

The Historiography of Archaeology and Canon Greenwell

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In this paper I will focus the bulk of my remarks on setting studies of Canon Greenwell in two broader contexts. The first of these comprises the general issues raised by research into the historiography of archaeology, which I will exemplify through reference to research and writing I have been doing on a new book *A History of Prehistoric Archaeology in England*, and a new single-volume history of archaeology *Milestones in Archaeology*, which is due to be completed this year. The second, somewhat narrower context, has to do with situating Greenwell within the discourse of mid-to-late 19th century race theory, an aspect of the history of archaeology that has yet to attract the attention it deserves from archaeologists and historians of anthropology (but see e.g. Morse 2005). Discussing both of these broader contexts will, I hope, help us address and answer questions about the value of the history of archaeology (and of research into the histories of archaeologists), and the links between these histories and a broader project of understanding the changing relationships between archaeology and its cognate disciplines such as anthropology and history.

My comments about the historiography of archaeology are in part a reaction to developments that have occurred over the last decade within archaeology, but in larger part a consequence of my own interest in the field. Of course the history of archaeology is not the sole preserve of archaeologists, and it is one of the most encouraging signs that historians of science, and especially historians writing essentially popular works (usually biographies), have paid growing attention to archaeology and its practitioners.

I will begin by presenting a brief overview of historiographic analysis in archaeology. I will then very briefly discuss some of the themes that have arisen from my own research in the histories of anthropology and archaeology, and outline the new book on the history of prehistoric archaeology in England where I have attempted to ground those themes in a specific longitudinal study. I will follow this with a brief discussion of *British Barrows* that focuses on the use by Greenwell, Rolleston, Davis and Thurnam and John Beddoe of crania and other human physical attributes, to both write a racial history of England and contribute to contemporary debate about the importance of race in human affairs.

There are two reasons for wanting to spend only a very short space to historiography *per se*. First, because (to put it bluntly) many of the methodological issues raised by exploring a history of archaeology are not unique to that discipline. Anthropology, geology and of course biology and physics have a far longer (and stronger) tradition in this area. Indeed such disciplines or fields have been significant contributors (either by way of methodology or examples) to the development of the history, philosophy and sociology of science, the perspectives of which will necessarily play an important role in the immediate future of the history of archaeology.

The second reason for moving discussion away from methodological considerations, is that it provides an opportunity to consider some of the consequences that an upsurge in research into the history of archaeology might have for our cognate disciplines of anthropology and history. Both disciplines have strong historiographic traditions, but I think that it is a fair

generalisation that the historians of neither discipline have paid much specific attention to archaeology. Of course George Stocking and many others have written about Sir John Lubbock when considering the genesis of an evolutionary anthropology (see e.g. Stocking 1968, 1987), some have further considered the work of Gordon Childe within the general context of discussions of the concept of culture, but apart from these, and a North American focus on the anthropology of Franz Boas and the work of theorists such as Julian Steward and Leslie White, interest has been generally slight.

So it might be interesting to consider how (if at all) recent explorations into the genesis of archaeology in Europe (for example) might affect the current story of the genesis of anthropology and history, primarily in the 19th century. This has been the focus of much of my own research in the history of archaeology and, as I have acknowledged many times elsewhere, this is not an innocent task. Although I am perfectly happy to accept (as many others have done) that writing the history of archaeology requires no other justification than inherent interest, my goals have more to do with diagnosing the condition of contemporary archaeology, and understanding the nature of its relationships with contemporary anthropology and history. But more of all of this after the very brief and partial historiographical survey.

Surveying the Historiography of Archaeology

These days almost everyone has remarked on the sheer amount of history of archaeology being written. At a recent Cambridge conference Bruce Trigger was moved to remark that the task of revising his influential *History of Archaeological Thought* had become very much more difficult in recent years. But Trigger was reflecting about the quantity of published work he had to synthesise rather than any inherent difficulty in the content of what was being written. This is because much of this history writing has been devoted to theories, methods, discoveries, and to the lives of 'great' archaeologists. While such studies are obviously important in establishing some of the aspects of archaeological practice, they alone do not produce satisfying accounts of the process of archaeological knowledge production.

Although historians of archaeology have become much more sensitive to the demands of context, there remain few analyses of the institutional structures of the discipline, of the wider intellectual context of archaeology, or of other sociological aspects of archaeological knowledge production (though the latter are increasing). The result of these shortcomings has been rightly criticized by some archaeologists, and by historians of the human sciences that have taken an interest in archaeology. Much of what has been produced is teleological, with the nature of archaeological knowledge transcending social and historical context. Until recent years analysis of the taken-for-granted of the history of archaeological practice such as institutional structures, relations with governments and the general public, organizing concepts and categories, and archaeology's relationships with its cognate disciplines, have been few and of variable quality.

After the late 1980s things began to change with the publication of two books. First, Bruce Trigger's *A History of Archaeological Thought* which, notwithstanding some significant shortcomings, represented a quantum leap from what was then available in English. Second, Alain Schnapp's *Conquest of the Past*, which has done so much to remind prehistoric archaeologists of the riches of 'The Great Tradition' as well as the great virtues of antiquarianism as a system of study. Around the same time archaeologists more versed in the history and philosophy of science such as Wiktor Stoczkowski and myself began deploying perspectives from that field, and serious discussion about the historiography of archaeology began to occur in mainstream contexts such as the Society for American Archaeology. Andrew

Christenson's *Tracing Archaeology's Past* (1989) was the first collection of essays in English from researchers strongly committed to writing the history of archaeology in North America. It is significant that at that early stage many of the issues raised by such history-writing (for example its justifications, the respective pluses and minuses of internalist and externalist perspectives, the perils of presentism, and that old favourite, whether the history of archaeology is better written by historians of science rather than by archaeologists) were all given a thorough airing. Subsequent discussion, for example Bruce Trigger's entry on historiography in the *Encyclopedia of the History of Archaeology* (2001), tended to reinforce these trends, which were also the subject of a really intense debate published by Raymond Corbey and Wil Roebroeks as *Studying Human Origins: Disciplinary History and Epistemology* (2001). Both Trigger and Corbey and Roebroeks sought to classify academic production either through a pretty straightforward division between popular, intellectual and social histories (Trigger) or through an application of Ernst Mayr's taxonomy – lexicographic, chronological, biographical, cultural and sociological, and problematic histories (Corbey and Roebroeks). But the editors of *Studying Human Origins* were after more than classification. Their goal was to seriously explore the *why, what, how* and indeed *whether* of such histories. Difficult questions such as why historians seemed to be ignoring the history of archaeology were asked, and the manifest shortcomings of archaeologists as historians of their own discipline were given thorough discussion. This is a common theme, sometimes taking on the characteristics of a turf war.

The sometimes casual disparagement of histories being written outside (or indeed sometimes in ignorance of) the canons of the history of science might be taken as clear testimony that we have a long way to go before the history of archaeology becomes a respectable pursuit. I do not think so. In fact I think that the contrary is the case, as archaeologists have become more skilled at articulating archives, oral histories and other testimonies in their analysis (Marc Antoine Kaeser's book on Eduard Desor (2004) and the *Ancestral Archives* issue of *Antiquity* edited by Nathan Schlanger are excellent examples). Historians of science have also become somewhat more understanding of the wide range of motivations archaeologists are responding to when they work in this area.

Archaeology and Anthropology

I came to the history of archaeology through undergraduate research in the history of anthropology, specifically the history of 19th century race theory. My first work focused on the monogenist/polygenist debate, as exemplified by the Scottish anatomist Robert Knox and his English disciple, James Hunt – the founder of the Anthropological Society of London, a great follower of Paul Broca, and the publisher of much European anthropology. Understanding Knox's most famous work *The Races of Men* (1850) posed significant intellectual challenges, not because so much of what he was saying was repugnant, but because at its core it represented a coherent and marvellous rich intellectual tradition spanning anatomy, philosophy, biology, ethnology, archaeology, and of course philology that was radically at odds with my own training as an anthropologist. Robert Knox's search for a scientific English anthropology that was both polygenist and anti-evolutionist provides an excellent example of how disciplines lose their histories, as dominant readings of disciplinary approach and purpose reinforce their dominance through the socialising power of disciplinary history.

But a case could be made that although it was distinctly marginal to contemporary philosophical orthodoxies in the mid 19th century, the transcendentalism of naturphilosophie played a significant role in the development of ethnology (particularly in the construction of the concept of culture). A close analysis of Knox's *The Races of Men* reveals something of the

spirit that drove this alternative anthropology, and the conflict between these alternative anthropologies and archaeologies in mid 19th century England also provides an opportunity to explore the ways in which the participants sought support from science and society, and the conditions under which that support was given.

I continued to explore these ideas in doctoral research focused on an inquiry into the authorities archaeologists appeal to in order to justify their knowledge claims in contemporary archaeology. The existence of such hidden histories in anthropology persuaded me that such were likely to exist unremarked in archaeology too, and that the naturalness of contemporary views of the archaeologist's project was illusory. In my view denaturalising such views could provide a basis on which to seriously address problems within contemporary archaeological theory. Historical research has helped broaden my approach to this problem from being narrowly epistemological to asking a more encompassing question: "what makes archaeological accounts of the past plausible?" A consideration of plausibility then led me to a more detailed investigations of the links between archaeology, and the society which sustains its practice. This, in turn, has greatly increased the significance of the history of archaeology as a primary source of information about related inquiries into disciplinary traditions and the 'culture' of archaeology.

What happened as a result of this research into the authority of archaeological knowledge claims (and related reflections on the nature of contemporary theoretical archaeology) is much more than can be dealt with in this short paper. What I can do in this context is to very briefly introduce the themes that have underwritten aspects of this inquiry under very broad umbrella raised by George Stocking over 20 years ago. My account of the history of archaeology is directed towards the identification of enduring structures of archaeological knowledge – those structures that provide the criteria in terms of which knowledge claims are justified as being both rational and reliable, and that also provide practitioners with the ability to distinguish meaningful knowledge and the relevance of models, theories and approaches drawn from archaeology's cognate disciplines. Stocking's cogent summary of the 'ethos' of anthropology as we have come to know it has been a great help here:

Another way of looking at the matter is to suggest that the general tradition we call retrospectively "anthropological" embodies a number of antinomies logically inherent or historically embedded in the Western intellectual tradition: an ontological opposition between materialism and idealism, an epistemological opposition between empiricism and apriorism, a substantive opposition between the biological and the cultural, a methodological opposition between the nomothetic and the idiographic, an attitudinal opposition between the racialist and the egalitarian, an evaluational opposition between the progressivist and the primitivist – among others (1984: 4).

Archaeology, through its close connections to anthropology and history has inherited these long-standing epistemological and ontological antinomies, which have at various times in the history of the discipline sanctioned historicist or universalist, materialist or idealist, empiricist or rationalist emphases within the practice of archaeology – precisely as they have done in our cognate disciplines.

In this view by the end of the 19th century the connections and distinctions between archaeology and anthropology and archaeology and history had essentially been established. Archaeology, its conceptual field defined and secure within various traditions of anthropological and historical research, and its methodology developed to a stage where the discussion of temporal and cultural classifications could appeal to a widening store of empirical phenomena, was free to pursue problems of largely internal moment. Although in the United States the predominance of cultural rather than social anthropology, meant that

the boundaries between archaeology and 'historical' anthropology were somewhat blurred, the same emphasis on the writing of prehistory, and on technical matters of classification and data retrieval was still present.

While it is the case that changes in fashion and orientation in anthropology and history directly affected the interests and approaches of archaeologists working under the aegis of either anthropological tradition, practitioners could keep pace with such changes in meaning by changing the terms of their translations of material phenomena into first, archaeological and subsequently anthropological, data. These changes were readily accomplished for four reasons.

First, archaeological data were considered to be impoverished testaments of human action in comparison with the richer data derived from socio-cultural anthropology. Meaning and the power to convince thus lay with the disciplines which 'managed' that latter data set.

Second, archaeological methodologies of description and classification were substantially relative rather than absolute. Given the anthropological and historical construction of archaeological data, there were few empirical grounds upon which those data, of themselves, could seriously disturb the intentions of their interpreters.

Third, despite the overt theorizing of practitioners such as Steward and Childe, the bulk of archaeologists were largely implicit consumers of theory, devoting their energies to methodological and technical issues of data collection and classification.

Fourth, given the essentially empiricist orientation of archaeologists in the years before the 1960s theoretical disputes were either settled on the authority of the archaeologists involved, rarely explicitly discussed because they were considered to be speculative and lacking the possibility of an archaeological contribution to their solution, or were simply set aside for some future time when the data were in. Thus, again with the exception of practitioners such as Childe, Steward and Clark, few archaeologists recognized that extant conceptual and epistemological relativisms within the source areas of archaeological theory could act as spurs to the development of such theory.

I have described the long and intense association between archaeology and anthropology, and between archaeology and history as being one of enrolment and symbiosis, beginning in the 19th century when all three disciplines began to take on their modern forms, and concluding around the end of that century (Murray 1987). This association, although differing in particulars over the course of the 20th century, continues to provide substantial aspects of the archaeological agenda and by far the most important body of theory used by archaeologists in their day-to-day practice.

But it is also the case that the process of translating archaeological data into anthropological or historical information (or indeed of applying the perspectives of those disciplines to archaeological data) did not (and does not) always go smoothly, and archaeologists might have had to take seriously the idea that such simple translations can be problematic. But has this really affected the way archaeologists seek to make the past meaningful – are practitioners able to abandon science in favour of intelligibility in conventional human science terms? I have sought to understand whether the plausibility of archaeological knowledge claims has been gauged primarily in terms of determinate rules of scientific method, or whether the real determinants of plausibility were 'cognitive' or 'cultural'. It was something of a surprise to find that even at the high point of empiricism in the mid 19th century where the methodological rhetoric held that archaeology contributed to the

development of an approach to understanding human prehistory that explicitly shunned myth and the *a priori* in favour of the objectivity of science, that the performance of practitioners fell way short of the mark. This difference between rhetoric and performance (especially as it applies to claims for the scientific status of archaeology) as continues to this day, mostly unremarked.

A History of Prehistoric Archaeology in England

I have sought to further explore these general themes in a longitudinal study of prehistoric archaeology in England. The scope of this new book is sufficiently broad (some 800 years) to allow me to demonstrate the genesis and development of the perspectives of prehistoric archaeology in that country, but my focus on the period between 1800 and 1980 will also help me to examine that history in light of the histories of anthropology and history over the same period.

Producing a comprehensive narrative history of English prehistoric archaeology is something of a challenge requiring those foolish enough to attempt it to take account of the fact that English prehistoric archaeology is a large and complex entity made up of a web of producers and consumers of archaeological knowledge who intersect with the fabric of the discipline through a wide range of institutional, social, political and cultural contexts. The difficulty of the task is increased by two related factors. First, the practice of English prehistoric archaeology has had global implications especially from the moment when Lubbock published *Prehistoric Times* in 1865. Much of the methodological and theoretical landscape of prehistoric archaeology (especially in the Anglo-Saxon world) has been strongly influenced by people based in England or working on English materials. By the same token the interpretation of English prehistory has relied on inferences drawn from all over the world. Second, each temporal division (Palaeolithic, Neolithic, Bronze Age and Iron Age) has its traditions and its rhythms, and it is uncommon for archaeologists to have an understanding and appreciation of these matters in all prehistoric periods.

Given the scale of the task my coverage is synoptic and selective. What I have been able to do is to provide a brief and very general narrative of evolution and to isolate several historically significant themes. I mentioned earlier that one of the consequences of the history of archaeology not being a mainstream area of archaeological research was the perpetuation of questionable perspectives or the burying of historical context that might be 'distasteful', 'dehumanizing' or 'unscientific'. In the new book this will be exemplified in several case studies – one of which explores the clear links between archaeology and racial Anglo-Saxonism. I have already explored some of the broader issues in papers on the Ancient Monuments Protection Act, the conflict between anthropology and ethnology at the British Association for the Advancement of Science, and the work of Robert Knox, but in this context I want to very briefly discuss some of the context of *British Barrows*, Greenwell's great work of 1877, and to take things a little further.

The Context of *British Barrows*

Historians of archaeology and of antiquarianism in late 18th and 19th century England have rightly focused on the evolution of landscape and topographical studies as a major driving force in the development of method (See e.g. Sweet 2004). Exemplified in the work of Richard Gough (particularly the *Anecdotes of British Topography*, in an expanded edition of Camden's *Britannia* (1789) and in *Sepulchral Monuments of Great Britain* (1799)), landscape and topographical studies taking place across the counties of England reached a large and expanding audience. Links between such studies, the writing of county histories and of

course folkloric studies became more common fostered by, among others, Charles Roach Smith whose 'Antiquarian Notes' in *The Gentleman's Magazine* and *Retrospections Social and Archaeological* (1883) are rich sources of perspective, as are the editorials of the relevant archaeological and antiquarian societies that grew up at the time. Here the *British Archaeological Association*, founded by Charles Roach Smith and the truly indefatigable Thomas Wright, is an excellent example. Major works on Romano-British sites and antiquities (by such as Wright) were matched by those produced by such as John Evans on the antiquities of earlier periods, but it was antiquaries such as Greenwell who greatly expanded the sheer mass of information on the sites and landscapes of pre-Roman times. These involved the acts of excavation, classification and comparison (the last of which was almost wholly dependent on timely and accurate publication, and the sharing of information at meetings and conferences) were now much more aware of what others were doing. In this sense the institutional structures of archaeological antiquarianism acted precisely as they should and the English scene expanded to the local to encompass regional, national and international scales of comparison.

Greenwell had a strong sense of the importance of what he was doing. In the preface to *British Barrows* he spoke of the various causes for the destruction of barrows observing:

still more have been destroyed under the influence of a curiosity almost as idle, by persons indeed of better education, but who thought that enough was gained if they found an urn to occupy a vacant place in the entrance hall, or a jet necklace or a flint arrow-point for the lady of the house to show, with other trifles, to her guests requiring amusement (Preface b).

Clearly the responsible antiquary should publish, but they should also have a proper appreciation of the history of their calling. The Preface to *British Barrows* has a comprehensive and generous appreciation of the work of predecessors – particularly Colt Hoare (Wiltshire), Bateman (Derbyshire), Carrington (Staffs) and Ruddock (North Riding of Yorkshire), but published works such as Warnes' *Celtic Tumuli of Dorset*, Borlase's *Nenia Cornubiae*, and more famously, Douglas' great *Nenia Britannica* and the Reverend Bryan Fausett's *Inventorium Sepulchrale*, were also acknowledged. These works covered much of England and allowed Greenwell (among others) to detect regional differences and similarities in site types and their contents (both skeletal and material cultural). However, it is the discussion of the crania (and the historical speculations of Greenwell and Rolleston about them) that most concern me.

Rolleston's discussion of the cranial series in *British Barrows* emulated Greenwell's preface in that included a long discussion of the history of cranial analysis in Britain, focusing on data that had been retrieved from excavated tombs, as well as more modern observations taken in Europe and elsewhere. Rolleston's survey dwelt on the work of Wilde in Ireland and of Daniel Wilson in Scotland, and of course Sven Nilsson in Scandinavia, to make the point that crania were important historical data. Indeed Davis and Thurnam's *Crania Britannica* (1865) were able to consider the issue of the Aboriginal races of the British Isles because of the crania excavated by Bateman and others. Moreover in Davis' subsequent *Thesaurus Craniorum* (1867) his sample of Aboriginal crania had increased to 36, all sourced to barrows dug by Bateman, Ackerman, Mayer and others. Thus there was already a clear tradition of making history from what was then called 'ethnological' or 'anthropological' analysis.

For Rolleston (as for Greenwell) there was no doubt about the cranial series could be classified in traditional terms:

A craniographer with Canon Greenwell's series before his eyes ... would be impressed with the fact that out of the series, two sets, the one with its length typically illustrative of the dolichocephalic, the other by its breadth as typically illustrative of the brachycephalic

form of skull, could at once be selected, even by a person devoid of any special anatomical knowledge. An antiquary similarly inspecting this series with a knowledge of archaeological history would, if he separated it into two groups, the one containing all the skulls of stone and bronze age, the other containing all the skulls of the bronze period, perceive that while the latter group comprised both dolichocephalic and brachycephalic crania and in very nearly equal proportions, none but the dolichocephalic skulls were to be found in any set of skulls from the barrows of the pre-metallic period (627).

But what did this mean? Both Greenwell (and especially Rolleston) understood that the cranial series they were working provided an exception to Thurnam's old rule that long heads went with long barrows (and were older) and broad heads went with round barrows (and were more recent). Yet neither the antiquarian nor the anatomist were prepared to argue as Davis was to do in his *Thesaurus Craniorum* that the skulls should be classified in one of the standard racial divisions (such as Gaelic) or one of the tribal divisions noted by the Romans (such as the Brigantes). The absence of secure absolute dates was obviously a problem here – both at the level of determining synchronicity or succession, as well as determining duration. But Greenwell had to account for the anomalous pattern, especially after he had accepted that Thurnam's rule generally held for the vast bulk of the data to hand and was strongly supported by the evidence drawn from material culture. It is worth quoting Greenwell's solution at length because of its focus on producing a racial history of subjection and eventual intermixture, one that seemed entirely reasonable having regard to history and contemporary circumstances:

This condition may have been brought about, and probably was, by the fact that the intruding round-headed people, smaller as they may have been in number, were gradually absorbed by the earlier and more numerous race whom, by force of one advantage or another, they had overcome. This subdued long-headed people may very possibly, in the earlier times of the conquest, have been kept in a servile condition, and therefore were not interred in the barrows, the place of sepulture reserved for the ruling race by whom they were held in subjection, and hence the numerical superiority of brachycephalic heads in the barrows. But as time went on and intermixture between the two peoples became common, a change would have gradually taken place in the racial characteristics, until at length the features of the more numerous body, that is to say the dolichocephalic, would become the predominant type of the united people (129).

So much for the past, but what about the present and the future? Much has been written by Stocking, Burrow (1966) and others about the history of 19th century anthropology and race theory, and space precludes a lengthy recapitulation here. Significantly both Rolleston and Greenwell were well aware of this larger dimension to their work, and Greenwell was absolutely right in his general methodological conclusion to *British Barrows*. By the end of the 19th century it was to become apparent that what English prehistoric archaeologists urgently needed to do was to write history, to make the classifications arrived at in England and on the continent relate in real historical terms to the patterns being noted in the field.

But prehistoric archaeology (as a part of anthropology) was far from alone in this concern with history and historicism. Although from the 1880s perceptions of human diversity made a forceful return to the ranks of anthropology, this diversity was clearly to be located in ethnic and cultural, rather than purely physical differences. Explanation for diversity and similarity was increasingly to be sought in cultural historical factors, rather than by appealing to the doctrine of independent inventions and the psychic unity of mankind. Real historical forces acting on real (different) groups of people, past and present, could explain the peculiar differences between human beings far more convincingly than generalised uniformitarian forces. Anthropology and prehistoric archaeology, previously focused on providing evidences

of the evolution of human beings and their societies and cultures, now became more firmly linked to a less encompassing task – writing the ethnic histories of European nations. Greenwell's grappling with the patterns established in *British Barrows* is an excellent exemplar of what was to be transformed into culture historical archaeology.

But there was always more to doing this than making claims for the reality of strict inductions and a freedom from the *a priori* (methodological strictures that were honoured far more in the breach than in the observance). Here I want to briefly touch on two works by the eminent 19th century anthropologist John Beddoe. The first is *The Races of Britain* (1885) which presented the fruits of many years data collection on (among many variables) height, the colour of skin, hair and eyes, and location from a broad sample of the British population. Here was contemporary race science in action, propounding the lessons of the past to chart the course of the present and future. I confess to a fascination with dismantling Beddoe's logic and method, teasing out the normative judgments and prejudices from his 'science' and laying bare the mechanics of racial Anglo-Saxonism, particularly its hatred of the Irish and its love for the active principle of race war (which was not just a favourite of Beddoe's but to be found in the work of mainstream scientists such as Boyd Dawkins). In *The Races of Britain* Beddoe does many memorable things – deriving the Irish from the Cro-Magnons and creating his startling 'Index of Nigrescence' are just two that are worth a little comment to reveal something of his approach.

Referring to the descendants of the 'palaeolithic race' still resident in the British Isles Beddoe observed:

There is an Irish type ... which I am disposed to derive from the race of Cro-Magnon, and that none the less because, like so many other Irish types, it is evidently common in Spain, and furnishes, as Maclean remarks, the ideal portrait of Sancho Panza. It is said to be pretty common in the Hebrides, but rare in the Highlands. In the West of Ireland I have frequently seen it; but it is curious, psychologically, that the most exquisite examples of it would never submit to measurement. Though the head is large, the intelligence is low, and there is a great deal of cunning and suspicion (10).

Small wonder! But if this element of method is questionable, consider how he arrived at his index of nigrescence, which can be expressed as $D+2N-R-F$. Beddoe wanted a scientific basis for comparing the colours of two peoples or localities so that he could link past, present and future.

The gross index is gotten by subtracting the number of red and fair-headed persons from that of the dark haired, together with twice the black haired. I double the black, in order to give its proper value to the greater tendency to melanosity shown thereby; while brown (chestnut) hair is regarded as neutral ... (5).

Beddoe took his observations from walking in the street not by carting willing or unwilling subjects off to his anthropometrical lair. Fieldwork (and his involved a lot of walking) posed some interesting problems.

When engaged in this work I set down in his proper place on my card of observation every person I meet, or who passes me within a short distance, say from one to three yards. As a rule, I take no note of persons who apparently belong to the upper classes, as these are more migratory and often mixed in blood ... Considerable difficulties are created by the freaks of fashion. I once visited Freisland, in order to study the physical type of that region. Conceive my disappointment when I found myself surrounded by comely damsels and buxom matrons, none of whom suffered a single yellow hair to stray beyond her lace cap or silver-gilt head-plate (5).

Beddoe was not so deluded as to think that his index was unimpeachable race science. Far better was to measure heads and to be entirely systematic. Here archaeology was particularly useful, and Greenwell among others got a big vote of thanks, but better still was for the state to be directly involved in data gathering, because the information was considered to be vital to the interests of the state. Here the development of modern armies in the USA and in Europe was to provide a first class source of data and the opportunity to tweak the various indices still further. We should not make the mistake of thinking that this was anything other than mainstream science, and Beddoe was absolutely convinced that further work would bear him out. His conclusion is clear enough:

But a truce with speculation! It has been the writer's aim rather to lay a sure foundation ... If these remaining questions are worthy and capable of solution, this can be solved only by much patient labour, and by the cooperation of anthropologists with antiquarians and philologists; so that so much of the blurred and defaced prehistoric inscription as is left in shadow by one light may be brought into prominence and illumination by another (271).

In the second of Beddoe's works, *The Anthropological History of Europe*, which was the Rhind lecture for 1891, and updated in 1912, he revisited his earlier work and extended the coverage to Europe in six lectures. The first "The Aryan Question and that of variation of type", the second "Variation – Primeval man – succession of races" which was then followed by another three lectures giving a synoptic coverage of Europe leaving the last lecture for those closest to home, "Scotland". In all of these Beddoe rehearsed the usual data – crania, indices of 'nigrescence', language and folklore, common understandings of racial temperament and the like – to arrive at prognostications about the future. For Beddoe (as with others of his time and disposition) race and racial conflict was not just a formative principle in human history – it was the formative principle of the future as well. As with much of Beddoe's reasoning, the tendency to elide from straightforward prejudice to normative judgment is quick and easy. Consider these insights into the war between the dolichos and brachycephals (as Beddoe liked to call them). First, the Jews:

But, of the increase of the Jews, at least there can be no doubt whatever. There are no data to show us whether of the two curiously discriminated Jewish types is gaining on the other; but I strongly suspect that it is the brachycephalic. However that may be, the Jews grow not only in number, living longer and dying less readily than the Gentiles among whom they dwell, but they are gradually attracting to themselves the whole moveable wealth of the earth; and wealth is power, and the world must move or halt as wealth bids it. It would be strange if, in spite of the community of religion and traditions and usages, there were not some moral or intellectual difference connected with the physical one between these two sections of the Hebrews. And I believe there is. The Shepardim, who have usually the rather small oval true semitic type of head, are said to be somewhat looked up to by the Ashekenazim, who are mostly of the broad-headed type. And whatever may be the case in the present time, in past times it has been individuals from the Shepardim who have distinguished themselves from the common herd of their fellow-believers, and that in ways more noble than money-making (183–184).

Next from somewhere closer to home, Cambridge:

Dr Venn has shown, that at Cambridge the first class men have proportionally longer as well as more capacious heads than the rest of the students. In our own islands where the breadth of head varies locally but little, and its general form more decidedly, while the complexion varies very considerably, it is safe to say that men of distinction are in large proportion natives of the more blond areas (185).

Last, a more general, perhaps more direct statement:

On the other hand, we are told that in common schools in France, the long-headed children surpass the broad-headed ones; that the world owes far more to the Englishman, the Scotchman and the Norman, than to the Kelt, the Rhaetian, the Rouman or the Slav; and that it would simply stagnate and putrefy were the northern long-headed race to be nipped and checked in its development, for the source of originality, of genius, of inventiveness, of the spirit of travel and adventure, would be cut off. "Better fifty years of Europe" they say in effect, "than a cycle of Cathay" (187–188).

Although Beddoe would conclude the lectures with an attempt at racial inclusiveness (stating that both long heads and broad heads have much to offer humanity and that diversity is a good thing) it sits oddly with the whole tenor of what he had been saying, and with the kind of racial Anglo-Saxonism that underpinned the widespread acceptance of the very close evolutionary relationship between the Irish and gorillas. The simple point to be made here is that this was preeminently popular science (although its scientific credentials even then were highly dubious), and that it was to provide a clear and direct framework within which archaeological and antiquarian studies were to gain great meaning and value well into the next century.

Concluding Remarks

Part of my goal in this all-too-brief discussion of just one aspect of the history of English prehistoric archaeology has been to support the case that the history of archaeology matters. Many histories of English archaeology have tended to gloss over aspects of past context that are either repugnant or seem to have been such obvious wrong-turnings on the path to truth. Detailed research into broader social and cultural contexts has the capacity to reveal a complex and frequently counter-intuitive history. Given the fact that English prehistoric archaeologists have long had considerable impact on the practice of prehistoric archaeology outside of Britain (particularly in the Anglo-Saxon world), a deeper understanding of its social and cultural history is as important for all archaeologists, not just those from Britain.

But having said this, I believe that while histories of archaeology should be sensitive to histories of other disciplines such as anthropology and history, historians of those disciplines should also not ignore what is happening in archaeology. Many prehistoric archaeologists in the English speaking world still adhere to the tenets of anthropological archaeology and the proposition that archaeology is a subset of anthropology, particularly in terms of the theories it deploys. Yet in recent times the naturalness of this relationship has been questioned as practitioners begin to comprehend that the archaeological record poses significant problems and issues that have never been considered part of anthropology or historiography. Thus archaeologists might yet face the prospect that other archaeologies are possible and possibly desirable, and new histories that might conceivably reassess the history of relations with anthropology and history will need to be written.

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Captain Nemo/Lt-General Pitt Rivers and Cleopatra's Needle — A Story of Flagships

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Recently re-reading Verne's *20,000 Leagues Beneath the Sea* for our children I was struck by the marked similarities between the novel's elusive protagonist, Captain Nemo, and the renowned later 19th century British archaeologist, Lt.-General Pitt Rivers. Could they have been the same person? How could something so seemingly blatant have gone unnoticed? These questions are, of course, only raised in a spirit of academic tongue-in-cheek. Yet, in an ethos of 'learning through amusement' (itself directly relevant to the themes of this study), exploring the parallels between these two 'heroic' individuals provides insights into the nature of 19th century science, Victorian edification and disciplinary institutionalisation (e.g. Levine 1986). This eclectic contribution will, moreover, be introduced with the third component of its headline title – *Cleopatra's Needle* – as this provides an appropriately quasi-nautical parable on the project of 19th century archaeology and the problem of 'deep time' (Murray 1993).

Cleopatra's Voyage

The transshipment of the obelisk, Cleopatra's Needle, is a tale of both imperial symbolism and high seas adventure, and was thoroughly covered in a series of articles in *The Illustrated London News* between March 1877 and September of the following year.⁽¹⁾ Embedded in Alexandria and threatened by construction adjacent to the City's railway station, it was only through the private benefaction of the eminent surgeon, Erasmus Wilson, that it was to be shipped to London; Britain's capital, of course, 'needing' an obelisk in the same way that they adorned Paris and Rome. The engineering solution hit upon by Messrs. Baker and Dixon was ingenious. It required building a great wrought-iron pontoon (*The Cleopatra*) around the prone, *in situ* 150 ton monument. Having a deckhouse and accommodation for three men, though 'submarine-like' it had to be towed by steamer (*The Olga*) during its voyage to London (see e.g. Harris 2001 concerning the history of submarine technology). However, in October 1877 a severe gale in the Bay of Biscay caused its abandonment, with six crew of *The Olga* losing their lives attempting to rescue those in their tow. Cast adrift, *The Cleopatra* was eventually picked-up by another steamer destined for Spain, and was left in the port of Ferrol with a claim of salvage against her. Only in January of the following year was she finally delivered by steam-tug to London (the cost of the enterprise rising by 50% to c. £15,000.00).

Not surprisingly, the arrival of the obelisk in the city drew great crowds. Having previously