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Learning to Do Things with Words

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A thesis submitted in partial fulfilment of the requirements for the degree  
of Doctor of Philosophy in Philosophy

Department of Philosophy, University of Warwick

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*To Susan Hurley  
and Greg McCulloch*

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# Preface and Acknowledgements

This thesis started life as an account of the ‘background’ of our linguistic practices: of the structures of the (perhaps bodily or affective) knowledge that we possess, and in virtue of possessing which we are able to communicate with others even in the absence of a common language, and even where our cultural backgrounds are very different. Central to the thesis that I envisaged would have been readings of Wittgenstein’s ‘form of life’, and of Heidegger’s *Being and Time* account of the phenomenon of *significance*. Both Wittgenstein and Heidegger put forward the idea that our linguistic practices emerge against a background of the purposive activities of agents whose interactions with others and the environment that they cohabit constitute some sort of foundation for their uses of words. Heidegger made this point with typical opacity when he wrote that:

[I]n significance itself, with which Dasein is always familiar, there lurks the ontological condition which makes it possible for Dasein, as something which understands and interprets, to disclose such things as ‘significations’; upon these, in turn, is founded the being of words and of language. (Heidegger, *Being and Time*, p.121/87)

As the thesis developed, I found it hard to develop such intriguing but under-developed ideas in constructive ways. Consequently the letter of this thesis contains nothing more of the original plan than a few suggestive allusions to Wittgenstein, and no mention at all of Heidegger. Nonetheless, I think that something of the spirit of Wittgensteinian and Heideggerian approaches to language remains, and I want to start by acknowledging this intellectual debt.

Since starting the thesis I've been fortunate to receive input from a great number of people. The finished result is much better for their input, and I'd like to thank them for that. As usual, I absolve them of responsibility for any remaining flaws.

After an awkward start in the Department of Philosophy at the University of Warwick, I found myself in a rich and exciting community of philosophers, whose pioneering interest in work at the boundary of philosophy and psychology made this a natural home for me. I feel extremely privileged and thankful to have spent the past four years here. In particular, for their helpful discussions of my work, I would like to thank: Gloria Ayob, Bill Brewer, Steve Butterfill, Walter Dean, Naomi Eilan, Alexander Kelly, Hemdat Lerman, Guy Longworth, Michael Luntley, Peter Poellner, David Smith and Matt Soteriou.

Outside Warwick, there are many others from whose difficult questions and generous comments I have also benefited. In the final year of my thesis I was fortunate to develop close links with the Department of Developmental and Comparative Psychology, at the Max Planck Institute for Evolutionary Anthropology in Leipzig. During a fantastic stay there in November 2008, my work benefited tremendously from conversations with many people - in particular, Gerlind Grosse, Elena Lieven, Hannes Rakoczy and Mike Tomasello. Meetings and conversations with others have also been of great help to me. Some (but certainly not all) of these others are: Anita Avramides, Brian Ball, Gergo Csibra, Gareth Fitzgerald, Shaun Gallagher, Peter Hobson, Dan Hutto, Pierre Jacob, Julian Kiverstein, Henrike Moll, Stephen

Neale, Matt Nudds, Barry C. Smith, Martin Stokhof, and Mike Wheeler. No doubt there are others whose contributions I cannot now recall. I thank them too.

In order of appearance, I would also like to thank my PhD supervisors: Michael Luntley, Susan Hurley and Steve Butterfill.

From start to finish, Michael was an encouraging and reassuring presence. In our monthly meetings he would gently push me to consider the bigger picture, and to explain the relevance of some empirical finding (often suggested to me by Susan the previous week), that I presented to him with great excitement but the significance of which I had yet to grasp fully. These meetings were never less than stimulating, productive and enjoyable. It was also reading Michael's paper 'On the Teaching and Learning of Words' (Luntley 2007) that showed me that the philosophical issues in which I was interested could best be approached through questions about how infants come to be able to use and understand words.

Susan Hurley supervised me weekly in the first semester of my thesis, and less regularly in the 18-months after that, when she had moved to Bristol and her cancer had returned. She died at the end of my second year. Her influence on this thesis is huge. Although its contents changed a lot in the year after Susan's death, we had previously discussed many of the issues that came to prominence. Foremost among these was the role of imitation in language-acquisition, the importance of which she pushed on me long before I understood the significance of what she was saying. It was also Susan, along with Martin Stokhof, who showed me the importance of empirical psychology to the development of an account of the mind. In our lengthy supervision meetings (the average was about four hours, even after chemotherapy; the record was seven), Susan would throw at me vast numbers of empirical studies on gaze-following in chimps, scrub jays' caching techniques, joint-action in dolphins, spatial neglect, implicit prejudice, facial expression, and feral children - only to apologise

for giving me more references than I could ever read. I did read (most of) them, though, and doing so shaped the way I came to think about many of the issues discussed in this thesis.

I miss Susan and it's a source of great sadness to me that she didn't live to see me finish this thesis. I hope that she would have been proud. Along with Greg McCulloch - another one of my inspirational teachers to have died too young - this thesis is dedicated to her.

Last but not least, Steve Butterfill took on a supervisory role after Susan had died - initially in order to guide my use of empirical work, but subsequently taking on a primary supervision role. Whereas Susan and Michael had both been happy for me to focus on the bigger picture - an approach that played to my strengths - Steve was not, and in the final two years of my thesis he forced me to engage with the details of my project in a way that I had previously not done. Initially this was painful and scary, but the result is a thesis that is more focussed and detail-orientated than I could ever have imagined writing. Without doubt, the biggest debt incurred in the writing of this thesis was to Steve. I enjoyed working with him greatly and simply could not have produced this thesis without him.

Two others gave extensive written comments on my thesis. After he arrived at Warwick in 2007, Guy Longworth came close to being my fourth supervisor - reading and commenting extensively on several parts of my thesis. Brian Ball gave a detailed response to a paper that I gave at the 2009 MindGrad conference at Warwick. Their input was invaluable in shaping the final stages of my thesis and I'm grateful to both of them.

Finally, I'd like to thank the University of Warwick and my parents and grandparents for their financial support at different times of my thesis. Without that, my thesis could never have been written. Additionally, I'd like to thank my family and Jitwipar Suwangbutra for the love and support that made the writing of it a good deal easier, and Brittany Pittman



(and Aaron Kagan) for giving Jit and I free reign of her apartment in a productive April 2009 spent writing up in New York. I'd also like to thank my GP, Dr Nigel Madagan, Henriette Zeidler at the MPI in Leipzig, and Aleksandra Olszynka at the DAAD in Bonn. Their help made it relatively easy to finish my thesis in a summer in which I also had to come to terms with a depressing series of medical setbacks and plan a move to Leipzig.

Thank you all.

*Richard Moore, University of Warwick, August 2009*

## Declaration

I declare that all of the material contained herein is my own work. None of the submitted material has either been published previously, or submitted for publication, or submitted for any degree at any other institution.

Various sections of the thesis have been presented informally and at conferences. Sections from Chapter Two were presented at the *15th Annual Meeting of the European Society for Philosophy and Psychology*, at the University of Geneva in July 2007, and at the *Cognition: Embodied, Embedded, Enactive, and Extended* conference, held at the University of Central Florida in October 2007. Sections of Chapter Four were rehearsed at the *Postgraduate Conference on Phenomenology and Philosophy of Mind*, at the University of Durham in July 2008, and at the *Language, Communication & Cognition* conference at the University of Brighton in August 2008. A paper incorporating aspects of Chapters Four and Five was presented to the Department of Comparative and Developmental Psychology at the Max Planck Institute for Evolutionary Anthropology, in Leipzig, in November 2008 and at the *MindGrad 2008* Graduate Conference in the Philosophy of Mind at the University of Warwick in December 2008. Further sections of Chapter Five were presented at the *17th Annual Meeting of the European Society for Philosophy and Psychology*, at the Central European University in Budapest, in August 2009. Sections from several chapters were presented at informal graduate work-in-progress meetings at the Universities of Warwick and Bristol.

# Learning to Do Things with Words

## Abstract

Around the age of fourteen months, infants start to use and understand others' uses of words in communicative interaction. What cognitive abilities must one attribute to them in order to explain this?

In this thesis, I set out a variety of features - including knowledge of reference, of (Grice-like) communicative intentions, and of (Lewis-like) linguistic conventions - of which one would need some grasp in order to be able to use and understand words in communicative interaction. I develop an account of the cognitive abilities that grasping such features would require, and defend the plausibility of attributing such abilities to infants around the beginning of their second year of life. I argue that prior to their first uses of words, infants already have some grasp of others' minds - in particular, of when others are trying to communicate with them, and of what it is that they are trying to communicate. On the account that I sketch, infants learn how to use and understand words because they grasp the ends to which those words can be used as means, and because they are able to imitate the purposive communicative actions of their caregivers, and thereby produce utterances of their own.

# Chapter 1:

## Introduction

At around 14 or 15 months of age, the human infant starts to use words in its communicative interactions with others, and to respond appropriately to others' uses of words. Soon they can - or, at least, appear to be able to - use language to issue requests, to make observations, and to give orders, and to understand (or at least respond appropriately to) others' uses of language to do the same. Some of these forms of linguistic interaction can be seen in Michael Tomasello's record of his daughter Travis's first uses of words, recorded in the book *First Verbs*. At 15 months and 20 days, Travis asked: "whereda bottle" while looking for and demanding a bottle (Tomasello 1992, p.286). Just a month later, she looked at and pointed to her bottle, before instructing "bottle get-it" (*ibid.*, p.305). These are just two of many examples. What are the cognitive abilities that one would need to attribute to an infant to explain such uses of language in communicative interaction? This is the central question of the thesis to come.

To ask about the cognitive abilities that one would have to attribute to infants to explain these communicative interactions is to ask, first of all, after the nature of the cognitive

abilities that anyone - infant or adult, ape or human - would need to possess in order to be able to use words communicatively. But what are these cognitive abilities? To answer this question is, perhaps, the task of philosophers. First it requires identifying features of communication - for example, word reference, the property of meaningfulness, or linguistic conventions - and specifying carefully what those features are, what contribution they make to our ability to use words to communicate. Only later, with these roles in view, is it possible to say something about the sorts of cognitive abilities that would be necessary for having or grasping these features, in whatever manner it is that they need to be had or grasped for communication to be possible. For example, one couldn't give an account of the cognitive abilities necessary for producing a meaningful action, or grasping the meaning of another's action, without first having some account of what it is for an action to be meaningful. The greater part of this thesis - particularly in chapters Four and Six, with the discussions of Grice and Lewis - will be given over to explicating and evaluating philosophical theories of particular aspects of using words in communicative interaction.

In addition to the theoretical project of this thesis, there is also an empirical aspect. The account of cognitive abilities that emerges here as necessary for word-use is not intended to be an exercise in abstract theorising. It's also intended that the story that emerges should provide some genuine insight into how it is that infants actually do come to use and understand words.

Not all philosophical accounts of the knowledge that one might use to acquire knowledge of others' linguistic utterances have this aspiration. For example, in the opening paragraph of his essay 'Radical Interpretation', Donald Davidson asks two questions: what could we know that would help us to know what, on occasion, another's words meant? And how could we come to know this? He responds:

The first question is not the same as the question [of] what we do know that enables us to interpret the words of others. ... The second question, how we could come to have

knowledge that would serve to yield interpretations, does not, of course, concern that actual history of language acquisition. It is thus a doubly hypothetical question: given a theory that would make interpretation possible, what evidence plausibly available to a potential interpreter would support the theory to a reasonable degree? (Davidson 1973, p.125)

Here and elsewhere, Davidson makes it clear that his theory of Radical Interpretation is not intended to constitute a theory of how interpreters actually do come to understand utterances in an unknown language. While I agree with the distinctions that Davidson draws, my primary interest is not just in the development of an abstract theory of what we *could know* that would make interpretation possible. It's intended that what should emerge is not just be an account of *a mind* that could, in theory, learn to use words in communicative interaction. Rather, it should be an account of *the infant's mind* - and of what the infant *actually knows*, in virtue of which it is able to learn to use words communicatively. In this case, although space and time constraints make it impossible to defend the empirical claim in full, the story that I tell should be at least prima facie plausible as an account of the cognitive abilities in virtue of possessing which infants do learn to do things with words.

This motivation imposes a further constraint on the account that is developed. If the cognitive abilities deemed necessary by philosophers for acquiring the ability to use words cannot be attributed appropriately to infants around the beginning of their second year of life, then they could not be the abilities that are actually employed by infants in their learning. In this case, if the empirical constraint is to be met, it's desirable that the abilities taken to be necessary for word-use are abilities that can be attributed to infants around the beginning of the second year of life. To this end, I include quite extensive discussions of empirical evidence about what it is that infants can do.

In relating philosophical approaches to aspects of word-use to empirical studies of infant cognitive abilities, I assume a fairly orthodox account of the relationship between

philosophy and psychology, according to which what the philosopher provides is a conceptual characterisation of a particular psychological state, the presence or absence of which in a subject can then be tested by the psychologist. This approach is just one way to address issues about the relationship between philosophy and psychology. More could (and should) be said about this relationship. However, since this is not a thesis about the relationship between philosophical and psychological approaches to the study of the mind, I will not undertake further discussion of those issues here.

Before turning to the details of the thesis, more needs to be said about what exactly is the explanandum of a theory of communicative interaction. The project is to give an account of the cognitive abilities necessary for using and understanding words in communicative interaction. But what is it to use and understand others' uses of words in communicative interaction?

Some examples might best illustrate the phenomenon that needs to be explained.

1. Suppose that you want me to pass you the salt, which is next to my plate. You ask me: "Are you done with the salt?", with the intention that I should thereby grasp that you would like the salt, and that you would like me to pass it to you.
2. I ask you "What time is it please?", with the intention that you tell me the time.
3. After falling in the street I cry out "Help!" in order to get help.
4. An infant reaches towards a bottle out of her reach, and calls out "Bottle get-it!" in order to get it.
5. I tell you that "It's raining", with the intention of letting you know that you should take an umbrella.

In each of these examples, a speaker performs an utterance in order to *do* something - to *make something happen*. Typically this something will be the elicitation of some particular response from an interlocutor or, failing that, from anyone who might happen to hear the speaker's words (as is the case in 3.). The utterances are therefore purposive: in each case,

what the speaker intends to do by speaking is to achieve a certain goal - to obtain the salt, to find out the time, to get the bottle, or to inform her interlocutor of the need to take an umbrella. In these cases, we'd say that a speaker's communicative goal had been understood (and so was successful) when others had grasped what it was that she was trying to communicate by speaking - for example, the fact that she needed help or wanted the salt.

These intentions are what Austin called "perlocutionary" intentions - literally, those intentions that one seeks to fulfil through speaking. A perlocutionary intention is the thought, action or other form of response that a speaker, in speaking, seeks to bring about. It may be that having perlocutionary intentions is not in any sense necessary for speaking meaningfully. Chapter Five includes discussion of cases in which speakers use words in communicative interaction without having any perlocutionary intentions. However, in a great many fairly representative cases of communicative interaction, a speaker produces an utterance with a perlocutionary intention. This is uncontroversial.

In some cases one might grasp a speaker's perlocutionary intention despite having only an imperfect - or even non-existent - grasp of the meanings of the words that she uttered. For example, I might grasp your pleas for help even if I don't speak the language in which you're uttering. Additionally, one might sometimes fail to grasp a speaker's perlocutionary intention in spite of knowing the meaning of her words. This happens when one fails to grasp why a speaker said what she did. What is uncontroversial is that by knowing the meaning of an utterance, one is better placed to grasp the perlocutionary intention that motivated its speaker's speaking. By using certain combinations of words, speakers can provide clues for their hearers about the perlocutionary intentions that they have in speaking, and by picking up on these clues, hearers are able to determine what it is that speakers are endeavouring to communicate.



The scenarios described above give us a better idea of what it is that an account of word-use in communication should explain, by providing us with some criteria for communicative competence. A speaker knows how to use words when she knows how to use the words of a language to communicate her communicative goals - typically in the form of perlocutionary intentions - and she becomes an understander of words when she is able to grasp the goals that others desire to communicate by speaking. The art of using words in communicative interaction might, then, be something like an art of using words (and ultimately, if not in the earliest cases, their arrangement) to best indicate what it is that one is trying to communicate; and of being receptive to the clues provided by others about what it is that they are trying to communicate. The first target of the thesis, then, should be an explanation of the cognitive abilities that are required by infants to do this. In the following chapters I identify some of the different ingredients of verbal communication of which it would be necessary to have some grasp in order to become a user or understander of words, and consider which cognitive abilities must be attributed to infants to explain this grasp. Additionally I address empirical questions about whether such abilities can be attributed to infants around the beginning of their second year of life.

Note that so far the discussion has been of abilities in the plural. This may need some clarification. For a start, one might wonder whether being a user of words is the same ability as being an understander of words, or whether these are two separate abilities. One might think that for an infant to qualify as genuinely understanding, it is necessary that there should be occasions on which it both comprehends the meaningful utterances that it hears and produces appropriately some meaningful utterances of its own and that these abilities of comprehension and production are two aspects of but one capacity of linguistic understanding. This view has been defended by John Campbell<sup>1</sup> and it's a view to which I'm sympathetic. However, I will not defend it.

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<sup>1</sup> J. Campbell: 'Knowledge and Understanding', *The Philosophical Quarterly*, January 1982, p.18.

It's not impossible that there might somewhere be an infant that could understand utterances but not produce any of its own, but it would be surprising if the reasons for this weren't relatively trivial. For example, a mute child might grasp others' utterances but would presumably not be able to reproduce them. Here what allows the hearer to grasp but not reproduce utterances is typically some physical or psychological scar, and not any deeper lack of knowledge of how to produce utterances of their own.<sup>2</sup> It would be surprising if the mute infant couldn't produce utterances in a gestural language - suggesting that it didn't lack a general ability for producing utterances. This is consistent with Campbell's position. For the purposes of this thesis, though, it isn't necessary to argue for the claim that only those capable of using *and* understanding words would count as word-users. This is because the goal here is simply to explain the empirical fact that typical infants are both users and understanders of words. While it's an empirical fact that users of words are almost invariably also understanders of words, this may not be conceptually necessary. In this case, it's simply not necessary to defend Campbell's conceptual claim. In what follows I'll generally refer to using and understanding as if they were two abilities. Not least, this is because, on the theory that emerges, understanding words is developmentally prior to and enables using. (This shouldn't rule out the possibility that infants sometimes reinforce and

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<sup>2</sup> The distinction that I have in mind here is analogous to Chomsky's distinction between performance and competence with respect to a language-user's knowledge of syntax:

We thus make a fundamental distinction between *competence* (the speaker-hearer's knowledge of his language) and *performance* (the actual use of language in concrete situations). (Chomsky 1965, p.4)

On Chomsky's account, it's a language-user's knowledge of syntax that explains her ability to use language (or 'perform') in communicative interaction, but there may be features of language of which she has knowledge but cannot use for a variety of reasons irrelevant to the question of what she knows - including "memory limitations, distractions, shifts of attention and interest, and errors" (*ibid.*, p.3).

perfect their understanding of words through using them - for example, by uttering partly or imperfectly understood phrases and observing the results.)

Whether or not using and understanding is a single ability or two, there's no doubt that there are many aspects of being able to use and understand words, which might require very different sub-abilities. In the chapters that follow, I'll discuss some abilities that have been taken to be necessary for acquiring the ability to use words in communicative interaction - but it would not be possible to discuss all such abilities. Consequently the abilities deemed necessary for using and understanding language in this thesis can't be taken to be sufficient for doing so.

Perhaps the most obvious absence will be of any discussion of syntax. The study of syntax concerns the rules for combining the words of a language into sentences and our knowledge of these rules. There's no doubt that knowledge of syntax (or, at least, something approximating to knowledge) is fundamental to some communicative abilities. For example, it's presumably in virtue of knowing the meanings of individual words and the rules for their composition that speakers are able to produce and understand sentences that they have never previously heard. While recognising the importance of syntax, I will say very little about it here and nothing at all about infants' understanding of syntax. Nor will I take a stand on issues about whether our knowledge of syntactic rules is innate or acquired. I take it that what I say is consistent with either possibility, and that the area of investigation here is of sufficient independence that nothing I say hangs on the answer either way.

It's because I won't have much to say about syntax that I will try to avoid making claims about infants' acquisition of *language*, over and above their acquisition of the ability to use words in communication. It may be that knowledge of syntax is necessary for knowledge of language. However, one could surely know how to use a sentence to communicate one's perlocutionary intention, without knowing how to recompose the elements of that sentence

into novel sentences. In this case, one would have some (perhaps limited) knowledge of how to use words in communicative interaction - but without having knowledge of syntax, and so without having knowledge of language. Since I will say nothing about infants' knowledge of syntax, it would be improper to present the account here as a story of language-acquisition, rather than merely a story about *some aspects of* language-acquisition. Thus, although I may talk about acquiring the ability to use the words of a language, or linguistic conventions, where what is being described are abilities necessary for word-use, I will endeavour not to use the word 'language' - unless, of course, the discussion concerns issues of language-use or language-acquisition more generally.

In addition to saying nothing about infants' knowledge of syntax, I will say nothing about either their ability to parse heard sounds into distinct words, or about their ability to see the world as consisting of distinct objects. Presumably such abilities are necessary for word-use. One couldn't, for example, learn a name for an object, if one could neither hear the word as distinct from those surrounding it nor perceive the object as being distinct from the other objects in its environment. There may be many further abilities that play supporting roles in infants' acquisition and use of language but these will not be discussed either.

One ability that is necessary for being able to use words in communicative interaction is knowledge of reference. To know the referent of a word is to know which object it picks out. Typically, if one couldn't grasp the referent of a word then one would struggle either to use or grasp others' uses of that word. In Chapter Two: *Associations*, I address Daniel Dennett's associationist account of infant cognition and language-acquisition, according to which the infant's ability to acquire language can be explained by attributing to it only the "initially stupid practices" of association and repetition. (Dennett's claim is presented as a thesis about language-acquisition in general, and not merely one about word-use.) One claim typical of associationist accounts like Dennett's is that infants learn the referent of a word by associating the sound that they hear with the object at which they are looking at

that time. I argue that such an account fails to explain how it is that infants could learn the referents of words. Furthermore, empirical evidence shows that infants are able to learn the referents of words in circumstances that are not easily explained on associationist accounts of reference. In this case, we have adequate grounds for concluding that a richer set of cognitive abilities than association and repetition is operative in infants' learning of word reference.

In Chapter Three: *Referential Intentions*, I consider a richer account of the cognitive abilities that infants possess, and in virtue of possessing which they are able to grasp reference. According to Paul Bloom, infants grasp the referents of words because they grasp others' 'referential intentions'. While sympathetic to this view I argue both that the notion of a referential intention is un-explicated and that knowledge of referential intentions could not be sufficient for understanding others' uses of words, even if it is necessary.

In Chapter Four: *Communicative Intentions*, I develop the claim that in order to understand others' utterances, infants would need to grasp their communicative intentions, where this means perlocutionary intentions. Bloom's referential intentions are instances of perlocutionary intentions, but since utterances with the same referent can be made in pursuit of very different goals, communicative intentions cannot be explained just as referential intentions.

I develop an account of what it is to act with or grasp communicative intent, taking as my starting point Paul Grice's analysis of speaker's meaning (which identifies a speaker's communicative intention with her perlocutionary intention). Grice's account constitutes a valuable starting point for discussion of the nature of communicative intentions, because it contains both a specification of what it is to produce or grasp a meaningful action (that is, to act with or grasp a communicative goal), and of the cognitive abilities that this would require. The Gricean account of the cognitive abilities necessary for acting with or grasping

meaning has sometimes been thought too demanding to be plausibly attributed to young infants. For example, Breheny (2006) has argued that infants could not grasp or act with Gricean intentions because they lack a concept of belief. I argue that Gricean intentions need not, as Grice himself had thought, be belief-involving and I defend the claim that infants of 15 months can act with and grasp communicative intentions of this revised Gricean character.

In Chapter Five: *Illocutionary and Perlocutionary Intentions*, I discuss two of John Searle's objections to the Gricean account of the communicative act. First, Searle objects that the Gricean account of meaning fails to account for the role of linguistic conventions - for example, words and sentences - in communication. I argue that while this may be true, the Gricean account still provides us with an adequate model of non-conventional communication that can be appropriated for the provision of an explanation of how infants come to acquire knowledge of linguistic conventions. Second, Searle objects that whereas on the Gricean account, communicative intentions are perlocutionary intentions, in fact the only intentions that must be had in the performance of speech acts are illocutionary intentions. Illocutionary intentions are not intentions to make one's interlocutor do or think something but intentions to be understood as having performed a speech act with a certain force - for example, to have made an assertion or given an order. I argue that while it's true that some communicative intentions are illocutionary intentions, there are insufficient grounds to attribute to young infants communicative intentions of this sort. In this chapter I also develop the claim that infants come to grasp the meaning of others' words because on occasion they grasp others' perlocutionary intentions, and because they take the meaning of the words that they hear and the speaker's message (which is closely related to her perlocutionary intention) to coincide.

In the final chapter, Chapter Six: *Imitation and Convention*, I return to consider the role of conventions in linguistic communication. It's possible to communicate without any

knowledge of linguistic conventions. However, since the words and sentences of a language are conventional - in the sense that countless arbitrarily different sets of marks and sounds might have been used with just the same meanings - in order to participate in linguistic communication, it's necessary that infants acquire some (if not much) knowledge of linguistic conventions.

In this chapter I consider David Lewis's account of convention and the cognitive demands that this account places on participation in conventional activities. Lewis argues that one can participate fully in a conventional activity only if one knows that one's activity is conventional. I argue that this is too strong, and that one can have knowledge of linguistic conventions that is sufficient for participation in linguistic communication, without knowing that one's activities are conventional. With this barrier to participating in conventions removed, I argue that one could come to participate in conventional activities in virtue of being able to *imitate* the conventional activities of others. Imitation is the ability to observe and act with not just the intended goal of another's action, but also the means that is employed in pursuit of that goal. Since there is robust empirical evidence that infants around 14-months are sophisticated imitators of others' actions, it becomes plausible to conclude that they acquire knowledge of linguistic conventions imitatively.

Note that the claim here is not that imitative abilities are necessary for learning to use the words of a language communicatively. Rather, it's necessary that infants be able to use words, and imitation is one way (but perhaps not the only way) in which they could learn to do this.

On the picture that I defend, two abilities are necessary for coming to use and understand others' uses of words: grasping the perlocutionary intentions with which others utter (which will typically require some knowledge of the speaker's intended reference, but which is not merely knowledge of reference), and knowing how to use words and sentences to

communicate one's own perlocutionary intentions. Knowledge of the latter is grounded in knowledge of the former. At least in some cases, infants can grasp others' perlocutionary intentions without having any prior knowledge of the meanings of the words uttered by the speaker. When grasping others' perlocutionary intentions, infants take their words to be means to the achievement of these goals: they grasp that those words can be uttered in order to fulfil perlocutionary intentions like the speaker's. This enables infants to grasp others' utterances of these words and, additionally, to speak for themselves by imitating the utterances of their peers. Of course, the nature of communicative intentions that infants might grasp prior to acquiring knowledge of linguistic conventions may be quite limited, but as their knowledge of the purposes with which words can be used increases, so does the complexity of the utterances that they are able to grasp and, in turn, reproduce.

The take home message of the thesis is that infants learn to use and understand words in communicative interaction because prior to their being able to use and understand words, they know something about the mental lives of others. However, what they know need not be others' beliefs states. (As is discussed in Chapter Four, whether or not infants grasp others' beliefs is controversial - although there is recent evidence that suggests that they do.) Rather, they know about others' purposive activities - and, in particular, their communicative activities: the perlocutionary intentions with which others utter. This characterisation of the infant's mind is very different from the one described by Daniel Dennett in Chapter One. On his associationist account, infants learn the meanings of words by associating heard sounds with seen objects, without having any prior insight into what it is that speakers are doing when they speak. Here infants merely *mimic* others' linguistic activities - reproducing the sounds that they make, without having any understanding of the reasons with which others' utter. On the account offered here, by contrast, even very young infants know something about what others are doing with the words that they utter. They don't just mimic what they hear, but *imitate* it.



## Chapter 2: Associations

In Chapter One, I posed the question: what are the cognitive abilities that must be attributed to infants to explain their ability to use and understand others' uses of words in communicative interaction? In the chapter to follow, I want to consider attempts to answer this question by attributing to infants only *associative abilities*. On the associationist view, infants' uses of words in communication need not be explained by attributing to them mental states like *thought*, *knowledge* or *understanding*. (In this case, if one thought that 'cognitive' meant 'thoughtful', the answer to the question posed at the outset would be: no cognitive abilities need be attributed to infants, only some non-cognitive ones.)

Precisely what exactly is being denied by the proponent of associationist accounts of learning is not always clear. For example, part of what motivates Daniel Dennett's associationism is the thought that we ought not to attribute *minds* to those who lack language. However, the term 'mind' is sufficiently broad that it's far from obvious what it is that one would be claiming by saying of an infant or animal that it lacks a mind. Given this lack of precision, I propose to spend only very little time on the associationist's negative

claim. For the purposes of the project in hand, a more interesting question can be found in the associationist's positive claim. This is the claim that one could explain how it is that an infant acquires a language, and so comes to be able to use words in communicative interaction with others, from the starting assumption that infants are capable of associative learning and nothing more.<sup>3</sup> The question that I shall pursue in this chapter is whether this claim is plausible. If one could not explain infants' acquisition of these abilities by attributing to them only associative mechanisms, then we have reason to think that language-acquisition must be explained by attributing to infants a more sophisticated set of cognitive abilities at the start.

In order to set out something more of the associationist's background story, I start off by considering just one philosophical concern that has motivated associationist accounts of language acquisition.

A fairly common historical view - defended, among others, by Daniel Dennett, Michael Dummett, and Donald Davidson<sup>4</sup> - has been that we should not attribute rich cognitive abilities - for example, the having of a *mind* or the ability to *think* - to those who lack language. On Dennett's view, which I'll explore here, only speakers can be thinkers. Our ability to think is made possible only because we have acquired a language that enables us

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<sup>3</sup> Note that Dennett does not distinguish between acquiring the ability to use words communicatively and acquiring a language, as I did in the previous chapter. Although, like me, he says nothing about syntax, his account is presented as an account of language-acquisition. Of course, since learning to use words communicatively is one aspect of language-acquisition, any theory of language-acquisition should also explain the possibility of using words in communicative interaction.

<sup>4</sup> In each case, this claim has been defended across many papers. Some representative samples are: Dennett 1991, 1994 and 1996; Dummett 1993a - especially essay 13, 'Thought and Language'; and several of the essays collected in Davidson 2001 - especially the 1982 essay 'Rational Animals', and the 1997 essay 'The Emergence of Thought'.

to think. In *Kinds of Minds* (hereafter *KoM*), Dennett spends some time sketching out, albeit only speculatively, an account of the cognitive abilities that might explain an infant's acquisition of language.

Dennett's argument for the claim that we should not attribute minds to those without language stems from his observation that it's primarily through language that we come to know about the minds of others. It's because we can *talk* to one another, because of the evidence presented to us in conversation with our peers, that we grasp that our peers are subjects - that is, creatures possessed of minds.

We human beings share a subjective world - and know that we do - in a way that is entirely beyond any other creatures on the planet, *because we can talk to one another*. ... Conversation unites us. (*KoM*, p.12; my italics)

Dennett argues that where we lack the evidence of minds that we acquire in conversation with others, we should err on the side of caution and not assume that others - for example, animals and pre-linguistic infants - have minds. Although we don't *know* that such language-less creatures lack minds, it would be intellectually careless to assume that they do, in spite of our lacking evidence for this claim (*ibid.*, p.212).

Dennett recognises that such caution does not come naturally to us. When watching one year old infants, he observes, "we have difficulty even entertaining the hypothesis that we are not watching a sentient being" (*ibid.*, p.123). Nonetheless, he continues, history has taught us that cases of "surprisingly mind-like behaviour can be reproduced by relatively simple, mechanical, un-mind-like control systems" (*ibid.*). As will become apparent, he thinks infants' uses of language one such case. He also gives a second example. Roboticists at MIT working under Rodney Brooks and Lynn Andrea Stein have successfully built a robot, Cog, the eyes and body parts of which move "in unnervingly human ways" (*ibid.*, p.21). These make it tempting for us to think that, in interacting with Cog, we are interacting with a fellow human being. Cog is not human and we know this, but the fact that it comes so

naturally to us to treat him as if he were should remind us that our instincts here are unreliable. Because our instincts are misleading, in cases where we lack the evidence to support attributions of minds to others, we ought not to do so on the basis of sentimental or intuitive grounds like the fact that it comes naturally to us to think of our young children and pets in the same sorts of ways that we think of ourselves. Since the primary evidence for attributing minds to others is linguistic, we ought to refrain from attributing minds to non-linguistic creatures.<sup>5</sup>

What Dennett thinks possessing a mind consists in is not always clear. A variety of claims that he makes shed light on what he envisages, though. One proposition to which Dennett is

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<sup>5</sup> Note that Dennett's position here (that is, in *Kinds of Minds*, published in 1996) is potentially inconsistent with views that he has expressed elsewhere - in particular, in the 1994 paper 'The Role of Language in Intelligence'. There, in his discussion of the cognitive abilities of adult language-less Deaf-mutes, Dennett acknowledges that such individuals are capable of "remarkably sophisticated" and thought-like abilities (Dennett, 1994, section 6). However, attributing such abilities to these individuals does not undermine the claim that only speakers can be thinkers because, according to Dennett, they are "still the beneficiaries of the shaping role of language" (*ibid.*). This is because even where adults lack the signed or spoken symbolic languages in terms of which (he thinks) language-using humans do our thinking,

we simply don't know - yet - what structures in their brains are indirect products of the language that most of their ancestors in recent millennia have shared. (*ibid.*)

That is, to the extent the evolution has been shaped by language-use, even language-less Deaf-mutes are not really language-less. In a relevant (neurological, but otherwise unspecified) sense, they do have language, and in virtue of this language their impressive cognitive sophistication can be explained. Dennett seems to be guilty of making inconsistent claims here: (i) the claim that the ability to think of any particular human is imposed on it by its acquisition of a language, and (ii) the claim that any particular human's ability to think is a consequence of its having a human brain, irrespective of whether or not it has acquired a language. Beyond pointing out this inconsistency, I won't discuss it further.

clearly committed is that the ability to *think* - by which he means the ability to consider objects "in general" or "in themselves", in abstraction from any context of activity - comes only with the acquisition of language. This is because thought requires concepts, which Dennett describes as the "tools of thought", and concepts come only with language. "Words," Dennett writes, "are the prototypes or forebears of concepts" (*Kinds of Minds*, p.200). Another part of what Dennett has in view in his argument also includes attributions of *consciousness* to those lacking language. Consciousness is the theatre in which emotions and desires are felt, where beliefs are entertained and evaluated, and where intentions are formed. Dennett argues that independent of evidence delivered through language, we ought not assume that anything has an inner life of the sort characterised by consciousness. There's not obviously, he thinks, anything that it's like to be an infant.

These are interesting and provocative claims. However, we can certainly agree with Dennett that it *would* be intellectually disreputable to attribute sophisticated cognitive abilities to non-linguistic infants on the basis of sentiment, and still reject his strong conclusions. If this were the only reason we had for attributing to infants a rich mental life, we ought not to do it - but against Dennett, it should be recognised that often when we attribute rich cognitive abilities to infants, we are not motivated by sentiment. We sometimes attribute to infants a rich mental life on the grounds that such ascriptions seem to be necessary if we are to be able to explain how it is that infants can do things that we know that they can do. Linguistic communication is a good example of this. We know that by the time they reach 24-months, infants are very good at using words to communicate. However, it might be that we can explain this fact only if we attribute to them a prior set of cognitive abilities in virtue of possessing which they could have acquired the ability to use words communicatively. If our best theory of the cognitive abilities that one would need to possess in order to do this requires possession of abilities X, Y and Z, then we have reason to attribute to infants the abilities X, Y and Z. If, in addition to this and by using the methods of experimental psychology, we could adduce independent empirical evidence for

attributing to infants the abilities X, Y and Z posited as necessary by the theory, then we might think we could have very good reason to attribute to infants such abilities as these. While it's true that our ability to use words to communicate gives us a wonderful insight into the minds of others, the techniques of experimental psychology can also be used to the same end.

A further cause for dissatisfaction with Dennett's negative account is the unfocussed terms in which he describes the cognitive abilities for which we lack evidence in non-linguistic creatures. We don't, as Dennett often acknowledges, have a clear view of what's really at stake when we ask questions as general as whether animals have minds, or whether they are conscious. Similarly, questions like: "How might an infant's being *conscious* enable it to acquire a language?" or "How might an infant's being able to *think* enable it to come to use words in communicative interaction with others?" would be made difficult to answer by the lack of focus in the questions, since it remains unclear what it is to have a mind, or to be conscious, or to be able to think. The view that only speakers can be thinkers imposes certain constraints on the nature of the abilities to which one could appeal in order to explain infants' first uses of words. However, without a more precise understanding of the abilities the ascription of which Dennett wants to withhold from infants - thought, consciousness, and having a mind, for example - we don't know which possible theories of acquisition would be ruled out by the hypothesis that non-linguistic individuals lacks these things.

In order to provide a detailed account of the abilities that make possible the feat of word-use, it will be necessary to paint with a much finer brush than concepts like *thought*, *mind* and *consciousness* allow. Many of the abilities that will be discussed in detail in later chapters - for example, knowledge of others' focus of attention, or of their communicative intentions - are of a sort that aren't obviously either 'thoughtful' or 'mindless', or 'conscious' or 'unconscious' on the Dennettian account. Given this, I think it's better to

steer clear of such terms altogether and I won't say much more about the nature of the cognitive abilities that Dennett wants to withhold from infants.

It's because these terms are too broad that Dennett's negative thesis is unfocussed. By contrast, Dennett's positive story contains a clear statement of the precise set of cognitive abilities that he thinks are sufficient to explain infants' acquisition of language. This makes for a much better starting point for the discussion of the central thesis question.

Because he is committed to the view that pre-linguistic infants lack minds, Dennett sets out a model of language-acquisition that does not make strong cognitive demands on the learners of language and that does not make reference to any of the cognitive abilities on his danger-list. In particular, he argues that infants' emergent ability to use words can be explained without attributing to them any cognitive abilities more sophisticated than the "initially stupid practices" of mechanistic *association* and *repetition* ((*KoM*, p.198). Association is the tendency to associate heard words with stimuli in the presence of which they were uttered. Repetition is the tendency to repeat previously heard utterances in situations like those in which they were previously heard. Neither of these processes assumes that a pre-linguistic infant has any sort of mental life, over and above the tendency to respond to and reproduce environmental stimuli (some of which stimuli will be word-like sounds). Dennett's claim is that although this starting point is very austere, it's nonetheless sufficient to explain how it is that infants come to be able to use language - and so one aspect of language, namely words - in communicative interaction. Whether this claim is plausible will be the subject of the rest of this chapter.

#### The infant as a mindless associative engine

According to Dennett, the infant's first steps on the way to language come when it hears, without understanding, the words used by its caregivers. The meanings of these words come to be understood as a result of the child's repeating them to itself in an internal

commentary on its activities that Dennett takes to be fundamental to its development.

Through the act of repeating others' words to itself as it moves about the world, the infant builds "up recognition links and association paths" (*ibid.*, p.197) between the sounds it hears and the worldly contexts in which they occur. I quote from Dennett at length:

Consider what happens early in the linguistic life of any child. "Hot!" says mother. "Don't touch the stove!" At this point, the child doesn't have to know what "hot" or "touch" or "stove" means - these words are primarily just sounds, auditory event types that have a certain redolence, a certain familiarity, a certain echoing memorability to the child. They come to conjure up a situation type - stove-approach-and-avoidance - which is not just a situation in which a certain prohibition is typically heard but also a situation in which a mimicking auditory rehearsal is encountered. Crudely simplifying, let's suppose that the child acquires the habit of saying to itself (aloud) "Hot!" "Don't touch!" without much an idea of what these words mean, voicing them merely as an associated part of the drill that goes with approaching and then avoiding the stove - and also as a sort of mantra which might be uttered at any other time. After all, children are taken with the habit of rehearsing words they have just heard[.]

... This process could have the effect of initiating a habit of which we might call *semi-understood self-commentary*. The child, prompted initially by some insistent auditory associations provoked by its parents' admonitions, acquires the habit of adding a soundtrack to its activities - "commenting" on them. The actual utterances would consist at the outset of large measures of "scribble" - nonsense talk composed of wordlike sounds mixed with real words mouthed with much feeling but little or no appreciation of their meaning, and a few understood words. There would be mock exhortation, mock prohibition, mock praise, mock description, and all of these would eventually mature into real exhortation, prohibition, praise and description. (p.196-7)

From a starting point of mere association of words and context, through a process of labelling - repeating the words of its caregivers in the contexts of their utterance - the child acquires the ability to use language to perform a variety of the communicative acts -



exhorting, prohibiting, praising and describing - of the sort described in the opening chapter.

Dennett emphasises that it is this "stupid" process of non-comprehending repetition that sets the child on the path to language, and by extension, to thought. This works because, through the labelling process, "nodes of saliency" are created in the infant's memory. These nodes are, Dennett says, concepts of the labelled objects. With the creation of concepts, features of the world are drawn into focus against the background of their surroundings - making those objects, and the words that label them, familiar to the infant in a manner that prefigures its coming to think about them.

I'm suggesting that it's such initially stupid practices - the mere mouthing of labels, in circumstances appropriate and inappropriate - that could soon turn into the habit of representing one's own states and activities to oneself in a new way. As the child lays down more associations between the auditory and articulatory processes on the one hand, and patterns of concurrent activity on the other, this would create nodes of saliency in memory. (*ibid.*, p.198)

#### Associative learning mechanisms

Dennett doesn't say much about how the associative mechanism at the heart of this word learning process is supposed to work. However, associative accounts of learning have been developed in much more detail by others. The central idea is that some cognitive processes can be explained independently of any appeal to the sorts of cognitive abilities that might not be attributable to those who lack minds (whatever these might turn out to be). Instead, they can be explained in terms of an organism's being sensitive to the co-occurrences of paired stimuli. These co-occurrences cause the formation of excitatory (or inhibitory)

connections between 'nodes' in the organism's memory that are activated by these events.<sup>6</sup> The presentation of certain stimuli (*s*) to an organism - in our case, an infant - in conjunction with a concurrent utterance (*u*), would serve to establish in it a law-like pattern of association between the two. Thus, suppose that a mother shouts out "Don't touch!" every time her infant approaches the stove. The events of approaching the stove (*s*) and the utterance "Don't touch!" (*u*) inscribe their co-occurrence in nodes in the infant's computational machinery.

On account of this co-occurrence of utterance and stimulus, a connective pathway is formed between the nodes corresponding to (*s*) (the situation of the infant's approaching and then avoiding the stove) and (*u*), (the utterance of the words "Don't touch!"), such that the excitation (or inhibition) of one causes the excitation (or inhibition) of the other. This connection is reinforced by subsequent co-presentations of the associated items and attenuated by separate presentations. Now whenever the infant approaches the stove, the approaching-and-avoiding-the-stove node fires in its computational machinery and, by virtue of the excitatory connection between this node and the "Don't touch!" node, the firing of the former causes the latter to fire. Similarly, upon the hearing of the words "Don't touch!", the infant's computational machinery causes the firing of its approaching-and-subsequently-avoiding-the-stove node. If, over time, the infant approached the stove without any longer hearing the words "Don't touch!", the excitatory pathway in its memory connecting (*u*) and (*s*) would degrade, and it would no longer respond in this way.

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<sup>6</sup> It's envisaged that these computational processes are realised in the infant's brain, but the associationist wouldn't yet claim to know exactly how. Consequently, talk of nodes and excitatory pathways occurs at a level of abstraction from talk about the brain. Additionally, note that although Dennett talks about 'memory', this may be misleading: what's envisioned cannot be a process of conscious recall, since the infant isn't yet conscious. It's more like an inscription in the infant's computational machinery, which will be realised in the infant's brain in some as-yet-unknown way.

Because infants are “are taken with the habit of rehearsing words they have just heard” (*KoM*, p.196), Dennett suggests that the infant is also disposed to repeat the words that it has heard as it approaches the stove as “part of the drill that goes with approaching and then avoiding the stove - and also as a sort of mantra which might be uttered at any other time” (*ibid.*). Thus, as the child approaches the stove, and perhaps at other times too, it is disposed to repeat - although without understanding - the words that it has heard its mother utter. Dennett takes this repetition to be important for two reasons. First, it explains the empirical fact that infants talk to themselves. Second, in playfully repeating utterances to itself while re-enacting the events with which they are associated, the infant reinforces the connective pathways between the nodes activated, respectively, by the utterance and the bit of the world with which the utterance co-occurs. In this way repetition helps to reinforce the development of associations between situations and utterances that Dennett takes to be fundamental to language-use and thought.

On the associationist account proposed by Dennett, understanding another’s utterance consists in its being associated with an object or situation, or a set of such objects and situations, such that the firing of a (*u*) node causes the firing of the nodes corresponding to all of the objects or situations (*s*) in which that sound has been uttered. So, in the case above, there would come a time when the connective pathways between the “Don’t touch!” node in the infant’s computational machinery and the various stimuli nodes with which that utterance is associated were sufficiently well-established that the excitation of the “Don’t touch!” node would cause the firing of all of those stimuli nodes, and vice versa. At this point, says Dennett, “we have become understanders of the objects [i.e. words] we have created” (*KoM*, p.200).

#### Evaluating Dennett’s view

One advantage of associationist models of learning is that they are cognitively undemanding. While it remains controversial to attribute to infants sophisticated cognitive

abilities like knowledge of others' minds, we know that they can associate pairs of co-occurring stimuli. Indeed, we know that not only infants but rats, pigeons and other non-linguistic creatures are able to form associations of the sort described by Dennett and others. In this case, if the associationist model of learning is fit to explain how a language could be learned, there would be no further problem of whether the necessary abilities for associative learning could be attributed to infants learning to do things with words. In the following paragraphs I want to consider in more detail whether the associationist account is adequate to characterise the phenomenon that it seeks to explain.

In order to determine whether or not associationist accounts of language acquisition are adequate to explain our infants' acquisition of the ability to use and understand words in communicative interaction, it will be helpful to isolate and study in detail one aspect of language in a way that Dennett does not. In what follows, I will discuss in detail only associative learning accounts of word reference. Knowing the referent of a word - that is, the object or feature of the world for which it stands, or which it names - is one aspect of knowing its meaning. It's not, as Frege showed, sufficient for knowledge of meaning.<sup>7</sup>

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<sup>7</sup> After Frege, it's important to distinguish between two properties of a word both of which are relevant to its meaning. The referent of a word is the object that it picks out. Thus, for example, the referent of a name would be the named object - Barack Obama in the case of the name 'Barack Obama', or the planet Venus in the case of 'Hesperus'. Over and above the property of *reference*, Frege showed that there exists a further property of meaning namely *sense*. Loosely speaking, the sense of a word is its "mode of presentation".

It is natural, now, to think of there being connected with a sign (name, combination of words, written mark), besides that which the sign designates, which may be called the *Bedeutung* of the sign, also what I should like to call the *sense* of the sign, wherein the mode of presentation is contained. (Frege 1892, p.152)

Sense is the ingredient of meaning the existence of which must be posited to explain the fact that propositions with the same truth conditions can differ in *cognitive value*. One way of explicating this

However, particularly when a word is the name of an object, it's natural to suppose that in order to learn the meaning of a word, one would need to be able to determine its referent. This will require using examples that are slightly different from Dennett's and which are not discussed by Dennett. However, Dennett would no doubt recognise that learning which words name which objects is an important part - if not the only part - of learning to use words in communicative interaction.

Although it should be emphasised that knowing the referent of a word is not sufficient for knowing how it's used in communicative interaction - a claim which will be developed in Chapter Three - there are good reasons to limit the discussion here to associationist accounts of reference. First and foremost, there exists a strong body of empirical work on the conditions in which infants are able to grasp the referent of another's uses of words. In this case, the associative account of learning can be tested empirically. If there are cases of an infant's knowing the reference of a word that are not explained by the associative learning account, then it seems reasonable to infer that associative learning could not, in empirical fact, be the correct way in which infants learn the referents of words. Of course, since knowing the referent of a word is not sufficient for understanding how it is used communicatively, even a complete associationist account of how we know the referents of

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concept is that two propositions differ in cognitive value when one can rationally believe one while disbelieving the other. An example of a pair of names that have the same reference, and so form propositions with the same truth conditions, but which differ with respect to their sense, is 'Hesperus' and 'Phosphorus'. We now know that both 'Hesperus' and 'Phosphorus' have the same referent - the planet Venus. But someone who did not know this could clearly believe the proposition expressed by the sentence "Hesperus is Hesperus" while disbelieving the proposition expressed by the sentence "Hesperus is Phosphorus". Thus the propositions differ in cognitive value. Since the terms have the same referent, there must be some feature of the meanings of 'Hesperus' and 'Phosphorus' other than their reference that explains this difference. This is the property of sense. I won't discuss it further here.

words could not constitute a complete account of our ability to use words in communicative interaction.

Associationist accounts of learning the referent of a word

A simple account of learning the referent of a word might be constructed as follows. A child learns the referent of a word just by associating that word with the object or objects for which it stands. Thus, 'apple' refers to apples because it can be used to stand for apples; 'cat' because it can do used to stand for cats, and so on. How is it that an infant comes to know the reference of a speaker's words - that is, the objects or features of the world about which a speaker speaks?

The answer to this question is not straightforward, since in any context of activity an utterance could be construed as relating to countless different features of an environment. This problem is prominent in Dennett's own example of a child approaching a stove. Dennett assumes that it's uncontroversial that the event with which the infant he describes associates the words that it hears is the "situation type [of] stove-approach-and-avoidance" (*KoM*, p.196). However, it's not clear why this should be uncontroversial. In that same scenario, the mother's words could have been used to refer to any number of features of the environment - the stove, or the child, the kitchen work-surface, or to a distant relative, to give just a few examples. How is the pre-linguistic infant, not yet capable of thought or language, able to know that the words that it hears aim at one particular bit of the world - namely the "situation type [of] stove-approach-and-avoidance" - and not something else? We might think it's obvious that the words point to the stove - but isn't this because we already understand the mother's utterance? The child, of course, doesn't.

One classical answer to the problem of reference has been that the infant associates heard words with the object to which it - the infant - is attending when it hears those words. I will call this view *simple associationism*. Paul Bloom gives the following example of the position

that he will go on to criticise:

[A] child will be regarding a rabbit, and as she does so, she will hear an adult say 'rabbit'. Through general principles of associative learning, the sound and the image become associated in the child's mind, so that one evokes the other and the child could be said to have learned the meaning of the word. (Bloom 2002, p.39)

The associationist's background assumption here is that the words that an infant hears typically refer to the objects to which she is attending at the time. (Of course, on Dennett's account, Bloom's talk of the child's 'mind' would be metaphorical, at best.)

One obstacle that such an account of reference must overcome is that there are a great many words that don't stand for objects to which one could attend - not, at least, straightforwardly. The names of abstract concepts, like 'happiness', 'space', and 'emotion' do not name objects in the way that words like 'cat' and 'dog' and 'book' do. Nor do connective words like 'and' and 'or' and quantifiers like 'some', 'every' and 'seven'; adjectives like 'honourable', 'good', 'greedy' and even Dennett's 'hot'; and adverbs like 'quickly' and 'often'. Similarly, imperative uses of language, like Dennett's "Don't touch!" also do not stand for - that is, name - objects in any straightforward ways. In this case, even if reference were sufficient for meaning, only a fraction of word or utterance meanings could be specified by appeal to the worldly objects that they named and which could be the object of one's attention. It may be that infants would learn the words for more abstract concepts only later, when in possession of more sophisticated cognitive abilities - but even so, the above examples suggest that the simple associationist story about reference learning would eventually require a good deal of supplementation. This is not the only problem for associationist accounts of reference, though. In fact, even in cases where one could attend to the referent of a speaker's words, substantial difficulties for the associationist account remain.

A further problem for simple associationism is that the environment in which infants learn the referents of words is wholly unlike the environment to which the simple associationist would need to appeal to support his or her assumptions about learning. One situation in which infants are particularly (and uncontroversially) successful at learning the referents of others' words is when infant and caregiver jointly attend to an object which the adult subsequently names.<sup>8</sup> Joint-attention describes the phenomenon in which infant and caregiver attend both to an object and to one another's attending to that object. Such cases might form a reliable basis for associative word-learning to take place. However, even though on many occasions adults conscientiously direct their speech toward infants and to features of the environment to which infant and caregiver jointly attend, there are still many occasions on which infants are not looking at the referents of their caregivers' words when their caregivers speak. Very often it will be the case that the words that an infant hears are of no relevance to its immediate environment - as would be the case when, for example, it hears its parents talking about the previous day's events, or about their plans for tomorrow, or about an absent friend. These words will often be directed to someone other than the child and irrelevant to the child's current activities. Mark Sabbagh and Dare Baldwin (Sabbagh & Baldwin 2005, p.167) report studies which estimate that between 30% and 50% of the object names that an infant hears concern objects that do not occupy the infant's attention.

This constitutes a serious problem for the simple associationist because central to his or her account of learning is the postulation of a law-like correlation between the situations in which words are uttered and the presence of those objects to the infant's attention. Only where such correlations occur could one form reliable associations between objects and the words that refer to them. But it's simply not the case, for example, that speakers say the word 'dog' only in the presence of dogs.

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<sup>8</sup> See Tomasello (1999) p.109ff for discussion of a variety of studies on this subject.



This not quite straightforward environment for word-learning ought, if the simple associationist is correct, to present some serious obstacles to infants' ability to learn the reference of others' words. In particular, if it were the case that the only mechanism at work in infants' learning the referents of words was a temporally contiguous presentation of object and utterance, then given that infants often hear words while looking at entirely unrelated objects, one would expect them to make characteristic mistakes, or 'mapping errors'. It would be natural if, under these conditions, infants regularly took heard words to refer to objects at which they had been looking when the words were uttered, even when this co-presentation of word and object was merely coincidental. For example, suppose that an infant attends to a stove while listening to its mother take a phone call in which she receives some shocking news. As it attends to the stove (perhaps, for the sake of argument, for the first time), the infant repeatedly hears its mother exclaim "Unbelievable! Unbelievable!". In such circumstances, the simple associationist should predict that the infant would come to associate the word 'unbelievable' with the stove, typically by taking the word to refer to the stove. In fact, however, there's no evidence that even very young infants make mistakes of this sort: their learning of the referents of words is largely errorless, even when they hear words uttered on only relatively few occasions (Bloom 2002, p.40; Sabbagh & Baldwin 2005, p.167). The largely errorless nature of infants' word-learning constitutes empirical evidence that simple associative mechanisms are not the basis on which infants learn the referents of others' words.

In addition, further evidence from empirical work on infant attention and 'discrepant labelling' shows that infants are able to learn the referents of the words that they hear even when they are not attending to the objects to which the speaker is referring. Discrepant labelling is the name given to cases in which a speaker names an object other than the one to which an infant attends. Such studies are designed to test the associative learning hypothesis, by testing whether infants are capable of learning words in situations

that require them to do more than just associate the sound that they hear with the current object of their attention. If infants can learn the referents of words in discrepant labelling conditions, this might suggest that infants are exploiting a more sophisticated grasp of the referents of others' words - and that simple associationism is wrong.

Baldwin & Moses (1994) describe the following discrepant labelling experiment. Infants in three age-groups - 14-15 months, 16-17 months and 18-19 months - were given an object to play with while a second object was placed in a bucket in front of the experimenter. As the infant attended to the object placed before him, the experimenter looked into the box in front of her and said "It's a modi". On the simple associationist model, the infant should come to think of 'modi' as the name of the object to which he (the infant) was attending. However, infants did not do this. Rather, in Baldwin's & Moses's words:

infants across the 14-19 months span showed signs of noticing the discrepancy of focus between themselves and the experimenter in the discrepant labelling condition. (Baldwin & Moses 1994, p.139)

Relative to cases in which infant and experimenter jointly attended to the object named by the experimenter, infants were more likely to look at the experimenter's face and/or to look away from their own toy, towards the object of the caregiver's attention when the object named by the experimenter differed from the object of their attention. According to Baldwin & Moses, this suggests that even very young infants "actively monitor and follow another's attentional cues when language is provided" (*ibid.*). In other words, infants don't merely associate the sounds uttered by speakers with the objects of their own attention. Rather, in at least some cases, they take a speaker's words to refer to the objects of her - the *speaker's* - attention.

In the same experiment, infants were also asked follow-up questions to test their comprehension of the speaker's words in the discrepant-labelling condition. In particular, they were asked to "Find the modi". They should be able to do this successfully only if they

had grasped that the referent of the speaker's words was the object of her attention and not the object of their own attention. Infants' performance on this comprehension test supports the conclusion that they do not merely associate heard words with the objects of their own attention. When asked to "Find the modi", infants in the oldest (18-19 months) age group reliably (74% of the time) pointed to the correct object when asked to identify the 'modi'. In other words, these infants were able to identify reliably the intended referent of the speaker's words, whether or not it was the same object to which they had been attending. In the same circumstances, infants in the younger age groups responded only unsystematically. However, they failed to make the *systematic errors* that they would have made if they had taken the speaker's act of naming to refer to the object of their own attention. This suggests that they were sensitive to differences between the discrepant and the ordinary case - that is, they noticed that the speaker's referent was not the object of their attention.

The conclusion to draw from this is as follows. If infants identified the referents of words with the objects of their own attention then, since it's often the case that speakers' words do not refer to the object of a hearer's attention, it should follow that infants often misidentify the referents of the words that they hear. In fact, though, they do not. This is because they do not identify the referents of words with the objects of their own attention. They can also grasp that a speaker's words refer (on at least some occasions) to the objects of her own (that is, the speaker's) attention. In this case, the abilities that they use to identify the referent of a word are not those identified by the simple associationist, according to whom infants should learn the names only of objects to which they are attending at the moment of speech.

Must all associationists be simple associationists?

The results of the Baldwin & Moses experiment show that simple associationism is empirically false: it cannot be the right account of how infants come to learn the referents

of speakers' words. If Dennett's account committed him to simple associationism, then his account, too, would be empirically false. However, it may be that the findings of the Baldwin & Moses study could be explained without giving up the central claim of the associationist model of learning - that learning can be explained on the basis of the operation of a simple mechanism. To this end, a contrast can be drawn between two different interpretations of the results of the discrepant labelling study. On the first account, which is favoured by Baldwin & Moses (1994, p.138), infants "appreciate that people (1) utter words because they *intend* to refer to things or to talk about things, and (2) emit *attentional* cues as to the target of their reference" - typically, by looking at or gesturing in the direction of that object. On this model of explanation, infants know the referents of others' words because they grasp their *referential intentions*. They have some insight into the minds of speakers that enables them to grasp what it is that speakers are doing and why. The associationist would not accept this thesis. Instead they might propose an alternative hypothesis of gaze-cueing and association. On this model, infants grasp nothing of the referential intentions of others, but are causally sensitive to the direction of their gaze. When prompted in certain ways (perhaps by a speaker's speaking), infants reorientate their own gaze to follow that of the speaker, and then associate the object to which they are now attending with the word that they hear, as on the standard associationist model of word-learning. On this model, the infant's grasp of the referent of the speaker's utterance is still to be explained only by appeal to the operation of a simple mechanism - it's just that this mechanism is more sophisticated than the simple associationist had envisaged.

Either of these explanations could, at least in theory, explain the findings of the discrepant labelling study. But which explanation would be the correct one? In order to determine the answer to this question, Michael Tomasello and colleagues undertook a series of studies on the conditions in which infants between 18- and 24-months could learn the referents of others' words. In Tomasello's words, the studies were all designed so that "eye-gaze

direction was never diagnostic of the adult's referential intention" (Tomasello 1999, p.114). More prosaically, the experiments were designed so that infants could not learn the referent of a word simply by associating the word with the object cued by the experimenter's line of sight. Instead, they would need to make some further inference about the object to which a speaker could intend to be referring in order to correctly identify the referent of her words. If infants could successfully learn the referents of others' words in these conditions, this would suggest that the gaze-cueing story favoured by the associationist could not be correct; and that the 'referential intentions' account preferred by Baldwin & Moses (and by Tomasello and Bloom) provides the better explanation of infant word-learning.

In the studies led by Tomasello, infants of 18-24 months were typically required to learn the name of an object that they had not seen. For example, in one study carried out by Tomasello and Nameera Akhtar (Tomasello 1999, pp.114-115), an experimenter told the infant "Let's find the gazzer", before trying and failing to open the door of the barn in which the toy was hidden. Unable to find the desired toy, the experimenter turned her attention to a different toy and played with that instead. Nonetheless, when the gazzer was later removed from the barn, infants demonstrated that they knew the referent of 'gazzer', in spite of not having seen any co-occurrence of the object and its name. This suggests that the simple gaze-cue and associate model could not be correct: because the concurrent presentation that would be necessary for associative learning had never taken place, and because in order to have learned the word correctly, infants would have been required to infer that the speaker had intended to refer to the object that was hidden within the barn.

In a further study, undertaken by Akhtar and Tomasello with Malinda Carpenter (and described at Tomasello, 1999, p.115) infants of 18-24 months demonstrated that they can also learn the names of objects on the basis of inferring which one of several similar objects would be exciting or interesting to someone. In an experimental set-up, the infant, an experimenter and the infant's mother played together with three toys, before the infant's

mother left the room. The experimenter then introduced a fourth toy, with which the infant and the experimenter played. When the infant's mother returned to the room, she looked at the four objects together and exclaimed "Oh look! A modi!". In follow-up testing, infants demonstrated that they had learned that 'modi' was the name of the fourth object. Since the mother did not let her gaze rest on the fourth object, her naming of this object could not be explained by any simple gaze-cueing and associating mechanism. Rather, the infant must have grasped something like the fact that its mother would be more excited by (and so more likely to name) a novel object - and so took her (correctly) to be naming the fourth object, which had been introduced in her absence. This conclusion was supported by further controls.

These studies present clear empirical evidence that infants can learn the referents of others' words in cases that couldn't be explained even on the basis of attributing to them more sophisticated associative abilities than the simple associationist had envisaged. Consequently, they suggest that the gaze-cueing and associating account does not tell the whole story about how infants learn the referents of others' words. As it stands, neither version of associationism is capable of explaining the empirical facts.

In principle, at least, the proponent of a sophisticated associationist view of word-learning could invoke ever more complex patterns of association to explain how it is that infants come to grasp the referents of others' words in situations like those described above. In this case, he or she would owe us an explanation of the sort of associating mechanism that he has in mind - for example, in the form of an account of the sort of complex associating mechanism that could explain how infants might learn the referents of others' words based on some causal sensitivity to what those others had previously seen. (Such a mechanism would seem to be necessary if the results of the Akhtar, Carpenter & Tomasello study described above are to be explicable in terms that would be acceptable to the associationist.) While it hasn't yet been shown that no associationist explanation of the

above cases could be given, it's clear that the models presented so far have failed. Infants don't need to see the object to which a speaker refers when she speaks in order to learn the name of that object, as is required on both the simple associationist and gaze-cueing and associating stories. In contrast, by attributing to infants cognitive abilities more sophisticated than those proposed by the associationist, we can explain how it is that they learn the referents of others' words in cases that cannot currently be explained on the associationist account. In this case, while it would perhaps be hasty to say that an associationist explanation of the above phenomena could never be given, there is surely sufficient reason to explore these alternatives in more detail and to reject the associationist's answer to the question posed at the outset.

In the next chapter I want to consider in more detail the prospects for developing an answer to the central research question by attributing to infants a grasp of others' *referential intentions*. This is the view defended by Baldwin and Moses, Tomasello and Bloom against the associationist view of word-learning. It has been developed in detail by Paul Bloom, in his book *How Children Learn the Meanings of Words*. It's to Bloom's account that I now turn.

## Chapter 3:

# Referential Intentions

In the previous chapter, I asked whether infants' acquisition of the ability to use words in communicative interaction could be explained by attributing to them only the abilities of association and repetition. It was concluded that on standard associationist models these abilities could not be adequate for learning even the referents of words, because the environment in which word-learning takes place does not support associations of the requisite sort, and because there is a wealth of empirical evidence that infants grasp reference even in cases that could not easily be explained on the basis of attributing to them only associative abilities. In this chapter, I want to consider whether a richer set of cognitive abilities, described by Paul Bloom in his book *How Children Learn the Meanings of Words* (hereafter *HCLMW*), could explain infants' ability to use and understand words in communicative interaction. The abilities that Bloom argues to be necessary for word-learning are social, conceptual and lexical. My primary interest will be in the nature of the *social* abilities that Bloom thinks necessary for word-learning.



The purpose of this chapter will not be to make any extensive critical evaluation of Bloom's account. Rather, I want to make just two points. First, I will argue that Bloom does not have an adequate account of what it is to grasp a speaker's communicative intention, but that he needs such an account. His account of referential intent comes close to this, but is insufficiently developed. Second, I will argue that knowing the meaning of a word could not be sufficient for grasping the perlocutionary intention that that word was being used to communicate. Consequently, I will argue that the conditions that Bloom sets out as necessary for infants' learning the meanings of words are not yet adequate to explain how it is that infants learn to use words in communicative interaction.

In contrast to Dennett's *associationist* account, which explained infants' language-acquisition from a starting point of only repetition and association, Paul Bloom defends what he calls an 'Augustinian' account of word-learning. As its name suggests, this account was first proposed by St. Augustine of Hippo. The following passage is taken from Augustine's *Confessions*:

When they (my elders) named some object ... I saw this and grasped that the thing was called by the sound they uttered when they meant to point it out. Their intention was shown by their bodily movements, as it were the natural language of all peoples: the expression of the face, the play of the eyes, the movement of others parts of the body, and the tone of voice which expresses our state of mind in seeking, having, rejecting or avoiding something. Thus as I heard words repeatedly used in their proper places in various sentences, I gradually learned to understand which objects they signified; and after I had trained my mouth to form these signs, I used them to express my own desires. (Augustine, *Confessions*, I.8; cf. *PI* §1; a different translation is also quoted by Bloom at *MCLNT*, p.39)

This passage has since become well known on account of being quoted and discussed at length by Wittgenstein in the opening paragraphs of the *Philosophical Investigations*.

In the quoted passage, Bloom identifies what he takes to be the defining characteristic of the Augustinian (or 'rationalist') model of word-learning: the fact that infants actively deploy their intellectual faculties in order to figure out what speakers are doing when they speak. This constitutes a central difference between the associationist account and the Augustinian account:

instead of passively associating sound and image, the child actively tries to figure out the meaning that the adult had intended to express. (Bloom, 'Mindreading, Communication and the Learning of Names for Things' - hereafter *MCLNT*, p.39)

On the associationist account, which demands only "sensitivity to covariation", the infant is just a passive recipient of language: repetition aside, infants need only be causally stimulated by others' words in order to acquire language. By contrast, on the Augustinian account, infants deploy "reasoning and inference" to figure out the relationship between speakers' words and the things to which they refer (*HCLMW*, p.59).

A second prominent difference between Bloom's Augustinian account and Dennett's associationist account can be found in what Bloom says about pre-linguistic infants' capacity for thought. Whereas Dennett argues that exposure to language imposes upon our mental lives a conceptual structure that was previously lacking, Bloom argues that the pre-linguistic infant can already think about the world and about others' minds, such that to learn a natural language is not to learn how to think, as Dennett claims, but rather to learn words with which to express thoughts already had. On Bloom's account, this attribution of thoughts to infants also explains their coming to understand the meanings of words. For reasons discussed in the previous chapter, without a precise claim about how thinking would enable learning, this claim would not be helpful. However, Bloom does make a precise claim: infants learn the meanings of others' words because prior to acquiring a language they already possess concepts (in Dennett's words, "the tools of thought"), and because they map these concepts to the words that they hear. The meaning of a word is constituted by the concept onto which it maps.

Before explicating Bloom's account in more detail, it should be noted that it differs from Dennett's in its motivation. Dennett's account of word-learning is motivated in part by the view that we should not attribute a rich mental life to creatures where we lack evidence to do so, and that in the case of pre-linguistic infants, we lack the primary (linguistic) sort of evidence that would motivate this attribution. His account of meaning as a causal process also falls out of his philosophical commitment to physicalism - the doctrine that everything is (or supervenes on) the physical. He envisages that the associationist processes central to his account will be realised in simple physical changes in the brain. (Of course, the objections to the associationist account of language-acquisition discussed in the previous chapter do not entail the falsity of physicalism. To deny that an account of word-learning can be given in terms only of associative processes is to deny only that association is the right sort of physical process by which to give an account of word-learning. One could give-up on associationism without thereby giving up on physicalism.)

In contrast to Dennett, Bloom is not a philosopher but an empirical psychologist. His motive for attributing to infants social, lexical and conceptual abilities is the need to explain two uncontroversial empirical facts about word-learning. The first of these is that words are learned by children. Even if infants have innate knowledge of concepts, as Bloom sometimes suggests, they do not have innate knowledge of the words that speakers use to represent these concepts. Word meanings could not be innate, since they vary arbitrarily from culture to culture and because infants learn only the words of the community in which they are raised (*MCLNT*, p.37). The second of these facts is that children are "strikingly good" at this sort of learning. For example:

Children start to produce words at about the age of 12 months, which, if we stick to the more conservative estimate of 60,000 [words learned over the course of a lifetime] equates to about 10 new words a day up until the end of high school. (*HCLMW*, p.6)

This feat of learning is particularly impressive given that the relationship between a sign and what it means is arbitrary. For the most part, says Bloom, the learning of arbitrary facts is slow and hard. By contrast, the learning of word meanings is something that infants seem to do with relative ease - they learn both quickly and largely without error.

Furthermore, infants learn the meanings of others' words successfully even where their learning could not be explained by appeal to simple associative processes, and even in cultures where their learning is obstructed by challenges not present in our own. Examples of the first sorts of cases were discussed in the previous chapter. A classic example of the second sort comes from Schieffelin's research on word learning among children of the Kaluli people, who live in the rainforests of Papua New Guinea (cited by Bloom at *MCLNT*, p.40; *HCLMW*, p.8). There adults show no interest in teaching their infants the names for things in the manner common to Western cultures - where, often, adults will point to an object to which it and infant jointly attend and then name the object for the infant. In Bloom's words:

These children grow up in a rich linguistic environment, surrounded by adults and older children who are talking to one another, including making observations about the infant himself - but there is no naming of objects and no labelling interactions. And when a child names an object for an adult, the adult's response is disinterest. (*MCLNT*, p.40)

In spite of this lack of teaching, the children of the Kaluli do learn the names for things. This suggests that whatever the abilities are that should be attributed to infants to explain this, they must be sophisticated enough to explain the possibility of learning even where others have made little effort to teach them. Bloom's attribution of concepts to infants, along with social and lexical abilities, is the hypothesis that he proposes to explain their word-learning. In contrast to Dennett, whose associationism is motivated in part by the project of explaining how simple physical systems could come to have mental states, Bloom's account is driven by a theoretical need to explain how it is that children can learn

the meanings of words. Bloom doesn't reject Dennett's claim that we shouldn't attribute sophisticated cognitive abilities where we lack the evidence for their presence. Rather, he takes the fact that infants can learn the meanings of words even in challenging circumstances to be language-independent evidence of their having these abilities.

How would the attribution of thoughts to infants enable their word-learning?

On Bloom's account, the meaning of a word has two parts:

- (i) a concept or mental representation
- (ii) that is associated with a certain material 'form' - namely a word.

Word meanings "are just those concepts that happen to have word forms associated with them" (Bloom 2001, p.1102). Knowing the meaning of a word therefore requires both possessing a concept and knowing the word with which that concept is associated. Note that this use of 'associated' is not the same as Dennett's: there is no suggestion here that the infant's ability to associate words with concepts exhausts its cognitive abilities (indeed, the appeal to concepts already implies that the child possesses cognitive abilities the attribution of which Dennett would resist). Here the word 'association' is just shorthand for something like the idea that a word stands for or represents a concept.

On this theory of word meaning, the central problem of word-learning is said to be that of establishing "form-concept mapping" (Bloom 2005, p.311) between a word and the concept for which it stands. To learn the meaning of a word is just to establish the concept onto which it maps. Infants are able to figure out the meanings of words because they are able to think about a speaker's words, and about the possible ways in which those words might map onto the world of which the infant already has a conceptual understanding. In Bloom's words:

rich, abstract thought is possible without words, and much of what goes on in word-learning is establishing a correspondence between the symbols of a natural language,

and concepts that exist prior to, and independently of, the acquisition of that language. (*HCLMW*, p.242)<sup>9</sup>

In particular, two cognitive abilities play a central (i.e. necessary but not sufficient) role in explaining infants' learning the meanings of words. First is infants' possession of a 'theory of mind', such that an infant can "figure out what [others] are intending to refer to when they use words" (Bloom 2001, p.1095). Second is their possession of concepts for the objects to which others attend, onto which infants can map the meanings of the words that they hear.

#### Knowledge of others' minds

Bloom argues that "children's word learning ... draws extensively on their understanding of the thoughts of others - on their theory of mind" (*HCLMW*, p.55). However, he distances his own use of 'theory of mind' from the controversial theoretical commitments with which the expression is sometimes associated. The term 'theory of mind' is sometimes used to characterise a position (defended by Gopnik & Meltzoff 1997, and Gopnik, Kuhl & Meltzoff 2001), according to which infants originally fail to understand others as minded, and so fail to comprehend their behaviour. In order to explain the behaviour of their peers, at around the age of three or four infants develop and test scientific theories - proponents of the view refer to the "scientist in the crib" - that purport to explain others' behaviour. They

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<sup>9</sup> Note that thus presented, Bloom's account is consistent with either the claim that concepts are innate (such that the ability to think would therefore also be innate, since concepts are the tools of thought); or the claim that concepts are not innate but are acquired prior to the infant's first transactions in language. Bloom declares himself "sympathetic" to the nativist view defended by Steven Pinker. According to this view, the "abstract understanding of the world that educated scientists possess is not a radical departure from [the infant's] initial innate understanding" of it (*HCLMW*, p.167). New born infants think much as we do. However, the correctness of the account of word-learning that Bloom defends does not depend on the correctness of nativism; it requires only that it is appropriate to say that infants possess concepts around the end of their first year of life.

ultimately settle on the attribution to those others of *minds*, where this consists in their grasping that others act on the basis of a conjunction of rationally coordinated beliefs and desires. Infants' 'theory of mind' is the basis on which they make this attribution, and is taken to constitute a theory in a very literal sense.

A related but subtly different use of the expression 'theory of mind' is to denote whatever it is (theory or otherwise) that infants acquire at the age of four, which explains their emergent ability to pass false belief tests. False beliefs tests were first carried out by Heinz Wimmer and Josef Perner (Wimmer & Perner 1983) in order to test the hypothesis that children are able to reason about, and act in light of, others' false beliefs. In the original experiment, infants were told a story about a boy, Maxi, who put his chocolate into a cupboard A. In Maxi's absence, his mother subsequently moved this chocolate into a different cupboard, B. Infants were asked: "Where will Maxi look for the chocolate?" The correct answer to this question is, of course, that Maxi would look in the box in which he (wrongly) believed the chocolate to be (namely A) and not that he would look in the box in which the chocolate actually was (namely B). However, only if the infant was able to recognise that Maxi could be mistaken about the location of the chocolate, would he or she be able to answer this question correctly. What the Wimmer & Perner experiment showed was that before they are four years old, infants tend systematically to give the wrong answer - saying that Maxi would look in container B, the container into which Maxi's mother moved his chocolate, and not container A where Maxi then mistakenly believed it to be. Only around four years do infants start to give systematically the right answer.

The traditional interpretation of false belief tests has been that what is required to pass a false belief test is the ability to recognise that others may not know what it is that the subject knows; that some of their beliefs may be false. Infants' performance on false belief tests is interpreted as evidence that infants younger than four years old lack the ability to recognise others' false beliefs. Furthermore, since it's an essential feature of beliefs that

they can be false, infants' inability to grasp the possibility of false beliefs is taken to be evidence that infants lack a concept of belief at all. In turn, at least on some accounts (including that of Gopnik, Kuhl & Meltzoff), this constitutes evidence that infants younger than four lack a grasp of others' minds, because grasping others' minds consists just in the possession of an ability to explain their behaviour by attributing to them beliefs and desires on the basis of a rational conjunction of which they act. The 'Theory Theory' proposed by Gopnik, Kuhn & Meltzoff is one explanation of what changes when infants reach the age of four: roughly speaking, it's around this age that they hit upon the right theory by which to explain the behaviour of their peers. However, whereas many who use the term 'theory of mind' think that possessing a theory of mind requires having a grasp of the concept of belief, not all of those who think this agree that our grasp of others' beliefs is to be explained by non-metaphorical talk of knowing a theory.

Bloom emphasises that he uses the term 'theory of mind' in a manner free from such theoretical baggage, such that there is no issue of, for example, his referring to 15-month-old infants' theory of mind. First, he makes no commitment to whether or not "theory of mind really is a theory in any non-trivial sense" (p.61); and second, he makes no claims about infants' abilities to pass false belief tests (*ibid.*). On Bloom's account, a theory of mind is just whatever it is that enables one to determine the objects to which speakers intend their words to refer.

As was discussed in the previous chapter, Bloom rejects the idea that an infant's grasp of the referent of another's words can be explained by attributing to it only a sensitivity to the direction of the gaze of others and tendencies to associate words and bits of the world the presentations of which co-vary. This can't be right, since infants learn the names of objects even when not attending to them at the moment of a speaker's speaking (Baldwin & Moses 1994), and even when the object to which the speaker is referring cannot be seen and has to be inferred (Tomasello 1999, p.114f.). In contradistinction to the associationist view,



Bloom argues that infants can determine the referents of others' words because they grasp others' *referential intentions*. As Bloom uses the term, in grasping a speaker's referential intention, one grasps that a speaker is using a word in order to thereby refer to an object. This requires that infants be able to attribute intentional states to others - in particular, intentions to refer. One grasps the objects to which speakers intend to refer by "attending to cues to the intention of the speaker" (*MCLNT*, p.39). These cues include (but are not limited to) his or her direction of gaze and body language with respect to an object, both of which the infant grasps as indicative of the speaker's attending to an object. The significance of these behavioural cues for determining the referent of a word is described by an analogy with arrows:

One way to look at it is that children use inferences about the referential intentions of others to create arrows, or pointers, from words to the world. A child hears the word "rabbit" and uses a speaker's direction of gaze to figure out what he or she is referring to. In the child's mind, an arrow is now going from *rabbit* to a rabbit. This understanding is necessary to learn the word. (*HCLMW*, p.87)

So, figuring out the meaning of a word involves working out the object or feature of the world for which a word stands, which the infants grasps by virtue of knowing the speaker's referential intention.

#### Knowledge of objects

Bloom thinks possession of a theory of mind necessary but not sufficient for word-learning. Further abilities also play a role. For example, children are able to learn the meanings of words because they already understand a great deal about the world characterised in language. At a very basic level, infants have a grasp of *objects* - that is, of "connected and bounded regions of matter" (*HCLMW*, p.95) that remain intact even when moved. This ontology of objects constrains form-concept mapping by constraining the ways in which the world is conceptualised. Infants conceptualise the world in terms of objects, rather than

connected concatenations of object parts like. Bloom argues that this constitutes a solution to a philosophical problem first described by Quine (1964, chapter 1).

The problem that Bloom identifies at the heart of Quine's discussion is this. Suppose that one hears a speaker of a foreign language utter "Gavagai!" while attending to a rabbit. It would be natural to infer in this scenario that that the speaker is intending to refer to the rabbit, and consequently that 'gavagai' is the name for rabbits in his language. But how does one know that what that speaker is referring to is the whole rabbit, and not merely "all and sundry un-detached parts"?<sup>10</sup> Unless one knows this, one couldn't know that 'gavagai' and 'rabbit' have the same meaning, and so one wouldn't know the correct concept onto which to map the native's word. Bloom's 'whole object preference' is an attempt to give a naturalistic solution to this problem: it is just a fact about people that we tend to use words to refer to whole objects.

[T]he naturalness of the rabbit hypothesis and the madness of the alternative is not a logical necessity; it is instead the result of how the human mind works. (*HCLMW*, p.4)

According to Bloom, infants' 'whole object preference' explains why they map object-concepts and not Quinean object-part-concepts to the words that they hear. Unless given reasons for taking the object of a speaker's reference to be a collection of objects or an object part, infants typically assume that novel words are being used to refer to individual whole objects (*HCLMW*, chapter 4).<sup>11</sup>

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<sup>10</sup> In fact, this is not the problem in which Quine is primarily interested. Quine's concern is not how we would know what the native meant, but whether behavioural data about the circumstances in which the speakers of a language use words could ever be sufficient to determine their ontological commitments. Although Bloom misses the philosophical thrust of Quine's argument, this falsifies neither his belief that the problem that he attributes to Quine would constitute a genuine obstacle to word-learning, nor the 'naturalistic' solution that he proposes to it.

<sup>11</sup> Of course, Bloom does not think that all words are the names of objects. However, he does think that object names occupy a privileged position in an account of language-acquisition:

## Individuals and kinds

A further obstacle to language learning that Bloom discusses at length is that even when one knows the object to which a speaker refers, there are still many ways in which his or her words might serve to characterise that object. Bloom acknowledges this:

It is one thing for a child to learn that a word is used to talk about a particular dog and quite another to know what the word means. (*HCLMW*, p.119)

For example, knowing that a speaker is referring to a dog is not sufficient for knowing whether, when she speaks, she's describing it as a dog or as a pet. Thus solving the mapping problem is necessary and sufficient for knowing the meaning of a word, but knowledge of a speaker's referential intention is only necessary and not sufficient for solving the mapping problem.

Bloom argues that infants can solve the mapping-problem even where knowledge of a speaker's referential intention is insufficient, because (among other things) they already possess the concepts for which the words stand (if they didn't, they couldn't learn the mapping central to grasping a meaning), and because they are sensitive to a wealth of different contextual cues that help them to decide which concepts map onto which words. The contextual cues that Bloom thinks important are diverse. Some examples should give the flavour of his position, but the following are by no means exhaustive and should not be taken to be so.

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Children also have to learn words that are not object names, such as determiners, prepositions, verbs, and abstract nouns. But this learning of simple object names is fundamental. If children are to start learning words - and, indeed, to start learning language - they need to begin by learning names for things. (*MCLNT*, p.38)

This explains the prominence of name learning in Bloom's account.

- *Lexical information* - infants know that if a word is preceded by an article ('the' or 'a') then a word is likely to be a common noun and not a proper name.
- *Knowledge of how naming practices relate to object kinds* - infants know that people and animals are more likely to have proper names than bricks.

*Antecedent knowledge of object names* - in Bloom's words:

if children know a proper name for an object, a different word referring to the same object is less likely to be a proper name, and if they know a common noun for an object, then a different word referring to the same object is more likely to be a proper name. (*HCLMW*, pp.130)

In some cases, not least that of the last example, infants must also exploit their knowledge of object categorisation to make sense of others' uses of language. This is necessary to explain why infants grasp that when I point to Fido and say "Dog!", the word 'dog' should be mapped onto the concept of *dog* and not that of *animal*. (Bloom tells a detailed story about how infants' mapping is constrained by the relative saliencies of different categories, the details of which need not be elaborated here.) Constrained by these and other cues, Bloom argues that infants are very good at inferring the correct mappings of words onto concepts.

Responding to Bloom's account

A more rigorous appraisal of Bloom's account would require a far more detailed engagement than has been attempted here. The purpose of this chapter, though, is not to undertake any such appraisal but merely to raise two issues that are not adequately discussed by Bloom. First, I will object that Bloom does not have an adequate account of what it is to produce or grasp a speaker's communicative intention, and of the cognitive abilities that this would require. Second, I claim that knowing the meaning of a word could not be sufficient for grasping the perlocutionary intention that that word was being used to communicate. In this

case, the conditions that Bloom sets out as necessary for learning the meanings of words are not sufficient for knowing how to use words in communicative interaction.

It should be emphasised that this second point is not a criticism of Bloom's position, and that the first points only to a lacuna, albeit one that Bloom needs to fill, and not to any falsity in what Bloom says. With respect to the second point, the central question of this thesis is: (A) what are the cognitive abilities that must be attributed to infants in order to explain their ability to use and understand others' uses of words in communicative interaction? Bloom's question, by contrast, is: (B) how (for example, in virtue of possessing which cognitive abilities) do infants come to learn the meanings of words? The two questions are obviously intimately related: at least on many occasions, one could not grasp others' utterances if one did not understand the meanings of the words that figured in their utterances. However, knowing the meanings of others' words is not sufficient for understanding the utterances in which those words figure. Given this distinction, it's not a criticism of Bloom to say that the answer that he gives to his question (B) doesn't constitute an answer to my question (A). The first point is a criticism of Bloom in the following respect. Bloom takes for granted a notion of understanding communicative intent that needs to be explained more substantially. Without saying more about what this consists in, his account fails to deliver an adequate specification of the cognitive abilities that infants would need to learn the meanings of others' words. As Bloom's project needs this explanation, so does mine.

It should be noted that I will not attempt to evaluate or discuss in any detail Bloom's central motif of form-concept mapping. The undertaking of such a project would be lengthy and difficult. In the first instance, this is because the plausibility of the mapping-metaphor, and so by extension the plausibility of Dennett's claim that one acquires a concept only by learning the meaning of a word, will ultimately hang on the provision of answers to two questions. The first concerns what it is to possess a concept. This is a huge question,

involving issues about what sorts of abilities - if any - concept possession affords the concept possessor. On a fairly standard view (for example, Peacocke 1992, 2000) possessing a concept of an *F* equips its bearer with the certain epistemic abilities, including abilities to make inferences about the relations between things that are *F* and things that are *G*, and, in some if not all cases, being able to discriminate *F*s from *G*s. For example, it might be that an individual with the concepts of *dog* and *animal* should be able to judge of some animals that they are dogs, but should not judge that all animals are dogs, and should be able to infer that if something is a dog, then it's also an animal, and so on. However, this view is not undisputed. For example, Jerry Fodor (Fodor 1998, 2004) has argued that concept possession brings no such abilities, and that to possess a concept of dog is just to be able to think about dogs "as such" - whatever this means. I lack both the time and the confidence to address these issues adequately here.

The second question concerns the empirical question of whether infants possess concepts to which word meanings could be mapped. Answering this question is difficult - first because it requires knowing what it is to possess a concept, and second because it requires attributing concepts to infants independently of their language-use. This is necessary in order to avoid circularity in the explanation: if knowledge of concepts is posited as the explanation for knowledge of word meanings, then one could not, without circularity, appeal to knowledge of meaning as the basis for explaining attributions of concepts. The problem here, as Michael Dummett put it in his essay 'What do I Know when I Know a Language?', is that "for any but the simplest concepts, we cannot explain what it is to grasp them independently of the ability to express them in language" (Dummett 1993b, p.97). It may be possible to give good accounts of both what it is to possess a concept, and of some language-independent basis for attributing concepts, but doing so would constitute the project of a whole thesis, and a very different thesis from this one.

Rather than undertaking the theorising that would be necessary to explicate and evaluate Bloom's mapping-metaphor, the project that I propose to undertake here concerns not what is involved in, for example, possessing the concept *dog*, but the nature of the abilities in virtue of possessing which infants are able to use the word 'dog' to communicate their dog-involving instructions and desires, and so on. These are abilities that we know infants to possess. No doubt, they are not unrelated to conceptual abilities, but the nature of the relationship is far from clear. It may be that an account of the ability to use words in communication could inform questions of what it is to possess or develop conceptual abilities, for example by providing a background story against which an account of the development of inferential abilities could be given. Thus, for example, it may be that infants acquire inferential knowledge through their uses of language - perhaps by learning that the word 'dog' can be used to point out to others the presence of dogs, but not of cats or pigs, and subsequently inferring that cats and dogs (and pigs) are not the same. If such a story falls out of my account, that would be a bonus - but it's not my intention to lay the foundations for such an account here.

I turn now to the issues that I raised previously.

First issue: what is it to grasp the meaningfulness of an utterance?

In the opening pages of *How Children Learn the Meanings of Words*, Bloom raises several problems that any linguist would have to encounter and resolve successfully if he or she were to learn successfully the meanings of another's words. He takes as his starting point Quine's discussion of a 'Radical Translator' learning to interpret the meanings of a native's utterances in a foreign tongue. Supposing the translator saw the native look to a rabbit and say the word "Gavagai!", there are several questions that it must answer before it can determine that the speaker is giving the rabbit's name. Bloom's answer to one of these questions has already been mentioned, albeit only briefly: when an infant sees a speaker look to the rabbit and speak, it assumes that her words refer to the rabbit and not to an

extensionally equivalent conjunction of un-detached rabbit parts. This is just a fact about “how the human mind works”. However, Bloom poses a further question that he doesn’t even attempt to answer: how does interpreter know “that *gavagai* is a name at all, as opposed to the native clearing his throat, or making a noise to warn the animal away[?]” (*HCLMW*, p.4). The question that Bloom is posing here, and which he never quite gets to grips with, is: how does the infant know when the sound (or action) that another performs is *meaningful*? I take this to be the same question as: how does the infant grasp that the action was performed with communicative intent? I’ll use the phrases synonymously.

This question is a fundamental one. In Bloom’s terminology (which I adopt only to illustrate this point), only a meaningful action should be mapped to a concept. If infants failed to grasp that some but not all sounds made by others should be mapped to concepts, one would expect them to make characteristic mistakes in their uses of language. For example, if they took words to be like coughs - sounds emitted by others not as the means to the pursuit of some communicative end but just as a consequence of some physical discomfort - infants might fail to grasp that words could be used communicatively, and so fail to reproduce them at all. Alternatively, if infants took coughs to be like words - meaningful utterances produced in the service of a communicative goal - they might reproduce coughs in the same sorts of ways that they do words. We might then expect infants to cough when it would be more appropriate to speak. In fact though, there is no evidence that infants make these sorts of mistakes.

From this, we might draw two conclusions. First, that in order to learn the meaning of a heard word, infants would need to grasp or at least be sensitive to the fact that the sound produced by a speaker had been produced with meaning - that is, with communicative intent. This would require that one had some grasp of or sensitivity to what it is for a sound to be invested with meaning. It wouldn’t be enough to know only that a property of some



sounds produced by others distinguished them from other sounds produced by others, since, for example (and borrowing Bloom's vocabulary, if only for illustrative purposes), it wouldn't follow from being able to distinguish two groups of sounds that one would map the sounds of one group but not the other to concepts. This would only follow if one grasped or was sensitive to the fact that the property in respect of which the two groups of sounds differed was the property of meaning. Second, since infants are able to grasp the meanings of others' words in a largely error-free manner, they must have some grasp of or sensitivity to the property of meaning. That is: they must have some grasp of or sensitivity to what it is for an utterance to be meaningful. But what would this knowledge consist in?

In his discussion of referential intentions, Bloom sometimes hints that he has an answer to this question. On this story, infants would grasp that some sounds are meaningful because they grasp that some sounds have been produced with *referential intent*; because, for example, they can grasp that when a speaker attends to a rabbit and says "Gavagai!", she does so with the intention to refer to the rabbit. But what is it that having such a grasp of referential intent consists in? Moreover, what are the cognitive abilities that grasping referential intent would require? An answer to the latter of these questions is surely central to the provision of an answer to the central research question of this thesis. An answer to the second of these questions, though, would require first that one knew what it was to act with referential intent. Unfortunately, Bloom answers neither in satisfying detail.

One possible answer to the question *what is it to act with referential intent?* can be ruled out immediately. One might think that to know that a speaker had produced a sound with referential intent consisted in knowing nothing more than knowing that its speaker was attending to an object at the time they produced that sound. This couldn't work, though. A minimum requirement of an explanation of what knowledge of referential intent consists in is that it should suffice to enable one to distinguish meaningful from meaningless sounds. Merely knowing that a sound was directed at a particular object though, would not meet

this requirement, since there can be sounds that are directed at objects without being meaningful. For example, if I were to honk loudly at a rabbit in order to scare it away, I would not be producing a meaningful sound. Bloom doesn't defend the claim that knowledge of a speaker's attention is sufficient for grasping that she is acting with referential intent. In fact, it seems clear that Bloom is committed to thinking that knowledge of referential intent consists in something more than this. But what?

While Bloom comes close to recognising that there is an important question to be answered here, it's clear that he neither has, nor even really considers, an answer to it. Nonetheless, it surely needs an answer - even by Bloom's own lights. Any account about language acquisition, and the cognitive abilities that language-acquisition requires, would be incomplete to the extent that it lacked a story about what it is to produce a sounds with meaning, or grasp that a sound is meaningful, and the cognitive abilities that this requires. In the next chapter I will take up this challenge. My starting-point will be Paul Grice's discussion of the property of non-natural meaning. In the meantime, I return to the second point that I want to raise in the context of a discussion of Bloom's account.

Second issue: meaning and perlocutionary intentions

Central to Bloom's Augustinian picture of meaning is his mapping-metaphor. This consists of two central claims:

- (1) The meaning of a word is the concept for which it stands.
- (2) Knowing the concept for which a word stands (and presumably also that the word should be mapped to that concept) is sufficient for knowing the meaning of that word.

However, knowing how to map a word to a concept is not, in at least many cases, sufficient for knowing how uses of that word should be understood.

Consider the example of the noun 'fire'. In one sense, Bloom accounts straightforwardly for an infant's learning the meaning of this word: it maps onto the concept of fire (and so

presumably refers to objects that fall under the set of the predicate *is a fire*). Suppose, if only for the sake of argument, that such a story is correct. Nonetheless, an utterance consisting of the word 'fire' could be used to perform a variety of different communicative acts - even in cases in which the intended referent of the word 'fire' is the same. For example, in a typically Augustinian case, an infant might be taught the name of fire by hearing a speaker say "Fire!" while attending to a fire. It's notable that almost all of Bloom's examples concerns cases like this, in which a word is used to give the name of some object or property. However, naming is but one of many different perlocutionary acts that we perform by speaking. One can imagine an activity in which a speaker called out "Fire!" in order to communicate to someone that they should bring him that fire. In this case, the word 'fire' wouldn't just serve to name that object. Although its message would be communicated in part by its naming the fire, it would also serve to give an order that fire should be brought. Alternatively, someone might shout "Fire!" in order to draw attention to a fire - to warn others of its presence, perhaps with the intention of causing them either to flee the fire, or to bring water, or merely just to look at it. In each of these cases the same word has been uttered with a different perlocutionary intention: to name an object; to request/demand the object; and to alert others to the presence of an object.

In this case, there must be more to understanding the use of the word 'fire' in communicative interaction than there is to knowing the concept to which it should be mapped. One must also grasp the perlocutionary intention in the service of which the word was uttered. Although sometimes, although by no means always, knowing the meanings of another's words will be necessary for this, it may not be sufficient. In this case it must be asked: how does one grasp a speaker's perlocutionary intention?

Bloom's account does not provide an answer to this question, but it's surely an interesting question that deserves an answer. From the perspective of finding a characterisation of the infant's mind that would explain how this understanding is possible, it would be convenient

if this answer could be explained by appealing to the sophisticated cognitive abilities that Bloom ascribes to the infant. As it stands, though, I suggest that Bloom's characterisation of the infant's mind is ill-equipped to provide this answer. In order to see why this is the case, it's necessary to see why Bloom could not, without substantial supplementation, explain this linguistic phenomenon.

What cognitive abilities are necessary for grasping others' perlocutionary intentions?

One possible approach to answering this question would be to explain the differences between the uses of 'fire' by appeal to lexical clues. As the discussion of Bloom's account illustrated, this is an approach that Bloom adopts elsewhere. For example, infants are said to be able to distinguish between proper names and category concepts because the latter but not the former are preceded by articles. One would speak of "a dog" or "the dog" but not, typically, of "a Fido" or "the Fido". By analogy, there might also be lexical principles that would serve as cues for determining that a word is being used with one perlocutionary intention and not another. This is often the case. For example, the order of subject and verb is typically reversed in questions, and imperatives typically use the second person (singular or plural) form of a verb. It may be that infants are sensitive to these differences. However, in many cases there aren't - or at least needn't be - any lexical differences that would suffice to determine a difference of perlocutionary intention. The examples above constitute one such case. The differences can all be marked by a difference in the use of a single word: 'fire'. In conversation, it's often the case that words are used in just this manner, such that one couldn't appeal to lexical differences to determine the perlocutionary intention with which a word is being used.

An alternative response would be to mark the differences of perlocutionary intention by appeal to syntactic features. (Since Bloom doesn't talk about syntax, this is a departure from Bloom's account.) Where 'fire' is used to give an order ("Fire!"), this imperative use would often be marked in writing by the presence of an exclamation mark that might be

absent in a case of naming ("Fire."). There are two problems here, though. The first is that it's not obvious that such syntactical distinctions are marked in speech as they are writing. The second problem is that even if relevant syntactical distinctions are marked in speech, they couldn't be sufficient for determining the perlocutionary intention that motivated a speaker's speaking. This is because syntactic distinctions (often described as differences in the 'mood' of an utterance) are insufficiently fine-grained to reflect the different sorts of perlocutionary intention that could motivate an utterance of the same sentence. Consider the following cases, similar to some discussed in chapter 1.

1. I utter the words "There's a fire!" with the intention of explaining to you why the room is so warm.
2. I utter the words "There's a fire!" with the intention of instructing you to evacuate the building.
3. I utter the words "There's a fire!" with the intention of pointing out to you the location of a fire, for which you've been looking long and hard.

In these cases, the utterances which are being uttered as means to different ends are nonetheless syntactically identical. One can't appeal to different syntactic features of them in order to explain the different perlocutionary intentions with which they were uttered, since there are no such syntactic features. In Michael Dummett's words:

It is ... evident that language has insufficiently few forms to differentiate the various types of linguistic act it may be used to effect. (Dummett 1991, p.116)

Consequently, knowledge of the mood of a sentence couldn't be sufficient for grasping the way in which an utterance should be understood. If not by appeal to either the lexical or syntactic properties of the uttered sentences, how should the differences between these utterances be explained?

A natural response is to fill this explanatory gap by appeal to something like an infant's knowledge of a speaker's mental states - that is, to the infant's knowledge of others' minds. Such a story is consistent with the possibility that we do use lexical and syntactic

clues in determining speakers' perlocutionary intentions, but recognises that these cues are not sufficient for the task. It's plausible that, in combination with lexical and syntactic clues where these are present, knowledge of a speaker's mental states would be sufficient for determining that he or she was using the word 'fire' to give a warning and not merely to give the name of fire. Although Bloom emphasises repeatedly the role that knowledge of other minds plays in his account of word learning (in the form of what he calls 'social' or 'theory of mind' abilities), it's remarkable that in his hands, infants' knowledge of others' minds is exploited only as a means to determining speakers' referential intentions. If knowledge of others' uses of words is to be explained by knowledge of their mental states, a richer account of infants' knowledge of other minds would need to be developed than can be found in Bloom.

In the next chapter, I will argue that the two points I've raised in this chapter are closely related: that, in fact, they permit the same solution. This is because what both of the issues above show is that what's lacking from Bloom's account is an appropriately robust notion of *communicative intent*. This is, I take it, a richer notion than Bloom's notion of referential intent. To grasp the communicative intention with which another makes an utterance is just to grasp the perlocutionary intention that motivated their speaking. However, grasping another's communicative intention is not merely a case of grasping their referential intention. On the contrary, what Bloom calls a referential intention can be understood (without loss) as one variety of communicative intention, but it is not the only variety of communicative intention, and it is certainly not the only variety of communicative intention that infants are capable of grasping. In developing this claim, I will address both of the concerns that I have raised in this chapter. That is, I will offer a more robust explanation of the cognitive abilities that explain infants' abilities to understand others' uses of words, and give a more substantial account what it is to produce or grasp a meaningful utterance.

## Chapter 4:

# Communicative Intentions

In the previous chapter, several questions were left hanging in the discussion of work by Paul Bloom. In this chapter I want to pick up on two associated questions that were raised and discussed in the previous chapter, but which Bloom failed to answer, or even to pursue. The first outstanding question raised in the discussion of Bloom was: what cognitive abilities must be attributed to infants' to explain their grasp of or sensitivity to the meaningfulness of others' utterances? I take this to be synonymous with the question: in virtue of possessing which cognitive abilities are infants able to grasp that another's utterance has been produced with communicative intent? The second question was: what cognitive abilities must be attributed to individuals if they are to be able to grasp what it is that their interlocutors sought to communicate by speaking?

In this chapter, I hope to answer both of these questions in the course of developing answers to two further but closely related questions.

(I) What is it to mean something by an utterance, or to grasp what a speaker means by uttering?

(II) What are the cognitive abilities that must be attributed to communicators for meaning something by an utterance, or for grasping the meaning that another's utterance has?

In ways that will become apparent, the first of these questions combines elements of the two questions raised in the previous chapter. It will be necessary to answer (I) in order to answer (II). The provision of an answer to (II) is central to the research project of this thesis, namely: the provision of an account of the cognitive abilities that one would need to attribute to young infants to explain their first uses of words in communicative interaction.

By answering (I) and (II) I hope to fill out the lacuna identified at the heart of Bloom's account. The answers that I defend will draw heavily on Paul Grice's analysis of the property of non-natural meaning. Additionally, I will defend the appropriateness of attributing to infants the ability to produce and grasp intentions not radically different from those that Grice thought necessary and sufficient for meaning something by an utterance. Before turning to Grice, though, it's worth spelling out question (I) above in some more detail.

What makes an action meaningful?

Suppose that one found oneself parachuted into a tribe of non-English speakers and charged with the task of trying to make sense of their words. One sees a native point to a rabbit, before uttering the expression "Gavagai!". Bloom asks (*HCLMW*, p.4): how does someone who hears this know "that *gavagai* is a name at all, as opposed to the native clearing his throat, or making a noise to warn the animal away"? One way of glossing Bloom's question is: how does one grasp that a sound produced by another has been made with communicative intent, in order to convey some message, as opposed to some meaningless sound produced just in order to startle another, or by someone's clearing their throat? An answer to this question must be central to an account of the cognitive abilities needed for communicative interaction. Only actions produced with meaning can be the bearers of



thoughts, vehicles for the telling of stories, or for the expression of instructions or desires. Meaningful actions are therefore special. They are to be contrasted with actions like coughs, growls and aimless wavings of the hands, which are produced without meaning and so could not be said to mean anything; are not the vehicles of any thoughts.

If one is to go about determining the meanings that others' utterances have, or if one is to produce sounds or gestures that are meaningful, one must know something about or be sensitive to what it is that distinguishes these sounds and gestures from sounds or gestures that are produced without meaning. As discussed in the previous chapter, if infants lacked this knowledge/sensitivity, one would expect them to make characteristic mistakes in their uses of language. However, since there is no evidence that infants do make such mistakes, we have reason to conclude that not only is it necessary that they should grasp or be sensitive to communicative intention, furthermore they do or are. What, though, does this consist in?

As Wittgenstein (*PI* §§139-141), among others, demonstrated, the answer to question (I) cannot be answered by an appeal to any intrinsic properties of words, signs or gestures. This is because words (and their non-verbal equivalents) are semantically inert: nothing intrinsic to the sounds that speakers produce in communication suffices to differentiate them from identical sounds being used without meaning. The same marks could always be meaningless. At the same time, however, there is clearly a difference between a set of sounds or marks that are being used with meaning and an identical set that are not. Only the former and not the latter could appropriately be interpreted as expressive of thoughts. What distinguishes meaningful from non-meaningful utterances, and on what basis could infants determine that an action had been produced with meaning?

Before considering an answer to this question, something should be said about the form the answer must take. It's surely wrong to think that prior to their learning the meanings of

others' utterances, infants have some explicit or even implicit theoretical understanding of what it is for an action to be meaningful, on the basis of applying which they then discriminate meaningful and non-meaningful actions and attempt to interpret only the former. Very few adults, let alone infants, possess such a theory: they could neither describe what it is about meaning that they know, nor recognise someone else's true description of this knowledge.<sup>12</sup> In this case, such knowledge cannot be necessary for meaningful communication. A better way to think about knowledge of meaning, then, would be as something that communicators are able to exploit in their particular interactions with others and their utterances, but which doesn't take the form of any theory of what it is for an utterance to be meaningful.

Additionally, it would be incorrect to accord general questions about the nature of meaning a higher explanatory priority than questions about the meanings of particular utterances. One might think that one could provide an answer to the general question (a) *what is it for an utterance to be meaningful?* independently of providing an answer to the question (b) *what is the meaning of a particular utterance?* This can't be right, though. For whereas one could grasp another's action as being meaningful without grasping the meaning that it had, one could not mean something by an utterance without meaning something in particular. That is, one could not produce a meaningful utterance that did not have a particular meaning. Consequently, even a general explanation of what makes an utterance meaningful should also yield a specification of the meaning that any particular utterance has. An answer to question (a) could not be given independently of answers to question (b).

In the work of Paul Grice, one finds an answer to question (l): what is it to produce a meaningful utterance, or to grasp the meaning that another's utterance has? It comes via an analysis of the conditions that are necessary and sufficient for meaning - or, as Grice calls

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<sup>12</sup> In this case, knowledge of meaning would be neither explicit nor implicit knowledge, as these terms

it, 'non-natural meaning' - that says both what it is for an utterance to be meaningful, and delivers a specification of the meaning that an utterance has. Grice's analysis of the property of meaning also delivers an answer to question (II) posed at the outset - in the form of a model of the cognitive abilities that would be necessary for meaning something by an utterance and understanding the meaning of another's utterance. However, the question of whether this model could be attributed appropriately to young infants has proved controversial.

Grice's distinction between natural and non-natural meaning

The property of 'non-natural meaning' (or 'meaning<sub>NN</sub>') attaches to the production of utterances in communicative exchanges. It's the sort of meaning that we have in mind when we say things like "John's wave means that he's coming over", or "His pointing means that the object you're looking for is in that box". It is to be distinguished from what Grice calls 'natural meaning'. Both properties can be picked out by the word 'means', but they are not the same. Grice thinks we have robust pre-theoretical intuitions about the natures of both sorts of meaning and, prior to undertaking a deep analysis of non-natural meaning, he provides two tests to make the envisaged distinction salient. (Since the analysis Grice provides applies only to the property of non-natural meaning, it's necessary to emphasise that not all uses of the word 'meaning' are intended to be explained by the analysis.) The tests that Grice offers for distinguishing between cases of natural and non-natural meaning are not intended to be exhaustive: they merely serve to mark an intuitive difference that can subsequently be investigated.

Natural meaning, indicated in expressions like "Those spots mean measles" or "Those dark clouds mean rain", is an entailment relation. The first test is, consequently, that if *A naturally means B*, then the presence of *A* entails (i.e. is sufficient for) the presence of *B*.

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are used by Michael Dummett (Dummett, 1991, pp.95-97).

By contrast, non-natural meaning yields no such entailment: John's wave is not sufficient for his coming over, and the pointing man may have been wrong or dishonest about the location of the object in the direction of which he pointed. Non-natural meaning is a property of actions (in Grice's terminology, 'utterances') that are performed by speakers with the intention of communicating some message, or 'content', to an audience. Although communicative, they need not be linguistic.<sup>13</sup> This forms the basis for Grice's second test. The contents of non-naturally meaningful utterances can be translated into language and restated in the form of a phrase contained within quotation marks. Thus, for example, John's wave can be translated by the words "I'm coming over". This is not true of objects that stand in a relation of natural meaning. Although it may sometimes be appropriate to say of a person's cough that it means (naturally) that she has a chest infection, it would not ordinarily be true that the cough could be translated by the words "I have a chest infection".<sup>14</sup>

Because it's more comfortable to do so, in the following discussion, I will refer to non-natural meaning by 'meaning' and not 'meaning<sub>NN</sub>' unless a specific contrast with what Grice called 'natural meaning' is being made.

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<sup>13</sup> Note that although meaningful utterances need not be verbal, in the following I retain the notation of referring to the speaker *S* of an utterance and its hearer *H*, rather than, as some others have done, referring to an utterer *U* and an audience *A*. This may seem odd. However, doing so allows me to exploit a convenience of the S-H notation: that it lends itself to a distinction between interlocutors on the basis of gender - S(he) and H(e). I've found this helpful when keeping track of examples featuring complex sets of intentions the owner of whom may otherwise be confused. Of course, this should not be taken to imply the obvious falsehood that speakers are always women and hearers always men.

<sup>14</sup> A speaker could cough with the intention of communicating, by coughing, that she had a chest infection (in accordance with clauses (1)-(3) of the Grice's analysis). In this case, her cough would be non-naturally meaningful, and apt for translation. However, this is not the typical case. Typically, people do not cough with any communicative intentions; they cough because they are unwell.

Grice's analysis of meaning - a first answer to (I)

On p.92 of *Studies in the Way of Words* (hereafter *SWW*), following several attempts in the earlier essay 'Meaning' (which is also reprinted in *SWW*), Grice proposes the following set of necessary and sufficient conditions for someone's meaning something by an utterance. Although intended to be something of a work-in-progress - partly in response to others' criticisms, Grice subsequently sharpened the details of his analysis - this formulation has been taken to be fairly representative of 'the Gricean view'.

"*S* meant something by uttering *x*" is true if and only if, for some audience [or hearer, *H*], *S* uttered *x* intending:

- (1) *H* to produce a particular response *r*,
- (2) *H* to recognise that *S* intends (1),
- (3) *H* to fulfil (1) on the basis of his fulfilling (2) - i.e. on the basis of his recognising *S*'s intention that he should *r*.

Grice then clarifies what is meant by (3) with the following:

And to suppose [*H*] to produce *r* "*on the basis*" of his thinking that [*S*] intends him to produce *r* is to suppose that his thinking that [*S*] intends him to produce *r* is at least part of his *reason* for producing *r*, and not merely the *cause* of his producing *r*. (*SWW*, p.92)

Although this analysis doesn't explicitly mention what an utterance of *x* means, nonetheless some specification of this is implicit. What a speaker means by uttering *x* on a particular occasion is closely related to the response *r* that she intends to produce in her interlocutor by uttering, and what the utterance *x* means is (at least in the first instance) derived from what a speaker meant by uttering it. Thus Grice's analysis explains both the meaningfulness of an utterance, and the meaning that it has. It also contains the basis for an analysis of the cognitive abilities that are involved in meaning and understanding meaning. Meaning something is a matter of performing an action for the purpose of making an interlocutor

produce some response  $r$ , and with the intention that he recognise this, and with the intention that he  $r$  on the basis of recognising the intentions with which the utterance is produced. Grasping the meaning of an utterance requires recovering what it is that one's interlocutor intends that one should do, where this is achieved in accordance with conditions (2) and (3).

Grice and Folk Psychology - a first answer to (II)

Grice's analysis also provides the basis for an answer to (II). It introduces into the understanding of meaning an appeal to 'theory of mind' abilities: to mean something requires having intentions with respect to others' mental states and actions; and understanding meaning requires recognising others' intentions with respect to one's own mental states and actions. However the 'theory of mind' abilities invoked here extend beyond those invoked by Paul Bloom to explain infants' knowledge of the referents of others' words. The abilities that Bloom invoked (and which were concluded to be insufficient to explain infants' grasp of others' uses of language in communicative interaction) extended, at least in most cases, little further than attributing to infants a grasp of the object to which a speaker intended to refer. Moreover, Bloom explicitly distanced himself from conceptions of 'theory of mind' abilities that involved attributing to infants a grasp of folk psychological concepts like belief. For example, Bloom specifically does not claim that infants need pass a false belief test, in order to count as possessing a 'theory of mind', as he used the term. By contrast, Grice's account of non-natural meaning leans heavily on the attribution of beliefs to others. It does so in two respects.

In the first instance, Grice carves the realm of utterances into two distinct types of perlocutionary intention: those that seek to produce beliefs in others (typically taking the form of utterances of indicative sentences), and those that seek to move others to act (typically taking the form of utterances of imperative sentences).

Indicative or quasi-indicative utterances are connected with the generation of beliefs,

imperative or quasi-imperative utterances are connected with the generation of actions. (SWW, p.105)

Given this, Grice specifies  $r$  in (1) as an intention either to produce a belief in one's interlocutor, or as an intention to have him or her produce an action. To produce or understand an utterance with content of the first kind would therefore require speaker and hearer to possess the concept of belief.

In a second invocation of folk psychological concepts, clause three suggests a further substantive claim about the cognitive abilities necessary for meaning. This is because it demands that a speaker intend her utterance to constitute a *reason* for her interlocutor's responding - either in the form of a reason for action, or in the form of a reason for belief. In the cases of both imperative and indicative utterances, these must be characterised by reference to belief. In the case of indicative style utterances, an appeal to belief is required to spell out what it is that a speaker intends in the first clause of the analysis. Given the presence of the third clause in the analysis, it is similarly implied in imperative utterances, since the intention behind utterances like these is said to be an intention to provide for one's interlocutor a reason for action. A subject has a reason for action when the action can be represented as the conclusion of a practical syllogism the premises of which are beliefs and desires. For example: if one wants  $a$ , and believes that doing  $q$  is sufficient to bring about  $a$ , then one has a reason to do  $q$ . Therefore utterances of both imperatival and indicative form, on the Gricean analysis, are specified in terms that would require that speakers possess a concept of belief.

#### Problems with Grice's analysis

The role of folk-psychological concepts in Grice's analysis allows us to read off his account not just an answer to question (I) posed at the outset, but also an answer to question (II). However, Gricean answers to both (I) and (II) are controversial. The plausibility of using Grice's analysis of non-natural meaning as the basis for an account of the cognitive abilities

that must be attributed to young infants to explain their grasp of communicative intent may yet prove problematic on both conceptual and empirical grounds. With respect to the conceptual issues, it has often been thought that Grice failed to identify necessary and sufficient conditions for a speaker's meaning something by an utterance. With respect to the empirical demands on meaningful interaction imposed by Grice's analysis, a recent argument from Richard Breheny (2006) has challenged the plausibility of attributing to pre-linguistic infants cognitive abilities as sophisticated as those deemed necessary and sufficient for meaning something by an utterance on the Gricean analysis. I start off by considering this empirical challenge to the attribution of Gricean intentions to pre-linguistic infants. In due course I will return to consider the philosophical question of whether Grice does indeed fail to identify necessary and sufficient conditions for a speaker's meaning something by an utterance.

#### An empirical challenge to the Gricean analysis

Breheny (2006) has recently argued that infants could not grasp Gricean intentions. This is because grasping such intentions requires a concept of belief. In Breheny's words, the idea that infants could be said to possess a concept of belief "conflicts with one of the more robust findings in developmental psychology: that children below the age of four years do not possess these abilities" (Breheny 2006, p.74). The "robust" empirical findings that Breheny has in mind are experimental data on young infants' performance on false belief tests, which were discussed in the previous chapter. Breheny argues that an ability to pass a false belief test is a necessary condition of having the concept of belief, because the capacity to grasp that beliefs can be false is necessary for having a concept of belief. Since infants do not systematically pass false belief tests until they are about four, Breheny concludes that they lack the concept of belief. Consequently they could not grasp Gricean intentions.

If Breheny is correct, the following problem arises. It seems necessary that for infants to



come to learn to use and understand others' uses of words, they should have some grasp of others' communicative intentions. However, on the Gricean account of what it is to grasp a communicative intention, this would require cognitive abilities that infants lack. In this case their ability to use and understand others' uses of language would seem to be impossible, despite the fact that infants actually do so. This conclusion is clearly unacceptable. However, it could be rejected in one or both of two ways.

First, it might motivate us to find fault with Grice's analysis of the communicative act. If infants must have some grasp of meaning in order to acquire language, then since they acquire language around the beginning of their first year, this understanding of meaning could not possibly demand cognitive abilities that they lack. If the Gricean analysis suggests otherwise, it cannot be the correct analysis of what it is to act with communicative intent. Here an empirical constraint is used to challenge the Gricean account: a correct analysis of meaning must be an analysis of something that users of language, some of whom are young infants, actually do. Consequently, it cannot invoke cognitive abilities that the users of language lack. This empirical constraint might be supplemented with further conceptual constraints. For example, it may be that the analysis of meaning that Grice articulates is conceptually inadequate, because it fails to provide either necessary or sufficient conditions for meaning something by an utterance.

Alternatively, rather than looking to find fault with the Gricean analysis, one could reject the conclusion that infants lack the folk psychological abilities that the Gricean posits as necessary for meaning. On this approach, the Gricean analysis might be defended as correct, but the existing accounts of infants' folk-psychological abilities wrong. The empirical claim here may well be correct. Since Breheny's article was written<sup>15</sup>, new research has emerged, by Onishi & Baillargeon (2005), and by Victoria Southgate and her

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<sup>15</sup> Breheny's paper was not published until 2006 but earlier versions existed in 2001.

collaborators (Southgate, Senju & Csibra 2007), that suggests that infants of fifteen months may have some grasp of the possibility of false belief. If this is true then infants of fourteen months may already possess the cognitive abilities necessary to understand meaning in just the manner specified by Grice.

Of course, even if the empirical evidence is consistent with the attribution of Gricean intentions to infants, it may still be the case that the Gricean analysis is wrong. In this case, it may be that no one - neither infant nor adult speaker - need have Gricean intentions to count as understanding meaning, and that questions about whether or not infants have a concept of belief will be simply irrelevant to questions about their grasp of meaning. Such knowledge would not figure in a true answer to question (II).

An adequate answer to (II) will need to be developed in light of both empirical and conceptual considerations. In what follows, I'm going to adopt the following approach. I'll give straightforwardly philosophical arguments to suggest that a modified version of Grice's analysis of non-natural meaning gives a good account of what it is to have or to grasp a communicative intention. I'll then return to empirical work to argue that the cognitive abilities posited as necessary and sufficient on this revised Gricean account of the communicative act can be attributed to infants taking their first steps on the road to language. I want to start by considering the necessity of the conditions that Grice posits.

Challenges to the necessity of Grice's analysis

Problems with clause (3)

A first challenge to the necessity of Grice's account of non-natural meaning takes issue with the third clause of his analysis. Grice introduced (3) to rule out candidate cases of meaning that he found intuitively unacceptable (*SWW*, p.218). This clause stipulates that a hearer's intended performance of 'r' should be tied essentially to her recognition of the speaker's intention in speaking. Stephen Neale (Neale 1992, p.548) interprets this as constituting a

prohibition on the meaningfulness of any utterance for which the means by which a speaker utters is such that a hearer will inevitably respond in the intended way independently of any recognition of the speaker's intentions (1) and (2). This will typically occur when the utterance itself bears something like a natural property the presence of which is sufficient for a hearer's *r*-ing in the intended way.

The most well-known of these cases is that of Herod's presenting Salomé with the head of St John the Baptist on a charger. According to Grice, even if Herod presented Salomé with this head, intending both:

- (1) That she should come to believe that St John the Baptist was dead, and
- (2) That she should recognise his intention (1),

nonetheless we "certainly do not think that we should want to say that we have here [a case] of meaning<sub>NN</sub>" (*SWW*, p.218). This is because in the above case, what leads Salomé to conclude that John the Baptist is dead is not any recognition of Herod's intention that she should know this, but the sight of his severed head on a charger. According to Grice, this makes any intentions with which Herod arranged the evidence for Salomé become irrelevant since, had they been absent, Salomé's conclusion would have remained the same. He thinks this unacceptable: unless the hearer's response is tied essentially to his recognition of a speaker's intentions, the analysis of meaning fails to register crucial distinctions between "for example, 'deliberately and openly letting someone know' and 'telling' and between 'getting someone to think' and 'telling'" (*ibid.*). Clause (3) is introduced to shore up this distinction.

Unfortunately for Grice's intuitions, there exist counter-examples that suggest that (3) rules that some uncontroversially meaningful utterances are not meaningful. Consequently (3) is too strong and should be dropped. These counter-examples are utterances that meet conditions (1) and (2) but not (3) but which are meaningful nonetheless. Neale (1992, p.548) gives two examples. This first of these (which he attributes to Neil Smith) is of a speaker's

saying, in a squeaky voice, "I can talk in a squeaky voice". The second (which he attributes to Schiffer and to François Recanati) is a speaker's yelling "I'm right here!" to someone known to be looking for her. In both cases, the 'naturally meaningful' properties of the utterance are sufficient for the possibility of a hearer's *r*-ing in the intended way, but there seems to be no reason to doubt the meaningfulness of utterances like these ones. Consequently, the claim that (3) is necessary for meaning should be dropped. That doesn't mean that speakers don't sometimes speak with intentions like those in (3) - it's just that one needn't do so in order to mean something by uttering.

This leaves just clauses (1) and (2).

Problems with clause (1)

A common approach to the specification of *r* in the first clause of the analysis has been that all meaningful utterances can be analysed either as indicatives, the intentions underlying which would be the formation of *beliefs*, or as imperatives, for which the intended responses would be *actions*.<sup>16</sup> This approach has been adopted not just by Grice, but also by Griceans such as Steven Schiffer (1972) and Anita Avramides (1989). On their accounts, *r* is taken to be specifiable in one of two ways. Either:

(1a) *S* intends, by uttering *x*, to produce the *belief that p* in *H*

or:

(1b) *S* intends, by uttering *x*, that *H should ψ*

where *ψ* ('*psi*') specifies an action that *S* intends that *H* should perform. I'll refer to (1a) and (1b) as *utterance types*, where what is typed here is a general specification of the sort

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<sup>16</sup> Presumably questions would be analysed as variants of indicative sentences, after Frege - although none of Grice, Schiffer and Avramides, so far as I'm aware, says this explicitly. Note also that none of these authors would deny that there are occasions on which sentences with an imperative mood can be used to make assertions, or that there are occasions when indicative statements can be used to issue orders, and so on. It is uncontroversial that mood and force can come apart.

of perlocutionary intention with which one could produce meaningful actions.

A problem for these formulations of (1) is that there exist utterances the purpose of which seems to be the production of neither an action, nor a belief. Recalcitrant examples include utterances more like (1a) than (1b), but which cannot be directed straightforwardly at the generation of belief as specified in (1a), since the speakers know that their interlocutors already know what they intend to assert. For example, a teacher, in examining his pupil, might ask who won the American civil war. It cannot be right to say of the schoolgirl that, in responding that "The North won the civil war," she intends to produce in her teacher the belief that the North won the American Civil War. This is because she knows that her teacher already knows this. A similar case is that of the wife who confronts her husband, whom she knows to have been cheating. When she tells him "You've been with a woman!" she hardly expects him to form a belief about what he already knows to be true.

A better construal of these cases might read the pupil as wishing to communicate to the teacher that she, *the pupil*, knows who won the war; and that the wife wishes to communicate to her husband that *she* knows that he's been cheating. In such cases, the relevant intention may not be the production of the *belief that p* in *H*, as the current analysis specifies, so much as the production in *H* of the *belief that S knows that p*. This view is defended by Grice (*SWW*, p.110), who amends his (1a) accordingly. However, it's rightly rejected by Schiffer on the grounds that a speaker need not speak with the intention of producing in her interlocutor beliefs about her own beliefs.

Schiffer's preferred reformulation of (1) appeals to the concept of *activated belief* (Schiffer 1972, p.51). Thus, for example, the schoolgirl would speak to her examiner not with the intention of producing in him a new belief, or producing in him beliefs about her own mental states, but with the intention of bringing to the front of the examiner's mind (i.e. 'activating') his pre-existing belief in the date of the American Civil War. However, this

analysis also fails to fit the facts of our language use and so cannot be correct. Even in the original case, it's perfectly plausible that the schoolgirl already knows her teacher's belief to be active, and so speaks without producing any such intention in him. Thus both Schiffer's and Grice's attempts to specify the response that a speaker intends in (1) permit counter-examples. How, then, should the response that a speaker intends in (1a)-type utterances be specified?

One possibility that Schiffer and Grice do not consider is that there may be no single type of perlocutionary intention with which all utterances similar to (1a) are uttered. Indeed, against Schiffer and Grice, I suggest that it's simply false that all of our utterances are intended either to lead others to act or to produce in them beliefs (as Grice's distinction between (1a) and (1b) suggests), and similarly false that all informative utterances are uttered with the intention to activate beliefs in others, or to inform them of one's own beliefs. Beyond the preference for an elegant and simple analysis of communicative intentions, there's no good reason for thinking that all of our utterances must be one of two types. Furthermore, there are good reasons - in the form of utterances that aren't at home in either (1a) or (1b) - for concluding that there are more than two utterances types.

One example of an utterance type not considered by Schiffer and Grice is suggested by Tomasello, Liszkowski and Carpenter (2007). According to them, there exists a type of utterance that, although it might typically be uttered using indicative sentences like those used for (1a) style utterances, is not uttered with the intention to inform others, and thereby instil in them beliefs. Rather, in these cases a speaker utters with the intention of bringing her interlocutor to *share* a *feeling* towards or an *interest* in or *attention* to an object with others. They argue that some of infants' early pointing gestures that are typically construed as precursors to assertions are actually less like (1a) type utterances, and are better understood as utterances of this type (Tomasello, Liszkowski & Carpenter 2007, pp.712-713). Here the point of uttering would not be to inform the hearer of

anything, but to engage him in a shared experience of attention or emotion that the speaker enjoyed. An example of similar cases of early word-use might also be given, once again taken from Tomasello's *First Verbs*. At 18 months and 6 days, Travis uttered "Raisins gone," after finding and looking into an empty box of raisins (p.288). Here it may be that she spoke only with the intention of sharing with her interlocutor her amusement at the disappearance of the raisins, without intending to produce in him any beliefs at all. This could be done whether or not a speaker understood that her interlocutor's grasp of a state of affairs could be false - and so whether or not she could pass a false belief test.

I suggest that we often we perform utterances like this, and that in doing so we communicate with neither the intention of producing beliefs in others, nor of activating their beliefs. I illustrate this case with an example of my own.

Imagine that my brother and I are walking through a park in Paris, and that we pass by a variety of objects and events that we call out to one another. We both speak good French, and we both know this of the other. Our dialogue runs as follows:

*S*: C'est un chien!

*H*: C'est un oiseau!

*S*: C'est la pelouse!

*H*: C'est la pétanque!

We carry on like this, pointing to the objects that are clearly visible and immediately in front of us (a dog, a bird, the lawn, a game of boules, and so on) and calling out their names. This interaction is clearly meaningful: there's no doubt that we are communicating with one another. However, our utterances aim neither at generating actions, nor at the production of beliefs, nor even at the activation of beliefs. My brother already possesses all of the beliefs - for example, the presence of the objects, or their names - that, on the existing analysis, I could be seeking to produce in him, and vice versa. Moreover, these beliefs are active: I know that my brother actively knows the names of these familiar objects, and vice

versa; there's no sense in which I'm speaking with the intention to remind him of their names, or reactivate some of his dormant beliefs. Since my brother already knows that I already believe everything that I'm saying, there's not even a sense in which I utter with the intention of producing in him beliefs about what I believe, and vice versa. Finally, nor is there any sense in which I speak with the intention of producing in my brother beliefs about what I actively believe, and vice versa, since he already knows that I have these beliefs and that they are active. Such uses of language cannot be explained by either the Gricean or the Schifferian analyses.

What's going on here, I suggest, is that my brother and I are speaking, not with the intention of informing one another about anything, but with the intention of thereby sharing our enjoyment of the scene - the experience of being in Paris together, and our enjoyment of speaking French, and of our attending to the different objects before us. Without too much effort one could come up with countless similar examples of our using language to share in this way. Not least, this is because when it becomes clear that sometimes the sharing of feelings and attention is the *only* plausible explanation of speakers' intentions in communication, so in other cases in which beliefs are produced incidentally, it will come to be seen that an intention to share experiences is a better explanation of a speaker's intentions in speaking than the Gricean and Schifferian explications. Cases like this require the introduction of a third disjunct in (1):

(1c) *S* intends, by uttering *x*, to *H* to share with *S* an interest in or feeling towards some object *o*.

Recognising this is consistent with recognising that even if the sorts of intentions described by Grice and Schiffer in (1a) and (1b) are fairly paradigmatic cases of perlocutionary intention, they are by no means the only ones.

I suggest that in addition to this third disjunct we should be open to the possibility that there may be further varieties of motives with which we communicate, over and above (1a),



(1b) and (1c), such that even with the introduction of (1c), a taxonomy of speech acts is far from complete. Additionally, each class of utterance types might need further subclasses. For example, the current formulation of (1c) might be better divided into subtypes of (1ci) intentions to share interest or attention and (1cii) intentions to share feelings. Similarly, informative uses of language like those of (1a) might not be produced with any particular intention common to each one, so much as a family of resembling intentions that need to be accommodated under two or more subgroups. This might be necessary in order to accommodate the sorts of cases over which Schiffer and Grice disagree. Subsets of (1a) might include (1ai) cases where a speaker intends to produce in her interlocutor beliefs about her own beliefs; (1aia) cases where she intends to activate her interlocutor's dormant beliefs, and so on. Into the subsets of (1a) might be added some cases of communication that are similar to other cases of informative utterances, but which nonetheless do not aim at belief production - perhaps because they were uttered by speakers who lack a concept of belief, and merely aim to describe how the world is, or how some act should be performed. Again, examples of such uses might be found in Tomasello's *First Verbs*. For example, at 20 months, 18 days Travis uttered "Daddy made this like this," holding up a drawing (p.297). Here Travis's utterance seems to have been intended to inform, since its character suggests that she was showing someone how a certain effect could be or had been achieved. But this might be possible whether or not she could grasp that others' perspectives on the world could be *false*. It's consequently not obvious that such intentions couldn't be entertained by those who couldn't pass false belief tests. I won't try to develop a more expansive taxonomy of utterance types and subtypes here, but it should not be difficult to imagine how the details might be developed.

One might worry that acknowledging the variety of types of perlocutionary intention with which one could act meaningfully threatens the necessity of the analysis of meaning. This is because, to the extent that it's been conceded that the existing taxonomy might be far from complete, it has been acknowledged that there may be types of meaningful utterance

that are not specified in the disjunction of acceptable formulations of *r* in (1) contained in the explication of the analysis, such that none of the utterances types listed in the disjunction is actually necessary for meaning something by an utterance.

In response to this threat to the necessity of the analysis, it might be tempting to leave open the specification of *r* in (1), as a means to acknowledging the possibility that there exist other types of meaningful act. This handling of *r*, though, needs to be approached with caution, since too lax a specification would threaten the sufficiency of the analysis by permitting actions that are clearly not meaningful. For example, if the types of *r* that are acceptable in (1) is left completely open, then it's consistent with the analysis of meaning that my utterance of *x* might consist of stabbing you, with the intention that you would bleed, and with the intention that you recognise that this is my intention. This satisfies (1) and (2) of Grice's analysis, but there's no inclination to say of me here that my stabbing meant (non-naturally) that you would bleed (although presumably it did mean (naturally) that you would bleed). On Grice's original analysis, cases like this are ruled out from being counted as meaningful twice over. First, they are ruled out by the presence of clause (3) in the analysis, since in the stabbing case the 'speaker' clearly does not intend that the 'hearer' should bleed in virtue of recognising the speaker's intention that he should, so much as in response to the natural properties of the 'utterance' - namely, his receipt of a knife wound. Second, they are ruled out by the nature of the perlocutionary intention specified in the first clause of the original analysis, according to which, if an utterance is to be meaningful it's necessary that it be produced with the intention of producing either a belief (under (1a)) in the hearer, or of having her perform an action (under (1b)) - and bleeding is neither a belief nor an action. Since (3) has been deemed to be unnecessary for meaning something by an utterance, it can no longer be used as a basis for discounting the stabbing case from being meaningful. But additionally, if any specification of *r* in (1) were tolerated, then the second basis for ruling out the recalcitrant case has also been lost. In this case, the sufficiency of the analysis would be threatened. Consequently, leaving *r* in (1)

completely unspecified is not an option.

I propose that the best way to accommodate this difficulty is simply to accept the possibility that there may be more utterance types than those listed under (1a)-(1c), and to acknowledge this as a limitation of the analysis. Even with this concession, the resultant analysis still provides an explication of the conditions that are necessary (and potentially also sufficient) for each of the utterance types (1a)-(1c) identified. This may be unsatisfying to some, since it falls short of an analysis of meaning that incorporates a full taxonomy of all utterance types. However, it's sufficient for the purposes of this chapter. The point of introducing Grice here was not, after all, to provide a complete taxonomy of all of our uses of language but to use his analysis to focus our understanding of what it is to act with or grasp at least some fairly paradigmatic cases of communicative intent, and of the cognitive abilities that would be necessary to do so. This has been done.

Setting this issue aside, it should be noted that the revisions outlined above provide us with two ways of resisting Breheny's objection that the contents of indicative-style utterances must involve belief specifications, and that consequently infants could not produce such utterances. First, it may be that the utterances are of the form of (1c) and not (1a), such that the utterances are not produced with intentions to inform, even if the sentences uttered are indicative in mood. Second, even if the utterances are more like (1a) than (1c), their contents might be spelled out without appeal to belief. Breheny's conclusion can therefore be met in the following way. We can agree with him that *if* infants of 15-months do lack a concept of belief - and recall that there is evidence to suggest that this conclusion is less "robust" than Breheny claims - then this would preclude their uttering with the intentions the contents of which are belief-involving. However, we can object that Grice (and following Grice, Breheny) over-stated the extent to which it is necessary to specify the contents of the intentions of many of our utterances in terms of intentions to produce beliefs. Since there are potentially many utterances the contents of which are not belief-

involving, there are many utterances, even of indicative sentences, that infants who might fail false belief tests could nonetheless produce.

What should be said about infants' ability to grasp utterances when others speak to them with intentions of producing in them beliefs? Here one might worry that Breheny's objection still has some force, since even if infants could produce and understand some communicative intentions (those without beliefs in the content), they still could not produce or grasp those utterances that aimed at belief-production. In the case of production, this wouldn't be a problem - infants just wouldn't produce some sorts of utterance. However, with respect to comprehension, it may be that others do sometimes speak to infants with intentions that aim at belief production. In this case infants could not grasp these utterances. In response to this objection I suggest that a plausible construal of cases like this is that infants might not grasp the exact intentions with which a speaker speaks, but they get something near enough and the difference doesn't matter to the speaker. If I speak to you with the intention of producing in you a *belief* that, for example, the raisins are gone, and if you grasp only that the world is such that the raisins are gone - independently of any grasp of my intentions with respect to your beliefs - then ordinarily my communicative intention will be fulfilled adequately to any standard of correctness that I care about (particularly if I know that I'm talking to a young infant).

Two conclusions can now be drawn with respect to the specification of *r* in the first clause of Grice's analysis. First, the dispute between Schiffer and Grice about how to specify the nature of the *r* in (1a) style sentences is a distraction: Grice's theory can tolerate a variety of different types of perlocutionary intention in the specification of the first clause and it's empirically appropriate that it should. Second, revising Grice's analysis to reflect this shows that Breheny's empirical challenge to the first clause of the analysis can be met, such that there is no obvious problem about attributing a great many of the communicative intentions like those in (1) to infants younger than four.

## Challenges to the sufficiency of Grice's analysis

The previous discussion started off by considering arguments that suggest that Grice's analysis is too restrictive - mistakenly counting as necessary inessential features of the communicative act. I now consider an argument that the account of the communicative act that Grice sets out is not strong enough to rule out some cases that are not appropriately thought of as communicative acts. A certain class of counter-examples show that Grice's analysis fails to provide sufficient conditions for a speaker's meaning something by an utterance, since in these cases conditions (1)-(3) are met, but still it would be counterintuitive to say of the speaker that she had meant something by uttering. What these counter-examples have in common is the presence of a 'sneaky' or deceptive intention on the part of the speaker. I'll give just one example. It comes from P.F. Strawson's 1970 paper 'Intention and Convention in Speech Acts' (reprinted in Strawson 2004, p.120) and was retold by Schiffer (1972, p.17ff.).

Suppose that *H* wants to buy a house that *S* believes to be rat infested, but which she cannot prove is rat infested. *S* wants to bring *H* to believe that the house is rat-infested and so contrives evidence to induce this belief in *H*. In the absence of adequate proof, *S* releases some rats into the house, knowing that *H* is watching her do so, but also knowing that *H* does not know that *S* knows that *H* is watching her. *S* intends that *H* should, in virtue of his seeing her releasing the rats, infer that the house is rat-infested. However, she intends that he should infer this not from the presence of the rats - since he has seen *S* release these into the house. Rather, *S* intends that *H* come to this conclusion because he recognises that *S* would not have released the rats into the house unless she had good reason for thinking the house to be rat-infested already.

In this example, the conditions deemed necessary and sufficient (by Grice) for a speaker's meaning something by an utterance (on the original analysis) are all met:

- (1) *S* intends, by releasing the rats in *H*'s view, to induce in him a belief that the house is rat-infested,
- (2) *S* intends *H* to recognise her intention that he come to believe the house is rat-infested, and
- (3) *S* intends the fulfilment of the intention in (2) - namely *H*'s recognising *S*'s intention - to be at least in part *H*'s reason for fulfilling the intention in (1).

However, as Schiffer, Strawson and Grice (*SWW*, p.96) agree, one would not say here that *S*'s releasing of the rats was meaningful: she did not so much communicate to *H* that his house was rat-infested as contrive (bogus) evidence to this effect. In response to this challenge, Strawson suggests a minimum further condition, namely that:

[*S*] should not only intend [*H*] to recognise [her] intention to get [*H*] to think that *p* ... [she] should also *intend* [*H*] *to recognise [her] intention to get [*H*] to recognise [her] intention* to get [*H*] to think that *p*. (2004, p.120)

This would generate a fourth clause in the analysis:

- (4) *S* intends that *H* should recognise *S*'s intention (2).

This fourth clause fixes the problem identified in Strawson's counter-example. However, Strawson worries (2004, p.121), and Schiffer confirms (1972, p.18ff.), that further counter-examples to the sufficiency of the revised analysis could be contrived - with the consequence that more clauses (possibly countless in number) would need to be introduced to make the analysis sufficient. However, as Grice himself notes (*SWW*, p.99), and as Neale also argues (Neale 1992, p.550), all of these potentially problematic cases in which a speaker has 'deceptive intentions' with respect to what she intends her interlocutor to do could be blocked by the introduction of a clause ruling out such cases. In this case, the introduction of the following clause can ensure the sufficiency of Grice's analysis for an account of communicative intention:

- (4) *S* does not intend that *H* should be deceived about her intentions (1) and (2).

Given that clause (3) of Grice's original analysis has been shown to be unnecessary, this fourth clause becomes (3) in the new analysis. The following characterisation of the communicative act can now be given.

"*S* meant something by uttering *x*" is true if and only if, for some audience [or hearer, *H*], *S* uttered *x* intending:

- (1) *H* to produce a particular response *r*,
- (2) *H* to recognise that *S* intends (1).

Additionally, *S* should *not* utter *x* with any further intention that:

- (3) *H* be deceived about her intentions (1) and (2).

In addition to the very different third clause suggested here, this revised Gricean account permits a greater variety of specifications of *r* in (1) than did the original analysis.<sup>17</sup> In a more expansive treatment, more would need to be said about the correct way to specify *r* in each of the utterance types and subtypes. It may also be that some further aspects of this characterisation of the communicative act stand in need of fine-tuning. However, I suggest that it now constitutes a good starting point for an account of the communicative act that is absent from Bloom's account. If it is also plausible that infants of 14-months possess the cognitive abilities that revised-Gricean account posits as necessary, then we have good grounds to adopt the above characterisation.

I also suggest that this account of the communicative act is the correct way to understand what Bloom has in mind by 'referential intentions' - a phrase that was, in the preceding chapter, deemed to be insufficiently clear. On the Gricean account, a referential intention would just be one variety of or one aspect of a communicative intention. In most of the cases that Bloom describes, a referential intention would be a communicative act in which a speaker uttered with the intention of giving the name of an object or property - for example, an intention in which the content of *r* in (1) is that *H* should grasp that *x* names

the object of *S*'s attention. Sometimes, giving the name of an object will be a speaker's perlocutionary intention. However, in other cases - like the different uses of "Fire!" discussed at the end of the previous chapter - knowing that a speaker's utterance refers to an object will be insufficient for knowing what it is that she's trying to communicate, because the *r* in (1) also specifies some further content. In these cases, it may be that one couldn't grasp a speaker's perlocutionary intention without grasping her referential intention - for example, without grasping that the uttered word 'fire' refers to the fire - but knowing the referential intention will not be sufficient for grasping the speaker's perlocutionary intention.

What the Gricean account adds to Bloom's unsatisfactory story is that referential acts have an intentional structure that can be spelled out in terms of clauses (1)-(3), and that they share this structure with other communicative acts. This explains the relationship between Bloom's account of referential intentions, which I previously rejected as underdeveloped and only part of the story, and the Gricean account of communicative intentions that I defend here.

Is the above characterisation empirically adequate?

The revised account defended here is theoretically more adequate than the original Gricean version. However, whether it is empirically adequate - whether the abilities that it posits as necessary and sufficient for meaning something by an utterance could also be attributed to young infants - is a further issue. To this end it should be asked: to what answer to (II) does the revised account of the communicative act commit us, and can the cognitive abilities stated to be necessary in this answer properly be attributed to young infants?

Given the changes made to (1) and the rejection of Grice's original clause (3), Breheny's

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<sup>17</sup> With the exception of the changes that I have recommended to the specification of the contents of *r*



empirical charge has been blocked. On the evidence produced here, possessing a concept of belief is not a necessary condition for either producing or grasping others' communicative intentions. Consequently, whether or not infants can pass false belief tests at 15-months is not obviously relevant to the question of whether they can use and understand language in communicative interaction. At the same time, the sorts of perlocutionary intentions that infants would need to grasp in order to use language communicatively are not merely referential intentions of the sort described (inadequately) by Bloom. In due course, the nature of these perlocutionary intentions will be illustrated with examples. In the meantime, I consider clauses (2) and (3). A wealth of evidence for whether such abilities can appropriately be attributed to young infants can be found in a series of recent papers on infant pointing.

A first point to make about the new third clause is that it does not require the presence of a further intention on the part of a communicator that her intentions (1) and (2) be sincere. It demands only that she does not intend to be engaging in any dissemblance: that she does not have a further deceptive intention. The third clause is activated only when a speaker has a further deceptive intention in addition to (1) and (2), at which point (3) rules the utterance of *x* to be non-communicative. Consequently, the presence of (3) places no further cognitive demands on the nature of the abilities required for the production of linguistic interactions. Where deceptive intentions are absent, (1) and (2) are together sufficient for acting with communicative intent. Additionally, for all that (3) is theoretically necessary, it may be empirically superfluous with respect to cases of infants' production of language. There is no evidence that infants of 1- and 2-years ever act with the dissembling intentions that would activate clause (3). In the words of Tomasello, Carpenter & Liszkowski: "1- and 2-year olds seemingly do not engage in this behaviour at all" (2007,

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in (1), the same characterisation is recommended by Stephen Neale (Neale 1992, p.550).

p.714).<sup>18</sup>

In addition to their not acting with intentions that would rule their utterances non-meaningful, a further infant pointing study shows that, on at least some occasions infants of 14 months are able to grasp others' first clause intentions - but only when their interlocutors also act with second clause intentions. In a very elegant experiment, Behne, Carpenter & Tomasello (2005) produced evidence that infants are able to *discriminate* communicative actions performed with an intentional structure like that of (1) and (2) from pseudo-communicative actions in which the second clause was absent. In addition, infants successfully identified the experimenter's intended message (that is, her perlocutionary intention) only in cases when the experimenter intended the infant to recognise her intention in (1) - that is, only where (2) was also present.

In this study, an experimenter produced a point-like gesture for infants of 14-24 months in two conditions: a communicative case, and a non-communicative case. In both cases, what was tested was whether an infant could grasp the experimenter's pointing-gesture as providing a clue to the location of a toy that the infant had previously seen and which had subsequently been hidden in one of two nearby buckets. The finding of this toy would

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<sup>18</sup> Tomasello, Carpenter & Liszkowski (2007) conclude that because infants don't entertain deceptive intentions, the intentions with which they communicate could not be like those with which adults communicate, but at best only a "primordial" analogue (p.715). This is because they take (a clause analogous to) the specification of (3) in the revised analysis to be a positive intention to act without dissemblance, rather than the absence of a deceptive intention, and so conclude that infants lacking any grasp of dissemblance could not act with the relevant blocking intentions. I see no reason to prefer this formulation, though. Moreover, it both prevents the adoption of a unified account of adult and infant communicative intentions and mischaracterises the phenomenology of mature communicative intentions. When communicating with others we don't do so with intentions not to deceive them, so much as without deceptive intentions.

constitute the speaker's perlocutionary intention  $r$  in (1). What was varied across the studies was whether or not the experimenter sought to bring the infant to recognise the communicative nature of her pointing gesture. In the communicative case, the experimenter produced clear indications of what we understand as communicative intent: she alternated her gaze between the bucket and the infant, raised her eyebrows and pointed to the correct bucket, with the intention of thereby informing the infant she intended it to grasp that the toy was in the correct bucket. In this study, infants of 14- and 18-months actively used her point to discover the location of the hidden toy.<sup>19</sup> In the non-communicative study, the experimenter produced gestures superficially resembling those of the first study, but without doing so in a manner expressive of her communicative intentions. She "held her hand in a pointing shape directed at the correct bucket (just as before), but while distractedly inspecting her wrist" (Tomasello et al. 2007, p.711) and gazed at the correct bucket with unfocussed eyes. In this study, in contrast to the study in which the experimenter used cues to express her communicative intent, infants selectively ignored her gestures and gazes and identified the correct location of the hidden toy only at chance.

The Behne, Carpenter & Tomasello study constitutes robust evidence that infants are able to identify others' communicative intentions only where a speaker's utterance of  $x$  is accompanied by signs - gaze-alternation, raised eyebrows, etc. - that would typically be indicative of intentions like (2); that is, that mature communicators would typically take to be indicative of a speaker's wanting others' to recognise her intention in (1). Furthermore,

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<sup>19</sup> As a fascinating point of comparison, it's worth noting that this is more than is managed by non-human primates. Describing a similar study, Tomasello, Carpenter & Liszkowski write:

Great apes typically follow the pointing gesture to the correct referent, the bucket, but they do not then know what the human intends by his pointing gesture and so fail to find the hidden food. (Tomasello, Carpenter & Liszkowski 2007, p.710)

just as infants are able to grasp others' perlocutionary intentions where these are accompanied by expressions associated with communicative intent, and unable to grasp others' intentions when they are not, in addition they also produce these cues for others when they are attempting to communicate messages of their own. From around their first birthday, before infants engage in communicative acts with others, infants "make sure they have the attention of the other, direct the act to them, make eye contact, and so forth" (*ibid.*). Consequently infants both act in light of and produce signs that would be characteristic of the intentions in the second clause of the revised account of meaning. This behaviour is consistent with its being the cases that infants, like adults and older children, produce and grasp intentions like those described in (2). However, these need not entail that infants actually grasp intentions like those in (2). There are (at least) two possible ways in which the results of the Behne et al. study could be construed.

One possibility is that infants understand that others alternate their gaze, raise their eyebrows, etc. in order to indicate that they have intentions like those in the first clause, and to express their intentions that others' recognise these intentions. Infants grasp that these signs are revelatory of underlying intentions, and they grasp the first clause intentions on the basis of grasping that speakers have second clause intentions. A second possibility that has yet to be ruled out is that (2) plays a causal role in producing a state of affairs in which the infant is brought to grasp what it is that the communicator intends her to do (the speaker's perlocutionary intention), but where the infant does not grasp that it is his interlocutor's intention that he should grasp her intention in (1). Rather, the infant's grasp of *r* in (1) is to be explained by some causal property of the communicative cues produced by the speaker. A characterisation of the role of communicative cues along these lines might be one possible application of the 'pedagogical' view of learning that has been developed and defended by György Gergely and Gergely Csibra (Gergely & Csibra 2005, Gergely, Egedy & Király 2007) and their collaborators. (Note, though, that this application to Grice hasn't been defended by any of the co-authors just listed. Additionally, the

application of one aspect of the pedagogical view that I have suggested here should not be taken as an endorsement of the whole view.)

According to the pedagogical view, there exists an adaptive mechanism, known as 'pedagogy', for the spontaneous transfer of social knowledge. This knowledge transfer is triggered by specific communicative cues including eye-contact and the prosodic pattern of motherese, among other cues - such that the ability to grasp intentions like those in (2), over and above those in (1), would not always be necessary for grasping the content that a speaker had intended to communicate. The presence of visual cues would cause hearers to respond *as if* they had grasped that their interlocutor had acted with communicative intent, causing them to acquire the information in (1) independently of any recognition of the intention in (2). The possibility of explaining this aspect of communicative interaction in either mechanistic or intentional terms is analogous to the possibility of giving either mechanistic (gaze-cueing and associating) or intentional explanations of an infant's grasp of the referent of a speaker's words, previously discussed in Chapter One.

Since there is robust evidence that by the age of 14 months infants already possess a strong understanding of the intentional character of others' activities that cannot be explained by appeal to simple associative mechanisms (see Chapter One), it may seem that such a mechanistic characterisation of this feature of communicative intent is unmotivated. It may also be thought that the superficial similarity of the speaker's communicative and non-communicative gestures in the Behne et al. study would suffice to rule out the non-intentional reading of (2). If extended fingers and direction of gaze are causal cues, then something like those cues was present in both the communicative and non-communicative cases. What was absent in the non-communicative case was not the cue but the communicative intention underlying it. These would constitute reasons for preferring the intentionalist reading of (2) - in other words, for taking infants to grasp signs of communicative intent in just the ways that we do. However, reasonable rejoinders can be

made to both of these points. First, it doesn't follow from the suitability of an intentionalist explanation for some aspects of infant communicative interaction (such as knowledge of reference) that a causal, non-intentionalist explanation may not be more suitable for the characterisation of other aspects. Second, the proponent of the non-intentionalist construal of (2) might object that Behne et al. simply failed to isolate the right causal cues to rule out a non-intentionalist interpretation of the results of their study. For example, it may be that certain patterns of gaze-alternation not common to the communicative and non-communicative cases tested by Behne et al. is causally responsible for an infant's grasping another's perlocutionary intentions. This possibility has not been ruled out.

I suggest that it's simply too early to make strong claims about whether the indicators of communicative intent typically associated with (2) play only a causal role, or are grasped by infants as indicators of an underlying intention. In what follows, I will continue to speak of the infant's grasp of communicative intent. Nonetheless, I don't want to rule out the possibility that a central feature of what I describe as the infant's *grasp* of communicative intent - namely the infant's relation to a speaker's intention in (2) - may be a simple causal mechanism. I don't believe that the plausibility of any claim I go on to make will hang on this qualification.

I want to finish this chapter by considering the range of perlocutionary intentions that infants are capable of grasping and which they produce in their earliest communicative interactions with others. In the previous chapter I argued that understanding another's utterance often requires not just grasping the referent of their utterance but also the perlocutionary intention in the service of which (as a means to achieving which) their utterance is produced. The same utterance might be produced in the pursuit of a variety of perlocutionary intentions, and unless one can grasp which one it is, one would fail to grasp the message that a speaker had intended to communicate. Empirical evidence supports the conclusion that prior to their acquisition of language, infants both point with and grasp that

others point with a variety of perlocutionary intentions, and that they do not intend that speakers merely recognise the referent of their points.

Tomasello, Carpenter & Liszkowski report a study by Liebal, Behne, Carpenter & Tomasello (eventually published in 2009) that sought to determine “how infants identify not the referent but the motive behind a pointing gesture” (Tomasello, Carpenter & Liszkowski, 2007, p.710). In two trials, an 18-month old infant and an experimenter would engage in gathering toys. In one trial, the toys would be tidied into a basket. In the second, the toys were hoops which would be stacked on a post. In both trials the experimenter would stop, mid-activity, and point to a particular toy. In each case the infant would take the point to be relevant to their shared activity: in the first trial, it would fetch the toy and place it in the basket; in the second, it would stack the hoop on the post. The infant acted differently in spite of the fact that in both conditions “the adult pointed to the same toy in the same way”. Thus it is concluded that “the infant extracted a different meaning in the two cases - based on the different joint attentional frame involved” (p.711). This is taken to constitute evidence that in different contexts of activity, infants grasp the different perlocutionary intentions with which the same ‘utterances’ are produced. Here the utterances in question are points: although they are clumsily characterised by the term ‘utterance’, the Gricean account of speaker meaning holds for both verbal and non-verbal communicative interactions.

Tomasello et al. produce evidence that infants point for a wide variety of reasons - for example, to request objects, to inform others of the location of things and to share attention with others, among others. They also produce evidence that infants are not satisfied that the recipient of a point should merely identify the referent of the infant’s point. Infants intend that others should not only recognise the referent of a point, but also their motive for which a referent has been picked out. A study by Liszkowski, reported by Tomasello, Carpenter & Liszkowski (2007, p.713), showed that when infants of 12-months

point to objects, they are typically unsatisfied (and so repeat their pointing gestures) when the experimenter responds to their point only by attending to its referent. Infants in the experiment appeared satisfied (and did not repair or repeat their pointing gestures) only in the condition in which the recipient engaged in a positive joint attention episode with the infant, alternating its gaze between the infant and the referent whilst emoting positively. This suggests that the infants (at least on some occasions) do not point merely with the intention of drawing others' attention to objects: they also point with further perlocutionary intentions.

### Conclusions

The purpose of this chapter was to set out an answer to (I), in the form of a theoretically adequate characterisation of what it is to act with and grasp a meaningful action, and an answer to (II), in the form of an account of the cognitive abilities that the answer to (I) required. It's my contention that the answers to (I) and (II) have been met, and furthermore that the answer to (II) has been shown to be empirically adequate. Acting with communicative intent, and grasping what another's means by an utterance, do not require cognitive abilities that could not be attributed to infants around the age of 15 months. What remains now is to show how it is that infants' ability to act with and grasp others' communicative intentions could feed into a story about their ability to use and understand others' uses of words in communicative interaction.



## Chapter 5: Perlocutionary and Illocutionary Intentions

In the previous chapter, it was argued that at the beginning of the second year of life, infants can produce and recognise intentions not very different from those described by Grice as necessary and sufficient for a speaker's meaning something by an utterance. On the revised account of a speaker's meaning something by an utterance that was defended:

"*S* meant something by uttering *x*" is true if and only if, for some audience [or hearer, *H*], *S* uttered *x* intending:

- (1) *H* to produce a particular response *r*,
- (2) *H* to recognise that *S* intends (1).

Additionally, *S* should *not* utter *x* with any further intention that:

- (3) *H* be deceived about her intentions (1) and (2).

In this chapter I want to consider two further objections to the Gricean analysis of speaker meaning, which have been developed in most detail by John Searle in his book *Speech Acts*

(hereafter *SA*).<sup>20</sup> Searle argues that Grice mis-characterises the nature of the communicative act, and that the account that Grice offers is “defective in at least two crucial respects” (*SA*, p.43). In particular, he objects to two claims that are made on the Gricean view. First, Searle objects that Grice “fails to account for the extent to which meaning can be a matter of rules or conventions” (*ibid.*). Second, Searle objects to Grice’s characterisation of an act of communication as a perlocutionary act - that is, an act motivated by an intention to make somebody do or think something by speaking. In contrast, Searle argues that communicative acts are illocutionary acts: acts motivated by intentions to perform speech acts with a particular force. In Searle’s words:

Grice in effect defines meaning in terms of an intention to perform a perlocutionary act, but saying something and meaning it is a matter of intending to perform an illocutionary, not necessarily a perlocutionary act. (*SA*, p.44).

If Searle’s objections to the Gricean account are well-founded, then the account of what it is to act with or grasp another’s communicative intent that I defended in the previous chapter would be wrong.

In the chapter to follow, I will discuss these objections to the Gricean account of communicative intentions. I will make two claims. First, I agree with Searle that meaning can be a matter of linguistic convention, but point out that it is not always so. While the Gricean model may not be adequate to characterise the intentional structure of communicative acts where linguistic conventions are known to speaker and hearer, it can be used to characterise those where conventions are not in play. This limits its application. Nonetheless it remains adequate to perform the task for which I propose to adopt it, namely the characterisation of those communicative interactions on the basis of participating in which infants are able to *acquire* knowledge of linguistic conventions. In this case, a Gricean account of communicative interaction can help us to explain how it could be that infants

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<sup>20</sup> A similar view is also contended by Max Black (Black 1973).

come to use and understand others' uses of words in communicative interaction. Second, I will argue that although some communicative intentions are illocutionary intentions, there is insufficient reason to conclude that infants' earliest communicative intentions are. In order to explain the communicative interactions of infants around the beginning of their second year, it's necessary to attribute to them an ability to produce and grasp perlocutionary intentions. It isn't necessary, though, to attribute to them any analogous understanding of illocutionary intentions.<sup>21</sup>

Before elaborating the first of Searle's objections to the Gricean account, it will be necessary to introduce some theoretical terms.

Two varieties of meaning

In his 1957 paper 'Meaning' (reprinted in Grice 1991), Grice distinguishes between various constitutive questions about meaning. For example, he distinguishes between questions about (A) what a speaker *S* meant by an utterance *x* on a particular occasion, and (B) the meaning of that utterance.

(A) What does it mean to say that "*S* meant by *x* that so-and-so (on a particular occasion)"?

(B) What does it mean to say that "*x* meant that so-and-so (on a particular occasion)"?

(A) asks what it is for a speaker to perform an act with meaning. What would make it true to say of a speaker (or utterer) *S* that she had meant (that so-and-so) by her utterance on a particular occasion? For example, under what conditions would it be true to say of me that by waving my hands when I did, I meant that *I'm coming over*? (B) asks about the meaning of an utterance - in this case, about what it is for an utterance to have a particular meaning on

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<sup>21</sup> It may also be that there exist philosophical grounds for thinking that perlocutionary intentions are conceptually basic with respect to illocutionary intentions. I am sympathetic to this possibility but will not argue for it here.

a given occasion. For example, what would it take for a given performance of my waving my hands to have a meaning equivalent to the English sentence "I'm coming over"? Or, to give an example from Grice (*Studies in the Way of Words*, p.88ff.), under what conditions would it be true that the words "I'll be helping the grass to grow" meant *I shall be dead*? (In what follows, I'll continue the practice of using italics to indicate the content of a communicative act.)

The difference between what Grice called (A) speaker (or utterer's) meaning and (B) utterance meaning is an important one. This is because a speaker can mean something by an utterance whether or not the vehicle of meaning that she uses to communicate her message has a meaning that is independent of its use by her - in Grice's words, whether or not an expression "has for some person or other an established standard or conventional meaning" (*SWW*, p.87). Furthermore, although it's often the case that what an utterance means and what a speaker means by uttering it coincide (as they do in Grice's analysis of non-natural meaning), this is not always the case. On occasion, words and sentences that have conventional meanings - that is, that are meaningful independently of their use by a speaker on a particular occasion - can be used to communicate messages very different from those that would ordinarily be communicated by utterances of those words and sentences. For example, in the right circumstances it may be that by uttering the sentence "John is punctual and has excellent handwriting," I can communicate to you a very different message, to the effect that John is not a good candidate for the job for which he's applied (*SWW*, p.33). In such circumstances, we might say - as Grice did - that what a *speaker meant* by an utterance was very different from what the utterance means. Since here the word 'meaning' is used to describe both the meaning of an utterance and what a speaker means by uttering it, this has the potential to be confusing. I will avoid confusion by using the word 'meaning' only to describe (B) utterance meaning. To talk about (A), I will refer to the speaker's message, or to what the speaker sought to communicate.

In addition to the distinction between (A) speaker meaning (i.e. the speaker's message) and (B) utterance meaning, a further distinction should also be drawn between different varieties of utterance meaning. Thus Grice asks a third question. Since the question pertains to a species of utterance meaning, I'll refer to it as (Bi):

(Bi) What does it mean to say that "x means (timeless) something (that so-and-so)"?

In many but by no means all cases, the action that a speaker performs as a means to the expression of her communicative intention is meaningful independently of its performance by a speaker on a given occasion. For example, the English sentence "I'm coming over" has a meaning in English quite independent of any individual's utterance of it. This is the sort of meaning to which Grice refers as 'timeless utterance meaning'. The words and sentences of a language, the meanings of which are the sorts of things that could be found in a dictionary or phrasebook, are paradigmatic cases of timeless meaning. As the earlier quote from Grice suggests, it's generally held that what determines the timeless meanings of words and sentences are the linguistic conventions of a community of language users. (More will be said about the nature of conventions in Chapter Six.) In contrast to the sentence "I'm coming over", the waving of one's hand does not have timeless meaning: at least in English, there is no standard or conventional meaning of waving. (Suppose, for the sake of argument, that the wave in question is idiosyncratic and visibly quite different from that with which we greet one another - which might be thought to have a timeless meaning analogous to an utterance of "Greetings!".) Where a speaker waved idiosyncratically with communicative intent, the meaning of her utterance would not be timeless. The sort of meaning that characterises utterances like this might be called (Bii) 'occasion utterance meaning'. Often these will be non-linguistic - as would be the case if I waved idiosyncratically as a means to telling you that I'm coming over. However, not all non-linguistic communicative acts lack timeless meaning. For example, in addition to the standard wave that we use to greet people, a pointing finger has, at least in Western culture, a timeless meaning roughly equivalent to the English sentence "Look there". (Of course, as with the sentences of a language, a pointed finger can be used to communicate a variety of messages - that is, can

be uttered as a means to the achievement of a variety of perlocutionary effects. Here the distinction between what an utterance means and what a speaker seeks to communicate by uttering remains pertinent.)<sup>22</sup>

Before discussing in more detail the role of timeless meaning in communicative interaction, it's worth remarking upon a further difference between utterances that have occasion meaning and utterances possessed of timeless meaning. There exists a form of normativity that attaches to utterances in a language but not to communicative interactions not couched in a language. This is the possibility of using words correctly and incorrectly - that is, in a way that is not licensed by or consistent with the 'rules' of a language (whatever form such rules may have). If a speaker of English used the word 'dog' to refer to cats, or 'Wednesday' to refer to the day before Tuesday, she would have spoken incorrectly. In utterances not made in a language governed by rules for use, this is not the case. There is still a sense in which communication is normative - insofar as it can communicate the speaker's intended message successfully or unsuccessfully; but this is not the sense of normativity characteristic of rule-governed language. My waving idiosyncratically with the intention of communicating to you that I'm coming over would be successful if my interlocutor thereby grasped that I was coming over, but there would be no sense in which I had used the vehicles in my utterance correctly, since there exist no 'rules' determining the communicative content of waving that could either be followed or violated in this case. By contrast, if I had uttered the words "I bought a dog on Wednesday" with the intention of

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<sup>22</sup> It should be noted that these are not the only distinctions pertaining to meaning between which Grice thinks it necessary to distinguish. For example, in the original article 'Meaning', Grice also distinguishes between timeless meaning and applied timeless meaning. Applied timeless meaning characterises terms like indexicals that are constant in meaning but which can be used to express different contents in different contexts. Furthermore, in Strands Four and Five of the 'Retrospective Epilogue' to *Studies in the Way of Words* yet more distinctions are introduced. Since these are not primarily relevant to the project in hand, I will not discuss them here.

stating that I had bought a cat on Monday, then there is a clear sense in which I have misspoken - even if I nonetheless succeed in communicating to you the message that I bought a cat on Monday. This distinction can be marked by noting that whereas linguistic (and other rule-governed) communicative interactions are characterised by correctness conditions and success conditions, utterances that have only occasion utterance meaning are governed only by success conditions. They can be successful or unsuccessful, but not correct or incorrect, since there exists no standard use of those signs relative to which an individual's uses could be correct or incorrect.

Grice took the notion of (A) speaker's meaning to be basic to the characterisation of (B) utterance meaning. That is, he thought that an account of sentence meaning could be given only by leaning upon the conceptual framework laid out in the analysis of (A), but that an account of (A) could be given independently of any account of sentence meaning. In particular, what Grice had in mind was that an account of (Bi) the timeless meanings of words and sentences should ultimately specify their meanings in terms of something like (A) what it is that speakers typically - if not always - seek to communicate by uttering those words. Thus at *SWW* p.101, he writes:

I would like, if I can, to treat meaning something by an utterance of a sentence as being only a special case of meaning something by an utterance (in my extended sense of utterance), and to treat a conventional correlation between a sentence and a specific response as providing only one of the ways in which an utterance may be correlated with a response.

For example, on such an account, the sentence "I'm coming over" might have the timeless meaning that it does because uttering that sentence is one way in which speakers typically communicate to their interlocutors the message that they are coming over, and so on. (There may be other ways of communicating this message, as Grice makes very clear.) If speakers typically uttered these words with different communicative intentions, then they

would not have the meaning that they do.<sup>23</sup> However, although Grice started to work on an account of the transition from (A) to (Bi), this was never completed.

Searle's first objection

With the distinction between (A) the speaker's message and (B) the meaning of her utterance now in place, Searle's first objection to Grice can be elaborated. Although it isn't always clear from what Searle says, there are two parts to his first criticism of Grice. These relate to two different contributions made by conventions to communication between a speaker and hearer - constitutive and epistemic. I discuss the epistemic contribution first.

Searle asks: how does one identify the meaning that a speaker's utterance has? His answer is that in a great many cases, at least, we understand what others have said in virtue of understanding the meanings of the words and sentences that they uttered. This is uncontroversial - as Grice, writing in response to Searle, acknowledges:

Of course, I would not want to deny that when the vehicle of meaning is a sentence (or the utterance of a sentence), the speaker's intentions are to be recognised in the normal case, by virtue of knowledge of the conventional use of the sentence[.] (*SWW*, pp.100f)

However, contra Grice, Searle holds that any explication of the intentional structure of communication should reflect the fact that interlocutors knowingly exploit their mutual knowledge of linguistic conventions - i.e. "the rules for using the expressions" (*SA*, p.45) - in order to make themselves understood. Since Grice's analysis fails to mention this fact, it's unsatisfactory.

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<sup>23</sup> In at least this respect, Grice would seem to be close to Wittgenstein's infamous claim that "the meaning of a word is its use in the language" (*PI* §43). However, it should be noted that Wittgenstein and Grice disagreed about how this appeal to use should be understood. See *SWW* chapter 1 for Grice's elaboration of some points of disagreement with Wittgenstein.



Searle's constitutive claim about the relationship between sentence meaning and the message that a speaker can intend to communicate is less obvious. He gives a rather contrived and intuition-laden argument for his constitutive claim using a now well-known example of an American soldier trying to persuade his Italian captors that he's a German soldier. Because a more economical and better argument for the same claim can be reconstructed, I won't repeat Searle's original argument.

Searle's constitutive question is: what are the features that determine the meaning that an utterance has? According to Searle, these features are (or can be) at least partly conventional, in addition to the intentional states mentioned by Grice. On Grice's account of speaker's meaning (if perhaps not his envisaged but not completed account of timeless meaning), only intentional facts about a speaker's psychology determine what it is that she should be said to mean by uttering. The speaker's message (A) is determined by the intentions with which she communicates; that is, the effect that she seeks to produce in her interlocutor by speaking. The meaning of a speaker's utterance  $x$  is also determined by the intentions with which  $x$  was uttered by  $S$ . Thus utterance meaning (B) is also closely related to the content  $r$  that the speaker intends her interlocutor to produce in (1). By contrast, Searle objects that where a speaker's utterance consists of words or sentences in a language, what determines the meaning of her utterance is not the intentions with which she speaks but the conventions of the language that she's speaking. He argues that this determines not only (B) the meaning of the speaker's utterance, but also (A) - the message she could be said to have meant by uttering. This is because "what we can mean is at least sometimes a function of what we are saying" (SA, p.45) - such that, in these cases, a speaker's message cannot be specified independently of the meanings of her words. This reverses the Gricean story: rather than a speaker's message determining the meaning of her utterance, on the Searlean account, a speaker's utterance determines - or at least constrains - her message.

An argument for this claim can be run as follows. As Grice himself recognises, one can intend to do only what one reasonably expects can be done. In his own words:

one cannot in general intend that some result should be achieved, if one knows that there is no likelihood that it will be achieved. (*SWW* p.101)

The impossibility of having intentions to do what one knows cannot be done constrains the message that a speaker could intend to communicate by an utterance. This is because where sentences have conventional meanings that are known (or assumed to be known) to both speaker and hearer, then it's typically the case that the hearer of an utterance will be inclined to take a speaker's words to be a reliable indicator of the message that she wishes to communicate. For example, unless given good reason not to do so, hearers would most likely take the utterance of a sentence like "I'm hungry" to express just the proposition that *I [the speaker] am hungry*. If one were to be greeted by an acquaintance who uttered that sentence, one would naturally take them to be expressing just that proposition - and perhaps suggesting that you should find somewhere to eat. Given this expectation, it's clear that one could intend, by uttering the sentence "I'm hungry", to communicate the message that one was hungry. However, from the fact that hearers are inclined to understand their interlocutors in this way, at least in the absence of evidence to the contrary, it also follows that they are wholly unlikely to take their interlocutors to be expressing very different messages. For example, if one were met by someone who uttered the words "I'm hungry", one would not be in the least bit inclined to take them to have meant that *the capybara is the world's largest species of rodent*.

That language-users have expectations like this constrains the messages that a speaker could seek to communicate by uttering a sentence, since it makes some communicative intentions impossible. Ordinarily, one could not intend to communicate the message *the capybara is the world's largest species of rodent* by uttering the sentence "I'm hungry", because competent speakers know full well that, in the absence of relevant stage setting, there is no likelihood that this intention would be fulfilled. That message would not be

communicated because there is ordinarily no likelihood that hearers could grasp the speaker's message on the basis of her utterance of that sentence. This argument establishes Searle's conclusion that what a speaker could communicate by an utterance is constrained by the meanings of the words that she utters.<sup>24</sup>

In light of Grice's failure to acknowledge the dual (constitutive and epistemic) role of linguistic conventions in communication, and his second illocutionary/perlocutionary objection that will be discussed in due course, Searle formulates a different account of the communicative act. He does not claim that his account is an analysis of what it is to mean something by an utterance, but rather of the slightly different concept of "saying something and meaning it". According to Searle, the analysis of such an act must be sensitive to "both the intentional and the conventional aspects [that govern the performance of a speech act] and especially the relationship between them" (*ibid.*). He modifies Grice's analysis of meaning to reflect the fact that:

in the performance of an illocutionary act in the literal utterance of a sentence, the speaker intends to produce a certain effect by means of getting the hearer to recognise his intention to produce that effect; and furthermore, if he is using words literally, he intends this recognition to be achieved in virtue of the fact that the rules for using the

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<sup>24</sup> Although a speaker's message is constrained by the words that she utters, it is nonetheless true that very often speakers say one thing and mean another. For example, it could be that on occasion a speaker uttered the sentence "I'm hungry" in order to communicate the proposition *I need a stiff drink*. However, in such cases, it would be necessary that a speaker provide her audience with clues that she does not expect to be taken literally. For example, she might raise her hand to her mouth in the manner of someone taking a shot of vodka, or rely on the fact that her interlocutor is a friend who knows that the speaker is an alcoholic who rarely eats. In such cases the communicative intention is possible, because there exists supplementary evidence on the basis of knowing which an interlocutor might grasp how the speaker intends to be understood - and because, in light of this information, the speaker can expect that her interlocutor grasp her message.

expressions he utters associate the expression with the production of that effect.

*(ibid.)*

In rewriting the Gricean analysis to take account of these facts, Searle gives a different account of the intentional structure with which one would have to act and that one would need to grasp in order to participate in communicative interaction. This positive account need not be elaborated in detail here, since it's both very long and of only secondary relevance. However, part of Searle's revised analysis holds that "S utters sentence T and means it" when she intends that her intention in speaking will be recognised by her interlocutor "in virtue of his [H's] knowledge of (certain of) the rules governing (the elements of) T", where T specifies a sentence in a language, the meaning of which is determined by rules that specify its correct use. This revision has implications for the application of the Gricean account of communicative interaction that I want to defend because it stipulates that acting with and grasping intentions over and above (1)-(3) are necessary for participating in communicative interaction.

Why is Searle's view problematic for the account that I want to defend?

There's no evidence that Grice intended that his account of (A) a speaker's message, and of the relationship between (B) utterance meaning and (A) speaker's message, should ever be appropriated for a developmental account of how it is that infants learn the meanings of others' words. Nonetheless, I suggest that in addition to its providing us with a workable account of what it is to act with or grasp another's communicative intention, the Gricean account of the communicative act also sheds light on how it could be that infants are able to learn the meanings of words. The claim that I will develop in Chapter Six is that infants are able to acquire knowledge of linguistic conventions because they are able to imitate others' intentions to produce perlocutionary effects, and the conventional means that they employ in pursuit of these ends. (This gives us a further reason for preferring to characterise young infants' grasp of communicative intentions in terms of perlocutionary intentions, since whereas it's fairly clear how one might grasp and imitate another's perlocutionary

intentions, it's less clear how one could grasp and imitate their illocutionary intentions.) Before developing this claim, though, I want to sketch out briefly the basics of how such a view might work - and the challenge to this view presented by Searle's rival account of the communicative act.

The revised Gricean account of the communicative act in the previous chapter constitutes the first stage of an answer to the question: what are the cognitive abilities that must be attributed to young infants to explain their ability to use language and understand others' uses of language in communicative interaction? Part of an answer to this question is that infants must have some grasp of linguistic conventions - but this too must be explained. Some story about how - in virtue of possessing which cognitive abilities - infants acquire knowledge of linguistic conventions is necessary. On the view of language acquisition that I want to defend, independently of any grasp of linguistic conventions, there are occasions on which infants grasp speakers' perlocutionary intentions. This makes possible the following explanation. Infants are able to acquire linguistic-conventions because prior to acquiring a language, and so prior to their grasping linguistic conventions, they can grasp what it is that others are trying to communicating to them. In these communicative interactions, they take the meanings of others' utterances (B) to be closely related to the messages they grasp others to be communicating (A), and thereby learn the meanings of the utterances that they hear. For example, it may be that an infant learns the meaning of the word 'dog' when it grasps that a speaker who looks to a dog and utters "Dog!" is doing so with the perlocutionary intention of naming the dog, because it takes the meaning of the word to be closely related to what it grasps that the speaker had intended to communicate. Here it's knowledge of a speaker's message that determines the infant's grasp of what the speaker's utterance could mean.

Central to the credibility of this view is the idea that infants can participate in communicative interactions with others without possessing any prior knowledge of the

meanings of the words that those others speak. In this case, it's central to the nature of the communicative interaction to which I want to appeal that knowledge of linguistic conventions should not be necessary for the possibility of communication. On the surface, though, it looks like Searle's explicit revisions to the Gricean account challenges this hypothesis.

Searle surely doesn't think that communication is possible only where a hearer grasps a speaker's message by virtue of knowing the meaning of the words that she utters. This would be undeniably false, since there are many cases in which communication can be successful even where speaker and hearer do not share knowledge of linguistic conventions. Two paradigmatic cases of such communication are where speakers speak different languages, or where one interlocutor lacks any language at all - for example, because the hearer is an infant of 12 months who has yet to acquire the speaker's language. In cases like these, if the hearer is able to grasp what it is that the speaker is trying to communicate to her, it cannot be because she has knowledge of the meanings of the words that the speaker utters. Since communication in these cases is clearly possible, it must be true that in some cases a hearer is able to grasp a speaker's perlocutionary intention independently of any knowledge of the language in which she speaks - and since these are undeniably cases of communicative interaction, any account of the communicative act that fails to explain how such cases can be possible is clearly lacking.

For all that Searle surely does not think knowledge of conventions necessary for the possibility of communication, on his account it is accorded a central role. For example, Searle writes:

On the hearer's side, understanding the speaker's utterance is closely connected with recognising his intentions. In the case of literal utterances the bridge between the speaker's side and the hearer's side is provided by their common language. (SA, p.48)

Indeed, one of Searle's explicit motivations for rejecting the Gricean view is that it accords too little significance to the role of linguistic conventions in our communicative interaction. Conventions are not always in play, though, and the prominence of the role of conventions in Searle's account renders it unsuitable for characterising the sorts of communicative acts that I want to describe - namely those in which infants who have no knowledge of linguistic conventions come to acquire such knowledge.

#### Responding to Searle's first objection

With respect to the constitutive and epistemic aspects of Searle's first objection to Grice, both of Searle's points should be conceded. However, they should be conceded only for cases of communicative interaction where linguistic conventions are known to both speaker and hearer. The epistemic claim is obvious, and even though Searle's own argument for the constitutive claim is unpersuasive, an argument can be reconstructed that shows that his conclusion is correct: the message that a speaker could intend to communicate by speaking is constrained by the meanings of the words that she utters. However, by revising the Gricean analysis to accommodate the fact that "meaning can be a matter of rules or conventions" (p.43), Searle sets out an account of the intentional structure of *conventional* communication - and thereby neglects the fact that although meaning can be a matter of convention, it is not always so. There are ways of communicating in which linguistic conventions are not known to both parties and these occasions are not well characterised by an account of the communicative act that makes speakers' and hearers' knowledge of linguistic conventions central. For all that Searle's objections are correct then, they are correct only for some cases of communication, and there remain others that are not well explicated by the account of communication that he proposes.

With respect to the positive account that Searle defends, it may be that where linguistic conventions are known to both speaker and hearer, the intentional structure of communication is less Gricean and more Searlean. In the absence of a more substantial

consideration of the Searlean account, I will remain agnostic on this point. In light of Searle's objections to the Gricean view, though, I propose to restrict the account of communicative intentions that I have proposed to cases in which linguistic conventions are not in play - that is, to cases in which speaker and hearer do not share knowledge of the conventions of a language. No stronger claim than this will be necessary for the view that I defend. I suggest that for a class of relatively simple utterances, no common language is necessary for communicative interactions to be successful. (How large this class is may be a function of many factors, including the resourcefulness and intelligence of the individuals involved, the environmental context of their interaction, and so on.) In these cases, perhaps with the support of environmental cues and other previously acquired items of knowledge, not to mention what Wittgenstein described as "the common behaviour of mankind ... by means of which we interpret an unknown language" (*PI* §206), hearers are able to grasp a speaker's perlocutionary intentions, even in the absence of a common language. For example, messages like "Look!" or "Follow me!" might be communicated by fairly intuitive gestures like points and waves, the meanings of which could be understood even in the absence of mutual knowledge of conventions. These interactions are adequately characterised by the Gricean account of communicative intentions defended in the previous chapter.

There are, of course, many complex utterances that could not be grasped by someone who did not know a speaker's language.<sup>25</sup> However, I suggest that there is a large class of fairly simple utterances that can be entertained and communicated without recourse to linguistic conventions, and that this class of utterances is sufficient to enable the infant to

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<sup>25</sup> It may also be that a class of utterances exists that could not even be entertained unless couched in the conventions of a language. This is partially but not wholly consistent with the Dennettian view discussed in Chapter One: while Dennett is wrong, I suggest, to think that only speakers can be thinkers, he may be right to think that there are thoughts that only speakers could entertain. Beyond noting this convergence, I will not discuss it further.



acquire a vocabulary on the basis of knowing which more sophisticated utterances could subsequently be understood. In this context, it's plausible to construe Searle's account and the Gricean account that I have defended here not as rival accounts of the same phenomenon but as consistent accounts of different phenomena, namely conventional and non-conventional communicative interactions. Certainly Searle's first objection does not show that the Gricean model elaborated in the previous chapter is not adequate to the task in hand.

Searle's second objection to Grice

Searle's second objection to Grice takes as its starting point Grice's characterisation of speaker's meaning in terms of the perlocutionary intention that motivated the speaker's uttering as she did. In contradistinction to Grice, Searle argues that communicative intentions are not perlocutionary intentions, but illocutionary intentions.

The illocutionary act is the minimal complete unit of human linguistic communication. Whenever we talk or write to each other, we are performing illocutionary acts. (Searle 1999, p.136)

The distinction between illocutionary and perlocutionary intentions was first made by J. L. Austin in his book *How to Do Things with Words*. In Austin's terminology, an illocutionary act (or: an act with illocutionary force) is an act performed with the intention of making oneself understood as having performed a speech act with a certain force. Examples of illocutionary forces include making a statement or a promise, or giving an order, or posing a question. Illocutionary acts are closely related to what Austin called locutionary acts. Locutionary acts are actions performed with the intention of being understood to have made an utterance with a certain meaning (or 'content'). In Austin's words, this is "roughly equivalent to uttering a certain sentence with a certain sense and reference" (p.109). The distinction between locutionary and illocutionary intentions is somewhat abstract. Austin remarks that "to perform a locutionary act is in general ... to perform an illocutionary act" (Austin 1962, p98). For example, someone who performs a locutionary act typically does so as a part of

making an assertion or giving an order or asking a question, and so on. Searle makes a similar point when he states that “when a proposition is expressed it is always expressed in the performance of an illocutionary act” (*Speech Acts*, p.29).<sup>26</sup> Furthermore, all illocutionary acts are locutionary acts. One could not make an assertion without making an assertion that had a certain content – and so on, for varieties of other illocutionary force. In Austin’s words:

to perform an illocutionary act is necessarily to perform a locutionary act ... for example, to congratulate is necessarily to say certain words[.] (Austin, p.114)

However, since sentences with the same content – i.e. the same locutionary acts – can be uttered with different illocutionary forces, and utterances with the same illocutionary force – for example, different questions – might have very different contents, it’s necessary to distinguish between the content and force of an utterance, and the corresponding intentions with which the utterance is performed.

On Austin’s view, what locutionary and illocutionary intentions have in common is that they are both *intentions to be understood*. While retaining this characterisation, in due course I will argue that there may be intentions to be understood that are neither locutionary nor illocutionary intentions, since they are not characterised by the force-content differentiation upon which this distinction turns.

Intentions to be understood are to be contrasted with intentions to produce perlocutionary effects – or, as I will sometimes refer to them, *intentions to make things happen*. A perlocutionary effect is the action (or series of actions) that the uttering of an utterance is intended to effect – in Austin’s words, the “certain consequential effects upon the feelings, thoughts, or actions of the audience, or of the speaker, or of other persons” (*HDTW*, p.101)

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<sup>26</sup> Searle’s ‘propositions’ correspond roughly but not exactly to Austin’s ‘locutionary acts’, the concept of which Searle finds “very unhelpful” (Searle 1968, p.405). However, the differences between the respective accounts are not relevant to the discussion here and so need not be elaborated.

that a speaker seeks to bring about by speaking. Such consequences include but are not restricted to having one's interlocutor believe in the truth of a proposition, or perform a certain action. In the previous chapter, I suggested that intentions to have one's interlocutor attend to something are also perlocutionary intentions.

Perlocutionary intentions correspond to (A) the speaker's message in the revised-Gricean account of the communicative act and are specified in terms of the response that a speaker would have her interlocutor produce. Consequently, Gricean communicative intentions are perlocutionary intentions. By contrast, on Searle's account, every act of saying something and meaning it is an illocutionary act. This has implications for the specification of *r* in the first clause of the account of a communicative act. If Searle is correct, the first clause of the analysis should always be specified not as an intention that one's interlocutor should do or think or attend to something (for example), but as an intention that they understand that one has performed a speech act with a certain (content and) force.

Searle gives three reasons for thinking that communicative intentions could not be perlocutionary intentions. Since one of these relates to the third clause of Grice's original analysis of meaning, the necessity of which for the performance of a communicative act was rejected in the previous chapter, I will discuss only the first and second objections. First, Searle objects that communicative intentions couldn't be perlocutionary intentions because "many kinds of sentences used to perform illocutionary acts have no perlocutionary effect associated with them" (SA, p.46). He gives the following example:

[T]here is no associated perlocutionary effect of greeting. When I say "Hello" and mean it, I do not necessarily intend to produce or elicit any state or action in my hearer other than the knowledge that he is being greeted. But that knowledge is simply his *understanding* what I said, it is not an additional response or effect. (*ibid.*)

Second, he argues that even where a perlocutionary effect is generally associated with the utterance of some sentence, one could always utter that sentence without intending that

effect to follow from one's uttering. On any given occasion, a speaker could speak only with the intention of being understood. Beyond being understood, she need have no interest whatsoever in having her audience respond in one way or another. In both cases, one speaks meaningfully without having any perlocutionary intention.

Since one can have communicative intentions without having perlocutionary intentions, the former cannot be identified with the latter. However, since meaning is an intentional action, there must be some intentions the having of which are necessary for a speaker to mean something by uttering. On the analysis favoured by Searle a speaker's intention in speaking is an intention to be understood. This is not a perlocutionary effect: it is not an intention to bring about any consequence over and above its being understood. Rather, it's an illocutionary intention. Searle argues that intending to be understood is both necessary and sufficient for successful communication.

Human communication has some extraordinary properties, not shared by most other kinds of human behaviour. One of the most extraordinary is this: If I am trying to tell someone this, then (assuming certain conditions are satisfied) as soon as he recognises that I am trying to tell him something and exactly what I am trying to tell him, I have succeeded in telling it to him. Furthermore, unless he recognises that I am trying to tell him something and exactly what it is that I am trying to tell him, I do not fully succeed in telling it to him. In the case of illocutionary acts, we succeed in doing what we are trying to do by getting our audience to recognise what we are trying to do. But the 'effect' on the hearer is not a belief or a response, it consists simply in the hearer understanding the speaker. It is this effect that I have been calling the illocutionary effect. (SA, p.47)

Responding to Searle's second objection

I suggest Searle's second claim here should be conceded - at least in part. Searle's first claim is less plausible in light of the insight of the previous chapter - that perlocutionary

intentions are not only intentions to have others perform actions or entertain beliefs, but can also be intentions to share experiences and attention, and possibly other responses too. Once it is recognised that the class of perlocutionary effects that one can intend is larger than has previously been thought, it is less difficult to think of perlocutionary effects with which utterances might characteristically be associated. For example, a greeting of the form "Hello!" might be associated with an intention that speaker and hearer share attention to one another.

If Searle's first claim is flawed, his second is surely not: on occasion, any utterance could be performed without an accompanying perlocutionary intention. Alston (2000, p.48) gives a good example, of a bored station officer who makes an announcement concerning the late arrival of a train. He needn't care at all whether anyone believes what he says, or even whether anyone is listening. His only concern is to do his job, by making the announcement. This can be done, though, without his having any perlocutionary intentions whatsoever - if he utters only with the intention of producing an utterance that others would understand as having a certain content and force. However, while the existence of such cases can readily be conceded, it doesn't follow from this that all communicative intentions are illocutionary. This might be the case if all communicative intentions were aptly characterised by Searle's analysis of saying something and meaning it, but I have already argued that this is not the case. In order to develop this claim, the question of whether young infants speak with illocutionary intentions, or only with perlocutionary intentions, should be considered.

In the previous chapter, an account of the intentional structure of having and grasping perlocutionary intentions was developed and defended. Furthermore, empirical evidence was adduced to support that claim that infants can act with and grasp such intentions. However, if infants communicate with illocutionary and not just perlocutionary intentions, this may not be adequate to characterise their communicative acts. Both the intentional structure of having and grasping illocutionary acts and the cognitive abilities necessary to

support such havings and graspings may differ from those that are necessary in the perlocutionary case. In this case it needs to be determined whether infants could speak with illocutionary intentions. To this end it will be necessary to elucidate some constraints that operate on the performance of illocutionary acts. Employing the same method adopted in the previous chapter, I propose to ask: (I) What are the conditions under which illocutionary acts can be performed? And, (II) what are the cognitive abilities that are required for the performance of illocutionary acts? As in the previous chapter, an answer to (II) will tell us whether or not it's plausible to think that young infants communicate with illocutionary intentions, but an answer to (II) will be possible only in light of an answer to (I). Even a partial answer to the question "What conditions are necessary for the performance of an illocutionary act?" might help us to characterise appropriately the communicative acts in which young infants participate. For example, if the performance of an illocutionary act requires the fulfilment of some condition that is not met by infants of a certain age, this would be evidence that those infants communicate with only perlocutionary intentions.

It should be emphasised that in what follows it is not my intention to develop a detailed account of what it is to perform an illocutionary act. Time and space constraints prevent this. Therefore I will only attempt to sketch a few constraints on what might reasonably be supposed to be features of illocutionary acts and the cognitive abilities required for their performance, without proposing any full analysis of the nature of illocutionary acts in the manner undertaken by Schiffer, Searle and more recently Alston (2000).

Under what circumstances could one perform an illocutionary act?

Searle's analysis of the concept of saying something and meaning it suggests that there may be some connection between the performance of an illocutionary act and the existence of linguistic conventions that govern the content and force of an utterance. Searle does not explicitly link the two changes that he makes to Grice's account. For example, he does not state that one can speak with illocutionary intentions only because language is

conventional. However, others have held that there is a connection between the possibility of acting with illocutionary intentions and the existence of linguistic conventions. According to Austin, illocutionary acts are necessarily conventional:

We must notice that the illocutionary act is a conventional act: an act done as conforming to a convention. (p.105)

Furthermore, Austin writes that utterances have the force that they do in virtue of “the conventions of illocutionary force as bearing on the special circumstances of the occasion of the issuing of the utterance” (p.115). An argument for this claim isn’t forthcoming in what Austin says. However, the assumption that all illocutionary acts are conventional is perhaps motivated by Austin’s interest in a certain class of illocutionary acts that are paradigmatic cases of social conventions - like the union of two people in marriage with the utterance of wedding vows, or the ceremonial naming of a ship.

An argument for thinking that one could speak with illocutionary intentions only where there exist linguistic conventions might be run as follows. When a speaker speaks with illocutionary intent, she must think there some likelihood that her interlocutor could grasp her to have performed a speech act with a certain content and force. This follows from the fact that one cannot act with intentions that one does not think possible. The best way to ensure that one’s interlocutors could understand one as having performed such a speech act would be if that speech act (both its content and its force) were codified in a language and further conventions governing force that were known to the hearer, so that he could be expected to grasp the speech act that the speaker intended to be understood as having performed. For example, if I want to be understood as having asserted that I’m coming over, the best way to achieve this outcome would be by my uttering a sentence like “I’m coming over” in a language known to you and in accordance with practices that govern the making of assertions and that I know you to know.

In this condition, it will not be sufficient for the performance of an illocutionary act that the utterance that a speaker performs consists of the utterance of a sentence in a language with a force established by the existence of further conventions. This is because there are many languages that a speaker's interlocutors could not possibly be expected to understand, and one could not intend to be understood as having said what one said had one spoken in one of those languages. For example, I could not intend to be understood as having told you that I'm coming over if I had spoken to you in Aramaic or Pirahã - because unless I had reason to think that you spoke these languages, I couldn't reasonably expect you to understand the content and force of the speech act that I intended to be understood as having uttered. What's needed then is not just the existence of a language and of further conventions for making assertions, etc., but some further knowledge or a reasonable expectation that one's interlocutor also knows that language and its accompanying force conventions. Perhaps it will also be necessary that one knows that one's interlocutor knows that one is also a speaker of the language in which one utters, so that he will identify one's utterance as a sentence of, say, English - and not merely something resembling it. Such knowledge would also enable a hearer to judge that the likelihood that what his speaker had intended to be understood as saying is likely to have been reliably indicated by her choice of words and so might also be thought necessary on this ground.

Suppose, if only for the sake of argument, that these conditions - the existence of conventions that determine the content and force of utterances in a language, and knowledge that these conventions are known to both speaker and hearer and also known by speaker and hearer to be known to both of them - are sufficient for uttering with illocutionary force. It still doesn't follow that on any occasion one would need to speak in accordance with those conventions in order to perform an illocutionary act. cursory reflection shows that one could perform an illocutionary act even when uttering in very unconventional ways. For example, it's very likely that anyone who could read this chapter could also participate in a game of illocutionary charades, in which participants have to



identify the illocutionary act performed by a player who is permitted to communicate to the others the content and force of the illocutionary act that she intends to perform only by improvised - that is, non-conventional - means. This game would differ from ordinary charades only in taking as its material speech acts possessed of content and force and not the names of films and books. Furthermore, the communicative acts that it requires are clearly intentions to be understood - since the point of charades is to make oneself understood and nothing more. This shows that even if it were sufficient to act with illocutionary intent that a speaker spoke in accordance with conventions known by speaker and hearer to fix the content and force of an utterance, nonetheless it's not necessary that one speak in accordance with these conventions. This doesn't show that one needn't have knowledge of the existence of some conventions governing the making of assertions, etc, in order to assert - only that one need not follow those conventions in order to make an assertion. However, it remains unclear what possessing such knowledge consists in.

One way to proceed now would be to consider in more detail what a speaker might need to know in order to act with an illocutionary intention - for example, in order to intend to be understood as having made an assertion. It may be that such an intention could be held only by one who possessed some minimal grasp of what it is to make an assertion. An answer to (I) might be well served, then, by some elucidation of what it is to make an assertion, or to give an order, and so on. Determining whether infants act with the intention to be understood as having performed an assertion would then be a matter of determining what is involved in intending to be understood as having made an assertion, and of acquiring empirical evidence that would illustrate the ages at which infants do or do not meet these constraints.

To this end, it's worth considering an alternative answer to (I), according to which making an assertion (or performing a different illocutionary act) consists in nothing more than acting with a certain type of perlocutionary intention. For example, all utterances of *x* that

have as their intended response a hearer's coming to grasp that such-and-such is true might be thought of as acts of assertion. Similarly, all utterances of *x* that aim at a hearer's performing some action would be possessed of imperative force. This view of the nature of force may be at work in some of the recent empirical research on early infant communication produced by Michael Tomasello and his collaborators. For example, Tomasello (2008, and as a co-author in Tomasello, Carpenter & Liszkowski (2007)) distinguishes between three basic types of infant pointing - the different motives for communication - that he suggests correspond to three of the basic types of illocutionary act described by Searle (1999).<sup>27</sup> On the view of the 'Leipzig school' (consisting of Tomasello and his co-authors), infants' pointing to objects that they want others to fetch for them are early manifestations of what Searle calls *directive* illocutionary acts, which include orders and requests. Informative pointing, such as when an infant points to inform her interlocutor of the location of a lost object, is an early form of *assertoric* illocutionary acts. Finally, cases where an infant points to an object in order to share attention to it with her interlocutor are identified as early forms of what Searle calls *expressive* illocutionary acts. This suggests a reductive view of illocutionary acts which entails an answer to (II) that

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<sup>27</sup> Whether or not Tomasello really thinks illocutionary acts mere species of perlocutionary acts is unclear, since he never talks about 'illocutionary intentions' - only about 'communicative motives' (2008, p.87) and 'basic speech acts functions' (*ibid.*, p.88), which he explicitly relates to Searle's taxonomy of illocutionary acts. On the subject of his taxonomy of the different types of infant pointing, he writes:

There is some correspondence here with the basic speech act functions posited by theorists such as Searle (1999), though the mapping is not totally straightforward. (*ibid.*)

Although there could be several reasons why the mapping of motives for pointing to illocutionary acts is not straightforward, Tomasello's point seems to be (in context) that Searle's account of the varieties of illocutionary acts is not quite right - and not that infants do not point with illocutionary intentions. It's therefore not clear whether Tomasello has any view at all about the nature of illocutionary intentions. Nonetheless, the view that I attribute to him is worth considering, whether or not it's really his.

places no further cognitive demands on the performance of an illocutionary act than on the perlocutionary acts described in the previous chapter, because to act with illocutionary intent is just to act with one of a certain class of perlocutionary intentions.

On this view of illocutionary acts, infants would be said to act with illocutionary intentions by virtue of their acting with correlated perlocutionary intentions. However, this view of illocutionary intentions presupposes a reductionist account of illocutionary to perlocutionary intentions that is false. Not all assertions are produced with the perlocutionary intention of informing one's interlocutor of the holding of a state of affairs, and nor is it the case that imperative illocutionary acts are always performed with the intention that a hearer should perform some action. Consider the following utterances:

(1) I utter "The door is behind you," as a way of ordering you to leave.

(2) I utter "Run!" with the intention of informing you that the enemy have seen us.

In the first of these cases, I speak with assertoric force but with the perlocutionary intention that you should leave - that is, perform an action. In the second case, I issue an order as a means to producing some belief in you. Cases like these are legion. Furthermore, as Searle himself has argued, there are occasions on which one performs an illocutionary act without having any perlocutionary intention at all. Since there is no straightforward correlation of illocutionary intentions to perlocutionary intentions, it can't be that illocutionary intentions are merely types of perlocutionary intention. Intending to perform an utterance with an illocutionary force of asserting or ordering could not be explained simply as a speaker's performing a speech act with a with perlocutionary intention typically associated with assertions or imperatives.

This lack of a straightforward reduction doesn't entail that there is *no* correlation between the force of an utterance and the perlocutionary intention with which it's uttered. Often a reliable indicator of the perlocutionary intention with which a speaker speaks is the force of her utterance. We *typically* - although by no means always - perform utterances with

assertoric force when speaking to inform our interlocutors. We also ask questions in order to acquire information and we typically perform utterances with imperative force when giving orders. Given these uses, it will often be the case that knowing the force of an utterance - for example, knowing that someone is asking a question or giving an order - is a reliable indicator of the sort of thing that she's trying to achieve by speaking; of one's interlocutor's perlocutionary intention. Additionally, speakers can provide clues to their interlocutors about what they are trying to achieve by uttering with a force typically associated with a certain perlocutionary intention. However, an account of a speaker's illocutionary intention cannot be specified just in terms of her perlocutionary intention.

For similar reasons, it's also worth noting that the force of an utterance isn't something that can be identified with the syntactic structure (or 'mood') of a sentence, and consequently that a speaker's making, for example, an assertion cannot be explained just in terms of her utterance of an indicative sentence. Often the mood of a sentence is a reliable indicator of the force of an utterance. For example, often we use indicative sentences like the one uttered in (3) to make assertions. By contrast, a question might be more often posed with the use of an interrogative sentence, as in (4):

(3) That's a fire.

(4) Is that a fire?

However, as with the relationship of the force of an utterance to the perlocutionary intentions with which it might be uttered, the mood of a sentence is only an imperfect guide to its force. One reason for this is that there are more illocutionary forces than there are sentential moods, so that there can't be a 1:1 correspondence of moods to forces. For example, since advising, warning and ordering might all typically be expressed sentences of imperative mood, there's no way to read the force of a sentence straight off its mood. Additionally, there isn't even a 1:many correspondence of moods to forces. In principle any sentential mood could be used to make an utterance of any force. The sentences uttered in (5)-(7) all have the same syntactic structure and yet differ in their force. (Suppose that in

each case, the speaker utters without any perlocutionary intention, or some perlocutionary intention common to all three cases.)

(5) "There's a fire."

(6) "There's a fire!"

(7) "There's a fire?"

The utterance in (5) is an assertion. By contrast, in (6) the utterance has an imperative force. Finally, in (7) the utterance has an interrogative force. Since the force of an utterance is not identical to the mood of a sentence, intending to be understood as having made an utterance with a certain force could not be the same as intending to utter a sentence with a certain mood.

Searle (1969, 1999) introduces a further feature of illocutionary acts that may be of help in trying to say more about what it is to act with illocutionary intent. This would help with the provision of an answer to (I). This feature - the direction of fit of an utterance - relates to the conditions in which a speaker's communicative intention is satisfied. Different illocutionary acts have different directions of fit. For example, an assertion has a word-to-world direction of fit because in making an assertion, a speaker intends to make herself understood as representing how the world is. In Searle's words:

The point of assertive speech acts is to commit the hearer to the truth of the proposition. ... Every assertion is an expression of a belief. (Searle 1999, p.148)

In making an assertion, a speaker consequently utters with the aim that her words should fit the world, by accurately representing how the world is. In contrast to assertoric speech acts, directive illocutionary acts have a world-to-word direction of fit. They are uttered with the intention that a speaker make herself understood not as representing the world, but as wanting to change it.

The illocutionary point of a directive act is to get the hearer to behave in such a way as to make his behaviour match the propositional content of the directive. ... Every

directive is an expression of a desire that the hearer should do the requested act.

(Searle 1999, pp.148-149)

If these characterisations are accurate, then it may be possible to elucidate some characterisation of an illocutionary intention by appealing to the notion of an intention to perform a speech act possessed of a certain direction of fit. However, it should be noted that Searle's characterisations here are simplistic - appealing to a relationship between acting with illocutionary and perlocutionary intent that he has himself explicitly denied.<sup>28</sup>

One advantage of appealing to direction of fit as a marker of a speaker's illocutionary intention is that Hannes Rakoczy and Michael Tomasello have recently undertaken an empirical study of two- and three-year olds' understanding of the direction of fit of others' utterances. Their results show that by the time they are three years old, infants have a basic grasp of the word-to-world and world-to-word directions of fit of assertoric and imperative (or 'directive') utterances respectively. By contrast, infants of two-years displayed no such grasp of the different direction of fit of different utterances - although the experimenters hold open the possibility that "the present methodology failed to unmask existing competence in the two year olds" (Rakoczy & Tomasello, forthcoming, p.13). *If* direction of fit is a good marker of the force of an utterance, then the preliminary findings of the Rakoczy & Tomasello study suggest a possible answer to (II). If grasping the force of an utterance consists in understanding its direction of fit, then there is some evidence that infants of three but not two years understand the force of an utterance. From this it would follow that infants younger than three may not act with or grasp illocutionary intentions.

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<sup>28</sup> The apparent contradiction here - between Searle's asserting (1999) and denying (1969) a reductive relation between illocutionary and perlocutionary intentions - need not be explained by Searle's changing his position. Rather, Searle's later book is a popular work, written for the lay-reader, and Searle acknowledges that details have been simplified in places (p.156f). Discussion of the relationship between illocutionary and perlocutionary intentions seems to be one such place. It's worth remarking that Tomasello's account of 'basic speech act functions' is drawn from Searle's later discussion.

Consequently, it may be that infants younger than three produce and grasp only the sorts of perlocutionary intentions described in the previous chapter. This would constitute evidence that the perlocutionary model of communicative intentions developed in the previous chapter might be more appropriate for the characterisation of infants' earliest communicative interactions than Searle's analysis of saying something and meaning it.

Although this would seem to present a partial vindication of my claim that infants' earliest communicative intentions are perlocutionary intentions, in fact it's not clear why grasping the direction of fit of an utterance indicates a grasp of its force, rather than of a speaker's perlocutionary intention. In this case, the Rakoczy & Tomasello study provides no basis for attributing illocutionary intentions to infants of either two or three years.

This point can be illustrated by reference to the Rakoczy & Tomasello study, which tests infants' ability to direct appropriately the corrections that they make to others' mistaken utterances. Infants were shown a scene of a speaker and hearer (both puppets), in which the speaker either described the action performed by the hearer, or issued him with an instruction. In such a scenario, were the speaker to misrepresent the hearer's action - for example, by saying "Peter is eating the carrot" when Peter is not eating the carrot but the cake - any attempt to repair the utterance should be directed at the speaker. It's the speaker who is in error here. By contrast, if the hearer failed to grasp the speaker's order - and so, for example, drank the milk when he had been ordered to "Eat the carrot!" - the mistake would be the hearer's and any corrective action should be directed at the speaker. What Rakoczy & Tomasello tested was young infants' ability to direct their corrections at the appropriate target. Three but not two year olds did this reliably. The problem with taking this to be a sign that infants of three grasp illocutionary intentions is that the evidence can be interpreted without loss by attributing to the same infants only a grasp of a speaker's perlocutionary intentions. For example, if the infant grasped that a speaker had spoken in order to get the hearer to do something that he subsequently failed to do, that

suffices for the infant's knowing that the hearer and not the speaker should be corrected, without the infant's having to grasp that the speaker intended to be understood as having given an order. Similarly, in the case of assertions, had the infant grasped that the speaker's intention was to inform it (the infant) of the holding of some state of affairs, this would suffice for knowing that the speaker had spoken incorrectly, and should be corrected, whether or not the infant also grasped that the speaker had intended to be understood as making an assertion.

This doesn't undermine the authors' tentative conclusion that infants of three but not of two grasp an utterance's direction of fit, but it does suggest that infants' understanding of direction of fit is an imperfect marker of whether or not they grasp illocutionary intentions. While this doesn't entail that the tested infants don't grasp illocutionary intentions, it's not necessary to attribute to them a grasp of such intentions in order to explain the results of the study. Consequently, it isn't sufficient evidence to warrant attributing to infants of three a grasp of speakers' illocutionary intentions.

The picture that should be emerging now is that saying what it is to make an assertion or ask a question, and consequently of what a speaker would need to know in order to act with that illocutionary intention, is actually rather difficult. I suggest that a better way to try to get to grips with what it is to produce an utterance with a certain force is to consider what it is that the concept of force is introduced to *explain*. This is because our most basic grasp of the concept of the force of an utterance is not reducible to a grasp of perlocutionary intentions, syntactic structure, linguistic conventions or the direction of fit of an utterance. Rather, it's an abstract theoretical tool that was first postulated in order to explain intuitions about linguistic utterances that could not otherwise be explained. Force is one property of an utterance (there may be others) the existence of which explains how it is that utterances that have the same mood and content and which were produced with identical perlocutionary intentions can still differ. However, because the property of force



is first and foremost an explanatory postulate the appropriateness of attributing to speakers illocutionary intentions becomes questionable where doing so is of no explanatory value.

Consider the following utterances:

(8) I tell you "It's time to leave," with the intention that you grasp my intention that we should leave.

(9) I ask you "It's time to leave?" with the intention that you grasp my intention that we should leave.

One basic way in which we can grasp the property of the force of an utterance is that it's the property the existence of which is postulated in order to explain our intuition that utterances like (8) and (9) constitute different types of speech act. If it's held that (8) and (9) are the same type of speech act, the fact that one is a question and the other an assertion is overlooked. However, if it's held that they are different speech acts, then some property must exist in virtue of which they differ. On the other criteria for distinguishing between communicative acts so far considered, this distinction is not preserved, because the syntax and content of the utterance and the perlocutionary intention with which it's performed are all identical. Since these features are common to both (8) and (9), they cannot be the basis for distinguishing between them. This leads us to look for a further property the existence of which would be sufficient to explain the difference. By attributing to the speaker of (8) an intention to be understood as having made an assertion, and to the speaker of (9) an intention to be understood as having asked a question we can explain the respect in virtue of which the two utterances differ.

With respect to the provision of an answer to (1), because the property of the force of an utterance is first and foremost a notion of an abstract explanatory property, it follows that an illocutionary act can be performed only where it is explanatorily appropriate to distinguish between the force of an utterance and its other properties. However, for a class of communicative acts - including infants' earliest communicative interactions - there's no

explanatory gap that needs to be filled by an appeal to a speaker's having illocutionary intentions like those of intending to make an assertion, or ask a question, and so on. This suggests that these utterances are not appropriately characterised by an appeal to linguistic force - in which case, they are not illocutionary acts. This point can be illustrated with some examples. Consider three cases of pointing of the sort characterised by Tomasello et al.<sup>29</sup>

(10) I point to the cup with the intention that you give it to me ('directive pointing').

(11) I point to the cup with the intention of informing you of its location ('informative pointing').

(12) I point to the cup with the intention that we jointly attend to the cup ('expressive pointing').

Suppose that we want to defend the intuition that these utterances are not utterances of the same type. On what basis could they be differentiated?

If the meaning of a point is taken to be timeless, as was suggested earlier in this chapter, then in each of utterances (10)-(12) the meaning of the utterance will be fixed and equivalent to something like "Look at that!". Since points don't have a syntactic structure, the different points cannot be differentiated on this basis. On Tomasello's account, it might be tempting to type the utterances as marked by different forces: (10) an imperative, (11) an assertion, and (12) an expressive speech act. If performing such an act consists in having an intention to be understood as having made an assertion, etc., as it does on Searle's account, then it will be necessary to attribute to the utterers of (10)-(12) *illocutionary* intentions of this sort. However, the differences between the utterances can be characterised adequately without attributing to infants illocutionary intentions, and instead by pointing to the different *perlocutionary* intentions with which the points were produced.

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<sup>29</sup> The research on pointing reported in Tomasello (2008) and Tomasello et al. (2007) was carried out, primarily, by Ulf Liszkowski, and published in a series of papers (for example, Liszkowski 2006) summarised in these references.

Consequently, the intuition that (10)-(12) are different types of speech act can be preserved independently of any appeal to their being possessed of different forces. There's simply no explanatory role that needs to be filled by the postulation of the property of force, since there are no longer salient differences that need to be explained.

This is true not only of pointing gestures, but also of infants' earliest linguistic utterances. To give a further example from *First Verbs* (p.339), around the age of 18-months, Tomasello reports a number of utterances made by Travis about hammering. I'll concentrate on two cases of single word utterance in which the word 'hammer' was used like a verb. At 17 months and 26 days, Travis uttered "Hammer" twice, while banging with her toy hammer. A month later, she made the same utterance but, Tomasello reports, as a means to expressing her desire to hammer. On this occasion, these differences are not characterised as differences of force, but one can see a temptation for doing so. It's natural to read the first utterance as a description and the second as a request. But should we therefore attribute to Travis illocutionary intentions, respectively, to have made an assertion or a request? Surely we should not. Here the differences between the utterances can be characterised just by pointing to the different perlocutionary intentions with which Travis spoke.

This is not to deny that the varieties of pointing produced by pre-linguistic infants, not to mention the linguistic utterances just described, in some sense resemble distinctions that adult speakers would draw along illocutionary lines. This is no doubt true. But whatever similarities do exist are not informatively characterised by claiming that infants are able to grasp illocutionary force, and by attributing to them illocutionary intentions. It doesn't follow from the fact *we* could produce an utterance with a particular illocutionary intention - for example, by saying "Hammer!" as a means to the giving of an order - that were an infant to produce an identical utterance, it would be doing so with the same intention. It may be that the resemblance between the infant's utterance and our own is only superficial - such that we should not attribute a grasp of illocutionary intentions to it.

In the cases just described there's no evidence that must be explained by attributing to infants intentions of that sort. In the absence of such evidence, I suggest that, contra Searle, it shouldn't be assumed that all speech acts are illocutionary acts. This is consistent with the claim that in the most basic forms of communicative interaction - linguistic and non-linguistic alike - the concept of force that is central to the possibility of acting with illocutionary intent simply fails to find an application. Its postulation becomes explanatorily informative only where, relative to a fixed (or absent) perlocutionary intention, utterances are of sufficient complexity that their differences cannot be typed only by a difference of mood or content. Since this is not the case in infants' earliest utterances, then their linguistic acts need not be explained by appeal to their having illocutionary intentions, and it ought not be assumed that they possess such intentions.

I want to finish this chapter by considering evidence that infants as young as 18-months do speak with intentions to be understood. I suggest that there exists plausible evidence for this conclusion - although it does not follow that these intentions to be understood are illocutionary intentions.

Recent empirical work by Gerlind Grosse et al. (Grosse, Behne, Carpenter & Tomasello, in press) suggests that infants as young as 18-months *do* perform utterances with the intention of being understood, and not just with the intention to make things happen (i.e. not merely with perlocutionary intentions). In test conditions, infants of 18, 24 and 30-months were shown an object - for example, a ball or a shoe - that was sufficiently exciting to them that they could be relied upon to ask for it (for example, because it was the last piece that they needed to complete a puzzle or game that they had been playing). The infant's request (for example, an utterance of "Ball!" accompanied by a reaching action) would be met by one of five responses. In the first two of these conditions but not the final three, the infant's (presumed) perlocutionary intention - namely its request for the ball - was met

immediately. In the first and third conditions, had an illocutionary intention been present, it too would have been met. The experiment recorded whether infants sought to repair their communicative interactions - for example by repeating their request, or reaching again in the direction of the ball. The character of the infant's communicative intentions could be inferred from the nature of its repairs.

In the correct response case, the experimenter would acknowledge the request and give the infant the ball. In a second 'happy accident' condition, the experimenter would turn to a distant object and, while indicating to it, reply "Oh, you want the cardboard do you?" However, as she did this, she would discreetly pass the ball to the infant, so that its request was fulfilled, in spite of the experimenter's giving the impression that the communication had failed. In a third condition, the request was acknowledged with the words "Oh, you want the ball? I'll give it to you in a moment." Here the infant's perlocutionary intention was not met, but her illocutionary intention (should one be present) would have been. Two further conditions were added to elicit information about how the infants sought to repair their interactions. In the first of these, the 'wrong referent' condition, the experimenter would acknowledge the infant's request but give it a distracter object and not the object that the infant had requested. In the 'wrong intent' condition, the experiment would correctly identify the referent of the infant's request but would respond only by sharing attention to the object with the infant and not passing it to the infant. For example, in this condition she might respond with the words "That's a nice shoe!" but fail to grasp that the infant was asking to be passed the shoe. In both 'wrong referent' and 'wrong intent' conditions, neither perlocutionary nor illocutionary intentions (should they have been present) were met.

The results were as follows. Infants in the correct condition did not attempt to repair their communications. (This should be unsurprising because here there was nothing to repair.) Where the infants received the ball in spite of the broken communication, around half of

the infants in the youngest category sought to repair their conversation - even though they had received what they had requested. This suggests that infants cared not only about receiving the object they had requested but also about being understood. In the wait condition, when the infant's utterance was understood but their request not fulfilled, about half of the very youngest children (but very substantially fewer of the children in the older age groups) sought to repair the communication. However, children in all age groups repaired their interactions much less in the condition in which they had been asked to wait than they did in the 'wrong referent' and 'wrong intent' conditions. This suggests that although infants were frustrated when their requests were understood but not fulfilled, they were more frustrated when their requests were neither understood nor fulfilled. Together these results suggest that even infants of 18 months utter not just with perlocutionary intentions, but that they also care about being understood. It's also worth remarking that infants of all ages produced repairs that were tailored appropriately to the experimenter's misunderstanding. For example, in the 'wrong referent' condition, infants tended to repair by pointing again to the intended referent of their request. Such evidence might suggest that infants younger than those tested by Rakoczy & Tomasello also understand the direction of fit of the utterances that they produce.

The results of the Grosse et al. study support the claim that infants of 18-months speak with the intention of being understood. However, this research is not yet sufficient to show that there isn't a stage of infant communication where intentions to be understood are not present. Until evidence can be elicited to show that even the youngest communicators - those around 14-months of age - communicate with intentions to be understood, it ought not to be assumed that they do. Furthermore, although the results of this study are consistent with the possibility that infants communicate with illocutionary intentions, they needn't be interpreted in this way. As in previous cases, the infants' repairs can be explained merely by attributing to them intentions to be understood as having acted with a particular perlocutionary intention, without any need to attribute to them a further

intention to perform an utterance typed by a certain force. In this case, it may be that their intentions to be understood are not appropriately characterised by the force-content distinction that would make them either locutionary or illocutionary intentions. Consequently, although it's natural to think that the intentions indicated by this study are developmental forerunners of what might later become illocutionary intentions, there is not yet sufficient evidence to attribute illocutionary intentions to the infants tested.

#### Concluding summary

Neither of Searle's arguments against Grice's account of meaning something by an utterance have shown that a Gricean account of communicative intentions cannot be the appropriate model for characterising infants' earliest communicative interactions. It may be that Searle's convention-driven account provides an accurate explication of some cases of communicative intent. However, it could not be the right characterisation of cases in which linguistic conventions are not known to speaker and hearer. Consequently it could not be the right characterisation of the communicative interactions through which infants acquire their first knowledge of linguistic conventions. Furthermore, one need not attribute to infants either a grasp of or intentions to produce utterances characterised by illocutionary force in order to explain their earliest communicative interactions with others.

## Chapter 6: Imitation and Convention

In a platitudinous sense, the words and sentences of a language are conventional. The speech acts for which given words and sentences are typically used could have been performed just as well by countless arbitrarily different vehicles of meaning, and the same vehicles of meaning might just as well have been used for the performance of very different speech acts. For example, the sentence "It's cold outside" could have meant many things. Nothing about those sounds, arranged in that order, makes them particularly suitable for expressing the thought that *it's cold outside*. Similarly, the same thought could have been expressed just as well by a different set of sounds - for example, by the French sentence "Il fait froid dehors". There's nothing intrinsic to the sounds that we use to communicate that makes them mean one thing and not another. What determines the fact that these marks and sounds have the meaning that they do is the existence of conventions that determine how words and sentences should be used. For example, it's the existence of a convention that determines the fact that speakers of English characteristically use the sentence "It's cold outside", and not a different sentence, to express the thought that *it's cold outside*.



This feature of our language was described by David Lewis in the following terms:

It is a platitude that language is ruled by convention. Words might be used to mean almost anything; and we who have used them have made them mean what they mean because, somehow, gradually and informally, we have come to an understanding that this is what we shall use them to mean. We could perfectly well use these words otherwise - or use different words, as men in foreign countries do. We might change our conventions if we like. (Lewis 1969, p.1)

Where convention is characterised just in these terms, there are few who would disagree with Lewis. For example, Stephen Laurence, while rejecting much of the rest of what Lewis says about the nature of convention, "completely agree[s] that language is conventional in the trivial sense" (Laurence 1996, p.272) outlined above. Similarly, Tyler Burge, who is also critical of Lewis's developed account of convention, writes that:

Language, we all agree, is conventional. By this we mean that some linguistic practices are arbitrary: except for historical accident, they could have been otherwise to roughly the same purposes. (Burge 1975/2007, p.32)

It just isn't an intrinsic feature of the words and sentences of a language that they mean what we use them to mean.

In the previous chapter, discussion of John Searle's account of saying something and meaning it brought to the fore considerations of the role of convention in our communicative interactions. As Searle argued, it's typically the case that in communicating we understand our interlocutors, and make ourselves understood to them, by exploiting our knowledge of how to use words and sentences to perform certain speech acts. While interlocutors certainly can perform speech acts without using conventions - for example, when by using spontaneous gestures and sounds to communicate with someone with whom one does not share a language, or whose understanding of language is idiosyncratic - in at least a great many cases they do not. For participation in *linguistic* communication,

furthermore, the ability to exploit the existence of conventions is not just typical but necessary. Were one of our peers unable to learn the meanings of at least some words and sentences, though we might be able to communicate with him, we would surely not count him as a user of language.<sup>30</sup> Consequently, in order to answer the question: what are the cognitive abilities that must be attributed to infants in order to explain their abilities to use and understand others' uses of words in communicative interaction, something must be said not only about the nature of the cognitive abilities possession of which allows infants to act with and grasp communicative intentions, but also about the abilities necessary for acquiring knowledge of and subsequently using linguistic conventions.

This is something that infants are able to do from at least as early as fourteen months, the age at which most start to use words in their communicative interactions with others. Consider the following examples, taken from Tomasello's *First Verbs*:

15 months 20 days: "whereda bottle" (looking for and demanding bottle) (p.286)

15.29: "where-go" (a dog leaves suddenly) (p.286)

16.14: "thanks" (pretending to take a bite of food from Daddy) (p.303)

16.24: "get-it" (pointing to a book) (p.304)

16.26: "bottle get-it" (looking and pointing) (p.305)

All of these utterances show clearly recognisable conventional forms. For interlocutors to be able to exploit intentionally the existence of conventional devices in communication there must be some sense in which they have knowledge of them. This knowledge, too, must be acquired: even if infants are born with sophisticated cognitive abilities, they are not born knowing the meanings of words and sentences. But what would one need to know to be able to exploit the existence of linguistic conventions in this manner?

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<sup>30</sup> Donald Davidson might disagree. His 1986 paper 'A Nice Derangement of Epitaphs' seems to commit him to the claim that one could count as a user of language in spite of failing to grasp adequately *any* linguistic convention. Beyond noting this potential disagreement, I won't discuss it further.

One answer to this question, developed and defended by Tomasello (2006), Gergely & Csibra (2005) and Susan Hurley (in conversation) among others, is that cultural knowledge, including knowledge of linguistic conventions, can be acquired through *imitation*. This is the ability to observe and reproduce not just the goal of another's action, but the means pursued in order to achieve it. The idea is that if one could imitate the linguistic actions of others - by using their words in pursuit of analogous communicative goals - that would suffice for being able to exploit the existence of at least some linguistic conventions. Tomasello goes so far as to assert that imitative learning is the defining feature of conventional activity. He writes that conventional activities are conventional "precisely because ... at least to some significant extent, they are acquired by imitation or some other form of social learning" (Tomasello 2006, p.30). He continues:

Thus, dancing the tango is conventional because in large measure males and females learn their roles by imitating other males and females, and using a fork (instead of the hands or chopsticks) is conventional in large measure because children copy the adults around them. And so I would simply say that to the extent that these behaviours are not copied they are not conventional, and to the extent that they are copied they are conventional. (ibid., pp.30-31)

This claim is surely too strong. There are many skills that we learn by watching others and reproducing their goal-directed activity, but which are clearly not conventional because not one of many arbitrarily different possible alternatives. For example, I might learn how to tie shoelaces by observing my caregivers do the same and imitating their actions. However, the appropriate way to tie shoelaces is clearly not just a matter of convention. If they are tied with the wrong knot, they might fail to stay tied, or get stuck in a knot. Some ways of tying laces are consequently intrinsically superior to others. In this case, Tomasello's identification of conventional activities with imitated activities can't be correct.

Nonetheless, Tomasello may be right that conventional activities can be learned imitatively. As the discussion at the end of this chapter will show, infants of 14-months are already

quite sophisticated imitators of others' actions. In this case, if linguistic conventions could be acquired imitatively, the fact that infants can imitate might constitute a starting point for the explanation of how they come to acquire knowledge of how to use words in communication. In this chapter, I want to start to develop a story along these lines. Of course, since this is a large and ambitious project, it won't be possible here to sketch out more than the first steps. Some of the preparatory work for this project was already undertaken in previous chapters, with the defence of the claim that infants grasp speakers' perlocutionary intentions - if not necessarily their illocutionary intentions. The distinction between illocutionary and perlocutionary intentions is relevant to issues of imitation, because it's very clear how one could learn to achieve a perlocutionary effect by imitating another's action. Typically perlocutionary effects are things that we can perceive: they bring about changes in the world. Illocutionary effects, by comparison, are far more abstract. It's far less easy to see how one could imitate an intention to be understood.<sup>31</sup>

A further stage in the account requires saying something about the cognitive abilities that are necessary for participation in a convention. In order to do this it will be necessary to say more about what it is for an action to be conventional.

In his canonical work *Convention*, David Lewis undertook an analysis of conventions that was intended to explicate our intuitive grasp of the platitudinous conventionality of language. As with the Gricean analysis of non-natural meaning discussed in Chapter Four, Lewis's account constitutes a valuable starting point for discussion of how infants could acquire knowledge of linguistic conventions because, in addition to its containing an account of what it is for something to be a convention, it also yields a partial specification of what one would need

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<sup>31</sup> Note that the claim of the last chapter was not categorically that infants do not act with or grasp illocutionary intentions; only that there is insufficient evidence to conclude that they do. Were it discovered that infants do grasp speakers' illocutionary intentions, the imitation hypothesis would not be threatened, since it may be that infants grasp both speakers' illocutionary and perlocutionary intentions, but imitate only the latter.

to know in order to count as participating in a convention. In what follows, my aim will be neither to undertake an exhaustive review of Lewis's account, nor to develop a full account of what it is for an activity to be conventional. Time and space constraints prevent this. Rather the scope will be less ambitious. According to Lewis's account, one can participate fully in a convention only to the extent that one knows that that activity "satisfies the defining conditions for a convention" (Lewis 1969, p.61). Since one who learned a conventional activity imitatively need not satisfy this requirement, it constitutes a barrier to the project of explaining the acquisition of conventional knowledge through imitation. My aim here is to argue that Lewis's knowledge constraint is too strong. By arguing that it is superfluous, I remove one obstacle to the possibility that infants to participate in conventional activities through imitation.

I turn now to Lewis's account of convention. My discussion contains a considerable amount of clarificatory reconstruction of Lewis's views.

Lewis's account of convention

In *Convention* Lewis outlined a theory of convention that would explicate the conditions under which arbitrary features could come to be associated with stable properties, as happens when words come to acquire timeless meanings. His starting assumption was that it need not be the case that participants in a convention have entered into any explicit agreement about how it is that each participant should behave. This is necessary because in many cases of conventional activity, no such agreement takes place. Indeed, in the case of language, Lewis suggests that linguistic conventions could not arise on the basis of such an agreement:

In any case, the conventions of language could not possibly have originated by agreement, since some of them would have been needed to provide the rudimentary language in which the first agreement was made. (Lewis 1969, p.2)

This same point holds not just for fixing conventions, but for learning them too. One couldn't learn all linguistic conventions by having them explained, since such explanations are characteristically provided in a language that exploits the existence of ... linguistic conventions. One would need to know some linguistic conventions before one could grasp the explanation. In contrast to the pre-theoretical statement of the conventional character of language, quoted at the outset, Lewis's developed theory of convention is not platitudinous. It makes claims about the nature of convention that are not implicit in the notion set out above - such that Lewis concedes that it may not be recognised as "an analysis of our common, established notion of convention" (*ibid.*, p.3). Nonetheless, he writes, "insofar as I had concept of convention before I thought twice, this is either it or its legitimate heir" (*ibid.*).

According to Lewis's account, conventions are solutions to coordination problems. Coordination problems are situations of interdependent decision making, where participants must choose between solutions (or 'coordination equilibria') that are or appear to be equally good (i.e. only arbitrarily different), and where the particular solution that is chosen by the participants is less important than the fact that they choose the same solution. For example, suppose that a group of people need to choose where to hold a meeting. Supposing that all meeting places are equally good, it doesn't really matter where they meet, so long as everybody meets in the same place. In this case, what each person decides to do is contingent upon what everybody else decides to do: there's no reason to go to one meeting place rather than another, unless the others are going there too. Another example of a coordination problem would be the language that we speak. Suppose that we live in a community without a common language and struggle to communicate. We decide that we should all learn a common language. Supposing that all languages are equally easy to learn and have the same expressive capacities, etc., then it doesn't really matter which language we learn, so long as we can all speak the same language. Beyond conformity, there exists no non-arbitrary reason that would rationalise one's choosing to learn one

language over another. What each person decides to do consequently depends only on what others decide to do.

Conventions constitute solutions to coordination problems because their existence gives rise to a precedent for the choosing of one possible solution over others. This provides participants in a coordination problem with a non-arbitrary reason for reproducing that solution. Lewis initially characterises convention as follows:

A regularity  $R$  in the behaviour of members of a population  $P$  when they are agents in a recurrent situation  $S$  is a *convention* if and only if, in any instance of  $S$  among members of  $P$ ,

- (1) everyone conforms to  $R$ ;
- (2) everyone expects everyone else to conform to  $R$ ;
- (3) everyone prefers to conform to  $R$  on condition that others do, since  $S$  is a coordination problem and uniform conformity to  $R$  is a proper coordination equilibrium in  $S$ .

By (3), an individual member of  $P$  will prefer to conform to  $R$  when others do, and will cease to prefer to conform in this manner when others do not, in the manner of a disposition that co-varies with the dispositions of others. The reason - and indeed the only reason - for his conforming to  $R_1$ , as opposed to some arbitrarily different  $R_2$ , is the fact that others conform in this manner too. However, as yet this need not be a reason of which any of the members of  $P$  need have any knowledge: it need not be a reason *for the subject*.

Lewis refines his analysis of convention with several further clauses. A fourth clause makes it a requirement that clauses (1)-(3) are *common knowledge* among the members of  $P$ . Common knowledge (or 'mutual knowledge' - I'll use the terms interchangeably) occurs when two or more individuals not only know that  $\varphi$ , but also know that each other knows that  $\varphi$ , and also that each other knows that each other knows that  $\varphi$  and so on. Thus, for example, it must be common knowledge among the population of England that we speak

English and not some different language and that any individual's preference for speaking English, at least in England, is contingent upon the preference of the other members of  $P$ 's preference for speaking thus. According to Lewis, this clause serves two purposes. First, it reflects the fact that conventions typically are common knowledge among the members of a community.

One reason to amend the definition of convention is simply that we want to write into the definition all of the important features common to our examples [of conventions], and common knowledge of the relevant facts seems to be one such feature. (Lewis 1969, p.59)

Second, it rules out from being conventional the following case that Lewis finds intuitively unsatisfactory. Suppose everyone speaks English because each supposes that others do and each wants to facilitate his or her own linguistic interactions. However, suppose that every speaker of English also holds the following false belief  $f$ : "I speak English because I expect others to do the same, but my peers are all stupid and would speak English regardless; they would continue to do so even if no one else did." In the absence of the common knowledge clause such a case would count as conventional. However, Lewis thinks this "intuitively unlike clear cases of convention" (*ibid.*) and so introduces the common knowledge requirement to rule such cases out.

#### Knowledge of convention

Given the introduction of the common knowledge clause into the analysis of convention, it becomes constitutive of conventions that, for a regularity of performance  $R$  in situation  $S$  among the members of a population  $P$  to be a convention, it must be true and common knowledge that  $R$  is a convention. This claim is elaborated as follows:

[I]f a convention ... holds as an item of common knowledge, then to belong to the population of which that convention holds - to be party to it - is to know, in some sense, that it holds. If a regularity  $R$  is a convention in population  $P$ , then it must be



true, and common knowledge in  $P$ , that  $R$  satisfies the defining conditions for a convention. (*ibid.*, p.61)

In this passage Lewis introduces two requirements. The first is a constitutive claim, relating to the question of what it is for some regularity  $R$  in  $S$  to be a convention; the second is an epistemic claim, relating to the question of what one would need to know to participate in a convention. The constitutive claim is that a regularity of performance  $R$  in  $S$  among the members of  $P$  counts as a convention only if it is mutually known by the members of  $P$  that  $R$  "satisfies the defining conditions for being a convention". In other words, there could not be a convention that the members of  $P$  did not know to satisfy these conditions. The epistemic claim requires that in order to participate in a convention, the members of  $P$  in  $S$  should mutually know that the  $R$  that they perform satisfies the defining conditions for being a convention. If the participants in a convention must know mutually that some  $R$  satisfies "the defining conditions for a convention", then it follows that any individual considered in isolation must also know (non-mutually) that that  $R$  satisfies the defining conditions for being a convention. I'll call this knowledge of conventionality requirement the **KC requirement**. It's the KC requirement that I want to discuss now: what sort of knowledge would one need to possess in order to satisfy it?

The first important change that is brought about by the KC requirement concerns participants' grasp of the basis for their conforming to a convention. In the original formulation of the analysis, it was required of the members of  $P$  only that they act in accordance with (3) - by preferring conformity to  $R_1$  and not some arbitrarily different  $R_2$  conditional on the fact that others also preferred conformity to  $R_1$  and not  $R_2$ . They did not need to grasp the basis of their preference for acting thus. That is, they did not need to know the reasons that they had for preferring to act in this manner. With the introduction of the KC requirement, this changes. Now any member of  $P$  in  $S$  must know the reason for their preference for conforming. They must know that their own preference for conforming to  $R_1$  and not  $R_2$  is conditional upon the preference of others for conforming in this manner,

and that absent this conformity they would not prefer conformity to  $R_1$ , and that everyone else knows this too. This is because they must know that the activity in which they participate is one of many arbitrarily different possible solutions to a coordination problem.

For all that this is something that any member of  $P$  must know, there may be a variety of ways in which this is known. Lewis sets out three constraints that operate on the sort of knowledge that one would need to possess in order to satisfy the KC requirement. He concedes that our “knowledge of our conventions - that minimum of knowledge everyone has in virtue of his own participation - maybe be quite a poor sort of knowledge” (*ibid.*, p.63). It is knowledge nonetheless.

First, one’s grasp of the nature of convention might be only “potential knowledge”, where this consists in just one’s having a grasp of features from which the character of conventionality could be inferred:

We must have evidence from which we could reach the conclusion that any of our conventions meets the defining conditions for a convention, but we may not have done the reasoning to reach the conclusion. (*ibid.*)

What this amounts to all depends on what it is to have evidence from which the conventionality of an activity could be inferred. Since Lewis doesn’t say much about what this might amount to, I propose simply to adopt Timothy Williamson’s (Williamson 2000, chapter 9) account of the nature of evidence and to read Lewis in light of this.<sup>32</sup> On Williamson’s account, what constitutes evidence is something of which a subject has knowledge. In the case of potential knowledge that some  $R$  is conventional, let what it is

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<sup>32</sup> Since Williamson’s account was published 30 years after *Convention*, it can’t be what Lewis had in mind at the time. It’s consistent, though, with the spirit of Lewis’s remarks. There may be a disagreement between Williamson and Lewis about whether evidence is *propositional* knowledge, because Lewis but not Williamson thinks that knowledge can be non-propositional. However, this debate doesn’t need to be settled here and I won’t take a stand on it.

that a subject knows be represented as  $\kappa$ . This  $\kappa$ , whatever it is, is such that one could infer from it that the activity in question satisfied Lewis's defining conditions of convention. However, since one could know  $\kappa$  and still fail to reason to the conclusion that some  $R$  is conventional, one might count as having potential knowledge of the conventionality of that  $R$  even if one were inclined to deny this when answering unreflectively the question: is that  $R$  conventional?

In addition to our knowledge of convention being merely potential knowledge, Lewis also holds that this knowledge need not be verbal knowledge. That is, one need not be able to state what it is that one knows when one knows that some  $R$  satisfies the defining conditions for convention. Lewis also rejects the idea that this knowledge could be characterised by what Michael Dummett later (and without reference to Lewis) called "implicit knowledge". This is "knowledge which its possessor is incapable, unaided, of formulating verbally, but of which he can recognise a formulation when presented with one" (Dummett 1991, p.95). One who had implicit knowledge that some  $R$  satisfied the defining conditions of being a convention could recognise an accurate description of that knowledge, without being able to produce any such description oneself. However, Lewis argues that, with respect to a description of our knowledge of the conventional basis of some  $R$ , it could be that "[we] can neither give it nor tell whether it is true if it is somehow given" (Lewis 1969, p.64).

Like it or not, we have plenty of knowledge that we cannot put into words. And plenty of our knowledge, in words or not, is based on evidence that we cannot hope to report. (*ibid.*)<sup>33</sup>

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<sup>33</sup> Here one might worry that evidence that one could not hope to report could not be propositional, and that since Williamson's account of evidence makes evidence propositional knowledge, adopting Williamson's account of evidence here must violate the spirit of Lewis's account. In fact, this does not follow. Williamson is happy to concede that the propositions of which one has knowledge might be expressed only inarticulately. For example, it's consistent with Williamson's account that in knowing of some  $a$  that it is  $F$ , one might know only the proposition that *a is like that*, where the demonstrative

In this case, someone who has potential knowledge that some  $R$  is conventional has knowledge of  $\kappa$  from which the conventional nature of  $R$  could be inferred. What it is that he knows in knowing  $\kappa$  need not be something that he could express verbally or even recognise when described, though. Perhaps because this knowledge is non-verbal, Lewis doesn't say much about what it consists in. Rather, he builds his discussion around an example of non-verbal knowledge possessed by two rowers in a boat. Having fallen into a certain steady rhythm  $R$  for rowing, neither rower need be able to describe or recognise a description of their preference for rowing at this rate. Nonetheless, its successful facilitation of their coordination provides both with non-verbal knowledge of  $\kappa$ , from which each could form an expectation that *ceteris paribus* the other will prefer to row at that rate, contingent upon the first individual's preference for rowing at that rate. In Lewis's words:

Our success in rowing at that rhythm for the last few strokes is evidence by which I arrive at my expectation that you will continue to row thus; that you prefer to row thus if I do; and that you expect me to go on rowing thus. (Lewis 1969, p.63)

But what is this evidence? What could knowledge of  $\kappa$  consist in?

Given the potential knowledge requirement, what knowledge of  $\kappa$  - the evidence available to the rowers - consists in must be (by definition) sufficient to license the inference that the rhythm at which they row has some feature (namely  $\kappa$ ) on the basis of knowing which each rower could reason to the conclusion that their activity satisfies the defining conditions for being a convention. That is, knowledge of  $\kappa$  must be sufficient for each to infer from it that

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picks out some property  $F$  that one couldn't articulate more adequately. See Stanley & Williamson (2001) for relevant discussion. Williamson has also endorsed this point in conversation with the following example. David Beckham might know of some football that if he kicked it like *that* it would go in the goal. However, it's not necessary for attributing to Beckham knowledge of that proposition that he be able to describe what kicking the ball like *that* consists in.

he prefers to row at rate  $R_1$  and not a different rate  $R_2$  only because the other prefers to row at  $R_1$  and not  $R_2$ , and that the other's preference for rowing at  $R_1$  and not  $R_2$  is similarly contingent. Given this requirement, it's clear that some possible specifications of  $\kappa$  could not be correct. For example, suppose the first rower took his cues from the second rower and therefore knew something like: *I'm doing this because you are*. This couldn't count as knowledge from which the conventional status of their action could be inferred, since this state of affairs is consistent with the possibility that the second rower is rowing at  $R_1$  because that is (or is believed to be) objectively the best rate at which to row. This would be consistent with the possibility that  $R_1$  is not (or is not believed to be) the solution to a coordination problem, in violation of (3).

While Lewis doesn't say so himself, a plausible candidate for  $\kappa$  in the case of the rowers is knowledge that although the rowers were currently rowing at  $R_1$ , some different rhythm  $R_2$  could equally have been used in place of  $R_1$ , because  $R_1$  and  $R_2$  were only arbitrarily different. Only in virtue of knowing this could each participant infer the absence of any reason for preferring to row at  $R_1$  and not  $R_2$  independently of the other's also preferring thus. A more general characterisation of  $\kappa$  might therefore identify it with something like knowledge of convention in the platitudinous sense. That is, someone who knows  $\kappa$  would know, if only non-verbally, that the effect achieved through the performance of some regularity  $R_1$  might just as well have been achieved through the performance of a different regularity  $R_2$ , and knows that other participants know this too. Consequently, I propose to read knowledge of  $\kappa$  as consisting in (non-verbal) knowledge of the arbitrariness of one's performance of  $R_1$  - knowledge that some  $R_2$  could have been used just as well. There may be other variables of  $\kappa$  that could also satisfy the potential knowledge requirement, but if they differ from the specification just suggested, it's not by much.

The third constraint that Lewis proposes on knowledge of convention is that one could possess it even if one lacked generalised conceptual knowledge of the variables specified in

his analysis. According to Lewis an individual with knowledge only of this sort would still count as having knowledge of convention in the following sense:

[G]iven any instance of *S*, he knows how he and each of his fellows would act therein (namely, in some way that we would call conformity to *R*). And he knows that they do so by convention; that is, given any of the defining conditions of convention as applied to a given agent in the given situation, he knows the condition is satisfied. But he cannot think of more than one instance - the given one - at a time. He has no general concept of an instance of *S*, of a member of *P*, or of an action in conformity to *R*.  
(*ibid.*, p.67)

This constitutes a requirement that a participant in a convention should, on any *particular* performance of *R* in *S*, know that other members of *P* conform to *R* in *S* only in the manner specified by conditions (1)-(3).

Let it be the case that someone who has knowledge that *R* is a convention in accordance with these three criteria *knows weakly* that *R* is a convention. The weakest formulation that permits someone to know weakly of some *R* that it is conventional would be something like the following. Upon the occasion of an individual's reproducing some action *R* in a particular instance of *S* in which others are also reproducing *R*, that individual *could* (although might not) reason from non-verbal knowledge of  $\kappa$  to an expectation about whether or not he would *R* in that situation on that occasion were it not the case that those others were also performing *R*. He should, if he reasons in this manner, be able to form expectations that, on this occasion, were it the case that the others did not reproduce *R*, nor would he and that absent general conformity to *R* among those present, any particular other member of that group in this same situation on this occasion would also no longer continue to *R*.

Before leaving the exposition of Lewis's analysis of convention behind, some further modifications to the theory made by Lewis should also be noted briefly. First, it is no longer required that every instance of *S* be a genuine coordination problem; only that there be

cases of or appropriately similar to  $S$  that are. Second, the requirement that *all* should conform to  $R$ , and that an individual should conform to  $R$  only on the expectation that all others will do likewise, is relaxed. An expectation of conformity by almost everyone is more realistic, because individuals are flexible and anticipate non-conformity and error by some, and it remains sufficient for the generation of higher-orders of expectation about others' activities that facilitate individuals' action in situations where coordination is necessary.

These changes entail the following revision of the analysis of convention:

A regularity  $R$  in the behaviour of members of a population  $P$  when they are agents in a recurrent situation  $S$  is a *convention* if and only if it is true that in any instance of  $S$  among members of  $P$ ,

- (1) almost everyone conforms to  $R$ ;
- (2) almost everyone expects almost everyone else to conform to  $R$ ;
- (3) almost everyone prefers to conform to  $R$  on condition that most others do, since  $S$  is (at least in many cases) a coordination problem and near-uniform conformity to  $R$  is a proper coordination equilibrium in  $S$ .
- (4) (1)-(3) is common knowledge among the members of  $P$ .

That is,  $R$  is a convention when it's a solution to a coordination problem  $S$  (in at least many  $S$ -type cases), and where the members of  $P$  prefer to reproduce  $R$  only because most others do so, and where this is common knowledge. Since it's characteristic of a coordination problem that it should have multiple arbitrarily different solutions, then it follows that members of  $P$  should, given the KC requirement, know weakly that the reason for their performance of some particular  $R_1$  is the adherence to  $R_1$  of other members of  $P$ , and not any intrinsic reason to prefer  $R_1$  as a solution to  $S$  over any arbitrarily different  $R_n$ .

What cognitive abilities are necessary for participating in a convention?

The question posed at the outset was: what are the cognitive abilities that one must possess in order to participate in a convention? Lewis's knowledge of convention requirement

entails a partial specification of an answer to this question. "Full" participation in a convention  $R$  in  $S$  requires knowing weakly that  $R$  satisfies the defining conditions for being a convention - what I have called the KC requirement. Despite the weak constraints that operate on satisfying the KC requirement, though, Lewis concedes that there might be individuals who could not participate in conventional activities - not, at least, in the fullest sense. Several times he notes that "infants and the feeble-minded" could not participate in conventions in the same ways that we do. For example:

[The] convention of a language is ... a regularity restricting one's production of, and response to, verbal utterances and inscriptions. Linguistic competence consists in part of a disposition to conform to that restriction with ease; and in part of an expectation that one's neighbours will be likewise disposed, with a recognition of their conformity as the reason for one's own. No doubt a child or an idiot may conform without reason; if so, he is not party to the convention and his linguistic competence is incomplete. (Lewis 1969, p.51)

Similar comments appear in several further places. What would be the grounds for thinking that infants could not partake in conventional activity in the same way in which we do?

It is evident that Lewis doesn't have a single, unambiguous answer to this question. In fact, in different places he gives at least three apparently distinct answers. At (p.51), he suggests (i) that what infants would lack is knowledge that others' conformity to  $R$  is the only reason for their own. This is akin to conforming in accordance with (3) but without having knowledge of (3), in the manner previously described. Here Lewis says that infants perform "without reason" and with only incomplete linguistic competence. A more careful specification is that infants do perform with reason - since there does exist a reason for which they perform - but that they lack knowledge of this reason; it is not a reason *for them*. In particular, what they lack is knowledge that their only reason for conforming to  $R_1$  and not  $R_2$  is that others also conform in this manner. This diagnosis seems to be echoed at (p.62) and again on (p.75), when Lewis speculates that infants fail to grasp that their



reproduction of  $R_1$  is merely conditional because they don't appreciate that they would prefer conformity to  $R_2$  and not  $R_1$  just in case others preferred to conform to  $R_2$  and not  $R_1$ . Lewis also states at (p.62) that (ii) infants might lack the expectation that others conform. Finally he considers (iii) the possibility that infants might possess the relevant contingent expectations and desires but not as items of common knowledge.

Lewis's assumption that infants don't participate in conventions in the manner required by his analysis has some intuitive pull. However, none of the assumptions (i)-(iii) is accompanied by much in the way of an argument. On Lewis's behalf, I'll now sketch out an argument that looks at least prima facie compelling. This argument corresponds to Lewis's (i). I won't yet try to relate it to Lewis's (ii) and (iii) in any systematic way (although I'll say more about these in due course).

As Lewis himself notes, one possible obstacle to complete participation in conventional activities might be the KC requirement, which is itself entailed by the requirement that the defining conditions of conventionality be mutually known. Even taking into account Lewis's relaxed constraints on knowledge, it seems plausible that an infant of 15-months might fail to satisfy this requirement - because it's also plausible that much older participants could fail to satisfy this requirement. This might be because, for example, satisfying the KC requirement would require one to grasp (3) by being able to form the expectation that were the other members of  $P$  in  $S$  to cease to prefer conformity  $R_1$  in  $S$  and instead to start to prefer conformity to  $R_2$ , then one would also cease to prefer conformity to  $R_1$ . But it may be that the participants in a conventional activity ordinarily did not form such expectations. In this case, they would fail to count as even weakly knowing that  $R$  satisfied the defining conditions of conventionality, even if they were competent in their use of the regularity  $R$  the nature of which was conventional. Problematically for Lewis, if the plausibility of infants' failure to grasp (3) stems from the fact that older participants in a convention could also fail to grasp (3), this constitutes a reason for thinking that a grasp of (3) is not

necessary for participating in conventional activity in the manner that Lewis requires. This is the claim for which I will argue now.

#### Criticisms of Lewis's account

Contra Lewis, I suggest that in at least some cases of conventional activity, including linguistic activity, in a population  $P$  the participants of which make use of a convention  $R$ , only very few or even no members of  $P$  need know that  $R$  is a convention (that is, that it satisfies Lewis's defining conditions for conventionality). In particular, requiring that participants in a convention  $R$  know that (3) is true of  $R$  too strong.

Language is a paradigmatic case of an activity in which conventions feature prominently, yet we can certainly make sense of there being a perfectly competent community of language users who lacked even weak knowledge of the conventionality of their linguistic practices. For example, as Tyler Burge notes (1975/2007, p.33), there have been in the past and perhaps even still are communities of language users who have no inkling of the existence of languages other than their own. The members of these communities might simply fail to grasp that their language was not uniquely suitable for the tasks for which it was used – just because it had never previously occurred to them that this might not be the case. On the interpretation of  $\kappa$  outlined previously, which takes knowledge of  $\kappa$  to consist in knowledge that other  $R$ s might equally have been used in  $S$ , individuals in this case would fail to know  $\kappa$  in the manner required for them to have potential knowledge of the conventional nature of  $R$ . Such individuals wouldn't obviously have knowledge from which they could infer that (3) is true of  $R$  and they might subsequently fail to form the appropriate expectations about how they would react were their peers to stop conforming to  $R_1$  and instead to start conforming to  $R_2$ . Perhaps they would respond only with bewilderment when asked what they would do should their peers cease to speak that language. Since it has never occurred to them that there might exist possible other

languages that their peers could speak instead, they might insist that anyone who stopped speaking thus would simply cease to be able to communicate.

Examples like this one can be taken further. In addition to there being linguistic communities unaware of the existence of different languages, there might also be those who explicitly denied that (3) is true of  $R$ . They might insist that their language is not at all arbitrary but uniquely fit for its task, each phoneme having a divine origin that tied it non-arbitrarily to the expression of a particular idea. On this view of language, the meaning of each word in the language would be a complex idea determined by the combination of its constituent phonemes. Although such a view now seems laughable, it has historical pedigree. In the *Cratylus*, Socrates entertained the possibility that in deciding upon the names for things, one should “know how to embody in sounds and syllables the name naturally suited to each thing” (*Cratylus* 389d). It’s also a central tenet of Kabbalistic thought that the relation of Hebrew words to the ideas that they can be used to express is not arbitrary, and the 15<sup>th</sup> century German philosopher and theologian of the occult Cornelius Agrippa once wrote that “names contain within them all the remarkable powers of the things that they indicate” (from *De occulta philosophia*, cited in Eco 1995, p.120). Proponents of this view might know of the existence of other languages but argue that each alternative was expressively inferior and consequently could not be substituted without loss. Such arguments were common among European linguists of the seventeenth and eighteenth centuries (Eco 1995, pp.95-103).

Subscribers to the view that there is a non-arbitrary relation between the meaning of a word and its intrinsic properties might deny that they would cease to conform to  $R_1$  were their peers in some  $S$  to switch to  $R_2$  instead, even if in practice once enough of the others had switched to  $R_2$  they would follow. Or, as Burge suggests (1975/2007, p.34), they might simply prefer to stop talking altogether, rather than give up on God's language. Cases like these constitute counter-examples to the KC requirement, since the members of  $P$  fail to

grasp that *R* satisfies (3), which is one of the defining conditions of conventionality. It might be objected that in cases where a religious belief prevents the acknowledgement that a language is not uniquely fit for task, participants do meet the KC requirement - for example, by possessing knowledge of  $\kappa$  in conjunction with a further religious belief in light of which they suppressed their knowledge of  $\kappa$  and which prevented them from reasoning from  $\kappa$  to the conclusion that some *R* was conventional. However, such blocking beliefs might still make it appropriate to withhold potential knowledge from these individuals, and in any case it's certainly not the only way in which we can make sense of the counter-example. It could simply be that the existence of religious beliefs precedes the members of *P*'s ever attending to the nature of their activity, and subsequently prevents them ever from acquiring the clear view of it that would be necessary for them to acquire knowledge of  $\kappa$ . Thus, for example, they might never reach a vantage point from which they could see that other languages really are equal in their expressive power, and so on. In this case they do not have potential knowledge that *R* is conventional, since they lack the knowledge of  $\kappa$  that would permit them to infer this.

A counter-argument here might try to accommodate this objection by stretching the concept of potential knowledge to incorporate cases in which the members of *P* currently lacked the requisite knowledge  $\kappa$  and might not be able to acquire it without some substantial investigation or education. On this reading, having evidence might not consist in knowing  $\kappa$ , so much as being in a position to acquire knowledge of  $\kappa$  by paying close attention to the nature of one's activity. However, such a move would surely stretch the concept of potential knowledge beyond credulity. Where participants' view of  $\kappa$  is obstructed such that it might take a willing anthropologist, philosopher or linguist many months or even years to disabuse the members of *P* of the idea that their language was uniquely suitable for task, attributing to them potential knowledge would empty the

content of such ascriptions.<sup>34</sup> In this case, this move to defend the Lewisian position fails. Either evidence is construed as knowledge of  $\kappa$ , in which case such knowledge is not necessary for participating in a convention, or having evidence does not consist in knowing  $\kappa$  so much as in having the ability to acquire knowledge of  $\kappa$ , and the concept of potential knowledge is empty. In conclusion, the participants in a convention need not have even potential knowledge that (3) is true of the activity in which they participate.

A different defence might challenge the idea that the activities described above are really conventional. Perhaps, one might think, there's a further feature of the cases described that means that they are no longer conventional. In this case, what the argument would show is not that Lewis's account of convention is wrong, but that language doesn't need convention.

This response surely isn't the correct one. Lewis's account of convention started as the investigation of a platitudinous concept of convention, of which language was taken to be a paradigmatic example. The linguistic practices of the communities described above still meet the requirements of the platitudinous notion of convention. The languages that they speak could have been very different, and the fact that they are not is merely a historical quirk. However, they don't meet the requirements of a conventional activity on Lewis's considered account. This is problematic because Lewis's analysis was proposed as an analysis of either the platitudinous notion of convention, or something very close to it ("a

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<sup>34</sup> Dummett makes a similar point with respect to attributions of implicit knowledge in *The Logical Basis of Metaphysics*. He observes that the concept of implicit knowledge is elastic, but not infinitely so (p.97). In some circumstances we might say of someone who failed to recognise a description of their practice that nonetheless they did have implicit knowledge of it. For example, someone woken in the middle of the night, or emotionally distressed or otherwise preoccupied, would surely be forgiven any failure to recognise what they knew implicitly. But in cases where serious education is required, the rationale for attributing such knowledge is much less clear.

legitimate heir"). While this might be consistent with an account that rejected as conventional cases that were previously thought peripheral candidates for conventionality, it is surely not consistent with an account of convention that rules as non-conventional cases that are as paradigmatic as language. Since language is a paradigmatic case of convention, and since there could be languages that speakers did not know to satisfy (3), it cannot be true that something can be conventional only in virtue of being known to satisfy what Lewis calls the defining conditions of conventionality.

Even where the participants in a convention don't know that (3) is true of the activity in which they participate, they might still act *in accordance* with (3). It's just that the reason that they take to be the reason for their acting thus is not the reason that Lewis requires of them. For example, suppose one rower, taking his cues from his partner, rowed at the rate that he did because the other rower did - and so took his reason for acting thus to be the fact that his partner was. As was discussed previously, this knowledge is not knowledge of  $\kappa$  (and therefore not potential knowledge of the conventionality of  $R_1$ ), since it's insufficient to license the conclusion that the performance of  $R_1$  is only arbitrarily different from other possible regularities. In other words, it's insufficient to license the conclusion that Lewis wants, which is that one's *only* reason for preferring to conform to  $R_1$  and not  $R_2$  is that others also prefer to conform at that rate. Nonetheless, the rower would act in accordance with (3) if he preferred to row at  $R_1$  only when his partner did.

Lewis's blindness to the possibility that participants in a convention might fail to know  $\kappa$  can be diagnosed as a consequence of the narrow focus of the examples with which he illustrates his discussion of convention. These examples tend to focus on cases in which participants find themselves actively engaged in a coordination problem that currently lacks a solution, and to which they are required to find a solution by somehow settling upon one regularity of performance that is only arbitrarily different from possible others. In such cases, the existence of possible other solutions to the coordination are particularly salient,

since the participants must choose consciously between arbitrarily different possibilities. In many other cases of convention, though, this situation never arises. For a great many of the conventions in which we participate, we are born into those conventions – such that choosing between arbitrarily different regularities of performance is not something that we ever have to do. For example, we never had to choose whether to speak in English or French: we simply inherited the practices of our peers. In such cases, in which the regularities with which we are to conform are long since established, it's not implausible that we should fail come to grasp the arbitrariness of our performance, since the property  $\kappa$  from which the conventionality of our activity could be inferred need not be something to which we ever have to attend. The arbitrariness of the activity might be covered up by a fog of cultural practices that have been inherited and accepted without question.

With respect to Lewis's motivation for introducing the mutual knowledge claim (of which the knowledge of convention claim is a part), two things should be said. First, Lewis states that we typically do have mutual knowledge of our conventions. While it may be true that we typically *do* know that our conventional activities are conventional, it isn't necessarily so. Second, Lewis claims that the mutual knowledge claim is needed to rule out cases that are intuitively different from standard cases of convention. But while the recalcitrant case that Lewis wants to rule out may be "intuitively unlike clear cases of convention" (Lewis 1969, p.59) – it doesn't follow from this that the mutual knowledge clause is the correct fix. Lewis simply does not consider any alternative ways of ruling out the recalcitrant case.

What becomes of the characterisation of convention now? One possibility is that in a situation  $S$ , the members of  $P$  expect to conform to  $R$  and expect that others will conform to  $R$ , and perhaps also know mutually that almost everyone in  $P$  conforms to  $R$  in  $S$ , but where their expectations about others' activities are not grounded in knowledge of (3).  $R$  functions in the manner that it does just because everyone takes it to do so – such that an individual's conformity to  $R$  is in fact counter-factually dependent upon the conformity to  $R$  of others.

But it may be opaque to the members of  $P$  that this is why  $R$  functions thus. They may simply lack the insight or historical knowledge that would reveal  $S$  to be a coordination problem and  $R$  to be a solution to it. The resultant account would have the following character:

A regularity  $R$  in the behaviour of members of a population  $P$  when they are agents in a recurrent situation  $S$  is a *convention* if and only if it is true that in any instance of  $S$  among members of  $P$ ,

- (1) almost everyone conforms to  $R$ ;
- (2) almost everyone expects almost everyone else to conform to  $R$ ;
- (3) almost everyone prefers to conform to  $R$  on condition that most others do, since  $S$  is (at least in many cases) a coordination problem and near-uniform conformity to  $R$  is a proper coordination equilibrium in  $S$ .
- (4) (1) and (2) are common knowledge among the members of  $P$ .

It should be emphasised that I do not wish to endorse this account of convention as the correct one. My goal here is not the development of any positive account of what it is for an act to be conventional. The above formulation is just what's left of Lewis's account when it has been revised in accordance with the argument against knowledge of the KC requirement that I have contended here. Taking this account of convention as my starting point, without thereby endorsing it, I will now consider the question: what are the cognitive abilities that one would need to attribute to an individual in order to explain his participation in a conventional activity?

#### Imitation and convention

One minimum constraint on knowledge of convention is that whatever one knows must be functionally adequate to enable participants to coordinate their activities in coordination problematic situations. Conventions constitute solutions to coordination problems because, by making it salient that one coordination equilibrium is to be preferred to the exclusion of others, the existence of a convention gives each member of  $P$  reason to pursue that activity



to the exclusion of other arbitrarily different activities. This in turn permits participants to generate further expectations about the behaviour of others and about others' expectations about their own behaviour, further facilitating the coordination of the activities of the members of *P*. In a situation in which coordination in *S* had already been achieved, that coordination could be maintained if the new members of the community were able to *imitate* the conventional actions of the existing members, since this would preserve the status quo in which coordination had already been achieved. Consequently imitation is a sufficient means for the transmission and perpetuation of conventional behaviour, including conventional linguistic behaviour. However, imitation does not require the imitator (or even the imitated) to grasp that the imitated behaviour is a conventional one, since imitators need not grasp that the action that they imitate is one of many possible arbitrarily different means to the achievement of that end. This is consistent with the claim that one need not know even weakly that (3) is true of a conventional activity in order to participate in it.

As Tomasello and Carpenter (2005) define the term, imitation is the ability to observe and act with not just the intended goal of another's action, but also to pursue that goal by the same means. Suppose, for example, that I see you score the peel of a banana with your finger nail before peeling it, in order to make the banana easier to peel. Usually I just twist the end but this is strenuous and it doesn't always work that well, particularly when the banana is unripe. Indeed, suppose I have a store of bananas that I've been longing to eat, but that I've so far failed to peel, because peeling bananas is harder than it looks, especially to the technique-impooverished. Having seen you successfully perform your action, I return to my pile and reproduce what I've see you do. I peel my bananas successfully. By employing the means that I saw you produce in pursuit of the same end, I have imitated you. A general characterisation of this process might be given as follows:

In imitation, an imitator *M* reproduces an action *A* in pursuit of some end *E* on the basis of having seen another individual *C* perform *A* in the pursuit of *E*.

The banana case illustrates the possibility of the transmission of knowledge through imitation. I learned a more efficient way to peel bananas by watching you do it, and by reproducing your action.

Tomasello & Carpenter (2005) also introduces helpful distinctions between imitation and several similar concepts relating to purposive activity, which I will adopt. In contrast to imitation, *mimicry* is when an action - for example, a bodily movement - is copied, but without reference to any goal-directed activity. In a case like the one described above, I might, having seen you perform your action, reproduce the scoring action that I saw you produce but without doing so for the purpose of making my banana easier to peel. Indeed, I may even struggle to grasp that that action could be performed as a means to some end, rather than as an end in itself. *Emulation* occurs when one acts with the same goal as in an observed action, but pursues this goal by a different means. If, on the basis of watching you peel your banana, I acted with the same intention, but without first scoring the skin with my nail, then I might be said to have emulated your goal. In some cases, emulation may be a form of discriminating imitation - for example, if the imitator perceives that the first individual is trying to reach her goal by means of some inefficient action.

On the characterisation of conventional activities defended here, conventions are purposive activities, since they are means to the end of coordination in coordination problematic situations.<sup>35</sup> We drive on the same side of the road as our peers so as not to crash into oncoming traffic, and we speak the language that they speak in order to communicate

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<sup>35</sup> It might be objected that not all conventions are purposive. For example, one might think it conventional that baby boys are dressed in blue and girls in pink, and that this serves no interesting purpose (although perhaps it could or did in the past). There may be more than one type of activity that others want to call 'convention'. If some of these activities are non-purposive (and therefore not solutions to coordination problems), the discussion of this chapter should be taken to apply only to conventions that are purposive in character.

efficiently. Because conventional activities are purposive - that is, means-end activities - they could in principle be learned imitatively.

As the discussion of Grice in Chapter Four illustrated, linguistic utterances are means to the achievement of linguistic ends. (On the Gricean account, these ends are perlocutionary effects. On some occasions speakers might also speak with the intention of producing illocutionary effects, although I have argued that there is currently no good reason to attribute illocutionary intentions to young infants.) Communicative goals might be achieved through both conventional and non-conventional means. My waving idiosyncratically might be an unconventional means of telling you that I'm coming over, but the same effect could be achieved conventionally by my addressing you with the sentence "I'm coming over". One way to think of learning a language is as a case of acquiring the conventional means with which to achieve one's communicative goals. One could acquire knowledge of a linguistic convention just by observing and reproducing imitatively the linguistic means to some end that one has seen others produce. Consider the following case.

Suppose that I watch you trying and failing to peel your bananas in just the manner that I have done. You sit in front of your stash looking glum, just as I am doing. However, you see a third person, to whom you call out "Help!" whilst pointing to your bananas. The third person walks over to you and peels your bananas. Your goal has been achieved. I see all of this. In this case, and particularly if I see it happen more than once, when I want my bananas peeled, I have reason to point to them whilst calling out "Help!" in the direction of the third person, just as you did - because I've seen that performing this action is an effective means to the achievement of my goal. The knowledge that *A* is an appropriate means to the achievement of *E* is for the imitator a reason in light of which he acts.

By imitating your linguistic action, I acquire knowledge of how to use a convention - in this case, the uttering of "Help!" - to achieve the end that I desired to bring about. In time,

either by experimenting by myself or by seeing others use the same expression for different ends, my grasp of the uses of this particular convention should extend to further cases. Additionally, different words could be learned in analogous ways. For example, I might learn that calling out "Look!", perhaps while pointing, is a good way of getting you to share with me attention to the object at which I pointed, or that calling out "Hammer!" is a good way to ask for the hammer.

The general characterisation of imitation outlined above can be rewritten in the same terms used in the Gricean analysis of the communicative act. The imitator *M* takes the role of (*H*)earer, and the imitatee *C* that of (*S*)peaker. Where a speech act is imitated, the imitated action *A* has the form of an utterance *x*, and the end *E* in pursuit of which it was produced can be specified in terms of the speaker's perlocutionary intention *r*. Using these variables, a general model for acquiring the ability to use linguistic conventions might be given as follows:

- (I) *H* grasps *S*'s desire to bring about *r* (where *r* is a perlocutionary effect).
- (II) *H* grasps that *S*'s production of a speech act *x* is a means towards the achievement of his (*S*'s) desired end *r*.
- (III) By imitating *x* in pursuit of *r*, *S* acquires the ability to exploit the existence of the linguistic convention *x*.

On this basis, by grasping that *x* can be used as a means to the achievement of the perlocutionary effect(s) specified in *r*, *H* acquires the ability to participate in the using of *x* as a solution to a certain coordination problem (namely, the problem of how to achieve *r*-type goals). This ability is presumably partly, although perhaps not wholly, constitutive of knowing the meaning of *x*. Recalling Wittgenstein's discussion of the relationship between meaning and use (*PI* §43, for example), it is to know how to use *x* in communication.

This is the model of utterance learning that was gestured at in the previous chapter. In a context of interaction, perhaps typically (but by no means always) between infant and

caregiver, the infant identifies the speaker's perlocutionary intention and grasps that the words she uttered are means to the achievement of that end. Thus, the infant's first grasp of meaning is of the perlocutionary effects for which words can be used as means in communication. It may be only later that the infant acquires a more sophisticated understanding of meaning - for example, one that would enable to utter with illocutionary intentions, in addition to or in the absence of perlocutionary intentions.

It's worth noting that there exists a difference between the case in which one peels one's banana by slicing it with one's nail and the case in which one asks another to help by calling out "Help!". The former case differs from cases of conventional activities because it is a non-arbitrary solution to the problem it is used to solve. Consequently, in the banana-peeling case, there exists a reason to act irrespective of whether or not anyone else does, because the action is intrinsically well-suited to the task for which it is performed. In the conventional case, by contrast, there exists a reason to imitate an action if and only if others also reproduce that action.

Although this difference is genuine, it's not obviously relevant to the possibility of learning by imitation. Although it may be that an utterance that employs linguistic conventions is effective only because of the existence of conventions for using those words, this need not be part of the knowledge in light of which an imitator imitates. In the context of activity in which the imitated action is observed, the imitated action presents itself as a solution to the task in hand in just the same way that a non-conventional action would. Consequently, where the imitated action is a conventional tool, it need not be evident to any of the participants in a situation that this is the case. This amounts to a phenomenological claim for the superfluity of knowledge of (3) for participation in a conventional activity. Where a convention already exists, problems like the one described above typically don't present themselves as coordination problems demanding one of potentially many arbitrarily different solutions. Rather, when we observe others perform a speech act in order to bring

about a certain effect, the words that they employ in the pursuit of the end that they pursue are simply given to us as the appropriate means with which to pursue that goal. We might learn that there are speech acts that should be performed in order to bring about certain effects long before it ever occurs to us that countless arbitrarily different speech acts might, in a different possible world, have done the job just as well. Nonetheless, it should be clear that where participants in a convention conform to *R* on the basis of learning to do so imitatively, the coordination to which linguistic conventions constitute a solution is achieved. Where a solution is learned imitatively the status quo will be persevered and so the ability of the members of *P* to coordinate their activities will be retained.

Common knowledge of (1) and (2)

On the account of convention that remains following the revision for which I have argued, knowledge of (3) is not required of participants in a convention. Nonetheless, clauses (1) and (2) of Lewis's analysis remain within the mutual knowledge clause. Consequently, for *R* to be a conventional activity, and for the members of *P* to participate in it, it must be common knowledge among them that almost everyone conforms and expects almost everyone else to conform to *R* in *S*.

Since it's not my intention to develop or defend a positive account of what it is to participate in a convention, I will not defend this positive account of convention. Suppose though, if only for the sake of argument, that it's true.<sup>36</sup> Is the possibility of acquiring

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<sup>36</sup> According to Tomasello, "conventions are shared or agreed upon - in the sense that we all know we all know them" (Tomasello 2006, p.31). This might be seen as an endorsement of the view that the participants in a convention *must* have mutual knowledge of (1) and (2). However, Tomasello's remark is consistent with its being a contingent fact that among the members of *P* almost everyone conforms and expects almost everyone else to conform to *R* in *S*, without its being necessary that the members of *P* know (1) and (2) mutually.

knowledge of linguistic conventions by imitation sufficient to support the requirement that it be common knowledge among the members of  $P$  that almost everyone conforms and expects almost everyone else to conform to  $R$  in  $S$ ? That is, could one acquire common knowledge of (1) and (2) in a situation in which one learned conventions through imitation?

Successful communicative interaction is a paradigmatic case in which common knowledge is generated. If I grasp your communicative intention and acknowledge to you that I have done so (for example, by answering your question or doing what you had asked) in a manner that you grasp, then you know that I know what you had intended to communicate, and that I know that you know this, and I know that you know this, and so on. It's consistent with this that where infants acquire knowledge of linguistic conventions through imitation, and subsequently are able to use this knowledge to communicate successfully, mutual knowledge is generated. For example, if I ask you to help me by calling out "Help!", and in response to my having uttered thus you help me, then I come to know that uttering thus is a way to get help, and you know that I know this, and so on. It's likely that in all such cases there will, in practice, be an upward limit on how many iterations of this knowledge an individual is able to track, and in the case of infants this is likely to be lower than in adults, but there is at least no principled obstacle to the acquisition of mutual knowledge in these circumstances.

It's a further question whether first-second person interactions with only one or a handful of interlocutors could be sufficient for acquiring mutual knowledge that almost all others conform and expect each other to use language in that way. Certainly inductive reasoning would not license this conclusion. (This is not to suggest that infants would employ inductive reasoning in such cases.) It may be then that mutual knowledge of (1) and (2) is acquired by infants only later. One possibility is that what the infant acquires in learning is a grasp of a general feature of an utterance  $x$ , namely that it is an effective means to achieving some communicative goal (or some range of communicative goals). In this case it

would start with a general expectation according to which all would use  $x$  thus. Mutual knowledge of this expectation might be acquired only later - after subsequent communicative interactions with others had borne out the original expectation. In this case, infant participation in conventional language use would differ from adult participation in a similar manner to that described by Lewis in (iii), where infants would possess the relevant contingent expectations and desires about the circumstances in which the members of  $P$  would perform  $R$  in  $S$  - although of course without the unnecessary expectation that (3) is true of  $R$  - but not as items of common knowledge. Alternatively, in a manner similar to Lewis's (ii), it may be that infants do not expect that others would conform to  $R$  in  $S$ , and that they acquire the expectations in (1) and (2) only later - perhaps again in light of exposure to others' uses of language. Ultimately, these are empirical claims and robust empirical evidence would be needed to rule either in or out. However, in each case the acquisition of the relevant knowledge/expectation is something that would come with greater experience of linguistic interaction, and would not require the postulation of any further cognitive ability. It's therefore not necessary to complicate the model of the cognitive abilities that must be attributed to infants in order to explain their coming to participate in linguistic conventions.

Do infants imitate?

Since infants of 15 months appear to participate in conventional activities, if this participation is to be explained, these abilities ought to be attributable to infants of that age. It therefore constitutes empirical support for this thesis that there exists a wealth of evidence that by the time infants are starting to use their first words, they already possess quite sophisticated skills for imitating the means-end activity that they observe others to produce. These skills would be consistent with the possibility of their learning to use language imitatively.



An early study by Andrew Meltzoff (Meltzoff 1988) showed that around 14 months infants spontaneously imitate the actions of others. In one experimental setup, infants looked on as the experimenter bent down and touched his head to the top of a box, which then lit up. The infants were not allowed to play with or touch the box until one week later, when they were observed to see if they imitated the unusual action that they had previously seen. Despite the rather odd nature of the action, they did imitate what they had seen, touching their heads to the box as the experimenter had done. They did this even though it would have been easier for them to emulate rather than imitate the adult's action - for example, by pressing on the box with their hands. Meltzoff's study doesn't yet show that the infants who imitated the experimenter's head-touching action were also seeking to reproduce the intention with which he acted. They may simply have been mimicking him - that is, reproducing the experimenter's movements without doing so as the means to any end. However, a further study by Carpenter et al. (Carpenter, Nagell & Tomasello 1998; described in Tomasello & Carpenter 2005) rules this conclusion out. Here Meltzoff's study was repeated on infants from 9-15 months. However, the illumination of the light when the box was touched was delayed. It was hypothesised that infants would look to the light expectantly if they had acted with the intention of turning the light on by touching their forehead to the box. From around 12 months infants did just this. When subsequently the light did not come on, they looked to the adults in a puzzled manner. In the words of Tomasello and Carpenter:

This shows that the infants were adopting the adult's means in order to achieve the same goal as the adult - to turn on the light. The infants were not just mimicking the adult's action but were instead engaging in imitative learning of her novel action.

(Tomasello & Carpenter 2005, p.138)

Additionally, Gergely & Csibra (2005, p.475) report a trial that they ran in which the adult's head did not touch the light switch, so that his action produced no discernible effect and appeared not to be purposive. Here, only 7% of infants imitated his action. This supports the conclusion that infants copy others' actions in order to reproduce their perceived goals.

Not only do infants imitate, further evidence also shows that they have a quite sophisticated grasp of which actions should and should not be imitated. A study by Carpenter et al. (Carpenter, Akhtar & Tomasello 1998; discussed in Tomasello & Carpenter 2005) showed that infants of 14- to 18- months reliably discriminate intentional and unintentional actions - that is, those actions in which the goal of the action is achieved, and those in which it is not. Where actions performed accidentally were flagged by the experimenter's saying "Oops!" during the performance and actions performed successfully were marked as intentional by an exclamation of "There!", infants reproduced the latter actions more often than the former. A natural way to interpret the infants' behaviour here is that they took the experimenter's utterance of "Oops!" to indicate that her action had not been intentional, and took this to be a reason not to imitate the performance just witnessed. A grasp of which actions are and are not intentional would help to explain the empirical fact, discussed in previous chapters, that infants rarely make mistakes even in their earliest uses of language. For example, it may be that one reason why infants don't imitate coughs in the way that they do language is because they know that coughs are not intentional acts.

This conclusion is supported by the results of another follow-up to Meltzoff's study, which provides further evidence of the selective nature the imitative activities of infants as young as 14 months. A study by Gergely, Bekkering & Király (2002) found that infants who observed the experimenter turn on the lamp with his forehead were less likely to imitate this action if they observed that his ability to perform the task was inhibited. When the experimenter performed his task with his hands wrapped in a blanket, only 21% of children imitated him by turning on the lamp using their head. In this condition, they were far more likely to emulate his action, by turning on the light with their hands. By contrast, they imitated him 69% of the time when the experimenter's hands were free. The natural way to interpret this evidence is that in this situation the infants judged that the adult had used his

head only because his hands were not free and, since their hands were free, grasped that they lacked a reason not to employ a different (more comfortable) means to achieve the same end. This shows that imitation is guided by the infant's knowledge of the situational constraints on the actor's performance of an action.

A final finding from the series of studies overseen by György Gergely and Gergely Csibra (and reported in Gergely & Csibra 2005) concerns the operation of a further constraint on infants' imitative activities - whether or not an actor's action is accompanied by signs of communicative intent. In Chapter Four it was argued that infants reproduce others' speech acts and not their coughs because they know that the former and not the latter are produced with communicative intent. It was argued that some grasp of communicative intent is necessary for language acquisition, although it was left open whether or not this grasp consists in a grasp of an intention like that in Grice's clause (2), or merely a causal sensitivity to the presence of communicative cues. In fact, there is evidence that infants look to the presence of signs of communicative intent in order to determine which actions should and should not be imitated. In modified version of the Gergely, Bekkering & Király study, half of the infants tested observed the experimenter interact with the box in both hands-free and hands-occupied conditions, while the experimenter provided the infant with explicit signs of communicative intent like those described in Chapter Four - eye contact, pointing and the use of the infant's name, for example. The other half of the infants observed the experimenter perform the action in the same conditions, but without the experimenter's attempting to engage with them communicatively. Where communicative intent had been manifested by the experimenter, infants imitated the head-touching acting in the hands-free condition significantly more than when indicators of communicative intent were not provided. This doesn't settle the issue of how Grice's second clause should be interpreted, but it does suggest that this clause of Grice's analysis picks out a property the presence of which determines whether or not infants grasp that a particular action should be imitated.

Together these results suggest that infants are competent and flexible imitators of others' actions before they start to use and understand others' uses of language. At the age of 14 months, they are able to identify the goals in light of which their caregivers act and to reproduce the means that they perceive these caregivers employ in pursuit of their intended goal. Furthermore, infants are able to imitate others' activities discriminatively. These imitative abilities make it plausible that they come to be able to exploit the existence of conventional features of language because they grasp perlocutionary effects to which speakers use these utterances as means, and imitate them.

#### Concluding remarks

In this chapter I have started to defend the claim that knowledge of linguistic conventions could be acquired through imitation - the reproduction of both the means and the end of another's action. Since infants of 12-months are capable imitators of others' activities, it's possible that they learn how to use words and sentences by grasping that speakers utter words and sentences as the means to the achievement of their perlocutionary intentions, which infants are also able to grasp. Additionally, infants might grasp that the same words and sentence can be uttered as means to the achievement of their own perlocutionary intentions. By reproducing the utterances that they have heard others perform, they would then learn to use words to fulfil their communicative goals.

Being able to use words in this manner is, I have suggested, sufficient for participating in linguistic conventions. Although one who participated in a linguistic convention in this manner need not know that their uses of words satisfied what Lewis called the "defining conditions of conventionality", I have argued - against Lewis - that it is not a requirement of participating in conventional activity that one possesses such knowledge. In particular, it's not necessary for participating in a convention that one knows that one's activity is one of potentially many different solutions to a coordination problem.

I have not claimed that imitative abilities would be sufficient to explain an infant's acquiring full competence with language. The story set out here concerns the imitative reproduction of heard speech acts in the context of purposive activity. However, speakers do not only utter speech acts that they have previously heard others utter. Language is productive: speakers also create new speech acts - for example, by recombining the elements of previously encountered sentences to form new sentences. In this case, it may be that imitative abilities are not sufficient to explain full competence with language. The claim here is only that imitation could plausibly explain infants' early acquisition of the use of linguistic conventions in communicative interaction.

## Concluding Remarks: The Infant's Understanding of Other Minds

I want to conclude this thesis in part by summarising its central claims, and in part by drawing together, if only very briefly, some strands of the material that would benefit from further discussion. I do this in order to make explicit some commitments that have perhaps, until this point, been only implicit. I start, though, by recapping.

The question posed at the outset was: what are the cognitive abilities that must be attributed to infants around the age of 14-months to explain their ability to use and to understand others' uses of words in communicative interactions?

In the Chapter Two I considered the fact that in order to use and understand words, one must be able to grasp the referents of those words - that is, the objects for which they stand. According to associationist accounts of learning, an infant learns the referent of a word when it hears that word while attending to an object, and comes to associate the sound that it hears with the object of its attention. No cognitive abilities beyond associative

mechanisms are said to be necessary for associationist learning. However, I argued that associationist accounts of learning the referent of a word must overcome some very substantial difficulties if they are to be credible. This is because infants don't just hear the names of objects to which they are attending; they often hear words that have nothing to do with the objects of their attention. If infants learned words just by associating them with the objects of their attention, then one would expect them to make characteristic mistakes in their learning. However, they make such mistakes only rarely. Furthermore, since infants are very good at learning the names of objects even in circumstances where existing accounts of associative learning could not explain their word-learning, we have good grounds for thinking that more sophisticated abilities are in play.

In Chapter Three I considered Paul Bloom's claim that infants learn the referents of words because they grasp speakers' 'referential intentions'. Although sympathetic to this claim, I argued that Bloom lacks an account of the cognitive abilities that one would need in order to grasp others' referential intentions. Furthermore, I argued that although grasping a speaker's referential intention would typically be necessary for grasping her communicative uses of words, it would not normally be sufficient - since words with the same referent could be uttered as a means of communicating very different messages. In order to understand others' uses of words in communicative interaction, one must be able to be grasp the perlocutionary intentions with which speakers utter.

In Chapter Four I argued that Bloom's idea of referential intentions should be replaced with a richer notion of communicative intentions, where these are here understood as perlocutionary intentions. I developed an account of what it is to act with or grasp a communicative intention, and of the cognitive abilities that this would require, with reference to Paul Grice's account of speaker meaning. With reference to recent empirical studies of infant pointing, I argued that infants of 14-months possess the cognitive abilities that are necessary for both having and grasping communicative intentions with a revised

Gricean structure.

In Chapter Five, I considered two of John Searle's objections to the Gricean account of communicative intentions that I developed in the previous chapter. Against Grice, Searle argues that communicative intentions are not perlocutionary intentions - that is, intentions to make others do or think something - but illocutionary intentions. These are intentions to be understood as having performed a speech act with a certain illocutionary force, and are (according to Searle) characterised by an intentional structure different from the account defended in Chapter Four. Furthermore, Searle argued, Grice's account of speaker meaning fails to account for the role of linguistic conventions in communication. Against Searle, and with reference to some recent empirical studies, I argued that there is insufficient evidence to attribute to young infants the ability to act with or grasp illocutionary intentions. Furthermore, I argued that while the Gricean account might fail to account for the role of linguistic conventions in communicative interactions in which speaker and hearer possess mutual knowledge of linguistic conventions, it might still be the right account for characterising infants' earliest communicative interactions, in which this mutual knowledge is lacking.

Finally, in Chapter Six, I considered the nature of the cognitive abilities that would be necessary for learning linguistic conventions - at least some knowledge of which is necessary for being a user of language. I argued that infants could learn linguistic conventions in virtue of possessing imitative abilities. Having identified the perlocutionary intentions with which others utter, and the words that they utter as a means to the achievement of those perlocutionary ends, infants might reproduce the words that they have heard others utter in pursuit of their own perlocutionary goals.

Over the course of the thesis, two abilities in particular have emerged as necessary for learning to use and understand others' uses of words in communicative interaction. In order



to grasp others' uses of words, infants must grasp the perlocutionary intentions with which they utter and in order to use words themselves, they must be able to use at least some words and sentences to communicate their own perlocutionary intentions. (Something like knowing how to use words in this manner is presumably necessary for knowing the meanings of those words and sentences, but I do not claim that it is sufficient.)

At the heart of the thesis lies the claim that infants are able to learn to use words because prior to their first uses of language, they already possess some insight into the minds of others. Independently of knowing the meanings of words, on at least some occasions infants grasp that others are trying to communicate, and what it is that they are trying to communicate. This view of the infant's understanding of other minds contrasts with one that would make our understanding of other minds a consequence of our knowledge of language. In the opening sections of Chapter Two, I quoted Daniel Dennett on the subject of human intersubjectivity:

We human beings share a subjective world - and know that we do - in a way that is entirely beyond any other creatures on the planet, *because we can talk to one another*. ... Conversation unites us. (*KoM*, p.12; my italics)

A natural way to construe this comment is as saying that the rich intersubjectivity of human life - the deep knowledge that we have of one another's minds - is to be explained by our ability to talk to one another. It's because we engage one another in conversation, and thereby share our thoughts and feelings, that we come to share in one another's mental lives in a way that other animals - even our nearest chimp relatives - do not. The implication seems to be that the world of the pre-linguistic infant, in which knowledge of language is absent, would not be a shared subjective world - not, at least, in the way that the adult world is.

Dennett's claim may not be that prior to their acquisition of language, infants have no insight into the mental lives of others at all. However, the account of language-acquisition

that he proposes does not presuppose any such insight. He sketches out an account of how an infant could learn words with which to communicate without its having any knowledge of what its caregivers are trying to achieve by speaking, in virtue of statistical correlations between the utterances of words and the objects of an infant's attention. As became apparent in Chapter Two, though, this acquisition story is fraught with difficulties. It struggles even to explain how infants could learn the referents of the words that they hear others use. The problems with this view motivate trying to explain the infant's learning to do things with words by attributing to it a richer set of cognitive abilities - in the form of an understanding of something of the mental lives of others - at the outset.

To claim that even prior to their first uses of words infants already have some understanding of the mental lives of others is not to deny that our ability to use language to communicate enriches our knowledge of our interlocutors' minds in important ways. However, it is to suggest that language is not the foundation of our intersubjectivity. Rather, linguistic communication builds on a foundation of intersubjectivity that exists prior to and makes possible our learning to do things with words. But what is the nature of this intersubjectivity? What sort of understanding of others' minds must be attributed to infants in order to make sense of the possibility of their learning to do things with words?

One question that arose in Chapters Three and Four concerned whether infants possess a 'theory of mind' prior to their acquisition of language. Whether or not they do all depends, of course, on what is meant by this phrase. Certainly it has not been claimed that infants' understanding of minds consists in their literal knowledge of a theory, in virtue of knowing which they would explain others' behaviour by attributing to them conjunctions of rationally coordinated beliefs and desires, in the manner claimed by Gopnik and Meltzoff (Gopnik & Meltzoff 1997, Gopnik, Kuhl & Meltzoff 2001) and discussed briefly in Chapter Two. Furthermore, it has nowhere been claimed that infants must possess a concept of belief, requiring knowledge of the possibility of false belief, in order to learn to do things with

words.<sup>37</sup> It may be that infants of fourteen months do possess such knowledge, but the cognitive abilities that I've argued are necessary for their learning to do things with words do not include this knowledge. An infant need not be able to pass a false belief test in order to learn to use words in communicative interaction. A third construal of the phrase 'theory of mind' is employed by Paul Bloom. As he uses the term, possessing a 'theory of mind' amounts just to being able to grasp others' referential intentions. On the account that I have defended, this ability is both necessary for learning to do things with words, and possessed by infants of 14-months. However, of the three uses of the phrase discussed in Chapter Three, Bloom's is the least common.

Rather than looking for a catch-all and potentially misleading phrase by which to characterise infants' understanding of other minds, it might be more fruitful just to say what their understanding of other minds consists in. The claim that I have made is that on at least some occasions, infants know when others are trying to communicate with them, and what it is that they are trying to communicate. Not only do they understand others' referential intentions; additionally, they grasp others' perlocutionary intentions - what it is that their interlocutors, by speaking, would have them attend to, think, or do (and so on). This is both necessary for learning to do things with words and something that infants can do. The empirical claim here is supported by a substantial body of empirical work, discussed

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<sup>37</sup> Here there exists a parallel between the position that I defend and a recent, as yet unpublished, claim developed by Ruth Garrett Millikan. Millikan writes that:

[N]o representational theory of mind is needed to understand that another's speech is purposefully produced and directed towards some effect, and to attend to what that effect might be, for example, to what part of the world a speaker is purposefully exhibiting to the child using words[.] (Millikan, unpublished talk, p.4)

By 'representational theory of mind', Millikan means a conception of the mind that requires a concept of belief. In future work it would be worthwhile to develop the comparison of my own account with Millikan's - but I will not do that here.

in Chapter Four, which demonstrates that from around 12-months infants both point with and grasp others' pointing with a variety of different perlocutionary intentions - for example, to share information, or attention, or to make requests and give orders. None of these perlocutionary intentions have contents that must be specified by appeal to a concept of belief.

Additionally, at the age of 14-months, infants possess imitative abilities. This makes it possible to explain their learning of words in virtue of their being able to imitate the communicative actions of their peers. The appeal to imitation highlights a further difference between the account that I have elaborated and the account attributed to Dennett in Chapter Two. On Dennett's account, infants repeat to themselves the words that they have heard others speak - but they do so blindly, without any reference to the purposes with which those words were uttered by their speaker. Recalling a distinction made by Tomasello and Carpenter (Tomasello & Carpenter 2005) and introduced in Chapter Six, on the Dennettian account infants merely *mimic* the words that they hear. I have argued, by contrast, that key to using words in communicative interaction is understanding the role of those words in the purposive activities of their speakers. On the story that I have sketched out, this understanding is built in at the start: infants don't merely mimic others' utterances; they imitate them - reproducing the words that they hear in pursuit of the perlocutionary intentions to which those words can be used as a means.

## Appendix: An a priori Objection to Associationism?

Is there a more principled argument against associationism?

The argument offered against associationism in chapter two took as its starting point the fact that existing associationist accounts of learning cannot explain particular empirical facts about the conditions in which young infants are able to learn the referents of others' words. Although this argument suggests that associationism is not the correct answer to the question posed at the outset, in principle it remains possible that there *could* be an associationist account of how infants learn the referents of others' words. No refutation of associationism has been offered. I want to finish by considering the possibility of a principled objection to the associationist account of learning to do things with words. This argument would not turn upon empirical arguments for the ascription of particular cognitive abilities to infants, and of possible associationist explanations of these abilities, but on purely philosophical considerations about the nature of language-use. In this case, the argument would, if successful, constitute an *a priori* rejection of the possibility of an associationist account of acquisition. It is a style of argument that might be particularly tempting to philosophers. Indeed, it takes its lead from objections to dispositional accounts

of meaning and understanding that can be traced back to Wittgenstein (*PI* §§138-242, especially §§193-195) and Kripke (1982, pp.22-37), and which have been developed more recently by Robert Brandom (1994, chapter 1), to name but a few. Although I'm broadly sympathetic to this *a priori* style of argument, in fact, I think it's a difficult argument to make work in developmental cases. Brief reflection should show why this is the case.

The argument starts by pointing out that, against the associationist, linguistic utterances (typically of indicative sentences uttered with assertoric force) are not merely responses to causal stimuli but judgements about how the world is. As such they can be true or false, and justified or unjustified. This motivates the following objection to the associationist: any account of how we learn to use words must be able to show how our mature uses of language can be judgements about the truth of certain states of affairs, as distinct from mere responses to causal stimuli. However, the associationist account fails to deliver such an output. On the associationist account proposed by Dennett, learning to use words consists just in acquiring dispositions to respond to environmental stimuli: understanding a sentence consists just in being caused to respond to the presence of associated stimuli in certain ways, and producing an utterance consists just in being caused to emit certain sounds by the presence of their associated stimuli. On this account, making an (assertoric) utterance is not a case of judgement about how the world is but a simple causal mechanism. Since the associationist account fails to explain how it can be true that our uses of language constitute judgements, it is demonstrably false.

Such a line of argument is, I suggest, almost certainly correct as an objection to dispositionalist accounts of the use and understanding of language, but it's not yet a refutation of associationist accounts of acquisition. This is because the associationist about learning to use words could deny that this commits him to being a dispositionalist about language-use. If the associationist makes this move, then the judgements-not-dispositions story about language-use and understanding cannot obviously be divorced from questions

about whether infants' uses of language are really like our own, or just resemble them in superficial ways, and the argument against associationism cannot be run *a priori*. The associationist can simply ask: how do you know that infants' uses of language are really instances of judgement? Couldn't this be something that one acquires only later - perhaps only once one has acquired the rudiments of thought that come on the associationist model? When Dennett contrasts "mock exhortation, mock prohibition, mock praise [and] mock description" with the acts of "real exhortation, prohibition, praise and description" (KOM, p.197) into which they eventually mature, it may be this distinction between judgemental and merely responsive uses of language that he has in mind.

A first response here would be to argue, against the associationist, that infants' uses of words are judgement-like and not merely responses to stimuli. In making this move, one would give up the claim that this objection to associationism is *a priori* and so apt to be run independently of any appeal to empirical facts about infants' use and understanding of language. Consequently, this response wouldn't necessarily be more decisive (because more principled) than the empirically-motivated rejection of associationism for which I argued at length in Chapter One. Nonetheless, if someone else were to marshal empirical evidence that infants' uses of language constitute judgements, this would certainly constitute valuable evidence against the associationist account of language acquisition. Since I don't know what such evidence would look like, I haven't attempted to argue along these lines here.

A second response to the associationist's defence would be to point out that, if word-use is like judgement, and if associations acquired in the infant's early years are not, then the associationist still hasn't provided us with a developmental story that ends with an account of how infants acquire the ability to use and understand words. In this case, he's failed by his own lights, by not explaining what he set out to explain and, as before, we are justified in looking to alternative answers to the question posed at the outset. Additionally, provided

he accepts that mature uses of words are not like dispositions, the associationist must surely also concede that he's provided only part of the story about how infants learn to do things with words. It now becomes incumbent upon the proponent of associationism to say more about how a faux-language grounded in stimulus-response mechanisms could ever mature into an ability to use words to express judgements. An explanation along these lines would certainly be necessary if his account were to be made plausible. However, it's not a priori obvious that an account of this maturation couldn't be given.

What such a maturation story would look like is so far beyond my ken that I couldn't credibly try to construct an answer to this challenge on the associationist's behalf. Consequently, I haven't tried to develop this line of argument here. Nonetheless, the challenge to the associationist remains. If associationism is to be made credible as an account of even part of the process of infants' learning to do things with words, then at some point the associationist must either deny that mature uses of words are cases of judgement, and not merely the operations of a stimulus-response mechanism, or provide us with a credible story that explains how it is that stimulus-response mechanisms could mature into mechanisms that deliver judgements. In the meantime, and even in the absence of a more principled objection to the associationist account, I suggest that by considering the inability of existing associationist accounts to explain simple facts about the conditions in which infants are able to learn the referents of others' words, enough has already been done to motivate looking elsewhere for an account of the cognitive abilities that must be attributed to infants to explain their learning to do things with words.



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