The First Humans

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The First Humans - Origin and Early Evolution of the Genus *Homo*

Contributions from the Third Stony Brook
Human Evolution Symposium and Workshop
October 3 - October 7, 2006

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ISBN: 978-1-4020-9979-3 e-ISBN: 978-1-4020-9980-9
DOI 10.1007/978-1-4020-9980-9

Library of Congress Control Number: 2009927083

Cover illustration: Photographs of KNM-ER 3733 (*left*) and KNM-ER 1470 (*center*) by Robert Campbell (© and courtesy of Robert Campbell and the Kenya National Museum), photograph of SK 847 (*right*) by Frederick E. Grine (© and courtesy of Frederick E. Grine and the Transvaal Museum). Image design by Luci Betti-Nash.

Printed on acid-free paper

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Preface

There are some issues in human paleontology that seem to be timeless. Most deal with the origin and early evolution of our own genus – something about which we should care. Some of these issues pertain to taxonomy and systematics. How many species of *Homo* were there in the Pliocene and Pleistocene? How do we identify the earliest members the genus *Homo*? If there is more than one Plio-Pleistocene species, how do they relate to one another, and where and when did they evolve? Other issues relate to questions about body size, proportions and the functional adaptations of the locomotor skeleton. When did the human postcranial "Bauplan" evolve, and for what reasons? What behaviors (and what behavioral limitations) can be inferred from the postcranial bones that have been attributed to *Homo habilis* and *Homo erectus*?

Still other issues relate to growth, development and life history strategies, and the biological and archeological evidence for diet and behavior in early *Homo*. It is often argued that dietary change played an important role in the origin and early evolution of our genus, with stone tools opening up scavenging and hunting opportunities that would have added meat protein to the diet of *Homo*. Still other issues relate to the environmental and climatic context in which this genus evolved. Were there global or pan-African climatic events that relate to the appearance and/or extinction of *Homo* species, and if so, can they be tied to the appearance or disappearance of these species in any meaningful way? Did *Homo* species live in environments that differed from those inhabited by earlier hominins, and can any general trends through time be inferred from paleontological and isotopic evidence?

The announcement, over 4 decades ago, of the fossil remains of *Homo habilis* from Olduvai Gorge by Louis Leakey, Phillip Tobias, and John Napier marked a number of major changes in our knowledge and interpretation of human evolution. We have certainly come a long way since the early 1960s towards appreciating the origin of our genus. New finds and analyses have provided some critical information, and have, at the same time, led to new questions. Still, there remain a significant number of unresolved issues. This is the way it should be, and what one expects. Although some of the questions appear to be the same as before, new techniques and interpretations have opened up other avenues of enquiry and have led to new questions for which answers can hopefully be found.

In an effort to update, address and hopefully synthesize our current understanding of this preeminently significant development in human evolution, we organized the Third Stony Brook Human Evolution Symposium and Workshop in 2006. An international group of acknowledged experts in their respective fields assembled for 5 days of discussion and debate on a wide range of topics related to the origin of the genus *Homo*. This volume is the result of those activities. The chapters they have contributed to it represent what we know, and what knowledge we still wish for in the quest to understand the evolution of the first humans.

The workshop was sponsored by Stony Brook University and the Turkana Basin Institute. Many people and organizations made it possible, including the Office of the Provost of Stony Brook University, the Wenner-Gren Foundation, and generous contributions from David and Maureen Acker, Ed and Frances Barlow, Bill and Kathy Cleary, Charles and Ursula Massoud, Allan and Diana Rothstein, Jim and Marilyn Simons, and Kay Harrigan Woods. The success of

the symposium and workshop owes much to the tireless efforts of Elizabeth Wilson, Christopher Gilbert, Danielle Royer, Matthew Sisk and Ian Wallace.

The chapters in this volume underwent formal peer-review, and we would like to take this opportunity to thank our colleagues who gave so freely of their time and expert opinion to assist in this process. We also thank Eric Delson, senior co-editor of the Vertebrate Paleontology and Paleoanthropology Series, for his support, assistance and encouragement in bringing this volume to publication. Robert Foley, Marta Lahr and the other faculty, staff and students at the Leverhulme Center for Human Evolutionary Studies, The University of Cambridge, graciously provided Fred Grine with accommodation and support over the 2007–2008 academic year while he was on sabbatical, and during the height of his editorial work on this volume.

Stony Brook University

Frederick E. Grine
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Part I
Retrospectives and Theoretical Perspectives

Chapter 1
Early Humans: Of Whom Do We Speak?

Richard E. Leakey

Keywords *Homo habilis* • *Homo erectus* • *Homo rudolfensis* • Koobi Fora • Turkana Basin • Olduvai Gorge • archeology • Oldowan • history of discovery

I would like to begin by saying that the manuscript in this volume by my dear friend and mentor, Professor Phillip V. Tobias, is particularly enjoyable to me because it allows me to reflect, and remember some of the events that he describes. I think my father probably thought that he *had* successfully browbeaten Phillip to accept the designation of *Homo habilis* as a new species of our genus. It is, I am told, a trait that I have inherited from Louis: we think we have achieved something, or persuaded someone and proceed on that basis when, in fact, we have made nowhere near the point we were trying to make. So I think that Phillip and my father were *both* correct in their beliefs: Philip was not browbeaten, but Louis certainly would have tried, although he would have denied all along that he was trying! I particularly like Phillip's reference to a characteristic that Louis had (and which I am told I also have). He would pick something up and say, "Isn't that *obvious*?" to which people would respond "No!" This is exactly the response I get, and the response that my father got from Phillip. In this case, at least, Phillip has had the good grace to say that Louis *had* been right, and that it *had* been obvious – Phillip just hadn't noticed the obviousness. So, for the family history, let me just give my Dad a little support, which I was loathe to give when he was alive, because I now find myself more inclined to appreciate his qualities.

I totally agree with the validity of the taxon *Homo habilis,* and I think that Phillip Tobias, along with John Napier and Louis Leakey, was perfectly correct in launching that species (Leakey et al., 1964). The type specimen, OH 7, which Phillip has alluded to as "Jonny's child," and whose mandible and parietals formed the basis of the new designation, is *Homo habilis*. And, I think it is correctly a species of the genus *Homo* (cf. Wood and Collard, 1999; Collard and Wood, 2007; Wood, 2009). By the same token, however, I think some of the specimens from Olduvai that Phillip (Tobias, 1991) and others (Johanson et al., 1987) have included in *H. habilis* still have a question mark next to that allocation.

Whilst I was active at Lake Turkana in northern Kenya at Koobi Fora, we found a great many fossils and we published them fairly rapidly through an initial announcement in the journal *Nature* and subsequently with more detailed descriptions in the *American Journal of Physical Anthropology*. It was a policy in the early days that the group working on the early hominids – myself, Alan Walker, Bernard Wood and Michael Day – would not engage in the naming game. We intended to find additional material, and needed time to study specimens in detail. Subsequently, this reserved group dispersed slightly, and we all went on to do different things, and some of us started talking about species. Specimens such as KNM-ER 1470 that were first identified simply as representing the genus *Homo* in the Upper Burgi Member of the Koobi Fora Formation (Leakey, 1973) were shortly thereafter allocated to a novel species *Homo rudolfensis* (Aleexev, 1986). Others found it appropriate to provide taxonomic names for other specimens we had discovered and described but not named, including a mandible from the Okote Member of the Koobi Fora Formation (KNM-ER 992), which was attributed to a species called *H. ergaster* (Groves and Mazak, 1975). The relationship between *Homo habilis* and *Homo ergaster* is not clear to me and I am of the opinion that there is a conundrum which arises in part from the incompleteness of material upon which early work was done. No one would question the quality of the work that was being done in the 1960s, but I think one could argue about the quality of the material and its real geochronological age. I do feel that the mandible, the parietals and the hand and foot bones of OH 7 and OH 8, which formed the basis of the original study, and to which were later added OH 16 and OH 24 (Leakey and Leakey, 1964; Leakey et al., 1971) are specimens that are so fragmentary that we don't really know that much about the anatomy of the type of *Homo habilis*.

Additionally, I am concerned that, although the stratigraphy and the chronology between the specimens at Olduvai is

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F.E. Grine et al. (eds.), *The First Humans: Origin and Early Evolution of the Genus* Homo,
Vertebrate Paleobiology and Paleoanthropology, © Springer Science + Business Media B.V. 2009

superb, there clearly is a problem insofar as there are at least 150,000 and possibly 280,000 years of time separating "Jonny's Child," OH 7, the type specimen of *H. habilis*, and OH 13 (Walter et al., 1991; Tamrat et al., 1995). We know from hindsight that at the upper end of that span, at around 1.65 million years, there are specimens known both in Africa and beyond that are the same age as or somewhat older than OH 13 and which are clearly *not* what OH 7 seems to be (Antón, 2003; de Lumley et al., 2006; Lordkipanidze et al., 2006; Rightmire et al., 2006; Spoor et al., 2007). They are more refined, and you might call them *Homo erectus* or, as some call some of them, *Homo ergaster*. Had we known that *H. ergaster/erectus* made its appearance by 1.6 Ma, a fragmentary skull of that age from the middle of Bed II at Olduvai might have been assigned to that taxon rather than to *H. habilis*. We now have quite a complicated set of finds in well-documented contexts (Brown et al., 2006; Gathogo and Brown, 2006a, b; McDougall and Brown, 2006; Spoor et al., 2007). With the advantage of hindsight, we will need to revisit some of the taxonomic assignments.

I also feel – and I will come back to this for the purposes of this discussion – that the efforts made to determine the cranial capacity of *Homo habilis* were faultless at the time those estimations were made (Leakey et al., 1964; Tobias, 1964; Holloway, 1965). However, when an estimation of cranial capacity is being attempted only from the parietals there is room for error (Pilbeam and Simons, 1965; Wolpoff, 1981). With the benefit of hindsight, and knowing that large-headed individuals such as KNM-ER 1470 and KNM-ER 1590 were about at the time of *H. habilis* (i.e., OH 7), a different guesstimate or calculation is possible. Does the real *H. habilis* have a cranial capacity of less than 750 cc is still a valid question today.

I now return to my opening remarks: I think that there is probably no other issue more important to the general public than the evidence for the first appearance (as it is usually referred to) of our own genus. We are special, we are different, and we are concerned. We face a very complicated future – the world is riven by all sorts of fundamentalist and other concepts – and if we can really pin down this issue of who we are and where we are from once and for all, it would be to everyone's greater good.

For that reason, I do not think it trivial to revisit the earliest species of the genus *Homo*, because the first publication in 1964 was based on specimens from Olduvai that at that time did not have the benefit of comparison with many other specimens that have since been found. If we can step back slightly, I think this workshop will allow us to consider this conundrum: if the sequence of discovery had been different, would Louis Leakey, Phillip Tobias, and John Napier (Leakey et al., 1964) necessarily have come to the conclusions that they came to? This is not to say that they made a mistake. We know, however, that hindsight is often clearer than foresight. My task, in part, is to provoke a discussion around these issues.

I have always been and remain impressed with what obviously impressed Louis – and led him, I think, to make his early mistakes – and that is the relationship between the manufacture of artifacts out of stone, the anatomical changes that would be necessary to manipulate stone as a cutting instrument, and the brain that would be necessary to think about breaking a stone in a certain way in order to do that. It is clear that at the time *Homo habilis* was launched, the oldest (Oldowan of Leakey, 1936) artifacts were the same age as the oldest fossils attributed to the genus *Homo*. That, of course, was in the lowest sequence at Olduvai, which is dated to some 1.87–1.78 Ma (Walter et al., 1991; Tamrat et al., 1995; Blumenschine et al., 2003). Since then, and new discoveries are constantly being made, the threshold for evidence of making stone tools has been pushed back to about 2.5 or 2.6 million years, with four sites in the Hadar Formation that possess lithic artefacts (Harris, 1983; Semaw et al., 1997, 2003; Stout et al., 2005; Roche et al., 2009) and one in the Hata Member of the Bouri Formation that bears evidence of bones both cut and percussed by stones (de Heinzelin et al., 1999). Certainly by 2.4–2.3 Ma ago in Kenya and Ethiopia, there are a number of sites that reveal artifacts that are very clearly quite sophisticated in the way that they were being made (Kimbel et al., 1996; Hovers et al., 2002; de la Torre, 2004; Kibunjia et al., 1992; Kibunjia, 1994; Plummer, 2004; Roche et al., 1999, 2009).

So, somebody was around who was fashioning a cutting edge out of stone that was quite complicated. Were there several contemporaneous species making cutting-edged stone tools? Or do the cutting edges that are represented by the archeological collection represent a single species but populations of it operating with different raw materials and doing things in their own way?

When we talk about a species – and I think there is a real issue here – are we talking about more than one species of *Homo* between 1.8–1.6 Ma, of which only one was ancestral to *Homo ergaster/erectus*? Were there several species – and one or two disappeared for various reasons – but that all had the same general adaptation that had shifted to become much more omnivorous because they could access meat by cutting with a stone through flesh? These are important biological and adaptive questions that we need to understand in terms of ourselves.

I think a more plausible suggestion would be that the adaptation of a slightly more complex, if not larger, brain and the development of a slightly more manipulative ability (*Homo habilis*) may only have happened once. This then was expressed and manifested further in different parts of Africa in different ways, depending on different circumstances, rather than having it happen twice or thrice in one geographical area at the same time. That would be my simplistic view,

but the thing about this issue is that we do not know because we do not find only one hominid.

When my mother, Mary, discovered the first hominid at Olduvai, which was the skull of *Australopithecus (Zinjanthropus) boisei*, Louis was convinced that she had found the maker of the tools. The reason was that he had been looking for 30 years with Mary at Olduvai for just that. He knew there were tools, and he knew that those tools had been made by somebody, so when somebody was found, he assumed that it had made the tools. It was therefore a shock to him a year and a half later, in 1960, when my older brother Jonathan came up with something that clearly was not *Zinjanthropu*s because of obvious differences in morphology. Louis therefore had to do a slight change of pace and say, well, maybe this second one was a toolmaker and this second one ate the first one. And so we got into a slight complication. The same problem arises today: we find hominids, and we find tools and, depending on our points of view, it is very easy to say, "this one must have been the toolmaker," or that "these were *all* toolmakers." We have to figure out how they relate to one another even though the tools may be different. Are they very different? Even though we use computers, are we different from those people who are still using stone flakes today? What is that difference? We are all the same species today. I believe that species designation as it pertains to our understanding of the evolution of the genus *Homo* is obviously an important and sensitive issue that we need to think about.

I also believe that the relationship between tools and hominins – that is, the relationship between the first appearance of what we call *Homo* and the link between the development of tools and human behavior (perhaps in terms of campsites) – is very critical (Davidson and McGrew, 2005). Hindsight might require a modification of ordering. I know that workers who champion the existence of "*Homo*" *rudolfensis* and support a complex species composition in the Late Pliocene – Early Pleistocene (e.g., Wood, 1992, 1993; Grine et al., 1996, 2009; Grine, 2001), regardless of whether forms such as *H. habilis* and *H. rudolfensis* should actually be considered as members of our genus (Wood and Collard, 1999; Collard and Wood, 2007; Wood, 2009), will possibly have a different standpoint. But I think the purpose of this workshop is to put away our previous convictions and put away our prejudices and say, "let us go back to basics." We need to think not about the sequence of things in which they were *found*, but about the sequence in which these individuals *lived* and about the completeness of the fossils. I believe the archeological record is particularly critical. The evidence for the use of stone is long and complex, and I think will be a very important insight into what was going on in the Late Pliocene and Early Pleistocene. Too often we deal with archeology and with the fossils in isolation, and we fail to talk together about what they may mean in context of one another.

My view is that the *Homo habilis* story is indeed a very complex story. I think specimens have been included in *H. habilis* that may not be *H. habilis*, that may belong to a species or lineage that is later than *H. habilis*, and I think the sequential relationship between these fossils is therefore very critical. Unfortunately, although we have spent an awful lot of time, and an awful lot of money obtaining dates for many of these important archeological and paleontological sites, there are still question marks about the temporal relationship between some of the fossils. We thought we had got it all tidied up at Lake Turkana, but as more people look at things, new questions inevitably arise. It is not possible at this time to say what is the right story. There are a lot of question marks, and we must not assume that we know the answers.

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Chapter 2
Homo habilis – A Premature Discovery: Remembered by One of Its Founding Fathers, 42 Years Later

Phillip V. Tobias

Keywords *Homo habilis* • *Australopithecus* • Olduvai Gorge • archeology • history of discovery

Introduction

One of the most dramatic contributions to human evolutionary studies made by the Leakey family was their discovery from 1959 onwards of the hominin fossils to which Louis Leakey, Phillip Tobias and John Napier in 1964 gave the name *Homo habilis*. The first specimens were found in the lower part of Bed I of the superlative Olduvai Gorge sequence in Tanzania. The then new dating method, the potassium-argon technique, shocked everyone by revealing an age of 1.75 million years for Olduvai Bed I (Leakey et al., 1961). This high antiquity was one of the factors conducive to the shocked rejection of *Homo habilis* by almost all our peers.

The delicate parietal bones and modest teeth, which seemed to differ from those of South Africa's *Australopithecus africanus*, were thus of high antiquity. Moreover, they were contemporaneous with an excessively large-toothed australopithecine whose remains were found in the same Bed I (Leakey, 1959a, b, c; Tobias, 1959, 1967). The history of early *Homo* contemporary with robust australopithecines has been recounted elsewhere (Tobias, 1978). The Olduvai discoveries of 1959–1961 were not the first evidence of an apparent *Homo* contemporary and sympatric with an australopithecine to emerge from Africa's soil. A decade earlier, fossils showing every justification to be included in the genus *Homo* had been found in the Swartkrans cave, in South Africa. Swartkrans had already proved to be an immensely rich source of fossils of *Australopithecus (Paranthropus) robustus*. In September 1949, a jaw fragment, SK 45, came to light in Member 1 of the Swartkrans Formation. This and other Swartkrans specimens were originally named by Robert Broom and John Robinson *Telanthropus capensis*. However, in 1957, Simonetta proposed to re-designate it *Homo erectus*, and Robinson (1961) agreed with this. By 1959, when Olduvai began to yield early *Homo* fossils, the existence of a *Homo* contemporary and sympatric with large-toothed robust australopithecines had been well-established in the paleoanthropological record for 10 years. When the Olduvai hominins were being studied from 1961 to 1963, the prevailing view was that *H. erectus* was contemporary with the robust australopithecines.

The Leakeys at Olduvai

Louis and Mary Leakey had searched assiduously for early humans at Olduvai Gorge from 1931 onwards. By 1955, the only hominin skeletal remains available were Hans Reck's 1913 human skeleton (Olduvai Hominid 1), and two thick fragments of cranial vault (OH 2) which Mary Leakey had found at the site of MNK. I studied these two fragments at the Natural History Museum in London in June 1964. They were recorded as *cf. H. erectus* in Mary Leakey's 1971 book. In 1955, a "giant molar tooth" and a canine (OH 3) were found in Upper Bed II at site BK. Much excitement was aroused at the time by the gigantic dimensions of the molar, especially as it showed the hallmarks of a deciduous tooth. It was served up as an hors-d'oeuvre for the sumptuous repast provided by the type specimen of *Australopithecus boisei* 4 years later.

That was all. After 28 years of slog at Olduvai (1931–1958), it could not but seem a poor return. Admittedly the dogged perseverance of Louis and Mary Leakey had been rewarded by thousands of comparative faunal remains and hoards of stone artefacts, but the stone tool-maker, that Louis had believed in perfervidly and sought for decades, remained elusive.

That was the position in 1957 when I paid my first visit to Olduvai, having been invited by Louis Leakey to join a safari he had organized for Richard Foster Flint of Yale University. Apart from some diverting encounters with large felines, one

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F.E. Grine et al. (eds.), *The First Humans: Origin and Early Evolution of the Genus* Homo, Vertebrate Paleobiology and Paleoanthropology, © Springer Science + Business Media B.V. 2009

of which came close to terminating the excursion and Dr. Flint's active life, the only fruits of our visit were sundry mammalian fossils, and serious, animated and often heated questioning by Flint of Leakey's paleoclimatic interpretation of the sequence of events at Olduvai. During these skirmishes I often found myself, willy-nilly, acting as referee!

A turning point came in 1959.

1959 – *Annus Mirabilis* at Olduvai

At the end of June or the beginning of July 1959, the Leakeys' longtime senior assistant Heselon Mukiri found a molar tooth in a lump of calcified tuff at MK in Bed I. It proved to be socketed in a small fragment of mandible. Slight as this evidence was, the specimen, OH 4, along with a premolar, represented the first remnant of *H. habilis* to be recovered. Only some weeks later came Mary Leakey's spectacular discovery, on 17th July 1959, of the cranium which became the type specimen of *Australopithecus (Zinjanthropus) boisei*. I called it colloquially a "hyper-robust" australopithecine. Needless to say, this magnificent specimen ousted all other considerations. The shy little jaw and teeth of the first *Homo habilis* (OH 4) were put on one side, and published only in 1964 as one of the designated paratypes of *H. habilis*.

Immediately after the *A. boisei* cranium was found, they brought it down to Johannesburg from Nairobi, the "Dear Boy" sitting on Mary's lap throughout the flight. Thus, it came about that Dart and I were the first two people to see and touch the fine specimen. Incidentally, OH 5 was the first australopithecine to be found away from South Africa, save for Kohl-Larsen's maxilla with two premolars from Eyassi not far from Olduvai in Tanzania. The Eyassi remains had been found shortly before the outbreak of the Second World War, and the news did not become generally available until after the war.

Shortly after the Leakeys' visit to Johannesburg in 1959, we flew to Kinshasa (then called Léopoldville), to participate in the IV Pan-African Congress on Prehistory. There Louis Leakey publicly announced the discovery of what he proposed to call *Zinjanthropus boisei*. In my monograph on this hyper-robust cranium, I proposed to sink the genus *Zinjanthropus* in *Australopithecus*, relegating the former to sub-generic status. Thus its revised nomen became *Australopithecus (Zinjanthropus) boisei*.

A personally momentous incident at the Kinshasa Congress was that Louis and Mary invited me to undertake the definitive study of the "Zinj" cranium. Thereafter all of the hominin fossils that Louis and Mary recovered at Olduvai and elsewhere were handed to me to analyse and describe. It was a major turning point in my life. It came at a juncture when I was inclining to devote my career to the study of the living peoples of Africa. That thrust had grown out of my researches on Bushmen or San in Botswana, and the Tonga people of Zambia. Thus, it came about that the Leakeys rather than my mentor Raymond Dart ushered me into a life among the fossils. It must be admitted that my earlier work on human genetics and physical anthropology of the living flavoured my subsequent interpretations of the fossil hominins.

Another career-fashioning event of 1959 occurred on the home front: at the beginning of the year I succeeded Dart as the Professor and Head of the Department of Anatomy, placing me as the youngest professor at Wits University in charge of a large and active department. It was a position I was to hold for 32 years.

By late 1961 the amount of East African skull material, teeth and postcranial bones had become voluminous. I suggested to Louis Leakey that John Napier of the Anatomy Department at the Royal Free Hospital Medical School in London, and a specialist in the functional morphology of the upper limb and of locomotion, be invited to study the postcranial bones. I would handle the crania, including jaws and teeth, and artificial endocranial casts. Napier accepted and, in turn, involved his colleagues, Peter Davis and Michael Day.

1960 – Jonny's Child

1960 was the first year in which the Leakeys' Olduvai project received major financial aid from the National Geographic Society. Excavation went ahead on an unprecedented scale. A few teeth and calvarial fragments (OH 6), and a tibia and fibula (OH 35) came to light. For a while these disparate remains were left in a suspense account. Louis Leakey was busy claiming that Nutcracker Man (a name I had lightheartedly suggested for "Zinj" at the Kinshasa Conference) was the long-sought Olduvai tool-maker. His view seemed reasonable in the absence of any more advanced hominin at the same time-level. But *was* there no more advanced contemporary? For the moment it seems that *Telanthropus* of Swartkrans was forgotten or overlooked, and so were the puny scraps of OH 4 and OH 6.

At this critical juncture, Jonathan Leakey, the eldest son of Louis and Mary, made a very important discovery on or close to his 20th birthday (4 November, 1960). At FLK NN, in the lower part of middle Bed I, he dug out a juvenile mandible, the greater part of an immature left parietal bone, fragments of a matching right parietal, some other cranial pieces, and 21 wrist-, hand- and finger-bones. Urgently I was called from Johannesburg to examine these new and perplexing specimens designated OH 7. The parietal bones were outsized. I brought with me measurements of the parietal bones

of the South African australopithecines. It immediately emerged that the parietals from FLK NN were greater in their measurable dimensions (except thickness), than any known parietals of crania of *A. africanus* and *A. robustus* from South Africa and of *A. boisei* from Olduvai. I was aware, from my earlier studies, how variable were the contributions of the three main vault-bones, frontal, parietals and occipital, to the size of the brain-case as a whole. Nevertheless, one could not escape a tingling feeling that a pair of parietals that were not only extensive from front to back, but laterally widespread, must have covered an endocranial cavity larger than those of any early hominids then available. Just how much larger was the first vexing question. Moreover, even if I could form an estimate of the endocranial capacity of OH 7, this fact on its own would not, of course, exclude OH 7 from having been a large-brained australopithecine.

For a start, I made an arch between the left and right parietals: there were sufficient landmarks to make this possible. The first arch was made on Monday, 5th June 1961, at the Centre for Prehistory and Palaeontology behind the Kenya National Museums. I was helped by Louis and Mary Leakey, Shirley Coryndon (later Shirley Savage), and the distinguished American palaeontologist, George Gaylord Simpson, on a visit from Harvard University, while his wife Anne Roe wrote down the measurements as I took them. We were all conscious of an air of ill-suppressed excitement as I manoeuvred the restored left and right parietals in relation to each other. If I moved the lower margins of the two bones closer together, a strange angulation became apparent along the mid-line (sagittal margins). On the other hand, if I distracted the two lower (squamosal) margins from each other, a negatively-angled hollow appeared along the line of the sagittal suture, flanked by an elevation to each side. Both extreme positions of the mid-line hinging gave us a bizarre and deformed-looking partial calvaria. Somewhere between these two extremes the correct alignment of the two parietals must have lain. Eventually we narrowed down the likely options to a very small range of widths between the lower margins (biasterionic widths) which appeared anatomically reasonable. We were awed by the size of the resulting biparietal arch, substantially greater than the corresponding measurements of the australopithecine parietal bones or biparietal arches.

I brought the specimens to my laboratory in the Wits Anatomy Department. There, Alun Hughes and I made another reconstruction of the arch, slightly different from the Nairobi arch. The difference in biasterionic breadth between the two reconstructions was scarcely 2.0 mm. Next, Alun and I made two partial endocasts that fitted snugly within the two biparietal arches. The mean volumes of these partial endocasts were almost identical (363.6 and 363.4 cm^3). At that time, the smallest total endocranial capacity recorded for an *A. africanus* cranium was 428 cm^3.

I was now confronted with the task of converting the capacity of the part-endocast of OH 7 to an estimated total capacity. Using a series of hominid analogues, I determined a capacity of 680 cm^3 (uncorrected for age – OH 7 was a juvenile). My latest revised estimate for OH 7 is 647 cm^3 (uncorrected for age) and 674 cm^3 (as an estimated adult capacity). Jonny's Child turned out to have an endocranial capacity nearly 50% greater than the average for half-a-dozen specimens of *A. africanus* from South Africa. Indeed, its value was well above the observed range and 95% population limits of the australopithecine capacities. This was the major morphological surprise posed by Jonny's Child who lived 1.75 Ma ago. But, it was not the only unusual feature.

The teeth of the type jaw of OH 7 impressed those of us who had studied the teeth of australopithecines in their relative slenderness. This buccolingual (BL) narrowing connoted the absence of the australopithecine BL broadening which I had called the "australopithecine bulge." In the Olduvai teeth the narrowing applied especially to the third and fourth premolars and the first molar. At the same time, the mesiodistal (MD) diameters of the teeth of OH 7, as well as of OH 4 and OH 6, were somewhat increased. When I compared the two crown diameters to obtain a crown-shape index, its values for the Olduvai little strangers fell outside the entire ranges of index values for the large sample of comparable teeth of *A. africanus*. Were these odd values of the tooth indices for the Olduvai sample the mark of individual variants, whose teeth extended the observed ranges of values for *A. africanus*? Or did they characterise a hominin population whose cheek–teeth lacked the australopithecine bulge?

Stratigraphically speaking, these enigmatic specimens from Olduvai came from a horizon well below the OH 5 or "Zinj" stratum within Bed I. For a time, therefore, Leakey called OH 7 "pre-*Zinjanthropus*", a nickname until we could make up our minds what manner of person this was. Some of our colleagues (such as Nesturkh, 1967) mistook Leakey's intention and used the term as the name of another genus!

With its strange teeth and large endocranial capacity, the youngster posed a real challenge to us. Not surprisingly, we did not come to any precipitate conclusion. For at least 4 years, from 1959 to late 1963, we subjected the new specimens to observation, reconstruction, mensuration and comparisons. From the beginning, Louis was convinced that Jonathan had found an early specimen of the genus *Homo*. There were times when I, too, thought we must have in our hands a representative of something new. More frequently, I held to the conservative view that the new juvenile bones might be regarded as simply widening our concept of the variability of *A. africanus*. The concept of how much *A. africanus* varied was based on the type specimen from Taung, and good samples from Sterkfontein and Makapansgat.

It was not until January 1964 that we finally decided upon the creation of a new species, and *Homo habilis* made its

debut through the pages of *Nature* in April 1964. I do not think that we who had cogitated for 4 years had been overhasty (although that was a criticism levelled against us – in this case by Sir Wilfrid LeGros Clark, 1964). Louis Leakey's mind seems to have been made up some time before, almost intuitively. What produced the change of mind – the conversion – of John Napier and myself?

Conversion in Science: The Case of *Homo habilis*

A myth grew up around my "conversion" to the view that OH 7 belonged to a hitherto unrecognised species of the genus *Homo*. The essence of the myth is that it was Louis Leakey's enormous powers of persuasion and forceful personality that eventually wore down my resistance – and John Napier's too, so that, on this version of the history, we were virtually bludgeoned into supporting Leakey's view!

As far as I can trace, the first published source of this unlikely story was Sonia Cole's (1975) biography of Louis, *Leakey's Luck*. She states (p. 256, emphasis mine):

> Tobias had always been a little unhappy about creating a new name for it, but he had been swayed by Napier's conclusions on the manipulative ability of the hand: 'man the toolmaker' was an accepted definition of man, *habilis* was apparently a tool-maker and therefore he must be a man, yet he did not fit into any of the known species of *Homo*. This argument, *plus the strong pressure brought to bear by Louis*, had persuaded Tobias to associate his name with the creation of the new species; as one colleague put it, *habilis* had been launched mainly by the power of Louis's personality.

In fact, it was neither Louis's persuasive skills nor Napier's views on the hand that converted me, as was clearly stated in contemporary publications. The same point recurred in *Lucy* by Don Johanson and M. Edey (1981: 288). The authors put this question into the mouth of Tim White: "It's not like Louis Leakey hammering and hammering on Phillip Tobias about *Homo habilis* until he had him beaten down?" Their source seems to have been Sonia Cole's book, according to personal communications I received from Don and Tim.

The third reference is by Mary Leakey in her autobiography, *Disclosing the Past*:

> Louis was predictably delighted by the new finds. In 1959 he had regarded Zinj as the maker of the artefacts at FLK, but here was a far better candidate in terms of both brain and hand. This was right in line with his own theories of *Homo* evolving during the earlier Pleistocene. This had to be *Homo*. *He directed his considerable powers of eloquent persuasion towards Phillip Tobias and John Napier*…. (M.D. Leakey, 1984, pp. 127–128; emphasis mine)

I set the record straight on the factors leading to my conversion in my monograph on "*The Skulls, Endocasts and Teeth of* Homo habilis" (1991). In any case, anyone who knows me and my personality well will be able to confirm that my individualism extends not only to my 40-years long fight against apartheid and the inroads against academic freedom by the apartheid government of South Africa, but also and overwhelmingly to my scientific studies and interpretations. It is inconceivable that I would be brow-beaten to a certain standpoint even by such fortissimo personalities as Raymond Dart, LeGros Clark and even Louis Leakey. Apart from these personality traits, contemporary letters and records testify to a different motivation in my conversion.

In the interests of historical accuracy, let me here correct the record as to my position in the saga of *Homo habilis*. It is true that I was hesitant to recognise the single specimen OH 7 as representing a new species. Although I pointed out its departures from typical australopithecine morphology, I felt that I needed more evidence before I could exclude the possibility that these were simply the features of an individual, perhaps slightly aberrant member of an extended *A. africanus* hypodigm.

In August 1961, I replied to a query from Sir Wilfrid LeGros Clark. After a long account of my findings on the teeth and endocranial capacity, I summed up as follows:

> Whether this puts it outside the range of the Australopithecinae (which Leakey believed – and which is in my view quite unlikely) or whether it gives us a better idea of what the australopithecine range really was (which is more likely – especially when the evidence of the parietals is coupled with that of the teeth and mandible) remains to be determined; but at the moment, until I can get down to my more detailed study of all these remains later this year, *my view is that it is an early and rather large-brained member of the Australopithecinae*. (Letter, Tobias to LeGros Clark, 1 August 1961)

Two years later, I had not changed this view. In reply to a written enquiry from Sherwood L. Washburn, I summarised the state of my interim conclusions on "Zinj" and "pre-Zinj." My reply was quoted in full by Washburn as a footnote to his chapter in *Classification and Human Evolution* (1963: 196). At that point, in late 1963, I could tell Washburn: "I do not think I have yet seen any features which, individually or collectively, place it outside the probable range for *Australopithecus sensu lato*." (Tobias, 1963). Right up to December 1963 – over 4 years after Jonathan Leakey had found OH 7 – I believed that I could explain the features of the OH 7 juvenile simply as those of an extreme variant of *Australopithecus sensu lato*. For 4 years, "the force of Louis Leakey's persuasive powers" had failed to bully me into accepting that OH 7 should be seen as representing a new early hominin species! On the contrary, my own hesitation may well have given pause to Leakey, so that he did not name the suspected new species, but waited until more and better specimens were discovered. Louis and Mary made this point clear in 1964.

It was only when additional facts and further appraisals were cast into the scale-pan that I felt forced *by the new*

evidence to abandon the null hypothesis and to conclude that the new Olduvai specimens represented something different from *Australopithecus*.

New Evidence of a Second Kind of Hominin

By 1963, teeth representing five individuals from three Olduvai localities were available. All of them showed the absence of the "australopithecine bulge." They were smaller teeth. In these respects they differed from those of *A. africanus*. Their traits pointed to a distinctive population, whatever its systematic position, that showed features approaching those of *Homo*.

Independently of my studies on the teeth, Napier, Davis and Day had been analyzing the hand- and foot-bones from FLK NN I. They concluded that they were close in their morphology to those of *Homo sapiens*. They could not, however, show that their structure was different from those of *A. africanus* or other australopithecines, because of the lack of adequate remains of the latter. It could be said that Bed I, Olduvai Gorge, included teeth that showed *Homo* departures and hand- and foot-bones that were remarkably like those of later forms of *Homo*. If the teeth and the limb-bones had belonged to members of the same hominin population, it was one characterised by *Homo*-like features not in a single, but in three major structural–functional complexes. It was now beginning to look as though we were dealing with a population with three character-complexes all pointing consistently in the same direction.

As a fourth and critical line of evidence, in 1963 I turned my attention to the parietal bones and the endocranial capacity. Working in Johannesburg, Alun Hughes and I made a new reconstruction of the biparietal arch of OH 7 and a partial endocast that fitted snugly within this arch. As analogues we made a series of part-endocasts of other specimens for which there were fairly reliable estimates of the total endocranial capacity and for which there were clear impressions of the outlines of the parietal bones. From these I devised a method by which to compute the estimated total endocranial capacity of OH 7. By the end of 1963 the first result was obtained. I was amazed at its high value, 675–680 cm^3. At that time, the mean capacity for *A. africanus* was 504 cm^3, though later studies by Holloway cropped this average to 441–442 cm^3. The mean for *Homo erectus* at that stage was 974 cm^3. Thus, the estimated value for OH 7 lay between the means for *A. africanus* and *H. erectus*. The OH 7 value also lay between the highest value in the *A. africanus* observed range and the smallest value in the *H. erectus* observed range. The value for OH 7 was 40–50% greater than the *A. africanus* mean. We needed more specimens with similarly large capacities. Without them, we could not, however, be sure if this large value, considered alone, was the trait of an isolated and perhaps freakish specimen – with, as some suggested, hydrocephalus! – or the hallmark of a population.

Therefore no fewer than four character-complexes – brain, teeth, hands and feet – were displayed by OH 7 and, at least in respect of the teeth, by the remains of four other individuals stemming from MK, FLK NN, and FLK. If all of those remains represented members of the same species, we were surely dealing not with an isolated extreme variant of the australopithecines, but with a population four of whose major character-complexes approached those of later members of the genus *Homo*. It was clearly a population that had moved away from the *A. africanus* pattern towards the *Homo* pattern of structure in all of the anatomical regions available for study and comparison.

At this crucial juncture late in December, 1963, Napier and I felt that we were able to recognize a new species at Olduvai. We were preparing to publish with Louis Leakey the evidence for this conclusion, when dramatic tidings reached us from East Africa.

Most important to the proving of the point came a shower of new hominin fossils from Olduvai Bed II in October and November 1963. These comprised OH 13, a delicately constructed, probably female hominin specimen from MNK II; OH 14 and OH 15; and OH 16, consisting of the teeth and hundreds of fragments of a probable male skull. This fresh haul of hominin specimens, showing some features in common with the Bed I specimens, was the main factor in clinching my realisation that we now had to deal with a population, not a few isolated sports. It was a population that seemed to be represented from the lowest part of Bed I into the middle of Bed II, some two metres below Tuff IIB: this time range was about a quarter of a million years.

The first news of the exciting 1963 finds was brought to Johannesburg by Louis and Mary Leakey late in December 1963. In January 1964, I left for Nairobi to finalize my volume on *A. boisei* and to study the newly discovered Olduvai specimens. Tentative reconstructions of the calvariae of both OH 13 and OH 16 enabled Ronald J. Clarke, then working at the Kenya National Museum, to make partial endocasts of both specimens. Using the part-endocast method with analogues that I had introduced, we obtained estimates of the total capacity of OH 13 and OH 16 and each was over 600 cm^3. It became plain that this Olduvai group of early Pleistocene fossil hominins had a mean capacity that was nearly 50% greater than the mean value for *A. africanus*. OH 7 was no hydrocephalic pathotype – but the type specimen of a population.

When Louis Leakey showed me the maxilla and mandible of OH 13, with their small, slender but long teeth, Ron Clarke who was standing by, recalls vividly how my eyes widened and shone: I turned to Leakey and said, "Louis, this is *Homo*". Clarke remembers how at that moment Louis gave vent to his characteristic, heaving, panting chuckle of sheer delight!

If there was a quintessential instant of conversion, that was it: no force of argument, coercion or power of personality, but the irresistible weight of evidence – and five or six lines of evidence at that! These were the factors that led me to have a change of mind, and to be converted to the view that we had a new and different species at Olduvai.

Conversion by the Hard Evidence

Just as the sheer weight of hard evidence led to my conversion, so too was it conversion of this sort that led a sceptical world eventually to accept Dart's claims for the Taung skull. The hard evidence also dictated Leakey's change of mind about the authorship of the Oldowan stone artefacts, and hard evidence converted Le Gros Clark to his acceptance, after his earlier rejection, of the hominin status of *Australopithecus*. These four examples show that conversions may be governed by rational factors, in a more reasonable, less subjective way than Thomas Kuhn (1962) would have us believe. Ernst Mayr (1972), likewise, has shown that the adoption of Darwinism, the change of paradigm epitomised in the phrase "the Darwinian Revolution," was a less sudden and a more rational set of events in the history of science than Kuhn's analysis proffered. So was it also with the Dartian revolution and the habiline revolution.

What Was the New Species?

Once we had determined that these Olduvai fossils represented a new species, we next had to decide to which of the existing hominid genera of the day the Olduvai fossils should be assigned. Contrary to John Robinson's (1965a) assertion that "insufficient morphological distance exists between (*A. africanus* and *H. erectus*) to justify the insertion of another species," I was able to show, from an analysis of several metrical features, that there was indeed an appreciable gap between the ranges of the two species. In 1964, Napier and I agreed that the new Olduvai fossil taxon "neatly closes the gap between the most advanced *Australopithecus* and the lowliest *Homo erectus*" (Tobias, 1964). Our careful analysis of morphological features revealed a number of apomorphic features of *Homo*.

So by February–March 1964, Napier and I had become convinced that the Olduvai pygmoids should be classified as a species within the genus *Homo*. Louis Leakey had had this conviction for over 5 years. One might say they were two different kinds of conviction – Leakey's being intuitive, prescient, inspirational, ours being dogged, statistical, functional anatomical – and perspirational!

Two things remained to be done: to find a name for the new species and to determine whether its inclusion in *Homo* necessitated a re-definition of the genus *Homo*.

It was chiefly in respect of the cranial capacity that we found it necessary to re-define *Homo*. It was a relatively small change. Yet some of our colleagues excoriated us for defiling the definition of *Homo*, as though it were sacrosanct like the laws of the Medes and Persians. They overlooked the fact that as respectable a scholar as LeGros Clark had changed the definition of *Homo* between the first and second editions of his seminal work, *The Fossil Evidence for Human Evolution* (1955, 1964). Robinson was to change the definition of *Homo* when in 1965 he proposed to lump *A. africanus* into *Homo,* while Bernard Campbell in 1978 re-worded our 1964 definition.

No-one need be coy or shame-faced about altering a generic diagnosis, if the growing and ever-changing state of knowledge and of taxonomy at any time reveals inadequacies in the previous diagnosis. Yet as recently as 1996, Walker and Shipman (1996: 90) quaintly described what we had done as "shifting the ground rules." In the words of Simpson (1963: 8): "The category genus is necessarily more arbitrary and less precise in definition than the species…There is no absolute criterion for the degree of difference to be called generic." Ernst Mayr, in the same year, wrote, "There is no non-arbitrary yardstick available for the genus as reproductive isolation is for the species." (Mayr, 1963: 340–341).

To find a name for the new species, I went to my predecessor and mentor, Raymond Dart, whose linguistic versatility had been one of his more remarkable skills. This man had invented such exorbitantly sesquipedalian words as *Australopithecus* and Osteodontokeratic. I hoped against hope he would abandon this style in favour of something short and simple. I told Dart what we had and that we had reason to believe that members of the new species were so manually proficient as to have been the probable fabricators of the earliest stone tools made to a set and consistent pattern. When the week-end had passed Dart offered the simplest, shortest and sweetest name he had ever invented – *Homo habilis*. The Latin word *habilis* means "able, handy, mentally skilful" and it is the etymon of the English words habile, able and ability. This was a functionally sound as well as mellifluous name, and I and my co-authors readily adopted it.

The name and species definition were announced in our paper, "A new species of the genus *Homo* from Olduvai Gorge" that appeared in *Nature* on 4th April 1964. Our article revised the diagnosis of *Homo*, created a new species *Homo habilis*, offered a diagnosis of it, and gave its geological horizon. A type specimen and paratypes were identified and briefly described. The essential points of difference from *A. africanus* were that *H. habilis* had smaller teeth that lacked the marked buccolingual bulge of the tooth crowns characteristic of australopithecine cheek–teeth; reduced third molar