

Review of: "Femmes finales: natural selection, physiology, and the return of the repressed"

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The author offers a valuable reconstruction of the positions of some of the leading figures in pre- and post-Darwinian natural history regarding final causes. This kind of analysis is always useful, because it can help to clarify the various positions regarding this central problem for the philosophy of biology. This, in turn, is of great value not only for philosophers of biology but also for biologists and, above all, communicators (I include in this category all those who must communicate concepts of biology, including teachers and science popularizers). I find especially suggestive and interesting the history of the feminine metaphors that have been used throughout the ages to refer to the confusing relationship between biologists and final causes (of course, the author is totally innocent in relation to the sexist connotations of these metaphors). As a minor comment, it strikes me that no reference is made to Francis Jacob (in *The Logic of Life*) when the author reviews the authors who throughout history have made use of this metaphor. It strikes me because in what I have read it is a frequent reference. In summary, I consider it an original and interesting contribution to the problem of the legitimacy of finalistic explanations in biology. I have only two critical remarks that, perhaps, may be useful to the author for some partial revision of his writing.

The first, rather pointed, refers to the author's statement that "Most working biologists have been indoctrinated that they should not use teleological language, but the purposive language used by Darwin survives in the writings of adaptationists, sociobiologists, and evolutionary psychologists. These fields are often considered soft and non-rigorous because of their teleological 'just-so stories'." This paragraph includes several highly questionable statements. First, it seems to confuse, or at least establish a special relationship between the focus of sociobiology and evolutionary psychology and the use or acceptance of teleological reasoning and expressions. I think that is a mistake. I believe that teleological expressions and reasoning are especially linked both to the question of the presumed adaptive character of a trait and to the type of explanation that natural selection offers when the adaptationist hypothesis is accepted. Thus, teleological language and explanations will appear in any case in which selection is used to explain certain evolutionary change, i.e., in any case in which an adaptationist approach is adopted. But this is not exclusive to sociobiology or evolutionary psychology, but is common to all those who resort to selection to explain some trait. No biologist denies that adaptive evolution exists and all accept that natural selection can explain most of these cases. Moreover, most biologists agree that natural selection is a sufficiently satisfactory explanation of adaptation and, in fact, is the only scientifically acceptable explanation available to us. And of all the biologists who resort to selection to explain cases of evolution, sociobiologists and evolutionary psychologists represent a small minority (the latter is due, to begin with, to the fact that these researchers have as their object of investigation a small subset of the traits of organisms, namely some behavioral patterns or human mental

structure). In sum, I think it is confusing to suggest that sociobiology and evolutionary psychology have any special relationship to teleology. Secondly, it is also confusing to include in the same list "adaptationists", "sociobiologists" and "evolutionary psychologists" since, from what has been said, the latter two would be a subtype of the former: why then mention sociobiologists and evolutionary psychologists and not, for example, those who study animal coloration from an adaptationist approach? Thirdly, to refer to Gould and Lewontin's criticism of adaptationism seems out of place (or out of time) since, beyond how salutary such criticism may have been at the time, the debate seems settled within biology: no one denies the important role of selection and no one believes that all traits are the product of selection or that selection is the only evolutionary mechanism. And, in any case, the discussion on the relative importance of selection would not be very relevant since it is enough to recognize the reality of adaptation (as Lewontin himself did) and to admit (as all do) that selection explains adaptive traits for teleological ideas and expressions to enter biology. On the other hand, the suggestion that research based on the adaptationist approach is in some sense less rigorous than other research I think is unfounded, and Lynch's suggestion in relation to population genetics is also very weak; it is quite clear that natural selection theory is not the same as population genetics (natural selection theory explains why a variant of a trait changes its frequency, whereas population genetics merely notes such a change). Finally, teleological language is not only linked to adaptationist evolutionary explanations but also to the notion of function, which is central to non-evolutionary biology (precisely, functional biology according to Mayr's distinction).

The second observation, more general, is that the author does not draw any kind of conclusion on the general problem he addresses. Of course, one cannot ask for a solution to such a complex and ancient problem, but it would be interesting if the author could offer at least a partial and provisional conclusion or some suggestions as to what the historical analysis he offers teaches us about some of the major issues at stake: are there teleological theories and concepts in biology or is it only a type of discourse, is it the expression of a metaphor (as Ruse suggests) and if so, what is the relationship between this metaphor and the theory of natural selection, etc.?

In any case, as I pointed out at the beginning of my review, the article is of great value for deepening these important debates.