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The Field of Human Evolution Within Evolutionary Biology and Anthropology: Historical and Epistemological Analyses Since Inception

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A Thesis Submitted to the Faculty of Science
in Fulfilment of the Requirements for the Degree
of Doctor of Philosophy

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In Memoriam...

in memory of my father
Jacques Delisle (1938-1992)

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PhD Thesis by Presentation of Published Papers

This thesis is made up of nine papers which are published or in process of being published. The status of each paper is as follows:

A) Already published:

1- "Human Palaeontology and the Evolutionary Synthesis during the Decade 1950-1960". In, *Ape, Man, Apeman: Changing Views since 1600*, R. Corbey and B. Theunissen (eds.), Leiden University, 1995, pp. 217-228.

2- <<Les origines de la paléontologie humaine: essai de réinterprétation>>, *L'anthropologie* (Paris), Tome 102 (1): 3-19, 1998.

B) Papers already published <u>in support</u> of the main basis of supplication:

3- "Dobzhansky, Theodosius (1900-1975)". In, *History of Physical Anthropology: An Encyclopedia*, F. Spencer (ed.), Garland Press, New York, Vol. 1, 1997, pp. 349-351.

4- "Mayr, Ernst (b. 1904)". In, *History of Physical Anthropology: An Encyclopedia*, F. Spencer (ed.), Garland Press, New York, Vol. 2, 1997, pp. 650-651.

5- "Simpson, George Gaylord (1902-1984)". In, *History of Physical Anthropology: An Encyclopedia*, F. Spencer (ed.), Garland Press, New York, Vol. 2, 1997, pp. 932-933.

C) In press:

6- <<La généalogie de l'homme et l'archéologie préhistorique, 1900-1950: une synthèse difficile>>. In, *La culture est-elle naturelle? Evolutions du concept de culture*, A. et J. Ducros, F. Joulian (eds.), Editions Errance, Paris, pp. 88-108.

D) Presently being edited or finalised for submission:

7- "Human Phylogeny and Evolutionary Biology During the Second Half of the Twentieth Century". To be published in *Studying Human Origins: The History and Epistemology of the Disciplines*, R. Corbey and W. Roebroeks (eds.), to be offered for editorial consideration to Cambridge University Press. (see Appendix A).

8- "Human Palaeontology and Prehistoric Archaeology, 1900-1950: Some Aspects of the Problem of Integration in Science". To be published in *The History of Palaeoanthropology as Seen from the New Millennium*, F. Spencer (ed.). (see Appendix B).

9- "The Rise of the Field of Human Evolution, 1860-1930: A Reassessment". To be published in *The History of Palaeoanthropology as Seen from the New Millennium*, F. Spencer (ed.). (see Appendix C).

Declaration

The candidate Richard G. Delisle declares that the work contained in this thesis is entirely his own work and has not been previously submitted for another higher degree.

The candidate also declares that the nine papers constituting the main body of this thesis were entirely written by him, and by him only.

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Abstract

Abstract

This thesis constitutes an effort to understand the nature of the field of human evolution since its inception and to gain a better understanding of both the forces and the constraints at work within its boundaries. To perform this task it was decided to analyse both the interrelationships of the several scientific disciplines and the interactions of the approaches contributing to the establishment of human phylogeny.

The historical and epistemological analyses presented here shed light on the main message to emerge from this thesis: the study of human evolution is a fairly recent scientific quest to which several scientific disciplines and approaches have contributed. This situation generated problems in the integration of knowledge that persist until today. This constitutes the epistemological landscape in which research on human phylogeny is conducted. The thesis aims to contribute to its understanding.

It is suggested that the institution of a strong research programme in history and philosophy would greatly benefit the further development of the field of human evolution.

Introduction

Introduction

The field of human evolution is anything but monolithic. As is probably the case in other scientific disciplines, it is constituted of several approaches and research strategies either competing with one another, or complementing each other.

Although this situation is acknowledged by contemporary actors in the field of human evolution, scientists seem usually to be more interested in promoting a particular approach to human evolution than in providing a reflection on the nature of their field. This situation is perfectly understandable; it is the very essence of the scientific process.

However, if contemporary actors are busy promoting how research on human evolution should be performed, the thesis presented here tries to establish how it was actually performed.

By so doing, it is our objective to get a better understanding of both the forces and constraints at work within the field. This thesis make an effort to understand the nature of the field of human evolution since its inception. Hence, there is a strong epistemological or philosophical emphasis in the analyses presented here.

However, this epistemological thrust is not rooted in any specific view of the nature of science as expounded by philosophers of science. Instead, it is rooted within the tradition of the history of science. The aspect of the history of science followed in this thesis is concerned entirely with internal scientific matters, as opposed to external social and ideological ones.

Obviously, the nature of the field of human evolution cannot be treated satisfactorily within the limits of a single thesis. In order to counteract this limitation, it was decided to cast the net of the thesis as widely as possible.

This thesis is constituted of a number of independent papers as recognized in the formula of "PhD thesis by presentation of published papers". This formula is ideal to venture into so ill-explored a subject of research as is ours. It permits one to cover a lot of ground and tackle several themes within the same research area.

To explore the nature of the field of human evolution, it was decided to analyse the relationships among the several scientific disciplines and approaches contributing to the research on human phylogeny.

Research on human phylogeny since its inception presents itself as a complex network of ideas. To break into this network, three major themes were investigated: 1) the impact of evolutionary biology on human phylogeny; 2) the impact of

anthropology on human phylogeny; and 3) the relations of biological and cultural factors and their impact on the reconstruction of human phylogeny. These themes constitute the grid within which the papers included here should be read.

These three main themes are not usually discussed separately in the papers constituting the thesis, but more often in association. The papers are presented in sequence within the body of the thesis, essentially in accordance with the time framework of the subject that each considers. Because the papers presented here were written in order to be gradually published during the course of PhD studies, they represent ideas "in progress". Consequently, there is some overlapping of content among some papers, as these ideas were further developed and refined. This being said, most papers are complementary and aim to shed a somewhat different light on a specific question or questions. Although each paper has its own pagination, a general system of pagination for the whole thesis was introduced in the lower right-hand corner, to facilitate indexing.

In chapter one, a general framework is established to analyse the rise of the field of human evolution in the nineteenth century. It is argued that its rise is not rooted in a single scientific tradition (evolutionary biology) as is often assumed, but in several fields among which figure philology, history and anthropology. The application of this

analytical framework permits us to recognize that the establishment of human phylogeny in the second half of the nineteenth century was not generally impaired by anti-evolutionary views but mainly by an epistemological problem. This problem expressed itself in the following way: some human evolutionary interpretations were based on the comparative anatomy of living non-human primate and human forms, while others were based on the hominid fossil record. In earlier phases, the fossil record was used to elucidate the prehistoric peopling of Europe, and not the evolutionary links between humans and non-human primate forms. This situation is explained by the fact that ancient human remains available, though somewhat abundant, were mostly of modern appearance and almost exclusively restricted to Europe. Chapter two is complementary to chapter one by taking the analysis of the second half of the nineteenth century to a greater depth. Chapter two applies this analysis also to the first third of the twentieth century.

Chapter three investigates how the biocultural evolution of humankind has been assessed by the disciplines of human palaeontology and prehistoric archaeology during the 1900-1950 period. It is argued that, regardless of the unity of the biological and cultural aspects in human evolution, human palaeontology and prehistoric archaeology were uncoupled, epistemologically speaking, because they had to forge distinct and proper analytical tools to analyse their

respective empirical bases. In chapter four, this analysis is presented in greater detail for the 1900-1950 period. Above all, however, chapter four pushes further the reflexion on the integration of human palaeontology and prehistoric archaeology by introducing a discussion on the problem of the integration of scientific disciplines. It is argued that human palaeontology and prehistoric archaeology had been rightly integrated at the theoretical level of scenario-building, while they were uncoupled at the level of their respective empirical bases. The content of chapter three is entirely incorporated in chapter four; this is the only instance in this thesis in which the content of one paper is entirely contained in another.

In chapter five, the impact of the evolutionary synthesis on the reconstruction of human phylogeny is assessed during the decade 1950-1960. In this chapter, the evolutionary synthesis is depicted as a complex research programme which has influenced the field of human evolution in several ways, most notably through the "new systematics" and population thinking. This has instituted a tension between population thinking and typological thinking. If the former way of thinking resulted in the recognition of less taxonomic diversity at and above the species level in human evolution, the contrary resulted from typological thinking. Chapters six, seven and eight are small biographical essays on three fathers of the evolutionary synthesis: Theodosius

Dobzhansky, Ernst Mayr and George Gaylord Simpson. These essays further discuss the research programme of the evolutionary synthesis and its impact on the field of human evolution. Lastly, chapter nine analyses the impact of major changes in evolutionary biology during the second half of the twentieth century on the reconstruction of human phylogeny. Two broad research programmes are encountered during this period: the evolutionary synthesis and cladistics. If the former leads to taxonomic reduction in human evolution, the latter leads to taxonomic inflation. Interestingly, this last chapter reveals a profound epistemological problem in the reconstruction of human phylogeny: are evolutionary patterns to be determined in the light of presumed evolutionary processes or, conversely, are processes to be established only once the patterns are presumably correctly identified?

Chapter 1

L'Anthropologie, (Paris)
Tome **102** (1998), n° 1, pp. 3-19

LES ORIGINES DE LA PALÉONTOLOGIE HUMAINE : ESSAI DE RÉINTERPRÉTATION

par
Richard DELISLE*

Résumé. — Une analyse historique et épistémologique de la Paléontologie humaine au XIX[e] siècle permet de repenser les rapports qu'elle entretient avec la Biologie évolutionniste. Si la naissance de la Paléontologie humaine est tributaire de l'avènement des théories de l'évolution par certaines de ses facettes, en revanche d'autres facettes la lie à l'anthropologie, l'histoire et la philologie. Il ne faut pas parler de l'origine de la Paléontologie humaine mais bien de *ses origines*. L'admission de la pluralité des sources de la Paléontologie humaine permet de mieux saisir le décalage observé entre les conceptions théoriques au sujet de la phylogénie humaine et l'interprétation de sa base empirique lors de la deuxième moitié du XIX[e] siècle. Cette analyse est précédée d'une réflexion sur l'historiographie de la Paléontologie humaine dans le but de favoriser une relecture de sa naissance.

Abstract. — **The Origins of the Field of Human Paleontology : An Interpretative Essay.** A historical and epistemological analysis of the field of human paleontology during the 19th century brings a new light on its relation with evolutionary biology. The rise of the field of human paleontology is indeed closely related to the establishment of theories of evolution. Yet, some aspects of human paleontology are not rooted within the tradition of evolutionary biology, but within a host of research traditions such as anthropology, history and philology. Therefore, we should not be speaking of a single but rather of the several origins of the field of human evolution. The recognition of such multiple sources to the field of human evolution is crucial to the understanding of the gap separating the theoretical conceptions on human phylogeny on one hand, and the interpretations given to the human fossil remains on the other, during the second half of the 19th century. If human fossil remains were not interpreted in a typically evolutionary fashion, so to speak, it is not because the field of human evolution was dominated by anti-evolutionists. Rather, the nature of the fossil record was of limited value to produce global phylogenetic interpretations. Instead, regional genealogies or phylogenies of the various European peoples were suggested. The problem was, therefore, essentially epistemological. The analysis of this episode in human evolution is preceded by a historiographical reflexion in order to permit a fresh reading of the historical "facts" surrounding the rise of the field of human paleontology.

INTRODUCTION

Cet essai porte sur la naissance de la Paléontologie humaine au XIX[e] siècle. Mais au-delà des considérations historiques qu'il comporte, ce texte est avant tout une réflexion épistémologique sur le domaine de l'évolution humaine en tant que discipline, définie par ses composantes théoriques et empiriques. Nous tenterons de démontrer que la conception d'une Paléontologie humaine naissant, toute entière, d'une Biologie évolutionniste fait obstacle à sa véritable nature. Une fois l'hégémonie de cette Biologie évolutionniste levée, la Paléontologie humaine nous apparaît dans toute sa pluralité. Cette pluralité appelle à repenser les forces en action au sein de la Paléontologie humaine d'hier et d'aujourd'hui.

Cet essai n'a pas la prétention d'entrer dans les détails qu'une analyse complète, si cela s'avérait même possible, requerrait. Plus modestement,

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nous tenterons ici de jeter les bases d'une réflexion épistémologique par l'établissement d'un cadre général d'analyse pour le XIX^e siècle.

LA PALÉONTOLOGIE HUMAINE ET LE DARWINISME

Au cours des années 1930 et 1940, la Biologie évolutionniste verra la Paléontologie, la Systématique, l'Embryologie, l'Anatomie comparée et la Génétique s'unir sous la coupole de la théorie dite « synthétique de l'évolution ». Pour reprendre la formule consacrée, la théorie synthétique de l'évolution fusionnera la conception darwinienne de l'évolution avec les développements récents en génétique des populations. Ainsi, l'évolution s'explique par les changements géniques (mutations et recombinaisons) qui sont par la suite organisés par la sélection naturelle.

Ce nouveau contexte scientifique sera favorable à une réinterprétation de Darwin, dont la place en Biologie évolutionniste était auparavant débattue. Il deviendra pour de nombreux chercheurs à la fois le père fondateur et la figure emblématique de la théorie de l'évolution ; l'initiateur d'une révolution dite « darwinienne » aux multiples ramifications scientifiques et sociales.

Depuis les années 1950, scientifiques d'abord, historiens ensuite et philosophes enfin se livreront à une véritable entreprise de réhabilitation et d'approfondissement de la pensée de Darwin et de ses sympatisants, et en concomitance, à une réinterprétation de l'histoire de la Biologie évolutionniste. L'ampleur de cette entreprise est telle qu'il arrive parfois qu'elle soit qualifiée « d'industrie darwinienne » (*cf.* Bowler, 1988 ; Greene, 1975 ; La Vergata, 1985 ; Roger, 1985). Il s'ensuivra une périodisation centrée autour de Darwin lui-même, par la définition des périodes pré et post-darwiniennes. Il n'est pas de notre ressort, ici, d'évaluer les bienfaits et méfaits de cette entreprise pour l'interprétation de l'histoire de la Biologie évolutionniste. Par contre, il est clair que les études sur le darwinisme ont eu certains effets pervers sur l'interprétation de l'histoire de la Paléontologie humaine.

D'abord, c'est en vain que l'on chercherait à établir une étroite relation entre le développement des études sur le darwinisme depuis 1950 et l'interprétation de l'histoire de la Paléontologie humaine. Tout au plus est-il possible d'établir certains liens lorsque des inflexions majeures se produisent au sein de l'historiographie darwinienne. Par exemple, il est de plus en plus question, dans la littérature anglo-saxonne, d'une révolution dite « non-darwinienne » au XIX^e et début du XX^e siècle, c'est-à-dire la réalisation que l'évolutionnisme était largement défendu hors du darwinisme (e.g., Bowler, 1983, 1988). Ces changements, qui se posent en extension aux études entourant le darwinisme, trouvent parfois certains échos lorqu'il s'agit d'interpréter l'histoire de la Paléontologie humaine (e.g., Bowler, 1986).

Non, l'impact réel qu'auront les études darwiniennes sur l'historiographie de la Paléontologie humaine ne tient pas aux dédales de ses ramifications et changements, mais plutôt à la fonction inhibitrice qu'elles joueront, et ce, jusqu'à ce jour, lorsqu'il s'agit de penser l'évolution humaine en des termes différents de ceux que l'on utilise pour penser le débat général de l'évolution au XIX^e siècle. Autrement dit, les considérations sur le passé évolutif de l'homme ne sont pas exclusivement un sous-produit ou une résultante de la nouvelle conception évolutive du monde, mais elles sont également régies par un impératif épistémologique différent qui, en apparence seulement, leur donnent des allures non conformes à la nouvelle vulgate évolutionniste.

Ce décalage, au XIX^e siècle, entre le débat général au sujet de l'évolution d'une part et de son application en évolution humaine d'autre part, a donné lieu à de nombreuses interprétations pour expliquer ce prétendu retard en Paléontologie humaine. La très grande majorité de ces interprétations se réduisent à un petit corpus d'idées toutes reliées les unes aux autres, et semblent reposer sur la base suivante.

Des scientifiques comme Darwin, Huxley et Haeckel occuperont une place de choix au sein de l'historiographie de la Biologie évolutionniste, pour cause d'ailleurs, puisque ceux-ci seront des émissaires de premier plan en faveur du transformisme. Qui plus est, ils aborderont également de plein front la question de l'évolution de l'homme, leur donnant ainsi une ascendance certaine au titre de fondateur de la Paléontologie humaine, puisqu'il semble logique *a priori* de faire découler entièrement l'évolution humaine du débat général de l'évolution. De cette perspective, la promotion qu'ils feront d'un lien étroit entre l'homme et les singes anthropoïdes, ainsi que de l'ascendance simienne de l'homme, sur la base de l'embryologie et de l'anatomie comparée, fournira une matrice interprétative naturelle pour comprendre la naissance de la Paléontologie humaine.

Voyons cinq interprétations de ce corpus d'idées défendues récemment et qui s'inscrivent dans cette suite :

1) Les opinions de scientifiques tels que Darwin, Huxley et Haeckel au sujet de l'évolution humaine constituent l'essentiel des évènements en Paléontologie humaine lors de la période 1860-1895 ; les découvertes de restes humains anciens et fossiles faites à l'époque étant trop limitées et peu pertinentes (Wendt, 1971 : 58, 77, 91-92 ; Jolly, 1972 : 1-6 ; Reader, 1981 : 36 ; Cartmill *et al.*, 1986 : 410 ; Theunissen, 1989 : 8-12).

2) Les découvertes de restes humains anciens et fossiles doivent être insérées à même les conceptions de scientifiques tels Darwin, Huxley et Haeckel. La Paléontologie humaine, dans cette perspective, est une entreprise à la recherche du « chaînon manquant » entre l'homme et une forme simiesque quelconque (Kennedy, 1960 : 8, 10 ; Stewart, 1959 : 23 ; Wendt, 1971 : 132). Les fossiles dits intermédiaires manquant à l'appel, la Paléontologie humaine entre 1860 et 1895 est alors essentiellement une discipline sans fossiles, ou du moins sans fossiles réellement importants pour éclairer l'évolution humaine (Weiner, 1959 : 32-33 ; Kennedy, 1960 : 9 ; Wendt, 1971 : 91 ; Reader, 1981 : 36 ; Cartmill *et al.*, 1986 : 410 ; Laurent, 1989 : 122 ; Theunissen, 1989 : 8-12 ; Richard, 1993 : 65 ; Tattersall, 1995 : 20-23).

3) Des commentateurs récents décrivent les découvertes de restes humains anciens et fossiles faites entre 1860 et 1895 qu'en nombre plutôt limité et de façon fort sélective (Hublin, 1989 ; Reader, 1981 : 20-36 ; Tattersall, 1995 : 13-30 ; Trinkaus et Shipman, 1993 : 46-133). Les spécimens anciens et fossiles tels que Engis, Néandertal, Gibraltar, Moulin-Guignon, la Naulette, Cro-Magnon, Spy et quelques autres encore sont souvent mentionnés. De plus, on ne mentionne souvent que les types de Néandertal et de Cro-Magnon, alors que d'autres groupes anciens avaient été envisagés à l'époque. Ces deux tendances amènent certains observateurs récents à considérer que la découverte du crâne de Néandertal, en Allemagne, en 1856, constitue l'acte fondateur de la Paléontologie humaine, car celle-ci aurait forcé une réflexion au sujet de l'évolution humaine alors que se constituait au même moment le débat général sur l'évolution. D'autres encore considèrent ce spécimen fossile comme étant peut-être le seul fossile humain découvert à l'époque pouvant fournir des éléments de réponse sur l'évolution humaine (McCown et Kennedy, 1972 : 95 ; Reader, 1981 : 36 ; Tattersall, 1995 : 21 ; Trinkaus et Shipman, 1993 : 3-7, 46).

4) Puisque plusieurs commentateurs récents défendront que le crâne de Néandertal, découvert en Allemagne en 1856, constitue peut-être la seule pièce pouvant nous donner des indications au sujet de l'évolution humaine, certains d'entre eux maintiendront également que les scientifiques de l'époque qui refusaient d'envisager cette possibilité en prétendant que cette pièce était récente, d'apparence moderne ou de conformation pathologique, étaient vraisemblablement des anti-évolutionnistes, alors que ceux qui reconnaissaient son ancienneté et certains traits dits primitifs étaient de véritable évolutionnistes (Hublin, 1989 : 158 ; Leguebe, 1986 : 19 ; McCown et Kennedy, 1972 : 93-94 ; Reader, 1981 ; 25-33 ; Spencer, 1984 : 2, 7 ; Trinkaus et Shipman, 1993 : 53-60 ; Wendt, 1971 : 56-58).

5) Enfin, certains commentateurs récents défendront que beaucoup des scientifiques qui tenteront d'organiser les restes humains anciens et fossiles au cours de la deuxième moitié du XIX^{e} siècle le feront dans une perspective non évolutionniste, parce que ceux-ci les disposeront au sein de cadres concernant la succession des « races » plutôt que des « espèces », cette dernière considération seulement étant véritablement évolutionniste (Leguebe, 1986 : 19 ; Spencer, 1984 : 10-11 ; Tattersall, 1995 : 15 ; Trinkaus et Shipman, 1993 : 109-110, 119).

Ce corpus d'idées au sujet de l'histoire de la Paléontologie humaine au XIX^{e} siècle, nous l'avons vu, est largement répandu dans une littérature plus ou moins spécialisée. Mais il se trouve qu'il le soit également dans la littérature éducative et de vulgarisation, au sein de ce que Kuhn (1970) considère comme le médium par excellence d'apprentissage pour le métier de chercheur (*cf.* Leakey et Lewin, 1977 : 31-32 ; 1985 : 32-34 ; Leakey, 1981 : 145-147 ; Johanson et Edey, 1981 : 27-30 ; 1983 : 37-40 ; Nelson et Jurmain, 1985 : 520-521 ; Campbell, 1988 : 23, 372-376 ; Poirier *et al.*, 1990 : 388). Ce corpus d'idées, en d'autres termes, constitue un peu la position historiographique officielle des paléoanthropologues contemporains puisqu'essentiellement identique à la fois dans la littérature spécialisée et non spécialisée. Cette concordance un peu inquiétante, il faut le préciser, est peut-être le signe d'un manque de renouvellement de l'historiographie de la Paléontologie humaine.

Il nous faut aborder une dernière interprétation qui ne s'inscrit pas à même le corpus d'idées que nous venons de présenter, quoiqu'elle y participe

indirectement par son esprit. Selon Bowler (1986), le domaine de l'évolution humaine serait largement dominé par des idées non darwiniennes, et ce jusque dans les années 1930. C'est-à-dire que l'évolution humaine serait dominée par des schèmes phylétiques décrivant l'évolution de plusieurs lignées en parallèle, alors que le schème darwinien se caractériserait par des lignées divergentes. Mais il semble que Bowler ne puisse appliquer concrètement ses théories dites non darwiniennes à l'évolution humaine qu'à partir surtout du XX^{e} siècle. Ce faisant, Bowler conforte indirectement le corpus d'idées présenté plus haut de deux manières. D'abord en faisant largement découler le domaine de l'évolution humaine du débat général de l'évolution, si ce n'est que par l'intermédiaire des théories non darwiniennes, et ensuite en contribuant implicitement à l'idée selon laquelle très peu de réalisations concrètes se sont produites en évolution humaine au cours de la deuxième moitié du XIX^{e} siècle.

Nous proposerons, dans ce qui suit, une alternative à ce corpus d'idées.

Mais avant, mentionnons que l'historiographie de la Biologie évolutionniste nous révèle le rôle marginal qu'a joué la France dans le développement de la théorie moderne de l'évolution (Boesiger, 1980 ; Bowler, 1983 : 107-117 ; Limoges, 1980 ; Roger, 1983, 1995). Il est ainsi souvent présumé que cette situation aurait trouvé également écho au sein de la Paléontologie humaine française lors de la deuxième moitié du XIX^{e} siècle (e.g., Conry, 1974 : 51-89). En réalité, comme nous le verrons, les chercheurs de langue française ont joué un rôle prépondérant dans une facette souvent négligée de l'histoire de la Paléontologie humaine.

LES ORIGINES MULTIPLES DE LA PALÉONTOLOGIE HUMAINE

Pour comprendre la naissance de la Paléontologie humaine vers 1860, il semble qu'il faille lui reconnaître au moins trois sources au cours de la première moitié du XIX^{e} siècle : 1) l'ethnogénie européenne, 2) le débat sur le monogénisme/polygénisme et 3) le débat qui consiste à organiser les nouvelles données provenant de la Paléontologie, l'Embryologie et la Systématique, et que nous reconnaîtrons arbitrairement sous l'appellation de débat sur « l'évolutionnisme ». Ces origines multiples de la Paléontologie humaine forme en réalité une sorte de réseau complexe constitué d'éléments empiriques, théoriques, conceptuels et méthodologiques. Ce réseau est une claire indication à l'effet que la connaissance scientifique est souvent un processus de nature réticulée. Considérons d'abord ces deux dernières sources.

L'acceptation par un nombre croissant de chercheurs après 1860 que l'homme est un produit de l'évolution amènera la fusion de ces deux sources. D'une part, certains scientifiques réinterpréteront le débat sur le monogénisme/polygénisme en des termes évolutionnistes, alors que d'autres poursuivront ce débat mais dans un contexte évolutionniste. Cette distinction, peut-être subtile à première vue, est une indication que les débats sur le monogénisme/polygénisme ainsi que celui sur « l'évolutionnisme » étaient, avant 1860, souvent menés indépendamment l'un de l'autre. Par contre, et ce fait est important, certains auteurs harmoniseront, avant 1860, les deux débats, c'est-à-dire que leur conception du règne animal s'appliquera également à l'homme.

Par exemple, s'il est vrai que Buffon est monogéniste, il est également vrai qu'il conçoit l'homme de la même façon que les animaux. Certes, Buffon instaure entre l'humanité et l'animalité une frontière infranchissable (Tinland, 1992). Néanmoins, l'explication de la formation des nombreuses variétés dans l'espèce humaine que Buffon donne, dès 1749, dans son *Histoire naturelle, générale et particulière* (1749-1789 avec les suppléments), soit celle où le climat au sens général du terme est agent modificateur par l'intermédiaire de la dégénération, est également appliquée aux animaux. De plus, Buffon inscrira plus tard l'homme et les animaux au sein d'une véritable histoire de la nature où des types primordiaux apparurent séparément sur terre par une sorte de génération spontanée pour ensuite disparaître ou se perpétuer en se modifiant dans les limites du type. C'est ainsi que Buffon réduit les deux cents espèces de quadrupèdes qu'il décrit, d'une part à vingt-cinq types composés chacun de plusieurs espèces liées de façon phylétique entre elles, et d'autre part à treize types composés d'espèces isolées et dont l'homme lui-même fait partie. N'étant pas transformiste, Buffon n'instaure pas de liens phylétiques entre ces trente-huit types de quadrupèdes (Roger, 1982, 1992).

Pour le transformiste Lamarck, la vie est dotée d'une force inhérente de modification, par la combinatoire d'une tendance à la complexification de l'organisation ainsi que d'une diversification adaptative par l'intermédiaire de l'hérédité de l'acquis. Pour Lamarck, l'homme est un produit de la chaîne animale ; chaîne qu'il décrira d'ailleurs comme

étant de plus en plus ramifiée entre 1800 et 1820 (Laurent, 1975). Mais même si Lamarck reconnaît que l'homme possède l'organisation la plus élevée d'entre toutes, et qu'il soit parvenu à creuser avec ses plus proches compétiteurs, les quadrumanes, une distance considérable, Lamarck fera néanmoins découler l'homme et ses multiples formes du processus général de la vie ; l'homme se pose en continuité avec lui, si ce n'est qu'à un degré qualitativement supérieur (Bourdier, 1971 ; Szyfman, 1982 : 237-240). Dans cette optique, Lamarck décrira clairement dans sa *Philosophie zoologique* (1809) ainsi que dans son *Système analytique des connaissances positives de l'homme* (1820), le processus d'anthropogenèse par lequel l'homme est passé pour acquérir sa forme actuelle, pour ensuite décrire la ramification de l'espèce humaine en six races géographiques à partir d'une seule d'entre elles.

Si Lamarck professe un transformisme de type matérialiste, Chambers, lui, défend un transformisme de source spiritualiste. Pour Chambers, dans ses *Vestiges of the Natural History of Creation* (1844), l'histoire de la vie à travers les âges se pose comme un développement progressif régi par une loi naturelle mais programmée à l'avance par le Créateur, à la manière du développement embryologique et ontogénétique, et dont l'homme en constitue à la fois le produit et l'apex (Bowler, 1976 : 53-62 ; Hodge, 1972). Si Chambers reconnaît que les conditions extérieures doivent être favorables pour permettre le processus de la transmutation, la finalité ou la direction de la transmutation est, pour l'essentiel, prédéterminée par le plan original. L'homme, de ce point de vue, est traversé d'un progressionnisme qui, selon Chambers, porte le type Indo-Européen au sommet de la forme humaine, alors que les autres formes humaines stagnent ou dégénèrent, et cela, malgré le fait qu'elles soient toutes dérivées d'une seule et même forme, probablement quadrumane. Si l'homme présente une grande variabilité, comme en témoigne les multiples variétés humaines que Chambers regroupent en cinq races principales, c'est que la plupart d'entre elles n'ont pas encore recontré les circonstances appropriées pour progresser.

Pour Agassiz, le créationniste, l'histoire de la vie sur terre s'inscrit dans le développement ou le déroulement progressif d'un plan divin dont l'homme est à la fois l'aboutissement et la finalité ; plan ponctué de créations miraculeuses et distinctes qui trouve son expression divine dans la triple parallélisation de l'embryologie, la paléontologie et la systématique des formes vivantes, spécialement chez les vertébrés. Si les espèces ne sont pas physiquement liées entre elles, elles le sont du moins dans l'esprit du Créateur qui a conçu le plan (Bowler, 1976 : 45-53 ; Laurent, 1987 : 305-317). Agassiz défendra ses opinions polygénistes en reconnaissant que les huit grandes divisions de l'humanité, tout comme les animaux et les plantes, ont été créées immuables et indépendamment les unes des autres, étant confinées à l'intérieur des huit provinces zoologiques que comporteraient le globe terrestre (Lurie, 1960 : 256-265 ; Stanton, 1960 : 100-108).

Il semble logique d'assumer, à première vue, que la rencontre de l'évolutionnisme et de la reconnaissance de l'antiquité de l'homme, vers 1860, constitue une condition nécessaire à la naissance de la Paléontologie humaine. Des auteurs comme Agassiz, Buffon, Chambers et Lamarck nous démontrent, au contraire, qu'il est possible de penser l'histoire naturelle de l'homme indépendamment d'une démonstration de son antiquité, et cela dans des perspectives évolutionnistes et non évolutionnistes. Nous ne saurions, dans ce cas, accorder une trop grande importance à la conception discontinuiste de la naissance de la Paléontologie humaine.

Si certains auteurs, au cours de la première moitié du siècle, ont présenté une vision unifiée au sein de laquelle l'homme et les animaux figurent parmi un système unique et identique de genèse, d'autres laisseront la question de l'homme en suspens. Pour Lyell (Bartholomew, 1973), Owen (Desmond, 1985) et Pictet (Laurent, 1987 : 457) la perspective d'une genèse matérialiste et évolutionniste de l'homme constituera un obstacle majeur à l'élaboration d'une conception de la genèse du règne animal en des termes équivalents. Pour Darwin (Durant, 1985 ; Herbert, 1977), Étienne Geoffroy Saint-Hilaire et Gérard (Laurent, 1987 : 350, 394-395) l'homme est bel et bien le produit d'un évolutionnisme généralisé, mais ceux-ci garderont le silence sur cette question et mettront en place une conception évolutionniste qui repose toute entière sur le règne animal. La question de l'homme est, de ce point de vue, souvent lourde de conséquence pour les conceptions de l'époque. Il est donc facile de comprendre certaines des raisons qui ont encouragé certains chercheurs à dissocier le débat sur « l'évolutionnisme » de celui sur le monogénisme/polygénisme.

Il n'est pas opportun ici d'insister sur le débat portant sur « l'évolutionnisme » au cours de la première moitié du XIX[e] siècle, si ce n'est que pour mentionner quelques tendances générales. Les

faits provenant de l'Anatomie comparée, la Systématique, l'Embryologie et la Tératologie, ainsi que de la Géologie et la Paléontologie, joueront un rôle grandissant dans l'explication de la genèse de la vie sur terre. À partir de ces données communes, au moins quatre grandes interprétations sur des fonds épistémologique, philosophique et théologique divergents seront proposées : 1) la création unique, 2) la création répétée, 3) l'évolutionnisme spiritualiste et 4) l'évolutionnisme matérialiste.

Si les partisans de Cuvier ou d'une théologie naturelle de type téléologique pourront défendre une conception discontinuiste de la vie lors des premières décennies du XIX^e^ siècle, par l'intermédiaire de la création répétée, les données provenant de la Paléontologie et de l'Embryologie, après 1840, rendront cette conception difficilement tenable au profit d'une continuité apparente et complexe (Bowler, 1976 ; Laurent, 1987).

Également, l'interprétation de la création unique qui stipule que toutes les formes vivantes sur terre ont été créées dès le début des temps pour ensuite graduellement disparaître dans une conception d'appauvrissement des formes au cours des âges, verra la Paléontologie du milieu du siècle être incapable de confirmer cette conception pour faire place, au contraire, à un certain progressionnisme (Appel, 1980 ; Laurent, 1978).

En réalité, les évolutionnismes matérialistes et spiritualistes s'imposeront comme étant les seules alternatives véritablement prometteuses à partir du milieu du XIX^e^ siècle. Le grand débat sur le darwinisme, lors de la décennie de 1860, débouchera sur l'acceptation majoritaire d'un évolutionnisme non darwinien, et confirmera la cohabitation et la persistance d'évolutionnismes matérialistes et spiritualistes jusque dans les dernières décennies du siècle.

Il n'est pas non plus opportun ici de présenter les vicissitudes du débat sur le monogénisme/polygénisme lors de la première moitié du XIX^e^ siècle (*cf.* Blanckaert, 1996 ; Grayson, 1983 : 139-167 ; Poliakov, 1971 ; Popkin, 1978-79). Ce débat, qui se penchait sur la question de l'origine de l'humanité à partir d'une ou plusieurs sources, constituait un cadre naturel pour penser la diversité humaine en terme généalogique. Ce cadre, d'ailleurs, puisait à même le débat déjà vieux de quelques siècles qui portait sur l'interprétation à donner au récit de la genèse biblique. Il est impossible, dans ces circonstances, que la naissance de la Paléontologie humaine au milieu du XIX^e^ siècle ne puisse avoir bénéficiée de ces considérations généalogiques antérieures.

Nous avons déjà eu l'occasion de mentionner que le débat sur le monogénisme/polygénisme sera, après 1860, réinterprété en des termes évolutionnistes ou encore reconduit dans un contexte évolutionniste. Nous retrouvons ici la jonction du débat sur « l'évolutionnisme » et sur le monogénisme/polygénisme comme deux des sources ayant contribué à la naissance de la paléontologie humaine. Nous avons évoqué comment ces sources, parfois indépendamment, parfois en association, ont cheminé jusqu'en 1860. Retenons pour le moment que ces deux sources contribueront à la Paléontologie humaine par leur apport théorique essentiellement, c'est-à-dire qu'il est défendu ici que pour comprendre la Paléontologie humaine après 1860, il nous faut lui reconnaître d'une part, des considérations d'ordres théoriques, et d'autre part, des considérations reliées à sa documentation.

Cette dernière facette de la Paléontologie humaine, sa documentation, tire son origine d'une troisième source : la généalogie des peuples européens ou l'ethnogénie européenne. L'interprétation ethnogénique la plus répandue, lors de la première partie du XIX^e^ siècle, est incontestablement celle promue par les philologistes qui stipulent que la plupart des peuples européens actuels, qui sont de langues indo-européennes ou aryennes, sont des immigrants d'une souche en provenance d'Asie (Poliakov, 1971). Les autres peuples européens de langues non-aryennes et parfois affiliés à une souche mongoloïde ou touranienne, étaient considérés comme les vestiges d'une époque antérieure à la migration aryenne.

Ce modèle ethnogénique où le peuplement de l'Europe connaît deux moments ethniques forts, trouvera une confirmation nouvelle lorsqu'au cours des années 1840 et 1850 Anders Retzius défendra que les peuples non-aryens auront des crânes de conformation brachycéphale alors que les peuples aryens ou celtiques seront de conformation dolichocéphale (Blanckaert, 1989). Ce modèle impliquera, pour la découverte de restes humains anciens en Europe, que les brachycéphales étaient plus anciens que les dolichocéphales, fournissant ainsi une matrice interprétative toute réglée pour une Paléontologie humaine naissante.

Les considérations au sujet de l'ethnogénie européenne au cours de la première moitié du XIX^e^ siècle seront certes souvent liées au débat sur le monogénisme/polygénisme. Soulignons d'abord trois développements d'ordre méthodologique et conceptuel fréquemment soulevés lors de ce dernier débat avant 1860. Le premier consistera à remplacer la conception voulant que les races

humaines soient dotées d'une grande souplesse d'adaptation et de possibilité d'altération par une conception décrivant la race comme une entité rigide et permanente. Les rapports de filiations sont, pour les Buffoniens, moins déterminants pour expliquer la ressemblance des formes que l'adaptation à des conditions similaires (Duchet, 1977 : 219-220). Pour d'autres, par contre, largement sous l'impulsion grandissante du polygénisme en début de siècle, le caractère inaltérable d'une race, au-delà des variations dites superficielles causées par les conditions extérieures ou introduites par les « lois » d'hybridation et de croisement, est gage d'identité généalogique (Blanckaert, 1988 ; Stanton, 1960 ; Stocking, 1973).

Le deuxième développement, essentiellement un corollaire du premier, consistait à remplacer les déductions généalogiques reposant à la fois sur la philologie, les traits culturels et les caractères morphologiques, par des déductions généalogiques entièrement construites à l'aide de données morphologiques. Cette transition sera l'indication d'un glissement d'une anthropologie éclectique aux sources multiples vers une anthropologie plus retreinte dite physique ou biologique. Dans ce contexte, les caractères crâniens seront particulièrement valorisés ; la crâniométrie connaîtra un essort important à cette époque (Blanckaert, 1987, 1989, 1990 ; Gould, 1981 : 30-112 ; Stanton, 1960 : 31-32).

Enfin, le troisième développement consistera à substituer aux généalogies établies à l'aide de données récentes et existantes, des généalogies reposant sur des données historiques et préhistoriques ou paléontologiques ; démarche qui ne verra son véritable envol qu'à partir du milieu du siècle.

Ces développements méthodologiques et conceptuels auront un impact à la fois sur le débat portant sur l'ethnogénie européenne ainsi que celui sur le monogénisme/polygénisme, les liant à travers ces changements. De plus, ces deux derniers débats seront liés par leur objet similaire d'étude, c'est-à-dire la généalogie humaine, quoiqu'ils portent sur des échelles différentes puisque l'un a des prétentions d'une généalogie humaine globale et mondiale et l'autre restreinte et locale (l'Europe). On pourrait ainsi défendre, sans avoir complètement tort, que l'ethnogénie européenne n'est que la portion européenne du débat sur le monogénisme/polygénisme. Mais au-delà de ces rapprochements évidents, il est clair que la différence d'échelle pèsera lourd et entraînera une rupture épistémologique : Broca le polygéniste et de Quatrefages le monogéniste en arriveront à partager, au cours des années 1870, une conception semblable de l'ethnogénie européenne comme nous le verrons plus tard.

L'opposition entre le vide empirique causé par l'acceptation de la grande antiquité de l'homme pour le débat sur le monogénisme/polygénisme d'une part, tel que mis au jour par l'archéologie préhistorique, et les découvertes de plus en plus nombreuses de restes humains plus ou moins anciens pour l'ethnogénie européenne d'autre part, entraînera cette divergence.

Après 1860, le débat sur le monogénisme/polygénisme en sera de plus en plus réduit à fournir une conception *théorique* de la généalogie humaine, faute de ne pouvoir y contribuer significativement par les faits ; perspective réaliste mais combien lointaine. L'ethnogénie européenne, quant à elle, pourra contribuer concrètement à *documenter* la généalogie humaine dans cette portion limitée du globe.

Dans ces circonstances, le passage entre le monogénisme/polygénisme d'avant 1850 et les phylogénies humaines d'après 1900 ne se produira pas nécessairement directement comme il serait tentant de le penser à la lecture de certains travaux (e.g., Vallois, 1952 ; Wolpoff et Caspari, 1997).

Tournons-nous maintenant vers cette Paléontologie humaine caractérisée à la fois par ses considérations théoriques et celles reliées à sa documentation.

UNE PALÉONTOLOGIE HUMAINE BI-DIMENSIONNELLE

Loin d'être en retard sur le fait évolutionniste, la Paléontologie humaine sera, dès les années 1860, vigoureusement conçue et pensée en ces termes. Par contre, cette vigueur évolutionniste sera contrainte à s'exprimer par le truchement de débats théoriques. La Paléontologie humaine lors de la deuxième moitié du XIX^{e} siècle ne sera pas exempte de données empiriques ; seulement, ces dernières seront inappropriées pour répondre aux questions pressantes des évolutionnistes de l'époque. Seul un nombre limité de restes humains anciens et fossiles seront mis à contribution, et encore en n'y reconnaissant que quelques caractères épars dits primitifs, et non des formes intermédiaires entières entre l'homme moderne et un ancêtre quelconque. L'ensemble des nombreux spécimens anciens et fossiles découverts lors de cette période se présenteront comme des formes humaines modernes ou plutôt modernes. Les questions évolutionnistes pressantes devront être abordées indirectement par

l'intermédiaire de l'Anatomie comparée et l'Embryologie, et à l'occasion à l'aide de la Paléontologie des mammifères et de l'Archéologie préhistorique. Trois débats ou thèmes plus ou moins distincts les uns des autres généreront ces considérations que nous qualifions de théoriques.

Les généalogies globales

Le premier d'entre eux concerne le lien taxonomique entre l'homme et les primates ; débat aux implications phylogénétiques, malgré le fait que la relation taxonomie/phylogénie ne soit pas obligatoirement directe. D'une part, selon Huxley (1896 [1863] : 146-147), Dally (1868 : 674), Darwin (1871 : 196-197), Topinard (1888 : 329-331) et Wallace (1889 : 456), le lien taxonomique relativement étroit que l'homme entretient avec les primates, spécialement les primates anthropoïdes, implique une ascension simienne de l'homme. D'autre part, le lien taxonomique distant implique une relation phylogénétique lointaine. Il faut préciser d'abord que plusieurs scientifiques qui défendront un lien taxonomique distant, à la fin des années 1850 et au cours des années 1860, seront anti-évolutionnistes (*cf.* Blanckaert, 1995). D'autres, par contre, seront évolutionnistes. Même si Owen récusait une ascension simienne de l'homme, dès les années 1830 (Desmond, 1985), ce fait n'est pas contradictoire avec la classification de l'homme dans sa propre sous-classe ainsi qu'avec la défense simultanée d'une forme d'évolutionnisme (Owen, 1857, 1868 : 799-809). Pour Mivart (1873), les ressemblances entre l'homme et les diverses formes primates sont probablement attribuables à l'acquisition indépendante de caractères par voie d'évolution parallèle plutôt que par cause de descendance commune (Bowler, 1986 : 114-115). Enfin, si de Quatrefages ne peut être qualifié d'évolutionniste, ayant rejeté l'ensemble des hypothèses de son époque, on ne peut le classer parmi les anti-évolutionnistes. De Quatrefages (1877) nous présente une vision complexe et instruite du monde animé avec ses causes connues de modifications intraspécifiques et ses causes toujours inconnues, de l'observation directe et de l'expérimentation, responsables de la succession des espèces dans le temps. Reconnaissant d'une part les différences structurelles et spirituelles entre le singe et l'homme, justifiant l'érection d'un règne humain, et d'autre part leurs rapprochements anatomiques, physiologiques et embryologiques favorisant sur certains points l'hypothèse d'une souche commune, de Quatrefages (1894 : 192-194) se verra incapable d'aller plus loin qu'une acception toute théorique de cette disposition généalogique, faute de faits, et malgré qu'il semble peut-être envisager une solution éventuelle qui tendrait vers la reconnaissance d'un ancêtre commun non anthropoïde et même non simien (Laurent, 1995 : 163).

Le deuxième débat ou thème qui nous préoccupe ici est celui de la reconstruction hypothétique des ancêtres de l'homme. Si Darwin (1871 : 206-207) nous propose une image d'un ancêtre commun à l'homme et aux primates de l'ancien monde en le décrivant comme un petit primate arboricole, c'est plutôt le processus d'anthropogenèse par lequel cet ancêtre est passé pour devenir humain qui intéresse Darwin (1871 : 140-151). Bien connu est le scénario où un primate arboricole forcé de vivre au sol connaîtra des transformations en cascade : adaptation corporelle à une locomotion bipède, libération des mains et fabrication d'outils, réduction du prognathisme et des dents, et enfin développement du cerveau et des facultés mentales. Ce scénario n'est pas sans rappeler celui de Lamarck (*cf.* Laurent, 1989). Insatisfait des reconstructions antérieures de l'ancêtre de l'homme, Hovelacque (1877 ; *cf.* Richard, 1993) exploitera systématiquement l'anatomie comparée entre les primates anthropoïdes et les races humaines pour présenter en détail le précurseur direct de l'homme au Tertiaire. Ainsi, l'ancêtre de l'homme présenterait à la fois des brachycéphales et des dolichocéphales, une capacité cervicale intermédiaire entre l'homme moderne et les anthropoïdes, un torus supraorbital développé tel qu'attesté par les restes humains de Néandertal, Eguisheim, Engis et Gibraltar, un prognathisme évident et une mâchoire simiesque comme la mandibule fossile de la Naulette, une bipédie imparfaite, etc. Enfin, pour de Mortillet (1883 ; *cf.* Pautrat, 1993), la conception du précurseur de l'homme ne découle pas tant de l'Anatomie comparée que de la Paléontologie des mammifères et de l'Archéologie préhistorique. En combinant le renouvellement des faunes, qu'il croit reconnaître au cours des époques géologiques tertiaires et quaternaires, avec la progression des industries lithiques au cours de la même période qu'il utilise comme un miroir de la transformation biologique de l'homme, de Mortillet en arrive à concevoir un précurseur hypothétique de l'homme au Tertiaire sous l'appellation générique d'*Anthropopithecus*, soit une sorte de grand singe anthropoïde. Sur la base de telles distinctions géologiques et lithiques uniquement, de Mortillet (1883 : 105-106) iden-

tifie pas moins de trois espèces différentes d'anthropopithèques au Tertiaire.

Le troisième débat ou thème qui nous intéresse est celui qui porte sur la relation des diverses races humaines dans le temps. D'entrée de jeu les protagonistes s'accordent tous à reconnaître la persistance des races dans la durée, comme en témoignent les restes humains déjà découverts au Quaternaire. D'une part, des chercheurs comme Wallace (1864), Huxley (1896 [1865]), Darwin (1871 : 235) et Topinard (1890 : 531) reconnaîtront la désuétude du débat sur le monogénisme/polygénisme et lui substitueront la perspective évolutionniste du monophylétisme/polyphylétisme. Wallace, Huxley et Darwin favoriseront cette première interprétation, Topinard possiblement la deuxième. D'autre part, Pouchet (1864), Hunt (1866), Broca (1870) et de Quatrefages (1877) maintiendront leur position dans le débat sur le monogénisme/polygénisme, mais portée dans un contexte évolutionniste. Pouchet, Hunt et Broca favoriseront le polygénisme, et de Quatrefages le monogénisme.

Enfin, si de nombreux auteurs contribueront de façon distincte aux trois débats ou thèmes présentés ici, d'autres utiliseront une combinaison de ceux-ci. Pour Vogt (1864 : 461-468), la proximité taxonomique entre l'homme et les singes anthropoïdes implique un lien phylétique étroit ; lien de nature polyphylétique où chaque grande race humaine est étroitement liée à une forme anthropoïde particulière plus qu'à une autre race humaine. L'œuvre d'un processus de convergence au cours du temps serait responsable de la proximité relative des races humaines actuelles. Selon Schaaffhausen (1868), tout comme Vogt, l'anatomie comparée, l'embryologie ainsi que certains caractères morphologiques de restes humains anciens fournissent des preuves convaincantes du lien étroit entre l'homme et les anthropoïdes ; lien que Schaaffhausen décrit également en des termes polyphylétiques similaires à ceux de Vogt. Enfin, la conception de Haeckel (1876), qui s'inscrit dans la même perspective que celle de Vogt et Schaaffhausen, n'en diffère que par son monophylétisme (1876 : 303-304). Même si Schaaffhausen (1868) a brièvement décrit la forme crânienne hypothétique du précurseur de l'homme, Haeckel (1876 : 293-301) sera beaucoup plus précis sur cette forme ancestrale. En effet, il proposera que les diverses races humaines sont issues d'une forme anthropomorphe transitoire nommée Pithécanthrope ; comme Lamarck et Darwin, Haeckel insistera sur le redressement de cette créature, la libération des mains, la réduction du prognatisme et de sa pilosité, ainsi que du développement de ses capacités cervicales.

Il est donc clair que la Paléontologie humaine de la deuxième moitié du XIX^{e} siècle sera vigoureusement conçue en des termes évolutionnistes. Il faut chercher ailleurs que dans l'anti-évolutionnisme *la cause principale* qui a poussé de nombreux chercheurs à s'intéresser aux liens préhistoriques entre les races d'Europe plutôt qu'à établir les relations phylétiques entre l'homme et les animaux ou encore entre les grandes divisions de l'humanité. La réponse n'est pas à proprement parler dans le manque de données empiriques ; cette façon de voir, sans être fausse, constitue une mauvaise conception qui a induit en erreur de nombreux historiens de la Paléontologie humaine. Non, il semble plutôt qu'il faille postuler la nature particulière du matériel empirique disponible plus que son absence.

Les généalogies restreintes

Nous avons déjà vu que le modèle ethnogénique européen le plus répandu, au cours des années 1850, était celui qui stipulait un peuplement de l'Europe en deux temps ethniques forts : un peuplement ancien brachycéphale non-aryen et une invasion postérieure dolichocéphale aryenne. Ce modèle en deux temps constituera un cadre tout désigné pour interpréter les découvertes de restes humains anciens et fossiles de plus en plus nombreux après 1860, quoique l'antériorité du type brachychéphale sur le type dolichocéphale ne sera pas toujours maintenue.

Selon Schaaffhausen (1861), le crâne dolichocéphale de Néandertal découvert en Allemagne en 1856, est celui d'un individu qui provient de l'une des races du Nord-Ouest de l'Europe, et serait antérieur à l'arrivée des Celtes et des Germains (les Aryens). Même si Schaaffhausen reconnaît que ce spécimen présente une conformation particulière qui diffère complètement des races « barbares » actuelles et des découvertes faites jusque-là, celui-ci le rapproche néanmoins des autres restes crâniens découverts en Allemagne (Schwaan et Plau) et en Russie (Minsk) ; vestiges d'anciennes populations européennes affiliées à celles d'Angleterre, d'Irlande et de Scandinavie.

Pour Pruner-Bey (1863, 1866a, 1866b, 1867), la race préhistorique la plus ancienne d'Europe est brachycéphale et de souche mongoloïde ; elle serait représentée par les restes d'Arcy, Aurignac, Bruniquel, Furfooz, la Naulette et Solutré. L'autre

race préhistorique d'Europe moins ancienne serait dolichocéphale et celtique ; les restes d'Engis, Lombrive, Moulin-Guignon, Néandertal et Vauréal la représenteraient. Après la découverte des restes à Cro-Magnon, que Pruner-Bey (1868a, 1868b) inclura dans le type ancien mongoloïde malgré leur dolichocéphalie évidente, ce dernier défendra d'abord que leur dolichocéphalie diffère de celle des Celtes, et qu'ensuite la dichotomie brachycéphale/dolichocéphale ne devrait être qu'un critère morphologique parmi d'autres pour établir des filiations.

Partageant essentiellement la vision ethnogénique de Pruner-Bey, de Quatrefages (1867 : 257-270, 480-487) acceptera l'attribution du crâne de Néandertal à un Celte, ainsi que l'antériorité générale des brachychéphales sur les dolichocéphales. Par contre, tout comme Pruner-Bey, de Quatrefages se questionnera sur la possibilité d'un peuplement mongoloïde ancien de l'Europe par une source polymorphe, c'est-à-dire comprenant à la fois des races brachycéphales et des races dolichocéphales, expliquant ainsi peut-être la présence de crânes dolichocéphales dans des dépôts apparemment anciens à Engis et Eguisheim. La même interprétation d'une source aryenne ou celtique polymorphe pouvait certes s'appliquer. Cette nouvelle conception d'un peuplement européen en deux temps ethniques mais de souches polymorphes mongoloïde et aryenne se délestait du carcan rigide de la supposée antériorité du type brachycéphale sur le type dolichocéphale ; ajustement approprié aux critiques de plus en plus nombreuses formulées à l'époque.

Cette conception ethnogénique de l'Europe devait être délaissée définitivement vers le début des années 1870. Déjà en 1863, Thurnam (1863-64a) attribuait aux plus vieilles populations de l'âge de pierre d'Angleterre une origine ibérienne et africaine, et non mongoloïde et asiatique. Puis l'année suivante, Thurnam (1863-64b), face à l'accumulation de découvertes de restes humains anciens d'Europe à Engis, Furfooz, Lombrive et Néandertal, se questionnera sur la possibilité de lier directement les types anciens dolichocéphale et brachycéphale d'Angleterre non aux peuples proto-historiques d'Europe et d'Afrique du Nord mais plutôt aux formes réellement anciennes de l'Europe de l'Ouest.

Une nouvelle conception ethnogénique devait naître et Broca, sans proposer une alternative précise, en indiquera l'esprit général. Il est vrai que Broca maintiendra l'antériorité du type brachycéphale sur le type dolichocéphale jusqu'en 1862. Après cette date, par contre, Broca (1864a, 1864b, 1867) défendra que l'Europe a été habitée depuis longtemps par de nombreuses races autochtones brachycéphales et dolichocéphales auxquelles sont venues graduellement se mélanger et se fondre, plus tard, les races dolichocéphales aryennes ou celtiques. Ce faisant, l'infériorité démographique des Aryens n'aurait pas altéré significativement les types morphologiques autochtones ; au contraire, le type morphologique aryen se serait tout simplement dilué dans cette masse autochtone. En guise de preuve confirmant le polymorphisme des anciennes populations autochtones, Broca (1867, 1868) mentionnera les restes de Furfooz et Solutré pour le type brachycéphale, et ceux de Bruniquel, Cro-Magnon, Eguisheim, Engis, Lahr et Néandertal pour le type dolichocéphale.

À la conception d'une ethnogénie européenne en deux temps ethniques forts se substituera celle d'une ancienne cohabitation complexe et prolongée de plusieurs types morphologiques. Si Thurnam et Broca tendent clairement vers cette alternative, c'est à Vogt (1864 : 370-401) que l'on doit peut-être la première synthèse. Ce dernier reconnaîtra quatre races préhistoriques qui possèdent encore aujourd'hui des représentants en Europe : une première race dolichocéphale et prognathe datant de l'ancienne période de l'ours des cavernes à Engis et Néandertal, une deuxième race plutôt dolichocéphale au front élevé de la période moins ancienne du renne à Lombrive, une troisième race plutôt brachycéphale de la fin de l'âge de pierre au Danemark et représentée par le crâne de Borreby, et enfin une race mésaticéphale représentée par de nombreux vestiges datant de la période de pierre et du métal en Suisse.

Il faudra attendre les années 1870 pour voir une synthèse encore plus imposante confirmer, par une majorité de chercheurs, la conception ethnogénique européenne d'une cohabitation complexe de plusieurs types morphologiques. Cette nouvelle synthèse, proposée par de Quatrefages et Hamy (1873, 1874a, 1874b, 1875) et réunie dans la première partie de leur *Crania Ethnica* (1882), constituera pour beaucoup le point de départ des travaux ultérieurs sur l'ethnogénie européenne jusqu'à la fin du XIX^{e} siècle. En fait, cette synthèse européenne aura des visées territoriales étendues, par la suggestion soit de liens ataviques entre l'Europe du Quaternaire et de nombreux individus actuels à travers l'ancien monde ou encore d'affiliations avec certaines populations vivantes des régions australes. De cette façon, le cadre ethnogénique européen s'offre comme point de départ potentiel à une

Paléontologie humaine mondiale, et qui ne viendra réellement qu'avec le XX^e siècle.

De Quatrefages et Hamy reconnaîtront trois races ou types préhistoriques principaux. La race ancienne de Canstadt sera décrite comme étant largement distribuée en Europe, de conformation dolichoplatycéphale, prognathe, au front fuyant et au torus supraorbital plus ou moins développé ; les restes crâniens et les mâchoires suivants la composeraient : Canstadt, Eguisheim, Brüx, Néandertal, Gibraltar, Denise, Staengenaes, Olmo, la Naulette, Arcy, Clichy et Goyet. La race relativement ancienne de Cro-Magnon sera également décrite comme étant dolichocéphale mais non platycéphale, avec une morphologie faciale différente de celle de Canstadt ; les restes de Cro-Magnon, Laugerie-Basse, Bruniquel, Menton, Isola del Liri, Grenelle, Solutré, Engis, La Madeleine, Engihoul et Smermaas en feraient partie. Enfin, le troisième type moins ancien nommé Furfooz que reconnaîtront de Quatrefages et Hamy serait composé en réalité de plusieurs races brachycéphales et mesaticéphales ; les spécimens de Furfooz, La Truchère, Grenelle, Moulin-Quignon et Nagy-Sap appartiendraient à ce dernier groupe. De Quatrefages (1988 [1883] : 77) précisera plus tard que tous ces types morphologiques persisteront de façon distincte tout au long du Quaternaire malgré les croisements ; ils seront absorbés par les migrations successives de races brachycéphales et dolichocéphales qui auront lieu au cours du Néolithique et après.

Broca (1877) partagera, pour l'essentiel, la conception ethnogénique de de Quatrefages et Hamy, si ce n'est qu'il semble considérer le type de Furfooz comme étant uniquement composé de formes brachycéphales, et que la mésaticéphalie de certains restes humains anciens serait, à ses yeux, le produit de croisements entre le type brachycéphale de Furfooz et le type dolichocéphale de Cro-Magnon. Broca (1873) avait déjà auparavant contribué à établir des filiations entre certaines races préhistoriques de l'Europe et des populations actuelles ; il postulera en effet la filiation possible entre le type de Cro-Magnon, les restes de la caverne de l'Homme-Mort au Néolithique et les populations actuelles berbères et basques.

Fraipont et Lohest (1887) proposeront une conception ethnogénique où les trois types majeurs reconnus à l'époque seront affiliés : le type néandertalien (race de Canstadt) où un type antérieur à celui-ci aurait donné naissance en Asie aux descendants du type européen de Cro-Magnon, alors que pour la filiation entre le type de Cro-Magnon et celui de Furfooz, Fraipont et Lohest postuleront le mélange des deux types sans savoir si Furfooz serait le résultat de la rencontre entre une race inconnue et Cro-Magnon. Ces auteurs maintiendront que beaucoup d'individus vivant actuellement en Belgique seraient directement affiliés au type de Furfooz, alors que les traces des types de Canstadt et de Cro-Magnon chez la population actuelle de l'Europe se seraient transmises par voie atavique uniquement. Prenant note de la transformation des races préhistoriques de l'Europe au cours du Quaternaire, Fraipont et Lohest affirmeront qu'il devrait être possible de suivre les lignées humaine et anthropoïde jusqu'à une jonction éventuelle à la période de l'Éocène ou à une période plus ancienne encore.

Enfin, Hervé (1893, 1994a, 1894b, 1895) défendra qu'il est impossible dans l'état actuel de nos connaissances de faire dériver le type moins ancien de Cro-Magnon du type plus ancien de Néandertal (Canstadt), compte tenu du hiatus morphologique qui les sépare. Suivant les transformations et ramifications nombreuses de la lignée de Cro-Magnon dans le temps et l'espace (des dizaines de spécimens seront mentionnés), Hervé établira des filiations entre certains segments de ce type et des populations actuelles comme les Esquimaux du Groenland et du Labrador. Comme Broca, Hervé maintiendra que le type de Furfooz (maintenant rajeuni au Néolithique) faisait partie à l'origine d'une souche uniquement brachycéphale et que les restes mésaticéphales sont le produit de croisements entre le premier type et le type dolichocéphale de Cro-Magnon. Les Ibères et les Ligures seraient, selon Hervé, des représentants actuels des dolichocéphales et brachycéphales du Néolithique.

CONCLUSION : LA PALÉONTOLOGIE HUMAINE COMME DISCIPLINE

Nous avons déjà mentionné l'impact qu'a eu la réinterprétation, suite à la synthèse de l'évolution, de la place de Darwin et de ses sympatisants sur l'historiographie de la Paléontologie humaine traitant de la deuxième moitié du XIX^e siècle.

Mais de plus, la synthèse de l'évolution influencera peut-être, de façon plus subtile, la conception des relations qu'entretiendraient entre elles les diverses disciplines de la Biologie évolutionniste. Les disciplines de la Biologie évolutionniste depuis la synthèse de l'évolution, dont la Paléontologie humaine (e.g., Delisle, 1995), sont interpré-

tées comme faisant partie intégrante d'une même structure ou d'un corpus unique, si ce n'est que de façon plus ou moins étroite (e.g., Beatty, 1986 ; Burian, 1989 ; Caplan, 1978 ; Collingridge et Earthy, 1990 ; Darden, 1986 ; Delsol, 1991 ; Provine, 1988 ; Rosenberg, 1985 ; Ruse, 1973, 1988 : 5-21 ; Tuomi, 1981). Cet état de fait a pu contribuer à renforcer l'idée, combattue ici, que la naissance de la Paléontologie humaine au XIX^{e} siècle serait exclusivement un produit de l'avènement de la théorie de l'évolution. Bien entendu, il est inadmissible, historiquement parlant, de transposer au XIX^{e} siècle les rapports que l'on croit reconnaître aujourd'hui entre les diverses disciplines de la Biologie évolutive.

Nous avons vu comment, pour se constituer vers 1860, la Paléontologie humaine a puisé à même trois sources plus ou moins distinctes les unes des autres : l'ethnogénie européenne, le débat sur le monogénisme/polygénisme et ce qu'il a été convenu d'appeler le débat sur « l'évolutionnisme ». La Paléontologie humaine puise-t-elle réellement à même ces sources pour se constituer ou si la rencontre de ces trois sources vers 1860 viendra à constituer ce qu'on appellera la Paléontologie humaine ? Cette question, un peu fausse il est vrai, est néanmoins éclairante pour comprendre la réelle difficulté à définir les frontières de la discipline ou de l'unité de recherche en science (e.g., Bechtel, 1986 ; Hull, 1985). La reconnaissance, après 1860, d'une Paléontologie humaine bidimensionnelle ne constitue donc en rien une singularité ; au contraire, elle indique la nature souvent réticulée du processus scientifique. Si la Paléontologie humaine est généralement liée à l'avènement de la théorie de l'évolution par sa facette théorique, une autre concernée par sa documentation la lie à l'histoire de la généalogie des peuples européens. Cette dernière facette se verra imposer une stratégie épistémologique différente par suite de contrainte empirique.

Mentionnons d'abord que l'écart enregistré entre la situation empirique de la Paléontologie des vertébrés/invertébrés et de la Paléontologie humaine, avant 1860, est attribuable en partie à deux raisons intimement liées :

1) À l'inhibition découlant de la foi religieuse interdisant de concevoir l'ancienneté de l'homme hors de la chronologie fort restreinte de la Bible.

2) Au positionnement d'une histoire naturelle de l'homme à la toute fin d'une histoire naturelle de la vie sur terre, reléguant, par le fait même, l'homme à la période la plus récente.

Contrairement à la période récente, les époques anciennes de la terre seront abondamment documentées à l'aide de fossiles (Rudwick, 1985 : 94).

Ces obstacles épistémologiques, au sens où Gaston Bachelard l'entend, contribuent à expliquer le retard de la Paléontologie humaine sur la Paléontologie des vertébrés et invertébrés vers 1860.

Une autre raison peut-être encore plus importante pouvant expliquer ce décalage est d'ordre empirique. Bénéficiant du recul actuel, il est convenu que les hominidés les plus anciens et les plus primitifs proviennent d'Afrique ; l'Europe aurait été colonisée tardivement par des formes beaucoup plus modernes. Si l'Europe présentait déjà aux paléontologues du XIX^{e} siècle une très longue suite de formes vertébrées et invertébrées, en revanche elle recelait en son sein qu'un échantillon fort limité et peu représentatif de l'ensemble du processus évolutif des hominidés.

Le recours à l'ethnogénie européenne, comme cadre interprétatif aux restes humains anciens et fossiles, après 1860, trouve ici une justification épistémologique certaine. L'Europe ne pouvait fournir le matériel empirique adéquat pour répondre aux questions concernant le lien phylétique entre l'homme et les primates ou encore entre les grandes divisions de l'humanité ; ces questions devaient rester théoriques pour le moment et être abordées indirectement.

Il n'est pas défendu ici que la stratégie épistémologique d'utiliser l'ethnogénie européenne comme cadre interprétatif reposait uniquement sur un impératif empirique, puisqu'il est possible que cette approche soit la continuité d'une tradition qui semble prendre racine parmi les philologistes de la première moitié du siècle. Par contre, la nature du matériel empirique européen imposait de réelles contraintes aux chercheurs de l'époque.

Alors que les paléontologues des vertébrés durant la deuxième moitié du XIX^{e} siècle seront contraints à présenter des schèmes phylétiques composés de groupes taxonomiques de niveaux supérieurs (genre, famille), compte tenu de la nature du matériel disponible (Rainger, 1985), les chercheurs en Paléontologie humaine seront, pour la même raison, contraints à présenter des schèmes phylétiques de niveaux taxonomiques inférieurs (individu, population, race).

Ici prend fin cette analyse préliminaire des rapports de la Paléontologie humaine avec la Biologie évolutionniste lors du XIX^{e} siècle. Celle-ci aura permis d'établir avec plus de précision la nature de la Paléontologie humaine en tant que discipline scientifique. Ainsi, les multiples sources qui con-

tribuent à son développement sont mises en lumière. De plus, le problème épistémologique de l'interprétation des restes humains anciens et fossiles entre 1860 et 1895, dissimulé en partie sous un faux problème d'anti-évolutionnisme, trouve ici une explication nouvelle.

Nous croyons que la prolifération d'analyses de type historique et épistémologique pourrait grandement contribuer à une meilleure compréhension du développement de l'Anthropologie lors des XIX^e^ et XX^e^ siècles.

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Chapter 2

The Rise of the Field of Human Evolution, 1860-1930: A Reassessment.

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Introduction

It is often assumed that the rise of the field of human evolution had to be a tributary of an evolutionary worldview that arose around the mid-nineteenth century. Furthermore, it is frequently held that the field of human evolution was first largely dominated by anti-evolutionary views during the second half of the nineteenth century, only to be penetrated

by truly evolutionary approaches after the turn of the century as seen through the debate between unilinear and multilinear interpretations of human phylogeny.

This essay attempts to present an alternative view to the historiography just mentioned. The key elements of this alternative view may be summarized as follow.

It is incontestable that the acceptance of the theory of evolution played an active role in the constitution of the field of human evolution. However, other very important factors were also at work. The assumption that the rise of the field of human evolution was a by-product of evolutionary theory led to important misconceptions. I have pointed elsewhere to a number of such misconceptions.[1] These and other such aspects are discussed throughout this essay.

At the heart of the field of human evolution has always lain the concern to establish the genealogy or phylogeny of humankind. At least three sources were used to establish it during the nineteenth century.

The first one came from the advent of the transmutation theory. From this view, supposed evolutionary links between humans and animal forms (usually primates) could be postulated. From the nature of the primate and human fossil record at the time, comparative anatomy and embryology of living forms were extensively used to postulate such relationships. Scientists like Büchner, Darwin, Haeckel,

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[1] R. Delisle, "Les origines de la paléontologie humaine: essai de réinterprétation," *L'Anthropologie*, 102 (1998), forthcoming.

Hovelacque, Huxley, Schaaffhausen, Topinard, Vogt, Wallace, and others, contributed to this approach after 1860.

The second source used to assess human phylogeny came from the monogeny-polygeny debate. This debate over whether the living human races were derived from one or several sources was rapidly recast in an evolutionary perspective after 1860 and replaced by a monophyly-polyphyly debate. At this juncture, the monophyly-polyphyly debate fused completely with concerns to establish evolutionary links between humankind and animal forms.

Yet, it should not be assumed that all the scientists involved in the monogeny-polygeny debate around the mid-nineteenth century automatically subscribed to the transition to the monophyly-polyphyly debate. During the first half of the nineteenth century, the monogeny-polygeny debate was conducted within a very restricted time frame of a few thousand years. When the great antiquity of humankind was accepted around 1860, as documented through prehistoric archaeology, the data previously used in the monogeny-polygeny debate were, to say the least, of very limited significance. In this great vacuum of knowledge, scientists such as Broca, by no means an anti-evolutionist, maintained his polygenist position. He argued that the data at his disposal only demonstrated that, as far back in time as the human races were traced, they were found to be as distinct as they were today. In this perspective, how could human races

be linked to any animal form, whether through monophyletic or polyphyletic conceptions? Broca, the positivist, might have judged that any stance on the monophyly-polyphyly debate was premature.

The third source used at the time to assess human phylogeny derived from considerations over the peopling of Europe as first evidenced by philology, and later by ancient human remains. At first, this source was restricted to a limited portion of human phylogeny (that of Europeans). Most if not all ancient human remains discovered during the second half of the nineteenth century were interpreted within this framework, even though a majority of scientists pursuing the quest to unravel man's phylogenetic past were evolutionists.

On the basis of the above discussion, a few important points should be stressed. First, contrary to what is often asserted, anti-evolutionary treatises did not constitute a key factor in the rise of the field of human evolution during the second half of the nineteenth century.

Secondly, if the rise of evolutionary theory led to phylogenetic reflections about man's past, considerations over the peopling of Europe were also concerned with human phylogeny. At first restricted to Europe, these latter considerations were not a by-product of evolutionary theory. This situation was not created mainly because of divergences regarding the transmutation theory as is often assumed, but because of the nature of the available fossil material.

Thirdly, the rise of the field of human evolution should not be interpreted only as a by-product of evolutionary biology, as already stated. It is viewed in this essay as a complex scientific discipline characterized by several sources which created internal tensions that persisted into the twentieth century.[2]

Lastly, the establishment of humankind's place in nature, phylogenetically speaking, was not arrived at by the exploiting of one approach or one source only. Rather, several of them had to be combined, so to speak, to establish as complete a picture as possible of humankind's place in nature. Unsurprisingly, this created problems of integration of knowledge.

At the heart of the field of human evolution was a gap between broad phylogenies and restricted phylogenies. This situation created what we call in this essay *an epistemological problem*. This problem implied that as early as the 1860s a broad phylogenetic framework had been worked out between living humans and primates on the basis of comparative anatomy and embryology. Yet, this broad framework could not at the time be tested against the fossil record. The ancient human remains evaluated between 1860 and 1895, though fairly abundant, were judged by most scientists to be essentially of modern appearance in addition to being

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[2] Some of these ideas are further discussed in R. Delisle, "Les origines de la paléontologie humaine: essai de réinterprétation," *L'Anthropologie*, 102 (1998), forthcoming.

almost exclusively restricted to Europe. However, these human remains were of great value to trace the prehistoric peopling of Europe. They were most fruitfully compared to living human races and not interpreted as any ancestral form linking humans and apes. Phylogenetic links between living humans and the ancient human remains were thus established through this restricted framework of analysis.

Stated differently, if broad phylogenies were established by comparing living humans to living primates, restricted phylogenies, on the other hand, were established by linking ancient human remains to the various living human races, especially Europeans. This situation persisted until the last decade of the nineteenth century. It is thus easy to understand the impact that this epistemological problem had on the assessment of humankind's place in nature. This situation will be analysed in the first half of this essay.

In the light of more primate and human fossil discoveries made throughout the world, the epistemological problem identified here expressed itself in a somewhat modified manner between 1895 and 1930. If the human fossil record was compared almost exclusively with living humans before 1895, it was from that time onwards compared also with other living and fossil primate forms by an increasing number of scientists. This situation will be analysed in the second half of this essay.

On the one hand, some scientists continued to integrate, after 1895, most if not all human fossils in a framework in which they were phylogenetically related to one or several of the living human races. In this view, which is often neglected by historians, most human fossils were comfortably integrated in the phylogeny of living humans.

On the other hand, an increasing number of scientists now based their knowledge of the phylogenetic link between humankind and primate forms, not only on comparative anatomy and embryology of living forms, but also on the human and primate fossil record. These phylogenetic interpretations were often expressed in multilinear schemes in which a great number of human fossils were put on side branches leading toward extinction.

The Field of Human Evolution, 1860-1895

During the 1850s and especially the 1860s, the idea of a universal transmutation of species gained increasingly in support. This situation has prompted a number of historians to interpret the rise of the field of human evolution as being a logical consequence of the acceptance of transformism. In this perspective, it seemed natural to turn to this period of history and expect that scientists would be busy establishing human ancestry by searching for

intermediate forms between humankind and animals. If scientists, or some of them, were not working on filling up this agenda, it must be either because they had not been converted to evolution or the fossil record was as yet too incomplete.

Although this view is not completely false, it misses the point that the field of human evolution was not exclusively the product of evolutionary biology. We should not speak of a single origin of the field of human evolution, but of its multiple origins. As stated in the introductory section of this essay, it is suggested here that a distinction between (1) broad phylogenies and (2) restricted phylogenies constitutes a useful way to analyse the influence of these multiple origins on the field of human evolution during the 1860-1895 period. Let us turn first to broad phylogenies.

Broad Phylogenies

This section aims at demonstrating how the idea of the evolution of humankind was widely embraced during the second half of the nineteenth century. This is most convincingly demonstrated through the phylogenetic interpretations held by scientists which linked living humans to other primates on the basis of comparative anatomy and embryology of these living forms. This framework of phylogenetic analysis is referred to in this essay as broad phylogenies. In order to

show how the idea of the evolution of humankind was widely embraced during that period, it will be necessary to consider the various avenues exploited by scientists to defend evolutionary views.

The establishment of broad phylogenies between 1860 and 1895 was prosecuted from different avenues and may therefore be fruitfully analysed when broken down into a number of more or less distinct debates. Three such debates are considered here: A) the taxonomic relationship of humankind with its closest living relatives, B) the hypothetical reconstruction of human ancestors, and C) the relationship of the races of humankind in time. Some scientists fused two or more of these debates, and they will consequently be analysed separately in section (D).

A) The taxonomic relationship of humankind with its closest living relatives.

Let us turn first to those scientists who defended a close relationship between humans and living anthropoids.

In the second part of *Evidence as to Man's Place in Nature* (1863), Huxley defended the taxonomic conclusion, based on embryology and comparative anatomy, that the "differences which separate Man from the Gorilla and the Chimpanzee are not so great as those which separate the Gorilla from the lower apes".[3] On that basis, he favoured

classifying humankind in a family of its own within the order *Primates*. From there, Huxley drew the following evolutionary implications:

> [I]f any process of physical causation can be discovered by which the genera and families of ordinary animals have been produced, that process of causation is amply sufficient to account for the origin of Man... [T]here would be no rational ground for doubting that man might have originated... by the gradual modification of a man-like ape; or... as a ramification of the same primitive stock as those apes."[4]

For such a process of physical causation Huxley accepted, at least publicly, Darwin's theory of evolution by means of natural selection.[5] Huxley was convinced that if not precisely true, Darwin's hypothesis was a good approximation to it. He contended, however, that one link in the chain of evidence was still wanting, that is, the production of cross- and hybrid sterility through selective breeding. This objection was maintained by Huxley for the rest of his life. It reflected his conception of the nature of science: to be perfectly proven, a theory required direct evidence from empirical observations.[6]

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[3] T.H. Huxley, *Evidence as to Man's Place in Nature* (London: Williams & Norgate, 1863), reprinted in *Collected Essays (1893-1894)*, Vol. 7 (London: Macmillan, 1894), p. 144.

[4] Ibid., pp. 146-147.

[5] C.R. Darwin, *On the Origin of Species by Means of Natural Selection: Or the Preservation of Favoured Races in the Struggle for Life* (London, 1859).

[6] M.J. Kottler, "Charles Darwin and Alfred Russel Wallace: Two Decades of Debate Over Natural Selection," in D. Kohn (ed.), *The Darwinian*

In fact, the interpretation of Huxley as a great defender of Darwin's theory has been reassessed during the past twenty years.[7] First, although Huxley had already favoured transmutation by the late 1850s, he only contributed positively to the evolutionary research programme by the end of the 1860s by proposing phylogenetic links among some vertebrate groups. Also, Huxley's concerns for morphology departed from Darwin's field naturalist considerations over problems of variation, selection and inheritance. Furthermore, Huxley never completely discarded the saltation mode of evolution and the possibility that internal factors might channel evolutionary change along definite lines regardless of the environment. In contradistinction, Darwin promoted a gradual mode of evolution produced by the action of natural selection on individual variations.

In this perspective, Di Gregorio may be right in pressing the case that Huxley's *Evidence as to Man's Place in Nature* (1863) was essentially a contribution to Linnaeus's classification that humankind and the living great apes were closely related, against Richard Owen (see below), rather

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Heritage (New Jersey: Princeton University Press, 1985), 367-432, see esp. pp. 387-407.

[7] See M. Bartholomew. "Huxley's Defence of Darwin," *Annals of Science*, 32 (1975): 525-535; M.A. Di Gregorio, "The Dinosaur Connection: A Reinterpretation of T.H. Huxley's Evolutionary View," *Journal of the History of Biology*, 15 (1982): 397-418; P.J. Bowler, *The Non-Darwinian Revolution. Reinterpreting a Historical Myth* (Baltimore: Johns Hopkins University Press, 1988), pp. 72-80; S.L. Lyons, "The Origins of T.H. Huxley's Saltationism: History in Darwin's Shadow," *Journal of the History of Biology*, 28 (1995): 463-494.

than a contribution to Darwin's theory. There might consequently be a weaker case than is usually assumed that Huxley's book was the beginning of the quest for an evolutionary anthropology, as Di Gregorio holds.[8] However, Huxley's clear evolutionary conclusion for human ancestry, no matter how secondary the issue was to the whole argument of his book, was clearly perceived and developed in the short term.[9]

Dally, who translated Huxley's *Evidence as to Man's Place in Nature* into French in 1868, followed Huxley's lead in attributing to humankind a familial rank within the *Primates*. Dally also drew the following evolutionary implication:

> "[S]i l'on pouvait, par hypothèse, assigner aux différents groupes simiens une origine commune, il n'y aurait aucune raison pour que l'on ne pût étendre aux races humaines le bénéfice de la même hypothèse et supposer que le groupe humain et les groupes simiens peuvent dériver d'une même souche."[10]

Not unlike Huxley, Dally distinguished between the Darwinian hypothesis of evolution and the general concept of

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[8] M.A. Di Gregorio, *T.H. Huxley's Place in Natural Science* (New Haven: Yale University Press, 1984), pp. 151-157.

[9] For a quick response in France see C. Blanckaert, "La question du singe et l'ordre des primates à la Société d'Anthropologie de Paris (1865-1870)," in R. Corbey and B. Theunissen (eds.), *Ape, Man, Apeman: Changing Views since 1600* (Leiden University: Department of Prehistory, 1995), 117-137, esp. p. 118.

[10] M.E. Dally, "L'ordre des primates et le transformisme," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 3 (1868): 673-712, see p. 674.

transformism. For Dally, the acceptance of the latter did not depend on the validity of the former. Darwinism was only one hypothesis among others.

Although Darwin published *The Origin of Species* in 1859, he was to wait until 1871 before openly and explicitly expressing his opinion about humankind in *The Descent of Man*. In Darwin's mind, taxonomy and phylogeny were intimately related. In considering the human place in relation to other primate forms, he acknowledged that some characteristics of the human structure may claim the rank of a suborder, or probably a higher grade if mental faculties were considered. However, he contended that "under a genealogical point of view it appears that this rank is too high, and that man ought to form merely a Family, or possibly even only a Sub-family".[11] Here, Darwin was only echoing a general tension between morphological characters and behavioral or mental ones in taxonomy. Darwin's phylogenetic assessment of humankind's place in nature read as follow:

> "There can consequently hardly be a doubt that man is an offshoot from the Old World Simian stem; and that under a genealogical point of view, he must be classed with the Catarhine division... If the anthropomorphous apes be admitted to form a natural sub-group, then as man agrees with them... we may infer that some ancient member [of them] gave birth to man."[12]

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[11] C.R. Darwin, *The Descent of Man, and Selection in Relation to Sex*, Vol. 1 (London: John Murray, 1871), p. 195.

[12] Ibid., pp. 196-197.

Although Darwin refrained from presenting a diagram of human phylogeny in *The Descent of Man*, one can see what he had in mind in the Darwin papers at the Cambridge University Library dated 21 April 1868.[13]

It seems at first sight superfluous to even mention what Darwinism is all about and what kind of theory of evolution Darwin had in mind. However, two reasons prove this to be a false assumption. First, intensive historical studies on the development of Darwin's ideas have brought a reassessment of his role and position in evolutionary biology.[14] Secondly, it should be noted that what is usually called Darwinism or Darwin's theory of evolution was a complex entity variably defined from time to time and from one author to another.[15]

A consequence of this historiographical reassessment is that the view we have of Darwin is changing. He is often depicted as a scientist of a new materialistic and

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13 Reproduced in H.E. Gruber, *Darwin on Man* (New York: E.P. Dutton, 1974), p. 197.

14 For a recent overview see P.J. Bowler, *The Non-Darwinian Revolution. Reinterpreting a Historical Myth* (Baltimore: Johns Hopkins University Press, 1988). See also E. Mayr, "The Myth of the Non-Darwinian Revolution," *Biology and Philosophy*, 5 (1990): 85-92.

15 See P.J. Bowler, *The Non-Darwinian Revolution. Reinterpreting a Historical Myth* (Baltimore: Johns Hopkins University Press, 1988); D.L. Hull, "Darwinism as a Historical Entity: A Historiographic Proposal," in D. Kohn (ed.), *The Darwinian Heritage* (New Jersey: Princeton University Press, 1985), 773-812; E. Mayr, "Darwin's Five Theories of Evolution," in D. Kohn (ed.), *The Darwinian Heritage* (New Jersey: Princeton University Press, 1985), 755-772; *One Long Argument: Charles Darwin and the Genesis of Modern Evolutionary Thought* (Cambridge: Harvard University Press, 1991), chap. 7; D. Recker, "There's more than one way to recognize a Darwinian: Lyell's Darwinism," *Philosophy of Science*, 57 (1990): 459-478.

naturalistic era devoid of influences from natural theology and progressionism. He is said to have stressed field problems related to adaptation, variation, speciation and geographic distribution to understand evolutionary biology, while neglecting, at the same time, more traditional morphological research areas such as palaeontology, comparative anatomy and embryology concerned with the reconstruction of life's pattern. Without being necessarily false, this view of Darwin needs to be carefully qualified.[16] He was a man of his time very much constrained by a number of contemporaneous themes.

The evolution of species was caused, in Darwin's approach, by the natural selection under changing conditions of individual variations that occurred randomly. Under the effect of such a mechanism, evolution often presented itself as a pattern of haphazard branching. The adaptation of a species to different local conditions tended to divide it, each population having its own evolutionary path diverging from each other to such an extent as to be distinguished with time at higher taxonomic levels (species, genera, families, etc.).[17]

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[16] See for instance D. Ospovat, *The Development of Darwin's Theory. Natural History, Natural Theology, and Natural Selection, 1838-1859* (Cambridge: Cambridge University Press, 1981); D. Kohn, "Darwin's Ambiguity: The Secularization of Biological Meaning," *British Journal for the History of Science*, 22 (1989), 215-239; R.J. Richards, *The Meaning of Evolution. The Morphological Construction and Ideological Reconstruction of Darwin's Theory* (Chicago: University of Chicago Press, 1992), chap. 4 and 5.

On a strictly morphological basis, Topinard united all the living anthropoids and monkeys into the same taxonomic category, leaving humankind standing by itself in a suborder of the *Primates*.[18] But this particular taxonomic disposition did not prevent him from postulating a simian descent for humans:

> "Il n'y a pour moi que deux doctrines: l'une qui fait venir l'Homme de la souche première des Mammifères en ligne directe et sans intermédiaire d'ordres... et l'autre qui accepte la branche ou l'ordre des Primates avec toutes ses conséquences, les Lémuriens ou Prosimiens à la base, les Singes ou Simiens ensuite, l'Homme tout seul au sommet... Nous descendons donc des Singes ou du moins tout se présente *comme si* nous en descendions. Mais de quel singe connu ou inconnu? Je l'ignore, aucun des Anthropoïdes actuels n'a assurément été notre ancêtre."[19]

In addition to information derived from living forms, some scientists tried, although somewhat superficially, to establish the human's place in nature with the assistance of mammalian palaeontology.

Wallace recognized the great affinity of humans and the living great apes. He asserted that the great apes had diverged less from one another than all of them from humans.

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[17] P.J. Bowler, *The Eclipse of Darwinism. Anti-Darwinian Evolution Theories in the Decades around 1900* (Baltimore: Johns Hopkins University Press, 1983), p. 21.

[18] P. Topinard, "Les dernières étapes de la généalogie de l'homme," *Revue d'Anthropologie*, 3 (1888): 298-332; "De l'évolution des molaires et prémolaires chez les primates et en particulier chez l'homme," *L'Anthropologie*, 3 (1892): 641-710.

[19] P. Topinard, "Les dernières étapes de la généalogie de l'homme," *Revue d'Anthropologie*, 3 (1888), pp. 329-331.

The phylogenetic implication was that "he [man] must, therefore, have diverged from the common ancestral form before the existing types of anthropoid apes had diverged from each other".[20] Wallace then introduced two distinct series of facts to specify his phylogenetic scheme. The first one concerned the discovery of fossil apes:

> "Now, this divergence almost certainly took place as early as the Miocene period, because in the Upper Miocene deposits of Western Europe remains of two species of ape [*Pliopithecus* and *Dryopithecus*] have been found allied to the gibbons... We seem hardly, therefore, to have reached, in the Upper Miocene, the epoch of the common ancestor of man and the anthropoids."[21]

In order to support humankind's great antiquity, Wallace then referred to a second series of facts: tools and human fossils in Europe. First, he pointed to ancient tools found both in the Old and New World that might be dated to the Pliocene period. Secondly, Wallace thought that the discovery of relatively modern looking skulls at Engis and Neandertal further supported the case for man's great antiquity.

Historians recognize that there were a number of distinctions between Wallace's and Darwin's conception of natural selection. Wallace expounded a relatively strict adaptationist approach and held that all traits, at one time

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[20] A.R. Wallace, *Darwinism: An Exposition of the Theory of Natural Selection With Some of its Applications* (London: Macmillan, 1889), pp. 455-456.

[21] Ibid., p. 456.

or another, were useful and resulted from a process of selection. For Darwin, a useless trait might be maintained by being sheltered by the positive selection of a useful one (principle of correlation). Darwin was more pluralistic in his approach for he maintained that, although natural selection was the main mechanism of evolutionary change, other factors of evolution such as sexual selection should be recognized. He thought that natural selection was constrained by modes of inheritance and development. Darwin and Wallace also disagreed on the levels of selection. Wallace thought that natural selection acted both on the individual as well as on the group, whereas Darwin, without completely discarding group selection, favoured the individual as the main target of selection.[22] Darwin also allowed a place for the inheritance of acquired characteristics.

Let us now turn to scientists who did not defend a close relationship between humans and the living apes. In the late 1850s and early 1860s, Owen specified his taxonomic standpoint by classifying humankind in a subclass of its own which he called *Archencephala* on the basis of a number of structural peculiarities of its brain.[23]

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22 M.J. Kottler, "Charles Darwin and Alfred Russel Wallace: Two Decades of Debate Over Natural Selection," in D. Kohn (ed.), *The Darwinian Heritage* (New Jersey: Princeton University Press, 1985), 367-432.

23 R. Owen, "On the Characters, Principles of Division, and Primary Groups of the Class Mammalia," *Journal of the Proceedings of the Linnean Society of London*, Zoology, 2 (1857): 1-37; "The Gorilla and

Although Owen never specified precisely the phylogenetic implication of his taxonomic view, it seems doubtful that he would have agreed with a close phylogenetic link between humans and the living apes, at least to judge by his position in the 1830s when he rejected the idea of an ape ancestry for humankind.[24] However, Owen mentioned that, independently of the psychological distinction separating humans from the other animals,

> "I cannot shut my eyes to the significance of that all-pervading similitude of structure - every tooth, every bone, strictly homologous - which makes the determination of the difference between *Homo* and *Pithecus* the anatomist's difficulty".[25]

Here again we find the tension between taxonomic evaluations based on morphological traits and those based on psychological or mental ones.

Although Owen was in favour of transmutation prior to 1860[26], he defended after that period transmutation through a

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the Negro," *The Athenaeum*, 1861: 395-396; 467. For some discussion of Owen's view see T.H. Huxley, "On the Zoological Relations of Man with the Lower Animals," *Natural History Review*, 1 (1861): 67-84; P. Broca, "Réponse aux observations de M. le professeur Richard Owen," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 5 (1870): 592-605. For an account of the confrontation between Owen and Huxley on this matter see A. Desmond, *Archetypes and Ancestors: Palaeontology in Victorian London, 1850-1875* (London: Blond & Briggs, 1982), pp. 74-83.

[24] See A. Desmond, "Richard Owen's Reaction to Transmutation in the 1830's," *British Journal for the History of Science*, 18 (1985): 25-50, esp. 40-43.

[25] R. Owen, "On the Characters, Principles of Division, and Primary Groups of the Class Mammalia," *Journal of the Proceedings of the Linnean Society of London*, Zoology, 2 (1857): 1-37, see note on p. 20.

law sometimes referred to as the derivative law.[27] This law derived species from one another by a preordained (a manifestation of creative power) and innate tendency to change independently of external conditions.

Owen's conception of evolution can be understood if we embrace a wider approach to evolutionary biology. Around the mid-nineteenth century, embryology came to play a crucial role in evolutionary biology. Numerous scientists thought that they could analogize the history of life on earth with the growth of an individual. The successive life forms in the time dimension resembled the various stages of an individual's development. As Bowler argued:

> "Once the growth of the embryo was taken as the model for the pattern in which life develops on the earth, the implication that the latter process might also unfold through gradual transformation could hardly be avoided. Instead of a planned sequence of miraculous creations [Agassiz for instance], surely the Designer could have programmed nature to reveal itself according to a preconceived pattern in which each form spontaneously transmutes itself into the next highest stage in the plan through some appropriate stimulus."[28]

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[26] See P.J. Bowler, *Fossil and Progress: Paleontology and the Idea of Progressive Evolution in the Nineteenth Century* (New York: Science History Publications, 1976), pp. 99-107; E. Richards, "A Question of Property Rights: Richard Owen's Evolutionism Reassessed," *British Journal for the History of Science*, 20 (1987): 129-171.

[27] R. Owen, "On the Aye-aye," *Transactions of the Zoological Society of London*, 5 (1863): 33-101, esp. pp. 89-96; *On the Anatomy of Vertebrates. Mammals*, Vol. 3 (London: Longmans, Green, and Co., 1868), pp. 799-809.

[28] P.J. Bowler, *The Non-Darwinian Revolution. Reinterpreting a Historical Myth* (Baltimore: Johns Hopkins University Press, 1988), p. 52.

This type of developmental approach to evolution during the nineteenth century, sometimes referred to as theistic evolutionism, could easily be associated with the concept of parallel evolution. Darwin's haphazard pattern of evolution can be understood as resulting from the pressure created by external conditions on a population. The adaptation of a population to different local conditions would tend to divide it, each sub-population taking a different evolutionary path to cope with its specific selective pressures. If evolution were directed by internal rather than external factors (the Designer's programme in Owen's view), theistic evolutionists were free to postulate that evolutionary lines were evolving in parallel. Under such circumstances, some similarities may be explained as the result of parallel evolution rather than by common descent.[29]

This avenue was explicitly exploited by Mivart. He contended that human structural characters were shared by a great many living forms, primates and non-primates alike. Accordingly, Mivart argued in favour of numerous morphological similarities acquired without any genetic affinity through parallel evolution. The human's phylogenetic position remained unspecified under such circumstances, but Mivart undoubtedly had in mind an independent origin of humans and living apes, just as he postulated that the primates of the Old and New Worlds had

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[29] Ibid., p. 96.

been created independently of one another, their similarities being explained by parallel adaptive modifications under similar conditions of life. In this view, Mivart dismissed Darwin's notions of common descent and undeterministic evolution.

For Mivart, the course of evolution might have resulted from the stimulation of external conditions, although the unfolding of an internal plan built into life itself seemed to have been much more important. He stated that:

> "...similarity of structure may arise without there being any genetic affinity between the resembling forms, as also that it is much rather to an internal cause or principle than to any action of surrounding external conditions that the origin of new specific forms is due."[30]

We have seen that Huxley insisted that to be satisfactorily proven, the theory of natural selection required direct evidence from empirical observation. As the best articulated conception of evolution of the time, Darwin's evolutionary theory came under close philosophical or epistemological scrutiny.[31] Although Huxley was not completely convinced that natural selection was powerful

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[30] St. George Mivart, *Man and Apes, an Exposition of Structural Resemblances and Differences Bearing Upon Questions of Affinity and Origin* (London: Robert Hardwicke, 1873), pp. 185-186.

[31] See D.L. Hull, *Darwin and His Critics. The Reception of Darwin's Theory of Evolution by the Scientific Community* (Cambridge: Harvard University Press, 1973), pp. 3-77; M. Ruse, *The Darwinian Revolution* (Chicago: University of Chicago Press, 1979), pp. 174-180, 234-239.

enough to produce distinct species, as might eventually be demonstrated through cross- and hybrid sterility, his defence of Darwin in public seemed to have stemmed nevertheless from his appreciation for the nature of Darwin's scientific approach: its naturalistic type of explanation. This may have been the very essence of Huxley's comment when he stated that natural selection was a good approximation to truth.

However, other scientists such as de Quatrefages would not accept this view of the nature of science, and required direct observations and experimental proof. Not repudiating the action of natural selection within species, de Quatrefages argued that it could not modify indefinitely organisms to such an extent as to encompass the species' limits.[32] His argument rested on the fact that the transmutation of one species into another and the interspecific sterility accompanying it have not been demonstrated by experimentation or directly observed. He would not recognize the process of race formation by artificial selection as a small-scale phenomenon that only needed to be amplified in the wild to permit species formation by natural selection.[33]

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[32] A. de Quatrefages, *L'espèce humaine*, 14e éd. (Paris: Alcan, 1905), pp. 68-70. The first edition of this book was published in 1877. It should be noted that de Quatrefages died in 1892.

[33] Ibid., pp. 70-73; *Charles Darwin et ses précurseurs Français. Étude sur le transformisme* (Paris: Baillière, 1870), pp. 152-154; *Les émules de Darwin*, Tome 1 (Paris: Baillière, 1894), pp. 28-36.

De Quatrefages recognized that the composition of the floras and faunas had changed during the course of time. He identified two distinct causes of change acting at two different levels. 1) An unknown cause manifested itself intermittently in the course of time (especially during important geological movements) and would have been responsible for the origin of species. This unknown cause might still be active presently in some regions of earth, and was therefore not necessarily out of reach for scientists. However, de Quatrefages contended that it had to be uncovered through experimentation and direct observation. 2) Another cause was responsible for subspecific changes that were moulded by a host of factors such as natural selection, acclimatization, crossbreeding, atavism, etc.[34]

De Quatrefages rejected all the transformist theories of his time. However, if no transformist in the usual sense of the word, it seems inappropriate to classify him as an anti-transformist. Scientists such as de Quatrefages show us that the dichotomy we usually make between transformists and anti-transformists is too restrictive and produces but little light.

Leaving the problem of the origin of species aside, de Quatrefages nevertheless expressed opinions about the

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[34] A. de Quatrefages, *Rapport sur les progrès de l'anthropologie* (Paris: Imprimerie Impériale, 1867), p. 242; *L'espèce humaine*, 14e éd. (Paris: Alcan, 1905), p. 75; *Les émules de Darwin*, Tome 2 (Paris: Baillière, 1894), p. 288.

taxonomic relationship of humans and the other living primates. Recognizing the general morphological similarity between them, de Quatrefages insisted that, whereas all primates where structurally built for climbing, humankind was built for walking, thus demonstrating that they had developed structurally in opposite directions.[35] Taxonomically, he classified humankind alone in a kingdom of its own. Recognizing that language and intelligence were only differences of degree rather than kind, de Quatrefages argued that such differences were insufficient to separate humans from the animals. Instead, de Quatrefages thought that morality and religion were exclusive attributes that justified the creation of a separate kingdom for humankind.[36]

Phylogenetically speaking, de Quatrefages granted that some anatomical, physiological and embryological facts could be interpreted in favour of a common ancestry between humans and the living anthropomorphs. Yet, when turning to the fossil record, he continued, no evidence could bring any support to the rapprochement between the most ancient human races and the twenty-one fossil species of primates that were

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35 A. de Quatrefages, *Charles Darwin et ses précurseurs Français. Étude sur le transformisme* (Paris: Baillière, 1870), pp. 366-367; C. Blanckaert, "La question du singe et l'ordre des primates à la Société d'Anthropologie de Paris (1865-1870)," in R. Corbey and B. Theunissen (eds.), *Ape, Man, Apeman: Changing Views since 1600* (Leiden University: Department of Prehistory, 1995), 117-137.

36 A. de Quatrefages, *L'espèce humaine*, 14e éd. (Paris: Alcan, 1905), chap. 1.

then known.[37] Therefore, not denying completely the theoretical possibility of a common ancestry between humans and the primates, de Quatrefages excluded it on the basis of a lack of any directly observable data:

> "...on n'a pas encore découvert l'*ancêtre commun* d'où seraient issus l'Homme et les singes; et on ne peut pas montrer un seul terme de la série d'êtres intermédiaires, qui est censée avoir existé entre lui et nous. La conception d'une *parenté collatérale* entre l'espèce humaine et les autres primates... [ne peut] en appeler au moindre fait d'expérience ou d'observation. Par conséquent... cette explication des origines humaines ne peut être acceptée par quiconque tiendra compte des justes exigences de la science moderne."[38]

Before leaving the debate on the taxonomic relationship between humankind and its closest living relatives, it should be noted that de Quatrefages' clear rejection of natural selection as a mechanism capable of producing new species (speciation), because it had not been demonstrated experimentally or by direct observation, was not necessarily reactionary. It is usually assumed today that if Darwin was unable to provide such a demonstration during his lifetime, subsequent development in evolutionary biology provided us with an understanding of the process of speciation. However, it has been argued that the evidence today is still

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[37] A. de Quatrefages, *Les émules de Darwin*, Tome 2 (Paris: Baillière, 1894), pp. 192-194.

[38] Ibid., p. 194; see also G. Laurent, "Idées sur l'origine animale de l'homme en France au XIXe siècle," in R. Corbey and B. Theunissen (eds.), *Ape, Man, Apeman: Changing Views since 1600* (Leiden University: Department of Prehistory, 1995), 157-171, esp. 163.

insufficient to discriminate between a number of competing models of speciation. Furthermore, it has been argued that our present understanding does not permit us to distinguish between the genetic changes occurring during anagenesis and cladogenesis.[39]

B) The hypothetical reconstruction of human ancestors.

One characteristic of research on human evolution has always been that artefacts recovered are substantially more abundant than fossil discoveries. In view of this, the field of prehistoric archaeology has been instrumental in the establishment of the presence of humans in ancient times.

After repeated claims of prehistoric finds during the first half of the nineteenth century, the scientific community finally recognized around 1860, after crucial and careful discoveries made in France and England, that human artefacts were found in contemporaneity with extinct animals of the Quaternary period.[40] Within a few decades,

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[39] See W.B. Provine, "Adaptation and Mechanisms of Evolution after Darwin: A Study in Persistent Controversies," in D. Kohn (ed.), *The Darwinian Heritage* (New Jersey: Princeton University Press, 1985), 825-866; "Founder Effects and Genetic Revolutions in Microevolution and Speciation: An Historical Perspective," in L. Giddings, K. Kaneshiro and W. Anderson (eds.), *Genetics, Speciation, and the Founder Principle* (Oxford: Oxford University Press, 1989), 43-76.

[40] Among recent accounts see A. Bowdoin Van Riper, *Men Among the Mammoths: Victorian Science and the Discovery of Human Prehistory* (Chicago: University of Chicago Press, 1993); C. Cohen and J.-J. Hublin, *Boucher de Perthes, 1788-1868: les origines romantiques de la préhistoire* (Paris: Belin, 1989); D.K. Grayson, *The Establishment of Human Antiquity* (New York: Academic Press, 1983); B.G. Trigger, *A History of Archaeological Thought* (Cambridge: Cambridge University Press, 1989).

prehistoric artefacts were firmly set within a framework of Palaeolithic and Neolithic industries, which names are still in use.

A number of scientists from the 1860s on thought that the existence of humankind could be positively extended to the early portion of the Quaternary period or even well into the Tertiary period.[41] Two issues kept recurring in what was often called the controversy over Tertiary man. First, the nature of the supposed artefacts was disputed because their crudeness made them look like natural objects. Were they man-made or not? Secondly, the geological provenance of the material was often questioned. Were they as ancient as some claimed? Despite uncertainties surrounding the existence of Tertiary man, it was by then clearly established that human-like creatures existed during the Quaternary period. In short, prehistoric archaeology provided a wealth of evidence for the existence of tool-makers in deep time.

This situation probably contributed to the creating of a sense of anticipation about the eventual discovery of fossil remains of the ancient tool-makers.[42] For some scientists,

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41 See for instance M. l'abbé Bourgeois, "Indices de l'existence de l'homme à l'époque tertiaire," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 1872: 81-94; J. Capellini, "Les traces de l'homme pliocène en Toscane," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 1876: 46-63; C. Ribeiro, "L'homme tertiaire en Portugal," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 1880: 81-117; J. Prestwich, "On the Primitive Characters of the Flint Implements of the Chalk Plateau of Kent," *Journal of the Anthropological Institute*, 21 (1892): 246-262.

the discovery of artefacts was more than just a sign of the presence of tool-makers, but also a valuable source of information about the tool-makers themselves. For instance, talking about the flint implements of Kent (England), Prestwich asserted in 1892 that:

> "...the very distinctive features of the plateau implements, such as their rudeness of make... constitute characters so essentially different from those which typify the later implements, that by those characters alone they might be attributed to a more primitive race of men...".[43]

This approach was exploited more extensively by other scientists. According to Roujou, the existence of artefacts in the Tertiary period comfirmed the expected presence of humankind in ancient times, a predictable outcome of the theory of evolution considering that humankind was derived from a simian form.[44] Just as the faunas changed in the course of time, so did humankind. Therefore, humankind was probably of a different appearance in Tertiary times from

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42 It has been suggested that such an anticipation prepared the ground for the Piltdown hoax, see F. Spencer, "Prologue to a Scientific Forgery: The British Eolithic Movement from Abbeville to Piltdown," in G. Stocking (ed.), *Bones, Bodies, Behavior: Essays on Biological Anthropology* (Madison: University of Wisconsin Press, 1988), 84-119; *Piltdown: A Scientific Forgery* (Oxford: Oxford University Press, 1990), esp. chap. 1.

43 J. Prestwich, "On the Primitive Characters of the Flint Implements of the Chalk Plateau of Kent," *Journal of the Anthropological Institute*, 21 (1892): 246-262, citation on p. 261.

44 M. Roujou, "L'homme miocène," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 2 (1867): 662-664; "Sur l'homme tertiaire," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 8 (1873): 675-678.

what it is today. The "primitive state" of early humans seemed to be confirmed, in Roujou's mind, by the crudeness of its artefacts in the Tertiary period: "Pour ce qui est des formes grossières des silex taillés miocènes, elles peuvent très-bien s'expliquer en admettant que l'homme de cette époque était extrêmement inférieur...".[45]

Of all the scientists who considered artefacts to be a most valuable source of information about the tool-makers themselves, de Mortillet was probably the one who made the greatest use of it. His approach will be discussed below.

If prehistoric archaeology was used to extrapolate information about human ancestors, other approaches also were exploited. For Darwin, humankind was descended from an ape-like form that stemmed from an Old World primate. He provided us with a general picture of its precursor on the basis of comparative anatomy and embryology.[46] Darwin contended that such an early progenitor was covered with hair, possessed pointed ears capable of movement, a tail, a prehensile foot, great canine teeth (especially in males), and lived in the trees.

Darwin also conjectured on the possible evolutionary scenario of human ancestors.[47] He explained that as soon as

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45 M. Roujou, "Sur l'homme tertiaire," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 8 (1873), p. 677.

46 C.R. Darwin, *The Descent of Man, and Selection in Relation to Sex*, Vol. 1 (London: John Murray, 1871), pp. 206-207.

47 Ibid., pp. 140-151.

some primate forms came to live on the ground, owing to changing conditions or new strategies of food gathering, such forms would have become modified to adapt either to a quadrupedal or bipedal locomotion. Human ancestors exploited the latter avenue. This new bipedal adaptation would have instigated or been based upon a series of morphological transformations in the course of time. Freed hands and arms being advantageous, the progenitors of humans became more and more erect. The upper part of the body was modified for prehension skills, the lower part for firm support on the ground. Indirect modifications followed from the free use of the upper limbs and hands. First, these permitted the use of tools for defence, reducing therefore the importance of large jaws and teeth in such an activity, and permitting ultimately the prognathism of the skull to be reduced. Secondly, the brain was enlarged and the mental faculties increased.

Dissatisfied with previous efforts of Haeckel (see below) and Darwin, Hovelacque tried to present a better reconstruction of the human precursor.[48] Hovelacque tried to reconstruct the ape-like creature of the Tertiary period that produced crude artefacts and referred to by de Mortillet as *Anthropopithecus* (see below). Hovelacque made it clear that he meant by the human precursor a general morphological type

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[48] A. Hovelacque, "Notre ancêtre: recherches d'anatomie et d'ethnologie sur le précurseur de l'homme," *Revue d"Anthropologie*, 6 (1877): 62-99; see also N. Richard, "La fabrique du précurseur," in C. Blanckaert, *Des sciences contre l'homme*, vol.1 (Paris: Autrement, 1993), 64-79.

composed of several distinct races that were the source of the present human diversity.

Taking for granted the common ancestry of humankind and the living anthropoids, Hovelacque's method of reconstruction rested on the systematic use of comparative anatomy. Two basic assumptions were among his premises. First, no particular living anthropoid was closer to humans than the others. Secondly, some human races presented more "primitive" characters than others, thus taking them closer to a simian ancestor. However, he made it clear that more "primitive" human races were no links between the more advanced ones and a simian ancestor.

The salient features of Hovelacque's reconstruction are summarized as follow. Like the anthropoid apes and the races of humankind, some human precursors had dolichocephalic skulls and others brachycephalic ones. They all had a lower mean endocranial capacity than any modern human races, but probably higher than the highest mean endocranial capacity values among living anthropoids. The frontal portion of the precursors' crania was greatly reduced and flattened as seen among human "lower races" and the anthropoids. The same applied to the supraorbital tori which were salient, as seen in the fossil crania of Neandertal, Eguisheim, Engis and Gibraltar. According to Hovelacque, the simian traits identified on the ancient human remains of the Quaternary period thus far discovered constituted positive proof for

both the transmutation theory and the origin of humans from a common stem with the anthropoid apes.[49] Our precursors also had greater prognathism, large canine teeth and a simian-like mandible as seen in the fossil jaw of La Naulette. Although bipedal, human precursors could not have been entirely erect.

C) The relationship of the races of humankind in time.

Wallace thought that the monogeny-polygeny debate could be reconciled under the theory of evolution by means of natural selection.[50] He conceded to the polygenists that they had the better of the argument, at least to judge by the apparent existence of human races in ancient times, and the lack of evidence that these ancient races converged towards each other as we go deeper in time.

Like the monogenists, however, Wallace held that humans originated from a single type. He argued that humankind had been, at a very early period, probably a widely distributed and homogeneous race living in warm climates. As some populations migrated to other parts of the world and became exposed to new and varied conditions, numerous populations would have differentiated and adapted under the pressure of natural selection. Thus arose the characteristics which

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[49] A. Bovelacque, *Lettre sur l'homme préhistorique du type le plus ancien; sur la structure de ses restes et sur son origine* (Paris: Reinwald, 1875).

[50] A.R. Wallace, "The Origin of Human Races and the Antiquity of Man deduced from the Theory of 'Natural Selection'," *Journal of the Anthropological Society of London*, 2 (1864): clviii-clxxxvii.

distinguished the living human races. As humankind's mental faculties gradually developed through time, Wallace thought that humans would have been able to cope with the demand of conditions by mental skills only (weapons, shelters, division of labour, etc.). At this juncture, Wallace conjectured that the effect of natural selection would have shifted from advantageous physical characters to advantageous mental faculties, bringing the tranformation of the former almost to a stop, and fixing permanently the physical characteristics of the human races.

On this view, Wallace argued that the presence of humans in remote geological times was not only rendered possible but necessary. He saw no theoretical reason against our finding of human fossils or artefacts in Tertiary deposits. For reasons not discussed here[51], Wallace started to express by the second half of the 1860s a number of objections to the application of natural selection to humankind.[52]

Huxley recognized that the application of Darwinian principles, as seen in the *Origin of Species* (1859), could reconcile the strong points of both monogenetic and

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51 Compare S.J. Gould, "Natural Selection and the Human Brain: Darwin vs Wallace," in *The Panda's Thumb. More Reflections in Natural History* (New York: Norton, 1980), 47-58 and J.S. Schwartz, "Darwin, Wallace, and the *Descent of Man*," *Journal of the History of Biology*, 17 (1984): 271-289.

52 A.R. Wallace, "The limits of Natural Selection as Applied to Man," in *Natural Section and Tropical Nature: Essays on Descriptive and Theoretical Biology* (London: Macmillan, 1891), pp. 186-214. This paper was first published in 1870.

polygenetic hypotheses, even though Darwin had not, at the time, directly applied these principles to humankind.[53]

On the one hand, Huxley thought he could identify among living humans eleven distinct groups or stocks on the basis of their skin complexion, head conformation and morphology of hair.[54] He asserted that these stocks remained mostly unchanged in time, even under the effect of migrations and intermixtures as determined by the historical and archaeological records. He stated:

> "[t]o sum up our knowledge of the ethnological past of man; so far as the light is bright, it shows him substantially as he is now; and, when it grows dim, it permits us to see no sign that he was other than he is now".[55]

On the other hand, Huxley contended that it was unnecessary to postulate multiple simian origins for humankind. He wrote that:

> "...it may safely be affirmed that, even if the differences between men are specific, they are so small,

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53 T.H. Huxley, "On the Methods and Results of Ethnology," *Fortnightly Review*, 1865, reprinted in *Collected Essays (1893-1894)*, Vol. 7 (London: Macmillan, 1894), pp. 209-252.

54 A few years later Huxley recognized four distinct races among living humans, see T.H. Huxley, "On the Distribution of the Races of Mankind, and Its Bearing on the Antiquity of Man," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 3e session (1868): 92-97.

55 T.H. Huxley, "On the Methods and Results of Ethnology," *Fortnightly Review*, 1865, reprinted in *Collected Essays (1893-1894)*, Vol. 7 (London: Macmillan, 1894), p. 240.

> that the assumption of more than one primitive stock for all is altogether superfluous".[56]

Huxley assumed that humankind arose probably in a single locality, from which it spread under the pressure of changing conditions. Such conditions permitted the action of natural selection, and thus created instances of isolation among groups which permitted the hereditary fixation of the characteristics now distinguishing various human stocks.

Like Wallace and Huxley before him, Darwin also considered the monogenetic and polygenetic views as being far from contradictory. He asserted in *The Descent of Man* (1871) that when the reality of evolution was accepted, "the dispute between the monogenists and the polygenists will die a silent and unobserved death".[57] Without being as explicit as others when discussing the origin of humankind, Darwin particularly insisted on the arbitrary nature, in his mind, of the taxonomic debate. He stated:

> "Whether primeval man... would have deserved to be called man, must depend on the definition which we employ. In a series of forms graduating insensibly from some ape-like creature to man as he now exists, it would be impossible to fix on any definite point when the term 'man' ought to be used... So again it is almost a matter of indifference whether the so-called races of man are thus designated, or are ranked as species or sub-species; but the latter term appears the most appropriate."[58]

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[56] Ibid., p. 248.

[57] C.R. Darwin, *The Descent of Man, and Selection in Relation to Sex*, Vol. 1 (London: John Murray, 1871), p. 235.

Then clearly, Darwin took for granted that all human races were descended from a single stock, which diversified under the pressure of natural selection. It must be specified that Darwin relied not only on natural selection to explain the formation of the human races but also on other sources of variation among which sexual selection figured prominently. It is unclear if Darwin ever accepted Wallace's (1864) view that the human mental faculties developed to such a degree that, after a particular juncture, natural selection would no longer have affected human morphology.[59] What is absolutely clear, however, is that once Wallace raised objections as to the application of natural selection and other forms of naturalistic explanation to humankind, he and Darwin were separated by an irreconcilable gap on the question of man.

We have seen that the synthesis of the monogenetic and polygenetic positions was easily achieved in the minds of scientists when approached from an evolutionary perspective from the outset. However, after 1860 the impact of a growing consensus in favour of transmutation could be seen also among scientists who were traditionally polygenists or monogenists.

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[58] Ibid., p. 235.

[59] Compare M.J. Kottler, "Charles Darwin and Alfred Russel Wallace: Two Decades of Debate Over Natural Selection," in D. Kohn (ed.), *The Darwinian Heritage* (New Jersey: Princeton University Press, 1985), p. 420 and J.S. Schwartz, "Darwin, Wallace, and the *Descent of Man*," *Journal of the History of Biology*, 17 (1984), pp. 272-275.

In the late 1860s and early 1870s, Broca made clear that he accepted evolution. As a well known polygenist before that period, however, Broca would not accept without demonstration that humankind originated and diverged from a single stock.[60] Broca presented this objection to both monogenists and Darwinian evolutionists alike. In fact, what Broca favoured was an evolutionary conception called "polygenetic transformism" in which populations developed in parallel.[61] Instead of a single or a very limited number of stocks from which life on earth originated, Broca believed it more probable that numerous such origins had occurred in different places and times.

Broca contended that the power of natural selection to induce organic change was real but insufficient to account for the important differences seen among the major morphological types (*embranchements*) on earth. He hinted that some persistent conditions other than natural selection were also at work in the organic world. One wonders if he was not referring here to conservative forces maintaining the stability of morphological types over time. Broca argued, however, that not all the causes responsible for evolutionary change were yet known.

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[60] See the excellent analysis of C. Blanckaert, "'L'anthropologie personnifiée': Paul Broca et la biologie du genre humain," in P. Broca, *Mémoires d'Anthropologie* (Paris: Jean-Michel Place, 1989), i-xliii.

[61] P. Broca, "Le transformisme," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 5 (1870): 168-239.

Broca contended also that the irregular distribution of some characters among closely related species argued against their common origin. For him, the homology of morphological structure did not constitute evidence for a common origin. His 1869 memoir on the primate Order is relevant.[62] Although he concluded in it that humans represented a family within the Order *Primates*, just as Huxley and others had done previously, Broca did not interpret this close taxonomic proximity as evidence for a common phylogenetic origin, as seen throughout his other writings.

On such arguments, Broca held that things presented themselves *as if* humankind was descended from several distinct stocks. To support this view, he mentioned that the known Quaternary human races were as distinct as modern races were, providing therefore no evidence of convergence as should have been expected if they were indeed derived from a single stock.[63]

Whether or not humankind's origins were at all related to simian ancestors was, in Broca's mind, an unresolved question as yet. He recognized, however, that the gap separating humans from the other primates had been bridged to some extent by palaeontological evidence, and that expected

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62 P. Broca, "L'ordre des primates. Parallèle anatomique de l'homme et des singes," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 4: (1869): 228-401.

63 P. Broca, "L'anthropologie en 1868," *Encyclopédie générale, Almanach de 1869* (Paris, 1870), pp. 49-54; reproduced in P. Broca, *Mémoires d'Anthropologie* (Paris: Jean-Michel Place, 1989), pp. 510-520.

discoveries should close the gap further. Broca was especially referring here to the few simian characters he recognized in the Neandertal skull and the La Naulette jaw.

Broca never believed in the great modifying power of external conditions as often expounded by monogenists and evolutionists. Races and species were not, in Broca's mind, endowed with great flexibility to change. What he envisaged, instead, was the production of variability through crossbreeding and hybridization of races and species. This position was consistent with his "polygenetic tranformism", and would be valid according to him until proved otherwise.[64]

By 1860, de Quatrefages was already a devout monogenist.[65] We have already seen that de Quatrefages identified two distinct causes of change acting on two different levels. An unknown cause was responsible for the origin of species and another for intraspecific changes.

For de Quatrefages, humankind constituted a single species, as demonstrated especially through the fluidity of crossbreedings taking place among its living

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64 P. Broca, "Discours de M. Broca sur l'ensemble de la question," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 2e session, 1867, p. 402; C. Blanckaert, "'L'anthropologie personnifiée': Paul Broca et la biologie du genre humain," in P. Broca, *Mémoires d'Anthropologie* (Paris: Jean-Michel Place, 1989), pp. xxi-xxxi.

65 A. de Quatrefages, "Histoire naturelle de l'homme. Unité de l'espèce humaine," *Revue des Deux Mondes*, 30 (1860): 807-833; 31 (1861): 155-175, 412-435, 628-656, 938-969; *Unité de l'espèce humaine* (Paris: Hachette, 1861).

representatives.[66] Living humans were all part of the numerous races or varieties belonging to a single human species; races or varieties derived from, or affiliated to, a single primitive stock. De Quatrefages envisaged, therefore, that an early single human primitive type was gradually superseded in time through the action of changing conditions. These natural agencies were responsible for the formation of the primary races of humans. Then, through crossbreeding, these primary human races were transformed into secondary races or varieties. If the original human type was completely superseded, several of its traits persisted until today. On this view, the initial human type was not much transfigured, meaning that all changes that occurred to humankind since its origin took place within the limits of the same species: "Dolichocéphale ou brachycéphale, grand ou petit, orthognathe ou prognathe, l'homme quaternaire est toujours homme dans l'acception entière du mot".[67]

When considering the dispute between monogenists and polygenists, Topinard flatly stated that "[i]t no longer has any interest".[68] What he meant, of course, was that within an evolutionary framework the question took a different meaning and was now only of relative importance. He

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66 A. de Quatrefages, *L'espèce humaine*, 14e éd. (Paris: Alcan, 1905).

67 Ibid., p. 220.

68 P. Topinard, *Anthropology*, new edition (London: Chapman and Hall, 1890), p. 531. This book was first published as *L'Anthropologie* (Paris: Reinwald, 1876).

expressed his preference and summarized the debate as follows:

> "As to the question of the most elementary human types to which we might go back, types utterly irreducible, ...are they the issue of many Anthropoid ancestors, Pithecoids or others; or are they derived from a single stock...? The anthropological data given in this work appear to us more favourable to the former opinion, if we accept the transformation theory. The most characteristic races, whether living or extinct, do not form one single ascending series, such as may be compared to a ladder or a tree, but... to a series of frequently parallel lines."[69]

Topinard found support for the idea that humans were derived from anthropoids by recognizing what he thought to be simian characters in the Neandertal skull and the La Naulette jaw.

D) For a number of scientists, debates on humankind's closest living relatives, the hypothetical reconstruction of its ancestors and the relationship of its races in time were not clearly distinguished from one another. For them, these debates were part of a single argument about human evolutionary history.

According to Vogt, humans and apes were built on the same morphological plan, which testified to their genetic relationship.[70] He thought the evidence of such a genetic

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[69] Ibid., pp. 531-532.

[70] C. Vogt, *Lectures on Man: His Place in Creation, and in the History of the Earth* (London: Longman, Green, Longman, and Roberts, 1864), esp. pp. 461-468. This book was first published in German in 1863.

link were threefold. First, he recognized in human races (especially the "Negroes") a number of characters approaching the ape-type. Secondly, he identified among the ancient human remains ape-like characters, such as the elongated form and the low arching of the skull (Engis and Neandertal skulls). Thirdly, he recognized in human microcephalics a morbid arrest of development which might indicate an intermediate form between humans and apes. As he stated, the microcephalic:

> "...indicates one of the stages which the human embryo must necessarily pass through, and which now in its abnormity represents that intermediate form, which at a remote period may have been normal".[71]

However, far from suggesting a single ape-type ancestor for humankind, Vogt favoured a polyphyletic origin. On the one hand, he held that the differences between some human races were greater than those between some species of apes. He contended that the distinctiveness or plurality of the human races could be traced back into the prehistoric period as demonstrated by the cave-men of Belgium and the Rhenish provinces, and the Lapps of the Stone Age. On the other hand, Vogt argued that the anthropoid apes developed not from a single ape-type but in parallel from distinct stocks, as demonstrated by the fact that each of them had some

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71 Ibid., p. 462. This subject was further developed in C. Vogt, *Mémoire sur les microcéphales ou hommes-singes* (Genève-Bâle: H. Georg, 1867).

characters in which it approached humans. From there, Vogt contended that:

> "If in different regions of the globe anthropoid apes may issue from different stocks, we cannot see why these different stocks should be denied the further development into the human type, and that only one stock should possess this privilege; in short, we cannot see why American races of man may not be derived from American apes, Negroes from African apes, or Negritos, perhaps, from Asiatic apes!"[72]

Vogt explained the relative unity of modern humans by a process of convergence that took place in the course of time. The gaps existing between the distinct "ape-man" stocks were gradually filled by the rise of "mongrel races" through admixture and interbreeding.

For Schaaffhausen, the human type had evolved from a lower type of organization. Like Vogt, he thought that the traces of such an ascent were threefold: 1) as seen in the lower organization of some human races approaching the anthropoid conditions; 2) as evidenced by the primitive characters of a number of human fossils (the forehead of the Neandertal skull, the dentition and shape of the La Naulette jaw and the prognathism of some juvenile jaws of the Stone Age); and 3) as seen in the clues provided by human embryological development.[73]

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[72] C. Vogt, *Lectures on Man: His Place in Creation, and in the History of the Earth* (London: Longman, Green, Longman, and Roberts, 1864), pp. 466-467.

[73] H. Schaaffhausen, "On the Primitive Form of the Human Skull," *Anthropological Review*, 6 (1868): 412-431.

This approach permitted Schaaffhausen to suggest a reconstruction of the skull of a human precursor. Among many anatomical details, he listed: thick cranial bones, a dolichocephalic skull (when dealing with the origins of the European stock), straight or linear sutures and a projecting jaw and teeth. When turning to human phylogeny, Schaaffhausen hinted at a preference for a polyphyletic hypothesis:

> "Setting aside the changes the human skull undergoes by culture, there remain two rude types - the dolichocephalic and the brachycephalic skull. We possess no facts for their common origin; but that they are of different descent may be deduced from the circumstance that those regions of the globe in which the above types are strongly represented... Duvernoy was the first who opposed the dolichocephalic Chimpanzee to the brachycephalic Orang. Agassiz has pointed out that in Asia and Africa the large apes and the human races have the same colour of the skin. I myself have drawn attention to the fact that the gorilla is also dolichocephalic, and that the approximation of two human races to the apes of the same countries in colour and cranial form appears the most formidable objection to the unity of the human species in the present state of our knowledge"[74]

According to Haeckel, the living anthropoids and humans were closely related, as evidenced by comparative anatomy and embryology. He thought this assertion to be reinforced by the ape-like features seen in the four species (races) of living woolly-haired men.[75]

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[74] Ibid., p. 418.

[75] E. Haeckel, *The History of Creation: Or the Development of the Earth and Its Inhabitants by the Action of Natural Causes*, Vol. 2 (London:

Haeckel suggested that humans stemmed from an extinct Old World man-like ape. He imagined an hypothetical intermediate stage between this man-like ape and living humans and created *Pithecanthropus*, a speechless ape-like man. Haeckel argued that the development of the man-like ape into modern humans through *Pithecanthropus* occurred in two major adaptational steps.

The first step saw the specialization of the upper and lower parts of the body: the upper part adapted to the function of grasping and handling, the lower part to standing and walking. These new adaptations instigated changes to the vertebral column, the pelvic girdle and the shoulders. At this *Pithecanthropus* stage, the body was supposed to be already modern in appearance, with the exception of the brain. Heackel described this creature as possessing a long skull, thick woolly hair, a dark skin, long and strong upper limbs, short and thinner lower limbs. The creature was said to be semi-erect.

The second adaptational step was related to speech. With the development of an articulate language, humankind was supposed to have completed its transformation from *Pithecanthropus*. This was accompanied by the development of the brain and mental skills.

For Haeckel, humans were not descended from several ape forms but from a single one. When he considered the

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H.S. King, 1876). This book was first published in 1868 in German under the title *Natürliche Schöpfungsgeschichte*.

monogeny-polygeny debate from an evolutionary viewpoint, Haeckel explained what he meant:

> "[I]n a *wide sense*, the monophyletic opinion is the right one. For even supposing that the transmutation of Man-like Apes into Men had taken place several times, yet those Apes themselves would again be allied by the one pedigree common to the whole order of Apes... In a *narrower sense*... the polyphylist's opinion would probably be right inasmuch as the different primaeval languages have developed quite independently of one another. Hence, if the origin of an articulate language is considered as the real and principal act of humanification, and the species of the human race are distinguished according to the roots of their language, it might be said that the different races of men had originated, independently of one another, by different branches of primaeval, speechless men directly springing from apes, and forming their own primaeval language. Still they would of course be connected further up or lower down at their root, and thus all would finally be derived from a common primaeval stock."[76]

Haeckel has often been depicted as Darwin's most fervent advocate in Germany. It is true that Haeckel was greatly influenced by some of Darwin's conceptions and envisioned with him, at least at a superficial level, life as a divergent pattern. Although Haeckel stated that he wanted to complete Darwin's view, in reality he presented us with a rather different view of the process of evolution. At a general level, Haeckel concurred with Darwin that the driving factor of evolution was the adaptation to changing external conditions, although they disagreed on the nature of this process.[77]

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[76] Ibid., pp. 303-304.

Transmutation was not, for Haeckel, internally driven as Owen, Mivart and others believed, for he held that heritability was a conservative factor tending towards stability whereas adaptation was progressive leading towards change.[78]

Haeckel departed most visibly from Darwin in his conception of the relationship between embryology and phylogeny. According to Haeckel's recapitulation theory or biogenetic law, an organism's ontogeny recapitulates its phylogeny because the past course of evolution imposes on living organisms how their embryogenesis must develop. Bowler explains the distinction between Darwin and Haeckel in the following way:

> "[Darwin's] mechanism provided no justification for recapitulation: once the later stages of growth have been diverted into a new channel by variation, the old adult form vanishes and only the earlier stages of development remain unchanged. Earlier adult forms are preserved as stages in the growth of the modern embryo only if variation is an addition to, rather than a disturbance of, growth... It was no accident that Haeckel, the architect of the recapitulation theory, was a Lamarckian... Lamarckism can justify a belief in recapitulation because it assumes that variation is an addition to the growth process. If changes in the adult organism (acquired characters) are to be inherited, individual growth must be speeded up so that the old adult form becomes merely a stage through which the organism must pass to reach its new stage of maturity.

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77 On the last point see J. Roger, "Darwin, Haeckel et les Français," in Y. Conry, *De Darwin au darwinisme: science et idéologie* (Paris: Vrin, 1983), 149-165; and S.J. Gould, *Ontogeny and Phylogeny* (Cambridge: Harvard University Press, 1977), p. 80.

78 L.K. Nyhart, *Biology Takes Form: Animal Morphology and the German Universities, 1800-1900* (Chicago: University of Chicago Press, 1995), p. 132.

> Evolution takes place by the continued addition of new stages to growth... The recapitulation theory thus illustrates the non-Darwinian character of Haeckel's evolutionism... through its use as a vehicle for promoting the belief that individual growth offers a model through which evolution can be understood."[79]

Relying heavily on previous works of Darwin, Haeckel, Huxley, Schauffhausen, Vogt and others, Büchner classified humankind in a family of the order *Quadrumana*; humans having close phylogenetic affinities with the living great apes through a common ancestor, as evidenced by comparative anatomy and embryology.[80]

Büchner further assessed that ancient human remains such as the Neandertal skull and the jaws of La Naulette, Moulin-Quignon, Arcis-sur-Aube (Burgundy), Le Frontal and Ipswich were all characterized by evident signs of an "inferior" conformation. However, Büchner specified that the truly intermediate form between humans and animals had not been found yet, so that the gap separating them was still great. He merely insisted that the ancient human remains thus far discovered indicated the existence of a ruder, more animal-like and more lowly developed human race.

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79 P.J. Bowler, *The Non-Darwinian Revolution. Reinterpreting a Historical Myth* (Baltimore: Johns Hopkins University Press, 1988), pp. 75-76, 84.

80 L. Büchner, *Man in the Past, Present and Future. A Popular Account of the Results of Recent Scientific Research as Regards the Origin, Position and Prospects of the Human Race* (London: Asher & Co, 1872). This work was first published in German in 1869.

To the question whether the human type had originated from one or several simian sources (monogeny-polygeny), Büchner pointed to the possibility that science might never give us any satisfactory information about it. He insisted on the subordinate significance of this question with regard to the main point: the unity of these two hypotheses in the general theory of descent.

Broad Phylogenies: An Overview

Let us pause here and extract from the above discussion three important points for our thesis. First, evolutionary considerations were vigorously applied to humankind in the 1860-1895 period, no matter what were the different bases of this evolutionism. As soon as transmutation was generally accepted by scientists, the question of human origins was rapidly recast within this evolutionary framework. This was done mainly through the comparative anatomical approach. The anatomy and embryology of living human races and close primate relatives were systematically exploited. In addition, scientists such as Büchner, Broca, Hovelacque, Schaaffhausen, Topinard and Vogt identified a number of so-called simian characters among some human fossils which might be indicative of the ascent of man from a lower primate form. However, a large consensus prevailed at the time that the hominid fossil record was virtually devoid of any specimen which could represent an intermediate between modern humans

and a supposed primate ancestor.[81] In order to fill the gap, some scientists turned to the reconstruction of such an intermediate hypothetical form. For most scientists, the hominid fossil record seemed to be composed of relatively modern looking specimens, thus demonstrating that humankind's origins had to be searched in remote geological periods. It was agreed that living human races had deep geological roots.

Secondly, a number of scientists concerned with human evolution were also evolutionary theorists who made important contributions to the understanding of evolutionary processes. However, their interpretations of the pattern of human phylogeny seemed not to be clearly linked to their respective evolutionary theories, except perhaps those of Mivart. In other words, their human phylogenetic interpretations were distinct neither from those of one another nor from those of other scientists who contributed little or nothing to evolutionary theories. It seems, therefore, that the field of human evolution was not permeated with specific evolutionary views on processes. Instead, what was suffused

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[81] Among others see T.H. Huxley, *Evidence as to Man's Place in Nature* (London: Williams & Norgates, 1863), reprinted in *Thomas Henry Huxley. Collected Essays (1893-1894)*, Vol. VII (Hildesheim: Georg Olms Verlag, 1970), p. 205; C. Vogt, *Lectures on Man: His Place in Creation, and in the History of the Earth* (London: Longman, Green, Longman, and Roberts, 1864), p. 463; H. Schaaffhausen, "On the Primitive Form of the Human Skull," *Anthropological Review*, 6 (1868), p. 431; C.R. Darwin, *The Descent of Man, and Selection in Relation to Sex*, Vol. 1 (London: John Murray, 1871), p. 185; E. Haeckel, *The History of Creation: Or the Development of the Earth and Its Inhabitants by the Action of Natural Causes*, Vol. 2 (London: H.S. King, 1876), p. 326; P. Topinard, "Les dernières étapes de la généalogie de l'homme," *Revue d'Anthropologie*, 3 (1888), p. 332.

into the field of human evolution was only a general evolutionary perspective. Undoubtedly, the nature of the human fossil record and the fact that scientists had to rely on comparative anatomy of living forms to suggest evolutionary links greatly obstructed the proposing of specific evolutionary patterns based on evolutionary processes.

This raises the interesting question of the nature of the link between evolutionary processes (theories) and evolutionary patterns (phylogenies) in the minds of scientists of the time. A number of palaeontologists during the 1860-1930 period reflected on evolutionary theories or processes.[82] For them, a commitment to a particular evolutionary process often influenced their interpretations of evolutionary patterns. On this view, Bowler rightly stresses the impact that evolutionary theories had on the assessment of human phylogeny during that time.[83] However,

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82 Consult for instance, P.J. Bowler, *The Eclipse of Darwinism. Anti-Darwinian Evolution Theories in the Decades around 1900* (Baltimore: Johns Hopkins University Press, 1983); S.J. Gould, "Dollo on Dollo's Law: Irreversibility and the Status of Evolutionary Laws," *Journal of the History of Biology*, 3 (1970): 189-212; R. Rainger, "Just Before Simpson: William Diller Matthew's Understanding of Evolution," *Proceedings of the American Philosophical Society*, 130 (1986): 453-474; "Vertebrate Paleontology as Biology: Henry Fairfield Osborn and the American Museum of Natural History," in R. Rainger, K.R. Benson and J. Maienschein (eds.), *The American Development of Biology* (Philadelphia: University of Pennsylvania Press, 1988), pp. 219-256; "What's the Use: William King Gregory and the Functional Morphology of Fossil Vertebrates," *Journal of the History of Biology*, 22 (1989): 103-139; W.-E. Reif, "The Search for a Macroevolutionary Theory in German Paleontology," *Journal of the History of Biology*, 19 (1989): 79-130.

83 P.J. Bowler, *Theories of Human Evolution. A Century of Debate, 1844-1944* (Baltimore: Johns Hopkins University Press, 1986).

the impact of evolutionary theories on the establishment of evolutionary patterns should not be assumed but rigorously documented. In other words, it should not be assumed that debates on evolutionary theories constitute the proper analytical framework against which the rise of the field of human evolution should be investigated. Rainger convincingly argued that post-Darwinian palaeontologists were influenced in their interpretations of the fossil record largely by their scientific training and especially by the limited data at their disposal, rather than by a commitment to particular evolutionary theories or by extra-scientific factors.[84] Although this essay is not concerned primarily with the evolutionary pattern/process question, it will be seen below that numerous scientists seemed to establish human phylogenies by merely recognizing morphological links between living and fossil forms, without an apparent commitment to any evolutionary process. This seems to have been true especially of scientists who were concerned with the prehistoric peopling of Europe, and later of the world.

Thirdly, the impact of a new evolutionary framework on the field of human evolution seems to have recast the monogeny-polygeny debate in this new context. The views presented by Büchner, Darwin, Huxley, Topinard and Wallace support this interpretation. On such an approach, an

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[84] R. Rainger, "Paleontology and Philosophy: A Critique," *Journal of the History of Biology*, 18 (1985): 267-287.

equation between monogeny-monophyly on the one hand and polygeny-polyphyly on the other existed.[85]

However, while the monogeny-polygeny debate was recast in an evolutionary perspective by a number of scientists, a scientist such as Broca would not have subscribed to this approach.[86] Although an evolutionist, Broca clung to his polygenist's stance for he argued that the data at his disposal demonstrated only that, as far back in time as the human races could be traced, these were as distinct as they were today. In this perspective, how could human races be linked to any animal form whether through a monophyletic or a polyphyletic conception? Although Broca recognized evidence in favour of a simian ancestry for humankind, he was too much of a positivist to take a clear stance in the monophyly-polyphyly debate; a debate he might have judged to be premature.

Restricted Phylogenies

We have seen above how evolutionary ideas were vigorously applied to human phylogeny during the 1860-1895 period. Yet, in the establishment of what we referred to as broad phylogenies, the hominid fossil record was of little

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[85] See H.V. Vallois, "Monophyletism and Polyphyletism in Man," *South African Journal of Science*, 49 (1952): 69-79.

[86] See also J. Hunt, "On the Application of the Principle of Natural selection to Anthropology," *Anthropological Review*, 4 (1866): 320-340; and G. Pouchet, *De la pluralité des races humaines. Essai anthropologique*, 2nd ed. (Paris: V. Masson, 1864).

use as the ancient remains were interpreted by a majority of scientists as of modern looking appearance. Instead, comparative anatomy and embryology were extensively used to suggest possible evolutionary links between humans and other primates. However, the full potential of the ancient human remains was exploited in the establishment of the prehistoric peopling of Europe to which we now turn. We have referred to this latter approach as the establishment of restricted phylogenies.

There were no differences between broad and restricted phylogenies except one of scale. In other words, what distinguished broad from restricted phylogenies was not mainly related to differences of conception as regards the transmutation theory. Instead, the nature of the fossil material obliged scientists to proceed first to the question of the prehistoric peopling of Europe, whilst leaving concerns for the evolutionary links between humans and primates to analyses based on comparative anatomy and embryology of living forms. The distinction between broad and restricted phylogenies was created by an *epistemological problem*.

A useful way to grasp the situation in human evolution during that period is to compare it with the practice of vertebrate palaeontology. Rainger argued convincingly that vertebrate palaeontologists were often constrained to produce phylogenetic trees of higher taxonomic levels (genera,

families), because of the paucity of vertebrate fossils, among other reasons.[87] In human evolution, as will be seen below, the fossil material at scientists' disposal constrained them to investigate human phylogeny at lower taxonomic levels (individuals, populations, races).

Two closely related assumptions made in the recent literature about the study of what we called restricted phylogenies during the 1860-1895 period should be qualified. First, as human fossils were often interpreted as being representative of ancient European races rather than of evolutionary stages in human evolution, it has sometimes been assumed that it is because the idea of evolution was not widely spread or accepted yet. Thus, Tattersall asserts:

> "But as long as the idea of the mutability of species (as opposed to varieties of species) was entertained only as an abstraction, as a philosophical concept rather than an organizing principle of the diversity of life, what... [everyone] necessarily lacked was the idea of common descent among species... That the Neanderthaler... might represent not a variety but a relative - perhaps even an ancestor - of modern humankind was totally foreign to the prevailing mindset."[88]

This alleged opposition between an evolutionary perspective and one concerned to establish human's racial past is maintained also by Leguebe:

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[87] R. Rainger, "Paleontology and Philosophy: A Critique," *Journal of the History of Biology*, 18 (1985): 267-287.

[88] I. Tattersall, *The Fossil Trail: How We Know What We Think We Know about Human Evolution* (Oxford: Oxford University Press, 1995), p. 15.

> "Une telle position [to consider the Neandertal as a race and not as a distinct species from modern human] est celle adoptée par beaucoup de naturalistes de l'époque: elle constitue un moyen de réduire l'impact que pourrait avoir, sur la généalogie de l'espèce humaine, la faveur grandissante que connaissent les thèses évolutionnistes...".[89]

We have already seen above how the transmutation theory was widely embraced in the second half of the nineteenth century. Therefore, we should look elsewhere to explain why ancient human remains were interpreted as representatives of prehistoric European races.

Secondly, a frequent assumption made about the reception given to human fossils is that they were interpreted either as representing an early stage of human evolution by evolutionists or as modern humans deformed by pathology according to anti-evolutionists. For instance, Reader wrote:

> "Broadly speaking there were two points of view. The physical peculiarities of Neanderthal Man represented either an early stage of human evolution linking man to an ape-like ancestor, or pathological deformities of modern man more gross than any medical science had ever encountered... Those willing to accept the theory [of evolution] believed the remains were very old and freely discussed their primitive, 'barbarous' and ape-like characteristics in evolutionary terms. Those opposed to the theory of evolution... believed the remains were of modern man and sought a modern, medical explanation for their peculiarities."[90]

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89 A. Leguebe, "Importance des découvertes de Néandertaliens en Belgique pour le développement de la paléontologie humaine," *Bulletin de la Société Royale Belge d'Anthropologie et de Préhistoire*, 97 (1986), p. 19.

90 J. Reader, *Missing Links: The Hunt for Earliest Man* (London: Collins, 1981), p. 25.

Again, Trinkaus and Shipman held:

> "[Considering the discovery of the Neandertal skull, Schaaffhausen] thought he had found direct evidence supporting his theory of the mutability of species and the descent of humans from apes... [Whereas] the Neandertal remains were pathological in Virchow's terms in that they were unlike modern humans... Underlying all this was his deep dislike of evolutionary ideas... In this regard, he fought acceptance of the Neandertals because they implied that evolution occurred... Virchow tried to suppress the acceptance of evolutionary ideas or any hominid fossils by every means possible until he died in 1902."[91]

Some anti-evolutionists might have held that some human fossils were modern pathological forms. However, it must be noted that some sympathizers of the transmutation theory, such as Blake and Hunt[92], also held that the Neandertal skull was pathological.[93] Before we accept the assertion that some scientists opted for the pathological interpretation of these remains because they wanted to counteract the evolutionists'

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91 E. Trinkaus and P. Shipman, *The Neandertals: Changing the Image of Mankind* (New York: Alfred A. Knopf, 1993), pp. 53, 58, 59-60.

92 C. Blake, "Man and Beast," *Anthropological Review*, 1 (1863), pp. 161-162; "On the Alleged Peculiar Characters, and Assumed Antiquity of the Human Cranium from the Neanderthal," *Journal of the Anthropological Society of London*, 2 (1864), p. clvii; "Comments on Wallace's Paper," *Journal of the Anthropological Society of London*, 2 (1864), p. clxxvii; J. Hunt, "On the Application of the Principle of Natural Selection to Anthropology," *Anthropological Review*, 4 (1866), p. 340; "On the Doctrine of Continuity Applied to Anthropology," *Anthropological Review*, 5 (1867), p. 112.

93 C. Blake, "On the Crania of the Most Ancient Races of Men," *The Geologist*, 5 (1862), p. 207; J. Hunt, "Editor's Preface," in C. Vogt, *Lectures on Man: His Place in Creation, and in the History of the Earth* (London: Longman, Green, Longman, and Roberts, 1864), p. xv.

movement, we require carefully documented cases. Two points must be insisted upon here.

A) Scientists repudiating a simian ancestry for mankind should not be considered as necessarily anti-evolutionists. After all, humankind could have been derived from non-simian forms as Mivart suggested.[94]

In fact, it is surprisingly difficult to find clear and open anti-evolutionary statements after 1870 in the circle of human palaeontologists. Without being avowed evolutionists, a great number of scientists after 1860 considered transmutation to be either a valid hypothesis or accepted it in principle as being highly probable, although perhaps insufficiently documented or explained as yet. Of great value here is the distinction introduced by Harvey between materialist scientists who judged hypothetical statements to be a crucial part of the scientific enterprise and positivist scientists who refrained from speculative statements, preferring more solid grounds to build science on.[95]

As the nineteenth century progressed and the doctrine of evolution was being accepted, it became more and more

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94 St. George Mivart, *Man and Apes, an Exposition of Structural Resemblances and Differences Bearing Upon Questions of Affinity and Origin* (London: Robert Hardwicke, 1873).

95 J. Harvey, "Evolutionism Transformed: Positivists and Materialists in the *Société d'Anthropologie de Paris* from the Second Empire to Third Republic," in D. Oldroyd and I. Langham (eds.), *The Wider Domain of Evolutionary Thought* (Dordrecht: D. Reidel, 1983), 289-310.

difficult to take a truly anti-evolutionary stance without suggesting a valid scientific alternative.

B) Beside an anti-evolutionary stance, there were positive scientific reasons to defend the interpretation that the Neandertal remains were pathological.

For instance, Davis held that the apparent simian configuration of the Neandertal skull was caused by a pathological deformation such as synostosis, that is, premature closing of sutures. He referred to Virchow, Lucae and Welcker as authorities who contributed brilliantly to the study of such processes. Far from being of rare occurrence, Davis pointed to ancient races in Italy and Britain that seemed to have been affected by synostosis to an uncommon frequency, thus breaking the apparent isolation of this phenomenon in the Neandertal specimen.[96] The understanding of the growth and development of skulls was considered at the time to be a key issue in craniology.[97]

Furthermore, pathology could also serve as a basis for support of an evolutionary perspective. For instance, Étienne Geoffroy Saint-Hilaire and Richard Owen were both

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[96] B. Davis, "The Neanderthal Skull: its Peculiar Conformation explained Anatomically," *Memoirs Read Before the Anthropological Society of London*, 1 (1863-1864): 281-295; and "De la valeur réelle de la forme spéciale d'un fragment de crâne trouvé dans la caverne de Néanderthal," *Bulletins de la Société d'Anthropologique de Paris*, 5 (1864): 708-718.

[97] G. Retzius, "Sur l'étude craniologique des races humaines," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 7e session, 1874: 693-740.

inspired, before 1860, by teratology (the study of abnormal ontogenetic development) as a possible mechanism of transmutation.[98]

For Virchow, every variation was a disruption of the organism and was therefore pathological by definition. But if such variations or deviations were passed on through the inheritance of acquired characteristics, the passage from one type to another could thus be completed. By such means, new varieties, species and maybe genera could have been formed.[99] However, Virchow considered human fossils such as the Neandertal skull and the La Naulette jaw to be only isolated instances of individual pathology[100]; they were not to be confused with new races that originated from pathology.[101] He contended that, although it was possible to imagine that new human races may arise by such a process, still there were no

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98 See J.-L. Fischer, "Le concept expérimental dans l'oeuvre tératologique d'Étienne Geoffroy Saint-Hilaire," *Revue d'Histoire des Sciences*, 25 (1972): 347-364; G. Laurent, "Le cheminement d'Étienne Geoffroy Saint-Hilaire (1772-1844) vers un transformisme scientifique," *Revue d'Histoire des Sciences*, 30 (1977): 43-70; E. Richards, "A Question of Property Rights: Richard Owen's Evolutionism Reassessed," *British Journal for the History of Science*, 20 (1987): 129-171.

99 F.B. Churchill, "Rudolf Virchow and the Pathologist's Criteria for the Inheritance of Acquired Characteristics," *Journal of the History of Medicine and Allied Sciences*, 31 (1976): 117-148.

100 R. Virchow, "Untersuchung des Neanderthal-Schädels," *Verhandlungen der Berliner Gesellschaft für Anthropologie, Ethnologie und Urgeschichte*, 1872: 157-165; "Der Kiefer aus der Schipka-Höhle und der Kiefer von La Naulette," *Zeitschrift für Ethnologie*, 14 (1882): 277-310.

101 R. Virchow, "Discussion," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 7e session, 1874: 327-328.

positive observations to support this.[102] The position of Virchow on human fossils will be further discussed below.

However, there is more to it. As will be seen below, ancient human remains were largely used to establish the prehistoric peopling of Europe during the 1860-1895 period. On this view, how could anti-evolutionists try to counteract the evolutionary interpretation of human fossils, if evolutionists themselves were not engaged in research activities that could be considered to be "typically" evolutionary? That is to say, they were not engaged in identifying evolutionary stages of humankind with the assistance of the fossil record. It might even be argued that there was no need to be an evolutionist in order to study the prehistoric peopling of Europe (see discussion below).

In fact, it is not clear at all who were the scientists that supposedly proposed such "typical" evolutionary interpretations. For instance, when considering the Neandertal skull, Blake, by no means an anti-evolutionist, wrote that some "writers authoritatively declared that we had at last discovered the 'missing link' which binds man and the apes", although he failed to identify these writers.[103]

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102 R. Virchow, "Sur les anciens crânes du nord-est de l'Allemagne et sur la méthode pour juger leurs particularités," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 2e session, 1867: 406-407.

103 C. Blake, "On the Alleged Peculiar Characters, and Assumed Antiquity of the Human Cranium from the Neanderthal," *Journal of the*

In the same fashion, Pruner-Bey, having in mind the Neandertal skull and the La Naulette jaw, declared that "on parle de crânes, de mâchoires simiennes ou intermédiaires à l'homme et au singe... qui laisse supposer que... l'homme pithécoïde... est un fait accompli", without referring to any scientist who supposedly held such a view.[104]

For his part, Davis contended that some authors seemed to be disposed to interpret the Neandertal skull as a link between humankind and the other primates, although he specified that this position had not been clearly stated by anyone, and also failed to name any scientist who might have alluded to it.[105]

At last, Mayer mentioned that Huxley and others would have held that the Neandertal skull was a kind of intermediate link between man and the apes.[106] Huxley rightly replied to this misconstrued view.[107]

Anthropological Society of London, 2 (1864), p. cxliv; "On Certain 'Simious' Skulls, with especial reference to a skull from Louth, in Ireland," *Memoirs Read Before the Anthropological Society of London*, 2 (1865-1866), p. 80.

[104] F. Pruner-Bey, "Discussion sur le transformisme," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 4 (1869), p. 674.

[105] B. Davis, "The Neanderthal Skull: its Peculiar Conformation explained Anatomically," *Memoirs Read Before the Anthropological Society of London*, 1 (1863-1864), p. 281; and "De la valeur réelle de la forme spéciale d'un fragment de crâne trouvé dans la caverne de Néanderthal," *Bulletins de la Société d'Anthropologique de Paris*, 5 (1864), p. 714.

[106] F. Mayer, "Ueber die fossilen Ueberreste eines menschlichen Schädels und Skeletes in einer Felsenhöhle des Düssel- oder Neander-Thales," *Archiv für Anatomie, Physiologie und Wissenschaftliche Medicin*, 1 (1864): 1-26.

It is true that scientists like Büchner, Broca, Hovelacque, Huxley, Schaaffhausen, Topinard and Vogt identified a number of so-called simian characters among human fossils. These were interpreted as support for the hypothesis of the human's simian ancestry, but everyone agreed that no intermediate form or stage between humans and a putative simian ancestor had yet been discovered. Also, it should be noted that terms such as "pithecoid", "apelike" and "simious" were often used at the time in a strictly descriptive sense without any real evolutionary implication, as Spencer correctly noted.[108]

In fact, most if not all human fossils discovered during the 1860-1890 period were considered to be, generally speaking, modern looking. Possibly the only clear statement to the contrary was King's proposition to classify the Neandertal skull in a new species (if not a new genus) called *Homo neanderthalensis*. King judged the Neandertal skull to have the closest affinity to the apes, especially the chimpanzee. Unfortunately, he failed to specify the

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107 T.H. Huxley, "Further Remarks Upon the Human Remains from the Neanderthal," *Natural History Review*, 4 (1864): 430-437.

108 F. Spencer, "The Neandertals and Their Evolutionary Significance: A Brief Historical Survey," in *The Origins of Modern Humans: A World Survey of the Fossil Evidence*, eds. F. Smith and F. Spencer (New York: Alan R. Liss, 1984), pp. 6-7. To be convinced of the real limitations of these terms in the minds of scientists see the exchange that occurred between Denicker, de Quatrefages, Fraipont, Manouvrier and Topinard in "Discussion," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 10e session, 1889: 348-362.

evolutionary implications of his position, if there were any.[109]

In this perspective, whether human fossils should be interpreted as modern pathological forms or as more or less ancient, modern-looking human representatives, the gulf separating these two views was far from being as irreconcilable as some recent observers have claimed.

Let us now turn to the analysis of what we called restricted phylogenies during the 1860-1895 period in order to further substantiate some of our claims.

One difficulty in the assessment by several modern observers of the rise of the field of human evolution has been their focus on the reception given to a limited number of human fossil discoveries. By so doing, they have extracted the field of human evolution from its historical context.

It is true that some discoveries made during the second half of the nineteenth century generated considerable discussion at the time, discussion that tried to establish the status of these remains. This is especially true of the Engis skull discovered in Belgium in 1833, the Neandertal skull discovered in Germany in 1856 and the La Naulette jaw discovered in Belgium in 1866.[110] These remains were

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109 W. King, "The Reputed Fossil Man of the Neanderthal," *Quarterly Journal of Science*, 1 (1864): 88-97.

110 In addition to the literature already cited, see C. Blake, "On a Human Jaw From the Cave of La Naulette, Near Dinant, Belgium,"

interpreted along the following lines: 1) they were held as being within the normal variation of the human species; 2) as instances of human pathological variation; 3) as human variation previously unknown; or, 4) as combination of such variants.

But the interpretation of these human remains was also part of a wider context that needs to be addressed.

If the monogeny-polygeny debate permitted scholars to think of humankind's genealogy in a pre-evolutionary context, as well as in an evolutionary one (under the monophyly-polyphyly form), such a debate would not constitute the main framework within which ancient human remains were to be interpreted. A less general framework was used for a discipline that was to be, after all, restricted to Europe for several decades to come. The consideration over the peopling of Europe constituted such a framework.

Anthropological Review, 5 (1867): 294-303; P. Broca, "Observations sur le crâne de Néanderthal," *Bulletins de la Société d'Anthropologie de Paris*, 4 (1863): 322-323; "Sur la mâchoire humaine de la Naulette (Belgique)," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 1 (1866): 593-601; T.H. Huxley, *Evidence as to Man's Place in Nature* (London: Williams & Norgates, 1863); F. Pruney-Bey, "Observations sur le crâne de Néanderthal," *Bulletins de la Société d'Anthropologie de Paris*, 4 (1863): 318-322; "Sur la mâchoire humaine de la Naulette (Belgique)," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 1 (1866): 584-593; H. Schaaffhausen, "On the Crania of the Most Ancient Races of Man," translated from Müller's Archiv (1858) with remarks by G. Busk, *Natural History Review*, 1 (1861): 155-176; P. Topinard, "Les caractères simiens de la mâchoire de la Naulette," *Revue d'Anthropologie*, 3e série, 1 (1886): 385-431; and W. Turner, "On Human Crania Allied in Anatomical Characters to the Engis and Neanderthal Skulls," *Quarterly Journal of Science*, 1 (1864): 250-259; "Additional Note on the Neanderthal Skull," *Quarterly Journal of Science*, 1 (1864): 758-760.

One such view or model of the peopling of Europe widely held during the first part of the nineteenth century suggested that most peoples of Europe originated in Asia. This view was based on the results of comparative philology that indicated the affiliation of the Indo-European tongues. It was often referred to as the Aryan origin. It led to the assumption that European races which belonged to the Indo-European family were all derived from a single stock. The non-Aryan tongues in use among Europeans, being sometimes affiliated to the Mongoloid or Touranian family, were attributed to remnant populations of past races that inhabited Europe before the arrival of the Aryans.

This view was strengthened when an anatomical dimension was added to it. The first anatomical investigations of the living races of Europe belonging to these two alleged ethnic groups (pre-Aryans and Aryans) by Retzius in the 1840s and 1850s tended to demonstrate that the first inhabitants of Europe were brachycranial (among whom the Finns, the Lapps, the Basques and sometimes the Estonians and Ligurians figured) while the invaders of the Aryan races were dolichocranial (generally designated as the Celts).[111] In Retzius's own words:

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111 C. Blanckaert, "L'indice céphalique et l'ethnogénie européenne: A. Retzius, P. Broca, F. Pruner-Bey (1840-1870)," *Bulletins et Mémoires de la Société d'Anthropologie de Paris*, new series, 1 (1989): 165-202, see esp. pp. 182-183.

> "This paper consisted in the Application of the theory of Arndt, Rask and others, as to the general diffusion of a race akin to the Finns over the Whole of Europe anterior to the immigration of the Indo-European tribes. The Celt, generally considered as the earliest inhabitants of the British Isles, has a skull remarkable for its diameter from front to back [dolichocrany]. Such, also, are the skulls found in barrows of secondary antiquity. In the most ancient, however, the skull has its chief development from side to side [brachycrany]; the conformation of the aboriginal nations hypothetically allied to the Finns and Laplander."[112]

This view of the peopling of Europe as a superposition of two or more main ethnic groups was to provide a framework to organize the discoveries of ancient human remains, at least for a number of scientists during the 1860s. It should be noted, however, that the precedence in geological times of the brachycranial type over the dolichocranial one, as Retzius maintained, was not always accepted by proponents of this model.

For the evolutionist Schaaffhausen, who held that the peculiar conformation of the Neandertal skull exceeded the known range and was not known to exist even in the most "barbarous" races, the Neandertal skull belonged to an individual who was probably derived from one of the wild races of north-western Europe at a period prior to the Celts and Germans, but encountered by the German immigrants on arriving in Europe. Far from being completely isolated in

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112 A. Retzius, "On certain American, Celtic, Cimbric, Roman and Ancient British Skulls," *Report of the Nineteenth Meeting of the British Association for the Advancement of Science, Notices and Abstracts* (London: J. Murrey, 1850), p. 86.

Schaaffhausen's mind, the Neandertal skull was compared to other more or less ancient remains:

> "The fragments of crania from Schwaan and Plau [Germany]... may probably be assigned to a barbarous, aboriginal people, which inhabited the North of Europe before the *Germani*; and, as is proved by the discovery of similar remains at Minsk in Russia, and in the Neanderthal near Elberfeld, must have been extensively spread - being allied, as may be presumed from the form of the skull, with the aboriginal populations of Britain, Ireland, and Scandinavia."[113]

Recognizing in some living humans traces of a markedly prominent supraorbital region, Schaaffhausen contended that this trait represented the vestiges of a primitive type which was manifested in the Neandertal skull.

When turning to the question of humankind during the Quaternary period, Spring suscribed to the evolutionary approach promoted by the palaeontologist d'Omalius d'Halloy. This approach recognized that species slowly transmuted from one stratum to another, with the introduction and extinction of a number of species in each time period.[114]

Not unlike de Mortillet at a later time (see below), Spring was to rely heavily on archaeological remains to

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113 H. Schaaffhausen, "On the Crania of the Most Ancient Races of Man," translated from Müller's Archiv (1858) with remarks by G. Busk, *Natural History Review*, 1 (1861): 155-176, citation on pp. 171-172.

114 For an account of the type of evolutionism expounded by d'Omalius d'Halloy from the 1830s through the 1870s consult G. Laurent, *Paléontologie et évolution en France de 1800 à 1860: Une histoire des idées de Cuvier et Lamarck à Darwin* (Paris: Édition du C.T.H.S., 1987), pp. 395-403.

establish both a chronological framework and the means to follow the ancient races of Europe.[115] The most ancient clearly recognizable human form Spring identified in the Quaternary he called the Engis race. This race, essentially composed of the morphological remains from Engis, Engihoul, Maestricht and Moulin-Quignon, was described as being dolichocephalic and orthognathic. Spring conjectured that this race migrated into the north-western part of Europe from the south.

After a long period of transition during which the modern fauna and flora of Europe took shape, Spring identified a new human race characterized by its small stature and its brachycephalic and orthognathic conformation which he named the Chauvaux race. This latter race, said to be a Nordic one that spread towards the south of Europe, was represented by the remains of Chauvaux, Bruniquel and a host of remains from France, Switzerland, Ireland and Denmark. The Basques, the Finns and the Lapps were, in Spring's view, all affiliated to this ancient race. After some time, the Chauvaux race was confronted by the invasion of Aryan peoples (the Celts and the Germans) of dolichocephalic conformation which were equipped with superior technologies.

Spring excluded the Neandertal skull from his discussion because he contended that it had as yet no known equivalent.

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115 A. Spring, "Les hommes d'Engis et les hommes de Chauvaux," *Bulletin de l'Académie royale des sciences, des lettres et des beaux-arts de Belgique*, 2e série, 18 (1864): 479-515.

Under such circumstances, he judged it preferable not to rely on the Neandertal skull when considering questions pertaining to transmutation (*doctrine du développement progressif*).[116]

Pruner-Bey was possibly the most influential scientist who promoted the Aryan origin model. He inserted into this model most human fossils known by the 1860s. Pruner-Bey recognized two morphological types in prehistoric Europe. The most ancient race of Europe, constituted essentially of brachycranial skulls and associated remains, was of short stature and part of the mongoloid stem that included numerous inhabitants living today in Europe, Asia and America. The other prehistoric race of lesser antiquity, mostly represented by dolichocranial skulls and associated remains, was of tall stature and part of the migration that brought the Celts (and their civilization) to Europe.

In the ancient mongoloid type, Pruner-Bey included human remains from d'Arcy, Aurignac, Bruniquel, Furfooz (trou du Frontal), La Naulette and Solutré. In the Celtic type, he gathered the remains from Engis, Lombrive, Moulin-Quignon, Neandertal and Vauréal.[117]

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[116] Ibid., footnote #3 on pp. 489-490.

[117] F. Pruney-Bey, "Discours de M. Pruner-Bey sur la question anthropologique," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 2e session, 1867: 345-360; "Sur le maxillaire humain fossile trouvé par M. Dupont et présenté par M. Pruner-Bey," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 1 (1866): 618; "Observations sur le crâne de Néanderthal," *Bulletins de la Société d'Anthropologie de Paris*, 4 (1863): 318-322; "Sur la mâchoire humaine de la Naulette (Belgique)," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 1 (1866): 584-593.

After the discovery of the Cro-Magnon remains in 1868, Pruner-Bey linked these skulls to his mongoloid type (being affiliated to the living Estonians) regardless of their dolichocranial conformation. He justified this latter view by arguing first that their dolichocrany differed from the Celts', and secondly that it became clear as the science of craniology progressed that the brachycrany-dolichocrany dichotomy was of limited value because both forms often existed in a single race. Therefore, he argued that other morphological characters needed also to be considered to establish race identity.[118]

Pruner-Bey held that far from being limited to the ancient period of Europe, these two morphological types persisted through time until today, although they both underwent some morphological change. For instance, he argued that the ancient Celts (including the Neandertal skull) had a sloping forehead whereas their modern representatives greatly attenuated this feature. Pruner-Bey also acknowledged that intermediate forms between the two morphological types existed, although he contended that their recognition depended first on the proper assessment of the types' limits, and secondly on the evaluation of the effect of their admixture.[119]

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[118] F. Pruney-Bey, "Sur les ossements humains des Eyzies," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 3 (1868): 416-432; "Les crânes des Eyzies et la théorie esthonienne," *Bulletins de la Société d'Anthropologie de Paris, 2e série*, 3 (1868): 511-514.

In essence, de Quatrefages was to agree with Pruner-Bey's conception of the peopling of Europe.[120] According to this view, the Neandertal skull was that of a Celt and brachycranial skulls antedated dolichocranial ones. Although agreeing that the peopling of Europe had occurred as a superposition of two distinct ethnic elements, de Quatrefages was to suggest, not unlike Pruner-Bey, that the first inhabitants of Europe might have been represented by both types of skull conformation (although brachycranial skulls greatly predominated), thus possibly explaining the discoveries of ancient dolichocranial skulls at Engis and Eguisheim. In other words, what de Quatrefages envisaged was the possibility of an early peopling of Europe from a polymorphic mongoloid stem comprising both brachycranial and dolichocranial races followed by a somewhat later and similarly polymorphic Celtic stem.

The models suggested by Retzius, Spring, Pruner-Bey and de Quatrefages to explain the peopling of Europe were rapidly challenged. They were challenged both by new empirical evidence against the view that brachycrany antedated dolichocrany, and by a new conception that envisioned prehistoric Europe not as a superposition of two distinct

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[119] F. Pruney-Bey, "Discours de M. Pruner-Bey sur la question anthropologique," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 2e session, 1867, pp. 353-357.

[120] A. de Quatrefages, *Rapport sur les progrès de l'anthropologie* (Paris: Imprimerie Impériale, 1867), pp. 251, 257-270, 480-487.

ethnic elements but as a complex cohabitation of several more or less ancient and distinct racial elements.

Early in the 1860s, it was realized that brachycrany did not antedate dolichocrany everywhere.[121] At the same time, it was realized also that some living populations, thought at first to be either dolichocephalic or brachycephalic after the investigation of small samples by Retzius, and on which the Aryan origin model was founded, were in reality of the opposite morphological type.[122]

These challenges were easily bypassed or incorporated by the proponents of the Aryan origin model or the Mongoloid theory, by either holding that the Mongoloid and Celtic stems were polymorphic, or by simply dismissing the supposed antiquity of some dolichocranial skulls. The latter procedure was far from being illegitimate considering the great uncertainty surrounding the dating of human remains at the time.[123]

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121 See for instance C. Blake, "On the Crania of the Most Ancient Races of Men," *The Geologist*, 5 (1862): 205-233; W. His, "Sur la population rhétique," *Bulletins de la Société d'Anthropologie de Paris*, 5 (1864): 868-880; G. Van Düben, "Sur les crânes de l'âge de pierre, en Suède," *Bulletins de la Société d'Anthropologie de Paris*, 6 (1865): 168-170.

122 P. Broca, "Sur les crânes basques," *Bulletins de la Société d'Anthropologie de Paris*, 4 (1863): 38-62; H. Welcker, "Sur les caractères des crânes esthoniens," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 3 (1868): 578-584.

123 See K.P. Oakley, "The Problem of Man's Antiquity: An Historical Survey," *Bulletin of the British Museum (Natural History)*, Geology, 9 (1964): 85-155.

However, some scientists were unconvinced by these accommodations and ventured into another conception of the peopling of Europe. For Thurnam, much more data was required before the origins of the different peoples of Europe could be properly settled.[124] He pointed out several elements that demonstrated the complexity of the task. These were of two orders:

1) Populations such as the Basques which were previously thought to be brachycephalic had now been demonstrated to be dolichocephalic. The reverse was true for populations such as the Germans.

2) The linkage between morphological type and linguistic family was broken as populations of both head shapes were speaking Aryan or Indo-European languages and non-Aryan or allophylian tongues.

On this view, Thurnam would not discard the possibility of a migration into Europe of dolichocranial peoples of Indo-European origins from Asia, or one of brachycranial peoples of Mongoloid origins, although he discarded the Aryan origin and Mongoloid models as explaining satisfactorily the peopling of Europe. Having demonstrated that in Britain a pre-Celtic, dolichocranial race was more ancient than the Celtic, brachycranial one (and not the reverse), Thurnam tried to explain the presence of these two types in Britain

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124 J. Thurnam, "On the Two Principal Forms of Ancient British and Gaulish Skulls," *Memoirs Read Before the Anthropological Society of London*, 1 (1863-1864): 120-168.

by deriving the older type from north Africa. It is worth quoting him at some length:

> "A few observations may be allowed on the possible or probable affinities of the two British types... I had the advantage of examining the series of sixty Basque skulls... I was at once struck with their great resemblance to the dolichocephalic skulls from the long barrows of this country... That they are [the Basques] the lineal descendants of the ancient Iberians, is now generally admitted by scholars. That the Iberians had their origin in northern Africa... is an opinion not without able advocates... Altogether, the doctrine of an Iberian, or Ibero-Phoenician origin of a very early, perhaps the earliest, population of at least part of Britain, though not as yet proved, derives much additional weight from the comparison here instituted of the skulls of the British dolichocephali of the stone period with those of the Basques. That the brachycephalic skull - form of the bronze-period in Britain was introduced into this island from Gaul, and was the type of the Celtic skull, at least that of the dominant race, appears to me to be proved. What may have been the origin of this 'Turanian' type of skull, and how it became that of a Celtic-speaking and so-called Indo-European people; or, conversely, how the Celtic became the language of a people with such a skull-form; are important questions for the investigation of the anthropologists of Europe.[125]

Thurnam's view was limited to the historical or immediately prehistoric times of Europe. Of even greater interest was Thurnam's reaction, a year later, to discoveries of human fossils in Europe at Engis, Furfooz, Lombrive and Neandertal.[126] Now, facing this new evidence, he wondered if his conclusions regarding the presence of two morphological types in Britain could not be reinterpreted by deriving them

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[125] Ibid., pp. 160-166.

[126] J. Thurnam, "On the two principal forms of Ancient British and Gaulish Skulls. Part II," *Memoirs Read Before the Anthropological Society of London*, 1 (1863-1864): 459-519.

both directly from these truly ancient human remains found in Europe:

> "Is it not probable that the long and short skulls found in the two classes of the most ancient tombs of England... are the direct and but slightly modified descendants of those truly primeval long-heads and short-heads whose remains... are found in the bone-caves of western Europe, in England, Belgium, Germany, France, and the Spanish peninsula?"[127]

In the first half of the 1860s, the evolutionist Vogt attempted to synthesize what was then known about the prehistoric races of Europe.[128] Although he did not discard completely the possibility of some ethnic elements having migrated into Europe from Asia, his conception rested on the recognition of a complex cohabitation of several morphological types that developed and persisted in Europe. In other words, his conception of the peopling of Europe was not rooted within the Aryan origin or Mongoloid models.

Vogt recognized at least four ancient prehistoric races in Europe: 1) a dolichocranial and prognathous Rheno-Belgian race of the cave-bear period represented by the Engis and Neandertal skulls; 2) a somewhat dolichocranial race with a high forehead of the reindeer period (following the cave-bear period) represented by skulls of the Lombrive cave; 3) a

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[127] Ibid., p. 519.

[128] C. Vogt, *Lectures on Man: His Place in Creation, and in the History of the Earth* (London: Longman, Green, Longman, and Roberts, 1864), pp. 370-401.

somewhat brachycranial race of the later Stone Age of Denmark represented by skulls from Borreby; and 4) a somewhat mesaticranial race represented by numerous skulls from the Stone and Metal Ages of Switzerland.

When considering the morphological differences that separated the Lombrive remains from those of Engis and Neandertal, Vogt judged it inadmissible to establish a relationship of descent or any type of affinity between them. He also held that the four prehistoric races identified above all had living representatives: the Rheno-Belgian skulls had their cognates in the long, narrow heads of Dutchmen, the skulls of Lombrive were allied to those of Basques, the ancient skulls from Denmark were linked to those of Lapps and Finns, while the ancient skulls from Switzerland persisted locally until today. Relying on the above facts, Vogt concluded that:

> "We thus find in the oldest prehistoric times everywhere very diversified races... but nowhere do we find proof of migrations or radiations from a common centre over the habitable globe. Though the short-heads [brachycranial] might be derived from Asia, it would not apply to the narrow heads, which claim the highest antiquity, as no such heads are met with in Asia. Thus, the facts we adduce from the earliest periods, merely represent man as an original product of the soil he then inhabited and still inhabits. In every such old race there is presented a remarkable constancy of form, the fundamental type of which is not obliterated, though various intermixtures have taken place with later immigrants."[129]

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[129] Ibid., pp. 391-392.

When considering later the persistence of the Neandertal type, Vogt explained that, although this ancient race disappeared through gradual extinction or by transmutation, it surely intermixed with other ancient races so that remnants of the former reappeared today partly through atavism.[130]

Although Broca thought that brachycrany antedated dolichocrany until 1862, he was soon to become a vocal opponent of the Aryan origin and Mongoloid models. The evolutionist Broca held after that time that Europe had been inhabited for a long time by a host of non-Asian peoples comprising brachycranial races as well as dolichocranial ones. Broca further contended that when the Celts or Aryans arrived from Asia at a later period, they found in Europe a "human fauna" (*faune humaine*) not substantially different from the one we find today. If the Aryans were of dolichocranial conformation when they first arrived in waves in Europe, they slowly mixed with the indigenous populations to the point where their morphological type disappeared without having changed significantly the local types. Although the Aryans themselves changed morphologically, they nevertheless managed, step by step, to impose their cultures, that is, their Indo-European languages and material cultures such as the working of metals. The Aryan invasion led to the

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[130] C. Vogt, "The Primitive Period of the Human Species," *Anthropological Review*, 5 (1867), p. 213.

transmission of cultures and ideas rather than the substitution of a morphological type by another.[131]

To defend his assertion that Europe had been inhabited for a long time by both brachycranial and dolichocranial races, Broca in 1867 considered the remains from Furfooz and Solutré as representing the former type, and those from Bruniquel, Eguisheim, Engis, Lahr and Neandertal, the latter. In 1868, Broca added to his list the remains from Cro-Magnon, a dolichocranial race he evaluated to be different from all other known races, ancient and modern.[132]

Let us pause to reflect on one aspect of the thesis presented here. We have already argued that the monogeny-polygeny debate or the monophyly-polyphyly one would not constitute the framework within which ancient human remains were mainly interpreted during the 1860-1895 period. We have said that a more restricted framework was needed and that considerations over the peopling of Europe constituted just such a framework. The discussion presented above, it is suggested, favours this interpretation.

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131 P. Broca, "Discussion sur les origines indo-européennes," *Bulletins de la Société d'Anthropologie de Paris*, 5 (1864): 193-196; "Sur les origines européennes," *Bulletins de la Société d'Anthropologie de Paris*, 5 (1864): 303-316.

132 P. Broca, "Discours de M. Broca sur l'ensemble de la question," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 2e session, 1867: 367-402; "Sur les crânes et ossements des Eyzies," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 3 (1868): 350-392.

Yet, is it not possible that some scientists might have been influenced in their conception of the peopling of Europe by their respective position in the monogeny-polygeny debate or in the monophyly-polyphyly one? Conversely, were models of the peopling of Europe used to favour a particular side of the monogeny-polygeny (monophyly-polyphyly) debate? Here, the nature of the link between the monogeny-polygeny (monophyly-polyphyly) debate and models of the peopling of Europe is raised.

It is true that Broca the polygenist and Vogt the polyphyletist held that present knowledge of ancient human remains did not favour the view that human races stemmed from a single stock.[133] Whether or not they judged this to constitute strong evidence in favour of multiple origins of the human races is an entirely different matter. Considering Broca's positivist inclination, it seems doubtful that he would have thought so, no matter how strongly he felt about this.

Even if some scientists defended a particular conception of the peopling of Europe because of their position in the monogeny-polygeny (monophyly-polyphyly) debate, a distinction should be made between their motive and their capacity to demonstrate it on a scientific basis. This last point is

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133 P. Broca, "L'anthropologie en 1868," *Encyclopédie générale, Almanach de 1869* (Paris, 1870); reproduced in P. Broca, *Mémoires d'Anthropologie* (Paris: Jean-Michel Place, 1989), p. 518; C. Vogt, *Lectures on Man: His Place in Creation, and in the History of the Earth* (London: Longman, Green, Longman, and Roberts, 1864), pp. 391-392.

clearly seen when scientists such as Broca and de Quatrefages were eventually to share a similar view of the peopling of Europe after 1870 (see below), regardless of the fact that they clung to opposite positions in the monogeny-polygeny debate. If the human fossil record suggested that the oldest human races yet found were as distinct from one another as living ones, a view widely supported at the time, this constituted in itself anything but a crucial argument in favour of polygeny or polyphyly. After all, it could be argued that the proof of the monogenist hypothesis lay still deeper in time and/or would eventually be found in other regions of the world.

Pruner-Bey clearly understood that. As someone who promoted the Mongoloid theory, Pruner-Bey explained, after Broca accused him of being a monogenist, that his conception of the peopling of Europe could not be taken as supporting the monogenist hypothesis because the search for ancient human remains, which had barely started in Europe, needed also to be expanded to other regions of the world. Pruner-Bey considered that the discovery of ancient races all over the world would either show signs of convergence as we go deeper into time, in which case the monogenist hypothesis would be greatly supported, or would show no signs of convergence. He admitted that the human remains discovered thus far in Europe showed that very little morphological change had occurred during ancient times. Pruner-Bey

concluded by saying that much remained to be done by the doctrinaires to demonstrate either view.[134] He perfectly understood that, empirically speaking, there was only a very weak connection between the ancient human remains yet found and the monogeny-polygeny (monophyly-polyphyly) debate.

If the 1860s were characterized by the competition of at least two broad models of the peopling of Europe, the 1875-1895 period was one of relative unanimity. Most scientists, including de Quatrefages, now discarded the Aryan and Mongoloid origin models in all their rigidity. These models implied strong ties with Asia; ties which could not be strongly supported in 1870 when the wealth and antiquity of the empirical material found in Europe relative to other parts of the world were considered. These alleged ties between Europe and the rest of the world remained to be demonstrated.

The 1875-1895 period was very much one of evaluation of the conclusions reached in a piece of work of unprecedented breadth. In a series of studies made in the first half of the 1870s, de Quatrefages and Hamy attempted a synthesis of all ancient human remains known at the time.[135] Published

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[134] F. Pruney-Bey, "Sur le maxillaire humain fossile trouvé par M. Dupont et présenté par M. Pruner-Bey," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 1 (1866): 616-620.

[135] A. de Quatrefages et E.-T. Hamy, "Races humaines fossiles - Race de Canstadt," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 8 (1873): 518-523; "La race de Cro-Magnon dans l'espace et dans le temps," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 9 (1874): 260-266; "Races humaines fossiles mésaticéphale et

collectively in the first part of the *Crania Ethnica* (1882), these studies recognized at least three morphological types in Europe during the Quaternary period.[136]

The most ancient type largely distributed in the substratum of Europe, which they named the Canstadt race, was composed of skulls and jaws from Canstadt, Eguisheim, Brüx, Neandertal, Gibraltar, Denise, Staengenaes, Olmo, La Naulette, Arcy, Clichy and Goyet. They described the Canstadt race as a dolichoplatycephalic and prognathous one with considerable variability, which was characterized also by a more or less developed supraorbital ridge and a low receding forehead. The law of atavism was judged responsible for occasional reappearances of such a physical type among living populations throughout a large part of the Old World. However, noting a close morphological resemblance between the Canstadt race and some living populations of Australia and India, de Quatrefages and Hamy alluded to a possible affiliation between them.

The second prehistoric type they identified was the Cro-Magnon race which was thought to have lived essentially in contemporaneity with the Canstadt race. The Cro-Magnon race was said to be restricted to western Europe. This race, represented by crania and jaws from Cro-Magnon, Laugerie-Basse,

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brachycéphales," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 9 (1874): 819-826; "Les Crania Ethnica," *Bulletins de la Société d'Anthropologie de Paris*, 2e série, 10 (1875): 612-619.

136 A. de Quatrefages et E. T. Hamy, *Crania Ethnica: Les crânes des races humaines* (Paris: J. B. Baillière, 1882), pp. 1-146.

Bruniquel, Menton, Isola del Liri, Grenelle, Solutré, Engis, La Madeleine, Engihoul and Smermaas. Also a dolichocephalic but not platychephalic race, the morphology of the forehead and facial regions was described as completely different from that of the Canstadt race. The Cro-Magnon type was said to be found erratically among living Europeans. However, this type, it was contended, was found in more pronounced form and more frequently in the populations of north-western Africa; the law of atavism explaining again such occurrences.

Their third prehistoric type comprised crania and jaws from Furfooz, La Truchère, Grenelle, Moulin-Quignon and Nagy-Sap. This type was in reality a group of brachycephalic and mesaticephalic remains that comprised possibly four races. Thought to have been contemporaneous at some times with the Canstadt and Cro-Magnon races, these former races persisted into more recent times. Traces of them were said to be found among recent and living populations. De Quatrefages and Hamy affiliated the remains from Grenelle and La Truchère, but not those of Furfooz, to a Mongoloid stem.

According to de Quatrefages, all these morphological types and races persisted distinctly throughout the Quaternary period, regardless of admixture. However, they were "diluted" when successive waves of migrations of dolichocranial and brachycranial races occurred in Europe since the Neolithic and up to the historic period.[137]

Broca was to agree in essence with the characterization of at least three fossil races in prehistoric Europe (Canstadt, Cro-Magnon and Furfooz) after de Quatrefages and Hamy.[138]

In 1873, Broca himself contributed to the establishment of affiliations between the tall Cro-Magnon type of the second half of the Quaternary period and the medium-size race of the Neolithic period represented in the cave of l'Homme-Mort.[139] He considered that this lineage might embrace living representatives including the Basques and the Berbers, thus demonstrating morphological changes in this lineage owing to external conditions and admixtures. By so doing, Broca hypothesized the possible migration of the Cro-Magnon type into Europe from northern Africa.

De Quatrefages and Hamy recognized in the Furfooz type possibly four races of brachycephalic and mesaticephalic conformations. For Broca, the Furfooz type comprised only a single brachycephalic stem that lived first in eastern Europe late in the Quaternary period only to migrate later into western Europe. Broca explained the presence of a mesaticephalic race in Belgium by an encounter and admixture

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137 A. de Quatrefages, *Hommes fossiles et hommes sauvages* (Paris, Jean-Michel Place, 1988), p. 77. This book was originally published in 1883.

138 P. Broca, "Les races fossiles de l'Europe occidentale," *Association Française pour l'avancement des sciences*, Congrès du Havre, 1877: 10-25.

139 P. Broca, "Les crânes de la caverne de l'Homme-Mort (Lozère)," *Revue d'Anthropologie*, 2 (1873): 1-53.

between dolichocephalic Cro-Magnon races and the new brachycephalic comers in western Europe.[140]

In 1886, two skeletons closely resembling the Neandertal remains found in Germany in 1856 were discovered at Spy in Belgium.[141] By comparing the differences between the skulls of Spy 1 and 2, the evolutionists Fraipont and Lohest suggested a redefinition of the Canstadt race as previously defined by de Quatrefages and Hamy in the *Crania Ethnica* (1882). Fraipont and Lohest held that if the two Spy skulls were those of a male and a female respectively, then the Canstadt race was more likely to be represented by the skulls from Brüx, Denise, Eguisheim, Canstadt and Spy, whereas the skulls from Castenedolo, Clichy, Olmo and Staengenaes (females of the Canstadt race according to de Quatrefages and Hamy) would have to be attributed to another race.

Fraipont and Lohest directly addressed the question of the relationship between the Canstadt, Cro-Magnon and Furfooz races. They wondered if these races could somehow all be related. They asked: "pouvons-nous rattacher directement à la race de Néanderthal ou de Canstadt celle de Cro-Magnon et à celle-ci celle de Furfooz?".[142] They answered this question

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[140] P. Broca, "Les races fossiles de l'Europe occidentale," *Association Française pour l'avancement des sciences*, Congrès du Havre, 1877, pp. 24-25.

[141] J. Fraipont et M. Lohest, "La race de Néanderthal ou de Canstadt en Belgique," *Archives de Biologie*, 7 (1887): 587-757.

[142] Ibid., p. 726.

affirmatively by suggesting a scheme linking the ancient Neandertal race to the Cro-Magnon race, and the Cro-Magnon race to the more recent Furfooz race. In support, they tried to bridge the morphological gap separating the Cro-Magnon type from the Neandertal or Canstadt type by establishing a morphological gradation from the females of the Cro-Magnon race, the remains from Hamoir and the females of the Canstadt race (as defined by de Quatrefages and Hamy). The affiliation between the dolichocephalic Cro-Magnon type and the brachycephalic Furfooz type was easily demonstrated by the discovery of intermediate forms. This phylogenetic scheme was a complex reticulate one:

> "Ce type [Neandertal type] ou un autre type antérieur à celui-ci a donné naissance, à un moment donné en Asie, probablement à ces hommes dont les descendants ont réalisé le type ethnique de Cro-Magnon qui à gagné l'Europe avec ses caractères distinctifs. Quant aux races de Furfooz.. il n'est pas moins acquis qu'en certains points... les deux types [Cro-Magnon and Furfooz] ont été mélangés. Est-ce là le point de départ de l'une des races de Furfooz? Ou bien le résultat du métissage entre la race pure de Cro-Magnon et une autre race non moins pure envahissante et d'une autre région?".[143]

According to Fraipont and Lohest, an appreciable number of individuals living today in Belgium were directly descended from the Furfooz races, whereas the Cro-Magnon and Canstadt characters occasionally found among living Europeans

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[143] Ibid., pp. 728-729.

were not transmitted by direct inheritance but cropped out through atavism.

Although Fraipont and Lohest recognized that the Spy remains were characterized by an unmatched number of pithecoid traits, they contended, as Huxley had previously done in 1863 about the Neandertal skull, that an abyss still separated those fossils from any living anthropoid. However, having noted the morphological transformation that human races underwent in Europe throughout the Quaternary period, Fraipont and Lohest supposed that it should be possible to pursue the ancestral type of humankind and that of the anthropoids much further into the past, possibly as far back as the Eocene period or even earlier. Later, Fraipont made it clear that he considered humans and apes not to be directly related out, to be descended from a common stem.[144]

In 1890, the evolutionist Huxley brought some precision to his view on human phylogeny. Already in 1863 he had stated that the Neandertal skull found in Germany in 1856 was the most pithecoid yet discovered, although he had asserted that in no sense could this fossil be regarded as intermediate between humans and apes.[145] Now, in the light of the Spy remains, he restated his view and asserted that the

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[144] J. Fraipont, "Discussion," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 10e session, 1889: 358-359.

[145] T.H. Huxley, *Evidence as to Man's Place in Nature* (London: Williams & Norgates, 1863), reprinted in *Thomas Henry Huxley. Collected Essays (1893-1894)*, Vol. VII (Hildesheim: Georg Olms Verlag, 1970), pp. 157-208.

genus *Homo*, and possibly the species *H. sapiens*, had existed as early as the Pliocene or even the Miocene.[146]

After noting that skulls occasionally approach the neandertaloid type among living dolichocephalic races, Huxley tried to explain such continuity. It is worth quoting him at some length:

> "It may be taken to be a pretty sure indication of the physiological continuity of the blond long-heads [dolichocephalic] with the pleistocene Neanderthaloid men. But this continuity may have been brought about in two ways. The blond long-heads may exhibit one of the lines of evolution of the men of the Neanderthaloid type. Or, the Frisians [a living dolichocephalic race] may be the result of the admixture of the blond long-heads with Neanderthaloid men... The same alternatives present themselves when Neanderthaloid characters appear in skulls of other races. If these characters belong to a stage in the development of the human species, antecedent to the differentiation of any of the existing races, we may expect to find them in the lowest of these races, all over the world, and in the early stages of all races. I have already referred to the remarkable similarity of the skulls of certain tribes of native Australians to the Neanderthal skull... Neanderthaloid features are to be met with, not only in ancient long skulls; those of the ancient broad-headed people [brachycephalic] entombed at Borreby in Denmark have been often noted."[147]

In the early part of the 1890s, the evolutionist Hervé[148] established more precisely than previously the affiliation of

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[146] T.H. Huxley, "The Aryan Question and Prehistoric Man," *Nineteenth Century* (November, 1890), reprinted in *Thomas Henry Huxley. Collected Essays (1893-1894)*, Vol. VII (Georg Olms Verlag: Hildesheim, 1970), pp. 271-328.

[147] Ibid., pp. 327-328.

[148] See A. Hovelacque and G. Hervé, *Précis d'anthropologie* (Paris: Adrien Delahaye et Émile Lecrosnier, 1887).

the Cro-Magnon and Furfooz types. When considering the Cro-Magnon lineage, he traced its transformation through time by comparing its oldest representatives from Chancelade and Laugerie-Basse (end of the Quaternary period) with its more or less transformed and recent remains from Cro-Magnon and Baumes-Chaudes (the Neolithic period).

Hervé held that it was impossible in the present state of our knowledge to derive the Cro-Magnon lineage from the neandertaloid one, considering the morphological gap that separated them, and although they were contemporaneous at some point. Turning to the relationship between the Magdalenians (Cro-Magnon people of the Quaternary period) and some living humans, Hervé claimed to be able to establish an intimate link between the former and the Eskimos of Greenland and Labrador.[149]

In contradistinction with de Quatrefages and Hamy but not unlike Broca, Hervé considered that the so-called Furfooz races of brachycranial and mesaticranial conformations, now considered to be only of the Neolithic and not of the late Quaternary[150], were all originally part of a truly eastern brachychephalic stem that intermixed in France and Belgium

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[149] G. Hervé, "La race des troglodytes magdaléniens," *Revue de l'École d'Anthropologie de Paris*, 3 (1893): 173-188; "Distribution en France de la race néolithique de Baumes-Chaudes-Cromagnon," *Revue de l'École d'Anthropologie de Paris*, 4 (1894): 105-122.

[150] See E.-T. Hamy, "Nouveaux matériaux pour servir à l'étude de la paléontologie humaine," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 10e session, 1889: 445-450.

with the older dolichocephalic type (Cro-Magnon), thus explaining the presence of several cranial conformations during the Neolithic. For Hervé, the Iberians and the Ligures were among living representatives of both dolichocephalic (Cro-Magnon) and brachycephalic (Furfooz) races of the Neolithic.[151]

Restricted Phylogenies: An Overview

Two points should be stressed regarding the phylogenies of the peopling of Europe suggested during the 1860-1895 period.

1) This essay has tried to show that anti-evolutionary tracts did not have any important impact on the development of the field of human evolution in the second half of the nineteenth century. If the human fossil record was dominated by phylogenies of the peopling of Europe (restricted phylogenies), it is certainly not because the field was dominated by anti-evolutionists.

To explain why the human fossil record was interpreted within the framework of restricted phylogenies, a number of recent observers have suggested that it was because the ancient, human remains were interpreted in a sort of race-succession paradigm, in which for the most part the prehistoric morphological types of Europe were not

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[151] G. Hervé, "Les brachycéphales néolithiques," *Revue de l'École d'Anthropologie de Paris*, 4 (1894): 393-406; 5 (1895): 18-28.

transmuting into one another but rather were merely replacing one another.[152] This race-succession paradigm implies that the human fossil record was not being interpreted in evolutionary terms.

We have seen above how avowed evolutionists such as Schaaffhausen, Vogt, Broca, Fraipont and Lohest, Huxley and Hervé all interpreted the ancient human remains by inserting them in complex phylogenetic hypotheses about the peopling of Europe involving both the replacement and transformation of races in the time dimension. Therefore, it is argued here that the race-succession paradigm, assuming that such a concept is judged of some utility, was by no means the main framework within which most scientists interpreted the human fossil record at the time.

Consequently, it is asserted here that there were no clear differences between the phylogenies expounded through the monophyly-polyphyly debate and those upheld in models of the peopling of Europe, except of scale. If the transmutation theory through the monophyly-polyphyly debate

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152 See A. Leguebe, "Importance des découvertes de Néandertaliens en Belgique pour le développement de la paléontologie humaine," *Bulletin de la Société Royale Belge d'Anthropologie et de Préhistoire*, 97 (1986), p. 19; F.H. Smith, "Neandertals," in F. Spencer (ed.), *History of Physical Anthropology: An Encyclopedia*, vol.2 (New York: Garland, 1997), p. 712; F. Spencer, "The Neandertals and Their Evolutionary Significance: A Brief Historical Survey," in F.H. Smith and F. Spencer (eds.), *The Origins of Modern Humans: A World Survey of the Fossil Evidence* (New York: Alan R. Liss, 1984), pp. 10-11; I. Tattersall, *The Fossil Trail: How We Know What We Think We Know about Human Evolution* (Oxford: Oxford University Press, 1995), p. 15; E. Trinkaus and P. Shipman, *The Neandertals: Changing the Image of Mankind* (New York: Alfred A. Knopf, 1993), pp. 109-110, 119.

permitted one to suggest human phylogenies, another source through models of the peopling of Europe also permitted one to do so. In other words, if evolutionary biology was responsible for the establishment of some phylogenies in human evolution, which are referred to here as "broad phylogenies", more "restricted phylogenies" of the peopling of Europe were not generated by evolutionary biology.

From our contemporary viewpoint, it would seem that, whereas phylogenies of the peopling of Europe were concerned with changes of human races (intraspecific changes), the transformation of one species into another (interspecifc changes) was left out, only to be explained by evolutionary biology. However, things were not so simple. For many scientists of the time, among whom Darwin and Huxley figured, the question whether all living humans were part of the same species was of minor importance. What really mattered to them was the recognition that morphological types changed through time. As we have seen, this is exactly what a number of scientists recognized when they assessed the peopling of Europe. That is not to say that no scientist posed a clear distinction between intra- and interspecific changes, for de Quatrefages did just that. But it must be recognized that the species question, to use a modern expression, was not neccessarily a key issue at the time.

2) The second point which we should like to stress here is Virchow's attitude towards the hominid fossil record.

We have seen that numerous scientists between 1860 and 1895 suggested more or less different views of the prehistoric peopling of Europe. In this, they were confident enough in their ability to identify ancient races. However, other scientists such as Virchow were less confident about it.

It is important to understand Virchow's view for two interrelated reasons. First, it throws considerable light on the difficulties and limitations of the establishing of human phylogeny through its fossil record. These difficulties were often silenced or neglected by scientists more optimistic about rapid success in this enterprise. Secondly, Virchow has been depicted by some modern observers as an anti-evolutionist trying to obstruct the rise of a field concerned with human evolution, whereas in reality he was only very cautious about it. Virchow's attitude was misconstrued as a confirmation that scientists not actively engaged in establishing human phylogeny were simply anti-evolutionists.[153]

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153 The discussion presented here about Virchow is based on the following literature: R. Virchow, "Discussion," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 6e session, 1872: 560-564; "Les peuples primitifs de l'Europe," *La Revue Scientifique*, 2e série, 1 (1874): 1-13; "Discussion," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 7e session, 1874: 318-321; "La liberté de la science dans l'État moderne," *La Revue Scientifique*, 2e série, 23 (1877): 534-543; "Darwin et l'anthropologie," *La Revue Scientifique*, 3e série, 14 (1882): 417-421; "Anthropology in the Last Twenty Years," *Annual Report of the Smithsonian Institution*, 1889: 555-570; "Les changements dans les problèmes du Congrès international d'archéologie et d'anthropologie préhistoriques," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 11e session, 1892: 223-228 and "Heredity and the Formation of Races," [1896] in *This is Race. An Anthology Selected from the International Literature on the Races of Men*, ed. E. Count (New

Far from being an anti-evolutionist Virchow, as already discussed, argued that transmutation from one type into another might have occurred if pathological variations or deviations were passed on by the inheritance of acquired characteristics. Virchow's sympathy towards evolution was theoretical; what was required, he insisted, was evidence, either through experimentation or direct observation, that transmutation occurred. On that count, Virchow's approach to science was not unlike that of de Quatrefages and Huxley. Virchow's cautious attitude concerning human evolution rested on this epistemological premise that science should rely on direct evidence and on his conception of what constituted direct evidence.

Therefore, when considering the evidence in favour of humans' animal ancestry, he could only note that no intermediate link had yet been found, and that all ancient human remains known were considered to be of modern appearance. On that basis, Virchow asserted that human ascent from any non-human form could not be considered as an established fact, although it bore a certain probability.

Even more serious than the lack of any intermediate form, in Virchow's view, was that the transformation of one human race into another had not yet been established or documented; skin complexion and skull conformation, for

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York: Henry Schuman, 1950), 178-193. See also E. H. Ackerknecht, *Rudolf Virchow. Doctor, Statesman, Anthropologist* (Madison: University of Wisconsin Press, 1953), pp. 199-223.

instance, seemed to be fairly stable traits. It is not that Virchow necessarily held that human races arose separately, for he often considered the possibility of deriving them from a single stock, but he simply insisted that the evidence at his disposal tended to establish the persistence of the races in time.

This explained Virchow's assertion that the first step to establish a transformist doctrine in anthropology should be to understand how human races acquired and transmitted their specific traits. Also explained is his cautious attitude regarding the recognition of Europe's prehistoric races. For Virchow, the prehistoric remains thus far discovered in Europe were only the result of a complex admixture between several distinct morphological types. The recognition of types such as Canstadt, Cro-Magnon and Furfooz was, in his mind, illusory and premature, although he did not completely give up on recognizing some possible links between ancient remains and some living races.

The Field of Human Evolution, 1895-1930

In dividing this paper between the 1860-1895 and 1895-1930 periods, it was not intended to assert that these were distinguished from one another other than in the dating. On the contrary, a continuity of approaches characterized the

two time periods. No sharp break punctuated the turn of the century. What took place around 1900 was a normal progression, in the light of more fossil discoveries, of the research activities already instituted during the 1860-1895 period. This division was introduced in this essay merely for practical purposes.

It has often been asserted by modern observers that a truly evolutionary approach to human evolution had arisen by the turn of the century. The debate after 1895 over whether or not *Pithecanthropus* represented an intermediate between humankind and a non-human primate is often considered to epitomize this state of affairs. We will come back to this episode shortly.

Furthermore, modern observers have often asserted that debates over human phylogeny during the first third of the twentieth century polarized around two alternatives: a unilinear hypothesis part of which was later referred to as the "Neanderthal Phase Hypothesis" or a multilinear hypothesis part of which was later referred to as the "Presapiens Hypothesis".[154] Within this polarization, the

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154 See P.J. Bowler, "Paleoanthropology Theory," in F. Spencer (ed.), *History of Physical Anthropology: An Encyclopedia*, vol. 2 (New York: Garland, 1997), 785-790; *Theories of Human Evolution: A Century of Debate, 1844-1944* (Baltimore: Johns Hopkins University Press, 1986); M. Hammond, "The Shadow Man Paradigm in Paleoanthropology, 1911-1945," in G.W. Stocking (ed.), *Bones, Bodies, Behavior: Essays on Biological Anthropology* (Madison: University of Wisconsin Press, 1988), 117-137; F.H. Smith, "Neandertals," in F. Spencer (ed.), *History of Physical Anthropology: An Encyclopedia*, vol.2 (New York: Garland, 1997), 711-722; F. Spencer, "The Neandertals and Their Evolutionary Significance: A Brief Historical Survey," in F.H. Smith and F. Spencer (eds.), *The*

place of the Neandertals is judged pivotal for the favouring of one alternative or the other.

Few comments need to be made here regarding these views. First, from what has been said above, it is clear that an evolutionary approach to human evolution did not arise at the very end of the nineteenth century, but much earlier. The seemingly anti-evolutionary interpretations of the human fossil record were, for the most part, only illusory.

Secondly, the focus during the 1900-1930 period on the unilinear-multilinear dichotomy just mentioned, while of some value, obscures the continuity of approaches before and after 1900. Thirdly, while the Neandertals held a pivotal role in the "Neanderthal Phase Hypothesis" and the "Presapiens Hypothesis", these remains were not always judged crucial in the assessment of human phylogeny.

A number of scientists at the time used an interpretative framework in which the Neandertals figured directly in the human phylogeny but only as contributors among others. On this view, the Neandertals constituted only a small part of the problem of human phylogeny. This meant that the type of phylogenetic interpretations held by most scientists prior to 1895 continued to be held in the twentieth century; these constituted genuine alternatives to both the "Neanderthal Phase Hypothesis" and the "Presapiens Hypothesis". Consequently, the unilinear-multilinear

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Origins of Modern Humans: A World Survey of the Fossil Evidence (New York: Alan R. Liss, 1984), 1-49.

dichotomy is too incomplete a tool to serve as an analytical framework.

It is suggested here that this dichotomy should be restated along the following lines: were human fossils interpreted by comparing them to living human races or, on the contrary, were they compared to living and fossil species of primates considered to be closely akin to humankind? The widely recognized empirical gap that separated broad phylogenies from restricted ones prior to 1895, was, after 1900, recognized by scientists as either having been filled, or not having been filled.

On the one hand, if human fossils were compared to living human races, all or most human fossils could comfortably be accommodated in a scheme within which they were phylogenetically related to one or several living human races. Proponents of this approach, in the tradition of scientists who established restricted phylogenies before 1895, did not necessarily hold that human fossils fitted well within the modern human range of variation. True, some of them held such a view. More importantly, however, most proponents of this approach held that morphological characters of hominid fossils made them suitable ancestors of one or several living human races.

On the other hand, human fossils were compared to living and fossil non-human primates. The stages of human evolution were established by identifying a morphological series

evolving from a simian to a human state. On this view, there could be only one true evolutionary line (or lines) linking living humans to a non-human primate ancestor. Human fossils judged not to be representatives of a stage in humankind's evolution leading to living humans were relegated to extinct branches. This explains why this approach was often associated with a multilinear scheme of human evolution.

The recognition of this new dichotomy implies that, at the heart of divergent human phylogenetic interpretations between 1895 and 1930, lay still the epistemological problem identified prior to 1895. We have seen how efforts to reconstruct human phylogeny were simultaneously directed at the establishment of broad and restricted phylogenies during the 1860-1895 period. The former established a general framework from which the affinity of humankind and its closest living primate relatives was evaluated in the light of comparative anatomy and embryology. The latter integrated the human fossils in a framework in which prehistoric races were related to living human races.

We have argued that this state of affairs was determined mainly not by the absence of the fossil material, nor by anti-evolution tracts, but by the very nature of the material itself, that is, by its relatively modern-looking appearance as those early finds were then judged to be and by its geographical restriction largely to Europe. The fossil material lent itself to be compared most fruitfully to living

human races, especially Europeans. Conversely, the then available fossil material was of limited use in establishing the broader evolutionary framework prevailing between humankind and the other living primates. Although this epistemological problem persisted into the twentieth century, it presented itself under a slightly modified guise.

By the end of the nineteenth century, the fossil material judged to be relevant to human evolution was being gradually incorporated into broad phylogenies. That is to say, in addition to comparative anatomy and embryology of living forms, a number of scientists now relied on some fossils to determine human phylogeny.

In fact, even prior to 1895 de Mortillet and Cope had moved, more or less completely, towards this approach.

De Mortillet's *Le Préhistorique* (1883) was a synthesis pulling together elements from geology, palaeontology and prehistoric archaeology.[155] His goal was to document human biological and cultural evolution. For lack of sufficient fossil material to fill up this agenda, de Mortillet relied heavily on prehistoric archaeology; for him, cultural evolution was a mirror of biological evolution.

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155 G. de Mortillet, *Le Préhistorique: antiquité de l'homme* (Paris: Reinwald, 1883). See also G. de Mortillet, "Le précurseur de l'homme," *Compte Rendu de l'Association Française pour l'Avancement des Sciences*, 2e session, Lyon, 1873: 607-613; "Revue préhistorique," *Revue d'Anthropologie*, 2e série, 8 (1879): 114-118. Consult also J.-Y. Pautrat, "*Le Préhistorique* de G. de Mortillet: une histoire géologique de l'homme," *Bulletin de la Société Préhistorique Française*, 90 (1993): 50-59.

De Mortillet subscribed to a view of transmutation that was derived from geology and palaeontology: the succession of strata showed that the constitution of faunas and floras inevitably changed with time; such changes were commonly not radical between two successive strata but rather partial and gradual, establishing phylogenetic links between them. De Mortillet referred to this as the "palaeontological law".

De Mortillet enshrined the fossil and artefactual material he judged relevant to the understanding of human evolution within this evolutionary framework.

Europe was, for de Mortillet, the seat of a gradual "co-evolution" of a simian form into modern humans as seen in its fossil remains, and especially of crude and primitive industries into complex ones.

De Mortillet turned to the oldest artefactual material, considered by him to be of the Tertiary period, and thought it justified to recognize the fabricators of these supposed artefacts as belonging to a different genus, for which he adopted the name *Anthropopithecus*. He recognized three hypothetical species of the genus *Anthropopithecus* on the basis of both the time dimension and distinctions among the artefacts themselves:

> "Ce genre évidemment devait contenir plusieurs espèces. En effet, l'anthropopithèque de Thenay [*A. bourgeoisii*], qui est aquitanien, ne peut appartenir à la même espèce que celui de Cantal [*A. ramesii*], qui est tortonien. Entre ces deux époques géologiques, la base et le sommet du miocène, il y a eu changement complet de faune. Nous

> pouvons aussi affirmer que l'anthropopithèque de Portugal [*A. ribeiroii*] ne se rapporte pas à ceux de France. En effet, son industrie a un caractère tout particulier. Nous devons donc admettre trois espèces d'anthropopithèques... La seule donnée comme description anatomique que nous puissions avoir sur ces anthropopithèques, c'est qu'ils étaient sensiblement plus petits que l'homme. Ce caractère existait surtout dans l'Anthropopithecus Bourgeoisii. En effet, les silex taillés de Thenay sont remarquables par leurs petites dimensions... Les pierres taillées de Portugal, trop petites aussi pour l'homme actuel, dénotent pourtant un anthropopithèque se rapprochant des proportions humaines."[156]

If as was supposed the Tertiary period was characterized by very crude stone and bone tools not very clearly distinguished from one another, the Quaternary period, continued de Mortillet, saw a succession in time of complex and very definite industries like the Chellean, Mousterian, Solutrean, Magdalenian and Robenhausian.

De Mortillet used this succession of industries in time as a chronological marker for the human fossil record. The fossil remains from Neandertal, Canstadt, Eguishem, Olmo and La Naulette, which were associated with the Chellean industry and possibly the Mousterian, were united in the Neandertal race. De Mortillet recognized a number of simian characters in the Neandertal race, thus demonstrating clearly that humans were related to other primates. Furthermore, de Mortillet recognized in the Neandertal "race" a species distinct from that of living humans.

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[156] G. de Mortillet, *Le Préhistorique: antiquité de l'homme* (Paris: Reinwald, 1883), pp. 105-106.

De Mortillet specified that a yet undiscovered human race was associated with the Solutrean industry, while the fossils from d'Arcy and Laugerie-Basse (Laugerie race) were associated with the Magdalenian industry, and the human fossils from Cro-Magnon (Cro-Magnon race) with the Robenhausian industry.

For Cope, living anthropoid apes and humans resembled one another more than either resembled the living monkeys. Also, he recognized that the tritubercular morphology of human molars approached that of the lemurs. On that basis, Cope hypothesized that the Anthropomorpha (humans and apes) were not descended from monkeys but directly from Eocene fossil lemurs, which he considered to be large-brained.[157] Cope was not holding that humans were directly descended from an ancient lemur form without having passed through an ape stage, for he held that it was "highly probable that Homo is descended from some form of the Anthropomorpha now extinct".[158]

Cope tried to determine human phylogeny by comparing the morphology of living human races to that of other Old World primates. He recognized that some living human races possessed a greater proportion of simian characters than others, hence supporting the transition of a simian type into

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[157] E.D. Cope, "The Genealogy of Man," *American Naturalist*, 27 (1893): 321-335.

[158] Ibid., p. 326.

a human one. Cope's contention was reinforced by the still greater number of simian traits in the fossil remains of the Neandertals, in which he included: the Neandertal skull, La Naulette jaw, Cannstadt skulls and Spy skeletons. Cope referred to these remains as belonging to a distinct species *Homo neanderthalensis*, the most primitive and simian human race that inhabited Europe yet discovered. Cope stated that some living humans might be directly descended from *Homo neanderthalensis*.

While de Mortillet and Cope felt advised to attempt a synthesis of data from comparative anatomy of living forms, prehistoric archaeology and the human and primate fossil records prior to 1895, most scientists resisted doing so. They judged the data to be too incomplete. However, the discovery of *Pithecanthropus* in the early years of the 1890s led a number of scientists to move towards such a synthesis.

The discovery of *Pithecanthropus* contributed, probably more than any other discovery, to the incorporation of fossil material into broad phylogenies. But contrary to what modern observers have often concluded, no new era dawned after this discovery.

Pithecanthropus: In the Midst of an Epistemological Problem

The episode following the discovery of *Pithecanthropus* in Java in 1891-1892 provides a good case with which to

illustrate the persistence of an epistemological problem in the reconstitution of human phylogeny. As this episode has been analysed at great length by Theunissen[159], the discussion here will be limited in scope.

Let us examine first the context in which *Pithecanthropus* was first interpreted. For Dubois, the discoverer of *Pithecanthropus*, this form was more a primate fossil resembling humans than a human fossil approaching the apes. On this view, Dubois interpreted *Pithecanthropus* by comparing it to the primate fossil record as then known.

Although primate fossils such as *Protopithecus brasiliensis*, *Mesopithecus*, *Sivapithecus* and *Pliopithecus* (*Pithecus*) *antiquus* were discovered in the first half of the nineteenth century[160], it seems that it is only after 1870 that statements were occasionally made to the effect that primate fossil discoveries could shed light on the evolution of humankind. Let us cite a number of such instances.

In 1878, Gaudry held that *Dryopithecus*, a fossil anthropoid ape of the European Miocene, presented some resemblances to humankind. He even suggested that *Dryopithecus* might have been responsible for the supposed

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159 B. Theunissen, *Eugène Dubois and the Ape-Man from Java: The History of the First 'Missing Link' and its Discoverer* (Dordrecht: Kluwer Academic Publishers, 1989).

160 See W.C. Hartwig, "*Protopithecus*: Rediscovering the First Fossil Primate," *History and Philosophy of the Life Sciences*, 17 (1995): 447-460.

stone tools in Tertiary deposits.[161] Following more fossil discoveries of *Dryopithecus*, however, Gaudry retracted his previous view and declared that *Dryopithecus* showed more differences from humans than he had expected.[162]

In 1879, Lydekker identified a fossil anthropoid ape of the Miocene of India which he called *Palaeopithecus sivalensis*.[163] He found that this fossil ape differed from all known living and fossil anthropoid apes, although it had some resemblances to the chimpanzee, orang, *Dryopithecus* and man. On morphology and geographical distribution, Lydekker conjectured that all these living and fossil forms might have had a common parentage and ancestral home in "Lemuria", a sunken continent in the Indian Ocean. This implied, argued Lydekker, that missing links binding humankind to anthropoid apes might be lost forever. Lydekker later referred *Palaeopithecus* to *Troglodytes*[164], and still later to *Anthropopithecus*.[165]

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161 A. Gaudry, *Les enchaînements du monde animal dans les temps géologiques. Mammifères tertiaires* (Paris: Hachette, 1878), pp. 235-241.

162 A. Gaudry, "Le Dryopithèque," *Mémoires de la Société Géologique de France*, 1 (1890): 5-11.

163 R. Lydekker, "Further Notices of Siwalik Mammalia," *Records of the Geological Survey of India*, 11 (1879): 33-57.

164 R. Lydekker, "Indian Tertiary and Post-Tertiary Vertebrata. Siwalik Mammalia - Supplement I", *Memoirs of the Geological Survey of India*, 10th series, 4 (1886): 1-21.

165 W.H. Flower and R. Lydekker, *An Introduction to the Study of Mammals Living and Extinct* (London: Adam and Charles Black, 1891), pp. 736-738.

Although the primate fossil record was still meagre in 1890, the diversity of specimens discovered by then was by no means insignificant. The collections of Tertiary and Quaternary fossils throughout the world contributed greatly to the knowledge of the families of *Primates*. In the early 1890s, attempts were made to survey the primate fossil record.[166]

In the first decade of the twentieth century, Ameghino held that South America had nurtured the first primate forms, from which stemmed *all* the others including humans before spreading throughout the world.[167] Ameghino argued that humans were immediately derived from South American fossil homunculid forms, which were themselves derived from an ancient line characterized by small, rounded skulls that gradually evolved towards increased encephalization. The ancestral prototype of this main lineage would be represented today by the modern genera of *Cebus* and *Saimiri* of the platyrrhine (New World) monkeys. While this main lineage evolved towards humanization, continued Ameghino, several stems diverged from it in the direction of a

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166 See W.H. Flower and R. Lydekker, *An Introduction to the Study of Mammals Living and Extinct* (London: Adam and Charles Black, 1891), pp. 680-754; E. Trouessart, "Les primates tertiaires et l'homme fossile sud-américain," *L'Anthropologie*, 3 (1892): 257-274.

167 F. Ameghino, "Les documents paléoanthropologiques du Sud-Américain et le processus évolutif des primates," *Revue de l'École d'Anthropologie de Paris*, 18 (1908): 20-30.

"bestialization", as seen in the monkeys and anthropoid apes of the Old World.

In 1912, Schlosser argued that a primate of the Oligocene from Fayoum (Egypt), which he called *Propliopithecus*, was more closely related to the hominids than any other fossil primate yet discovered.[168] Schlosser held that *Propliopithecus* probably gave rise, through *Pliopithecus* of the European Miocene, to the following genera: *Dryopithecus*, *Simia* (orang), *Gorilla*, *Troglodytes* (chimpanzee) and *Homo*.

In 1915, Pilgrim described a number of new species of *Dryopithecus* and two new genera he called *Sivapithecus* and *Palaeosimia* from the Siwalik Hills of India.[169] He believed that *Sivapithecus* resembled humankind more closely than any other fossil primate yet discovered, except perhaps *Pithecanthropus* and *Eoanthropus*. From there, he argued on the one hand that *Sivapithecus* represented an early offshoot of the gibbon line which gave rise to humans, and on the other that *Dryopithecus* was closely related to the lineages that gave rise to both the orang and the gorilla.

From this brief survey, it is clear that new fossil discoveries of primates since the 1870s stimulated new

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168 M. Schlosser, "Les Singes fossiles du Fayoum," *L'Anthropologie*, 23 (1912): 417-423.

169 G.E. Pilgrim, "New Siwalik Primates and Their Bearing on the Question of the Evolution of Man and the Anthropoidea," *Records of the Geological Survey of India*, 45 (1915): 1-74.

reflections on human phylogeny. Maybe unsurprisingly, their discoverers or describers often insisted on the importance of these primate fossils in unravelling the ancestry of humankind.

When Dubois discovered *Pithecanthropus*, he first attributed its remains to a fossil ape he called *Anthropopithecus*.[170] He was convinced that he had discovered a kind of fossil chimpanzee from Java, not unrelated to the creature from the Siwalik Hills of India described previously by Lyddeker, but more human-like than any known anthropoid ape because characterized by a bipedal gait. Dubois considered his fossil to be an intermediate form which linked humankind more closely with his next of kin among the mammals.

At this stage, it is not completely clear if Dubois intended his fossil ape to be a genuine precursor of humankind. As will be seen below, scientists such as Boule, Hrdlicka and Mahoudeau evaluated Dubois's fossil as intermediate between humankind and the anthropoid apes without holding, however, that it was ancestral to humankind. They held instead that Dubois's fossil was an ape that evolved some humanoid characters in parallel to the true human stem or stems. In other words, "intermediate" and "ancestral" should not be equated. The view that Dubois's

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170 B. Theunissen, *Eugène Dubois and the Ape-Man from Java: The History of the First 'Missing Link' and its Discoverer* (Dordrecht: Kluwer Academic Publishers, 1989), pp. 54-58.

fossil was that of an ape not ancestral to humans was not without able advocates at the time. Among them we find also Klaatsch, Kollmann, Krause and Virchow, to name a few.[171]

As shown by Theunissen, after realizing that the endocranial capacity of his fossil specimen was larger than previously evaluated, Dubois changed its name to *Pithecanthropus erectus*. From *Anthropopithecus*, the man-ape chimpanzee of Java, Dubois now evaluated his fossil specimen as *Pithecanthropus*, the ape-man from Java.[172] After 1894, Dubois made it clear what he thought the place of *Pithecanthropus* should be in the human family tree.[173] Dubois conjectured that all the anthropoid apes, including humans, stemmed from a hypothetical Tertiary gibbon-like form he called *Prothylobates*. From this form, the human line went

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171 J. Kollmann, "Commentary on Dubois' Pithecanthropus erectus, betrachtet als eine wirkliche Uebergangsform und als Stammform des Menschen," *Zeitschrift für Ethnologie*, 27 (1895): 740-744; W. Krause, "Discussion of Dubois' Pithecanthropus erectus, eine menschenähnliche Uebergangsform aus Java," *Zeitschrift für Ethnologie*, 27 (1895): 78-81; R. Virchow, "[On Pithecanthropus]," *Zeitschrift für Ethnologie*, 27 (1895): 81-88, 435-440, 648-657, 744-747; H. Klaatsch, "Des gegenwärtige Stand der Pithecanthropus-Frage," *Zoologisches Zentralblatt*, 6 (1899): 217-235.

172 B. Theunissen, *Eugène Dubois and the Ape-Man from Java: The History of the First 'Missing Link' and its Discoverer* (Dordrecht: Kluwer Academic Publishers, 1989), pp. 58-60.

173 E. Dubois, *Pithecanthropus erectus, eine menschenähnliche Uebergangsform aus Java* (Batavia, 1894); "On *Pithecanthropus erectus*: A Transitional Form Between Man and the Apes," *Scientific Transactions of the Royal Dublin Society*, 6 (1896): 1-18; "Le 'Pithecanthropus erectus' et l'origine de l'homme," *Bulletins de la Société d'Anthropologie de Paris*, 4e série, 7 (1896): 460-467; see also B. Theunissen, *Eugène Dubois and the Ape-Man from Java: The History of the First 'Missing Link' and its Discoverer* (Dordrecht: Kluwer Academic Publishers, 1989), pp. 65-66, 94-95.

through a *Pithecanthropus* stage before giving rise to *Homo sapiens*. Dubois considered *Pithecanthropus* to be either directly ancestral to humans, or very nearly related to the ancestral form. Maybe not unexpectedly, Dubois's phylogenetic view was supported by de Mortillet and Haeckel.[174] However, Dubois also gained the support of Manouvrier and Marsh, among others.[175]

As may already be clear, Dubois arrived at his phylogenetic conclusion regarding *Pithecanthropus* by comparing it almost exclusively to other living and fossil primate forms, and not to living human races. The nature of Dubois's approach is nicely captured by Theunissen when he states that:

> "Dubois thought of his missing link as an ape-like form which because of its development in the direction of Man could be regarded as a transitional form, rather than as a human-like form still at too low a stage of development to be called *Homo*".[176]

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174 G. de Mortillet, "Précurseur de l'homme et Pithécanthrope," *Revue Mensuelle de l'École d'Anthropologie de Paris*, 6 (1896): 305-317; E. Haeckel, "On our present knowledge of the origin of man," *Annual Report of the Smithsonian Institution*, 1898: 461-480.

175 L. Manouvrier, "Deuxième étude sur le 'Pithecanthropus erectus' comme précurseur présumé de l'homme," *Bulletins de la Société d'Anthropologie de Paris*, 4e série, 6 (1895): 553-651; "Réponse aux objections contre le Pithecanthopus," *Bulletins de la Société d'Anthropologie de Paris*, 4e série, 7 (1896): 396-460; O.C. Marsh, The Ape-Man From the Tertiary of Java," *Science*, n.s. 3 (1896): 789-793.

176 B. Theunissen, *Eugène Dubois and the Ape-Man from Java: The History of the First 'Missing Link' and its Discoverer* (Dordrecht: Kluwer Academic Publishers, 1989), p. 71.

In this perspective, Dubois contributed greatly to bringing the primate fossil record into "broad phylogenies". As *Pithecanthropus* was considered by a number of scientists to be more nearly related to humankind than any other anthropoid, this discovery was important in promoting the reading of the entire hominid fossil record from an interpretative framework based on living and fossil primates.

I subscribe to Theunissen's view that *Pithecanthropus* was important for the development of the field of human evolution.[177] The discovery was indeed intrumental in promoting what Theunissen calls a "phylogenetic interpretation" of the hominid fossil record, that is, the incorporation of some human fossil remains into a framework of analysis that used also knowledge from comparative anatomy and embryology of primates in order to assess human phylogeny.

However, I do not agree with Theunissen when he states that the discovery of *Pithecanthropus* gave rise to what he calls an "evolutionary paleoanthropology". The notion that a pre-evolutionary palaeoanthropology was supposedly displaced by a truly evolutionary one at the turn of the century is untenable as previously argued.

If a number of scientists evaluated *Pithecanthropus* to be a primate more or less closely related to humans, others judged it to be a representative of humankind. In

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[177] Ibid., pp. 108-117.

contradistinction to Dubois's approach, the latter scientists compared *Pithecanthropus* not to non-human primates but to modern humans. Turner made it clear that this approach revealed that the skullcap of *Pithecanthropus* presented many similarities with both the living human races now dwelling under "savage" conditions (Australians) and prehistoric races of Europe (e.g., Neandertals). *Pithecanthropus*, declared Turner, was human.[178] Among scientists who defended human status for *Pithecanthropus* were Houzé, Lydekker, Martin, Pettit and Topinard.[179]

It is true that the incompleteness of the *Pithecanthropus* remains permitted a variety of opinions about its status and phylogenetic position, as just seen. This factor should not be underestimated in this episode of the history of human evolutionary studies.

However, the *Pithecanthropus* episode reveals also what we have already identified during the analysis of the 1860-1895 period: an epistemological problem in the reconstruction of human phylogeny. After 1895, this epistemological problem

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178 W. Turner, "On M. Dubois' Description of Remains Recently Found in Java, Named By Him *Pithecanthropus erectus*. With Remarks on So-called Transitional Forms Between Apes and Man," *Journal of Anatomy and Physiology*, 29 (1895): 424-445.

179 E. Houzé, "Le Pithecanthropus erectus: discussion," *Revue de l'Université de Bruxelles*, 1 (1895-1896): 401-438; R. Lydekker," Comments on Dubois' *Pithecanthropus erectus, eine menschenähnliche Uebergangsform aus Java*, *Nature*, 51 (1895): 291; R. Martin, "Kritische Bedenken gegen den Pithecanthropus erectus Dubois," *Globus*, 67 (1895): 213-217; A. Pettit, "Le Pithecanthrope erectus," *L'Anthropologie*, 6 (1895): 65-69; P. Topinard, "Revue de Turner sur la description de M. Dubois des restes récemment trouvés à Java et attribués par lui à un Pithecanthropus erectus," *L'Anthropologie*, 6 (1895): 605-607.

entered a new phase with the institution of two interpretative frameworks for the analysis of human fossil remains. In one such framework, hominid fossils were evaluated in the light of human races. In the other, fossil remains were compared to other primate forms.

Human Fossils and Living Human Races

Let us turn first to the interpretations in which all or most human fossils were comfortably accommodated in a model in which they were phylogenetically related to one or several living human races. These interpretations established a continuity between those held during the 1860-1895 and the 1895-1930 periods. However, from phylogenies essentially restricted to Europe prior to 1895, the 1895-1930 period saw a number of phylogenies with worldwide implications.

In considering the human remains discovered at Galley Hill (England) in 1888, Newton tried to establish their affinities with known ancient remains and living forms, although he arrived at no definite conclusions.[180] He was inclined to think that the Galley Hill remains and the Borris skull (Ireland), both dolichocranial, were early representatives of a Palaeolithic race of Great Britain and Ireland from which the Neolithic people of Long Barrow type were descended. This meant that Long Barrow people, also

180 E.T. Newton, "On a Human Skull and Limb-Bones Found in the Palaeolithic Terrace-Gravel at Galley Hill, Kent," *Quarterly Journal of the Geological Society of London*, 51 (1895): 505-527.

dolichocranial, would have developed through time higher crania while losing their strong superciliary ridges.

Although Newton recognized some similarities between the Galley Hill skull and those of the Eskimo and the dolichocephalic races of the south (ancient Basques and Australians), he favoured deriving them not directly from the Galley Hill race but from early Palaeolithic representatives of continental Europe such as the Spy and Neandertal remains.

Newton's interpretation of these ancient human remains was not the result of an anti-evolutionist viewpoint, for he judged *Pithecanthropus* to be possibly a genuine "missing link" between the other primates and humankind.[181]

According to Ripley, science "proceeds best from the known present to the remote past, in anthropology as in geology or astronomy". From there he contended further that the "study of living men should precede that of the dead".[182] Ripley's view of the peopling of Europe was based on the idea that the remains of populations all over Europe had been laid down in more or less horizontal strata. This racial structure of Europe had been determined by historical, social and especially geographical factors. In this great swirlpool of migrating human races colliding and assimilating, these

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181 E.T. Newton, "Palaeolithic Man," *Nature*, 57 (1897-1898), p. 355; "The Evidence for the Existence of Man in the Tertiary Period," *Proceedings of the Geologists' Association*, 15 (1987-1898), p. 81.

182 W.Z. Ripley, *The Races of Europe: A Sociological Study* (New York: Appleton, 1899), p. 17.

were confronted by environmental influences and ethnic competition. Natural selection favoured the fittest. This process, contended Ripley, had been going on in Europe for a long time. Where nature had isolated little pools of populations, continued Ripley, one finds humans with an unbroken ancestral lineage reaching deeply into the past and which had changed little. Such was the case of the supposed link between the Cro-Magnon race and living people of the Dordogne region in France.[183]

According to Ripley, Europe was presently composed of three main racial types: 1) the Mediterranean race (dolichocephalic, of dark complexion, medium in size and slender) which extended beyond the confines of Europe into Africa; 2) the Teutonic race (dolichocephalic, of light complexion and tall) which was restricted to northwestern Europe; and 3) the Alpine race (brachycephalic, medium in size and stocky) which also extended beyond the confines of Europe but into Asia.

Assuming that all human races had stemmed from a common type, Ripley proposed that Europe had first been invaded by an Africanoid type, now represented by living members of the Mediterranean race, and of which the Neandertal type and the Cro-Magnon race were probably the earliest known Palaeolithic representatives. The living members of the Teutonic race were said to represent a somewhat modified offshoot of this

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[183] Ibid., pp. 174-179.

former stem. As for the living members of the Alpine race, Ripley derived them from more recent invaders of Asiatic affinities.[184]

According to Deniker, the controversy between monogenists and polygenists was futile because a much better knowledge of extant and extinct human races was required before it could be settled.[185] However, regardless of the resolution of this debate, continued Deniker, "we shall always be obliged to recognise the positive fact of the existence in mankind of several somatological units having each a character of its own, the combinations and the intermingling of which constitute the different ethnic groups".[186] On this view, when considering the two most ancient Palaeolithic types of Europe (Neandertal-Spy and Laugerie-Chancelade types), Deniker contended that these have persisted until today:

> "Like all the other prehistoric races, that of the Neanderthal or Spy has not entirely disappeared; Neanderthaloid skulls are found, few in number it is true, in several prehistoric or historic burial-places... Scattered here and there, some rare individuals may still be observed in the populations of the present day showing the characters of this race... [Following the Laugerie-Chancelade type through time we] see then, at the beginning of the neolithic period, the second quaternary dolichocephalic race still existing slightly modified,

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184 Ibid., pp. 457-475.

185 J. Deniker, *The Races of Man: An Outline of Anthropology and Ethnography* (London: Walter Scott, 1900).

186 Ibid., p. 8.

but we also see the earliest brachycephals [a third type] appearing along with it."[187]

For G. Sergi, the origin of numerous living Europeans could be traced back to a stem he called the Eurafrican species.[188] Having its origin in Africa, this species was defined as possessing uniform cephalic (dolichocephalic) and facial forms. It was further divided into three main races on the basis of skin colour: 1) the African race of dark complexion, 2) the Mediterranean race of brown complexion, which inhabited the great Mediterranean basin including part of northern Africa and a portion of western, central and eastern Europe, and 3) the Nordic race of light complexion found largely in Scandinavia, north Germany and England. Sergi traced the arrival, in waves, of the representatives of the Eurafrican species in Europe during the late Quaternary epoch as seen through the fossils of Laugerie-Basse and Chancelade and later in the Neolithic period with the Cro-Magnon and Baumes-Chaudes discoveries.[189]

Sergi denied that *Pithecanthropus* represented an intermediate between humankind and other primates which became transformed into the Neandertal type, only to evolve

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[187] Ibid., pp. 312-313.

[188] G. Sergi, *The Mediterranean Race: A Study of the Origin of European Peoples* (London: Walter Scott, 1901).

[189] Ibid., p. 211.

further into successive European forms (Chancelade and Cro-Magnon). Instead, Sergi held that:

> "Neanderthal type seems to me a species distinct by itself, the most ancient that we know in quaternary times... leaving few but sure records of its existence even in the present epoch... *Homo Neanderthalensis* is thus... a European species, originating in Europe in early quaternary, or possibly late tertiary times... *Homo Neanderthalensis* has not completely disappeared in Europe in spite of the arrival of a new species from Africa [the Eurafrican species]... It is desirable to note this survival of the Neanderthal man for [it shows] the persistence of cranial forms through many thousand years and in spite of mixture with other species; it also shows that the forms subsequently prevailing [Chancelade and Cro-Magnon] are not... derived from *Homo Neanderthalensis*."[190]

During the Neolithic period, contended Sergi, the ethnic unity that had prevailed in Europe since the arrival of the Eurafrican species in the late Quaternary was disturbed by the arrival, from Asia, of the Eurasiatic species as seen in the Neolithic brachycephals.

Later, Sergi renamed the three species he had identified in 1901 (*Homo neanderthalensis*, Eurafrican species and Eurasiatic species). He now referred to them as three distinct genera composed of several "species": *Palaeoanthropus* (Neandertals), *Notanthropus* (Eurafrican) and *Heoanthropus* (Eurasiatic). To these he added *Hesperanthropus* comprising the native people of America.[191] In Sergi's view,

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[190] Ibid., pp. 202-204.

[191] G. Sergi, *Le origini umane: Ricerche paleontologiche* (Torino: Fratelli Bocca, 1913).

these human genera had evolved in parallel, with a number of other primate forms, each in its respective geographical region, since the hominid group had originated from its hypothetical forerunner. Of course, this did not necessarily exclude a certain amount of migration as already seen concerning Europe. Sergi viewed human forms as entities which presented fixed characters; hybridization between "species" being a means to create new races or varieties.

For Bloch, the origins of the present European races of dolichocephalic conformation and light complexion (Finns, Lapps and Celts), could be ultimately traced back to the dolichocephalic races of the Palaeolithic period.[192] Bloch contended that the European races of light complexion were not recent comers from Asia, as had often been assumed, but rather had developed their peculiarities under the environmental conditions of Europe. He linked these living members through the Palaeolithic dolichocephals of the Neandertal race and the Neolithic dolichocephals of the Teutonic type. Bloch further contended that races of light complexion were once widely distributed in Europe. As a great number of them eventually became transformed into brachycephalic races of brown complexion, under environmental

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192 A. Bloch, "Origine et évolution des blonds européens," *Bulletins et Mémoires de la Société d'Anthropologie de Paris*, 6e série, 2 (1911): 54-78.

conditions, this explained their limited representation in Europe today.[193]

It is sometimes held that Verneau was a proponent of the "Neanderthal Phase Hypothesis". This view is incorrect. For Verneau, the Neandertal type was not *a stage* in human evolution for it was merely a form among others leading to living humans. As early as 1906, Verneau stated his position:

> "...on dirait que la race de Spy [Neanderthals] et celle de Grimaldi soient deux branches collatérales d'un tronc qu'il est encore difficile de préciser, mais qui devait resembler dans une certaine mesure au *Pithecanthropus*. Chez elles, l'évolution s'est faite en sens divergents: l'Homme de Spy ou de Néanderthal a conservé dans le crâne... une grande partie des caractères de son ancêtre, mais il en a perdu le prognathisme exagéré... L'Homme de Grimaldi, au contraire, s'est développé considérablement sous le rapport du volume de la boîte encéphalique, mais sa face n'a pas évolué dans les mêmes proportions, surtout au point de vue du prognathisme".[194]

For Verneau, prehistoric Europe has been successively inhabited by the Spy or Neandertal race, then by the Grimaldi race and much later by the Cro-Magnon race.[195] As we come

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193 See also A. Bloch, "De l'origine des brachycéphales néolitiques de la France," *Congrès International d'Anthropologie et d'Archéologie Préhistoriques*, 12e session, 1900: 271-277.

194 R. Verneau, Les grottes de Grimaldi: résumé et conclusions des études anthropologiques," *L'Anthropologie*, 17 (1906): 291-320, citation on p. 316.

195 R. Verneau, "La race de Cro-Magnon: ses migrations, ses descendants," *Revue d'Anthropologie*, 3e série, 1 (1886): 10-24; "Les fouilles du Prince de Monaco aux Baoussé-Roussé: un nouveau type humain, *L'Anthropologie*, 13 (1902): 561-585; "La race de Spy ou de

closer to the present time these races were further removed from the ancestor or ancestors that might have looked like *Pithecanthropus*; the Grimaldi race being an intermediate (though not a phylogenetic link) between the Neandertal and Cro-Magnon races. All these races, contended Verneau, still possessed some living representatives that retained, sometimes in attenuated form, features of the original type.

Verneau's conception of human phylogeny rested on the conviction that humankind was constituted, at a very early period, by one or several ape-like negroid types which evolved and spread.[196] The prehistoric races of Neandertal and Grimaldi were considered as two distinct branches of this evolving negroid type, a type still represented today in attenuated form in Australia and Neo-Caledonia, and which could be followed in time through the Talgaï (Australia) and Wadjak (Java) remains, and which is found only scattered in Europe. On this view, the origins of races of dark complexion preceded everywhere those of lighter complexion.

According to Dixon, the problem was to determine how many human races were to be recognized: their origin, distribution and interaction.[197] On the basis exclusively of

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Néanderthal," *Revue de l'École d'Anthropologie de Paris*, 16 (1906): 388-400.

196 R. Verneau, "La race de Néanderthal et la race de Grimaldi; leur rôle dans l'humanité," *Journal of the Royal Anthropological Institute*, 54 (1924): 211-320.

197 R.B. Dixon, *The Racial History of Man* (New York: Charles Scribner's Sons, 1923), p. 3.

three skull indices (cephalic, altitudinal and nasal), Dixon tentatively defined eight fundamental types or races from which all the living ones were derived by blending. These fundamental races were: Caspian, Mediterranean, Proto-Negroid, Proto-Australoid, Alpine, Ural, Palae-Alpine and Mongoloid.

When considering the human fossil remains, Dixon comfortably inserted all of them in his racial system. In Europe for instance, the Neandertal race (Le Moustier, La Chapelle-aux-Saints, La Quina) was judged to belong to the Proto-Australoid type; the early brachycephalic element to the Mongoloid type (La Chapelle-aux-Saints); the Grimaldi race to the Proto-Negroid type; dolichocephalic skulls of the Palaeolithic period in France to the Caspian type (Chancelade and Combe Capelle); and late Palaeolithic remains to the forerunners of the Mediterranean type (Cro-Magnon, Laugerie-Basse, Combe Capelle). Dixon contended that the great Neolithic migrations into Europe of the Mediterranean and Palae-Alpine types shattered and destroyed the people of the older Proto-Australoid and Mongoloid types, although some traces of them still survived while some admixture also took place.[198]

Dixon held that the Palaeolithic populations of Africa had been represented by four main racial elements (Proto-Australoid, Proto-Negroid, Mongoloid and Palae-Alpine types)

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[198] Ibid., pp. 27-33, 46-48.

as represented by the fossil remains of Gibraltar, Boskop, Broken Hill and some Egyptian palaeoliths.[199]

As for southeast Asia, Dixon recognized it as the probable homeland of the Proto-Australoid type, and alluded to *Pithecanthropus erectus* as a very early representative of this type before it migrated to Australia as reflected by the Talgai skull.[200]

Describing himself as a polygenist, although he recognized ultimately a single ancestral prototype for humankind, Dixon thought of his fundamental human racial types as arising in definite areas at different times:

> "... from the phylum which branched off from that of the anthropoid apes, a number of distinct types arose, just as among the anthropoids; and that just as the latter varieties and species spread from their several areas..., so did these originally distinct forms. The degree of divergence in the human phylum was less great than in that of the apes, the separate varieties were fertile *inter se* and have blended and crossed in every imaginable fashion to produce the existing races of man".[201]

According to Pycraft, most living "species" of humankind were directly descended from a common and differentiating trunk of the Upper Palaeolithic period.[202] The fossil remains

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199 Ibid., p. 180.

200 Ibid., pp. 475-476.

201 Ibid., pp. 504-505.

202 W.P. Pycraft, "On the Calvaria Found at Boskop, Transvaal, in 1913, and its Relationship to Cromagnard and Negroid Skulls," *Journal of the Royal Anthropological Institute*, 55 (1925): 179-198.

of Boskop, Cro-Magnon and Grimaldi were part of this trunk. However, these living human species were said to have interbred to a certain extent with the Mousterian or Neandertal element. Pycraft further argued that the living Australians and Dravidians showed a conspicuous strain of Neandertal "blood"; a phylogenetic line which also gave rise to the Rhodesian Man (Kabwe or Broken Hill of Zambia). Not unlike Verneau, Pycraft explained the origins of living humans in the following way:

> "...we may fairly postulate the divergence of the human race, at a very early stage of its development, into two branches, one with relatively feeble, and the other with strongly developed brow-ridges: *Eoanthropus* stands for the one, *Pithecanthropus* and Neander-man for the other... That the 'beetle-browed' people were the dominant race - or races - for long ages, seems to be shown by the fact that all the skulls of the Lower and Middle Palaeanthropic Periods are of the Neander type. But the fact that they were preceded, in Europe, at any rate, by the smooth-browed people represented by Eoanthropus, suggests that these two types may have been living in close proximity throughout the whole of this long period, and furthermore, that interbreeding took place between them".[203]

Pycraft's view of human phylogeny was clearly presented in a phylogenetic tree.[204]

For Ekholm, the living dolichocephalic north Europeans were not descended from the Cro-Magnon race. Instead, Ekholm linked them to *Homo Aurignacensis*, a type said to comprise

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203 Ibid., pp. 193-194.

204 Ibid., p. 193.

fossil remains from Aurignac and Combe Capelle (France), Galley Hill (England), Brüx (Bohemia), Brünn and Predmost (Moravia).[205] Immigrants from Asia to Europe, this Aurignac race was said to have mixed to some extent with the Neandertal race in the early Palaeolithic period. Ekholm argued that the representatives of the Aurignac race may not necessarily have been the first inhabitants of Scandinavia, in which case the unknown early inhabitants could not have constituted the ancestors of the present population.

In Taylor's theory of racial evolution, the very early history of human races was evaluated in the light of their migration and differentiation under changing environmental conditions of the Pleistocene.[206] In Taylor's view, the living human races (Negritos, Negroes, Australoids, Mediterraneans and Alpines) were distributed in a series of racial zones centering about south central Asia. This pattern he explained by the following process: ever since the Pliocene or early Pleistocene, after humankind had split from

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205 G. Ekholm, "The Original Home of the Indo-Europeans," in H. Lundborg and F.J. Linders (eds.), *The Racial Characters of the Swedish Nation* (Uppsala: Almquist & Wiksell, 1926), 27-30.

206 G. Taylor, "Climatic Cycles and Evolution," *The Geographical Reiew*, 8 (1919): 289-328; "The Evolution and Distribution of Race, Culture, and Language," *Geographical Review*, 11 (1921): 54-119; *Environment and Race: A Study of the Evolution, Migration, Settlement, and Status of the Races of Man* (Oxford: Oxford University Press, 1927); "Racial Migration-Zones and their Significance," *Human Biology*, 2 (1930): 34-62; "The Ecological Basis of Anthropology," *Ecology*, 15 (1934): 223-242; "The Zones and Strata Theory - A Biological Classification of Races," *Human Biology*, 8 (1936): 348-367; *Environment, Race, and Migration: Fundamentals of Human Distribution* (Toronto: University of Toronto Press, 1937).

the anthropoids, central Asia has been, throughout this period, the cradle from which the main racial movements have sprung, successively and repeatedly, to people the world. Taylor held that central Asia constituted a stimulating milieu producing, continually, more advanced human types that pushed more primitive ones to the periphery. This explained why, generally speaking, the more "primitive" human types were found at the periphery of the Asian cradle. This also explained why the distribution of human forms through time could be seen as a succession of ancient primitive types being gradually replaced by more recent advanced ones. Taylor accommodated within this racial model all or most of the human fossil remains known at the time by relating them to one of the main living races.

At first, Taylor was inclined to favour a single Asian trunk (with *Pithecanthropus* possibly at its base), from which all subsequent living and fossil human forms stemmed and migrated.[207] Later, following the discovery of so-called neandertaloid fossils throughout the Old World (Galilee, Broken Hill, Wadjak, Talgaï...), Taylor affiliated the Neandertal type to most, but not all, living races. He wrote that it was logical "to assume two precursors of modern man living in middle Palaeolithic times... These two were Neanderthal man and Proto-Negrito (Grimaldi?) type".[208]

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207 G. Taylor, "Climatic Cycles and Evolution," *The Geographical Review*, 8 (1919): 289-328.

Taylor established the Neandertals at the base of four living races (Negroes, Australoids, Mediterraneans and Alpines), while the Negrito race was independently derived from another type, possibly the Grimaldi one. On this view, the Neandertal type gave rise to the Negroid and Australoid types, from which in turn the Mediterranean and Alpine types later sprung.

In Taylor's scheme, Europe constituted a marginal land that received successive and interrupted migrations of all the racial types, the European fossil record being filled with representatives of these various races, which still have some living members.[209]

The possibility of a triple origin for the living human races was not completely discarded by Taylor:

> "I think that the simplest explanation is that the ancestors of the Negrito and Neanderthal types developed independently from the primitive Catarrhine ape-stock, in Pliocene or even Miocene times. They lived side by side in Asia until the immense climatic changes of the Pleistocene greatly quickened evolution. Whether the negro [not to be confused with the Negrito] and later races [Australoids, Mediterraneans and Alpines] developed in Asia from a Neanderthaloid man, as seems probable to the writer, or whether there was a third 'Proto Cro-Magnon' type must be left to the future to decide".[210]

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208 G. Taylor, *Environment, Race, and Migration: Fundamentals of Human Distribution* (Toronto: University of Toronto Press, 1937), p. 272.

209 Ibid., pp. 131-134, 158-166, 255-258.

210 Ibid., pp. 261-262.

It is not possible within the limits of this essay to review the researches of all scientists who interpreted the human fossil record by comparing the remains to living human races.[211] It need only be mentioned that this approach was used by a not inconsiderable number of scientists during the first third of the twentieth century, although this chapter in the history of human evolutionary studies has been neglected by historians. As already mentioned, this approach

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211 Among other scientists who seemed to have chosen this approach see also, K. Stolyhwo, "Homo primigenius appartient-il à une espèce distincte de Homo sapiens?," *L'Anthropologie*, 19 (1908): 191-216; *Le crâne de Nowosiolka considéré comme preuve de l'existence à l'époque historique de formes apparentées à H. primigenius* (Cracovie: Imprimerie de l'Université, 1908); "Zur Frage der Differenzierung der fossilen Menschenrassen," *Verhandlungen der Gesellschaft für Physische Anthropologie*, 2 (1927): 52-56; "La race néanderthalienne est-elle homogène?," *L'Anthropologie* (Prague), 6 (1928): 61-70; W.J. Sollas, "On the Cranial and Facial Characters of the Neandertal Race," *Philosophical Transactions of the Royal Society of London*, series B, 199 (1908): 281-339; *Ancient Hunters and Their Modern Representatives* (London: Macmillan, 1911); W. Turner, "The Craniology, Racial Affinities, and Descent of the Aborigines of Tasmania," *Transactions of the Royal Society of Edinburgh*, XLVI (1908-1909): 365-403; F. Corner and P. Raymond, "Le crâne de Galley Hill," *Bulletins et Mémoires de la Société d'Anthropologie de Paris*, 5e série, 10 (1909): 487-489; G. Retzius, "The So-Called North European Race of Mankind," *Journal of the Royal Anthropological Institute*, 39 (1909): 277-313; J. Beddoe, *The Anthropological History of Europe*, 2nd edition (Washington: Cliveden Press, 1912), pp. 37-63; H. Obermaier, *Der Mensch den Vorzeit* (Berlin, 1912), 365; *Fossil Man in Spain* (New Haven: Yale University Press, 1924), pp. 277-321; R.J.A. Berry and A.W.D. Robertson, "The Place in Nature of the Tasmanian Aboriginal as Deduced from a Study of his Calvaria - Part II. His Relation to the Australian Aboriginal," *Proceedings of the Royal Society of Edinburgh*, 34 (1913-1914): 144-189; A. Burkitt and J. Hunter, "The Description of a Neanderthaloid Australian Skull, With Remarks on the Production of the Facial Characteristics of Australian Skulls in General," *Journal of Anatomy*, 57 (1922-1923): 31-54; R.A. Dart, "Boskop Remains from the South-east African Coast," *Nature*, 112 (1923): 623-625; R. Nordenstreng, "The 'Cro-Magnon Race'," in H. Lundborg and F.J. Linders (eds.), *The Racial Characters of the Swedish Nation* (Uppsala: Almquist & Wiksell, 1926), 147-149; E.-G. Dehaut, *Études d'anthropotomie et de zoologie générale* (Paris: E. Le François, 1927); F. Weidenreich, "Entwicklungs - und Rassentypen des *Homo primigenius*," *Natur und Museum*, 58 (1928): 1-13, 51-62; M.R. Drennan, "An Australoid Skull from the Cape Flats," *Journal of the Royal Anthropological Institute*, 59 (1929): 417-427.

to the hominid fossil record followed in the tradition of previous efforts to establish the peopling of Europe ("restricted phylogenies") prior to 1895.

Human Fossils, Comparative Anatomy and Primate Palaeontology

Let us now turn to interpretations of human phylogeny which compared human fossils not to living human races but to living and fossil species of primates considered to be closely akin to humankind. This approach tried to establish the evolutionary line or lines that linked living humans to primate ancestors. If fossils were not placed on a direct line leading to living humans, they were put on side branches leading towards extinction. On this view, the dating of fossil specimens was often considered crucial to the determining of phylogenetic relationships. Before the end of the nineteenth century, the approach of "broad phylogenies" was being gradually supplemented with discoveries of fossils. We have already seen how the *Pithecanthropus* episode played a key role in this new phase.

According to Schwalbe, the proper way to assess the evolutionary history of humankind was by combining the results of palaeontological investigations with those derived from comparative studies of both living humans and their closest relatives.[212] As a number of scholars have already

documented[213], it is precisely this task that Schwalbe set out to accomplish.[214]

For Schwalbe, humankind's closest living relative had to be sought among the anthropoid apes as evidenced by comparative anatomy, embryology and physiology (blood relationship). While Darwin had been without any serious candidate to link humans and the apes, maintained Schwalbe, we were now in possession of transitional forms presenting apelike characters in *Pithecanthropus* and *Homo primigenius* (Neandertals): "I consider [*Pithecanthropus*] as the root of a branch which has sprung from the anthropoid ape root and has led up to man".[215] In this scheme, continued Schwalbe:

> "*Homo primigenius* must also be regarded as occupying a position in the gap existing between the highest apes and the lowest human races, *Pithecanthropus*, standing in

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212 G. Schwalbe, "Ziele und Wege einer vergleichenden physischen Anthropologie," *Zeitschrift für Morphologie und Anthropologie*, 1 (1899): 1-15.

213 See P.J. Bowler, *Theories of Human Evolution: A Century of Debate, 1844-1944* (Baltimore: Johns Hopkins University Press, 1986), pp. 68-70, 83-85; F.H. Smith, "Gustav Schwalbe: Neandertal Morphology and Systematics, 1899-1916," *Physical Anthropology News*, 6 (1987): 1-5; "Schwalbe, Gustav (1844-1916)," in F. Spencer (ed.), *History of Physical Anthropology: An Encyclopedia*, vol. 2 (New York: Garland Publishing, 1997), 916-918; E. Trinkaus and P. Shipman, *The Neandertals: Changing the Image of Mankind* (New York: Alfred A. Knopf, 1993), pp. 156-158.

214 G. Schwalbe, "Studien über Pithecanthropus erectus Dubois," *Zeitschrift für Morphologie und Anthropologie*, 1 (1899): 16-240; *Studien zur Vorgeschichte des Menschen* (Stuttgart: E. Schweizerbartsche, 1906); "The Descent of Man," in A.C. Seward (ed.), *Darwin and Modern Science* (Cambridge: Cambridge University Press, 1909), 112-136.

215 G. Schwalbe, "The Descent of Man," in A.C. Seward (ed.), *Darwin and Modern Science* (Cambridge: Cambridge University Press, 1909), p. 135.

> the lower part of it, and *Homo primigenius* in the higher, near man".[216]

In Schwalbe's mind, this morphological series (anthropoid apes, *Pithecanthropus*, *Homo primigenius* and *Homo sapiens*) did not necessarily constitute a phylogenetic series. He proposed two possible evolutionary interpretations: 1) a unilinear scheme directly linking *Pithecanthropus*, through *Homo primigenius*, to *Homo sapiens*, and 2) a multilinear scheme putting *Pithecanthropus* and *Homo primigenius* on side branches not leading to humans, but revealing nonetheless important clues as to the evolutionary stages in human evolution. Schwalbe's phylogenetic viewpoint rested on two key elements: the morphological value of fossil specimens and their stratigraphic position. For instance, Schwalbe held that *Homo primigenius* represented either a true phylogenetic link or the remnant of an intermediate stage between humans and apes. This view was based both on the recognition of a morphological hiatus separating *Homo primigenius* from *Homo sapiens* and a chronological gap separating the former (early Diluvium) from the latter (late Diluvium).

It must be noted that Schwalbe first restricted the definition of the Neandertals or *Homo primigenius* to the

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[216] Ibid., p. 129.

fossil specimens of Neandertal, Spy, Krapina and possibly Gibraltar. Later, he added other specimens to this list.

Not unlike Schwalbe, Mahoudeau also analysed human phylogeny by combining results from comparative anatomy of living humans and closely related primates, primate palaeontology and a chronological framework.[217]

Mahoudeau derived the hominid lineage from a very ancient anthropoid ape which closely resembled the gibbon. *Pliopithecus*, the Miocene ape of Europe, and *Propliopithecus*, the Oligocene ape of Egypt, were clear indications of a common stem linking humans and anthropoid apes, contended Mahoudeau. On this view, *Pithecanthropus* was intermediate between humans and apes, giving indications as to the morphological stages of the pre-hominid form, but not a true phylogenetic link because, in the view of Mahoudeau, *Pithecanthropus* was probably too late in time and too generalized a form to be a direct ancestor of the fossil races of the European Quaternary.

When considering the prehistoric races of Europe, Mahoudeau recognized in them, especially the Neandertal type, numerous apelike characters in somewhat attenuated form. Recognizing a morphological hiatus between the early Quaternary Neandertal type from the late Quaternary Cro-

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217 P.-G. Mahoudeau, "Indication des principales étapes de la phylogénie des hominiens," *Revue de l'École d'Anthropologie de Paris*, 14 (1904): 1-20; "Le Pithécanthrope de Java," *Revue Anthropologique*, 22 (1912): 453-472; "La recherche du début de l'Ère humaine," *Revue Anthropologique*, 24 (1914): 323-341.

Magnon type, Mahoudeau left open both possibilities, that the former gave rise to the latter or that the latter replaced the former.

If Schwalbe and Mahoudeau were somewhat hesitant, at one time or another, to claim with certainty that the Neandertals were directly ancestral to *Homo sapiens*, Hrdlicka championed unambiguously this view in the 1910s and after under the concept of the "Neanderthal Phase of Man".[218]

According to Hrdlicka, *Homo sapiens* could not have stemmed from an anthropoid stock (which he assumed) without having passed through a phase similar to that of the Neandertal type. Like several of his contemporaries, Hrdlicka insisted on the numerous simian features of the Neandertals. He also based his argument in favour of the Neandertal phase of man, among other things, on the chronological position of this fossil group. The traces of a modern human type never preceded or coexisted with, but always followed, the Mousterian Man (Neandertals), contended Hrdlicka. On this view, the diversification of the living human races was judged to be a fairly recent event.

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218 F. Spencer and F.H. Smith, "The Significance of Ales Hrdlicka's 'Neanderthal Phase of Man': A Historical and Current Assessment," *American Journal of Physical Anthropology*, 56 (1981): 435-459; A. Hrdlicka, "The Most Ancient Skeletal Remains of Man," *Annual Report, Smithsonian Institution*, 1914 (1913): 491-552; "The Peopling of Asia," *Proceedings of the American Philosophical Society*, 60 (1921): 535-545; "The Peopling of the Earth," *Proceedings of the American Philosophical Society*, 65 (1926): 150-156; "The Neanderthal Phase of Man," *Journal of the Royal Anthropological Institute*, 57 (1927): 249-274; *The Skeletal Remains of Early Man* (Washington: Smithsonian Miscellaneous Collections Vol. 83, 1930).

Hrdlicka postulated that the Neandertals were not an homogeneous group, but a variable one whose most advanced segments evolved into primitive *Homo sapiens* (Krapina, La Ferrassie, La Quina), while its less progressive ones (Neandertal, Spy, La Chapelle-aux-Saints, Le Moustier) might have become extinct.

It should not be assumed, however, that Hrdlicka necessarily defended a unilinear interpretation of human phylogeny. When speaking of humankind's early precursors, he stated that:

> "These earliest men doubtless from the beginning lacked in uniformity; some strains of them, in all likelihood, lacked also in vitality or in sufficient adaptability to changing conditions and have disappeared; but others kept on modifying in the upward direction until in the course of long ages they reached the various somewhat unequally advanced types of man of the present day".[219]

On this view, Hrdlicka evaluated *Pithecanthropus* to be intermediate between apes and humans, a form that might or might not have been directly ancestral to humankind. However, because Hrdlicka considered southwestern Europe to be the cradle of humankind's latest stages of development, as evidenced by the fossil remains of Europe and their almost complete absence from Asia, he favoured a western and African origin of the earliest stages of human evolution. This

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219 A. Hrdlicka, "The Most Ancient Skeletal Remains of Man," *Annual Report, Smithsonian Institution*, 1914 (1913), p. 494; see also "The Peopling of Asia," *Proceedings of the American Philosophical Society*, 60 (1921), p. 541, and "The Peopling of the Earth," *Proceedings of the American Philosophical Society*, 65 (1926), p. 151.

implied for him that the progeny of *Pithecanthropus* probably became extinct.[220]

For Boule, human evolutionary history was to be conceived by applying the principles of palaeontology.[221] Boule took them to say that humankind was closely related to the anthropoid apes, although it was not descended from the anthropoid stem. Rather, humans were probably derived from a much earlier undetermined primate common to humans and all the other Catarrhines. On this view, the living humans and apes have long been separated, their resemblance being explained more by the similarity of the evolutionary stages they went through (parallelism) than by their retention of characters from a common ancestor.

On that basis, Boule held that *Pithecanthropus* and *Homo neanderthalensis* were intermediate between humans and apes as their numerous apelike characters testified, but he held also that the distinct evolutionary trees leading to living humans and apes were highly branched ones characterized by numerous extinct branches not leading directly to living forms, among which *Pithecanthropus* and *Homo neanderthalensis* figured.[222]

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220 A. Hrdlicka, "The Peopling of Asia," *Proceedings of the American Philosophical Society*, 60 (1921), p. 541.

221 G. Laurent, "Les idées sur l'origine de l'homme au début du XXe siècle: Les conceptions de Marcellin Boule (1861-1942)," in *Nature, Histoire, Société: Essais en hommage à Jacques Roger* (Paris: Klincksieck, 1995), 433-442.

222 M. Boule, "L'homme fossile de La Chapelle-aux-Saints (Corrèze)," *L'Anthropologie*, 19 (1908): 519-525; "L'*Homo neanderthalensis* et sa place dans la nature," *Congrès International d'Anthropologie et*

In fact, Boule affiliated *Pithecanthropus* to the anthropoid stem, but recognized that it evolved, in parallel, a number of human-like traits.

When considering the human stem, Boule hypothesized at least two distinct and long separated lineages. The first lineage led to *Homo neanderthalensis*, possibly through *Homo heidelbergensis* (represented by the Mauer jaw). This lineage was described as morphologically homogeneous, and clearly distinguished by numerous primitive, apelike characters from all living and fossil forms of *Homo sapiens*. The fact that *Homo neanderthalensis* has not contributed to any living representatives was further indicated, according to Boule, by its contemporaneity with more advanced forms such as the Grimaldi type.

The second lineage Boule recognized was considered truly ancestral to *Homo sapiens*. It might have been represented at an early time by the Piltdown cranium (Boule excluded the jaw which he considered to belong to a fossil chimpanzee). This lineage could be followed towards the end of the Pleistocene in Europe through the Cro-Magnon, Grimaldi and Chancelade races, by which time the major divisions of living humans already existed.

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d'Archéologie Préhistoriques, 14e session, vol. 2, pp. 392-395; "L'homme fossile de La Chapelle-aux-Saints," *Annales de Paléontologie*, 6 (1911): 109-172; 7 (1912): 21-56, 85-192; 8 (1913): 1-70; "La paléontologie humaine en Angleterre," *L'Anthropologie*, 26 (1915): 1-67; *Les hommes fossiles: éléments de paléontologie humaine* (Paris: Masson, 1921). See also M. Boule and R. Anthony, "L'encéphale de l'homme fossile de La Chapelle-aux-Saints," *L'Anthropologie*, 22 (1911): 129-196.

According to Keith, the evidence afforded by comparative anatomy and embryology was not precise enough to establish the natural histories of humans and apes. He contended that the palaeontological evidence might be the ultimate source of evidence. Turning to this source, Keith held that only four fossil humanoid specimens were complete enough to permit reconstruction: *Pithecanthropus*, Neandertal and the two Spy specimens.[223] From them, Keith became convinced that humankind has not changed much since the end of the Tertiary period, although brain size increased while the masticatory system was reduced, features that linked humankind to other primate ancestors.

Bringing later some precision as to the phylogenetic implication of this view, Keith argued that *Pithecanthropus* (which he called *Homo javanensis*) and Neandertal types represented stages in the evolution of modern humans from the apes.[224] He contended that the Neandertal type was both an extinct precursor and direct ancestor of modern humans, while he judged *Pithecanthropus* to be either a true ancestral link or a late surviving form of a more ancient stage in human evolution.[225]

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223 A. Keith, "Pithecanthropus erectus - A Brief Review of Human Fossil Remains," *Science Progress*, 3 (1895): 348-369.

224 A. Keith, *Ancient Types of Man* (London: Harper, 1911).

225 Ibid., pp. 78-79, 136-137.

Although he might have been inclined at first towards a unilinear scheme, Keith soon changed his mind and proposed a multilinear one.[226] He now concurred with the view that the Neandertal type was totally distinct from all living human races by virtue of its numerous simian traits, and represented a side branch of human evolution. Simultaneously, he also held that the existence of fossil remains of modern appearance such as Galley Hill, Bury St. Edmunds, Moulin Quignon, Grenelle, Clichy, Denise, Olmo, Castenedolo, Grimaldi and Combe Capelle, which Keith assumed were contemporaneous with, or older than, the Neandertal remains, meant that modern human types or races had started their differentiation as early as the late Pliocene or early Pleistocene periods. Instrumental in Keith's shift of view about human phylogeny was the discovery of presumably very ancient remains with modern features at Piltdown (*Eoanthropus*).[227]

In 1915, Keith distinguished at least three major branches on the human stem:

> "The common Pliocene ancestor which gave origin to three such types could not be of a very low form. At least in

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226 A. Keith, "The Relationship of Neanderthal Man and Pithecanthropus to Modern Man," *Nature*, 89 (1912): 155-156; *The Antiquity of Man* (London: Williams and Norgate, 1915). See also J.R. Moir and A. Keith, "An Account of the Discovery and Characters of a Human Skeleton Found Beneath a Stratum of Chalky Boulder Clay Near Ipswich," *Journal of the Royal Anthropological Institute*, 42 (1912): 345-379.

227 See F. Spencer, *Piltdown: A Scientific Forgery* (Oxford: Oxford University Press, 1990), esp. chap. 2 and 3.

> Eoanthropus [Piltdown], as in Neanderthal man, the brain was equal in size to that of modern man. If we suppose that in an early part of the Pliocene period there was a form of man in which the brain had attained a human size, but in which the mandible, the teeth, and the skull still remained anthropoid in conformation, we have such a type as would serve as a common ancestor... It will thus be seen that I look on Eoanthropus, as on Neanderthal man, as a representative of an extinct form of man. Further, we realise that the three or four human types so far discovered represent but a few fossil twigs of the great evolutionary human tree. We may hope to find more branches."[228]

On this view, *Pithecanthropus* figured as a late representative (late Pliocene or early Pleistocene) of an extinct stem that split as early as the Miocene period from the humanoid stem. This humanoid stem was said to have separated from the great anthropoid stem during the Oligocene period.

In 1925, Keith recognized that the fossil material he relied on to prove the great antiquity of the modern human types or races turned out to be unreliable. He also mentioned that the anticipated length of time of the geological periods had been overestimated. Consequently, he held that "evolution has proceeded at a more rapid pace in the fashioning of man than some of us have hitherto thought".[229] This being said, Keith's conception of human phylogeny remained unchanged. To his previous scheme, he

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228 A. Keith, *The Antiquity of Man* (London: Williams and Norgate, 1915), pp. 505, 506.

229 A. Keith. *The Antiquity of Man*, Vol. 1, 2nd edition (London: Williams and Norgate, 1925), p. xiii.

added another side branch represented by the Rhodesian specimen, while fossil remains such as Wadjak, Talgai, Boskop, Galley Hill, Grimaldi, Combe Capelle, Cro-Magnon and Obercassel were all affiliated to living human races or *Homo sapiens*. In 1931, Keith added another important side branch to his phylogenetic scheme, the one represented by *Sinanthropus* or Peking Man.[230]

In a thesis that Gregory maintained throughout the 1910s, 1920s and 1930s, he argued that humankind and the anthropoid apes shared a common ancestor of fairly late period.[231] In this, Gregory strongly opposed those who favoured that humans and apes acquired most of their similarities through parallel evolution rather than by direct kinship. Furthermore, Gregory's view implied that the precursors of humans should have been late ground apes of large size closely allied to *Pithecanthropus* (small-brained and bipedal), rather than early large-brained primates of small size.[232]

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230 A. Keith, *New Discoveries Relating to the Antiquity of Man* (London: Williams and Norgate, 1931).

231 W.K. Gregory, "Studies on the Evolution of the Primates," *Bulletin of the American Museum of Natural History*, 35 (1916): 239-355; *The Origin and Evolution of the Human Dentition* (Baltimore: Williams & Wilkins, 1922); "How Near is the Relationship of Man to the Chimpanzee-Gorilla Stock?," *Quarterly Review of Biology*, 2 (1927): 549-560; "The Origin of Man from the Anthropoid Stem - When and Where?," *Proceedings of the American Philosophical Society*, 66 (1927): 439-463; "A Critique of Professor Osborn's Theory of Human Origins," *American Journal of Physical Anthropology*, 14 (1930): 133-164; *Man's Place among the Anthropoids: Three Lectures on the Evolution of Man from the Lower Vertebrates* (Oxford: Clarendon Press, 1934).

Gregory's thesis, although based on comparative anatomy in general, was derived largely from studies of the primate dentition. He held that the separation of Hominidae from Simiidae probably took place as late as the Upper Miocene period; the ancestral man-chimpanzee-gorilla stock being represented by the fossil *Dryopithecus-Sivapithecus* group. On this view, the chimpanzee-gorilla group retained most of the ancestral condition, while humankind's forerunners went through profound structural changes in a fairly short time under pressure of their new adaptive zone.

When turning to human phylogeny, Gregory put *Pithecanthropus* and *Homo neanderthalensis* on side branches from the main lineage leading to *Homo sapiens*[233]; *Homo heidelbergensis* being considered directly ancestral to both *Homo sapiens* and *Homo neanderthalensis*, while *Pithecanthropus* was relegated to the base of the hominid stem and depicted as a form closely related to the entire *Dryopithecus-Sivapithecus-Homo* group.[234] Gregory supported his view on human phylogeny by pointing to numerous dental characters

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232 W.K. Gregory, *The Origin and Evolution of the Human Dentition* (Baltimore: Williams & Wilkins, 1922), p. 413.

233 It must be noted that Gregory had not excluded completely the possibility of the Neanderthals being ancestral to later human forms, see Ibid., p. 458.

234 See Gregory's 'phylogenetic tree' in "Studies on the Evolution of the Primates," *Bulletin of the American Museum of Natural History*, 35 (1916), p. 338.

linking human fossils with the *Dryopithecus-Sivapithecus* ancestral group.[235]

Osborn's conviction that human phylogeny had to be worked out by comparing humankind with its closest relatives was simply stated as follow: "we cannot interpret the anatomy of the men of the Old Stone Age without a survey of the principal characters of the existing anthropoid apes...".[236] Osborn held that humans and anthropoids had no direct link but were descended from a common stock of the late Oligocene or early Miocene periods, as seen in his phylogenetic tree.[237] During the Miocene and Pliocene times, argued Osborn, the human lineage acquired a combination of key traits such as the erect posture, an opposable thumb, increased brain size and the power of speech.

When reviewing the fossil human remains, Osborn persistently stressed their human and simian features alike. For instance, he wrote:

> "the Heidelberg race [Mauer jaw] will be found to be a Neanderthal in the making, ...[a] more ape-like ancestral form. In the matter of the retreating chin, the true Neandertals of Spy, Malarnaud, Krapina, and La

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235 W.K. Gregory, *The Origin and Evolution of the Human Dentition* (Baltimore: Williams & Wilkins, 1922), pp. 365, 426-459; "The Origin of Man from the Anthropoid Stem - When and Where?," *Proceedings of the American Philosophical Society*, 66 (1927), p. 446.

236 H.F. Osborn, *Men of the Old Stone Age: Their Environment, Life and Art* (New York: Charles Scribners's Sons, 1915), p. 51.

237 Ibid., p. 54.

Chapelle rank exactly half-way between the most inferior races of recent man and the anthropoid apes".[238]

Osborn illustrated humankind's family tree as being composed of a number of entirely separate branches with two main stems: one stem grouped *Pithecanthropus*, *Homo heidelbergensis* and *Homo neanderthalensis*, all evolving separately toward extinction, while another stem led to modern human types and was represented by the fossil races of the Upper Palaeolithic (Cro-Magnon, Brünn, Grimaldi, Furfooz...).[239] On this view, Piltdown was judged to represent an extinct offshoot from the base of the stem leading to living humans. It was supposed also that the five or six branches of *Homo sapiens* uncovered in the Upper Palaeolithic separated from one another in Lower Palaeolithic times in Asia.

During the 1920s, Osborn somewhat modified his view on human phylogeny. He now discarded the "Ape-Man" ancestry of humankind and replaced it by the "Dawn-Man" theory of human descent, that is, the view that humans had spung from an Oligocene "neutral" stock. That is, the so-called simian characters of the human lineage were acquired either by parallel evolution or by a common inheritance with all the other primates.[240] "I agree with my colleagues", said Osborn,

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238 Ibid., pp. 100-101.

239 Ibid., pp. 489-491.

"that man passed through an arboreal stage, but I believe that this stage did not progress so far as to carry man into a stage approaching that of the anthropoid apes".[241]

This new view would not alter significantly Osborn's phylogenetic tree of human evolution.[242] What changed, instead, was his interpretation of the morphological characters of human fossils. He now insisted on the human and non-anthropoid traits of all human fossils: large brain, erect posture, proportions of the upper and lower limbs, configuration of the hands and feet. In short, Osborn now held that human characters were acquired early. He predicted the discovery of a large-brained pre-man in the Pliocene, which he now claimed was proved by the supposed new earlier age for the Piltdown remains. The relatively small brain of *Pithecanthropus* was explained as a case of arrested development (pathology).

Osborn sought further support in palaeontology by arguing that living members of mammalian families were often

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240 H.F. Osborn, "Recent Discoveries Relating to the Origin and Antiquity of Man," *Proceedings of the American Philosophical Society*, 66 (1927): 373-389; *Man Rises to Parnassus: Critical Epochs in the Prehistory of Man* (Princeton: Princeton University Press, 1927); "Is the Ape-Man a Myth?," *Human Biology*, 1 (1929): 4-9; "The Discovery of Tertiary Man," *Science*, 71 (1930): 1-7.

241 H.F. Osborn, "Recent Discoveries Relating to the Origin and Antiquity of Man," *Proceedings of the American Philosophical Society*, 66 (1927), p. 389.

242 Compare H.F. Osborn, "The Discovery of Tertiary Man," *Science*, 71 (1930), p. 3, with his previous diagramme in *Men of the Old Stone Age: Their Environment, Life and Art* (New York: Charles Scribners's Sons, 1915), p. 491.

represented in the Pliocene by fairly modern looking forms. Furthermore, he turned to embryology in order to demonstrate that there was no evidence of the human form having passed through an ape-like prehensile lower limb stage.

It is not possible within the limits of this essay to review the researches of all scientists who interpreted the human fossil record by comparing the remains to living and fossil primate forms.[243] As already mentioned, this approach followed in the tradition of previous efforts to establish a phylogenetic framework on the basis of comparative anatomy and embryology of living forms ("broad phylogenies") without the benefit of fossil human remains prior to 1895.

A Synthesis of Approaches

As seen above, the recognition of two distinct interpretative frameworks within which human fossils were received between 1895 and 1930 is of analytical value. It betrays an epistemological problem in the establishment of human phylogeny. Human fossils were directly aligned to living human races if their human traits were stressed, whereas numerous human fossils were relegated to side

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243 Among other scientists who contributed to this approach see also C. Hill-Tout, "The Phylogeny of Man From a New Angle," *Transactions of the Royal Society of Canada*, 15 (1921): 47-82; E.A. Hooton, *Up from the Ape* (New York: Macmillan, 1931); R.S. Lull, *Ancient Man* (New York: Doubleday, Doran & Company, 1928); G. Elliot Smith, *The Evolution of Man* (London: Oxford University Press, 1924); H.H. Wilder, *The Pedigree of the Human Race* (New York: Henry Holt, 1926); A. Smith Woodward, *A Guide to the Fossil Remains of Man* (London: British Museum, 1915).

branches leading toward extinction if their simian traits were emphasized.

However, a number of hominid phylogenetic interpretations are more easily classified as a complex synthesis between the two interpretative frameworks. Let us review the researches of some scientists favouring this approach.

In Klaatsch's view, human races and anthropoid apes had an intertwined evolutionary history.[244] Having compared the Neandertal type with the Aurignacian type (*Homo aurignacensis hauseri*: Combe Capelle, Brünn I, Chancelade, Galley Hill, Krapina and Engis), Klaatsch became convinced that these two fossil human types had originated from distinct stocks; stocks which were thought to have given rise to both anthropoid and human groups.

Klaatsch postulated the origin of all these forms from a common south Asian stock of man-like apes (not ape-like men) he called *Propithecanthropus*. The *Pithecanthropus* offshoot might have stemmed directly from this primaeval group. From this primaeval stem, Klaatsch envisaged repeated outpourings of populations over Asia and Africa, these being organized around two main stems, and each stem being composed of

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244 H. Klaatsch, "Die Aurignac-Rasse und ihre Stellung im Stambaum der Menschheit," *Zeitschrift für Ethnologie*, 42 (1910): 513-577; *The Evolution and Progress of Mankind* (London: T. Fisher Unwin, 1923). See also R.N. Wegner, "A New Theory of the Descent of Man," *Nature*, 85 (1910): 119-121; G. von Bonin, "Klaatsch's Theory of the Descent of Man," *Nature*, 85 (1911): 508-509.

several evolutionary branches. Representatives of these two stems subsequently reached Europe: an eastern stem was said to have given rise to the orang, the Aurignacian type and several human races such as the Mongoloid and the white races, whereas a western stem gave rise to the gorilla, the Neandertal type and the negroid races of humankind.

Klaatsch recognized a third stem that originated directly from the *Propithecanthropus* group and led to human forms such as the Australians: the latter forms he described as a "stationary remnant of a primitive humanity".

In Klaatsch's view, the Neandertals were not derived from the gorilla, but both were derived from a common stock of man-like apes that split into a form that degenerated (gorilla), while another developed further along a humanoid line to the Neandertals.

Gray concurred with Klaatsch, but introduced some modifications. He maintained the association between the Neandertal type and the gorilla, but considered that the Aurignacian type, which he called the Galley Hill type, was more closely related to the chimpanzee than to the orang-outang. Like Klaatsch, Gray seems to have associated the orang with the Mongoloid races.[245]

Not unlike Klaatsch, Crookshank judged human races and anthropoid apes to have an intertwined evolutionary past.[246]

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245 J. Gray, "The Differences and Affinities of Palaeolithic Man and the Anthropoid Apes," *Man*, 11 (1911): 117-120.

Approaching the question from a medical viewpoint, Crookshank drew a parallel between the three main modern human races (Black, White and Yellow) and specific diseases associated with each of them (Black and the type of idiot called the "Ethiopic" variety; White and a mental illness called Dementia Precox; Yellow and the Mongolian imbecile). Taking these diseases to represent remnants of past evolutionary stages for each main human race, Crookshank established another parallel between such diseases and three great apes: gorilla, chimpanzee and orang-outang. By so doing, Crookshank hypothesized phylogenetic links between respectively the Black and the gorilla, the White and the chimpanzee and the Yellow and the orang. He tried to reinforce these phylogenetic links by noting numerous homologies in morphology and behaviour. The presence of Mongolian imbeciles among the White races was explained, for instance, by historic or prehistoric crossing with the Yellow or Mongolian races.

The human fossil record was said to provide further proof of this evolutionary scheme: the Grimaldi remains were associated with the Black-gorilla branch; the Cro-Magnon and Piltdown remains with the White-chimpanzee branch and the Chancelade and Mauer remains with the Yellow-orang branch. Like Klaatsch, Crookshank postulated that each of the three

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246 F.G. Crookshank, *The Mongol in Our Midst* (London: Kegan Paul, Trench, Trubner & CO., 1924); *The Mongol in Our Midst*, 3rd edition (London: Kegan Paul, Trench, Trubner & CO., 1931).

main phylogenetic branches he identified was characterized by a progressive form (human) and a degenerate one (ape).

The evolutionary schemes briefly reviewed here intertwined human races and anthropoid apes. However, in some schemes human races were intertwined with Catarrhine, Platyrrhine and even Lemurine primates as seen especially in the works of Horst and Sera.[247]

Conclusion: Human Evolution as a Discipline

At the heart of the field of human evolution has always lain the concern to establish the phylogeny of humankind.

Traditionally, it has been assumed that the rise of evolutionary biology around the mid-nineteenth century constituted the proper context in which to formulate human phylogenetic interpretations. On this view, human evolutionary studies were a logical consequence or a by-product of evolutionary biology. But as human fossils were not interpreted as "missing links" between humans and apes during the second half of the nineteenth century, it was further assumed that the field was dominated by anti-evolutionists. It was held that evolutionary approaches to

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247 For a discussion of these schemes see for instance H.V. Vallois, "Y a-t-il plusieurs souches humaines?," *Revue générale des Sciences pures et appliquées*, 38 (1927): 201-209.

human phylogeny were prosecuted only after the turn of the century as in the unilinear-multilinear debate.

It is argued in this essay that the rise of the field of human evolution was not simply a by-product of evolutionary biology. Consequently, the nature of human evolution as a scientific discipline is changed significantly. The institution of human evolutionary studies in the second half of the nineteenth century involved a complex scientific process in which several sources contributed simultaneously, sometimes at different levels, to the establishment of phylogenetic relationships. The implication of this is simple: there were no clear differences, except of scale, between the phylogenies based on the monophyly-polyphyly debate and those on models concerned with the peopling of Europe. What distinguished these levels at which the phylogenetic analyses were performed was not related to different conceptions of the transmutation theory, but to the nature of the empirical evidence. Anti-transformist views played but an unimportant role.

We have recognized three sources of phylogenetic hypotheses: 1) the transmutation theory, 2) the monogeny-polygeny debate which was largely fused with the preceding source, as seen through the monophyly-polyphyly debate, and 3) the prehistoric peopling of Europe.

The establishment of humankind's place in nature, phylogenetically speaking, was arrived at only by combining

several approaches. Unsurprisingly, this created problems of the integration of knowledge which resulted in internal tensions that persisted into the twentieth century.

The field of human evolution was characterized by a gap between broad phylogenies and restricted phylogenies during the 1860-1895 period. This situation was created by what we call in this essay *an epistemological problem*. This problem implies that as early as the 1860s a broad phylogenetic framework was worked out between living humans and other primates on the basis of comparative anatomy and embryology. Yet, this broad framework could not be tested against the fossil record. The ancient human remains available between 1860 and 1890, though somewhat abundant, were mostly of modern appearance and almost exclusively restricted to Europe. However, these human remains were of great value for tracing the prehistoric peopling of Europe. They were most fruitfully compared to living human races and so were indeed not suitable to establish ancestral links between humans and apes. Phylogenetic links between living humans and the ancient human remains were thus established. At this stage, broad and restricted phylogenies were essentially complementary sources that contributed to the establishing of a picture as complete as possible of human phylogeny.

In the light of more primate and human fossil discoveries made throughout the world, the epistemological problem mentioned above expressed itself in a somewhat

modified manner between 1895 and 1930. If the human fossil record was almost exclusively compared with living humans prior to 1895, it was thereafter compared also with living and fossil non-human primates by an increasing number of scientists. By then, two distinct approaches to the hominid fossil record constituted largely antagonistic forces at work in the establishing of human phylogeny.

On the one hand, a number of scientists who worked on the prehistoric peopling of Europe before 1895 continued subsequently to integrate human fossils into a framework in which they were phylogenetically related to one or several living human races. While restricted to Europe prior to 1895, this approach was later often applied worldwide. On this view often neglected by historians, most human fossils were comfortably integrated into the phylogeny of living humans.

On the other hand, an increasing number of scientists now integrated the fossil record into their knowledge of the phylogenetic link between humankind and non-human primates, based on comparative anatomy and embryology of living forms. These phylogenetic interpretations were sometimes expressed in unilinear hypotheses but most often in multilinear schemes in which a great number of human fossils were put on side branches leading toward extinction.

Since several sources and approaches contributed to the study of human phylogeny between 1860 and 1930, the

interesting question arises whether the field of human evolution could have developed without any theory of evolution at all. Of course, this question is purely hypothetical. We have shown in this essay the role played by evolutionary biology in human evolutionary studies. Yet, this question may usefully point to a number of misconceptions about the history of research on human phylogeny.

From our contemporary viewpoint, it would seem that, whereas phylogenies of the peopling of Europe, and later of the earth, were concerned with changes of human races (intraspecific changes), the transformation of one species into another (interspecifc changes) was left to be explained by evolutionary biology.

As seen in this essay, things were not so simple. For many scientists of the time, among whom Darwin and Huxley figured, the question whether all living humans were part of the same species was of minor importance. Furthermore, for scientists like Sergi and Pycraft members of different species could interbreed successfully. It must be recognized that the species question, to use modern terminology, was not a key issue at the time.

What really mattered for a number of scientists was the recognition that morphological types changed through time. This was exactly what a number of evolutionists such as Schaaffhausen, Vogt, Broca, Fraipont and Lohest, Huxley,

Hervé, Verneau, Dixon, Pycraft and Taylor, among others, recognized when they assessed the human peopling of Europe or the earth.

To complicate things further, two different and not necessarily connected ideas should be distinguished. It is generally assumed that the theory of transmutation went hand in hand with the idea that humans were descended from a simian ancestor. However, Stoczkowski argued that the first scenario of hominization had not arisen within a transformist context, but from a "modificationist" one.[248] He has shown that scientists like Delamétherie, Fortis, Holbach, Lacépède and Virey held during the late eighteenth and early nineteenth centuries that humankind was not derived from a simian ancestor by transmutation, but rather that it became modified from a simian-looking creature into a modern human one. In this perspective, humankind had no genetic link with the other living primates, although its original form was ape-like.

It seems, therefore, that the simian ancestry of humankind had been assessed from at least three viewpoints between the end of the eighteenth century and the first part of the twentieth century. Some of these views were complementary while others were antagonistic:

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248 W. Stoczkowski, "Portrait de l'Ancêtre en Singe: L'Hominisation sans Évolutionnisme dans la Pensée Naturaliste du XVIIIe Siècle," in R. Corbey and B. Theunissen (eds.), *Ape, Man, Apeman: Changing Views since 1600* (Leiden University: Department of Prehistory, 1995), 141-155.

1) Proponents of a "modificationist" theory such as Delamétherie, Fortis, Holbach, Lacépède and Virey derived living humans from a simian-looking form which was not genetically related to the other non-human primates.

2) The strict application of the transmutation theory led to the view that humankind's resemblance to living apes implied that both were derived from a common ancestor. This approach was expounded by numerous scientists such as Darwin, Huxley, Haeckel, Vogt, Topinard, etc.

3) The establishment of human phylogeny by affiliating human fossils to living human races, at first in Europe and later worldwide, led to the recognition that some morphological tranformation had occurred during the late Pliocene and Pleistocene periods. The application of this approach to the hominid fossil record led a number of scientists such as Fraipont and Lohest, Verneau, Dixon and Taylor to suggest possible evolutionary links between human races and apes through the fossil record. Although proponents of this latter viewpoint were for the most part evolutionists, as seen in this essay, it seems not unreasonable to speculate that as more human fossils were recovered the application of this approach might eventually have led to the discovery of genetic links between humans and apes without the formulation of any transmutation theory at all. Although this essay is not concerned with the evolutionary pattern/process question, numerous scientists

seem to have proposed human phylogenies by merely recognizing morphological links between living and fossil forms, without apparent commitment to any evolutionary process. This seems to have been especially true of scientists who were concerned to establish the prehistoric peopling of Europe, and later of the world.

The derivation and transformation of living humans from a simian-like creature, whether or not this latter form was actually genetically linked to other primate forms, was arrived at by several routes and not exclusively through the intermediary of the transmutation theory.

Human evolution as a scientific discipline should be envisioned as a complex entity characterized by many approaches which have contributed to the establishment of human phylogeny. By so doing, new light is thrown on how the hominid fossil record was interpreted in the phylogenetic debates.

Although this essay is restricted to the analysis of the 1860-1930 period, the thesis presented here may shed new light on the understanding of the debates on human phylogeny in the rest of the twentieth century.

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Chapter 3

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La généalogie de l'homme et l'archéologie préhistorique, 1900-1950 : une synthèse difficile

L'homme est à la fois une créature biologique et culturelle. Dans la deuxième moitié du XIXe siècle cette dualité s'exprime dans un cadre évolutif la plaçant à la croisée de nouvelles perspectives. Comment ce dualisme est-il rendu intelligible durant la première moitié du vingtième siècle ? L'est-il dans un cadre disciplinaire unique ou multiple ? Observons-nous une synthèse épistémologique ou une rupture ? Le texte qui suit n'entre pas dans une approche historique de la paléontologie humaine et de l'archéologie préhistorique, mais vise plutôt à une analyse de la relation entre les deux disciplines.

Gabriel et Adrien de Mortillet : l'analyse unifiée du passé de l'homme

Pour Gabriel et Adrien de Mortillet, l'analyse de la préhistoire envisagée tant du point de vue de la culture que de l'aspect physique de l'Homme — et telle qu'elle apparaît dans la troisième édition de l'ouvrage *Le Préhistorique* — s'intègre dans une discipline unique, la palethnologie : "La palethnologie, comme son nom l'indique, [s'occupe] essentiellement de l'ethnologie la plus ancienne, étudiant par conséquent l'Homme primitif, son développement physique, moral et industriel..." (Mortillet G.,

Mortillet, A., 1900 : 230). La palethnologie se caractérise par l'association étroite qu'elle établit entre un type morphologique et une culture donnée de la même époque. Cette relation étroite est maintenue, même en l'absence d'ossements d'hommes fossiles, là où seul le matériel culturel en traduit la présence. Par exemple, reconnaissant une distinction entre deux cultures éolithiques, cultures dont l'existence même est controversée à l'époque et sera rejetée plus tard, Gabriel et Adrien de Mortillet les attribuent à deux espèces fossiles encore hypothétiques, *Homosimius bourgeoisii* et *Homosimius ramesii* : le fameux Anthropopithèque.

Gabriel et Adrien de Mortillet ne peuvent pas, même si c'est leur intention, associer de façon rigide un type humain morphologique à un niveau culturel puisque, selon eux, l'évolution de la morphologie s'effectue de façon graduelle et forme un continuum qui ne peut être aisément coupé. Les niveaux industriels ou culturels vers lesquels ils se tournent, selon une séquence qui à leur avis est naturellement discontinue, sont des témoins repères dans cette évolution morphologique graduelle. Sa continuité est arbitrairement découpée selon la spécificité culturelle. Ainsi, le continuum morphologique de la race de Néandertal, par exemple, associé aux trois niveaux culturels que sont le Chelléen, l'Acheuléen et le Moustérien, peut être divisé sur la base de cette séquence culturelle. Comprenons-nous, le continuum ne s'estompe pas, mais les niveaux industriels qui l'accompagnent sont les indicateurs de la transformation d'un type morphologique au cours du temps. Même si un type ne se déduit pas de façon stricte et rigide de chaque niveau culturel, et réciproquement, G. et A. de Mortillet maintiennent tout de même la relation entre morphologie somatique et culture. Si on considère l'ensemble des types morphologiques et des niveaux culturels, on conforte l'étroitesse de cette relation : "Le paléolithique ne peut, au point de vue des types humains, se diviser qu'en deux grandes coupes... [qui] correspondent tout à fait aux deux grandes divisions de l'industrie. Pendant le paléolithique inférieur et moyen... [race de Néandertal/Chelléen-

Acheuléen-Moustérien] emploi exclusif de la pierre. Pendant le Paléolithique supérieur... [race de Laugerie/Solutréen-Magdalénien-Tourassien] adjonction des instruments en os. La classification par l'Homme, si elle n'est suffisante par elle-même, vient du moins pleinement confirmer la classification par l'industrie" écrivent-ils dans *Le Préhistorique* (*ibid.* : 346-347).

Au sein de la palethnologie, les évolutions morphologique et culturelle de l'Homme sont inextricablement liées et fournissent un cadre épistémologique précis pour construire son passé : d'un niveau culturel se déduit un stade morphologique et inversement, malgré les difficultés à établir une association stable entre les deux.

Archéologie préhistorique : rupture épistémologique et disciplinaire

De nombreux préhistoriens et archéologues briseront le lien épistémologique unissant la morphologie de l'Homme fossile et ses industries, traçant de ce fait même de nouveaux contours disciplinaires. Au début du vingtième siècle, Breuil, par exemple, illustre clairement cet état de chose. Il écrit : "Il devient de plus en plus évident que ce qu'on a pris d'abord pour une série continue, due à l'évolution sur place d'une population unique, est au contraire le fruit de la collaboration successive de nombreuses peuplades réagissant plus ou moins les unes aux autres, soit par une influence purement industrielle ou commerciale, soit par l'infiltration graduelle ou l'invasion brusque et guerrière de tribus étrangères" (Breuil, 1912 : 169).

Reconnaître qu'une séquence lithique est le produit d'interactions entre une multitude de peuplades, et non le fruit d'une seule d'entre elles, rend caduque l'association que l'on pourrait d'avance établir entre le type humain et l'industrie, et vice-versa. Seule la découverte effective de telles associations peut le confirmer. Les caractéristiques d'une séquence lithique

ne correspondent en rien aux caractéristiques des peuples ou types morphologiques responsables de ces industries. Breuil ne cherche pas à associer une forme humaine particulière à chacune des industries, mais plutôt à établir la séquence lithique dans une région précise, en proposant une nouvelle série linéaire détaillée du Paléolithique supérieur et qui comprend neuf époques entre l'Aurignacien et l'Azilien. D'entrée de jeu, la conception d'un foisonnement de peuplades et la découverte d'une séquence industrielle linéaire produit une rupture de l'association type physique et culture. De plus, comment chaque époque industrielle prise individuellement pourrait-elle correspondre à un peuple ou type morphologique, puisque chacune d'elle est le fruit d'une synthèse, d'une construction, tirée de plusieurs sites archéologiques ? Cette procédure archéologique, habituelle pour l'époque, fait que toute forme de spécificité morphologique ou ethnique s'en trouve occultée (voir Groenen 1994 :162-195).

Au cours des années 1920 et 1930, les recherches intensives réalisées par les préhistoriens français — qui cultivaient un goût particulier pour l'empirisme et dont l'un des buts principaux consistait à construire des schèmes lithiques spatio-temporels —, mènent à reconnaître la contemporanéité de certaines époques lithiques (Sackett 1981 : 87). Pour le Paléolithique supérieur, par exemple, Garrod (1928) soutient que l'Aurignacien moyen n'est pas un stade intermédiaire entre l'Aurignacien supérieur et inférieur, mais une culture parallèle et étrangère à l'Europe. Peyrony (1933, 1936) propose le parallélisme des époques aurignacienne et périgordienne, époques définies habituellement en succession. Pour le Paléolithique ancien et moyen, Breuil (1932) propose un scénario qui aura une forte influence, et comprenant deux grandes traditions lithiques évoluant en parallèle : la tradition des outils à éclats (Clactonien-Levalloisien-Moustérien) et la tradition des outils à bifaces (Abbevillien-Acheuléen-Micoquien). La présence d'industries lithiques mixtes indique qu'elles ont fusionné par moment. Difficilement conciliable avec la

conception du foisonnement d'une multitude de peuplades responsables de ces vestiges industriels, ce dernier schème lithique — à moins de stipuler qu'à la source s'y dissimulent deux groupes ethniques ou types morphologiques — s'avère découplé de la réalité biologique.

Simultanément, avec le développement de cette conception du parallélisme des périodes industrielles, les préhistoriens exploitent plus systématiquement certaines régions de l'Ancien Monde. Le panorama lithique, établi essentiellement à partir des données européennes, voit sa prétention universelle contestée. Il sert pourtant de référence pour établir de nouvelles séquences lithiques. Toutefois, l'arrivée massive de nouvelles données empiriques en un court laps de temps entraîne des problèmes de synthèse. L'examen de ce processus de synthèse est instructif pour comprendre les difficultés qu'eurent les préhistoriens à donner un sens ethnographique au matériel lithique. Procédons donc à présent à l'examen rapide de l'établissement de quelques séquences d'Afrique et d'Asie.

Les industries hors d'Europe

Breuil (1930) trouve la séquence sud-africaine conforme à celle d'Europe en reconnaissant ce qu'il nomme des "civilisations familières", telles que le Chelléen, le Clactonien, l'Acheuléen, le Levalloisien et le Moustérien. En revanche, les travaux de Goodwin (1926) et de Goodwin et Van Riet Lowe (1929) en Afrique du Sud se concluent par une interprétation autre. Ils proposent une terminologie typologique tout à fait différente, avec des industries nommées Stellenbosch, Victoria West, Fauresmith pour le *Early Stone Age*, Still Bay et Glen Grey pour le *Middle Stone Age*, enfin Smithfield et Wilton pour le *Late Stone Age*. Certaines de ces industries sont dérivées d'industries nord-africaines, d'autres sont jugées comme étant le produit d'innovations locales.

En Afrique de l'Est, Leakey (1931) propose une séquence lithique comprenant des industries dont certaines sont jugées semblables à celles

d'Europe, d'autres à celles d'Afrique du Sud et enfin des dernières comme étant des productions indigènes. Il est important de noter toutefois que la ressemblance des industries est-africaines avec celles d'autres régions de l'Ancien Monde n'implique pas nécessairement, dans l'esprit de Leakey, que ces industries soient contemporaines, ou encore, qu'il y ait eu contact entre elles : "Quand ils décrivent les cultures et les industries trouvées hors des régions classiques d'Europe centrale et occidentale, de nombreux archéologues tendent à utiliser une terminologie nouvelle qui leur est propre [...] Ils soutiennent que la terminologie originale ne devrait être employée que si la contemporanéité et la similitude peuvent être démontrées [...] Je ne suis pas disposé à adhérer à cela car il serait absurde de dire que tous les outils chelléens sont contemporains [...] C'est pourquoi je préfère considérer que les mots Chelléen, Acheuléen, Moustérien, Aurignacien, etc., désignent une phase culturelle très définie au sein de laquelle il peut y avoir des variations de détail en différents endroits [...] et leur appliquer des noms nouveaux seulement s'ils sont si différents de ceux déjà nommés qu'on ne peut les considérer comme de simples variations locales" (Leakey 1931 : 27-28).

Pour Choukoutien, en Chine, les préhistoriens s'entendent sur l'originalité absolue de la séquence chinoise : séquence qui se veut uniforme au cours du temps, et dont la composition typologique de nature mixte se rapproche en même temps d'industries européennes des Paléolithiques ancien, moyen et supérieur. Breuil classe cette industrie dans une catégorie à part : "l'industrie choukouténienne" (Breuil, 1935 : 744). Teilhard de Chardin et Pei (1932 : 354) écrivent à son sujet : "l'industrie de Choukoutien, par son association de choppers et d'une industrie presque microlithique, représente un type culturel très caractéristique, unique en Chine jusqu'à maintenant."

Enfin, à Java, Movius (1948) reconnaît d'abord une industrie ancienne nommée Patjitanien se composant à la fois d'éclats et de bifaces, contrairement à la polarisation typologique proposée en Europe, et une

industrie plus récente dite rudimentaire nommée Ngandong et composée de pierres et d'os, quoique distincte des industries du Paléolithique supérieur d'Europe. Sur cette base, Movius classe les industries du Paléolithique ancien d'Asie comme faisant partie d'un complexe industriel dit de choppers (*chopper-chopping-tool*) en opposition au complexe dit de bifaces (*hand-axe*) des industries de la même période d'Europe, d'Afrique et du Proche Orient. Ce complexe industriel asiatique est divisé de nouveau en quatre zones culturelles présentant des spécificités locales, telles celles rencontrées à Java et en Chine.

Ce bref survol de l'élaboration de quelques séquences lithiques à travers l'ancien monde nous révèle la difficulté à identifier les populations humaines derrière le matériel archéologique. Alors que Breuil conçoit la séquence sud-africaine comme étant essentiellement conforme à celle d'Europe, Goodwin et Van Riet Lowe y reconnaissent quelque chose de fort différent. La séquence est-africaine proposée par Leakey a ceci de particulier que, même si les noms de ces industries sont tirés des séquences européenne et sud-africaine, cette similitude terminologique n'implique pas nécessairement qu'il y ait équivalence chronologique ou contact entre les cultures de même nom. De plus, les variations typologiques de nature géographique de moindre importance ne sont pas traduites par une terminologie distincte. Les séquences lithiques asiatiques sont distinguées de façon non équivoque de celles du reste de l'Ancien Monde : Movius y reconnaît un nouveau complexe industriel présentant des spécificités typologiques régionales. Avant même d'associer une culture à un groupe humain, les préhistoriens doivent, entre eux, faire la synthèse du nouveau matériel archéologique, éviter de distinguer de fausses industries dans du matériel similaire en créant de nouvelles terminologies et d'englober des industries différentes sous des termes identiques. Cette étape décisive, qui ne peut être que le fruit d'un consensus disciplinaire, n'en est qu'à ses premiers balbutiements en 1950. A ce propos, Daniel fait ce commentaire : "L'accroissement considérable de l'information sur le Paléolithique partout

dans le monde a conduit à d'importants problèmes de nomenclature et d'archéologie comparée. Il n'y a pas encore d'accord général sur les corrélations des séquences culturelles les unes avec les autres" (Daniel, 1950 : 284). A ces problèmes de classification viennent s'ajouter les difficultés de corrélation chronologique entre les séquences lithiques d'une même région géographique et entre des régions séparées parfois par des distances considérables.

L'industrie, un domaine en soi

Une des caractéristiques du discours du préhistorien de la première moitié du XXe siècle est que le matériel archéologique se présente souvent comme se suffisant en soi. La transformation des industries au cours du temps est présentée comme celle d'un organisme en évolution, hors du fait qu'elle est en réalité le résultat du changement des groupes humains. La façon dont s'exprime Garrod en donne un bon exemple : "Nous voyons le Châtelperronien [...] qui émerge [...] il envoie deux branches, l'une [..] qui influence l'évolution du Capsien, l'autre [..] qui se développe en Gravettien" (Garrod, 1938 : 24). Les préhistoriens traitent le matériel archéologique comme s'il possédait sa logique propre, indépendamment des producteurs. Sackett juge que dans l'esprit des préhistoriens modernes : "souvent, les outils de pierre semblent posséder une vie qui leur est propre, au sens où l'évolution de la technologie lithique présente une sorte de logique interne dont les modalités lui sont intrinsèques, et peuvent, dans une large mesure, être étudiés sans se référencer directement au substrat culturel dans lequel ils étaient utilisés" (Sackett, 1981 : 98).

A partir de 1925, certains préhistoriens abordent le matériel archéologique dans une nouvelle perspective. A la définition des séquences lithiques selon un simple découpage spatio-temporel dérivé d'une approche géologique et paléontologique, Childe (1925) préfère un concept de culture archéologique basé sur les caractéristiques individuelles

des artefacts. A la reconnaissance de stades au sein d'une séquence culturelle se substitue la tentative de retrouver trace d'une multitude de peuples (Trigger 1989 : 163-173). Selon Daniel et Renfrew : "dans les années trente et quarante, la Préhistoire a commencé à faire comme si elle voulait se consacrer à retracer les petits mouvements culturels et invasions [...] non [en recherchant] la marque de l'expansion d'une race mère, mais en enregistrant celles de centaines de cultures, chacune se distribuant de façon variée dans le temps et l'espace (Daniel et Renfrew, 1988 : 77-78). Dans cette perspective, les représentations d'une Europe préhistorique caractérisée par la persistance de deux grandes traditions lithiques parallèles, ainsi que la division de l'Ancien Monde en deux complexes industriels distincts sont mises en doute (Daniel 1950 : 285-286). La conception d'une préhistoire composée d'une mosaïque de cultures implique qu'elles doivent être définies sur la base des artefacts les composant (Trigger 1978 : 75). Dans *The Danube in Prehistory*, Childe donne la définition de telles cultures archéologiques : "Nous découvrons certains types de vestiges — poteries, outils, ornements, rites funéraires et formes de maisons — se retrouvant constamment ensemble. Nous appellerons "groupe culturel" ou simplement "culture" un tel ensemble d'éléments associés. Nous admettons qu'un tel complexe est l'expression matérielle de ce qu'aujourd'hui on appellerait un peuple. Ce n'est que là où l'ensemble en question est associé régulièrement et exclusivement aux restes squelettiques d'un type physique spécifique que nous remplacerions "peuple" par le mot "race" (Childe, 1929 : V-VI).

La récurrence de l'association constante de plusieurs artefacts devient le moyen d'identifier cette mosaïque culturelle. Une fois identifiés, ces groupes, dits culturels, sont qualifiés de race lorsqu'ils sont exclusivement et régulièrement associés à un même type physique ou morphologique. Mais Childe doute de cette éventualité : "Ce n'est que tout à fait exceptionnellement que les restes squelettiques associés à une culture donnée appartiennent exclusivement, ou même de façon prédominante, à

un seul type physique [...] C'est alors un peuple auquel doit correspondre la culture définie par l'archéologue. Si ethnique est l'adjectif correspondant à cette population, nous pouvons dire que l'archéologie préhistorique a un bon espoir d'établir une histoire ethnique de l'Europe, tandis que celle de l'histoire raciale semble désespérément lointaine » (Childe, 1933 : 198-199).

Childe abandonne le lien entre la morphologie et la culture en insistant sur son aspect exceptionnel, à la faveur de l'observation d'associations fréquentes d'une même culture avec des types humains différents ou de la possession de cultures différentes par les mêmes hommes fossiles. Loin de révéler l'existence d'un type physique particulier, l'industrie nous indique un groupe culturel ou ethnique, mais non un type morphologique ou racial, sauf en de rares exceptions.

Une rupture épistémologique claire s'instaure alors dans l'esprit de plusieurs préhistoriens et archéologues (nous avons vu que cette rupture était déjà effective de façon implicite) entre le culturel et le morphologique. Le lien de cette dualité étant brisé, les contours disciplinaires sont précisés. Issue d'une approche géologique et paléontologique - du moins en France et en Angleterre - l'archéologie préhistorique est saisie, lors du deuxième tiers de ce siècle, d'une réflexion sur les contributions respectives des sciences naturelles et sociales dans son développement. Childe nous en indique clairement la teneur. Dans les années 1940, loin d'être parfaitement intégrée en préhistoire, cette réflexion est jugée par Garrod comme appliquée au Néolithique et au Paléolithique de façon inégale, cette dernière période étant davantage redevable aux sciences naturelles. Garrod insiste sur cette situation liée au Paléolithique en écrivant que : "[...] certains résultats ne sont peut-être pas aussi heureux que cela [...] car les sciences naturelles ne devraient pas monopoliser un domaine qui ne leur appartient pas strictement [...] [nous] devons faire la distinction, entre les différentes disciplines impliquées, et appliquer aux aspects variés du sujet les méthodes propres à chacun" (Garrod, 1946 : 7, 10). Cette réflexion sur l'interface entre la définition archéologique et ethnologique de la culture

est poussée beaucoup plus loin au cours des années 1950 et 1960 par des chercheurs tels que François Bordes et Lewis Binford.

Les préhistoriens se sont écartés très tôt de la démarche de Gabriel et Adrien de Mortillet qui liait inextricablement la culture matérielle et la morphologie humaine. La préhistoire a, épistémologiquement parlant, divergé vers des méthodes et concepts dont l'objectif était de comprendre et traiter le matériel archéologique, et lui seul.

Paléoanthropologie et Archéologie : un couple durable et difficile

Avec l'arrivée du XXe siècle, de nombreuses associations directes d'outils et de restes humains sont découvertes. Si beaucoup de préhistoriens restent discrets sur l'Homme fossile, les textes et traités de paléontologie humaine sont, en revanche, souvent accompagnés de considérations archéologiques. Plusieurs paléontologues et anthropologues insistent sur le lien entre morphologie et culture. Des résultats plus ou moins heureux accompagnent cet exercice.

La persistance de cette association, dans le cadre de l'analyse de la phylogenèse humaine, s'exprime de plusieurs façons. L'une d'elle consiste à accorder les connaissances en archéologie préhistorique avec l'interprétation phylogénétique. Par exemple, Hrdlicka (1927) propose un schème phylogénétique où le type de l'Homme de Néandertal est à la source de toutes les formes humaines modernes via les types fossiles d'*Homo sapiens* (Cro-Magnon, etc.) (1). Ce scénario, dit de la phase néandertalienne de l'évolution de l'Homme, implique un gradualisme morphologique dans le temps, sans rupture ou spéciation aucune. La séquence généalogique est corroborée par l'affirmation de l'existence d'une succession lithique également graduelle et sans rupture entre l'industrie moustérienne, associée à l'Homme de Néandertal, et les industries antérieure et postérieure à celle-ci. Ainsi, selon Hrdlicka : "Plus on connaît les stades initiaux et finaux de l'industrie moustérienne, de même que

l'Acheuléen tardif et le début de l'Aurignacien, moins tranchées apparaissent leurs différences" (Hrdlicka, 1927 : 259). L'industrie moustérienne se trouve intégrée dans une linéarité tout comme le type morphologique qui y est associé. Au contraire, pour Boule (1911-1913), le type néandertalien représente une espèce distincte, nommée *Homo neanderthalensis*, se plaçant en rupture par rapport aux formes fossiles récentes d'*Homo sapiens* (Grimaldi, Cro-Magnon, Chancelade) à la source de l'Homme moderne. La discontinuité dans cette phylogénie, correspondant à une spéciation au sein d'un continuum d'évolution morphologique, est corrélée avec une rupture analogue de la séquence lithique, entre les industries associées aux formes fossiles d'*Homo sapiens* (Aurignacien, Solutréen et Magdalénien) d'une part et, d'autre part, celle de l'Homme de Néandertal (Moustérien). D'après Boule : "l'Age du Renne présente un ensemble de caractères qui lui impriment une grande unité et qui marquent un progrès considérable sur le monde moustérien. C'est qu'il y a aussi un véritable contraste entre les Hommes de l'ancien et du nouveau Paléolithique" (Boule, 1921 : 247-248). La défense de telles harmonisations entre des schèmes phylogénétiques et lithiques implique le maintien de liens étroits entre l'évolution morphologique et culturelle de l'Homme. Ce lien épistémologique n'est pas sans nous rappeler l'approche préconisée par Gabriel et Adrien de Mortillet.

L'affirmation de l'association entre morphologie et culture est également illustrée par les anticipations qu'elle autorise. La découverte en place d'une association entre un certain type physique et une industrie particulière dans un site donné fait figure de règle générale : elle permet de prévoir la découverte du même type humain en un autre lieu où l'industrie correspondante serait trouvée. Ainsi, pour Keith : "[...] on n'a pas encore découvert d'ossements de Néandertal en Angleterre, mais les vestiges de sa culture lithique sont abondants ; nous pouvons bien nous attendre à ce que ses os fossilisés soient découverts un jour" (Keith, 1948 : 264). La position de Leakey concernant la relation entre l'Homme fossile et

les industries lithiques en donne une autre illustration intéressante. D'abord, Leakey est très critique à l'endroit de la terminologie typologique, spécialement lorsqu'elle concerne l'association entre l'Homme de Néandertal et l'industrie moustérienne. Il la conserve, cependant, mais à un niveau plus fin de résolution, en associant à des industries lithiques mieux définies des groupes distincts de Néandertaliens : "Les différents restes [de Néandertaliens] ne sont en aucune façon trouvés toujours en association avec une "culture moustérienne" sauf au sens abusif le plus large et le plus vague du terme. Certains d'entre eux sont associés à une pure culture Levallois, d'autres à une pure culture moustérienne, et d'autres encore à une culture de contact moustérienne-levalloisienne [..] Selon quelques indices, ces variantes [de Néandertaliens] peuvent être corrélées avec des différences de cultures" (Leakey, 1934 : 200).

Cette conception est encore plus évidente chez Leakey quand il déduit que l'artisan de l'Acheuléen en Europe est de type *Homo sapiens*, et cela sur la base de l'existence d'une association entre un spécimen d'*Homo sapiens* et l'industrie chelléenne en Afrique de l'Est, et du fait que l'Acheuléen dérive du Chelléen (Leakey, 1934 : 133) ! La conviction de l'association morphologie et culture en certains cas, comme entre Néandertal et Moustérien, est très répandue au cours de la première moitié du XXe siècle. Elle n'était pas fausse, bien des données l'attestaient, mais n'avait pas, cependant, la généralité qu'on voulait lui attribuer. Nombre d'outils avaient été découverts sans restes humains, et inversement. Aussi, l'éventualité de découvrir des Néandertaliens avec des industries du Paléolithique supérieur ou des formes modernes d'*Homo sapiens* en présence d'outils moustériens subsistait, ce qui fut d'ailleurs le cas plus tard. La pratique fréquente d'associer industrie et forme humaine vient moins d'une façon de penser que de l'observation réelle de ce lien en certains cas. Cette observation autorisait d'anticiper sur la généralité du phénomène. De nature épistémologique différente, l'association morphologie-culture chez Gabriel et Adrien de Mortillet impliquait que la

découverte d'une nouvelle industrie devait mener à la découverte éventuelle d'une nouvelle forme humaine, et réciproquement.

La certitude chez les paléontologues et anthropologues de cette association rigide va très tôt rencontrer des difficultés. Si les Néandertaliens sont seuls associés à l'industrie moustérienne, comment expliquer que les formes d'*Homo sapiens*, telles Grimaldi, Cro-Magnon et Chancelade, entre autres, qu'on distinguait souvent clairement, puissent partager les mêmes industries du Paléolithique supérieur ? Chacune des trois industries généralement reconnues au Paléolithique supérieur (Aurignacien, Solutréen et Magdalénien) n'étaient pas spécifiquement associées à l'une ou l'autre de ces formes humaines. Cela impliquait que la même industrie était partagée par des types humains distincts ou que chacun possédait plusieurs industries, contredisant en cela la règle de l'association entre les caractéristiques de la culture et celles de l'aspect somatique de ses auteurs. Cette contradiction est généralement passée sous silence. Boule, conscient de cette difficulté, fait la proposition suivante : "Chacun de ces types [Grimaldi, Cro-Magnon, et Chancelade] a bien sa physionomie propre, mais simplement au même titre que les diverses races d'aujourd'hui [...] Nous sommes arrivés à un moment à partir duquel l'évolution physique de l'Humanité peut être considérée comme terminée ; le problème des origines humaines perd son caractère zoologique pour devenir purement anthropologique et ethnologique" (Boule, 1921 : 248).

En d'autres termes, quand il existe une distinction dans la morphologie d'ordre spécifique (distinction entre espèces), l'association reste valable. Lorsque cette distinction se produit au sein de la même espèce, elle ne le serait plus. Comment Boule peut-il défendre une telle position ? Les variantes morphologiques au sein d'une même espèce auraient-elles un statut différent de celles qui caractérisent des espèces distinctes ? Boule évacue le problème en prenant un chemin détourné : il met en doute la valeur universelle de la définition même des industries

du Paléolithique supérieur, au profit d'une conception ethnographique de ces dernières. Il soutient ainsi : "Ces subdivisions [Aurignacien, Solutréen, et Magdalénien] ne sauraient avoir le caractère général qu'ils [les préhistoriens] veulent leur attribuer. Tout paraît démontrer, au contraire, que les gisements de chaque pays, et même de chaque province, nous présentent, avec un fond archéologique commun, tels et tels caractères ethnographiques particuliers, qui ne sont que des *faciès* locaux" (Boule, *ibid.* : 252).

Boule ne fait que déplacer la question au Paléolithique ancien et moyen. Comment pouvait-il, sans rupture épistémologique avec la conception des industries de Paléolithique supérieur, refuser d'appliquer au Moustérien cette conception ethnographique du matériel archéologique ? La question demeure sans réponse évidente.

Pour donner un autre exemple de la difficulté de maintenir un lien étroit entre un certain type morphologique et une industrie particulière, tournons-nous vers l'harmonisation des schèmes généalogiques avec l'archéologie préhistorique. Nous avons déjà vu que Boule et Hrdlicka avaient accordé la séquence des industries et leurs schémas généalogiques respectifs à propos de la relation entre *Homo neanderthalensis* et *Homo sapiens*, ce qui permettait ainsi un renforcement mutuel des données paléontologiques et lithiques. Mais le passage à des schèmes généalogiques plus généraux fait apparaître plus nettement les difficultés d'harmonisation. Klaatsch (1923) propose l'hypothèse selon laquelle l'ensemble des races humaines actuelles dérive de deux sources principales : la souche commune du gorille et des Néandertaliens menant aux variétés humaines actuelles africaines, et la souche commune de l'orang-outan et des aurignaciens (*Homo sapiens* fossiles) menant aux variétés humaines actuelles non africaines. Reconnaissant que la séquence archéologique définie à l'époque possédait un caractère progressif indéniable, Klaatsch attribue la section la moins raffinée de cette séquence à la souche néandertalienne (Chelléen, Acheuléen et Moustérien), et la section la plus

raffinée et récente à la souche aurignacienne (Aurignacien, Solutréen et Magdalénien). Enfin, Hooton (1946) présente un arbre généalogique se caractérisant par une multitude de branches évoluant vers l'Homme moderne et de façon plus ou moins parallèle, tout en s'entrecroisant ou s'hybridant. Un certain nombre d'hommes fossiles sont rattachés aux mêmes souches que certaines formes humaines actuelles : *Pithecanthropus* est lié aux Australiens, l'Homme de Broken Hill aux Boschimans, *Sinanthropus* aux Néandertaliens. Un groupe de Néandertaliens s'hybride avec la souche menant à la race dite "blanche" (Piltdown, Swanscombe, Galley Hill, Combe Capelle) via les fossiles de Mont Carmel, alors qu'un autre groupe est voué à l'extinction. Au fil de certaines considérations archéologiques, Hooton qualifie la séquence lithique proposée par Leakey en Afrique de l'Est de "typologie bizarre" parce que, entre autres, l'Aurignacien et le Moustérien se retrouvent côte à côte, et non disposés successivement, comme cela apparaît parfois dans la séquence européenne à l'époque : "les séquences archéologiques postulées par le Dr Leakey et ses collègues semblent très bizarres. L'Aurignacien est contemporain du Moustérien selon ses trouvailles" (Hooton, *ibid.* : 392). Ce commentaire surprend toutefois puisque la succession généalogique complexe de Hooton s'accommode beaucoup mieux d'une séquence lithique buissonnante que linéaire.

Conclusion :

Le programme de la palethnologie proposé par Gabriel et Adrien de Mortillet unifiant le biologique et le culturel dans l'étude du passé évolutif de l'Homme était justifié d'un point de vue théorique. Cependant, la conviction de la nature dualiste de l'Homme et de la nécessité de l'appréhender globalement ne constitue pas une raison suffisante pour postuler, *a priori*, qu'il y a une corrélation entre caractères physiques et traits culturels. La science doit souvent se soumettre plus aux réalités

observables qu'aux déductions théoriques. Dès lors, dans le domaine abordé ici, la paléontologie humaine et l'archéologie préhistorique se sont, épistémologiquement parlant, découplées. D'ailleurs, au cours de la première moitié du XXe siècle, de nombreux préhistoriens et archéologues ont reconnu ou entretenu, implicitement ou explicitement, cette rupture épistémologique et disciplinaire. A l'inverse, les textes et traités de paléontologie humaine ont souvent maintenu, non sans difficulté, la force de la corrélation entre la morphologie de l'Homme et sa culture. Nous en avons détaillé des exemples. Il semble qu'une certaine méconnaissance de l'archéologie préhistorique et de ses développements au cours du XXe siècle par les chercheurs concernés par la phylogénie de l'Homme soit en partie à l'origine de ces difficultés. Il est possible que l'abondance du matériel lithique, en comparaison de la pauvreté relative des restes humains fossiles, rendait le couple morphologie-culture séduisant pour la construction d'arbres phylogénétiques, comme si le matériel lithique était une extension empirique du matériel osseux qui faisait parfois cruellement défaut.

En apportant cette contribution à une analyse de la relation entre l'archéologie préhistorique et la paléontologie humaine au cours de la première moitié du XXe siècle, nous n'avons pas soutenu que les deux disciplines n'avaient rien en commun. Elles ont connu, bien au contraire, une croissance plus ou moins parallèle, avec des moments de chassés-croisés. Par exemple, elles partagent certains impératifs épistémologiques et des modalités de développement analogues : acquisition de données empiriques au cours d'un long processus historique de découvertes ; sources de base limitées géographiquement et temporellement à l'Europe d'abord, avant de s'étendre à l'échelle mondiale ; problèmes voisins de synthèse dans l'intégration de toutes les données factuelles. Malgré ce parallélisme, l'harmonisation entre les chaînes des changements somatique et lithique, ne peut se faire sur la base d'une concordance étroite entre l'évolution morphologique et celle de l'industrie. Ceci ne signifie pas

qu'on ne puisse fréquemment établir leur association. Cependant, la base épistémologique ne peut se fonder sur une équation morphologie-culture *a priori*, mais plutôt sur une suite de découvertes réelles la démontrant. Pourtant, cette procédure simple se heurte à de nombreuses difficultés. Suivant leur nature, le rythme des découvertes est dissemblable, et on trouve beaucoup plus d'outils que de restes osseux. De plus, les uns et les autres ne sont pas toujours en association directe, et la connaissance des deux chaînes progresse différemment.

La nature même des matériels en cause, outillage ou ossements humains, impose des méthodologies et des concepts divergents. Si la classification est la première étape commune aux démarches du préhistorien et du paléoanthropologue, la reconnaissance et l'analyse des entités diffèrent : la définition de "populations", "races" ou "espèces" n'a pas de correspondance avec la définition d'"outils", d'"assemblages" ou d'"industries" : elles naissent de débats et de consensus relevant de disciplines séparées.

(1) Note :

A l'époque, on considérait l'Homme de Néandertal et ses successeurs du Paléolithique supérieur comme des espèces différentes : *Homo neanderthalensis* et *Homo sapiens*. Aujourd'hui, tous les deux sont des *sapiens*, séparés au niveau subspécifique (*Homo sapiens neanderthalensis* et *Homo sapiens sapiens*).

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Chapter 4

Human Palaeontology and Prehistoric Archaeology 1900-1950: Some Aspects of the Problem of Integration in Science

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Introduction

The interrelated nature of the biological and cultural aspects of humans has long been recognized in the Western tradition. When addressed in an evolutionary perspective, this duality has sometimes been presented under the form of evolutionary scenarios of hominization.

Numerous scenarios of hominization suggested during the nineteenth and twentieth centuries (e.g., Stoczkowski, 1994) have proposed feedback systems which involved humans' morphological transformation and reliance on material culture (tools). Darwin's (1871:140-151) scenario explaining the tranformation of a quadrupedal primate living in the trees into a bipedal, intelligent and culture-bound human is probably the best known of all. His scenario involved causally linked elements such as bipedality, freed hands, manufacture of tools, reduction of prognathism and teeth, increased encephalization and mental faculties.

Clearly then, human morphological and cultural aspects have long been fused into a single framework of analysis when one considers scenario-building. This latter practice reflects the conviction that this duality characterizes humankind, and that it has been a key factor in human evolutionary history, at one period or another.

On the other hand, the biological and cultural aspects of human evolution have traditionally been studied by two distinct disciplines since the 1860s: human palaeontology which is concerned with fossil remains and prehistoric archaeology with cultural vestiges.

This situation is a clear indication that, although the biology/culture link is an important part of the study of human evolution that can be expressed through scenario-building, important epistemological problems are raised by the integration of data generated by human palaeontology and prehistoric archaeology. This paper analyses how these two disciplines were related during the first half of the twentieth century. This historical analysis may be of interest especially when evaluated against proposed models of integration in science as conceived by philosophers of science.

The most popular model of integration in science during the twentieth century has been one called theory reduction (e.g., Nagel, 1979). As its name implies, scientific fields or disciplines were often equated each with specific

theories, which in turn were thought to be, in theory, all reducible to a single over-arching theory encompassing them all. Within this perspective of science, unification or fusion of fields was considered to be a sign of progress. It is now realized that a scientific field is much more than a theory, but is composed of key elements such as: a central problem, facts related to this problem, explanations and goals as to how the problem is to be solved, a special vocabulary, techniques, methods and, sometimes, concepts, laws and theories (Darden and Maull, 1977:44).

From this, it is clear that the reduction of one field to another, or their fusion, does not necessarily constitute progress, but may also entail the risk of a loss of information (Burian, 1993:314; Gusdorf, 1977:595-598; Van Der Steen, 1993:262-263). Integration in science should be understood as something that varies between pseudo-integration, peripheral integration and substantive integration (Van Der Steen, 1993). Consequently, several models of integration not entailing reduction or fusion of fields are now being uncovered by philosophers of science (Bechtel, 1989, 1993; Darden and Maull, 1977; Maull, 1977; Van Der Steen, 1993).

Let us now turn to the analysis of the relations of human palaeontology and prehistoric archaeology between 1900 and 1950, while keeping these models of integration in mind. Further reflections on integration in science and the nature

of the documentation of the biology/culture link in studies of human evolution are postponed until the end section of this paper.

The unified analysis of man's past

In the third edition of *Le Préhistorique* (1900), Gabriel and Adrien de Mortillet made it clear that morphological and cultural aspects in prehistory should be investigated within the single discipline of palaeo-ethnology: "La palethnologie, comme son nom l'indique, [s'occupe] essentiellement de l'ethnologie la plus ancienne, étudiant par conséquent l'homme primitif, son développment physique, moral et industriel..." (1900:230). What characterizes the de Mortillets' palaeo-ethnology is the relatively tight association of human morphological types with specific cultures.

For instance, they instituted the following associations: During the Quaternary period, the morphological evolution of the Neandertal race is correlated with the cultural transformation of the following epochs or industries: Chellean, Acheulean and Mousterian. Assuming the evolution of the Neandertal race into the Laugerie race at the end of the Quaternary, Gabriel and Adrien de Mortillet then associated the Laugerie race with the Solutrean, Magdalenian and Tourassian industries.

De Mortillets' conviction that a particular morphological type was associated with a distinct cultural production, and conversely, is clearly seen when they named the two hypothetical species *Homosimius bourgeoisii* and *Homosimius ramesii* on the basis only of a distinction recognized between two eolithic cultures. It will be remembered that eoliths were, at the time, controversial cultural products of great antiquity and crude appearance that were later discarded.

According to Gabriel and Adrien de Mortillet, prehistoric times should ideally be based on the morphological transformation of humans themselves. However, they held the view that the gradual nature of morphological evolution prevented them from doing so. Cultural products were, they thought, more convenient because abundant and less gradual. Commenting on this discrepancy, they contended that the major cultural distinctions were, nevertheless, correlated with major morphological stages:

> "Le paléolithique ne peut, au point de vue des types humains, se diviser qu'en deux grandes coupes... [qui] correspondent tout à fait aux deux grandes divisions de l'industrie. Pendant le paléolithique inférieur et moyen... [Neandertal race and associated cultures] emploi exclusif de la pierre. Pendant le paléolithique supérieur... [Laugerie race and associated cultures] adjonction des instruments en os. La classification par l'homme, si elle n'est suffisante par elle-même, vient du moins pleinement confirmer la classification par l'industrie (1900:346-347)".

The discipline of palaeo-ethnology has an important epistemological implication for the process of knowledge acquisition in human evolution: man's cultural and morphological evolution is interrelated by programmatic decision: roughly, there is a strong correlation between a morphological type(s) and a cultural level(s).

However, we will see how some developments in prehistoric archaeology during the first half of the twentieth century uncoupled, on an epistemological basis, the search for human's cultural and morphological past.

Prehistoric archaeology as an autonomous discipline

The relatively tight association which was thought to exist in the mind of some scientists between man's morphological and cultural evolution was disturbed, implicitly or explicitly, by a number of prehistorians and archaeologists. For instance, Breuil (1912:169) contended in the early decades of the twentieth century that the cultural sequence found in Western Europe could not be explained only by a single population inhabiting and evolving there, but rather by the migrations and interactions of numerous ones. In this perspective, the archaeological investigation, if not completely uncoupled, is not directly related to the morphological or ethnographic one. The documentation of an archaeological sequence in a geographic area does not

correspond to the investigation of the morphological type or types responsible for this cultural production.

Breuil's main thrust in his 1912 paper concerns a re-valuation of the current cultural epochs of the Upper Palaeolithic of Europe in use at the time. He promoted a more precise and detailed archaeological framework for the Upper Palaeolithic of Europe (c.f., Groenen, 1994:162-195). Although it might have been produced by a number of migrating populations, this archaeological sequence presents itself as a linear succession of eleven industrial periods from the ancient Aurignacian to the recent Azilian. Each of these industrial periods was defined by synthesizing information gathered from several archaeological sites or localities (Groenen, 1994:184), rendering the possibility of identifying populations or ethnological groups most difficult because such a procedure blurs any local specificities. This was a standard procedure in prehistoric archaeology at the time.

Another development in prehistoric archaeology at the time relates to the parallel phyla concept. Prehistorians, and most notably the French ones, cultivated a special devotion to the empirical content of the archaeological record, having for their main goal the construction of schemes of time-space systematics (Sackett, 1981:87). This intensive empirical research eventually led to the recognition that some typological industries or cultures were not necessarily lineally and chronologically disposed, but

instead were contemporaneous. For instance, Garrod (1928) suggested that the Middle Aurignacian might not be an intermediate stage between the Lower and Upper Aurignacian, but rather a separate culture originating in Palestine. Peyrony's (1933, 1936) studies in the 1930s argued in favour of the parallel evolution of Aurignacian and Perigordian industries. These had previously been lineally disposed. Hence, a multilinear conception of prehistoric artifact assemblages developed during the first half of the twentieth century. The parallel phyla concept was clearly exemplified when Breuil (1932) presented his influential archaeological framework of the Lower and Middle Palaeolithic in Western Europe. He proposed the transformation of a flake-tool tradition (Clactonian-Levalloisian-Mousterian) in parallel with, and more or less independently of, the core-biface tradition (Abbevillian-Acheulean-Micoquian). These two traditions were eventually to fuse, at least to a certain extent, in some industries which contained both flake and core tools.

This general scheme applying to broad and long lasting traditions in Western Europe could not easily be reconciled with the investigation of human populations behind the material. Under the recognition of these archaeological traditions, the precise ethnographic distinctions are blurred, unless it is postulated that they were intimately

associated with two widespread human groups that were eventually to meet and fuse their lithic traditions.

Developing simultaneously with the parallel phyla concept during the early decades of the twentieth century, the concepts of diffusion and migration to explain change in lithic sequences were superseding that of evolutionism (gradual change) in prehistoric archaeology. Their acceptance was concomitant with the development of an Old World strategy of research.

This broader perspective on prehistory created some difficulties. According to Grahame Clark (1953:347), as "results began to come in from different parts of Africa and Asia..., it became obvious that the new material would not fit the classic sequence". Europe, and especially Western Europe, represented only a local sequence of no universal value, although it still constitutes today a useful referential framework to investigate the rest of the Old World. By the end of the 1930s, Old World prehistory was something different from a simple succession of epochs; it started to look like the product of complex historical developments (Grahame Clark, 1953:349). This process of empirical collection and then synthesis of Old World archaeological material required the establishment of guidelines among archaeologists. Garrod (1938:2) suggested that the three main typological groups thus far recognized (hand-axe, flake and blade cultures) be retained, with the

addition of names necessary for the many variations found within these groups. She warned against the tendency to coin new labels for industries that are essentially similar, even though they are found in quite distinct areas. Such a practice obscured the possibility of identifying the spread of cultures.

This synthesizing process is of crucial importance for the determination of human populations behind the archaeological material. A broad application of European typological categories in the rest of the Old World might blur cultural distinctions from other areas, while an overemphasis on local differences might obscure links between these areas. A short survey of some areas documented in Africa and Asia between 1920 and 1950 might be indicative of how this synthesizing process proceeded.

Breuil (1930) concluded that the archaeological sequence of South Africa fitted into the major lines of the Stone Age of Europe, India, the Middle East and North Africa. He recognized what he called "familiar civilizations" such as the Chellean, Clactonian, Acheulean, Micoquian, Levalloisian and Mousterian (Gowlett, 1990:19). However, Goodwin's intensive surveys in South Africa during the 1920s had resulted in something distinct from the European sequence. In an avowed effort to break away from the preconceived pattern, such as the French one, Goodwin (1958:25) wrote that as "a result of combined field-work and museum investigation

I at once appreciated the absolute necessity for evolving an entirely new cultural terminology for southern Africa".

Goodwin and Van Riet Lowe (1929) presented what was to become a lasting synthesis for the South African lithic sequence. Their Earlier Stone Age comprised three succeeding industries, the Stellenbosch, the Victoria West and the Fauresmith; the Middle Stone Age was composed of a variety of industries showing some Mousterian origins such as the Still Bay and the Glen Grey; and finally the Later Stone Age was represented by the Smithfield (A, B, C) and the Wilton (see also Goodwin, 1926). They envisioned the southern part of Africa as a cul-de-sac, the Sahara constituting an obstacle preventing the southern ("lower") cultures from moving north, while permitting the northern ("higher") cultures from moving south. Under such circumstances, major technological innovations were thought to be foreign to the region. For instance, during the Earlier Stone Age, they derived the Stellenbosch from the Lower Paleolithic of North Africa, while the Victoria West and the Fauresmith were interpreted as the fruits of local evolution. This South African archaeological synthesis aimed at constructing "a time-space framework with the expectation that migration and diffusion of people and ideas could be traced through similarities in the stone artefact assemblages" (Deacon, 1990:46). Unfortunately, this archeological model (like most of them) lacked crucial support from dating (a clear chronology) and

past climates (an important factor in determining the migration routes). Instead it was limited to a relative chronology and to general past climatic inferences provided by L.S.B. Leakey's controversial hypothesis linking northern glaciations to African pluvials (Deacon, 1990:47-48).

By 1930, an East African archaeological sequence was proposed by L.S.B. Leakey. This sequence survived with no major modifications for the next thirty-five years. Applying available names when similar industries were discovered in East Africa to those of the European and South African sequences, Leakey used new names only when no equivalent ones were known (Robertshaw, 1990:80-81). Starting from the oldest Kenyan cultures dated by the system of relative chronology provided by his pluvial/interpluvial hypothesis, Leakey (1931) presented the following sequence: the Kenya Chellean, the Kenya Acheulean, the Nanyukian, (in contemporaneity) the Kenya Mousterian and Aurignacian, (in contemporaneity) the Kenya Stillbay and late Aurignacian, the Elmenteitan, the Kenya Wilton and so on.

What was the meaning, in Leakey's mind, of finding industries in East Africa that were similar to those already named in Europe or elsewhere? It is worth quoting him at some length:

> "...many archaeologists when describing the cultures and industries found in areas other than the classic ones of Central and Western Europe tend to use a new terminology of their own... They argue that the original terminology should not be used unless contemporaneity as well as

> similarity is demonstrable... I am not prepared to subscribe to [this view since] it would be absurd to say that all Chellean implements were contemporary... I therefore prefer to regard the words Chellean, Acheulean, Mousterian, Aurignacian, etc., as denoting a very definite culture phase, within which there may be variations of detail in different areas..., and only employ new names for them when they are so different from those which have been already named that they cannot be regarded simply as local variations. In addition care should be taken that in all publications the statement is made that the culture name used does not imply any date, or even a necessity of culture contact (1931:27-28)."

Eventually, the specificity of the lithic sequences in Africa relative to those found in other regions of the world was recognized. A resolution was passed to such an effect during the first Pan-African Congress held in Nairobi in 1947, although this was not unanimously approved (see Leakey and Cole, 1952:8-9).

In Asia, the Chinese prehistoric framework was established at Zhoukoudian during the decades after 1920 (Chang, 1963). When considering the Zhoukoudian industry in relation to Western Europe, Breuil bluntly answered that no comparison was possible. Although some analogies were feasible, with some flake industries such as the Clactonian and the Tayacian of the Lower Palaeolithic, the Mousterian of the Middle Palaeolithic, several industries of the Upper Palaeolithic, or even the microlithic industries of the Mesolithic, Breuil (1935:744) preferred to classify the Zhoukoudian industry in its own category: "l'industrie choukouténienne". According to Teilhard de Chardin and Pei

(1932:354), the "Choukoutien industry, by its association of choppers and an almost microlithic industry, represents a very characteristic cultural type, so far unique in China". When comparing the Chinese industry to the European ones, Pei (1937:221-222) acknowledged the impossibility of correlating them on a typological basis, because the "characteristic types of tools upon which European prehistorians base their claims for such a great number of [Paleolithic] sub-divisions, are not yet known in China". He instead suggested a correlation between China and Europe based on geological and palaeontological considerations.

In Java, an archaeological framework was first proposed in the 1930s and 1940s (Movius, 1948). The oldest tools recognized in this region were of the Patjitanian culture, an industry composed largely of flake implements with the addition, however, of some core tools. For this reason, "it is impossible to classify the Patjitanian in terms of the core or flake traditions recognized in other regions" (Movius 1948:355). The presence of hand-axes, well-made flake implements and coarse blades in this industry, has been interpreted as signifying a more evolved industry than any other Lower Palaeolithic culture in the Far East. The attribution of the Patjitanian culture to the Lower Palaeolithic was based on typological considerations, independent of any geological results since the terrace deposits in which this industry was found "are of little help

in trying to establish the age of the Lower Palaeolithic of the Patjitan site. Nor can the question be definitely settled until an intensive physiographical survey of the region is undertaken" (Movius, 1948:352). A late Middle or early Upper Pleistocene date was suggested. Succeeding the Patjitanian culture, and dated to the Upper Pleistocene, was a completely different culture named Ngandong, and composed of stone, bone and antler implements. The stone artefacts of the Ngandong culture were characterized by crude and small flakes and blades, of which the "flaking technique is crude and degenerate in comparison with that of the classic Upper Palaeolithic cultures of Europe and the Middle East" (Movius, 1948:354).

Embracing a wider view, Movius classified the Lower Palaeolithic cultures of the Far East as being part of a chopper-chopping-tool complex, in contrast to a hand-axe complex found in Europe, Africa and the Near East. However, this Far East chopper-chopping-tool complex was further divided into four cultural areas expressing some cultural specificities, such as those already mentioned for Java and Zhoukoudian.

This short survey of the establishment of the Old World prehistoric framework between 1920 and 1950 shows the difficulties involved in dealing with this new empirical input.

Daniel (1950:284) commented that this "great accession of information about the Palaeolithic all over the world has brought with it great problems of nomenclature and comparative archaeology, and there is still no general agreement on the correlations of these cultural sequences with each other". This situation constituted an important obstacle to the identifying of the manufacturers of the industries, because even before such associations could be sought, the archaeological material needed to be compared, agreed upon, and named. Another important obstacle was the great difficulty of producing non-typological correlations among the sequences of distinct regions, owing to the lack of any reliable stratigraphical information, either locally or over great distances.

The prehistorian's quest during the first half of this century seems often to have proceeded from an internal logic not entirely applicable to the investigation of the producers, focusing instead on the evolution of the artefactual material itself. The descriptions provided by a number of prehistorians often took the narrative form pertaining to organic evolution: "we see the Chatelperronian... emerging,... it sends out two branches, one... to influence the evolution of the Capsian, the other... to develop into the Gravettian" (Garrod, 1938:24; see also Robertshaw, 1990:81 about L.S.B. Leakey). Sackett (1981:98) argued that in the minds of modern prehistorians,

"stone tools do often seem to possess a life of their own in the sense that the evolution of lithic technology displays a kind of internal logic whose patterns are intrinsic to it and can to a great extent be studied without direct reference to the cultural background in which they functioned".

However, a number of prehistorians made significant efforts to clarify the association between the products and the producers. The publication of V. Gordon Childe's *The Dawn of European Civilization* (1925) was a turning point in prehistoric archaeology. Childe maintained that cultures could not be defined simply by subdividing archaeological sequences spatially and temporally, but that they had to be defined individually on the basis of constituent artifacts. Instead of identifying stages in cultural sequences, some prehistorians were trying to define prehistoric peoples (Trigger, 1989:163-173).

As Daniel and Renfrew (1988:77-78) stated: "Prehistory began to look in the thirties and forties as though it would resolve itself into the exercise of tracing small cultural movements and invasions..., not [by searching] the record of the spread of a master race, but a record of hundreds of cultures each varying in distribution in time and space". In this perspective, Daniel (1950:285-286) had commented that the characterization of European prehistory as comprising two great civilizations (flake and core traditions) might be more a conceptual tool than an empirical reality, whereas the

distinction between a chopper-chopping-tool complex in the Far East and a hand-axe complex in the rest of the Old World might be premature, because it ran the risk of invalidating further research in the Lower Palaeolithic. The implementation of the concept that the archaeological record was a mosaic of cultures required that they had to be built up from assemblages of artefactual types (Trigger, 1978:75).

In *The Danube in Prehistory* (1929:v-vi), Childe defined an archaeological culture in the following terms:

> "We find certain types of remains - pots, implements, ornaments, burial rites and house forms - constantly recurring together. Such a complex of associated traits we shall term a 'cultural group' or just a 'culture'. We assume that such a complex is the material expression of what today would be called a 'people'. Only where the complex in question is regularly and exclusively associated with the skeletal remains of a specific physical type would we replace 'people' by the term race."

Childe's definition encapsulates with great clarity the distinction between ethnology, archaeology and morphology. The definition of an archaeological culture is an inference from artefacts of a real social grouping (a 'people') producing ideas as well as material (e.g., McNairn, 1980:50). This social grouping inferred from archaeology may only be termed a "race" when tightly associated with a specific physical type. On that last count, Childe (1933:198, 199) remained highly skeptical:

> "But only quite exceptionally do the skeletal remains associated with a given culture belong exclusively or even predominantly to a single physical type... It is, then, a people to which the culture of the archaeologist must correspond. If ethnic be the adjective for people, we may say that prehistoric archaeology has a good hope of establishing an ethnic history of Europe, while a racial one seems hopelessly remote."

Here, Childe broke the morphological/cultural association by clearly pointing at the frequent occurrence of distinct morphological types sharing the same cultural industry, while logically the reverse might also be true.

Prehistoric archaeology was more or less a product of the methods developed in geology and palaeontology during the nineteenth century, at least in France and England, but applied to artefacts (e.g., Trigger, 1989:73-147). However, the 1930s and 1940s saw some prehistorians clearly distinguishing between the role played by the natural sciences (geology, palaeontology) and by the social sciences (archaeology, ethnology, history) in the quest for man's past. Childe's clear distinction between an archaeological culture representing a social entity and a race or physical type is part of this movement. This clear distinction among scientific disciplines entailing divergent epistemological strategies of research, however, was not completely integrated by the 1940s. As Garrod complained, the prehistory of the Palaeolithic is more closely associated with the natural sciences than is that of the Neolithic, as if archaeologists were leaving behind their own standards and

points of view when considering man's remote past. On the strictly archaeological side, she continues:

> "...it has had certain results which are perhaps not quite so happy... [since] the natural sciences should not go on to monopolise a field which does not strictly belong to them... [W]e must distinguish... the separate disciplines involved, and apply to the various aspects of the subject the methods proper to each" (Garrod 1946:7, 10)."

Most developments in prehistoric archaeology presented here contributed to the forging of an epistemological and disciplinary break between the documentation of man's cultural and morphological past. Let us now turn to some developments in human palaeontology and their relation to prehistoric archaeology during the first half of the twentieth century.

Human palaeontology and the morphological/cultural link

Between 1850 and 1930 a number of human fossils were discovered. Many discoveries were directly associated with stone tools. A number of scientists concerned with human palaeontology tried to integrate knowledge of prehistoric archaeology with their genealogical or phylogenetic propositions, demonstrating thereby the existence in their minds of a morphological/cultural link. As will be seen, this approach created a number of difficulties.

Some scientists defended the view that most human fossils could be comfortably integrated within the racial history of the living varieties of humankind. This approach to human evolution was expounded largely during the second half of the nineteenth century, and it continued to be defended during the first half of the twentieth century. For Hrdlicka (1927), all the varieties of living humans are descended from a common Neandertal type, meaning that *Homo sapiens* went through a Neandertal phase. Acknowledging that the Neandertal race was associated with the Mousterian culture, Hrdlicka held that this archaeological association was not sundered from previous and succeeding cultures as a number of scientists thought (see below), but rather was in archaeological continuity, in correlation with the assumed morphological continuity of the Neandertal phase of mankind: "the more the initial and the terminal stages of the Mousterian industry are becoming known, together with the late Acheulean and the earliest Aurignacian, the less abrupt... appear their differences..." (Hrdlicka, 1927:259). Hrdlicka seems to have felt compelled to show that his phylogenetic scheme of human evolution was in harmony, or at least not in contradiction, with the archaeological sequence.

Klaatsch (1923) divided the living varieties of humankind into two main groups which he believed were more closely related to some living anthropoids than to each other. Most human fossils were related to one of these two

main stems. Postulating the existence of a common ancestral group for humans and apes, Klaatsch established a parallel between the living anthropoids and the living humans in the eastern and western parts of the Old World. In the west, with the extension of "this comparative study to the negro [of Africa], we find that amongst the latter there are some groups which may be regarded as offshoots from the common stem of the Neanderthal race and the gorilla" (Klaatsch, 1923:269). It was specified, however, that the Neandertal race was not descended from the gorilla, but rather that the common ancestor was much more human-like; this in turn, would imply that the larger amount of modification occurred on the side of the gorilla. In the east, "the Aurignac type differs most from Neanderthal human [and] it agrees most with the eastern representatives of the human race [southern Asia, Pacific Islands, Australia] and of the anthropoid apes [orang-outang]" (Klaatsch, 1923:279). The living human races of Europe were integrated within the eastern group. The Cro-Magnon and the Chancelade races were interpreted as being a mixture between the Aurignacian and Neandertal fossil races.

Accepting the general archaeological sequence defined in Europe at the time (Chellean, Acheulean, Mousterian, Aurignacian, Solutrean and Magdalenian), Klaatsch (1923:244) recognized in it a progressive trend: "When we survey these various types of culture, we see very clearly that there has been a successive improvement in the making of the

implements". The Neandertal race was here associated with the Acheulean and Mousterian cultures, while the Aurignacian race was associated with what was described as contrasting and higher level cultures such as the Aurignacian, Solutrean and Magdalenian. The cultural distinction was considered by Klaatsch to be so clear that it connoted distinct morphological types of humans, hence demonstrating a tight morphological/cultural association: "When we study the stone implements of the Ice Age in Europe we are compelled to suppose that at least two different types of men are responsible for the manufacture of them... The bones we have found in the last few decades have made these two types known to us" (Klaatsch, 1923:248).

A number of difficulties flow from Klaatsch's interpretation. First, he associated the Aurignacian race with the last three cultures of the European sequence, but he also associated the Cro-Magnon and the Chancelade races with the same cultures. If the cultural distinction between the Mousterian and the Aurignacian-Solutrean-Magdalenian cultures can bespeak the presence of at least two quite distinct morphological types, implying a strong correlation between a morphological type and a culture or group of cultures, how can the association between the three different morphological types (the Aurignacian, Cro-Magnon and Chancelade races) and the cultures they share be explained? Far from being unique to Klaatsch's conception, this association was widely held

during the first half of the twentieth century. The difficulties it generated often went unnoticed or unaddressed.

Another difficulty with Klaatsch's association between morphological types and the recognition of his so-called progressive archaeological sequence in Europe (see above) is that this sequence is in reality broken-up, in his conception, into two sequences: the Acheulean-Mousterian cultures of the Neandertal stem, and the Aurignacian-Solutrean-Magdalenian cultures of the Aurignacian stem. Then, how can this seemingly progressive archaeological sequence be explained when Klaatsch's scheme implies that such a lithic sequence is the product of two distinct and unrelated hominid evolutionary stems?

Thus far, we have seen that some scientists comfortably integrated most, if not all, human fossils at the base of, or within the history of, the living varieties of humankind. However, a number of scientists rejected this interpretation. Instead, they opined that most of these fossils were evolutionary side branches not ancestral to any living humans. For Boule, the Neandertals were representatives of a distinct species (*Homo neanderthalensis*) related to living humans (*Homo sapiens*) only through maybe a distant common ancestor. In this perspective, the origin of *Homo sapiens* was to be sought much more deeply into the past since the varieties of humankind were already well differentiated

before the end of the Quaternary (Boule, 1911-1913, 1921). The only human fossils directly related to living humans are the Upper Pleistocene varieties (*Homo sapiens*) found in Europe of the Grimaldi, Cro-Magnon and Chancelade types. These latter varieties were said to be separated from the other human fossils by a morphological hiatus.

Like most scientists of the time, Boule associated, on the one hand, the Mousterian culture and *Homo neanderthalensis*, and on the other hand, the Aurignacian-Solutrean-Magdalenian cultures and Upper Pleistocene representatives of *Homo sapiens*. The distinction between these two species is, according to Boule (1921:247-248), correlated with a cultural one: "l'âge du Renne présente un ensemble de caractères qui lui impriment une grande unité et qui marquent un progrès considérable sur le monde moustérien. C'est qu'il y a aussi un véritable contraste entre les Hommes de l'ancien et du nouveau Paléolithique". Here, Boule mentioned that his phylogenetic scheme was concordant with the archaeological sequence.

Again, how can the association between *Homo neanderthalensis* with its Mousterian culture and *Homo sapiens* with its non-Mousterian cultures be maintained, if these early *Homo sapiens* forms are represented by several morphological types (Grimaldi, Cro-Magnon and Chancelade) sharing the same cultures? Maybe anticipating such a difficulty, Boule (1921:248) had the following reflection:

> "Chacun de ces types [Grimaldi, Cro-Magnon and Chancelade] a bien sa physionomie propre, mais simplement au même titre que les diverses races d'aujourd'hui et, dans l'ensemble, ils ne diffèrent pas plus des Hommes actuels que ceux-ci ne diffèrent entre eux. Nous sommes arrivés à un moment à partir duquel l'évolution physique de l'Humanité peut être considérée comme terminée; le problème des origines humaines perd son caractère zoologique pour devenir purement anthropologique et ethnologique".

Because there is a zoological distinction (among species), the morphological/cultural association is valid, while this association is not valid when applied to an anthropological distinction (within a species). How can this situation be defended? Are there two different types of morphological variations? Boule (1921:252) took another path to explain this entire situation:

> "Ces subdivisions [Aurignacian, Solutrean, and Magdalenian] ne sauraient avoir le caractère général qu'ils veulent leur attribuer. Tout paraît démontrer, au contraire, que les gisements de chaque pays, et même de chaque province, nous présentent, avec un fond archéologique commun, tels et tels caractères ethnographiques particuliers, qui ne sont que des *facies* locaux".

Here, Boule doubted the universal value of the Upper Palaeolithic subdivisions put forward by the archaeologists, and embraced instead the notion of numerous ethnographic cultural distinctions produced by a host of racial types or populations on the common theme of the Aurignacian-Solutrean-Magdalenian cultures. However, by denying or not applying

such an ethnographical conception to the Mousterian culture or any other cultures, Boule only displaced the whole question to a different level. Now a cleavage was created by an ethnographic concept of the cultures of the Upper Palaeolithic and a non-ethnographic concept of the other cultures. Apparently, Boule would not address this situation.

For L.S.B. Leakey (1935), the main human stem reached back to the Oligocene only to split into two stems during the mid-Miocene. One led to forms such as *Pithecanthropus*, *Sinanthropus* and *Palaeoanthropus* (Mauer, Broken Hill, Neandertals), and the other towards the separation of the living varieties of humankind in the mid-Pleistocene, through early representatives of modern appearance such as his proposed *Homo kanamensis*.

As a scientist who contributed to prehistoric archaeology in East Africa, Leakey was in an interesting position to discuss the association between morphological types and cultures. From the outset, Leakey was critical of the utilisation of the typological terminology. Considering the usual association made between the Mousterian culture and the Neandertal specimens found at Spy, he wrote that "no one had begun to recognize the differences between Mousterian, Levalloisian and Upper Clactonian, &c., and as far as I know the material has not been re-examined in the light of our present knowledge" (Leakey 1935:190). This critical

evaluation of the Mousterian culture was also applied at sites such as Krapina and La Chapelle-aux-Saints.

After recognizing the wide range of variation found among the Neandertal morphological remains, Leakey (1935:200) contended that:

> "The various remains are not by any means always found to be associated with a 'Mousterian culture' except in the widest and vaguest misuse of that term. Some of them are associated with a pure Levalloisian culture, some with pure Mousterian culture, and still others with a Mousterian-Levalloisian contact culture... There is some indication that these variations [morphological ones] can be correlated with the differences in cultures".

Instead of using this opportunity to develop an ethnographic approach to the Mousterian culture and break the association between that culture and the Neandertal forms, Leakey maintained it at a greater level of resolution by associating races or varieties of the same species with better defined typologies.

The persistence of a tight cultural/morphological association is especially evident in Leakey's (1935:133) approach when he deduced that the European makers of the Acheulean culture were of the *Homo sapiens* type, based on the association found between the Chellean culture and a *Homo sapiens* specimen in East Africa, in addition to the fact that he asserted that the Acheulean was a direct derivative of the Chellean!

By the 1940s, a number of scientists who had once promoted side branches not related to the living varieties of humankind had changed their minds. Instrumental to this shift of opinion were a number of fossil discoveries made in the 1930s and 1940s.

Hooton (1931) previously held that most human fossils were on side branches not directly related to living humans. By the mid-1940s, however, Hooton (1946) had completely changed his mind. Now, most human fossils were directly or very nearly related to the living varieties of humankind. Hooton presented a scheme where numerous instances of hybridization occurred among various stems. *Pithecanthropus*, Solo and Wadjak were related to the living Australians. Broken Hill was a side branch of a stem leading to the living Bushmen (San) through Florisbad and Boskop. *Sinanthropus* was the ancestor of the Neandertaloid forms; the latter might have gone extinct or were absorbed and swamped by a dominant *Homo sapiens* type. For Hooton, all these stems derived directly or secondarily from the main Pliocene *Homo sapiens* trunk that led directly to the basic "white" stock through *Eoanthropus*, Swanscombe, Galley Hill and Combe Capelle.

When considering prehistoric archaeology, Hooton accepted the European archaeological sequence involving the Abbevillian, Acheulean, Mousterian, Aurignacian, Solutrean and Magdalenian. However, when faced with the East African sequence established by L.S.B. Leakey, Hooton referred to it

as being a "bizarre archaeological typology". More precisely, he stated that the "archaeological sequences postulated by Dr. Leakey and his associates seem very peculiar. Aurignacian is contemporaneous with Mousterian, according to his findings, and Aurignacian includes pottery - never found in Europe or Asia before Neolithic times" (Hooton, 1946:392). Hooton's surprise at the possible contemporaneity of the Mousterian and Aurignacian cultures is not entirely clear since he himself defended a complex phylogenetic tree involving numerous stems evolving more or less in parallel and sometimes interbreeding.

Although Keith (1915) had previously stated that most human fossils were not directly related to the living varieties of humankind, he also changed his mind by the mid-1930s (Keith, 1936). He now maintained that the separation of the modern races was very ancient (early Pleistocene), that each race evolved in its own geographical area, and that they went through similar or parallel changes (Keith, 1949). From this perspective, most human fossils were directly or very closely related to the living varieties of humankind. According to Keith, one of the most convincing lines limited to a restricted area was that of *Pithecanthropus*, Solo, Wadjak, Talgai, Keilor and living Australasians. Among other such lines or stems we find on the African line the fossil specimens of Kanam and Broken Hill, *Sinanthropus* is related to living Sinasians, while the Caucasians are directly

related to the Mount Carmel forms and the so-called Cro-Magnon types.

Commenting on the association of some morphological forms and archaeological material, Keith (1949:264) wrote that the "bones of Neanderthal human have not been found as yet in England, but remains of his stone culture are plentiful; we may well expect that his fossil bones will turn up some day". This is surprising when considering that Keith envisioned that during "the whole period of human evolution mankind had been divided into a vast number of isolated local communities, each inhabiting a delimited area of territory" (Keith, 1949:3), local groups estimated from living "primitive" peoples to vary from 50 to 150 individuals. This conception should have moved Keith to be more cautious about the morphological/cultural association, because distinct social groupings may have had different cultural products, as some archaeologists had already pointed out by that time.

Discussion

The analysis presented here of the fields of human palaeontology and prehistoric archaeology between 1900 and 1950 cannot pretend to be exhaustive and complete. Yet, it suffices to highlight a number of key elements characterizing the nature of their relationship.

Nobody would seriously doubt that man's biological and cultural aspects were, at one period or another of human

evolutionary history, intricately related and part of a whole. To uncouple them is, in a sense, arbitrary. However, if the understanding of the biology/culture link is theoretically sound and desirable, its actual understanding in the studies of human evolution posed important epistemological problems as already seen. The limitations and difficulties of the integration of scientific disciplines imposed restrictions.

The scenarios of hominization proposed during the last two centuries are highly theoretical constructions integrating loosely a number of variables thought to have been important to explain the process of hominization (e.g., Stoczkowski, 1994). From the outset, the programme in palaeo-ethnology put forward by Gabriel and Adrien de Mortillet, that established a relatively strict association between the evolution of a morphological type and an industry, was sound but highly theoretical.

In contrast, the fields of human palaeontology and prehistoric archaeology between 1900 and 1950 were, epistemologically speaking, uncoupled at the level of analysis at which most scientists of the time were working, although this was not recognized by all of them (see below). The bulk of the research activity in human palaeontology and prehistoric archaeology had been directed at low-level theories or generalizations, that is, at producing generalizations that are relatively close to the empirical

evidence. As higher-level theories or generalizations are postulated, more complex systems of inference are required and the further removed these assessments are from the empirical basis.

It is not maintained here that the fields of human palaeontology and prehistoric archaeology in the first part of this century had nothing in common. On the contrary, they had developed more or less in parallel, by being exposed to similar epistemological imperatives such as: a knowledge historically acquired through slow discoveries, a range of research limited first to Europe in time and space and gradually expanded to other geographical areas and temporal zones, and problems of integration and synthesis of knowledge related to the expansion.

However, regardless of these similarities, the synthesis or harmonization of both empirical chains (fossils and tools) could not be anticipated from a morphological type or from cultural products only, although one empirical chain is the product of the other.

Epistemologically speaking, these associations could be made only through the simple exercise of documenting them passively, that is by waiting for them to be discovered. This apparently simple exercise gets complicated when the relevant facts are recovered at different rates (many more artefacts have always been found than hominid skeletal

remains) and not always in direct association with one another.

The nature of the material itself also required different conceptual and methodological assessments because, although classification is at the heart of both disciplines, the entities involved in them need different evaluations. The definitions of biological entities (populations, races, subspecies, species and genera) and of cultural ones (tool types, assemblages, industries and cultures) flow from distinct approaches, and different disciplinary debates and searches for consensus.

I have tried to demonstrate in this paper that human palaeontology and prehistoric archaeology have been, on low-level theories or generalizations, distinct research entities that had to develop independently from one another between 1900 and 1950 in order to forge proper methodological, conceptual and theoretical tools. On higher-level theories or generalizations, these fields have been rightly unified, most notably in scenario-building, because the biology/culture link is inherent in humankind's evolution.

Also, this paper has tried to promote the view that scientific fields are changing historical entities with complex and multi-layered structures that must be clearly envisioned as such if we want to understand the processes by which they change and the nature of their integration. They cannot easily be completely integrated at any one time, for

they are each driven, to some extent, by an internal logic. If this latter aspect of science is overlooked, the risk is increased that scientists and philosophers alike might attempt, in the name of well intended integration, a harmful fusion. Scientists may want to force or reduce all the elements that characterize the research activity into the same epistemological plane, that is, under a single set of methodological, conceptual and theoretical rules.

To discover exactly how man's biological and cultural aspects were related in the past was indeed an important and legitimate task of studies of human evolution. However, the complete integration of human palaeontology and prehistoric archaeology between 1900 and 1950 would have entailed the risk of a great loss of information.

An interesting feature of the analysis presented here is its contribution to the problem of integration in science. In addition to models of integration in science mentioned in the introductory part of this essay, this analysis reveals to us how integration is largely contingent upon the views of the scientists involved in the scientific process. In other words, the problem of integration in science should not be analysed independently of the views of the actors involved in the process of knowledge building. It must also be assessed by being carefully contextualized in a proper historical setting. This has been clearly demonstrated in the analysis

presented here. It can be summarized in the following points.

Numerous prehistorians during the first half of the twentieth century recognized, implicitly or explicitly, the epistemological and disciplinary break between human palaeontology and prehistoric archaeology. The documentation of lithic sequences was performed independently of that of the peoples producing the stone industries. The industries constituting these lithic sequences were created by pulling together the assemblages from a number of sites, and by overlooking some local variations. The synthesis of different lithic sequences from throughout the Old World created important problems of nomenclature or of comparative archaeology that required debates to reach a resolution. This process was barely started by 1950. Prehistorians often described the transformation of their artefactual material through time and space as if it had evolved in an organic fashion, independent of any reference to the ethnographic background. Eventually, a number of archaeologists contributed to the uncoupling of an archaeological culture from a morphological type, by clarifying the concepts of culture as expressed in ethnology and archaeology. It was realized that similar morphological types could produce quite distinct cultural products and conversely, thus breaking any necessary association between morphology and culture. It was recognized also that prehistorians should be looking for

individual cultures and not stages in a lithic sequence. Inspired at an early period by the methodological and conceptual approaches developed in palaeontology and geology, at least in France and England, the field of prehistoric archaeology was by the 1930s moving slowly towards historical and ethnographical approaches.

In contrast, however, scientists concerned with human palaeontology during the same period often maintained the morphological/culture association by trying to integrate the archaeological sequence with their phylogenetic schemes. When attempted, these associations revealed clearly that they were sometimes contradictory. For instance, Hrdlicka defended the view that the morphological continuity between the Neandertals and *Homo sapiens* was correlated with a continuity in the archaeological sequence, while Boule held that a morphological discontinuity between *Homo sapiens* and *Homo neanderthalensis* was correlated with an archaeological break. In other instances, the morphological/cultural associations were simply uneasy. Examples are the association of one morphological type with a single cultural product (Neandertals and the Mousterian) and several morphological types sharing several cultural products (Cro-Magnon, Grimaldi, Chancelade and the Aurignacian, Solutrean and Magdalenian). The difficulty of superposing specific phylogenetic interpretations on archaeological sequences is another example. Klaatsch recognized the so-called

progressiveness of the lithic sequence in Europe, but his phylogenetic scheme implied that this sequence was produced by two quite distinct and unrelated hominid stems. Human palaeontologists were often uncritical about their morphological/cultural associations, although a number of prehistorians asserted in the 1930s and 1940s that similar morphological types could produce quite distinct industries and vice versa, and that scientists should be looking for individual cultures and not stages in a lithic sequence. Hooton and Keith remained, by the late 1940s, unaffected by such developments in prehistoric archaeology. This shows clearly the slowness with which knowledge in prehistoric archaeology was incorporated into human palaeontological thinking.

In addition to the models of integration in science already proposed by philosophers (pseudo-, peripheral and substantive integration), a truly historical dimension needs to be incorporated in order to make them as complete as possible.

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Chapter 5

17

Human Palaeontology and the Evolutionary Synthesis during the Decade 1950-1960

Richard G. Delisle

This chapter analyzes the influence of the evolutionary synthesis on human paleontology during the decade 1950-1960. It is suggested that the evolutionary synthesis directly influenced every day practitioners in human paleontology almost solely through the general concepts and methods of the new systematics. Instead of being only a common core shared by a host of disciplines, the evolutionary synthesis should also be defined by the extent to which that core has been guiding current research in any one field. The reception of the evolutionary synthesis in human paleontology reveals the - understudied - specific character of that discipline.

The study of human evolution in the twentieth century has the particularity of being under the influence of at least two different sources: developments in paleontology, since human evolution is subjugated under the general law of evolution, and, second, developments in physical anthropology, since man is also a living species with a particular diversity. The conjunction of the fields of paleontology and physical anthropology is rendered possible since geologically speaking human evolution occupies a relatively short time span while at the same time living relatives exist. It is not surprising that, during the first half of the twentieth century, when the earliest representatives of the human family were all estimated to be less than half a million years old, questions of variability (races) were intricately linked with questions of evolution. Nowadays, with human evolution extending almost over four million years, paleontology and physical anthropology may be more loosely linked, especially where the evolution of early hominids is concerned.

Prior to the advent of the evolutionary synthesis, which took place between 1936 and 1947 approximately, both paleontology and physical anthropology were essentially descriptive in nature and morphologically oriented. Several reasons have been proposed for such an approach in paleontology (*cf.* Rainger 1985). Palaeontologists have essentially been trained in morphology and geology, a background that incited them toward description, classification, and identification of stratigraphic relations of fossils. Even for those palaeontologists who broached theoretical implications of paleontology for evolutionary theory, description still occupied an important place. When describing new species and genera, they eschewed variations, especially intraspecific variations, in order to reconstruct the studied entities from a very often fragmentary record and to insert them within a classification. Also, with their concern for documenting changing structures over time, and hence possibly recognizing descendance, palaeontologists neglected spatial variations, stressing temporal ones. Even though some were considering intraspecific variations, among them most notably W. D. Matthew in the mid-1920's (Rainger 1986), it seems that nonetheless a large majority were not. More generally speaking, palaeontologists were reluctant in diverging from a descriptive attitude, even after G. G. Simpson's pioneering works. Although Simpson (1937; 1940; 1944; see also Laporte 1991) specifically advocated the use of the population concept of specie, and a less descriptive paleoanthropology, he had to notice that no changes occurred in the attitudes of palaeontologists, judging from the principal papers published in the 1939 and 1949 volumes of the *Journal of Paleontology*: *"It is the opinion of some palaeontologists that there has been some change in the scope and emphasis of palaeontological*

research in recent years ... some sort of trend ... to concentrate less exclusively on routine description and nomenclature and perhaps to give more attention to such subjects as ecology, functional and broadly comparative morphology, evolutionary processes, or, in general, studies involving a wider scope, either in geology or in biology, and more concern with theory and principles ... These figures certainly reveal no trend away from straight descriptive studies ..." (Simpson 1950: 498-499).

Though concerned with the diversity of living humans, physical anthropology was essentially descriptive in nature and morphologically oriented. Human variation was dealt with in terms of racial taxonomies (Weiner 1957), a tendency already well developed within the anthropological circles in the second half of the nineteenth-century in Europe, be it in the Société d'Anthropologie de Paris, the Ethnological Society of London, the Anthropological Society of London, or other societies in Europe (Hammond 1980; Stocking 1971; see also Riquet 1978; Odom 1967). In America, the institutionalization of physical anthropology was mainly built along the lines of the French anthropology and the so-called Institut d'Anthropologie (a federation of several organizations), through the foundation of the *American Journal of Physical Anthropology* in 1918, and the American Association of Physical Anthropologists in 1930 (Spencer 1981). As it was, physical anthropology drew upon essentially descriptive disciplines like anatomy, zoology, and medicine (Shapiro 1959). Prior to 1950, physical anthropology in America, for example, was concerned with a rigid conception of races and their genetic nature, in terms of fixed morphologies, physiologies, and behaviors; mankind was depicted as a composed of several primary races, while composite races were thought to have been formed through the hybridization of those primary races (Birdsell 1987; Little 1982).

In the foregoing, I have only very briefly presented some issues in the history of paleontology and physical anthropology during the late nineteenth and first half of the twentieth century, stressing the descriptive nature and morphological orientation of these disciplines which have influenced the study of human evolution. I will try to evaluate the influence the evolutionary synthesis had upon the field of human paleontology by, first, defining the evolutionary synthesis; second, presenting those aspects which influenced the field of human paleontology; and third, measuring the impact those aspects had upon reconstructions of human phylogeny during the decade 1950-1960.

The Evolutionary Synthesis

According to Mayr (1980), the situation in biology by the end of the 1920's did not seem promising for an eventual synthesis. Taxonomists were divided between typological and population thinking, while paleontologists were mostly advocating saltationism or orthogenetic theories of evolution, and geneticists still worked within a typologically oriented mutationist framework. But during the 1930's changes occurred that would facilitate a rapprochement (Mayr 1980; see also Delisle, submitted). With the publication of Theodosius Dobzhansky's *Genetics and the Origin of Species* in 1937 a new branch of genetics emerged, called 'genetics of populations' and concerned with processes, it was held, taking place in populations. The laws governing the genetic structure of individuals and of populations are distinct processes under the influence of natural selection and genetic drift. In systematics, population thinking was adopted, and led to the conception of 'polytypic species', a species inherently variable composed of semi-isolated populations, and a biological species concept which defines species not in terms of morphological gaps but in terms of reproductive gaps - ecologically, geographically, and morphologically. These new developments, often referred to as the New Systematics, were most effectively presented by Ernst Mayr's *Systematics and the origin of species* in 1942, stressing the implication of the polytypic species concept for systematic categories. Paleontologists tried to demonstrate that new developments in

genetics were consistent with the fossil record - that there was an important link between microevolution and macroevolution. George Gaylord Simpson's *Tempo and mode in evolution* (1944) was based on the premises that some, if not all, mechanisms involved in paleontology are the same as those found in laboratory organisms, but at the same time defended that palaeontologists are in presence of patterns that require new explanations, such as the one he calls quantum evolution (a rapid transition of a small population from one adaptive zone to another through an inadaptive phase). This rapprochement between genetics, systematics, paleontology, and other disciplines, came to be known as the Evolutionary Synthesis. This evolutionary synthesis essentially came to the conclusion that natural selection is the most important mechanism for evolutionary change, and that it is acting on a store of variations within populations - variations produced from small genetic change - that produce a gradual change at the microevolutionary levels as well as the macroevolutionary ones (Charlesworth *et al.* 1982; Gould 1980; Mayr 1980; Smocovitis 1992).

But to what extent are the conclusions of the evolutionary synthesis, that we will here refer to as its 'core', actually shared by disciplines such as genetics, systematics, paleontology, ecology, comparative anatomy, embryology, and some mathematics? Is the core of the evolutionary synthesis indeed guiding the bulk of research activities in these disciplines? Interesting questions come up when the core of the evolutionary synthesis is not defined as a common basis for all disciplines involved but when, reversely, we ask to what extent the core of the evolutionary synthesis is guiding current research in any one field. The structure of the evolutionary synthesis as presented by Provine (1988), Burian (1989), and Caplan (1978), is revealing on that count.

Provine (1988) interestingly argues that the evolutionary synthesis as it is defined here - from approximately 1930 to 1950 - was not resting on a concept or a theory that could be considered as a center, even though several concepts and theories played an important role - like Fisher's fundamental theorem of natural selection, Wright's shifting balance theory, and Mayr's founder effect, to name just a few. Instead, Provine argues, the evolutionary synthesis focused on a relatively small set of variables considered to be crucial for the understanding of evolution (effective population size, population structure, mutation rates, migration rates, etc.), while there was disagreement as to their relative importance in evolution.

According to Burian (1989), the evolutionary synthesis was not a single paradigm shared by all adherents, since these were drawn from a host of disciplines to which they, even though their commitments were overlapping, nonetheless kept allegiance. Burian hence sees the evolutionary synthesis not as a theory but as a treaty where conflicts between disciplines are neutralized by the common conviction that population genetics through natural selection accounts for all evolutionary phenomena at all levels, whereas the patterns and results of evolution are drawn from specific disciplines. Each discipline possesses a more or less important part of a grand scheme explaining evolution, but at the same time depends on other disciplines.

According to Caplan (1978), the modern synthetic theory of evolution is not a single one but represents an amalgam of theories, sub-theories, and theoretical programmes, while its contributing disciplines - like population genetics, demography, mathematical ecology, and behavioral biology - are not deductively related. Any subspeciality within this structure would be independent from the other subspecialities to proceed freely with its research program, even though the overarching structure is not a composition of elements lumped together randomly, but an ordered and unified set of disciplines, meaning that some elements are uniting all those disciplines. For example, the levels of biological organization unite a host of disciplines, but each at its own level; disciplines are united by their common interest in particular biological phenomena; disciplines have a large mass of observations, empirical facts, and scientific descriptions in common; certain disciplines depend on theories of others for particular explanations.

I do not aim at an evaluation of the propositions presented here but I do wish to stress that they all favor a complex evaluation of the modern synthetic theory of evolution by reducing the

importance of a definable 'core'. Any analysis involving the evolutionary synthesis from the 1930's onwards should take into account its complex structure, should it be the actual and internal debate over the expansion, replacement, or maintenance of the evolutionary synthesis, or any comparison between the evolutionary synthesis and other disciplines, which have not contributed to it. We will now turn to the influence of the evolutionary synthesis on human paleontology.

Human paleontology and the Evolutionary Synthesis

Three contributors to the evolutionary synthesis in particular directly applied its concepts and methods to human evolution: Theodosius Dobzhansky, Ernst Mayr, and George Gaylord Simpson. In an article entitled 'On species and races of living and fossil man', published in 1944, Dobzhansky discusses the thesis, proposed by Franz Weidenreich, that hominid evolution took place within a single species subdivided into several races - a thesis based on a morphological approach. Against the view that mankind is to be considered a group of species, Dobzhansky holds that *"these opinions are due to unfamiliarity with or to misunderstanding of the polytypic species concept of the modern systematists and the race and species definitions based on genetic considerations"* (254). Whereas species are groups of populations reproductively isolated from each other, races are populations differing in gene combinations but exchanging or potentially exchanging genes across boundaries. Dobzhansky demonstrated effectively how genetics could be applied to fossil hominid populations by showing how morphological species and races are inferred from genetical species and races. Peking and Java men were two different races and not distinct species, for the magnitude range of their differences are well within that of living human races. He also pointed out that the biological status of the Neanderthal was elucidated by a fossil population that consisted of individuals ranging from a Neanderthalian to a *Homo sapiens* at Mount Carmel in Palestine. Contrary to the interpretation that the Mount Carmel population was in a process of splitting into two types (Neanderthal man and *Homo sapiens*), Dobzhansky held that *"two types differing in a system of genes can not crystallize out of a mixed population without the interbreeding of these types being prevented by a spatial separation"* (258); hence this population arose by the hybridization of the two types, meaning that Neanderthals and Homo sapiens are races of the same species and not distinct species. On that ground, Dobzhansky (1944: 261-262) defended the idea that *"no more than a single hominid species existed at any one time level"*, meaning that hominid evolution has been *"taking place within a single genetic system"*. In this context, Dobzhansky (1950) presented species not as types established by a describer but as a changing population in a dynamic state of equilibrium between intra- and interpopulational variability, with race divergence taking place but also being arrested by the melting down of race differences (see also Dobzhansky 1963).

Mayr (1942) contrasts the old with the new systematics on the ground that the old systematics is characterized by negligence of infraspecific categories and a purely morphological definition of species, whereas the new systematics highlights infraspecific categories, thus reducing the importance of the species level. It sees the population as the basic taxonomic unit, and morphological species definitions are replaced by biological species definitions, taking into account ecological, geographical, and genetical factors. Morphological species definitions in terms of groups of individuals or populations with similar morphological characters do not discriminate between morphological distinctions at the subspecific, specific, and supraspecific levels since the same features are used to characterize these. The biological species concept, being based on reproductive gaps, uses morphological differences as a yardstick in cases where reproductive isolation cannot be determined by using known species to determinate whether a new form is specifically distinct or not. Whereas any clear-cut morphological difference is a species distinction for a proponent of the morphological species definition, it is simply an indication of a possible

species distinction for a proponent of the biological species definition, morphology being subordinated to other biological factors. In an article entitled 'Taxonomic Categories in Fossil Hominids', Mayr (1950) holds that the problem of man's origin depends largely on a proper definition and evaluation of taxonomic categories. He boldly suggests to reduce the bewildering diversity of names attributed to fossil remains (*Australopithecus*, *Plesianthropus*, *Paranthropus*, *Pithecanthropus*, *Sinanthropus*, *Palaeoanthropus*, to name just a few) to a single genus: *Homo*. This genus is biologically meaningful and defined accordingly as the occupancy of a specific adaptive plateau, by the acquisition of the upright posture. *Homo* entered a new adaptive zone that increased selective pressure and accelerated the evolutionary changes that resulted in phyletic speciation up to *Homo sapiens*. Although biologically meaningful, the recognition of the genus category is not based on any absolute characteristic, whereas the species category is based on reproductive isolation. In accordance with the polytypic nature of species, Mayr recognizes geographical variation among populations within the genus *Homo*, holds that *"never more than one species of man existed on the earth at any one time"* (112), for the *"amount of geographical variation and the degree of difference among the geographical subdivisions of a species are different from case to case"* (111). Although modern man is relatively homogeneous because of the importance of interbreeding between different races, primitive man was divided into in small groups, with reduced interbreeding favoring a greater variability than in modern man (see also Mayr 1963).

Simpson (1940) argues that a species is not a subjective concept, but on the contrary a more or less real entity in nature. This entity, constituted by a group of individual animals, may be a real species in nature, but it cannot be the species of taxonomy since the taxonomic species is an estimate of the real species. On this basis, Simpson proposes a system of inference which takes taxonomy as a representation of real entities in nature. In this system, specimens are a sample from which population or species characters are inferred. A constant re-evaluation of the inferred variability of the population or species takes place, by looking whether excluded specimens (or newly discovered ones) fall within the boundaries, in order to either reassess the variability or reject these specimens as candidates for the inferred population or species. This system sharply contrasts with the classical approach where a single specimen or a series is used as an inflexible standard or type for a species. All sampled specimens are given equal weight. This procedure leaves no place for distinguishing between primary and secondary types. In an article entitled 'The Meaning of Taxonomic Statements', published in 1963 in a conference volume on human evolution and classification, Simpson again presents this inferential system, with some modifications. Individual specimens represent a sample of a population; populations represent a taxon; taxa are assigned a hierarchic category, and incorporated in a classification. He focuses not primarily on what, but on how opinions should be expressed, taking taxonomy as a tool for communication among scientists that should be free of ambiguity (see also Simpson 1961 for some other points on taxonomy). He notes the chaos in Hominid nomenclature, and points at two possibilities explaining this situation. Firstly, the failure to use consistent designations for specimens, populations, and taxa: *"A truly eminent anthropologist insisted on using the ... nomen* Sinanthropus pekinensis *for specimens and a population although he concluded that this nomen does not designate a taxon specifically distinct from* Pithecanthropus erectus *or indeed from* Homo sapiens. *The example is far from unique"* (6). Secondly, a lack of experience in evaluating taxonomic categories: *"Many fossil hominids have been described and named by workers with no experience in taxonomy. They have inevitably lacked the sense of balance and the interpretive skill of zoologists who have worked extensively on larger groups of animals"* (6). Simpson also warns against the typological tendency of naming taxa on the basis of a single specimen without any reference to population.

The Evolutionary Synthesis and human phylogeny

As we have already seen, the evolutionary synthesis is in fact a complex structure of theories, propositions and explanations, shared and used differently from one discipline to another. The field of human paleontology as defined earlier, as a conjunction of some elements of paleontology and physical anthropology, has not been influenced by the entire corpus of the evolutionary synthesis. From the analysis presented above, it would seem that a group of closely related concepts and methods referred to as the new systematics has influenced the field of human paleontology: the polytypic species, population thinking, and their implications for defining categories. From the viewpoint of human paleontology, it seems that the new systematics has been potentially the most influential aspect of the evolutionary synthesis. Through the new systematics, of course, but indirectly, other concepts and methods could have influenced the field of human paleontology. Interventions - in human paleontology - by Dobzhansky (1944), Mayr (1950) and Simpson (1963), respectively a geneticist, a systematist, and a palaeontologist-geologist, have essentially been concerned with a reinforcement of the new systematics from the point of view of genetics, taxonomy and palaeontology. It is important that the new systematics does not correspond rigidly to a discipline, but rather represents a group of concepts and methods which was largely developed within the boundaries of the discipline of systematics, but also to a certain extent within genetics and paleontology, even though some concepts and methods in genetics and paleontology could hardly have had a direct impact on human paleontology. This situation is understandable, since human palaeontologists, in their daily practice of classifying fossils - without implying, of course, that they are only classifiers), could hardly have been influenced directly by considerations on chromosomal changes, Wright's shifting balance theory, Mayr's founder effect, genetic drift and flow, or debate over the nature of the link between microevolution and macroevolution, whereas general concepts and methods of the new systematics could have a direct and immediate impact on human paleontology since fossils are interpreted within the conception of a polytypic species (based on the biological species definition by inference) and consequently named in a meaningful system of categories. Mayr's founder effect, genetic flow, or the biological species definition, for example, can only be in a human palaeontologist's eye, indirectly inferred from the fossil record, in itself already based on the inference of the polytypic species concept. For the sake of simplification and presentation it may be argued that biology is divided between descriptive and experimental disciplines (*cf.* Allen 1979; 1981), concerned with the *description* and *explanation* of natural phenomena respectively, while human paleontology by its very nature is primarily describing through the polytypic species concepts, population thinking, and their implications for defining categories, and only explaining or justifying afterwards through the genetic drift or flow, the founder effect, natural selection and adaptation, etc. Other methods and concepts used by disciplines such as cytology, embryology, and mathematics, are even more remotely related to human paleontology, if at all. In short, the evolutionary synthesis had its impact directly through the new systematics (corresponding roughly with a large part of the systematics and less important parts of genetics and paleontology), and only indirectly through other concepts and methods (if at all), meaning that the evolutionary synthesis influenced human paleontology essentially through the new systematics. I will refer to this group of related concepts and methods of the new systematics as 'population thinking', as opposed to a descriptive and strictly morphologically oriented approach with no consideration for the biological meaning of categories that will be referred to as 'typological thinking'.

The distinction between population thinking and typological thinking was an important issue in human evolution studies around 1950. Concerning a symposium organized by Theodosius Dobzhansky and Sherwood L. Washburn in 1950, on the *Origin and Evolution of Man*, Washburn (1983: 15-16) wrote that *"although one of the main purposes of the conference was to stress the importance of thinking in terms of populations, typology continued to be used by a number of the participants. Population vs type was probably too fundamental an issue to be discussed usefully in*

a public meeting". The tension between population and typological thinking can be clearly understood by its impact on assessing variability. Following Mayr's (1969: 24-29; see also Mayr 1942 and Simpson 1937) analysis, the typological definition of species assesses the variability of individuals as belonging to a limited number of types in nature, variation being considered as trivial and irrelevant; the biological definition of species assesses the variability of individuals as belonging to a reproductive community, an ecological unit, and a genetical unit. Accordingly, typologists tend to exaggerate the constancy of taxa and the sharpness of the gaps separating them (Mayr 1969: 67). Typologists were more inclined toward a multilinear scheme, or a splitting attitude, whereas populationists were inclined toward an unilinear scheme - toward lumping. What follows is a presentation of some interpretations of human phylogeny during the decade 1950-1960, in order to evaluate the impact of (some elements of) the evolutionary synthesis on human phylogeny.

The changing interpretation of Le Gros Clark is very characteristic of the impact of population thinking on human phylogeny. At first, Le Gros Clark (1954) recognized that a speciation occurred between *Homo sapiens* and *Homo neanderthalensis*, and that a specific distinction should be retained. While this author proposed that all Australopithecine fossils should be lumped into the single genus *Australopithecus*, it was also considered that *"there may be more than one species"* (1954: 389). Later on, Le Gros Clark (1958) ventured that the tendency towards taxonomic individualization of every fossil by attribution to a new species or genus unnecessarily complicated the picture of human phylogeny. Instead, he suggested that the genus *Australopithecus*, like the genus *Pithecanthropus*, shows *"a considerable range of variation in skull form and cranial capacity"* (1958: 352) and that this genus is not composed of different species but of regional varieties of a local nature comparable to the racial varieties found in modern *Homo sapiens*. The Early Mousterian populations of Europe represented the variability of a single taxonomic group that *"provided the genetic material for a subsequent diversification on the one hand into the extreme (or "classical") Neanderthal type of later Mousterian date, and on the other into the modern races of* Homo sapiens" (1958: 353).

According to Breitinger (1957), fossil hominids were still partly classified in a formal manner, using taxonomic names as mere labels without any biological meaning and thus renouncing any biological significance and phylogenetic implications. *"The only safe way out"*, Breitinger proposed, is by *"getting in step with the new knowledge and principles of taxonomy, which have led to a biologically based definition in place of the morphological definition of species"* (1957: 445). From the perspective of the new systematics, the phyletic species Homo neanderthalensis should exhibit a polytypy in space and time comprising geographical *"races of the 'extreme' forms of Neanderthal in Europe, Africa ... and Asia ... and in geochronological range all the populations represented by the 'generalized Neanderthal' and the 'Preneanderthals'"* (1957: 453). Breitinger (1959: 188) concluded that hominid evolution was punctuated by *"only one single a priori certain case of a complete speciation and splitting"*: the one separating the Hominids from Tertiary primate species. As we have just seen, population thinking was accompanied by a defense of the unilinear scheme in human evolution (see also Dobzhansky 1944; Mayr 1950). But population thinking did not necessarily imply an unilinear scheme. Howell (1951) envisioned the Neanderthal as a morphologically and temporally heterogeneous population. This was an important consideration in phylogenetic discussions, for early Neanderthals were considered to be similar to modern man, while classic Neanderthals were deemed to exhibit *"a pattern peculiar to, and distinctive of, a geographically isolated group"* (1951: 406), and to be subjected to extreme environmental conditions. Under those circumstances, *"selection would be severe, chance for genetic drift at an optimum, and opportunities for migration reduced to a minimum"* (1951: 409). Accordingly, Howell's analysis implied that the early Neanderthals were similar to modern man, contrary to the later classic Neanderthals, and thus suggested that this later group constituted a

sharp break; modern man and classic Neanderthals descending from early Neanderthals. *"The concept of a 'Neanderthal' phase or stage of man's evolution"*, Howell wrote, *"is no longer useful since there was a marked variability from one such group to another"* (1957: 343). Howell's analysis is of interest, for he shows how population thinking and its related concepts (the polytypic nature of species and the genetical inference from morphology) are not strictly associated with a rigid unilinear scheme, but nonetheless point to a lumping attitude since there is no need to postulate the origin of modern man and classic Neanderthals from different and remote ancestors.

Even though the impact of some elements of the evolutionary synthesis is evident among the authors just presented, others were less receptive to these new developments. Heberer (1959) depicts a highly bushlike human phylogeny with several evolutionary lines at the Australopithecine level (bipedality), and at least three evolutionary lines at the *Homo* level (big brain and culture): the *Pithecanthropus-Sinanthropus* line, the palaeoanthropine sequence including the Neanderthal line, and the *sapiens* line - *Homo sapiens* having early representatives. According to Heberer (1959: 242), many discoveries could not be accommodated in these clearly recognizable lines, for *"many 'Neanderthal Men' were discussed which by no means fit in the Neanderthalian line"*. This approach, claiming rigid evolutionary lines without reference to population variability, is rooted in typological thinking. The *Homo* level would, according to Heberer, illustrate the radiation of the Euhominid.

Without considering populations, Vallois (1954) was in favor of the pre-sapiens theory based on the early modern looking specimen from Swanscombe (England) and Fontéchevade (France) with their lack of a supra-orbital torus, a morphological characteristic of the Neanderthals. From this, Vallois inferred that *"the European Homo sapiens is not derived from the Neanderthal men who preceded him"* (1954: 128). His (Vallois 1958) multilinear scheme was inspired by the patterns of parallel evolution frequently found among mammals after an adaptive radiation - a mode of evolution also applicable to fossil men. Three simultaneous lines are encountered during the Mindel-Riss epoch with the men of Ngandong, the Preneanderthals, and the Presapiens, lines that earlier were believed to have succeeded one another chronologically.

For Leakey (1959), the taxonomic debate over the generic status of the Australopithecine did not seem to be biologically meaningful, since he assigns the name *Zinjanthropus boisei* to the newly discovered skull at Olduvai on a purely morphological basis, for it *"differs from both Australopithecus and Paranthropus much more than these two genera differ from each other"* (491). More precisely, taxonomic issues were for Leakey arbitrary matters: *"inevitably, different scientific workers have different ideas of what characters justify specific, generic, and even superfamilial rank. After all, this is purely a question of artificial labels"* (1960: 458).

This last statement resembles one by Broom (1950: 13) about the Australopithecine ten years before: *"The classification of ape-man will be as difficult as that of man is today. There can be no finality. 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."* This point, the arbitrariness of taxonomic categories, makes any phylogenetic interpretation difficult to assess. Interestingly, a large part of the debate over the Australopithecine level during the 1950's centered around generic questions, a taxonomic category only remotely involved, if at all, in biological considerations of populations. Mayr (1969: 91) wrote that *"the category species is <self-operationally> defined by the testing of isolating mechanisms in nature while non-arbitrary definitions for the supra-specific categories are not available"* (see also Mayr 1950). Dart lumped all Australopithecine and possibly also Meganthropine remains into a single human species. This decision was based on an analysis of the variability of the Australopithecines in comparison with the variability of modern *Homo sapiens*. On taxonomic issues he states: *"The final 'pigeonholing' by names of apes, man-apes, ape-men and men, whatever their temporary names today, can safely be left to the zoological systematists of the future after their number, distribution and range of*

variation have been resolved by the strenuous field work of and descriptive study by their excavators" (Dart 1960: 268-269; *cf.* his 1955).

The tension between population thinking and typological thinking had a profound influence upon phylogenies in human paleontology during the decade 1950-1960. Some proponents of population thinking (Breitinger 1957; Le Gros Clark 1958) made explicit reference to elements of the new systematics, while Howell (1951) implicitly based his analysis on population thinking and justified his phylogenetic scheme with genetic concepts. Among the proponents of typological thinking, some made little reference to populations, basing their analysis of taxa on individual fossils (Heberer 1959; Vallois 1954), while others were attributed no or just little biological meaning to categories (Broom 1950; Dart 1960; Leakey 1960).

During the 1960's, on the other hand, a large portion of scientists involved in human phylogeny began to accept population thinking, thereby contributing to the appearance of modern palaeo-anthropological research (Delisle, in prep.). From then on, most interpretations in human evolution were free of evident typological thinking, permitting the establishment of a universal terminology based on concepts and methods of the new systematics. A particular designation - subspecies, species, genus - now referred to a precise conception of animals in nature: races as semi-isolated populations, species as isolated by a reproductive gap, genera as groups of associated species - but with no absolute criteria.

Conclusion

Each discipline centrally or more marginally involved with the evolutionary synthesis seems to have retained specific elements of its own particular approach. Whether or not the contributing disciplines shared some common elements remains an open question that only further historical analysis will resolve. It was suggested here that, historiographically speaking, the evolutionary synthesis should not only be defined as a common basis for all disciplines, but also by assessing to what extent it was guiding current research in any one field. This paper contributes to such an effort. Human paleontology seems to have taken over directly from the evolutionary synthesis general concepts and methods of the new systematics, such as the polytypic species, population thinking, and their implications for defining categories. More specifically, we believe that the field of human paleontology, by being directly influenced by the new systematics or by selecting elements from it which were incorporated into its research program, was revealing some facets of its specificity as a field in science. By the way, we do not mean to imply by this that human paleontology was the only discipline to have been influenced by the evolutionary synthesis the way it was). It is not suggested that human paleontology is an autonomous field but rather that it has been and will continue to be influenced by other disciplines, in its own specific way. Thus, human paleontology was not born from the evolutionary synthesis - it has a history going well into the nineteenth-century - but it did borrow much of its terminology from it: a common language, referring to precise concepts and methods and some possible evolutionary *explanations*. Further research is needed to define the specificity of human paleontology.

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Chapter 6

Díaz Ungría, Adelaida (1913–)

Born in Spain, Díaz Ungría graduated in the natural sciences from the University of Barcelona in 1941, whereupon she commenced graduate studies at the University of Madrid. On receiving her doctorate in anthropology, she moved to Caracas, Venezuela, where she was one of the founders of the Department of Sociology and Anthropology at the Central University of Venezuela, Caracas. Since then, she has played a central role in the development of the discipline in Venezuela.

Dinorah Castro Guerre

See also Spain; Venezuela

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Dobzhansky, Theodosius (1900–1975)

Born in Nemirov, Ukraine, Dobzhansky majored in biology at the University of Kiev, from which he graduated in 1921. In 1924, he moved to the University of Leningrad, where he held the position of lecturer from 1924 to 1927. Dobzhansky's early work in the Soviet Union (1918–1927) was mainly on ladybird beetles (*Coccinellidae*), with an emphasis on systematics. Under the influence of his teacher Sergei Chetverikov (1880–1959), however, his interest turned to genetics and evolution. He left Russia after receiving a fellowship from the Rockefeller Foundation in 1927 to work in T.H. Morgan's (1866–1945) laboratory at Columbia University, New York. He followed Morgan to the California Institute of Technology in Pasadena and taught there from 1929 to 1940. He then moved back to Columbia, where he remained until 1962. While working with Morgan between 1927 and 1933, Dobzhansky devoted himself primarily to problems in classical genetics, making little reference to evolution theories. Among other things, he studied the mechanism of sex determination in the fruit fly *Drosophila*.

From the vantage point of his own research in evolutionary systematics and experimental genetics, Dobzhansky attempted a synthesis that he presented in his now classic *Genetics and the Origin of Species* (1937b), which went through two more editions, in 1941 and 1951. It is generally agreed that this book heralded the emergence of the modern evolutionary synthesis. This work was not a formal presentation of the state of research at that time, but rather a proposition for the development of a specific research agenda. Specifically, he was concerned with the problem of biological diversity and the investigation of the genetic basis of this variation. To Dobzhansky, evolution at all levels is rendered intelligible through experimental work in the laboratory and the utilization of genetic principles. His conception of evolution is rooted in Sewall Wright's (1889–1987) "adaptive landscape" imagery that depicts species or races as gene combinations that make their carriers fit for survival in a given environment perched on "adaptive peaks"—the "valleys" being the less favorable gene combinations.

In addition to his important contribution to the evolutionary synthesis, Dobzhansky's scientific career was largely devoted to the study of population genetics of *Drosophila*. Together with twenty-four of his collaborators, he participated in a series (from 1937 to 1976) of forty-three publications in the journal *Genetics* dealing with the "Genetics of Natural Populations." Taken as a whole, this publication series has been called the "most important single corpus in modern evolutionary biology" (cf. Lewontin et al. 1981). Although the series comprises only 40 percent of all of Dobzhansky's publications on this subject, the articles in it record the progress of experimental population genetics until the late 1960s. All of the papers except one deal with *Drosophila*, and they are concerned with topics such as geographical pattern and temporal changes of genetic variation. Some papers also deal with population structure for the

effective breeding sizes and level of migration of local populations.

Dobzhansky viewed anthropology as both a biological science and a science of culture, and human evolution as simultaneously involving two kinds of evolutionary development: the biological and the cultural. He perceived the biological and the cultural spheres as intertwined and manifesting a positive feedback system, in which biological change increases the fitness for cultural development, which in turn instigates further genetic change. According to Dobzhansky, a race is a biological phenomenon and merely a stage in the evolutionary development of species. Thus, while biological species may be regarded as genetically closed systems, races are open ones. Hence, race boundaries are blurred by the more or less slow gene exchange. The gene exchange between neighboring populations is a regular occurrence that serves to maintain genetic links between populations. Viewed from this perspective, Dobzhansky defined races as populations that differ in gene frequencies, rather than as racial types (cf. Dobzhansky 1950, 1951, 1963a, 1963c).

Dobzhansky's contribution to human paleontology stems largely from an article of his published in the *American Journal of Physical Anthropology* entitled "On Species and Races of Living and Fossil Man" (1944). In it, he reiterated the idea that "no more than a single hominid species existed at any one time level" and "taking place within a single genetic system, a species consist[ed] of geographically, but not reproductively, isolated races." Accordingly, he argued that "the differences between Peking and Java men are easily within the magnitude range of the differences between the living human races." Applying these same principles to the two coeval hominid forms found at Mount Carmel, near Haifa, Israel, Dobzhansky argued that "two types differing in a system of genes can not crystallize out of a mixed population without the interbreeding of these types being prevented by a spatial separation." He concluded that this population arose by the hybridization of two types, meaning that they were races of the same species and not distinct species, as some workers had supposed.

In addition to making significant contributions to the development of population genetics, Dobzhansky also assisted in the integration of human paleontology within the modern evolutionary synthesis by showing how genetics can be applied to the human fossil record, and by strongly defending a phylogenetic interpretation (Boaz 1981; see also Dobzhansky 1944, 1963b).

Dobzhansky died in Davis, California, on 18 December 1975.

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See also Asian Paleoanthropology; Evolutionary Theory; Genetics (Mendelian); Mount Carmel; Neo-Darwinism; Russia

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Doornik, Jacob Elisa (1777–1837)

Born in Leiden, The Netherlands, Doornik was the third son of the elderman Frederik Wolphert Doornik and Henriette Elisabeth van Baerle. He enrolled at Leiden University in 1792 and graduated as a medical doctor in 1797. During the next twenty years, Doornik practiced medicine, first in Amsterdam (1797–1813) and then in Apeldoorn and Velp. In 1817, he left the Netherlands for the Dutch East Indies. He settled in Batavia (now Jakarta) and became the general inspector of local hospitals; from 1820 onward, he was a medical practitioner. In 1826, Doornik became embroiled in a political controversy following the publication of an essay he wrote criticizing the governor-general of the Dutch colony. The scandal produced by this essay resulted in Doornik's removal from office and subsequent departure from Batavia for the United States in 1827.

Shortly after arriving in Philadelphia in the spring of 1828, Doornik made contact with the American craniologist Samuel George Morton (1799–1851), and, sharing a common interest in paleontology and anthropology, the two men quickly became close friends. During the next six years, first in Philadelphia and then in Baltimore, Doornik struggled in vain to build a new career as a public lecturer.

In 1835, Doornik moved to New Orleans, where he hoped to improve his financial situation. From all indications, things did not improve, and in the summer of 1837 Doornik died, but not before bequeathing the residue of his crania collection to his "faithful friend in Philadelphia," Samuel G. Morton.

Doornik's scientific interests centered on philosophical anthropology. In his speculative philosophical and anthropological writings, he showed himself to be an adherent of Immanuel Kant's (1724–1804) dynamical system, yet he rejected the ideas of F.W.J. von Schelling (1775–1854), Lorenz Oken (1779–1851), and the Naturphilosophen. He also rejected Franz Joseph Gall's (1758–1828) phrenological system. In his *Wijsgeerig-natuurkundig onderzoek aangaande den oorspronglijken mensch* (Philosophical-Physical Investigation Concerning Primitive Man), published in 1808, Doornik stated that, in the beginning, there had been six principal races of the human species, each with its own place of origin. He adduced numerous arguments taken from physical anthropology, history, and ethnology to substantiate this claim for the plural origins of humankind. In his "Verhandeling over het begrip van levenskragt uit een geologisch oogpunt beschouwd" (Essay on the Idea of Vital Force Considered from a Geological Point of View), also published in 1808, Doornik rejected Georges Cuvier's (1769–1832) doctrine of catastrophism in favor of the idea of transformism, claiming that the different forms in which life had revealed itself in successive periods had developed gradually one from the other.

Comparing the skeleton of a Negro with that of the European and the Asian pongid, orangutan, Doornik concluded that originally human beings had belonged to the latter family, and that the African Negro represented an intermediary form between the European and the Asian apes. The decisive steps in the transformation of ape to human, Doornik believed, had occurred in the bones of the skull and face.

During his sojourn in the United States, Doornik published, in 1829, an article in Benjamin Silliman's *Journal of Science and Arts*,

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Mayr, Ernst (1904–)

Born in Kempten, Bavaria (now Germany), Mayr earned his Ph.D. in biology at the University of Berlin (UB) in 1926. Two years later, as a member of the university's museum, he led the first of three field expeditions (1928–1930) to New Guinea and the Solomon Islands. It was largely this experience that deepened his interest in the question of speciation and geographic distribution of animal species. In 1931, Mayr left Germany for the United States, where he continued his studies at the American Museum of Natural History in New York City as curator of birds—a position he held until 1953. At that time, he moved to Harvard University, where he was installed as the Alexander Agassiz Professor of Zoology; in 1961, he became director of the Agassiz Museum of Comparative Zoology—a position he retained until retiring in 1970. Since then he has been emeritus professor of zoology at Harvard.

Among those who influenced the direction of Mayr's work was Erwin Stresemann (1889–1972), his adviser at UB, who had defended the idea that morphological divergence is independent of physiological divergence (implying that reproductive isolation is the decisive species criterion), and the biologist Bernhard Rensch (1900–), who revived the idea of a correlation between geographic variation of characters and climatic conditions.

In 1942, Mayr published his now classic work *Systematics and the Origin of Species*, a book that made an important contribution to the modern evolutionary synthesis. Specifically, this book is largely devoted to the implications of the biological-species concept from the viewpoint of evolutionary theory and systematics. Mayr stressed that species are inherently variable groups of semi-isolated populations (polytypic species), and that the origin of higher categories can be explained by extrapolation from speciation and intraspecific variation. The biological-species concept required a shift from typological to population thinking (and all that this implied), as well as the adoption of a species definition based on a reproductive gap rather than morphological or ecological differences. Mayr defined species as "actually or potentially interbreeding natural populations, which are reproductively isolated from other such groups." This latter proposition posed the problem of isolation and the process by which the diversity and discontinuity between species is obtained. On this issue, Mayr proposed the idea of geographic or allopatric speciation—that is, "a new species develops if a population which has become geographically isolated from its parental species acquires during this period of isolation characters which promote or guarantee reproductive isolation when the external barriers break down."

Having stressed the polytypic nature of species, Mayr (1954) went on to note and explain the surprising morphological uniformity of most widespread species. Prior to this time, evolution had traditionally been defined as a simple change in gene frequencies. Mayr, however, contended that there was a difference between genetic adjustments in local populations and significant changes during macroevolutionary phenomena, even though all macroevolutionary events can be explained by intraspecific variation (microevolution). These ideas are developed and presented in his *Animal Species and Evolution* (1963a) and the abridged edition, *Populations, Species, and Evolution* (1970).

Mayr's particular contribution to anthropology rests largely with his application of the new systematics to human evolution. In 1950, at the symposium on the "Origin and Evolution of Man" held at Cold Spring Harbor, Long Island, New York, he addressed the issue of the bewildering diversity of names attributed to the fossil hominid remains, arguing for their reduction to a single genus, *Homo*. He also contributed to the movement

for the acceptance of the then controversial South African australopithecines as hominids.

At the species level, and in accordance with its polytypic nature, Mayr recognized the considerable geographic variation among hominid populations, but he defended the view that "never more than one species of man existed on the earth at any one time." He also proposed a unilinear scheme for all ape-like and man-like fossils in human evolution. Succinctly, his views (1) contributed significantly to the integration of a previously typologically oriented field within the new evolutionary synthesis, and (2) launched a taxonomic trend toward lumping rather than splitting.

Richard Delisle

See also Evolutionary Theory; Neo-Darwinism; Paleoanthropology Theory; Systematics (1960–1990s)

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Mendel, Gregor Johann (1822–1884)

Born in Heizendorf (Hyncice) in the province of Moravia (now in the Czech Republic), Mendel began his education at the Gymnasium in Troppau (Opava). In 1840, he enrolled in the philosophy courses at the Philosophical Institute at Olmütz (Olomouc) in preparation for entering the University of Vienna. At the Olmütz institute, he became acquainted with the elements of philosophy, physics, and mathematics. He intended to continue his studies, but his parents were unable to support him. Thus, in 1843, he entered the Augustinian monastery in Brno, where he took the name Gregor. He was ordained in 1847. From 1849 to 1851, he was a teacher of Greek and mathematics in a secondary school at Znaim (Znojmo). Between 1851 and 1853, he attended the University of Vienna, where he took courses in mathematics, physics, chemistry, botany, and zoology. On returning to Brno he taught natural sciences at a secondary school from 1854 to 1868—though he never succeeded in passing the examination for a teacher's license. In 1868, he retired from teaching when he was made abbot of his monastery—a position he retained until his death on 6 January 1884.

The work that led Mendel to the discovery of the basic principle of heredity was the outcome of ten years of laborious experiments with growing and crossing varieties of garden peas (what he called "plant hybridization") in the monastery garden at Brno. According to Mendel, his interest in this subject had been aroused by the desire to produce new colors in his flowers. Artificial fertilization led to surprising results that could not be accounted for in the literature. As such, Mendel decided to investigate the

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Simpson, George Gaylord (1902–1984)

Born in Chicago, Illinois, Simpson grew up in Denver, where as an undergraduate he studied geology at the University of Colorado (1918–1922). He received his Ph.D. from Yale University in 1926. While studying vertebrate paleontology at Yale, Simpson had the opportunity to learn about zoology. In 1927, he became assistant curator in the Department of Vertebrate Paleontology (DVP) at the American Museum of Natural History in New York; in 1945, he was named chairman of the DVP—a position he held until 1959. During this same time period, he was professor of geology at Columbia University. In 1959, he was appointed Alexander Agassiz Professor at Harvard University, a position he retained until 1970. In that year, he moved to the University of Arizona, where he spent the remainder of his career.

Among those who greatly influenced the direction of Simpson's work were his adviser at Yale, Richard S. Lull (1867–1957); the paleontologist William Diller Matthew (1871–1930) of the American Museum, for whom Simpson did fieldwork in 1924; and the geneticist Theodosius Dobzhansky (1900–1975).

Without question, Simpson's major work is the now classic book *Tempo and Mode in Evolution* (1944), a work that contributed to the modern evolutionary synthesis that began to emerge in the late 1930s and early 1940s (cf. Mayr & Provine 1980). In this context, Simpson attempted a synthesis of paleontology and genetics by assuming that some, if not all, mechanisms involved in paleontology are the same as those found in laboratory organisms, and asserting at the same time that only paleontologists can determine whether laboratory principles are sufficient to explain macroevolutionary phenomena. *Tempo and Mode* is, in a way, a system of loosely linked elements providing generalizations about evolution. Simpson argued that the evolutionary rate (or tempo) among fossil groups varies considerably and that population size is an important determinant for explaining it. By linking evolutionary rate, or tempo, with population size and equivalent selective pressure, he loosely linked the tempo of evolution with the pattern, or mode, of evolution—hence explaining his book's title.

In 1953, Simpson published *The Major Features of Evolution*, which can be regarded as an extension of his *Tempo and Mode* text. The most important difference between these two books is his treatment of the concept of quantum evolution. In its earlier form, it was explained in terms of small populations shifting from one adaptive peak to another through a nonadaptive phase, propelled by genetic drift (a nonselective mechanism). In the 1953 work, quantum evolution is depicted as a special case of phyletic evolution involving selective factors—though Simpson still retained the "conviction that the basic processes are the same at all levels of evolution, from local populations to phyla [and that the]. . .circumstances leading to higher levels are special and the cumulative results of the basic processes are characteristically different at different levels" (Gould 1980; cf. Simpson 1953, 1959b).

As a paleontologist, Simpson's contribution comes not so much from specific propositions about human evolution as from the clarification of evolutionary principles and taxonomic concepts. Among the "clarifications" are those he presented at the Cold Spring Harbor Symposia on Quantitative Biology (Simpson 1950b). There he discussed, among other things, the importance of the time dimension for determining phylogenetic relationships, the principles of morphology correlation, parallelism, convergence, divergence, reversibility, and trends. Later, at the Wenner-Gren conference on "Classification and Human Evolution," held at Burg Wartenstein in Austria in 1962 (cf. Simpson 1963), he stressed the biological meaning of taxonomy and its importance as an unambiguous language for scientific communication. In so doing, he noted the prevailing tendency in human paleontology at that time of naming individual fossils without any reference to a population, and the failure therein to understand the biological meaning of taxonomy.

Simpson died in Tucson, Arizona, on 6 October 1984.

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See also Dobzhansky, Theodosius; Evolutionary Theory; Matthew, William Diller; Neo-Darwinism; Punctuated Equilibrium

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Šipka

On 26 August 1880, Karel J. Maška (1851–1916), who is credited by many as a founding force in Czech paleoanthropology and Paleolithic prehistory, recovered a fragment of a human lower jaw near the entrance of Šipka Cave (Schipka-Höhle in German), situated near Štramberk in northern Moravia (Czech Republic). The specimen consisted of the mandibular symphysis of a juvenile with three erupted permanent incisors and three other teeth (the right canine and premolars) still in their crypts. Maška's careful excavation demonstrated a direct association between the human specimen, Mousterian artifacts, Pleistocene animal remains, and traces of hearths. Because the specimen was found during the incipient stages of discussion of the role of Neandertals in human evolution, the Šipka specimen attracted considerable scientific attention.

The first description and assessment of this find was made by the German anatomist-anthropologist Hermann Schaaffhausen (1816–1893) (1880), who, based largely on the robust appearance of the specimen, attributed it to *Homo primigenius* (a term later synonymous with Neandertals). This conclusion was supported by the large size of the anterior teeth and the virtual absence of a mental eminence. However, attribution of the specimen to a Neandertal was immediately rejected by the German pathologist-anthropologist Rudolf Virchow (1821–1902). Virchow (1882) explained the robustness of the specimen by attributing it to an adult modern human characterized by the pathological retention of deciduous canines and molars. Much later, the French paleoanthropologist Marcellin Boule (1861–1942) dismissed it as too fragmentary to be diagnostic (1923:182); whereas his contemporary, the Czech-American physical an-

Chapter 9

Human Phylogeny and Evolutionary Biology During the Second Half of the Twentieth Century

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Introduction

Historical and philosophical studies of the field of human evolution are relatively scarce. On the brink of the 21st century, it is suggested here that a sustained and continued effort at historical and philosophical analyses should be instituted. As part of a host of scientific disciplines that were born during the nineteenth century, the field of human evolution has reached both a sufficient level of maturity and time depth to justify such studies. Recently, historical and philosophical approaches to the fields of evolutionary biology have greatly contributed to current debates. It is the contention of this author that the field of human evolution would greatly benefit from studies concerned with its history and philosophy.

This essay investigates the impact of evolutionary biology on human phylogeny during the second half of the twentieth century. Its purpose is twofold: first, to

contribute to the understanding of the nature of the field of human evolution; secondly, to stimulate further historical analyses and syntheses.

Two main time periods are considered here. 1) The impact of the evolutionary synthesis on human phylogeny from 1950 to 1975. This period is further divided into two sections: a) the impact of the evolutionary synthesis on systematics from 1950 to 1960, and b) the integration of the concept of culture within the realm of the evolutionary synthesis from 1960 to 1975. 2) The impact of cladistics and new developments in evolutionary theory on human phylogeny after 1975.

Some limitations of this essay should be clearly stated. First, it is concerned almost exclusively with the impact of evolutionary biology on human phylogeny, although developments in molecular anthropology are not considered. This should not be taken to imply that evolutionary biology is the only agent acting upon the field of human evolution. Clearly, it is not. Developments in prehistoric archaeology and anthropology are either ignored or are not fully considered here. Secondly, the time periods defined here should not be regarded as rigid or formal, for the impact of the scientific developments during each of them is not necessarily restricted to the time period here discussed, nor are all the scientific developments in each of them considered. Thirdly, although a great number of scientists

have contributed to research on human phylogeny since 1950, it is not possible within the limits of this essay to do justice to them all.

The evolutionary synthesis, systematics and human phylogeny: 1950-1960

During the 1930s and 1940s, it is agreed today that the evolutionary synthesis combined Darwinian evolution with Mendelian genetics, by demonstrating that the raw material of evolution is randomly produced by small genetic changes upon some of which natural selection may act to "direct" evolutionary change. Genetic drift was also sometimes recognized as an important factor of evolutionary change. In this view, evolutionary phenomena at all levels were consistently explained by known mechanisms, that is to say, microevolution explains macroevolution (c.f., Antonovics, 1987: 321-322; Charlesworth *et al.*, 1982: 474; Gould, 1980a: 119-120; Mayr, 1980a: 1). Although this is not the place to get embroiled in debates about the exact nature and structure of the evolutionary synthesis since its inception (c.f., Burian, 1989; Caplan, 1978; Gould, 1983; Mayr, 1980a, 1988; Provine, 1988), suffice it to say that the evolutionary synthesis played a key role in unifying numerous fields concerned with evolutionary studies. Let us stress only two aspects of the evolutionary synthesis.

1) The unity of evolutionary patterns and processes is a

key characteristic of the evolutionary synthesis. Population geneticists set the stage for this by formulating competing models of evolution, such as Fisher's model of large and panmictic populations evolving under natural selection, or Wright's model of populations subdivided into more or less isolated groups evolving under genetic drift and natural selection (Provine, 1978, 1985, 1986). This approach to evolutionary studies, as largely established through Theodosius Dobzhansky's *Genetics and the Origins of Species* (1937; see also Adams, 1994), greatly inspired paleontologists such as George Gaylord Simpson. In his *Tempo and Mode in Evolution* (1944) and *The Major Features of Evolution* (1953), Simpson applied a number of proposed processes to the time dimension in evolutionary change, that is, to evolutionary patterns. For instance, by linking evolutionary rate (or tempo) with population size and various degrees of selective pressure, Simpson loosely linked the rate of evolution with the pattern (or mode) of evolution (c.f., Delisle, 1997a; Eldredge, 1985: 59-83; Gould, 1980b; Laporte, 1983, 1991; Simpson, 1984: xxii).

2) Another key characteristic of the evolutionary synthesis concerns its emphasis on populations, that is, on entities composed of breeding members. This dynamic conception of evolutionary studies instigated profound changes in the view of the process of speciation and the practice of systematics. "A true understanding of natural

selection, speciation, and adaptation", wrote Mayr (1980a: 18), "was not possible until population thinking had displaced typological thinking". In this view, a "new systematics" accompanied the rise of the evolutionary synthesis, one that led to the conception of species as inherently variable entities and as often being composed of semi-isolated populations (polytypic species). Concomitantly, the biological species concept based on reproductive gaps replaced the morphological species concept based on physical differences. These new developments were effectively presented and developed in Ernst Mayr's *Systematics and the Origin of Species* (1942). They helped to reduce the importance of the species level in evolutionary studies since most work is done below the species level (c.f., Eldredge, 1985: 33-59; Mayr, 1980b, 1982: 559-570). The practice of systematics under the evolutionary synthesis was formally presented in Simpson's *Principles of Animal Taxonomy* (1961) and Mayr's *Principles of Systematic Zoology* (1969). The evolutionary synthesis also generated a number of competing models to explain speciation (e.g., Provine, 1989), all of which were inspired by the model of geographic or allopatric speciation.

Human paleontology did not participate actively in the effecting of the evolutionary synthesis. On the contrary, the evolutionary synthesis instigated changes in human paleontology (Delisle, 1995). Symptomatic of this changing

period were some of the presentations made at the *Cold Spring Harbor Symposia on Quantitative Biology* held in 1950, under the theme of *Origin and Evolution of Man*. According to Washburn (1983), who co-organized this symposium with Dobzhansky, it was designed to bring genetics into the realm of physical anthropology and to stress the importance of population thinking rather than typological thinking. As early as 1944, in an article entitled *On Species and Races of Living and Fossil Man*, Dobzhansky (c.f., Delisle, 1997b) showed how genetics could be applied to hominid fossil populations by arguing that morphological species and races are inferences to genetical species and races. He put this concept to work by contending, among other things, that Peking and Java fossil men were representatives of two different races and not of distinct species, for the extent of their differences was well within that of living human races. On these grounds, Dobzhansky (1944: 261-262) at first defended the idea that "no more than a single hominid species existed at any one time level", meaning that hominid evolution has been "taking place within a single genetic system".

Mayr's (1950; c.f., Delisle, 1997c) contribution to this symposium came from his application of the new systematics to human evolution. The problem of human origins, he insisted, depended largely on the proper definition and evaluation of taxonomic categories. Mayr boldly suggested

that the bewildering diversity of names attributed to fossil remains (*Australopithecus*, *Plesianthropus*, *Paranthropus*, *Pithecanthropus*, *Sinanthropus*, *Paleoanthropus*, to name a few) should be reduced to a single genus, *Homo*. The genus is biologically meaningful (up to a certain level, see below) and is defined accordingly to its occupation of a specific adaptive plateau: the acquisition of the upright posture was the key generic character of *Homo*. Thereby, the genus attained a new adaptive zone which led to an increase in selective pressure and accelerated evolutionary change that resulted in a gradual transformation (phyletic speciation) to *Homo sapiens*. Following Dobzhansky, Mayr (1950: 112) at first held that "never more than one species of man existed on the earth at any one time". However, from the polytypic nature of some species, he recognized the existence at any one time of geographical variation among populations of this lineage.

Therefore, the distinction between population and typological thinking was, in the 1950s, an important issue in human paleontology. According to Mayr (1969: 24-29, 67), the typological definition of species assesses the variability of individuals as based on the existence of a limited number of types in nature. Hence variation is considered as trivial and irrelevant, whereas the biological definition of species assesses the variability of individuals as flowing from their membership of a

reproductive community, an ecological unit, and a genetical unit (for a somewhat different assessment see Sober, 1980). Accordingly, typologists would tend to exaggerate the constancy of taxa and the sharpness of the gaps separating them. It is thus easy to understand the phylogenetic consequences of both forms of species definitions, for typologists would be more inclined towards a multilinear scheme or a taxonomic inflation, whereas populationists would be more inclined towards a unilinear scheme or a taxonomic reduction.

Also marking this changing period, Washburn (1951) opposed an old physical anthropology dominated by a static attitude with an emphasis on typological classification, to what he called "the new physical anthropology" seeking to understand primate evolution and human variation in conformity with modern evolutionary theory. "Having passed through its initial descriptive phase, physical anthropology is now entering into its analytic stage" (Washburn, 1953: 715). Instead of the presence or absence of a particular morphological trait being noted, the understanding of the process by which this trait was produced leads to a different evaluation of it:

> "If these two skulls were fossil men, differing in the same way, and if they were treated by the usual anthropological methods, they would be found to differ in numerous observations, measurements, and indices. Yet one may be transformed into the other by a simple reduction in mass of face... Perhaps many fossils are far less different than we have supposed (Washburn,

1951: 303)."

In a logical continuation of Washburn's work and its encouragement of the understanding of processes, Bartholomew and Birdsell (1953) presented an ecological approach to the study of hominid populations, and proposed to explore concepts like "spatial variation of population density", "growth or decline in numbers", "rates of movement as influenced by migration and gene flow", and "shifts of populations into new climatic situations". In this perspective and although not discarding the possibility of a rapid and dramatic replacement of one fossil group by another, Bartholomew and Birdsell thought it more likely that:

> "population change must have resulted from gradual genetic penetration, and much of human evolution in the Pleistocene could easily have been powerfully affected by introgressive hybridization. In this regard it should be remembered that anatomical differences do not necessarily indicate genetic incompatibility between groups... (1953: 495)."

All these changes related to the new systematics, genetical concepts, processes of variation and ecology, favour a taxonomic reduction towards fewer hominid species with more intraspecific variability. The application of these changes under the influence of a renewed evolutionary biology, gave an alternative to the typological species concept with its static and descriptive approach to

morphology. Typologists tend to favour taxonomic inflation by using the type concept and by defining taxa on the basis of specimen fossils. Population thinking and typological thinking, in this context, directly influence the assessment of human phylogeny. This is clearly detectable in the standpoints of some authors.

The new systematics provided one of at least two ways to promote population thinking in human paleontology in the 1950s. The changing interpretation of Le Gros Clark is illustrative of such an impact on human phylogeny. At first, Le Gros Clark (1954) recognized that a speciation occurred between *Homo sapiens* and *Homo neanderthalensis* from a type possibly not clearly distinguishable from *Homo sapiens*. He thus defended the opinion that a specific distinction should be retained. Again while he proposed that all australopithecine fossils should be lumped into the single genus *Australopithecus*, it was considered also that "there may be more than one species" (Le Gros Clark, 1954: 389). Later, Le Gros Clark (1958/1962) recognized that the tendency for the taxonomic individualization of almost every fossil, by attributing each of them to a new species or genus, complicated unnecessarily the picture of human phylogeny. Instead, he (1958/1962:352) proposed that the genus *Australopithecus*, like the genus *Pithecanthropus*, shows "a considerable range of variation in skull form and cranial capacity" and that this genus is not composed of

different species but possibly of regional varieties of a local nature comparable to the racial varieties found in modern *Homo sapiens*. Also, the Early Mousterian populations of Europe would represent the variability of a single taxonomic group that "provided the genetic material for a subsequent diversification on the one hand into the extreme (or 'classical') Neanderthal type of later Mousterian date, and on the other into the modern races of *Homo sapiens*" according to Le Gros Clark (1958/1962: 353).

Similarly, criticizing very explicitly the typological way of thinking about taxonomy, Breitinger (1957/1962) contended that fossil hominids are still partly classified in a formal manner by the use of taxonomic names as mere labels without any biological meaning. "The only safe way out", proposed Breitinger, is by "...getting in step with the new knowledge and principles of taxonomy, which have led to a biologically based definition in place of the morphological definition of species" (1957/1962: 445). From the perspective of the new systematics, held Breitinger (1957/1962: 453), the phyletic species *Homo neanderthalensis* should exhibit polytypy in space and time comprising geographical "races of the 'extreme' forms of Neanderthal in Europe, Africa..., and Asia..., and in geochronological range all the populations represented by the 'generalized Neanderthal' and the 'Preneanderthals'".

Population thinking can be applied not only through the

new systematics, but also with the assistance of genetical and ecological concepts. For instance, Howell (1951) envisioned the Neandertals as a morphologically and temporally heterogeneous population, an important consideration in phylogenetic discussion, for early Neandertals were considered to be more similar to modern man, while the later classic Neandertals had "a pattern peculiar to, and distinctive of, a geographically isolated group" (1951: 406), and subjected to extreme environmental conditions. Under those circumstances, "selection would be severe, chance for genetic drift at an optimum, and opportunities for migration reduced to a minimum" (1951: 409). Accordingly, Howell's analysis pointed to the conclusion that *Homo sapiens* possessed an ancestral relationship with the early Neandertals, but not to the later classic Neandertals, and so he suggested that this later group stemmed from a sharp break. "The concept of a 'Neanderthal' phase or stage of man's evolution", wrote Howell, "is no longer useful since there was a marked variability from one such group to another" (1957: 343). Howell's analysis is of interest for it shows how population thinking is not necessarily associated with a unilinear scheme, but points nonetheless to a taxonomic reduction since he found no need to postulate the origin of modern humans and classic Neandertals from different and remote ancestors, but from a single variable or polytypic one.

In contradistinction with previous authors, typological thinking continued to be actively applied to the assessment of human phylogeny during the 1950s. It seems to have been defended on at least two different but closely related grounds. The first one was based on little reference to the concept of population, and focused the analysis of taxa on individual fossil specimens. This approach was often applied in conjunction with parallel evolution in the so-called radiation hypothesis. For instance, Heberer (1959) depicted a highly bushlike human phylogeny with several evolutionary lines at the level of the australopithecine plateau (earliest bipedality), and at least three evolutionary lines at the level of the *Homo* plateau (big brain and culture), namely the *Pithecanthropus-Sinanthropus* line, the paleoanthropine sequence including the Neandertal line, and the *sapiens* line (*Homo sapiens* having early representatives). According to Heberer, many discoveries could not be accommodated in these clearly recognizable lines, for he wrote that "many 'Neanderthal Men' were discussed which by no means fit in the Neanderthalian line" (1959: 242). The *Homo* plateau would, according to Heberer, illustrate a radiation hypothesis.

In the same perspective, Vallois (1954) favoured the Presapiens theory (*Homo sapiens* having early representatives) based on the early, modern-looking, individual specimens of Swanscombe (England) and

Fontéchevade (France), specimens apparently lacking the supra-orbital torus characterizing the Neandertals. From this, Vallois established the fact that "the European *Homo sapiens* is not derived from the Neanderthal men who preceded him" (1954: 128). Vallois's multilinear scheme is inspired by the frequent pattern of parallel evolution found among mammals after an adaptive radiation, a mode of evolution he claimed to be also applicable to fossil men where three simultaneous lines during the Mindel-Riss epoch are encountered with the men of Ngandong (as then dated), the Preneandertals, and the Presapiens (Vallois, 1958/1962).

The second basis on which typological thinking was applied in human paleontology in the 1950s, was the attribution of little biological meaning to taxonomic categories and procedures. For L.S.B. Leakey, the taxonomic debate over the generic status of the australopithecines was one which is relative and does not seem to be meaningful from a biological viewpoint. He assigned the newly discovered skull at Olduvai in 1959 to the proposed new genus and species *Zinjanthropus boisei*, since it "differs from both *Australopithecus* and *Paranthropus* much more than these two genera differ from each other" (1959: 491). In contrast, Robinson assigned this new skull to a separate species *Paranthropus boisei*, because "separate generic status seems unwarranted and biologically unmeaningful" (1960: 458), while later Tobias (1967) carried this

reductionism further and assigned this specimen to the genus *Australopithecus* and the species *boisei*. Taxonomic issues were for Leakey arbitrary, since he stated that "inevitably, different scientific workers have different ideas of what characters justify specific, generic, and even superfamilial rank. After all, this is purely a question of artificial labels" (1960: 458). This last statement is not without resemblance to the one made earlier by Broom (1950: 13) about the australopithecines:

> "The classification of ape-man will be as difficult as that of man is today. There can be no finality. 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."

This vision of the arbitrariness of the taxonomic categories makes phylogenetic interpretations difficult, if not impossible, to assess. Interestingly, a large part of the debate over the australopithecines during the 1950s centred around generic questions, although the genus is a taxonomic category less directly involved in questions of populations and genetics than the species category. On that count, Mayr (1969: 91) explained that "the category species is 'self-operationally' defined by the testing of isolating mechanisms in nature while non-arbitrary definitions for the supra-specific categories are not available".

The evolutionary synthesis, culture and human phylogeny: 1960-1975

The 1960s saw in human evolution the integration of the concern for evolutionary processes (e.g., adaptation), as derived from the evolutionary synthesis, with the concept of culture. The impact of this integration on human phylogeny can best be explained by the association of two ideas: 1) The competitive exclusion principle states that no two species can occupy the same ecological niche because competition would prevent one or other species from surviving. 2) Cultural ability was considered to be a most efficient means for opening ecological niche exploitation. Mayr first set the stage for this in 1950, by importing the competitive exclusion principle in human paleontology:

> "What is the cause for this puzzling trait of the hominid stock to stop speciating in spite of its eminent evolutionary success? It seems to me that the reason is man's great ecological diversity. Man has, so to speak, specialized in despecialization... If the single species man occupies successfully all the niches that are open for *Homo*-like creatures, it is obvious that he cannot speciate... Man is apparently particularly intolerant of competitors. The wiping out or absorption of primitive populations by culturally more advanced or otherwise more aggressive invaders... has presumably happened many times...(Mayr, 1950: 116)."

Here, Mayr explains clearly the basis of the assertion he made in the 1950s that only one species of man has ever existed at any one time level. Taking the matter further, Robinson agreed with Mayr that the ability for cultural

adaptation was a powerful deterrent to speciation, but he drew a crucial distinction:

> "In the very early stages of euhominid-hood this capacity for artificial adaptation was slight compared to that of the present time... It seems to me, therefore, that one cannot agree with Mayr that in the early stages of euhominid development intraspecific variability was even greater than at the present time. The australopithecines had not yet reached the artifact-manufacturing stage and were, on the whole, rather small brained... Their evolutionary pattern would therefore have conformed more closely with that of non-hominids, and ordinary mammalian taxonomic standards would apply to them but not so well to euhominids (Robinson, 1954: 182-183)."

The application of the competitive exclusion principle in human evolution takes here a precise form. For Robinson, the australopithecines with their low artifact-manufacturing level were not eligible for the competitive exclusion principle. Their morphological variability was, consequently, attributable not to a single species, but to several. On this thinking, the competitive exclusion principle in human evolution is to be applied on the basis of a cultural threshold or a certain artifact-manufacturing level. It defines the applicability or the non-applicability of that principle after or before a certain point in cultural evolution. Debates over such a criterion played a central role in the assessments of human phylogeny between 1960 and 1975.

Such debates brought human paleontology, prehistoric archaeology and ethnology in very close contact during this

period, culture being the unifying concept. The symposium *Man the Hunter* (Lee and DeVore, 1968) testifies well to the effort to integrate all these avenues. Cultural factors under diversified forms, such as the hunting hypothesis, gained crucial importance for human evolution. For instance, it was widely held that hunting as a way of life instigated most traits that made us human. Inherent in this approach was the vision that tool-making or tool-using hominids were living in fairly similar social organization to modern hunter-gatherers. The debate at the time centred around the question whether all hominids, australopithecines included, were more like modern humans, or whether only the genus *Homo* was eligible for this title. Policing the animal-human boundary was therefore an underlying theme of the period (Cartmill, 1993; Cartmill *et al.*, 1986).

The association of the competitive exclusion principle with the capacity for cultural adaptation, explains why only a single, culture-bearing, hominid lineage should have subsisted on earth. Researches on the unilinear hypothesis, based on such premises, were prosecuted in the 1960s. Under such an hypothesis, morphological variability was interpreted as being intraspecific, the consequence of sexual dimorphism, individual and population variation, allometry, etc. Brace (1964) depicted human evolution in four stages within a single lineage: australopithecine-pithecanthropine-Neandertal-modern man. Those morphological

changes were correlated with cultural changes. He defended the view that culture was the principal adaptive mechanism changing the selective pressure acting on morphology. The variation found among the Neandertals was, according to him, not outside the range of modern man. At the australopithecine stage, increased cultural adaptation through the proposed "mutation effect" led to a reduction of the anterior dentition and an increase in body size. At the end of this latter phase, a single successful hominid species spread throughout the southern part of the Old World and provided "the base for all subsequent human evolution" (1967: 70). Brace's conception of human evolution as having proceeded in a single lineage was rooted in his recognition of the importance of a cultural niche. It also relied on the apparent presence of a single cultural tradition. He wrote:

> "...not only is culture a single ecological niche within which one would expect to find only a single species, but since there is only one cultural tradition visible in the early Pleistocene, the probability is greatly increased that there has been only one hominid species at any one time, and that the hominids of different time levels are lineally related (1967: 56)."

This unilinear scheme of human evolution came to be designated the "single-species hypothesis" by Wolpoff (1968). Culture was envisaged as a multiplier of usable niche exploitation making long sympatry of different culture-bearing hominids impossible. Wolpoff also defended

the view that, morphologically speaking, there was no solid basis to support a separation between gracile and robust australopithecines since, even if there were some differences, individual variations overlapped extensively, while there was no indication of differences of adaptation based on diet, culture or osteological features except inferred body size (Wolpoff, 1971a, 1973). On that basis, he explained australopithecine variations as intraspecific differences. Also opposed to the recognition of several lineages during the later stages of human evolution, Wolpoff argued that fossil specimens such as Swanscombe (England), Vértesszöllös (Hungary), and Fontéchevade (France) are not early *Homo sapiens* as the presapiens hypothesis held, but forms documenting a continuum from *Homo erectus* to Neandertals, and hence confirming the presence of a single hominid lineage during the Middle and Upper Pleistocene (Wolpoff, 1971b; Brose and Wolpoff, 1971).

The debates over the significance of culture, as represented by artifacts found in deposits, became crucial for the determination of the number of contemporaneous lineages. Such debates were especially acute where the australopithecines were concerned. On the one side, some defended, after Dart, a significant culture among this fossil group (e.g., Ardrey, 1961; Brace, 1967; Wolpoff, 1968). Dart wrote that:

"...Makapansgat bone-breccia demonstrates the activities

> of carnivorous creatures, whose counterpart can be found only in the bone-deposits formed by other primitive human beings... We must therefore keep open minds about the possibility of *A. prometheus* [*A. africanus* from Makapansgat] being shown in the future to have used stone as well as osteodontokeratic tools...(1957: 171)."

On the other hand, some opposed the idea of a significant culture among this fossil group (e.g., Coon, 1962; Robinson, 1954; Tobias, 1965; Washburn, 1963). Washburn contended that:

> "...the brown hyenas in Kruger Park collected the heads of medium sized antelopes, baboons, and few carnivores. This is the kind and distribution of bones found in the australopithecine deposits. Combined with the fact that hyena coprolites have been found in the deposits, this makes it probable that the australopithecines were themselves the game, rather than the hunters (1957: 614).

In fact, at the early stages of hominid evolutionary studies, detractors of the single-species hypothesis could defend the existence of several lineages by discarding the importance of the material culture, or by contesting the association between artifacts and the australopithecines. As previously discussed, Robinson (1954) proposed to judge the australopithecines by non-hominid and mammalian taxonomic standards for he believed they had not reached the artifact-manufacturing stage. He (1960, 1961) recognized two distinct genera exploiting divergent adaptive avenues with, on one hand, *Paranthropus* on an evolutionary path towards extinction, and on the other *Australopithecus* on an

evolutionary path towards the genus *Homo*. Subsequently, however, Robinson (1963) abandoned this view and came to consider that the *Paranthropus* line represented the primitive condition of australopithecines with its anatomical shift from quadrupedal to bipedal locomotion, a primitive condition which eventually gave rise to *Australopithecus*, by a speciation process, in response to changing environmental conditions and adaptation to a new diet placing a premium on tool-using. Such speciation was made possible by a lack of cultural factors. Robinson wrote:

> "The Australopithecines in the initial phases of their evolution will have had no more than a rudimentary level of culture, hence the slowing down of the rate of speciation will not have applied. In the later phases of their evolution cultural development may have been sufficient to make its effect felt to some extent (1963: 394-395)."

Tobias also proposed a bilinear scheme of this fossil group, but with some distinctions. Contrary to Robinson's interpretation, Tobias (1965, 1968) was inclined to favour the view that the primitive condition of the australopithecine stage was an unspecialized form with small teeth and an omnivorous diet. This scheme implied that this unspecialized form then diversified in two principal directions, one along specialized macrodontic and megadontic lines through the *A. boisei- A. robustus* lineage, and the other along an unspecialized and little changed line

comprising *A. africanus- H. habilis- later Homo*. To defend at least one speciation at this stage of hominid evolution, Tobias doubted the cultural production of the australopithecines by arguing that:

> "...if we make a survey of all the evidence from South and East Africa, we see that *Australopithecus* alone has not yet been found with stone objects which are undoubtedly tools, except where advanced hominid remains were present as well... [I]t seems very probable that *H. habilis* was the maker of the Oldowan stone tools, while *H. erectus* made later (Chelles-Acheul) implements (1965: 30)."

Tobias did not strip the australopithecines of all cultural ability, on the contrary, for he specified that they were tool-users and possible tool-modifiers (the osteodontokeratics being one possible cultural outlet) whose survival depended on implemental activities. However, he contended that they "fell short of one intricate conceptual and technological mechanism: the ability to use a tool to make a tool" (Tobias, 1967: 239). The australopithecines, and most notably the robust forms, were, in Tobias's (1967: 244) opinion, "a line which was to prove unequal to the rigorous challenge of highly competitive hunter-hominines".

Contrary to discussions on culture in the early stages of human evolution, debates concerning the significance of culture during the later stages were almost unanimous:

> "Wherever archeological evidence can suggest the beginnings of movement over large ranges, cooperation, and sharing, it is dating the origin of some of the most

> fundamental aspects of human behavior - the human world view. We believe that hunting large animals may demand all these aspects of human behavior which separate man so sharply from the other primates. If this is so, then the human way appears to be as old as *Homo erectus* (Washburn and Lancaster, 1968: 297)."

This position about the later stages of human evolution was based on archeological evidence from sites such as Zhoukoudian (China), Olduvai (Tanzania), Terra Amata (France) and Torralba/Ambrona (Spain). Some of these sites documented hominid skeletal remains, and also various types of tools, traces of hunting and butchering, and construction of shelters (e.g., Howell, 1967, 1977; de Lumley, 1969).

The two views on culture in relation to hominid evolution could lead interestingly to dual views on human phylogeny, one envisioning the genus *Homo* in a unilinear fashion and culture-bound, and the australopithecines in a multilinear fashion and not culture-bound (according to the detractors of a significant culture at this stage of hominid evolution). Morphological variability is therefore interpreted either as intraspecific or as interspecific.

By the middle of the 1970s, the single-species hypothesis as applied to the entire hominid fossil record was largely discarded, in the minds of a vast majority of scientists, because of empirical evidence. Numerous students of human evolution dealing with the early stages defended, in the 1960s and early 1970s, at least one speciation (e.g., Campbell, 1963; Dobzhansky, 1963;

Gutgesell, 1970; Leakey, 1966, 1971; Mayr, 1963; Pilbeam and Zwell, 1972; Robinson, 1963, 1972; Tobias, 1965, 1968). This was based on the fact that several evolutionary lineages were potentially synchronic, if not simply sympatric. Among those potentialities were *Meganthropus* and *Homo erectus* in Java, *"Zinjanthropus"* (*Australopithecus boisei*) and *Homo habilis* at Olduvai, *Australopithecus robustus* and *Homo erectus* (*"Telanthropus"*) at Swartkrans, and finally *Australopithecus* and *Paranthropus* at maybe several sites in South and East Africa (Leakey *et al.*, 1964; Tobias, 1965).

Proponents of the single-species hypothesis were able, during the 1960s, to defend their unilinear scheme throughout all stages, on the basis of the competitive exclusion principle and the capacity for cultural adaptation. In this sense, the empirical data of the period were equivocal, in that alternative interpretations were defensible on varying theoretical, conceptual and methodological grounds.

However, the application of the single-species hypothesis broke down with the discovery in 1973 of two morphologically very distinct specimens undoubtedly contemporaneous in East Africa, one (KNM-ER 3733) resembling the *Homo erectus* specimens of Beijing and the other (KNM-ER 406) resembling the very robust type of australopithecine (Leakey and Walker, 1976).

Afterwards, the most vocal proponents of the single-species hypothesis such as Brace and Wolpoff recognized one speciation at this earlier stage of hominid evolution, although they maintained their view for later stages. Both of them attributed the ineffectiveness of the competitive exclusion principle to the fact that culture was a less effective means of adaptation at this earlier stage than at later ones (Brace, 1979a: 55, 64-65; Wolpoff, 1980a: 154, 181).

Although a number of scientists defended a multilinear scheme of the genus *Homo* during the 1960-1975 period (e.g., Leakey, 1966, 1971), an increasing majority of them favored a more or less unilinear scheme under the influence of the evolutionary synthesis (e.g., Campbell, 1962, 1963, 1972; Coon, 1962; Howells, 1967; Le Gros Clark, 1964; Pilbeam, 1972, Piveteau, 1973; Thoma, 1971, 1973, 1975; Tobias, 1968, 1974).

Human phylogeny, cladistics and some developments in evolutionary theory after 1975

The concept of the influence of culture on human evolution was sometimes severely criticized by 1975. The hunting hypothesis was assaulted from several standpoints:

1) Based on tools and male dominance the hunting hypothesis was opposed by a growing emphasis on the role of females (Tanner and Zihlman, 1976; Zihlman, 1978).

2) The re-examination of key archeological sites thought to document the hunting hypothesis such as Olduvai, Torralba/Ambrona and Zhoukoudian, led to serious doubts that cutmarks on bones deemed to have been caused by butchering activities were of frequent occurrence (Binford and Ho, 1985; Binford and Stone, 1986; Potts and Shipman, 1981; Shipman, 1986; Shipman and Rose, 1983).

3) Methodological criticisms were formulated regarding the difficulty of inferring hunting behavior from the archeological material (Binford, 1981, 1987).

More generally, it was argued that most hominids were less modern in their behavior than was previously thought (Binford, 1985; Foley, 1987; Klein, 1995).

These assaults could, in theory, weaken the importance granted to culture as a factor in human evolution. Accordingly, since the later stages of human evolution were based, in part at least, on culture as an adaptation that permitted *Homo erectus* to inhabit a large part of the Old World, and avoid speciation through gene flow and the competitive exclusion principle, these assaults could indirectly open the door to a taxonomic inflation or a multilinear scheme.

This effect, however, was clearly generated by an approach to taxonomy and phylogenetic reconstruction called phylogenetic systematics or cladistics. Although cladistics is often considered to be a method concerned exclusively

with the establishment of some aspects of the evolutionary pattern, it is sometimes envisioned also as a research programme concerned with the evolutionary process. As such, cladistics, like most scientific movements, is neither immutable nor monolithic (Hull, 1979, 1984, 1988: 232-276).

It is not possible within the limits of this essay to present all the aspects that govern the practice of cladism (cf. Hennig, 1966; Eldredge and Cracraft, 1980; Nelson and Platnick, 1981; Wiley, 1981). Only a number of them are considered here.

At the heart of the cladistic analysis is the aim to establish the degrees of "phylogenetic relationship" within a given group of taxa (say taxa A, B and C) by evaluating which two taxa are more closely related to each other than either is to the third one. The end result is the expression of relationships in terms of relative recency of common ancestry. This is expressed as sister-group relationships in a diagrammatic form called a cladogram (Hennig, 1965, 1966; Schaeffer *et al.*, 1972). Phylogenetic relationships are established by distinguishing among primitive (plesiomorphic), shared derived (synapomorphic) and uniquely derived (autapomorphic) characters. The sharing of derived characters and not primitive ones is regarded as indicative of an evolutionary relationship, for two taxa may share primitive characters only because they have not yet lost them, and not because they are closely

related.

What is the meaning of dichotomies on a cladogram? Although cladists have always maintained that this is only a methodological principle and not an empirical claim, an ambiguity or a confusion has reigned in the writings of a number of cladists, especially during the 1970s (e.g., Brundin, 1966, 1972; Cracraft, 1974; Eldredge, 1979a; Hennig, 1966; Patterson, 1980; Platnick, 1979; see also Hull, 1979, 1984, 1988: 232-276). To quote Hull (1979: 425) on this:

> "Although no cladist has ever maintained that the principle of dichotomy is an empirical claim about the speciation process, few cladists have been able to resist the temptation to justify it by reference to empirical considerations".

This has prompted criticisms to be directly aimed at questions pertaining to evolutionary patterns and processes alike, on the part of opponents of cladistics, and not only at methodological principles (e.g., Bretsky, 1979; Halstead, 1981; Mayr, 1974; Simpson, 1975; Szalay, 1977).

It is in this context that cladistics was first introduced in the field of human evolution. According to Eldredge and Tattersall (1975), the methodology in paleontology had been almost exclusively dedicated to an evolutionary model known as phyletic gradualism (gradual transformation). They asserted that phyletic gradualism was not the only evolutionary process, but that it might be

subordinate to the more general picture of evolutionary change furnished by the model of speciation (punctuated equilibrium). Consequently, if it were accepted that speciation was the more general picture of evolutionary change, then we would require a different method of phylogenetic reconstruction. On this view, it was argued that shared primitive characters shed no light on the nature of the relationship between two similar taxa. What was needed was a precise method of phylogenetic reconstruction based on derived characters that permitted one to detect morphological gaps or changes in the fossil record, because, argued Eldredge and Tattersall (1975: 228-229), while "phyletic gradualism produces a smoothly continuous intergradation among successive taxa, speciation only approximates a continuum by producing a series of steady-state taxa separated, among true sister species, by rather small, yet discrete, morphological gaps".

In fact, cladistics was often entangled in a wider scientific context. In contradistinction to researchers' themes in evolutionary biology prior to 1970, a number of developments during the 1970s and early 1980s favored a renewed interest in species, speciation and evolutionary events at and above the species level. This situation was contrasted by Eldredge (1979b) who distinguished between the "transformational approach" and the "taxic approach". He insisted that the former was committed to the idea that

lower level mechanisms as recognized by population genetics (i.e., natural selection and adaptation) sufficed to explain all evolutionary events at the species level and above (e.g., Bock, 1970, 1979; Charlesworth *et al.*, 1982). The "taxic approach", on the other hand, recognized that species' diversity (and that of genera, families etc,) was a valid subject of study in itself, since based on specific but not necessarily distinct evolutionary processes during speciation. Among the most important developments in evolutionary biology during that period were the following:

1) The suggestion that species were not classes composed of individual organisms but could be regarded as entities or individuals themselves. A new ontology of species defined as spatiotemporal entities was introduced to replace the one based on arbitrarily delineated segments of a continuum (Ghiselin, 1974; Hull, 1976).

2) Patterns observed at the species level and above (macroevolution) were not exclusively, maybe not predominantly, the result of processes below the species level (microevolution); species selection being possibly the main macroevolutionary process (Stanley, 1975, 1979: 181-212; Vrba, 1984; Cracraft, 1985).

3) The evolutionary theory based on the extrapolation of microevolutionary processes to explain macroevolutionary patterns should be replaced by one embodying explicitly the notion of hierarchy, that may or may not involve distinct

processes and patterns at each level (Gould, 1980a; Vrba and Eldredge, 1984; Eldredge, 1985; Vrba and Gould, 1986).

Although these developments arose more or less independently of one another, they sometimes came to be incorporated in a single but complex conceptual network (e.g., Gould, 1980a; Cracraft, 1981a; Eldredge, 1985: 117-138; Depew and Weber, 1995: 359-391). It was debated whether these developments and others could be incorporated within the existing structure of evolutionary theory, the evolutionary synthesis (e.g., Gould, 1980a, 1982; Stebbins and Ayala, 1981; Maynard Smith, 1983; Eldredge, 1985; Burian, 1988; Mayr, 1988). The formulation of the theory of punctuated equilibria (Eldredge and Gould, 1972) played a key role in triggering a number of these developments in evolutionary biology. At first, though, this theory was considered to be entirely compatible with the existing structure of evolutionary theory, being restricted solely to the empirics of the fossil record. It was essentially in such a form that it was debated in human evolution (e.g., Cronin *et al.*, 1981; Rightmire, 1981; Wolpoff, 1984; Tobias, 1985). By the end of the 1970s, however, punctuated equilibrium often played an active part in the complex conceptual network described above (Gould, 1992; Hoffman, 1992; Ruse, 1992; Gould and Eldredge, 1977, 1986, 1993).

Cladistics, when first introduced in the field of human evolution, seemed to be intimately related to the theory of

punctuated equilibria, although it should be noted that Gould, one of the founders of punctuated equilibria, specifically denied that he was a cladist. By the early 1980s, the key elements of this wider scientific context, that is, the theory of punctuated equilibria, species as individual or entities, species selection, the theory of evolution as a hierarchic theory and cladistics, were all being applied to human evolution (e.g., Eldredge and Tattersall, 1982).

The use of cladistics in this context favored the recognition of more species and speciation events in the hominid fossil record. For instance, Tattersall and Eldredge (1977; see also Eldredge and Tattersall, 1975) proposed two phylogenetic possibilities when considering the genus *Homo*: 1) *H. erectus*, being directly descended from *H. habilis*, could have speciated on one hand into *H. sapiens sapiens* and on the other into the Neandertal forms; 2) *H. habilis* could have been the source of two lineages, one leading to *H. sapiens sapiens*, while the other led to *H. erectus* and speciated again into the Neandertal forms. Later on, Tattersall (1986, 1992, 1995) was more specific about the taxa he recognized in the genus *Homo*: *Homo rudolfensis*, *H. habilis*, *H. ergaster*, *H. erectus*, *H. heidelbergensis*, *H. neanderthalensis*, *H. sapiens* and possibly *H. steinheimensis*. He (1995: 234) inserted these taxa in a phylogenetic tree that specified a number of

speciation events. According to Tattersall (1986: 166, 169), the field of human paleontology had been dominated by the "triumph of the lumping ethic" and the ascendance of "a destructively minimalist approach to hominid systematics" in the last three and a half decades. It seemed that this so-called "lumping ethic" or taxonomic reduction had been more acute at the level of the genus *Homo* because, continued Tattersall, it had begun to be realised that a significant degree of taxonomic variety existed among australopithecines, while the genus *Homo* had been expanded to cover two million years with numerous distinct morphs (morphological clusters) but was viewed as containing only three species: *Homo habilis*, *H. erectus*, and *H. sapiens*.

The premises of some cladists, at least when applied in human evolution, were pointing to a taxonomic inflation, because morphological clusters or morphs were identified, which in turn were equated to species. It must, however, be specified that cladistics has sometimes been explicitly used for subspecific categories (e.g., Hublin, 1985, 1986). These morphs or species were frequently inserted in multilinear schemes because it was often asserted that the pattern of speciation reflected the more general picture of evolutionary change.

If there was an ambiguity or a confusion in the cladistic approach between methodological principles and empirical claims, this situation became somewhat clarified

starting at the end of the 1970s. From then on, cladistics became, in the minds of numerous scientists, simply a *method* of classification and phylogenetic reconstruction to be kept distinct from claims about evolutionary processes. In fact, it may be possible to recognize a split among proponents of cladistics after that period (e.g., Charig, 1982; Hull, 1983, 1984, 1988: 232-276). One group seemed essentially concerned with phylogeny reconstruction by establishing some aspects of evolutionary patterns independent of any notion of processes. From there, evolutionary processes may or may not be sought to explain the uncovered patterns. The other group of cladists, who will not concern us here, seemed solely concerned with the establishment of patterns, not only independent of any evolutionary processes but also independent of evolution in general. The detection of patterns in nature, should they be generated by biological evolution or any other causes, was the object of this approach, which was sometimes referred to as "pattern" or "transformed" cladistics. The nature or validity of such distinctions among cladists are still presently debated (e.g., Brooks and Wiley, 1985; de Queiroz and Donoghue, 1990; Nelson, 1989; Platnick, 1985).

There has been, therefore, a clear tendency since the late 1970s to view cladistics solely as a method of classification and phylogeny reconstruction. Statements to such an effect abound in the literature, and it has often

been expressed in the following terms: cladistics does not attempt to reconstruct the actual course of evolution for it is only concerned with degrees of relatedness (Schoch, 1986: 36-37). Or, the sharing of derived characters indicates an evolutionary relationship whose exact nature is unspecified and beyond which the cladogram carries no implication (Tattersall and Eldredge, 1977: 204). It may be possible to find contributions in human evolution that apparently come close to this practice (e.g., Chamberlain and Wood, 1987; Stringer, 1987).

However, the elaboration of cladograms is rarely, if ever, a means in itself, at least in human evolution. When used, cladistics is often so as a method to investigate the actual course of human phylogeny, by establishing first a cladogram and then by using it as a guide to propose a phylogenetic tree. Tattersall and Eldredge (1977), after Gareth Nelson, distinguished between a cladogram and a phylogenetic tree, and insisted that the latter adds to the information contained in the former by specifying the nature of the evolutionary relationships postulated: ancestor-descendant relationship (linear evolution) or daughter species relationship derived from a common ancestor (speciation). In this view, it is clear that a cladogram is not a diagram about the actual course of evolution, because a number of phylogenetic trees can be generated from any one cladogram (see also Platnick, 1977). Yet, in the hands of

some scholars, the cladogram/tree distinction has been narrow in human evolution. This may be more easily explained by one considering the genus *Homo*.

We have already mentioned that since the 1960s the genus *Homo* was considered by numerous scientists to be represented by a single, polytypic lineage evolving towards modern humans. Let us imagine for a moment that a cladist were to analyse a segment (temporal slice) of this lineage. He might well argue that it did not represent a single population, because the constituent fossil specimens were not all bound together by the same derived characters (apomorphies), but instead were united by a combination of primitive and derived ones. On that basis, our cladist might recognize two distinct species rather than a single one in this temporal slice. In this particular instance which defines taxa cladistically, the recognition of a dichotomy on a cladogram leads directly to the recognition of a speciation event. It should be mentioned that when taxa are defined cladistically, this is often done only in terms of possession of unique character states (autapomorphies), although not always (e.g., Wood, 1984, 1993, 1994: 153).

The impact of the cladistic method on the reconstruction of phylogenetic patterns can also be seen when relationships among taxa are investigated on the basis of uniquely shared characters (synapomorphies). For instance, let us assume

that our cladist faces again a unilinear interpretation of the genus *Homo* (e.g., *Homo habilis - H. erectus - H. sapiens*). He might argue that, considering the great number of uniquely derived characters (autapomorphies) present in *H. sapiens*, in comparison with the few of them present in his supposed ancestor *H. erectus*, it would seem more plausible to establish a sister-group relationship directly between *H. sapiens* and *H. habilis* rather than between *H. sapiens* and *H. erectus* (because *H. sapiens* shares many features with *H. habilis*). This time, the recognition of a particular sequence of dichotomies on a cladogram directly leads to the recognition of one or more speciation events.

From what has just been said, it is clear that the use of cladistics *as a method* of phylogenetic reconstruction might lead to a somewhat different assessment of the actual course of human evolution. For instance, proceeding to a cladistic analysis of the three taxa usually included in the genus *Homo*, Wood (1984) proposed that they were not linearly disposed in a single lineage as often assumed, but were to be included in a phylogenetic scheme involving probably one speciation event. Two possibilities were proposed: 1) *H. habilis* could represent the common ancestor of *H. erectus* and *H. sapiens*; 2) the *H. habilis -H. sapiens* lineage might have speciated from the *H. erectus* lineage. Later on, Wood (1996) was inclined to recognize, in addition to *Homo sapiens*, more species diversity at the late *Homo* stage with

the inclusion of *H. neanderthalensis* and *H. heidelbergensis*. The same applied at the early *Homo* level for, in addition to *H. erectus*, Wood (1992a, 1992b) recognized several more species, namely as *Homo habilis*, *H. rudolfensis* and *H. ergaster*. He argued that the emergence of the genus *Homo* might have been characterized by a substantial radiation of hominid species, each demonstrating a significant and distinctive shift away from the australopithecine adaptive plateau.

One last aspect of cladistics must be considered here. It has been argued that Hennig demonstrated the inadequacy of the synthetist paradigm by showing that we cannot achieve any understanding of evolutionary processes before we have achieved an understanding of the patterns produced by such processes (Platnick, 1979). Whether or not Hennig intended to be so critical of the evolutionary synthesis need not concern us here. However, Hennig (1981 [1969]: 38-42) definitely stressed the difficulties of mixing phylogenetic researches (patterns) with causal researches (processes). For some cladists, therefore, cladistics represented a new research programme competing with, and critical of, evolutionary systematics and the synthetic theory of evolution (or the evolutionary synthesis). For instance, Schafersman (1985) argued that, whereas evolutionary systematics was wedded to the synthetic theory of evolution, cladistics, for its part, was independent of any

evolutionary theory; a situation he judged preferable. But as cladistics matures, continued Schafersman, questions pertaining to evolutionary processes would fall under its purview, to the extent that patterns could be tested independently of evolutionary processes.

This perception of cladistics being a new research programme has been best described by Rosen, Nelson and Patterson (1979: ix):

> "Hennig established a criterion of demarcation between science and metaphysics at a time when neo-Darwinism had attained a sort of metaphysical pinnacle by imposing a burden of subjectivity and tautology on nature's observable hierarchy. Encumbered with vague and slippery ideas about adaptation, fitness, biological species, and natural selection, neo-Darwinism (summed up in the "evolutionary" systematics of Mayr and Simpson) not only lacked a definable investigatory method, but came to depend, both for evolutionary interpretation and classification, on consensus or authority."

Such criticisms were expressed also about human evolution. Some cladists (e.g., Delson *et al.*, 1977) criticized the phylogenetic trees for being represented as scenarios, that is amalgams of propositions on ancestor-descendant relationships based on *a priori* evolutionary processes, interpretations of stratigraphic and geographic information, and reconstructions of adaptation through functional anatomy and paleo-environments. This approach to the constructing of hominid phylogeny was held responsible for most of the controversies in human phylogeny. To avoid

this, cladists proposed that only morphology should be used to evaluate evolutionary history. Also, at first, cladists defended the view that paleontologists should limit themselves to simpler evolutionary statements presented under the form of a cladogram, in which ancestor-descendant relationships were not specified (Cracraft, 1974; Delson *et al.*, 1977; Engelmann and Wiley, 1977; Schaeffer *et al.*, 1972). Eventually, however, information summarized as scenarios was judged acceptable, as long as the three different levels of complexity involved in cladogram, phylogenetic tree and scenario building were clearly perceived; each was successively further removed from the basic data available, the cladogram being the simplest form of all (Eldredge, 1979a; Tattersall and Eldredge, 1977).

These considerations bring us to the heart of the crucial differences between cladists and evolutionary systematists. We have seen that many cladists established a sharp distinction between inquiry into evolutionary patterns and into processes. This was based on the assumption that it was possible to inquire into evolutionary patterns independently of processes. In other words, there was no need to rely on any evolutionary theory or mechanism to formulate hypotheses about evolutionary patterns, as long as it was recognized that evolution had occurred (de Queiroz and Donoghue, 1990: 61-62; Gaffney, 1979: 86; Platnick, 1979).

For evolutionary systematists (e.g., Bock, 1974; Mayr and Ashlock, 1991; Simpson, 1975), on the other hand, taxonomic characters were expressions of the biology of their carriers, that is, they were part of an integrated living system in relation to other organisms and environmental conditions. The assessment or evaluation of a particular morphological trait might be changed when other types of information were considered. For some evolutionary systematists (e.g., Bock, 1977, 1981; Szalay, 1981, 1991, 1993; Szalay and Bock, 1991), in contradistinction with cladists (e.g., Cracraft, 1981b; Eldredge and Novacek, 1985), evolutionary processes could not be tested against evolutionary patterns, for the simple reason that the order found among organisms in phylogenies reflected their evolutionary histories, which could be understood only by reference to the mechanisms of evolutionary change. Therefore, it was assumed by these evolutionary systematists, that patterns and processes could not be uncoupled. According to Bock (1977: 864), a classification and a phylogeny:

"...must be in agreement with the whole of evolutionary theory, including laws, mechanisms of change and subfactors thereof. By the whole of evolutionary theory, I mean... all studies of function, biological role, behavior and environmental factors required to understand the evolutionary mechanisms... [and also] factors of the formation of genetical mechanisms and of natural selections...".

Such differences between cladists and evolutionary

systematists continue to constitute a deep philosophical distinction which can have an important impact on phylogeny reconstruction. We have seen that the unilinear interpretation of the genus *Homo* was based, in part at least, on culture as an adaptation that permitted *Homo erectus* and subsequently *Homo sapiens* to inhabit a large part of the Old World and avoid speciation through gene flow and the competitive exclusion principle. The *nature* of this interpretation is in line with the evolutionary systematist's approach, because morphology is evaluated in the light of supposed evolutionary processes as derived from the evolutionary synthesis.

This way of proceeding in human evolution has prompted criticisms (e.g., Eldredge and Tattersall, 1982; Groves, 1989; Kimbel, 1991; Kimbel and Rak, 1993; Tattersall, 1986, 1992, 1995). This may have been best summarized by Kimbel (1991: 355, 368):

> "[Paleoanthropologists are reluctant] to allow perturbation of their mainstream preoccupation with adaptative scenarios and taxonomic grades, [for they] have clung to a process-oriented view of the fossil record of human evolution... Only within the past few years have serious attempts been made to test conventional evolutionary schemes within a framework of refined hypotheses of phylogenetic pattern. Accompanying this trend is the recognition that the *species* is the fundamental evolutionary unit in human paleontology... Clearly, our bias toward a simple, linear progression of hominid taxa through time, and an attendant preoccupation with ever-expanding envelopes of intraspecific variation, will fail to detect taxonomic diversity, and thus risks yielding an inaccurate picture of hominid evolutionary history."

Underlying this whole approach to hominid systematics is the idea that a "taxic" approach should be preferred to a "transformational" one (Eldredge, 1979b; Eldredge and Novacek, 1985).

It should be mentioned here that the cladists' criticisms directed at the consideration of adaptation for phylogeny reconstruction were formulated also by a number of non-cladists. For instance, Gould and Lewontin (1979) held that natural selection should not be viewed as the sole agent involved in the evolutionary drama, for the course of evolution might also have been determined by constraining factors such as phyletic heritage and pathways of development. But if these latter criticisms were intended to promote a more pluralistic approach to evolution, the cladists' criticisms, on the other hand, seem to be rooted in the philosophy of science promoted by Karl Popper (e.g., Platnick and Gaffney, 1977; Wiley, 1975). Cladists argue that their approach should be preferred because their inferences are more easily refutable than the evolutionary systematists' ones. This seems to be the philosophical basis on which rests the distinction made between cladogram, phylogenetic tree and scenario building on the one hand, and the claim that the investigation of evolutionary patterns and processes should be uncoupled on the other. The various avenues from which criticisms were formulated at the

"adaptationist programme" are yet further examples of the complex conceptual network that has prevailed in evolutionary biology since the 1970s, and in which cladistics is sometimes entangled.

From what has been said above, it is clear that cladistics has been a multi-facetted movement with deep implications for phylogeny reconstruction in human evolution. For that reason, it did not go uncriticized. Among the criticisms are the following. It has been argued that cladists' procedures to identify taxa on the basis of uniquely derived characters (autapomorphies) can be misleading, because what some consider to be a single polymorphic species in space and time might be divided by others into two or more species (Turner, 1986; Turner and Chamberlain, 1989). Habgood (1989) and others hold that cladistics cannot deal properly with morphological variations below the species level, because it emphasises discontinuities such as the presence or absence of characters. Even though cladistics provides useful methodological principles, argues Trinkaus (1990), it can only be a device of heuristic value for it cannot deal properly with the biological realities of hominids. For Brace (1988, 1993), consideration of evolutionary processes cannot be abandoned to investigate the course of human evolution as cladists propose. Lastly, Tobias (1995a) pleads for greater care in the recognition of polarity

states, and urges scientists to retain from the cladistic approach some tenets to serve only as guidelines that are reasonable and biological, and to purge it of some of its rigid rules which are un-biological.

When applied in human evolution, cladistics has often been responsible for taxonomic inflation, especially when contrasted with a unilinear interpretation of the genus *Homo*. It must be clearly stated here that cladistics does not or must not *necessarily* lead to taxonomic inflation, but simply that it often has had this effect in human evolution. We have seen three aspects of cladistics that can lead to such a taxonomic inflation: 1) because of its link, in the thinking of some scientists, to the theory of punctuated equilibria; 2) as a consequence inherent in its method; and 3) because of the break it has instituted between evolutionary patterns and processes. Also, cladistics is not the only cause for taxonomic inflation in human evolution after 1975 (c.f., Gould, 1976, 1987). For that reason, the resurgence of taxonomic inflation in human evolution cannot be causally attributed only to cladistics. Instead, cladistics, when applied in human evolution, is part of a wider scientific context that favours taxonomic inflation. We have seen earlier that the following elements were part of this new context: the theory of punctuated equilibria, species as individuals or entities, species selection, the theory of evolution as a hierarchic theory

and cladistics. If the evolutionary synthesis encouraged research to be performed below the species level, this new scientific context, on the other hand, encouraged research at and above the species level.

What has been the impact of this new scientific context in general, and of cladistics in particular, on the assessment of human phylogeny since 1975? Our analysis will be restricted only to a portion of the hominid fossil record: that of the genus *Homo*.

It is not argued here that the new developments in evolutionary biology and systematics had no impact on investigations performed at the australopithecine level. Rather, the view is defended here that its impact is more easily documented when analysed against a unilinear interpretation of the genus *Homo*. It will be remembered that one speciation (or more) at the australopithecine level had long been recognized by numerous scientists. This situation persisted after 1975 (e.g., Boaz, 1988; Coppens, 1983a, 1983b; Corruccini and McHenry, 1980; Delson, 1978, 1986, 1987; Falk *et al.*, 1995; Grine, 1988; Groves, 1989; Johanson and White, 1979; Kimbel *et al.*, 1984; Kimbel *et al.*, 1988; Leakey, 1981; Leakey and Lewin, 1992; Lewin, 1986; Olson, 1981; 1985; Rak, 1983; Skelton *et al.*, 1986; Skelton and McHenry, 1992; Strait *et al.*, 1997; Tobias, 1980, 1988; Walker *et al.*, 1986; Walker and Leakey, 1988; White *et al.*, 1981; Wood, 1992a, 1996). For reasons of

space limitations and the goals of this paper, it suffices here to restrict our analysis to the genus *Homo*.

Before assessing the impact that this new scientific context had on the phylogenetic pattern of the genus *Homo* after 1975, let us first briefly consider specific debates on each of the three taxa (*Homo habilis*, *H. erectus* and *H. sapiens*) usually recognized in the unilinear interpretation of this genus.

The creation of the taxon *Homo habilis* (Leakey *et al.*, 1964; see also Tobias, 1966) on the basis only of fossils from Olduvai Gorge (East Africa) did not receive, at first, universal acceptance. This situation generated debates on the validity of this taxon (Tobias, 1991a: 16-49). Were specimens attributed to *Homo habilis* distinct enough from those of *Australopithecus africanus* or *Homo erectus* (e.g., Brace *et al.*, 1972; Le Gros Clark, 1964; Montagu, 1965; Simons *et al.*, 1969; Robinson, 1965, 1966)? However, by the 1980s new fossil discoveries made in the 1960s and 1970s had convinced a majority of scientists of the validity of *Homo habilis*, but generated debates on the definition and the hypodigm of this taxon. Which fossil specimens were properly to be attributed to *Homo habilis* (e.g., Johanson and White, 1979; Tobias, 1978a; Walker and Leakey, 1978)?

Although these latter debates about the hypodigm of *Homo habilis* did not originate from considerations of cladistics nor from the new developments in evolutionary biology after

1970, they were eventually sucked into it. Within this new scientific context and in addition to new empirical data from East and South Africa, a number of scientists persisted in recognizing only a single, variable species called *Homo habilis* (e.g., Howell, 1978; Johanson, 1989; Johanson *et al.*, 1987; Miller, 1991; Tobias, 1989, 1991a). On the other hand, an increasing number tended to recognize in its place two or more species (e.g., Bilsborough, 1992; Chamberlain, 1989; Grine *et al.*, 1996; Groves, 1989; Kramer *et al.*, 1995; Lieberman *et al.*, 1988; Lieberman *et al.*, 1996; Rightmire, 1993; Stringer, 1986; Wood, 1985, 1992a, 1992b, 1993). The names *Homo rudolfensis* and *H. ergaster* were suggested to accommodate the supposed non-*habilis* specimens.

In contrast with *Homo habilis*, the validity and definition of the taxon *Homo erectus* was, by the late 1960s, accepted by a majority of scientists. Fossil specimens recognized under generic names such as *Africanthropus*, *Atlanthropus*, *Homo*, *Pithecanthropus*, *Sinanthropus*, possibly *Tchadanthropus* and *Telanthropus* were all subsumed under the genus *Homo* and the species *erectus* (c.f., Campbell, 1965). Regardless of new empirical data and a new scientific context after 1975, many scientists seemed to hold on to the view of a single, polytypic species widely distributed throughout Asia, Africa and possibly Europe (e.g., Bilsborough, 1992; Bräuer and Mbua, 1992; Howell, 1978; de Lumley and de Lumley, 1979; Howells, 1980; Hublin, 1986;

Kennedy, 1991; Kramer, 1993; Krantz, 1993; Rightmire, 1984, 1986, 1990; Tobias, 1995a; Turner and Chamberlain, 1989).

However, the new scientific context, especially through the use of cladistics, prompted a number of scientists to question the definition of *Homo erectus* (e.g., Andrews, 1984; Clarke, 1990; Groves, 1989; Stringer, 1984a; Wood, 1984, 1992a, 1992b). It has been suggested that at least two species might be recognized instead of a single one. The names *Homo ergaster* and *Homo leakeyi* are sometimes used to accommodate the supposed non-*erectus* specimens.

Not unlike *Homo erectus*, the taxon *Homo sapiens* has been, since the 1960s, accepted as being a widespread, polytypic species composed of various morphological clusters recognized as formal subspecies, such as *Homo sapiens sapiens*, *H. sapiens neanderthalensis*, *H. sapiens rhodesiensis* and *H. sapiens soloensis*, or under informal appellations like "anatomically modern humans", archaic *H. sapiens*, Neandertals and pre-neandertals. The scope of the taxon *Homo sapiens* was expanded by subsuming under it forms previously labelled by names such as *Cyphanthropus*, *Europanthropus*, *Homo*, *Paleoanthropus*, *Protanthropus*, *Javanthropus* and, for some workers, specific names such as *Homo heidelbergensis*, *Homo neanderthalensis*, *Homo rhodesiensis*, *Homo soloensis* and *Homo steinheimensis* (c.f., Campbell, 1965).

With the advent of a new scientific context in human

evolution, a number of scientists now recognize, in addition to *Homo sapiens*, one, two and possibly three additional species at this late stage of human evolution (e.g., Rightmire, 1988, 1990, 1996; Stringer, 1990, 1994; Stringer and Gamble, 1993; Tattersall, 1986, 1992, 1995; Wood, 1996). The names *Homo neanderthalensis*, *H. heidelbergensis* and *H. steinheimensis* have sometimes been suggested to identify these presumptively non-*sapiens* specimens.

In sharp contrast to the latter view, an increasing number of scientists may not only be disposed to include all recent hominids in *Homo sapiens*, but also to sink *Homo erectus* into *Homo sapiens* by including a large number of hominids of the Pleistocene (e.g., Aguirre, 1994; Bilsborough, 1976, 1978, 1992; Hublin, 1986; Jelinek, 1978, 1980, 1981; Leakey, R.E.F., 1989; Tobias, 1995a; Wolpoff and Thorne, 1992; Wolpoff *et al.*, 1984; Wolpoff *et al.*, 1994).

As mentioned, such deep differences in the assessment of taxonomic units in human evolution can be attributed largely to divergent philosophical bases on which rest the phylogenetic practice. Let us now analyse how such philosophical distinctions are reflected in the assessment of the phylogenetic pattern of the genus *Homo* after 1975.

We have already seen that the unilinear interpretation of the genus *Homo* was challenged on the basis of a new scientific context (Eldredge and Tattersall, 1975, 1982; Tattersall, 1986, 1992, 1995; Tattersall and Eldredge, 1977;

Wood, 1984, 1992a, 1992b, 1996). A multilinear scheme was suggested to replace it. Many scientists contributed to this alternative view of the phylogeny of the genus *Homo*. According to Groves (1989: 191), "what we may call residual unilinealism is alive and well, indeed tends to be an unspoken assumption in many analyses". Groves (1989) set out to test that assumption on the basis of the new developments that took place in evolutionary biology in the last decade or so. As an opponent of the "adaptationist programme" and a proponent of cladistics and of punctuational models of evolution (including models of sympatric speciation), Groves depicted the evolution of the genus *Homo* as being a bush-like pattern characterized by speciation and quasi-speciation events, thus recognizing numerous species but especially a great number of subspecies. In addition to the three species usually recognized in the genus *Homo*, Groves recognized *Homo rudolfensis*, *H. ergaster* and an unnamed species of *Homo* of the *erectus* grade.

For Rightmire (1984, 1985, 1986, 1990), the tempo of human evolution throughout the Pleistocene was not a steady and gradual one. He embraced the cladistic method, a punctuational model of evolution and a definition of paleontological species based on the ontology of discrete entities in time and space (and not an arbitrarily delineated segment of a continuum). On these bases,

Rightmire held that *Homo erectus* was a widespread species that remained in relative stasis for a long period of time. In opposition to the interpretation of a gradual evolution of a single lineage in the genus *Homo*, Rightmire (1990, 1993, 1996) favoured the view that one of the two species of early *Homo* (*H. habilis* and an unnamed one) gave rise through a speciation event to *H. erectus* in Africa. After a prolonged period of relative stasis, the non-Asian segment of *H. erectus* would have given rise, through a speciation event, to *H. heidelbergensis*. This latter species, said to be closely related to *H. sapiens* and the Neandertals, might have replaced and extinguished *H. erectus* throughout a large portion of its territory.

According to Stringer (1992: 21), the occurrences of "apparent punctuational change, cladogenesis and parallel evolution are common evolutionary messages from the fossil record, and yet many palaeoanthropologists still focus on unilinear gradualism as the mode of hominid evolution". A proponent of the cladistic method, but not neccessarily of punctuational models of evolution and their accompanying ontology, Stringer (1984b, 1985, 1994) is strongly inclined to split *Homo habilis* and *H. erectus* respectively into two species each (Stringer, 1984a, 1986; see also Stringer and Gamble, 1993). With a number of his colleagues, Stringer holds that the late stages of human evolution might have been characterized by several speciation events with the

appearance, from an *erectus*-like ancestor, of *H. heidelbergensis* in the Middle Pleistocene of Europe and Africa, *H. neanderthalensis* in the late Pleistocene of western Eurasia and a late appearance of *H. sapiens* in Africa before its dispersal throughout the world (Stringer, 1990, 1992, 1994; Stringer and Andrews, 1988; Stringer and Gamble, 1993). Stringer (1985: 295) and Rightmire (1990: 228) both cautioned about the use of the subspecific category; they argue that it might obscure evolutionary events.

From what we have seen, it is clear that the new scientific context that arose in human evolution after 1975 had a great impact on the assessment of human phylogeny.

Far from being consensual, however, the rise of this new scientific context was counterbalanced in the field of human evolution by the persistence of an approach that was generated by the evolutionary synthesis. The following elements are part of this latter context: an eclectic approach to both taxonomy and models of evolution, the importance of infraspecific categories, the biological species concept and its related concepts, an ontology of species based on classes composed of individual organisms and the assumption that evolutionary patterns and processes cannot be uncoupled. Let us survey a number of scientists promoting this approach to human phylogeny after 1975.

According to Tobias (1983, 1991a, 1992), the

phylogenetic scheme of the genus *Homo* is best described as a unilinear one composed of three consecutive chronospecies: *Homo habilis - H. erectus - H. sapiens*. Tobias (1981, 1991a, 1991b) holds that, with the appearance of the genus *Homo* and the species *H. habilis*, a new level of organization was attained in evolution. The increase in endocranial capacity and material culture were just two elements among others that were part of a positive reciprocal feedback system that gave successive species of the genus *Homo* an adaptational edge. Culture became a major component of human adaptational processes, thus permitting the early colonization of most of the Old World without regional adaptation being accompanied by geographical speciation. One of the types of evolutionary pattern that Tobias (1978b, 1985, 1991a), like J.S. Huxley, has stressed for the genus *Homo* is reticulate evolution, that is, a divergence-reconvergence pattern of microtaxa such as subspecies or geographical races within a single lineage.

For Brace (1979a, 1991, 1992, 1995), the evolution of the genus *Homo* also takes place within a single culture-bound lineage: *Homo erectus - H. sapiens*. Culture, says Brace (1991: 89), "comprises a single ecological niche within which one would expect to find only a single species", on the competitive exclusion principle. As the cultural milieu became increasingly a part of the hominids' adaptational processes, key cultural aspects altered the

forces of selection in specific ways, thus explaining the picture of mosaic evolution. For instance, regional cultural innovations, by usually lifting selective pressures, permitted the expression of specific morphological traits in these regions. The appearance of modern humans in several regions of the Old World was explained by the spread of such cultural innovations (see also Brace, 1979b; Brace *et al.*, 1984; Brace *et al.*, 1987).

According to Thorne, Wolpoff and a number of their colleagues, the evolution of the genus *Homo* is interpreted within a "multiregional hypothesis". The evolution from *H. erectus* onwards is interpreted as that of a single polytypic species. Following the migration of *H. erectus* out of Africa, man's evolution was under selective forces and genetic drift maintaining regional distinctions, while gene flow between regions created cohesion (Thorne and Wolpoff, 1981; Wolpoff *et al.*, 1984). They held that regions are characterized by morphological continuity between earlier and later hominids within those regions, as evidenced in Europe and the Levant, East and South-East Asia, Africa and Australasia. The multiregional hypothesis tries to explain the worldwide pattern of Pleistocene hominids by claiming that modern human features originated at different times in different places (Frayer *et al.*, 1993; Thorne and Wolpoff, 1981, 1992; Wolpoff, 1980b).

Although it is not possible here to survey all the

scientists who promoted an essentially unilinear interpretation of the genus *Homo* after 1975, it must be stated that their bases for doing so stem largely from the scientific context generated by the evolutionary synthesis (c.f., Bilsborough, 1976, 1978, 1992; Ferembach, 1979, 1986; Jelinek, 1978, 1980, 1981; R.E.F. Leakey, 1989).

In concluding this section, a brief comment should be made regarding the origins of modern humans. Ever since the 1970s and especially after 1980, the origins of modern humans have attracted considerable attention (e.g. Aiello, 1993; Bräuer, 1984, 1989, 1992; Delson, 1988; Demars and Hublin, 1989; Frayer *et al.*, 1993; Frayer *et al.*, 1994; Groves, 1994; Howells, 1976, 1993; Hublin, 1985; Hublin and Tillier, 1991; Lahr, 1994, 1996; Lieberman, 1995; Mellars, 1989; Minugh-Purvis, 1995; Pilbeam, 1986; Rightmire, 1991; Smith, 1985, 1993; Smith and Trinkaus, 1991; Smith *et al.*, 1989; Stringer, 1992; Stringer and Andrews, 1988; Stringer and Bräuer, 1994; Stringer and Gamble, 1993; Tattersall, 1986, 1991, 1992; Tobias, 1995b, 1995c; Vandermeersch, 1989; Wood, 1996). Although this debate is somewhat influenced by the tension generated by the two opposing scientific contexts discussed in this section, it is most strongly influenced by the empirical data coming from molecular anthropology and, to a lesser extent, from prehistoric archaeology, and new dating of fossil specimens. The consideration of this debate is, therefore, beyond the scope

of this essay.

Conclusion

We have seen how debates on human phylogeny were entangled in the complex swirl of evolutionary biology. Yet, beneath this complexity, a crucial underlying theme runs through the entire second half of the twentieth century: The tension, under different disguise, between human phylogenetic interpretations assessed on the basis of evolutionary processes versus those assessed independently of them. At the heart of this tension lies the deep epistemological question whether evolutionary patterns must imperatively be established from within a theoretical structure providing evolutionary mechanisms or, conversely, if such mechanisms can only be established once the evolutionary patterns are well known. This question of importance for phylogeny reconstruction is by no means easily resolved.

This essay also permitted us to highlight the immense importance of theoretical issues in determining the views in human evolution. Are we well advised in thinking that our views on human phylogeny are determined not by empirical evidence but by theoretical issues? It should not be forgotten that a number of fossil discoveries made throughout the history of the research on human phylogeny had a great impact. The single-species hypothesis was

largely discarded in the 1970s because of empirical evidence. Above all, however, the scientific enterprise should not be reduced to a confrontation between theoretical and empirical contents. These are only two sides of the same coin.

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Conclusion

Conclusion

As the papers constituting this thesis stand by themselves, this is not the place to repeat the specific conclusions reached independently in several of them. Instead, this final part reiterates a number of generalizations which are extracted from the thesis.

Because of the limitations alluded to in the Introduction, no firm conclusions regarding the nature of the field of human evolution have been reached. However, the historical and epistemological analyses presented here shed light on the main message that emerges from this thesis:

The study of human evolution is a recent scientific quest that started less than two hundred years ago and to which several scientific disciplines and approaches have contributed, by necessity or historical circumstances. Unsurprisingly, this situation created difficulties in the process of integration of knowledge among disciplines and between approaches that persist until today. This epistemological landscape imposes constraints and guides forces at work in research on human phylogeny since its inception. The understanding of these forces and contraints is a prerequisite to the progress of knowledge of our evolutionary history.

This thesis has tried to contribute to this effort as in the three following cases:

1) As long as it was thought that research on human phylogeny was only a by-product of evolutionary biology, it could not be realized how deep were the epistemological problems that plagued research on humankind's place in nature. In this thesis the rise of the field of human evolution during the nineteenth and first part of the twentieth centuries has been reinterpreted in a way which has made these epistemological problems more easily detectable.

It was argued that in addition to evolutionary biology, other disciplines akin to, or approaches derived from, the anthropological sciences contributed greatly to human evolutionary researches. During the 1860-1895 period, these various disciplines and approaches contributed, at different levels, to human phylogeny. Broad phylogenies were based on comparative anatomy of living non-human primate and human forms, while restricted phylogenies were established largely through the hominid fossil record. We have argued that this situation resulted from an epistemological problem. At this juncture, the various disciplines and approaches which contributed to the establishment of human phylogeny were essentially complementary to each other.

In other words, only by pulling together the results of these disciplines and approaches could a somewhat complete picture of human phylogeny be established. Yet, we have seen

that this integration of knowledge resulted in a very partial view of humankind's place in nature. Many questions on human phylogeny remained unanswered for lack of knowledge about them.

During the 1895-1930 period, considerable progress was made in the recovering of fossils pertinent to human evolution. This led to the institution of two distinct interpretative frameworks from which human fossils were evaluated. In one, human fossils were compared to living humans and were therefore related to one or several living human races. In the other framework, human fossils were evaluated in the light of their simian characters providing clues as to which fossils were on the "true" line (or lines) leading towards living humans. On this view, numerous human fossils were put on side branches leading to extinction.

At first glance, the setting up of these two distinct interpretative frameworks seems to be attributable only to competing views about the available data. However, this is true only to the extent that an epistemological problem lay at the heart of research on human phylogeny. The state of the hominid fossil record permitted scholars to interpret the fossils by relating them either to living humans or to living and fossil non-human primates. This situation was created by both a problem of integration and a vacuum of knowledge on several issues.

2) It is demonstrated in this thesis that the establishment of human phylogeny during the second half of the twentieth century often polarized around unilinear or multilinear interpretations. We have documented a number of possible factors which appear to justify either view of human phylogeny.

Most interestingly, the debate on the reconstruction of human phylogeny since 1950 shows that the relationship between evolutionary processes and patterns is a point of contention.

For a number of scientists, consideration of evolutionary processes is an integral part of research on evolutionary patterns. Only by understanding how the evolutionary processes work are we able to decipher correctly the evolutionary patterns. On this view, patterns and processes are integrated. However, other scientists hold that evolutionary processes and patterns ought to be uncoupled. In their view, the reading of evolutionary patterns in the light of pre-established processes can only obscure the identification of patterns. The procedure should be reversed by first establishing the patterns independently of processes. From there, the processes at work could be unravelled on the basis of the already recognized patterns.

This question concerning the correct procedure for phylogeny reconstruction hides a genuine epistemological problem that cannot be easily resolved. Ultimately, this is

a problem of limited knowledge in our capacity to assess the evolutionary history of the living world.

3) It is widely recognized that biological and cultural aspects of humankind were intricately related at one period or another of human evolution. Yet, this observation raises innumerable problems of knowledge integration in research on human phylogeny. We have seen that as early as the second half of the nineteenth century a number of scientists used the abundant prehistoric tools to extract knowledge about the tool makers themselves. These scientists assumed that cultural evolution was a mirror of biological evolution. On that basis, they conjectured about the hypothetical existence of ancient human races, species and even genera. Yet, at that juncture, the link between prehistoric archaeology and human palaeontology was weak, to say the least.

As more fossils and tools were recovered during the first half of the twentieth century, and in better stratigraphical and chronological context, an increasing number of scientists associated specific fossil groups with particular industries, thus seemingly contributing to the integration of human palaeontology and prehistoric archaeology. We have argued in this thesis that this integration was only very partial. Human palaeontology and prehistoric archaeology were well linked at the theoretical level of scenario-building, whereas they were essentially uncoupled at the level of their respective empirical bases,

although this was not fully realized by some scientists. The disciplines of human palaeontology and prehistoric archaeology had to develop independently in order to forge appropriate scientific tools to analyse their empirical bases, irrespective of the fact that they constituted two sides of the same coin. It is theoretically sound to attempt their integration, yet it can only be partial because of epistemological constraints.

Furthermore, we have seen that the biology/culture link got entangled during the second half of the twentieth century in the debate over whether the determination of evolutionary processes should precede the establishment of evolutionary patterns, or conversely. We have already mentioned to what extent this is a problem of limited knowledge in our capacity to assess the evolutionary history of living beings.

The three cases mentioned here permit us to get a better understanding of the epistemological landscape in which research on human phylogeny has been performed since its inception. It permits us to identify both the forces and constraints at work within the field of human evolution, and therefore to get a better understanding of its nature.

Historical and epistemological analyses of the field of human evolution may not be able to resolve the problems identified. Solutions must stem entirely from the scientific arena. However, the potential usefulness of such analyses lies precisely in their capacity to identify and delineate

unsuspected epistemological problems that plague research on human phylogeny. Well identified or defined problems would be prerequisites to their solutions.

This work favours the incorporation of historical and epistemological analyses as integral parts of the scientific process concerned with human evolution. There are some who think of history as an approach dedicated merely to the recounting of how we got where we are today. This is not the view expounded in this thesis. *When a strong analytical or philosophical component is added to history, it constitutes a powerful tool that may contribute to the scientific process itself.* It is suggested here that the institution of a strong research programme concerned with history and philosophy would greatly benefit the further development of the field of human evolution.

Appendix A

Faculteit der Archeologie

to: Mr. R. Delisle - Québec
00 - 1 - 514 - 381 0092

2/25/98

Dear Mr Delisle/ Dear Richard:

To our pleasure we can now let you know that your paper "Human phylogeny and evolutionary biology during the second half of the 20th century" has been accepted for publication in R. Corbey & W. Roebroeks (eds.), *Studying human origins: The history and epistemology of the disciplines*, to be offered for editorial consideration to Cambridge University Press at the explicit request of that publisher.

Sincerely,

R. Raymond Corbey

Dr R. Corbey
Prof. Dr. W. Roebroeks

ps. looking forward to reading the other paper you announced!

Adres: Archeologisch Centrum
Postbus 9515
NL 2300 RA Leiden
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Rijks Universiteit [redacted]

Appendix B

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FAX TRANSMISSION

Date 21 April 1998

To Richard Delisle Esq

From Dr Frank Spencer

Dear Richard:

Thank you for your letter and enclosures.

I would like very much to include your paper entitled:

Human Palaeontology and Prehistoric Archaeology: 1900-1950

in the proposed volume *The History of Palaeoanthropology as Seen from the New Millenium* [HPNM] (edited by Frank Spencer). Although at this juncture I am unable to state who the publisher will be, I do have an interested party -- and I am confident that a contract will be ultimately secured. In the meantime, however, I trust you will accept this offer to publish your paper in the proposed HPNM volume. If you have any questions, please, do not hesitate to contact me.

With best wishes.

Sincerely

Frank Spencer
Professor

Appendix C