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Abstract: In *What Biological Functions Are and Why They Matter*, Justin Garson offers a novel theory of biological functions, the generalized selected effects (GSE) theory.¹ He presents the theory in a clear and comprehensive way, defends it against various objections and applies it to different areas of philosophy, including the philosophy of psychiatry, the debate about mechanisms and the debate about teleosemantic theories of mental content.² Like other proponents of the aetiological approach to functions, Garson maintains that a trait's biological functions are grounded in its selectional history. However, his GSE theory differs from standard versions of the aetiological approach – so-called selected effects (SE) theories – in a crucial respect: while SE theories assume that all biological functions must ultimately be grounded in phylogenetic processes of natural selection or other (closely analogous) processes of differential reproduction (Millikan 1984: 15–82, Neander 1991, Godfrey-Smith 1994...

DOI: <https://doi.org/10.1093/analys/anab015>

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ZORA URL: <https://doi.org/10.5167/uzh-216503>

Journal Article

Accepted Version

Originally published at:

Schulte, Peter (2021). No Functions for Rocks: Garson's Generalized Selected Effects Theory and the Liberality Problem. *Analysis*, 81(2):369-378.

DOI: <https://doi.org/10.1093/analys/anab015>

Link to the published version: <https://doi.org/10.1093/analys/anab015>

No Functions for Rocks: Garson's Generalized Selected Effects Theory and the Liberality Problem

PETER SCHULTE

1. Introduction

In *What Biological Functions Are and Why They Matter*,¹ Justin Garson offers a novel theory of biological functions, the Generalized Selected Effects (GSE) theory. He presents the theory in a clear and comprehensive way, defends it against various objections, and applies it to different areas of philosophy, including the philosophy of psychiatry, the debate about mechanisms, and the debate about teleosemantic theories of mental content.²

Like other proponents of the etiological approach to functions, Garson maintains that a trait's biological functions are grounded in its selectional history. However, his GSE theory differs from standard versions of the etiological approach – so-called selected effects (SE) theories – in a crucial respect: while SE theories assume that all biological functions must ultimately be grounded in phylogenetic processes of natural selection or closely analogous processes of *differential reproduction* (Millikan 1984: 15-82, Neander 1991, Godfrey-Smith 1994), the GSE theory allows that functions may also be grounded in processes of *differential retention* (see §2 for details). Garson forcefully argues that this broader account is theoretically well-motivated and has significant advantages over

¹ *What Biological Functions Are and Why They Matter*. By Justin Garson. Cambridge University Press, 2019. xii + 235 pp. (Unless otherwise noted, all quotations are from this book.)

² For another (much briefer) statement of the GSE theory, cf. Garson (2017).

standard SE accounts when it comes to accommodating certain important classes of ontogenetically determined functions.

Moreover, Garson contends that the GSE theory has important implications far beyond the philosophy of biology. For instance, he uses the GSE theory to show that mental disorders cannot be generally characterized as ‘harmful dysfunctions’ because some disorders may well be functional (170-186), and he suggests that teleosemantic theories of mental content, when combined with the GSE theory of function, have better resources to explain how organisms can represent evolutionarily novel features of their environment (187-212).³

However, since it is a crucial feature of the GSE theory that it applies more broadly than standard SE accounts, a natural worry is that it might apply *too broadly*, entailing function-ascriptions in cases where they are clearly not warranted. This is the *liberality problem*, of which Garson is well aware, and which he discusses at length in his book. Although the book also contains insightful discussions of many other topics, some of which I have just mentioned, my focus here will be on this central issue.

I will proceed as follows. In §2, I give an outline of the GSE theory, as it is presented in Garson’s book. In §3, I then turn to the liberality problem. After reviewing Garson’s response to it, I argue that it does not ultimately succeed: there are counterexamples which show that the GSE theory, as spelled out by Garson, does indeed apply too broadly. However, I argue further (in §4) that careful reflection on the crucial notion of a *population* makes it possible to restate the GSE theory in a way that makes it immune to

³ This last point is further elaborated in Garson & Papineau (2019).

these counterexamples. I end with some concluding thoughts about the prospects of the GSE theory in §5.

2. Garson's Generalized Selected Effects Theory

The biological function of the orchid's scent is to attract pollinators. One key function of red blood cells is to transport oxygen. And the putative function of zebra stripes, according to recent studies, is the deterrence of parasitic insects (9-10). How should we conceive of the functions ascribed by such statements?

In order to get a clear understanding of Garson's answer to this question, it is best to start with the answer provided by standard SE theories. According to (what I will call) the basic SE account, all the biological functions of present traits of an organism are grounded in the organism's phylogenetic history. The idea is, very roughly, that a trait T has the function to φ iff T has been selected for φ -ing, where 'selected for' is understood in terms of ordinary natural selection (Neander 1991: 174). The scent of a present-day orchid, for instance, has the function to attract pollinators because (i) the orchid's ancestors produced that type of scent, thereby attracting pollinators and enhancing their reproductive success, and (ii) this helps to explain why the trait (the scent or, more precisely, the scent-producing disposition) spread through the population, so that it is now generally present in orchids. In short, the basic SE account construes functions in terms of natural selection, and natural selection in terms of differential reproduction.

The standard SE approach to biological functions has much to recommend it, as Garson nicely demonstrates. In the first part of his book, he argues that the approach can accommodate two puzzling features of functions ascriptions in biology: (a) their

“explanatory depth”, i.e. the fact that biologists “often purport to give causal explanations for why [...] traits exist” when they ascribe functions to them (11), and (b) their “normativity”, i.e. the fact that they allow for the possibility of dysfunction or malfunctioning (10-11). If we adopt the SE approach, we can say that function ascriptions have explanatory depth because they are “condensed causal explanations” (26) – selectionist explanations, to be precise – , and we can maintain that they are normative (or ‘malfunction-permitting’) because a trait token may well be unable to φ while belonging to a type that has been selected for φ -ing (28-29).

After defending these (familiar) claims about the virtues of the SE approach against recent criticisms (32-38), Garson argues that all the other approaches to biological functions currently on offer are deficient in comparison. In particular, none of them provides an adequate account of the explanatory depth of function ascriptions (43-62), which seems to give the SE approach a decisive advantage over its rivals. Furthermore, Garson maintains that even the basic SE account can, with a “slight adjustment” (38), accommodate function loss in the case of vestigial traits (38-40), and that it is not threatened by several alleged counterexamples (40-42).

One important problem remains, however. Most theorists acknowledge that the basic SE account is *too narrow*, since it is unable to account for those functions that appear to arise from certain ontogenetic processes (processes occurring during the lifetime of an individual). Accordingly, proponents of the SE approach have tried to extend the scope of their theories, either by adopting an abstract notion of differential reproduction that also applies to certain learning processes (e.g. Papineau 1984: 557-559), or by supplementing their account with a theory of derived functions (e.g. Millikan 1984: 39-50).

This is the point where Garson departs from standard SE theories in a crucial way.⁴ He argues that we can generalize the selected effects approach – or, as he also puts it, “[drop] some unnecessary limitations” (3) of the approach – by accepting that functions are not always grounded in processes of differential reproduction, but may also be grounded in processes of differential persistence or ‘differential retention’. More precisely, Garson’s GSE account of functions can be stated as follows: “A function of a trait is an activity that led to its differential reproduction, or retention, in a population” (93). (Note the qualifier “in a population”, which will become important later on.)

Garson mentions numerous candidates for processes of differential retention in a population, including clonal selection and different types of neural selection (78-92). In many of his arguments, however, he focuses on the clear-cut (hypothetical) case of synapse selection in an octopus preying on a crab (originally proposed by the neuroscientist J. Z. Young).

With some slight embellishments, the case runs as follows (87-89). An octopus encounters a new type of crab and has to decide whether to attack or to retreat. Focusing on the octopus’s brain processes, what happens now is this: the presence of the crab activates a ‘classification’ neuron N, which is connected (through synapse S1) to a neuron that controls attack behavior, and (through synapse S2) to a neuron that controls retreat behavior. What type of behavior is triggered by the activation of N depends on the relative strength of the (excitatory) synaptic connections S1 and S2. (In this simple example, the two synapses are supposed to stand *pars pro toto* for a great number of different synaptic connections – “a random profusion of synapses” (88), as Garson puts it.)

⁴ I am concentrating here on Garson’s main line of argument. It should be mentioned that Garson is, in fact, quite sympathetic to derived functions (101), but this is not an integral part of his theory.

On the octopus's first encounter with this new type of crab, the strength of both connections is equal, so that for each behavior, the probability that it will be produced is 0.5. However, the octopus possesses a learning mechanism that adjusts synapse strength depending on the results of the behavior that is produced: if the results are positive (consisting, e.g., in the ingestion of food), the synapse is strengthened; if they are negative (consisting, e.g., in some form of bodily harm), it is weakened. Ultimately, the weaker synapse is completely eliminated.⁵

Since, as a matter of fact, the new type of crab turns out to be very strong and aggressive, the results of retreat behavior are much better (i.e. much less harmful) for the octopus than the results of attack behavior. Hence, S1 is continually weakened and finally eliminated. After this has happened, we can say: S2 was selected over S1 because it produced retreat behavior. Furthermore, it seems very plausible to hold that S2 now has the *function* of causing retreat behavior – or, more precisely, the function of causing retreat behavior in response to N-activation (89-90).

It is clear, however, that S2 was not selected through a process of differential reproduction (i.e. through a process where items of a certain type outcompete items of different types by producing more offspring). Instead, S2 was *selectively retained* over S1 because of the behavior it produced. Hence, standard SE theories cannot accommodate S2's newly acquired function (at least not in any straightforward way), but the GSE theory can. And this, Garson rightly maintains, is a significant advantage for his proposal.

3. The Liberality Problem

⁵ Again, I am following Garson's (and Young's) way of filling out the scenario at this point. An alternative learning mechanism might not eliminate the synapse, but only (e.g.) dramatically reduce the number of neurotransmitter receptors on the postsynaptic membrane.

The GSE theory is more inclusive than standard SE theories. In cases of individual learning, such as the octopus case, this is a clear advantage. According to the liberality objection, however, there are other cases where the inclusivity of GSE theory counts strongly against it.

The simplest version of this objection, as stated by Garson, runs as follows (102-103). Consider a number of rocks that are scattered on a beach. Some of them are harder, some are softer. The harder rocks erode at a much slower pace, thus remaining on the beach long after the softer rocks have all disappeared. In this scenario, we can say that the harder rocks' hardness, by improving their resistance to erosion, accounts for the fact that they persist longer than the other rocks on the beach. In other words, this is a case of differential persistence. Hence, GSE theorists seem to be committed to the highly implausible view that the hardness of the harder rocks has the *function* of improving their erosion-resistance.⁶

However, Garson quickly points out that this counterexample does not work: while it is indeed a case of differential persistence, it is not a case of differential persistence *in a population*, and thus not the kind of process that (according to the GSE theory) would be sufficient to ground a biological function for rock hardness. To appreciate this point, it should be noted that not every collection of individuals counts as a population: in order to form a population, individuals must interact in the right way. What this 'right way' consists in, is a matter of some controversy, but according to Garson, "[t]he emerging consensus is that populations require *fitness-relevant* interactions" (103).

⁶ Note that the GSE theory (just like standard selected effects theories) does not contain any requirement stating that function bearers must be traits of 'organisms', or of 'living things'.

Of course, in order to fit with the GSE theory, the term ‘fitness-relevant interactions’ has to be understood in a relaxed sense: it cannot be restricted to interactions that affect *reproductive success*, but must also include interactions that affect (mere) *success in persisting* (104). Hence, strictly speaking, the term ‘fitness- or persistence relevant interactions’ would be more appropriate here. In any case, since the rocks on the beach do nothing to affect the chances of each other’s persistence, they fail to constitute a population. Consequently, the GSE theory does not entail that rock hardness has a function in this scenario.

A slightly modified version of the rock example (due to Karen Neander) is more problematic (106). Suppose that the rocks are not scattered, but heaped up into a pile. When the waves hit the pile, the rocks grind against each other, so that the harder rocks causally contribute to the (accelerated) erosion of the softer rocks. In this scenario, we do have persistence-relevant interactions between the rocks. Hence, it seems that GSE theorists must admit that the rocks form a population, and that the hardness of the harder rocks qualifies as a function-bearing trait.

Garson’s reply to the modified rock example (106-108), which draws on work by John Matthewson (2015), is quite intricate, but the gist of it is this. First, the notion of a population is a *graded* concept: collections of individuals can be more or less population-like. Secondly, the degree to which a collection of individuals qualifies as population-like depends on the degree of ‘linkage’ between those individuals. Very roughly, if individuals have fitness- or persistence-relevant interactions with many other individuals in the

collection, the degree of linkage is high; if they only have such interactions with a few others, the degree of linkage is low.⁷

Now, since every rock in the pile only interacts with its immediate neighbors, the degree of linkage that this collection of rocks exhibits is very low. The collection is thus not very population-like. Hence, “the rocks in our pile have, at best, a very low degree of functionality” (108).

So far, so good. However, couldn't we block this kind of reply by modifying the rock example further? Consider, for instance, the following case sketched by Brandon Conley in a recent review of Garson's book:

[P]ut all the rocks in a spacious container and have a machine shake the container so hard that the rocks are worn away by constant collisions as they fly around inside. As with the original example, harder rocks will be retained while softer rocks wear away to dust. (Conley 2020)

In this case, the group of rocks has a high degree of linkage, thus qualifying (according to Garson's standards) as very population-like. But, according to Conley, it still seems very implausible to ascribe a function to the hardness of the harder rocks.

Some might disagree with this intuitive verdict. After all, Conley introduces an intentional agent into the picture – an agent who uses a machine to manipulate the rocks –, and this may lead us to view the harder rocks that are retained as artifacts, and to attribute artifact functions to them. In any case, Conley's appeal to an agent invokes a set of intuitions that makes the scenario somewhat difficult to evaluate.

⁷ Among other things, a more precise formulation would also take into account that these interactions differ in strength, and assign greater weight to stronger interactions (cf. Matthewson 2015: 183-186).

However, we can easily construct an analogous case where no agent is involved. Imagine, for instance, a rockfall in which a number of rocks roll down a long mountain slope, constantly bouncing against each other, with the same end result as in Conley's example: the harder rocks, in virtue of their hardness, reach the bottom of the slope, while the softer rocks are destroyed on the way. I take it that, in such a case at least, the implausibility of the function-ascription is manifest.⁸ (I will refer to this counterexample as 'the rockfall case' in the following.)

4. A New Answer to the Liberality Problem

In my view, the preceding discussion shows that the GSE theory, as it is formulated in *What Functions Are and Why They Matter*, cannot overcome the liberality problem. This does not mean, however, that the problem must be fatal. In this section, I will present a novel answer to it, which will enable GSE theorists to refute putative counterexamples like the rockfall case. The gist of my answer is this: Garson is right to counter the liberality objection by focusing on the notion of a population. What the rockfall case shows, however, is that requiring fitness- or persistence-relevant interactions between individuals in a population is not enough. Instead, we should say that individuals only form a population if they are connected by relations of *competition*.

My proposal starts from Peter Godfrey-Smith's seminal work *Darwinian Populations and Natural Selection* (2009) (which, incidentally, is also the main inspiration for Matthewson 2015). In this book, Godfrey-Smith aims to give a general account of natural selection, and the notion of a 'Darwinian population' plays a central role in this account.

⁸ Another type of counterexample involves a small pile of (say) five rocks that all grind against each other. According to Garson's standards, this small group of rocks should also qualify as very population-like, since it seems to exhibit a high degree of linkage. (Thanks to Fabian Hundertmark for suggesting this case to me.)

As Godfrey-Smith argues, one crucial feature of paradigmatic Darwinian populations is that the members of these populations are linked by relations of ‘reproductive competition’.⁹ Importantly, this not only means that some of these group members do things that have a negative influence on the reproduction rate of some of the others. It also means that “there is a *causal dependence* between how many offspring each individual has”, such that “a slot I fill in the next generation is a slot you do not fill” (Godfrey-Smith 2009: 51; my emphasis). Or, as Godfrey-Smith puts it in another passage, “the extent of reproductive competition” is “the extent to which adding reproductive success to one individual reduces another’s” (Godfrey-Smith 2009: 52). Hence, reproductive competition means that individuals exert a negative influence on the reproductive success of others *because* they behave in a way that enhances their own reproductive success.

Straightforward examples of this relationship are cases of competition for food, mating opportunities or other limited resources. If, for instance, an individual obtains food from a limited supply, it not only improves its own chances of reproduction, but *ipso facto* lowers the chances of other individuals relying on the same food supply.

Of course, from the standpoint of GSE theory, requiring relations of *reproductive* competition between members of a population will not do, since such a requirement would entail that only entities that reproduce can form populations (and acquire biological functions). However, just like the notion of fitness-relevant interactions, the notion of reproductive competition can be generalized. We can say (again, somewhat roughly) that an individual A stands in a relation of competition to B iff the following

⁹ Note that what I simply call ‘reproductive competition’ is Godfrey-Smith’s ‘reproductive competition in the stronger sense’ (cf. Godfrey-Smith 2009: 51).

holds: A exerts a negative influence on B's chances of reproduction *or persistence*, because A behaves in a way that enhances its own chances of reproduction *or persistence*.

With this generalized notion in hand, we can say that in order to form a population, individuals must be (directly or indirectly) connected by relations of competition. How extensive do these relations have to be? Here, I think it is best to adopt Garson's gradualist strategy, and to say: the higher the degree of linkage between individuals of a collection, the more population-like will this collection be. The difference is that 'linkage' is now to be understood more narrowly in terms of relations of *competition*: on my proposal, a collection's degree of population-likeness is not grounded in fitness- or persistence-relevant interactions of any kind, but in *competitive* fitness- or persistence-relevant interactions.

If we combine the GSE theory with this conception of a population, we can easily show that the rockfall case no longer constitutes a genuine counterexample. While it is true that the rocks are tightly linked by persistence-relevant interactions, these interactions are not competitive: it is not the case that the harder rocks exert a negative influence on the chances of persistence of the softer rocks *because they behave in a way that enhances their own chances of persistence*. When one of the harder rocks hits a softer rock, the chances of persistence of the softer rock are lowered by this interaction, but it is *not* the case that the harder rock's chances of persistence are increased (quite to the contrary, in fact!). Hence, the collection of rocks tumbling down the mountain slope do not form a population, and the hardness of the harder rocks does not qualify as a function-bearing trait.

This contrasts nicely with the cases of synapse selection that Garson describes. As a representative example, consider again his octopus case. In this scenario, the octopus's learning mechanism strengthens or weakens the synapses S1 and S2 according to the

(positive or negative) results produced by them, and ultimately eliminates the weaker synapse. This means that S2, by regularly causing retreat behavior that is more positive (less harmful) than the attack behavior caused by S1, enhances its own chances of persistence, *and in doing so, reduces the persistence chances of S1*. (We may also assume that S1, in the few instances in which it causes successful feeding behavior that is not harmful, enhances *its* own chances of persistence at S2's expense, even though S2 prevails in the end.) So the synapses S1 and S2 do compete with each other. Hence, they clearly fulfill the new requirement for population status that I have proposed, and can thus be bearers of genuine biological functions. (The same holds, *mutatis mutandis*, for the other putative cases of selective retention discussed by Garson (89-92).)

This completes my alternative proposal for answering the liberality objection. As I have argued, the GSE theory can successfully exclude the rockfall case – as well as the other counterexamples discussed in section 3 – if we define populations in terms of competitive interactions between individuals (or, in the more precise gradualist formulation, if we define population-likeness in terms of linkage by relations of competition).

Before I conclude, let me briefly address a potential objection. A critic may worry that my proposal unduly privileges relations of competition over relations of cooperation. Modern biology teaches us that cooperation is a major factor in evolution, so why are cooperative interactions ignored in the proposed definition of a population?

My answer to this worry is two-fold. First, I would like to point out that there is a good reason for privileging competitive relations when we are trying to define the notion of a population. The reason is, very briefly, that the (relevant) notion of a population is supposed to play a role in accounting for selection processes, and competitive relations

seem to be essential for selection processes in a way that cooperative relations are not (see, e.g., Matthewson 2015: 193).

More importantly, however (and this is my second point): if we define populations in terms of competitive *or cooperative* interactions,¹⁰ as the critic suggests, a slightly supplemented version of my answer to the liberality problem will still succeed. With the arguments given above, I have shown that the persistence-relevant interactions between rocks are not competitive. However, even without a detailed analysis of cooperative relationships in hand, we can be sure that they are not cooperative either: after all, the harder rocks *reduce* the chances of persistence of the softer rocks they come into contact with, thus no proper analysis will categorize their mutual relationship as cooperative. Consequently, function ascriptions in the rockfall case can still be rejected on the grounds that the rocks do not form a population.

5. Concluding Thoughts

In this critical notice, I have summarized Garson's GSE theory, as it is presented in *What Biological Functions Are and Why They Matter*, highlighted the advantages that it has over standard SE accounts, and then focused on a central problem for the theory, the liberality problem. I have argued that Garson's own answer to this problem is ultimately unsuccessful, and I have proposed a modification of the theory – or, more precisely, of the notion of a population that it rests on – in order to remedy this defect. In conclusion, I want to address two remaining issues that concern Garson's original answer to the liberality problem as well as my alternative proposal.

¹⁰ Cf. Millstein (2009) for an account of populations that appeals to both competitive and cooperative interactions.

First, some critics have objected that the notion of a population that Garson relies on in his answer to the liberality problem is too broad (Conley 2020). According to Garson's definition, groups of animals that are composed of predators and their prey qualify as populations (because they interact in fitness-relevant ways), and this remains true under my modified proposal (because the interactions are competitive). However, such groups of animals are generally *not* characterized as populations by evolutionary biologists.¹¹

In reply to this objection, I would like to point out, first, that GSE theorists are committed to a broadened notion of a population in any case. After all, the notion is supposed to apply (e.g.) to groups of synapses, which are (typically) not classified as populations by evolutionary biologists either. In light of this, GSE theorists should argue that their notion of a population is a *natural extension* of the narrower notion that is used in evolutionary biology. Moreover, GSE theorists can readily accept a pluralist position, according to which this narrower notion is, in fact, the appropriate notion in evolutionary biology (and maybe also in other theoretical contexts). What GSE theorists must maintain is that their account of biological functions appeals to *a* valid and well-defined notion of a population, not that this is *the* (only) well-defined notion available.

Secondly, one might worry about the relatively narrow range of counterexamples that are discussed in Garson's book (and, consequently, also in this paper). Couldn't there be other kinds of counterexamples – cases involving non-living things which interact in more complex ways than rocks do, and which therefore qualify as forming populations (even according to my modified definition)?

¹¹ To exclude such groups, theorists regularly appeal to relationships of reproduction or genetic exchange (compare, e.g., the appeal to "exchangeability" in Matthewson 2015: 188-192), but that option is not available to the GSE theorist.

Concerning this very general worry, there are (again) two things that we should note. First, it may well be that such counterexamples exist, and that the notion of a population has to be refined further in order to exclude them. This is a possibility that the GSE theorist can readily acknowledge. Secondly, however, we should remember that there are also putative counterexamples to standard SE theories that involve non-living things interacting in complex ways (see, e.g., Bedau 1991). Hence, we should be careful to investigate in each case whether a novel counterexample constitutes a problem for the GSE theory specifically, or for selected effects theories in general. In the latter case, given the force of the principled arguments in favor of the selected effects approach (cf. section 2), the weight of the counterexample would be greatly diminished.¹²

In sum, my discussion shows that there is still some work ahead for Garson and other proponents of the GSE theory – work that will centrally involve a more in-depth analysis of the notion of a population. But it also shows, in my view, that there are good reasons to be optimistic about the prospects of the theory. In light of this, I think it's safe to say that *What Functions Are and Why They Matter* will prove to be a central point of reference for future debates about the nature of biological functions.¹³

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¹² It should be noted that, according to Garson, Bedau's main counterexample (a case involving self-reproducing clay crystals) can be dealt with in a more direct way, again by appealing to the notion of a population (42).

¹³ First, I would like to thank Justin Garson for very generous and helpful feedback on a previous draft of this paper. I would also like to thank Hannah Altehenger, Hanjo Glock, Elia Hämmerli, Fabian Hundertmark, Christoph C. Pfisterer, Franziska Poprawe, Nicole Rathgeb, and Arthur Schwaninger for further helpful comments and suggestions.

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