UNIVERSITY OF CALIFORNIA, SAN DIEGO

Experts and Australopithecines: Credibility and Controversy in the Science of Human Evolution, 1924-1959

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by

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The Dissertation of Jesse Richmond is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

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ABSTRACT OF THE DISSERTATION

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By

Jesse Richmond

Doctor of Philosophy in History (Science Studies)

University of California, San Diego, 2009

Professor Naomi Oreskes, Chair

This dissertation investigates debates in the early and middle parts of the twentieth century over the significance of the Australopithecine fossils discovered in South Africa. The initial specimen, famously known as the "Taung Child", was characterized by Raymond Dart in 1925 as a possible evolutionary ancestor of human beings, linking our species to a distant past in which our anatomical similarity to the apes was much more conspicuous. Most of the recognized scientific authorities disagreed with Dart's assessment, instead seeing the specimen as a mere extinct ape, without any special place in

humankind's evolutionary history. My narrative examines the debates that ensued over the next three and a half decades, closely following the changing credibility of Dart's initial claim, as well as subsequent claims by Dart and other scientists about the Australopithecines, through the shifting networks of objects, texts, people, practices and institutions that made up the infrastructure of paleoanthropological knowledge. The narrative demonstrates that the determinants of credibility in the Australopithecine debates were strongly tied to the particulars of local circumstances and personal relationships, and cannot be reduced to any normative, *a priori* account of how credibility is or ought to be achieved in science.

Introduction

> *Undoubtedly, one of the main factors responsible for the frequency with which polemics enter into controversies on matters of paleo-anthropology is a purely emotional one. It is a fact (which it were well to recognize) that it is extraordinarily difficult to view with complete objectivity the evidence for our own evolutionary origin, no doubt because the problem is such a very personal problem. Even scientists of today may not find it easy to clear their minds entirely of an emotional element when they come to consider the evidence in detail, and this emotional element is only too frequently betrayed by the phraseology with which disputants claim with equal insistence to be assessing the same evidence dispassionately.*
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> - Wilfred E. Le Gros Clark, "Bones of Contention," Huxley Memorial Lecture, delivered November 28, 1958[1]

The distinguished British anatomist and anthropologist Sir W.E. Le Gros Clark opened his 1958 Huxley Memorial Lecture by pointing to what he perceived as the peculiar character of paleoanthropology – the scientific study of human evolution – among the sciences. For a century, every fossil discovery that could potentially be taken as evidence of an evolutionary connection between human beings and some "lower form" had precipitated a large number of mutually contradictory interpretations from ostensibly authoritative voices, often wildly divergent in their assessments of the fossil's significance. More often than not, and certainly more often than Clark took to be the norm in most scientific fields, this proliferation of views had descended into "controversies of a polemical nature."[2]

[1] Subsequently published as Clark, "Bones of Contention" (quote from p.132).
[2] Ibid., p.131.

Clark identified one major culprit in paleoanthropology's pattern of contentiousness – and this *was* an issue of culpability, for the continuance of this pattern was in no way desirable from his point of view – as *emotion*. Scientists, being human, were liable to have very personal reactions to evidence concerning human origins, and the interpolation of such personal feelings into the assessment of fossil evidence led to a loss of objectivity and the propagation of as many interpretations as there were persons to do the interpreting. The personal, emotionally charged connection that investigators felt towards their own interpretations tended to inhibit their ability to dispassionately consider other possibilities, and often resulted in the undue use of polemical language. Disputants often claimed to be dispassionate in their respective assessments of the evidence, Clark pointed out, but their raucous dissensus gave them the lie.

What was so objectionable to Clark about the persistent and radical disagreement among students of paleoanthropology? It was not controversy itself that bothered him. In fact, he granted the central role of controversy in science, saying that "the sparks of controversy often illuminate the way to truth."[3] Rather, it was the form and duration which controversy seemed so often to take in paleoanthropology that troubled him. Due to their emotionally charged character, controversies in paleoanthropology were "apt to be prolonged beyond their usefulness."[4] Whatever sparks such disagreements produced eventually went out, after which the continued controversies worked to obscure rather than illuminate the way to truth.

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[3] Ibid., p.143.
[4] Ibid., p.132.

Viewed through the lens of recent work in the social studies of science, Clark's address might be interpreted as speaking to a problem that paleoanthropology had historically had with the establishment of *credibility*. If various parties, all with some claim to speak with authority, held stubbornly to mutually exclusive accounts of the same fossil evidence, then which account was to be regarded as credible? Put another way, if some factor like emotion was preventing the proper (in Clark's view) conduct of scientific debate, then how was credibility to be assessed?

Clark himself did not use the language of credibility in expressing his concerns about the patterns he saw recurring throughout the history of paleoanthropology. Rather, he spoke in terms of truth: it was "the way to truth," not "the way to credibility," that was being blocked by continual, rancorous disputes. Nonetheless, we might fairly assume that for Clark, truth and credibility in science were not separate. As Steven Shapin has shown, the union of truth and credibility has been a hallmark of the dominant view of scientific knowledge since early modern times. For the modernist, according to Shapin, the "credibility and the validity of a proposition ought to be one and the same."[5] From this perspective, the solution to the problem Clark perceived in paleoanthropology's attempts to ascertain the truth about human evolution from fossil evidence would be the same as any problem paleoanthropology had with the establishment of credibility: remove the obstacles to truth (constant impassioned disputations or whatever else) and credibility would follow naturally.

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[5] Shapin, "Cordelia's Love: Credibility and the Social Studies of Science," pp.255-56.

But what if it is not assumed that credibility in the sciences flows naturally and unproblematically from truth? Such a disjunction between truth and credibility has in fact become a common methodological principle in the social studies of science.[6] Without such an assumption, one could not be assured that the removal of such things as one considered obstacles on the way to the truth would lead to a particular claim about some piece of evidence becoming credible; or, viewed from the perspective of the historian of science, one could not assume that a historical account of the development of conditions for truth was sufficient to explain how a scientific claim became credible. Indeed, without the classical modern assumption tying credibility to truth, the cause (or causes) behind any claim or belief gaining credibility becomes an open, empirical question.

This dissertation is an examination of the dynamics of credibility in science as they manifested themselves around a particular set of fossil evidence that came under investigation beginning in the nineteen twenties. It was this set of evidence, and the controversies that had swirled around it for more than three decades, that prompted Clark to give his address calling for reflection on how debate was conducted in the science of human evolution. In 1924, Raymond Dart, a young Australian professor of anatomy who had recently taken up a position at the University of the Witwatersrand in Johannesburg, South Africa, came into possession of the fossilized skull of a primate that had been blasted out of a limestone-filled cave by one of the lime-mining

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[6] Shapin credits this methodological insight to David Bloor: "It was David Bloor who made the disjunction between validity and credibility into a maxim of method in the social studies of science, and so it has become for those few specialist scholars who work in this idiom," Ibid., p.257; Bloor, *Knowledge and Social Imagery* 2nd ed.

operations that were then common on the high-plateau of central South Africa. In his published account of the fossil, Dart claimed that it displayed a set of characteristics that placed in mid way, morphologically speaking, between human beings on the one hand and apes on the other. For Dart, the fossil's morphology justified the claim that the creature to which the skull had belonged, which he named *Australopithecus africanus*, represented an evolutionary ancestor of human beings, linking our species to a distant past in which our anatomical similarity to the apes was much more conspicuous.

Most of the recognized scientific authorities in the field disagreed with Dart's assessment, instead seeing the specimen as a mere extinct ape without any special place in humankind's evolutionary history. Initially, skepticism about Dart's claim carried the day, but Dart nonetheless held to his original assessment. He had also managed to convince a few others, including the paleontologist Robert Broom, Dart's colleague in the South African scientific community. More than a decade after the original discovery, excavations led by Broom began to yield more fossils belonging to similar creatures. The torrent of new fossil material and Broom's steadfast defense of Dart's original claim led to renewed debate over the nature of these creatures, which became collectively known as the Australopithecines. Eventually, some scientific authorities from overseas, among them W.E. Le Gros Clark, began to take a more sympathetic view of the idea that the Australopithecines represented a vital link in the evolution of human beings. Nonetheless, skepticism persisted in some quarters, leading to ongoing disputes of the sort that Clark derided as harmful. Even among

those who had come to find merit in Dart's claim that the Australopithecines were ancestral to modern humans, controversies erupted over the nature of the creatures' bodies and habits. In 1959, when Dart published a memoir looking back on the last three and a half decades, he could justifiably report a major shift of opinion in favor of the position he had originally advocated. However, even in "victory", Dart was himself stirring up new controversy in a way that Clark judged to be destructive to the advancement of scientific knowledge.

The story of the Australopithecines is well known in its broad outlines. Indeed, it forms part of the "lore" that gets passed down to advanced students of paleoanthropology and comes to inform their sense of place in the internal history of their discipline. In addition, there have been several scholarly historical studies devoted in whole or in part to changing views of the Australopithecines in the decades after Dart first announced their existence. Recently, Tom Gundling, a biological anthropologist, has written a book devoted wholly to this subject, while the historian of science Peter Bowler's now classic survey of theories of human evolution from the mid-nineteenth to mid-twentieth centuries devotes a significant part to the Australopithecines.[7] For both Gundling and Bowler, the main question concerning the Australopithecines is: what can account, first, for the initial rejection of Dart's claim that they represented human ancestors, and second, for the much improved fortunes of that claim in the nineteen forties and fifties? For both authors, the answer is the same: the crucial factor in making the Australopithecines acceptable human ancestors in the

[7] Gundling, *First in Line: Tracing our Ape Ancestry*; Bowler, *Theories of Human Evolution: A Century of Debate, 1844-1944*.

eyes of many scientists was a broad shift in the dominant understanding of evolutionary theory in the nineteen thirties and forties. This change, the "Modern Synthesis" or "Neo-Darwinian Synthesis," is usually taken to have begun with the development of population genetics by R.A. Fischer, Sewall Wright, and J.B.S. Haldane in the nineteen thirties. Prior to this, so the story goes, many theorists had departed from a strictly Darwinian model of evolution in which change was largely a product of contingent environmental circumstances. Instead, scientists in the decades around the turn of the century developed models in which internally driven determinative trends were largely responsible for evolutionary change. A consequence of such models was that shared traits did not necessarily mean that two species shared a common ancestor from whom they had inherited such traits, as the similarity could be explained as the result of law-like "trends" operating to cause "parallel" developments in unrelated species. However, scientists who adopted the insights of the population geneticists questioned the legitimacy of such models, and advocated a return to a more "Darwinian" view in which shared traits represented strong evidence of shared ancestry.[8] The consequence for the Australopithecines, in Gundling and Bowler's accounts, was that under the earlier model the traits they shared with both humans and apes were not seen as decisive evidence of common ancestry. With the theoretical changes wrought under the Modern Synthesis, such evidence became far more compelling and the Australopithecine's claim to a near relation with human beings was thereby improved.

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[8] For an account of the changes in biology associated with the Modern Synthesis, see Provine, *The Origin of Theoretical Population Genetics*.

I have no quarrel with claim that the theoretical changes that occurred in evolutionary biology in the decades subsequent to the initial discovery of *Australopithecus* played a role in the growing acceptance of its status as a human ancestor towards mid-century. I do, however, wish to inquire into the changing fortunes of *Australopithecus* in terms other than those used by Bowler and Gundling. How to account, I ask, for the changing *credibility* of the claim that the Australopithecines were nearly related, and possibly ancestral to human beings? Though Bowler and Gundling do not use the word, their arguments could be conceived in terms of credibility, where the credibility of the claim of Australopithecine ancestry was chiefly determined by its plausibility within the dominant theoretical context in evolutionary biology. The story I will tell is broader than this, following a methodological point set out by Shapin in his analysis of the place of credibility in the social studies of science: "In principle, there is no limit to the considerations that might be relevant to securing credibility, and, therefore, no limit to the considerations to which the analyst of science might give attention." The theoretical context of a claim might be relevant, just as, following Clark, the emotion with which a claim is put forward and defended might be relevant. The important thing, as Shapin writes, is that "the relevance of *nothing* can be ruled out in advance of empirical inquiry."[9] The point of asking after credibility is not to criticize certain things as being inappropriate to the determination of credibility in paleoanthropology or to defend others as necessary to the alignment of credibility with truth or any other

[9] Shapin, *Cordelia's Love*, pp.260-61.

necessary precondition. The point is to find out, through empirical investigation of the documentary record, how scientists interested in human evolution negotiated the terms of the Australopithecine's inclusion or exclusion from the human family tree.

Shapin's inquiries into the nature of credibility serve as a good model to help orient us in what is a very broad framework of analysis. Admitting that "the picture framed by the unqualified study of credibility is just too big," Shapin makes a number of distinctions to help identify the issue at stake. One set of useful distinctions concern what he calls "economies of credibility," and one such economy he is that which obtains between experts and laity: How do claims made by scientific experts achieve credibility, or fail to do so, among the general public? Another is the economy obtaining between expert groups in different fields: How might a claim developed and accepted by paleoanthropologists achieve credibility among astrophysicists? Lastly, there is the economy of credibility obtaining between individual specialists within a scientific field. This essay will be largely concerned with this last type of economy as it manifested itself among specialists in human evolution during the Australopithecine debates, though issues pertaining to the other economies will occasionally interpolate themselves.

Shapin argues that the norm for credibility-economies internal to scientific fields is a state of mutual trust, since the "practitioners involved are likely to know each other very well and to need each other's findings in order to produce their own." "Here," Shapin continues,

> the immediate fate of one's claims is in the hands of familiar others, and the pragmatic as well as the moral consequences of distrust and

> skepticism are likely to be high. In such social settings the analyst should take care not to explain the achievement and maintenance of mutual credibility too aggressively. In a world characterized by familiarity – whether in lay or expert society – taking each other's claims at face value is *normal*, and it is distrust, skepticism, and the demand for explicit warrants for belief that need specially to be justified and accounted for. It is, indeed, hard to conceive how small groups of familiar others could long maintain cohesiveness were the situation otherwise.[10]

Insofar as Shapin's characterization is accurate, we can better understand what motivated Clark's concern with the nature of paleoanthropology, and what makes the Australopithecine controversy a worthy topic for study under the lights of the social studies of science. Why was this field, with its limited number of practitioners who were for the most part known to each other, seemingly characterized by persistent *distrust* when it came to the hominid fossil record? Did the field maintain its cohesiveness in the face of persistent controversy, and if so, how?

Another way to frame the problem of credibility is in terms of the concept of *expertise*. A small group of specialists characterized by the recognition of mutual credibility can be characterized as a group whose members have a high degree of trust in each other *as experts* in their shared field: insofar as *A* is an expert in our field, I can trust the claims she makes within the subject matter of that field. This need not imply that shared expertise is a sufficient condition of mutual trust in a logical sense, but only that it functions to maintain the social conditions for the production of specialized knowledge. Because the actors in the narrative to follow were all trained specialists, their judgments of the credibility of each other's claims within their shared field

[10] Ibid., pp.269-70.

involved judgments about each other's status as experts. This does not mean that every claim about the Australopithecines was met with explicit statements judging the claimants expertise in the matter. It does mean, however, that when one trained specialist failed to credit another trained specialist's claim as valid in a matter pertaining to their shared specialty, the former has in a sense also failed to credit the expertise of the latter. Hence my chosen title of *Experts and Australopithecines*, for in a fundamental way, deciding *what* the Australopithecines were involved deciding *who* the experts were.

In more general terms, we can say that the credibility of claims and the credibility of people are interdependent in practice.[11] I add "in practice" because the distinction is easy enough to maintain in the abstract. Indeed, we will see that the actors in the Australopithecine debates sometimes tried to give the impression of making a credibility-judgment about a particular claim while remaining neutral, or even being complimentary, about to the credibility of the claimant. However, from the actors' reactions to such ostensibly circumscribed judgments of their work, and from the overall dynamics of debate, it seems clear that the abstract boundary was permeable in practice. These scientists' identities were *made* of the claims they put forth.

While no set of factors can be enumerated *a priori* to account for the dynamics of credibility in science, I can highlight certain themes and issues which I have found,

[11] Shapin develops this point through a study of experimental philosophy in sixteenth-century England; see Shapin, *A Social History of Truth*.

in the course of my study of the Australopithecine controversy, to have played important roles. The list is by no means exhaustive; factors relevant to the establishment of credibility will come up in the course of the narrative that are not explicitly enumerated here in advance. Nonetheless, the list might serve to attune the reader to issues that the historical record suggests were of particular importance.

i. The Fossils and other Objects

It may seem trivial to claim that the fossils themselves played important roles in the determination of credibility in the Australopithecine debates. However, given the dominant role ascribed to theory in prior studies, the issue seems to bear mentioning. Consider Bowler's judgment of the comparative importance of theories and fossils in the history of paleoanthropology:

> The fascination of the fossil hominids is easy to understand – after all, they are the only tangible evidence of our origins. Yet it is obvious from the differing interpretation offered by scientists at the time that fossils had meaning only to the extent that they could be fitted into theories of how human evolution occurred. Whatever the potential interest of the discoveries themselves, it seems obvious that a comprehensive study of how understanding of human evolution has developed must focus on the theories, not on the fossils.[12]

Such a sweeping statement seems odd when we consider how much time and effort the historical actors themselves devoted to talking about the fossils. Were they deluding themselves? Was such talk only ostensibly about the fossils when it was really about theories? In emphasizing the role of theory to such a degree, Bowler is likely trying to avoid the opposite extreme, where the development of a science is said

[12] Bowler, *Theories of Human Evolution*, pp.5-6.

to develop unproblematically from the accumulation of material evidence without the ascription of theoretical meaning. Tropes of this kind are often to be found in scientists' own description of their activities, and historians have rightly questioned their adequacy and coherence. However, a denial that the simple discovery and accumulation of fossils can account for the development of a scientific understanding of human evolution does not require that the fossils take a back seat to theories in an account of that development. What is needed, rather, is a way of taking the role of fossils (or other scientific objects) into account in a way that does not entail a denial of the role of humans in ascribing meaning to those fossils.

The study of credibility provides one perspective from which to elucidate the role of fossils and other scientific objects without unduly obscuring the role of human actors. In the context of the Australopithecine debates, the determination of who was in a position to make credible statements hinged in part on how the interested scientists were distributed spatially with respect to the fossils. For instance, Dart and other scientists in South Africa who had constant, ready access to the original Australopithecine fossils could, and did, assert (or at least imply) that direct inspection of the specimens lent credibility to their claims about the significance of those specimens. Conversely, claims made from afar often had their credibility questioned for not being based on direct inspection of the original specimens. At the same time, the original Australopithecine fossils were not the only objects whose physical presence bore on the negotiation of credibility. Claims and criticisms were often based on objects standing in for the fossils, such as casts, photographs, drawings, and

sets of measurements. The credibility of such claims and criticisms was subject to questions about the faithfulness with which such proxies represented the original specimens, and the aptness of a particular representational mode to the claim being made. Objects whose relation to the original specimens did not hinge on issues of representation were also brought to bear on the debates. For instance, large collections of primate fossils for comparative study were said to be necessary to the proper evaluation of the Australopithecine fossils, implying that access to such collections a precondition of credibility.

It will thus be seen that the development of the Australopithecine debates cannot be adequately accounted for by the vicissitudes of theory alone. While the fossils and other objects did not dictate the procession of debate, the particulars of their presence in the world were consequential to the shape of the debate in that they constituted resources to support (or undermine) the credibility of claims.[13]

ii. Careers and Institutions

The importance of large comparative fossil collections demonstrates one way in which the institutional context of claimants was relevant to the negotiation of credibility in the Australopithecine debates. Scientific institutions – primarily universities and museums – varied widely in their possession of resources relevant to the practice of paleoanthropology, and thus what scientists had available to bring to

[13] There has recently emerged a movement in the history of science, spearheaded by Lorraine Daston, to reconceptualize the idea of a "scientific object" in a way that moves beyond old debates about the comparative influence of theory and evidence; see Daston (ed.), *Biographies of Scientific Objects*; and Daston (ed.) *Things that Talk: Object Lessons from Art and Science*.

bear in their defense or criticism of claims. Besides collections of fossils and other scientific objects, institutions varied in their human resources, which affected how much time different scientists could devote to the Australopithecine debate versus their institutional responsibilities, and how much help he could rely on from assistants and research students. Finally, institutions differed in terms of their prestige – an amorphous, overarching attribute that, while hard to pin down to particulars, was readily identifiable by scientists.

All of these institutional factors came into play during to the Australopithecine debates because all of the individual scientists involved were, for at least some time, employed by scientific institutions. This situation contrasts with earlier centuries when many scientists (Charles Lyell and Charles Darwin, for instance) did not depend on employment or institutional affiliation for the practice of their science. To one degree or another, almost all of the scientists dealt with in this study needed their institutional affiliation not only to practice their science, but simply to make a living. The need to gain institutional employment, and the benefits of rising higher in terms of institutional position and prestige, meant that participants in the Australopithecine debates at different stages in their career faced differing levels of personal risk in making scientific claims. A junior scientist with a desire to advance his career had a lot more riding on how other professionals judged his claims than did a senior scientist at a prestigious institution. This is not to say that senior scientists at prestigious institutions were unconcerned with the credibility achieved by their claims, but only

that in terms of careers in science, much more was at stake for the less established ones.

iii. Temperament, Comportment, and Language

Clark, as we saw, was concerned that a tendency to become emotional when assessing the significance of hominid fossils was a problem for paleoanthropologists. He was not the only one to suggest during the Australopithecine debates that certain temperaments were detrimental to the advancement of knowledge. On many occasions, scientists raised questions about the credibility of claim by making reference to the temperament of the claimant, as evidence either in his comportment or the language he used. The credibility of a claim might be questioned, for example, on the basis that the claimant had acted rashly in making the claim, where he ought to have been cautious and methodical. Where language was concerned, the credibility of a claim might be questioned because the language in which it was couched was deemed overly emphatic or needlessly ornamented. Raymond Dart was pursued by such criticisms of his temperament throughout the Australopithecine debate, and his attempts to achieve credibility for his claims were made so much more difficult as a result.

Of course, there was no absolute formula for a temperament most conducive to achieving scientific credibility, even in the limited sphere of the Australopithecine debates. While criticisms similar to the examples above were perhaps the most commonly leveled, their opposites could equally be brought to bear as credibility was

being negotiated. For instance, Robert Broom asserted that bold and brash behavior was just the way a scientist ought to act. Cautiousness and temerity were for Broom not evidence of credibility, but rather of something not worth taking notice of. Broom was contrary to the norm in many respects, but his example will serve to show that the causes of credibility are always perspectival.

iv. Relationships and Correspondence

The difference between the present study of the Australopithecine debates and previous ones might be explained in large part by the difference in the source materials employed. Previous studies have been based primarily on examinations of published texts, with the notable addition of personal recollections by Raymond Dart, who lived until 1983.[14] While I have made use of a large body of published materials, I have also examined and incorporated into the narrative evidence from the personal correspondence and other unpublished papers of many of the historical actors involved. Historians have long recognized that much of what shapes scientific knowledge is not acknowledged, and is sometimes even misrepresented, in print by the authors of published scientific texts. Hence, the use of non-published materials can drastically alter the image we have of the historical development of the sciences.[15]

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[14] Raymond Dart, *Adventures with the Missing Link* (New York: Harper & Brothers, 1959); Bowler's study is based entirely on published sources, while Gundling makes use of some archival material from the United States and England. This study adds a good deal of archival material from South Africa, and the narrative is much more highly driven by non-published material than is Gundling's.

[15] An important model for the way I tried to use personal correspondence in this study is Martin Rudwick's study of the "Great Devonian Controversy" in early Nineteenth Century geology. As in the present study, Rudwick is concerned with how scientific knowledge is shaped through the interactions of a relatively small group of specialists, the nature of which he gleans as much or more from the

With respect to the issue of credibility, the use of personal correspondence in addition to published materials is especially revealing. I have found that during the Australopithecine debates, the same author writing privately to an individual colleague often expressed quite different concerns about a particular claim, or at least expressed such concerns in a very different way, than he did when addressing the whole field in a published text. Further, while participants in the debates (and, I think, scientists more generally) tended to avoid openly questioning the personal credibility of other scientists in print, such concerns were often expressed to other colleagues in private correspondence.

This brings up the issue of personal relationships among the participants in the Australopithecine debates, and how such relationships affected the dynamics of credibility in the debate. From the perspective of the published record, the sciences may seem to develop in a "flat" relational space, with all parties bearing the same relation to one another and all exchanges affecting the entire field in the same way. However, attention to correspondence reveals that the participants in the Australopithecine debates had a variety of different sorts of relationships with their co-participants. Some conducted lengthy correspondences with a particular colleague to whom they felt comfortable revealing things that they preferred not be known publicly, while at the same time having little or no private correspondence with colleagues whom they trusted less with personal disclosures. Some professional relationship developed into friendships, such that concern for the preservation of that

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personal correspondence between the actors as from their published work; see Rudwick, *The Great Devonian Controversy*.

friendship affected what the parties were willing to say publicly about one another's scientific work. Mentor-protégé relationships were present in the debates, bringing issues of respect and loyalty into the negotiation of credibility. Friendship, betrayal, respect, loyalty, even spite – these are not factors usually included in discussions of why scientists hold certain beliefs. Yet if we are to take private correspondence seriously as a source of historical evidence, these common elements of everyday human relationships must be recognized as important factors in shaping how scientists thought about the evolution of our species.

This essay is not structured so as to examine each of themes above and their respective role in the Australopithecine debates in separate chapters; they are too often mixed up together in negotiations of credibility. Instead, the reader will find that these themes, and others, come up in various places and in various mixtures throughout the narrative where the documentary evidence shows them to have been pertinent to the words and actions of the historical actors. The basic organizational principle of the narrative is chronological, with chapters one, two, and three following one another in a basically linear way with little overlap, and collectively covering the period from 1924 to 1946. However, the proliferation of the Australopithecine debate into various semi-discreet sub-debates and controversies in the post-World War II period did not allow for the continuation of a strictly linear chronology. Thus, chapters four, five, and six overlap to a significant degree in the time period they address (roughly 1947-1959),

but each concentrates on different parts of the Australopithecine debate, differing in the issues at stake and the cast of historical actors taking center stage.

The project as a whole is motivated by my fascination with the development of modern scientific accounts of human evolution, and a desire to know how those few humans who claim a special expertise in elucidating that process have gone about determining the route by which our humanity has been achieved.

Chapter 1 – *Australopithecus* as Dart's Ambition, Part I

On February 6, 1924, Thomas Wingate Todd, professor of anatomy at Western Reserve University in Ohio (now Case Western Reserve), wrote to Raymond Dart to congratulate him on his appointment to the Chair of Anatomy at the Medical School of the University of the Witwatersrand in Johannesburg, South Africa, which Dart had then occupied for just over a year. Congratulations notwithstanding, Wingate Todd warned Dart about the difficulties he was likely to face as a young professor looking to establish his place in the scientific community. Having seen an advertisement Dart had placed seeking a lecturer to assist him in his duties, the older professor drew from his own experience to offer a rather disheartening prospect of the obstacles the younger man would likely face as he tried to build up the necessary human infrastructure for the prosecution of successful scientific work:

> At first, I was astonished and not a little chagrined to find that people did not seem to want to join my staff and they were, as I see it now, kind enough, but also brutal enough to tell me why. The statement ran something like this. When a young man ties up his future in any degree with an older man, he thinks of three things: immediate financial returns, opportunity for and stimulus in research, ultimate prospects and influence. The argument then continued to point out to me that I myself was young, could offer no enticing financial rewards, had built up no well known type of research, and had no great influence to place my men later.

Dart's own placement at the University of the Witwatersrand was largely due to the influence of an older scientist with whom he had tied up his future several years earlier, at the close of the First World War. He had worked as a senior demonstrator in anatomy at University College London under the well known professor Grafton

Elliot Smith. Everything whose absence Wingate Todd warned would militate against his ability to attract a scientific staff in Johannesburg had been available to Dart in London. Elliot Smith was recognized for his writings on neuroanatomy and the evolution of the human brain, areas of research that Dart adopted as his own. Further, Elliot Smith's influence was such that he was able to attract funding from the Rockefeller Foundation to promote collaboration in anatomical research between the United Kingdom and the United States, which enabled Dart to travel to a number of American institutions to gain research and teaching experience. Judging by the criteria that Wingate Todd enumerated in his letter, Dart had been well placed in London to advance his fledgling scientific career.[16]

The move to South Africa in 1923 was not exactly equal to the vision Dart had come to adopt for his scientific future. In his autobiography, he recalled having eventually allowed Elliot Smith to persuade him to make the move, but still left England "feeling more like an exile than a man elevated to a professorship." He wondered if the ambitions that had been allowed to grow inside him in such a fertile scientific environment could be fulfilled at his new post: "I hated the idea of uprooting myself from what was then the world's center of medicine and leaving my research and studies with the giants of the profession to take over the Anatomy

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[16] Wingate Todd to Dart, February 6, 1924, Bundle #4, Raymond Dart Papers, University of the Witwatersrand, Johannesburg, South Africa (hereafter RDP). Dart, who was born and raised in Australia, served in the Australian Army's Medical Corps in Europe during the last few years of World War One. This assignment followed the medical studies which he had undertaken at the University of Sydney. He describes his early life and his time at University College London in his autobiography: Dart, *Adventures with the Missing Link*, Ch.3. For a synopsis of Wingate Todd's scientific career see Keith, "Thomas Wingate Todd (1885-1938)." For a biographical account of Grafton Elliot Smith, see Elkin, *Grafton Elliot Smith*.

Department at Johannesburg's new and ill-equipped University of the Witwatersrand." The scene when he arrived confirmed his fears: he found an anatomical laboratory lacking the physical infrastructure that he had come to see as basic to research and instruction in that science, and none of the libraries or museum collections that had surrounded him in London. It was even worse than Wingate Todd feared.[17]

It was in this context, just two years after his arrival, that Dart would come into possession of the fossil skull on which he would base the claim that human beings had evolved in Africa from a genus of man-like apes, to which he would give the name *Australopithecus*. In his autobiography, Dart recalled his state of mind as he prepared for the journal *Nature* the scientific article in which he would announce his claim: "I worked away happily, and I am not ashamed to say, proudly. I was aware of a sense of history, for, by the sheerest good luck, I had been given the opportunity to provide what would probably be the ultimate answer in the comparatively modern study of the evolution of man."[18] Such happiness and pride contrasted sharply with the emotions he had thus far been feeling about his fortunes in South Africa. Dart's happiness would prove short lived: his confidence that he had revealed a crucial stage in human evolution would be met with doubt and sometimes even dismissal from the "giants of the profession" in London, increasing his sense of exile. This chapter follows the first year of scientific controversy over Dart's claim, showing how *Australopithecus* represented not only a possible human ancestor, but also new opportunity for Dart to

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[17] Dart, *Adventures with the Missing Link*, pp.9 and 31. The University of The Witwatersrand had only been granted a full university charter shortly before Dart's arrival there. It had been born out of a succession of technical and mining colleges that had been operating for a number of years before. See Murray, *WITS: The Early Years*.

[18] Dart, *Adventures with the Missing Link*, p.17.

fulfill his scientific ambitions. The debates over the credibility of his claim were, in this way, also debates over Dart's professional standing in science.

Professional Insecurity

Despite being less than pleased at his circumstances South Africa, Dart strove to do what he could to continue with his scientific work and advance himself professionally, but he faced genuine challenges. In the summer of 1924, he to submitted himself as a candidate for the advanced degree of Doctor of Medicine from his alma mater, the University of Sydney in his native Australia. In fulfillment of the requirement that the candidate present a thesis to the Faculty of Medicine, Dart submitted a paper that he had recently published in the *Journal of Comparative Neurology*. It was based on neuroanatomical researches he had conducted while working in Elliot Smith's department in London, and consisted, in the first place, of a highly technical discussion of the nature of the nerve supply to different types of muscle tissue. However, from his neurological investigations Dart went on to make broad inferences about the evolution of the vertebrate musculature and nervous system which put him at odds with much of the received wisdom about the evolution of the nervous system. Scientists had previously argued that mesodermal (striated or voluntary) muscle had evolved earlier than dermal (smooth) muscle, which was derivative of the former. Dart now argued the opposite. He had observed that the mesodermal muscle of a python was innervated by both sympathetic and somatic nerve fibers and the dermal muscle by sympathetic fibers alone. Dart argued that the

mesoderm's possession of a unique neurological trait (somatic innervation) on top of a trait held in common with the dermal muscle (sympathetic innervation), showed the mesoderm to be an evolutionary derivative of the more primitive dermal muscle. This pattern of derivation also put phylogeny in harmony with vertebrate ontogeny, since it had been shown that the mesoderm develops from the dermal musculature during embryological development. Such was the sort of high level discussion of evolution to which Dart had been exposed during his time in England, and in which he was determined to participate with an authoritative voice.[19]

Despite the fact the paper had already been published, Dart was rebuffed by his former university. Several months after making his submission, he received a terse reply from Arthur E. Mills, the Dean of Medicine at Sydney. Mills explained that Dart's thesis could not be accepted as part of the requirements for the degree he sought because its conclusions had recently been shown to be false by John Hunter, a professor in Sydney's Department of Anatomy. He suggested that Dart send some other paper to replace the offending piece.[20]

At the time the thesis was received, Dart's old mentor Grafton Elliot Smith had happened to be in Sydney.[21] Elliot Smith had agreed that Dart ought to substitute some other papers for the original one, and wrote separately to Dart telling him that in addition to withdrawing the thesis, he ought to acknowledge his error in the same journal in which the original article had appeared. As Hunter (a senior and respected

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[19] Dart to Mills, June 10, 1924, Bundle #7, RDP; Dart, "Some Notes on the Double Innervation of Mesodermal Muscle."
[20] Mills to Dart, September 29, 1924, Bundle #7, RDP.
[21] Elliot Smith, like Dart, was an Australian by birth. He maintained ties with old colleagues at the University of Sydney, where he had worked before moving to UCL, and visited periodically.

authority) would be giving a high profile endowed lecture on his findings in New York soon, Elliot Smith advised Dart that "a frank confession of error on your part would be politic and wise." Not only was he telling Dart that he was wrong, but that the best way to salvage some scientific credibility from the situation was to admit it.[22]

Dart was understandably chagrined. Two draft letters found among Dart's papers testify to his complex reaction to having his claims dismissed in this way. In responding to Elliot Smith, Dart adopted a tone of familiarity and modesty, addressing his old boss as "My dear Professor" and apologizing for having put him in what must have been an awkward position. He pronounced himself ready to make whatever changes were called for by the discoveries made in Sydney, but noted that those discoveries had never been communicated to him: "It is a pity, in view of the dissonance in interpretation," he wrote, no doubt having chosen the word 'interpretation' with care, "that the authors have never sent me any reprint or notice of their findings. I shall look forward to seeing their work and doing whatever the circumstances demand." Never deviating from his polite tone, Dart was suggesting that it was not he who was guilty of the offence in this case, but rather those in Sydney who had not followed proper scientific protocol by informing him of their work.[23]

Dart's response to Mills could not have been more different. Whereas he had presented himself to Elliot Smith as calm, confident, and not overly bothered by the situation, the letter to Mills suggests an author trembling with indignation at the treatment he was receiving. His humility replaced by anger and vindictiveness, Dart

[22] Smith to Dart, September 2, 1924, Bundle #7, RDP.
[23] Dart to Smith October 17, 1924, Bundle #7, RDP.

questioned whether the rules of scientific priority had been properly observed: "I do not consider that any researches published subsequently to the approval of my paper for publication by the Editorial staff of the *Journal of Comparative Neurology* (October 1923) could be reasonably regarded as precluding the favourable consideration of my thesis as a thesis." Dart also seethed with anger at what he evidently saw as the presumptive authority of the Sydney professors who opposed his work, and he returned the insult he felt he had received:

> [The] whole thesis is at variance with the conclusions & therefore the work of scientists many of whose names are even more distinguished that those of the workers in the Department of Anatomy in Sydney & consequently if anybody's conclusions should be respected they are those of the more distinguished specialists.

If he was going to have his credibility questioned, then his critics were going to have their scientific standing challenged in equal measure.[24]

Did Dart ever post one or both of these letters? The historical record does not tell us. Nonetheless, they provide a window into Dart's feelings about his own standing in science at this early stage of his career. Which more accurately reveals his reactions, the humble but confident letter to Smith, or the brusque and defensive letter to Mills? The sentiment in both seems genuine, if contradictory. The raw anger and pronounced insecurity that practically jump off the page in the draft to Mills clearly reflect Dart's real feelings. On the other hand, the letter to Elliot Smith, so measured in comparison to the other, suggest a desire not to offend a man who he regarded as a mentor and a friend. Despite the humility of Dart's letter to Elliot Smith, one might

[24] Dart to Mills, October 31, 1924, Bundle #7, RDP.

wonder if anger and insecurity expressed in the draft to Mills stemmed more from the rejection from Sydney, or from a feeling that his mentor had betrayed him by not supporting him rather than his critics.[25] Perhaps the anger he directed at Mills was the displaced anger he felt towards Elliot Smith, but could not bring himself to express. As we will see later, the issue of loyalty in Dart's relationship with Smith would continue to be an issue as the former felt his credibility as a scientist increasingly under attack.

Whatever sense of betrayal he may have felt, Dart conceded the matter and replaced the offending paper with several others, as had been suggested he do, and was awarded the degree he sought. Meanwhile, more than a year had past, and Dart's attention had been diverted by the arrival of a new and seemingly more powerful platform from which he might seek the advancement of his scientific career. In late 1924, Dart came into possession of a crate of fossils that had come out of a lime-filled cave being commercially mined near the town of Taung, west of Johannesburg. Among these were the fossilized face and endocranial cast of what he would name *Australopithecus* – the Southern ape (Fig. 1.1). [26]

A New Platform for Advancement

Having worked alongside Grafton Elliot Smith in London, Dart knew the intense scientific and lay interest that the study of human evolution then enjoyed. During his time there, discussions about the significance of the Piltdown remains

[25] Though Elliot Smith also had ties to the faculty at the University of Sydney
[26] University of Sydney Registrar to Dart, October 14, 1926 & University of Sydney Registrar to Dart, May 3, 1927, Bundle #7, RDP.

abounded in both professional and popular literature, though it had been almost a decade since the remains had been discovered. Dart would have also known the importance of the fact that this fossil was in his hands, and thus that he had the opportunity to set the initial terms of the discussion that would inevitably ensue. The first move was his.

After having cleared away most of the rocky matrix that encased the fossil, Dart penned a short article to the journal *Nature*, the premier scientific publication in Britain, if not the world, describing the fossil and offering his interpretation of *Australopithecus* as an evolutionary ancestor of human beings. When he had mailed off the article, Dart gave the news of his discovery to a friend of his who was also an editor at the local newspaper, the *Johannesburg Star*. In his autobiography, Dart wrote that he had made the editor promise to withhold publication of the news until after the article had appeared in *Nature*, which he expected to be on February 3rd, 1925. Presumably, this measure was taken in order to conform to expectations held by the scientific community that it should have priority in the consumption of new discoveries.

However, Dart's first move did not go quite as he planned. According to his autobiography, the editors at *Nature* decided to delay publication beyond the date that Dart had expected because "the discovery and claims were of so unprecedented a character that the account had been referred to various experts in England who had been asked to give opinions on whether or not it should be published." Having agreed to delay publication only to February 3rd, the *Johannesburg Star* considered its

obligations fulfilled and broke the news in advance of the article's appearance in *Nature*, and thus set off a chain reaction that rippled through the press worldwide.[27]

In a letter to Dart later that year, H.A. Harris, a former colleague from his time at University College London informed him of the ferocity with which the story had been taken up by British the press: "The papers here are indebted to you as the whole affair came out during a quiescent period when Parliament was in recess. Fleet St. let it rip!!!" The appearance of the news in the popular press forced the hand of British scientists who had a stake in the science of human evolution. Like politicians who are forced to comment on an issue raised in the press if they are to remain credible on the subject, scientists known for their work on human evolution would have felt obligated to respond to Dart's discovery in order to maintain an air of public authority. Confronted by *The Times*, Grafton Elliot Smith endeavored to place Dart's find in the context of other fossil discoveries that bore on the question of human ancestry. He testified to the undoubted interest of the fossil, and even linked it to Charles Darwin's well known hypotheses that the evolutionary origins of human beings would be discovered in Africa. So far, despite the mix-up over publication dates, things boded well for Dart. He certainly had the attention of both the press and his fellow scientists.[28]

The *Nature* article appeared just a few days later on February 7, 1925. Dart wrote in it that his fossil belonged to a juvenile individual belonging to "an extinct

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[27] It seems odd that Dart would not have expected this step to be taken, given the commonality of peer-review in scientific publications. Nonetheless, he seems to have expected that the article would appear on February 3rd, and to have been genuinely taken aback by its delay.

[28] Dart, *Adventures with the Missing Link*, pp.23-4 and 33; H.A. Harris to Dart, November 25, 1925, AU8, RDP; "The Taungs Skull. Professor Elliot Smith's View," *The Times* (Feb. 5, 1925) p.11.

race of apes *intermediate between living anthropoids and man.*"[29] Further, he asserted that "[i]t is manifest that we are in the presence here of a pre-human stock, neither chimpanzee nor gorilla, which possesses a series of differential characters not encountered hitherto in any anthropoid stock. This complex of characters exhibited is such that it cannot be interpreted as belonging to a form ancestral to any living anthropoid." Dart was here formally introducing to the scientific community what he claimed to be a very special character in humanity's evolutionary heritage: the prehuman, one who had parted ways with the ancestors of the living apes, and who was beginning the process of shedding the marks of its apish ancestry in favor of a glorious bipedal, big-brained future. [30]

Dart argued for the prehumanity of the creature on several fronts. Following his training in anatomy, he emphasized the several features of the fossil in which he discerned a closer resemblance to humans than to apes. First, he pointed to the shape of the skull, which was relatively long and thin from front to back (a condition anatomists called dolichocephaly), whereas apes' skulls tended to be shorter and wider (brachycephaly). Further, the face dropped downward at a relatively vertical angle, lacking the protruding snout of the apes. Second, Dart the fossil possessed only its milk dentition and first permanent molars, showing that it was a juvenile, and the shape and alignment of the teeth corresponded more closely to those of a young human that a young ape. Third, the contour of the jaw and the arrangement of the teeth along that contour formed a parabolic curve "comparable only with that

[29] Dart's emphasis.
[30] Dart, "*Australopithecus africanus*:," pp.195 and 198.

Figure 1.1 – A frontal view of the Taung Skull from Dart's 1925 article[31]

presented by mankind among the higher primates." Fourth, the position of the foramen magnum, the aperture through which the spinal column passes from the neck into the skull, was positioned farther forward on the base of the skull than in apes, though not so far as in humans. This, Dart argued, was evidence of a creature making the transition to bipedal locomotion. Last on the anatomical front, the endocranial cast pointed to the fact that "the brain in this group of fossil forms was distinctive in type and was an instrument of greater intelligence than that of living anthropoids." While he did not venture an exact value for the cranial capacity, he pointed out that the relative position of two cerebral fissures, called the parallel sulcus and the lunate sulcus, visible on the surface of the endocranial cast, indicated a movement away from

[31] Ibid., p.195.

the anthropoid brain and towards the human. Elliot Smith had previously argued that an increasing distance between these fissures and the corresponding increase in the size of that region of the brain was a crucial pattern in the evolution of the human brain. Dart now drew upon his former mentor's work to show that *Australopithecus* fit well into that pattern as an intermediary between apes and humans (Fig. 1.2).[32]

Dart also argued from an ecological front, describing how an ape-like creature from the Central African jungles would have had to evolve in a human direction in order to have braved the dangerous and barren savannah covering most of the continent's southern regions. He cited a geological study that said that the climatic conditions met with in the present in Southern Africa had not changes appreciably since Cretaceous, suggesting that no mere ape could have survived there. So harsh a climate would have provided just the sort of challenge to survival that would have precipitated the "the higher manifestations of intellect" that were so distinctive of the evolutionary success of human beings.[33]

Dart's argument in favor of Southern Africa as the cradle of humanity stood against the widespread belief that central Asia was more likely to have played that role. His reference to Darwin's prediction regarding the African roots of humanity was no doubt intended to appropriate some authority from the father of evolutionary theory, and to draw eyes to Africa rather than Asia. The latter's candidate for evolutionary fame, was Java Man, or *Pithecanthropus*, named by Eugene Dubois when he discovered its remains in 1890. Dart dismissed that competitor with a bit of

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[32] Dart, "*Australopithecus africanus*," pp.195-198.
[33] Dart, "*Australopithecus africanus*," pp.198-199; Rogers, "Post-Cretaceous Climates of South Africa."

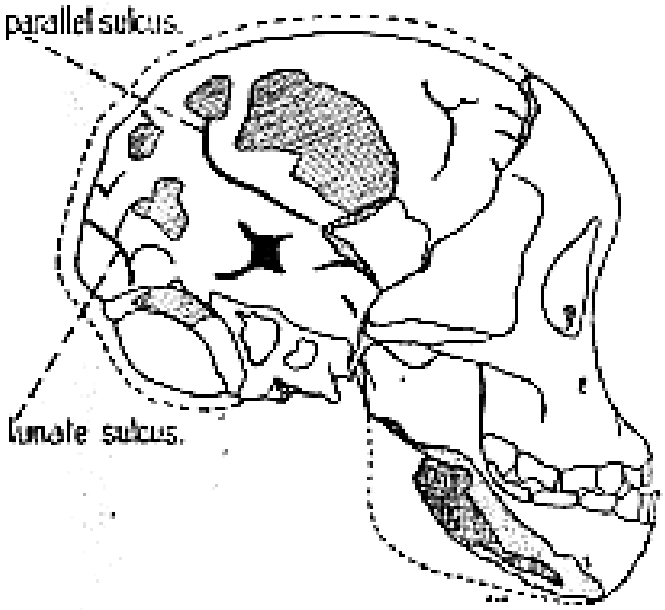

Figure 1.2 – Dart's tracing showing the position of brain fissures in the endocranial cast of the Taung skull[34]

negative campaigning, calling Java man "a caricature of a precocious hominid failure," while hailing his own skull's possession of "just those characters, facial and cerebral, which are to be anticipated in an extinct link between man and his simian ancestor." In order to emphasize the truly intermediate character of the fossil, Dart proposed that an entirely new taxonomic family, the *Homo-simiadae*, be created to accommodate his interpretation of *Australopithecus* as neither an ape nor a human, but a form that bridged the evolutionary gap between the two.[35]

Dart had worked alone to prepare the material for these events, not consulting privately with any other scientists, nor giving anyone of them priority in finding out his news. Perhaps he thought of his behavior as manifesting an impartiality that would

[34] Dart, "*Australopithecus africanus,*" p.197.
[35] Dart, "*Australopithecus africanus*," p.198; For an account of the discovery and reception of Java Man, see Theunissen, *Eugene Dubois and the Ape-Man from Java*.

testify to his scientific credibility. However, H.A. Harris candidly told Dart that the public way in which the information was released may have done harm to his cause in certain quarters. In the same letter as wrote of the British Press's reaction to the news of *Australopithecus*, Harris related that "The people here [at UCL] were very surprised that you had not sent a word to us and they were a little bit annoyed. They felt that it hurt their dignity to see the announcement in *Nature* & the papers… They think they should have had it straight from the stable… [Grafton Elliot Smith] is very enthusiastic about it all but a little hurt underneath I think. This is strictly entre nous." If Harris is to be believed, Dart would have done well to show some partiality towards his old colleagues, especially with respect to his old chief and mentor at UCL. [36]

Why did Dart not inform Elliot Smith and his old colleagues of such an important find? Did they deserve his good graces? One can imagine Dart, upon reading Harris's letter, thinking that Elliot Smith might similarly have shown him more respect in the matter of his doctoral dissertation. Moreover, by the time Harris's letter was written in November of 1925, Dart had been further offended by what he saw as Elliot Smith's failure to stand behind his claim about *Australopithecus*. We will deal with this matter later, but suffice it to say for now that both seem to have expected favorable treatment from the other without necessarily feeling it necessary to reciprocate.

Four leading British experts in the science of human evolution, including Elliot Smith, were invited to comment on Dart's discovery and interpretation in the February

[36] H.A. Harris to Dart, November 25, 1925, AU8, RDP.

14 issue of *Nature*. Two of them took the opportunity to testify to Dart's abilities as a scientist. The anatomist W.L.H. Duckworth wrote that "the fact that the fragments came immediately under notice of so competent an anatomist as Prof. Dart establishes confidence in the thoroughness of the scrutiny to which they have been subjected." Commenting on the significance of brain anatomy to the issue at hand, Elliot Smith said of his former demonstrator that "no one is more competent than Prof. Dart to observe the evidence and interpret it." Despite such praise, the commentators all questioned the evidential basis of his claim that *Australopithecus* was a human ancestor rather than just an interesting new form of ape. Elliot Smith pointed out that Dart had not yet exposed the crowns of the fossil's teeth (since the jaw and skull were fused together in the rock), the evidence of which would allow comparison with other known fossil primates from Asia which were known only by their dental remains. "Until this has been done," he wrote, "…it would be rash to push the claim in support of the South African anthropoid's nearer kinship with man." Arthur Smith Woodward, curator of paleontology at the British Museum of Natural History, wrote that since no skulls had been found of Asiatic fossil apes, "it is premature to express any opinion as to whether the direct ancestors of man are to be south in Asia or Africa," to which he appended, rather dismissively, "[the] new fossil form from South Africa certainly has little bearing on the question." He further regretted that Dart had chosen for his discovery "so barbarous (Latin-Greek) a name as Australopithecus." Three out of four commentators expressed the opinion that geological evidence of the age of the fossil, of which little had been said in Dart's article, was of pre-eminent

importance in determining what could credibly be said about *Australopithecus*'s evolutionary relationships. Arthur Keith, Conservator of the Museum at the Royal College of Surgeons, suggested that those issues that had not been settled in the minds of scientists by Dart's preliminary account would have their deficiencies remedied when Dart published a full monograph on his discovery, which several of the commentators assumed would be forthcoming. So the pre-eminent authorities on human evolution in England set out their demands for Dart's next move if he wanted them to get behind his bold but, according to them, somewhat premature claim.[37]

On the same day these comments appeared in *Nature*, Keith published a separate commentary on Dart's discovery in *The British Medical Journal* entitled "The New Missing Link". Paradoxically, Keith here gave even more reasons to doubt whether *Australopithecus* was a missing link than he had in his commentary for *Nature*. He heaped praise on Dart and the importance of his discovery. describing Dart's announcement of the find as having "fallen like a bombshell on anthropological Europe," and stating that "there can be no doubt of the importance of Professor Dart's discovery." So important, in fact, that "it is a discovery which places Professor Dart's name in the front ranks of students of the evolution of man and anthropoid." But what exactly had Dart discovered? In his article Dart had stressed many features of the skull that he described as more human than ape, and these had underlain his argument for *Australopithecus* being ancestral to humans. All of these, however, Keith summed up in a few dismissive lines:

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[37] Arthur Keith, G.Elliot Smith, Arthur Smith Woodward, W.L.H. Duckworth, "The Fossil Anthropoid from Taungs," *Nature* 115 (February 14, 1925), 234-36.

> The face in all its lineaments is that of an anthropoid; there are blended in it some features of the chimpanzee, others of the gorilla, and some which belong to neither. But of humanity there is no trace save in one respect – its jaws are smaller and its supraorbital ridges less developed than in a chimpanzee of a corresponding age… Further, the milk canines are less pointed than are those of the young chimpanzee, and the interdental space in front of the upper canines is less.

One might wonder whether all this constituted "no trace of humanity", one trace, or four. Regardless, according to Keith's rhetoric any sign of embryonic humanity was easily dismissible before the apishness of the face. In addition, Dart had stressed that the position of the foramen magnum, the opening through which the spinal column passes into the brain, indicated an upright posture in *Australopithecus*. Keith did not mention Dart's argument on this feature; he did, however, comment on the likely stature the creature held: "We cannot be certain of posture until we find a bone of the lower limb. One cannot see any character in the skull which justifies the supposition of an erect posture." As he and the other commentators had done in *Nature*, Keith made the specification of geological age a requirement for establishing the relation of the Taung skull to the ancestry of humanity: "To have any claim to stand in or near the human line of descent it must be able to claim an early Miocene date at least." The possibility of Dart's skull being of that age was dim, for Keith asserted that the limestone caves in which the skull had been found "may not be older than late Pliocene or early Pleistocene", thus in all probability relegating *Australopithecus* to the rank of apish cousin in the human family tree. In a few short pages, Keith had reinterpreted much of Dart's evidence for the ancestral status of *Australopithecus* as evidence that it was an ape with little relevance to human ancestry. That Keith

thought it was a particularly interesting ape would have been of little comfort to Dart.[38]

How is it that Keith believed he had the authority to assert the skull's "apishness," when Dart possessed the crucial physical evidence? It was certainly true that he was among the most recognized scientists in the field, and that he held an appointment at a prestigious institution. Keith suggested that the material resources conferred by his presence at the Museum of the Royal College of Surgeons gave him a particular advantage over Dart in the matter of interpretation. He wrote that "those who have charge of much larger collections of anthropoid and human skulls and brains than were at the disposal of Professor Dart have a somewhat unfair advantage over him." For anyone that might suggest that Dart's possession of the skull in question was an advantage at least equal in weight, Keith wrote that Dart's drawings and descriptions were "[so] exact and clear… that those who have studies his preliminary account in *Nature* have all the data placed at their disposal for coming to an independent opinion." Taking Keith at his word, one must surmise that in his opinion it was Dart's artistic and descriptive skill that was going to launch him to fame in the study of evolution, rather than his lackluster interpretive acuity. Keith's words outlined a very different demand from those that had been issued in *Nature*. There, the commentators had asked for more evidence if Dart wished to make his claim credible. Here, Keith implied that the limitations of Dart's institutional and geographical setting precluded him from producing a credible interpretation of the

[38] Keith, "The New Missing Link," pp.325-26.

skull he had discovered. Given Dart's earlier worries about leaving London for the ill-equipped University of the Witwatersrand, we can imagine that Keith's comments cut to the bone. The situation that had allowed him to come into possession of the fossil also undermined his ability to interpret it credibly.[39]

Until this point, Dart had been alone among students of human evolution in strongly asserting the prehumanity of *Australopithecus* in print. In the April 18 issue of *Nature*, he gained an ally in the South African paleontologist Robert Broom, known for his work on the early evolution of mammals as evidenced by South African fossils. Evidently, Broom had been convinced of the essential rightness of Dart's interpretation of the skull from the moment he read of it. The day after Dart's article had appeared, Broom wrote him a letter telling him how "blessed by fortune" he had been to come into possession of such a specimen, and that his "missing link is really glorious." He inquired if he might visit Dart in Johannesburg in order to "pay my respects to my distinguished ancestor," which he subsequently did.[40]

Broom began his note to *Nature* by indicating that, unlike the critics from abroad, he had seen and examined the skull itself. Lest anyone doubt the fact of his experience, or the propriety of his pronouncements in public, Broom made explicit that "Prof. Dart not only allowed me every facility for examining the skull, but also gave me with almost unexampled generosity full permission to publish any observations I made on it, and further suggested that I send to *Nature* any notes that might amplify the account he had already given." Broom began by addressing the

[39] Keith, "The New Missing Link," p.325.
[40] Broom to Dart, February 8, 1925, AU8, RDP.

issue of the geological age of the skull, information which he recognized as "the first demand" of scientists abroad. He related at some length his knowledge of the geological formations of the region, and gave a physical description of the caves at Taung, the name of a nearby village, concluding that "it can be safely asserted that the Taungs skull is thus not likely to be geologically of great antiquity – probably not older than Pleistocene… At present, all we can say is that the skull is not likely to be older than what we regard as the human period."[41] Keith had made similar conclusions, which he took to be evidence against the possibility of *Australopithecus* representing a form relevant to human ancestry. However, in a thinly veiled shot at Keith's position, Broom asserted that "the age of the specimen in no way interferes with its being a true 'missing link,' and the most important hitherto discovered." In other words, Broom had not bowed to the idea that a Miocene age was required in order for *Australopithecus* to fall within the boundaries of human ancestry.[42]

The majority of Broom's account was taken up by a comparison of various anatomical features between Dart's skull and those of apes and humans, throughout which he echoed Dart in drawing the reader's attention those parts in which he saw *Australopithecus* drifting away from the ape and towards the human. Perhaps the most significant addition he made was a more detailed discussion of dental anatomy, as Elliot Smith had asked for in his commentary. It is interesting to note that in the end, Broom judged that, anatomically speaking, *Australopithecus* was "nearer to the

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[41] Early discussions of Dart's skull referred to the local of it discovery as "Taungs," though the actual name of the village was "Taung." The mistake was not rectified in the scientific literature until years later. I will be using the corrected version when writing in my own voice, while leaving the incorrect version in place when quoting directly from texts.

[42] Broom, "Some Notes on the Taungs Skull," 569-71.

anthropoid apes than man," as distinct from the Piltdown skull (*Eoanthropus*), which, while also possessing both ape and human characters, Broom placed closer to the human than the ape. However, this did not prevent him from advising readers that "[w]e seem justified in concluding that in this new form discovered by Prof. Dart we have a connecting link between the higher apes and one of the lowest human types," namely, the type represented by Piltdown, which even Keith took to be a true ancestor of modern human beings. To make his position all the more visible, Broom included in his note the first phylogenic "tree" diagram to include *Australopithecus*, whose name marks the tree at a point above the divergence of the great apes from the human line, but below, and hence ancestral to, all living and extinct hominids (Fig. 1.3). Thus, Broom kept the prehumanity of *Australopithecus* in play, adding a second voice from South Africa to contest the critical pronunciations from Britain.

The June 6th, 1925 issue of *Nature* saw Dart make his first rejoinder to his critics in print since he published his initial report in that journal several months before. He did not use the opportunity to defend his substantive characterization of *Australopithecus*, as Broom had done for him in April, against the alternatives that had been proposed. Rather, the short note defended the word *Australopithecus* against the charge made by some, most prominently Woodward, that such a designation was an improper hybridization of a Latin prefix meaning "southern" with the Greek word for ape. Dart invoked the authority of a professor of classics at the University of Witwatersrand, who had assured him "that *pithecus* was a recognized naturalized Latin word in Rome." Further, in case Woodward or anyone else should spurn the

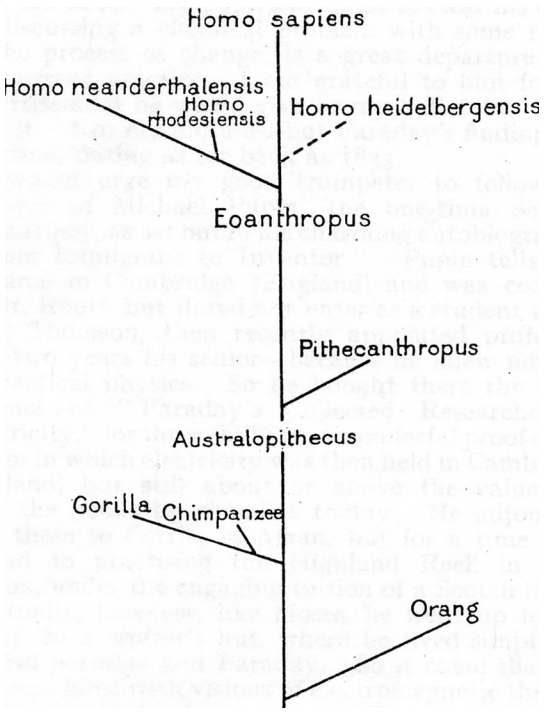

Figure 1.3 – Broom's phylogeny of apes and humans[43]

authority of a mere scholar from a small colonial university, Dart wrote that the word *pithecus* "was used by Cicero's own secretary Tiro and by other accredited writers, and more than a century before Cicero's time Plautus employed the diminutive *pithecium*. It is, therefore, not surprising that both of these words are to be found in a standard Latin dictionary, such as that of Lewis and Short." Critics like Woodward had mistakenly thought that they were questioning only the humanistic learning of one man when they refused the name *Australopithecus*; Dart sought to set them straight by showing that they in fact took on the authors of classical Latin themselves.[44]

[43] Ibid., p.571.
[44] Dart, "The Word 'Australopithecus' and Other," p.875.

In the very next issue of *Nature*, Dart garnered another valuable ally in the contest, this one from England. W.J. Sollas, Oxford professor, influential geologist and author of the book *Ancient Hunters,* had acquired a sketch of a sagittal section through the Taung skull from Broom, which, he wrote, "is extremely interesting and completely confirms, so far as it goes, the statements of Prof. Dart." As Broom had done, Sollas began his short article by giving explicit thanks to those (namely, Dart and Broom) who had afforded him the material for study and the permission to publish his thoughts at will. These published expressions of gratitude began to define a party of individuals interested in arguing the prehumanity of *Australopithecus*, now including Sollas. "It is abundantly clear," he wrote, "that in a number of significant morphological characters… Australopithecus makes a nearer approach to the Hominidae than any existing anthropoid ape." Sollas had made this determination by comparing the section provided by Broom with sections of chimpanzee skulls preserved in the University Museum of Oxford – comparisons that he sketched for readers so they might see for themselves. Keith had previously demeaned Dart's conclusions by casting doubt on the resources he had at his disposal with which to make a conclusive comparison. Could the same be said of the collections of Oxford University?[45]

But while these exchanges were taking place in the pages of *Nature*, Dart was preparing to make a move through a different medium – that of public display. His

[45]Sollas, "The Taungs Skull," pp.908-909

actions would further complicate the effort to build credibility in the scientific community.

Australopithecus at Wembley

A little more than a month after the announcement of *Australopithecus*'s discovery appeared in *Nature*, Dart was sent a letter from Kenneth Harvey, the secretary of the British Empire Exhibition Committee for the Union of South Africa. The Exhibition – the largest event of its kind ever staged – had then already been underway at Wembley for just under a year. Harvey suggested to Dart that "the exhibition of a cast, or casts, of the Taungs skull in the South African pavilion at Wembley this year would be of considerable interest, and I am to ask if you are prepared to assist my Committee in this direction." If Dart were prepared to donate, rather than loan, such casts to the committee, they were prepared to assist him in the costs of preparation.[46]

Dart was sympathetic, but made very clear that he would only agree to proceed if a number of conditions were met. First, he would need to prepare casts of each of the two fragments of the specimen (the face and the endocranial cast) separately and also joined together, as well as a model of the "soft parts" of the creature to show what it might have looked like in life. Dart indicated that if the committee would subsidize the undertaking to the amount of £20, he was prepared to do the work. Second, upon the closing of the Exhibition, the display should either be given to the South Africa

[46] Harvey to Dart, March 12, 1925, AU8, RDP

House in Trafalgar Square, or placed on permanent loan to the British Museum of Natural History at South Kensington. Third, Dart emphasized to Harvey, "such an exhibit may not be copied without my consent."[47]

The Committee apparently agreed to Dart's terms, and he began to prepare the materials for exhibition. By mid-May of 1925, the papers reported that it was being prepared, and Arthur Keith wondered when he would be able to get his hands on the precious objects. "Don't forget the R.C.S [Royal College of Surgeons] and its conservator. Let me know as soon as you can where casts may be purchased," he wrote to Dart, anxious that he be given prompt access to all the evidence.[48]

In his reply, Dart blamed his circumstances for the delay in circulating casts of the skull and his as-yet unwritten full monograph on the subject. The casts he had made up on short notice for Wembley, he told Keith, were such not as such as "I would like to be in the hands of people like yourself." Further, he had recently been made Dean of the School of Medicine at the University of the Witwatersrand, which left him with more administrative responsibilities and less time for research and writing. "If I had the resources of London at my disposal these things could have been done long ago," he wrote, echoing the judgment that Keith himself had previously made. It seems that distance was proving a hindrance to both: Keith had all the scientific resources of London at his disposal and wanted only the Taung skull, while Dart had the skull but lacked the resources and the time to make full use of it.[49]

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[47] Dart to Harvey, March 21, 1925, AU8, RDP
[48] Keith to Dart, May 13, 1925, AU8, RDP
[49] Dart to Keith, June 3, 1925, AU8, RDP.

Broom had also read in the papers of the plans to exhibit the skull at Wembley. He had also read reports of the opposition of a number of British scientists, including Keith, to the claim that *Australopithecus* stood in the line of human descent: "I see by the papers that some the English Scientists – whether out of jealousy I know not – are trying to make light of your missing link," he wrote to Dart. He was especially upset that Elliot Smith, who he presumably felt ought to have been on board with his former student's interpretations, had expressed doubts about the claim of ancestry. Indeed, in an article entitled "'Missing Link' still to be found," *The Times* had recently reported on a lecture given by Elliot Smith in which he echoed Keith's view that if Dart had had access to more comparative materials (such as that available to scientists in London, as Dart well knew), he would have realized that the Taung skull showed features nearly identical to those of infant gorillas and chimpanzees.[50] Under the mistaken impression that Dart planned to send the specimen itself to England, Broom implored him not to give their opponents too much ammunition until he had established his claims more authoritatively in the scientific literature:

> Send a cast if you like but don't let the skull out of yours hands and I really think you should not send it to London at all. Do a big paper on it here. After the sneers of the English I don't think you ought to favour them by anything but casts or photographs. Lie low, keep quiet and do a fine big paper with all details. You have the goods – stick to them. Give the English & Scots professors no further information till you get out your [illegible word] paper with all the facts.[51]

Broom understood the importance of priority in the establishment of scientific claims, and since possession is nine tenths of the law, Dart risked the credibility of his claims

[50] *The Times* (May 23, 1925), p.18.
[51] Broom to Dart, June 3 1925, AU8, RDP.

should he let the skull out into the world without the guidance of a full published account to entrench its identity as a human ancestor.

Dart, of course, had never intended to let the originals out of his grasp. However, when the casts arrived and were installed at Wembley, the presence of the originals was not required for other scientists to move in on Dart's priority. In a letter dated June 4, 1925, the Exhibition Commissioner of the South African section, one E.H. Lane, wrote to Dart from England informing him that the exhibit had been installed and thanking him for all his work. Lane also informed Dart that the he had been in touch with Elliot Smith, who had taken the liberty of making some alterations to Dart's original design. "I think you will agree with me," wrote Lane, "that it is a matter of great congratulation to us that we have been able to secure the advice of this able and influential gentleman." Lane told Dart that when Elliot Smith came to view the exhibit, he brought with him a number of skulls and brain casts of apes and humans, including the brain casts of the famous Java Man *Pithecanthropus*, in order "to illustrate the growth of the human brain" which Elliot Smith took to be the primary pattern of human evolution. Lane also informed Dart that Elliot Smith was in the process of writing up a brochure to accompany the exhibit, which would be available for the edification of visitors. Finally, on the subject of the reconstructions of *Australopithecus* in the flesh which Dart had provided, Lane suggested some further adjustments to the exhibit:

> They are, I understand, built up on the supposition that the being whose brain is represented walked upright. This is, I believe, your theory. In the event of anybody holding the theory that your discovery belonged

> to a being who did not walk erect, have you any objection to a plaster cast being introduced to demonstrate that aspect?

Lane emphasized that he would not take the latter step before hearing Dart's view, and that such suggestions were in no way meant to be prejudicial against his credibility: "Please do not think that I wish in any way to minimise the very valuable work that you have done, or detract from the exhibit as it now stands." How it then stood, however, was already quite different from what Dart had intended.[52]

Dart did not appreciate the "help" he was receiving at Wembley. In a draft letter, with many crossed out and re-written sentences, Dart was at pains to find the right words to express the proper degree of indignation. "I am glad the material arrived safely," Dart finally wrote, "but I am astounded that that a copyrighted exhibit should have been tampered with and given a character other than that elaborated and intended by the exhibitor." He objected to the fact that the exhibit had been turned into an explanation of human evolution, when in fact he had wanted "to show the Taungs material & something South African only." But what did Dart really want? His claim here seems rather unconvincing, as he had included in the display a diagram echoing Broom's phylogeny which showed *Australopithecus* in a position ancestral to all modern and fossil forms of human beings. No doubt Dart resented that someone *else* was asserting control over how the material was to be interpreted. "I should have appreciated your consulting me in this matter and also in that of issuing a brochure

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[52] Lane to Dart, June 4, 1925, AU8, RDP. A copy of the brochure written up by Elliot Smith, also found among Dart's papers (AU8), seems to take a fairly sympathetic view towards Dart's interpretation of the Taung skull, especially in that it is subtitled "Man's Nearest Relation". In any event, Elliot Smith refrained from repeating his criticism reported in the papers (see note 29) that the skull most resembled those of infant chimpanzees and gorillas. No reference is made to the question of the creature's bipedalism.

prior to their execution," wrote Dart. Since these changes had already been made, he could only make reference to the offence after the fact, but the matter of the reconstructions was still pending: "I do not feel that the South Africa Pavilion forms the proper milieu for the discussion of the propriety or otherwise of the reconstructions carried out under my supervision & I am glad that you have waited to hear from me in that regard." The careful politeness of Dart's prose hardly concealed the frustration he felt as others took control over the fate of his discovery.[53]

The reconfiguration of the Wembley exhibit was not the first time that Elliot Smith had intervened in Dart's scientific endeavors in ways that the latter did not appreciate. As we have seen, in the matter of Dart's submission of a thesis to the University of Sydney for the Doctor of Medicine, Elliot Smith had supported the faculty's decision to reject Dart's thesis due to their disagreement with his findings, and he had been reported in the papers as lecturing against the claim that *Australopithecus* was a missing link. How did Dart feel about all this? A letter from Elliot Smith to Dart (found among the latter's papers) suggests that he felt ill-treated. "I am sorry that you seem to have misunderstood my actions both in Sydney last year and here [in England] during the present year," wrote Elliot Smith, evidently in response to a complaint he had received from Dart," claiming that he was only trying to help:

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[53] Dart to Lane, July 22, 1925, AU8, RDP. Once again, since this was a draft letter, and since I have not been able to locate any response from Lane, or any repository of letter received by Lane in England, I cannot be entirely sure that Dart ever sent it. Nonetheless, it demonstrates Dart's sensitivity towards acts that he saw as challenge to his dignity as a scientist. Further, since the brochure says nothing about alternative reconstructions of *Australopithecus*, I suspect that the non-bipedal version was not introduced into the exhibit, which may in turn indicate that Dart's letter was received and that his wishes expressed therein were respected.

> In both cases I was trying to keep up your end and doing what I could to help your real interests. A good deal of malicious criticism of the Taungs papers was going on here last session, and I imagined that I could help you best not by indiscriminate acceptance of the whole of your claims (which would have invalidated my contribution) but by adopting the attitude of serious appreciation and constructive criticism. I am afraid you have not realized that my sole aim was to back you in what I conceive to be the only effective way.

Exactly what he understood to be the difference between "malicious" and "constructive" criticism Elliot Smith does not say, but it seems that he believed too much credulity to be anathema to the scientific ethos. In other words, Elliot Smith worried that by showing too much trust in Dart, he would render both of them less trustworthy. Only by appearing to work against Dart could he work for him.[54]

Much of what Elliot Smith called the "malevolent criticism" of Dart's claims had been coming from Arthur Keith, and the Wembley exhibit – both its contents and location – only inflamed Keith's opposition. In a letter to Dart, Keith warned him in advance that he was sending a note to *Nature* "grumbling at having to visit Wembley & summing up dead against your theory that Australopithecus is in any sense a missing link or intermediate type." Nonetheless, he attempted to end the letter on a friendly note. "I know how much you have on your shoulders," he wrote, acknowledging Dart's apology and explanation for not having made casts or a longer memoir available to the scientific community. And lastly, "give our united love to Mrs. Dart for if we quarrel about fossil apes we shall not quarrel over our wives."[55]

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[54] Elliot Smith to Dart, November 26, 1925, Bundle #4, RDP.
[55] Keith to Dart, June 30, 1925, AU8, RDP.

When Keith's letter to *Nature* appeared on July 4, however, it was clear that he was unwilling to take the forgiving stance in public that he had in private. He and his colleagues, he wrote, had been patiently waiting for a chance to examine exact casts of Dart's specimens before they came to a definite conclusion regarding the status of *Australopithecus*. "For some reason, which has not been made clear," he continued rather disingenuously, since Dart had in fact explained the delay to him, "students of fossil man have not been given an opportunity of purchasing these casts." As result, specialists were forced to "visit Wembley peer at them in glass case" along with the masses. However, despite the unprivileged position from which he was forced to direct his gaze, he could see enough to write confidently that "the casts exhibited at Wembley will satisfy zoologists that [Dart's] claim [that the skull is that of a human ancestor] is preposterous."[56]

At the same time as Keith asserted the outlandishness of Dart's claims from an examination of the contents of the display, he complained that the very same content was insufficient to see for himself the basis on which Dart had made his conclusions. Keith told the reader that the display carried a drawing of the skull alongside the cast, showing clearly the position of a certain brain fissure that Dart had claimed in his original article was evidence for the prehumanity of *Australopithecus*. However, Keith protested that "when we examine the brain cast at the site where the fissure is shown on the drawing, we find only a broken surface where identification becomes a matter of guess-work." Beyond the evidence at hand in the display, Keith reiterated

[56] Keith, "The Taungs Skull," p. 11.

his call for more geological evidence before a judgment of the meaning of the fossil was decided. On this front, he appropriated Broom's judgment that the caves at Taung were of relatively recent date, taking this as further confirmation of his original position. Keith did not mention that Broom thought his judgment of recent geological age no argument against *Australopithecus's* prehumanity at all. Rather, for Keith, the characterization of *Australopithecus* both as having lived relatively recently and also as a human ancestor was tantamount to claiming "a modern Sussex peasant as the ancestor of William the Conquerer." Dart had to cease pressing for more than was his due, leaving matters of human evolution to those with legitimate evidence: "[Dart's] discovery throws light on the history of anthropoid apes but not on that of man. Java-man (*Pithecanthropus*) still remains the only known link between man and the ape, and this extinct type lies on the human side of the gap." If the gauntlet had not already been thrown down, it was now.[57]

The duel was set for the September 26, 1925 issue of *Nature*, and letters to the editor chosen as weapons. As many a politician has done on the campaign trail, Dart began by claiming to be confused as to what his critic's position really was. Why, Dart asked, had Keith published an article in February's *British Medical Journal* with the title "The New Missing Link," only to claim in his latest letter to *Nature* that any claim to such status for *Australopithecus* was "preposterous"? In addition to "this reversal of opinion," Dart wondered why Keith had overlooked several of the anatomical features of the skull judged to be human-like by Broom and Sollas, whose

[57] Ibid., p.11.

names Dart used liberally to adorn his defense. How was it that Keith thought he could invoke Broom's judgment of the geological age of the skull as evidence against Dart's claim for the ancestral status of *Australopithecus* when Broom himself in no way shared that view? "I take it… as a mark of his personal favour," Dart wrote sardonically, "that Sir Arthur should have attacked my utterance and spared Dr. Broom's." And what of Keith's inability to see a vital sign of *Australopithecus*'s prehumanity as he gazed through the glass at Wembley? "The fact that Sir Arthur was unable to find the *parallel sulcus*[58] depression in the replica cast sent to Wembley," Dart wrote, "illustrates how unsatisfactory the study of the replica can be in the absence of the original," which, of course, had not left South Africa. Dart closed his letter with a rhetorical flourish:

> Sir Arthur need have no qualms lest his remarks detract from the importance of the Taungs discovery – criticism generally enhances rather than detracts. Three decades ago Huxley refused to accept Pithecanthropus as a link. To-day Sir Arthur Keith regards Pithecanthropus as the only known link. There is no record that Huxley first accepted it, then retracted it, but history sometimes repeats itself.

Dart's strategy was both diplomatic and subversive. Who would doubt the greatness of Huxley as a figure in the history of evolutionary thought? Yet the passage of history demanded that even great men be superseded by a new generation. The wheels of history had not ceased to turn.[59]

In his rejoinder, Keith did his best to keep Dart's conception of history at bay by rolling out his own cast of allies, and by throwing doubt on Dart's priorities in

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[58] The technical name for the brain fissure in question.
[59] Dart, "The Taungs Skull," p.462.

reasoning about the skull. Several current scientific names, along with their analyses of variation in the morphology of ape skulls, were deployed to counter Dart's claim that *Australopithecus* ought to be viewed as more than an ape. Keith also showed that he, too, could dig into the history of the life sciences to find help in a present contest. In Keith's view, Dart's attempts to justify the creation of a new family for *Australopithecus* were superfluous and confusing because, from what he saw in the identifying anatomical features of the skull (so far as it was available to him), it was manifest that "the group to which this fossil ape belongs has been known and named since the time of Sir Richard Owen." Despite these claims on the anatomical front, it was becoming clear that Keith's primary defense was to be grounded in geology, where Dart and his allies were having trouble mustering positive evidence. Keith questioned whether Dart was aware of proper protocol in matters of establishing evolutionary lineage, for "[b]efore making such a claim [as to the ancestral status of *Australopithecus*] one would have expected that due inquiry would first be made as to whether or not the geological evidence can justify such a claim." Were Dart's values really proper for one engaged in the science of human origins? "From his letter," Keith counseled readers, "one infers that Prof. Dart does not set much store by geological evidence. Yet it has been customary and I think necessary, to take the time element into account in constructing pedigrees of every kind." Far from the new generation overcoming the orthodoxy of the old, the younger scientist was showing

signs of a lack of scientific discipline that should make readers of *Nature* think twice before getting behind Dart and his supposed missing link.[60]

In criticizing what he saw as Dart's under-appreciation of the geological facts, Keith made reference to the fact that Dart's colleague at the University of the Witwatersrand, the geologist Robert Young, had lately made a study of the geology of the limestone caves at Taung. In his paper on the subject, Young had been circumspect about assigning a date to the limestone deposits in the caves, but seemed to be of the opinion that they were probably not older than the Pliocene, and possibly only dated from the Pleistocene. Further, he opined that the *Australopithecus* remains had likely been washed into the caves and thus embedded in the nascent deposits, such that the fossils could be considered of the same age as the limestone. All this Keith took as further confirmation of his view that since other, more modern human types were already in existence at this time, that *Australopithecus* could not be an ancestral link.[61]

The meddling and criticisms of British Scientists had turned the Wembley exhibit into a liability for Dart. Dart's past association with some of the same scientific authorities who were now working against him made the cold reception in Britain hard to swallow. However, Britain's scientific community was not the only forum in which *Australopithecus* had generated interest.

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[60] Keith, "The Taungs Skull," pp.462-63.
[61] Young, "Calcareous Tufa Deposits."

Australopithecus in America

Amidst the many letters that Dart received in the weeks following the appearance of his initial article in *Nature* was one from Aleš Hrdlička of the Smithsonian Institution in Washington D.C. Hrdlička congratulated Dart on his find, and told him that he was already planning to undertake a scientific tour that year to India, Java and Australia, and would like to add South Africa if he could find a way to cover the extra transportation costs.[62]

Hrdlička's proposed visit represented an opportunity for Dart gain an ally by allowing another scientist to examine the original evidence and possibly testify to his interpretation. This would serve as ammunition against the British scientists who voiced their doubts without ever having seen the actual fossil; Dart had already gained Broom as an ally in this manner. However, whereas Broom had assured Dart of his support before ever having seen the fossil, there was no guarantee that Hrdlička's visit would convince him of the veracity of Dart's claim. There was thus a risk involved in allowing him to come, a risk compounded by Hrdlička's stature in American physical anthropology. In addition to his position at the prestigious and well-endowed Smithsonian, Hrdlička was the editor of the *American Journal of Physical Anthropology*, an influential organ for the field. It could very well help carry *Australopithecus* decisively into human ancestry, or add considerable weight to the body of doubt already facing Dart's claim.

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[62] Hrdlička to Dart, Feb 21, 1925, AU8, RDP.

Hrdlička did end up adding South Africa to his list of destinations, arriving in the summer of 1925 as Dart's exhibition went on display at Wembley. Dart gave him access to the specimen, and helped him secure access to the lime caves out of which the skull had been blasted, suggesting that Dart was optimistic about the outcome of the visit. However, the American scientist's pronouncements in a long article for the *American Journal of Physical Anthropology* were largely equivocal. Though he wrote an extensive description of the geological situation in which the fossil had been discovered, he stopped short of making any judgment on the age of the cave deposits, the main sticking point for Keith's objections to Dart's position. His table of cranial and facial measurements contained more quantitative information about the fossil than had been published by any other investigator, including Dart, and yet he stated only that "in all probability it is a new species, if not genus, of the great apes." As for the ancestral status of the type represented by the fossil, Hrdlička declined to judge in the absence of adult specimens, writing only that we might "say with confidence that it is doubtless *a* missing link, one of the many still missing links in the realm of Primate ancestry." Hrdlička's visit thus provoked little change in the debate.[63]

Around the time Hrdlička's article was published, another opportunity for Dart to influence the debate came from a different American source. However, rather than have an American scientist travel to South Africa and report back as Hrdlička had done, this opportunity was for Dart to send an article of his own to be published in an American scientific periodical. The invitation came from William K. Gregory, curator

[63] Hrdlička, "The Taungs Ape," pp.379-392.

of comparative anatomy at the American Museum of Natural History in New York, and it was to contribute a piece on *Australopithecus* for the journal *Natural History*, published by the Museum. Here was a chance for Dart to put forth his own case for the ancestral status of *Australopithecus,* rather than rely on the unpredictable judgments of another scientist.[64]

Dart now shifted strategies. Geology had proved a hindrance, and his attempts to secure allies via anatomy were proving vulnerable in the absence of more specimens. Both had given critics and reticent allies the opportunity to state that there were unsatisfactory grounds on which to ascribe any human evolutionary significance to *Australopithecus*. In order to secure new footing, Dart went back to an element of his argument in his original report to *Nature* that had largely been neglected in the debate thus far. Dart had drawn attention to the fact that *Australopithecus* was the only anthropoid ape, living or fossil, that had been found south of the great forest of central Africa. Between the current haunts of gorillas and chimpanzees and the quarry near Johannesburg lay a vast stretch of desert and savannah, impenetrable to the quadripedal, arboreal apes of Central and West Africa. This geographical feature, he argued, could be shown through geological investigation to have been present for a long stretch of geological time. Given this, how could an "ape" have traveled so far south? Here, Dart argued in his new article, lay the solution to an early step in human evolution.

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[64] Gregory to Dart, February 1, 1926 & Gregory to Dart, March 4, 1926, Bundle #4, RDP.

Dart began with a characterization of the dichotomy separating man and ape: "Man is an omnivorous, bimanous, and bipedal, almost erect, terrestrial mammal. The highest living anthropoid apes are frugiverous, quadrumanous (or quadripedal), semi-arboreal, semi-erect mammals." If, he continued, it was true that both man and the apes arose from a totally arboreal primate, as was generally believed, then there would have been required, according to the principles of evolution, a process by which the ancestors of man became semi-arboreal like the apes, and then completely terrestrial, before attaining the state currently enjoyed by our species. Since no living species existed as an analogy to this fully terrestrial ancestor, as the great apes were to the semi-arboreal ancestor, it had theretofore existed only as far as it could be inferred from evolutionary theory. However, Dart had recently changed all this:

> The Taungs remains show that this second phase was a living reality; they demonstrate that man did not arise "Minerva-like in full panoply" as a sudden sport from some semi-arboreal ape, but that the anthropoid achieved human status by laborious passage through the terrestrial man-ape phase; finally they indicate, if they do not actually prove, the quarter of the earth upon which this penultimate act in the drama of humanity was staged.

Here, rather than in geology or anatomy, lay the script in which the significance of *Australopithecus* could be read, and Dart meant to finally reproduce the "drama of humanity" that would demonstrate that fact to the skeptical scientific community.[65]

The original producer of this drama was the rough environment of Southern Africa, which (he argued) had existed since the Cretaceous. After all, why bother evolving if your species is cozy and comfortable in the fertile, edenic womb of the

[65] Dart, "Taungs and its Significance," p.315.

jungle, with shelter and fruit aplenty? Out on the open desert and plain, on the contrary, one needs the means to flee quickly or defend against "the fearsome carnivorous enemies of Primates, whose possession it was." Thus,

> [i]t is obvious, *prima facie*, that the Australopithecoid group which forced this barrier into the remote Southland had evolved an intelligence (to find and subsist upon new types of food and avoid the dangers and enemies of the open plain) as well as bodily structure (for sudden and swift bipedal movement, to elude capture) far in advance of that of the slothful, semi-arboreal, quadripedal anthropoids.

Like his imagined Australopithecoids, Dart too was having to adapt to an unfriendly environment, intellectual rather than ecological, by adopting strategies better suited to survival. He was, after all, directly subject to the scientific exigencies of Southern African Nature, while his British colleagues, in their established intellectual environment, did not feel the pressure to change in the face of new challenges such as that posed by the Taung skull.[66]

Why was this line of argumentation only emerging in full now? Dart advised his readers that he "was unable to devote more than passing attention to it in my preliminary discussion of the Taungs man-ape." That said, he went on to lament that the resulting neglect of this factor was doing a disservice to the fair evaluation of his position:

> I feel that certain of the criticisms that have been made of my conclusions have been prompted by a failure to appreciate the presence of this [geographical] barrier and the unquestionable nature of the results which the barrier and the terrestrial life, to which it committed [the *Australopithecoids*], must have had upon the bodily form of an anthropoid group which, prior to their leaving the forest, were semi-arboreal in habit.

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[66] Ibid., p.317

Just how important was Dart now claiming the environmental factor to be? So important that he concluded with the astounding judgment that were he never to have come upon the *Australopithecus* remains that had occasioned all this debate, the existence of such a creature in Southern Africa might have been inferred from the environmental argument alone![67]

This whole line of argumentation was designed to deflect attention away from the unprofitable arenas of geology and anatomy. However, Dart did not fail in the rest of the article to once again address the by now conventional objections to his conclusions. Against the idea that the remains were washed into the caverns from the surface, he presented an argument that *Australopithecus* was a troglodyte, which both explained the location of the remains and foreshadowed the living circumstances of early humans. Against the charge of geological recency, he invoked the uncertainty of any such determination on current evidence. And regarding the comparative facial and cranial anatomy of the *Australopithecus* and the living anthropoid apes, Dart, unsurprisingly, produced diagrams and measurements that supported his position and undercut Keith's dismissal. If Dart could neutralize the opposition with such arguments, he might be able to shift to debate onto a more favorable field of engagement.

However, if a 1927 review of Dart's article in Hrdlička's *American Journal of Physical Anthropology* is any sort of guide, Dart's latest effort had gained him no new ground at all. Hrdlička described the piece from *Natural History* as "[a]n article in

[67] Ibid., p.318

which the author ingeniously, but it seems obvious more or less artificially, endeavors to humanize the 'Australopithecus.'" As far as the wider reaction to the article among interested scientists, Hrdlička wrote euphemistically that if "any other student who gave truly earnest and critical attention to the otherwise very interesting and important Taungs skull" had been positively moved by Dart's argument, he had not heard about it.[68]

Indeed, the rest of the scientific community had not thus far been greatly moved to adopt Dart's interpretation of the *Australopithecus* fossil. After having come to South Africa feeling pessimistic about the scientific prospects such a place could offer him, Dart believed that the discovery of *Australopithecus* would vault him to the position in science that he ambitiously envisioned for himself. However, those at the top of the professional hierarchy in the science of human evolution did not permit him to join their ranks on this basis alone. His claim had been an ambitious one, and they demanded that the steps taken in support of this claim be commensurate with its extraordinary nature. Some of those steps were evidential, such as the comparison of the skull with other specimens (which Dart did not possess), and a better determination of the geological age of the fossil (which seemed unlikely to emerge). But the critics also wanted Dart to take certain steps with respect to the distribution of information to the scientific community. We have seen how Keith complained that specialists had not been given access to casts on which they could base their opinions. In addition to casts, it was expected that final conclusions should

[68] *American Journal of Physical Anthropology* 10 (1927), p.130.

await Dart's production of a monograph length study of the fossil. If Dart wanted credibility for his ambitious claim, then he would at least have to adhere to certain norms of scientific conduct. The next chapter takes a closer look at Dart's thorny engagement with such procedural concerns, and the consequences for his own and *Australopithecus*'s place in science.

Chapter 2 – *Australopithecus* as Dart's Ambition, Part II

In late November of 1926, a little over a year since their spat over the Wembley exhibit, Arthur Keith wrote to Dart with a proposal. Would it not be possible, he asked, for Dart to come to the meeting of the British Association for the Advancement of Science in Leeds the following year so that they could "have a Royal Row over Australopithecus?" Keith expressed the opinion that the South African government might even be persuaded to pay Dart's way as a delegate to the meeting. Further, Keith suggested playfully, a trip made in person would allow Dart to bring the original specimens along "to convince we thickheads" of his claims regarding *Australopithecus*'s ancestral status. [69]

One must doubt whether Keith actually thought his own position thick-headed, but the fact remained that the crucial evidence remained half a world away in Dart's hands, and Keith was willing to cater to the opposition if it meant he would get a chance to examine the fossil in person. For his part, Dart knew that as long as he held the fossil evidence he retained a certain amount of control over the debate. In fact, probably unbeknownst to Keith, Dart had already turned down the opportunity to travel abroad in connection with *Australopithecus*. The Witwatersrand Council of Education had written him offering to subsidize a research trip to England for the purposes of making a study of relevant comparative material, the dearth of which in South Africa had already led to criticisms that Dart would be unable to make a

[69] Keith to Dart, November 23, 1926, Bundle #5, RDP.

satisfactory judgment on the zoological position of *Australopithecus*. However, Dart was unwilling to accede to their one condition, which was that he surrendered ownership of the skull to the University. In his autobiography, Dart admitted that his failure to take up this offer may have ultimately hurt his ability to convince the scientific community of his claims, but at the time he could not bring himself to give up the one trump card he felt he retained.[70]

Neither did Dart take up Keith's suggestion that he attend the 1927 BAAS meeting in Leeds. Indeed, it would be several years before the original skull made its way to England to be examined by Dart's critics. In the interim, interested scientists clamored for casts of the specimens and a full, monographic study from Dart before they would grant credibility to his claims. This chapter examines how the dissemination of casts, photographs, and texts between 1926 and 1931 affected the fortunes of Dart's claims about *Australopithecus*'s ancestral status, as well as his professional status as a scientist. What objects and texts Dart allowed to flow outward into the hands of others and whose hands received them affected the flow of credibility back to Dart. As we will see, Dart ultimately failed to build a network of support sufficient to entrench his claims for *Australopithecus* as scientific facts. The period ends at a low point for Dart and his ambitions, leaving *Australopithecus* in obscurity as the first phase of its story comes to close.

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[70] Dart, *Adventures with the Missing Link*, p.51.

The Business of Casting

In the months following the publication of his note on the discovery of *Australopithecus* in *Nature*, Dart received numerous requests from investigators around the world for casts of the fossil. In his replies, a seemingly overwhelmed Dart pleaded with his correspondents to be patient and forgive the time it was taking him to disseminate the materials through the scientific community, as his many duties and limited resources at the University of the Witwatersrand prevented any faster action. As we have seen, he did manage to send one set of casts abroad for exhibition at the British Exhibition at Wembley, though the cast was not of such quality as Dart thought necessary for the purposes of scientific research. This action led to Arthur Keith's jealous outburst in *Nature,* complaining of Dart's decision to share his fossil wealth with the general public before the scientific community.

Since Dart did not bring his fossil to England in 1927 as Keith had hoped he might, the only cast of *Australopithecus* available in that country remained the one sent for the Wembley Exhibition. Upon the closing of the Exhibition, the casts and photographs that made up Dart's exhibit went to the British Museum (Natural History) in South Kensington, as Dart had directed they should be. This was too much for Keith, who could not bear that an institution other than his Museum at the Royal College of Surgeons (and investigators other than himself) be the first in Britain to possess the casts of *Australopithecus*. Writing to Dart in September of 1927, Keith abandoned the strategy of wooing Dart with light-hearted familiarity, instead appealing to Dart's loyalty and reminding him of the unrequited debt he owed to the

Museum at the Royal College for the resources it had afforded him in his London days. "It isn't myself I'm thinking of when I beg a copy of the Australopithecus cast," wrote Keith,

> it is the Museum which you know & which has been of some use to you & will be to many others. I do think deserves as well of you as South Kensington does – and has a history behind it which every Britisher may be proud of. So even if the copy you could deposit on loan is not what reaches your ideal it would be welcome until the final thing is in the market.

As Keith indicated in his letter, Dart had not been happy with the quality of the casts he had so far succeeded in producing, using this as an excuse for the delay in providing Keith and others with the casts they desired. In spite of this, Keith's imploring must have succeeded in wearing Dart down, for just a month later Keith reported gratefully to Dart that his Museum had finally received its own cast of the *Australopithecus* skull.[71]

Dart's gift to Keith did not signal that *Australopithecus* casts had become widely available by the end of 1927. Rather, the donation to the Royal College, like that to the Wembley Exhibition, seems to have been a special concession of Dart's part while he continued to withhold any mass production of casts due to his lack of resources and inability to produce casts of a quality that satisfied him. Several years earlier (in fact only shortly after the announcement of the original discovery in 1925) Dart had been contacted by a man who, rather than seeking a cast for himself,

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[71] Keith to Dart, September 14, 1927, Bundle #5, RDP; Keith to Dart October 27, 1927, Bundle #5, RDP. I have not been able to determine with certainty the provenance of the cast given to the Royal College of Surgeons in 1927. I strongly suspect that it was made from the originals in South Africa rather than copied from the casts at the British Museum, as there is evidence that these latter were only copied for the first time at a later date, as will be shown.

proposed that he be permitted to produce and distribute casts of the *Australopithecus* fossils on Dart's behalf, offering a potential solution for Dart's difficulties. Frank Oswell Barlow held the position of Technical Assistant at the British Museum (Natural History), a position he inherited from his father. Additionally, Barlow first worked for, and eventually came to own, the private firm R.F. Damon & Co., which produced and distributed models and casts of fossils as a profit-making business. It was in this latter capacity that Barlow wrote to Dart suggesting that the fossil be entrusted to him for reproduction. Barlow had already taken charge of the reproduction of a number of the most famous specimens of human prehistory known to science, including the Gibraltar Skull (a Neanderthal specimen), the fossil fragments that comprised the Piltdown remains, and the skull of Rhodesian man, the most well known specimen bearing on humanity's prehistory in Africa up until Dart's discovery. Dart was receptive to Barlow's suggestion, but thought it might be most prudent, given the value of the specimen, to wait until he had the chance to bring to England in person.[72]

However, nearly a year following this initial exchange of correspondence Dart had not made any arrangements to bring either himself or the skull to England, leading Barlow to suggest that Dart send some casts of his own making to England for Barlow to reproduce in larger quantities. Like Dart, Barlow was receiving numerous requests

[72] For biographical information on Barlow, see Keith, "Frank Oswell Barlow: 4 October, 1880 – 12 November 1951." Upon inheriting his father's position in the Geology Department of the British Museum (Natural History), Barlow official title was "mason". It was only in 1928, after some difficult budgetary negotiations, that the position of "Technical Assistant" was specially established in recognition of Barlow's special abilities and contributions to the Museum. I have not been able to locate the initial letter that Barlow sent to Dart, nor the letter that Dart sent in return. Their contents are broadly inferable from Barlow's reply to Dart: Barlow to Dart April 18 1925, AU8, RDP.

for casts of the Taung skull from eager scientists, and tried to use the demand to spur Dart to action: "I have constantly to reply to enquiries concerning these specimens and assure you that you would be conferring a boon on a great number of anthropologists in making copies available."[73]

By the beginning of 1928, though he had made casts available to Arthur Keith as well as to some of his colleagues in South Africa, Dart had not succeeded in producing casts that he thought of sufficient quality to be reproduced and distributed *en masse* by Barlow. In his capacity as an employee of the Natural History Museum, Barlow had been able to examine the casts which had been sent there at the close of the Wembley Exhibition, and he suggested to Dart that these were really not so poor as their maker seemed to think. Additionally, their readiness at hand would allow copies to made beginning right away to avoid further delays in getting casts onto the open market. As he had with Keith, Dart relented in his desire for better casts and declared himself ready, in mid 1928, for Barlow to proceed with the work using the casts at the Natural History Museum. They agreed that Dart would also send sculpted reconstructions of *Australopithecus* in the flesh (one with flesh covering the entire head and neck, and another with half of the skull exposed beneath the flesh) to be reproduced and sold along with the fossil casts.[74]

Having agreed that Barlow ought to proceed with the reproduction of the *Australopithecus* material, it remained only to set the prices at which the casts would be offered and the percentage of that amount that Barlow would pay to Dart in

[73] Barlow to Dart, March 15, 1926, AU8, RDP.
[74] Barlow to Dart, February 18, 1928, AU8, RDP; Dart to Barlow, June 29, 1928, AU8, RDP.

royalties. This turned out to be a matter of some delicacy. Upon Barlow's inquiry as to what price Dart thought should be applied to the casts, he had replied that the reproductions of the items at the Natural History Museum ought to be offered at £10 each, with the full and partial reconstructions to be offered at £5 each. The Natural History Museum held three separate casts – the facial fragment and endocranial casts joined together, the facial fragment alone, and the endocranial cast alone – and Barlow interpreted Dart's figure to mean that he intended that a set of all three casts should be offered at £10, with a complete set including both reconstructions offered at £20. However, after a further exchange of letters, Barlow learned that Dart had actually meant that the cast of the facial fragment and endocranial casts joined together should alone be priced at £10, with the separated pieces costing a further £5 each. Along with the two reconstructions, this would have brought the cost of the entire set to £30. It would be impossible for him to offer the casts at the lower price, wrote Dart, as he had already charged several customers in South Africa the higher price and did not wish to be seen as having defrauded them in any way.[75]

Barlow took several months to reply to this revelation from Dart, confessing that the delay was "partly the result of the natural inclination to put off that which it is unpleasant to do." "The fact is," wrote Barlow, "I cannot bring myself to advertize the Taungs casts at the prices you propose." He implored Dart to recognize that he was quite experienced in such matters, and that to offer the casts at the higher prices would actually result in lower total returns by killing the demand. By way of justification,

[75] Barlow to Dart, August 3, 1928, AU8, RDP; Dart to Barlow, August 29, 1928, AU8, RDP.

Barlow enumerated the prices at which he sold casts of the Rhodesian Skull and the Piltdown remains, both far lower than that which Dart proposed to charge for the Taung skull. At base in all this lay the fact that Barlow was the owner of a private business from which he derived a large part of his income, and to charge such exorbitant amounts "would create in my customers a feeling of resentment which I am not disposed to incur." In the matter of Dart's South African customers, Barlow suggested that whatever resentment they might feel towards Dart might be mitigated by reminding them that they had had the opportunity to acquire the specimens years before anyone else, and that this fact in itself might be thought of as worth the extra cost. Additionally, there would be the opportunity to offer those who had paid the higher price a discount on whichever items they did not already possess.[76]

In the face of Barlow's forceful yet sensitively worded letter, Dart backed down and accepted the lower terms. The fact was that Dart's ability to meet the international demand for casts of *Australopithecus* on his own was severely limited, and the lack of availability already threatened to further undermine his credibility with other investigators. The value of Barlow's help in the matter of production and distribution was simply too great to give up over a few pounds. For his part, Barlow pronounced himself relieved that Dart had received his suggestions with such equanimity and assured him that work would begin at once. Thus, by the end of 1928

[76] Barlow to Dart, October 17, 1928, AU8, RDP.

the terms had been set to finally make *Australopithecus* available to all who could spare £20.[77]

A few months after the business end of the deal with Barlow had been agreed upon, Dart succeeded in separating the mandible (lower jaw) from the rest of the skull, to which it had been cemented by the rocky matrix in which the fossil had been found. This opened up the possibility of casting the crowns of the upper and lower dentition. Teeth represented (and still represent) a highly important diagnostic resource for paleontologists. Due to their material properties, teeth are preserved in more quantity than bones, and hence a large proportion of criteria used to classify extinct creatures and determine evolutionary affinities have long been focused on dental anatomy. The newly exposed teeth of *Australopithecus* would prove vital in the evolution of the debate over its evolutionary significance.

Dart made casts of the upper and lower dentition and sent a pair along to Barlow for him to reproduce and include in the set of *Australopithecus* casts for sale. Perhaps tired of wrangling over price and resigned to the fact that Barlow was the expert in such matters, Dart left it to him to decide on what additional cost should be added to the set in view of the new additions.

Dart also sought to capitalize on the new evidence afforded by the exposed dentition in terms of its value for his argument about the human character of *Australopithecus*. Putting good relations with his most important scientific patrons

[77] Dart to Barlow, November 7, 1928, AU8, RDP; Barlow to Dart, November 28, 1928, AU8, RDP.

before monetary gain, he instructed Barlow to make copies of the dental casts available at his (Dart's) expense to Keith and Elliot Smith.

The Importance of Teeth

Shortly after Dart had sent casts of the separated upper and lower jaws of the skull to Barlow in England, he had the opportunity to reinvigorate the debate by showing the original specimens themselves to an important audience. The occasion was the annual meeting of British Association for the Advancement of Science for 1929, to be held in South Africa. This was the second time that the BAAS had come to South Africa to hold it annual meeting; the first was in 1905.

It did not bode well for Dart when in the Presidential Address that opened the South African Meeting, the South African politician Jan Hofmeyr failed to mention either Dart or the Taung skull in his speech entitled "Africa and Science," though a good many other discoveries, and even other skulls, formed the basis of the talk.[78] Nonetheless, on Friday August 2, 1929, Dart did get the chance to exhibit the original skull and give a summary of his position, which was later published in the *South African Journal of Science*. He took the opportunity to deliver a polemic against the objection that the geological position in which the skull had been found made it too recent to be considered ancestral to human beings, and to defend the priority of his own expertise in making the necessary judgments. "The actual geological age," said Dart, "does not affect the question of the proximity in relationship of the Taungs ape

[78] Hofmeyr, "Africa and Science."

to man." How then should that relationship be determined? "The question of the nearness in relationship of *Australopithecus* to Man can only be determined by its anatomy." Dart the anatomist then summarized at great length all the points on which he had previously argued his position, adding the new observations on the dentition made possible by the exposure of the crowns of the teeth. He concluded confidently that "[a]s far as the present state of knowledge goes, Australopithecus fulfills every known criterion of ancestral relationship to humankind. The Australopithecidae gave birth to the Hominidae, and South Africa was the land of their travail."[79]

Dart was not pleased with the reactions of his foreign colleagues. As he recalled in his 1959 memoir *Adventures with the Missing Link*, while "some [scientists at the meeting] examined and made noncommittal comments, it was obvious that few regarded it as anything of real importance in the evolutionary story." Seeing the specimens first hand was evidently not enough to bring the scientific community around to Dart's point of view.[80]

While the exposure of the teeth may not have impressed the attendees of the BAAS, photographs of the dentition did succeed in gaining Dart an important new ally in William K. Gregory at the American Museum of Natural History. Gregory, with whom Dart had corresponded in 1926 regarding his contribution to *Natural History,* was a renowned expert on the comparative dental anatomy of both living and extinct vertebrates. Shortly before the discovery of *Australopithecus*, he had gathered together a series of his papers into a monograph entitled *The Evolution of the Human*

[79] Dart, "A Note on the Taungs Skull," pp.649 and 658
[80] Dart, *Adventures with the Missing Link*, p.54

Dentition, which traced the evolution of human teeth from early, ocean-dwelling ancestors through the origin of the Mammals, then the Primates, and finally to modern human beings. If Dart could convince Gregory, who was not in attendance at the South African meeting of the BAAS, that *Australopithecus* had its place in this procession of human dental evolution, it would be a great advance for the credibility of his position.

In late 1929 Dart sent a set of photographs of the exposed dentition to Gregory in New York. "In consequence of your great interest in teeth," wrote Dart, "it occurred to me that you would like to have copies of the photographs… I think you will be stuck by the 'human' characters of the dentition." Gregory had been on an expedition in Africa under the auspices of his Museum when the photographs arrived, but when he returned they seemed to have the effect on him that Dart hoped they would. Gregory wrote to Dart that he and his colleague, Dr. Milo Hellman, had been "astounded at its 'human-like' characters," and he predicted that upon the fuller publication of Dart's position "many of those who have dismissed Australopithecus as just another anthropoid will be discounted in the opinion of those best qualified to judge impartially." Gregory further offered to showcase *Australopithecus* at several scientific meetings in the coming months, and inquired if he could have Dart's permission to show the photographs. Here was a chance for Dart to have an

influential and apparently sympathetic scientist act as an American spokesperson for his position.[81]

Dart gave Gregory permission to do what he liked with the photographs, and his generosity paid off. Gregory exhibited the photographs as part of a presentation he made in April of 1930 before a joint meeting of the American Association of Anatomists and the American Association of Physical Anthropologists, entitled "The Origin of Man from a Brachiating Anthropoid Stock." Gregory had been involved for the past five or so years in a debate concerning human origins with the man under whom he had long worked at the American Museum of Natural History, the palaeontologist Henry Fairfield Osborn. Beginning around the time of the discovery of *Australopithecus* (though seemingly unrelated to that event), Osborn had become convinced that the idea of an "ape-man" stage in human evolution, which had been central to theories of human descent at least since Darwin published his theory, was false. This did not mean that Osborn denied that apes and humans shared a common ancestry. Rather, he only denied that the line of human descent had passed through a stage in which our ancestors had closely resembled the apes. As a group, argued Osborn, the living and fossil apes were defined by specialized adaptations for climbing and swinging – that is, for life in the forest. Citing the Belgian palaeontologist Louis Dollo's law of the irreversibility of evolution, Osborn argued that once these specializations had evolved, it was impossible that they could subsequently be

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[81] Gregory, *The Origin and Evolution of the Human Dentition*; Dart to Gregory, October 18, 1929, William King Gregory Papers, American Museum of Natural History, New York (hereafter WKGP); Gregory to Dart, January 14, 1930, WKGP; Gregory to Dart, February 14, 1930, WKGP; Dart to Gregory, March 17, 1930, WKGP.

transformed into the anatomical equipment necessary for the human mode of life. Osborne's position was that it was more conceivable that the human form had evolved independently from an earlier, more primitive ancestor whose anatomy had not yet become so specialized as to preclude the potentiality of human evolution. The fact that humans and apes resembled one another in many respects was due, according to Osborn, to the inheritance of similar evolutionary potentialities from their common, primitive Primate ancestor.[82]

Osborn was a powerful figure in American science and had recently aired his views in his address as the retiring President of the American Association for the Advancement of Science at its annual meeting. Gregory felt compelled to counter what he thought of as the harmful effect of Osborn's conception of human evolution on science. Firstly, Gregory objected to what he was as the narrowness of Osborn's interpretation of Dollo's law. While it was true that once an evolutionary step had been taken, such as an organ being lost, it was highly improbably or even impossible that it should be regained. However, to Gregory this did not mean that evolution could not change direction, for instance, if a change in an organism's environment precipitated a change in the function of a particular organ. Thus, just because at a certain point in evolutionary history the ape's hand had been adapted for brachiation (movement by swinging suspended from the forelimbs held above the head) did not mean it was forever fated to be adapted only for that purpose. For Gregory, the

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[82] For a concise statement of Osborne's position in this matter, see Osborn, "Is the Ape-Man a Myth?"; for broad accounts of Osborn's, and to a lesser extent Gregory's, role in the development of the sciences of prehistory in the United States, see Rainger, *An Agenda for Antiquity* and Regal, *Henry Faifield Osborn*.

evolution of humans from a brachiating anthropoid stock (the title of his paper) facilitated rather than hindered the problem of human origins, for it gave provided a plausible scenario for the evolution of an upright spine, thus laying some of the anatomical groundwork from which bipedalism could have evolved. Secondly, Gregory objected to the fact that Osborn's mode of reasoning seemed to undercut the foundations of paleontology as grounded in Darwinian evolution. By any measure, the apes resembled humans in more traits than did the more primitive Primate ancestors that both shared. Such degrees of resemblance were the chief resource that paleontologists had to work with, and the only resource with which paleontologist could be expected to infer the relationships of species according to the central principles of Darwinian evolution. If Osborn's reasoning were to prevail, wrote Gregory, "then what sort of morphological evidence would ever be accepted as evidence of genetic affinity?"[83]

If, Gregory continued, humans had evolved through an ape stage, then scientists would be wanting evidence of a fossil form that betrayed such an ancestry while moving in the direction of such peculiarly human traits as bipedal locomotion. This brought him to *Australopithecus*. Of course, there were no limb bones available to show whether or not the creature had walked upright, but being an expert on teeth and having Dart's photographs at hand, Gregory shifted the discussion to dental anatomy. In their general form, Gregory argued that *Australopithecus*'s dental anatomy closely resembled that of the fossil Miocene ape *Dryopithecus*, of which

[83] Gregory, "The Origin of Man from a Brachiating Anthropoid Stock," p.647.

many teeth had been found. Secondly, when compared with the teeth of chimpanzees, gorillas, and humans with respect to twenty six separated characters, *Australopithecus*'s teeth were common to all in three cases, nearer to those of the gorilla in zero cases, nearer to those of the chimpanzee in two cases, nearer to both the chimpanzee and the gorilla in one case, and nearer to those of humans in twenty cases. Thus, according to the evolutionary principles which he had defended against Osborn, Gregory asked his audience "if *Australopithecus* is not literally a missing link between the old dryopithecoid group and primitive man, what conceivable combination of ape and human characters would ever be admitted as such?... *Australopithecus*, to judge from its skull and dental characters, was a pioneer in the new line, as held from the first by Dart."[84]

The respective debates in which Dart and Gregory were embroiled were not identical, nor were their interpretations of the *Australopithecus* fossil. This fact is demonstrated by a letter from Dart to Gregory in which he disagrees with the latter's position that *Australopithecus* was derived from the Miocene form *Dryopithecus*. However, such differences did not get in the way of good relations. As Gregory wrote to Dart, "These differences of interpretation between us arise inevitable (sic.) from the fact that our respective points of view and fields of training are and have been different. It is gratifying to realize, however, that sometimes even a radical difference in point of view and a lively criticism of opposing theories may coexist with sincere personal friendship and regard." Despite any differences, Dart's fossil had brought the

[84] Ibid., p.7.

two scientists together, and the relationship had served Dart well in his battle to win recognition for *Australopithecus* as a human ancestor.[85]

Perils of Publication

Dart's geographical isolation from the world's centers of scientific work made his network of professional relationships all the more important for the diffusion of knowledge about *Australopithecus*. His correspondence with Gregory allowed him to establish a scientific ally in America who could spread the gospel of *Australopithecus* without setting off across the Atlantic. As we have seen, the men who Dart would have liked to have as allies in England had not been as supportive of his views as Gregory was now showing himself to be. He had been especially stung by his old chief Elliot Smith's failure to support him more strongly and presumptuousness in meddling in the exhibit at Wembley. In 1926, after the Exhibition had closed, Elliot Smith sought to reassure Dart that he stood on his side. Several years previous, just prior to the discovery of the Taung Skull, Elliot Smith had published a collection of essays on human evolution. Now he was releasing a second edition of that work in which, he wrote Dart, he would "try to and put straight before the public here your case in respect of Australopithecus." Hoping that he might now receive some much needed support such influential quarters, Dart replied that Elliot Smith's "kind offices

[85] Dart to Gregory, February 28, 1930, WKGP; Gregory to Dart, May3, 1930, WKGP.

with respect to the Taungs problem in 'Essays of the Evolution of Man' will be greatly appreciated by myself."[86]

When the new edition of Elliot Smith essays was published in 1927, it turned out that his claim to be making Dart's "case" was somewhat of an overstatement. He did offer some support by writing that *Australopithecus* seemed "to reveal definite evidence of a nearer kinship with Man's ancestors that any other Ape presents." Furthermore, he endorsed Dart's argument that this "ape" had left the forests for life on the open savannahs, a likely prerequisite to bipedalism. Yet he continued to call *Australopithecus* an ape, refusing to go along with Dart in arguing that there was evidence that the creature had achieved an erect posture. More important, by imputing "nearer kinship" with the evolutionary ancestors of human beings to *Australopithecus*, Elliot Smith conspicuously denied it membership within that ancestry. If this was Dart's case, it was certainly a circumscribed version.[87]

Dart does not seem to have complained to Elliot Smith about the less than spectacular endorsement in the latter's book, but Elliot Smith was sensible to the fact that Dart had not always been happy with the way his old mentor had treated him. In an exchange of letters in 1928, Elliot Smith pronounced himself pleased that Dart had kept up their correspondence, as he feared that Dart might have perceived some slight in his actions: "I need not assure you that your interests I have always tried to serve to the best of my ability, but I have at times feared that something I may unwittingly

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[86] Elliot Smith to Dart, April 27, 1926, Bundle #7, RDP; Dart to Elliot Smith. May 26, 1926, Bundle #7, RDP.
[87] Smith, *Essays on the Evolution of Man*, pp.62-67.

have done or something I have left undone may have been misinterpreted. Hence I am very much relived to get your letter."[88]

Whatever offence Dart may have taken at Elliot Smith's actions, he continued to consider the maintenance of their good relations important to his own scientific standing. This was not least because they had previously agreed that when Dart completed his monograph length work on the Taung skull, he would submit it through Elliot Smith to the Royal Society of London for publication in the prestigious *Philosophical Transactions*. In addition to the clamoring for casts, scientists had been for some years badgering Dart about the appearance of his full analysis of the skull. In response he would often cite his lack of resources and heavy workload as Dean of Medicine at the University of the Witwatersrand, just as he had done to explain the long wait for casts. However, by mid 1929 Dart was writing to Elliot Smith assuring him of the imminent arrival of his manuscript. Time was of the essence, as Elliot Smith was currently chairing the committee at the Royal Society that dealt with publications, an appointment which would expire in November.[89]

More than just the publication of Dart's monograph was at stake. In what must have been perceived as a most encouraging gesture to Dart, Elliot Smith had offered to nominate his younger colleague for Fellowship in the Royal Society. Elliot Smith thought it best to delay that action until Dart had submitted the manuscript on *Australopithecus*. As Elliot Smith wrote, "I shall do everything I can to push it, but the best inauguration it can have would be as a companion of the chief work upon

[88] Elliot Smith to Dart, March 23, 1928, Bundle #4, RDP.
[89] Elliot Smith to Dart, August 1, 1929, Bundle #4, RDP; Elliot Smith to Dart, September 16, 1929, Bundle #4, RDP.

which the claim for you will be based." This prize, held out before Dart, would make the disappointment of what was to follow all the more devastating.[90]

Dart was happy to be relieved of the manuscript which had been hanging over him for five years. Writing to Gregory shortly after having shipped it off to Elliot Smith in England, Dart predicted that the monograph "should see the light of day some time during the present year. At any rate," he continued, "I have relieved myself of it & given it a good riddance." Freed of that burden, Dart planned to treat himself to the journey of a lifetime by accompanying an Italian scientific expedition on an overland trek from South Africa to Egypt (truly "Cape to Cairo"), through the Near-East and Asia Minor and finally across the Balkans and Central Europe to Italy. From there, Dart would continue on to England, where he hoped to arrive by the beginning of 1931. This would presumably allow him to be present to witness the early reactions to the publication of his magnum opus. He arranged for his wife Dora to meet him in Naples and accompany him the rest of the way to England, bringing with her the original Taung remains to be unveiled in concert with the monograph.[91]

Before Dart could set off across Africa and truly bid "good-riddance" to his monograph, however, a letter arrived from Elliot Smith with less than encouraging news. Elliot Smith had read through Dart's work, and though he called it a "great achievement," he was unwilling to put forth the monograph for publication in its present form. It was simply too big. In his opinion, "both the Royal Society and the Anthropological Institute would refuse to publish a paper of the size of your

[90] Elliot Smith to Dart, December 20, 1929.
[91] Dart to Gregory, March 17, 1930, WKGP; Dart to Gregory, April 26, 1930, WKGP.

manuscript." Elliot Smith suggested to Dart that "the cogency of your argument and the impressiveness of the evidence would be definitely increased if drastic cuts were made" to the sprawling 259 typed pages that Dart had sent to England. Mixing praise with implicit criticism, Elliot Smith continued by writing that Dart's work was "so important" that "it should be presented in the crispest and most readable form, and that any matter which is not directly relevant to the important issues you have to set forth ought with advantage to be eliminated." Elliot Smith was careful to intersperse encouraging words even as he pointed out the deficiencies of Dart's work, as he himself had a stake in seeing it put into publishable form rather than withdrawn completely. For more than a year, he had been pushing the Royal Society to allow space in the *Philosophical Transactions* for Dart's authoritative report on the Taung Skull. His own credibility was thus to some degree tied up in Dart's success. It was perhaps for this reason, combined with his unenthusiastic reception of Dart's initial effort, which led him to ask Dart to give him "a free hand to deal with your report so as to make it acceptable to the Royal Society." By way of reassurance, Elliot Smith told Dart that another scientist (with whom Dart was acquainted) had previously given him the authority to edit his manuscript freely in a similar situation and the result had been a favorable reception by the Royal Society.[92]

I have not been able to locate Dart's reply to Elliot Smith's entreaty, but evidence that Elliot Smith subsequently submitted the manuscript to the Royal Society (after stating that he was unwilling to do so without substantial changes being made)

[92] Elliot Smith to Dart, April 16, 1920, Bundle #4, RDP.

suggests that Dart agreed to allow Elliot Smith to do what he thought was needed. However, even Elliot Smith was apparently unable to put the manuscript in a form that the Royal Society could see fit publish in full. Instead, the Assistant Secretary wrote to Elliot Smith to tell him that they could no "see their to way to recommending for publication more than Section 4…which deals with the Teeth."[93]

This was the situation when Dart arrived in London after his long journey. In his autobiography, Dart recalled that upon his arrival in London, he hurried to get in touch with Elliot Smith, Keith, and Smith Woodward. When he did so, "[t]hey were all friendly and hospitable but were much more interested in telling me about the recently discovered Peking Man remains than in listening to my story." At the close of 1929, W.C. Pei, the Chinese director of the excavations at Chou Kou Tien Cave near what was then Peking, unearthed a remarkably complete skull, which was quickly correlated with the hominid remains that been found previously in the cave under the new generic designation *Sinanthropus*, but known more affectionately as Peking Man.[94] Remains of these creatures, which were thought to be allied to Dubois' *Pithcanthropus* from Java, had been recently discovered in China for by Chinese scientists and by the Canadian anthropologist Davidson Black. In August of 1930 (shortly after the Royal Society had rejected all but the section on the dentition of Dart's monograph), Elliot Smith had travelled to China at Black's invitation to see the excavation at Chou Kou Tien first hand. In a long survey article of 1931 on all the

[93] Assistant Secretary of the Royal Society to Elliot Smith, July 4, 1930, AU8, RDP.
[94] For an account of the Peking Man discoveries written by a Chinese scientist who took control of the excavation in the 1930's, see Lanpo and Weiwen, *The Story of Peking Man*.

discoveries related to Peking Man, Grafton Elliot Smith had the following emotional revelation to make to his readers:

> In attempting to give you some idea of the impression made upon my mind during this mission of ancestor worship in China, I find it somewhat difficult, without using adjectives that seem extravagant, to express my feelings when I saw the remains of the most primitive member of the human family so far recovered.[95]

Now everyone, including Dart, would have agreed that *Sinanthropus* and *Australopithecus* were not competing for the same spot in the human family tree: the latter was clearly more ape-like and would have to be regarded as an earlier phase of human evolution that *Sinanthropus*, if it were recognized as any kind of human ancestor at all. Yet one imagines that Dart might have appreciated it had he heard similarly ebullient words from his former teacher upon the discovery of his prized skull. As it was, Elliot Smith had always been cautious and diffident in his support of Dart's claims, even while he told Dart that he stood squarely behind him.

The London visit then got even worse for Dart. Elliot Smith invited him to give a presentation on the Taung Skull before a meeting of the Zoological Society of London, at which Elliot Smith was scheduled to demonstrate some casts of *Sinanthropus* that he had brought back from the Far East. Dart recalled in his autobiography the dramatic presentation Elliot Smith made on Peking Man which was greeted with resounding applause, as contrasted with Dart's ill-prepared and anti-climactic (this is Dart's characterization) presentation that followed.[96]

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[95] Smith, "Sinanthropus – Peking Man," p.193
[96] Dart, *Adventures with the Missing Link*, pp.57-58.

While Elliot Smith seemed to Dart to be far more interested in *Sinanthropus* than *Australopithecus*, the elder scientists had apparently not halted his efforts to secure the publication of at least the section of Dart's monograph on the dentition before Dart returned to South Africa. The paper had been circulated to referees, and their criticisms were sent to Elliot Smith since he had been handling the issue of publication Dart's behalf. Combining these with his own criticisms, Elliot Smith sent to Dart at his London hotel five and a half pages worth of severe assessment, indicating that even this one section of the monograph was still far from being in a form acceptable for publication. Perhaps realizing that this latest damning critique would dissuade Dart from continuing with the project at all, Elliot Smith added a quick post-script in free-hand designed to encourage the younger man: "I have written thus bluntly," he wrote, "because it is of the utmost importance to get the account of your wonderful specimen into an objective and dispassionate form so that it may become the material for real discussion and not the flinging of shibboleths." Elliot Smith evidently believed that a good way of lending support to a beleaguered Dart would be to suggest that the younger scientist's work was not "the material for real discussion."

It seems difficult to believe that Dart would have taken these as the words of a friend or ally, given their glaring condescension. Such a tone had always characterized Elliot Smith's correspondence with Dart, who, for want of support from an influential scientist and former boss, seemed helpless to contest it.[97]

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[97] Elliot Smith to Dart, February 25, 1931, AU8, RDP.

Dart returned to South Africa with this letter in hand, along with the rest of his manuscript, itself covered in Elliot Smith's unforgiving scrawl. Not even the title survived Elliot Smith's censure: where Dart had entitled his paper "Australopithecus and his place in human origins," Elliot Smith crossed out all but the word "Australopithecus".[98]

While any prospect for the prompt publication of Dart's monograph seemed dim at the time of his departure from England, others were preparing to pre-empt him with their own publications on the *Australopithecus*. Arthur Keith informed Dart before his departure of his intention to devote a large section of his forthcoming book on new discoveries relating to human evolution to *Australopithecus*. The publication of Keith's new book later that year made it clear that Dart's visit and the chance to examine the original specimen (as opposed to gazing at a poor reproduction in a glass display case) had not made him any more sympathetic to the idea of *Australopithecus* being an evolutionary ancestor of human beings. At greater length and with more extensive analytical detail than any of his previous writings on the subject, Keith reiterated his argument that the anatomical and geological evidence ran against Dart's interpretation, and corroborated his own belief that *Australopithecus* was a closer relative of the chimpanzee and gorilla than of human beings. Those characteristics of the skull that appeared to suggest an affinity with humans were either due to the juvenility of the individual (just as the anatomy of a baby chimpanzee in some ways

[98] MS of Dart's monograph "Australopithecus and his place in human origins," w/ Elliot Smith's annotation, AU8, RDP.

resembles that of a human being more than does the adult form) or to evolutionary parallelism.

Keith did attempt to represent Dart's position, even as he disagreed with it. He allowed what he took to be their two views on the place of *Australopithecus* in evolutionary history to be displayed visually alongside one another in a phylogenic diagram included in his book. The image shows Keith's conception of the evolutionary differentiation of the apes from the Eocene epoch to the present. It shows the common stem leading to the Chimpanzee, Gorilla, and Orangutang separating from that leading to the various forms of modern human and their extinct predecessors in the middle Oligocene. Near the base of the Miocene, one branch diverges from the human stem and is marked "Australopithecus (Dart's Conception)." At the boundary between the Pliocene and Pleistocene, another is marked "Australopithecus (Keith's Conception)", and diverges from a branch leading up to the modern chimpanzee and gorilla. In fact, Keith was here taking liberties with his representations of Dart's position and its relation to his own. Dart had never claimed that the fossil was as old as the Miocene. Rather, the placement of *Australopithecus* this far back in geological time represented Keith's idea of how ancient the creature *would have to be* if, as Dart insisted, it was an ancestor of human beings. This made Dart's position less plausible than it might have been, as no one believed that the fossil could be that old. With Dart's own publication put on ice, he could offer readers no alternative to Keith's (mis)representation (see Figure 2.1).

A second attack in 1931 came from the continent, when the Austrian anatomist Wolfgang Abel published his own hundred-page monograph on Dart's fossil. The opportunity for him to conduct research for this paper had come somewhat fortuitously. Before meeting up with Dart in Italy at the end of his expedition, his wife Dora had given a demonstration of the skull (which she had carried with her from

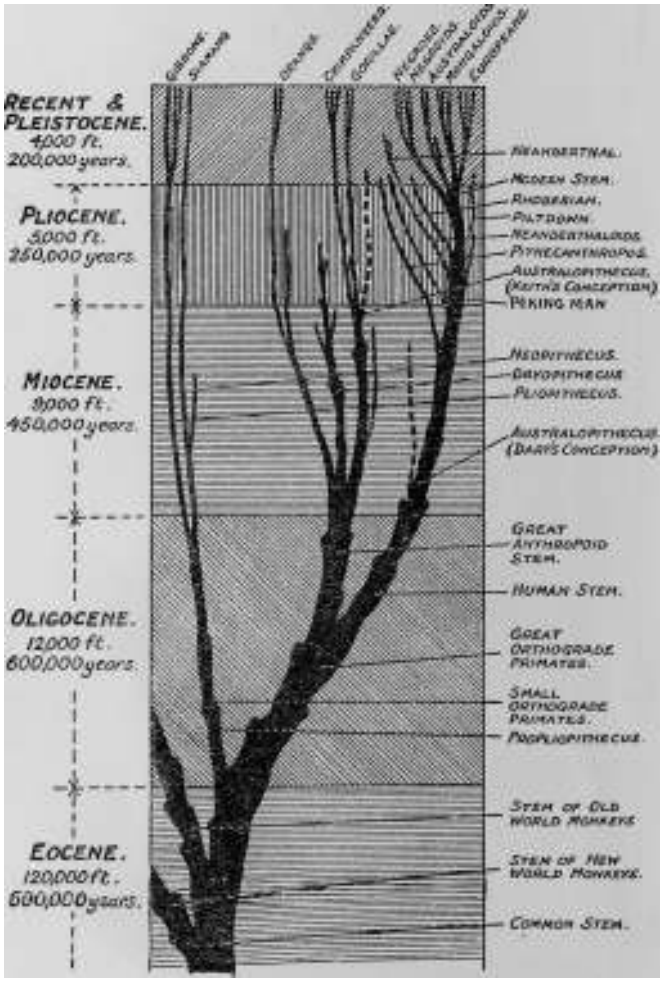

Figure 2.1 – Keith's 1929 diagram showing both his and Dart's view of the phylogenic position of *Australopithecus*[99]

[99] Keith, *New Discoveries Relating to the Antiquity of Man*.

South Africa) to a group of Austrian scientists, among whom was Abel. He was able to make detailed comparisons between Dart's skull and those of extant apes, which led him to the conclusion that *Australopithecus* was closely related to the gorilla and had nothing to do with human ancestry. Elliot Smith wrote to Dart encouraging him not to be discouraged by Abel pre-emptive publication: "I suppose you have seen Wolfgang Abel's monograph on Australopithecus! This of course will not deter you form your job; on the contrary it makes it more than ever desirable that you should submit the authoritative report." However, as was his practice, Elliot Smith could not let Dart get to work without some fatherly advice. In this case, he prompted Dart to take a lesson from his opponent: "I think you would be wise to deal with the problem in much the same way as Abel has done. Your real business is to give a complete and impartial report of the material – a full description with a minimum of comment or quotation. Write such a description and comparison as will bring out your points naturally as the inevitable consequences of the evidence you submit." How it was that Elliot Smith expected Dart's conclusions to be brought out "inevitably" by dealing with the material in the same way as one who had reached contradictory conclusion, he did not say.[100]

Dart had indeed already seen the Abel monograph by the time Elliot Smith's letter reached him, but he did not see it, as did Elliot Smith, as further reason to continue working on his own contribution. Rather, he was becoming resigned to the idea that his monograph would never see the light of day. Writing to Gregory some

[100] Abel, "Kritische Untersuchungen uber *Australopithecus africanus*"; Elliot Smith to Dart, August 13, 1931, Bundle #4, RDP.

months after his return to South Africa, Dart tried to justify his surrender: "A… fate of non-appearance has met hitherto my big monograph. It was too big! But now the essential facts are known & casts are available I am not exercised – the remains can speak for themselves." Of course, they had not and would not speak for themselves at all, and Dart's naïve paean to empiricism betrays the defeatism of one who had been beaten to the podium by competing spokespeople who would have the remains tell a different story. Years later, Dart again tried to validate his decision not to continue the fight in 1931 by writing in his autobiography that Keith and Abel's publications had "made any further publication by me in Europe superfluous." However, he could not wholly contain his disappointment over such an ignominious end for what was supposed to be his magnum opus: "The only thing I regret is having spent so much time on it when so many other important things called for attention."[101]

The "other important things" to which Dart might have been able to devote more of his attention had he not been occupied with his abortive monograph no doubt consisted mostly of his many responsibilities as Dean of the University of the Witwatersrand's Medical School. However, there is evidence that following all the disappointment surrounding *Australopithecus* in 1931, Dart's old pessimism about the limitation of his professional prospects in South Africa had returned. Indeed, if he was not to be allowed to achieve the standing which he sought for himself via his arguments for the ancestral status of *Australopithecus*, then his peripheral geographic position was nothing but a ball and chain. Thus, when the Chair in Anatomy at the

[101] Dart to Gregory, June 17, 1931, WKGP.

University of Birmingham became vacant in 1931, Dart jumped at the chance to apply. Writing to the former South African Prime Minister Jan Smuts to request a letter of support for the application, Dart wrote that while it "would be impossible, after eight years of service in this country, to leave it without many regrets," he nonetheless felt that "many advantages would accrue both to myself and to my work if I secured the post under consideration."[102]

Dart was not offered the Birmingham position, but his desire to return to England was not thereby abated. In 1933, Dart unexpectedly received notification that the post of Conservator of the Museum at the Royal College of Surgeons had become vacant with the retirement of Sir Arthur Keith. It was not that Keith's retirement was a surprise to Dart, but rather that he had received this implicit invitation to apply for the post. As he wrote to Elliot Smith when requesting a letter of support for the application, he had imagined that only Fellows of the Royal College (which he was not) would be considered, but a friendly colleague (H.A. Harris) had persuaded the relevant authorities to consider Dart. Painting himself as the outsider with the cards stacked against him, Dart opined to Elliot Smith that "in view of the antagonism which an application from myself is certain to evoke, I shall not only be pleased but staggered if the appointment comes my way." But even the smallest possibility of holding such a prestigious post filled Dart with longing: "I do not know of any appointment which could come closer to my conception of realizing my ambitions than this one in particular." No doubt he imagined himself sitting amongst the vast

[102] Dart to Smuts, May 2, 1931, Bundle #6, RDP.

collections of the Museum, where no one could criticize him for lacking adequate comparative material for making judgments about the affinities of *Australopithecus*.[103]

Keith himself offered some encouragement after Dart informed him of his intention with a copy of his prospective application. He described Dart as having an "amazingly active record such as few of your contemporaries can show." However, Keith informed Dart that he was bound to remain neutral in the election of the new Conservator, presumably to prevent quasi-dynastic successions to supersede the interests of the College.[104]

In the same year, Dart also sought Elliot Smith's written support for an application for the vacant Chair in Anatomy at the University of Bristol. In the letter that Elliot Smith submitted on Dart's behalf, he praised his former assistant's abilities and accomplishments in less than enthusiastic language, calling him a "competent" anatomist. He kept mention of Dart's association with *Australopithecus* to a single sentence, perhaps believing that such a connection was more of a hindrance than a help to Dart's prospects given the balance of professional opinion on the subject. All in all, one can legitimately wonder whether Elliot Smith believed that Dart was a good candidate for this or any of the posts he desire in England, or whether he wrote such letters more out of a sense of paternal duty than genuine support.[105]

Whatever the case, neither Elliot Smith's letters nor any other references were sufficient to allow Dart the fulfillment of his ambitions. He was passed over for the posts at the Royal College of Surgeons and the University of Bristol, both of which

[103] Dart to Elliot Smith, September 12, 1933, Bundle #6, RDP.
[104] Keith to Dart, October 3, 1933, Bundle #5, RDP.
[105] Elliot Smith to the Registrar, University of Bristol, November 16, 1933, Bundle #6, RDP.

were filled (coincidentally or otherwise) by men who had held posts at the McGill University in Montreal. Perhaps they too had felt that scientific life in a British Dominion was simply not equal to that in Britain itself.[106]

So both Dart and his monograph were denied entry into Britain. Dart never submitted a revised version of the section of the monograph of the dentition to the Royal Society, as the editors had suggested. His own judgment in hindsight was that Keith and Abel's publications had made his own contribution superfluous in Europe. It was perhaps with that in mind that he submitted a paper on the dentition of *Australopithecus* to the Japanese journal *Folia Anatomica Japonica*. Perhaps, to take a skeptical view of Dart's own account, he had simply had enough of the incessant criticism and rejection of the scientific community in England and was looking for a venue that would take his work at face value. The paper was duly published, but one must question the value of Dart's action. The scientists he wanted to convince (and needed to convince if he were ever to achieve his ambitions) were in Europe, not Japan. But by this time Dart despaired of ever achieving such an end. For almost a decade he had pinned his hoped for a rise to the top of the scientific world on *Australopithecus*, but it had failed to carry him where he wanted to go. His ambitions drained by continuous criticism and rejection, Dart put a lid on further attempts to convince his peers of the ancestral status of *Australopithecus.* He now resigned himself life as the Dean of Medicine at the University of the Witwatersrand.

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[106] Cowell (Secretary of the Royal College of Surgeons) to Dart, November 10, 1933, Bundle #6, RDP; Shapland (Registrar of the University of Bristol) to Dart, July 11, 1934, Bundle #6, RDP.

However, one of his few supporters was not content with such a fate for *Australopithecus*. It is to his efforts that we now turn.[107]

[107] Dart to Okajima, August 31, 1933, Bundle #4, RDP; Dart, "The Dentition of Australopithecus africanus."

Chapter 3 – *Australopithecus* as Broom's Mission

On March 5, 1929, the attendees of a meeting of the Zoological Society of London were read a note on the milk dentition of *Australopithecus* that had been received from Robert Broom, who was a corresponding member of the Society. Broom had recently paid visits to both England and the United States, where he "was rather surprised to find that the scientific world has apparently quietly dismissed any claims the Taung Anthropoid Ape might have to be near the point of origin of Man, and that the general opinion seems to be that it is only a chimpanzee." The blame for this dismissal lay partly with Dart himself, who, Broom regretted, had failed to defend his position with further publications on the subject. Additionally, Broom reiterated Dart's old worry that the casts available in London were of a quality so poor as to do the fossils themselves as disservice. The purpose of the current note, which was concerned primarily with a comparison of the milk dentition of Dart's skull with that of young apes and young humans, was to raise a bulwark against the naysayers:

> Holding, as I do, that *Australopithecus* is not a *Chimpanzee*, and that it is an Anthropoid Ape very much nearer to the form which gave rise to Man than either the Chimpanzee or the Gorilla, and that it is thus by far the most important fossil ever discovered, I feel I must do what little I can to prevent a final decision which I am convinced is wrong.

He proceeded to argue that the angle at which the teeth of the Taung skull erupted from the jaw was far more vertical, as in human beings, than in chimpanzees or gorillas, whose milk teeth are directed forward from the jaw. Broom also added a few remarks on the debate over the geological provenance of the skull, arguing that even if

it were to prove too recent to be itself ancestral to modern humans, it still could be representative of a type that had long existed, and that could thus have given rise to man at an earlier date.[108]

Broom reminded his British audience that the next meeting for the British Association for the Advancement of Science was to take place in South Africa in the summer of 1929. "I hope," he wrote, "that one of the chief results of this visit will be a thorough examination of *Australopithecus* by European and American experts, and I have little doubt what the results will be." Given such certainty, Broom must have been disappointed, as was Dart, when *Australopithecus* failed to make waves at the meeting.[109]

As we saw in the last chapter, the difficulties of conducting research alongside his duties as Dean of Medicine, the demands of foreign colleagues for casts and publications, and the frustration of the scientific community's intransigence in the face of his claims, had all taken their toll on Dart. By the early 1930's, he had virtually abandoned the debate. For the next decade and a half, Broom would become the chief proponent of the claim that *Australopithecus* represented a form ancestral to human beings. Consistent with his character, Broom would add color and eccentricity to the debate, willing as he was to use more unorthodox approaches to defending his position than Dart had been. Nor was he burdened with an ambition to work his way up the institutional ladder in science. Without any qualms about offending potential patrons (and even a desire to do just that), Broom pursued his goal of convincing the world of

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[108] Broom, "Note on the Milk Dentition of *Australopithecus*," p.85.
[109] Ibid., p.85

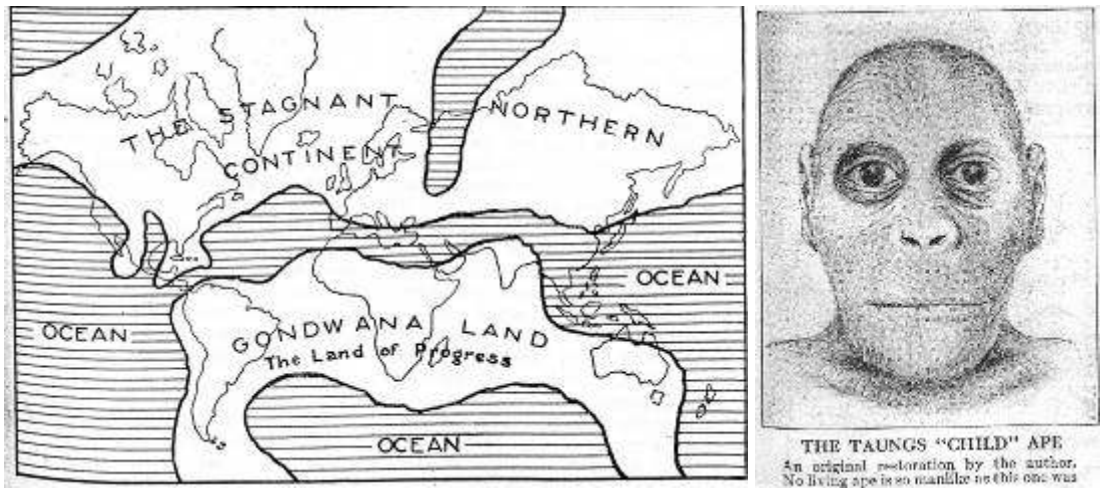
the ancestral status of *Australopithecus* with almost messianic vigor. His idiosyncratic character, energetic drive, and inflated sense of self-importance would play a great part in shaping the direction of the *Australopithecus* debate over the next several decades.

Broom's Rebellion

In May 1929, Broom wrote to encourage Dart not to fall into despair regarding the cool reception their claims had been receiving from a majority of scientists overseas. He was sure that they and their few allies, despite their small numbers, were "in a position to hold the fort against the world." The fact that they were in South Africa, while the majority of their high-profile critics were from Britain and America, was very significant for Broom. For years, Broom would stick to the idea that they were leading a scientific insurgency from the southern periphery of the Empire against the centre, with its so-called experts entrenched in powerful institutions. In this vein, Broom wrote an article for *Scientific American* in 1929 entitled "What the World Owes to South Africa," in which he suggested an alternative model for proper scientific conduct to those (mostly English) scientists who had criticized Dart for having made his judgment about *Australopithecus* too rashly: "One much prefers a scientist who will boldly show where he thinks the evidence leads than one who is too cautious to express any opinion."[110] He even attempted to parallel the unappreciated scientific creativity of South Africa with its status, at least according to him, as a

[110] Broom, "What the World Owes to South Africa," p.121.

centre of crucial evolutionary advancement. Broom claimed that in addition to being the birthplace of Dart's exciting new scientific advance, South Africa had been the evolutionary birthplace of fruits and grains, flowering plants, birds, mammals, and human beings. In a map of the world showing the division of the earth's landmass into two super-continents in the Permian period, Broom labelled the southern landmass, which included the area preserved in the geological record of South Africa, "The Land of Progress," and the other, which included Britain, "The Stagnant Northern Continent" (Fig.3.1). Adding to the visually supported rhetoric of the article, Broom also included his new representation of the face of *Australopithecus* in the flesh, staring out at the reader with its unmistakeably human eyes (Fig.3.2).



Figures 3.1 and 3.2 – Broom's propagandistic illustrations[111]

[111] Ibid., pp.119 and 121, respectively.

Broom was also engaged at this time in trying to outflank the critics in the field of geology. From early on, the difficulty of dating the cave deposits from which the Taung skull had been blasted had been cited as an obstacle for the acceptance of Dart's claim. Past analyses of faunal remains in similar cave deposits had revealed them to be of Pleistocene age, and most scientists assumed that this was the case for Taung. Since most scientists believed that human beings had already evolved by the earlier Pliocene period, it was objected that an ape-like creature from the Pleistocene could not be a human ancestor. Both Dart and Broom had long responded to this criticism by pointing out that even if the Taung skull itself had belonged to a creature from the Pleistocene, the type which it represented might have already been in existence since the Pliocene or earlier and hence there was no inconsistency in claiming that *Australopithecus* was a human ancestor. Even so, Broom knew that it would be advantageous to his and Dart's cause if other scientists could be convinced that the Taung deposit was of a Pliocene rather than a Pleistocene age, a position that Dart had already advocated without much success. To this end, he secured from Dart some pieces of bone breccia from the same formation that had yielded the skull in order to see if the fauna associated with the skull might leave open the possibility of the deposit being older than Pleistocene. Knowing the uncertainty of this method of dating, Broom counselled Dart that he should not use too aggressive rhetoric if he were to speak on the subject: "Please don't say that the deposit is unquestionably Pliocene – say merely that the evidence so far points to the deposit being probably Pliocene. It is safer & quite as good." If the critics could use uncertainty to cast doubt

on Dart's claim, Broom knew that they could just as easily employ the same weapon in the other direction. In the short article resulting from his analysis of the breccia, Broom concluded that since none of the forms present conformed exactly to those from more reliably dated deposits, the age of the Taung skull could not be determined with certainty. However, he counselled that the fact that all the mammalian remains came from extinct forms justified the tentative inference of a Pliocene provenance.[112]

Broom's enthusiasm for taking on scientific orthodoxy (or at least what he perceived to be such) was not limited to the case of *Australopithecus*, or even to matters that most would have considered within the bounds of science. This was put on display in 1933 with the publication of his book *The Coming of Man: Was it Accident or Design?* One of the chapters was devoted entirely to *Australopithecus* and continued Broom's unwavering defence Dart's claim against his many critics.[113] However, as the title indicated, the evolutionary position of Australopithecus was not the only subject which Broom intended to tackle. After a journey through all of the evolutionary stages from fish to human being, Broom devoted a final and climactic chapter to his speculations as to the ultimate causes and purposes of evolution and to criticisms of such explanations, or lack thereof, which he perceived to be the reigning orthodoxy in the field. Broom suggested that in the course of his observations and researches as a palaeontologist, he had been led to conclude that rather than Darwinian natural selection or Lamarckian use and disuse, the cause of evolution was to be sought in a number of what he called "spiritual agencies", some of which pervaded

[112] Broom to Dart, March 18, 1930, RDP, AU8; Broom, "The Age of *Australopithecus*," p.814.
[113] Broom, *The Coming of Man*, ch.4.

nature, and at least one of which transcended it. Further, Broom disagreed with the purposelessness he felt the Darwinian position implied. He suggested that the emergence of human beings, especially those of the so-called "highest type", signalled the preordained culmination of physical evolution as designed by a transcendent spiritual agency. Humankind had now to look forward to a sort of psychic evolution, the result of which had yet to be realized, but which was surely being guided in some way by that same transcendent agency. Broom did not make these claims in the service of any particular religion, though he did suggest that religion generally speaking was likely a feature of humankind's psychic evolution as it attempted to come to grips with the nature of the spiritual agencies whose invisible hands they sensed but could quite grasp. The end point, Broom's ruminated, would come when humans came to a full realization of the spirits around them and harmonized their lives with the intentions of those agencies.[114]

Broom was fully aware that his scientific colleagues would not appreciate what he referred to as the 'philosophical' part of the book. He wrote Dart that he imagined they would differ on this front, and similar expectations of non-support for this aspect of his work pepper his scientific correspondence at the time. In the introduction to the book, he explained his reasons for putting forward what would likely be so unpopular a position among his colleagues: "It seems to me…," he wrote, "there are times when one must quite deliberately risk criticism, and even invite it. And if the scientist, working with his microscope or his fossil bones, catches as he believes any rays of

[114] Ibid., ch.7.

light on the dark mysteries of creation, it seems to be his duty to give his conclusions to the world that others may at least consider them."[115] This justification echoed the defence Broom had previously made of Dart's "bold" hypothesis regarding the ancestral status of *Australopithecus*. By publicly revealing his belief in an evolution designed and guided by spiritual agencies despite, and even because of the opposition that it was likely to occasion, Broom was acting precisely in the way that he believed his virtue as a scientist compelled him to act. Knowledge that challenged orthodoxy was the only sort of knowledge worth pursuing.

Writing to Dart to inform him of the impending publication of his book, Broom confidently predicted that after reading the chapter on Australopithecus, "no man will ever again say that A[ustralopithecus] is only a chimp." In a light-hearted, but also significant, moment about a year later, he boasted to Dart that his chapter on Australopithecus "rather disposes of 'all the Keith's horses and all the Keith's men,'" referring, of course, to Arthur Keith and the leading role he had taken in opposing Dart's claim. Such statements indicate that Broom believed that his book would be a step towards improving the scientific standing of Dart's claim. However, given that he apparently knew that his speculations on the spiritual foundation of evolution would occasion a healthy round of eye-rolling amongst the majority of scientists, we might inquire whether Broom did not make strategic error in associating these with his defence of Dart's claim. Could he have genuinely thought that his book would actually benefit Dart's cause?

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[115] Ibid., pp.10-11.

Broom surely did believe that he was helping Dart's cause, but his strange way of doing so was bound up with his enthusiasm for clothing himself in the guise of an outsider waging war against the forces of orthodoxy. As we've seen, Broom imagined the virtue of his vocation to lie in the readiness of the scientists to dissent from received ideas. However, unlike many scientists in his world, he did not define his identity as a scientist or a cultural dissenter to an opposition to religion or mysticism per se. He was just as willing to manifest his idea of scientific virtue by bringing unpopular quasi-religious ideas to bear against the received wisdom of the majority of scientists. If this meant going up against both Darwinian and Lamarckian theorists then so much the better. In Broom's mind, to bring Dart's already controversial claim about the evolutionary status of Australopithecus into association with even more unpopular claims about the spiritual causes and purposes of evolution was to do Dart's idea credit. The more out there the idea – scientific, religious, or otherwise – the more virtue that could be claimed by those with the courage to speak it.[116]

Broom's ideas in this regard were, it must be said, rather self-deluding. *The Coming of Man* does not seem to have been widely read, and it certainly did not have the influence of Dart's critics that Broom predicted it would. His lasting contributions to the *Australopithecus* debate would come later, as we will see, and without the association with his 'spiritual agencies'.

[116] I develop these ideas further in a forthcoming article: Richmond, "Design & Dissent."

New Discoveries

Broom's character and scientific work were less marked than Dart's by an ambition to make a career for himself at a prestigious institution, and his chosen places of work up until the nineteen thirties were apparently determined more by his curiosity about certain scientific subjects than his ability to find remunerative employment. Shortly after taking his medical degree in Scotland, Broom relocated to Australia, because he had become interested in the anatomy of the primitive fauna peculiar to that country. There, he practiced medicine in country towns in order to make enough to survive, but employed much of his time researching marsupial and monotreme anatomy. The publication of these researches resulted in Broom's coming to the attention of a number of scientists interested in the subject, including many of the men who would be influential to the career of a young Raymond Dart. On a visit back to Britain, he had the chance to examine some fossil reptiles from South Africa that showed peculiar anatomical traits that linked them in certain respects with mammals. His Australian researches on marsupials and monotremes had in part been motivated by an interest in the evolutionary origin of mammals, and the South African fossils seemed to him to represent another avenue for researching that problem. On this basis, he uprooted his life (and wife) in 1897 and moved to South Africa. He lived in various locations around the country, determined more by the abundance of fossiliferous deposits nearby than his ability to make money through the practice of medicine or scientific teaching. Broom supplemented his meagre income by selling fossils from his increasingly large private collection to various museums around the

world, culminating in 1913 with the sale of the entire collection to the American Museum of Natural History. This practice had the effect of alienating Broom from the scientific community in South Africa, who resented the international sale of their countries treasures for individual financial gain, even while the increasing knowledge of South Africa's mammal-like reptiles increased Broom's scientific stock abroad. In the early nineteen twenties, the animosity grew to such a point that Broom was barred from researching in the collections of the South African Museum. Dart once noted in a letter that when he arrived in South Africa and was put on the council of the South African Association for the Advancement of Science he "found that any mention of Broom's name evoked bitter reactions… his name literally stank!" Never one to bow to institutional pressure, Broom seems to have returned the contempt. However, partly due to Dart's diplomatic efforts, Broom's domestic relations seem to have cooled by the end of the decade, and he even became the president of SAAAS in 1933.[117]

Broom's finances, however, remained precarious. When Dart learned that Broom did not even possess the funds to travel to the annual meeting of the SAAAS over which he was to preside, he decided to seek out patronage on Broom's behalf. In April of 1933, he wrote to General Jan C. Smuts and Jan Hofmeyr, both of whom were past presidents of the SAAAS and had just been elected to national office. "It would be a graceful and provident act on the part of the new Government," wrote Dart to Smuts, "…to liberate him from the gruelling necessity of winning his daily bread; and

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[117] For biographical details of Broom's life, see Watson, "Robert Broom. 1866-1951." The quote from Dart, as well as an account of Broom's trouble with the SA scientific community, come from a letter from Dart to Broom's son Leonard, who was seeking details of his late father's life and career: Dart to L. Broom, November 8, 1954, AU8, RDP. See also Reader, *Missing Links*, pp.114-115.

to save the remaining years of his life for bringing to fruition in books or other publications, the whole of the knowledge he has amassed during his lifetime. The investment would more than repay the country." Both Smuts and Hofmeyr had scientific interests, but the nationalistic overtones could not have hurt. He suggested various means by which the Government could support Broom, even suggesting that he be made a senator.[118]

A year on, Dart had heard nothing from the prospective patrons in government. He wrote to Broom in March of 1934 to tell him of the efforts that had been made on his behalf and lamenting that Broom continued to work under difficult circumstances: "It seems to me a hell of a waste for you to be doling out salts and No.9's instead of getting on with the work of the time." However, just a few months later (and without any indication that the action had resulted from Dart's plea for government support) Broom informed Dart that he had been offered the post of Curator of Fossil Vertebrates and Anthropology at the Transvaal Museum in Pretoria, South Africa's capital. Broom immediately set about making the collections of the museum an important centre for the study of the mammal-like reptiles and other extinct South African animals. Further, as we will see, the work he would do from that post, which he held until his death, would be of crucial importance to the fortunes of *Australopithecus*.[119]

The task of finding further specimens of *Australopithecus* was foremost in Broom's mind in the years after he took up his post at the Transvaal Museum. There

[118] Dart to Smuts, April 26, 1933. I have not found the letter to Hofmeyr, but Dart mentions having written it in a later correspondence.
[119] Dart to Broom, March 21, 1934, AU8, RDP; Broom to Dart, July 22, 1934, AU8, RDP.

were a great number of limestone filled caves around Pretoria, and given that the Taung skull had been found in a cave deposit, Broom set about searching the area. At first, his only successes came in the form of other extinct species. As he wrote to Dart on the 10th of August, 1936, "I have been working at the limestone cave fossils from the caves around Pretoria and have got about a dozen new mammals… I have so far got no trace of man or Australopithecus." At around the same time, Broom ventured a little farther afield to some caves which were being mined for lime and bat guano at a place called Sterkfontein, after two students of Dart's told him that some interesting baboon fossils were to be found there. Some days after asking the mine proprietor to be on the lookout for primate fossils, Broom was handed, on August 16th, the brain cast of a large primate. The next day, after searching around in the rubble produced by the blast which had uncovered the brain cast, Broom succeeded in recovering several fragments of the cranium and face, a large portion of the base of the skull and a portion of the upper jaw with four teeth still attached. With many of the fragments embedded in rocky matrix, Broom was not able to tell immediately whether or not he had found that for which he had been searching: he wrote to William K. Gregory on the 18th to say he would need about a week to know whether he had a giant baboon or something near Dart's *Australopithecus*.[120]

As soon as he had uncovered enough of the crowns of the teeth to make a diagnosis, Broom was sure that the creature had been no baboon. He told Dart that the "beast is near ally of Australopithecus africanus but probably a new species." Further,

[120] Broom to Dart, August 10, 1936, AU8, RDP; Broom to Gregory, August 18, 1936, WKGP; Reader, *Missing Links*, pp116-118.

it was not a juvenile specimen as the Taung skull had been, so any human-like features could not be dismissed on developmental grounds. Shortly thereafter, Broom sent off a note to *Nature* in which he bequeathed to the creature the name *Australopithecus transvaalensis* after the Transvaal region of South Africa in which it had been found. The note was conservative in tone, consisting mainly of a description of the teeth and photographs of the main fragments together with a provisional sketch of that skull as a whole. However, while the note to *Nature*, entitled "A New Fossil Anthropoid Skull from South Africa," may have been the product of deliberate restraint of Broom's part to appease a scientific community critical of quick judgments, he also managed to satisfy his penchant for bold pronouncement by sending a simultaneous article to the *Illustrated London News* entitled "On a New Ancestral Link Between Ape and Man." His scientific colleagues may have required careful handling, but the lay public, always ready to hear about new 'missing links' required no such care.[121]

Further discoveries came in torrents through the rest of 1936. Even before the publication of the articles announcing the new find (though after Broom had already sent them off), Broom recovered most of a femur (thigh bone), a tibia (shin bone) in nearly perfect condition, an os calcis (heel bone), several vertebrae, a more complete maxilla (upper jaw) than in the initial find, and a number of other upper teeth and some cranial fragments. "So you see," Broom wrote Gregory announcing the new finds, "we don't do things by halves in S.Africa!" From the limb bones, Broom gathered that the legs of the creature had been "almost typically human but

[121] Broom to Dart, August 24, 1936, AU8, RDP; Broom, "A New Fossil Anthropoid Skull from South Africa"; Broom, "On a New Ancestral Link Between Ape and Man"; also see Reader, *Missing Links,* 117-118.

considerably shorter," while the arms seemed to have been longer. Nonetheless, he was sure that it had been bipedal, as demonstrated by a rough sketch in a letter to Gregory showing a short slightly hunched but still upright creature with its hands free to wield an implement in conspicuously human fashion (Fig. 3.3). Before the end of the year, he also had a sacrum (the triangular bone at the base of the spine and rear of the pelvic cavity) and several partial ilia (hip bones). He judged the sacrum to be very similar to the human, but was less sure about the affinities of the ilia. Given his uncertainty on this front and knowing the cost to his credibility if he got his identifications wrong, he sent some preliminary drawings and descriptions of these to Gregory for confirmation: "I should never be able to raise my head again," he wrote, "if I described them as Primate and it turned out afterwards that they belonged to a bear or a tiger!" This was merely dramatic overstatement, for Broom's worry was actually that the hips seemed more chimpanzee or baboon than human. This did not, however, deter him from his confident belief in the essentially proto-human character of the creature. One of the only elements whose lack Broom felt was a serious disadvantage was the mandible (lower jaw) and lower teeth, since these were often used to determine the affinities of fossil primates.[122]

[122] Broom to Gregory, September 13, 1936, WKGP; Broom to Gregory, October 23, 1936, WKGP; Broom to Gregory, November 14, 1936, WKGP.

Figure 3.3 – Broom's 1936 sketch of *Australopithecus*, next to which he scrawled the words "Probable proportions"[123]

A New American Ally

During this period of rapid discovery, a shift occurred in Broom's correspondence. Whereas the majority of his letters on the subject of *Australopithecus* had previously been directed to Dart, he now began to address most of his epistolary energies towards William King Gregory. Much of this was due to Dart's seeming lack of interest in continuing his participation in the debates. Dart had expressly informed Broom that he did not intend to write anything further on the subject. Though Broom

[123] Broom to Gregory, September 7, 1936, WKGP.

continued to encourage Dart to change his mind, he also asked permission to include pictures and discussions of the Taung skull in his own intended publications summing up the available knowledge of the South African creatures. Dart does not seem to have had a problem with this arrangement, and thus his contributions remained limited to what he had written up until the early thirties. Nonetheless, Broom assured him that he would benefit from the new discoveries as the man who had first seen the truth: "I must congratulate you on the fact that the new find completely vindicates your position at least in all essentials."[124]

Gregory had gone some way towards vindicating Dart's claim when he had confirmed that the teeth in the Taung skull bore more similarities to human teeth than to those of any of the living apes. However, Gregory's support did not amount to the partisan enthusiasm of Broom, perhaps making him an even more valuable ally to the two South Africans whose perceived lack of self-restraint did not ingratiate them with many in the scientific community. Gregory even managed to transmit some of his temperament to Broom, perhaps saving the latter from the consequences of his own zeal: upon comparing the drawing of the vertebrae and pelvis that Broom had sent, Gregory found that they looked suspiciously similar to those of a "very large old baboon". Knowing Broom's tendency to publish his views the moment they were formed (though, to be fair, Broom had in this instance sought confirmation from

[124] Broom to Dart, September 18, 1936, AU8, RDP.

Gregory), he sent cable across the Atlantic that read simply "Caution". Broom was persuaded to put the matter on hold.[125]

Gregory's cautious approach to the *Australopithecus* question had also been on display in a synthesis of his views on human evolution published as *Man's Place among the Anthropoids* in 1934. The fact that he adopted a somewhat more hesitant tone than he had in defending Dart's impression of the Taung skull's dentition several years before may have been a matter of the audience, for the book was a compilation of lectures delivered in England, where, as Broom had observed, the consensus was that *Australopithecus* was no more than a cousin of the African apes. Without conceding the morphological similarities he had observed, Gregory prevaricated on the question of ancestry: "[*Australopithecus*] may well have been related on the one hand to the African apes and on the other hand to the prehuman stock…This fossil tends as a whole to bridge the phylogenetic gap between the two families even though it may not be in the direct line of human ascent." Both camps could find some validation for their positions in a statement such as this.[126]

For Broom, however, Gregory's earlier and more straightforward statement of support for the dental affinities of *Australopithecus* was the more effective ammunition, and it came in handy when the first attack on his interpretation of the 1936 discoveries appeared in *Nature* at the end of that year. They came courtesy of the German born zoologist Ernst Schwarz, then working at the Natural History museum in London. Concurring with Wolfgang Abel's 1931 analysis, Schwarz

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[125] Gregory to Broom, January 20, 1937, WKGP; Broom to Gregory, January 9, 1937, WKGP.
[126] Gregory, *Man's Place among the Anthropoids,* pp.71-72.

argued that the dental anatomy of the Sterkfontein ape agreed in almost all respects with that of the modern gorilla, and that its comparatively small size was indicative of the former existence of a pigmy variety of gorilla, rather than a prehuman or even prechimpanzee stock. In his rejoinder, Broom declined to answer Schwarz's list of anatomical details with a counter-list of his own. Instead, he invoked Gregory as "probably the greatest living authority on mammalian teeth" whose 1929 analysis had already shown the human affinities of *Australopithecus*'s dentition. Broom failed to mention that Gregory had analyzed casts of the juvenile Taung skull, while Schwarz's criticisms were levelled at Broom's analysis of the more recently discovered adult remains from Sterkfontein. The omission speaks volumes: what mattered was the invocation of sympathetic authority, not the details of history.[127]

If association with Gregory was valuable to the furthering of Broom's scientific interests, the converse was no less true. The store of fossil materials from which Gregory had built his renowned knowledge of mammalian evolution owed something to the man who had opened a window into the early stages of that process with his discovery of the fossilized mammal-like reptiles of South Africa, not to mention his sale of many of those remains to Gregory's place of work (to the dismay of South African colleagues). Thus, it is not surprising that in 1936 Gregory sought to further cultivate their relationship by inviting Broom to come to the United States. Beyond Gregory's personal interest in Broom's scientific work, the main motivating factor behind the invitation was Gregory's involvement in the planning of a

[127] Ernst Schwarz, "The Sterkfontein Ape," p. 969; Broom, "The Sterkfontein Ape," p.326.

Symposium on Early Man to be held in commemoration of the one-hundred and twenty fifth anniversary of the Academy of Natural Sciences of Philadelphia in March of 1937. When Broom sheepishly admitted that expenses were an issue, Gregory arranged for funds to be made available for the purpose. He also suggested that he could help Broom could further defray the costs by arranging some lectures outside the auspices of the Symposium for which he would receive honoraria (and the opportunity to highlight South Africa's contribution to the study of prehistory at that many more venues). At first, it was intended that Broom might confine his lecture circuit to the East, with the possible addition of Chicago. As it turned out, Broom took his show on the road, giving 28 lectures from New York to California through more than two months after the meeting in Philadelphia.[128]

Inclusion in the Philadelphia meeting was an implicit vote of confidence for Broom and *Australopithecus*, as the list of participants was a veritable who's who of the field. Among the Americans there was Gregory, along with Ales Hrdlicka from the Smithsonian Institution. The aging discoverer of "Java Man" (*Pithecanthropus*), Eugene Dubois, came over from the Netherlands. Also crossing the Atlantic were the French theologian-prehistorian Pierre Teilhard du Chardin, the British archaeologist Dorothy Garrod, and Broom's sometimes nemesis Arthur Keith (he spoke on remains from Mt. Carmel in Israel, so Broom had *Australopithecus* to himself.)

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[128] Broom to Gregory, December 12, 1936, WKGP; Gregory to Broom, January 20, 1937, WKGP; the Gregory papers at the American Museum of Natural History also contain Broom's American lecture schedule, showing dates, location, the sponsoring organizations and the subjects on which Broom was to speak.

Broom's paper, delivered on the twentieth of March, did not contain much information beyond a narrative recounting of the events that had led to his recent discoveries and a reiteration of his confidence that *Australopithecus* was more likely than any known fossil form to have been the common ancestor of the African apes and human beings, if not a little advanced along the human line of descent. Far more interesting was the way in which Gregory and his associate from the American Museum of Natural History, Milo Hellman, integrated Australopithecus into their paper, which, like so much of Gregory's work, focused on comparative dental anatomy and the evolutionary inferences that could be drawn therefrom. More specifically, the paper described the significance of what Gregory had been calling for two decades the "*Dryopithecus* pattern" of cusps and grooves on the lower molars of humans, apes, and certain of their fossil relatives.

The extinct ape genus *Dryopithecus* had been named in 1856 by the palaeontologist Edouard Lartet from fossils discovered in his native France. Later discoveries from Miocene deposits in the Siwalik Hills of northern Indian revealed the genus to have occupied a wide range on the Eurasian landmass. A number of similar extinct genera of apes had also been discovered in the Siwaliks. It was Gregory's position that the pattern of cusps on the lower molars of *Dryopithecus* revealed a generalized pattern from which variants observed in later forms, including the living apes as well as human beings, could plausibly said to have been derived. The significance of *Australopithecus* for Gregory and Hellman was that "[of] all the known variations of the *Dryopithecus* pattern, that of the first lower molar of *Australopithecus*

africanus… is the closest to the human modification of the *Dryopithecus* pattern" (the comparison was made with Dart's *africanus* rather than Broom's *transvaalensis* as none of the latter's lower molar's had then been discovered) (Fig. 3.4). This did not for Gregory translate into evidence that *Australopithecus* was a direct ancestor of humans. In fact, he argued that *Australopithecus* was "obviously too late in time to be a direct ancestor to man." However, Gregory was less interested in whether this or that fossil form was a direct ancestor than he was in establishing general morphological patterns that demonstrated the common ancestry of humans and apes, and to this end he found *Australopithecus* extremely valuable. His take on the South African fossils contrasts somewhat with those of Dart and Broom, as well as many of their critics, for whom the question of direct ancestry loomed large in evaluating the significance of *Australopithecus*.[129]

The extension of the *Dryopithecus* pattern from *Australopithecus africanus* to *Australopithecus transvaalensis* came shortly after Broom's return from the United States, for in September of 1937 he discovered an isolated third lower molar which he attributed to the creature whose remains he had been finding over the past year. He took only one day to fire off an excited letter to Gregory detailing the find and indicating his intention to send a note to *Nature* on the subject. When that note was published a month later, Gregory's influence was much in evidence. "The crown pattern," wrote Broom, "will be seen… to be a modification of the well known *Dryopithecus* pattern," and indeed seven of the ten molars represented in Broom's

[129] Broom, "On Australopithecus and its Affinities"; Gregory & Hellman, "The Evidence of the Dentition on the Origin of Man."

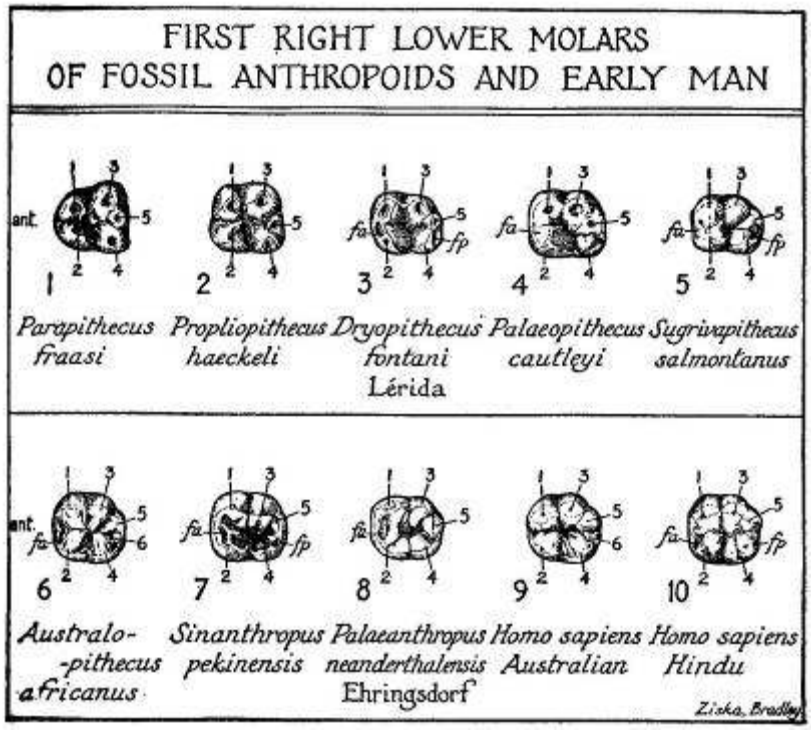

Figure 3.4 – Variations on Gregory's "Dryopithecus pattern" of molar cusps[130]

figure were drawn after illustrations previously published by Gregory. Of the three others, one was the new *Australopithecus* molar, while a second was that of a female Australian Aborigine. Here, Broom continued to press harder than Gregory for the claim of direct ancestry, stating that "the [*Australopithecus*] tooth in its crown pattern agrees more closely with that of an Australian native than it does with any of the known anthropoids," and concluding in the form of a phylogenetic diagram that such morphological similarity put *Australopithecus* in the line of human descent (Fig. 3.5). He unearthed five more teeth in the next two weeks (apparently after having sent off the note, as these were not described therein), accompanying each discovery with a

[130] Ibid., p.249.

new letter to Gregory containing descriptions and rough sketches. "Not too bad for an old man of 71 years!" he congratulated himself in one.[131]

These new discoveries of Gregory's favorite kind of anatomical specimens undoubtedly influenced the American's decision to plan a trip to South Africa, along with Milo Hellman, in order to examine the material. In informing Broom of his desire to make the journey, Gregory was careful not to assume any privileges, as he was aware that many of the fossils had not yet been described in print and that Broom, as the discoverer, had a right (according to generally accepted norms of scientific practice) to priority in publication. "We, of course, would not want to 'horn in'," he wrote, "but in view of our close friendship it may be that you would be glad to have us confirm your findings and build up a palate of Australopithecus which we could then compare with your restoration. We presume that long before our findings would be

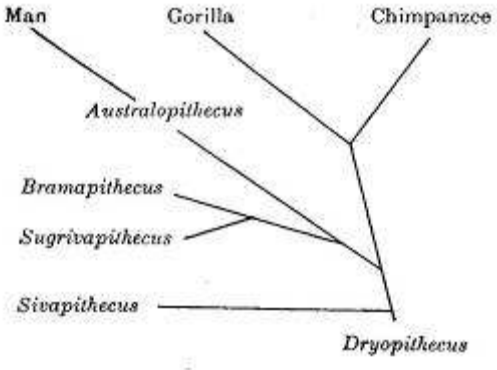

Figure 3.5 – Broom's 1937 phylogeny of humans and their near relations[132]

[131] Broom to Gregory, September 16, 1937, WKGP; Broom to Gregory, September 30, 1937, WKGP; Broom to Gregory, October 1, 1937, WKGP; Broom, "Discovery of a Lower Molar of *Australopithecus*," p.681-682.
[132] Broom, "Discovery of a Lower Molar of *Australopithecus*," p.682.

ready for publication yours would be published and distributed." It turned out, however, that there was little need for such polite assurances, as Broom related that they could have access to all the material and that the Director of the Transvaal Museum was anxious to have them publish a paper in the Museum's Annals, and further that Gregory would "be at liberty to express any opinions you form whether or not they conform to contradict those of the local workers." Both Gregory and Broom also wrote to Dart asking that he afford similar privileges to the visiting Americans, which he duly did.[133]

Before Gregory and Hellman's departure in early June on an ocean-liner bound for Cape Town, Broom made a further discovery of high significance, especially given Gregory's stated desire to reconstruct the palate of *Australopithecus* for comparison with other primates. This was a mostly complete right maxilla (upper jaw) which Broom attributed to *Australopithecus transvaalensis* containing four teeth, including the first known incisor and canine of that species. The latter was nearly as small as the corresponding human tooth, Broom remarked in announcing the discoveries in *Nature*, contrasting markedly with the modern chimpanzee's large canine. Shortly thereafter, Broom decided that the teeth of the Sterkfontein creature differed sufficiently from Dart's *Australopithecus* to merit a separate generic designation, and it thus became *Plesianthropus transvaalensis*. Additionally, probably while the Americans were in transit, Broom obtained a palate with a single molar from the operator of the lime mine at Sterkfontein, who indicated that it had been given to him by a local schoolboy

[133] Gregory to Broom, March 14, 1938, WKGP; Broom to Gregory, March 12, 1938, WKGP; Broom to Gregory, March 22, 1928, WKGP; Gregory to Dart, April 1, 1938, WKGP; Broom to Dart, April 20, 1938, AU8, RDP.

who had found it in a separate cave nearby. Broom tracked down the boy, from whom he obtained more teeth that corresponded to the palate, and who showed him their place of origin. Broom managed to gather up a number of broken fragments which, when cleaned and joined, gave him the lower left portion of skull and a right lower jaw, both containing several teeth. Broom claimed that these new remains differed markedly from those of *Plesianthropus transvaalensis*, especially in the shape and much larger size of the molars, as well as being somewhat older, leading him to create a new genus and species to accommodate the creature: *Paranthropus robustus*. However, the impression of a canine left in the matrix (the tooth itself was lost) showed that this form shared with *Plesianthropus* small canines, in contrast to the living apes. This, then, was the second genus and third species of South African man-ape, leading Broom to make a wide-ranging conclusion in an article published just after Gregory's departure from South Africa: "Clearly, during the Pleistocene there lived in South Africa a number of large-brained anthropoids which resemble man in the shape of their premolars and in having relatively small canines." In deference to the widespread view that the Pleistocene was too late in time for these to have been direct ancestor of humans, Broom maintained their evolutionary importance by claiming that they were "probable the modified descendants of forms that may have been widely distributed over Africa in Pliocene times, and it is probable that from one of the Pliocene members of the group that man arose."[134]

[134] Broom, "More Discoveries of *Australopithecus*," pp.228-229; Broom, "The Pleistocene Man-Apes of South Africa," pp.377-79.

Gregory and Hellman remained in South Africa over most of the summer month of 1938. They made a number of casts of the teeth and palates of the various man-apes to bring back to New York, along with copious notes, measurements, drawings and photographs – enough material that the full report took the rest of the year to prepare before they sent it back to South Africa where it would be published in the *Annals of the Transvaal Museum* shortly before Europe descended into war. Broom published his own report on the dentition in the *Annals* simultaneously with that of the Americans. He explicitly noted that neither he nor the Americans had seen a word of the other report before publication, hoping to add credibility to the results should they demonstrate agreement.

Gregory and Hellman's report continued their efforts to stress the centrality of the *Dryopithecus* pattern in ascertaining the path of human evolution. What was new were the numerous statistical tables showing that according to a number of different measurement of the dentition and dental arch, the South African ape-men were transitional between the older South Asian anthropoids like *Dryopithecus* and human beings. Significantly, they also found that the dentition of the South African forms were in some respects even more similar to a nearly human genus from China, *Sinanthropus*, which would tend to suggest that these represented successive structural stages in the evolution of modern humans. However, the problem of chronology remained: the South African remains were not thought to be any older than those of *Sinanthropus*, such that the structural series could not represent a genetic series. Gregory and Hellman's conclusions with respect to the question of ancestry remained

circumspect. The one thing they were sure of was that the South Asian Dryopithecine group represented the common stock of human, the living apes, and the Australopithecines. They left unanswered the question of whether the Australopithecines were more closely related in a genealogical sense to modern humans or to the living apes. They called the Australopithecines "the conservative cousins of the contemporary human branch," implying (not without ambiguity) that those creatures may have represented a third branch (in addition to those leading to humans and to the modern apes) issuing directly from the ancestral Dryopithecine stock but tending to conserve the ancestral condition (at least with respect to the dentition) than either of the other two. The significance of the Australopithecines with respect to human evolution was thus more to support the ancestral position of the Dryopithecines than place to the former within the direct line of human descent. Broom agreed that the remains he had discovered were too late in time to be direct ancestors of human beings, but he continued to press for a more direct role for the Australopithecines in human evolution. Differences of interpretation thus continued to characterize the scientific study of *Australopithecus*, but those differences had become far narrower than they had been between the primary writers on the subject a decade earlier.[135]

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[135] Broom, "The Dentition of the Transvaal Pleistocene Anthropoids, *Plesianthropus* and *Paranthropus*," pp.303-14; Gregory and Hellman, "The Dentition of the South African Man-Ape *Australopithecus* (*Plesianthropus*) *transvaalensis* Broom. A Comparative and Phylogenetic Study," pp.339-73; Notice that Gregory and Hellman refer to *Australopithecus transvaalensis* rather than to *Plesianthropus*. They doubted that Broom was justified in creating a new genus to distinguish his discoveries from Dart's.

Subsequent to Gregory and Hellman's departure from South Africa but before the publication of the reports of the dentition of the man-apes, Broom made several further discoveries which made a start towards giving some evidence of the creatures' affinities beyond those of the teeth and skull. From the Kromdraai cave, from which the *Paranthropus* remains had come, he found the lower end of a right humerus (upper arm bone), the upper end of a right ulna (one of the bones in the forearm, the other being the radius), and a toe phalanx, all of which he attributed to *Paranthropus* by virtue of their robust morphology and the their proximity of to where the skull had been found. From Sterkfontein, he got the lower end of the femur (thigh bone), which he attributed to *Plesianthropus*. These were the first post-cranial remains that had been attributed to the man-apes, as Broom had held back attribution of the pelvis after Gregory's warning that it looked like that of a baboon. All of these fragments Broom deemed "nearly human." And since the morphology of long bones and toes is closely correlated with an animal's mode of locomotion, Broom felt comfortable in asserting that both *Paranthropus* and *Plesianthropus* had been bipedal, with their forelimbs therefore free to engage in "the manipulation of sticks and possibly tools." Thus Broom gave the first indication of moving from anatomy to behavior in his efforts build up a credible image of a human ancestor.[136]

While these post-cranial remains were of strong interest due to their comparative rarity, the more plentiful cranial and dental remains continued to be the focus of the scientific work being conducted on both sides of the Atlantic (the

[136] Broom, "Further Evidence on the Structure of the South African Pleistocene Anthropoids," pp.897-99.

Americans had left before the post-cranial remains had been found). After Gregory's South African visit, both he and Broom embarked separately on a project to reconstruct the full skull of Plesianthropus from the known fragments. Gregory and Hellman completed theirs and published a description in 1940 in the *American Journal of Physical Anthropology*. The article was designed to demonstrate how a complete restoration of the skull could be made by inferring the structure of the missing parts from a functional analysis of the traits observed in the known fragments. They began with the upper dental arch, of which a large fragment was known, and worked outwards towards a skull that could conceivable have functioned as an integrated whole. Where parts were missing, analogies were made with living organisms to aid in the reconstructions. All parts involved in chewing, for instance, had to be consistent with *Plesianthropus*'s small human-like canines as well as its large ape-like molars. The authors remained silent on the question of ancestry in the article, but the use of such functional analogies at least forced reflection on the very conceivability of an "ape-man" and all the evolutionary scenarios that depended on such a creature.[137]

By the time that Gregory and Hellman's restoration appeared in the *AJPA*, the Second World War was a year old. The Americans were of course not yet directly involved in the conflict, and the aged Broom was not part of the South African war effort. Nonetheless, the war did have some impact on the progress of their work. As Broom informed Gregory in August of 1940, the Transvaal Museum's preparator was

[137] Gregory and Hellman, "The Upper Dental Arch of Plesianthropus transvaalensis Broom, and its Relation to Other Parts of the Skull."pp.211-228.

off with the South African expedition to confront the Italians in Abyssinia, and the quality and speed with which casts could be prepared were hindered accordingly. Broom's restoration was finally sent of in May of 1941, arriving in New York several months later. This restoration had benefited from the integration of several fragments that had not formerly been available to Gregory and Hellman, prompting Gregory to plan a second restoration. This did not mean, however, that he accepted Broom's model without exception; he felt the canines had been made much too large, and the ascending rami (the back parts of the lower jaw that extend vertically up to the joints with the skull) were too long.[138]

Owing to the diversion of resources for the war effort and the need to work up the available evidence for full publication, new finds from the caves became more sporadic in the first years of the new decade. Not content to wait for his monograph to be complete (the expected date of its appearance kept being pushed back, despite his repeated assurances to Gregory that it was imminent), Broom continued to publish short notes in *Nature* as more fossils emerged from the matrix that had already been taken from the caves. In a 1941 summary of the current evidence, Broom included a new phylogenic diagram that more forcefully showed his position that the South African ape-men could be considered representative of a type ancestral to humans despite the fact that the known fossils were of relatively recent provenance (Fig.3.6). He depicted the three genera of ape-men as twigs extending from a main branch which he labelled *Australopithecinae* (a family designation coined by Gregory).

[138] Broom to Gregory, August 8, 1940, WKGP; Gregory to Broom, October 2, 1940, WKGP; Gregory to Broom, May 4, 1942, WKGP; Broom to Gregory, July 11, 1941, WKGP; Gregory to Broom, July 3, 1941, WKGP.

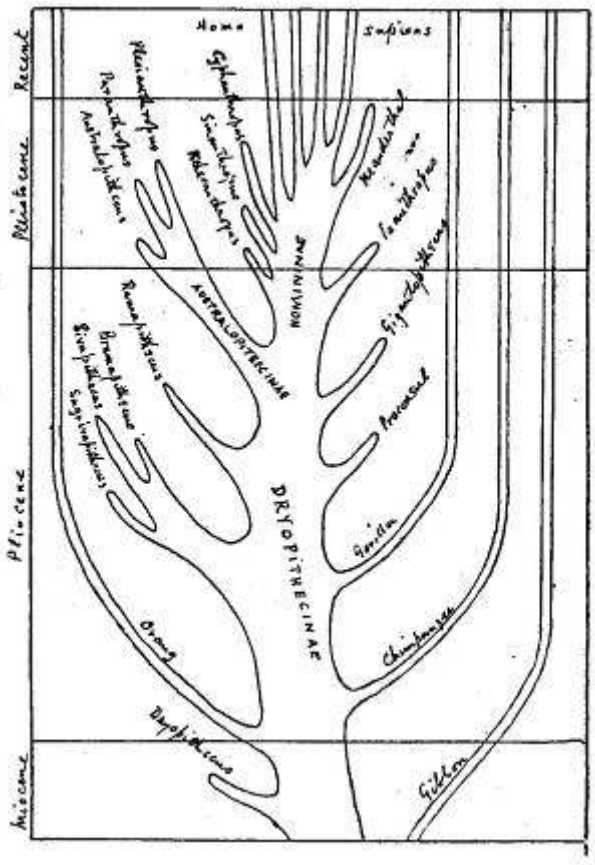

Figure 3.6 – Broom's 1941 phylogeny of human and their near-relations[139]

Significantly, he physically extended that label down into the main trunk of the phylogeny to a point prior to the evolution of modern humans and their near kin but subsequent to the divergence of all extant and extinct apes. This represented visually how an (as yet unknown) Australopithecine from the Pliocene could have been both the ancestor of the later ape-men (which Broom thought it would have resembled very closely) as well as human beings. As Broom put it, "I think there can be no reasonable doubt that man arose in Middle or more probably Upper Pliocene times from a large

[139] Broom, "The Origin of Man," p.13.

Australopithecine ape. The known Australopithecines are near relatives of this ancestor. The living anthropoids are much more remote relatives."[140]

As time passed, Broom's monograph became more conspicuous by its continued absence. Gregory and Hellman had planned to use their second restoration as the basis for a longer and more definitive paper setting out their position on the affinities of *Plesianthropus*. Both restoration and essay were ready by the summer of 1943, but out of a sense of scientific propriety, Gregory told Broom that they would hold back until Broom's expected memoir became available. The failure of Broom's memoir to materialize by this time tried Gregory's patience somewhat, but since he had already been given so much latitude by Broom to publish on the South African fossils, he felt he had to respect Broom's right to priority. As he wrote to an American colleague, Broom was "the still living, still writing discoverer of Plesianthropus and, cranky and queer as he is, I am loth (sic) to deprive him of any publicity."[141]

The causes of the long wait for Broom's memoir were twofold. The first was a matter of collaboration. Broom had originally asked Dart if he would act as co-author of the major memoir. The primary reason for this was that a significant portion of the evidence consisted of endocranial casts which had not yet been satisfactorily analyzed in print, and since Dart was a trained neuroanatomist he was better placed than Broom to do this work. Dart had first accepted the invitation, but subsequently withdrew his involvement in favour of his former student G.W.H. Schepers. He later explained in a

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[140] Ibid., p.13. Other articles published by Broom on the Australopithecine fossils between 1941 and 1943 include: "Structure of the Sterkfontein Ape," p.86; "Mandible of a Young Paranthropus Child," p.607; "The Hand of the Ape-Man, *Paranthropus robustus*," pp.513-514; "An Ankle-Bone of the Ape-Man, *Paranthropus robustus*," pp.689-699.

[141] Gregory to Broom, August 6, 1943, WKGP; Gregory to Howells, November 19, 1943, WKGP.

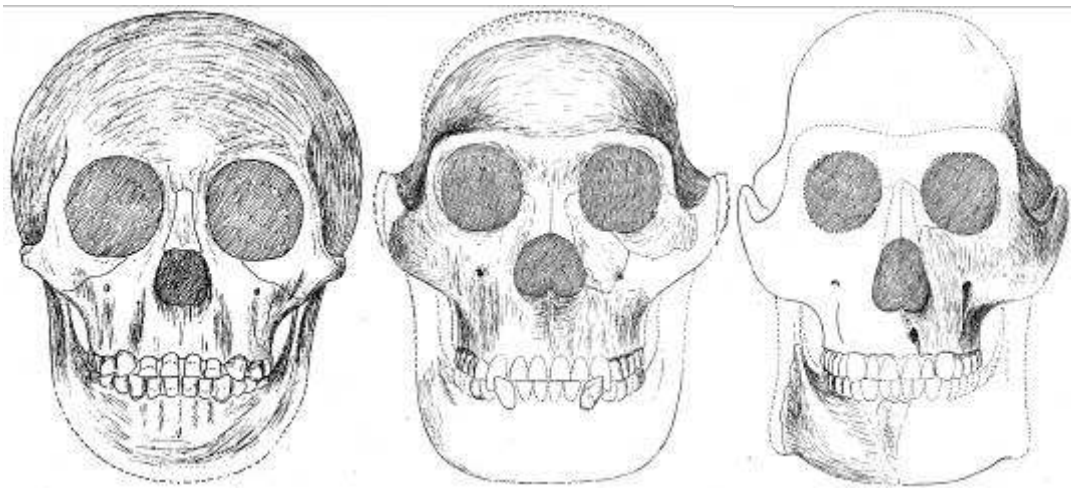
letter to Broom's son (who was researching the life and work of his father) that he feared that his part in the earlier negative reception of Australopithecus by the scientific community would constitute a liability to the credibility of the new work and that "the material itself would be more acceptable if I faded into the background." The second cause for delay was a lack of funds. The memoir had grown over the years into a very large, lavishly illustrated affair that was simply beyond the capabilities of the Transvaal Museum to publish. Dart suggested to Broom that he make an appeal for government funds to the current Prime Minister, General Jan C. Smuts (recall that Dart had previously written to Smuts asking if the state could support the again Broom). Despite the fact that Smuts was head of both the South African government and armed forces and heavily involved internationally in the war effort (he would become one of the authors of the UN Declaration of Human Rights), his enthusiasm for the sciences led him to come to Broom's aid and also to agree to write the forward to the memoir. In Broom's mind, such prestigious support and affiliation were more than warranted by the nature of project at hand. With characteristic immodesty, he wrote to Gregory in late 1944 anticipating the imminent appearance of what he felt would be "the greatest work ever published on the origin of man."[142]

At long last, the monograph appeared in 1946, crediting assistance received in the form of a grant from the National Research Council and Board of South Africa, as arranged by Smuts. The book was divided into two separate parts, the first authored

[142] Dart, *Adventure with the Missing Link*, pp.79-80; Dart to L. Broom, November 8, 1954, RDP, AU8; Broom to Gregory, November 7, 1944, WKGP.

by Broom and the second by Schepers, and ran to a combined two hundred and seventy two pages. The financial support was especially evident in the thirteen glossy plates detailing each piece of fossil evidence individually – sometimes form several aspects (there were one hundred eighty eight figures included in the plates) – not to mention the numerous sketches included amidst the main text. Here was perhaps the sort of thing that Dart would have like to produce fifteen years earlier if only he had enjoyed the sort of patronage that Broom now had.

Broom divided his part into four subsections, the first three describing the remains of each of the genera (Figs. 3.7-3.9) and the fourth dealing with their evolutionary relationship to humans and apes. Much of what he had to say was merely a more detailed version of what he had been arguing for years in shorter form: the teeth were far more human than ape; both cranial and post-cranial remains indicated a creature that walked upright and used its hands for manipulation; the creatures, if too late in time to be direct ancestors themselves (and he did not concede that there was enough geological evidence to make this certain) were doubtless the little changed descendants of creatures who had also spawned the human line of descent. However, in the section on the affinities of the Australopithecinae, he introduced a radical break with all of his previous theorizing on the subject of human evolution. The change did not affect his view of the relationship of the South African ape-men to human beings, but rather his view of the relationship of both of these to all living and extinct forms of ape. Previously, as we have seen, Broom had been happy to adopt Gregory's position that the Australopithecines, along with humans and all the



Figures 3.7, 3.8, and 3.9 – Broom's frontal view sketches of the type specimens of *Australopithecus, Plesianthropus, and Paranthropus*, respectively, with the known parts shaded and the rest inferred[143]

living species of great ape, displayed variations on the *Dryopithecus* pattern of molar cusps, which Gregory argued was evidence that all of those forms had evolved from the Miocene ape *Dryopithecus* or something very similar. All of the structural similarities between the Australopithecines, humans, and apes could thus be said to have resulted from this common ancestry. This had been the basis of Gregory and Hellman's support for Broom's contention that the Australopithecines were very near relatives of humans, if not their direct ancestors, and Broom had made liberal use of Gregory's standing among palaeontologists to help secure his position. Now Broom abandoned the *Dryopithecus* theory altogether.

To make his new position all the more clear and contrast it with the of Gregory and several others, Broom included a set of four phylogenic diagrams in the final

[143] Broom & Schepers, *The South African Fossil Ape-Men,* pp.32, 45, 91. It is not clear why Broom shaded the cranial dome of *Australopithecus*, as this was never discovered.

section of his part (Fig. 3.10). The phylogeny attributed to Gregory's position in 1937 (when he had come to South Africa to examine the Australopithecine material) shows humans and their near relatives, Australopithecines, the living great apes, and *Dryopithecus* all descending from a common Miocene ancestor, presumably very close to *Dryopithecus*. In contrast, Broom's new phylogenic arrangement had the common lineage of the Australopithecines and humans diverging from that of all the apes (including Gibbons) as far back as the boundary between the Oligocene and Eocene in the early Tertiary period. This distant ancestor, Broom now argued, was nothing like an ape but rather a much more primitive primate that had none of the characteristics of the *Dryopithecus* pattern. He wrote that "all the apparent resemblances between the gorilla and chimpanzee and man are due to parallel developments," the very sort of argument that Gregory had long held to be antithetical to the foundational principles of evolutionary reasoning, and which Broom had himself derided in his 1933 book.[144] Broom's view now looked much more like that of Gregory's late superior at the American Museum of Natural History, Henry Fairfield Osborn, who had argued for an even more distant relationship between the apes and human (the phylogeny attributed to Osborn's 1927 position does not give a place to the Australopithecines, as Osborn never commented on the South African ape-men). This would have been particularly hard to swallow for Gregory, as his arguments had largely been motivated by his intense opposition to Osborn's view and

[144] Ibid., p.140.

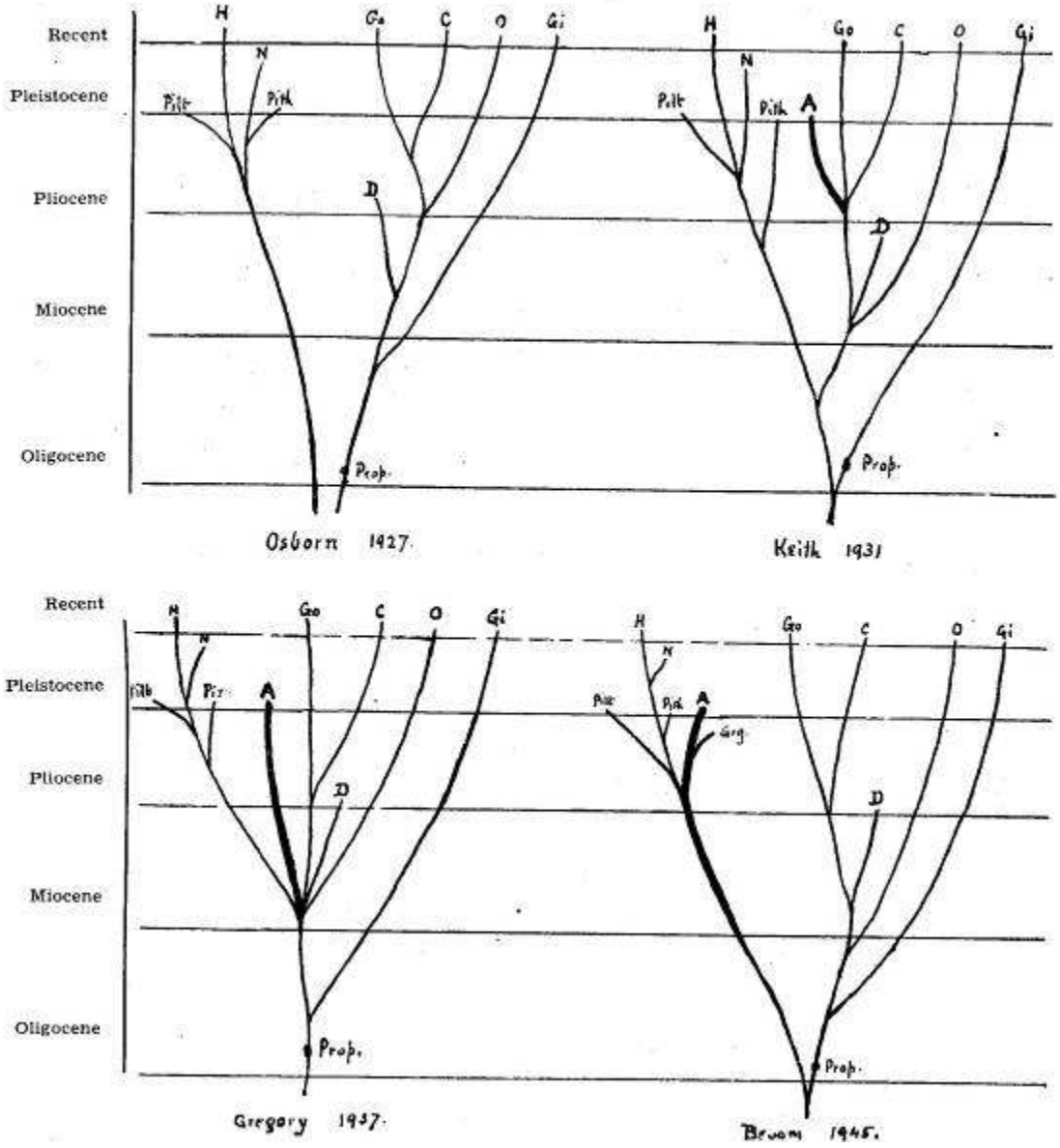


Figure 3.10 – Broom's comparison of his 1946 revised phylogeny with those of Osborn, Keith and Gregory (A=Australopithecinae, H=Homo, D=Dryopithecinae)[145]

[145] Ibid., 139.

what he took to be its negative implications for evolutionary science in general. Ironically, Broom's phylogeny in some ways now resembled that which his erstwhile nemesis Arthur Keith had advocated in 1931, except of course that Keith had allied *Australopithecus* to the living apes rather than to humans.

Broom's justification for changing his view of the relationship between humans/Australopithecines and the apes lay in dental anatomy – as did Gregory's justification for holding the opposite view! Aware that his previous endorsement of Gregory's position had also been largely founded on dental anatomy, Broom now explained that the change of mind had come as a result of further discoveries of the milk molars of the Australopithecines (formerly, evidence of the milk dentition had come only from the Taung skull, whereas Broom now had evidence of the milk dentition of *Paranthropus*). According to Broom, the milk molars of the Australopithecines were almost identical to that of humans, and very different from that of the apes. Further, he argued that the Australopithecine/human milk molars were the more "primitive" of the two types, meaning that they more closely resembled the form that existed in the common ancestor of apes and humans than did the milk molars of the modern apes. Conversely, chimpanzee and gorilla milk molars were highly "specialized", meaning that such forms had only evolved subsequent the divergence of the human/Australopithecine and ape lineages. The upshot of this for Broom was that the common ancestor of apes and humans could not have been at all "ape-like", since Broom's view of directional and irreversible evolution (as laid out in his 1933 book) did not permit "primitive" or "generalized" structures to evolve from

"specializations". As he put it, "while the anthropoid type could be evolved from the hominid type there seems no possibility of the hominid type having been evolved from the anthropoid type." Thus did Broom's commitment to a particular view of evolutionary change trump his scientific alliance with Gregory on the question of the ape/human relationship, though they remained largely as one on the question of the close relationship between humans and the Australopithecines.[146]

Arguing from the neuroanatonical evidence provided by the several endocranial casts of the Australopithecines, Schepers also supported the view that the human/Australopithecine lineage had evolved from a pre-anthropoid primate, and that apparent similarities with the apes were the result of parallel evolution. Indeed, he cast himself and Broom as a radical challengers to the orthodox view of human beings having descended from an ape. Such a characterization would likely have brought a smile to the face of the self-styled anti-authoritarian Broom, despite the fact that he had until recently advocated the opposite conclusion. Schepers's section was radical in several other respects, not least in the fact that he claimed that the physical evidence of the Australopithecine brain structure left in the endocranial casts allowed him to concluded that

> these fossil types were capable of functioning in the erect posture, of using their hands in a limited sense not associated with progression, of interpreting their immediately visible, palpable, and audible environment in such detail and in with such discrimination that they had the subject matter for articulate speech well under control, and, of having developed motoric centres for the appropriate application; they were also capable of communicating the acquired information to their

[146] Ibid., p.140.

> families, friends and neighbours, thus establishing one the first bonds of Man's complex social life.[147]

Such claims as these harkened back to Dart's sweeping characterization of the waking life of *Australopithecus* from the original 1925 article. For all the criticism that Dart had received about being overly liberal in his inferences of behavioral facts from physical evidence, a major goal for writers like Broom and Schepers had remained to show that the Australopithecines had not only looked like humans in a number of respects, but had also acted like them.

The publication of the 1946 monograph in many ways represents the end of one era in the scientific debates over the Australopithecines and the beginning of another, in which a full description of all the fossil evidence was available to scientists worldwide in a single work. This was significant advance whether or not one agreed with the authors' conclusions. In the next chapter we will begin to look at the fallout from the first era among scientists in Britain and America, whose reactions were by no means homogeneous. For Broom, whose delusions of grandeur always masked the uncertainty of scientific debate, the outcome was certain: "When my book comes appears I think the opposition will not have a leg to stand on," he wrote to Gregory before the publication date, "But what a time it had taken 1925-1945! 'What fools these mortals be!'"[148]

With the publication of the 1946 monograph, Broom's scientific stock reached its zenith. Without letting go of his self-image as a radical and an outsider, Broom had

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[147] Ibid., p, 253.
[148] Broom to Gregory, November 7, 1944, WKGP.

nonetheless become a knowledge broker with as much leverage in the field of human origins as any other living worker. He was also eighty years old, with no intention of slowing down with age. Writing to Gregory two years previous, having read of his retirement from the American Museum of Natural History, Broom challenged the American to keep up with his scientific work by boasting of the intensity of his own continued lecturing, excavating, and writing.[149]

As we have seen, Broom's recent work had been helped by government funding secured largely through the intervention of the war-time Prime Minister and military commander Jan Smuts. Such was Smuts's interest in Broom's continued research that he contacted Broom to encourage the resumption of excavations (with funding guaranteed), and this while he was preparing to travel abroad to engage in the weighty matters of post-war statesmanship. However, just as Broom was about to resume work, he was unexpectedly told by the South African Historical Monuments Commission that they would not permit him to continue his individual work in the caves. Rather, they wanted a "systematic" excavation by a team of experts, notably a "competent field geologist." Broom later reported that while had wanted to simply flout the directive, the board of his Museum was reluctant to have one of their workers break the law (the Commission was legally entitled to regulate excavations). The strategy was to wait for Smuts to return, and when he did, as Broom expected, he

[149] Broom to Gregory, November 7, 1944, WKGP.

asked the head of the Commission, the prehistorian C. Van Riety Lowe, not to interfere with Broom's researches. Once again, political patronage had paid off.[150]

As Broom recounts the subsequent events in his autobiography, he resumed work at Kromdraii upon receiving Smuts's assurances, only to receive a permit to do so from the Commission after the fact. True to his iconoclastic nature, he decided to move all work to Sterkfontein precisely because the commission had not given him permission to do so. The result was a propagandistic coup. In April of 1947, Broom and his assistants unearthed the most complete and best preserved skull of an Australopithecine that had yet been found. His first action, before even removing the skull from the matrix, was to telephone the *Johannesburg Star*, which sent out a reporter and a photographer, and, in addition to a report in *Nature*, news of the find quickly spread through the work press. However, the Commission was not impressed, and they sent a deputation to Smuts arguing that Broom's illegal excavations paid insufficient evidence to stratigraphy, and that his focus on blasting out new fossils was destroying evidence that might be used to settle the ongoing questions as to the age of the specimens. Broom was made to stop work, but when an independent party was sent to Sterkfontein to settle the question, he reported that the deposit contained no discernable stratigraphy and thus no question of geological dating. The Commision was forced to let Broom resume work.[151]

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[150] Smuts to van Riet Lowe, December 31, 1946, *Selections from the Smuts Papers*

[151] Broom, "Discovery of a New Skull of the South African Ape-man, *Plesianthropus*," p.672; "The Most Perfect Prehuman Skull Ever Found. The Discovery of a Nearly Perfect 'Missing Link' Skull," pp.505-509; the account of the conflict comes from Broom's autobiography (Broom, *Finding the Missing Link*, pp.62-68), and while the tenor of his recounting is clearly biased towards his own

True to his word, Broom's torrent of discoveries and publications, now largely undertaken in partnership with his assistant and eventual successor John T. Robinson did not slow down right up to his death in 1951. Among the more notable were the right pelvic bone of *Plesianthropus* (this time clearly not that of a baboon), various remains of a new species they called *Paranthropus crassidens*, and a new genera that Broom and Robinson believed to be more advanced towards the human than the other Australopithecines, which they called *Telanthropus*. As a last triumph over those who had questioned his geological competence, Broom was awarded one of the field's highest honors, the Wollaston Medal from the Geological Society of London.[152]

Beyond the individual achievements of Broom's twilight years, he continued to play an important role in bringing important international scientists into contact with the Australopithecine material. In 1949, his work at the Swartkrans cave where the new *Paranthropus crassidens* was found was done in conjunction with the University of California African Expedition, led by Wendell Phillips, whose Cairo to Cape Town voyage was then on its last leg. More importantly for the fortunes of the ancestral claims of the Australopithecines, he played host in late 1946 and early 1947 to the Oxford anatomist and primate expert Wilfred E. Le Gros Clark, just as he had done earlier with Gregory. As we will see, Clark would become the chief supporter of Dart

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righteousness in the matter, there is no evidence to suggest that the facts reported therein are misrepresented.

[152] These discoveries are documented in a number of publications. The following represent the first announcement of each, respectively, in the scientific literature: Broom & Robinson, "Further Remains of the Sterkfontein Ape-Man," pp.430-431; Broom, "Another Type of Fossil Ape-Man," p.57; Broom & Robinson, "A New Type of Fossil Man," pp.322-323. The announcement of Broom's receipt of the Wollaston Medal can be found in *Nature* (January 29, 1949), p.165.

and Broom's claims in Britain throughout the nineteen fifties, when opposition to those claims took a new and unexpected turn.[153]

[153] The University of California African Expedition has recently been documented in a memoir by two of its non-scientific participants in Terry, *An African Expedition*.

Chapter 4 – Measuring *Australopithecus*

By publishing a monograph-length report on the Australopithecine discoveries, Broom succeeded where Dart had not. A number of factors contributed to the different outcomes: Broom's accumulation of new fossils continued to arouse new interest throughout the time it took him to prepare the monograph, whereas Dart's isolated skull was rather old news by the time he got around to writing his; Broom had gained the support of a prominent international expert after Gregory had examined the fossils, whereas Dart had been continuously bedeviled by Keith's attacks and Elliot Smith's reluctance to take a unequivocal stance in the controversy; Broom was able to publish through the auspices of his home institution, whereas Dart sought publication through the Royal Society of London through Elliot Smith's mediation, and was denied.

By placing his fortunes in the hands of Smith and the Royal Society, Dart was seeking more than just the appearance of his work in print: he wanted the publication of his monograph to give his claims about *Australopithecus* the credibility in Britain that they had so far been denied. Broom may have avoided Dart's difficulties by keeping the work of publication closer to home, but he also left open the question of whether the publication of his monograph would have any impact on the perception of the Australopithecines in British scientific circles. Thus, the appearance of a lengthy and very positive review in *Nature* by the eminent Oxford anatomist Wilfrid E. Le Gros Clark could not have been more significant. "Dr. Broom has demonstrated

beyond any doubt at all," wrote Clark, "that the Australopithecines are extremely important for the study of human evolution, since they present an astonishing assemblage of simian and human characters. Such an assemblage, indeed, might well be postulated, entirely on indirect evidence, for hypothetical ancestors of the Hominidæ." Clark even extended his praise retrospectively, offering Dart the sort of endorsement that he had sought from British authorities more than a decade earlier: "Thus it should be said at the outset of this review that Dart's original interpretation of the *Australopithecus* material had in several respects been completely vindicated."[154]

If this review had been the full extent of Clark's contribution to the matter, his importance to trajectory of the Australopithecine debate might not have exceeded Gregory's. However, shortly after his review was published, Clark traveled to South Africa to examine the fossils for himself. His return to Britain marked a crucial shift in the geography of scientific opinion about the status of the Australopithecines: for the first time, an influential British scientist began to publish actively in support of the ancestral claim. Gregory's public support had been important, but his subsequent involvement in the debate was minimal. In contrast, Clark made the Australopithecines a centerpiece of his career and staked his scientific credibility on the claim of human ancestry. The strong interpersonal and institutional ties in the British scientific community that underpinned Clark's credibility contrasted with Dart and Broom's marginality, and he was able to use those ties to secure the ancestral status of the Australopithecines more effectively than the South African workers had

[154] W.E. Le Gros Clark, "Significance of the Australopithecinæ," p.863.

so far been able to do. In effect, Clark's credibility as a scientist became the Australopithecines' credibility as human ancestors.

That said, skepticism about the ancestral claim did not suddenly vanish with Clark's intercession. The difference was that questions about the ancestral status of the Australopithecines were now also questions about the scientific judgment of one of Britain's most respected anatomists and primate experts. Earlier skepticism had, of course, born on Dart and Broom's ability and credibility as scientists, but questioning the credibility of the Oxford professor was simply a weightier matter. Thus, when the anatomist Solly Zuckerman, who had previously worked under Clark at Oxford, began in 1949 to question the statements that Clark and the South African workers had made, he had his work cut out for him.

A stubborn and irreverent character, Zuckerman was not one to shy from argument, and his insistent criticism led Clark into an acrimonious controversy that lasted through the mid-fifties. While some of this newly rekindled controversy recalled elements of the earlier disagreement pitting Dart and Broom against Keith, the central aspect of Zuckerman's dissent from the position being advocated by Clark and the South Africans was something new and more fundamental: method. How did one arrive at a properly scientific judgment in comparative anatomy – for instance, about relative similarity or difference of the Australopithecine fossils to the analogous structures in human beings and the living species of ape? In Zuckerman's estimation, modern biology, having recognized the pervasiveness of variability within taxonomic groups, required two things: extensive measurement and rigorous statistical analysis.

Zuckerman claimed that assertions that the Australopithecine remains showed those creatures to have been very different from the living apes had not been arrived at through proper biometric and statistical methods, and hence could not be counted scientific. He launched a series of studies using the methods he advocated, and then claimed to have found that the Australopithecines were in fact not so unlike the apes after all.

Clark took the criticism as an attempt to discredit him personally, and launched a counteroffensive on the same methodological plane as Zuckerman's attack. In print, he questioned every aspect of Zuckerman's method, from the way the latter parsed his data to the very relevance of statistical analysis to the problems at hand. In private correspondence, he characterized Zuckerman's actions as personal attacks unbefitting proper scientific conduct, and derided him for blocking scientific progress with needless and inappropriate controversy. By the latter part of the fifties, Clark had managed to see off Zuckerman's challenge, though Zuckerman himself never changed his mind.

This period in the history of the Australopithecine debate was in a sense an inversion of that which had preceded it. For most of the first two decades of the debate, the advocates of humankind's Australopithecine ancestry had found themselves in the position of dissenters from British expert opinion. Clark shifted the balance of the debate, and while it would be too strong to claim that he forged a complete consensus around his opinion, he did bring the ancestral claim into the establishment of British science. Broom had worn the mantle of the dissident outsider

with pride as he assailed what he saw as the unjustified consensus that Australopithecus was an ape; that mantle now passed to Zuckerman, equally at home playing the pugnacious rebel, who questioned the science behind the increasing number of assertions that the South African creatures were not in fact apes. As for themselves, the fossils continued in their mediatory role as the human players switched sides around them.

Le Gros Clark

After serving in World War I, W.E. Le Gros Clark (1895-1971) – Le Gros to his intimates – was appointed Chief Medical Officer for the British Colony of Sarawak on the island of Borneo. There, he developed his interest in primate anatomy and evolution by collecting specimens of Spectral Tarsiers and Tree Shrews, some of which he sent to Grafton Elliot Smith at UCL. From 1934, he served as Professor of Anatomy at Oxford University, where he led a modernization of the institution's laboratory facilities. That same year saw the appearance of his first major book on Primate Evolution, *Early Forerunners of Man*. As the title indicated, Clark was mainly interested in establishing the affinities of the most primitive groups of Primates such as those he had collected in Sarawak. Dart's *Australopithecus* was limited to a single mention in a list of extinct anthropoids (*Dryopithecus, Sivapithecus* etc.) to demonstrate that this group had seen a marked radiation of forms during the late Tertiary period, without acknowledging the claim that *Australopithecus* bore any special relation to humans. Two years later, in a paper comparing the endocranial cast

of Dart's *Australopithecus* with those of chimpanzees published jointly with the young South African Solly Zuckerman and another investigator, Clark argued *contra* Dart that "there is nothing to suggest that *Australopithecus* possessed in its cerebral anatomy any features indicative of an approach to the human status."[155]

Overall, primate fossils were not one of Clark's primary interests in the early decades of his scientific career, but that changed with the appearance of Broom and Schepers' monograph. In December of 1946, shortly after the appearance of his review, Clark set out on a trip that was to take him first to South Africa to examine the Australopithecine remains, and then on to Nairobi, Kenya, where Louis and Mary Leakey had organized a Pan-African Congress on Prehistory, to be held in the new year. Unlike earlier transcontinental visitors, Clark took advantage of the new post-war availability of commercial air-travel, making the journey a matter of days rather than weeks. For their part, Dart and Broom were prepared to offer their important visitor all the hospitality they could muster. Dart and his new wife met Clark at the Johannesburg airport, and he stayed at their home while he was in Johannesburg. He traveled with Dart and Broom to the various caves at which discoveries had been and were being made, and studied the fossils that each held in their respective institutions. While Clark was examining the fossils held at the Transvaal Museum, Broom even insisted on taking him to lunch every single day at a local hotel. Evidently such

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[155] Clark, *Early Forerunners of Man;* Clark, Cooper, and Zuckerman, "The Endocranial Cast of the Chimpanzee," p.268; For biographical information on Clark see Clark, *Chant of Pleasant Exploration*, and Zuckerman, "Wilfred Edward Le Gros Clark, 1895-1971." For reasons that will become clear as this chapter progresses, Zuckerman's assessment of Clark's work must be taken with a grain of salt.

efforts made their mark, for Clark's diary and letters home were full of praise for the hospitality shown him and the free access he was given to all the specimens.[156]

Clark, Broom and Dart traveled together to Nairobi in January, 1947, to attend Leakey's Pan-African Congress on Prehistory, where they were joined by a good number of the world's leading students of prehistory,. The venerable old French prehistorian Abbé Henri Breuil was elected President of the Congress, with Broom as Vice President, and Dart chairing (and Clark vice-chairing) the section on Human Palaeontology (the other sections were i. Geology, General Palaeontology and Climatology and ii. Prehistoric Archaeology). The delegates resolved, among other things, to hold the Congress quadrenially and to encourage various governments of the continent to take an interest in and support the study of African prehistory. The Australopithecines were well represented among the papers given, as Clark, Dart, and Broom all based their comments around the South African creatures, as did Camille Arambourg from the Musee Nationale d'Histoire Naturelle in Paris. Broom's paper involved only a recounting of his discoveries and the interpretations of the fossils that had frequently appeared elsewhere.[157] Clark's report on his recent study of the South African fossils made the novel move of describing them as part of the superfamily *Hominoidea*, a term recently coined by George Gaylord Simpson (W.K. Gregory's successor at the American Museum of Natural History) to contain humans, apes, and

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[156] Diary of Trip to Africa, 1946/47, Ms. Eng. Misc. f. 838, Le Gros Clark Papers, Bodleian Library, Oxford University (hereafter LGCP); the letters with relevant passages include Clark to "Everyone", December 19, 1946, Clark to "Everyone", December 25, 1946, Clark to Everyone, January 1, 1947, Ms. Eng. Lett. c. 585, LGCP.

[157] Robert Broom, "The Fossil Ape-Men of South Africa."

their extinct ancestors.[158] Here, Clark stated publicly for the first time his support for the claim that "the Australopithecinae represent an extinct group of the Hominoidea which must be associated with the line of hominid evolution rather than that leading to the modern large apes, and which almost certainly were closely related to (and perhaps survivors of) the ancestral stock from which *Homo* was derived," which, Clark stressed, Dart and Broom had argued all along.[159]

In addition to the Australopithecines, Clark cited another, more primitive branch of extinct African Hominoidea that was known from the environs of Lake Victoria courtesy of Leakey, among others. These Miocene forms, which included the genera *Proconsul* and *Limnopithecus*, represented for Clark evidence for the early radiation of the Hominoidea, allowing for a remarkably rich, multi-stage picture of that group's evolution in Africa.[160] Arambourg took things even further, showing in diagram his belief that *Limnopithecus* represented an ancestor from which the Australopithecines and ultimately humans sprang, while *Proconsul* represented the basal stock form which the great apes sprang (Fig.4.1).

Upon his return to England, Clark's prominent position in British science gave him ample opportunity to air his newfound support of Dart and Broom's position. His first venue was the 1947 meeting of the Anatomical Society. In the written version of his remarks, published in the Society's journal, Clark noted that while his observations were for the most part repetitions of those already made by the South African workers,

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[158] G.G. Simpson, "The Principles of Classification and a Classification of Mammals."
[159] W.E. Le Gros Clark, "Anatomical Studies of Fossil Hominoidea from Africa," pp.113-14.
[160] Ibid., pp.114-15.

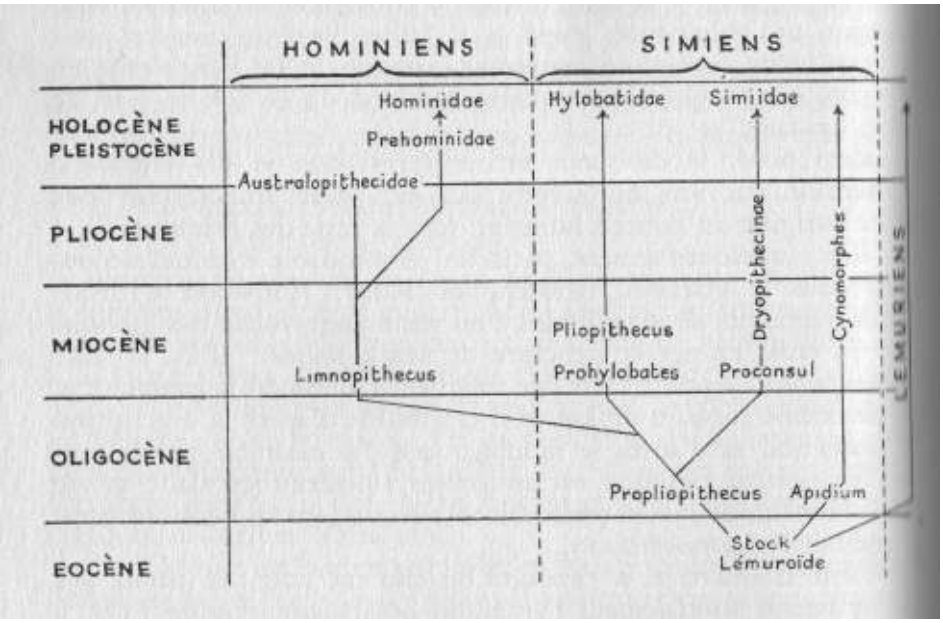

Figure 4.1 – Arambourg's phylogeny of the Primates from the 1947 Pan-African Congress[161]

their reemphasis was made necessary by the fact that "several anatomists of recognized distinction have, by their interpretation of the evidence, tended to belittle [the Australopithecine fossils'] importance."[162] The implication was that Clark's authoritative voice, made all the more so in this case by his examination of the original specimens, would serve as a corrective. His main points were indeed familiar: the Australopithecine teeth were virtually human; the anatomy of the femur suggested an upright gait; if the geological evidence was such that the South African remains were too recent for the creatures to have been direct ancestors of humans, they could nonetheless represent little changed survivors of an ancestral stock.

[161] Arambourg, "Observation sur la Phylogenie des Primates et l'Origine des Hominiens," p.118.
[162] Clark, "Observations on the Anatomy of the Fossil Australopithecinae," p.300.

Clark made these remarks before he knew of Broom's discovery of the remarkably complete right pelvic bone. When he was invited by the Geological Society to give the annual William Smith lecture for 1949, he took the opportunity to illustrate how the short, wide shape of the Australopithecine hip bone conformed far more closely to its human counterpart than to the long, narrow hip bones of the living apes (Fig. 4.2). For Clark, this nailed down the argument for the Australopithecines' bipedalism: as in humans, the wide surface of the ilium (the upper part of the hip bone) would have anchored a robust gluteal musculature (absent in apes), the function of which is to propel the body forward in the act of walking.[163]

Clark also took the opportunity to follow up on his earlier remark about the tendency of critics to belittle the importance of the Australopithecine remains by considering the causes of their resistance. One obvious reason was that most non-South African scientists had not had the opportunity to see the original fossils. These were now so abundant that no charge of insufficient evidence could be maintained, giving Clark "the impression… that some critics are reluctant to accept the evidence at face value precisely because it *is* so abundant and so consistent – because it seems, in fact, almost 'too good to be true.'" His counsel to colleagues was to withhold judgment until they had seen all the evidence for themselves, and to "avoid confusing the real issues by the introduction of unnecessary invective into discussions." Until then, they would have to rely on Clark, who had seen all the evidence for himself, and

[163] Clark, "New Palaeontological Evidence Bearing on the Evolution of the Hominoidea," p.255.

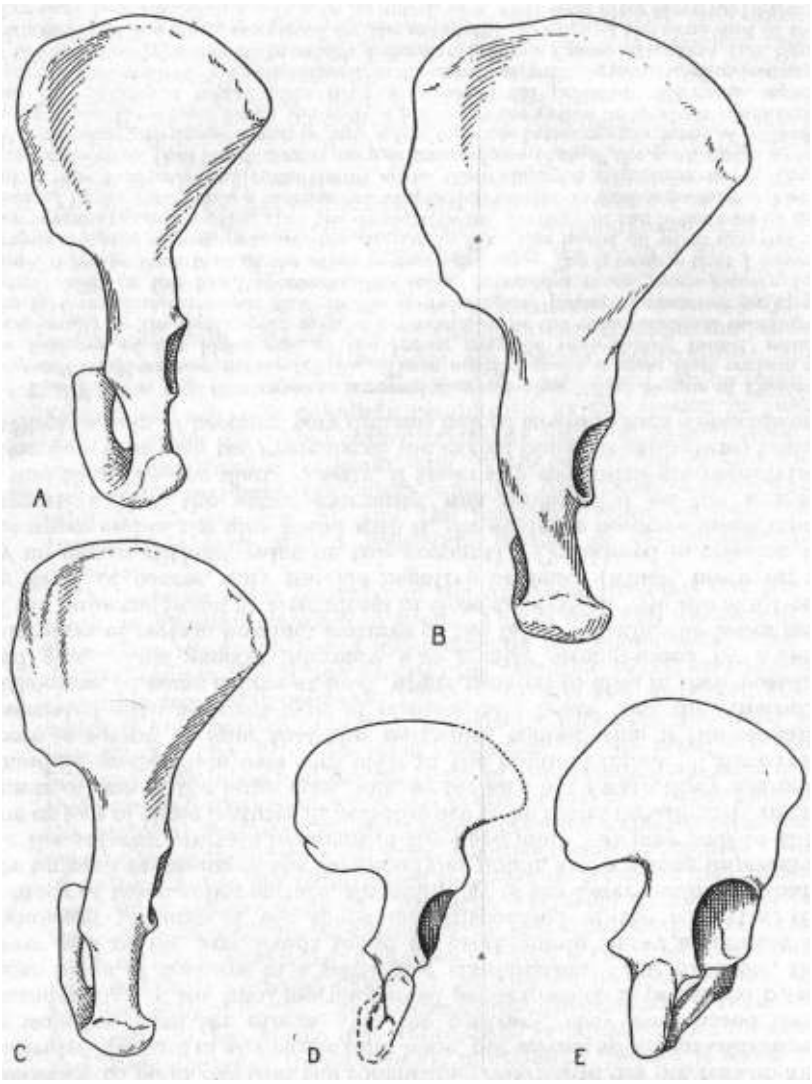

Figure 4.2 – Clark's comparison of the os innomatum (hip bone) of an A) Orangutan, B) Chimpanzee, C) Gorilla, D) Australopithecine and E) Human[164]

[164] Ibid., p.254.

who, presumably, had kept such dangerous emotional baggage out of his assessment.[165]

It was not just professional scientists who would be relying on Clark for insight into the nature of the Australopithecines. Students and interested members of the public were also provided with a guide to Clark's view of human evolution in his *History of the Primates: An Introduction to the Study of Fossil Man*, published in 1949.[166] The thin volume, published by the British Museum of Natural History, was intended as a replacement of the old guide to the Museum's collection of fossil remains of man by A.S. Woodward, which had last been updated in 1922. It was a testament to Clark's scientific stature that he was asked to bring the guide up to date. Unlike the old version, Clark's was not really organized as a guide to the particular holdings of the British Museum, but rather as a general introduction to primate evolution and survey of all the fossil evidence bearing on the emergence of human beings. Nonetheless, anyone who bought the volume in the Museum shop and consulted it as they perused the casts of the Australopithecines in the displays would have read that these were the remains of a bipedal creature whose anatomical similarities to human beings made them central figures in the evolution of our species.

Zuckerman

Clark had come to believe that the question of the Australopithecine's relative proximity, morphologically speaking, to humans or apes had been settled firmly on the

[165] Ibid., p.258.
[166] Clark, *History of the Primates*. The guide went through five editions and was printed into the nineteen-seventies, demonstrating Clark's continuing influence over the field.

human side, and he urged his peers to follow him in this judgment. Not all scientists were persuaded, however. Indeed, the one scientist who was to prove the most persisted thorn in Clark's side in the years that followed was one of his own former protégés: Solly Zuckerman.

Zuckerman (1904-1993) was born in South Africa, and, like Dart before him, went to London as a young researcher to work with Grafton Elliot Smith at University College.[167] During his time there he undertook, at Elliot Smith's behest, a study of age-changes in the chimpanzee in order to secure data for comparison with Dart's recently discovered juvenile Australopithecine. His conclusions cast doubt on Dart's claims about the human affinities of the skull, and he argued that the adult skull and brain would likely fall within the ranges presented by living apes. From 1928 until 1932, Zuckerman worked as the staff anatomist of the Zoological Society of London, during which time he developed an interest in the social relations among monkeys and apes.[168] In 1934, Clark, who had just taken up the professorship at Oxford, invited Zuckerman to work with him and successfully helped him secure funding to do so. As we have seen, in 1936 Clark and Zuckerman undertook a comparative study of the endocranial cast from the Taung skull with the brains of chimpanzees, concluding that in the *Australopithecus* brain did not demonstrate any human affinity.

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[167] Zuckerman published two volumes of autobiography: Zuckerman, *From Apes to Warlords,* and Zuckerman, *Monkeys, Men & Missiles*. Further details can be found in Krohn, "Solly Zuckerman, Baron Zuckerman, of Burnham Thorpe, O.M., K.C.B."

[168] In 1932, Zuckerman published *The Social Life of Monkeys and Apes*. He was an important figure in the development of Primatology in the 20th century; see Burt, "Solly Zuckerman."

After the war, Zuckerman took a position at Birmingham University, where he would remain until 1968.[169] Unlike his former mentor, Zuckerman's skepticism about the human affinities of the South African fossils was not altered by the publication of Broom's 1946 monograph. He felt dismayed that Clark had gone over to Dart and Broom's side when, in Zuckerman's mind, no improvement had been achieved in securing a sound scientific basis for the Australopithecine claim. His objection was methodological: in order to say definitively that the Australopithecines differed significantly from any of the living apes in some respect, the scientist should have in mind the range of variation demonstrated by each of the apes for the trait in question – as determined by a statistical analysis of measurements taken from a sample large enough to be representative of ape populations as a whole. The claims that had been made on behalf of the Australopithecines seemed to Zuckerman to have based on mere qualitative comparisons of the fossils with individual examples of modern apes. It was in this sense that he found Clark's report to the Anatomical Society in 1947 wanting, as he expressed to the Johns Hopkins physical anthropologist Adolph Schultz: "I am afraid [Clark] hardly improved on Broom. Comparisons were made with the chimpanzee or the gorilla, when what was in mind was a single skull which had been used as a standard, and not a series of observations sufficiently reliable

[169] Zuckerman contributed heavily to the British war effort, beginning with studies conducted on animals to precisely determine the effects that high explosives would have on humans. Zuckerman's work became increasingly influential as the war progresses, determining in some cases the manner in which bombing campaigns were conducted. See the relevant sections of his autobiography as well as Krohn, "Solly Zuckerman," pp.581-83.

statistically to form a basis for comparison."[170] The letter was headed "Confidential," suggesting that Zuckerman believed that any publicizing of his criticisms would arouse the sensitivities of those criticized. His suspicion would soon be confirmed.

Over the next several years, Zuckerman and his Birmingham colleague E.H. Ashton compiled a large number of measurements of the teeth of the living great apes from collections around England. They measured each tooth in several dimensions, and used those to calculate various morphological indices.[171] Corresponding measurements and indices were grouped by kind of tooth (milk/permanent, incisor/canine/premolar/molar), species (chimpanzee, gorilla, orangutan), and sex, and each subset was subjected to statistical analysis to determine the mean and standard deviation. The resulting values constituted the sort of base upon which Zuckerman believed that a properly scientific assessment of the affinities of the Australopithecines could be made.

Zuckerman made public the preliminary results of his comparative study in August 1949, at the fourth Summer Seminar in Physical Anthropology in New York. The meeting was notable, among other reasons, for the presence of Dart himself, who had recently re-entered the debate with finds from a new site in South Africa and a theory about the Australopithecines' tool-using ability.[172] Before Dart and the rest of the attendees, Zuckerman voiced his dissatisfaction with the lack of biometric analyses and told the audience that his comparison of the various dimensions and indices of

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[170] Zuckerman to Schultz, 5 May, 1947, SZ/GEN/SCHULTZ, Solly Zuckerman Papers, University of East Anglia (hereafter "SZP").
[171] An index, in this sense, is the ratio of one measurement to another; for example, width/height.
[172] Dart's involvement in the debate in the post-war period will be examined at length in Chapter 6.

Australopithecine teeth with analogous measurements from the living apes had not borne out the claim that Australopithecine teeth were more human that ape.[173]

Shortly after the meeting, a summary of the proceedings was published, and Zuckerman's position became a matter of public record. It soon came into the hands of Robert Broom, who wrote to Zuckerman to insist on his ability "to distinguish any Aust. tooth from that of any anthropoid or cercopith."[174] What Broom does not seem to have understood, at least in writing this particular letter, is that it was precisely this purported ability to discriminate, without broad and explicit quantitative underpinning, to which Zuckerman objected. What those with access to the original specimens claimed to see plainly in the fossils mattered less than what Zuckerman could show with numbers. Zuckerman showed a copy to Broom's letter to Ashton, who evidently read into it evidence that their work had been taken as a threat: "Ashton thinks fear rather than anger," Zuckerman scrawled across the top. Responding to Broom, Zuckerman pulled no punches: "The fact of the matter is that whatever qualitative impressions you may have, the dimensions of the Australopithecine teeth that have been published do not differ significantly from those of existing anthropoid apes – a conclusion which is supported by any test of significance that one could devise."[175]

The published summary of the New York seminar had noted that some of the participants "regarded the emphasis placed upon single unit comparisons as a weakness of Zuckerman's study," and that the seminar's "consensus was that

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[173] Kaplan, "The Fourth Summer Seminar in Physical Anthropology," p.27. The proceedings of the seminar were summarized by Kaplan, rather than recorded verbatim.
[174] Broom to Zuckerman, 26 Feb, 1950, SZ/GEN/BROOM, SZP.
[175] Zuckerman to Broom. 20 Mar, 1950, SZ/GEN/BROOM, SZP.

acceptance of the results of Zuckerman's study regarding the status of the Australopithecinae would have to be withheld until such time as both the Australopithecine and human series and the hominid and anthropoid ape series have also been compared in like fashion."[176] Not wishing this to remain the final word in the public record until the full publication of the study – then still in press – Zuckerman sent a short note to *Nature* assuring readers that he and Ashton had subsequently combined the dimensional measurements and indices for a tooth by tooth comparison (rather than dimension by dimension, or index by index) of the fossil teeth with those of living apes, and with those of Australian Aboriginals and Ancient Egyptians. The results, he wrote, showed "that almost every one of the fossil teeth can be matched in both dimensions and shape by corresponding teeth of the great apes." Moreover, the comparison of the fossil teeth with those of the Australians and Egyptians showed that "the African fossils resemble those of the two human types far less than they do those of the existing apes." He promised that the quantitative data underlying these statements would be available in the full study, which would be appearing shortly.[177]

Zuckerman's note roused Clark to respond in *Nature* in advance of the full study's publication. The battle lines were drawn from the title of the note onwards, for where Zuckerman's had been headed "South African Fossil Anthropoids," Clark's was titled "South African Fossil Hominoids." According to Clark, it was "open to serious question whether the major dimensions and indices of individual teeth can by

[176] Kaplan, "Fourth Summer Seminar," p.28.
[177] Zuckerman, "South African Fossil Anthropoids," p.652

themselves provide adequate information on which to base statements regarding the affinities of primitive hominoids and anthropoid apes." One ought to "take a comprehensive view of the total morphological pattern presented by the dentition," rather than "rely for the assessment of affinities on the dimensions of individual teeth treated as abstractions." The Australopithecine dentition, Clark stressed, consistently showed a combination of traits that were remarkably hominid in character,[178] whereas such a combination had never been shown to exist in the dentition of a living or extinct ape. He challenged critics to produce even a single specimen that spoke to the contrary, in the absence of which "it does not seem possible to seriously controvert the conclusions expressed by a number of competent palaeontologists that, in their dentition, the fossil Australopithecinae show a closer structural resemblance to primitive hominids than do any of the known anthropoid apes." The implication, intended or not, was that Zuckerman's statements had opened his own scientific competence to question.[179]

There was time for one more reprise before the actual publication of Zuckerman and Ashton's study, and the former took the opportunity to indicate, again via a note to *Nature*, that Clark had understood neither the aim of his communication nor the importance of biometric and statistical approaches to the comparative study of Primates. First, Zuckerman wrote, he had never claimed, as Clark has said he had,

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[178] The traits that Clark listed were "the small size (relative and absolute) and the spatulate form of the canine combined with its flat wear… and it relative position in the tooth row, the small incisors, the non-sectorial bicuspid form of *Pm*3…, the evenly curved parabolic contour of the dental arcade with a *consistent* absence of any diastema, the flat type of wear on the premolars and molars in the earlier stages of attrition, and, in the temporary dentition, the shape and dimensions (relative and absolute) of the milk canine combined with the details of the cusp pattern of *dm*1."

[179] Clark, "South African Fossil Hominoids [A]," pp.893-94.

that "the major dimensions and indices of individual teeth can by themselves provide adequate information on which to base statements regarding the affinities of primitive hominoids and anthropoid apes." Rather, his object has been "to show that adequate comparison by proper statistical procedures fail to substantiate a commonly stated view that the teeth of the South African Australopithecines apes differ significantly in size and general shape from those of existing apes," a conclusion that he insisted had not been controverted. Zuckerman criticized Clark's "total morphological pattern" as consisting of "an arbitrary and variable number of different items," and insisted that it was "no less an abstraction than the dimensions of individual teeth." Zuckerman repeatedly lambasted Clark's reliance on "qualitative" features: most of those statements which Clark might have expressed quantitatively were simply wrong; others were not subject to quantitative statement, and thus impossible to assess with any accuracy. The main value of Clark's communication might lie in these latter statements, according to Zuckerman, for the confusion they engendered served to underline the essential need for biometric and statistical approaches to resolve such matters with clarity and exactitude.[180]

In August 1950, the promised study finally appeared in the *Philosophical Transactions of the Royal Society of London*, consisting of two separate papers – the first dealing with the quantitative dental characters of the living great apes and the second giving a comparison of those with Australopithecines dentition. Explaining the motivation behind their work, the authors noted the lack of available information

[180] Zuckerman, "South African Fossil Hominoids [A]," pp.158-59.

about the statistical variation of dental characters among living apes. They lamented the fact that those workers who wished to make comparisons of the dental characters of fossil apes with those of living apes had not undertaken the necessary preliminary biometric and statistical work:

> The usual procedure in such cases as, for example, in all recent statements about the south and east African fossil anthropoids, has been for the comparison to be made with a small number of skulls of chimpanzee or gorilla closest to hand. Such comparisons do not involve an adequate estimate, if indeed any at all, of the variance of the dimensions of the teeth of either the extant or the fossil apes. In general, the result is that the conclusions to which they point may have little scientific validity.[181]

The aim of their study was thus to bring the discussion of the Australopithecine dentition into the properly defined boundaries of science.

The first paper contained six pages of densely packed tables showing, for each measurement or index, the mean, the number of animals used, the standard deviation from the mean, and the standard error of the mean. [182] In the second paper, they compared their data for the living apes with published data on the Australopithecines.[183] For each comparison, they calculated a value *P* to represent the significance of the difference observed. The authors chose to consider significant those differences which gave values of *P* less than or equal to 0.02, which, they explained, "meant that there is less than one chance in fifty that difference observed would have been due to chance, or alternatively, that at most one in fifty modern apes

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[181] E.H. Ashton and Solly Zuckerman, "Some Quantitative Dental Characteristics of the Chimpanzee, Gorilla, and Orang-Outang," pp.472.
[182] Ibid, pp.471-84.
[183] E.H. Ashton and Solly Zuckerman, "Some Quantitative Dental Characteristics of Fossil Anthropoids," pp.485-520.

would deviate as much from the mean of its own species as a fossil specimen which showed such a difference."[184] By the standards set out, in order for the claims by other workers that the Australopithecine teeth differed from those of the living apes to be substantiated, the fossil teeth would have to differ from those of *all* of the living apes in their dimensions and indices by a value of *P* less than or equal to 0.02. However, as Zuckerman had indicated in previews of the study, they found that, within the standard of significance laid out in their statistical methodology, "hardly one of the [fossil] teeth considered in this paper cannot be matched in dimensions and shape by the corresponding tooth of at least one type of modern ape."[185] Anyone who wished to take issue with this conclusion now had also to deal with the reams of numbers that underpinned it (Fig. 4.3).

One way to do that was to claim that all those numbers were simply inappropriate to the problem they purported to address, and this was the angle that Clark took. Indeed, as he expressed to Kenneth Oakley of the British Museum of Natural History, he considered Ashton and Zuckerman's work "open to such serious criticism that I wonder how it passed the referees, & I feel rather disturbed." He had consulted some statistical experts at Oxford, who had assured him that "Zuckerman's case is really rather fantastic." He felt that he and others had been misquoted "in a way which seems to me rather dishonest," which compelled him to spend a holiday remeasuring chimpanzee teeth and composing notes to several journals in defense of his position. "I don't like these controversies," he assured Oakley, "partly because

[184] Ibid., p.486.
[185] Ibid., p.517.

they take up a lot of time, but I feel I must do something." Evidently Clark felt that science would proceed far more efficiently if everyone would simply take his word as authoritative.[186]

Clark had two items published in *Nature* shortly thereafter. One was primarily an announcement and summary of the latest publications from Broom and his colleagues, which contained descriptions of the huge amount of *Plesianthropus* material discovered at Sterkfontein subsequent to the publication of the 1946 monograph. However, Clark used the occasion to launch a broad attack on the relevance on Ashton and Zuckerman's work. He claimed that the huge amount of material now available

> [eliminates] any further need for relying for comparative or statistical study on single, isolated specimens which, it might be argued, are of an exceptional nature. Attempts have already been made (somewhat prematurely, as it seems to me) to subject to statistical analysis some of the limited and very fragmentary Australopithecine fossils discovered several years ago, and certain inconclusive results (based on measurement taken at second hand or on plaster casts) have unfortunately led to controversies which the much more abundant evidence since become available now show to have been unnecessary. Thus, well over two hundred teeth of the Australopithecinae have now been recovered, providing a much more reliable series for the study of variability than the first few specimens which have been used for this purpose.[187]

This barrage may have seemed all the more impertinent to Zuckerman's eyes for not having identified its target by name. However, Clark's second item in the same issue named its target straight away. This time, rather than a general

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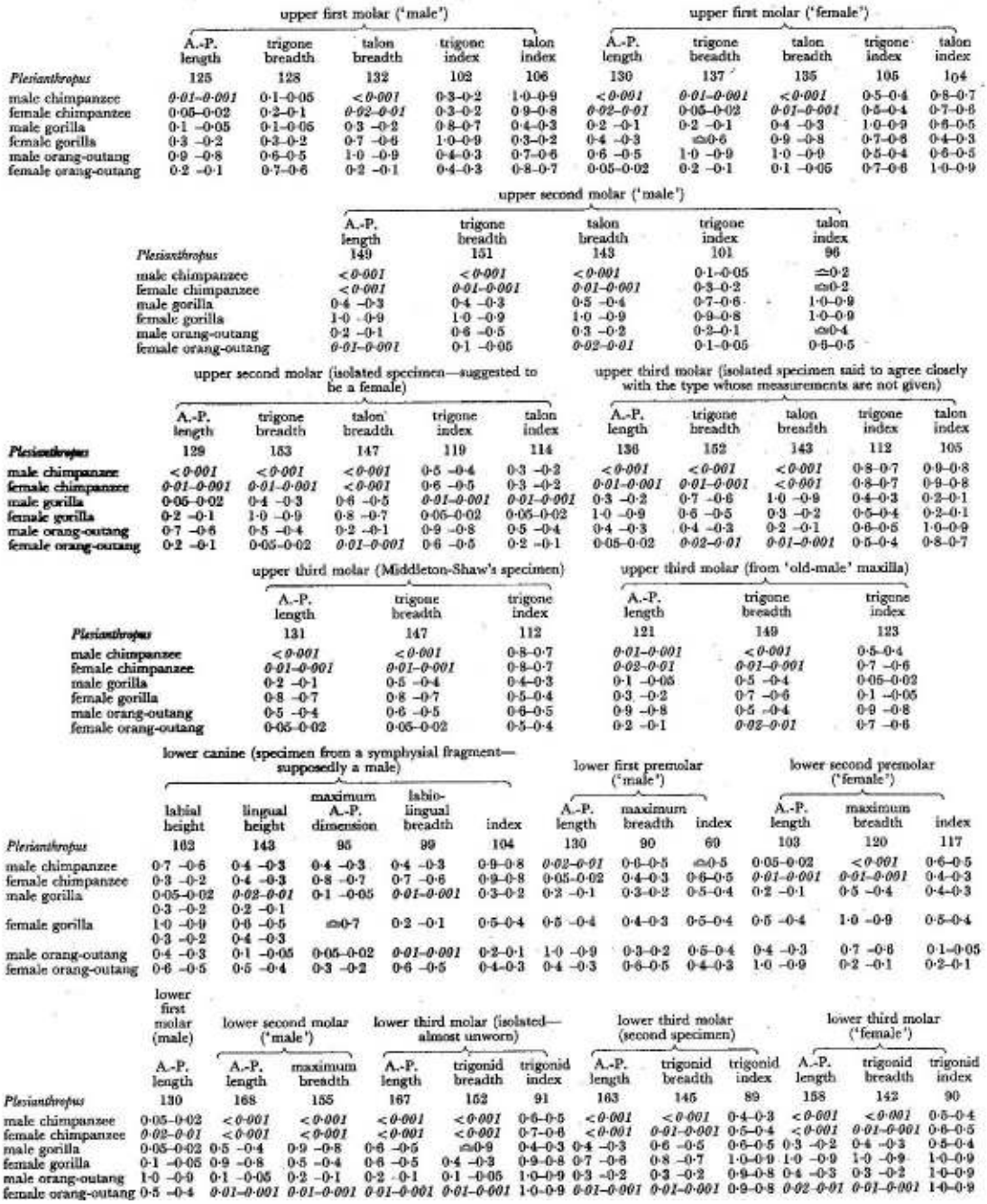
[186] Clark to Oakley, 20 Sep, 1950, DF 140/6, British Museum of Natural History Archives, London (hereafter BMNH)..
[187] Clark, "New Discoveries of the Australopithecinae," p.759.



upper first molar ('male')

	A.-P. length	trigone breadth	talon breadth	trigone index	talon index
Plesianthropus	125	128	132	102	106
male chimpanzee	*0·01–0·001*	0·1–0·05	*< 0·001*	0·3–0·2	1·0–0·9
female chimpanzee	0·05–0·02	0·2–0·1	*0·02–0·01*	0·3–0·2	0·9–0·8
male gorilla	0·1 –0·05	0·1–0·05	0·3 –0·2	0·8–0·7	0·4–0·3
female gorilla	0·3 –0·2	0·2–0·2	0·7 –0·6	1·0–0·9	0·3–0·2
male orang-outang	0·9 –0·8	0·6–0·5	1·0 –0·9	0·4–0·3	0·7–0·6
female orang-outang	0·2 –0·1	0·7–0·6	0·2 –0·1	0·4–0·3	0·8–0·7

upper first molar ('female')

	A.-P. length	trigone breadth	talon breadth	trigone index	talon index
Plesianthropus	130	137	135	105	104
male chimpanzee	*< 0·001*	*0·01–0·001*	*< 0·001*	0·5–0·4	0·8–0·7
female chimpanzee	*0·02–0·01*	0·05–0·02	*0·01–0·001*	0·5–0·4	0·7–0·6
male gorilla	0·2 –0·1	0·2 –0·1	0·4 –0·3	1·0–0·9	0·6–0·5
female gorilla	0·4 –0·3	≏0·6	0·9 –0·8	0·7–0·6	0·4–0·3
male orang-outang	0·6 –0·5	1·0 –0·9	1·0 –0·9	0·5–0·4	0·6–0·5
female orang-outang	0·05–0·02	0·2 –0·1	0·1 –0·05	0·7–0·6	1·0–0·9

upper second molar ('male')

	A.-P. length	trigone breadth	talon breadth	trigone index	talon index
Plesianthropus	149	151	143	101	96
male chimpanzee	*< 0·001*	*< 0·001*	*< 0·001*	0·1–0·05	≏0·2
female chimpanzee	*< 0·001*	*0·01–0·001*	*0·01–0·001*	0·3–0·2	≏0·2
male gorilla	0·4 –0·3	0·4 –0·3	0·5 –0·4	0·7–0·6	1·0–0·9
female gorilla	1·0 –0·9	1·0 –0·9	1·0 –0·9	0·9–0·8	1·0–0·9
male orang-outang	0·2 –0·1	0·6 –0·5	0·3 –0·2	0·2–0·1	≏0·4
female orang-outang	*0·01–0·001*	0·1 –0·05	*0·02–0·01*	0·1–0·05	0·6–0·5

upper second molar (isolated specimen—suggested to be a female)

	A.-P. length	trigone breadth	talon breadth	trigone index	talon index
Plesianthropus	129	153	147	119	114
male chimpanzee	*< 0·001*	*< 0·001*	*< 0·001*	0·5 –0·4	0·3 –0·2
female chimpanzee	*0·01–0·001*	*0·01–0·001*	*< 0·001*	0·6 –0·5	0·3 –0·2
male gorilla	0·05–0·02	0·4 –0·3	0·6 –0·5	*0·01–0·001*	*0·01–0·001*
female gorilla	0·2 –0·1	1·0 –0·9	0·8 –0·7	0·05–0·02	0·05–0·02
male orang-outang	0·7 –0·6	0·5 –0·4	0·2 –0·1	0·9 –0·8	0·5 –0·4
female orang-outang	0·2 –0·1	0·05–0·02	*0·01–0·001*	0·6 –0·5	0·2 –0·1

upper third molar (isolated specimen said to agree closely with the type whose measurements are not given)

	A.-P. length	trigone breadth	talon breadth	trigone index	talon index
Plesianthropus	136	152	143	112	105
male chimpanzee	*< 0·001*	*< 0·001*	*< 0·001*	0·8–0·7	0·9–0·8
female chimpanzee	*0·01–0·001*	*0·01–0·001*	*< 0·001*	0·8–0·7	0·9–0·8
male gorilla	0·3 –0·2	0·7 –0·6	1·0 –0·9	0·4–0·3	0·2–0·1
female gorilla	1·0 –0·9	0·6 –0·5	0·3 –0·2	0·5–0·4	0·2–0·1
male orang-outang	0·4 –0·3	0·4 –0·3	0·2 –0·1	0·6–0·5	1·0–0·9
female orang-outang	0·05–0·02	*0·02–0·01*	*0·01–0·001*	0·5–0·4	0·8–0·7

upper third molar (Middleton-Shaw's specimen)

	A.-P. length	trigone breadth	trigone index
Plesianthropus	131	147	112
male chimpanzee	*< 0·001*	*< 0·001*	0·8–0·7
female chimpanzee	*0·01–0·001*	*0·01–0·001*	0·8–0·7
male gorilla	0·2 –0·1	0·5 –0·4	0·4–0·3
female gorilla	0·8 –0·7	0·8 –0·7	0·5–0·4
male orang-outang	0·5 –0·4	0·6 –0·5	0·6–0·5
female orang-outang	0·05–0·02	0·05–0·02	0·5–0·4

upper third molar (from 'old-male' maxilla)

	A.-P. length	trigone breadth	trigone index
Plesianthropus	121	149	123
male chimpanzee	*0·01–0·001*	*< 0·001*	0·5–0·4
female chimpanzee	*0·02–0·01*	*0·01–0·001*	0·7 –0·6
male gorilla	0·1 –0·05	0·5 –0·4	0·05–0·02
female gorilla	0·3 –0·2	0·7 –0·6	0·1 –0·05
male orang-outang	0·9 –0·8	0·5 –0·4	0·9 –0·8
female orang-outang	0·2 –0·1	*0·02–0·01*	0·7 –0·6

lower canine (specimen from a symphysial fragment—supposedly a male)

	labial height	lingual height	maximum A.-P. dimension	labio-lingual breadth	index
Plesianthropus	102	143	95	99	104
male chimpanzee	0·7 –0·6	0·4 –0·3	0·4 –0·3	0·4 –0·3	0·9–0·8
female chimpanzee	0·3 –0·2	0·4 –0·3	0·8 –0·7	0·7 –0·6	0·9–0·8
male gorilla	0·05–0·02 0·3 –0·2	*0·02–0·01* 0·2 –0·1	0·1 –0·05	*0·01–0·001*	0·3–0·2
female gorilla	1·0 –0·9 0·3 –0·2	0·6 –0·5 0·4 –0·3	≏0·7	0·2 –0·1	0·5–0·4
male orang-outang	0·4 –0·3	0·1 –0·05	0·05–0·02	*0·01–0·001*	0·2–0·1
female orang-outang	0·6 –0·5	0·5 –0·4	0·3 –0·2	0·6 –0·5	0·4–0·3

lower first premolar ('male')

	A.-P. length	maximum breadth	index
Plesianthropus	130	90	69
male chimpanzee	*0·02–0·01*	0·6–0·5	≏0·5
female chimpanzee	0·05–0·02	0·4–0·3	0·6–0·5
male gorilla	0·2 –0·1	0·3–0·2	0·5–0·4
female gorilla	0·5 –0·4	0·4–0·3	0·5–0·4
male orang-outang	0·2–0·1	0·3–0·2	0·5–0·4
female orang-outang	0·2 –0·1	0·6–0·5	0·4–0·3

lower second premolar ('female')

	A.-P. length	maximum breadth	index
Plesianthropus	103	120	117
male chimpanzee	0·05–0·02	*< 0·001*	0·6–0·5
female chimpanzee	*0·01–0·001*	*0·01–0·001*	0·4–0·3
male gorilla	0·2 –0·1	0·5 –0·4	0·4–0·3
female gorilla	0·5 –0·4	1·0 –0·9	0·5–0·4
male orang-outang	0·4 –0·3	0·7 –0·6	0·1–0·05
female orang-outang	0·2 –0·1	0·2–0·1	

lower first molar (male)

	A.-P. length
Plesianthropus	130
male chimpanzee	0·05–0·02
female chimpanzee	*0·02–0·01*
male gorilla	0·05–0·02
female gorilla	0·1 –0·05
male orang-outang	1·0 –0·9
female orang-outang	0·5 –0·4

lower second molar ('male')

	A.-P. length	maximum breadth
Plesianthropus	168	155
male chimpanzee	*< 0·001*	*< 0·001*
female chimpanzee	*< 0·001*	*< 0·001*
male gorilla	0·5 –0·4	0·9 –0·8
female gorilla	0·9 –0·8	0·5 –0·4
male orang-outang	0·1 –0·05	0·2 –0·1
female orang-outang	*0·01–0·001*	*0·01–0·001*

lower third molar (isolated—almost unworn)

	A.-P. length	trigonid breadth	trigonid index
Plesianthropus	167	152	91
male chimpanzee	*< 0·001*	*< 0·001*	0·6–0·5
female chimpanzee	*< 0·001*	*< 0·001*	0·7–0·6
male gorilla	0·6 –0·5	≏0·9	0·4–0·3
female gorilla	0·6 –0·5	0·4 –0·3	0·9–0·8
male orang-outang	0·2 –0·1	0·1 –0·05	1·0–0·9
female orang-outang	*0·01–0·001*	*0·01–0·001*	1·0–0·9

lower third molar (second specimen)

	A.-P. length	trigonid breadth	trigonid index
Plesianthropus	163	145	89
male chimpanzee	*< 0·001*	*< 0·001*	0·4–0·3
female chimpanzee	*< 0·001*	*0·01–0·001*	0·5–0·4
male gorilla	0·4 –0·3	0·6 –0·5	0·6–0·5
female gorilla	0·7 –0·6	0·8 –0·7	1·0–0·9
male orang-outang	0·3 –0·2	0·3 –0·2	0·9–0·8
female orang-outang	*0·01–0·001*	*0·01–0·001*	0·9–0·8

lower third molar ('female')

	A.-P. length	trigonid breadth	trigonid index
Plesianthropus	158	142	90
male chimpanzee	*< 0·001*	*< 0·001*	0·5–0·4
female chimpanzee	*< 0·001*	*0·01–0·001*	0·6–0·5
male gorilla	0·3 –0·2	0·4 –0·3	0·5–0·4
female gorilla	1·0 –0·9	1·0 –0·9	1·0–0·9
male orang-outang	0·4 –0·3	0·3 –0·2	1·0–0·9
female orang-outang	*0·02–0·01*	*0·01–0·001*	1·0–0·9

Figure 4.3 – A sample page from the second part of Ashton and Zuckerman's statistical study, showing the *P* values for comparisons between the teeth of *Plesianthropus* and those of living apes (Figures in italics show instances in which the comparison showed no significant difference, when $P \leq 0.02$)[188]

[188] Ashton and Zuckerman, "Some Quantitative Dental Characters of Fossil Anthropoids," p.495.

critique of the relevance of Ashton & Zuckerman's work, Clark took aim at a particular claim about two pieces of fossil evidence. Zuckerman had claimed that the lower milk canines of the Taung skull (Dart's original find) and another juvenile australopithecine from Kromdraii did not differ in dimensions or shape from those of juvenile chimpanzees. Clark attacked the methodological adequacy of relying on comparisons of a small number of arbitrary measurements considered individually, and further noted that while Zuckerman appeared to have used only fifteen chimpanzee specimens for his comparison, Clark had undertaken the task with fifty (the proceeds of his work over the holiday). Attempting to show his own biometrical mettle, Clark argued that in contrast to Zuckerman's arbitrarily selected dimensional measurements, "measurements… carefully selected for their relevancy in testing the obvious differences in shape observed on direct visual comparison…can, if necessary be expressed metrically." He gave a number of examples, promising to publish more extensively elsewhere, but it was obvious that he thought such data superfluous beside the qualitative conclusions of a well-trained anatomical eye. To hammer home this point, he included a sketch of the lower milk canine and first premolar of a *Homo sapiens*, the Kromdraii australopithecine, and ten chimpanzees selected at random from his sample of fifty (Fig. 4.4). Where the chimpanzee canines were long, triangular and sharply pointed, those of the Homo sapiens and australopithecine were short and pentagonal, with rather obtusely pointed tips. The premolars of the young chimps all showed a single cusp rising to a peak above the rest of the crown, whereas those of the Homo sapiens and australopithecine had undulating crowns with no one

dominant cusp. To Clark, the conclusion, drawn from examples which "could be multiplied if this were really considered necessary," should have been obvious: "the Australopithecine milk canine is certainly much more hominid in its shape and proportions than any chimpanzee in our series of fifty skulls."[189]

Clark's claim to speak more authoritatively on the comparative anatomy of the South African fossils and the living apes was largely based on a vision of anatomical expertise that privileged the practiced observer of qualitative patterns over the statistical analyst. He repeatedly referred to the fact that Zuckerman had been relying on second hand data and casts, whereas he had been to South Africa to examine the original specimens. In his note demonstrating the hominoid qualities of the Australopithecine milk canines, he thanked several South African workers for rechecking his data on the fossil teeth, thus reminding readers that his scientific

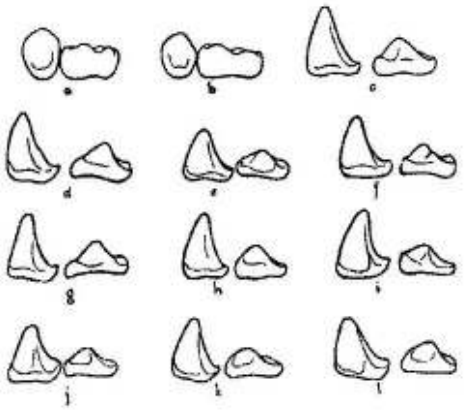


Figure 4.4 – Clark's comparison of two Australopithecine milk canines and first premolars (a and b) with those of ten chimpanzees[190]

[189] Ibid., pp.791-92.
[190] Clark, "South African Fossil Hominoids [B]," p.792.

relationships allowed him access to the original specimens even at a geographical remove.[191] As we will see, Clark would continue to use his special access as a European worker with the African fossils and scientists to his own advantage, and to question the validity of Zuckerman's continued criticism.

Zuckerman's response to the latest criticisms aimed his way reiterated his position – with a palpable sense of exasperation – that Clark had failed to understand the nature of his statistical studies. Zuckerman was happy to grant Clark that the dimensions used in his statistical analysis did not exhaust the possible observations one could make of Primate teeth, and that it was perfectly possible that the sort of qualitative comparisons that Clark had made would reveal similarities between the dentition of the Australopithecines and hominids. However, the aim of his work was only to test the validity of published statements by Clark, Broom and Dart about the difference in metrical attributes between the Australopithecine dentition and that of the living apes. Through the use of statistics, he had found such statements wanting in justification, whatever broader significance one wished to attribute to this fact. "It can scarcely be premature," he wrote in response to one of Clark's charges, "to examine the accuracy of scientific statements."[192]

While this had indeed been his stated intention in the studies he had published so far, Zuckerman also wanted to go beyond the published data and study the African fossil material himself. After all, Clark had criticized his reliance on data relating to comparatively few the pre-1947 remains, suggesting those who had access to the large

[191] Ibid., p.792.
[192] Zuckerman, "South African Fossil Hominoids [B]," p.953.

amount of material subsequently found (such as Clark himself) were in a better position to make satisfactory judgments. In order to better acquaint himself with some of this material, Zuckerman wrote to Clark in early 1951 (despite the increasingly acrimonious tone of their exchanges) asking if he could take measurements of some of the teeth of the East African genus *Proconsul* that Clark had been studying in conjunction with Louis Leakey.[193] This was one of several Miocene Primates from the environs of Lake Victoria which Clark believed to be early representative of the superfamily family *Hominoidea*, which later included the Australopithecines and human beings. The material that Clark had to hand, however, had not yet been sorted out and analyzed for publication, and he therefore resisted giving Zuckerman priority in examining it. "If you don't mind," wrote Clark with annoyance, "may we follow the usual procedure in the matter of fossils of this sort? That is to say, the material would be available for study by others when those who are concerned with examining and reporting on them in the first instance have completed their studies?"[194]

Writing to Oakley, Clark described himself as "astonished" that Zuckerman could be so "importunate" as to ignore the norms of scientific priority so blatantly. "I feel the situation is really getting rather unpleasant," he wrote, "for it seems to me that Zuckerman is trying by every possible means (in some of his publication, I am sorry to say, by misrepresenting my views) to discredit me and others." He felt that it would best to "keep aloof from further controversy," but he feared that Zuckerman might use the current situation to "spread the story that we are deliberately withholding the

[193] Zuckerman to Clark, 6 Jan 1951 and 13 Jan 1951, SZ/GEN/CLARK, SZP.
[194] Clark to Zuckerman 17 Jan 1951, SZ/GEN/CLARK, SZP.

fossils from him." He confessed that the controversy caused him to feel rather depressed, and asked Oakley if he could persuade his superior at the British Museum (Natural history), the influential evolutionary biologist Gavin De Beer to "use his influence in restraining Zuckerman from going to far."[195]

Clark's stated intention to "keep aloof" from the controversy did not stop him recruiting others to argue his side for him. While he argued that statistical analysis was an unnecessary and possibly even misleading approach, he nevertheless approached his friend the mathematician Jacob Bronowski for help in assessing the merits and flaws of Ashton and Zuckerman's analysis.[196] Bronowski's assistant W.M. Long was trained in statistical analysis, and together they produced a critique of Ashton and Zuckerman's work, published in late 1951. The fact that Ashton and Zuckerman's results differed from those based on classical morphological comparison was, the authors argued, the result of faulty method. Since a tooth or a bone was a unit, it could not be treated as "a discreet assembly of independent measurements." Ashton and Zuckerman failed to take into account that each of the measurements they used were correlated with the others and thus could not be treated independently. What was required was a multivariate analysis that took account of such correlation. Bronowksi and Long demonstrated the technique by using a multivariate analysis of forty human and forty four chimpanzee milk canines to construct a discriminant

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[195] Clark to Oakley, 5 Feb 1951, DF140/6, BMNHA.

[196] This was before Bronowski, who was then employed as the Director of Research at the National Coal Board in the United Kingdom, had become famous as public intellectual through his appearances on television. He maintained a strong interest in human evolution, culminating in his thirteen part television series examining humanity's biological and cultural evolution, *The Ascent of Man*, in 1973. That work appeared in book form the following year.

function, which they used to show that two Australopithecine milk canines from Taung and Kromdraii fell within the human range rather than the chimpanzee. The upshot was that Ashton and Zuckerman would have to construct more complex discriminant functions that took into account the multivariate analysis of teeth from all the living ape species if they wanted to properly accomplish their stated goal of applying modern statistical methods to anthropological problems.[197]

Zuckerman then recruited his own outside expert: the head of statistics at the Rothamsted Experimental Station, Frank Yates. The benefits of this intercession turned out to be mixed. On the positive side, Yates and his colleague J.R. Healy did question Bronowski and Long's assertion that multivariate analysis was necessarily superior to the comparison of individual measurements in resolving questions such as those at hand. Regardless, they had been surprised that Bronowksi and Long's discriminant function should yield such different results from Ashton and Zuckerman's comparisons of individual measurements, since mathematically there seemed little reason for the two procedures to differ so radically in their outcomes. Zuckerman had given them his data on the milk canines, and the discriminant functions they constructed largely confirmed the findings by Bronowski and Long. Further, and even more damning, their replication of Ashton and Zuckerman's procedure of comparing of individual measurements did not seem to bear out the latter's published results.[198]

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[197] Bronowski and Long, "Statistical Methods in Anthropology," p.794; Another paper dealing with the use of statistics in anthropology appeared later as Bronowski and Long, "Statistics of Discrimination in Anthropology," p.385.

[198] Yates and Healy, "Statistical Methods in Anthropology," pp.1116-17.

While Zuckerman had recruited Yates and Healy as collaborators, their reanalysis of the data revealed an error in Ashton and Zuckerman's statistical analysis. Ashton and Zuckerman admitted in a paper published simultaneously with that of Yates and Healy that the difference between the two sets of results from the same data "was due to a misunderstanding [on their part] in the interpretation of the analysis of variance which was carried out to separate the component of variance that is due to differences between corresponding teeth in the same jaw and that due to differences between animals." It turned out that their previously published values for the standard deviations of the individual measurements were thus too large by a factor of $\sqrt{2}$. Correcting for this error resulted in an increased number of significant differences between the South African fossil teeth and those of living apes. As a result, whereas they had previously claimed to be able to quantitatively corroborate only twenty percent of previously published statements claiming a significant difference between the overall dimensions of the Australopithecine teeth and those of living apes, their revised figures corroborated fifty percent.[199]

Despite the embarrassing revelation, Ashton and Zuckerman refused to see their error as fatal to the credibility of their method. When they published their revised figures, they insisted that their method was still valuable as a test for the many statements being made about the relative size of Australopithecine and living ape teeth. Even though they had to admit the number of significant difference in dental

[199] Ashton and Zuckerman, "Statistical Methods in Anthropology," pp.1117-18.

anatomy was greater than they had originally found, their corrected results still showed a lack of accuracy in significant number of those statements.[200]

At any rate, they had already moved beyond dental morphology in their campaign. Taking aim this time at a study published by Clark which compared a *Plesianthropus* skull to three species of great ape in terms of three separate cranial indices. These indices were the "nuchal height" index, which gave a ratio between the height to which the neck muscles extend of the back of the skull to the total height of the skull; the "supra-orbital height" index, which gave the ratio of the height of the brain case above the eye-sockets to the total height of the skull; and the "condylar position" index, which was meant to show the position of the occipital condyle on a straight line representing the maximum horizontal extension of the skull. For the nuchal height index, they confirmed Clark's finding that the *Plesianthropus* skull fell outside the ape range and within that of humans. In the case for the supra-orbital height index, *Plesianthropus* fell within the ranges demonstrated by both humans and gorillas, and outside that of the rest of the primates measured. In terms of its condylar position, *Plesianthropus* fell near the edge of the range displayed by gorillas, but otherwise in between that displayed by apes and monkeys on the one hand and humans on the other. This last fact led the authors to surmise that "[if] *Plesianthropus* did in fact walk upright, the only conclusion one can draw from our figures is that its skull was not balanced like the human skull."[201]

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[200] Ashton and Zuckerman, "Overall Dental Dimensions of Hominoids," pp.571-72.
[201] Ashton and Zuckerman, "Some Cranial Indices of Plesianthropus and Other Primates," p.293. Clark's observations to which they were referring came from Clark, ""New Paleontological Evidence Bearing on the Evolution of the Hominoidea."

Throwing his pledge to keep aloof from controversy to the wind, Clark responded forcefully. He had intended the condylar position index to serve as an indication of the balance of the skull on the vertebral column, and this was mainly a function of how much brain case sat in front of or behind the occipital condyles. Ashton and Zuckerman's results showing that *Plesianthropus* fell within the human gorilla range were misleading, as they had included in their measurements the length added to the gorilla skulls by their crests, which were immaterial to the balance of the skull. They were thus unjustified in stating that *Plesianthropus* must have carried its head in the manner of the apes, and a correction for this mistake would show that it would have been carried more vertically than in apes, though less so than in humans. Similarly, Ashton and Zuckerman had included the gorillas' crest in the measurement of the supra-orbital height index, which resulted in *Plesianthropus* falling within the range of both humans and gorillas. Clark objected that his original purpose in using this index had been to demonstrate the height of the brain-case above the eyes, and thus the inclusion of crests did not change his findings. Ashton and Zuckerman had fallen prey to "the fallacy of comparing measurements which are not in the strict sense morphologically comparable. For it is clear that, however elaborate the statistical methods employed and however careful the computations, morphological conclusions must be at once invalidated if they are found to be based on dimensions which have no exact morphological equivalence." The apparent controversy was in this sense an

illusion, argued Clark, for if the differences in the dimensions measured were taken account of, everyone's results were more of less in accord.[202]

Undeterred, Zuckerman continued with his procession of studies of different aspects of the Australopithecine anatomy as they compared with apes and humans. Again with Ashton, he expanded the study of the condylar index by taking into account the changes that occurred as chimpanzees and gorillas aged. Their results showed that *Plesianthropus* fell within the ranges displayed by both species of apes in a number of age groups and well away from humans. Thus, they argued contra Clark, the similarity was not just due to the large crests on old male gorillas, and they were perfectly justified in stating that *Plesianthropus* must have carried its head more like and ape than a human.[203] In study of the of the order of eruption of the permanent dentition in apes, humans, and those Australopithecine specimens for which data of this sort were available (conducted with E.M.B Clements), he found a high variability among humans, and no consistent pattern among the Australopithecine skulls. He thus counseled that little evolutionary significance could be drawn on this score from the presently known fossils.[204] In a study of the articular fossa in apes and humans, Ashton and Zuckerman argued that, contrary to previously published opinion, this

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[202] Clark, "A Note on Certain Cranial Indices of Sterkfontein Skull No.5," pp.119-120.
[203] Ashton and Zuckerman, "Age Changes in the Position of the Occipital Condyles in the Chimpanzee and Gorilla," pp.277-288.
[204] Clements and Zuckerman, "The Order of Eruption of the Permanent Teeth in the Hominoidea," pp.313-337.

structure was not materially different in the Australopithecines than in the living ape.[205]

Around the same time, Zuckerman was given an opportunity to publish an essay that synthesized many of his specific technical comparisons into a broader criticism of a supposed human-Australopithecine affinity. It appeared in a volume of essays edited by Julian Huxley, the famous evolutionary scientist who had coined the phrase "Modern Synthesis" to describe the integration of genetics and evolutionary theory that had been taking place over the last two decades. The list of contributors included several other luminaries associated with this process, including Ronald Fisher, J.B.S Haldane, and Ernst Mayr, putting Zuckerman in very prestigious company. Zuckerman charged that those responsible for the discovery of the Australopithecine fossils were possessed by a preconceived notion that those creatures bore an ancestral relationship to humans, and that their anatomical descriptions were consistently biased to demonstrate this view. His own anatomical studies, free from any phylogenetic prejudices, had not shown any evidence that the Australopithecines departed anatomically from the apes in any appreciable manner. He dwelt on the issue of posture, important as it was for inferences regarding the ancestry of human bipedalism, reiterating his view that there was nothing in the cranial anatomy to show that the creatures must have walked upright. He added to this by casting doubt on the idea, felt to be so incontrovertible by those he was criticizing, that the innominate

[205] Ashton and Zuckerman, "The Anatomy of the Articular Fossa (Fossa Mandibularis) in Apes and Man," pp.29-61.

bones of the Australopithecines proved that they had human-like hips and could thus be said to have been bipedal.[206]

Clark was predictably annoyed. He wrote to John Robinson in South Africa to complain about it, expressing a fear that Zuckerman's essay could "have a bad effect on zoologists who have no personal acquaintance with the evidence," since it was "cleverly written." He felt something ought to be done to point out the more grievous "fallacies" in the essay, and suggested that Robinson might write something since he had recently uncovered some new evidence bearing on the posture of the Australopithecines which might be deployed against Zuckerman.[207] He admitted in a later letter to being "very angry" that Zuckerman's essay should have been published in such a collection, especially "with a 'boost' from Huxley in the Introduction."[208]

It had now been around three years since Broom's death, and since then Robinson had been carrying on the prolific pace of his former chief's schedule of excavation and publication. At Clark's suggestion, he agreed to send a note to *Nature* correcting some of the "fallacies" in Zuckerman's arguments.

Zuckerman had noted in his essay that two specimens of *Paranthropus crassidens* from the Swartkrans site possessed high sagittal crests, and that among living Primates, such structures were invariably associated with a pronounced occitipital , or nuchal, crest running perpendicular to the sagittal crest along the back of the skull. The most pronounced of such paired crests existed in the gorilla, where

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[206] Zuckerman, "Correlation of Change in the Evolution of Higher Primates."
[207] Clark to Robinson, Mar 22 1954, Box 305A, Transvaal Museum, Pretoria, South Africa (hereafter TM).
[208] Clark to Robinson, May 17 1954, Box 305A, TM.

the nuchal crest served as an anchor for the powerful musculature on the back of the neck. The occipital portion of the *Paranthropus* skulls in question had not been preserved, but Zuckerman inferred from their sagittal crests that they must have possessed gorilla-like nuchal crests, and that, hence, they would have carried their head hunched forward in the manner of the gorilla.[209] Robinson replied that direct evidence of the non-existence of nuchal crests in the Australopithecines did in fact exist. A skull uncovered by Dart several years previously, wrote Robinson, had a reasonably complete occiput and showed no evidence of the large nuchal crest that might have implied the existence of gorilla-like neck musculature. Further, two skulls from the Swartkrans site showed no sign of a nuchal crest despite heavily pronounced sagittal crests. Zuckerman had referred to one of these in his essay, claiming that the presence or not of the nuchal crests could not be determined form the partial preservation. Robinson now claimed that though the occiput of the skull in question was partial and "a bit distorted and displaced" but still sufficient to see that no nuchal crest was present. Further, he pointed out that Zuckerman had seen this very specimen in 1951 when Robinson had brought it to London on a visit, the implication being that Zuckerman was willfully ignoring the evidence in making his criticisms.[210]

Zuckerman reply contained insinuations of its own. His reading of Broom and Robinson's initial description of the *Paranthropus* skulls in a 1952 monograph seemed to indicate that the occiput was so crushed as to make the determination of its structure impossible. Why did Robinson now claim that it had retained enough

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[209] Zuckerman, "Correlation of Change of Higher Primates," pp.327-328.
[210] Robinson, "The Australopithecine Occiput," pp.262-63.

integrity to refute Zuckerman's criticism? He confided privately to Oakley (from whom he had requested permission to examine the casts of the skulls in question held by the British Museum of Natural History) that Robinson was engaged in what seemed to him "suspiciously like the actual distortion of facts." No such outright accusation appeared in print, but its implication was clear.[211]

Though Robinson's letter had contained insinuations of its own, he evidently felt that Zuckerman's reply had gone too far. As he told Clark, he reserved some of the blame for the journal itself: "I am… somewhat surprised that a journal of *Nature*'s standing printed so clearly insulting a note as that of Zuckerman…in it he deliberately seeks to disparage me personally instead of confining himself to facts."[212] He further complained that the Editor had required that he condense his original note, only to allow Zuckerman more space for his reply. Clark agreed that Robinson had been wronged in both cases, and even suggested that underlying political allegiances might be to blame: "I have gained the unhappy impression that Z. has some 'pull' with the Editor, for he is given so much space & license."[213] Whatever the biases of the Editor, he allowed Robinson and Zuckerman one more response each (Robinson's was longer this time) before this episode of the controversy ended without either side giving an inch.[214]

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[211] Zuckerman, "The Australopithecine Occiput," pp.263-64; Zuckerman to Oakley, 19 Jun 1954, SZ/GEN/Oakley.
[212] Robinson to Clark, 17 Sep 1954, Box 305A, TM.
[213] Clark to Robinson, 26 Sep 1954, Box 305A, TM. The editorship of *Nature* at this time was actually shared by A.J.V. Gale and L. J. F. Brimble.
[214] Robinson, "Nuchal Crests in Australopithecines," pp.1197-98; Zuckerman, "Nuchal Crests in Australopithecines," p.1198.

Clark now shifted strategies. Rather than engaging Zuckerman directly, as he had not been able to restrain himself from doing previously, he began to write as if the dispute had been resolved in his favor. This was in evidence in 1955 when he published *The Fossil Evidence for Human Evolution: An Introduction to the Study of Paleoanthropology* as part of the University of Chicago series "The Scientist's Library: Biology and Medicine." The purpose of this series, in the words of its editor, was "to provide authoritative information about the growth and status of various subjects in such a fashion that the individual books may be read with profit not only by the specialist but also by those whose interests lie in other fields."[215] Thus licensed as an authority before an audience largely comprised of scientists without intimate connection to paleoanthropology's recent discordance, Clark was free to cast the field as he saw fit. As it happened, he saw fit to refer directly to Ashton and Zuckerman's work only once, in a footnote, and that to say that the revelation of their error in calculating the standard deviations in their dental study rendered their conclusions "invalid."[216] Elsewhere, without mentioning Ashton and Zuckerman by name, Clark declared what controversy had existed over the affinities over the Australopithecine dentition close by Bronowski and Long's multivariate analysis, writing that "by applying it to a controversial issue which had arisen in regard to certain teeth of…*Australopithecus*…they were able to resolve the controversy by demonstrating very positively their hominid character."[217]

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[215] Clark, *The Fossil Evidence for Human Evolution*p.V. The quote is taken from the "Preface to the Series," written by its editor, Peter P.H. DeBruyn.
[216] Ibid., p.145.
[217] Ibid., p.29.

Despite the paucity of explicit references, readers familiar with the recent controversy would have been able to appreciate Zuckerman's presence in some of Clark's points of emphasis. This was particularly so in the section entitled "The Quantitative Assessment of Taxonomic Relationships." While granting that biometry could be of great benefit, Clark was most concerned to enumerate (at considerable length) the various "fallacies" to which student were liable to fall prey when relying on quantitative methods. Presumably, these warnings were meant to inoculate readers against infection if they should subsequently come into contact with such nefarious fallacies as "treating characters separately and independently, instead of in combination," or "inadequate or inaccurate statistical treatment." Clark mentioned no names, but warned that "cases have occurred in which such errors have led to rather serious misstatements and misunderstanding."[218]

One of the "fallacies" in particular went to the heart of the disagreement between Clark and Zuckerman (though, again, without mentioning names), pointing to a fundamental divergence in the two men's conception of proper methods and modes of reasoning in their discipline. On a number of occasions, most strongly in his contribution to the Huxley volume, Zuckerman had criticized much of the current writing of the Australopithecines for being overly caught up in certain hypotheses about the phylogenic position of those creature; namely, that they were more closely related, and possible ancestral, to humans rather than apes. This conception of the Australopithecine's evolutionary relationships had, according to Zuckerman,

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[218] Ibid., pp.28-29.

prejudiced other workers' assessment of the anatomical facts. His stated aim was to supply comparative metrical data on the South African fossils, the living apes, and human beings as a matter of fact, without advancing any hypotheses about the evolutionary relationships between them. From this perspective, all metrical data were of equal value for comparative studies; that is to say, any given measurement or index could be used to say that one set of specimens was "similar" or "different" to another. To Clark, this "fallacy of treating all metrical data as of equal taxonomic value" threatened not only to cloud the field with irrelevant data, but possible also to lead evolutionary science down the wrong path altogether. To avoid such a fate, Clark counseled that before embarking on biometric and statistical studies, the investigator should already have in mind "those features which are known to have taxonomic value for the problem at hand."[219] Clark granted that this raised the question of how taxonomic relevance was to be assessed before any measurements had been taken. His answer was that the investigator needed to be able to appreciate, through direct visual inspection, the similarities and differences in the total morphological patterns presented by the groups to be compared. He asked his readers to consider the underlying basis of taxonomy in modern biology: related groups should ideally reflect a shared evolutionary origin. That shared origin in the past would be reflected in the present in a certain morphological pattern which served as a diagnostic criterion for the taxonomic group. Certain elements of their respective morphological patterns might unite two groups in a common taxonomic family, while others might allow

[219] Ibid., p.28.

them to be differentiated into separate genera. Focusing narrowly on measurements of those features which differentiated the groups generically could be deeply misleading if not accompanied by an appreciation that those differentiating features existed within a wider community of shared descent. Indeed, no comparative measurement of a particular feature had any significance real significance at all without some pre-existing idea of the phylogenic relationship between the groups being compared. Studies like those conducted by Ashton and Zuckerman amounted to groping around in the dark, and thus taking the risk of blindly following false paths. What Zuckerman had criticized as putting the cart before the horse – that is, having biometric studies follow from phylogenic hypotheses – was, in Clark's estimation, a necessary part of reasoning in evolutionary science.[220]

For Clark, that reasoning led to the conclusion that the Australopithecines, by virtue of a morphological pattern that extended from their dental anatomy to the evidence of bipedal gait from the pelvic and limb bones, shared a community of descent with the genus *Homo* and its primitive Asian relative *Pithecanthropus*. As far as the problem of actual descent went, there still remained the difficulty that the geological evidence seemed to indicate that known Australopithecine remains came from creatures that had lived at the same time as *Pithecanthropus* in Asia, and so the latter could obviously not have descended from those particular creatures. Nonetheless, Clark reasoned that the hypothesis of a morphological series leading from *Australopithecus* through *Pithecanthropus* to *Homo* was strong on

[220] This paragraph is a condensed reading of Clark's section on "The Fallacy of Treating all Metrical Data as of Equal Taxonomic Value" on pp.25-28. Again, Clark refrained here from mentioning Zuckerman explicitly.

morphological grounds alone, and that the creatures that had left the remains discovered in South Africa were likely the descendants of similar creatures from an earlier period that could have themselves given rise to two divergent lines that continued to exist for a time alongside one another: one little-modified line that would

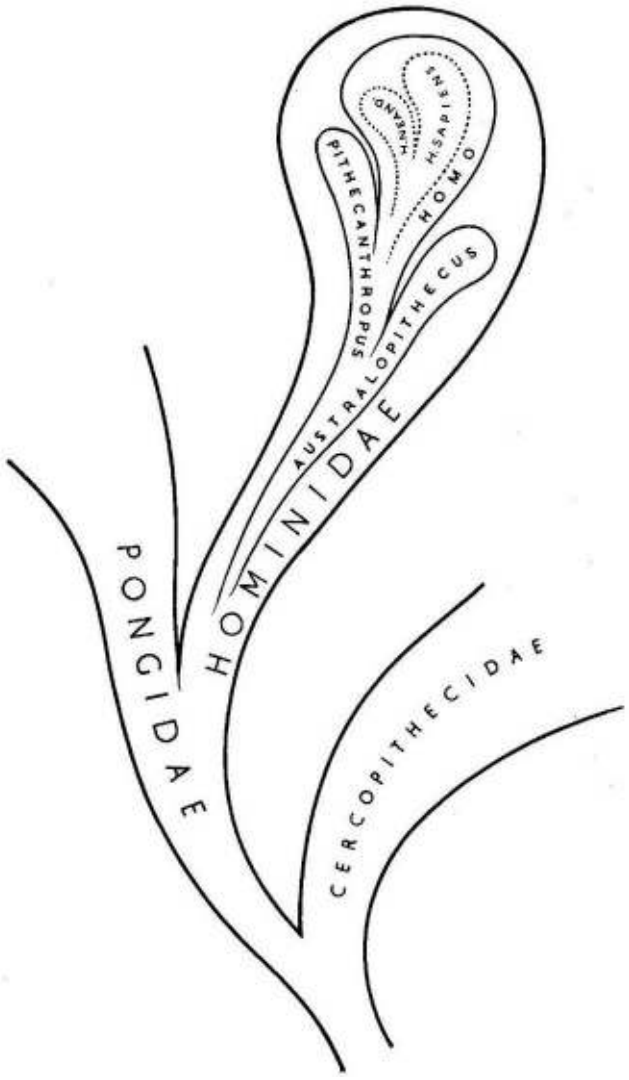

Figure 4.5 – Clark's 1955 phylogeny of the *Hominidae*[221]

[221] Ibid., p.8.

eventually die out, and another that would eventually lead to modern humans. He gave this vision form in what was possibly the most striking visual representation to date of Hominid phylogeny (Fig. 4.5). Looking like a sprouting plant, the illustration showed the genus *Australopithecus* forming the early core of the family *Hominidae* as it budded from its pongid (ape) base. The *Hominidae* then diverged itself, with *Pithecanthropus* splitting from *Australopithecus* even as the latter persisted in its own development. *Pithecanthropus* then gave rise to *Homo* in like fashion, with the species *neanderthalensis* and *sapiens* eventually going their separate ways, towards very different fates.

With Ashton, Zuckerman continued to publish studies for the next several years questioning the difference between the Australopithecines and the living apes with respect to particular anatomical features, or arguing for the merits of his brand of metrical analysis in morphology.[222] However, Clark no longer worried as he once had. As he told Robinson in 1957, "Zuckerman has become entirely discredited in this field, and his continual return to the subject with his specious arguments is regarded as rather a joke (and a joke in bad taste!). He has in fact defeated his own ends by the ridiculous mistakes which he has made."[223] This was not, of course, the assessment of a disinterested observer of the debate. That said, Clark was not, like Broom, prone to

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[222] These included (with E.H. Ashton), "The Base of the Skull in Immature Hominoids," *American Journal of Physical Anthropology* 14, pp.611-624; "Mass und Zahl in der Morphologie (The Use of Measurement and Numbers in the Study of Morphology)" *Ergebnisse der Medizinischen Grund1agenforschun* (ed. KF.Bauer), (Stuttgart: Georg Thieme Vedag, pp.737-771); "Age Changes in the Position of the Foramen Magnum in Hominoids, *Proc.Zoo.Soc.Lond*. 126: pp.315-325; "Cranial Crests in the Anthropoidea," *Proc.Zool.Soc.Lond*. 126: pp.581-634; "The Infraorbital Foramen in the Hominoidea," *Proc.Zool.Soc.Lond*. 131: pp.471-485.

[223] Clark to Robinson, 15 Jan 1957, TM305A.

exaggerate the success of his own side of a debate in the face of considerable evidence to the contrary. In the early fifties, Clark had taken Zuckerman's criticisms extremely seriously, and had devoted considerable effort over a number of years to arguing against them. At that time, he had revealed genuine anxiety that he might not be able to counter Zuckerman's influence. No such anxiety is evident in his letters from later in the decade, and it thus seems fair to judge that the situation had truly changed. He did still keep his ear to the ground as far as Zuckerman's activities were concerned, but he no longer anticipated any threat to his own position: "I gather, incidentally," he told Robinson, "that Zuckerman has 2 young men desperately searching for evidence that the pelvis is really an ape's pelvis, & not indicative of an erect bipedal stance, but heaven known how he is going to prove this!"[224] In any event, Clark could finally retire from controversy, as he had so wanted to do in the first place, with the confidence of one who had prevailed.

For his own part, Zuckerman never accepted that his position had in any way been discredited. More than a decade later, when a reviewer of one of his books remarked that Zuckerman, in his reminiscences of the debate, seemed to have omitted "certain aspects of the controversy in which he was confuted," Zuckerman angrily wrote the author challenging him to say precisely in what respect he had been proved wrong.[225] Even in writing a biographical memoir of Clark upon the older man's death,

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[224] Clark to Robinson, 28 Jan 1957, TM305A. I have found no evidence that any publications resulted from this particular effort.

[225] Anthony Storr, review of *Beyond the Ivory Tower* by Solly Zuckerman, *The Sunday Times*, (November 29, 1970); Zuckerman to Storr 4 Dec 1970, WLGCP Doc. #145.

Zuckerman persisted in characterizing their disagreement as a failure on Clark's part in a debate that had never been brought to a close.[226]

The Australopithecines did remain at the center of scientific debate past the publication of Clark's book, but that debate was no longer, as Zuckerman would have had it, a matter of deciding the fundamental anatomical resemblance of those creatures to humans and their close ancestors rather than the living apes. To consider the South African creatures as central to the elucidation of human evolution was no longer to place oneself outside the scientific mainstream, as it had been in the thirties. With they new found status, more specific question were now actively being asked of the creatures, and they by no means answered everyone with the same voice.

[226] Zuckerman, "Wilfrid Edward Le Gros Clark." One wonders at the wisdom of the editor of *Biographical Memoirs* in selecting Zuckerman for this task. He and Clark had worked closely together at one time, but presumable that would have been true of a number of other colleagues as well.

Chapter 5 – Classifying *Australopithecus*

The March, 1950 issue of the *American Journal of Physical Anthropology* (hereafter the *AJPL*) saw the publication of an essay by Broom, now eighty four years old, entitled "The Genera and Species of the South African Ape-Men." He wanted to address concerns that had been expressed of late by some scientists about the proliferation of taxonomic groups among the ape-men over the last decade and a half, most of them Broom's creations. There was, of course, Dart's original *Australopithecus africanus* from Taung.[227] Dart had recently reentered the fray with fossil discoveries at a new site called Makapan, which he assigned to a different species but the same genus as his original find with the name *Australopithecus prometheus* (to be discussed more fully in Chapter 6), though Broom was of the opinion that Dart's new finds ought to be classified in a separate genus, and possibly even a separate sub-family, from *A. africanus*. Beginning in 1936, there were Broom's finds from Sterkfontein, which he had originally called *Australopithecus transvaalenis*,[228] but later changed to *Plesianthropus transvaalensis*.[229] From Kromdraii, there was *Paranthropus robustus*, first named in 1938,[230] which Broom assigned to a third sub-family. This last was joined at a generic level in 1949 by *Paranthropus crassidens*, discovered at Swartkrans during excavations conducted

[227] Dart, "*Australopithecus africanus*."
[228] Broom, "A New Fossil Anthropoid Skull from South Africa."
[229] Broom, "The Pleistocene Man-Apes of South Africa."
[230] Ibid.

jointly by the Transvaal Museum and the University of California.[231] Broom argued that all of these types differed to some degree or another in their anatomies. Further he emphasized that it "must be remembered that we have caves that seem to range in age from Upper Pliocene to Upper Pleistocene," and as such there need not be any surprise "at having found a number of different species and genera."[232]

Beyond the three sub-families, four genera, and five species, that he enumerated, Broom speculated that "in the next 5 or 10 years we shall have doubled the number of species, and in 20 years it is likely that we shall have the complete history of pre-man in South Africa, and the main facts of the evolution which led from higher Primate to man." This increase, Broom continued, would result in a "chain" of "connecting links" continuous enough anatomically that it would become near impossible to strictly define species on natural grounds. Scientists ought thus to make liberal use of taxonomic distinctions for the sake of convenience, lest they drown in the increasingly complex evidence: "There can be no finality [in taxonomy]. It is only a matter of convenience, and I think it will be much more convenient to split the different varieties into different genera and species than to lump them."[233]

Broom did not mention a further addition to the field of taxonomic groups that he and his assistant John Robinson had perpetrated the year before. The omission was not an oversight. In April of 1949, Robinson discovered at Swartkrans what he and Broom referred to in print as "the lower jaw of what is manifestly a new type of man." Since they considered this specimen "man" rather than "ape-man," it had no place in a

[231] Broom, "Another New Type of Fossil Ape-Man."
[232] Broom, "The Genera and Species of the South African Ape-Men," p.12.
[233] Ibid., pp.12-13.

discussion of ape-man taxonomy. While Swartkrans had been yielding a steady stream of *P. crassidens* specimens, this new jaw had come from what Broom and Robinson argued was a considerably younger deposit in the cave. Further, the jaw was very small compared to the large, thick jaws that seemed to characterize *P. crassidens*, and displayed too distinct a structure to be regarded as a mere variant of that form. They named the creature *Telanthropus capensis*, and declared it "intermediate between one of the ape-men and true man."[234] About a year later, Broom and Robinson described another partial jaw from Swartkrans that "appears to be the jaw of an early type of man." This jaw, however, had been found in a position that made it contemporaneous with *P. crassidens*. While declining to say with certainty whether this jaw had also belonged to *Telanthropus*, Broom and Robinson were adamant that it had not belonged to *P.crassidens*, preferring the explanation that the South-African ape-men had shared the land with creatures that had taken the next step towards humanity.[235]

Even as Broom's ally in the struggle to have the South African fossils recognized as important to human evolution, W.E. Le Gros Clark had trouble justifying the steady procession of new names. He wondered whether the obvious variability of the South African ape-men as a whole might not be better represented as occurring within a taxonomic group rather than between groups. "I myself feel in considerable doubt about this tendency to taxonomic subdivision," he wrote in one of his papers, "for it has yet to be demonstrated that the range of variation [among the

[234] Broom and Robinson, "A New Type of Fossil Man," p.321.
[235] Broom and Robinson, "Man Contemporaneous with the Swartkrans Ape-Man," pp.151-155.

South African ape-men] is any greater than that which obtains in a single genus (or even species) of the modern anthropoid apes." The creation of "yet another new genus, *Telanthropus*" on the basis of a single specimen only exacerbated the problem.[236]

Beginning in the late forties and continuing through the fifties, the nomenclature and classification of the South African fossils posed a central problem for scientists interested in human evolution. This fact alone is indicative of a shift from earlier debates over the Austalopithecines: with an increasing number of scientists sympathetic to the idea that the Australopithecines as a group offered important evidence for human evolution, debates could focus on other issues, such as the validity and internal relationships of the various taxa that Broom had created to accommodate the fossils. This does not mean that the question of the evolutionary place of the Australopithecines was settled in the mind of every scientist – Zuckerman, for instance, continued his general resistance even while others moved on. Further, the increasing credibility of Dart's original claim among scientists did not mean that the Australopithecine fossils ceased to be the foci of struggles for scientific credibility. This chapter investigates how the naming and classifying of the increasing number of specimens became a common subject of debate, and how this new debate, like those that preceded it, challenged scientists around the world to decide which claims, and which claimants, should be judged credible.

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[236] Clark, "New Paleontological Evidence Bearing on the Evolution of the Hominoidea," p.257.

In 1950, a number of the world's best known biological and anthropological scientists gathered to discuss human evolution at the Biological Laboratory at Cold Spring Harbor on Long Island. At this conference, the basic idea of humankind's Australopithecine ancestry was endorsed by influential scientists who had not previously been engaged in the debate, demonstrating the degree to which that position had gained ground in the wider scientific community. However, some of these outside authorities also posed new questions which, partly because of the strong influence of the speakers, acted as catalysts to new debates. The paper given by the well-known systematist Ernst Mayr, in particular, worked to make taxonomy a new focus for debate. Their debate moved back to South Africa, where the task of reconsidering the classification of the fossils fell to John T. Robinson, Broom's successor at the Transvaal Museum. Like Dart and Broom before him, geographical circumstances made Robinson the primary link between the fossils and an international community of scientists who wanted information about the specimens. Robinson's scientific credibility, like that of his predecessors in South Africa, was thus crucial to the shape of the debate. Like his mentor Broom, Robinson was not content to simply disseminate information about the fossil material without exercising a degree of control over its interpretation. Managing relations with the sometimes prickly Robinson was thus a necessary part of participating in the Australopithecine debate for foreign scientists. Through Robinson's correspondence with two foreign scientists, this chapter will examine contrasting ways in which these scientists sought

to manage Robinson as a link to the crucial fossils, and the different results their efforts yielded.

Cold Spring Harbor

In June of 1950, 129 scientists from eight countries met at the Biological Laboratory at Cold Spring Harbor on Long Island for the fifteenth in the series of Symposia on Quantitative Biology, the first of which had been held in 1933. The topic for the 1950 meeting was "The Origin and Evolution of Man". The program had been organized by Sherwood Washburn, an anthropologist from the University of Chicago, and the Columbia University geneticist Theodosius Dobzhansky. The home disciplines of the two organizers mirrored the general character of the meeting's attendees, with anthropologists and geneticists making up the vast majority, though it also included the odd paleontologist, systematist, ecologist, zoologist, sociologist, and psychologist. The main topics covered in the nine sessions that made up the program included the study of population, the evolutionary origins of human beings, the classification of hominid fossils and the genetics of race.

The object of the meeting, as stated by the Director of the Laboratory, Milislav Demerec (as well as a number of the speakers) was to promote dialogue between the various represented disciplines, especially anthropology and genetics, which had theretofore developed along largely separate paths. This situation, it was repeatedly emphasized, had led to confusion and incompleteness in the knowledge available to any individual scientist interested in human evolution. In the words of one of the

session chairmen, it was hoped that the meeting would result in some mental cross-fertilization and produce offspring full of hybrid-vigor. He hoped that the divergence of disciplines had not proceeded so far that the results of their intercourse would prove sterile.[237]

The envisioned "synthesis" of disciplinary knowledge around the subject of human evolution was designed to do for that field what many scientists believed had lately been done for evolutionary biology more generally. The "Modern" or "Neo-Darwinian Synthesis" had begun in the thirties with technical work in population genetics by Sewall Wright, R.A. Fischer, and J.B.S. Haldane. Their writings were adapted by other scientists in the following decades to forge an evolutionary theory that was consistent with both Darwinian Natural Selection and modern genetics. Several scientists prominently associated with the Neo-Darwinian Synthesis presented papers at the Cold Spring Harbor meeting. Dobzhansky, the primary architect of the genetic side of the Synthesis, co-organized the event and presented a paper on diversity and adaptation in human beings.[238] George Gaylord Simpson of the American Museum of Natural History, chief paleontologist of the Neo-Darwinians, spoke on the application of broad paleontological principles to the particular problem of human origins.[239] And Ernst Mayr, whose well-known work attempted to bring biological systematics into line with the rest of the Synthesis, gave a paper on the

[237] Demerec, "Forward."
[238] Dobzhansky, "Human Diversity and Adaptation."
[239] Simpson, "Some Principles of Historical Biology Bearing on Human Origins."

taxonomic categorization of fossil hominids – among which he included the Australopithecines.[240]

Mayr began his paper by paying tribute to the stated socio-epistemic purpose of the symposium: the bringing together of specialists and specialized knowledge from different fields. He himself, he quickly added, had no "first-hand knowledge of paleoanthropology," and could only comment on the classification of fossil hominids from the point of view of a systematist armed with the progress that had "been made within recent years among biologically thinking taxonomists in the understanding of the categories of subspecies, species and genus." However, it turned out that this was not a very limiting perspective at all, since in Mayr's view "[the] whole problem of the origin of man depends, to a considerable extent, on the proper definition and evaluation of taxonomic categories." There seemed to be less agreement about such definitions as regarded man and the primates than was the case with any other group of animals. At the extreme end, Mayr singled out those, like Broom (whom he named), who "use specific and generic names merely as labels for specimens without giving them any biological meaning." Mayr blamed the resulting "bewildering diversity of names" on two factors: first, a myopic concern with only a "small fraction of the animal kingdom" had resulted in taxonomic standards in anthropology developing without reference to standards in the rest of zoology; and second, "the attempt to express every difference of morphology, even the slightest one, by a different name and to do this with the limited number of taxonomic categories that are

[240] Mayr, "Taxonomic Categories in Fossil Hominids."

available." He illustrated the difference in standards by way of comparison: the fly genus *Drosophila* contained about six hundred species, and these differed more from each other than did the higher Primate taxa. This would be obvious to anyone, he added, if the flies were enlarged to the size of a human being. Thus, what "in the case of *Drosophila* is a genus has almost the rank of an order or, at least, suborder in the Primates."[241]

Mayr's solution to the problem he perceived was to rigorously apply the knowledge of what he called the "new systematics", which held that taxonomic categories had real biological meaning that could be studied and discerned, rather than being merely labels of convenience. He began with the genus. Genera were conventionally understood as "one species, or a group of related species of common ancestry, which differ in pronounced manner from other groups of species and are separated from them by a decided morphological gap." However, Mayr added that recent studies had shown that "the genus is not merely a morphological concept but that it has a very distinct biological meaning. Species that are united in a given genus occupy an ecological situation which is different from that occupied by the species of another genus." This consideration did not eliminate morphology as a generic determinant, but necessitated that whatever morphological criteria were cited as the bases of generic separation be tied to the ecological situation of the species in question. Applying this to fossil hominids, Mayr found that far from requiring Broom's multiple genera for the South African creatures alone, it was not even clear

[241] Ibid., p.109.

that they should be separated generically from modern humans! "After due consideration of the many differences between Modern Man, Java Man, and the South-Africa ape-man," he wrote,

> I did not find any morphological characters that would necessitate separating them into several genera. Not even *Australopithecus* has unequivocal claims for separation. This form appears to possess what might be considered the principle generic character of *Homo*, namely, upright posture with its shift to a terrestrial mode of living and the freeing of the anterior extremity for functions which, in turn, have stimulated brain evolution.

Mayr continued that his radical revision of hominid taxonomy was based on "two major points," both of which signaled how much change had come to the study of human evolution since *Australopithecus* entered the field twenty five years earlier. One was "the overall picture of morphological resemblance with a deliberate minimizing of the brain as a decisive taxonomic character." The other was "the assumption that all these forms, including *Australopithecus*, are essentially members of a single line of descent." He admitted that this last assumption could very well be disproved with further discoveries, but that, contrary to Broom, "taking all the available evidence together, it seems far more logical and consistent at the present time to unite the hominids into a single genus than to continue the current multiplicity of names."[242]

The new "biological" definition of a species as "a group of actually or potentially interbreeding natural populations that is reproductively isolated from other such groups" posed more difficulty for analysis of fossil hominids, since their

[242] Ibid., pp.110-111.

reproductive patterns could not be ascertained directly. However, since this definition was not morphological in its essence, it allowed for the recognition that species could be "polytypic," varying both spatially and temporally. Further, even within intraspecific groups, individuals could vary according to sex, not to mention age. Could this not explain the variation among South African hominids just as well as Broom's separate genera and species? "It seems possible, if not probable," Mayr said, "that various South African finds, *Australopithecus, Plesianthropus*, and *Paranthropus*, might well be age or sex stages of a few related tribes, notwithstanding Broom's (1950) assertions to the contrary." Even if one did not accept this explanation, Mayr thought Broom's taxonomic divisions were still no help since they "may not have any validity, according to the Rules of Zoological Nomenclature, Article 25A, as revised in 1930." This article required that any new name be accompanied by a description specifying diagnostic characters separating the named group from other related groups. This required consistency, but Mayr pointed out that since "one of these names [for the genera of ape-men] was based on a child, another on an adult female, a third on an adult male, an enumeration of diagnostic differences is virtually impossible." In the meantime, he felt that "[to] consider [the South African ape-men] all as one species is the simplest solution that is consistent with the available evidence." He called that species *Homo transvaalensis*.[243]

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[243] Ibid., 112-115. Mayr does not say why he chose *Homo transvaalensis* as opposed to *Homo africanus*, which should have been the species name if temporal priority were the only issue. The choice was probably made because diagnostic differences were normally assessed with respect to adult specimens, and Broom's *Plesianthropus transvaalensis* was the first named group to include an adult specimen.

Mayr's concerns were those of a systematist, but dissatisfaction at Cold Spring Harbor with the current taxonomic situation in the study of human evolution also came from within the ranks of physical anthropologists. Sherwood Washburn, then an associate professor of anthropology at the University of Chicago, hoped that the Symposium would help achieve his goal of bringing the recent revisions of the rest of biology to his own field. This required that students of human origins concern themselves with broad evolutionary principles rather than focusing on morphological minutiae. Like Mayr, Washburn argued that those morphological characters used to make taxonomic distinctions must have their biological importance specified in order to be valid. Characters of primary importance were those responsible for major evolutionary "radiations," meaning groups of related organisms that had adapted to significantly different ecological situations from that of their common ancestor. Considering the origin of hominids as a radiation within the Primates, Washburn argued that the adoption of bipedalism was the crucial change, and that scientists should thus look first to the morphology of the pelvis as a determining factor. It seemed to him that the known fossil pelvises associated with the Australopithecines were sufficient evidence to regard them as having achieved a fully bipedal gait. While Mayr had used a similar line of argument to place *all* hominids into a single genus, Washburn took the comparatively conservative position of placing them in the same family (recall that Broom suggested three families). He suggested dividing the family into two genera, *Homo* for modern humans and the relatively large-brained Asian

hominids like Java Man, and *Australopithecus* for the relatively small-brained hominids of South Africa.[244]

Washburn ended by making a partial concession to Broom's theory of taxonomic practice, but one that implicitly contained a harsh criticism. He translated Broom's idea that taxonomy was primarily a matter of convenience to the idea that "[the] number of names is a function of the kind of interest of the investigator." "If one is primarily interested in classification, in type specimens and priority," wrote Washburn,

> then the less there is known about fossil primates the more names there will be. If one is interested in the mechanics of evolution, in the understanding of process, a cumbersome and constantly changing classification is a great liability and the tendency will be to lump, to leave fragmentary bits unnamed, and to create new groups only when absolutely necessary.[245]

Clearly, Broom was meant to be identified with the former group, one whose plethora of names betrayed a fundamental and even willful ignorance of evolutionary theory and a fetishization of fossils and names. In contrast, Washburn was to be identified with the latter, for which fossils and names were also important, but as means to a deeper understanding of evolution.[246]

For those, like Clark, who had for a few years already been pushing for the acceptance of the Australopithecines as near –relations of human beings, the change in the nature of the debate in evidence at Cold Spring Harbor was a very welcome

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[244] Washburn, "The Analysis of Primate Evolution with Particular Reference to the Origin of Man."
[245] Ibid., p.76
[246] The following year, Washburn made a condensed version of this argument for a wider audience in an article for *Nature* written with a University of Chicago colleague: Washburn and Patterson, "Evolutionary Importance of the South African 'Man-Apes'."

development. "I have just received the volume of the Cold Spring Harbour Symposium on the Evolution of Man," he wrote to Kenneth Oakley upon seeing the printed version of the proceedings, "…There seems to be a consensus of opinion that the Australopithecines really are affiliated to the Hominidae & should be included in this family. In fact, the main point at issue seemed to be whether they ought to be included in the genus Homo!"[247] Oakley agreed: "There is no doubt about the swing of opinion in regard to the Australopithecines."[248]

Clark and Robinson: Appeasing the Locals

On April 6th, 1951, at eighty-five years of age, Robert Broom died. He had lived just long enough for his quest to achieve recognition for the Australopithecines as close relatives of human beings to be fulfilled in the papers given at the Cold Spring Harbor Symposium. He worked right up until the end, completing a monograph on fossils from the Swartkrans site just before his death. As with many of his publications from the last five years of his life, the Swartkrans monograph was co-authored with his assistant at the Transvaal Museum, John T. Robinson. Not yet thirty years old, Robinson was appointed to fill Broom's post at the Museum, and it now fell to him to act as the primary conduit between the growing collection of Australopithecine fossils in South Africa and the international scientific community.

The increasingly sympathetic attitude internationally towards the claim of the Australopithecines' human affinities had been made possible in large part by the

[247] Clark to Oakley July 18 1951, DF140/6 BMNH.
[248] Oakley to Clark, July 19 1951, DF140/6 BMNH.

relationships between Broom and Gregory on the American side, and Broom and Clark on the British side. The relationships had been mutually beneficial, with Broom receiving international support for his views, and the foreign scientists receiving special access to information about the specimens. Robinson was an unknown quantity, and those who had enjoyed special access to the materials under Broom's stewardship were anxious to establish ties to the man who would now presumably control the dissemination of information. As Clark wrote to Oakley in 1951 upon hearing that Robinson was making a visit to Britain, "I do not know what sort of chap he is – nobody seems to know for certain whether he has a degree or any other academic qualification – or what his experience has been (apart from working as assistant to Broom). I would like to see him as soon as conveniently possible."[249]

Robinson did in fact have a degree (two, in fact), but, unlike any of the major characters so far discussed, he had studied at a South African university. Indeed, unlike Dart and Broom, Robinson had been born in South Africa, just two years before Dart announced the original Australopithecine find. He took a Bachelor of Science degree from the University of Cape Town in 1943 and a Masters of Science in Zoology from the same institution a year later. In 1945 he began doctoral studies in marine biology at Cape Town, but left off that endeavor to become an assistant to A.J.T. Janse, a specialist on moths at the Transvaal Museum. Shortly thereafter he transferred to the Division of Physical Anthropology and Vertebrate Paleontology, where he became Broom's assistant. During the five years of their collaboration

[249] Clark to Oakley Mar 25 1951, DF 140/6, BMNH. Robinson was bringing casts to Britain to stage an exhibition being put on by the Museum of Natural History, where Oakley worked as a curator and research scientist.

before Broom's death, they co-authored no fewer than twenty three publications. Robinson also began to take primary responsibility for excavations as Broom's health deteriorated.[250]

One of the proceeds from Robinson's excavatory work while Broom was still alive was the discovery of the jaw of *Telanthropus capensis* in 1949 at Swartkrans. As we have seen, Broom and Robinson were of the view that *Telanthropus* was a different, and far more human-like creature from *Paranthropus crassidens*, to which most of the fossils from Swartkrans belonged. The *Telanthropus* jaw was found in a geological context that made Broom and Robinson believe that it was younger than the *Paranthropus* material. However, a second jaw was found not long after that seemed to indicate that a similar creature had been contemporaneous with *Paranthropus*.[251] They declined to say definitively whether the second jaw had also belonged to *Telanthropus*, but declared the significance of these two finds to be the fact that humans, or something very near-human, may have lived contemporaneously with and in close proximity to the ape-man *Paranthropus*. In the co-authored 1951 monograph on the Swartkrans materials, Robinson was given the task of describing and interpreting the *Telanthropus* dentition, and he defended the separate classification of the two forms found at Swartkrans. There were similarities between the two forms, to be sure, but "the resemblances… are most easily explained by the assumption that [*Paranthropus* and *Telanthropus*] have arisen from a common ancestor, an earlier Australopithecine." While *Paranthropus* had failed to evolve appreciably from this

[250] This biographical information on Robinson was derived from Tobias, "The South African early fossil hominids and John Talbot Robinson (1923–2001)."
[251] See n.5.

earlier form, *Telanthropus* had paved the way for the future: "*P. crassidens* doubtless remained an Australopithecine and eventually became extinct. *Telanthropus* had already evolved out of the Australopithecine group and reached true human status."[252] These were remarkable claims, as they accorded to South Africa a central place in human evolution not only as home to the Australopithecines, but also as a possible theater for the transition from Australopithecine to human.

It was on the question of *Telanthropus* in particular, and on the taxonomy of the South African fossils in general, that Clark began his correspondence with Robinson in the early fifties, after having met him in Britain. Echoing Mayr's criticism at the Cold Spring Harbor Symposium, Clark complained that no formal diagnostic criteria had yet been given for the taxonomic distinctions that had been proposed, and wondered "whether all the specimens found in S. Africa are actually no more than variants of a common species (or certainly a common genus)." By way of example, Clark wrote "[it] seems to me that the ordinary chimpanzee & the pigmy chimpanzee show just as much difference as Australopithecus, Paranthropus & Telanthropus, & yet they are not separated generically."[253]

Robinson's response has not been preserved, but from Clark's next letter it can be inferred that Robinson responded by, among other things, echoing Broom's line about the arbitrariness of taxonomic categories. Clark was conciliatory, writing that Robinson's explanatory letter "clarifies your position quite considerably," and agreeing that "definitions of taxonomic categories are not only difficult – they are

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[252] Broom and Robinson, *Swartkrans Ape-Man: Paranthropus crassidens,* p.118.
[253] Clark to Robinson, Dec 18 1952, Box 305A, TM.

bound to be arbitrary since presumably they are all ultimately linked by interpretations." However, he continued to echo Mayr by pressing Robinson to explain how his interpretation squared with modern findings in ecology and genetics:

> If "Telanthropus" really represents a distinct genus (or, for that matter, a distinct species) it is difficult to understand how it could be strictly contemporaneous with "Paranthropus" and in the same locality. The geneticists would argue against the idea that they represent two diverging groups on the grounds that you cannot have speciation going on unless the groups are segregated, for only in this way would genetic diversification proceed.

Clark suggested an alternative model of continuous variation within a single group, which would circumvent concerns based on the current theory of speciation: "It seems to me that, taking the Australopithecines as a whole, you have a continuous gradation linking up 'Telanthropus' through 'Plesianthropus' with 'Paranthropus'. In fact, that seems to me to be one of the most interesting points about these S. African fossils."[254]

Clark's began his next letter by thanking Robinson for writing "at such length on this problem of Telanthropus," suggesting that Robinson was very concerned to make himself understood to the older, more established scientist. One of his defenses was the familiar line that one really had to see the specimens to speak credibly about them. Having used this argument himself against Zuckerman, Clark was not in a position to disagree: "Probably, as you say, one is not really in position to adjudicate on the problem without examining the actual material." Robinson also argued that there existed too much morphological discontinuity between the various taxa for

[254] Clark to Robinson, Jan 18 1953, Box 305A, TM.

Clark's model of continuous variation to hold. However, Clark had a proposal to explain this as well:

> I have wondered whether the Australopithecines may have shown rather a marked degree of sexual dimorphism which could have expressed itself in pronounced differences in tooth size (& therefore also in the shape of the mandible & the sub-nasal part of the maxilla). If this were so, it would satisfactorily explain the obvious discontinuity to which you refer as distinguishing "Telanthropus" from "Paranthropus".

So as not to offend Robinson with presumptuousness, Clark added a caveat to his hypothesis: "But I hesitate to make this suggestion, for you will presumably already have given it consideration."[255]

Whether or not Robinson had already given consideration to the possibility of marked sexual dimorphism in the Swartkrans specimens, he did address the matter, among others, several months later in a paper for the *AJPA*. Here, Robinson laid out at length his arguments for regarding *Telanthropus* as something essentially different from the Australopithecines. At Swartkrans, he wrote, they had found the remains of at least 35 (and probably more) individuals of *P.crassidens* showing only moderate variation in size. This indicated to Robinson that *P.crassidens* had not shown marked sexual dimorphism, and thus that the few (now three) specimens from obviously much smaller creatures were more likely a different species rather than the female *P.crassidens*. Further, in species of higher primates that showed significant sexual dimorphism, the difference was one of size only, whereas the jaws and teeth (the only

[255] Clark to Robinson, Feb 2 1953, Box 305A, TM.

known parts) of *Telanthropus* also differed in proportion and structure from the corresponding parts of *P.crassidens*.[256]

Having argued for the separate taxonomic status of *Telanthropus*, Robinson went on to make a case for seeing that creature as a link between the Australopithecines and more "advanced" hominids such as Asia's *Pithecanthropus.* The link was not a chronological one, for the *Telanthropus* and *P.crassidens* specimens at Swartkrans showed the two creatures to have been coeval, and thus one could not have been descended from the other. The important fact for Robinson was that *Telanthropus* represented a morphological stage between the Australopithecines and more advanced hominids. *Telanthropus* had likely been derived from an earlier Australopithecine, with the changes it underwent representing a move in the direction of more advanced hominids. In contrast, the later Australopithecines such as *P.crassidens* who lived alongside *Telanthropus* had retained essentially the same form as their ancestors. Robinson adopted an unusual nomenclature to express the phylogenetic distinction he wanted to make: borrowing from the German anthropologist G. Heberer, Robinson referred to the various genera of Australopithecines (*Australopithecus, Plesianthropus, Paranthropus*) as "prehominids", and to all more advanced hominids, including modern humans, as "euhominids." *Telanthropus* had either completed the transition to euhominid status,

[256] Robinson, "Telanthropus and its Phylogenetic Significance," pp.484-85.

or had very nearly done so, while at the same time retaining enough traces of its prehominid past to be recognized as an important link between the two groups.[257]

Clark wrote to congratulate Robinson on the article, telling him "I think you have presented the evidence very fairly." However, he was not ready to endorse Robinson's position: "I shall probably sit on the fence for the moment (at least as regards a generic distinction)." Always careful not to endanger his primary connection to the fossil material, Clark assured Robinson that his ambivalence was "not, as you will understand, because I doubt your judgment on the matter," but rather because he tended "to be inhibited by a sort of (uniform?) streak of conservatism in my temperament!" Clark had another reason to keep his differences with Robinson from forming a barrier between them: he was attempting (successfully, as seen in the previous chapter) to enlist Robinson on his side in the controversy with Zuckerman, and did not want to make an enemy of his South African colleague.[258]

Robinson designed the *Telanthropus* article to defend his and Broom's interpretation of that creature as distinctly more human than its Australopithecine neighbors and ancestors against the criticism of skeptics. In a further article for the *AJPA*, published about six months after the *Telanthropus* piece, Robinson continued to try to assert local control over the interpretation of the South African materials on an issue that foreign scientists had long thought in need of revision: the taxonomy of the Australopithecines. This time, Robinson heeded that call, at least to a certain extent, and for the first time made a significant break with his late mentor Broom. He

[257] Ibid., pp.481-500.
[258] Clark to Robinson Mar 22 1954, Box 305A, TM.

began by contrasting the two most extreme positions yet taken: on the one side, there was Broom's taxonomy, consisting of three sub-families, four genera, and five species of Australopithecine; on the other, Mayr's "lumping" of all the Australopithecines into a single species within the genus *Homo* (and consequently also within a single sub-family), which they shared with all hominids right up to modern humans. Robinson took a middle course, arguing for a single sub-family, two genera, and two species for the South African Australopithecines, with a third species to house the remains of a Primate discovered in Indonesia that Robinson had become convinced was an Australopithecine. The most important distinction, for Robinson, was that between the two genera, which he believed represented two separate evolutionary lines. The first genus, *Australopithecus*, contained Dart's original find from Taung, all the Sterkfontein material, and Dart's newer finds from Makapan. Going further, Robinson lumped all of these into a single species, *A.africanus*, rejecting Broom's position that the Sterkfontein remains represented a separate genus *Plesianthropus* and the Makapan remains a separate sub-family *Archanthropinae*. Robinson allowed only that this species might be divisible into two sub-species. The second genus, *Paranthropus*, contained all the remains from Kromdraii and Swartkrans (except those ascribed to *Telanthropus*) as well as the Indonesian specimen. The South African members of this genus were put into a single species, *P.robustus*, with the former specific distinction between *robustus* and *crassidens* demoted to sub-specific rank. The Indonesian form was given its own species, *P.paleojavanicus*. Robinson provided

formal diagnostic criteria for each of the distinctions he proposed, fulfilling one of the major concerns that had been expressed by Mayr and Clark.[259]

While Robinson's article attempted to address the concerns expressed by other scientists about the taxonomy of the Australopithecines, it also served as a vehicle to further explain and advance Robinson's controversial position on *Telanthropus*. As he had argued in his previous paper, Robinson wrote that *Telanthropus* had achieved a more advanced stage of organization, which he labeled "euhominid," than had the Australopithecines, whose stage he labeled "prehominid." *Telanthropus* had presumable evolved from some prehominid ancestor, but because the fossil remains of the two known genera of Australopithecine were, according to Robinson, approximately contemporaneous with *Telanthropus*, they could not themselves represent that ancestor. Thus, Robinson reasoned that the prehominids must have diversified into at least three phyletic lines, one of which had advanced to euhominid status and given rise to *Telanthropus* and later hominids, while the other two gave rise to the two genera of Australopithecine described in the current article, both of which eventually went extinct. He represented his scheme visually in a phylogenetic tree with three branches (Fig. 5.1) diverging in the early Pliocene. One of these led to *Telanthropus* and then to later hominids, and the other two to the known forms of Australopithecine. No fossils existed to represent either the common ancestor of these evolutionary lines, nor any to represent the inferred transformation from that prehominid common ancestor to *Telanthropus* and the euhominids. Robinson

[259] Robinson, "The Genera and Species of the Australopithecinae."

acknowledged that the precise position of the evolutionary divergences that produced the separate lines and the exact number of such lines were uncertain due to lack of fossil evidence from the earlier part of the Pliocene. Nonetheless, he argued that the scheme in broad outline could be justifiably inferred from the known facts.[260]

Robinson hoped that his taxonomy paper would satisfy the demands that had been made for clarification in this area, and settle the issue on his interpretation. It was therefore somewhat of a disappointment when in 1955 he received a copy of Clark's *The Fossil Evidence for Human Evolution* and saw that Clark had not adopted his division of the Australopithecines into two major genera, and had expressed skepticism about the generic and specific names that had been applied to the fossils by workers in South Africa. As Figure 4.5 shows, Clark preferred to consider the South African creatures as a single evolutionary phenomenon linking more ape-like ancestors to later hominids.

Robinson suspected that Clark's book may have been written prior to the publication of his taxonomy paper, and he wrote to Clark inquiring whether he felt the same now that the paper had been published. He added that since then new evidence had only increased his confidence that *Paranthropus* and *Australopithecus* were separate lines. Based on dental anatomy, Robinson told Clark, he now believed that *Paranthropus* "had a diet similar to that of the great apes [i.e. herbivorous and

[260] Ibid., pp.196-99.

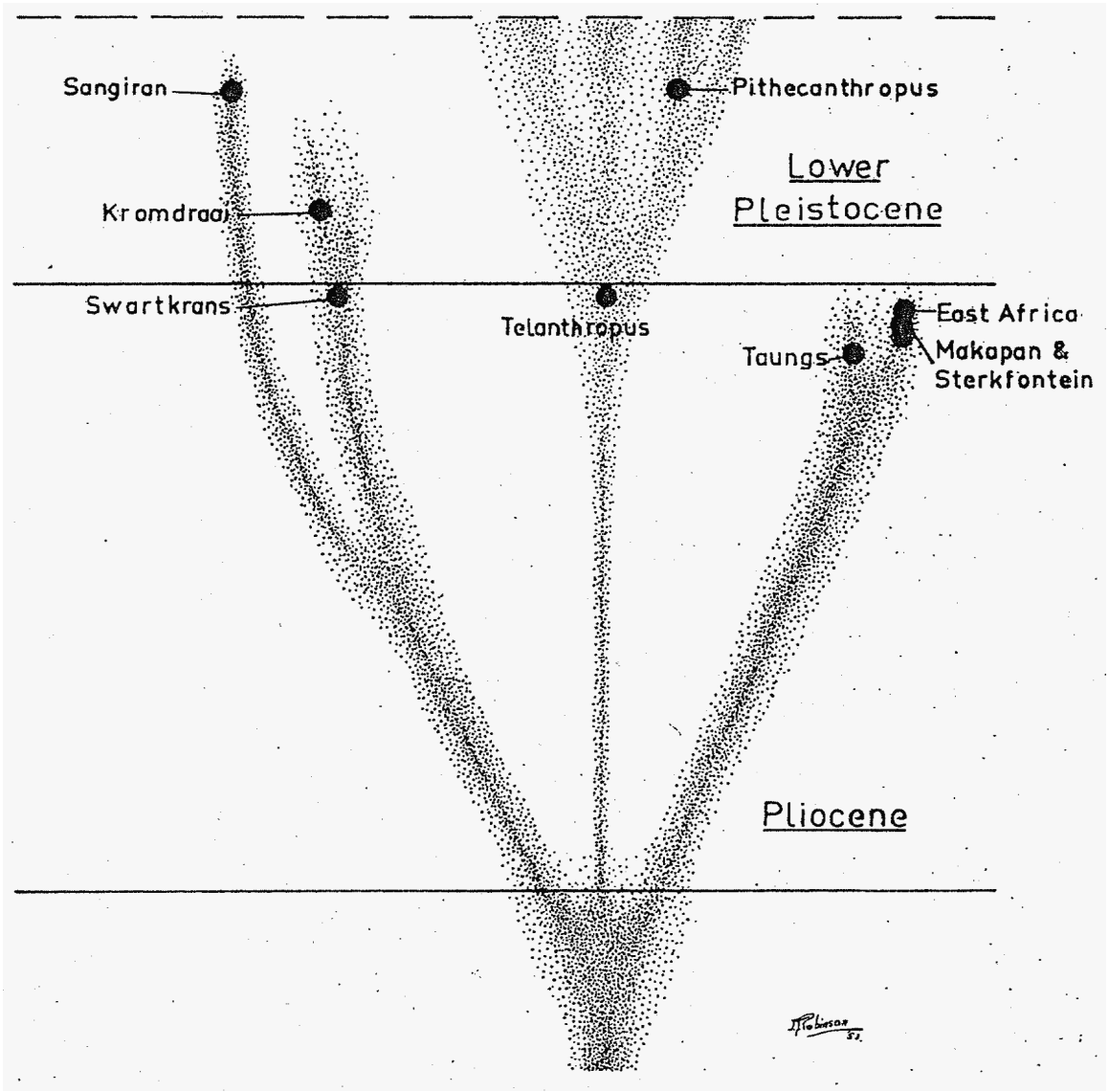

Figure 5.1 – Robinson's phylogeny of the "prehominids" and "euhominids"[261]

[261] Ibid., p.197.

frugivorous], whereas Australopithecus had one similar to that of early man [i.e. omnivorous]."[262]

Clark confirmed that his book had indeed gone to press before Robinson's taxonomy paper had appeared, and that "the additional evidence you presented in that article is quite convincing for your separation of the fossils into two groups." Clark had an additional excuse for not adopting Robinson's taxonomy: he did not want to provide Zuckerman with any openings for further attacks on the idea that the Australopithecines *in general* were ancestral to later hominids. As he put it to Robinson,

> I have quite deliberately expressed myself in cautious (even over-cautious) terms about the taxonomy, simply as a matter of tactics. I wanted to get thoroughly and finally established (as I think it is now) the position of the group as a whole in the phylogeny of the Hominidae, and to avoid giving Zuckerman and his people any loophole for diverting attention from this main point into irrelevant side-channels of argumentation about taxonomic details.[263]

Of course, such details were not irrelevant to Robinson, but rather integral to his argument about the status of *Telanthropus*, something Clark avoided mentioning in his conciliatory letter. On the other hand, as discussed in the previous chapter, the two had by this point become allies in the controversy with Zuckerman, so Robinson may well have accepted Clark's reasoning.

Whatever the case, their further correspondence reveals that Clark had succeeded in developing a trusting and collegial relationship with Robinson, even while they continued to disagree over the issue of *Telanthropus*. Most scientists in the

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[262] Robinson to Clark May 2 1955, Box 305A, TM.
[263] Clark to Robinson May 6 1955, Box 305A, TM.

field, in fact, disagreed with Robinson's differentiation of *Telanthropus* from the Australopithecines, and he became increasingly frustrated at his inability to convince them of his point of view. The trust that had built up between him and Clark allowed Robinson at least one avenue through which to express his feelings of irritation and isolation. Such feelings became more acute after 1956, when pebble artifacts, which appeared to be deliberately crafted tools, were discovered at Sterkfontein very nearby where much of that site's Australopithecine material had been recovered. No lithic culture had ever been found in association with Australopithecine remains, and the new discovery therefore raised the question of whether those creatures could have manufactured the tools. For Robinson, the resolution of that question hinged on the matter of *Telanthropus*, for if that creature was, as he believed, distinctly more advanced that the Australopithecines in the direction of more modern hominids, then a more reasonable interpretation might be that *it* was responsible for the manufacture of the tools (while no remains of *Telanthropus* had been found at Sterkfontein, Swartkrans was only a mile away). He cautiously suggested as much in a short article describing the tools, but, as he related privately to Clark shortly afterwards, he felt anxious about pressing the matter further because of the skepticism with which his ideas on *Telanthropus* had so far been received. Only with Clark did he feel comfortable expressing his ideas openly:

> It seems to me that much of the difficulty [regarding the manufacture of the tools] hinges of the matter of Telanthropus and here I find myself very diffident over discussing the point with other workers as I am well aware that most of my colleagues feel that this is a private little mania of my own. However, I am much happier to discuss this point frankly with you than with anyone else.

He declared himself baffled by "the general scepticism (sic.) or even outright opposition of my colleagues to Telanthropus," as he had gone over and over his reasoning on the matter only to arrive time and time again at the same conclusion:

> I am very well aware that the biggest fight one has as a scientist is the fight to control oneself, to attempt to keep one's judgments as dispassionate and objective as possible and so I have repeatedly taken out the material and examined it afresh to see if I am just deluding myself. But the evidence to me seems to be simply opposed to regarding Telanthropus as an australopithecine in any really meaningful sense of the term.

He implored Clark, as one who disagreed with him but whom he trusted, to help him understand the gulf between himself and the rest of the field: "I value your judgment more highly than that of any other worker in the field and you opinion may very well illuminate what for me is a great puzzle."[264]

Clark tried to calm Robinson's anxieties by disputing whether anyone truly regarded the matter of *Telanthropus* merely a "little mania" of Robinson's. He counseled Robinson to see the matter as a case of multiple of interpretations of what remained fragmentary evidence – a common enough situation.[265]

Robinson would not be appeased. He responded to Clark with accusations against specific persons he felt had singled out his views for belittling beyond the bounds of reasonable scientific disagreement. "In fact," he wrote, "both Sherry Washburn and Kenneth Oakley have indicated that they felt that I was sticking to a point of view unsupported by any evidence, because I had found all the Telanthropus

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[264] Robinson, "Occurrence of Stone Artefacts with *Australopithecus* at Sterkfontein"; Robinson to Clark Nov 13 1957, Box 305A, TM.
[265] Clark to Robinson Nov 20 1957, Box 305c, TM.

specimens and therefore wished to make much of them. We had a very heated discussion over this in Chicago last year." We will return to Robinson's relationship with Washburn later. It will be useful to look into Robinson's strained relations with Oakley here, as they provide further evidence of how Clark worked as mediator between the disgruntled Robinson and the rest of the field.

Kenneth Oakley, a paleontologist by training, had by the mid-fifties worked at the Museum of Natural History in London for a number of years, primarily on questions relating to human antiquity, and had become a respected authority in the field. At the same time as Clark was asked to write *History of the Primates* as a guide to the Primate fossil material held by the Geology Department of the Museum, Oakley wrote a similar booklet focusing on the cultural side of human antiquity, entitled *Man the Tool-Maker*. "Man is a social animal," he wrote to open the volume, "distinguished by 'culture': by the ability to make tools and communicate ideas. Employment of tools appears to be his chief biological characteristic, for considered functionally they are detachable extensions of the forelimb."[266] Tool manufacture as a definitional characteristic of "man" as opposed to earlier ancestors became a central part of Oakley's anthropological researches. It was not, however, his only interest: in the forties he began experimenting with a method for the absolute dating of fossils by way of measuring their fluorine content. The method had been known since the late

[266] Oakley, *Man the Tool-Maker*, p.1.

nineteenth century, but Oakley was the first to attempt a systematic investigation of fossil remains relating to human antiquity using the method.[267]

In the early nineteen fifties, Oakley decided that he wanted to try out the fluorine dating method on the Australopithecine material. With assurances from local workers, including Robinson, that he could have access to the material, he obtained a grant and made the journey in 1953. Upon his return, he wrote to Robinson thanking his for his help and hospitality, seeming to indicate the trip had been friendly and collegial: "I appreciate immensely the way in which you and your colleagues made everything available to me."[268]

Oakley published the results of his studies on the dating of the Australopithecine material in the *AJPA* in 1954. He concluded that known Australopithecine sites covered a range of geological time, with Makapan representing the oldest and Kromdraii the newest. He had also used the dating technique to assess the age of certain deposits in which stone tools had been found (in 1954, none of the Australopithecine deposits had yielded stone tools), and had found that these were of the same age as some of the Australopithecine deposits. He considered the possibility that the Australopithecines were the tool-makers, but rejected this hypothesis, citing the creatures' small brain and the fact that only large-brained hominids had ever been associated with tool-making. More likely, Oakley wrote, that at least some of the later Australopithecines had lived contemporaneously with some larger-brained descendant of earlier Australopithecines, and these were responsible for the tools. He

[267] Goodrum and Olson, "The Quest for and Absolute Chronology in Human Prehistory."
[268] Oakley to Robinson Jun 19 1953, Box 305B, TM.

acknowledged that the mandible from Swartkrans that Robinson and Broom had described as "human" might be a representative of this early tool-maker, even while he disputed Robinson's later ascription of this fossil to *Telanthropus*, whose legitimacy as a taxonomic category he doubted.[269]

Robinson was not happy about these results. He grumbled to Clark that Oakley had made a mistake in characterizing Makapan as older than Sterkfontein, and had ignored certain evidence to the contrary. He had also, according to Robinson, made errors in his description of the faunal remains at Makapan, and had in general dated the whole Australopithecine era too recently. More serious, however, were Robinson's allegations (again, expressed privately to Clark) that Oakley had acted improperly both during his visit and in his subsequent actions. "He has done some peculiar things lately," wrote Robinson, "and altogether he has gotten himself an unfortunate – to say the least – name out here as a result of his actions." His transgressions were numerous: "Much of the material which he has published since his return has been unpublished data, supplied to him out here by various people, which he just used without permission." After a collecting trip that resulted in the discovery of some stone implements, Oakley "secreted six (out of a total of about thirty) on his person and admitted this only after returning to England." Robinson also alleged that since the trip Oakley had been attempting to dictate future research in South Africa without local consultation. He complained that Oakley had asked him not to attend a meeting in London (which, he told Clark, he had wanted to attend at his

[269] Oakley, "Dating of the *Australopithecinae* of Africa."

own expense) at which recommendations for future research in paleoanthropology were discussed, and was then told by Oakley that "[the French paleontologist] the Abbé Lavocat had agreed to work up our rodent material and when could he come, without even asking us if we were in agreement with this course of action." When he protested, Robinson continued, "[Oakley] got very annoyed about this, no longer sends me reprints, pointedly omits my name in his writings and I hear from several sources says that little is going on in my department, that I am unco-operative and do not let other people see any of my material."[270]

There is no evidence to say who these "several sources" were, or that Oakley was actively denouncing Robinson after the trip. Robinson did bring up the matters of the unpublished data and the Abbe Lavocat in correspondence with Oakley, whereupon Oakley (who had not in fact been the one to invite Lavocat to work on the material) wrote that he was sorry if Robinson had been put "in an awkward position" and offered to make excuses to Lavocat if Robinson wished.[271] Oakley also wrote that he hoped Robinson would "not think that I fail to acknowledge how much help I received from you and your colleagues,"[272] and by way of evidence sent him an advance copy of his paper for review, including the acknowledgment section, which in the published version read "I am deeply indebted to the many workers who enabled me to visit sites or examine material which they are themselves investigating, and for

[270] Robinson to Clark Nov 10 1954, Box 305A, TM.
[271] Oakley to Robinson Oct 20 1953, Box 305B, TM.
[272] Oakley to Robinson Oct 17 1953, Box 305B, TM.

freely placing unpublished data at my disposal," among whom was "Mr. J. T. Robinson (who in the first place suggested my visiting South Africa)."[273]

Clark did not want to alienate Robinson by in any way implying that he was not on his side, but he was also close to Oakley and doubtful if Robinson's allegations were truly warranted. He thus responded with sympathy, but at the same time suggested that Oakley would not deliberately have tried to do Robinson any harm: "I am very sorry to hear that you are disturbed by Oakley's pronouncements. Of course I did not know anything of the matters about which you write, but I do feel that anything he may have done amiss was done quite inadvertently." In fact, Robinson would likely have been quite disturbed at how much of what he expressed "privately" to Clark was actually being shared with Oakley. When in 1957 Robinson was becoming more and more disturbed about what he perceived as the unduly negative reception of his view on *Telanthropus*, Clark began showing their correspondence to Oakley. Clark was worried that Robinson's anxiety might negatively affect the work being done in South Africa and Robinson's willingness to share it with the rest of the field, and he wanted to bring Oakley in on his strategy of placation so that they could work together to avoid such a consequence. As Clark told Oakley upon making him aware of Robinson's latest allegation against him,

> I have thought it desirable to let you see John's letter even though he attributes to you a criticism which I have no doubt you did not make. But it is well for us to be aware of his rather distorted impressions, because only so can we, with tact & care, rectify them. I fell it so very important that one should do all one possibly can to make John feel that he is not so isolated as he appears to be - & that his views

[273] Oakley, "Dating the *Australopithecinae*," p.21.

> (however much we disagree with him) meet with sympathy. All this partly for John's own sake – but also largely because of the importance of the work he is doing. Please treat all these letters most confidentially.[274]

Oakley understood Clark's reasons for sharing the letters with him, and, despite himself being the target of much of Robinson's ire, expressed a sympathetic understanding of the sources of the South African's unease: "I believe it is very fortunate that the relations between you and John are so cordial, and that he can turn to you for advice in this way without feeling that he is 'losing face'. It is none too easy for him working in the Transvaal atmosphere."[275] Replying to Robinson, Clark again tried to undo his "distorted impressions" of Oakley: "I feel that perhaps you may have misunderstood Kenneth Oakley (for I have always heard him speak so very well of you and your work and, of course, I have never heard him even suggest that your opinions were in any way coloured by the fact that you yourself had made the discoveries [of *Telanthropus*]."[276]

Robinson's behavior, however, was beginning to color Clark's opinion of his South African colleague. Time and time again, Clark advised Robinson to be cautious in putting forward interpretations based on as little material as was available for *Telanthropus*, and to emphasize the provisional quality of such interpretations and the need for further evidence to settle the question. In early 1958, local newspaper coverage of a scientific meeting in South Africa at which Robinson spoke was picked up by the international press, which characterized his talk as a field-changing

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[274] Clark to Oakley Dec 11 1957, DF140/6 BMNH.
[275] Oakley to Clark Nov 21 1957, DF140/6 BMNH.
[276] Clark to Robinson Dec 11 1957, Box 305A, TM.

announcement of the discovery of a new and distinct form of tool-making hominid in South Africa, without any mention of disagreements within the scientific community. This pushed Clark to let loose a flurry of pent-up frustration in a letter to Oakley:

> The whole history of these S.A. discoveries has been bedeviled by the unbalanced claims made by the local workers – even Broom himself confused the situation by his uncontrolled multiplication of genera & species etc. What a pity! I rather think it may be time to say that if it had not been for you & me – who have championed the obviously right interpretations & have tried to present the evidence in a systematic & judicious fashion – probably even now the Australopithecines would have fallen into entire disrepute. Perhaps the best thing now is to cut oneself off from the whole subject of those fossils – we have done all we can, & we want to avoid getting entangled in personal disputes.[277]

Whether or not Clark's estimation of his and Oakley's role versus that of the South African workers was fair, it is clear that he felt they had mismanaged their own credibility, and insofar as he had tied his fortunes to theirs, had risked his credibility as well. The radical suggestion that he and Oakley disengage completely was a suggestion that they save themselves from a sinking ship before it took them under with it. As it turned out, Robinson told Clark that what he had said at the scientific meeting had been misrepresented in the press.[278] Clark seems to have accepted that explanation and opted to continue in his role as Robinson's most trusted international correspondent. As we will see, Robinson's main American correspondent, Sherwood Washburn, was much less adept than Clark at managing Robinson's anxieties, and their relationship suffered accordingly.

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[277] Clark to Oakley Feb 11 1958, DF140/6 BMNH.
[278] Robinson to Clark Feb 11 1958, Box 305A, TM.

Washburn and Robinson: Antagonizing the Locals

After travelling to Britain in 1951, where he met Clark for the first time, Robinson continued on to the United States, where he met Sherwood Washburn and other scientists interested in the discoveries being made in South Africa. Their relationship began well: Washburn wrote to Robinson upon his return to South Africa to tell him that his "visit in the USA was 100% successful as far as we are concerned" and that he had had "[n]othing but good reports on all sides from those who met you and heard your paper."[279] Additionally, he offered to make Robinson a member of the American Association of Physical Anthropologists and encouraged him to submit articles on the South African material to the organization's journal. Over the next few years, Robinson had several pieces published in the *AJPA*, including his papers on *Telanthropus* and the taxonomy of the Australopithecines.

In 1954, Washburn took over the editorship of the *AJPA*, and told Robinson of his desire to publish a series of synthetic articles bringing together all the available Australopithecine materials, with Robinson covering the anatomical side of things. However, when Robinson sent in a manuscript, it turned out to be something quite different from what Washburn had wanted from him. The paper was a response to certain criticisms that had been made of Robinson's taxonomic scheme for the Australopithecines, specifically his inclusion within that group of the Indonesian form *P.paleojavanicus*. The discoverer of the fossils on which that species was based, G.H.R Von Koenigswold, had originally created the separate, non-Australpoithecine

[279] Washburn to Robinson Oct 2 1951, Box 305A, TM.

genus *Meganthropus* for the Indonesian creature and had since criticized Robinson's attempted revision. To Washburn, this seemed a rather trivial point that hardly merited an extended article. Additionally, like Clark, he saw in Robinson's work a tendency to render disagreements into personal disputes – a strategy that he felt would hinder progress:

> I feel…that this is not one of your best efforts and that you might reconsider it. I feel the essential difficulty comes from trying to criticize specific statements by others and restating your own position at the same time…It is most unfortunate that the discussion of the Australopithecines is degenerating into a set of personal controversies. But I honestly feel that progress will come from keeping things as impersonal as possible.[280]

He assured Robinson that the article was accepted for publication, though he hoped Robinson would cut it down substantially. Perhaps thinking that he had not made clear what it was that he wanted for his journal, Washburn asked Robinson to step back and consider the needs of the profession more widely:

> It is my feeling that you people [in South Africa] are so close to the actual materials that you don't quite realize how much the profession needs an elementary illustrated statement of what the specimens are actually like. For the purposes of teaching and making people realize what the australopithecines are like, we need a different kind of publication. The literature on classification and relations of fossils is piling up far faster than the basic descriptions of the specimens themselves. The sort of thing I have in mind is a kind of round-up of the material, numerous pictures of the best specimens, followed by articles on dating, associated fauna etc.[281]

Disputes over the classification of isolated specimens were of little help to those who did not have the material context of that dispute immediately before them.

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[280] Washburn to Robinson Feb 11 1955, Box 305A, TM.
[281] Washburn to Robinson Mar 16 1955, Box 305A, TM.

Robinson responded defensively, ignoring Washburn's plea for a more general, synthetic sort of paper. "Yours and the referee's comments have puzzled and disturbed me not a little," he told Washburn. He had gone through the paper and could not find anything "scathing or derogatory" therein. Washburn was wrong to characterize the criticism of specific claims made by other scientists as in any way personal, as it seemed to Robinson "the most honest way of dealing with the situation – to demonstrate the errors of fact inherent in the basis for the point of view which is being opposed to the one I had put forward." Further, Washburn's criticism of the paper's length was a question of national styles of scientific writing, and Robinson had no intention of adopting what he characterized as American brevity: "I know that the American tendency in scientific writing is for highly condensed papers – but I don't care for that style as it leaves a lot unsaid which often gives rise to misunderstandings on the part of the reader." He grudgingly agreed to revise the paper "specifically to avoid the impression of fighting" with his critics, but warned that if Washburn was still not satisfied he would rather just scrap the paper.[282]

The paper was published in the *AJPA,* but Washburn was not finished trying to communicate his point of view to Robinson.[283] After all, at stake was no less than international access to the fossil material at the center of current paleoanthropological interest. Robinson was wrong to say that the criticism of others' claims was the best way to address disagreement; rather, the best medicine would be a full description of all available material, which had so far not been forthcoming: "There is a wealth of

[282] Robinson to Washburn Mar 17 1955, Box 305A, TM.
[283] Robinson, "Further Remarks on the Relationship between 'Meganthropus' and the Australopithecines."

material available which has not been fully published as yet and the controversies over the autralopiths [sic.] will be resolved by the publication of this material, rather than by the controversies which are going on at present." As the editor of a major journal, he had to consider the economics of how best to move the field forward: "Journal pages are very expensive and we are struggling to publish everything we can. A full account of the anatomy is so much more important than controversies that we would all like to see the former maximized and the latter minimized." To show that Robinson was not being singled out, Washburn gave the example of Clark's recent book, which, he told Robinson, had "marred by the fact that so much of it is directed against Zuckerman." As we saw, Clark had only mentioned Zuckerman by name once, but to anyone familiar with recent controversies in the field, it was obvious that he was pushing back against Zuckerman's criticisms. Clark's book, Washburn counseled Robinson, would have been "far better if he had stated what he had to say and not let the controversy he had with Zuckerman change the perspective of a large part of the book." Concluding with a little flattery, Washburn emphasized how much he and others relied on Robinson for access to information about the material, making it all the more important that he remain aloof from counterproductive controversy: "We look to you as the person who knows the African material best, and certainly want factual articles and your statement on classification. But one does hope that the emphasis will be on getting the basic descriptive facts into the literature. The controversies can, and probably will, go on forever."[284]

[284] Washburn to Robinson Apr 15 1955, Box 305A, TM.

In 1956, Robinson travelled to Chicago, where Washburn worked, indicating that the relations between the two had not degenerated so much as to prevent face to face interaction. Washburn seemed to feel that the visit had been a success – "We certainly enjoyed having you here in Chicago," he told Robinson, "and hope that something like that may be worked out again in the future."[285] However, from Robinson's point of view it had further revealed the gulf separating him and his overseas colleague. Complaining to Clark about the reception of his ideas on *Telanthropus*, Robinson wrote that Washburn had humiliated him during his Chicago visit: "in front of a class of students, Sherry told me that my point of view was 'just silly and solved no problems'."[286] Further, around the same time, Robinson submitted another paper to the *AJPA* that provoked a similar reaction from Washburn as the last one. This paper – already mentioned in the last chapter – dealt with the development of crests on the skulls of apes and fossil hominids, and was written largely in response to publication on the subject by Zuckerman. Once again, Washburn indicated that papers motivated by disagreements on specific points were of limited value, and once again Robinson refused to accept the validity of Washburn's criticisms.[287]

In an attempt to be conciliatory, Washburn tried to characterize the present impasse impersonally, as "the sort of thing where authors and editors never agree." He assured Robinson that the issue of cresting was important and worthy of publication "provided reference is not made all the time to Zuckerman." Further, as of the beginning of 1958 Washburn's tenure as editor of the *AJPA* had ended, and he

[285] Washburn to Robinson Nov 5 1957, Box 305A, TM
[286] Robinson to Clark Nov 27 1957, Box 305A, TM.
[287] Robinson to Washburn Jan 6 1958, Box 305A, TM.

suggested that the new editor "might feel differently about the article." Washburn had, however, just become the editor of a planned series of monographs for the Wenner-Gren foundation, and he once again sought to persuade Robinson to write a general text: "What I have in mind is a technical monograph reviewing the finds with lots of illustrations, and aimed at the teacher of anthropology or student who had had one or two courses."[288]

Far from interpreting the offer of publishing a book as a conciliatory gesture or signal of confidence from Washburn, Robinson took it as a misguided attempt to prolong what was obviously a dysfunctional scientific relationship. What was the point, Robinson wondered, of him sending a manuscript when Washburn had not been impressed with the papers he had previously submitted to the *AJPA* and had called Robinson's views "silly" in front of students. Further, Robinson wrote Washburn, "I have never heard you approve of anything I have written." Robinson insisted that he was not listing these perceived wrongs in order to hold them against Washburn. Rather, he wanted to emphasize what he saw as the fruitlessness in pursuing their relationship as author and editor: "[Since] all the evidence goes to show that you have in the past disapproved of both my attitude and my writing it seems to me totally unrealistic to expect that you would approve of any book I might write for the Wenner-Gren series." Robinson laid the blame for the aborted endeavor squarely at Washburn's feet: "I had long wanted to do a book for the university teacher and student and when you asked me, in Chicago, for such a book it gave impetus to my

[288] Washburn to Robinson Jan 21 1958, Box 305A, TM.

wish to write it. I have in fact already completed four chapters. But I see no future in sending it to you, on completion, and have you request me to re-do it differently."[289]

Washburn took Robinson's letter as evidence of a grudge, Robinson's assurances to the contrary notwithstanding: "Apparently you have been working up considerable resentment against me," he wrote back, "and think that I do not think highly of your work." He begged Robinson to consider that he had in the past helped him arrange visits to the U.S., invited him to contribute to the series he was editing, and offered to help him secure research funds – not actions to be expected from one who did not think highly of Robinson's work: "Now people do not just try to get funds for others, unless they think highly of them," wrote Washburn, "And most of them do not do it then." Instead of thinking about their differences in terms of personal animosity, Washburn suggested that the conflict might be an artifact of the different national traditions in academia: "Going from the US to South Africa, even Europe, one is struck with the much greater demand for conformity. One either agrees, or does not appreciate. Here we take it for granted that there is no agreement on the subject of fossil man and that that is to be expected." He was perfectly ready to publish things with which he did not agree, he told Robinson, but he was not willing to burden the *AJPA* with a Clark-Zuckerman style controversy, which he felt very strongly "did no one any good." On the substantive points on which they disagreed, for example the validity of *Telanthropus*, Washburn counseled that only time would tell who was

[289] Robinson to Washburn Jan 28 1958, Box 305A, TM.

right, but the current disagreement was irrelevant to his desire to publish Robinson's work and help him in any way that he could.[290]

To Robinson, in turn, Washburn's interpretation of his letter as evidence of resentment only confirmed his assessment of the differences between them. He had "tried to write a dispassionate letter" giving his reasons for not thinking that any good would come from his sending a manuscript to Washburn. "The letter was not written in anger," he wrote, "nor did I feel that it gave that impression." Nonetheless, Washburn had "drawn exactly the conclusion [he] wished to avoid." To Robinson, this sort of miscommunication was of the same sort that had led Washburn to interpret his several of his submissions to the *AJPA* as unduly polemical. In the case of the cresting paper, he told Washburn that he had expressly written it to avoid the impression of fighting with Zuckerman, going so far as to ask several colleagues whether they thought he had succeeded in this (they did). Still, Washburn had criticized it for encouraging further controversy with Zuckerman. Robinson agreed that national differences might be at work here, but rather than different degrees of tolerance of difference, he believed it was largely a matter of personality and language-use. While in the U.S., it had struck Robinson "that talkative people who are full of bounce and wise-cracks are much appreciated." In contrast, he was "silent unless spoken to and much [preferred] to keep in the background," an attitude which seemed "to render [him] liable to be misunderstood much of the time." As regarded *Telanthropus*, Robinson wrote that he bore no ill will towards Washburn for

[290] Washburn to Robinson Feb 8 1958, Box 305A, TM.

disagreeing with him. He had no desire to force others to conform to his point of view, but only wished to know people's reasons for disagreeing with him, and this required frankness of expression. By way of example, he told Washburn of the lengthy and rewarding correspondence he maintained with Clark over their disagreements about *Telanthropus*, something that would not had been possible if he "resented people disagreeing with [his] views." So, he concluded, there was no question of resentment in his decision not to contribute a book manuscript to Washburn's series, only a perception of irreconcilable differences. If Washburn genuinely thought Robinson was wrong in this assessment, he would reconsider.[291]

Robinson's contrasting of his relationship with Clark to that with Washburn is instructive. To him, Clark was a correspondent who, while disagreeing on some points, respected Robinson's point of view and recognized his priority as the scientist with the most intimate knowledge of the fossil material. In contrast, Robinson saw Washburn as overly demanding and high-handed. Clark and Washburn had the same interest in keeping up correspondence with Robinson: he was the gatekeeper to the fossil material that fueled the scientific work in which they all had a stake. Both, in a way, also shared a view of Robinson as an obstacle standing between the scientific field and the fossil material that fed it. Washburn's strategy was to discourage Robinson from interpolating himself between the fossils and the field, encouraging him to act instead as a disseminator of descriptive facts free from polemical entanglements. Clark, too, wanted 'the facts' and worried about their distortion at the

[291] Robinson to Washburn Feb 19 1958, Box 305A, TM.

hands of the local workers, but unlike Washburn, his strategy was to carefully cultivate a trusting relationship with Robinson through which he could maintain access to new information and exert some degree of restraining influence on Robinson's pronouncements. Washburn thought the facts could manage themselves; Clark saw that you had to manage people in order to manage the facts.

Chapter 6 – *Australopithecus* as Dart's Ambition, Part III

In March of 1947, the 81 year old Sir Arthur Keith, having poured over Broom and Schepers's monograph on the Australopithecinae, said the following in a short note to *Nature*:

> When Prof. Raymond Dart… announced in *Nature* the discovery of a juvenile *Australopithecus* and claimed for it a human kinship, I was one of those who took the point of view that when the adult form was discovered it would prove to be near akin to the living African anthropoids – the chimpanzee and the gorilla. Like Prof. Le Gros Clark, I am now convinced, on the evidence submitted by Dr. Robert Broom, that Prof. Dart was right and I was wrong; the Australopithecinae are in or near the line that culminated in the human form.[292]

With that, the man who had led the opposition to Dart's claim in the early years of the debate removed the source of the acrimonious relations which had characterized that period. His only remaining complaint was the onerously long name with which the creature's had been saddled, but he turned a criticism into a compliment with the suggestion that the Australopithecines be colloquially known as "Dartians." The relevance of Keith's retraction for the field might best be understood as symbolic, since he was retired and no longer at the center of discussions in the science of human evolution. Even when he produced a new book in 1948 called *A New Theory of Human Evolution,* in which the "Dartians" took up their newly important role, little stir was felt in the field.[293] The subject had been taken up by new hands.

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[292] Keith, "Australopithecinae or Dartians," p.377.
[293] Keith, *A New Theory of Human Evolution*.

For Dart personally, Keith's *mea culpa* must have been eminently meaningful as a justification for the grief he had suffered in his bid to achieve recognition for the human affinities of the Australopithecines. He had, in a sense, "won" the long battle for the credibility of his original claim. One can imagine that Dart might have made this achievement a capstone of his anthropological career, spending the rest of his scientific days reaping the rewards of his recognition as "the one who got it right" about the phylogenetic position of the Australopithecines. However, he was not content to rest on his laurels. Even as Keith was formulating his retraction, Dart was wading back into scientific discussions of the Australopithecines and into controversy.

Dart had begun a series of studies in which he argued that the Australopithecines were not just near-human in certain aspects of their morphology, but that they were also hunters, tool-makers, and fire-users. If the former had now become a relatively non-controversial position to take, Dart's new claims put him back in the position of having to defend the credibility of his propositions to skeptical colleagues. This time, he had the added benefit of having been "right" once already, but some other workers felt that he was endangering that hard-won victory by piling on top of it new controversial claims. To Dart, the new claims were logical extensions of the claim of the Australopithecines' human affiliation. As Dart would point out repeatedly, he had suggested from the very beginning that Australopithecus has possessed such human behaviors, but the controversy over the morphological issues had prevented the argument from being developed. His victory so far was thus only partial, and he intended to make it complete.

Makapan

In 1925, not long after the discovery of the Taung skull, Dart was sent some pieces of bone breccia that had been blasted from a lime mine in the Makapansgat Valley, northeast of Johannesburg. Dart noticed that many of the bones looked as if they had been burnt, leading him to suspect the presence of early humans – though not Australopithecines. Dart submitted samples of the bones to two chemists in order for them to test his hypothesis, and when they confirmed the presence of a significant amount of free carbon, Dart published a short note characterizing Makapan as a site of early human occupation.[294] In the 1930's, the archaeologist C. Van Riet Lowe investigated a cave high up the valley wall that had been exposed by mining where he discovered stone tools made by Stone Age inhabitants of the valley, leading to its designation as a historic monument. Further work was delayed by the war, but in 1945, a group of Dart's students, led by P.V. Tobias, visited the site. Investigating the limeworks, which were about a mile lower down the valley from the caves containing the Stone Age tools, they failed to find any stone tools but did return with more chunks of bone breccia like the samples that had been sent to Dart in 1925.

Dart would later single out the contents of the samples brought back by his students from Makapansgat as the motive force behind his re-entry into the Australopithecine discussions. As he related to the audience at the First Pan-African Congress on Prehistory in 1947 – the same meeting at which Clark first announced his support of Dart and Broom's claims – he was surprised to find amongst the jumble of

[294] Dart, "A Note of Makapansgat, a Site of Early Human Occupation."

bones the extinct baboon species *Parapapio broomi*. His surprise came from the fact that this creature had previously been found at Sterkfontein, where Broom had shown it to be part of the fauna associated temporally and spatially with Australopithecine remains. The presence of this baboon, along with subsequent geological investigations to assess the relative ages of the limeworks and the Stone Age caves, led Dart to tell the audience that at least part of the limeworks breccia was contemporaneous with the Australopithecines. The inferences cascaded from there. Consequently, Dart now argued that the presence of free carbon that had been detected two decades earlier must have been the result of fires made by Australopithecines with "promethean" habits, even though none of their own remains had been found at the site. As a second line of evidence for the presence of fire, Dart also cited some recently collected samples of a brown glassy substance from the breccia, which he claimed to be the result of fire burning in the presence of lime, phosphates, and ash. Besides being fire-users, Dart claimed that these Australopithecines must have been great hunters, as evidenced by the number and variety of bones left in the cavern. Dart argued that these hunters, not yet having developed stone tools, had used weapons derived from the bones and horns of the animals they killed: "The Makapansgat valley limeworks fire-middens," he told the audience in a flurry of extravagant prose, "indicate that the South Africa man-apes were hunters of large game in terrifying possession of Heraclean club, Samsonian jawbone and Mowglian firebrand."[295]

[295] Dart, "Faunal and Climatic Fluctuations in Makapansgat Valley."

While Dart's predictions might have seemed reckless, they were soon vindicated. The work that resulted in the that vindication was made possible in part by funds provided by Dr. Bernard Price, who after the war had endowed the new Institute for Palaeontological Research at the University of the Witwatersrand. After being shown the baboon skulls from Makapansgat and hearing from Dart that Australopithecine material might follow, Price agreed to fund work at in the valley, both in the Stone Age caves and at the more ancient limeworks. In September of 1947, a member of the newly-funded excavation team, the paleontologist James Kitching, came across an Australopithecine occiput (the rear part of the skull) while searching the mine's debris dumps.[296] In describing the fossil as a new species within the genus *Australopithecus*, Dart cited a number of anatomical and geological differences from previous specimens, but he emphasized that his main reasons for separating this form from the others were the behavioral inferences he had drawn from the associated evidence of advanced hunting techniques and fire-use. He named the new species *A. prometheus*, associating its supposed technological advance over its near kin to that of the mythical Greek who stole fire from the gods (though Dart did not draw any parallels with the divine retribution suffered by Prometheus as a result of his actions).[297]

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[296] Dart describes events leading up to the discovery in Dart, *Adventures with the Missing Link*, pp.97-100.

[297] Dart, "The Makapansgat Proto-Human *Australopithecus prometheus*"; "A Promethean *Australopithecus* from Makapansgat Valley"; These publications were followed shortly by the announcement of the discovery of a mandible, ascribed by Dart to the same species: "The Adolescent Mandible of *Australopithecus prometheus*."

Dart's next move was to develop the evidentiary base of his contention that the creatures hunted with weapons, specifically clubs made from the bones of their kills, in two similar articles published simultaneously in American and South African journals.[298] He reminded readers that he had not advanced this claim only with the discovery of the Makapansgat bone breccia, but had in fact maintained it since 1926, when he had interpreted the faunal remains associated with the Taung skull as victims of implement-assisted Australopithecine hunting.[299] He had already claimed, and Broom and Schepers has concurred in their monograph, that many of the baboon skulls associated with Australopithecine remains showed depressed fractures of a sort consistent with a violent, localized blow to the head. [300] Baboon skulls had been found at Taung, Sterkfonetein and Makapansgat, and Dart's new study brought this material together to assess the validity of the hypothesis that *Australopithecus* was a club wielding baboon hunter. For added credibility, he had consulted a number of experts, chief among them Professor R.H. Mackintosh, head of the Department of Forensic Medicine, who, Dart imparted, had "a life-long experience with, and has made a special study of, cranial injuries inflicted by lethal implements." The results were overwhelmingly in favor of Dart's hypothesis, showing that "out of more than 50 baboons from these three sites approximately 80% exhibit evidence of having been subjected to purposeful violence, which could only have been inflicted by implements

[298] Dart, "The Predatory Implemental Technique of Australopithecus"; "The Bone-Bludgeon Hunting Technique of Australopithecus."
[299] The claim was made in Dart, "Taungs and its Significance"; see Ch.2.
[300] In Dart, "The Dentition of *Australopithecus africanus*."

held in hand or by the crushing hands themselves."[301] A number of skulls and endocranial casts belonging to Australopithecines themselves displayed similar sorts of depressed fractures, suggesting to Dart that clubs had also been employed in intraspecific violence. The recurrence of such similar injuries at the three sites had led Dart to search at Makapansgat for objects that might have been used to inflict the blows. Not having discovered any stone tools, Dart advanced the claim that the long bones of ungulates had been used as clubs. A number of ungulate humeri showed evidence of pre-fossilization damage to one end, possibly the result of being repeatedly struck against hard objects. "The evidence indicates," wrote Dart, "that the characteristic implement of *Australopithecus* was a mace or bludgeon of bone, preferably the humerus of an ungulate."[302] A minority of the injuries to the baboon skulls seemed to indicate puncturing rather than bludgeoning, and Dart suggested that the sharp end of long bones broken to extract marrow might have served as thrusting weapons.[303]

Before the forties drew to a close, several more Australopithecine specimens from Makapansgat were announced. One was a cranio-facial fragment which included the right maxilla with several teeth attached, as well as most of the nasal aperture and part of the right orbit.[304] Another comprised two fragments of the pelvis, which, along with other pelvic bones already found by Broom at other sites, added to the evidence of Australopithecine bipedalism. For Dart, such evidence of the creatures' posture

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[301] Dart, "Predatory Implemental Technique," p.5.
[302] Ibid., p.12.
[303] Ibid., p.14.
[304] Dart, "The Cranio-Facial Fragment of *Australopithecus prometheus*."

also served as an important underpinning of their ability to wield, throw, and strike with objects held in the hand – motions requiring a mode of bodily torsion made possible by an upright stance.[305] Several isolated teeth and a second mandible were also found, and the excavation teams continued to remove faunal remains to be evaluated as possible evidence of hunting and tool-use.

Polemic by Hyena

Dart had thus far been allowed to advance his contentions about the fire-use and hunting technique of the Australopithecines without open criticism. That came to an end in the early fifties, when Kenneth Oakley, after his tour of South African sites described in the last chapter, publicly suggested alternative explanations that might account for the apparent carbon deposits and the accumulation of broken bones at Makapansgat.

Evidence from correspondence between Dart and Oakley before the latter's visit to South Africa suggests that Oakley did not set out with a prejudice against Dart's claims. Indeed, as a curator responsible for setting up displays at the British Museum, he asked Dart in 1949 if specimens showing evidence of these claims might be sent to London for exhibition:

> The evidence which you have obtained suggesting the use of fire and weapons by Australopithecines is of outstanding interest to students of the subject over here. We should like to be able to exhibit a specimen of one of the bones showing signs of bruising, and also a specimen of the marly deposit in which you have found indications of calcinations. Would it be possible for you to send us such samples, either on loan or

[305] Dart, "Innominate Fragments of *Australopithecus prometheus*."

> as an official donation? Anything which you can do to help us in this connection would be much appreciated here.[306]

Dart was happy to cooperate, and specimens were duly sent. Similarly, when Oakley came to South Africa in 1953, Dart seems to have been happy to aid him in his endeavors by allowing him access to his sites and specimens.

However, the hospitality and collegiality with which Dart received Oakley did not translate into the latter supporting Dart's claims about the habits of the Makapansgat Australopithecines. After his return to England, Oakley proposed several alternative explanations for the brown glassy material that Dart had attributed to deliberately set fires acting on the minerals in the cave. Initially, Oakley favored the explanation that this material was in fact due to the action of fire in the cave, though not any fire set by Australopithecines. It seemed to him unlikely that the origin of fire-use had preceded the origin of tool-manufacture, and there was as yet no evidence that Australopithecines had manufactured tools (though he did not dismiss Dart's claim that they may have *used* tools). A more likely explanation, as he suggested to Dart in a letter before putting into print, was a natural conflagration:

> Such evidence of fire as there may be in the Limeworks deposits at Makapan could I think be accounted for by natural grass fires spreading to the edge of the cave, and igniting bat guano, which is very inflammable on account of the insect wings it contains. I notice that Chiroptera are recorded from the Limeworks deposits.[307]

Oakley had in fact developed this hypothesis not from direct analysis of the glassy substance from the Limeworks breccia, but from the analysis of a layer of "cindery

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[306] Oakley to Dart Feb 21 1949, DF 140/6, BMNH.
[307] Oakley to Dart Sep 10 1953, DF 140/6, BMNH.

ash" from one of the much more recent Stone Age caves further up the valley. That layer contained no pieces of charcoal or burnt bone as might be expected if it had resulted from fires used for warmth or cooking by humans. Chemical analysis revealed the layer to contain significant amounts of phosphate, such as could be expected in burnt bat guano. If the guano-covered floor of a Stone Age cave had been set ablaze naturally, Oakley reasoned, one had to consider the possibility that this might have occurred in earlier times as well: "If the brown glass-like fragments found… in the *Australopithecus* deposit at the Makapansgat Limeworks were in fact produced by the fusion of sandy cave-earth, one has to allow the possibility that it was brought about by the burning of bat guano ignited at the cave entrance by a natural grass fire."[308]

This explanation later turned out to be moot, as analysis revealed the brown glassy material to be a calcium-phosphate based mineral whose formation had nothing to do with burning. However, Oakley still had to reckon with Dart's earlier evidence of apparently charred bone fragment and the analysis of those samples that had revealed free carbon. He revealed his skepticism of the sufficiency of this evidence to justify Dart's inference of the creature's 'promethean' habits while participating in an international anthropological symposium shortly after his 1953 South Africa trip: "As far as I can see," he told the other participants, "one bit of microscopic smut has been used to create the specific name of *Australopithecus Prometheus*."[309] In 1955, back in Africa for another meeting of the Pan-African Congress on Prehistory (at which Dart

[308] Oakley, "Evidence of Fire in South African Cave Deposits," p.261-62.
[309] Oakley, "Culture and the Australopithecines," p.29.

was also present), Oakley further diminished the evidential power of that "smut" when he revealed that his attempts to replicate the earlier findings by analyzing samples of the breccia had "failed to confirm the presence of free carbon," and instead "the blackness of the bone fragments in all the specimens which he collected proved to be due to oxides of iron and manganese." These results raised the question of how the chemists to whom Dart had originally submitted samples could have found free carbon to be present. As the site had once been an active mine where explosives had been used to dislodge the limestone, Oakley suggested that the carbon they detected might have been "introduced by a blasting charge." Whatever the explanation, he could not lend his support to the Dart's promethean hypothesis: "Until the free carbon can be detected in samples of the Australopithecine breccias collected under test conditions it would seem best to regard the case *Australopithecus* being a fire-user as non-proven."[310]

There remained the other parts of Dart's characterization of Australopithecine habits, namely the use of bones as weapons and the accumulation of a huge amount and variety of prey in caves (especially Makapansgat), including baboons that seemed to have had their skulls smashed in. On the one hand, Oakley thought that *some* of the evidence for baboon hunting was convincing, especially the skulls with depressed fractures that had been recovered at Taung.[311] That particular site, in his opinion, might well have been an Australopithecine living site. However, he doubted whether, in general, Australopithecines had frequented, let alone lived in, caves. This judgment

[310] Oakley, "Earliest Use of Fire," p.385.
[311] Oakley, "Culture and the Australopithecines," p.29.

was related to his skepticism about the creature's use of fire, for, as he wrote in 1954, "[there] are indications that caves were rarely if ever frequented by hominids before they had use of fire." But if Australopithecines did not frequent caves, then how to explain the discovery of Australopithecine fossils inside several caves? Oakley had an explanation: "more probably the deposits were formed in the dens of carnivores which preyed upon this hominid."[312] If this were the case, then the actions of a carnivore could also account for the accumulation of bones inside caves like Makapansgat. In fact, shortly after Dart had come out with his new claims, a South African scientist had suggested that hyenas, and not Australopithecines, might have been responsible for accumulating the bones and breaking them to access the edible material inside.[313]

The "hyena theory" thus represented a direct challenge to Dart's characterization of the Australopithecine as weapon-wielding hunters, since it suggested that both the supposed weapons and the fruits of the hunt were the mere leavings of scavenging carnivores. Dart decided to meet the challenge head on. Or rather, at least to begin with, his chief assistant undertook to test the hypothesis that hyenas accumulated bones in caves. Alan Hughes had been Dart's assistant at the Medical School of the University of the Witwatersrand for a number of years. He had also been among those responsible for the initiation of work at the Makapansgat Limeworks and had been in charge of scientific operations there since the late forties. Like Dart, though with less public exposure, Hughes had become invested in the claim of the Australopithecines' implemental hunting technique. In order to find out

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[312] Oakley, "Dating of the Australopithecinae of Africa," pp.17-18.
[313] Jeffreys, "Did Australopithecius Wield Weapons?"

whether the habits of hyenas matched the claims made about them by critics, Hughes set out a two-part investigation. First, both in the press and by private correspondence, he sought the advice of individuals in Africa who "might, from their vast experience with wild game, be able to give an authoritative opinion" on the habits of hyenas. Second, he excavated a lair recently occupied by modern hyenas to see whether or not there was evidence of bone accumulation. In the resulting paper, published in 1954 in the *AJPA*, Hughes argued that his investigations had decisively refuted the hyena theory. Of the seven respondents to his pleas for information cited in the paper, only one believed that hyenas accumulated in bones and other animal detritus in the caves they inhabited. All the others, among whom were park rangers, wildlife photographers and artists, game hunters, and even the Warden of Kruger National Park, denied that hyenas accumulated bones in their lairs. Among the refuting facts cited by one or more of these correspondents were that hyenas tended to chew up bones into small fragments and swallow them rather than breaking them and leaving them aside; that hyenas would be unlikely to sleep or rear their young in a lair littered with detritus; that hyenas generally ate what they found on the spot rather than dragging it away; and that hyenas frequented caves only when they had their young. For the second part of the investigation, Hughes and his team excavated a known hyena lair on a farm abutting Kruger National Park. The lair had originally been a network of tunnels excavated by ant-bears, which had subsequently been taken over by hyenas. Evidence of their recent presence came in the form of urine, hairs, and a distinctive odor that locals could readily identify as characteristic of hyenas.

However, upon excavating the lair Hughes and his team found no bones at all. Searching the area around the lair, they found only a few bone fragments and one instance of hyena droppings – nothing atypical of any stretch of bush in the area. Further, they discovered the carcass of a recently killed wildebeest nearby with its skeleton largely present and articulated – not what one might expect if the local hyenas were in the habit of carrying bones away to their lair. All in all, according to Hughes's report, the hyena theory had little if any scientific merit.[314]

How, then, had scientists gotten it into their minds that accumulations of bones in caves could be attributed to hyenas? Hughes attributed the origin of this belief to the nineteenth century British geologist William Buckland, whose book *Reliquiae Diluvianae* had had such a profound influence that since its publication in 1823 "anthropologists and paleontologists [had] accepted unquestioningly [Buckland's] theory that the agent chiefly responsible for large accumulations of animal bones in caves and rock fissures is the hyena."[315] Hughes did not elaborate on this historical thesis, but Dart took it up in a follow-up article which he titled "The Myth of the Bone-Accumulating Hyena." As far as Dart was concerned, Hughes's investigation had shown that the hyena theory had no scientific basis – that is, the existence of the theory could not be explained by reference to any evidence in nature. Consequently, its origin had to be sought in the records of human history, the way that a folklorist might seek the origin of any "myth." In Dart's scenario, those who believed the hyena theory were the "folk," adopting a story passed down to them without regard to the

[314] Hughes, "Hyenas versus Australopithecines as Agents of Bone Accumulation."
[315] Ibid., p.467.

facts, while he played the scientist disabusing the naïve of their belief by revealing its mythical origins.

Dart began by going into more detail about Buckland's role. He recognized that the idea of the bone-accumulating hyena had not first come into the scientific literature with the publication of *Reliquiae Diluvianae*, but had in fact appeared a year earlier in Buckland's report on the contents of the recently excavated Kirkdale cave in Yorkshire. According to Dart, the myth almost immediately became enshrined as truth primarily because of the authority of the author and the organ in which the paper was published: "The myth became an article of faith probably because this apparently unimpeachable document was published in the *Philosophical Transactions of the Royal Society* in 1822 and the distinguished author was awarded forthwith the Copley Medal."[316] The authority with which the hyena myth had been instilled at its scientific inception then carried it down through the decades without anyone feeling the need to give it further examination.[317]

Those who continued to accept the hyena theory, Dart counseled, would do well to recall that Buckland's purpose in his cave investigations was to uphold the Mosaic tradition in geology. He had two explanations for the presence of the bones of ancient animals in caves: either they were the result of the actions of hyenas, as in

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[316] Dart, "The Myth of the Bone-Accumulating Hyena," p.42.

[317] Martin Rudwick has written extensively about Buckland's work on Kirkdale cave. For Rudwick, Buckland's analysis of the cave's fossils was a significant event in the history of the earth sciences because it represented an early and influential attempt by a scientific investigator to reconstruct, using fossil and geological evidence, a scene from a prehistoric world that was qualitatively different from the present world and separated from it by some catastrophic event, such as a massive flood. See Rudwick, "Encounters with Adam, or at Least the Hyenas," ch.7; *World's before Adam: The Reconstruction of Geohistory in the Age of Reform*, Ch.6.

Kirkdale cave, or, in other cases, the bones had been washed into caves as a result of a universal Flood. The correct alternative from Dart's point of view "would have been for Buckland to accept the truth… that *accumulations of broken bones in caves are due mostly to prediluvial man* – i.e., to attribute to man a pre-Noachian antiquity. This, [Buckland] and contemporary science were determined to resist." And resist they did, Dart continued, when in 1825 the provincial clergyman John MacEnery discovered flint tools alongside the bones of extinct animals in Kent's Hole Cavern in the southwest of England. The resistance of Buckland and others to this "proof" delayed recognition of MacEnery's feat until after mid-century. Since that time, geology had "outgrown the basic flood myth," but, to Dart's dismay, paleontology had "preserved the incidental hyena-collecting myth that Buckland propagated."[318]

While Buckland had been responsible for the fixation of the hyena myth in the scientific literature, the myth itself, according to Dart, had far more remote origins. Dart found that Buckland had quoted from a sixteenth century narrative by the Flemish author Ogier Ghislain de Busbecq, who had served in Constantinople as the ambassador of the Holy Roman Emperor to the court of Suleiman the Magnificent. Busbecq described the Turkish mode of burying the dead under cairns in order to protect the bodies from hyenas, which, Busbecq reported, would otherwise drag the bodies to their lairs where they accumulated all sorts of bones.[319] Dart suggested that stories such as that reported in Busbecq's narrative likely had their origins in the taboos of ancient Near-Eastern agricultural people, for whom hyenas, dogs and the

[318] Dart, "Bone-Accumulating Hyena," p.50; for the role of Kent's Hole Cavern in the 19th Century debate about human antiquity, see Bowdoin Van Riper, *Men Among the Mammoths*.
[319] Ibid., p.47.

like were "unclean" animals. The transition from a nomadic, hunting-based economy to a settled, agricultural one, Dart argued, had led to the demonization of carnivorous habits, including those of the hunting peoples who continued to exist alongside those who had switched to agriculture. In Dart's reconstruction of the moralistic fables of ancient agricultural society, hyenas came to serve as symbols of farming peoples' despised hunting brethren, and the bone-accumulating habits of hunting peoples thus came to be associated with hyenas. In developing these stories, agricultural people had forgotten that their own ancestors, reliant on the hunt, had in fact been responsible for the sorts of bone-accumulations that they now attributed to the demonic habits of hyenas. In florid language, Dart summed up his quasi-psychoanalytic account the origin of the hyena myth:

> The hyena tradition is explicable on the basis that socialized agricultural man needed such stories in the process if dissociating himself from hunting and cannibalism and of developing customs of human burial instead… For man to desist from such flesh-easting practices it was necessary for him not merely to become separated from them but to learn to regulate and even despise them and to preserve a state of revolt against those who practiced them. In that process of separation it was serviceable for communal humanity to find suitable scapegoats – to create symbols of wickedness out of the unalterable Carnivora. For such evil prototypes nothing was more suitable than to select the disgusting, skulking creatures with habits far more revolting that any brave, sociable human being could ever imagine his own to have been and to call them abominable, cowardly, filthy, carrion-loving, necrophilic, body-defiling, bone-accumulating, demoniacally-laughing swine- in brief, **hyenas!**[320]

This process of historical memory-loss had not been eased by the advent of modern scientific prehistory, and the bone-accumulating hyena had "skulked" its way into

[320] Ibid., pp.52-53.

Buckland's work and that of those who followed him. Dart was now casting himself as the analyst, revealing the origins of the myth and thereby curing science (not to mention all civilized humanity) of the collective neurosis that allowed the myth of the bone-accumulating hyena to persist. In doing so, he hoped that other scientists would accept that humanity had always been the perpetrators of bone-accumulations in caves, and that human beings had inherited the habit from their ancestors, the Australopithecines.

The Osteodontokeratic and its Consequences

In the aftermath of the discovery of the Taung skull, Dart had tried, and failed, to publish a monograph setting out his views on *Australopithecus*. At that time, when the young professor still felt the sting of his "exile" from England and the company of his mentors, *Australopithecus* had seemed a powerful enough vehicle to return him to the professional path wanted to follow. For this purpose, it had seemed appropriate to seek publication under the prestigious imprint of the Royal Society of London, but that organization had rebuffed him, and the monograph never appeared.

By the mid-fifties, armed with loads of material from the Makapansgat limeworks and a theory of Australopithecine tool-culture, Dart was again ready to publish a monograph. This time, however, he took a different approach – one that demonstrated the development of his own professional identity and relationships. *The Osteodontokeratic* (meaning literally bone, tooth, and horn) *Culture of Australopithecus prometheus*, was published in early 1957 in South Africa as part of

the Transvaal Museum's Memoir series, the same imprint under which Broom had published his 1946 monograph. Dart did not submit the manuscript through an intermediary, as he had with Elliot Smith in the case of his earlier, abortive attempt, and there were no overseas editors to demand that he alter the style or reduce the length. If there were going to be critics, they would have to wait to receive the published volume.

Dart began the monograph with a preface to address what he felt were some misconceptions about those early years of his dealings with *Australopithecus*. He readily admitted that the paucity of his writings on the subject between the early articles following the initial discovery and his reentry into the fray in the late forties was due primarily to the fact that his claims were "received with such incredulity by so many physical anthropologists in influential positions." However, he found that others – including several authors of recently published popular science books, from which he quoted – had interpreted his silence as the consequence of his resentment of critics, a charge he denied: "This seeming void of interest in *Australopithecus* has resulted in a popular but unfounded legend that, wounded or wroth, Achilles-like I retired into some parochial tent to brood upon the unresponsive attitudes of my overseas colleagues."[321] The truth of the matter, according to Dart's account, was far more mundane. He reminded readers that in 1926 he has assumed "pioneering administrative duties…as Dean of the rapidly expanding Medical school in the

[321] From Moore, *Man, Time and Fossils*, p.264: "Dart himself was hurt by the treatment he has received and felt discredited. He did not attempt to follow up his work with further explorations."; from Wendt, *I Looked for Adam*, p.495: "The other gentlemen greatly enjoyed themselves at his (Dart's) expense and that of his 'Baby'. Their jokes went on so long that Dart gave up his investigations in disgust."

University of the Witwatersrand and its representative on the South African Medical Council," tasks which took up the majority of his time and energy. In other words, it would have been remiss on his part to pursue a narrow debate on the significance of one skull amidst all his responsibilities. The thorough description of the Taung skull in Arthur Keith's 1931 *New Discoveries Relating to the Antiquity of Man*, though made in the context of an argument running counter to Dart's own, had made the publication of a devoted technical monograph superfluous. War brought additional duties, so much so that "by 1944 [Dart] was sent on long leave to recover from the strain." According to Dart, there never any question of resentment, and the record of his other achievements ought to be enough to show that his professional life was more than just *Australopithecus*.[322]

Dart wrote that he might never have returned to the study of *Australopithecus* had it not been for of the fortuitous discoveries by his students at the Makapansgat limeworks just after the War.[323] But return he had, and the result was this monograph, the purpose of which was to demonstrate that the Australopithecines of Makapansgat had employed a variety tools made from animal remains to enhance their prodigious hunting ability.

Dart's arguments about the habits of the Australopithecines and the uses to which they had put bones, teeth, and antlers were based in part on numbers. He and his assistants had counted up all of the bone fragments that had been recovered from the Limeworks, 7159 in all, and classified those which could be identified (4607) by

[322] Dart, *The Osteodontokeratic Culture of Australopithecus,* pp.vii-viii.
[323] Ibid, p.vii.

the animal from which they had come as well as by type of bone. The relative frequency of certain types of bone from certain types of animals suggested to Dart that the Australopithecines had actively selected certain parts to take away from their kills, and that these parts had served specific purposes in their material culture. For example, of the bones that derived from animals other than antelopes, 85.1 percent were cranial remains, which indicated to Dart that the heads of these animals were of particular value to the hunters. As Dart colorfully put it, "*A. prometheus* was essentially a head hunter!"[324] Since a large majority of the bones from the Limeworks came from antelopes, and since these animals were well represented by post-cranial remains, Dart argued that venison had served as the primary meat for the Australopithecines. However, of the antelope cranial remains that were present, most were horns, mandibles, and maxillae, suggesting to him that these bones were valued as tools. Indeed, Dart inferred a specific (and usually violent) use for almost every bone or fragment that had been recovered from the Limeworks. He illustrated many of these in line drawings of disembodied (human) hands wielding the remains: a hyena mandible with the sharp canine still attached used for ripping and slashing (Fig. 6.1); an antelope femur used for bludgeoning (Fig.6.2); a broken antelope ulna used for stabbing (Fig.6.3); an antelope scapula used for splitting (Fig.6.4); a pair of gazelle horn-cores used for digging (Fig.6.5); a reedbuck palate with teeth attached used for scraping (Fig.6.6); a partial antelope jaw used for sawing (Fig.6.7). *A. Prometheus*, Dart argued, used these tools to kill and process animals from which it took its

[324] Ibid., p.84; A summary of the inferences Dart made about the use that the Australopithecines made of the various parts of animals can be found on pp.84-86.

nourishment, and, with forethought that suggested its human affinities, selected certain parts for use in the next hunt.

Dart supported his inferences by reference to the recent and even present day use of osteodontokeratic tools in certain "primitive" cultures in places such as Papua New Guinea and the Arctic fringe of North America. These tools and the objects found at Makapansgat were "separated by an immense, spatial and temporal gap," Dart conceded, and yet he insisted that they were "united by a common and fundamental, osteodontokeratic tradition."[325] This analogy (or perhaps homology) between the recent or extant use of animal body parts as tools, and the use of the same by Australopithecines, was vital to the culminating point of Dart's argument. The discovery of the osteodontokeratic culture of *A. prometheus*, Dart wrote, had doomed all attempts to erect definitional barriers between human beings and their immediate evolutionary forbearers. Dart dismissed Oakley's attempt to make a distinction between tool-using and tool- making, with the latter being unique to human beings – to the exclusion of the Australopithecines. He gave little reason for this dismissal, though it seems that to Dart, such an act as breaking a bone to make a sharp edge with which to cut up a carcass was tantamount to "making" a tool, thus foundering the distinction. More important, Dart wished to address a trend in anthropology that cited the use of *symbols* as the trait that set humans apart from all other animals. Dart cited a number of recent studies by linguists and anthropologists which related the emergence of social life, tool-use and language. Tools and communication were

[325] Ibid., p.19.

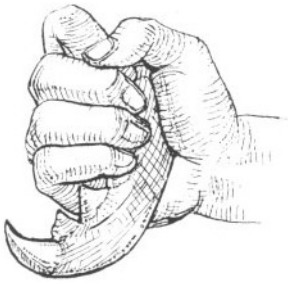

Fig.6.1

Fig.6.2

Fig.6.3

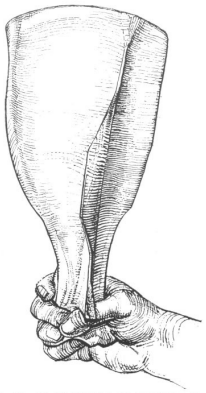

Fig.6.4

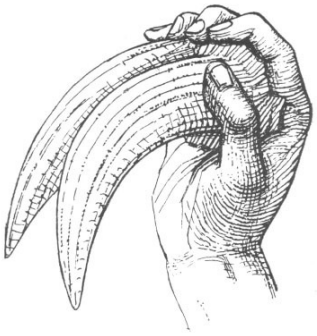

Fig.6.5

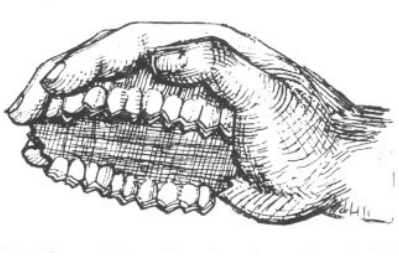

Fig.6.6

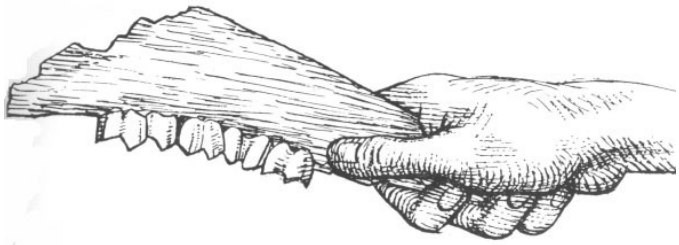

Fig.6.7

Figures 6.1 to 6.7 – Drawings of Dart's imagined uses for various osteodontokeratic specimens from Makapansgat[326]

[326] Ibid., pp.30, 52, 54, 61, 71, 73, 81 (page nos. for Figs. 6.1-6.7, in order).

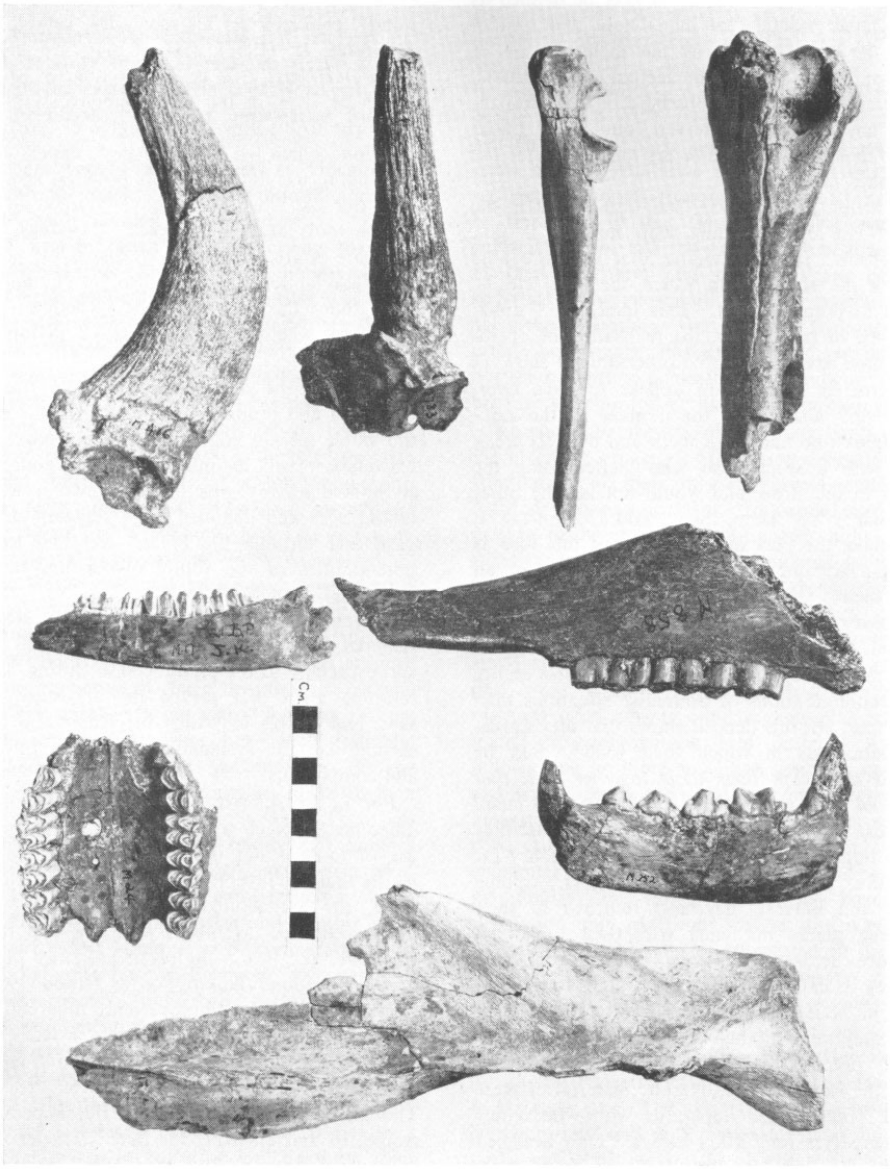

Figure 6.8 – A photograph of several of the actual specimens from the Makapansgat limeworks[327]

[327] Ibid., p.96.

vital the tasks of hunting and living together and, as such, had evolved in close tandem. Further studies had shown, Dart argued, that modern peoples the world over, both "primitive" and "civilized," were united by the prominent place that bodily parts played in their languages and systems of cultural symbolism. To Dart, the natural explanation for this cultural unity was that all living peoples had their common origin in a group of beings whose pioneering use of animal parts as tools formed the cultural bedrock on which the rest of human history had unfolded. For Dart, this was the capstone of his argument that Australopithecines had used tools made from bones, teeth, and antlers; let any reader who believed he had let his imagination run amok stand up against the evidence afforded by not only the fossils themselves, but the entirety of human culture:

> The uses to which these several types of osteodontokeratic tools have been applied did not spring unassisted from the writer's imagination! Such understanding as we have of their purpose has emerged step by step during the past thirty years through the process of striving to explain the facts, both specific and statistical, presented not by cogitation but by the broken skulls and bones found in these australopithecine breccias. The interpretations proposed have been discovered subsequently to be consistent with, and therefore corroborated by the uses to which these several types of osteodontokeratic tools were apparently put by primitive Asiatic and European mankind and are still being applied today; or have been applied within recent recorded memory by sapient peoples practicing both primitive and advanced lithic and even metallic cultures. The explanations given have finally found confirmation in the far-ranging symbolism that has become attached to bodily, and especially osteodontokeratic bodily parts in early human tradition, myth and culture, and is still found embodies in western European thought. The persistent place that bodily parts have occupied in human thinking is due to their role as tools.[328]

[328] Ibid., pp.99-100.

Dart had waited thirty years to publish a monograph on the Australopithecines. His first attempt had been censured for its verbosity and for the distance separating the fossil evidence and the inferences derived therefrom. Freed from the circumstances that had constrained him in those days, Dart chose not to heed the criticisms he had received. In characteristically grandiose style, he did not confine himself to writing about fossils that had been pulled from the ground – their measurements, their geological provenance, their anatomical classification – but instead painted a whole world-picture of the beings ostensibly responsible for those remains. And, just as characteristically, he challenged anyone to claim that he ever taken even one step beyond what was justified by the evidence.

The opportunity to meet that challenge was presented, soon after the appearance of the monograph, to W.E. Le Gros Clark. This "opportunity" was not one that Clark relished, but he felt that it was his duty to the field, as one who had championed the human affinities of the Australopithecines, to respond to Dart's latest claims. He had been wary for some time about the direction of Dart's work at Makapansgat and the effect it would have, and the new monograph confirmed his worst fears. "I have just received from Dart his monograph on the Osteodontokeratic Culture," Clark wrote to Oakley in March of 1957, "It is most depressing! What a lot of harm he has done, not only to himself, but to the proper appreciation of the fossil creatures which he was the first to discover!"[329] He may not have engaged the matter any further, but was forced to decide on a role for himself when the editors of *Nature*

[329] Clark to Oakley Mar 6 1957, DF 140/6, BMNH.

asked him to review Dart's monograph. He was torn by the request, and sought advice from Oakley, who, Clark felt, might have been the better choice of reviewer given his work on the place of tools in human evolution:

> I have been asked to review Dart's recent effort for 'Nature'. I don't feel it is very appropriate that I should do this. You are, of course, the obvious person. On the other hand you might find it rather embarrassing. If I do review it, I shall be quite outspoken, for I feel it is time to put a stop to this sort of wild speculation - & also to dissociate ourselves from it. But the question arises whether it is worth reviewing at all? I myself think it should be – if only to give some sense of proportion. Of course, one can so word a review to avoid too unkind a criticism – but it would in any case be bound to offend Raymond. But perhaps this is a situation which really does call for an honest & outspoken statement. What do you think?[330]

As we have seen, Oakley had become a trusted confidant to whom Clark felt comfortable venting his frustrations at the behavior of scientists who Clark felt threatened the credibility of his own "proper appreciation" of the Australopithecines either by contradiction or unrestrained speculation. The question now put to him was: which would be worse for the Australopithecine cause, allowing baseless claims about their habits to pass without comment, or risk offending their original discoverer and a friend? Oakley's answer was that there should be a response to Dart's book, but that it ought not to come from him:

> I am entirely with you in thinking that Dart's recent effort should be reviewed in an outspoken fashion. I am hesitant to undertake this myself, because I have for long been a thorn in Dart's side… It would I believe be unfortunate if the relations between the [British Museum] and Dart's school deteriorated. This may seem cowardly on my part, but I feel sure that he would take criticism better from you than from me.[331]

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[330] Clark to Oakley Apr 20 1957, DF 140/6, BMNH.
[331] Oakley to Clark Apr 23 1957, DF 140/6, BMNH.

Oakley's reasons for not wanting to undertake the review himself were thus twofold: first, since he had already publicly criticized some of Dart's recent work, his review was more likely to seem personally biased; second, he was an official of the British Museum of Natural History, and to risk being cut off from an important source of material for research and display would be irresponsible.

Clark took Oakley's advice and drafted the review. The matter was important enough that he again sought Oakley's guidance to make certain that he had struck the right tone: "Would you kindly look at this review of Dart's book for Nature & make any comments you think desirable? I hope I have not expressed myself too harshly – but I think that one's duty to science should come before questions of personal relationship."[332] Oakley responded that far from being to harsh, he thought Clark's review was "worded most generously."[333] However, to be seen as too generous in the matter, and hence biased due to the special relationship he had enjoyed with Dart, was equally of concern to Clark as being too harsh. After some difficult deliberation between his conflicting motives, he decided that he had achieved a reasonable balance and that the intended message would be received: "I have found this review extremely difficult – Raymond has always been so kind to me. If I <u>seem</u> to have been too generous to him, I suspect those 'in the know' can read between the lines [Clark's emphasis]."[334]

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[332] Clark to Oakley May 13 1957, DF 140/6, BMNH.
[333] Oakley to Clark May 14, 1957, DF 140/6, BMNH.
[334] Clark to Oakley May 16, 1957, DF 140/6, BMNH.

Clark's review appeared in the summer of 1957. He began not by directly addressing Dart's monograph, but by reminding readers of how far the Australopithecines had come in the last thirty years. The accumulation of skeletal material had, Clark wrote, served to dispel "doubts which had been expressed about their taxonomic position," and had led to "a general consensus" among scientists that the creatures had definite hominid affinities. However, while recognizing the hominid affinities of the Australopithecines was one thing, losing sight of the differences that separated them from their human descendants was quite another. He referenced Oakley's suggestion that "human" be a term reserved for tool-making hominids, and wrote that all evidence presented so far purporting to show that the Australopithecines had made tools had "either proved to be faulty, or, at the most, no more than suggestive." Here he introduced Dart's effort to show that such evidence was "much more than suggestive," but promptly rejected both his methods and conclusions. It was "one thing to show, as Dart endeavours to do, that these fragments of bones and jaws [from the Makapansgat Limeworks] could have been used as tools and weapons, and quite another to prove that they were used as such." "Without a doubt," Clark continued, "Dart goes far beyond the legitimate limits of his evidence in propounding these claims." This was all the more unfortunate because, in Clark's opinion, the material itself was important and in need of explanation. Further, Dart had made some reasonable inferences, but readers were bound to be prejudiced against these due to his "over-emphatic style of writing." As an example of the better aspects of the book, Clark cited Dart's investigation into the supposed bone-accumulating habits of hyenas.

Here was a claim backed up by evidence drawn from observations. Clark also allowed that the statistical analysis of the faunal remains was interesting in that it seemed to reveal trends in accumulation that might be phenomena worth explaining. However, it was not enough to attribute the remains to "deliberate and systematic manufacture" by Australopithecines "only because there does not seem to be any other ready explanation for them." Clark suggested that Dart's investigation might have been helped by comparison with sites of bone accumulation were there was no possibility of human agency to see if it differed from the Makapansgat deposit. In the absence of such comparisons, however, the suggestion that Australopithecines had developed a sophisticated bone culture was "surely premature."[335]

While Clark struggled in his review to find an appropriate balance between condemnation of Dart's claims and sensitivity to their friendship, he felt no need to contain himself in private correspondence with Oakley. At the same time as he was dealing with the review of Dart's book, Clark had been engaged, as we saw in Chapter 5, in an effort to temper the public pronouncements of a frustrated and volatile John Robinson. It was shortly after the publication of his review of Dart's book that press coverage of a talk by Robinson about the tool-making abilities of the Australopithecines caused Clark, in a letter to Oakley, to lash out at "the local workers" in South Africa and their "unbalanced claims." He contrasted the local workers to himself and Oakley who had "championed the obviously right interpretations" which had prevented the Australopithecines from falling into "entire

[335] Clark, "Hominids and 'Humans'," p.156.

disrepute."[336] Though the proximate cause of Clark's outburst was Robinson, it was surely also motivated by Dart's recent claims. These had added to the burden that Clark felt it was his duty to carry, a burden made of truth obscured by intemperance. However much Clark may have wanted to make the source of intemperance go away, he himself knew that he could do nothing without the risk of being cut off from the material source of the truth he wanted to preserve. Dart, and the other workers in South Africa, derived something that might best be described as "clout" from the geographical situation that made them indispensable to the process of knowledge-making in the field of paleoanthropology. When the Australopithecine material had consisted of only the Taung skull, comparatively little leverage had flowed from mere proximity. Now, late in the nineteen-fifties, as new material flowed consistently from the various Australopithecine sites, proximity meant power. No one was forced to believe Dart's claims now any more than they had been in 1925, but the ability to censure or marginalize him had been greatly reduced.

"How the Past Reveals the Future"

Despite Clark's repudiation of the osteodontokeratic culture, and his dire prediction of the effect it would have on Dart's credibility, Dart's professional standing was stronger than ever. In a sign of how far he had come since the difficult years following the discovery of the Taung skull, Dart was presented with the 1957 Viking Medal in physical anthropology – one of three medals presented annually by

[336] Clark to Oakley Feb 11 1958, DF 140/6, BMNH; also see Ch.5, pp.31-32.

the Wenner-Gren Foundation, which had become one of the premier granting agencies for all branches of anthropology. The selection was made on behalf of the Foundation by the American Association of Physical anthropologists, who cited Dart's defense of his interpretation of the Taung skull "against widespread skepticism" and his continued researches in the field which had "led the way for the intensive research in physical anthropology in South Africa today."[337] The "skeptics" in the Taung debate had criticized Dart for going beyond what the evidence warranted and being intemperate in defense of his claims – very similar accusations to those Clark now leveled at Dart for his work on the osteodontokeratic. Yet, with the power of hindsight, the Viking Medal celebrated Dart for doing just what the critics had denounced. With the change in opinion about the Australopithecines over thirty years, Dart's vices – intemperance, immoderacy, intransigence – had become his virtues: courage, perseverance, resolve.

The award of the Viking Medal had other benefits. Dart had recently been contacted by the managing editor of Harper & Brothers' publishing house about the possibility of a popular scientific book about the Australopithecines. As the Wenner-Gren Foundation provided return passage to New York for Dart and his wife, he was able to visit the editors in person with a draft manuscript. He reported in the acknowledgments of the resulting book that without the help from the foundation it would probably never have appeared.[338]

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[337] "Viking Awards," *American Anthropologist* 60:4, p.760.
[338] Dart, *Adventures with the Missing Link*, p.xvi.

Written with the help of Dennis Craig, a Johannesburg writer and journalist, *Adventures with the Missing Link* allowed Dart to consolidate in narrative form the turnaround in his scientific fortunes that had taken place over the last three and a half decades. In part an autobiography, the book devoted some space to Dart's early life and education in Australia and England. However, these details came only in the third chapter. The book opened instead with the discovery of the Taung skull, and thus represented this event as Dart's true "beginning." The second chapter took several steps backward in time, briefly recounting the history of western science's encounters with apes and debates about the evolutionary origins of human beings. The end of this potted, progressive history brought the story up to the point it had been left off at the end of the previous chapter, placing Dart and *Australopithecus* at the leading edge of a long line of illustrious figures and ideas. After a detour through Dart's early life, the narrative recounted the skeptical reception given to Dart's interpretation of the Taung skull, describing many of the same events and interactions described in first two chapters of the present study. Unlike the present study, however, Dart devoted few pages to the process that led from the initial skepticism of the scientific community to the context in which he now wrote. It was not that Dart wanted to deny the roles played by others during the years in which he was less active in the debate. Rather, it was that in this *personal* narrative, the fact that recognition of the Australopithecines' human affinities had gone from general skepticism to general acceptance was not something that required explanation. The "Raymond Dart" presented to readers of this book had never doubted that his original interpretation was correct, and, because this

was a narrative of his personal adventures with *Australopithecus*, the time it took others to recognize their error was of little consequence. This was not a story of how the nature of *Australopithecus* was negotiated amongst a network of scientists with varying ideas and interests; it was a story of how one scientist knew the nature of *Australopithecus* from the beginning and had never swayed from that knowledge. A promotional synopsis on the dust jacket of the book accurately captured the trope in which Dart and his co-writer presented the story, promising the reader "An exciting narrative of exploration, mystery, startling deduction and personal courage." Such a characterization was not necessarily a misrepresentation of historical fact. However, it did rely heavily on hindsight, for while in 1959 it could be plausibly argued to a sizeable audience that Dart's "startling deduction and personal courage" were the essential ingredients in the story of *Australopithecus*, one would have been hard pressed in 1931 or even 1941 to find more than a handful of souls who would believe that such virtue was in operation.

Adventures with the Missing Link was not, however, merely a story of Dart's triumph. Indeed, less than half the book dealt with the period up to the publication of Broom and Schepers' influential 1946 monograph. The rest was devoted to Dart's more recent, and still ongoing, researches and Makapansgat and his argument for the Australopithecines' employment of an osteodontokeratic tool industry. This part of the book was not so much a memoir as a continuation of the arguments Dart had been running over the past decade in the face of skepticism from such erstwhile allies as Clark and Oakley. As he had done in his monograph on the osteodontokeratic, Dart

freely exercised his artistic imagination – with the help of an artist named William Papas – to convey to readers his vision of Australopithecine life. This time, rather than disembodied hands wielding tools, the illustrations rendered the creatures in full, engaged in various activities that, for Dart, defined their mode of existence. With low, sloping foreheads but otherwise far more human than ape in appearance, *Australopithecus* was depicted dragging a recently clubbed baboon (Fig. 6.9), fighting one of its own kind with club and dagger (Fig. 6.10), skinning a warthog with bone and tooth implements (Fig. 6.11), and setting upon a doomed antelope in a collective, coordinated hunt (Fig. 6.12). Dart himself also served as a model of Australopithecine behavior for the benefit of his audience; a photograph shows him wielding the shoulder blade of an ox, demonstrating how *Australopithecus* might have used the cleaver like bone to split and crush a pig's skull (Fig. 6.13).

Dart titled his final chapter "How the Past Reveals the Future." The "past" he referred to was the deep, evolutionary past of human beings, and the revelations of this past included the disheartening fact that the aggression and bloodthirstiness that seemed to characterize the present state of the human species was firmly rooted in its prehistory. Far from being pessimistic, however, Dart counseled that by studying the roots of human nature as revealed by the fossilized traces left by distant ancestors, modern humans could "appreciate more fully with what dark forces externally and internally man has contended both individually and collectively from the time he first became conscious of the power of the weapon, in order to become and to stay as

Fig.6.9

Fig.6.10

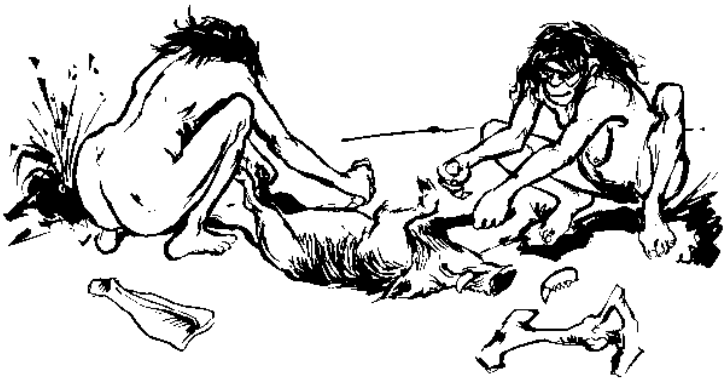

Fig. 6.11

Fig. 6.12

Figures 6.9 to 6.12 – Drawings of imagined scenes of Australopithecine life from Dart's 1959 memoir[339]

[339] Dart, *Adventures*, pp.109, 113, 147, 148 (page nos. for Figs.6.9-6.12, in order).

Figure 6.13 – Dart demonstrating the use of a scapula as a weapon[340]

civilized as we happen to be."[341]

There is another way in which Dart's book demonstrated how the past might reveal the future, though this was not explicitly stated. The autobiographical story he told was of a scientist whose radical claim was at first dismissed, but later recognized as the starting point for a new understanding of human ancestry. That same scientist had now made a series of new and equally radical claims, which again had provoked the censure of his colleagues. If there was a moral to the first part of the story, it might be that the controversies aroused by Dart's claims, far from being a discredit to him, were actually evidence that he saw farther than his skeptical colleagues. Such a sentiment was captured by the anthropologist Ashley Montagu, whose praise adorned

[340] Ibid. p.149.
[341] Ibid., p.238.

the dust-jacket of Dart's book: "Not everyone will agree with many of his conclusions," Montagu wrote, perhaps signaling his own skepticism; "The reader should, however, be warned that Professor Dart's conclusions have an uncanny way of turning out to be right in spite of all the experts' disagreement." According to this reading of history, the renewed controversy signaled that it was only a matter of time before Dart once again turned out to be right.

Conclusion

The publication of Raymond Dart's memoir and defense of the Osteodontokeratic culture was not the biggest event to befall the science of human evolution in 1959. That year, the field was shaken, as it had been many times before and would be many times after, by the discovery of a fossil skull. The discovery had been made by Mary and Louis Leakey at Olduvai Gorge in what was then Tanganyika (present-day Tanzania), the site that would become synonymous with the Leakey family name over the next few decades through their public exposure in the pages of *National Geographic*. Louis called the creature *Zinjanthropus boisei*, and, like Dart before him, claimed for its kind a privileged position in human ancestry. Further, in championing the evolutionary role of the newly discovered creature, he emphasized its differences from the known South African Australopithecines, relegating those to a lesser role occupying a side-branch of the human family tree. Unsurprisingly, the result was renewed controversy.[342]

This dissertation began with a quote from a 1958 lecture by W.E. Le Gros Clark in which he expressed anxiety over the persistence of controversy in paleoanthropology. The problem, Clark believed, was that the deeply personal subject of human origins tended to cause people – even scientists – to react emotionally to the evidence, clouding objectivity and hindering rational discussion. Yet for all his

[342] Leakey announced the find and gave a preliminary description and interpretation in both the scientific and popular presses: see Leakey, "A New Fossil Skull from Olduvai," and Leakey, "A 'Stupendous' Discovery"; for a detailed and comprehensive look at the Leakey family's role in the science of human evolution see Morrell, *Ancestral Passions* (especially Chs.12-13 for the discovery of *Zinjanthropus*).

concerns about the character of the field, Clark could not have denied that the study of human origins – especially as they concerned the place of the Australopithecines in human ancestry – had in recent decades moved in a direction that he, as a supporter of Dart's original claim, found largely positive. From a situation in late nineteen twenties in which the claim of humankind's Australopithecine ancestry had been accepted only by a marginal few and relegated to the sidelines of scientific discussions, the science of human evolution evolved to a point in 1959 where the possibility of an Australopithecine ancestry had become among the more important, if not *the* most central issue in the field. In fact, there was enough agreement on the hominid affinities of the Australopithecines for Clark to describe it as a consensus.

Given the obstacles to proper scientific judgment and conduct that Clark saw littered across the field of paleoanthropology, one might wish to ask him how it is that the field had experienced a broad trend in the direction of consensus on the issue of the Australopithecines. Indeed, given the frequency and intensity of the conflicts over the Australopithecines, one might even ask how it is that competing scientists managed to maintain *any* cohesiveness as a field, let alone move towards consensus about the importance of the Australopithecines for unraveling the mystery of human evolution.

To answer the second question first, we might recall the passage from Shapin's account of credibility in science, already quoted in the Introduction, in which he doubts the ability of a group of a community of specialized practitioners to long maintain their cohesiveness in the face of persistent distrust, given the need to share

findings.[343] Without a doubt, the participants in the Australopithecine debates frequently and persistently expressed distrust towards each other's claims, and sometimes also about each other's methods. Yet the fact that the field remained cohesive enough to change discernibly *as a field* suggests that mutual trust among the participants in the debate cannot have been entirely absent. Rather, I would suggest that it lay hidden in plain sight. Despite the distrust that separated the scientists over substantive claims and scientific methods, not one of them ever suggested that the motivating interests behind the others' involvement in the debate were suspect. Put another way, each trusted that the others made the claims they did, rightly or wrongly, because they were interested in the collective project of learning the origin of human beings.

The fact that the scientists involved in the Australopithecine debate shared the goal of learning the origin of human beings may seem too obvious to be accorded any explanatory power, but this common epistemic interest lies at the heart of how these individuals maintained cohesiveness as a field even as that field was mired in controversy. They could not pursue their interest as individuals only – it was not enough for each to become convinced that *his* claim about the place of the Australopithecines inside or outside of human ancestry was the most credible. Rather, success entailed persuading others with the same goal of the veracity of one's particular claim about human origins. The need to persuade others in order to fulfill the epistemic goal of learning the origin of human beings in turn necessitated a

[343] See Introduction, pp.9-10.

commitment to the maintenance of certain broad attributes of established scientific practice, without which the controversies would have had little if any meaning or importance to any of the actors, no matter which side they were on. Such attributes would have had to have been general enough to be necessary to the achievement of credibility for *all* parties. Chief among these, I would suggest, lay in a collective commitment to entrenched and accepted communicative practices in science. Despite their differences, all parties to the Australopithecine controversies remained committed to seeking credibility for their claims through the communicative practice of publishing in professional scientific journals – a commitment that allowed for the maintenance of a community of specialists even in the face of controversies over substantive scientific issues.

Raymond Dart and Arthur Keith may have been set apart by a sometimes acrimonious struggle over the interpretation of *Australopithecus* in the nineteen twenties, but they were united in their practice of making their cases by publishing their findings and arguments in journals read by other specialists. Dart did briefly circumvent this convention when he presented his interpretation as a *fait accompli* at the British Exhibition at Wembley – for which he was mightily criticized – but he also went to considerable lengths in his ultimately unsuccessful effort to publish his monograph through the Royal Society of London. Dart had a vision of success for his professional career as a scientist, and the realization that vision required a certain conformity to existing structures and processes even when those same did not work in his favor.

Robert Broom might seem a harder case, given his persistent rhetoric decrying the power structure in science, and his apparent pride in flouting norms and conventions. Yet, at a certain point one must try to see Broom's rhetoric as just that: rhetoric. For all his talk of not being interested in what "the experts" in high places thought, Broom consistently (and prolifically) published his findings and arguments in the professional journals that were sure to pass before the eyes of those same experts. His actions demonstrate that he was just as interested in defending the credibility of his positions through conventional media as any of the other specialists whom he sought to persuade.

Washburn's concern over the tone of Robinson's submissions to the *American Journal of Physical Anthropology*, with their tendency to render criticisms in personal terms, might also be explained in part by reluctance to compromise the medium that held the field together. If the pages of a professional journal became so mired in acrimony that scientists' commitment to publishing in them was weakened, what would be the consequences for the advancement of knowledge? So, too, with Clark's anxiety over whether there was any value in entering into public controversy with Zuckerman. He ultimately decided that he had to engage Zuckerman to defend his own credibility, but the use of journal pages for such an exercise clearly worried him, as it did Washburn. Publication was the medium through which individual scientists connected with the field as a whole. If that medium became compromised there would be no field at all, and the question of credibility would become meaningless.

Something as seemingly banal as scientists' commitment to communicating publically through professional journals may seem an odd issue to dwell on in conclusion, but it is just this sense of its everyday commonplaceness, which scientists today share with their counterparts from the earlier part of the twentieth century, that marks its importance as part of an unquestioned normative order with the power to bring together scientists in a shared field of inquiry even when the issues at hand are contentious.

A commitment to publication in professional journals was of course not particular to those interested in the science of human evolution. More particular to this field, along with the related field of paleontology, was a commitment to the centrality of fossils to their science. Again, it may seem too obvious to point out that paleoanthropologists and paleontologists are committed to the study of fossils. However, as with publication practices, this commitment helps to make sense of how the field retained any cohesiveness in the face of persistent controversies. The various actors in this narrative often ascribed very different meanings to the same fossil material, and these differences were the raw material of controversy. Yet the power of these objects to focus debate around themselves, no matter the character of that debate, should not be overlooked. At the same time as the interpretation of Australopithecine and other fossils set scientific workers apart in the intellectual realm, the excitement that new fossils generated and the sense that these objects should serve as the foundation of scientific knowledge of prehistory consistently brought them together to interact as a scientific field. This field proved a stable enough and fertile enough

ground for the Australopithecines to develop in their identities as objects of scientific scrutiny along a coherent path from 1924 to 1959 despite the swirling winds of controversy around them. That they would eventually be widely credited by scientific experts as human ancestors was never a foregone conclusion, but the fact that they remained consistently before the gaze of a group of specialized practitioners committed to collective inquiry ensured that the opportunity to take up the exalted role that Dart had originally claimed for them remained open.

The fact that the Australopithecines *did in fact* become more widely credited among specialists as human ancestors between 1924 and 1959, however, requires some additional explanation. The field might conceivably have survived decades of controversy intact without collective belief moving appreciably in the direction of Dart's original claim. What can account for the improved of credibility of that claim?

To Clark, writing in the late nineteen fifties as a proponent of the Australopithecines' ancestral status, the explanation lay in the removal of obstacles that had impeded scientists' recognition of the truth. The chief obstacles had been, first, insufficient fossil evidence to warrant the acceptance of Dart's claim, and second, a veil of emotions that had prevented scientists from accepting that claim when the first obstacle had been removed. The tendency of scientists to react emotionally to the evidence for human origins had not been entirely overcome by the end of the nineteen fifties, but the improved fortunes of Dart's claim was itself evidence, in Clark's view, that objectivity was winning out over emotion and making the truth of the matter visible to an increasing number of specialists. Clark's view of

his own role in this process was less as an advocate for Dart's claim than as a neutral conduit through which others, who were perhaps having trouble shedding their emotional attachment to the matter, could see clearly. Credibility then followed unproblematically, without the need for advocacy.

Clark's explanation for the improved fortunes of Dart's claim and his own self-presentation as a neutral conduit are ultimately unsatisfactory. Like all the other specialists engaged in the Australopithecine debates, he was an advocate for a particular view of those creatures' place in evolutionary history. And like the other specialists, his advocacy for a particular position staked his credibility as a scientific expert on matters of human evolution on the outcome of the debate. That he cared deeply about both the substance of the debate and about his own status as a scientist is made evident by the character of his correspondence. Apparently unaware of the irony, he scarcely tried to hide his own frustration and anger as he lamented the inability of Zuckerman, Dart, and Robinson to control their emotions. The fossil evidence never spoke for itself, and it was no less free of human emotion by having passed through his hands.

If a "neutral" presentation of the fossil evidence is not an adequate explanation for the improved fortunes of Dart's claim, we should not therefore conclude that the fossil evidence was of little consequence to the direction of the debate. This was the conclusion of several previous studies of the Australopithecine debates, discussed in the Introduction,[344] which emphasized the role of contemporaneous changes in

[344] pp.6-9, 12-13.

evolutionary theory – specifically, the advent of the "Neo-Darwinian Synthesis – to explain the increased acceptance of the Australopithecines as human ancestors. But such a conclusion seems at odds with a documentary record that shows that the participants in the debate constantly referred to the fossils when assessing the relationship of the Australopithecines to human beings.

The fact that the claim of an Australopithecine ancestry for human beings had become credible to a larger number of scientists by the nineteen fifties than it had been several decades earlier does not seem amenable to any single, sweeping explanation. Rather than looking for a unitary causal factor, we are better off conceiving of an explanation that brings together many different kinds of factors and weaves them into a narrative that takes us from Dart's initial failure to persuade other specialists of his claim to a time when scientists on multiple continents could confidently advocate the ancestral status of the Australopithecines.

The fossil evidence certainly played a significant role, if not a singularly determinative one. For Arthur Keith, among others, seeing the cumulative whole of the available fossil evidence described for the first time between two covers in Broom and Robinson's 1946 monograph was a major turning point. For others, such as Gregory and Clark, the opportunity to examine the original specimens in person was a critical factor in cementing their alliance with Dart and Broom. The contact these scientists had enjoyed with the Australopithecine fossils also provided rhetorical ammunition that could be deployed against skeptics who had not themselves had the same opportunities.

Theoretical developments also had a role to play, if not to the exclusion of the fossils and other factors. Most of the scientists who had been associated with the debates over the Australopithecine fossils never concerned themselves much with the broad change in evolutionary theory associated with the Modern Synthesis, but the several of the most prominent theorists of the Synthesis did, in the nineteen fifties, concern themselves with the Australopithecines. When a theoretical biologist of such international stature as Ernst Mayr went so far as to propose the inclusion of the Australopithecines within the genus *Homo*, he lent his considerable personal credibility as a scientific authority to the claim of humankind's Australopithecine ancestry – a risk he would not have taken had he not believed that recent development in evolutionary theory bore on the question of human origins.

Theory and evidence are routinely recognized as integral parts of science, and their importance to the Australopithecine debate is in that sense unsurprising. However, the narrative has shown that these are not the only elements to have come into play in the debate. The dynamics of credibility were also affected by factors less commonly acknowledged as relevant to the development of science. Consider, for example, the geography of the debate. Raymond Dart worried about the effect on his scientific stature of working in South Africa, so far from the people and institutions that formed the core of scientific authority in his field. And, indeed, the initial divisions of the controversy largely fell between scientists in England, who would not accept Dart's claim, and those in South Africa, who generally did. In the rhetoric of some of the English critics, the geographical split was mirrored by a difference in

scientific temperament. The skepticism of the English was often accompanied by rebukes to their South African counterparts for such offences as insufficient caution and overly emphatic language, demonstrating that their assessment of the credibility of Dart's claim was based on more than a consideration of theory and evidence. The relevance of the divide between European scientific center and African periphery is further demonstrated by the fact that the reversal in the fortunes of Dart's claim had much to do with the advocacy of W.E. Le Gros Clark – an English professor. Even Clark, who worried about his own credibility after allying himself with the advocates of Dart's claim, perpetuated the image of the South African scientists as compromised by what he viewed as their emotional, controversy-perpetuating approach to the debate.

We may agree with Clark in the view that the individual personalities and styles of self-presentation of scientists did affect the development of the Australopithecine debates. Further, a perceived emotional attachment to the idea of humankind's Australopithecine ancestry did, as Clark worried it would, sometimes negatively impact the credibility that claim, in that many scientists saw such an attachment as antithetical to proper scientific judgment. However, if Clark genuinely believed that the Australopithecines were truly the ancestors of human beings, and that the role of science was to make that truth available, then must equally acknowledge that emotion was ultimately also indispensable to the eventual acceptance of that claim. For what if Robert Broom had not held so passionately (and unscientifically, according to critics) against the force of majority opinion that Dart had been right

from the beginning? Who, then, would have kept the Australopithecines in the pages of scientific journals when others had all but forgotten them? Who would have taken the time and effort to fervently search the caves dotting the high plains of central South Africa for further fossils with which to convince more emotionally "detached" scientists that they had been wrong?

These are counterfactual questions, but they nonetheless serve to show the relationship of emotion to the credibility of scientific claims cannot be easily reduced to a single formula, and that attempts by historical actors to do just this must be viewed themselves as part of ongoing negotiations over how credibility ought to be assessed. Paleoanthropologists today continue with this agonistic process of deciding how and when to trust each other's claims about our common evolutionary ancestry, and controversy remains a staid feature of the field. Yet, despite continued worries about the steady diet of controversy, knowledge of human origins continues to change. New fossils are discovered, new techniques and theoretical perspectives are brought to bear on existing evidence, and new people with new ideas enter the field. The image of our ancestry has been made more complex as a result, but the Australopithecines have retained their central place within it. At least for the time being, the controversy has moved on to other things.

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