Reply to Schulkin

Jay Schulkin has several reservations about simulation theory (ST) as a modern variant of the argument from analogy: we have reservations about his reservations. Schulkin classifies ST as a modern variant of the argument from analogy (for other minds), but thinks this argument has long been refuted. Several comments are in order. First, not all simulation theorists construct the theory as a form of the argument from analogy.\(^1\)

However, in our view, the resemblance is close enough to merit discussion.\(^2\) We do not agree that traditional criticisms of the analogy-based argument render it hopeless for cognitive science. The chief philosophical criticism of it, in fact, concerns an epistemological question: could anyone be justified in believing in the mental states of others solely on the basis of their own mental states? This would be an induction from a single case, a very problematic induction. Whatever one's conclusion about this epistemological issue, it does not concern the cognitive science of mind-reading. Cognitive science is interested in the processes by which children and adults actually arrive at third-person mental-state attributions, whether or not these processes yield justified beliefs. The evidential warrant for interpreters' attributions is beside the point, because there is no guarantee that the basic cognitive mechanisms supporting these processes do not yield evidentially impeccable inferences.

Schulkin criticizes the analogical approach on the grounds that "we know a lot about the world and other people's experiences, but when there is no corresponding experience in us." In offering this as a criticism, he seems to assume that ST is committed to the notion that each and every instance of mind-reading involves a simulation heuristic. That is not the version of the approach that we take (see Ref. 3). People often engage in mind-reading based on rules of thumb that may be derived from external stimuli, but do not require current simulation. This is compatible with the idea that we use our own mentality as the "home base" for interpreting others. Our folk psychological knowledge of the mental states of others ultimately rests on our own experience even if it doesn't mean that every instance of new mentalistic knowledge of others requires a corresponding experience in ourselves. It is noteworthy that even some psychologists who otherwise characterize themselves as theory-theorists (e.g. Andrew Meltzoff) explicitly appeal to an analogical inference: "When infants see others acting "like me", sharing their behavioral states, they project that others are having the same mental experience that is 'complemented' by their own."\(^5\) Infants infer intentionality in others based on an analogy to the self.\(^5\) So the analogical inference idea is not a dead letter within cognitive science, even outside the simulation-theory camp.

Schulkin contends that simulation is a computationally more laborious process than theoretical inference, especially when the target's mental state is quite different from that of the attributor. This contention needs much more careful spelling out. The crux of ST is that an attributer attributes mental states to a target by pretending to be in his initial situation and then running a simulation heuristic to determine further states. It is pretending to be in a very different state from one's current state computationally more difficult or time-consuming than pretending to be in a similar state? That isn't obvious. Nor is it obvious that dealing with different states is not in fact more difficult and time-consuming for attributors. That is an empirical question. So even if ST has the implication that Schulkin claims, that is not necessarily a Raw in ST.

On the topic of computational complexity, theory-theory (TT) seems to have a greater burden than ST. TT posits a body of generalizations known by the attributer. In an attributional task, the attributor must posit the target's initial state, access the appropriate laws, and then apply those laws to the initial states to infer further states. TT avoids the need for knowing and accessing generalizations because it posits the use of one's own computational operations. These operations can just be used without the attributer necessarily knowing and accessing their lawful properties. Finally, because of the holism involved in standard forms of TT, the meanings of one's mental-state concepts will be constantly changing as one acquires new laws (in terms of which the concepts are defined). The constant updating of the meanings of mental-state concepts adds a further computational burden.

Schulkin likes the TT story because of the belief that the contents of the mind are as theoretical as the contents of the kidney. Does he mean that both are unobservable? The claim that mental states are unobservable is one that many theory theorists make. But is it correct? Certainly it is true that someone else's mental states are as unobservable to an attributer as someone else's kidneys. But mental states in general may not be unobservable if it is possible to see inner observation of one's own mental states and learn properties of these states with the help of such "observation," self-monitoring, or introspection. The idea of internal observation, or self-monitoring, continues to be a widely accepted approach, if not a consensus approach, among both philosophers of mind and cognitive scientists.\(^3\) This approach should not be tarnished with the brush of Cartesianism. Internal monitoring is not committed to Cartesian infallibility or indubitability. Moreover, not that ST does not necessarily require conscious processing or the use of introspection. Mirror neuron activity, for example, is presumed to be automatic and unconscious. Our paper did not explicitly invoke introspection for any point. However, we do think that, unlike TT, folk-psychological simulation is sometimes conscious and sometimes employs introspection.

If the mental states posited by folk psychology were just as unobservable as the states of the kidney, they should never be part of our conscious experience. Folk psychology ought to posit states that are wholly unobservable in the sense that their instances are never observable by external perception or internal monitoring. At least some of the instances of folk psychology should be like the states of the kidney. The fact of the matter is quite different. However, the stock inventory of folk psychological states - seeing, believing, wanting, intending, fearing, and so forth - are all states some of whose tokens are conscious, and hence introspectively accessible. This is not what one would expect if the basic cognitive operations of mentalistic concept formation paralleled that of theoretical science.

Schulkin spends a lot of time discussing the term "detached" - this is unfortunate. The word occurred once in our article, and as readers familiar with the debate will know, it does not play a central role in our case. Rather, it is a better articulation of the ST/TT contrast (a contrast more fully detailed in our original Fig. 3; see Ref. 9) to say that ST postulates "shared" states in mind-reading, whereas TT postulates no such sharing. According to ST, when I "mind-read" your intentionality, I form a pretense intention with the very same content. According to TT, there is no correspondence between ST's and TT's representation of the real issues.

The idea of "shared" states is the focal point of our interest in mirror neurons (MNs). In observationally stimulated MN activity, the observer shares a certain state with that of the target actor. If this sort of mechanism is the basis for, or a precursor of, interpersonal mind-reading, then this suggests a process of mind-reading that is more like simulation than like theorizing. The fact that mirroring activity does not take place only in the premotor cortex. A similar phenomenon is apparently found in pain-related neurons. Hutchison et al. have studied pain-related neurons in the human cingulate cortex.\(^4\) Cingulotomy procedures for the treatment of psychiatric disease provided an opportunity to examine whether neurons in the anterior cingulate cortex of awake humans
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Irregular transcripts?

One of the most famous phenomena in language acquisition is the over-regu-
larization of the past tense of English. According to conventional wisdom, chil-
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shaped curve: initially, children use all past forms correctly; then, correct per-
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duce all forms correctly. The meaning of this pattern was hotly debated –
does the dip reflect the over-applica-
tion of a newly learned rule, or is it a natural by-product of a connectionist association network? – until Marcus et al. pulled the rug out from under the controversy. After a close examination of transcripts of children’s speech, they found that actual rates of over-regu-
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culties with the irregular past forms, which normally serve to block the regular forms.

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frequency verbs, the over-regularization stage would last only a matter of weeks and therefore be largely invisible on the transcripts. Only for relatively infrequent verbs will the over-regularization period last long enough to be detectable in the transcripts. Maratos undertook a verb-
by-verb analysis of past tense forms and showed that indeed, for relatively infre-
frequent verbs, over-regularization rates were quite high (on the order of 30 to 50%).

References

Summaries of recently published papers of interest to cognitive
scientists. Readers who would like to contribute to this section, by identifying appropriate papers and writing short summaries, should contact the Editor (tics@current-trends.com).

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