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

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Haig has offered a tremendous tour of the role of teleology in the biological sciences. I paused when typing [biological sciences] because Haig makes it very clear that the fortunes of teleology throughout history, and most especially in the 19th and 20th centuries, have been tied to disciplinarity and efforts to demarcate biology from those disciplines without, and also to apportion sub-disciplinary effort within. Teleology's role in some parts of the biological sciences has been its conspicuous removal and absence, we learn.

The paper is also interesting as a product in its own right. It amounts to a modern evolutionary biologist directly interpreting the historical record in order to derive possible philosophical interpretations, and to chase down the source of some modern preoccupations. Whilst this might initially seem Whiggish to historians and philosophers of science, it clearly is not. There is no veneration of great scholars but rather an honest scrutiny and commitment to exposing inconsistencies and changes across time. No easy line of progression is traced, and the closing statements indicate other sources of contemporary confusion around how to treat purpose. What Haig does, though, is to make clear available errors in reasoning.

These available errors are, for the most part, founded on differences in explanatory aims. Later in the paper Haig quickly references ideas of alignment with the physical sciences. This reminded me of Smocovitis (1992, 1996) and her discussion of how the evolutionary synthesis can be situated in the unity of science effort beginning early in the 20th century. She specifically places Mayr, whom Haig discusses, as trying to promote unification within biology whilst maintaining its separation from physics etc. What was not wanted was a reduction of biological phenomena to physics. His 1961 paper is seen, by Smocovitis, as a clear statement of intent in this domain. It would be interesting if Haig were to reflect on this a little further in the context of his comments about Mayr's version of the proximate-ultimate distinction. Haig argues Mayr initially meant ultimate to mean how-come, the historical causes of a trait. Not the why of a trait, which is exceedingly function in modern terms. But Haig notes Mayr came to shift ground a little here. I wonder why this was so, and if it related to his discipline founding efforts.

Haig's principal focus is upon the differences in biological science between the physiologists and evolutionists. Some of the former saw no role for evolutionary speculation in the generation of mechanist accounts of physiology. Others granted evolutionary accounts a limited utility. There was much to contemplate here, and Haig's writing is nuanced from his close reading. A significant service to his own argument and to the field would be to try and tabulate his biologists, their disciplinary affiliations, and their stated view on teleology (along with a clear statement of the type of teleology). If this were to come early it would help as a map to what is to come. Similarly, I think the latter distinctions and interpretations might be prefigured earlier in the paper in order to help the reader follow through the historical material and the many (but all entirely appropriately deployed) quotations. In counter to this I did rather enjoy the emergence of view as I read through. But this recommendation would make the paper a ready tool of reference for others in the field.

Returning to differences in explanatory aim, I think Haig's broader project perhaps beyond this paper, would benefit from looking into the recent philosophical discussions of idealisation and its role in causal models. Key authors here are Elgin, De Regt, and Potochnik. But others abound, for example Levy on the relation between abstraction and idealisation. Put simply idealisation refers to the introduction of simplifying falsehoods to gain explanatory traction. As Potochnik notes, real causation is messy and multivariate to an order that cannot be meaningfully encapsulated by our cognition. This puts scientific explanation firmly in a pragmatic light, and allows for multiple pragmatic purpose. To chase down an epistemic end is to decide upon what is important to treat as factive, and what can be idealised. Thus we are happy with accounts of objects that slide down [frictionless] planes, even though we know such planes do not exist. In population genetics we have infinite panmictic populations - an idealisation that Michael Lynch is happy with, I assume from Haig's account of him. I cannot remember, but I am sure Lynch would see the utility of optimisation idealisations in behavioural ecology not least because the target of explanation is different. Subtly different perhaps, and it would be useful for Hiag perhaps dwell on a few modern and detail cases to make this point in any further work.

So one version of Haig's history might be that it is an account of changing explanatory goals. That the debates between scholars were often conducted without this being fully acknowledged or even realised. Or perhaps that they felt they could close down certain explanatory ventures by asserting their own decision making processes when constructing theory. The modern taste is for pluralism of this sort, for understanding science as a diverse activity with no singular method, and many of the historical paroxysms we have seen as simply the result of scholars trying to fit all to one size. Perhaps… I still have much time for versions of falsification as a universal ideal for science. But that does not preclude different projects and idealisations etc.

One thing I should be most interested to learn is Haig's thoughts on modern views of causation. I sensed, but could be in error, that the physiologists who were focused upon mechanism were also committed to a view of cause based on intervention. They were experimentalists. Their disdain, where it existed, might have been for the association data that evolutionists relied upon to support their view. This might be what the original [just-so] criticism of adaptationism amounted to. The associations are not sufficient to launch causation, and most folk see causation as something with ends. Gould's constraints are nicely mechanistic, and explain measurable form, but they are in fact no less problematic. Counterfactual views must be pursued, and there is a role for prediction as Darwin demonstrated with his hypothesised pollinators that were eventually discovered.

I feel this is enough. The paper has clearly stimulated me to think, and to that end I am grateful for it. I hope my comments will prove of some use, if perhaps for the next paper in what must surely be a continuing series of reflections in this realm.