

The *A Posteriori* Armchair

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A lot of philosophical work appears to be done in the armchair. Observe many philosophers at work, and you will see them sit and think, and then write. Philosophers do many other things as well when they research – they talk to each other, they read things others have written after sitting and thinking, and so on. But most of them can rarely be found in a laboratory, nor can they be found out on the streets with surveys. Furthermore, in most cases, they are not regularly reading lab reports or survey results in order to inform their theorising. Many philosophers give at least the appearance of being cut off from scientific evidence gathering.

Nevertheless, many of the same philosophers purport to be telling us about the world, and not just e.g. what went through their minds while they sat and thought. Philosophers come to conclusions about the nature of space and time, or the workings of convention, or the principles of morally right action, or how the past can tell us about the future, and many other matters that are not merely intellectual autobiography. A central methodological puzzle is whether this armchair way of proceeding is a good way to answer typical philosophical questions, and if it is, how that could be.

This paper defends the view that broadly armchair methods are one good way to approach standard philosophical questions, but that despite its armchair character, much philosophical work is best seen as largely, or perhaps entirely, *a posteriori*. In doing so, it takes a stand against at least two common responses to our methodological puzzle.

The first, which is found explicitly in Kant, is to suggest that philosophy has some distinctive access to the facts it investigates, often some form of *a priori* access. When Kant famously asks “how is synthetic *a priori* knowledge possible?” he is asking what he takes to be the central methodological question in metaphysics, because he supposes that metaphysical matters are to be discovered *a priori* but are about the world (and so synthetic). Some contemporaries wishing to ascribe philosophy a distinctive method that can be carried out in the armchair appeal to a faculty of rational intuition (see Bealer 1996), or a capacity to engage in an *a priori* science (Lowe 2011). Or they seek systematic knowledge about the world through reflection on concepts: see, in their different ways, Dummett 1991, Jackson 1994 or Jenkins 2009.

Another common response to the methodological puzzle is to deny that the armchair methods of philosophers is a good way to find out about the world in the first place. This response comes in many forms, but I will mention two recent forms of this critique in particular. The first is the doctrine that traditional philosophical techniques should in large place be replaced by techniques drawn from the natural sciences. (And perhaps the social sciences, if we are feeling generous.) Metaphysics should be largely replaced by theoretical physics, for example (with perhaps some leftovers available for other special sciences). Epistemology should be replaced by psychology (Quine 1969). Philosophy of language by linguistics. And so on. Some forms of this critique come from non-philosophers, but often they come from philosophers as well. One hears criticisms of

metaphysics from philosophers who hope the physics overlords will treat them well when the revolution comes, for example.

The second form in which criticisms of armchair philosophy has been found has been in the “experimental philosophy” movement, in particular the “positive” arm of that movement, who claim to be able to better contribute to traditional philosophical problems through surveys of philosophically naive informants. (See Alexander et al 2010). While there is not full agreement within the experimental philosophy movement about methodological questions, a common thought is that the answers of these informants will yield information about the application of concepts of philosophical interest. Were it the central task of philosophy to determine the application of widely held concepts of philosophical interest, then traditional philosophy would indeed be under serious threat from experimental philosophy: though even then, it is a threat that might be able to be resisted successfully. This challenge is much less direct, however, for those of us who reject the idea that discovering criteria of concept application is central to philosophy. How the person on the street is inclined to apply her concepts is not terribly relevant to the question of the structure of spacetime or the coherence of physical infinities; and for that matter, I think it is only dubiously and indirectly relevant to whether slavery is wrong, or whether chemistry yields knowledge. Some experimental philosophy appears to challenge armchair philosophy only because of an error about what philosophers were to do in the armchair in the first place.¹

¹ This brief remark should not be read as being dismissive of experimental philosophy in general: when carried out well, experimental philosophy projects tell us things of interest, and indeed sometimes point to phenomena that are philosophically illuminating. A sweeping rejection of the conclusions of experimental philosophers would be only slightly sillier than a sweeping acceptance of their conclusions. (And given

I wish to speak in favour of armchair methods in philosophy, and offer a positive alternative to the challenges of scientism and experimental philosophy. We should be happy to continue to use armchair methods to tell us about the world. But I want to offer a positive alternative that is itself naturalistic, and which does not require anything methodologically special for philosophy like a faculty of rational intuition or Kantian transcendental access to the synthetic *a priori*.² As it happens, I think we can vindicate a lot of what happens in the philosophical armchair without appeal to *a priori* justification or *a priori* knowledge at all.

The account of what philosophers are doing in their *a posteriori* armchairs will generalise to many other theoretical areas to some extent, and just as well. For it is not only philosophers who sit in offices to work, rather than chasing down the results of observations or experiments. In theoretical physics, theoretical biology, theory of literature, foundations of economics, and many other areas, you can find researchers publishing interesting and apparently valuable work, even though they are not close to the empirical edge of their disciplines. What I have to say about philosophy will carry over, to some extent, to theoretical work in other disciplines.

Lest I be misunderstood, I should be clear about what I am not arguing. I am not arguing that there is no *a priori* access to philosophical truths. For all I say here, there might be a

that experimental philosophers reach conclusions that sometimes contradict those of other experimental philosophers, accepting them all or rejecting them all would both be inconsistent attitudes to take.)

² Those who believe in these methods are unlikely to concede they are “philosophically special” in the sense that they are only, or primarily, needed for philosophy. According to many proponents of these special faculties, they are useful and indeed necessary in many areas: see e.g. Bealer 1992, 1996.

priori knowledge and some of it might be interesting (though I have some suspicions on both counts). I discuss in Nolan 2009 pp 283-286 some of the ways that conceptual truths might help improve our metaphysical theories, for example. On the other hand, I am also not arguing that there is anything wrong with using non-armchair methods to answer philosophical questions. Nothing I say here should suggest that it is a mistake to pay close attention to current physics or psychological data when doing philosophy, for example.

One more caveat is important. When arguing that there are things that are, and should be, done in an *a posteriori* armchair, I am making a claim about whether philosophers in fact rely on justifications derived from experience when engaged in armchair methods, and whether there is a role for them doing so. Whether questions in fact pursued, reasonably successfully, through *a posteriori* means *could* in principle be settled *a priori* is not really relevant, unless that option is in practice an available one. It is important to distinguish between those propositions in fact known or justified *a priori*, and those that in principle are knowable or justifiable *a priori*, and likewise for the *a posteriori*: and I am concerned here with what is *known* or *justified a posteriori*, not what is *knowable* only *a posteriori*, nor what is *justifiable* only *a posteriori*.

What I Mean by “Armchair”

A lot of philosophy is done without the direct help of lab experiments or surveys. Most philosophy articles do not cite published experimental or observational work, and even work that does often cites empirical studies only for some of the overall project, sometimes relatively peripheral bits of the overall project. I want to label what is going

on when philosophers are doing this, in a way that can seem at first blush to not involve detailed investigation of the external world. Call it “armchair philosophy”, and the methods employed “armchair methods”. (The origin of the expression “armchair philosophy” might have been disparaging—compare “armchair general” or “armchair quarterback”—but it is undergoing a process of reclamation by at least some of its targets.)

Of course, philosophers do more than sit by themselves and then write things down. Doing philosophy involves a lot of library visits (or modern on-line equivalents), arguing with colleagues, going to conferences, and so on. But even in the libraries, philosophers are often only reading other philosophers, who in turn did their research sitting in offices or engaging with other philosophers. Philosophy is a social enterprise, but this shared enterprise seems to be largely carried out in the armchair—a group armchair, perhaps, to stretch the metaphor. (It may be testimony provides *a posteriori* justification even if the testator’s knowledge is *a priori*—but this sense in which some philosophers’ justifications are *a posteriori* is not one I will be relying upon, or interested in, in this paper.)

In using “armchair” as a label for a phenomenon, without building any analysis of that phenomenon into the definition of “armchair”, I take it I am using the expression slightly differently from some others. I am using it differently from Timothy Williamson, for example, who uses “armchair knowledge” to mean “knowledge in which experience plays no strictly evidential role, while remembering that such knowledge may not fit the

stereotype of the *a priori*, because the contribution of experience was far more than enabling” (Williamson 2007 p 169). I am also not going to be following Williamson in claiming that armchair knowledge is the result of some “third way” that is not quite *a posteriori* and not quite *a priori*, either.³ Nor, I suspect, is my usage quite like Frank Jackson’s when he used the expression “Armchair Metaphysics” in the paper of the same name (Jackson 1994). There Jackson is of the view that what the philosopher does in the armchair is conceptual analysis: but as I read him, this is not meant by Jackson to be controversial, but what rather is controversial is whether the armchair has an important part to play in metaphysics, given that what is done in the armchair is just conceptual analysis. (Conceptual analysis, for Jackson, is the discovery of *a priori* truths through exercising our concepts to classify possible cases: see chapter 2 of his 1998 for a defence of the role of conceptual analysis in philosophy.) As I am using the expression “armchair”, it is up for grabs whether conceptual analysis is the thing that philosophers are doing in the armchair: indeed, I want to argue that a lot of important activity in the philosopher’s armchair is distinct from conceptual analysis, especially as traditionally conceived by people like Jackson.⁴

While I hope I have conveyed what I mean by “armchair”, I hope to avoid having to adopt a very specific account of what it is to be *a priori* or *a posteriori*. As I think of them, knowledge, or justification, or warrant provided *a priori* is knowledge or justification or warrant received not via the senses, or in circumstances where the senses

³ The best attempt I know of to find a suitable third way between an *a posteriori* status and traditional *a priori* status for a lot of armchair knowledge is that argued for by Jenkins 2005, 2009. But I am not going to follow her lead in this area, either.

⁴ I fear Jackson and I may be at different points of the spectrum I describe in Nolan 2009 when it comes to how to treat “platitudes”, for example.

function only in an enabling rather than a justificatory (/warrant providing) role. *A posteriori* knowledge/justification/etc. requires the senses to play an epistemic role beyond mere enabling. Even this characterisation leaves many questions unanswered, such as the question of where the boundary lies. Instead of stating and defending a particular account of the *a priori* and the *a posteriori*, however, I intend this discussion to establish that there is an important role for armchair *a posteriori* techniques regardless of exactly which account of the nature of the *a priori* and the *a posteriori* is adopted.

Two things are worth noting, however: I am concerned with how philosophical matters are, or may be, in fact established, rather than the question of how they may be establishable in principle (what is known or justified rather than what is *knowable* or *justifiable a priori* or *a posteriori*). Things discovered in an *a posteriori* armchair may often be discoverable outside the armchair, and for all I say may be discoverable *a priori* as well (though I doubt it). The second thing worth noting is more minor: I am willing to take the deliverances of introspection to be *a posteriori*, even though it is controversial how “sensory” introspection is. This will make little difference to the discussion except on p 24.

Things Philosophers Can Do in an A Posteriori Armchair

There are many things philosophers can do in the armchair that are methodologically useful. Some of the useful contributions armchair philosophers make may well be *a priori*, for all I say here. For example, pointing out that a superficially plausible option is implicitly contradictory, for example, might well count as an *a priori* discovery that the option is false—and it is the sort of thing that philosophers sometimes do. But despite

the fact that *a priori* contributions may be important (see Nolan 2009 section 3 for more discussion of some of them), my focus here will be on the more clearly *a posteriori* contributions that can be made from the armchair.

I will identify four useful tasks for an armchair *a posteriori* philosopher. She can assemble and evaluate commonplaces which, even though they are not *a priori*, are philosophically significant. She can work out a range of theoretical options, adding theoretical choices and options for understanding matters that were not previously available. She can respond to discoveries of specialised inquiry that were perhaps once cutting-edge enough to not be armchair, but now have achieved sufficiently common currency that being aware of them no longer requires specialist expertise. Finally, and in my view most importantly, she can assess rival philosophical views in the light of general standards of theoretical virtue, such as relative simplicity, unificatory power, explanatory potential, and so on.

Assembling and Evaluating Commonplaces

One starting point for a lot of metaphysics, for example, is the assembly of commonplaces. Puzzles about material constitution can be brought out by considering Dion and his unfortunate foot-removal, or the Ship of Theseus, or the statue Goliath and the clay Lump of which he is made. Commonplaces about time, including the observation that it seems to flow in some metaphorical sense, that fully past objects seem to not exist any more, that one and the same person could have been a bawling infant some decades ago and an academic philosopher now, and so on, give us a lot of material to begin work on. Examples could of course be multiplied—reflection on everyday

opinion serves to deliver up many puzzles in the metaphysics of mathematics, time, space, material objects, events, mind and body, and so on. Indeed, many of us introduce metaphysics to our students by starting from these commonplaces. Metaphysics is not alone in this respect: introducing the theory of descriptions, or arguments for or against utilitarianism, or many other topics can be done by reminding students of commonplaces.

Most of these commonplaces are *a posteriori*—that there are statues made of clay, and that you can squash a figure in clay without destroying the clay, for example. So this is one *a posteriori* source of information for metaphysical investigations. But is it an interesting source? After all, doesn't everyone, or pretty much everyone, already know about clay and statues and replacing rotten planks on ships? Well, yes—these things would hardly be commonplaces if very few people knew about them. But philosophers can sometimes bring something to our attention about these commonplaces that we did not see before, or combine a number of claims that seem unobjectionable to present us with a paradox. Consider, for example, D.C. Williams' paper "The Myth of Passage" (Williams 1951). There Williams reminds us of the common temptation to describe time as flowing, or passing, and the temptation to see this as some kind of "movement" of the present through events: but also shows how attempts to make non-metaphorical sense of this suggestion often result in absurdities. (The suggestion that if there is something like passage of time, there should be a rate of its passage, but attempts to supply rates e.g. in seconds per second of passing seem absurd, is a well known argument from this paper.) Williams's paper could have been written in the armchair (and probably was), but it is an important piece of philosophy that many have found convincing for all of that. However,

that time seems to involve a “flow” or “passage” is plausibly a statement deriving its plausibility from our experience of temporal duration rather than some *a priori* truth, as are commonplaces about what moving spotlights do or what happens when we play movie reels.

Or for a more ancient example of using some *a posteriori* knowledge as part of a philosophical discussion, consider Chrysippus’s paradox of Dion and Theon. (See, for example, Long and Sedley 1987 pp 171-2.) It is *a posteriori* that people have feet and sometimes lose them without dying. By itself, that’s not a very impressive metaphysical discovery. But it is interesting that put together with some other reasonable commonplaces, we seem to be able to derive a contradiction from supposing this happens (or more generally from things gaining and losing parts). That was an important piece of philosophy. Noticing what is philosophically interesting about commonplace pieces of knowledge (or at least firm opinion) is an important philosophical task. And Chrysippus did not need to do much medical science or sociological surveys to bring this out—he did it in the Ancient Greek equivalent of the armchair.⁵

It could be objected, of course, that the real story in cases like these is an *a priori* one. For example, it might be claimed that all we need to get the puzzle of Dion and Theon going is to notice the metaphysical possibility of Dion losing a foot: and while it is hard to argue that it is *a priori* that people sometimes lose feet, it is more feasible to argue that it is *a priori* that it is metaphysically possible for people to lose feet. For example, one

⁵ Well, perhaps it was Academic philosophers who he was responding to who deserve the credit for the paradox. But whoever it was, they were in an Ancient armchair.

might think that someone with the concepts of humans, bodies, feet, change, amputation, and all the rest, is in a position to see that there is nothing incoherent or otherwise impossible about there being a human person who survives the amputation of a foot. And that is all Chrysippus needs to uncover his paradox.

Well, maybe the possibility of surviving amputation can be discovered *a priori*, though I am dubious myself about how much information about genuine possibility is in fact *a priori*. Even if we grant it is so discoverable, however, I do not think this is how it in fact happens. Even if various metaphysical possibilities are discoverable *a priori*, we often find out the premises for this metaphysical work by observing the world around us. Maybe we *can* discover that statues can be destroyed by flattening without destroying the piece of material they are made of: but I think I discovered it by playing with play dough. I suspect Chrysippus discovered about foot amputations by seeing people hobble around minus a foot, or from hearing about people losing feet, rather than purely *a priori* cogitation. Even if it is true that we can establish results similar to those of Williams or Chrysippus without relying on *a posteriori* commonplaces, that does not show that the *a posteriori* pieces of the reasoning were not *in fact* relied upon or do not *in fact* support the philosophical conclusions arrived at. And as a methodological recommendation, I suggest relying upon commonplaces that can be arrived at in a fairly everyday manner, when they are available and useful, rather than trying to establish the metaphysical possibility of those commonplaces by purely *a priori* investigation. Using entirely *a priori* methods to establish whether there is a possible world where people lose feet might be an interesting exercise for illuminating the *a priori*—but I suspect it's much easier,

and less controversial, to show people can lose feet by relying on our commonsense knowledge that it does sometimes actually happen.

In some areas, putting together commonplaces about a subject matter and seeing what tensions there are between them, and what options they suggest when put together, may indeed be a big part of the philosophical task. My impression is that is a big part of the contemporary free will debate: a lot of inputs seem to concern what we count as free action, what sorts of influences free actors are commonsensically affected by, and so on. Some of the investigation does not involve this, of course—deciding whether the world is in fact macroscopically deterministic, or close enough, for example, or whether there is a spiritual entity directly causing our bodily movements or at least some of our brain state transitions. Or those parts of the debate that cite recondite facts about brain function or the psychology of decision. And quite a lot of the free will debate is cast as linguistic or conceptual analysis, where various conclusions are supposed to be more or less obvious to those with clear and correct grasp of meanings or concepts associated with freedom and action. Some or all of that could be based on *a priori* grounds. But to the extent it is a controversial *a posteriori* matter which human actions, if any, are free, it may turn to some extent on commonplaces about human action and what sorts of things in fact influence it.

Relying on commonplaces brings many epistemic risks, of course. Sometimes ordinary understanding of the world can be overturned by subsequent scientific investigation. Sometimes they are incoherent mishmashes of conflicting thoughts, or the results of

thoughtless tradition rather than an epistemically good window on the world. Relying on commonplaces seems to me particularly useful when the commonplaces concern matters that we have frequent practical experience with: commonplaces about human action are to be trusted more than commonplaces about the far side of the moon or the mathematics of infinity. Even in favourable cases, commonplaces are often better suited to be the starting points of inquiry than the conclusion of it. [Removed for blind review].

New Theoretical Alternatives

Another important contribution metaphysicians can make from the armchair is to come up with new theoretical alternatives. Constructive work can be carried out on one topic alone: a new theory of tropes can be developed, for example (e.g. Bacon 1995), or new versions of presentism, such as the ersatzist presentism pioneered by Chisholm 1979 and Zalta 1987, resurrected by Bourne 2006 and Crisp 2005. Or constructive work can be carried out in relating answers to different questions: coming up with a unified framework to treat events, properties and objects, for example, or action, freedom of the will, and responsible belief formation. One particular kind of constructive work that goes in and out of fashion is the construction of entire metaphysical systems. David Lewis's metaphysics is a recent example of a systematic metaphysical picture: and however much of it survives criticism, the edifice by itself is a remarkable philosophical achievement. To take just one part of his system, his neo-Humean proposal for interrelating laws, causation, counterfactuals, chance, and dispositions is a valuable new proposal, which often sets the terms of debate even among those who disagree with him.

Of course coming up with a new theoretical alternative is not the same as answering questions about what is the case: a new theory of tropes will not enable us to tell, by itself, whether there are tropes, and the existence of a new version of presentism will not, by itself, resolve the disagreements between presentists and eternalists. Still, part of getting the right answers to philosophical questions is *coming up with* that answer: arguing for it, or establishing its epistemic credentials, is a vital part of the process as well, but we must not imagine that a philosopher starts with every coherent alternative crisply defined in front of her, and her only challenge is to choose the right one out of the laid-out contenders. Coming up with new ideas and new ways of looking at things is part of the challenge, and this particularly so in metaphysics, where so much seems up for grabs at once and so many phenomena dealt with still seem mysterious.

Another important part of the philosopher's task is to not only come up with particular new proposals, but also to provide maps of the intellectual terrain, so that we can see what the space of likely proposals are going to be, and to notice whether there are any places on the map that should be explored more carefully. I think this synoptic view of philosophical debates is very important, and I think it's a pity that it seems to be undervalued by journal editors: it is very hard to publish unsolicited survey papers, for example. I suppose much of this terrain-mapping happens in books: consider, for example, the effect Timothy Williamson's book on vagueness (Williamson 1994) had on how the vagueness literature construed the intellectual terrain.

Good philosophers seek to do more than just generate not-yet-discussed theoretical positions, even in their theory-building moments. A new theory might be developed to avoid problems or objections lodged against previous variants, or it may be advanced as capturing perceived benefits offered by other theories. Whether responding to standing problems or objections of previous theories counts as both armchair and *a posteriori* will depend to some extent where the discovery of the previous problems came from: if it came from the cutting edge of out-of-the-armchair inquiry, coming up with a theoretical response will seem less armchair, and of course if the objection was *a priori* the theoretical repair will probably seem more appropriately put on the *a priori*-side of the ledger.

Leave aside, for now, the aspect of theory-construction which consists of improving previous theories (though this aspect is not easily separated in practice). What should we say about the status of discovering new options, and relating existing options and possible options to each other? Is this kind of inquiry armchair, and is it *a posteriori*, or at least compatible with *a posteriori* method? One thing that makes it difficult to slot this activity into the usual epistemological categories is that it need not contribute new knowledge about the subject matter at all—knowing what some theories are about the nature of spacetime does not, by itself, tell me much, if anything, about the nature of spacetime. Insofar as it is not bringing knowledge, or even candidates for knowledge, about the object of inquiry, its contribution is neither *a priori* nor *a posteriori*, at least when “*a priori*” and “*a posteriori*” are meant to be features of knowledge or beliefs or evidence. This serves as a useful reminder that there are things that are methodologically

relevant besides improving the epistemic status of our beliefs about the object of inquiry. If this procedure is neither *a priori* nor *a posteriori*, it will, obviously, not be *a posteriori*: but it should still be something a theorist committed to a generally *a posteriori* method should be able to avail herself of. (Re-ordering lab supplies is not *a priori* investigation or *a posteriori* investigation either, for example: but a theorist committed to *a posteriori* inquiry is still allowed to buy herself some new test tubes.)

While locating new theoretical alternatives and discovering the relationships between options may not be inquiry about the topic of central concern, it is the formation of new beliefs and coming to new knowledge about something: about what the theoretical options are, about what theories could be believed, and so on. What about our knowledge, beliefs, and evidence about those topics? It seems uncontroversially available from the armchair. Some of it seems clearly *a posteriori*: discovering what other people in fact believe, or which other options have *in fact* been developed, or could easily be developed. (For example, part of the enabling conditions for options being easily developed include that the general outlines have already been thought of, or that the new option is similar in important respects to a well-studied alternative. And knowing whether those conditions obtain seem an eminently *a posteriori* matter.) Perhaps some of what is discovered are candidates to be *a priori*, though.

Related to the project of discovering new options and gaining information about what options there are and how they are related, is the project of rediscovering and reappreciating already developed options. More historical philosophical documents have

already been preserved than anyone could read in a lifetime. As well as staying in touch with philosophers from the past, following what our contemporaries are doing is not a trivial matter, and I suspect it takes up a significant part of many conscientious philosophers' research time. As we know, properly absorbing, and engaging with, philosophical work requires a lot more than a once-through read.

There are many ways that sitting around reading other philosophers, dead or alive, can contribute to philosophical progress. (And progress towards other goals, as well, such as contributing to the history of ideas.) But *one* of the good things that it does is that it lets us see theoretical options we have not seen before, and appreciate others that we had a more cursory acquaintance with. And if that appreciation of options and their relationships is compatible with being in an *a posteriori* armchair, as argued above, then this aspect of studying past and contemporary philosophers is as well.⁶

Of course, it is not only philosophers who engage with theories this way in the armchair: some inquiries are in touch with their histories to a lesser extent, but nearly everyone at least studies the ideas of their contemporaries. Every theoretical discipline has this armchair component.

Integrating Past *A Posteriori* Investigation

I think that the integration of *a posteriori* investigation by philosophy is one of its most important tasks. One of the jobs of philosophers is to see what the discoveries of particular disciplines should inform our overall world view, and how they should relate to

⁶ I have discussed the value of this engagement at greater length, particularly with reference to metaphysics, in Nolan 2007.

each other. Not only philosophers should be doing this, of course: philosophers should be interested in the links between the epistemology and metaphysics of physics, on the one hand, and biology, on the other: but so should physicists and biologists.

A lot of that work, however, is not particularly armchair. It can involve close engagement with cutting-edge experimental work, as for example when issues at the boundary of psychology and linguistics are being tested. Philosophy's role as synthesiser and go-between may be a place where *a posteriori* engagement is needed, but if I want to claim any of this for the armchair, it seems I have my work cut out for me.

Let me attempt to claim some of it for the armchair. Some of the information we have from scientific inquiry is cutting-edge, and not available from the armchair. But other pieces, central and important pieces, are so widespread they are taught in schools or can be gleaned by reading magazines. That the Earth goes around the sun, that we are the result of millions of years of natural selection, that most of the things we see around us are made up of atoms that come in only just over a hundred varieties of elements, and so on. Some of this information is so obvious, now, that it probably counts as the sort of commonplace I discussed above. But some of it, while not entirely everyday, is still not something we can expect most people to know, even people who were once tested on it in high-school or the first year of college.

Furthermore, some of this widely-available old scientific news has important philosophical repercussions. Determinism has been discussed since the start of

philosophy, but the particular sort of causal determinism that was suggested by Newton's laws raised important issues in the free will debate. The microphysical randomness of quantum mechanics, as ordinarily interpreted, urgently raised puzzles about indeterminism again. Maybe these served as nothing more than prompts for a debate that never relied on this information. But I suspect the debate was informed by the corresponding scientific theories at both points. Whatever we want to say about the free will debate in the immediate aftermath of the scientific discoveries, the usual free will debate among philosophers *today* should still count as armchair even if the participants are informed about the Newtonian picture of the world, or even a pop understanding of quantum mechanics.

Let me describe in a little more detail a real-life example of a philosophical theory that looks rather armchair but relies, to some extent, on scientific information that goes beyond the commonplace. One proposal in the philosophy of biology that is widely recognised in that field as an important contribution, even if it is not uncontroversial, is the Millikan-Neander account of natural functions (Millikan 1984, 1989 and Neander 1991a, 1991b). Neander's presentation of the view is as follows:

It is the/a proper function of an item (X) of an organism (O) to do that which items of X 's type did to contribute to the inclusive fitness of O 's ancestors, and which caused the genotype, of which X is the phenotypic expression, to be selected by natural selection. (Neander 1991a p 174)

Arguments offered for this account involve pointing out how this account of function can illuminate the explanatory role appeals to functions play in biology, for example, or how it smoothly accounts for the existence of malfunction (Millikan 1989, for example, criticises rival proposals for not adequately allowing for malfunctions). Millikan in particular is unlikely to welcome the claim that her account of function is a piece of armchair philosophy, but while both Millikan and Neander know a lot of contemporary biology, that knowledge is not needed to appreciate the arguments they offer, nor, I suspect, to formulate those arguments: though a general familiarity with the theory of evolution and explanations in terms of selection are required.

One way to construe what Neander is up to, at least, is that she is engaged in *a priori* conceptual analysis, albeit of a specialised concept—the concept of natural function as used by contemporary biologists. If that is right, you might want to argue that the truth or otherwise of the theory of evolution is beside the point, or relevant only causally. Once you have that concept, whatever the origin, you can perform conceptual analysis on it, it might be thought. If Neander has an *a priori* result, albeit one that you probably have to be exposed to post-Darwinian biology to have the concept needed for understanding it, then her work, while maybe armchair, is not *a posteriori*. Or so an objection to this example might run.

That might be so, as far as the conceptual analysis project goes, and it is true that Neander sells her project as one of conceptual analysis (Neander 1991a). But there is something else she is offering as well. She is also offering to tell us what truth it is we

are tracking when we say that a heart is for pumping blood but not for timing minutes, or a cockatoo's wing is for flight and not for hiding feathers. To illuminate not only the concepts of biologists, but what in the world we are reacting to with our talk of natural functions, we need the understanding of the biological world and its history that the theory of natural selection provides. She is also offering a "theoretical definition" in Millikan's sense (Millikan 1989 p 291), which requires a characterisation of a thing referred to by an expression, and not just information about the concept. Neander herself claims to be providing both a conceptual analysis and a theoretical definition in Neander 1991a.

So, Millikan and Neander are offering us an account of natural functions that is not *a priori* (even if part of Neander's project might be argued to be), and which relies upon discoveries made about the natural world by Darwin and those since. But it did not require any particular novel population studies or biochemical results. While, no doubt, both Millikan and Neander do have plenty of knowledge about cutting-edge empirical results, it was not needed for this piece of philosophy: the general outline of basic natural selection, the difference between phenotypes and genotypes, and so on are high-school biology (or they should be). If relying on that much background knowledge is enough to take us out of the armchair, we are out of the armchair much more often than it appears: conversely, if one can take notice of the fact that life evolved through natural selection without leaving the armchair, impressive results like Millikan's and Neander's are possible from the armchair.

To take another example from political philosophy. Why do so few Anglo-American political philosophers champion feudal aristocracy as an excellent form of government? (As far as I know, the number of advocates for feudal aristocracy in Anglo-American political philosophy is *zero*.) No doubt part of the reason is fashion, part of the reason is that some uncritically imbibe a prejudice against it from their upbringing or broader society. Part of the reason might be access to timeless truths about communal flourishing gained through *a priori* inquiry. Maybe. But surely part of the reason is that any political philosophers interested in feudal aristocracy are aware of facts about the track record of those forms of government. They are aware of the lack of capital formation and mobilisation, the discontents of underclasses, the problems dynastic principles have for ensuring competent government officials, and many other features that historical feudal aristocracies have had and that contemporary systems that approximate feudal aristocracies still face. It is *a posteriori* that those forms of government in fact work less well than e.g. Canadian democracy, but it is nevertheless information that many political philosophers are likely to know and to use were they called upon to argue against establishing a feudal aristocratic form of government in their own country.

This is an extreme example, and maybe it strays into the category of using commonplaces—maybe it's platitudinous that feudal aristocracy is a rubbish form of government compared to contemporary alternatives. But the broader point could presumably be made with less extreme examples. Political philosophers all know some background economics and history that inform their normative views about what is both better and feasible for societies, especially societies somewhat like ours. Some of that

information goes beyond mere political or social commonplaces, or it does for most political philosophers at least. Some political philosophers, it is true, should not count as armchair theorists, in my sense—they do applied work sensitive to relatively cutting edge inquiry about politics and society. But even the ones that appear relatively armchair are influenced, I conjecture, by information about how political structures and societies have been organised and are in fact organised, information that goes beyond mere commonplaces. And so they should be.

I have been arguing that relying on long-established widespread knowledge about empirical discoveries can sometimes count as armchair work, even if those empirical discoveries relied upon were once not armchair results. I suspect some of my readers will be unconvinced, and, almost as bad, some will think I am right, but only because I have stretched the notion of “armchair” to fit some cases—some readers may think I have scored no more than a stipulative victory. I would like to point a fallback position that may be of interest to both classes of readers. Whether or not the cases I have been talking about fall under core uses of “armchair inquiry”, they are the sort of thing that philosophers indulge in, *a posteriori*, while not apparently being closely engaged with methods of inquiry like experiment or observation. Philosophy, as it is already practiced, involves this kind of work, and this sort of work should be accepted as legitimate, at least in principle, even by people who, for example, reject *a priori* methods. However we classify it, it should be recognised as a significant part of what philosophers are doing, and could help explain the *appearance* of philosophy as an armchair discipline, even if

you would prefer to classify this activity as in the end not armchair, on the border, or inside the margin only because of my stipulation.

Applying Theoretical Virtues

Philosophers are not the only inquirers who work in their offices. Some of what inquirers do in their offices is calculation—or these days, running computer programs to do calculations, or writing scripts to produce calculations. And some of that work is data collection: we can get data out of books and other files, as well as from laboratories and questionnaires. But a lot of that work, in any area, might be best described as *theorising*. Even once data has been collected and numbers crunched, there are still theoretical questions to be answered, and theorists will tell you that this step is not always straightforward, whether they are theorists in physics or ecology or sociology... or philosophy.

This theorising is not just the free play of creative genius—though of course it is often creative. I think there are very important things to do when assessing theories, which are more complicated, and more of an art, than just checking whether the theories are contradicted by any of the data. Virtually everyone agrees with that much, of course. Dealing with theories can be hard. It is more controversial to hazard a story about what *is* going on when we theorise, especially when we theorise well.

The outline of the story I would like to tell is that we judge theories by assessing them for a range of *theoretical virtues*: internal consistency, external coherence, simplicity,

explanatoriness, fertility, unificatory power, and a number of others.⁷ We can make assessments of a number of these by inspecting a theory on its own: we can assess its simplicity or internal consistency and perhaps its potential to be explanatory. We can also make comparative judgements of theories on a number of these criteria without needing to do more investigation than inspection of the theories. Indeed, these comparative judgements are often what we need. I am not sure what an absolute metric of unificatory power would look like, for example, but I have some sense of when one theoretical option might count as providing a *more* unified treatment of certain phenomena than another.

I think we apply many of these criteria in assessing philosophical theories, and it is an important part of our job as philosophical theorists.⁸ This is not to say that this is what we take ourselves to be doing: we need not explicitly think of ourselves as “applying theoretical virtues” to be engaged in practices like finding some consequence of a theory unattractive, or some posit unwieldy or unnecessary, or judging an explanation to be unsatisfactory. I think that many of these theoretical virtues play an epistemic role—insofar as a theory displays these virtues, it is *ceteris paribus* better, epistemically speaking, though I will not try to defend that claim here. But even if these virtues were only pragmatic—valuable, for example, because they make theories easier to use or evaluate, or even valuable for some aesthetic purpose disconnected from getting things

⁷ One recent statement of the view that applying such criteria is an important part of metaphysics, at least, is Sider 2009 p 385.

⁸ This is left out of a number of accounts of philosophers’ armchair methods, including Jackson 1998 and Williamson 2007.

right—then it would still be part of our job *qua* theorists to evaluate theories with respect to them and modify theories to possess them.

Those wanting to argue that some of these virtues are epistemic face the question of whether we are correct in thinking we are getting any closer to the truth in philosophy by employing these criteria. Or failing that, whether we are acquiring better warranted or more justified views of the topic, if checking our theories for truth directly is too demanding. There are a variety of ways of trying to justify theoretical virtues. One is that one theoretical virtue can sometimes be justified in terms of other theoretical virtues. To take just one example, it could be argued that unificatory power is valuable because of the gains of simplicity of an overall theory from unification—*given* that the relevant sort of simplicity was valuable, we would have a demonstration that unificatory power was as well. Or we may construct a theory of methodology and point out the virtues of it: if a theory that claimed simplicity was a fundamental epistemic virtue *itself* made confirmed predictions, was explanatory, etc. etc., then given that such features are virtuous that would give us an argument that simplicity was, indeed, a theoretical virtue. Of course such justifications of theoretical virtues tend to bring the problem of justifying the other virtues into sharp focus: but to some extent this sort of bump-shifting is unavoidable unless there is a way to justify a method without using any method. And there isn't.

Another way to try to justify employing a given theoretical virtue is to look at past successes in inquiry and diagnose what was good about the method that yielded those successes. If simplicity was crucial to Einstein's breakthroughs about relativity, or the

Copernican/Galilean/Newtonian revolution in cosmology, for example, then that may provide us with good reason to think that simplicity, of the relevant sort, is a theoretical virtue. I think of this as a sort of naturalism about method—instead of treating methodology as a prior discipline we have to do before other inquiry, it instead looks around on Neurath’s boat and looks at the method we have already been using. Of course, this approach has several challenges it must face. How do we evaluate “success”? If we just award high marks to those theories that display the methodological characteristics we antecedently liked, the exercise risks turning into uninformative self-congratulation. When a theory has been successful and has properties P_1 through P_n , which features do we count as virtuous and which not? These and other problems are not negligible, nor are they insurmountable—it is almost beyond dispute that sometimes people discover what works in inquiry by looking at what has worked before.

Finally, of course, it is worth remembering that judging theories by how virtuous they are in various respects can be good practice even if we are not in a position to argue that possessing those virtues makes for a good theory. It is notoriously difficult to explicitly articulate how to do something well sometimes, even when we are capable of doing it well: and just as a hunter might be good at hunting without being able to say why she does all the things she does, an inquirer might be a good inquirer even if her grasp of good method is only implicit: indeed, even if she has a false methodological theory about what she is in fact doing. So avoiding inelegant or arbitrary solutions, or finding some explanations satisfying and others lacking, and other such activities can proceed in

advance of being able to say what one is doing and why, just as we can justifiably use induction before being able to satisfactorily answer the riddle of induction.

Theorising does not usually operate in a vacuum—usually there is a body of evidence to be used as a basis for theorising, though theories often do, and often should, have implications beyond the evidence already in front of us. In many disciplines, the evidence gathering needs to be done outside the armchair. But once evidence is available, the comparisons, assessments and repairs of theories can be a largely armchair activity, at least until it prompts further non-armchair investigation.

The case that this theoretical work is armchair is particularly strong when the inputs are available from the armchair. If I am right that gathering and reflecting on commonplaces, spelling out new theoretical alternatives, and integrating well-known but not cutting-edge empirical investigation are all armchair matters, then taking the results of these activities and working out what the best theory is that incorporates them all, or which is best out of a particular range we have to hand so far, will be a process that can be armchair from start to finish.

Judging theories by how well they do by various of the criteria I have discussed is an important part of any theorising, if I am right, and is particularly important in many areas of philosophy where the issues dealt with seem less amenable to direct testing, compared with e.g. conjectures about the spread of HIV or the geology of the Jurassic period.

However, this is not because of some peculiarly philosophical method: theory evaluation using criteria that go beyond mere consistency with data is part of all theoretical inquiry.

But one challenge remains. I claim this is work to be done in the *a posteriori* armchair, but why think applying theoretical virtues and guiding our beliefs by the results is a matter of doing *a posteriori* investigation? Paul 2012, for example, asserts that assessing theories for qualities like simplicity is *a priori* (p 1, 9, 11, 19), though she does not say why. Perhaps she, and those who think like her, take this to be obvious. So why take the process of applying theoretical virtues to be *a posteriori* rather than *a priori*? Isn't rejecting a theory because of a lack of consistency close to the paradigm of *a priori* logical investigation, for example?

Some would be happy to argue that even applying logic is *a posteriori*. But I need not argue for anything quite so radical here. For current purposes, all I want to claim is that a lot of the application of theoretical virtues is *a posteriori*: if some is conceded to be *a priori*, then that will not compromise the point that there is an important *a posteriori* job to be done here. Nevertheless, even the claim that a lot of the applications of theoretical virtues are *a posteriori* is a claim that needs some defence. Let me then defend this claim.

The first thing to note is that evaluating how a theory does with respect to our current body of evidence is a process that requires us to have a body of evidence—and gathering this evidence is usually an eminently *a posteriori* procedure. So when we want to know

whether our theory captures the evidence simply, or whether it explains the evidence, or whether it unifies several phenomena, we usually must rely on our *a posteriori* grasp of the evidence as part of working that out. Perhaps some virtues are entirely internal to a theory (such as consistency or simplicity), and so will be unaffected by this point—but since so much of knowing when to apply simplicity, for example, is knowing how to trade it off against other virtues, if the things we are trading simplicity off with have to be assessed *a posteriori*, then an important part of the process of weighing simplicity against other factors will, after all, have to be *a posteriori* too.

In response to this, our critic can agree that evidence gathering is *a posteriori*, but that the crucial armchair component of applying theoretical virtues is not. What the virtue of theory *X* is, *given* evidence *Y*, is the thing we work out in the armchair, and we could have worked it out even if evidence *Y* had never come in. Consider an analogy. A statistician collects numerical data, and then, in the armchair, runs a range of statistical algorithms on the data. Of course, the process taken as a whole is *a posteriori*. But it does not follow that the mathematics used is *a posteriori*. Those who think that mathematics is an *a priori* discipline will want to say that there is an *a posteriori* part—getting the input numbers—and an *a priori* part—given those input numbers, what are the results of applying these equations?

This challenge is worth some attention. I think there are two ways the process of applying theoretical virtues is *a posteriori*, even leaving aside the *a posteriori* nature of the evidence we are assessing the theories with respect to. (Or, again, that at least most

of the process is *a posteriori*—as I said above, I am not here interested in defending the stronger claim that there is no *a priori* influence.)

The model suggested by the statistician analogy is that coming to statistical conclusions can be divided into two parts—an *a posteriori* part of coming up with data, and an *a priori* part of establishing data-to-conclusion conditionals: if the data is such-and-such, then the mean is M, the standard deviation is N, the correlation between variables *x* and *y* is statistically significant, and so on. The *a posteriori* antecedent plus the *a priori* conditional puts us in a position to infer the consequent. But perhaps there is no equivalent of *a priori* conditionals available when we evaluate theories with respect to evidence.⁹ Plausibly, what we often do is react to the evidence and the theory we are considering, and make a judgement about the worth of the theory. That epistemic process may bring us new knowledge, but there seems to be no reason to think it does so via informing us of a conditional. Nor do we need to have much reason to think we would be as good at evaluating the relationships between various pairs of theories and evidence that we do not accept.

Perhaps our “offline” simulations of our epistemic processes enable us to do this—and obviously we have some ability to tell what we would think given various counterfactual courses of experience—but I suspect they do not in general. Compare: we think a small child who touches the hotplate on a stove can come to learn that the hotplate is dangerous. Indeed, she can still know that it is dangerous five minutes later when it is

⁹ See Hawthorne 2002 for a discussion more sympathetic to the view that evidence-to-fact conditionals are available *a priori*.

still red in colour and warm when she goes near it. Can she work out *a priori* (or even in the armchair) that if she had that course of evidence, she would have had good justification for thinking the stove was dangerous? Plausibly not—she employs an inferential mechanism that is not available, at least for her, to be pulled apart in an “offline” introspective inspection. We sometimes use epistemic mechanisms, when faced by evidence, that we cannot use to get the same result just by thinking about the evidence in an “offline” way. Furthermore, even when we can reason about the epistemic significance of evidence in an “offline” way, this can be the result of introspection of our epistemic mechanisms, and not purely *a priori* cogitation. (Unless we use “*a priori*” broadly enough to cover introspection—but I do not.)

All in all, I do not think the case is very strong that judging explanatoriness of theories, or whether a theory seems too simple to deal with the puzzles we face, or perhaps other kinds of ampliative support of theories by evidence and other believed theories, is to be treated as being closely analogous to what the statistician does, as opposed to what the child at the hotplate does. Exactly what is going on here is a difficult and interesting question, of course, so this route to establishing the *a posteriori* nature of employing theoretical virtues is rather speculative. My remaining reason for thinking the results of the process are *a posteriori* are on firmer ground, I hope.

My remarks immediately above apply with the most force when philosophers apply theoretical virtues relatively instinctively. But the process of gaining justified beliefs, or knowledge, through applying theoretical virtues appears even more clearly *a posteriori*

when we consider more reflective uses of theoretical virtues. When we explicitly use an understanding of what simplicity is, or what makes for genuine explanation in an epistemically relevant way, then what we are doing is less like the unarticulated inferential jumps I was emphasising in the previous paragraphs—though there is almost bound to be some inferential work being done that is not due to the explicit application of articulated rules, of course.

When our understandings of the theoretical virtues are more explicit, though, this is often because we employ theories of the theoretical virtues that are themselves justified *a posteriori*. My view of simplicity and why it matters, for example, is based at least in part on looking at the track-record of theories that have featured various sorts of simplicity.¹⁰ It is also based on reading what experts have to say, both about their experiences of developing and deploying theories, and on their own researches into simplicity—which in turn often rely on case studies and other *a posteriori* justification.

Much of our understanding of what makes for good theories and what does not is based on experience and *a posteriori* study of the phenomenon of theorising. And not just in the last few decades, or through work done by professional philosophers. I think one of the most important discoveries in the “scientific revolution” in the natural sciences in the period from Copernicus to the generations after Newton was how to come up with good scientific theories. We are far from perfect at it yet, of course, and I do not want to suggest there were no good theories of the natural world before the sixteenth century.

¹⁰ I think there are *a posteriori* elements to my discussion of quantitative parsimony in Nolan 1997, for example.

But the fitful process of discovery of how to do science in this period is undeniably impressive even though there is plenty of controversy about what was discovered about method, and how. It does seem to me that those methodological discoveries tended to go hand in hand with discoveries about the physical world—when we discovered that the solar system behaved largely as predicted by Newton’s theory, we also discovered that Newton’s methods led to an empirically successful outcome.

I am enough of a methodological holist to think that some of the insights of the scientific revolution apply to our methods in other fields, including philosophy. But even if we accepted that philosophy has its own methods, some entirely dissimilar from those employed in the natural sciences, presumably part of the evaluation of those methods will be through experience, trial and error, and other such *a posteriori* means. And that means that when we use our conscious understanding of theories of good method to improve our first-order theories, we are relying on an *a posteriori* resource, albeit one that we employ in the armchair.

One interesting question that can be raised is whether the improved understanding we gain of theoretical virtues, gained in practice through trial and error, testimony from old hands, and the rest, could in principle have been discovered *a priori*. For example, could there have been transcendental arguments, or conceptual truths, or somesuch, that would have convinced us that Enlightenment science was a better way to gain knowledge or justification about the natural world than medieval methods, even if, in fact, that was not the way we found it out?

I do not think there are such arguments available, but this would be a difficult negative claim to prove without either a detailed investigation of the virtues in question or some general argument against a priorism in much of epistemology. For current purposes, however, this question does not need to be answered. The issue I want to deal with is to ask whether there is an important role for the *a posteriori* armchair, both as an explanation of actual practice and as a justification of good armchair philosophy.

Whether questions in fact pursued, reasonably successfully, through *a posteriori* means *could* in principle be settled *a priori* is not really relevant, unless that option is in practice an available one. It is important to distinguish between those propositions in fact known or justified *a priori*, and those that in principle are knowable or justifiable *a priori*. I have been arguing that employing theoretical virtues, as it is in fact done, is a procedure that has important *a posteriori* components, and that it rightly employs those. I have *not* been arguing that there is no *a priori* way to do the same work: for all I have said Leibniz's God can come to know all the same things *a priori*. Still, philosophers like us, in the epistemic situation we find ourselves in, would be best to carry on our *a posteriori* way until we have a better alternative.

Conclusion

The purposes of this paper have been partly diagnostic and partly prescriptive. I have been concerned to characterise a lot of what philosophers are already doing without needing to appeal to any special, non-naturalistic philosophical method, and to show that these methods are reasonable ones that we should think are able to significantly improve our epistemic position over a pre-philosophical starting point. To vindicate the armchair

as an important place for philosophy, we need not vindicate *a priori* methods (*pace* Jackson 1994).

I have not tried to show that *a posteriori* armchair methods are the only way, or the best way, to solve philosophical problems. Even if there are better ways in principle, however, I suspect it will be quite a while before we are in a position to replace our *a posteriori* methods entirely with out-of-armchair investigation, or *a priori* cogitation, or some mixture of the two. This is partly because of humility about some areas of philosophy: in many areas we are still at the equivalent of the “natural philosophy” stage of chemistry, or perhaps even the pre-axiomatic stage of geometry found in Ancient Egypt in 1000 BC. But it is partly because of optimism about *a posteriori* armchair methods: the natural and social sciences still have places for theorists, and some of what they do is enough like the activities I describe so that progress will not quickly eliminate the armchair.¹¹

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