



Representing the Mind as Such in Infancy

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Abstract

Tyler Burge claims in a recent high-profile publication that none of the existing evidence for mental-state attribution by children prior to the age of four or five really supports such a conclusion; and he makes this claim, not just for beliefs, but for mental states of all sorts. In its place, he offers an explanatory framework according to which infants and young children attribute mere information-registering states and teleologically-characterized motivational states, which are said to lack the defining properties of the mental. I argue that Burge's claims are poorly motivated and irrelevant to the goals of developmental psychology.

1 Introduction

Burge (2018)¹ mounts a systematic attack on the widely accepted claim that infants, young children, and some nonhuman animals represent the minds of others, or at least some aspects of the minds of others. His argument is that while members of these groups may be representing what are, in fact, mental states in other agents, those states aren't represented *as* mental ones. That is, infants and young children don't represent mental states *as such*.

Burge tells us at the outset (p.409) that philosophers are agreed that there are two general marks of mentality. In order to represent a state as mental, one must either represent it as conscious, or as representational, or both. This is a strong claim. Burge thinks that *conscious or representational* is a necessary condition on mentality. As a result, he argues that someone who represents an internal state of an agent but without representing it as having either one of these properties isn't representing that state as a mental one. This is, he says, the situation of infants and young children. Most of his focus is on the representation-clause, since almost all of the evidence collected by developmental psychologists concerns children's alleged attribution of representational states like perception, belief, desire, and intention. He argues that while infants,

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toddlers, and young children attribute states to others that play many of the causal roles of such representational states, there is no evidence that the states attributed are represented *as* representational. And this is even true, he thinks, of most of the verbal theory-of-mind tasks conducted with young children, which a majority of four-year-olds can pass.

The most that the evidence supports, Burge thinks, is that young children employ what he calls the “generic” action-explanation scheme.² This explains the activities of agents in terms of information-bearing sensory states, stored information-bearing states, and motivating states that play a teleological function in the life of the organism. Burge’s parade example of a creature that satisfies the generic scheme while lacking mentality is the slug. A slug can register the presence of a lettuce leaf in the vicinity. That is, it has an internal state that is reliably caused by the presence of lettuce leaves and that plays a role in initiating and guiding behavior that issues in feeding. Likewise, slugs are motivated to move in the direction of a detected lettuce-leaf. But most people will accept that the slug lacks mental states of perception and intention. This is because the states in question aren’t genuinely representational ones, Burge thinks.

The explanatory scheme that we need to see infants and young children as deploying, if we are to explain the data collected by developmental psychologists, is a good deal more complex than that needed to explain the behavior of a slug, as Burge himself emphasizes.³ But it is still of the same generic (non-mental) sort, he argues. This more complex scheme includes not just information-bearing sensory states, stored information-states, and motivating states, but also considerations of efficiency, sensory-state anticipation, and individual differences. Importantly, it also includes sensitivity to failures of matching between states that normally function to carry information about the environment and the actual state of the environment. Still, the capacity to represent this sort of mismatch doesn’t amount to representing the state in question as representational, in Burge’s view; and so it doesn’t amount to representing mentality, either.

Burge contrasts his view with a number of other deflationary accounts on the market. Notably, he contrasts it with associationist and behavior-rule explanations of the infancy and toddler data (Perner and Ruffman 2005; Penn and Povinelli 2007; Heyes 2014a, b). He claims (correctly, in my view) that these are inadequate to account for the full range and richness of the data. His own account, while sharing the idea that young children aren’t attributing mental states to other agents, nevertheless sees them as attributing internal states that mediate between the environment and behavior, with those states interacting richly with one another to do so.

Among other competitors, Burge also contrasts his generic scheme with dual-system theories of the sort proposed by Apperly & Butterfill (2009; see also Butterfill and Apperly 2013). These postulate an innate, early-developing, mentalizing system for

² It is *generic* because it contains our adult mentalistic action-explanation scheme as a special case, or species. The two schemes can be structurally identical, but only the mentalizing one employs a (full-blown or mentalistic) notion of *representation*, Burge thinks.

³ For a review of the many and varied ways in which infants and young children can display sensitivity to the mental states of others—albeit not representing them *as* mental, if Burge is correct—see Baillargeon et al. (2016). For a review, more specifically, of the many ways in which infants and toddlers can display sensitivity to the false beliefs of others—albeit not representing them *as* beliefs, if Burge is right—see Scott and Baillargeon (2017). Note that Burge himself doesn’t challenge the reliability of these data, and says nothing about alleged problems of replication. His challenge is to their interpretation. I will return to this point in footnote #9. For discussion of the data collected with somewhat older children, see Wellman (2014).

tracking belief-states and goal-states. This exists alongside a later-developing theory-of-mind system, and continues to operate alongside the latter in adulthood. Such views have been heavily critiqued recently, on both theoretical and experimental grounds (Carruthers 2016, 2017; Elekes et al. 2016; Westra 2017; Király et al. 2018). But Burge's criticism is different: it is that the authors are mistaken in thinking that the initial system represents mental states as such at all. And Burge himself thinks that his postulated generic explanatory scheme develops (after the age of four or even later) into full-blown theory of mind, rather than continuing to exist alongside the latter.

Burge's account has recently been criticized by Jacob (2020). The latter's focus is on the adequacy of the generic scheme to explain the data. He points out, for example, that if young children are operating with that scheme, then it is hard to explain how they can display differential sensitivity to an agent who is ignorant of something versus one who holds a false belief about it. This is because in both conditions the agent can lack any opportunity to sense or track the target object. My own critique, in contrast, will focus on quite general features of Burge's proposal, especially the claim that an understanding of the representational character of (many forms of) mentality is a necessary condition for representing those types of mental state in others (Section 2), and the particular notion of representation that is in play (Section 3). In Section 4, I will briefly discuss whether possession of concepts like *FALSITY* and *PROPOSITION* are required for representing false beliefs and propositional attitudes as such. And then in Section 5 I, too, will question the explanatory adequacy of Burge's generic scheme, specifically in explaining some of the data on appearance / reality tasks that Jacob doesn't discuss.

2 Is Representation Necessary?

Burge implies that there is agreement among philosophers that representation is a necessary property of mental states like perceptions, beliefs, and intentions. He writes:

Philosophical tradition over the last century has reached a near-consensus on the most general marks of mentality. To specify a state as mental, a scheme *must* specify it either (a) as conscious or (b) as involving representation or intentionality in a distinctively mental sense—or both (a) and (b). (pp.409–10.) [Emphasis added.]⁴

It is plain that Burge intends the force of the modal “must” in his statement to be conceptual rather than metaphysical. For he immediately continues, in the footnote attached to the passage quoted above (#2), by describing the near-consensus as being about the correct *notion* of mentality. And he charges those who might disagree with him of *conceptual* unclarity. Moreover, if it were metaphysical necessity that were in question—if the property *conscious or representational* belonged to the essence of mentality—then it is quite unclear why this should place constraints on what it takes for children to represent mentality as such. For the goal of developmental psychology,

⁴ Although the necessity described here is a disjunction, in fact almost all of Burge's focus in the paper is on the representation clause. This is because the developmental data mostly concern representational states like perceptions and beliefs. I will follow him in this.

surely, is to explain when and how children acquire the same folk-understanding of the mind as normal adults, not an understanding that can only be achieved (if at all) via philosophical reflection.

Might the modal “must” in the quotation above have been a slip? Might it be said that all Burge really needs or intends is that representation is widely believed to be an *important property* of mental states like belief? But this can’t be right. For there could be no argument from a claim of this sort, when combined with the claim that young children don’t represent the representational character of mental states, to the conclusion that they don’t represent mental states (as such) at all. At the very least some additional premise would be needed, such as that representation-as requires knowledge of all significant properties of the thing represented, or something of the sort. There is no hint of such a premise in Burge’s paper. In contrast, if his claim is (as it seems to be) that being representational is a conceptually necessary property of mental states like belief, then it makes good sense that he should go on to claim that children must understand that beliefs are representational if they are to be capable of representing (conceptualizing) them as such.

Thus understood, Burge’s claim of near-consensus is false, however, or at best controversial. For many philosophers deny that there are *any* conceptually necessary truths at all (outside of logic, perhaps). This is a well-known and widely-endorsed philosophical position, tracing back to Quine (1951). Moreover, cognitive science itself makes no use of the notion of conceptual or necessary truth. Many theorists appeal to networks of belief, of course, whether conscious or unconscious (Murphy 2002; Carey 2009), just as Quine did; and some people within this framework theorize about dependency relations among the beliefs in the network, resulting in some being characterized as more central than others (Keil 1989; Thagard 1989; Sloman et al. 1998). There is no doubt, of course, that we treat representational content—or “aboutness”—as one of a cluster of properties of mental states like belief, alongside a variety of functional properties. And quite plausibly people treat this property as central. But we also have many other beliefs about mental states, particularly concerning their functional roles. Some of these have an equal claim to be regarded as central.

As the quote above illustrates, Burge appeals at the outset to an (alleged) consensus among philosophers to ground his disjunctive necessary-condition on mentality. He tells us that philosophers are agreed that to qualify as mental, a state must be either conscious or representational or both. And it is true that there has been intense focus on these two properties of the mental by philosophers over the last 40 years or so. This might easily create the impression that there is consensus that they are necessary for mentality. But in fact most of this discussion has occurred as part of a very different kind of project, not as part of an attempt to analyze the concept of the mental, nor while attempting to characterize the folk conception of the mental as such.⁵

In fact, when philosophers have debated the nature of consciousness and intentionality, the problem in each case has been to understand how the properties in question

⁵ Even those who defend representationalism about the mind in general, for example (Tye 1995, 2000; Seager and Bourget 2007)—and who thus claim that *all* mental states (including moods and bodily feelings) are representational ones—do so not as part of a conceptual analysis of the concept *mental*, nor as part of an explication of the folk conception of the mind. Rather, they advance their claim as part of their naturalizing project, attempting to reductively explain phenomenal consciousness in representational and functional terms.

form part of the natural order of the world, which is taken to be ultimately physical in nature. In connection with consciousness, there have been extensive debates about the so-called “hard” problem and the explanatory gap (Dennett 1991; Tye 1995, 2000; Chalmers 1996; Strawson 2006). And in connection with representation, likewise, philosophers have tried to understand how *aboutness* can be a natural (ultimately physical) property of physical agents (Millikan 1984; Papineau 1987; Dretske 1988; Fodor 1990). Moreover, many of those who have addressed the latter question have been especially focused on the way representation should be characterized for the purposes of cognitive science itself. Part of their interest has been in the question whether some appropriate notion of *representation* will ultimately form part of a mature science, or will rather be eliminated from science (Churchland 1979; Shea 2018).

These discussions have definitely *not* taken place in the context of trying to characterize conceptually necessary conditions on mental states, nor as part of an inquiry into what it would take to represent a mental state *as* mental. Even less have they formed part of an attempt to characterize the common-sense (or “folk”) notion of the mental. Rather, philosophical discussions have focused on these two properties (phenomenal consciousness and representational content) as especially puzzling properties that many mental states possess.

In order to find philosophical discussions of the nature of our concepts of the mental as such, one has to go back to an older literature, on functionalism and naïve theories (Lewis 1966, 1970; Putnam 1967; Armstrong 1968). Here something like a consensus did emerge (at least, if the problem of consciousness is set to one side, as for the most part Burge himself does). It is that what constitutes a state as a mental one is its place in the causal system described by our folk theory of the mind, where this can be thought of as constituted by, or embedded in, the set of platitudes (whether explicit or merely tacit) that ordinary people accept about mental states, their causal roles, and their other properties.⁶

Moreover, psychologists who investigate the development of the folk understanding of the mind have almost universally converged on some or other version of “theory-theory”, too (Wellman 1990; Perner 1991; Gopnik and Wellman 1992; Wellman et al. 2001; Baillargeon et al. 2010). They have differed over whether, or how much of, the folk theory of the mental is explicitly represented or left implicit in the processing principles employed. And they have differed over how much of the theory is innately specified, as well as over the manner in which normal development proceeds from whatever is the initial starting-state. But all agree that the upshot is a theory-like structure. Even those who have defended introspection-based simulation, rather than theorizing, as the main engine of acquisition acknowledge that the outcome of development includes a body of theory-embedded concepts and causal principles (Goldman 2006).

⁶ Seen in this light, it isn’t clear that phenomenal consciousness would figure in the folk-definition of mentality at all. For it is a term of philosophical art. The distinction between phenomenal (or “what-it-is-like”) mental states and others is pretty much invisible from the perspective of common-sense psychology. *Agents* can be conscious as opposed to asleep, of course; and likewise the folk make use of the notion of an agent being *conscious of* some thing or event, meaning that the agent perceives it. But neither of these is the same as phenomenal consciousness, which is thought by philosophers to be an introspectively-accessible—and especially puzzling—property that some mental states possess.

According to the folk-theory account of the mental, then, the representational properties of (some) mental states will be one sort of property of those states among others. It is unclear that they should be accorded any special status, let alone a conceptually-necessary one. Indeed, like theories generally (whether scientific ones, or the common-sense theories of the folk) it is doubtful that any of the properties described by the theory get picked out as strictly necessary for the theory to apply. Rather, while some properties and some causal principles may be more central to the theory than others, all have the same causal-law-like status. And systems that possess only some of these properties, or satisfy only some of the causal generalizations, will qualify as instances of the completed theory *to some significant degree*, at least. Given the likely structure of folk-psychology, then, any given individual's grasp of the theory will be a matter of degree. And we might thus expect that infants and young children acquiring the theory piecemeal over time could be said to be representing the mental states of other agents *as* mental to some significant degree.

In footnote #2 of his paper Burge addresses the possibility that infants are representing the mental states of others to some lesser degree, but says "I caution against [it]." He says that this would be to trade on conceptual unclarity, as well as begging the central question at issue. But in fact it does neither of these things. The theory-theory account of mental-state (and other) concepts is both theoretically and empirically well-founded, not based on any sort of conceptual confusion. And the central question at issue in Burge's paper is whether infants and young children represent mental states as such. But a prior question, which is the one that is at stake here, concerns what it is for *adults* to represent mental states as such. That they do so, at least in part, by deploying a naïve theory of the mental (whether explicit or implicit) seems well established.

One potential objection to this point is that the whole false-belief-task literature seems premised on the idea that being able to represent (mis)representation is necessary for possessing the concept of belief. So when it comes to belief, at least, there *is* a consensus among philosophers and psychologists that the property of representation has special status—perhaps even a conceptually necessary status. But this would be to mis-characterize why the false-belief task acquired such a central role. When Premack and Woodruff (1978) first suggested that chimpanzees have a theory of mind, three philosophers all raised essentially the same objection (Bennett 1978; Dennett 1978; Harman 1978); and it was an attempt to meet these challenges that gave rise to the false-belief task (Wimmer and Perner 1983). The objection was that if tests with chimpanzees were confined just to true-belief situations, then one couldn't really tell whether the chimp was thinking about the mind of the other at all, or just thinking about the situation or problem on its own behalf. It is only when the mental states of the target agent *contrast* with the chimp's own states that one can be confident that the former are represented. For these purposes, note, mismatching states drawn from Burge's (allegedly non-mental) generic scheme would do just as well as fully representational ones. The false-belief task was thought to be needed to test for representations of mental states generally (whether or not they are represented as such), not specifically as a test of representations of the representational character of those states.

Now admittedly, as the field progressed the false-belief task did come to be considered (at least by some of the main players in the field) to be the mark of children's understanding of what was called "the representational theory of mind"

(Wellman 1990; Gopnik and Meltzoff 1997). But this was decidedly *not* thought to be the first point at which children represent mental states as such (desires and perceptions were said to be represented at earlier ages), nor even when they first represent beliefs as such. Younger children were thought to have a different *theory* of belief—the copy theory—rather than failing to represent beliefs altogether.

In treating our representation of mental-statehood as all-or-nothing, then, Burge seems deeply at odds with the relevant sciences of the mind—specifically, the sciences that study conceptual structure in general, and the science of developmental psychology in particular. This doesn't necessarily mean that he is wrong. But it does mean that he needs to provide developmental psychologists with an argument. No such argument is given. We are merely told (incorrectly) that philosophers are agreed on the matter. Thus, even if developmental psychologists were to accept that the explanatory scheme deployed by young children is well-characterized in terms of generic action-explanation of the sort outlined by Burge, it is open to them to say that such children are nevertheless representing mental states as such *to some significant degree*.

Would this commit us, then, to saying that the slug has some degree of mentality, after all? Not necessarily. While this might be one way to go, there are other possibilities. For if a body of theory is stripped down too far there comes a point where one might want to say that it is no longer the same theory, and that it is no longer appropriate to use concepts drawn from the full theory. But the explanatory scheme employed by infants and young children—even on Burge's own telling—is much richer than the one needed to explain the behavior of a slug. It may be the richness of this scheme, and the vastly more flexible patterns of behavior that it can describe and explain, that marks the division between mindedness (or some degree of mindedness) and its lack.

Moreover, even if we set aside the point that the explanatory scheme employed by young children is much richer than is needed to explain the behavior of the slug, there are important differences between the slug's informational states and the belief-like states ascribed by children. Indeed, the reasons Burge has for denying real attributions of mentality to the slug don't obviously apply to the states attributed to others by children. This is because the slug detects its food sources by smell, and as Burge himself is well aware (2010, p.415), the sense of smell in general seems to lack the sorts of perceptual constancies that characterize vision, and that play such a large role in Burge's conception of genuine representation, as we will see in Section 3. The sense of smell is mediated by multiple chemical-specific detectors. These give rise to experiences of smell in something quite close to a one-to-one manner. There is nothing resembling the extraction of a common object underlying multiple variations in input of the sort that gets produced by the visual system (Barwich 2019).

The states represented when young children represent belief-like states much more closely resemble vision than smell, however. Indeed, one might go so far as to say that there are informational-state constancies, just as there are visual constancies, and that children represent the former even if they don't represent the latter. For even young children can attribute states with the same content (such as, *the ball is in the box*) across wide variations in the inputs that have been made available to the agent in question. The same belief-like state will get attributed despite all sorts of minor variations in the behavior of the person placing the ball in question, for example, as well as some that are not at all minor. These include the agent being *told* that the ball is in the box rather than

seeing it placed there, or such as the agent *hearing* or *touching* it rather than seeing it (Scott et al. 2010; Träuble et al. 2010). Despite such variations the agent is able to form a common belief-like state, and young children will ascribe the same belief-like state despite those varied inputs.

Even if the sorts of correctness and incorrectness that can be attributed to states in the full generic scheme described by Burge *aren't* enough to qualify those states as genuinely representational, however, one might still think that when embedded in the rich patterns of causation and interaction that can be tracked by infants and young children, children's representations of those states can qualify as representing mental states *simpliciter*. This would parallel the way in which one might think that a three-year-old child who is ignorant only of the fact that tables are items of furniture nevertheless has the concept TABLE, not to some degree, but *simpliciter*. For everyone must allow that it is possible to be ignorant of some properties of a thing while nevertheless fully possessing the concept of that thing. So it is possible that this is the right thing to say about the concept of the mental employed by infants and young children, too.

The important point to appreciate here, however, is that the issue doesn't matter for anything of scientific significance. Once we recognize the theory-like nature of people's representation of the mind, then the question of whether, and when, infants and young children have acquired enough of the theory to qualify as representing the minds of others as such, or whether and when they only do so to some degree, is of no importance for science. What matters is the nature of the starting-state for development, the nature of further learning and acquisition, and the order and timing with which different components of the theory are understood (whether explicitly or implicitly). To inquire at what point it becomes appropriate to say, without qualification, that children are representing the mental as such is to raise a question about something that is really just a matter of stipulation. Indeed, all the facts that matter for developmental science can be described without settling it.

3 What Notion of Representation?

As we noted above, a specific notion of representation lies at the heart of Burge's argument. While he allows that infants and young children employ the generic scheme of action-explanation, appealing to motivating states and informational states, he thinks that the evidence fails to support attributing to children capacities to attribute mental states as such. This is because the states involved in the generic scheme aren't represented *as representational*, despite the fact that they can be mistaken or can mismatch the state of the world they normally function to carry information about, and despite the fact that young children can reason appropriately from such mismatches. What is required, then, for a state to be a representational one? And what is required for someone to represent such a state as such?

We know from Burge's (2010) book that he eschews all attempts at reductive definition of the notion of representation, of the sort offered by Millikan (1984), Dretske (1988), Fodor (1990), and others. Rather, he thinks that the notion of representation is primitive, and is vindicated for deployment in a physical world not via reductive analysis, but rather by the central explanatory role that it plays in cognitive

science. On this I agree with him (Botterill and Carruthers 1999). We know that representations are real, and are part of the natural order, because cognitive science makes ineliminable use of them. So the kinds of representation we have reason to believe to be real are those that figure in well-developed scientific theories. But Burge goes further: he takes it to be these latter kinds of representation that people need to be capable of representing if they are to represent minds at all. This is made clear in the passage near the outset of his 2018 paper where he elucidates the notion of *representation* that will form his target throughout. The paragraph is worth quoting in full.

Representation will be our main focus. A state is *representational in a distinctively mental sense* if and only if the state is, constitutively, either itself capable of being accurate or true under certain specific conditions, or is a representational part of such a state (as the state of attributing a property is a representational part of a perception or thought). For such a capability to mark a kind of state (help making it the kind of state that it constitutively *is*), this capability must figure in law-like patterns. Common sense has a role here. But the best sign for whether being capable of being accurate or true helps mark a kind of state is whether stable science makes systematic reference to the state as having truth- or accuracy conditions in the science's law-like explanations. Nonscientific explanations in mentalistic terms are cheap. One can explain the growth of a tree or the movements of bacteria or ticks, or even movements of planets, in terms of their wants, perceptions, and beliefs. But science does not use mentalistic explanations in such cases. By contrast, perceptual psychology, which has become stable science, gives law-like explanations that refer systematically to states capable of being accurate. (p.410) [Emphasis in original.]⁷

However, it is one thing to look to science to vindicate the reality of the mental in general, and mental representation in particular; and it is quite another thing to look to science to provide the target for developmental psychology. The condition on mentality that Burge puts forward is a condition on real minds, as scientifically understood. But the task for developmental psychology is to explain how children come to represent mental states in whatever way normal adults understand them. These may or may not be the same thing.

The question whether, and when, children acquire a *scientific* understanding of the mental is a legitimate target of inquiry, of course. Similar questions have been addressed in other domains of knowledge, especially physics and biology where children's naïve theories can interfere with their grasp of important aspects of the mandatory school curriculum (Clement 1982; McCloskey 1983; Shtulman and Valcarcel 2012). But psychology and cognitive neuroscience aren't generally taught in school, so the corresponding question about scientific psychology has not seemed urgent. And certainly it hasn't been the goal of the extensive theory-of-mind literature. On the contrary, what developmental psychologists have been seeking to understand is how and when children acquire something resembling an adult (common-sense) understanding of the mind.

⁷ Note that although common sense is mentioned here, it plays no further role in Burge's argument. Or rather, the role that it *does* play is to provide a *contrast* with the sorts of representations that he thinks are the signature of true mentality. For example, on p.414 he writes: "As far as current evidence has shown, an infant's representation is like the common-sense attribution of generic agency to a snail in being silent about whether the agent has a mind."

It is instructive to note that the tests of children's capacities to represent mentality that Burge thinks come closest to being successful (while still falling short) are those conducted by Flavell et al. (1986). These tested children's grasp of the distinction between the color an object actually has and the color it appears to have. Burge then writes:

Flavell in effect recommends finding a sensitivity to perceptual constancies. Different states present something to the individual in representationally different ways. I think perceptual constancies mark the boundary between nonmental sensing and [genuine mentalistic] perception. (p.427).

Burge had argued in his (2010) book that perceptual constancies are the earliest, most primitive, form of genuine mentality. Earlier states within the visual system carry information about the environment, and can fail to match the environment, but talk of them having correctness-conditions does no serious scientific work, he argues. It is only with the emergence of perceptual constancies (such as representations of color that are invariant across differences in illumination) that correctness-conditions do real explanatory work.

Given that the notion of *representation* Burge operates with throughout restricts it to those states where cognitive science makes serious use of correctness-conditions in its explanations, we can see that Burge is here reasoning as follows: perceptual constancies form the earliest stage at which real mentality makes its appearance; so a test of children's capacities to understand perceptual constancies would be a test of their capacity to represent mental states as such. But this reasoning is flawed. The boundaries of real minds (minds as cognitive science conceives of them) may or may not coincide with the boundaries of the common-sense mind. It is children's emergent understanding of the latter that developmental psychologists seek to chart and explain.

In asking whether young children's sensitivity to the correctness or incorrectness of information-bearing states amounts to representing those states as representational, then, it is simply inappropriate to insert the cognitive-science conception of representation into the question. Rather, we need to be asking whether children appreciate the representational character of the states involved in whatever way the folk understand representation, or in whatever way normal unscientific adults do. Which prompts the question: how *do* ordinary people understand representation or "aboutness"?

I am unaware of any psychological investigations of this question. But a reasonable proxy might be to consider what philosophers have said when they debate the nature of aboutness, relying just on their intuitions and attempts to regularize and explain those intuitions.⁸ However, the upshot of these debates has not been a consensus. On the contrary, two broad models of representational content have emerged. One is a fine-grained or so-called "Fregean" account (Peacocke 1983, 1986). On this view, the content that Clark Kent has left the building is distinct from the content that Superman has left the building, even though Clark Kent *is* Superman, and even though most philosophers think that identities like this are necessarily true (true in all possible worlds). It is plain that Burge is deeply committed to some version of this view. (See, for example, his 2010, p.385.)

⁸ In the course of building their models of aboutness philosophers often employ concepts like POSSIBLE WORLD, or even PROPOSITION, that the folk may lack. But this is, arguably, to enable a rigorous treatment of people's intuitions when employing ordinary concepts like MEANING and SAMENESS OF MEANING.

A Fregean conception of aboutness is by no means universally accepted among philosophers, however. Many think that representational content is coarse-grained, constituted, for example, by sets of possible worlds (the worlds at which they are true). On this “Millian” account, the two propositions about Clark Kent / Superman are actually one and the same, and what differentiates them are merely pragmatic factors (Lewis 1986; Stalnaker 1999; Saul 2007). On this view, then, the belief that Clark Kent has left the building and the belief that Superman has left the building have the same representational content, as reflected in the fact that ordinary belief-reports will often be indifferent as to which one gets used to describe the person’s belief. (In cases where the two reports are *not* equally acceptable this is said to result from situational pragmatic constraints.)

It seems plain that if the coarse-grained conception of representation is correct (that is to say: is the conception tacitly employed by the folk), then the competencies of infants and young children characterized by Burge’s generic scheme must actually involve full-blown representations of mentality as such. For plausibly, if one understands, and can draw appropriate inferences from, the informational-content of states that normally guide agents towards their goals, but which lead to the failure of those goals in cases of informational mismatch, then one *does* represent those states as having coarse-grained truth conditions. At least, this is so provided one grasps the causal roles and modes of normal interaction of the states in question (which Burge grants young children can do). On this view, then, the generic scheme *is* (or has all the main properties of) the mentalizing scheme. (No doubt children still have much to learn about the mind.) There is no principled distinction to be drawn between them.

Section 5 will consider some evidence that young children actually operate with a fine-grained understanding of representational content, in which case they should count as representing mental states as such on any reasonable account of what ordinary adults understand representation to be. But first I want to take up the question whether, on a coarse-grained account, children need to have a detachable (separately symbolically represented, and in this sense *explicit*) conception of truth- or correctness-conditions if they are to qualify as representing mental states as such.

4 What is Required for Representing *as such*?

Consider a simple change-of-location false-belief task, of the sort that even quite young infants can solve (as young as seven months in some studies), as measured by looking time (Baillargeon et al. 2010) or anticipatory mirror-neuron activity (Southgate and Vernetti 2014).⁹ Whatever representation infants employ to index the previous location of the object to the target agent (whether this is *THE AGENT THINKS*, on the one hand, or

⁹ Although there are now well over 30 studies that provide evidence of false-belief understanding in infants and young children, using a variety of materials and methods, and coming out of a number of different labs (Scott and Baillargeon 2017), there have recently been some failures to replicate individual findings (for examples: Dörrenberg et al. 2018; Kammermeier and Paulus 2018). But Baillargeon et al. (2018) point out the methodological weaknesses of many of these attempted replications, while also acknowledging that some methods (specifically anticipatory looking) might not be reliable. And in the meantime, new studies both replicating and extending previous findings continue to be published (Király et al. 2018). Moreover, Burge himself doesn’t challenge the reliability of the data; his challenge is to its interpretation, and that is my own primary focus here too.

THE AGENT HAS STORED INFORMATION, on the other), the fact that the state in question has correctness-conditions can be (and likely is) left implicit in the updating principles that the infant employs. In the true-belief condition, for example, the infant first encodes THE AGENT THINKS: THE BALL IS IN THE BOX, and then updates this to THE AGENT THINKS: THE BALL IS IN THE BASKET when the ball is moved in the agent's presence, forming expectations for the agent's search-behavior accordingly. In the false-belief condition, in contrast, the former attribution is left untouched (not updated) when the ball is moved during the agent's absence. And then that attribution, in combination with THE AGENT WANTS THE BALL, will lead the infant to expect the agent to move toward the box.

It is often said that false-belief tasks require subjects to hold in mind both the real state of the world (the content of their own beliefs) together with the world as represented by another agent. But this is actually not true of simple change-of-location tasks. The two things don't need to be brought together in a single inference for the infant to form an expectation of the target agent's behavior. All that needs to be in play are procedures for updating or failing to update attributions of information or belief to another agent. So at this point in development the infant need not explicitly represent the information-state of the target agent as being the sort of state that has correctness conditions. Rather, sensitivity to (coarse-grained) truth-conditions is implicit in the inferential and updating procedures that the infant employs.

Now consider, in contrast, an active helping task of the sort conducted by Buttelmann et al. (2009) with somewhat older infants (18-month-olds). The infant has watched the agent play with a toy before placing it in box A. In the agent's absence the toy is switched to box B and both boxes are locked. The agent then returns and attempts to open box A without success. The infant is encouraged to help. Those who succeed move to open box B, reasoning that box B is where the desired toy really is. (In the true-belief condition, in contrast, infants mostly pass by helping the agent to open box A, reasoning that he must be wanting to open it for some other reason, since the toy is known to be in B.) In order to succeed in this task, the infant needs to do more than just track and reason from the agent's belief-state or information-state. To figure out what counts as helping, in these circumstances, the infant has to reason not only that the agent is trying to open box A because that is where he thinks the toy is, but needs to put this together with her own knowledge that the toy is in B, in order to realize that helping to open box B, and not box A, is what is needed for the agent to achieve his goal. So the infant must bring together two distinct thoughts. She must deploy in the same inference both THE AGENT THINKS: THE TOY IS IN A together with the content of her own belief THE TOY IS IN B. But this just *is* to represent, fairly explicitly, that the agent's belief as false. (An agent falsely believes that $p = \text{not-}p$ & the agent believes that p .)

What should be said about these two cases? Consider the active-helping task first. Plainly, I think, given the richness of the surrounding evidence of tasks that infants this age can pass, we should allow that the agent's mental states *as such* are represented by the infant—at least, provided a coarse-grained conception of representational content is assumed. For infants of this age appear to have a pretty good grasp of the functional roles of belief and desire, and they seemingly have a capacity for explicit representation of the truth or falsity of belief. But should we say that mental states are represented as such *only* if an explicit—detachable, symbolically represented—understanding of truth- or correctness-conditions is employed?

One view is that all representing *as* requires deployment of detachable concepts. To represent an animal *as* a dog, for example, requires deployment of the concept DOG (Fodor 2015). But Burge himself has shown us that such a view is by no means mandatory. One of the main themes of his (2010) book is that organisms can represent an objective world *as* objective without possessing an explicit concept of objectivity. Rather, the objectivity of the world is implicit in the processing principles that creatures employ, especially those that give rise to visual and other representational constancies. In zeroing in on a stable state of the world despite differences in sensory stimulation one *is* representing that state *as* objective, Burge thinks. And in that case it is open to us to say the same thing here: we can say that even the youngest infants are representing the internal states of other agents *as* representational ones possessing correctness-conditions, since they are capable of zeroing in on those conditions appropriately via the attribution principles they tacitly employ, despite wide variations in the input. And in that case even seven-month-old infants can be representing mental states *as* such (or are doing so to a significant degree, modulo their partial grasp of the functional roles of the states involved).

Similar issues arise when we consider whether infants and young children represent propositional attitudes *as* such. There is little doubt that they *have* propositional attitudes themselves, and deploy the contents of their own attitudes when solving mentalizing tasks involving other agents. It is by seeing a ball being put in a box in the presence of another agent, for example, and forming a belief with the content, *the ball is in the box*, that the infant is capable of representing the agent as thinking that the ball is in the box. Moreover, we know from the flexible ways in which infants draw inferences across a range of false-belief tasks that they are sensitive to the internal structures of the beliefs they ascribe. But it seems unlikely that they have the explicit, detachable, concept PROPOSITION. (Come to that, it seems unlikely that many adults do, either.) Still, if representing *as* is possible in the absence of the relevant concept, we might reasonably claim, on Burgean grounds, that infants can represent propositional attitudes *as* such.

I conclude, then, that if the end-state of normal theory-of-mind development is a coarse-grained conception of the representational content of belief and other attitudes, then there is a case to be made that even very young infants can correctly be said to represent representational states like belief *as such*, despite lacking explicit concepts of TRUTH, FALSITY, CORRECTNESS, or PROPOSITION. Indeed, there is a case for saying that even Burge himself should allow this, given the similarities between informational-state constancies and perceptual constancies that we noted in Section 2.

5 Appearance/Reality Tasks

Surprisingly, Burge has little to say about the various misleading-appearance and object-individuation tasks that have been conducted with infants and young children. For one might think that these involve just the sorts of fine-grained correctness-conditions that he seems to regard as necessary for real mentality. He does briefly mention the “Which Penguin?” study of Scott and Baillargeon (2009), and he does discuss, in a little more detail, the “blue-haired doll” study of Song and Baillargeon (2008). The adequacy of Burge’s handling of these findings is a central component of Jacob’s (2020) critique, and won’t be reviewed here. However, Burge doesn’t even

mention the misleading-appearance tasks conducted with eighteen-month-old infants by Buttelmann et al. (2015), which are arguably even more challenging for him to account for. (These are only mentioned in passing by Jacob, without discussion.) I will say something about these findings here.

Buttelmann et al. (2015) modelled their tasks on the classic appearance/reality experiments of Flavell et al. (1983). Four types of deceptive object were employed: a sponge that looks like a rock; a box that looks like a book; a pencil that looks like a twig; and a nail-brush that looks like a toy bath-duck. The true-belief and false-belief conditions were exactly alike, except that in the former the experimenter was present, and in the latter absent, when the deceptive nature of the object was demonstrated to the child. The object in question was then placed on a high shelf, and shortly thereafter the experimenter repeatedly attempted to reach it, but without success. At that point two additional objects were revealed to the infant. One of these shared the functional properties of the real nature of the object (e.g. a box that looked nothing like a book, or a sponge that looked nothing like a rock), while the other's properties matched the apparent nature of the object (e.g. a book that looked nothing like a box; or a pumice-stone that looked nothing like a sponge). The infant was encouraged to help. The dependent measure was which object the infant would choose to offer to the experimenter. In the true-belief condition the infant should realize that the experimenter wants an object like the real nature of the target (e.g. a box), since she knows that the thing she is reaching for is really a box. But in the false-belief condition the infant should assume that the experimenter is misled about the nature of the object, and offer the one whose functional properties match its appearance (e.g. a book), since that is what she thinks she is reaching for. This is just what the experimenters found.

How can Burge explain these data? Using explanatory resources similar to those he employs in other cases, he can say that in the false-belief condition the infants represent the experimenter as being informationally linked, via sensing, to the property of being book-like (say). Although the infant herself knows that the object is a box, that information isn't relevant in predicting what the experimenter will do. Since the experimenter is engaged in actions whose teleological outcome is obtaining something sensed as being book-like, helping, here, suggests giving her the book (which has book-like properties) rather than the box. So far so good.

In the true-belief condition, in contrast, the experimenter is not only informationally-linked via sensing to the property of being book-like, but she also has a previously-acquired stored informational-link to the property of being box-like. The challenge for Burge is to explain why the latter should be prioritized. That is, why should the infant expect the experimenter to want (that is, have as her teleologically-characterized goal-state) the box-like property in particular? For the infant should represent the experimenter as having informational-links to *both* properties (one currently, one stored). Why should the stored one dominate? Unless the infants are attributing representations of what the object *really* is versus *appears* to be, it is quite unclear why one property should be prioritized over the other.

The capacity to represent the aspectuality of perception and belief is often thought to be the hall-mark of a grasp of the fine-grained character of mental representation (Butterfill and Apperly 2013). But Burge cautions that it isn't sufficient, because states described using his non-mental generic scheme can have analogs of aspectual representation, and thus can also explain the data (p.426). It is far from obvious that this is

true, however, as we have just seen (and as Jacob 2020, argues). But it is instructive to note how Burge continues. For it is at this point in his paper that he introduces (with qualified approval) the experiments of Flavell et al. (1986) mentioned earlier, which come close to attributing to young children a capacity to attribute representations of perceptual constancies. He appears to be insisting (inappropriately, as we saw in Section 3) that to qualify as representing mentality, infants should be ascribing states whose representational character figures ineliminably in some well-established branch of psychological theory. Yet to repeat: the task of developmental psychology is to explain how infants and young children come to understand mentality in whatever way normal adults do, not in the way that scientific psychology does. Whether or not the states that infants and adults understand and attribute to others figure in some set of stable psychological laws is simply irrelevant.

6 Conclusion

Burge has mounted a systematic attack on almost the entire sub-field of developmental psychology devoted to theory-of-mind acquisition. He claims that none of the experimental paradigms employed with infants and toddlers, and indeed few if any of the verbal tasks employed with young children, demonstrate an understanding of mental states as such. I have argued that his attack is ill-motivated and ultimately irrelevant.

One reason the attack is ill-motivated is that it turns on the undefended claim that an understanding of the representational character of mental states like perception and belief is conceptually necessary for representing them. Burge does nothing to challenge the view in the field that mental states are represented, instead, via theory-like structures, in which representational properties are some among others, and according to which representation of mental states as such can be a matter of degree.

Another reason Burge's attack is ill-motivated is that even if we grant that infants need to represent the states they ascribe to others as representational, there are substantive (and much disputed) issues about the way in which common-sense psychology understands representational content (whether coarse-grained or fine-grained). Burge takes for granted (again without argument) that attributions of fine-grained contents are required. So there is nothing here to move those who disagree with him. And even if one *does* agree with him, it is doubtful whether he can accommodate all the data successfully.

Moreover, Burge's attack is irrelevant to the goals of developmental psychology. This is because it presumes that the target of inquiry is to discover when children represent *real* mental states as such—where real mental states are those that figure in the laws of a well-established psychological science. This is, indeed, a possible topic of investigation. But it is not the question that has been addressed with increasing success by developmental psychologists for the past four decades, since Wimmer and Perner's (1983) groundbreaking study. The goal, here, is to understand when and how young children approximate to the adult conception of the mind. Whether the latter conception coincides with a scientific one is simply irrelevant to the inquiry.

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