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**Methods, Processes, and Knowledge**

*Jack C. Lyons*

Methods have been a controversial element in theories of knowledge for the last 40 years. Recent developments in theories of justification, concerning the identification and individuation of belief-forming *processes,* can shed new light on methods, solving some longstanding problems in the theory of knowledge. We needn’t and shouldn’t shy away from methods; rather, methods, construed as psychological processes of belief-formation, need to play a central role in any credible theory of knowledge.

The belief-forming “method”—the way in which one comes to believe something—has played an important role in some theories of knowledge since Nozick (1981) incorporated them into his sensitivity-and-adherence theory. From the beginning, however, methods have often seemed like a kludge, as a somewhat embarrassing and possibly erroneous afterthought in the theory of knowledge, one that distracts from the core idea of beliefs tracking truth. Some authors reject any appeal to methods altogether, some sneak them in but redirect our attention to other parts of the theory. I think that methods—understood properly—should occupy a central position in the theory of knowledge.

Although methods do heavy lifting for many theories, the proponents of these theories, even those who are explicit in their reliance on methods, do very little to tell us what methods are. Process reliabilism, which is primarily a theory of justification rather than knowledge, has had to confront a related issue: that of type individuating processes and specifying the process type whose degree of reliability determines the degree of justification of the belief. I want to argue here that what we’ve learned about processes in the theory of justification can shed light on the proper role of methods in the theory of knowledge.

Process reliabilism faces the well-known “generality problem” (Goldman, 1979; Pollock, 1984; Feldman, 1985; Conee and Feldman, 1998): the process reliabilist owes us a story about how processes are being individuated if the theory is to be at all substantive. My current belief that there’s a coffee cup on my desk, is that the result of the process of *vision*? of *cognition*? of *forming beliefs about brown liquids*? of *forming beliefs while tired*? etc. Fortunately, I think there’s a good solution to the generality problem, one that doesn’t just parry a famous objection to an important view but that brings genuine epistemological insight in ways that are not directly connected to reliability, and all the while making our epistemology consonant with and grounded in the empirical sciences.

This much I’ve argued and developed elsewhere (Lyons, 2019), where my concern was exclusively with the theory of justification. Here I want to argue that this “algorithm and parameters” (A&P) theory of process individuation, which solves the generality problem by offering a rigorous and scientifically respectable type individuation of processes, can also do important work for theories of knowledge, even those that are not committed to process reliabilism. In particular, I will argue that processes thus construed should be incorporated into all of the prominent externalist theories of knowledge of the sorts currently being defended. In addition, having a theory of processes makes it possible to articulate new and better versions of safety and sensitivity accounts of knowledge.

I want to say up front that I will remain neutral on a number of issues and will not be endorsing any particular theory of knowledge. I will discuss safety, sensitivity, adherence, and competence theories of knowledge, but I won’t be choosing among them. I won’t try to solve any of the well-known problems facing such views except for those problems directly connected to processes, and more generally, the role in the theory of knowledge of *why* the agent holds the belief.

**1. A&P Scheme for Process Individuation**

The overarching idea behind process reliabilism is that a belief is justified just in case it is formed in a good way,[[1]](#footnote-2) where a “good” way is one that’s generally conducive to truth, and the “way” of interest is the psychological process by which the agent arrives at the belief.

Perhaps the most obvious contrast is with internalist evidentialism (e.g., Conee and Feldman, 2004), which holds that a belief is (doxastically) justified just in case it’s held on the basis of good evidence, where “good” evidence is evidence that “fits” the belief. Internalists never tell us what fit is, but whatever it is, it’s not a matter of reliability, or truth-conduciveness, certainly not a matter of *contingent* reliability. Merely having good evidence is sufficient for propositional justification (i.e., the proposition in question is a right thing for the agent to believe, whether or not the agent does believe), but doxastic justification requires an actual belief and requires it to be based on, i.e., held on the basis of, that evidence. This requires (roughly speaking) that that evidence is part of why the agent holds that belief. Note that, at least on the official statements and the standard forms of evidentialism, there are no constraints on the *route* from the evidence to the belief; so long as *h* “fits” *e*, *any way* of getting from *e* to *h* is a good one.

An indicator reliabilism combines features of both views, holding that a belief is justified just in case it’s based on something that reliably indicates its truth (Alston, 1988).[[2]](#footnote-3) Truth conduciveness is back in the picture, but now we’re talking about a truth conducive *basis*, not a truth conducive *process*: again, so long as prob(*h|e*) is sufficiently high (mutatis mutandis for other possible ways of spelling out indication), any route from *e* to *h* is a good one.

Although it’s the reliabilist part of process reliabilism that gets most of the press, the process part is at least as important. That’s the part that distinguishes process reliabilism from both internalist evidentialism and indicator reliabilism: how you get from *e* to *h* does matter, and a process view—reliabilist or otherwise—is required if we are to make sense of this fact. I want to summarize my view about how to type individuate processes and then return to this point.

A fuller articulation, elaboration, and defense of the theory is given elsewhere (Lyons, 2019), but the general idea is that the way a reliabilist (and perhaps anyone else) needs to type processes is by an “algorithm and parameters” (A&P) scheme, whereby

Process tokens x and y are tokens of the same (relevant) cognitive process type iff the complete algorithmic characterization of x is the same as the complete algorithmic characterization of y, and x and y have the same parameter values. (Lyons, 2019, p. 474)

(The “relevant” process type is the one that determines, perhaps in conjunction with other factors, the justificatory status of the belief.) The notion of algorithms here will be familiar to anyone who follows the cognitive scientific literature; it’s the middle of Marr’s (1982) famous three levels; it’s the level at which two systems must be the same to satisfy what Pylyshyn (1984) calls “strong equivalence”. An algorithm is a step-by-step procedure for computing cognitive functions (i.e., mapping inputs to outputs, where the inputs or the outputs, or both, are cognitive, i.e., psychological, states). In an algorithmic characterization, these steps are also cognitive functions, and all of these functions must be further decomposed, until we reach the hardware level, to achieve a *complete* algorithmic characterization. A parameter is a general psychological variable that systematically affects processing, where to say that it is *general* is to say that it figures in psychological laws.[[3]](#footnote-4) For example, it is plausible given what we currently know (although these are all empirical claims), that visual object recognition for artifacts, natural objects, and faces all involve distinct algorithms (Biederman, 1987); while visual object recognition for cups and for books does not. Parameters plausibly include things like degree of attention, time spent processing, degree of match to stored template, amount of contrast in retinal image, etc. This rules out would-be processes that are too general, like *vision* or *cognition*, or those that incorporate irrelevant factors, like *visual belief formed by a middle-aged man on a Tuesday*, and ensures that relevant factors, like lighting conditions and the sleepiness of the cognizer, will be incorporated.

But a trivialization worry threatens this and every other attempted solution to the generality problem. In this particular context, the worry takes the following form: a function can be viewed as simply a set of ordered input/output pairs. So too with algorithms, since an algorithm is just a number of functions strung together. So an algorithm is (or can be represented as) just a set of ordered pairs. But for any given set, why should we settle on that one, rather than one of its subsets, or one of its supersets? This just *is* the generality problem, and it’s surprising how silent some prominent solutions are on exactly this question (see Lyons, 2019, section 2.5). Obviously the solution is to preclude the sets that make up algorithms from being gerrymandered in this way. But how? I think the answer is to require that our algorithms specify no more and no less than that processing performed by a “cognitive system” in the sense of Lyons, 2001: in particular, the underlying mechanisms that compute that function must be capable of doing so in isolation, and no proper part of that mechanism computes any subset of that function (for details see Lyons, 2001, sec. 4; 2019, sec. 2.3). If, say, visual recognition of natural objects is isolable from visual recognition of artifacts—i.e., someone could retain the one capacity while losing the other, then they’re distinct algorithms, and the reliability of the one doesn’t count toward the reliability of the other. Conversely, if, say, cat recognition is *not* thus isolable from dog recognition, then they don’t involve separate algorithms, even though one could gerrymander the cat-related input/output pairs away from the dog related input/output pairs. This, in conjunction with what I assume are the empirical facts about isolability, has the important consequence that algorithms have to be somewhat *general*, and in particular, that differences of “mere content” do not make for differences of algorithm; the fact that two beliefs have different contents does not by itself allow the claim that they result from different processes.

This way of individuating algorithms is, I think, assumed in the cognitive sciences, and ‘algorithm’, thus construed, picks out a natural kind. But it does not support a typing of processes that is fine enough for epistemological purposes. Not all my visual beliefs about natural objects are equally justified. Parameters solve this problem by allowing us to type-distinguish process tokens that involve differences in lighting conditions, distance from stimulus, length of time looking at stimulus, number of stored internal matching templates, their tuning curves and distance from neighboring templates, etc. Parameters are variables that figure into psychological laws, and thus, the A&P typing is firmly grounded in natural, psychological kinds.

The A&P typing looks well poised to make the distinctions the reliabilist wants to make, counting inattentive, System 1 answers to logic problems as less justified than careful, System 2 answers to those same problems;[[4]](#footnote-5) and counting strong, vivid, recent memories as more justified than old, hazy ones, etc. Nearly equal in importance is the simple fact that A&P is *committal*: that it gives specific, fairly rigorous answers to questions about type individuation of the relevant processes. This is true even if we don’t currently possess the empirical facts needed to know exactly what those answers are. It is thus not prone to charges of cherry-picking: charges that the reliabilist is tailoring their choice of process type with an eye to matching the intuitive degree of justification.

A&P is “internalist” in the sense of holding that any two individuals alike in all psychological respects are using the same processes. It is “externalist” in the sense that there are no phenomenal or introspectible constraints on processes: one needn’t have any idea which process she’s using, since two processes could differ in type only in virtue of factors completely invisible to introspection. One needn’t be a reliabilist to think that *how a belief was arrived at* is highly relevant to its epistemic status. One could endorse A&P or something very much like it but hold that the good processes are necessarily, rather than contingently, justification-conducive. Such a move would be compatible with internalism in the sense of mentalism, but not in the sense of accessibilism.[[5]](#footnote-6)

A&P assumes scientific realism; it assumes that there are facts of the matter about what processing could counterfactually occur in the absence of other mechanisms; and it assumes that there is a distinction between those variables that figure in psychological laws and those that don’t. These limit the amount of rigor we can claim on behalf of A&P: we can aspire only to scientific rigor, not mathematical rigor. Nevertheless, this is considerably more than we are accustomed to in this debate, and it is *vastly* more rigorous than simply relying on our intuition to choose the relevant type, on an ad hoc basis.

I will assume A&P in what follows, and I will use the term ‘process’ as a technical term whose meaning is given by A&P; I will often just write about “the process” responsible for some belief, when it’s clear that I mean the *relevant* process *type*.

**2. Process Theories and Indicator Theories**

One helpful feature of A&P is that it highlights the difference between process reliabilism and indicator reliabilism, and between process theories and indicator theories more generally. If you let processes be individuated so narrowly that each input/output pair counts as the relevant process type (e.g., Comesaña, 2006), then the resulting view is effectively a kind of indicator reliabilism. A&P, on the other hand, is quite different, and this is part of what’s at stake in denying that differences of mere content can make for differences of process type. If I’m in fake barn country, my current visual experience (or retinal stimulation, or cortical activity, whatever we’re taking the input to be) does not reliably indicate the presence of barns, so that very narrow belief-forming “process” is highly unreliable; hence, counterintuitively, my barn beliefs would be unjustified in such an environment, according to indicator reliabilism. A&P, on the other hand, is required to claim that the process that produces barn beliefs is the very same process that produces house beliefs, car beliefs, etc., so its reliability isn’t significantly threatened by the presence of fake barns. Different house or barn beliefs will differ in their degree of justification, depending on lighting conditions, distance, and so on—that is, depending on the values of the operative parameters—but not depending on whether they’re about houses or barns. Notably, the presence or absence of barn facades in the vicinity is not a psychological variable, so it is not a parameter.

In fake barn country, indicator reliabilism makes more distinctions than we want, treating vision for barns differently from vision for houses. But indicator reliabilism also makes too few distinctions. Suppose I infer q from p and ‘p ⊃ q’. Indicator reliabilism (and evidentialism more generally, if we take the theory at its word) has to say that every instance of this inference is as justified as every other, independently of how drunk and inattentive the agent is at the time, and independently of whether they’re using a quick-and-dirty, semantically driven System 1 heuristic or a slow deliberate syntactic analysis in System 2.[[6]](#footnote-7) Similarly with bad inferences, like affirming the consequent. The indicator reliabilist has to say that affirming the consequent is always (much) worse than modus ponens. A&P says otherwise. Suppose I sometimes solve logic problems by quickly and unconsciously imagining a model where the premises are true and trying to imagine a model where the conclusion is false.[[7]](#footnote-8) How reliable an instance of that process will be will depend on general factors, like how good I am at thinking of counterexamples when there are some, and the specific parameters, like how many premises there are, how long I spend searching for models where the conclusion is false, how much of my attention is diverted elsewhere, etc. But whether the inference is an instance of modus ponens or affirming the consequent is not, for A&P, a parameter: the hypothesized psychological mechanism is one that is not sensitive to that difference. Therefore, the validity judgments that turn out to be true (e.g., those conforming to modus ponens) will, everything else equal—i.e., under the same parameters—be no more justified than the ones that turn out to be false (e.g., those conforming to affirming the consequent). Although perhaps initially counterintuitive, this strikes me on reflection as exactly the right result, and it’s an advantage of process reliabilism over indicator reliabilism, and over evidentialism more generally, that it can get this result. Reliabilisms aside, I think the sorts of cases at issue in this paragraph provide a strong argument in favor of some kind of process-oriented theory of justification over an evidentialist/indicator-oriented theory. Even if it’s not the *reliability* of the process that accounts for its epistemic status, it’s the *process* used that determines (doxastic) justification, not mere fit with the evidence.

Just as A&P’s typing differs from that of the evidentialist, it differs also from the “intuitive” typings one often sees in the literature. We just saw that *modus ponens* and *affirming the consequent* aren’t processes: they’re functions that can each of which be executed by the same, or by very different processes, depending on what algorithm is used and what the parameter values are on that occasion. For similar reasons, neither will *hasty generalization*, or *looking at a barn*, or *reading the newspaper*, etc. count as relevant processes.

**3. Methods, Ways, Bases, Competences, and Processes**

When we move from the justification literature to the knowledge literature, we tend to see a lot less talk about processes as such, perhaps in part because theorists who identify as process reliabilists haven’t been very active in the discussions of knowledge. Thus, instead of the term ‘process’, we see theorists talking about “ways,” “methods,” (e.g., Nozick, 1981; Pritchard, 2009; Becker, 2012) “bases,” (Sosa, 2007; Pritchard, 2009) and “competences” (Sosa, 2007; 2015; forthcoming). Each of these, in its own way, aims to capture the idea that knowledge depends in part on *why* the agent holds the belief.

As most readers will recall, Nozick (1981) starts out with a very simple account of knowledge as (true) sensitive and adherent belief.[[8]](#footnote-9) S’s belief that p is *sensitive* iff S wouldn’t believe that p if p were false; it’s *adherent* iff if p were true, S would believe that p—i.e., in all the nearby worlds where p is true, S believes p. But this view quickly needs to be complicated because in the other possible worlds relevant to whether S knows that p, S might have different evidence regarding p. So S might have just chanced to witness some event, e.g., Jesse James’s mask slips off for a moment during the robbery; the witness knows it’s Jesse James even though there are nearby worlds where she doesn’t see his face and so remains agnostic, so her belief is inadherent. Or a grandmother sees her grandson when he comes to visit and believes him to be alive and well; but if he hadn’t been, others would have lied about his health to spare her the worry. Thus her belief is insensitive, even though it’s a case of knowledge (Nozick, 1981, p. 179). The intuitively appealing solution to these difficulties is to bring in the *methods*, or *ways*, by which the agent comes to hold the belief. Then, for example, we can replace the sensitivity requirement with a method-sensitivity requirement: S’s belief that p, formed via method M, is *method-sensitive* iff if p were false and S were to use M to arrive at a belief whether (or not) p, then S wouldn’t believe, via M, that p (Nozick, 1981, p. 179). Similarly for *method-adherence*.

Some prominent writers (e.g., Roush, 2005; Zalabardo 2012) disavow any reliance on methods. Some (e.g., Adams and Clark, 2005; Becker, 2012) explicitly embrace Nozick’s terminology. Others are more circumspect but seem to have something like methods in mind. Pritchard (2009), for example, states:

S’s belief is safe iff in most near-by possible worlds in which S continues to form her belief about the target proposition *in the same way* as in the actual world, and in all very close near-by possible worlds in which S continues to form her belief about the target proposition *in the same way* as the actual world, her belief continues to be true. (p. 34, italics added)

He sometimes (including in that same article, but see also, e.g., Pritchard, 2020) writes about belief formed “on the same basis as” the original, rather than formed “in the same way as”. The “basis” locution suggests something like evidence, while the “way” locution suggests something like processes; but I think Pritchard means for the two phrases to be equivalent, in both cases to be pointing to something more like a process than like a piece of evidence.

Let’s use Nozick’s term ‘method’ to mean a causal pathway to belief formation. Methods, so generically construed, can be very broadly or very narrowly individuated. There is thus a difference in method between System 1 reasoning and System 2 reasoning; and at the same time, there is also some more general method that encompasses both. Methods make up a genus of which processes—in the present, technical, sense—are a species. A different species of method was alluded to above, in connection with evidentialism and indicator reliabilism. I’ll stipulate that an ‘indicator’ is whatever it is that a belief is based on. By “based on” here I mean that well-known but difficult to characterize relation that makes the difference between propositional and doxastic justification (see Carter and Bondy, 2019). Everyone agrees that beliefs can based on other beliefs; most epistemologists think they can also be based on nondoxastic experiences; and some even hold that they can be based on extramental facts (e.g., my belief that dinner is ready might be based on the fact that there’s a particular odor coming from the kitchen). For the present purposes, I’ll be inclusive and allow all these to count as indicators.[[9]](#footnote-10) What matters is that indicators are *particular*, and that they’re inputs to the psychological processes that result in belief.

Some “on the basis of” talk seems to be aimed at indicators, rather than processes. This is what Sosa (1999; 2007), for example, seems to have in mind by “basis-relativity”: “What is required for a belief to be safe is … that it be based on a reliable indication. … Indications are deliverances, as when you ostensibly perceive, or remember, or deduce something or other” (1999, p. 149). But not all “basis” talk can be understood in this way. If we say that S believes p “on the basis of flipping a coin” (Pritchard, 2013, p. 158) or on the basis of reading the newspaper, etc., we must mean for “basis” talk not to pick out an indicator (even allowing that *the coin’s coming up heads* might be an indicator, *flipping the coin* is not) but a method of some other sort. However, I think it’s clear from section 2 that that method isn’t a process, either.

I won’t try to figure out who is or was committed to which understanding of “bases”. What matters is that claims about method-sensitivity, or method-safety, or method-adherence—in other words, claims about the basis-relativity of sensitivity, safety, or adherence—can be understood in at least three different ways: as tying the epistemic status of the belief to (a) the indicator on which it’s based, (b) the psychological process by which it’s formed, or (c) the overt actions one took in coming to that belief.

This point deserves elaboration. Retaining ‘method’ as a very broad and inclusive term, it seems to me that there are four main ways in which methods are construed, individuated, and taxonomized in the literature. This is almost always inexplicit, and there may be others, but here are the four that are most obvious.

An *evidential* construal of methods individuates methods entirely on the basis of the evidence/belief pairings (i.e., the indicator/belief pairings) involved. This construal individuates methods by input/output functions, where the only inputs we are concerned with are indicators. Patterns of retinal activity, for example, are inputs but not indicators (if visual beliefs are *based on* anything, they’re based on experiences, not retinal patterns), so the restriction to inputs that are indicators is a substantive one. These functions can be individuated broadly or narrowly, a point I’ll return to below.

By contrast, an *algorithmic* construal of methods identifies and individuates methods partly on the basis of the psychological mechanisms by means of which the inputs are paired up with outputs. Specifying the algorithm requires specifying the function, but it also requires specifying the route for getting from the inputs to the outputs. A&P, obviously, offers an algorithmic construal of processes. A&P does not limit the inputs to indicators, though some different algorithmic construal could do so.[[10]](#footnote-11)

A *procedural* construal of methods identifies and individuates them on the basis of the overt actions one takes in the way of coming to believe. So I might come to believe p by looking, or flipping a coin, or consulting Wikipedia, or phoning my father. None of these specifies a function (except perhaps of a wildly disjunctive and grue-like sort), so they don’t specify an algorithm either. Of course, we might consider *believing p in response to q* as an action, in which case, the procedural and the evidential would overlap. To keep the categories separate, I’ll take the “actions” relevant to the procedural construal to be only *overt* actions that aren’t reducible to cognitive functions or algorithms.

Lastly, an *organonic* construal of methods individuates them by the rules for reasoning that they satisfy or violate. Thus, we might think of the method used as *modus ponens*, or *hasty generalization*, or *wishful thinking*, etc. As far as I can tell, this way of individuating methods is more common in the justification literature than in the knowledge literature (see Lyons, 2019, sec. 4.1), but I mention it here for the sake of relative completeness. The organonic approach could be viewed as a species of the procedural approach, although there’s some overlap with the evidential construal, as in the case of modus ponens, depending on how broadly the evidential construal individuates its types. In what follows, I’ll concentrate on the first three: the evidential, the algorithmic, and the procedural construals of methods.

It is clear that each of these three construals of methods is subject to the generality problem or something very much like it. I discussed this in section 1 above, in connection with the algorithmic construal. There I treated algorithms as strings of functions and functions as sets of ordered pairs. Since the evidential construal is a kind of functional construal, it’s clear that the generality problem is at least a prima facie problem for it. But maybe the restriction to *evidential* inputs—indicators—fixes things? No, there’s still a generality problem here, in fact, two of them: a problem of breadth and a problem of depth. The depth problem is about where in the causal chain we want to locate the evidence, the indicator. This is clearest in the case of perceptual judgment, where there are actual debates in the literature (and not just philosophical worries about indeterminacy) about whether my belief that there’s a cup on the table is based on my visual experience of the cup or based on the fact that there’s a cup. This is part of the debate about epistemological disjunctivism, the debate about whether the agent has the same evidence in the case of hallucination that they have in the case of veridical perception (McDowell, 1982; Millar, 2011; Byrne, 2014; Schellenberg, 2015). Similar worries could be raised for memory belief: is my current memory belief based on the old, say visual, experiences; or is it based on my current, seeming-to-remember, experience? These problems are exacerbated further if we take seriously the possibility that the unconscious states involved in the formation of percepts (Siegel, 2017; Ghijsen, 2018) or memory seemings (Salvaggio, 2018) play an evidential role. The breadth problem is simply the one canvassed in section 1 above: how big is our set of inputs and outputs going to be? is it just the singleton set? if not, which other indicator/beliefs are included? Suppose S infers from ‘Most mammals have fur’ and ‘Mauyak is a mammal’ that ‘Mauyak has fur’. Considering only the premises and conclusion, we can describe this in a number of different ways: *nondemonstrative reasoning*, *statistical syllogism*, *inference about biological matters,* or *reasoning from (the specific beliefs that) ‘Most mammals have fur’ and ‘Mauyak is a mammal’ to ‘Mauyak has fur’*.[[11]](#footnote-12)

If we think of methods procedurally, the threats from the generality problem are even more obvious. Suppose I decide my lottery ticket lost by consulting a newspaper. Is the method *consulting the Democrat-Gazette (specifically)*? *Consulting whatever the local newspaper happens to be*? *Believing the first thing I read*? *Forming a belief about numbers, on a Tuesday, after looking at black squiggles on a white background*? Does it matter that I’m reading the report, rather than listening to it on the radio? Etc. Gerrymandered methods are easy to come by. Is *believing that p iff John is not both fat and not fat* (Luper-Foy, 1984) a method? Is *believing that p just in case your crystal ball doesn’t shatter when stroked with a feather* (Zalabardo, 2012)?

In lieu of “methods” talk, we sometimes encounter talk about belief that results from or manifests “abilities,” “skills,” or “competences” (Greco, 2010; Sosa, 2015; forthcoming). All these terms seem to be picking out ways of coming to believe something, but again, the authors who rely on these tell us relatively little about what they are and how to individuate them. In case this isn’t obvious, consider a well-developed and well-known framework of Sosa’s (2007; 2015; forthcoming). At the core of Sosa’s view is the claim that (animal) knowledge is apt belief: that is, belief that is true because it results from the exercise of a belief-forming competence. A belief that results from a competence is at least in some sense justified, though justification doesn’t figure as prominently in Sosa’s epistemology as it does in some others’. So how are we to understand competences? A competence is a disposition to get it right, but this doesn’t answer the question. Suppose I’m very good at identifying magpies when I see them, but less reliable in identifying birds more generally. It looks like we have two different competences here: a weak *bird-identifying* competence, and a strong *magpie-identifying* competence. And of course, there are more: a *biological species identifying* competence, a *visual categorization* competence, etc. Which of these many competences determines whether the belief is justified and/or known?

Sosa (2015; forthcoming) carefully distinguishes the “skill,” “shape,” and “situation” involved in having a competence. (You might, for example, retain the *skill* of driving a car, even when drunk or asleep (in bad *shape*) and/or on icy roads or in space (bad *situation*).) This allows him to say that brains in vats have competences in one sense but lack them in another: they have the “innermost” competence—the skill; even though they lack the competence in situ—the situation is one in which, like an expert archer in gusty winds, the agent is not likely to achieve success. Thus, he claims that the brain in a vat is in some sense epistemically justified, but in another sense unjustified.

These distinctions don’t help with the question at hand, however, which could be framed as concerned only with the individuation of “skills”. And the ecumenical response would not seem at all appropriate in this case, anyhow. Stipulating that the agent is in good shape and in a good situation, we still need to know whether the skill being exercised is the highly reliable magpie identification skill, the fairly unreliable bird identification (non-)skill, the moderately reliable species identification skill, etc., and it wouldn’t be at all plausible to say that the agent is justified in one sense but not in another sense, merely because these skills are nested in this way.

More generally, it is not clear, to me at least, what kinds of things Sosa takes doxastic skills to be, in particular, whether he takes them to be (reliable, given cooperative shape and situation) dispositions to form certain beliefs, dispositions to arrive at beliefs *by certain routes*, or something quite a bit less committal? That is, is he thinking of skills evidentially, algorithmically, or procedurally? Or is he even thinking of them as methods—i.e., ways of coming to believe—at all?

At this point, Sosa faces a trilemma of sorts: either employments of competences are individuated evidentially, algorithmically, or procedurally. If he individuates them evidentially, then his view is subject to the generality problem; and it’s also false—the evidential individuation of methods gives us the wrong answer about specific cases, as I’ve been arguing. (These two complaints are compatible, because even though there’s an indeterminacy about methods that’s crippling in its own right, it’s also true that however that indeterminacy is resolved, it gives the wrong answer.) If competences are individuated procedurally, then the generality problem renders the view a non-starter: any belief is the result of vastly many widely divergent *procedures*; the individuation is so unconstrained that the view doesn’t even have enough content to be false. Finally, if he individuates them algorithmically, Sosa’s view is really just a form of process reliabilism: an apt belief is one that is true because it was produced by a reliable cognitive process, in something very much like the A&P understanding of ‘process’. The theory is of course motivated in a distinctive way and emphasizes distinctive features (and of course, Sosa’s larger theory covers much more than just aptness), but is still a version of process reliabilism. As the reader will have guessed, I think that by far the best response to this trilemma is to grasp the third horn and embrace the algorithmic construal and process reliabilism. The only reason I present this as a trilemma and not just exegesis is that I suspect that some fans of the competence view—including maybe Sosa himself—think it’s importantly different from process reliabilism.

It’s possible, as alluded to above, that Sosa isn’t thinking of competences as methods at all, but bare dispositions to get things right. Such a view might hold, for instance, that S’s belief that p was competent just in case prob(p|S believes p (while in good shape and situation)) was relatively high. This would avoid the generality worries of the previous paragraph, but it’s hopeless for providing a theory of justification or knowledge for exactly the reason that it makes no reference to methods. We saw above that even the evidential construal of methods fails to take sufficiently seriously *why* the agent believes as they do. All the worse for a theory that omits methods altogether.

In all of this discussion of competence, I’ve been using ‘competences’ as a noun, a count noun, no less. But nothing really hinged on that; all the same points could have been made using only the adjectival and adverbial forms. Was my magpie belief competently formed? The answer depends on whether or not my incompetent performance with other birds counts against the competence of the magpie belief. It also depends on whether competence only requires that this output is the appropriate response to this input, or whether it also requires the right kind of causal route from inputs to outputs. In essence, all the same problems still arise, whether we think primarily of “competences” or “competent performances”.

**4. A&P as Supplying Methods for a Theory of Knowledge**

A&P is a theory of process types, and those processes, I have argued, are central to doxastic justification: whether a belief is (prima facie, doxastically) justified is determined by the (A&P) type of process that produces it. The knowledge literature in epistemology is rife with methods talk, although methods are not typically, and never explicitly, understood as processes in the A&P sense. I want to argue now that anyone who appeals to methods in their theory of knowledge should understand these methods as processes. Again, I’ll use ‘process’ often to mean ‘process type’ and always to mean processes as identified and individuated by A&P.

The first reason to think we should equate methods and processes is just considerations of theoretical parsimony. We already need processes to make sense of justification, or so I’ve argued. If we need methods to make sense of knowledge, and if processes will do the work that methods are required to do (I haven’t argued for either of these yet, but I will below), then, everything else equal, we should if possible rely on the already developed concept, rather than try to develop a whole new, different one.

Second, although I cannot argue for it here, I find it very plausible (as do many others) that knowledge requires justified belief; I’m unaware of any remotely convincing instances where an agent knows that p but is simultaneously unjustified in believing that p.[[12]](#footnote-13) This gives us further reason to equate methods and processes. Not only are we making do with one theoretical problem/posit rather than two, as was the previous point; but if our K-methods and J-methods were different—i.e., if the “way” of belief-forming that determines whether that belief is knowledge were different from the “way” that determines whether it’s justified—this would sit uncomfortably with the idea that justification contributes constitutively to knowledge. On the other hand, if we identify methods with processes, we tighten the connection between justification and knowledge and offer some hints of an explanation for why they’re so closely related. This consideration, of course, won’t impress those who think that knowledge is simply safe (or sensitive, etc.), even if unjustified, belief. But it gives the rest of us some reason to equate methods with processes.

Third, and independent of the previous points, I think a theory of knowledge needs methods anyway, and processes offer the best way to make sense of methods. This claim has two conjuncts, and each needs argument. The first reason to think a theory of knowledge needs to make mention of methods is simply that it is required to handle the cases. The grandmother and Jesse James cases, for example, suggest that we need to relativize sensitivity and adherence to methods; Sosa (2007) uses similar cases to argue that safety needs to be relativized to methods.[[13]](#footnote-14) A second and more general reason to think we need methods is the simple fact that whether someone knows something depends, clearly, on *why* they believe it. Just as it’s possible to have propositional justification but lack doxastic justification, by “believing the right thing for a bad reason,” something analogous seems possible in the case of knowledge: if we believe p for a bad reason, then we don’t know p, regardless of how safe, sensitive, adherent, or whatever, that belief happens to be.[[14]](#footnote-15) Since *non*-method-relative safety, sensitivity, adherence, etc. don’t seem to guarantee the impossibility of believing for a bad reason, some explicit relativization to methods is necessary.

But this is only an argument for methods in some generic sense. Why think that methods should be understood as processes (i.e., A&P processes) specifically?

An initial reason is that we need an understanding of methods that is consistent. Nozick, for example, requires different and incompatible construals of methods at different points. What little he says about method individuation commits him to an evidential construal, at least for methods that involve experiences: for methods that “have an upshot in experience”, “(a) no method without this upshot is the same method, and (b) any method experientially the same, the same ‘from the inside’, will count as the same method” (1981, pp. 184-5).[[15]](#footnote-16) And this isn’t optional; he needs this to make his adherence condition work. The witness who sees Jesse James’s mask slip has an inadherent belief: in nearby worlds the mask doesn’t slip and they don’t form a belief about who’s robbing the train. If methods are going to come to the rescue, they can’t be methods like *vision*, because in those same nearby worlds where the mask doesn’t slip, vision doesn’t produce the belief that it’s Jesse James. Methods only rescue adherence here if sameness of method requires sameness of visual (/experiential) *input*. Elsewhere, however, (only a page later) he needs a procedural construal of methods. A person who dogmatically believes whatever his parents tell him fails to know, even when his parents happen to tell him a necessary truth. That’s because *dogmatically believing whatever your parents tell you* is a bad method, one that could have just as easily resulted in different, false beliefs (1981, p. 186). This, obviously, assumes a procedural rather than an evidential construal of methods. The *experiential* input in this case is something like the auditory experience of the parent saying “349 is prime.” Believing *on that basis* that 349 is indeed prime will lead to truth in all possible worlds. Thus, for this sort of case, methods *cannot* be experientially individuated. If we settle on processes as the way to construe methods, we won’t be tempted to contradict ourselves by switching back and forth between different and incompatible understandings of methods.

A second reason is that any construal of methods is subject to a kind of generality problem, and A&P offers the only way (that I know of) to solve the generality problem. In Nozick’s grandmother case, for example, the methods solution only works if seeing her grandson involves a different method from being convinced by his friends that he’s alive and well. The mere fact that the two tokens fall under different descriptions does not indicate that they’re type-distinct. The token methods in this case do seem intuitively quite different, but the question is whether it’s possible to make good on the intuitions, especially in light of the worry that our intuitions about sameness of method might be driven by prior judgments about whether the case is an instance of knowledge. This particular example is sufficiently extreme that we might think the worry rather academic. But there are other cases where sameness or difference of methods is far from obvious in the absence of a general theory. Does seeing a barn in a normal environment involve the same method as seeing one in fake barn country? Does seeing a barn involve a different method from seeing a house? from seeing a barn facade? Does making a modus-ponens-conforming inference by use of a mental models heuristic involve a different method from affirming the consequent through that same heuristic? Does it differ from explicitly reasoning through modus ponens?

Without at least some hint of how to answer these and similar questions, the appeal to methods seems to be mere bluffing. On a procedural construal of methods, anything goes; even on an evidential construal, there are too few constraints. Suppose I’ve just taken a strong hallucinogen that makes me very likely to hallucinate hammers; right now I’m seeing (not hallucinating) a hammer (Luper-Foy, 1984). Do I know there’s a hammer? It depends on whether seeing and hallucinating a hammer involve the same method. Without pinning down what we mean for methods to be, we give ourselves a reusable blank check; “methods” could range from indicators to procedures and back again, as the need arises and without any guiding or constraining principles. A&P, on the other hand, gives fairly definitive answers to these questions,[[16]](#footnote-17) and (again, as far as I know) no other theory of processes or methods does.

Third, a theory of knowledge that takes its methods to be processes can thereby avail itself of all the other positive features of A&P: that it integrates our epistemology with the empirical sciences, that in doing so it opens up the possibility of empirical discovery about epistemological facts, etc. Of course, not all epistemologists will think of these as especially positive features, but I do, and I suspect that some others will, too.

**5. The Primacy of Methods**

Perhaps the best argument for identifying methods with processes is that having a fairly rigorous understanding of processes allows us to be wholly unapologetic about our reliance on methods and thus also about the centrality of methods in the theory of knowledge. We can then move from thinking of knowledge primarily as a mental *state* (belief) that has a certain relation to truth (tracking, in some sense) to thinking of knowledge primarily as the result of the employment of a *process* that has a certain relation to truth. If you don’t know what methods really are, or you suspect that invoking methods is just bluffing, or if you need methods to sometimes be one thing and sometimes another, then you’re not going to want methods to appear as the centerpiece in your epistemology. Hence some of the ambivalence and shiftiness we find in the literature surrounding methods. But A&P solves all those problems, allowing methods to serve as the core concept in our understanding of knowledge.

Up until now, I’ve been discussing methods/processes in the abstract, in isolation from any specific proposal about the nature of knowledge. Now I want to show how a process construal of methods can figure into four well-known types of theory of knowledge: the sensitivity, safety, aptness, and adherence views. Partly just to avoid unnecessary disputes, I will consider safety, sensitivity and the rest only as proposed *necessary* conditions for knowledge. Once again, I’m not trying here to defend any specific version of any of these four, or even any of the four general types over the others.

First, recall Nozick’s original development of sensitivity. He initially formulates this strictly in terms of beliefs, adding methods in almost as an afterthought. Thus, we get a move from

S’s belief that p is *sensitive* iff S wouldn’t believe p if p if p were false

to

S’s belief that p is *method-sensitive* iff S believes p via M, and if S were to use M to determine whether or not p, S wouldn’t believe p via M if p were false.

To fix problems related to one-sided methods (e.g., one that could recommend belief that p but couldn’t recommend belief that not-p, as with a medical test that had very few false positives but lots of false negatives), Luper-Foy (1984) provides a simpler and better account of method-sensitivity:

S’s belief that p is *method-sensitive* iff S believes p via M, and S wouldn’t believe p via M if p were false.

Nozick himself tries to fix these problems by offering a rather clumsy theory about one method “outweighing” another. The result is that an initially simple and attractive idea gets weighed down by a series of epicycles.

This is all, I think, because he started with beliefs rather than methods/processes. If we start with methods, however, we can attribute sensitivity to processes rather than beliefs.[[17]](#footnote-18) Then we just say something like this:

**(MSen):** M is *sensitive* with respect to p iff M wouldn’t have produced the belief that p if p were false.

Sensitivity has to be relativized to particular contents because, for example, a particular visual process is sensitive in fake barn country with respect to house beliefs but not with respect to barn beliefs. Then the sensitivity requirement for knowledge is simply that:

S knows that p only if S’s belief that p is produced by a process that’s sensitive with respect to p.

We don’t really need to talk about “method-sensitivity” since that’s the only kind of sensitivity we care about; “belief-sensitivity” (S wouldn’t have believed p if p were false) has little to do with knowledge, as the counterexamples that motivate the Nozickian epicycles illustrate. Knowledge is simply properly formed belief, and sensitivity is one way to try to unpack proper-formedness. Again, I’m not trying to defend a sensitivity theory of knowledge; I’m claiming that if you already like a sensitivity theory, giving processes a central role in the theory gives you what you want, without all the epicycles.

We could characterize my suggestion here as rejecting a belief-first methodology in favor of a process-first methodology. Alternatively, we could describe it in terms of an analogy with doxastic and propositional justification. Some authors (e.g., Conee and Feldman, 2004) hold that propositional justification is the fundamental epistemic concept and that we can only understand doxastic justification as belief that’s based on what propositionally justifies it—that *contents* are the primary bearers and conveyors of epistemic status, and psychological events have it only secondarily. Others (e.g., Pollock, 1986; Lyons, 2016b; Kornblith, 2017; Graham and Lyons, forthcoming) hold just the opposite: that doxastic justification is fundamental and propositional justification can only be understood in terms of it. Similarly with knowledge: it’s not primarily beliefs themselves that are epistemically good or bad; rather what’s primarily good and bad are the processes by which the beliefs are formed.

The Nozickian approach sees knowledge as attaching primarily to beliefs, in virtue of their modal profiles, where specifying the relevant modal profile requires reference to methods. The approach I’m recommending sees knowledge as attaching primarily to the employment of methods (/processes). In the end, the difference may be a merely rhetorical difference, or a difference in framing, rather than one where the different views make different predictions about cases. But framing differences matter, especially since the aim of philosophy is at least as much to understand as to save the phenomena. There is a significant difference between thinking of knowledge as belief that tracks truth and thinking of knowledge as properly formed belief, even if by the time we’ve cashed out tracking and proper formation they’re extensionally equivalent. Especially given that, once again, belief-sensitivity (similarly for belief-safety, etc.) has approximately nothing to do with knowledge.

Safety can be treated in a very similar way to sensitivity:

**(MSaf):** M is safe iff M wouldn’t easily have produced a false belief,

where the righthand side is to be read (as is standard) as:

M doesn’t produce a false belief in any sufficiently close possible worlds.

Safety, unlike sensitivity, doesn’t have to be relativized to contents; in fact, it can’t be.

I want to come back around to this last point, but I want to do it indirectly. Recall the distinction between a belief-first and a process-first approach to knowledge. If knowledge is a matter of the modal properties of some belief, B, then whether or not B is knowledge depends on what features B has at other possible worlds. Now if beliefs have their contents essentially, as is standardly thought, then B will have the same content in all possible worlds, and if B is necessary, then its relevant modal profile will be determined entirely by its truth value. And yet we want some beliefs about necessary truths to count as knowledge, and we want others not to.

I think there’s a way to work this out in a belief-first framework, but it isn’t pretty. Let B be the belief I formed just now, on the basis of my unreliable parents’ testimony, that 349 is prime. Could B have easily been false? Yes, *if we can make sense of B’s having had a different content*. It could not possibly (and hence not easily) have been the case that 349 wasn’t prime, of course. But if B could easily have been the belief that 347 is prime, then B is unsafe, even when its content is the necessarily true proposition that 349 is prime.[[18]](#footnote-19) So how to make sense of B’s having different contents in different possible worlds? The only idea that makes much sense is to identify B’s trans-world counterparts (or what have you) by their origins: the things that count as B in those worlds are the beliefs that have the same origins as B in this world. This brings us back to methods all over again, but methods forced into the service of a clumsy, controversial, and unnecessary metaphysics.

Better, I think, to simply drop the commitment to a belief-first methodology and just attach the relevant modal properties and epistemic properties to the methods themselves. This is an important advantage of the process-first methodology. To exploit this advantage, we need a formulation of safety that is not relativized to contents.

I want to consider how (MSen) and (MSaf) handle the standard cases, but before I do, I want to note one important difference between them, one that is highlighted by centralizing methods. The difference between a safe method and a sensitive method is basically this:

**Sensitive:** M wouldn’t have produced p unless p.

**Safe:** M wouldn’t have produced a falsehood.

Because safety isn’t relativized to contents, *and because differences of mere content don’t make for differences in process*, safety and sensitivity will come apart in the following sorts of case:

I’m currently looking at a house in fake barn country. Had there not been a house there, there’d have been a barn facade, which I would have mistaken for a barn.

Had there not been a house there, the process used would have produced a false belief that there’s a barn, and—we’ll suppose—that possibility is easily realized. Then the process, and by extension, the belief is unsafe: it could have easily gotten things wrong. But the process and belief are sensitive: it wouldn’t have been wrong *about the house*. The safety view implies that I don’t know there’s a house; the sensitivity view allows that I do know. I don’t have strong intuitions about this case one way or the other, so I’m not inclined to level it as an argument against the safety view or against the sensitivity view. Rather, I think it’s a mark in favor of the process understanding of methods that it forces our hand and makes clear verdicts in this case, where a less committal, “bluffier” understanding of methods would not have.

What about the standard cases, then? I won’t try to cover them all, but here’s a sampling:

*Grandmother case*: the grandmother who sees that her grandson is alive and well uses a different process from her counterpart, who believes the same on the basis of false testimony. By centralizing methods, insisting that knowledge is well-produced belief, and that well-producedness is at least partly a matter of the safety or sensitivity of the generating or sustaining process, we see that the “correct” verdict about the grandmother case is both inevitable and natural, without having to tack on any ad hoc epicycles. The method she uses when she sees him is safe and sensitive; the method when she hears he’s ok is neither. But the unsafety and insensitivity of the latter method don’t mean that she doesn’t know in the original case; they mean only that she could easily have not known.

*Wolf/Dachshund*: you see a dachshund and thereby come to believe that there’s a dog in the yard. If there hadn’t been a dachshund (and there easily might not have been!), there would have been a wolf, which you’d have mistaken for a dog, since you can’t visually tell the difference (Goldman, 1976). Intuitively, you know in the dachshund case, even though the *belief* is insensitive and unsafe. My treatment of this case will parallel what I’ve just said about the grandmother case. Belief-sensitivity/safety is irrelevant; it’s method-sensitivity/safety that we care about. I’ve argued on independent grounds (and with neither this particular case nor theories of knowledge in mind) that dachshund identification and wolf (mis-)identification utilize different processes, due to differences in parameters: your dachshund detector is narrowly tuned and in a sparse neighborhood of visual templates; your wolfish-dog detector is broadly tuned and in a dense neighborhood (see Lyons, 2019, pp. 487-89 for more). Thus, the *process* that produces the dog belief when the stimulus is a dachshund may well be both safe and sensitive, even though you’d have believed p even if p were false.

*Barn facades*: According to A&P, the agent who’s looking at a real barn is using the same process as the agent who’s looking at a barn facade; the difference in distal stimulus does not make for a difference in process.[[19]](#footnote-20) Thus, in fake barn country, that process is both unsafe and insensitive: that very process might well have produced the belief that there’s a barn even though it was only a facade. At the same time, we’re able to say that the beliefs in both cases are equally justifed. These are very standard things to say, but with a real theory of methods in hand, we can be assured that this isn’t just bluffing.

*Brain in a vat*: This is controversial, of course, but sensitivity fans *like* the fact that a sensitivity requirement implies that we don’t know we’re not BIVs, even though we’re truly not. By contrast, safety fans like the fact that a safety requirement allows us to know we’re not BIVs. (It doesn’t *require* us to know that, unless we take safety to be sufficient for knowledge, not just necessary.) Both camps should be happy. The way A&P individuates processes, the BIV has all the same processes as a non-BIV. Thus, whatever the process is by which I form the belief that I’m not a BIV, it’s the same process as the one used by my envatted counterpart. His belief is false, so the process is insensitive. But he’s also quite far away, so the process is safe.

*Necessary truths*: one advantage sometimes claimed on behalf of safety theories over sensitivity theories is their handling of belief in necessary truths: if I’m just guessing, or using a heuristic process, then the method is unsafe, even if the belief is necessarily true. This works in the desired way because (and insofar as) the thing that has to be true in all the nearby worlds is not p (which is true in all worlds) but rather, whatever it is that the process produces; and the process will produce falsehoods in nearby worlds. For this line of reasoning to work, we need a method individuation scheme that allows a given method to produce different outputs. A&P does that. Not all schemes do (in fact, Nozick’s official scheme doesn’t, though he also has an incompatible, unofficial one that does).

This is just a sampling, and there may be other cases that the theory doesn’t handle so well.[[20]](#footnote-21) But the general idea should be clear: having a theory of processes means we don’t have to shy away from reliance on methods in handling these cases. The replies I’ve just given would likely be given by anyone who includes methods in their epistemology, including someone who sometimes construes them sometimes evidentially and sometimes procedurally. The difference is that without a theory constraining method individuation, these answers are cheats: for any two cases that aren’t exactly the same, we can always find descriptions that separate them and descriptions that unite them. So of course you *can* describe barn viewing and barn facade viewing as involving the same methods, or wolf viewing and dachshund viewing as involving different methods; but you could just as easily do the reverse. A theorist working without constraints on method individuation gets to choose the rules on a case-by-case basis, to make sure they win. But that’s cheating.

A&P, on the other hand, imposes significant and general constraints on method individuation, preventing this sort of cheating. The fact that there’s now a theory behind the concept of methods means that we can have independent confirmation or refutation of the claims made in the course of dealing with these cases. This is perhaps most obvious in the wolf/dachshund case, where specifically *empirical* findings about the nature of the perceptual identification process could lead to shifts in the theory’s verdict about the case. Again, I recognize that not all epistemologists are on the same page as I’m on here, but I find the prospect of empirical testing of epistemological theories exciting.

I’ve been discussing a way to simplify and clarify sensitivity and safety views by starting with a robust theory of processes and making it figure centrally in the account, rather than trying to squeeze it in after the fact. Of course, processes fit nicely into an aptness theory of knowledge as well. Because methods were already central to an understanding of competences, incorporating A&P doesn’t involve any significant recentering, although the generality worries I discussed in section 3 above are significant, so settling these would strengthen the theory. Plugging in processes in place of competences, we get something like:

**(MApt):** A belief B is an instance of knowledge (apt belief) iff one or more of the cognitive processes responsible for B is reliable, and B is true *because* of that reliability.

As mentioned above, this makes the view explicitly a kind of process reliabilism, which some of its proponents may not like (Sosa, forthcoming, ch. 2, sec 6F, for instance, seeks to distance his view from reliabilism), but as I argued above, a competence theory of *justification* can’t get off the ground unless competences are understood as processes. All this is of course compatible with the idea that this form of process reliabilism is motivated in substantively different ways than other forms, due to its emphasis on performance normativity more generally.

**6. Adherence as Outlier**

I’ve saved adherence for last, partly because it doesn’t figure prominently in the current literature, and partly because it’s importantly different from safety, sensitivity, and aptness. For this second reason, however, and for relative completeness, it’s worth discussing.

Adherence as originally proposed is this:

S’s belief that p is *adherent* iff if p were true S would believe p,

which we’re to interpret as requiring that in all the (sufficiently) nearby worlds where p is true, S believes p (Nozick, 1981). Recentering on methods, we get:

**(MAdh):** M is *adherent* with respect to p iff M produces p at all the nearby p-worlds.

One difference between safety and adherence is that safety requires M to get it right at *all* the nearby worlds, not just the p-worlds. A more far-reaching difference is that safety allows ignorance in cases that adherence does not. Safety doesn’t require M to produce p (or anything) at all the nearby p-worlds, just to not produce falsehoods. Thus, while safety, sensitivity and aptness are all concerned with avoiding—in the modally relevant way—*false* belief, adherence is concerned with avoiding the *absence* of belief. It’s not obvious why or whether such a requirement needs to be part of a theory of knowledge.

I argued above that methods won’t help with the Jesse James case unless we use an evidential construal of methods: *vision* doesn’t produce the belief that Jesse James is robbing the train in many nearby p-worlds, because his mask doesn’t slip there. So in order to avoid the mistaken verdict that the witness in this case doesn’t know, the adherence theorist would have to individuate methods in terms of their inputs, or require a kind of method-adherence that is relativized to inputs. The first of these options is bad, partly because of the general problems with process-free evidentialism that I’ve been pressing throughout this paper. But there are also problems specific to adherence. Suppose I’m very good at determining validity through a conscious deliberate process if I take the time to do so, but I’m very often too lazy and just guess or use an unreliable heuristic. I should count as knowing when I use that deliberate process. But if methods are individuated just by their inputs and outputs (or if we avoid reference to methods altogether) the belief counts as inadherent. Thus, what we would need, to save adherence, is something like this:

**(InpMAdh):** M is *adherent* with respect to p, given input i iff when given i, M produces p at all the nearby p-worlds.

This would fix the problem, but it’s starting to get a bit unwieldy and unprincipled, and since it’s not clear anyway why our methods shouldn’t yield agnosticism in nearby worlds, maybe it’s not worth the cost to maintain adherence.

Recall that adherence was first introduced to solve problems for a belief-sensitivity theory, i.e., one that attached sensitivity to beliefs without any reference to methods. Nozick first articulates adherence to help with the case where neurosurgeons directly stimulate a BIV to make it think it’s a BIV (1981, p. 175). That belief is sensitive—the BIV wouldn’t have had it had it not been envatted—but it’s not knowledge, so something more is needed. If we require adherence, then this belief won’t count as knowledge, because the neurosurgeons don’t stimulate the brain in all the nearby worlds.

Of course, and as mentioned above already, there are good reasons for a sensitivity fan to claim that sensitivity is only necessary, not sufficient, for knowledge, and that unjustified beliefs aren’t known. But also, the problematic belief is not obviously method-sensitive, anyhow (the neurosurgeons are not part of the process, according to A&P, since they aren’t cognitive states), so if we’re thinking of sensitivity as method-sensitivity rather than belief-sensitivity, the adherence solution is additionally unnecessary.

The other cases Nozick mentions in favor of adherence are Harman’s assassinated leader case (S reads the newspapers that correctly report the assassination but doesn’t read the false coverups that are published in all the other papers and read by everyone else; intuitively S doesn’t know), and knowledge of necessities. The former seems not significantly different from a fake barn case: reading the later (false) newspapers doesn’t involve a different process from reading the earlier (true) newspapers, so however we characterize the method, it’s unsafe and insensitive. And we saw above that methods are needed to handle knowledge of necessities, whatever else we might want to say. This is particularly obvious in the case of adherence, since we don’t want dogmatic (and thus held in all nearby worlds) belief in some randomly chosen necessity to thereby count as knowledge. Adherence is no help with knowledge of necessities unless it’s method-adherence (and, to repeat, it needs to rely on a construal of methods that allows different inputs and outputs, which is incompatible with what’s needed to solve the Jesse James problem), but once we’ve brought in methods, we can account for knowledge of necessities without appeal to adherence.

**7. Conclusion**

A&P offers the best way we know to solve the generality problem for a process reliabilist theory of justification. Even if you’re not a process *reliabilist*, however, A&P seems to mark out the process differences that are relevant to justification; a process-free evidentialism, for example, won’t be able to account for doxastic justification. Especially if we’re already committed to A&P elsewhere in our epistemology, we might as well use it to make sense of the “methods” that are plausibly relevant to knowledge. It turns out that when we understand methods in terms of A&P processes, we can give processes pride of place in the theory and get a version of our favorite safety, sensitivity, or aptness account that’s more elegant and that works better. Adherence is also susceptible to this process-recentering, but with considerably less elegance. Once we’ve put methods at the center of our theory of knowledge, however, we see that whatever appeal adherence might have had has largely vanished anyway. If we therefore decide to do without it, we’re probably not missing anything.[[21]](#footnote-22)

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1. More strictly, we should talking about *prima facie* justification, rather than justification simpliciter, and we should be talking about producing *or sustaining*, etc., although nothing here will hang on the differences. I’ll take all that as read and stick with the simpler wording. [↑](#footnote-ref-2)
2. A number of authors who don’t *call* themselves indicator reliabilists nevertheless endorse it or something very much like it (e.g., Dretske, 1971; 1981; Williamson 2000; Comesaña, 2006; Zalabardo, 2012). [↑](#footnote-ref-3)
3. For more details on all of this, see Lyons (2019). [↑](#footnote-ref-4)
4. For the System 1/System 2 distinction, see Kahneman (2011). Very roughly, we have fast, automatic, effortless heuristic methods that operate unconsciously, for solving certain problems (System 1), and slower, conscious, deliberative and introspectively transparent methods, for solving many of the very same problems (System 2). [↑](#footnote-ref-5)
5. As I defend and articulate A&P in Lyons (2019), A&P could be incorporated into a fairly strong (though I think standard) form of mentalism, which holds that any two agents alike in all *nonfactive* psychological respects are justificationally identical. One could develop a close cousin of A&P by allowing external states of affairs, or factive mental states, to figure among the inputs to a given process. The resulting process typing would still work with an unorthodox version of mentalism (perhaps not worth the name), which held that any two agents alike in all *factive and nonfactive* psychological respects are justificationally identical. [↑](#footnote-ref-6)
6. The evidentialist should probably try to pack some or all of these factors into the evidence on which the agent is basing their belief. This seems implausible, however, since some of these factors aren’t consciously accessible, and many that are seem to count as the agent’s basis for belief only on a highly hyperintellectualized construal of basing. When I perform modus ponens is my basis/evidence not just p and ‘p ⊃ q’ but also my sense of how long I’m taking, and my sense of whether I’m attending to the syntax or the content in performing the inference? [↑](#footnote-ref-7)
7. This is a simplified version of Johnson-Laird’s (2012) mental models theory of reasoning. See Lyons (2019, pp. 490-91) for a somewhat more detailed discussion. [↑](#footnote-ref-8)
8. These aren’t his terms but are the terms that have since become standard. [↑](#footnote-ref-9)
9. But only for the present purposes. I think that the basing relation only holds among nonfactive mental state, in fact, only among *beliefs*—or at least, it’s only beliefs that can *justify* beliefs by serving as their bases (Lyons, 2009; 2016a; 2020). [↑](#footnote-ref-10)
10. Although one could endorse an algorithmic theory of process or method individuation that differs from A&P, no one has developed a plausible alternative in any significant detail. In what follows, therefore, I will take A&P as so paradigmatic of the algorithmic approach as to be nearly synonymous with it. Readers who prefer a close cousin to A&P could just read that in for A&P in what follows and embrace a slightly broader conclusion in the end. [↑](#footnote-ref-11)
11. Again, I think all of these are wrong ways to think about the method, since all of them ignore the way in which the inference is made: is it content-driven? syntactic? is the agent paying attention? guessing? etc. But I’m not pressing that issue at this point. [↑](#footnote-ref-12)
12. The idea that there could be knowledge without justification is most plausible if we read ‘justification’ as picking out a narrower normative property than the one I have in mind. Burge (2020), for example, uses ‘warrant’ as I (and most epistemologists) use ‘justification’ and uses ‘justification’ to mean a specific kind of warrant, one that results from actual or possible propositional reasoning. Many of us would agree that knowledge doesn’t require justification in *that* sense. In roughly this spirit, Goldman (1979) explains his earlier (e.g., Goldman, 1967; 1976) claims that knowledge doesn’t require justification, as having intended merely that knowledge doesn’t require *internalist*, “Cartesian,” justification. Some, however, clearly hold a stronger view about the nonnecessity of justification. Kornblith (2008) insists that knowledge requires reliably-formed belief but denies that we should understand justification in terms of (de facto) reliability. Lasonen Aarnio (2010) goes further, claiming that one could be positively irrational in believing p and still know that p. The cases she offers are cases of safe but defeated belief. I myself don’t find the cases to be intuitively plausible cases of knowledge; I see them as better arguments for thinking that a safety theory of knowledge needs a justification requirement, than for thinking that unreasonable knowledge is possible. In any case, my concern throughout has been on safety, sensitivity, and the like as *necessary* conditions for knowledge, not sufficient conditions. [↑](#footnote-ref-13)
13. As mentioned above, some authors, like Roush (2005) and Zalabardo (2012) eschew methods; part of their response to Nozick’s grandmother case is therefore to treat the belief as inferential and require sensitivity only for noninferential knowledge. But surely there are noninferential versions of the grandmother case: I see that there’s something red in front of me (or substitute your favorite noninferential belief here), but if there hadn’t been anything red in front of me, the redness police would have turned the lights off and talked me into believing there was something red there in the dark in front of me. This seems to elicit just the same intuitions as does Nozick’s original case, but the Roush/Zalabardo solution won’t work. [↑](#footnote-ref-14)
14. Such a possibility seems especially clear in the case of beliefs about necessary truths. It’s worth noting that Zalabardo (2012), who avoids method-relativization, also restricts his theory to beliefs about contingent truths. [↑](#footnote-ref-15)
15. Nozick’s view about beliefs with experienceable bases thus gets quite close to Dretske’s view. Dretske (1971; 1981) holds that S can know that p on the basis of r only if r wouldn’t *obtain* unless p were true. Nozick’s official view about methods turns his sensitivity requirement into the claim that S can know that p on the basis of r only if r wouldn’t *produce the belief that p* unless p were true. [↑](#footnote-ref-16)
16. There’s not just one process involved in seeing a barn, because some agent may have more than one visual identification algorithm that works for barns, and differences in situation and individual differences will make for differences in parameters. Nevertheless, we can construe the questions ending the previous paragraph as getting at classes of process types and asking whether the cited differences per se (e,g., being in fake barn country) are relevant to distinguishing those classes. Thus construed, the answers A&P gives to the questions of the previous paragraph are, respectively: yes, no, no, no, yes. [↑](#footnote-ref-17)
17. Sensitivity will be a contingent feature of a process, relative to its current circumstances, just as it was with belief. (A belief or process that is sensitive at t may be insensitive at t+n.) [↑](#footnote-ref-18)
18. Pritchard (2013, pp. 157-8) seems to be making this sort of move, although a bit less explicitly. [↑](#footnote-ref-19)
19. I say this in part because I think that algorithms need to take only (nonfactive) psychological states as inputs; but even without this, the difference between barns and barn facades, like a difference of mere content, would not make for a difference in process. [↑](#footnote-ref-20)
20. What I’m probably going to end up saying about the aforementioned hammer hallucination case, for example, might seem counterintuitive, especially to those who have strongly internalist intuitions. Partly this is because I think hallucination is epistemologically more complicated than we normally think. (Readers of Lyons, 2019, sec 6.2 will be able to guess my view, though it’s too complicated to defend here.) Again, I should point out that I’m not trying to defend a sensitivity (or safety, etc.) account of knowledge, only to argue that those accounts need a certain reliance on processes. If there are residual problems with (say) a sensitivity theory, this may be the fault of the sensitivity theory, not of my assistance. [↑](#footnote-ref-21)
21. Thanks to Adam Carter, Sanford Goldberg, Peter Graham, Christoph Kelp, and Mona Simion for helpful comments on an earlier draft. [↑](#footnote-ref-22)