Richard Lewontin and the Argument from *Ethos*

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Abstract: This essay uses rhetorical analysis to defend the population geneticist Richard Lewontin from accusations made by E. O. Wilson and others that his Marxist social philosophy distorts his empirical science. I suggest that Lewontin's appeal to his own authority as an experimental evolutionary biologist supports his claim that racism has no biological justification and that it is his opponents whose assumptions about society distort their scientific arguments.

Keywords: behavior genetics, Darwinism, *Ethos*, Dobzhansky, T., eugenics, Jensen, A., Herrnstein, R., Lewontin, R., Marxism, population genetics, scientific racism, rhetoric of science, Wilson, E.

Authority in Scientific Argumentation

Richard Lewontin, whose education in biology at Harvard was heavily weighted toward statistical mathematics, earned his doctorate in zoology at Columbia under the Russian-born geneticist Theodosius Dobzhansky in 1954. Arguably, he was, and in some sense still is, the most insightful inheritor of his mentor's effort to use the study of how genes are distributed in populations to turn Darwin's theory of natural selection in an anti-racist, anti-eugenic direction (Dobzhansky, 1962; Jackson and Depew, 2017). "My own problematic is the problematic of my professor," he later wrote. "I recognize that everything I do in science I get in one way or another from the program he initiated" (Lewontin *et al*., 2000, 29; Lewontin, 1989, 44). Lewontin was not alone in defending what has been called "the biology of democracy" (Beatty, 1994). Still, he was more inclined to deflate his opponents by attacking their credentials and character along with their reasoning than, for example, his Harvard colleague, ally, and sometime collaborator Stephen Jay Gould, the genial tone of whose popular science writing was also aimed to laughing his professional enemies out of their follies (Gould and Lewontin, 1979). Lewontin is the most obstreperous of Dobzhansky's heirs.

As a graduate student, Lewontin found himself in a rhetorical situation that Dobzhansky himself was ill-equipped to confront: the

molecular revolution in genetics led by James Watson and Francis Crick, who in 1953 jointly unraveled the information-carrying mechanism of DNA. This revolution, in whose long wake we are still sailing, was at its best indifferent and at its worst hostile to the egalitarianism that Dobzhansky, by working closely with heirs to the anthropologically based anti-racism of Franz Boas, had impressed on the genetic theory of natural selection (Smocovitis, 2012; Jackson and Depew, 2017). From the start, Lewontin's strategy was to turn the molecular revolution, whose roots lie in biochemistry, against the prejudices brought with them by many of its adepts when they began poaching on the territory and authority of evolutionary naturalists. He did so by using the new technique of gel electrophoresis to experimentally demonstrate what Boas and Dobzhansky might have presupposed: Even at the molecular level genetic variation is too plentiful and too widely disbursed to give biological standing to traditional racial divisions, let alone to privilege one over another, or to permit eugenic meddling by identifying "types" of people, such as criminals (Boas, 1911; Dobzhansky, 1941; Lewontin and Hubby, 1966; Lewontin, 1970, 1972).

Partly because of his conception of the duties of a scientist and partly because he thought that push-back against the fragile gains of the civil rights movement had raised the social and political stakes, Lewontin launched withering attacks on the mathematics, motives, and authority of those who in his view ignored or misread the evidence he had found. These attacks were intense enough when they were directed against the anti-egalitarian social scientists Arthur Jensen and Richard Herrnstein. But they were no less intense when Lewontin began hurling variations on the same arguments at his fellow evolutionary biologist, colleague, and department chair, Edward O. Wilson, who did not obviously deserve them, and his 1974 book *Sociobiology*. Wilson had brought Lewontin (back) to Harvard from the University of Chicago in 1973 to help him counter the growing influence of his unwelcome departmental colleague Watson, whom Wilson called "Caligula" (Wilson 1994, 219). Understandably, he was surprised, disconcerted, and hurt when Lewontin turned on him (Seegerstrale 2000).

In defending himself, Wilson called attention to the rather hardline but anti-Stalinist and to that extent social democratic Marxism Lewontin had professed since his youth. His charge was and is that Lewontin's ideology, not scientific empiricism, was guiding his claims and distorting his arguments (Wilson to

Lewontin, January 5, 1975; Wilson, 1978).[1] The accusation stuck. Even Lewontin's admirers and collaborators have worried about it (Singh *et al*., 2001b, 5). Robert Trivers, a Harvard biologist who from the start was sympathetic to Wilson's *Sociobiology*, did not have to expect much blowback when not long ago he described Lewontin as "a man with great talents who often wasted them on foolishness, on preening and showing off, on shallow political thinking and on useless philosophical rumination while limiting his genetic work by assumptions congenial to his politics" (Trivers, 2015).

In this essay, I will shine a more positive light on Lewontin's polemics by putting them in historical perspective. I will do so with help from rhetorical approaches to scientific argumentation. It is well known to rhetorical scholars, if less often to their colleagues in the philosophy and history of science, that in the give and take of controversy scientists effectively persuade both public audiences and their professional peers by appealing, in addition to logically concatenated evidence (*logos*), to their own authority (*ethos*), the emotions of their audiences (*pathos*), and the exigencies of specific situation (*kairos*).[2] Rhetorical scholars of science also know that, like other rhetors, scientists tend over the course of their careers to favor as much as politicians or preachers one sort of argumentative strategy (*topos*) over others (Weaver, 1953). My aim in appealing to rhetorical analysis is not to re-litigate point and counterpoint in the debates that marked Lewontin's career. Instead, it is to document, in the first instance, how fond he has been of the argument from his own authority and, second, to suggest that highlighting this characteristic shows why his Marxism may after all be in the service of science. Even so, my aim is less to insist that he was right than to explain how he went about trying to persuade.

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[1] All quotations from Lewontin's correspondence are from the papers of Richard C. Lewontin, American Philosophical Society, Philadelphia, PA. A Mellon Emeritus Fellowship funded research in this archive. This essay details, documents, and developments points sketched in Jackson and Depew, 2017. My thanks to the Mellon Foundation and to John P. Jackson, Jr., who helped me interpret these sources and arguments. He is not to be held responsible for the result.

[2] My stress in this essay is on *ethos*. See Garver, 1995 on what Aristotle's *Rhetoric* calls the proof (*pistis*) from the e*thos* or character of a speaker. On the self-presentation or *persona* of the scientific expert, Keränen, 2010; Hartelius, 2011.

The tendency to stress his own authority first appears in Lewontin's efforts to wrest control of Dobzhansky's research program on the ground that his former mentor's experimental and mathematical skills were inadequate to refute the strain of genetic determinism in molecular evolutionists. After suggesting the technical reasons for this "anxiety of influence," I will show why Lewontin's criticisms took on a more political, and ultimately a more personal, tone in the 1970s. Dobzhansky's failure to dissociate himself far enough from the Behavior Genetics Association's positive response to Jensen's genetic explanation of the lower performance of African-American in IQ tests seemed to Lewontin to confirm weaknesses he had already spotted in Dobzhansky's interpretation of the anti-racist and anti-eugenic research program they shared. I go on to discuss Lewontin's efforts to destroy the credibility of Herrnstein and Wilson in the light of his concerns about these weaknesses. By recounting these episodes, I hope to suggest why Lewontin's "dialectical biology," as he and his frequent co-author Richard Levins call it, is simultaneously scientific and social-critical.

How Lewontin Got His Scientific Mojo

In the first third of the 20th century, the new science of Mendelian genetics was at loggerheads with Darwin's stress on natural selection (Provine, 1970). They were integrated in the 1920s and 30s by shifting the discussion away from individual organisms to the level of interbreeding populations, which were modeled as statistical arrays and so were amenable to calculations of probability. Population genetics mathematically tracks how the frequency of genotypes over multi-generational time shifts in populations of interbreeding organisms under the influence of various evolutionary factors, such as mutation, fixation of genes by chance in small populations ("genetic drift"), and, preeminently, natural selection. The aspiration of the Modern Evolutionary Synthesis of the 1940s and 1950s was to unify all biological fields by recasting them in population-genetic terms (Huxley, 1942). Of the founders of its American branch, Theodosius Dobzhansky, Ernst Mayr, George Gaylord Simpson, only the first was a geneticist. The focal claim of his *Genetics and the Origin of Species*, which catalyzed the formation of the Synthesis, is that because environments are constantly shifting, not least in virtue of changes wrought by organisms themselves as they go about living, natural selection favors wherever it can the evolution of mechanisms that create and preserve genetic diversity that it can use later to keep populations adapted when environments change (Dobzhansky,

1941). If the vast majority of lineages we see around us today possess such mechanisms it is because the others never got off the ground or have gone extinct.

Prominent among diversity-preserving and hence diversity-increasing mechanisms are the diploid chromosome we see in many animal lineages and its analogue in some plants, polyploidy. Mendel's laws of inheritance were built, at first unwittingly, on diploidy. They assume that each site or locus on a chromosome has room for two alternate forms of a gene ("alleles," from Greek *allêllos*). A single locus of a diploid chromosome might contain either two dominant alleles, or two recessives, or one of each, all drawn from a larger "gene pool" of alleles distributed across populations. Loci with two dominants and two recessives are called homozygotes. The mixed configuration is dubbed heterozygotic. Unless natural selection and other forces of evolutionary change are at work, these three configurations will recur generation after generation in a fixed proportion: For every two heterozygotes, there will be one homozygote with two dominants and one with two recessives.[3] Since evolutionary forces are constantly in play, however, this equilibrium state is almost always being disturbed.

Dobzhansky focused on why the frequency of heterozygotes often climbs disproportionately above equilibrium in a population. The answer is heterozygote reproductive advantage or heterosis. It explains the hybrid vigor that farmers have long noticed and seed companies commercially exploit. The best-documented case is the spread of the heterozygote configuration of the alleles of red blood cells that confer protection against illness or death in populations afflicted by sickle cell anemia. = The biological world is highly dynamic. What is adaptive is tied to and in turn affects environmental contingencies. Thus in some situations, a double dominant, which constitutes the healthy "wild type" in species at most times, may actually lower reproductive output. Similarly, it is possible for natural selection to amplify double recessives. Still, Dobzhansky maintained that natural selection generally favors heterozygotes both because they enhance hybrid vigor and because the genetic variation banked in their recessive alleles might become adaptive when environments change. By the beginning of the 1950s he was arguing that heterozygote superiority is itself an adaptive effect of natural selection. It evolved in order to maintain a balance

[3] This is so because two alleles in three possible configurations follows the quadratic equation: aa + 2ab + bb. This is called the Hardy-Weinberg Equilibrium Law, after two mathematicians who independently derived it in 1908.

between the current and prospective utility of alleles. His graduate students were tasked with finding experimental evidence for this adaptationist version of the "balance hypothesis" (Beatty, 1987; Depew 2011).

Diploidy and polyploidy are not the only means of generating genetic variation and making it available to natural selection. Watson and Crick's 1953 discovery threw the emphasis on mutation in DNA sequences: the random substitution in someone's genome of adenine for thymine in making an amino acid, for example. Still, Dobzhansky was more impressed by mechanisms for generating variation in which a vast array of genetic diversity becomes available when segments of DNA invert their order or change their position on a chromosome. For this reason, he claimed in 1959 that, "Suppression of mutation process would probably have little effect on evolutionary plasticity for some time to come" (Tax and Callender, 1960, III, 115).

Lewontin's lasting claim to scientific fame, and the source of his *ethos*, is that in 1966 he found a way to substantiate an assumption of Dobzhansky's view that natural selection maintains a balance between current and future utility. With help from the experimentalist Jack Hubby, his colleague at the University of Chicago, he used gel electrophoresis to show that the amount of genetic variation found in proteins, the immediate products of DNA and RNA, is as heterozygotic as it is at the organismic level. In gene pools there may be as many as eight or ten alleles for amino acids, the building blocks of protein, that can slot into either position at a diploid locus (Lewontin and Hubby, 1966).

It did not take Lewontin long to use this result to firm up a claim that had drawn him to Dobzhansky's lab in the first place. Even at the molecular level the amount of heritable variation within conventionally designated human races is much higher than the amount of variation between them:

> Although there is variation between loci in their relative contributions, the average values show that 85 percent of human genetic diversity is within national populations and only 7.5 percent between nations within races and 7.5 between major races . . . Since most of the world's population is made of Chinese, Indians, Europeans, and the recently hybridized populations of South America, who vary less from each other than do small isolated groups, the correct proportion of human genetic variation that is within nations or tribes is [actually]

> closer to 95 than to 85 percent (Lewontin 1974a, 155-156).

This result has held up.[4] This being so, Lewontin's point is that even on the genetic-reductionist terms favored by molecular biologists one must either demote the term 'race' to a social construction with no foundation in biology, as the anthropologist Ashley Montagu had long urged (Montagu, 1942), or redefine it in population-genetic terms that carry none of the non-egalitarian implications of the conventional understanding of race, as Montagu's sometime collaborator Dobzhansky argued (Jackson and Depew, 2017). In the second case, it might well be that by the standards used to identify biological races in other species there are no races of human beings, a position first urged by Frank Livingstone and widely accepted by biological anthropologists to this day (Livingstone and Dobzhansky, 1962).

Lewontin got the attention of his molecular colleagues. "I was the [molecularists'] captured population geneticist," he wryly noted in retrospect (Lewontin 1989, 40). In 1968, a fellow population-geneticist who was visiting Caltech reported back on a lecture Lewontin had recently given to its hard-nosed molecular reductionists. "They were impressed by your talk … especially [Max] Delbrück," wrote James Crow. "He would like more. Can you send me some of your data or mss?" (Crow to Lewontin, March 6, 1968; Lewontin *et al*. 2000, 39). A month later Crow congratulated Lewontin on his election to the National Academy of Science (Crow to Lewontin, April 24, 1968). Lewontin declined the honor because of the Academy's involvement in weapons work during the Vietnam War. Still, he was pleased that he had "sideswiped" the molecular "bandwagon," as he later put it (Lewontin *et al*. 2000, 35). He was acutely aware that by the late 1950s not just the balance hypothesis, but the entire field of population-genetic evolutionary studies might be verging on

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[4] Current estimates are that only 2.5% of the genetic variance in *H. sapiens* is between national or ethnic populations within conventional races and only 4.3% between conventional races. Alan Templeton is the latest biological anthropologist to conclude that by most measures of genetic variance there is not enough discontinuity between human races to support the claim that there are any biological human races (Templeton, 2013, 6). By these lights chimpanzees have three races, differences between which account for 30% of the total genetic variance (25% is usually taken to be necessary for positing a race). Lewontin's results have been challenged by Edwards, 2003, but see Winther, 2014 for an argument claiming that what Edwards questions is slightly different from what Lewontin was addressing and how he addressed it.

"collapse" in the face of the molecular juggernaut (Lewontin *et al.*, 2000, 35). If he failed to validate Dobzhansky's claims about how much and precisely how genetic variation is distributed in natural populations at the level of proteins a key line of defense against racism and eugenics would be breached.

True, many molecularists held that most of the genetic variation in the structural gene products Lewontin uncovered is not subject to natural selection and so has no biological function at all, leaving the question of races and their purportedly unique adaptations moot. They claimed it is fixed in populations by chance (Kimura, 1968). Lewontin had a healthy respect for the role of chance mutation in evolution. Nonetheless, in his 1974 treatise *The Genetics of Evolutionary Change* he shifted the burden of proof to "neutral mutationism" or what several of its advocates called "non-Darwinian evolution" by framing it as a disguised version of Hermann Muller's claim that there is one and only one fit genotype for each locus (King and Jukes, 1969; Lewontin, 1974a). Lewontin was implicating their neutralist opponents in Muller's eugenic enthusiasms and in begging the question not only against balancing selection, but also against Lewontin's own insistence that genetic fitness at any particular locus is sensitive to what is happening elsewhere in the genome as much as to even slight environmental changes (Lewontin, 1974a). Lewontin's "genetic relativity" signaled his early opposition to the notion that there are "genes for" this or that trait. It also meant that he was suspicious of aspirations to manipulate them. Even if "gene therapy" were to succeed, he suspected it would still carry eugenic implications and racist biases (Lewontin, 1993).

Grateful for Lewontin's help in dealing with the molecularists, supporting him in his running argument with Muller, and refuting racism once again, Dobzhansky told him *The Genetics of Evolutionary Change* was "a great book" (Dobzhansky to Lewontin, January 5, 1973). But the very fact that Dobzhansky needed help seemed to intensify Lewontin's growing sense of himself as a molecular and mathematical virtuoso and increasingly to lead him to dissociate himself from Dobzhansky's technical inadequacies: his dependence on others to solve even the simplest problems in mathematical population genetics (Lewontin, 1989, 88-89); his failure to run proper tests of statistical significance on his experiments (Lewontin, 1995a); his use of examples rather than complete data sets as evidence for his interpretations of evolutionary phenomena (Lewontin, 1989, 91; 1995a, 93); his willingness to discard unwelcome experimental results (Lewontin, 1989, 33; 1995a, 93); and his tendency to turn the adaptive

superiority of heterozygotes into something close to a definitional truth when experiments appeared to challenge it (Lewontin, 1989, 97; 1995, 97; Lewontin *et al*., 2000, 31).

The affective aspects of his assertion of scientific authority are recorded in correspondence that makes for painful reading. When he came to Columbia as a graduate student Lewontin idolized Dobzhansky (Lewontin 1989, 2). As early as 1963, however, his mentor sensed that for some time "There has not been a case when anything that I said, wrote, or done has met with your approval… I am so used to your disapproval that it no longer hurts me as much as it used to" (Dobzhansky to Lewontin, May 7, 1963). Responding to a letter in which Lewontin congratulated him on receiving an honorary degree, Dobzhansky wrote, "Your congratulations are greatly appreciated as [is] any sort or kind of approval from you" (Dobzhansky to Lewontin, December 18, 1964). Nonetheless, in 1970 Lewontin complained that Dobzhansky was slighting *him* by strenuously attempting to find a suitable appointment and professional honors, such as election to the National Academy of Science from which Lewontin had resigned, for Bruce Wallace, a former Dobzhansky student whose elegant experiments had done much to support the balance hypothesis. Lewonin had to be reassured that *he* was Dobzhansky's best student and reminded that he was capable of looking after his own interests in ways Wallace was not. "[Bruce] lacks your magnificent verbal ability," Dobzhansky reminded Lewontin. "He has not fully overcome his folksy early background" (Dobzhansky to Lewontin, October 21, 1970). Lewontin was close to Wallace. Why he could not see this for himself until Dobzhansky pointed it out is hard to grasp. In a handwritten note he apologized, blaming his "selfish" involvement in his own career and a life-long tendency to "criticize my betters" that he didn't really seem to regret (Lewontin to Dobzhansky, October 24, 1970).

This is not the last we will see of Lewontin's anxiety of influence. Dobzhansky and Lewontin's correspondence shows that, despite mutual misunderstandings, genuine disagreements, and emotional dustups like the one I have recounted, they continued to work in tandem to advance and defend the research program they shared until Dobzhansky's death. That occurred in 1975, just as Lewontin's disputes with Herrnstein and Wilson were reaching a boiling point. Thereafter, his privately nursed objections to Dobzhansky's ideas morphed into a growing habit of disparaging him in public. Why?

Behavior Genetics: Jensen, Dobzhansky, Lewontin

The Behavior Genetics Association (BGA) met formally for first time in 1971. Its aim was to take up the hereditarian case for human behavioral traits after the transformation of the American Eugenics Society into the Genetics Society of America in response to the postwar discrediting of eugenics (Barkan, 1992). For this reason, even at the risk of protesting too much, the BGA's rhetoric placed great emphasis on the scientific objectivity of its research (Panofsky, 2014). Dobzhansky allowed himself to be elected its first President. Those who nominated him believed that his eminence would lend luster to their cause. For his part, Dobzhansky's goal was to commend to the BGA the research program that he and his Columbia colleague L. C. Dunn had set afoot in their Institute for the Study of Human Variation.

Against a background in which blood types were construed as carrying risks when transfused across conventionally demarcated races--as recently as World War II wounded white soldiers were not given blood from African Americans--the Institute helped map the worldwide distribution of blood types and show that, while populations carry different proportions of types like O or A, blood of the same type can be successfully transfused to anyone. Another project focused on genetic diseases. Recessive genes are recessive for a reason. Double recessives will often show up as "inborn errors of metabolism," as the early twentieth-century physician Archibald Garrod called them. Dobzhansky and Dunn searched for genetic disorders that statistically plague small and reproductively isolated populations. The dominant strain in the BGA, however, was quite different. It was to find heritable differences in individuals. "The main business of behavior geneticists has always been individual, not group differences," wrote one of its founders, "and the day-to-day research of most behavior geneticists questions about group differences are at best an unwelcome distraction" (Loehlin, 2009, 7). Many behavioral geneticists focused on studies of twins separated at birth to show inherited similarities that manifested themselves in individuals raised in what they presumed to be relevantly different environments.

Although Dobzhansky's effort to set his new colleagues straight about "population thinking" met with little success, these tensions seemed manageable until the publication in 1969 of a study of racial differences in IQ in *Harvard Educational Review*. The psychologist Arthur Jensen's answer to the question his title asked, "How Much Can We Boost IQ and Scholastic Achievement?" was: not much

(Jensen, 1969). The article resuscitated the case for racial differences in intelligence in an already vexed rhetorical situation in which Lyndon Johnson's push for a Great Society was putting teeth into the 1954 *Brown v. Board of Education* Supreme Court decision, the Civil Rights Act of 1964, and the Voting Rights Act of 1965.

Jensen argued that IQ is an objectively measurable and strongly heritable trait and endorsed studies showing that, while African Americans as a group outscore whites from an early age in motor ability and are comparable in associative learning, they fall between 11 and 15 points behind whites in terms of a measure of conceptual and inferential intelligence called "g" by the English psychologist Charles Spearman. Jensen inflamed matters further by inferring that the recently passed Head Start Program, which uses early education to raise the problem-solving intelligence of minorities and help them gain access as adults to the skilled work that confers middle class status in America, was a waste of time, money, and effort:

> The evidence so far suggests the tentative conclusion that the pay-off of preschool and compensatory programs in terms of IQ gains is small … The techniques for raising intelligence *per se*, in the sense of g, probably lie more in the province of the biological sciences than in psychology and education (Jensen 1969, 108).

In spite of Jensen's statistical competence, which he conceded, Lewontin had reason to smell a rat (Lewontin, 1970; 1976b). As a visiting scholar in the UK, Jensen had come under the influence of the hereditarian psychologist Hans Eysenck and Eysenck's mentor, Cyril Burt. These people passed as reputable scientists until Gould, with characteristic story-telling verve, showed that Burt's data were invented (Gould, 1981). Having returned to the US, Jenson was encouraged to write his article by the physicist-engineer William Shockley. After doing research that led to the invention of the transistor, Shockley began a second career as a latter-day eugenics advocate and outspoken proponent of the genetic inferiority of blacks to whites in intelligence. He had no professional training in biology or any social science. In defending his claim when it came under attack, Jensen tended to rely on conservative polemicists such as Charles Murray of the American Enterprise Institute, who later co-authored with Herrnstein a best-selling Jensenist tract, *The Bell Curve* (Herrnstein and Murray, 1994). As late as 2005 Jensen was still claiming that the expansion of opportunities afforded African-Americans by liberal policies had affected his original findings not a whit (Jensen, 2005).

Fear that Jensen's article might be taken seriously galvanized opposition from a coalition of left-leaning student activists, professors, and citizens who were already mobilized to resist the draft and oppose the Vietnam War. Student radicals of that era believed the war had been brought on by people whom the journalist David Halberstam called with dripping irony "the best and the brightest:" defense intellectuals, denizens of think tanks like the Rand Corporation, academic game-theorists, and the like (Halberstam, 1972). The New Left assimilated Jensen to this picture. He was picketed and shouted down when he spoke at Berkeley and elsewhere. In his early years back at Harvard Lewontin spent a considerable amount of time developing arguments to support this stance in conjunction with the local branch of a national inter-campus network, modeled to some extent on Students for a Democratic Society, called Science for the People. His theme was that empirical evidence that rises to the level of scientific proof is hard to come by and that those who are not aware of the ideological distortions to which scientific inquiry is prone, or who through self-interest ignore them, are likely to jump to conclusions and light on hypotheses that support entrenched misdistributions of power, not least endemic, if sometimes subtle, racism. When a Statement signed by fifty members of the American Psychological Association, including a sprinkling of Nobel laureates, appeared in *American Psychologist* lamenting "the climate of suppression, punishment, and defamation of scientists who emphasized the role of heredity in human behavior," it was met with a Lewontin-influenced *Resolution Against Racism* that denied the very possibility that Jensen's work could have any scientific value (Page, 1972). Signed by over a thousand people, it was published on September 28, 1973 as a half-page advertisement in the *New York Times*.

Seven members of the BGA had signed the statement in *American Psychologist*. As a result, its leadership was forced to determine how best to insulate their newborn field, sensitive as it was about its scientific credibility, from the *Resolution Against Racism* (Panofsky, 2014). Dobzhansky used his address as outgoing President to articulate a *via media* between genetic determinism and environmental-cultural explanations of human traits (Dobzhansky, 1976; Dobzhansky to Osborne, May 7, 1973). He undercut racial essentialism by stating that even if some genetic markers are associated with nationality or race myriad others are not. He pointed out that behavioral traits are linked to many genes and are highly sensitive to even slight environmental changes. He argued that heritability is not equivalent to and does not entail

fixity or fate. He warned that projecting population-level statistics onto individuals is perilous and pointed out that phenotypically flexible heterozygotes express themselves in too many ways to allow simple links between genotype and phenotype.

All these things being so, Dobzhansky stressed the importance of carrying on research into behavioral genetics in ways that follow the principles of the Modern Evolutionary Synthesis. The BGA's new leaders did not accept these principles, however, and so rejected their President's input into a proposed Statement on Jensen to be issued in the BGA's name (Osborne to Dobzhansky, May 11, 1973; Loehlin to Dobzhansky, May 11, 1973). Instead, they opted for a harder line, circulating among themselves a draft statement in which they drew distinctions between "racial differences and racist ideas" and between "ideas and actions" in order to say that Jensen's freedom of inquiry was being violated. They went on affirm the right and desirability of conducting research on whether there are "genes for" this or that behavioral trait that might be attributed to individuals and are more prevalent in races. Although a shorter Statement was released, its message was that significant cognitive and behavioral differences between African-Americans and whites are a live possibility. The conception of race at work was conventional, not population-genetic. In vain Dobzhansky complained that although he agreed with the statement "as far as it goes" it should also have contained an explicit repudiation of racism. "All my life I have been fighting the prostitution of biology by racists," he told them (Dobzhansky to Jensen and Page, January 17, 1972; Dobzhansky to Lewontin, January 5, 1973).

Among those urging the BGA to take a harder line in defense of Jensen was Sandra Scarr-Salpatek, a member of a group of twin studies specialists at the University of Minnesota. In a letter to one of the BSA's founders, she wrote, "I am not sure why the number of BGA members who signed the letter should be distressing. It seems particularly appropriate for many of our members to lend support to a statement on the genetic factors in behavior and the necessity of free inquiry" (Scarr-Salpatek to Osborne, May 7, 1973). Badly misreading him, Scarr-Salpatek asked Lewontin to sign the draft statement and, in view of his prestige as Dobzhansky's heir, allow his name to appear as its co-author (Scarr-Salpatek to Lewontin, May 4, 1973). Lewontin replied:

> I am afraid I cannot possibly associate myself with the sentiments in your letter or a joint statement … You have been taken in by a false issue. The issue is not and never has been one of academic freedom … The simple and

> direct fact is that genetics is being used as a weapon in a social battle and that we disarm ourselves completely if we allow that battle to be fought on the terms laid down by racists … The … distinction between ideas and actions [is] metaphysical nonsense (Lewontin to Scarr-Salpatek, May 8, 1973).

In saying this, Lewontin was positioning himself to Dobzhansky's left. To be sure, in his Presidential Address to the BGA Dobzhansky repeated the message he had been sending ever since 1937: It makes no sense to talk about genetic and environmental factors without recognizing that in populations gene, organism, and environment are as dynamically inter-defined as speed, distance, and time in Galileo's equations. Apart from these relationships these terms have no scientific meaning. Lewontin credited Dobzhansky with having made this key point. "It is to Dobzhansky," he later wrote,

> that I owe my preoccupation with a correct relationship between organism, gene, and environment. Dobzhansky constantly reiterated that the fitnesses of genotypes are dependent on the environment and that … one could make no prediction whatsoever of which genotypes would get favorable and which unfavorable in a new environment (Lewontin, 1989, 25, 44).

Nonetheless, for Dobzhansky to acknowledge that the BGA's statement might be even partially right was according to Lewontin to lend credibility to a position that deserved no public or professional hearing at all, as he made clear to Scarr-Saltapek. Ten years previously Dobzhansky had taken a stance against the physical anthropologist Carleton Coon's *The Origin of Races* not unlike Lewontin's view of Jensen. It and its author deserved only to be discredited (Collopy 2015; Jackson and Depew, 2017). I can find no evidence that Lewontin reminded Dobzhansky of this fact, but I do find evidence that he saw Dobzhansky's dealings with the BGA as a retreat from his own principles.

For some time, Lewontin had taken exception to an image Dobzhansky began deploying in the late 1950s according to which changing environments "challenge" Mendelian populations to "solve the problems" they pose to organisms by shifting gene frequencies in an adaptive direction over trans-generational time. This trope, according Lewontin and his Marxist tutor and collaborator Richard Levins, implies that "the organism is molded to fit into a preexistent niche just as a key is cut and filed to fit into a lock" (Levins and Lewontin, 1985, 98). It portrays living beings as

passive objects that are as mechanistically shaped by external, pre-existing environments as they were for Herbert Spencer, the ur-theorist of "social Darwinism." Lewontin began arguing that organisms are not a disaggregated collection of adapted traits selected by their environments, but holistic beings that construct their own niches by evolving the agency to exploit them and live in them (Lewontin, 1982; Odling-Smee *et al.,* 2003). In this spirit, he remarked that in the late eighteenth century Jean-Baptiste Lamarck, although he had the wrong explanation for it, correctly described the active, striving, self-making agency of organisms (Levins and Lewontin, 1985, 85). Not coincidentally, Lamarck supported the social mobility promised by the French Revolution.

Lewontin believed that the fissure between environment, development, and gene implied by Dobzhansky's challenge-response model opened wider in his *Mankind Evolving* (Dobzhansky, 1962). In this influential book, his former mentor moved beyond opposing genetic arguments for eugenics and racism, which he rightly took to be a yoked pair,[5] to arguing that the primacy of balancing selection in populations offers evolutionary support for the liberal-democratic pluralism that was beginning to take hold in postwar America (Dobzhansky, 1962; Beatty, 1994). On the balance hypothesis, he maintained, freedom to choose one's occupation and one's mate, including interracial marriage, will make for far fitter populations than any kind of eugenic tinkering or caste-making. That is because on Dobzhansky's theory heterozygote superiority, and with it hybrid vigor, environmentally sensitive phenotypic flexibility, and genotypic capacity to use stored variation to adapt to new circumstances, will increasingly prevail when freedom to marry whomever one wishes and generally do what one wishes are politically protected.

But Dobzhansky also argued in *Mankind Evolving* that only under liberal-democratic conditions can genetic causes be separated from environmental effects (Dobzhansky 1962, 1973).[6]

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[5] In the postwar period, there were many authoritative geneticists who believed that repudiation of racism made scientific eugenics possible for the first time. Among them were Julian Huxley and Hermann Muller.

[6] Dobzhansky was arguing against Hermann Muller, a geneticist he respected but who held that only the social leveling of communist societies, by making the environment uniform, will reveal the true effects of genes (Beatty, 1989, 1994; Jackson and Depew, 2017, 121-123).

The causes of genetic diseases like Tays-Sachs or thalassemia are covered over by caste formation in traditional societies. In such societies, you cannot disentangle the effects of social prejudice or political regimentation from biological inheritance. By contrast, Dobzhansky maintained that in liberal democratic societies genetic diseases can be seen *as* genetic and treated as such. The reasoning is as follows. Given the proportions in which they recur in populations, wherever there are heterozygotes there will be a predictable proportion of double recessives, some lethal. So if liberal democracy favors heterozygotes it will still bring genetic diseases in its train. Perhaps with help from a reformed BGS, Dobzhansky wanted to track the incidence of these diseases, especially in liberalizing societies, because they would help confirm his balance hypothesis. But he also recognized that the inevitability of genetic diseases raises difficult questions about how such societies should cope with them.[7] We continue to struggle with these questions. The Eugenics Society of America may have become the American Genetics Society, but many people suspected, and indeed still suspect, that its liberal, informed-consent approach to genetic medicine still carries traces of eugenic thinking (Paul, 1998).

Lewontin is one of those people. He found remnants of eugenic thinking in Dobzhansky's view that genes are not just correlated with but are the proper causes of genetic diseases (Levins and Lewontin, 1985, 109-122).[8] He concluded that by thinking of genes as causes of pathological conditions his former mentor was letting his liberal politics weaken or betray his own principle that gene, organism, and environment are dynamically intertwined. Identifying genes as causes combined with Dobzhansky's model of environments as pre-existing niches into which organisms are inserted in ways that further fractured the bond between these factors. This being so, Lewontin felt he had to rescue Dobzhansky's

[7] It also caused Dobzhansky, who was religious--"I am a Christian," he told a correspondent (Dobzhansky to J. Greene, Nov. 25, 1961)--to worry in his last, illness-plagued decade about what God was up to when he allowed such a two-edged sword as heterosis to be the primary motor of evolutionary advance. See Dobzhansky, 1967.

[8] The issue turns on what Dobzhansky may have meant by leaving room for what he called "this much of eugenics" in allowing for research into how to eliminate double recessives from the gene pool without disturbing the creative work of heterosis or individual choice (Dobzhansky, 1962, 332-33; 1973, 105; see Jackson and Depew, 2017, 121-22).

"biology of democracy" from its founder's backsliding by envisioning both biology and democracy differently.

Lewontin's response to the problem of genetic diseases has been to make more than his mentor of the fact that a truly egalitarian society--for him a social democracy, in which the self-undermining tendencies and injustices of liberalism's free market capitalism are acknowledged and either abolished or compensated for--can obviate or mitigate many problems ascribed to the power of genes-gone-bad by providing universal access to everything from eyeglasses to dietary regimes to good housing that will change the environments in which genes express, or fail to express, themselves. If they are expressed, they will be treated fairly (Lewontin, 1993).

When Lewontin says that human traits are "not in our genes" he can easily be misunderstood (Lewontin *et al.*, 1984). He does not mean that they are the results of learning and cultural transmission alone. He means that the habit of dividing causal factors into a binary between "nurture and nature" is inconsistent with our most well founded knowledge of evolutionary biology, social dynamics, and their interaction. That biology is one thing and social facts another is almost an imperative in our discursive milieu. No sooner do people announce a solution to the nurture-nature divide than they repeat it (Keller, 2010). Things do not appear that way, however, to Lewontin and Levins, for whom Marx and Engels' dialectical materialism treats culture and biology as a monistic whole and so takes seriously the idea that social-political changes can alleviate suffering far more effectively than dualistically conceived, industrially financed, and unfairly applied gene therapy (Levins and Lewontin, 1985; Lewontin, 1993).[9] To us something seems wrong with the notion that we can affect biology by passing out eyeglasses. But, Lewontin writes, "Changes in sanitation, public health, and disease control have reduced the infant mortality rates of disadvantaged urban Americans well below those of even the richest members of seventeenth century society" (Lewontin, 1970, 7). This lesson, he said, should always be borne in mind.

Lewontin was nurturing these arguments even while serving as the principal editor of Dobzhansky's most important scientific papers, offering him congratulations on being awarded the National Medal of Science, and taking a hand in nominating him for a Nobel

[9] For Lewontin Descartes is the paradigmatic bad guy who introduces non-materialistic and individualistic conceptions of the soul, mechanistic notions of the body, and a dualistic gulf between them (Levins and Lewontin, 1985, 1, 82, 133; Lewontin, 1993).

Prize (Dobzhansky, 1981; Dobzhansky 1974c; Lewontin to Dobzhansky, November 14, 1964). Still, it is probably no accident that Dobzhansky first sensed Lewontin pulling away from him soon after *Mankind Evolving* appeared and that this distance intensified in the mid 1970s when Lewontin was grappling with Jensen, Herrnstein, and Wilson. The last glimpse we have of the two together shows Lewontin berating Dobzhansky with a convoluted argument taxing him with the genetic determinism he had spent his career opposing. If Dobzhansky is right about recessives, he insisted, even heterozygotes will not be as phenotypically plastic as their most vocal advocate touted them as being (Lewontin *et al.* 2000, 30; Lewontin 1989, 29-30).

This dispute took place in 1974 on a fruit fly hunting expedition in Anza Borrego Desert State Park in California, not too far from the San Bernardino Mountains where some forty years earlier Dobzhansky had begun his field research on the humble and surprisingly diverse organism on which almost all his subsequent claims were founded. Lewontin says he remembered the occasion because, city-boy that he was, he sprained his ankle in the course of trying to pin genetic determinism on Dobzhansky. Absorbed in his own argument, he wasn't watching where he was going (Lewontin *et al.* 2000, 30; Lewontin 1989, 29-30). The outdoorsy Dobzhansky did fine under this peripatetic assault even though he was suffering from the advanced leukemia that would lead to his death only a year later.

After Dobzhansky's death, Lewontin wrote several only sporadically critical appreciations of his lifework (Lewontin, 1974c, 1989, 1994). In a volume meant to honor him twenty years after his death, however, he called him a "theorist without tools" (Lewontin 1995a). He meant that Dobzhansky lacked the mathematical and experimental skills around which Lewontin had by then formed for himself a *persona* that to his mind was better able to manage the scientific underpinnings of the anti-racist, anti-eugenicist values he and Dobzhansky shared in a rhetorical situation in which those values were under renewed attack. By 2000, Lewontin was offhandedly remarking that Dobzhansky could "barely add 2 + 2" (Lewontin *et al.* 2000, 29). A righteous crusader was sallying forth to slay monsters. And he was mounted on a very high horse.

Lewontin Takes on Herrnstein

Lewontin's blunt-talking, science-respecting, politically censorious *persona* became fully visible in his attack on the social psychologist Richard Herrnstein, which began almost as soon as he became

Alexander Agassiz Professorship of Zoology at Harvard in 1973, and so Herrnstein's colleague. Piggybacking on Jensen, Herrnstein had argued in an article in *The Atlantic* that the increasingly meritocratic political system of the US would eventually result in a class structure that mirrors genetically based IQ scores (Herrnstein, 1971). Lewontin was incensed not just because Herrnstein was a Jensenist, but also because he was blundering into Lewontin's area of expertise: how evolution structures populations over time, including human populations. He challenged Herrnstein to a debate. "My purpose," he informed him in a letter replying to Herrnstein's wary inquiry about why Lewontin was picking a fight,

> is in some small way to counteract the rubbish that you and others have been producing. I do not accuse you of being primarily racist; I do not think you are. Your purpose is to convince people that their position in society is biologically determined in large part. [But] since in America black people are disproportionally represented in the lower classes, people will believe that you think black people are genetically inferior. You may say they are special, but you have no proof of that either. You have no particle of evidence one way or the other. You are a political propagandist masquerading as a scientist. I will continue to struggle against you in any way at my command. P.S. Why did you copy [Lewontin's chairman] E. O. Wilson and Dean [Henry] Rosovky? Did you imagine they would chastise me? I shall let you know (Lewontin to Herrnstein, August 27, 1973).

Herrnstein sued Lewontin for painting him as a racist in public while in their private correspondence he claimed only that others would inevitably construe Lewontin's research as racist. To Herrnstein this meant that he had been defamed: publicly characterized in ways known by the offender to be false (Herrnstein to Lewontin, Sept 10, 1973). The suit didn't go anywhere; courts are generally loath to intervene in the obscure quarrels of academics. But in the course of bringing the lawyer he had retained up to speed, Lewontin remarked that Herrnstein "is threatened by my expertise … I have a high status in the field and have not hesitated to trade on it to discredit the pseudo-science being peddled by Herrnstein, Jensen *et al*." (Lewontin to Sidney Schreiberg 1973, reordered). Herrnstein didn't budge in response to this display of *ethos*. He went on to co-author with the conservative political polemicist Murray his suspiciously best-selling *The Bell Curve* (Murray and Herrnstein, 1984). It can plausibly be maintained,

however, that Lewontin did more or less permanently shift the burden of proof onto Jensen and Herrnstein by throwing his weight around.

Lewontin claims that he became aware of "the immense importance of status," and hence of the argument from *ethos*, when, testifying in a trial in 1979, his word as a "named professor at Harvard" was given more weight than that of an expert witness from the University of South Carolina (Lewontin *et al.*, 2000, 59). We see a clear example of this strategy in his reply to some sociologists whose statistical analysis of the self-reported sexual practices of American men and women he had taken to task:

> Although a biologist, I have a graduate degree in mathematical statistics and have taught the subject for forty years. About 10% of my technical publications, including a textbook of statistics, have been devoted to problems of statistical sampling, estimation, and hypothesis testing. More important, my biological work must be classified as methodological, my chief contribution to the field having been an analysis of the deep epistemological difficulties posed by the data of evolutionary genetics and the introduction of new experimental approaches specifically designed to overcome these ambiguities. Finally, my work on epistemological problems, produced both alone and together with philosophers of science, appears in standard philosophical journals. Whatever may be at issue here, it is not competence (Lewontin, 1995b).

Lewontin is a man of the Enlightenment. For him experimentally acquired scientific knowledge is about the best thing humans have produced, in part because it is the best guide to public policy. But having found a bit of it he knows how hard it is to acquire. He is also aware that the history of science, and not least of evolutionary biology, demonstrates that the gap between what scientists claim and what they actually prove is filled with interpretative stuffing that invariably supports not just old prejudices, but repressive distributions of political and economic power (Depew 2013). There is nothing worthier of suspicion than assurances that deviations from good methodology and fidelity to data may have been true of yesterday's science, but not today's. Look at claims of this sort made a hundred or even twenty years ago, however, and at best you will find fruitful discoveries mixed with fanciful and overly ambitious misinterpretations of them along with a generous helping of falsehoods that perpetuate biases and injustices that

science can and should combat. Why shouldn't the same be true today, or of any today? (Laudan, 1981).[10]

In light of such considerations Lewontin built something like the following reasoning into the *persona* he forged for himself as Dobzhansky's usurping heir. Those who naively presume that science is inherently transparent and progressive are more likely to fall into error than those who admit its entanglement in their society's deep-seated discursive tendencies to justify social injustices by attributing them either to how God or nature arranges things (Lewontin 1993, 3, 7). An example is Lewontin's claim that the large gap between the equality that liberal capitalist democracy promises its citizens and what it actually delivers is rationalized by widely disseminating and having everyone internalize the belief that "the position of those without power is the inevitable outcome of their own innate deficiencies," in recent times their genetic differences. If "nothing can be done about it," it is because a supposedly "natural sorting process" ranging over differential capacities "is to decide who gets the status, wealth and power and who does not" (Lewontin 1993, 20, rearranged). Lewontin rightly pins this presupposition on Herrnstein and his "meritocratic sorting process" (Lewontin, 1993, 21). But the shift to an ideal of equality of opportunity in postwar liberal democracies rather than to a more socialist equality of result implicated Dobzhansky too. In *Mankind Evolving* he rejected equality of result as utopian and defended equality of opportunity as more or less defining democracy (Dobzhansky, 1962).

Occasionally, Lewontin's censoriousness has led him to shift from what's wrong with other people's arguments to what's wrong with other people. Dobzhansky, we are told, was imperious to his wife (Lewontin *et al*. 2000). Watson's investment in genetic technology companies links his crude genetic reduction with greed (Lewontin, 1991, 51-2, 75). Nothing in Lewontin's argument about how society systematically impinges on science entails, however, that the tendency to insert ideology into the space between one's evidence and one's conclusion need be anything more than unwitting. Lewontin does himself no favors when he finds character faults and bad intentions in his opponents. Excessive appeal to the argument from *ethos* encourages efforts to undermine the *ethos* of others.

[10] An extreme version of this so-called "pessimistic meta-induction" is the Social Constructionist claim that scientific objectivity is illusory; what prevails at any time and place is a function of a theory's ability to justify existing or prospective social orders. I do not accept this argument.

Lewontin and Wilson: Is Adaptationism Racist?

Unlike Jensen and Herrnstein, the Harvard entomologist E. O. Wilson is an adherent of the Modern Synthesis, like Lewontin. Wilson first got to know him when in the early 1960s they attempted, together with Levins and a talented ecologist named Robert MacArthur, to integrate ecology into the Synthesis (Wilson, 1994, 252-31; Erickson 2015). They made some progress (MacArthur and Wilson, 1967). Still, Wilson's most abiding ambition was to "extend population biology and evolutionary theory to social organization" (Wilson 1978, x). The ambition was well motivated. If the Modern Synthesis was to triumph it would have to find a way to dispose of the long-standing objection that, while Darwinian natural selection can explain competition, it can't explain the cooperation we find in social species, including our own.

Wilson endorsed a solution proposed by William Hamilton, who calculated that social insects, including the ants Wilson studies, are more likely to evolve cooperative role-division because they are all members of a class, *Hymenoptera*, whose odd chromosomal structure makes it easier to evolve sterile and hence self-sacrificing castes without threatening the reproductive chances of the species but, on the contrary, enhancing it through division of labor. The argument depends on the linked notions of "inclusive fitness" and "kin selection." The first measures reproductive success not just by an organism's genetic contribution to its immediate offspring, but to the offspring of brothers, sisters, and in ever-more-diluted proportion *their* offspring. The second means that natural selection can favor adaptations that benefit not just individuals, but close relatives. In *Sociobiology* Wilson summarized evidence showing that these ideas are in play in natural populations of social insects[11] (Wilson, 1975).

The relevance of this claim to the problem of human cooperation is doubtful, since we are not social insects and our chromosomes are straightforwardly diploid. So, at the end of his book Wilson appealed to "reciprocal altruism," which depends on

[11] In 2010, Wilson withdrew kin selectionism as an explanation of caste dynamics in ants (Nowak *et al.*, 2010). Forms of group selection that were developed in response to *Sociobiology*, he judged, made for a better fit with the evidence. This belated admission had the effect, if not the intention, of burnishing the reputation for being an honest empiricist that Wilson began cultivating, perhaps in response to Lewontin's attack, but also reinforces Lewontin's assertion that when he wrote *Sociobiology* Wilson was bewitched by ideology. Lewontin, too, embraces some forms of group selection.

anticipated rewards, to apply kin selection to humans. This concept was championed by Wilson's Harvard colleague Robert Trivers, who went on to develop other theorems that were soon incorporated into the emerging field of sociobiology and its successor program, evolutionary psychology (Trivers, 1971). One such idea is "parental investment." It maintains that the degree of care expended on offspring is proportioned to the scarcity of genetic resources allocated to the task (Trivers, 1972). On this theorem, the supposed natural promiscuity of males and the supposed female instinct to nurture are traced to the fact that mammalian eggs and the genes they contain are scarce while sperm is not a limiting resource. Any man has millions of them. An implication is that the hostility between human tribes may be moderated by the elaborate exogenous marriage exchanges documented by anthropologists, but beyond the charmed circle of genetic relatedness it will be endemic.

Wilson might have expected Lewontin's praise for defending the consistency of Darwinism with cooperation. Had not *Sociobiology* given the lie to the Hobbesian conception of human nature as self-centered, individualistic, competitive, and prone to violence, which Marx himself had spotted as a capitalist prejudice marring Darwin's *Origin of Species? Sociobiology* also beat back the "naked ape" hypothesis, which takes up Konrad Lorenz's theory of aggression as a sudden, unstoppable instinctual discharge whenever any perceived enemy comes into sight (Segerstrale 2000, 28, 95; Lorenz 1966; Morris 1967). We are by nature cooperative, says Wilson, at least with our kin.

Accordingly, Wilson was taken aback when he discovered that in an office a floor below his own Lewontin had been composing and was distributing to anyone who wanted it a "white paper" setting out the case against *Sociobiology* on behalf of a hastily convened Sociobiology Study Group (SSG) made up of local academics who had been keeping an eye on Jensen and Hernnstein for Science for the People. Lewontin was a busy man in the year after *Sociobiology* was published. In addition to keeping the wheels of the SSG turning, he co-wrote a rebuttal of a letter in *Science* favoring behavior genetics and sociobiology that the editor repeatedly refused to publish because "it was so full of rancor" (Philip Abelson to Lewontin, June 17, 1976); delivered a paper at the Philosophy of Science Association's Spring, 1976 meeting entitled "Sociobiology--A Caricature of Darwinism" (Lewontin, 1976a); and wrote a negative reader report of a manuscript on the weaknesses of behavior genetics on the ground that it didn't hit the ideological offenses of its target hard enough (Segerstrale, 2000, 18, 28-30).

In "Sociobiology--A Caricature of Darwinism," Lewontin argued for what on its face seems an implausible claim: that Wilson, a straight-up liberal supporter of civil rights, should be charged with the same ideological sins as Herrnstein, Jensen, and the BGA (Lewontin, 1976a; Wilson, 1994, 338-339). One might see racism in Jensen, and, in spite of his protestations, Herrnstein, but it is a stretch to see it in Wilson. In a reply to Wilson's plaintive request that his colleague confirm or deny that he had been going around saying that "my book is not science," Lewontin recapitulated his insulting letter to Herrnstein in 1973 and denied that Wilson's book was a contribution to the Modern Synthesis by citing Dobzhansky against him (Wilson to Lewontin, October 27, 1976):

> We [SSG] said it wasn't a work of science … It couldn't have a real impact on science as opposed to public consciousness because to do so a book must have the effect of reorienting the thinking of working scientists, like Doby's [1937, 1941] book did, which almost single handedly created a school of experimental and natural historical studies and gave strong impetus to a number of theoretical developments as the result of its re-orientation of [views] about race formation and genetic variation in populations. Your book can't do this because it does not propose any testable hypotheses. The book has a lot of science in it but it is not [a book] of science. It not only is not a science, but a religion. [It is] a piece of scientific public relations (Lewontin to Wilson, October 28, 1976).

Sensing an argument from authority, Wilson weakly replied by citing biologists who thought *Sociobiology* was not only science, but good science (Wilson to Lewontin, January 5, 1976). Once Wilson regained his footing he took care to construct a pleasant, avuncular *persona* with which to address the public. This voice, so unlike Lewonin's, can be heard in the popular science he continues to write to this day.

Lewontin's attack on Wilson became legendary when at a meeting of the American Academy for the Advancement of Science partisans of Science for the People ran on stage and poured a bucket of water over the speaker's head, shouting "Wilson, you're all wet!" (Segerstrale 2000, 23). The argument from *ethos* had turned into an *argumentum ad baculam*. Such antics made it difficult to see the thrust of Lewontin's reasoning. Indeed, it is difficult to see his objections to sociobiology, let alone appreciate them, unless one has become familiar with the lines of argument (*topoi*) Lewontin had long been honing against Dobzhansky,

Jensen, and Herrnstein. Not even the most well informed student of these "colleagues in conflict," Ullica Segerstrale, does that systematically (Segerstrale, 2000). This is why I have reviewed the sequence of controversies in which these reinforcing and cumulating lines of argument emerged.

What lies at the heart of Lewontin's attack on Wilson is his extension of his objections to fracturing the bonds between organism and environment, gene and organism, and culture and nature to atomizing the organism itself into a collection of separately evolved adaptations, or what, writing with Gould, Lewontin called adaptationism (Gould and Lewontin, 1979). Wilson's commitment to adaptationism became even more ardent in essays in which he defended sociobiology than in *Sociobiology* itself. In *On Human Nature* we are told that our species-defining set of behavioral traits evolved in the long Neolithic period in which natural selection worked freely and gradually. Since then "the genes have [had] us on a leash, even if it is a long one" (Wilson 1994, 338-339). It is a good thing, too, that we are weakly determined by our genes to cooperate with our kin, because left to its own devices culture is destructive. It can cause runaway disasters like Aztec sacrifice (Wilson 1994, 167, 207).

In his critique, Lewontin charged Wilson with making worse the faulty notions that genes solve problems posed by environments and that the properties of those environments are somehow imprinted into a particular sequence of DNA that codes for this or that behavioral trait. Behind his technical objections to these now widespread conceptions, however, lies a more fundamental one. The ideas to which Lewontin was objecting change the relation between culture and biology that Dobzhansky had articulated by working closely with anthropologists like Ashley Montagu and Sherwood Washburn (Smokovitis, 2012; Jackson and Depew, 2017). Wilson, Trivers, and other sociobiologists reject the idea that our genes flexibly (and equally) adapt us to our cultural niche. Instead, they insist that they keep tugging us back to the Stone Age. "Having dissolved society [into atomized individuals]," Lewontin and Fracchia write, "Sociobiology's next step is… to neutralize culture" by dissolving the organism into atomistic adaptations and the genes they code for (Fracchia and Lewontin 1999, 71, reordered).

This issue is of great moment. For Lewontin, shifting this boundary means that the Modern Synthesis would no longer be able to protect biology from the racism and eugenics that by 1960 it had pushed to the margins of both public and scientific discourse (Jackson and Depe, 2017). The inference has nothing to do with

whether Wilson is a racist. He is not. But the atomistic ontology and genetic determinism with which Lewontin saw Dobzhansky flirting could easily metastasize into things Wilson took himself to oppose: social Darwinism, eugenics, and racism (Lewontin *et al.* 1984). To these sins we can add sexism. Trivers' notion of parental investment naturalized the gender roles feminists were beginning to challenge in the 1970s. Wilson predicted that male domination would persist indefinitely.

To make this point, Lewontin went beyond Dobzhansky's repeated insistence that proving a trait to be an adaptation is difficult to asserting that the very idea of adaptation is contaminated by the creationist ideology Darwin tried to refute but actually restated by describing natural selection as taking God's place as a designer (Lewontin 1978; 1993; Levins and Lewontin, 1985). An empirically grounded, experimentally based science of evolution that makes use of natural selection, Lewontin maintained, can thrive only by ridding itself of adaptationist thinking, even of Dobzhansky's sort.

This and related arguments will be inaccessible to anyone who, like Wilson or Trivers, thinks of ideology merely as a set of beliefs that someone might have about social policy or party politics. For Marxists of every stripe the function of ideology is to justify contingent, conventional, and local social practices by making them appear necessary, natural, and universal. Ideology confers legitimacy on those who exercise power through discursive practices--Marx's "superstructure"--that attempt to write systematic injustices and inequalities into the nature of things. These discursive practices are contained more basically in the conceptual frameworks in which we express our beliefs than in our beliefs themselves. In entrenched capitalist societies, these categories are heavily weighted toward individualistic, atomistic, dualistic conceptions of the person and economistic conceptions of our motives such as those we see projected onto genes in parental investment theory. The very fact that we think of an ideology as a particular person's belief or, worse, "belief system" testifies to the individualistic categories in which we think about ourselves and others.

This astigmatism dooms us to neglect social structures. If we say, "The tubercle bacillus is *the* cause of tuberculosis," Lewontin and Levins argue, we are singling out from a host of interacting factors a pathogen as "*the* cause," and hence *the* explanation, of tuberculosis, thereby averting our eyes from changeable social factors that in promoting inequality increase the likelihood of this and other diseases, including genetic diseases (Levins and

Lewontin 1985, 270; Lewontin 1993, 45). Likewise when the BGA suggests that a gene might be *the* cause of a propensity for crime (Levins and Lewontin 1985, 270). By making everything we do result either from individual self-interested choices or inborn defects in our chromosomes we simultaneously blind those affected by policies and those who make them to the fact that these arrangements are neither necessary nor natural.

Sticking Up for Lewontin

It might be argued that Lewontin's high-handed mode of address leads him to ignore the rhetor's first rule: Cultivate the good will of the audience (*captatio benevolentiae*). It is also potentially self-defeating. If he is right about ideology American audiences will have been too deeply formed by the ideological forces he reports to understand or accept his insights. After all, most Americans have never separated Marxism from Communist tyranny. They are likely to turn off a scold who, unlike Gould, does not at least profess to share their perspectives and values.

This objection leads to another. How can Lewontin, who claims to be so scrupulous about what is and isn't good science, be so cocksure about social theory, which falls outside his sphere of expertise? How, too, can an enemy of biological determinism take such an uncommonly deterministic, old-fashioned Marxist view of the etiology and functions of socially disseminated ideas? Ever since Gramsci there have been versions of Marxism that complicate the causal relation between economic base and ideological structure. They include the New Left Marxism that flourished in Lewontin's time and place. Why shouldn't his worry about how power distorts scientific inquiry--a thesis thoroughly established by sociologists, historians, and feminist philosophers of science--be just as telling if it is framed in terms of "humanistic" Marxism, which loosens the stranglehold the economic base is supposed to exert on the ideological superstructure, or even in the idiom of non-Marxian critics of ideology like Bruno Latour, who is as holistic about causes as Lewontin (Latour 1988)?

Our inquiry suggests an answer. Whether rightly or not, Lewontin and Levins do not read Marx and Engels' theory as deterministic, reductionistic, or even as functionalist, but as interactively holistic in ways that integrate biology and culture in an expanded naturalism. In their view, reflexive, self-critical dialectical argumentation, so long as it is materialistic, enables biologists, ecologists, and social theorists to see culture, nature, organism, environment, and gene as a complex, dynamical,

processive whole. By contrast, science's computational-logistic method and the pragmatic-problem solving idiom that Dobzhansky picked up at John Dewey's Columbia break this unity into a raft of dualisms that can never be put together again.[12] Seen from this angle, Trivers' and Wilson's accusation that Lewontin's ideology leads his science around by the nose is misplaced. His appeal to dialectical materialism as an *ancilla scientiae* reflects his search for a philosophy that can do justice to the biological complexity that he has had a hand in revealing and that is now a major theme in evolutionary inquiry.

I find in these reflections a way of understanding what motivates Lewontin's stress on his own authority. It is true but irrelevant that he has probably always presented himself as the smartest kid on the block. What is important is his tacit fear that if he were to address his audiences in the genial tones of a Dobzhansky or a Gould or a Wilson he would risk falling into the dualism, atomism, determinism, and mechanism on which he pounces. He would be unable to vouch for his own scientific claims or recognize any complicity in the dominant ideology they might have. He wants to communicate this reflexivity about the tacit premises and implications in his audience and encourage them to cultivate it. His censoriousness is principally directed at his fellow scientists. If they assume that science is as pure as the driven snow they are more likely to fail to hold themselves to high standards of evidence and to lend their authority to the dominant ideology. They will betray science's claim to knowledge. He is less critical of the general public. As a Marxist he presumes they are already on the right side. They need merely to be liberated from the discursive conditions that disempower them. Lewontin may be wrong about whether his know-it-all *persona* and his "dialectical biology" achieve, or even serve, these goals. Even so, in the land of the blind the one-eyed man is king.

[12] On Dobzhansky's professed pragmatism, see Jackson and Depew, 2017, 54n5, 114.

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