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**The Evolution of  
Theodosius Dobzhansky**

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*Essays on His Life and Thought  
in Russia and America*

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## Dobzhansky in the "Nature-Nurture" Debate

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Diane B. Paul

In "How Much Can We Boost IQ and Scholastic Achievement?" the psychologist Arthur Jensen asserted that genetic differences probably explain at least half of the black-white gap in IQ test scores (Jensen 1969). His article produced a storm of controversy. Initially Jensen was criticized for exaggerating the significance of heritability estimates and for using statistics on the heritability of IQ within races to draw conclusions about the genetics of IQ differences between them. Even his severest critics took for granted, however, that IQ differences within populations were to some degree heritable (Kagan 1969; Hirsch 1970; Lewontin 1970; Bodmer and Cavalli-Sforza 1970).

Within a few years the character of the critique had markedly shifted. Jensen was now faulted, not just for extrapolating from within- to between-group heritability, or presuming that a high heritability implied that environmental measures could do little to change IQ, but for his assumption that genes affect individual differences in intellectual performance. Other behavior geneticists, some quite critical of Jensen, were also indicted for the same offense.

One of the participants in the genetics of IQ debate was Theodosius Dobzhansky. In the 1950s and 1960s, when he published several popular books and articles on the nature-nurture controversy, Dobzhansky had been widely considered an "environmentalist." In the early 1970s, however, he expressed considerable sympathy for Jensen (whose 1972 book, *Educability and Group Differences*, Dobzhansky reviewed favorably in manuscript) and for the besieged community of behavior geneticists in general. He was also angry at some of Jensen's critics. From his letters, we know that Dobzhansky was particularly distressed by these geneticists' refusal to acknowledge that genes made any contribution to individual differences in human cognitive abilities and aptitudes. Indeed, he felt personally offended by their critiques of human behavior genetics.

As well he might. It was a discipline Dobzhansky had done much to foster. He often said that the ultimate aim of any genetics, includ-

ing his own work with *Drosophila*, was a better understanding of people, especially their behavior. Indeed, he considered the genetics of behavior the next frontier in biology; perhaps "as important and exciting in the near future as molecular genetics has been for our present generation" (1972, p. 523).

In the 1950s and 1960s, Dobzhansky encouraged many young geneticists to enter the field. At the Institute for the Study of Human Variation at Columbia University he provided training and support for behavior genetics research. He served as president of the Behavior Genetics Association and as a member of its Executive Committee. (The association recognizes special achievement in the field with a Dobzhansky Memorial Award). He also convinced some influential social scientists, most notably Gardner Lindzey, then president of the Social Science Research Council, to pay greater heed to genetic determinants of behavior. Dobzhansky himself served on the Research Council's Committee on Genetics and Behavior. There was no basis for human behavior genetics, however, if there was no selective variance for human mental and personality traits.

Dobzhansky believed that human variation in virtually every trait was genetically influenced. "It is a fair statement that whenever a character variable in human populations has been at all adequately studied, genetic as well as environmental components in its variability have been brought to light," he wrote in a typical passage. "This applies to characteristics of all sorts—physical, physiological, and psychological—from skin color, stature, and weight, to intelligence, special abilities, and even to smoking habits" (1973a, p. 283). Dobzhansky made many similar statements—then wholly uncontroversial—in the 1940s, 1950s, and 1960s. However, in the wake of the Jensen controversy, the "interactionist" position Dobzhansky represented came under attack. Within a remarkably brief period, and with his own views having shifted little if at all, Dobzhansky had apparently moved from one side of the nature-nurture debate to the other.

What had changed, with amazing rapidity, was the social context of behavior genetic research. In the politically charged 1970s, the nature-nurture controversy had been transformed, and with it the meanings of key concepts and terms. Views that in the 1960s were considered "environmentalist" now marked one as "hereditarian." And "hereditarian" in the 1970s implied politically reactionary. Dobzhansky (like many of his colleagues) was bewildered by this reversal. He did not consider himself a hereditarian. An active partici-

pant in many left-liberal causes, above all antiracialism, he certainly did not consider himself a reactionary.

In this essay I explore Dobzhansky's own (sometimes shifting and contradictory) views on the nature-nurture issue and the allied question of eugenics. To what extent did he think individual and group differences in mentality and behavior were attributable to differences in genes? Did he believe the answer mattered for questions of social policy? If so, in what ways? Above all, how did circumstances change, such that a vigorous and consistent critic of genetic determinism came, at least partially, to sympathize with some of those tagged as hereditarians?

Following World War II Dobzhansky wrote frequently about genetics and society in general and the nature-nurture question in particular. Most of these works, such as *Heredity, Race, and Society* (Dunn and Dobzhansky 1946), *Mankind Evolving* (1962), *Heredity and the Nature of Man* (1964), and *Genetic Diversity and Human Equality* (1973b), addressed a popular audience. In these books and in a host of articles, Dobzhansky portrayed himself as a moderate, rejecting the "extremes" of environmentalism (defined as the view that genes are irrelevant to mentality and behavior) and hereditarianism (defined as the view that only genes matter). The following passage is typical: "It would please many simplifiers to have the diversity of human abilities and behaviors either all due to training or all predetermined genetically. The partisans of these two oversimplifications engage in interminable polemics . . ." (1972, p. 529).

In fact, these positions have had few exponents in America, at least among geneticists. Even extreme eugenicists have generally accorded some role to the environment. And, excepting a brief period in the 1970s, few environmentalists have insisted that genes contribute nothing to mental and personality differences. For most of the history of the nature-nurture debate, the real differences have been ones of degree and of perceived social implications.

In general, environmentalists have accorded greater weight to nurture in explaining human differences, have challenged the equation of "genetic" with "fixed," and have denied that genetic knowledge justifies prevailing social arrangements. Hereditarians, on the contrary, have given greater weight to genes, which are seen as determining both individual fates and social institutions.

In his 1949 book *The Nature-Nurture Controversy*, Nicholas Pastore described the positions as they existed about the time the subject first captured Dobzhansky's attention—descriptions that remained

applicable throughout the 1950s and 1960s. According to Pastore, a hereditarian "accepts statements of the following type: heredity is more important than environment; individual and group differences are the result of innate factors (either in totality or predominantly); innate characteristics are not easily modified. Where a choice of interpretation is possible, the explanation in genetic terms is the one advanced and favored. To the hereditarian way of thinking, the problem of differential fecundity looms as a most significant one for society" (p. 14). An environmentalist, by contrast, "accepts statements of the following type: environment is more important than heredity; existing individual and group differences reflect (much more than is commonly thought) differences in opportunity; innate characteristics are easily modified. Furthermore, the 'plasticity' of the child is emphasized. Of possible alternative explanations, he chooses the one emphasizing environment. In addition, the environmentalist minimizes the importance of natural inequities in the attainment of success and rejects the eugenic program (as usually conceived)" (p. 14).

If Dobzhansky rejected the label, he nonetheless held generally "environmentalist" views. He insisted that we are not blank pages on which the environment writes, but he also insisted on the significance of social and cultural differences. While believing that populations, as well as individuals, vary genetically in respect to many traits, he valued these genetic differences and wished to preserve rather than reduce them. ("Differences are not deficits" was one of his favorite maxims.) He was thus generally critical of eugenics. He argued strenuously against the use of genetics to justify existing class structures or racial prejudice. Above all, he did more than anyone to undermine the assumption that heritability can be equated with insensitivity to environmental change.

Dobzhansky took every opportunity to note that the same genotype may be expressed differently in different environments. Human environments are both diverse and everchanging. "Invention of a new drug, a new diet, a new type of housing, a new educational system, a new political regime introduces new environments" (1955, p. 75). Thus we can have at best only incomplete knowledge of the "norm of reaction" of any human genotype. If we cannot identify the range of environments over which genotypic expression varies, we cannot predict how much the intellectual performance (for example) of an individual or population might rise in another environment.

Estimates of heritability (the proportion of phenotypic variance attributable to genetic variance) thus provide local rather than global information; they apply only to a specific population in a specific range of environments. "Heredity is often spoken of as 'destiny,'" he wrote. "It is destiny largely in proportion to our biological ignorance" (1950, p. 162).

Yet Dobzhansky sometimes wrote in quite a different vein. He favored a meritocracy, in which the occupational differentiation of the population would comport with its genetic polymorphism. "Civilization fosters a multitude of employments and functions to be filled and served—statesmen and butchers, engineers and policemen, scientists and refuse collectors, musicians and sales clerks," he wrote in *Mankind Evolving* (1962, p. 243). The ability and desire to do these jobs, in his view, had a strong genetic component. "A society benefits from the fullest development of genetically conditioned and socially useful talents and abilities of its members" (1973a, p. 283); it should capitalize on genetic diversity in the service of social efficiency.

In his comments on the natural division of labor, Dobzhansky tended to write as if environments were static and heredity were indeed destiny. However, when discussing the significance of the norm of reaction concept, he emphasized the difficulty of knowing who was best at what, given that the same genotype may be expressed differently in different environments. Thus he wrote, "in different environments and under different social systems the present failures might be successes and the successes failures" (1962, p. 314). As Michael Ruse notes in his essay in this volume, Dobzhansky's social attitudes were complex and sometimes contradictory. In this case he appears torn between commitments to the values of social efficiency and social experimentation.

Dobzhansky's surge of writing on these issues seems to have been prompted by his association with Frederick Osborn, the director of the American Eugenics Society. Following World War II, Osborn embarked on an effort to repair the society's image, which had been badly tarnished by its association with crude class and race prejudice. Osborn aimed to place eugenics on a firmer scientific foundation and, to this end, tried to attract distinguished scientists to the organization. Among those he courted most assiduously was Dobzhansky, who finally joined the society's Board of Directors in 1964. By then Dobzhansky and Osborn had become quite close. During the 1950s and 1960s, Osborn's views became increasingly

"reformist"; i.e., he came more and more to emphasize the importance of environment in explaining both individual and group differences and to move the society away from propaganda for action toward support for scientific research. Osborn's move in this direction seems to have been principally a result of his association with Dobzhansky.

At the same time, Dobzhansky became embroiled in a dispute with H. J. Muller over the nature and consequences of genetic diversity—a debate that came to be known as the "classical-balance" controversy. Their quarrel began with Muller's 1949 presidential address to the American Society of Human Genetics, published the following year as "Our Load of Mutations." The essay was extremely influential. *Genetic load* and various associated terms came to dominate the discourse of population genetics. This is not the place for a detailed explanation of these concepts, which have been discussed by John Beatty in this volume and elsewhere. Suffice it to say that *genetic load* is not a neutral expression; it implies that variation is a burden that we carry, i.e., that it is bad.

This attitude toward genetic variability followed logically from Muller's view that organisms are generally extremely well adapted to their environments. Thus nearly all mutations are bad and will be removed by selection. Of course favorable mutants sometimes appear, and these provide the raw material for evolution. But they are extremely rare and rapidly become the new normal or "wild type." Most genetic variation is therefore transitory. More accurately, it would be transient in nature. However, humans have both increased the rate of mutation (through increased exposure to mutagens, especially ionizing radiation) and decreased that of selection (primarily through improvements in medicine and public health). As a result, the species is genetically deteriorating. Given these assumptions, the social policy implications seemed clear: there was an urgent need for a eugenics program.

Dobzhansky was perhaps the severest and certainly the most influential critic of Muller's eugenics and of its underlying assumptions. In Dobzhansky's view also, some variation was unreservedly bad. But he stressed the heterogeneous and changing character of environments and hence the need for a store of genetic variability. Given this need, selection would generally act to preserve variation. As he wrote in a letter dated 11 July 1953 to Julian Huxley: "It does look that balanced polymorphism is of greater importance in adaptive

evolution . . . than we have imagined . . . This may mean that what we regarded as lethals and hereditary diseases are in reality the raw materials from which the species constructs the co-adapted gene combinations. It will be very useful to consider from this standpoint some of the old problems of human genetics—and eugenics, of course" (Huxley correspondence, Rice University). In other words, disability and disease may be the price a species pays for evolutionary adaptability.

By the late 1950s Dobzhansky had come to focus on a single form of balancing selection: heterozygote advantage, or "overdominance." If heterozygotes are generally fitter than homozygotes, then genetic variability is good for individuals as well as species. Dobzhansky's student Bruce Wallace had irradiated fruit flies and found that the treated group, with their induced heterozygosity, had a greater viability than the controls. These experiments were sometimes cited as evidence for the virtue of heterosis or heterozygosity per se. Heterosis explained why some harmful genes were maintained at high frequency in the population; for example, the allele that in double dose produces a serious disease, sickle-cell anemia, produces, when paired with a normal allele, only mild effects and a more than offsetting protection against malaria. One could not, and would not want, to select against genes of this type. If overdominance were common, eugenics would thus be futile. (Muller conceded the sickle-cell example but denied its generality.)

In Dobzhansky's view, Muller seriously underestimated the value of diversity, both genetic and social. Muller's eugenics was misguided (according to Dobzhansky) because it aimed at an evolutionarily disastrous uniform type. It was really Muller's positive eugenics, rather than his negative eugenics, that roused Dobzhansky's ire. His attitude toward the latter was actually quite complex and perhaps contradictory.

Dobzhansky often wrote as if genetic defects were the necessary price the species pays for evolutionary flexibility. However, he also sometimes wrote that carriers of serious diseases should be convinced—and failing that compelled—not to reproduce. Thus, in *Mankind Evolving* he suggested that: "Persons known to carry serious hereditary defects ought to be educated to realize the significance of this fact, if they are likely to be persuaded to refrain from reproducing their kind. Or, if they are not mentally competent to reach a decision, their segregation or sterilization is justified. We

need not accept a Brave New World to introduce this much of eugenics" (1962, p. 333).

Dobzhansky was interviewed for Columbia University's Oral History Project a few months after the book appeared. Asked to elaborate on this passage from *Mankind Evolving*, he discussed the case of retinoblastic children. If you save their lives, he said, in each generation you would create more lives of the same kind to be saved. Thus, those saved ought to be prevented from reproducing (1962-1963, p. 443). In cases of serious disease, Dobzhansky thought that all reasonable people would agree with this prescription. But consensus would not be reached in respect to more common ailments, like diabetes. And as regards which traits actively to foster, consensus would completely break down. Thus his ire was aroused much more by Muller's schemes to improve humankind mentally and morally than by his negative eugenics, which aimed at reducing disease.

In his 1935 tract *Out of the Night*, Muller had proposed mass artificial insemination of women with the sperm of men superior in intellect and character. "In the course of a paltry century or two," he predicted, "it would be possible for the majority of the population to become of the innate quality of such men as Lenin, Newton, Leonardo, Pasteur, Beethoven, Omar Khayyam, Pushkin, Sun Yat Sen, Marx . . . or even to possess their varied faculties combined" (p. 113). Dobzhansky was horrified by this proposal. In *Mankind Evolving*, he charged (with a certain degree of exaggeration) that the logical extension of Muller's philosophy would be selection of "the ideal man, or the ideal woman, and to have the entire population of the world, the whole of mankind, carry this ideal genotype" (p. 329). He thought that prospect disastrous not only from a genetic but also from a social perspective.

Dobzhansky prized human social and cultural differences. And he saw these as linked to—indeed dependent on—genetic diversity. As we have seen, Dobzhansky believed as strongly as Muller that differences in intelligence and temperament are genetically influenced. Indeed, he sometimes expressed stronger views, as in his (often repeated) comment that equality of opportunity "would be meaningless if all people were genetically identical" (1962, p. 244). (Such a statement would itself be meaningless unless all variation were genetic). The real argument between Dobzhansky and Muller was not over the existence but over the value of these differences. Dobzhansky always insisted that "Genetic diversity is a blessing, not a curse" (1971, p. 23). Muller did not really wish to create a population of

human clones. But he would certainly have welcomed a significant increase in the population average for intelligence and various skills and personality traits. When Dobzhansky asked "Do we really want to live in a world with millions of Einsteins, Pasteurs, and Lenins?" the question was rhetorical, for the answer was self-evidently in the negative (1962, p. 330). He was willing to eliminate genes for clear-cut diseases. But he would not interfere when it came to psychic traits. Dobzhansky sometimes wrote that "Eugenics will eventually come into its own" (1973b, p. 49). What (if anything) he meant by that is never clearly expressed. In respect to their cognitive abilities and temperaments at least, Dobzhansky seemed quite satisfied with people the way they were.

That attitude was at least in part a reflection of the view that all kinds of people were needed to do the world's work. As noted earlier, Dobzhansky believed that "Any human society, from the most primitive to the most complex (the latter more than the former), needs a diversity of men adapted and trained for a diversity of functions" (1973b, p. 44; see also 1962, p. 243). In his view, it was unlikely that the requisite diversity could be achieved simply by differential training. Most people could certainly do most jobs—but not all of them. Dobzhansky believed that he himself could have been brought up to be a peasant, a clerk, an engineer, or a soldier, but not a concert pianist, painter, boxer, sprinter, or mathematical prodigy. "Educability is not limitless," he wrote (1973a, p. 287). Moreover, the fact that one can do a job does not mean that one does it easily or with pleasure. Thus the ideal is a society in which all choose the occupation for which they are "most qualified genetically" (1973a, p. 284).

To make the most of genetic diversity, society must equalize opportunities. Only with uniform environments will differences in genetic merit be manifest. The result will be a genetic meritocracy. "With anything approaching full equality, every trade, craft, occupation, and profession will concentrate within itself those who are genetically most fit for those roles" (1973b, p. 45). Dobzhansky stressed that equalizing environments would enhance genetic differences among both individuals and groups since the greater the equality of opportunity in a society, the more the differences among its members are likely to reflect genetic differences (1962, p. 247; 1973b, pp. 29, 33).

However, Dobzhansky's social views were considerably more radical than his comments on meritocracy might suggest. In particular, he argued that the link between occupational status and financial

reward should be broken. If there were to be any economic inequalities, they should favor those who do the dirty and dangerous tasks, not the high-status, pleasant ones. "Manual labor is not intrinsically inferior to intellectual labor," he wrote, "even though more people may be adept at the former than at the latter" (1973b, p. 49), and he explicitly endorsed Marx's dictum "From each according to his abilities, to each according to his needs" (1973b, p. 42). Elsewhere he quoted approvingly the sociologist Christopher Jencks's comment that progress toward the goal of economic equality requires establishing political control over our economic institutions and that "this is what other countries call socialism" (1973a, p. 288). However, Dobzhansky's view that history was moving inexorably in the direction of greater economic equality was perhaps rather unrealistic.

In any case, until the 1970s Dobzhansky's views on the nature-nurture question would certainly have marked him as an "environmentalist." Consider in this respect Pastore's analysis (1949) of the political correlates of attitudes to the nature-nurture question. His book consists of twenty-four profiles of scientists active in the debate; twelve are characterized as hereditarians, twelve as environmentalists. H. S. Jennings could have spoken for most of Pastore's environmentalists when he wrote: "It is certain that all the things that affect character and conduct are deeply influenced by the hereditary materials. There is no characteristic or quality that is exempted from its influence. This conclusion is confirmed by all the many studies that have been made on the two types of twins. And it is in harmony too with all that we know of the science of genetics" (Jennings 1935, p. 204).

In 1949 few would have quarreled with Pastore's assignments to either category. Through the 1950s and 1960s, the positions remained stable. Among geneticists, Jerry Hirsch would emerge as one of Arthur Jensen's severest critics. But in the 1960s Hirsch did not dispute the heritability of human mental traits. Indeed, only two years prior to Jensen's article he wrote: "As the social, ethnic, and economic barriers to education are lowered throughout the world and as the quality of education approaches a more uniformly high level of effectiveness, heredity may be expected to make an ever larger contribution to individual differences in intellectual functioning and consequently to success in our increasingly complex civilization" (1967, pp. 434-435). As noted earlier, the first scientific critiques of

Jensen's article assume a fairly high heritability of intelligence; they object principally to the import accorded heritability estimates and the extrapolation from individuals to races.

But in the highly charged atmosphere of the early 1970s, the critique of Jensen began to broaden. This shift in approach was spurred by the scandal involving the work of British psychometrician Cyril Burt. In a 1974 book, *The Science and Politics of IQ*, Leon Kamin charged that Burt's influential results (which apparently demonstrated an 80+ heritability of IQ) were, statistically speaking, too good to be true. This is not the place to review the history of the scandal that followed. Suffice it to say that Kamin's suspicions were justified; Burt had apparently fabricated at least some of his work. The ensuing scandal led to a reanalysis of other classic studies of the heritability of IQ. All were judged and found wanting by contemporary methodological standards. In light of these critiques—which focused on studies purportedly demonstrating a high heritability of IQ within the white population—some began to question conventional assumptions concerning individual differences in intelligence and personality.

Within just a few years, the critical position had been transformed. For example, after reviewing all the classic studies of the heritability of IQ, Kamin concluded: "There exist no data which should lead a prudent man to accept the hypothesis the IQ test scores are in any degree heritable" (1974, p. 1). Soon, critics of Burt, Jensen, and Richard Herrnstein (who advanced an argument in respect to social class analogous to Jensen's concerning race; 1971, 1973) appeared to commit themselves to the proposition of zero heritability of any interesting skill or behavior. That was obviously not a perspective that Dobzhansky could share. Indeed, it denied assumptions he considered self-evidently true.

The debate of the 1970s focused on two issues: whether it was possible to design human behavior genetic studies that met reasonable methodological standards; and whether the effort to do so was justified by their potential scientific or social value. For most critics, the answer to the first was "probably," and to the second, a certain "no." For Dobzhansky, the answer to both was a certain "yes." That is why, in spite of a position remarkable for its consistency over a thirty-year period, he came to find himself first on one side and then the other of the nature-nurture debate. He was both baffled by and indignant at this development. Had he lived another five years, however, he

would have seen the debate shift once again—virtually back to where it was in the 1960s.

There remain important issues in dispute. But for the most part, they are disputes among interactionists. Moreover, the point now stressed by critics of contemporary behavior genetics is in fact the point Dobzhansky himself did most to popularize (even if he did not always acknowledge its full implications): Heritability estimates apply only to a specific population in a specific range of environments. Unless we know the full range of environments over which genotypic expression may vary, we are not justified in assuming the ineffectiveness of environmental change. And that is information we do not, and indeed cannot, possess.

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