

Recursive Tracking versus Process Reliabilism

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Sherrilyn Roush's *Tracking Truth* (2005) is an impressive, precision-crafted work. Although it sets out to rehabilitate the epistemological theory of Robert Nozick's *Philosophical Explanations* (1981), its departures from Nozick's line are extensive and original enough that it should be regarded as a distinct form of epistemological externalism. Roush's mission is to develop an externalism that averts the problems and counterexamples encountered not only by Nozick's theory but by other varieties of externalism as well. Roush advances both a theory of knowledge and a theory of evidence; I focus entirely on knowledge. I shall pinpoint a few respects in which Roush's theory is not wholly successful. In particular, it works less well than process- (or method-) oriented externalisms like process reliabilism.

Nozick's initial tracking account of knowledge was formulated as follows:

S knows that p IFF

- (1) p is true,
- (2) S believes that p,
- (3) if p were not true then S would not believe that p, and
- (4) if p were true then S would believe that p (and would not believe that not-p).

He said that the conditionals in clauses (3) and (4) should be understood as subjunctives, clause (3) expressing what Roush calls the *variation* condition, and clause (4) the *adherence* condition. Nozick later

incorporated into these conditions a restriction on the method S might use in forming the belief.

Nozick welcomed the fact that, according to his theory, knowledge isn't closed under known implication. A salutary upshot of rejecting closure is to avoid wholesale skepticism by preserving the common-sense view that we know things like 'There is a vase on the table.' At the same time it throws a bone to the skeptic, by conceding that we don't know we are not mere brains-in-vats receiving stimulation that makes it look as if there is a vase on the table. Roush is not attracted by Nozick's slant on skepticism. More importantly, she finds closure entirely appealing; so her own theory combines the rudiments of Nozick's tracking approach with an important "positive" place for closure.

Roush proposes that Nozick-knowing, i.e., tracking, is one way to know, but not the only way. You can also know by deducing (or being in a position to deduce) further propositions from Nozick-known propositions. The deducible propositions may not themselves be Nozick-known. Roush therefore offers a *recursive* approach to knowledge in which tracking serves as the base clause while a different, closure-related, clause serves as a recursive clause. In first approximation, the account runs as follows:

S knows that p IFF either (1) S Nozick-knows that p, or (2) p is true, S believes p, and there is a q not equivalent to p such that q implies p, S knows that q implies p, and S knows that q. (Roush, 2005, 43)

The analysis isn't circular because Nozick-knowing may replace the final clause 'S knows that q', and it doesn't itself use the concept of knowledge.

Roush makes another main revision in Nozick's theory. She substitutes a *conditional probability* interpretation for the counterfactual interpretation of the tracking conditionals in the variation and adherence conditions. This is spelled out as follows.

S knows p by tracking p [i.e., Nozick-knows p] if and only if

- I. p is true,
- II. S believes that p,
- III. $P(-b(p)/-p) > T$, and
- IV. $P(b(p)/p) > T$, and $P(b(-p)/p) < 1 - T$. (Roush, 2005, 45)

III and IV are Roush's probabilistic counterparts of Nozick's (3) and (4). To be concrete, she sets threshold T at 0.95 for both III and IV.

Let me now illustrate the rationale for Roush's theory by giving two examples in which her account outperforms Nozick's. Roush accounts for Oscar's knowledge in Goldman's (1976) dachshund-wolf example by saying that Oscar Nozick-knows there is a dachshund in front of him, knows that a dachshund is a dog, and knows that it follows from these two facts that there is a dog in front of him. Oscar's knowledge that what he sees is a dog isn't achieved by pure tracking but rather by deducing it from other propositions that he tracks.

A second case is Nozick's grandmother example. An elderly woman sees her grandson in front of her and believes that he is ambulatory because she sees him walk up to her. This belief, though true, fails condition (3) (and III), because if the grandson weren't ambulatory, the rest of the family would shield her from awareness of that, and she would still believe it. Nozick sought to handle this case by introducing the specification of the actual belief-forming method into the analysis. If S actually uses method M to arrive at her belief, then the same method M must be used in any counterfactual case bearing on condition (3) or (4). Thus, although the grandmother would come to believe (falsely) that the grandson is ambulatory even if he wasn't, this wouldn't transpire by means of *seeing* him walk up to her, but rather by being told by the family that he's fine. Since that belief-forming method would be different, it doesn't constitute a violation of the (expanded) variation condition.

Roush dislikes the incorporation of methods, and excises methods from her own tracking view. She deliberately formulates her recursive theory so that it contains no reference to the method or causal process actually used. Still, she contends, her theory handles the grandmother case just fine. Grandma tracks many things from which it obviously follows that her grandson is ambulatory. One is: 'My grandson is walking towards me.' From this tracked piece of knowledge it follows that her grandson can walk, so it follows that he is ambulatory, and grandma knows this.

Roush's excision of belief-causing methods doesn't work, in my opinion, because a family of counterexamples are in the offing. Consider propositions that are epistemically *basic* (i.e., directly or immediately known) and *maximally specific*, in the sense that the subject knows no other, deductively stronger, proposition. Roush won't be able to use her recursive analysis to account for such instances of knowledge. Yet cases can be constructed in which the known proposition violates a tracking condition that omits a restriction on the method (or process) used.

Here is such a case involving a physical world proposition. Let $p =$ ‘There is a sphere in front of me.’ Suppose p is true and S comes to know it by running his hand around the object (with his eyes closed). If there weren’t such a sphere there, however, S would use *vision* rather than *touch* to try to detect objects in front of him. (We can easily elaborate the story to make this plausible.) Further suppose that S ’s vision is poor, and he is prone to see spherical things when they aren’t there. In short, S violates the variation condition with respect to p . Assuming we have chosen a maximally specific physical object proposition, so there aren’t any propositions $q_1, q_2, \text{etc.}$ that S knows by tracking from which p can be derived. Thus, Roush’s recursive theory decrees that S doesn’t know that p . Surely this is wrong. The fact that if the sphere were absent, S would form a visual hallucination or illusion of a sphere and form a false belief that a sphere is there, doesn’t undercut his tactually acquired knowledge. However, if we return to some variant of a method-restricted subjunctive (or perhaps conditional probability) condition, the case can be handled straightforwardly. So it’s a mistake to excise an actual-method element from the theory. Such an element is just what the doctor ordered.

Here’s another example. Let $p =$ ‘There is an itchy feeling in my right big toe.’ At a given moment, p is true for subject S . S surveys his field of sensation and introspectively forms a belief in p . Does he know p ? A group of neuroscientists are monitoring S ’s brain. If they detect an incipient itch in the right big toe, there is a 0.50 probability that they will prevent such an itch from occurring and will instead feed phantom voices into S ’s mind, which announce, “You are having an itch in your right big toe.” They will also make S (temporarily) susceptible to suggestion by such voices, so if he hears them, he will believe what they say. In actuality, S ’s true belief in p is formed by introspection. But given the facts about the neuroscientists, there’s a substantial probability that if p weren’t true, S would still believe p , though by a different method (testimony). Intuitively, S does know that p . But Roush’s recursive theory can’t account for this knowledge, because S lacks any tracking-based knowledge of propositions distinct from p that jointly imply p . However, if we had a theory in which knowledge-disqualifying scenarios were restricted to ones in which the actual method is used, the counterexample would disappear.

The foregoing discussion suggests that Roush’s theory is less attractive than some of its externalist rivals, for example, process reliabilism. What about her criticisms of these rivals? Some of these criticisms are less than fully compelling. This holds, for example, of her criticism of process reliabilism by appeal to the putative connection between knowledge and power (2005, 126-128). Roush endorses the Baconian

dictum that knowledge confers a capacity to make technological progress because it gives us an ability to maintain true belief about a matter over time and changing circumstances. The fulfillment of tracking conditions, she contends, explains this ability directly. The sensitivity property of a knowing belief (that is, fulfillment of the variation condition) explains a subject's ability to maintain a true belief about that matter through time and changing circumstances. Process reliabilism, by contrast, is declared inferior in this respect:

One can imagine a subject with normal sense organs who nevertheless has very little disposition to use them to form her beliefs; instead, say, she has a strong tendency to fabricate things to believe. At a certain minute of a certain day she uses her sense organs and comes to believe in the normal way that there is a table in front of her (which there is). Such occasions happen only once in a while, though, and are followed by periods of fabrication.

I do not see how the process reliabilist can deny that the belief the subject formed in the normal way when looking at the table was knowledge. ... There is some intuition supporting this judgment, but not one we can indulge in if we want an account of knowledge to explain why knowledge is power. ... To explain the power property, it must be that having knowledge at a given time tends to give one power at later times due to an ability to maintain a true belief about the matter that is gained at the time, and due to the fact that one does have knowledge. (2005, 127-128)

Contra Roush, I think process reliabilism gets the frequent fabricator case exactly right. Knowing *p* at time *t* does not give one power at later times. Only the possession of knowledge (or true belief) at *later* times provides such power. It may not be difficult to attain subsequent knowledge starting from prior knowledge, but it does require the use of a certain *process*, namely, memory. If you don't remember what you knew earlier, then, barring new discoveries, you won't know later. So process reliabilism renders exactly the right judgment about the frequent fabricator. This individual knows whenever she uses reliable processes to form true beliefs, even if she doesn't do this very regularly. Roush admits to having an intuition to this very effect. It's a mystery why she says that one shouldn't "indulge" this intuition. A person doesn't have to be a regular user of reliable processes to acquire knowledge by reliable processes on *some* occasions. Roush seems to be excessively entranced by the knowledge-power connection. She leans in this direction because she somehow interprets the tracking requirements to be future-oriented. But under the subjunctive interpretation the possible scenarios needn't be taken as specifying responses one would make to

future events, only responses to earlier or concurrent events in different possible worlds.

I turn now to Roush's conditional probability version of the tracking approach. I'll pose the question of how probability is to be interpreted in her account of knowing. Roush says little about her intended interpretation of probability, but here is one relevant passage:

The conditional probabilities that I will substitute for these [subjunctive] conditionals have a further advantage that will be attractive to those of a Humean persuasion: whereas it is controversial whether counterfactuals can be interpreted purely in terms of correlations among things in the actual world, it is uncontroversial that probability can be interpreted so. Not every interpretation of probability is so, but one kind of frequency interpretation would have a statement of probability be a prediction of outcomes of future actual trials (2005, 45)

Roush is clearly talking about an objective interpretation of probability, and leaning toward a frequency interpretation in particular. I disagree with her claim that it is "uncontroversial" that probability can be interpreted in terms of correlations among things in the actual world. As she says, the frequency interpretation is the standard attempt to interpret probability in actual-world terms, but it is notoriously problematic whether probabilities about the items in question can be sensibly interpreted under a frequency approach.

How do actual frequencies figure in Roush's story? She first introduces a probability function P_u (the "Ur" probability function) that assigns to all the relevant statements of our language "the probabilities those statements have in the actual world" (2005, 83). When it comes to evaluating probabilities of statements in the hypothetical situations introduced by III and IV, other probability functions are introduced, reflecting changes in the situation from the actual world. Roush offers an elaborate set of "rules of application" for what to vary and what to hold fixed. Ignoring details, the main point is that the probabilities of various statements change in the hypothetical scenarios as compared to their probabilities in the actual situation. However, all probability assignments in the hypothetical scenarios are rooted in probability assignments in the actual situation. The biggest question is what fixes *these* probability assignments. I don't find Roush's answer to this question in the previously quoted passage. (Roush says "future" actual trials, but why restrict frequency determinations to future trials?) The trouble is that the frequency approach has poor prospects for successful application, especially in a domain of singular propositions. I shall discuss finite frequentism in particular, because there is no reason to

think, when discussing actual trials or outcomes, that infinitely many trials or outcomes occur.

Finite frequentism attaches probabilities to either event-types or attributes in a finite reference class. The probability of an attribute A in a finite reference class B is the relative frequency of actual occurrences of A within B. Finite frequentism counts only actual outcomes, not merely possible ones. Does this approach comport with the ordinary understanding of probability? One major problem concerns cases for which a smallish number of actual outcomes occur. Suppose a fair coin is tossed only 10 times during its existence and comes up heads 9 times. Finite frequentism must say that the probability of the coin coming up heads in the reference class of total tosses is 0.90. But intuitively, the probability of a fair coin coming up heads should be 0.50. Even worse, a coin that never gets tossed lacks a probability for heads altogether.

Another core problem with frequentism is that of assigning probabilities to singular events or states of affairs. There is no clear way to do this because there is no unique reference class. Frequentism requires relative frequencies, which involves relativization to a reference class. But when we talk about singular propositions or states of affairs, how should a reference class be chosen? For example, what is the probability of the proposition that I will live to age 90? According to frequentism, this depends on the chosen reference class, and there are innumerable candidates, each of which generates a different frequency value. There is the probability of a human male surviving to 90 years, of a human American male surviving to 90 years, of a non-smoker human male surviving to 90 years, etc. (see Hayek, 2003). The trouble is that all the propositions in question in a theory of knowledge concern singular states of affairs, for example, the probability of Jones believing p at time t. It is especially unclear how to get intermediate rather than extreme values (1 or 0) for such propositions. Shouldn't every true proposition be assigned probability 1.0 and every false proposition probability 0? Roush says no. Indeed, she stipulates that contingent true propositions should receive a value less than one, e.g., 0.999. But why that number? There is no principled treatment here under an actual frequentist interpretation. To be sure, Roush's main interest is not the probabilities of atomic propositions but conditional probabilities. But conditional probabilities raise all the same questions. So I don't see how the P_u -probability function, P_u , gets to be well defined. And this infects the prospects for determinate probability functions in all the hypothetical scenarios that conditions III and IV invite one to contemplate, even if we find Roush's rules of application completely convincing.

Before concluding my (familiar) worries about the frequency interpretation, let me make a dialectical point against Roush's (tentative) deployment of it. The reference-class problem seems to pose a problem for the probability approach that parallels the generality problem for process reliabilism. If the generality problem is a serious liability for process reliabilism, the analogous reference-class problem is at least as serious a liability for the probability approach. This weakens the case for the superiority of her probabilistic version of the tracking theory.

But let me return to my main theme. There is much that is clever and neat in Roush's form of epistemological externalism, a great deal that I haven't had space to touch on. The view could be further strengthened, however, if it didn't deliberately neglect the processes or methods of arriving at beliefs and other doxastic states.

References

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