

Mental Simulation: the Old-Fashioned Dispute¹

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We attribute psychological states and intentional actions to others. We also predict such states and actions and explain them. I attribute to Fred the belief that that mountain in the distance is the Schneeberg as well as the thought that it would be pleasant to climb it. I predict that later in the week when there is some free time Fred will form the intention to climb the mountain that very afternoon. And when one morning later in the week Fred says, "Let's drive to Puchberg [at the foot of the Schneeberg] this afternoon," I have no problem explaining what is up: Fred has Schneeberg plans. Further, we often do this spontaneously, effortlessly, and, in the majority of cases, successfully.

How do we do this? How do we attribute, predict, and explain the psychological states and intentional actions of others? This seems to be mainly a question for psychology, or, more broadly, cognitive science. Let us call it the 'cognitive-science' question. In the last fifteen years or so there has been a lively dispute over the answer to the cognitive-science question (Davies, 1995a and Davies, 1995b contain representative articles). The theory-of-mind theory (TT) holds that we handle the above psychological phenomena by deploying a psychological theory, which will contain, among other things, familiar commonsense generalizations. The simulation theory (ST), on the other hand, denies that we employ psychological generalizations; rather, we exploit the similarities between others and ourselves and we simulate them. We put ourselves in the other's shoes, see what we would think or do, and go on to infer that the other will think or do the same. Turning my gaze in the direction of Fred's, I think, "It would be nice to climb that mountain," and go on to infer that Fred thinks so as well.

Interestingly enough, the recent dispute grows out of a much more traditional dispute. Over a half century ago, in his brief foray into the philosophy of history, Carl Hempel held that scientific historical explanation "can be achieved only by means of suitable general hypotheses, or by theories." He attacked "the familiar view that genuine explanation in history is obtained by a method which characteristically distinguishes the social from the natural sciences, namely, *the method of empathetic understanding*" (Hempel, 1959, p.352). To use the method of empathy the historian must imagine himself in the place of the historical figures whose thoughts and actions he wants to explain and see how "he himself would act under the given conditions" (pp. 352-3). In other words, although Hempel does not use the expression, the historian must "simulate" the person he is studying. Such a procedure, according to Hempel, "may sometimes prove heuristically helpful," but it does not establish the "soundness" of a historical explanation. Nor is such a method indispensable for historical explanation (p. 353). Hempel's attack on the method of empathy was directed to thinkers in the *verstehen* tradition, such as Dilthey and Collingwood.

Hempel is not really concerned with the question of what historians, or ordinary people, do. Rather, he is

concerned in the main with the following question: Whether or not historians or ordinary folk do use the method of empathy, or simulation, is such a method a *good* method (a permissible, justifiable, or even indispensable method) for arriving at truths about the psychological states and intentional actions of historical figures or ordinary people. The traditional dispute pitting theory against empathy/simulation, in which we can count Hempel as a representative participant, is thus different from the cognitive-science dispute. Let us call the traditional dispute the 'normative' dispute.

The traditional normative question seems out of fashion today. I want to revive it. My question, then, is this. In the light of the recent cognitive-science dispute what can be said in answer to the normative question of whether the method of empathy/simulation can provide us with a good method of acquiring knowledge about others' thoughts and actions? I shall argue that the cognitive-science discussion, not surprisingly, can shed some light on the old normative dispute, but that that the light that it sheds leads us to some pessimistic conclusions about at least the short-term prospects for empathy/simulation as a good method.

1 The old normative question is that of whether in addition to the theory method used in the natural sciences --- the method of theory construction, testing, and application --- there is also a method of *verstehen*, empathy, or simulation that can be used to acquire knowledge in the human sciences. But what is a method of empathy or simulation? Here the cognitive-science discussion has been very helpful. A representative and useful cognitive-science characterization of simulation is Goldman's. Simulation involves the investigator's going through a succession of states that is isomorphic to the actual succession of states of her subject. Further, the investigator's succession of states must be "process" rather than "theory" driven, that is, driven by psychological mechanisms similar to those of her subject that are deployed but not represented in her mind (Goldman, 1995, p. 85).

Consider an example (following Harris, 1995, pp. 210-11). I am asked to predict whether my friend Peter, from Michigan, will think that the following sentence is good English: "The piano weighs this pound." I reply spontaneously, or at least with little conscious thought, "Of course not!" Here it does seem plausible that I do use simulation along the lines suggested above. I repeat silently and represent to myself the sentence about the piano. This input is then processed by linguistic mechanisms that I share with Peter but which I do not represent and theorize about --- I am not a theoretical linguist and at least in this case do not have much of a clue as to what relevant rule is operating. Finally, the output of the process, which I use to make my prediction about

¹ This paper expresses in briefer, but hopefully clearer form, my views in Fuller, 2002. Some short passages, as well as some examples, have been drawn from that article.

Peter, is my thought that the sentence is indeed grammatically deviant.

Contemporary ST is a cognitive-science account of how we do handle psychological attributions, irrespective of whether we know we are using simulation or not. Simulation as a normative method, however, must meet higher standards. Scientific methods of inquiry, after all, should consist of a set of explicit requirements that the investigator can ascertain to have been met. Can Goldman's account of simulation be turned into such a normative method? It seems that it can. A scientific method of simulation would have to meet the following requirements, among others. The investigator must have good reason to believe that her psychological mechanisms are relevantly similar to those of her subject. She also must have good reason to believe that her inputs and those of her subject are relevantly similar. And finally, she must have good reason to believe that her output state is produced by her psychological mechanisms and not by some theoretical inference.

The good news is that there is some reason for believing that these requirements for simulation as a normative method can be met in many cases. A few years ago I wanted to get my sister a present for her birthday. I explicitly put the method of simulation into practice. I was thinking of getting her Glen Gould's version of the late Brahms Intermezzi. But would she like the CD? I had, and still have, inductive reasons for believing that our musical psychologies are similar. She and I had the same musical education when we were young, including the same piano teachers and the same exposure to classical recordings. We also have tended over the years to agree in our likes and dislikes of classical pieces of music in a wide range of cases and circumstances. I exposed myself to an input that was obviously relevantly similar to her future input: I bought a copy of the Gould recording and listened to it. I was enthralled. I predicted that she would be as well. She was. The example of my sister's present and many others, however anecdotal, do give us some reason for thinking that simulation can provide a justifiable, and even a sometimes practically indispensable, method for arriving at knowledge of others' psychological states.

2 I said earlier that the cognitive-science discussion leads to some pessimistic conclusions about simulation as a normative method. After the optimism above, how can that be? What is the problem? The problem is that the type of simulation about which there is some cause for optimism is actual (or actual-situation) simulation in which the inputs and outputs of the simulation are 'real.' In the case of my sister's present, for example, I really do listen to the Gould CD and enjoy it. Opportunities for using actual simulation in attributing psychological states in everyday life, and certainly in history, are quite limited. A more comprehensive and significant account of simulation will have to include off-line (or pretense-driven off-line) simulation, in which at least some of the inputs are pretend ones (see Stich and Nichols, 1997). Indeed, ST has been mainly concerned with off-line simulation.

In off-line simulation the inputs, or at least some of them, are pretend ones. (And, of course, so are the outputs.) When I explain what Fred is up to in wanting to drive to Puchberg, I may actually believe that it is Aspang that is at the foot of the Schneeberg; nevertheless, I form the pretend belief that it is Puchberg. Or, suppose that I am told that Mrs. Smith has just sat down at a table in her local restaurant and is being handed the menu by a new waiter, when she notices a distinctive scar on the waiter's hand and thereby recognizes him as a wanted criminal. I

try to predict what she will do by forming, among other states, the pretend beliefs that I am in the restaurant and that the waiter is a wanted person (adopted from Heal, 1996b, p.79). Finally, the historian, in trying to explain Caesar's famous return to Rome, forms the pretend belief that the Rubicon is in front of her. Or at least this is so if the supporters of ST are right.

Off-line simulation, it seems, can also be reformulated into an explicit method with similar requirements to that of actual simulation, although two important points need to be stressed in this case. First, to get himself into the initial pretend state, the off-line simulator must focus on the outside world. To form the pretend belief about the waiter with the scar, I must imagine: There is a waiter with a scar in the restaurant. Second, to insure that the pretend inputs are being handled in a process rather than a theory driven way, the simulator must ask not "What *would* I do if I believed the waiter was a wanted criminal?" but rather "What *is* the thing to do in a situation like this?"

It is here that we start getting into trouble. The cognitive-science discussion has shown that off-line simulation, or better, off-line attempted simulation, does not work in many cases. It may work in some cases of off-line simulation of perceptual inputs (imagine that the mouth of a 'smiley' face is turned downwards), but it clearly does not work in others (imagine that you are looking at two equal lines encased by pairs of wedges arranged in the familiar Muller-Lyer way). It does not work in predicting what people, including oneself, will do in Milgram experiments. Stich and Nichols have provided some evidence that it does not work in predicting the behavior of subjects in the Langer lottery experiment. In that experiment subjects buy a lottery ticket and are later given the opportunity to sell the ticket. Subjects who initially are just handed their ticket are willing to sell it for less than those who initially *chose* their own ticket (Stich and Nichols, 1997, p. 311). In other words, even if the investigator shares psychological mechanisms with the subject and her pretend inputs have similar contents to the actual inputs of the subject, the two outputs may be different.

What turns this initial trouble into bad news is that, at least at present, no one has a systematic account, or even a good sketch, of how to draw the distinction between cases where off-line simulation works and those where it does not. Jane Heal's view that off-line simulation works for *rational* transitions between states with propositional contents is as good as any (Heal, 1996b, p. 56). There are, however, at least three serious problems with it. First, the rational-irrational distinction is far from clear. Are the subjects in the Langer experiment really irrational? Second, even if the rational-irrational distinction can be clarified, there may be no easy way to apply it on the basis of recognizable features. Assuming Heal is right, if I am to make a defensible decision of whether or not to use off-line simulation to predict what Langer subjects will do, I will need to know beforehand whether rational or irrational processes will be involved. Finally, there may well be many 'rational' cases where off-line simulation does not work. I suspect that it will not work in many cases of predicting emotions, even where the emotions are ordinarily thought to be quite rational. Again, it may not work in cases of predicting the results of reasoning which involves a subtle weighing of evidence. This ignorance about how to draw the distinction between good and bad uses of off-line simulation gives rise to doubts about almost any particular use of off-line simulation as a scientific method. Of any particular use of off-line simulation I do not know whether it is of a type that is likely

to work or not, and hence I am unjustified in using it on that occasion.

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