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# *Realism, Conventionalism, and Irrealism about Biological Functions: A Reply to Schyfter*

**Justin Garson, Hunter College, City University of New York**

As a philosopher of biology who works on the topic of biological function, I found Schyfter's article, "Function by Agreement," intriguing and challenging. Above all, I was happy to see the topic get some interdisciplinary attention. There are three points I'd like to raise; two are questions and one is a comment. First, I wondered whether Schyfter's "communitarian" approach to biological function really differs from one well-established tradition in the philosophy of biology spearheaded by Robert Cummins (1975). Second, if it does differ, why and how is that difference particularly beneficial to the sociology of scientific knowledge, as he claims? Finally, I wanted to defend a theory of function that Schyfter criticizes, the "etiological" theory, to which I am quite sympathetic (Garson 2014, particularly Chapter 7, and references therein).

First, a word by way of introducing the philosophical functions debate. The notion of biological function (as in, "the function of the heart is to pump blood") has been something of a recurring nightmare for philosophers and scientists. The problem is simple. Since the time of Aristotle, biology has been infused with final causation, or teleology. The reason we have teeth, Aristotle observed, is to help us chew food. The reason bees have stingers is to help them protect the hive. And so on. It's hard to imagine what biology would look like without such appeals, which are routine even today.

On the other hand, it's hard to see how such appeals could possibly be correct. How can a potential effect of my having teeth (namely, helping me chew food) explain why I have teeth? Such explanations seem to confuse the order of causation. Of course, if one believes that there is an intelligent and benevolent deity that designed us with teeth so we could chew food, then there's no conceptual problem. We have teeth in order to chew food because that's what the deity had in mind when he/she/it made us. But such a postulate rejects the naturalism that has become part and parcel of modern science.

Why is all this a problem for biological function? Because on one natural interpretation of such locutions, to say that "the function of the heart is to beat," or "the function of the teeth is to chew," or "the function of the stinger is to protect the hive," one is trying to explain why hearts (teeth, stingers) exist by referring to their beneficial effects. That's a teleological explanation. So if there's something wrong with teleological explanations, then there's something wrong with biological functions. That suggests that *either* biologists should stop talking about them, *or* that we give a respectable naturalistic foundation to such talk.

This dilemma has given rise to three major viewpoints in the philosophy of biology: realism, conventionalism, and irrealism. Function *irrealists*, as I'll call them, hold that parts of organisms don't really have functions, any more than organisms have bodily humors or entelechies. At best, function talk is a convenient fiction or heuristic. Michael Ruse (2002) is the most prominent defender of this view. Function *realists*, like myself, think that functions can be given a respectable biological foundation. The etiological view, for example, is a version of realism that holds, roughly, that the function of a trait is

just whatever it was selected for by natural selection. Functions are as real (or unreal) as selection processes, and they are no big deal. Amongst philosophers of biology, Ruth Millikan (1989) and Karen Neander (1991) are major proponents of this view.

Function conventionalists think that functions are real, but relative to our epistemic practices. On this view, whether a trait has a function, and which function it has, depends on the community of researchers who investigate it. Robert Cummins (1975) is the chief source here. The motive, as I can tell, is to lend credence to function talk amongst biologists but recognize that it is, in some ways, an artifact of our explanatory purposes. Relative to our interest in health and survival, for example, the function of the heart is to pump blood. Relative to our interest in having a quick and ready diagnostic tool for heart disease, the function of the heart is to make beating sounds that one can hear through a stethoscope. Carl Craver, one of the prominent defenders of this view, calls it "perspectivalism" about function (e.g., 2001, 2013). Other defenders include Valerie Hardcastle (1999; 2002) and Paul Davies (2001). If I understand correctly, Schyfter is also in this camp.

Sometimes it is hard for me to see how Schyfter's view differs from the standard theory. Schyfter says, "Rather than being conceptualized as a property of traits or structures, function should be understood as a status granted by communities acting in accordance with specific domains of knowledge and practice" (2014, 2). Also, "function is not a property of things, it is a standing within collectives" (2014, 7). Compare Craver: "I embrace a form of perspectivalism about both functions and mechanisms … Specifically, talk of functions and final causes is not legitimized by or reduced to privileged kinds of etiological histories … or to certain special effects of the item in question. Rather, they are imposed from without by creatures seeking to understand how a given phenomenon of interest is situated in the causal structure of the world" (2013, 134-135). Cummins, Hardcastle, and Davies repeatedly make similar claims, namely, that functions are relative to the goals and interests of researchers. To that extent, Schyfter's view seems like a straightforward application of the Cummins-type view to a project in the sociology of science. That seems like a valuable interdisciplinary extension of the basic theory and I applaud it.

If I understand correctly, however, Schyfter wants to do something else, something he thinks philosophers should pause to take note of: "my goal is not to simply modify Cummins' account" (2014, 5). The alleged difference is that Schyfter wants to avoid what he calls the "will to 'naturalise' functions as properties of things" (2014, 5). If I understand right, the idea is the following: proponents of the Cummins-type view articulate their views within a framework of realistic and naturalistic commitments. They think, for example, that there are organisms, that organisms are made up of components (parts, properties, activities), that those components evolved over time, that they engage in various cause-and-effect relations, that those components and their relations explain observable phenomena, and that people sometimes make discoveries about those things. In contrast, Schyfter wants to shift attention to how thoroughly conventional our categories are: that even "the act of sectioning an organism into sub-systems and components" (2014, 9) is a conventional practice, the way we "select a specific cause-effect sequence of interest" is a conventional choice, and the "assignment of ends and

purposes" (2014, 10) depends on our epistemic goals. Taking this step will avoid "a number of complications and undesirable implications" (2014, 6) (though I must note that I didn't find any of the implications particularly undesirable).

I'm not sure, however, how wide the divide actually is between Schyfter and the proponents of Cummins' theory. Philosophers, particularly those in the Cummins tradition, have emphasized repeatedly the conventional elements involved not only in attributing functions to entities (e.g., Davies 2001; Hardcastle 2002), but also in the way that we analyze organisms into parts (Kauffman 1970; Wimsatt 1972), and the way that we think about cause and effect (e.g., Craver 2007, 100). Of course, those I've cited believe that there is a real world that constrains our explanatory projects and ambitions, but Schyfter doesn't dispute this: "the empirical world certainly constrains the list of plausible claims an observer might make regarding a trait and function …" (2014, 8). He also says that, "the material qualities and behavior of living things unquestionably matter" (2014, 9). So, is there a disagreement after all? I would have thought that Cummins' own view, and that of his followers, could be summarized as follows: whether a trait has a function, and which function it has, is based on nature and convention — or perhaps better, convention constrained by nature.

My second point is tied to the first. Suppose there is a real disagreement here, and that the philosophers I've cited are wedded to a naturalistic viewpoint in a way that Schyfter is not. If this is what distinguishes his position from others in the Cummins' tradition, why is this distinction useful or valuable for sociologists of science? I'm not a sociologist, so I'm outside my disciplinary competence here, but I wonder what sorts of things Schyfter can do that people like Craver, or Cummins, or Hardcastle, are barred from doing because of their misplaced naturalistic commitments. Schyfter emphasizes the utility of his approach for sociology of knowledge and science studies. For example, Schyfter spends some time talking about a research project involving transplanting light-sensitive cells from the cyanobacterium *Synechocystis* to *E. coli*, and he tells us that his theory of function allows him to have some novel insights about scientific practice that would have remained closed to more traditional function theorists. But then he seems to grant that Cummins' theory would yield pretty much the same insights: "a Cummins account of function provides some quite useful analyses" (2014, 16). His criticism, then, seems to boil down to a purely philosophical one: he dislikes the "unnecessary naturalism" inherent in the account (2014, 16). To the extent that his view differs from a Cummins-type view, then, it's not clear to me what that difference actually buys in the context of field research.

My third point is a word of defense of the etiological theory, which came in for some critical scrutiny in Schyfter's paper. (A bit of philosophical repartee seems warranted here, given that Schyfter's aims are, in part, to "challenge basic positions frequently held by philosophers of function" [2014, 12]). As I noted above, the etiological theory is one version of the "realist" view of functions. It holds that the function of a trait is what it was selected for by natural selection (with some minor qualifications to include, for example, traits that were recently selected for). The etiological theory is specifically a theory of *biological* functions; it's not a theory about, say, artifact functions. As such, etiological theorists are usually pluralists about "function:" they think that "function" is

ambiguous. There are biological "functions," there are artifact "functions," there are mathematical "functions," and so on. The fact that, in an era of synthetic biology, some "artifact" functions are implemented in biological materials adds an interesting wrinkle here but I don't think it affects the basic analysis.

The basic motivation for the etiological theory is to solve the problem of final causation. The problem, as noted above, is this: how can it be that the effect of a trait explains the existence of that very trait? How can the fact that teeth chew food explain why we have teeth? The etiological theory gives the following answer, crudely put: "long ago creatures with teeth were able to outlive creatures that didn't have teeth. That's because our ancestors with teeth were able to chew things (or chew things better) than those without teeth. Thanks to the fact that teeth are good for chewing, you and I have teeth today." If our picture of natural selection is right, then the following statement is literally true: *we have teeth because teeth are good for chewing*. Problem of final causation solved. Biologists can sleep at night again. (Of course there are numerous qualifications to add, in order to make the sketch given above resemble what we actually know about evolution, to avoid hyper-adaptationism, and so on; see Garson 2012; 2014.)

Sometimes philosophers don't completely appreciate the motivation behind etiological theories. They seem to think that the reason etiological theorists define function in terms of selection processes is because those theorists are obsessed with selection, or those theorists think that evolutionary biology is the only legitimate kind of biology, or those theorists forget that people in other disciplines might possibly be interested in things other than selection. Hardcastle gently chides etiological theorists by reminding them that, "no version of an etiological theory can work without first assuming some sort of framework in which we can pick out a trait and the relevant effects … functions can be supplied only relative to an explanatory backdrop" (2002, 147). Schyfter makes a similar point: "[S]imply because a programme of research based on evolutionary theory recognizes the survival of organisms as a useful heuristic in inferring function from structure does not give to organism survival indisputable validity or universal epistemic priority" (2014, 11). But that is not why etiological theorists define function in terms of selection. They define function in terms of selection because it seems to be the only naturalistically plausible way to solve the problem of final causation.

Again, I don't want my criticism to be seen as eclipsing my appreciation for Schyfter's work and his project. I applied his critical efforts here, his articulation of theories of function in the sociology of science context, and his willingness to engage philosophers on those issues.

**Contact details: jgarson@hunter.cuny.edu**

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