian hieroglyphs had already lost this significance. The script was predominantly phonological; that is, each symbol, even if pictorial, now usually represented a consonant or longer sequence of phonemes, regardless of meaning. And the Semitic abjad scripts are undoubtedly closely connected with each other. There may well be an older Egyptian source for some of the Semitic letters; but, if there is, it involves this whole family of scripts, not just Hebrew. In fact, the evidence for the specific connections proposed – even where the words themselves are or may be related – is mostly impressionistic. Many cases involve special pleading or outright contrivance. It is easy to find accidental parallels between Hebrew letters and hieroglyph-parts.

Another issue in this area is that of Latter-Day Saints sources which argue for the presence of languages and scripts used in ancient Israel in ‘inscriptions’ found in the Americas and seek to relate known languages of the Americas to Hebrew (e.g. Harris’ works).

#### **Hindu creationist linguistics (Sanskrit, Indus Valley Script, etc.)**

See my recent piece in the ASKE newsletter (2004) on this, the most salient aspect of ‘fringe’ linguistics involving India.

#### **Other claims regarding Sanskrit, etc.**

See my recent article in the ASKE newsletter (2004) for discussion of Tregear, who traced Maori/Polynesian to Sanskrit, and of other non-standard diffusionist claims regarding New Zealand. There are also some such claims regarding Australia, some of which I have again discussed in the newsletter (but note also e.g. Gerritsen on alleged early modern links between Dutch and Aboriginal languages).

#### **Basque, Etruscan etc.**

Alonso and others have claimed that the contemporary ‘genetically’ isolated language Basque is related to an ancient, poorly understood and ‘genetically’ isolated language, Etruscan. This is in no way impossible, but the evidence offered here is at the usual inadequate level. (Compare earlier twentieth century attempts to relate

Basque to the Cretan Linear scripts; see my paper on the Phaistos Disk for details.) Associated with these ideas are attempts by Khvevelidze and others to link Basque with Caucasian, a language group which shares some general typological features with Basque but is not otherwise similar to it. (It is a common error of more sophisticated amateurs to imagine that if two languages are typologically similar – e.g., both highly inflected, both having mainly monosyllabic words – they are very probably related, even if there are no clear cognates etc. At best this is one piece of evidence that they **might** be, unless the number of such features is very high indeed. Manansala and Covey are among the writers referred to above who have also made this mistake.)

There are also claims about relationships between Etruscan and Caucasian, e.g. Celayin’s attempt to link Laz with Etruscan (as well as with Greek). In addition, there are various authors with theories of other links involving Etruscan, including dos Santos again and also South American writers positing dramatic links with Aymara.

Going much further than Alonso or Khvevelidze, Nyland – who is himself Dutch rather than Basque – argues that almost all forms of almost all languages are the result of deliberate manipulation and concoction involving the distortion of Basque forms along syllabic lines. The culprit was the Roman Catholic Church in the ‘Dark Ages’ and mediaeval times; it was so diligent and successful that it not only concocted Latin, Italian, French, English and many other complex languages (including features of linguistic interest not even discovered until much later!) but also persuaded entire populations to use these languages instead of Basque **and** concealed the existence of this vast conspiracy until Nyland discovered it in the 1980s! This is one of the best examples of a type (b) proposal. Nyland knows some linguistics but misinterprets some of the mainstream ideas to which he refers. So far he has given no adequate grounds for taking his ideas seriously. Subject to the constraints involved in assessing type (b) claims (see above), it appears that the likelihood of the identifications he proposes arising by accident is far too high for these identifications to be accepted – even if the proposal were not implausible on other grounds.

#### **Maxwell and associates on language and religion**

In a somewhat similar vein, Maxwell and his colleagues are inspired by the late nineteenth century diffusionist writer Massey, who believed he could trace all religions back to a small number of linked cults (stellar, lunar, solar). Massey merged the genuine knowledge that was emerging from Egypt with the early modern fantasies – now largely debunked – about Egyptian mystery religions. Maxwell *et al.* focus mainly on the religious issues in the usual historical revisionist manner, finding a huge number of possible links but arguing persuasively for very few. However, they also present linguistic ideas taken from a

three-volume work published around 1940, apparently anonymously (adding more examples of their own). This book has the overall title *Priesthood of the Ills* and contains a large amount of non-standard philology, adduced as support for these diffusionist theories of religion. The author believes that there is a Language Conspiracy, which involves (a) keeping humanity divided by enforcing the use of many mutually unintelligible languages and (b) blocking humanity from discovering the original ('true') meanings of words. This suggests that all changes in the meanings of words are illegitimate, which of course is nonsense. However, the author believes that the meanings of some of the key words in ancient languages were very different indeed from those of the English words normally used to translate them, and that this has been deliberately concealed by the forces of Evil. These 'true' meanings are implicated in huge numbers of unrecognised links between languages. These writers suggest that simply focusing on pronunciation rather than spelling will enable one to **hear** which words are **really** connected, because they **sound** similar! Once again, the last 200 years of historical linguistic scholarship is simply ignored. (The purported ancestor language implied here is not identified.)

#### **Korean and Japanese**

There is a cluster of non-standard claims regarding Korean. For example, Park (himself Korean) regards Japanese as a 'distorted' dialect of Korean. There may be a political dimension to this case, given the recent history of the two countries (compare Kaya on Greek, discussed below). Both Japanese and Korean are in fact regarded as 'genetically' isolated, but it is possible (not necessarily probable) that they are related in deep time. (Elsewhere Park displays utter ignorance/confusion about some mainstream philological claims, which he misinterprets badly; he attacks some ludicrous 'straw men'.)

#### **(Modern) English as a recent common ancestor language!**

Harper's book *The History Of Britain Revealed* presents the astounding view that Modern English, while related to Old English, is not descended from it (and that Middle English therefore did not exist except as a highly artificial literary variety); that Modern English has existed since ancient times, when it was current across Western Europe, and is indeed the ancestor of most modern western European languages, including the Romance languages; that Latin was therefore not the ancestor of these languages and was in fact invented; and that the vast majority of etymologies given for English words are therefore wrong. See the review of this book by Thomason and me in the present issue.

#### **Greek as the ancestor language**

Yahuda, supported by Efstathios-Georganas and others, believes that Ancient Greek was the *Ursprache* (and upholds other extreme views about Greek). Initially his claim was that Hebrew specifically is disguised Greek, most words being composed of one or more distorted Greek roots. This was later extended to advocacy of Greek as the oldest language of all and denial of the existence of Proto-Indo-European as an ancestor for Greek and other languages. In addition, examples of early pre-linguistic symbolisation from the Aegean area are (not for the first time) reinterpreted as involving the Greek alphabet. The undoubted Phoenician origin of this alphabet is also denied and it is wrongly stated that it is derived from the syllabic Linear B script used to write earlier Greek. Even where these claims are not actually refuted by evidence, the 'evidence' in their favour is of the usual inadequate kind. (Compare Kaya's much more negative view of Greek, as discussed below.)

#### **Hungarian as the ancestor language**

Simon, a Hungarian author, argues for a wide-reaching version of the Atlantis story (see above) incorporating Noah's Ark and tracing as much as possible back to a Hungarian-using civilisation in the remote past which was linked culturally and linguistically with Sumer and other civilisations in both the Old World and the New. The linguistics does not loom so large here, but where it does appear it is on much the same level as elsewhere. Simon also works on his own computerised lexically-based dialect atlas of Hungary and does seem to know something of the more traditional branches of the subject, but even here he often displays confusion and the appearance of deliberate dissidence. His work links in with that of the American epigraphists; along with some other Hungarian enthusiasts, he accepts a Hungarian version of the Norse 'Vinland Map'. Most scholars consider that the Hungarian map is probably a recently forged special version of this map (which itself may very well be a forgery).

#### **Hungarian as close to the ancestor language**

Vomos-Toth, a second Hungarian writer, has developed a rival view of Hungarian as close to the ultimate ancestor language. Drawing off Lahovary and others, as well as his own investigations, he believes that Hungarian retains many features of a language called Tamana used universally before a catastrophe several thousand years ago (compare the material on Atlantis and catastrophism as rehearsed above); cognates also appear in Dravidian, Sumerian and African languages. Endrey also links Hungarian and Sumerian. But these authors' methodology is again on the same level.



**Latvian as the ancestor language**

Kaulins, a Latvian author, has been claiming since 1977 that Latvian is the oldest known language (and has therefore been remarkably static over a long period). He supports his claims with analyses of cultural manifestations and of blood-group distribution (there is of course a serious tradition of work in this latter area, which has produced some very thought-provoking results but has not overthrown any important linguistic arguments). However, Kaulins' main evidence is, naturally, linguistic. Unlike most writers discussed here, he knows enough linguistics to recognise his situation with respect to the mainstream, and he thus explicitly rejects rather than ignores contemporary ideas on the adequacy of evidence. His position thus resembles that of e.g. Ruhlen, although his reasons for adopting it are not entirely clear to me. On this basis, Kaulins identifies many words in Ancient Egyptian, Greek, even Sumerian, etc. as 'corruptions' or (later) cognates of Latvian words. Naturally, he also rejects the mainstream view that Latvian has a mixed structure because of influential contact with Finno-Ugric and is the *least* conservative of the Baltic languages.

**Turkish as the ancestor language**

In the 1920s, the new republican regime in Turkey tried to persuade Turks that their language was the ancestor of all human languages. This was partly a political move, made with a view to persuading conservative Turks to accept borrowed words for innovations (if all words were originally Turkish, it was surely legitimate for Turkish to 'reclaim' them); but nationalistic ideas were again a factor here. The linguistic evidence is of the usual kind. In a contemporary version of this viewpoint, Kaya and others have revived the idea that all other languages are derived from Turkish, in this case largely by way of a type (b) style conspiracy involving considerable amounts of deliberate concoction and wholesale misrepresentation of the alleged truth. This is similar to Nyland's proposal, but Kaya knows much less linguistics and his theory is even weaker than Nyland's. At one point the two were actually arguing on a web site, each accusing the other of ignoring the obvious truth that their own favoured language was in fact derived from the other's! Kaya is especially concerned to detract from the status of Greek; as with Park on Japanese, there may very well be a political dimension to this. A similar conversation between Kaya and Yahuda would be of great interest! However, Kaya knows far too little about Greek for his purposes, adopting ludicrously wrong and naïve alternative interpretations even of the Greek alphabet where it suits his case to do so.

**Mayan, Greek and Aramaic**

Churchward (proponent of the lost continent of Mu, supposedly revealed in the 'Naacal Tablets') claimed that the Greek alphabet, as normally recited, is really a poem in

Mayan! Of course, Le Plongeon claimed a century ago that Jesus spoke Mayan on his cross, not Aramaic, and Mayan and the Maya are still very popular among 'fringe' thinkers. Leaf in New Zealand traces all languages to Mayan, which she believes was used in Mu.

**Marr**

Marr was a Soviet-era linguist whose Marxist-based ideas about language change became more and more 'fringe' in nature but were endorsed by Stalin, and this protected him from criticism (a linguistic Lysenko). Eventually he came to hold (on less than persuasive grounds of the usual kind) that **all** words in **all** languages derived ultimately from the four syllables *sal*, *ber*, *yon* and *rosh* in combination. After his death his ideas continued in favour in the USSR for 16 years until Stalin himself finally pointed out some unrelated inconsistencies in his theories.

**Ior Bock**

A more down-to-earth version of Marr is Ior Bock the Finnish sperm-drinker, who manages the 'Lemminkainen Temple' near Helsinki and claims that his family possesses a tradition of 'the oldest language in the world', known as Rot (pronounced like English *root*). This unwritten and allegedly unwritable language (naturally spoken in ancient Finland!) is based on a 'ring' of 23 'sounds' (mainly syllables), each with a specific meaning, which combined in many different ways in the remote past to form a large proportion of the other human languages. This theory thus also resembles White's in general terms. But again the resemblances are often very approximate indeed, and the derivations are typically far-fetched and naturally in conflict with those generally accepted.

In a series of bizarre twists (and, it must be said, without much clarity), Ior Bock and his followers suggest (a) that Rot is in fact identical with Finnish Swedish, the dialect of Swedish used by the ethnically Swedish minority in Finland (which is, obviously, routinely written); (b) that another such language 'Van' (which is in fact Finnish) is derived from Rot; and (c) that all other languages not directly derived from Rot are derived from Van. These claims imply – very implausibly – that both Finnish Swedish and Finnish have covert structures which are much more important than their known structures as analysed by linguists, language teachers and language-minded users; that they are much more seriously associated than is believed; and that Finnish Swedish is much older than accepted evidence suggests.

This theory has type (b) elements, but, while the above 'facts' about language origins are said to have been deliberately obscured (mainly by 'the churches'), there is no clear suggestion that the relevant linguistic forms have themselves been manipulated.

### Other theories involving very short morphemes (including type (b) theories)

A claim promoted by Alferink involves new etymologies for modern words such as *Australia*, which are held to be constructed out of basic, allegedly ancient syllables or other short sequences; these have somehow retained fixed meanings (obscure to modern scholars and the general public) over long periods. Yet another, somewhat ludicrous proposal is that of Hietbrink (Dutch), who believes that many words and phrases in a variety of languages are ‘corruptions’ of meaningful sequences of mostly very short words of (Modern) Dutch! Shaver’s ‘Mantong’, published by Palmer in *Amazing Stories* as part of the 1940s ‘dero’ cycle, was again rather similar, although here again the original language was regarded as mysterious. A slightly (but only slightly!) more sophisticated version of this kind of idea was developed by Cohane in his 1969 book *The Key*. This work focuses on Ireland and Gaelic, and also involves a great deal of very loose philology of the more usual type as discussed in earlier sections of this paper. The language reconstructed here is called ‘Atlantean’ and again has very short morphemes. (There has in fact been a string of works, notably those of Quinn, promoting Ireland as a major unrecognised centre of early civilisation or the remains of Atlantis; Quinn’s material is relatively sober but still clearly dubious in respect of the linguistics.) Among these writers listed here, Shaver and Palmer especially believed in conspiracy along type (b) lines, aimed at concealing the truth.

### Conclusions

It will be seen from the above that the general nature of the main problem with the linguistic aspects of most of these theories/claims is very much the same. The authors, relying largely on ‘common sense’ examination of superficial similarities and knowing little or nothing of historical linguistics itself, are ‘stuck’ in the eighteenth century; they are not even failing to re-invent the ‘wheel’ of careful comparative reconstruction, because they have not seen that this ‘wheel’ is necessary, and because the ‘easy’ method of relying on superficial similarities can readily be applied in such a way as to ‘support’ their nationalistic ideas or their revisionist histories. Being isolated, private workers or small groups of the like-minded, each with a conviction that they alone are right, they do not talk to each other, and so they do not observe that the same unreliable methods ‘work’ more or less equally well for all of their mutually contradictory claims. (If they do ever talk to each other, the discussion usually descends rapidly into mutual

vituperation, as noted in the case of Nyland and Kaya.) One can persuade oneself, using such methods, that **any** two languages are related; as noted, linguists faced with such ideas have occasionally done just this (e.g., for Mayan and English), as a tour-de-force. Even when linguists do make a supportive contribution, they are mainly those who are themselves on the ‘fringe’ of academic scholarship; if they were not, they would scarcely be involved in such ideas.

But in some areas there is hope! I referred above to the occasional involvement of mainstream linguists in commenting on such views; and I myself have been used by the Saturnists as a consultant! They know very well what my own views are, and they have their own ‘pet’ linguists already; but unlike many amateurs they do seem to have some respect for my expertise and they say they intend to try to take my criticisms on board. Of course, I will not induce them to abandon Saturnism; the linguistic nonsense (for so it is) is only a small part of their system of ideas. But perhaps, with my (to them, novel) criticisms, I will be able to show them why their method of finding linguistic connections around the world is as dangerous as it is; and just maybe, if they fail to defend this method even to their own satisfaction, they **may** even give it up and rely only on non-linguistic evidence. I have now indeed managed to exert similar influence on Seath, who was already the most moderate of the New Zealand diffusionists.

Where such success is obtained, specialists in other disciplines can then chip away further at the non-linguistic aspects of each case, if so motivated. In the meantime, non-linguists who are inclined to become followers of any of these claims can be given authoritative information which should deter them from accepting the linguistic arguments, specifically, as valid. And at the very least I myself am continuing to learn more about these dark outer regions of the world of linguistics.

### Notes

1. An earlier and shorter version of this article appeared as ‘Linguistic reconstruction and revisionist accounts of ancient history’ in: *The Skeptic* (Australia) **20:2** (2000), pp 42-47.
2. References for any specific section will be provided on request. Please contract me at [mnewbroo@aol.com](mailto:mnewbroo@aol.com).

## BOOK REVIEW

### *The History of England Revealed*

by M.J. Harper, London: Nathan Carmody, 2002. Pp. 141. £20.00.

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#### Reviewed by Mark Newbrook and Sarah Thomason

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In this curious little book Harper proposes a radically revisionist view of the history of the modern English language, continuing his record of promoting dramatically nonstandard historical theories. Here he argues that Modern English, while related to Old English, is not descended from it (and that Middle English never existed, except as a highly artificial literary variety). Modern English, according to Harper, has been in existence since ancient times, and is in fact the ancestor of most modern western European languages. On page 134 he presents a family tree in which English, at the apex (or root), splits on the one hand into French and thence into Provençal, Catalan, Spanish, Portuguese, Italian, and (in parentheses) Latin, and on the other hand into German, from which Anglo-Saxon springs. In Harper's schema, Latin was thus not the ancestor of the Romance languages, but was instead an invented language. A further upshot of all this is, as he himself emphasises, that the vast majority of etymologies traditionally given for English words are wrong.

His book thus challenges all scholarly opinion on the subject. But it does not fulfill the standard obligations of scholarship: there is no scholarly apparatus of any kind. For instance, and perhaps most strikingly, there are no references to the scholarly literature in the book, and opposing views and scholars are mentioned only to be dismissed with often facetious contempt. A typical example is his description of historians' professional behavior (page 7): 'These strategies are wholly successful in preserving academic disciplines as cosy niches for clever but intellectually uninquiring people.' Another typical example: [The Scots are] 'especially enthral [sic] to academic paradigms' (page 19).

Harper's evidence and argumentation in support of his views are mixed in type. One major argument involves critiques of evolutionary biology (which we shall not treat here), but the bulk of his material is either historical-cum-archaeological or linguistic. Harper writes as if he is an authority in these areas, and suggests that his novel ideas

have been culpably ignored by mainstream scholarship. He does seem to have some specialist knowledge of history – as far as we can tell, given that we are not professional historians – but on the evidence before us his knowledge of linguistics is definitely not adequate for the task he undertakes here. He is out of his depth in both factual and theoretical linguistic matters. In this review we will focus exclusively on his linguistic arguments; this focus should not be taken as an implicit endorsement of his arguments in history and related domains.

It is impossible, in a brief review, to do more than convey a general sense of the idiosyncratic nature of the proposals in the book. Like many amateur critics of the scholarly mainstream, Harper repeatedly seizes on individual 'anomalies' as weapons with which to belabour scholarship. Some of these are spurious or at the very least exaggerated. Others are genuine, and therefore require study, but for the most part these are already (contrary to what he suggests) very familiar to linguists and indeed the subject of intense study.

One good example is the apparently rapid series of changes which distinguish Middle English from Old English. The genuinely rapid lexical changes can be attributed to the flood of French loanwords that entered English after the Norman Conquest of 1066; but a major reason for the grammatical differences lies in the fact that literary Middle English was based on a midland dialect, while literary Old English was almost entirely based on a southern dialect. The two dialects were already divergent before the Norman Conquest, and many changes that affected midland dialects did not take place in southern dialects; there is no evidence that the changes in the midland dialects were any more rapid than any other linguistic changes.

This particular case also illustrates the general point that, like many non-linguists who venture into the discipline, Harper grasps issues involving vocabulary much more readily than structural issues involving

phonology (pronunciation) and grammar. He never comes to grip with the former of these two levels of analysis, and his treatment of the latter incorporates some of his more obvious errors.

Because of his limited knowledge of linguistics, including the crucially relevant historical and social branches of linguistics, Harper makes sweeping over-generalisations about what scenarios and changes are or are not plausible. The case for the mainstream account of the history of English is much stronger than Harper thinks, and the alleged anomalies much less damaging. And, even if Harper were correct in his arguments against the standard view, he does not give us sufficient reason to accept his alternative story.

One of the major difficulties is Harper's idea that two diachronically related languages could equally well be related in either order. This is simply false: it is easy to show, by demonstrable and largely predictable cross-linguistic evidence on the nature of linguistic change, that (for instance) Italian is descended from (Vulgar) Latin rather than vice versa.

Romance is also the locus of one of Harper's most telling errors of fact. He argues correctly that it would be strange if a whole raft of identical grammatical changes were to occur independently in languages which are descended from a common ancestor but which are not currently in contact. Under such circumstances, some identical and numerous similar changes would actually be expected, thanks to shared structural pressures among the related languages, but we would not expect globally identical changes. He uses this point to attack the standard model of Romance. But in fact most of the features that distinguish early Romance from Classical Latin were already found in Vulgar Latin, among them the reduction of the case system and the collapse of the neuter gender. There is no mystery here, contrary to Harper's impression.

Harper has a weak understanding of language history and language contact, including language replacements. We will give a few examples. On page 8 he asserts that, on the mainstream view, 'the Anglo-Saxons were [supplanted] by the Normans in the eleventh century'. Not so: there were perhaps 20,000 Norman French speakers versus about 1.5-2 million English speakers (Thomason & Kaufman, 1988:268), and there is contemporary evidence that many or most of the Normans were bilingual in French and English within a generation or two after the Conquest (Mellinkoff, 1963:68). The Normans did not supplant the English, except in government, and they did not suppress the English language. On page 9: "Persuading" the natives to speak the invaders' language normally happens when the invaders are culturally in advance of the natives...'. This too is false. The Sumerians all shifted to the language of invaders who did not possess the glory of inventing writing; almost all of their Akkadian successors shifted to the language of their successors, the Egyptians all shifted

from Egyptian to Arabic; almost all the Greeks in Turkey shifted to Turkish within a few centuries of the Turkish invasion of Asia Minor; and so forth. Military superiority is not always accompanied by cultural superiority.

Harper's lack of knowledge of linguistic issues is most revealingly indicated by two general claims about language change (page 30):

*The languages we speak today, and can study in detail, have not been written down for very long and therefore cannot be studied in much historical depth...We know almost nothing about how unwritten languages change over time.*

Part of the problem here is factual: the Indo-European language family has in fact been written for over 3,000 years (both Greek and Hittite are attested in the second millennium BCE), so Greek is one language spoken today that has quite a long written history.

In fact, Harper argues elsewhere that, once established, written languages actually change very little over time in any case, and that no case is known – as opposed to hypothesised – of one such language developing into another. Here he ignores the fact that the concept of 'Language X being descended from Language Y' is really only the concept of 'Language Y having changed' writ large. Sudden large sets of changes are rare, but after a while the accumulated changes are sufficiently large and numerous for a new identity to emerge (especially where the original language diversifies markedly, as in the case of Romance). And, contrary to his claims, in very many cases change within a language – written as well as spoken – can indeed be observed in the data and can be systematically analysed and described. See for instance the case of Greek, mentioned just above. (Nowadays, ongoing changes can actually be tracked in real time, by repeating sociolinguistically-informed surveys of spoken or written usage at suitable intervals.)

Actually, given that Harper does accept the notion of two or more languages being 'genetically' related, he is in fact committed to accepting that one language **can** change enough, given enough time, to be regarded as now being another language. The former implies the latter.

Further examples of Harper's factual errors include the assertion that 'languages persist with quite extraordinary tenacity so that even today, in the face of the fiercest cultural pressure from the "majors", quite tiny language groups hang around and even modestly flourish' (page 23). This confident statement will surprise experts on language endangerment, who know that minority languages all over the world have been vanishing at such a horrific rate that even conservative estimates predict the demise of 50% of the world's 6,000 or so languages by the end of this century.

On Harper's claim that Language A cannot be 'grammatically and syntactically distant from Language B and yet share a vocabulary with it' (page 92): yes, it can. To give just one of many possible examples, Tok Pisin, in origin a pidgin language and now one of the official national languages of Papua New Guinea, shares almost its entire vocabulary with English, but its grammar is wildly different from English grammar.

On the question of loanwords in English (page 95): the vast majority of them cluster in the non-basic vocabulary; the basic vocabulary contains only about 7% loanwords, some from French and some from Old Norse.

Much of the difficulty with Harper's claims, however, is conceptual rather than merely factual. It is true that historical linguistics is an arcane field that is not easily accessible to non-specialists, but even a modest amount of research should have disabused Harper of some of his notions (see any standard textbook account, e.g. Campbell, 1998). For instance, returning to his two erroneous general claims about language change: historical linguistics, in roughly its modern form, was developed in the nineteenth century. Darwin drew on our methods and results in *The Descent of Man*, and the anthropologists' cladistic approach is again based on our methods. The comparative method used by historical linguists is powerful and reliable, as shown by tests of various kinds, and it applies equally well to written and unwritten languages. Using this and other extremely successful methods, historical linguists have established dozens of language families all over the world, reconstructed sizable chunks of undocumented parent languages, and developed detailed accounts of enormous numbers of linguistic changes (including changes involving languages in contact), with results that extend back in time to at least 6,000 years BP.

There are also problems with Harper's reasoning. For instance, on the relationship between the origin of a language and its first date of attestation (page 92): the fact that Latin is not recorded until the first millennium BCE does not mean that it did not exist until then, any more than the fact that Navajo was not written until after European

contact means that the language itself sprang into existence at the moment Europeans discovered it.

There are many other errors in Harper's book, both major and minor. He attacks straw men, and throughout the book he ignores scholarly traditions, for instance the entire body of linguistic work on Old and Middle English. Elsewhere, apparently randomly, he assumes the validity of outdated positions, and he refers to contentious nonstandard accounts of the past as if they were facts.

Harper posts frequently about these and other partly linguistic issues to historically-oriented web discussion groups, and these posts reveal that his ideas are highly dubious on a broader front. He has more publications planned, dealing with the wider history of languages; but on the evidence before us these are likely to be vitiated by similar errors.

Most historical linguists who are not active skeptics will probably not hear of Harper's work, and if they do hear of it, they will not think it worthy of a response. To earn a hearing from experts on the history of English, Harper would need to offer much better reasons to accept his linguistic case and also take the counter-evidence to his proposals seriously enough to address it in a more scholarly manner.

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## SPECIAL FEATURES by 'Skepticus'

### Solution to the Crossword in the 2003 Skeptical Intelligencer

Many apologies for the mistake in the clue to 8 down which referred to the wrong clue and should have been 'Conditions in which **11** is not relevant?' Two completed puzzles were returned and both were correct. In view of the above mistake **both** competitors are awarded the prize, namely free ASKE membership for 2005.

The winners are David McKeegan and Carol Bedwell.

#### Solution

**Across:** 1 Bach; 3 greengages; 9 biologic; 10 chilli; 13 evens; 14 grandiose; 15 pouring down; 18 precedented; 20 scavenger; 22 torso; 23 artist; 25 quackery; 26 homeopathy; 27 stay

**Down:** 1 Bible code; 2 close up; 4 ruing; 5. NYC; 6. galloon; 7 spire; 8 moistureless; 11 hydrostatics; 12 tangle; 16 ice age; 17 androgyny; 18 phantom; 19 darkest; 20 smash; 21 rough; 24 tip

### VISIONS OF THE YEAR 2005

The year 2004 once more revealed the unmatched prescient abilities of *The Grand Oracle of the Pentacles* (see last year's *Skeptical Intelligencer*). Unfortunately, owing to unforeseen circumstances, *The Grand Oracle* has been unable to provide ASKE with her prophecies for 2005. *The Grand Oracle* wishes to convey her deepest regrets to her many followers who have been eagerly waiting her pronouncements and who will no doubt be disappointed not to have the benefit of these during 2005. Following receipt of this news the Editor immediately emailed ASKE members to encourage them to provide their own prophecies for 2005 and his office has been inundated by one reply. Below are the visions of ASKE member Adrian Shaw, to whom we express our gratitude and whose courage we admire in standing in for *The Grand Oracle*.

There will be large-scale power-cuts in the UK during Jan & Feb 2005.

There will be a major maritime disaster in the spring.

May 2005 - Tony Blair wins an unprecedented 3rd term as PM for the Labour party.

There will be a terrorist attack on French soil during 2005.

A major company in the Financial Sector will collapse during 2005; huge repercussions in the City.

A senior member of the Royal Family will pass away.

The Queen gives her blessing to Prince Charles's marrying Camilla Parker Bowles to pave the way for her abdication.

A high profile Russian industrialist will be subject of a criminal investigation.

Paula Radcliffe at last wins a gold medal in a major athletics championship.

Proof of life on Mars is discovered.

The Grand National is won by a rank outsider.

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