

The Limits of Spectatorial Folk Psychology

DANIEL D. HUTTO

Abstract: It is almost universally agreed that the main business of commonsense psychology is that of providing generally reliable predictions and explanations of the actions of others. In line with this, it is also generally assumed that we are normally at theoretical remove from others such that we are always ascribing causally efficacious mental states to them for the purpose of prediction, explanation and control. Building on the work of those who regard our primary intersubjective interactions as a form of 'embodied practice', I defend a second-personal approach in this paper.

1. Introduction

It is almost universally assumed that the main business of commonsense psychology is to provide generally reliable *predictions* and *explanations* of the actions of others.¹ Some bold claims have been made concerning its importance. For example, we are told that: 'We have no other way of describing behaviors and their causes' (Fodor, 1987, p. 8); that 'everyday affairs are well nigh unthinkable without the apparatus of the attitudes' (Baker, 1995, p. 4); and that it is, 'the best way to make sense of the action of others' (Baron-Cohen, 1995, p. 21).

Baron-Cohen goes so far as to claim that the development of an innate 'mind-reading' module was the ancient solution to an adaptive problem that arose for our ancestors during the Pleistocene epoch: 'the rapid comprehension and prediction of another organism's behavior' (Baron-Cohen, 1995, p. 12). This and his other remarks reveal that he holds that folk psychology plays a part in even the most basic forms of social navigation and co-ordination. Of course, if commonsense psychology did have such absolutely *groundfloor* utility it would certainly legitimise

I am indebted to the National Endowment for the Humanities for providing a stipend allowing me to attend Robert Gordon's summer seminar on theory-theory and simulation theory in June and July 1999. Many of the views expressed in this paper have been influenced by discussions with my fellow seminarians and by the fine series of talks organised by Bob Gordon. I am also grateful to the participants of the Ninth Annual Meeting of the European Society for Philosophy and Psychology, 2001 especially Bob Gordon and Alvin Goldman. Finally, I would like to thank my colleagues at Hertfordshire and those who took part in the Leeds Senior Seminar, who commented on an earlier version delivered in February 2002 and those who attended a later version of the paper at the University of Lund in September 2002.

Address for correspondence: Department of Humanities, University of Hertfordshire, de Havilland Campus, Hatfield AL10 9AB.

E-mail: phlqddh@herts.ac.uk

¹ Baker exemplifies the received view when she defines commonsense psychology as being, 'primarily the systems of describing, explaining and predicting human thought and action' (Baker, 1999, p. 3). This claim is found nearly everywhere; for but a few examples see (Bogdan, 1997, p. 105; Botterill, 1996, p. 107; Carruthers, 1996, p. 24.).

the great energy that its friends have put into determining exactly by what means it is conducted; deciding whether its feats are achieved by the deployment of some kind of theory of mind or by a process of simulative imagining.

Yet, in the rush to enter into this debate, philosophers have tended to make a number of ultimately questionable inter-related assumptions about the context in which we engage in commonsense psychology, assumptions that affect our thinking about its very nature. Chief amongst these is that we are normally at a theoretical remove from others. The attitude we adopt towards others is thus on a par with that deployed when understanding 'foreign bodies' quite generally: We ascribe causally efficacious inner mental states to them for the purpose of prediction, explanation and control. As a consequence, this fosters the idea that our initial stance with respect to others is essentially estranged. Bogdan conveniently labels this 'the spectatorial view of interpretation', as it portrays 'the subject as a remote object of observation and prediction' (Bogdan, 1997, p. 104).

This cluster of ideas has been seriously challenged by advocates of second-personal approaches that regard our primary intersubjective interactions as a form of 'embodied practice'.² They maintain that it is simply false to suppose that commonsense psychology explains what underpins our most basic forms of interaction with others. Rather the success in this arena depends on our being embedded in 'normal' contexts, in which we are naturally attuned to their responses and cues. Accordingly:

Understanding others in everyday life does not usually involve either taking a theoretical stance or deploying a simulation routine. It depends instead on a capacity for embodied practice that begins early (and is likely to be partially innate) and continues through normal (non-pathological) experience (Gallagher, 2001, p. 103).

I explicate and defend this work in the first section of this paper. I maintain that a strong case can be made for Gallagher's developmental claim about the primacy of embodied practice as well as for what he calls his 'strong pragmatic' claim that these intersubjective engagements remain in play throughout our normal adult interactions.³ But at best, contrary to the hyperbolic rhetoric of its supporters, if these

² For Gallagher, these include, 'imitation, intentionality detection, eye-tracking, the perception of intentional or goal-related movements, and the perception of meaning and emotion in movement and posture' (Gallagher, 2001, p. 90). Thus he claims that, 'before we are in a position to theorise, simulate, explain or predict mental states in others, we are already in a position to interact with and to understand others in terms of their gestures, intentions and emotions, and in terms of what they see, what they do or pretend to do with objects, and how they act towards ourselves and others' (Gallagher, 2001, p. 91).

³ He writes, 'Primary, embodied intersubjectivity is not primary simply in developmental terms. Rather it remains primary across all face-to-face intersubjective experiences, and subtends the occasional and secondary intersubjective practices of explaining and predicting what other people believe, desire or intend' (Gallagher, 2001, p. 91).

claims were accepted it would only restrict the scope and importance of mentalistic ‘commonsense psychology’.

In the second section, I argue that taking seriously the second-personal starting point ought to provoke us to reconsider the above-mentioned prevailing views about the function and context of much commonsense psychology, even when it comes to its most characteristic activity of providing reason explanations. In abandoning the idea that the contexts in which we make sense of others are normally spectatorial, we can recast and re-orient our thinking about the nature of our expectations about each other and about how such explanations are ordinarily achieved. I expand upon Bruner’s idea that it is through hearing and learning from narratives that norms about what is to be ‘expected’ in particular situations are instilled, and that this is what establishes common expectations that underwrite our ease in understanding one another. Likewise, I defend his claim that the narratives others supply when explaining their actions ‘negotiate’ such aberrations, either by filling in missing details or helping us to appreciate the wider background against which an action took place. In this way, where possible, narratives domesticate the seemingly eccentric, exotic or somehow extraordinary.

Building on this, in the final section, I demonstrate that commonsense psychology involving mentalistic *ascriptions* plays an even more limited role than revealed by the arguments in the first half of the paper. I demonstrate that explaining and predicting actions from a third-personal stance is not only late developing, it is relatively infrequent and far less reliable than our normal intersubjective means of coming to understand others through dialogue and conversation.

2. Everyday Engagements and Primary Intersubjectivity

Against the tradition, Gallagher has recently argued that the way we understand others is ‘primarily neither theoretical nor based on an internal simulation, but is a form of embodied practice’ (Gallagher, 2001, p. 81). He advances the idea that long before we begin to engage in ‘mentalistic’ folk psychology the rudimentary ‘basis for human interaction and for understanding others has already been laid down by certain embodied practices—practices that are emotional, sensory-motor, perceptual and nonconceptual’ (Gallagher, 2001, p. 81).

Gallagher advances this as a developmental claim, the key idea of which is that humans have basic capacities that make them naturally attuned to the expressive responses of others in specific types of situations. These are the sorts of cases in which, for example, we are transformed physiognomically, emotionally or bodily in response to what others do. Thus, we find ourselves, often irresistibly, moved by another’s expression, activity or situation. We can focus on any of these. For example, it is a primitive natural reaction for us to be affected by another’s twinge of pain, embarrassment or shock. Often we are caused to laugh because of someone else’s laughter, not necessarily because we find anything funny. These phenomena

are variously known as emotional or subliminal contagion. The idea that they are developmentally basic is supported by the observation that 'most infants show . . . emotional contagion and even sophisticated empathic behaviour by 18 months' (Nichols *et al.*, 1996, p. 64).

Other instances of embodied engagement include facial and motor mimicry. Consider that neonatal infants show a capacity for facial imitation; even at the tender age of thirty-two hours old (cf. Bermúdez, 1998, p. 126). Consider also that we often find ourselves adopting bodily stances similar to those of others when we take an interest in their projects (cf. Goldman, 1992b, Nichols, *et al.* 1996, p. 63–4). Hence we pull back, lurch forward, flinch or sit uneasily when we see someone doing something frightening, dangerous, painful or unsteady. Anyone who has ever been absorbed in an occurring drama and found themselves reacting to what is happening to its participants (e.g. even to what is happening to a character in a film) will recognise this phenomenon. The ease with which we identify genuine expressions of emotion from mere play-acting suggests we are calibrated for such interaction.

This ability is sometimes called 'mind reading' by behavioural ecologists, but if Gallagher is right this is unfortunate for it suggests an overly intellectualised picture of what underwrites it. Accordingly, he employs the lingo of 'body reading' in order to remind us that our basic interactions with others are such that 'one perceives the emotion in the movement and expression of the other's body' (Gallagher, 2001, p. 90). Consider Bermúdez's way of characterising the problem that infants must solve in order to imitate faces.

Facial imitation involves matching a seen gesture with an unseen gesture, since in normal circumstances one is aware of one's own face only haptically and proprioceptively. If successful facial imitation is to take place, a visual awareness of someone else's face must be apprehended so it can be reproduced on one's own face (Bermúdez, 1998, p. 125).

If we model what is involved in neonatal imitation on the way we might solve this task by means of explicit instructions it would demand the manipulation of a fair bit of knowledge by means of inferential reasoning. But such basic forms of interaction need not be understood as intellectually governed enterprises. It is much more plausible to think that cases of emotional contagion, imitation and motor mimicry are better characterised as instinctive responses to situations or other people, for which our innate systems naturally are calibrated. Such engagements need not involve the application of principles at any level. In particular, they need not require capacities for implicit, sub-personal theorising although it can be taken as read that such responding requires informational sensitivity. Importantly, this requirement can be understood without supposing that 'principles' or 'theories' are deployed. We can think of such 'information' as being bound up with a kind of intentional directedness informed by the biological needs of creatures; an approach I have defended at length elsewhere

(see Hutto, 1999b, ch. 1–4). For example, this passage from Millikan captures this basic idea well:

Neither the beaver nor its relatives need a concept of danger—a way of collecting information that regard just danger, as such, over time—in order to produce appropriate beaver slaps or to respond to them appropriately by diving under. Similarly, an animal’s perception of the spatial layout of its environment for the purposes of moving about in it . . . need not involve any concepts. Being guided by perception of a tree so as to avoid it as you run by does not require a concept of it, not even merely as an obstacle. You need not be collecting information about it, nor about trees or obstacles generally, for future use nor need you be making any inferences concerning these obstacles based on previous experience (Millikan, 2000, p. 199).

Although we should be alive to other relevant differences, if we think of our basic interactions with others as involving dedicated forms of nonconceptual ‘perception’ this approach will apply equally well in the social arena (see Costall, 1995). The key point is that: ‘Perceptual representations that guide immediate action need to be rich in specific kinds of information . . . And because they need to be constructed quickly and reliably, they may be constructed by modular systems that are relatively cognitively impenetrable’ (Millikan, 2000, p. 201). This would explain why creatures whose systems have been groomed to deal with particular environments do so poorly when they play away from home.

Of course, many theory-theorists agree that the systems we deploy in understanding and interacting with others are built-in ‘procedural, innate and modular’ (Bogdan, 1997, p. 104). The difference is that they suppose that *at least at some level* these systems deploy ‘principles’ and operate with ‘concepts’. In the case of ‘folk psychology’, the principles provide a basic schema or framework that outlines how mental states and specifically propositional attitudes interact in the production of actions, as epitomised by the practical syllogism (cf. Hutto, 1999b, p. 12–3, 25–6). For instance, Botterill holds principles such as the following are central to our theory of mind:

[Inference Principle] When an agent A acquires the belief that p and a rational thinker ought to infer q from the conjunction of p with other beliefs that A has, A comes to believe that q (Botterill, 1996, p. 116).

Certainly to engage in acts of imitation and mimicry infants and animals do not need to rely on folk psychological principles detailing how *beliefs* enter into inferential liaisons. Moreover, there are problems with this idea even if we allow that the principles in question only constitute a framework of, ‘general theoretical knowledge—that is the sort of non-content specific knowledge that might very plausibly be held to be innately given’ (Carruthers, 1996, p. 24). First of all, the

core principles make use of rather sophisticated abstract concepts such as: 'agent'; 'rational thinker'; 'belief'; and 'desire'.⁴ To properly support the idea that we operate with a theory at this early stage therefore ultimately requires a worked out theory of content.

But it also demands defence of the claim that *at least some* contents are innate, since these tacit theories of mind must be in place long before children have an explicit grasp of psychological concepts (or indeed long before our ancestors had any explicit concepts at all).⁵ But this means we are owed an account of how this core framework of principles finds itself in place prior to our engaging in sophisticated 'folk psychology'. It is well known that children get better at wielding and understanding psychological concepts as they get older, crucially between ages three and five they normally gain an increasingly mature understanding of beliefs. For those who hold that we have an innate theory of mind, these changes must be explained—or at least explained away. Some, like Segal, hold that the theory of mind itself develops through a series of timed diachronic modular developments. Others, like Fodor, favour the idea that our mature 'folk psychological' theory is in place from the off such that, 'the child's theory of mind undergoes no alteration; what changes is only his *ability to exploit what he knows*' (Fodor, 1995, p. 110). Accordingly, young children use only some of the theory's basic principles, effectively operating with a 'very simple theory of mind'.⁶

Either way, we are asked to accept that we inherit our fully developed theory of mind (though not an immediate competence in using it) or a simpler version which has a built-in capacity for augmentation, from our evolutionary forefathers. We are told:

It is not implausible to suppose that the early theories of mind—the two-year-old's, three-year-old's and so on—are a *hangover from phylogenetically prior*

⁴ Recognising this Segal holds that, 'The psychological faculty certainly appears to be an intentional module. The faculty has a definite and self-contained *body of knowledge* that is framed in terms of a specific *network of interrelated concepts*' (Segal, 1996, p. 147, emphasis mine).

⁵ Jane Heal has raised the worry that a strong version of theory-theory would require a complete theory of relevance or an extensive theory of concepts to deal with all the legitimate inferences that might be made in relation to possible subject matters that others might be thinking of. But if this were set out in the form of conceptual knowledge then we would need to know practically everything in order to be able to use theory-theory. As Clark notes, 'a little reflection suggests that there would be no obvious end to the "common-sense" knowledge we would have to write down to capture all that an adult human knows' (Clark, 1997, p. 6). Moreover even if we countenance the existence of such a vast and all-encompassing theory there would be further questions about how we could wield it sensitively in real time circumstances.

⁶ Fodor writes, 'Adults can afford to insist on better warranted predictions — ones that take into account more of what the predictor knows about the predictee's cognitive circumstances. But the price of warrant is computational complexity and since, as a matter of fact [the heuristic of simply predicting that the other will act to satisfy their desires] is reliable most of the time, the advantages of growing up, though no doubt real, might be easy to overestimate' (Fodor, 1995, p. 118).

stages . . . The false representations of, say, the three-year-old are a product of a *relatively primitive system that was present in the species some while ago*. The system was, indeed, good enough for survival (Segal, 1996, p. 156, emphases mine).

The thought is that despite their crudity, these primitive modes of response were reliable enough to enable our ancestors to deal with each other. This assumes that their motives were relatively unsophisticated in that they could be inferred more or less directly from what they were doing. This in turn suggests that the contexts in which they were operating were stable. However these observations are double-edged. For they raise the question: why would young children or our remote ancestors have had any need for core principles constituting a schema outlining the interaction of full representational beliefs and desires in the first place, if the kind of cases with which they had to deal were of a much simpler nature? If less than the whole of our mature 'folk psychological' theory of mind was in fact *good enough* for our ancestors, what possible pressures could have driven its early development? Why would nature have developed something that would have lain presumably dormant or latent within them? Without a pressing need, at best this would have been an evolutionary extravagance.

Worse still, as with the simpler cases of imitation and motor mimicry, the very fact that our native capacity to see that another desires something is so basic and systematic is what drives us to think that 'the psychological faculty is a computational module' (Segal, 1996, p. 149). But this should also lead us to doubt that we need to make any inferences or attributions in those simple sorts of cases in which we can read another's basic desires and goals straight off their reactions to some state of the world. Seeing what another desires is particularly easy for us when there is no conflict between their perspective and ours. Indeed, I am suggesting that as children we begin our interactive careers with others lacking the resources to recognise alternative perspectives. Coming to see that others may view things differently is a relatively late development, as 'false belief' experiments and other findings help to show (see Hutto, 2003).

Alternatively, I promote the idea that in the basic cases we are able to 'read' others reliably and *vice versa* and that when we are in our historically normal environments this is no accident. For, like all creatures, due to long periods of tinkering and adjustment, we have been shaped precisely to respond to such environments, be they biological or social.⁷ Taking this idea to heart makes the alternative claim that our basic social interactions are made possible by means of the tacit predictions and explanations of commonsense psychology deeply suspect.

That no 'theories' or 'principles' are involved in our basic social interactions and engagements bears some *prima facie* resemblance to the sort of view advanced by supporters of simulation. But there are crucial differences between what is being proposed here and the standard rendering of what simulation involves. For example, Gopnik and Wellman are typical of many in likening the latter process to the

⁷ Clark captures the spirit of the approach when he encourages us to adopt the motto, 'Ask not what's inside your head, but what your head's inside of' (see Clark, 1989, p. 66).

manipulation of small-scale models, as is standard practice in planetaria (cf. Gopnik and Wellman, 1992, p. 159, see also Baron-Cohen and Cross, 1992; Perner, 1996, p. 90). Their thought is that when, say, we use a planetarium for predictive purposes, we put the model through a number of counterfactual situations. We might move our mock sun, earth and moon so as to simulate an eclipse. In using such a device it is not necessary to understand *why* the modelled entities behave as they do. Nevertheless, such a lack of knowledge does not necessarily impair our ability to predict how they will behave under particular conditions. Thus if the planetarium is a good one, say if God built it for us, then despite our lack of detailed understanding of cosmology, we could still use it as an accurate guide to the potential behaviour of the planets.⁸

It is with this in mind that many describe the process of simulation as one of placing ourselves, figuratively speaking, in the shoes of the other and predicting or explaining their actions on the basis of that projection. Goldman, who is credited with being a father of the approach, also holds that when simulating we first identify, by means of introspection, which mental states we would have in such-and-such a circumstance before projecting them onto the target individual. What normally justifies such projections is the relatively safe assumption that others are generally like us in most respects. Yet, as Goldman is aware, since the simulator and their target are probably not exactly psychologically alike, decent predictions normally require feeding pretend inputs into the relevant psychological mechanisms. These are then run off-line, resulting in inferences, predictions or explanations, as opposed to actions or responses.⁹ Vitally, even noting these caveats, simulation always requires at least implicit appeal to 'a general premise stating that the model is relevantly similar to the [thing modelled]' (Fuller, 1995, p. 22).

But it has been argued that this last requirement threatens to collapse simulation into a form of theorising. For example, Jackson maintains that *any* appeal to knowledge or background assumptions of *any* sort constitutes 'operating with a theory', making the truth of theory-theory 'near enough analytic' (Jackson, 1999, p. 80). Currie and Ravenscroft underline this further, noting that:

'Simulation' as it is currently used, is ambiguous; it has a narrower and a broader meaning. Suppose I try to predict your behaviour by imagining myself in your situation. There are three things that must go on if I am to get the answer by simulation. The first is to acquire knowledge, or at least some beliefs, about your situation. The second thing is for me to place myself,

⁸ In this way simulation 'bypasses conceptual understanding by operating a working model of the mind and reading output. Fortunately, [the simulator] has such a model easily available, as all humans do, namely [their] own mind' (Gopnik and Wellman, 1992, p. 145). As this last remark makes clear, the introspective modelling proposal rests on the controversial premise that our own minds are transparent and easily known.

⁹ A major attraction of this approach lies in its cognitive economy. One and the same mechanism is postulated in order to explain: (1) how we deliberate and generate actions; (2) how we consider possible actions in counterfactual situations; and (3) how we manage to predict and explain the actions of others.

in imagination, in that situation and to see, what, in imagination, I decide. The third is to draw a conclusion from this about what you will do. Sometimes 'simulation' refers to the whole three-tier process, sometimes just to the bit in the middle (Currie and Ravenscroft, 2003, p. 54).

Insisting that simulation must be understood in its broader sense, they agree with Jackson that, 'simulations never work without assistance from theory' (Currie and Ravenscroft, 2003, p. 54).

But this is not the only way to understand what simulation involves. For example, Robert Gordon explicitly rejects the idea that simulation should be understood in terms of some kind of introspective modelling; persistently stressing that simulation is not a process of transportation but rather one of transformation (Gordon, 1995; Gordon, 1996, p. 12). The flavour of his alternative proposal is captured in the following quotation:

In seeking an explanation of your friend's action, you were looking for features of the environment (features you believed it to possess) that were menacing, frightening, attractive, and the like. This is not a matter of looking dispassionately for features believed to produce certain characteristic actions or emotions. Rather, it is a search that essentially *engages your own practical and emotional responses* (Gordon, 1992, p. 15, emphasis mine).

He regards this as a hot methodology precisely because the imaginative transformations it involves 'exploit one's own motivational and emotional resources' (Gordon, 1996, p. 11 cf. Hutto, 2000, ch. 1). Moreover, they operate 'primarily at the sub-verbal level' (Gordon, 1986, p. 170). Crucially, such transformations do not involve, as he puts it, any 'inference from me to you'. More would need to be said about how this idea would play out when it comes to understanding others by the lights of a mature folk psychology, but clearly in simpler cases of responding it gets the direction of affection the right way around, unlike projective varieties of simulation. When mimicking or imitating others we mirror the *other*, we do not project ourselves onto them. Thus, when engaging others in these kinds of social interactions it is not necessary for to us make any assumptions about our similarity to them, implicit or otherwise.¹⁰ McVeer gets it just right when she remarks, 'This attunement does not depend on putting ourselves in others' shoes. We are already in their shoes. We are already in their shoes, as they are in ours'

¹⁰ This fits with Wittgenstein's reminder that it is as much a primitive reaction to attend to the wounds of others as to attend to our own (cf. Wittgenstein, 1967, §540). For Wittgenstein this is not because we first neutrally observe their pain behaviour and on this basis make a judgement about their inner state by means of reasoning or by analogy. We react because we see that they are in pain and it is natural for us to be moved thus and so by this. To think that we respond to others on the basis of inference and analogy is, as he suggests, 'putting the cart before the horse' (Wittgenstein, 1967, §542).

(McVeer, 2001 p. 121).¹¹ In proposing this radical form of simulation, Gordon was an early champion of the idea that intersubjective engagements are primarily a form of 'embodied practice' because he also rejected the thought that our primary relations take place against the backdrop of strong first/third person divide.

This idea has respectable empirical backing, if we consider afresh the importance of Gallese and Goldman's findings concerning the discovery of 'mirror neurons' in macaque monkeys. There is a specific type of neuron which fires when one monkey observes the actions of another. But what is fascinating is that these are the same neurons that fire when the monkey itself performs such actions. In the words of Gallese and Goldman:

[Mirror neurons] respond both when a particular action is performed by the recorded monkey and when the same action performed by another individual is observed. All MNs, as mentioned above, discharge during specific goal-related motor acts. Grasping, manipulating and holding objects are by far the most effective actions triggering their motor response... [and] an equivalent system has also been demonstrated in humans (Gallese and Goldman, 1998, p. 495).

Gallese and Goldman regard this as a 'primitive version' or 'precursor' to simulation, but this implies that, at best, it underpins the more basic types of intersubjective engagement common to many animals, particularly primates; calibrating them for shared interactions with a common world.

The moral is that we should not assume that the processes involved in basic acts of recognition, even intersubjective ones, tacitly mimic those of mature reasoners who would tackle the same problem using a set of abstract concepts and general principles so as to make explicit inferences. We are systematically misled on this score because in the very act of classifying such behaviour we must employ our own conceptual scheme of reference. But it is nothing more than an intellectual bias to suppose that, for example, young children or animals must be tacitly employing it. On these grounds, we ought to question whether *any* tacit principles—especially those of a 'very simple theory of mind'—underwrite routine forms of social responding. Taking this line is proof against Jackson's claim that theory-theorists are entitled to an automatic victory over all simulationists, even if we understand 'theory' in the most anaemic way to mean only the 'making of an implicit assumption'. For that argument only works if we accept that we always start from a detached point of view in our dealings with others.

Still, none of this need worry defenders of mentalistic folk psychology overly much. It is still possible to maintain that spectatorial folk psychology tells the central story about how we understand others in sophisticated cases even if it is

¹¹ Autistics lack just this sort of 'insider's view', whereas the normal tendency for infants, as it was for our adult ancestors, is to see agency everywhere, even in objects and things that lack it (McVeer, 2001, p. 231).

conceded that: (i) it does not make possible basic forms of interaction; (ii) it is developmentally reliant on more basic modes of interaction; and (iii) our capacity to make mentalistic attributions works alongside and is continually supported by these basic modes of social interaction. At worst, to accept this would imply that the scope of mentalistic commonsense psychology is more restricted than the rhetoric of its promoters has hitherto implied.

In the next section I take the basic insight that we start from the ‘second person’ a step further, arguing that the cases in which we ‘predict’ and ‘explain’ the actions of others from a third personal perspective are at best peripheral, even when we are on commonsense psychology’s home turf of mature reason explanation.

3. Expectations and Explanations in Second-Person Contexts

I advance what might at first seem a radical claim. It is that even in understanding the reasons for which others act, including adults, we often do not make any attribution of beliefs and desires *at all*. However, the reason for this is utterly banal: we simply do not need to make such ascriptions in most everyday, second-personal contexts. An ordinary example from adult life will hopefully serve to illustrate this.

Imagine that you see a man approaching the closed door of a shop while struggling with bags of groceries. We would hardly be surprised to see him put these down in order to open it or to see him or wait until someone came to his aid. Ought we suppose that our lack of surprise indicates that we were predicting, albeit tacitly, that this man might do either of these things?¹² If we build on the thinking of the first section of this paper, we might suppose that we would not need to precisely because we already know what to expect from others and they know what to expect from us in familiar social circumstances. This obviates the need to employ any mediating knowledge or theoretical principles. The thought is:

If we make ourselves more readable to one another by conforming to shared norms of readability, it follows that much of the work of understanding one

¹² Gallagher is right to stress that, ‘a more basic question is whether our ordinary attempts to understand other people are best characterized as explanations and predictions’ (Gallagher, 2001, p. 96). For one thing, it is hardly plausible that we could take an abstract interest in the movements of all those we encounter, for to do so would surely sap our intellectual resources. It is much more likely we only tend to track those actions of others that have some potentially direct impact on us, since doing so is vital if we are to successfully co-ordinate our actions (cf. Morton, 2003, ch.1). Compare this with Wittgenstein’s astute remarks about ‘recognising’. He writes: ‘Asked ‘Did you recognise your desk when you entered your room this morning?’ – I should no doubt say ‘Certainly!’ And yet it would be misleading to say that an act of recognition had taken place. Of course the desk was not strange to me; I was not surprised to see it, as I should have been if another one had been standing there, or some unfamiliar kind of object’ (Wittgenstein, 1953, §602). He adds shortly afterwards: ‘It is easy to have a false picture of the processes called “recognising” (Wittgenstein, 1953, §604).

another in day-to-day interactions is not really done by us at all, explicitly or implicitly. The work is done and carried by the world, embedded in the norms and routines that structure such interactions (McVeer, 2001, p. 119).

In McVeer's words, this is how we gain our insight into 'the insider's view'—through training—not by relying on a set of innate principles or absorbing any explicit ones.¹³ By being brought into 'the fold'—learning what to expect of others and *vice versa*—is what it is to gain a second nature or a common sense.¹⁴ With this observation in hand, we can begin to see even our quite sophisticated practices of giving and understanding reasons in a new light.

Specifically, I want to consider Bruner's claim that narratives play two vital roles in commonsense psychology. The first is to shape our expectations by making us familiar with a vast stock and wide range of 'ordinary' situations and the sorts of actions normally related to them. This enables us to judge the appropriateness of reasons for acting. Thus Bruner claims of the 'canonical status of folk psychology' that 'it summarizes not simply *how things are* but (often implicitly) *how they should be*' (Bruner, 1990, p. 40).

Like more direct forms of training, much story-telling instils and inculcates values in children. Narratives impart norms, providing a platform from which we judge reasons and actions to be acceptable or otherwise. In the process of listening to stories, real or fictional, we learn what others will expect from us and, importantly, what we ought to expect from them. It is thus central to moral education and the development of ethical points of view (see Morton, 2003, ch.1 and 2). Thus, in learning commonsense psychology through narratives we develop a properly common sense of what is 'obvious' and 'significant'. In this way 'folk psychology' is an instrument of culture, giving us the grounds for our evaluative expectations about what constitutes good reasons. This is not the same as merely providing a framework for disinterested prediction and explanation.

Obviously, this idea has the potential to dispel the long-standing worries about how we first acquire our commonsense psychology. For while it is true that 'very few children have carers who utter [folk psychological] platitudes' (Goldman, 1992a, p. 107), nearly all children have mothers who tell them stories; unlike

¹³ I have defended an account of nonconceptual content and connectionist processing in chapters 3 and 4 of *The Presence of Mind*, which would suit this style of approach (cf. Hutto, 1999b). Henderson and Horgan also develop an explanation of the relative reliability of simulation along these lines (cf. Henderson and Horgan, 2000).

¹⁴ According to the Oxford English Dictionary, common sense is defined as: 'normal understanding, good practical sense in everyday affairs, general feeling of mankind or community'. This is apposite in that it resonates well with the term's etymology, as Taylor relates: 'Aristotle's language about 'common sensibles' is, of course, the source of our expression 'common sense', which, however, has an entirely different meaning. The shifting of sense has apparently been effected through Cicero's employment of the phrase *sensus communis* to mean tactful sympathy, the feeling of fellowship with our kind on which the Stoic philosophers laid so much stress' (Taylor, 1955, p. 83–4).

theories, stories are something we give to our children from an early age onward. As Bruner remarks, 'We learn our culture's folk psychology early, learn it as we learn to use the very language we acquire and to conduct the interpersonal transaction required in communal life' (Bruner, 1990, p. 35). In all, he holds:

Folk psychology is invested in canonicity. It focuses upon the expectable and/or the usual in the human condition. It endows these with legitimacy or authority. Yet it has powerful means that are purpose-built for rendering the exceptional and the unusual into comprehensible form (Bruner, 1990, p. 47).

This last sentence underlines the second role narratives can play. They can also serve to mediate in cases in which another deviates from our expectations by helping us to tame the extraordinary. Bruner recognises that narrative 'specializes in the forgoing of links between the exceptional and the ordinary' (Bruner, 1990, p. 47). A narrative can serve as explanation by smoothing our 'understanding' of others in the cases where their actions or accounts do not make sense.

While a culture must contain a set of norms, it must also contain a set of interpretative procedures for rendering departures from those norms meaningful in terms of established patterns... It is narrative and narrative interpretation upon which folk psychology depends for achieving this kind of meaning (Bruner, 1990, p. 47).

Crucially, such explanations are *only* needed in the sorts of cases in which we are surprised or perplexed by another's actions. For, 'When things 'are as they should be', the narratives of folk psychology are unnecessary' (Bruner, 1990, p. 40). With this in mind, we might reconsider the implications of Fodor's claim that commonsense psychology 'works so well it disappears' (Fodor, 1987, p. 3).

Narratives function as 'normalising' explanations, allowing us to cope with 'unusual' or 'eccentric' actions either by helping us to see them as familiar or by *making* them so. This is achieved either by supplying missing details that reveal an action to be in the fold of the ordinary already—despite appearances—or by fleshing out a larger context such that we come to find it acceptable. This use usually entails that we extend the range of what we think of as falling within the scope of the 'normal'. But it goes without saying that this sort of 'negotiation' requires a prior fluency with 'the normal'. As Bruner puts it in such cases, 'the function of the story to find an intentional state that mitigates or at least makes comprehensible a deviation from a canonical cultural pattern' (Bruner, 1990 p. 49–50, entire sentence emphasised in original).

Of course, this is not always possible. Sometimes the 'behaviour' of others is so erratic that we have no option but to regard them in the same light as we do 'objects'. Stich, who once observed that folk psychology is best regarded as a kind of domestic anthropology, provided us with a plethora of such cases involving exotic subjects, such as children, animals and confused or demented folk in which

it necessarily fails (see Stich, 1983, p. 163). Faced with these subjects we may have to resort to the postulation of theoretical inner states, but these will not be of the mentalistic variety. But it must be stressed that this only occurs when our normal way of understanding others breaks down and no mediating narratives can be brought to bear or are of any use (cf, Gallagher, 2001, p. 95).

All of this leads Bruner to the bold and powerful idea that the 'organizing principle of folk psychology' is 'narrative rather than conceptual' (Bruner, 1990, p. 35). To take this seriously would upset the current orthodoxy, endorsed by theory-theorists and simulationists alike, which maintain that in 'making sense of others' we attribute causally efficacious mental states to them in order to 'rationalise' their behaviour and that this requires a general schema or framework.

This flies in the face of the prevalent view that reason explanations are a sub-species of theoretical explanation, the logic of which is structurally identical to the kind found in explanations throughout the natural sciences. Fodor speaks for this tradition when he tells us that when folk psychological explanations are made explicit:

They are frequently seen to exhibit the 'deductive structure' that is so characteristic of explanation in real science. There are two parts to this: the theory's underlying generalizations are defined over unobservables, and they lead to its predictions by iterating and interacting rather by being directly instantiated (Fodor, 1987, p. 7).

This is to adopt a broadly Hempelian approach, according to which the explanation of a particular action requires that we subsume it under a general law that reveals the relation between the events in question. It is a feature of this way of thinking that, despite facing in different directions, predictions and explanations are regarded as having the same structure. Ideally, a reliable theory that is based on information about past cases, known regularities and re-occurring patterns enables us to work backwards from known effects to the causes of specific happenings, in just the way that we work forward from known, or presupposed, causes in order to predict future effects.¹⁵

As long as we believe that our basic relation to others is a detached one this idea will seem almost irresistible. In the abstract, how else ought we to characterise and, indeed, rank the assumptions needed for predicting and explaining such 'mental events' as the occurrence of specific thoughts and desires? How else would someone who had no knowledge of others get by?

¹⁵ It is hardly surprising that philosophers of mind schooled in a neo-positivistic views of science often mix talk of prediction and explanation interchangeably. For example, in making a case for the pervasiveness and indispensability of commonsense psychology, Fodor long ago argued that, 'the burden of predicting behaviour... is routinely taken up by theory' (Fodor, 1987, p. 3). Yet, within the space of a few pages he switches without warning from talk of 'predictive adequacy' to talk of 'explanation'.

However, a little reflection shows that everyday practical explanations should not be modelled on explanations in the purely theoretical, abstract sciences. Firstly, as is well known, not only would ‘folk psychology’ have its own unique set of laws or generalizations, it must invoke the slippery notion of ‘rationality’ which sets them apart from all other theoretical sciences. Commonsense psychological explanations come with built-in reference to rational agents, such that its laws take the form: ‘In a situation of type C, any rational agent will do x’ (Kögler and Stueber, 2000, p. 13–5).¹⁶ The need to recognise rationality (or as Fodor put it more cautiously, non-irrationality) is important, for if a person is irrational then he or she falls outside the scope of our folk theory of mind.¹⁷

Secondly, as theories only ever operate with general, abstract information they are fundamentally incapable of providing the sort of explanations needed in applied or forensic sciences (and in other areas such as history and psychoanalysis). Explanations in applied domains are always more than mere chronicles of what has happened during a particular time frame. They involve discriminating and selecting which specific event—under a particular description—is the important one for the purposes of explanation. They all:

- i. Select the appropriate events;
- ii. Order them within a temporal series;
- iii. Isolate their relevant properties with a view to making them intelligible within a particular idiom.

It is with this in mind that we should regard such explanations as having a narrative form. For, as Roth observes:

Narratives give [events] a connection which is not merely chronological. The process of presenting a narrative about one’s past [or the historical past] requires identifying which events are significant and why (Roth, 1991, p. 178).

¹⁶ Proponents of the ‘rationality’ view are notoriously bad at spelling out exactly what ‘rationality’ is supposed to mean. For example, Dennett writes, ‘the concept of rationality is systematically pre-theoretical... When one leans on our pre-theoretical concept of rationality, one relies on our shared intuitions’ (Dennett, 1987, p. 98). In this respect, the notion of an ‘ideal rational agent’ could be replaced by something like ‘any typical person’, as Perner suggested (Perner, 1996, p. 92, cf. also Hempel, 1966 p. 472; Arkway, 2000, p. 118–120).

¹⁷ Nevertheless, too much weight is sometimes placed on the need to presuppose rationality in our explanations of others (see Fodor, 1981, p. 109; Dennett, 1985, p. 16–22, 1987, ch. 4; Stich, 1990, ch 2–3). For example, it is a great advantage of Gordon’s approach that it, ‘predicts—correctly—that we will sometimes attribute irrationality to others, something that would not be readily predicted by... the [strict] “rational norms” approach’ (Gordon, 1992, p. 15).

This requirement is common to all singular causal explanations. They too are contextual in a way that distinguishes them from purely theoretical 'explanations'. Consequently, what constitutes a sound abstract explanation of the general causes of carburetor failure differs altogether from what is required from a singular causal explanation that identifies why a particular carburetor actually failed on a specific occasion. Although general theories are no doubt useful in framing such specific investigations, it is clear that even in the natural sciences the two must not be conflated.

In this light, we would be well-advised to adopt Woodward's approach to singular causal explanations, accepting that they constitute 'a distinct genre of explanation, which does not possess anything remotely like a covering-law structure' (Woodward, 1984, p. 232). He maintains that what they seek to explain 'is simply the occurrence of a particular event . . . rather than some more complicated *explanandum*' (Woodward, 1984, p. 232).¹⁸ He argues that at best the covering-law model is appropriate for all-inclusive, purely theoretical forms of prediction and explanation. He contrasts these with contextual explanations in which we deliberately avoid mentioning certain features in tune with the context of inquiry, since these can be taken for granted or regarded as 'idle' or 'irrelevant'. This last point is worth noting, for if everyday explanations function by supplying *relevant* information, then exactly which details will be significant will vary from context to context.

This fits well with the fact that even in cases in which we explain why someone acted in terms of their beliefs and desires, we tend to highlight the relevant belief or desire, for only these are explanatorily significant. In maintaining saliency, we do not mention the partner desires and beliefs nor do we attempt to fill in *all* the background details. Normally, these are simply taken for granted. Thus, if asked why Ms. Y was late for the 3 o'clock meeting we might report that she believed that it was to be at 4 o'clock. To make best sense of her tardiness there is simply no need to say that she must also have desired to attend it (at whatever time she thought it was scheduled). We do not need to say that she also did not hold any contradictory beliefs such as that the meeting was at 5, 6 or 7 o'clock. Nor do we have to mention other beliefs, such as that she also believes that attendance at the meeting requires her physical presence, even though these might be salient in another context. The fact is that we only make certain aspects of the other's reasoning explicit on a need-to-know basis. Of course, we would assent to these attributions about Ms. Y's psychological set if pressed, in the way Socrates teases mathematical principles out of the slave boy in the *Meno*, but this is not a compelling reason to think that we must somehow be tacitly aware of all these possibilities at some level when we give an explanation of her lateness.

¹⁸ Following Dretske's lead, he regards their structure as essentially contrastive in nature, in that they answer what-if-things-had-been-different questions. By contrast, theoretical explanations are designed to answer a whole range of what-if-things-had-been-different questions at once.

Although practical reasoning *de facto* implies a certain structure—which can be made explicit to varying degrees for certain purposes—we cannot assume that, as a default, ordinary practitioners of commonsense psychology must ‘have some sort of awareness of the principles involved’ (Botterill, 1996, p. 114). In many cases, what is being made explicit is just the structure of practical reasoning itself, not the contents of our minds. Such background details regularly *go without saying*. I hold it is just as plausible that they *go without thinking*. Although we can abstractly reconstruct what has to be the case if our physical and psychological explanations are to work, there is no justification for reading all of this back into the minds of those doing such explaining. If we did, where exactly would one stop? In ordinary contexts in which we can reliably take most things for granted, good explanations stop precisely when enough has been said to make an action intelligible. The context determines just *how much* of this structure and the peculiar character of the psychology of target individuals we will need to reveal in order to make sense of their actions or to fine-tune our predictions. We are normally only interested in tips, not whole icebergs.

For example, it is often good enough to explain someone’s action by simply mentioning their emotional condition. Goldie labels such cases as those involving ‘action out of emotion’. For example, I might explain my unusually meagre or lacklustre attempts at conversation on a certain day by admitting that I am in a sullen mood. As he puts it:

An emotional thought or feeling is primitively intelligible if it cannot be better explained by anything else other than the emotion of which it is a part; thus, Jane’s desire to get her own back on Jim is primitively intelligible—it is just the sort of desire which angry people paradigmatically have (Goldie, 2000, p. 43).¹⁹

A similar idea is plausible for all sorts of everyday explanations if we accept that practical explanations are not designed to make an occurrence *fully* intelligible, as if we were always explaining what was happening to a complete outsider or newcomer to our world or practices: one who lacked *all* information. This turns the standard picture on its head. It is only the citing of *relevant* details in a given context that does the *explaining*. Far from being supplementary and peripheral in everyday contexts our narratives turn out to be the *explanans*—unlike the presupposed core framework.

¹⁹ Freeing up our understanding of the nature of such explanations pays other dividends. It is a nice feature of this sort of account that we can make sense of a person’s reasons for action in terms of their emotions, obviating the need to determine whether these reasons are ‘rational’ or not (see Goldie, 2000, p. 3). This would directly challenge the idea that ‘belief-desire reasoning forms the core of commonsense psychology’ (Baker, 1999, p. 3). If Goldie is correct commonsense psychology is not only richer than is generally supposed, but beliefs and desires are not at its heart.

This is the crux, for the traditional picture is only attractive if we assume that in giving explanations we always occupy an estranged, spectatorial point of view. Yet, in ordinary cases the other is not at arm's length. For this reason the *standard* way we come to determine the reasons for which others act is dramatically unlike that employed in forensic investigations that seek to locate the cause of particular events. We cannot use the same sorts of methods we would deploy in determining, say, the cause of a plane crash. Rather we usually rely on the revelations of others: They explain their actions for themselves. Of course, their admissions are defeasible and often people are self-deceived about their reasons for acting. But we have fairly robust methods for testing, questioning and challenging such claims when it is important to do so, as in legal cases. We compare one person's avowals with the accounts of others, uncovering lies or internal contradictions that will invalidate either their testimony or their credibility. Countless everyday conversations involving the explanation of actions in terms of reasons mimic this process to a greater or lesser degree. Also, as Gallagher stressed, when directly engaging with others in 'normal' contexts there are a wealth of cues, expressions, and responses upon which to guide the feedback we give to others.

4. The Limits of Spectatorial 'Folk Psychology'

Understanding others in our normal contexts of interaction is not a spectator sport. This is not to claim that we never adopt such a stance, only doing so is the exception, not the rule. As Gallagher observes:

Even in cases where we know (or think we know) a person very well, we may express puzzlement about their behaviour. In discussing a friend's behaviour with someone who doesn't know her well, we may come to devise a theory about why she is acting in a certain way. It seems very possible to describe such cases in terms of a theory of mind (Gallagher, 2001, p. 92).

He also notes, 'In the situation of talking with someone else about a third person, it seems possible to describe our attitude toward the person under discussion as theoretical or as involving a simulation of the other person's mental states' (Gallagher, 2001, p. 93). Still these heuristics remain at the periphery, coming into play only in special cases, such as when we do not accept another's account of their reasons. Driven by suspicion we may be left with nothing but speculation and supposition about their motives. That is to say, we may be forced to make predictions and explanations of actions precisely in the sorts of cases in which we do not know what to expect from others or when we cannot engage them directly. But for this very reason these sorts of approach are bound to be, on the whole, much less reliable than our second-person modes of interaction.

Consider that most third-person predictions operate with framing information about the other's background beliefs and desires already in hand, as in the following paradigm case:

[S]uppose I wish to predict what John will think of the new jacket; will he think it garish? Suppose further that I know that John believes the jacket to be scarlet and he thinks all bright colours to be garish. I will, of course, expect him to think the jacket garish (Heal, 1995, p. 39).²⁰

Unlike other simulationists, Heal regards the scope of folk psychology quite modestly restricted to predictions of the above sort. Thus whereas Gordon and Goldman frequently speak of applying simulation to obtain both predictions and explanations, she holds that ‘forward-moving theory-invoking prediction is quite a different matter from backward-looking theory-invoking explanation’ (Heal, 1998a, p. 87; see Gordon, 1986, pp. 158, 163–4, 1992, p. 12).²¹ Indeed, in insisting on this distinction, she claims that neither theory-theory nor its chief rival are in the business of accounting for how we go about, ‘interpreting and explaining behaviour’ (Heal, 1998a, p. 86).

²⁰ Heal’s co-cognition variant of the simulation theory only seeks to illuminate how we come to understand and predict the thoughts of others in cases of just this kind. Specifically, those in which the object is to determine: (a) which thoughts will likely ensue from the other’s confrontation with their environmental circumstances, (b) which future thoughts will likely ensue from the other’s current thoughts, and (c) which bodily movements will likely ensue from the other’s current thoughts. In such circumstances, she claims that we manage by replicating the target’s thoughts in ourselves and by observing the outcome (cf. Heal, 1998b, p. 491). This process of co-cognition is one of, ‘harness[ing] our own cognitive apparatus and mak[ing] it work in parallel with that of the other’ (Heal, 1998a, p. 85). Accordingly, conclusions about what another is thinking can be justified either by appeal to analogy or by assuming that they have a minimal competence in dealing with aspects of a common world. Given this, Heal does not construe the process as involving pretend ‘beliefs’, as opposed to seriously held beliefs entertained in a hypothetical context (Heal, 2000, p. 12). She writes, ‘Given the assumption of such very minimal rationality, we can show why reliance on co-cognition is a sensible way to proceed in trying to grasp where another’s reflections may lead. The other thinks that $p_1 - p_n$ and is wondering whether q . I would like to know what will she conclude. So I ask myself “Would the obtaining of $p_1 - p_n$ necessitate or make likely the obtaining of q ?” To answer this question I must myself think about the states of affairs in question, as the other is also doing, i.e. I must co-cognise with the other. If I come to the answer that a state of affairs in which $p_1 - p_n$ would necessitate or make likely that q , then I shall expect the other to arrive at the belief that q ’ (Heal, 1998b, p. 487).

²¹ Goldman openly claims that ‘The simulation procedure can also be used for explanatory, or ‘retrodictive’ assignment of mental states’ (Goldman, 1989, pp. 169, 170). Whereas Gordon has emphasised that his version of simulation is meant to explain how we provide reason explanations that ‘perform an explanatory function beyond merely portraying the action as reasonable from the agent’s point of view. They surely do answer “Why?” questions, and they may have the form “A X’d because p”, where “p” states a reason for A’s X’ing’ (Gordon, 2000, p. 63). This undermines Arkway’s demand that if the simulationists are to challenge theory-theory meaningfully they must, ‘claim to account for the same sort of explanation-giving that the theory-theory claims to account for, [namely, causal explanations]’ (Arkway, 2000, p. 129). For she doubts whether they really have such ambitions on the grounds that she claims they do ‘not say whether correct simulation-produced explanations explain in the same robust sense that scientific explanations do’ (Arkway, 2000, p. 120).

But if this were so, commonsense psychology would either be a very dull business or a very uncertain one indeed. For the cost of having reliable predictions is inversely proportional to the need to make them at all. To come back to Bruner's point about our expectations, if a situation is familiar there will be little need to make any predictions about what another will do. In contrast, in third person cases, we are faced with two extremes. At one pole, if we already have framing information that is good enough for us to deduce logically what another will think or do it is unclear what need such predictions might serve. At the other pole, to the extent that we lack such information the need to make predictions will be clearer but they will be less reliable.

Consider how folk psychological predictions are allegedly achieved on Goldman's account of the simulation process. In taking my practical reasoning mechanism off-line, I shelve certain of my beliefs and desires and allow others—which perhaps I don't genuinely harbour—to go 'live'. In order to get any interesting results, I need to make a number of such fine tuned adjustments. Clearly, the more I know about my target the better my chances of successful prediction.

Precisely the same challenge attends the generation of retrodictive explanations from a third-personal perspective. If X is to simulate Y, X cannot simply imagine what it would be like to be in Y's position, they must take into account the relevant differences between the simulator and their target. Thus in his very first paper on this topic Gordon stressed that simulation:

...is not the same as deciding *what I myself would do*. One tries to make *adjustments for relevant differences*. In chess, for example, a player would make not only the imaginative shifts required for predicting 'what I would do in his shoes', but the further shifts required for predicting what *he* will do in his shoes. To this purpose the player might, e.g. simulate a lower level of play, trade one set of idiosyncrasies for another, and above all pretend ignorance of his *own* (actual) intentions. Army generals, sales-people, and detectives claim to do this sort of thing (Gordon, 1986, p. 162, emphases mine).

The point is that I look not at what I would do as the other but at the situation *as it might be for him*. But, if I were in a position to determine in what respects the other's perspective is relevantly different from mine this would obviate the need for the explanation. Yet without such information the simulation procedure would leave us with nothing more than *possible* explanations. This reminds us that unless this activity is constrained in some further way it is unlikely to yield good results. There simply are too many possibilities about what someone might think or do in any 'situation', all within the rational spectrum. One is faced with an embarrassment of riches. At best we are left with *mere* hypotheses about what the other will do. Once again, unless we already know (or can safely assume) enough about the other's background beliefs, desires and psychological attitudes there is a good

chance that our simulated predictions will go astray. The problem is that, unlike ordinary conversations, on its own the simulation heuristic has very meagre resources for determining *which* psychological states to put aside and which to keep in play.

The point can be illustrated graphically if we examine Goldman's schematic, as presented in Figure 1 (adapted from Nichols *et al.*, 1996, p. 40). Although an appropriate box is set aside to perform the function of generating pretend propositional attitudes, it is interesting that it alone stands free of input. Since everything is clearly labelled, it is easy to see what is left crucially unexplained: the means by which particular beliefs and desires are selected to act as pretend inputs.

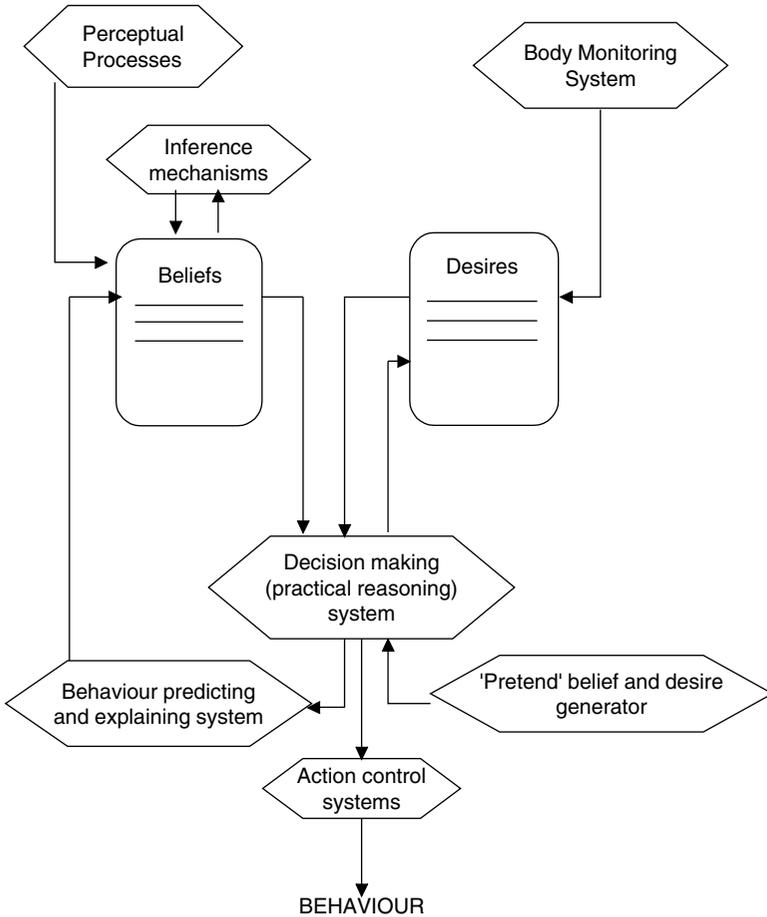


Figure 1 *Practical Reasoning Mechanism*

These criticisms of simulation theory are well rehearsed and they have led some to hold that when interpretation gets tough, simulators need to get a theory. Proposing such a hybrid would be in line with what many supporters of simulation advocate in any case, presumably encouraged by the thought that somehow theory could reliably provide causal explanations that designate another's reason for acting.²² Allegedly, it is because theories employ subsuming causal laws that 'The mechanism posited by the theory-theory is supposed to underpin the giving of robust psychological explanations of behaviour by the folk' (Arkway, 2000, p. 135). But as suggestive as this thought may be, theory is in no position to deliver where simulation cannot. It fares no better when it comes to dealing with difficult cases.

This should not be surprising since internal resources of general theories of mind are as limited as those of simulation heuristic. This is because in one way or another, both regard folk psychology as grounded in our shared rationality. Indeed it is just this need to appeal to the 'rational agent' that renders a general theory of mind in no better place to arbitrate between alternative reason explanations.

An example helps to make this clear. As I was preparing for a long visit to St. Louis, I asked my wife to arrange for my car to be serviced and kept in my local garage while I was abroad. I supplied her with its telephone number and she kindly made the booking for me. On the morning of my flight, she agreed to drive me to Heathrow after I dropped my car in at the garage. We set off, each in our own car with her in the lead, her boot laden with my luggage. As we came up to the turning for the garage, she stopped at a set of red traffic lights but unusually failed to signal. This surprised me because my wife is a stickler for such things. But then something even odder happened. To my amazement when the lights changed she did not turn, but began driving toward the town centre, straight past the garage at which she herself had made the booking! As time was against us, this alarmed me. I raced to make sense of her action, assuming that, very uncharacteristically her mind must have been elsewhere. At first, I flashed my lights with my signal light blinking, expecting her to realise that I was no longer following and hoping that she would notice her mistake. Things went from bad to worse when I saw her cast a glance in her rear view mirror without stopping. At this point, I was faced with a rather tricky interpretative problem. Given that my wife is very competent and reliable, lacking any malicious streak or any reason to act so, I was at an utter loss to make sense of her actions. Although a number of possible explanations sprang to mind, knowing my wife, none of these looked plausible. I was unable to make any sense of her actions.

²² For example, Goldman tells us that nothing 'excludes the mixed hypothesis that people use both a simulation heuristic and some form of nomological information. Some kind of mixed theory, I suspect, is unavoidable' (Goldman, 1992b, p. 22). In a similar spirit, Heal instructs us that, 'philosophers and psychologists should not oppose simulation to theory, but should rather ask what is the appropriate realm of each and how they interact' (Heal, 1996, pp. 75, 50).

Assuming my wife had not become irrational, even appealing to what rational agents might do in such circumstances did not narrow the field of possibilities nearly enough. No general theory of mind would have been any use. However, it might be thought that in order to deal with such difficult cases, a core theory of mind need only be augmented by yet more theory: perhaps we might get a viable result by means of a graft. Perhaps the main theory need only be reinforced with a clutch of auxiliary hypotheses, taking the form of mini-theories about the dispositions and traits of the person in question. Certainly, we often explicitly call on such information when speculating about others, thus if we are prepared to take a more relaxed view of what constitutes a theory it might be that person-specific theories could be formed on the basis of regular encounters and prolonged experience with particular people. No doubt this would help in weeding out *some* explanations. For example, Stich and Nichols consider a case in which we might use such detailed information about personal characteristics in order to evaluate the suggestion that a person is speaking in a foreign language for purposes of comic effect. They write, 'some belief/desire pairs will be easy to exclude. Perhaps the agent is a dour fellow; he never wants to make anyone laugh. If we believe this to be the case then [this proposal] won't be very plausible' (Stich and Nichols, 1992, p. 43).

Although such detailed information would help us to make more refined guesses about why someone *might* have acted, in most cases these would still fall short of providing the explanation of an action. For nothing ensures in any given instance that an action, even if it is not irrational, stems from a fixed set of dispositions: It is always possible the person is, in fact, acting 'out of character' or is spurred on by an unpredictable emotion. Moreover, many situations have novel features that we just cannot guess accurately. Luckily, this is what explained the otherwise disturbing behaviour of my wife on the way to the airport. After the incident, she explained that although it was true that she had phoned the garage to make the appointment herself, she had used the number I had given her which she believed was the number for our *old* garage, in the next village.

Neither theory-theory, simulation theory, nor the two in concert, could have reliably generated this explanation. But as it is just explanations of this sort that we require in our everyday affairs, this suggests that they lack the resources to reliably and successfully deal with many ordinary cases that cry out for explanation. Put otherwise, given that proper reason explanations require us to designate *the* reason for acting—as opposed to simply offering a possible reason for acting—all such third-personal approaches are of limited use. Ironically, if we accept the idea that folk psychology is central to our explicit reason-giving practices and reliably so, then it cannot be conducted by any of these ways.

*Philosophy, Department of Humanities
University of Hertfordshire*

References

- Baker, L. R. 1999: What is this thing called 'commonsense psychology'. *Philosophical Explorations*, 1, 3–19.
- Baker, L. R. 1995: *Explaining Attitudes: A Practical Approach to the Mind*. Cambridge: Cambridge University Press.
- Baron-Cohen, S. 1995: *Mindblindness: An Essay on Autism and Theory of Mind*. Cambridge: MA: MIT Press.
- Baron-Cohen, S. and Cross, P. 1992: Reading the eyes: evidence for the development of a theory of mind. *Mind & Language*, 7, 172–86.
- Bermúdez, J. 1998: *The Paradox of Self-Consciousness*. Cambridge, MA: MIT Press.
- Bogdan, R. 1997: *Interpreting Minds: The Evolution of a Practice*. Cambridge, MA: MIT Press.
- Botterill, G. 1996: Folk psychology and theoretical status. In P. Carruthers and P. Smith (eds.), *Theories of Theories of Mind*. Cambridge: Cambridge University Press.
- Bruner, J. 1990: *Acts of Meaning*. Cambridge, MA: Harvard University Press.
- Carruthers, P. 1996: Simulation and self-knowledge: A Defence of Theory-theory. In P. Carruthers and P. Smith (eds.), *Theories of Theories of Mind*. Cambridge: Cambridge University Press.
- Clark, A. 1997: *Being There*. Cambridge, MA: MIT Press.
- Clark, A. 1989: *Microcogniton*. Cambridge, MA: MIT Press.
- Costall, A. 1995: Socializing affordances. *Theory and Psychology*, 5(4), 467–81.
- Currie, G. and Ravenscroft, I. 2003: *Recreative Minds*. Oxford: Oxford University Press.
- Dennett, D. C. 1987: *The Intentional Stance*. Cambridge, MA: MIT Press.
- Dennett, D. C. 1985: *Brainstorms*. Cambridge, MA: MIT Press.
- Fodor, J. 1995: A theory of the child's theory of mind. In Martin Davies and Tony Stone (eds.), *Mental Simulation*. Oxford: Blackwell.
- Fodor, J. 1987: *Psychosemantics: The Problem of Meaning in the Philosophy of Mind*. Cambridge, MA: MIT Press.
- Fodor, J. 1981: *Representations: Philosophical Essays on the Foundations of Cognitive Science*. Cambridge, MA: MIT Press.
- Gallagher, S. 2001: The practice of mind: Theory, simulation or primary interaction? *Journal of Consciousness Studies*, 8, 83–108.
- Gallese, V. and Goldman, A. 1998: Mirror neurons and the simulation theory of mind-reading. *Trends in Cognitive Sciences*, 2, 493–501.
- Goldie, P. 2000: *The Emotions*. Oxford: Oxford University Press.
- Goldman, A. 1992a: In defense of the simulation theory. *Mind & Language*, 7(1–2), 104–19.
- Goldman, A. 1992b: Empathy, mind, and morals. *Proceedings and Addresses of the American Philosophical Association*, 66(3), 17–41.
- Goldman, A. I. 1989: Interpretation psychologized. *Mind & Language*, 4, 161–85.
- Gopnik, A. and Wellman, H. M. 1992: Why the child's theory of mind really 'is' a theory. *Mind & Language*, 7(1–2), 145–71.

- Gordon, R. M. 1986: Folk psychology as simulation. *Mind & Language*, 1, 158–71.
- Gordon, R. M. 1992: The simulation theory: Objections and misconceptions. *Mind & Language*, 7(1–2), 11–34.
- Gordon, R. M. 1995: Simulation without introspection or inference from me to you. In M. Davies and T. Stone (eds.), *Mental Simulation*. Oxford: Blackwell.
- Gordon, R. M. 1996: Radical simulationism. In P. Carruthers and P. Smith (eds.), *Theories of Theories of Mind*. Cambridge: Cambridge University Press.
- Gordon, R. M. 2000: Simulation and the explanation of action, In H. Kogler and K. Stueber (eds.), *Empathy and Agency*. Boulder: Westview Press.
- Heal, J. 1995: How to think about thinking. In M. Davies and T. Stone (eds.), *Mental Simulation*. Cambridge: Blackwell.
- Heal, J. 1996: Simulation, theory and content. In P. Carruthers and P. Smith (eds.), *Theories of Theories of Mind*. Cambridge: Cambridge University Press.
- Heal, J. 1998a: Understanding other minds from the inside. In A. O’Hear (ed.), *Current Issues in Philosophy of Mind*. New York: Cambridge University Press.
- Heal, J. 1998b: Co-cognition and off-line simulation: Two ways of understanding the simulation approach. *Mind & Language*, 13(4), 477–98.
- Heal, J. 2000: Other minds, rationality and analogy. *Proceedings of the Aristotelian Society, Supplementary Volume*, 1–19.
- Hempel, C. 1966: Laws and their role in scientific explanation. In R. Boyd, P. Gasper, and J.D. Trout (eds.), *The Philosophy of Science*. Cambridge, MA: MIT Press.
- Henderson, D. and Horgan, T. 2000: Simulation and epistemic competence, In H. Kogler and K. Stueber (eds.), *Empathy and Agency*. Boulder: Westview Press.
- Hutto, D. D. 2003: Folk psychological narratives and the case of autism. *Philosophical Papers*, 32(3).
- Hutto, D. D. 2002: The world is not enough: Shared emotions and other minds. In P. Goldie (ed.), *Understanding Emotions: Mind and Morals*. Burlington, VT: Ashgate Publishers.
- Hutto, D. D. 2000: *Beyond Physicalism*. Amsterdam/Philadelphia: John Benjamins.
- Hutto, D. D. 1999a: A cause for concern: Reasons, causes and explanations. *Philosophy and Phenomenological Research*, 59(2), 381–401.
- Hutto, D. D. 1999b: *The Presence of Mind*. Amsterdam/Philadelphia: John Benjamins.
- Jackson, F. 1999: All that can be at issue in the theory-theory simulation debate. *Philosophical Papers*, 28(2), 77–96.
- Kögler, H. H. and Stueber, K. R. 2000: *Empathy and Agency*. Boulder: Westview Press.
- Lamarque, P. and Olsen, S. 1994: *Truth, Fiction and Literature*. Oxford: Oxford University Press.
- McVeer, V. 2001: Psycho-practice, psycho-theory and autism. *Journal of Consciousness Studies*, 8, 109–32.
- Millikan, R. 2000: *On Clear and Confused Ideas: An Essay About Substance Concepts*. Cambridge: Cambridge University Press.
- Morton, A., 2003: *The Importance of Being Understood*. London: Routledge.

- Nichols, S., Stich, S., Leslie, A. and Klein, D. 1996: Varieties of off-line simulation. In P. Carruthers and P. Smith (eds.), *Theories of Theories of Mind*. Cambridge: Cambridge University Press.
- Nichols, S., Stich, S., Leslie, A. and Klein, D. 1996: Varieties of off-line simulation. In P. Carruthers and P. Smith (eds.), *Theories of Theories of Mind*. Cambridge: Cambridge University Press.
- Perner, J. 1996: Simulation as explication of prediction-implicit knowledge about the mind: Arguments for a simulation-theory mix. In P. Carruthers and P. Smith (eds.), *Theories of Theories of Mind*. Cambridge: Cambridge University Press.
- Roth, P. A. 1991: Truth in interpretation: The case of psychoanalysis. *Philosophy of the Social Sciences*, 175–95.
- Segal, G. 1996: The modularity of theory of mind. In P. Carruthers and P. Smith (eds.), *Theories of Theories of Mind*. Cambridge: Cambridge University Press.
- Stich, S. 1983: *From Folk Psychology to Cognitive Science*. Cambridge, MA: MIT Press.
- Stich, S. 1990: *The Fragmentation of Reason*. Cambridge, MA: MIT Press.
- Stich, S. and Nichols, S. 1992: Folk psychology: Simulation or tacit theory? *Mind & Language*, 7(1–2), 35–71.
- Taylor, A. E. 1955: *Aristotle*. New York: Dover.
- Wittgenstein, L. 1953: *Philosophical Investigations*. Oxford: Basil Blackwell.
- Wittgenstein, L. 1967: *Zettel*. 2nd ed. Oxford: Basil Blackwell.
- Woodward, J. 1984: A theory of singular causal explanation. *Erkenntnis*, 21, 231–62.