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Lewontin's Legacy and the Influence of Engels: A View from the Trenches

Sahotra Sarkar

ABSTRACT: Dick Lewontin's passing on the 4th of July, while this journal's inaugural issue was being compiled, marks the end of a dominating, and yet troubled, era for radical science, especially in the United States. He was preceded in death by his long-term comrade and friend, Richard Levins, who died in 2016. Another occasional collaborator, Stephen Jay Gould, the third member of Harvard's most distinguished triumvarite had died in his relative youth, almost twenty years earlier in 2001. All three were at the forefront of the social—and socialist—struggles of their generation, starting with the movement against the Vietnam War and continuing with the resistance to repeated outbreaks of well-organized scientific racism. It is apt to memorialize Lewontin here—and also mention Levins and Gould—because all three of these figures were explicit in acknowledging their debt to Engels.

KEYWORDS: Richard Lewontin, science, biology, Marxism, Richard Levins, Stephen Jay Gould.

LEWONTIN'S INTELLECTUAL LEGACY

Lewontin received his PhD in Zoology from Columbia University in 1954 under the supervision of Theodosius Dobzhansky, a collaborator of Sewall Wright, and an eminent evolutionary geneticist in his own right. (Earlier, in 1952, he had received a master's degree in statistics, also from Columbia.) Dobzhansky worked on Drosophila species that had been made central to research in genetics by Thomas Hunt Morgan's Columbia lab in the 1910s and 1920s, and these remained Lewontin's most favored experimental system for his entire scientific career.

Following his doctoral degree, Lewontin was an assistant professor at North Carolina State University from 1954 to 1958 and then moved to the University of Rochester where he was granted tenure and promoted to professor. In 1964 he moved again, to the University of Chicago where he

• *Correspondence:* Sahotra Sarkar, University of Texas at Austin.
• *e-mail:* sarkar@austin.utexas.edu
• DOI: 10.56063/MS.2201.01102
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remained until 1973 when he went to Harvard (where he had been an undergraduate, getting his degree in 1951) for the rest of his career, effectively closing his laboratory and retiring in 2003 but remaining active until about 2015. He was elected to the National Academy of Sciences in 1968 but resigned in 1971 because of its involvement in weapons research in the context of the Vietnam War.

Describing Lewontin's scientific achievements adequately would require a volume in itself. He made seminal contributions to theoretical population genetics starting in the 1950s, especially with his analysis of complex modes of natural selection and his work on two-locus models of selection (e.g., Lewontin and Kojima 1960). However, he is best-known for his collaboration with Jack Hubby at the University of Chicago to establish the unexpected extent of variation at the molecular (protein) level in natural populations of *Drosophila pseudoobscura* (Hubby and Lewontin 1966; Charlesworth et.al. 2016). Harris (1966) found the same pattern in humans. Together these results set the stage for Kimura's (1968) challenge to the selectionist interpretation of evolution by formulating a neutral theory that suggested that most variation was not subject to no selection at all or, perhaps, very mild negative selection. Though Lewontin was a strong critic of the received interpretation of evolution—and famously rejected adaptationism in a critique coauthored with Gould (Gould and Lewontin 1979)—, in the neutralist-selectionist controversy he sided with selectionism (Lewontin 1974b).

Lewontin was a founding member of Science for the People in 1969 and his scientific work in the decades that followed was more often than not motivated by social imperatives of the day. That began with a struggle to distance his own discipline of genetics from the scientific racism with which it had often been associated (Lewontin 1970a, b; Provine 1986). This work included, first, a critique of heritability analysis which, in a human context, has always been associated with racism (Sarkar 1998). Next, it included an analysis of human diversity. I will treat this work in some detail below. In the wake of the inititation of the Human Genome Project, Lewontin challenged the science behind genomics-based forensics. With Hartl, he disputed the claim that DNA fingerprinting can identify a suspect with a negligible probability of error (Hartl and Lewontin 1993). It drew the ire of U.S. federal prosecutors but Lewontin held firm.

In tandem with this technical work, Lewontin produced philosophical interpretations of biology to a greater extent than any biologist since J. B. S. Haldane. Much of this work was eventually jointly published in book

form with Richard Levins even if it had been initially authored by only one of them. It is not at all controversial to say that no biologist has influenced the philosophy of biology to a greater extent than Lewontin. I turn to that work next.

THE INTERPENETRATION OF SCIENCE AND POLITICS

In my case, for a biologist and philosopher coming of scientific age in the late 1970s and early 1980s, Lewontin was a towering figure not just because of his science but because his public commitment to social justice led him to draw out the ideological implication s of his and others' work including the scientific critiques he provided of genetics and evolutionary biology. At Columbia University, where I was an undergraduate, we were proud to have him as an alumnus.

Many of us worked our way through his dissection of heritability analysis and its irrelevance to the problem of establishing genetic etiology. In the 1970s, an explosion of scientific racism erupted thanks to the work of Jensen (1969). On the basis of a supposedly high heritability value for IQ, Jensen claimed that IQ (which was supposed to measure intelligence—a claim that I have recently critiqued following a path laid out by a large cadre of professional psychologists [Sarkar 2021a]), was genetically determined. Moreover, lower IQs were associated with being Black in the United States. The supposed implication was that Blacks were socially and economically situated where they were because of bad genes, not the pervasive racism of U.S. society.

Lewontin's (e.g., 1970a, b) response constituted a penetrating rebuttal not only of Jensen's scientific racism of but also the versions propounded by figures such as Schockley, and, in a more subtle form, found in the human sociobiology of Wilson (1975). The response was two-pronged. First, in a superb piece of epistemological analysis he showed that the analysis of variance (on which heritability analysis was based) was *not* an analysis of causes (Lewontin 1974a). He then showed why heritability analysis, by its very nature, cannot establish genetic causation (Feldman and Lewontin 1975). This work complemented that of Layzer (1974), Kempthorne (1978), and, slightly later, Jacquard (1983); heritability analysis has never quite recovered its scientific mystique since then among biologists (Sarkar 1998) though psychologists sometimes seem blissfully unaware of the seriousness of its problems (see, e.g., Plomin and von Stumm 2018).

Second, Lewontin laid down the foundation for the genetic analysis of human genetic (and genomic) diversity within and between groups that are resplendent in their cultural diversity in spite of biological homogeneity. He showed that genetic diversity within human so-called races is as great as diversity between races (Lewontin 1972). He took these insights to a popular audience in book form, originally arguing at the level of protein variation (Lewontin 1982) but later at the level of DNA variation when sequences began to become available (Lewontin 1995). The foundation that he established is periodically under attack by new avatars of a desperate scientific racism but, now as in Lewontin's time, these attacks reflect the structural disparities of social power rather than any credible biology.

In the joint work of Lewontin and Levins there are many other contributions of politically committed science especially in ecology, agriculture, and epidemiology all of which were central to Levins' career. An analysis of these will be left for another occasion. Suffice it here to note that it is simply a mistake to regard this work as somehow being politically debauched science in the sense of the work of Jensen or, later, in Herrnstein and Murray's (1994) less than successful attempt to revive scientific racism. Intellectual standards were never compromised. Rather, commitment guided the choice of problems for analysis. It is hard to imagine a worthier approach to science event though I do not intend to disparage the work of those who prefer the safety of the ivory tower.

PERSONAL RECOLLECTIONS

I first met Lewontin in 1982, through our mutual connection with Bill Wimsatt of the University of Chicago. Wimsatt had been a post-doc with Lewontin in the early 1970s at the university; in the early 1980s he was one of my graduate advisers. In 1982 Lewontin had visited the University of Chicago to speak at an event to mark the centenary of Darwin's death. Tellingly, while this was a lavish occasion at that university, the historians of science there (some of whom, such as Robert Richards, were notoriously unsympathetic to progressive causes) did nothing to mark the perhaps more important occasion a year later, the centenary of Marx's death. In later years, Lewontin and I were bemused by that fact though neither of us was particularly surprised.

In the early 1990s I spent two years partly in Lewontin's lab at Harvard while being on the faculty at Boston University and then a fellow at the Dibner Institute at MIT. Technically I was a post-doc at Harvard. The noted

historian of eugenics, Diane Paul, and I shared an office where I had inherited a desk from the eminent geneticist and historian of biology, Raphael Falk, who has also recently died (Sarkar 2021b). Of the other lab members, I had scintillating conversations with Lloyd Demetrius. When nothing else interfered I went over to Steve Gould's office close by typically to discuss baseball. Paul introduced me to Ute Deichmann who had written a brilliant history of German biologists under Hitler. Short-term visitors to the lab who had once worked with Lewontin included Philip Kitcher and Bill Wimsatt. Both Wimsatt and I encountered Dan Hartl in Lewontin's office during the period he was being recrutied to Harvard. We were encouraged to work on three-locus models of selection; neither of us followed through.

Lewontin only came in about once a week, typically on the day that the lab had a seminar (which was often Wednesday), and those were the occasions for most of my interactions with him. The seminars had a university-wide reputation. On one later occasion when I was speaking on heritability (in 1995), even Ernst Mayr, who had few intellectual sympathies with either Lewontin or me, showed up for the seminar (Sarkar 2005). Mayr did not say a word which was rare for him. Later he was surprised that Lewontin had not disagreed in spite of my deviation from the work on heritability with Feldman (Feldman and Lewontin 1975). However, Lewontin sometimes attended events organized by the Boston Center for the Philosophy of Science, including its once-famous Colloquium, and those were occasions for more personal conversations. At the Boston Colloquium, Gould and Lewontin were both commentators on my presentations (in 1990 and 1991, respectively). Levins was occasionally present at those events.

Lewontin provided me an introduction to another geneticist, Jon Beckwith, who was one of the organizers of a genetic discrimination study group that had been spawned by the Science for the People movement. Discussions in that group helped develop the critiques of the Human Genome Project (HGP) that Fred Tauber and I published early in the 1990s (Sarkar and Tauber 1991; Tauber and Sarkar 1992; 1993). Because he then lived in Vermont, Lewontin did not attend meetings of that group. Though he was equally critical of the HGP, he would not be drawn into discussions about whether it was a waste of money pointing out that any money spent on science was preferable to the other uses it was likely to be put by the United States government.

My most vivid intellectual memories of Lewontin come from a graduate seminar in the philosophy of biology organized in Spring 1991 by Harvard philosophy faculty, Amartya Sen and Robert Nozick. The participants included Gould, Falk, and Lewontin besides me. Faculty sat in an inner circle with the organizers; students were relegated to an outer circle. Faculty talked interminably and disagreements were severe; I don't recall any student ever saying anything. It was undoubtedly a peculiar way to teach a seminar. But there were bright moments. Sen took joy in pointing out how often Nozick, a committed libertarian, inadvertently invoked Marx. One occasion was particularly telling. At stake were properties of groups the could not be reduced to those of its individual members. The example being discussed was the cardinality of a set, say, n (Sarkar 2008). Nozick countered that an individual could have the property of being a member of a set with cardinality n. Sen pointed out that the observation went back to Marx in the *Grundrisse*.

Lewontin dominated that seminar and Falk and I often met later over coffee to mull over his remarks. Arguing with Lewontin about the units of selection during that seminar led to my perspective that genes versus organisms was a false dichotomy in disputes over the units of selection. In his influential 1970 paper on the units of selection Lewontin (1970c) had pointed out the potential conflicts between levels of selection. But, within the organism, he had not mentioned genes; rather he had mentioned gametes. From my perspective, genes and gametes belonged to different hierarchies of life (Sarkar 1994, 1998). Genes were abstract entities; gametes were phsyical components of organism.

I am not sure that I ever convinced him of a view that I associate with Williams (1985): that the organism could be the unit of selection *and* that selection could favor one gene (allele) at the same time. But he listened to what I had to say, and that is the part of him that made him such a positive influence on the philosophy of biology. He was also silent in response to my claims and that alone gave me confidence that I was not entirely wrong. In later years our views diverged even more. I had always been sympathetic to the neutral theory of evolution, but I eventually endorsed a much more radical rejection of natural selection as a major cause of evolution (Sarkar 2015) than Lewontin ever accepted. But he always listened.

OF ENGELS AND MARXISM

Lewontin and Levins were publicly committed Marxists; in my view Gould was, at most, a fellow traveler. Lewontin and Levins implicitly suggested that they were also committed dialectical materialists almost in the old-fashioned Soviet doctrinaire sense. But close examination of their work reveals much more subtlety, in fact, more subtlety than, say, in Haldane's Marxist writings. They also succumbed to the allure of Mao's Cultural Revolution but that infatuation did not last. All three figures were explicit of their debt to Engels. Of them, again in my view, Gould used Engels' arguments in more detail than Lewontin and Levins but that is also a discussion that will be left for another occasion.

For Lewontin and Levins, the primary source for their theoretical framework is the 1985 book, *The Dialectical Biologist*, which is dedicated to Engels (Lewontin and Levins 1985). The book is a collection of articles published earlier elsewhere, mostly coauthored by them. However, there is a concluding chapter explicitly on dialectics that will be my focus here. Another important source is a much later compilation, *Biology under the Influence* (Lewontin and Levins 2007).

In this work we do not find any doctrinaire invocation of *diamat* laws even though the transformation of quantity into quality is taken for granted in their work. More importantly, and this comes out very vividly in their discussions of agriculture and epidemiology, they show convincingly the social roots of problems. If they were active today, they would emphasize the social causes of pandemics. There is an obvious sense that our ongoing pandemic is the result of the SARS-CoV-2 virus. But this obvious sense is also trivial.

It is a mistake to call SARS-CoV-2 the *cause* of the Covid-19 pandemic. SARS-CoV-2 has probably existed for generations without a pandemic. Rather, at the very least, we must invoke the volume, intensity, and particularities of travel to explain why there is a *pandemic*, the spread of infectious disease across the world. Globalization was already predicted and criticized by Marx and Engels in *The Communist Manifesto*. It is the most important outcome of late capitalism in the pandemic context; without it, there would be no pandemic. Only because China's state capitalism has successfully made it the world's factory could an infection introduced to humans, most likely in Wuhan, spread as rapidly as it did to the farthest corners of Earth. The volume of travel from and through Wuhan has been of critical importance to the emergence of Covid-19; travels volumes elsewhere generated its explosive spread. Patterns of trade in a globalized

world explained this volume, not the biology of SARS-CoV-2. Similarly, the containment of the pandemic requires social choices, most importantly, isolation measures and social vaccine mandates, all in turn more dependent on politics than on biology. (Here, I am indulging in some Levins-style reasoning found in *Biology under the Influence*.)

The social determination of disease emergence underscores the complexity of what we new call social-ecological systems, a theme that goes back to Lewontin and Levins (and especially the latter) in the 1980s. In the chapter on dialectics in *The Dialectical Biologist*, they emphasize the limitations of reductionism in biology in spite of its apparent success in molecular biology. (This was also a major theme of Lewontin's 1992 lecture at the University of Chicago when I first heard him speak). With Engels they seemed to accept that matter exhibits novel phenomena at each succeeding level of organization. While I know of no source in which Lewontin and Levins explicitly invoke Engels in this way, its consequent critique of reductionism was a pervasive theme of their view of dialectics. Here they followed a tradition introduced several generations earlier, most notably by Haldane (1939) in *The Marxist Philosophy and the Sciences*.

Though I have defended reductionism *within* molecular biology, there is no question that Lewontin and Levins' dialectical point is correct: there is much more to biology than molecular biology as, for instance, our singular failure to cure diseases at the molecular level indicates. (Lewontin made this point multiple times in the essays collected in *Biology under the Influence*.) As Lewontin showed in the 1960s, it is possible to both accept and exploit the power of molecular biology and remain open to complexit and dialectics in the study of organism and environment (Gilbert and Sarkar 2000).

FINAL REMARKS

As we move forward we will miss Lewontin's towering intellect in biology. But, even more than that, we will miss his understanding of science as an ideological construct reflecting social relations, as much part of the superstructure as forming the base. Keeping that understanding alive, reminding ourselves that science is valuable primarily as a potential tool for liberation would be the best way to honor Lewontin. Science for the People, which had become defunct in the 1980s, was revived in 2014 and that is one place where his legacy, as also in the pages of this journal, will continue to be extended. Science and society will both be better for it.

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