Why are there limits on theory of mind use? Evidence from adults' ability to follow instructions from an ignorant speaker

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Keysar et al. (Keysar, Barr, Balin, & Brauner, 2000; Keysar, Lin, & Barr, 2003) report that adults frequently failed to use their conceptual competence for theory of mind (ToM) in an online communication game where they needed to take account of a speaker's perspective. The current research reports 3 experiments investigating the cognitive processes contributing to adults' errors. In Experiments 1 and 2 the frequency of adults' failure to use ToM was unaffected by perspective switching. In Experiment 3 adults made more errors when interpreting instructions according to the speaker's perspective than according to an arbitrary rule. We suggest that adults are efficient at switching perspectives, but that actually using what another person knows to interpret what they say is relatively inefficient, giving rise to egocentric errors during communication.

Keywords: Theory of mind; Adult; Cognition; Executive function; False belief.

Theory of mind (ToM)—the everyday ability to reason about agents in terms of mental states such as beliefs, desires, and intentions—has been the subject of much research in developmental psychology (e.g., Doherty, 2008) and, more recently, in neuroscience (see e.g., Frith & Frith, 2003). Developmental research suggests that many basic abilities are present by 4 years (e.g., Wellman, Cross, & Watson, 2001), and perhaps much younger (Onishi & Baillargeon, 2005; Surian, Caldi, & Sperber, 2007). However, relatively little research has been conducted on older children, and only recently has ToM in adults begun to receive sustained empirical attention (see Apperly, Samson, & Humphreys, 2009, for a recent review). This work suggests that ToM

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in adults depends, to a significant degree, on cognitive resources for executive control. For example, inferences about an agent's belief may not be made automatically (Apperly, Riggs, Simpson, Chiavarino, & Samson, 2006), and holding in mind information about someone's false belief carries a processing cost for judgements about that belief or the corresponding real situation (Apperly, Back, Samson, & France, 2008). Performance on ToM tasks is disrupted by the simultaneous performance of an executive task (Bull, Phillips, & Conway, 2008; McKinnon & Moscovitch, 2007), and individual differences in performance on a belief–desire reasoning task correlate with individual differences in general processing speed and executive function (German & Hehman, 2006). Moreover, impairment of executive function may lead to a strong tendency for egocentric errors on ToM tasks (Samson, Apperly, Kathirgamanathan, & Humphreys, 2005). Finally, even when given the opportunity to infer another person's belief or knowledge in advance, adults are prone to error when *using* this information when following instructions from that person (e.g., Keysar, Barr, Balin, & Brauner, 2000; Keysar, Lin, & Barr, 2003). The current studies investigated this difficulty with ToM use and examined factors that might give rise to these effects.

Keysar et al. (2000, 2003) used a task in which participants viewed a 4 × 4 vertical grid containing objects in different slots. Participants were instructed by a "director" to move objects around the grid. The participant's view of this experimental set-up is depicted schematically in Figure 1a, where the director is the male figure on the opposite side of the grid. As can be seen in Figure 1a, some of the slots in the grid allowed both the participant and the director to see any object inside. Other slots were closed on the director's side, creating a perspective difference between the participant (who could see the objects in these slots) and the director (who could not see into these slots and did not know what was inside them). Critical instructions required participants to take account of the director's perspective in order to identify the object to which he was referring. For example, when the director said "move the small

ball one slot down", he was referring to Object x in Figure 1a, which is the smaller of the two balls that he could see. He could not be referring to Object y, the smallest ball in the grid, because he could not see that ball and did not know it existed. Thus, when following the director's instruction, participants had to ignore objects such as Object y, which were valid referents from their own perspective, but not from the perspective of the director. Keysar et al. (2000, 2003) found that adult participants frequently failed to take the director's perspective into account. For example, in Keysar et al. (2003) 71% of participants incorrectly selected an object about which the director was ignorant to at least one of four critical instructions; 46% did so on two or more occasions.

Several potential explanations for this surprising finding can be ruled out. First, adults' difficulty
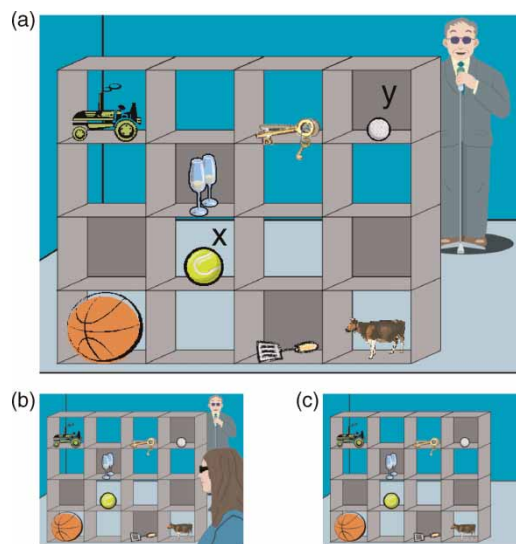
Figure 1. *Examples of stimulus items from an experimental relational trial from each experiment. Figure 1a represents the "standard" condition similar to Keysar et al. (2000) and as used in the ToM-use condition of Experiment 3 of the current study. For the instruction "Move the small ball one slot down", Object x is the correct referent, and Object y is the distractor. A matched control stimulus is formed by swapping Object y for a different object (e.g., an aeroplane). Figure 1b represents stimuli with an informed as well as an ignorant director, as used in Experiments 1, 2a, and 2b. Figure 1c represents a stimulus from the non-ToM condition of Experiment 3, in which there was no director. To view a colour version of this figure, please see the online issue of the Journal.*

was not a lack of the requisite concepts for perspective taking. The task described above requires conceptually simple visual perspective-taking skills that are present by 2 years of age (Flavell, Everett, Croft, & Flavell, 1981). Second, it seems unlikely that participants failed to notice the different perspective of the director. This perspective difference was emphasized at the beginning of the experiment by having participants walk around to the other side of the apparatus so that they experienced for themselves how the grid appeared from the director's point of view. Third, it seems unlikely that participants thought the director "must know" what was in the occluded slots of the grid, even if he could not see. In the experiments by Keysar et al. (2000, 2003), participants believed that the director was a second naïve participant (she or he was in fact a confederate), and in some conditions participants were actively engaged in hiding objects in the grid, clearly emphasizing the fact that the director did not know about the critical objects. So it appears that participants had the means and the motivation to appreciate that the director had a different perspective from them. What Keysar et al.'s findings appear to show is that, in spite of this, participants did not *use* these theory of mind abilities reliably when interpreting the director's instructions. Participants' errors suggest that they did not consistently restrict the domain of potential reference to the "common ground" of objects mutually known by them and by the speaker (e.g., Clark & Marshall, 1981). This conclusion is reinforced by the observation that participants' eye movements frequently fixated potential referents that the speaker could not see, even if participants ultimately responded correctly by selecting the correct referent from common ground.

It is important to be clear that the paradigms used by Keysar et al. (2000, 2003) may overestimate the tendency for error in adults' everyday communication. Natural communication often gives listeners valuable cues to reference resolution, such as the eye gaze of the speaker (e.g., Hanna & Brennan, 2007), which were not present in Keysar et al.'s paradigms. Equally, however, it is clear that if we wish to study adults' ability to use

inferences about what the speaker thinks or knows to interpret what the speaker says, it is necessary to eliminate from the experimental paradigm other cues (such as eye gaze) present in natural discourse that might allow the participant to assign reference correctly without thinking about the speaker's mental states. For these purposes Keysar et al.'s paradigm is particularly useful.

Keysar et al.'s (2000, 2003) findings suggest that making a ToM inference (about what someone else sees, thinks, or knows) is no guarantee that this information will be used to guide communication. These results fit with other suggestions that adult perspective taking is subject to egocentric or "reality" bias (e.g., Birch & Bloom, 2004, 2007; Epley, Keysar, Van Boven, & Gilovich, 2004a; Mitchell, Robinson, Isaacs, & Nye, 1996). However, the findings of Keysar and colleagues are surprising because research on everyday communication (e.g., Clark & Marshall, 1981) and conversational pragmatics (e.g., Sperber & Wilson, 1995, 2002) has pointed out that successful communication appears to depend upon speakers and listeners taking account of one another's knowledge, beliefs, and intentions. This research assumes either implicitly or explicitly that such inference and use of information about mental states occurs quickly and efficiently, whereas the studies of Keysar and colleagues suggest that this may not be a safe assumption.

Other empirical findings do indicate that ToM use may sometimes be more efficient than Keysar et al. (2000, 2003) have suggested. For example, by using a much simpler task (only four slots in the grid, with only one slot occluded) Nadig and Sedivy (2002) found evidence that 5- and 6-year-olds behaved nonegocentrically by substantially constraining their eye fixations to common ground objects. In eye-tracking studies of adults' online communication, Hanna, Tannenhaus, and Trueswell (2003) found that adults were more likely to fixate potential referents in common ground (that both they and the speaker could see) than referents in privileged ground (that only they could see), though privileged ground items still had a distracting effect on performance,

suggesting that egocentrism was not entirely overcome (see also Barr, 2008). Other evidence comes from a cross-cultural study, in which Wu and Keysar (2007) found that Chinese participants did not show the errors made by American participants on the referential communication task. The authors argued that a culture that emphasizes interdependence among individuals (Chinese) can reduce the likelihood of egocentric errors in ToM use. These findings clearly suggest that ToM use is not always subject to egocentric errors, but they do not explain why.

In short, the presence of errors of ToM use shows that using ToM abilities in communication or the following of instructions requires much more than just having the requisite ToM concepts. As already mentioned, the relevant conceptual abilities for the visual perspective taking required by Keysar et al.'s (2000, 2003) communication tasks are in place by early childhood. Difficulty with ToM use is often manifest in egocentric errors. However, while "egocentrism" may be an informative description of the observed phenomenon, it provides little insight into the underlying cognitive processes that give rise to this phenomenon. Investigating why errors of ToM use arise in adults offers one way to shed light on the nature of these additional processes.

Why, then, might the use of ToM information to guide communication often be so demanding? In the current work, we explored two possible explanations. One possibility is that ToM use is demanding because it requires *switching* between self-perspective and the perspectives of others, flexibly and appropriately. It has been suggested that participants' default is to interpret language from their own self-perspective and only take account of the speaker's perspective by an effortful process of adjustment (e.g., Epley et al., 2004a). Moreover, everyday conversation not only requires the ability to overcome any bias towards self-perspective, but frequently requires coordination with multiple speakers who will, inevitably, have perspectives that differ from self-perspective and from each other. In referential communication tasks such as those of Keysar et al. (2000, 2003), this leads to the expectation that participants

might find it easier to take account of a director's perspective on trial N if they had received an instruction from the same director on trial $N - 1$ than if they had received an instruction from a different director on trial $N - 1$. Therefore, in our first two experiments we varied the demands on perspective switching in a ToM-use task based upon Keysar et al.'s paradigm but adapted to have two directors with different perspectives.

A second possibility is that it is demanding to use ToM to guide communication because this requires us to *infer, hold in mind*, and *use* information about someone else's perspective that may be in conflict with our own perspective. Existing evidence suggests that adults do indeed show processing costs for inferring (Apperly et al., 2006) and holding in mind (Apperly et al., 2008) information about other people's beliefs. In Experiment 3 we tested whether this was also a significant demand for ToM use. To evaluate whether ToM use is relatively costly or relatively efficient we compared a ToM-use condition with a matched non-ToM condition. In the non-ToM condition participants heard the same instructions and viewed the same objects as those in the ToM-use condition and had to constrain their interpretation of the instructions to the same subset of objects as in the ToM-use condition, but there was no perspective-taking requirement. If ToM use makes cognitive demands over and above the general demands of following the verbal instructions, then performance in the ToM-use condition should be poorer than that in the non-ToM condition. In contrast, if ToM use is a privileged or specialized cognitive process, as has sometimes been suggested (e.g., Clark & Marshall, 1981; Sperber & Wilson, 1995, 2002), then performance in the ToM-use condition should be better than that in the non-ToM condition.

EXPERIMENTS 1–2: PERSPECTIVE SWITCHING

We adapted Keysar et al.'s (2000, 2003) task so that instructions could be presented from two

directors. As in Keysar et al.'s tasks, one director could see some but not all of the objects visible to participants. Although this director's ignorance was only partial, we refer to him henceforth simply as the "ignorant director". A second director shared the participants' visual perspective, and we refer to her as the "informed director".

General method

Participants
A total of 20 participants (7 males) took part in Experiment 1, 20 (3 males) in Experiment 2a, and 20 (5 males) in Experiment 2b. All were undergraduate students ranging in age from 18–31 years. All received course credits or a small cash honorarium.

Design and materials
We presented Keysar et al.'s (2000) task on a computer. Stimuli consisted of a 4 × 4 grid, with cartoon figures representing the directors (see Figure 1b; note that Figure 1 presents actual stimuli from the current experiments) and prerecorded sound files for the directors' instructions. The participant followed the directors' instructions by clicking on the appropriate object with a computer mouse and moving the mouse cursor to the appropriate slot (the object itself did not move). This enabled us to measure accuracy of responding and gave an estimate of response time from the onset of each instruction.[1]

Experimental stimuli consisted of 32 different grid arrays, each with 16 slots. For each grid, 5 slots were occluded from the point of view of the ignorant director (a male figure, with a male voice) but visible to the participant and the informed director (a female figure, with a female voice). The remaining 11 slots were visible to the participant and both directors. The occluded slots were arranged in one of four different patterns. Each grid contained

eight items, depicted by simple cartoon images. The number of instructions for each grid varied between 3 and 5, with 128 instructions in total. There were also 2 practice grids generated to the same specifications. No instructions in the practice grids required participants to take account of the ignorant director's perspective.

The 16 grids of the experimental condition each included one "critical instruction" (e.g., "move the small ball one slot down"). If interpreted from the participant's perspective or that of the informed director, the instruction would refer to an item in an occluded slot (e.g., the smallest ball), but if interpreted from the ignorant director's perspective it would refer to an item in an open slot (e.g., the midsized ball—see Figure 1). The position of this critical instruction varied, between first and fourth in the sequence for each grid. All other instructions were filler items, using the same verbal formulation as the critical instructions, but always referring to items that could be seen by both director and participant.

There were two types of experimental trial. In 8 *relational* experimental trials the critical instruction defined its target object by relative size or position (e.g., "Move the *small* ball one slot left"). From either perspective these instructions uniquely identified a single object (they were not ambiguous), but a different object was identified depending on the perspective taken. From a participant's perspective (and that of the informed director) "the small ball" unambiguously referred to the smallest of three viewable balls. However, the smallest ball was not viewable by the ignorant director, and so, if spoken by the ignorant director, this instruction unambiguously referred to the midsized ball that everyone could see. These relational trials were directly analogous to those used by Keysar et al. (2000, 2003).[2] All relational instructions followed the formula "Move the [adjective noun] one slot

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[1] We only considered this measure an estimate of response time because it included time taken to identify the correct referent and execute the motor response of clicking the mouse on the referent. Nonetheless, this measure does afford a check that participants are not merely trading off accuracy against speed.

[2] Confusingly, the illustrative example used in the text of Keysar et al. (2003) is actually ambiguous rather than relational, but the stimuli used in these experiments and in Keysar et al. (2000) were all relational.

left/right/up/down", and offset of the noun occurred on average 1,047 ms after the start of the instruction. Participants were told that "left" and "right" corresponded to their own left and right sides.

In 8 *ambiguous* experimental trials, the critical instruction defined its target with a simple noun phrase (e.g., "the mouse"). From a participant's perspective (and that of the informed director) such instructions were ambiguous because two different types of mouse (a computer mouse and cartoon animal) were visible. However, only one mouse could be seen from the ignorant director's perspective, so when the instructions were spoken by the ignorant director, the participant could always identify a unique intended referent. Trials of this kind were used by Nadig and Sedivy (2002) but not by Keysar et al. (2000, 2003), who reasoned that the ambiguity of the instruction might prompt participants who had initially failed to take the perspective of the director to repair their interpretation of the instruction, so obscuring any failure of ToM use. We reasoned that such a repair strategy should be detectable in longer response times and so included ambiguous trials as an interesting test of participants' ability to repair their interpretation of instructions. All ambiguous instructions followed the formula "Move the [noun] one slot left/right/up/down", and offset of the noun occurred on average 724 ms after the start of the instruction.

To create the 16 grids of the control condition, the critical instruction's potential referent object in the occluded slot (e.g., the smallest ball) was replaced with an item that could not be a potential referent (e.g., a duck). Each control grid used the same sequence of instructions as the corresponding experimental grid. No more than 2 experimental or control grids were presented in a row, and consecutive grids always had different patterns of occluded slots. Matched experimental and control grids were always separated by 8 or more other grids, and each experimental grid occurred equally often before and after its matched control. Half of the participants were presented with one fixed sequence of grids, the other with the reverse sequence.

Each new grid appeared for 5,000 ms of study time (so that participants could infer the ignorant director's view of the grid) before the verbal instructions began. Verbal instructions for a given grid were then presented at 5,000-ms intervals. The entire experiment was run in a single block lasting approximately 20 minutes. The experiment was run on a standard desktop PC using E-prime experimental software.

EXPERIMENT 1

Method

In Experiment 1, 50% of all instructions were given by the ignorant director and 50% by the informed director. The critical (experimental and ambiguous) instructions were always spoken by the ignorant director. These critical instructions were further divided into "switch" and "no-switch" trials. On "switch" trials (50% of total critical trials) the filler instruction immediately preceding the critical instruction was spoken by the informed director. On "no-switch" trials the filler instruction immediately preceding the critical instruction was spoken by the ignorant director.

Procedure

Participants were shown an example grid. They were told that the informed director could see all the items in the grid because she was on the same side of the grid as them. They were told that the ignorant director could not see what was in the covered slots, and to emphasize the difference in perspective, participants were shown the same grid from the ignorant director's perspective. It was emphasized to participants that the ignorant director did not know what was behind the covered slots and that it would be important to take the directors' points of view into account when following their instructions. Participants were asked to respond as quickly and accurately as possible. They then completed two practice grids before proceeding to the main experiment.

Results and discussion

Our primary interest was in participants' error rate because our measure of response time lacked the fine-grained temporal resolution of the eye-tracking data of Keysar et al. (2000, 2003) and of Nadig and Sedivy (2002). However, we did analyse response times to check for condition differences and in order to check for any signs of trade-offs in speed and accuracy.

Error analysis

For 101/114 errors (88.6%) participants selected the distractor object, for 4/114 (3.5%) participants selected a different object, and for 9/114 (7.9%) participants responded too slowly (more than 5,000 ms from the start of the director's instruction). All other experiments in the current paper also showed a similar high proportion of distractor errors, and since the other error types could not be interpreted easily, only the rate of distractor selection was counted for analysis in any experiment.

Consistent with the findings of Keysar et al. (2003), participants frequently made errors on relational experimental trials (e.g., erroneously selecting the smallest ball from privileged ground (that only they could see) in response to the instruction "move the small ball . . . "), but never

made errors on control trials where there was no potential referent in privileged ground. This difference was significant for both switch and no-switch conditions (both $ps < .001$ by sign test). However, participants were not ignoring the director's perspective in either switch or no-switch conditions: If they had, they would have made errors 100% of the time, and this was not the case (49% in switch condition, 46% in no-switch condition): one-sample t test, both $ts(19) > 5.6$, both $ps < .001$. Nor was it the case that this level of performance corresponded to some participants consistently taking the director's perspective into account while others consistently ignored the director's perspective. It is clear from Table 1 that most participants made some correct and some incorrect responses. This clearly suggests that participants understood the requirements of the task and were trying to take the director's perspective into account, but did not always manage to do so.

A similar pattern was observed for ambiguous trials. Participants sometimes selected an item from privileged ground on experimental trials, but never on control trials. This difference was significant for both switch and no-switch conditions (both $ps < .002$ by sign test). Once again, participants were not ignoring the director's perspective:

Table 1. The number of errors made by participants in the experimental conditions of Experiments 1–3

	Condition	Number of errors								
		0	1	2	3	4	5	6	7	8
Experiment 1	Relational (no-switch)	4	4	5	5	2	—	—	—	—
	Ambiguous (no-switch)	10	8	2	0	0	—	—	—	—
	Relational (switch)	7	1	2	6	4	—	—	—	—
	Ambiguous (switch)	9	9	2	0	0	—	—	—	—
Experiment 2a	Relational (ignorant instructor)	2	5	1	8	4	—	—	—	—
	Relational (informed instructor)	9	8	3	0	0	—	—	—	—
Experiment 2b	Relational (informed instructor)	6	8	4	0	0	0	1	0	0
Experiment 3	Relational (ToM use)	1	2	2	3	3	3	3	4	1
	Ambiguous (ToM use)	4	6	6	2	2	0	0	0	0
	Relational (non-ToM)	6	4	2	3	2	1	1	0	1
	Ambiguous (non-ToM)	8	7	2	2	1	0	0	0	0

Note: In Experiments 1 and 2 it was possible to make up to four incorrect responses, and in Experiment 3 it was possible to make up to eight incorrect responses.

If they had, they would have made errors 50% of the time, and this was not the case (16% in switch condition, 15% in no-switch condition) one-sample t test, both ts(19) > 9.0, both ps < .001.

To examine the effect of perspective switching we entered the percentage error on experimental trials[3] into an analysis of variance (ANOVA) with problem type (relational vs. ambiguous) and perspective switching (switch vs. no-switch) as within-subject factors. This analysis revealed a significant effect of problem type, F(1, 19) = 25.5, p < .001, with more errors on relational trials than on ambiguous trials. There was no effect of switching perspective, F(1, 19) = 0.32, p = .58, and no interaction, F(1, 19) = 0.02, p = .90.

Response time analysis

Response time was measured from the onset of each instruction, and because the instructions for ambiguous trials were shorter than instructions for relational trials, we considered data from these trials separately. Data from incorrect responses were excluded (87/320 = 27.2% of total responses). For relational trials, 12/233 (5.2%) of the remaining response times falling more than 2 standard deviations from the sample mean were excluded. The resulting loss of data (due almost exclusively to the high error rate) meant that 6 participants had no data for one or more cells of the experiment. These participants were excluded from this analysis.[4] Data were entered into an ANOVA with condition (experimental vs. control) and perspective switching (switch vs. no-switch) as within-subject factors. This analysis revealed no significant effects (all Fs < 1.43, all ps > .25), suggesting that the lower accuracy in experimental relational trials was not a result of faster responding.

For ambiguous trials 27/320 (8.4%) data points were excluded as errors, and 15/293 (5.1%) outlying data points were removed. An ANOVA with

condition (experimental vs. control) and perspective switching (switch vs. no-switch) as within-subject factors revealed a significant effect of condition, F(1, 19) = 9.08, p = .007, with faster responses on experimental trials than on control trials. This raises the possibility that the greater error rate on experimental trials may have been the result of participants trading accuracy for speed. There was also a significant effect of switching, F(1, 19) = 9.20, p = .007, with slower responses on switch trials than on no-switch trials, but the interaction between switching and condition was nonsignificant, all F(1, 19) = 2.72, ps = .115, indicating that switching did not specifically slow down the perspective taking necessary on experimental trials, but just resulted in slower responses in general.

The results from Experiment 1 are consistent with Keysar et al.'s (2000, 2003) finding that adults often fail to use ToM information (about what a speaker does or does not know) to constrain interpretation of a speaker's utterance. The lower error rate on ambiguous trials than on relational trials is consistent with Keysar et al.'s (2003) prediction that participants might be prompted to take the speaker's perspective more often when an egocentric interpretation of the utterance was ambiguous and so could not be reliably interpreted from self-perspective. However, we cannot interpret this result with confidence because the baseline for this condition was 50% compared with 100% errors in the relational condition, and participants were also faster in the ambiguous condition than in the control condition, suggesting a potential speed–accuracy trade-off. Interestingly though, participants remained error-prone in the ambiguous condition, suggesting that they may not have been prompted to take the director's perspective or repair their interpretation of the instructions on all trials.

Importantly, although participants were prone to egocentric errors this tendency was not

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[3] Here and throughout, control trials were not included when comparing error rates across conditions because they showed zero variance (there were no errors).

[4] An alternative analysis in which the missing data were replaced with the group mean values from the relevant condition yielded exactly the same pattern of significant and nonsignificant results.

increased on switch trials, when the critical instruction from the ignorant director followed an instruction from the informed director. This would suggest that the tendency for egocentric errors is unaffected by perspective switching. However, an alternative possibility is that participants actually took the ignorant director's perspective throughout the task (i.e., even when they should have taken the informed director's perspective), but were not wholly successful at using this information to guide their responses, resulting in the observed errors. This was a viable strategy because although some (filler) instructions came from the informed director, these instructions could always be answered correctly on the basis of the ignorant director's perspective. If participants pursued this strategy, our putative "switching" manipulation would not in fact have required perspective switching because participants would be continually adopting the ignorant director's perspective. This might have led us to underestimate the errors made on switching trials.

To address this possibility in Experiment 2a we made a minor alteration to the design so that critical instructions for relational trials sometimes came from the informed director. If participants consistently adopted the ignorant director's perspective, they would tend to make errors on critical instructions from the informed director. For example, if the informed director instructed them to "move the small ball" then the correct response is for participants to select the smallest of the three visible balls. However, if participants were simply adopting the ignorant director's perspective, they would mistakenly move the medium-sized ball. We reasoned that the presence of such instructions from the informed director might have two effects. First, if participants were not switching perspectives according to speaker in Experiment 1 but were doing so in Experiment 2, then we should see more errors in Experiment 2 than in Experiment 1 on trials where participants had to switch perspectives to that of the ignorant director. Alternatively, if participants used a strategy of inflexibly adopting the ignorant director's perspective throughout, this should increase the error rate when responding to critical instructions from the

informed director, even though the informed director shared the participant's perspective. To evaluate this error rate for critical instructions from an informed director, we needed a baseline condition in which we were sure that participants would not be taking the perspective of an ignorant director. For this purpose we ran Experiment 2b, in which the informed and ignorant directors were visible, but all instructions were spoken by the informed director. All instructions were therefore given from the same perspective as that of the participants.

EXPERIMENTS 2A AND 2B

Method

Both experiments were based upon the same picture and sound stimuli from Experiment 1 (see Figure 1b). The only difference in Experiment 2a was that the 4/8 critical relational trials and their 4 matched control trials from Experiment 1 were now spoken by the informed director. Whether spoken by the ignorant or informed director, all relational trials were "switch" trials, because the foregoing instruction was always delivered by the other director. The same manipulation could not be applied to ambiguous trials, because these instructions were genuinely ambiguous when delivered by the informed director. Thus, in Experiment 2a instructions for ambiguous trials were always spoken by the ignorant instructor, but these data were not analysed. In Experiment 2b all experimental and filler instructions were delivered by the informed director, including instructions for ambiguous trials. Once again, the data from ambiguous trials were not analysed.

EXPERIMENT 2A

Results

Only data from relational trials were considered for analysis.

Error analysis

As observed in Experiment 1, when the critical instruction involved a switch to the ignorant director's perspective, participants were prone to make errors on experimental trials (where the discrepant perspective affected how the instruction should be interpreted) but not on control trials. This difference was significant ($p < .001$ by sign test). The absolute error rate did not differ from that observed in Experiment 1: independent-samples t test, $t(38) = 1.05$, $p = .30$. The same pattern was observed when the critical instruction involved a switch to the informed director's perspective ($p < .001$ by sign test). However, as is clear from Figure 2, in experimental trials the error rate for switch-to-ignorant was very much higher than that for switch-to-informed. A paired-samples t test showed this difference to be significant, $t(19) = 5.82$, $p < .001$.

In both conditions participants performed significantly better than the baseline of zero correct responses, one-sample t test, both ts(19) > 4.3, both ps < .001, showing that they were not ignoring the directors' perspectives. In the ignorant director condition most participants made some correct and some incorrect responses (see Table 1), suggesting that they understood the instructions and were trying to take the director's perspective into account, but did not always manage to do so.

Response time analysis

Data from correct responses were preprocessed in the same way as for Experiment 1, resulting in the exclusion of 117/320 (36.5%) errors and 13 further data points that fell 2 standard deviations beyond the mean. A total of 6 participants lacked data for one or more cells of the design, principally due to the high error rate, and these participants were excluded from analysis. An ANOVA with condition (experimental vs. control) and perspective (informed vs. ignorant) as within-subject factors showed no significant effects of condition
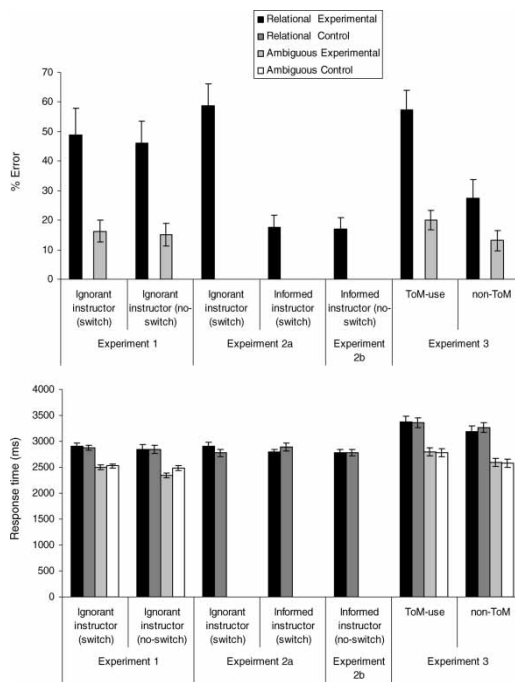
Figure 2. *Upper panel shows percentage error for the experimental and control conditions of Experiments 1–3 (N.B., error rate was zero for all control trials). Lower panel shows response time for the experimental and control conditions of Experiments 1–3 (N.B., only data from relational trials were analysed for Experiments 2a and 2b.) Error bars represent standard errors in all cases.*

or perspective (both Fs < .045, both ps > .84) and a nonsignificant trend for an interaction, $F(1, 13) = 3.42$, $p = .087$.[5]

EXPERIMENT 2B

Results

Only data from relational trials were considered for analysis.

Errors

Although the speaker's perspective was congruent with that of the participant, and there was no

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[5] This interaction (but not the main effects) was significant in an alternative analysis in which the missing data were replaced with the group mean values from the relevant condition. Separate comparisons of experimental and control trials for informed and ignorant perspectives showed neither difference to be significant, but the trend was for slower responses to experimental trials than to control trials when the speaker was ignorant and the opposite trend when the speaker was informed.

switching of perspectives in Experiment 2b, participants remained somewhat prone to error on experimental trials (e.g., sometimes selecting the medium-sized ball when requested to move the small ball), but never made errors on control trials, and this difference was significant ($p < .001$ by sign test). Our particular interest was to compare the error rate in this experiment with that in the switch-to-informed condition of Experiment 2. As can be seen from Figure 2, these error rates are similar, and an independent-samples t test confirmed this difference to be nonsignificant, $t(38) = 0.45$, $p = .66$.

Response time analysis

Data from correct responses were preprocessed in the same way as for Experiment 1, resulting in the exclusion of 143/320 (44.7%) errors and 14/177 (7.9%) data points that fell more than 2 standard deviations outside the mean. A paired-samples t test showed no difference in response time for the experimental and control trials, $t(19) = 0.12$, $p = .91$. Our particular interest was to compare response times in this experiment with those for the switch-to-informed condition of Experiment 2. An ANOVA with condition (experimental vs. control) as a within-subject factor and experiment (Experiment 2a vs. Experiment 2b) as a between-subjects factor showed no significant effects of condition or experiment and no interaction (all Fs < 2.05, all ps > .16).

Discussion of Experiments 1 and 2

In Experiment 1, participants were prone to error when responding to instructions that required them to take account of the ignorant director's perspective, but no more so when the critical instruction followed a previous instruction from the ignorant director (no-switch trials) than when it followed an instruction from the informed director (switch trials). Our concern was that this pattern could arise because participants simply tried to adopt the ignorant director's perspective throughout the experiment, thus undermining our perspective-switching manipulation. Experiment 2a

was designed to detect or negate any such strategy by including critical instructions from both the ignorant and the informed director. We found no evidence of such a strategy. Participants made significantly fewer errors responding to critical instructions from the informed director than from the ignorant director, suggesting that participants were indeed switching perspectives. However, participants made no more errors when switching to the ignorant director's perspective in Experiment 2a than in the switch trials of Experiment 1. Moreover, when responding to instructions from the informed director participants made no more errors when they had to switch perspective (Experiment 2a) than when they did not (Experiment 2b). The simplest explanation of these patterns is that participants were switching perspectives when necessary in both Experiments 1 and 2a but that this made no difference to participants' ability to use a director's perspective in interpreting his or her instructions. On this evidence, participants seemed remarkably adept at perspective switching, and it seems unlikely that difficulty with switching to the ignorant director's perspective is a reason for egocentric errors on this task in adult participants. This finding is consistent with Nadig and Sedivy's (2002) observation that the effect of the speaker's discrepant perspective was detectable very quickly, within 500 ms of the onset of the critical noun in the director's instruction (see also, Barr, 2008).

EXPERIMENT 3

The findings reported by Keysar et al. (2000, 2003) and in Experiments 1 and 2 are surprising given the expectation that communication should be guided very efficiently by information about the speaker's perspective (e.g., Clark & Marshall, 1981; Sperber & Wilson, 1995, 2002). However, it remains possible that ToM use is *relatively* efficient, just less efficient than originally supposed. If the efficiency of ToM use is to be properly evaluated, we need to compare a condition that requires ToM use with a minimally different condition that

does not require ToM. Thus, in Experiment 3 we created a non-ToM condition by removing directors from the scene and introducing the arbitrary but simple rule that participants should follow the verbal instructions but avoid objects in slots of the grid that had a grey background. Performance in this non-ToM condition was compared with performance in a ToM-use condition with a single, ignorant director. In both the ToM-use and the non-ToM conditions participants viewed the same grids of objects, heard the same verbal instructions, and had to limit the field of potential referents for the instructions to the same subset of objects. However, in the ToM-use condition the reason for limiting the field of potential referents was the fact that the director did not know about the hidden objects whereas in the non-ToM condition the reason was that an arbitrary rule dictated that these objects in slots with a grey background were to be ignored.

One possible pattern of data was that even if participants were error-prone in the ToM-use condition, they would be much more error-prone at using a rule that is arbitrarily related to the task of communication in the non-ToM condition. If ToM information were privileged in this way, this would be consistent with theories that see computation of an interlocutor's perspective as a necessary step in communication and the apparatus for such computations as an integral part of the cognitive apparatus for language processing (e.g., Sperber & Wilson, 1995, 2002). Another possibility was that participants would be no better—or possibly even worse—at using ToM information to guide communication in the ToM-use condition. If ToM information is not privileged then the more parsimonious explanation would be to assume that ToM use involves generic executive processes.

Method

Participants

A total of 40 participants took part in Experiment 3. All were undergraduate students ranging in age from 18–28 years. A total of 8 were male. All received course credits or a small cash honorarium.

Design and materials

There were two conditions: the "ToM-use" condition and the "non-ToM" condition. In the ToM-use condition all instructions were delivered in the voice of one director, who was unable to see items in occluded slots of the grid (i.e., this was a "standard" version of the ToM use task used by Keysar et al., 2000, 2003). In the non-ToM condition there was no director visible on the screen, but identical instructions were delivered in the same male voice as that in the ToM-use condition. Participants were given the simple rule that the instructions they would hear would not refer to items in the dark-backed slots (the same as those that the instructor could not see in the ToM-use condition). Thus, the non-ToM condition did not require perspective taking; it did, however, require a rule to be held in mind and used to narrow the field of potential referents for the messages to exactly the same set of objects as that in the ToM-use condition. Participants were randomly assigned to either the ToM-use or the non-ToM condition.

Both conditions of the experiment were based upon the same grids and objects as those used in Experiments 1 and 2, except that the ToM-use condition pictures had a single director (see Figure 1a), and the non-ToM condition had no director (see Figure 1c). The sound files were based on a slightly different instruction. For relational trials participants were instructed to "Move the [adjective noun] left/right/up/down one slot", and the offset of the noun occurred an average of 1,402 ms after the start of the sentence. For ambiguous trials participants were instructed to "Move the [noun] left/right/up/down one slot", and the offset of the noun occurred an average of 1,027 ms after the start of the sentence.

Procedure

Participants in the ToM-use condition were shown an example grid. They were told that the director could not see what was in the covered slots and did not know what was in these slots. To emphasize the difference in perspective, participants were shown the same grid from the ignorant director's perspective. They were told that it would be

important to take the director's perspective into account when following the instructions.

Participants in the non-ToM condition were shown an example grid. Their attention was drawn to the fact that several slots in the grid had a dark-grey back (these were the occluded slots in the ToM-use condition) while some had a clear back (the slots that could be viewed by the director in the ToM-use condition). Participants were told that the instructions they would hear would only refer to items in the clear slots, and that it was important for them to take this into account when following the instructions.

Results

Error analysis

Consistent with our earlier findings, participants frequently made errors on relational and ambiguous experimental trials, but never on control trials, whether they were in the ToM-use or non-ToM condition (all $ps < .001$ by sign test). Nonetheless, in all cases they performed significantly better than the baseline of 0% correct in relational trials and 50% correct in ambiguous trials, one-sample t test, all $ts(19) > 6.7$, all $ps < .001$, showing that they were not ignoring the director's perspective or the simple rule. As for the earlier experiments most participants made some correct and some incorrect responses (see Table 1), suggesting that they understood the instructions and were trying to take the director's perspective into account, but did not always manage to do so.

To compare the ToM-use and non-ToM conditions we entered the percentage error in experimental trials (see Footnote 3) into an ANOVA with problem type (relational vs. ambiguous) as a within-subject factor and condition (ToM use vs. non-ToM) as a between-subjects factor. As expected, this analysis revealed a significant effect of problem type, $F(1, 38) = 36.7$, $p < .001$, with more errors on relational trials (baseline error rate = 100%) than on ambiguous trials (baseline error rate = 50%). There was a significant effect of condition, $F(1, 38) = 10.2$, $p = .003$, with more errors in the ToM-use condition than in the non-ToM condition, and a significant

interaction, $F(1, 38) = 7.28$, $p = .01$. This interaction was investigated with post hoc t tests, which showed that for relational trials there were significantly more errors in the ToM-use condition than in the non-ToM condition, $t(38) = 3.34$, $p = .002$, whereas for ambiguous trials the condition means differed in the same direction, but this was nonsignificant, $t(38) = 1.44$, $p = .16$.

Response time analysis

Data from correct responses to experimental and control trials were preprocessed separately for relational and ambiguous trials in the same way as for Experiment 1. As can be seen from Figure 2, overall response times were somewhat slower for this experiment, but this can be accounted for by the fact that the offset for nouns in the instructions for this experiment occurred some 300–400 ms later than in the instructions for Experiments 1 and 2. For relational trials preprocessing resulted in the removal of 185/640 (28.9%) errors and the exclusion of 15 data points that fell 2 standard deviations beyond the mean. A total of 4 participants lacked data for one or more cells of the design, mainly due to errors, and these participants were excluded from the analysis. An ANOVA with trial type (experimental vs. control) as a within-subject factor and condition (ToM use vs. non-ToM) as a between-subjects factor showed no significant effects (all $Fs < .72$, all $ps > .40$; see Footnote 4). For ambiguous trials, preprocessing of the data resulted in the exclusion of 27 data points. An ANOVA with trial type (experimental vs. control) as a within-subject factor and condition (ToM use vs. non-ToM) as a between-subjects factor showed a marginally significant effect of condition, $F(1, 38) = 3.72$, $p = .061$, with a trend for slower responses in the ToM-use condition. No other effect was significant (both $Fs < 0.38$, all $ps > .54$). In sum, the response time analyses gave no reasons for suspecting that the differences between conditions observed in errors were the result of a trade-off between speed and accuracy.

Discussion

These results provide no support for the prediction that information about a speaker's knowledge is a

privileged constraint on interpreting instructions in referential communication tasks. Instead, the results suggest that participants experienced some difficulty when interpreting instructions according to a simple, arbitrary rule unrelated to communication, but experienced even greater difficulty when they interpreted instructions on the basis of the speaker's knowledge.

Of course, the finding that ToM use is relatively difficult compared with a matched non-ToM task does not of itself rule out the possibility that ToM use depends upon specialized cognitive apparatus for integrating communication with ToM that just happens to be relatively inefficient. However, this interpretation seems unattractive since superior efficiency is the primary motivation for suggesting that ToM use depends upon specialized cognitive apparatus (e.g., Sperber & Wilson, 1995, 2002). A simpler explanation is that ToM use (at least of the kind studied here—see General Discussion) is relatively error-prone because it makes significant use of capacity-limited executive control processes for inferring and holding information in mind and using it when necessary to constrain interpretation of the director's instructions. Direct evidence for this hypothesis comes from preliminary observations that individual differences in adults' error rates on a ToM-use task are significantly correlated with individual differences in performance on tests of executive function (Qureshi, Apperly, & Samson, 2007).

What specific aspects of ToM use gave rise to adults' errors? Experiment 3 suggests that adults' difficulties are not merely with processing verbal instructions, with processing a complex visual array, or with narrowing the field of potential referents to a subset of the visible objects: These features were common to the ToM-use and non-ToM conditions. A distinctive feature of the ToM-use condition was the need to infer the director's perspective, and there is independent evidence that such inferences carry a processing cost for adults (Apperly et al., 2006). However, participants had considerable time (5 seconds) to study the grid of objects before hearing any verbal instructions, and we think it is unlikely

that the need to infer the director's perspective was the source of participant's errors. Instead, we suggest that having inferred the director's discrepant perspective in the ToM-use condition, participants found it more difficult to hold this information in mind than to hold in mind the same set of items to select and avoid in the non-ToM condition. There is existing evidence to suggest that, when holding in mind a discrepant perspective, participants are particularly vulnerable to interference from their own knowledge. A recent study found interference between information about the real colour of an object and a person's false belief about the object's colour when this information had been held briefly in mind (Apperly et al., 2008). In the current task the problem of holding in mind the set of items about which the director is informed and ignorant presents a substantially more complex task for participants than that used by Apperly et al. (2008), so it is plausible that this was a significant source of difficulty. Moreover, in the current task participants not only had to hold the director's perspective in mind but actually needed to use this information to interpret instructions. It is possible that actually using information about a conflicting perspective (as opposed to merely remembering and reporting it; Apperly et al., 2008) made participants yet more vulnerable to interference between the perspectives of self and other.

GENERAL DISCUSSION

The current experiments (like those of Keysar et al., 2000, 2003) presented adult participants with the problem of interpreting instructions from a speaker (the "director") who only partially shared their own knowledge of an array of objects. The interesting finding from earlier studies (Keysar et al., 2000, 2003) was that despite the conceptual simplicity of this task, adult participants often failed to take the director's lack of knowledge into account, sometimes selecting items that he did not know about as referents of his instructions. Keysar et al.'s tasks were run with real arrays of objects and real speakers, whereas the current

studies were based upon simple cartoon stimuli and prerecorded instructions. Despite these changes in method, our basic findings resemble those of Keysar et al. (2000, 2003), with adult participants showing a strong tendency for egocentric errors when interpreting instructions from a speaker who did not share their knowledge of possible referents. The novel contribution of the current studies is to cast light on the reasons for these errors. In Experiments 1 and 2 we tested whether egocentric errors might arise as a result of a failure to switch to the perspective of the ignorant director. We found that participants' rate of egocentric errors was not affected by the need to switch between perspectives. That is to say, participants were no more egocentric when responding to instructions from an ignorant director on trial N when trial $N - 1$ involved an instruction from an informed director who shared the participant's perspective (so perspective switching was necessary on trial N) than when trial $N - 1$ involved an instruction from the very same ignorant director as that in trial N (so perspective switching was unnecessary on trial N). In Experiment 3 we found that participants made more errors in a ToM-use condition than in a closely matched non-ToM condition, suggesting that, beyond any general processing demands, participants were specifically limited in their ability to hold in mind the director's perspective and use this to guide their responses. We suggest that this limitation may be in generic executive processes, rather than in processes specialized for communication, and that such executive processes therefore have a specific role in ToM use, not merely in general task performance.

We believe this provides a plausible explanation for the puzzling difference between Nadig and Sedivy's (2002) finding that 5- to 6-year-old children showed sensitivity to the speaker's restricted perspective in a ToM-use task and Keysar et al.'s (2000, 2003) findings that adults are prone to egocentric errors on a similar task. Although the conceptual demands of the perspective-taking tasks used in these studies were essentially the same, Nadig and Sedivy's task employed far smaller grids with far fewer items, so plausibly made lower executive demands than the tasks employed

by Keysar and colleagues (and in the current experiments). Several direct predictions follow from this interpretation: First, adults should perform significantly better on simpler tasks and worse on more complex tasks; second, children should perform well on increasingly complex tasks with increasing age; and third, in all cases, individual performance should correlate with individual differences in the relevant executive processes. More generally, in development, a significant role for executive functions in ToM use would help explain improvements in children's everyday ToM abilities long after they pass standard tests of their possession of ToM concepts. Converging evidence for the plausibility of this suggestion comes from the fact that executive functions and the neural regions supporting these functions (predominantly regions of prefrontal cortex, e.g., Stuss & Knight, 2002) continue to mature throughout later childhood and adolescence (e.g., Casey, Tottenham, Liston, & Durston, 2005). And indeed, a recent study using a similar ToM-use task to that used in Experiment 3 found significant improvements in ToM use throughout late childhood and adolescence, with disproportionate improvement in late adolescence (Dumontheil, Apperly, & Blakemore, in press).

It is important to reemphasize that our conclusions in the current paper concern the role of representations of the speaker's mental state in guiding interpretation of the speaker's utterances and are not intended to assess the likelihood of egocentric errors in naturally occurring communication. We think the current task is a good laboratory model for certain aspects of ToM use. It is a common enough situation that we explicitly know some facts about what a speaker knows or wants and use this to interpret what they mean by what they say. We might expect that the conclusions from the current studies would extend to the opposite problem of designing one's own language on the basis of explicit knowledge of what our listener(s) know. However, it is clear that this single task will not capture the roles of a variety of more complex and more simple processes that enable communicators to coordinate their mental states.

We think it highly likely that more executive functions play a significant role beyond those discussed here. One such function is dealing with "ill-structured situations" (e.g., Burgess, Gilbert, Okuda, & Simons, 2005; Goel & Graffman, 2000) where the appropriate response is not determined exclusively by the physically present stimuli or the task set. Everyday communication represents a clear case of an ill-structured situation, because it is frequently necessary to go beyond the words and objects that are currently present and the general objective of taking the speaker's perspective into account. One has to decide *when* it is appropriate to take the speaker's perspective, and *which* aspects of his or her perspective are actually relevant to interpreting what they are currently saying. The ToM-use task employed in the current studies lacks this real-life uncertainty and so does not test this aspect of executive function.

Equally, there may be many ways in which listeners and speakers adapt to one another's mental states without inferring mental states and without placing a significant burden on executive processes, but the necessary cues that are freely available in everyday communication were not available in the task used here. For example, listeners can often see a speaker's eyes, and there is evidence that information about eye gaze is used rapidly online to resolve ambiguous reference (Hanna & Brennan, 2007). In our task we had the ignorant director wear dark glasses, following pilot work in which even the static cartoon eyes of our director were sufficient to give a compelling impression that he was referring to specific objects in the array. Moreover, as Keysar et al. (2003) point out (see also Pickering & Garrod, 2004) communicators do not typically have to interpret raw instructions in the manner of the present task, because everyday communication gives many opportunities for verbal and nonverbal feedback, by which comprehension difficulties can be signalled and extra information provided. The elimination of such cues in the present task is essential if we wish to study a listener's ability to use explicit ToM inferences to guide online interpretation, but equally this means we are most certainly not tapping all of the processes by which communicators coordinate their mental states for effective communication. The present findings suggest that this is probably a very good thing, since even adults are relatively poor at using explicit inferences about what someone else can see or know to interpret what they say. On the one hand, explicit ToM inferences undoubtedly play a role in guiding communication. On the other, much of this burden of coordinating speakers and listeners is likely to be carried by a variety of more efficient processes that operate without explicit computations about their mutual mental states.

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