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1. Introduction

Ladyman and Ross (2007) do not think that contemporary metaphysics is in good standing. However, they do think that there is a version of metaphysics that can be made to work—provided we approach it using appropriate principles. My aim in this paper is to undermine some of their arguments against contemporary metaphysics as it is currently practiced.

2. A *partial* defence of contemporary metaphysics and its method

In contemporary metaphysics there is a reasonably well-established methodology. It is not the only methodology available. I do not say that it's a good methodology. But it *is* well established. The methodology that I have in mind is Lewisian. It is 'weights and balances' metaphysics. Very roughly, the idea is this. We work out a range of internally consistent theories. We must then select a theory from this pool of theories to be adopted as our preferred theory. To do this, we must weigh the pros and cons of the theories in our pool. If a theory is particularly parsimonious, that's a benefit of the theory. If a theory offers great explanatory power, that's a benefit of the theory. If, however, a theory is particularly inelegant or ad hoc, then that's a cost of the theory. Similarly, if a theory looks like it contradicts our best physics, then that is a cost of the view.²

Of central concern to us here is the fact that whether or not a theory is *intuitive* is also taken to be salient to our choice of theory. Some claim that if a theory is particularly intuitive or commonsensical, then that is a benefit of the theory; if a theory is particularly counterintuitive then that is a cost of the theory. In quite a range of different cases it can seem as if the *only* evidence being given in favour of a particular theory in philosophy *is* that it is intuitive. As Ladyman and Ross put it, for 'neo-scholastic metaphysicians' intuitive judgments are typically all that ever passes for evidence' (2007: 15).

For my own part, I think that's an overstatement. But set that to one side. Let us allow—as we surely should—that philosophers frequently write as if intuition is being appealed to as evidence. Some are forceful in the claim that they *are* appealing to common-sense, or intuition, *and* that they take it to be evidence in favour of their theory.³ Consider the following:

‘What it is for some statement to be commonsensical is just for it to seem obviously true to most sane human beings...All I insist upon is that something's being

¹ I'm very grateful to a referee for *Ratio* for several rounds of comments on earlier drafts. Their comments have significantly shaped the paper. I flag some points, below, where their influence is especially visible, but the whole work has benefited greatly from their advice and expertise.

² Those who explicitly describe this methodology as one present and available in contemporary metaphysics include: Dyke and Maclaurin (2011: 296), Lewis (1986: 3-5) and Sider (2009: 385). Markosian (2008) is, as a whole, a nice example of a philosopher engaged in an extensive programme of weighing and balancing the costs and benefits of a view—pp. 359-60 even gives us a table laying out the costs and benefits of all of the views under discussion.

³ Thanks, here, to the referee for forcing me to get clear on precisely the philosophers and contexts that are the focus of my concern.

commonsensical must be allowed to count very strongly in its favor' (Zimmerman, 2008: 222)

If it is true, as I think it is, that our pre-philosophical singular intuitions about objecthood exhaust the data of the theory of composition, then being in accord with such intuitions is in some good sense not just a *theoretical* virtue of an account of composition (as conservatism is) – it is something more like an *empirical* virtue. (Kriegel, 2008: 363)

In addition to those who are *explicit* that they are treating intuitions as of evidential weight, there are those who seem to speak implicitly, in the same way. Here is Sider (1996: 463—my italics)

we don't *say* "here are two coin-shaped objects in the same place". This talk is clearly intended to be literal (as opposed to talk of "the average family"), and is accompanied by robust intuitions ("shouldn't two coin-like things weigh twice as much as one?"). While not decisive, these *intuitions* create an at least *prima facie* reason to look for a theory that respects them.

This certainly *seems* like a case where a metaphysician is treating intuitions as evidential—else why look for a theory that respects our intuitions? Ladyman and Ross themselves cite Paul (2004: 172—my italics), who, in discussion of primitivism about the *de re* properties of objects, notes:

A related worry involves the consequences implied by the claim that the lump and the statue are not identical: the lump and the statue coincide, so two numerically distinct objects share their matter and occupy the very same spatiotemporal region. But without any explanation, this result is radically *counterintuitive*, for it seems to contradict our usual way of thinking about material objects as individuated by their matter and region.

This seems a clear case of a view being counterintuitive and this counting against it. This seems to be another case where intuitions are playing the role of evidence to be accommodated. Indeed, earlier in the piece, Paul (2004: 171) has already noted that one of the key *points* of her paper is to 'defend essentialism against the variability of (some of) our modal intuitions'.

Lastly, consider Markosian (2008: 360). Markosian considers the special composition question that asks us 'when does composition occur?'⁴ In considering the various different answers that have been put forward in the literature, Markosian produces a table listing the various costs and benefits of those views. Throughout the paper, Markosian proceeds by arguing that the theory that has the most benefits is to be accepted. Thus, these benefits, are playing something *like* an evidential role. In the 'Main benefits' column, there are 7 explicit references to intuition. In each case, that a theory 'fits', 'preserves' or is 'consistent with' intuition is cited as a benefit. Notably, only 16 benefits are described. Thus, only just under half of all of the benefits to be attributed to these philosophical theories by Markosian concern the fact that they map particularly well to intuition. *These* philosophers, who appeal

⁴ The question is given its most extensive treatment in Van Inwagen (1990).

to intuitions as evidence; they are the target of Ladyman and Ross's arguments. They are the subjects of my (partial) defence.

But before we move to that defence, a concern. Dorr (2010), in reviewing Ladyman and Ross, says: 'saying 'Intuitively, P' is no more than a device for committing oneself to P while signalling that one is not going to provide any further arguments for this claim.' Zimmerman and Kreigel (et al.) are (or so it seems to me) using talk of what is common-sense or intuitive as *evidence* in favour of a particular view. But Dorr has argued that metaphysicians *do not typically grant* evidential weight to intuitions. Dorr's response to Ladyman and Ross has been influential and was endorsed by others. If Dorr is wrong and Ladyman and Ross are right, that intuition is taken to be evidential in analytic metaphysics, this needs to be established and discussed.⁵

So, what to say? Well, I've cited Kriegel, Paul, Sider, Markosian and Zimmerman as among those who appeal to intuitive as evidence, but they may not be representative of metaphysicians. If the established methodology of metaphysics involved taking intuitions to have some probative force what explains Dorr (and perhaps also others) insistence to the contrary?

By way of response, let me offer three comments. As I've already said, the defence here is only intended to be *of* those who *do* use intuitions as evidence. So, first, I don't much mind whether the view I'm defending here is a little outside the mainstream (though see point 3). As is clear from the forgoing, there clearly are those who are invested in the philosophical methodology that I've described—or, at least, there are those who have practiced it in the not-to-recent-past. That methodology seems to be one that explicitly gives evidential weight to intuitions. Dorr seems to simply be wrong in the cases I've described.

Second, of those that do in fact cite Dorr's review, it seems at least some folks think he's wrong on this score. I would echo the response to it given by Maclaurin and Dyke (2011: 296)

Cian Dorr [2010] attempts to take the sting out of L&R's critique of the methodology of appealing to intuition, by suggesting that it is not really an argumentative strategy at all. Instead, Dorr claims, it is merely a stylistic device for indicating that one is assuming some proposition as a premise, and intends to provide no further argument for it. If that's right, Dorr claims, then one can no more criticize it as a methodology than one can criticize the methodology of giving arguments that rely on premises. We don't think that all the appeals to intuition that one finds in non-naturalistic metaphysics can be characterized in this way. Those that occur in Lewisian style cost-benefit analyses, for example, do not fit this model. In such analyses two theories are compared with respect to a number of theoretical virtues, one of which is the preservation of common-sense intuitions. With respect to this particular theoretical virtue, the theory that requires us to relinquish the fewest intuitions wins.

It is precisely that Lewisian-style metaphysics that I wish to defend. (Note: I don't say that this *is* Lewis's view. I will happily defer to Lewis-scholars for judgments on exactly how to understand Lewis's view.) My point is that there is a distinctive way of doing metaphysics, practiced by the likes of Zimmerman, Sider, Paul, Markosian and Kreigel, recognized here by

⁵ I'm very grateful to the referee for focusing my attention on this point.

Maclaurin and Dyke (2011), and that I will try to defend from Ladyman and Ross's attacks. It may be that there are other parts of analytic metaphysics where intuitions are not appealed to as having evidential weight; that might explain Dorr's judgment. But, even if that is so, there remains a part of analytic metaphysics that seems to require some defence.

Third, and finally, Kreigel, Markosian Paul, Sider and Zimmerman *are*, so far as I can tell, in the business of practicing mainstream metaphysics. I'm not quite sure how to argue for that claim. I don't know how to determine definitively what counts as 'mainstream' and what counts as of more parochial interest. Nonetheless, as I understand mainstream metaphysics, those philosophers mentioned a moment ago are its practitioners. A defence of their approach, as that approach is described in the quotations above, is therefore, a defence of a mainstream approach to metaphysics.

The structure of the defence, to which I will now turn, is as follows. To begin, I note Ladyman and Ross's concern with the use of intuition in metaphysics. I consider the response that by using intuitions in metaphysics we fare no worse than the scientist who also uses intuition in their practices. I register Ladyman and Ross's claim that this defence fails because it's clear that scientists use 'intuition' in only a very limited sense. I argue, through consideration of cases, that Ladyman and Ross seem wrong about the range of uses of intuition deployed by scientists. I conclude that it is at least an open question as to how scientists use intuition and, as such, that it is an open question as to whether or not metaphysicians are any worse placed than scientists. I suggest that more needs to be done to show that the metaphysician is behaving in an irresponsible fashion.

2.1 Worries with intuition

The specific concerns raised by Ladyman and Ross are that these intuitions that are being appealed to are a product of our environment and are of value only in a very limited range of cases. As they make clear (2007: 11), most of our experiences serve to acquaint us with how the world behaves at spatial distances to be measured in a scale ranging from around a millimetre to a few thousand miles. Similar remarks can be made about our familiarity with temporal durations that are neither particularly short, nor long. Further, our intuition seems to have furnished us with a model of reality such that we are surprised when objects behave in non-standard ways.

If our intuitions are effectively only formed in response to goings on at these relatively pinched perspectives, and in response to relatively slow-moving and massive macroscopic objects, then it is unclear as to why we should think that these intuitions are of any use when it comes to discerning the fundamental nature of reality. Since that nature is the target of inquiry among contemporary metaphysicians, and since (at least some of) these metaphysicians are using intuitions as evidence, the work produced by these metaphysicians is of little to no value. Such work is, after all, fundamentally misguided.

But establishing that intuitions *are not* of any use in helping us to choose between theories about the fundamental structure of reality requires more than this. That my intuitions are formed *in* a domain and *about* that domain doesn't mean that they *cannot* be usefully ported to *other* domains.

Here is one way that such a 'porting' might occur. Suppose that intuitions of the very same sort that Ladyman and Ross wish to object to in metaphysics were used in physics. Physics—like metaphysics—is supposed to uncover the fundamental structure of reality. Ladyman and

Ross are of the mind that physics is in good standing. So, were physics to rely upon intuitions as evidence, in the way that (at least some parts of) metaphysics seems to, then we should either conclude that physics is not in good standing, or else conclude that metaphysics is none the worse for treating intuitions as sources of evidence in favour of a particular theory. And since the success of physics does not seem to be in question, that would naturally lead us to the conclusion that metaphysics is in good standing.

Ladyman and Ross are, however, quite clear that this defence cannot be mounted. It is clear to them that the uses of intuition in the sciences are of a very particular type and that this lends no support to the metaphysician. Here are Ladyman and Ross (2007: 15) making their point.

We do not deny that intuitions in one sense of the term are important to science. It is frequently said of, for example, a good physicist that he or she has sound physical intuition. Economists routinely praise one another's 'economic intuitions'—and routinely break the bad news to struggling graduate students that they lack such intuitions. However, the meaning of 'intuitions' in these uses differs sharply from the metaphysician's. The physicist and economist refer to the experienced practitioner's trained ability to see at a glance how their abstract theoretical structure probably—in advance of essential careful checking—maps onto a problem space. Intuitions in this sense have nothing to do with deliverances of putative untrained common sense. Furthermore, even the intuitions of the greatest scientist are regarded by other scientists as heuristically and not evidentially valuable.

In my view, this argument fails.

2.2 Analysing the argument

The point being claimed by Ladyman and Ross is empirical. They are claiming that, as a matter of fact, scientists *do not* work in a particular way; scientists *do not* treat their intuitions as evidence. Apparently, *no* good science is being done by scientists who treat the deliverances of common-sense as evidence. Perhaps that's true. But where is the evidence? It is, after all, a claim with very wide scope and within the various sciences there is clearly some scope for moderate differences in approach. In the absence of *any* evidence why should we think that Ladyman and Ross are right? I don't see that *a priori* speculation could establish this point either way.

One might think that there is a good deal of data available on when scientists use their intuition and that this clearly reveals that Ladyman and Ross are quite right about the role that intuition plays in physics. After all, the methodology deployed by practicing scientists is well studied and well understood. Whatever the findings of these studies, they will surely reveal that physicists only use their intuitions in the way that Ladyman and Ross describe.

This response fails. In point of fact, there isn't too much work on the role that intuition plays at the point of theory evaluation in the sciences. In particular, there isn't very much literature that attempts to systematically track the range of uses to which intuitions are put in all sciences.

This brings us on to the second point of concern: what the studies actually show. I want to consider two different studies that explicitly *are* focused on intuitions in science, and one discussion of ‘gut feelings’.

2.2.1 Data on intuitions in physics

One recent study reveals some interesting data, but does not speak in favour of Ladyman and Ross’s claims. Indeed, it *seems* to speak *against* them. I (Tallant, 2013) track the range of uses to which the term ‘intuition’ is put in a range of scientific journals. In that paper I try to identify a range of reasonably distinctive uses, including: results being described as intuitive; explanations/theories being described as intuitive; pictures being described as intuitive; a particular way of thinking about a given subject matter being described as intuitive; cases that enable us to gain intuition, and expectations being particularly intuitive. The study also notes that there are a very significant number of other uses that don’t fit cleanly into any of these categories.

I think that the data cited in that paper gives us some evidence that Ladyman and Ross speak falsely when they claim that the *only* use to which intuitions are put in the evaluation of scientific theories are ‘to see at a glance how their abstract theoretical structure probably—in advance of essential careful checking—maps onto a problem space.’ Where Ladyman and Ross say that the term applies *to scientists and their abilities*, it *seems* from the data that the term is frequently applied *by* scientists *to* explanations; to pictures; to results. Ladyman and Ross appear to simply be wrong in their claims as to how intuitions are used in science.

2.2.2 Intuitions of Nobel Laureats

Marton, Fensham and Chaitlin (1992) carried out an ethnographic study of interview data from interviews with 83 Nobel Laureats, drawn from physics, chemistry and medicine from 1976 to 1986. The interviewees had been asked, *do you believe in scientific intuition?* Of the 83, 72 explicitly declared that they did or else made comments that took its existence of granted. As Fensham and Marton (1992: 115) note, ‘this study supports very strongly the importance of intuition in science’.

What interests us here is the question of *how* these Laureates thought that scientific intuition might be deployed. Is it, as Ladyman and Ross would have us believe, merely that talk of ‘scientific intuition’ *merely* refers to the *ability* of a practitioner to see at a glance how their theoretical structure maps onto a problem space? It seems not. As noted by Fensham and Marton (1992: 115), there *is* talk of abilities. But, as they also note, there are three distinct ways in which these Laureates describe using scientific intuitions: ‘As an *outcome*, intuition denotes an idea, a feeling, a thought, or an answer. As an *experience*, or *something that happens* intuition refers to acts or events. The third use was to *individual capabilities*. Some examples are revealing.

And so... as we did our work, I think, we almost felt at times that there was almost a hand guiding us. Because we would go from one step to the next, and somehow we would know which was the right way to go. And I really can’t tell how we knew that, how we knew that it was necessary to move ahead [Michael S Brown, winner of the Nobel Prize for Medicine in 1985]

I don’t want to suggest that this is a use of intuition that is the same as that of the metaphysician. The very meagre claim that I want to make here is that it is very hard to see this as a case in which those in question are talking about the ability of a trained practitioner

to see at a glance how their theoretical structure maps onto a problem space. Indeed, the description given by Brown almost invites the charge of the process of deploying intuitions being mysterious.

Finally,

...to me it is a feeling of...’Well, I really don’t believe this result,’ or ‘This is a trivial result,’ and ‘This is an important result,’ and ‘Let us follow this path.’ I am not always right, but I do have feelings about what is an important observation and what is probably trivial.’ [Rita Levi-Montalcini, *Medicine*, 1986]

Again, this is not a scientist speaking of an ability to see how a model maps onto a problem space. This is a scientist speaking of *feeling* particular ways about particular routes forward and particular results. Once more, I don’t claim this to be the same kind of activity as that engaged in by the philosopher. Rather, my concern is with showing that Ladyman and Ross seem quite wrong to insist that the only use of intuition in science is to talk about the ability of someone to see how a model maps onto a problem space.

Consider one further objection to my point. What the discussion of the Laureats shows is that scientists do often appeal to intuition. However, that does not show that intuition is *evidential* as opposed to heuristic. The Nobel prize winners would not be successful scientists if their theories only had intuition to support them. In addition to (perhaps) being intuitive theories, they were also empirically successful. The result may be that intuition may well play a role in the context of discovery but not in the justification for why we should believe our best scientific theories.

I think that the objection is quite right—to a point. Nothing that I’ve said here serves to fully disclose whether the use of intuition in science is heuristic or evidential. I agree with that. But showing otherwise was not my intention. As noted, my goal here is to show that Ladyman and Ross are wrong about the scope of the use of intuitions—that intuitions are *merely* used to see at a glance how their theoretical structure maps onto a problem space. If intuitions are also being used as an heuristic, to guide the shape of inquiry, then Ladyman and Ross are wrong about the role that intuition plays in science.

2.3 Surveying the scientists⁶

Most striking of all the data presented here, and of direct relevance as to whether or not intuition plays an evidential role in science, is that reported by Martinson, Anderson and de Vries (2005). In the course of their research, they surveyed in excess of 3,000 scientists, and asked them to (anonymously) confirm or deny that they had taken part a variety of practices, including fraud, plagiarism and fabrication. The point most significant to us, however, is that in excess of 15% of those who responded admitted to ‘dropping observations or data points from analyses based upon a gut feeling that they were inaccurate’.

This ‘gut feeling’ seems very close to intuition. Somewhat leadingly, these seem to be cases where scientists are rejecting particular data because they ‘just seem wrong’. And, in this

⁶ Once again, I’m very grateful to the referee for persuading me to gather data on this point.

context, it seems to be the case that this ‘gut feeling’ is playing an evidential role (else why drop the data point without further checking?). We should concede, of course, that this is far from definitive. But this would seem to be a place in which intuition—or gut feeling—is playing an evidential role in theory choice. Gut feeling tells us which data-points are ‘right’ and which are not; which data points are ‘right’, of course, determines which theory is correct. Thus, ‘gut feeling’ would seem to be playing an evidential role in our theory choice.

2.4 Where does this leave us?

Caution should be urged here—for two reasons. First, I have only claimed that Ladyman and Ross *appear* wrong. We can claim nothing more than that. As I try to (Tallant, 2013) make clear, (2.1) the data provided only gives us a sense of how the term ‘intuitive’ is used by scientists in their writings. The data from the Laureates gives us only self-reports (2.2). The data on the self-reports (2.3) is in need of further exploration.

These need not be taken to definitively reveal anything about how *intuitions themselves* really are used by practicing scientists. If we wish to understand how scientists in fact use their intuitions, then we will need to engage in much more extensive study, involving a range of different approaches. So the very best that we can hope to show here is that what data there is *suggests* that Ladyman and Ross are wrong.

Second, note that nothing that has been said here establishes what we were looking for on behalf of the metaphysician: namely, something that will clearly and obviously help us to rehabilitate the use of intuitions in metaphysics. The reason for this, of course, is that we have established that what little data there is on the range of ways that scientists use their intuitions is unclear.

Now, though this point may appear *initially* dispiriting, I think that it ought not to leave us to downhearted. Simply, the current data that we have (Tallant, 2013) *suggests* that there is a range of different ways in which intuition appears to be used in physics. It is not at all implausible to think that those uses will be found in other disciplines. Perhaps philosophers make use of their intuitions in these ways. There may even be other uses of intuitions (in the form of ‘gut feelings’) to be found in other sciences. Although the data I’ve presented is suggestive, it is far from exhaustive.

Suppose that’s right. It might then be the case that intuitions *are* being appealed to as evidence—in some way—in a variety of places in the sciences. If that’s right, then it would go some way to providing the metaphysician with justification for the methodology outlined earlier. Were it to turn out that a complete mapping of the role played by intuitions in science included the use of intuitions as evidence in support of a theory, then it would seem much harder to object to the metaphysician that they may not also appeal to intuitions as evidence.

But suppose that we do not find intuitions being used as evidence. What then? Well, this presents the metaphysician with an opportunity. Suppose that we can, in these future studies, identify the range of ways in which practicing scientists use intuition. Rather than sounding the death-knell for contemporary metaphysics, it would likely suggest a profitable way to re-orientate the philosophical methodology described above. For, as is familiar, there is a reasonably venerable tradition of philosophers looking to refocus their methodology along the lines of those adopted in the physical sciences. If we are to carry out that project properly,

then we will need to have a clear sense of the ways in which those engaged in scientific enquiry deploy their intuitions.

2.5 But were they trained?

However, Ladyman and Ross argue both that scientists' intuitions are the result of an assimilation of successful theories and that their intuitions are not treated as evidence. I've argued only against the latter. But if the former is true then even if the latter is false scientific intuitions would be of a different standing to metaphysical ones. This is, of course, a question of origin. Scientist's intuitions come from one source (the assimilation of successful theories). This is a reliable source. The intuitions of philosophers come from another source: our common sense and common experience. This source is far less reliable.

Once again I think that this argument provided by Ladyman and Ross is suspect. First, we require a distinction. I shall refer to 'trained intuitions' as the sort of intuition that Ladyman and Ross seem to think is used and permitted in scientific inquiry. I shall refer to 'untrained intuitions' to refer to the deliverances of untrained common sense.

Nothing that Ladyman and Ross say (2007: 14-15) establishes that physics does not use everyday intuition (commonsense) as a factor to be weighed when judging theories. Rather, following Wolpert (1992), they (2007: 14-5) cite some cases where particular scientific theories commit us to especially counterintuitive conclusions, but are accepted nonetheless. This is supposed to demonstrate that untrained intuitions do not play an evidential role in science.

But the evidence falls short of establishing their conclusion. All that the evidence shows is that there are accepted, counterintuitive scientific theories. What is ignored is whether or not untrained intuition *is considered* at the point of theory evaluation. If it is, if it plays some non-zero role in determining whether or not a theory is to be accepted, then whether or not a counter-intuitive theory is accepted simply isn't salient here. To repeat: what matters is whether or not untrained intuition is considered *at all* when evaluating a theory. And, once more, there is no data yet available that will conclusively decide the matter either way.

In that case, the way forward for contemporary metaphysics does not look all that bleak. Either, look to the use of intuition in science as a form of justification for the use of intuition in metaphysics (perhaps helping metaphysicians to acquire 'trained' intuitions), or modify the metaphysical method in light of a future data-set from the study of science—perhaps requiring us to see intuitions as of mere heuristic value, or perhaps as something more evidential. At the current time, however, we have some reason to think that intuitions are used in a range of different ways in the sciences. We have no data *at all* that looks to rule out the possibility of there being science done in such a way that untrained intuitions are playing an evidential role. It is, then, hard to see how to sustain the line taken by Ladyman and Ross that the role of untrained intuitions in science is clearly and obviously distinct from the role of untrained intuitions in metaphysics.

3. Wither peer review?⁷

⁷ Again, I'm very grateful to the referee for getting me to say more about this issue.

Ladyman and Ross insist that science involves collective and institutional processes. Ladyman and Ross argue that these processes exist precisely to wash out the effects of differing intuitions of individual scientists. The issue is whether scientific disputes about theory choice are settled by appeal to intuitions and citing evidence of scientists using the term does not amount to that especially since Ladyman and Ross themselves mention that trained scientific intuition is an important component of scientific thought.

A brief digression into some of the kinds of institutional filters that are at work in the sciences might one extremely sceptical of the claim that such filters plays the role of weeding out intuitions. Let's consider just three concerns.

First, for the most part, scientific journals do not practice double-blind peer review.⁸ However, at least one study (Budde *et al.* 2008) found that double-blind peer review significantly increases the chance that work published by female authors will be accepted for publication. Peer review not being anonymous appears to allow significant gender bias's to effect the outcome of review. If such bias's are present in the peer review process, it's hard to put too much stock in peer review as a means through which to *weed out* the judgments of individual scientists—where those judgments may reflect gender-bias, common-sense, intuitions, etc. Certainly, what is clear from this particular study is that the institutional process constituted by peer-review and publication is quite significantly effected by cognitive processes not introspected as present by those involved in the peer review process. If gender bias demonstrably plays a role in peer review, I do not see that we have any grounds to *rule out* the possibility that intuition is playing a significant role in the peer review process.

Second, studies within the psychological literature have demonstrated that confirmation bias is a genuine phenomena; it is the tendency to seek or evaluate evidence in ways that are partial to current beliefs and theories. There are studies that suggest that confirmation bias is present in significant cases in the history of science.⁹ We know, then, that confirmation bias is a genuine issue in scientific inquiry. Again, if this kind of cognitive bias is in play during the peer review process, then it would seem unreasonable to think that the intuition of the reviewer might not also be in play.¹⁰

Third, there is some suspicion that the increased professionalization in science has led to quite serious issues. For instance, Anderson *et al.* 2007, record some of the ways in which practicing scientists describe the peer review process being *consciously* manipulated in the self-interest of the reviewer (and of this practice being ubiquitous), and Fanelli (2010) describes quantitative data that is consistent with the hypothesis that increased professionalization in science leads to increase in bias (in research and review).

There is, therefore, some evidence that the peer-review process preserves some unconscious bias's and is prone, in at least some cases, to being manipulated to the ends of the reviewer. It is hard, I think, to view such an institution as one where the intuitions of scientists might not get some grip. If both conscious and unconscious biases of reviewers are demonstrably present, why not also their intuitions?

⁸ The review process is 'double blind' iff the identity of the reviewers is kept from the authors and the identity of the author is kept from the reviewers.

⁹ See, inter alia, Nickersen (1998).

¹⁰ In fact, current journal practices might be thought to exacerbate the phenoma in view of the advice given to reviewers by (e.g. *Nature* 2014): 'Authors of papers that blow against the prevailing winds bear a far greater burden of proof than normally expected in publishing their challenge to the current paradigm.'

At the current time we have some reasons (given in sections 2.1-2.3) to think that intuitions are used in a range of different ways in the sciences. We have no *data at all* that looks to rule out the *possibility* of there being science done in such a way that untrained intuitions are playing an evidential role. If we wish to advance Ladyman and Ross's case further, we must have that evidence.

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