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Quine, Science and Political Liberalism: A Reply to Bonotti, Badano, and Gómez-Aguliar

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In “The Quinean Assumption: The Case for Science as Public Reason” (Bellolio 2019), I argue that liberals need to introduce a crucial assumption into the argument that scientific reasons are public in the Rawlsian sense. This assumption—taken from W.V.O. Quine’s *naturalized epistemology*—states that scientific reasoning and everyday reasoning are not two different kinds but form an epistemic continuum, in which the latter is but an upgrade of the former (see Quine 1957). Once this assumption is introduced, *public reason* liberals—as they have become known for the centrality they give to public justification—can argue that the complexity and elaborateness of specific scientific arguments does not imply that they are inaccessible in the relevant sense, since everyone nonetheless acknowledge the underlying logic of scientific reasoning in their ordinary affairs, a universal trait that Quine called a “lay flair for evidence” (1957, 6).

To this article, Matteo Bonotti, Gabriele Badano and Ivan Gómez-Aguliar have elaborated three different responses for which I am very grateful. In their own way, each identifies blind spots of the argument that should be clarified, specified, amended and improved. I proceed to reply in turn.

### **Scientific Reasoning as Common Evaluative Standard**

Matteo Bonotti (2019) raises three concerns:

- a) Scientific consensus at any detailed rate is very hard to attain, you may only find it at a very general level. My argument thus leaves us with too few scientific reasons that count as public;
- b) We should not overlook the “prescriptive” dimension of even the most “descriptive” of all arguments: the scientific;
- c) It does not matter that citizens controvert the scientific consensus, as long as public institutions, and those who play key roles within them, abide by it.

I do not have quarrels with b) and c), so I will dispatch them quickly.

First, my claim that scientific arguments alone lack normativity, and hence they should be accompanied by other value-judgments and moral considerations in political deliberation, does not conflict with Bonotti’s claim “that prescriptive and descriptive reasoning standards are jointly necessary for public reason” (2019, 20). Furthermore, I think that the standards of scientific reasoning are themselves expressing epistemic virtues—clearness instead of vagueness, openness to criticism instead of self-immunization, capacity to make predictions instead of merely arguing in hindsight, explanatory unification instead of fragmentary targets, a positive account of how things happened instead of a purely negative account of how they did not, et cetera—that should permeate public inquiries in all fields. Although epistemic in nature—they aim for knowledge—I take these as ethical guidelines too—they aim for knowledge that should be acknowledged by fair-minded people. All the same, coercive

norms and policies do not rest solely on guidelines of inquiry, no matter how ethically loaded they are. Substantive political values are needed.

Second, I agree that someone outside the scientific community should carry the evaluation of their reasons if they are to be public, but I do not think—like Bonotti seems to believe—that ordinary citizens should play that role, at least not for public justification purposes. Once we agree that scientific reasons are public reasons, the constraints of public reason will still apply to public officials such as executive branches, courts of justice and parliaments. To be sure: my claim is that, for the Quinean Assumption to hold, ordinary citizens should be *in principle* capable of scientific reasoning. But beyond the business of justifying political coercion, they can argue as they please. For the purposes of public justification, then, I subscribe Vallier (2014), and Bonotti’s, “indirect approach”: the constraints of evaluating whether scientific arguments are public in the Rawlsian sense only burden public officials.

Regarding a), which is the most interesting point, I am indeed referring to scientific consensus at a very general level. This does not amount to underestimate the degree of disagreement that characterise scientific debates at a more specific level. Rightly so, Bonotti points to the fact that experts disagree on their models and predictions about climate change’s pace and the magnitude of its effects. I can contribute with examples from evolutionary biology. To what extent the course of biological evolution is gene-centred is a matter of ongoing discussion. The role of alternative evolutionary drivers such as genetic drift or sexual selection are not entirely dismissed.

The late palaeontologist Stephen Jay Gould criticized what he called *Darwinian Fundamentalism*, referring to the “conviction that natural selection regulates everything of any importance in evolution, and that adaptation emerges as a universal result and ultimate test of selection’s ubiquity” (1997, 2). Gould’s discrepancies with the ethologist Richard Dawkins are usually highlighted by Darwinian sceptics to show that the theory is not consensual. Instead of phyletic gradualism—the idea that evolution runs smoothly and continuously through whole lineages (a process known as *anagenesis*)—Gould famously proposed the punctuated equilibrium hypothesis—the idea that species become stable after they first appear, only to break into different ones by rare and sudden events (a process known as *cladogenesis*).

Notwithstanding these internal disagreements, Gould was a full-blooded Darwinian in all relevant respects. Another example: the once much-maligned theory of group selection—instead of natural selection operating only at the level of the individual—is experiencing a sort of revival as an evolutionary mechanism, specially through the work of E.O. Wilson (2013). This theory suggests that single organisms in animal societies have good reasons to offer up their wellbeing in the name of the group when fighting predators or competing against rival groups of the same species. In all these cases, the individual will not enhance his own reproductive fitness, but this type of heroic behavior will enhance the chances of the group to overcome its adversaries. However, as with the previous topic, there is ongoing disagreement about this matter.

Are these reasons non-public because scientists disagree at this more specific level? Not necessarily. One thought is that we should adopt a less demanding test for a scientific reason to be public, one that rules out only those claims that reject established scientific facts, either

general or specific. We may call it the “exclusion of scientific denialism” criterion. Arguably, even if their theories remain minoritarian within the relevant community of experts, neither Gould nor Wilson are evolutionary denialists. On the other hand, Young-Earth creationists deny the basics of evolutionary biology, and much of other science too. The case is seemingly harder with Intelligent Designers, who accept large parts of mainstream Darwinian evolution but reject others.

By applying the “exclusion of scientific denialism” criterion to Bonotti’s example, arguments provided by scientists that disagree on how much sea levels will rise due to increased carbon emissions may remain public, while arguments provided by deniers that climate change is happening or that it is man-made are not. But even if we do not buy the “exclusion of scientific denialism” criterion, there is another route. It is provided by Bonotti and Badano’s (2019) notion of accessibility, which is the idea that we do not have to accept each scientific finding that is produced through the scientific method in order for that finding to be construed as a public reason, but we do need to abide by the underlying reasoning standards of such method. Once we accept the latter, its products are public arguments, regardless of the empirical fact that those findings turn out to be unpalatable in the face of our comprehensive views and prior assumptions.

I have articulated this distinction between methods and conclusions of science elsewhere (Bellolio 2018). It is expressed in a very similar fashion in Bonotti and Badano (2019). With them, I believe that Rawls is wrong requiring non-controversiality for both methods and conclusions of science. For roughly the same argument underlying their notion of accessibility, only the former requires it. In my terms, scientific standards of reasoning should be acknowledged as epistemically reliable by a wide extra-scientific consensus to be public, while that requirement does not apply to specific scientific conclusions. For the latter, it suffices an intra-scientific consensus among the relevant experts, at the right level of abstraction. Using Vallier’s (2014) nomenclature, I do not aim for *shareability* at the level of specific conclusions, as Bonotti suggests. However, it seems to me that certain shareability is still needed at the level of the methods. The recourse to a naturalized epistemology, for which every citizen has a *lay flair for evidence*, is meant to show precisely that: the reasoning standards of science are, ultimately, common evaluative standards.

### **How Controversial is QA?**

Let us turn to Gabriele Badano’s reply. While he is sympathetic, like Bonotti, with the overall project of assigning science the status of public reason, Badano warns that the introduction of a controversial philosophical theory to justify such a status, namely, Quine’s naturalized epistemology, defeats the very same purposes that political liberalism is set to attain. This is a fair point: if the Quinean Assumption cannot itself be portrayed as neutral and impartial enough among competing epistemological theories, then, as political liberals, we are prevented to make use of it to justify the neutrality and impartiality of scientific reasons. If experts on cognitive psychology and social epistemology currently agree that scientific and commonsensical reasoning are “discontinuous”, as Badano (2019) points out, then he may have a defeater.

We should bear in mind the structure of my argument: *if* liberals want to retain noncontroversial scientific arguments within public reason, they have to introduce a crucial epistemological *assumption*. By assumption, I mean a basal premise playing an axiomatic role before the whole normative structure is mounted. In this restricted sense, crucial philosophical assumptions could be emancipated from the constraints imposed by political liberalism. Rawls himself believed that “moral philosophy must be free to use contingent assumptions and general facts as it please” (1999, 44). Without going any further, this is what the Rawlsian *original position* thought experiment does: while individuals ignore all the arbitrary features of their actual identity, they retain knowledge about general facts of psychology.

In Samuel Freeman’s understanding of the *original position*, for instance, parties retain their knowledge about general facts such as “relatively uncontroversial laws and generalizations derivable from economics, psychology, political science, and biology and other natural sciences” (2016). However, this line of response is vulnerable on two counts. First, it does not consider Rawls’s *political turn*, which, as Badano recalls, place additional constraints on the liberal philosopher’s task. Second, even if we grant that philosophers may employ assumptions and general facts in political liberalism, such as the general facts of psychology, these should be relatively uncontroversial all the same. Pointing to some well-known research on cognitive mechanisms, Badano’s case is that Quine’s naturalized epistemology is far from uncontroversial.

A more promising way to address this seeming inconsistency is coming back to the levels of generalization and detail that scientific research admits. Quine’s naturalized epistemology may contain specific claims that remain controversial within the relevant community of experts, but it could be broadly understood as an evolutionary approach on cognition. Such evolutionary approach is less disputed, to the extent it asserts that the human capacity for inferential reasoning is an adaptation to our ecological niche. That’s why it is universal. At this level of abstraction, I think it is fair to say that an evolutionary approach to human cognition should be regarded as a general fact of psychology, regardless of the disagreements that we will surely find at a more detailed level.

Let us take a look. The dual process—intuition vs. reason—to which Badano refers to prove that everyday reasoning and scientific reasoning are two quite distinct types, has been recently challenged—some would even say debunked—by more ‘integrationist’ perspectives. Hugo Mercier and Dan Sperber, chiefly, have contended that “*reasoning is not an alternative to intuitive inference; reasoning is a use of intuitive inference about reasons*” (2017, 133). This means that we usually infer our reasons in order to support our intuitive conclusions, as an after-the-fact justification process. We do that because reason did not evolve to find true answers, but to help us navigating the social world. Reason, according to Mercier and Sperber, “evolved as a response to problems encountered in social interaction rather than in solitary thinking” (2017, 183). In this sense, either lay people or professional scientists, we all *reason* the same way, since we all want to be perceived as being right.

As a general fact, we do not apply very demanding standards to the reason we produce, but we are tough evaluating the reasons that others produce. Thus, taken individually, scientists are *as* biased as lay people are. They produce trustworthy knowledge not because they employ an inferential module that ordinary citizens do not use, but because their social

activity involves a series of checks and control processes that are crucial to keep their biases at bay. No wonder, Mercier and Sperber argue that “scientists’ reasoning is not different in kind from that of laypeople” (2017, 320).

My point, obviously enough, is not to pit experts against experts. It is to show that the view that Badano suspects it has been wholly superseded, it is actually compatible with groundbreaking insights in the field of social cognition and evolutionary psychology. As it were, following Badano and Bonotti (2019), we may choose to argue that Kahneman and Tversky’s seminal work, and Mercier and Sperber’s recent findings, stand before each other in the same way that divergent models and predictions of climate change do, or as Gould’s and Dawkins’ theories of Darwinian evolution do, that is, as disagreements internal to the expert community, but drawing their public character—therefore, their neutrality in the relevant political sense—from the scientific standards of reasoning from which they were produced. This notwithstanding, it may be the case that we have to abandon the Quinean Assumption in its original form and instead adopt a broader and relatively uncontroversial perspective such as an evolutionary approach, thus allowing competing theories of human cognition under this umbrella. But I remain skeptic that we could justify science as public reason without resorting to any epistemological theory that help us to believe—like Badano and I believe—that scientific reasoning is *in principle* accessible to all citizens. We need to say something about why we think citizens share basic rules of inferential reasoning.

### **Science as Social Institution**

Finally, I move to Gomez-Aguliar (2019). I can distinguish three different points. First, he thinks that I am requiring too much (epistemic work) from ordinary citizens, hence similar to one of Bonotti’s point. I thus provide a similar answer: citizens are entitled to their own cognitive biases. I do not think that a different perspective could remain liberal. So, it is not the case that I want to force scientific reasoning into ordinary citizens, as if embracing a kind of epistemic perfectionism. To be sure: I do think that everybody has the right to an adequate scientific education within the mandatory school curriculum, and I do believe that democratic deliberation gets better when the public opinion knows the relevant scientific components of the debate. But ordinary citizens are not the ones deciding on coercive norms and policies. If any burden at all, it rests on the shoulders of public officials and state institutions, since they are in the business of political justification.

As a second point, Gomez-Aguliar calls to view science not as the pursuit of truth, but often as a mere consensus among experts. I do not disagree. For strictly political purposes, we do not need strong theories of correspondence nor truths in any substantive sense. A mere consensus among experts, provisional as it is, is well enough. My claim, which is epistemic in nature, is that such a consensus—even if intellectually ungraspable for actual ordinary citizens—should be recognized as the product of a method which reasoning standards are understood as common. Otherwise we cannot speak of science as public reason.

For similar reasons, I do not disagree with Gomez-Aguliar’s third point either: expert knowledge is not an epistemic gift but a social enterprise. Most citizens trust science not because they have performed the experiments themselves, but because they acknowledge the

underlying general logic of the scientific method: to check confirmation biases. Of course, as Gomez-Aguliar recalls, this method is embedded in practices and institutions. Think about the relation between faith and the church. I am mostly talking about faith, primarily because I am doing *ideal* political theory. Accordingly, my treatment of science is fairly idealized. If scientists do not behave *in reality* like they should *in principle*, that does not invalidate the principles that I am discussing. While sociologists and political scientists are after reality, political philosophers are after principles.

For instance, Gomez-Aguliar's last remark is both philosophical and sociological. Beyond the plausibility of Quine's claim about the continuity between commonsensical and scientific reasoning, which he does not dispute, Gomez-Aguliar argues that "we need to explain and prove why we can trust, in epistemic terms, institutions to provide expert evidence" (2019, 42). There are two issues here: on the one hand, the question of why science should be trusted in epistemic terms, i.e. as a mode of inquiry; on the other hand, the question of why the scientific community or any other community of experts should be trusted, i.e. as a social institution. I am tempted to say that we trust the social institution because we believe they are trained to apply the mode of inquiry that we regard as epistemically trustworthy. But in order for political liberals to consider scientific reasons as public, we should disentangle the mode of inquiry from the social institution. If we look at the later, I am afraid, scientific reasons will be seen as associational reasons, and therefore paradigmatically non-public in the Rawlsian sense (see Mackinnon 2012; Bellolio 2018).

In sum, I take from Bonotti his notion of accessibility to explain that a scientific argument qualifies as public even if actual citizens dispute it, as long as such argument has been produced by a method which standards of reasoning are fundamentally accepted. I take from Badano the necessity to justify the Quinean Assumption in a way that is compatible with the philosophical abstinence of political liberalism, and that may lead us to present it as a more ecumenical epistemological approach. Finally, I take from Gomez-Aguliar the importance of unpacking science as an epistemic method and science as a set of social practices and institutions, emphasizing that, for my philosophical argument to work, I am chiefly concerned about the former.

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