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Commentary

Alternative routes to perspective-taking: Imagination and rule use may be better than simulation and theorising

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This article is a commentary on 'Two routes to perspective: Simulation and rule-use as approaches to mentalizing' (Mitchell *et al*. in press).

Thirty years after Premack and Woodruff (1978) asked whether the chimpanzee has a theory of mind, the literature on how humans and non-human animals explain and predict each other's behaviour in terms of unobservable mental states is in a curious condition. On the one hand, there are clear markers of success, such as the hundreds of published studies on the causes and consequences of theory of mind in 3- to 5-year-old children (e.g. Milligan, Astington, & Dack, 2007; Wellman, Cross, & Watson, 2001), and the impact of the hypothesis that impaired theory of mind may be a core feature of some developmental and psychiatric disorders, such as autism and schizophrenia (e.g. Baron-Cohen, Leslie, & Frith, 1985; Frith, 1992). On the other hand, research on theory of mind has been strangely hidebound. One symptom is that by far the most empirical attention has been centred on a very narrow age range of preschool children, tested on a very narrow range of tasks, raising the possibility that critical phenomena are being missed entirely. Another is that, while theoreticians have discussed whether theory of mind abilities are better characterized as a common sense 'theory' or a species of mental simulation (e.g. Carruthers & Smith, 1995), it has proved surprisingly difficult to cash these theories out in empirical predictions. As a result there is a tendency for empirical studies on theory of mind (henceforth ToM) only to pay lip service to these theories, or else ignore them completely. Mitchell *et al*.'s (in press) paper is a very welcome attempt to tackle both of these general symptoms.

The authors draw on a much wider range of evidence than is common in discussions of ToM, including work on autism, and exciting recent developments in infancy research and cognitive neuroscience. Perhaps, most unusually, they consider what can be learned

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from behavioural studies of older children and adults who would pass the 'standard' tasks most often used in the literature. Their approach considers novel tasks, and novel measures such as response times or response bias. This requires careful consideration of just how an experimental participant may approach the ToM problem in hand, and leads the authors to the view that mature participants may have two alternative strategies available. They suggest that, like young children, older children and adults can solve ToM problems by imagining themselves in the position of the other person, and seeing how they would think or feel in those circumstances. The authors suppose that this strategy would be flexible but cognitively effortful (among other things, requiring the participant to set aside their own salient perspective). The authors' more unusual suggestion is that older children and adults have also had the opportunity to abstract from their experience of imaginatively projecting themselves into the shoes of others some rough-and-ready rules, such as 'people think things are where they last saw them'. Although these rules would not always yield the correct answer (for example, because people sometimes get suspicious about what might have happened while they could not see), they might offer a quick solution in simple cases, without the need for cognitively effortful imagination and projection. This way of thinking is likely to prove productive in future work, partly because some of the specifics may indeed be correct, and also because this way of thinking throws new light on enduring questions. I will elaborate on some of these points in the following sections. However, where I depart most strongly from the authors' way of thinking is their alignment of their account with the theoretical debate between simulation theories (STs) and 'theory' theories (TTs). Here I think the authors' account fails, not because of any weakness in the data, or the other ideas fielded in their article, but because it remains extremely difficult to derive testable predictions from these theories.

Simulation theory and theory-theory

The authors begin by outlining the two main theoretical accounts of ToM. According to so-called 'theory-theories', our ability to think about the minds of others depends upon us having concepts of mental states (beliefs, desires, etc.) and a set of principles that describe how they arise and how they interact (e.g. perceptual access to an object leads to knowledge about that object; people act to satisfy their desires on the basis of their beliefs and knowledge). STs, by contrast, argue that ToM does not require an exhaustive theory of mental states. The critical observation is that biology ensures that we each have a mind that operates on the same causal principles as anyone else's (i.e. beliefs and desires have just the same role in my cognitive economy as they do in yours). This being the case, I might not need a theory of how your mind works if I could imagine myself in your situation and allow my own mind to simulate what you would do or think. Originally, ST and TT were viewed as alternative accounts of ToM, but many theorists now consider that people may have both ST and TT strategies available (e.g. Carruthers & Smith, 1995; Currie & Ravenscroft, 2002; Nichols & Stich, 2003), and indeed that any given instance of mentalizing may involve both ST and TT processes. This is all well and good in theory, but a consistent sticking point has been the difficulty of using these accounts to generate empirically testable predictions. This is one of the challenges taken on by the current authors.

The first component of the authors' strategy is to equate TT with rule use: 'When we adopt a rule-based approach we treat agents as objects of investigation, much as when we investigate the behaviour of planets and electrons: we look for rules and initial conditions from which we can predict or explain their behaviour, though the rules we use in the case of agency may be different in kind from those we look for in the case of planets and electrons'. The second component is to argue that there is limited empirical support for rule use in ToM, and conclude instead that simulation is the primary ToM process. I will treat these components separately.

Firstly, the authors' rule-based account does not accommodate all versions of TT. Although some theory-theorists do hold that ToM consists in explicit, personal-level reasoning about the concepts and principles governing the behaviour of agents (e.g. Gopnik & Wellman, 1992), other theory-theorists see ToM as subpersonal computation in a symbolic language of thought, perhaps instantiated in a specialized processing module (see Stich & Nichols, 1992). Between these extremes are theory-theorists with a variety of shades of opinion, many of whom would not feel that their particular version of TT had been undermined if the current authors' arguments against a rule-based account went through (see, e.g. Davies & Stone, 1995a,b). This is a big problem for the current authors if our only interest is in the debate between ST and TT, but I think this would miss the point. The notion of rule use discussed by the authors is a psychologically plausible way in which some of the work of ToM might be achieved, and so the authors' evaluation of the evidence for and against such rule use is of considerable interest.

Rules

One piece of evidence discussed by the authors is whether the shape of children's developmental trajectory on ToM tasks such as the false belief task is compatible with developing rule use. The authors sketch a plausible scenario for how children's success might change if performance on the task was governed by the possession of a single rule about beliefs. At an early point in development children might have no rule at all, so when presented with the two-alternative-forced-choice available in some (though not all) false belief tasks, they should simply guess, scoring 50% as a group. Somewhat older children might acquire an incorrect rule, such as 'people think what I think', leading to systematic error (0% correct). Only subsequently would children adopt a correct rule about how beliefs are formed, and so answer false belief tasks correctly.

The authors observe that such U-shaped development does not fit the evidence. The largest meta-analysis of false belief task performance to date (Wellman *et al.*, 2001) suggests that children initially perform significantly below chance, rather than the chance performance predicted by a rule-based account. The authors argue that this pattern is in fact more compatible with children using a strategy of simulation that leaves them vulnerable to egocentric interference. Whether or not this is so for simulation (see below), there are reasons to be cautious about the authors' interpretation of the Wellman *et al.* findings. The problem is that Wellman *et al.* systematically excluded studies in which the initial sample of children made errors on control questions more than 40% of the time. This is perfectly sensible as a way of ensuring that most children taken into account in their meta-analysis had the cognitive wherewithal to follow the scenario of the false belief task. But it also means that the very group of children who are most critical to Mitchell *et al.*'s hypothesis – those who do not yet even have an incorrect rule about belief – may have been largely excluded from Wellman *et al.*'s analysis. So Wellman *et al.* (2001) may not provide the evidence that Mitchell *et al.* require on the shape of developmental change.

A second important question discussed by the authors is whether the rate of children's developmental change is 'gradual' or 'sudden', and whether this has any bearing on rule use versus simulation. It must be said that even on the best-studied example of false belief tasks, there is considerable disagreement on whether children's development is gradual or sudden (see Wellman *et al.* (2001) and the commentaries that follow). But let us accept, for the sake of argument, Mitchell *et al.*'s contention that developmental change is 'gradual'; should this cause us to doubt that children pass false belief tasks by acquiring new rules?

I think the answer depends critically on how many rules we think might be at work. Mitchell *et al.* suppose that a single rule is relevant, and so might be entitled to expect 'sudden' changes in performance as children acquire this new insight. But this is surely not necessary. It might equally be that children learn a variety of rules about the causes and consequences of beliefs (and other mental states), some more complex than others and some more accurate than others. The probability of a giving a correct response on a given trial of a given false belief task would then depend on which rules the child had acquired and which were selected, and this selection might depend on a variety of contextual and processing factors. Accounts of just this kind have been developed to explain gradual change in performance in other domains, such as number concepts or folk physics (e.g. Siegler, 1995; Siegler & Chen, 2008). And indeed, some of the strongest evidence in favour of gradual change in performance on false belief tasks – which comes from longitudinal microgenetic studies (e.g. Flynn, O'Malley, & Wood, 2004) – have been interpreted in terms of children having multiple strategies simultaneously at their disposal (Siegler, 2007). Thus, the pace of developmental change does not appear to be decisive in questions about the role of rule acquisition in ToM development unless we know the number and nature of the rules. Although the authors assume that a single rule is relevant, it is not clear why this should be so.

Leaving these issues aside it is important to note that the authors do not aim to deny that there is any role for rules in ToM development, but rather to say that ToM via simulation is developmentally prior to ToM via rules. They also offer some revealing hints about the cognitive effects that they think rule use might achieve: 'Using rules offers a shortcut [around simulation], and has added value in conferring protection against systematically reporting . . . [the participant's] . . . own knowledge'. For what it is worth, I think the authors are on to something very important here. But at first blush their statement seems curious. If we were to admit the possibility that a large number of rules might be at work (as many theory-theorists would) then it is not at all clear that rule use should offer a shortcut, by which I take the authors to mean a quicker and perhaps less cognitively demanding processing route than simulation. Nor is it obvious why rule use *per se* would confer protection from any tendency for interference from one's own knowledge (egocentric interference could surely arise out of biases in the context-sensitive rule selection proposed by Siegler). These considerations suggest that the authors have in mind a much more restrictive sense of what it would be to use rules for ToM than is fully spelled out in their paper.

In order to offer a processing shortcut it would have to be that the number of rules was small enough that selecting the appropriate rule did not require complex decision making. To illustrate, we might imagine the rule 'If agent A was present when fact X was manifest then A knows about X'. This clearly falls well-short of an exhaustive theory of knowledge, and we might be tempted to add further rules about the need for the A to be sentient, to be attentive, and to know enough already to grasp X when X was made manifest. But in doing so the cognitive economy of the simple rule would rapidly be lost. Moreover, the larger the number of rules and the more complex the information that they draw upon, the less likely it seems that the rules could be selected and followed without interference from one's own knowledge. Thus, to meet Mitchell *et al.*'s demands we should not expect these rules to constitute anything like an exhaustive theory of the mind, and we should not expect them to draw on sophisticated background knowledge. Combine this with Mitchell *et al.*'s suggestion that the rules are derived from prior experience with ToM via effortful simulation, and this position meshes in an interesting way with a suggestion from Suddendorf and Whiten (2003; see also Povinelli & Giambrone, 1999), who propose that humans' initial effortful attempts at ToM may become automatized with practice. The unique addition from Mitchell *et al.* would be to make the potential costs and benefits of automatization explicit. The product of automatization would need to be a relatively small number of rules in order for these rules to be used efficiently, and this would mean that rule-based judgements would be prone to over-simplification. But one critical benefit, besides speed and efficiency, is that the participant's rule-based judgements might be protected from interference from their own knowledge.

Egocentrism, executive function, and developmental origins

One pervasive phenomenon in research on ToM is that errors tend not to be random. Instead, across a variety of tasks and a variety of ages, errors tend to consist of reporting one's own belief, desire, or knowledge rather than that of the target person whose perspective you are supposed to be taking. This systematic bias goes under a variety of names, including 'egocentrism' (e.g. Epley, Morewedge, & Keysar, 2004), 'reality bias' (e.g. Mitchell, Robsinson, Isaacs, & Nye, 1996) and 'the curse of knowledge' (e.g. Birch & Bloom, 2007). It is revealing because it suggests that participants who make errors are not merely confused or forgetful, nor simply in the grip of some incorrect theory or another: something about how we take the perspective of others disposes us to respond, mistakenly, in terms of our own point of view.

To explain this tendency for systematic bias on perspective-taking tasks, the authors adopt the idea that self-perspective forms a default setting when thinking about the perspective of others. This default may be a helpful bias for the great many circumstances in which the other person thinks, knows, or wants the same thing as the self. However, when self and other perspectives are different (as in many laboratory tasks, such as the false belief task) the default needs to be set aside. According to the authors, the difficulty of this setting-aside depends upon the relative salience of self and other perspectives. And the likelihood that the participant will meet this difficulty depends upon the executive resources that they have available. In support of this, they cite evidence that performance on false belief tasks may be improved by manipulations that make the self perspective less salient or the other's perspective more salient, and that limitations on executive processing due to immaturity, brain injury, or concurrent task performance all result in limitations in performance on ToM tasks. I think this evidence is indeed persuasive, as is the authors' recognition that merely naming the phenomenon of 'egocentrism', 'reality bias', or 'curse of knowledge' does not provide any cognitive explanation for why the phenomenon exists or how it achieves its effects. However, this leads the authors into a discussion of ToM abilities in infants, which I did not find successful.

In support of their contention that processing factors rather than conceptual change are responsible for 3- to 4-year-olds' changing performance on ToM tasks, the authors discuss recent findings that suggest that infants as young as 12 or 15 months may pass false belief tasks (e.g. Onishi & Baillargeon, 2005; Surian, Caldi, & Sperber, 2007). In these tasks, infants view a non-verbal scenario in which an agent comes to have either a true or false belief about the location of a hidden object. The critical finding is that infants are 'surprised' (i.e. they look longer) when the agent searches for the object in a way that is inconsistent with their true or false belief, compared with when the agent searches in a way that is consistent with their belief. These findings are certainly exciting, though their interpretation remains controversial (e.g. Perner & Ruffman, 2005), but let us for now take them at face value as evidence that infants understand about beliefs. How does this support the authors' argument that success on false belief tasks is determined by the relative salience of self and other perspective and the availability of executive resources for selecting one perspective over the other?

One thing we can be pretty sure of is that infants who 'pass' these false belief tasks have more limited executive resources than 3-year-olds who fail more standard false belief tasks, so the availability of executive resources cannot be the critical factor here. Therefore, the burden rests on showing that some methodological factor results in the salience of the infants' self-perspective being lower and/or the salience of the agent's perspective being higher than on standard false belief tasks. Here, I am afraid, the authors offer us nothing. One of the powerful things about the tasks used with infants is that the event sequences and the belief ascriptions that they seem to involve are very similar to the tasks used with older children. The mere fact that the tasks used with infants are non-verbal is clearly insufficient to explain good performance, since other non-verbal tasks appear just as hard, if not harder, than standard verbal false belief tasks (e.g. Call & Tomasello, 1999; Grant *et al.*, 2007). The authors ' . . . suggest that the young children understood the principle that other people hold beliefs, but were pone to error when performing the task of working out what that belief actually was . . . '. This might just explain the discrepancies between eye-movements and explicit pointing responses in studies of 2- and 3-year-olds (Clements & Perner, 1994; Ruffman, Garnham, Import, & Connolly, 2001), but it cannot explain why infants were 'surprised' in Onishi and Baillargeon's (2005) study, because 'surprise' at a belief-incongruent action clearly entails that the infants were committed to what the agent's belief actually was. Like other authors who infer from Onishi and Baillargeon's findings that infants 'understand beliefs' (e.g. Leslie, 2005; Surian, Caldi, & Sperber, 2007) Mitchell *et al.* do not explain why this understanding is manifest in one paradigm and not another. This means that their account does not explain the infancy data, nor do the infancy data offer any real support Mitchell *et al.*'s account.

Of course, this also leaves us with the genuine puzzle of how to explain infants' good performance on false belief tasks. Here, I think we would do well to look to other conceptual domains, such as number and physics, which have already been through the revolution of finding that infants have abilities that we once thought were only present in older children. In these domains, the post-revolutionary settlement seems to be that infants do indeed have abilities in number and physics, but these competencies fall qualitiatively short of the abilities of adults, which are developmentally slow to acquire and depend on executive function and language. My money is on something similar being true for ToM.

Simulation and counterfacutals

The authors suggest that the primary ToM process is cognitively effortful mental simulation, and support this contention with several lines of argument. Above I questioned whether either the implausibility of 'rule following' or the existence of egocentric errors offers any reason for supposing that most ToM reasoning uses simulation. The current failure of neuroscience to provide clear evidence that propositional attitudes – such as beliefs and desires – are ascribed via simulation has been discussed elsewhere (e.g. Apperly, 2008). In a further line of argument, the authors suggest that both ToM reasoning and counterfactual reasoning may involve mental simulation. In support of this, they note that children's ability to pass false belief tasks is correlated with their ability to pass tests of counterfactual reasoning. However, I think the first suggestion is questionable, while the empirical phenomena could be explained by other means.

On my reading, there are potentially different notions of simulation at work for counterfactual reasoning and ToM reasoning, which Goldman (2006) describes as 'theory-driven' and 'process-driven' simulation, respectively. The sense in which simulation might be involved in counterfactual reasoning is the same sense in which we might use a computer to simulate a hurricane. There is no special relationship that makes computers good at simulating hurricanes: the accuracy of the simulation will depend on the accuracy of the model that the computer is running, and that model may depend on an explicit theory of hurricanes, a massive database of previous hurricane behaviour or most likely some combination of the two. Similarly, my ability to reason counterfactually will depend on whether I have a sufficiently accurate model of the world to run a simulation of how the world might be according to counterfactual premises. Now, it may indeed be that simulation is involved in ToM in precisely this sense. That is to say, it could be that both counterfactual reasoning and ToM involve theory-driven simulation. If this were so, then it would provide a ready explanation for the empirical phenomena of a developmental correlation between ToM and counterfactual reasoning[1].

Critically though, theory-driven simulation is not the sense of simulation that distinguishes ST from TT. As mentioned earlier, the distinctive point made by simulation theorists is that there *is* a special relationship that could make minds good at simulating minds, which is that one mind instantiates a system operating on the same fundamental principles as any other. If this unique relationship could be exploited, my mind would not need a theoretical model of a mind or a massive database of previous mind-governed behaviour to simulate your mind: that information is already implicit in the functional architecture of my own mind. 'All I would need to do'[2] is imagine myself in your shoes and let the functional architecture of my mind work out what I would do if I were you. The problem is that this distinctive feature of STs is not shared with counterfactual reasoning (except for the special case of counterfactual reasoning about other minds), so developmental correlations between counterfactual reasoning and false belief reasoning do not provide evidence to support ST.

Conclusion

Altogether then, Mitchell *et al.* assimilate a very impressive range of data and provide valuable suggestions about the nature of theory of mind processes, how they may be developmentally linked, how they may be impaired, and the different means by which they may be studied. What I take to be one essential component of their view is that

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[1] *Another, less theory-laden, interpretation of the developmental correlation between false belief reasoning and counterfactual reasoning data is that both depend on executive function, which is also developing between the ages of 3 and 5 years (e.g. Carlson, Moses, & Hix, 1998; Riggs & Beck, 2007; Russell, 1996).*

[2] *Although it is often supposed that generating the starting conditions for a simulation would be straightforward I suspect that it is far from simple in terms of its cognitive demands (e.g. Apperly, 2008; Nichols & Stich, 2003).*

effortful processes of imagination, introspection, and projection are the primary means of mental state ascription for children. Another is that recurrent patterns in mental state ascription become abstracted into rules, which allow efficient ascription in commonly occurring cases. I do not think that these distinctions cut at the joints between ST and TT, but I am not sure that this should be seen as a serious weakness. The distinctions offered are insightful and psychologically plausible, and clearly offer ways of generating testable hypotheses for further work. This fits with a more general pattern of difficulty finding generalisable methods for deciding whether a given instance of ToM involves ST or TT processes. Where it was once debated whether ST and TT were 'the only games in town' (Astington, 1995), in the case of the propositional attitudes most typically studied in ToM research I wonder whether we should be asking whether they are games worth playing at all.

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