

# The species *Homo habilis*: example of a premature discovery

Phillip V. Tobias

*Tobias, P. V., Palaeo-anthropology Research Unit, Department of Anatomy and Human Biology, University of the Witwatersrand, Johannesburg*

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The meaning of prematurity in scientific discovery is addressed and examples are given of premature discoveries. The history of the early hominid *Homo habilis* is outlined, from its discovery in 1959 and its launch in 1964 to the general acceptance of the new species in the 1980s. It is shown that the history of this species distinguishes it as a premature discovery, whose acceptance was delayed until the tenets of the old paradigm had one by one been demolished or modified.

“... practically all of the epoch-making discoveries of new kinds of fossil hominids have been received with doubts and opposition from most contemporary anthropologists.”

*Björn Kurtén*

## 1. Introduction

There are times in the history of science when the making of a new discovery or the advancement of a new hypothesis is followed by years of rejection, intellectual isolation and conceptual solitude before it gains acceptance. The degree of scepticism, even scorn, that may attend such notions, seems in keeping with the measure of their prematurity, while the amount of dissen-

sion seems to be in proportion to the margin of dissentience.

An early example is furnished by the remarkably prescient deduction of John Frere, in 1797, that some flint implements he had collected at Hoxne near Diss in Suffolk, England, were not only “fabricated and used by a people who had not the use of metals”, but were to be referred “to a very remote period indeed, even beyond that of the present world”. His hypothesis presaging the great antiquity of man was printed in *Archaeologia* of 1800. Then it was “forgotten by most people for sixty years” (Daniel 1981).

A second example of such a premature discovery was Gregor Mendel’s revelation of the laws of inheritance. Though announced and published in 1865, they failed to gain recognition or appreciation until they were “re-discovered” and independently confirmed in 1900.

Thirdly, we should perhaps place in this category, the discovery of penicillin by Fleming in 1929, for it had to wait until 1939–40 before its potentialities were developed at Oxford by Florey and Cheyne, under the impact of World War II.

Fourthly, we may cite the delayed acceptance that attended the proof offered by Avery, McLeod and McCarty (1944) that DNA was the basic hereditary substance: six to eight years were to elapse before this seminal discovery was recognised (Stent 1972).

In the field of palaeo-anthropology, our fifth example is a particularly telling instance, namely the reception of Dart's (1925) claims on the ancestral near-hominid status of the early genus and species, *Australopithecus africanus*. Those claims had to wait in limbo for 25 years before Dart was vindicated (Tobias 1984).

The case of *Homo habilis*, I submit, was another example of a premature discovery. Although initially rejected and vigorously denounced by many, perhaps most, palaeo-anthropologists, it has come into its own after close on 20 years of rejection.

The first specimen of *Homo habilis* was found at Olduvai Gorge in Tanzania in 1959. The creation by L. S. B. Leakey, Tobias and Napier of a new species of this name did not take place until five years later — in 1964. The general acceptance of the validity of the new species was delayed by another sixteen or more years — until the 1980's, although the first individual presentation on the new species at a world congress was given at the International Union of Anthropological and Ethnological Sciences Congress in Moscow in 1964 (Tobias 1968).

In an age when scientific knowledge is said to double every 10 years, the 20 years' delay in the acceptance of *H. habilis* is an extraordinary phenomenon. It is to be compared with the 25 years' deferment, from 1925 to 1950, in the acceptance of *Australopithecus africanus*. The visitor from Mars might consider these two episodes to reflect a natural cautiousness or even conservatism on the part of those engaged in the study of fossil man. The down-to-earth scholars on this planet, on the other hand, know that these are not the character traits of many members of the palaeo-anthropological fraternity. On the contrary, they might be forgiven for detecting a

certain debonair rashness and a tendency to jump to quick conclusions, as the marks of some of the most famous of this community of scholars. Where then do we have to look for an explanation?

## 2. What is a premature discovery?

Stent (1972), in seeking to understand the delay in the acceptance that DNA was the basic hereditary substance, sought a criterion of a premature discovery, other than its failure to make an impact at the time it was announced (which would have been simply tautologous). He proposed that "A discovery is premature if its implications cannot be connected by a series of simple logical steps to canonical, or generally accepted knowledge." On this basis, the measure of Frere's prematurity was 60 years, that of Mendel 35 years, that of Dart 25 years (22–28 years, according to which date of ultimate acceptance one accepts), that of Avery and his colleagues 6–8 years.

Was *Homo habilis* ahead of its time? On Stent's analysis, we could assert this prematurity for it, only if the recognition of *H. habilis* as a new and separate species could not be connected by simple, logical steps to the generally accepted concepts, or to the paradigm prevailing prior to the proposed recognition of the new species.

Elsewhere, I have attempted to analyse some major tenets of the earlier paradigm to which the recognition of *H. habilis* constituted a threat (Tobias 1989). These included the notion that there was not sufficient "morphological distance" between *A. africanus* and *Homo erectus* to accommodate another species between them; the idea that a creature with a brain-size (or endocranial capacity) smaller than that of *H. erectus* could not be considered a member of the genus *Homo*; the preconception that the definition of the genus *Homo* was sacrosanct and immutable; the supposed inadmissibility of ethological evidence (such as stone cultural evidence) to support the defining and diagnosing of a hominid species; and the unacceptability and inappropriateness of any suggestion that the Pleistocene genus *Homo* could have emerged in the Pliocene: Tertiary *Homo* was inconceivable! These were

some of the features of the paradigm of hominid evolution that prevailed shortly before the species *Homo habilis* was launched upon a reluctant world.

From that analysis, I was led to conclude that those who criticised and rejected the claim that a new lowly species of the genus *Homo* — *H. habilis* — had existed in the late Tertiary and earliest Quaternary of Africa, relied on arguments almost all of which were overtly or covertly predicated upon, and designed to defend, those and other major tenets of the prevailing paradigm. There was sufficient evidence to regard the resistance as being based upon the prematurity of the discovery, in Stent's sense. It was premature because it flew in the face of the pre-existing paradigm or at least, of major tenets of that paradigm. These tenets required to be addressed, one by one, revised and replaced, before the substitution of the new paradigm could be accomplished.

In the light of this analysis, it is appropriate to place on record here a brief chronicle of some of the major events in the launching of *Homo habilis*, the reaction of the world of palaeo-anthropology to the claimed new species, and key happenings during the 20 years of rejection of the species.

### 3. The launching of *Homo habilis*

The article, "A new species of the genus *Homo* from Olduvai Gorge", by L. S. B. Leakey, Tobias and Napier, appeared in *Nature* on April 4, 1964. In the same issue there appeared my work on the endocranial capacity of the Olduvai hominid 7 (OH 7), which was selected as the holotype of *H. habilis* (Tobias 1964a). A paper by L. S. B. and M. D. Leakey (1964) announced the discovery of a number of new Olduvai specimens (OH 12, OH 13, OH 14, OH 15 and OH 16), as well as of the *A. boisei* mandible that K. Kimeu had just found (January 11, 1964) at Peninj on the west side of Lake Natron in northern Tanzania.

Our joint paper of 1964 presented a revised diagnosis of *Homo*, created the new species *H. habilis*, offered a diagnosis of it and gave its geological horizon. OH 7 was named as the type specimen and OH 4, OH 6, OH 8 and OH 13 as

paratypes. There followed brief descriptions of the paratypes by myself. OH 14 and OH 16 were listed as referred material. Notes followed on the implications for hominid phylogeny, cultural associations and relationship to *Australopithecus* (*Zinjanthropus*).

We concluded: "It thus seems clear that two different branches of the Hominidae were evolving side by side in the Olduvai region during the Upper Villafranchian and the lower part of the Middle Pleistocene."

### 3. The assault begins

Our initial publication failed to convince many colleagues. For several years *H. habilis* and we were subjected to severe criticism from such formidable figures as W. E. Le Gros Clark, K. P. Oakley, B. G. Campbell, T. Bielicki, D. Pilbeam, E. L. Simons, F. C. Howell and J. T. Robinson. The controversy raged in the columns of *Nature*, *Science*, *Discovery*, *Current Anthropology*, *The Times* (of London) and *The Star* (of Johannesburg).

### 4. *H. habilis* and *Telanthropus*

Just over a month after the appearance of our joint paper, Oakley and Campbell (1964) wrote to *Nature* about the relationship between *H. habilis* and *Telanthropus capensis* (= *H. erectus*) of Swartkrans. We had included this comment in our joint article: "The specimens originally described by Broom and Robinson as *Telanthropus capensis* and which were later transferred by Robinson to *Homo erectus* may well prove, on closer comparative investigation, to belong to *Homo habilis*." With hindsight we might well have worded this passage more clearly, for it misled several colleagues. Thus Oakley and Campbell (1964) chid us for not making "careful comparative investigation with closely related taxa ... before the creation of new species" (op. cit., p. 732). In *The Times* (May 29, 1964a) and again in *Discovery* (June 1964b), Campbell went so far as to state: "To the zoologist's astonishment, he reads in *Nature* (April 4) that Dr. Leakey and his colleagues have not yet compared the new

*finds* with what by general agreement are the most closely related forms, that is, those known as *Telanthropus capensis* from South Africa, which are now generally classified as a subspecies of *Australopithecus africanus*" (Campbell 1964b:38) (emphasis mine).

We immediately corrected Campbell's obvious misreading or misinterpretation of the joint *Nature* article (Napier & Tobias in *The Times* of June 5, 1964; Tobias (1964b) in *Discovery*). We pointed out that we *had* made detailed comparisons with all available fossils, with originals where accessible or, where these were unavailable, with casts, as well as through published descriptions. "Among the fossils compared in this way was *Telanthropus*, which Dr. Campbell singled out for special mention and which he erroneously stated is 'generally classified' as a member of *Australopithecus*." (It was seven years since Simonetta, 1957, and three years since Robinson, 1961, had reassigned *Telanthropus* to *H. erectus*.) "We reached the conclusion that the two forms (the Olduvai habilines and Swartkrans *Telanthropus*) must be closely related, *at least as far as their teeth were concerned*." (Napier & Tobias 1964). We pointed out also that the remains attributed to *Telanthropus* were so fragmentary that a complete identification was impossible at that stage. "We could not therefore draw any final conclusions on the possible relationship between the Olduvai fossils and those from Swartkrans, and stated accordingly that *Telanthropus* 'may well prove, on closer comparative investigation, to belong to *H. habilis*'."

Years later R. J. Clarke was to show that the maxilla SK 80 which Robinson (1953) had identified as *Homo* (*Telanthropus*) and SK 847 which had been classified as *Australopithecus* (*Paranthropus*) fitted together to form parts of the same cranium! (Clarke et al. 1970, Clarke & Howell 1972, Clarke 1977). This composite cranium, now known as SK 847, was left *incertae sedis* by Clarke in 1977 but, after the discovery of the fine *H. erectus* cranium KNM-ER 3733 at East Lake Turkana (R. E. F. Leakey & Walker 1976, Walker 1976, 1981), Clarke (1985) became convinced that SK 847 is not *H. habilis*, as some colleagues have assumed (e.g. Blumenberg & Lloyd 1983), but an 'early *Homo erectus*', like KNM-ER 3733.

Thus, the non-australopithecine specimens of Swartkrans have found a satisfactory systematic home in early *H. erectus*. They represent a population differing in several important respects from *H. habilis*.

Suppose for the moment that the Olduvai hominines *had* proved to be conspecific with *T. capensis*. Then as Oakley & Campbell (1964) pointed out, although the name *capensis* would have priority over *habilis*, the former name is not available in the genus *Homo* as Broom (1917) had designated the Boskop skull *Homo capensis*.

## 5. Was *Homo habilis* sufficiently differentiated from *Australopithecus*?

One of the serious rejections of *H. habilis* came from Sir W. E. Le Gros Clark. In the July 1964 issue of *Discovery*, he dismissed the claimed differences of the Olduvai hominines from *Australopithecus* as trivial and went on to say: "All in all, then, the similarities of Leakey's fossils with known specimens of *Australopithecus* are so remarkable, and the differences from known fossil remains of *Homo* are so great, that their relegation to the former genus can hardly be in doubt."

This forthright statement came from Sir Wilfrid when he himself had not studied the original fossils! Nevertheless, he went on to say that it was particularly unfortunate that the new species should have been announced "long before a full and detailed study of all the relevant fossils can be completed ..." (Le Gros Clark 1964). His final comments read: "But from the brief accounts which have been published one is led to hope that he [*Homo habilis*] will disappear as rapidly as he came. He certainly does not appear to merit a prolonged controversy." (Le Gros Clark 1964:49)

Coming from this cautious and quietly-spoken scientist, that ringing peroration was little short of a deafening blast!

My reply to Sir Wilfrid reminded him that the studies he wished to see we had already carried out over the previous four years. Almost prophetically, in the light of the inordinately long gestation of my *H. habilis* volumes (1991), I added, "It may take many years before we shall have had the time to complete the monographic writing up of the specimens, but this is no reason why our scientific colleagues should in the meantime be denied access to the views we have reached from our detailed study, and a summary

of the evidence from which we have reached our conclusions. Such a summary we have published in at least nine or ten papers in *Nature*.” (Tobias 1964b).

Several attacks were launched by Robinson (1965a, 1965b, 1966). Mostly he was concerned to show that the shape index values of the tooth crowns of *H. habilis* were not really different from those of *A. africanus* and that, even if they were different, crown shape was of low phyletic valence; that the relationship between the thickness of the corpus mandibulae and the width of the space between the left and right corpora mandibulae of OH 7, was like that of *Australopithecus*, as he conceived it, and not like his view of this relationship in *Homo*. These assertions started a long controversy over relative space and body widths (Tobias 1966a, 1974, 1991). Furthermore Robinson dismissed the endocranial capacity of *H. habilis* as differing little from that of the australopithecines.

The author's later work has disproved all of these three objections. The crowns of *H. habilis* premolars and first molars are elongated and attenuated, as compared with those of *A. africanus* which are mesiodistally truncated and buccolingually expanded; these differences are less evident in the second and third molars. Mean crown shape index values for the anterior three cheek-teeth consistently distinguish between the two taxa (Tobias 1991).

Secondly, Robinson's claims about the relative space width of the mandibles of the two taxa have been disproved and it has been shown that from study to study that author shifted the position at which he made the relevant metrical comparisons. Clearly the space width to body width ratio, which Robinson imported into the evaluation of *H. habilis*, has little or no bearing on the systematic status of *H. habilis* and does not align *H. habilis* with *A. africanus* (Tobias 1966a, 1974, 1991).

Thirdly, the mean endocranial capacity of the available sample of *H. habilis* specimens is 640 cm<sup>3</sup>, whilst that of *A. africanus* is 441.2 cm<sup>3</sup>. The mean value for *H. habilis* is 10.2 standard deviations greater than the mean value for *A. africanus*. The cerebral pre-eminence of *H. habilis*, not only absolute but relative, has been recognised by many workers (Pilbeam & Gould

1974, McHenry 1982, Hofman 1983, and see Tobias 1971, 1987, 1991).

## 6. Was there enough of *Homo habilis*?

Another criticism was that the new species was erected “on the basis of material so scanty and incomplete” (Bielicki 1966:576). It was necessary to point out that the new species was set up on the hypodigm of 40 teeth, 2 tolerably complete mandibles, a fragment of a third mandible, parts of a pair of maxillae, varying portions of 4 calvariae, the hand-bones of at least two individuals, foot-bones, a clavicle, a tibia and a fibula (probably of *H. habilis*). By any reckoning, this was a tolerable sample in palaeontology (Tobias 1966b).

## 7. Was the estimate of endocranial capacity of Olduvai hominid 7 correct?

Also criticised was the technique I used to estimate the endocranial capacity of OH 7. I had reconstructed the biparietal arch of OH 7, produced a part-endocast that fitted snugly within the arch and estimated the total endocranial capacity from the volume of the part-endocast by use of a series of hominid analogues. Pilbeam & Simons (1965) criticised my method of assembling the parietals and the figures obtained. They held that the slightest inaccuracy in the alignment of the left with the right parietal would make a significant difference to the biparietal endocranial capacity.

After this criticism was published, unbeknown to L. S. B. Leakey and myself, Holloway (1965) conducted a series of experiments to test my claims and the criticism of Pilbeam and Simons. On representations of the two Olduvai parietals, he studied the effects of varying the angle between the two parietals and concluded that, since the height of the arch increases as the width decreases, “the volume remains essentially constant”. Thus, Holloway provided unexpected, independent confirmation of the validity of the method I had employed and of the results obtained. Amid the storm-clouds of censure that had broken about our heads, Holloway's results were a small but impressive silver lining.

## 8. Why not place the Olduvai hominines in *Homo erectus*?

Most critics berated us for taking the Olduvai taxon out of *Australopithecus*, and suggested that its distinctness rated no more than subspecific rating as '*A africanus habilis*' or, at best, '*A. habilis*'.

However, some felt we had not gone far enough in distancing *H. habilis* from *A. africanus* and that the Olduvai specimens should be placed in *H. erectus*. One was D. R. Hughes of the Duckworth Laboratory of Physical Anthropology at Cambridge University.

At that time, I was a visiting professor at Cambridge. To the Duckworth Laboratory had been brought all of the Olduvai hominid fossils and the Peninj mandible. Hughes thus had the opportunity of examining these fossils directly. In a letter published in *The Times* (London) on June 10, 1964, he lauded the more flexible approach to hominid taxonomy that had led us to propose to modify the diagnosis of the genus *Homo*. He added: "Whilst entirely agreeing with them that this new manlike fossil should be classified in the genus *Homo* rather than in the genus *Australopithecus*, I suggest that a revision of the range of variability for *Homo erectus* is now called for, with *habilis* being recognized as the earliest representative so far discovered, and its importance being recognized by the award of sub-specific status, i.e. *Homo erectus habilis*."

D. R. Hughes was not the only one to suggest that *habilis* should be accommodated in *H. erectus* in those early days. Another was Ashley Montagu. After "Early Man in East Africa" appeared in *Science* on July 2, 1965 (Tobias 1965a), Montagu wrote in *Science* on August 27, 1965: "Tobias believes that *H. habilis* stands in a position intermediate between the australopithecines and the pithecanthropines. It is a reasonable conclusion. But to judge from the available data, it would be equally reasonable to conclude that *H. habilis* was, in fact, an early pithecanthropine. There is nothing in the published data that would not conform to the requirements of the latter hypothesis. Applying Occam's razor, *H. habilis* could perhaps more appropriately be regarded as an early representative of *Homo erectus*." (Montagu 1965:918).

In my response, I commented that this suggestion "goes further than I believe the available evidence permits. Between the two extremes of this view and the opposite one, that we should call the hominid *A. habilis*, the interim solution of a lowly species of *Homo* seems a reasonable compromise. Only the discovery of more specimens and refined statistical comparisons can resolve these slightly diverging viewpoints." (Tobias 1965b). For many years, some colleagues were to prefer the nomen *A. habilis* (e.g. Simons et al. 1969, Pilbeam 1970, 1972).

Thus within 15 months of the publication of the joint paper that launched *H. habilis*, no fewer than six nomina<sup>1</sup> had been proposed to accommodate these Olduvai fossils:

*A. africanus habilis*

*A. habilis*

*H. habilis*

*H. erectus habilis*

*H. erectus* (subspecies unspecified).

## 9. Cultural status and the genus *Homo*

In the paper that created the species *H. habilis*, the species diagnosis given was a conventional, morphological one and no reference was made to cultural behaviour as part of the definition. In the same paper, however, we stated: "While it is possible that *Zinjanthropus* and *Homo habilis* both made stone tools, it is probable that the latter was the more advanced tool maker and that the *Zinjanthropus* skull represents an intruder (or a victim) on a *Homo habilis* living site." In relation to the artificially constructed circle of stones that had been excavated by M. D. Leakey in the lower part of Bed I of Olduvai, we added the

<sup>1</sup> A quaint variation was *Homo Palaeoanthropus Habilinensis* proposed by Prof. Enver Bostanci (1974). In an unforgettable foot-note, he added a sentence that is reminiscent in some ways of F. A. Bather's (1925) scolding of R. A. Dart for inventing a word (*Australopithecus*) stemming partly from Latin and partly from Greek! — "Because there is a famous story in history Habil and Kabil, we should have not mixed them with fossil man." (Bostanci 1974:566). I understand that Kabil and Habil refer to Cain and Abel.



thought: "It seems that the early hominids of this period were capable of making rough shelters or windbreaks and it is likely that *Homo habilis* may have been responsible." (L. S. B. Leakey et al. 1964:9).

A little later that year, Napier and I indicated in our letter of response to Campbell in *The Times* (June 5, 1964) a stronger role for the cultural evidence: "On the basis of our comparisons, we concluded that the anatomy of the new fossils is about midway between that of *Australopithecus* and of *Homo erectus*. But anatomy alone could not tell us whether the new creature was the most advanced *Australopithecus* or the lowliest *Homo*. The answer was provided by a strong body of evidence that the habilis-man was the maker of early stone tools ...." and "The only reasonable conclusion to draw is that *Australopithecus* had not attained to the crucial stage of stone tool-making to a set and regular pattern; true, he was a tool-user, a tool-modifier and perhaps even an *ad hoc* tool-maker, but we have no good evidence that he went beyond this .... the more advanced man was the maker of tools of a definite culture and with a definite trend of progressive development. Such activities have thus far been associated only with members of the genus *Homo* and not with *Australopithecus*. It seems clear therefore that our new type of man should be classified in the genus *Homo* rather than with the genus *Australopithecus*." (Napier & Tobias 1964).

Our importing of cultural evidence to help weigh up the generic status of *H. habilis* was in keeping with accepted procedure that ethological evidence may be added to morphological evidence in the assessment of the systematic status of a group. As one example, Robinson (1962a, b) had already defined the sub-family Homininae as including "forms broadly distinguished morphologically by having erect bipedal posture and a large brain, and behaviourally by relatively complex cultural activity" (Robinson 1962a:130).

The interpretation by S. Cole of Napier's and my remarks about culture in our letter to *The Times* is entirely false. In 'Leakey's Luck' she states, "They (Napier and Tobias) were, in fact, getting cold feet after hearing of the reaction of Le Gros Clark and others." She added, "Very

naturally Louis was most upset that his colleagues should have turned traitor in this way." (Cole 1975:256). This is a strange and untoward judgment. It was our considered opinion that we were strengthening the argument in favour of *H. habilis*, not deserting nor betraying it!

Our view was that, when the inferred ethological traits were added to the morphological criteria, they strongly supported the affinity of *habilis* with *Homo* and not with *Australopithecus*.

While there were critics of our importation of cultural evidence into the recognition of *Homo*, there was early support for our approach by Heintz (1966) and by Comas (1968) in their definitions of *Homo* and by Parenti (1966, especially pp. 58–59).

## 10. A cutting attack by Brace

One of the biggest altercations was over dental measurements — and the most devastating onslaught was that of C. L. Brace et al. (1973). Even the title of their report was tendentious: "Tooth measurements and the rejection of the taxon '*Homo habilis*'". An earlier draft of their detailed analysis, they admitted, had been described as "statistical overkill when we turned the computer loose on the inoffensive early hominid dental data". Their study was based on only tooth crown area values and thus missed the striking dental shape contrasts, step-indices, chord indices, 'tooth material' values, root number, non-metrical morphological traits and other odontographic features which have been shown to distinguish *H. habilis* from *A. africanus*. Brace and his colleagues concluded that OH 7, OH 13, OH 16 and OH 24 "cannot be distinguished from *A. africanus* and should be so considered unless proven otherwise".

Having thus, to their satisfaction, demolished OH 7 and the rest, they declared, "Since the taxon *Homo habilis* is without a type specimen, a usable paratype or any unequivocal referred material, it is an empty taxon inadequately proposed and should be formally sunk."

Being busily occupied with the measuring of teeth and endocranial capacity of the newly-discovered habilines of East Africa and sundry other

endeavours, I did not reply at the time<sup>2</sup>. Perhaps the contents of the recent 2-volume work on *H. habilis* (Tobias 1991) will lay to rest some of these critics' haunting doubts.

## 11. A late arrival: Twiggy at Olduvai

In October 1968, field work at Olduvai was resumed. Almost immediately, P. Nzube discovered the crushed but nearly complete cranium of OH 24 at DK East. The new specimen was embedded in a mass of calcareous matrix: it had been much flattened, hence its nickname Twiggy. The cranium was meticulously reconstructed and restored by R. J. Clarke.

In her first announcement of the discovery, M. D. Leakey (1969) indicated that "the appearance of the parts now visible and of the teeth indicates that it closely resembles the hominid remains that have been referred to *Homo habilis* and that it is dissimilar from *Australopithecus boisei*" (M. D. Leakey 1969). In their description of this specimen, M. D. Leakey et al. (1971) inclined to the view that this was a female of *Homo habilis*.

An unsigned comment in *Nature*, headed "Confusion over fossil man" (Anon. 1971), questioned the attribution of OH 24 to *H. habilis* and claimed that the new cranium did not fulfil all the generic criteria outlined in the definition of *Homo* proposed by Leakey, Tobias and Napier in 1964. In particular, it was claimed by this anonymous Palaeoanthropology Correspondent that OH 24, first, did not possess the minimum required cranial capacity of "about 600 cm<sup>3</sup>", and, secondly, it possessed a "dished" face such as characterizes many known australopithecine crania but not *Homo*. In my reply, I pointed out that the comment was erroneous in both respects (Tobias 1972). The cranial capacity was about 590–600 cm<sup>3</sup>, which certainly is "about 600 cm<sup>3</sup>".

As regards the "dished face", the anonymous correspondent had evidently misunderstood the way in which the term had been used in the palaeo-anthropological literature. On the usage of L. S. B. Leakey et al. (1964), OH 24 certainly did not have a "dished face". Because of the confusion which this term had occasioned, I recommended that it be dropped from the literature. I added a few other morphological features which OH 24 shared with *Homo* and in which it differed from the australopithecines, and ended: "It is not unreasonable to conclude that OH 24 adds powerfully to the growing body of evidence from Olduvai, East Rudolf and elsewhere that an early member of the genus *Homo* existed alongside australopithecines in the Lower Pleistocene." (Tobias 1972:469).

The Palaeo-anthropology Correspondent of *Nature* was not satisfied and returned to the fray (Anon. 1972). He or she felt that the fracturing and distortion of the cranium was such as to render the estimate of endocranial capacity too high. By means of an English dictionary, the correspondent proved that "dished" is "dished" and that OH 24 showed it. The anonym agreed that "there is growing, and perhaps incontrovertible, evidence of the existence of a more advanced hominid in the early Pleistocene parts of Africa (sic)." But the sting in the tail read: "I must, however, emphatically deny that "Twiggy" is part of this evidence." (Anon. 1972:469).

In the event, Twiggy has proved a remarkably fine specimen of *H. habilis*, in respect of her calvaria, basis cranii, endocranial cast, facial skeleton and dentition. Indeed she has been accorded the position of being the first described specimen in the *Homo habilis* volumes (Tobias 1991).

## 12. The turn of the tide

The tide began to turn in 1977 with the descriptions of a reconstructed cranium of *H. habilis* from Omo in southern Ethiopia by Boaz & Howell (1977) and of one from Sterkfontein Member 5 in the Transvaal by Hughes & Tobias (1977). Between 1959 and 1986, *H. habilis* remains have accumulated from Ethiopia, Kenya, Uganda and the Transvaal. By 1980 Wolpoff could claim,

<sup>2</sup> Had I done so, I should have been tempted to convey the point that was so succinctly and compellingly made nearly a decade later by Wolpoff (1980:155): "The recently reported discovery of australopithecine sized teeth among U.S. Navy recruits does not demonstrate that there are australopithecines in the Navy." The original report on the U.S. Marine Corps Air station recruits was by Keene (1967).



"*H. habilis* is a taxon whose time has come". It had been a premature discovery and the measure of its prematurity was close on 20 years.

Today it is widely accepted as a good taxon and one that represents a critical stage in the evolution of modern man. To refocus the words of my esteemed friend Björn Kurtén, "... at this point new evolutionary vistas are opened. The invention of language and the largescale manufacture of durable stone tools are the starting point for a new kind of evolution in which the cultural evolution becomes inextricably combined with the genetic. In this situation, brainpower becomes more important than ever before ..." (Kurtén 1972).

If these trends showed their hesitant and facultative beginnings with the Dartian australopithecines, they attained their obligate and puisant flowering with *Homo habilis*.

The tenets of the old paradigm have been effectively demolished or modified, and replaced by those of a new paradigm for human evolution.

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