University of Alberta

Beyond the Racial Paradigm: New Perspectives on Human Biological Variation

by

Michael S. Billinger ©

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ABSTRACT

Beyond the Racial Paradigm: New Perspectives on Human Biological Variation

by

Michael S. Billinger

Claude Levi-Strauss referred to race as 'the original sin of anthropology,' and Ashley Montagu called it 'Man's most dangerous myth.' But the problems of racial classification are not solely anthropological; they are great consequence to the study of evolutionary biology in general. Anthropologists, even in their lucid criticisms of the race concept, commonly overlook the fact that the race concept itself has been vigorously debated in terms of its general applicability to any biological species. A comprehensive analysis of the race concept across various disciplines, examining the nature and validity of the concept itself, especially its differential application to humans, reveals that the persistence of racial categories has dramatically limited our understanding of the patterns and processes of human variation. Philosophical barriers to understanding human variation have resulted from the inability to escape the fallacious race concept. The resulting biological issues, such as how biological 'subspecies' or 'varieties' fit into the processes of microevolution and speciation (and their taxonomic implication) are analysed, particularly in the context of physical anthropology. This dissertation argues that the problems inherent in traditional taxonomic schemes are complex and do not simply stem from the clash between classical Linnaean classification and Darwinian evolution in the late 19th century. This analysis, combining theory and methodology in

physical anthropology, provides a conceptual basis for developing new perspectives on human variation, and the biological history of the species *Homo sapiens*, arguing that the race concept be replaced with a biological concept of ethnicity.

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INTRODUCTION

> *The original sin of anthropology ... consists in its confusion of the idea of race, in the purely biological sense (assuming that there is any factual basis for the idea, even in this limited field – which is disputed by modern genetics), with the sociological and psychological production of human civilizations (Levi-Strauss, 1958:8).*

One of anthropology's pre-eminent and most prolific scholars of the 20th century, Ashley Montagu (1941:243), wrote of the anthropological enterprise: "The idea of race is one of the most fundamental if not *the* most fundamental of the concepts with which the anthropologist has habitually worked." As such, race remains a persistent conceptual and terminological artefact in the contemporary study of human biology and culture. While much has been written about the intellectual history of the anthropological race concept, clarification of how this concept fits practically into contemporary scientific scholarship remains inadequate; race continues to be one of the most ambiguous terms in both social and scientific usage. *We now understand that 'race' is not real, and there are decades of research proving this point, but as of yet, there have been no comprehensive models to replace the race concept.* Rather than simply recounting the history of the development of the race concept, the objective of this dissertation is to explore specifically how racial science, or more precisely, anthropological racialism, has created a conceptual barrier for understanding human variation. The main overarching questions are therefore quite simple, dealing with the 'why' and 'how' of the anthropological race concept: *why has the race concept remained so pervasive in shaping our modern understanding of human variation, and how has it limited our understanding of human evolution?* Moreover, *what* *will it take, both philosophically and methodologically, to move beyond the racial paradigm?*

In dealing with these questions, the primary focus of this dissertation will be on anthropological theory and method and how they fit into the general history and philosophy of science, particularly biology. The subject matter covered here is relatively broad, and this dissertation does not look much like a standard physical anthropology study. The focus is not on 'what the bones tell us,' but rather on how anthropologists approach skeletal data in method and theory. Instead of presenting a specific hypothesis about the nature of human skeletal variation, this dissertation builds a theoretical model that brings together cultural and biological perspectives (see Dufour, 2006) and provides a demonstration of how that model might be applied methodologically to the analysis of skeletal data. The overarching theme of this dissertation is, therefore, how contemporary conceptions of cultural and biological variation have been (at least perceived as) shaped by unresolved tensions between static and dynamic approaches to the general 'order of things' (Foucault, 1970), which has impeded our understanding of the patterns and processes of evolution.

Chapter 1 builds a general argument for why it is that anthropological researches show very little progress in dealing not only with the place of humans in nature, but with the place of anthropology in the study of natural history, particularly natural selection and evolutionary theory. A problematic relationship exists between anthropological and 'scientific' knowledge, which provides a major obstacle for the development of progressive methodologies in all areas of the discipline. Chapter 2 takes this argument further by looking at the practical aspects of the anthropology-science relationship, demonstrating that the methodologies used by anthropologists in studying human biological variation often have little in common with those used by evolutionary biologists to study non-human organisms. The focus of Chapter 3, which presents the central argument of this dissertation, offers a survey of alternative models for explaining variation and a discussion of why such models have failed to replace or even circumvent racial or racialist explanations of human population biology in light of earlier arguments for a biological concept of ethnicity. Chapter 4 shifts the focus in a more practical direction, examining some popular methodologies employed by physical anthropologists in the study of human skeletal variation and phylogeny. Chapter 5 provides an example of how the model of biological ethnicity presented in Chapter 3 might be developed methodologically by exploring the results obtained utilising some of the methods of craniometric analysis outlined in Chapter 4. This chapter also peripherally explores problems of forensic 'racial' determinations. Chapter 6 explores future directions for research in human biology, variation, and taxonomy, and solutions for physical anthropologists.

ANTHROPOLOGICAL CONTEXT

While much of this dissertation is a critique of general anthropological theory and method, the particular focus is on the subdiscipline of physical anthropology. A source of inspiration comes from Sherwood Washburn, who in 1951 encouraged a 'new physical anthropology,' which was a call for the replacement of the static typological approach to human variation by a dynamic, evolutionary, adaptive approach (Marks, 2000a:225). In a

1963 article dealing with the race issue specifically, Washburn (1963:522) argued that the dynamic approach was an absolute necessity:

> In the first place, anthropology's main subject, the subject of race, disregarded to an amazing degree the evolution of the human species. Anthropologists were so concerned with the subdivisions within our species and with minor detailed differences between small parts of the species that the physical anthropologists largely forgot that mankind is a species and that the important thing is the evolution of this whole group, not the minor differences between its parts.

Although it is generally understood that the main unit of natural selection is the individual, the race concept remains a contested but common feature for explaining population-level variation, since there have been no theoretical models developed that have been adequately comprehensive to replace the relatively simple notion of race.

Recent debate over the practical usefulness of such categorisation has been most prominently played out in forensic anthropological literature in the 1990s, dealing with the medico-legal application of physical anthropology (Sauer, 1992; Brace, 1995; Kennedy, 1995). Although many of its practitioners contend that more advanced methods of ancestral determination are necessary (Brues, 1992; Kennedy and Chiment, 1992), and that the race debates have only served to retard such progress (Sauer, 1993), arguments that racial categories are necessary to convey socially understandable (identifying) information in a medico-legal context are persistently made. In failing to provide progressive methods of ancestral determination, and continually relying on outdated methods of racial determination, forensic anthropologists are neglecting the scientific questions that lead to increased understanding of human variation and accuracy in their determinations.

The race issue in anthropology as a whole is a double-edged sword. On the one hand, it is bound up in political meaning that goes well beyond the discipline, and on the other, it is an issue far too complex biologically to be adequately understood by simple models. For both reasons, the study of race, outside of the context of racism (as opposed to racialism), provides significant obstacles for researchers in terms of funding and other types of organizational support. It is exceptionally difficult to produce an encompassing research proposal that is feasible enough to attract funding attention, and to build a sufficient argument that the proposed project is not simply based in the political correctness movement. Brace (2005:ix) explains this problem well in the context of professorial tenure. He explains that had he not received tenure prior to developing his critical approach to the race concept, he would never have been qualified for tenure. Subsequently, his refutations of race and rebuttals against racialist researchers resulted in rejected funding applications as well as a cessation in professorial 'merit raises.' These types of issues have obvious consequences for the state of the discipline in terms of theoretical and pedagogical innovation.

Surveys of the current state of theoretically-based racialism in anthropological literature demonstrate that while attitudes have shifted significantly in favour of the rejection of racial categories, the use of race as an explanatory model is persistent. Cartmill (1998) reported that articles appearing in the *American Journal of Physical Anthropology (AJPA)* dealing with human variation remained consistent over 30 years in their utilisation of the race concept, with it appearing in 34% of articles in 1965 and 35% in 1996. Lieberman et al. (2003) have challenged Cartmill's results using a slightly different methodology, explaining that the initial attacks on the racial paradigm by

Montagu in 1941 and 1942, Washburn in 1953, Livingstone in 1958, Brace in 1964, and Lieberman in 1968, saw the use of the race concept decline in the *AJPA* from 78% of articles in 1931 to 36% in 1965 and down to 28% in 1996.

Lieberman et al. (2003) also surveyed American Anthropological Association (AAA) members in 1985 and again in 1999, asking respondents to agree or disagree with the statement: "There are biological races in the species *Homo sapiens.*" In 1985, 41% of physical anthropologists surveyed disagreed with the statement, increasing to 69% in 1999. Interestingly, 53% of cultural anthropologists disagreed with the statement in 1985, with that figure rising dramatically to 80% in 1999. Striking differences were also found in physical anthropology textbooks, which predominantly presented racialist models of human variation between 1932 and 1979, but showed an almost complete abandonment of the race concept as an explanatory tool between 1980 and 1999. These results prompted Lieberman et al. (2003:112) to conclude: "[D]ata indicate that the paradigm of race has approached the point where its survival is in doubt. Even those who continue to use the concept have serious doubts about its utility." It is also interesting to note that 42% of first authors of the included textbooks published in 1998–99 were AAA members, whereas only 4% of first authors of *AJPA* articles in 1996 were AAA members. Cartmill and Brown (2003) suggest that this difference in AAA membership indicates that the textbook authors are more representative of American anthropology, whereas the *AJPA* authorship is international; therefore, this may be more indicative of an American abandonment of the race concept, but a continued international usage.

These two studies demonstrate a significant point: a sweeping philosophical trend occurred between 1931 and 1965, in which there was a 42–43% decline in the use of racial models in the *AJPA*, followed by a period of relative stability or slight decline between 1965 and 1996. I suggest that this represents a paradigm shift in American anthropology, but a shift that has been stunted by methodological stagnancy. While the race concept itself has been questioned or abandoned, methodology has not advanced, and no workable non-racialist models for explaining human population variation yet exist. Thus, while the concept of race has changed through time, the methodologies that utilise the concept remain static. The biological aspects of this problem are further illustrated by the disjunction in results between cultural anthropologists and physical anthropologists who answered Lieberman et al.'s (2003) questionnaires. In 1985, 12% more cultural anthropologists than physical anthropologists rejected the race concept, and 11% more (even after the marked increases in rejections by both groups) in 1999.

Following Cartmill (1998), Wang et al. (2002a, 2002b) report that of 324 articles directly related to human variation printed in *Acta Anthropologica Sinica*, China's only journal dedicated to physical anthropology, none questioned the validity of human racial classification. Rather, several articles were mainly concerned with the biological differences among or between 'major races.' Wang et al. (2002b:96) suggest that this focus can be considered a continuation of Weidenreich's (1943) work in China, which emphasized regional continuity, suggesting Mongoloid roots extending back to *Homo erectus.* The irony of this focus on the so-called Mongoloid race is that studies of intra-group variation reveal that subdivision by north and south, and even further down to the ethnic or tribal level is possible (Wang et al., 2002b:96). However, in China, race has proven to be a powerful political tool for uniting diverse human groups within the country, since Chinese physical anthropologists have been portraying the Chinese (Mongoloid) people as a discrete biological group with a long evolutionary history.

Results of studies of Polish physical anthropologists using questionnaires based on those developed by Lieberman et al. (1989) reveal a more encouraging picture of human variation studies in central Europe. The first study, conducted in 1999, revealed that out of 55 respondents, 31% agreed with the statement "There are biological races (meaning subspecies) within the species *Homo sapiens*," while 62% disagreed, and 7% had no opinion (Kaszycka and Štrkalj, 2002). The authors explain that there are general trends in age: Polish physical anthropologists born before, during, or shortly after World War II had a much stronger sense of race, and racial typology was taught as a major university course in some universities until the 1970s, while younger anthropologists who received more modern training were much more sceptical about the existence of biological races.

In a 2001 follow-up study, Kaszycka and Strzałko (2003) found that offering the respondents different racial concepts as options resulted in significantly different results. 75% of 100 respondents (of whom three-quarters had also responded to the 1999 survey) agreed that there are human races when allowed to choose between races defined as geographical (17%), typological (13%), populational (35%), subspecies (3%) or a combination of two of these options (7%). The rejection of race by the majority of respondents of the 1999 survey versus the much lower rate of rejection in the 2001 survey suggests that race, when construed only as a subspecies, is more problematic (at least in the Polish context) than when the term is attached to other biological concepts (Kaszycka and Štrkalj, 2002:334).

Thus, the results of these studies indicate that in North America, the rejection of the race concept on an intellectual basis is more widespread amongst those who deal with it only as an *organizing category* (cultural anthropologists) than those who utilize race as an *explanatory model* (physical anthropologists) of how human groups vary biologically. In the Chinese example, race continues to be uncritically accepted by physical anthropologists, and the authors suggest that this is a result of socio-political context rather than scientific discovery. In Central Europe,[1] the Polish studies suggest that there is general trend toward the rejection of race as a biologically meaningful concept in a strict taxonomic sense by physical anthropologists, but that it remains a persistent organizing principle in general. In order to bridge these gaps not only between applications of the race concept, but between national or regional anthropological communities, new perspectives on human variation must be developed.

THEORETICAL PERSPECTIVE

The inherent problems of using race as an anthropological concept stem from the ambiguous nature of the term itself and the lack of any pragmatic or encompassing definition. Chiarelli (1996:226) asks the question "Race: what is it?" but offers only, "The concept is ... misleading as it tends to confuse Man's physical characteristics with those of culture, religion, or of the economy. The cultural and historical characteristics of 'ethnic groups' cannot be intermingled with the physical characteristics of different human populations, as the former depends on imprinting and not on genes." This

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[1] See also Teschler-Nicola (2004) for a critical analysis of National Socialist race theory in Austria and central Europe, which also demonstrates the changing perceptions of human classification in the post-war period.

statement, though it offers no real answer to the race question, brings together three key points to be addressed in this dissertation, based in the significant literature on the topic.

First, the existing literature on race and/or racism is lacking due to the almost exclusive focus on the historical development of the race concept rather than proposing alternative methods or taxonomic schemes. Second, the confusion of physical characteristics with social, cultural, or psychological traits has been a common feature of human racial classifications. While some species concepts, and thus subspecies designations, are based on the recognition of behavioural traits specific to particular taxa, the majority of species concepts and speciation theories are based only on morphological or genetic evidence, which themselves show incongruent or inconsistent microevolutionary patterns. Third, the relationship between ethnicity and human biology has been treated in far too simplistic a manner (see Chapter 3; Crews & Bindon, 1991; Chapman, 1993; Montagu, 1997).

In dealing with the specifically anthropological issues, the arguments of Stoler (1997) serve as a logical starting point for this discussion. Stoler relies heavily on the work of Michel Foucault in arguing that racial histories are subject to regimes of truth. That is, historiographies tend to focus too narrowly on accounts of the emergence of racism, its datings and its unique and recurrent attributes, while neglecting the issue of the changing meanings of race in an attempt to claim the originary moment of racial and racist thought. This argument will be expanded to focus on the domain of science, exploring how it connects to anthropological study. In this way, it is possible to move beyond a critique of institutional biopower[2] in the creation of race and/or sex categories

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[2] According to Foucault (1990[1978]:140–41), 18th century Europe saw new forms of governmental population control, particularly the emergence of demography, in order to ensure the maintenance of

that are typical of Foucault (1978; Stoler, 1995) and Laqueur (1990) (see Billinger, 2000), pushing forward in the pursuit of new knowledge. Stoler's interest lies not in the object of racism, but with scholarly accounts of its emergence. Similarly, this dissertation will not deal explicitly with the topic of racism, but in the problematic nature of scientific accounts of race and their effects on contemporary biological understandings of human variation. Stoler (1997:185) argues that historiographies of the development of race are typically "contingent on a basic and historically problematic contrast between a biologized, physiological and somatic racism of the past held up as fundamentally distinct from a more nuanced culturally coded and complex racism of the present." The result, according to Stoler, is that histories of racism often appear as narratives of redemption.

Tension thus exists between contemporary social and scientific perspectives: racial histories often do appear in the form of redemptive narratives, which in many ways invalidates the authority of such writing, yet scientists persist in bringing us continually closer to uncovering the 'real' and complex nature of human variation. While sociological and historical writings focus on revealing the fiction of somatic fixities (Stoler, 1997:186–187), scientific attacks on racial taxonomy by means of genetic or morphological evidence have also been criticized as attempts at redemption, or motivated more by political correctness movements than by evidence. Stoler's (1997:196–197) critical approach illustrates that a substantial problem associated with traditional discourse of the post-war period is the assumption that racisms rise and fall on the scientific credibility of the concept of race. The certainty of scientific data is not necessary for racism to thrive, according to Stoler, but that racial discourses proliferate

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production relations through the control of the social body. Such instruments of the State – institutions of power – created new forms of segregation and social hierarchy.

and produce new relations of power and knowledge in the contest over which linkages are considered 'true' or 'false.'

Using the example of phrenology – the pseudoscience based on the theory that a person's character and intelligence could be deduced from the shape of the skull[3] – Stoler explains that while it was quickly discredited in the 19th century as an inaccurate measure of racial endowment, little or nothing was accomplished toward undermining the discussion of, or attention to, the relationship between physical measurements and mental aptitude. Rather, phrenology provided a 'placeholder' in which the search for a relationship between phenotype and genotype was not generally disqualified, but only its specific coordinates, and that the search for a link remained an active and reasonable quest in scientific folk theories of race.

Taking Stoler's work a step further, this dissertation will not focus on the appearance and proliferation of the scientific race concept, but rather will view race as a concept that exists scientifically without clear definition, and around which ideas of human variation have developed. Even in light of new scientific evidence, perspectives on human variation continue to be structured within the racial paradigm. As in the example of phrenology, the notion of distinguishable racial differences (stemming from individual variation) was *a priori* to any reasonable understanding of biological difference. In modern biological science, racial language, and thus the race concept, remains the basic starting point in discussions of human variation.

The relationship between culture and biology is an important topic of research, because these relationships are not nearly as insidious or closely connected as once

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[3] Phrenology, or cranioscopy, was popularised by Franz Josef Gall around 1800, based on the notion of 'functional localization' of the brain (Simpson, 2005). See also Brace (2005:66–75).

thought (as in reductionist neo-evolutionary sociobiological theories); they are mediated by environmental factors, whether social or physical. Stoler (1997:198) explains that there are 'fixities and fluidities' in racial discourse, and that as such, this "'proof' that racial categories are fluid and not fixed confirms our political convictions that they can and should be undone." Thus, from this perspective, I will focus on why changing notions of the relationship between ontology and taxonomy, culture and biology, genotype and phenotype, the 'seen' and the 'unseen,' have been constantly re-evaluated, but the language of race in science has remained constant, providing a major obstacle to scientific advancement.

The first significant issue to be drawn from the literature then is the historical focus of the anthropological enterprise on the delimitation and classification of human races – not on the historical development of classifications or typologies – but the underlying notion of direct relationship between biology and culture that was implicit in anthropological theory. In this, I will focus specifically on the notion of anthropology as the 'science of culture' (Tylor 1871; White 1949), which I suggest has formed a deep philosophical rift within the discipline, and has led its practitioners away from finding answers to questions about race and human diversity because the discipline itself remains in a state of internal crisis over the authority of anthropological writing (Sass, 1986). The relationship between anthropological and scientific knowledge is complex, and warrants investigation into how anthropological and biological approaches to evolutionary theory affect, and are affected by, each other. However, there are issues relating to scientific authority coming from within the biological sciences as well.

Recently, some historians and philosophers of science have become critical of the supposed age-old dichotomy between constructivism and essentialism in biology, a dichotomy that has become rooted in the orthodoxy of Ernst Mayr's account of the advancement of evolutionary biological thought (Amundson, 1998; Greene, 1999; Winsor, 2003). Mayr, who was both a main actor in the development of 20th century evolutionary biology and a historian of biology, sees a distinct split between pre-Darwinian biology, typified by the essentialist (and typological) Linnaean hierarchy, followed by a great transformation into a fluid evolutionary mode of thought following the publication of Darwin's *Origin of Species* (1859). This would finally materialize in the evolutionary synthesis of the 1930s and 1940s, with Mayr's work at the centre of the synthesis. In order to further explore these issues, we can, as Latour (1987; Bowker and Starr, 1999) suggests, either look at what scientists say they are doing – working within a purified realm of knowledge – or at what they are actually doing: manufacturing hybrids of nature/culture. The latter suggestion is what Latour (1987) calls 'science in action,' and I believe that it is particularly interesting to contrast what scientists say and do, specifically regarding the issue of racial classification, and the relationship between theory and methodology. An entire dissertation could be written applying Latour's actor-network theory (ANT) to Mayr's role in the history of biology, but I will touch on this only peripherally here.

These issues are theoretically significant because the race concept is bound up in misunderstandings about evolution, the principal theory that occupies much of Western philosophy. Evolutionary ideas, as we can see in the epistemology of evolutionary biology, form the basis of contemporary understandings of the natural world, in social, economic, and biological contexts. Again, when attempting to deconstruct the race concept, the goal is not to focus too closely on the various classification schemes that have been proposed, but rather to ask questions such as that posed by Eze (2001:21): “I ask, then, not if Hume or Kant, as individual, was racist. It is more philosophically interesting, I believe, to inquire about the ways modern philosophy articulated and used the idea of race, and to what ends?” Further, Eze asks (2001:29–30): “If geneticists have shown that ‘race’ is not a useful concept, it may be up to the social scientists to invent new ways of making this development available to the general culture.” Here is where the contribution of the history and philosophy of science lies: exploring the interface between social and biological sciences.

This is where I believe the strength of this project lies. If the ‘race problem’ in anthropology can be clarified rather than simply re-articulated, by demonstrating the problems of race and subspecies designations in other disciplines as well as our own, then new ways of exploring human variation can be developed. Histories of racial thought have filled innumerable volumes, and they have served us well, but such treatments, including my own Master’s thesis (Billinger, 2000; see also Smedley, 1999; Brace, 2005), should now simply provide a background from which a new line of argumentation will take shape. The goal, then, is to bring together theory and method in the analysis of human biological variation.

Race is a classificatory problem, and the main problem of classification generally is that the history of science has consistently found that there is no such thing as a natural or universal classification system. Foucault gives one of the best-known examples of the general disorder of taxonomy in his preface to *The Order of Things* (1970[1994]:xv).

Borrowing from Jorge Luis Borges' quotation of "a certain Chinese encyclopaedia," he describes the following constructed list:

> animals are divided into: (a) belonging to the emperor, (b) embalmed, (c) tame, (d) sucking pigs, (e) sirens, (f) fabulous, (g) stray dogs, (h) included in the present classification, (i) frenzied, (j) innumerable, (k) drawn with a very fine camelhair brush, (l) *et cetera*, (m) having just broken the water pitcher, (n) that from a long way off look like flies.

What this rather comical example tells us is that any system of ordering can result in a classification scheme, but not necessarily a systematic one; *most* methods of systematic taxonomy in the biological sciences are based on (assumed) natural relationships derived from the analysis of functional anatomy and/or genetics. In attempting to clarify this problem, I want to first assert that in traditional anthropological usage, racial categories serve simply as *nomenclature* – they are not based on any systematic form of classification (at least by modern standards). Anthropological categories of race, before and after the works of Blumenbach (1795; 1865) in physical anthropology and Darwin (1859; 1871) in biology, who both set new standards in their respective fields, were based primarily on external physical features such as skin colour, hair texture and distribution, and nose and eye form. The result is a classification that is qualitative or descriptive; the analyzed features are plastic, and such classifications are therefore highly subjective, and of little value if any for understanding genetic patterns. Underlying all of these issues is a general confusion between homology and analogy (Lewontin, 1991:69). Homologous traits are derived from a common biological origin, whereas analogous traits look superficially alike, but are not (necessarily) directly related genetically or morphologically. This confusion will be a running theme throughout the dissertation, since it has been at the root of both cultural and biological analyses of human diversity and variation.

Physical anthropologists today study historic and prehistoric populations through osteological analysis. Similarly, paleontologists use fossil evidence to explore evolutionary change and relationships. If we look at what physical anthropologists such as Earnest Hooton and Carleton Coon were both saying and doing in the 1930s through to the 1960s (see Chapter 2), their research programs went well beyond the study of human anatomical variation, focusing on the evolutionary relationship between heredity and behaviour/intelligence as the units of phenotypic analysis. Thus, the historical quest to equate skull shape and intellectual and cultural achievement was perpetuated well into the 20th century.

Even if we are to divorce 'hard' morphological analyses from the soft phenotypic variables, we still need to be cognizant that the diagnostic characters of morphology are far removed from the immediate sites of gene action, and typically, variation in these characters is, at least, partly due to the direct environmental variation on growth and development. Traditionally, many physical anthropologists, like many geneticists, tended to regard environmental effects as little more than a nuisance that conceals the nature of hereditary variation (Harrison, 1969). But if we understand that the human skeleton can provide information about both the patterns and processes of evolution, we can push ahead toward better understanding the nature of our biological variation. Adaptive or continuous morphological traits allow us to explore not only the plasticity of skeletal structures, but also the processes of natural selection in various geographic environments, and human migration around the globe.

Further, non-functional skeletal or epigenetic traits, as has been demonstrated recently by Hanihara et al. (2003), generally follow genetic patterns of clinal variation as demonstrated by Cavalli-Sforza et al. (1994), thereby suggesting that we can have at least limited access to genetic biological relationships through bone. Interpreting craniometric and epigenetic results together reveals that while clinal variation occurs across geographic regions, morphological discontinuity exists in isolated populations suggesting that limited gene flow and local adaptation allows peripheral groups to maintain their genetic distinctiveness (Hanihara et al., 2003:247). These peripheral groups occupy what Cavalli-Sforza et al. (1994) call 'ethnic regions.' However, the isolation mechanisms at play may not simply be geographic or result in a biologically significant level of morphological or genetic differentiation as racial categories assume.

There is a direct relationship between human variation and geographic ancestry, but large geographic groupings explain little of the variation between smaller local groups, which themselves are diverse. The problem with geographic ancestry is that human groups do not form discrete entities at either the continental or the local level. While local or ethnic groups may generally practice endogamous or internally-selective mating, this pattern is by no means absolute, and relations between such groups are historically contingent. Some anthropologists do see the preference for endogamy as an essential process rather than the result of sociological phenomena, however. Gil-White (2001) makes interesting arguments in this regard, in which he claims that the human mind processes ethnic groups as if they were natural species. This ties also to the works of Hirschfeld (1996) on the cognitive development of racial categories and Atran (1990) on folk taxonomy. These issues provide another line of investigation to be explored in Chapter 3.

Deniker (1900[1904]) first introduced the idea that human biology is ethnically structured, and this notion was more clearly elaborated by Huxley and Haddon (1935). Though this ethnic concept was adapted by Ashley Montagu as a genetical theory in 1942, little is understood about how it can be applied to re-building human taxonomy. This leads to another key question: *do (or can) humans fit into systematic models of evolutionary and phylogenetic change?* If we can understand how to conceptualize the flow and/or isolation of genes and the relation to endogamous or exogamous practices, and human mobility through time, then we will move much closer toward understanding both the patterns and processes involved in creating the morphological variations of prehistoric, historic, and contemporary populations. The following pages take up this challenge.

Chapter 1

ANTHROPOLOGY AND SCIENCE

As indicated in the Introduction, race remains a problematic scientific concept with serious methodological ramifications for the various disciplines dealing with social and biological aspects of human evolution. Both the general and specific problems of racial classification will be addressed in the following chapters, but the focus of this particular chapter is not so much on the complexity of the taxonomic issues in light of natural selection and speciation. Rather, it will concentrate on the evolution of the anthropological enterprise itself, exploring how anthropologists have created their own barriers for exploring deeper explanations of human variation and diversity, whether cultural or biological. I will argue that a preoccupation over the status of anthropological knowledge in the history and philosophy of science has provided a conceptual barrier not only to understanding how to move beyond racialist explanations, but in understanding the ways in which culture and biology relate to one another.

The results of the surveys conducted by Lieberman et al. (2003) discussed in the Introduction demonstrate that there are divergent opinions about anthropological subject matter and anthropological knowledge, even from within. Physical and cultural anthropologists most certainly experience the discipline in disparate ways. This too might also be expected of archaeologists and linguists. Each of the subdisciplines of anthropology subscribes to its own specialized methodology, theoretical frameworks, and internal debates (Stocking, 1974:35). However, what stands out as an apparent and considerable issue is the lack of connection between the subdisciplines in a discipline that has a history built upon the notion of holism, at least in its North American incarnation. This issue was addressed as early as the 1930s by two of the main figures of American anthropology, Boas (1940[1932]:243–259, 1940[1936]:172–175) and Montagu (1940), with particular focus on the relationship between social and physical anthropology. While physical anthropologists and archaeologists often have little problem convincing others of the scientific nature of their research because of their implicit connections to biology, geology, and chemistry, socio-cultural anthropologists[1] appear to struggle to demonstrate authority in their accounts of the cultural domain. Consequently, the gap between the anthropological subdisciplines has widened.

According to Stocking (1968:vii), a historian of anthropology, anthropological research from about 1800 to the mid 1930s was primarily concerned with the interrelated ideas of race, evolution, and culture, which were all linked to understandings of the diversity of modern humans. The result of this widening gap between social and physical anthropology has been the failure of a concerted effort to clarify the ambiguous and arbitrary concepts that have continually plagued the discipline of anthropology as a whole – particularly *race* and *culture*, and the misapplication of Darwinian evolution. Race has historically bonded culture and evolution together; like race, the concepts of culture and evolution remain problematic in modern anthropology. On evolutionism in anthropology, Marks (2004:181) asserts, "much of what is out there in the literature of so-called Darwinian anthropology is a paradoxical attempt to claim the ground of science and yet present a Darwinism that is often only metaphorical, and sometimes even unrecognizable

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[1] 'Socio-cultural anthropology' will be the preferred terminology from this point on. 'Social anthropology' and 'cultural anthropology' are generally used synonymously, though social anthropology is derived from the 19th century focus on ethnology, particularly in British anthropology, whereas cultural anthropology has its basis in 20th century ethnographic study.

as Darwinism." On the topic of culture, Douglas (1975:886), though somewhat facetiously, suggests, "never was such a fluffy notion at large in a self-styled scientific discipline, not since singing angels blew the planets across the medieval sky or ether filled in the gaps of Newton's universe." It is clear that the main themes of anthropological study are not without controversy, which only serves to complicate the notion of a scientific anthropology.

Anthropology as Science

Anthropologists have historically considered their discipline a science, likely stemming from the early connection to cognitive psychology, with the basic comparative method in 19th century ethnology relying on the notion of *psychic unity* (Stocking 1968:115). The concept of psychic unity was predicated on the belief that the mind of the 'savage man' had no appreciable biological difference from that of the 'rational man,' and that they had the same potential for intellectual abilities and civilization, but were, rather, products of environment (Smedley, 1999:252). It was this environmentally bound notion of psychic unity that intertwined the concepts of evolution, culture, and race.

Spencer (1855, 1862) believed that structure could be understood by comparing the mind of 'primitive man' to his external environment. Rather than *examining* the relationship between culture and environment (which would form the basis of Darwin's biological functionalism and the cultural ecology movement in early 20th century anthropology), Spencer *assumed* that the simplicity of the savage mind was the result of its adaptation to a simple environment, requiring only impulsive, reflexive, or imitative responses to various stimuli. This perspective would indeed legitimize class differences as essential for Spencer and his followers, and would provide a solid grounding for explaining the selective processes leading to the mental and moral development of various populations, particularly Europeans (Stocking, 1968; Smedley, 1999). The idea that culture depended on intelligence in order to flourish was popular throughout the latter-half of the 19th century even though the mechanisms of genetic inheritance were unknown until the re-discovery of Mendel's pea plant experiments in 1900.

Toward the end of the 19th century, Tylor (1871) and Morgan (1877) both espoused the notion of psychic unity and unilineal evolution as applied to human cultures, arguing that societies progressed through parallel (but independent) evolutionary stages ranging in order from savagery to civilization (Stocking, 1968; McGee and Warms, 1996; Mukhopadhyay and Moses, 1997; Carneiro, 2003). The basic assumption that all cultures are different but that they progress through similar evolutionary stages that can be studied systematically and comparatively (i.e. scientifically) served as the philosophical foundation of 19th and early 20th century anthropology.[2]

Tylor (1871) first used the phrase 'science of culture' to describe anthropology in *Primitive Culture* (White, 1949:xix). According to Harris (1968:1), "Anthropology began as the science of history. Inspired by the triumphs of the scientific method in the physical and organic domains, 19th century anthropologists believed that socio-cultural phenomena were governed by discoverable lawful principles." The implication that biology and culture are implicitly connected brought psychology and anthropology closely together, but the notion that culture is shaped principally by biologically inherited traits would

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[2] According to Shore (1996:18), "The idea of general cultural evolution made possible a comparative ethnology by providing anthropologists with a single evolutionary track and an associated taxonomy of historical types for classifying human variability."

ultimately rise and fall with phrenology. The assumption of a connection between cultural variation and biological inheritance came under widespread scrutiny through the 20th century (even with a minor influx of sociobiological theories in the 1970s) and particularly with the rejection of the scientific mandate by the critical social sciences in the 1980s and 1990s (Morell, 1993). Anti-metaphysical doctrines common to Friedrich Nietzsche, Martin Heidegger, Jacques Derrida, and particularly Michel Foucault, characterize this rejection of universal explanations of reality or knowledge (Rorty 1999:120). Foucault's (1970, 1978) works were particularly influential for social and cultural analyses, exploring the shifting patterns of power within societies and the ways in which power relates to the self, and seeing knowledge as an inherently subjective enterprise.

Since this 'postmodern turn' in anthropological theory in the 1980s, anthropologists have necessarily had to reconsider the state of their own disciplinary (and subdisciplinary) knowledge and its relationship to other disciplines in the social and natural sciences (Morell, 1993). The notion of a scientific anthropology suffered a severe blow when traditional methodological texts were replaced with Clifford and Marcus' (1986) *Writing Culture*, which served to question the anthropological past, particularly the notion of objectivity and authority in ethnographic writing. More recently, however, the relationship between science and culture has become increasingly problematized. In this light, perhaps more interesting than the question of whether or not anthropology is a science is why this question is important to anthropologists.

The science question has profoundly affected contemporary anthropology by thoroughly permeating the anthropological study of cultural and biological variation, based on an evolutionism better attributed to Spencer than Darwin (Marks, 2004:182). Borrowing from the general body of literature known as 'science studies,' which flourished in the 1990s (see particularly Biagioli, 1999), many anthropologists have become critical of the role of science not only as it relates to anthropology, but view science as an anthropological belief system itself. However, this recent trend toward science and technology studies within socio-cultural anthropology has only served to compound the problematic nature of a scientific approach to the study of culture by shifting to a cultural approach to science, while neglecting the integration of other forms of anthropological strategies of addressing key issues.

For Harris, this shift away from the search for lawful principles was apparent from the beginning of the last century, but reflecting back upon the disciplinary changes he has witnessed since his writing of the seminal work, *The Rise of Anthropological Theory* (RAT) in 1968, he explains the impact of these intellectual shifts in *Theories of Culture in Postmodern Times* (1999:13):

> I must confess that the turn theory has taken – away from science-oriented processual approaches and toward an 'anything goes' postmodernism – has been far more influential than I thought would be possible as I looked ahead from the end of the 1960's. So influential, indeed, that I was tempted to call this FAT – the Fall of Anthropological Theory ... But the 'pomos' victory is far from total and certainly not permanent. There are increasing signs that the influence of interpretationism, ethnopoetics, and other 'crit lit' approaches to culture has peaked. I hope this volume will help to push the pendulum back toward the science-oriented side. (Let the grinches who stole culture give it back).

Although Harris has great faith that the dominant anthropological paradigm will shift back to a scientific epistemology, we should explore what this actually means and how it relates to the integration of cultural and biological approaches.

On the roots of the nature of anthropology itself, Geertz (1995) suggests that anthropology may be prone to internal crisis because of its unstructured nature:

> Of all the human sciences, anthropology is perhaps the most given to questioning itself as to what it is and coming up with answers that sound more like overall worldviews or declarations of faith than they do like descriptions of a branch of knowledge ... The first index entry nowadays in books surveying the field is often: 'anthropology, crisis of ...'
>
> Yet, the 'crisis' may be an optical illusion – the result of trying to define 'anthropology' as one would define 'English,' or 'linguistics,' or 'entomology,' as the study of something or other, rather than as a loose collection of intellectual careers.

If a definition of 'anthropology' is seemingly impossible, it is no wonder that acceptable definitions of 'culture' and 'race' remain so enigmatic. First, we should consider what anthropologists mean when they refer endlessly to 'culture.' Boas' consideration of culture as a shared body of knowledge leads to quite different questions from those engaged by A.R. Radcliffe-Brown, with his interests in society as a functional and interlocking set of relationships (Barnard, 2000:xi). These competing definitions also beg the question of the difference between society and culture, and thus the perceived need of many socio-cultural anthropologists to distinguish themselves from sociologists. According to Barnard (2000:10–11), the main subject of sociological inquiry is that of the social unit, whereas anthropologists are concerned with analyses of culture as a shared set of ideas, skills, and objects.

Boas used his historical leaning to approach history scientifically, incorporating a multi-strategic method for recording ever-changing (evolving) cultures as they existed in a particular historical moment ('historical particularism'). Bronislaw Malinowski, on the other hand, focused strictly on a systematic methodology for recording cultural data significant to the functional analysis of the psychology of primitive societies and their cultural institutions. The common link between these two scholars was that their scientific approaches were both informed by formal training in the field of physics, particularly their interests in the issues of movement in terms of population, politics, and economics (migration, colonialism, and the power of movements of capital) (Ruiz, 1999:74–75).

Even though Boas (1940:258) considered anthropology a 'historical science,' he was not at all fond of generalizations. Referring to the notion of scientific laws as applied to the anthropological enterprise, Boas (1940:311) explained:

> In my opinion a system of social anthropology and 'laws' of cultural development as rigid as those of physics are supposed to be are unattainable in the present stage of our knowledge, and more important than this: on account of the uniqueness of cultural phenomena and their complexity nothing will ever be found that deserves the name of law excepting those psychological, biologically determined characteristics which are common to all cultures and appear in a multitude of forms according to the particular culture in which they manifest themselves.

The focus on cause and effect that came out of Boas' and Malinowski's physics backgrounds surely influenced their anthropology, but this scientific influence does not necessarily mean that anthropology is science. Boas did consistently attempt to bring both the social and cultural realms together with an understanding of the mechanisms of biological evolution, as is evident in his words above. He quite obviously doubted the existence of universal laws of culture, and though he may have promoted a scientific approach, he quickly advanced the notion of cultural relativism in American anthropology (Boas, 1911), which is seen as his primary achievement.

The main premise of Boas' relativism was anti-evolutionary in a cultural sense: that no objective standards exist for evaluating cultures, but rather they should be understood as products of a unique history (Stocking, 1968:195–238; Handler, 1990:258;

Carneiro, 2003:107). Thus, the concepts of cultural relativism and historical particularism are intertwined. In this sense, Boas conceived of culture as one of the *contents* of the mind rather than as a defining *attribute* of the mind (Shore, 1996:22). When Boas speaks of biologically determined characteristics, he insists that only an adherence to a strict scientific method can demonstrate their existence – if they actually do exist – rather than assuming that certain characteristics can or should be attributed to different evolutionary trajectories.

Kroeber and Kluckhohn (1952:145) built upon Leslie White's (1949) cultural ecology and neoevolutionism, but moved away from deterministic arguments. They define culture as "a set of attributes and products of human societies, and therewith of mankind, which are extrasomatic and transmissible by mechanisms other than biological heredity," whereas White firmly believed that culture is firmly rooted in evolutionary biology. The esteemed Japanese ecologist Kinji Imanishi (1952) provides a definition of culture similar to that of Kroeber and Kluckhohn as the non-genetic transmission of habits in the context of primate social structure (see particularly Itani and Nishimura, 1973, for an English interpretation of Imanishi's Japanese writing; see also de Waal, 1999). Even the seemingly subtle differences in definition and approach demonstrate the plurality of conceptions of 'culture,' and thus of epistemological underpinnings of modern anthropological approaches. In reality, even divorcing the notion that biology and culture are interconnected does not move away from the essentialism of cultural evolution.

Neoevolutionists such as White and Harris have fought forcefully against Boas' relativism (Lewis, 2001), though they have somewhat similar notions of culture on the surface. These different interpretations became much more explicit, however, with the development of competing anthropological paradigms throughout the intellectual history of the latter 20th century. Harris (1999:19) explains:

> For some anthropologists, culture consists of overarching values, motives, and moral-ethical rules and meanings that are part of a social system. For others, culture embraces not only values and ideas, but the entire set of institutions that humans live by. Some anthropologists see culture as consisting exclusively of learned ways of thinking and behaving, while others emphasize genetic influences on the repertory of cultural traits. Finally, some see culture as consisting exclusively of thoughts and ideas, while others maintain that culture consists of thoughts and ideas plus associated activities.

Contemporary anthropologists tend to rely less on strict definitions of culture than on assumptions of what culture entails, and how they can gain special access to it, whatever they assume *it* to be. Thus, since science has traditionally been seen as providing special or authoritative access to knowledge about the physical world, anthropologists have everything to gain by claiming that their discipline is scientific, providing authoritative access to knowledge about the social world and the role of culture in structuring it. Gould (1980; see also Greene, 1999:107) refers to this scientific quest as "physics envy," which he explains has been a common feature in psychology as well as paleontology.

Like Harris, many anthropologists continue to argue that anthropology in general, and socio-cultural anthropology in particular, is indeed a science (Calcagno, 2003:10). Those who are less sure of a positive answer usually offer qualified rejections. The problem here is that the scientific aspect exists only in assumptions of methodology, while the concept at the centre of anthropological inquiry – culture – may not be an object suitable to scientific explanation. It is important to delve into this because the concepts of race, culture, and evolution are so intricately tied through the extension of individual inheritance to that of racial heredity (Boas, 1940:265). These concepts are fundamentally, though not explicitly, connected as causal factors: race has been considered the cause, evolution the process, and culture the result of essential biogenetic processes. Even with the rejection of race and culture being intertwined, the notion that race and evolution are inextricably linked persists in a wide range of literature. In terms of the science of race, much of the contemporary physical and forensic anthropological literature also offer a series of qualified rejections – explanations of why race is a problematic concept (in social terms) but how it still has utility (Gill, 1990; Gordon, 1993; Church, 1995:2–6) – and the core concept, however troubling and confused, perpetuates.

We must necessarily revisit the basis of modern anthropological theory in order to critically examine the methodical strategies of the ‘science of culture’ and to re-evaluate the traditional scientific position of the discipline in academia. Calder (1963) makes two important arguments that need to be examined here in this regard. The first is that judgements about science are often made by those who know little about science but who are highly impressed by its results. It seems obvious that Boas and Malinowski were both impressed by scientific knowledge and well versed in the physical and biological sciences, but they are not typical of socio-cultural anthropologists, many of whom may indeed be highly impressed by science regardless of their knowledge of it. The second argument is that the social sciences are underprivileged in relation to the ‘hard’ or ‘exact’ sciences but have never been more necessary than they are today to deal with the confusions and conflicts the exact sciences are themselves producing.

Though critically important, the arts and social sciences are still struggling for status in a highly competitive market for institutional and governmental research funding, which seems to be flowing mostly in the direction of the natural sciences, engineering, and medicine (Cairney, 2002). Despite increasing enrolment in university-level anthropology programs, the discipline remains a low priority in the allocation of research funding for both universities and museums in North America and in Central Europe (Khittel et al., 2004:14–15), and likely world-wide.

What is Scientific Method?

In anthropological literature dealing with the topic of science and/or methodology, most often a discussion of what constitutes a scientific method is absent. This question has been of major concern to historians of science, however, who trace the origins of modern science to the scientific revolution of the sixteenth and early seventeenth centuries. A classic monograph on the scientific revolution is Butterfield's (1950) *The Origins of Modern Science*, which details the breakdown of ancient grand (theological) explanations of the world in the early 17th century with the advancements of mathematics and geometry and the development of scientific instruments, allowing for accuracy of observation and measurement, particularly as applied to experimentation.

According to Butterfield, modern science has its origins in the discovery of new facts about the universe by Copernicus, Kepler and Galileo, but modern scientific method emerged out of Isaac Newton's hypothetico-deductive method, which was a synthesis of the empirical, experimental and inductive method championed by Francis Bacon with the rational, mathematical and deductive system developed by Rene Descartes (Butterfield, 1950; Blake et al., 1989). Perhaps the most important criteria set out in the post-scientific revolution era are those of testability and falsifiability. Most proponents of science studies (i.e. history and philosophy of science, sociology of scientific knowledge, anthropology of science) turn to either Kuhn (1962) or Popper (1934; English translation, 1959) for a general philosophy, and Popper specifically for the methodological prescriptions of falsification (Bloor, 1991:57, 61). Though the status of Kuhn's (1962) *The Structure of Scientific Revolutions* is not without controversy for its rather simplistic account of the 'paradigm shift' (Fuller, 2002; Sankey, 2002), and Popper's (1959) rationale for his theory of falsifiability are considered by some to be weak (Maxwell, 1972), or thoroughly misunderstood by others (Hull, 1999a), many still find these works generally suggestive (Nickles, 2003).

Popper's (1959) *The Logic of Scientific Discovery* outlines an experimental methodology and theory of falsification, in which he argues that a scientific theory can never be accepted more than provisionally, since no theory can be subject to verification, only falsification. Popper's notion of falsifiability relies on a deductive method of testing a theory. This deductive cycle relies on the derivation of an entailed proposition from a general theory that must hold true if the theory is true, followed by an attempt to determine the validity or impotency of the proposition through testing by empirical data. If the entailed proposition is shown to be false, then the general theory can be considered to have been falsified. For Popper (1963), falsification is not simply about the evolution of scientific explanations, but relates to a more fundamental question: when should a theory be ranked as scientific?

Kuznar relies heavily on both Kuhn and Popper in asserting that a scientific anthropology must adhere to the notion of a reality independent of humans, and that with this assumption of reality, anthropologists must clearly differentiate scientific research from metaphysics. According to this argument, anthropologists can scientifically address any issue that is amenable to empirical study and that anthropologists can and are willing to find false. With this neo-positivist approach, which seeks to understand causal relationships, Kuznar (1997:214) argues, "anthropologists must be able to deduce hypotheses from theories that predict specific states of nature so that they can then do further research to test the hypotheses and in turn the theories. This testing is absolutely crucial and represents, more than anything else, the hallmark of scientific research." Kuznar (1997) provides a good example of the current thought of anthropologists who associate themselves with the anthropology-as-science position, in his attempt to re-establish the core arguments of some of what he considers foundational works: Binford (1977, 1982, 1983), Harris (1979), Watson (1992), Bernard (1994), and D'Andrade (1995a,b), among others.

Guille-Escuret (1999) argues that contemporary anthropology generally appears non-scientific, but that this appearance lies within the discipline itself and not with the appearance that is expected; anthropology has trapped itself within the idea of 'exact' and 'inexact' science. He claims that the basis for this condition is the tension between the British model of social anthropology, focusing on invariables, and the ethnological basis of American socio-cultural anthropology. Further, this split, which has led to a move away from the search for a unifying theory of social structure, fuels the epistemological stance that humanistic theories have no place in the scientific world of mathematics and physics. Focusing on the comparative nature and subsequent ostracism of contemporary anthropology, Guille-Escuret argues that the scientific enterprise has not yet recognized the relationship between experiment and comparison and that there is no epistemological theory explaining why non-experimental comparison is incapable of producing a refutation.

What this has meant for anthropology is that scientific-cultural writings in the style of anthropology's early masters such as Boas, Radcliffe-Brown, Malinowski, and E.E. Evans-Pritchard are no longer acceptable in today's academia:

> One thousand, two thousand, five thousand pages can no longer claim to define a culture – or twenty years in the field. To do that, one has to make a critique of previous inadequacies and list the descriptive imperatives the disastrous absence of which has gradually been revealed. In short, a chronic work of refutation has to be produced – methodically, even when not methodologically done (Guille-Escuret, 1999:10).

Thus, Guille-Escuret argues that the misapplication of Karl Popper's theory of falsification, which acts to reinforce scientific gatekeeping, along with the strong relativist position that has solidified in the post-*Writing Culture* age has, in effect, reduced ethnographic writing to literary fiction. Such refutation, at the same time, strictly adheres to scientific principles, but by systematically breaking down its own bases (i.e. pre-existing works), it becomes not only refuting, but also refutable itself. However, in the context of natural science, this process results in accepted wisdom being invalidated and facts re-evaluated, whereas in the social sciences, refutation takes aim at entire disciplines. Therefore, the perpetuation of such principles in anthropology only serves to fuel the denial of the capacity of science to incorporate the social.

Kuznar (1997:17) suggests that the scientific aspect of the anthropological enterprise is much more deeply entrenched than simply in the academic backgrounds of our anthropological heroes:

> Anthropologists have developed a more-or-less coherent scientific approach during this century that has the following central features: an assumption that there exists an external, knowable world; a realization that theories are humanly constructed and thereby require evaluation; the adoption of the procedural rule that a theory's fit to all relevant data is the most important criterion for acceptance or rejection of a particular theoretical position.

Following the assumption that there is a real and knowable world is a belief that it is orderly and can be predicted and explained. Shifting paradigms are a cause of great anxiety for such-minded anthropologists like Kuznar (1997:7), who explains:

> My perception is that the tide, especially within anthropology, is turning against science as an epistemological approach, and I am concerned that anthropology may abandon methods that have yielded much sound knowledge in favor of other epistemologies that will turn out to be arbitrary, unreliable, and subject to political fiat. Such a shift will result in the generation of less objective knowledge.

This quest for objectivity and apparent need to distinguish anthropology from the rest of the humanities or social sciences is manifest in traditional socio-cultural anthropology textbooks, which are based on the belief that dispassionate cultural knowledge is attainable through participant-observation research.

Perhaps the best example of this focus on systematic data collection that is part of participant-observation is the publication of the landmark text *Notes and Queries on Anthropology*, first published in 1874 by the British Association for the Advancement of Science. Through six editions (1874–1951),[3] this manual presented standard methods for all areas of anthropology, but particularly ethnographic research, compiled by a

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[3] First edition: British Association for the Advancement of Science, 1874; Sixth edition: Royal Anthropological Institute of Great Britain and Ireland, 1951.

committee of pre-eminent anthropological scholars. This focus on a systematic study of culture according to strict methodological guidelines was necessary in order to build an objective appearance for anthropological knowledge. Though anthropologists may claim that their observations are recorded conforming to strict scientific methods, it does not automatically follow that the data collected and analysed are 'scientific.' Even though Boas and Malinowski approached the recording of culture scientifically, they would not suggest that their results were essential and objective, but simply that they were methodologically sound, and therefore reliable documents.

Comparisons of anthropological methods to those of modern science reveal many incompatibilities, such as the lack of hypothetical models and experimental procedures by anthropologists, contrary to Kuznar's urging. According to Marks (2004:182), the attractiveness of incorporating a Newtonian perspective into the social sciences and humanities lies in the desire to reduce complex phenomena to simple, more fundamental processes as Newton did for matter and motion. The failure to develop a positivist or neo-positivist social science lies in the extension of Newton's model of the physical world to that of the social world because of the inability to develop social laws of causation (O'Meara, 1997:399). D'Andrade (2000:226) argues that much of the problem for scientific anthropology lies in the fact that one cannot undertake cross-cultural research without a working knowledge of statistical analysis, and that statistics and quantitative methods are disliked because "they are always a challenge to those who believe they already know the truth." The problem here lies in the lack of methodological rigour, which is followed by the inability to reduce 'culture' to something quantifiable. However, with the race concept, one of the main factors is that there have been endless attempts to quantify it, with little understanding of what that quantification actually demonstrates theoretically.

Because of these methodological failures, the history of anthropology was largely ignored as a subject of study for most of the 20th century (see Darnell, 1977, 2004)[4] in favour of anthropological theory, which is more focused on the analysis of anthropological perspectives or paradigms, and their connection to the philosophy of science in general. While many traditionally-minded anthropologists feel that this conceptual shift has nearly destroyed the authority of anthropological inquiry, others derive hope from the creation of many new areas of inquiry, with the cultural critique of science being one of the most popular emerging areas. Barnard (2000:8) explains, however, that such conceptual shifts are not altogether unlike those in other disciplines:

> There is some dispute about whether or not anthropology can really be considered a science in the sense that physics is, but most would agree that anthropology at least bears some relation to physics in having a single overarching framework (in this case, the understanding of humankind), and within this, more specific paradigms (such as functionalism and structuralism). Within our paradigms we have particular facts and explanations which make up any given anthropological study. Anthropology goes through 'revolutions' or 'paradigm shifts' from time to time, although the nature of ours may be different from those in the natural sciences. For anthropology, fashion, as much as explanatory value, has its part to play.

If the concept of culture is problematized, such an overarching framework must lose its ostensibly objective stance. Culture is simply one explanatory strategy, one that cannot be subject to absolutist claims, because it is, like its definitions, a fluid notion. Anthropologists who are concerned with maintaining the authoritative boundaries of the discipline will be constantly at a loss because culture itself has no boundaries, and in a

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[4] Darnell (1977: 399) argues that while there have been a few anthropologists who have insisted that the history of anthropology is integral to anthropology's current practice, this body of research has not been given a significant role in the teaching or practice of contemporary anthropology.

scientific sense, it is far too broad to be designated as either a causal factor, or as a concrete structural effect. Further, revolutions or paradigm shifts in socio-cultural anthropology, as in the arts, humanities, and social sciences, are generally fuelled not by new discoveries (or new interpretations of old discoveries) as in the natural sciences, but by philosophical trends or, as Barnard puts it, fashion.

With this anthropological questioning of science becoming increasingly fashionable, Franklin (1995) explains that the dominant constructivist view in the 1990s created a shift from an anthropological focus on gender and culture [and race] to a cultural analysis of science. Franklin (1995:165) presents three key arguments to explain this shift. First, anthropology is a science and has the tools to understand science as a form of culture. The culture concept has been reshaped by the necessity for anthropology to interrogate its own knowledge practices. Second, the fact that any attempt to question a foundational belief system such as Western science makes its practitioners feel threatened is not difficult to understand. Finally, science is defended so vehemently because it is cultural, not because it is extracultural.

These three arguments relating to science-as-culture do not help the cause of the proponents of anthropology-as-science. If science is a form of culture (i.e. a foundational belief system), and defended because it is not extracultural, the science-as-culture position must necessarily result in the destruction of scientific authority, since it could never provide any greater knowledge than that which is culturally accepted folk knowledge, which is regionally and temporally specific. This Boasian relativism – that culture is always relative to time and place – remains a central concept in modern anthropological thought. In the wake of the influx of (extreme) relativism brought by the 'postmodern turn' and *Writing Culture*, if science is to be regarded as a belief system, then the questioning of such cosmology goes firmly against the principles of relativism from which these critiques arose. Socio-cultural anthropologists simply cannot have it both ways. If there is an implicit need for anthropologists to claim to be scientific, which, in effect, leads to the claim of belonging to a higher intellectual culture – creating a split between notions of modern and primitive – it has come at the expense of a workable solution to the changing and inconsistent definitions of culture.

Interpretive and Experimental Sciences

For a definition of culture as applied specifically to intellectual traditions, we can turn to Snow (1959), whose famed work, *The Two Cultures and the Scientific Revolution*, draws a comparison between two traditionally polarized groups: scientists and traditional (literary) culture. In utilizing the term 'culture,' Snow explains that he simply uses it in an anthropological sense, and in writing about the culture of scientists, he states, "Without thinking about it, they respond alike. That is what a culture means" (Snow, 1963:10). Snow (1963:14) further argues of the literary culture, "They dismiss them [scientists] as ignorant specialists. Yet their own ignorance and their own specialization is just as startling." Franklin (1995:165) draws a comparison between Snow's two cultures and Geertz's (1973:5) contrast between "an experimental science in search of laws" and an "interpretive one in search of meaning." Do these cultural distinctions need to be so fundamental?

Bourdieu (as cited in Rabinow, 1999:321) sees little problem with looking beyond the 'types' of science but rather is interested in actual production of science:

> I hold that, all the scholastic discussions about the distinctiveness of the human sciences notwithstanding, the human sciences are subject to the same rules that apply to all sciences ... I am struck, when I speak with my friends who are chemists, physicians, or neurobiologists, by the similarities between their practice and that of a sociologist. The typical day of a sociologist, with its experimental groping, statistical analysis, reading of scholarly papers, and discussion with colleagues, looks very much like that of an ordinary scientist to me.

A typical day for an anthropologist, whether socio-cultural, physical, or archaeological, is no different than that which Bourdieu describes. Nonetheless, the split between the search for laws and the search for meaning has caused great philosophical division within the discipline of anthropology, even within the subdisciplines. In this regard, Mintz (1999) describes his last encounter with the late anthropologist Eric Wolf during the 1998 American Anthropological Association meetings. Wolf asked him: "Do you realize ... that the three or four thousand people who have come here, and who describe these meetings as anthropological and themselves as anthropologists, for the larger part no longer have anything to say to each other?"

Has this quest to be considered science effectively fragmented the discipline of anthropology into a number of incompatible micro-cultures? What about the discontinuities between national anthropologies, colonialist, orientalist, and nativist perspectives, etc? Has sub-disciplinary specialization completely dissolved a common anthropological discourse? The at least honorary scientific status of physical anthropology and archaeology, due to their close relationship to the 'hard' or natural sciences and the knowledge derived from actual physical discoveries, could certainly be cause for the maintenance of boundaries within the 'interpretive science' of socio-cultural anthropology and its subdisciplinary offshoots. What does the future hold for the entire anthropological enterprise in light of this fragmentation? According to Snow (1963:18), "There is only one way out of this: it is, of course, by rethinking our education."

Annette Weiner's 1993 presidential address to the American Anthropological Association called for an integrative engagement with postmodern culture, returning to the traditional American four-field approach (Weiner, 1995). According to Franklin (1995:164), "Although many would support Weiner's exhortation that biological anthropology graduate students become conversant with the cultural construction of genetic research, and the future cultural anthropologists of science and technology 'intensively study biological anthropology,' such crossovers are fraught with controversy amid the 'science wars' of the mid-1990's." Both Hacking (1999) and Rorty (1999), two of the most renowned philosophers of science, have expressed doubts over the necessity of the intellectual polarization between essentialists and relativists that has been characterized as the 'science wars,' yet tension remains between those on the extremities of the two intellectual camps.

Both traditional and critical views of the anthropological enterprise allow these issues to be reconciled. If we can set the inconsistencies of the concept of culture aside temporarily, we can see that, traditionally, each of the sub-disciplines of anthropology have simply developed different strategies to answer the same basic questions about humanity. Historically, the earliest anthropological issues concerned the nature of society and how humans came to associate with each other, and how and why societies changed through time (Barnard, 2000:11). From Buffon (1749), whose *Histoire Naturelle Générale et Particulière des Animaux* is generally regarded as the beginning of true anthropological thought, detailing the division of the study of man into four distinct but complimentary subdisciplines (Comas, 1960; Marks, 1995), to Boas, the originator of American anthropology, who argued that anthropology was unified by an underlying historical perspective, the combination of linguistics, ethnology, archaeology, and physical anthropology have all aimed at providing evidence of the recent historical differentiation of human groups. This holistic notion has been more recently referred to as anthropology's 'sacred bundle' (Stocking, 1988).

Within the anthropology-as-science stance, there are many anthropological generalists who argue that if anthropology is to be considered scientific, it must embrace a study of 'all that is man' – whether the particular interest is with the human organism, human behaviour, individuals, social groups, or the distribution of humans as they exist in space and through time (Marshall, 1967:61), closely following Boasian principles. Stocking (1988) argues that such generalism is artificial (see also Borofsky, 2002), and that centrifugal forces have been resisted due in part to the potency of a normative image of a 'holistic' anthropology that has been infrequently and imperfectly realized in actual disciplinary practice, and in part to the pragmatic need to represent a unified 'anthropology' to the world outside the discipline. The main reason for this, Stocking suggests, is that anthropologists' claim to an integrated embracive discipline implies the status of a 'science,' and appears more effective than a congeries of independent sub-disciplines in representing the needs of professional anthropologists. Even though the main anthropological organization in North America, the American Anthropological Association, extends membership to all anthropologists regardless of subdiscipline, institutional power struggles continue to threaten the generalist.

In order for the fragmentary nature of contemporary anthropology to be resolved, it may be more useful to view its subdisciplines as methodological means to theoretical ends, rather than viewing them as distinct areas of inquiry. Unfortunately, even though the postmodernist movement has exploited the congruence between various disciplines and subdisciplines, the barriers are still well guarded in many areas. According to Marshall (167:62), "Not only do modern scholars raise strong barriers between the arts and sciences, they tend to deny the anthropological generalist any significant role. Scientists no longer agree with Francis Bacon, who claimed, '[A]s vast contemplative ends as I have moderate civil ends. For I have taken all knowledge to be my province.'" This is not to say that any one person can become an expert in numerous disciplines, but they should be expert in their own general discipline if they are to become professionals in that area. Anthropological knowledge will only be at a loss if the growing interests in the relation of culture to human population genetics, growth and development, variability and adaptability, the evolution of language, human relationships to ecosystems, etc. are overlooked (Thompson, 1967:67). Overlooking the actual relationships between these variables is what has allowed the use of race as a causal explanation for cultural variation to perpetuate.

Although Boas (1899[1940]:165) strongly believed in integrating anthropological approaches, he felt that the split between cultural and biological approaches to anthropology was inevitable. The living representatives of the various races of man were originally described according to their general appearance – the color of the skin, the form and color of the hair, the form of the face, etc. Later this general description was supplemented by the study of the skeletons of various races, and a number of apparently characteristic differences were noted. One of the principal reasons that led to a more detailed study of the skeleton and to a tendency to lay the greatest stress upon characteristics of the skeleton was the ease with which material of this kind could be obtained. The desire to find good specific characters in the skeleton has also been stimulated by the necessity of studying extinct races. This approach was solidified and thus the subdisciplines emerged and split when human skeletal studies revealed that it was not quite easy to determine racial characteristics with sufficient accuracy by mere verbal description. This led to the introduction of measurements as a substitute for, and perhaps a more valuable method than, verbal description (Boas, 1940[1899]:166). However, even as cultural and biological approaches grew further apart, the questions regarding the relationship of biology to culture remained the same.

If the contemporary anthropological manifestations of these questions about cultural and biological relationships stem from the same root, why can we not maintain a common discourse? Socio-cultural anthropology, which has been the main focus of this discussion, has become preoccupied with the study of science-as-culture, without first critically examining the culture of anthropology itself. What questions can be adequately answered about the 'culture of science' from a discipline struggling to gain the authority of hard science itself? Traweek (1988:17) writes of the culture of high-energy physicists:

> They have a passionate dedication to this vision of unchanging order: they are convinced that the deepest truths must be static, independent of human frailty and hubris. Simultaneously, they believe that this grand structure of physical truth can be progressively uncovered, and that this is the highest and most urgent human pursuit.

If we were to substitute 'cultural or social truth' for 'physical truth,' we could be just as accurately writing about socio-cultural anthropologists. Ethnographic accounts of scientific communities view these cultures as “local strategies of making sense” (Franklin, 1995:174), yet their supposed search for truth is essentially how anthropologists approach their own work. The recent emphasis on theory rather than on methodology within socio-cultural anthropology may also be a result of this focus on the subjective nature of local strategies (Barnard, 2000:7).

Both Clifford (1988) and Geertz (1988) have written about anthropologists as writers first and foremost, meaning writers of fiction, who create for themselves a ‘writerly personality’ from which they relate their authoritative accounts, and this authority is derived from the text itself and its presentation. The consequence of this relationship between the writer and the text is that reliability of the knowledge produced is far less interesting or effective than the inventiveness and persuasiveness of the text (Carrithers, 1990:263). Geertz (1988:10) also remarks on “the oddity of constructing texts ostensibly scientific out of experiences broadly biographical.”

Anthropologists may lay claim to a scientific approach to culture, but simply developing a research strategy or writing style according to a scientific attitude of objectivity rather than based on an experimental method cannot give reliable, testable, predictive, or falsifiable results. Contrary to the arguments of Guille-Escuret, the comparative method has little in common with the experimental method. It may be refuting, and also refutable itself, but it does not fit into Popper’s model. What Franklin (1995:179) argues, however, is that anthropology is “uniquely positioned to attest to the value of a multiperspectival science, which situates itself as partial in the representation of its objects.” Similar to the explanation of the anthropological crisis that Geertz (1995) provides, Franklin (1995:179) suggests, “Anthropology is arguably a better, more inclusive, less naively Eurocentric and even more objective form of scholarly inquiry because of the sustained critique of its own practices that has kept it 'in crisis' since at least mid-century."

Again, these assumptions about science and objectivity are problematic. Ethnographic methodologies such as participant observation can only result in partial perspective and cannot result in an objective science – and the partial perspective entails a rejection of the notion of scientific objectivity/subjectivity dichotomy in theoretical terms.[5] Perhaps the conundrum here results from the relationship between the anthropological 'other' and the 'self;' on the question of science, anthropologists are blurring the boundaries between the two categories – categories that they themselves have created – by inserting themselves into both emic (insider) and etic (outsider) positions, attempting to overcome the subjectivity/objectivity divide. Further, the claim that anthropology can truly arrive at objectivity because of its self-reflexivity seems naïve, since the questions causing the current crisis have consistently come from within. Anthropologists have, on the other hand, dismissed the questioning of their authority that has come from other disciplines such as history, philosophy, and sociology. Martin (1998:24) explains the reaction of science studies scholars to the anthropology of science: "It has been said that studies of science by anthropologists lack 'theoretical purchase' (Woolgar, 1989) and that it is nearly impossible for anthropologists to carry out useful studies of science (Latour, 1990)." While Harris (1999:13) argues that postmodernism has brought an "anything goes" approach to anthropology, the discipline has historically been considered unstructured by outsiders based on the constant internal crises.

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[5] For a discussion of partial perspective, see Haraway (1999).

Is anthropology to gain an authoritative position in the explanation of human interactions and relationships of social and ethnic groups, nature and environment, or science and technology by attempting to be objective and scientific, or by simply endeavouring to move beyond these arguments and focus more directly on producing exemplary research based on experience and original ideas? In many areas of both the 'hard' and 'interpretive' sciences, the objective/subjective debate is fading from fashion, and researchers taking a more moderate position are moving on:

> Since the decline, in most quarters, of belief in a single and sovereign scientific method and the associated notion that truth is to be had by radically objectivizing the procedures of inquiry, it has become harder and harder to separate what comes into science from the side of the investigator from what comes into it from the side of the investigated (Geertz, 1995:20).

While social scientists continually attempt to expose the social contingency of scientific knowledge, the struggle of anthropologists to gain scientific authority continues, even though many, particularly the Boasians, would agree that:

> Recognizing the empirical incompatibility of the event account with human affairs but still believing that it fits the physical world, most researchers have just concluded that humans have a special quality that renders the science of human affairs inherently peculiar in some way. As a result, the centuries-old struggle to develop an autonomous science of society or culture has pressed on, yielding a mass of conundrums and unresolved controversies (O'Meara, 1997:400).

According to Gieryn (1983:13), because the boundaries of science are ambiguous, flexible, historically contingent, contextually variable, and internally inconsistent, the assumption that there is a clear demarcation between the scientific knowledge and other forms of (social) knowledge is a poor heuristic for the sociology (or anthropology) of science. The 'experimental sciences' and the 'interpretive sciences' serve to provide different types of knowledge, but at some point, they begin to overlap. Traditionally, this overlap has been the domain of anthropology. In Gould's (1980) view, disciplines dealing with evidence both idiographic (unique, unrepeatable events) and nomothetic (lawlike, repeatable events) provide particular problems, but that they reside somewhere near the middle of a continuum stretching from historical to the non-historical science.

On this notion of a scientific continuum, Midgley (2001:138) believes, "Social facts, then, are entirely continuous with biological facts." There should be substantial overlap between areas of anthropological expertise. This is because "The traditional opposition that we tend to make between biology and culture is as misguided as the traditional opposition between body and mind ... Culture is the form that biology takes" (Midgley, 2001:138). Or, as Latour (1987) explains, the establishment of a fact has little to do with the actual nature of the discovery (whether it comes from the social or natural realm), it is neither fact nor fiction, but when it becomes accepted as fact by a network of authoritative authors, that discovery will have become tacit knowledge.

The anthropology-as-science argument has been set forth almost exclusively in the context of socio-cultural anthropology. It has already been suggested that biological anthropologists and archaeologists are considered much more scientific in focus, so it would not be expected that such issues would be of concern to them. However, both of these areas of research rely implicitly on ethnographic and ethnohistorical data for their analyses (Calcagno, 2003:6), yet socio-cultural anthropologists generally do not rely on biological data or material culture for their theories, most likely because they simply do not possess the specific knowledge required to decode the language of these subdisciplines. The solution here is exactly as Snow (1959) suggests: education. Innovative approaches to both culture and biology should be mutually inclusive, and an engagement with historical, philosophical, and sociological analyses of science can only serve to add extra layers of interest to such studies. These studies should involve, of course, a solid grounding in the history of anthropological method and theory in a general sense. This should not look like the superficial or artificial generalism that is typical of the modern four-field approach to anthropology. Rather, it should focus on the complexity of the connection between culture and biology: culture is not shaped by biology (i.e., genetics) so much as human biology is shaped by cultural processes, which are, in essence, environmental.

Beyond the Science Question: Reintegrating Anthropology

Montagu (1940:46) describes his view of anthropology and its subdisciplines (Figure 1.1), arguing that archaeology represents a bridge between social and physical anthropology, but he suggests that what is necessary for the discipline is the establishment of an area of study similar to that of primatology: "Such a field of study is best defined as the study of man in relation to his environment or environments, that is, in relation to the totality of factors operating upon him. This field of study should constitute the science of Human Ecology – the study of man as a whole in relation to his total environment." It seems that the future of anthropology rests on the integration of the various subdisciplines into something more cohesive and coherent.

The current lack of coherent and cohesive anthropological subject matter leads to significant problems of scientific approach, both from culturally-oriented researchers making uninformed evolutionary inferences and from biologically-oriented researchers extending their knowledge too far into the social realm. An unfortunate effect of the

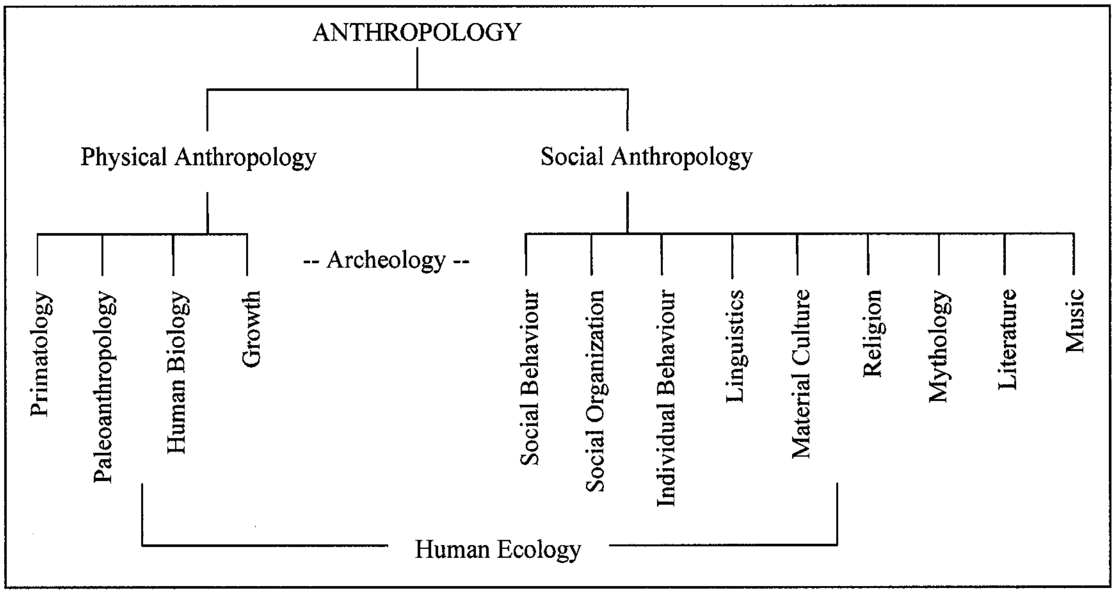

Figure 1.1 The divisions of anthropological study. Adapted from Montagu (1940:41).

Darwinian approach to anthropology, despite the fact that it was embraced by such important figures in the history of anthropology as Boas and Malinowski, was that others have maintained a belief that culture is determined by biology and thus the search for universal (structural) laws of culture continues. If one assumes that culture has a strong deterministic genetic component that can be subjected to systematic empirical study, as in modern sociobiology (see particularly Lumsden and Wilson, 1981), this would surely be accompanied by a claim of scientific objectivity, and this artifact of 'false evolutionism' (Levi-Strauss, 1958:14–15) may be one of the underlying problems in contemporary anthropology that has allowed the fallacious race concept to persist.

Though this approach may not be explicit, the discipline of anthropology arose out of the structuralist paradigm, and a structural argument incorporates elements of essentialism and predictability that must be tied to underlying genetic mechanisms, although this point is often overlooked. This basic structuralism has led to what Stocking

(1988:3) characterizes as a dialectical tension between human unity and diversity in anthropology. Shore (1996:39–40) argues that the relationship between culture and mind is more complex than this, and that the irresolvable dichotomy or dialectical tension that Stocking desribes is in fact false: "If mind exists at the intersection of brain and extrinsic models, we need to model brain-culture interactions so that they reveal at one and the same time the general cognitive processes of information processing and meaning construction as well as the culturally diverse manifestations of those processes in action."

The result of misguided evolutionary approaches (such as psychic unity, socio-biology, or 'evolutionary psychology')[6] is the confusion between homology and analogy. According to Lewontin (1991:61–78), this simply results from a misunderstanding between the observation of a specific type of behaviour and its possible causes, since we know little or nothing about the heritability of human temperamental and intellectual traits which are considered to be the basis for social organization. This is perhaps the main flaw of traditional structuralist arguments, at least in the anthropological context. If culture is predictable, and self-organizing, then it must have a strong genetic component, and in this case it *could* be studied scientifically. Sociobiology and evolutionary psychology have done little to prove that a connection between genetics and cultural development or social structure exists. Rather, genes are inherited by individuals, not by groups, and human cultures are the products of vast arrays of genetic mixing (Olson, 2002). It would therefore be a great intellectual leap to suggest that culture can or should be studied as a predictable phenomenon, since similarities or analogies in no way guarantee that there is a causal evolutionary relationship. This is one of the fundamental

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[6] For example, evolutionary psychologists such as J.P. Rushton argue that there is a high degree of correlation between intelligence and brain size, which has a strong racial-genetic component. For a critique of Rushton's psychological studies of race, see Brace (2005:255–263); Lieberman (2001).

problems of modern anthropology – because of fragmentation, few anthropologists have adequate scientific training to make valuable structural inferences.

The focus on both anthropology-as-science and the culture-of-science has only served to contribute to the internal crises of anthropology, and done nothing to look at the problems that underlie the discipline. As such, the race concept remains an artifact of the underlying genetic arguments for cultural processes, which may not be explicitly elucidated in anthropological writings, but are implicitly tied to the essentialist epistemologies. This is the reason that race and culture remain confused, and race has been able to perpetuate in the cultural realm. Any singular attempt at clarifying the race concept will suffer dramatically from the fragmentation of anthropology: "How can any discussion of the problematic concept of race, as perhaps the most obvious example, be satisfactory without some consideration of human biological variation? Doing so would be as ludicrous as addressing the concept without any reference to cultural views on race" (Calcagno, 2003:8).

Cultures can only be understood partially and locally, yet they are exceedingly complex in social structure, genetics, and environmental variables. "The yearning for 'the system' is a powerful one; the faith that our world must be rational, well ordered through and through, plays a role where only evidence should matter" (Cartwright, 1999:17). Indeed only evidence should matter (as was Boas' perspective), but the preoccupation of numerous socio-cultural anthropologists with being the 'science of culture' only serves to weaken the authority of anthropological knowledge. In order for anthropology to be considered a 'hard' science, developing standardized operational definitions of 'race,' 'culture,' and 'anthropology' would be a necessary first step, but it seems doubtful that this can or will happen. Carrithers (1990:2) has succinctly summarized the current state of anthropology: “Whatever anthropology is, it is not a science, and the knowledge anthropologists create is in no single sense scientific. Anthropological knowledge is interpretative and hermeneutic rather than positive, tentative rather than conclusive, relative to time, place, and author rather than universal.” But it is that yearning for *the system* that leads researchers to use contested concepts as causal explanations. In order to move beyond the fallacies of human racial classification, anthropologists must bring together social-cultural and biological knowledge in order to present real solutions to the problems that have traditionally kept anthropology in crisis.

The following chapter explores this further, focusing specifically on the topic of race, discussing how biological groups, which are historically contingent, are conceptualized and classified not only by anthropologists, but also by evolutionary biologists and geneticists. It is important not only to understand how the study of humans has been approached scientifically and systematically, but how anthropologists both borrow and contribute to biological evolutionary studies. Biological classification and systematic taxonomy provides yet another area for the exploration of human variation and clash between traditional and contemporary paradigms.

Chapter 2

THE ROLE OF RACE IN THE EVOLUTIONARY SCIENCES

This chapter moves beyond the analysis of race as an underlying concept in anthropology to its explicit use as an organizing principle in the evolutionary sciences. The concept of race has retained its vitality in a number of scientific disciplines despite the inability to provide an accurate and encompassing definition. The criteria used in racial classification depend upon the purpose of the classification itself (Molnar, 2002:18), which may not only differ between disciplines, but also within them. Dunn (1958:13), writing in the UNESCO publication *Race and Biology*, explains that "although there has been for some time a considerable measure of agreement amongst biologists about the concept of race in plants, animals and man, the word *race* as used in common speech has no clear or exact meaning at all, and through frequent misuse has acquired unpleasant and distressing connotations." While race is generally characterized as a contested term in anthropological discourse, the assumption that it has been unproblematically applied to interspecies variation by biologists is inaccurate (see particularly Livingstone, 1962:279; Templeton, 2002a). The key argument of this chapter is that the race concept itself is not simply a problem in the human realm or only in popular usage, but a problem that also exists in evolutionary thought in general. This chapter focuses on operational definitions of race in a contemporary context across the disciplines in which it is most often referred: evolutionary biology, genetics, and anthropology.

Evolutionary Science: The Problem of Genotype and Phenotype

In a survey of the geographic distribution of genes throughout historically situated populations, Cavalli-Sforza et al. (1994) produced nearly five hundred maps of numerous allele frequencies from genetic samples of individuals from nearly two thousand communities, and the surprising result of these genetic analyses was that the map of world genetic variation shows Africa on one end of the spectrum and Australian aborigines at the other. What this tells us is that patterns of adaptation follow models of clinal variation,[1] with Australian aborigines showing the greatest genetic distance from Africans, but the most similarity in terms of phenotypic constitution, according to the so-called Bergmann-Allen rules. Bergmann's rule explains that in warm-blooded species, as groups move geographically towards more polar (northern) regions, overall body mass is expected to increase. Similarly, Allen's rule explains that as groups move towards warmer (equatorial) geographic areas, the length of the extremities increases. In recent human groups, this clinal variation shows a very strong negative correlation (-.60) between body mass and mean annual temperature (Marks, 1995; Molnar, 2002:199–201). Cavalli-Sforza et al. (1994) suggest that this morphological similarity between indigenous African and Australian populations is the simple product of generally similar climates in sub-Saharan Africa and the traditional territory of the Australian Aborigines.

The problem that the results of these genetic frequencies create for physical anthropologists is that they do not match the population distances derived from skeletal data. Craniometric data collected by the physical anthropologist William Howells have been used to test the results of the Cavalli-Sforza et al. genetic study (Cavalli-Sforza et

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[1] Clinal variation is the graded intensity of adaptive traits according to geographic distance. Thus, genetic distance and geographic distance are highly correlated (Templeton, 1999:639).

al., 1994:72; Cavalli-Sforza and Cavalli-Sforza, 1995:116–118), consistently grouping Australians and sub-Saharan Africans as closely cognate populations based on cranial measurements. These results are not surprising since the human body adapts to climate more readily than genetic changes allow, and anthropometrics provide some indication of these adaptations. Again, this problem of conflicting results leads us back to the (morphological) anthropological view of human variation versus the genetic view. However, the results have provided four basic rules of human variation that will certainly be important considerations for physical anthropologists in discussing human evolution (Cavalli-Sforza et al., 1994):

1. Individual variation is much larger than group variation.
2. In combination with the fossil record, it can be confirmed that Africa was the birthplace of humanity (i.e., *Homo sapiens*). Gene frequencies indicate a large genetic difference between present-day Africans and non-Africans.
3. All Europeans are thought to be hybrid populations, with approximately 65% Asian and 35% African genes (attesting to the greater antiquity of African and Asian populations).
4. Indigenous North American populations were found to be divisible into three distinct groups by blood type, representing three separate migrations from Asia.

Therefore, there is a vast amount of diversity in human populations, and as such, evolutionary relationships cannot possibly be accounted for by osteological analysis alone.

As we try to reconcile the differences in relation to the race concept that we encounter between physical anthropology and genetics, we should not leave out the perspectives of evolutionary biology. Evolutionary biologists typically refer to *subspecies* when discussing intraspecies variations, with the terms ‘subspecies’ and ‘geographic race’ being used somewhat interchangeably in the taxonomic literature (Mayr, 2002). It is easy to provide arguments against human racial classification because of the sociological implications, but we must not forget that humans are biological organisms like any other, and that our cultural status does not exempt us from the selective forces of evolution. It would therefore be illogical to simply conclude that human races are mythical or fallacious creations, but other species have numerous racial variants.

Before attempting to define how race is conceptualized in each of these fields (evolutionary biology, genetics, anthropology), it should first be understood how these disciplines are defined in the current context, since there is significant overlap in many aspects of these three general areas. Evolutionary biology is distinguished from genetics by the unit of analysis – the biology of whole organisms (i.e., phenotypes) as opposed to the biochemical processes that occur at the cellular level (i.e., genotypes, heredity) (Mayr, 1997:111). Evolutionary biology therefore includes zoology and paleontology within its scope, with both areas focusing on systematics and evolutionary relationships, the morphological diversity of living and extinct (fossil) organisms, adaptations, and the mechanisms, including natural selection, involved in evolutionary change. Genetics, on the other hand, is part of the more general field of biology, but is distinguishable as a scientific discipline by a relatively well-circumscribed focus on a core group of questions. According to Burian (2005:146), these core questions include: What is genetic material? How is genetic material organized? How does gene structure relate to gene expression and gene function? By what mechanisms are genes or genetic information transmitted from one generation to the next? How does genetic material affect particular traits of organisms?

Distinguishing between evolutionary biology and genetics here is not to deny the relationship between genotype and phenotype, but rather to explore approaches to taxonomic problems that exist at each level of variation. Perhaps the key common feature between evolutionary biology and genetics is interest in the mechanisms by which evolutionary change occurs, and how these mechanisms affect gene expression. Evolutionary biology and genetics both fall under the general rubric of biological science, but for the purposes of discussion here, it is essential to distinguish between macro and micro level approaches.

In contrast to both evolutionary biology and genetics, physical anthropology deals exclusively with the diversity and classification of humans – both extant and extinct (fossil) hominids – into discrete groups, whether social, cultural or biological, while also focusing on evolutionary relationships. Traditionally, physical anthropology has been concerned with building classifications based upon observable features of the body, but its primary focus is on human skeletal anatomy. Though physical anthropologists may incorporate genetic evidence and general evolutionary theories, anthropology is much more easily distinguished from the other two disciplines discussed here by the overall focus on human variation. All three of these areas are unified in a historical reliance on the Linnaean principles of classification.

Evolutionary Biology

Modern understandings of biological variation from within the biological sciences stem from the substantive redefining of the field that occurred in the 1940s with the 'evolutionary synthesis,' flourishing out of the combination of natural selection and genetics in the study of species and speciation, led by Dobzhansky (1937), Huxley (1942), Mayr (1942), Simpson (1944; 1961), and Rensch (1947) (see Mayr, 1997; 2004). This reworking of evolutionary understandings brought together Darwinism and Mendelian genetics, thus providing a mechanism for understanding the processes of natural selection, which had previously been limited by a cursory understanding of trait inheritance. With this new approach to evolution, environment, and systematics, species were viewed as dynamic units in a particular environmental context. There is some theoretical debate over the actual nature of this shift, suggesting that pre-Darwinian approaches may not have been particularly anti-evolutionary, with their underlying premises clouded by the obvious clash between creationist and evolutionist perspectives (Farber, 1976; Amundson, 1998; Winsor, 2003). The typical approach to the static/dynamic conundrum is likely due to the orthodoxy of Ernst Mayr's own account of the synthesis (Smocovitis, 1996:186; Greene, 1999); however, it is perhaps best to begin with a rather conservative approach to how evolutionary biologists deal with variation at and below the species level.

Mayr (1982:273) defines *species* as a "reproductive community of populations (reproductively isolated from others) that occupies a specific niche in nature." However, wide-ranging diversity within species and distinguishable local populations (sometimes also referred to as *demes*) can be observed depending upon the rate of interbreeding or degree of isolation (Molnar, 2002:5). A greater interest in intraspecific categories developed, which led to numerous works on the concepts of subspecies and geographical races (Ehrlich and Holm, 1964), which Mayr (1942:106) defines as follows: "the subspecies, or geographic race, is a geographically localized subdivision of the species, which differs genetically and taxonomically from other subdivisions of the species."

Two problems are immediately noticeable here. First, as a zoologist, Mayr is an expert in morphological variation, but he refers specifically to genetic differentiation. In the absence of genetic information, one cannot be certain that morphological variation is the result of differing genotypes. Second, Mayr's reference to taxonomic differentiation is not without problem: a population can exist as a genetically or geographically localized subdivision of a species, but its taxonomic recognition is contingent upon demonstrating the discrete nature of the population. Therefore, taxonomic recognition does not go hand-in-hand with genetic or geographic differentiation, but must follow the recognition of an appropriate degree of differentiation.

The problem of phenotypic versus genotypic evidence led to the development of two major schools of thought in the biological sciences at the time when the synthesis was bringing together Linnaean taxonomy and Darwinian evolution: *typologists* and *populationists*. Mayr (1984a:16) explains that both saw races in certain species but conceptualized them in different ways:

> The typologist stresses that every representative of a race has the typical characteristics of that race and differs from all representatives of all other races by the characteristics "typical" for the given race. All racial theories are built on this foundation. Essentially, it asserts that every representative of a race conforms to the type and is separated from the representatives of any other race by a distinct gap. The populationist also recognizes races but in totally different terms. Race for him is based on the simple fact that no two individuals are the same in sexually reproducing organisms and that consequently no two aggregates of individuals can be the same. If the average difference between two groups of individuals is sufficiently great to be recognizable on sight, we refer to such groups of individuals as different races. *Race, thus described, is a universal phenomenon of nature occurring not only in man but in two thirds of all species of animals and plants* [emphasis added].

Though different perspectives existed within the biological sciences, the race concept was generally accepted as a means for explaining the diversity within species and building sufficiently representative taxonomic models.

The interest in classifying biodiversity not only at the species level but at the subspecific level following the evolutionary synthesis led to the revision of Linnaean taxonomy as a binomial classification (genus, species) to that of a trinomial system (genus, species, subspecies). This is not to say that naturalists had not previously been interested in both inter and intra-species variation. Linnaeus himself, as well as Buffon and Blumenbach were greatly interested in the 'varieties' within species (Billinger, 2000:45-62), but the systematic recognition of variation at this level (i.e., the labelling of subspecies) would have to await the evolutionary synthesis. This revision allowed biologists to organize the regional variants of polytypic species, though the overall hierarchical structure of systematics remained intact. The introduction of the subspecies designation would prove to be a problematic endeavour for many biologists, however, since it is a microevolutionary perspective that relies on typological thinking. As a result, species are seen as polytypic aggregates of monotypic subspecies (Keita, 1993:420), rather than singular evolutionary units, or simply as general groupings of unique individuals. The subspecies designation became increasingly problematized in the 1950s, a stance which solidified in the controversial recommendation by Wilson and Brown (1953) that the category no longer be used due to its highly arbitrary nature.

In favour of the abandonment of the trinomen, Wilson and Brown (1953:100) argue, "From our experience in the literature we are convinced that the subspecies concept is the most critical and disorderly area of modern systematic theory – more so than taxonomists have realized or theorists have admitted." In this argument, there are five major indictments against subspecies (Wilson and Brown, 1953; Brown and Wilson, 1954; Keita and Boyce, 2001:570):

1. The various degrees of overlap (or lack of it) required to establish different groups.
2. The meaningful or required levels of divergence that would legitimize distinguishing subspecies.
3. The related issue of whether subspecific systematics should reflect microevolutionary relationships.
4. The corollary requirement that the taxonomic schema have predictive value.
5. The problem of ideal typological thinking.

While Mayr is generally considered the leading expert on systematics and trinomial taxonomy, he also wrote of the arbitrary nature of classification:

> We have stated repeatedly that every one of the lower systematic categories grades without a break into the next one; the local population into the subspecies, the subspecies into the monotypic species, the polytypic species into the superspecies, the superspecies into the species group. It simply means that in the absence of definite criteria it is, in many cases, equally justifiable to consider certain isolated forms as subspecies or species, to consider a variable species as monotypic or to subdivide it into two or more geographical races, to consider well-characterized forms as subspecies of a polytypic species or to call them representative species (Mayr, 1942:172).

Nonetheless, Mayr is a strong proponent of the retention of the subspecies designation, though recognizing its inherent subjectivity, arguing that by calling the subspecies into question, all levels of taxonomy would eventually fall to the same fate. But the problem of subspecies in the 'new systematics' was simply a surface issue; at the root of the tensions between taxonomy and evolutionary theory would emerge the concept of species itself, which was not necessarily a self-contained evolutionary unit as had been presumed (Ehrlich and Holm, 1964).

Mayr (1984b) sees three distinct species concepts that have developed historically in the biological sciences, which he labels the *typological species concept*, the *nominalistic species concept*, and the *biological species concept*.[2] The first two concepts are mainly applicable to inanimate objects, and Mayr argues these are the main sources of confusion historically in the biological sciences. The typological species concept, rooted in Platonic and Aristotelian essentialism, became manifest in the taxonomic designation given by Linnaeus, as mentioned above. This concept was morphological in nature, and the observed diversity of the universe was thought to reflect the existence of a limited number of underlying universals or types. In contrast, the nominalistic species concept views species as man-made abstractions, since only individuals exist in nature. Mayr claims that his own biological species concept (BSC) represents an objective understanding of nature and its processes, based on the evolutionary function of adaptation.

The BSC views animal species not as human constructs, nor are they seen as typological in a Platonic or Aristotelian sense; rather, they are something for which there is no equivalent in the realm of inanimate objects. This notion of species is based on a shared (evolved) genetic program, in which the members of a species constitute: 1) a reproductive community, 2) an ecological unit, and 3) a genetic unit. A key principle of the BSC is reproductive isolation. According to Mayr (1984b:539), "The reproductive isolation of a species is a protective device that guards against the breaking up of its well-integrated, coadapted gene system. Organizing organic diversity into species creates a

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[2] This is a rather conservative assessment of the competing species concepts. Coyne and Orr (2004) detail no fewer than nine contemporary species concepts.

system that permits genetic diversification and the accumulation of favorable genes and gene combinations without the danger of destruction of the basic gene complex."

However well-defined the biological species concept had become in the wake of the new systematics, many still were unable to get past the fact that Darwin himself was so impressed by the variability and intergradations in the material he studied. Darwin considered the term 'species' to be arbitrary – not differing in essential features from 'variety' (Sokal and Crovello, 1984:541), the term that Linnaeus had used to refer to variants within his static species. Thus, the species concept conceptually falls back into the ambiguity of the subspecies designation, leading some to question the value of the biological species concept: "On asking some essential questions about the value of the BSC to taxonomy and evolution, we find that the BSC is not necessary for practical taxonomy, is neither necessary nor especially useful for evolutionary taxonomy, nor is it a unique or heuristic concept necessary for generating hypotheses in evolutionary theory" (Sokal and Crovello, 1984:564). New strategies became available for clarifying the species problem during the late 1950s and early 1960s using computers for high-speed data processing. This allowed for the development of taxonomic systems based on large numbers of morphological characters in order to build phenetic relationships based on degrees of similarity (Ehrlich and Holm, 1964:497). At the forefront of this digital movement were Sokal and Sneath (1963a; 1963b), who developed this 'numerical taxonomy.'

Numerical taxonomy entails the observation and quantification of large numbers of physical characteristics, usually seventy-five or more, which vary in the organisms studied. The computational analysis of this data involves a coefficient of similarity among the units under study based upon the observed characteristics. The derived coefficients are then used as the basis for a taxonomic system by the clustering of statistically similar entities. The resulting classifications show relationships based on physical resemblances rather than genetic or phylogenetic hypotheses; the results are strictly contextual, based only on the observed characteristics of the particular organisms under study (Ehrlich and Holm, 1964:497–499). Computational analysis of numerical taxonomy is neither evolutionary nor strictly typological – it provides a view of evolutionary relationships only in a single temporal period, rather than allowing for an analysis of change through time. Nor is it typological in the sense that it derives objectivity from artificial groupings. Rather, its supposed objectivity comes from groupings based strictly upon numerical values of observed variables, removing the onus from the biologist to draw subjective distinctions between populations. With complex and powerful statistical packages available for computers today, numerical taxonomy has still yet to reach its potential and, consequently, the biological species concept remains fundamental to the study of biology.

Hull (1984) believes that there are three competing systematic philosophies within the biological sciences in the latter 20th century, which need to be understood in order to address issues of species and speciation. Numerical taxonomists use a phenetic method of exploring taxonomic relationships in terms of numeric similarity without any *a priori* weighting. These *pheneticists* are philosophically opposed to the methods of systematics traditionally used by *evolutionists* such as Mayr, Rensch, Dobzhansky, and Simpson, in that their approach to systematics is based only in the results of statistical analyses and not in theoretical assumptions. Entering into the debate is a third group of taxonomists, the *phylogeneticists*, led by Hennig (1950, in German; 1966, in English translation), Kiriakoff (1959), and Brundin (1966), who agree with the evolutionists that evolutionary theory and taxonomy are essentially interrelated endeavours but disagree over the precise nature of that relationship. The primary difference between the phylogenetic school and the evolutionary school of thought is that the phylogeneticists see classification as reflecting only cladistic affinity (i.e., clades or evolutionary lineages), whereas the evolutionists argue that classification should also reflect such factors as degree of divergence and amount of diversification.

A clade is defined as a monophyletic group that contains all (and only) the species descended from a given ancestral species.[3] Where cladists diverge philosophically from evolutionary systematists is in the recognition of two kinds of characters in species (whereas evolutionists do not differentiate): primitive (ancestral) and derived characters. Both types of characters are homologous – present in a common ancestor of a set of species – but ancestral characters evolved before the common ancestor whereas derived characters evolved originate with the common ancestor (Mettler et al., 1988:40; Ridley, 2004). Hull nonetheless groups these two schools, the evolutionary taxonomists and phylogenetic taxonomists, together as *phyleticists* (concerned with the development of species, i.e., processes) in contrast to the computer-aided pheneticists (relationships, i.e., patterns).

Cladistics has become popular recently due to its preference of homologous traits over analogous traits. While Hull groups cladists and evolutionists together, cladistics rivals both evolutionary systematics and numerical taxonomy in its use only of characters

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[3] Phylogeneticists or cladists group species solely according to their most recent common ancestor. During the process of speciation, species usually split and form two dependant 'sister species,' which are grouped together as a clade, forming a branching hierarchy of ancestral relations (Ridley, 2004:479).

with demonstrated evolutionary relationship, whereas the other methods simply treat similar characters as being derived from a common evolutionary origin, which may not always be a reasonable assumption. The most important distinction underlying the phenetic-phyletic debate is the difference between explicit and implicit or intuitive taxonomy:

> Simpson (1961) has argued that taxonomy, like many other sciences, is a combination of science and art. For example, tempering vertical with horizontal classification, dividing a gradually evolving lineage into species, deciding how much interbreeding is permissible before two populations are included in the same species, the assignment of category rank above the species level, choices between alternative ways of classifying the same phylogeny, balancing splitting and lumping tendencies, and the inductive inferences by which phylogenies are inferred are all to some extent part of the art of taxonomy. The question is whether the intuitive element in taxonomy should be decreased and, if so, at what cost (Hull, 1984:597).

What would the cost of a non-intuitive taxonomy be?

Gould (1992) argues that those who suggest that a species must be an arbitrary division of an evolutionary continuum are incorrectly interpreting evolutionary theory and that species are *almost always* objective entities in nature. He explains that the general historical conception is that evolution is gradual and continuous (i.e., phyletic gradualism), and that most of a species duration is spent in a state of incipient formation, so species cannot be objectively definable during most of their geologic lifetimes. However, because speciation follows the principle of punctuated equilibria (Eldredge and Gould, 1972), new species do (and must) have a period of initial ambiguity, but their emergence is relatively quick compared with their period of later stability, and species then live for long periods, often millions of years, with minimal change. As a result, if a species spends approximately one percent of its geologic history in a state of rapid change, then taxonomies should be correct 99% of the time. Therefore, Gould (1992) suggests that since species are defined as "a population of actually or potentially reproducing organisms sharing a common gene pool," he considers them, "nature's objective packages."

In Gould's (1992) opinion, subspecies are also partly objective but also partly based on human decision. A subspecies must be a distinct geographic subpopulation within the entire range of a species, and not yet evolved far enough to become a separate species in its own right, though differing enough from other subpopulations either anatomically, genetically, physiologically, or behaviourally, that a taxonomist chooses to memorialize the distinction with a name. However, subspecies cannot be irrevocably unique natural populations (like full species) for two reasons: first, the decision to name them rests with human taxonomists, and is not solely dictated by nature; second, they are, by definition, still capable of interbreeding with other subpopulations of the species and are, therefore, impermanent and readily subject to hybridization or re-amalgamation.

Gould's definitions of species and subspecies are remarkably similar to those of Mayr. The main taxonomic problems that the BSC causes for evolutionary biologists is that it is inherently rooted in genetic theory, but relies on evidence that is generally physiological or osteological. Therefore, it cannot be intuitive, because a direct relationship between phenotype and genotype cannot be drawn. On the other hand, phylogenetic and phenetic approaches are designed to approximate evolutionary relationships based on biological evidence for similarities between populations at all levels of taxonomy, whether the evolutionary relationships are assumed (phenetic) or demonstrated (phylogenetic). Thus, genetic mechanisms underlie the processes of physical differentiation, but the classifications of pheneticists and phylogeneticists (as opposed to evolutionists) make no specific appeal to genetic explanations.

Genetics

Genetics and evolutionary biology are very closely related disciplinarily; both follow the principles of Darwinian evolution, though advancements in the study of genetics and heredity led to the evolutionary synthesis in the 1940s. In a sense, the two disciplines developed separately, converging at the point of the evolutionary synthesis, and subsequently diverging but following a path of parallel evolution. The sense here in which I invoke genetics is in terms of (quantitative) evolutionary or population genetics,[4] rather than medical or developmental genetics, which also have roles in shaping our understandings of variation, as will be discussed later in this chapter. Since the phenotypic traits studied by evolutionary biologists limit study to only the *patterns* of evolutionary change (and osteological or fossil evidence often only allow for the study of discontinuous temporal sequences), evolutionary studies in general have emphasized the work of molecular or evolutionary geneticists in studying the *processes* involved in inter- and intra-species variation and phylogenetic branching.

Geneticists typically refer to races and subspecies in the context of evolutionary relationships as a stage in allopatric speciation where species form in isolated geographic areas with little or no gene flow. Dobzhansky (1937), who was trained as an evolutionary

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[4] A contrast can be drawn between classical population genetics, focused on Mendelian principles, and modern molecular genetics, which focuses specifically on gene expression. Here, I group classical population genetics with molecular genetics because the classical approach served as a precursor to modern molecular methods, and adheres to the same basic set of questions and interests: the material of which genes are made, their physical structure and organization, how they are transmitted, how they function, and how their functions are controlled (Burian, 2005:147).

biologist but later made his greatest accomplishments in population genetics, was a main architect of the evolutionary synthesis, articulating many of the genetic requirements of allopatric speciation (along with R.A. Fisher, J.B.S. Haldane, and Sewall Wright; see Burian, 2005:106-107). Dobzhansky's genetics were informed by research on a diverse set of organisms: fruit flies (*Drosophila)*, Asiatic beetles (*Harmonia axyridis*), and desert plants (*Linanthus parryae*), to name just a few examples (see Dobzhansky, 1951:135-178). It was only from the variation observed in these species that Dobzhansky extended his genetics to humans since the majority of (experimental) genetic research had been conducted on species other than humans in the pre-DNA era (see Dobzhansky, 1963). According to Dobzhansky (1951:177),

> To a geneticist it seems clear enough that all the lucubrations on the 'race problem' fail to take into account that a race is not a static entity but a process. Race formation begins when the frequency of a certain gene or genes becomes slightly different in one part of a population from what it is in other parts. If the differentiation is allowed to proceed unimpeded, most or all of the individuals of one race may come to possess certain genes which those of the other race do not. Finally, mechanisms preventing the interbreeding of races may develop, splitting what used to be a single collective genotype into two or more separate ones. When such mechanisms have developed and the prevention of interbreeding is more or less complete, we are dealing with separate species. A race becomes more and more of a 'concrete entity' as this process goes on; what is essential about races is not their state of being but that of becoming.

Dobzhansky's primary concern regarding race was the articulation of biological principles that were being unduly rejected by those who argued against the existence of human races. He felt that race was both a biological phenomenon and unit of classification. As such, Mendelian populations (such as those within the genera *Drosophila* or *Homo*) which differ in the frequencies of some genetic variables are racially distinct, but racially distinct population do not necessarily need to be assigned racial or subspecific names:

> Discovery of races is a biological problem, but naming them is a nomenclatorial problem. There is nothing arbitrary about the criteria for determining whether racial differences between two population do or do not exist, but it is a matter of convenience and judgement to decide when and which racially distinct populations should or should not be given race names (Dobzhansky, 1963:1132).

Modern molecular genetics recognizes that the amount of genetic variation at the level of morphology, karyotype, proteins and DNA within species is substantially greater than between species (Lewontin, 1972; Mettler et al., 1988:269; Keita and Kittles, 1997:537; Templeton, 1999; Griffiths et al., 2000:782). Therefore, in textbooks dealing with population genetics and evolution, it is quite common for authors to give an explanation of this sort: "a geographical race is a *phenotypically* distinguishable local population within a species that is capable of exchanging genes with other races within that species. Because nearly all geographical populations are different from others in the frequencies of some genes, race is a concept that makes no clear biological distinction" (Griffiths et al., 2000:783). Similarly, authors may also suggest something like: "Although the differences between races are objectively ascertainable facts, the number of races we choose to recognize is a matter of convenience, a cataloguing device used to organize and record observed intraspecific diversity, which allows intelligible communication among students of taxonomy and evolution" (Mettler et al., 1988:48).

Such explanations are relatively ambivalent in their criticism of the race concept in genetic terms. There have, however, been a number of geneticists who, like Dobzhansky (along with their anthropological colleagues such as Ashley Montagu), have forcefully argued against simple notions of racial variation since the early 1960s, mostly in reaction to the strong human eugenics movements developed in the United States and spreading throughout North America and Central Europe during the earlier half of the century. Where evolutionary biologists consider the environment to be the primary factor in the forces of natural selection, geneticists may be prone to focusing specifically on heredity, neglecting the role of environment on the expression of genes. Hiernaux (1964:487) wrote of the indeterminate nature of gene/environment interaction at the level of the phenotype:

> Suppose two groups of people have identical gene pools, but differ phenotypically because of the imprint of different environments. Would it be useful to call them races A and B, knowing that by reversing the environmental conditions race A would become race B in one generation and vice versa? A negative answer seems evident to me as to many others: in order to be useful, a concept of race must be genetical.

Thus, with no clear genetic conception of race, Hiernaux suggested the total abandonment of the concept by all related disciplines. In response, many geneticists began to research clinal variation, revealing the fact that genetic distance and geographic distance are highly correlated (Brace, 1996).

Population geneticists generally deal with the biochemical makeup of organisms and changes in gene frequencies in populations over time, revealing micro-level variations that are not necessarily expressed at the macro level. Evolutionary lineages proposed by evolutionary biologists through morphological variation therefore tend to display different patterns than genetic relationships. Genetic polymorphisms and rates of mutation allow for estimates of degrees of divergence between populations, but they do not show transitions, and therefore typological thinking is not an adequate strategy in this discipline. In terms of the subspecies designation, "Genetics shows us that [racial] typology must be completely removed from our thinking if we are to progress"

(Washburn, 1963:523). According to Washburn (1963), the conditions under which the races evolved no longer exist, having been replaced by new causes of mutation, new kinds of selection, vast migration and interaction (see also Cavalli-Sforza et al., 1994; Cavalli-Sforza and Cavalli-Sforza, 1995). In terms of evolutionary perspective, geneticists are interested in the causes of genetic differentiation leading to evolutionary change (i.e., mutation and selection), whether hereditary, environmental, or social.

Many geneticists in the 1950s and 1960s attempted to move beyond racial classification in humans by comparing gene frequencies at one or a small number of genetic loci, such as ABO, MN, and Rh blood group loci (Nei and Roychoudhury, 1997:29). In general, according to genetic principles, a species can be divided into races when it can be regarded as an essentially discontinuous set of individuals, and blood groups do show, to an extent, discontinuous patterns of variation. However, it was not well understood during that time what the biological function of such traits might be and the significance of their geographic distribution, since blood groupings show differing patterns (both clinal and discontinuous variation) from other measures of variation at single (monogenic) and multiple (polygenic) loci (Molnar, 2002:88–179).

The genetic exploration of phylogeny advanced greatly with the discovery that mitochondrial DNA (mtDNA) can be used to trace maternal lineages, and that all humans can be traced back to a single (female) ancestor in Africa approximately 200,000 years ago (Cann et al., 1987).[5] This ancestor became known as the hypothetical 'mitochondrial Eve.' Cann et al. (1987) also claimed from their study of mtDNA from 147 individuals drawn from five geographic populations that each geographic area (outside of Africa) has

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[5] More recent research suggests that this date is likely closer to 120,000 years ago (Cann and Wilson, 2003).

multiple origins, resulting from repeated episodes of colonization. This was a landmark study in human genetic analysis because it demonstrated that mtDNA gives a magnified view of the diversity present in the human gene pool because mutations accumulate in mtDNA several times faster than in the nuclear DNA and occur at a predictable rate. Also, because mtDNA is inherited maternally and does not recombine like nuclear DNA, it is a good tool for relating individuals to one another. The direct relationship between time and mutational distance for mtDNA would not be expected for nuclear DNA, since gene frequencies can be influenced by recombination, genetic drift, selection, and migration. Additionally, there are about 10^{16} mtDNA molecules within a typical human and they are usually identical to one another, thus making it much easier to obtain samples for sequencing than nuclear DNA, particularly when dealing with fossil humans or hominids.

Mutations hold the key to both reconstructing evolutionary lineages through mtDNA and building a timeframe for ancestor-descendant sequences. For instance, if the hypothetical 'mitochondrial Eve' had two daughters, d1 and d2, and d2 had a single mutation in her mtDNA that was not inherited from her mother but rather was a spontaneous mutation, then all subsequent female descendants of d2 would also carry the same mutation, while all descendents of d1 would not carry the same mutation. Therefore, d2 could be placed into a distinct mitochondrial sequence from d1 and Eve (who can also be grouped into a distinct mitochondrial sequence), that geneticists call a *haplotype*, and groups of related haplotypes are *haplogroups* (Olsen, 2002:35). More recently, Y chromosomes have been used in much the same way to study ancestor-descendant sequences in males. Since the Y chromosome carries information about the evolutionary past of paternal lineages, it provides complementary information to that carried by the matrilineal mtDNA molecule (Stumpf and Goldstein, 2001).

Genomic data (i.e., the full set of DNA) in humans is particularly interesting to geneticists because our genome sequences are approximately 99.9% identical to each other. Each individual carries in the order of 30% of the entire haplotype variation of the human gene pool. Both mtDNA and Y chromosome haplotypes form 'blocks,' and any single human chromosome is a mosaic of different haplotype blocks, where each block has its own pattern of variation (Pääbo, 2003). Gabriel et al. (2002) studied 928 such haplotype blocks in humans from Africa, Asia, and Europe, discovering that 51% were found on all three continents, 72% on two continents and only 28% on one continent. Of those haplotypes that were found on one continent only, 90% were found in Africa, and African DNA sequences differ on average more amongt themselves than they do from Asian or European DNA sequences. This evidence (and the low amount of overall genetic diversity) suggests that humans began to rapidly increase in numbers somewhere between 50,000 and 200,000 years ago from a population of about 10,000 individuals, probably a small African population. Thus, from a genomic perspective, humans are all Africans, either living in Africa or in quite recent exile outside Africa (Harpending and Rogers, 2000; Pääbo, 2003:410).

The antiquity of human 'races' has been vigorously debated by anthropologists for well over a century (Cartmill, 1997; Billinger, 2000), and genetic evidence has proven a highly valuable resource for exploring this issue. According to Pääbo (2003:410), claims about fixed genetic differences between races have proved to be due to insufficient sampling. Because the main pattern of global genetic variation is one of clinal gene-frequency gradients, the contention that significant differences between races can be seen in frequencies of various genetic markers is very likely due to sampling of populations separated by vast geographical distances.

While genetic evidence conclusively points to Africa as the origin of modern humans, theories regarding the processes by which humans came to occupy various regions of the globe remain controversial (Collard and Wood, 2000; Hawks and Wolpoff, 2003). There are three main competing theories of human phylogeny. The *Out-of-Africa* hypothesis is by far the most popular. Based on the mtDNA evidence of Cann et al. (1987), it claims that modern humans evolved in Africa approximately 200,000 years and spread throughout the Europe and Asia in two or three waves beginning around 100,000 years ago and reaching Asia around 60,000 years ago.

The *Multiregional Continuity* hypothesis is an anthropological model based on a combination of morphological and genetic evidence, which claims that the origin of races began with the migration of *Homo erectus* out of Africa two million years ago. *Homo erectus* then split into a number of different groups spreading throughout Northwestern Africa, Asia, and Europe. Through natural selection, these *H. erectus* groups adapted to their local environments thus developing a number of morphological variations distinct from the original *erectus* form. These multiple new forms of *H. erectus* began to evolve independently (though with limited gene flow), which by between 250,000–200,000 years ago, formed the distinguishable populations that some *Out-of-Africa* proponents recognize as *H. antecessor* (Europe), *H. heidelbergensis* (Africa and Europe) and *H. neanderthalensis* (Europe and Asia)[6]. According to this model, these distinct populations

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[6] The taxonomic status of the Neanderthals (*H. neanderthalensis*) has been particularly controversial since the discovery of the first Neanderthal specimen in 1856 (Stringer, 2002). Multiregionalists prefer the

eventually evolved into regionalized variants of *Homo sapiens*, the early ancestors of modern races, though none ever reached a degree of differentiation warranting species designations (see Thorne and Wolpoff, 2003; contra Wood and Richmond, 2000; Tattersall, 2003; earlier perspectives on multiregionalism were proposed by Weidenreich 1943, 1947; Brace, 1967).

More recently, the *Weak Garden of Eden* (GOE) hypothesis (which is marginally compatible with the multiregional view) suggests that modern *Homo sapiens* evolved from a subpopulation of *Homo erectus* and spread slowly over several tens of thousands of years, then later expanded from separated daughter populations (Harpending et al., 1993:484). However, Harpending and Rogers (2000) caution about over-extending the usefulness of genetic data for resolving problems of human phylogeny:

> Five years ago, we would have said that genetic evidence provided unambiguous support for the GOE model of human origins. Today, the case is far less clear. Several loci indicate that the human population passed through a bottleneck—a period of small population size. These loci seem to support the GOE hypothesis. Yet other loci indicate just as strongly that no bottleneck has occurred within the past several hundred thousand years. Any attempt to use genetic data in unravelling human history must deal with the discrepancy between these sets of loci.

Further problems with genetic data are that they contain no information about functional changes that occurred during the transition of archaic populations into modern humans. Therefore, debates over the efficacy of competing human evolutionary models are debates about history rather than functional biological change (Harpending and Rogers, 2000:364). In reconstructing this history, the apportionment of human genetic diversity has been the primary focus.

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subspecific taxonomic designation *H. sapiens neanderthalensis* and reject both *H. heidelbergensis* and *H. antecessor* based on the fragmentary nature of the known specimens.

Barbujani et al. (1997) studied within and between population distances at numerous independent loci, with their results showing that only one of the 109 loci studied demonstrated a within-population component of variance less than 50% of the total. The authors concluded, “If loci showing a discontinuous distribution across continents exist, they have not been observed in this study, and so the burden of the proof is now on the supporters of a biological basis for human racial classification” (Barbujani et al., 1997:4518). Further support for these conclusions comes from their observation that gene frequencies do, in fact, form smooth clines over all continents, and that while there are zones of discontinuity in human gene frequency distributions, local gradients are so small that they can be identified only by simultaneously studying many loci using complex statistical techniques. Barbujani et al. (1997:4518) therefore conclude:

> Probably any two populations compared at a sufficient number of loci may be shown to differ, as suggested by the fact that several variances among populations, although low in relative terms, are statistically significant in this study. However, this has little to do with the subdivision of the human population into a small number of clearly distinct, racial or continental, groups. The existence of such broad groups is not supported by the present analysis of DNA.

While genetic analyses give little indication of racial patterns, numerous landmark studies in the field of genetics using various loci, from blood groups to proteins, and mtDNA (Lewontin, 1972; Latter, 1980; Ryman et al., 1983; Seielstad et al., 1998; Excoffier et al., 1992; Dean et al., 1994; Barbujani et al., 1997; Nei and Roychoudhury, 1997), show that so-called racial variation accounts for anywhere between 2.8% and 28.0% of human variation, depending upon the method employed, with individual variation accounting for between 74.7% and 97.8%, the vast majority of overall genetic variation (Brown and Armelagos, 2001).

Anthropology

In terms of anthropological approaches to race, two distinct currents have developed in contemporary anthropological study. This section will focus on the development of race as a historical analytical tool in both cultural and physical anthropology, though the primary concentration will be on biological usages.

As demonstrated in the previous chapter, the race concept has been implicit in anthropological study since the inception of the discipline separate from natural history in the 19th century. However, the domain of anthropologists is strictly the human realm, so the development and usage of racial theories and knowledge have taken on different philosophical meanings in the debates that have arisen in human evolutionary studies in general. Questions about the meaning of racial variation in anthropology have been raised due to the indiscriminate use of racial terminology by anthropologists in describing stages of cultural evolution, groups associated by language, religion and nationality, assemblages of phenotypic traits, assemblages of osteological markers and anthropometric variables, and genetic groupings. Anthropologists have borrowed much of their knowledge of biological variation from evolutionary biology and genetics, so there will be significant overlap in many areas, though again, anthropologists apply this information strictly to humans.

In terms of anthropological conceptions of race and the development of race as a social concept, the roots of modern anthropological study are derived from the 19th century European tradition of classification and comparison. The inherent connection between biology and psychology discussed in the previous chapter would remain at the foundation of physical anthropology in the 20th century, based on the craniometric studies of Samuel Morton and Paul Broca in the early part of the 19th century, which equated skull size (from which cranial capacity is derived) with innate intelligence. In response to this trend, Boas (1910–1913[1940]; 1916[1940]) demonstrated the artificial nature of craniometric evidence of human intellectual variation, showing that external (environmental) forces are capable of producing significant morphological change even within a single generation, indicative of the great plasticity of the human body. Despite the fact that Boas had essentially created American anthropology and had numerous students espousing the 'no-race' position, interest in the relationship between inherited characteristics and social and psychological achievement would become the primary focus of physical anthropology in North American under Earnest Hooton and Aleš Hrdlička (Brace, 1982; 1996; Mukhopadhyay and Moses, 1997). These views would transcend the morphological emphasis of systematic taxonomy, attempting to correlate differences in cultural and intellectual achievement with any number of phenotypic traits, particularly skin colour, hair, eye, and nose form.

Hooton's biological perspective, in particular, was heavily clouded by external factors, mainly capitalist-economic concerns involving 'positive' eugenics and what he perceived as the betterment of the human species through controlled mating, arguing, "Society ... cannot withstand that drain upon economic and biological resources which results from subsequent proliferation by breeding of the constitutional inferiors, hereditarily diseased, feeble-minded and insane" (1940:xvii). In terms of his specific views on race and human taxonomy, Hooton (1940:141) strongly asserted,

> Now this procedure [taxonomic sectioning] evokes immediate and violent objection from certain anthropologists who feel that a continuous distribution of anthropological characters should be inviolable and that any one who splits it up is committing a biometric rape. Personally, I have

> no respect whatsoever for the virginity of a normal curve which owes the symmetry of its distribution merely to the chance occurrences of promiscuity. We have every taxonomic right to put individuals who resemble each other in classes, irrespective of residual or dump groups of other individuals who are so scrambled in their physical characteristics that they resemble others only in their heterogeneity.

Hooton's views would gain popular and academic acceptance when he presented them in the context of human evolution, which he based on a comprehensive review of fossil evidence and osteological analysis in *Up From the Ape* (1931).

For Hooton, taxonomy must begin with phenotype, investigating which traits are obvious indicators of difference that can be easily appreciated by the eye of the ordinary observer. He felt that the observation of external variation was elementary. This is perhaps a product of his education being primarily in the field of classics rather than biology (Brace, 2005:234). In attempting to reconcile the relationship between genotype and phenotype, he contended,

> Racial classifications, in so far as they are to be employed by scientists or anybody for the betterment and the alleviation of human struggles, must conform to the ancient, natural, and, on the whole, correct procedure of taxonomy. They must be based upon observable characters of human phenotypes. Yet these classifications, if they are to be valid and meaningful, must be brought into line with the discoveries of modern genetics. If the physical groupings of mankind are assumed to be based upon combinations of inherited rather than acquired characters, it is necessary to demonstrate that the phenotypes involved do, in all probability, represent the visible manifestations of genetic factors – that behind these phenotypes are inferential genotypes which are responsible for them, in conjunction with the inevitable modifying effects of environment (1946:441).

Whereas Hooton's interest was in the evolution (i.e., natural and artificial section) of human intelligence, he also, somewhat contradictorily, suggested that "Even the term 'race' as applied to man is commonly employed with no accurate and well defined meaning … Such confusions of usage are usually confined to the non-anthropological writing public. All anthropologists agree that the criteria of race are physical characters" (1946:447). Again, Hooton (1946:570) made inconsistent claims regarding racial taxonomy, stating,

> [S]omething has to be done to bring order into the chaos of racial classificatory schemes. Many of them seem to have been drawn up rather irresponsibly by arm-chair anthropologists who have listed as characters of this or that race whole arrays of subjectively established variations or supposititious metric feature for which there is very little scientific basis. Frequently these racial classificationists, immured in their imitation ivory towers, establish wholly new and purely hypothetical racial types or subtypes without bothering to find out whether they exist, and where, and in what numbers.

Hooton (1946:571) then asserted that "There is no radical disagreement among physical anthropologists in the actual recognition of contemporary human types, because these are natural, observable combinations in individuals and groups of anatomical features, whether of exclusively hereditary or of partially environmental origin." It is clear that anthropological conceptions of race have been predominantly clouded with such inconsistencies in interpreting the relationship between heritable factors, genotypes and phenotypes, and the usage of concepts and terminology within anthropology and other disciplines.

From the 1930s to the 1950s, a student of Hooton's, Carleton Coon, attempted to provide some clarification to the race issue, and particularly to debates over the number of races represented in the human species through paleoanthropological evidence. Coon's focus was primarily on the process of speciation, rather than raciation, though he viewed human groups as part of a single unified species. His hypotheses focused on multiple lineages of *Homo erectus* evolving at different times in five different geographic locales. Many anthropologists were critical of this view (see especially Montagu and

Dobzhansky, 1947; Marks, 1995:57–60), arguing that five subspecies of *Homo erectus* could not actually be identified, but Coon (1939) argued in *The Races of Europe*,

> The present races of Europe are derived from a blend of food-producing peoples from Asia and Africa, of basically Mediterranean racial form with the descendants of interglacial and glacial food-gatherers, produced in turn by a blending of basic *Homo sapiens* related to the remote ancestor of the Mediterraneans [*Homo erectus* based populations], with some non-*sapiens* species of general Neanderthaloid form. The actions and interactions of environment, selection, migration and human culture upon the various entities within this algorithm, have produced the white race in its present complexity

Placing 'whites' at the top of his racial hierarchy, Coon suggested that the evolution from *Homo erectus* to *Homo sapiens* occurred at different times for each of his five races – Caucasoids first, Mongoloids next, and finally Africans, Native Americans, and Australian aborigines. Coon argued that this late evolution meant these races were intellectually and physically inferior. With the outrage that Coon's racial classification produced, his view of multiregional evolution became seen as inherently racist rather than racial (Cartmill, 1997).

Despite his controversial theories, Coon would later team up with Garn and Birdsell to produce *Races: A Study of the Problems of Race Formation in Man* (1950), in an attempt to synthesize new genetic data to build a definitive classification. Moving beyond the typical recognition of between three and six races, they presented a 'functional classification' according to three basic criteria (1950:115–5):

1. Evolutionary status as reflected in differences in tooth and jaw size, skull thickness, browridge size and the presence or absence of other archaic features.
2. Body build as reflected in special adaptations to environment, (deserts, mountains, heat, and cold).
3. Special surface feature, such as black skin, flat faces, hair distribution, etc., which appear to be adaptations to heat, light and cold.

Following these specific criteria, the authors built a classification of thirty distinct micro-races. This classification was based on geographical zones inhabited by distinct or heterogeneous populations (geographic or micro-races), encompassed by six larger 'racial stocks': *Negroid, Mongoloid, White, Australoid, American Indian* and *Polynesian.*

While the research of Coon, Garn and Birdsell was considered a significant step in understanding the clustering of populations by geographic region, many anthropologists felt that they could not support such classification in light of genetic evidence and Coon's history of racist writing. Even before Coon's racial theories, it was being argued that, "An extensive refinement of terminology is required if the classifications of physical anthropology are to be brought into harmony with genetic principles, and this will necessitate a more modest estimate of the theoretical conclusions deducible from purely anatomical data" (Hogben, 1932:476). However, in the mid 20th century, Ashley Montagu emerged as the champion of the anti-race movement in the sciences following the work of Boas before him. Montagu (1964:71; previous versions appear in Montagu, 1941:247, 1942a:36) specifically argued,

> The indictment against the older, or classical, anthropological conception of 'race' is that: (1) it is artificial, (2) it does not correspond with the facts, (3) it leads to confusion and the perpetuation of error, and finally, (4) for all these reasons it is meaningless, or rather, more accurately, such meaning as it possesses is false. Based as it is on unexamined facts and unjustifiable generalizations, it were better that the term 'race,' being so weighted down with false meaning, be dropped altogether from the vocabulary.

But race has not been dropped from either the scholarly or popular vocabulary. Montagu (1942a:38–29, 1964:74) thus attempted to use a 'genetical theory of race' to redefine racial taxonomy by emphasizing ethnic group differences rather than continuing to use traditional terminology, presenting four fundamental postulates to support his new system:

1. That the original ancestral species population was genetically relatively homogenous;
2. That by migration away from this original ancestral group, individual families became dispersed over the earth;
3. That some of the groups thus dispersed became geographically isolated from one another and remained so isolated for more or less considerable periods of time;
4. That upon all these isolated groups several of the following factors came into play as conditions leading to evolutionary change:
   a. The genetic drift or inherent variability of the genotypic materials composing each individual member of the group and,
   b. Physical change in the action of a gene associated, in a partial manner, with a particular character, that is, gene mutation.

Although not unproblematic, Montagu argued that ethnicity was far more useful as a means of identifying humans by specific population rather than by large geographic ancestral groups or races, as ethnicity can be characterized as any population with shared communal characteristics – linguistic, ancestral, regional, religious, etc. – which he suggested are the causal factors of distinctive identity at the regional or micro level. Even though Montagu's focus on ethnic groups seemed in tune with the understanding of genetics of the day, it failed to gain momentum, and few anthropologists followed suit, continuing to see diversity through the persistent racial template.

According to Brace et al. (1993:19), "The pragmatic solution to the problem of designation is best dealt with by the use of simple geographic terms ... Not only is there no invidious loading involved, but the focus can be expanded or contracted in simple and efficient fashion as, for example, by specifying direction such as Northwest Europe, Central Europe, West Africa, Southeast Asia, and the like." These geographical referents can be determined by (phenetic) clustering of anthropometric data, which Brace has successfully applied to ancient Egypt (see also Brace, 2005:17–18).

This strategy is borrowed from the pheneticists in evolutionary biology, who refer to these clusters as 'populations,' and this terminology is sufficiently ambiguous to describe a number of kinds of human groupings: cultural, linguistic, religious and ancestral. Such an analysis uses the principles of numeric taxonomy to develop biological distance dendrograms in order to map microevolutionary relationships in historic populations. These clusters may form in a certain geographic location because local (or micro-geographic) populations have traditionally had significantly higher levels of endogamous than exogamous mating:

> Where human traits have adaptive significance, their distributions are determined by the distribution of the controlling selective forces and 'there are no races, only clines.' Where traits have no adaptive significance, neighbours will share traits with neighbors and the analysis of adjacent samples will show that they cluster together ... Neighboring populations share trivial traits with each other to the extent that they form clusters based on relationships and strictly in proportion to breeding distance (Brace et al., 1993:26).

The ambiguity of the term 'population' allows anthropologists to avoid falling into the essentialism of typologies, though it remains unclear how using population as a referent will change anthropological perspectives. Molnar (2002:55) has argued of this perspective,

> The populationist view … considers the frequency distribution of traits throughout the group examined. Comparisons of the similarity or dissimilarity between adjacent populations then can be determined by the degree of overlap of these normal curves of the populations' traits. The results often show a distribution which forms a continuum over a broad geographic area encompassing numerous populations. Considered in this way, clines of frequency distribution may replace the rigid classifications of types of race/subspecies.

It remains to be seen whether this perspective will be adequately developed in anthropological methodology in order to make a sufficient impact. According to Cartmill (1998:655), most researchers studying human variation do not make use of the concept of race in gathering and analysing their data; however, a consistently large minority continue to do so.

Across the Disciplines

The three disciplines described above are all interconnected, all deriving from the natural history of the 18th century, but representing different levels of analysis. While anthropology is generally only related to evolutionary biology in terms of analogy in evolutionary trends between humans and other organisms, genetics and evolutionary biology are implicitly connected through the investigation of forces of selection. Anthropology has grown closer to the study of genetics with the introduction of 'molecular anthropology.' Marks (2002a:134) suggests that by crossing the boundaries between anthropology and genetics, a new understanding of human diversity (i.e., reconciling the anthropological focus on phenotype rather than genotype) can be developed: "I am proposing the development of a molecular anthropology that is generally harmonious with the field of anthropology and complementary to molecular genetics. This field should engage issues of race beyond the geneticist's tired proclamation that it doesn't exist and then paradoxically proceeding to use it." The following section of this chapter will deal with the problems of race and the misuse of terminology across the disciplines of evolutionary biology, genetics, and anthropology.

The main issue in both the social and biological sciences concerning race is the problematic nature of its usage. Though a large number of biologists attest to the ambiguous nature of the geographic subspecies, geneticists have clearly shown that genetic differences within so-called races far outweigh those between them, and the majority of anthropologists have dismissed race as either a social or biological fallacy, all three disciplines are linked by their indiscriminate and continual usage of racial terminology. As Marks suggests, geneticists may claim that there are no races, yet they use race to describe genetic groups; biologists may also recognize the problems of racial categories in relation to humans, but see no problem is discussing races of other species, and anthropologists, similarly, speak frequently of *Caucasoid*, *Negroid* and *Mongoloid* traits, all of which are racial designates.

For anthropologists, the work of Montagu represents perhaps the only real concerted effort from within anthropology to propose solutions to the problems associated with racial categorisation.[7] Montagu was well versed in human genetics, and often enlisted geneticists to comment on the problems of using phenotypes as indicative of ancestral group association. He tackled the issues of race and racism together, viewing them as stemming from, and promoting, essentially the same process of intellectual differentiation (Montagu, 1942a; 1964; 1997). However, where Montagu (1997:186) and his followers argue that the term 'ethnicity' or 'ethnic group' is more reflective of human diversity, representing "one of a number of populations, which grade into one another and together comprise the species *Homo sapiens*, but individually maintain their differences, physical and cultural, by means of isolating mechanisms such as geographic and social

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[7] Livingstone (1962) presents a strong argument for clinal variation, using it as the basis for his rejection of racial categorization, but does not present a generalized model for conceptualizing human variation.

barriers," the term 'ethnicity' has not been generally accepted as having significant biological meaning (see Chapter 3 for a more in-depth analysis of ethnicity).

That race has more social than biological meaning is perhaps the fundamental reason why racial terminology is continually used in the social and biological sciences, though it is evident that it is certainly highly ambiguous and uncritically applied. This issue is well illustrated in forensic anthropology, where practitioners commonly refer to racial traits in the identification of skeletonized individuals: "[A]lthough forensic anthropologists recognize (indeed exploit) the multivariate and clinal nature of human skeletal variation, their success as part of the overall medicolegal process depends upon making a single 'archetypal' racial identification that is based on a classification system that is *geographically relevant* and *locally understood* by law enforcement officers (Gordon, 1993:4). The argument here is that while race may not be an objective category, its sociological implications are easily understood. However, Sauer (1993:81–83) even goes as far as to suggest,

> Contrary to some of my colleagues, I do not believe that the traditional race concept is in any way vindicated by such forensic identification ... The work of researchers like Howells (1973) and Brace and Hunt (1990) on human cranial variation suggests that we ought to be able to do better than estimate a major race ... I could not agree more. I find it quite interesting that so much of the research into the systematic variation in human skeletal form has been framed in the traditional race construct. In forensic anthropology there may be some rationale, but perhaps if we had treated the issue differently from the start, we might be further ahead today.

Even though many of those anthropologists who continue to use racial classifications for pragmatic reasons do not believe that essential divisions exist within the human species, there has been little in terms of the philosophy of human variation to allow them to move beyond the old race concept.

It has only recently been understood that a high degree of discrepancy exists between osteological data and genetic data taken from various loci. The basic genetic understanding of human diversity traditionally espoused by physical anthropologists is based upon three main principles (Ehrlich and Holm, 1973:503):

1. There is geographic variation in numerous human phenotypic traits.
2. The geographic variation has a largely genetic basis.
3. Variation in many instances cuts across cultural lines.

But this only confuses the relationship between genotype and phenotype, for the phenotypic expression of genes is always subject to the forces of the immediate environment, and that genes are pleiotropic, meaning that a single gene may affect multiple phenotypes, or conversely, several genes may affect a single phenotype (Griffiths et al., 2000:106). Traditional notions of human variation by physical anthropologists draw a direct correlation between genotype and phenotype, which is not necessarily the case. In response to this, the physical anthropologist Krogman (1943:104), asserted that:

> The term race as we use it today is a recognition that group differences do in fact exist, it does not imply, scientifically and biologically, homogeneity such as demanded by geneticists. When our knowledge of human heredity enables us to classify the peoples of the worlds genotypically we will gladly accept that classification – we will substitute it for the one we now have. Until then, and with full and complete recognition of all of its many inadequacies we will use the system at hand.

Although it was widely understood that the dominant system of classification was fundamentally flawed, it has been persistently used in response to the lack in genetic knowledge before the introduction of molecular anthropology.

What is perhaps of most concern to all disciplines that use racial classifications, whether historically or contemporarily, is the diagnostic use of race as a biological and social category for the purposes of medical research. This usage differs substantially from the theoretical perspectives of evolutionary geneticists as already discussed, and like anthropologists, medical scientists deal specifically with humans. The uncritical and undefined use of racial terminology in medical research and literature leads to three sorts of problems in the conduct and reporting of research: 1) non-equivalent uses of race within one research report; 2) inverting the relationship between genetics and race, or studying race as an end in itself; and 3) an overemphasis on race (Sankar and Cho, 2002). Taken together, the result of these three problems is an unnatural comparison between taxonomic, political and geographic referents when dealing with cultural or biological groups (e.g. referring synonymously to *Mongoloid*, Chinese and Asian patients), and in general, using the race concepts as a starting point for research, rather than a result.

Many medical practitioners feel that either race is an important variable in their research, or that simply, although race groupings are not biologically or anthropologically relevant, they should stay intact for the sake of continuity, and their removal would be just giving in to political correctness. This issue has taken up many pages in the *New England Journal of Medicine* (see particularly vol. 348, no. 12, 2003). Burchard et al. (2003) argue that the racial or ethnic groups differ not only in terms of genetic makeup, but that socioeconomic status is strongly correlated with race and ethnic background and is a robust predictor of access to and quality of health care and education, which may be associated with differences in the incidences and outcomes of various diseases. On the other hand, Cooper et al. (2003) explain that genomics have provided no evidence that race can act as a surrogate for genetic constitution in medicine or public health, and that at the continental level, race has not been shown to provide a useful categorization of genetic information about the response to drugs, diagnosis, or causes of disease.

In response to the *New England Journal of Medicine* articles, *Nature Genetics* (vol. 36, no. 11, supplement 1, 2004) has also dealt with the race issue. Collins (2004) suggests that while 'race' and 'ethnicity' are used without agreement over definition, there may in fact be a connection between self-identified race or ethnicity and the frequency of various genetic traits. Similarly, Jorde and Wooding (2004) believe that race and ethnicity may, in some cases, provide useful information in biomedical contexts, but the potential usefulness of such categories must be measured against the hazards: "The general public, including policy-makers, are easily seduced by typological thinking, and so they must be made aware of the genetic data that help to prove it wrong" (Jorde and Wooding, 2004:S32; see also Witzig, 1996).

The majority opinion, however, was that populations do cluster by broad geographic region roughly corresponding to socially recognized races, but the distribution of genetic variation is quasicontinuous in clinal patterns (Tishkoff and Kidd, 2004); racial designations are not discrete, but fluid categories with poorly understood correlations with various biological elements and health outcomes (Royal and Dunston, 2004); macro-geographic generalizations invoking genetic explanations should be avoided unless warranted (Keita et al., 2004); when appropriately large numbers (thousands) of polymorphism are analysed, individual (nation-level) populations can be delimited and members of American admixed populations can be accurately identified (Jorde and Wooding, 2004). Perhaps the most valuable suggestion in this context is given by Royal and Dunston (2004:S5):

> Today, scientists are faced with this situation in genomics, where existing biological models or paradigms of 'racial' and 'ethnic' categorizations cannot accommodate the uniqueness of the individual and universality of humankind that is evident in new knowledge emerging from human genome sequence variation research and molecular anthropological research. The paradigms of human identity based on 'races' as biological constructs are being questioned in light of the preponderance of data on human genome sequence variation and reflect the need for a new explanatory framework and vision of humankind with different fundamental assumptions about biological groups that can accommodate new knowledge from a new generation of research.

The concept of race remains one of the most ambiguous within the various scientific disciplines. The objective of this chapter was to outline the ways in which different disciplines utilise the race concept in their various analyses, and it can easily be seen how problematic race is when applied specifically to humans, relative to all other species. Therefore, while it is common for the concept to be dismissed by anthropologists for sociological and cultural reasons, its usefulness for other disciplines does not face the same scrutiny. However, issues strictly at the level of taxonomic applicability do become obvious as well. There is an implicit need to develop new concepts and terminology acknowledging the complex nature of biodiversity, whether human or not, by species biohistory, phenetic and genealogical affinities, and gradients of differentiation (Keita and Kittles, 1997:541). It should be the goal of every social and biological scientific pedagogue or practitioner who uses race (or "race") as a variable in their research and teaching to understand the underlying issues and to work towards the clarification, both theoretical and practical, of the analysis of patterns of diversity. In order to move beyond the race concept, which has been shown to be arbitrary and with limited utility, new models for explaining the biological variation and evolution of humans and other species are necessary.

Chapter 3

ETHNICITY AS A NON-RACIAL MODEL OF HUMAN VARIATION

Each discipline that incorporates biological classification into its studies has necessarily evolved strategies for dealing with race or subspecies, which are in most instances used interchangeably, but none of these attempts have been remarkably successful. The question to be asked in this chapter therefore is specific and of paramount importance for moving beyond the racial paradigm: are there any workable models available that would allow us to move conceptually beyond the flawed race concept by accommodating the uniqueness of the individual and universality of humankind? This chapter looks at ethnicity as a non-racial model for conceptualizing human variation and analyzes its potential as an explanatory framework. The necessary shift away from the race concept will not be successful until an appropriate theory and method are in place, and that will only be possible when not just the patterns of evolutionary change are accounted for, but also the processes causing such change.

Franz Boas was the first to mount a concerted attack on the idea of race from within anthropology. He did it not only on philosophical grounds, but also with extensive empirical data. Building on his earlier studies of cephalic index (1899) and heredity in head form (1903), Boas undertook a vast study of cranial morphology, from which he published a number of papers between 1910–1913 (see Boas, 1970[1911], 1912, 1940) under the heading "Changes in the Bodily Form of Descendants of Immigrants." This collection has become classic in anthropology for its demonstration of the plasticity of the human form. The premise of this project was, according to Boas (1970[1911]:1), that "the principal data to be collected in an investigation of this kind must relate to the differences in composition of the immigrants that arrive in this country at different periods, and to the changes that may take place among their descendants born in this country." Boas analysed anthropometric data from over 13,000 European immigrants to the United States and their descendants, focusing primarily on the head form of living individuals using the cephalic index (calculated by dividing the head breadth by length and multiplying by 100) in order to study change in constitution over time. It had been previously assumed that the cephalic index demonstrated heritability and was resistant to environmental influences (Halloway, 2002; Gravlee et al., 2003) and therefore average values for the index should remain constant between types. The immigrant groups selected for study were chosen because they represented both 'the most distinct European types' and the largest groups of immigrants to the United States at that time.[1]

It seems that even Boas (1970[1911]:2) himself was surprised by the results that he obtained, which he subjected to thorough statistical analysis:

> From a practical point of view, it seemed all-important to know whether the American environment had a favourable or unfavourable effect upon the descendants of immigrants. The investigation has shown much more than was anticipated. There are not only decided changes in the rate of development of immigrants, but there is also a far-reaching change in type – a change which can not be ascribed to selection or mixture, but which can only be explained as due directly to the influence of environment.

Boas (1912:530) drew many conclusions from the analyses of the extensive data he collected, but his conclusions based on morphology by cephalic index calculations are most relevant to the present discussion:

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[1] Boas (1970[1911]:55-56) classified the immigrants groups as: Central European – Bohemians, Slovaks and Hungarians, Poles; Hebrews (Russia, Poland, Germany, Austria, Switzerland, and Romania); Mediterraneans (Sicilians, Neapolitans); Scotch. It is not clear, however, how the Scotch actually fit into this typology.

1. American-born descendants of immigrants differ in type from their foreign-born parents. The changes that occur among various European types are not all in the same direction. They develop in early childhood and persist throughout life (see Table 3.1).
2. The influence of American environment makes itself felt with increasing intensity, according to the time elapsed between the arrival of the mother and the birth of the child (see Figure 3.1).

Boas' results were complicated and his methodology was often unclear and as a result, he was subjected to numerous criticisms of his original conclusions (Boas, 1912:533), prompting him to publish his entire data set in 1928.

The results of Boas' study remain controversial even today. Two recent re-assessments of Boas' 1928 data have reawakened the debate over the role of environmental factors in determining the size and shape of the skull. Sparks and Jantz (2002) applied modern statistical techniques to Boas' data concluding that the results demonstrate that heritability is stronger than Boas' results suggest, while Gravlee et al. (2003) also statistically analysed the results, concluding that Boas was essentially correct in his conclusions. The prolonged debate over the relationship between heritable and environmental factors in shaping physical constitution gives testament to the complexity of this relationship, which anthropologists have attempted to reconcile for well over a century (see also Relethford, 2004). It should be noted, however, that the simplicity of the cephalic index, based on only two variables, cannot adequately account for the relationship between heritable and environmental factors, and that more complex methods rather than more complex statistical analyses are necessary. However, the results are suggestive that perhaps the best model for explaining this relationship as a non-racial phenomenon is the concept of the ethnic group, though the concept is in need of critical appraisal.

Table 3.1 Increase (+) or decrease (-) in measurements of children of immigrants born in the United States compared with those of immigrants born in Europe, weighted according to number of cases (from Boas, 1970[1911]:56; 1940:60). Measurement in millimetres.

Race/nationality and sex		Length of head	Width of head	Cephalic index	Width of face	Stature	Weight
Bohemians	*male*	-0.7	-2.3	-1.0	-2.1	+29.0	170
	female	-0.6	-1.5	-0.6	-1.7	+22.0	180
Hungarians and Slovaks	*male*	-0.5	-1.1	-0.7	-1.0	+59.0	54
	female	-0.3	-0.9	-1.0	-2.2	+10.0	38
Poles	*male*	-0.3	+0.2	+0.2	+0.7	+42.0	22
	female	+0.9	-1.6	-1.4	-1.3	+17.0	27
Hebrews	*male*	+2.2	-1.8	-2.0	-1.1	+17.0	654
	female	+1.9	-2.0	-2.0	-1.3	+15.0	259
Sicilians	*male*	-2.4	+0.7	+1.3	-1.2	-1.0	188
	female	-3.0	+0.8	+1.8	-2.0	-5.0	144
Neapolitans	*male*	-0.9	+0.9	+0.9	-1.2	+6.0	248
	female	-1.7	+1.0	+1.4	-0.6	-18.0	126
Scotch	*male*	+1.4	-0.5	-0.8	-1.5	+18.0	39
	female	-0.3	+0.3	+0.2	+1.9	+39.0	33

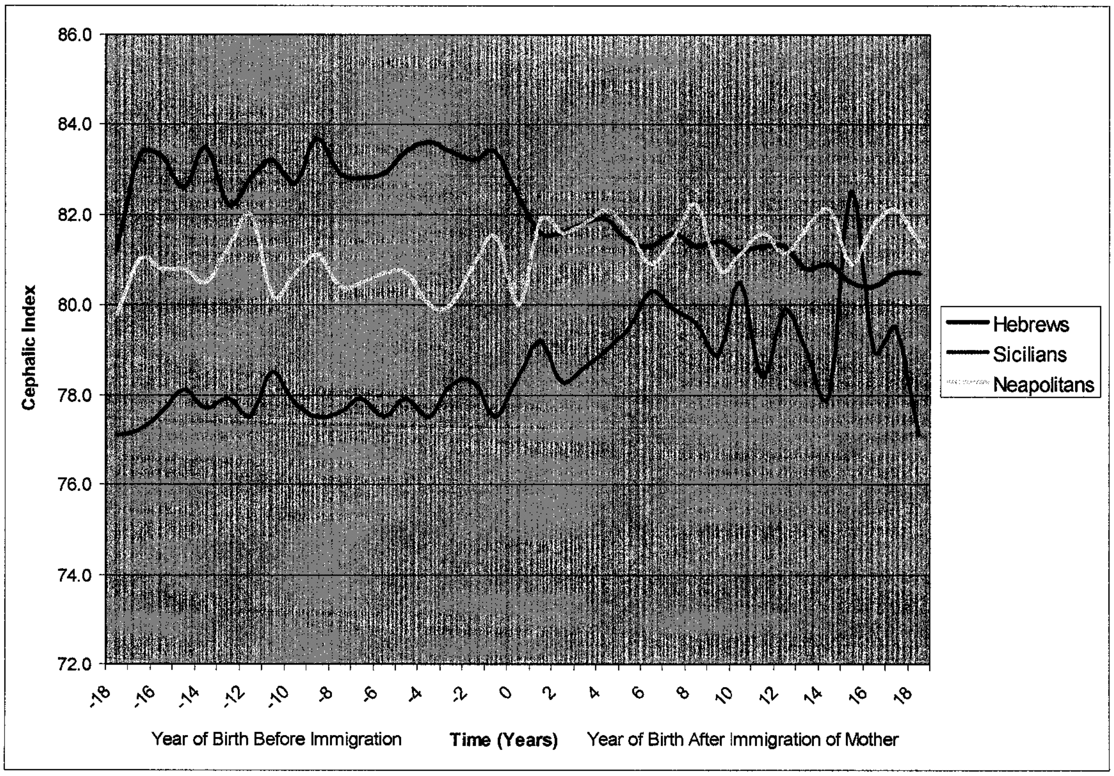

Figure 3.1 Cephalic index of foreign-born and American-born adult males, arranged according to time elapsed between birth and immigration: Hebrews, Sicilians, and Neapolitans (data plotted from Boas, 1970[1911]:61; 1940:61).

In 1900 [1904], the chief librarian of Le Muséum National d'Histoire Naturelle in Paris, Joseph Deniker, published *The Races of Man*. In it, he looked at the study of anthropology and ethnography and the problems of racial classification. This book represents what is likely the first published argument that the traditional anthropological race concept should be replaced by the notion of ethnicity. What is most remarkable about this book is how Deniker seemingly anticipated many of the debates that would arise in the biological sciences and anthropology during the mid 20th century and into the 21st century, drawing on many problems with systematic taxonomy in general, and its application to humans in particular.

Deniker appreciated the great cultural and biological variability of human groups, and was sceptical of attempts to give a systematic view of all the peoples of the earth, either socially or physically. He was primarily interested in fundamental questions about the nature of human groups and their relations to zoological phenomena, leading him to ask:

> Do these real and palpable groupings represent unions of individuals which, in spite of some slight dissimilarities, are capable of forming what zoologists call 'species,' 'subspecies,' 'varieties,' in the case of wild animals, or 'races' in the case of domestic animals? One need not be a professional anthropologist to reply negatively to this question. They are *ethnic groups* formed by virtue of community of language, religion, social institutions, etc. ... and are by no means zoological species (Deniker, 1904:2–3).

Deniker views races as theoretical groupings, but sees ethnic groups and individuals of ethnic identity as directly observable. Further to the argument against human groups as zoological species, Deniker (1904:3–4) believes that there could be no typical specimen discovered for each or any theoretical racial group:

> Here are, then entities, theoretic conceptions exactly like 'species' in zoology; only instead of having within our reach the 'types' of these species as in zoological collections, we are obliged to rest content with approximations thereto, for it is a very rare occurrence to meet with an individual representing the type of the somatological unit to which he belongs. Most frequently we have to do with subjects whose forms are altered by blendings and crossings, and in whom, setting aside two or three typical traits, we find only a confused mixture of characters presenting nothing striking ... In reality, those peoples are almost undiscoverable who represent 'somatological units' comparable to the 'species' of zoology.

In fact, Deniker (1904:4) not only believes that external physical traits are of little heuristic value in terms of real biological relationships, but argues that none of the zoological terms – species, variety, or race – can be accurately applied to the genus *Homo*.

This objection to the application of zoological terminology such as species or variety not only to *Homo sapiens*, but to the entire genus *Homo*, was based on Deniker's notion of zoological species living under the influence of nature; such natural groupings could only occur in wild animals. In contrast, race could only be the result of the artificial conditions of animal domestication, which created well-defined breeds (Deniker, 1904:4). In essence, Deniker's argument is that man is not subject to nature in the same ways that animals are – although humans live in artificial environments, these environments are created by ourselves, not by outside forces. However, because these conditions are self-imposed and not created by domestication, man cannot be directly comparable to animal in terms of bodily constitution. Specifically, Deniker (1904:4) argues, "The data relating to the formation of varieties, species, and races can therefore be applied to the morphological study of man only with certain reservations."

The discussion in the previous chapter of the problems of taxonomy across various disciplines demonstrates that race is not simply an anthropological problem, but one of general biological application. This certainly was not lost on Deniker (1904:5), who recognized this point a full century ago: "[L]et us bear in mind that even the distinction between the species, the variety (geographical or otherwise), and the race is anything but clearly marked. Besides, this is a question of general biology, and it is no more settled in botany or in zoology than in anthropology."

Deniker explains that the main barrier to building an accurate classification of human groups is that the recognition of natural biological groups relies on proven interfertility, and while it may be inferred that all human groups have the potential to interbreed, as Darwin (1871) suggested in *The Descent of Man* (Chapter 7), Deniker believes that such interbreeding cannot be proven experimentally among humans of different geographic populations. Deniker also disagreed with Darwin's (1888:280) suggestion that "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 more appropriate." Rather, Deniker (1904:7) rejected the term sub-species: "The word 'race' having been almost universally adopted nowadays to designate the different physical types of mankind, I shall retain it in preference to that of 'sub-species,' while reiterating that there is no essential difference between these two words and the word species."

Even though Deniker (1904:7) disagreed with Darwin in terms of terminology, he agreed that any such division was arbitrary, and because of this, he felt that the debates between those who believed that humans are part of a single unified species and those who believe that human races are separate species were moot:

> The whole of this ancient controversy between monogenists and polygenists seems to be somewhat scholastic, and completely sterile and futile; the same few and badly established facts are always reappearing, interpreted in such and such a fashion by each disputant according to the necessities of his thesis, sometimes led by considerations which are extra-scientific.

Over one hundred years later, the problem remains essentially the same – poorly established facts about human evolution are always reappearing, and are often uncritically applied. In his attempt to push the study of human variation further, Deniker (1904:8–9) believes that humans cannot accurately be studied either zoologically or sociologically. Deniker claims that ethnic groups are the result of a tension between two factors: variability, or dissimilarity, and heredity, or the perpetuation of the similar. Man must therefore be studied as both an individual of a zoological group, and groupings of individuals in societies.

At this point, particularly in Europe, anthropology was seen as the study of the somatological characteristics of the genus *Homo* as a whole or in relation to other animals, whereas ethnography or ethnology were concerned with the study of ethnical characteristics. Deniker (1904:9) was interested in bringing together both these areas of study to build a more comprehensive view of human variation: "However, there is a convergence of characters in mankind, and we find even to-day the trace of savagery in most civilized peoples. Ethnical facts must not then be considered separately." A complete social and biological description of each of the known human populations was Deniker's goals, but in providing such a classification, he reverted to the term "race" for describing what he had already called ethnic groups, and based his classification solely on external physical features. Perhaps the fundamental flaw of this scheme was lack of a definition of ethnic group, which seems to have limited Deniker's own use of the term for describing the variation of man that he saw.

Although Huxley and Haddon (1935:19) make no mention of Deniker's earlier work, they view the relationship between zoological notions of race and its inapplicability to human populations in much the same way, though with a seemingly greater sense of urgency:

> The word 'race' soon acquired a vagueness that it has never since lost. It is probably that this vagueness, together with the occasional employment of the word by certain scientific men of a previous generation and the supposed parallel between zoological and human 'races' have combined to give it a special popularity with a group of writers who deal with scientific themes without adequate scientific equipment. From them it has descended to the literature of a more violent nationalism.

Their strongest indictment against anthropological approaches to taxonomy is that the application of modern (post-synthesis) genetical theory to the problems of biology was not being adequately felt in the domain of the anthropologist (Huxley and Haddon, 1935:60). In an attempt to bring anthropological study into the fold of Mendelian genetics, Huxley and Haddon (1935:74) made it their goal to elaborate "the fundamental distinction between the *phenotype* or visible appearance of an organism and its *genotype* or transmissible constitution."

Huxley and Haddon (1935:104) contend that practically all human groups had a mixed origin, and as a result, all possess a great degree of genetic variation. For them, the true value of studying variation is the range:

> The expectation of the Mendelian geneticist, knowing the facts of inheritance and the migratory habits of man, is of groups possessing a large range of variation, often concerned with striking characters of a qualitative nature as well as with quantitative ones; such groups can only

> be distinguished from each other by statistical methods. In such groups the *mean values* for characters, though still useful, no longer have the same theoretical importance. The *range of variation* of characters is of far greater practical importance, as is also the range of qualitatively different recombination-types. The two resultant 'racial' or ethnic concepts are fundamentally dissimilar.

The emphasis on range over averages in mixed populations lead Huxley and Haddon (1935:107) to argue that race was a non-entity, prompting their famous claim: "In the circumstances, it is very desirable that the term *race* as applied to human groups should be dropped from the vocabulary of science."

The poignancy of their argument is found not only in the social aspects of race that they outline, but they provide concrete genetic arguments as well, citing the vast migration of humans as the principle underlying factor:

> In other animals, the term *sub-species* has been substituted for 'race.' In man, migration and crossing have produced such a fluid state of affairs that no such clear-cut term, as applied to existing conditions, is permissible. What we observe is the relative isolation of groups, their migration and their crossing. In what follows the word *race* will be deliberately avoided, and the term (*ethnic*) *group* or *people* employed for all general purposes (Huxley and Haddon, 1935:107–108).

The relationship between phenotype and genotype continues to present a great problem for anthropologists and biologists alike, and when dealing with such mixed ethnic communities, classifications based on phenotypic evidence, particularly soft tissues, do not correspond well with direct genetic evidence, which is much more difficult to ascertain:

> The method of characters and the method of genes differ in their scientific value and in the practicability. It is much easier to attempt a classification in terms of characters, and indeed this is the only method that is immediately practicable (as well as a necessary first step towards the classification in terms of genes). But it is less satisfactory from the scientific point of view. This is partly because apparently similar characters may be determined by different genes, and conversely because

> the same gene in combination with different constellations of other genes may produce very different characters. It is also less satisfactory because a character is always the result of an interaction between constitution and environment. To disentangle the genetically unimportant effects of environment from the genetically essential action of genes is difficult in all organisms and especially so in man, where the social and cultural environment – unique characters of the human species – play predominant parts (Huxley and Haddon, 1935:108–109).

This focus on social and cultural environment playing predominant roles in shaping the genetics and physical structures of human populations is reminiscent of the perspective taken by Boas in his immigrant studies.

Huxley and Haddon (1935:129) argue that classifications are arbitrary and that any classification of human aggregates by any set of criteria would produce a classification that would poorly fit any other set of criteria. As a result, they were very clear that "If *race* is a scientific term, it must have genetic meaning" (1935:129). However, whereas Deniker rejects the use of the term subspecies to describe human populations, Huxley and Haddon (1935:136) give the subspecies the same hypothetical role that Deniker gave race. But in dealing with real human populations, they also prefer the term ethnic group:

> [T]o avoid the unfortunate connotations of the word race, the term sub-species is preferable. It should be emphasized, however, that the existence of such human sub-species is purely hypothetical. Nowhere does a human group now exist which corresponds closely to a systematic sub-species in animals, since various original sub-species have crossed repeatedly and constantly. For the existing populations, the non-committal term *ethnic group* should be used.

Their idea of ethnic classification is one that should be quantitative rather than qualitative, and multivariate rather than based on single characters:

> To sum up, the first aim of ethnic classification should be to give an accurate descriptive picture of the physical characteristics of different regional groups, in terms of certain agreed physical characteristics. For

> this we require not only averages and statistical estimates of variability for single characters, but curves showing their distribution in adequate samples of the population. We further require numerical estimates (correlation coefficients) of the degree of association between different characters. This procedure will enable us to give a descriptive classification of human populations in different geographical regions of the world, in terms of ethnic groups with certain physical peculiarities (Huxley and Haddon, 1935:143).

This way of thinking about human variation allows for a degree of flexibility that was not common in previous taxonomic thought. Where taxonomies were rigid and ignored microevolutionary processes, Huxley and Haddon (1935:143) believe that the notion of typology could be accurately employed when researchers recognized that types were not static or pure, but theoretical models:[2]

> This descriptive classification may then be interpreted in terms of ideal types which are presumed to have combined to form the existing mixed populations, and of hypothetical major and minor sub-species into which the human species must be presumed to have differentiated in the course of its evolution. But the types must not be regarded as fixed, and the sub-species will never have been genetically pure.

This tension between the notion of type and the appreciation of microevolutionary forces is perhaps the main barrier to moving beyond traditional classificatory attempts, but Huxley and Haddon were clear that the key to understanding biological groups and their relatively rapid evolution was genetic and not morphological, but the problem in their methodology was that they had no way to directly access genetic material in the pre-DNA

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[2] Although Huxley and Haddon make no reference to Max Weber in their discussion of the *ideal type*, they use this notion in much the same way as Weber. Weber believes that concepts must be clear, distinct, and precisely defined in order for the scientific mind to comprehend reality, and that the perfection and purity of the ideal type make concepts clear, distinct and intelligible (Schwartz et al., 1995:425). According to Weber (1949[1903-1917]:90), "an ideal type is formed by the one-sided *accentuation* of one or more points of view and by the synthesis of a great many diffuse, discrete, more or less present and occasionally absent *concrete individual* phenomena, which are arranged according to those one-sidedly emphasized viewpoints into a unified *analytical* construct (*Gedankenbild*). In its purity, this mental construct (*Gedankenbild*) cannot be found empirically anywhere in reality. It is a *utopia*. Historical [or anthropological] research faces the task of determining in each individual case, the extent to which this ideal-construct approximates to or diverges from reality."

age, and were forced to rely primarily on soft tissue variation (hair form and colour, skin colour, eye form and colour) and physiological and psychological factors (body temperature, pulse rate, respiration, onset of puberty, metabolic changes, colour vision, sense perception) along with minimal anthropometric variables (stature; head form; nasal form) and blood group data to form their ethnic classification.

The most comprehensive view of ethnic classification and its value over racial classification is that developed by Montagu (1942a; 1942b; 1962; 1997), as introduced in Chapter 2. Montagu outlines and closely follows the arguments of his predecessors, focusing on both the taxonomic issues that Deniker had outlined and the genetic problems elaborated by Huxley and Haddon (see particularly Montagu, 1962:921–922). Montagu was clear and concise in his criticism of the race concept, and brought this clarity to his arguments for ethnicity as a replacement for race. Earlier attempts to incorporate the social world into the structure of biological systems suffered from a lack of definition for the term 'ethnic,' a problem that Montagu (1942b:374) recognized and attempted to rectify, stating, "An ethnic group represents part of a species population in process of undergoing genetic differentiation; it is a group of individuals capable of hybridizing and intergrading with other such ethnic groups, to produce further genetic recombination and differentiation." More specifically, Montagu (1942b:375) defines the ethnic group as:

> [O]ne of a number of populations comprising the single species *Homo sapiens*, which individually maintain their differences, physical and cultural, by means of isolating mechanisms such as geographic and social barriers. These differences will vary as the power of the geographic and social barriers, acting upon the original genetic differences, vary. Where these barriers are of low power, neighboring groups will intergrade, or hybridize, with one another. Where these barriers are of high power such

> ethnic groups will tend to remain distinct or replace each other geographically or ecologically.

Montagu's (1941:247) indictments against the race concept are direct and well informed, and his belief that the concept is fundamentally flawed is based upon three basic premises: (1) it is artificial; (2) it does not agree with the facts of human variation; (3) it leads to confusion and the perpetuation of error. These arguments are based firmly in the primacy of genetic evidence, which remained lacking in anthropological classifications:

> The immediate task of the physical anthropologist interested in the origins of human variety, is to investigate the problem presented by that variety not as a taxonomist but as a geneticist, since the variety which is loosely called 'race' is a process which can only be accurately described in terms of the frequencies with which individual genes occur in groups which represent adequate ecologic isolates. If 'race' and 'racial' variability can best be described in terms of gene frequencies, then among the most important of our tasks must be that of discovering what roles the primary and secondary factors play in producing that variability (Montagu, 1942b:373).

This perspective entails a complete rejection of Hooton's claim that taxonomy must begin at the level of the phenotype (see Chapter 2).

Montagu refined his argument over a period of about twenty years. Regarding the taxonomic issues first raised by Deniker, Montagu (1962:919) similarly suggested that we are subject to the same forces of evolution as all other species, however, in a form that has been modified by culture:

> At the outset it should, perhaps, be made clear that I believe, with most biologists, that evolutionary factors, similar to those that have been operative in producing raciation in other animal species, have also been operative in the human species – but with a significant added difference, namely, the consequences which have resulted from man's entry into that unique zone of adaptation in which he excels beyond all other creatures, namely *culture,* that is to say, the man-made part of the environment.

This distinction between environment in a natural state, to which all non-domesticated animals are subject, and the artificial environment created by human language and technology is the key to understanding human variation as opposed to species variation in general. For Montagu (1962:919), human variation follows unique patterns due primarily to mobility and social selection:

> On the evidence it would seem clear that man's cultural activities have introduced elements into the processes of human raciation which have so substantially modified the end-products that one can no longer equate the processes of raciation in lower animals with those which have occurred in the evolution of man. The factors of mutation, natural selection, drift, isolation, have all been operative in the evolution of man. But so have such factors as ever-increasing degrees of mobility, hybridization, and social selection, and it is the effects of these and similar factors which, at least so it has always seemed to me, makes the employment of the term 'race' inapplicable to most human populations as we find them today.

It is because of these factors that Montagu argues that traditional biological terminology such as race or subspecies is wholly inadequate in the realm of human biology.

Montagu makes clear that his perspectives on human variation and the unity of species are not to deny the variation that exists in human groups, but to adequately explore the processes that have created the great diversity that is seen. It is therefore Montagu's (1962:919) primary goal to urge the usage of terminology that encapsulates these processes while circumventing the deficiencies seen in the use of the terms race and subspecies in the human realm:

> Of course there exist differences, but we want a term by which to describe the existence of these differences. We do not want a prejudiced term which injects meanings which are not there into the differences. We want a term which as nearly mirrors the conditions as a term can, not one which falsifies and obfuscates the issue.

The limiting factor in the exploration of patterns of human variation is not solely terminological; rather, it must be conceptual. Montagu's criticism of race as *Man's Most*

Dangerous Myth (1942a; 1997) was based not simply in misapplication of racial theory, but in the fallacy of the concept itself. Montagu's main accomplishment in this regard was not only in elaborating the "long and tortured history" (Montagu, 1962:920) of the race concept, but his terminological deconstruction was an attempt to provide a new conceptual landscape for human variation studies.

> The term 'race' takes for granted what should be a matter for inquiry. And this is precisely the point that is raised when one uses the noncommittal 'ethnic group.' It encourages the passage from ignorant or confused certainty to thoughtful uncertainty. For the layman, as for others, the term 'race' closes the door on understanding. The phrase 'ethnic group' opens it, or at the very least, leaves it ajar (Montagu, 1962:926).
>
> The term 'ethnic group' is concerned with questions; the term 'race' is concerned with answers, unsound answers, where for the most part there are only problems that require to be solved before any sound answers can be given (Montagu, 1962:927).

In fact, Montagu (1945) had much earlier measured an attack on the misuse of systematic terminology in the study of human evolution, particularly the terms 'primitive,' 'advanced,' 'specialized,' and 'atavism.'

While Montagu's constant criticisms were focused and well-informed, many of his colleagues were highly critical of his approach. General debate over the race concept was particular bitter between Montagu and Coon, as well as Hooton (Marks, 2000b; see also Dobzhansky et al., 1963), and many who agreed with the inadequacy of the race concept were also critical of the notion of ethnicity that Montagu proposed. Where Montagu argued that physical anthropologists must understand the implications of culture for biology, Martin (1963) argued that the domain of physical anthropologists is biology, dealing with "physical or genetic" differences separate from cultural considerations. To this criticism Montagu (1963:1352) replied, "The very grounds which he finds constituting objections to the usage of that term are those which make it so appealing to me."

In reaction to Montagu's usage of 'ethnicity,' Martin (1963:403) believes that Montagu takes the race concept in the wrong direction, and that the term 'population' is perhaps a better alternative to 'race':

> It is disappointing that Montagu did not directly discuss the term 'population.' It is a term commonly used in current literature and one which Montagu himself employs frequently. 'Population' implies interbreeding individuals, or groups, with varying degrees of genetic exchange. It is admittedly a general and vague term but one which, because of its unspecific nature, involves no assumptions, and erects no barriers, save that of genetic exchange. It is adaptable, and yet meaningful, to virtually every physical study.

Montagu's biocultural approach, in essence, is not particularly different linguistically from the vague and unspecific terminology suggested by Martin; however, Montagu (1963:1352–1353) insists that any new concept or term reflect the desire to explain the mechanisms involved in physical differentiation:

> Physical (genetic) and cultural evolution are not mutually exclusive processes. And, indeed, since man has become man principally because his chief means of adapting himself to the physical environment has been through culture, I hope that no student of physical anthropology will ever again be misled enough to believe that we shall ever intelligently be able to investigate the 'physical or genetic' differences without any implications to culture. The cultural implications are virtually always there.

This notion of human biology as intrinsically cultural remains controversial and the culture concept itself is somewhat problematic to arguments against racial or subspecific classification. Because of this controversy, Montagu suffered much harsh criticism, even from his own anti-race allies, who view physical anthropological evidence as primarily genetic.

Both Polgar (1964) and Brace (1964b) believe that Montagu's concept of ethnicity, and the intentional vagueness of the concept, is virtually indistinguishable from the race concept, and should be recognized as an unwarranted substitution. As a result, Brace (1964b:313) argues that such approaches as those of Deniker, Huxley and Haddon, and Montagu have not led to any significant progress in understanding human variation and diversity because they do not represent an alternative approach. Polgar (1964:424) believes that use of the race concept should involve an explanation of its context, and outlines three operational definitions: populations (geographic groupings), cultural groups or communities, and perceptual sets (based on lay classification of phenotypic attributes). He concludes that the study of phylogenetic relationships between geographic, genetic, or morphological groups would be better served by the use of 'population clusters' or 'phylopopulations.' However, Brace (1964b) takes a strong position against the use of the term 'population,' which he believes obscures correlated clines of trait and selective force. To this, Huxley (Comments, in Brace, 1964b:316) critically replied,

> There is no doubt in my mind that the human species in its early evolution started to form geographical subspecies, and that these became quite sharply differentiated from each other, and gave rise to the so-called primary races. The distinctions between them have, of course, been blurred by migration and crossing ever since that time, but the basic differentials remain the same.

Huxley's (Comments, in Brace, 1964b:316) main interest was how local groups formed within these 'populations,' and it is in relation to these smaller local subspecies or races that he suggests, "The neutral term 'ethnic group' seem to be the scientifically most respectable designation." Clearly, the difference in perception is in the level of analysis.

While Brace's arguments are not only focused upon the recognition of the fundamental flaws of racial taxonomy, but also the flaws of 'ethnicity' or 'population' as replacement terms or concepts, he fails to provide any solutions to the outlined problems. What should be evident from this debate is that the argument is entirely semantic, and the underlying genetic principles are being completely disregarded or overlooked. In response to Brace's comments, Coon (Comments, in Brace, 1964b:314) tells us, "Brace writes as if the concept of 'race' were unique in man and the question of the existence of races could be decided on human materials alone." Similarly, Count (Comments, in Brace, 1964b:315), also commenting on Brace, clearly states,

> [M]an participates in exactly the same evolutionary processes as any other animal – *and in no other.* When cultural elaboration is called evolution in the same breath with organic evolution, it is logical confusion. It is an easy step thereupon to argue that culture has mediated to abrogate the operations of 'natural selection.' I have never encountered any valid evidence in support of such an assertion, and only miracles can abrogate the operation of natural law.

These arguments demonstrate an inherent bias in favour of biology as a reflection of an underlying 'natural' system or structure, at least at that particular historical moment. Neither Coon nor Count provide convincing explanations of why culture can not or should not be considered an external factor with biological outcomes.

Montagu (Comment, in Brace, 1964b:317) sought to clarify the fact that ethnicity was in fact a viable alternative approach not simply because it would replace the word race in the anthropological vocabulary, but because in his view, biological and environmental forces cannot be studied as isolated factors, but should be viewed as integrated causal explanations. Thus, Montagu argues that ethnicity represents a new way of conceptualizing *human* variation, since he believes that social environment has demonstrable effects on biological evolutionary patterns. Montagu (Comment, in Brace, 1964b:317) clearly insisted:

> I have repeatedly emphasized the pathetic fallacy, the reductionist malaise, of regarding populations of human beings as biological races from the purely zoological point of view, for the simple reason that populations of men have never behaved as 'purely zoological' taxa.
> Human populations have behaved in uniquely cultural ways, and these cultural ways have produced an amalgam of variability in genetic, morphological, and cultural traits which require far more profound and extended forms of investigation than the simple-minded rubber-stamping approaches of the classifiers of 'races.'

Even though Montagu was quite explicit in his reasoning for rejecting the traditional biological approach and adapting a biocultural approach, many of his critics (see, for example, Count, 1951; Garn, 1961; Brace, 1964b) maintain that his reasons are sociological and not based in biology. To these claims, Montagu (1965:326) quite definitively responds:

> I was, of course, concerned, among other things, on humanitarian grounds about the social and biological consequences of the misuse of the idea of race. But what especially concerned me was the fuzziness with which the concept was used by physical anthropologists. The artificial groupings called races by most physical anthropologists seemed to me not to correspond to the realities which were claimed for them. It was on scientific grounds that I criticised the orthodox anthropological conception of race – not on humanitarian or sociological grounds. On similar grounds a growing number of physical anthropologists and biologists find the concept of race unsatisfactory.

While the biocultural concept of ethnicity was elaborated and encouraged by some of the most esteemed anthropologists of their particular times, Deniker, Huxley and Haddon, and Montagu, little progress has been made in understanding the benefits of such an approach. Montagu, being one of the most prolific anthropological writers of the 20th century in both biological and cultural anthropology, pushed the biocultural approach further than any other. Aldous Huxley (Foreword to Montagu, 1942a:vii) quite correctly states that most anthropological writers suffer from over-simplification, whereas Montagu insists on the principle of multiple and interlocking causation. Brace (Foreword to Montagu, 1997:16) suggests that while Montagu understood the deep issues on a theoretical level, the full range of basic biological data necessary to sustain his theories was never produced. In the absence of such data, as is often the case, simple explanations perpetuate in the absence of causal explanations of such complex factors. On the topic of race and subspecies, this remains exactly the case; biocultural approaches remain generally ignored or misrepresented with only a few rare exceptions.

Perhaps the best example of such an exception is a symposium of the Eugenics Society, held in London in 1968, entitled "Biosocial Aspects of Race" (Harrison and Peel, 1969). Of particular interest to the present discussion are the papers presented there by Hulse (1969) and the keynote address (The Galton Lecture) by Harrison (1969).

Hulse (1969:31), a student of Hooton's, quotes his teacher and mentor as frequently stating, "When people meet they sometimes fight, but they always mate." While Hooton remained faithful to the racial approach to human biology, this particular outlook is fundamental to the biocultural (or biosocial) approach, in that discrete or genetically closed 'populations' are rare, if they even exist, since miscegenation seems to be the general rule throughout human history. If such isolated groups do exist, they exist in specific local environments and are therefore not compatible with generalized racial categories. It is on this perspective that Hulse breaks from Hooton's insistence on phenotype as the basis for classification. Arguing from what appears to be a much more Boasian perspective, Hulse (1969:32) calls into question the sets of traits that have been used by his colleagues and predecessors for declaring racial groups:

> Traits which are glaringly cultural in origin and modes of transmission have been recklessly attributed to race. Traits which are demonstrably plastic, and subject to environmental modification, have been listed by reputable scholars as racial characteristics. To complicate matters still

> more, we often find that a feature – stature is a good example – may have a strong genetic component, yet be subject to considerable variation for environmental reasons.

Though Hulse does not mention the previous work of Montagu in his discussion, he nonetheless adopts a remarkably similar definition of ethnic group as a non-rigid or stable socio-cultural unit unified by sentiment and tradition.

The term 'population' that was suggested as a replacement for race by Martin was problematic for Hulse. Hulse (1969:33) believes that human populations are of a different type from general biological populations:

> A genetically distinct breeding population is an entity of a thoroughly different sort, since it may be characterized in biological terms. Castes and ethnic groups are found only in the human species, but breeding populations exist in most if not all bisexual animal species. The barriers between castes and ethnic groups are the result of human culture and human imagination. The barriers between breeding populations may be oceans, mountains, deserts, climatic zones as well ... In many cases, at least within the human species, social regulations may be effective in causing genetic distinctions to be retained, but it is far more doubtful that social regulations caused them to originate.

These comments seem reminiscent of those of Huxley and Haddon in that what might be considered a hypothetical primary race evolved particular physical characteristics in response to specific environmental stimuli, but the distribution and maintenance of such traits within and between groups across geography depends not only on geographical barriers in a larger sense, but in social barriers on a local level. Specifically in relation to physical environment, Hulse (1969:34) suggests,

> The genetic characteristics of a population have, as a rule, evolved in response to environmental stress. They presumably reflect adaptive requirements; and adaptation is to the ecology as a whole, not simply to the social aspects of ecology. Consequently we find that human breeding populations whose ancestors lived for thousands of years in different parts of the globe have evolved varied peculiarities.

Hulse therefore relies on an implicitly interconnected view of social and geographic environments.

Harrison (1969), on the other hand, chooses to focus more specifically on how physical anthropologists have approached the study of such trait variation in light of the biocultural approach, suggesting that the main focus of human morphological studies has been narrowly focused on form rather than function, neglecting the related biological processes and their explanatory value: "Most of the characters studied by early physical anthropologists, the visually obvious ones of morphology, are far removed from the immediate site of gene action, and typically, variation in these characters is, at least, partly due to the direct effect of the environmental variation on growth and development" (Harrison, 1969:129–130). Morphological variation and its taxonomic implication has been the primary domain of physical anthropologists trained by Hooton or his disciples, even though Hooton himself was interested in genetic inheritance and eugenics. Such a morphological perspective, however, follows the same issue that has formed much of the argument of this dissertation: morphological classification is based entirely on analogous relationships, and may give no indication of actual evolutionary relationship. The lack of concern for such problems results in the assumption that morphology is directly indicative of underlying genetic patterns. According to Harrison (1969:131), "Some physical anthropologists, like some geneticists, have tended to regard environmental effects, generally, as little more than a nuisance which conceal the nature of hereditary variation."

For Harrison (1969:132), the main problem in focusing on specifically genetic evidence to explore hereditary variation is that the genetic characters anthropologists have shown interest in, such as blood group systems, haemoglobin structure, serum proteins, blood enzymes, etc., are considered to be simply inherited, but in most cases they are genetically complex. Such polymorphic character systems differ in the frequency of genes rather than in absolute presences or absences, therefore distinguishing between monomorphic and polymorphic traits in population-level research is essential, since some populations may be monomorphic for a particular character that is polymorphic in other populations. It is, however, extremely rare that any single population would be monomorphic for one variation while other populations are monomorphic for an alternative variant in the same character system. Therefore, classifications based on such traits are particularly difficult, since the level of variation within each population is generally quite high.

Harrison deals with the geographical variation of traits by referring to clinal variation, a concept first introduced by Huxley (1938; 1939) explaining the gradual variation of the measured value of a character (phenotypic, genetic, behavioural, etc.) along a geographic axis.[3] According to Harrison (1969:132), changes in both gene frequency and quantitative traits (in terms of mean values) tend to occur gradually and evenly, though there may be some instances of relatively abrupt changes. Gene frequencies (genoclines) and quantitative traits (phenoclines) do not necessarily follow the same patterns; therefore, assumptions about changing gene frequencies based on geographical gradients in physical characters may be inaccurate at best. This leads Harrison (1969:140–141) to conclude:

> The clinal nature of the variation and the discordance of character distributions makes any classificatory system of geographical variety not

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[3] Brace (1964a, 1964b) also refers to the cline concept and Livingstone's (1962) insistence that human variation is clinal rather than racial.

> only arbitrary but also artificial. Livingstone (1962), indeed, has gone so far as to say that there are no races, only clines. This seems to be to me too extreme a position. As already stated, races are acceptably defined biologically as populations which differ in gene frequency and, since such populations undoubtedly exist, races must exist.

This position is confusing since Harrison claims that races exist but their classification is arbitrary and artificial.

Although he does not give credence to the idea of ethnic classification as Hulse did, Harrison (1969:141) does see racial classification in humans as being fundamentally different from other species:

> It may be concluded that the race concept is of little value in the scientific study of man; it has no exactitude and it, in itself, explains nothing. Even the use of racial names, like mongoloid, negroid, caucasoid, has no precision, though they serve as rough and ready summaries of a number of physical features which certain populations tend to possess. Nevertheless, in my personal view, it is both scientifically and ethically wrong for human biologists to deny the existence of race. The amount of geographical variation in genetic systems is great, despite the clinal nature of the variation fairly abrupt changes do occur, races palpably exist, and to my mind it helps nothing to call them ethnic groups. The important point is that the biological variety which I have discussed and which is the basis of the race concept, tells us nothing about the nature and variety of other human attributes.

Though Harrison dismisses ethnicity outright and problematizes the use of race while seemingly contradictorily upholding its value, it appears that his view of human variation is not all that dissimilar from those held by Deniker, Huxley and Haddon, Montagu, and Hulse, only his retention of traditional terminology differs. What is at odds with these writers is Harrison's perspective that human variation must fit into traditional biological (or natural historical) schemata.

The biological implications of ethnicity have been treated more recently by Crews and Bindon (1991), Corcos (1997), and Molnar (2002), but only in a cursory fashion.

Kitcher (2003), however, provides a more thorough treatment of the issues from a philosophical perspective, in which he claims that anthropologists who take an eliminativist approach to race base their reasoning on two main points: 1) phenotypic traits have no intrinsic value nor do they correlate with other characters with intrinsic value; 2) 'intraracial' diversity is far more pronounced than 'interracial' diversity in genotypic terms. Kitcher does not disagree with either of these points, but does not believe that they provide adequate support for racial eliminativism. Rather, he suggests that eliminativists have failed to recognize more subtle ways in which racial divisions might have biological significance (Kitcher, 2003:232). Focusing on interconnections between race, ethnicity, biology, and culture, Kitcher (2003:234) explores ways in which a concept of race might be developed which is compatible with current understandings of human variation. He also asks what the consequences of replacing the biological concept of race with the social concept of ethnicity might be.

Kitcher (2003:234) outlines what he believes are three necessary conditions of any concept of race:

> (R1) A racial division consists of a set of subsets of the species *Homo sapiens*. These subsets are the pure races. Individuals who do not belong to any pure race are of mixed race.
>
> (R2) With respect to any racial division, the pure races are closed under reproduction. That is, the offspring of parents both of whom are of race *R* are also of race *R*.
>
> (R3) With respect to any racial division, all ancestors of any member of a pure race belong to that race. The parents of an individual of race R are of race R.

The essential point that Kitcher (2003:236) wants to make is that "*The concept of race is a historical concept*" relying on an idea of a historical lineage that is more or less discrete due to a general pattern of inbreeding, or endogamous mating. Kitcher further explains that the notion of reproductive isolation is commonly misunderstood, since isolation need not be absolute, and hybrid zones in other species provide various examples of this, but that interbreeding can occur between two or more discrete populations at low levels. On this point, Kitcher (2003:238) asserts:

> If there is a workable biological conception of race, then it must, I believe, honour (R1)–(R3), employ the historical construction in terms of founder populations and inbred lineages, and finally, demand that, when the races are brought together, the differences in intraracial and interracial mating probabilities be sufficiently large to sustain the distinctive traits that mark the races (which must, presumably, lie, at least in part, in terms of phenotypes, since the organisms have no direct access to one's genes).

Thus, Kitcher's concept of race relies on the notion of at least approximately isolated breeding populations with sustained similarities in distinguishable physical traits. Drawing on American census data from 1970 and various research on mixed or interracial marriage, Kitcher attempts to demonstrate empirically that patterns of ancestry and descent similar to his notion of isolated breeding populations exist in humans, which sustain phenotypic differences developed in geographically separated founder populations.

Obvious problems with Kitcher's conclusions come directly from the data he uses to support his assumption that the low frequency of intermarriage between some American populations, particularly between African Americans and Caucasians, suggests that these populations are behaving as separate units from an evolutionary (and perhaps ecological) perspective. With ever-increasing globalisation, relying on data that are over thirty-five years old is of limited relevance at best. In order to be convincing, a sustained historical trend toward 'intraracial' mating must be demonstrated.

Canadian data from the 2001 Census show several interesting trends in terms of 'mixed unions' (Milan and Hamm, 2004), which contradict Kitcher's intraracial mating theory, at least in the Canadian context. Mixed unions represented 3.2% of all persons in couples (marriages and common-law unions) in Canada in 2001, compared to 2.0% in the United States in 2000. These mixed unions are comprised of one visible minority and one non-visible minority or two different visible minority group members.[4] While this may seem a low percentage of overall unions, it is a 35% increase from the 1991 Census. The most common type of mixed unions were between a visible minority and a non-visible minority (i.e., Caucasian), accounting for 2.8% of all couples in 2001, increasing from 2.4% in 1991. While no specific data for Black-Caucasian mixed unions are given, 57% of Blacks (African Americans) partnered with other Blacks, with the remaining 43% in mixed unions. In contrast to Kitcher's findings, Blacks had the largest number of mixed unions of all couples in Canada in 2001 in terms of absolute numbers. The 2001 Census data also demonstrates that mixed union rates differ not by racial group, but by individual ethnic groups (Table 3.2), and are affected by age, location, education, and place of birth (Table 3.4).[5]

Wilson (2004) is also critical of Kitcher's use of these data and his assumptions regarding reproductive isolation, since patterns of racial self-identification and classification in census data collections are constantly shifting and tend to blend together

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[4] Visible minorities are defined by the Canadian *Employment Equity Act* as "persons, other than Aboriginal peoples, who are non-Caucasian in race or non-white in colour" (Milan and Hamm, 2004:2).

[5] The 2001 Census data also shows: (a) Japanese have the highest proportion of mixed couples but are the smallest minority group, whereas Chinese and South Asians form the largest minority groups but have the lowest proportion of mixed couples; (b) young urban dwellers are most commonly in mixed unions; (c) persons in mixed unions are likely to have higher education and be foreign-born; (d) mixed unions are more frequent for common-law unions than marriages (Milan and Hamm, 2004).

Table 3.2 Proportion of mixed couples from 2001 Canadian census (from Milan and Hamm, 2004:3).

Selected visible minority groups	Total couples	Partners within the same visible minority group	Mixed Unions
	Number	% of couples	
Japanese	25,100	30	70
Latin American	57,800	55	45
Black	117,800	57	43
Filipino	78,700	67	33
Southeast Asian	45,200	74	26
Arab/West Asian	73,800	76	24
Korean	24,800	82	18
Chinese	26,500	84	16
South Asian	232,000	87	13

Table 3.3 Factors affecting mixed unions from 2001 Canadian census (from Milan and Hamm, 2004:5)

		Mixed unions	
	Total	Two different visible minorities	One visible minority and one non-visible minority
		% of people in unions who are in mixed couples	
Age			
15 and over	3.2	0.4	2.8
15 to 19	5.4	0.7	4.6
20 to 29	5.3	0.6	4.6
30 to 44	4.3	0.6	3.8
45 to 64	2.5	0.3	2.2
65 and over	1.0	0.1	0.9
Education			
Less than high school	1.5	0.2	1.3
High school	2.3	0.3	2.0
Some postsecondary	3.6	0.5	3.1
University degree	5.6	0.7	5.0
Place of birth			
Canadian-born	2.1	0.1	2.0
Foreign-born	6.7	1.5	5.2

historical, biological, cultural, and ethnic categories. Therefore, the approximate patterns of isolation that Kitcher claims are also shifting, thus providing no 'real' example of the phenomenon that he wants to prove. Still, Kitcher (2003:247) concludes that these data demonstrate that:

> [W]e might say that races are *both* socially constructed and biologically real. Biological reality intrudes in the objective facts of patterns of reproduction, specifically in the greater propensity for mating with other 'blacks' (or other 'whites' respectively); the social construction lies in the fact that these propensities themselves have complex social causes.

The recognition that a concept can be both socially constructed and biologically real should come as no surprise, since this idea was quite eloquently explained a few years earlier by Hacking in *The Social Construction of What?* (1999). Kitcher claims that this social and biological interaction results in a significant difference in the probabilities of interracial and intraracial mating, maintaining distinguishable phenotypic and genetic characteristics present in the founding populations. Therefore, he believes that such groups are, in a microevolutionary sense, separate evolutionary units, and classifiable as races, though perhaps not in the traditional sense of human racial categories.

The mechanisms of Kitcher's (2003:247) anti-eliminativist argument apparently work on two levels:

> While the concept of human races may have biological significance, in the sense that there are differences in gene frequencies which can be preserved because of low probabilities of interracial mating, the explanation of the mating preferences may have no biological significance. Race may *quite literally* be socially constructed, in that our patterns of acculturation maintain the genetic distinctiveness of different racial groups.

This social construction aspect of what have been considered 'racial' groups is exactly what was at the root of the notion of ethnicity promoted by Deniker, Huxley and Haddon, and especially Montagu. For Kitcher (2003:248), however, he views such variation on the ethnic level as paralleling that of the biological:

> (E1) An ethnic division consists in a division of *Homo sapiens* into non-overlapping subsets. These subsets are the pure ethnicities. Individuals who do not belong to any of the subsets are of mixed ethnicity.
>
> (E2) Pure ethnicities are closed under cultural transmission. That is, the cultural 'offspring' of 'parents' all of whom are of ethnicity *E* are of ethnicity *E*.
>
> (E3) All cultural 'ancestors' of any member of any pure ethnicity are of that ethnicity. If someone is of ethnicity E, then all their cultural 'parents' are of ethnicity E.

This parallel is somewhat dubious, since Kitcher's emphasis on purity in both the biological and social levels of his theory seems unrealistic, even in light of his relative approach to genetic and cultural exchange as approximately isolated.

Kitcher's (2003:249) belief in an implicit relationship between biology and culture leads him to suggest that there is a process of harmonization and reinforcement constantly at work:

> On the biological level, interracial mating is limited through the differences in the cultural items acquired by members of different races. On the cultural level, patterns of culture are preserved because culture is usually primarily transmitted by parents and other family members (who may also influence the receptivity to other potential cultural parents), who belong to the same race and share the same ethnicity. One particular consequence ... is that past racism shapes the attitudes of people today, in particular their attitudes to sexual union, and that this can maintain patterns of mating that are skewed toward one's own group.

It seems that Kitcher should be pointing to something more fundamental than social structure and the possible outcomes of racist thought, though he does open the door to the perspective that humans may have a 'hardwired' predisposition toward exclusionary breeding. What is perhaps the greatest problem in his approach is that he draws an implicit connection between biology and culture, and sees a direct relationship between race and ethnicity, but fails to recognize the possibility that race and ethnicity may actually be one and the same.

The racial groupings Kitcher refers to could also be sub-racial groups, kin-groups, varieties, or any other sub-specific grouping in terms of the formal Linnaean taxonomic hierarchy or any system of social or biological folk taxonomy (Wilson, 2004:9). If ethnicity is a fluid social term it cannot adequately parallel or harmonize with a static biological conception of race. Rather, if this relationship exists, ethnicity would necessarily be the causal factor (in the absence of geographic-environmental barriers) and the 'racial' group would be the result, but since it is highly unlikely that such a group would be discrete, the notion of racial categorization fails to explain any aspect of human variation. If racial divisions have social causes, then they are not races in a biological sense. Whether this is a condition that exists only in human populations is enough to dismiss the notion of race at least in reference to modern *Homo sapiens*.

Kitcher's reference to the 'hardwiring' of a preference for inbreeding leads to more interesting questions about social and biological structures, though it also leads us into a more dangerous essentialist territory. It would, however, be fruitless to pursue a non-racial method for explaining human variation if it could be demonstrated that – regardless of whether biological races can be shown to exist or not – the human mind will inevitably categorize the world based on the assumption that groups are discrete social or biological entities. Races would then be real, at least in a cognitive sense. Gil-White (2001) has made claims about the cognitive role of human ethnic categories that go far beyond the cultural understandings of general 'natural kind' categories that have been espoused by Atran (1990) and Hirschfeld (1996; 1998).

Neither Atran nor Hirschfeld sees race as an innate concept, but rather a result of an interaction between culture and cognition. Atran (1990:49) believes that some cognitive processes such as colour classification or 'living-kind' categories are only marginally affected by social change, while other forms of cultural knowledge, totemism or molecular biology, for example, depend on specific cultural institutions for their existence and transmission. Humans, therefore, commonly classify living kinds according to type, and apparent morphological distinctions between human groups *may* be conceived as natural biological divisions, from which social hierarchies develop (Atran, 1990:78). Hirschfeld (1996:13) believes that humans generally conceptualize the human world as composed of distinct types – what he calls 'human kinds,' which are predicated on the attribution of common inherent or intrinsic features. Hirschfeld (1998) demonstrates that although human biological variation is poorly captured by existing systems of racial classification, systems of 'natural' human taxonomy exist in virtually every culture, and even very young children preferentially sort people by race and gender. However, he also explains that children do not see race as being constant, and that the development of deep-seated natural human kind preference is based on a manipulation of natural categorization by the political economy.

Gil-White (2001) pushes the cognitive argument further into essentialism, arguing that humans intellectually process ethnic groups as species, resulting from an evolutionary adaptation that discriminates in favour of in-group relations and endogamy. If such a perspective can be shown to have any significant degree of explanatory value from within the psychological and philosophical disciplines, this may present an important focal point from which we can discuss the biology of ethnicity. If, as Gil-White (2001:519) explains, "it is true that we naively and intuitively process ethnies[6] [ethnic groups] as species, this is likely to improve our understanding of the behaviour of ethnic actors in different contexts." Indeed, if we can understand the ways in which the human mind views others in terms of relationships and phenotypes, which will in turn explain the structure of local communities, then we can positively assert that the concept of ethnicity is biologically meaningful and represents a viable vehicle for shifting the study of human variation away from the dominant racial epistemology.

Utilising Hirschfeld's notion of 'human kinds,' Gil-White (2003:518) proposes that the favouring of like ethnies is an essential cognitive process with adaptive evolutionary significance: "Keeping track of these 'kinds' is important, for attempted interactions with aliens with different standards of performance will more likely lead to failed than to mutually profitable interactions." Gil-White (2003:514) also suggests that the identification and order of natural human kinds results in an attachment of putative essences. With these putative essences comes the mistaking of race and ethnic group: "we essentialize races because we mistakenly 'think' they are ethnic groups. We thus process 'races' as ethnies even though not by the longest stretch of the imagination can they be characterized as representing norm or behavioural boundaries of any kind, which is the original reason for exapting the living-kinds module" (Gil-White, 2003:534). As interesting as these arguments might be, they suffer on numerous levels, and do not make

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[6] According to Smith (1986:32), an 'ethnie' is a "named human population with shared ancestry myths, histories and cultures, having associations with a specific territory, and a sense of solidarity." Gil-White (2001:515) prefers the term 'ethnie' to 'ethnic group' because he feels that ethnic groups are generally misleading categories that do not adequately reflect groups of people. 'Ethnic group' remains the preferred terminology in this dissertation, since the general focus in on biological groups or 'populations.'

any more significant contribution to the understanding of human biology or psychology than do the works of Atran and Hirschfeld, upon which Gil-White's argument is based. His approach is more properly sociobiological than biocultural.

First, the entire premise of the argument is based upon ethnobiological studies indicating a significant overlap between traditional and Western scientific models of bio-classification. There is also an underlying assumption that a species is an essential unit in biology, since many cultural folk taxonomies closely mirror those of systematic taxonomies, and the biological species concept is based on the notion that species are naturally discrete breeding populations. That species do in fact represent discrete breeding populations is assumed, but has not been conclusively demonstrated (see Chapter 2). Second, if 'races' and 'ethnies' are confused by the mind, and ethnies are understood essentially as species, then logically should races also not be processed as species? Gil-White's conception of the difference between race and ethnic group is unclear. But we need not talk about ethnic groups in this case, because races would be real biological entities created by the mind and produced by self-domesticative breeding. Third, the assumption that such a hypothetical cognitive process has adaptive evolutionary significance is presumably false, since exogamy is genetically preferable to endogamy, introducing new and favourable biological elements.

If Gil-White's prepositions were correct, there would be self-evident biological races, not ethnic groups, since they would be discrete and discriminating breeding populations. As we know, human mobility and curiosity has resulted in vast degrees of gene flow, which have destroyed any sense of racial 'purity' that *may* have existed during early human evolution. This perspective is also held by multiregional evolutionists who believe that *Homo erectus* populations evolved independently into *Homo sapiens* throughout Africa, Asia, and Europe, with a degree of gene flow between all populations (see Wolpoff and Caspari, 1997; 2000). It is perhaps more appropriate then to utilise Hirschfeld's work on the cognition of categorization which sees classification as an essential process of the mind, yet flexible and subject to political economic forces. These categories seem to represent something very close to what Deniker, Huxley and Haddon, and Montagu called ethnic groups, which are flexible and historically contingent, but not without real biological meaning.

This chapter has provided a mainly historiographical look at the arguments for the concept of ethnicity not simply as a social category, but one with real biological meaning. In doing so, it has been necessary to take a significant look at the underappreciated literature signalling a call for a biocultural approach to studying human variation and away from simple taxonomies. Shifting to contemporary scholarship on ethnicity, philosophical and psychological literature dealing with the essential nature of specific and subspecific taxonomy has also been critically evaluated. This chapter represents a call for a functional rather than a structural approach to studying human biology, which must fundamentally be biocultural. Patterns are of limited relevance without discussion of the processes that created them. Humans create a unique problem in biology, since culture is an adaptive evolutionary strategy (as opposed to Gil-White's claim that ethnic classification is an adaptive strategy; see Richerson and Boyd, 2005). Harrison (1969) argues that invalidating the race concept in humans for social/cultural reasons does not necessarily invalidate the biological race concept. This is indeed true, but it does not mean that the concept can or should not be invalidated in its application specifically to humans, and quite obviously, if it can be invalidated in relation to other species, it will naturally be considered invalid in the human context. It should already be evident that the race and subspecies concepts are not only problematic in general biological usage, but especially ineffective for exploring human variation.

We have also seen in this chapter that the term 'population' has become a popular alternative to racial terminology. A population in a Mendelian sense is defined by both the rate of isolation (endogamy) and gene flow (exogamy). Various gene frequencies exist in each gene pool (population), depending on the degree of isolation. Similar to Kitcher's definition, Molnar (2002:55, 253) explains the gene pool as a group of interbreeding individual organisms forming what might specifically be called *breeding populations* or *demes*, which constitutes a series of mating circles relatively isolated from other such groups. Of course, this isolation can be created by various types of barriers (i.e., physical, cultural, social, linguistic, etc.) and can generally be understood as an interaction between multiple biological and behavioural forces. It should again be emphasized that this sort of definition has a distinct similarity to the concept of ethnicity that has been detailed throughout this chapter. Ethnic groups are defined by a historically specific limitation on gene flow, or a *general* preference for endogamy. This preference is likely socially conditioned or ecological, rather than cognitively essential. However, where the breeding population assumes a high degree of isolation, ethnic groups can be open systems.

While humans do not exist outside of the biological world and forces of evolution, technological control of fertility, health, and mortality make it arguable that modern genetic structures throughout much of the world are affected more by patterns of mating and mobility than by natural selection (Macbeth, 1993:49; 81). Human variation must, then, be biocultural. Some believe that ‘population’ and ‘ethnicity’ take on similar meanings when applied biologically (see Crews and Bindon, 1991:45), but ‘population’ simply acts as a placeholder for ‘race’ (see Caspari, 2003:73). It does not represent a conceptual shift but a terminological one. Attached to the ‘population’ is the hierarchical aspect of race and subspecies, and the problem for ethnic biology is that it represents various levels of organization, and is not especially conducive to taxonomic treatment. Montagu’s attempts to advance the biocultural approach were certainly limited by his inability to provide data demonstrating how various types of biological analyses could be of taxonomic implication. It should be recognized, however, that the appropriate collection of data must be conducted on solid theoretical foundation before it can be of any analytical value. This dissertation represents a prolegomenon to such biological work. The following chapter focuses on the methodological aspects of such a project.

Chapter 4

MODERN METHODOLOGY IN PHYSICAL ANTHROPOLOGY

The problem of evidence has been an ongoing concern throughout this dissertation, since explanatory frameworks have relied more on synthetic arguments than on methodological applications (see Chapter 1; Cartwright, 1999:17). In the absence of a new conceptual model of human biological variation, arguments over the objective existence of races or the abandonment of the concept remain almost exclusively in the theoretical realm. The result has been a conundrum where researchers are looking for new ways to analyse the patterns and processes of (micro)-evolution and speciation but are constrained by outdated methods and models. Various strategies for dealing with evolutionary and phylogenetic problems in evolutionary biology and genetics have been slow to permeate into the discipline of anthropology. This short chapter explores ways in which methodologies might be applied to the study of human biological variation in accordance with critical perspectives on racial and subspecies designations, in order to test general assumptions about human migration and phylogeny. Though Brace et al. (1993:3) take the perspective that the past failures of physical anthropology are the result of flaws in theoretical expectations rather than methodological inadequacies, *methodology* is used hereafter with a broader definition, encapsulating the epistemological approach to the analysis of human biological data rather than simply the ways in which data are collected.

The inability of anthropologists to move beyond the racial paradigm can be argued from numerous perspectives. The perspective taken in this dissertation is, simply stated, that: 1) the discipline of anthropology has traditionally been plagued with misguided neo-evolutionary thought that held the concept of race at its very core; 2) despite numerous arguments in favour of the abandonment of the race concept altogether as applied to humans, the 'subspecies problem' in the evolutionary sciences in general has largely been overlooked by anthropologists; 3) biocultural approaches to studying human variation have lost out to more simple analogous methods. Associated with these issues is that of the disjunction between local-level or regional studies that are typical of physical anthropologists and the large-scale (between-species) phylogenetic reconstructions of paleoanthropologists.

Studies of human phylogeny and variation are not necessarily one and the same. Phylogenetic analyses involve the study of evolutionary relationships, which must necessarily be a study of historical lineages through morphological evidence, whereas human variation can be studied in an ahistorical fashion (i.e., a single point in time). Human variation studies typically involve comparative analyses of small regional populations, and rely primarily on genetic and biometric (physiological) data. Such studies of the genetic structure of small populations have made great contributions to regional history and prehistory, but have done little to advance our understanding of evolution in a global sense (Harpending, 1974:229). Fundamental to the problem of connecting past to present in the study of historical lineages is the availability of evidence for study.

As introduced in Chapter 1, the historic split between social and physical anthropology was based on the availability of evidence and the methods of analysis necessary to collect relevant biological information. While anthropometric studies of living populations are relatively simple to conduct when there are willing participants, information about past populations can only be garnered from the scarce information left behind, primarily skeletal evidence. The quantification of morphological traits became easier through skeletal analysis and human osteology subsequently became the foundation of physical anthropology. Unfortunately, however, while the collection and compilation of data became easier, many physical anthropologists utilized anthropometric and descriptive data from living populations with osteometric data from past populations, assuming that they represented the same patterns of genetic inheritance.

Though progressive in theory, the biocultural studies of Deniker (1904) and Huxley and Haddon (1935) both suffered from the reliance on osteological and biometric or 'somatological' data, which were used interchangeably. This indiscriminate mixing of traits has been replicated in innumerable anthropological studies throughout the 20th century. Fundamental to the study of historical lineages, particularly when comparing past to present species or local 'ethnic' groups, are the types of data that are being collected. Osteological analyses, based on the quantification of various morphological features of bone, must necessarily fit into a morphological (phenetic) species concept, as is common in paleontological research. In terms of bone morphology, the cranium is the richest source of skeletal information about human variation (Brues, 1992:126)

Farber (1976) provides some interesting discussion on the use of morphological evidence in the study of natural history in the 19th and early 20th century, looking especially at the notion of the 'type specimen.' For anthropologists, the type specimen concept is applied to paleoanthropological and archaeological finds, where the type specimen represents a newly discovered *type* of early primate or human in the case of paleoanthropology or artefact in the case of archaeology. The problem with such type specimens is that there is no way of being sure that they are typical of the species or culture: there is no way of telling how they fit into the range of variation. When looking only at morphological evidence, in such paleontological cases where no genetic information can be extracted from a specimen, there has been a general emphasis on the primacy of form over function (Farber, 1976:109), particularly in distinguishing between hominid species. The notion of the type specimen, whether implicit or explicit, embodies a fixed pattern of organization that can be demonstrated as an archetypal representation. It would undoubtedly be an impossible task to find any single human that would serve as the archetype for any population, whether based on external physical features or bone morphology.

The biological species concept (BSC) does not easily fit anthropological study because bone morphology cannot directly demonstrate genetic principles of variation, since morphology is greatly shaped by environmental factors. Mayr's (1982:273) claim that species are discrete populations that are distinguishable by the ability of individuals within a population to reproduce only with other individuals of the same population would be an objective fact of nature if there were no exceptions to this rule (i.e., 'hybrid zones'). However, exceptions aside, the BSC does seem to have a *general* application for distinguishing species between extant populations. However, skeletal data are often difficult to interpret at the species or subspecies level. Methodologically, Mayr's view of subspecies is that subspecific taxa can be declared according to the so-called 75-percent rule: 75 percent of individuals of a given population must differ physically or genetically from all of the individuals of another previously recognized subspecies (Mayr, 1969:190).

Applying the 75-percent rule will have different outcomes if morphological evidence is used rather than genetic evidence, and patterns of biological variation are not always intuitive. A fine example of this was given in Chapter 2, where Howells' study of global human diversity by craniometric analysis shows that indigenous Australians cluster closely together with sub-Saharan Africans, while Cavalli-Sforza and colleagues show that indigenous Australians and sub-Saharan Africans exist on the extremities of a continuum of human genetic variation (Cavalli-Sforza and Cavalli-Sforza, 1995:117-119; see also Billinger, 2000, Chapter 5). It seems almost inconceivable that any one human population could differ morphologically or genetically from any other by 75 percent.

Howells' (1973, 1989, 1995) studies of craniometric variation among globally distributed populations show that no human populations are extreme outliers based on morphology. Rather, they exist in regional clusters closely aligned with local ethnic groups, while fitting into a general pattern of clinal variation. As also discussed in Chapter 2, genetic evidence shows that so-called racial variation accounts for anywhere between 2.8 and 22.0 percent of overall human variation, depending on the data type (blood group, protein, or enzyme) and statistics used (Brown and Armelagos, 2001). It is therefore unlikely that human genetic variation would support Mayr's subspecific criteria either. These percentages show an even less remarkable picture of between-population variation when we understand that since DNA's double-helical strands are comprised of nitrogen base pairs (nucleotides or subunits) of four molecules – adenine and thymine, cytosine and guanine – the zero mark of a DNA comparison between any biological organisms is not zero percent similarity, but 25% similarity (Marks, 2000, 2002b, 2003; Griffiths et al., 2000).

Another example of the unexpectedly incongruent physical and genetic evidence can be shown at the species level in other mammals such as bears (Family Ursidae). Yu et al. (2004) analysed both nuclear and mitochondrial DNA in order to explore phylogenetic patterns within Ursidae and found that polar bears (*Ursus maritimus*) are much more closely related genetically to the brown bear (*Ursus arctos*) than either is related to the American black bear (*Ursus americanus*) or the Asiatic black bear (*Ursus thibetanus*). Obviously, when it comes to implying phylogeny from phenotypic evidence, whether somatological or morphological, looks can be deceiving when compared to genetic evidence.

The main problem of racial classification or subspecific taxonomy in anthropology is its perceived usefulness despite the widespread knowledge of its biological inadequacy. Forensic anthropology has served as the main site of debate over the use of racial categories. Many of the arguments surrounding the use of racial classification by forensic anthropologists are relatively simple. Numerous published papers by forensic anthropologists demonstrate that the majority do not necessarily believe that racial categories are valid taxonomic units, but that they are of value for identifying skeletonized individuals when the classification systems are "geographically relevant and locally understood by law enforcement officials" (Gordon, 1993:4; see also Gill & Rhine, 1990; Sauer, 1992, 1993; Hinkes, 1993; Rhine, 1993; Church 1995).

The main issue then for forensic anthropologists is whether their use of racial classification validates historic hierarchical notions of race. Many argue that it does not. Rather, as Sauer (1992:110) explains, forensic anthropologists provide "a prediction, based upon skeletal morphology, that a particular [social] label would have been assigned to an individual when that individual was alive." Brace (1995:172) makes the important point that forensic racial categories correspond primarily to skin colour, upon which social race labels have developed, but skeletal morphology has little or no connection to skin pigmentation in process or pattern. According to Ousley and Jantz (1996; see also Williams et al., 2005), social race is an approximation of biological affinity, which, regardless of the individual's actual ancestry, represents the most practical means for identification.

Methodologically, forensic ancestral determination has proven to be highly problematic. An unfortunate result of a narrow approach to variation that is typical of forensic anthropological research has been a primary focus on historically defined geographic populations rather than on actual patterns of human evolution (Kennedy, 1995). Standard formulae for determining race in North America have been adopted from Giles and Elliot (1962). Based on discriminant functions, two formulae are used to distinguish either between American 'Blacks' and 'Whites,' or 'Whites' and American Indians. The population samples of Blacks and Whites (defined according to skin colour) came from modern anatomical specimens, while the American Indian sample is an archaic (5500-year-old) sample from the Indian Knoll population from Kentucky. Brues (1992:127; see also Armelagos and Van Gerven, 2003) notes several problems in using samples of specific regional populations as a source for generalized data: "If we wish to get the greatest efficiency in forensic identification, we should stop trying to do it in a way in which the very real local variation becomes more static to confuse the results, and try to do it in a way in which local variation makes a positive contribution to correct diagnosis." It is particularly puzzling that an archaic sample would be used as an archetype for contemporary Amerindian populations.

Populations are regional and do not fit well into generalized models (Relethford, 2002, 2001), and this regionalization can be well demonstrated using data collected from Amerindian populations (see Cybulski, 2001, 1992, 1975). Billinger (2000) also argues that for forensic purposes, osteobiographical information provides a better indicator of identity then numerous popular methods of racial determination found in the forensic literature. In contrast to race determination, osteobiography refers to indicators of an individual's personal history, such as healed injuries, dental work, occupational stress markers, pathologies, etc. This is what Saul (1976) calls "life history recorded in bone." For forensic purposes, context and indicators of individual identity rather than a hypothetical affinity to archetypal racial groups are key features for positive and accurate identification. Figure 4.1 lists some of the questions and potential answers based on contextual osteobiographical information.

Traditional methods of forensic racial determination have used complex statistics to reinforce outdated notions of human biological variation, at least as it is manifested in the skeleton. Attempting to move away from such a narrow perspective, such as archetypal data, Kennedy and Chiment (1992) ask some fundamental questions aimed at building a better understanding of variation: (1) how do we establish that comparative analysis of morphometric (size and shape) data can determine degrees of biological distance and provide evidence for continua over time? (2) Can we distinguish trait distributions indicative of close genetic association from those which are the evolutionary products of similar selective factors among unrelated populations? The authors stress the

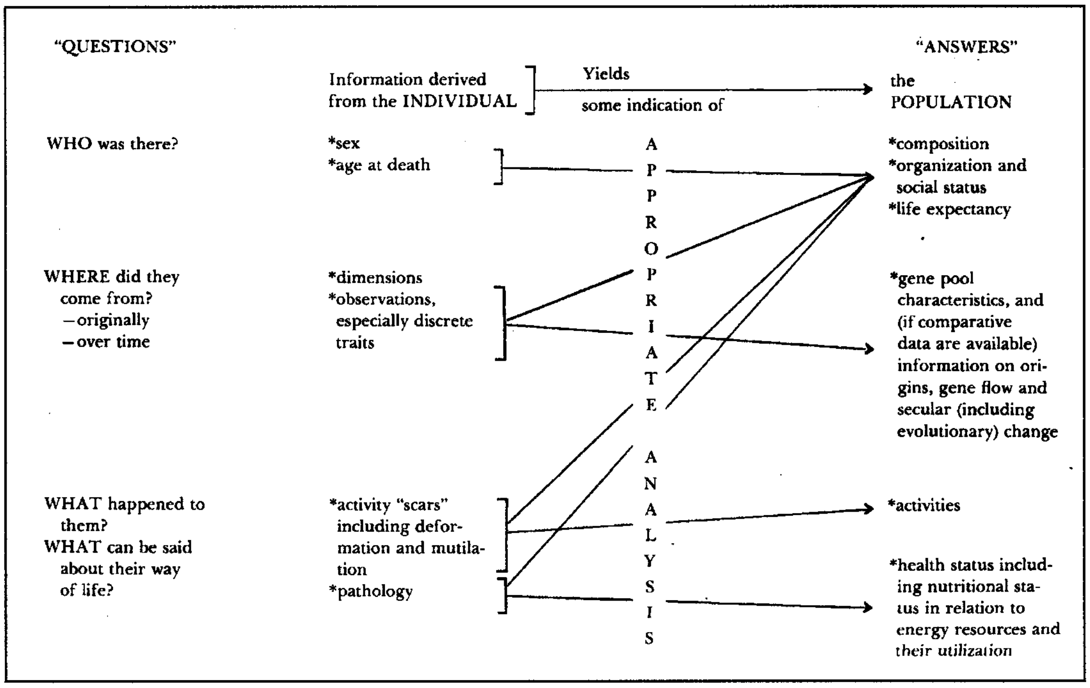

Figure 4.1 Potential applications of osteobiographic analysis. From Saul (1976:374).

need for exercising caution in attributing similarities of morphometric traits over time and space to biological affinity, outlining the fact that population distances based on phenetic clustering of morphometric skeletal and dental data seldom reproduce those patterns found from molecular genetic data.

Traits considered to be distinctive characters of an ancestral linage may be the result of parallel evolution in separate populations and not hallmarks of biological affinity, leading Kennedy and Chiment (1992:122) to conclude, somewhat pessimistically:

> [F]orensic anthropologists are practicing race identification in a scientific environment that is suspicious of racist and typological overtones but is tolerant of biological affinity studies based upon genetic, molecular, and biochemical methodologies which replace anatomical and morphometric methods. A synthesis of these approaches will emerge one day, but for the present forensic anthropologists remain dependent upon morphometric

> analysis in determining individual racial identity and in discerning the biological affinities of archaeological skeletal specimens.

Where Kennedy and Chiment mention morphometric methods, they are referring to the study of size and shape through multivariate statistical analysis rather than on traditional cranial indices (length/width or height/breadth ratios). However, morphometric analysis beyond cranial indices in the forensic context often consists of nothing more than the 'eyeball' method: identification based on general appearance of a skull and classification according to shape categories (Howells, 1973:1, 1995:1).[1] In contrast, multivariate morphometric relationships using data from large batteries of traits have been explored by physical anthropologists in the study of large-scale population variation.

Even in cases where forensic determinations rely on multivariate morphometric data, often the assumptions underlying their application are fundamentally flawed. Williams et al. (2005) tested the popular © Fordisc 2.0 software package used for forensic sex and racial determination against an ancient Nubian sample from Egypt. The results from the Nubian sample were erratic: while 10 of the 42 crania used in the study were ascribed to the Late Period Dynastic Egypt category using Howells' database as a comparative sample,[2] the remaining crania were ascribed to black, white, Hispanic, Chinese, Japanese, or Native American. Not only are there problems with the terminology of these general categories (i.e., the very general categories of 'black' and 'white' as opposed to the national or ethnic labels used to identify other groups), identification of individuals is unlikely when they have no affinity to the samples used for comparison. For example, in the case of the Williams et al. study, even the Late Period Dynastic Egypt sample used for comparison in the Howells database was significantly

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[1] A critical evaluation of such visual methodology can be found in Billinger (2000, Chapter 4).

[2] Howells' worldwide craniometric database has been incorporated into Fordisc 2.0.

dissimilar from the Nubian sample. Williams et al. (2005:344) provide a valuable explanation of the problems inherent in such forensic methods:

> The idea that head type is stable through time and thus resistant to environmental factors was very popular among natural scientists from the seventeenth to the nineteenth century. Mid-twentieth-century physical anthropology sought to trace independent human lineages by associating craniofacial remains from past populations with their supposed modern counterparts (Boule and Vallois, 1957; Coon, 1962). During the same period, several studies demonstrated plasticity of body type through such processes as developmental acclimatization (Baker, 1969; Frisancho, 1970) and migration to new areas (Shapiro, 1939; Bogin, 1988). This may help to explain why European Upper Palaeolithic populations do not resemble modern Europeans (van Vark, 1994), why Kennewick man does not resemble modern Native Americans (van Vark et al., 2003; contra Jantz and Owsley, 2003), why American-born children of immigrants resemble their parents less than European-born children of immigrants do (Boas, 1912; contra Sparks and Jantz, 2002), and why Mesolithic Nubians do not resemble Meriotic Nubians (Van Gerven et al., 1979). This reflects simply what is known about the patterns of variation in the human cranium: it is to some extent an inborn characteristic and also developmentally very sensitive to the conditions of growth.

The authors reject altogether the notion of classification by racial or geographic affinity by multivariate statistical means.

W.W. Howells and C.L. Brace have been most active in pushing forward multivariate morphometrics in physical anthropology in order to analyse the patterns of variation rather than relying on outdated models of racial classification. Howells (1973) was interested in taxonomy, but saw it not as a means to an end, but rather as a tool for comparative description: as an exploration of genetic populations rather than types. In his first volume of three based on his collection of a worldwide craniometric data based on 57 different measurements, which he subjected to multiple discriminants and factor analysis, his goal was to:

> [G]enerate new information from the skull by applying multivariate statistics to population comparisons, partly simply to demonstrate that such

> information exists. By doing so, it hopes that objective methods of population analysis, with any material and not cranial alone, can be expanded, to replace some of the area of subjective judgements prevailing in the past (Howells, 1973:2).

In order to accomplish this, Howells (1973:3) called for a more complex method of analysis of craniometric variation:

> Populations have been compared (normally two populations at a time) in one measurement at a time. Such estimates of difference, or distance, do not, however, allow consideration of the shape of the skull as a whole, as this is expressed by the relations *between* measurements (though of course two measurements have been commonly related in a ratio or index). As a result, there had been little consideration as to whether the measurements in use do in fact reflect the total configuration of the skull adequately for taxonomic purposes (although traditional lists naturally have attempted to cover many aspects of the skull and face).

For Howells (1973:3), the move towards more statistically complex methods of analysis represented a new direction for the underlying theories of human variation as well:

> Present methods of multivariate analysis ... allow a skull to be treated as a unit, i.e., as a configuration of the information contained in all its measurements ... That is the importance of multivariate statistics: they fit the model populations looked on not as centroids or means, but as swarms of the varying individuals who compose them; and the differentiation of these swarms from one another constitutes a statement of the degree and nature of the difference between populations.

In Howells' (1989:1) second volume, he was particularly interested in the variation of both the size and shape of the skull, which he saw as the key to moving beyond outdated racial analyses. In treating the data as a single morphometric unit for multivariate analysis, the raw data for each craniometric measure are transformed into a single correlation coefficient for each individual, which is then expressed in terms of a population average.

In order to achieve this, individual data are first converted from their raw form into a standardized format (Z-scores). This is achieved by subtracting the raw value for each (craniometric) trait from the calculated (local) group mean, and dividing the resulting sum by the calculated standard deviation (of all individuals in that same group) for the same trait. The Z-score is a common data treatment in statistical analyses (Kaufman and Rousseeuw, 1990:9; Jackson, 1995:199), representing (in this example) a transformed value in which the individual's measured values for each trait is expressed in terms of the overall variation of that same trait within the population. Howells (1989:9) wanted to enhance the comparability of both size and shape, which he suggests can be done by double-centering the measurements, resulting in what he calls the C-score.

In order to calculate C-scores, the average Z-score (of all variables) for each individual must be calculated. The result is what Howells (1989:9) calls the PENSIZE, based on the formulae of the statistician Penrose (1954). C-scores are then calculated by subtracting the PENSIZE of each individual from the Z-score for each measurement of that same individual. C-scores attempt to statistically separate size and shape by transforming each variable into a unit indexed relative to the total configuration of the skull. Mean C-scores for each group can then be compared directly or subjected to group-by-group comparison and Euclidean distance cluster analysis in order to determine biological distance between groups.

In his third volume, Howells (1995:1) specifically dealt with the problem of assessing population affinity with individual specimens, which he referred to as "ethnic identification." On cranial form, region, and racial classification, Howells (1995:103) concluded that only limited success can be achieved in distinguishing regions craniometrically, and that individuals assign themselves to specific populations better than to 'races' or regional samples. Thus, as demonstrated by Williams et al. (2005) in the forensic context, only erratic results are obtained when individuals are placed in a general sample for which there is no matching reference population.

Following Howells' example, C-scores have similarly been used by Brace and Hunt (1990) for 57 samples from Asia, the Pacific, the aboriginal Western Hemisphere, and Europe, as well as by Brace et al. (1993) on an ancient Egyptian sample. The results of both these studies demonstrate that variation in craniofacial measurements when compared to other world populations follows a clinal pattern generally punctuated by intermittent clusters, similar to those patterns shown by Howells. In interpreting these results, Brace et al. (1993:26) suggest, "Where human traits have adaptive significance, their distributions are determined by the distribution of the controlling selective forces and 'there are no races, there are only clines.' Where traits have no adaptive significance, neighbors will share traits with neighbors and the analysis of adjacent samples will show that they cluster together."

Following the principle of numerical taxonomy, Brace (2005:15–16) therefore advocates using a large battery of craniofacial traits[3] without regard for their adaptive significance in order to obtain the best possible picture (which, he argues, accords quite well with results obtained from DNA analysis) of what he calls "family resemblance writ large." Since inherited biological aspects may have little or no adaptive value, they are controlled exclusively by relatedness. Craniofacial dimensions without obvious adaptive

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[3] Brace and Hunt (1990), Brace et al. (1993), and Brace et al. (2006) use a battery of 24 craniofacial and dental traits in their analyses, which have a demonstrated high utility for distinguishing between historic and prehistoric populations.

value behave according to 'neutral theory,'[4] in that they are under genetic control and that the results of the observed differences between groups are the result of genetic drift (Brace et al. 2006:242). However the usefulness of adaptive versus nonadaptive traits for studying phylogenetic relationships remains controversial (see Brace and Hunt, 1990:342; Ackermann and Cheverud, 2004:17951). This issue will be further discussed in Chapter 5, following the work of Howells and Brace in order to demonstrate the problems associated with using craniometric evidence as taxonomic indicators using a few simple examples of craniofacial analysis.

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[4] Neutral theory posits that most evolution at the molecular level occurs by neutral (generic) drift (Ridley, 2004:157–158, 686).

Chapter 5

APPLYING MODERN METHODS: TOWARD A NON-RACIAL MODEL OF HUMAN VARIATION

The goal of this chapter is not to build a new taxonomic scheme, but rather to evaluate the methods of craniometric analysis presented in the previous chapter and to discuss the possibility of building a workable model of ethnic variation following the arguments presented in Chapter 3. Therefore, there is no null hypothesis. Rather, this chapter is exploratory in the sense that it demonstrates how skeletal analyses have advanced, which leads into a discussion of future directions for methodology – the focus of the following (concluding) chapter. The approaches presented here include the application of both univariate and multivariate morphometric methods of craniofacial analysis to a sample of 24 world populations. The purpose of this study is to demonstrate the variability of results obtained from large-scale population analyses and also the problem of fitting individuals into population-level analyses as would be done for the purposes of forensic 'racial' determination.

It is perhaps more interesting to frame this examination not simply according to how the 24 populations relate to each other based on cluster analyses of cranial data, but to superficially explore further Szathmary and Ossenberg's (1978) still somewhat unresolved question, "Are the biological differences between North American Indians and Eskimos truly profound?" This is an interesting question both in terms of the biology of indigenous North American groups and in relation to the distance between other world populations, particularly because racial models typically see all indigenous North American and Asian populations as part of the same (Mongoloid) racial group. However, if a significant degree of biological (or morphological) distance can be demonstrated between Amerindian, Inuit/Eskimo (Arctic), and Asian populations, we can move at least as far forward as to discount the existence of a general Mongoloid classification. This particular demonstration also allows for a forensic-type analysis of the controversial Kennewick Man skull, in order to discuss problems associated with the assignment of individuals into larger biological groups.

Materials and Methods

In order to place the diversity found between various North American populations Eskimo/Inuit and Amerindian[1] in the context of global variation, craniometric data collected from four North American groups was added to a general worldwide sample selected from published data included in Howells' global study of craniometric variation (1973, 1989). The total number of individuals in this study is 2028, comprising six general geographic groups,[2] which is broken down by sex and by data source in Table 4.1. Howells' published data are an excellent resource for physical anthropologists, since each population was carefully selected by Howells to represent distinguishable cultural and/or geographic units at the local level and their relationships within generalized regional geographic groupings (Howells, 1973, 1989; Relethford, 2001). Each (ethnic) group was selected by Howells based on the state of preservation and the availability of approximately equal numbers of males and females in each group, totalling approximately 100 specimens for each local group. Full descriptions of the groups chosen

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[1] These two regional groups will herein be referred to as Arctic and Americas/American respectively.

[2] The six geographical grouping here differ slightly from the five originally used by Howells. In the current study, groups that do not fit well into continental grouping for historical reasons have been assigned to the group 'Other,' and North American groups have been divided into two groups (see note above).

Table 4.1 Number of cranial samples used in analysis by sex and by researcher, organized according to general geographic region and local group designation (Howells, 1973, 1989; Powell and Rose, 1999; Chatters, 2000).

	Generalized Geographic Region	Local Group	Male	Female
Howells	Europe	Norse (Norway)	55	55
		Zalavar (Hungary)	53	45
		Berg (Austria)	56	53
	Africa	Egypt	58	53
		Teita (Kenya)	33	50
		Dogon (Mali)	47	52
		Zulu (South Africa)	55	46
		Bushman (South Africa)	41	49
	Australo-Melanesia	Australia	52	49
		Tasmania	45	42
		Tolai (Melanesia)	56	54
	Far East	N. Japan (Hokkaido)	55	32
		S. Japan (Kyushu)	50	41
		Hainan Island (China)	45	38
	Other	Andaman Islands (India)	35	35
		Ainu (Japan)	46	38
		Buriat (Siberia)	55	54
		Eskimo (Greenland)	53	55
	Americas	Arikara (South Dakota)	42	27
		Peru (Yauyos)	55	55
		Howells Total	987	923
Billinger	Arctic	Siberia	14	17
		NWT (Northwest Territories)	14	19
	Americas (Amerindian)	Navajo (New Mexico)	19	10
		Mexico	16	8
		Billinger Total	63	54
		OVERALL TOTAL	1050	977
Powell & Rose/Chatters	Prehistoric Americas	“Kennewick Man”	1	

by Howells can be found in both his *Cranial Variation in Man* (1973) and *Skull Shapes and the Map* (1989).

Data collected by the author (Billinger) for two Amerindian and two Arctic populations were obtained from the study of the skeletal collections of the Anthropology Division of the American Museum of Natural History (AMNH). Data collection occurred in August of 2003 under a collection study grant from the AMNH. Craniometric data were collected according to the methods outlined in Chapter 7 of Buikstra and Ubelaker's (1994) *Standards for Data Collection from Human Skeletal Remains*, which are compatible with those used by Howells, though less comprehensive, since Howells includes numerous measurement that require specialized equipment. Sex determination was also made according to the criteria from the same volume (Buikstra and Ubelaker, 1994:19-21), and also with reference to White and Folkens (1991:321-323). Only adult crania were examined, which was determined by observing a combination of the eruption of the third molars and closure of the cranial sutures (Helmuth, 1998:91-110). Data was also collected from a number of other Amerindian samples in the AMNH collections but excluded from the present analysis because of small sample size. One prehistoric specimen has also been included in the study, which will be specifically addressed below.

In total, 24 local or ethnic groups are included (plus one prehistoric specimen from North America). For this study, 10 craniometric traits were selected based upon their inclusion in the calculation of cranial indices by Bass (1995), which have traditionally been used for the classification of crania according to simple shape categories. These 10 traits were also shown to have significant value in Howells' discriminant analysis and a probable high genetic basis (Keita, 1993:434). These particular 10 traits were selected in order to provide a demonstration of the different results obtained through the analysis of simple cranial indices, as was traditional anthropological practice (comparable to the example of Boas' use of cephalic index to explore changes in the cranial shape of immigrants in Chapter 3), with those obtained through morphometric analysis, which is common today, using the very same measurements for each of these two analyses. As such, the results will have little taxonomic significance, since they are based on too few craniometric traits to provide a comprehensive view of the full configuration of the skull. However, they do provide a valuable demonstration of the necessity for theoretical and methodological advancement in terms of ethnic biology.

Descriptions of methodology for the collection of each of the 10 craniometric traits are included in Appendix A. Mean values and standard deviations for each trait by local group and by sex are found in Appendix B, as well as the number of individuals included in each local group used in the analysis. The data are shown graphically (by sex) using a parallel coordinate display and Andrews' Fourier plot.[3] The inclusion of these 10 craniometric traits will allow for the comparison of cranial indices to multivariate morphometric treatments of the same data.[4] Such morphometric comparisons are interesting because shape is considered to be of significantly higher value for taxonomic

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[3] According to the SYSTAT (statistical analysis software package) v.11 manual, a parallel coordinate display draws an 'axis' for each variable and positions them side by side so they are parallel. The same scale is used for each axis. The values for a case are plotted on the axes and connected with a line segment. The patterns of the connecting lines vary by subgroup. An Andrews' Fourier plot constructs a waveform (for each case) made up of sine and cosine components using a different sine or cosine component for each variable. Cases with similar values have waveforms with similar shapes, making it easy to recognize distinct subgroups of cases.

[4] Note: only 9 of the 10 craniometric traits used in the multivariate analysis are included in the calculation of cranial indices. Maxillo-alveolar breadth (MAB) and maxillo-alveolar length (MAL) are used in the calculation of the Maxillo-alveolar index (MAI) (Bass, 1995), and data for both traits were collected according to Buikstra and Ubelaker (1994), but MAL was not collected by Howells, so the MAI could not be calculated, yet MAB may have relevance as an adaptive trait, so it has been retained in the multivariate analysis.

purposes than size (Corruccini, 1973, 1987). Descriptions of 8 cranial indices are located in Appendix C.

The calculation of mean cranial indices was achieved using average group values for the applicable individual traits, since Howells' data gives only group means and standard deviations for each trait, not individual scores. These results appear in Appendix D. The resulting indices, therefore, allow only for a very general comparison, essentially presenting a single composite value for each group in each index, devoid of information regarding the variation within each local group. The resulting indices are also shown graphically a using parallel coordinate display and Andrews' Fourier plot for each sex. Average scores were then converted to correlation coefficients by group-by-group (pairwise) comparison producing a dissimilarity matrix for each index. Since cranial indices represent a mathematical transformation of linear data into a generalized combined measure of size and shape, the resulting sum of the distance matrices for each index can simply be input directly into a Euclidean distance cluster analysis without further transformation for size and/or shape correction. The resulting Euclidean distance dendrograms appear in Appendix E.

All of the Euclidean distance dendrograms appearing in the Appendices were created with the statistical program © SYSTAT v.11 using its hierarchical clustering function. This program offers numerous linkage methods for calculating and plotting Euclidean distance dendrograms (hierarchical clustering), but only the average linkage and Ward linkage methods are used here, since they were shown in numerous studies to produce the most satisfactory clusters when dealing with continuous data such as craniometrics (see Wilmink and Uytterschaut, 1984:150; Cybulski, 2005). SYSTAT automatically colour-codes terminal nodes of (major) clusters at a level of significance of 0.5 when producing dendrograms. Distance scales in each plot are relative to the actual results input into the SYSTAT program.

Each distance matrix input into SYSTAT for clustering was also plotted using SYSTAT's multidimensional scaling (MDS) function, a function related to principal components and factor analysis. According to the SYSTAT manual, MDS computes coordinates for a set of points in space such that the distances between pairs of these points fit as closely as possible to measured dissimilarities between a corresponding set of objects. The Young loss function, which is included in other popular statistical software packages, tends to favour large distances over small distances (see also Cybulski, 2005), providing generally good data visibility.

Each of the 10 individual cranial measurements is treated by Mayr's (1969:187-197) coefficient of difference (CD), a simple calculation that incorporates the mean values and standard deviations of each trait for each local group. Applying this measure to the data will shed light on whether, using the selected craniometric traits, the groups used in the analysis are differentiated enough to form a subspecies according to Mayr's definition. Accordingly, a CD value of 1.28 reveals that 90% of individuals in population *a* are different from 90% of individuals in population *b*, which is equal to 75 percent of individuals from population *a* differing from all individuals of population *b*, which is the premise of the so-called 75-percent rule. This method was previously used by Keita (1993) on a small representative sample from Howells' published data using four craniometric traits and six geographically diverse populations (one for each continental divide). Keita (1993:436) concluded from the limited study that "it would be better to think in terms of trends or tendencies and not distinct isolated groups."

The specific calculation used in determining the CD is the absolute (positive) value of (M*b* – M*a*)/(SD*a* + SD*b*), where M is the mean group value for each trait and SD is the standard deviation for the same trait for groups *a* and *b*. Group-by-group comparisons were made for each of the 24 groups for each of the 10 craniometric traits. Triangular data matrices containing the sum of the results of each group-by-group comparison for all 10 traits for each sex shown in Appendix F. Average values above 12.80 appear in bold print.[5] Mayr makes no mention of applying the CD to sex-specific groups, but because of the dimorphic nature of modern humans, the male and female data have been treated separately. The average results (e.g. sum of the coefficients) from each dissimilarity matrix were then subjected to a Euclidean distance cluster analysis, and the results of the clustering and multidimensional scaling are presented in Appendix F.

Mayr's univariate method is also adapted for multivariate application in this study by combining it with Penrose's (1954) shape distance (SHPD) and mean square distance (MSD). The calculations for both the SHPD and MSD (and also size distance, which is not applied here, since the CD is a measure of size difference) were done together using the mean values for 10 craniometric traits for each of the 24 local groups using the method outlined by Penrose (1954:339). In his example, Penrose input craniometric data in standard deviation units – dividing the group mean by the standard deviation – but in this analysis, Mayr's CD equation, (M*b* – M*a*)/(SD*a* + SD*b*), was used for making group-by-group comparisons since it incorporates a measure of overall standard variation

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[5] When using the sum of the correlation coefficients, Mayr's baseline figure of 1.28 should be multiplied by the number of traits used. In this case, there are 10 traits: 1.28 x 10 = 12.80.

between the groups being compared instead of the simpler standard deviation units. However, in this case, both positive and negative values are used for the pairwise comparisons rather than the absolute values. MSD is calculated for each group-by-group comparison by calculating the sum of the squares of the coefficients for each trait and dividing by the square of the number of traits used. SHPD is calculated by finding the result of the sums of coefficients obtained in each group-by-group comparison, divided by the square of the number of traits used, then subtracting the resulting value from the value obtained for the MSD for each group-by-group comparison (see Figure 4.2).

Penrose's MSD was developed as a test of Pearson's (1926) coefficient of racial likeness, which had been heavily criticised by Fisher (1936). MSD emerged as a more viable mathematical alternative to the commonly applied but more complex generalized distance of the Mahalanobis D^2 method (Penrose, 1954; Rightmire, 1976; Cybulski, 2001). According to Corruccini (1973:744), the popularity of the Mahalanobis D^2 lies more in its traditional use as a measure of distance by anthropologists than from the empirical establishment of its usefulness for comparing related taxa. Penrose's MSD remains a well-known alternative to Mahalanobis' D^2 because of its ease of use as a measure of biological distance, though Penrose's SHPD is less often applied. In a comparison of numerical taxonomic methods, Corruccini (1973:751) found a dichotomy between the results and taxonomic conclusions derived from size and shape measures of similarity, and therefore concluded that size statistics or generalized distances such as Penrose's MSD or Mahalanobis' D^2 are much less reliable than measures of shape for taxonomic purposes. Treating the data to both the MSD and SHPD and each resulting data set to Euclidean distance clustering will allow for direct comparison of the shape-

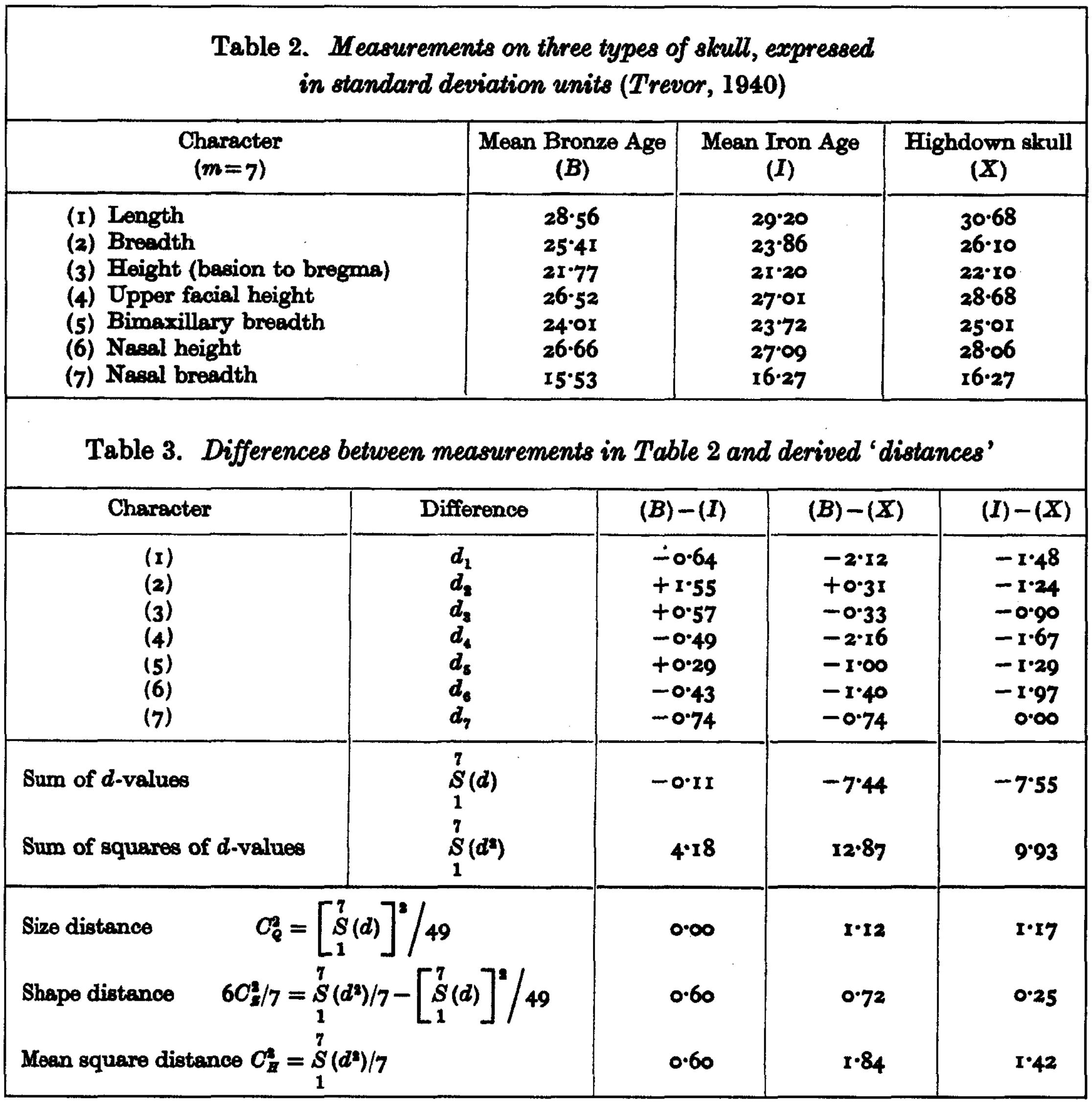

Table 2. *Measurements on three types of skull, expressed in standard deviation units* (*Trevor*, 1940)

Character ($m=7$)	Mean Bronze Age (B)	Mean Iron Age (I)	Highdown skull (X)
(1) Length	28·56	29·20	30·68
(2) Breadth	25·41	23·86	26·10
(3) Height (basion to bregma)	21·77	21·20	22·10
(4) Upper facial height	26·52	27·01	28·68
(5) Bimaxillary breadth	24·01	23·72	25·01
(6) Nasal height	26·66	27·09	28·06
(7) Nasal breadth	15·53	16·27	16·27

Table 3. *Differences between measurements in Table 2 and derived 'distances'*

Character	Difference	$(B)-(I)$	$(B)-(X)$	$(I)-(X)$
(1)	d_1	−0·64	−2·12	−1·48
(2)	d_2	+1·55	+0·31	−1·24
(3)	d_3	+0·57	−0·33	−0·90
(4)	d_4	−0·49	−2·16	−1·67
(5)	d_5	+0·29	−1·00	−1·29
(6)	d_6	−0·43	−1·40	−1·97
(7)	d_7	−0·74	−0·74	0·00
Sum of d-values	$\overset{7}{\underset{1}{S}}(d)$	−0·11	−7·44	−7·55
Sum of squares of d-values	$\overset{7}{\underset{1}{S}}(d^2)$	4·18	12·87	9·93
Size distance $C_Q^2 = \left[\overset{7}{\underset{1}{S}}(d)\right]^2\Big/49$		0·00	1·12	1·17
Shape distance $6C_Z^2/7 = \overset{7}{\underset{1}{S}}(d^2)/7 - \left[\overset{7}{\underset{1}{S}}(d)\right]^2\Big/49$		0·60	0·72	0·25
Mean square distance $C_H^2 = \overset{7}{\underset{1}{S}}(d^2)/7$		0·60	1·84	1·42

Figure 4.2 Example of calculations for Penrose's shape distance (SHPD) and mean square distance (MSD). In this example, only 3 groups are compared, whereas in the current study, 24 groups are compared, creating triangular dissimilarity matrices from which the SHPD and MSD are calculated. From Penrose (1954:339); table numbers refer to original publication.

specific distance calculations to those of the generalized size/shape distances (SHPD, Appendix G; MSD, Appendix H).

It would be possible to apply Penrose's SHPD and MSD calculations to craniometric C-scores following the method outlined by Brace and Hunt (1990:347).

However, it would be necessary to use Z-scores to calculate average C-scores, and obtaining Z-scores relies on the availability of data from individuals rather than group means. Therefore, the multivariate treatment of craniometric data is limited by the availability of appropriate data, which is a great problem when conducting analyses at the global level, since published data must be used. However, the results of this analysis can be compared to those obtained by Howells and Brace and Hunt to see if the same or similar patterns emerge.

In order to test the ability of multivariate methods to place individual specimens into clusters based on large-scale data sets, data for the so-called Kennewick Man is included as a prehistoric specimen from the Americas, in order to explore the temporal dimensions of biological distance measures. Data was obtained primarily from Powell and Rose (1999), with the addition of a single value, bizygomatic diameter (ZYB), which was reported by Chatters (2000) but not by Powell and Rose. The Kennewick skull, from the shores of the Columbia River in Washington State, is one of the oldest known cranial specimens in North America, at 9500–9000 years old (McManamon, 2000). This specimen is particularly interesting for this study because the assignment of ancestral affinity to this skull has been a highly contentious endeavour (Thomas, 2000; Morell, 1998).

The Kennewick Man cranial data can be simply inserted into the male mean raw data set used in the prior analysis (sex is known in this case so the Kennewick skull will only be compared to the male data sets) as in Appendix B. Mayr's CD cannot be applied due to the absence of a standard deviation values for the Kennewick Man skull. In this case, the group averages (and Kennewick measurements) were treated according to

Penrose's (1954) method. Cluster analysis of Penrose's SHPD and MSD based on group means for 24 groups plus the Kennewick skull appear in Appendix J and K respectively.

Results of Analyses

The diagnostic value of the obtained clusters should be assessed according to the static nature of the clusters themselves: they should retain their similarity irregardless of sex or linkage method used to obtain the clusters (Swedlund and Anderson, 2003:165). The results below give the number of statistically significant (major) clusters as determined by the SYSTAT program (these are the colour-coded groupings that appear in the dendrograms in the Appendices), which is dependant upon the linkage method. Distinguishable micro-clusters also appear within each of these major clusters; the following sections describe the relationships between these micro-clusters and major clusters according to the different methods of analysis. The numbers of cluster will not be of concern when discussing the Kennewick skull, since the purpose of adding that specimen to the analysis is to find the groups that it most closely associates with morphologically.

Cranial Indices (Appendix E):

Sex	# of Major Clusters *Average Linkage*	# of Major Clusters *Ward Linkage*
Male	4	2
Female	2	3

Micro-clusters that remain stable between sex and linkage methods.	1	[Bushman-Tasmania]
	2	[Australia-Teita-Zulu-Tolai]
	3	[Egypt-Zalavar-Norse]
	4	[Berg-Buriat]
	5	[Dogon-Ainu]
	6	[Arikara-Peru]
	7	[Eskimo-NWT]

In general, an African/Australo-Melanesian (Bushman-Tasmania-Australia-Teita-Tolai-Zulu) cluster is observed. Dogon-Ainu group together in 3 of the 4 dendrograms, but the ordering of that group is not regular. Similarly, Andaman does not seem to be an easy fit in any of the dendrograms, and the Peru-Arikara group is equally variable in its placement relative to other micro-clusters. NWT-Eskimo group together consistently, but are variously connected to African, Asian, Arctic/American groups depending on sex and linkage method. Berg-Buriat remain constant, and associate strongly with Egypt-Zalavar-Norse. According to the average linkage method, Navajo are significant outliers in both male and female dendrograms, to the exclusion of all other groups. The results of the MDS show significant distance between all of the groups that form the general African/Australo-Melanesian cluster. The Asian groups, N. Japan-S. Japan-Hainan, are closely situated in comparison, but it is difficult to distinguish between the Asian, European, and Arctic/American groups due to a high degree of overlap.

Average Coefficient of Difference (CD) (Appendix F):

The values in the charts below show the group comparisons in which the resulting mean CD value is above the level of 12.80 (see note 7, above), as well as the average, standard deviation, minimum and maximum values for each mean CD matrix.

Sex	*Mean*	*STDEV*	*Min*	*Max*
Male	5.27	2.70	1.52	14.64
Female	4.98	2.68	0.96	13.51

Sex	*Group Comparison*	*CD Value*
Male	Bushman/Siberia Andaman/Buriat Andaman/Siberia Andaman/NWT	13.25 14.64 14.49 12.90
Female	Bushman/NWT Andaman/NWT	13.14 13.51

Sex	# of Major Clusters *Average Linkage*	# of Major Clusters *Ward Linkage*
Male	5	3
Female	5	2

Micro-clusters that remain stable between sex and linkage methods.	1 2 3 4 5	[Siberia-NWT-Eskimo] [Peru-Berg-Norse-Zalavar-Egypt] [Andaman-Bushmen] [Tasmania-Australia-Tolai-Teita-Dogon-Zulu] [Hainan-S. Japan-N. Japan]

According to this method, an association between Andaman-Bushman to the exclusion of all other groups is seen. An African/Australo-Melanesian cluster, this time including Dogon, is evident. Siberia-NWT-Eskimo form a strong micro-cluster that associates well, though in variable degrees, with Arikara, Buriat, Mexico, and Navajo. Rather counter-intuitively, Peru has joined the Berg-Norse-Zalavar-Egypt cluster. Hainan-N. Japan-S. Japan cluster closely together, with Ainu joining this cluster in 3 of the 4 dendrograms. Mexico-Buriat-Arikara-Navajo remain associated across sex and linkage method, but the degree of relationship is highly variable. The MDS plots show very similar patterns to those seen in the dendrograms. Andaman and Bushman both lie at the extreme right side of the plot, a general African/Australo-Melanesian cluster can be seen in the bottom-right quadrant, Europe in the middle, and Asia and Arctic/Americas moving left, again with a significant amount of overlap between the latter three groups.

Adapted Penrose's Shape Distance (SHPD) (Appendix G):

Sex	# of Major Clusters *Average Linkage*	# of Major Clusters *Ward Linkage*
Male	3	3
Female	2	3

Micro-clusters that remain stable between sex and linkage methods.	1 2 3 4 5 6	[Bushman-Teita-Tolai-Australia-Tasmania] [Eskimo-NWT-Siberia] [Arikara-Peru] [Egypt-Zalavar-Norse] [Berg-Buriat] [Hainan-S. Japan-N. Japan]

A general African/Australo-Melanesian cluster is seen again, though Dogon males do not directly associate with this cluster as the Dogon females do. The Arctic cluster of Eskimo-Siberia-NWT appears in each dendrogram as a significant unit, and Peru-Arikara are closely associated, though variously connected to Mexico, Buriat, Berg, the Asian groups (Hainan-S. Japan-N. Japan), and the Arctic cluster. Zalavar-Norse-Egypt remain stable, but have various associations to other micro-clusters in each dendrogram. MDS show a distinguishable Arctic cluster for both male and female, but a large degree of overlap in the distribution of the Asian, European, and American groups. Both Andaman and Dogon appear to associate more closely with the Asian groups than the Australians or Africans.

Adapted Penrose's Mean Square Distance (MSD) (Appendix H):

Sex	# of Major Clusters *Average Linkage*	# of Major Clusters *Ward Linkage*
Male	3	3
Female	4	2

Micro-clusters that remain stable between sex and linkage methods.	1 2 3 4 5 6	[Bushman-Andaman] [Tasmania-Australia-Tolai-Teita-Zulu-Dogon] [Eskimo-Siberia-NWT] [Buriat-Navajo] [Hainan-S. Japan-N. Japan-Ainu] [Egypt-Zalavar-Norse-Berg-Peru]

Variously joining the Arctic Eskimo-Siberia-NWT cluster are Buriat, Navajo, Arikara, and Mexico. From the female data, Mexico and Arikara group closely together but form a major cluster with the Asian groups, whereas the male results cluster Arikara and Mexico with Peru and the European-Egypt cluster. Ainu clearly join a distinguishable Asian cluster according to this method. Bushman and Andaman form a distinct cluster to the exclusion of all other groups in all dendrograms. Again, a general African/Australo-Melanesian cluster is seen. MDS shows a much closer association between the groups than the previous methods. Navajo can be seen as lying quite far outside the average range of variation for the Arctic and American groups, and Andaman and Bushman again appear at the far right half of the plot. The plots are difficult to interpret based on the density of the plotted points at the centre, but the Siberia-NWT-Eskimo cluster can be seen in both males and females. There is also a close association between Australia and Tasmania in both sexes, and further with Teita-Tolai-Zulu-Dogon. These groups overlap significantly with the European groups when Egypt is taken into account. Peru resides much closer to the European cluster than to any of the American groups.

Penrose Shape Distance (SHPD) w/ Kennewick Skull (Appendix I):

Based on shape, the Kennewick skull groups with the Arctic (Siberia-NWT-Eskimo) cluster that was seen in the previous analyses. According to both linkage methods, the Kennewick skull is most morphologically similar to the Eskimo group, in terms of SHPD. MDS shows that while the Kennewick-Siberia-NWT-Eskimo do form a major cluster, that there is a high degree of differentiation between these groups, with the Kennewick skull lying at the greatest distance.

Penrose Mean Square Distance (MSD) w/ Kennewick Skull (Appendix J):

The Kennewick skull clusters with the Arctic (Eskimo-NWT-Siberia) cluster as in the previous method, forming a distinguishable cluster. Though at a much greater distance, the Kennewick skull also associates with Ainu in terms of size and shape. The results using the Ward linkage method show Kennewick as part of an overall Arctic/Americas cluster, which includes all of the Arctic and American groups with the exception of Mexico, which associates most closely with S. Japan, and Berg has joined this general cluster due to its overall close similarity to Peru. MDS shows that Kennewick groups closely with NWT, but Eskimo, with which Kennewick clustered most closely in the dendrograms, cannot be seen due to the overlapping of data. Plotting MDS using the Guttman loss function rather than the Young loss function reveals an equidistant relationship between Kennewick and both NWT and Eskimo.

Interpretation of Results

The graphic representation of the raw data for both males and females shown in Appendix B illustrates that the most variation in mean scores and standard deviations[6] between groups occur for the following traits: maximum cranial breadth (XCB), upper facial height (NPH), and to a lesser degree, orbital height (OBH). The number of plotted lines on the graphs make them difficult to read directly, but referring back to the raw data shows that Buriat and Navajo have the (absolute) largest measurements for XCB, while Teita and Tolai have the smallest, thus accounting for the extremities of the wave peak shown in the Andrews' Fourier plots (in the case of the female data, Australia and Tasmania are slightly smaller than Tolai, but Teita is the smallest in terms of XCB). In terms of cranial indices, the greatest amount of variation is seen in the cranial breadth-height index (CBH) and nasal index (NI), regardless of sex. Considering the individual measurements and indices together, the overall morphological difference seen between groups is primarily a result of the length and width of the skull, and variation in the nose and midface, at least as can be reasonably extrapolated from the 10 cranial traits and 8 indices used in this study.

In general, three groups remain consistent throughout the various methods of analysis: Norse-Zalavar-Egypt, Africa/Melanesia (Australia-Tasmania-Tolai-Teita), and Eskimo-NWT. While there is some consistency seen between the clusters produced from cranial indices and those produced from the univariate and multivariate morphometric analyses, it is limited. For example, while Peru groups most closely with Arikara in terms of cranial indices, which is not unexpected based on geographic proximity, Peru groups most closely with Berg using all other methods, while Berg clusters with Buriat in terms

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[6] See note 5 regarding the plotting of data for parallel coordinates display in Appendix B.

of indices. Clusters produced from cranial indices are unable to distinguish between Asian, Arctic, and American groups.

As might be expected, the results obtained from clustering the simple cranial indices were not as satisfactory as the multivariate morphometric analyses, or even when compared to the univariate CD, although some of the same clusters were produced. This method (i.e., comparing cranial indices) may give an indication of the general shape of the skull but may not account for minor morphological variations that may have high adaptive value, and important effects on the overall morphology of the skull. Penrose's SHPD seems to provide a better indication of overall shape than the clustering of cranial indices.

The CD's ability to distinguish subspecies demonstrates that the degree of variation between the groups used in this study was surprisingly low and nowhere near sufficient to declare the existence of any human subspecies. Though there were some group comparisons that resulted in values above the subspecies 1.28 [12.80] or 75-percent rule, they were limited to Bushman-Siberia and Andaman-Siberia/Buriat/NWT. Taking the average values, standard deviation, and range into account for each mean distance matrix, we can easily see that the amount of continuous variation that exists between these extreme group comparisons signifies significant overlap between groups (see also Keita, 1993:436). The CD does not work on individuals since standard deviations are required in each calculation. Rather, the CD was developed to be applied only to representative samples or populations, therefore the smaller the number of individuals in each group, the less reliable the data. A further problem with the CD is its ability to only account for single measurement; therefore, the results are based purely on average size differences seen between a set of 10 single measurements.

The multivariate adaptation of the CD calculation to Penrose's SHPD and MSD provides much more satisfying results. Though Corruccini (1973, 1987) argues that shape is a more important taxonomic indicator than generalized size/shape distances, it is easy to understand why generalized distances such as the MSD or Mahalanobis' D^2 are favoured by physical anthropologists, since they produce patterns closer to those that might be expected based on geographic proximity.

Taking all of the results into account, it is not possible to unequivocally claim that there are profound differences between Arctic and Amerindian cranial morphology. Although Eskimo and NWT cluster together by all methods, and Siberia joins that cluster in all cases except cranial indices, Buriat do not fit particularly well into this general group. However, if there was sufficient reason to suggest that the Siberia and Buriat groups are not as closely associated temporally or biologically as one might assume based on geographic proximity, then they could be excluded from the general Arctic group. This cluster could then be considered physically distinguishable from all other American groups. Further ethnohistorical research would be valuable in this case.

The morphological relationships between the groups in the Americas are much more problematic than in the Arctic. There appears to be a wide range of diversity, and the Navajo are particularly problematic, since they have an uncommonly large variation in cranial dimensions. The wide range of variation within Arctic and American groups in general results in significant overlap with various Asian groups and European groups, with a particularly strong relationship between Peru and Berg. Though the degree of similarity between some of the American groups and some European groups was unexpected, the pattern seen between the Asian/Arctic/American groups is most likely indicative of the strong biological relationship between Asian and American populations in terms of the overall sample. One might also suggest that perhaps the bottleneck effect of relatively small American founder populations can be characterized by high levels of regional diversity (Relethford, 2001). Some of the greatest overall distances between groups seen in various dendrograms were within the Arctic-American clusters. This provides a particularly interesting context in which to attempt to place the Kennewick skull.

When the Kennewick skull is inserted into the overall sample, it clusters with the Arctic groups according to shape and generalized size/shape distance. Kennewick Man has been previously considered to most closely resemble Ainu in terms of overall morphology (Morell, 1998). According to Penrose's MSD, while Kennewick associates more closely with Ainu than any other group outside of the Arctic cluster, the amount of physical differentiation is far too high to claim any direct relationship, at least based on the 10 cranial traits used in this study. According to these results, it is relatively certain that the skull belongs within the general cluster of the Americas, but not within any of the groups used in these analyses.

Having narrowed down a general geographic location, specific information about the affinity of the skull may be found if analysed according to a smaller regional sample. Values obtained from Cybulski (2005) for various historic and prehistoric North American (Amerindian) groups form a good comparative sample to add to the Arctic/Americas data used in this study (Appendix K, group data obtained from Cybulski are indicated with asterisks). Although data for only four craniometric traits and two indices are published, Steele and Powell (1992) believe these to be the most valuable in distinguishing prehistoric from historic Amerindian crania. A high degree of variation can be seen from both the parallel coordinate display and Andrews' Fourier plot.

Since standard deviations were not published for this data, it is again not possible to calculate a CD matrix. Applying the group means to the two Penrose transformation methods (SHPD, Appendix L; MSD, Appendix M), the Kennewick skull groups closely with Blue Jackets Creek (BJC), in a general cluster with Paleoindian and Eskimo. Based on the 9500-9000 year age of the Kennewick skull as determined by C^{14} dating (McManamon, 2000), it would not be unreasonable to assume that this specimen should more closely associate with the Paleoindian group, however, regionally, Kennewick and BJC are in relatively close proximity on the Northwest coast of North America. Unfortunately, no strong assumptions can be made about the affinity of the Kennewick skull beyond that fact that it has apparent morphological similarity to the BJC, Paleoindian, and Eskimo groups, since only four craniometric traits are used in the analysis. A larger battery of traits may increase the accuracy of the analysis, though the main problem in this analysis may be exactly what Williams et al. (2005) found: a high degree of variation in local regional groups means that no group will readily accept the individual data.

Discussion

Determining the biological relationships between groups is a highly subjective undertaking. The results are determined not only by the type of evidence collected, but in the number and type of variables collected and analysed. Brace and Hunt (1990) and Brace et al. (1993) raise the issue of adaptive versus nonadaptive morphological traits, but there should be little doubt that morphological variation is a reflection of genetic processes where even the smallest variation in a single element can have an overall effect on the entire configuration of cranial morphology. The primary goal of multivariate morphometrics is to account for these differences. Brace and Hunt (1990:344) concluded from their morphometric study of cranial variation that, "Although we did succeed in getting a picture of population relationships that served our purposes, we discovered that the picture became even sharper when we simply added more measurements without any regard for adaptive significance of the underlying dimension." As such, *the larger the number of craniometric traits used in an analysis, the closer the results should be to those obtained from molecular genetic analysis. This is important, of course, because genetic material often cannot be extracted from historic or prehistoric skeletal specimens, and there is an obvious need for a high degree of comparability between past and present populations for taxonomic purposes.*

There are many ways of transforming and visualizing multivariate data, and the results of this study demonstrate that the resulting configurations are as variable as the methods themselves. This is not to discount methods such as Euclidean distance dendrograms altogether, since some significant patterns were revealed by the dendrograms. The results from Penrose's MSD are difficult to interpret without good knowledge of the regional history of the groups used in the analysis. Generally, the results agree well with Howells' (1989) results based on the clustering of correlation coefficients of craniometric C-scores for 57 craniometric traits (Figure 4.3) and the calculation of Mahalanobis' D^2 from the same coefficients.[7]

The results seen in the present study are indicative of the large regional variation seen within the Americas and Arctic clusters, but the interconnections between the clusters may be better interpreted in the context of historic trade relationships, such as those between the Athapaskans from the Northwest Territories, through the Northwest

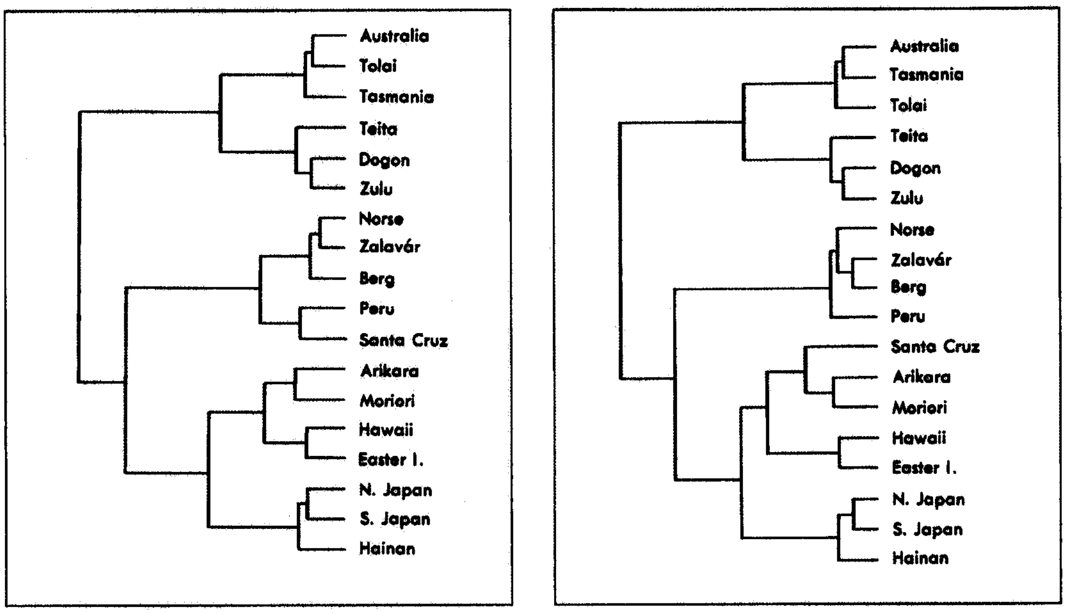

Figure 1. Cluster analysis of 18 male groups based on intergroup correlations.

Figure 2. Cluster analysis of 18 female groups based on intergroup correlations.

Figure 4.3 Results of Howells' analysis using craniometric C-scores from 57 craniometric traits. From Howells (1989:38); figure numbers refer to original publication.

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[7] It should be noted here that Pietrusewsky and Chang (2003:306) suggest that the removal of size-based components using C-scores measures has little or no effect in interpreting patterns of craniometric variation.

Coast possibly into the Great Basin, and down to the American Southwest.[8] These results, however, do show that the Kennewick skull clusters closely with the Blue Jackets Creek, Paleoindian, and Eskimo samples, based on the four craniometric traits used for comparison.

The resolution of cluster analyses can be improved with the addition of larger numbers of groups (comprised of significant numbers of individuals) and traits as the basis for comparison. Relationships that look significant when groups are analysed on a global scale may be significantly altered when locally relevant data is added. For example, the relationship between Kennewick and Siberia-NWT was diminished when other geographically relevant data was added. In terms of global patterns, geographic proximity as an indicator of 'racial' affiliation is inadequate. Since a distinguishable Arctic cluster was seen, this demonstrates that Asians/Arctic/Americans are significantly dissimilar to invalidate the general Mongoloid racial category. Further, the fact that the European Norse-Zalavar-Berg cluster associates strongly with Egypt and Peru indicates that the idea of a European or Caucasoid race is also inadequate. The close relationship seen between the African and Australo-Melanesian groups is not surprising given Howells' craniometric analyses (as discussed in Chapter 2), but in the light of genetic evidence, we understand that the similarities seen are a product of evolutionary processes rather than biological relationship. Therefore, while such methodologies may be useful

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[8] Tracing stylistic patterns in moccasin design, Ives and Billinger (2003) argue that 250 moccasins uncovered in the Promontory Cave in Utah by Julian Steward dating to A.D. 1110 ± 75 show remarkable similarity to those seen throughout the Athapaskan range, and thus may help to refine our understanding of the relationship between Northern and Southern Athapaskan-speaking groups and migration patterns throughout aboriginal North America. It is therefore not implausible that a constant flow of genes from North to South over approximately one thousand years had resulted in a higher degree of morphological similarity than might otherwise be expected based on physical proximity.

for revealing patterns of morphological similarity, their taxonomic implications are highly dubious in the absence of genetic information.

The results of the various analyses are telling: moving from simple univariate measures and indices into multivariate morphometric approaches dramatically improves the quality of results, since they incorporate the full configuration of the skull (but the degree to which this configuration is incorporated depends upon the number of traits used in the analysis). C-scores provide results similar to those obtained using the adapted Penrose MSD measure used here, and C-scores *may* represent a more refined analysis, but they can only be applied to complete data sets – not based on group means – but at the level of the individual. While there have been improvements in the statistical analysis of biological distance using morphometrics, the results are limited by the amount of published data with which researchers can use for comparison.

Overall, the results of all analyses demonstrate that taxonomy and phylogeny are indeed not one and the same. The resulting clusters are reflections only of morphological similarity based on select traits. Microevolutionary forces constantly affect these traits, across distance and across time. Calculations of biological distance do not give direct access to these variables, since the patterns of variation seen are not so simply distributed. *In comparing data on a global or regional level, it is necessary to do so in terms of general historical context.*[9] *In conclusion, such contextual interpretation, along with more refined techniques incorporating a more intensive battery of traits in morphometric analyses, would produce, in essence, ethnic data, in accordance with the arguments set*

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[9] In this instance where Howells' data is used, it would be extremely difficult to interpret the biological relationship of the Andaman Islanders to any of the other groups without the historical context that Howells provides. The origins of aboriginal Andamanese remains unknown, and the island served as a British penal colony following the Indian Mutiny of 1857 (Thangaraj et al., 2002).

forth in Chapter 3. Phenetic results do not sort themselves accurately, and the groupings based on phenetic clustering are, at least in part, a product of the groups that the researcher has chosen to include in the analysis. In order to adequately interpret the results, researchers must sort the data based on prior knowledge of geographic patterns and historical relations between groups. These groups then, based on the necessary historical aspect, are ethnic groups. The value gained from multivariate techniques and taxonomic inference is, therefore, entirely dependant on multiple methods, and an informed interpretation of the results, not as simple biological data, but as historically-situated biocultural data.

Chapter 6

CONCLUSION: THE FUTURE OF RACE IN HUMAN VARIATION STUDIES

Human racial groups, which are akin to subspecies in other biological organisms, remain problematic from a taxonomic standpoint not only because of their socio-political connotations. Since the 1950s, biologists such as Wilson and Brown (1953; Brown and Wilson, 1954) have argued against the subspecies concept and trinomial nomenclature due to the arbitrary nature of such classifications in terms of real or presumed geographic discontinuities. The root of the problem is that taxonomists have traditionally relied too heavily on typological thinking, viewing species as polytypic aggregates of monotypic subspecies (see Chapter 2; Keita, 1993). Underlying this problem is the poorly understood relationship between genetic and morphological data and how they could be influenced by environmental constraints. Current anthropological models of migration and phylogeny in modern *Homo sapiens* are further complicated by the combination of complex layers of evidence, such as genetic, morphological, linguistic, and archaeological data.

Misguided taxonomic modelling by physical anthropologists incorporating non-morphological traits has resulted in confusion and irregularity at all levels because it is simply not systematic in terms of biological taxonomy. From generic to species-level disputes over the taxonomic placing of fossil hominids (Wood and Collard, 1999),[1] to the

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[1] An excellent and current example of the problems associated with the assumptions of hominid taxonomy is the recent discovery of an abnormally small hominid discovered in Indonesia. In the abstract to the article publishing the results of the preliminary examination of the specimen, Brown et al. (2004) write: "The combination of primitive and derived features assigns this hominin to a new species, *Homo floresiensis*."

characterization of microevolutionary units as subspecies, races, ethnic groups or breeding populations (Huxley and Haddon, 1935; Montagu, 1997; Molnar, 2002), existing classification schemes should be viewed simply as nomenclature and of little biological implication due to the lack of methodological rigour. If a valid taxonomic scheme applicable to humans can be developed, it must follow the principles of systematics that are accepted for biological organisms in general, and must directly address the issues of speciation and phylogeny. It appears that human history and migration is far too complex to fit into any single taxonomic model, at least in terms of subspecific recognition. While molecular genetic evidence is invaluable for studying population biology, osteological data must remain paramount for physical anthropologists, since the discrimination of taxa and description of ontogenetic (growth and development) or evolutionary change requires the analysis of morphological features of bone in the absence of genetic evidence (Weber et al., 2001). Furthermore, genetic evidence sheds no light on functional biological change.

Perhaps the most diagnostic area for study of human morphology and phylogeny is the skull. Anatomically modern humans show considerable geographic variation in the form of the facial skeleton. The study of craniometric variation presented in Chapter 5 is based on phenetic clustering according primarily to Howells' use of a geographically-ordered data set. Howells was aware that his selection of representative groups was not a full representation of either the average level or range of diversity within each general geographic area and that the geographic clusters are themselves not discrete. However, as

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However, the authors do not explain what these primitive and derived features are. Therefore, the taxonomic classification of this specimen in not particularly meaningful in the absence of discussion of the criteria on which the classification is based. It also remains unclear how many individuals are represented with this discovery, or whether the unusually small size of the crania is pathological.

demonstrated in Chapter 5, the results of hierarchical clustering are dependent on historical and geographic sortings of the data input into each analysis. The primary factors determining the outcome of such analyses are the type of data available and the type of analysis applied to them. Even when using a single data set in this study, the results were inconsistent depending upon the method of analysis employed.

The results of the CD analysis show that the 24 groups represented in this study form 7 general clusters by using average linkage Euclidean distances (which joins European and Africans groups, Australo-Melanesian and Asian groups forming 5 clusters of seven general groups). However, the use of morphological data in Mayr's CD or Penrose's distance measures uses phenotypic data as analogous to genetic data, which is not unproblematic even when the analogous nature is recognized. Thus, biological distance calculated from morphological data does not give an accurate approximation of genetic distance, which is problematic because biological distance is the main factor in determining taxonomic groups and hierarchical ordering.

The fundamental problem of such taxonomic thinking is in the assumption that evolution can be studied as branching patterns that form phylogenetic trees. Andreasen (1998, 2000, 2004) has argued that human races can be defined cladistically in such a way using molecular data when geographic groups are understood as evolutionarily significant ancestor-descendant sequences of breeding populations that share a common origin. This approach is similar to that taken by Kitcher (2003) in that it is based on the notion of approximate isolation, which may have a geographic or socio-cultural basis. This phylogenetic perspective in intricately tied to a single-origin model of modern human dispersal: that modern humans originated as a racially undifferentiated population of modern humans in Africa approximately 200,000 years ago,[2] migrating out of Africa and forming various reproductively isolated breeding populations, which can be represented by branches on a phylogenetic tree. The main requirement for cladistic analysis is that evolution must take the form of a branching process, but it does not require significant phenotypic or genetic differences among clades as in phenetic models (Andreasen, 2004:426–431). Thus, Andreasen argues, when human evolution can be understood as a pattern of evolutionary branching, the terminal nodes of a cladistic (phylogenetic) tree represent extant (monophyletic) racial groups, whose historical lineages can be traced backwards to a common ancestor.

Numerous problems exist in this cladistic argument, but only a few key points will be discussed here. First, cladistic classification was developed for defining higher taxa, which must be monophyletic groups (i.e., of a single origin), but this is a difficult assumption below the species level. Since species are reproductively isolated groups, they can form cladistic taxa, but there can be no sure way to prove isolation, whether absolute or relative, between groups that have no biological barriers to reproduction. Subspecies have no such barriers; they can easily exchange genetic material, hybridize, or recombine in the absence of physical barriers.

Morphologically, the discrimination of discrete populations becomes an even more complex and controversial endeavour below the level of species in terms of competing phylogenetic hypotheses. This is particularly problematic when dealing with

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[2] Conflicting human evolutionary models remain unresolved in this respect (see Chapter 2; Collard and Wood, 2000; Hawks and Wolpoff, 2003). Andreasen's approach inherently favours the branching pattern of the *Out-of-Africa hypothesis* of human origins (Stringer 1996; Templeton, 2002b) over the *Multiregional Continuity Model* (Brace, 1995[2000]:221; see also Wolpoff et al., 1984; Wolpoff and Caspari 1997, 2000). For a discussion of the competing models of human evolution and phylogeny and how they relate to race or subspecies, see particularly Cartmill (1997) and Billinger (2000, Chapter 3).

prehistoric specimens for which genetic information is not attainable, as morphology is the only option as a basis for phylogenetic reconstruction. If one is to argue that there are cladistic races of modern *Homo sapiens* morphologically, then *Homo erectus* should also be assigned subspecific taxa, similar to those of modern human races. If this is true, then it lends credence to the multiregional view of human evolution, which does not necessarily agree with monophyletic presumptions. Since hominid fossil evidence is so fragmentary, the issue of variation within and between early hominid species is still debatable on many levels. The main problem in this regard is in determining whether the amount of variation observed between prehistoric specimens is indicative of regional variation, or of a completed speciation event.

Second, a 'breeding population' is defined as a set of local populations that are reasonably reproductively isolated from other such breeding populations. If a breeding population is equal to a race or subspecies, then can the local populations therefore be considered subraces, and can these be broken down into sub-subraces? The definition of population here is quite problematic, and the assignment of clades is based solely on this type of populational thinking. There is no less arbitrary aspect to this type of classification at this taxonomic level than by any other method, though Andreasen (1998:209) believes that such groups can be considered monophyletic, and are therefore objective: "Since this process [of evolutionary branching] exists independently of human classifying activities, phylogenetic classifications are themselves objective."

While it may be true that evolutionary processes are objective, it seems a dubious philosophical claim that we can have direct access to these processes. Rather, the processes are only inferred from the study of patterns. In studying microevolutionary patterns, how would homologous traits be distinguished from analogous traits? At and above the species level, there is significant structural and genetic change that warrants the division into ancestral and derived trait types. But when dealing with microevolutionary change, particularly on the small scale of human variation, differentiation occurs in minute variations either in morphological structure or in genes frequencies. All traits – whether morphological or genetic – must then be homologous, since they are present in all human populations, differing only in the degree of expression. Therefore, there can be no essential difference between cladistic and phenetic approaches to taxonomy below the level of species.

Finally, Andreasen (1998:200) states plainly that according to her cladistic viewpoint, "Races once existed, but they are on their way out ... human activity is causing race to lose its biological reality."[3] It is exactly this human activity that would lead one to doubt the present or past existence of human races or subspecies. Migration patterns in branching phylogenies are seen as unidirectional and linear, and cannot demonstrate the dynamic and historical nature of human groups. Even if we were to assume that all modern humans evolved in Africa and migrated outward, it is entirely possible that this happened in multiple successive waves (Templeton, 2002b). If this is the case, then modern human variation would be the result of complex prehistoric genetic mixing, for which linear phylogenetic models cannot account.[4] It is also possible that some of the migrants or their descendants returned to Africa after some length of time, or that there was a constant flow of genes between founder and migrant populations, which

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[3] Andreasen (1998:215) further claims that "The cladistic concept of race ... shows that biological races are dynamic categories; races once existed, but due to recent historical events, are on their way out."

[4] If there were multiple waves of migration from Africa, even if early migrant groups became physically and/or genetically differentiated geographic populations, successive waves of new migrants would bring an influx of genetic traits from Africa, thus resulting in complex mixture of genetic traits.

may have splintered along the way but maintained supportive relationships. These possibilities are better characterized as webs of relationships rather than as single lineages.

A hypothetical situation can further illustrate the problems with the monophyletic assumption: group *a* and group *b* are both isolated groups who traditionally lived in areas A and B respectively. Neither group has encountered the other or the other's relatives in the past. Resources become scarce in both areas A and B, and in response one-quarter of the families in group *a* decide to migrate in search of better food sources, and one-quarter of the families in group *b* do the same, at about the same time. Both splinter groups arrive in area C, which is equidistant to areas A and B, at about the same time, and they find adequate food resources. Both groups stay and decide to settle in area C and begin to intermix, becoming group *c*. Who would be the common ancestor to group *c*? In this case, group *c* would be of mixed ancestry. What if food resources were to improve over time and group *c* was to prosper in area C and eventually spread out and recombine with both groups *a* and/or *b*? Cladistic theory simply could not adequately deal with this lineage.

Where pheneticism assumes that morphological similarity is the result of evolutionary processes, phylogeneticism makes no assumptions about evolutionary relationships, but is based only on demonstrated evolutionary relationships. However, in the case of so-called cladistic races, the notion of common evolutionary history – traced back through single lineages – is taken for granted rather than demonstrated. Perhaps the main flaw in any of the models of theoretical population biology is that they are based more on mathematical than biological principles (Corruccini, 1973).[5]

Moore (1994:931) uses ethnographic evidence to support the notion that human groups have complex and intertwined historical lineages rather than being unilineal ancestor-descendant sequences:

> The criticisms of synthetic theory currently being developed come largely from experienced field-workers and are based on the observation that the historical scenarios postulated by synthesists – in which ethnic groups split, evolve homogeneously within ethnic boundaries, and then split again in a cladistic manner – simply do not seem familiar ethnographically or ethnohistorically. How many tribal societies are there in which all the members are monolingual in the same language, marry only among themselves, and are homogeneous in their traditions and material culture?

Beyond the problematic assumption that terminal units of cladograms are biological populations or demes, cladistic theory explains less and less the farther into the past it is extrapolated. Similarly, however, generalized biological distances such as Penrose's MSD or Mahalanobis' D^2 sharply decrease in value with increasing separation (Corruccini, 1973).

Moore (1994) argues that synthetic theories such as cladism, which have been used to explain relationships and geographical distributions of language, culture, and physical types are generally weak theoretically and not particularly suitable for the study of modern human groups. He contrasts *cladistic* with *rhizotic* theories, explaining that where cladism emphasizes historical parent-daughter (or ancestor-descendant) relationships, rhizotic theories emphasize the extent to which each human language, culture, or biological group is derived from or rooted in several ancestral groups or

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[5] Corruccini (1973:751), in discussing biological distance measures, specifically states, "although generalized distance has long been a popular statistic with physical anthropologists, it is a statistical distance and as such it is not necessarily related to morphological, genetic, or taxonomic distance."

lineages, which he suggests is better characterized as the process of *ethnogenesis*. In Moore's (1994:927) view, ethnogenesis provides a logical alternative explanation for the global distribution of languages, cultures, physical types, and archaeological traditions, and he makes the important point that ethnogenesis stands in contrast to all hierarchical taxonomies, which, regardless of their aim or theoretical bases, are clearly meant to represent cladistic relationships, not phenetic relationships. Such hierarchical models are based on the presumption, in contrast to the hypothetical example above, that once a daughter species (or subspecies, race, deme, etc.) differentiates from its parent, it will not thereafter incorporate any genetic material from its siblings (Moore, 1994:928).

Cladograms and other hierarchical methods of phylogenetic reconstruction demand that each terminal unit,[6] whether genus or species, or so-called cladistic race, require that each entity have only one parental unit, whereas rhizograms show the convergence of diverse roots (possibly from multiple parental stock) forming hybridized or amalgamated descendant groups (Moore, 1994:930).

In a survey of tribes in California, the Great Basin, Sonora, Tierra del Fuego, and Australia, Owen (1965) found that instead of ethnically discrete bands or tribes, that multilingualism, intermarriage across supposedly profound ethnic boundaries, and enormous cultural variation was the norm. In a study of aboriginal band types on the Canadian plains, Sharrock (1973) found that there were three common types of interethnic social organization: alliance, intermarriage and polyethnic coresidence, and fused ethnicity (Moore, 1994:936). Moore (1994:938) believes that the essential difficulty between cladistic and ethnogenetic theory lies in the long term stability of

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[6] In numerical taxonomy, Rohlf and Sokal (1965) refer to these units generically as operational taxonomic units (OTUs).

ethnic groups: cladistic theory requires that ethnic groups remain stable for hundreds or thousands of years, whereas ethnogenesis attempts to understand the processes in which ethnic groups are transformed through time. While this contrast might not be as problematic as Moore suggests, the general point of incommensurability is that cladistics focuses on the biological fissioning of groups, whereas ethnogenetic theory deals with transition, which in many cases involves the rearrangement or fusion of groups.

It should be well demonstrated at this point that there is no single clear way in which biological variation can be viewed. The conservative approach to Mayr's view of the evolutionary synthesis that was presented in Chapter 2 was necessary for explanatory purposes, but may in fact be questionable. Mayr's (1959) contrasting of Darwinian population thinking with Platonic typological thought brought together the three interrelated concepts of essentialism, idealism, and typology as the philosophical core of anti-evolutionism (Amundson, 1998:156). Farber (1976) demonstrates that multiple forms of typology existed in the 19th century, and that the essentialist argument it entails is flawed by the failure to separate ontology and epistemology (Winsor, 2003:389). Not only did Mayr likely underestimate the prior existence of pre-Darwinian evolutionary thought and overestimate the unification of the evolutionary synthesis (Smocovitis, 1996), but Greene (1999:107) suggests that the "champions of the synthesis," including Mayr, were struggling for academic and scientific prestige in the face of formidable competition from 'physicalists,' who were applying the concepts and methods of physics and chemistry to biology:

> The discovery and demonstration of the double helix by Watson and Crick in 1953 and the subsequent rise of molecular genetics dealt a body-blow to the naturalist-systematist tradition in biology, a blow so severe that Ernst Mayr began to devote increasing time and energy to historical and

> philosophical studies aimed at discrediting physics-oriented history and philosophy of science and fostering a rival 'organicist' view of science and nature.

The most significant effect of Mayr's own version of the history and philosophy of biology is that his biological species concept is presented as the dominant view. In the philosophy of science, a general body of literature dealing with the 'poverty of the Linnaean hierarchy' (Ereshefsky, 2000) in the application of classification systems has demonstrated a lack of an all-encompassing theory of systematics. As a result, some philosophers of science have suggested an exploration of the application of pluralistic species concepts (Dupré, 1999; Ereshefsky, 2001; contra Hull, 1978, 1999b, who argues that only "classes" of individuals exist in nature), since monistic accounts of species and speciation are fundamentally flawed in many respects. This has importance for the methodology of physical anthropology and all other disciplines that deal with both morphological variation of hard tissues such as bone or fossilized bone, as well as genetics.

Craniometric analyses (such as those presented in Chapter 5) are a mainstay of anthropological taxonomic and variation studies, but serve as only one type of evidence. Morphological (phenotypic) evidence is inherently influenced by environmental factors,[7] and the discordance between genetic and morphological traits remains a concern for physical anthropologists in building (systematic) taxonomic or phylogenetic models, especially when relating prehistoric and historic populations to living populations (Howells, 1976; Cavalli-Sforza & Cavalli-Sforza, 1995; Relethford, 2002).[8] Non-metric

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[7] The general genetic principle of phenotypic expression is $P = G + E$, where P is the resulting phenotype, G is the genotype, and E is environmental influence (Ridley, 2004:229).

[8] Many taxonomic studies, particularly those using museum samples, which are generally not well dated, have indiscriminately mixed groups from various time periods as in the example of the Fordisc software

(epigenetic) skeletal traits,[9] which are non-functional morphological features, have been studied extensively in terms of their frequency of occurrence in various populations. With more than 200 epigenetic traits described for the human skull, many of which are expressed in various degrees, craniometric data in particular (and osteometric data in general) remains a more easily collected and interpreted source of biological information (Hauser and DeStefano, 1989; Ossenberg, 1977; Sjøvold, 1973; Wood-Jones, 1931–1933).

Berry and Berry (1967) argue that epigenetic variant incidences have considerable advantages over morphological measurements, while Jantz (1970) claims that metrical distances are more genetically meaningful. Corruccini (1974) suggests that both non-metric and craniometric analyses yield genetically significant and interpretable results: discrete traits have great potential as controls versus other data for determining the process and pattern of affinity, while morphology could yield valuable information about microevolution, population structure, and social biology (see also Rightmire, 1976). Hanihara et al. (2003) recently analysed the frequency distribution of 20 discrete cranial traits in 70 major human populations, finding clustering patterns similar to those based on classic genetic markers, DNA polymorphisms, and craniometrics, and that such trait distributions exist in clinal patterns. However, in relation to taxonomic issues, the authors suggest that when more than one model of explaining population relationships is applied

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discussed in Chapter 4. Taxonomic studies need to account for variation through time if they are to increase in accuracy, and this can only be achieved by using various sets of contemporaneous reference samples. In dealing with historic and prehistoric samples, this may depend on the feasibility of various dating techniques.

[9] Morphological traits are generally considered *continuous* variables while non-metric or epigenetic traits are considered *discrete* variables by physical anthropologists and analysed as binary or ordinal variables.

to the data and the models are compared statistically, satisfactory solutions to phylogenetic problems might be possible (see also Spuhler, 1972).

Even in light of this evidence, the relationship between osteometric and epigenetic traits remains debatable. Moore (1994:939) argues that similarities found between different data sets may merely be a product of using the same graphical techniques to display qualititatively different data sets that may have been collected and organized by different methods. Mishler (1994:148, 153) suggests that cladistic analyses would benefit from the inclusion of all appropriate data, regardless of one's own preference for, or expertise in, morphological or molecular variation, since the principle of parsimony[10] works best with copious, independent, historically informative characters.[11] Craniometric and cranial epigenetic traits have been compared in numerous studies, but are always analysed as separate data sets.

According to Kaufman and Rousseeuw (1990:32–37), mixing variables such as discrete binary and ordinal variables with continuous variables is not only possible, but practical, forming a single data set that can then be subjected to various types of cluster analyses. No examples of such multi-data analysis could be found within the physical anthropological literature, however. This may be one possible (at least preliminary) solution to the phenotype/genotype conundrum, since both data sets contain biologically useful information. While many researchers believe that the future of evolutionary studies is in genetic and genomic research, Marks (1994:70) explains, "Genetics provide the

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[10] Parsimony: a principle of phylogenetic reconstruction in which the phylogeny of a group of species is inferred to be the branching pattern requiring the smallest number of evolutionary changes (Ridley, 2004:687).

[11] Much the same can also be said for phenetic approaches, according to Brace and Hunt's (1990:344) explanation that their multivariate study of cranial morphometrics became sharper as measurements were added, regardless of their assumed adaptive significance (see Chapter 4).

mechanism of evolution, and in that sense evolutionary process has been reduced to genetics. It does not follow, though, that any data provided by genetic analysis are more fundamental, and therefore credible, in the analysis of evolutionary *patterns*."

Standard craniometric methods do provide interesting results for studying the patterns of evolution and speciation, but greater potential lies in moving beyond simple linear craniometric morphometrics into 'modern morphometrics' (Slice, 2005). Advancements in three-dimensional (3D) digital imaging provide new opportunities for progressive study of size and shape space through *geometric morphometrics*. Comparison of geometric morphometric coordinate data to traditional multivariate morphometrics show that although traditional caliper measurements can provide adequate results (Reig, 1996), 3D data produces more powerful statistics with higher correlation and lower probabilities of spurious results, providing a clearer picture of variation with more distinct clusters (McKeown and Jantz, 2005).

There remain, however, two fundamental questions to be asked regarding morphometric analyses and the relationship between continuously distributed morphological traits and their phylogenetic importance: can continuously distributed variables be used to make phylogenetic inferences, and can morphometric variables be used to test phylogenetic hypotheses (MacLeod and Forey, 2002)? Although the statistical complexity of shape distances lead Rohlf and Marcus (1993) to caution against assumptions that they can be safely used as measures of taxonomic distance, numerous recent studies attest to the *potential* for phylogenetic reconstructions using geometric morphometric data in modern humans and other hominid species. However, no studies have yet provided conclusive results. Bookstein et al. (1999) believe that refinements in methods of morphometric analysis will lead to new perspectives on phylogenetic and functional relationships among hominid species.

Geometric morphometrics yield highly visual and readily interpretable results (Collard and O'Higgins, 2001; Vidarsdottir et al., 2002), offering multivariate techniques that cover all possible shape measurements of a region separately, exploring the patterns of their correlations with all possible shape measures of other regions in one single computation. Further, such analysis allows for the comparison of two distinct integrative factors – ontogeny and phylogeny – as they apply to a shared regionalization within samples of modern *Homo sapiens* or prehistoric hominoid crania (Bookstein et al., 2003). Distinguishing between analogous and homologous characters is essential for cladistic analysis, which may provide useful information at the species level. Geometric morphometrics provides insight into the usefulness of various traits for taxonomic purposes through the analysis of allometric (size and shape covariation) and ontogenetic trajectories for determining homology (Lieberman, 2000; Humphries, 2002; Bastir and Rosas, 2004; Mitteroecker et al., 2004). Looking at homologies between species may provide the key to isolating important microevolutionary traits within species. These trajectories also give insight into the effects of age and sex related variation in population data.

Neanderthals provide an especially interesting case for studying inter and intra-species variation, since it is only recently that it has been demonstrated both (geometric) morphometrically (Harvati, 2004; Harvati et al., 2004) and through mitochondrial DNA

(Serre et al., 2004; Currat and Excoffier, 2004)[12] that the Neanderthals (*Homo neanderthalensis*) represent a distinct species and are not a subspecies of *Homo sapiens*, following years of bitter debate (see Chapter 2; Stringer, 2002). Such phylogenetic analysis may result in immediate practical advancements as well. The results of these analyses will be important in the context of forensic anthropology, particularly in dealing with subadult forensic remains. There is, at present, no way of reliably assigning subadult remains to ancestral groups based on skeletal morphology (Vidarsdottir et al., 2002). Bringing forensic anthropological methods into the realm of human variation studies will be a benefit not only for forensic identification purposes, but also for the perceived misrepresentation of human biological patterns by forensic anthropologists.

Andreasen seems preoccupied with what she calls folk or "common sense" racial categories, which are essentially the same as the social race categories used by forensic anthropologists, and the relationship between biological races and racism (2004:428–430; 1998:216). This should not be our concern. This dissertation has focused specifically on the biological problems of racial and subspecies classifications rather than on their sociological implications. *Neither races nor subspecies exist in humans, and should be rejected on their biological inadequacy alone.* For instance, in response to Montagu's insistence on viewing human populations as ethnic groups, Wierciński (1962:11) wrote that ethnicity is "an even more dangerous concept, opening possibilities for racist theories about the biological superiority of entire nations or nationalities." Contrary to Wierciński's protestation, if human races do not exist, then better scientific explanations of human variation and better education will assist in the eradication of simple pseudo-

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[12] Templeton (2005:52) finds the designation of Neanderthals as a separate species based on mtDNA evidence to be questionable, arguing that genetic, fossil, and archaeological data should be integrated in order to draw significant conclusions about evolutionary models.

scientific theories that have become a large part of racist rhetoric. This was Montagu's goal.

As in the example of the uncritical use of race by Chinese physical anthropologists, socio-political context is a strong factor in determining the epistemology under which racial analyses have been undertaken and in the arguments elsewhere for or against various theories and methods used in analyses and education, which may vary by continent or region (see Introduction). Race, then, remains a fixed and fluid concept: even with the rejection of race as a biological reality, anthropologists hold onto popular notions of race and racial terminology. As argued at the outset of this dissertation, race remains a methodological reality because it has not been replaced conceptually. Rather, multiple attempts to define or redefine race have failed, and will likely continue to fail in the absence of more complex models. Race remains an *a priori* assumption that humans fit into discrete groups, either phenotypically or genotypically, rather than a logical conclusion in human variation studies, although the resolution of these studies has increased.

But the purpose of anthropological classifications has traditionally been the definition of racial categories, assuming that they exist in simple geographic patterns. For instance, the results of the craniometric analyses in Chapter 5 showed 7 geographic clusters rather than the typical 3, 4, or 5-race models, but these outdated models perpetually reappear and old names remain attached to individuals based primarily on skin colour. These terms should not concern physical anthropologists; our concern should be genetics *and* morphology. Leaving behind the assumptions of the past is the first necessary step in finally ridding our discipline of the artefacts of racial thought. Those 7 geographic groupings seem to represent the "ethnic regions" referred to by Cavalli-Sforza et al. (1994), but the terminal nodes on the dendrograms are varieties in a Darwinian sense (see Chapter 2) – in the form of individual ethnic groups – with appreciable distance between each in most cases. Recent research indicates that the application of a combination of craniometric, radiographic, and epigenetic analyses can distinguish between family related skulls when the historical context is known (Zupanič Slavec, 2004). *New methodologies such as geometric morphometric analyses offer the potential to finely tune our methodology and to look at variation in a more systematic fashion, which should certainly result in more accurate results and a better understanding of both evolutionary patterns and processes.*

Accurate results will not alone solve the problem of race. Methodology must be developed on a strong theoretical basis, and as such, the methods of phylogenetic reconstruction will continue to suffer if they remain focused on unilineal patterns of evolutionary branching. Humans do not follow these simple theoretical patterns. This dissertation has argued that (a) humans follow ethnogenetic processes, and the study of these processes requires a much higher level of theoretical sophistication to decode the multiple and interlocking causation of patterns by which humans have grouped together – factors such as language, political divisions, religion, etc., not just by geography; (b) humans form ethnic groups, and such groups are fluid and historically contingent; (c) the degree to which physically or genetically distinguishable groups form is unlikely to be near a level warranting subspecies designations. In conclusion, that this fluid biosocial condition exists only in human populations is enough to dismiss the notion of race at least in reference to modern *Homo sapiens*. The main challenge it presents is that it is not simply enough to insist on the substitution of racial terminology for neutral referents, but a wholesale re-evaluation of human taxonomy may be necessary to get at the true patterns of variation.

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Definitions of Cranial Measurements

From Buikstra and Ubelaker (1994)
Reproduced with permission from the Arkansas Archaeological Survey

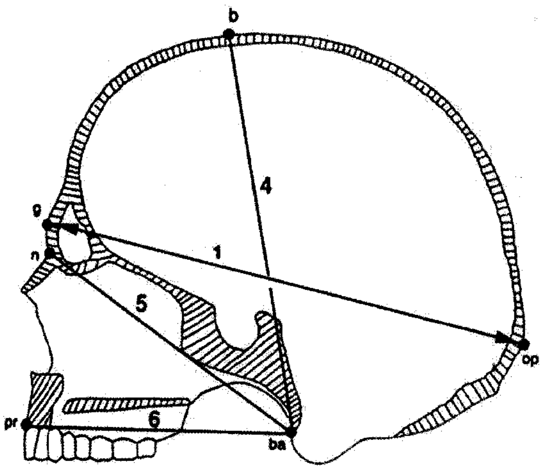
Numbers correspond to drawings

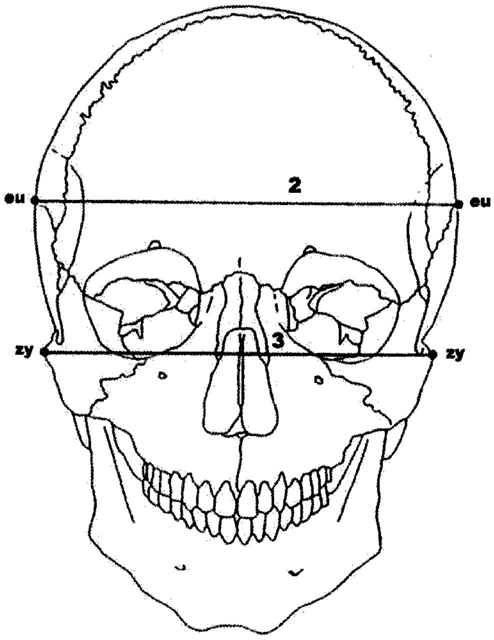
1. **GOL** – Maximum Cranial Length (g-op): distance between glabella (g) and opisthocranion (op) in the midsagittal plane, measured in a straight line. *Instrument*: spreading caliper.

2. **XCB** – Maximum Cranial Breadth (eu-eu): maximum width of skull perpendicular to midsagittal plane wherever it is located, with the exception of the inferior temporal lines and the area immediately surrounding them. *Instrument*: spreading caliper.

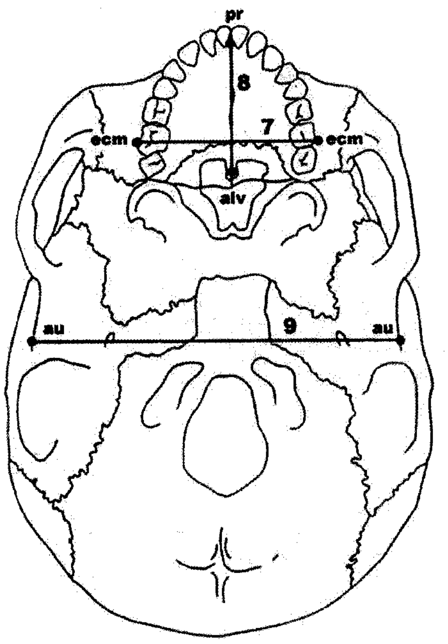
3. **ZYB** – Bizygomatic Diameter (zy-zy): direct distance between most lateral points on the zygomatic arches (zy-zy). *Instrument*: spreading or sliding caliper.

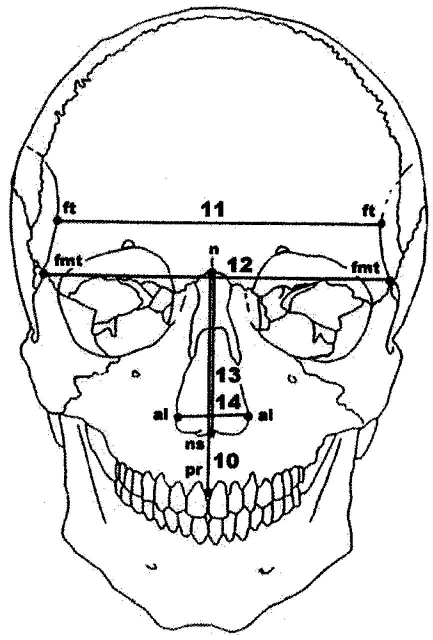
4. **BBH** – Basion-Bregma Height (ba-b): direct distance from the lowest point on the anterior margin of foramen magnum (ba), to bregma (b). *Instrument*: spreading caliper.





7. **MAB** – Maxillo-Alveolar Breadth (ecm-ecm): maximum breadth across the alveolar borders of the maxilla measured on the lateral surfaces at the location of the second maxillary molars (ecm). *Instrument*: spreading caliper.





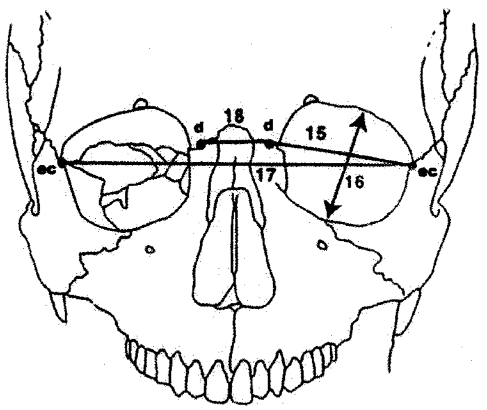
10. **NPH** – Upper Facial Height (n-pr): direct distance from nasion (n) to prosthion (pr). *Instrument*: sliding caliper.

13. **NLH** – Nasal Height (n-ns): direct distance from nasion (n) to the midpoint of a line connecting the lowest points of the inferior margin of the nasal notches (ns). *Instrument*: sliding caliper.

14. **NLB** – Nasal Breadth (al-al): maximum breadth of the nasal aperture (al-al). *Instrument*: sliding caliper.

15. **OBB** – Orbital Breadth (d-ec): laterally sloping distance from dacryon (d) to ectoconchion (ec). *Instrument*: sliding caliper.

16. **OBH** – Orbital Height: direct distance between the superior and inferior orbital margins. *Instrument*: sliding caliper.

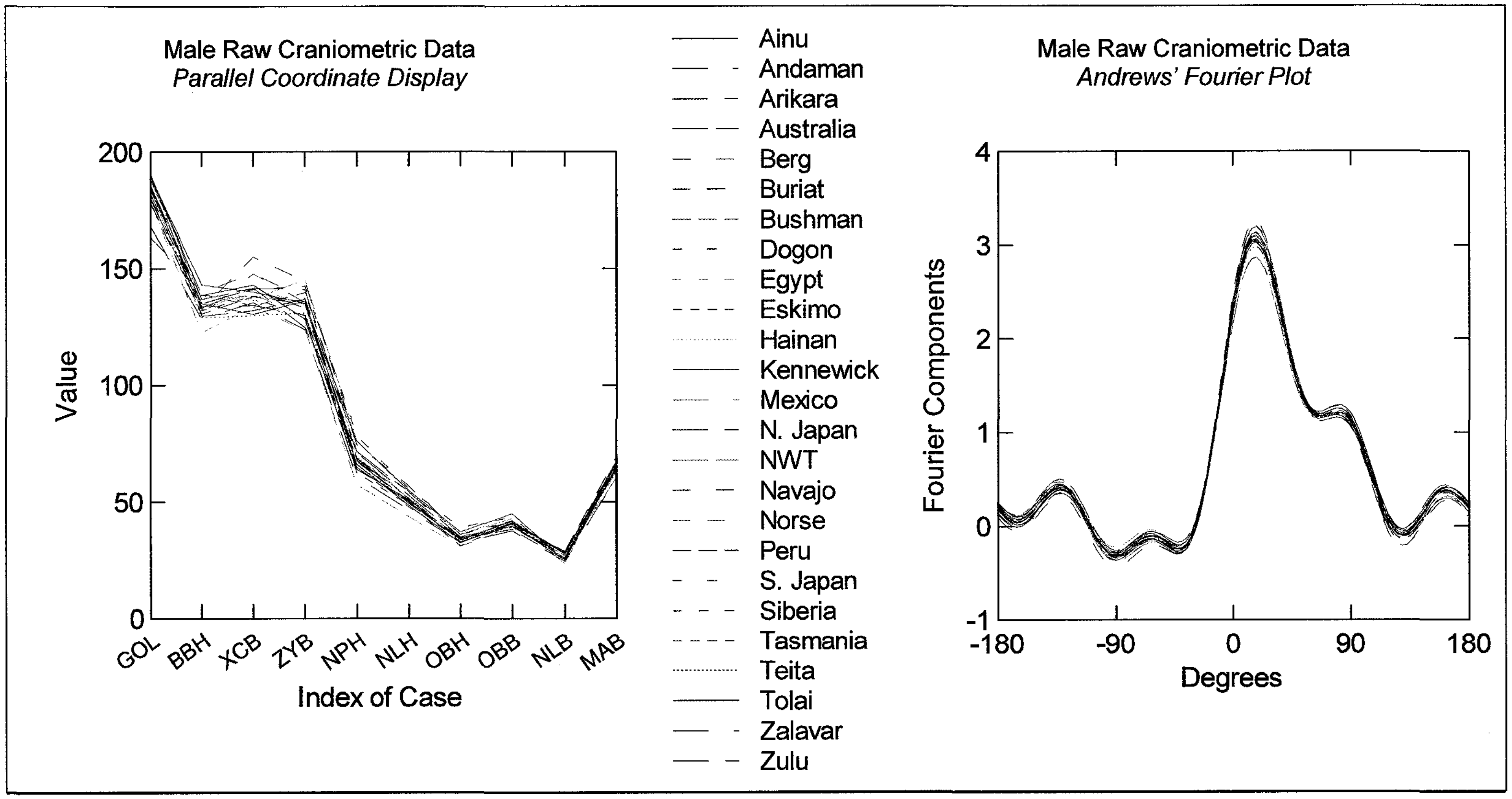


Raw Craniometric Data: Male (N=1050)

N	MALE	GOL		BBH		XCB		ZYB		NPH		NLH		OBH		OBB		NLB		MAB	
55	Norse	188.47	5.25	131.73	5.13	141.87	4.72	134.44	3.87	68.93	3.37	51.96	2.65	33.74	2.21	40.38	1.43	25.42	1.45	63.62	3.21
53	Zalavar	185.22	5.76	134.96	5.16	141.39	3.99	133.06	3.53	68.50	4.20	51.41	2.89	32.65	1.98	39.98	1.16	25.37	1.51	64.13	2.79
56	Berg	180.32	7.35	130.25	4.31	147.61	5.52	135.55	4.89	67.89	4.17	51.71	2.94	33.75	1.83	40.14	1.44	25.46	1.98	63.88	3.25
58	Egypt	185.62	6.15	133.74	5.16	139.22	4.97	128.83	4.22	68.43	3.00	51.74	2.69	32.95	2.04	39.50	1.78	24.83	1.69	62.81	3.05
33	Teita	183.88	5.09	129.03	4.75	129.85	4.26	131.00	4.12	66.00	3.94	50.09	2.80	33.29	1.85	39.65	1.53	27.91	1.80	62.71	3.58
47	Dogon	177.85	5.04	132.19	4.67	137.29	4.72	129.56	3.98	64.85	3.81	47.83	2.68	33.79	1.91	39.71	1.58	28.35	1.82	64.56	3.05
55	Zulu	185.13	5.92	133.67	5.95	134.11	5.09	129.94	4.08	67.33	4.07	50.00	2.56	33.76	1.76	40.44	1.91	28.65	1.94	65.34	3.03
41	Bushman	178.37	6.23	122.54	4.71	133.58	5.12	123.56	4.69	57.51	5.32	43.76	2.94	30.83	2.41	39.27	1.86	27.17	2.27	60.05	3.22
35	Andaman	167.81	5.72	128.23	4.62	135.38	3.60	123.69	4.32	60.69	3.17	46.54	1.73	32.58	1.17	37.54	1.55	24.50	1.24	60.15	2.09
52	Australia	190.31	5.42	129.61	5.36	131.94	5.10	136.77	4.17	64.77	4.16	49.69	2.68	33.46	1.91	41.86	1.52	27.88	1.73	66.88	3.31
45	Tasmania	185.29	5.81	131.93	4.97	138.18	5.72	135.73	5.14	62.41	4.98	48.70	3.10	31.04	2.64	40.70	1.96	28.86	2.26	67.11	2.99
56	Tolai	183.53	5.28	134.94	4.03	130.36	3.92	136.00	3.09	66.07	4.01	48.44	2.80	32.24	1.98	41.18	1.70	27.82	1.84	65.96	2.95
55	N. Japan	189.09	6.13	136.84	6.06	141.62	4.46	124.56	4.43	70.96	3.85	52.80	2.88	34.56	1.42	39.64	1.52	25.93	1.94	67.22	3.91
50	S. Japan	181.30	5.66	138.34	4.11	138.30	4.58	124.80	3.92	69.02	3.66	51.76	2.71	34.04	1.60	39.30	1.61	26.10	1.67	66.32	3.69
45	Hainan	176.30	6.14	136.71	4.47	138.44	4.26	125.55	4.63	69.69	3.76	52.36	2.53	33.60	2.13	38.67	1.64	27.31	1.87	64.80	3.33
46	Ainu	189.98	5.03	138.56	3.49	142.96	4.26	128.24	5.16	64.00	3.94	50.96	2.81	34.15	1.89	41.75	1.82	27.73	2.09	66.77	3.30
55	Buriat	181.83	6.20	132.56	5.11	154.96	6.64	144.43	5.34	74.50	4.25	56.89	3.06	35.87	1.86	41.52	1.84	28.48	2.04	68.87	3.86
14	Siberia	184.36	5.29	137.00	4.97	141.00	4.13	142.42	6.23	78.43	3.59	55.64	2.41	38.93	2.20	43.54	1.98	25.21	1.89	65.43	3.55
53	Eskimo	188.30	5.74	139.06	4.93	133.94	4.85	139.59	4.69	71.70	3.63	54.11	2.49	36.18	1.74	41.96	1.77	23.68	1.86	65.94	2.79
14	NWT	187.79	7.29	138.43	4.59	137.00	4.39	145.08	5.30	76.23	3.59	54.38	3.33	36.31	1.11	42.85	3.63	23.46	1.94	66.38	4.19
42	Arikara	179.48	5.81	133.36	4.15	141.55	5.33	140.88	5.39	71.69	3.87	54.45	2.54	34.95	1.86	40.55	1.15	27.09	1.74	66.88	2.88
19	Navajo	163.13	6.20	139.69	4.21	147.87	6.24	137.08	4.50	70.60	4.03	49.27	2.58	34.25	2.21	39.87	1.19	26.40	1.84	66.06	5.27
16	Mexico	180.19	5.96	138.15	3.65	136.31	4.98	134.20	4.87	72.75	4.82	50.38	2.85	34.56	1.86	41.06	2.64	25.88	1.93	66.00	3.95
55	Peru	177.96	5.22	130.53	5.22	137.94	3.98	134.93	4.28	67.78	3.59	50.34	2.24	34.27	1.47	38.25	1.42	25.24	1.78	64.60	3.34
	MEAN	182.15		133.84		138.86		133.33		68.36		51.05		33.99		40.39		26.45		65.10	
	STDEV	6.60		4.22		5.79		6.48		4.78		2.94		1.71		1.41		1.58		2.13	
1	Kennewick	189.00	.	143.00	.	140.00	.	135.00	.	76.00	.	55.00	.	36.00	.	45.00	.	25.00	.	66.00	.
	MEAN	182.42		134.20		138.91		133.40		68.67		51.21		34.07		40.57		26.39		65.14	
	STDEV	6.61		4.52		5.67		6.35		4.92		2.98		1.73		1.66		1.58		2.09	

**Mean values for each group are given in the left-hand column under each trait and standard deviations (stdev) are found in the right-hand column. Grand means and standard deviations for each trait are given at the bottom of each trait column, with raw and adjusted mean values for the added Kennewick specimen.*

Graphic Representation of Raw Craniometric Data: Male



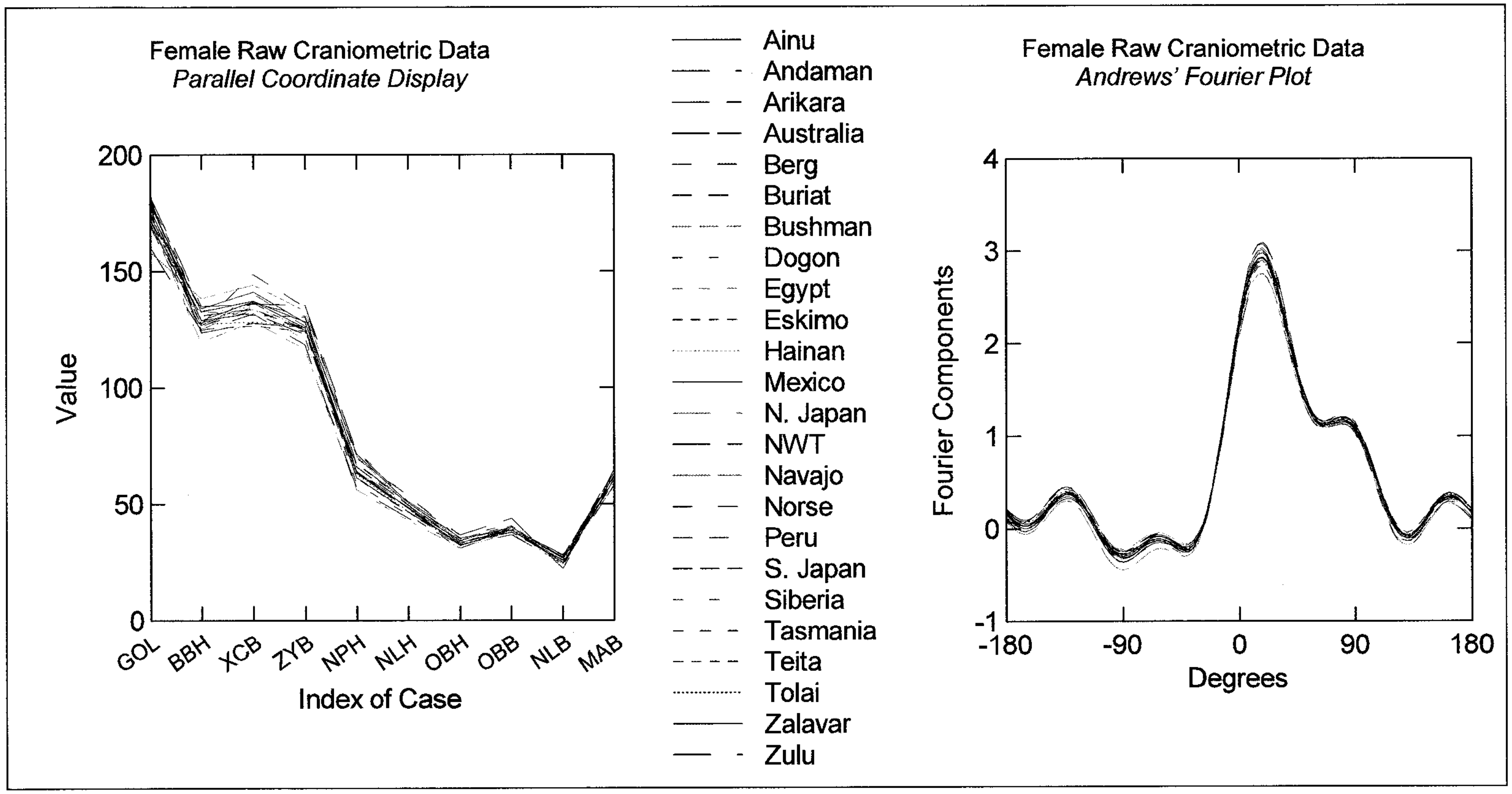
**Group means plotted in both Parallel Coordinate Display and Andrews' Fourier Plot (except for "Kennewick," which is an individual).*

Raw Craniometric Data: Female (N=977)

N	FEMALE	GOL		BBH		XCB		ZYB		NPH		NLH		OBH		OBB		NLB		MAB	
55	Norse	179.98	4.66	125.96	4.10	136.29	4.31	124.44	3.76	64.25	4.04	49.16	2.67	33.22	2.05	39.20	1.57	24.18	1.90	60.11	2.22
45	Zalavar	176.44	5.96	128.76	5.11	136.89	4.44	125.44	4.10	63.18	3.59	48.49	2.54	32.09	1.76	38.67	1.72	24.67	1.65	60.70	3.30
53	Berg	170.53	6.51	124.47	4.77	140.36	4.63	126.38	3.97	63.49	4.05	48.23	2.97	32.75	1.86	38.38	1.20	24.89	1.72	60.60	2.98
53	Egypt	175.58	4.49	127.41	3.95	135.57	4.36	120.06	3.39	64.06	3.34	48.96	2.27	32.83	1.82	37.87	1.61	24.02	1.56	59.68	2.84
50	Teita	174.72	5.09	125.12	4.10	126.46	4.33	124.24	3.95	60.94	4.22	46.42	3.21	32.26	1.88	37.86	1.64	27.26	1.94	59.28	2.51
52	Dogon	169.83	6.18	127.98	4.37	132.21	4.52	121.09	3.60	61.43	3.78	46.09	2.37	32.75	1.66	38.07	1.66	27.70	1.58	61.13	3.25
46	Zulu	179.38	5.71	128.77	5.21	131.91	5.19	122.89	3.98	63.40	4.12	47.34	2.59	32.91	2.04	39.25	1.39	27.98	1.75	62.68	3.30
49	Bushman	171.71	5.67	119.51	5.04	128.37	3.93	116.53	5.30	56.12	4.26	42.86	2.93	30.96	2.12	37.67	1.72	25.92	2.10	57.96	2.86
35	Andaman	160.61	5.26	124.36	5.15	131.61	5.17	118.18	4.62	56.68	2.93	43.79	2.06	32.39	1.52	36.61	1.20	24.07	1.65	58.29	2.26
49	Australia	181.10	6.36	123.53	4.68	127.51	4.68	125.78	4.67	61.14	3.95	46.51	2.54	33.10	1.86	39.96	1.51	26.24	1.51	62.63	2.74
42	Tasmania	177.81	5.97	126.36	5.17	127.40	5.17	125.45	4.91	58.24	3.92	46.59	2.56	30.69	1.87	39.52	1.61	27.55	1.67	63.71	3.72
54	Tolai	174.74	5.27	127.27	3.70	128.11	4.05	126.40	5.22	62.80	3.67	46.65	2.40	32.29	1.73	39.07	1.64	26.67	1.78	62.24	2.84
32	N. Japan	171.09	6.55	129.53	5.69	133.47	4.33	124.56	4.81	66.16	3.52	50.28	2.64	34.41	1.92	38.22	1.81	25.69	1.57	62.88	3.49
41	S. Japan	172.09	5.53	130.85	4.24	133.71	4.08	124.80	4.44	65.85	3.73	49.37	2.47	33.93	1.51	37.71	1.36	25.41	1.69	62.34	3.10
38	Hainan	170.63	5.67	132.13	3.69	134.97	4.81	125.55	4.72	65.39	3.89	49.32	2.78	32.84	1.81	37.61	1.50	26.03	2.06	62.34	2.12
38	Ainu	178.74	5.35	132.47	4.97	137.11	3.61	128.24	3.76	64.00	4.57	48.18	2.89	34.24	1.72	39.95	1.83	26.45	1.80	62.26	2.58
54	Buriat	171.82	5.38	127.25	4.96	148.42	5.18	134.45	4.81	69.45	4.17	53.43	2.86	34.91	1.66	39.78	1.61	26.82	1.90	64.76	3.28
17	Siberia	174.76	5.11	132.88	3.90	135.82	3.56	131.31	3.88	72.50	5.05	52.88	3.91	36.41	2.03	40.94	0.75	24.13	2.13	62.19	3.99
55	Eskimo	180.81	4.82	132.74	4.53	131.02	4.15	130.17	3.89	67.06	3.73	50.39	2.49	35.13	1.87	40.46	1.41	23.31	1.34	61.78	2.88
19	NWT	181.79	5.16	134.79	4.52	135.74	3.72	135.23	8.69	72.18	4.33	50.94	3.03	36.71	2.08	43.71	2.31	22.06	1.78	65.06	2.91
27	Arikara	171.11	6.06	126.81	4.77	136.48	4.97	130.67	4.44	67.63	3.81	50.52	2.10	34.63	1.73	39.22	1.31	25.81	1.62	62.07	2.85
10	Navajo	158.17	4.96	138.14	8.59	144.00	6.09	132.83	7.76	70.38	2.07	49.44	1.59	34.56	1.24	39.33	1.41	24.30	1.70	63.78	2.68
8	Mexico	173.29	7.41	133.86	6.31	141.00	5.45	127.80	6.14	71.25	2.66	50.38	1.19	35.38	2.45	38.63	1.19	25.63	1.92	62.50	3.16
55	Peru	169.00	5.20	124.91	4.05	134.93	4.48	125.60	4.09	63.65	3.67	47.65	2.50	34.15	1.39	36.82	1.29	23.96	1.60	61.09	3.01
	MEAN	173.57		128.58		134.56		126.17		64.63		48.49		33.56		38.94		25.45		61.75	
	STDEV	5.87		4.25		5.36		4.70		4.48		2.50		1.54		1.50		1.51		1.81	

**Mean values for each group are given in the left-hand column under each trait and standard deviations (stdev) are found in the right-hand column. Grand means and standard deviations for each trait are given at the bottom of each trait column.*

Graphic Representation of Raw Craniometric Data: Female



**Group means plotted in both Parallel Coordinate Display and Andrews' Fourier Plot.*

Definitions of Cranial Indices

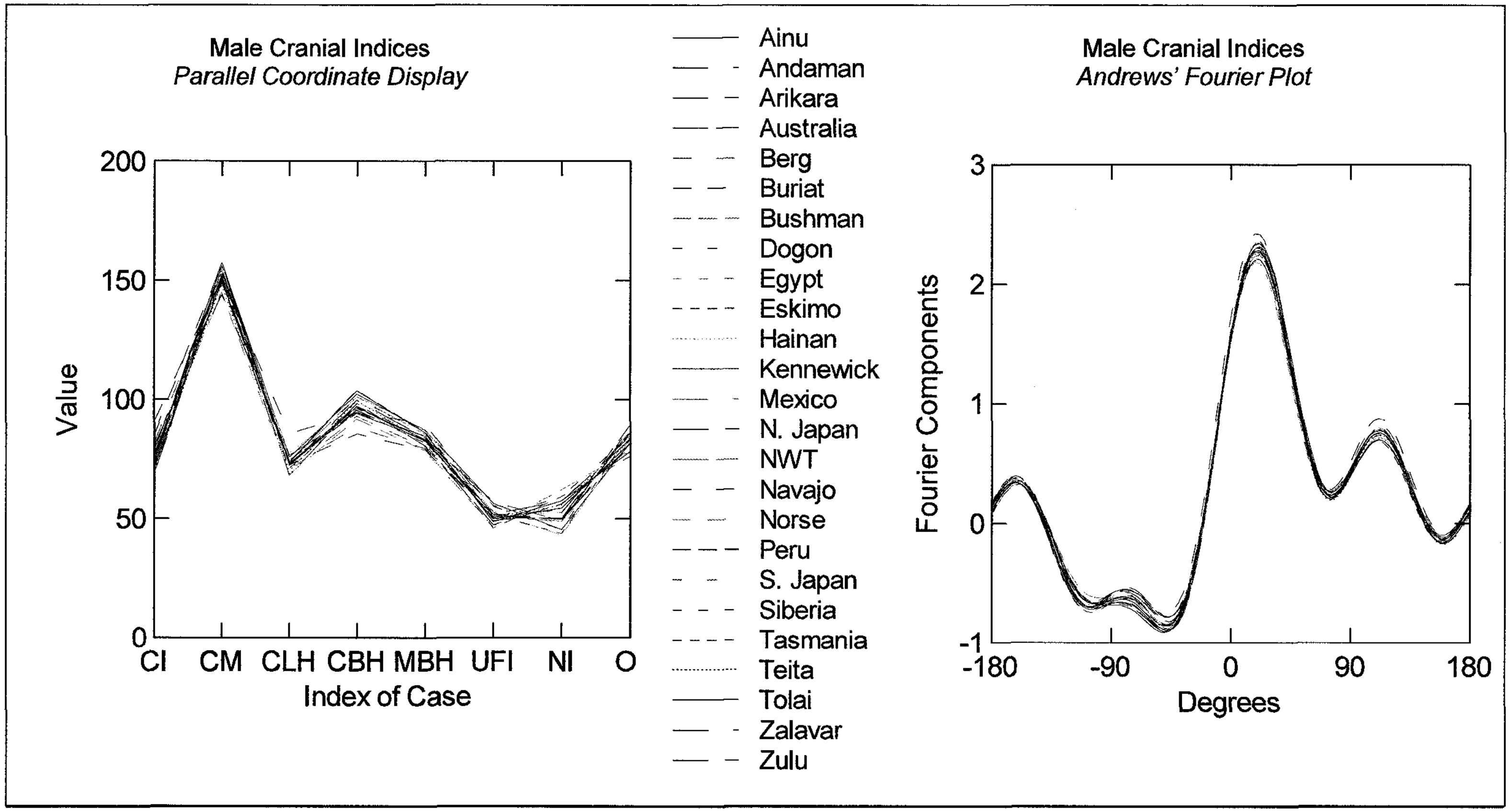
From Bass (1995)

CI – Cranial Index	$\frac{\text{maximum cranial breadth (XCB) x 100}}{\text{maximum cranial length (GOL)}}$
CM – Cranial Module	$\frac{\text{cranial length (GOL) + breadth (XCB) + height (BBH)}}{3}$
CLH – Cranial Length-Height Index	$\frac{\text{basion-bregma height (BBH) x 100}}{\text{maximum cranial length (GOL)}}$
CBH – Cranial Breadth-Height Index	$\frac{\text{basion-bregma height (BBH) x 100}}{\text{maximum cranial breadth (XCB)}}$
MBH – Mean Basion-Bregma Height	$\frac{\text{basion-bregman height (BBH) x 100}}{\text{cranial length (GOL) + breadth (XCB)/2}}$
UFI – Upper Facial Index	$\frac{\text{upper facial height (NPH) x 100}}{\text{bizygomatic breadth (ZYB)}}$
NI – Nasal Index	$\frac{\text{nasal breadth (NLB) x 100}}{\text{nasal height (NLH)}}$
OI – Orbital Index	$\frac{\text{orbit height (OBH) x 100}}{\text{orbital breadth (OBB)}}$

Craniometric Data: Male Indices

MALE	CI	CM	CLH	CBH	MBH	UFI	NI	OI
Norse	75.27	154.02	69.89	92.85	79.75	51.27	48.92	83.56
Zalavar	76.34	153.86	72.86	95.45	82.64	51.48	49.35	81.67
Berg	81.86	152.73	72.23	88.24	79.44	50.08	49.24	84.08
Egypt	75.00	152.86	72.05	96.06	82.34	53.12	47.99	83.42
Teita	70.62	147.59	70.17	99.37	82.26	50.38	55.72	83.96
Dogon	77.19	149.11	74.33	96.29	83.89	50.05	59.27	85.09
Zulu	72.44	150.97	72.20	99.67	83.74	51.82	57.30	83.48
Bushman	74.89	144.83	68.70	91.74	78.56	46.54	62.09	78.51
Andaman	80.67	143.81	76.41	94.72	84.59	49.07	52.64	86.79
Australia	69.33	150.62	68.10	98.23	80.44	47.36	56.11	79.93
Tasmania	74.57	151.80	71.20	95.48	81.57	45.98	59.26	76.27
Tolai	71.03	149.61	73.52	103.51	85.98	48.58	57.43	78.29
N. Japan	74.90	155.85	72.37	96.62	82.76	56.97	49.11	87.18
S. Japan	76.28	152.65	76.30	100.03	86.57	55.30	50.43	86.62
Hainan	78.53	150.48	77.54	98.75	86.87	55.51	52.16	86.89
Ainu	75.25	157.17	72.93	96.92	83.23	49.91	54.42	81.80
Buriat	85.22	156.45	72.90	85.54	78.72	51.58	50.06	86.39
Siberia	76.48	154.12	74.31	97.16	84.22	55.07	45.31	89.41
Eskimo	71.13	153.77	73.85	103.82	86.31	51.36	43.76	86.22
NWT	72.96	154.40	73.72	101.04	85.24	52.54	43.14	84.74
Arikara	78.87	151.46	74.30	94.21	83.08	50.89	49.75	86.19
Navajo	90.64	150.23	85.63	94.47	89.83	51.50	53.59	85.91
Mexico	75.65	151.55	76.67	101.35	87.30	54.21	51.36	84.17
Peru	77.51	148.81	73.35	94.63	82.64	50.23	50.14	89.59
MEAN	76.36	151.61	73.57	96.51	83.42	51.28	52.02	84.17
STDEV	4.76	3.31	3.50	4.32	2.88	2.80	4.93	3.39
Kennewick	74.07	157.33	75.66	102.14	86.93	56.30	45.45	82.22
MEAN	76.27	151.84	73.65	96.73	83.56	51.48	51.76	84.10
STDEV	4.69	3.43	3.45	4.38	2.90	2.91	5.00	3.34

Graphic Representation of Cranial Indices: Male

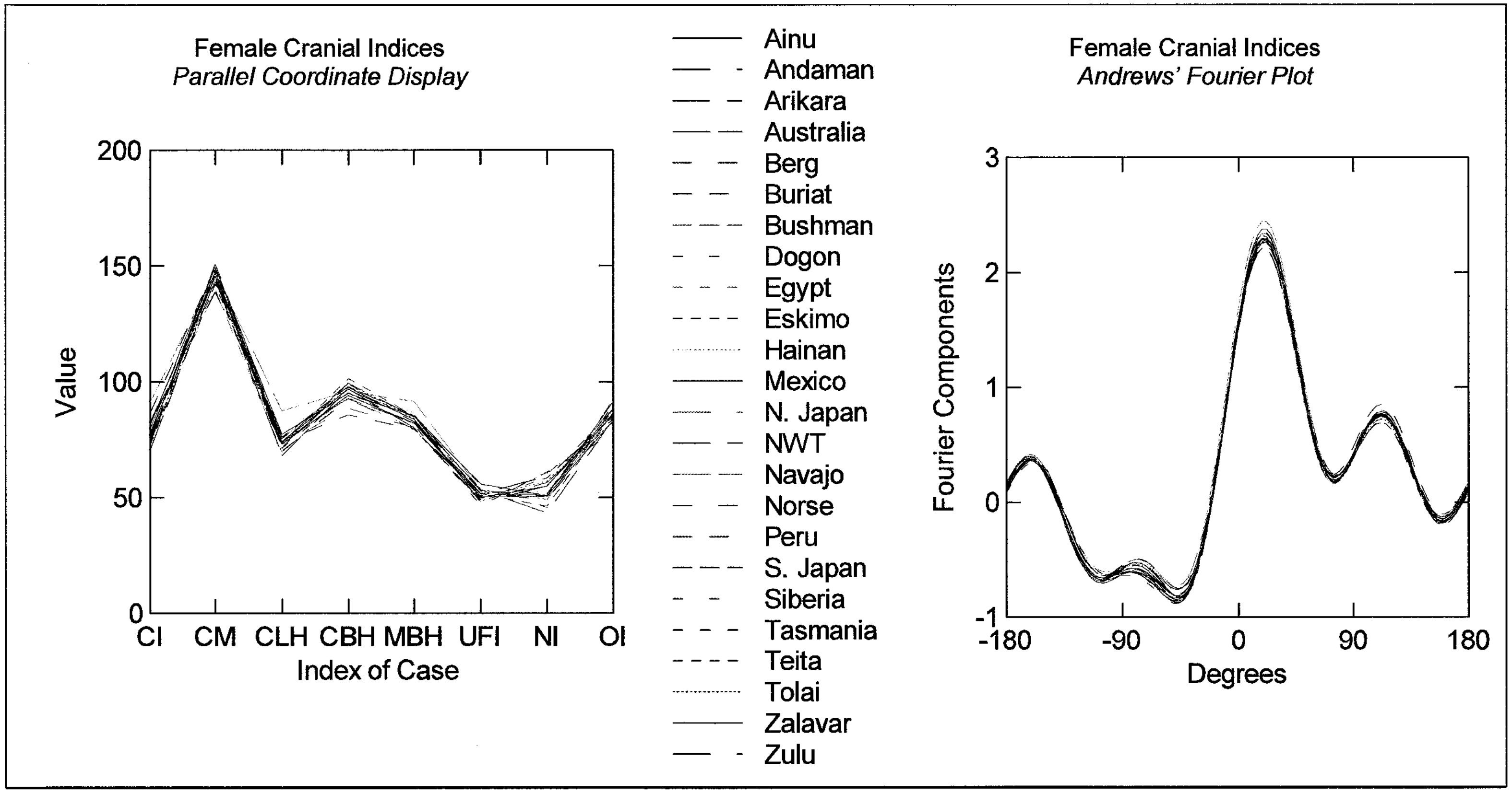


**Group means plotted for both Parallel Coordinate Display and Andrews' Fourier Plot (except for "Kennewick," which is an individual).*

Craniometric Data: Female Indices

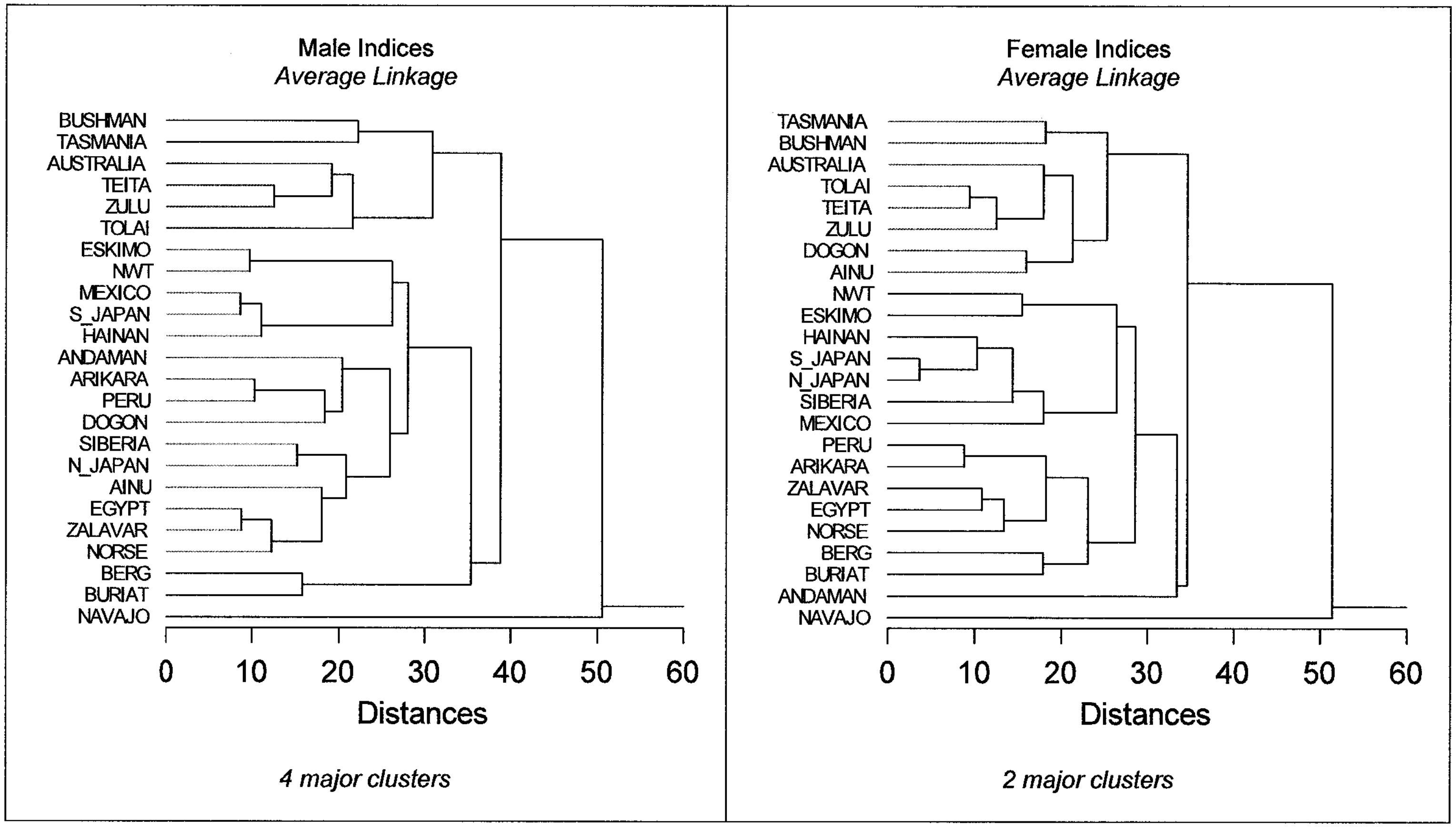
FEMALE	CI	CM	CLH	CBH	MBH	UFI	NI	OI
Norse	75.73	147.41	69.99	92.42	79.65	51.63	49.19	84.74
Zalavar	77.58	147.36	72.98	94.06	82.19	50.37	50.88	82.98
Berg	82.31	145.12	72.99	88.68	80.07	50.24	51.61	85.33
Egypt	77.21	146.19	72.57	93.98	81.90	53.36	49.06	86.69
Teita	72.37	142.01	71.62	98.96	83.10	49.12	58.54	85.25
Dogon	77.85	143.34	75.36	96.80	84.74	50.73	60.10	86.03
Zulu	73.54	146.69	71.79	97.62	82.73	51.59	59.10	83.85
Bushman	74.76	139.86	69.60	93.10	79.65	48.16	60.48	82.19
Andaman	82.20	138.69	77.67	94.49	85.26	47.96	54.97	88.47
Australia	70.41	144.05	68.21	96.88	80.06	48.61	56.42	82.83
Tasmania	74.77	145.84	71.16	95.17	81.43	46.46	60.93	77.65
Tolai	73.31	143.37	72.83	99.34	84.05	49.68	57.17	82.65
N. Japan	78.01	144.70	75.71	97.05	85.06	53.11	51.09	90.03
S. Japan	77.70	145.55	76.04	97.86	85.58	52.76	51.47	89.98
Hainan	79.10	145.91	77.44	97.90	86.47	52.08	52.78	87.32
Ainu	76.71	149.44	74.11	96.62	83.88	49.91	54.90	85.71
Buriat	86.34	149.11	74.01	85.72	79.44	51.65	50.20	87.76
Siberia	77.72	147.82	76.04	97.83	85.57	55.21	45.63	88.94
Eskimo	72.46	148.19	73.41	101.31	85.14	51.52	46.26	86.83
NWT	74.67	150.77	74.15	99.30	84.90	53.37	43.30	83.98
Arikara	79.76	144.80	74.11	92.91	82.45	51.76	51.09	88.30
Navajo	91.04	146.77	87.34	95.93	91.43	52.98	49.15	87.85
Mexico	81.37	149.38	77.25	94.93	85.18	55.75	50.87	91.59
Peru	79.84	142.95	73.91	92.57	82.20	50.68	50.28	92.47
MEAN	77.78	145.64	74.18	95.48	83.42	51.20	52.73	86.22
STDEV	4.64	2.98	3.73	3.50	2.77	2.25	4.88	3.36

Graphic Representation of Cranial Indices: Female

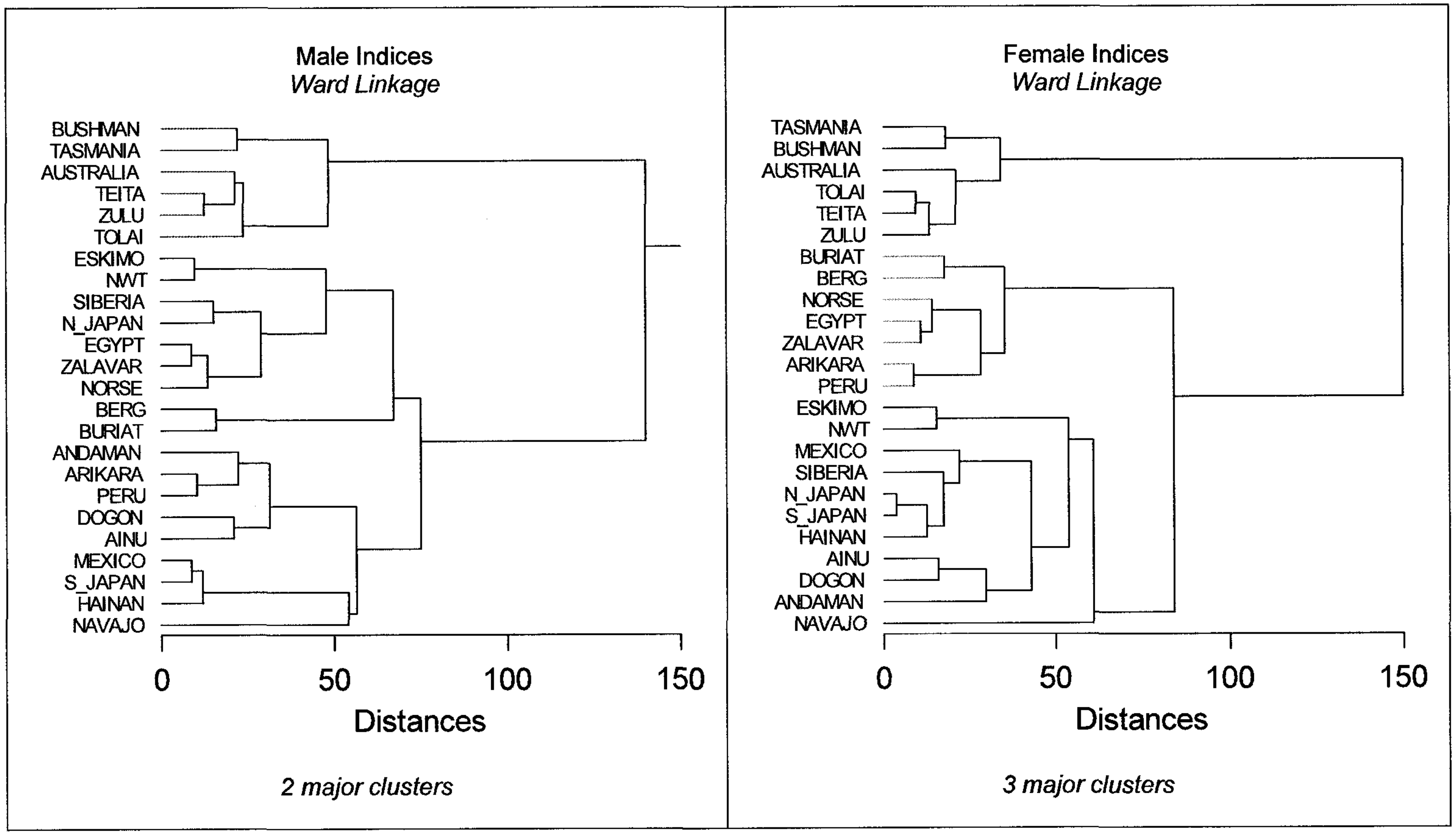


**Group means plotted for both Parallel Coordinate Display and Andrews' Fourier Plot.*

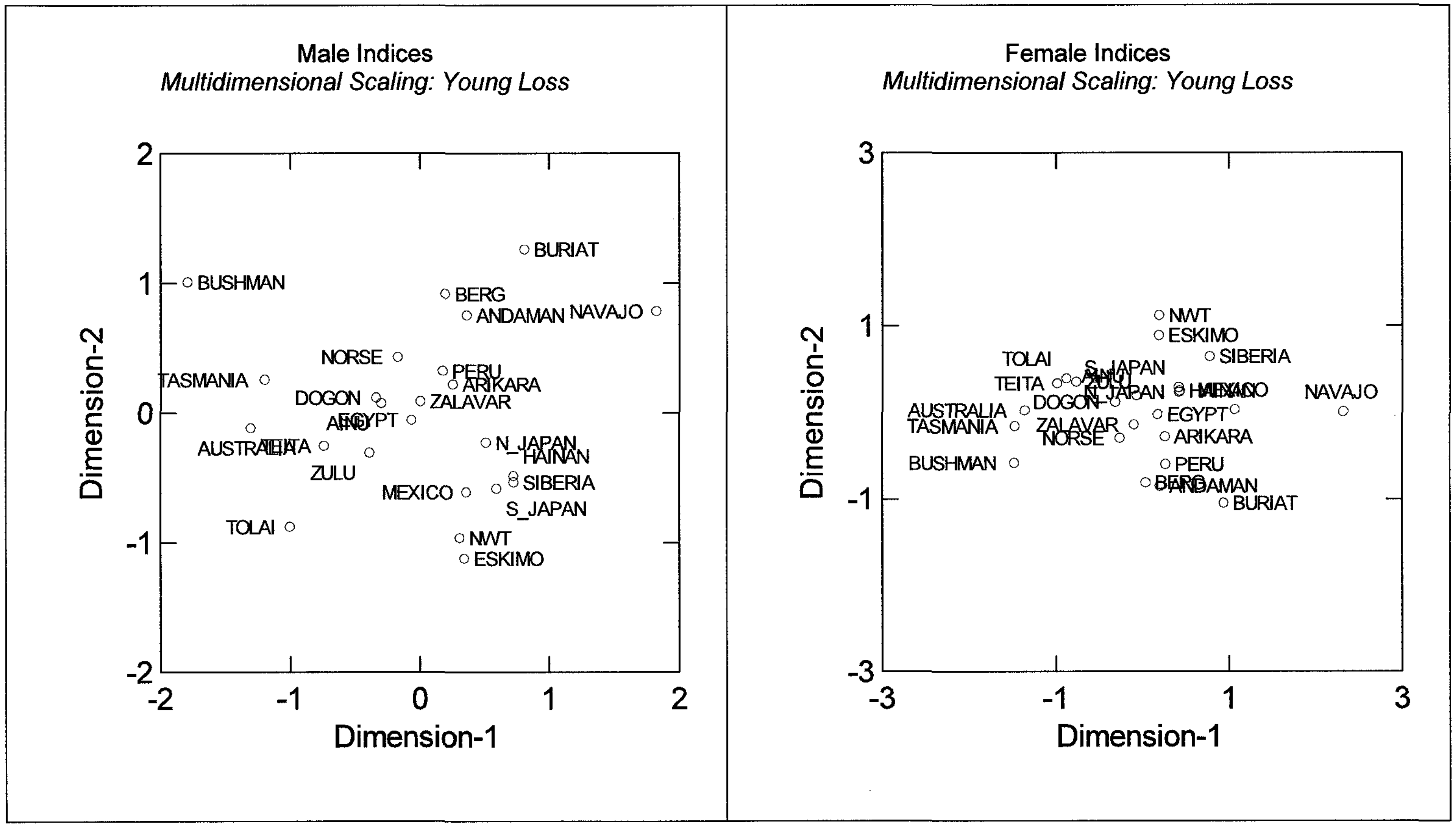
Cranial indices



Cranial indices



Cranial indices



Average Coefficient of Difference (CD) Data Matrix: Male

MALE	Norse	Zalavar	Berg	Egypt	Teita	Dogon	Zulu	Bushman	Andaman	Australia	Tasmania	Tolai
Norse	0.00											
Zalavar	1.52	0.00										
Berg	1.81	2.36	0.00									
Egypt	2.31	1.64	3.13	0.00								
Teita	4.49	4.21	4.37	3.59	0.00							
Dogon	4.81	4.36	4.19	3.96	3.05	0.00						
Zulu	3.67	3.34	4.11	3.11	2.27	2.33	0.00					
Bushman	8.70	8.11	7.96	7.17	5.70	5.49	6.77	0.00				
Andaman	9.13	8.36	8.16	6.88	7.17	6.39	8.18	4.46	0.00			
Australia	4.50	5.38	4.96	5.74	3.20	4.51	3.18	7.80	9.44	0.00		
Tasmania	4.29	4.08	4.60	4.78	4.12	3.33	2.98	6.43	8.05	2.94	0.00	
Tolai	5.13	4.39	5.17	5.26	3.04	4.07	2.91	7.53	9.16	2.56	2.63	0.00
N. Japan	3.29	3.45	4.64	3.31	5.93	5.48	4.44	8.55	9.37	6.14	5.81	6.77
S. Japan	4.09	3.51	4.22	2.80	5.07	4.13	3.68	7.35	7.93	6.39	5.48	5.76
Hainan	4.54	3.92	4.50	3.23	4.90	3.43	3.73	6.90	7.34	6.55	5.65	5.79
Ainu	4.26	4.30	5.26	4.52	5.50	4.64	3.60	9.02	10.18	3.73	4.26	5.11
Buriat	6.98	7.65	5.93	8.30	8.69	8.06	7.20	12.50	**14.64**	7.37	6.81	7.84
Siberia	6.32	6.25	7.01	7.20	9.63	9.48	7.93	**13.25**	**14.49**	8.49	8.08	8.35
Eskimo	5.01	5.67	6.29	5.86	7.55	8.16	5.78	11.64	12.29	5.46	6.79	5.98
NWT	6.11	6.57	7.19	6.77	9.01	8.80	7.28	12.66	**12.90**	7.16	7.56	7.77
Arikara	4.00	4.38	3.89	5.24	6.35	5.43	4.85	9.82	11.59	5.60	4.92	5.47
Navajo	5.57	5.11	4.06	6.22	7.29	5.89	5.99	10.11	9.25	7.06	6.30	6.27
Mexico	3.66	3.35	3.84	4.14	4.96	4.32	3.43	8.46	8.76	4.72	4.55	3.86
Peru	3.22	3.32	2.57	3.39	4.25	3.31	3.92	7.38	6.76	5.21	4.65	5.17

MALE	N. Japan	S. Japan	Hainan	Ainu	Buriat	Siberia	Eskimo	NWT	Arikara	Navajo	Mexico	Peru
Norse												
Zalavar												
Berg												
Egypt												
Teita												
Dogon												
Zulu												
Bushman												
Andaman												
Australia												
Tasmania												
Tolai												
N. Japan	0.00											
S. Japan	2.09	0.00										
Hainan	3.05	1.79	0.00									
Ainu	3.28	3.80	4.69	0.00								
Buriat	7.15	8.01	7.95	7.35	0.00							
Siberia	6.46	7.37	7.67	7.49	5.44	0.00						
Eskimo	5.09	5.87	6.67	5.75	6.24	4.46	0.00					
NWT	5.90	6.54	7.26	6.71	5.31	3.15	1.99	0.00				
Arikara	4.04	4.78	4.59	5.43	3.75	4.85	4.27	4.69	0.00			
Navajo	5.48	5.09	5.26	6.13	7.18	8.22	6.75	7.47	4.96	0.00		
Mexico	3.61	2.76	3.71	4.20	6.15	5.30	3.67	4.36	3.41	3.92	0.00	
Peru	5.19	3.79	3.50	6.12	8.14	7.91	6.50	7.08	4.82	5.19	3.06	0.00

**Average calculated by taking the sum of the correlation coefficients for each group-by-group comparison for each trait.*

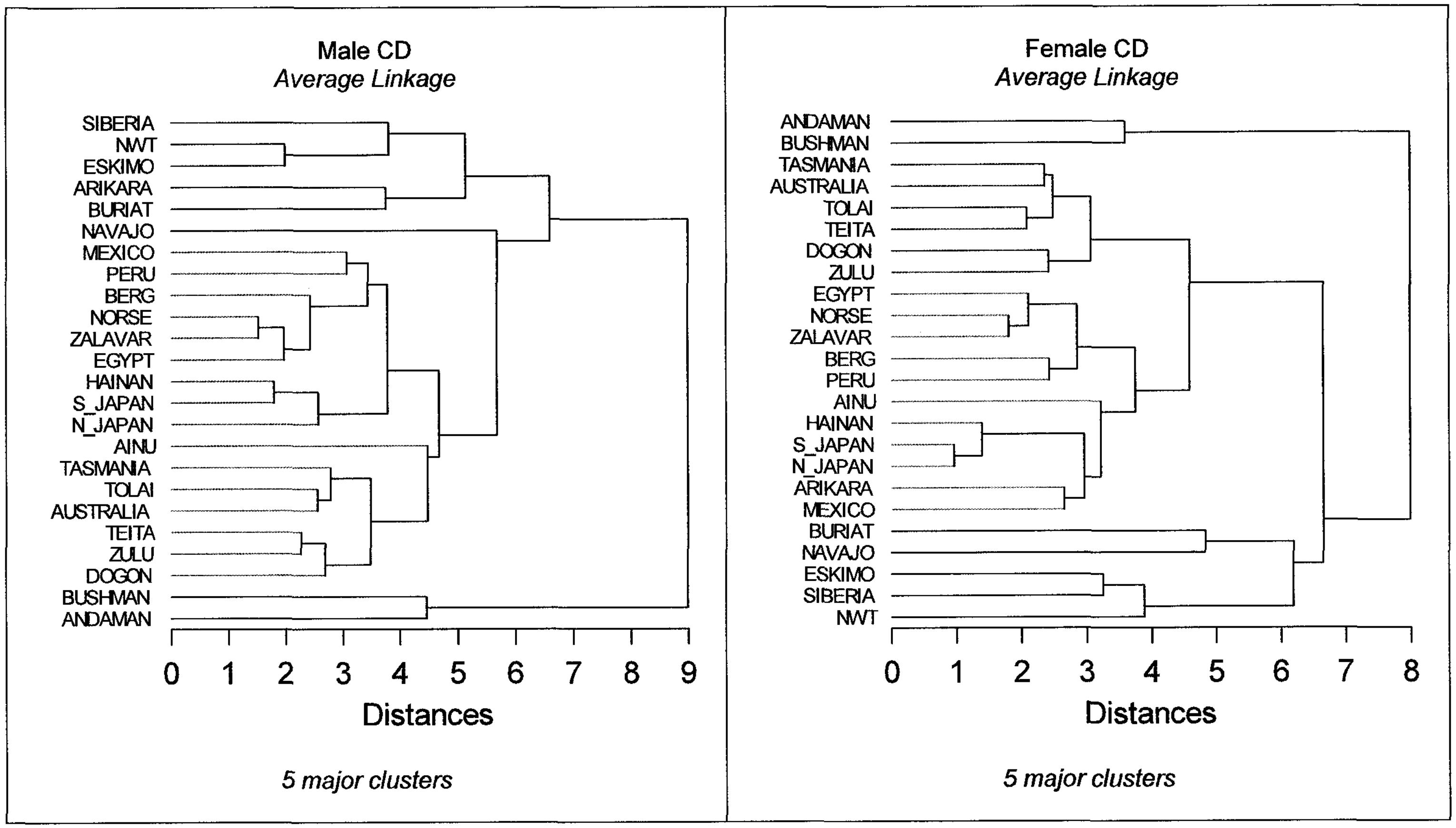
Average Coefficient of Difference (CD) Data Matrix: Female

FEMALE	Norse	Zalavar	Berg	Egypt	Teita	Dogon	Zulu	Bushman	Andaman	Australia	Tasmania	Tolai
Norse	0.00											
Zalavar	1.80	0.00										
Berg	2.69	1.86	0.00									
Egypt	2.07	2.14	3.03	0.00								
Teita	4.31	3.79	4.06	4.03	0.00							
Dogon	4.74	3.81	3.74	3.62	2.61	0.00						
Zulu	3.07	3.01	4.22	3.88	3.42	2.41	0.00					
Bushman	7.31	6.57	6.28	6.11	4.07	4.89	6.61	0.00				
Andaman	7.19	6.65	6.09	5.76	5.36	5.09	7.53	3.59	0.00			
Australia	3.80	4.13	4.43	5.46	2.90	4.01	2.73	5.67	7.32	0.00		
Tasmania	4.84	4.30	5.36	5.93	2.73	3.79	2.81	5.04	7.22	2.35	0.00	
Tolai	3.96	2.89	3.65	4.17	2.08	2.83	2.27	5.40	6.88	2.08	2.22	0.00
N. Japan	3.47	3.15	3.31	3.56	4.66	3.70	3.44	6.87	7.51	4.63	5.38	3.78
S. Japan	3.43	2.94	3.35	3.19	4.60	3.81	3.65	6.90	7.42	4.92	5.68	3.86
Hainan	3.74	2.76	2.96	3.28	4.60	3.41	3.54	6.87	7.30	4.75	5.30	3.56
Ainu	3.21	2.87	3.89	4.59	5.65	5.08	3.00	8.61	9.38	3.68	4.74	3.68
Buriat	6.98	6.87	6.06	7.92	8.56	7.90	6.94	11.18	12.31	7.61	7.85	6.86
Siberia	5.71	5.65	6.87	6.63	8.79	8.47	6.95	11.29	11.83	7.48	8.36	6.93
Eskimo	4.35	5.04	6.35	5.93	7.54	7.58	5.29	10.18	10.57	5.37	6.97	5.78
NWT	6.95	7.50	8.90	8.23	10.94	10.41	8.11	**13.14**	**13.51**	8.69	9.73	9.28
Arikara	3.65	3.82	3.57	4.75	6.01	5.37	4.69	8.32	9.28	5.24	6.09	4.26
Navajo	7.00	6.82	6.06	7.82	10.12	8.87	8.00	12.24	10.82	8.97	9.71	8.40
Mexico	5.01	4.47	4.15	5.59	7.17	6.75	5.67	9.85	11.05	6.82	7.59	5.85
Peru	3.27	3.11	2.42	3.17	4.46	4.01	4.83	6.68	5.21	5.04	6.14	4.42

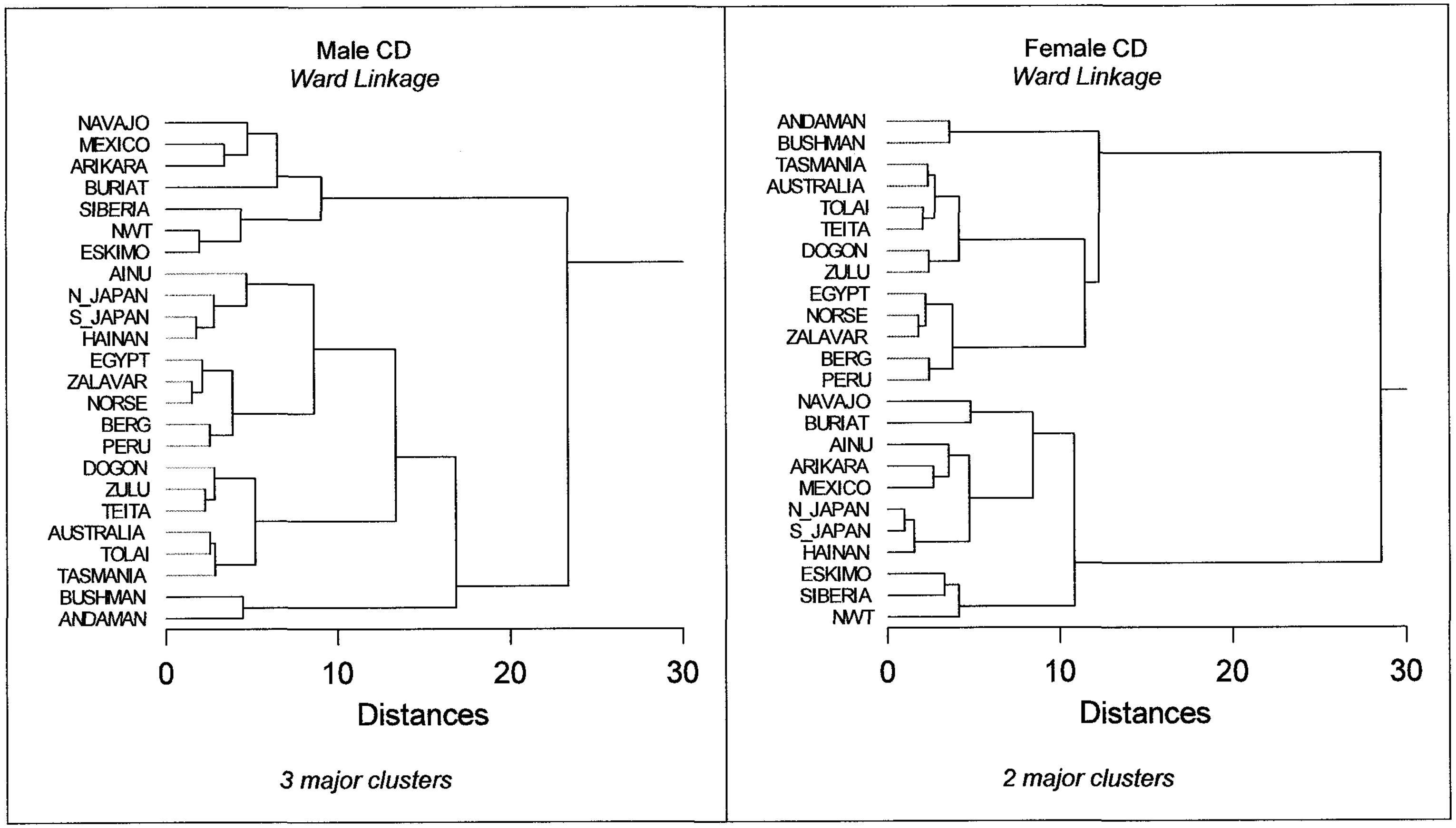
FEMALE	N. Japan	S. Japan	Hainan	Ainu	Buriat	Siberia	Eskimo	NWT	Arikara	Navajo	Mexico	Peru
Norse												
Zalavar												
Berg												
Egypt												
Teita												
Dogon												
Zulu												
Bushman												
Andaman												
Australia												
Tasmania												
Tolai												
N. Japan	0.00											
S. Japan	0.96	0.00										
Hainan	1.66	1.11	0.00									
Ainu	3.30	3.20	2.94	0.00								
Buriat	5.08	6.04	6.13	5.49	0.00							
Siberia	4.96	5.46	5.47	4.15	5.05	0.00						
Eskimo	4.12	4.53	4.96	3.54	6.54	3.25	0.00					
NWT	6.71	7.30	7.44	5.96	6.79	3.47	4.30	0.00				
Arikara	2.04	2.94	3.11	3.09	3.51	3.90	3.78	6.29	0.00			
Navajo	5.31	5.75	5.85	5.97	4.82	5.57	6.30	6.88	4.47	0.00		
Mexico	2.86	3.36	3.44	3.54	4.21	3.63	4.25	5.23	2.65	3.73	0.00	
Peru	3.15	2.94	3.14	4.48	7.41	7.10	6.11	8.78	4.14	6.85	5.72	0.00

**Average calculated by taking the sum of the correlation coefficients for each group-by-group comparison for each trait.*

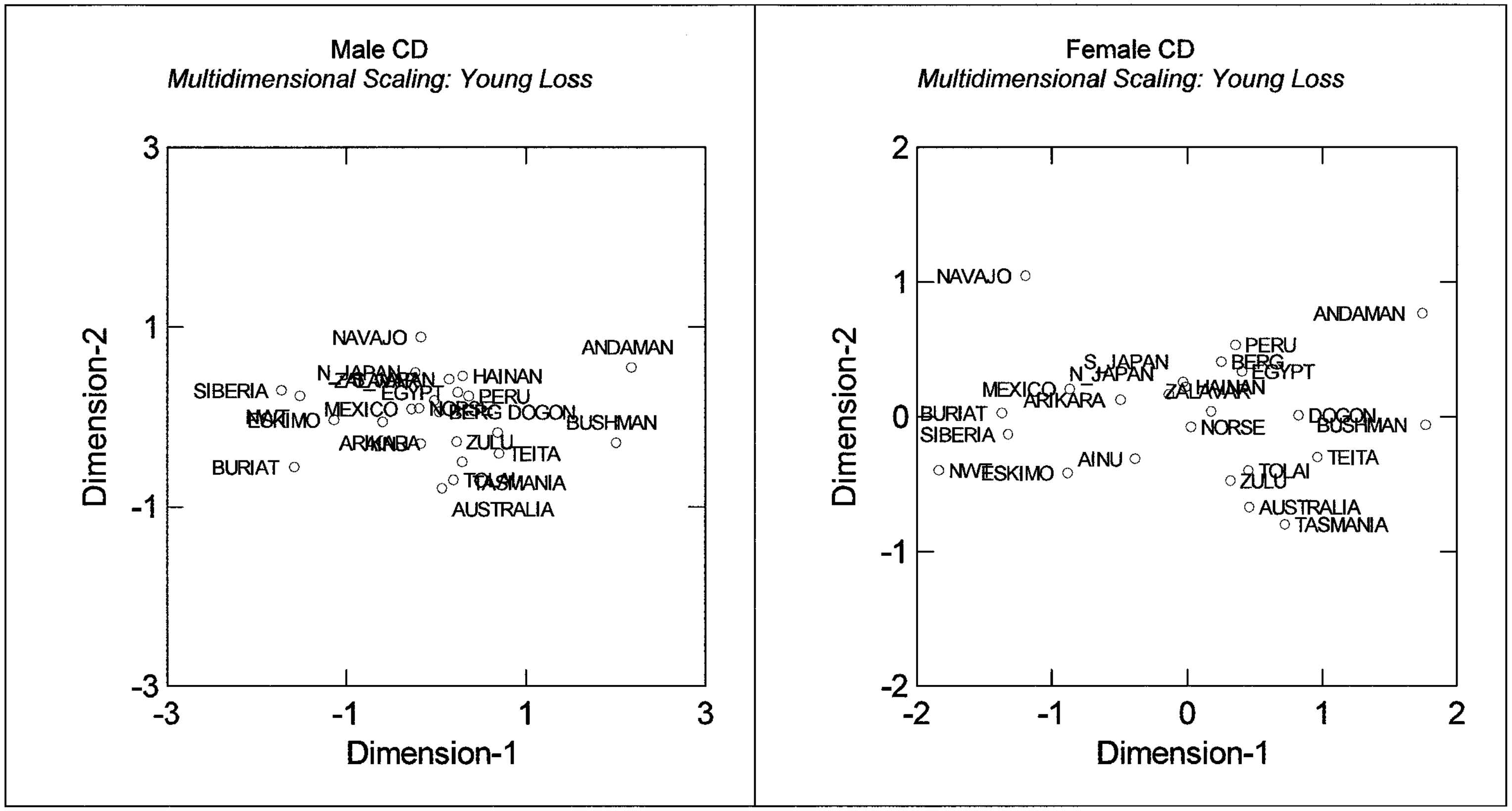
Average Coefficient of Difference (CD)



Average Coefficient of Difference (CD)



Average Coefficient of Difference (CD)



Adapted Penrose's Shape Distance (SHPD) Data Matrix: Male

MALE	Norse	Zalavar	Berg	Egypt	Teita	Dogon	Zulu	Bushman	Andaman	Australia	Tasmania	Tolai
Norse	0.000											
Zalavar	0.028	0.000										
Berg	0.079	0.098	0.000									
Egypt	0.046	0.032	0.141	0.000								
Teita	0.233	0.268	0.366	0.224	0.000	0.000						
Dogon	0.281	0.217	0.239	0.246	0.147	0.000						
Zulu	0.217	0.197	0.306	0.170	0.033	0.063	0.000					
Bushman	0.287	0.282	0.302	0.321	0.176	0.102	0.116	0.000				
Andaman	0.237	0.165	0.144	0.200	0.331	0.092	0.193	0.276	0.000			
Australia	0.277	0.335	0.405	0.358	0.109	0.272	0.140	0.198	0.487	0.000		
Tasmania	0.259	0.223	0.277	0.318	0.179	0.144	0.138	0.132	0.356	0.109	0.000	
Tolai	0.345	0.332	0.478	0.369	0.126	0.216	0.129	0.309	0.455	0.089	0.114	0.000
N. Japan	0.211	0.189	0.303	0.081	0.343	0.295	0.218	0.352	0.233	0.526	0.430	0.603
S. Japan	0.280	0.196	0.349	0.109	0.318	0.213	0.171	0.407	0.150	0.544	0.410	0.497
Hainan	0.307	0.212	0.304	0.152	0.300	0.177	0.173	0.406	0.152	0.570	0.388	0.473
Ainu	0.233	0.177	0.324	0.172	0.319	0.188	0.146	0.216	0.211	0.313	0.188	0.373
Buriat	0.245	0.263	0.061	0.314	0.448	0.350	0.448	0.448	0.272	0.528	0.370	0.582
Siberia	0.304	0.333	0.352	0.317	0.470	0.552	0.506	0.661	0.487	0.614	0.661	0.570
Eskimo	0.245	0.282	0.401	0.242	0.352	0.536	0.390	0.669	0.521	0.389	0.517	0.350
NWT	0.326	0.373	0.426	0.366	0.504	0.650	0.551	0.794	0.608	0.527	0.641	0.499
Arikara	0.167	0.152	0.105	0.197	0.236	0.231	0.261	0.403	0.185	0.364	0.288	0.313
Navajo	0.656	0.481	0.330	0.572	0.835	0.460	0.638	0.772	0.287	0.982	0.648	0.730
Mexico	0.189	0.124	0.262	0.137	0.220	0.173	0.150	0.395	0.146	0.316	0.294	0.179
Peru	0.134	0.129	0.123	0.155	0.249	0.187	0.218	0.373	0.139	0.377	0.291	0.335

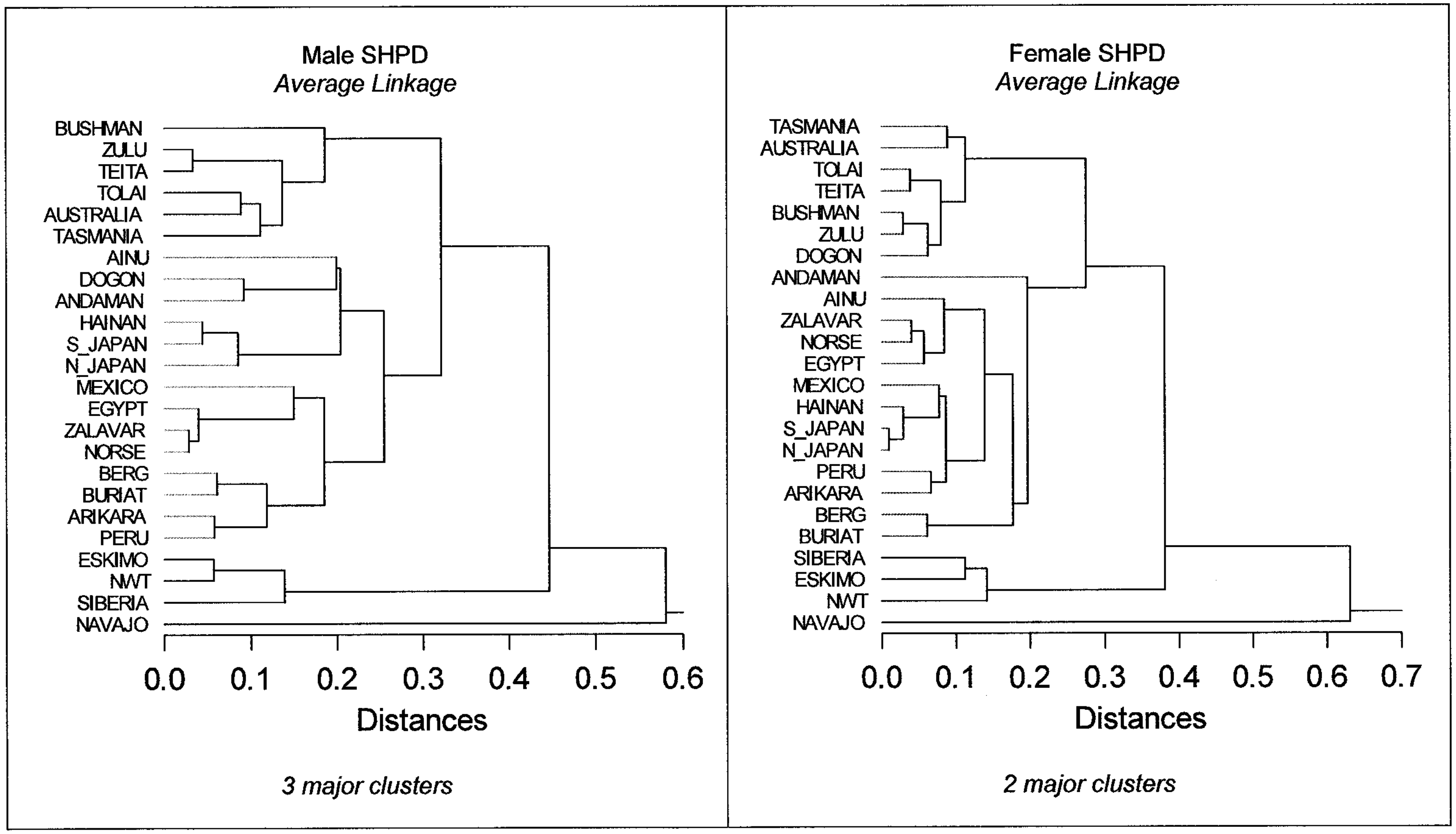
MALE	N. Japan	S. Japan	Hainan	Ainu	Buriat	Siberia	Eskimo	NWT	Arikara	Navajo	Mexico	Peru
Norse												
Zalavar												
Berg												
Egypt												
Teita												
Dogon												
Zulu												
Bushman												
Andaman												
Australia												
Tasmania												
Tolai												
N. Japan	0.000											
S. Japan	0.048	0.000										
Hainan	0.123	0.044	0.000									
Ainu	0.171	0.176	0.271	0.000								
Buriat	0.496	0.502	0.367	0.568	0.000							
Siberia	0.492	0.459	0.420	0.691	0.398	0.000						
Eskimo	0.436	0.420	0.475	0.498	0.522	0.165	0.000					
NWT	0.559	0.569	0.556	0.699	0.484	0.113	0.057	0.000				
Arikara	0.359	0.318	0.223	0.444	0.083	0.238	0.283	0.264	0.000			
Navajo	0.725	0.549	0.412	0.791	0.342	0.640	0.764	0.646	0.353	0.000		
Mexico	0.231	0.146	0.151	0.287	0.358	0.185	0.164	0.192	0.157	0.335	0.000	
Peru	0.290	0.254	0.211	0.374	0.164	0.273	0.269	0.257	0.058	0.388	0.108	0.000

Adapted Penrose's Shape Distance (SHPD) Data Matrix: Female

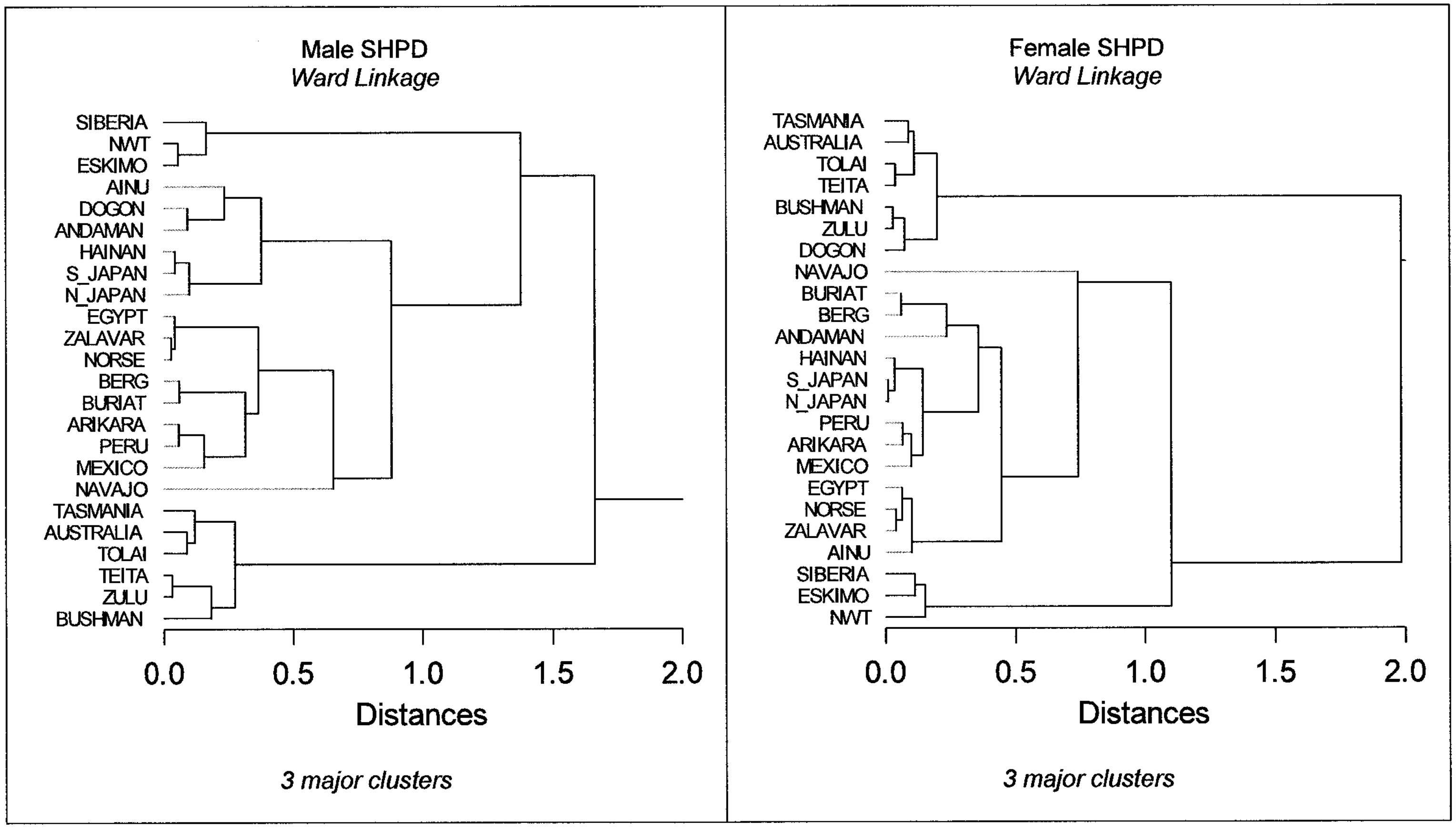
FEMALE	Norse	Zalavar	Berg	Egypt	Teita	Dogon	Zulu	Bushman	Andaman	Australia	Tasmania	Tolai
Norse	0.000											
Zalavar	0.040	0.000										
Berg	0.115	0.062	0.000									
Egypt	0.055	0.060	0.141	0.000								
Teita	0.215	0.196	0.296	0.268	0.000							
Dogon	0.270	0.194	0.229	0.241	0.099	0.000						
Zulu	0.175	0.152	0.252	0.194	0.048	0.050	0.000					
Bushman	0.178	0.179	0.244	0.232	0.084	0.076	0.029	0.000				
Andaman	0.303	0.175	0.125	0.252	0.252	0.090	0.223	0.198	0.000			
Australia	0.213	0.229	0.343	0.334	0.100	0.220	0.092	0.097	0.353	0.000		
Tasmania	0.328	0.267	0.393	0.426	0.103	0.181	0.084	0.109	0.387	0.089	0.000	
Tolai	0.219	0.165	0.273	0.258	0.039	0.100	0.049	0.101	0.227	0.070	0.077	0.000
N. Japan	0.153	0.115	0.135	0.096	0.175	0.145	0.169	0.228	0.146	0.260	0.348	0.153
S. Japan	0.167	0.099	0.148	0.090	0.184	0.141	0.176	0.243	0.128	0.311	0.370	0.164
Hainan	0.225	0.088	0.143	0.132	0.200	0.110	0.170	0.265	0.125	0.352	0.318	0.164
Ainu	0.088	0.041	0.115	0.125	0.141	0.117	0.100	0.110	0.102	0.166	0.206	0.115
Buriat	0.324	0.236	0.061	0.329	0.496	0.416	0.491	0.470	0.259	0.572	0.620	0.478
Siberia	0.225	0.233	0.305	0.283	0.388	0.443	0.422	0.429	0.423	0.417	0.566	0.316
Eskimo	0.170	0.208	0.368	0.260	0.344	0.560	0.412	0.418	0.478	0.314	0.515	0.281
NWT	0.295	0.354	0.495	0.345	0.597	0.713	0.604	0.564	0.603	0.517	0.746	0.480
Arikara	0.164	0.130	0.073	0.195	0.219	0.258	0.277	0.288	0.209	0.295	0.413	0.198
Navajo	0.828	0.573	0.351	0.688	0.927	0.669	0.887	0.959	0.583	1.044	1.189	0.803
Mexico	0.183	0.159	0.119	0.115	0.348	0.315	0.329	0.471	0.404	0.515	0.640	0.357
Peru	0.178	0.133	0.090	0.154	0.281	0.254	0.306	0.303	0.133	0.373	0.512	0.262

FEMALE	N. Japan	S. Japan	Hainan	Ainu	Buriat	Siberia	Eskimo	NWT	Arikara	Navajo	Mexico	Peru
Norse												
Zalavar												
Berg												
Egypt												
Teita												
Dogon												
Zulu												
Bushman												
Andaman												
Australia												
Tasmania												
Tolai												
N. Japan	0.000											
S. Japan	0.010	0.000										
Hainan	0.037	0.021	0.000									
Ainu	0.110	0.094	0.097	0.000								
Buriat	0.234	0.272	0.238	0.340	0.000							
Siberia	0.165	0.240	0.276	0.210	0.373	0.000						
Eskimo	0.232	0.242	0.301	0.212	0.578	0.112	0.000					
NWT	0.373	0.393	0.447	0.358	0.672	0.166	0.117	0.000				
Arikara	0.061	0.092	0.120	0.137	0.104	0.138	0.249	0.387	0.000			
Navajo	0.368	0.412	0.364	0.632	0.309	0.456	0.800	0.754	0.317	0.000		
Mexico	0.085	0.069	0.079	0.184	0.183	0.215	0.285	0.391	0.090	0.214	0.000	
Peru	0.060	0.065	0.114	0.133	0.142	0.305	0.297	0.402	0.066	0.380	0.091	0.000

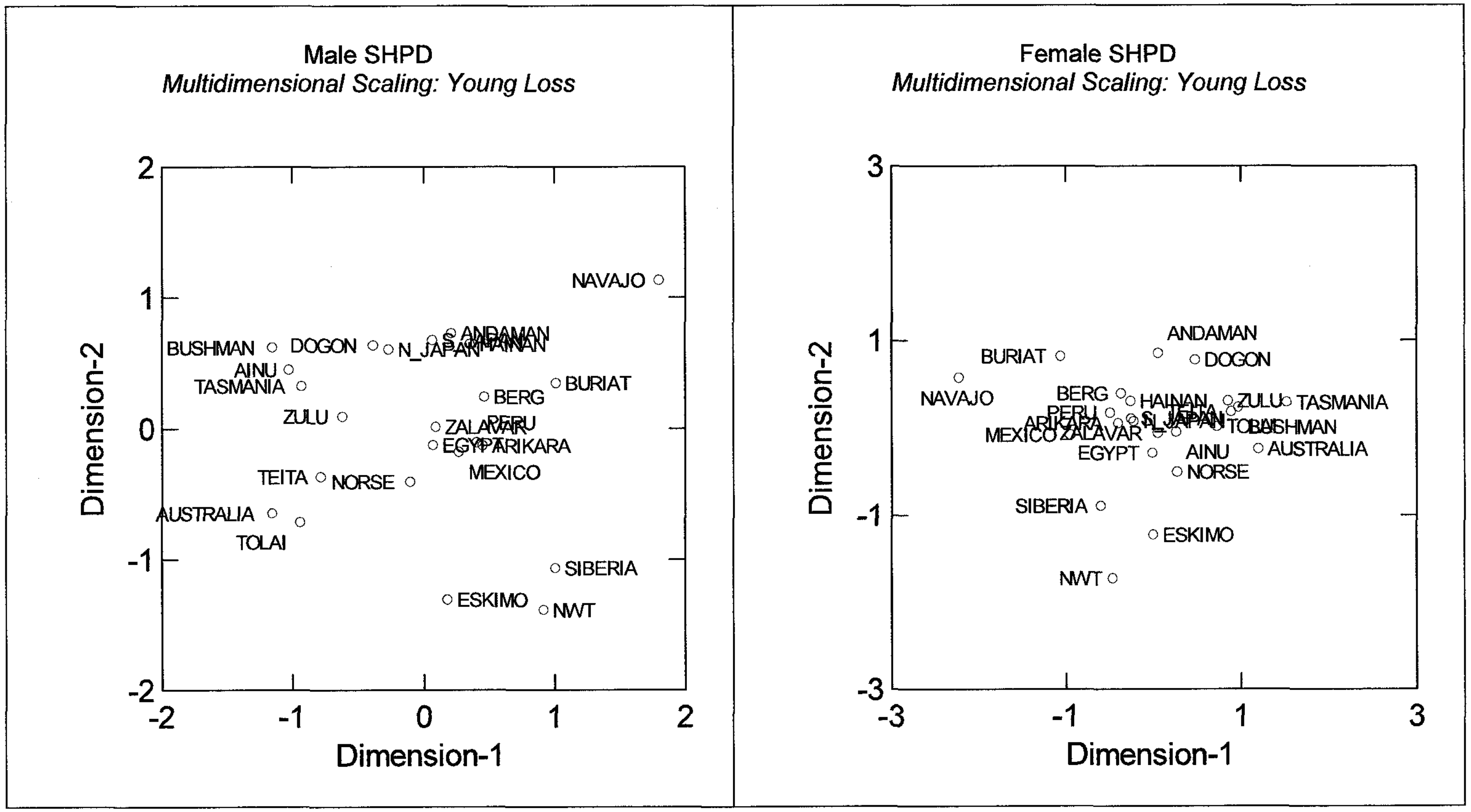
Adapted Penrose's Shape Distance (SHPD)



Adapted Penrose's Shape Distance (SHPD)



Adapted Penrose's Shape Distance (SHPD)



Adapted Penrose's Mean Square Distance (MSD) Data Matrix: Male

MALE	Norse	Zalavar	Berg	Egypt	Teita	Dogon	Zulu	Bushman	Andaman	Australia	Tasmania	Tolai
Norse	0.000											
Zalavar	0.034	0.000										
Berg	0.080	0.100	0.000									
Egypt	0.083	0.049	0.162	0.000								
Teita	0.321	0.323	0.428	0.235	0.000							
Dogon	0.349	0.251	0.279	0.250	0.149	0.000						
Zulu	0.224	0.197	0.308	0.180	0.072	0.091	0.000					
Bushman	0.889	0.795	0.814	0.680	0.417	0.394	0.574	0.000				
Andaman	1.070	0.864	0.810	0.673	0.662	0.500	0.814	0.277	0.000			
Australia	0.277	0.341	0.406	0.388	0.183	0.332	0.145	0.758	1.236	0.000		
Tasmania	0.266	0.224	0.279	0.325	0.213	0.167	0.139	0.545	0.880	0.115	0.000	
Tolai	0.361	0.335	0.486	0.374	0.157	0.235	0.130	0.774	1.034	0.101	0.115	0.000
N. Japan	0.212	0.200	0.308	0.135	0.455	0.383	0.235	0.985	1.111	0.529	0.452	0.635
S. Japan	0.296	0.200	0.353	0.115	0.350	0.232	0.172	0.871	0.779	0.555	0.410	0.497
Hainan	0.334	0.224	0.316	0.153	0.318	0.185	0.178	0.792	0.690	0.589	0.391	0.474
Ainu	0.247	0.214	0.351	0.252	0.504	0.333	0.184	1.029	1.247	0.334	0.236	0.444
Buriat	0.584	0.700	0.412	0.871	1.141	0.999	0.854	2.010	2.416	0.854	0.738	1.075
Siberia	0.577	0.681	0.656	0.804	1.136	1.159	0.865	2.176	2.587	0.914	0.979	1.030
Eskimo	0.296	0.371	0.475	0.413	0.627	0.778	0.488	1.658	1.825	0.449	0.612	0.489
NWT	0.469	0.568	0.588	0.673	0.943	1.027	0.764	1.982	2.108	0.706	0.848	0.798
Arikara	0.221	0.245	0.176	0.368	0.495	0.455	0.350	1.359	1.529	0.414	0.370	0.435
Navajo	0.656	0.484	0.332	0.598	0.897	0.509	0.641	1.303	1.004	0.983	0.652	0.740
Mexico	0.191	0.134	0.271	0.184	0.323	0.252	0.163	1.009	0.912	0.320	0.311	0.208
Peru	0.197	0.159	0.162	0.156	0.254	0.188	0.235	0.702	0.595	0.426	0.306	0.343

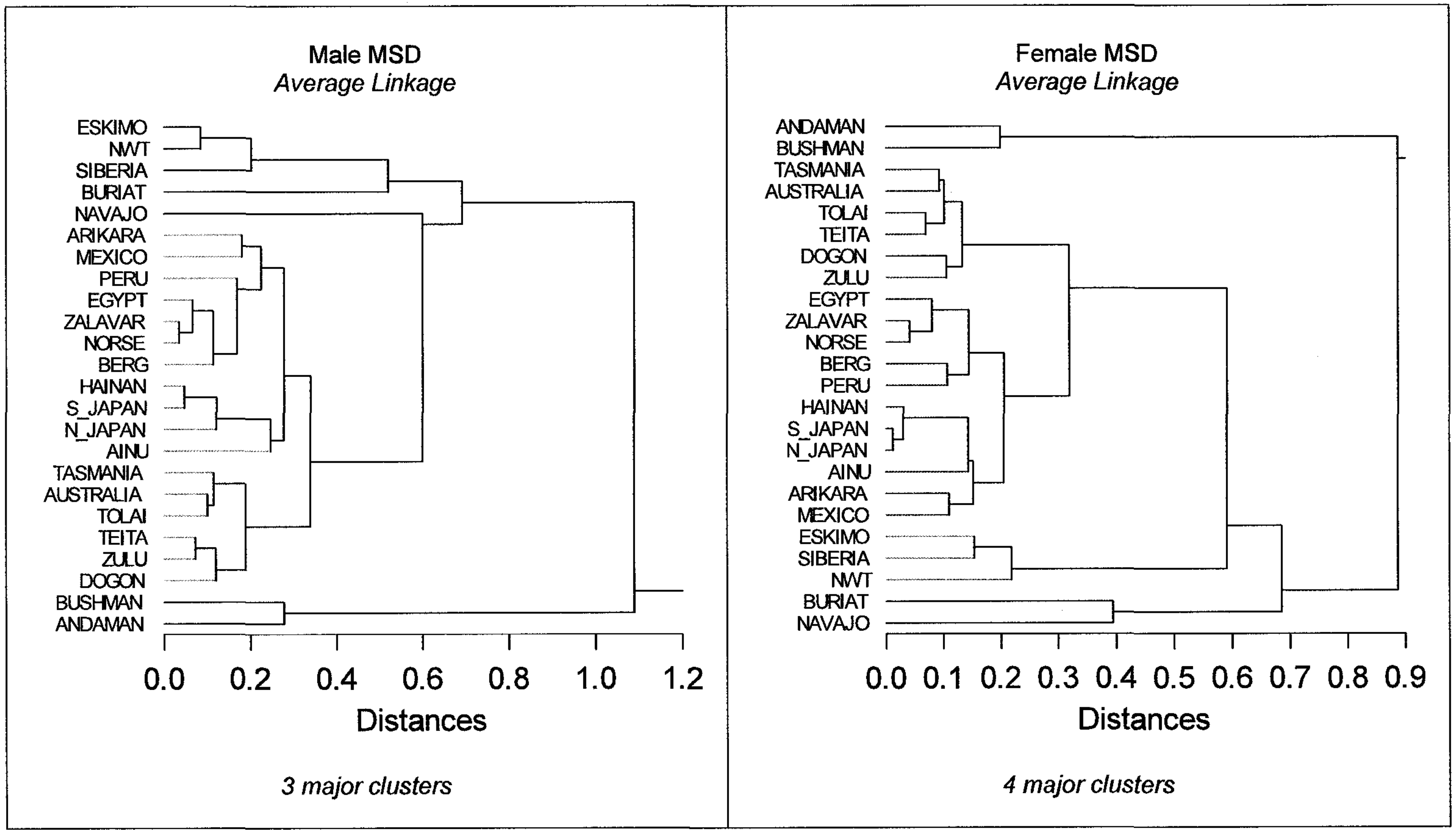
MALE	N. Japan	S. Japan	Hainan	Ainu	Buriat	Siberia	Eskimo	NWT	Arikara	Navajo	Mexico	Peru
Norse												
Zalavar												
Berg												
Egypt												
Teita												
Dogon												
Zulu												
Bushman												
Andaman												
Australia												
Tasmania												
Tolai												
N. Japan	0.000											
S. Japan	0.075	0.000										
Hainan	0.168	0.046	0.000									
Ainu	0.173	0.224	0.344	0.000								
Buriat	0.767	0.958	0.869	0.760	0.000							
Siberia	0.707	0.859	0.850	0.844	0.401	0.000						
Eskimo	0.466	0.539	0.615	0.511	0.640	0.266	0.000					
NWT	0.660	0.798	0.797	0.773	0.519	0.138	0.085	0.000				
Arikara	0.386	0.423	0.359	0.449	0.211	0.344	0.283	0.290	0.000			
Navajo	0.728	0.560	0.432	0.809	0.661	0.945	0.831	0.774	0.408	0.000		
Mexico	0.231	0.165	0.182	0.294	0.596	0.391	0.195	0.289	0.181	0.339	0.000	
Peru	0.369	0.268	0.216	0.505	0.827	0.897	0.494	0.605	0.291	0.434	0.172	0.000

Adapted Penrose's Mean Square Distance (MSD) Data Matrix: Female

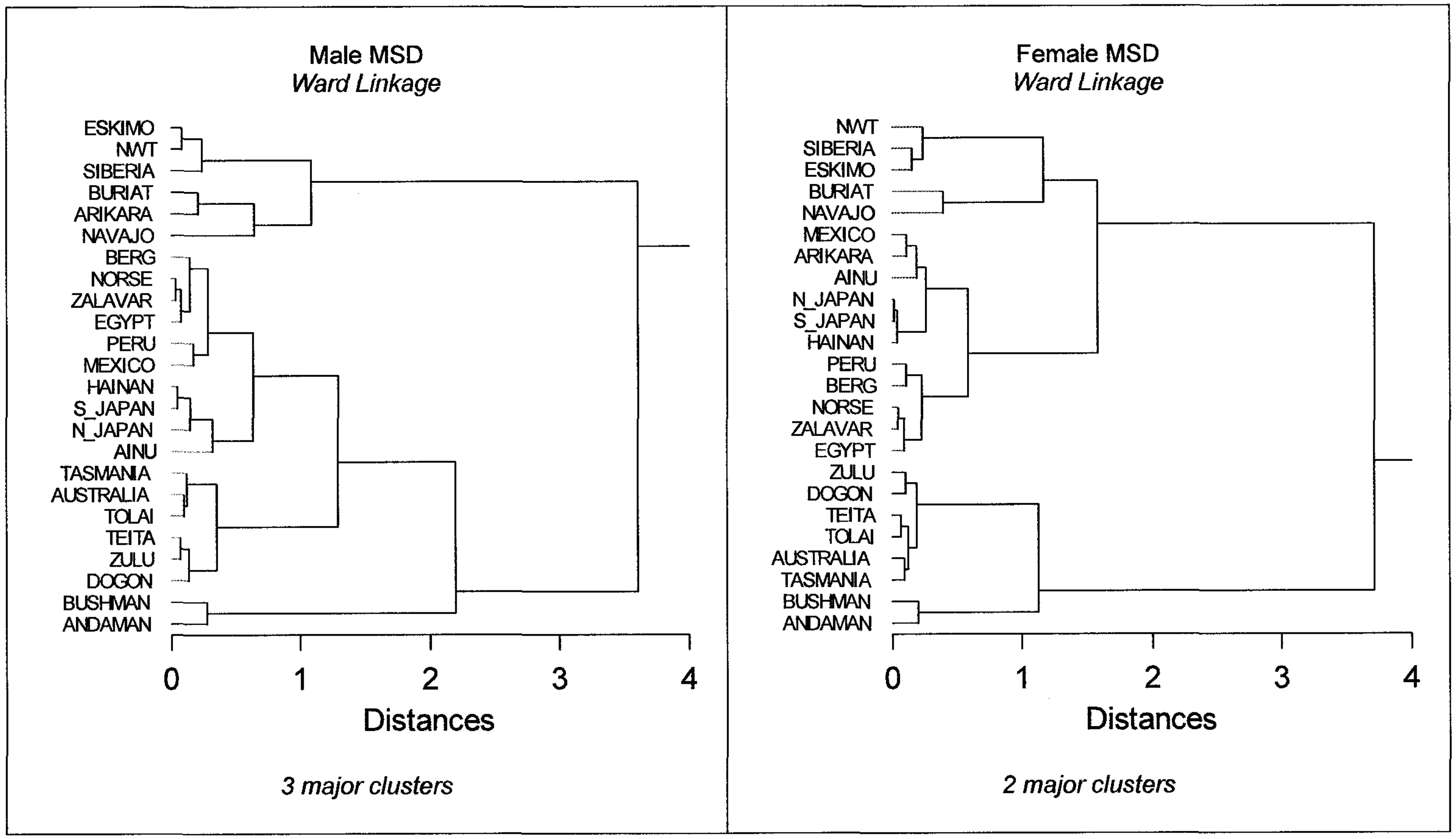
FEMALE	Norse	Zalavar	Berg	Egypt	Teita	Dogon	Zulu	Bushman	Andaman	Australia	Tasmania	Tolai
Norse	0.000											
Zalavar	0.041	0.000										
Berg	0.120	0.062	0.000									
Egypt	0.084	0.076	0.150	0.000								
Teita	0.288	0.247	0.332	0.278	0.000							
Dogon	0.305	0.214	0.241	0.241	0.105	0.000						
Zulu	0.178	0.160	0.269	0.246	0.143	0.105	0.000					
Bushman	0.593	0.528	0.551	0.489	0.214	0.285	0.466	0.000				
Andaman	0.821	0.593	0.496	0.580	0.429	0.350	0.791	0.199	0.000			
Australia	0.216	0.230	0.343	0.344	0.138	0.229	0.106	0.397	0.753	0.000		
Tasmania	0.343	0.276	0.396	0.427	0.118	0.183	0.115	0.329	0.679	0.093	0.000	
Tolai	0.229	0.169	0.274	0.261	0.069	0.107	0.072	0.386	0.595	0.071	0.079	0.000
N. Japan	0.158	0.126	0.151	0.147	0.274	0.203	0.170	0.668	0.710	0.280	0.388	0.183
S. Japan	0.168	0.103	0.157	0.128	0.273	0.188	0.177	0.682	0.678	0.321	0.398	0.184
Hainan	0.227	0.093	0.154	0.172	0.294	0.159	0.171	0.706	0.658	0.363	0.342	0.183
Ainu	0.153	0.117	0.210	0.307	0.413	0.305	0.135	0.851	0.982	0.264	0.339	0.242
Buriat	0.611	0.568	0.429	0.837	1.100	0.941	0.706	1.719	1.775	0.922	1.028	0.876
Siberia	0.428	0.463	0.592	0.700	0.913	0.871	0.580	1.518	1.824	0.675	0.881	0.631
Eskimo	0.236	0.284	0.469	0.451	0.607	0.745	0.444	1.168	1.464	0.402	0.642	0.401
NWT	0.616	0.679	0.877	0.843	1.260	1.212	0.830	1.808	2.125	0.896	1.176	0.929
Arikara	0.204	0.190	0.149	0.337	0.426	0.413	0.297	0.954	1.070	0.365	0.516	0.284
Navajo	0.889	0.679	0.478	0.859	1.197	0.890	0.923	1.740	1.653	1.152	1.354	0.947
Mexico	0.304	0.316	0.290	0.386	0.720	0.624	0.414	1.413	1.625	0.699	0.876	0.582
Peru	0.220	0.156	0.106	0.155	0.286	0.254	0.375	0.511	0.398	0.391	0.516	0.272

FEMALE	N. Japan	S. Japan	Hainan	Ainu	Buriat	Siberia	Eskimo	NWT	Arikara	Navajo	Mexico	Peru
Norse												
Zalavar												
Berg												
Egypt												
Teita												
Dogon												
Zulu												
Bushman												
Andaman												
Australia												
Tasmania												
Tolai												
N. Japan	0.000											
S. Japan	0.012	0.000										
Hainan	0.039	0.021	0.000									
Ainu	0.139	0.147	0.144	0.000								
Buriat	0.450	0.543	0.488	0.433	0.000							
Siberia	0.320	0.465	0.480	0.243	0.379	0.000						
Eskimo	0.256	0.295	0.352	0.212	0.671	0.153	0.000					
NWT	0.579	0.682	0.736	0.456	0.675	0.197	0.240	0.000				
Arikara	0.078	0.124	0.147	0.139	0.227	0.219	0.251	0.540	0.000			
Navajo	0.401	0.472	0.431	0.633	0.394	0.505	0.800	0.869	0.320	0.000		
Mexico	0.159	0.181	0.184	0.200	0.226	0.237	0.298	0.438	0.110	0.223	0.000	
Peru	0.127	0.119	0.167	0.334	0.690	0.809	0.524	0.988	0.237	0.603	0.418	0.000

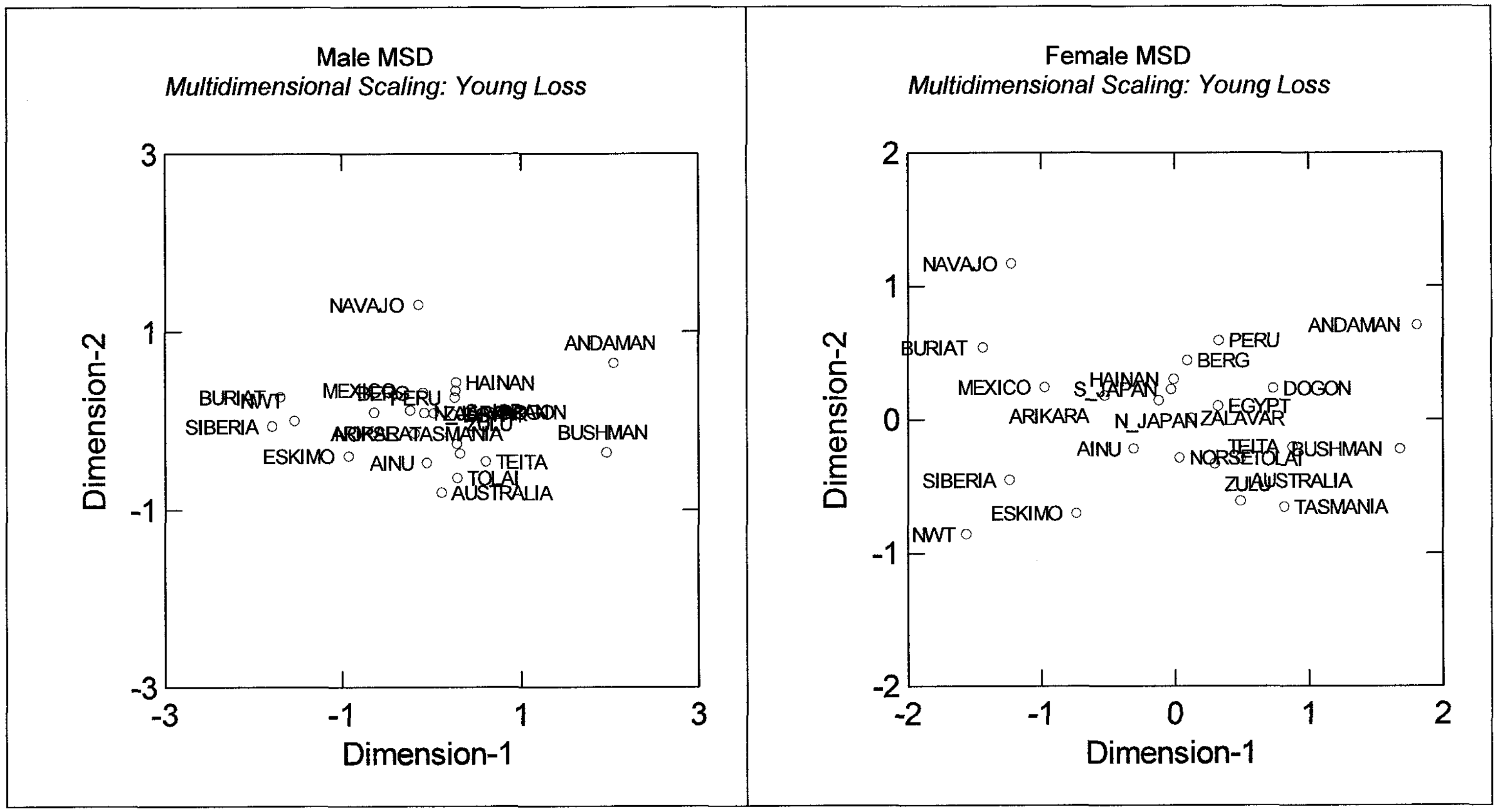
Adapted Penrose's Mean Square Distance (MSD)



Adapted Penrose's Mean Square Distance (MSD)

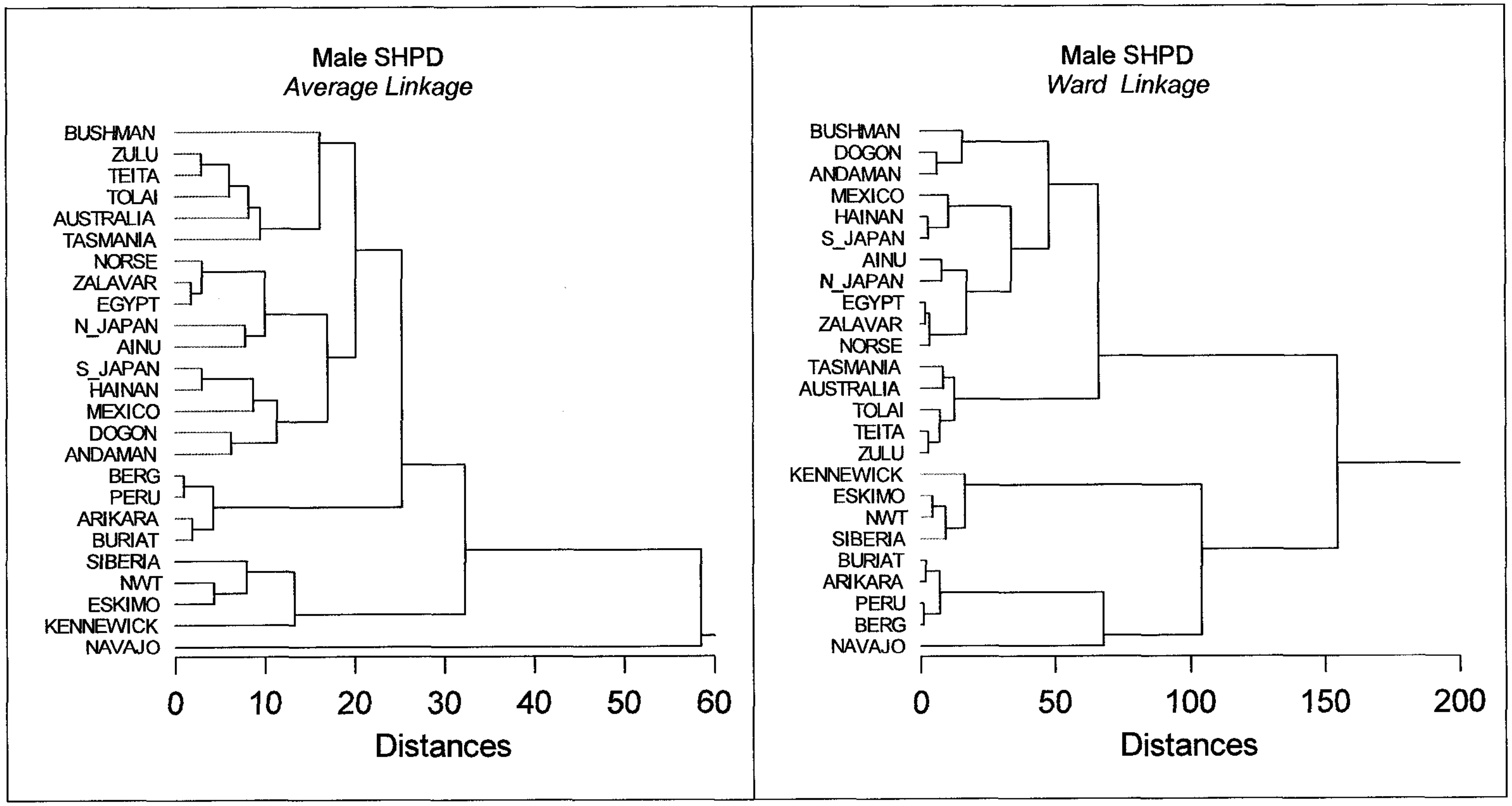


Adapted Penrose's Mean Square Distance (MSD)

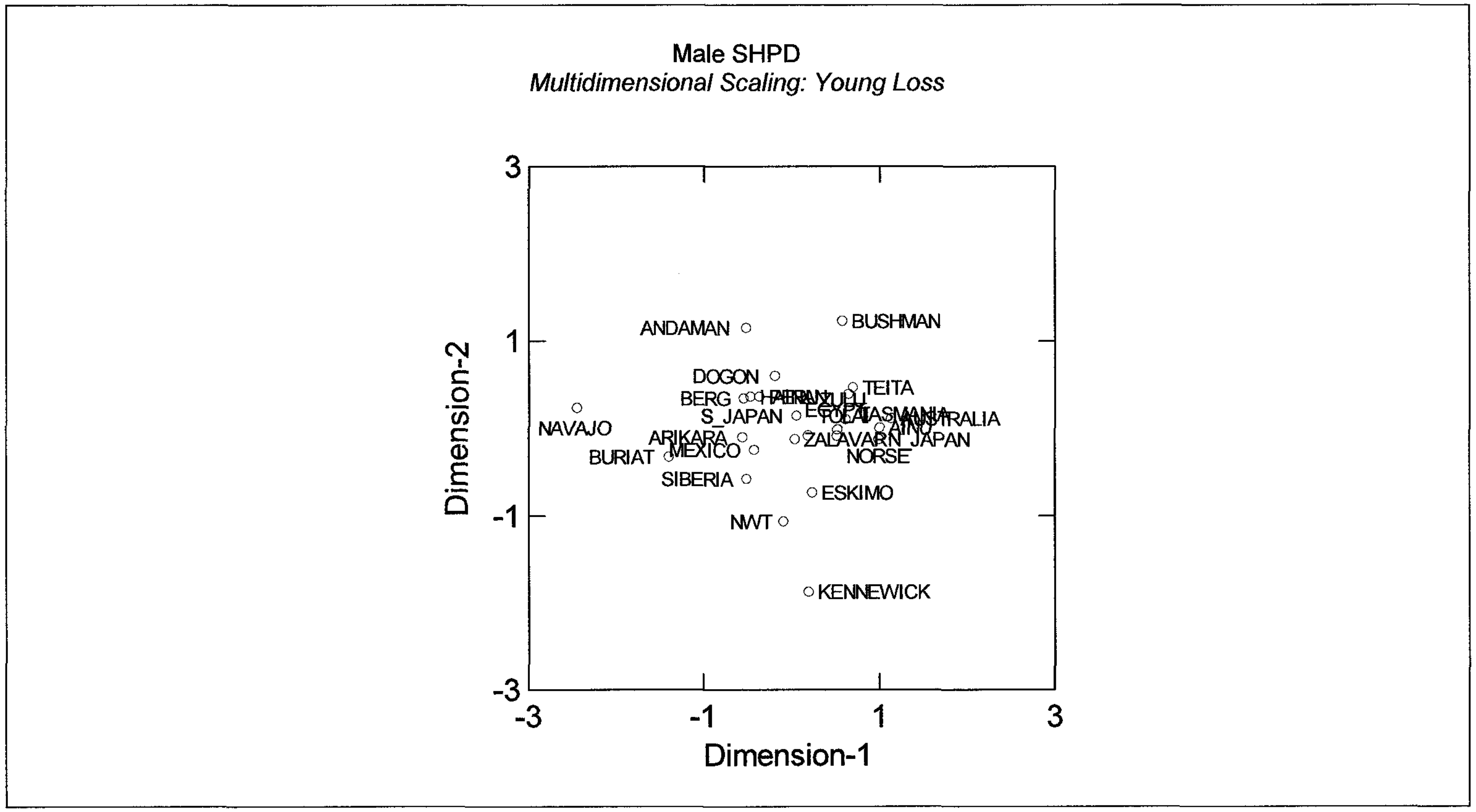

Male MSD
Multidimensional Scaling: Young Loss

Female MSD
Multidimensional Scaling: Young Loss

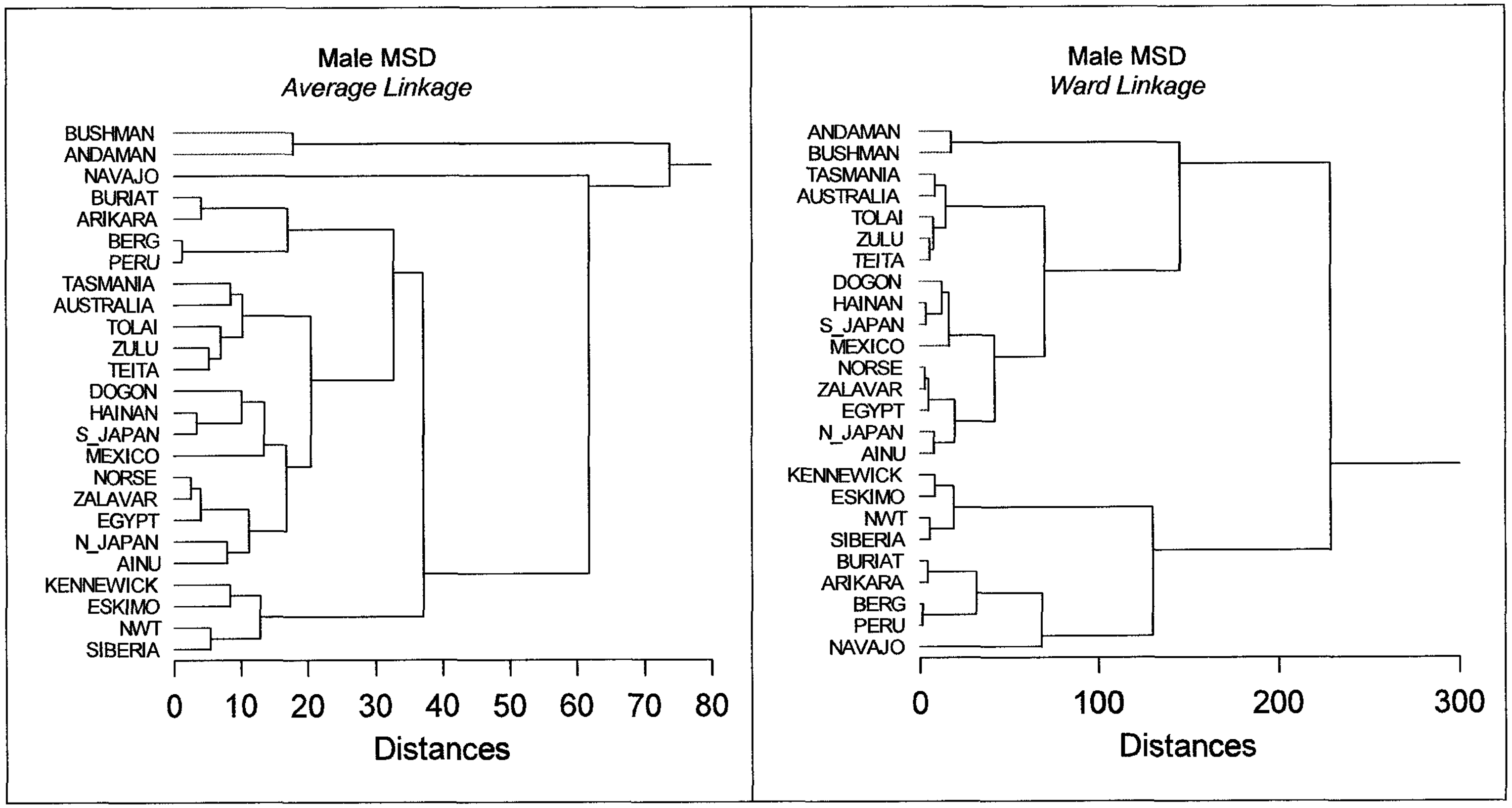
Penrose's Shape Distance (SHPD) w/ Kennewick Skull



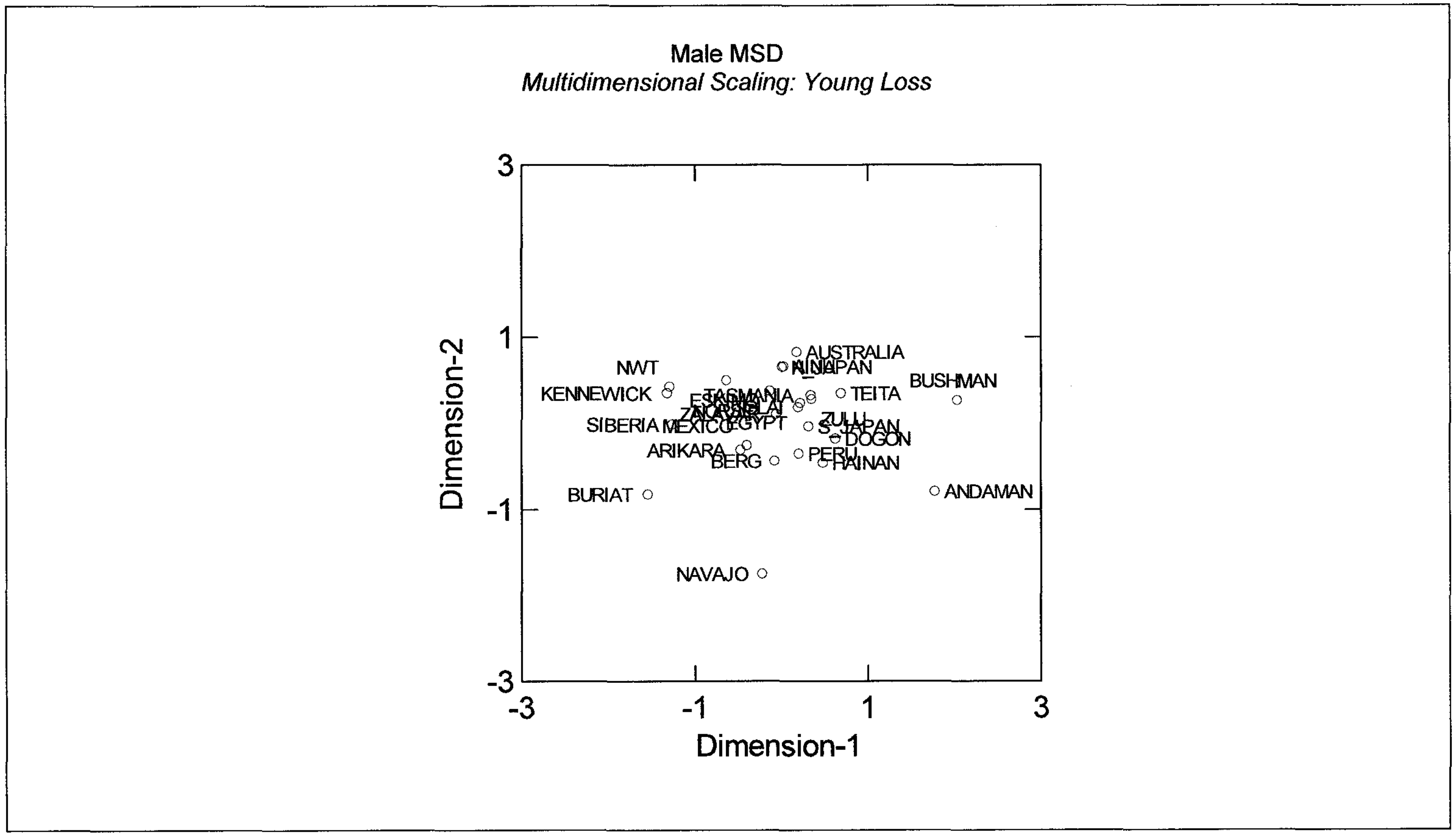
Penrose's Shape Distance (SHPD) w/ Kennewick Skull



Penrose's Mean Square Distance (MSD) w/ Kennewick Skull



Penrose's Mean Square Distance (MSD) w/ Kennewick Skull



Male MSD

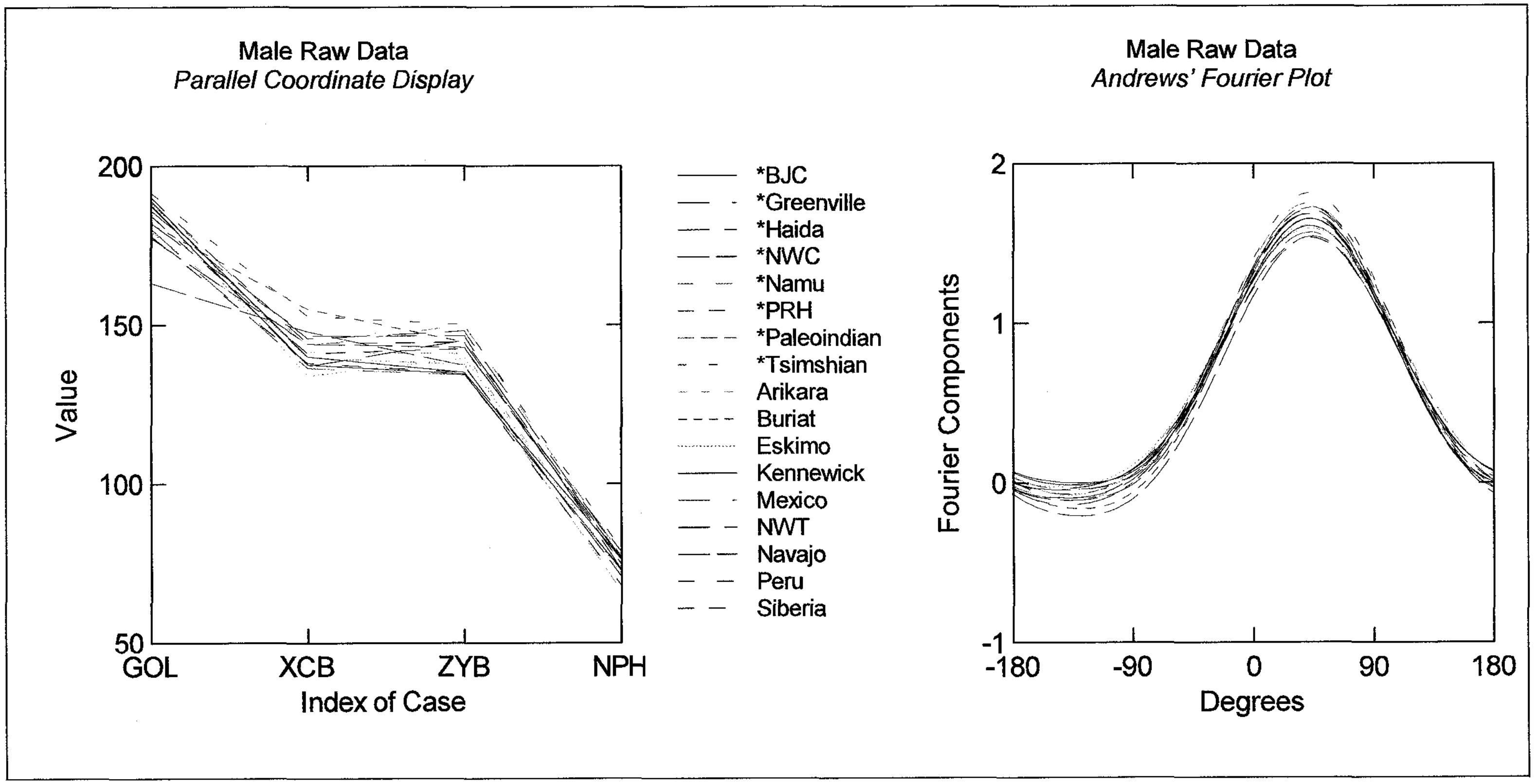
Multidimensional Scaling: Young Loss

Historic and Prehistoric Arctic/Americas Raw Data

N	MALE	GOL	XCB	ZYB	NPH	UFI	CI
55	Buriat	181.83	154.96	144.43	74.50	51.58	85.22
14	Siberia	184.36	141.00	142.42	78.43	55.07	76.48
53	Eskimo	188.30	133.94	139.59	71.70	51.36	71.13
14	NWT	187.79	137.00	145.08	76.23	52.54	72.96
11	*Northwest Coast - NWC	177.40	144.00	142.80	76.00	53.22	81.17
53	*Haida (Historic)	186.00	145.70	148.00	76.80	51.89	78.33
2	*Tsimshian (Historic)	191.50	152.80	150.00	79.00	52.67	79.79
8	*Greenville (1400-700 BP)	182.10	146.50	146.50	72.80	49.69	80.45
77	*Prince Rupert Harbour (3500-1500 BP) - PRH	190.10	144.20	144.50	74.30	51.42	75.85
8	*Namu (5000-4000 BP)	190.00	143.30	150.00	77.10	51.40	75.42
9	*Blue Jackets Creek (5000-4000 BP) - BJC	187.60	137.40	134.50	72.60	53.98	73.24
3	*Paleoindian (10,000-8500 BP)	188.70	139.70	137.50	66.00	48.00	74.03
1	Kennewick	189.00	140.00	135.00	76.00	56.30	74.07
42	Arikara	179.48	141.55	140.88	71.69	50.89	78.87
19	Navajo	163.13	147.87	137.08	70.60	51.50	90.64
16	Mexico	180.19	136.31	134.20	72.75	54.21	75.65
55	Peru	177.96	137.94	134.93	67.78	50.23	77.51

**Data from Cybulski (2005)*

Graphic Representation of Historic and Prehistoric Arctic/Americas Raw Data

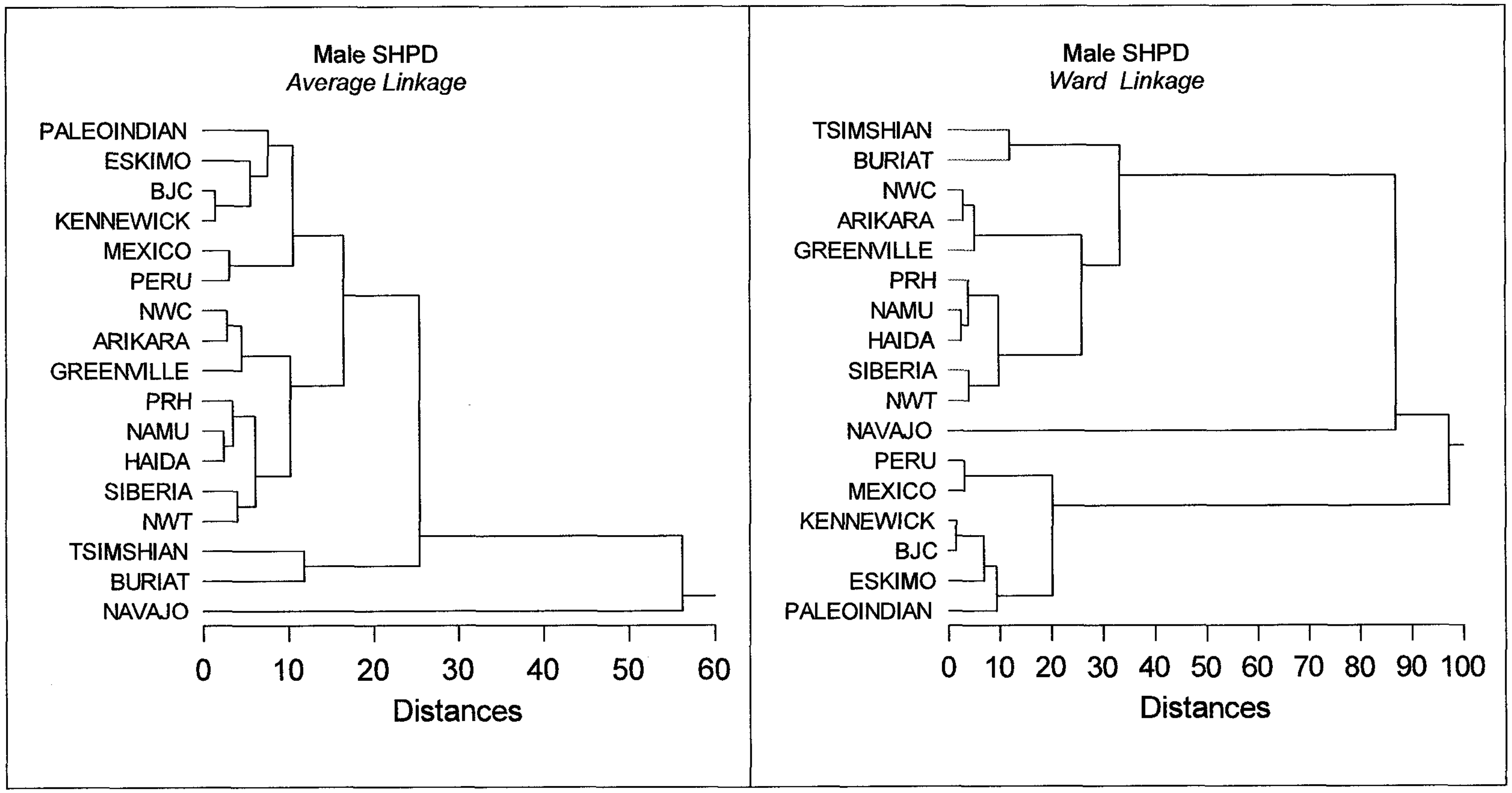


**Group means plotted in both Parallel Coordinate Display and Andrews' Fourier Plot (except for "Kennewick," which is an individual).*

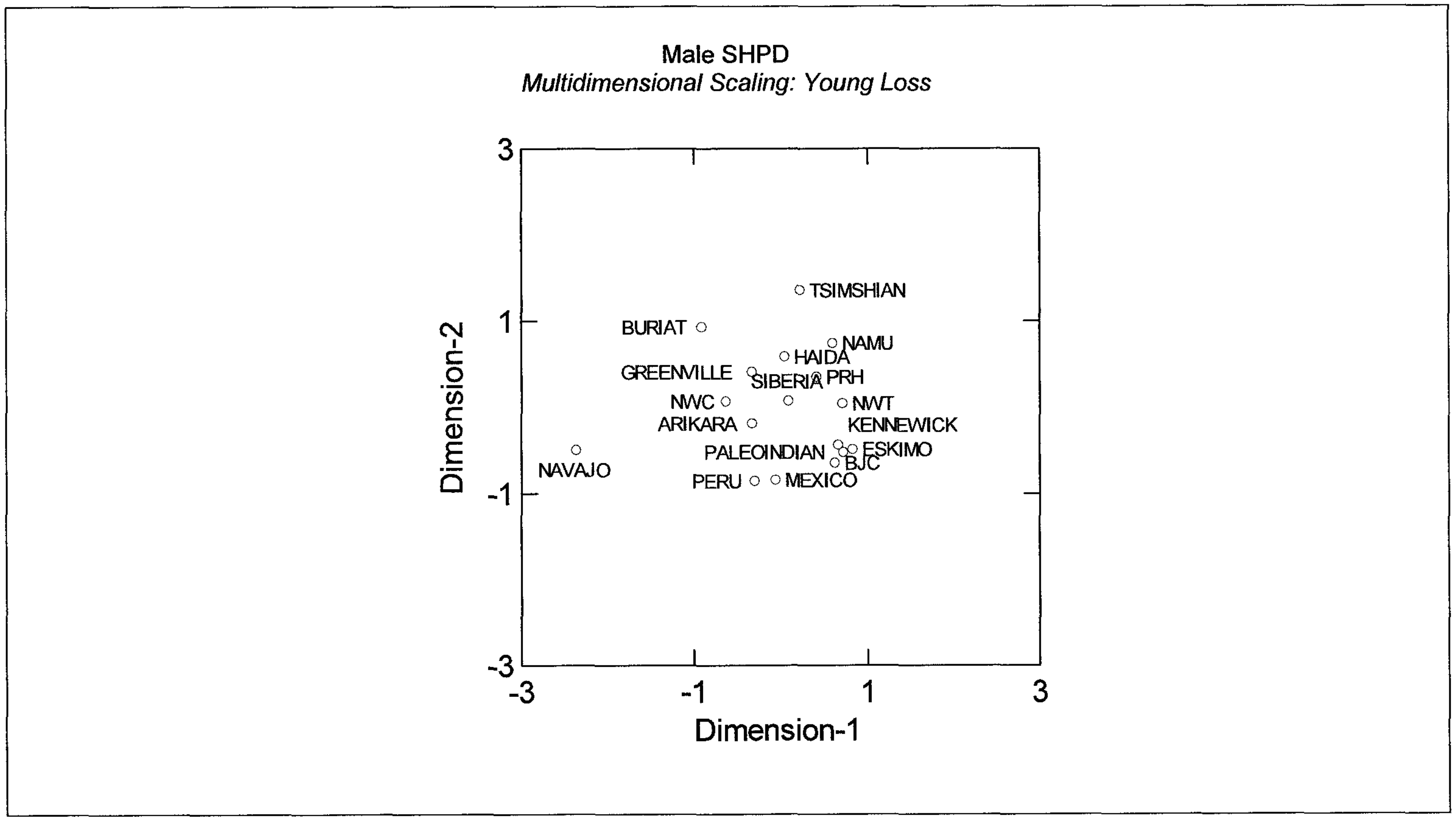
Penrose's Shape Distance (SHPD) Data Matrix: Historic and Prehistoric Arctic/Americas

MALE	Buriat	Siberia	Eskimo	NWT	NWC	Haida	Tsimshian	Greenville	PRH	Namu	BJC	Paleoindian	Kennewick	Arikara	Navajo	Mexico	Peru
Buriat	0.000																
Siberia	21.170	0.000															
Eskimo	46.573	10.259	0.000														
NWT	35.220	3.970	4.453	0.000													
NWC	12.057	5.984	24.436	15.865	0.000												
Haida	12.111	4.801	18.756	7.689	7.796	0.000											
Tsimshian	11.854	17.467	36.943	22.129	22.753	6.128	0.000										
Greenville	7.270	8.341	22.450	13.603	4.634	2.670	11.422	0.000									
PRH	18.353	5.985	10.108	5.636	15.047	3.640	8.746	7.284	0.000								
Namu	23.830	7.625	15.583	4.907	17.150	2.433	7.947	8.777	3.358	0.000							
BJC	38.807	10.023	3.898	10.578	22.149	21.180	36.384	23.214	11.128	22.098	0.000						
Paleoindian	34.360	17.874	6.993	15.014	26.738	20.966	33.330	19.144	9.516	21.289	5.906	0.000					
Kennewick	34.167	7.961	7.259	10.848	21.140	18.391	29.320	22.611	9.483	19.648	1.429	9.987	0.000				
Arikara	15.692	5.598	13.737	11.235	2.824	8.410	25.004	4.373	10.170	15.435	12.446	13.194	14.209	0.000			
Navajo	33.189	51.160	81.276	74.636	23.403	54.218	76.811	36.975	69.089	77.773	70.084	72.421	71.903	30.047	0.000		
Mexico	35.401	8.735	9.144	13.747	12.333	21.952	44.001	19.896	18.105	27.714	4.876	13.325	7.507	6.338	43.501	0.000	
Peru	30.235	14.360	13.774	19.636	11.993	23.348	45.804	16.369	20.171	31.302	9.844	11.056	14.795	4.356	33.096	3.050	0.000

Penrose's Shape Distance (SHPD): Historic and Prehistoric Arctic/Americas



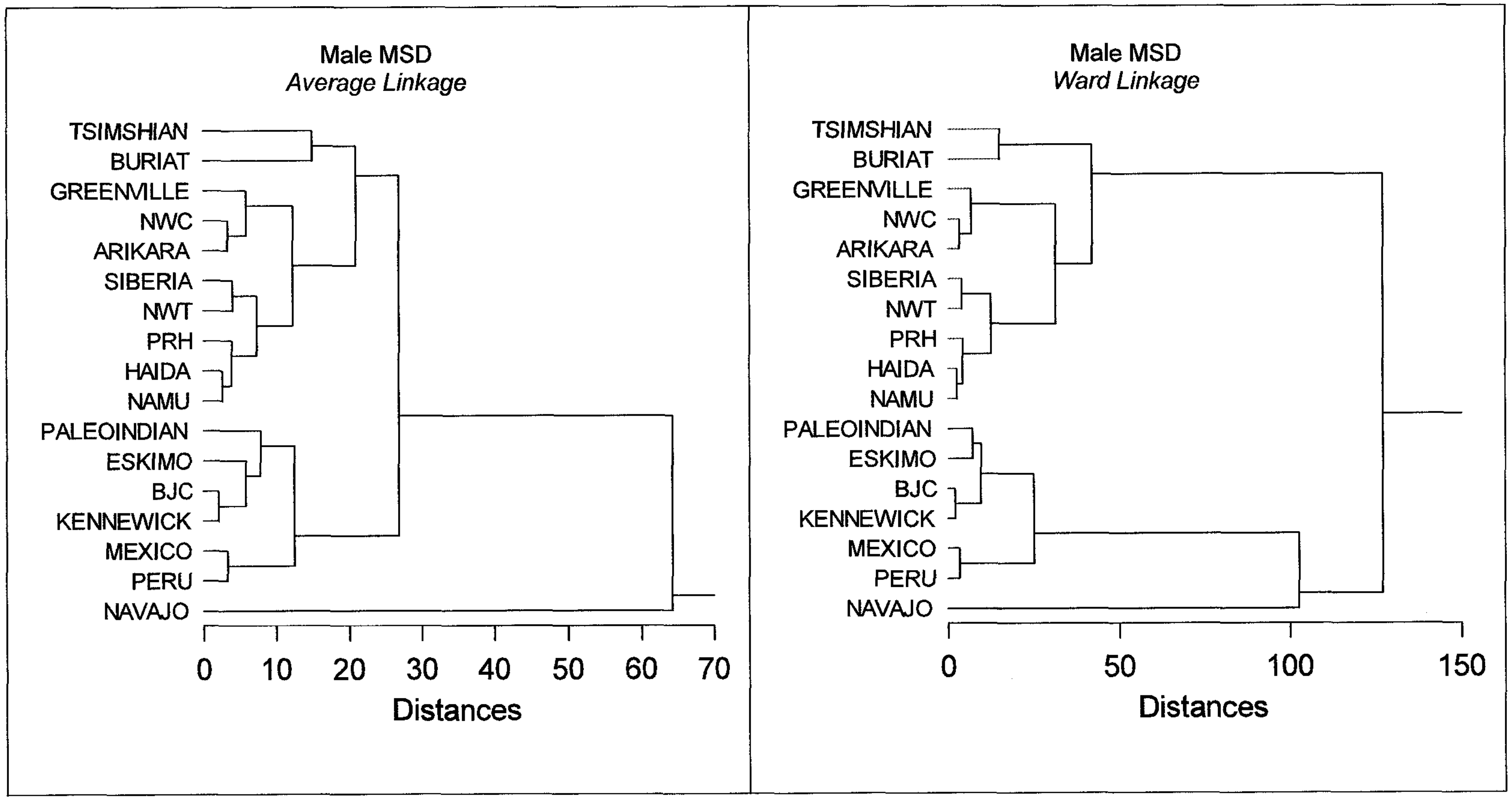
Penrose's Shape Distance (SHPD): Historic and Prehistoric Arctic/Americas



Penrose's Mean Square Distance (MSD) Data Matrix: Historic and Prehistoric Arctic/Americas

MALE	Buriat	Siberia	Eskimo	NWT	NWC	Haida	Tsimshian	Greenville	PRH	Namu	BJC	Paleoindian	Kennewick	Arikara	Navajo	Mexico	Peru
Buriat	0.000																
Siberia	22.076	0.000															
Eskimo	51.497	11.865	0.000														
NWT	36.145	3.970	6.033	0.000													
NWC	14.465	6.345	24.881	16.213	0.000												
Haida	12.117	5.861	24.033	8.771	10.453	0.000											
Tsimshian	14.945	*24.809*	*52.760*	*29.528*	*33.709*	*8.950*	*0.000*										
Greenville	7.882	8.370	24.515	13.635	5.227	3.410	17.874	0.000									
PRH	18.422	6.461	13.938	6.126	16.711	3.756	12.826	7.554	0.000								
Namu	24.049	9.640	22.803	6.952	21.230	2.585	9.611	10.339	3.891	0.000							
BJC	44.386	12.012	3.918	12.538	22.805	27.134	53.358	25.710	15.538	30.107	0.000						
Paleoindian	40.034	19.919	7.020	17.030	27.427	27.018	50.470	21.704	14.010	29.411	5.906	0.000					
Kennewick	36.639	8.346	7.677	11.220	21.140	21.113	40.409	23.235	11.199	23.810	2.053	10.643	0.000				
Arikara	20.585	7.186	13.737	12.797	3.259	13.654	40.765	6.418	13.972	22.618	12.469	13.223	14.618	0.000			
Navajo	46.906	58.733	83.481	82.153	28.033	68.519	106.641	45.512	80.934	95.176	71.884	74.168	76.447	32.272	0.000		
Mexico	45.814	13.912	10.160	18.877	15.139	32.875	68.851	25.874	26.896	41.367	5.624	14.039	10.246	7.368	43.728	0.000	
Peru	44.007	21.973	16.000	27.193	16.654	37.704	75.714	24.948	32.066	48.766	11.664	12.822	19.370	6.603	33.096	3.284	0.000

Penrose's Mean Square Distance (MSD): Historic and Prehistoric Arctic/Americas



Penrose's Mean Square Distance (MSD): Historic and Prehistoric Arctic/Americas