Reliability and Future True Belief: Reply to Olsson and Jönsson

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Abstract: In 'Process Reliabilism and the Value Problem' I argue that Erik Olsson and Alvin Goldman's conditional probability solution to the value problem in epistemology is unsuccessful and that it makes significant internalist concessions. In 'Kinds of Learning and the Likelihood of Future True Beliefs' Olsson and Martin Jönsson try to show that my argument does "not in the end reduce the plausibility" of Olsson and Goldman's account. Here I argue that, while Olsson and Jönsson clarify and amend the conditional probability approach in a number of helpful ways, my case against it remains intact. I conclude with a constructive proposal as to how their account may be steered in a more promising direction.

1. Introduction

Alvin Goldman and Erik Olsson have responded to the common charge that process reliabilism cannot account for the surplus value of knowledge over mere true belief. The heart of their proposal is what they call the 'conditional probability solution'. It maintains that "the probability of having more true belief (of a similar kind) in the future is greater conditional on S's knowing that p than conditional on S's merely truly believing that p" (Goldman & Olsson 2009, 28). Their argument proceeds as follows:

The conditional probability solution (CPS):

(1) If an epistemic subject S acquires a true belief B by employing a reliable cognitive process (or method[1]) R, then – given certain 'empirical regularities' (to be discussed below) – S is likely to re-employ R on suitable future occasions.

(2) The (objective) probability that S acquires more true belief, of a relevantly similar kind, on those future occasions, given that S re-employs R, is greater than the (objective) probability that S acquires more true belief of a relevantly similar kind on those future occasions, conditional on the fact that S does not re-employ R.

[1] In earlier work, Goldman distinguishes belief-forming processes from belief-forming methods (Goldman 1986, 92-95; 1992, 128 f.). The former are, roughly, "wired-in features of our cognitive architecture". Methods, by contrast, are "learnable algorithms, heuristics, or procedures for forming beliefs" (1992, 129). CPS is apparently supposed to work for both

(3) Hence forming B through a reliable cognitive process (or method) R is more valuable than forming B without employing R.

I don't wish to dispute that, if (1) and (2) were true, (3) would be true, in which case the authors would indeed have presented a promising solution to the value problem as it arises for (process) reliabilism.[2] However, as I argue in 'Process Reliabilism and the Value Problem' (PRVP, this volume), CPS confronts two crucial problems. First, Olsson and Goldman do not provide a convincing argument for premise (2). Second, certain 'empirical conditions' they postulate in their argument for (1) involve higher-order doxastic conditions. CPS thus makes a concessive bow before internalism, thereby violating a central tenet of Goldman-style reliabilism.

In their reply 'Kinds of Learning and the Likelihood of Future True Beliefs: Reply to Jäger on Reliabilism and the Value Problem' (KL, this volume), Erik Olsson and Martin Jönsson argue that my reasoning against Olsson and Goldman's argument for premise (2) of CPS is mistaken. Second, although they concede that Goldman and Olsson's original argument for premise (1) draws on higher-order constraints, Olsson and Jönsson believe that this problem can be remedied by introducing a 'hard-wired' hypothesis concerning people's dispositions to learn about, and reuse, belief-forming mechanisms. Olsson and Jönsson's piece contains a number of helpful clarifications and embellishments of CPS. Yet their defence of premise (2) is still unsuccessful (section 2). Moreover, even if we agree that a hard-wired account of the learning condition would not betray externalism, the authors owe us an argument that human epistemic subjects actually are hard-wired in appropriate ways. An alternative, much more plausible view is that people tend to re-employ belief-forming mechanisms if they believe that these mechanisms are epistemically reliable (section 3). Must this be anathema to reliabilism? I do not think so. Reliabilists can stick to an externalist analysis of the concept of knowledge, but they had better bite the bullet and incorporate higher-order doxastic constraints in their solution to the value problem (section 4).

2. Does re-employment of reliable mechanisms raise the probability of having more true belief in the future?

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belief-forming processes and belief-forming methods. In what follows I shall often use the term 'mechanism' as an umbrella term covering both processes and methods.

[2] For a discussion of Goldman and Olsson (2009) that also questions the validity of their argument see Davis and Jäger (forthcoming).

CPS has primarily been spelled out by Olsson, while Goldman has mainly formulated the account of 'value autonomization' proposed in (Goldman & Olsson 2009).[3] Note, however, that the latter account is designed to explain why it is that, while "reliabilist knowledge is normally but not always more valuable than mere true belief ... some philosophers think that we are always prepared to attribute greater value to knowledge than to mere true belief" (Goldman & Olsson 2009, 31). Goldman's account of value autonomization is based on CPS; its declared aim is merely to explain why some people may reject the restriction that only under certain contingent conditions should knowledge count as more valuable than mere true belief. Olsson and Jönsson's statement that, while Olsson is more inclined to defend CPS, "Goldman has reported that he favors the [value autonomization] solution" (KL, ###) is thus somewhat misleading. Goldman's account works with the assumption that CPS is a viable solution to the value problem and on that basis tries to elucidate "the psychological mechanisms whereby reliable belief-forming processes come to be accorded 'autonomous' value" (Goldman & Olsson 2009, 31). CPS is thus the more fundamental part of the overall account proposed in Goldman and Olsson (2009), and my discussion will therefore focus on CPS.

Why should we think that (2) is true? One question that raises its ugly head here is how to individuate 'beliefs of a similar kind'. In the present context, this notion had better not bring in the belief-forming mechanisms that produced the original belief. Otherwise, the fact that one acquires true beliefs of a similar kind in the future would entail that the belief-forming mechanism R is re-employed, while the probability that S will acquire more true belief of a similar kind, conditional on the fact that S does not re-employ R, would be zero. So, premise (2) would be true if the probability that S acquires more true belief on the relevant future occasions, given that S re-employs R, were greater than zero. (2) would then only require that it be possible that S acquires more true belief if S re-employs R. That is an easy standard. Even if you don't (re)employ a reliable cognitive mechanism it is possible that you acquire a true belief by using that mechanism. Hence under this interpretation, Olsson and Goldman's argument would not show that using a reliable belief-forming mechanism is any better from an epistemic point of view than using an unreliable mechanism, and (3) would not follow. Here I shall put this problem to one side, however, and for the sake of argument assume that 'beliefs of a similar kind' can be individuated without invoking the processes that produce them.

Another problem is that Olsson and Jönsson interpret talk about 'objective probability' in premise (2) in terms of statistical probability. I doubt that this is a promising course.

[3] Personal conversation and correspondence with the authors.

(Where are the relevant statistics? What about mechanisms that are employed only once?) However, let us postpone this problem until the end of this section. The main issue with premise (2) is this. Olsson and Goldman's case for (2) is unsuccessful, I argued, because nothing in their account "rules out that, if some reliable epistemic mechanism R were not operating in a given belief-forming process, this might cause a still more reliable mechanism R* to operate ... on the relevant future occasions" (PRVP, ###), in which case the subject would acquire more true belief if he/she had not employed R.

Suppose for example that in a situation such as the one just sketched no reliable mechanism were operating on the original occasion. In that case it is more probable that S will acquire more true belief on the relevant future occasions if on the present occasion S employs neither R nor any other reliable process or method, and thus does not generate reliabilist knowledge[4]. In support of this argument I presented a counterexample, which Olsson and Jönsson however reject as mistaken. It is worth briefly repeating the example and trying to make the case as clear as possible:

> Scenario 1: You are travelling to Larissa and will come upon five crossroads. During your whole trip, you employ a navigation system N1 that by suitable (process) reliabilist standards counts as reliable. N1 gives you the correct route at the first and second crossroads, but not at following three. (Since reliable mechanisms can fail to deliver correct results, this scenario is of course consistent with the assumption that N1 is generally reliable. Moreover, suppose that when you make your first and second wrong turns there will still be, in both cases, a right and a wrong option at the next crossroads, the right choice being one that would take you back on a road to Larissa.) Compare this to scenario 2: You do not use N1, or any other navigation system, at the first or second crossroads, but on both occasions just guess correctly where to go. (Suppose for example that you believe that N1 is malfunctioning.) Suppose also that in this alternative scenario, when you have passed the first two crossroads, you buy a new top-quality navigation system N2 at the next service station and start using it for the rest of your trip. Unlike N1 in scenario 1, N2 gives you the correct results at the three remaining crossroads.[5]

[4] I use the term 'reliabilist knowledge' as shorthand for 'knowledge acquired in a reliabilist way'.

[5] It may be doubtful whether guessing can be construed as a belief-forming method at all. Readers who are skeptical about this may replace 'guessing' in my argument by their favorite unreliable belief-forming method.

This is a counterexample to premise (2) of CPS. For we have the following situation: were S not to use some reliable mechanism R (N1, in the example), S would employ another, more reliable mechanism R* (N2, in the example) on suitable future occasions. In consequence S would – contrary to what CPS predicts – acquire more true beliefs on the relevant future occasions, compared to a situation (scenario 1) in which S employs R on the original occasion.

Olsson and Jönsson object that one of the necessary conditions that had been postulated for CPS – conditions the authors claim to be typically fulfilled in the actual world – is violated in my story. Here is how they describe these 'empirical conditions':

> "By non-uniqueness, the same kind of problem will tend to arise more than once. ... By cross-temporal access, a method that was used once will tend to be available when the problem arises in the future. The method of using the GPS navigation device in your car would be a case in point. By the learning assumption, if a particular method solves a problem once, and you have no reason to think otherwise, you will tend to use the same method again. By generality, finally, if a method was reliable in the past, its reliability is unlikely suddenly to be discontinued" (KL, this volume, ###).

Now, an "immediate problem" with my (alleged) counterexample, Olsson and Jönsson argue, is that

> "the learning condition is not satisfied in the hypothetical scenario [scenario 2, C.J.] in which you simply guess (correctly) where to go, buy the top N2 system and use that system from there on. The switch from guessing to using N2 violates the learning condition because it constitutes an unprovoked shift from one method to another" (KL, this volume, ####).

This objection misses its mark. I have three comments.

(i) First, note that both Olsson and Goldman's original characterization of the learning condition (in Goldman and Olsson 2009) as well as Olsson and Jönsson's portrayal quoted above strongly suggest that this condition concerns re-employment of reliable mechanisms. (Their illustration is a well functioning GPS, after all.) In the N2 scenario of my counterexample, however, there is no such re-employment of reliable mechanisms at the time

in question. When S starts using N2, there has been no prior consultation of N2 or any other reliable mechanism. Hence the learning condition, under this interpretation, is not violated in my counterexample.

(ii) There is a more liberal construal of the learning condition, however, according to which people tend to re-employ any belief-forming mechanism, whether or not it is (perceived as) reliable, as long as it has 'solved a problem' on a given occasion and the subject has no reason to think otherwise. In fact, such an idea seems to underlie the official account Olsson and Jönsson eventually settle for. I will shortly discuss this account in more detail. To begin with however I should like to note that, if we interpret the learning condition along these lines, what it requires is most certainly false.

Suppose that in a given epistemic situation you must rely on guesswork. For example, consider a choice between two trails in the mountains, only one of which will take you back into the valley. You have no idea which trail is the right one (you have lost your maps, GPS device, etc.); but being under practical pressure you eventually decide, on the basis of sheer guesswork, to take the trail on your left. As a matter of luck you get it right. Will you continue to rely on guesswork when on your next hike you are confronted with a similar choice? Certainly not if this time you are well equipped with maps, compass, GPS, etc. Anyone who is minimally rational and has the background beliefs typical of a human adult will be aware that guessing is an unreliable method (and hence that the former success was a matter of sheer luck) whereas using maps and a GPS is a reliable method for acquiring true geographical beliefs. The unqualified claim that people tend to re-employ processes or methods whenever they think these processes or methods 'solved a problem' on a given occasion, is false.

A more promising suggestion would be that people tend to re-employ mechanisms which they have no reason to think are unreliable. But that's a horse of a different colour (to be investigated in some detail below). And what's more, it would not help Olsson and Jönsson's criticism of my counterexample. For in that example, the subject – conceived as a standard, minimally rational human adult – does have good reason, in scenario 2, for believing that guessing is an unreliable belief-forming method and thus that switching to using the reliable GPS will raise his chances of success. So the switch from mere guessing to using N2 is by no means 'unprovoked' or unmotivated.[6] I thus conclude that the learning condition is clearly not violated in my counterexample if we interpret that condition as

[6] What may reasonably be argued with respect to my counterexample is that S has not learned that guessing is unreliable from employing that method in the situations mentioned in the example (i.e., when getting to the first two crossroads). S's act of guessing was successful in these situations. However, I cannot see how this should be relevant for the present argument.

applying to reliable processes or methods. And interpreted more liberally as concerning belief-forming mechanisms in general, the condition – re-employment of any mechanisms that solved a problem – is false. No wonder, then, that a realistic (counter) example does not fulfil that condition.

(iii) The advocates of CPS construe the learning condition as a condition for premise (1) of CPS. My version of the Larissa story however is a counterexample to premise (2). The crucial point is that, for whichever reasons you re-employ a certain reliable belief-forming mechanism, it is – contrary to what CPS claims – in fact not guaranteed that chances are higher that you acquire more true belief. In any event, Olsson and Jönsson themselves argue that my counterexample could quite easily be reformulated in a way that meets the empirical constraints proposed for premise (1). A slightly altered thought experiment, they maintain, would indeed warrant the conclusion that "reliabilist knowledge may fail to make [true] future beliefs more likely, even if the learning condition is satisfied, and even if the other empirical conditions are satisfied as well" (KL, this volume, ###). Olsson and Jönsson appreciate this as "a novel observation that is worth making" (###), but insist that it

> "... doesn't threaten the CP solution to the value problem. The CP claim is a statistical claim about the relation between conditional probabilities or, in frequentist terms, proportions. As such it is not jeopardized by reference to special cases involving some rarely occurring happenings, and this is so even if the empirical conditions that are central to the CP solution are not violated in those cases" (###)

I do not think that it is wise to appeal to statistical probability in this context. If that is the idea, then where are the relevant statistics? Where are the data about people's success when they reuse cognitive processes? Olsson and Jönsson do not present any such data. This may not be surprising, for so far – at least to my knowledge – such data do not exist. On a frequentist reading, the proportion of times a method produces the desired result has to be a proportion of some total. But where does this total come from? Note that, even if we could avail ourselves of empirical data about successful and unsuccessful performances of a given cognitive mechanisms M, such data would not allow us to determine whether M is reliable. In all samples considered, M may have delivered true beliefs, and yet M may be unreliable. (In all or most future performances, the mechanism may deliver false beliefs). At the same time, a competing mechanism M* may have delivered only, or mostly, true beliefs in all its employments so far considered, and yet M* may be unreliable. (As a limiting case, consider a

mechanism that operates just one time, but is successful that time.) I shall not press this point, however, for even if we put this problem to one side Olsson and Jönsson's defence founders.

The authors maintain that my objection to premise (2) is unsuccessful because counterexamples of the kind I suggest involve very special cases and "rarely occurring happenings". Hence, they think, these examples are quite compatible with CPS since it is a mere statistical claim. It is not clear to me why we should think my example involves a very special case. As far as I can see, Olsson and Jönsson don't tell us, and it may be doubted that their claim is correct. First, note once more that, under a statistical interpretation, whether such happenings are 'rare' depends entirely on what the total number of events is on which this claim is based. Moreover, it does seem true that – maybe due o 'cognitive conservatism' – we frequently continue to employ cognitive mechanisms that have occasionally proven to be successful but which, were we to stop using them, we would replace with better ones. In any event, the main problem with Olsson and Jönsson's objection is that it misconstrues the dialectic.

Premise (2) does not maintain that re-employing some reliable cognitive process or method under the conditions in question entails that one acquires more true belief; that much is correct, since (2) only talks about probability. Olsson and Goldman's claim does maintain however that whenever one employs some reliable cognitive process R (under the conditions in question), one raises the relative probability that one will acquire more true belief (of a similar kind) on suitable future occasions. (2) does not just say that, under the conditions in question, the probability that one acquires more true belief on suitable future occasions is sometimes, or occasionally, or often greater. Olsson and Goldman's claim was that the probability is always greater. And precisely that is false, witness my counterexample. The twist of the tale is that, even under the contingent conditions Olsson, Goldman, and Jönsson sketch, employing a reliable process or method does not always raise the conditional probability that one will acquire more true belief (of a similar kind) in the future.

3. Can CPS avoid higher-order constraints?

I now turn to premise (1) of Olsson and Goldman's main argument. Olsson and Jönsson concede that the reasons given for the truth of (1), as presented in Goldman and Olsson (2009), draw upon higher-order doxastic conditions. (The subject is said to "have reasons to believe that [the reliable mechanism used on a given occasion] ... should work again", etc.). This does not blend well with externalism. As a constructive proposal on behalf of

externalism I considered the following move. Could not CPS claim that we are simply hard-wired in such a way that we reuse reliable belief-forming processes?

There are two reasons, I argued, why this reply would not take us very far. First, is such a wired-in hypothesis true? We don't know, and so far as I can see neither Goldman and Olssson (2009) nor Olsson and Jönsson (KL) present any data, or any argument, to the effect that it is true. Note that, since we are dealing with an empirical claim, an argument for it would have to rely on empirical data. It may not be a philosopher's task to collect such data, but so long as none are on the table the hard-wired hypothesis remains a mere speculation.

The second problem, I argued, is that such a hypothesis fails in any event to answer our original question, which is why an arbitrary, single instance of reliabilist knowledge is more valuable than a corresponding instance of mere true belief. A wired-in mechanism which is not accessible on reflection alone but which is responsible for re-employing reliable processes and methods, I maintained, must be able to distinguish between reliable and unreliable cognitive mechanisms. This however would involve complex inductive procedures of assessing those mechanisms' track records. But in this case the objects of surplus axiological value would not be single beliefs but rather entire series of beliefs and their cognitive histories.

Olsson and Jönsson think that this argument "makes good sense" on a certain strong understanding of the learning condition, but that CPS works perfectly fine with a more parsimonious requirement. Consider the idea that:

> "(Strong learning) a) People have a disposition to reuse a belief formation process if that process has been shown to be reliable.
> b) People have a disposition to replace a belief formation process if that process has been shown to be unreliable" (KL, this volume, ###).

Contrast this with:

> "(Learning) People have a disposition to reuse a belief formation process if that process has not been shown to be unsuccessful" (###).

CPS, Olsson and Jönsson maintain, requires only (Learning). One crucial difference between (Learning) and (Strong Learning) is that according to the latter the "retention [of belief-forming processes or methods] is conditional on reliability", whereas according to (Learning)

people continue to employ cognitive processes or methods "until unsuccessfulness obtains" (###). Hence in contrast to (Strong Learning), (Learning)

> "does not require that the reliability of a process be demonstrated prior to the reemployment of that process. ... It doesn't require that anything be demonstrated before a process can be reused. In order to satisfy learning, it is sufficient that an organism is hard-wired in accordance with the law of least resistance, the most economical way of reacting to stimuli being to continue responding in the old way rather than trying something new. This clearly does not require higher order powers of representation" (###).

One question here is how we are to understand that a process or method has been "shown" or "demonstrated" to be reliable or unreliable. Been shown to whom? Presumably to the subject of the process. I agree (and never argued to the contrary) that people need not be shown, or be in a position to show (or demonstrate, or prove) that a belief-forming process or method they used is reliable, in order to be disposed to (re-)employ it. My claim is that – just as Olsson and Goldman's original formulation of the learning condition suggested – the re-employment of reliable processes and methods can most plausibly be explained by appealing to certain beliefs on the part of the subject concerning his or her epistemic situation. These beliefs will typically include the belief that certain belief-forming mechanisms are reliable and others unreliable. But this is weaker than the requirement that the subject have (true) beliefs to the effect that a certain mechanism has been shown, or conclusively demonstrated, to be reliable. In fact, if we take problems of epistemic circularity and other sceptical worries seriously, such a task may be impossible to accomplish.

Apart from this it remains unclear how we are to understand the suggestion that a belief-forming process will be reused. Processes can be reused only qua types. Qua tokens they constitute events and are thus not repeatable. Olsson and Jönsson's (Learning) may thus be interpreted in two different ways. Either their claim is that:

> (Learning*) If a belief-forming process type (or method) P an instantiation of which S has employed on a given occasion has not been shown to S to be unsuccessful, S has a disposition to reuse processes (or methods) of that type on suitable future occasions.

It is not clear however what it would mean to show that a process qua type is unsuccessful. (Learning*) does not seem to be the intended reading. A second interpretation is:

> (Learning**) If a belief-forming process token (or method) of type P that has generated a belief in S on a given occasion has not been shown to S to have been unsuccessful on that occasion, S has a disposition to use processes (or methods) of that type on suitable future occasions.

As a general claim however (Learning**) is – at least for normal, minimally rational human adults – in all probability false. When is a belief-forming process token unsuccessful, in the epistemic sense under consideration here? Presumably if it fails to produce a true belief. Yet, first, even if it has not been shown (or demonstrated, or proven) to S that a certain belief-forming mechanism S used was unsuccessful, S may still have good reasons to believe – and on the basis of those reasons actually form the belief – that a given performance of the process did not deliver a true belief. Second, even if S does not believe that a certain performance was unsuccessful in this sense, S may still have good reasons to believe – and on the basis of those reasons actually form the belief – that the mechanism is nevertheless unreliable. For example, an informant S knows to be reliable may tell S that the mechanism S employed is unreliable and that chances are high that, even though on the present occasion it delivered the correct result, it will not do so on the next occasion. In such cases, too, S will not be disposed to re-employ the mechanism. In light of these reflections, a first pass at embellishing Olsson and Jönsson's proposal is as follows:

> (Learning***) If S does not believe that a belief-forming process type (or method) P an instantiation of which has generated a belief in S on a given occasion is unreliable, S is disposed to use processes (or methods) of that type on suitable future occasions.

This is still weaker than the original condition proposed in Olsson and Goldman (2009). According to that condition, we recall, "if you have used a given method before and the result has been unobjectionable, you are likely to use it again on a similar occasion, if it is available", provided that "you have reason to believe that it should work again" (###). Properly spelled out, I argued, this condition requires that S hold the higher-order belief that he or she has acquired a true belief produced by a reliable mechanism. (Learning***), by contrast, does not require that S 'positively believe' that a given cognitive mechanism is

reliable, but only that S not believe that it is unreliable. (For minimally rational subjects, the former entails the latter, but not vice versa.) However, is (Learning***) true? Or do we at least have good reasons to believe that it will typically be met by actual epistemic subjects?

No. Note, first, that it often happens that, even though we do not believe that a belief-forming mechanism we employed is unreliable, we still do not reuse it on suitable future occasions since we meanwhile come to believe that better, even more reliable mechanisms are available. Consider the following variation of our Larissa example. Tom uses the fairly reliable, but only medium quality, navigation system N1 at the first two crossroads. When he stops at the next service station, he unpacks a birthday present from his wife and finds a new, top navigation device (a Garmin GPS, let us suppose) which he correctly believes to be more reliable than N1. Under normal circumstances, and if he is minimally rational etc., Tom will use N2 for the rest of his trip. The upshot is that we have good reasons for thinking that S is disposed to reuse a belief-forming mechanism on suitable future occasions only if S believes neither that that mechanism is unreliable nor that better mechanisms are available.

Another worry calls for mention at this stage. Even if the above condition is fulfilled, people will be disposed to reuse cognitive mechanisms on suitable future occasions only if they identify these occasions as suitable. In other words, not only must S encounter epistemically similar situations in the future; S must also believe that these situations are of the right kind, and that they involve similar cognitive tasks than before. Hence the most promising reformulation of Olsson and Jönsson's version of the learning condition I can think of is something like this:

> (Learning****) If S believes neither (i) that a belief-forming process type (or method) P, an instantiation of which has generated a belief in S on a given occasion O, is unreliable, nor (ii) that better processes (or methods) are available, then S is disposed to use a process (or method) of type P on suitable future occasions, provided that S believes these occasions to be relevantly similar to O.

Can this condition explain the re-employment hypothesis without introducing positive doxastic higher-order conditions? In particular, could (Learning****) be spelled out in terms of a hard-wired hypothesis?

CPS does not maintain that people always re-employ reliable belief-forming mechanisms. Premise (1) of the Olsson-Goldman-Jönsson argument, you recall, only tells us that under certain conditions people are "likely to", or "tend to", or "are disposed to", re-

employ such mechanisms. So what is required for people not to tend to re-employ reliable belief-forming mechanisms? The learning condition has an answer. (Learning****) tells us that if S fails to harbour certain beliefs about a given cognitive mechanism, this is sufficient for S's being disposed to re-employ that mechanism (provided that S identifies the situation as suitable). This is equivalent with the claim that, if S is not disposed to reuse a given belief-forming mechanism on suitable future occasions S believes to be suitable, it is not the case that: (i) S neither believes that P is unreliable nor (ii) that better processes (methods) are available. (Learning****), in other words, also tells us that:

> (Learning*****) If S is not disposed to reuse a belief-forming process (or method) of type P on suitable future occasions (which S believes are relevantly similar to an earlier occasion O on which a mechanism of type P has generated a belief in S), S believes either (i) that P is unreliable or (ii) that better processes (methods) are available.

So (Learning****) indirectly specifies circumstances under which one will not be likely to re-employ a given belief-forming mechanism, and maintains that such circumstances require positive beliefs on the part of the subject about his or her doxastic condition. Hence even if one does not wish to classify requirements regarding the absence of beliefs as (internalist-type) higher-order conditions, (Learning*****) does impose a robust higher-order requirement.

Note that an analogous argument also applies to the less complex formulation of the learning condition, (Learning), that we began with. Even if the advocate of CPS does not want to bid farewell to that initial formulation, he cannot avoid higher-order implications. Olsson and Jönsson's initial (Learning), which they present as an alternative to (Strong Learning), says that if a given belief-forming process "has not been shown to be unsuccessful", people are disposed to reuse it. Olsson and Jönsson maintain that "there is nothing [in this condition] corresponding to the b) clause of Strong Learning (i.e. no thesis concerning when an agent will change a belief forming process" (KL, this volume, ###). But that is at best half the truth. (Learning), that much is correct, does not specify sufficient conditions for changing a belief-forming process. But it is equivalent to the claim that, if people are not disposed to reuse a process, the latter has been shown (to them) to be unsuccessful. Since, first, the epistemic situations envisaged by CPS are such that subjects encounter epistemic "problems of the same kind" they confronted on certain earlier occasions and, second, these problems are

presumably such that the subjects must, or want to, solve them, not reusing a certain mechanism will result in using another one. Hence for situations of this kind (Learning) does specify a necessary condition for people to change from one belief-forming mechanism to another: the former must have been shown to them to have been unsuccessful. Furthermore, if it has been shown to someone that p, the person has good reason to believe that p and will on that basis, if she is minimally rational, actually form the belief that p. It thus follows from (Learning) that normally, if S is not disposed to reuse a given belief-forming mechanism when encountering an epistemic problem (and thus switches to a different mechanism), S believes that the mechanism is, or has been, unsuccessful. (Learning), it emerges, invokes higher-order doxastic conditions as well. It seems that Olsson, Goldman, and Jönsson are fighting an uphill battle when they try to endow their learning condition with a strict externalist interpretation free of any higher-order doxastic constraints.

4. Conclusion

Consider a topic that has so far not been discussed in this paper. For whom does reliabilist knowledge have extra value? Until now we have been grappling with an extra value hypothesis as applied to the subjects of epistemic processes or methods. I argued that a plausible extra-value theory along the lines CPS proposes will have to invoke higher-order doxastic constraints. Could one perhaps avoid that conclusion by switching from a first-person to a third-person perspective? Surely there is a social aspect to the value of knowledge as well. S's knowing that p, it may be argued, has extra value not only for S but, plausibly, also for S's fellow inquirers. If I know that you know whether p, can I not put more confidence in your judgement (and/or statement) that p (or that not-p), compared to a situation in which I only know that you hold a mere true belief about wheter p – possibly hit upon by sheer luck, wishful thinking, hasty generalization, or other unreliable belief-forming mechanisms?

On reflection it soon becomes clear that such a third-person shift does not help with our problem, at least not without once more invoking beliefs on the part of the subject about his or her epistemic position. Why should the hearer's belief that an informant has a reliably generated true belief be more valuable for the hearer than his belief that the informant has a mere true belief? At least for all practical purposes, both kinds of belief on the part of the hearer seem to be equally valuable for him. If I want to get to Larissa and truly believe that your contention, and corresponding claim, about the shortest and safest route is true, this is just as helpful and valuable for me as my true belief that you hold a reliably generated true

belief about the best route to Larissa. What could have surplus value for me (and other travellers) might, under certain circumstances, be the fact that I know that you are in general a reliable travel guide. Such knowledge however would involve true beliefs on my part about how likely it is that your future performances in this business will be successful. However, the quality of these performances depends on the reliability of the belief-forming processes that you will employ and re-employ. That brings us back to square one. For, as argued, unless we have seen convincing arguments that suitable hard-wired mechanisms exist, the most plausible view is that people tend to re-employ reliable belief-forming mechanisms only if they believe that these mechanisms are reliable.

Does this conclusion drain CPS of all its life blood? I do not think so. The externalist analysis of knowledge proposed by reliabilism remains unshaken. But Olsson, Goldman, and Jönsson should bite the bullet and concede that according to CPS the value of reliabilist knowledge exceeds the value of mere true belief only if the subject believes that he/she employed reliable belief-forming mechanisms. In that case we have reason to think it likely that the subject will re-employ these mechanisms. It seems plausible that this conclusion applies, not only to process reliabilism, but also to other forms of reliabilism, including recent forms of agent or virtue reliabilism. The details of such extensions of the above arguments however must await other occasions.

Unfortunately, accepting higher-level constraints will not in the same fell swoop solve the problem for premise (2) of the Olsson-Goldman-Jönsson argument. The chief problem with (2) is to rule out that re-employment of a reliable belief-forming mechanism R may in fact lower the probability that more true belief is acquired, because otherwise a still more reliable mechanism would have been used. I cannot see how this problem could be remedied other than by restricting CPS to situations in which precisely that will not be the case. Yet, why should one not add one more 'empirical condition' to our list, to the effect that it must not be the case that using a reliable belief-forming mechanism R on a given occasion results in the subject's employing a still more reliable mechanism in the future? As argued above, we have no reason to think that this condition is generally fulfilled in the actual world. But that does not prevent us from offering this response to the value problem for those situations in which this condition is fulfilled. This move, and supplementing the conditional probability account with the belief constraints I have discussed, may take us on the right route.[7]

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