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L.L. Cavalli-Sforza: A bird in a gilded cage

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Foreword

Luigi Luca Cavalli-Sforza is a complex figure. On the one hand, he has publicly backed those who assert that human races do not exist. On the other hand, by aggregating large volumes of genetic data, he has proven the existence of large continental races, as well as smaller regional and micro ones. By developing the theory of gene-culture co-evolution, he has also shown that humans did not stop evolving genetically when they began to evolve culturally. In fact, the two processes have fed into each other, with humans having to adapt not only to the natural portion of their environment (climate, vegetation, wildlife, etc.) but also to the portion they themselves have created (mode of subsistence, behavioral norms, gender roles, class structure, belief system, etc.).

This has led some to see a double game at work. While bowing to the mainstream taboos, Cavalli-Sforza has quietly amassed evidence that human races not only exist but also differ in ways that are more than skin deep. In time, his weighty tomes will speak louder than his official statements on race. This may indeed be how he sees himself, and it might explain certain contradictions between his public persona and his academic self. Oh, those naïve antiracists, if only they knew how they're being outfoxed!

Time will tell who is outfoxing whom. To date, the results speak for themselves. When in 1994 Cavalli-Sforza published *The History and Geography of Human Genes*, academics and non-academics alike were talking more openly about race, as seen by the publication the same year of *The Bell Curve* and by the willingness of previously silent anthropologists, like Vincent Sarich, to step forward and speak out. That interval of glasnost soon ended, in no small part because of Cavalli-Sforza's apparent conversion, as attested in his book, to the view that human races do not exist in any meaningful sense.

Why did he convert? And did he really? I doubt there was any conversion. His change of heart was too rapid, and it happened while the zeitgeist was moving in the other direction. Perhaps he saw a chance to gain acceptance for his new tome. Or perhaps he received a letter one day, detailing his wartime record, the people he worked with, and the testing on human subjects …

Cavalli-Sforza had to remake his life when the war ended. He never denied the nature of his wartime research (the time it takes for anthrax to kill its host) but tried to create the impression that he was doing pure research with no military implications. Yet this was Berlin, in 1943-1944. There was no money for pure research. Was he motivated by opportunism, the chance to gain experience in his field of study? Or did he feel loyalty to the Axis cause? It is difficult to say, and perhaps it doesn't matter. It is enough to say that he later saw his wartime research as a

stain on his record and tried to minimize it as much as possible. He was thus vulnerable to blackmail, or rather to his chronic fear of blackmail.

We will probably never know the full story. One thing is sure. If Cavalli-Sforza is playing a double game, he has been playing it far too long. Such a strategy is excusable for an academic who is young, untenured, poorly known, and far from retirement, but these excuses hardly apply to a professor emeritus like Cavalli-Sforza. The time is overdue to speak frankly and, if need be, pay the price. Anyway, what else can he do now with his vast reserves of public esteem? Take it with him to the next world?

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This ebook is based on a series of posts that appeared on my blog *Evo and Proud* in the fall of 2010. I wrote them largely because of the gap I noticed between Cavalli-Sforza's public pronouncements and his more academic writings, particularly those on gene-culture co-evolution. I was particularly appalled by his musings on the need to "ban racism." When things are not what they are believed to be, the answer is not to ban those who fail to believe strongly enough.

There are few real secrets in this world, but some information sources are less accessible than others. In this case, I was able to draw on sources that are largely unknown to North Americans: Cavalli-Sforza's wartime publications (published under the name of L.L. Cavalli), his autobiography (issued only in Italian and French), and my personal knowledge of his project on gene-culture co-evolution.

Luigi Luca Cavalli-Sforza, October 2010

(credit: Luca Giarelli, CC-BY-SA 3.0)

Introduction

In an interview with *Human Biology*, Luigi Luca Cavalli-Sforza voiced his concerns over the growing rift between anthropology and biology:

> Unfortunately, at least in the United States, anthropology is in decline: The cultural moiety is separating from the rest and loves to declare that it is not science—what is it, instead? The rest of the U.S. anthropology departments (variable according to places, but mostly a mixture of physical anthropology, primatology, archaeology, linguistics) fortunately seem to resist this trend, but anthropology seems to be losing ground. There is a need for a new anthropology capable of keeping unity, and to develop human science curricula that may promote an original, fresh outlook. (Manni, 2010)

Why this retreat from biology? Ask almost any anthropologist. You will be told that genetic traits vary much more within human populations than between them, yet the pattern reverses with cultural traits: the differences are mainly between populations. So what can biology tell us about culture?

And you will learn that this view is endorsed by leading geneticists, like Cavalli-Sforza. One has only to read the same interview:

> Don't you feel that results pointing to intracontinental genetic differences can reinforce racist theories, as some pharmacogenomic studies recently did?
>
> LLCS: The between-population genetic variation observed with 650,000 SNPs on the 52 populations of the HGDP is 11% (Li et al. 2008) with a very small standard error. It becomes 16% for the X chromosome, as is expected if nearly all the genetic variation is due to drift—that is, the role of natural selection is very limited. The ca. 30-year-old estimate by Lewontin (1972) of this quantity (15%) was based on other markers and populations and was a reason to encourage banning the use of the word race in humans. In any case the new value is even more supportive of dropping the word race. What we really need to ban is racism, and this is not a socially easy-to-do program. Charles Darwin, already, was skeptical about the usefulness of the race concept in humans, having noted that human variation is geographically almost continuous for most traits.

> Man has been studied more carefully than any other animal, and yet there is the greatest possible diversity amongst capable judges whether he should be classed as a single species or race, or as two (Virey), as three (Jacquinot), as four (Kant), five (Blumenbach), six (Buffon), seven (Hunter), eight (Agassiz), eleven (Pickering), fifteen (Bory St. Vincent), sixteen (Desmoulins), twenty-two

> (Morton), sixty (Crawfurd), or as sixty-three, according to Burke. This diversity of judgment does not prove that the races ought not to be ranked as species, but it shows that they graduate into each other, and that it is hardly possible to discover clear distinctive characters between them. (Darwin, 1871: 226)

Darwin was not "skeptical about the usefulness of the race concept in humans." In the above quote, he denied that humans had diverged into different species, but he accepted the reality of human races, as he wrote a few pages previously:

> There is, however, no doubt that the various races, when carefully compared and measured, differ much from each other,—as in the texture of the hair, the relative proportions of all parts of the body, the capacity of the lungs, the form and capacity of the skull, and even in the convolutions of the brain. But it would be an endless task to specify the numerous points of difference.
>
> […] If a naturalist, who had never before seen a Negro, Hottentot, Australian, or Mongolian, were to compare them, he would at once perceive that they differed in a multitude of characters, some of slight and some of considerable importance. On enquiry he would find that they were adapted to live under widely different climates, and that they differed somewhat in bodily constitution and mental disposition. If he were then told that hundreds of similar specimens could be brought from the same countries, he would assuredly declare that they were as good species as many to which he had been in the habit of affixing specific names.

But why bother quoting Darwin when we can quote Cavalli-Sforza?

> The differences that exist between the major racial groups are such that races could be called subspecies if we adopted for man a criterion suggested by Mayr (1963) for systematic zoology. Mayr's criterion is that two or more groups become subspecies when 75 percent or more of all the individuals constituting the groups can be unequivocally classified as belonging to a particular group. As a matter of fact, when human races are defined fairly broadly, we could achieve a much lower error of classification than 25 percent, implying, according to Mayr, the existence of human subspecies. (Cavalli-Sforza & Bodmer, 1977)

Yes, the above text goes back to 1977. But five years had already elapsed since Richard Lewontin's finding that genes vary much more within human populations than between them. Cavalli-Sforza was still using race terminology much later, as in this article from the late 1980s:

> The first split in the phylo-genetic tree separates Africans from non-Africans, and the second separates two major clusters, one corresponding to Caucasoids, East Asians, Arctic populations, and American natives, and the other to Southeast Asians, (mainland and insular), Pacific islanders, and New Guineans and Australians. (Cavalli-Sforza et al., 1988)

His belief in human races would remain intact until the 1990s, when he published *The History and Geography of Human Genes*.

Why the change of heart? Had Lewontin's 1972 paper been bolstered by new data? Not really. In fact, the 1980s and 1990s saw growing evidence that the same genetic overlap that Lewontin

had found between human populations also occurs between many species that are nonetheless distinct in anatomy, physiology, and behavior (Frost, 2011).

As one anthropologist told me: “I don't think our perception of the general patterns of genetic variation changed much from '76 to '94, but the intellectual climate that geneticists operate in sure did.”

Beginnings

Cavalli-Sforza has become the foremost authority on human genetics through a career spanning almost three quarters of a century.

It all began in the early 1940s at the University of Pavia, in northern Italy. While studying medicine, he listened to lectures on the new field of genetics and thought of pursuing his own work on bacteria within this field, but the possibilities seemed meager in his country and even more so in Great Britain and the United States. This was during the Second World War, and the young Italian scholar was a citizen of an Axis power. There remained only Germany, where concerns were mounting over reports of Allied efforts to develop bacteriological weapons despite the international ban on germ warfare.

He consulted one of his professors:

> I went to see him [Emilio Veratti] to ask him for advice about a certain area of bacteriological research that I had worked on with Giovanni Magni, my college and faculty classmate. It had to do with a study on bacterial virulence using a mathematical method. Veratti told me that to his knowledge the only person who could help us was a German professor, Richard Prigge. We later discovered he was right.
>
> […] With Magni, in Como, we studied anthrax, which is still talked about today because it is one of the most fearsome bacteriological weapons
>
> […] it was not so difficult to measure bacterial virulence [in mice] and we continued our research because we really wished to study this mechanism with a view to creating possible vaccines.
>
> At this point, the advice of Veratti, who had suggested that we try to obtain a scholarship to study at Frankfurt am Main with Prof. Prigge, the only European scientist who could understand the worth of our experiments, proved to be useful. We had the luck of getting a scholarship to spend the summer [of 1942] in Germany. (Cavalli-Sforza & Cavalli-Sforza, 2008, pp. 31-34)

The experiments would be written up as three articles in the journal *Zentralblatt für Bakteriologie*, where the young scholar is credited under his original name of L.L. Cavalli (Bonezzi, Cavalli, & Magni, 1943; Cavalli & Magni, 1943a; Cavalli & Magni, 1943b; see also Cavalli & Magni, 1947). The authors showed how long it took for mice to die from progressively higher doses of anthrax and pneumococci. Sulfanilamide was also investigated as a treatment for anthrax infection.

There are scattered references in Cavalli-Sforza's later writings to other wartime research, where the aim was to develop bacterial strains that can better resist radiation and nitrogen mustard (mustard gas):

> Starting in 1941, bacteria had become my major interest and in 1948 I gave a paper at the International Congress of Genetics in Stockholm on cross resistance to radiation and nitrogen mustard in E. coli based on work done earlier in Milan with Niccolo Visconti. (Cavalli-Sforza, 1992)

This account, written forty years after, is consistent with a description from 1950:

> Nitrogen mustard resistance was found to be gradual or abrupt in increase in different experiments, only a moderate degree of resistance being acquired, which made a detailed analysis difficult. In E. coli K-12 nitrogen mustard resistance is not accompanied by higher resistance to radiations, as in the case of E. coli B. (Cavalli & Maccacaro, 1950)

In the same article, he also described experiments to make *E. coli* more resistant to chloromycetin, an antibiotic. It is not clear where, when, or for whom this work was done, although some of it seems to have been after the war. His co-author, G.A. Maccacaro, received a postwar grant from the Rockefeller Foundation, and the K-12 strain was supplied by an American researcher.

The radiation and mustard gas work seems to have been associated with another stay in Germany, this time with the renowned expert in radiation genetics Timofeev-Ressovsky:

> Buzzati asked us, Magni and me, to join him in Berlin, in August 1942, at the Berlin-Buch Kaiser-Wilhelm Institute of Genetics (now the Max-Planck Institute). It was a bit before our research stay in Frankfurt with Prigge.
>
> At the time, the Institute was run by the famous Russian geneticist Nikolay Vladimirovich Timofeev-Ressovsky, a man of an extraordinary personality, intelligent, likeable, and enthusiastic; in sum, very Russian.
>
> […] After my meeting with N.V. Timofeev, I decided that I would devote my career to genetics research. (Cavalli-Sforza & Cavalli-Sforza, 2008, pp. 39)

The above gives the impression that his Berlin work with Timofeev-Ressovsky was confined to a short stay, perhaps a single visit. Yet, when Cavalli-Sforza dedicated his genetics textbook to Timofeev-Ressovsky in 1947, he praised him as "a friend and teacher, with the wishes that he will be able to continue his work" (Cavalli & Buzzati-Traverso, 1947). These words suggest a longer working relationship.

Cavalli-Sforza's autobiography is the best account of his wartime research, but some details contradict those of earlier accounts. For one thing, his initial accounts refer with one exception

to *E. coli* and not to anthrax, although the contradiction may be more apparent than real. He could have been working with *E. coli* as a prelude to riskier work with anthrax. Alternatively, the *E. coli* work may have been postwar.[1]

The contradiction is more fundamental when it comes to research aims. His earlier accounts have him seeking to create more resistant strains of bacteria, and not better vaccines. These two aims are contradictory because vaccines are normally made from weaker strains. Perhaps he also wished to study drug resistance—a legitimate subject of medical enquiry. But why was he also interested in bacterial resistance to radiation and mustard gas?

This sounds like germ warfare research. More precisely, the aim seems to have been to combine anthrax with chemical and radioactive agents, presumably as part of a single warhead. Was this another one of those *Wunderwaffen* that failed to work out? Perhaps Cavalli-Sforza himself remained unsure of the ultimate aim of his research. And does it matter? The Axis powers, like the Allies, never did use bacteriological weapons, for fear that the other side would retaliate in kind. Yes, we had our own germ warfare program.

This chapter of his life would nonetheless matter a lot to Cavalli-Sforza. He feared having his wartime record brought up, and this fear would haunt him later on.

Note

1. *Google Scholar* mentions a publication on *E. coli* by L.L. Cavalli, dated 1945, in an Italian journal.

Anthrax production facility, Fort Detrick, Maryland, in operation from 1943 to 1969
Credit: Fort Detrick Public Affairs Office

Myths and realities of Fascist anthropology

What were Cavalli-Sforza's initial views on race? The subject does not come up in his pre-1970 publications, so we can only presume that his beliefs were like those of his wartime Italian peers, particularly in the field of anthropology.

But just what were those beliefs? A Latin version of Nazi racism? This seems to be the premise of the film *Life is Beautiful* (*La vita è bella*). The hero visits a local school and ridicules "racist Italian scientists" by posing as the perfect Italian:

> Our race is superior. I've just come from Rome, right this minute... to come and tell you in order that you'll know, children... that our race is a superior one. I was... chosen, I was, by racist Italian scientists... in order to demonstrate... how superior our race is. Why did they pick me, children? Must I tell you? Where can you find… someone more handsome than me?

In reality, Italian anthropologists of the Fascist era did not view the Italian people as a race, let alone as one that is superior to all others. Nor did they conceptualize races as sharply defined entities. Renato Biasutti (1878-1965), Raffaello Battaglia (1896-1958), and others promoted the idea that human populations are dynamic, variable, and evolving. Meanwhile, Adriano Buzzati-Traverso (1913-1983) was developing the new field of population genetics in collaboration with American biologists. Italian anthropology was thoroughly mainstream for its time. The outlier was the nascent American school of Boasian anthropology with its blank-slate view of human nature.

Wartime Italian anthropology continued to seem mainstream long after the war. If we take "the most important single work produced during the war period" (Cooper, 1946), *Le razze e i popoli della terra*, first published by Biasutti in 1941, we find that it was republished several times in the 1950s and 1960s. In 1959, it earned a favorable review from *American Anthropologist*, the harshest comments being: "The theoretical positions […] are often uncongenial to those reared in the epigonous Boasian tradition. […] Social structure is inadequately handled, and in contrast, minor racial differences are lavishly presented" (Hewes, 1959, pp. 618, 620). Clearly, wartime Italian anthropologists did endorse the race concept and accept that human populations differ statistically not only in anatomical traits but also in mental and behavioral ones. In this, they resembled most of their British and American counterparts, only a minority of whom were starting to entertain the notion that races do not exist.

Such was the reality of 'race science' in Fascist Italy. But reality is not everything. There is also mythology—the popular narratives that help us make sense of reality. The Second World War became woven into the moral narrative of the postwar era and would energize the quest for social justice on many fronts: the Civil Rights Movement; the struggle against colonialism; the

Anti-Apartheid Movement; and so on. Wartime epithets like ‘racist’ and ‘fascist’ took on a new purpose and would be used much more often after 1945 than before.

In pursuing his postwar career, Cavalli-Sforza soon saw his past as a handicap. There were others like him: Kurt Waldheim, François Mitterand, Pierre Elliot Trudeau ... For such people, Fascism was not the god that failed. It was the god that died. The world had changed and the time had come to bury the past and move on.

After 1947, he stopped citing his wartime publications. He then changed his name from L.L. Cavalli to L.L. Cavalli-Sforza:

> My father, Pio Cavalli, had died (while we were at Cambridge, in 1949), and Francesco Sforza, the second husband of my maternal grandmother, Maria Fumagalli, widow Manacorda, wanted to adopt me, in order to join his name to my family name. (Cavalli-Sforza & Cavalli-Sforza, 2008, p. 107)

The autobiography places the name change in 1950.[1] In that year, he would have been 28, already married, and a family man with children of his own. Such circumstances were not normally a basis for adoption under Italian law or by custom. Even more inexplicably, he was still publishing under his old name as late as 1953, four years after his father’s death. *Google Scholar* lists three publications by L.L. Cavalli in 1950, one in 1951, four in 1952, and two in 1953.[2]

Notes

1. Stone and Lurquin (2005, p. 27) state that he changed his name at the age of 27, hence in 1949 (date of birth = Jan. 25, 1922). This is impossible, since it was not until the summer of 1950 that he returned to Italy after two years of research abroad.

2. Sometimes more than a year will elapse between the submission of a manuscript and its publication. Perhaps this explains the 3-year lag in implementing his name change. On the other hand, minor changes can easily be added before a manuscript goes to press, particularly when the author gets the galley proofs.

Guido Orefice (Roberto Benigni) explaining Italian race science to a class of schoolchildren. *La vita è bella* (1997)

The gathering storm

The mid-1950s saw Cavalli-Sforza turn his attention from bacteria to humans. He collected data from blood samples to study genetic drift in the villages of Italy's Parma valley. In the mid-1960s, he began to use the same kind of data to trace human ancestry from an apparently African founder group to the many populations of today.

What were his views on race back then? On this, he seems to have kept silent. He apparently followed the example of other postwar geneticists who were conscious of the cloud of suspicion that hung over their heads. Cultural anthropologists talked about race, but only to downplay its importance.

The silence was eventually broken in 1969. A psychologist, Arthur Jensen, argued in the *Harvard Educational Review* that African-American children had lower IQs for genetic reasons and that efforts to close the IQ gap, like the Head Start Program, were doomed to failure.

Cavalli-Sforza had meanwhile come to an American university, Stanford, for a trial year in 1968-1969. In the genetics department, Joshua Lederberg had written a letter attacking attempts to link race to IQ. One of its signatories was Walter Bodmer (Stone & Lurquin, 2005, p. 98).

Bodmer came from a half-Jewish family that had fled Nazi Germany.[1] He saw Jensen's paper as a new form of "racism" (a wartime epithet for Nazism) and wanted to nip it in the bud. Unfortunately, he had little credibility as an expert in human genetics, as Cavalli-Sforza later discovered when the two of them began writing *The Genetics of Human Populations*:

> W. Bodmer spent several months in Italy, and a year in the United States for this collaboration. He was much stronger than me in mathematics, but he did not know human genetics and had only worked with the genetics of bacteria and fungi. (Cavalli-Sforza & Cavalli-Sforza, 2008, p. 169)

Bodmer asked Cavalli-Sforza to coauthor a paper on race and IQ for *Scientific American*. It is doubtful that Cavalli-Sforza had much more credibility on the subject, but it would have been difficult to refuse. He had no tenure at Stanford and his only friends there were Lederberg and Bodmer. Lederberg in particular had helped him rebuild his career after the war and was instrumental in getting him a position at Stanford. There was thus an implicit exchange of services. In return for past and future favors, Cavalli-Sforza lent credibility to an article that might otherwise have never been published. It certainly allowed Bodmer to write the following 'expert opinion':

> As geneticists we can state with certainty that there is no a priori reason why genes affecting I.Q., which differ in the gene pools of blacks and whites, should be such

> that on the average whites have significantly more genes increasing I.Q. than blacks do. (Bodmer & Cavalli-Sforza, 1970, p. 28)

According to Bodmer, such a statistical difference would be unexpected for two reasons. First, it could not result from random events, such as genetic drift or founder effects, because intelligence is polygenic. The laws of chance would prevent the many genes involved from having, on balance, more intelligence-boosting variants in one human population than in another. Second, natural selection could not have created the black-white IQ difference because black Americans have lived in the United States for only two hundred years. This is far too short a time for them to have diverged in IQ from white Americans, even if selection for intelligence had differed greatly between the two groups.

The first reason is wrong. Although stature is polygenic, it will differ significantly among random samples taken from a single population. Because some genes have much stronger effects than others, variation at such loci is not drowned out by variation at other loci. The second reason is likewise wrong. It assumes that the black-white IQ difference is limited to the United States. Yet no one had ever made this assumption, except Bodmer.

After being published in 1970, the paper appeared the next year in a textbook by the same two authors, *The Genetics of Human Populations*. There were now several counter-arguments to the argument that IQ varies with racial background:

1. Although IQ is highly heritable, with estimates ranging from 40 to 80%, it does not follow that the black-white difference in IQ is 40-80% genetic. The difference could be entirely environmental. Heritability studies use twins who share a common environment. In contrast, black and white Americans inhabit very different environments.

2. Because of their unusual *in utero* environment, twins may provide inaccurate estimates of heritability.

3. Black Americans reportedly have higher IQs when tested by black Americans. Thus, cultural factors, including the design of the test itself, might account for the black-white difference.

4. Other contributing factors might include maternal malnutrition and/or a deficient home environment.

5. Even if the black-white IQ difference is shown to be mainly genetic, this knowledge has no practical applications in a free and democratic society. In contrast, a putative environmental cause may be used to justify better nutrition, more money for schooling, and stronger measures against discrimination. Even if the expected benefits fail to materialize, the costs of failure will be minor.

Bodmer's natural selection argument was quietly dropped from *The Genetics of Human Populations*.

The two authors nonetheless acknowledged that the black-white IQ difference might be genetic:

> In summary, therefore, we do not exclude the possibility that there could be a genetic component to the mean difference in IQ between black and white Americans, but simply maintain that presently available data are inadequate to resolve this question in either direction. (Cavalli-Sforza & Bodmer, 1971, p. 799)

This position was surprisingly moderate and already at the limit of acceptable opinion. The early 1970s saw the onset of efforts to purge academia of "racist" professors. After Jensen's 1969 article, students and faculty staged protests outside his Berkeley office. He was denied reprints by his publisher and not allowed to answer letters of criticism. Similar harassment was aimed at other academics, such as the psychologist Richard Herrnstein and the sociologist Edward Banfield.

As a concession to this growing antiracist movement, Cavalli-Sforza and Bodmer added the following caveat:

> We are, of course, aware of the dangers of either overt or implicit political control over scientific inquiry. The suppression of Galileo and the success of Lysenko are two notorious examples of the evils of such control. Most scientists, however, do submit to certain controls over research on human beings such as, for example, the right of an individual to be experimented on, and the confidentiality of the information collected by the census bureau. These controls are imposed to protect the individual from possible direct detrimental effects of scientific investigations. The treatment of the Jews in Nazi concentration camps is a testimonial never to be forgotten to the needs for such controls. There can be no doubt that in the present racial climate of the United States, studies on racial differences in IQ, however well intentioned, could easily be misinterpreted as a form of racism and lead to unnecessary accentuation of racial tensions. Since we believe that no good case can, at present, be made for such studies on scientific or practical grounds, it follows naturally that we do not see the point in particularly encouraging the use of government or other funds for their support […] (Cavalli-Sforza & Bodmer, 1977, pp. 801-802)

This reasoning is disingenuous. Existing controls on scientific enquiry were motivated by feasibility concerns. Above all, they were not systematic; an unhappy researcher could seek other funding sources or move to another institution. Here, the two authors were arguing for systematic controls on research aims.

It is also silly to suggest that the Holocaust would have happened differently if consent forms had been provided. Many deportees had in fact signed forms promising that they would be sent to labor camps. Evidently, such documents were a lie. But how do you fight a lie in a society that criminalizes the mere fact of saying what you think? Nazi Germany practiced too much control, not too little.

Cavalli-Sforza saw this reality at first hand while working with Dr. Prigge:

> Professor Prigge was in no way a Nazi, but of course we spoke about the government with much precaution, whereas in Italy the criticisms against fascism were frequent and overt. Among all the people we met in Germany, none had heard about the Shoah or the concentration camps. We learned about their existence, in Italy, only after the war. (Cavalli-Sforza & Cavalli-Sforza, 2008, p. 35)

Of course, bad things can happen even in a free society. But their worst effects can still be curtailed—by protesting, by denouncing the people in charge, or simply by informing the public. During the last war, the American and Canadian governments interned people of Japanese origin on the west coast. This was an injustice but it did not lead to mass murder. Elsewhere, similar internments did.

Ironically, by endorsing controls on research, Cavalli-Sforza may have been acting on fears he had learned in a less free world. Today Jensen, tomorrow me? Perhaps someone had warned him against sitting on the fence. And then there was his wartime past ... Yes, a fearful mind is the devil's playground.

By coauthoring the 1970 *Scientific American* article, Cavalli-Sforza helped legitimize an unofficial system of censorship that would take on a life of its own.

Note

1. Bodmer's father was Jewish and had lived in the same German city where Cavalli-Sforza had done much of his wartime research:

> Walter Bodmer was born in Germany, in the city of Frankfurt am Main which Cavalli had coincidentally visited during World War II. […] Sir Walter's early infancy had been deeply disrupted by events unfolding in Nazi Germany. His father was a Jewish medical doctor (his mother was a Gentile) with aspirations to academia. However, years before the Nazis took power in Germany, during the period known as the Weimar Republic, Bodmer's father had already been told that his hopes of becoming a university professor were futile, given his "racial" background. In 1938, threatened by the Nazi political regime, he left Germany under the pretext of taking a vacation. He went to England, where he was soon followed by his wife and young son, who was then only two and a half years old. (Stone & Lurquin, 2005, p. 79)

Races do not exist!

The early 1970s saw two papers move the goalposts on race: one by Walter Bodmer and L.L. Cavalli-Sforza, the other by a third geneticist, Richard Lewontin.

Bodmer and Cavalli-Sforza (1970) conceded that human races exist while denying they differ statistically in intellectual capacity, at least on the basis of current evidence. Lewontin (1972) took a different tact: human races do not exist. No races, no race differences.

He came to this conclusion after analyzing the way different genes vary among human populations, specifically genes whose alleles produce different blood groups, serum proteins, or red blood cell enzymes. Surprisingly, he could explain only 6.3% of this variation by large continental races (i.e., 'Caucasoids', 'Mongoloids', and 'Negroids') and another 8.3% by sub-racial populations. The rest—over 85% of human genetic variation—existed only among individuals of the same population.

This pattern had been known for some time with respect to blood groups. But researchers had assumed that some kind of balanced polymorphism was inflating within-population variation, perhaps as a way to hinder the spread of contagious diseases.[1] By the early 1970s, however, the same pattern was appearing with other 'structural proteins.' The building blocks of flesh and blood were turning out to be remarkably the same in all humans. As Lewontin concluded:

> It is clear that our perception of relatively large differences between human races and subgroups, as compared to the variation within these groups, is indeed a biased perception and that, based on randomly chosen genetic differences, human races and populations are remarkably similar to teach other, with the largest part by far of human variation being accounted for by the differences between individuals.
>
> Human racial classification is of no social value and is positively destructive of social and human relations. Since such racial classification is now seen to be of virtually no genetic or taxonomic significance either, no justification can be offered for its continuance. (Lewontin, 1972, p. 397)

How did Cavalli-Sforza react? In a recent interview, he spoke of this paper as a landmark in human genetics (Manni, 2010). Yet his reaction was initially one of silence. From 1972 to 1989, *Google Scholar* lists 426 publications for which Cavalli-Sforza was an author or coauthor.[2] None of them cited Lewontin's 1972 paper. He first commented on it in 1993, some twenty years after.[3]

Why the two-decade silence? If Lewontin's paper had been such a landmark, why did Cavalli-Sforza take so long to acknowledge this fact?

It is hard to enter a silent person's mind. A better approach would be to ask whether there were arguments against Lewontin's finding, or rather the way he spun it. Such arguments usually take three forms:

1. A small genetic difference can make a big cultural difference

Even if human populations differ only slightly in certain genetic predispositions, these slight differences can have big effects.

For instance, the historical economist Gregory Clark has argued that the slow but steady demographic expansion of the English middle class from the 12th century onward gradually raised the population mean for predispositions to non-violence, deferment of pleasure, and other future-oriented behavior. Although the embryonic middle class was initially a small minority in medieval England, its descendants grew in number and gradually replaced the lower class through downward mobility. By the 1800s, its lineages accounted for most of the English population (Clark, 2007, pp. 124-129, 182-183; Clark, 2009).

The 1800s also saw the triumph of Victorian morality in England. This triumph was due not to a massive change in the gene pool, but rather to a slow incremental change that had finally reached a critical mass. The English middle class could now impose its behavioral norms on the whole population, thereby abandoning the two-tier morality of other class-stratified societies.

2. Lewontin's finding is true only if you look at one gene at a time

Genes vary much more within than between human populations only if we take one gene at a time. This pattern reverses if we aggregate variation at several gene loci. The more we aggregate, the more this genetic variation exists between populations and not within them.

Cavalli-Sforza knew this when constructing his first phylogenies of human populations: "it is desired that the number of genes considered be as high as possible in order to increase the reliability of the conclusions" (Cavalli-Sforza, 1966). When he and another colleague later aggregated data from 75 gene loci of 144 individuals from 12 human groups in Africa, Asia, Europe, and Oceania, he found very little genetic overlap among the groups. Most individuals clustered with other members of their regional group (Mountain & Cavalli-Sforza, 1997). This point has also been made by Mitton (1977, 1978), Edwards (2003), and Sesardic (2010).

Clearly, two groups are easier to tell apart with several criteria than with one. With enough criteria, any overlap shrinks to zero and all individuals can be unambiguously assigned to either group. Of course, this merely proves that human populations are identifiable. It does not prove that the differences between them are greater than the differences within them.

3. Genetic variation between populations differs qualitatively from genetic variation within a population

When genes vary between populations, they do so usually because the population boundary separates different environments with different selection pressures. Genes that differ across the boundary are genes that have higher selective value. Conversely, genes that differ within a population have lower selective value, since they differ despite similar selection pressures. The two kinds of variation are therefore not comparable.

This leads to another problem. We generally get data on genetic variation from genes that have little or no selective value—the very ones that are most likely to differ within populations! There are two reasons. First, population geneticists tend to investigate genes that respond weakly to natural selection. If a gene is close to selective neutrality, it will change at a predictable rate (i.e., only through random mutations) and thus provide a reliable 'clock' for measuring a population's age and rate of evolutionary change.

Second, population geneticists prefer genes whose protein products are easy to find and measure in body tissues. Such structural proteins are similar in different species or even different genera. Humans and chimps, for instance, look very much alike in the proteins that make up their body tissues. They have diverged from each other largely through changes at a higher level, particularly changes to regulatory genes that control the pace and timing of development.

This point was grasped by Stephen J. Gould (1977, p. 406). He explained how such genes distort our picture of genetic variation:

> The most important event in evolutionary biology during the past decade has been the development of electrophoretic techniques for the routine measurement of genetic variation in natural populations. Yet this imposing edifice of new data and interpretation rests upon the shaky foundation of its concentration on structural genes alone (*faute de mieux*, to be sure; it is notoriously difficult to measure differences in genes that vary only in the timing and amount of their products in ontogeny, while genes that code for stable proteins are easily assessed).

Others have confirmed this point. The same genetic overlap that Lewontin found between human populations also occurs between many species that are nonetheless distinct in anatomy, physiology, and behavior (Frost, 2011). Genes that vary across a population boundary differ qualitatively from those that do not.

What did Cavalli-Sforza think?

He was certainly aware that culture can amplify slight genetic differences. This was, in fact, part of his dual transmission theory—now known as gene-culture co-evolution (Stone & Lurquin, 2005, pp. 104-108).

He had also been aware since the mid-1960s that the genetic overlap among human populations is a function of the number of genes under study. In addition to his 1997 article with Joanna Mountain, this principle has been implicit in most of his work on human populations. Indeed, when questioned about Edwards' criticism of Lewontin (Edwards, 2003; Khan, 2006a), he diplomatically answered that both were right:

> Edwards and Lewontin are both right. Lewontin said that the between populations fraction of variance is very small in humans, and this is true, as it should be on the basis of present knowledge from archeology and genetics alike, that the human species is very young. It has in fact been shown later that it is one of the smallest among mammals. Lewontin probably hoped, for political reasons, that it is TRIVIALLY small, and he has never shown to my knowledge any interest for evolutionary trees, at least of humans, so he did not care about their reconstruction. In essence, Edwards has objected that it is NOT trivially small, because it is enough for reconstructing the tree of human evolution, as we did, and he is obviously right. (Khan, 2006b)

What about the third counter-argument? Was Cavalli-Sforza aware that genetic variation within populations is not comparable to genetic variation between populations? We see some awareness in his 1971 textbook, where he argues that most polymorphic genes have little selective advantage. Only in two cases are they subject to strong selection pressures. One case involves balanced polymorphisms. The other involves transient polymorphisms—genes quickly moving to fixation in those populations where they are advantageous (Cavalli-Sforza & Bodmer, 1971, pp. 732-735). Such genes are more likely to vary between than within populations.

So perhaps he was aware. Or perhaps not. Even less clear is the reason for his long silence on Lewontin's 1972 paper. This paper was in Cavalli-Sforza's field of study and was widely commented on by other human geneticists. He could have explained its flaws, yet he preferred to remain silent during the long period when it was steadily gaining acceptance.

One reason was his tenuous professorship at Stanford. It was this position that had propelled him to academic stardom, and he may have decided against rocking the boat, at least until he got tenure. His pragmatism is recounted by former colleague Anthony Edwards:

> When in the 1960s I started working on the problem of reconstructing the course of human evolution from data on the frequencies of blood-group genes my colleague Luca Cavalli-Sforza and I sometimes unconsciously used the word 'race' interchangeably with 'population' in our publications. In one popular account, I wrote naturally of 'the present races of man'. Quite recently I quoted the passage in an Italian publication, so it needed translating. Sensitive to the modern misgivings over the use of the word 'race', Cavalli-Sforza suggested I change it to 'population'. At first I was reluctant to do so on the grounds that quotations should be accurate

> and not altered to meet contemporary sensibilities. But he pointed out that, as the original author, I was the only person who could possibly object. (Sesardic, 2010)

What did others think?

Lewontin's paper met with either enthusiasm or silence. Two attempts at rebuttal were published in 1977 and 1978 by Jeffrey Mitton, a zoologist at a second-tier university. Another one was made much later, in 2003, by Anthony Edwards, a geneticist who no longer held an academic position. All three papers used the second counter-argument, i.e., within-population diversity exceeds between-population diversity only if one gene at a time is considered. Although many zoologists are aware of the third counter-argument, none has ever written it up for publication.

Why did Lewontin's paper meet with so little opposition? First, there was the wave of attacks on 'racist' professors during the early 1970s, and the chill that subsequently spread through academia. Many felt it best to be prudent. Second, there was the tenure-track system, which compelled untenured professors to ingratiate themselves with key academic members. This system had always existed but was now being manipulated to advance an ideological agenda.

Thus began the soft totalitarianism of the late 20th century, not with a bang but with a whimper—or rather a silent acquiescence.

Notes

1. If your surface proteins differ from those of your neighbors, you are less likely to be infected by contagious pathogens. There is thus selection for variability in surface proteins within each population.

2. In some cases, several references actually refer to a single paper (because of errors or variations in transcription). This overcount would not cause an undercount of references to Lewontin's 1972 paper.

3. He first cited Lewontin's 1972 paper in 1990. His earliest substantive comments came three years later, when he argued that genetic variation occurs mainly within human populations whereas cultural variation occurs mainly between them (Cavalli-Sforza, 1993). This seems to be the only one of his publications that discusses the implications of Lewontin's 1972 paper. All in all, he cited it three times in the 1990s and once in the 2000s.

An ill-fated project

The 1970s brought L.L. Cavalli-Sforza to the height of his career. His meteoric rise was made possible by two textbooks co-authored with Walter Bodmer: *The Genetics of Human Populations* (1971) and *Genetics, Evolution, and Man* (1976), as well as several joint articles in leading journals.

Nonetheless, he ended his collaboration with Bodmer in the late 1970s.[1] Why? No answer appears in his autobiography or in Stone and Lurquin's biography. In fact, the autobiography has only three mentions of Bodmer. Two are single sentences. The one lengthy mention seems to damn him with faint praise.[2]

To understand how this collaboration ended, one must understand how it began … in a triangular relationship that brought together not only Cavalli-Sforza and Walter Bodmer but also Joshua Lederberg, Stanford's leading geneticist. It was the latter who helped him during the postwar years, who invited him to Stanford in 1968, and who got him a permanent position there in 1972. It was also Lederberg and one of his protégés, Bodmer, who showed him the ins and outs of American academia, especially textbook publishing. Cavalli-Sforza naturally felt indebted.

By the late 1970s, he had a long list of publications and felt reasonably secure. Perhaps he began to feel hindered by his academic partnership. Or perhaps Bodmer seemed to be taking more out of it than he was putting in. One thing is clear. Cavalli-Sforza was planning to study how genes have evolved in our species under the influence of history and culture. Such plans could not include Bodmer, who knew little about human genetics and even less about the other two fields.

Cavalli-Sforza wanted to bring all three elements together to show that our species has a unique relationship with its environment. This point was already being made as an argument against genetic determinism. Humans reshape their environment, making it adapt to them much more than the reverse. It is the environment that has done the changing, not the genes.

To Cavalli-Sforza, this argument seemed incomplete. If humans have reshaped their environment, this adaptive landscape must now be dominated by their cultural creations: oral and written language, social organization, means of subsistence, technology, and so forth. Humans adapt primarily to their cultural environment and only secondarily to their natural environment. Thus, genetic change must be driven primarily by cultural change. In short, we have been created by our own creations.

This in turn leads to two conclusions:

1. Human genetic evolution has accelerated with the quickening pace of human cultural evolution.

2. Each human culture has created its own adaptive landscape. Genetic differences between human populations have been primarily responses to cultural differences.

This is what we now call gene-culture co-evolution. The idea can be traced back to Darwin but was formally worked out in the early 1980s by Cavalli-Sforza and Marcus W. Feldman in *Cultural Transmission and Evolution: A Quantitative Approach* (1981), by Charles J. Lumsden and Edward O. Wilson in *Genes, Mind, and Culture. The coevolutionary process* (1981), and by Robert Boyd and Peter Richerson in *Culture and the Evolutionary Process* (1985). The ground zero seems to have been a cultural evolution class that Cavalli-Sforza taught at Stanford to Boyd and Richerson in 1978-1979 (Stone & Lurquin, 2005, p. 108).

Cavalli-Sforza wanted evidence of gene-culture co-evolution. In the mid-1980s, he organized a project with several professors from two Canadian universities: Queen's and Université Laval.[3] The aim was to determine whether natural selection favors a different sort of mental toolkit in hunter-gatherers, as compared to agricultural peoples. This rationale was later described by one of his project associates, John Berry, a psychologist at Queen's:

> Hunters, by this way of thinking, require good visual acuity, keen disembedding skills and a well-developed sense of spatial orientation. To hunt successfully, the hunter must be able to discern the object of the quest (which is often embedded in a complex visual landscape), then disembed the object, and finally return to home base. In contrast, agriculturalists need not develop these particular skills, but rather they need to invest in other areas of development, such as conservation (in both the economic and the Piagetian senses) and close social interactions. (Berry, 2008, p. 3)

In this joint project, Cavalli-Sforza wished to study Inuit artists to see whether their talent came from a genetic predisposition or from socio-cultural learning. This aim is spelled out in an unpublished report he wrote with Berry:

> One of the most remarkable phenomena in the contemporary Canadian Arctic is the presence of highly-acclaimed art forms — carving in stone and ivory, and printing on paper. The question we ask is: how can we account for the wide-spread distribution of such talent in a small dispersed population?
>
> […] Is it possible that artistic talent is transmitted culturally (from parents to offspring, from others in society to the artist, and from peers to artist)? How can we assess these types of transmission?
>
> Is it possible that artistic talent is transmitted genetically (from parents to offspring)? How can we assess such transmission? (Berry & Cavalli-Sforza, 1986, p. 2)

The Inuit were ideal for such a study:

> With most individuals having had a reasonably fair chance and stimulation to become artists, one is in a better condition to study possible genetic factors contributing to artistic talent, if any. Another great advantage of carrying out this study among the Inuit is the frequency with which adoptions (also early ones, at birth) occur in this population. Frequencies of adoptions reported during the meeting varied from 15% to 30%. Adoptions allow one to distinguish cultural from biological inheritance by studying correlations of adopted children with foster relatives on one hand and biological relatives on the other.
>
> The general strategy will be to select artists in specific communities (to be discussed later), and to study artistic talent in particular. Also to be studied are their biological and foster relatives (if any), including parents, brothers, sisters, children, and more remote relatives (when this is feasible and convenient). "Controls", i.e. individuals who lay no claim to artistic talent, in spite of adequately trying, may also have to be selected and studied in a similar fashion.
>
> […] The study of traits other than artistic talent per se, that may be correlated with it (and indeed may be components of it) will also be of interest. Given enough information one can hope to separately estimate two quantities, called respectively cultural and genetic heritability (see examples in analysis of IQ). (Berry & Cavalli-Sforza, 1986, p. 5)

The project fell through. At Laval, we thought there had been a problem with funding. At Queen's, Cavalli-Sforza explained he could no longer continue because of illness.

In their biography, Stone and Lurquin (2005) mention no illness during this period, the only bouts of ill health being an operation for bladder cancer in 1976 and a heart attack in 1991. In any case, ill health would have been a reason for postponing the project, not for canceling it.

This project is not mentioned in any of his publications, be they books, journal articles, conference proceedings, or poster sessions. The paper trail is limited to one unpublished report (Berry & Cavalli-Sforza, 1986). A similar blank appears in his writings on gene-culture co-evolution. Although he has written abundantly on this concept, there is little on one key element: how cultural evolution has shaped genetic evolution. Examples are confined to the usual suspects: lactose tolerance in cattle-raising societies, and malaria resistance in tropical farming peoples (Cavalli-Sforza & Cavalli-Sforza, 2008, p. 264). There is no published statement of his belief that natural selection has favored different mental traits in different cultural environments.[4]

And yet this was what he believed back in the mid-1980s, when he almost became his own man.

Notes

1. *Google Scholar* lists 26 joint publications by Cavalli-Sforza and Bodmer from 1970 to 1976. From 1977 to 2010, there are only 10 joint publications, 6 of which are re-editions or translations of textbooks from the 1970-1976 period. The remaining 4 are multiple-author articles where Cavalli-Sforza and Bodmer appear amid a long list of contributors.

2. Does Cavalli-Sforza make an oblique criticism of Bodmer in his autobiography?

> […] unfortunately, [ambition] often makes you lose sight of the essential virtues, like honesty, moderation, and altruism. It can blind you. Some of my colleagues distinguish themselves more by their ambition than by their genius. I know others—these are the most dangerous or the most ridiculous—whose ambition is much greater than their intelligence. (Cavalli-Sforza & Cavalli-Sforza, 2008, p. 303)

3. This was the only time I met him. He sat in on my thesis committee and looked on good-naturedly. Of the three other professors present, only one seemed to know how important he was. Afterwards, that one professor was dumbfounded by our ignorance: "You think Claude Lévi-Strauss is important? This man is the Lévi-Strauss of human genetics!"

4. Cavalli-Sforza has denied ever having such a belief. When interviewed by Stone and Lurquin, he attributed his interest in culture to the very opposite idea:

> Yet another source of his interest in culture was the idea that the concept of human cultural learning was a valid weapon against racist arguments that differences between people (for example, different IQ scores among ethnic groups) were due to biologically determined "racial" differences. (Stone & Lurquin, 2005, p. 86)

Pins and brooches carved from walrus and narwhal ivory by Inuit residents of Repulse Bay. For a hunter, survival depends on being able to disembed objects from complex visual landscapes. Hunting peoples are thus better at imagining objects and translating this mental representation into a tangible work of art.

Credit: Mike Beauregard, Nunavut, Canada

Success, but at what price?

With his sudden abandonment of gene-culture co-evolution, Cavalli-Sforza returned to population genetics. Actually, he had never left it. He had always been looking for new data to plug into his map of global genetic variation.

For any one gene, the map typically showed much more variation within than between human populations, as Lewontin had found in 1972. The pattern changed, however, when several gene maps were superimposed on top of each other. With data coming in from more and more genes, there emerged a distinct pattern of continental populations—the same ones that Cavalli-Sforza had once called "races." This composite map would eventually appear on the cover of his tome *The History and Geography of Human Genes* (1994).

In 1991, he announced a new phase of his research: the Human Genome Diversity Project (HGDP), a program to collect and analyze DNA from all human populations. It would be his crowning achievement. Among other things, it would "produce a mine of data to comprehensively explore human prehistory, determine the genetic relationships between the earth's populations, and provide valuable information on human genetic diseases" (Stone & Lurquin, 2005, p. 160).

Cavalli-Sforza did not expect controversy. After all, he had been collecting this kind of data for the past quarter-century. He had also done all the right things, including not trying to link genetics to mental traits. There was of course his Inuit study, but it had been aborted at the last minute. Finally, the project had support from a broad range of scientists, including his old colleague Walter Bodmer. How could it go wrong?

It did go wrong. In fact, he stuck his hand into a hornet's nest. For the first time in his life, people were denouncing him as a racist and the HGDP as a "vampire project." He was flabbergasted by what seemed to be a big misunderstanding.

Yet there was no misunderstanding. The new project ran counter to the very beliefs that Cavalli-Sforza had now made his own. This point was made by two opponents, Joseph Alper and Jon Beckwith, who denied that the HGDP would ever contribute anything worthwhile:

> First, […] human beings share more than 99% of their DNA. Second, most of the total genetic variation in the human population occurs within any single group. Intragroup genetic variance is much larger than intergroup variance. Sampling U.S. residents alone with their enormous range of ethnic and racial backgrounds would probably encompass the vast bulk of human variation. It would be hard to justify the cost of the HGDP if its goal were merely to obtain the remaining small amount

> of genetic variance not accounted for by sampling only U.S. and/or European residents. (Alper & Beckwith, 1999)

This is the same argument that Richard Lewontin had made in 1972.[1] With so little opposition from other geneticists, including Cavalli-Sforza, it had steadily won over more and more people, eventually becoming conventional wisdom by the 1990s.

There was nothing racist in the HGDP by itself. It did, however, draw attention to the existence of human populations and could therefore frame questions about the ways people differ from each other:

> […] we argue that because the aim of the HGDP is to define genetic differences and similarities among peoples, the potential for racism is inherent in the study design of the project. (Alper & Beckwith, 1999)

> What most concerns us is not competition for research funds, although this is an issue in a lightly funded discipline. Rather, what most concerns us is the construct of human variation that the project might embody and reify, and the type of training and socialization that the project will provide for young physical anthropologists. (Goodman & Armelagos, 1996, p. 182)

In the face of these attacks, he took the path of least resistance. He began to cite Lewontin's 1972 paper after having ignored it for two decades. He also began to argue that the HGDP provided further proof for Lewontin's conclusions:

> Last year the Human Genome Diversity Project used 1990s genetics to extend Lewontin's analysis. Its conclusion: genetic variation from one individual to another of the same "race" swamps the average differences between racial groupings. The more we learn about humankind's genetic differences, says geneticist Luca Cavalli-Sforza of Stanford University, who chairs the committee that directs the biodiversity project, the more we see that they have almost nothing to do with what we call race. (Begley, 1995, p. 67)

When his opus *The History and Geography of Human Genes* came out in 1994, he inserted a chapter titled "Scientific failure of the concept of human races." He made a point of referring to Lewontin's paper — "All populations or population clusters overlap when single genes are considered" — even though geneticists were already finding the same overlap between many species. He also added: "None of the genes that we consider has any accepted connection with behavioral traits, the genetic determination of which is extremely difficult to study and presently based on soft evidence." Scarcely eight years before, he not only believed in such genetic determination but was actively trying to prove it.

All of these frenzied efforts were beside the point. If Lewontin was right, the HGDP was wrong—or at least not needed, as two of its opponents noted:

> Perhaps, in the right hands, the data will prove once and for all that races are abstractions, that, as Marie-Claire King (1993) says, we will find so much within group variation that the project will be a key to a non-racial science. But, we have known for at least twenty years that within group variation is so much greater than between group variation (Lewontin, 1972). We do not need a very expensive data collection exercise to show this. The data are already at hand. (Goodman & Armelagos, 1996, pp. 181-182)

Cavalli-Sforza failed to see the substantive nature of the attacks on the HGDP. There was no horrible misunderstanding, but rather a sincere belief that very little genetic variation would ever be found. The project was therefore at best unnecessary and at worst mischievous.

He could have argued against that belief. Instead, he embraced it. Perhaps he thought he could outfox his opponents by appearing to give them everything they wanted while keeping the essential. Whatever his strategy was, it failed. Funding dried up and many initially interested researchers began to shy away. Contrary to what Cavalli-Sforza states in his autobiography, the HGDP remains uncompleted to this day, at least in its originally intended form.

Note

1. While not specifically criticizing Cavalli-Sforza's project, Richard Lewontin did attack HUGO (the Human Genome Project). He was in fact one of the first, if not the first, to denounce human genome studies:

> […] simple internal forces, the genes, are now held responsible not only for human health in its normal medical sense but for a variety of social problems, among them alcoholism, criminality, drug addiction, and mental disorders. […] The current manifestation of that belief in the importance of our inheritance in determining health and disease is the human genome sequencing project, a multibillion-dollar program of American and European biologists that is meant to take the place of space programs as the current great consumer of public money in the interest of conquering nature. (Lewontin, 1991, p. 46)

It is unclear what role Lewontin played in mobilizing subsequent opposition to the HGDP. The usual view is that this opposition was spontaneous and broad-based, being largely made up of critics from post-colonial, non-Western societies. This view ignores the role of a vanguard of Western academics, like Lewontin, who had earlier identified such research as a target for attack.

Cover map of *The History and Geography of Human Genes* (1994)
(Credit: Ephert)

When maps of genetic variation are successively plotted for different human genes, and then superimposed one on top of the other, we see a composite map of large continental populations.

A bird in a gilded cage

Cavalli-Sforza's last big undertaking was *The History and Geography of Human Genes*, which came out in 1994. Since then, he has kept himself busy tying up loose ends.

Advancing age is only one reason for his reduced activity. Originally, he had planned to work on two big projects until the end of his life. One dealt with gene-culture co-evolution. It would have involved studying the Inuit to see how their hunting lifestyle had selected for certain mental traits, specifically the ability to disembed an object from a larger visual landscape, then store it as a spatiotemporal model in the mind, and then convert it back into a real-world object, such as a soapstone carving. Adopted and non-adopted Inuit would have been studied to find out how much of this ability was innate and how much learned. The project would have then served as a springboard for comparative studies of other hunter-gatherers and, later, farming peoples.

That project suddenly aborted, for nebulous reasons. Its place was taken by the Human Genome Diversity Project. This would have been a continuation of work that Cavalli-Sforza had been pursuing off and on since the mid-1960s, the main aim being to reconstruct how ancestral humans split up as they spread out of Africa to other continents. That project too came to a sudden end—in the face of violent accusations of racism. Funding dried up and researchers shied away. Today, research is still ongoing unofficially, the unspoken premise being that an unofficial project is less likely to catch flak than an official one. And the less Cavalli-Sforza is involved, the better.

So what should he do in his twilight years? One possibility would be a second edition of *The History and Geography of Human Genes*. This tome draws on data collected up to 1986 and is now more than a quarter-century out of date (Cavalli-Sforza & Cavalli-Sforza, 2008, p. 281). An update is sorely needed and Razib Khan (2010) has shown how some of the gene charts could be redone. Such reediting would not be difficult, since the work could be delegated to other people with Cavalli-Sforza keeping editorial control. His opus would thus gain a new lease on life and remain in university classrooms for another quarter-century.

Yet he seems content, or perhaps obliged, to rest on his laurels. He will certainly not suffer for lack of recognition. When his final departure comes, it will be met with eulogies that befit a great man of science, and probably a state funeral in his home country.

And then his works will fade into obscurity. Ironically, his final tome will gather dust while his earlier works continue to be consulted. But those, too, will become footnotes to history. In twenty years, he will be remembered as we now remember anthropologists like William Sumner or Lewis Morgan.

But who knows? These are the shadows of what might be, not what must be. Cavalli-Sforza may still surprise us. Let me give him the last word:

> In all the cases where we feel powerless before the unknown, we should simply keep our eyes wide open and face the situation, if possible, with a certain fatalism, as befits the old saying, "whoever will live will see." (Cavalli-Sforza & Cavalli-Sforza, 2008, pp. 326-327)

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