Pluralism, realism and the units of selection

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I consider two attempts to combine realism with pluralism about the units of selection: Sober and Wilson's combination of "model" and "unit" pluralism, and Sterelny and Griffiths' "local pluralism". I argue that both of these attempts fail to show that realism and pluralism are compatible. Sober and Wilson's pluralism turns out, on closer inspection, to be a kind of monism in disguise, while Sterelny and Griffiths' local pluralism involves a combination of realism and anti-realism about interactors, and the units of selection, that is fundamentally unstable. My conclusion is that one must choose whether to be a realist or a pluralist in this area: one cannot be both. The question of which we should choose is a further question that I do not take a stand on.

Introduction

Combinations of realism and pluralism have been defended in several different areas of the philosophy of biology in recent decades.[1] The case for pluralism (of perspectives, concepts, models, and explanatory strategies) has become increasingly persuasive with respect to a range of issues; yet most philosophers of biology would still like to be realists of some variety. Thus some form of "pluralistic realism" has often been the aim, and has been defended by a number of leading theorists, in different areas.[2]

In this article I consider two attempts to combine realism with pluralism about the units of selection – Sober and Wilson's combination of "model" and "unit" pluralism, and Sterelny and Griffiths' (and others') "local pluralism". I argue that both of these attempts fail to show that realism and pluralism are compatible.

In recent years "model pluralism" with respect to the units of selection issue has become a popular position. This is the view that a plurality of selection models, identifying different units of selection, can be true of one and the same selection process (the plurality is in the models, not in the world). It contrasts with "unit pluralism", the view that there are a number of distinct units of selection operative in the natural world (the plurality is in the world, not [just] the models) (Wilson 2005). Typically, unit pluralism has been thought of as a realist view, while model pluralism has been thought of as an anti-realist or conventionalist view, by both its supporters and opponents. Sober and Wilson (1998) have sought to combine these forms of pluralism. They claim in particular that model pluralism is compatible with a robust realism about the units of selection. I argue that in fact their model pluralism is pluralism in name only: their realism means they are not able to consistently advocate genuine model pluralism. A genuine model pluralism, I suggest, must be a thorough-going anti-realist pluralism.

I also consider the relationship between "local" and "global" pluralism about the units of selection (Okasha 2006). Local pluralism is the view that model pluralism is true of some selective episodes, while in other cases there is a fact of the matter about the unit(s) of selection that has been operative (model pluralism is false for those cases). Sterelny and Griffiths (1999) defend a form of local

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1   Realism is roughly the view that the entities referred to in the relevant theories exist objectively and mind-independently, and the theories are true or approximately true; and pluralism is roughly the view that different (apparently incompatible) accounts of the phenomena in question are equally acceptable.

2   For example, pluralistic realism about species has been defended by Dupre (1981), Kitcher (1984) and Boyd (1999).

pluralism: model pluralism is true of what they call population-structured evolution, but it does not apply to the case of the evolution of superorganisms. I argue that local pluralism of this kind is also unstable, leading to a combination of realism and conventionalism that is hard to sustain. A genuine model pluralism must, I argue, be global in nature.

My conclusion is that one must choose whether to be a realist or a pluralist with respect to the units of selection. The question of which we should choose is a further question that I do not consider. I also do not argue that the failure of attempts to combine realism and pluralism about the units of selection entails that similar attempts in other areas, for instance with respect to species, must also fail. I am in fact sceptical about these other attempts, but do not claim that my arguments in this article, which are quite specific to the units of selection issue, generalise to them.

Pluralism(s) and realism about the units of selection

In this section I will say more about the two kinds of pluralism about the units of selection. Sober and Wilson (2002, 529–530) put it this way:

> First, there is a pluralism of perspectives, wherein one can represent a single process in different equally compatible ways. Second, there is a pluralism concerning what happens in nature – different types of selection process can and do occur in nature, and they occur in different combinations. Whereas the first type of pluralism brings with it a kind of conventionalism, the second involves a kind of realism. It is a matter of convention which of two equivalent models you use to describe the evolution of a given trait. But it is a factual matter, not a matter of convention, whether the process of group selection occurred in the evolution of that trait, and the same goes for whether individual or genic selection played a role as well.[3]

As Sober and Wilson note, model pluralism, or "neckercubing"[4] has generally been thought to entail anti-realism or conventionalism about the units of selection. For instance, Sterelny and Kitcher (1988) endorse the position that there are typically several equally correct ways of representing or modelling a selection process – "There is no privileged way to segment the causal chain and isolate the (really) real causal story…there are alternative, maximally adequate representations of the causal structure of the selection process" (1988, 358), calling this a kind of "instrumentalism", according to which certain putative entities, "targets of selection", "do not exist" (1988, 359; see also Kitcher, Sterelny and Waters 1990; Waters 1991; 2005; Godfrey-Smith and Kerr 2002; Kerr and Godfrey-Smith 2002; Kitcher 2004; 2008; Godfrey-Smith 2008). Sterelny and Kitcher are following Dawkins (1982), who defends the gene's eye view in conventionalist terms as the most illuminating heuristic or perspective on evolution, not as factually correct: "The vision of life that I advocate…is not provably more correct than the orthodox view…They are equally correct" (Dawkins 1982, 1).[5]

These theorists can be interpreted as endorsing conventionalist-pluralism (model pluralism) while denying realist-pluralism (unit pluralism). Their position would be that one cannot argue that, on the one hand, it is a nonfactual matter of convention whether, say, a selection process should be modelled by assigning fitness values to groups, or to individuals in the context of a certain population structure, and on the other hand, that there is a fact of the matter about whether or not group selection has taken place.

Thus, model pluralism has been understood as an anti-realist position by model pluralists. But it has also been so understood by realists. Gould (2002, 640; emphasis in original), for instance, rejects Dawkins' position on the grounds of its anti-realism:

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3   See also Sober and Wilson (1998). They seem to confuse the two kinds of pluralism on p. 100.

4   Okasha (2006) rightly notes that model pluralism can be either local (pertaining only to particular selective episodes) or global (applying to the units of selection in general), but we will see there are reasons to question the coherence of local pluralism.

5   Within the model pluralist camp, "pluralistic gene selectionists" such as Sterelny and Kitcher (1988) argue that the fact that changes in gene frequencies provide a common currency for representing all forms of evolutionary change gives us a pragmatic reason for privileging the genic perspective. For discussion, see Lloyd (2005), Lloyd, Dunn, Cianciollo, and Mannouris (2005) and Waters (2005).

> Among professional philosophers, such thinking goes by the name of *conventionalism*…an argument that frameworks of explanation [are judged as] more or less preferable by such nonfactual criteria as depth of insight provided or satisfaction gained in understanding.

He goes on to argue that the genic and individual-level approaches are not equally correct perspectives on the same facts. Firstly, they each make distinct factual claims about nature, which are capable of being empirically verified or refuted, and secondly, the genic approach, as understood by Dawkins, does not even count as a potential answer to the units of selection problem, since it has nothing to say about the causes of selection, and confuses causation with bookkeeping. Elsewhere, he indicates the general philosophical basis for his opposition to conventionalism:

> I espouse a…rather old fashioned "realist" view that an objective factual world exists "out there"…That is, either Darwin is right and effectively all natural selection occurs at the organismic level, or the hierarchical theory is right… (Gould 2002, 31).

Gould is endorsing realist-pluralism while rejecting model, or conventionalist-pluralism. Thus he would agree with the model pluralists mentioned above that these two pluralisms are incompatible. Turning their *modus ponens* into a *modus tollens*, Gould would argue that if there is a fact of the matter about whether group selection or individual selection or gene selection (or some combination of these) has been operating in a given case, it follows that it cannot be purely a matter of convention which selection-perspective we use to model the process.

   Despite the broad consensus that model pluralism and unit pluralism are incompatible, Sober and Wilson (1998) say they endorse both kinds of pluralism (see also Sober 2011). I turn now to consider their arguments for this surprising view.

Can realism and pluralism be combined? Sober and Wilson on reconciling model pluralism and unit pluralism

Extending the pluralistic family

Model pluralists hold that the same selection process can typically be represented from the standpoint of more than one level in the biological hierarchy. But they differ in the number and kinds of models they are willing to allow. Dawkins (1982), for instance, accepts that the individual-level perspective and the genic-level perspective are equally correct ways of describing the same facts. But he suggests that other perspectives, such as the group and species-level ones, are not to be thought of in the same way: rather than representing alternative *perspectives* on the reality described by the genic and individual perspectives, he argues that they describe different *processes* in nature – processes the operation of which he suggests we lack compelling evidence for. Thus these perspectives are factually distinct from the equivalent genic and individual perspectives.

   This assessment has been strongly contested by Sober and Wilson (1998). Sober and Wilson offer a general defence of group selection as an explanation for altruism, based on their "trait group" model. A trait group is a group of individuals in which the possession of some trait by a member of the group affects the fitness of all and only the other members of the group (Sober and Wilson 1998). The concept is connected to the idea of "common fate". The cells in an organism share a common fate, so an organism is an interactor. Similarly, some groups of organisms, such as a group of beavers, share a common fate, hence beaver groups are interactors. However, groups share a common fate only with respect to particular traits. If beavers work together to build and maintain their dam, the relevant traits will have a common effect on all the beavers in the group. Thus the beavers form a "trait group" with respect to their dam-building trait (Sterelny and Griffiths 1999).

   Mirroring Dawkins' defence of the gene's eye view, Sober and Wilson do not, at least at first glance, assert the factual superiority of group selection over its rivals. Rather, they claim that Dawkins' and others' limited pluralism should be extended to include the group-level perspective. The opposition to group selection theory has been based, they argue, on a "massive confusion between process and perspective" (1998, 98). They argue that the gene's eye view, inclusive fitness theory, and group selection are different, equally legitimate perspectives on or ways of looking at

the same phenomenon, i.e. evolution in group-structured populations (Sober and Wilson 1998). The different perspectives are not

> competing theories that invoke different processes, such that one can be right and the others wrong. They are simply different ways of looking at the same world. When one theory achieves an insight by virtue of its perspective, the same insight can usually be explained in retrospect by the other theories (ibid., 98).

They are here describing the way the relation among the various *non*-group selection theories has been interpreted – they agree with the assessment, only wishing to include group selection in the "happy pluralistic family" (ibid.). They also argue, echoing Dawkins' pragmatic defence of the usefulness of the gene's eye view, that

> in practical terms, a perspective deserves to exist to the extent that it provides new insights, even if they can be accounted for in retrospect by other perspectives. Multilevel selection theory allows us to see possibilities that have not been obvious from other perspectives (ibid., 118).

In the conclusion of their book they argue that multilevel selection theory enables us to "see the whole [evolutionary] stage" rather than being narrowly focused on a small part of it (ibid., 332). That is, it is a powerful "way of seeing" with which to view and make sense of facts that are nonetheless accessible (in perhaps less vivid and more obscure and confusing form) from other standpoints. So it is distinctive and valuable not in virtue of its uncovering of new facts, but in virtue of the orientation it provides on facts that can be represented from alternative points of view.

Sober and Wilson on broad individualism

Given this professed pluralism, Sober and Wilson's attitude towards the perspective on the units of selection problem known as "broad individualism" is initially surprising. Broad individualism reinterprets (putative) instances of group selection as instances of context- or frequency-dependent individual (or genic) selection, with groups interpreted as part of the environment. (Frequency-dependent selection occurs when the fitness of a phenotype [or genotype] is contingent on the frequency of that phenotype [or genotype] in the organism's environment.)

There are cruder and more sophisticated versions of broad individualism (see Sterelny and Griffiths 1999). Given Sober and Wilson's model pluralism, we would expect them to accept that the best, most sophisticated broad individualist models are just as legitimate as group selection models as accounts of evolution in group-structured populations. This is the view that other model pluralists have taken. They (the others) assert that the two perspectives agree on what the facts are, and are simply using alternative frameworks to represent those facts. But this is not the view of Sober and Wilson. They reject *all* individualist redescriptions of group selection processes on the grounds that they all commit the "averaging fallacy" (Sober and Wilson 1998, 31–35).[6]

Consider two populations made up of altruists and non-altruists. In one, altruists make up the majority; in the other, they are the minority. Non-altruists out-compete altruists within each group, but the altruist group out-competes the non-altruist group, meaning the former grows in size relative to the latter. In the later generation, there will be more non-altruists relative to altruists *within* each group, but paradoxically, because the altruist group leaves more descendants than the non-altruist group, *overall* the number of altruists will have risen relative to the number of non-altruists.

In a case like this, altruists are, averaged across both groups, fitter than selfish individuals. (The average altruist is fitter than the average selfish individual across both groups, even though within each group the average selfish individual is fitter than the average altruist). The averaging fallacy is the attempt to infer from this that altruism evolved by individual selection. The problem with

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6   Dugatkin (a model pluralist) predicts (2002) that multilevel selectionists like Sober and Wilson will object to Kerr and Godfrey-Smith (2002) that the individualist framework they discuss cannot tell us about the causes of selection processes, that is, it too falls foul of the averaging fallacy. In fact, Sober and Wilson (2002) accept that Kerr and Godfrey-Smith do not commit the crudest form of the averaging fallacy; but they argue that their analysis does not make it clear that whether group selection has taken place is always a question of fact, not convention.

the averaging approach is that "it fails to identify the separate causal processes that contribute to the evolutionary outcome" (Sober and Wilson 1998, 32). Altruistic traits typically evolve because the power of group selection, which favours altruism, has overcome the power of within-group individual selection, which favours selfishness. The (correct) statement that altruists had a higher average fitness than selfish individuals, and therefore altruism evolved, tells us nothing about the causal processes that led to that outcome. (The averaging approach is analogous, Sober and Wilson argue, to focusing only on net, rather than component forces, when analysing why an object has the trajectory it does.)

Sterelny and Griffiths (1999) have argued that while some crude forms of individualism commit the averaging fallacy, more sophisticated versions are not vulnerable to the charge. They offer the following example. Imagine a group of beaver populations. The beavers vary with respect to their propensity to contribute to collective dam-building endeavours. (Those inclined to help build dams are altruists, those who would rather not are non-altruists.) Suppose further that dams have a positive effect on beaver fitness. According to the averaging approach that Sterelny and Griffiths consider, there are two components of beaver fitness:

1. Their social environment – whether they live with other altruists (dam builders) or selfish individuals (those disinclined towards cooperative building projects). Beavers who live alongside altruists get a boost to their fitness as a result; those who live with less-motivated individuals incur a resultant fitness cost.

2. Their role within their group – selfish individuals, in both mainly altruist and mainly selfish groups, are fitter *in their group* than altruists. (They enjoy the benefits of the dam-building efforts of the altruists, without incurring the cost of contributing themselves.)

Given this, it follows that the fittest beavers overall will be defectors (selfish individuals) living in altruistic (dam-building) groups: they get a boost to their fitness from living with altruists (the first component) and, as a consequence of their role in the group as defectors, get a further fitness boost from not having to contribute to the building (the second component). The least fit overall will be altruists living in selfish groups.

So there are two kinds of groups: mainly altruist and mainly selfish groups. Everyone in the altruist group gets a fitness boost as a result of the altruistic character of their group; within that group, selfish individuals are fitter than altruists. Everyone in the selfish group incurs a fitness cost as a result of the selfish character of their group; within that group, selfish individuals are fitter than altruists. Sterelny and Griffiths argue that in this scenario altruists can be, on average, fitter than selfish individuals (and thus altruism can evolve by individual selection) because *altruists are likely to live with other altruists and selfish individuals are likely to live with other selfish individuals*. So the very fittest type overall – the defector in an altruist group – is relatively rare. By definition, most who get a fitness boost from living in an altruistic group are themselves altruists: few selfish individuals get this boost. And, similarly, by definition, most who incur the fitness cost of living in a selfish group are themselves selfish. Few altruists incur this cost.

We have four types: (a) selfish individuals in altruist groups; (b) altruists in altruist groups; (c) selfish individuals in selfish groups; (d) altruists in selfish groups. (a) is the fittest type, followed by (b), followed by (c) followed by (d). The most common types will be (b) and (c). (a) and (d) will be rare. This means that, despite the fact that the fittest type is selfish and the least fit type is altruistic, *most altruists will be fitter than most selfish individuals*. Most altruists will be (b)-type altruists, and most selfish individuals will be (c)-type selfish individuals. And (b)-types are fitter than (c)-types. So *on average* altruists will be fitter than selfish individuals.

Just as with the simpler individualist-averaging approach, we have the result that altruists are fitter on average than selfish individuals across both groups, even though within each group selfish individuals are fitter than altruists. But the difference lies in the fact that in the more sophisticated model, the result is obtained by taking into account the character of one's group as a determinant of fitness. Thus it respects the importance of the division of the population into groups: it is able

to represent the nature of the population-structure and its role in the evolution of altruism – it does not bleach out this information the way the cruder individualist-averaging approach does. Thus it is, arguably, an individualist-averaging approach which is not vulnerable to the kind of charge Sober and Wilson level against other forms of this approach. Sterelny and Griffiths suggest that it gives us all that we could want, in terms of retaining information about the role of groups in the evolution of altruism, within an individualist framework, that is, without bringing in group fitness. "Both views recognise the importance of the division of the population into groups, and both recognise that an organism's fitness depends both on the character of the group it inhabits and its own character" (Sterelny and Griffiths 1999, 169).

Model pluralists ought to accept Sterelny and Griffiths' claim that the more sophisticated averaging approach and Sober and Wilson's group selectionist approach are equivalent – that they agree on all the biological facts, and are alternative perspectives on the same reality. But Sober and Wilson do not regard them as equivalent: they hold that all versions of broad individualism are necessarily inadequate *as explanations of the causal processes at work*, even if they are predictively useful. The issue here is that Sober and Wilson are committed to *realism* about the units of selection. Yet, acknowledging the equivalence of the two perspectives inevitably leads to anti-realism. The two perspectives differ with respect to: the level at which selection is acting; the entities that benefit from the relevant traits (more precisely, the entities whose benefit is causally implicated in the selection process); the entities that are the relevant interactors; the entities that are the relevant bearers of fitness; and the entities for whom adaptations exist. Put in Wilson's terms (2005), they identify different "agents of selection". If the perspectives are equally correct, then these cannot be factual differences, and anti-realism with respect to the relevant entities is unavoidable.[7]

It is their commitment to realism that lies behind Sober and Wilson's (1998) protest that despite its greater sophistication, Sterelny and Griffiths' individualist approach still lacks the conceptual resources required to answer the "basic question" about group selection: "Can traits evolve by benefiting whole groups, despite being selectively neutral or disadvantageous within groups?"[8] The broad individualist approach gives us a means, they argue, of cataloguing the outcomes of selection processes, and in some cases of predicting which traits will evolve. But it does not give us any information about the causes of those processes. Kerr and Godfrey-Smith note that[9]

> Sober and Wilson agree that it is possible to devise an evolutionary model of altruism that gets the mathematical results right, while only assigning fitnesses to individuals. However, Sober and Wilson insist that only a multilevel description of these systems can yield real understanding of the evolution of altruism, because only a multilevel description is faithful to the causal structure involved…The individualist perspective, for Sober and Wilson, is only predictively, and not explanatorily, adequate[10] (2002, quoted in Sober and Wilson 2002, 530).

Sober and Wilson's claim that the different perspectives are predictively, but not explanatorily, equivalent, as only the multilevel perspective is "faithful to the causal structure involved" recalls

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7   Wilson is a realist about biological agency, and is "suspicious of [model pluralists'] ways of deflating prima facie ontological disagreement" (2005, 222). He views "model pluralism…as a mistaken way to spell out the intuition that there may not be a determinate answer to the question of just which level is 'the' level at which selection occurs in any particular case" (2005, 231). Thus he would presumably deny the equivalence in question. He raises some problems for technical frameworks that have been proposed for translating between the multilevel and individualist perspectives. As his arguments, and those of the theorists he is challenging (Godfrey-Smith and Kerr) demonstrate, the question of the equivalence or non-equivalence of these perspectives is not just a question of whether they intuitively agree on the important biological facts, but also depends on the technical mathematical details involved. (Although one of Wilson's points [2005] seems to be that it is equally mistaken to regard this issue purely in mathematical terms.) I do not propose to go into these details, since my purpose is not after all to defend model pluralism, but rather to argue that model pluralism must be an anti-realist pluralism.

8   The natural response to this objection is that the basic question has realist assumptions built into it: it assumes that there is a determinate, objective answer to the question of which entities benefit from which traits. Thus the model pluralist is not obliged to take the question seriously.

9   It should be noted that Sober and Wilson (2002) do not entirely accept this characterisation of their view (but neither do they think that it is a major distortion of it).

10   See also Barrett and Godfrey-Smith (2002).

the realist position in the philosophy of science that we can choose between empirically equivalent but incompatible theories on the basis of which provides the best *explanation* of the phenomena. Their position seems to be that while the multilevel perspective and the individualist perspective are empirically equivalent, the former is preferable to the extent that it provides a superior explanation of the relevant phenomena. The opposing, model pluralist view can then perhaps be assimilated to the empiricist position that explanatory power is not an epistemic virtue of a theory, but is at best a pragmatic virtue (van Fraassen 1980), and therefore cannot supply reasons to think a theory true independent of empirical adequacy. All that matters is whether a theory "saves the phenomena". The empiricist does not necessarily want to suggest (as the logical positivists did) that in every case (strongly) empirically equivalent theories are ipso facto factually equivalent; but this is perhaps the position the empiricist ought to take with respect to the units of selection issue. And just as van Fraassen allowed that non-empirical criteria could help to determine which theory is "accepted" (as opposed to believed), so long as these are understood as pragmatic and not truth-relevant criteria, the model pluralist suggests that perspectives are to be adopted on purely pragmatic grounds. Another way of interpreting Sober and Wilson's view is that the perspectives are empirically equivalent but *metaphysically* distinct (compare different interpretations of quantum mechanics). They have the same empirical consequences, but only one gets the metaphysical facts right – only one is faithful to the actual causal structure in the world. But I now set these interpretive issues aside.

Pluralism in name only?
Sober and Wilson are willing to accept that broad individualism is legitimate as, in a sense, a variant of group selection, but not as an *alternative* to it.[11] That is, it is acceptable as a way of modelling group selection, but not as a rival factual hypothesis about causes. Alternative frameworks "look at the process of multilevel selection in different ways" (1998, 100). If broad individualism is thought of as an alternative to group selection (and a superior one) then they do not accept it. So they are happy for there to be different ways of expressing – different perspectives on – group selection theory; but people are wrong if they think these are alternatives to it.

   So it is not enough simply to ask whether they accept individualism along with group selection. They accept that broad individualism can be a (somewhat impoverished) way of representing a group selection process, or at least, representing the consequences of the process (see Sober and Wilson 1998; 2002). But they do not accept that it is legitimate as an alternative causal story, one that identifies individuals rather than groups as the 'agents' of selection (Wilson 2005). (This is similar to saying genic selection is useful as a way of representing the consequences of a selection process – a way of keeping the books – but not as a way of representing the causes.) It does not have the resources to answer the "basic question". So if the causal facts are that some trait evolved because it benefited groups, that is, as a result of selection on groups (selection on the basis of differential group fitness), the broad individualist approach can at most give an alternative description of these facts, by (artificially) ignoring group fitness, and averaging individual fitnesses in ways that are adequate to the phenomena and generate the right predictions. What the approach does not do, and cannot do, is *deny* the above causal facts, and replace them with a different causal story according to which the trait evolved because it benefited individuals, not groups, that is, as a result of selection acting on individuals, with groups interpreted as part of the individuals' environment (Sober and Wilson 1998).[12]

   Simplifying a little, I take the situation to be as follows. Sober and Wilson believe that in many cases, it is an objective fact that group selection has taken place.[13] It follows that group selection models of such processes are to be treated realistically; the objects, properties, processes and states they quantify over are real and mind-independent: group fitness is real, group-level adaptations

11   Similarly, Sober and Wilson claim (1998) that kin selection theory is a variant of group selection, not an alternative to it. See Birch and Okasha (2014) for a recent discussion of the relationship between kin selection and group selection.

12   They also reject the inference from their pluralism to the conclusion that "everyone has been right and that all of the differences have been merely semantic" (Sober and Wilson 1998, 99).

13   In their response to Godfrey-Smith and Kerr, Sober and Wilson insist that whether group selection has occurred is a question of "fact", and not of "perspective" (2002, 531).

are real, selection acting on groups is real, and group-interactors are real. It also follows that to the extent that there are individualist models of such processes that are empirically adequate, generating the right predictions and getting the mathematics right, they are to be treated instrumentally.[14] Such individualist redescriptions of group selection processes are perhaps useful tools for predicting what will evolve, but they do not accurately represent what the reality of the situation is.[15]

Now given Sober and Wilson's premise that group selection sometimes really takes place, their conclusions about the differing statuses of group selection theory and individualism with respect to such cases are hard to avoid. But, their opponents will ask, what reason do we have for accepting their premise? After all, this premise is precisely what opponents of group selection deny. It could just as easily be argued, they may assert, that it is an objective fact that group selection happens rarely if ever in nature, and that as a result group selection models must be treated instrumentally, and individualist models realistically (see footnote 14). (The anti-realist pluralist argues for a pox on all houses: there are no objective facts about whether *any* kinds of selection "really" happen in nature, and thus *all* selection models are to be treated instrumentally; see below.) Part of the problem is this premise (that group selection sometimes really takes place) is not always made explicit; rather, Sober and Wilson tend to imply that once we have grasped the basic logic of the multilevel selection perspective and the individualist perspective, we will see that the former has a claim to realism and objectivity that the latter lacks. But this begs the question against their opponents, and cannot hope to persuade them.

To return to neckercubing, Sober and Wilson's opponents will surely agree that if neckercubing is right, the different perspectives cannot be invoking different processes, and cannot be factually distinct. But the question is, *which perspective is the one of which the others are variants?* Sober and Wilson insist the apparently non-group-selection perspectives are different perspectives on group selection; but their opponents insist that group selection, if it is legitimate, is just a variant of, say, genic selection,[16] as all non-genic perspectives are just different perspectives on genic selection. Neckercubing/pluralism is more like monism when all alternative views are regarded as legitimate only to the extent that they are different ways of expressing one's favoured view. This is pluralism in name only. Sober and Wilson hold that non-group selection theory is acceptable as an alternative perspective, but wrong if it invokes processes other than group selection; their opponents hold that group selection is acceptable as an alternative perspective (on individual or genic selection), but wrong if it invokes different processes. They may both be nominally neckercubists, but still disagree as strongly as possible about the relations among the different perspectives.[17] The happy pluralistic family is not so happy when *a* insists that *b*'s theory is correct only if it is a variant of *a*'s, and *b* insists that *a*'s theory is correct only if it is a variant of *b*'s.[18]

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14   Compare Sterelny (1996): if trait groups are not real interactors, theories that postulate selection acting on them must be interpreted instrumentally. The entities – trait-group interactors – that the theories quantify over are not objectively real, they are merely "as-if" interactors (or "stance-dependent" interactors). This is Dugatkin and Reeve's position (however, they do not actually say they are not interactors, they say there is no fact about whether they are or not). Sober and Wilson imply on the other hand that if trait groups *are* interactors, theories that *do not* treat them as such must be interpreted instrumentally.

15   "There is nothing wrong with computing the average fitness of genes, or of the phenotypic traits exhibited by individuals, if the goal is to predict what will evolve. But the availability of this procedure does nothing to solve the problem of identifying the units of selection" (Sober and Wilson 1998, 331). Given this, it is surprising that they claim that perspectives other than multilevel selection theory are usually able to explain/account for the insights achieved by multilevel selection theory (Sober and Wilson 1998, 98). If the other perspectives cannot answer the "basic question" about the units of selection, how can they explain/account for the insights offered by multilevel selection theory? How can they access the same facts that multilevel selection theory does, if they are necessarily silent on the question of what the units of selection are?

16   For Dawkins, group selection is an instance of genic selection in as much as groups, like organisms, are vehicles that exist in order to further the replication prospects of the genes that build them. "Group selection is represented [by Dawkins] as a *kind* of gene selection, not an *alternative* to it. Groups are candidate vehicles that genes may construct and exploit for their own advantage" (Sober 1984, 254; emphasis in original). See also Bourke and Franks: "colony-level, group, individual and kin selection are all aspects of gene selection" quoted in Wilson (2005, 13).

17   See Wilson (2005, 231). He notes the "[d]isagreements that lie beneath the surface of the pluralist consensus…" and suggests that model pluralism "stops short of dissolving the disagreement between individualists and multiselectionists".

18   This point is important to keep in mind when considering Sober and Wilson's citing of other theorists whom they claim have come to the same conclusions they have regarding the relationship of group selection to individual and genic selection. They cite an impressively large number of biologists and philosophers who, they contend, are now in agreement with them about these questions. But it is arguable that

A genuine pluralism?

If these pluralisms are only monisms in disguise, what would a genuine pluralism be like? I have argued that the views I am discussing contain an important realist component. As I noted above, Sober and Wilson[19] take it that group selection is a real process, that group selection theory describes real causal structures, and that it is an objective fact that some groups are interactors, and that selection sometimes acts on groups in virtue of differential group fitness, resulting in group adaptations. Inasmuch as these are facts, alternative perspectives are legitimate insofar as they respect these facts, and do not contradict them. As long as they are different ways of describing *these facts*, they are acceptable.

> Nature exists as a nested hierarchy of units, and the process of natural selection operates at multiple levels of the hierarchy. Other frameworks may examine these facts from different perspectives but they are just plain wrong if they deny them as facts (Sober and Wilson 1998, 100).

Recall the distinction between realist-pluralism and conventionalist-pluralism. It is as if Sober and Wilson are saying that we must first put on our realist hats, and decide what the facts of the case in question are, that is, whether *in fact* group selection or individual selection or gene selection has taken place. Once we have answered this question, we can put on our conventionalist hat, and decide which of the several equivalent frameworks would be most helpful in representing those facts. But when we do this, we must remember what the facts are, and must be true to them.

Similarly Dawkins, in some moods, seems to regard it as an objective fact that properties of genes are ultimately causally responsible for all selection processes, that selection acts only on genes, that traits evolve because they benefit genes, and that all adaptations are ultimately adaptations of particular genes. If alternative perspectives are to be acceptable they must respect, and not contradict, *these* facts.

A genuinely pluralist position must, I suggest, be anti-realist with regard to "facts" such as these. Claims regarding the level(s) at which selection is acting, the entities whose benefit is causally implicated in the selection process, the entities that can be interactors, the entities that are the bearers of (causally salient) fitness, and the entities for whom adaptations exist, must be treated instrumentally, rather than realistically. This can lead to a genuine pluralism. Since the theories are not describing objective facts, alternative perspectives do not have to "respect" them – they can be genuinely alternative.

To summarise this section: Sober and Wilson profess to be model pluralists as well as unit pluralists. Yet, they reject the equivalence of group selection and sophisticated broad individualism. This is initially puzzling, but makes sense when we realise that to accept the equivalence amounts to denying the realism of their unit-pluralism. Their unit pluralism, I argued, constrains their model pluralism to such a degree that the latter becomes pluralism in name only: alternative perspectives to theirs are only acceptable if they respect the facts about group selection established by their unit pluralism. A genuine model pluralism must, I argue, reject the realism of unit pluralism; it must be an anti-realist pluralism.

Can realism and pluralism be combined? Sterelny and Griffiths' local pluralism

The second, quite different way in which pluralism has been combined with realism about the units of selection has been in the idea of *local pluralism*. Recall that local pluralists are pluralists about some selective episodes, while global pluralists are pluralists about all selective episodes. Local pluralism has been defended by Sterelny and Griffiths (1999), among others.

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all that unites these theorists is a commitment to pluralism of *some* kind and, as we have seen, such shared surface commitment can mask profound disagreements about the relations among the various theories and perspectives within the pluralist family. For example, they cite Dugatkin and Reeve (1994) as sharing their view but, as the quote from Sterelny above indicated, Dugatkin and Reeve deny that there are facts about whether trait groups are vehicles of selection (interactors), whereas Sober and Wilson are adamant that there are such facts. Dugatkin and Reeve should, I think, be interpreted as (local) anti-realist pluralists, opposed to the realist-pluralism of Sober and Wilson.

19 See Sober and Wilson (2002), where they identify as realists in this sense.

Stance-dependent interactors

Sterelny and Griffiths appear, initially, to be defending a general anti-realist pluralism, as we have seen with their example of beaver altruism. For instance, in their response to Sober and Wilson, they reject the claim that (the sophisticated version of) broad individualism tends to get the causal story wrong in the relevant cases. We have seen that Sober and Wilson assert that broad individualism is only acceptable if it is seen as describing the consequences of a group selection process. Sterelny and Griffiths by contrast see it as correct as an *alternative putative causal hypothesis* that does not involve group selection. Perhaps the key here is that they do not see the facts about group selection mentioned above as *the facts that have to be respected* by rival accounts. Rather, the important facts have to do with the population-structure and its effects on evolution. *These* facts are the relevant ones, and so long as broad individualism respects them, it is acceptable.[20] The "facts" concerning the level at which selection is "really" acting are not to be thought of as facts at all. Thus their position here seems to be consistent with anti-realist pluralism.

In this context, Sterelny and Griffiths (1999) invoke Dennett's theory of the intentional stance, and raise the possibility that a similar instrumentalist/pragmatist approach could apply to the problem of identifying interactors.

> According to this line of thought, it can often be useful to take the "interactor stance" toward beaver families, baboon troops, chimp coalitions, and the like. We can treat these as interactors, but we need not. It is equally legitimate to treat them merely as being composed of interactors (Sterelny and Griffiths 1999, 170–171).

That is, there are no objective facts about whether something is an interactor. This is neckercubing: we can treat something *as if* it is an interactor, if it is useful to do so, but there are no objective facts (that is, facts in the world, as opposed to facts about us and our interests and purposes) that determine whether we are right to do so. The perspective according to which a baboon troop is an interactor and the perspective according to which it is not, but is made up of interactors, are not making incompatible claims about the biological world; the biological facts are neutral between these two perspectives. Thus the two perspectives do not differ in empirical content, and are equally correct (at least as far as the biological facts are concerned; one may be preferable to the other on pragmatic grounds). They are alternative "ways of looking", not competing factual hypotheses.

This is, of course, closely related to neckercubing about the units of selection; treating groups as interactors is associated with group selection, and treating them as being composed of interactors is associated with individual-level selection.

Superorganisms and population-structured evolution

Yet Sterelny and Griffiths say they *reject* the neckercube view of interactors, despite the fact that they endorse the equivalence of group selection and sophisticated broad individualism. This is partly because it follows from the view in question that there is no fact of the matter about whether organisms are interactors, and they think that organisms clearly are interactors (1999, 171). So, at least some things are interactors in an objective sense, i.e. organisms.[21] The authors also argue (1999) that what they call "superorganisms" – termite nests, ant colonies, etc. – are good candidates to be objective interactors. This is because they are more cohesive, structured, and integrated than groups such as baboon troops.[22] Thus Sterelny and Griffiths distinguish between two issues that they suggest have been conflated in the group selection debate: the issue of population-structured evolution, and the issue of the evolution of superorganisms. On the first issue, they take the

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20 Recall: "Both views recognise the importance of the division of the population into groups, and both recognise that an organism's fitness depends both on the character of the group it inhabits and its own character" (Sterelny and Griffiths 1999, 169).

21 We may wonder whether this view of organisms as being objective interactors is compatible with Sterelny's instrumentalism about the gene selection versus individual selection issue. If organisms are objective interactors, presumably they must be "targets of selection". This seems to conflict with the explicit anti-realism espoused in Sterelny and Kitcher (1988).

22 Sterelny (1996) argues that aside from their greater cohesion and integration, superorganisms are distinguished from "mere" trait groups by the fact that the fitness of the group can vary independently of the fitness of the individual organisms within it. In Okasha's terminology, superorganism evolution is an example of multilevel selection 2, not multilevel selection 1; see below.

neckercubist line: the different perspectives (group selection and sophisticated broad individualism) are equally correct ways of describing the same facts. On the second, they reject neckercubing: just as it is an objective fact that organisms are interactors, it seems to be an objective fact that some higher-level superorganisms are interactors. The perspective according to which they are not is therefore not equally legitimate – it is factually incorrect. This is the sense in which they are attempting to combine realism with pluralism. They are realists about the interactor-status of some entities, and pluralists about the interactor-status of other entities.

But it is hard to see how these views can be reconciled. On the face of it, if there are facts about whether organisms and superorganisms are interactors, presumably there are facts about whether baboon troops and the like are interactors. But then group selection and broad individualism would not be equivalent with respect to the latter, as group selection has it that these are genuine interactors, and broad individualism that they are not. If there is a fact of the matter about whether or not they are interactors, either group selection or broad individualism must be right, and the other wrong. To reconcile their two views, Sterelny and Griffiths must hold that there are facts about whether organisms and superorganisms are interactors, but not facts about whether more temporary groupings and coalitions are, and that would be an odd position. It does not help to say that the less-cohesive groups are not really interactors, but can be treated *as if* they are, and therefore that group selection is acceptable as an account of their evolution, since the debate is precisely over the question of whether they are *really* interactors or not; if they are not (and are merely "as if" interactors), the broad individualists are right and the group selectionists are wrong.[23]

Similarly, Sterelny and Griffiths argue that there is often no objective, clear boundary separating putative adaptive unit from surrounding environment, and that this supports pluralism with respect to such cases. We can "boundary-shift" (Sterelny and Griffiths 1999): the question of where we draw the boundary separating adaptation and environment (and thus whether the collective-level or individual-level perspective is correct) is a non-factual matter of convention. This is clearly an argument against treating these cases as group selection in an objective sense. But it is not obvious that it supports pluralism as opposed to broad individualism with the group perspective in play merely as an instrumentalist option. If "baboon troops…do not have determinate phenotypes" (Sterelny and Griffiths 1999, 176), it follows that baboon troops are not objective (but at best stance-dependent or "as-if") interactors, and that the perspective according to which selection acts on baboon troops must be interpreted instrumentally. But insofar as baboons themselves have determinate phenotypes, the individualist-contextualist perspective may be interpreted realistically.

Sterelny and Griffiths (1999) note that the "interactor-stance" view can be expressed as the claim that there is no objective count of interactors. The objection I am pressing can then be expressed as follows: presumably either there is an objective count of interactors, or there is not. No intermediate position seems possible. It might be suggested that an intermediate position could be that there is a count of interactors, but it is vague; some entities in nature can be unambiguously classified as interactors, and some can be unambiguously classified as non-interactors, but others, such as temporary trait-groups, are borderline cases, possessing some features characteristic of interactors but lacking others (Okasha 2006). (Compare borderline cases of organisms, such as the famous siphonophores; see Sterelny and Griffiths 1999).

This vagueness approach is realist in a way that the model pluralist approach is not. Whatever the interactor-status of the trait-groups is, on this view, they possess it absolutely and objectively, independently of all perspectives. So even if their status is intermediate between being interactors and being non-interactors, this is an objective and perspective-independent fact.[24] The vagueness approach does entail that sometimes there is no fact of the matter about interactor-status, but it is a

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23   See Wilson (2005): he suggests that Sterelny holds that group selection only takes place when superorganisms are involved, because less cohesive trait groups do not count as interactors. When the trait groups are not interactors, broad individualism is more adequate than group selection. However, Sterelny and Griffiths (1999) do not privilege broad individualism over group selection for trait groups that are not superorganisms.

24   Similarly, for Gould (1985), siphonophores are intermediate between organisms and colonies, so their status is indeterminate, but it is an absolute, objective, perspective-independent fact that their status is indeterminate.

realist way of there being no fact of the matter. There is indeterminacy, but that is because the world is objectively indeterminate.[25]

I take it that this is not Sterelny and Griffiths' position (even though it may appear to be). They hold that group selection models and individualist models of trait-group-structured evolution can both be maximally adequate. The former posits trait-group-interactors, while the latter does not. It would be a strange way of interpreting this pluralist position to say that it involves the view that trait groups are borderline cases of interactors. Either we can adopt the group-selection approach, in which case they are unambiguous interactors, or we can adopt the individualist approach, in which case they are unambiguous non-interactors. There is seemingly no neutral standpoint, independent of these approaches, from which to assert that they are vague interactors. The term "interactor" is highly theory-laden. Interactors are the selectively salient entities posited as agents of selection by a particular theory/model. We have no way of identifying interactors independently of such a theory/model. In particular, we have no way of stepping outside the theories/models to determine that, as a matter of objective fact, certain entities in nature possess some, but lack other, properties associated with interactors. Sterelny and Griffiths are model pluralists about the entities and processes in question; they are not, or ought not to be, advocating the realist-vagueness view. The reason they nonetheless seem to be attracted to the realist-vagueness view may have to do with different ways of understanding what interactors are. If we think of interactors as *selected entities*, it is hard to see how there could be borderline cases: either selection has acted on an entity or it has not – it is not clear what it could mean to say that an entity is intermediate between being selected and not being selected. On the other hand, if we focus on interactors as *cohesive, integrated objects that actively interact as wholes with their environment*, intermediate cases appear more plausible: trait groups perhaps satisfy this description only to a certain extent (i.e. to a lesser extent than say ant colonies, but more so than unambiguous non-interactor groups), making them borderline cases of interactors. Given that a major reason Sterelny (1996) offers for thinking of ant colonies etc. (superorganisms) as objective interactors is that they are more cohesive, structured and integrated than "mere" trait groups, he may, to that extent, be open to the possibility of borderline interactors, as clearly cohesion, integration, and so on, are matters of degree.[26] We face the same problem, however: according to model pluralism, trait groups can be treated as interactors – in which case they are metaphysical individuals, assuming the widespread view that interactors, i.e. Darwinian individuals, are metaphysical individuals – or as non-interactors, in which case they are not metaphysical individuals. One can adopt one or other of these positions, but one cannot, I claim, step back from them both and declare that the entities in question are borderline interactors in an objective sense, and hence borderline metaphysical individuals in an objective sense. Model pluralism and vagueness-realism are not compatible.[27]

It is possible that their pluralist position is a kind of contextualism, according to which whether a trait group is an interactor is relative to a context of inquiry. But presumably they do not think organisms and superorganisms are interactors relative to context: they are interactors in an absolute sense, they argue. So we face the same problem: surely either interactor-status is absolute, or relative to a context. It is odd to say some things are interactors in an absolute sense, while others are interactors only relative to a context.

Sterelny and Griffiths appear to be committing themselves to a problematic view about the reality of the units or levels or selection. If superorganisms are genuine interactors, it follows that group selection is a factually correct account of their evolution, and rival accounts are factually

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25 Boyd (1989) suggests that the existence of this kind of vagueness and indeterminacy is in fact a prediction of scientific realism (at least, given the homeostatic property cluster account of natural kind definitions that he defends), contra Dummett (1978), who argues that the acceptance of vagueness with regard to some domain, and the resultant failure of bivalence for sentences quantifying over elements of that domain, constitutes anti-realism about that domain.

26 Indeed Sterelny (1996, 578) says: "Though of course there will be borderline cases, I suggest the distinctions between population-structured evolution and superorganism evolution are quite robust". But note that the borderline cases he is referring to here are not intermediate between interactors and non-interactors; rather, they are intermediate between objective interactors (superorganisms) and stance-dependent (context-relative) interactors (mere trait-groups).

27 Thank you to an anonymous reviewer for encouraging me to clarify this issue.

incorrect. Thus they reject anti-realist pluralism in this instance: it is a fact that selection acts on superorganisms. Yet they are in favour of anti-realist pluralism with respect to temporary trait groups: there are no facts about whether selection really acts on these entities or pseudo-entities. But it is hard to make sense of this local pluralism about the units or levels of selection. One would have thought that talk of these units or levels is either to be treated realistically across the board, or treated instrumentally/metaphorically across the board. The local pluralist's attempt to defend realism in some cases and instrumentalism in others is of doubtful coherence (and it conflicts with the across-the-board instrumentalism – global pluralism – that Sterelny[28] at least has defended elsewhere). I recognise that there may be objections that I am begging the question here – am I not just asserting that local pluralism is false? But I think it is important to appreciate how prima facie puzzling local pluralism is as a metaphysical claim, at least when it involves a combination of realism and pluralism within the one domain. If, for instance, someone were to claim to be a local pluralist about the domain of fundamental physical entities, claiming that while electrons exist as a matter of objective, mind-independent fact, and electron theory should be regarded as true or approximately true, there is no fact about whether protons exist, as proton theory is merely an instrument for predicting and systematising the observable phenomena, we would be hard-pressed to know what to make of such an odd hybrid position. As far as I am aware, there are no local pluralists of this kind. It is not clear to me that local pluralism about the units of selection is a more coherent position.

Local pluralism and the MLS1-MLS2 distinction

Sterelny and Griffiths' distinction between population-structured evolution and superorganism evolution maps onto an important distinction between the two types of multilevel selection, multilevel selection 1 (MLS1) and multilevel selection 2 (MLS2), that have sometimes been conflated in the units of selection literature (Damuth and Heisler 1988; Okasha 2006). In group selection of the MLS1 variety, the focus is on changes in the prevalence of types of *individuals* over time. Group selection of this type leads to the evolution of properties of individuals (such as altruism), not groups, and while it is groups (as well as individuals) that are selected, and are the bearers of fitness, it is the individuals that reproduce and pass on their traits. The fitness of a group in MLS1 is defined as the average fitness of the individuals composing the group. In MLS2, the focus is on both changes in the prevalence of types of individuals, and changes in the prevalence of types of groups, over time. Groups, as well as individuals, reproduce, and pass on their traits to offspring groups. In MLS2, group fitness is independently defined in terms of number of offspring groups, and can vary independently of the fitness of the individuals that compose the groups (Okasha 2006). The focus of the disputes about broad individualism discussed in the second section was entirely on MLS1 cases.

By population-structured evolution Sterelny and Griffiths appear to mean MLS1, and by superorganism evolution they appear to mean MLS2. Is their combination of pluralism and realism (pluralism about population-structured evolution and realism about superorganism evolution) *built into* the MLS1-MLS2 distinction? Okasha (2006) rejects the claim, made by some, that MLS1 is not really a form of group selection at all (although his argument for this – that if MLS1 was not a form of group selection, those who offer MLS1 models would all be wrong in thinking of them as group selection models – is perhaps a little weak). But it does not follow that he advocates realism about MLS1-type group selection; he may, in the spirit of Sterelny and Griffiths' pluralism, regard the view that MLS1 models are group selection models as *one acceptable way* (but not the only acceptable way) of understanding them.[29] And indeed his claim that in MLS1 models groups are

28   For example in Sterelny and Kitcher (1988). However, Sterelny's own view (1996, 571) may be that the position defended in this article was a limited (local), rather than an across-the-board (global) pluralism/instrumentalism. So he notes that the "focus" of the earlier paper was "gene vs. individual organism selection, not group selection". Nevertheless, a number of formulations in the earlier paper do strongly suggest a generalised pluralism/instrumentalism (Okasha 2006). For example: "Monists err…in positing entities, 'targets of selection', that do not exist" (Sterelny and Kitcher 1988, 359). Presumably Sterelny now accepts that some things (organisms and superorganisms) are targets of selection.

29   Okasha (2006) does claim that it is a fact whether MLS1 has happened, as this is a causal process in nature. This suggests realism, but it

fundamentally part of the environment of the individuals which are the primary focus of the models, suggests that he would accept that MLS1 "group selection" is redescribable in terms of population-structured individual selection, which is in line with Sterelny and Griffiths' pluralist view.[30]

Plainly Sober and Wilson could only accept that the type of group selection they defend is of the MLS1 variety if this does not rule out a realist interpretation of it, as they are, as we have seen, committed to such an interpretation. If the MLS1-MLS2 distinction entails that MLS1 group selection is only group selection in a neckercube (non-realist) sense, they would have to reject the distinction. But, although it is a little hard to tell from Okasha's discussion, I take it he does not hold that the MLS1-MSL2 distinction has built into it the neckercube interpretation of MLS1 selection. As we have seen, according to Okasha, in MLS1 models, while groups are selected, and are bearers of fitness, they do not reproduce, and the effect of selection on them is only that they contribute a certain number of individuals with certain adaptive traits to future generations, not that they themselves reproduce as groups, passing on group traits. But if it is an objective fact that groups are being selected on the basis of differential group fitness, and the *cause* of the differential production of individuals by different groups is that some groups are out-competing other groups as adaptive units, it follows that groups are interactors and (in Wilson's [2005] terms) agents of selection, in an objective sense.[31] Such, at least, would be Sober and Wilson's view. Thus, MLS1-type group selection can, it seems, be understood as group selection in a realist sense. It follows that merely accepting the MLS1-MLS2 distinction does not automatically commit one to accepting Sterelny and Griffiths' combination of realism and pluralism. If one accepts that distinction, one *may* – at least as far as the distinction is concerned – be a realist about MLS2 and a neckercubist about MLS1; but equally one may be a realist about both, or a neckercubist about both.[32] The distinction itself doesn't settle these questions.

Contrasting the two types of pluralistic realism
Sterelny and Griffiths' attempt to combine realism and pluralism is very different from Sober and Wilson's. Sober and Wilson are realists about all selective episodes – this is their unit pluralism, and it applies to both MLS1 and MLS2-type cases – but wish to combine this with a pluralism of perspectives on the facts about multilevel selection (at least for MLS1 cases that they are interested in, such as the evolution of altruism). Sterelny and Griffiths, by contrast, are realists only about MLS2-type cases of group selection; they defend conventionalism about MLS1-type cases. Thus they do not attempt to combine realism and model pluralism about the *same* selective episodes: they are full-blooded realists about some episodes, and full-blooded conventionalists about others.

Conclusion
I have examined two quite different attempts to combine realism with pluralism about the units of selection. Sober and Wilson profess to advocate model pluralism, but they reject broad individualist redescriptions of instances of group selection as anything more than alternative perspectives on the group selection "facts". Their commitment to realism prevents them from accepting broad individualism on its own terms (as a putative causal hypothesis), and this, I suggested, undermines their claim to be supporting a genuine model pluralism. Once we accept realism about particular selective episodes, any pluralism of perspectives on those episodes must be pluralism in name only. A genuine model pluralism would have to be an anti-realist pluralism.

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does not speak to the issue of realism as I have focused on it. Even if we accept that MLS1 has really happened as a matter of objective fact, the question remains, *what is it* that has happened? *What is* MLS1? Real group selection, or just individual selection? Or a *something* that can be described either as group selection or individual selection?

30   Okasha (2006) notes that in MLS1 cases, but not MLS2 cases, pluralism is theoretically possible, as only with MLS1 cases are individualist redescriptions possible. But he does not say pluralism is correct, just that it is *possible.*

31   Lloyd (2017) suggests that "under MLS1, the lower level particles are the interactors as well as the replicators, while in MLS2, both the upper level collectives as well as the particles are interactors". If group selection requires groups to be interactors, it would follow that MLS1 is not real group selection. If we think it is real group selection, groups can be interactors in MLS1 as well as MLS2.

32   As I noted above, Okasha denies that pluralism is possible for MLS2-type cases.

Sterelny and Griffiths defend model pluralism with respect to population-structured evolution, and realism with respect to the evolution of superorganisms. This leads to the view that in some cases there is a fact of the matter about which entities count as interactors, and in other cases there is not. This, I argued, is a metaphysical picture of the natural world that is hard to make sense of. Global realism about interactors and the units of selection (such as Sober and Wilson defend) is a coherent view (whether or not it is correct), and global anti-realism about interactors and the units of selection (such as Kitcher, and Sterelny, and Waters defend) is a coherent view (whether or not it is correct). But Sterelny and Griffiths' half-way house between these positions is not, I argued, a stable resting place.

Thus, neither the combination of realism and pluralism about *particular* selective episodes, nor the combination of realism and pluralism across *different* selective episodes can be accepted. It seems that when it comes to the units of selection, one must choose whether to be a realist or a pluralist; one cannot be both.[33]

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References
Barrett, M., and P. Godfrey-Smith. 2002. "Group Selection, Pluralism, and the Evolution of Altruism." *Philosophy and Phenomenological Research* 65(3): 685–691. doi:10.1111/j.1933-1592.2002.tb00233.x

Birch, J., and S. Okasha. 2014. "Kin Selection and its Critics." *Bioscience* 65(1): 22–32. doi:10.1093/biosci/biu196

Boghossian, P. A. 1990. "The Status of Content." *Philosophical Review* 99 (2): 157–184. doi:10.2307/2185488

Boyd, R. 1989. "What Realism Implies and What it Does Not." *Dialectica* 43(1-2): 5–29. doi:10.1111/j.1746-8361.1989.tb00928.x

Boyd, R. 1999. "Homeostasis, Species, and Higher Taxa." In: *Species: New Interdisciplinary Essays*, edited by R. A. Wilson, 141–186. Cambridge, Massachusetts: MIT Press.

Damuth, J., and I. L. Heisler. 1988. "Alternative Formulations of Multi-level Selection." *Biology & Philosophy* 3(4): 407–430. doi:10.1007/BF00647962

Dawkins, R. 1982. *The Extended Phenotype*. Oxford: Oxford University Press.

Dennett, D. 1987. *The Intentional Stance*. Cambridge, Massachusetts: MIT Press.

Dugatkin, L. 2002. "Will Peace Follow?" *Biology & Philosophy* 17 (4): 519–522. doi:10.1023/A:1020545311555

Dugatkin, L. A., and H. K. Reeve. 1994. "Behavioural Ecology and Levels of Selection: Dissolving the Group Selection Controversy." *Advances in the Study of Behaviour* 23, 101–133.

Dummett, M. 1978. *Truth and Other Enigmas*. Cambridge, Massachusetts: Harvard University Press.

Dupre, J. 1981. "Natural kinds and Biological Taxa." *Philosophical Review* 90(1): 66–90. doi:10.2307/2184373

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33 I have been interpreting anti-realist pluralism about the units of selection as a form of instrumentalism. But this is not the only way to understand the view; it may instead be thought of as a form of *non-factualism*. Instrumentalism would involve an error theory – units of selection discourse would be taken at face value (the predicates express properties and the sentences possess truth conditions), but the sentences would be taken to be systematically false (although heuristically useful). Non-factualism on the other hand would be the view that the predicates of the discourse do not denote properties and the sentences do not have truth conditions. The sentences would therefore not be thought of as systematically false, but rather as neither true nor false (see Boghossian 1990 on the distinction between instrumentalism and non-factualism). Whether antirealism about the units of selection should be interpreted in instrumentalist or non-factualist terms is, I believe, an open question.

Godfrey-Smith, P. 2008. "Varieties of Population Structure and the Levels of Selection." *British Journal for the Philosophy of Science* 59(1): 25–50. doi:10.1093/bjps/axm044

Godfrey-Smith, P., and B. Kerr. 2002. "Group Fitness and Multi-Level Selection: Replies to Commentaries." *Biology & Philosophy* 17(4): 539–549. doi:10.1023/A:1020583723772

Gould, S. J. 1985. "A Most Ingenious Paradox." In: *The Flamingo's Smile*, 78–95. New York: W. W. Norton.

Gould, S. J. 2002. *The Structure of Evolutionary Theory*. Cambridge, Massachusetts: Harvard University Press.

Kerr, B., and P. Godfrey-Smith. 2002. "Individualist and Multi-Level Perspectives on Selection in Structured Populations." *Biology & Philosophy* 17(4): 477–517. doi:10.1023/A:1020504900646

Kitcher, P. 1984. "Species." *Philosophy of Science* 51(2): 308–333. doi:10.1086/289182

Kitcher, P. 2004. "Evolutionary Theory and the Social Uses of Biology." *Biology & Philosophy* 19(1): 1–15. doi:10.1023/B:BIPH.0000013273.58226.ec

Kitcher, P. 2008. "Carnap and the Caterpillar." *Philosophical Topics* 36(1): 111–127. doi:10.5840/philtopics20083617

Kitcher, P., K. Sterelny, and C. K. Waters. 1990. "The Illusory Riches of Sober's Monism." *Journal of Philosophy* 87(3): 158–161. doi:10.2307/2026634

Lloyd, E. A. 2005. "Why the Gene will not Return." *Philosophy of Science* 72(2): 287–310. doi:10.1086/432425

Lloyd, E. A. 2017. "Units and Levels of Selection." In: *Stanford Encyclopedia of Philosophy* [online]. https://plato.stanford.edu/entries/selection-units/

Lloyd, E. A., M. Dunn, J. Cianciollo, and C. Mannouris. 2005. "Pluralism without Genic Causes?" *Philosophy of Science* 72(2): 334–341. doi:10.1086/432451

Okasha, S. 2006. *Evolution and the Levels of Selection*. Oxford: Clarendon Press. doi:10.1093/acprof:oso/9780199267972.001.0001

Sober, E. 1984. *The Nature of Selection: Evolutionary Theory in Philosophical Focus*. Chicago: University of Chicago Press.

Sober, E. 2011. "Realism, Conventionalism, and Causal Decomposition in Units of Selection: Reflections on Samir Okasha's *Evolution and the Levels of Selection*." *Philosophy and Phenomenological Research* 82(1): 221–31. doi:10.1111/j.1933-1592.2010.00471.x

Sober, E., and D. S. Wilson. 1998. *Unto Others: The Evolution and Psychology of Unselfish Behavior*. Cambridge, Massachusetts: Harvard University Press.

Sober, E., and D. S. Wilson. 2002. "Perspectives and Parameterizations: Commentary on Benjamin Kerr and Peter Godfrey-Smith's 'Individualist and Multi-Level Perspectives on Selection in Structured Populations'." *Biology & Philosophy* 17(4): 529–537. doi:10.1023/A:1020575531347

Sterelny, K. 1996. "The Return of the Group." *Philosophy of Science* 63(4): 562–584. doi:10.1086/289977

Sterelny, K., and P. Griffiths. 1999. *Sex and Death: An Introduction to Philosophy of Biology*. Chicago: The University of Chicago Press. doi:10.7208/chicago/9780226178653.001.0001

Sterelny, K., and P. Kitcher. 1988. "The Return of the Gene." *Journal of Philosophy* 85(7): 339–358. doi:10.2307/2026953

Waters, C. K. 1991. "Tempered Realism about the Forces of Selection." *Philosophy of Science* 58(4): 553–73. doi:10.1086/289640

Waters, C. K. 2005. "Why Genic and Multilevel Selection Theories are Here to Stay." *Philosophy of Science* 72(2): 311–333. doi:10.1086/432426

Wilson, R. A. 2005. *Genes and the Agents of Life: The Individual in the Fragile Sciences, Biology*. Cambridge: Cambridge University Press.

van Fraassen, B. C. 1980. *The Scientific Image*. Oxford: Clarendon Press. doi:10.1093/0198244274.001.0001