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REVIEW

Biology as Politics: The Direct and Indirect Effects of Lewontin and Levins

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ABSTRACT *Lewontin and Levins's contributions are viewed from four angles: a more vigorous culture of science criticism; a visible college of Marxist scientists in the USA; inquiries into the diverse social influences shaping science; and motivating readers who want to pursue their science as a political project. Indirect contributions—influences on and appropriations by other actors in the wider realm of biology as politics—are discussed as well as the more direct effects.*

KEY WORDS: Dialectics, Marxism, biology, ecology, agriculture, health

Biology Under the Influence: Dialectical Essays on Ecology, Agriculture, and Health, by Richard Lewontin and Richard Levins, Monthly Review Press, 2007, 400 pp., $22.95.

In 'A Program for Biology', one of this collection's 31 essays, the Marxist biologists Richard Lewontin and Richard Levins (hereon: L&L) list recent 'big mistakes' in scientific approaches to complex phenomena: 'the green revolution, the epidemiological transition [from infectious to chronic diseases], sociobiology, the reification of intelligence testing, and the current fetishism of the genome'. They attribute such mistakes to the 'posing [of] problems too narrowly, treating what is variable as if it were constant and even universal, and offering answers on a single level only' (p. 81). What they point to is not simply the 'philosophical

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tradition of reductionism', but also 'the institutional fragmentation of research, and the political economy of knowledge as a commodity' (p. 9). Indeed, their critical position extends beyond science to rejection of 'the greed and brutality and smugness of late capitalism' (p. 373).

Their anti-capitalist stance notwithstanding, the foci or starting points of L&L's essays, like their 1986 collection, *The Dialectical Biologist*, lie in research in the life sciences. Regarding the green revolution, for example, L&L see:

> . . . that a view based on unidirectional causation leads to the expectation that since grasses need nitrogen, a genotype that takes up more nitrogen would be more productive; since pesticides kill pests, their widespread use would protect crops; and since people eat food, increased yields would alleviate hunger (p. 84).

The actual outcomes did not end up matching such simple causation because:

> . . . the increase in wheat yield was partly achieved by breeding for dwarf plants that are more vulnerable to weeds and to flooding; the killing of pests was accompanied by the killing of their natural enemies, their replacement by other pests, and the evolution of pesticide resistance. The successful yield increases encouraged the diversion of land from legumes. The technical packages of fertilizers, pesticides, irrigation, and mechanization promoted class differentiation in the countryside and displacement of peasants (p. 84).

'A Program for Biology' ends with three fundamental questions for the study of complexity:

> Why are things the way they are instead of a little bit different (the question of homeostasis, self-regulation, and stability)? Why are things the way they are instead of very different (the question of evolution, history, and development)? And what is the relevance to the rest of the world? (p. 86).

The third question, rephrased in a later essay as 'how [do we] intervene in these complex processes to make things better for us?' (p. 115), invites *Science as Culture* readers to ask what L&L's essays tell us about having an effect—direct or indirect—on the complex processes of the production and application of scientific knowledge. This essay review approaches this third question as it relates to social studies of science and technology and L&L's contributions from four angles: a more vigorous culture of science criticism; a visible college of Marxist scientists in the USA; inquiries into the diverse social influences shaping science; and motivating readers who want to pursue their science as a political project. Indirect contributions—influences on and appropriations by other actors in the wider realm of biology as politics—are discussed as well as the more direct effects.

Towards a Culture of Science Criticism

Fifty years after C. P. Snow's essay, laments can still be heard over the divide between the 'two cultures'. However, science popularization through newspaper journalism, television documentaries and book publishing that has flourished since the early 1980s means that many non-scientists are paying closer attention to science. (The posterchild here is Hawking's *A Brief History of Time*—no easy read—of which an estimated 10 million plus copies have been sold.) Not confined within academic disciplines, people from diverse sectors of society make sense of their experience in terms drawn from what they hear and read about science. In short, science is very much part of our culture.

Now, L&L's collection is not designed for mass marketing, but their essays can be seen as fitting somewhere in this terrain. Their writing is challenging to biologists but still accessible to interested non-scientists. Readers will be stimulated—or provoked—to think about a wide variety of themes: the lack of historical contingency in chaos, catastrophe, and complexity theories; the role of social disruptions in infectious disease outbreaks; the need to understand the causes and implications of variations around averages as well as of averages; randomness as an intersecting of multiple determining forces, not the absence of causation; the different ways that the biological and social codetermine what is possible (think, for example, of how human beings are able to fly); the implicit theoretical assumptions in crude attempts to detect life on Mars; the vigilance or skepticism needed when using technological metaphors, such as the computer program, for explaining living processes; assumptions about causes and applications built into statistical analysis; Schmalhausen's law that beyond some limit of buffering or canalization, small changes may have dramatic effects; and many more.

Each theme, as it is played out by L&L, provides food to chew on. Indeed, once digested, L&L's themes might make it hard for readers to keep swallowing anodyne formulations, such as, common medical conditions 'are all determined partly by multiple genes, and partly by environmental factors' (Imperial College, 2002). Just what, L&L would have us ask, can such separation or partitioning of determining factors mean? After all, no individual human can fly using its own biological capacities, but organized as a society we can use our minds and technologies to overcome our constraints. What then is biological and what is environmental in such an example? And, if it is hard to answer that question, what does it mean to speak of biological and environmental interacting? The same questions can be asked of the etiology and treatment of any disease.

If not all of L&L's readers are led to reject conventional perspectives, at the very least L&L provide them alternatives that allow for critical thinking in the sense of 'placing established facts, theories, and practices in tension with alternatives' (Taylor, 2008, p. 160). 'Science criticism', unlike art and literary criticism,

is not a widely accepted enterprise in our culture, but that would be an apt label for these essays.

A Visible College of Marxist Scientists and Critics

L&L point such science criticism towards social interpretation of science when their introduction acknowledges science's 'dual nature': an internal logic of knowledge development combined with a function as 'the increasingly commodified specific product of a capitalist knowledge industry' (p. 8). L&L's take on science recalls historian Gary Werskey's 'visible college' of Marxist scientists in Britain in the 1930s: Bernal, Haldane, Needham, Blackett, Levy and others (see Werskey, 2007). L&L—together with Barry Commoner (Fuller, 2007) and Hilary and Steven Rose—could be labeled as a second generation of Marxist scientists. Following Hamlin (2007), such a label would invite us to pay attention to the changing contexts—historical and national—in which such scientists have tried to express their dual commitments—to science and to political change. There is an interesting historical project here for someone to take up, especially in contrasting how Levins, Lewontin, and Commoner made their way in the anti-communist post-war decades in the USA and subsequently played out or downplayed their Marxism in their writing about science and society. This review (and this reviewer) won't take on that task, other than to note that the book's closing autobiographical sketch by Levins provides an entry point. 'When I was a boy', he recalls, 'I always assumed that I would grow up to be both a scientist and a Red' (p. 363).

The title of this last essay—'Living the 11th thesis'—highlights the question of how one goes about changing what one studies and interprets. In this vein let me note that Werskey's (2007) choice for the second generation of the Marxist critique of capitalist science was not scientists, but interpreters of science in its social relations; not his American compatriots (L&L and Commoner), but the British Radical Science movement centered around Bob Young and the *Radical Science Journal* (hereon: RSJ; which metamorphosed into *Science as Culture* in the 1980s). The basis of Werskey's choice, he acknowledges, is autobiographical; following his lead in that regard, let me inject some relevant personal background into this review.

I left Australia in the late 1970s with the idea of studying informally with biologists whose work on complexity in ecology, evolution, and development interested me politically as well as intellectually (Taylor, 2005, p. xvi ff). I eventually ended up in the USA to undertake a PhD with Levins and Lewontin, but spent some time first eking out a living in England. During that time I participated in the 'Dialectics of Biology' conference in Bressanone (Rose, 1982a, 1982b) and the monthly RSJ meetings. I especially recall one RSJ meeting in which a working group reported on their inquiries and introspections about why social change was so difficult at a personal level—their domestic and political

collectives and open relationships seemed to have generated many failures and 'psychopathologies of left-wing groups'. Soon afterwards RSJ piloted and then spun off *Free Associations*, a new journal on 'psychotherapy, groups, politics, institutions, culture'. The journal is very active to this day and psychotherapy, more than Marxist critique of capitalist science, has become the center of Bob Young's work. While in England I also spent time with scientists who had worked in RSJ but then moved away when they felt the critique of the capitalist social relations and labor process in science left no room for them to be scientists. The challenge that I took with me, then, as I headed west across the Atlantic, was not only to follow L&L's lead in pursuing one's science as a political project, but to do so by building an understanding of the social and psychological dynamics of changing science in ways that did not lead to the one-or-the-other experience of the English ex-RSJ scientists.

Certainly there is no single-level answer to that challenge! As I explored various dimensions of it in the ensuing years, I have found L&L's work contributing more to my critical thinking about the life sciences than to my analyses of the social and psychological dynamics of changing science [analyses that have turned out to emphasize the complexity of resources or practical commitments involved in knowledge construction in any particular area (Taylor, 1986, 2005, 2009a, 2009b)]. This personal experience may, I think, be matched by readers' experience of the essays in this collection. Indeed, Lewontin's advice when I arrived in the USA eager to pursue science as politics was to build my scientific career and from that basis exert an influence. Correspondingly, the politics that comes through most strongly in these essays is one of solidarity with movements to resist oppression and exploitation, for whom L&L's status as scientists is a valuable asset. What is not so obvious is that L&L's politics is also one of considerable generosity to students and colleagues, even those who may have discounted their advice or moved away from the collectives they brought them into.

If one tracked the contributions of L&L's former students and colleagues in groups such as the Sociobiology Study Group of Science for the People, the Marine Ecosystems Research Group, the New World Agriculture Group, and the Working Group on Emerging and Re-emerging Infectious Diseases, a third generation of Marxist or radical scientists would be quickly visible. A supplement then to the historical project suggested above would be to analyze the changing contexts faced by those who followed L&L (or Commoner), and to include, in acknowledgement of the direction Bob Young has taken, some probing of the diverse subjectivities and individual biographies. Again this review and reviewer won't take on that task. [My recommended starting point, however, would be the 1995 essay of Marxist geographer, David Harvey, on Raymond Williams and the tension between solidarities forged through working and living together in particular places and the application of abstractions, perspectives, and resources drawn from beyond the local and particular (see Taylor, 2005, p. 207ff).]

Reciprocal Animation between Science Criticism and Social Contextualization

If we view as direct effects of L&L's essays science criticism and visibility of Marxist or radical scientists and critics, we can also identify indirect effects (or influences and appropriations). Critical examination of concepts and methods within any given episode or current of the life and environmental sciences—which L&L provide in abundance—can stimulate readers' and students' inquiries into the diverse social influences shaping that science. Social contextualization can, in turn, suggest alternative lines of scientific investigation. This two-way interaction or 'reciprocal animation' between science and social contextualization of science can enlarge significantly the sources of ideas about what else could be or could have been in science and in society (Taylor, 2009b). For example, Lewontin is cited by many for making clear that genetic does not mean unchangeable. In the human sphere, this means that reports of high heritability of IQ test scores do *not* imply that persistent differences between average scores for different racial or ethnic groups (as such groups are defined in the USA) have to be necessarily so. Once that point is appreciated, we might go on to look at what has been learned from efforts to boost IQ test scores and analyses of those efforts (e.g. Woodhead, 1988; Ou, 2005), as well as from research attempting to explain the large gains in average IQ test score between generations (Dickens & Flynn, 2001). What kinds of interventions, observations and analytic skills are involved? Where is such work taken up and by whom? Who funds them? What has been done with the results (in terms of improving such efforts, policy, media publicity, building of careers etc.)?

In two senses indirect effects is an apt description of the preceding questions of social interpretation of science. Although L&L would surely have interesting observations and hypotheses to offer, their essays do not delve far in that direction—it is left for readers to take up such inquiries. Moreover, if readers do so, they will have to go beyond L&L's broadbrush themes about capitalist knowledge industry (as well as beyond much of mainstream science and technology studies). For, if the point is to change the science, readers have to address diverse practical considerations of their own particular position as they attempt to influence the social organization of research so as to address better the questions opened up by critical examination of concepts and methods (Taylor, 2005, p. 93ff).

Continuing Conversations about Biology with Political Implications

An even less direct effect of L&L's essays is to stimulate further conversations about complexities in ecology, agriculture, health, and other areas of the life sciences. Participants in such conversations who want to pursue their science as a political project have to explore 'off-stage' just how to make opportunities for, and deal with obstacles to, alternative processes of the production and

application of scientific knowledge. (Social and power relations would, of course, be front and center of such off-stage explorations.) In this vein, namely, promoting conversations about complexities but not taking up any political project implied by the conversations, let me discuss two sets of issues about interpreting and changing heterogeneous complexity stimulated by, but taking us beyond, L&L's essays.

Development and Evolution

Development is a key aspect of dialectics. L&L observe that biological development involves: *historical contingency* (so we shouldn't expect to detect life on other planets with methods based on the processes of life as it evolved on earth; p. 47ff; see also p. 227ff); *noise* (fruit flies differ in unsystematic ways in the numbers of bristles they have on their left and right sides; p. 243); and *thresholds* (small differences in an environmental variable may have no effect within the normal range and major effects outside that range; p. 76ff). Because development involves structuring of living materials and their ongoing restructuring it has many other features that are not only fascinating biologically, but also thought-provoking for the dialectical materialist project of emphasizing connectedness, integration of levels of analysis, and '"things" as snapshots of processes' (p. 10).

Consider, for example, gastrulation both as an embryological phenomenon and a metaphor [drawing here from Taylor (2009b, p. 450)]:

> The original ball of cells in an embryo folds into itself and the body plan of the organism-to-be emerges. At the end of gastrulation many of the outside cells have given rise to inside cells. The subsequent fate in development of both kinds of cells depends on their interaction.

Taken as a metaphor, gastrulation 'combines restructuring over time with connections across particular, place-centered capacities'. Teasing out the metaphorical associations further,

> context is not an add-on to the essential thing . . . but rather the thing only takes its properties . . . through its relation with surrounding layers. Moreover, the relationship between layers has an indirect or surprising history. Inside and outside cells that are now neighbors arose from cells some distance apart in the earlier embryo.

[See also the discussion of intersecting processes in Taylor (2005, p. 159ff, 2009b).]

The fact that characters of organisms have to be reconstructed through development every generation also has implications for thinking about biological evolution in ways that leave behind the constraining dichotomies L&L identify

(p. 23). For example, take Conrad Waddington's idea of genetic assimilation, which L&L discuss as the lowering, through evolution, of some threshold so that a trait that had previously required an environmental stress to appear (e.g. calluses on our hands resulting from physical labor) arises in advance of that stress (e.g. ostriches hatch with calluses where they will rub against the ground). In Waddington's words, the environmental induction has become genetically assimilated (p. 76). Of course, there is nothing in the phenomenon that says genes or genetic switches have taken the role of the environment; all that we know is that developmental processes generate the trait before the external stress is experienced. Once we focus on developmental processes, we can envisage more active or agent-like organisms in evolution in place of organisms that are passively, naturally selected. To illustrate the difference in perspective this could produce, let me use an example discussed in [Taylor 1998; see Gilbert and Epel (2009) for more discussion of ecology, development, evolution].

A goat described by the Dutch anatomist, Everhard Slijper (1942) was born without front legs, but managed to hop round the pasture on its hindlimbs alone. After its death, Slijper dissected it and discovered an S-shaped spine, similar to ours, thickened bones, modified muscle insertions, and other correlates of moving on two legs (Figure 1). It would be very strained to argue that goats have been selected in the past to exhibit these changes when they are born without forelimbs. Instead the goat demonstrated a potentiality of the mammalian muscular–skeletal system to develop during an individual's lifetime so as to balance gravitational and other forces. Moreover, this developmental potentiality pre-existed the condition (an environmental chemical or a mutation) that revealed that potentiality.

Of course, the bipedal goat grew less vigorously than its quadrupedal relatives and would have been unlikely to survive if not cared for by humans. We do not have to think that new evolutionary lineages originate with such drastic alterations. We can, however, imagine some less extreme congenital change, say, slightly shortened forelimbs, that also elicited compensatory muscular–skeletal development and locomotory changes, and resulted in new life habits or enhanced old ones, such as feeding from the growing tips of bushes. Moreover, the goats

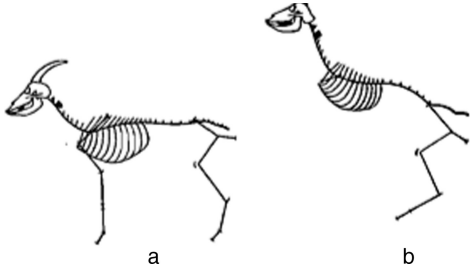

Figure 1. Schematic figures of the skeletons of (a) normal and (b) bipedal goats. *Source*: from Slijper (1942, p. 289), with permission of the Royal Netherlands Academy of Arts and Sciences.

need not compensate in the same way (i.e. through identical developmental pathways) or equally appropriately to the environmental conditions (picking up on themes from 'Genes, Environment, and Organisms'; p. 221ff). Over generations, matings among those that compensated appropriately enough to survive leave offspring possessing more of the various developmental pathways or potentialities. We should not be surprised, then, if some of them showed the trait without the lifetime of compensatory development. In other words, rather than assume *genetic* assimilation, environmentally induced traits can be *developmentally* assimilated.

If evolutionary change can arise from developmental flexibility like that revealed by Slijper's goat, evolutionary thinking needs to include the developmental processes, not just the differential representation over time of some isolated, static traits. This would be akin to dialectical thinkers allowing for both change and continuity (p. 43ff) as they theorized how social agents live and work within structured social arrangements and in doing so reproduce such structures but always imperfectly.

Hidden and Heterogeneous Variables

A second set of issues about interpreting and changing heterogeneous complexity stimulated by L&L's work concerns the hidden complexity in simple models and confounding complexities when we try to pay more attention to heterogeneous complexity than is conventionally the case. Here I am picking up on the use of graphs of feedback relations, developed by Levins, to draw implications based on the *sign*, not the *size or other details*, of the feedbacks ('loop analysis'; p. 189ff). This qualitative analysis is especially effective in showing that quite different outcomes arise when variables treated as 'external' to the system are brought into the picture. For example, a model of the physiology of anxiety and stress for a person changes if that person is a worker and the overseer's actions are brought into the picture. It changes again if the worker's union is brought in (p. 193ff; Figure 2).

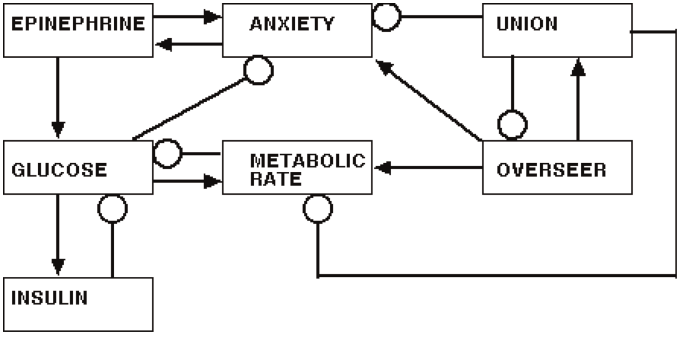

Figure 2. A psychological network in the context of social relations. *Source*: Lewontin and Levins (2007, p. 195).

I have found that, ironically, the methods of loop analysis are sensitive to the dynamics of other external variables not yet brought into the picture—*hidden* variables. For example, if two species share the same resource, we would intuitively think of them as competitors and in the loop analysis graph they would be given a negative effect on each other. However, if the resource is a prey species that has dynamics of its own, it turns out that the feedback relationship that best fits the two competitors (i.e. that incorporates the indirect effects of the omitted variables) need not be the intuitive one—it could be like a predator and prey. Whether or not this is the case depends on the *details*, not only the *sign*, of the relationship with the omitted or hidden variable (Taylor, 2005, p. 17ff).

Of course, no problem arises when the omitted variables are effectively constant or not linked in any way to the variables in our picture. This independence of variables can be achieved if the system can be engineered so that it is well bounded and so that influences across the boundaries are simple inputs, such as electrical current flowing through a computer microchip. [L&L suggest that 'conservative ideology' advocates a focus on 'narrowly circumscribed objects and accepting [of] boundary conditions' (p. 94), but I would suggest that we all rely in work and life on engineering of controlled systems.] However, when well-bounded systems cannot be engineered, as is usually the case for situations of interest to ecologists and social scientists, there will be variables not immediately in focus that have their own dynamics. Anyone playing with qualitative or mental models in such fields should be troubled by the problem of hidden variables and the hidden complexity of (seemingly) simple models. (This potentially profound problem should be added to the other putative differences between natural and social sciences that L&L skeptically review; p. 39ff.)

An analogous issue arises if we pay attention to heterogeneity *within* variables, replacing, say, the population in a country with classes having unequal access to political, economic, agricultural, and other resources and having dynamic interactions with other classes. L&L take a step towards addressing heterogeneity when they discuss the relation between averages and variation among those averages (p. 23ff). They note that a left-wing response to the urban riots of the 1960s would be that the average conditions had deteriorated enough that, although there was variation in people's responses, the number willing to rebel exceeded a threshold and riots occurred. The right-wing response was that, because many did not riot, the riot must have been the 'work of a small group [that] had a biological predisposition to riot' (p. 25). L&L's point, which implies that we should focus on improving the average conditions, invites further discussion.

Notice first that action based on averages has affinities with action based on *types*. Now, typological thinking is not necessarily illegitimate (and it is hard to avoid; e.g. L&L state that 'regulation of heart function is different in working-class and middle-class teenagers'; p. 62). As I note in Taylor (2009c), we can readily imagine, for example, a comparison of the dental health of two communities that are similar except that the one with better average dental health has

naturally high level of fluorides in its water supply. If the variation around the averages is small relative to the differences in the two averages, it seems reasonable to prescribe fluoridization of water supplies lacking natural fluoride. In doing so—or supporting state authorities that do so—we discount the variation around the average and other deviations from type, such as teeth discoloration that occurs in some individuals. We discount the variation because the benefits exceed the costs when summed up for the community. Public health policymakers are able to do this as long as the individuals who bear disproportionate cost do not effectively mobilize resources and allies to resist.

But this line of thinking has its complications, especially from a political perspective of social versus individualized interventions. For instance, suppose now that two 'racial' groups show persistent differences on average in some scholastic achievement tests. By analogy with the fluoride case, we should ascribe the difference to race, that is, to some social or biological variable or variables that differ from one race to the other. Granted, identifying those variables won't be as simple as noting the presence or absence of fluoride, but should we even seek to find them? What if we were to succeed?—If the variable were unchangeable (say, a matter of possessing a genetic sequence or rearrangement), would we resign ourselves to the difference? If the variables were biologically or socially changeable, would we administer the same 'antidote' to *every* person in the lower-achieving group?

It would be consistent with the fluoridization case to say yes to this last question. We would, however, be discounting the possibility that many subsets of one racial group might share more biological and social factors influencing the development of their scholastic achievement test score with subsets of the other group than they do with other members of their own group. Yet, the best way to improve the average test score of the group with a lower average might be to explore the particular combinations of factors underlying the development of test scores for the various different subsets of the two groups, not to look for and act upon the two groups as distinct *wholes*. Indeed, this approach would counteract racial stereotyping, which builds on the assumption that factors shared within racial groups predominate over the particular combinations of factors for subsets of individuals within and across groups.

But then, if the point is to change the world, and we try to move beyond typological thinking about race in education, we face even more complications. Ironically, by contributing to the routine treatment of individuals in US society according to their racial group membership, stereotyping may well have *generated* some important factors that *are* shared within each group and not across. On the other hand, if we try to shift the focus from group membership to paying attention to diverse pathways of scholastic development, we risk bolstering the fiction that racial group membership no longer brings social costs or benefits—or, at least, that racial categorization should not be used in policy as an indicator of whose development has been hindered or enhanced by those costs or benefits. Such

are the confounding complexities when we try to pay more attention to heterogeneous complexity. If, as L&L advocate, we dispose of the idea of race in human genetics and instead use information about a patient's particular ancestry, do we really want to get involved with variation, heterogeneity of development, and educational change without making reference to past and future data that are based on racial classifications?

Conclusion

For *Science as Culture* readers, L&L's collection of essays has the direct effect of contributing, as does the wider field of science and technology studies, to a more vigorous culture of science criticism and of highlighting the vision and challenges of a visible college of Marxist scientists in the USA (and the next generation of their students). Less directly, their critical examination of concepts and methods in the life and environmental sciences opens up inquiries into the diverse social influences shaping science. By stimulating further conversations about complexities in ecology, agriculture, health, and other areas of the life sciences, they motivate readers who want to pursue their science as a political project to explore for themselves ways to make opportunities for, and deal with obstacles to, alternative processes of the production and application of scientific knowledge. The challenge that L&L's essays pursue and reinforce is that of disciplining without suppressing the heterogeneous complexity of developing structures in biological and political systems. To tackle this challenge, we could hardly do better than to engage with biology under the influence of Lewontin and Levins. **Q1**

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