6 The ABO blood groups

Mapping the history and geography of genes in *Homo sapiens*

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**Introduction**

The ABO blood groups, and other blood group systems as they were discovered, provided an empirical foundation for human genetics research during the first half of the twentieth century. However, the legacy of the ABO blood group research is a matter of some debate, a debate that has emerged recently as a result of controversy surrounding the Human Genome Diversity Project (HGDP). HGDP proponents such as L. Luca Cavalli-Sforza and Mark Stoneking tend to regard the ABO blood group research as foundational to the discipline of anthropological genetics and the study of human genome diversity. Critics of the HGDP such as Jonathan Marks tend to view the ABO blood group work as marginal, in several ways: as scientifically suspect, as negative in its influence, as outdated, and even as racist. The controversy surrounding the HGDP heightens disciplinary tensions that preceded it, between geneticists, physical—biological anthropologists, and social—cultural anthropologists. Yet, the debate raises a number of methodological and conceptual issues concerning the investigation of human genome diversity, issues too important to allow these disciplinary maneuverings to obscure the methodological and conceptual issues we focus on here concern classification: relations between a priori and a posteriori groupings in classification, intersections and tensions between social—political and biological—anthropological categories of classification, the privileging of genetic over traditional anthropological traits in classification, and “racial” classification.

Our approach is to examine these important methodological and conceptual issues within a theoretical framework that makes use of cartographic representations and situates these representations within the history of mapping more generally. Attention to approaches taken by several researchers in mapping ABO blood group distributions permits appreciation of the interplay between objectivity and judgment that is involved in the construction of maps as representations of reality (Galison 1998), the construction of different notions of objectivity at different periods in the history of Western science (Daston and Galison 1992), and the circulation of reference that occurs in the construction of objects of knowledge (Latour 1999). Maps, as historical products of scientific activity, become useful tools in the hands of analysts of science as indicators of changing
They published their findings in 1919, in two articles: in *L’Anthropologie*, "Essai d’Application des Méthodes Sérologiques au Problème des Races," and, in *Lancet*, "Serological Differences Between the Blood of Different Races: The Results of Researches on the Macedonian Front." It is the Hirsfelds’ research that is portrayed today as marking the inception of a new discipline, anthropological genetics, by proponents of the HGDP like Cavalli-Sforza and Stoneking. By 1961, ABO data for over seven million people living in areas stretching across the globe had been published (Mourant 1961:1).

The human genome diversity project: contesting disciplinary boundaries and the historical legacy of ABO blood group research

The use of the ABO blood groups to map the history and geography of genes in *Homo sapiens* throughout much of the twentieth century is considered by some scientists to provide methodological and theoretical foundations for contemporary human genome diversity research. The serological research carried out by Ludwik and Hanna Hirsfeld during the First World War is portrayed as marking the inception of a new discipline.

According to human population geneticist L. Luca Cavalli-Sforza, “This early work with ABO gave birth to anthropological genetics” (2000:15). In a recent article in *Nature* that introduces that issue’s presentation of single nucleotide polymorphism (SNP) data, anthropological geneticist Mark Stoneking similarly represents the Hirsfelds’ work as a “seminal study,” the first attempt to study genetic variation in human populations (2001: 821). Stoneking commends the Hirsfelds’ research for its scientific rigor: “This work was notable for its broad coverage of the world’s populations, large sample sizes and scrupulous attention to anthropological details” (2001: 821). He believes that the significance of the Hirsfelds’ contributions has finally been recognized by the scientific community because of increased appreciation of the importance of human genome diversity: “Happily, times have changed, and diversity is all the rage” (2001: 821).

Cavalli-Sforza traces a path of continuous linear progress in the investigation of human evolution since these beginnings—from the use of biological reagents to determine blood group differences in the first decades of the twentieth century, to the mid-century introduction of electrophoresis to discern protein differences, to, finally, during these past couple of decades, the availability of restriction enzymes and PCR to identify DNA differences. He sees the latest developments as building upon but in no way replacing earlier research: “The future of the analysis of genetic variation is clearly in the study of DNA, but results accumulated with the old techniques based on proteins have not lost their value… Results with DNA have complemented but never contradicted the protein data” (2000:18). Cavalli-Sforza’s own career path follows this same trajectory. After gaining his MD in the 1940s, he studied population genetics, immunology, and blood group techniques at Milan’s serum institute. While at Cambridge on a research post-doc from 1948–50, he worked with R.A. Fisher who was using the
blood groups to study human evolution (Cavalli-Sforza and Cavalli-Sforza 1995: 106). During the early 1960s, while at Pavia University, Cavalli-Sforza and Anthony Edwards launched a comprehensive study of human evolution. They developed quantitative methods that calculated genetic distances based on blood group differences between populations in order to establish a phylogenetic tree. An evolutionary tree constructed in 1962 was based on five blood group systems in fifteen populations, with three populations taken from each continent (Cavalli-Sforza et al. 1994: 31–2; Cavalli-Sforza and Cavalli-Sforza 1995: 111–3). Cavalli-Sforza’s current research expands the project he began in the 1960s, with a great deal more data available. A 1984 evolutionary tree was constructed on the basis of 110 genes in forty-two “native” populations from around the world (Cavalli-Sforza and Cavalli-Sforza 1995: 119). His vision for the HGDP calls for the immortalization of cell lines from up to 500 “aboriginal” populations worldwide, many of which are at risk of disappearing. He estimates that comparing approximately 100 gene loci for each cell line will allow researchers to address a variety of questions about the history of human evolution and patterns of human genome diversity (Bowcock and Cavalli-Sforza 1991; Cavalli-Sforza et al. 1991).

This representation of the historical legacy of ABO blood group research, which sees the contributions of the Hirschfelds and their successors as providing methodological and theoretical foundations to contemporary human genome diversity research and physical–biological anthropology, is, however, contested. Prominent among the critics is biological anthropologist Jonathan Marks (1996, 2000). Marks is dismissive of the significance of the blood group research, and genetics more generally, for developments in his discipline. While he agrees with Cavalli-Sforza and Stoneking that there is historical continuity leading from the blood group research to contemporary approaches to the study of human genome diversity and human evolutionary history, Marks does not characterize this legacy in positive terms. The ABO blood group research is viewed by Marks, not as foundational, but as marginal to contemporary research efforts in a number of ways: outdated, scientifically suspect, negative in its impact on anthropology, and even racist. This contrasts with Stoneking’s commendation of the scientific rigor of the Hirschfelds’ efforts and regret for the delay of scientists in recognizing the importance of these efforts. Marks claims that, already in the 1920s, anthropologists were skeptical of the ABO blood group research because of the unlikely groupings that resulted. Marks contends that “racial serology” managed to “reinvent” its hold on physical anthropology from the 1920s to the 1940s only by making illegitimate use of blood group data to “confirm” racial divisions that, in actuality, were “imposed” on the data. By 1945, according to Marks, the ruse was up, with serological methods cast, deservedly, to the margins of the discipline of physical anthropology. The HGDP, Marks writes, “seems to fit snugly with the mold established earlier in the century by racial serology—high technology wedded to folk concepts about human biological diversity and conceptually antiquated approaches to the study of the peoples of the world” (1996: 360–1).

This distance between hagiographies presented by Cavalli-Sforza and Stoneking, on the one hand, and Marks, on the other hand, is unsurprising. The debate over the historical legacy of ABO blood group research has been shaped by controversies surrounding the HGDP. Proponents like Cavalli-Sforza and Stoneking tend to portray the history of ABO blood group research and its contributions to the contemporary study of human evolution in a favorable light. An opponent like Marks’ interpretation of the historical record is couched in terms of disciplinary differences, whereby ignorant and narrowminded geneticist outsiders repeatedly challenge the authority of experienced and broad-thinking anthropologists. Marks believes that the successful use of genetic techniques and DNA data requires anthropological knowledge, and it is at their peril that geneticists try to supersede anthropologists. He draws an analogy between the failure of “racial serology” and the difficulties that the HGDP, as conceived by molecular geneticists, has faced in gaining support. Of course, the debate over the historical legacy of ABO blood group research, as it has emerged within the context of controversies concerning the HGDP, is embedded in longstanding disciplinary conflicts over the study of human biological and cultural diversity and the history of human evolution. Jennifer E. Reardon’s recent doctoral dissertation, Race to the Finish: Identity and Governance in an Age of Genetics, details ways in which these conflicts structured negotiations over the HGDP by a number of stakeholder groups, including population geneticists, physical–biological anthropologists, social–cultural anthropologists, and indigenous peoples. Physical–biological anthropologists questioned the qualifications of geneticist organizers of the HGDP to study human, rather than zoological, populations and criticized their focus on genetic, instead of phenotypic, differences. When HGDP organizers responded by inviting anthropologists to participate, the hand extended did not unequivocally welcome fellow experts in place of assistants who could facilitate access to populations of interest. These difficulties were compounded by HGDP organizers’ surprising lack of awareness of other sources of conflict: politically charged disciplinary disputes between physical–biological and social–cultural anthropologists, the growth in political activism among indigenous groups, and the history of racism and colonialism. Reardon argues that the HGDP could not proceed without the “co-production” of accommodating natural and social orders (for example, workable categories of “race” and “expertise”), and this explains its failure. Although disciplinary conflicts between geneticists, physical–biological anthropologists, and social–cultural anthropologists, heightened by recent controversies concerning the HGDP, play a significant role in shaping understandings of the historical legacy of the ABO blood group research, it would be unfortunate were these maneuverings to obscure the importance of the methodological and conceptual questions raised. Rhetoric that fosters discipline formation in the relative short term by minimizing continuities and exaggerating discontinuities with neighboring disciplines may, in the long term, impoverish the methodological and conceptual foundations of all of these disciplines by failing to interrogate, in an intellectually honest and fruitful manner, those interesting questions that are prone to arise, sometimes unexpectedly, in border regions. In the case of the HGDP, the politics of representation associated with discipline formation is
embedded in, and incorporates the meanings of, wider social discourses surrounding the history of racism and colonialism. Consequently, historiographic choices, and the continuities or discontinuities they privilege or downplay, receive moral signification. Diane B. Paul's (1994a,b, 1995) work on eugenics, human genetics, and the Human Genome Project beautifully illustrates similar developments in a related area and how contesting politics of representation can result in important questions being overlooked. In assessing the implications of the history of eugenics for contemporary human genetics, supporters of the project emphasize discontinuities while critics of the project emphasize continuities. The result is not only the selective misrepresentation of historical developments, but that questions we ought to take seriously, like what it is people fear about eugenics, become overlooked.

The debate between HGDP proponents and opponents over the historical legacy of ABO blood group research raises a number of methodological and conceptual issues. These issues concern the classification of human groups: relations between apriori and a posteriori classifications, intersections and tensions between social-political and biological-anthropological categories of classification, the privileging of genetic over traditional anthropological traits in classification, and the "racial" classification of groups. In the following sections, we examine these issues within the context of the debate over the historical legacy of the ABO blood group research.

Mapping the history and geography of genes in *Homo sapiens*

In a companion chapter (Gannett and Griesemer, Chapter 4), we argue that the historiographic of discontinuity used to interpret the history of classical genetic mapping neglected important continuities of mapping practice that were deflected, but maintained, as fly researchers shifted their attention and operations from lab to field in order to address questions of the genetics of natural populations. Specifically, we trace a continuous historical chain of representational forms in linkage and geographical maps as Theodosius Dobzhansky, in collaboration with A.H. Sturtevant, adapted laboratory one-species linkage mapping practices to work on the geographical distribution of genes and chromosomes in natural populations.

Here, we take for granted the deflected continuities of mapping practice between lab and field to address a different historiographic problem about continuities of genetic mapping. An additional continuity of Dobzhansky's and other geneticists' mapping practices in the early twentieth century concerns the common use of mapping techniques by fly and human researchers, and recognition of these efforts as directed to similar ends. For instance, observations about the distribution of human genes appeared in Dobzhansky's publications, very often immediately following his observations about distributions of fly genes, with both represented through the technology of geographical distribution genetic mapping.

Human variation and its classification began to receive a great deal of attention in the nineteenth century, long before the genetic mapping carried out by Dobzhansky in flies and the ABO researchers in humans. Notoriously, "racial" classifications based on the physical characteristics of humans were subject not only to observations of the appearance of variation and difference, but also to judgments of categorical difference and to ethnic, cultural, and national biases that served as a priori classifications guiding sampling protocols for research on biological traits. As geneticists and physical anthropologists turned their attention to genetic mapping in humans, classification practices of population geneticists came into direct contact with classification practices of anthropologists. Classifying humans genetically, by mapping geographical distributions of serological traits-blood groups-seemed to afford mappers an opportunity to improve on the objectivity of human "racial" classifications based on physical characteristics because, they reasoned, distributions of blood group genes could not be affected by the mating choices and preferences of humans carrying the genes, nor would the measurement of human differences be affected by the racial judgments and biases of investigators (Davis 1935).

In this section, we examine methodological and conceptual questions associated with classification practices within a theoretical framework that makes use of cartographic representations, in particular, the geographical maps of ABO blood group distributions favored by researchers like Ludwik and Hanna Hirsfeld, Laurence Snyder, J.B.S. Haldane, and William C. Boyd, and spanning a period from 1919 to 1950. We attempt to situate these cartographic representations within the history and epistemology of mapping more generally.

Our historiographic project concerns the relation between classification judgments and scientific objectivity in the emerging epistemology of human blood group mapping. Peter Galison and Lorraine Daston have argued for a periodization of the historical development of notions of objectivity from the eighteenth to the twentieth century which opposes, as alternative stances between which investigators faced a fundamental epistemic choice, the judgment of the trained, twentieth-century scientific expert to the objectivity of the dispassionate, nineteenth-century mechanical recorder or the inspired discovery of the eighteenth-century genius (Daston 1992; Daston and Galison 1992; Galison 1998).

Galison and Daston argue that the eighteenth-century conception of being "true to nature" depended on a notion of artistic interpretation in the representation of nature's essences discovered through acts of genius. Artistic judgment was needed to extract truth from the distractions of accidental variation, for example, in the visual representation of the essential healthy body or organ from observations on individually varying, anachronistic, even diseased subjects (Daston 1992). Thus, accuracy in representation of objective, essential truths about nature was a product of judgment that transcended the brute facts presented to the observer.

By the late nineteenth century, Daston and Galison argue, a new vision of mechanical objectivity emerged, in which the potentially biasing judgments of an artist-representer who is not a professional scientist had to be removed from the
path from data to visual representation of nature (Daston and Galison 1992). In his place would go a machine—a device lacking any capacity for judgment or bias—that would register and render visual the objective truth about nature, variation and all. Thus, objectivity in representations of nature was to be a product of machine discipline and the “self-abnegation” of the investigator, who would avoid the temptation to interpret and depict unseen essences as products of observation.

Galison (1998) extends this periodization of objectivity, arguing that somewhere between the 1920s and 1950s, judgment again gained ascendancy against (mechanical) objectivity in the stance of scientific atlas- and visual image-makers, though in a new form—that of the trained, self-confident scientific expert and judge rather than that of an inspired artistic and temperamentally genius. Galison links his periodization to the professionalization and institutionalization of science. The profession of scientist emerged in the nineteenth century and mechanical objectivity can be seen as the stance of the disinterested professional, with specialized equipment and training, against that of the biased, judgmental, interested artist-amateur. In the twentieth century, as science became bureaucratized and rationalized with an elaborate division of scientific labor, the mechanically-objective equipment operated became a technician—a low-paid, theoretically unmotivated, and therefore (it was assumed) unbiased helper, often a woman. The scientist, in contrast, became an expert trained in the exercise of scientific judgment.

This historiographic model, based on an opposition of judgment to objectivity and their conceptual transformation with the emergence of expertise, depends on a vision of the relation between representation and reality as a large and fundamental gap to be bridged. The “old settlement” of this relation by the linguistic turn toward correspondence between ontologically distinct categories of world and words ill fits the complexities of scientific practice in general and the historically and socially situated negotiations of scientific objectivity in particular (Latour 1999). We argue that Galison’s periodization would be better served if the twentieth century is viewed in terms of an emerging conception of objectivity with judgment rather than against it. The conceptual and material displacements in each of the many small steps it takes to extract representations through scientific work require a form of abstraction that supports a new image of objectivity. However, as we show in the case of human blood group mapping, there does remain a tension between the older mechanical view and the emergent vision of expertise in the attempt to discharge subjectivities of judgment in expert scientific practice. Thus, we endorse Galison’s periodization of the changing rhetoric of epistemic virtue, but suggest that the history of scientific practice puts different music to the libretto. As judgment becomes essential to the circulation of reference between representations and reality, it becomes inextricable from scientific work.

The image of subjectivity

Bruno Latour’s provocative account of the circulation of reference, which aims to solve sociologically the ontologist’s problem of the relation between abstract wordy representations and concrete worldly nature, involves a chain of small abstractions (1999: ch. 2). At each step in the scientific process, he argues, matter extracted from a scene of investigation is converted into form for the purposes of the next step’s extraction. Since each step represents only a small displacement from the context of the investigative scene, reference can “circulate.” Representations of soil scientists in Paris can successfully refer to dirt in Brazil because the chain of connections, so carefully produced and maintained by back-breaking scientific work, can be successfully traced and retraced. Thus, the seemingly large, unbridgeable ontological gap between words and the world is made of a series of small, bridgeable ones. Understanding abstraction becomes an empirically manageable problem in the sociology of science.

We wish to analyze these small abstractions further to understand the way in which objectivity depends on the circulation of reference and, for our study of human blood group mapping, how objectivity can be understood as the “image” of subjectivity. Objectivity and subjectivity are reflections along the line of circulating reference created by scientific work, just as wordy abstractions and worldly concretions are reflections across the ontologists’ “line of being” (Jubien 1997). In our study, the a priori subjective judgments of national, ethnic, and racial categories are used to initiate the scientific process of building a chain of circulating reference leading to a posteriori classification schemes. But in order to sustain the claim of objectivity for the resultant scientific classifications, it seems that the subjective a priori assumptions that bridged the gap between abstractions (hypotheses and a priori categories) and concrete research subjects to initiate research must be discharged. However, because they are linked to the a posteriori classifications through the reference chain, subjective judgments are ineliminable if science is to achieve objectivity.

Each of Latour’s small abstractions is composed of a small extraction and a small judgment (see Figure 6.1). The abstraction is not a Platonic one as Latour suggests, as though matter itself could jump ontological categories into form. Rather, the abstraction is Aristotelian: a subtraction in thought of some of the properties of concrete matter, facilitated by the extraction of a material sample from its context. A lump of dirt is extracted from a hole in Brazil. Thus, the dirt’s qualities and structure can be contemplated in abstraction from its relations to the surrounding environment. The lump is trimmed to a neat cube—extracted from its irregular surfaces—so as to fit a cubicle in the pedocomparator, thus preparing the extracted lump for comparison with other, similarly prepared lumps. And so on, until the chain of references leads to a series of numbers in a published data table and a specimen cabinet. Each step provokes a small jump, a small gap between one material object and another. The gaps allow the extracted matter to be moved away from the sampling point, but they open up new problems of judgment—what to do next with the extracted sample, where to take it, where should it go?—but always making a trail that can be retraced. Hence, reference can circulate back down the trail established by a sequence of small judgments, and it is this possibility of referential backtracking that secures objectivity, that is, a return to the subject as object. The gaps are not the unbridgeable ones of
blood group frequencies among ethnic groups and recognizes one of their successors, William C. Boyd, for his use of three blood group systems (ABO, RH, MN) to differentiate populations on the five continents from one another (2000: 14–6). In other words, Cavalli-Sforza believes that the blood group researchers were able to discover existing genetic differences among ethnic and/or racial groups. Jonathan Marks, in contrast, is dubious about the validity of classifications based on blood group differences. He claims that serologists like Boyd, simply used blood group data to validate the existence of groups they had assumed from the outset, and not as an empirical basis for classification. Here, Marks finds continuity between the blood group researchers and contemporary geneticists associated with the HGDP. He contends that among the “conceptually antiquated approaches to the study of the peoples of the world” today’s population geneticists share with their “racial serologist” predecessors a reliance on a priori “racial” divisions (1996: 360–1): “population geneticists still commonly assign individual genotypes a priori into races, and ask their computers about the genetic relationships among the races they have constructed” (1996: 358).

This point of disagreement between Cavalli-Sforza and Marks raises important questions about the nature of a priori and a posteriori classifications of human groups and the relations between them. What is the status of the a priori groupings assumed by researchers? Must these a priori groups be validated by empirical data? Do the data alone determine a posteriori classification schemes or are unre- examined a priori assumptions involved? The nature of biological variability in Homo sapiens constrains the answers that are available to these questions. Researchers cannot forego a priori classification altogether or, on the basis of empirical evidence, reconstitute these a priori groups entirely. Methods of a posteriori classification inevitably impose discontinuities upon continuities. This is because historical and geographical patterns of genetic variability among human groups are overwhelmingly quantitative, not qualitative. Both closely and distantly related groups share most of their alleles in common and differ only in the frequencies with which these are found. And, at any given time slice, the geographical sampling of genes reveals that the vast majority of alleles are found throughout the world, though at varying frequencies in different places. In their mid-twentieth century efforts to integrate anthropology into the modern evolutionary synthesis through its incorporation of the methods and concepts of population genetics and the replacement of “typological thinking” by “population thinking,” biologists like Theodosius Dobzhansky and Ernst Mayr emphasized the prevalence of genetic continuities over discontinuities across the distribution of Homo sapiens.

The prevalence of genetic continuities over discontinuities was not news to serologists, however. When the ABO blood groups were discovered at the beginning of the twentieth century, researchers thought they might be able to identify qualitative serological differences among “races” that would be analogous to those blood characters that definitively sort individual organisms into distinct species. But, in 1919, the Hirsfelds found that blood group differences vary quantitatively rather than qualitatively among the “races” with A and B “present in all races examined” (1919b: 37), though “in different proportions in different

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**A priori and a posteriori classifications of human groups**

L. Luca Cavalli-Sforza’s interpretation of the historical legacy of the blood group research credits the Hirsfelds for their demonstration of differences in ABO
It is unlikely that the Hirszfelds’ failure to discover qualitative ABO blood group differences among national [presumed, as we discuss later, to be “racial”] groups, and the resulting inability to rely on these differences to sort individuals into “racial” groups, would have been surprising. Because of inheritance studies of the ABO blood groups, and the use of blood group differences to sort donor and recipient individuals for transfusion services, it was well established by this time that individuals belonging to the same ethnic group, and even the same family, can have different blood types. The significant empirical finding would have been the variation in relative proportions of the blood types exhibited by different national groups. Further attempts to isolate blood group differences other than ABO that could serve as “racial” characters proved unsuccessful. Close to a decade later, serologists like Landsteiner and Levine had accepted "the idea that the serological make-up of races is determined by varying combinations of a number of characteristics" (1928: 130).

Given that genetic differences among human groups involve "varying combinations of a number of characteristics," the "typological" sorting of people into "racial" groups, based on specific alleles or genotypes each person possesses, is unavailable as a technique that can be used for a posteriori classification. Groups, not individuals, are the basic units of classification: a constellation of ABO blood group or allelic frequencies is a description of a group, not an individual, and the pattern with which these frequencies vary from one place to another sorts groups, not individuals, into "races." It is impossible to forego a priori classification altogether; some method of a priori grouping is required to establish a basis for calculating frequencies. Thus, judgment is required in a priori classification. Thrown into the mix of a priori classifications is quite a range of categories: political, linguistic, geographical, religious, national, racial, and ethnic. The methods of a priori classification used by several ABO blood group researchers like the Hirszfelds, Snyder, and Boyd reveal the nature of the categories used and the relations of their classification schemes to the empirical data. We need to consider whether the data—for example, differences in blood group allele or genotype frequencies—can be used to validate or reconstitute these groups, or whether the data simply classify, on the basis of a biological criterion, groups that may be of dubious biological significance. In addition, because alleles are rarely wholly present in one group and/or absent in another, a posteriori classification methods cannot be entirely objective but must involve judgment about where to draw lines of statistical distinction. We need also to consider, then, what a priori assumptions are implicated in the imposition of discontinuity upon continuity in a posteriori classification and in the illusions of continuity in map representations of frequency data that seem to discharge the a priori assumptions.

A sampling from anthropological genetics research—as the field is identified by Cavalli-Sforza—over its early decades reveals a transition in views on a priori classification. The Hirszfelds, pioneers of the field, simply assumed that the prevailing ways in which people were categorized in the social milieu were both biologically and anthropologically meaningful. In their a priori groupings, the Hirszfelds used the term “race” or “people” to refer to the group membership of the soldiers and refugees they tested. They understood such “racial” designations to be based on nationality or “national type.” The “races” or “nationalities” identified by the Hirszfelds, in examining “500–1000 persons of each race” (1919b: 37), were: “English,” “French,” “Italians,” “Germans,” “Austrians,” “Serbs,” “Greeks,” “Bulgarians,” “Arabs,” “Turks,” “Russians,” “Jews,” “Malagases,” “Negroes (Senegal),” “Annamese,” and “Indians.” At least to our ears, the Hirszfelds’ “nationalities” are not strictly political groups. Some of the designations like “English” and “German” carry the sense of “ethno-nation” that is prevalent in Europe. Others are more evidently what might be considered to be “ethnic” categories, for example, “Jewish” and “Arab.” Religion becomes a determinant of national identity in the use of “Mohammedan Macedonians” to represent “Turks.” When they questioned this designation because the “Mohammedan Macedonians” “must certainly contain a large admixture of Slav blood” (1919b: 37), the Hirszfelds seem to be assuming an ethnic category of “Slav” that is broader than “Serb,” “Bulgarian,” or “Russian.” And the representation of the Senegalese as “Negroes” appears to import the racial categories of the nineteenth century. The Hirszfelds’ demonstration that these “national types” differed in their frequencies of A and B was, in the late 1910s, a novel discovery. The Hirszfelds did not use the blood group data to reconstitute these groups in an a posteriori classification, though they did, in a sense, appeal to the data as validation for the a priori groups, in remarking that “the distribution of A and B corresponds with surprising accuracy to geographical situation” (1919b: 41).

During subsequent decades, we find increased attention to the biological and anthropological validity of a priori groupings. Snyder, during the 1920s, was skeptical about the Hirszfelds’ assumption that the ways in which people are classified in the social milieu are necessarily meaningful from the perspective of biology or anthropology. He believed that, while some prevailing modes of classification will be biologically or anthropologically significant, others are merely social or political. Snyder exhibited a greater wariness than the Hirszfelds in employing the term “race” to describe these categories. He referred to the groups he sampled as “nationalities” or “peoples” and insisted that these “nationalities” or “political groups” were not the units of “racial classification.” “[T]he German nationality,” he wrote, “is not a racial unit” (1926: 236). The “real races” that are the objects of anthropological research were those that have been identified by a “true racial study of blood groups” (1926: 235). Snyder considered his serological research on indigenous peoples of the Americas to come close to this standard. Presumably, then, “real races” cannot be identified on an a priori basis, but require a posteriori sanction. Nevertheless, a priori groups identified by Snyder such as “American Indians,” “Senegalese,” “Turks,” “Spanish Jews,” and “French” remained intact in his table of blood allele correlation (1926: 248, figure 4). So, while Snyder was skeptical about the Hirszfelds’ assumption that prevailing modes of classification were biologically and anthropologically meaningful, and though he hoped that blood group data would pick out “real races,” these data were not used to reconstitute the a priori groupings, which therefore remained the units of “racial” classification.
By the 1940s and 1950s, the evolutionary synthesis had influenced human geneticists like Boyd. Boyd’s fundamental units of “racial” classification were populations presumed to be random mating and in genetic equilibrium (Boyd 1953: 496); his a priori classification used the terms “population” or “ethnic group” instead of “race,” “nationality,” or “people.” Boyd’s tables of blood group frequencies included the geographical location where sampling took place, as well as “ethnic” or “tribal” identities. Whereas an earlier tabulation of blood group frequencies made only occasional reference to the geographical location where sampling took place (Snyder 1926), this was standard practice by the time Boyd published his Genetics and the Races of Man in 1950. Sample a priori designations found in Boyd’s charts detailing ABO frequencies include “American Indians (Kwakiutl), British Columbia,” “Basques, San Sebastián,” “Irish (Dublin),” and “Chinese (Peking)” (1950: 223–5). While Boyd’s a posteriori racial classification was to be established on the basis of differences in blood group frequencies among these groups, it can be seen that there was a mélange of identities involved in his a priori designations—racial, ethnic, national, and geographical. These designations were supposed to be justifiable on biological, and therefore, at least potentially, empirically ascertainable grounds. Individuals belong to the same biological population because of the increased likelihood that they will find a mate within the group. For Boyd, then, there were biological and anthropological criteria that justify a priori groupings: breeding populations are the basic units of “racial” classification. However, it is rare for these initial groupings to be arrived at empirically; more often, they are simply assumed, and therefore remain a priori designations that have been influenced by already-existing, but often highly contested, social and political categories. In the section that follows, we consider the intersection of social–political and biological–anthropological categories of classification.

Blood group data—similarities and differences in frequencies of alleles among these designated groups—are the basis for a posteriori (or “racial”) classification. These data achieve objective status through a chain of extractions carried out in the scientific work of: (1) identifying subjects from a priori classified populations; (2) sampling their blood; (3) determining blood type from hemagglutination tests; (4) aggregating data into population frequencies; and (5) locating frequencies on geographical maps or in quasi-geographical tables (Figure 6.2).

Objectivity is achieved, notwithstanding questions surrounding the scientific status of the a priori groups themselves, because of the abstraction of results (represented in maps and tables) from the particular, local, material, social, and cultural circumstances to which the scientists were led by their judgment in the use of a priori classifications of potential research subjects. However, it does not follow from scientific success in such objectifying movements that the subjectivities of judgment are dispelled. The notion of abstraction must first be resolved. Under the “Platonic” large gap notion of abstraction as the identification of a nonconcrete entity, subjective judgments are discharged when the formal relation between abstract (proposition, model) and concrete (inscription, phenomenon) is discovered. Subjectivity is mere means to that formal end. Under the

"Aristotelian" small gap notion of abstraction as the extraction in thought of a concrete entity from some of its properties, subjective judgments are integral, not only to the production of objectivity, but to its maintenance. The extracted data, results, and even representations alone do not determine a posteriori classifications; a priori assumptions remain involved. Indeed, what is depicted in map and table representations of population blood group frequencies could be said to be objectified judgments. Given that historical and geographical patterns of genetic variability between human groups are overwhelmingly quantitative, not qualitative, a posteriori (or “racial”) divisions inevitably impose discontinuity, represented by vertical jumps across the “line of being,” on continuity, represented by the horizontal chains of extractions and judgments (see Figure 6.2). There is interplay between objectivity and judgment, and cartographic representations are indicative of tradeoffs that are made.

Although the Hirszfords remarked on the geographical pattern of variability in the blood group frequencies of national groups, the articles in L’Anthropologie and The Lancet contain no geographical maps. The data are presented in a bar graph that orders the "national types" according to the relative frequency of B and a measure, invented by the Hirszfords, called the "biochemical race-index" that represents the ratio of A to B. The bar graph exhibits general geographical
biochemical race-index” generally coincide, the percentage frequency of B wins out where the indices are equal and, in the one case, the “Arab” and “Turk” groups, where these measures differ. Ordering by percentage frequency of A instead would have altered the representation significantly, for example, by moving “Turks” from the “intermediate type” to the “European type” and “Italians” in the opposite direction, and placing “Greeks” into proximity with the “French” and “Serbians” alongside the “English.” While the bar graph representation permits a visualization of the gradual variation moving from one “national type” to another, it also reveals that the boundaries of the three “types” impose discontinuity upon a continuous, roughly geographical, distribution pattern of percentage frequencies of A and B agglutination reactions. Clearly, there was a judgment to forego proportional spacing in the graph of the “national types” relative to one another in a way that corresponds to the quantitative differences among them. Increased and equal graph spacing is used to separate the three “types”—“European,” “Intermediate,” and “Asio-African”—from one another. Within the “Asio-African type,” the “Indian” group is offset more from the African groups than these are from each other. Thus, the space separating “Bulgarians” (“European type”) and “Arabs” (“Intermediate type”) is greater than the space separating the “English” and the “French” (both “European type”) though the differences in “biochemical race-index” and percentage frequency of B are less (index difference of 1.0 versus 1.3 and percentage frequency difference of 3.6 versus 4.0). The overall effect is that between-group differences are exaggerated and within-group differences are minimized in the Hirszfelds’ study.

Despite Snyder’s later insistence that the “nationalities” or “political groups” sampled by blood group researchers were not units of “racial classification” and that “real races” must be identified a posteriori on the basis of empirical evidence, it was the higher-order classification of the “nationalities” or “political groups” that the evidence bore on and not the constitution of these groups themselves. In his a posteriori classification, Snyder drew a distinction between “races” and “types.” He stipulated that when groups exhibiting similar blood group allele frequencies are classified as belonging to the same “type,” it should not be assumed that they belong to the same “race.” Two groups classified as belonging to the same “type” may have arrived at their similar allelic frequencies in different ways, for example, by mixture rather than descent. In other words, contemporary genetic composition alone determines “type,” whereas ancestry, in addition, determines “race.”

Snyder recognized the arbitrariness of the boundaries he drew. In his correlation table grouping “peoples” “more or less” into “natural groups,” he noted that it is, of course, arbitrary to use five-percent intervals of allelic frequencies to distinguish groups. The boundaries of these “natural groups” may just as well have been drawn at two-percent or ten-percent intervals (Figure 6.4). Snyder also stated explicitly that his combination of these “natural groups” into seven “types” (“European,” “Intermediate,” “Human,” “Indomanchurian,” “Afro-Malaysian,” “Pacific-American,” and “Australian”) was done “for convenience.” These “convenient” boundaries were not wholly arbitrary, however; certain a priori assumptions can be observed to lead to preferences for some boundaries over others. For example, although on the correlation table, “Danes” are as similar to “Australians” as they
are to (white) "Americans" or "Germans," they are included in the "European" and not the "Australian" type. And discrete boundaries for the "types," indicated by definite bounding lines, are also incorporated on the geographical distribution maps that appear at this time (Figure 6.5). These judgments, therefore, combine with more objective features of the maps such as the locations where sampling took place and the frequencies of the a priori groups themselves.

Boyd's mapping approach, a quarter century after Snyder, reveals a similar interplay between objectivity and judgment. Boyd's a posteriori "racial" classification, one he claimed to base on gene frequencies, includes a "hypothetical Early European group" “[r]epresented today by their modern descendants, the Basques,” a “European (Caucasoid) group,” an “African (Negroid) group,” an “Asiatic (Mongoloid) group,” an “American Indian group,” and an “Australoid group” (1950: 268). Classification was based on the MN blood groups, Rh factor, PTC tasting, and the "secreting gene," in addition to ABO (1950: 268–9). On the accompanying geographical map, the six “racial” groups are represented as discrete homogeneous types, their boundaries (apart from the Basque) coinciding with continental divisions (Figure 6.6). Boyd appreciated the geographical pattern of variability in gene frequencies: “It is encouraging that this classification corresponds well, on the whole, with the facts of geography” (1953: 506).

However, the map’s touted demonstration of the “correspondence” of Boyd’s "racial" classification with “the facts of geography” belies the interplay of objectivity and judgment involved in its construction. Boyd, like Snyder, made explicit mention of several of these judgments. He recognized that the discrete boundaries between a posteriori “racial” groups were imposed: “The method of gene frequencies is completely objective (subject to the qualification that our decision as to what boundary between frequencies is to separate two races remains always a man-made and arbitrary decision)” (1950: 273). He justified the inclusion of only indigenous peoples by appealing to the theoretical preferences of researchers:

Since we are not most interested in recent historical migrations, we have tried to avoid the confusing effect of recording data on recent migrants, and for America, the Pacific Islands, and Australia, we have plotted information only on the aborigines, omitting the results for the modern inhabitants, whose blood group frequencies, of course, are characteristic of their origins.

(Boyd 1950: 226–7)
Additional judgments, though, are obscured. There are no “objective” frequencies of blood group alleles at all, without categories of a priori group classification. Recall that Boyd’s a priori classification attempted to identify populations that are random mating and in genetic equilibrium, groups he referred to as “tribes” or “ethnic groups.” These populations would presumably be the units of a posteriori “racial” classification, placed into higher-order types on the basis of blood group allele frequencies. Although Boyd justified his use of an additional a priori category of classification—a distinction between “aborigines” and “nonaborigines” (or “original inhabitants” and “modern inhabitants”)—in terms of researchers’ lack of interest in recent migrations, this lack of interest itself calls for explanation. There is also judgment involved in deciding which contemporary populations to count as indigenous to a geographical location—Boyd believed in a single origin for *H. sapiens*, but not that all regions of the world were settled only once, each by a single group. The fixing of population to place is accompanied by an additional a priori category of classification that yields an a posteriori “racial” classification that is based on the comparison of blood group frequencies among continental groups, not the local populations that are supposed to be the units of classification. Justification is provided by the potential for continental borders to serve as barriers to migration and gene exchange. But judgment is involved in privileging these over other potential barriers and thereby ignoring the possibility of intra-continental frequency gradients more pronounced than inter-continental frequency gradients.

That humans differ quantitatively, not qualitatively, in the distribution of blood types among groups means not only that a priori classification is necessary, since it is groups not individuals that are the units of a posteriori classification, but that there is significant room for blood group researchers to diverge in their a posteriori approaches to classification. That judgment plays this role leaves the blood group researchers vulnerable to charges of lack of objectivity by HGDF proponents like Marks. It is important not to overlook that researchers like Snyder and Boyd explicitly recognized at least some of the “convenient” or “arbitrary” aspects of their classification schemes. It is also important to realize that this dichotomy between arbitrary/subjective and objective is misleading, and risks the too rapid dismissal of some interesting questions. The play of judgment is unlikely to be entirely arbitrary; theoretical preferences and commitments, practical concerns, or social values may well be implicated. And, crucially, as we argue in this chapter, such judgments may well be necessary for scientific objectivity. If we are to reach an adequate understanding of the historical legacy of the ABO blood group research and its implications for contemporary studies of human genome diversity, it is beneficial to explore reasons why particular choices are made, rather than others. In subsequent sections, we consider judgments and choices implicated in disputes that have arisen in a posteriori classification regarding the relative weight to assign serological data over traditional anthropological traits and whether “racial” classifications based on differences in blood group frequencies are unwarranted impositions of invented discontinuity on actual continuity.

**Intersections of social-political and biological-anthropological categories of classification**

As we saw in the last section, the Hirsfelds treated the “races,” “peoples,” and “national types” they sampled as biologically and anthropologically meaningful, despite the political foundations of these groups. Snyder recognized the Hirsfelds’ assumption to be problematic and argued that “nationalities” and “political groups” are not the appropriate units of “racial” classification, but he proposed no genuine solution. For Boyd, influenced by the evolutionary synthesis, breeding populations were the basic units of “racial” classification. Although such populations can be identified by ostensibly biological and anthropological criteria, social and political categories are frequently imported: groups referred to by Boyd include the “Irish” and the “Basque.” Contemporary population geneticists like Cavalli-Sforza consider the substitution of social-political group boundaries for biological-anthropological group boundaries to be expedient in many cases. This facilitates resampling and averts problems with statistical significance that arise in applying Hardy-Weinberg to small populations (Cavalli-Sforza et al. 1994: 20–1). Biological-anthropological entities—Mendelian populations or demes—are assumed to exist independently of the social-political units that substitute for them, even if they are not always easy to delineate or retrieve. However, Latour’s (1999) concept of circulating reference suggests that social-political categories may not be theoretically expendable. In this section, by exploring the relationships
between the groups delineated by the Hirszfelds and the political boundaries and social categories of their time, we consider ways in which social–political and biological–anthropological categories of classification intersect. 9

The social–political origins of the a priori classification used by the Hirszfelds can be understood, in part, due to the context provided by the First World War. Most of the “national types” identified by the Hirszfelds correspond to the nation-states at war: Britain, France, Italy, Serbia, Russia, and eventually Greece, on the side of the allied powers, and Germany, Austria, Bulgaria, and Turkey, on the side of the central powers. The allied forces occupied Salonika throughout the war. British and French naval forces landed at Salonika in October 1915, just over a year after the war began, and too late to save Serbia from the attack launched by Germany, Austria, and Bulgaria the preceding month. With the assistance of local Greeks, British and French soldiers fortified the city of Salonika, building an “entrenched camp” during the first four months of 1916 (Price 1918). By the summer of 1916, the Anglo-French forces in Salonika had been joined by Russian and Italian troops, as well as Serbian forces reconstituted after their retreat across Albania and evacuation to Allied-occupied Corfu. Greece was neutral when the British and French landed at Salonika in the fall of 1915, but eventually entered the war on the side of the Allies in 1917. The Hirszfelds drew blood from soldiers who found themselves in Salonika, both members of the allied forces and, in the case of captured POWs, members of the central power forces. The uniforms worn by these soldiers provide the evident basis for the Hirszfelds’ classification into “national types.”

This classification is less straightforward than it may appear at the outset, however. British forces included English, Welsh, Irish, and Scottish battalions, and therefore these categories were also available to the Hirszfelds. It is possible that the soldiers were collectively designated as “English,” because the other categories would not be as meaningful to the Polish Hirszfelds, as they would be to Britons. And not all soldiers belonging to the British and French forces were classified as “English” or “French”: colonial subjects of Britain and France, originating from India (a colony of Britain) or the Malagasy Republic (Madagascar), Annam (Vietnam), or Senegal (colonies of France), were grouped according to the boundaries of these colonial possessions. The Senegalese were the only “national type” that was also described in terms of the traditional racial categories of the nineteenth century, in this case, as “Negroes.” This is no doubt because European eyes saw “race,” not just “nationality,” when it came to sub-Saharan Africans. In this respect, the Hirszfelds’ classification is not so different from the characterization of the composition of the Allied forces present in Salonika by official war correspondent, G. Ward Price, of the Daily Mail:

SALONICA is a very museum of the Allies. Of the principal Allied Armies in the field only representatives of the Americans and Portuguese are lacking, and there used to be rumours that even they were coming. In the Balkans there is none of the isolation that keeps the armies of different nationalities apart in France. All of us rub shoulders at our common base of Salonica. The Annamite and the Serbian sit side by side in the tram without either finding the juxtaposition odd. A brigade of blond Russians may be relieved by a brigade of black Senegalese. Italian, Frenchman, Englishman and Greek will share a table in a restaurant, and it is very satisfactory to find that in spite of his customary ignorance of any language but his own,—in which respect he is no worse than the average Frenchman, however,—the Englishman seems as generally popular all round as any of the Allies.

(Price 1918, in Chapter IX, The Coming of the Russians and the Italians)

Not all individuals from whom blood was taken as representatives of the “national types” mentioned thus far were uniformed soldiers. Some of the designations, such as “Jews,” also fall outside of the “national type” categories as constituted by the at-war nations. To understand the origins of these designations, the history of the region of Salonika also needs to be considered.

Salonica was a pluralistic and cosmopolitan port city, part of the Ottoman Empire from the fifteenth century until Turkey’s defeat in the Balkan wars of 1912–13. Greece occupied Salonika in the war and formally took it over in 1913, despite Austria’s preference that it become an international city and Bulgaria’s own claims upon it. Surrounding Macedonia was divided between Greece, Serbia, and Bulgaria. Thus, at least for residents of the Salonika region, “Greek” had become only very recently a national identity when the Hirszfelds were taking blood samples at the end of the 1910s. Ottoman administrative units, millets, were based on religious affiliation. The 1900 census has Salonika’s 173,000 residents divided among some 80,000 Jews, 60,000 Muslims, and 30,000 Christians. Muslims answered to the Ottoman caliph (who was also the sultan); non-Muslims had their own patriarchs in Constantinople. These religious identities intersected with a range of linguistic, ethnic, and national identities. Ladino-speaking Sephardic Jews vastly outnumbered Aghahazi Jews. There were Greek- and Slavic-speaking Orthodox Christians who answered to the Greek patriarch and Bulgarian-speaking Orthodox Christians who had by this time gained their own patriarch. The Armenian patriarch oversaw the affairs of Armenian Christians whose church had always been independent of the Roman and Byzantine churches. There were also Gypsies, and when Serbia, Greece, and Bulgaria became independent states in the nineteenth century, and sought to extend their boundaries to include their Macedonian compatriots, Serbian, Greek, and Bulgarian became available as national identities. The decade leading up to the Balkan wars (1912–13) was a time of much repression in the region during the rule of Abdul Hamid who played these groups and the European powers off one another, and due to the failure of the “Young Turks” upon seizing power in 1908 to unify these groups under a secular Turkish national identity. Hence, the “national types” that comprised the Hirszfelds’ a priori classification were quite contested identities. Local “Mohammedan Macedonians” were used to represent “Turks,” but this substitutes a religious identity for nationality and ethnicity. Similarly, refugees from Monastir, whom the Hirszfelds used as representatives of a Jewish “national type,” shared a common religious identity but had varied ancestral roots.
Thus, the a priori classification used by the Hirszfelds reflects the composition and structure of WWI military forces, regional and global geo-political struggles, and local history. H.R. Wilkinson's book *Maps and Politics: A Review of the Ethnographic Cartography of Macedonia* looks at how maps have been used as political devices in the region of Macedonia, put forward to substantiate the claims of rival powers. Any assignments of individuals to one “national” category or another will necessarily privilege some set of competing interests over others. Yet, “national type” and the particular array of nationalities used in their study were not assumed by the Hirszfelds to be at all problematic as an a priori categorization for anthropological research.

The access the Hirszfelds had to soldiers and refugees from different parts of the world because of the war provided them with an unusual opportunity, but even they used the results of studies that had been previously carried out for a portion of their data. Subsequent efforts by blood group researchers to map the global distribution of the ABO alleles relied to a far greater degree on data from multiple sources. Researchers like Snyder and Boyd were responsible for some of the data included in their comprehensive studies; these were data obtained from the analysis of blood samples they themselves collected, or blood samples sent to them by anthropologists in the field or physicians in the clinic. Other data were taken from the published or unpublished results of researchers working in genetics, physiology, medicine, and anthropology. Extensive data, especially for large cities in industrialized countries, became available to researchers through the cooperation of agencies involved in transfusion services and paternity testing. Hence, the data that are presented in comprehensive global surveys of blood group distributions have been gleaned from a vast number of sources and a wide array of research studies. These studies have been informed by a diversity of aims and interests, both practical and theoretical. This raises questions about methodology: how people are sampled, how samples are transported, stored, and analyzed, and how classifications travel between lab and field. Do the group classifications found in published tables and maps of ABO frequencies refer to real human groups?

Latour criticizes philosophers for attempting to solve the problem of reference while ignoring the details of scientific practice. Latour's concept of circulating reference calls attention to the careful and painstaking work scientists perform that establishes and maintains concrete ties to and in the world, in their travels to the field and back to the lab and desk. Adopting an empirical approach to the problem of reference, Latour travels to the Amazon basin with a botanist, a geographer, and two soil scientists. The goal of the scientific study is to collect data on the characteristics of the soil in a region where the savanna borders the forest (a “natural” not “man-made” boundary, Latour points out) in order to address the question whether the forest is receding and the savanna advancing, or vice versa. Latour follows the “movement of abstraction” that proceeds from the Amazonian landscape to its diagrammatic representation in the publication that ensues, in a series of intermediary steps that “pack the world into words” (1999: 24). Each of these steps involves the extraction of matter to be used subsequently as a form in the representation of a new phenomenon, or referent. The bagged soil sample is marked with the number of the hole and the depth at which it was taken. The field notebook records, for each sample, the location's coordinates, the number of the hole, the time and depth at which it was taken, and qualitative data like color and texture. Soil samples are sorted by depth and compared by being placed into location-coded cardboard cubes arranged in a coordinate system within a transportable wooden frame. Soil color is attributed a numerical value according to a common standard. These quantities and relations are represented in charts, diagrams, equations, maps, or sketches. Latour points out that at no point along this chain of reference is an opening created that resembles the traditional “gap in representation” that must be bridged by “correspondence.” Discontinuity is introduced: the diagram does not resemble the tray of samples, the notebook etchings do not resemble the landscape. But continuity is always maintained; in each successive phenomenon, there is a “trace” of the former. This means that the chain of reference is reversible. “Traceability” in the “downstream” direction permits the circulation of truth-value in scientific discourse and connects the scientific text, and its internal referents like charts and diagrams, to the world through each intermediary step.

Latour emphasizes that the circulation of reference that is responsible for the construction of objects of scientific knowledge, and the ability to make truth claims about these objects, would not be possible without preexisting formal structures: “Yes, scientists master the world, but only if the world comes to them in the form of two-dimensional, superposable, combinable inscriptions” (1999: 29). At no point do investigators engage in the “observation of raw data,” as traditional empiricists assume. The Amazonian field site is a “minimalist” laboratory: the botanist tags trees to establish a system of Cartesian coordinates for her plot of land, the soil scientist marks his holes by using a compass for angles and the unraveling spooled thread of a “topofil” for distances, a numbering system is devised for the soil samples, the protocol for notebook records is agreed upon, the “pedocomparator” furnishes a two-dimensional coordinate system that can be transposed to a paper diagram, soil scientists join cartographers and painters in adopting the Munsell code of colors as a common standard, and so on. And knowledge of this small part of the world is possible only if the investigators can locate the site and make their way there. For this, they need the help of additional inscriptions, an atlas map of Amazonia juxtaposed with aerial photographs—inscriptions—Latour notes, that would not exist without the disciplines of trigonometry, cartography, and geography, the labor of draftsmen, engravers, and printers, and technologies like plane radar and orbiting satellites. They also need access to funding sources and institutional support. This entire network is required for the circulation of reference. If the network breaks down, reference disappears.

Just like the atlas map and aerial photographs that permit this research team to travel to the Amazonian location that interests them, political maps are far more useful than geographical maps in facilitating field work in biological and anthropological studies of most human populations. Planes and trains travel to countries

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and cities, not river valleys and mountaintops. Because human population geneticists and biological anthropologists rely on multiple sources for genetic data and may be involved in collaborations with cultural anthropologists, linguists, medical researchers, and others, social and political boundaries may provide the best common denominator. The group consent model for population-based biological and anthropological research also relies on establishing contact with leaders of social or political units (Reardon 2002: ch. 5). There are many reasons why researchers find it expedient to substitute social-political categories of classification for biological-anthropological ones. At the same time, they consider these to be practical concerns; from a theoretical point of view, they do not doubt the independent existence of biological-anthropological entities (Mendelian populations or demes), and they seek to eliminate the social and political “contaminants” of “objective” knowledge. However, Latour’s concept of circulating reference suggests that these assumptions may be false. If reference is lost once the network that supports its circulation breaks down, any social-political categories imported by a priori classifications cannot simply be abandoned.

Recall Latour’s remark that the researchers he was accompanying were interested in a boundary between savanna and forest that was “natural,” not “man-made.” Biological-anthropological categories of classification similarly aim to represent “natural” discontinuities. Presumably, adopting a naturalistic approach to the study of human genome diversity would avoid making a priori assumptions about population or, for the Herszfelds, “racial” boundaries, permitting such boundaries to be revealed instead by existing genetic discontinuities. This approach might take blood samples from a specified number of individuals found at, or within a specified distance from, intersecting points of a geographical grid where the coordinates of the grid have been established by a map that includes natural landmarks such as oceans, rivers, or mountain ranges. This would conceivably establish a chain of reference, connecting sampling location, the individual sampled, the labeled blood sample transported from field to lab, the coded vial of blood stored in the lab refrigerator, the written record of the blood group based on agglutination tests, and, finally, the data point on a chart or map. The avoidance of social-political categories of classification would also protect objectivity, understood in the traditional sense.

There are problems with the above scenario, however. As we have already discussed, blood group or allelic frequencies describe groups, not individuals, and it is these values that are tabled and mapped as the bases for a posteriori classification. To retrace the chain of reference for a particular data point on a geographical map is to end up with the collection of individuals from whom blood was drawn and at the location where blood was drawn. Of course, specific individuals do not interest the population geneticist or biological anthropologist; just as the numbered bag of soil is understood to be representative of soil found at a given location, so the sampled individuals are taken to be representative of a larger group that resides at the location. Continuity in reference over time is also assumed for both the soil and the group; an increase in clay content of the soil or a decrease in frequency of a particular allele is likely to be of theoretical interest. The chain of reference will be broken if researchers return to the field location at a later date and do not encounter individuals belonging to the same group that had been sampled. While, for the soil scientist, geographical location may suffice to fix reference, it cannot satisfy the population geneticist or biological anthropologist. This is reflected in the limitations of an ostensibly naturalistic geographical grid strategy for sampling human genome diversity.

As plans for the HGDP unfolded, a debate arose concerning whether a geographical grid or population-based approach should be used for sampling (Roberts 1991). Even proponents of the grid approach like Allan Wilson did not rely simply on geographical location to fix reference. They called for only “indigenous” peoples to be sampled at each point in the grid. This is a strategy that permits reference to circulate by maintaining a connection between people and place. But, at the same time, it demands the creation of boundaries, both spatial and temporal. What are the boundaries of a person’s territory to which she is indigenous? How many of a person’s ancestors must have lived in that territory, and for what duration of time, for him to be indigenous? The natural landmarks of geographical maps that serve as grid coordinates cannot provide the basis for a priori classification. Natural landmarks serve as boundaries for people only if these people represent them as boundaries, that is, imbue them with social-cultural meanings and transmit these meanings across generations. And if people represent these as group boundaries, social organization into groups must already exist. Thus, social-political categories of a priori classification are not simply a matter of practical expedience for biologists and anthropologists. The naturalistic approach is unrealistic because it prohibits the circulation of reference. If we expect evolutionary narratives and explanations to say something about the world, social-political categories of a priori classification are not even theoretically expendable.

Privileging blood groups over traditional anthropological traits in classification

Particularly significant to Cavalli-Sforza, in his consideration of the historical legacy of ABO blood group research, is the pioneering by the Herszfelds of a new approach to the study of human variation through “the introduction of genetic markers, which are strictly inherited and basically immune to the problem of rapid changes induced by the environment” (Cavalli-Sforza et al. 1994: 18). The use of biological reagents to determine blood group differences continued to be used over the next few decades, until the mid-century introduction of electrophoresis to discern protein differences, and, finally, in recent years, the availability of restriction enzymes and PCR to identify DNA differences. In contrast, Marks (1996) regards the serological research initiated by the Herszfelds as an early attempt by non-anthropologists to supersede physical anthropologists by treating their own data, for example blood group frequencies, as more important in the classification of human groups than traditional anthropological characters like skin color and stature. Marks contends that the “racial serologists” improperly substituted data on
blood groups for data on a range of characteristics, including those traditionally used by physical anthropologists. According to Marks, physical anthropologists had “effectively” integrated genetics into their discipline by the 1960s, analyzing blood samples they collected in the field. But blood group data never trumped other data. Today’s focus on DNA data continues this usurping trend.

Given the interplay between objectivity and judgment, it is worth examining the justifications blood group researchers like the Hirsfelds, Snyder, Haldane, and Boyd provided for abandoning the anthropometric data traditionally used by physical anthropologists for “racial” classification and the study of human evolution. The blood groups are not evidently useful traits for classification. Since the Hirsfelds’ time, it has been recognized that blood group allele frequencies vary quantitatively, not qualitatively, which means that the blood groups cannot provide “racial” characters, or markers, that sort individual humans into discrete groups. Also, since the Hirsfelds’ time, it has been recognized that serological characteristics appear to vary independently of the characteristics that anthropologists had traditionally used for classification (Davis 1935).

The Hirsfelds noted that the blood groups appeared to be inherited as independent traits in conformity with Mendel’s law (though mistaken in their belief that A and B are inherited as dominant alleles segregating at two loci), to stay constant in individuals over time even in the presence of conditions affecting the blood like anemia, malaria, and typhoid, and to remain intact when passed from parents to offspring. They also noted that environmental factors like climate, diet, and disease could not have affected the differences in blood group frequencies because these were shared by members of all of the “national types” represented in Salonika during the war (aside from the vegetarianism of the Indians). It was thus the hereditary nature of the blood groups that recommended them to the Hirsfelds. Their decision to privilege serological over traditional anthropological traits in classification therefore did not deviate from, but concurred with, anthropologists’ views that “racial” characters should be inherited traits and, by implication, that “races” should be hereditary groups.

Snyder took a similar tack: “In the human race the blood groups occur as fixed bi-chemical conditions, subject to the laws of heredity. As such they provide a method of studying racial origins and relationships” (1926: 233). Snyder presented a couple of additional reasons for adopting a serological approach to “studies of racial relationships”: the blood groups appeared to be unaffected by constitutional factors like age or sex and environmental factors like climate, living conditions, or x-rays, and, apart from the unresolved question of linkage to disease traits, “it is difficult to conceive of any effect of selection on the proportions of the groups” (1926: 251). For Snyder, then, the constancy of the blood groups over time promised access to knowledge of genealogical origins and relations; “races” became not just hereditary, but genealogical, groups. However, in his 1929 book, likely in response to serology’s critics, Snyder emphasized that serology is not to be taken as authoritative: “the blood groups are simply additional anthropological characters which must take their place along with other better-known criteria in the study of racial relationships” (1929: 117). This served as no

more than a disclaimer, though; Snyder proceeded to use only blood groups for “racial” classification and even to invent four “laws of serological race-classification” based on the correlation of proximity of genealogical relationships to similarities in blood group frequencies.

Haldane (1931) defended outright the superiority of “racial” classification on the basis of blood group frequencies. In contrast to traits traditionally used by anthropologists, ABO differences appeared to be monogenic, genetically determined, unsusceptible to environmental modification, and selectively neutral. Haldane believed that traditional anthropological traits like skin color are often adaptive and consequently of limited value for the study of evolutionary history. One reason is convergent evolution: adaptive traits may arise independently in different populations exposed to similar environments—for example, dark skin color across tropical regions in response to sunlight—and thus confuse questions of origins. In contrast, Haldane asserted, “we may take the proportions of the blood group genes in any population as indicating racial origin rather than effects of climate or other environmental influences” (1931:1933: 67). A second reason is that adaptive traits may increase in frequency relatively quickly in populations, whereas selectively neutral traits, unless the population size is small, will stay fairly constant in frequency. As a result, Haldane concluded, adaptive and neutral traits can serve different purposes for “racial” classification:

We may perhaps compare the information given by different characters on the structure of a population with that given by different rocks on the structure of a country. The more highly adaptive characters, such as pigmentation, give most information on the immediate past (for example, the racial origins of the peoples of the United States), just as the recent and Pliocene deposits tell of recent glaciation, vulcanism, and so on. The blood-groups on the other hand give information of a more fundamental character on racial structure, just as do the palaeozoic rocks on geological structure. The contradiction between the two sources of information is thus only apparent.

(Haldane 1940: 477)

Haldane, along with the Hirsfelds and Snyder, by privileging the blood groups as inherited traits, constructed a hereditary “race” concept and “races” as hereditary groups. For Haldane, like Snyder, “race” was about origins and genealogical relations, not adaptive similarities; “races” were genealogical groups or clades, not ecotypes. In Haldane’s case, the deepest origins and most distant genealogical ties were “fundamental”; the blood groups were preferred not just because they provide the most reliable passport to our human evolutionary past but because they are supposed to be the longest persisting record of that past.

On the one hand, the blood group researchers privileged serological over traditional anthropological traits because they shared a desire for objectivity: the hereditary transmission of ABO was believed known and environmental influences on the development of the blood group phenotypes were believed controlled or irrelevant. On the other hand, judgments were involved, judgments
which at the same time served to construct a concept of “race” and “racial” groups themselves. Such judgments are not arbitrary; they reflect the researchers’ shared theoretical interests in historical questions concerning origins, migrations, and genealogical relations rather than causal questions directed to mechanisms of evolutionary change like selection and drift. These judgments, and the theoretical preferences and commitments that direct them, are implicated at each link of the chain of reference that connects blood samples in the field to published tables and maps of blood group frequencies. Although this “movement of abstraction” obscures such subjectivities as judgment, it may be possible to recover them by attending closely to the mapping representations.

Besides the bar graph and geographical distribution map representations found in papers by the Hirsfelds and Snyder, blood group researchers made use of geographical frequency maps to address historical evolutionary questions about origins, migrations, and genealogical relations. In a 1931 article, “Prehistory in the Light of Genetics,” Haldane published a geographical frequency map for the B allele that made use of isolines to connect equivalent values for allelic frequencies (using five-percent intervals) at different geographical sites. Assumptions built into a diffusion model allow the isolines to represent a third dimension on the map, that of time. The model assumes that a given allele originated at the geographical location where its present frequency is greatest. The allele is assumed to have “diffused” outwardly from its site of origin in all directions—witness the curvature of the isolines that connect points of equal frequency on the map (Figure 6.7). The movement of people is assumed to account for the “diffusion” of alleles. Historical migrations are assumed to have extended as far in distance from the site of origin as the particular allele is found. Multiple sites of origin for an allele are entertained as a possibility if centers of high frequency accompanied by a radiating pattern of diminishing frequencies occur in different regions of the globe. Additional assumptions permit the further temporalization of events. If a gene mutates a number of times, and it is possible to ascertain which alleles are ancestral to others, the relative timing of different historical migrations might be surmised. Haldane assumed that the most geographically widespread alleles are the oldest.

On the basis of the map of B allele frequencies, and discussion of the geographical distribution of A allele frequencies, Haldane concluded: “The general result of blood group studies . . . is to point to a migration in all directions from Central Asia into a more primitive population” (1931[1933]: 71). Haldane held that the B allele arose in Central Asia with migration outward during prehistoric times extending as far as Western Europe but not reaching the Americas or Australia. The more widespread A allele he believed arose either far earlier than the B allele or in several locations. Since the O/R allele was found to be most prevalent across the globe, Haldane considered it to be the ancestral state and characteristic of the most “primitive” humans. This ruled out a historical migration outward from the center of the O/R allele’s highest frequency in North America.

In a 1935 article, “Human Blood Groups and Anthropology,” Leland C. Wyman and Boyd presented geographical frequency distribution maps using isolines for the A and B genes (p and q alleles). The map for B was based on Haldane’s 1931 map and additional data (Figure 6.8). One notable difference is that where Haldane’s map superimposes isolines on raw frequency data at five-percent intervals, Wyman and Boyd’s map includes only the isolines themselves, a judgment that maximizes the map’s appearance of continuity in frequencies. Wyman and Boyd noted that their map seemed to support Haldane’s hypothesis: “If the map for the gene B (q) is examined, it will be seen that the high center in Asia near the Punjab, and the way the contour lines surround it, are strongly suggestive of the origin of this factor in the region of northern India, and its subsequent spread into other parts of the world, including Europe” (1935: 186). However, Wyman and Boyd did not accept this hypothesis. They contended that blood group differences reflected a pattern of dispersal that was older than “the differentiation of the present races” (1935: 192). They believed that there had been inadequate time for multiple independent mutations of A and B to have arisen and spread following inhabitation of the New World, unless the blood groups are affected by selection which appeared unlikely. In addition, the B gene had been discovered in indigenous South Americans, its frequency higher than could be accounted for by recent mixture with Europeans and without additional genetic evidence of

Figure 6.7 Geographical frequency map for the blood-group B allele in human populations using isolines to connect equivalent allelic frequency values (five-percent intervals) at different geographical sites. Both values and isolines are plotted (Haldane 1931, figure 1). Reprinted by permission of HarperCollins Publishers Inc.
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Ancestral–descendant relations hold between groups of people. The use of isolines (and isopleths where intervening spaces are filled in) effaces the discontinuities of assumed group boundaries. By replacing discontinuities with continuities, this step in the “movement of abstraction” attempts to discharge the subjectivities associated with the application of a priori categories of group classification that is required for the objectification of quantitative differences in blood group allele frequencies.

To this point, we have interpreted the maps in terms of a fairly straightforward Latourian story of small judgments and small extractions generating a reference chain and objectivity secured by the possibility of backtracking to the source. The steps needed to produce geographic maps, particularly the isoline maps we have been discussing, require further cartographic consideration.

Thematic mapping represents the geographic occurrence and variation of one or a few phenomena. This cartographic form matured in the mid-nineteenth century as a result of advances in cartographic technique such as lithography and the use of tones, shading, and color, of advances in world-wide base map production (as, for example in Berhaus’ 1848 atlas and Johnson’s English language reprint), and with increased interest in and development of statistical data (Robinson 1982).

The epistemology of thematic mapping work—the discovery of geographical structure through mapping phenomena—we argue, is caught between the imagery of nineteenth-century mechanical objectivity and twentieth-century scientific expertise. On the one hand, maps epitomize a form of mechanical, if humanly constructed, objectivity. The rhetoric of mapping places humans outside the epistemic loop since maps are supposed to express isomorphisms between representation and geographic reality. Thus, thematic mapping, which involves plotting data on maps, is a quintessential “objectifying move” in the mechanical sense that data are extracted from the geographic setting and plotted in a mechanical way according to a mathematical coordinate system that avoids human bias, judgment, and interpretation. Thus, any discovered patterns are “there” to be discovered. On the other hand, the discovery of geographic pattern requires some classification system, else the pattern is a mere report of uninterpreted data. This “perennial cartographic problem” (Robinson 1982: 139) is reflected in the tension between mechanical objectivity and expert judgment in the mapping of human blood group frequencies. “The method of gene frequencies is completely objective,” Boyd (1950: 273) wrote, in the cartographic sense that frequency data correctly plotted on accurate base maps cannot lie. However, the method is dependent on initiating and continuing scientific judgments of an appropriate classificatory scheme if geographical patterns in the data are to be discovered. Without an a priori classification of either data groups or geographic units, there can be no pattern “in” the data.

The maps do not plot actual blood type data, but rather, blood group frequencies. Frequencies are population properties which thus do not occur at points on geographic maps, but rather cover geographic regions. The numbers indicating frequencies are instances of “map generalization.” Awareness of the man-made...
and arbitrary decision to assign a mapped point to cover a geographic area is carried throughout the debate about anthropological genetics as evidenced, for example, by this statement of Cavalli-Sforza's: "Gene frequencies are not geographic features like altitude or compass direction, which can be measured precisely at any point on the earth's surface; rather, they are properties of a population that occupies an area of finite extent" (2000: 26). Moreover, because geographic patterns might not be discernible even if the frequencies are plotted, isolines are often drawn to connect equal blood group frequencies. Similar to topographic lines of equal elevation, albeit not applying literally to geographic points as do elevations, blood group frequency isolines seem to indicate geographically continuous frequency distributions. However, the isolines represent deliberate falsehoods in two respects. First, the lines very rarely connect all the equal frequencies on the world base map and sometimes they connect areas that cannot represent equal frequencies. Continents and other widely separated geographic areas having equal frequency points are typically not connected by isolines and, just as clearly, frequencies of blood groups across continents could not be so connected since blood group frequencies in the oceans (and across other remote geographic features) are zero. Second, there is the arbitrary and man-made decision to choose a frequency interval between isolines to plot data on geographic maps, with the implication that the interpolated values between isolines represent valid inferences of blood group frequency. Both of these idealizations reflect the view that frequency data must be continuous, but the lines are no mere interpolations: they give the impression of data where none exists.

Once it is recognized that the ABO system has always existed in H. sapiens, leaving room neither for material overlap of ancestral and descendant genes nor for hypothesized mutation events or "pure races" of A, B, and O individuals, historical narratives about origins and migrations become problematic. Left are similarities in relative ABO frequencies as the basis for inferring the proximity of genealogical ties between populations separated in space. The continuity of these populations over time is assumed, just like the continuity of chromosomes over time is assumed when numbers of inversions are used in D. pseudosauca as a measure of genealogical proximity. It is also assumed that selection is not operating, as geographical location would then matter. Latour's concept of circulating reference is again helpful. We have seen that attempts to discard social-political categories of a priori classification interrupt the "traceability" of the chain of reference and consequently interfere with the circulation of reference. Historical questions concerning origins, migrations, and genealogical relations make further demands on fixing reference. Reliance on an a priori category of classification like indigenous-nonindigenous to fix people to place becomes unavailable to the researcher whose interests lie with these historical questions. Migrations occur between geographical locations and genealogical ties connect people living in different places. For the circulation of reference to be maintained, researchers need to fix group identity across space and time. This need is reflected in Boyd's (1950) presentation of a series of four maps that postulates a hypothetical history of human migration that could account for contemporary distributions of blood allele frequencies. The series begins with an original population in Asia that possesses a characteristic distribution of blood allele frequencies and then traces the dispersal of descendant populations of this group to all continents of the world (Figure 6.9). In contrast to "isogeme" maps, these geographical frequency maps are more strongly suggestive of the existence of underlying groups since the relative frequencies of all alleles are included.

In his critique of the HGDP, Marks takes contemporary human population geneticists to task for sharing with "racialserologists" a reliance on simplistic migration hypotheses: "the major historical processes invoked to explain contemporary patterns of diversity are essentially the same as the most quickly discredited ideas of early serologists: mass invasions of pure and qualitatively distinct primordial races imposing their gene pools upon others" (1996: 359). The Hirsfelds did indeed subscribe to the a priori assumption that groups once existed that were "pure" or genetically homogeneous—an A "race" and a B "race." The story of human evolution became a narrative of originally "pure" races blending through migration to become the "mixed" groups—the "national types" and the continental "types"—of our time. Without doubt, the Hirsfelds were mistaken in this assumption, but historical explanations of blood group frequencies in terms of migration and mixture were common among the serologists, and such evolutionary narratives continue to be employed today. But more important than this continuity itself are a number of issues implicated in it.

Vestiges of long-disproved assumptions of "pure and qualitatively distinct primordial races imposing their gene pools" persist because of the need to fix group identity across space and time if circulating reference is to be maintained when historical questions concerning origins, migrations, and genealogical relations are addressed. We cannot speak of group origins or unique common ancestors without well-delineated "primordial" groups locatable in space and time. We cannot speak of dates and routes of group migrations without assuming the constancy and integrity of these groups over space and time. We cannot speak of genealogical relationships other than between "qualitatively distinct" groups for which there are sorting criteria for inclusion and exclusion. We cannot speak of the admixture of groups without some modified sense of "purity" in terms of the relative homogeneity and heterogeneity of "qualitatively distinct" "gene pools" with characteristic compositions. The bounding of genes—and the people who possess and pass on these genes—across space and time is a necessary a priori assumption for all such narratives or explanations. Strict endogamy will, of course, do the trick, if it can be established that there exists a socially discernible group such that mates are chosen only within the group and group members can trace all of their ancestral roots back to that group, or to a well-delineated ancestral group.

Hence, we find in Haldane's interpretation of the Hirsfelds' results, an attempt to establish the legitimacy of a priori assumptions that are required if blood group frequencies are to be informative about "racial" origins and relationships. Haldane noted the anthropological value of the human blood groups as nonselective traits that made it possible to "determine the proportions of pure
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The proportions [of genes A, B, and R] are constant in a given race, and are not affected by changed environment in the course of a few centuries. Thus at Salonika there have been endogamous Greek, Turkish, and Jewish communities since the late fifteenth century. The Greek proportions are typically Balkan. The Jews are much the same as Arabs. The Turks are decidedly Asiatic.

Clearly, then, we may take the proportions of the blood group genes in any population as indicating racial origin rather than effects of climate or other environmental influences.

(1931[1933]: 67)

This mention of "purity" seems uncharacteristic of a scientist Elazar Barkan describes as one of a small group of British biologists who contributed to the fight against scientific racism prior to the Second World War by demonstrating the incompatibility of Mendelian principles with notions of "racial purity" and the construction of "racial" typologies (1992: 162). Haldane appealed to "purity" in a relative sense. Because according to Hardy-Weinberg, genetic equilibrium is reached in one generation of random mating, it is possible to determine the relative proportions two or more parental populations have contributed to a "mixed" or "heterogeneous" population. The parental populations are regarded as relatively unmixed or homogeneous and in possession of characteristic constellations of allelic frequencies treated as genetic identities that remain constant across space and time.

We see this today in HGDP plans to sample "isolated" and "primitive" populations, and the urgency attached to carrying this out before these groups disappear. Just as geneticists in the USA have used African-Americans and Asian-Americans as proxies for African and Asian populations that are difficult to access because of spatial distance, the peoples targeted by the HGDP are to be proxies—regarded as "remnants"—for temporally distant populations living centuries and millennia ago. Historical narratives about migrations and mixtures cannot be told without these proxies, and which groups become proxies will depend on researchers' interests—and assumptions about which groups' origins matter, a choice that is deeply embedded in social and cultural milieu. This is illustrated in Nurit Kirsh's (2003) "Population Genetics in Israel in the 1950s: The Unconscious Internalization of Ideology." Kirsh points out ways in which post-WWII population genetics research in Israel was shaped by the Zionist narrative. This narrative about the return of Jews to an ancient homeland from which they had long ago been dispersed assumes the maintenance of biological as well as religious identity across space and time. Kirsh presents evidence that Israeli geneticists were influenced, perhaps unconsciously, by Zionism. More
so than their non-Israeli counterparts, they tended to stress genetic similarities and minimize genetic differences among Jews across space and time. When attention to genetic differences could not be avoided, explanations were preferred that did not make reference to intermarriage and gene influx from non-Jewish populations. Israeli Arabs, and therefore their genealogical ties to Israeli Jews, were virtually ignored in the studies.

Marks’ criticism of population geneticist proponents of the HGDP for sharing “racial serologists”’ reliance on simplistic anthropological hypotheses implicates an additional question, that is, whether alternate hypotheses, besides migration and “racial” mixture, were seriously entertained by the blood group researchers.

Recall the Hirszfeld’s bar graph representation of frequencies of A and B in “national types” and their a posteriori grouping of these into three higher-order “types”: “the European type,” “the intermediate type,” and the “Asiatic-African type” (Figure 6.3). The phenomena of interest to the Hirszfelds were “marked differences in the incidence of A and B in the different races” (1919b: 38) and “that the distribution of A and B corresponds with surprising accuracy to geographical situation” (1919b: 41). The Hirszfelds considered two possible explanations of the geographical distribution of blood group differences they observed, one causal and one historical. They rejected as “improbable” a role for natural selection due to climatic differences, pointing, for example, to similar proportions of B in people from Russia and from Madagascar. Instead, they favored a hypothesis that “two different biochemical races…arose in different places” and have since mixed (1919b: 42). Suggesting a “possible double origin of the human race,” the Hirszfelds placed the “cradle of one part of humanity” in India where the frequency of B is highest and the “cradle” of the other part in “North or Central Europe” where the frequency of B is highest (1919b: 42).

The Hirszfeld’s approach mirrors that of Latour’s soil scientists who, from a sample of Amazonian soil, followed a “movement of abstraction” that generated factual support for a hypothesis about whether the savanna or the forest is advancing. The Hirszfelds’ efforts began with blood samples of individuals grouped by nationality, proceeded through intermediary steps such as agglutination tests and statistical analysis, and culminated in a journal article where evidence depicted in words, tables, and a bar graph representation was marshaled in support of a hypothesis about human evolution. The Hirszfelds’ hypothesis that H. sapiens originated separately in two places and subsequently spread across the globe through migration and “racial” mixture can aspire to explain the world in so far as the chain of reference is reversible. “Traceability” connects words to the world, beginning with the scientific text and its internal referents like charts and diagrams, proceeding through intermediary steps of statistical analysis and agglutination testing, and ending up with actual blood samples and people sampled.

“Packing the world into words” enables scientific theories to find, or fail to find, empirical support. The process of objectification that accompanies abstraction in the movement from blood samples to texts and purports to discharge the subjectivities of a priori and a posteriori classification makes this portrayal of the evidential weighting of competing hypotheses possible. However, the Hirszfelds’ hypothesis can claim to provide a superior explanatory account of the blood group frequency data only if its competitors—a causal hypothesis about natural selection due to climatic differences or a historical claim of a single origin for the species—are genuine contenders. Genuine contenders share the same referent, or, rather, chain of reference. At each and every link along the chain of reference that connects words back to world, the abstract judgments that made material extraction possible must be shared. It is both abstract and material ties between world and word that make the circulation of reference, and the evidential weighting of competing hypotheses, possible.

As we have seen in this section, choices that privilege certain traits over others in a posteriori classification contribute to a process that constructs both “race” concepts and “racial” groups. The privileging of traits other than the blood groups may have resulted in quite different “race” concepts and different “racial” groups. This raises the question whether any particular system of “racial” classification can be authoritative, and on what basis. The next section looks specifically at “racial” classification.

“Racial” classification

Marks argues that the “real units of human diversity” are the “small biopackages” called populations, not “races” (1995: 116, 274). Races do not exist, because populations cannot be classified into higher-order types due to the fact that genetic differences between them are quantitative, not qualitative. Species genome diversity, Marks writes, is “geographically patterned”; it is generally the case that “people are similar to those nearby and different from those far away” (2000: 9). Given that gene frequencies change gradually and continuously across neighboring populations, Marks rejects the typological division of humans into a small number of discrete “races” (corresponding, e.g., to the longstanding division of “Caucasoid,” “Negroid,” and “Mongoloid,” or some related rendition).

Marks criticizes the “racial serologists” for failing to appreciate the clinal pattern of ABO blood group distribution. He believes that this was due to their a priori expectations of finding differences between groups they considered to be “races”:

Today we use ABO as a paradigmatic demonstration of the absence of discrete racial groupings among the aboriginal populations of the world. Populations differ from one another in a quantitative, gradualistic manner…Curiously, though, that clinal pattern was not immediately put forth as inherent in these data by the earliest exponents of blood-group data for physical anthropology. What they presented as results, rather, was what folk wisdom and contemporary anthropology had led them to expect to find: qualitatively different major groupings of people, or races.

(Marks 1996: 346)

These invented “racial” divisions are described by Marks as the imposition of discontinuities on an underlying reality of blood group frequencies continuously
distributed across the species. The result was the biological reification of fictive “races” illegitimately assumed to exist from the outset. It is along these lines that Marks and Rachel Silverman (2000) characterize Boyd’s “racial” classification as circular. Recall Boyd’s a priori and a posteriori classifications discussed earlier—the a priori classification of breeding populations and an a posteriori “racial” classification consisting of six groups (“hypothetical Early European,” “European (Caucasoid),” “African (Negroid),” “Asiatic (Mongoloid),” “American Indian,” and “Australoid” (Figure 6.6)). Whereas Cavalli-Sforza credits Boyd for his use of three blood groups (ABO, RH, MN) to differentiate populations on the five continents from one another (2000: 16), Silverman argues that Boyd used blood group data simply to validate his a priori assumption that continental borders constitute “racial” divisions: “In essence, Boyd’s races were based on large continental divisions on which he imposed gene-frequency differences. He first marked out racial delineations, and then made the blood group frequencies fit within these divisions” (2000: 14; italics in original).

Marks recounts a history of ambivalent relationships between blood group researchers and their anthropologist contemporaries. Anthropologists greeted the Hirsfelds’ work with skepticism, according to Marks, because “racial” classification based on ABO differences failed to coincide with anthropological classifications of the time. From the 1920s to the 1940s, “racial serology” managed to “reinvent” its hold on anthropology, because the circularity of “racial” classifications based on blood group differences meant they corresponded more closely with anthropologists’ expectations. Marks argues that anthropologists of Boyd’s own time criticized this circularity, and by 1945, this contributed to “racial serology’s” marginal status with respect to physical anthropology. Ultimately, Boyd’s “racial” classification was undermined by the very blood group data on which it was supposed to be based. According to Marks, Frank Livingstone’s 1963 argument for the nonexistence of human races given the clinal distribution of genetic variation among human groups demonstrated that Boyd’s approach was “fundamentally archaic” (1996: 354). Marks characterizes Boyd’s “racial serology” as “intellectually tangential to the main lines of thought in physical anthropology” during the 1960s. These years were a period when physical anthropologists were making increasing use of genetics to study the micro-evolution and demography of populations, and coming to recognize the “major qualitative divisions of the human species… as illusory” (1996: 357).

When considered from perspectives that recognize the interplay of objectivity and judgment and requirements for maintaining the circulation of reference, Marks’ and Silverman’s criticisms of Boyd and other blood group researchers raise some interesting questions. What is to be concluded when serological “racial” classifications fail to coincide with “racial” classifications based on other traits? One possibility is that there are no mind-independent “races,” for otherwise, these objects would be picked out by numerous traits. Another possibility is that “race” concepts and “racial” groups themselves are constructed by the privileging of certain traits over others—different choices, different “race” concepts, different “racial” groups. What about the circularity of Boyd’s approach to “racial” classification that Marks and Silverman point out? If attempts to discharge subjectivities associated with a priori assumptions—for instance, social-political categories of classification—compromise the circulation of reference, might it be the case that a posteriori classifications will inevitably be circular? Given that the blood group researchers were not ignorant of the fact that the populations they were classifying differed quantitatively not qualitatively in gene frequencies, how did they attempt to justify their imposition of “racial” divisions?

The Hirsfelds noted the difference in classifications produced by the blood groups and by “anthropological characteristics”.

We see thus that A and B are present in different proportions in different races. The serological formula for a particular race is in no way affected by the anthropological characteristics. The Indians, who are looked on as anthropologically nearest to Europeans, show the greatest difference from them in the blood properties. The Russians and the Jews, who differ so much from each other in anatomical characteristics, mode of life, occupation, and temperament, have exactly the same proportion of A and B.

(Hirsfeld and Hirsfeld 1919b: 41)

The Hirsfelds did not speculate on reasons for this difference. They were confident, however, that common ancestry explained the geographical distribution of the blood groups. The Hirsfelds’ successors were more pressed to account for differences between their classifications and those of their anthropologist peers. Although Snyder remarked that “in general the blood group data conform remarkably well to the known anthropological facts” (1925: 407), he addressed the “comparative value” of the blood groups and “racial characters” used by anthropologists. Snyder believed that the blood groups were potentially superior for investigating the “problem of the origin and relationships of races” (1925: 407). As particular characters, they would remain intact across successive generations after “race crossing,” providing an accurate record of the past, of even “a little crossing” (1925: 407). In contrast, “blending” characters like those traditionally used by anthropologists would be submerged. But Snyder saw no reason why suitable traits would not be useful alongside the blood groups: “It must not be thought… that because the groups are hidden in the blood, they possess some mysterious power of providing a basis for racial classification” (1929: 126). Snyder did not doubt that “races” exist independently of any such choices; “racial” classifications ought, then, to coalesce.

Haldane was explicit about the failure of classifications based on different traits to coincide: “The plain fact is that the distributions of pigmentation and skull-shape are pretty well independent. And it is at once clear that that of blood groups is independent of either” (1940: 475). This cross-classification of “racial” types provides a potential argument for their nonexistence. Lancelot Hogben’s 1931 Genetic Principles in Medicine and Social Science, for example, expressed doubt that “racial” classification along genetic lines is achievable even in principle given differences in classifications produced when different combinations of characters are selected—for instance, skin color and head form versus hair texture and nasal index. Hogben treated the blood groups as just one anthropological character
among many, arguing that there was no justification for overcoming such discrepancies by taking serological differences as authoritative (Barkan 1992). Haldane's belief that traditional anthropological traits like skin color are often adaptive provided an explanation for the observed discordance of traits in "racial" classification: "if we map the world [using blood groups]," he wrote, "we shall expect to get information of racial origins quite different to that given by such a character as skin colour" (1931/1933: 67). The distribution of skin color would depend on climate as well as origins, and because pigmentation differences may have evolved fairly rapidly in response to selection pressure, their distribution would provide information only about the recent evolutionary past. The blood groups, in contrast, since they are "not adaptive, or very slightly so," would shed light on "remoter origins" with the result that "[t]he contradiction between the two sources of information is thus only apparent" (1940: 477). Given his view that the blood groups provide "information of a more fundamental character on racial structure" (1940: 477), unlike Hogben, Haldane had no qualms about treating serological differences as authoritative.

The argument that the nonexistence of "racial" types is demonstrated by their cross-classification rests on a typological concept of race. This concept treats "races" as classes whose members share certain traits. This means that individuals belonging to the same "race" would be similar in blood type, facial structure, skin color, hair texture, and so on. With respect to blood types, it suggests "pure races" of AA, BB, and OO individuals, at least in the past. However, beliefs in such "races" were short-lived among serologists who understood the proportions of blood group alleles to be characteristic of "racial" types, and groups not individuals to be the units of "racial" classification. The discordance of traits in classification challenges the existence of "racial" types conceived in this way as well. Indigenous peoples of Australia and sub-Saharan Africa would not be combined in the same "racial" type if blood groups were used as a criterion for classification, but they may, if skin color was used. Haldane's preference for a genealogical concept of race legitimized his approach, which viewed the choice of different criteria as picking out "racial" groups at distinct time levels. Criteria with similar temporal significance would be expected to yield "racial" classifications that coalesce. Haldane's approach assumes a typology of well-delineated populations across space and time. When Barkan describes Haldane as a "major critic of racial typology" prior to the Second World War (1992: 162), it bears clarification that "racial typology" in this sense can refer only to the categorization of individual people by similarities in traits believed to reflect shared ancestry. Haldane was not a critic of a "racial" typology of groups.

Marks' characterization of serology, particularly Boyd's work, as marginal to physical anthropology by 1945 deviates from the dominant historical interpretation of the mid-twentieth century influence of the evolutionary synthesis on physical anthropology. According to Barkan, pre-war physical anthropology was "a once important branch in human biology that had lost touch with scientific progress. Only after the War was the discipline invigorated and brought up to date with biology" (1992: 160–1). Similarly, Nancy Stepan views the mid-century transition in physical anthropology in terms of an old science, that of "racial biology," giving way to a new science, that of "genetical anthropology":

"The new genetical anthropology represented not merely a correction of old ideas, but the substitution of one way of looking at the biological world by another. The units of analysis, the methods and procedures, and the goals of the new science were quite different from those of the old, and entailed a fundamental alteration in the perception of the biological significance of human races."

(Stepan 1982: 176)

Nevertheless, Stepan's portrayal of the "old racial biology" is remarkably similar to the picture Marks paints of "racial serology":

In the old racial biology, racial classification was linked directly to the reconstruction of racial history. Since races were thought of as relatively independent biological units, groups of individuals separated by time and space could, nevertheless, be joined together by racial descent if their skulls and other traits were similar.

(Stepan 1982: 180–1)

Marks and Stepan are in agreement that, by the 1960s, physical–biological anthropologists had come to focus their attention on micro-evolutionary processes, studying genetic changes in populations due to mechanisms like mutation, selection, and drift. As Stepan points out, "racial affinity" was no longer the only acceptable explanation for genetic similarities in populations; convergent evolution, once just adaptive noise, became an object of study in its own right.

On this account, Haldane's theoretical interests in origins, migrations, and genealogical relations, at least in H. sapiens, place him squarely among the "old racial biologists." However, he is also a transitional figure, given his contributions to theoretical population genetics, his population-based "racial" typology, and his focus on genetic rather than phenotypic properties of populations. Although Marks characterizes Boyd as relegated to the margins of physical anthropology by the end of the war and remaining "intellecutally tangential to the main lines of thought in physical anthropology" through the 1960s, in many ways he too is an important transitional figure, more so even than Haldane. In fact, tensions in Boyd's work reflect some of the differences that shape disputes among population geneticists and biological anthropologists today.

The tensions involved in Boyd's work are those of a scientist embedded in, not left behind by, the evolutionary synthesis. In Genetics and the Origin of Species, widely regarded as the fundamental text of the evolutionary synthesis, Dobzhansky urged evolutionary geneticists to focus on "the causal rather than the historical problem" (1937: 6). He characterized genetics, like physiology, as a "nomenclothetic" (law-creating) science and recommended the investigation of "the common properties of living things" rather than the study of "the peculiarities of separate species" through phylogenetic reconstruction (1937: 6). Dobzhansky's favorable
review of Boyd's *Genetics and the Races of Man* expressed agreement with Boyd's definition of "races" as genetically distinct "Mendelian populations" kept apart by geography or social forces as well as hope that the cooperation of anthropologists and population geneticists would lead to "important developments in our understanding of human evolution, and particularly of the mechanisms of race formation" (1951: 266). This attention to the "mechanisms of race formation" focuses scientific attention on potential barriers to gene flow and constructs a concept of "race" that is distinct from the genealogical concept of "race" preferred by blood group researchers like the Hirszfelds, Snyder, and Haldane. Applying "the genetic race concept" to *H. sapiens*, Dobzhansky wrote:

The human species is compounded of numerous subordinate Mendelian populations, which form an intricate hierarchy, beginning with clans, tribes, and various economic and cultural isolates, and culminating in "major" races, and finally the species. Now, not only the major but also the minor populations often differ in gene frequencies. They are "races" by definition. (Dobzhansky 1951: 265)

In a departure from Marks' skepticism about the reality of higher-order classifications, Dobzhansky held that barriers to gene flow occur at all levels (between continents as well as neighboring villages) which meant a hierarchy of Mendelian populations that combines the smallest groups, panmictic populations, into more comprehensive units.

Dobzhansky’s "genetic race concept" lends legitimacy to the attention Boyd’s "racial" classification paid to continental differences, and for which he is criticized by Marks and Silverman. Boyd's approach to "racial" classification was largely consistent with the "new systematics" that architects of the evolutionary synthesis like Dobzhansky and Mayr urged biologists to adopt. The aim was to produce "natural" classification systems, whose taxonomic boundaries coincide with actual discontinuities in nature, as a preliminary step in the investigation of dynamic evolutionary processes. Boyd would not have been surprised to find that his "races" corresponded to "the facts of geography"; his a priori assumption was that oceans once constituted geographical barriers to gene flow between populations indigenous to the five continents. Because these barriers have been incomplete and overcome by migration throughout the course of human evolution, isolation has not been "absolute," and there are only quantitative genetic differences between the "races." Hence, although Marks is correct to claim that Boyd imposed "racial" discontinuities on a continuous pattern of blood group frequencies across populations, Marks is mistaken in saying that Boyd did so without awareness. Boyd stated explicitly that "our decision as to what boundary between frequencies is to separate two races remains always a man-made and arbitrary decision" (1950: 273). Marks and Silverman are also correct to claim that Boyd's "racial" classification is circular. Boyd's report that "[t]he method of gene frequencies is completely objective" (1950: 273) ignores the subjectivities associated with judgments of a priori classification already discussed. And yet, the inability to discharge these subjectivities if the circulation of reference is to be maintained casts doubt on whether the circularity involved in the use of a priori categories of classification can be avoided or may be, perhaps, even desirable.

Evidence suggests that Boyd quite consciously sought to adopt the methods and concepts of the evolutionary synthesis despite Marks' claim that he was yesterday's man. Boyd was well acquainted with Dobzhansky's research on the genetics of natural populations and cited him frequently. Boyd's attention to geographical barriers to gene flow and their role in the "racial" differentiation of *H. sapiens* corresponded with Dobzhansky's mapping efforts in *D. pseudoobscura*. His definition of "race" was taken from a 1944 article on the genetics of *Drosophila* populations by Dobzhansky and Carl Epling: "A race is not an individual, and it is not a single genotype, but it is a group of individuals more or less from the same geographical area (a population), usually with a number of identical genes, but in which many types may occur" (Boyd 1953: 497). This definition foregrounds geographical proximity and relegates genealogy to the background, a background that is nevertheless necessary given that genealogy is part of the fundamental process that structures genetic similarity in virtue of geographical (and reproductive) proximity. Boyd recognized that privileging geographical proximity and genetic similarity over genealogy could result in contradictory "racial" classifications. Unlike Dobzhansky, he was not willing to forego genealogy entirely: "We do not mean to assert that the geneticist can classify mankind with no regard to his recent geographical distribution, and cultural factors such as language, since it is obvious that race, as we understand the term, involves common descent" (1953: 496). "Common descent" served as a constraint for Boyd on "generalizations" based on "abstractions" about "race": "The value of the abstractions will be shown when we apply them to new examples. Thus any combination of gene frequencies which we abstract as characteristic of Africans must not reveal little islands of 'Negroes' in northern Europe or pre-Columbian America" (1953: 496). Boyd was open to Dobzhansky's invitation to address "causal problems" but not led, as a result, to renounce his interest in "historical problems." As we saw, still, in 1950, origins, migrations, and genealogical relations remained of interest to Boyd (Figure 6.9).

Marks' characterization of Cavalli-Sforza as heir apparent to the blood group researchers legitimately draws attention to shared theoretical interests in historical questions concerning origins, migrations, and genealogical relations. But just as Marks' charge that "racial serologists" like Boyd believed in the existence of "qualitatively different major groupings of people, or races" lacks nuance, so does his similar portrayal of Cavalli-Sforza:

Whether we refer to races as low-tech impressionistic color-coded subspecies as did Linnaeus, or as hi-tech computerized color-coded "ethnic regions" (Cavalli-Sforza et al. 1994), the act of imposing qualitative differences on them does not help us understand the biological patterns structuring human variation. What the act represents is the Linnaean, essentialist, pre-evolutionary approach to human diversity.

There is no scientific basis—genetic, phenotypic, or eco-geographic—for asserting that a Persian and a Belgian are qualitatively the same, and a
privileging of a given time slice with its particular geographic distribution of populations over another, a concern that coincides with Marks' objection.\textsuperscript{13} Additional ironies unite these two parties to debates over the HGDP and the legacy of the human blood group research. One is that there are no scientific referents \textit{without} the subjectivities associated with judgment, for it is these that permit the material extraction that occurs along the chain of reference. Another is that Marks' and Cavalli-Sforza's shared choice of discretion as the criterion for reality places human populations in just as much trouble as "races."

\textbf{Conclusion: mapping people, mapping flies}

The debate over the historical legacy of the ABO blood group research that has emerged amidst controversies surrounding the Human Genome Diversity Project has a realist tenor. Jonathan Marks emphasizes the failure of abstract categories of serological classification to identify genuine biological objects. This failure, he argues, was a reflection of the social and political biases of blood group researchers who wished to find "races" where there were only populations. L. Luca Cavalli-Sforza believes that the ABO blood group researchers were successful in discovering genuine biological group differences. Today, with improved technologies and the ability to secure data at the level of DNA, he is hopeful that scientists continuing in the tradition of the blood group researchers will be able to reconstruct human evolutionary history by sampling indigenous populations across the globe. Marks and Cavalli-Sforza are in agreement that "objective" data on human genome diversity provide the only basis for a posteriori classification. They disagree about whether the blood group researchers successfully bridged the representationalist gap between abstract words and concrete world.

Bruno Latour's concept of circulating reference promotes a different understanding of scientific objectivity and the problem of representation. Researchers are responsible for constructing chains of reference that connect abstract words to the concrete world via series of numerous small and painstaking steps. Judgments that both Marks and Cavalli-Sforza would regard as a priori and subjective are indispensable to the processes of abstraction and extraction that make each link in the chain possible. These subjectivities of judgment cannot be discharged in the production of a posteriori classifications and tabular and mapping representations of data without the loss of reference. It is, however, possible to ask why certain judgments are made rather than others. At each link of the chain of reference, a range of choices is available to researchers (Figure 6.2). This coheres with the pragmatic approach to explanation we defend in our chapter on mapping Drosophila.

How genes or chromosomes are ordered in space and time, whether or not they become bounded in populations, and the extent to which genetic continuities or discontinuities are privileged depend on pragmatic features arising within specific contexts of investigation. Some philosophers would argue that explanatory content is shaped by context-dependent aims, interests, and values only when humans are objects of knowledge but not otherwise. The suggestion is that the
methodological care scientists generally take to ensure the objectivity of their findings becomes overlooked when issues strike close to home. Hence, we have emphasized that explanations in population genetics are pragmatic in flies no less than in humans (Gannett and Griesemer, Chapter 4). It is nevertheless possible that certain aims, interests, and values are more likely to arise in human than in nonhuman research contexts. A pragmatic approach to explanation allows us to make theoretical generalizations about such aims, interests, and values while rejecting traditional assumptions that view them as “contaminants” of “objective” science. Rather than detecting contaminants, recognition that objectivity emerges in scientific work to build circulating reference reveals that subjectivity, in the form of scientific expert judgment, is integral to objective science.

We have seen that Ludwik and Hanna Hirsfeld, Laurence Snyder, J.B.S. Haldane, and William C. Boyd shared theoretical interests in human evolutionary origins, “racial” ties, and migration histories. Thus, the ABO blood group researchers, like A.H. Sturtevant, as we point out in “Classical Genetics and the Geography of Genes,” were interested more in “historical problems” than in the causal mechanisms of evolution. Their approach, consequently, is not unlike that taken in Dobzhansky and Sturtevant (1938). In this paper, Thedosius Dobzhansky and Sturtevant present phylogenetic and geographical distribution maps of gene arrangements in chromosomes of two races of D. pseudoobscura and combine information provided by these maps to propose a “working hypothesis” concerning the likely evolutionary history of the species. However, comparisons of the use of maps to address “historical problems” in the 1938 Dobzhansky-Sturtevant paper and in the ABO research of the same period reveal some interesting differences in fly and human research contexts. Questions concerning origins and migration histories are addressed at the level of groups in H. sapiens and at the level of chromosomes in D. pseudoobscura. While there may be underlying assumptions about the continuity of D. pseudoobscura populations over time, and evidence of willingness to compare patterns of variability in different chromosomes in order to begin to delineate populations, there is little serious attempt to fix group boundaries based on the evidence available.

Dobzhansky’s theoretical interests in the study of D. pseudoobscura began to shift around 1936, moving away from his collaborative work with Sturtevant on “historical problems” and toward the investigation of causal mechanisms of evolution in natural populations (Gannett and Griesemer, Chapter 4). It is this approach that was the basis for the Genetics of Natural Populations (GNP) series (Lewontin et al. 1981). Dobzhansky evidently believed that geographical variation in the frequencies of chromosomal types in D. pseudoobscura and blood group alleles in H. sapiens represent analogous processes in both species. Dobzhansky’s 1937 Genetics and the Origin of Species included charts of blood group frequencies from Snyder; subsequent texts such as the fourth and fifth editions of Principles of Genetics (1950, 1958; co-authored with Sinnott and Dunn) included geographical distribution maps of ABO blood group genes originally published by Bertil Lundman and A.E. Mourant. When he discussed “race formation,” Dobzhansky presented chromosomal inversion maps for Drosophila in one section and ABO maps for humans in the section immediately following. We have seen that Boyd’s 1950 Genetics and the Races of Man was, in turn, greatly influenced by Dobzhansky. Again, despite similarities in the approaches of Boyd and Dobzhansky, the investigation of causal mechanisms of evolution differed in people and flies. As in the case of historical questions and the construction of phylogenetic maps, geographical patterns of genetic variability were mapped at the level of chromosomes for D. pseudoobscura and at the level of groups in H. sapiens. Whereas the geographical inversion maps and geographical frequency maps of Dobzhansky’s GNP series emphasize variability within populations, co-temporaneous ABO maps ignore this variability by representing either frequencies of single alleles or homogeneous “racial types.” The ABO maps also represent human groups as more-or-less discrete. Only when the focus was on studying genetic drift in D. pseudoobscura were efforts made to delineate population boundaries. Generally, in the GNP papers, flies sampled at a given geographical location are taken to be representative of the population in the area without the need to introduce divisions between neighboring populations.

While Boyd shared Dobzhansky’s interest in evolutionary mechanisms and favored a geographical-genetic concept of race, unlike Dobzhansky, he was not led to relinquish his interest in historical questions concerning origins, past migrations, and genealogical relations. Historical questions remain at the center of human population genetics research. Cavalli-Sforza, for example, has published a number of phylogenies (e.g. Cavalli-Sforza et al. 1994: 78). These phylogenies represent attempts to reconstruct human evolutionary history in terms of branching events that separate human groups. Plans for the HGDP include sampling DNA from individuals belonging to up to 500 indigenous populations in an effort to recover this history (Cavalli-Sforza et al. 1991). This demonstrates another important difference between mapping people and mapping flies. D. pseudoobscura populations are composed of whatever flies reside in an area at the time they are sampled, for example, those flies that congregate at a particular feeding station. The ABO blood group maps, in contrast, are based on the agglutination reactions of only those individuals whom investigators consider to be indigenous, that is, whose ancestors were “in situ” prior to European colonization. We saw in Boyd’s work that this a priori group boundary was justified by researchers’ theoretical interest in origins and not recent migrations. But once any such a priori group boundaries are drawn, only some stories become possible and others are ruled out. Inevitably, those who author narratives of evolutionary history will believe that certain moments in our human past are more significant than others. These choices, and the values that inform them, direct the writing of the history of human evolution, a history that, unlike for flies, is written by, for, and about those for whom there exists an interest in such a history.

The reflected continuities uncovered in our companion chapter on flies can now be linked to the historiographic analysis here. Rather than a demonstration that the deflections mark underlying continuities of mapping practice, in this chapter we have argued for a new form of subjectivity with judgment rather than against it, building on the insights of Peter Galison, Lorraine Daston and Bruno Latour. Judgment in the twentieth-century science we discuss is the exercise of
scientific expertise in the pursuit of scientific work to form chains of "small abstractions" that are integral to the production and maintenance of objectivity. In virtue of the meticulously built and maintained chains through which reference can circulate, objectivity is secured. That is, representations of nature can be traced to their source objects and objectivity can result because the world can be unpacked from the words provided the judgments that guided chain formation in the first place have not been discharged to the metaphysical ether. Thus, the new objectivity, in virtue of the circulation of reference, is the image of subjectivity, not the break of it.

In our investigation of human blood group mapping, we traced the abstractions and judgments necessary to produce a posteriori classifications and cartographic representations. The circularity of these taxonomic productions is displayed, but unlike the critics of genetic anthropology, we argue that the story of circularity should not be made to tell only a tale of disciplinary rhetoric: the problem of "race" is too important, as is the problem of epistemic virtue in twentieth-century science. Subjectivity cannot be discharged from scientific work, nor would that be a desirable result. To attempt it is to seek a return to nineteenth-century (or earlier) standards of epistemic virtue, to mechanical objectivity or even to artistic genius, rather than to work out a conception of science appropriate to our time and circumstances. To embrace circulation is not to submit to the old charges of subjectivity (or relativism), but to recognize a social transformation of virtue.

Notes

1 This was based on evidence that AB individuals do not arise from RO mothers and that RO individuals do not arise from AB mothers (Mazumdar 1996).
2 Reardon explains "coproduction" as follows: "detailed empirical work by scholars interested in coproduction has demonstrated that science and society exist in a mutually constitutive and stabilizing relation to one another..." In addition to calling into question analyses that reduce science to a knowledge-producing activity separate from society, a coproductionist framework also calls into question analyses that reduce science to social relations" (2002: 15-6).
3 We do not propose to argue that each of the periods described by Daston and Galison can be understood as the transformation of an objectivity/judgment pair rather than of a sequential replacements of epistemic virtues. Here, we argue for a refinement of the complex interplay of objectivity and judgment in the twentieth century.
4 See Griesemer (1990) for another case study of abstraction by extraction.
5 Note that the chain branches—one line goes to Paris and one goes to Manaus. Latour gives the impression that reference circulates along chains, but since these typically branch (and intersect) in scientific practice, reference circulates in complex networks.
6 One might argue that this notion of subjective judgment leading to objectivity is also implicit in nineteenth-century mechanical objectivity, but hidden away in the theory of the instrumentans (Hacking 1983) which take the human interpreter out of the path from phenomenon to data representation. Rather than artistic judgment in the overt representation of nature, the engineering judgment of the instrument maker is implicit in mechanical objectivity. If one were to trace the circulation of reference in the nineteenth century, it would pass, significantly, through the instruments. Thus, the same argument applied here to the small judgments of the twentieth-century scientific expert to produce circulating reference and objectivity may apply to the nineteenth-century instrument maker as well.

7 It would be inaccurate to assume that this departure of Snyder's from the Hirsfelds can be explained by the greater tendency for Americans to racialize continental differences and for Europeans to racialize national differences. Snyder makes several references to "race" that are quite localized, for example, his indication that, in Syria, "the races divide along religious lines" (1929: 123).
8 Sampling location is in addition to the national identity of the group, for example, "Germans in Hungary," "Germans (Berlin)," and "Middle Koreans (Seoul)."
9 As we will see, geographical representations of distributions of group properties similarly must make use of a priori classifications. The "base maps" upon which "thematic" data are plotted must "be a record of the location and identity of geographical features" (Robinson 1982: 16). But locations and identities of such features can only be made on the basis of a priori decisions to represent features in particular ways and according to a chosen system of boundaries, whether political (e.g. national, country), geographic (continents), ethnic, economic, linguistic, religious or otherwise. Such representational choices reflect a priori classifications, for example, that national and racial boundaries coincide or are closely related except for perturbing migration "events." Since it appears likely that the representational practices of genetic anthropology follow those of European ethnographic practices emerging in the nineteenth century (see Robinson 1982: ch. 5), the geographic components of its a priori classifications are likely to be ethno-national-political as we describe in the Hirsfelds' work.
10 According to N. Dwight Harris, the name "Macedonia" was used "in its most restricted—and probably its most correct—meaning to designate that region of the Balkans embraced within the three Turkish vilayets of Salonika, Monastir, and Kosovo, and lying between the districts of Adrianople and Albania" (1913: 205).
11 An outsider's impression of this collection of diverse identities in Salonika is available from war correspondent, G. Ward Price:

For the spy, Salonica is Paradise. He thrives and multiplies there like a microbe in jelly. If a spy had the chance of creating an ideal environment to work in he could not improve upon Salonica. Imagine a town where the languages commonly and regularly spoken are old Spanish, much adulterated, Greek, Turkish, Italian, Bulgarian, Serb, Roumanian, and French; where every one has changed his subject at least once during the last five years—from Turkish to Greek—and where before that several thousands of people had all sorts of claims to European nationalities, based on the complicated Turkish system of the Capitulations (under which one brother in the same family would be "French," another "English," another "Italian," perhaps without one of them being able to speak a single sentence in the tongue of the nationality he claimed.

(Price 1918, in Chapter VI, Ourselves and the Greeks: Relations at Salonica)

12 This notion of a deliberate falsehood is also clearly part of cartographic tradition and reflects a form of expert judgment weighing against accurate presentation of mapped data. As Kombos said, writing about thematic maps in 1848, distinct outlines are to be "preferred to an overstated accuracy in the colouring, which gives only a confused representation" (quoted in Robinson 1982: 139).
13 By conceiving races as monophyletic lineages, Robin Andreaesen (1998) successfully responds to these objections to defining races genealogically. On Andreaesen's account, the number of races that exist depends on the specifics of the branching process that has occurred during evolutionary history and is not constant but varies between time levels. At any given time level, since these clades represent a nested hierarchy, there is a variable but determinate number of monophyletic lineages that biologists, depending on their research interests, might single out as races. However, there are other problems with Andreaesen's approach to defining races as clades (see Gannett 2004).
Bibliography


Classical Genetic Research and its Legacy
The mapping cultures of twentieth-century genetics

Edited by
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This book is dedicated to the memory of Lily E. Kay
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