Measuring visual perspective taking in the brain with avatars and arrows: Which question are we asking?

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ARTICLE INFO

ABSTRACT

There is an ongoing debate about the involvement of Theory of Mind (ToM) processes in Visual Perspective Taking (VPT). In an fMRI study (Schurz et al., 2015), we borrowed the positive features from a novel VPT task – which is widely used in behavioral research – to study previously overlooked experimental factors in neuroimaging studies. However, as Catmur et al. (2016) rightly argue in a comment on our work, our data do not speak strongly to questions discussed in the original behavioral studies, in particular the issue of implicit mentalizing. We appreciate the clarification of these interpretational limitations of our study, but would like to point out the differences between questions emerging from behavioral and neuroimaging research on VPT. Different from what Catmur et al. (2016) discuss, our study was not intended as a test of implicit mentalizing. In fact, the terms "automatic" and "implicit mentalizing" were never mentioned in our manuscript. Our study addressed a methodological gap between ToM and VPT research, which we identified in two previous meta-analyses on the topics (Schurz et al., 2013, 2014). With this difference in mind we show that the critical points levelled by Catmur et al. (2016) cease to apply.

1. Background: a difference between VPT and ToM imaging studies

Visual Perspective Taking (VPT) is often seen as a component of Theory of Mind (ToM), that is, people's ability to understand the mental states of others. Extensive literature exists on the neural correlates of ToM (for meta-analyses see e.g. Molenberghs et al., 2016; Schurz et al., 2014; Van Overwalle, 2009) and VPT (see Schurz et al., 2013 for a meta-analysis). Despite strong conceptual and developmental links between the two topics (e.g. Hamilton et al., 2009; Moll and Meltzoff, 2011), imaging research has found surprisingly little overlap between brain areas underlying these two abilities. In an fMRI study (Schurz et al., 2015), we attempted to explain this absence of overlap by following-up a methodological difference between VPT and ToM studies using neuroimaging methods (as we stated in the introduction of our article, see p. 386). VPT studies have largely focused on comparing brain activation for other-versus self-perspective judgments

(but see Ramsey et al., 2013 for an exception), whereas ToM studies have focused on other-mental-state versus no-mental-state judgments. This methodological difference is critical, as overlapping activations for other- and self-related judgments were found in ToM research (e.g. Mitchell, 2009; Murray et al., 2012). To clarify if similar shared activations are underlying imaging findings on VPT, we used the innovative features from a novel VPT task used in behavioral research (the so-called "avatar task" or "dot-perspective task", see e.g. Samson et al., 2010, Santiesteban et al., 2014, Qureshi et al., 2010). First, we systematically manipulated the consistency between participants' and the avatar's perspective (as in Samson et al., 2010). Second, we introduced several non-mental control conditions, one of them presenting an arrow (as in Santiesteban et al., 2014). In a recent comment, Catmur et al. (2016) critically discussed our study in light of the "implicit mentalizing" debate, which takes place mainly in the field of behavioral research (see e.g. Apperly, 2010; Heyes, 2014; Heyes and Frith, 2014). We agree with Catmur et al. (2016) that our data do not provide strong

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support for the implicit mentalizing hypothesis, but would like to highlight that our study was carried out with a different aim: To deconstruct brain activation for other-versus self-perspective judgments that were dominating VPT imaging research so far. This different aim explains the way we discussed our findings, and – as we will argue – resolves the critical comments raised by Catmur et al. (2016).

1.1. Did we claim to show implicit mentalizing?

Initially, behavioral results of the avatar task, and in particular the finding of altercentric intrusion (prolonged reaction times during self-perspective judgments for trials presenting inconsistent self- and avatar perspectives), were interpreted as indication of an efficient mechanism for quickly computing the avatar's perspective via his/her line of sight (Samson et al., 2010). To our understanding, no strong claims were made about the domain specificity of this process, or the potential integration between parsing of social stimuli and allocating visual attention (Samson et al., 2010; see also Furlanetto et al., 2016). More recently, others (Santiesteban et al., 2014) have framed the altercentric intrusion effect in the context of two mutually exclusive interpretations. On the one hand, the implicit mentalizing hypothesis, which states that altercentric intrusions reflect a domain-specific process, through which a rich representation of what the avatar can see is provided. On the other hand, a domain-general attention shifting hypothesis by which the directional features of the avatar trigger shifts in attention that lead to prolonged reaction times on inconsistent trials. These two hypotheses create a theoretical dichotomy, which overlays accounts provided in original papers (see e.g. Samson et al., 2010; Ramsey et al., 2013), and is also different from the research question we followed up in Schurz et al. (2015). Specifically, Catmur et al. (2016) suggest that we interpreted the results in Schurz et al. (2015) as "… supporting their claim of 'implicit mentalizing' – the automatic ascription of mental states to another representing what they can see" (Catmur et al., 2016, abstract, p. 8). However, our article did not make any assumptions about "implicit" and "automatic" processes. One part of our experiment tested uninstructed processes linked to the avatar and his/her perspective – which we referred to using the term "spontaneous". Participants completed self-perspectives judgments in the first block and had no training or practice of other-perspective judgments (i.e., for the first 24 minutes of the experiment).[1] Therefore, our research question was whether uninstructed processes are taking place in the context of a typical VPT imaging task – i.e. in a set-up similar to what was used in previous level 1 VPT imaging studies (e.g. Aichhorn et al., 2006; Vogeley et al., 2004; Kaiser et al., 2008; Kockler et al., 2010; Ramsey et al., 2013). These uninstructed processes are different from what Catmur et al. (2016) referred to as "automatic" and "implicit" processes. Both of the latter concepts have been linked to a number of requirements that our study could not test. For example, Frith and Frith (2008) linked implicit processes to two characteristics - being uncontrollable and occurring without awareness. Bargh (1994) linked automatic processes to four characteristics - being unintentional, unconscious, uncontrollable and attentionally efficient. Except for intentionality (i.e. instructed vs. uninstructed processing), none of these characteristics were addressed in our study, and therefore our data cannot be taken as strong evidence regarding questions of automatic/implicit processes. To illustrate, researchers assessed absence of awareness in Theory of Mind experiments with debriefing questionnaires (e.g., Schneider et al., 2011, 2012), and tested for attentional efficiency (i.e. effortlessness) with dual-task paradigms (e.g., Qureshi et al., 2010; Schneider et al., 2012). Nothing like that was done in our study.

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[1] Concretely, we instructed participants to judge for each picture how many discs are placed on the walls of the room, by selecting one option out of "1", "2" or "3" by button press.

2. Directional features and the arrow control condition

A main goal in Schurz et al. (2015) was moving away from self-perspective judgments as control condition for other-perspective taking. We presented three novel controls – showing an arrow, a lamp, and a brick-wall – and considered a combination of findings from these three conditions in our interpretation. By contrast, Catmur et al.'s (2016) comment focuses on the arrow control condition. The authors point out a difference between Schurz et al. (2015) and Santiesteban et al. (2014), based on which they rightly argue that our study cannot be taken as a replication study of Santiesteban et al. (2014). We used an arrow stimulus with a canonical horizontal orientation (Schurz et al., 2015; see Fig. 1, p. 387), whereas Santiesteban et al. (2014; see Fig. 1, p. 930) used a partly vertically oriented arrow. Arguments can be made in favor of either version. We presented a canonical arrow because it has been found to cue attention in other task set-ups, sometimes even to a similar extent as social cues like faces (e.g., Tipples, 2002). Santiesteban et al. (2014) presented an arrow that was closely matched to the avatar in terms of

Eye-tracking shows the deployment of attention in the avatar task (Ferguson et al., 2017)

A. Distribution of all fixations
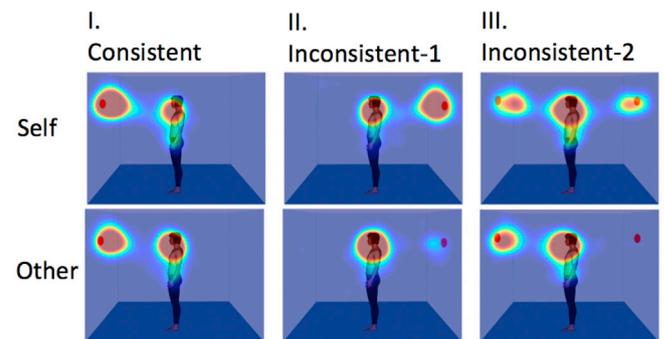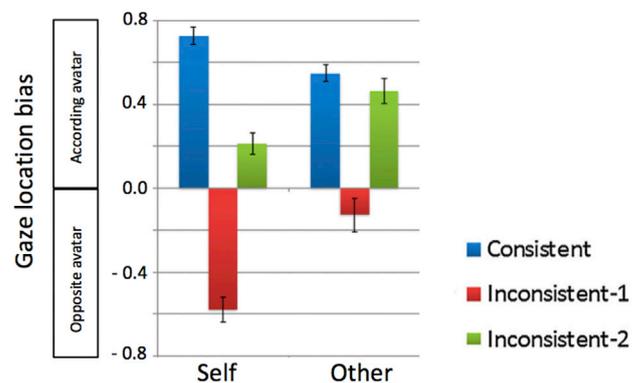
B. Gaze location bias (based on first fixations)

Fig. 1. (A) Heatplots showing the location of all fixations in the avatar task, separately for self- and other-perspective trials (top and bottom row, respectively). Inconsistent-1 trials showed a disc only on the wall behind the avatar, Inconsistent-2 trials showed a disc on both walls. For brevity, we only show heatplots for trials where participants were asked for the same perspective-judgment as in the previous trial (so called "stick" trials, for details see[2]) – as this corresponds to the blocked presentation in our fMRI experiment. (B) Probabilities (mean, standard errors) of fixating either the gaze location (positive) or the no-gaze location (negative values) of the avatar, separately for self- and other-perspective. Figure from Ferguson et al. (2017). Adapted by permission of SAGE Publications, Ltd.

visual features. On the one hand, this rules out differences in visual salience and how well the directional properties of each stimulus are discriminable. On the other hand – as we argued in our manuscript "… the interpretation of Santiesteban et al.'s (2014) arrow that shared visual features with an avatar (e.g., standing upright and having a directional top) is less clear to us. It cannot be ruled out that these features added a rudimentary form of "biological plausibility" or "animacy" to the arrow." (p. 393). This rudimentary biological plausibility of the arrow could elicit similar cognitive and neural processes as the avatar, which makes the comparison avatar > arrow hard to interpret. For example, simple geometric shapes can activate ToM areas (including bilateral TPJ) if they contain features of animacy (e.g., Blakemore et al., 2003; Castelli et al., 2000; see also Furlanetto et al., 2016 for discussion). In their study comparing avatars and arrows, Santiesteban et al. (2014) noted "… Perhaps everyday experience with arrows, in which interesting or important stimuli are more likely to be located near the head than the tail, results in habitual representation of what arrows can "see". " (p. 935). Taken together, these reasons guided our decision to present a canonical horizontal arrow in the fMRI study in order to reduce the potential for anthropomorphization.

In light of these considerations, it is important to mention recent data showing that controlling appropriately for the directional features of the avatar turns out to be less relevant than assumed. Ferguson et al. (2017) recorded eye-movements to show, for the first time, how visual attention is allocated around the scene in the avatar task. The authors found no evidence for attentional orienting being predominantly driven by directional features. Fig. 1 shows the scene of the avatar task and the location of fixations made by participants. As Ferguson et al. (2017) argued, a domain general attentional orienting account (driven by directional features of avatars and arrows) predicts that visual attention automatically shifts to the dots in the avatar's field of view, regardless of whether participants are instructed to judge their own versus the avatar's perspective. A perspective-taking account, on the other hand, predicts that participants should show a reduced bias to fixate the avatar's gaze location when they take their own compared to the avatar's perspective. By this account, participant's attention should be divided between the two possible locations for discs for self-perspective judgments. To differentiate between the two accounts, Ferguson et al. (2017) calculated the gaze location bias (see Fig. 1B) based on the target area of participant's first fixation: If participant's first fixation was directed towards the side of the scene where the avatar was looking, a positive gaze location bias was measured. If the first fixation was directed to the opposite side from where the avatar was looking, a negative gaze location bias was calculated. Because trials showing discs only on one side of the scene (see displays I and II in Fig. 1A) may produce attentional biases in the corresponding direction, Ferguson et al. (2017) focused on trials where both sides of the scene show discs (see display III in Fig. 1A). For such trials, participants showed a significant gaze location bias when judging the avatar's perspective, but no significant bias to either the gaze or the no-gaze location when taking their own perspective. This pattern was supported by a significant main effect of perspective on gaze location bias and by follow up one-sample t-tests (Ferguson et al., 2017; p.1655).[2] Taken together, results point out that when participants are instructed to take the avatar's perspective in the task, they are more likely to first fixate

the wall at which the avatar is looking. However, when participants are instructed to respond according to their own perspective, they divide their attention equally between the wall within and the wall behind the avatar's view. This illustrates that participants are directing their visual attention to the relevant locations in the scene according to the perspective-taking instructions of the task, and speaks against the concern that participants' attention is merely driven by directional properties of the avatar in the task.

3. Interpretation of neuroimaging data

Catmur et al. (2016) argue in their comment that a number of statements in our discussion of imaging findings are not supported by our data, as specific interaction patterns were not found. We would like to reply to this criticism by highlighting important differences between the neuroimaging questions based on which we wrote our discussion and the behavioral research context that Catmur et al. (2016) cast on our manuscript. In the implicit mentalizing debate, a particular interaction pattern is of critical relevance for showing a specialized neurocognitive mechanism that is tracking others' mental states in an automatic, fast and efficient manner. An implicit mentalizing area is expected to show an interaction between animacy (i.e. avatar vs. arrow) and consistency of perspectives (i.e. inconsistent vs. consistent), with a higher activation level for avatar inconsistent versus avatar consistent, but the same activation level for arrow inconsistent and arrow consistent. As Catmur et al. (2016) rightly point out, we did not find this pattern in any brain area in our study, and thus, do not provide any evidence for such implicit mentalizing on the neural level. However, our discussion followed the aims of our study in addressing a neuroimaging-based research question. Overlapping activations for other- and self-related judgments have been found in imaging studies on ToM (e.g. Mitchell, 2009; Murray et al., 2012), and we hypothesized that similar overlaps are also present for VPT studies. These overlapping activations were not appropriately picked up and potentially even cancelled out by the other > self and self > other contrasts used in previous VPT research. We therefore combined two task elements – (i) adding non-mental control conditions and (ii) varying the consistency between self- and other-perspectives as an experimental factor – to detect any previously overlooked brain activation during VPT tasks. Motivated by these questions, we sought for effects *irrespective* of (i) whether they were present only for avatars or also for arrows and (ii) whether they were present in self- and/or other-perspective trials. Therefore, we also tested consistency effects for avatars and arrows with post-hoc tests in our ROIs, although we did not find an initial interaction pattern between animacy and consistency in these ROIs. What we found were simple main effects of consistency and animacy in a number of these areas. We thank one of our reviewers for pointing out this absence of an initial interactions in our ROIs, which calls for caution when interpreting the results. However, we would also like to point out that these ROI results are supported by corresponding findings from our whole brain analysis. For all but one of the statements criticized by Catmur et al. (2016), we found converging evidence from whole-brain and ROI results. For sake of brevity, our statements in the discussion integrated and summarized across these results, without repeating them in full detail. To avoid further ambiguities in interpretation, we provide a detailed account of whole brain and ROI results supporting each of the five statements cited by Catmur et al. (2016) in Supplementary Materials S1. To illustrate with an example (see Supplementary Materials S1 for details), our statement "theory of mind areas engage when the scene shows a perspective difference" (Schurz et al., 2015, p. 395) was not referring to an interaction, but to the main effect of consistency found in our whole-brain analysis for self-perspective and other-perspective judgments.

Moreover, and of equal relevance for arriving at our conclusions in the discussion (see again Supplementary Materials S1 for details), are data from two complementary control conditions in our task, which are not further considered in Catmur et al.'s (2016) comment on our

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[2] For completeness, we also note that Ferguson et al. (2017) found a significant main effect of trial type (see p. 1655/1656). Trials that were preceded by a trial of the same type ("stick" trials) were compared to trials that were preceded by a trial from a different type ("switch" trials). An example for the former is a trial asking for a self-perspective judgment that is preceded by another self-perspective trial. An example for the latter is a self-perspective trial that is preceded by an other-perspective trial. Importantly, however, the interaction between trial type and perspective on the gaze location bias was not significant (see p. 1656), indicating that the main effect of perspective (to which we refer in the main text) is present for both kinds of trial type.

manuscript. To sort out areas sensitive to directional features/attentional orienting per se (contained by both avatars and arrows), we tested for the main effect arrow > brick-wall (non-directional control). None of the areas found for avatar > arrow overlapped with what was found for arrow > brick-wall (see Fig. 5, p. 393), speaking against the concern that activations for avatar > arrow are merely reflecting a quantitative difference in directional features. To check whether a difference in visual object complexity is driving our observed activations, we furthermore tested the main effect of lamp > arrow (Fig. 5, p. 393). Again, no activations were found in the ToM areas identified by avatar > arrow (mPFC, precuneus, right TPJ), ruling out a further source of activation that is unspecific for avatars.

Acknowledgments

Appendix A. Supplementary data

Supplementary data related to this article can be found at https://doi.org/10.1016/j.neuroimage.2018.07.048.

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