The Illusory Riches of Sober's Monism

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frequency of some gene in the gene pool" (339). They claim that Dawkins advances a second thesis, however, one that is supposed to be biologically important to the units-of-selection problem; this is the idea that "barring complications, the average ability of the genes in the gene pool to leave copies of themselves increases with time" (340). My view is that this second thesis is more or less beside the point.

When group selection manages to increase the global frequency of the a gene, it will be true that the average fitness of genes in the global population increases. The average fitness of the alleles in the population (where these are perfectly correlated with their respective phenotypes) is depicted in the above figure by $\bar{w}$. On the other hand, when purely individual or genic selection within a single cohesive population eliminates both the A phenotype and the a allele, $\bar{w}$ will decline. Group selection can lead the average fitness of genes to increase and genic selection can lead the average fitness of genes to decline. How often this quantity goes up or down has little to do with what the units of selection are.[17]

In my book, I claimed that it often is useful to represent selection processes in a genic vocabulary. This technique has generated some confusions; it also has provided a number of important insights. But the utility of a mode of representation is not what the units-of-selection problem has been about. It is about the levels at which adaptations have accumulated. The pluralism of Sterelny and Kitcher confuses these two questions, in much the same way that Williams and Dawkins did earlier.

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B iologists have discovered that a wide variety of population-genetics models apply to cases of natural selection in the wild. The outcome of selection may depend on the number

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[17] Also, I am not aware that Dawkins has ever taken a stand on whether the average fitness of genes in a population typically increases. In *The Extended Phenotype* (p. 193), he mentions in passing that "many population geneticists are, for reasons best known to themselves, very interested in another quantity which is called the mean fitness of a population."

of loci, linkage among the loci, the number of alleles at a locus, whether or not selection is frequency-dependent, the relations among fitness parameters, and so forth. Some biologists want to make a further distinction among types of selection. They think that selection sometimes acts on the gene, sometimes on the organism, and sometimes on the group. They assume that, for each selection episode, there is a unique account that will identify the level of selection. Elliott Sober[1] hopes to legitimate this discourse and to explicate it in a way that will enable the biologists who like it to settle their differences empirically. We believe that asking about the real unit of selection is an exercise in muddled metaphysics.

Sober outlines an example that is intended to embarrass pluralism. Imagine a haploid plant species in which bearers of the A allele manufacture and leach into the soil a substance that benefits them and their neighbors, while bearers of the S allele do not. If one assumes that like plants associate, then a little algebra reveals that the A allele can be favored by selection. Imagine that Sober's example were expanded to a genuine model in which conditions were imposed to require that the associations would be stable across generations. Then we would indeed have a case that could be described from any of three perspectives: there would be group-level, organism-level, and genic-level descriptions in the manner of his statements (i)–(iii) (*ibid.,* p. 156). Biologists, we imagine, would agree on the algebra. We claim that there is no point in fighting about the "correct" interpretation. None of the descriptions should be singled out as the correct analysis that exposes the "true unit of selection."

Sober is opposed to this ecumenical conclusion because he thinks that the discovery of an example of this type would vindicate those who have been contending for group selection. It would. For such a discovery would show something not known in advance, namely, that some episodes can be adequately described by supposing that selective forces act on groups. That would be a real empirical discovery. Whether it would vindicate traditional Wynne-Edwards style group selection is another matter. After all, this style of evolutionary analysis was invented by their opponents.

Still, the empirical discovery of a process that can be described as group selection does not invalidate individual-level or gene-level descriptions. It does not begin to establish that there is a uniquely correct description of the causal process. The discovery that we can describe the visual system as applying an algorithm to infer shape

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[1] See "The Poverty of Pluralism: A Reply to Sterelny and Kitcher," this JOURNAL, this issue, 151–8.

from motion does not demonstrate that we cannot also describe it in terms of interacting neural assemblies.

Many events can be analyzed from more than one perspective. All selective episodes (or, perhaps, almost all) can be interpreted in terms of genic selection. That is an important fact about natural selection. It is not, as Sober appears to think (*ibid.*, p. 151), something that is quasi-analytic on our view. That genic selection could be found in hard cases—those addressed by Richard Dawkins, John Maynard Smith, and (to a far smaller extent) ourselves—is surely on any view a major discovery.

Furthermore, the question whether there are real examples of processes that can be modeled as group selection can be asked and answered entirely within the genic point of view. Whereas Sober prefers to describe the possibility in terms of group selection, genic selectionists can couch it in terms of the existence of special genetic environments and/or populations. The empirical issue about the existence of scenarios of the type Sober imagines does not vanish within the genic image, but is recast in different terms.[2]

We have pointed out that genic selectionists have to be careful in characterizing populations and environments—for some genic accounts see selection as context-dependent—but there is no bar to taking the gene's-eye-view.[3] Nor is there any point in insisting that selection is really "context-independent higher-level selection" rather than "context-dependent lower-level selection." To justify that insistence, one requires a clear criterion for distinguishing between accounts of selection processes that are context-dependent and those which are not—thus identifying those causal profundities which shallow pluralists miss.

Sober used to have a crisp formulation of the causal commitments of selection models. The account, given in *The Nature of Selection*, proposed that the units of selection are those things which bear the properties for which selection occurs, and explicated selection for a property by demanding that the presence of the property raise fitness in at least one relevant context and not lower it in any. On the basis of the account, genic selection was supposed to be demonstrably inadequate. We demur. We have argued that Sober's explication of *selection for a property* will not sanction everyday (paradigm!) cases of natural selection, since there are typically microenviron-

² See C. Kenneth Waters, *Models of Natural Selection: From Darwin to Dawkins* (doctoral dissertation, Indiana University, 1986); and "Tempered Realism about the Forces of Selection" (forthcoming).
³ See Kim Sterelny and Philip Kitcher, "The Return of the Gene," this JOURNAL, LXXXV, 7 (July 1988): 339–361; and Waters, *op. cit.*

ments encountered by members of the population in which the prob-
abilistic inequalities are reversed.[4] We further pointed out that elab-
oration of the notion of an allelic environment would enable claims
about selection for genic properties to conform to Sober's principle.

   Sober sees our position as impoverished; not so. The fact that the
genic perspective gives us a general picture of selection processes is
an important idea. The extent to which individual- and group-level
descriptions are available remain open empirical questions. Some-
times, however, scientists, as well as philosophers, debate pseudo
problems. Once the possibility of many, equally adequate, represen-
tations of evolutionary processes has been recognized, philosophers
and biologists can turn their attention to more serious projects than
that of quibbling about the real unit of selection. The riches accruing
to those who insist on the right to quibble are purely illusory.

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BOOK REVIEWS

Descartes. MARJORIE GRENE. Minneapolis: University of Minne-
sota Press, 1985. 225 p. Cloth $35.00, paper $14.95.

Marjorie Grene displays in *Descartes* three distinctive trademarks of
her distinguished career in philosophy. First, and above all, she
shows her great capacity to engage in a philosophical dialogue with a
past thinker which is both sympathetic and combative. In order to
achieve such a spirited dialogue, she mobilizes the multiple skills of a
historian, metaphysician, and philosopher of science. This second
trademark of her work has as its purpose not only the fashioning of a
full treatment of her subject matter but also the development of the
philosophy of tacit knowledge, for which, following Michael Polanyi,
she has been a principal spokesperson. Readers of *Descartes* will be
pleased to discover that, in keeping with her third trademark as an

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[4] See Sterelny and Kitcher, *op. cit.,* and Waters, *op. cit.*