



<http://social-epistemology.com>  
ISSN: 2471-9560

## Collective Belief Questioned

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Smith, Nicholas D. 2020. "Collective Belief Questioned." *Social Epistemology Review and Reply Collective* 9 (7): 58-63. <https://wp.me/p1Bfg0-5f0>.

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In their impressive article, “Collective Belief Defended,” Michael G. Bruno and J. M. Fritzman argue “that there are no fundamental obstacles to ontologically countenancing conscious cognitive collectives” (Bruno and Fritzman 2020, 1). In an article I published in such an unlikely place that it is remarkable that Bruno and Fritzman found it (Smith 2015), I argued that there are serious obstacles to such an idea, where my main reluctance was to counting such collectives as “conscious.” As for whether they might be “cognitive,” my concern was that the only argument for thinking that they could be cognitive was that collectives could do things that would reasonably qualify as functionally equivalent to cognitive functions performed by human (and perhaps non-human) epistemic agents.

No one can deny that attributing to collectives not just epistemic states, but also desiderative states and agency, is something with which we are all quite familiar and also comfortable (as the studies in experimental philosophy they cite clearly indicate). But the question is worth asking: if collectives can do things that we freely describe as “deciding,” “preferring,” “desiring,” and “believing,” is there any good reason *not* to credit them with the kind(s) of consciousness that is engaged when individual human beings do such things? I think there are several good reasons for rejecting this inference. Bruno and Fritzman do not think they are good reasons.

Before I review the relevant arguments, a stipulation is in order: my disagreement with the notion of collective conscious cognition a metaphysical one, not a lexical one. I have no problem with the ways in which we use language that might seem to attribute psychological states to collectives. I am not claiming that those who use language in this way somehow demonstrate any kind of linguistic incompetence. I believe I made my actual complaint clear in my earlier paper:

According to those who support the new collective social epistemology, as Goldman puts it, “[G]roups have ‘minds of their own.’ They are agents that are distinct from, and not reducible to, the minds of their members” (Goldman 2015, 227). But this is precisely what is wrong with the new collective social epistemology: groups do not have beliefs or “minds of their own” because they do not have minds; only their members do (Smith 2015, 8).

In this reply, I propose to review what seemed to me to be good arguments for what I said here, and respond to the assessments of those arguments by Bruno and Fritzman.

### **Learning and New Beliefs**

The first of the arguments that I gave concerned one of the processes that generates new beliefs: learning. I argued that learning cannot be understood in an anti-summative way (in my example, a teacher cannot claim that her class has learned something if, in fact, not a single member of the class had learned that thing). Of course, this was not intended as a refutation of the whole idea that groups can have (conscious) beliefs, because there might be

other etiologies of belief-formation that would apply to groups in the way anti-summativism requires (Smith 2015, 2-3).

Bruno and Fritzman argue that I got this wrong, giving an example where each student in a class is allowed to answer one question on an exam with the name of another student in the class who could answer that question correctly. Using this grading method, as long as the other student named in each case actually does give the correct answer, it would turn out that each and every student in the class would get a perfect grade on the exam. Bruno and Fritzman rightly conclude that such a class, as a team, could win a competition based on the relevant material (in this case, multiplication), even if they competed against other classes that included members who could get every answer right without relying on others in the class (Bruno and Fritzman 2020, 4). But then they make an inference that I find jarring. They say, “it makes sense to believe that this class has fully learned the multiplication table for the whole numbers less than 100, even though no student has fully learned it” (Bruno and Fritzman 2020, 4).

My own intuitions go in the opposite direction. It seems to me, on the contrary, that the class has *not* fully learned the multiplication tables involved in the contest. To my mind, no class can be said to have “fully learned” a subject as long as *even one* student in the class has not fully learned that subject. In this example, however, not only has one student not fully learned the subject, in fact not *any* of the students has fully learned it. This may simply be a case where appeals to intuitions do not generate consensus, since Bruno and Fritzman seem to take this case as intuitively going their way. Such “dueling intuitions” also seem to occur in each of the other differences I found between their view and mine.

In my paper, and in Bruno’s and Fritzman’s, there is some discussion of a case originally provided by Alvin Goldman (Goldman 2015, 227). In this case, 20% of the guards at the British Museum believe (A) that one of the other guards, Albert, is plotting to steal an artwork from the museum; 20% believe (B) that another guard, Bernard, is plotting to steal an artwork from the museum, and 20% believe (C) that yet another guard, Cecil, is plotting to steal an artwork from the museum. Each member of each of these groups correctly makes the existential generalization to a new belief (T) that some other guard is plotting to steal an artwork from the museum. Accordingly, a majority of the entire group of guards will believe T, but will not believe A, or B, or C. Goldman contends that it will be correct to claim, of the entire group of guards, that they collectively believe T but do not believe A, or B, or C.

In my paper, I note that unless members of each of the sub-groups shared their suspicions with others outside of their own sub-group, none of the members of the entire group will be aware that the whole group has the alleged belief (T). So if Goldman is right that the whole group believes T, no one in the group will be aware of this fact. I immediately go on to concede that individuals can have beliefs of which they are unaware, as well (Smith 2015, 7). Bruno and Fritzman repeat this observation (Bruno and Fritzman 2020, 6), so there is no debate here. But I go on to discuss how we would normally attribute a belief to someone who was not aware of having the belief, and note that we do this by observing how the one who has the belief behaves: we attribute beliefs to individuals in virtue of the fact that they behave in ways that reveal what they believe. I then ask how this would work in the case of the collective belief the entire group of guards is said to have, which *ex hypothesi* none of the members of the group would be aware the collective had.

I considered what kinds of group behaviors might indicate this collective belief, and could only imagine that it might be inferred from observations of increased vigilance with respect to the activities of other guards. But I then argue that an appropriate understanding of the greater vigilance we might reasonably expect does not give any indication of the alleged collective belief that T. Members of the first group would be especially vigilant about the activities of Albert. The second group would watch Bernard more carefully, and the third, Cecil. A belief in T might be indicated if, as an entire group (or perhaps some majority of it), there was more vigilance regarding other guards more generally. But there is, in fact, not the slightest reason to suppose that the whole group of guards or even a majority of them would behave in that way. Or so I claimed (Smith 2015, 7-8).

### **Responses, Disagreements, and Mousetraps**

Bruno and Fritzman argue that I have over-simplified what would actually occur in such a case. They point out that any increase of vigilance by any members of the group would likely lead to increased vigilance by others in the larger group, and also argue that those who made the inference from A or B or C to T would likely also recognize that “it is more probable that someone is plotting a heist than that any specific individual is,” and thus conclude that group vigilance would actually exceed the specific vigilant responses towards Albert, Bernard, and Cecil (Bruno and Fritzman 2020, 5).

As for the first of these responses, I agree that social dynamics are likely to be in play here. But I do not see that those social dynamics have to be understood as explicable only in terms of individual guards responding to a fact about the whole group of guards, rather than a (perhaps unconscious) response to the other individual guards whose additional vigilance they observe or somehow sense. If the latter is true, then nothing in the example (even once we include recognition of the social dynamics) has to be taken in a way that attributes psychological attributes to the entire group, rather than to (some) members of that group. In brief, it seems to me that however we conceive of the social dynamics, it will be in a summative way—and that is not what is needed for collective belief of the sort that Bruno and Fritzman (or Goldman) wished to defend.

As for their second response, I simply disagree. Imagine that I suspect a specific one of my students (Ms. X) is planning to steal a book from me. I have watched her, and she seems shifty, cunning, and dishonest, and she asked about the book in a way that, given my other observations about her, lead me to believe that she plans to steal that book. I then make the existential generalization, and also am willing to accept that the existential generalization is actually more probable than my original suspicion about Ms. X. Would this lead me now to be more vigilant about all students? A few others? I think not. I expect my (reasonable) expectations regarding other students will be just as they were before I started doing logic with my specific suspicion. If individuals will not become extra vigilant in general because they have a specific suspicion about someone else and then do an existential generalization, then I see no reason why such a response should be expected (again, in a non-summative way) from a collective.

There was one further disagreement between my earlier article and what Bruno and Fritzman have argued, and this disagreement has to do with what we make of functional equivalence. Their approach is bold and important: “it is not fallacious to identify functionally equivalent states or processes” (Bruno and Fritzman 2020, 2). My reply is that it really depends on what kind of things we are talking about. If the kind of thing we are talking about is conceived entirely in functional terms (as is the case in their examples of mousetraps and bicycles [Bruno and Fritzman 2020, 13]), then functional equivalence between two things that may look very different permits us to regard them as the same kind of thing. Let us take the case of the mousetrap. A common mousetrap and a Rube Goldbergian contraption that is also well designed to trap mice are both rightly identified as mousetraps.

In fact, there are importantly different kinds of mousetraps, and the different kinds are to be understood in functional terms. Most common mousetraps are probably only euphemistically called “mousetraps.” What they really are is devices that kill mice, not just capture them for later release. There are, however, devices that capture mice without harming (or at least without killing) them in the process. Both sorts of devices are called “mousetraps,” but those in the market for a mousetrap might well prefer one of these sorts of devices over another, precisely on the ground that the actual function of each kind of “mousetrap” is importantly different.

For clarity, let us focus on the lethal variety of mousetrap and see what we can make of certain functional equivalents to those. I have often witnessed with a cringe how very adept my pet cats are at catching mice. In some cases, they practice catch and release, like the kinder forms of mousetraps. But usually not: usually they torment their prey to death and then (usually only partially) eat them. Even though my cats are remarkably good at catching and killing mice, it nonetheless seems to me that it would be a dreadful metaphysical error to conclude from that fact that cats and mousetraps are the same kinds of thing.

Bruno and Fritzman concede this point, however. They only argue that the inference from “same function” to “same kind of thing” is warrant-conferring but defeasible (Bruno and Fritzman 2020, 13). Accordingly, they do not commit the fallacy that I claimed in my earlier paper was common in arguments about group belief. Instead, by arguing that “similarity of function” is (defeasible) evidence of similarity of kind of thing, they take themselves to have shifted the burden of proof (Bruno and Fritzman 2020, 13). Since I agree that groups can perform the functional equivalents of conscious epistemic (or conative) processes, the burden of proof is on me to show that such processes are not conscious (or epistemic, or whatever).

But I wonder about this argument, and for at least two reasons. As my example of cats and mousetraps shows, the inference from the fact that two entities seem to perform the same function to the conclusion that they are the same kind of thing does not work when the nature or essence or what-it-is-ness of one of the relevant entities is not reasonably taken to be conceivable or analyzable in purely functional terms. Accordingly, I take their argument about functional equivalence simply to beg the metaphysical question of whether or not beliefs, desires, and the other sorts of mental states are to be conceived as essentially functional states. It is one thing to accept that mental states have functions; but it is another thing altogether to assume that they are what they are simply because of the functions they

perform (such that anything else that performed such functions would be the same kinds of things).

Mousetraps are mousetraps because they do what it is that makes a mousetrap a mousetrap. But other things that do what makes a mousetrap a mousetrap and are nonetheless not mousetraps. The same, I contend, is true for beliefs, desires, and all the other mental states, which non-mental entities may duplicate in (merely) functional terms. The question as to whether groups actually are conscious or have minds of their own is simply left unanswered by showing that they can process the same kinds of inputs and produce the same kinds of outputs as we do when we believe, or desire, or practice agency.

### **Shifting the Burden**

The other reservation I have about Bruno's and Fritzman's burden-shifting argument is the idea that, in order to shoulder that burden, I would somehow need to provide some kind of evidence for thinking that things that I suspect *do not* have minds, really don't have them. This is a kind of logical opposite to the old "problem of other minds": how does one show that something we observe does *not* have a mind?

I had thought, actually, that I had given such evidence in my paper. I considered the fact that, in the way collective belief is construed by anti-summativists, there might be collectives that in their entire existence only had one single belief. I called this the problem of "cognitive singularity." The idea of a believer that had, in its entire existence, just a single belief (and no other cognitive or mental state of any kind), struck me as so bizarre as to count all by itself as a reason to reject the whole idea of collective belief. What sort of belief requires no conceptual apparatus, no cognitive system of any other kind, to be realized? But why should Bruno and Fritzman accept that this is sufficient to exclude such examples as authentic examples of cognitive states achieved by collectives? They would no doubt simply characterize my treatment of such possible anomalies of collective belief as begging the question in favor of "epistemic individualism": why should we accept that what is typically or even always true of individual epistemic agents must also be true of (putative) collective epistemic agents?

I have simply focused here on the disagreements between my earlier paper and this new paper by Bruno and Fritzman. In each case, I suspect that what is occurring is simply that there is a clash of intuitions at work—what seems very anti-intuitive to me seems perfectly plausible to them, and vice versa. If this diagnosis is correct, then it is perhaps best simply to identify where such clashes are, in the hopes that others can help to find ways through them to achieve consensus in some other way.

### **References**

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