

Where is the 'Theory' in Theory of Mind?

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ABSTRACT. Why did 'Theory of Mind' take off when it did, and what, if anything, holds this very diverse approach together? The focus of research within developmental psychology since the 1960s had been the examination of Piaget's claims about cognitive development. The agenda had quickly become to demonstrate, primarily through experiments, that young children could (contrary to Piaget's claims) succeed on various 'cognitive' tasks given the right context. Yet, by the 1980s, the research had still neither undermined Piagetian theory, nor opened up any radical alternative. Theory of Mind was conveniently to hand to provide another 'grand' theory to subject to experimental test, and in many ways business continued as usual. However, 'context' came to be regarded as no longer an important issue for research but a contaminant, and the experiments became stipulative rather than exploratory. In the attempt to eliminate context in order to test children's real understanding of other people, the experiments themselves have come to constitute the largely implicit 'theory' behind Theory of Mind. The experiments presuppose that making sense of other people is essentially about making indirect inferences from the apparent (observable behaviour) to the real (hidden mental structures). It is this experimental paradigm of signification—of an indirect or 'round-about' relation between observations and the object of study—that ultimately holds the Theory of Mind approach together.

KEY WORDS: history of psychology, Piaget, problem of other minds, Theory of Mind

The guiding point of view in the selection of material and in determining the rules for its employment was, as is evident, the attempt to simplify as far as possible, and to keep constant as possible, the conditions under which the activity to be observed . . . came into play. Naturally, the better one succeeds in this attempt the more does he withdraw from the complicated and changing conditions under which this activity takes place in ordinary life and under which it is of importance to us. But that is no

objection to the method. The freely falling body and the frictionless machine, etc., with which physics deals, are also only abstractions when compared with the actual happenings in nature which are important to us. We can almost nowhere get a direct knowledge of the complicated and the real, but we must get at them in roundabout ways by successive combinations of experiences, each of which is obtained in artificial, experimental cases, rarely or never furnished in this form by nature. (Ebbinghaus, 1885/1964, p. 26)

Although *theorizing* is purportedly the subject matter of Theory of Mind, the theoretical implications of this approach are difficult to tie down. Astington (1993) has suggested three different ways we might take Theory of Mind. It can refer simply to a field of research, linked to the earlier research in social psychology and developmental psychology on ‘role-taking’ and ‘perspective-taking’. According to this first sense, the term ‘Theory of Mind’ involves no specific commitments on the part of the researcher about *how* people go about making sense of other minds (e.g. Astington & Gopnik, 1991, p. 7).

The term may also refer ‘in a general way to children’s folk psychological knowledge’, where ‘theory’ could be replaced, among other things, by ‘conceptual system’ (p. 170). Again, many psychologists would probably not regard even this sense of the term as involving any specific or contentious assumptions on their part.

Finally, according to Astington, there is the full-blown sense of Theory of Mind, the explicit theoretical proposal that children, in coming to understand their own and other people’s minds, are themselves engaged in theory development analogous to theory formation and transformation in science. This specific theoretical commitment has come to be distinguished by the awkward term ‘theory-theory’ (e.g. Perner, 1991; Wellman, 1990), but which (following Sharrock & Coulter, this issue [2004]) we will call ToMism.

However, even in 1994, when Astington was setting out these different senses of Theory of Mind, there were distinct alternatives to ToMism already available that do not fit easily into her scheme. Most notably, there is the ‘ToMM’ approach, which, although postulating a biological module devoted explicitly to ‘Theory of Mind’, goes on to deny that any reference to theory is anything more than a ‘metaphor’:

. . . evolution has equipped the human brain with a special module, a theory of mind mechanism (ToMM), that helps normally developing children to attend to the invisible mental states of others. . . . This social instinct owes little to general reasoning powers. . . . (Leslie, 2000, p. 61)

It is difficult to see how to fit this appeal to a Theory of Mind module into Astington’s three-fold division. It seems too specific for inclusion under her second category, but neither will it squeeze into the third, unless this module is envisaged as a kind of theorizing implant—an intelligence by proxy,

deriving not from God (as in pre-Darwinian instinct doctrine), but from another potent and potentially mysterious 'agency', natural selection:

We are 'mindreaders' by nature, building interpretations of the mental events of others and feeling our constructions as sharply as the physical objects we touch. Humans evolved this ability because, as members of an intensively social, cooperative, and competitive species, our ancestors' lives depended on how well they could infer what was on one another's minds. Precisely because such an interpretative system does model the world in terms of unobservable entities (thoughts, intentions, beliefs, and desires), it needs to be coupled to confederate modules that can construct a bridge from the observable to the unobservable. *Unobservable entities are invisible to association-learning mechanisms, but they are 'visible,' over the long run, to natural selection.* As chance created alternative cognitive designs, this process 'selected' those that implemented the best 'betting' system. (Tooby & Cosmides, 1995, p. xvii, emphasis added.)¹

Then, at the other extreme, there is what Astington and Gopnik (1991) refer to as 'folk psychology as a form of life':

The aim and nature of psychological understanding might not be to explain or predict experience or behaviour, but to regulate our interactions with others. On this view folk psychology is not a theory, is not indeed any kind of knowledge, but is instead what Wittgenstein would call a 'form of life', a set of social and cultural practices and conventions. The mechanism for development, on this view, would be socialization or enculturation—children would learn how to psychologize appropriately in the way that they learn how to dress properly or eat politely. Theories always develop with reference to the outside world; put very simply, a theory former wants to get closer to the truth. Cultural practices on the other hand, are, at least largely, self-constitutive: they make themselves the case. Theories are true or false, cultural practices are right or wrong. (pp. 19–20)

According to this approach, the child, in learning to *act* appropriately in relation to other people, is clearly not regarded as being engaged in a detached process of theorizing, but nor would what the child is acquiring obviously count as a 'conceptual system' (Astington, 1993, p. 170) in the standard, cognitivist sense of that term.

Thus, the Theory of Mind approach seems more like a loose coalition than a unified movement. Not all of its proponents are ToMists, going along with the explicit claim that people, when making sense of other minds, are actually engaging in *theory*—whatever 'theory' may mean. Nor did ToMism itself rise without trace. Many of the assumptions of ToMism were current within psychology several years before the wider Theory of Mind approach came to take such a dominant position within psychology (see Leudar & Costall, this issue [2004]).

So why did Theory of Mind take such a hold over psychology when and where it did? ToMism, we shall argue, came along at just the right time, when developmental psychologists studying early childhood were in serious

need of a grand new theory to subject to close experimental scrutiny. But what now holds together the wider approach—the ToMists and the various kinds of non-ToMists? First of all, with the notable exception of the ‘form of lifers’, there is the common assumption that making sense of other people involves the traditional ‘problem of other minds’, a problem which children, in particular, must supposedly resolve before they can relate properly to others (see Leudar & Costall, 2004). However, the assumption that the mind is, in effect, occult is hardly confined to Theory of Mind. What is distinctive about Theory of Mind as an approach within *experimental psychology* is the widespread acceptance (1) that both the subjects of psychological research and the scientists studying them are, in an important sense, on a similar epistemological footing in their efforts to understand ‘mind’, and (2) that the psychological experiment represents the problem people in general face when trying to understand minds, and (given the way the problem is defined) also the general form that the successful understanding of other minds must take. In the end, what holds Theory of Mind together, we will be proposing, is not so much any shared explicit theoretical claims, but the institution of the psychological experiment itself, which serves as a largely tacit model, a metaphor or ‘paradigm’ of how we must all be managing to understand one another.

Piagetian Theory Meets Experimental Psychology

There are three good reasons why developmental psychology should *not* have been receptive to Theory of Mind. First, largely through the important lead of Jerome Bruner (e.g. 1975, 1978), there was a growing commitment among the young developmental psychologists of the time—and most of them were then *young*—to a pragmatic, and essentially interpersonal, approach to development. Second, there was serious and scholarly interest in the cultural psychology of Lev Vygotsky, and, to a lesser extent, the social interactionism of George Herbert Mead (e.g. Leudar, 1991; Minick, 1985; Morss, 1985; Sinha, 1989; Wertsch, 1985). Finally, great emphasis was placed upon *context* as a corrective to both the individualism and universalism of standard developmental theory (e.g. Buck-Morss, 1979; Donaldson, 1978; Richards & Light, 1986; Valsiner, 1987).

One of our first serious encounters with Theory of Mind was at a symposium held at the Annual Conference of the Developmental Psychology Section of the British Psychological Society in Birmingham in 1993, organized to present critiques and alternatives to that approach. Of course, we were already aware of the existence of the Theory of Mind framework through the work of Premack and Woodruff (1978) and Baron-Cohen, Leslie and Frith (1985), but supposed that it was still a minority position that might simply fade away. Yet, not only were the alternatives presented at the

symposium largely framed within the constraints of the Theory approach (Costall, 1993), but nearly a quarter of the conference papers and posters was devoted to it, 24/91 and 12/54 respectively (British Psychological Society, 1994). So why did Theory of Mind take such a hold among developmental psychologists, given not only that they tend to be more thoughtful than most other psychologists, confronting fundamental questions about the origins and nature of mind, but also that they have more promising theoretical alternatives already to hand?

By the 1960s, developmental psychology began to take on a new lease of life, through a curious convergence of Piagetian theory and tough-minded Anglo-American experimental psychology. Until the 1950s, researchers studying mental development had been mainly based, not in experimentally oriented psychology departments, but in child institutes, paediatrics, education, clinical psychology and nursing (Miller, 2002, p. 74). The attraction of Piagetian theory, with its cognitive emphasis, was that it provided a theoretical alternative to the dominant, but increasingly unconvincing, 'learning theory' approaches:

Academic psychology had pushed behaviorism in general and learning theory in particular to their limits and found them wanting. Even when learning theory was modified by such notions as verbal mediation, social reinforcement, modeling, intrinsic reinforcement, and attention, it did not completely satisfy psychologists. There was dissatisfaction with the explanation of language development in terms of imitation, practice, and reinforcement. (Miller, 2002, p. 74)

Yet, in a discipline so anxious about its scientific credentials, this interest in Piaget's theoretical innovations was not matched by any fundamental changes in method. Although new research techniques were being developed in related disciplines, such as computer simulation, these have never taken a central place within psychology itself. Certainly, the techniques being deployed by the Chomskyan linguists² looked nothing like proper science to the psychologists:

A few years ago some linguists demonstrated to psychologists that one could answer penetrating psychological questions about the nature of human language without experimentation or even the explicit collection of data in the statistical tradition of psychology. The initial reaction of many experimental psychologists was to say that what these linguists were doing was not science. It was not science because it could not be found in the laboratory and it did not seem to lead to the laboratory. It was not science because there was nothing to which one could apply statistics. (Deese, 1972, p. 22.)³

Methodological conservatism has been an enduring feature of psychology (Walsh-Bowers, 2004; Winston, 2004). This extends not just to experimentation, but also, for example, to the hypothetico-deductive method, to the use of inferential statistics, and to the very language in which the

research findings are framed. Inferential statistics, which presuppose that individuals generally respond in more or less the same way under the same experiment conditions, are still routinely used, even though one of the main tenets of cognitive theory has been that people can ‘represent’ the same ‘stimulus’ in qualitatively different ways, or use quite different strategies when solving problems (e.g. Bruner, Goodnow, & Austin, 1956). And the terminology of stimulus and response is still the norm within cognitive psychology, continuing to constrict the meaning of ‘cognition’ to that which intervenes between stimulus and response. Thus, according to Hebb (1960), ‘the whole meaning of the term “cognitive” depends [on the stimulus–response formula], though cognitive psychologists seem unaware of the fact’ (p. 737). And, if Baars and McGovern (1994) are to be believed, the fundamental insight of the cognitive revolution is that ‘scientific psychology, contrary to behavioristic doctrine, must make inferences beyond the bare observable stimuli and responses’ (p. 370).⁴

Many of the early proponents of the cognitive approach have themselves expressed their regrets about the way method and theory eventually passed one another by. According to Wendell Garner (1999), ‘cognitive psychology lost out to the received view, with its operational and reductionistic methods’, so that the ‘old won out over the new’ (p. 21; see also Bruner, 1990; Neisser, 1997).

Although there has always been a place for a range of methods in psychology, the experiment has remained for most psychologists the final arbiter.⁵ And the case for the experiment as the acid test of people’s abilities always sounds so reasonable, as in this passage from a recent text on the psychology of language acquisition:

Picture the following situation. A mother instructs her child to ‘put the spoon in the cup,’ and the infant performs the action correctly. *But can we really take this as evidence that the infant fully comprehended the sentence on the basis of its words and structure?* Joint attention (mother and infant attending to the same object or event) and gestures such as the mother pointing may have contributed to directing the infant’s attention. Past experience of spoons and cups may then have guided her actions to place the spoon inside the cup, without the child necessarily understanding the preposition ‘in.’ Physical constraints—you cannot put cups in spoons—might also play a role. It is quite feasible . . . that, in this case, word order is not vital in determining the successful outcome of the exchange. So how much of the sentence (that is, the strictly linguistic context) did the infant *really* understand? The only way to answer such questions is to devise controlled experiments to gauge the precise level of the infant’s language comprehension. (Karmiloff-Smith & Karmiloff-Smith, 2001, pp. 11–12, emphases added)⁶

This, then, is the context in which Piagetian theory was taken up by American and British developmental psychologists. The researchers had

turned to Piaget because he was offering an exciting new kind of theory, quite different from the largely discredited associationist models currently available. But Piaget's claims had been based on observational data, single case studies, not on experiments with large groups of children. Would his claims stand up when tested under the more stringent conditions of the experiment? This methodological issue soon became *the* issue, and, in place of serious theory development, 'Piaget bashing' (an unpleasant term widely used at the time) dominated the research agenda.

Whereas learning theory emphasized *continuity* in development, Piagetian theory insisted that there were *qualitative* differences between children at various developmental stages, up to, and including, adulthood. The main issue for the experimental research therefore became whether babies and young children are, in fact, capable of doing many of the things that Piagetian theory had deemed *impossible* at particular 'stages' of development. The research explored how *context* influenced the 'performance' of children, ranging from the wider culture of the child to the minutiae of the experimental procedure, such as the instructions and the presentation of the material. The research included investigations of children's understanding of 'conservation' (e.g. the appreciation that certain properties such as weight and number are conserved across various transformations of shape or layout), and also the understanding of other people's perspectives, including the issue of 'egocentrism'.

The researchers examined a limited set of experimental 'tasks', since their main objective was to discover different ways of presenting those tasks so that children might manifest the abilities denied to them by Piaget. Among all of the meticulous experimental work, there were impressive high points, such as the demonstration of the surprising perceptual and social abilities of very young babies (see Butterworth, 1981), and the dramatic ways in which the performance of older children in the standard Piagetian tasks could be improved by apparently minor manipulations in the ways those tasks were presented (e.g. Donaldson, 1978).

By the 1980s, however, it was no longer obvious where all the research was leading. This was not such a problem for the infancy researchers. They could search for evidence of neonatal competences at progressively younger ages, and usually find them, and even extend their studies to the prenatal period (e.g. De Caspar & Spence, 1986; De Vries, Visser, & Prechtl, 1984). In fact, many of those researchers have remained content to examine and re-examine a single 'task' (e.g. imitation, reaching for an object, or the visual cliff) over the course of what are now long careers. Furthermore, many of the key infancy researchers readily succumbed to Chomskyan nativism (see Spelke & Newport, 1998).

However, new-variant nativism did not seem like an option for the experimentalists studying 'older' children (i.e. 1-year-olds onwards), even though both the successes of the neonatal researchers in demonstrating the

competences of infants, and also their own emphasis upon continuity, seemed to challenge the idea that there were any fundamental developmental problems left for them to investigate.⁷

Furthermore, the effects of context were not leading to any radical challenge or alternative to the Piagetian schema, since context (like 'friction' in Newtonian mechanics) could always be accommodated as a secondary complication. Margaret Donaldson (1978), a driving force in this research, made it perfectly clear that, although proposing 'certain re-interpretations', she had largely arrived at a position 'in no way incompatible' with Piaget's (p. 9).

Eventually, for a change if not a rest, several of the Piagetian researchers turned, perhaps in desperation, to a lesser known theorist, Georges-Henri Luquet, and what they mistook to be his stage theory of the development of children's drawings (see Costall, 2001). And, for a while, the cycle repeated itself. The basic experimental task involved presenting children with a familiar object, such as a cup, but in such a way that a salient feature, such as the handle of the cup, was out of view. Typically, when asked to draw the object, children up to the age of 7 or 8 include the hidden feature in the drawing. The main objective of the experimental research was to show that under appropriate conditions, young children would become aware of their own 'view' of the object, exclude the occluded feature from their drawing, and hence draw 'in perspective'. A body of research was once more built up, again primarily demonstrating the importance of 'context', and thus the limitations of Luquet's 'stage theory', but, again, this research gave rise to no serious theoretical alternatives.

By the 1980s, it was beginning to feel as if Piaget, and perhaps everyone else, had suffered enough. Unfortunately, for the researchers, the obvious alternatives to Piaget, the Vygotskian and Meadian theoretical perspectives, seemed to lend themselves to observational and naturalistic studies, but not to 'neat' experiments. These alternative approaches stress the contingency of the psychological and the social and thus call for methods of investigation other than experiments. In fact, the attempts at Vygotskian experiments have seldom ended up looking like real experiments. Nevertheless, most of the researchers studying cognitive development continued (as they still do) to regard experiments as the method of choice.

This is where Theory of Mind came in, not only in its 'classic' form of ToMism, which explicitly invoked *theorizing* as the way to span the Cartesian divide between bodily behaviour and mind, but soon also a range of alternatives premised upon the same divide, most notably ToMM. Like the Chomskyan 'preformationism' adopted within the infancy research, ToMM was also a form of nativism—'predeterminism'—proposing pre-programmed developmental changes coming into effect, not at birth, but in early childhood.⁸

These alternative positions, like Piagetian theory, made predictions about

when children should—or should not—be able to make sense of mind;⁹ they suggested surprising new theoretical connections, for example, between ‘mind reading’ and pretend play, and they also made claims about the specific deficits associated with autism. All of this lent itself readily to experimental investigation. And the topics with which Theory of Mind was concerned were nevertheless reassuringly close to some of those already pursued in the Piagetian-based research.¹⁰ Thus, when Maureen Cox revised her widely read text, *The Child's Point of View*, in 1991, just five years after it first appeared, she simply added a new, final, section on ‘the recent surge of research activity’ on the child’s understanding of people’s mental states to her existing overviews of the research relating to Piaget and Luquet on perspective taking, and you can hardly see the join.

Thus, the experimenters could remain in business as usual, and very much in the usual business. And the new theory threw in as a bonus its own new experimental task, the false belief task. As the bemused Ian Hacking (1999) observed:

A single ingenious experiment originally suggested by philosophers has spawned an experimental industry. That is often the case in psychology, where new experimental ideas are as rare and as hard to invent as deep mathematical proofs or truly new magic tricks. (p. 115)¹¹

Indeed, this new task was rapidly taken up, not just as a research procedure, but also a *criterion* of understanding other minds. As Jerome Bruner complained:

I was struck at the European Conference on Developmental Psychology held in Scotland in 1990 that theory of mind researchers seemed to have got stuck on the criterion of false belief. . . . To equate grasping other minds with getting a False Belief Diploma at Graduation Day is to oversimplify its form and function. To equate ‘having’ a theory of mind with grasping the epistemological distinction between true and false belief obscures the contribution of the three or four years of development that preceded it. (Bruner & Feldman, 1993, p. 269)

Finally, in turning their attention to ToMism, it was not as though the researchers had to compromise their scientific objectivity or intellectual integrity. They did not have to *be* ToMists to engage in the research on Theory of Mind—any more than previously you once had to *be* a Piagetian to conduct Piagetian research. In conducting such research, they surely could remain perfectly neutral or even adversarial.

Theory of Mind Meets Experimental Psychology

The Theory of Mind approach can no longer be identified with the specific assumption (now apparently confined to the ToMists) that people, in making

sense of other people in their everyday lives, are engaged in theorizing—whatever ‘theorizing’ might happen to mean (see Samet, 1993). In any case, the idea that ordinary people are ‘theorists’ is not unique to ToMism, though previously, having been ‘set up’ as theorists, the point had usually been to demonstrate that ‘people’ were woefully incapable of engaging in such an elevated activity best left to scientists. What holds together much of the Theory of Mind approach is the assumption that the ordinary person and the scientific psychologist are essentially confronting the same problem—‘going from observed data (behaviour) to hidden structure (mind)’—and solving that problem with comparable success, if by different means. Once Theory of Mind was adopted by experimental developmental psychologists, this assumption became attached to, and partly transformed, the psychological experiment itself. In turn, the psychological experiment has provided a ‘paradigm’¹² for the researchers, not just in the sense of a procedure for testing knowledge of other minds, but as a tacit model of what making sense of other people must entail.

The standard line, within psychological theory, has been that ordinary people are epistemological dupes. This has been most blatant within perceptual theory, where the researchers continually appeal to objective evidence to argue, in effect, that objective evidence is in principle unattainable for people (if not for *scientists*):

It used to be thought that perceptions, by vision and touch and so on, can give direct knowledge of objective reality. . . . But, largely through the physiological study of the senses over the last two hundred years, this has become ever more difficult to defend. . . . ultimately we cannot know directly what is illusion, any more than truth—for we cannot step outside perception to compare experience with objective reality. (Gregory, 1989, p. 94)

Such an epistemological double standard between the scientists and other people has also been basic to social psychology and the field of social cognition. According to the textbooks, ‘people’ can only know about other people in a necessarily indirect and generally hazardous way, given the limited and ambiguous evidence available to them:

It is generally no more difficult to judge the height of a person than it is to judge the height of a bookcase. The same is true of weight, skin color, or style of clothing. It is also fairly easy to make judgments about somebody’s social role. . . . But person perception becomes more difficult when we try to infer internal states—traits, feelings, emotions, and personalities. . . . Judgments of such internal states as emotions, personality traits, and attitudes are often *extremely difficult*. The person’s internal state cannot be observed directly—it must be inferred from whatever cues are available. (Taylor, Peplau, & Sears, 1994, p. 51, emphasis added)

In addition to the problem of limited available information, our judgements are further clouded by a host of selective biases to which we are all subject

(Smith & MacKie, 2000, p. 85). The psychologists committed to this 'error paradigm' (Funder, 1995) have, in contrast, been somehow in a quite different epistemological position, encountering no serious problems in ascertaining how people think and feel, and hence able to assess the general inaccuracy of the rest of us in our attempts to make sense of other people.

This epistemological asymmetry is still maintained in the general introductory textbooks. On the one hand, there are the substantive chapters about how ordinary people make sense of other ordinary people, and, on the other, there are the methodological chapters on how a science of psychology is possible. Seldom is any connection made between them.¹³

This dualism between those *in the know* and those who are *not* is neither new nor unique to psychology. It has its origins in the general project of modern science to penetrate beyond mere appearance to reality, to go beyond the observable to the essentially *unobservable*:

The universe is not as it appears in common experience; its nature and structure do not lay themselves open to perception; on the contrary, they have to be uncovered by means of mathematical notions. In reality, then, the universe is not as it seems to be, but as it is conceived and constructed by the mathematical physicist. (Gurwitsch, 1978, p. 72; see also Blackmore, 1979)

Gurwitsch traces this revival of a Platonist dualism of appearance and reality to the physics of Galileo, where the realm of appearance 'is assumed to be subordinated to the other and to lead a merely borrowed existence . . . to be explained in terms of the domain of higher order' (p. 84). However, according to Gurwitsch, it was Descartes who made this dualism fundamental to the philosophy of the new mechanical science.¹⁴

Furthermore, as Kearney (1971) has noted, Descartes elevated mechanism itself to the realm of abstraction:

The Cartesian universe was mechanical in the sense that it existed as a machine and nothing else. Descartes stripped away from his view of the universe all that was extraneous to its mechanical functioning. It became the equivalent of a blue-print which was transformed into matter (extension). It was thus more mechanical than a machine, which at least possessed certain qualities such as colour. The Cartesian universe, therefore, was a machine stripped down to its absolute essentials. (pp. 156–158)

The problem about mind *as a subject for scientific investigation*, within the Cartesian scheme, was not that it is 'hidden' but that that it was deemed by Descartes to be the wrong kind of 'substance' to be treated in terms of mechanism (although, in the end he proved surprisingly open-minded even about this; see Cottingham, 1992). Indeed, according to the Cartesian or 'realist' ideal of science, a mind that can be 'neither seen, heard nor felt' (German & Leslie, 2000, p. 230) could only be an *asset*. The proper subject of psychology, as for any other science, *ought* to be unobservable. This is one further important, if seldom noted, sense in which Chomsky (along with

most other cognitive psychologists) is truly a Cartesian. According to Chomsky, science in general is in the business of advancing hypotheses about internal states (Chomsky, cited in Peck, 1987, p. 181), and for psychologists not to do likewise would be to indulge in an unwarranted 'methodological dualism' (Chomsky, 1997, p. 20; 2003, p. 283):

The goal is to find evidence that provides insight into a hidden reality. Adopting this approach, we abandon *the hopeless search for order in the world of direct experience*, and regard what can be observed as a means to gain access to the inner mechanisms of mind. (Chomsky, 1997, p. 19, emphasis added)

Chomsky's view is hardly idiosyncratic. Here is Rom Harré (2002) making the same basic point in his recent 'philosophical introduction' to cognitive science. He notes: 'Physicists are happy with quarks. Chemists have no trouble with atomic structures. Biologists are comfortable with genes. Geologists talk freely about tectonic plates, and so on.' Hence, cognitive science too should 'pass beyond what can be perceived by the senses, into the deeper realms of material reality' (p. 2).

And it surely works. Just as scientists, in general, are largely in the business of making inferences from data to hidden explanatory structures, and doing so very effectively, so are psychologists, in particular, routinely finding more and more about their own subject, the essentially unobservable mind. Thus, just as computers are supposed to provide an 'existence proof' for the representationalist theory of mind, psychologists themselves constitute the living proof that the philosophical problem of other minds is no insurmountable problem at all:

In retrospect, it is hard to understand how American behaviorists could have taken such an antitheoretical stand and clung to it for so long. Just because introspection proved to be unreliable did not mean that it was impossible to develop a theory of internal mental structure and process. It only meant that other methods were required. In physics, for example, a theory of atomic structure was developed, although that structure could only be inferred, not directly observed. . . . *The success of cognitive psychology during the later part of the twentieth century in analyzing complex intellectual processes testifies to the utility of postulating mental structures and processes.* (Anderson, 2000, p. 10, emphasis added)

Much of the prestige of modern science has derived from the claim that scientists have privileged access to reality, since only they are able to go beyond mere appearance, and 'see', as it were, the invisible. But, in the end, this self-serving 'them and us' dualism will not do. Scientific theories need to be reflexive, at least to the extent that they should be compatible with the fact that it is people who do science. And, as the philosopher Edwin B. Holt (1914) noted, the roles of subject and scientist, within experimental psychology, are usually interchangeable:

The psychological experimenter has his apparatus of lamps, tuning forks,

and chronoscope, and an observer on whose sensations he is experimenting. Now the experimenter by hypothesis (and in fact) knows his apparatus immediately, and he manipulates it; whereas the observer (according to the theory) knows only his own 'sensations', is confined, one is requested to suppose, to transactions within his skull. But after a time the two men exchange places: he who was the experimenter is now suddenly shut up within the range of his 'sensations', *he* has now only a 'representative' knowledge of the apparatus; whereas he who was the observer forthwith enjoys a windfall of omniscience. He now has an immediate experience of everything around him, and is no longer confined to the sensation within his skull. Yet, of course, the mere exchange of activities has not altered the knowing process in either person. (p. 149; see also Wilcox & Katz, 1984)

Within the long tradition of modern science, the commitment of the Theory of Mind approach to an epistemological continuity between people and psychologists is unusual. 'Both children and psychologists', as Terwogt and Stegge (1998, p. 247) put it, 'face essentially the same problem: How to understand human action?' And once children stop being 'behaviourists' seeking explanations 'exclusively in terms of situational determinants', they soon recognize the error of their ways, and move on to become mentalistic psychologists instead.

Thus Theory of Mind seems so refreshingly democratic. Rather than trying to find a place for people within science, it apparently founds scientific practice upon human nature. Indeed, according Gopnik and Wellman (1992), science might itself be regarded as no more than an incidental, late-arriving and less secure 'spin-off' of biologically given knowledge-gaining capacities critically important in early development, but largely redundant by adulthood (when we have all, of course, got everything other than science nicely sorted out!). Science, according to Gopnik and Wellman, is just an after-thought, 'a sort of spandrel, parasitic on cognitive development itself' (p. 168).

Yet despite claiming the primacy of everyday life in relation to the specialized activity of science, Theory of Mind assimilates our social life and its development to the paradigm of modern experimental cognitive psychology.¹⁵ It works backwards from this paradigm, setting it up as a paradigm—a not entirely articulated model—of 'the problem of other minds'.

Knorr-Cetina (1992) has identified three distinct technologies involved in experimentation, that of representation, of treatments and interventions, of signification:

The construction placed upon the objects of research varies accordingly: in the first case, objects in the laboratory are representations of real-world phenomena; in the second, they are processed partial versions of these phenomena; in the third, they are *signatures* of the events of interest to science. (p. 123, emphasis added)

In the standard experiment within cognitive psychology the object of study is no more than an index. Indeed, what the participants are doing in most experiments—for example, pressing buttons or keys on computer keyboards—could be of no intrinsic interest to experimental psychologists (cf. Leudar & Costall, 2004). Cognitive psychology is not about what people can be observed to be doing, but, instead, about the hidden structure behind what they are doing:

To take behavior as the focus of attention for psychology is as big an error as to take tracks in cloud chambers as the main object of study in particle physics. Such tracks are interesting only as clues to the existence of certain particles and to their properties. (Macnamara, 1999, p. 241)

. . . psychology is the science of behavior only . . . in the sense that physics is the science of meter reading and chemistry the science of observing changes in the color of paper. (Deese, 1972, p. 12)

. . . a physicist's data often consists of things like meter readings, but nobody calls physics, 'meter reading science.' Similarly, the data of a psychologist is behavior, in a broad sense. But to call a field 'behavioral science' is to say it's a science of behavior in the sense in which physics is a science of meter reading. (Chomsky, 1986, p. 346)

A gulf between behaviour and mind—between appearance and the reality—is thus embodied within the 'technology' of the standard psychological experiment, as is the *problem* of what bridging that gulf might entail.¹⁶

First of all there is the issue of even realizing that there might be a reality beyond the appearance. As Alan Leslie (1987) has put it: 'It is hard to see how perceptual evidence could force an adult, let alone a young child, to invent [*sic*] the idea of unobservable mental states' (p. 422). Then there is the further problem that the evidence for mind is typically impoverished and ambiguous. Claims about mental states, cognitive processes and structures are underdetermined by the available data.¹⁷

There are, however, more than hints within Theory of Mind (and cognitivism more generally) that the relation between behaviour and mind—between what we can observe and what we 'infer'—is tenuous in the extreme. Clearly, the proponents of ToMM share Chomsky's view (about grammar) that the 'theory' is '*hopelessly* underdetermined by the fragmentary evidence available' (Chomsky, 1975, p. 11, emphasis added; see also Tooby & Cosmides, 1995). Yet even the ToMist Alison Gopnik (1993) has claimed that the derivation of the 'theory', if not its application to particular cases, does not depend, and could not depend, on empirical data: 'I would not want to suggest that this theory is reducible to behavior, but more strongly I would also deny that it is based on behavior' (p. 10).

Nevertheless, the 'imagery' of the experimental paradigm persuades us not only that there is a problem of other minds, but that it has a solution along the lines implied by that paradigm. Yet the experiment within

developmental psychology has itself undergone an important, if largely unnoticed, transformation. Whereas, within the earlier Piagetian experiments, the importance of context became of focal interest, in the Theory of Mind experiments, context is eliminated in order to reveal the essence of children's theory of mind (Astington, 1993). The decontextualized experimental tasks become the 'criteria' of understanding minds (cf. Bruner & Feldman, 1993, p. 269). The experiment, far from being exploratory, has become stipulative.

The experiment embodies not simply the Cartesian duality of appearance of reality, but also the Cartesian isolated subject:

On the Cartesian picture of things we find a subject abstracted from ordinary intercourse with the world—including the world of other subjects. . . . The knower and the world are taken to be isolable units; and any individual subject's knowledge of the world is conceptually isolable from the existence of other subjects. Scepticism is forever threatening; the possibility of radical doubt about the existence of the world and other subjects is built into this picture. This is a philosophy of atoms: mental and physical atoms. It is an important part of such a philosophy that its conception of a subject is one isolated from other subjects. Thus, Descartes can sit alone in his study and contemplate his knowledge of the world. (Avramides, 2001, pp. 41–42)¹⁸

Are these really the kinds of problems we normally encounter in our interactions with other people in our everyday lives? After all, experimentation is not necessarily the best basis for an *empirical* psychology. Now, there has already been research conducted on how children and indeed adults engage with other people in everyday settings (e.g. Dunn, 1988; McCabe, Leudar, & Antaki, 2004; Reddy, 1990; Wootton, 1997). And these researchers have used observational methods precisely because they regard the experimental research as marginalizing 'the social procedures and social knowledge which create bases for mutual alignment' between people (Wootton, 1997, p. 190; see also Roberts & Lee, 2002).

It is not as though this alternative research is completely ignored in the mainstream Theory of Mind literature. It is duly noted, as are the stark discrepancies between how well young children relate to other people in their everyday lives, and how poorly they perform in experiments. For example, Astington (1993, p. 12) takes note of the paradox of how well youngsters appear to be tuned in to other people, and the failure of older preschoolers in experimental tests of their understanding of another's point of view. And similar discrepancies are now showing up in relation to the more recent observational studies with adult schizophrenics.

There have typically been two responses to the claims from the observational studies, and their discrepancy with the experimental data. The first has been a dogmatic insistence that what is being observed is mere *appearance*. Vasu Reddy and Paul Morris (this issue [2004]) provide an extensive

discussion of such responses in relation to infancy and early childhood, where whatever the child is doing that the 'theory' claims they should *not* be doing is deemed to be a 'pseudo' version of the real thing: pseudo-lying, pseudo-conversation or pseudo-pointing. And this scepticism extends to the clinical literature too. Here is the response of Beate Sodian and Uta Frith (1993) to the surprising finding that children diagnosed as autistic can (contrary to their theory) engage in 'double-bluff', telling the truth when a lie is expected: 'That some autistic subjects could solve this task is remarkable. However, it cannot be ruled out that *they worked out the solution by some surface-level strategy*' (p. 174, emphasis added). And, as Uta Frith (1992) earlier explained: 'Autistic people who succeed on some mentalising tasks demonstrate how experience and compensation may camouflage a deficit' (p. 17).

Similarly, if, according to Chris Frith (1992), adult schizophrenics seem to manage to understand other people much better than his theory would predict, then it is because they 'still have available ritual and behavioural routines for interacting with people, which do not require influences about mental states' (p. 121). As he has more recently gone on to explain: 'There is a fundamental difference between the use of mentalising in discourse and the use of mentalising in theory of mind tasks. . . . During discourse mentalising is used implicitly and automatically in the service of communicating' (C. Frith, 2004, p. 388).

The second, and seemingly more open-minded response to the discrepancy between the 'real life' studies and the experimental work is to propose that more research—more *experimental* research—will need to be conducted.

But the experiment in modern developmental psychology is not a neutral arbiter. Its technology of signification is based upon, and perpetuates, a dualism between appearance and reality, where appearance can never (unless tested by the stringent procedures of experimentation) be deemed other than *appearance*. The experiment embodies and perpetuates a radical theoretical commitment:

We can almost nowhere get a direct knowledge of the complicated and the real, but we must get at them *in roundabout ways* by successive combinations of experiences, each of which is obtained in artificial, experimental cases, rarely or never furnished in this form by nature. (Ebbinghaus, 1885/1964, p. 26, emphasis added)

The paradigm of 'roundabout' experiments has become an important constituent of the implicit *theory* behind Theory of Mind.

Notes

1. Compare and contrast Tooby and Cosmides' claims about the all-seeing powers of natural selection, and the Duke of Argyll's 19th-century account of the nature

of instinct in birds, and other animals. In this case, the 'Agency' in question is the all-knowing Divine Creator:

All the knowledge and all the resources of Mind which is involved in these instincts *is a reflection of some Agency which is outside the creatures which exhibit them*. In this respect it may be said with truth that they are machines. But then they are machines with this peculiarity, that they not only reflect, but also in various measures and degrees partake of the attributes of Mind. (Argyll, 1884, p. 106, emphasis added)

2. This would include the use of 'baby diaries', regarded by developmental psychologists as an old-fashioned and amateurish technique (see Wallace, Franklin, & Keegan, 1994).
3. Reacting to what he regarded as Chomsky's anti-empiricist method, Donald Broadbent (1973) insisted that in psychology 'knowledge of human nature is to be gained by controlled experiment and observation, rather than by the intuitive exercise of the imagination' (p. 187). Indeed, since, according to Broadbent, empirical method can arbitrate between conflicting positions and avoid 'emotion polemic', the way of experimentation should be regarded as 'morally better' (pp. 206–207).
4. If this retention of the language of stimulus and response is less apparent within cognitive science, it could be because in that field the study of action is reduced to study of planning and, in turn, problem solving. Concrete activity hardly figures in cognitive science at all (Leudar & Costall, 1996).
5. Alternatively, the experimentally trained *psychologist* may, under some circumstances, be the arbiter. John O'Donnell (1985), in his account of early American experimental psychology, has described how the experimentalists did not always spend much of their careers experimenting:

For most psychologists the laboratory represented not the workshop where they spent their professional lives but the seminary in which they were originally trained. Psychologists involved with practical questions never tired of explaining that the problems with which they dealt might eventually become susceptible to experimental solutions. Yet whether this attitude could be translated in specific instances into actuality or whether it remained an unfulfilled expectation, non-experimental endeavor continued apace and partook of the aura of scientific legitimacy that the laboratory bestowed. (p. 8)

6. Note, however, that such an experiment constitutes not only the test but also the model of what 'full comprehension' should *mean*.
7. Indeed, a new line of research opened up on 'metacognition' based on the assumption that the primary problem facing young children is not that they lack the basic cognitive abilities, but instead they do not yet know when to deploy them (Flavell, 1979).
8. The terms 'preformationism' and 'predeterminism' are taken from Bremner (1994, p. 5).
9. Hardly more than a two stage theory initially, but it has subsequently become much more elaborate (e.g. Baron-Cohen, 1995).
10. 'Theory-theory' of mind could be viewed as a differentiation of Piagetian theory, alongside other 'theory' approaches concerned with the development,

not of general intelligence, but of domain-specific knowledge. The theory-theory of mind, however, does have specific links with Chomskyan pragmatics (see Leudar & Costall, 2004).

11. The philosophers in question were Bennett (1978), Dennett (1978) and Harman (1978).
12. A paradigm, that is, in Kuhn's specific sense of 'various research problems and techniques that . . . relate by *resemblance* and by modeling', and not explicit rules and theoretical claims (Kuhn, 1970, pp. 43–44, emphasis added).
13. The connection was made in a once influential textbook by McKeachie and Doyle (1966) in their introductory chapter on methodology, but it is not pursued in their chapter on perceiving other people:

Most modern psychologists agree with Watson that the evidence on which psychological theories are based must be publicly observable. However, they also agree with the introspectionists that the processes of mind are of psychological interest and that a person's report of his private experiences may be of scientific value. And they recognize, as Freud is, that a person is not aware of all the factors that are affecting his thinking and behavior. How can these apparently conflicting points of view be reconciled?

The key to resolving the problem is the distinction modern psychologists make between *data* and *inference*. . . .

Inferences are made in all sciences and in everyday life as well. Whenever you say 'He's hungry,' 'She is intelligent,' 'That man is unhappy,' you are referring to characteristics you cannot observe directly. You are using the constructs, hunger, intelligence, and unhappiness. Your constructs, like those of the psychologist, are inferences you made from your observation of people's behavior in various situations. Like the psychologist, you may be correct or mistaken in your inferences, and you discover your errors when your predictions are wrong. (pp. 6, 8)

14. It could be that critics of cognitivism and dualism lost the plot. The Cartesian dualism of mind–body may not, after all, have been the fundamental issue. Much more relevant to modern psychology are the Cartesian dualism of appearance and reality, and (as we shall see) the abstraction of mechanism as a blue-print.
15. Piaget, in his genetic epistemology, explicitly took science as an ideal example of knowledge, and was trying to establish the preconditions for reliable knowledge of the world, and hence could be taken to be setting out, in his own way, a similar continuity between cognitive development and science. Yet in many ways, Piaget, even in his conversations with his own children, sets up the child as an alien, primitive being (see Johnson, 1995).
16. There are, of course, other kinds of psychological experiment even within developmental psychology. Whereas much of the research of the 1950s and 1960s regarded what the child was observed to be 'doing' as an indirect index (as in the habituation and conditioning studies), an important and distinctive line of research opened up focusing specifically on the various activities of babies and young children, such as reaching for objects, crawling and pointing to interesting objects.
17. Although this assumption of 'lack of specification' has been challenged by a

- minority of social psychologists (e.g. Zebrowitz, 1990), they seldom follow through with an argument against the very idea that mind is unobservable.
18. As Merleau-Ponty (1962) has noted, nowhere in the *Meditations* is it mentioned that 'Descartes, and *a fortiori* his reader, begin their meditation in what is already a universe of discourse' (p. 401).

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