

What Is Molecular Anthropology? What Can It Be?

JONATHAN MARKS

This first part of this essay explores the relationship of molecular studies to the field of anthropology, setting out the history and present status of that relationship and focusing on two issues in contemporary molecular anthropology. In the second part, I set out a program for a molecular anthropology that is both "molecular" and "anthropology," centering on the application of genetic data to primate, including human, populations, and its intersection with the issues and approaches of traditional anthropology.

"Molecular anthropology" was coined by a biochemist, Emile Zuckerkandl,¹ at an anthropological conference in 1962 sponsored by the Wenner-Gren Foundation, called "Classification and Human Evolution." He intended the phrase to designate the study of human evolution by recourse to the differences in the structure of biomolecules. The paradox is that although this sounds like a kind of anthropology, a *molecular* kind of anthropology, it really was merely the technology of biochemistry being applied to classically anthropological questions. And because it was technology that was driving this new field, anybody could do "molecular anthropology," regardless of how much anthropology they really knew.

While that may sound harmless enough, consider the opposite case. What would constitute "anthropological biochemistry" if you didn't need to know any biochemistry to do it?

In the same article, Zuckerkandl set out the reductive agenda of "molecular" anthropology in a particularly egregious form. Zuckerkandl was impressed by the fact only one of the 287 amino acids in hemoglobin could be found to differ between a human and a gorilla. He was so impressed, in fact, that he proclaimed that "from the point of view of hemoglobin structure, it appears that gorilla is just an abnormal human, or man an abnormal gorilla, and the two species form actually one continuous population."

This assertion sufficiently inflamed another symposium participant, paleomammalogist George Gaylord Simpson,² that he rose to defend what we might now call the integrative or organismal perspective in the pages of *Science*. "From any point of view other than that properly specified," he wrote, "that is of course nonsense. What the comparison really seems to indicate is that . . . hemoglobin is a bad choice and has nothing to tell us about affinities, or indeed tells us a lie." In other words, humans and gorillas can be distinguished readily by virtually any form of competent biological comparison. If you can't tell them apart by hemoglobin or more generally by their genes, just look at something else! The idea of encapsu-

lating the relationships of human and gorilla in the relationships of human and gorilla hemoglobin was as ridiculous as encapsulating those relationships in human and gorilla phlegm or eyelashes.

There was considerably more here at stake, however, than just the gorilla and the human. Molecular evolution was a young field, and its practitioners were perceived as technologists, not scholars. They had in their favor the short and selective memory of science and the cultural cachet of heredity.

At nearly the same time, human population geneticists were studying the relationships of the human races serologically. William C. Boyd³ published a definitive genetic classification of the human races in *Science* in 1963, identifying thirteen of them: five in Europe; one each in Africa, Asia, the Indian subcontinent, and America; and four more in Oceania. This, of course, was a year after Frank Livingstone⁴ had epigrammatically concluded that "there are no races, there are only clines." At the time, no one queried the discovery of five times more taxonomic diversity in Europe than in Africa or the equivalence of "Africans" with the Basques.

Cavalli-Sforza and Edwards⁵ statistically analyzed morphometric and genetical data from diverse human populations to find out which two of Europeans, Asians, and Africans were most closely related. The pattern they saw was that physical traits tended to cluster Europeans and Asians, while genetic traits associated Europeans and Africans. They interpreted this to mean that genetics gives the "right" tree, while physical traits give the "wrong" tree⁵—a self-serving conclusion, at best. However, this conclusion stood and was reiterated⁶ until the advent of "mitochondrial Eve" in the

Jonathan Marks teaches at the University of North Carolina at Charlotte, and is the author of *Human Biodiversity* (1995) and *The Chromosomes in Human Evolution* (forthcoming, 2003), both published by Aldine de Gruyter. This essay is adapted from his book, *What It Means To Be 98% Chimpanzee: Apes, People, and Their Genes*, published in April 2002 by the University of California Press. He is currently President of the General Anthropology Division of the American Anthropological Association.
E-mail: jmarks@email.uncc.edu

1980s,⁷ in spite of alternative statistical treatments of the genetic data actually producing the “morphological” tree.⁸

Even so, there was a deeper history for molecular anthropology.⁹ Racial serology dates to World War I, and in the 1920s generated results that were so difficult to assimilate that Earnest Hooton,¹⁰ the leading student of race, was forced to conclude: “We can make little or nothing of [blood-group analysis] from the point of view of racial studies” (p. 490).

The field of systematic serology dates to the discovery by George H. F. Nuttall¹¹ that the blood reactions of animals generally recreate their Linnean relationships. But because tarsiers and prosimians failed to react with human blood, it followed that they could not be primates, a conclusion that mystified Zuckerman¹² in his review of primate biology.

Molecular/serological/genetic/hematological approaches to anthropological questions have thus been around for a very long time, with an uneven track record. The famous genetic clustering of humans with the African apes¹³ and the debunking of *Ramapithecus* as a hominid¹⁴ are noteworthy not as the starting point of the field, but as rare occurrences in the relevant intellectual history when the genetic approach turned out to be “right.”

I argue that molecular anthropology can be a viable field only if it combines the anthropological with the molecular rather than simply applying the molecular to the anthropological. The latter approach is not only intellectually problematic but self-defeating as well, for it fails to differentiate a “molecular geneticist” from a “molecular anthropologist”—except that the geneticist will likely have access to better facilities and better technological training, the very factors that give the molecular anthropologist an identity within anthropology. I argue instead for the conjunction of molecular data and an anthropological approach in molecular anthropology. I consider an “anthropological” approach to be one that is classically holistic as opposed to reductive; approaches questions of nature from the standpoint of historical and social perspectives in addi-

tion to natural; is sensitive to issues of gender, power, and difference instead of adopting an aloof stance; is engaged in ethical and social issues; and constantly interrogates the “given” status of natural facts and looks for ways in which they may rather be the products of human agency.

EXAMPLE 1: HUMANS AND APES

Perhaps the best-known factoid of modern science is the high level of genetic similarity between humans and African apes. This is often given as: “Humans are over 99% genetically identical to chimpanzees,” and has been invoked in arguments as diverse as the innateness of male aggression¹⁵ and the conferral of human rights on the apes.¹⁶ It is also frequently presented as a paradox: genetic identity in the face of physical diversity. But it is far from a self-explanatory datum; rather, it is an artifact of our complacency about the physical differences between human and ape and our unfamiliarity with genetic comparisons.

The first component of the human-ape comparison must involve recognition that when, at the turn of the eighteenth century, the chimpanzee first became known to European scholars it was seen as strikingly similar to humans—so similar, in fact, that it was sometimes regarded as simply a different kind of person. Thus, Lord Monboddo¹⁷ could declare “that the Orang Outang is an animal of the human form, inside as well as outside,” (p. 270) two centuries before Jared Diamond¹⁸ invoked genetic data to proclaim us to be “The Third Chimpanzee.” Rousseau held the apes to be merely variant mute people, while even Linnaeus¹⁹ famously classified the apes as both *Simia satyrus*, the first of the monkeys, and *Homo troglodytes* (or *nocturnus*), the second of the humans.

Obviously apes were correctly perceived as being very similar in form to humans. But such comparisons defied quantitative assessment. The contribution of genetics, after two hundred years of analysis of the ape body, is to be able to put a number on the degree of similarity. Because it compares linear entities (proteins or DNA), genetics can represent the extent of similar-

ity as a scalar entity, the degree of matching.

Not only is molecular genetic analysis readily amenable to quantification, it also subtly removes any context from the comparison. Thus, human DNA and chimpanzee DNA can be compared to one another without a benchmark or reference comparison. The fact is that comparisons of the nuclear DNA of humans, chimps, and gorillas do indeed show them to be more than 98% identical. But what does percentage of similarity or identity mean here? We culturally frame it as a linear scale in which 100% is perfectly identical and 0% is totally different. But the structure of DNA gives this a statistical idiosyncrasy.

DNA sequence is a linear array of four bases, A, G, C, and T. At any specific DNA position of the 3.2 billion comprising the human genome, there are only four things you can be. Two random DNA sequences that have no ancestry in common are consequently obliged by the laws of chance to match at about one site in every four. Thus the zero mark, the random similarity of two unrelated DNA sequences, is not 0% identical, but 25% identical. (You can, of course, generate DNA sequences more different than that, but not randomly.) The most different two DNA sequences can be, then—and what can be more different than entirely unrelated?—is 75% different.

But all multicellular life on earth is related. A human and a chimpanzee share very recent common ancestry; a human and a banana share a remote common ancestry, but a common ancestry nevertheless. Therefore, any particular DNA sequence compared between a human and a banana would have to be more than 25% identical. Here is a benchmark by which to assess the human-chimp genetic similarity. There are rather few ways, other than genetically, in which a human could be shown to be more than a quarter banana. DNA comparisons thus overestimate biological similarity at the low end.

What about the high end, where the human-chimp comparison actually lies? Here some context is valuable. Rather than the decontextualized com-

parison of the DNA of human and the DNA of chimp that yields such a high degree of similarity, let us consider the overall physical similarity of, say, a human, a chimpanzee, and a starfish. Once the echinoderm enters the comparison, the human and the chimpanzee appear far more physically similar. Given that the human and the chimpanzee correspond in virtually every facet of the skeleton, musculature, central nervous system, and internal organs, while the starfish is discordant even for bilateral symmetry, we are obliged to conclude, with the echinoderm as a frame of reference, that the human and chimpanzee are not 99% identical physically, but 100% identical. Similarly, with use of the aforementioned banana in a three-way comparison, the human and banana are more than 25% identical genetically and 0% physically, whereas the human and chimpanzee are 100% identical physically and more than 98% genetically. Thus, a DNA comparison not only overestimates similarity at the low end but underestimates it at the high end.

We might add as well that such linear sequence comparisons also ignore differences in genome structure and organization that may exist even between closely related species, which arguably is the principal discovery of molecular genetics in the last quarter-century. Examples of qualitative differentiation between the genomes of human and chimpanzee are well known.^{20,21}

Thus, the overwhelming genetic similarity of human and chimpanzee is not so much a fact of nature, but rather an artifact of culture, technologically constructed and ideologically situated. The 98% genetic similarity is not wrong, it simply lacks context and meaning, which are far from self-evident. Consequently, the fact that our DNA is over 98% that of a chimp's is not a transcendent statement about our natures, but merely a culturally understood, or misunderstood, datum.

The seeming paradox of their genetic similarity and physical difference also evaporates when some context is added to both comparisons. In other words, the pattern we encounter genetically is actually the same as the pattern we encounter anatomically. Humans are very similar to, yet diag-

nosably different from, the apes. There is no paradox: It was a result of how mundane the apes have become and how exotic their DNA really is. This is a valuable site for a true molecular anthropology—contextualizing, relativizing, and culturally localizing genetic knowledge to help us make sense of heredity—that is related to, but distinct from, a molecular genetics.

EXAMPLE 2: THE HUMAN GENOME DIVERSITY PROJECT

Although the weakness of the Human Genome Project's Platonic conceptual basis was noted as early as

... the overwhelming genetic similarity of human and chimpanzee is not so much a fact of nature, but rather an artifact of culture, technologically constructed and ideologically situated. The 98% genetic similarity is not wrong, it simply lacks context and meaning, which are far from self-evident.

1986,^{22,23} in 1991 a group of population geneticists formally proposed to augment that project with a large-scale study of human genetic variation, a human genome diversity project.²⁴

The collection of genetic materials had been proceeding on a small scale as a part of anthropological research since the 1920s, when Carleton Coon retrieved blood from the Rif in Morocco to compare their racial affinities serologically versus anthropometrically. Coon,²⁵ while not remembered as being among the most "politically correct" anthropologists of his gener-

ation, nevertheless articulated the difficulties of such an endeavor with some sensitivity:

Blood-letting for blood-group analysis falls into the class of blood-letting in general, and evokes the whole ideology of blood-brotherhood, the fear of injury by contagious magic, and that of ritual contamination based on the analogy of menstruation.

Blood is never, as cultural anthropologists have been known to say, just blood. An anthropological perspective might have been a valuable contribution to such a large-scale project, which was bound, because of the publicity it was generating, to raise questions. These questions had been circumvented previously because the collection of blood as an anthropological object was proceeding on a small scale and "flying under the radar," as it were. Among the earliest questions asked of the Human Genome Diversity Project was the impending genetic reification of culturally constituted groups as natural entities,²⁶ which would seem retrogressive.

The Human Genome Diversity Project was also cast within a series of political contexts that were quite different from those of earlier eras. The first context was the Native American Graves Protection and Repatriation Act of 1990. This legislation was designed to acknowledge the sacred status of Indian remains, which belonged not to science but to the tribes from whom they had been "acquired." This significant affirmation of the rights of indigenous Americans came at precisely the same time that the Human Genome Diversity Project was naively planning to collect different bio-anthropological objects of sacred value on a large scale.

A second significant political context arose when widespread rumors within indigenous communities, to the effect that "white people" were plotting to steal the body parts, bodies, or simply the blood of indigenous people, finally began to receive attention. In some cases they were right: A trade in body organs has begun to develop, as anthropologist Nancy Scheper-Hughes²⁷ has discussed. Lo-

cal people abducted a paleoanthropologist from an East African site, fearing he was interested in stealing the blood from their babies. Once again, this is surprisingly close to what the Human Genome Diversity Project was naively proposing.

Third, corporate exploitation of indigenous people's knowledge of their environment was becoming known under the label of "bio-colonialism." Moreover, patent law in biotechnology was strongly favoring scientists, as shown by the unsuccessful cases of John Moore²⁸ and the descendants of Henrietta Lacks,²⁹ neither of whom was permitted to share in the profits from cell lines derived from their bodies. In the case of the blood of indigenous people, the National Institutes of Health applied for patents for cell lines derived ultimately from the blood of a Hagahai (New Guinea), a Solomon Islander, and a Guaymi (Panama). This seemed to recreate the scenario of the rapacious American capitalist, looting not merely the land or artifacts or knowledge of tribal people, but now their very blood.^{30,31} Small wonder that the Human Genome Diversity Project came quickly to be known as "the Vampire Project"³² or that the project's protestations that it had no financial interests were perceived as disingenuous.

Even in an anthropological venue, the Human Genome Diversity Project was presented without any discussion of the ostensibly anthropological concerns.³³ To what extent, for example, was the voluntary informed consent of each participant elicited? Many tribal people would feel very uneasy to learn that their blood, or a product derived from it, was sitting in a laboratory in Palo Alto, California, and being manipulated exclusively for the benefit of American scientists.³⁴ Moreover, any adequate concept of disclosure and consent would necessitate that scientists explain to participants that there are indeed financial stakes, in which the researcher could get wealthy without precedent for the subject sharing in that wealth.

Because culturally designated groups were its focus, the Human Genome Diversity Project developed a concept of "group consent," whereby

permission would also be sought from the polity itself. This, however, raised other concerns, given the hierarchical and fluid nature of human groups. If the Northern Paiutes decide not to participate in a study and the Southern Paiutes decide to participate as Paiute representatives, would the rights of the Northern Paiutes thereby be violated?³⁵

Further, the solicitation of permission from a political entity representing the people raises the problem of possible coercion. If the leaders agree to participate, does an individual still have free rein to refuse or can the leaders now simply act as agents for

I am proposing the development of a molecular anthropology that is generally harmonious with the field of anthropology and complementary to molecular genetics. This field should engage issues of race beyond the geneticist's tired proclamation that it doesn't exist and then paradoxically proceeding to use it.

the scientists and subtly compel compliance? More specifically, does everyone in the decision-making complex understand fully what the scientists want, why they want it, and what they plan to do? For people who do not share scientific comprehensions of blood, cells, DNA, identity, life, illness, and medical genetics, the elicitation of fully and acceptably informed consent would seem to necessitate the development of a crash biology course in local idioms.³⁶

The National Research Council failed to endorse the Human Genome

Diversity Project in a report issued in 1997,³⁷ and its current status is ambiguous. But once again, we see a venue here for a molecular anthropology that is distinct from, and complementary to, a molecular genetics. In this case, it ultimately failed to appear, and could conceivably have saved the Project.

PROSPECTS FOR A MOLECULAR ANTHROPOLOGY

If molecular anthropology is to be indistinguishable from molecular genetics, it faces complimentary difficulties. On one hand, anthropology departments will become increasingly reluctant to house such a field, and will query its relevance if, indeed, it professes little that is recognizable as an anthropological approach or knowledge. On the other hand, the recruitment of "molecular anthropologists" will be inhibited by the greater research opportunities, relevant contacts, and technical facilities offered by biology departments.

I am proposing the development of a molecular anthropology that is generally harmonious with the field of anthropology and complementary to molecular genetics. This field should engage issues of race beyond the geneticist's tired proclamation that it doesn't exist and then paradoxically proceeding to use it. It should be a central and definitive voice in public discourses about genetic issues like human cloning; to date, that has been discussed in the context of emerging ideas about identity and kinship by cultural anthropologists,³⁸ but not by biological, much less molecular, anthropologists. It should engage ethical and social concerns. Most importantly, it should be the area that recognizes and teaches the "cultural" within the "natural."

There is a three-pronged program that is intrinsic to a truly molecular anthropology. The first prong involves the classic agenda, collecting data relevant to anthropological issues using molecular genetic technologies; that is, the study of human or primate evolution through biomolecules. This may subsume phylogenetic questions, biodiversity questions, or socio-eco-

logical questions. As applied to human populations, however, this takes on an additional burden, that of comprising the basis of a hegemonic origin narrative. The geneticist who believes "we're going to tell these people who they really are and where they really came from" is less likely to secure the confidence and future participation of people who knew who they are and where they came from than is an anthropologist who appreciates the value of nonscientific origin narratives without trying to undermine them. This is where a pluralistic, holistic anthropology may be more helpful than a relentlessly reductive and arrogantly colonialist genetics.

The second prong of the molecular anthropology I envision involves the critical and cultural reading of genetics. *The DNA Mystique: The Gene as Cultural Icon* is about as central a work of molecular anthropology as one can imagine, although written by a sociologist and a historian.³⁹ One can profitably approach this area through a critique of "folk heredity," first applied by A. L. Kroeber to attack eugenics.⁴⁰ Modern cultural ideologies about heredity cluster into four categories: taxonomism, the idea that humans can be naturally grouped into a small number of biologically meaningful units; racism, the belief that an individual embodies properties assigned to the taxonomic group; essentialism, the pursuit of imaginary transcendent uniformity in the face of empirical diversity; and hereditarianism, a belief in the overarching importance of genetics as a shaper of human affairs.⁴¹

Third, a molecular anthropology should represent the interface of human genetics with bioethics.⁴² This is also a crucial site of convergence of science and humanities, representing a middle anthropological ground that can differentiate a molecular genetics from a needed molecular anthropology.

It is notable that with all the current interest and public discussion of cultural issues in genetics, molecular anthropologists have been reluctant or unable to focus the famously critical anthropological gaze on this work, and have tended to

leave that work for others. Yet two cultural anthropologists studying the deCode, Inc. genome program in Iceland recently issued a clarion call to the field: "We are convinced that the time is ripe for a molecular anthropology that includes scientific, technological, political, cultural, and ethical dimensions."⁴³

It remains to be seen whether or not that call can be answered.

REFERENCES

- Zuckerland E. 1963. Perspectives in molecular anthropology. In: Washburn SI. Classification and human evolution. Chicago: Aldine de Gruyter. p 243-272.
- Simpson G. 1964. Organisms and molecules in evolution. *Science* 146:1535-1538.
- Boyd WC. 1963. Genetics and the human race. *Science* 140:1057-1065.
- Livingstone FB. 1962. On the non-existence of human races. *Curr Anthropol* 3:279-281.
- Cavalli-Sforza LL, Edwards AWF. 1965. Analysis of human evolution. In: Geerts SJ. Genetics today: Proceedings of the XI international congress of genetics. Oxford: Pergamon. p. 923-933.
- Cavalli-Sforza LL. 1974. The genetics of human populations. *Sci Am* 231:81-89.
- Cavalli-Sforza LL, Piazza A, Menozzi P, Mountain J. 1988. Reconstruction of human evolution: bringing together genetic, archaeological, and linguistic data. *Proc Nat Acad Sci USA* 85:6002-6006.
- Nei M, Roychoudhury AK. 1974. Genic variation within and between the three major races of man, caucasoids, negroids, and mongoloids. *Am J Hum Genet* 26:421-443.
- Marks J. 1996. The legacy of serological studies in American physical anthropology. *History and Philosophy of the Life Sciences* 18:345-362.
- Hooton EA. 1931. Up from the ape. New York: Macmillan.
- Nuttall GHF. 1904. Blood immunity and blood relationship. Cambridge: Cambridge University Press.
- Zuckerman S. 1933. Functional affinities of man, monkeys, and apes. New York: Harcourt, Brace.
- Goodman M. 1963. Serological analysis of the systematics of recent hominoids. *Hum Biol* 35:377-436.
- Sarich VM, Wilson AC. 1967. Immunological time scale for hominid evolution. *Science* 158:1200-1203.
- Wrangham R, Peterson D. 1996. *Demonic males: apes and the origins of human violence*. Boston: Houghton Mifflin.
- Cavaliere P, Singer P, editors. 1993. *The great ape project*. New York: St. Martin's Press.
- Monboddo, Lord (James Burnet). 1778. *The Origin and progress of language; Book II*.
- Diamond J. 1992. *The third chimpanzee*. New York: Harper Collins.
- Broberg G. 1983. *Homo sapiens: Linnaeus' classification of man*. In: Frängsmyr T, editor. *Linnaeus: the man and his work*. Berkeley: University of California Press. p 156-194.
- Marks J. 1993. Hominoid heterochromatin: terminal C-bands as a complex genetic character linking chimps and gorillas. *Am J Phys Anthropol* 90:237-246.
- Ijdo JW, Baldini A, Ward DC, Reeders ST, Wells RA. 1991. Origin of human chromosome 2: an ancestral telomere-telomere fusion. *Proc Nat Acad Sci USA* 88:9051-9055.
- Walsh J, Marks J. 1986. Sequencing the human genome. *Nature* 322:590.
- Lloyd E. 1994. Normality and variation. In: Cranor CF, editor. *Are genes us? the Social Consequences of the new genetics*. New Brunswick, NJ: Rutgers University Press. p 99-112.
- Cavalli-Sforza LL, Wilson AC, Cantor CR, Cook-Deegan RM, King M-C. 1991. Call for a worldwide survey of human genetic diversity: a vanishing opportunity for the human genome project. *Genomics* 11:490-491.
- Coon CS. 1954. *The story of man*. New York: Alfred A. Knopf.
- Marks J. 1995. The human genome diversity project: good for if not good as anthropology? *Anthropol Newslett* 36:72.
- Scheper-Hughes N. 1996. Theft of life. *Anthropol Today* 12:3-11.
- Greely HT. 1998. Legal, ethical, and social issues in human genome research. *Ann Rev Anthropol* 27:473-502.
- Jackson F. 2001. The human genome project and the African American community: race, diversity, and American science. In: Balint RA, Zilinskas PJ, editors. *The human genome project and minority communities: ethical, social, and political dilemmas*. Westport, CT: Praeger. p 35-52.
- Cunningham H. 1997. Colonial encounters in post-colonial contexts. *Critique Anthropol* 18:205-233.
- Mead ATP. 1996. Genealogy, sacredness, and the commodities market. *Cultural Survival Quarterly* 20:46-53.
- Gutin JC. 1994. End of the rainbow. *Discover* (November):70-75.
- Weiss KM, Kidd KK, Kidd JR. 1992. Human genome diversity project. *Evol Anthropol* 1:79-81.
- Liloquela R. 1996. Value of life. *Cultural Survival Quarterly* 20:42-45.
- Juengst ET. 1998. Groups as gatekeepers to genomic research: conceptually confusing, morally hazardous, and practically useless. *Kennedy Institute Ethics J* 8:183-200.
- Harry D, Marks J. 1999. Human population genetics versus the HGDP (Comment on the paper by Resnick). *Politics and the Life Sciences*, September 1999:303-305.
- National Research Council. 1997. *Evaluating human genetic diversity*. Washington, D. C.: National Academy Press.
- Franklin S. 1999. What we know and what we don't about cloning and society. *New Genet Soc* 18:111-120.
- Nelkin D, Lindee MS. 1995. *The DNA mystique: the gene as cultural icon*. New York: Freeman.
- Kroeber AL. 1916. Inheritance by magic. *Am Anthropol* 18:19-40.
- Marks J. 2001. Folk heredity. In: Fish J, editor. *Race and intelligence: separating science from myth*. Mahwah, NJ: Lawrence Erlbaum. p 95-116.
- Foster MW, Sharp MW, Freeman WL, Chino M, Bernsten D, Carter TH. 1999. The role of community review in evaluating the risks of human genetic variation research. *Am J Hum Genet* 64:1719-1727.
- Palsson G, Rabinow P. 1999. Iceland: the case of a national human genome project. *Anthropol Today* 15:14-18.