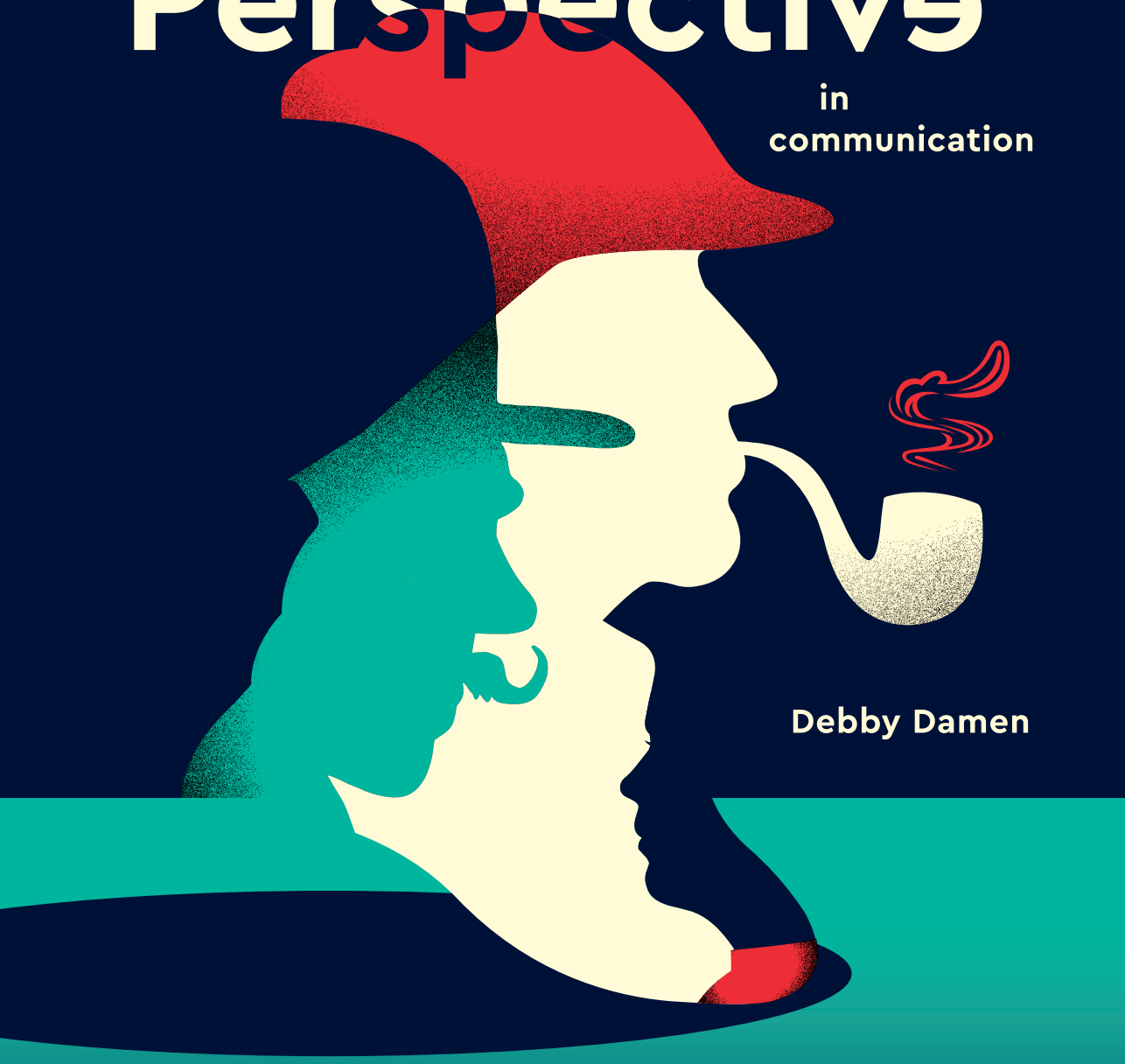
Taking Perspective

in communication



Debby Damen

Exploring what it takes to change perspectives

Taking Perspective in Communication: Exploring What it Takes to Change Perspectives

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Taking perspective in communication:
Exploring what it takes to change perspectives

Debby Damen
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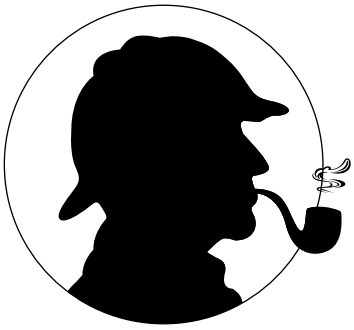
Read at once if convenient—if inconvenient read all the same.
– S.H.

Table of Contents

Chapter 1	General Introduction: The Curious Case of the Perspective Taker	**9**
Chapter 2	Changing Views: The Effect of Explicit Perception-Focus	**21**
Chapter 3	Perspective-Taking in Referential Communication: Does Stimulated Attention to Addressees' Perspective Influence Speakers' Reference Production?	**47**
Chapter 4	Can the Curse of Knowing Be Lifted? The Influence of Explicit Perspective-Focus Instructions on Readers' Perspective-Taking	**87**
Chapter 5	Lifting the Curse of Knowing: How Feedback Improves Perspective-Taking	**125**
Chapter 6	General Discussion and Conclusion	**163**
	Summary	**177**
	References	**187**
	Acknowledgments	**201**
	Publication List	**209**
	TiCC Ph.D. Series	**215**

Chapter 1

General Introduction:
The Curious Case of the Perspective Taker



Perhaps the most famous quote from Conan Doyle's illustrious consulting detective Sherlock Holmes concerns Holmes' explanation of his deductive reasoning, which helped to unravel the crime described in "A Study in Scarlet". When Watson asks why Holmes does not yet reflect upon the crime scene they are about to encounter, he answers: "It is a capital mistake to theorize before you have all the evidence. It biases the judgment" (p. 69, 2015). This quote illustrates the character's striking ability to acknowledge how his preconceptions can result in misconceptions, prejudices or general narrow-mindedness. In every story, Holmes broadcasts his ability to, literally, take a step back and to view the situation from a vantage point that is not clouded by his own view on it. In fact, in order to decipher the motives of the most wanted criminals, Holmes argues that he not just imagines himself in the criminal's shoes, but that he becomes the criminal, by taking each step as if he were the criminal himself. In doing so, Holmes is able to put aside his own thoughts and feelings as he acknowledges that they can bias his ability to understand the criminal's perspective. While observing the situation from the criminal's point of view, the consulting detective even learns to appreciate a mind that is so different from his own.

Like Sherlock Holmes, we regularly try to deduce what other people desire, know, believe and feel. Our ability to have these "theories of mind" (e.g., Premack & Woodruff, 1978; see also Moses, 2005) allows us to attribute mental states to others. In addition, it also allows us to realize that our view on the world might not be shared by those around us. However, unlike Holmes, research has shown that we do not always acknowledge or represent this difference in perspectives in our communication to others. In fact, we often do not fully take into account that others might be paying attention to different things or might evaluate things differently than we do (Epley, 2014). For example, we sometimes do not realize that "on the right-side" from our vantage point can actually mean someone's left side (e.g., Tversky & Hard, 2009), that our request to receive that "*big* bottle" is overinformative and, therefore, confusing to someone who can only see one big bottle (e.g., Kaland, Krahmer & Swerts, 2014; Wardlow Lane, Groisman, & Ferreira, 2006), or that others do not detect a speaker's sarcasm because they – unlike us – lack the necessary background information (e.g., Epley, Keysar, Van Boven & Gilovich, 2004; Kruger, Epley, Parker, & Ng, 2005; Keysar, 1994). Instances such as these show us that we are, unfortunately, not always like Sherlock Holmes. As perceivers of other minds, we often find it difficult to appreciate another person's different perspective because our own knowledge and attentional status biases our social judgment. The question that arises here is *whether* and *how* we can learn to observe other minds like Doyle's fictional character. In this dissertation, we address this question and examine whether perceivers' *perspective-taking* during both language understanding and production processes benefits from a stimulated attention to another person's point of view. We adopted the term "perceivers"

from the social psychology literature to refer to the people who try to perceive and deduce the mental states of others in communication. In this introductory chapter, we provide background information for our research question and subsequently introduce the studies that are part of this dissertation.

When and How Individuals Engage in Perspective-Taking

Our ability to interpret what others are thinking and feeling allows us to anticipate other people's behaviour and to adapt our communication accordingly. In this way, perspective-taking enables us to successfully interact with those around us, thereby improving the quality of our friendships (e.g., Brackett, Mayer, & Warner, 2004; Lopes et al., 2004) and romantic relationships (e.g., Brackett, Warner & Bosco, 2005; Schröder-Abe & Schütz, 2011). One prevalent question in perspective-taking research is *how* we try to understand other people's perspectives. In order to answer this question, research distinguishes two important components, namely our common- and privileged ground information (Clark, 1992; Clark & Marshall, 1981; Horton & Keysar, 1996). Common-ground information encompasses all the knowledge, beliefs, and attitudes of which we know it is shared (known) between our interlocutor and ourselves. Common-ground information includes our knowledge about our interlocutor's perspective, but excludes information that is exclusively accessible to ourselves. This private information is termed as our privileged ground information.

Various studies have focused on the question when common-ground information is taken into account during language understanding and production, and what role our privileged information plays in this process. One line of research claims that common-ground information constrains the way we as interlocutors produce and understand language. This entails that we are expected to be cooperative in interaction with one another (Grice, 1975) and to adhere to the so-called audience-design principles of communication (Clark, 1992; Clark & Carlson, 1982; Clark & Murphy, 1982). According to these principles, we should always produce and comprehend language based on only the knowledge that is shared and known between our interlocutor and ourselves if we want our communication to be successful. In this regard, we are expected to refrain from comprehending or referring to information that is privileged to ourselves, as that would violate the cooperativeness and, hence, successfulness of our communication.

In support of this audience-design view, research has shown that we can easily make a distinction between common- and privileged ground information, and rapidly integrate our interlocutor's perspective in the early stages of language processing (e.g., Brown-Schmidt, Gunlogson, & Tanenhaus, 2008; Brown-Schmidt & Tanenhaus, 2008; Hanna, Tanenhaus, &

Trueswell, 2003). In these studies, addressees interpret their speaker's message solely on the basis of common-ground information, and speakers are able to tailor their messages in such a way that they are informative to their addressees (see also Krauss & Fussell, 1991).

Another line of research showed, however, that we do not always succeed to be fully cooperative in our communication. Sometimes, we might interpret a speaker's message on the basis of their own perspective (e.g., Keysar, Lin, & Barr, 2003; Horton & Keysar, 1996), or disclose information to our addressee that is privileged to ourselves (e.g., Kaland et al., 2014; Wardlow Lane et al., 2006). These studies argue that we make these egocentric errors because it is often too time consuming to integrate our interlocutor's perspective from the onset of language production and comprehension. Instead, we are more likely to base our communication on information that is immediately accessible (or known) to ourselves, regardless of its shared nature. Common-ground information – that includes our interlocutor's perspective – is then only integrated when we realize we have made a mistake (e.g., Keysar, Barr & Horton, 1998; Keysar et al., 2000).

In line with this latter view, studies have repeatedly shown that we are likely to rely first on self-referential information when we try to predict what other people desire, know, think or feel. This self-referential mentalizing (Mitchell, 2009), simulation (e.g., Gallese & Goldman, 1998; Goldman, 1989; Tamir & Mitchell, 2013; see also Davies, 1994), egocentric projection (Ames, 2004ab; see also Nickerson, 1999), or egocentric anchoring (Epley et al., 2004; see also Nickerson, 1999), is often followed by a so-called adjustment phase (e.g., Barr, & Keysar, 2005; Epley, 2008; Epley & Gilovich, 2004). During this phase, we try to take into account any difference that might exist between our and our interlocutor's perspective by correcting our egocentric interpretation. This takes up time and effort, however, because our egocentric thoughts come easily to mind and are, therefore, hard to ignore when we try to predict another individual's perspective (Epley et al., 2004; for a discussion see **Chapter 4** in this dissertation). Consequently, the corrections we make to our self-perspective are often inadequate, meaning that our predictions of other people's perspective are very likely to be biased in the direction of their own (e.g., Epley & Gilovich, 2004; Epley & Gilovich, 2006). Eventually, this egocentricity bias in our communication causes us to often overestimate the extent to which others view and evaluate the world in a similar manner (e.g., Keysar, Barr, & Horton, 1998; Gilovich, Medvec, & Savitsky, 2000; Keysar et al. 2000, 2003; see also "false consensus" in Ross, Greene, & House, 1977; Krueger & Clement, 1994).

Even though egocentric projection might be beneficial if we and our interlocutor share desires, beliefs, attitudes and so on (e.g., Hoch, 1987), assuming similarities when there are – in fact – none is detrimental for an accurate understanding of another person's perspective. Of course, this relationship also works the other way around. That

is, misunderstanding and conflict might arise when we fail to see commonalities when they actually exist (e.g., Thompson, Nadler, & Lount Jr., 2006). In both cases, we do not reach an accurate understanding of the other person's mental state. Both processes are of interest in the field of perspective-taking. However, in this dissertation, we focus on those instances in which we as perceivers falsely impute our perspective onto others because our privileged point of view – due to the ease by which it is retrievable – biases our social judgment. We aim to investigate in what way we as perceivers can be stimulated to put less emphasis on our egocentric perspective when we try to read other minds.

Increased Attention to Another Point of View

The conflicting picture about perceivers' propensity to engage in perspective-taking during communication requires more research that attempts to replicate previous proposed mechanisms. In turn, if egocentric biases arise in our communication to others, it is definitely worth investigating how they can be countered. If the availability and, hence, saliency of our privileged point of view biases our ability to acknowledge perspective-differences in our communication, then stimulating us to attend to common-ground information may help us to put less emphasis on our privileged point of view (see also Mitchell, 2009). That is, if we are explicitly focused on information that *is* accessible to our interlocutors *before* we produce or understand language, we might be less likely to project our own perspective onto our interlocutors when communicating.

Studies have indeed shown that shifting perceivers' focus away from their own perspective seems to improve the accuracy of their social predictions. For instance, people are less likely to overestimate how harsh others will judge them for their social failure if they focus less on their personal mishap and more on all other situational factors observers can take into account while judging them (Savitsky, Epley, & Gilovich, 2001). Individuals are also less likely to overestimate the amount of work they have done when they focus less on their own contribution and more on the contributions of other group members (Caruso, Epley, & Bazerman, 2006). Moreover, evidence from studies focusing on visual perspective-taking even seems to suggest that an awareness of other individual's perspective makes perceivers slower to respond from their predominant egocentric point of view (Elekes, Varga, & Király, 2016; Ferguson, Apperly, & Cane, 2017; see also Samuel, Roehr-Brackin, Jelbert, & Clayton, 2019). In these studies, perceivers spontaneously "computed" another individual's perspective because they were aware that this different perspective was at stake. We still do not know, however, whether this focus on another person's perspective is also beneficial during language processes in which perceivers are likely to make egocentric errors. We question whether perceivers are less likely to produce

and understand language on the basis of privileged information if they are attending to their interlocutor's knowledge and attentional status. In this dissertation, we therefore aim to answer the following research question:

Research Question: To what extent does an explicit attention to another person's perspective help perceivers to acknowledge this perspective during perspective-taking?

Dissertation Outline

Our main research question is addressed in the individual chapters of this dissertation, each chapter focusing on either perceivers' perspective-taking during language production (**Chapter 2** and **3**) or language comprehension (**Chapter 4** and **5**). In addition, we examine whether perceivers are more likely to adopt another person's vantage point (**Chapter 2**), and to make more accurate predictions of another person's perspective (**Chapter 3** to **5**) when they are made aware of another person's attentional and knowledge status. Hence, each individual chapter will address a specific research question that is outlined below:

- **Chapter 2**: To what extent does an explicit focus on another person's point of view promote (visual) perspective-taking?
- **Chapter 3**: To what extent does speakers' referential communication benefit from an explicit focus on addressees' perspective?
- **Chapter 4**: To what extent does an explicit focus on another person's perspective influence readers' perspective-taking accuracy?
- **Chapter 5**: To what extent does feedback improve perceivers' perspective-taking accuracy?

As will become apparent throughout this thesis, the manner by which the perceivers in this dissertation are explicitly instructed to pay attention to another person's perspective becomes progressively more explicit from study to study. More specifically, we test whether perceivers' perspective-taking benefits from an explicit and repeated attention to another person's knowledge and attentional status when this perspective is highlighted either before (**Chapter 2** and **3**), during (**Chapter 3** and **4**) and after (**Chapter 5**) perceivers estimated another person's perspective. These questions are examined in four individual studies, presented in **Chapters 2** to **5**. Each chapter reports on an individual study that either has been published or is under review as a full paper in international peer-reviewed journals. Although the chapters are connected to one another, they are self-contained

in the sense that they each consist out of an abstract, theoretical and methodological framework, discussion and reference list. In the final chapter, (**Chapter 6**), we present a general discussion and conclusion with regards to this dissertation as a whole. Below, we will briefly discuss the methodological and statistical approach of this dissertation before we discuss the research questions addressed in each individual chapter into further detail.

Methodological and Statistical Approach

A secondary theme of this dissertation is to contribute to the discussion that scientific research should be transparent and accessible in such a way that research findings can be replicated and verified by independent researchers (see Zwaan, Etz, Lucas & Donnellan, 2018). Hence, in this dissertation, we examined our research questions by independently replicating the experimental designs of influential studies in the field of perspective-taking, and by subsequently adapting and extending these designs to fit our research's purpose. Moreover, two of the replication studies presented in this dissertation (**Chapter 3** and **4**) are preregistered in the Open Science Framework (osf.io), and the data of all four studies are available on the OSF platform.

The individual chapters in in this thesis each report inferential statistical analyses consisting of parametric and/or non-parametric tests, and – if the experimental design allowed for it – random mixed effects analyses to take into account the individual variation of the participants and stimuli. Any existing differences in the manner in which the statistical analyses are reported are due to the different focuses of the scientific journals to which the research was submitted. The statistical analyses are always documented as a replication recipe to aid the replicability of the research that is conducted in this dissertation (Zwaan et al., 2018).

Current Studies

Chapter 2. In this chapter, we present the first study that examines whether perceivers can be stimulated to inhibit their (predominant) egocentric frame of reference by adopting another person's point of view. We argue that an explicit focus on other-related information rather than on self-related information should attenuate the adoption of an egocentric anchor during spatial perspective-taking (Tversky & Hard, 2009). We assess this assumption in two experiments. In the first experiment, we replicate the experimental design of Todd, Hanko, Galinsky and Mussweiler (2011), and test whether perceivers primed to acknowledge self-other differences in a prior task are more likely to adopt another person's visual perspective in a subsequent spatial perspective-taking task than perceivers

primed to acknowledge self-other similarities. In the second experiment, we extend Todd et al.'s (2011) experimental design by intensifying perceivers' awareness of perspective-differences. In this experiment, perceivers are explicitly instructed to acknowledge another person's viewpoint during the spatial perspective-taking task, and we test whether these explicit instructions to acknowledge another frame of reference helps perceivers to inhibit an egocentric interpretation by adopting this other vantage point.

Chapter 3. In our second study, we investigate perceivers' perspective-taking during a task in which perspective-taking has been argued to be essential for communicative success. Hence, our second study focuses on perceivers' perspective-taking accuracy and examines how an explicit attention to another person's perspective affects this accuracy. We invite speakers and addressees to take part in a collaborative referential communication game, replicated and extended from Kaland, Krahmer and Swerts (2011, 2014). Speakers are instructed to refer to common-ground objects in such a way that their addressee can select the intended object. We focus on speakers' tendency to engage in accurate perspective-taking by construing referential messages that optimally adhere to their addressees' informational need. We assess the extent to which speakers are likely to leak information that is privileged to them (e.g., Wardlow Lane, Groisman, & Ferreira, 2006) while they refer to an object in common-ground. We further question whether explicit and repeated instructions to focus on addressees' knowledge and attentional status before speakers produce a referential expression affects speakers' reference production. More specifically, we test the assumption that explicit attention on addressees' perspective will help speakers to inhibit the leakage of privileged information, thereby stimulating them to construe a message that is optimally tailored to the addressees' perspective.

Chapter 4. The third study also addresses perceivers' perspective-taking accuracy, but now focuses on perspective-taking during language comprehension. Moreover, we delve deeper into the explanation as to why egocentric biases might prevail by examining perceivers' egocentric anchoring and adjustment during perspective-taking. In a reading task, perceivers now judge a protagonist's perspective when the difference in perspectives is less clear and, hence, the situation is more ambiguous. We investigate whether and how an explicit focus on a protagonist's perspective affects perceivers' egocentric anchoring and adjustment both prior and during reading. We try to achieve this aim by replicating and extending the experimental design of Keysar (1994) in Experiment 1 and of Epley, Keysar, Van Boven and Gilovich's (2004) in Experiment 2. In both experiments, we assess perceivers' tendency to overestimate the similarity between their perspective and the perspective of protagonists in a story. Both experiments are extended by explicitly instructing readers to focus on the information that is accessible and, thus, known by the protagonists before judging this protagonists' perspective. In doing so, we test whether the accessibility of

other-related information by explicit instructions to focus on protagonists' perspective prior (Experiment 1) and during (Experiment 2) reading diminishes perceivers' egocentric projection during language comprehension.

Chapter 5. The three previous chapters all focused on stimulating perceivers' attention on another person's knowledge and attentional status before they estimated this person's perspective. In this study, we examine whether perceivers can learn to predict another person's perspective more accurately when they are confronted with the inaccuracy of their judgments. We replicate and extend our third study by providing perceivers with the opportunity to learn from their egocentric projection mistakes. That is, we highlight another person's perspective by presenting perceivers with performance feedback about their perspective-taking accuracy. We further test whether perceivers are more likely to learn through this feedback if they are explicitly informed they have made an error and why their prediction is inaccurate (explicit feedback) than when they have to derive the inaccuracy of their prediction from a description of the protagonists' true perspective (implicit feedback).

Chapter 2

Changing Views:
The Effect of Explicit Perception-Focus Instructions on Perspective-Taking

This chapter is based on:

Damen, D., Van Amelsvoort, M., Van der Wijst, P., & Krahmer, E. (2019). Changing views: The effect of explicit perception-focus instructions on perspective-taking. *Journal of Cognitive Psychology, 31*(3), 353–369. The anonymized data are accessible via osf.io/by47d/

Abstract

In two experiments, we examined whether explicit attention to another's perspective fosters perspective-taking. In the first experiment, we attempted to replicate previous findings showing that a mind-set focusing on self-other differences incites speakers to adopt another's viewpoint in a subsequent task. However, our results showed that speakers focusing on self-other differences were just as likely to describe an object's location from their egocentric perspective as speakers focusing on self-other similarities. In the second experiment, we intensified speakers' awareness of perspectives by explicitly instructing them to regard their own (self-focus) or another's (other-focus) viewpoint during the perspective-taking task. Participants allocated to the baseline did not receive explicit focus instructions. Findings revealed that other-focused speakers were more likely to adopt another's perspective than self-focused speakers. However, compared to the baseline, an explicit other-focus did not foster perspective-taking. We conclude that an explicit awareness of perspective differences does not attenuate speakers' egocentricity bias.

Keywords: perspective-taking; self-other differences; egocentricity bias; experimental research.

Introduction

Many things in social life rely on our ability to imagine ourselves in another person's shoes. Whether we buy a present for our beloved partner, communicate to friends or colleagues, or bargain at a local market, we often imagine how others view the world around them so that we are able to fulfil our common social needs. Although perspective-taking is entrenched in all our daily activities, this does not imply that all perspective-taking acts are actually successful.

A large body of research paints a conflicting picture with regard to communicators' ability to spontaneously represent another person's perspective. On the one hand, studies argue that interlocutors rapidly integrate another person's perspective during communication (Brown-Schmidt, Gunlogson, & Tanenhaus, 2008; Brown-Schmidt & Hanna 2011; Brown-Schmidt & Tanenhaus, 2008; Hanna, Tanenhaus, & Trueswell, 2003; Hanna & Tanenhaus, 2004; Heller, Grodner, & Tanenhaus, 2008; Heller, Gorman, & Tanenhaus, 2012; Nadig & Sedivy, 2002), especially when mutual understanding is in danger of being jeopardized (Mainwaring, Tversky, Ohgishi, Schiano, 2003; Schober, 1993). In support of this view, studies have shown that speakers are able to automatically (unconsciously and unintentionally in Schneider, Slaughter, & Dux, 2017) process what (Qureshi, Apperly, & Samson, 2010; Samson, Apperly, Braithwaite, Andrews, & Bodley-Scott, 2010; Schneider, Nott, & Dux, 2014; Surtees & Apperly, 2012; Surtees, Samon, & Apperly, 2016; Shurz et al., 2015), and *how* others represent the world around them (Elekes, Varga, & Király, 2016; Tversky & Hard, 2009). In contrast to these findings, other research suggests that perspective-taking does not always occur automatically or spontaneously. These studies have shown that communicators' egocentric perspective often has primacy (Apperly, Back, Samson, & France, 2008; Epley, Morewedge, & Keysar, 2004; Ferguson, Apperly, & Cane, 2017; Keysar, Lin & Barr, 2003), and that even in situations that require explicit perspective-taking communicators often fail to accurately regard their interlocutor's perspective (Damen, Van der Wijst, Van Amelsvoort, & Krahmer, 2019, **Chapter 3**; Horton & Keysar, 1996; Kaland, Krahmer, & Swerts, 2014; Wardlow Lane, Ferreira, 2008; Wardlow Lane, Groisman, & Ferreira, 2006; Wardlow Lane & Liersch, 2012). These failed attempts at perspective-taking are argued to be the result of egocentric-intrusion effects (Apperly et al., 2010; Ferguson et al., 2017; Samson et al., 2010) during which communicators find it difficult to inhibit their egocentric representation when trying to represent the perspective of others. Egocentric intrusions are explained by the communicators' egocentricity bias (Barr & Keysar, 2005; Birch & Bloom, 2007; Epley, Keysar, Van Boven, & Gilovich, 2004; Keysar, Barr, & Horton, 1998; Krueger & Clement, 1994). According to this bias, the ease by which communicators have access to their own perspective - in contrast to the

impermeable nature of the other's mind - makes communicators likely to anchor their perspective-judgments on their egocentric representation. This leads to instances in which communicators might falsely project (Ames, 2005) their own perspective onto others, thereby failing to appreciate the other's potentially different vantage point.

In two experiments, we investigate communicators' tendency to engage in spontaneous perspective-taking and question how we can stimulate communicators to inhibit an egocentric interpretation by adopting another person's perspective. In the first experiment, we build on the assumption that a clear distinction between the self and the other incites perceivers to spontaneously adopt another person's point of view (Decety & Sommerville, 2003; Mitchell, 2009; Santiesteban et al., 2012; Todd, Hanko, Galinsky, & Mussweiler, 2011). Under this assumption, we follow Todd et al.'s (2011) predictions that perceivers are more likely to take another person's perspective if they are (made) aware that this person's representation of the world differs from their own. In the second experiment, we explore the extent to which perceivers' explicit focus on another person's viewpoint might help them to inhibit an egocentric interpretation.

The Self-Other Distinction and Perspective-Taking

Prior research has shown that a feeling of similarity rather than dissimilarity between the self and the other is positively related to interpersonal attraction and attitude and behavioral change. Not only are we more attracted to others who think alike (Byrne, 1961; Byrne & Nelson, 1965; Festinger, 1954; Stroebe, Insko, Thompson, & Layton, 1971), we are also more likely to positively evaluate experiences we share with similar rather than dissimilar others (Boothby, Smith, Clark, & Bargh, 2017). People who share our opinions and attitudes are also found to be more persuasive (Brown & Reingen, 1987; Simons, Berkowitz, & Moyer, 1970), and we are more likely to help others who are like us (Maner et al., 2002). Research has argued that feelings of similarity might even help us to better understand others (Stotland, 1969; Elfenbein & Ambady, 2002). However, these feelings of similarity between the self and the other cause a sense of self-other overlap (Aron, Aron, Tudor, & Nelson, 1991) that might not be beneficial for accurate perspective-taking (see also Cheek, 2015). That is, being able to imagine the feelings and beliefs of others requires that people recognize that the 'the self' and 'the other' are still two distinct and unique identities (Decety & Sommerville, 2003) who do not necessarily share perspectives. When people fail to realize self-other differences, they may falsely believe that a similarity between their thoughts and feelings and those of others exists. False beliefs of similarity can thus lead to instances in which people wrongly assume that others perceive the world as they do, causing them to inaccurately project the self onto (dissimilar) others (Mitchell,

2009; Mitchell, Macrae, & Banaji, 2006; Mussweiler, 2003; Santiesteban et al., 2012; Savitsky et al., 2011; Simpson & Todd, 2017; Todd et al., 2011). Following the egocentric anchoring and adjustment approach to perspective-taking (Epley, Keysar, Van Boven, & Gilovich, 2004), if perceivers experience a (false) sense of similarity, they might see no reason to adjust for their initial egocentric interpretation and may thus fail to realize that others can have a representation that differs from their own. In this case, people's interpretation of the other's perspective will be anchored on an egocentric interpretation. Perspective-taking, in the true sense of the word, does not occur. To prevent egocentric anchoring, perceivers need to be aware that self-projection is inappropriate (Ames, 2004b; Krueger & Clement, 1994), and it has been suggested that one way to do so is by raising perceivers' awareness that differences in mental representations do exist (Santiesteban et al., 2012; Todd et al., 2011).

Research by Todd and his colleagues (2011) has shown that perceivers are likely to adjust away from an egocentric interpretation if they see themselves and others as being unique and distinct. Their research showed that visually priming perceivers with a mind-set that focuses on visual differences between pictures resulted in – spontaneously - acknowledging differences in perspectives in a subsequent spatial perspective-taking task. That is, those primed with a cognitive orientation to acknowledge self-other differences rather than self-other similarities were more inclined to adopt another person's perspective. Todd et al. (2011) achieved these cognitive orientations by asking participants to complete a picture-comparison task (following Mussweiler, 2001) prior to the spatial perspective-taking task. During this picture-comparison task, participants noted down either the differences (priming a difference-mind-set) or the similarities (priming a similarity-mind-set) between pairs of pictures. According to Todd et al. (2011), acknowledging the differences or similarities between the picture-pairs translated to participants also acknowledging the differences in perspectives. That is, in Todd et al. (2011) first experiment, participants primed with a mind-set focusing on differences were more likely to locate an object from another person's visual perspective than the participants who were primed to focus on similarities. In subsequent experiments, participants' focusing on self-other differences were less likely to project their privileged knowledge about a communicative intention (Experiment 2) and about an object's location (Experiment 3) to an uninformed other than those who were primed to focus on self-other similarities. The activation of self-other differences in perceivers' mental representation thus seemed to reduce egocentrism and to stimulate perspective-taking. In this study, we investigate whether we can replicate Todd et al.'s findings (2011). In particular, we examine the question whether a picture-comparison task prior to the spatial perspective-taking task activates a mind-set that stimulates spatial perspective-taking.

EXPERIMENT 1

In the first experiment, we investigated whether a primed difference-mind-set incites participants to spontaneously adopt the visual perspective of another person. For this, we directly replicated the experimental design of Todd et al.'s (2011) first experiment, and asked participants to take part in a spatial perspective-taking task (Tversky & Hard, 2009). During this task, participants described the location of an object that could be located on the basis of participants' own spatial perspective or from the perspective of another person. This task taps into spontaneous perspective-taking because it measures individuals' *propensity* to adopt another person's frame of reference without being explicitly instructed to do so. Object locations that orient the object from the other person's perspective show that people appreciate the unique vantage point of this person[1], and thereby prioritize this vantage point over their predominant egocentric frame of reference (see also Tversky & Hard, 2009). As in Todd et al. (2011), we predicted that the participants primed with a difference mind-set would be less influenced by their egocentric perspective and thus more likely to adopt an other-oriented perspective than participants primed with a similarity mind-set or participants in a control condition.

In addition to the replication of Todd et al. (2011), this study also takes into account possible individual differences that might exist with regard to individuals' ability and propensity to engage in perspective-taking. A large body of research has shown that people differ in the extent to which they have the social and cognitive capacity to engage in perspective-taking (e.g., Baron-Cohen et al., 2001a, 2001b; Brunyé et al., 2012; Bukowski & Samson, 2017; Ryskin, Benjamin, Tullis, & Brown-Schmidt, 2015; Wardlow, 2013). For instance, whereas first was believed that especially people with developmental disabilities, such as autism spectrum disorders, were "poor" perspective-takers, it is increasingly acknowledged that the characteristics associated with the autism spectrum can even be found in the non-clinical population at large (e.g., Baron-Cohen et al., 2001a, 2001b; Brunyé et al., 2012). Furthermore, research showed that individuals differ in the extent to which they have the cognitive capacity to perform the cognitive tasks that are associated with (accurate) perspective-taking performance (e.g., Bukowski & Samson, 2017; Ryskin, Benjamin, Tullis, & Brown-Schmidt, 2015; Wardlow, 2013). That is, individuals who have a higher cognitive capacity to direct their attention to relevant perspective-information (i.e., working memory), or those who are more able to inhibit their egocentric perspective (i.e., inhibitory control) outperform individuals with a lower working memory and/or inhibitory

[1] This has also been referred to as "level-2" visual perspective-taking (see Flavell, Everett, Croft, & Flavell, 1981).

control capacity (e.g., Bukowski & Samson, 2017; Brown-Schmidt, 2009; Carlson & Moses, 2001; Carlson, Moses, & Claxton, 2004; Lin, Keysar, & Epley, 2010; Nilsen & Graham, 2009; Wardlow. 2013). In this study, we therefore anticipate the existing individual differences in perspective taking by increasing the sample size of the original study and by measuring individuals' perspective-taking propensity (self-report), and their ability to engage in spatial (Huttenlocher & Presson, 1973) perspective-taking (Baron-Cohen et al., 2001b).

Method

Participants and Design

128 participants (50 more than in the original study) were recruited from the university and randomly assigned to one of the three conditions (difference-mind-set, similarity-mind-set, or control condition). Participants gave their informed consent to partake in the study. The data of four participants were excluded from the analysis, due to an error in the experimental procedure (N = 2), or due to them guessing the actual purpose of the experiment during the debriefing (N = 2). This resulted in 43 participants in the difference-mind-set condition, 39 in the similarity-mind-set condition and 42 in the control condition. The age of the participants ranged from 17 to 36 years (M = 21.55; SD = 3.28), and the majority of participants (72%) was female.

Procedure and Materials

Priming a Difference or Similarity Mind-Set. The priming materials used by Todd et al. (2011) were obtained from Todd (p.c.) and translated into Dutch. On entering the lab, participants took place in private cubicles and were told that they were participating in a study investigating the effectiveness of several experimental stimuli. Participants were blind to the experimental conditions. To prime participants with either a difference- or similarity-mind-set, we replicated the picture-comparison task from Todd et al. (2011). In this task, participants compared four pairs of pictures of drawn houses and listed either three differences (difference-mind-set) or three similarities (similarity-mind-set) between each presented pair. Participants in the control condition were only confronted with four singular pictures of drawn houses for which they were asked to describe them by listing three attributes for each picture. An example of a trial used during the picture comparison task is presented in Figure 1. As in Todd et al. (2011), participants received the priming materials in booklets. The singular pictures (control) and picture-pairs (difference- and similarity-mind-set) were presented on separate pages.

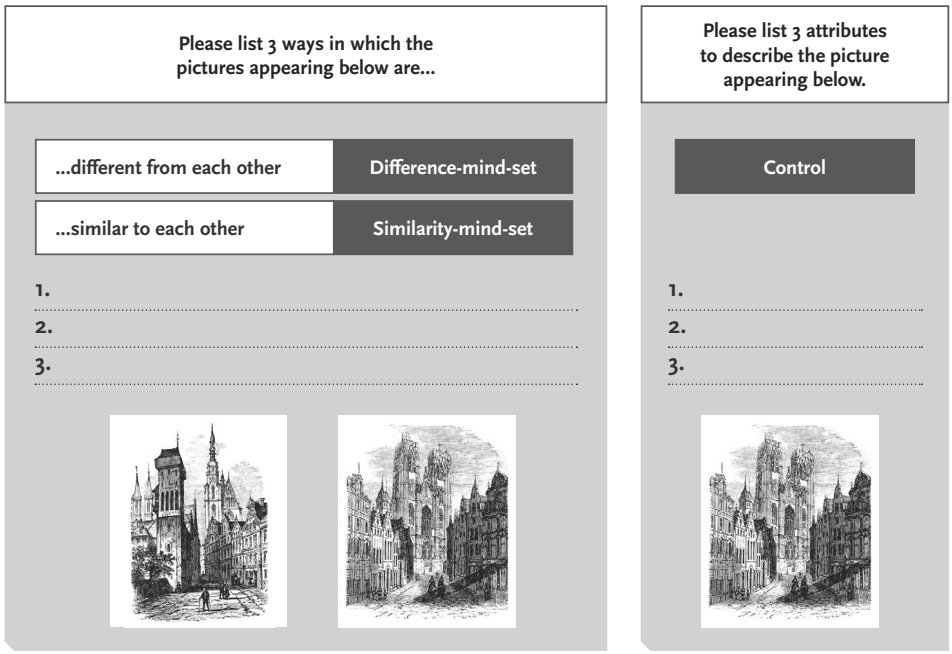


Figure 1. An example of a picture-pair shown to participants during the picture-comparison task (Todd et al., 2011). Due to copyright, the example portrays dummy pictures instead of the original ones. Participants listed either the three similarities (evoking a similarity-mind-set) or three differences (evoking a difference-mind-set) between the two pictures. In the control condition, only one picture of the pair was shown and participants listed three attributes that described that picture.

Spatial Perspective-Taking. After the priming task, participants completed a spatial perspective-taking task on the computer screen in front of them. In this task, participants were shown a photographed scene of a man seated behind a table facing the participants (Figure 2). We re-enacted Todd et al. (2011) visual scene, because we wanted to use different versions of this scene in Experiment 2. On the table, a book and bottle were placed using a clear left and right distinction. Participants answered five filler questions about the picture. These questions were asked by the computer and participants typed in their answer in response boxes. The filler questions were translated from Todd et al. (2011) and asked participants to comment on other properties of the picture unrelated to perspectives, such as "How would you judge the brightness of this picture?" and "How old would you say the man is?". Among these filler questions, participants answered the target question "On what side of the table is the book?". As in Todd et al. (2011), this target question measured participants' perspective-taking in a single trial. Participants' answers to the target question were coded according to the guidelines set by Todd et al. (2011), and Tversky and Hard (2009). We coded participants' answers in terms of the perspective

Figure 2. The photographed scene in the spatial perspective-taking task. Participants indicated on what side the book on the table was placed.

they mentioned first and scored answers that located the book from participants' own viewpoint ("right side") as self-oriented responses (0), and answers that located the book from the man's viewpoint ("left side") as other-oriented responses (1). Descriptions that fit in neither category (e.g., "at the top" or "in the middle") were excluded ($N_{difference\text{-}mind\text{-}set} = 6$; $N_{control} = 4$). As in the original study, participants completed this task without time pressure.

On top of replicating the experimental procedure of Todd et al. (2011), we administered three subsequent tasks that measured participants' (self-reported) perspective-taking, their mental rotation ability and their ability to engage in perspective-taking. This way, we were able to account for possible underlying mechanisms that could influence perceivers' spatial perspective-taking, without harming the replication study.

Self-Reported Perspective-Taking. We assessed participants' (self-reported) tendency to regard the man's perspective by six items. Participants indicated how much they agreed with the declarative sentences (e.g., "I generally tried to imagine how the man in the picture looked at the situation") on a 7-point scale (1 = strongly disagree; 7 = strongly agree). The scale had a high reliability ($\alpha = .77$), and the items represented a one-dimensional scale with all factors loading above .40 (see Table 1).

Table 1. Items of the Self-Reported Perspective-Taking Scale for Experiment 1 and Experiment 2

Item	Factor Loading	
	Experiment 1	Experiment 2
1. During the task, I mainly took into account my own view of the situation (R)	.76	.76
2. During the task, I found it difficult to put myself into the position of the man presented in the picture (R)	.42	.58
3. While answering the questions, I generally tried to imagine how the man in the picture looked at the situation	.73	.73
4. I performed the task from my own point of view as much as possible	.85	.84
5. During the task, I was especially aware of how the objects in the picture appeared to me (R)	.76	.78
6. I was aware that the man in the picture had a different view of the situation than I	.53	.38*

Note: The (R) signals that the scores were recoded before the analysis. *We chose to maintain this item, since Cronbach's Alpha only improved from .78 to .80 if this item was deleted from the scale.

Mental Rotation Ability. Being able to imagine how an object appears to an observer has been argued to depend largely on one's ability to mentally rotate the object in question (Huttenlocher & Presson, 1973), especially when observers look at the object from an angular disparity from 90 degrees and onwards (Roberts & Aman, 1993). To account for the possible influence of participants' mental rotation ability on their propensity to regard the spatial perspective of another person, participants took part in a shortened version (24 experimental and 8 practice trials) of Cooper and Shepard's (1973) mental rotation task. This task was administered on the computer, using E-prime version 2. Participants indicated whether visual displays of a letter (R) and a number (2) were presented normally or reflected. Participants were first shown the identity of the visual display (i.e., R/2) during a 2 second time span, followed by a visual display of the orientation the letter or number would later appear in during a 700 millisecond timespan. We based this 700 millisecond time span on the findings by Cooper and Shepard (1973) who showed that between 700 and 1000 milliseconds the difficulty to represent the normal or reflected objects disappears. The visual displays were presented in orientation degrees ranging from 0 to 360 degrees (Figure 3). Since a 180-degree rotation increases error rate and determination time (Cooper & Shepard, 1973), we included four practice trials in which the designated object was rotated 180 degrees away from its initial upright position, two practice trials depicting a 60-degree orientation, and two practice trials in which the target did not depart from the initial orientation (0 degrees). In the experimental trials, items were presented in a

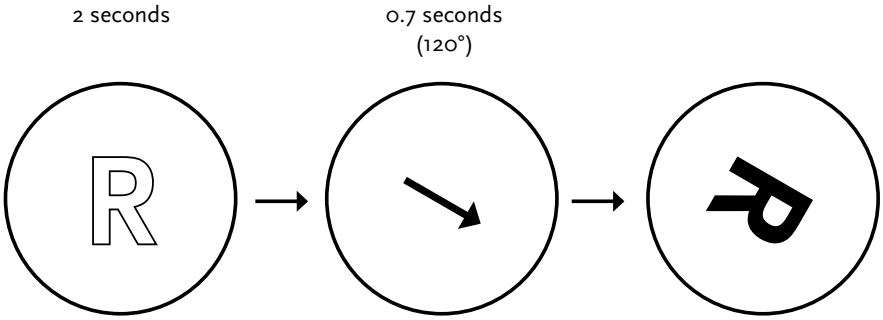


Figure 3. Example of a trial from the mental rotation task (Cooper & Shepard, 1973). Participants were first shown the identity of the visual display, followed by the orientation this display would later appear in, followed by the target stimulus. Participants indicated whether the target was presented normally or reflected.

randomized order and the orientation degrees were equally represented. Whether the target was presented as a number or letter, and whether the designated object would appear normal or reflected was balanced across all trials. Participants were instructed to respond as quickly and as accurately as possible. Participants' mental rotation proficiency was estimated by calculating their overall error rate (in proportions). Overall, participants were accurate on 80% of the trials ($M_{control}$ = 0.79, *SD* = 0.02; $M_{similarity\text{-}mind\text{-}set}$ = 0.80, *SD* = 0.02; $M_{difference\text{-}mind\text{-}set}$ = 0.82, *SD* = 0.02).

Autism-Spectrum Quotient Scale. Previous research indicated that people vary in their social and cognitive ability to engage in perspective-taking (e.g., Baron-Cohen et al., 2001a, 2001b; Brunyé et al., 2012; Wardlow, 2013). The Autism-Spectrum Quotient Scale (AQ; Baron-Cohen et al., 2001b) is a validated and reliable scale that measures these individual differences in perspective-taking ability. As a final step in the experimental procedure, we asked participants to respond to an abridged and Dutch translated version of the AQ construed and validated by Hoekstra and colleagues (2011). In this way, we were able to account for the possible influence of individual differences in perspective-taking ability on subsequent perspective-taking behavior. The abridged version consisted out of 28 declarative sentences (e.g., "Reading a story, I find it difficult to work out the character's intention") that were measured on a 5-point scale (1= strongly disagree; 5 = strongly agree). Higher values indicated that participants had a low social and cognitive ability to engage in perspective-taking. The AQ had a very good internal consistency (α = .89). After filling out the AQ-Short, participants' demographics were collected. Afterwards, participants were debriefed and thanked for their participation. Participants were rewarded by course credits. The ethics review committee of the Tilburg School of Humanities and Digital Sciences has approved this experiment to be in full compliance with the relevant codes of experimentation and legislation.

Results

The dataset of this experiment can be accessed via osf.io/by47d. In Table 2, the mean proportions of other-oriented location descriptions in the original Todd et al. (2011) study and in our replication study are presented. The proportions of other-oriented responses did not differ much between the control (*M* = .39, *SD* = .50), similarity-mind-set (*M* = .31, *SD* = .47) and difference-mind-set (*M* = .27, *SD* = .45) conditions. The participants in the difference-mind-set condition in our replication study, however, were two times less likely to produce an other-oriented response, than those participants in the original study (*M* = .62, *SD* = .50).

Table 2. Mean Proportions of Other-Oriented Location Descriptions as a Function of Condition

Condition	Other-Oriented Responses	
	Todd et al. (2011)	Experiment 1
Control	.34 (.48)	.39 (.50)
Similarity-mind-set	.27 (.45)	.31 (.47)
Difference-mind-set	.62 (.50)	.27 (.45)

Note: Standard deviations are presented in parentheses.

Todd and his colleagues (2011) performed a one-way analysis of variance (ANOVA) with planned comparisons to investigate the influence of the primed mind-sets on the probability of an other-oriented location description to occur. We replicated this method of analysis and did not find a significant main effect of condition, $F(2, 111) = 0.69$, $p = .503$. Participants with a difference-mind-set (*M* = .27, *SD* = .45) were just as likely to provide a location description that was oriented from the perspective of the man in the photograph as the participants with a similarity-mind-set (*M* = .31, *SD* = .47), $t(111) = 0.35$, $p = .365$, and the participants in the control condition (*M* = .39, *SD* = .50), $t(111) = 1.14$, $p = .128$. Participants' propensity to provide an other-oriented location description also did not differ between the control and similarity-mind-set condition, $t(111) = 0.81$, $p = .210$.

Moderation Analysis. To investigate whether participants' mental rotation (MR) and perspective-taking abilities reflected by their score on the autism-quotient (AQ) moderated the relationship between the primed mind-sets and participants' perspective-taking behavior, a moderation analysis was performed using the procedures developed by Hayes and Preacher (Hayes, 2013; Hayes & Preacher, 2014). For this analysis, we employed Hayes' PROCESS Procedure for SPSS. We construed a conceptual model (model

2) in which the primed mind-set was entered as a predictor to generated other-oriented location descriptions, and participants' AQ and MR scores were entered as moderators. We dummy coded our predictors so that the difference-mind-set condition was used as the reference category to which the other two conditions were contrasted. We therefore construed two models, the first exploring the association between the difference- and similarity-mind-set conditions on the probability of other-oriented responses to be given (D_i), and the second exploring the relationship between difference-mind-set and control condition on the probability of participants providing an other-oriented location description (D_j). We corrected for multiple tests by employing the Bonferroni correction. This entails that we used a *p*-value criterion of .025 to reject our null hypothesis (Hayes & Preacher, 2014). The bootstrapped confidence intervals were obtained over 10.000 iterations, and predictors were centered before the analysis.

The results of the one-way ANOVA were reflected in the PROCESS analyses (see Table 3) as the mind-set condition did not have a direct effect on other-oriented location descriptions (b_i = 0.15, *SE* = 0.52, *z* = 0.29, *p* = .770, 95% BCa CI [-0.86, 1.16]; b_j = -0.42, *SE* = 0.52, *z* = -0.81, *p* = .420, 95% BCa CI [-1.44, 0.60]). Further, participants' score did not moderate the relationship between the primed mind-set and the occurrence of other-oriented responses (b_i = -0.28, *SE* = 1.47, *z* = -0.19, *p* = .848, 95% BCa CI [-3.17, 2.61]; b_j = 0.18, *SE* = 1.25, *z* = 0.14, *p* = .887, 95% BCa CI [-2.27, 2.63]), nor did their mental rotation ability (b_i = 1.55, *SE* = 4.21, *z* = 0.37, *p* = .713, 95% BCa CI [-6.70, 9.79]; b_j = 3.03, *SE* = 4.59, *z* = 0.66, *p* = .510, 95% BCa CI [-5.97, 12.02]).

Table 3. Parameter Estimates of Mind-Set on Other-Oriented Responses Moderated by AQ and MR

	b	SE_b	*z*	*95% CI*
D_i				
Constant	-0.91	0.26	-3.52	-1.41, -0.40
Mind-Set	0.15	0.52	0.29	-0.86, 1.16
AQ	0.29	0.73	0.40	-1.14, 1.73
AQ x Mind-Set	-0.28	1.47	-0.19	-3.17, 2.61
MR	-2.01	2.09	-0.96	-6.11, 2.09
MR x Mind-Set	1.55	4.21	0.37	-6.70, 9.79
D_j				
Constant	-0.74	0.26	-2.85	-1.25, -0.23
Mind-Set	-0.42	0.52	-0.81	-1.44, 0.60
AQ	0.35	0.62	0.56	-0.86, 1.56
AQ x Mind-Set	0.18	1.25	0.14	-2.27, 2.63
MR	-4.33	2.29	-1.89	-8.83, 0.16
MR x Mind-Set	3.03	4.59	0.66	-5.97, 12.02

Note: D_i compares difference-mind-set to similarity-mind-set condition, D_j compares control to difference-mind-set condition.

Participants' Perspective-Taking Tendency. After the spatial perspective-taking task, participants reported the extent to which they regarded the man's perspective during the task. Participants in the control (M = 3.92, SD = 1.23), similarity- (M = 3.85, SD = 0.99) and difference-mind-set (M = 3.85, SD = 1.16) condition reported the same perspective-taking tendency, $F(2, 121) = .04$, $p = .958$. A follow-up logistic regression revealed that participants' self-reported perspective-taking tendency did, however, significantly predict their behavior during the spatial perspective-taking task (b = .84, SE = .21, $p < .001$, 95% CI [0.46, 1.39]), representing a positive association (see Table 4). As participants' perspective-taking increased, so did the likelihood of them providing an other-oriented response that located the book from the man's perspective.

Table 4. Parameter Estimates of the Model Predicting Other-Oriented Responses from Participants' Self-Reported Perspective-Taking Tendency (PT)

	b	SE_b	*95% CI*
Experiment 1			
Constant	-4.12	0.89	
PT	0.84**	0.21	0.46, 1.39
Experiment 2			
Constant	-5.32	1.27	
PT	1.05**	0.28	0.62, 1.73

Note: Confidence intervals were estimated by bootstrapping over 5.000 iterations. Experiment 1: Model $\chi^2(1) = 19.78$, $p < .001$, $R^2 = .16$ (Cox & Snell), .22 (Nagelkerke); Experiment 2: Model $\chi^2(1) = 19.20$, $p < .001$, $R^2 = .23$ (Cox & Snell), .32 (Nagelkerke), $^{**}p < .001$.

Intermediate Discussion

The first experiment investigated whether a mind-set that affords a focus on self-other differences rather than self-other similarities stimulates perceptual perspective-taking. We replicated the perceptual perspective-taking experiment of Todd and colleagues (2011, Experiment 1) and tried to evoke participants' difference-mind-set by employing the visual priming method. Whereas Todd and his colleagues found that priming participants with a mind-set that focuses on differences rather than similarities increased the likelihood of participants adopting another person's visual perspective, we did not replicate this finding. In our experiment, participants with a primed difference-mind-set were just as likely to provide a location description that oriented the target object from another's visual perspective as participants without or with a primed similarity-mind-set. The results from participants' self-reported perspective-taking tendency and its positive correlation to actual

perspective-taking behavior strengthen these findings. Participants who reported that they had regarded the man's perspective during the spatial perspective-taking task had also been more likely to locate the book from the man's perspective. Interestingly, regardless of the activation of a self-other difference-, self-other similarity- or no mind-set, these self-reported tendencies did not differ between the three conditions. This strengthens the conclusion that the picture-comparison task did not influence participants' propensity to adopt another person's viewpoint. This replication study also showed that perspective-taking was not dependent on perceivers' social and cognitive ability to regard others' perspectives (AQ-score) nor by their ability to mentally represent and rotate objects.

Our first experiment did not replicate the finding that priming perceivers with a cognitive orientation that focuses on self-other differences rather than on self-other similarities or no particular mind-set stimulates them to spontaneously adopt another person's perspective. What factors could have contributed to the failed replication? This study directly replicated the experimental procedure and materials from Todd and his colleagues (2011), leaving no differences in how the visual priming method and spatial perspective-taking task were administered. In addition, similar to the original study, we conducted our replication among a student sample (with also a majority of female students). Although Todd et al. (2011) did not report the age-range of their sample, we do not expect there to be (large) age differences between our undergraduate sample and the undergraduate sample of the original study. To our knowledge, the only difference between the original study and its replication is the cultural background of the participating students. Todd et al. (2011) conducted their study among German undergraduates, whereas we invited Dutch undergraduates to participate in the study. Research has shown that cross-cultural differences might explain differences in perspective-taking (e.g., Chopik, O'Brien, & Konrath, 2017; Kessler, Cao, O'Shea, & Wang, 2014; Wu & Keysar, 2007). However, as the samples used in the original and replication study have a similar cultural context (e.g., House et al., 2004), we do not expect that this difference explains the failed replication.

As in Todd et al. (2011), remember that we elicited the mind-sets by the picture-comparison task in which participants noted down either the differences or similarities between picture-pairs. We could question whether this visual priming method elicited different cognitive representations or, if the priming method did elicit participants' awareness of self-other differences, this awareness did not influence subsequent perspective-taking. The findings of participants' self-report seem to propose that the visual priming method did not elicit differences in participants' awareness that another perspective existed. That is, not only was participants' awareness of the man's perspective low (around the midpoint of the scale), this awareness did also not differ between the three conditions (control, difference-mind-set, similarity-mind-set). The ineffectiveness of the visual priming method on fostering

perspective-taking has recently been supported by Eyal, Steffel and Epley (2018). In Eyal et al.'s Experiment 8, 113 participants filled out Todd et al.'s (2011) picture-comparison task before they completed an emotion recognition task (DANVA for faces, Nowicki & Duke, 1994). Not only were participants' accuracy scores overall low (< 20%), mean accuracy scores between the control (*M* = 18.38, *SD* = 2.67), similarity-mind-set (*M* = 18.57, *SD* = 2.63) and difference-mind-set (*M* =18.50, *SD* = 2.25) conditions did not differ. These findings combined support the argument that the visual priming method did not stimulate an awareness of differences in perspectives, thereby reducing the likelihood that another perspective was adopted.

It is likely that the visual priming method does not stimulate spontaneous perspective-taking, because this priming method is unrelated to the perspective-taking task that follows it. If participants had been put into a difference-mind-set, participants were not made aware that they could apply this notion of dissimilarity to subsequent perspective-taking. We question whether perspective-taking is stimulated when this priming method is *explicitly* related to the perspective-taking task. In particular, we question whether raising communicators' awareness of a different *spatial* perspective does foster spatial perspective-taking, especially when this awareness is raised during the perspective-taking task itself.

EXPERIMENT 2

The second experiment examined whether explicit instructions to acknowledge another person's viewpoint might serve as a better stimulant to incite perceivers to adopt this person's perspective. We argue that communicators are more likely to adopt another person's perspective once they become explicitly aware of this person's divergent vantage point. For this, we build on the assumption that perspective-taking might benefit from communicators' ability to inhibit an egocentric representation. Following this argument, making other-related information more accessible than self-related information should attenuate the adoption of an egocentric anchor. Indeed, recent studies have shown that highlighting certain aspects of an agent's perspective facilitates the processing of this perspective (Elekes, Varga, & Kiraly, 2016), and that instructing communicators to process an altercentric perspective makes them more likely to inhibit their egocentric representation (Ferguson et al., 2017; Samuel et al. 2019). In the same vein, an experimental study investigating how consumers process ambiguous reviews, Naylor and colleagues (2011) found that highlighting consumers' attention to think about others increased the accessibility of other-related information, thereby reducing consumers tendency to overestimate the similarity between themselves and these others (Naylor, Lamberton, & Norton, 2011). Building on these findings, we argue that drawing communicators' explicit attention to an altercentric perspective will make

them more likely to adopt this perspective. In particular, we expect that explicit instructions to acknowledge another person's different vantage point helps perceivers to inhibit their egocentric perspective when locating an object that is presented before them and this person. In contrast, we expect that explicitly instructing perceivers to acknowledge their egocentric perspective will make them less likely to inhibit an egocentric interpretation when judging an object's location. To test these hypotheses, we replicated the previous experiment and intensified the perspective-awareness manipulation. Instead of visually priming self-other differences prior to the spatial perspective-taking, we raised perceivers' awareness of self-other differences by explicitly instructing them to regard another person's viewpoint during the spatial perspective-taking task. We explored the extent to which these explicit instructions stimulated perceivers to step in another person's shoes.

Method

Participants and Design

We recruited 80 participants from the university and randomly assigned them to one of the two perspective conditions (self-focus, other-focus). For the control condition, we used the data of the 42 participants that were recruited during Experiment 1. Participants gave their informed consent before partaking in the study. The data of two participants participating in either the other-focus and self-focus condition were excluded from the analysis, due to them having prior knowledge about the actual purpose of the experiment. The age of the remaining 120 participants (32 males, 88 females, $N_{self\text{-}focus}$ = 38, $N_{other\text{-}focus}$ = 40, $N_{control}$ = 42) ranged from 17 to 36 (M = 21.39; SD = 3.01).

Procedure and Materials

We replicated the procedure from the first experiment with one important difference: instead of priming participants with a difference- or similarity-mind-set before the spatial perspective-taking task, we explicitly stimulated participants' self- versus other-focus during the task itself. At the start of the experiment, all participants filled out a control version of the picture-comparison task. For this control version, participants described four singular pictures of drawn houses by listing three attributes for each picture. Hereafter, the spatial perspective-taking task was administered (see Figure 2). However, before participants indicated the location of the book, they answered four explicit perception questions that were embedded among fillers. Participants were explicitly instructed to indicate how objects appeared to themselves (self-focus) or to the man in the photograph (other-focus) (see Table 5).

Table 5. The Objects, Scenes and Object-Rotations Used for the Explicit Perception Instructions

	Object	Rotation	Option 1	Option 2
Laptop		Front/Back		
Picture frame		Front/Back		
Lamp		Left/Right		
Mug		Left/Right		

Note: Participants in the self-focus condition indicated how the object appeared to them, whereas participants in the other-focused condition indicated how the object appeared to the man in the photograph. Participants indicated their choice by selecting the option that depicted the right orientation of the object.

For example, the first question presented participants with a scene in which a man looked at a laptop placed before him. Below this picture, participants saw two pictures of the laptop: one showing the laptop from the front (option 1) and one showing the laptop from the back (option 2). Participants in the self-focus condition answered the explicit self-perception question: “How does the laptop appear to you?”, whereas participants in the other-focus condition answered the explicit other-perception question: “How does the laptop appear to the man in the picture?”. Participants selected the option that depicted the laptop in the right rotation. To ensure the intrusiveness of the perspective-awareness training, we chose two different object rotations. Two objects could be distinguished by a clear front versus back

rotation (i.e., laptop and picture frame), and two objects could be distinguished by a clear left versus right rotation (i.e., lamp and mug). If participants chose the wrong option, they had to answer the question again. Participants who answered the questions more than two times incorrectly were not asked to re-answer the question again, but they were forwarded to the rest of the questionnaire. To disallow routineness, we scrambled the options for the repeated questions. Afterwards, participants indicated the location of the book. We repeated the coding procedure from the first experiment and excluded four responses ($n_{self\text{-}focus}$ = 1, $n_{other\text{-}focus}$ = 3, $n_{control}$ = 4) that located the book "in the middle" or "on the upper side".

Training Performance. The low error-rate (n = 5) across both perspective-focus conditions showed that the explicit perception instructions helped participants to acknowledge the situation from their own or from the man's point of view. Interestingly, perspective-focus errors mainly occurred in the other-focused condition in which participants indicated how the objects appeared to the man in the picture ($n_{other\text{-}focus}$ = 4), in comparison to the self-focused condition in which they indicated how the objects appeared to themselves ($n_{self\text{-}focus}$ = 1). One participant in the self-focused condition made the same error (for the same object) twice, whereas the other participants only made the error once.

The spatial perspective-taking task was followed by recording participants' self-reported perspective-taking tendency (α = .78; see Table 1), and their mental rotation (MR) and perspective-taking (AQ, α = .91) abilities. In the MR task, participants gave accurate responses on 80% of the trials ($M_{other\text{-}focus}$ = 0.80, SD = 0.02; $M_{self\text{-}focus}$ = 0.83, SD = 0.02).

After collecting their demographics, participants were thanked, debriefed and given a small remuneration for their participation. The ethics review committee of the Tilburg School of Humanities and Digital Sciences has approved this experiment to be in full compliance with the relevant codes of experimentation and legislation.

Results

The dataset of this experiment can be accessed via osf.io/by47d. The mean proportions of other-oriented location descriptions as a function of the perspective-focus condition are presented in Table 6. Participants oriented the object's location from the

Table 6. Mean Percentage of Other-Oriented Location Descriptions as a Function of Condition

Dependent Variable	Condition		
	Control	Self-Focus	Other-Focus
Other-oriented descriptions	.39 (.50)	.16 (.37)	.51 (.51)

man's perspective the most in the other-focus condition (*M* = .51, *SD* = .51), followed by the control (*M* = .39, *SD* = .50) and the self-focus (*M* = .16, *SD* = .37) conditions.

Moderation Analysis. As in the first experiment, we construed a conceptual model (PROCESS model 2) that investigated the relationship between the perspective-focus condition and the other-oriented responses, while controlling for participants' mental rotation (MR) and perspective-taking (AQ) abilities. We dummy coded our predictors and construed three models: control vs. self-focus (D_i), control vs. other-focus (D_j), self-focus vs. other-focus (D_k). We employed the Bonferroni correction to correct for multiple tests ($\alpha \leq .017$). The parameter estimates of the conceptual models are presented in Table 7. Results showed that the direct effect of the explicit self- versus other-focus on oriented responses was significant (b_k = 1.69, *SE* = .59, *z* = 2.84, *p* = .005, 95% BCa CI [0.52, 2.85]). However, the direct effect of the control versus self-focus condition (b_i = -1.56, *SE* = 0.82, *z* = -1.89, *p* = .0585, 95% BCa CI [-3.17, 0.06]), and the control versus other-focus condition (b_j = 0.11, *SE* = 0.71, *z* = 0.15, *p* = .880, 95% BCa CI [-1.28, 1.49]) on other-oriented responses were both non-significant.

Table 7. Parameter Estimates of Condition on Other-Oriented Responses Moderated by AQ and MR

	b	SE_b	*z*	*95%*
D_i				
Constant	-1.18	0.41	-2.90	-1.98, -0.38
Condition	-1.56	0.82	-1.89	-3.17, 0.06
AQ	0.37	0.58	0.65	-0.76, 1.51
AQ x Condition	0.23	1.17	0.19	-2.06, 2.51
MR	-1.08	2.80	-0.39	-6.57. 4.41
MR x Condition	9.62	5.63	1.71	-1.41, 20.65
D_j				
Constant	-0.31	0.35	-0.88	-0.99, 0.38
Condition	0.11	0.71	0.15	-1.28, 1.49
AQ	0.40	0.53	0.75	-0.64, 1.44
AQ x Condition	0.27	1.08	0.25	-1.84, 2.39
MR	-3.46	2.07	-1.67	-7.52, 0.60
MR x Condition	4.87	4.11	1.18	-3.20, 12.93
D_k				
Constant	-0.89	0.30	-2.99	-1.48, -0.31
Condition	**1.69**	**0.59**	**2.84**	**0.52, 2.85**
AQ	0.51	0.74	0.70	-0.93, 1.95
AQ x Condition	0.05	1.47	0.03	-2.83, 2.93
MR	1.45	2.67	0.54	-3.77, 6.67
MR x Condition	-4.75	5.29	-0.90	-15.13, 5.62

Note: D_i compares control to self-focus condition, D_j compares control to other-focus condition, and D_k compares self-focus to other-focus condition. Significant results are presented in bold.

A follow-up analysis in which we compared the self-focus and other-focus conditions revealed that other-focused participants (M = .51, SD = .51) were 5.4 times more likely to provide a location description that oriented the book from the man's perspective, than self-focused participants (M = 0.16, SD = .37), $\chi^2(1) = 10.21$, p = .001, representing a medium association (*Cramer's V* = .37).

Participants' AQ score did not moderate the relationship between the explicit perspective-focus condition and the occurrence of other-oriented responses (b_i = 0.23, SE = 1.17, z = 0.19, p = .847, 95% BCa CI [-2.06, 2.51]; b_j = 0.27, SE = 1.08, z = 0.25, p = .800, 95% BCa CI [-1.84, 2.39]; b_k = 0.05, SE = 1.47, z = 0.03, p = .973, 95% BCa CI [-2.83, 2.93]), nor did participants' mental rotation ability (b_i = 9.62, SE = 5.63, z = 1.71, p = .087, 95% BCa CI [-1.41, 20.65]; b_j = 4.87, SE = 4.11, z = 1.18, p = .237, 95% BCa CI [-3.20, 12.93]; b_k = -4.75, SE = 5.29, z = -0.90, p = .369, 95% BCa CI [-15.13, 5.62]).

Participants' Perspective-Taking Tendency. Participants' self-reported perspective-taking tendency significantly differed between the three conditions, Welch's $F(2, 75.92)$ = 49.79, p < .001. Bonferroni post-hoc comparisons revealed that self-focused participants (M = 3.37, SD = .68) reported a significant lower perspective-taking tendency than the other-focused participants (M = 5.15, SD = .88), p < .001, and the participants in the control condition (M = 3.92, SD = 1.23), p = .04. Participants in the control condition also reported a lower perspective-taking tendency than the other-focused participants, p < .001.

In a follow-up logistics regression analysis, we examined the extent to which the self-reported perspective-taking tendency of the other-focused and self-focused participants predicted their perspective-taking during the spatial perspective-taking task. The analysis revealed a significant positive relation between participants' perspective-taking tendency and other-oriented location descriptions (b = 1.05, SE = 0.28, p < .001, 95% CI [0.62, 1.73]; see Table 4).

Discussion

The second experiment investigated the influence of explicit perception instructions on perceivers' tendency to adopt another person's viewpoint. Results showed that perceivers who were explicitly instructed to acknowledge another person's perspective were more likely to spontaneously adopt this person's perspective than those stimulated to be self-focused. Other-focused participants also reported a higher perspective-taking tendency than self-focused and control participants, and this tendency was positively correlated to actual perspective-taking behavior. Those with a higher self-reported perspective-taking tendency had also been more likely to adopt another person's viewpoint.

Interestingly, in the control condition in which perceivers did not receive explicit self- or other-perception instructions, the majority (61%) located the object on the basis of their own spatial perspective. Explicit other-focus instructions did not decrease this egocentric anchoring tendency. That is, participants in the control condition were just as likely to provide an other-oriented spatial description as other-focused and self-focused participants. This finding supports the argument that an egocentric approach is the most natural response while judging social situations (e.g., Ames, 2005; Epley et al., 2004; Levelt, 1989 as cited in Schober, 1993; Tversky & Hard, 2009). This experiment showed that enhancing the accessibility and, thus, saliency of another person's different perspective compared to a baseline in which this increase was absent did not reduce egocentric anchoring during spatial perspective-taking.

General Discussion

Although perspective-taking is considered to be a central component for successful social functioning, research showed that perceivers' often fail to accurately acknowledge another person's different vantage point. One important reason as to why perceivers often do not engage in perspective-taking is because their egocentric perception biases (Birch & Bloom, 2007; Keysar, Barr, & Horton, 1998) their perspective-judgments. That is, the accessibility of perceivers' private cognitions causes perceivers to first interpret the other's perspective on the basis of their own and, subsequently, adjust away from this egocentric interpretation in cognitive effortful steps (Epley et al., 2004). However, because perceivers' private perspective is most salient, perceivers find it hard to adjust their egocentrism in order to form an interpretation that more accurately reflects another person's perspective. Perspective-adjustments are, therefore, often insufficient, resulting in perceivers being likely to judge social situations from their own perspective instead of from someone else's.

The current study investigated what perceivers need in order to overcome this egocentric anchoring during spatial perspective-taking. In the first experiment, we built on the assumption that perceivers' awareness of differences in perspectives (Decety & Sommerville, 2003; Santiesteban et al., 2012; Todd et al., 2011) might stimulate them to describe an object location from another person's vantage point. We addressed this assumption by directly replicating the experimental design of Todd and his colleagues (2011). In particular, we tested Todd et al.'s (2011) hypothesis that perceivers primed with a cognitive orientation to draw a distinction between the self and the other would be more likely to engage in perspective-taking than the perceivers primed with a cognitive orientation to focus on similarities between the self and the other. Our findings did not

support this hypothesis and thereby failed to replicate Todd et al. (2011). In contrast to Todd et al. (2011), the individuals in our study were all very likely to locate the object from their own spatial perspective, regardless of their primed cognitive orientation. This failed replication falls in line with recent findings by Eyal and colleagues (2018) who also administered Todd et al.'s (2011) priming method in order to stimulate perspective-taking. In line with our findings, Eyal et al. (2018) showed that a primed difference-mind-set - in contrast to a primed similarity-mind-set and a baseline – did not stimulate individuals' recognition of emotional expressions (DANVA; Nowicki, & Duke, 1994). Hence, these findings underline the importance of (direct) replications in order to further our understanding of the phenomenon being examined (e.g., Moonesinghe, Khoury, and Janssens, 2007; Open Science Collaboration, 2012; Simons, 2014; Zwaan, Etz, Lucas, & Donnellan, 2018).

The second experiment investigated whether perceivers' explicit awareness of another person's perspective fosters perspective-taking. We addressed this question by explicitly instructing perceivers to regard visual scenes from the perspective of a photographed man before they located an object that was placed before the man in the picture. Before perceivers located the target object, they were explicitly instructed to acknowledge the man's different perspective (other-focus condition) or to acknowledge their egocentric viewpoint (self-focus condition). Findings showed that other-focused perceivers were more likely to inhibit an egocentric interpretation and to locate the object from the man's perspective than self-focused perceivers. However, the findings of the second experiment also show that the majority of perceivers were very likely to locate the object from their egocentric spatial perspective. That is, when the spatial responses were contrasted to the control condition in which perceivers did not receive explicit perception instructions, explicit other-focus instructions did not increase perceivers' perspective-taking, nor did the explicit self-focus instructions reduce perceivers' perspective-taking.

Important to note is that the spatial perspective-taking task administered in this study tapped into *spontaneous* perspective-taking. That is, we examined the extent to which perceivers spontaneously let go of their predominant egocentric frame of reference (Tversky & Hard, 2009) by adopting another's frame of reference because they had been primed with a specific cognitive orientation (Experiment 1) or because they had been primed to focus on a different perspective (Experiment 2). In this sense, the spatial perspective-taking task differed from tasks that measure perspective-taking accuracy (such as the Director Task; Epley et al., 2004) in which egocentric responses are termed to be inaccurate. In the spatial perspective-taking task administered in this study, egocentric responses were not termed to be inaccurate nor were altercentric responses termed to be accurate. Egocentric responses in the spatial perspective-taking task showed that people stuck with their predominant frame of reference, whereas altercentric responses showed

that people inhibited an egocentric response and prioritized another frame of reference by adopting this frame (spontaneously).

In addition, even though the majority of speakers used their egocentric perspective as a spatial anchor point to describe the object's location, this does not imply that these perceivers did not recognize that the object, from another person's perspective, was located on the other side of the table. For instance, most of participants' location descriptions not only included a specific reference point (e.g., "on *my* right side", "on the left side of *the man in the picture*"), participants' responses sometimes also included both perspectives (e.g., "on my right side, but on the left side of the man in the picture"). Recall that we coded the perspective that was mentioned first by participants (following Todd et al. (2011), and Tversky & Hard (2009). However, answers that included both participants' self- as the other's perspective clearly indicated that participants were aware that different *perspectives* were at stake while describing the object's location. In addition, the low error rate of the perspective-awareness training in the second experiment clearly indicates that speakers were able to regard the situation from the other person's vantage point. The explicit other-focus instructions during the perspective-awareness training thus helped perceivers to acknowledge the other person's different spatial perspective. However, when we contrast the two perspective-focus condition to the baseline, it appears that an explicit focus on another person's different perspective did not influence how speakers would actually describe the situation that is presented before them. In addition, findings of both experiments show that perceivers across the various conditions (difference-mind-set, similarity-mind-set, self-focus, other-focus and control) were very likely to locate the object from their egocentric spatial perspective. These results support previous findings that speakers prefer to describe spatial relations from their egocentric point of view (Levelt, 1989 as cited in Schober, 1993; Tversky & Hard, 2009). Our findings further suggest that enhancing speakers' attention to another person's different vantage point does not stimulate spontaneous perspective-taking.

It could be questioned whether an explicit awareness of self-other perception differences encourages perspective-taking when the demand to engage in perspective-taking is emphasized by the task. In the experiments presented in this study, inferring the other person's point of view was not a crucial component for successfully completing the task. It could be the case that an explicit awareness of self-other differences encourages perspective-taking behavior when it is necessary to adopt another person's perspective. In addition, some social situations cause perceivers to be fixed onto their own egocentric interpretations and beliefs (Thompson, Nadler, Lount, 2006). Especially in the case of interpersonal conflict, interlocutors find it hard to let go of pre-established beliefs and to allow for an interpretation that is in line with another person's perspective. Results of the

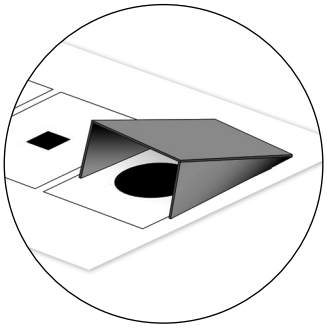
second experiment seem to suggest that instructing to take over the perspective of their counterpart might stimulate disputants to see past their initial egocentric interpretation. In this sense, explicit instructions, could not only lead to a more correct understanding of self-other differences, but they might also stimulate disputants to see similarities - of which they first thought they did not exist - between themselves and their counterpart. Especially in the case of conflict, failing to see similarities in viewpoints, thoughts and wishes reduces disputants' chance to resolve their conflict (Thompson et al., 2006). Future research might investigate how explicit perspective-taking instructions might help interlocutors to update existing false-beliefs. Answers to this interesting question will shed more light on the precise workings of the self-other mechanism that underlies perspective-taking.

Conclusion

The findings of the two studies presented in this chapter support the existence of perceivers' egocentricity bias and its robustness. Neither a primed mind-set focusing on self-other differences (Experiment 1) nor explicit and repeated instructions to acknowledge the visual perspective of another person (Experiment 2) reduced participants' egocentric anchoring tendency. Speakers were very likely to interpret the situation from their own visual perspective, even though they were made aware of the other person's different point of view.

Chapter 3

Perspective-Taking in Referential Communication: Does Stimulated Attention to Addressees' Perspective Influence Speakers' Reference Production?



This chapter is based on:

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Abstract

In two experiments, we investigated whether speakers' referential communication benefits from an explicit focus on addressees' perspective. Dyads took part in a referential communication game and were allocated to one of three experimental settings. Each of these settings elicited a different perspective mind-set (baseline, self-focus, other-focus). In the two perspective settings, speakers were explicitly instructed to regard their addressees' (other-focus) or their own (self-focus) perspective before construing their referential message. Results evidenced speakers' egocentricity bias. Even though speakers were explicitly aware of addressees' informational need, speakers still referred to information not known to their addressee. Speakers' self-reported perspective-taking behavior correlated with their actual reference behavior. Those who reported to have regarded addressees' perspective were also less likely to have leaked information about their own knowledge and attentional state. Findings are discussed in light of speakers' egocentricity bias and the role of speaker-addressee collaboration in language production.

Keywords: perspective-taking; referential communication; egocentricity bias; privileged information.

Introduction

Engaging in successful referential communication implies that addressees are able to select the intended referent on the basis of speakers' descriptions. For this, speakers are expected to design their message optimally (i.e., audience design in Clark & Murphy, 1982) so that it adheres to addressees' informational need (Clark, 1992; Clark & Carlson, 1982). Speakers are supposed to exchange just the right amount of information – neither too little nor too much – (Grice, 1975) and base their contributions on the knowledge, beliefs and assumptions that are shared or salient between themselves and their addressee (i.e., common-ground information; Clark & Marshall, 1981). This is necessary, because addressees will rely on this shared, salient knowledge when interpreting speakers' reference (Arnold et al., 2013). Referential communication thus relies a great deal on interlocutors' ability to accurately engage in the process of perspective-taking; the ability to take into account the knowledge and attentional state of their interaction partner at each step in the conversation. The questions that arise here are whether interlocutors are inclined to regard the other's perspective accurately during interaction, and if this is not the case, whether a stimulated attention to another's perspective would be beneficial for the referential communication process.

The literature shows a contradictory picture with regard to interlocutors' ability and propensity to regard the other's perspective during referential communication. On the one hand, it is argued that interlocutors are successful perspective-takers. In support of this view, studies have shown that addressees (e.g., Brown-Schmidt, Gunlogson, & Tanenhaus, 2008; Brown-Schmidt & Hanna, 2011; Brown-Schmidt & Tanenhaus, 2008; Hanna, Tanenhaus, & Trueswell, 2003; Hanna & Tanenhaus, 2004; Heller et al., 2008) as well as speakers (e.g., Heller et al., 2012; Nadig & Sedivy, 2002) are able to rapidly integrate common-ground information (including their interlocutor's perspective) from a very early stage in language comprehension and production processes. On the other hand, studies have shown that common-ground information (including the other's perspective) is sometimes not fully integrated in the early stages of language processing, but only when interlocutors detect perspective-errors (e.g., Horton & Keysar, 1996; Keysar, Barr & Horton, 1998; Keysar et al., 2000; Keysar, Lin & Barr, 2003). According to this view, interlocutors sometimes rely more on information that is not shared, but *privileged* to themselves, thereby disregarding the other person's knowledge and attentional status (e.g., Keysar et al., 1998, 2000). These studies suggest that the production and comprehension of referential utterances are not necessarily constrained by the needs of the other person in the interaction, but more by one's own knowledge and attentional status, resulting in perspective-judgments that are primarily based on information that is immediately accessible to oneself. In this sense, the

other person's knowledge is only considered in a later, optional stage in which interlocutors can choose whether to adjust their judgments to the common-ground status or not (Horton & Keysar, 1996). These latter studies provide arguments for an interlocutor's egocentricity bias (e.g., Keysar et al., 1998), demonstrating how interlocutors use their own mental state as a representational default to infer the one of their interaction partner (Epley et al., 2004). Engaging in perspective-taking is then considered to be a cognitive effortful process that can result in egocentric judgments when interlocutors do not correct their automatic response. Combining these two opposing views, research suggests that interlocutors integrate *both* common- and privileged-ground information during language processing, but that differences in interlocutors' ability to inhibit their egocentric perspective might explain why egocentric biases sometimes prevail (e.g., Barr, 2008; Brown-Schmidt, 2009). Research has even shown that interlocutors are able to *switch* between visual self- and other perspectives, although perspective-switching requires cognitive effort and irrelevant perspectives might still interfere with accurate perspective-judgments (e.g., Apperly et al., 2010; Ferguson, Apperly, & Cane, 2017; Samson et al., 2010). Additionally, a more recent study sketches an even less pessimistic view on interlocutors' egocentrism by suggesting that interlocutors might be flexibly egocentric (Samuel, Roehr-Brackin, Jelbert, & Clayton, 2019). Although an egocentric perspective may still have primacy, findings by Samuel et al. (2019) showed that once interlocutors adopted the other person's perspective, they had a hard time to switch back to their egocentric default interpretation. Altogether, these findings suggest that interlocutors might be successful perspective-takers once they learn to inhibit an egocentric interpretation. Failure to suppress one's own knowledge and attentional status during referential communication might lead to instances in which addressees sometimes select objects that are not visible to speakers (Apperly et al., 2010; Keysar et al., 2000; Keysar et al., 2003; Legg et al., 2017), and speakers sometimes refer to information not known to their addressee (Horton & Keysar, 1996) or even leak privileged information that should have stayed confidential (e.g., Kaland et al., 2011, 2014; Wardlow Lane et al., 2006).

In a referential communication task, Wardlow Lane et al. (2006) evidenced speakers' informational leakage even when it had negative consequences. During the task, speakers described geometrical objects to their addressee, with the goal of earning both of them points if the addressee correctly identified the referent. Before every description, speakers hid one object from their addressees' view. On critical trials, this object always differed in size from the target object speakers had to describe. On control trials, the hidden object differed in shape from the target object. Addressees could earn additional points by correctly guessing the identity of the hidden object. Although speakers were instructed not to let their addressee gain additional points, results showed that, on critical trials, speakers were very likely to cue the identity of their privileged object by referring to the

size contrast they themselves were seeing. For instance, when speakers were instructed to hide a large square from addressees' view and were subsequently asked to describe the target object that depicted a smaller square, speakers were very likely to indicate the size contrast they themselves were seeing by referring to the target as "*the small square*". As addressees were only confronted with one square in common-ground, speakers' inclusion of the size property of the target object was irrelevant and thus redundant. From addressees' perspective, speakers' reference thus contained too much information (Grice, 1975), enabling addressees to use this redundant information to correctly guess the identity of speakers' privileged object (Wardlow Lane & Liersch, 2012).

Subsequent studies replicated findings of Wardlow Lane et al. (2006) by showing that speakers especially leak privileged information when this leakage is informative (Kaland et al., 2011, 2014; Wardlow Lane & Liersch, 2012). Recall that on the critical trials in Wardlow Lane et al. (2006), speakers' privileged object (a large triangle) and target object (a small triangle) were similarly shaped, but differed in size. On control trials, however, the privileged object (a small square) and the target (a small triangle) were presented in similar sizes, but in different shapes. This means that, only on the critical trials the target object's size was meaningful to speakers. Therefore, on critical rather than on control trials, the contrast presented was salient to speakers, stimulating them to refer to the object's size. Since referring to the size of objects is only relevant in relation to other objects, speakers did not refer to the size contrast on control trials. In a study that was inspired by Wardlow Lane et al. (2006), Kaland and colleagues (2011, 2014) presented speakers with size contrast on all trials. Kaland et al. (2011, 2014) showed that speakers were more likely to leak information about the target object's size when the contrast between their privileged object and the target was meaningful (on salient trials: large triangle, small triangle) than when this was not the case (on non-salient trials: large square, small triangle). That is, speakers were more likely to refer to the "*small triangle*" when it was contrasted to a larger triangle (salient trials) than to a larger square (non-salient trials) in speakers' privileged-ground. The informativeness of the size-contrast on critical rather than on control trials boosted its salience to speakers, making it more likely that speakers would refer to it. Kaland et al. further showed that this boost in salience did not affect object features that are inherently salient (such as color). That is, speakers were not more likely to refer to the object's color when the color-contrast was informative (blue triangle, red triangle) than when it was not (blue square, red triangle).

Speakers are also very likely to leak information non-verbally, (Kaland et al., 2011, 2014), and especially when they do not have enough cognitive resources left to correct perspective mistakes (Wardlow Lane & Ferreira, 2008). Intriguingly, speakers are even more likely to refer to privileged information when they are motivated to keep it confidential. The motivation to keep private information privileged further enhances its salience which,

as a consequence, can ironically (Wegner, 1994) result in a stronger tendency of it being revealed (Wardlow Lane & Liersch, 2012). It seems that despite their efforts, speakers are not always able to monitor for perspective mistakes or to adjust their egocentric errors to addressees' informational need. This raises the question whether speakers' audience design would benefit from a constant reminder of addressees' perspective.

Research investigating the influence of perspective-taking on reference comprehension has shown that addressees are less influenced by their privileged perspective if they are instructed to take the speaker's perspective (Ferguson, Apperly, & Cane, 2017). With regard to reference production, research suggests that speakers are more likely to engage in an accurate audience design if they are (made) aware that such design is needed. For instance, evidence from eyetracking research shows that interlocutors engaging in perspective-taking are less influenced by privileged perspectives (Ferguson, Apperly, & Cane, 2017), and speakers who requested information from their addressee rather than informed them were more likely to adjust their references to the perspective of the addressee (Yoon et al., 2012), as were the speakers who had an interdependent- rather than an independent focus deriving from their cultural background (Wu & Keysar, 2007). Furthermore, research has shown that speakers learn through repeated experience how their references should be adapted to the informational need of the addressee (Horton & Gerrig, 2002). In Horton and Gerrig (2002), speakers gained their experience by receiving feedback from their addressee about the informativeness of their reference. By means of this feedback, addressees cued the knowledge they required from their speaker. This is an interesting finding that also seems to suggest that speakers' audience design benefits from an explicit attention to their addressees' knowledge and attentional state. If speakers are able to adjust their reference production to their addressees' perspective through repeated experience (Horton & Gerrig, 2002), what will happen to speakers' audience design if they explicitly attend to addressees' informational need before they even start producing their reference? Guiding speakers through an explicit perspective-taking process before reference production might inhibit egocentric anchoring, and might stimulate speakers to monitor for perspective mistakes. This might incite speakers to correct for egocentric errors such as the leakage of privileged information (Horton & Keysar, 1996), resulting in references that are more accurately based on addressees' perspective, and less on speakers' own knowledge and attentional state.

From a pragmatic point of view, it is interesting to investigate whether previously found egocentric errors (e.g., Horton & Keysar, 1996; Kaland et al., 2011, 2014; Wardlow Lane et al., 2006, 2008, 2012), can be countered by an explicit mental activation of the others' informational need. What if interlocutors in the abovementioned studies were made *explicitly* aware of the others' perspective, would their reference production still be influenced by privileged information? This question is also interesting for social practices

that try to enhance perspective-taking during social interaction, using explicit perspective-taking instructions (e.g., Brown, 1997; Brown, 2010; Fleuridas, Nelson, & Rosenthal, 1986; Penn, 1982; Selvini-Palazzoli et al., 1980; Tomm, 1985). This research provides a first step in investigating the fundamental role these explicit perspective-taking instructions can play during perspective-taking, and, in particular, the extent to which these explicit perspective-taking instructions stimulate speakers' audience design during reference production.

The Current Research

Two experimental studies examine the question whether speakers' referential communication benefits from an explicit focus on addressees' perspective. This question is investigated among student dyads taking part in a referential communication game in which they were randomly assigned the role of the speaker or addressee. In both experiments, we test the hypothesis that speakers are less likely to refer to information privileged to them when they are explicitly instructed to regard addressees' perspective than when these explicit perspective-taking instructions are absent. The second experiment intensifies the perspective-manipulation used in Experiment 1 and investigates the role of speakers' self- versus other-awareness during perspective-taking. Results of both studies provide more insight in the role perspective-taking processes play during the production of referential descriptions, thereby providing further insights into when and how common-ground information is incorporated in the process of language production (e.g., Bezuidenhout, 2013; Horton & Gerrig, 2002).

EXPERIMENT 1

Experiment 1 investigates whether speakers' elicited attention to addressees' perspective influences their reference production. Following the assumptions of the egocentricity hypothesis (Keysar et al., 1998), speakers in a natural communicative setting (i.e., baseline) are likely to anchor their referential expressions to their own knowledge and attentional state, increasing the likelihood they will refer to information that is privileged to them. We therefore expect that other-focused speakers, i.e., whose attention is explicitly focused on their interlocutor's perspective, will be less likely to refer to privileged information than the speakers in a baseline setting. Furthermore, since speakers are expected to be naturally biased to anchor their reference production to their own perspective, we hypothesize that self-focused speakers, i.e., who are made explicitly

aware of their own perspective, are even more likely to refer to privileged information than speakers referring in a baseline setting.

In addition to these expectations, we hypothesize that speakers' egocentric anchoring will be influenced by the salience of speakers' privileged information. As Wardlow Lane et al. (2006) and Kaland et al. (2011) have shown, the salience of speakers' privileged knowledge can incite speakers to unintentionally refer to information they want to keep concealed. It is thus expected that, overall, speakers will refer more to privileged information when this information is salient versus non-salient to them. Since self-focused speakers are explicitly instructed to pay attention to the information that is available to themselves, we expect that these speakers will be influenced more by the salience of their private information than speakers without an induced perspective-focus (i.e., baseline). Compared to the speakers communicating in the baseline setting, we thus hypothesize that self-focused speakers will be more likely to leak privileged information when this information is salient than non-salient to them. This in contrast to the speakers with a stimulated other-focus. Since other-focused speakers are explicitly instructed to focus their attention on their addressees' perspective, we expect that these speakers will be influenced less by the salience of their own knowledge and attentional state. That is, in contrast to speakers in the baseline setting, we hypothesize that other-focused speakers will be less likely to leak privileged information, regardless of its salience.

Method

Participants

In total, 93 student-dyads (N = 186) participated in this study. The data of three dyads were excluded from analyses, due to an error in the experimental procedure (N = 2), or due to a low proficiency in the language of the experiment (Dutch) (N = 4). The analyses were thus based on 90 dyads in which the participants were randomly assigned either the role of the speaker (55 women, 35 men, M_{age} = 22.0 years; age range 18-34 years) or the role of the addressee (59 women, 31 men, M_{age} = 21.3 years; age range 17-27). All participants were fluent in Dutch, did not experience problems at discerning the colors used in the study, and received course credits for their participation.

Design

The experimental design and procedure were replicated from Kaland and his colleagues (2011, 2014), which in turn were inspired by Wardlow Lane et al. (2006). The

experiment consisted of a collaborative referential communication task in which speakers were asked to describe mutually visible geometrical figures in such a way that the addressee could indicate the intended one out of a set of four. Speakers and addressees were seated across from each other at a table. Out of these four figures, three were visible to both addressees and speakers, and one was privileged to speakers and thus hidden from addressees' view. The three mutually visible figures were differently shaped (e.g., circle, triangle, square) and could thus be distinguished by mentioning its shape. A schematic of the four figures was physically presented on the table in front of both interlocutors. The same schematic was depicted on speakers' private computer screen. From their private computer screen, speakers were instructed to block one figure from their addressee's view and, subsequently, to identify another figure that was mutually visible to both speaker and addressee on the table in front of them (Figure 1). The occluded figure differed either in size or in color from the three mutually visible figures. In our experiment, we replicated Kaland and his colleagues' (2011, 2014) privileged situation and added a perspective-taking manipulation. In this privileged setting, one object was always blocked from addressee's view and thus belonged to speaker's privileged ground.

Figure 1. The experimental setting in which the speaker (on the bottom) identified the target figures to the addressee (on the top).

Materials

Eliciting Self- versus Other-Focus. Speakers' self- versus other-focus was manipulated by either asking them explicitly to regard their own (self-focus) or their addressees' (other-focus) perspective before they identified the target object. Participants were randomly assigned to one of the three communication settings (self-focus, other-focus, baseline), resulting in 30 speakers per setting. The self- versus other-focus was operationalized by asking speakers to answer a perspective question portrayed on the computer screen next to them. In the self-focus setting, speakers answered the question reinforcing their egocentric perspective: "Which four figures are visible to you?". This in contrast to the speakers in the other-focus setting who were asked to regard the perspective of their addressee: "Which three figures are visible to your addressee?". Speakers answered the question by selecting the figures on their private computer screen. To eliminate the possibility that the self-focused speakers would simply select all figures as a response to the question, a fifth figure was added to the schematic presented on the computer screen. The fifth figure's position and shape were balanced across all trials. To examine how speakers' reference production in the self- versus other-focused setting diverged from a baseline situation, we allocated one third of the speakers to a setting in which a self- versus other-focus was not reinforced.

Salience of Privileged Information. Participants were confronted with 40 trials, consisting of 20 salient and 20 non-salient trials. In the salient trials, speakers' privileged figure was similarly shaped to the target figure to be identified, but differed from this target on one feature (size/color). In the non-salient trials, the privileged figure could be distinguished on two features (shape, size/color). For example, when the target figure depicted a small triangle, it could be contrasted to a privileged large triangle (salient trial) or to a privileged large circle (non-salient trial). The relation between the hidden and privileged figure is thus more salient when they differ on only one feature than on two. Successive figures were not similarly shaped, and half of the figures contrasted in size (large, small) and the other half in color (red, blue, green, black, grey, yellow). When the contrast was presented in size (small privileged triangle, large target triangle), all figures contained the same color. In turn, when the contrast was presented in color (red privileged triangle, blue target triangle), all figures contained the same size. We replicated the number of features (size/color) on which speakers' privileged and the target figure showed a contrast, so that we were able to adhere as closely as possible to the original experimental design of Kaland et al. (2011, 2014). The figures' shape, color, and position in the four-card schematic were balanced across all trials. This resulted in a 3 x 2 x 2 design, with the communicative setting (self-focus, other-focus, baseline) as a between subjects factor, and trial type (salient, non-salient), and contrast type (color, size) as within subjects factors.

Procedure

A role of the dice decided which participant took the role of the speaker. Participants were told that, when the addressee was able to correctly identify the target figure, both the speaker and the addressee would obtain one point. Participants were further told that failing to identify the target figure would result in zero points obtained, and that the goal of the game was to obtain the maximum number of points. Speakers and addressees were completely free during the interaction and did not receive additional instructions on how to play the game. That is, speakers were not told how to structure their reference and we left it to the addressees to decide whether they wanted to provide feedback.

Speakers and addressees sat down on opposite sides of a table. Speakers were seated next to a computer screen on which the experimental trials were presented using E-Prime version 2. At the beginning of each trial, addressees closed their eyes while the experimenter placed four cards on the table. Addressees' eyes remained closed until the speaker identified the target. When the four cards were put in place, speakers (a) hid one figure from their addressees' view by placing an occluder between the figure and their addressee. Subsequently in the other- and self-focused setting, speakers (b) answered a perspective question by selecting either the three figures visible to their addressee (other-focus) or the four figures visible to them (self-focus). Hereafter, speakers (c) described the target object. Speakers were instructed to look at the four cards on the table when referring to the target object. While hearing speakers refer to a figure, addressees opened their eyes and pointed at the intended figure on the table in front of them. Speakers subsequently (d) informed their addressee whether their selection was correct. Since speakers in the baseline setting were not confronted with a perspective-taking manipulation, these speakers only performed actions (a), (c), and (d).

The experimental game ended after 40 rounds. After the final round, speakers indicated on a 10-point scale to what extent they took into account their addressees' perspective during the game (1 = not at all, 10 = very much). Addressees indicated on a 10-point scale how much the speaker had used redundant (i.e., size or color) information to describe the targets to them (1 = not at all, 10 = very much). Since audio recordings were made of all sessions[1], participants' consent to making these recordings and using them for scientific purposes were collected. Afterwards, all participants were debriefed.

[1] Speakers' speech-onset times were also recorded. Since we did not restrict speakers in how they should structure their identifications, large variations in how speakers started their referential utterance existed. We thus concluded not to include this data into our analyses.

Coding

To measure speakers' reference to privileged information (RPI), we counted the adjectives that matched the contrast (in size or color) between the target and privileged figure. Adjectives that did not contrast the target figure to the privileged one were not taken into account. For example, if speakers were to refer to the target as "the *small* triangle", information about the object's size was only considered as informational leakage when speakers' privileged figure depicted a similarly shaped figure (i.e., a large triangle). Speakers' RPI was calculated as a proportion (1 = contrasting adjective uttered; 0 = no contrasting adjective uttered).

Results

All dyads obtained the maximum of 40 points, indicating that they were able to correctly identify all targets. Per communicative setting, speakers provided 1200 object references (30 speakers * 40 trials). Out of the total of produced references (n = 3600), 10 ($n_{baseline}$ = 2, $n_{other\text{-}focus}$ = 5, $n_{self\text{-}focus}$ = 3) were excluded due to errors in the experimental procedure. Speakers' references consisted of noun phrases that contained zero to three adjectives. To estimate the amount speakers referred to privileged information, we counted the adjectives that matched the contrast presented in the stimuli (n = 1486). In Figure 2, the mean proportions of speakers' informational leakage as a function of the perspective

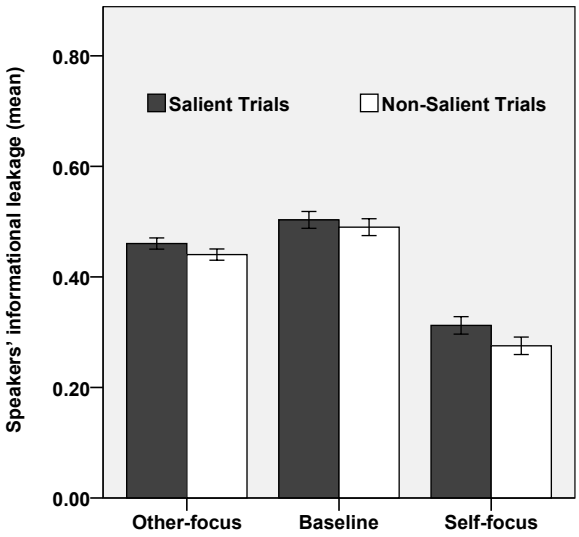


Figure 2. Mean proportions of speakers' RPI for salient and non-salient trials. Error bars represent 95% confidence intervals.

manipulation (baseline, other-focus, self-focus), and whether the target and speakers' privileged figure were similarly (salient trials) or differently (non-salient trials) shaped are shown. Overall, speakers in the baseline setting referred to privileged information in half of the produced references (50%), followed by the other-focused (45%), and self-focused speakers (29%). Across the three communicative settings, speakers seem to have referred to privileged information to the same degree on salient (43%) and non-salient (40%) trials.

The influence of the perspective manipulation and the interplay with the salience of speakers' privileged information on the probability of privileged information to be mentioned was analyzed using a generalized linear mixed model analysis with a binomial distribution. For this, we used the GLMER function from the lme4 package in R (version 3.3.0; CRAN project; R Core Team, 2017). In order to obtain all comparisons appertaining to our hypotheses, four models were constructed. All these models treated the baseline setting as the reference category to which the two perspective settings were contrasted, and each model's reference category also contained a different level of the two within-factors trial type (salient, non-salient) and contrast type (color, size). We constructed maximal models that included a full random effect structure (Barr et al., 2013). This maximal model included the perspective manipulation (self-focus, other-focus, baseline), the salience of the trials (salient, non-salient), the contrast (color, size) presented in the trials, and the setting*type and setting*contrast interactions as fixed factors. We included random intercepts and slopes for both speakers and experimental trials. Information about the models' reference categories, and fixed and random-effects structures is presented in Table 1 to 4. We report the results from the first model containing the comparison and

Table 1. Estimated Coefficients and Standard Errors for the Mixed Model (M1) Fitted to Speakers' RPI Scores, Using the Baseline Setting, Non-Salient Trials and Size Contrasts as Reference Categories

	b	SE_b	*95% CI*
Intercept	-13.93	1.74	-15.38, -8.56
Setting Other-focus (Non-Salient, Size)	0.80	2.07	-3.02, 5.11
Setting Self-focus (Non-Salient, Size)	1.28	1.66	-1.98, 4.52
Type (Salient, Size in Baseline)	1.53	0.81	-0.33, 2.86
Contrast (Non-Salient, Color in Baseline)	-1.91	1.93	-6.59, 0.97
Setting Other-focus * Type (Salient in Size)	-0.88	0.90	-2.61, 0.93
Setting Self-focus * Type (Salient in Size)	-0.30	0.90	-2.03, 1.51
Setting Other-focus * Contrast (Color in Non-Salient)	0.64	3.45	-5.32, 8.20
Setting Self-focus * Contrast (Color in Non-Salient)	-4.24	5.11	-14.67, 5.37

Note: A comparison with the intercept-only model proved that the inclusion of the by-participant random slope for Contrast and the by-item random slopes for Type in M1 was justified by the data, $\chi^2(4) = 173.8$, $p < .001$.

Table 2. Estimated Coefficients and Standard Errors for the Mixed Model (M2) Fitted to Speakers' RPI Scores, Using the Baseline Setting, Non-Salient Trials and Color Contrasts as Reference Categories

	b	SE_b	*95% CI*
Intercept	-15.08	1.75	-17.80, -10.94
Setting Other-focus (Non-Salient, Color)	1.24	1.90	-1.67, 5.76
Setting Self-focus (Non-Salient, Color)	-3.31	4.41	-12.06, 5.21
Type (Salient, Color in Baseline)	0.46	2.04	-3.05, 4.93
Contrast (Non-Salient, Size in Baseline)	1.67	2.36	-2.23, 7.01
Setting Other-focus * Type (Salient in Color)	-0.79	1.13	-3.10, 1.32
Setting Self-focus * Type (Salient in Color)	-0.24	1.16	-2.58, 1.97
Setting Other-focus * Contrast (Size in Non-Salient)	-0.17	3.12	-6.55, 5.66
Setting Self-focus * Contrast (Size in Non-Salient)	4.79	4.81	-4.41, 14.43

Note: A comparison with the intercept-only model proved that the inclusion of the by-participant random slopes for Type and Contrast, and the by-item random slope for Type in M2 was justified by the data, $\chi^2(7) = 174.7$, $p < .001$.

Table 3. Estimated Coefficients and Standard Errors for the Mixed Model (M3) Fitted to Speakers' RPI Scores, Using the Baseline Setting, Salient Trials and Size Contrasts as Reference Categories

	b	SE_b	*95% CI*
Intercept	-12.40	1.78	-14.24, -7.27
Setting Other-focus (Salient, Size)	-0.07	1.61	-2.92, 3.38
Setting Self-focus (Salient, Size)	0.98	1.56	-1.86, 4.25
Type (Non-Salient, Size in Baseline)	-1.53	1.00	-3.29, 0.65
Contrast (Salient, Color in Baseline)	-1.91	2.23	-6.81, 1.95
Setting Other-focus * Type (Non-Salient in Size)	0.88	1.05	-1.33, 2.79
Setting Self-focus * Type (Non-Salient in Size)	0.30	0.96	-1.70, 2.08
Setting Other-focus * Contrast (Color in Salient)	0.64	2.71	-4.33, 6.27
Setting Self-focus * Contrast (Color in Salient)	-4.24	4.43	-13.71, 3.64

Note: A comparison with the intercept-only model proved that the inclusion of the by-participant random slope for Contrast, and the by-item random slope for Type in M3 was justified by the data, $\chi^2(4) = 173.81$, $p < .001$.

of the maximal random effects structure that first converged (Barr et al., 2013). We used a Bonferroni correction to correct for multiple comparisons. The probability distribution was set on binomial with a logit link function and we used parametric bootstrapping over 100 iterations to estimate the confidence intervals and *p*-values. When the maximal model did not converge, we excluded random slopes with the lowest variance until convergence was reached.

Table 4. Estimated Coefficients and Standard Errors for the Mixed Model (M4) Fitted to Speakers' RPI Scores, Using the Baseline Setting, Salient Trials and Color Contrasts as Reference Categories

	b	SE_b	*95% CI*
Intercept	-14.31	1.49	-16.20, -10.34
Setting Other-focus (Salient, Color)	0.57	2.36	-3.38, 5.88
Setting Self-focus (Salient, Color)	-3.26	4.41	-12.64, 4.67
Type (Non-Salient, Color in Baseline)	-1.53	0.90	-3.31, 0.23
Contrast (Salient, Size in Baseline)	1.91	2.32	-2.01, 7.08
Setting Other-focus * Type (Non-Salient in Color)	0.88	0.87	-0.78, 2.64
Setting Self-focus * Type (Non-Salient in Color)	0.30	1.03	-1.73, 2.32
Setting Other-focus * Contrast (Size in Salient)	-0.64	3.12	-7.02, 5.21
Setting Self-focus * Contrast (Size in Salient)	4.24	4.62	-4.19, 13.94

Note: A comparison with the intercept-only model proved that the inclusion of the by-participant random slope for Contrast, and the by-item random slope for Type in M4 was justified by the data, $\chi^2(4) = 173.8$, $p < .001$.

Influence of Perspective on Speakers' RPI

Speakers' RPI in the self- and other-focused setting did not significantly differ from speakers' RPI in the baseline setting. For non-salient size trials, speakers in the other-focused (*M* = .33, *SD* = .45, *b* = 0.80, *SE* = 2.07, BC 95% CI: [-3.02, 5.11]), and self-focused setting (*M* = .24, *SD* = .41, *b* = 1.28, *SE* = 1.66, BC 95% CI: [-1.98, 4.52]), were just as likely as the baseline speakers (*M* = .44, *SD* = .50) to refer to privileged information. The same held for non-salient color trials: other-focused (*M* = .55, *SD* = .47, *b* = 1.24, *SE* = 1.90, BC 95% CI: [-1.67, 5.76]), and self-focused speakers' RPI (*M* = .31, *SD* = .43, *b* = -3.31, *SE* = 4.41, BC 95% CI: [-12.06, 5.21]) did not significantly differ from the baseline (*M* = .54, *SD* = .50). This pattern also held for salient size trials: speakers' RPI in the other- (*M* = .34, *SD* = .44, *b* = -0.07, *SE* = 1.61, BC 95% CI: [-2.92, 3.38]), and self-focused setting (*M* = .27, *SD* = .41, *b* = 0.98, *SE* = 1.56, BC 95% CI: [-1.86, 4.25]) did not significantly differ from the baseline (*M* = .46, *SD* = .50). Finally, speakers' RPI on salient color trials in the other- (*M* = .58, *SD* = .46, *b* = 0.57, *SE* = 2.36, BC 95% CI: [-3.38, 5.88]), and self-focused setting (*M* = .35, *SD* = .42, *b* = -3.26, *SE* = 4.41, BC 95% CI: [-12.64, 4.67]) did also not significantly differ from the baseline (*M* = .55, *SD* = .49).

Influence of Salience on Speakers' RPI

In the baseline setting, the salience of privileged information did not influence speakers' RPI. Baseline speakers were just as likely to refer to privileged information on non-salient (*M* = .44, *SD* = .50) and salient (*M* = .46, *SD* = .50) size trials (*b* = 1.53, *SE* = 0.81,

BC 95% CI: [-0.33, 2.86]), and on non-salient (*M* = .54, *SD* = .50) and salient (*M* = .55, *SD* = .49) color trials (*b* = 0.46, *SE* = 2.04, BC 95% CI: [-3.05, 4.93]).

Baseline speakers' RPI was also not influenced by the contrast presented in the trials. Speakers were just as likely to refer to privileged information on non-salient size (*M* = .44, *SD* = .50) and non-salient color (*M* = .54, *SD* = .50) trials (*b* = -1.91, *SE* = 1.93, BC 95% CI: [-6.59, 0.97]), as on salient size (*M* = .46, *SD* = .50) and salient color (*M* = .55, *SD* = .49) trials (*b* = -1.91, *SE* = 2.23, BC 95% CI: [-6.81, 1.95]).

When the two perspective settings were contrasted to the baseline setting, no significant differences were found. For size contrasting trials, no significant differences were found between the non-salient trials in the baseline setting (*M* = .44, *SD* = .50), and salient trials in the other-focused setting (*M* = .34, *SD* = .44; *b* = -0.88, *SE* = 0.90, BC 95% CI: [-2.61, 0.93]), nor between non-salient trials in the baseline setting and salient trials in the self-focused setting (*M* = .27, *SD* = .41; *b* = -0.30, *SE* = 0.90, BC 95% CI: [-2.03, 1.51]). The same held for color contrasting trials. There were no significant differences between non-salient trials in the baseline setting (*M* = .54, *SD* = .50), and salient trials in the other-focused (*M* = .58, *SD* = .46; *b* = -0.79, *SE* = 1.13, BC 95% CI: [-3.10, 1.32]), or in the self-focused setting (*M* = .35, *SD* = .42; *b* = -0.24, *SE* = 1.16, BC 95% CI: [-2.58, 1.97]).

In addition, the contrast presented in the trials did not influence the extent to which speakers' RPI differed in the other-focused or in the self-focused settings from the baseline. For non-salient trials, differences between size contrasting trials in the baseline setting (*M* = .44, *SD* = .50) and color contrasting trials in the other-focused (*M* = .55, *SD* = .47; *b* = 0.64, *SE* = 3.45, BC 95% CI: [-5.32, 8.20]), or color contrasting trials in the self-focused setting (*M* = .31, *SD* = .43; *b* = -4.24, *SE* = 5.11, BC 95% CI: [-14.67, 5.37]) were non-significant. For salient trials, differences between size contrasting trials in the baseline setting (*M* = .46, *SD* = .50) and color contrasting trials in the other-focused setting (*M* = .58, *SD* = .46; *b* = 0.64, *SE* = 2.71, BC 95% CI: [-4.33, 6.27]), or color contrasting trials in the self-focused setting (*M* =.35, *SD* = .42; *b* = -4.24, *SE* = 4.43, BC 95% CI: [-13.71, 3.64]) were also non-significant.

Speakers' Introspective Perspective-Taking

Exploratory analysis showed that speakers' introspective perspective-taking tendency was significantly non-normal in the baseline, *D*(30) = 0.17, *p* = .032, other-focus, *D*(29) = 0.18, *p* = .019, and self-focus condition, *D*(30) = 0.27, *p* < .001. We therefore performed a non-parametric Kruskal-Wallis test which revealed that speakers' introspective perspective-taking tendency significantly differed between settings, *H*(2) = 7.07, *p* = .029. Step-down follow-up analysis that looks for homogeneous subsets showed that the perspective-taking tendencies of the other-focused speakers (*MRank* = 40.03) and the speakers in the baseline

setting (*MRank* = 38.98) were homogeneous, $H = .017$, $p = .896$. Hence, the perspective-taking tendency between the other-focused speakers and the speakers in the baseline did not seem to significantly differ. The perspective-taking tendency of self-focused speakers (*MRank* = 54.67) did not belong to this homogeneous subset ($p < .05$). It therefore seems that self-focused speakers ($M = 7.73$, $SD = 2.94$) reported a significant higher perspective-taking tendency than the baseline speakers ($M = 5.60$, $SD = 3.51$) and the other-focused speakers ($M = 5.62$, $SD = 3.63$). To investigate whether speakers' introspective perspective-taking tendency corresponded to their actual behavior during the game, a follow-up logit mixed model analysis was conducted. The full model included speakers' self-report as fixed effect, a random intercept for subjects and items, and by-subject and by-item random slopes for the effect of speakers' self-report. *P*-values were obtained using the Likelihood Ratio Test (LRT) in which we compared the full model with the intercept only model. The LRT revealed that speakers' self-report was a significant predictor of their actual RPI, $\chi^2(5) = 120.41$, $p < .001$. As speakers' introspective perspective-taking tendency increased, they were less likely to have leaked privileged information during the game, $b = -3.80$, $SE = 0.52$, $p < .001$.

Addressees' Perception of Speakers' Redundancy

Addressees' perception of speakers' redundancy was significantly non-normal in the baseline, $D(30) = 0.42$, $p < .001$, other-focus, $D(29) = 0.33$, $p < .001$, and self-focus condition, $D(30) = 0.32$, $p < .001$. A Kruskal-Wallis test showed that addressees' perception of speakers' redundancy during the game differed between settings, $H(2) = 6.81$ $p = .033$. Step-down follow-up analysis showed two homogeneous subsets. First, addressees' perception of speakers' redundancy tended to be homogeneous in the baseline (*MRank* = 36.50, $M = 1.60$, $SD = 1.57$) and in the self-focused setting (*MRank* = 47.30, $M = 2.93$, $SD = 2.80$), $H = 3.83$, $p = .050$. Moreover, these perception scores were homogeneous in the self-focused and in the other-focused setting (*MRank* = 51.41, $M = 3.55$, $SD = 3.36$), $H = 0.48$, $p = .490$. Addressees' perception of speakers' redundancy in the other-focused setting did not form a homogeneous subset with this perception score in the baseline. Since these perception scores were not equivalent, addressees seem to have indicated that other-focused speakers had provided them with significantly more redundant information than the speakers in the baseline.

Intermediate Discussion

This first experiment examined whether eliciting speakers' self- versus other-focus would influence their subsequent reference production. We found that speakers in the

other- and self-focused settings were just as likely to refer to privileged information as the speakers whose perspective-taking was not manipulated. We did not replicate the results of Kaland et al. (2011) and Wardlow Lane et al. (2006) as the speakers in our study were just as likely to refer to private information, regardless of its salience.

An interesting finding of this study is the result of speakers' introspective perspective-taking tendency and its relation to their reference production. Ironically, speakers with an elicited self-focus reported to have regarded their addressees' perspective more than the speakers in the other two settings. These self-reported tendencies correlated negatively with speakers' previous leakage behavior, indicating that speakers with a self-reported high perspective-taking tendency were less likely to have leaked private information during the game. In addition, speakers' higher informational leakage in the other-focused setting was detected by the addressees who reported to have experienced more redundancy in the other-focused setting than in the baseline setting. It thus seems that not an elicited other- but *self*-focus activated speakers' awareness of their interlocutor's informational need, reducing the likelihood of speakers' referring to privileged information. In a second experiment, we examine whether the explicit self-focus instructions could have made speakers' more self-aware about their referential behavior than the speakers who were explicitly instructed to focus on their addressees' perspective (Wicklund, 1975), reducing the extent to which the self-focused speakers leak information that is privileged to them.

One limitation of the previous experiment we want to address is the fact that the majority of speakers (65.56%) persisted in a certain reference strategy throughout the experiment. That is, 59 speakers either referred to color and size contrasts on all of the trials, or they refrained from including any adjectives at all throughout the game. This tendency to retain a certain reference strategy could have interfered with the speakers' audience design (Horton & Gerrig, 2002), and thus to the extent to which they were influenced by the elicited perspective and the salience of their privileged knowledge.

One factor that could have contributed to that speakers' consistency is the self-paced method by which speakers completed the trials. In a self-paced manner, speakers clicked on their private computerscreen to receive the 'occlude' and 'identify' instructions and the perspective manipulation. This self-paced method could have resulted in a routine by which speakers performed the instructions and completed the trials. Moreover, the fact that the perspective-taking manipulation appeared on speakers' private computer screen – in which it was not made visible which figures were commonly versus privately known – could have reduced the intrusiveness of the perspective-taking manipulation. Although speakers were explicitly trained to return their attention from their private screen to the physical context shared between them and their addressee *before* they identified the target figure, the possibility exists that speakers were still regarding their private screen (in which

perspectives were not marked) while formulating their reference. This raises the question whether speakers were actually regarding the common-ground status before they referred to the target figure. In a second experiment, we address this issue by asking speakers explicitly to indicate on the common-ground cards before them and their addressee which figures were visible to either the speakers themselves or to their addressee.

Another possible explanation as to why speakers maintained a consistent reference strategy is the fact that the six color manipulations used in the experimental design (following Kaland et al., 2011, 2014) could have inspired speakers to refer to color contrasts on all trials (e.g., Koolen et al., 2013). In a second experiment, we address this issue by reducing the obtrusiveness of the color manipulation.

EXPERIMENT 2

The second experiment addresses the issue of speakers' retained reference strategy by amplifying the intrusiveness of the perspective-taking manipulation, and reducing the obtrusiveness of the color-attributes. Recall that the first experiment exposed speakers to the perspective-taking manipulation on their private computerscreen. On this screen, speakers indicated which figures were either visible to their addressee or to themselves. Differences in perspectives were not visibly marked on this screen, but they were visibly marked on the cards lying on the table in between speakers and addressees. To guarantee that speakers are explicitly aware of the common-ground status before they produce a referential message, we asked our speakers in the second experiment to use the four figures lying in between them and their addressee to indicate which ones are either visible to their addressee (other-focus) or to themselves (self-focus). Moreover, instead of employing the six color versus the two size manipulations, the second study reduces the color manipulations from six to two (blue, green), thereby equalizing the amount of color manipulations to the two size manipulations (large, small) employed. With this more robust design, we test the hypotheses that, compared to the baseline, other-focused speakers will be less likely and self-focused speakers will be more likely to refer to privileged information. Furthermore, we explore the assumption that speakers will be more likely to leak information privileged to them when this information is salient rather than non-salient, and test the hypothesis that the salience of speakers' privileged information will interact with the explicit perspective-focus (other-focus, self-focus). In addition, we address the finding from the previous experiment that the self-focused speakers in the first experiment seem to have been, ironically, more aware of the required audience design than the other-focused speakers or the speakers partaking in the baseline setting. Previous studies indicated that strengthening people's

attention to the self can reduce egocentric behavior (Hass, 1984; Stephenson & Wicklund, 1984). According to the objective self-awareness theory (Wicklund, 1975), persons who are privately self-aware act on their cognitions that are salient at that specific moment in time, whereas publicly self-aware (Fenigstein, 1987; Govern & Marsch, 2001) persons reflect on themselves as if they are an object under scrutinization. Under this scrutinization, the difference between their actual and required behavior, derived from the standards that apply to the interaction, becomes salient. Our self-focused speakers could have found themselves in such a reflective state, especially since a cue of their addressees' different perspective was present (Gendolla & Wicklund, 2009). Speakers were able to see which figures were available for addressees' selection process (and which one was not). A higher self-awareness in the self-focused rather than other-focused speakers could have caused the self-focused speakers to engage in a more accurate audience design, thereby being less likely to leak privileged information. Hence, in addition to our hypotheses from Experiment 1, we explore the possibility that (a) public self-awareness will be higher for speakers focusing on their own perspective (i.e., self-focused speakers) than those focusing on their addressees' point of view (i.e., other-focused speakers) or those speakers without an induced perspective (i.e., baseline), and (b) public self-awareness will influence the extent to which speakers oblige with the salient social standard (i.e., engaging in an accurate audience design), expressed by the reduced probability of speakers leaking information about their egocentric perspective.

Method

Participants

114 student-dyads (N = 228) participated in the second experiment. None of the participants had participated in the first experiment. The data of 11 dyads were excluded from analyses, due to an error in the experimental procedure (N = 22). The analyses were based on 103 dyads in which the participants were randomly assigned either the role of the speaker (69 women, 34 men, M_{age} = 21.8 years; age range 17-52 years) or the role of the addressee (74 women, 29 men, M_{age} = 21.7 years; age range 17-47). All participants were fluent in Dutch, did not experience problems at discerning the colors used in the study, and received course credits for their participation.

Design, Materials and Procedure

Following the first experiment, speakers were randomly allocated to one of the experimental settings (self-focus, other-focus, baseline). This resulted in 33 dyads in the

self-focused setting, 35 dyads in the other-focused setting, and 35 in the baseline setting. We replicated the design, materials and procedure from the first experiment, with the exceptions that (a) the experiment was now paced by the experimental leader, and (b) the number of colors used was reduced from six to two (blue, green). To ensure the salience of the size contrasts used, we amplified the size difference between the target and speakers' privileged figure from 1:2 (Experiment 1) to 1:4.

Eliciting Self- Versus Other-Focus. The computerscreen was omitted from the experimental procedure and all instructions took place on the table in front of the speakers and addressees. The experimental leader made sure that addressees kept their eye shut during the entire procedure. After speakers had blocked one figure from their addressees' view, speakers in the two perspective settings were provided with five carbon cards depicting the four figures lying on the table in front of them and one filler figure. The experimental leader asked speakers in the self-focused setting to indicate *which four figures were visible to them* by moving the intended four figures upwards (Figure 3a). The experimental leader would then check speakers' selection and confirm speakers' egocentric perspective by turning the filler figure – so that the blank side of the card was visible – and by stressing that the four selected cards were indeed the four figures speakers were seeing. In the other-focused setting, the experimental leader asked speakers to move upward *the three figures that were visible to [name of the addressee]*, eliciting addressees' perspective of the situation. In contrast to Experiment 1, the other-focus prompt included the name of the addressee to enhance speakers' focus to their addressee's perspective. The selected figures were checked by the experimental leader and, as in the self-focused setting, she would turn speakers' privileged figure and the filler after which she would confirm speakers' selection by stressing that the three figures moved upwards were indeed the ones their addressee was seeing (Figure 3b). As in the first experiment, speakers in the baseline setting were not confronted with perspective questions. As such, these speakers' self- or other-focus was not reinforced.

Speakers' Introspective Perspective-Taking. Note that in the first experiment speakers' introspective perspective-taking was measured by only one item on a 10-point scale (i.e., "To what extent did you take into account your addressee's perspective during the game?"). To increase the validity of the self-reports, we measured speakers' perspective-taking tendency by a multiple-item scale, adjusting the perspective-taking items presented in Davis' Interpersonal Reactivity Index (IRI; 1980) to the referential communication setting. Our introspective perspective-taking scale measured speakers' consciousness about the difference in perspectives presented to them (e.g., "I was aware that the addressee had a different view of the situation than I"), and speakers' acknowledgement of these differences in perspectives expressed by their (adjusted) referential communication (e.g., "I adjusted

Self-focused Setting

Figure 3a. The perspective-taking manipulation in the self-focused setting. The speakers indicated which four figures were visible to them by moving the intended four figures upward.

Other-focused Setting

Figure 3b. The perspective-taking manipulation in the other-focused setting. The speakers indicated which three figures were visible to their addressee by moving the intended three figures upward.

my instructions to the informational need of the addressee"). The eleven introspective perspective-taking questions were alternated by fourteen filler questions (e.g., "Playing the game made me enthusiastic"). Speakers' answered the declarative sentences on a 7-point scale (1 = totally disagree, 7 = totally agree). The introspective perspective-taking scale had a high reliability (Cronbach's α = .89). The individual questions are presented in Table 5.

Table 5. Items of Speakers' Introspective Perspective-Taking Scale

Item
1. During the game, I was especially aware of the figures that were visible to me (R)
2. During the game, I found it difficult to put myself in the addressee's position (R)
3. I was aware that the addressee had a different view of the situation than I
4. During the game, I was especially aware of the figures I was seeing (R)
5. During the game, I mainly took into account my own view of the situation (R)
6. I played the game as much as possible from addressee's point of view
7. Before I gave a description, I tried to imagine the situation from the addressee's perspective
8. Placing myself in the addressee's position allowed me to take into account which information the addressee needed in order to select the intended figure
9. I have described the figures with just enough (not too much, not too little) information
10. The addressee did not need much of the information provided by me to select the intended figure (R)
11. During the game, I adapted my descriptions to addressee's informational need (i.e., the information the addressee needed to select the intended figure) as much as possible

Note: The (R) signals that the scores were recoded before the analysis.

Speakers' Situational Self-Awareness. To explore the possibility that an elicited self-focus enhanced speakers' public self-awareness, speakers filled out a Dutch translated version of the Situational Self-Awareness Scale (SSAS; Govern & Marsch, 2001). The SSAS measures two dimensions of speakers' self-awareness, namely their private and their public self-awareness. According to the objective self-awareness theory (Wicklund, 1975), privately self-aware persons will act on salient inner cognitions, whereas publicly self-aware persons will modify their behavior so that it adheres to the social standard in order to prevent a negative evaluation (Froming et al., 1982; Gendolla & Wicklund, 2009). The scale also takes into account the non-self-aware individuals by measuring the extent to which these persons are focused on their surroundings. This resulted in a three-factor scale (Surroundings, Private, Public), and each factor was measured by three items phrased as declarative sentences (e.g., "Right now, I am conscious of my inner feelings"). Speakers responded how much they agreed with the sentences on a 7-point scale (1 = strongly disagree; 7 = strongly agree). The items, their factor loadings and Cronbach's Alpha's are presented in Table 6.

Table 6. Items of the Situational Self-Awareness Scale (Govern & Marsch, 2001)

Item		Factor	Factor Loading		Cronbach's Alpha	
			Govern & Marsch (2001)	Experiment 2	If Item Deleted	Based on All Items
1.	Right now, I am keenly aware of everything in my environment	Surroundings	.81	.92	.78	.85
2.	Right now, I am conscious of my inner feelings	Private	.68	.69	.72	.74
3.	Right now, I am concerned about the way I present myself	Public	.80	.83	.81	.86
4.	Right now, I am self-conscious about the way I look	Public	.88	.87	.76	.86
5.	Right now, I am conscious of what is going on around me	Surroundings	.75	.73	.83	.85
6.	Right now, I am reflective about my life	Private	.76	.64	.74	.74
7.	Right now, I am concerned about what other people think of me	Public	.85	.83	.82	.86
8.	Right now, I am aware of my innermost thoughts	Private	.84	.89	.46	.74
9.	Right now, I am conscious of all objects around me	Surroundings	.78	.84	.76	.85

Note: Item 6 loaded .55 on factor Public and item 2 loaded .41 on factor Surroundings. All other items loaded below .40 on the other two factors in the scale.

Addressees' Perception of Speakers' Perspective-Taking. The eleven items from the speakers' introspective perspective-taking scale described above were reformulated into addressees' perspective. In this way, addressees reported their perception of speakers' perspective-taking behavior during the referential communication game. The reformulated items are presented in Table 7. Addressees' version of the scale had a high reliability (Cronbach's $\alpha = .78$).

Results

All dyads were able to correctly identify the forty targets. Out of the total of produced references ($n = 4160$), 15 references ($n_{baseline} = 2$, $n_{other\text{-}focus} = 4$, $n_{self\text{-}focus} = 9$) were excluded due to errors in the experimental procedure. As in the previous experiment, we counted the adjectives that leaked information about speakers' privileged perspective ($n = 1353$).

Table 7. Items of Addressees' Perception of Speakers' Perspective-Taking Scale

Item
1. During the game, the speaker mainly took into account his or her own view of the situation (R)
2. The speaker described the figures with just enough (not too much, not too little) information
3. The speaker played the game as much as possible from my point of view
4. Before the speaker gave a description, (s)he tried to imagine the situation from my perspective
5. I did not need much of the information that was provided by the speaker to select the intended figure (R)
6. During the game, the speaker was especially aware of the figures (s)he was seeing (R)
7. During the game, the speaker found it difficult to place him or herself in my position (R)
8. During the game, the speaker adapted his or her descriptions to my informational need (i.e., the information I needed to select the intended figure) as much as possible
9. The speaker was aware that I had a different view of the situation than he or she did
10. During the game, the speaker was especially aware of the figures that were visible to him or herself (R)
11. By placing him or herself in my position allowed the speaker to take into account which information I needed in order to select the intended figure

Note: The (R) signals that the scores were recoded before the analysis.

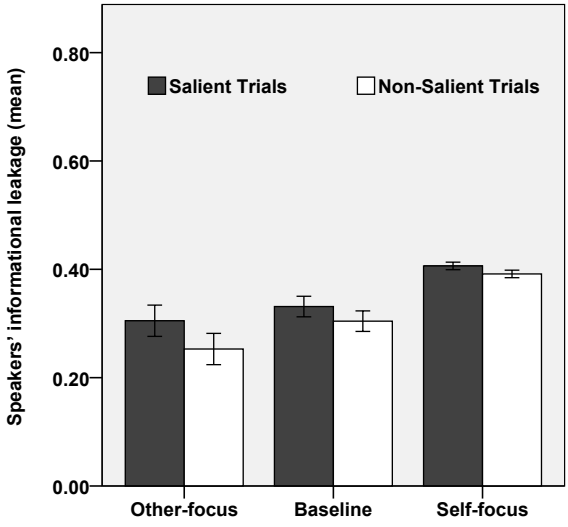


Figure 4. Mean proportions of speakers' RPI for salient and non-salient trials. Error bars represent 95% confidence intervals.

In Figure 4, the mean proportions of speakers' informational leakage as a function of the perspective manipulation (baseline, other-focus, self-focus), and whether the target and speakers' privileged figure were similarly (salient trials) or differently (non-salient trials) shaped are presented. Overall, speakers in the self-focused setting referred to privileged

information in 39% of the produced references, followed by speakers in the baseline setting (32%), and speakers in the other-focused setting (28%). As in Experiment 1, speakers across the three perspective settings seem to have referred to privileged information to the same degree on salient (35%) and non-salient (31%) trials.

We replicated the method of analysis from the previous experiment, and we excluded random slopes to reduce multicollinearity (< 3). The models' fixed and random effects structures, beta coefficients and confidence intervals are presented in Table 8 to 11.

Table 8. Estimated Coefficients and Standard Errors for the Mixed Model (M5) Fitted to Speakers' RPI Scores, Using the Baseline Setting, Non-Salient Trials and Size Contrasts as Reference Categories

	b	SE_b	*95% CI*
Intercept	-9.60	2.86	-13.84, -2.63
Setting Other-focus (Non-Salient, Size)	2.49	2.94	-2.74, 8.79
Setting Self-focus (Non-Salient, Size)	5.02	3.07	-1.36, 10.69
Type (Salient, Size in Baseline)	0.72	0.51	-0.34, 1.68
Contrast (Non-Salient, Color in Baseline)	**7.72**	**3.11**	**2.00, 14.20**
Setting Other-focus * Type (Salient in Size)	0.53	0.44	-0.29, 1.42
Setting Self-focus * Type (Salient in Size)	-0.09	0.38	-0.78, 0.71
Setting Other-focus * Contrast (Color in Non-Salient)	-3.68	2.56	-8.80, 1.24
Setting Self-focus * Contrast (Color in Non-Salient)	-2.43	3.26	-9.74, 3.05

Note: Significant results are represented in bold. A comparison with the intercept-only model proved that the inclusion of the by-participant random slope for Contrast and the by-item random slopes for Setting and Type in M5 was justified by the data, $\chi^2(11) = 402.80$, $p < .001$.

Table 9. Estimated Coefficients and Standard Errors for the Mixed Model (M6) Fitted to Speakers' RPI Scores, Using the Baseline Setting, Non-Salient Trials and Color Contrasts as Reference Categories

	b	SE_b	*95% CI*
Intercept	-1.89	4.55	-9.06, 8.79
Setting Other-focus (Non-Salient, Color)	-1.17	4.57	-8.93, 8.97
Setting Self-focus (Non-Salient, Color)	2.61	4.91	-8.71, 10.53
Type (Salient, Color in Baseline)	0.61	0.70	-0.78, 1.98
Contrast (Non-Salient, Size in Baseline)	**-7.78**	**3.34**	**-14.56, -1.48**
Setting Other-focus * Type (Salient in Color)	0.57	0.58	-0.62, 1.67
Setting Self-focus * Type (Salient in Color)	0.03	0.54	-1.10, 1.02
Setting Other-focus * Contrast (Size in Non-Salient)	3.68	3.02	-2.53, 9.31
Setting Self-focus * Contrast (Size in Non-Salient)	2.43	3.33	-2.78, 10.25

Note: Significant results are represented in bold. A comparison with the intercept-only model proved that the inclusion of the by-participant random slopes for Type and Contrast, and the by-item random slopes for Setting and Type in M6 was justified by the data, $\chi^2(14) = 404.97$, $p < .001$.

Table 10. Estimated Coefficients and Standard Errors for the Mixed Model (M7) Fitted to Speakers' RPI Scores, Using the Baseline Setting, Salient Trials and Size Contrasts as Reference Categories

	b	SE_b	*95% CI*
Intercept	-9.06	3.49	-13.71, -0.02
Setting Other-focus (Salient, Size)	3.09	-0.27	-2.36, 9.08
Setting Self-focus (Salient, Size)	5.07	3.53	-2.14, 11.68
Type (Non-Salient, Size in Baseline)	-0.61	0.77	-2.22, 0.79
Contrast (Salient, Color in Baseline)	**7.78**	**2.96**	**2.59, 14.20**
Setting Other-focus * Type (Non-Salient in Size)	-0.57	0.54	-1.65, 0.46
Setting Self-focus * Type (Non-Salient in Size)	-0.03	0.63	-1.27, 1.19
Setting Other-focus * Contrast (Color in Salient)	-3.68	2.40	-8.12, 1.29
Setting Self-focus * Contrast (Color in Salient)	-2.43	2.80	-8.83, 2.15

Note: Significant results are represented in bold. A comparison with the intercept-only model proved that the inclusion of the by-participant random slopes for Type and Contrast, and the by-item random slopes for Setting and Type in M7 was justified by the data, $\chi^2(14) = 404.97$, $p < .001$.

Table 11. Estimated Coefficients and Standard Errors for the Mixed Model (M8) Fitted to Speakers' RPI Scores, Using the Baseline Setting, Salient Trials and Color Contrasts as Reference Categories

	b	SE_b	*95% CI*
Intercept	-0.67	3.44	-6.01, 7.46
Setting Other-focus (Salient, Color)	-0.18	3.21	-6.16, 6.44
Setting Self-focus (Salient, Color)	1.41	3.77	-6.56, 8.22
Type (Non-Salient, Color in Baseline)	-1.08	1.29	-3.16, 1.91
Contrast (Salient, Size in Baseline)	**-8.02**	**2.65**	**-13.40, -3.01**
Setting Other-focus * Type (Non-Salient in Color)	-0.39	0.91	-2.20, 1.35
Setting Self-focus * Type (Non-Salient in Color)	0.66	1.00	-1.62, 2.30
Setting Other-focus * Contrast (Size in Salient)	3.57	2.45	-1.00, 8.61
Setting Self-focus * Contrast (Size in Salient)	2.87	2.76	-1.95, 8.86

Note: Significant results are represented in bold. A comparison with the intercept-only model proved that the inclusion of the by-participant random slopes for Type * Contrast, and the by-item random slopes for Setting and Type in M8 was justified by the data, $\chi^2(18) = 416.42$, $p < .001$.

Influence of Perspective on Speakers' RPI

Speakers' RPI in the self- and other-focused setting did not significantly differ from speakers' RPI in the baseline setting. For non-salient size trials, speakers in the other-focused ($M = .22$, $SD = .37$, $b = 2.49$, $SE = 2.94$, BC 95% CI: [-2.74, 8.79]), and self-focused setting ($M = .31$, $SD = .43$, $b = 5.02$, $SE = 3.07$, BC 95% CI: [-1.36, 10.69]), were just as likely as the baseline speakers ($M = .15$, $SD = .32$) to refer to privileged information. The same held for non-salient

color trials: other-focused (*M* = .29, *SD* = .41, *b* = -1.17, *SE* = 4.57, BC 95% CI: [-8.93, 8.97]), and self-focused speakers' RPI (*M* = .45, *SD* = .47, *b* = 2.61, *SE* = 4.91, BC 95% CI: [-8.71, 10.53]) did not significantly differ from the baseline (*M* = .45, *SD* = .47). This pattern also held for salient size trials: speakers' RPI in the other- (*M* = .25, *SD* = .40, *b* = 3.09, *SE* = -0.27, BC 95% CI: [-2.36, 9.08]), and self-focused setting (*M* = .32, *SD* = .44, *b* = 5.07, *SE* = 3.53, BC 95% CI: [-2.14, 11.68]) did not significantly differ from the baseline (*M* = .17, *SD* = .33). Finally, speakers' RPI on salient color trials in the other- (*M* = .36, *SD* = .40, *b* = -0.18, SE = 3.21, BC 95% CI: [-6.16, 6.44]), and self-focused setting (*M* = .49, *SD* = .45, *b* = 1.41, SE = 3.77, BC 95% CI: [-6.56, 8.22]) did also not significantly differ from the baseline (*M* = .49, *SD* = .45).

Influence of Salience on Speakers' RPI

Baseline speakers' RPI was influenced by the contrast presented in the trials. Speakers were more likely to refer to privileged information on non-salient color trials (*M* = .45, *SD* = .47) than on non-salient size (*M* = .15, *SD* = .32) trials (*b* = 7.72, *SE* = 3.11, BC 95% CI: [2.00, 14.20]). This pattern also held for salient trials as speakers were more likely to leak information on salient trials depicting a contrast in color (*M* = .49, *SD* = .45) than on trials depicting a contrast in size (*M* = .17, *SD* = .33) trials (*b* = 7.78, *SE* = 2.96, BC 95% CI: [2.59, 14.20]).

However, the salience of privileged information did not influence baseline speakers' RPI. Baseline speakers were just as likely to refer to privileged information on non-salient (*M* = .15, *SD* = .32) and salient (*M* = .17, *SD* = .33) size trials (*b* = 0.72, *SE* = 0.51, BC 95% CI: [-0.34, 1.68]), and on non-salient (*M* = .45, *SD* = .47) and salient (*M* = .49, *SD* = .45) color trials (*b* = 0.61, *SE* = 0.70, BC 95% CI: [-0.78, 1.98]).

The salience of the trials did not influence the difference in speakers' RPI between the baseline and other-focused setting, and the baseline and self-focused setting. For size contrasting trials, no significant differences were found between non-salient trials in the baseline setting (*M* = .15, *SD* = .32) and salient trials in the other-focused setting (*M* = .25, *SD* = .40), *b* = 0.53, *SE* = 0.44, BC 95% CI: [-0.29, 1.42], nor between non-salient trials in the baseline setting and salient trials in the self-focused setting (*M* = .32, *SD* = .44), *b* = -0.09, *SE* = 0.38, BC 95% CI: [-0.78, 0.71]. The same held for color contrasting trials, as there were no significant differences between non-salient trials in the baseline setting (*M* = .45, *SD* = .47) and salient trials in the other-focused (*M* = .36, *SD* = .40), *b* = 0.57, *SE* = 0.58, BC 95% CI: [-0.62, 1.67], or self-focused (*M* = .49, *SD* = .45) setting, *b* = 0.03, *SE* = 0.54, BC 95% CI: [-1.10, 1.02].

Further, the contrast presented in the trials did not influence the extent to which speakers' RPI differed between the baseline and other-focused setting, nor between the

baseline and the self-focused setting. For non-salient trials, differences between size contrasting trials in the baseline setting (M = .15, SD = .32) and color contrasting trials in the other-focused setting (M = .29, SD = .41), b = -3.68, SE = 2.56, BC 95% CI: [-8.80, 1.24], and color contrasting trials in the self-focused setting (M = .45, SD = .47), b = -2.43, SE = 3.26, BC 95% CI: [-9.74, 3.05], were not significant. In addition, for salient trials, differences between size contrasting trials in the baseline setting (M = .17, SD = .33) and color contrasting trials in the other-focused setting (M = .36. SD = .40), b = -3.68, SE = 2.40, BC 95% CI: [-8.12, 1.29], and self-focused setting (M = .49, SD = .45), b = -2.43, SE = 2.80, BC 95% CI: [-8.83, 2.15], were non-significant.

Speakers' Introspective Perspective-Taking

Speakers' self-reported perspective-taking tendency was not normally distributed in the baseline, $D(35)$ = .11, p =.200. A Kurskal-Wallis analysis revealed that speakers' introspective perspective-taking tendency did not significantly differ between settings, $H(2)$ = 0.28, p = .854. Speakers in the baseline (M = 5.17, SD = 1.02), other-focused (M = 5.08, SD = 1.31), and self-focused setting (M = 4.95, SD = 1.29) reported to have engaged in perspective-taking behavior to the same degree. As in the first study, we conducted a follow-up logit mixed model analysis to investigate whether speakers' introspective perspective-taking tendency corresponded with their tendency to refer to privileged information (RPI) during the game. This (full) model included speakers' self-report as fixed effect, by-subject and by-item random intercepts, and a by-subject random slope for speakers' self-report. LRT Model comparison revealed that a by-item random slope for speakers' self-report did not increase the model's fit, $\chi^2(3) = 0.33$, p = .847. LRT comparison between the intercept only and the full model revealed that speakers' self-report was a significant predictor of their RPI during the game, $\chi^2(3) = 29.63$, $p < .001$. As speakers' perspective-taking tendency increased, they were less likely to have leaked privileged information during the game, b = -2.16, SE = 0.48, $p < .001$.

Speakers' Situational Self-Awareness

We investigated the extent to which the perspective manipulation (other-focus, self-focus, baseline) would influence speakers' amount of experienced non-(surroundings), public and private self-awareness. The self-awareness scores per communicative setting are presented in Figure 5.

Exploratory analyses revealed that we had to exclude the data of one participant in the self-focused setting who appeared to be an outlier. Normality slightly improved, but

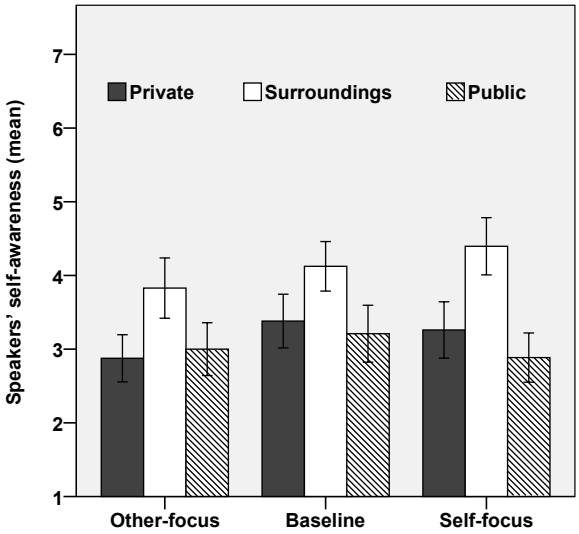


Figure 5. Means of speakers' self-awareness (private, surroundings, public) in the other-focused, baseline and self-focused settings. Error bars represent 95% confidence intervals.

the public self-awareness scores remained significantly non-normal in the baseline, $D(35) = 0.13$, $p = .029$, and in the other-focused setting, $D(35) = 0.17$, $p = .006$, as the private self-awareness score in the other-focused setting, $D(35) = 0.16$, $p = 0.16$. For public ($z = 2.61$, $p < .05$) and private ($z = 1.70$, $p > .05$) self-awareness, the data were slightly positively skewed. We employed a Kruskal-Wallis test on the data of the remaining 102 participants ($N_{baseline} = 35$, $N_{other\text{-}focus} = 35$, $N_{self\text{-}focus} = 32$). Findings showed that perspective-focus did not have a significant effect on speakers' amount of public self-awareness, $H(2) = 0.22$, $p = .897$, surroundings, $H(2) = 2.35$, $p = .309$, and private self-awareness, $H(2) = 3.82$, $p = .148$. Contrary to our expectations, self-focused speakers ($M = 2.89$, $SD = 1.02$) were not publicly more self-aware than other-focused speakers ($M = 3.00$, $SD = 1.37$) or the speakers partaking in the baseline setting ($M = 3.21$, $SD = 1.57$). In addition, no differences in the extent to which speakers were privately self-aware were found between the other-focused ($M = 2.88$, $SD = 1.48$), self-focused ($M = 3.26$, $SD = 1.26$), and baseline ($M = 3.26$, $SD = 1.26$) settings. This also held for the extent to which speakers were focused on their surroundings between the baseline ($M = 4.12$, $SD = 1.33$), other-focused ($M = 3.83$, $SD = 1.50$), and self-focused ($M = 4.40$, $SD = 1.37$) setting.

A follow-up logit mixed model analyses was conducted to test the hypothesis that public self-awareness would influence the extent to which speakers would oblige with the required audience design, expressed by less informational leakage. The full model included speakers' public self-awareness as fixed effect, and by-subject and by-item random

intercepts. Model comparison (LRT) revealed that by-subject and by-item random slopes for public did not increase the model's fit, $\chi^2(4) = 0.33$, $p = .988$. Comparison between the intercept only and the full model indicated that speakers' amount of public self-awareness was not a significant predictor of informational leakage during the game, $\chi^2(1) = 0.16$, $p = .685$, $b = 0.16$, $SE = 0.39$.

Addressees' Perception of Speakers' Perspective-Taking

Addressees' perception of speakers' perspective-taking was normally distributed in all three conditions. Addressees in the baseline ($M = 5.17$, $SD = 0.98$), other-focus ($M = 5.21$, $SD = 0.87$), and self-focus ($M = 5.29$, $SD = 0.77$) setting perceived speakers' perspective-taking tendency to the same degree, $F(2, 100) = 0.16$, $p = .850$.

Discussion

With a more robust experimental design, we examined the influence of explicit perspective-taking instructions on speakers' tendency to leak information privileged to them. In our previous experiment, factors such as the experimental procedure and design features could have influenced the extent to which speakers retained a certain reference strategy throughout the game, interfering with the perspective-taking manipulation. By addressing these factors, we were able to successfully reduce the number of speakers routinely persisting in using a certain reference strategy from 59 (65.56%) in the first experiment to 44 speakers (42.72%) in the second one. We replicated the results of the first experiment by showing that the perspective-manipulation did not influence speakers' reference production. Other-focused and self-focused speakers were just as likely to refer to private information as the speakers who were not confronted with an explicit perspective-focus. The second experiment did also show that speakers were just as likely to refer to information not known to the addressee, regardless whether this privileged information was salient to them.

As in the first experiment, speakers' introspective perspective-taking tendency predicted their reference production during the experiment. In particular, speakers who reported to have regarded their addressees' perspective were less likely to have leaked information about their private knowledge to their addressee. However, these tendencies did not depend on the induced speakers' perspective-focus. Whether speakers were explicitly focused on their own perspective or on the perspective of their addressee, they all reported to have regarded the addressees' perspective to the same degree. Addressees

also reported that speakers had taken their perspective into account, regardless of speakers' stimulated perspective-focus (self-focus, other-focus, baseline). The results of speakers' and addressees' self-reports additionally suggest that the explicit self- versus other-focus did not influence speakers' reference behavior.

In this second experiment, we also tested the hypothesis that an explicit self-focus, rather than an explicit other-focus, would make speakers' more aware of their own reference production, and whether this awareness would result in less informational leakage. Results showed that speakers' amount of experienced public, private or non-self-awareness did not differ across the communicative settings. In addition, whether speakers were publicly, privately or non-self-aware, speakers were just as likely to refer to information not known to their addressee.

General Discussion

Two experiments investigated the question whether an explicit attention to addressees' perspective influences speakers' audience design during reference production. The intriguing finding of the research presented here is that making speakers explicitly aware of their addressees' perspective did not appear to influence the extent to which they adjust their reference production to addressees' knowledge and attentional status. Contrary to what theories of audience design suggest (Clark & Murphy, 1982), speakers still leaked information privileged to them, regardless of their explicit awareness of addressees' informational need. The results of the two experiments presented in this study indicate the complex nature of perspective-taking. Even during an easy collaborative task in which speakers had enough cognitive resources left to engage in perspective-taking, explicit instructions to regard common-ground knowledge did not reduce speakers' egocentrism.

That we evidenced speakers' informational leakage in the baseline as well as in the perspective-taking (i.e., other-focus) setting seems to suggest that perspective-taking – in the form of an accurate audience design – is not necessarily incorporated in the planning of utterances during language production. In this sense, the findings support previous research claiming that speakers plan their referential utterances on the basis of the knowledge that is immediately available to themselves, regardless of whether this information is shared and commonly known between them and their partner (e.g., Barr & Keysar, 2005; Epley et al., 2004). In these two experiments, perspective-taking did not constrain speakers' reference production. In this sense, to take over the other's perspective during conversation and using this information to construe a referential utterance that corresponded to the addressee's perspective was optional instead of obligatory. By comparing speakers' informational

leakage in a baseline referential setting to a setting in which they were self- versus other-focused and finding that neither an induced self- nor other-focus influenced speakers' informational leakage[2], we conclude that egocentrism prevailed. Even when speakers had an explicit knowledge of addressees' perspective, they were just as likely to refer to their egocentric knowledge, disregarding the perspective of their addressee.

We have two reasons to believe that the speakers in these two studies did engage in perspective-taking, but that they did not use this knowledge during reference production. First of all, as communication theories propose (e.g., Arnold et al., 2013; Clark, 1992; Clark & Carlson, 1982; Clark & Marshall, 1981; Clark & Murphy, 1982; Grice, 1975), the success of referential communication relies to a great deal on speakers' ability to regard the perspective of their interlocutor. Without the speakers' ability to take into account the knowledge and attentional status of the addressee, their reference might not be in line with the required audience design and cooperative principles of communication. Referential communication thus presupposes perspective-taking. Secondly, the data of both speakers' and addressees' self-reports support the assumption that speakers were aware of the addressee's perspective throughout the game. That is, after Experiment 1 and 2, both speakers and addressees filled out a perspective-taking questionnaire in which they were explicitly asked to indicate the extent to which speakers had been aware of the addressee's perspective, and the extent to which they had adjusted their reference production to the addressee's knowledge and attentional status. The results of the self-reports (at least for speakers) correlated with the amount of overspecification. In particular, when speakers reported that they had used redundant information on the task, they had also been more likely to *leak* information about their privileged perspective. Hence, the results of the self-reports and their relation to speakers' actual reference behavior during the game seem to suggest that speakers were aware which information addressees needed, but that this awareness did not overrule the influence of their egocentric perspective on their subsequent reference production. The self-reports did not reflect speakers' judgment of their accuracy in taking perspectives, as accuracy should be framed here as the extent to which speakers *leaked* information about their own perspective. By leaking information about their egocentric point of view, speakers provided their addressee with redundant information that could possible cue addressees about speakers' privileged perspective (Wardlow Lane & Liersch, 2012).

One tentative explanation as to why addressees' perspective was not taken into account when speakers were explicitly made aware of their partner's informational need

[2] The proportions of speakers' informational leakage were not higher in the self-focus condition compared to the baseline, suggesting that explicitly drawing speakers' attention to their privileged object (i.e., self-focus instructions) did not make them more egocentrically biased than when these instructions were absent (i.e., in the baseline).

is the possibility that speakers relied on other-generated feedback to infer whether their reference was successful or not. If speakers relied on addressees' contribution to make communication successful (Clark & Brennan, 1991; Clark & Krych, 2004), they might not have felt the need to correct for perspective mistakes themselves. That is, being under- or overinformative might be less cognitively demanding than integrating the addressee's perspective before producing a referential utterance. This reliance on addressees' cueing speakers' under- or overinformativeness could have allowed speakers' egocentrism to surface (e.g., Krauss & Fussell, 1991; Horton & Keysar, 1996). If speakers are able to rely on addressees' feedback in deciding whether a message was formulated correctly, they do not have to rely on their own cognitive judgment whether their message adheres to addressees' perspective. In this sense, self-generated feedback – by constantly monitoring whether the to be disclosed information corresponds to addressees' knowledge and attentional state – is more cognitively taxing than being able to rely on others to detect perspectives mistakes. Speakers are therefore expected to only rely on self-generated cues when other-generated feedback is not available to them (Gann & Barr, 2014). This raises the question whether our speakers would have been more attentive to the elicited other-perspective if they were not able to rely on addressees' collaborative contribution, but were designated to their own judgments. This interesting question could be explored in a research design in which addressees are not able to provide their speaker with feedback.

In the present two studies, speakers leaking information about their own perspective overspecified their referential messages. That is, speakers provided their addressee with information that they did not need to select the intended target. In this sense, addressees did not need to provide their speaker with feedback as speakers' overspecification did not result in addressees' misunderstanding. Regardless of the overinformativeness of speakers' references, addressees were always able to select the intended referent, thereby providing their speaker with positive feedback that the reference had been successful. Perspective mistakes were thus not detected, simply because informational leakage was not regarded as a violation. Though, interesting to note here is that only one addressee did cue his speaker's overinformativeness during the game. Whereas the remaining addressees remained silent on speakers' overspecfication during the game, this particular addressee asked his speaker why he included color (/size) information when the target could be distinguished from all the other common-ground figures by just its shape. Prompted by the addressee's feedback, this speaker reduced his overspecfication after a few more trials, ending up with a reference that adhered to the Gricean maxim of quantity (Grice, 1975). Importantly, during debriefing, almost all addressees asked their speaker whether the occluded figure inspired them to include redundant color (/size) adjectives to their referential message. This clearly indicates that overinformativeness communicated relevance (e.g., Davies &

Katos, 2013; Engelhardt et al., 2011) to at least these addressees, perhaps allowing these addressees to form an unintended conclusion (e.g., Wardlow Lane & Liersch, 2012). This example illustrates the importance of addressees' collaboration during referential communication. Without cues that speakers are being overinformative, speakers seem to maintain their egocentric reference strategy.

Since the explicit perspective-focus instructions did not reduce speakers' informational leakage, it could be questioned whether it would not have been better to prime attention to the self- or other-perspective by means of a prompt that was unrelated to the specific stimuli used in the experimental trials (as in Santiesteban et al., 2012; Todd, Hanko, Galinsky, & Mussweiler, 2011). However, the main objective of the research in this study was to investigate whether explicit instructions to engage in perspective-taking behavior during the task that presupposes perspective-taking would incite speakers to engage in accurate audience design and, thus, reduce the influence of speakers' egocentric perspective on reference production. This question is extremely relevant for everyday situations in which people have to engage in (accurate) perspective-taking, without having the opportunity to prime interlocutors with a certain mind-set stimulating perspective-taking prior to the interaction (e.g., Brown, 1997; Brown, 2010; Fleuridas, Nelson, & Rosenthal, 1986; Penn, 1982; Selvini-Palazzoli et al., 1980; Tomm, 1985). The explicit perspective-taking manipulation used in these two experiments should highlight for speakers which information is relevant (Grice, 1975) to include in their reference, thereby reducing the extent to which speakers overspecify or, more specifically, leak information about their privileged perspective. Hence, if these explicit instructions to regard the addressee's mental state that is active during the referential communication do not influence speakers' audience design or the extent to which they refer to private information, it is unsure whether a prior prompt or primed mind-set that do not address these particular mental states will achieve this.

An interesting topic to discuss is whether the explicit perspective-taking instructions induced a meta-cognitive awareness of perspective overall. That is, speakers being made aware of the figures they themselves were seeing could, as a result, have been made aware of the fact that different perspectives existed, including their awareness of addressees' divergent perspective. In other words, self-focused speakers could have experienced an altercentric interference – computing the addressee's perspective alongside the egocentric interpretation – and other-focused could have experienced an egocentric interference – because the egocentric, divergent perspective became more salient (Ferguson, Apperly, & Cane, 2017; Samson et al., 2010; Samuel et al., 2019). This perspective-switching might weaken any difference we might find in informational leakage between the self- and other-focused speakers, and thus seems plausible considering the insignificant leakage-differences

between the two perspective-focus conditions. However, perspective-switching imposes a cognitive burden on interlocutors (Ferguson et al., 2017; Samuel et al., 2019), and this burden would have manifested itself in significant leakage differences between the two perspective-focus conditions (self- and other-focus) and the baseline in which speakers did not receive explicit self- or other-focus instructions. Additionally, since an egocentric interpretation requires less cognitive effort than an allocentric interpretation (Apperly, Samson, & Humphreys, 2009), we are unsure whether self-focused speakers would really have experienced altercentric interferences. Recent findings by Ferguson and colleagues (2017) validate our initial expectations by showing that interlocutors do not compute the other perspective if they are instructed to remain focused on their egocentric perspective throughout the whole perspective-taking task. Furthermore, the research design of the two experiments presented in this study did not explicitly invite speakers to switch between perspectives. For instance in contrast to Ferguson et al. (2017) and Samuel et al. (2019), speakers in the self- and other-focus conditions were not imposed with different perspective-switch tasks (i.e., from informed to uninformed addressee), as they received only one perspective-prompt during all 40 trials to either regard the addressee's perspective (in the other-focus condition) or their own visual perspective (in the self-focus condition). These perspective-focus instructions were also very explicit. That is, speakers explicitly indicated, for each trial, the visual perspective of the addressee, thereby highlighting the common-ground objects including their shared object features addressees were seeing. Although speakers were made aware that addressees were only seeing those three common-ground objects that shared color and size features, speakers were still inclined to refer to the feature of the object that stood out for themselves the most. Even though speakers were aware of the other's perspective, they did not use this awareness for their subsequent audience design (e.g., Apperly et al., 2010).

An alternative explanation as to why the explicit perspective-taking instructions did not influence speakers' reference production could be that speakers engaged in a submentalizing process (Heyes, 2014) by not representing the visual scene from their addressees' perspective, but by merely coding which objects were in their addressees' line of sight. Following this object-centered spatial coding hypothesis (Santiesteban et al., 2014, 2015), speakers could have represented which figures were in front of their addressee, without mentally visualizing these common-ground figures including their shared object-features (i.e., color/size). This would entail that, across the three communicative settings (i.e., baseline, other-focus, self-focus), speakers' egocentric representation of the figures, especially the contrast between their privileged figure and the target, was the only one that was mentally activated. This could have stimulated speakers to keep referring to the contrasts they themselves were seeing, thereby leaking information about their

privileged perspective. However, it is unclear whether speakers' stimulated visual or an object-centered spatial representation of the common-ground situation would influence speakers' reference production differently. For both the visual and object-centered spatial representations, speakers' attention is still focused on the common-ground objects, including the object features that are shared between the objects (size and color). Hence, in both cases, speakers should become aware that referencing the object's shape (i.e., the only unique property of the object) is enough to allow addressees to successfully identify target. In addition, to our knowledge, the object-centered hypothesis (Santiesteban et al., 2015) has currently only been investigated in the context of language comprehension processing (e.g., Apperly et al., 2010; Keysar et al., 2000; Keysar et al., 2003). Future research thus could examine whether similar submentalizing processes during language comprehension are also at work during language production.

The two experiments described in this study did not replicate the effect of the salience of privileged information on speakers' information leakage, as evidenced by Wardlow Lane et al. (2006) and Kaland et al. (2011). An important aspect to consider is the experimental setup in which the salience of privileged information was manipulated in Wardlow Lane et al. (2006) and Kaland et al. (2011). To manipulate informational leakage, the researchers in both studies employed a within-subject design in which speakers received different instructions (in separate experimental blocks) as to how to play the referential game. In the privileged block of trials, speakers received the instruction to identify the target so that addressees were able to correctly identify the referents. In the conceal block of trials, speakers received the additional instruction to keep the identity of the hidden figure concealed from addressees. Results of Wardlow Lane et al. (2006) showed that, in the privileged block, the difference between salient (5.4%) and non-salient (0.5%) trials was marginally significant by speakers (F_1, $p < .06$) and significant by items (F_2, $p < .05$). However, when speakers were instructed *not to leak information about the concealed figure* (in the conceal trials), leakage increased to 14.4% on salient trials versus 1.4% on non-salient trials. Kaland and his colleagues (2011) supported these findings and indicated that leakage increased for the salient versus the non-salient trials when speakers received the additional instruction to conceal their private knowledge. The researchers claimed that the additional conceal instruction called for an ironic process (Wegner, 1994). When speakers were instructed not to provide information about the hidden figure, they ironically did so because they started to actively think about suppressing leakage behavior, thereby enhancing their attention to their private information. This increased attention to private information helped thoughts of actual leakage to spring to mind. Perhaps this ironic process incited by the conceal instruction caused information to become salient in speakers' mind, not the other way around. That is, even though the concept of "size" is

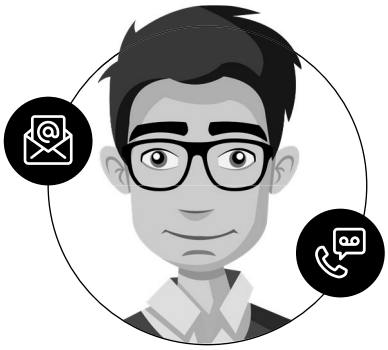
most salient in speakers' mind when speakers are presented with conceptually matching objects (e.g., a large privileged circle and a small common-ground circle) versus conceptually mismatching objects (e.g., a large privileged triangle and a small common-ground circle), the conceal instruction could have let speakers to actually pay attention to the salient size contrast presented to them. Are we able to explain why the researchers also found more informational leakage for salient versus non-salient trials on the privileged blocks in which the speakers were only instructed to identify the referents? Perhaps being confronted with different tasks instructions (i.e., identify and conceal) taxed speakers' working memory, thereby placing a higher task demand on speakers in comparison to a situation in which they are only confronted with one task instruction (i.e., identify) as in our two experiments. It could be that this more cognitively involving experimental setup inspired leakage behavior not only to occur more on salient versus non-salient trials when speakers tried to conceal their private figure, but also on trials in which they only identified the target in common-ground. This would explain why the two experiments in this study failed to replicate Wardlow Lane et al. (2006) and Kaland et al. (2011) findings. Since our speakers only received one task instruction – to identify the targets – we assume they all paid the same amount of attention to both salient and non-salient trials, probably causing informational leakage to occur for both trials equally. Given our findings, we would suggest that the salience of privileged information is not necessarily derived by the (conceptual) relationship shared between the speakers' privileged and target figure, but more by the attention this relationship receives. In other words, the size difference for two conceptual matching objects might be salient only when the instruction to conceal this difference increases one's attention to it.

Conclusion

This research showed that a stimulated attention to addressees' perspective did not influence speakers' audience design during reference production. Even during a relatively easy task and with explicit instructions to take addressees' knowledge and attentional status into account, speakers were very likely to refer to information that was not known to addressees.

Chapter 4

Can the Curse of Knowing Be Lifted? The Influence of Explicit Perspective-Focus Instructions on Readers' Perspective-Taking



This chapter is based on:

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The preregistration and the anonymized data are accessible via osf.io/kv5mu/

Abstract

Perceivers of other minds often overestimate the similarity between their own and other people's perspectives. This egocentric projection during perspective-taking is argued to originate from perceivers' tendency to use their own perspective as a referential anchor from which they insufficiently adjust away to account for an alternative interpretation. We investigated whether an explicit focus on another person's point of view allows perceivers to make sufficient perspective-adjustments, thereby attenuating their egocentric projection. Findings showed that we successfully replicated Keysar's (1994) illusory transparency of intention effect (Experiment 1) and Epley et al.'s (2004) findings that confirm perceivers' egocentric anchoring and (insufficient) adjustment during perspective-taking (Experiment 2). Interestingly, we further showed that enhancing perceivers' attention to another person's perspective both prior (Experiment 1) and during (Experiment 2) perspective-taking did not diminish egocentric projection. Findings are discussed in light of the role of feedback in making accurate perspective-adjustments.

Keywords: perspective-taking; curse of knowledge; egocentrism, egocentric anchoring and adjustment, replication.

Introduction

Perspective-taking entails that people imagine the situation from another person's viewpoint, thereby taking into account this person's thoughts, feelings, knowledge and intentions. Knowing what others might feel or think in a certain situation allows for successful interaction. Perspective-taking can thus be considered to be a vital process for social functioning (Davis, 1983). Ample research has shown, however, that perceivers of other minds often fail to appreciate the other's different vantage point, even when the social context requires them to do so (Damen, Van der Wijst, Van Amelsvoort, & Krahmer, 2019; Wardlow Lane, Groisman, & Ferreira, 2006). The question whether and under what circumstances perceivers are able to successfully infer what is going on in the mind of others has received great scholarly attention. It has been scarcely investigated, however, how people can be stimulated to successfully engage in perspective-taking. In this study, we investigate whether and how perceivers can learn to appreciate the differences that might exist between their own perceptions and that of others.

In an early study investigating perspective-taking during reading, Keysar (1994) showed that readers' knowledge about how a message was intended to be perceived cursed their ability to appreciate an alternative interpretation. In Keysar's (1994) study, participants read several scenarios in which a speaker protagonist informed an addressee protagonist – either verbally or via written messages – about a past experience. Participants read in the Restaurant scenario, for instance, that protagonist Tom took his parents to a restaurant recommended by his colleague June. Participants learned that Tom's dining experience had either been remarkable or miserable. Subsequently, participants read that, the following day, Tom left a note for June in which he wrote: "You wanted to know about the restaurant: well, marvelous, just marvelous". When participants were asked to indicate how June would interpret Tom's comment, Keysar (1994) found that the note that communicated a sarcastic intention (when the dining experience had been miserable) caused participants to wrongly infer that June too would interpret Tom's comment to be sarcastic. This in contrast to the message that communicated a sincere intention (when the dining experience had been remarkable). Note that only the participants were privileged with the knowledge that Tom's dining experience had either been miserable or remarkable. For both experiences, June had no reason to believe that Tom was being sarcastic. Keysar (1994) termed this interesting finding as the "illusory transparency of intention" phenomenon, explaining how readers thought that Tom's intention was clear to others to the extent it was clear to them, "despite the ambiguity of his utterance, and despite June's lack of access to critical information" (Keysar, 2000, p. 162). Following this line of thought, we could argue that participants who learned about Tom's miserable

experience were *cursed* (i.e., the curse of knowledge effect in Birch & Bloom, 2004, 2007) by their own knowledge of Tom's sarcastic intention. Keysar (1994) evidenced this curse of knowledge effect using different speaker protagonists who communicated with different addressees (Experiment 1), and using the same speaker who communicated with different addressees (Experiment 2 to 4). In all his experiments, readers were unable to suppress their interpretation of the speaker's communicative intention, which led them to overestimate the extent to which their interpretation of the speaker's message was shared by the naïve addressee.

Epley, Keysar, Van Boven and Gilovich (2004) showed that the inability to suppress private cognitions during perspective-taking can be explained by perceivers' tendency to use their own perspective as reference when assessing others' perspective. In an experimental design inspired by Keysar (1994), participants in Epley et al. (2004) read stories in which protagonist Tom sent ambiguous voicemail messages instead of notes (or telephone messages in Keysar, 1994) to his friends. Subsequently, participants indicated either Tom's intention with his voicemail or how they thought Tom's friend would interpret the message. Epley et al. (2004) showed that the participants perceived Tom's intention to be sarcastic when he referred to a negative experience as opposed to a positive experience. More importantly, participants used their interpretation of Tom's intention to infer that Tom's friends would too perceive Tom's sarcasm. Participants' perception of sarcasm was, however, more moderate when they judged how Tom's friends would perceive the voicemails than when they judged their own interpretation of the messages. According to Epley et al. (2004), the more moderate scores in the perspective-taking condition showed that participants did acknowledge that the voicemails would sound more ambiguous to Tom's friends than to themselves. However, since the participants believed that Tom's friends would perceive Tom's sarcasm, their perspective-judgment still reflected their own knowledge of the Tom's communicative intention. The participants thus used their own interpretation of Tom's sarcastic intention as a referential anchor from which they – although insufficiently – adjusted to infer the recipients' perspectives.

In subsequent experiments, Epley et al. (2004) showed that perceivers adjust their egocentric judgment in effortful, sequential steps to allow for a perspective-judgment that more accurately reflects the other's perspective. For instance in their "Under Pressure" experiment, Epley et al. (2004) found that perceivers need time and the attentional resources to adjust away from a self-generated egocentric anchor in judgment. In this experiment, participants performed the same task as in the "Sarcastic Messages" study, but now they judged the addressees' perspective either at their own pace or under time pressure. Results showed that perceivers would more quickly judge the addressees' perspective when their privileged interpretation of the voicemail matched (i.e., sincere

interpretation) rather than mismatched (i.e., sarcastic interpretation) with the addressees' sincere interpretation of the speaker's message. More importantly, participants were more likely to assume that addressees would also perceive the speaker's sarcasm when they were pressured to judge the addressees' perspective than when they were able to perform the task unhurriedly. Hence, when perceivers had limited resources to take into account that the message would sound more ambiguous to addressees than to themselves, they were more likely to accept an interpretation of the addressees' point of view that was biased towards their own perception of the speaker's sarcasm.

Epley et al. (2004) further evidenced that perceivers can be motivated to accurately adjust away from an egocentrically biased interpretation. In a related experiment, participants drank very similar tasting colas (i.e., Coke and Pepsi) and subsequently estimated the number of peers who would correctly identify the two soft drinks. To manipulate the extent to which participants would put effort in construing the correct estimate, one group of participants received a monetary incentive to make an accurate guess. Interestingly, the group receiving the incentive predicted that a lower percentage of peers would correctly identify the identity of the two soft drinks than the group who did not receive an incentive. Hence, the motivation to use (additional) cognitive resources might allow perceivers to correct their judgment in order to consider the possible differences between their and other people's perspectives.

Although attentional and cognitive resources could allow perceivers to adjust away from an egocentric interpretation, these adjustments made away from the self-perspective do not mean that the resulting judgment is error prone or bias free (Epley et al., 2004; see also Epley & Eyal, 2019). In fact, because perceivers are likely to accept a perspective-judgment that easily springs to mind – which so happens to be their own –, adjustments made away from the self-perspective are often insufficient and likely to err in the direction of perceivers' egocentric perspective. Epley et al. (2004) evidenced perceivers' insufficient adjustment during their study that replicated their Sarcastic Messages experiment. This time, participants estimated the number of addressees perceiving the speaker's sarcasm in either percentages (ranging from 100% sarcastic to 100% sincere) or by providing the lower and upper boundaries of these percentages (range). When the estimated percentages were compared to the estimated ranges, they did not significantly differ from the lower boundary close to a 100% sarcastic interpretation, but they did significantly differ from the upper boundary close to a 100% sincere interpretation. Epley et al. (2004) argued that these findings demonstrated that perceivers stopped adjusting when they reached an estimate of addressees' perspective that seemed plausible and satisfying on the basis of their privileged information. In this sense, perceivers' privileged information cursed the adjustment process in such a way that it was terminated too soon, resulting in a biased

judgment reflecting more of perceivers' privileged information than the addressees' true perspective.

This insufficient adjustment process during perspective-taking might explain why egocentric projection occurs more often when perceivers try to understand others in ambiguous situations, such as in the situations described above (e.g., Epley et al., 2004; Keysar, 1994; Weingartner & Klin, 2005; 2009). The ease at which perceivers' private cognitions are accessible makes it hard for perceivers to ignore or suppress them as plausible estimates of the other's perspective. In this way, perceivers' own knowledge biases (Keysar, Barr, & Horton, 1998) or 'curses' (Birch & Bloom, 2007; Camerer, Loewenstein, & Weber, 1989; Keysar, 1994) perceivers to overestimate the extent to which their private perspective is shared by others.

The Role of Focus Manipulations on Perspective-Taking Accuracy

Weingartner and Klin (2005) showed that perceivers' curse of knowledge effect during perspective-taking also generalizes to natural reading. In an experiment in which they tracked participants' reading time, Weingartner and Klin (2005) presented participants with a target line that described the addressee's (June) sincere interpretation of the speaker's (Tom) message. For example in the dining experience where the speaker (Tom) emails that his experience at the restaurant has been "(…) marvelous, just marvelous", participants read that "June thought that Tom really liked the restaurant". Weingartner and Klin (2005) showed that this target line did not cause any interpretation problems when participants knew the experience had been marvelous. However, when participants had previously learned that the speaker's dining experience had been miserable, reading the addressee's (June) reaction did cause interpretation problems that were reflected in a slowdown in reading. When participants' privileged information suggested a sarcastic intention, reading times on the target line were longer than when privileged information suggested that the speaker (Tom) was being sincere.

Weingartner and Klin (2005) showed that this curse of knowledge effect on perspective-taking slightly reduced when the addressee (June) was brought into focus before participants read the speaker's (Tom) message. According to the authors, in previous studies that investigated the same phenomenon, the speaker protagonist (Tom) was still in the foreground when participants read his message (e.g., "When Tom arrived at work the next morning, he left a note for June, saying (…)"). This focus on Tom would make all information associated with him accessible (e.g., Garrod & Sanford, 1990; Sanford, Clegg, Majid, 1998), including participants' knowledge of the speaker's experience. This attention on the speaker's perspective would make it hard for participants to disregard their knowledge of

the speaker's experience while judging the addressee's (June's) perception of the message. Therefore, Weingartner and Klin (2005), argued that bringing the addressee (June) instead of the speaker (Tom) into focus (e.g., "When June arrived at work the next morning, she read the following note from Tom, saying (...)") would make information associated with the addressee accessible, including the information that is known by this addressee. Findings showed some preliminary support for this perspective-focus hypothesis. When the addressee's perspective was brought into focus before the target line ("June thought that Tom really liked the restaurant") appeared, participants' reading time on the target line did not significantly differ between negative and positive experiences during the analysis over items (F2), but it did for the analysis over subjects (F1). Hence, only the F2 analysis showed that the perspective-focus on the addressee helped participants to realize more quickly that their privileged information was not accessible to this addressee. According to the F1 analysis, however, participants' privileged information about the speaker's experience still influenced their judgment of the addressee's interpretation of the message. Weingartner and Klin (2005) concluded that a stronger perspective-focus manipulation is needed to improve readers' perspective-taking accuracy.

The purpose of the current study is to investigate whether explicit and repeated instructions to focus on the addressee's perspective serves as a stronger perspective-focus manipulation. Following the egocentric anchoring and adjustment view (Epley et al., 2004), perspective-judgments are likely to be anchored in an egocentric perspective, because this self-perspective is highly accessible. In turn, enhancing the accessibility of the other-perspective could decrease this egocentric anchoring and stimulate perceivers to adjust away from this egocentric frame of reference. The literature, however, paints a conflicting picture with regard to this line of reasoning. On the one hand, a small number of studies have shown that enhancing the accessibility of other-related thoughts allows individuals to sufficiently adjust away from an egocentric interpretation (e.g., Elekes, Varga, & Kiraly, 2016; Naylor, Lamberton, & Norton, 2011; Ferguson et al., 2017; Samuel et al. 2019). For instance, perceivers are very likely to spontaneously represent another person's visual perspective if they are aware that this different perspective exists (Ferguson et al., 2017; Samuel et al., 2019) and what this different perspective entails (Elekes et al., 2016; see also Schneider, Slaughter, & Dux, 2017). That is, in Elekes et al. (2016), participants were slower to identify whether a number they heard (e.g., "6") was the same number that was visually presented before them when they performed this task with a partner rather than alone. This slowdown especially occurred when participants' partner had an asymmetrical (e.g., "6") rather than symmetrical (e.g., "8") point of view on the same visual scene. Elekes et al. (2016) argued that participants were slower to respond from their egocentric perspective because they experienced so-called altercentric interferences

(see also Ferguson et al., 2017), during which they spontaneously computed their partner's (asymmetrical) perspective without having the explicit goal to do so.

Furthermore, studies have even shown that perceivers find it difficult to adopt an egocentric frame of reference once they learn to represent an altercentric perspective (Samuel et al., 2019) or when they learn that they cannot use the self as a point of reference (Naylor et al., 2011). Naylor et al. (2011), for example, examined the extent to which online consumers would be persuaded by reviewers of online products whose identity was unknown (hence, ambiguous) or known to be similar or dissimilar to consumers themselves. The authors followed the line of reasoning that individuals are more likely to be persuaded by similar rather than dissimilar others (e.g., Simons, Berkowitz, & Moyer, 1970). Interestingly, their findings showed that consumers were likely to perceive ambiguous reviewers in the same manner as similar reviewers. This overestimation in similarities caused consumers to be equally persuaded by reviewers whose identity was unknown as by reviewers who were identified as being similar. This egocentric projection was attenuated when consumers were instructed to think about dissimilar others (study 3) or were informed about the possible dissimilarity of the other person (study 4) before they read the ambiguous reviews. Hence, the accessibility of other-related thoughts decreased individuals' tendency to use their own preferences as a referential anchor when they tried to understand others' preferences.

In contrast to these findings, two recent studies showed that highlighting another person's point of view does not always seem to increase perceivers' tendency to engage in spontaneous (Damen, Van Amelsvoort, Van der Wijst, & Krahmer, 2019) and accurate (Damen, Van der Wijst, & Krahmer, 2019) perspective-taking. In Damen, van Amelsvoort et al.'s (2019) spatial perspective-taking task, speakers were still very likely to locate an object from their egocentric perspective, even after they received explicit and repeated instructions to focus on how the objects appeared to another person. In support of this view, another study by Damen, Van der Wijst et al. (2019) showed that speakers in a referential communication game were still very likely to refer to objects in their privileged ground, even though they received explicit and repeated instructions to focus on the objects that were visible to their addressee before they identified the common ground target (see also Kaland, Krahmer & Swerts, 2014; Wardlow Lane et al., 2006). Regardless of an explicit focus on their another person's perspective, speakers in Damen, Van Amelsvoort et al. (2019) and in Damen, Van der Wijst et al. (2019) were very likely to anchor their language production on information available and salient to themselves, thereby disregarding their addressee's perspective.

Previous studies investigating the extent to which the accessibility of another person's perspective influences perceivers' perspective-taking accuracy used different methods to operationalize accuracy. That is, studies either operationalized accuracy as the extent to which perceivers experienced altercentric interferences (e.g., Elekes et al., 2016; Ferguson

et al., 2017; Samuel et al., 2019), prioritized another person's visual frame of reference (Damen, Van Amelsvoort et al., 2019), or produced a reference that is optimally designed from an addressee's point of view (Damen, Van der Wijst et al., 2019). It remains unexplored, however, whether the accessibility of another person's perspective helps individuals to make an accurate judgment of another person's state of mind when they are explicitly asked to judge this person's perspective. In addition, additional research is necessary to understand the extent to which the accessibility of other-related thoughts helps perceivers to appreciate another person's unique vantage point when the situation is more ambiguous, such as when perceivers need to attribute intentions to ambiguous written correspondences (e.g., Keysar, 1994; Epley et al., 2004; Weingartner & Klin, 2005, 2009).

The Current Study

In this study, we investigate whether instructions designed to enhance attention to another person's perspective helps perceivers to successfully track which knowledge is available to this person. We investigate this question in two experiments. In the first experiment, we aim to replicate Keysar's illusory transparency of intention effect (termed here as the curse of knowledge effect) on perspective-taking during reading. Instead of using a student (undergraduates) sample as in the original and previous replication studies (e.g., Gerrig, Ohaeri, & Brennan, 2000; Mante-Estacio & Bernardo, 2014; Moreno-Ríos, Rodríguez-Menchen, Rodríguez-Gualda, 2011; Weingartner & Klin, 2005, 2009), we aim to replicate Keysar's (1994) findings with a non-student population, using a sample that is more representative of the population at large. In order to achieve this aim, we targeted adults varying in educational backgrounds who were – at the time of the study – employed by a large organization in the Netherlands. Since written (e-mail) correspondences are paramount in organizations, employees regularly need to attribute intentions to ambiguous information. Hence, in this way, we are able to strengthen the generalizability of the research findings (Peterson, 2001) and investigate whether the curse of knowledge effect dictates the perspective-taking process of adults who regularly infer intentions through written (e-mail) correspondences. In the first experiment, we subsequently examine whether explicit instructions to focus on a protagonist's perspective prior to the reading task attenuates readers' tendency to assume that this character shares their privileged knowledge. In the second experiment, we aim to replicate Epley et al.'s (2004) egocentric anchoring and adjustment process of perspective-taking. Additionally, we examine whether readers are more able to assess the knowledge of characters when they receive explicit and repeated instructions to keep track of this character's knowledge before judging this character's perspective.

EXPERIMENT 1

In this experiment, our first aim is to replicate the curse of knowledge effect. In particular, we expect that participants are more likely to perceive the speaker's sarcasm when they know the speaker is referring to a negative rather than a positive experience. Subsequently, we expect that participants will use this privileged information to judge the addressee's interpretation of the speaker's message. That is, we hypothesize that participants will judge that the addressee will also perceive the speaker's sarcasm more when participants know the speaker is referring to a negative than to a positive experience. We further examine whether an explicit focus on addressee's knowledge and attentional status diminishes the curse of knowledge effect on perspective-taking. In Weingartner and Klin (2005), the perspective-focus manipulation consisted of participants reading the text from the point of view of the protagonist (the addressee) who received the ambiguous message. In this experiment, we enhance this perspective-focus manipulation by explicitly instructing participants to acknowledge the addressee's perspective in a prior perspective-focus session. We investigate whether these explicit instructions to regard the addressee's point of view stimulates participants to suppress private cognitions during subsequent perspective-taking. We hypothesize that participants who are explicitly instructed to focus on the addressee's perspective are less likely to think that this addressee will also perceive the speaker's sarcasm more for negative than for positive experiences, than the participants who do not receive such explicit perspective-focus instructions.

Method

Participants

An online questionnaire was sent to 700 employees of a bank-holding organization in the Netherlands. Out of these 700 employees, 325 accessed the online questionnaire. We excluded the participants who did not fully complete the questionnaire and based our analyses on the remaining 229 participants (116 females, 111 males, 2 unknown, M_{age} = 48.0 years, age range 27-65). This sample size was more than 7 times larger than the size of the original sample reported in Keysar (1994)[1]. Since we distributed the questionnaire among employees working at different levels in the organization, we also collected employees' educational background. This educational background ranged from preparatory secondary

[1] Keysar (1994) examined the illusory transparency of intention effect in four experiments that contained a within-subjects design of which the sample sizes ranged from 16 to 56 participants.

vocational education (5.7%) to a PhD (0.9%), with the majority of the participants having completed a higher vocational education (33.6%). As a remuneration, participants were able to take part in a perception workshop conducted by the first author after data collection had ended[2].

Design

Participants were randomly allocated to either the addressee-focus (*N* = 118) or the no-focus (*N* = 111) condition. In each condition, participants read two scenarios in which the same speaker protagonist (Tom) referred to a past experience (positive, negative) to the same addressee protagonist. In contrast to Keysar (1994), we used the same addressee protagonist for both stories because we wanted to examine the extent to which participants' focus on the addressee protagonist would diminish their curse of knowledge effect during reading. We balanced the order in which participants learned about the speaker's negative versus positive experience. This resulted in a 2 (perspective-focus: addressee-focus, no-focus) x 2 (event information: negative, positive) x 2 (presentation order: negative-positive, positive-negative) design in which perspective-focus and presentation order were treated as between-subjects factors and event information as a within-subjects factor.

Materials and Procedure

Event Information. The experimental design was inspired by Keysar's (1994) first and second experiment. After having given their consent, participants read a scenario that described the speaker's (Tom) experience that had either been negative (sarcastic intention) or positive (sincere intention). At the end of the scenario, the speaker made a comment to the addressee (June) about this experience. Information about the speaker's experience was privileged to participants and not accessible to the addressee. In both cases, the addressee had no reason to believe that the speaker was being sarcastic. After reading the speaker's comment, participants indicated how the addressee would interpret the speaker's message. In the original study, participants responded to the question "Did the addressee (June) take the speaker's (Tom's) comment as sarcastic?" by circling one of the three options (yes, maybe, no). To allow for a more nuanced judgment in which participants could also select a sincere interpretation (see also Galinsky, Magee, Inesi & Gruenfeld, 2006), we asked participants to answer the question "How did the addressee (June) interpret the speaker's (Tom's) comment?" on a seven-point scale (1 = very sincere, 7 = very sarcastic)

[2] There were 35 employees who attended this workshop.

instead of using Keysar's three answer-option. The same held for the follow-up question that asked participants to indicate the speaker's intention with his conveyed message. In the original study, this question was formulated as "Did you think the speaker (Tom) was being sarcastic?" (yes, maybe, no). In our study, we also nuanced this question and asked participants to answer, "What was the speaker's (Tom's) intention with the comment that he sent to the addressee (June)?" on a seven-point scale (1 = very sincere, 7 = very sarcastic).

The participants in Keysar's study received a booklet containing eight different scenarios that appeared in four different versions. These versions were based on whether Tom spoke or wrote his sincere versus sarcastic message in the scenario. In contrast to

Table 1. Introductory Scenario and Scenarios Describing the Speaker's Experience (Negative, Positive)

Introductory Scenario		
June is 32 years old and employed at the marketing department of a well-known company in the Netherlands. She manages a very diverse team with great enthusiasm. June is a true adventurer. Her adventurous character and her love for nature are perfectly manifested in her hobby mountain climbing. June spends all her free hours climbing. She particularly enjoys reaching the top after a few hours of great exertion. The fact that no mountain is high enough for June is apparent from her recent victory on the Kilimanjaro in Tanzania; Africa's highest mountain. This year, she challenges herself to climb the highest mountain in the world, the Mount Everest in Nepal. June combines her love for nature and sport with her love for animals. She regularly travels across the Netherlands to find sponsors for the foundation "Animal Care"; a foundation she more than happily supports.		
	Scenario 1	Scenario 2
Background Information		
	Tom asked his colleague, June, to recommend a restaurant. Tom's parents were in town and he wanted to take them to a good place. June told Tom: "I can strongly recommend this new Italian restaurant, called Venezia. I just had dinner there last night and it was marvelous. Let me know how you all enjoy it". That evening, Tom took his parents to Venezia.	Within a few days, a former colleague of Tom will be joining June's marketing team. June is curious about this new colleague and she decides to send Tom an e-mail: "How is Katherine as a colleague? Is she nice?". As it turns out, Tom knows Katherine well. At his former employer, Tom worked frequently with Katherine.
Event Information		
Negative	The food was unimpressive and the service was mediocre. The following morning, Tom e-mailed to June: "You wanted to know about the restaurant, well, marvelous, just marvelous."	Tom hadn't always gotten along with Katherine, because she had been rude to him. With that in mind, Tom responded by e-mail: "Oh yeah, Katherine is really nice."
Positive	The food was indeed delicious and the service impeccable. The following morning, Tom e-mailed to June: "You wanted to know about the restaurant, well, marvelous, just marvelous."	Tom had always gotten along with Katherine. With that in mind, Tom responded by e-mail: "Oh yeah, Katherine is really nice."

Note: These texts were presented to participants in Dutch.

Keysar, we were mainly interested in the extent to which participants would attribute their privileged knowledge onto an uninformed other, regardless of the modality in which the speaker conveyed his message. We therefore translated two of Keysar's scenarios (see Table 1) in which Tom's message was conveyed by e-mail. To make sure our employee sample could still identify with the described social situations, we adjusted the protagonists' student/professor roles from the second scenario into colleague/colleague roles. The scenarios are presented in Table 1.

Perspective-Focus. We allocated half of the participants to an addressee-focus condition. In this condition, perception questions explicitly instructed participants to infer the perspective of the protagonist (June) who assumed the role of addressee in the two subsequent scenarios. The addressee-focus session started by asking participants to read an introductory scenario (see Table 1) that introduced the addressee's character and preferences. This information could later on be used when participants made choices based on the addressee's perspective. Subsequent to this introductory scenario,

Table 2. Perception Questions (Q) and their Answer Options (A)

	Question		Answer
Q1	Soon, June's employer organizes an annual outing. To make sure that the activities are adapted to the wishes of the employees, everybody is asked to choose one activity out of two available options. **Which activity will June choose?**	**A1**	**A workshop skydiving**
		A2	A visit to the local beer brewery
Q2	The mountain sport magazine "To the top" has a special offer for June. Since she has been a loyal customer, she will receive three editions of a magazine of her choice. June is able to choose one magazine out of the two following options. **Which option will June choose?**	A1	In Picture; a magazine for film fanatics
		A2	**Nature Life; a magazine for nature enthusiasts**
Q3	The organization were June is working wants to donate a particular amount to a good cause. Employees are asked to vote for one of the two proposed foundations. **Which foundation will June choose?**	**A1**	**Animal Foundation**; **devoting itself to protect animals from negligence and maltreatment**
		A2	Make a Wish; foundation for children with a severe disease
Q4	This year, June is responsible for the organization of the monthly teambuilding outing. Her staff let June know to be interested in two possible activities. It is up to June to decide which one it will be. **Which activity will June choose?**	A1	City game; a joint quest through a city of choice.
		A2	**Wall-climbing; a sport activity for the whole team**

Note: Optimal choices are presented in bold.

participants answered four consecutive perception questions that asked participants to take the addressee's perspective (see Table 2). For example, one question described how the organization of the addressee asked her employees to choose an outing out of two available options (A. Workshop skydiving, B. A visit to the local beer brewery). Participants answered the perception question "Which activity will the addressee (June) choose?". If participants were to regard the addressee's perspective, they would choose the option that adhered the most to her perspective (A). The options for all four questions were presented in a counterbalanced order.

The addressee-focus session elicited participants' focus on the addressee's perspective. That is, out of the 118 participants, no one answered all four perception questions incorrectly. Most participants (N = 49) answered three out of four questions from the addressee's point of view (M = 3.08, SD = 0.84). Results of the one-sample t-test indeed showed that the addressee-focus instructions helped readers to answer at least two (test value) out of 4 questions from the addressee's perspective, $t(116) = 13.83$, $p < .001$. After the addressee-focus session, participants were directed to the two scenarios. Participants in the no-focus condition did not receive explicit focus instructions and were at the start of the experiment immediately directed to the two scenarios. Afterwards, demographics were collected. Participants were debriefed via e-mail one week after the data collection had ended.

Results

Two separate factorial ANOVAs for repeated measures were run to test for the effect of event information (positive, negative) on (a) participants' own perception of speaker's sarcasm and on (b) participants' judgment of addressee's perception of the speaker's sarcasm. Both ANOVAs included event information as a within-subjects factor and presentation order (negative first-positive second, positive first-negative second) as a between-subjects factor. The second ANOVA investigating participants' judgments of addressee's perception of sarcasm also included perspective-focus (no-focus, addressee-focus) as a between-subjects factor. Participants' educational background was added as a covariate to control for the large variety in participants' education level. We reduced the number of educational backgrounds to two (lower, higher); the lower- till intermediate vocational education profiles were allocated to the lower level, and the higher vocational education through PhD profiles were allocated to the higher level. Inspection of the data resulted in the exclusion of three cases that were considered to be outliers (z = 3.11, deviance = 3.84). The analyses were thus based on 117 participants in the addressee-focus condition, and on 109 participants in the no-focus condition.

Participants' Perception of Sarcasm

We expected that participants would interpret the speaker's message on the basis of event information. In line with our expectation, participants perceived more sarcasm in the speaker's reference to his past experience when this experience had been negative (M = 5.33, SD = 1.54) rather than positive (M = 1.75, SD = 0.91), $F(1, 223) = 33.34$, $p < .001$, r = .36, $\eta^2 = .13$[3]. This effect was the same for participants with a lower and higher educational level, $F(1, 223) = 3.11$, $p = .079$, $r = .12$, $\eta^2 = .01$. The analysis revealed a main effect for presentation order on participants' perceptions of speaker's sarcasm, $F(1, 223) = 18.31$, $p <$ .001, $r = .28$, $\eta^2 = .08$. Participants' overall perception of sarcasm (i.e., across both scenarios) was higher when the first scenario they read communicated a negative (M = 3.78, SE = 0.08) rather than a positive experience (M = 3.31, SE = 0.08). Presentation order did not interact with event information, $F(1, 223) = 0.37$, $p = .543$. This means that participants did not perceive more sarcasm in the scenarios that communicated a negative versus a positive experience when the negative experience (M = 5.05, SD = 1.46) was preceded by the positive experience (M = 1.57, SD = 0.80), than when the negative experience (M = 5.61, SD = 1.57) was followed by the positive experience (M = 1.94, SD = 0.98).

Participants' Judgment of Addressee's Perception of Sarcasm

We hypothesized that participants would use privileged information about the speaker's past experience to infer the addressee's perception of sarcasm. Conform this expectation, participants thought the addressee would perceive the speaker's sarcasm more when privileged information suggested that the speaker was being sarcastic (M = 3.07, SD = 1.67) rather than being sincere (M = 2.11, SD = 1.14), $F(1, 221) = 14.98$, $p < .001$, r = .25, $\eta^2 = .06$[4]. Event information interacted with participants' educational background, $F(1, 221) = 4.33$, $p = .039$, $r = .14$, $\eta^2 = .02$. A follow-up MANOVA revealed that the participants with a lower educational level were more strongly affected by their privileged information than speakers with a higher educational level. Participants with a lower educational level ascribed higher perceived sarcasm scores when privileged information suggested that the speaker was being sarcastic (M = 3.43, SD = 1.72) rather than being sincere (M =

[3] The positive and negative sarcasm scores showed a skewed distribution. An additional Yuen's trimmed mean t-test that can be generalized to repeated measurement designs (Mair & Wilcox, 2016) replicated the main effect of event information, $t(135) = -26.88$, $p < .001$, $d = .91$, trimmed mean difference = -3.99, 95% CI [-4.29, -3.70].

[4] In both conditions, the sarcasm scores (positive, negative) were positively skewed. An additional ANOVA using a robust measurement relying on 20% trimmed means (Mair & Wilcox, 2016) replicated the main effect of event information, $F(1, 139.68) = 10.50$, $p < .001$, and the non-significant effect of perspective-focus, $F(1, 139.90) = 0.99$, $p = .322$, and the non-significant perspective-focus and event information interaction, $F(1,139.68) = 0.37$, $p = .543$.

2.01, *SD* = 1.0[6]), in comparison to the participants with a higher educational background ($M_{negative\ experience}$ = 2.91, *SD* = 1.63; $M_{positive\ experience}$ = 2.15, *SD* = 1.17).

There was no significant main effect of presentation order on participants' judgments of addressee's perception of sarcasm, $F(1, 221) = 1.57$, $p = .212$. Presentation order did, however, interact with event information, $F(1, 221) = 5.29$, $p = .022$, $r = .15$, $\eta^2 = .02$. Participants thought the addressee would perceive sarcasm more when the negative experience (M = 3.15, *SD* = 1.59) was preceded by the positive experience (M = 1.87, *SD* = 1.13), than when the negative experience (M = 2.98, *SD* = 1.85) was followed by the positive experience (M = 2.35, *SD* = 1.09).

Perspective-Focus on Judgments of Perceived Sarcasm

We subsequently examined the hypothesis that participants explicitly instructed to focus on the addressee's perspective would be less likely to overestimate the similarity between their and the addressee's interpretation of the speaker's message than the participants who did not receive these explicit focus instructions. The mean scores of participants' judgments of addressee's perception of speaker's sarcasm as a function of perspective-focus (no-focus, addressee-focus) and event information (positive, negative) are presented in Figure 1.

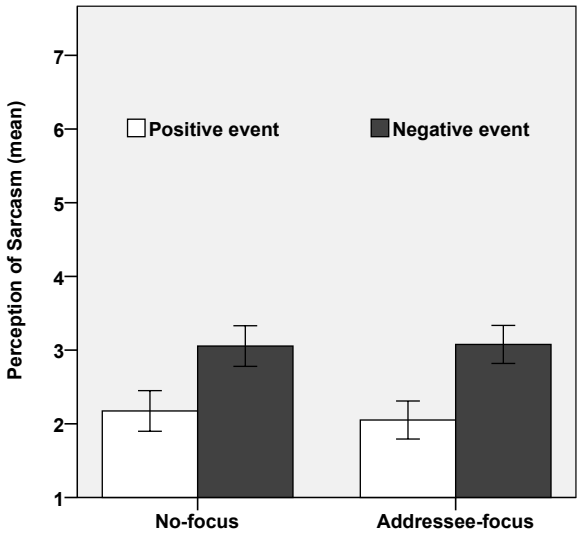

Figure 1. Mean scores of participants' judgment of addressee's perception of sarcasm (1 = very sincere, 7 = very sarcastic) as a function of perspective-focus (no-focus, addressee-focus) and event information (positive, negative). Error bars represent 95% confidence intervals.

Analyses revealed no significant main effect of perspective-focus on participants' judgments of addressee's perception of sarcasm, $F(1, 221) = 0.11$, $p = .741$. Participants' judgments of addressee's perceived sarcasm did not differ between those participants who had followed an antecedent addressee-focus session, ($M_{negative\ experience} = 3.08$, $SD = 1.74$; $M_{positive\ experience} = 2.05$, $SD = 1.14$), and those who did not follow such session ($M_{negative\ experience} = 3.06$; $SD = 1.60$; $M_{positive\ experience} = 2.17$, $SD = 1.13$). In contrast to our hypothesis, perspective-focus did not interact with event information, $F(1, 221) = 0.33$, $p = .566$. The difference in participants' judgment of the addressee's perceived sarcasm was the same between negative ($M = 3.06$, $SD = 1.60$) and positive ($M = 2.17$, $SD = 1.13$) experiences in the no-focus condition, as between negative ($M = 3.08$, $SD = 1.74$) and positive ($M = 2.05$, $SD = 1.14$) experiences in the addressee-focus condition. We ran our analyses again while controlling for age and gender, but our initial findings remained unchanged. Both age and gender did not influence our results.

Intermediate Discussion

Experiment 1 successfully replicated Keysar's (1994) illusory transparency of intention effect (termed here as the curse of knowledge) in a non-student population. Readers were more likely to infer that an uninformed addressee would perceive a speaker's sarcasm when participants' privileged information suggested that the speaker was being sarcastic rather than being sincere. Findings further showed explicit instructions to focus on the addressee's perspective did not diminish the curse of knowledge effect. Readers overestimated the extent to which their privileged perspective was shared by an uninformed addressee, regardless of their explicit focus on addressee's knowledge and attentional status. It could be that the stimulated addressee-focus did not lead to sufficient perspective-adjustments because readers did not select the most optimal choice from the addressee's perspective in the addressee-focus session. In the subsequent scenario study, these readers could therefore have been more aware of their own perspective at the expense of being aware of the addressee's perspective. To test this assumption, we computed the mean-difference score ($M_{diff} = 1.59$) of participants' judgments of addressee's perception of sarcasm between the negative and positive event information and tried to predict this score by the number of non-optimal choices ('errors') made during the addressee-focus session. This follow-up regression analysis revealed that the number of 'errors' made during the addressee-focus session did not predict this mean-difference score, $F(1,115) = 0.92$, $p = .341$. Perceivers who did not 'optimally' complete the addressee-focus session were just as likely to overestimate the accessibility of their privileged knowledge to the addressee as those readers who did choose the optimal choice from the addressee's perspective. In

addition, in the instances in which participants did not select the most optimal choice from the addressee's perspective during the addressee-focus session, they still had been forced to represent the addressee's perspective. This seems to suggest that the addressee-focus session caused perceivers to be aware of the addressee's perspective (whether correct or incorrect), but this awareness did not cue them to adjust their egocentric perspective-judgment in the subsequent scenario study.

Perception questions are not only used to create a general awareness of interlocutor's mental representation of the world, but are especially used to address the false-beliefs that exist between these interlocutors (e.g., Tomm, 1985). In the previous study, readers' general awareness of the addressee's perspective was stimulated by asking readers to answer questions from the addressee's perspective in a prior addressee-focus session. These questions addressed the addressee's attitude and her preferences that were unrelated to the subsequent scenario study. This addressee-focus session did not address the addressee's interpretation of the speaker's message. Therefore, readers' false-belief that the addressee would interpret the speaker's message in a similar way as them was not addressed. In the second experiment, we examine whether an explicit focus on addressees' uninformed perspective helps readers to acknowledge that their privileged information is not accessible to these addressees. We examine this question by replicating and extending the "Sarcastic Messages" experiment by Epley and colleagues (2004).

EXPERIMENT 2

The aim of the second experiment is twofold. The first aim is to replicate Epley et al.'s (2004) anchoring and adjustment findings. In particular, we hypothesize that participants will judge the speaker's message to be more sarcastic when clarifying event information suggests a sarcastic intention than when it suggests a sincere intention. More importantly, we expect that participants will use their own interpretation of speaker's sarcasm as an anchor from which they adjust way to account for addressees' alternative interpretation. However, we expect that these perspective adjustments are insufficient and that participants' judgements of addressees' perspective still reflects their own interpretation of the speaker's message. Secondly, we aim to investigate whether perceivers' anchoring and adjustment process is affected by explicit instructions to regard the addressee's uninformed perspective. It is suggested that perceivers might be better at suppressing private information during perspective-taking when they become aware that they cannot use their own mental states as predictor for the states of others (Mitchell, 2009). In addition, it is argued that the more cues perceivers receive about the knowledge status of others, the less likely it is that their

predictions about the others' perspective will err in the direction of the self-perspective (Keysar, Barr, & Horton, 1998; Mitchell, 2009; Naylor, Lamberton, & Norton, 2011). Following this line of thought, highlighting the uninformed perspective of the addressee would make this perspective more accessible, enabling participants to more accurately adjust away from an egocentric interpretation. We hypothesize that, compared to the baseline, explicit instructions to attend to the uninformed perspective of the addressee will help participants to acknowledge which information is accessible to this protagonist, thereby stimulating participants to sufficiently adjust away from an egocentric interpretation. In turn, we expect that, compared to the baseline, egocentric biases will increase when participants are explicitly instructed to attend to their own informed perspective. The preregistration of our hypotheses and analyses can be consulted through the Open Science Framework (osf.io/8759w/; osf.io/vxa8u/; see Damen, Van der Wijst, Van Amelsvoort & Krahmer, 2018, March 19).

Method

Participants and Sample Size

In contrast to Experiment 1, this study was conducted among students who were recruited from the campus of Tilburg University in the Netherlands. Epley and colleagues (2004) allocated 30 participants to the intention condition and 40 participants to the interpretation condition. For our replication and extension study (4 x 2 design), we assumed a medium effect[5]. Calculating the required sample size using G*Power (version 3.1.9.2) indicated that we would require a total sample size of only 72 participants (19 per experimental condition) to obtain an error probability of .05 and a power of .95. On the other hand, Simonsohn (2015) recommends collecting at least 2.5 the original sample size to obtain sufficient power to detect an effect. However, considering the effect sizes of the original study and our power calculation, we aimed to recruit a minimum of 50 participants per experimental condition. After a period of three months, we reached a total sample size of 231 participants. We had to exclude 20 participants because they were either non-native speakers of the language of the experiment (N = 4), recognized the voice-actor (N = 11), were not a student (N = 2), had already participated in the rating experiment (N = 1) or did not finish the whole experiment (N = 2). Our analyses were based

[5] On the basis of the information provided in Epley et al. (2004), we were able to calculate the effect sizes of the main effect of event information, $F(1, 68) = 172.65$, $\eta_p^2 = .71$, of the simple effect of event information in the intention condition, $F(1, 68) = 11.33$, $\eta_p^2 = .14$, and of the simple effect of event information in the interpretation condition, $F(1, 68) = 6.99$, $\eta_p^2 = .10$. These F-values and effect sizes constitute one large and two medium effects.

on the 52 participants in the intention condition, 54 in the interpretation condition, 52 in the speaker-focus condition, and 53 in the addressee-focus condition (142 women, 69 men, M_{age} = 21.62 years, age range 17-43).

Design

Participants were randomly allocated to one of the four conditions (intention, interpretation, speaker-focus, and addressee-focus). In each condition, participants read twelve scenarios in which a speaker referred to a past experience (positive, negative) to an addressee. This resulted in a 4 (condition: intention, interpretation, speaker-focus, addressee-focus) x 2 (event information: positive, negative) design in which condition was treated as a between-subjects factor and event information as a within-subjects factor.

Materials

The 12 original scenarios and voicemail messages were obtained from Keysar (p.c.) and are available via osf.io/rpn2z. As in Epley et al. (2004), we presented the scenarios to participants in booklets, but displayed these booklets on a computer screen[6]. Half of the scenarios in these booklets described a positive event, whereas the other half described a negative event. Following the guidelines set by Epley et al. (2004), we created two versions of these booklets. One booklet contained a random order of negative versus positive events, and another one contained its mirror image. We determined the order of the positive versus negative scenarios by a coin flip and we made sure that no more than three scenarios of the same valence appeared consecutively. Unlike Epley et al. (2004), we counterbalanced the order in which the scenarios appeared in the booklets. That is, for both booklets we construed two versions in which the second version contained a reversed order of the scenarios. We translated the scenarios and voice-mail messages into Dutch (the language of the experiment).

Adjustments to the Materials. We made small adjustments to the voicemail messages based on the concerns raised by Gerrig, Ohaeri, and Brennan (2000). Gerrig et al. (2000) replicated Keysar (1994) and found that certain messages in the original materials were perceived to be particularly sincere or particularly sarcastic in the absence of clarifying information. To construe true ambiguous voicemail messages in our replication study, we (a) changed possible biased sentences in the voicemail messages into more neutral versions and (b) asked uninformed listeners to rate the voicemail messages in the absence of contextual information in a rating experiment.

[6] In their second experiment, Epley et al. (2004) also presented the scenarios to participants in digital booklets.

Rating Experiment. To ensure that the voicemail messages would sound truly ambiguous to uninformed listeners, we invited 26 undergraduates to rate the 12 voicemail messages and one practice item. The data of one participant were excluded because she recognized the voice-actor. This resulted in 25 participants who participated for course credits (20 women, 5 men, M_{age} = 21.80, age range 18-40). Participants rated three versions of each voicemail. These versions differed with regard to the speaker's intonation (sincere, ambiguous, sarcastic). We recorded and normed the voicemail messages that differed in their degree of stress and intonation (from sincere to sarcastic) to ensure that we would select those voicemails that were normed to be ambiguous. For the sincere messages, the speaker refrained from stressing sentences or words that referred to his experience. In the sarcastic version, the speaker explicitly stressed counterfactual information, but reduced this stress in the ambiguous versions. The resulting 36 voicemail messages were presented within-participants in a maximized random order so that different versions of the same voicemail were not rated subsequently. The voicemails started automatically and were played to participants over headphones. At the end of each voicemail, a navigation button appeared that allowed participants to navigate to the next page. On this new page, participants answered the question: "How do you interpret this voicemail message from the speaker (Tom)?" on a 7-point scale (1 = as very sincere, 7 = as very sarcastic). Participants listened to the voicemails once and were unable to pause or replay the voicemails. The messages sound truly ambiguous if their mean-score falls between the scores of 3 (mildly sincere) to 5 (mildly sarcastic) (Gerrig et al., 2000; Weingartner & Klin, 2009). Based on this criterion, we accepted five voicemail messages that were normed with a mean-score between the 3 and 5, and for which their scores fell in between the two other variants of the same voicemail. The overall mean of these five voicemail messages showed that they sounded truly ambiguous in the absence of clarifying event information (M = 3.73, SD = 0.83).

The three versions (sincere, ambiguous, sarcastic) of the seven remaining voicemail messages were adjusted and re-normed in a second rating experiment. We invited 29 new participants to rate the 21 voicemails. We excluded one participant who did not complete the entire survey and one participant who recognized the voice-actor. This resulted in 27 participants who participated for course credits (21 females, 6 males, M_{age} = 24.63, age range 18-50). Participants rated 21 voicemail messages and 3 practice items. Based on participants' ratings, we accepted the seven remaining ambiguous voicemails (M = 3.70, SD = 0.81). The ratings of the 12 accepted voicemails from both rating experiments showed that the messages were rated as mildly sincere (M = 3.54, SD = 0.74), reflecting a score that is close to the midpoint of the 7-pointscale.

Procedure

All participants gave their informed consent before participating in the study. We replicated the experimental procedure of Epley et al. (2004). Participants received booklets that contained twelve scenarios that described the life of the speaker "Tom". For example, for scenario "The Dance Class" participants read the following text:

> Tom was on his way to the first night of his ballroom dancing class when he saw Eileen, an old friend from his dorm last year. When he told her that he was on his way to a ballroom dancing class, she excitedly replied, "I'm thinking of taking that class, but I can't make it to tonight's class--I am having dinner with friends. Could you call me when you get back and tell me how it is?"

Each scenario ended with either a positive or a negative experience. For instance, "The Dance Class" scenario ending with a positive experience ended with: "During class, the instructor spent the entire time teaching the class fun, new dances and he even allowed Tom to practice for a few minutes after class." In contrast, the scenario ending with a negative experience ended with: "During class, the instructor spent the entire time taking attendance and filling out lengthy forms and questionnaires. Tom didn't even have a chance to practice dancing." Both endings followed with: "When Eileen looked at her mobile after dinner, she found a message from Tom: (…)" Subsequently, participants listened to the speaker's voicemail message. For the dance class scenario, this voicemail sounded like:

> Eileen, this is Tom. Hope you enjoyed your dinner. About that ballroom dancing class: judging from tonight's class, I can't think of a better way to spend my Tuesday evenings. Anyways, give me back a call and I'll fill you in on the details. Bye.

After listening to the speaker's voicemail, participants were told they had to answer a few questions about the scenario and voicemail. Participants in the intention condition indicated whether the speaker intended his message to be sarcastic, whereas participants in the interpretation condition indicated whether the addressee would perceive the speaker's message to be sarcastic. In Epley et al. (2004), participants made their choices by circling one of the three options (yes, maybe, no). To allow for more nuances in participants' responses, we asked participants to make their judgements on a 7-point Likert-scale (1 = as very sincere, 7 = as very sarcastic). Afterwards, participants answered a comprehension question (i.e., "What was the story about?") with three answer options (i.e., "A. Break dance lesson, B. Street dance lesson, C. Ballroom dance lesson) to encourage participants to carefully attend to the materials. These questions did not target information expressed

in the clarifying event information and were relatively easy (n = 6) or more difficult (n = 6) to answer. When participants answered the comprehension question incorrectly, they were informed to attend to the materials more carefully. Overall, participants answered almost all questions correctly (M = 10.74, SD = 1.07), and the number of correct responses was not significantly different between conditions, $H(3) = 2.84$, $p = .417$. At the end of the experiment, demographics were collected and participants were debriefed and given course credits for their participation.

Perspective-Focus Conditions. New to the experimental procedure was the addition of two perspective-focus conditions (speaker-focus, addressee-focus). We replicated the procedure of the interpretation condition and, additionally, manipulated the extent to which participants were explicitly instructed to acknowledge their own knowledge of the speaker's perspective (speaker-focus) or the extent to which they were explicitly instructed to acknowledge the uninformed perspective of the addressee (addressee-focus). This manipulation took place before participants indicated how the addressee would interpret the speaker's comment. For example in the addressee-focus condition, after listening to the speaker's voicemail he left behind for the addressee (Eileen), participants read that they were going to be asked a few questions about the scenario and voicemail. The instructions continued with: "When answering the questions, try to take on the addressee's (Eileen's) perspective and try to imagine yourself in the addressee's (Eileen's) situation" with the addressee's name highlighted in bold. Subsequently, participants answered two focus-questions. The first being: "What does the addressee (Eileen) expect of the dance class?" followed with two answer options: A) "Eileen expects that the dance class will be fun and informative", B) "Eileen expects that the dance class will be boring and useless". If participants selected the right option (A), they read: "Right! Eileen expects that the dance class will be fun and informative". If participants selected the wrong option (B), they read: "Wrong! Eileen expects that the dance class will be fun and informative". Afterwards, participants answered a second perspective-focus question: "What does the addressee (Eileen) know about Tom's first dance class?" with the answer options (A) "Eileen knows what Tom did during the first dance class", (B) "Eileen does not know what Tom did during the first dance class". Again, participants were informed whether their selection had been wrong (A) or right (B) from the addressee's perspective. In the speaker-focus condition, participants were instructed to "(…) try to take on the speaker's (Tom's) perspective and to imagine yourself in the speaker's (Tom's) situation" with the speaker's name highlighted in bold. For example, after "The Dance Class" scenario and voicemail, the first focus-question asked participants: "What does the speaker (Tom) think about the first dance class?". Participants could either select: A) "Tom thinks the class was fun and instructive" or B) "Tom thinks the class was boring and useless". Depending on the valance of event

information, either A or B could be correct. Participants were informed whether they had made the right or wrong choice from the speaker's perspective.

Participants' Perspective-Taking Tendency. Additionally to the replication study, we asked participants to fill out a questionnaire that measured the extent to which they thought they had acknowledged the addressees' perspective. This questionnaire contained 8 items (e.g., "I was very much aware of what the speaker's (Tom's) friends knew about his experience"). The items were alternated by 7 fillers questions (e.g., "I liked to read the stories"). Participants answered the declarative sentences on a 7-point scale (1 = totally disagree, 7 = totally agree). The scale had a high reliability (Cronbach's $\alpha = .70$). The individual items are presented in Table 3.

Table 3. Items of Participants' Perspective-Taking Tendency Scale

While reading the stories, listening to the voicemails and while answering the questions that followed the voicemails:
1. I especially took into account what I knew about Tom's experience (R)
2. I found it difficult to imagine how Tom's friends would interpret the voicemails (R)
3. I especially took into account what Tom's friends knew about Tom's experience
4. I could easily imagine how Tom's friends would interpret the voicemails
5. I was especially aware of what Tom's friends knew about Tom's experience
6. I tried to imagine how Tom's friends would understand the voicemails as much as possible
7. I was especially aware of what I knew about Tom's experience (R)
8. I was aware that Tom's friends could interpret the voicemail messages differently from me

Note: The (R) code signals that items were recoded before analysis.

Minor Adjustments to the Procedure. Epley et al. (2004) presented the scenarios in printed booklets to participants. Epley et al. (2004) did not specify how the voicemail messages were played to participants, nor did the authors indicate whether participants were able to read the scenario text while listening to the voicemail and when answering the dependent variable. In their second "Under Pressure" study, Epley et al. (2004) presented the scenarios to participants on a computer screen. In order to listen to the voicemail and to respond to the dependent variable, participants had to navigate to a new blank screen. After the voicemail message had been played, participants answered the dependent variable. This procedure separates the scenario information from the voicemails and the dependent variable and, therefore, does not allow participants to re-read the scenario after hearing the voicemails. For this replication study, we therefore followed the procedure of Epley et al.'s (2004) "Under Pressure" study (see also Moreno-Ríos et al., 2011). We presented the

scenarios (visually) on a computer screen. When participants navigated to the next page, the voicemail messages would start to play automatically. These voicemail messages were presented only auditorily to participants. Participants navigated to a new blank screen to answer the dependent variable. Participants were not allowed to go back to previous pages, nor were participants allowed to listen to the voicemail message more than once. To familiarize participants with the experimental procedure, the experiment started with one practice trail in which participants read another story in the life of speaker Tom. We based the content of this practice item on the 13th voicemail message we received from Keysar (p.c.) that did not have a corresponding scenario. This story described a neutral event and clarifying event information was not manipulated in this scenario.

Results

Anchoring and Adjustment

We examined participants' egocentric anchoring and (insufficient) adjustment during perspective-taking. In particular, we investigated the hypothesis that participants would judge the speaker's message to be more sarcastic when participants' clarifying event information suggested the speaker had the intention to be sarcastic (negative event) as opposed to being sincere (positive event). We further examined the hypothesis that participants would judge the addressees' interpretation of the speaker's message on the basis of their privileged clarifying event information, and that participants would insufficiently accommodate for addressees' lack of information by providing a judgment of the addressees' perspective that would be more moderate than their own. In Figure 2, we presented the mean scores of participants' own perception of the speaker's sarcasm (intention condition) and participants' judgement of addressees' perception of the speaker's sarcasm (interpretation condition) as a function of event information (positive, negative).

We repeated Epley et al.'s (2004) statistical analyses and submitted the average sarcasm scores to a 2 (condition: intention, interpretation) x 2 (event information: negative, positive) analysis of variance (ANOVA) with repeated measures on event information. The analysis showed a significant main effect of event information, $F(1, 104) = 205.41$, $p < .001$, $\eta_p^2 = .66$, that was qualified by a significant interaction between event information and condition, $F(1, 104) = 66.00$, $p < .001$, $\eta_p^2 = .39$[7]. In line with our expectations, participants in the intention condition rated the speaker's comment to be more sarcastic when they received negative event information ($M = 5.77$, $SD = 0.77$) than when they received positive event information

[7] The original findings remained unchanged when we controlled for the presentation order of the scenarios.

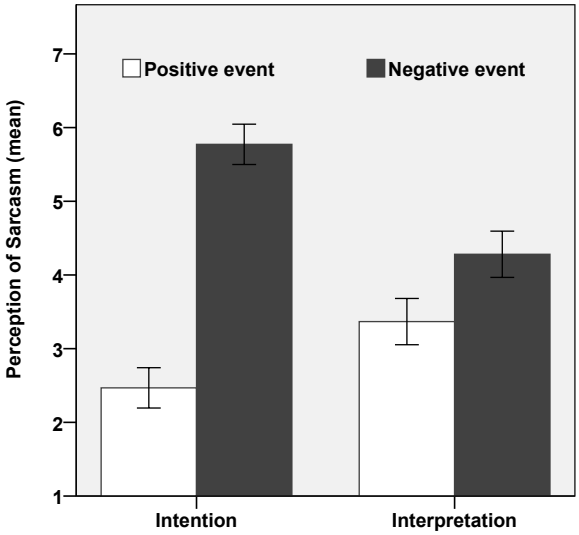

Figure 2. Mean sarcasm scores (1 = very sincere, 7 = very sarcastic) as a function of condition (intention, interpretation) and event information (positive, negative). Error bars represent 95% confidence intervals.

(M = 2.47, SD = 0.92). Interestingly, the difference between participants' perception of sarcasm between positive and negative event scenarios remained, but was significantly smaller when participants judged how the addressee would perceive the speaker's message ($M_{negative\ event}$ = 4.28, SD = 1.25, $M_{positive\ event}$ = 3.37, SD = 1.22)[8]. In addition, for both negative and positive event scenarios, participants' adjustments were significant. As we hypothesized, for negative event scenarios, the mean sarcasm score was significantly lower in the interpretation condition (M = 4.28, SD = 1.25) than in the intention condition (M = 5.77, SD = 0.77), $t(104) = 7.35$, $p < .01$. Furthermore, for positive event scenarios, the mean sarcasm score was significantly higher in the interpretation condition (M = 3.37, SD = 1.22) than in the intention condition (M = 2.47, SD = 0.92), $t(104) = -4.26$, $p < .01$. These findings replicate Epley et al.'s (2004) anchoring and adjustment effect. When judging the addressees' perception of sarcasm, participants first anchored their judgment onto their own interpretation of the speaker's message and adjusted away from this interpretation to account for the more uninformed perspective of the addressees. These adjustments were, however, still insufficient, because participants still thought that addressees would perceive more sarcasm when participants knew the speaker was being sarcastic than when participants knew the speaker was being sincere.

[8] The variances were unequal for negative, $F(1, 104) = 4.66$, $p = .03$, and positive event scenarios, $F(1, 104) = 14.03$, $p < .01$. However, Hartleys' F_{max} (Pearson & Hartley, 1954 in Field, 2013) showed that the variance ratio was .84, which is below the critical value of 2 for a sample size between 30 and 60. We therefore continued with the parametric analysis.

We repeated our analysis using a linear mixed effects model, using the LMER function from the lme4 package in R (version 3.4.2, CRAN project; R Core Team, 2017). This allowed us to control for random item (scenarios) and subject effects (Baayen & Milin, 2010). Following Barr, Levy, Scheepers, and Tily (2013), we construed a maximal model that included condition (intention, interpretation), event information (positive, negative), and the condition and event information interaction as fixed factors. We report the results from the maximal random effect structure that first converged (Barr et al., 2013). The confidence intervals were estimated by parametric bootstrapping over 100 iterations. The estimated coefficients, standard errors and the structure of the maximal model are presented in Table 4.

Table 4. Estimated Coefficients and Standard Errors for the Mixed Model Fitted to Participants' Judgment of Addressees' Perception of Sarcasm as a Function of Condition and Event Information

	b	SE_b	*t*	*95% CI*
Intercept	4.08	0.23	17.65	3.66, 4.55
Positive Event Information	**-0.54**	**0.24**	**-2.36**	**-1.04, -0.08**
Intention	**1.70**	**0.30**	**5.69**	**1.10, 2.27**
Intention, Positive Event Information	**-2.76**	**0.34**	**-8.11**	**-3.44, -2.12**

Note: The interpretation condition and negative event information were treated as the reference categories to which the other levels were contrasted. Significant results are presented in bold. We included random intercepts for both items and subjects. A comparison with the intercept-only model proved that the inclusion of the by-subject random slope for event information and the by-item random slope for the condition and event information interaction were justified by the data, $\chi^2(11) = 143.14$, $p < .001$. A Likelihood Ratio Test (LRT) revealed that adding presentation order to the model did not improve the model's fit, $\chi^2(8) = 13.38$, $p = .099$.

The findings of the linear mixed effect analysis showed that we replicated the main effect of event information, $b = -0.54$, $SE = 0.24$, 95% BCI [-1.04, -0.08], and the event information and condition interaction, $b = -2.76$, $SE = 0.34$, 95% BCI [-3.44, -2.12], from our analysis of variance.

Perspective-Focus and Perspective-Taking

We investigated the extent to which the focus manipulation (addressee-focus, speaker-focus) influenced participants' judgment of addressees' perception of sarcasm. More specifically, we expected that, compared to a baseline in which explicit focus instructions were absent (interpretation), an explicit focus on addressees' perspective (addressee-focus) should decrease participants' tendency to overestimate the similarity between their and the addressees' interpretation of the ambiguous voicemail message. In addition, we expected that, compared to the baseline, this egocentric projection should

increase when participants were explicitly focused on their own privileged information (speaker-focus). Figure 3 shows the mean scores of participants' judgement of addressees' perception of the speaker's sarcasm as a function of condition (interpretation, addressee-focus, speaker-focus) and event information (positive, negative).

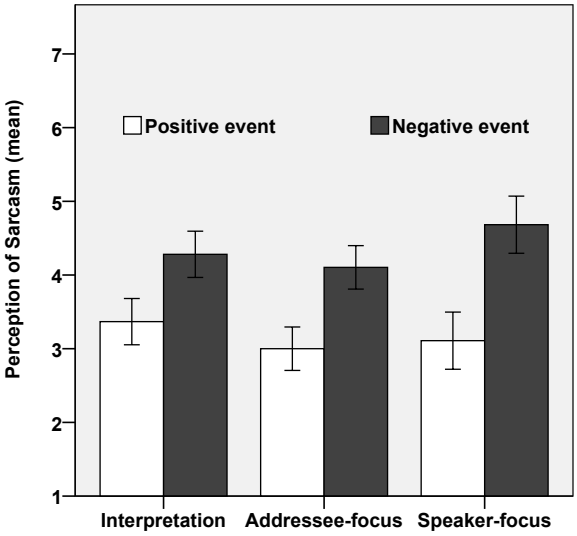

Figure 3. Mean sarcasm scores (1 = very sincere, 7 = very sarcastic) as a function of condition (interpretation, addressee-focus, speaker-focus) and event information (positive, negative). Error bars represent 95% confidence intervals.

To investigate our hypotheses, we submitted the average sarcasm scores to a 3 (condition: interpretation, addressee-focus, speaker-focus) x 2 (event information: positive, negative) analysis of variance (ANOVA) with repeated measures on event information[9]. We treated the interpretation condition as the baseline to which the other two conditions were contrasted. The analysis revealed a main effect of event information, $F(1, 156) = 77.74$, $p < .001$, $\eta_p^2 = .33$, but a non-significant main effect of condition, $F(2, 156) = 2.35$, $p = .099$, $\eta_p^2 = .03$, and a non-significant interaction between condition and event information, $F(1, 156) = 2.08$, $p = .129$, $\eta_p^2 = .03$. Simple contrasts revealed that the difference between positive ($M = 3.37$, $SD = 0.17$) and negative event ($M = 4.28$, $SD = 0.16$) information in the interpretation condition remained unchanged in both the addressee-focus ($M_{negative\ event} = 4.10$, $SD = 0.17$; $M_{positive\ event} = 3.00$, $SD = 0.17$; $p = .104$) and speaker-focus ($M_{negative\ event} = 4.68$, $SD = 0.17$; $M_{positive\ event} = 3.11$, $SD = 0.17$; $p = .668$) conditions. In contrast to our hypotheses,

[9] The sarcasm scores (positive, negative) were normally distributed in all conditions.

addressee-focused readers still projected their perception of the speaker's sarcasm onto addressees, and this egocentric projection did not increase for the speaker-focused readers.

We additionally performed a linear mixed effect model to take random item and subject effects into account. We construed a maximal model that included condition (interpretation, addressee-focus, speaker-focus), event information (positive, negative), and the condition and event information interaction as fixed factors. The interpretation condition was treated as the reference category to which the other two conditions were contrasted. The confidence intervals were estimated by parametric bootstrapping over 100 iterations. The estimated coefficients, standard errors and structure of the maximal model are reported in Table 5.

Table 5. Estimated Coefficients and Standard Errors for the Mixed Model Fitted to Participants' Judgment of Addressees' Perception of Sarcasm as a Function of Perspective-Focus and Event Information

	b	SE_b	*t*	*95% CI*
Intercept	4.29	0.21	19.72	3.89, 4.71
Positive Event Information	**-0.93**	**0.22**	**-4.20**	**-1.37, -0.51**
Addressee-focus	-0.19	0.25	-0.77	-0.69, 0.28
Speaker-focus	0.39	0.26	1.60	-0.10, 0.93
Addressee-focus, Positive Event Information	-0.16	0.37	-0.50	-0.86, 0.59
Speaker-focus, Positive Event Information	**-0.64**	**0.33**	**-1.70**	**-1.31, -0.01**

Note: The interpretation condition and negative event information were treated as the reference categories to which the other levels were contrasted. Significant findings are presented in bold. We included random intercepts for both items and subjects. A comparison with the intercept-only model proved that the inclusion of the by-subject random slopes for event information and the by-item random slopes for condition and event information were justified by the data, $\chi^2(22) = 144.08$, $p < .001$. A Likelihood Ratio Test (LRT) revealed that adding order to the model did not improve the model's fit, $\chi^2(9) = 10.97$, $p = .278$.

The results of the linear mixed effect analysis reflected the findings of the mixed analysis of variance, but revealed an additional interaction effect between condition and event information. In particular, the difference between negative and positive event information was higher in the speaker-focus condition ($M_{difference}$ = 1.57) than in the interpretation condition ($M_{difference}$ = 0.91), b = -0.64, SE = 0.33, BC 95% [-1.31, -0.01]. This finding suggests that participants were more likely to project their own knowledge of the speaker's sarcastic intention onto uninformed addressees when they had been explicitly instructed to focus on what they themselves knew about the speaker's experience (speaker-focus condition) than when these explicit focus instructions were absent (interpretation condition).

Accuracy on Perspective-Focus Questions. In both the addressee- and speaker-focus conditions, participants answered two focus-questions for each of the 12 scenarios. Participants' responses to the 24 focus-questions were coded as correctly (1) or incorrectly

(0). Overall, participants answered most focus-questions correctly (M = 22.71, SD = 1.30). We examined whether the number of correct responses was different as a function of condition. Since the number of correct responses was non-normal in both the addressee-focus, $D(53) = 0.23$, $p < .001$, $Z_{skewness} = -2.33$, and speaker-focus, $D(52) = 0.25$, $p < .05$, $Z_{skewness} = -2.32$, condition, we performed a non-parametric Kruskal–Wallis test. The analysis revealed that speaker-focused readers made fewer errors (M = 23.13, SD = 0.91) than addressee-focused readers (M = 22.30, SD = 1.49), $H(1) = 8.65$, $p = .003$.

Participants' Perspective-Taking Tendency. Exploratory analyses revealed that participants' self-reported perspective-taking tendency was normally distributed in both the addressee- and speaker-focus conditions ($p = .2$), but significantly non-normal distributed in the interpretation condition, $D(54) = 0.13$, $p = .019$; $Z_{kurtosis} = 2.01$, $p < .05$. We therefore performed a non-parametric Kruskal-Wallis test to examine whether participants' self-reported perspective-taking tendency differed across conditions. The analysis revealed that this was the case, $H(2) = 17.25$, $p < .001$. Pairwise comparisons with adjusted p-values showed that participants' perspective-taking tendency was significantly lower in the speaker-focus (M = 3.83, SD = 0.64) than in the addressee-focus condition (M = 4.54, SD = 0.93), $H(1) = 25.70$, $p < .05$. Additionally, speaker-focused participants reported a lower perspective-taking tendency than participants in the interpretation condition, (M = 4.30, SD = 0.95), $H(1) = 36.27$, $p < .001$. Participants' perspective-taking tendency did not differ between the interpretation and addressee-focus condition, $H(1) = -10.57$, $p = .703$.

In a follow-up linear mixed effect analysis, we investigated whether participants' self-reported perspective-taking predicted their actual perspective-judgment. The full model included participants' self-report as a fixed effect and random intercepts for both subjects and items. The p-values were obtained using the Likelihood Ratio Test (LRT) in which we compared the full model with the intercept only model. The LRT revealed that participants' self-reported perspective-taking tendency was a significant predictor of their misattribution of the speaker's sarcasm, $\chi^2(1) = 8.40$, $p < .001$. As participants' perspective-taking tendency increased, they had been less likely to impute their knowledge of the speaker's sarcasm onto the uninformed addressees, $b = -0.22$, $SE = 0.07$, $p < .01$.

Discussion

Experiment 2 replicated Epley et al.'s (2004) egocentric anchoring and adjustment approach to perspective-taking. Readers who received information about how an ambiguous voicemail should be interpreted used this information to judge the speaker's intention. More importantly, readers used their own perception of the speaker's intention to infer how

an addressee protagonist – to whom this clarifying information was not accessible – would perceive the speaker's message. Readers used their own interpretation of the speaker's message as an anchor from which they insufficiently adjusted to infer the addressee's perspective. Findings from our second experiment further showed that instructing readers to focus on privileged information attenuated their perspective-adjustment. Interestingly, however, explicit instructions to focus on the addressee's uninformed perspective did not stimulate readers to make sufficient adjustments away from their self-perspective. Whether readers received explicit instructions to focus on a perspective that was dissimilar to their own (addressee-focus), readers still judged the addressee protagonist's perspective on the basis of their own interpretation of the speaker's message.

Readers' accuracy score on the perspective-focus questions showed that readers were able to correctly assess the perspective of both the speaker and addressee protagonist. Exploratory analyses revealed, however, that accuracy decreased when readers had to take a perspective that was different (addressee-focus) rather than similar (speaker-focus) to their own. Exploratory analysis further revealed that readers' self-reported perspective-taking tendency predicted their behavior during the experiment. Participants with a higher perspective-taking tendency had been less likely to overestimate the similarity between their privileged knowledge and the knowledge of the addressee. The self-reported perspective-taking tendency partly depended on readers' stimulated perspective-focus. Speaker-focused readers reported a lower perspective-taking tendency than addressee-focused readers. Interestingly, readers' perspective-taking tendency did not differ between those who received explicit addressee-focus instructions and those who did not (interpretation condition). This suggests that explicit addressee-focus instructions did not stimulate readers to accurately engage in perspective-taking.

General Discussion

Readers do not always successfully track which information is known by characters portrayed in a story. Especially in ambiguous situations, when readers receive information about how to interpret a message, they fail to acknowledge that this information might not be accessible to others. This leads to instances in which readers' privileged perspective curses their tendency to acknowledge an alternative interpretation (e.g., Epley et al., 2004; Keysar, 1994; Moreno-Ríos et al., 2011; Naylor et al., 2011; Weingartner & Klin, 2005, 2009; West, 1996). Readers who overestimate the similarity between their knowledge and the knowledge of uninformed protagonists fall prone to this egocentric projection because they fail to monitor and correct for perspective-mistakes (e.g., Barr & Keysar, 2002; Horton

& Keysar, 1996; Keysar, Barr, Balin, & Paek, 1998; Keysar, Barr, & Horton, 1998). The two experiments presented in this study further showed that neither an enhanced accessibility of other-related thoughts prior (Experiment 1) nor during (Experiment 2) reading (and listening in Experiment 2) helped readers to correct their egocentric projection. Despite readers' attention to protagonists' uninformed perspective, readers still assumed that privileged information was accessible to uninformed others.

In this study, we successfully replicated both Keysar's (1994) illusory transparency of intention (termed here as the "curse of knowledge") effect on perspective-taking (Experiment 1), and Epley et al.'s (2004) perspective-taking process of egocentric anchoring and adjustment (Experiment 2). Although the curse of knowledge effect had been previously reproduced in a student sample (e.g., Gerrig et al., 2000; Mante-Estacio & Bernardo, 2014; Moreno-Ríos et al., 2011; Weingartner & Klin, 2005, 2009), findings of our first experimental study showed that the curse of knowledge effect also generalizes to a non-student population. In addition, to our knowledge, this was the first attempt of an independent research team to replicate Epley et al.'s (2004) findings directly. The size of the reproduced effects corresponds to the original effects, strengthening the reliability of the original and reproduced findings (Open Science Collaboration, 2015). The description of the experimental design and statistical analyses in Epley et al. (2004) created a 'replication recipe' (Zwaan, Etz, Lucas, & Donnellan, 2018) that allowed us to examine the soundness of the egocentric anchoring and adjustment framework. Moreover, preregistering our hypotheses and analyses before conducting the direct replication and extension study allowed us to build on this framework by examining the extent to which readers' could be stimulated to more accurately adjust their perspective-judgment. In this way, we have shown the importance of both conceptual and direct replications to further our understanding of the scientific phenomenon being examined (Zwaan et al., 2018).

In the two experiments in this study, readers did not make use of information that was shared between protagonists (i.e., their common ground), but readers relied more on information that was privileged to themselves when making a perspective-judgment. Following the anchoring and adjustment model of perspective-taking, readers monitor their judgment for perspective-errors and adjust these errors when they are detected. We predicted that highlighting the knowledge state of the addressee protagonist before and during reading would help readers to detect perspective-errors and to adjust possible violations. However, readers still engaged in egocentric projection and highlighting the addressee's perspective did not seem to help readers to detect and adjust perspective-taking errors.

It could be that readers did not detect perspective-errors because they believed that both protagonists were operating according to the principles of cooperative communication (Grice, 1975). According to this audience-design explanation (Clark & Murphy, 1982),

readers assumed that the addressee protagonists would perceive the speaker's sarcasm because they believed that the speaker had designed his message in such a way that the addressees would be able to understand his communicative intention (see also Gerrig, Brennan, & Ohaeri, 2000; Gerrig, Ohaeri, & Brennan, 2000). In this case, readers assumed that the speaker and the addressees shared a common ground to which the ambiguous message could be interpreted. For example, readers could operate on the belief that addressees had some prior knowledge about their speaker (e.g., that the speaker generally is sarcastic) of which the speaker was also aware that this knowledge was available to the addressees (i.e., their common ground; see also Keysar, 1994). Following this line of argumentation, egocentric projection should only diminish when readers have no reason to believe that the protagonists share a common ground. However, findings of two studies (Weingartner & Klin, 2009; Moreno-Ríos et al., 2011) that replicated and extended Keysar (1994) dispute that the curse of knowledge effect relies on readers' believe in a shared common ground between protagonists. Weingartner and Klin (2009) manipulated the common ground of the protagonists by introducing an "unintended" addressee who accidentally read the speaker's message. Moreno-Ríos et al. (2011) manipulated the transparency of the speaker protagonist's communicative intention by introducing a third character who transmitted the speaker's message to the addressee. Interestingly, readers still misattributed their perception of sarcasm onto uninformed addressees even when the speaker's message was read by an unintended addressee (Weingartner & Klin, 2009) and when the message was transmitted by an unfamiliar speaker who had no intention to be sarcastic (Moreno-Ríos et al., 2011). It thus seems that the audience-design hypothesis cannot explain why readers' curse of knowledge influences perspective-taking.

We believe that the focus-manipulation employed in these two experimental studies did indeed stimulate readers to focus on a different perspective, but that these readers did not sufficiently consider this alternative perspective during perspective-taking. In line with previous studies (Damen, Van Amelsvoort et al., 2019, **Chapter 2**; Damen, Van der Wijst et al., 2019, **Chapter 3**), we have shown that an awareness of perspective differences is not enough to stimulate people to acknowledge perspective differences sufficiently during perspective-taking. Perceivers were still very likely to overestimate the similarity in perspectives, regardless of their explicit and repeated awareness on another person's different knowledge and attentional status. On the basis of our findings and the previously discussed literature, we argue that this egocentrism prevails due to insufficient adjustments made during perceivers' egocentric anchoring and adjustment process (e.g., Barr & Keysar, 2005; Epley, 2008; Epley & Eyal, 2019; Epley et al., 2004; Epley & Gilovich, 2006; Keysar & Barr, 2002). The accessibility (see also "fluency" in Birch, Brosseau-Liard, Haddock & Ghrear, 2017) of perceivers' privileged information acts as a "curse" (Birch &

Bloom, 2004) during this adjustment phase. More specifically, the ease by which perceivers are able to solve the message's ambiguity on the basis of privileged information stagnates the adjustment phase in the sense that perceivers are likely to accept an interpretation of another person's perception that is biased to their own perspective.

The patterns found in our current study further support this anchoring and insufficient adjustment account. First of all, recall that, for both experiments, readers allocated to the addressee-focus condition answered several focus questions that highlighted the addressee's perspective. Readers' accuracy on these focus trials was high as readers answered almost all focus questions correctly. This finding supports the assumption that the perspective-focus manipulation was able to stimulate readers to focus on a different perspective than their own. Secondly, the data of readers' self-report also indicated that readers were aware of the perspective they were manipulated to focus on. That is, after Experiment 2, readers filled out a perspective-taking questionnaire in which they indicated the extent to which they had been aware of the addressee's perspective and the extent to which they had acknowledged this more naïve perspective during the perspective-taking trials. Findings of this self-report showed that addressee-focused readers were indeed more aware of the addressee's uninformed perspective than the speaker-focused readers were. Moreover, readers' self-report also correlated with their perspective-taking accuracy. More specifically, the more readers reported they had been aware that the addressee did not have access to readers' privileged information, the less likely they assumed that the addressee would also perceive the speaker's sarcasm. Combined, these findings show that an explicit focus or awareness of a different perspective is not enough to stimulate perspective-taking accuracy.

Limitations and Future Directions

The question that could arise here is whether the scenarios used in the present study map onto the type of e-mails (Experiment 1) and voicemail messages (Experiment 2) people are likely to encounter. The topics addressed in the scenarios used in the present study were carefully selected to represent the type of correspondences individuals are likely to encounter in everyday life, either when conversing with co-workers (Experiment 1) or with peers (Experiment 2). In addition, the scenarios used in the current study were conceptually (Experiment 1) or directly (Experiment 2) replicated from previous investigations. Moreover, the validity of the topics addressed in the scenarios has been shown by related studies that investigated perspective-taking during reading (e.g., Keysar et al., 1994; Weingartner & Klin, 2005, 2009), and listening (Epley et al., 2004). Nevertheless, the e-mail correspondences between co-workers in Experiment 1 did not contain task related topics or judgments.

Hence, it might be worth investigating whether the curse of knowledge effect is replicated in ambiguous task related communications.

Furthermore, it could be questioned whether the explicit perspective-taking measurements used in the present study align with perspective-taking that occurs during uninterrupted reading or listening. In our experiments, participants read a text and subsequently judged a protagonist's perspective, possibly disrupting the natural flow of reading. Previous studies focusing on perspective-taking during online reading have shown, however, that participants were slower to read a character's perspective when this perspective mismatched rather than matched with participants' perspective (Weingartner & Klin, 2005; see also Weingartner & Klin, 2009). In our study, we have shown that this slowdown in reading generalizes to an offline, egocentric response when participants' explicitly judge story characters' perspective. Even though our findings correspond to these online perspective-taking studies, we invite future studies to examine whether our findings replicate to situations in which readers are not disrupted during reading.

Trying to shift readers' attention to an alternative perspective prior and during reading did not improve readers' perspective-taking accuracy. Readers' perception of the speaker protagonist's sarcasm seemed to be encoded so strongly that readers did not sufficiently engage in a monitoring and adjustment process (e.g., Epley et al., 2004; Epley, 2008; Keysar & Barr, 2002). One thing that could have contributed to readers not adjusting their judgment is the fact that they were never confronted with the consequence of their misattribution of sarcasm. In other words, readers were never shown how the addressee actually interpreted the speaker's comment and, hence, that the speakers' intention was not transparent to the addressee. It could be that receiving information about the correctness of their interpretation (i.e., feedback) signals to readers that, in future assessments, they should look more closely for egocentric projection mistakes. In a study that investigated perceivers' ability to learn from another person's feedback, West (1996) examined the influence of feedback on perceivers' ability to correctly predict a target's preference for quilt patterns. For each quilt pattern, perceivers first made a prediction about the target's preference for this pattern, using a 7-point rating scale (1 = dislike very much, 7 = like very much). This prediction was followed by the target's own rating of the quilt. This rating provided perceivers with feedback showing how much their judgement deviated from the target's true preference. The more the perceivers' rating deviated from the target's perspective, the more informative was the feedback. Subsequently to this feedback, perceivers provided their own preference-rating of the pattern. Perceivers' first assessment of the target's preferences all showed egocentric projection. That is, if perceivers liked the pattern, they thought the target did too. Interestingly, this egocentric projection decreased on subsequent trials when the feedback became more informative.

The more perceivers learned about the target's preferences, the less likely they were to project their egocentric preferences onto the target (see also 'perspective-getting' in Eyal, Steffel, & Epley, 2018). This finding suggests that confronting perceivers with the accuracy of their assessments allows them to encode the other's perspective more accurately on subsequent trials. Future research might investigate whether feedback allows readers to be less cursed by privileged information when assessing another person's interpretation of an ambigeous message.

Conclusion

This research successfully replicated Keysar's (1994) illusory transparency of intention effect (Experiment 1), and Epley et al.'s (2004) findings confirming perceivers' egocentric anchoring and (insufficient) adjustment during perspective-taking (Experiment 2). Perceivers were very likely to overestimate the similarity between their own and other people's perspectives because their privileged information biased their ability to take into account perspective differences sufficiently. We further showed that perceivers' egocentricity bias did not diminish when perceivers were stimulated to attend to another person's perspective both prior (Experiment 1) and during (Experiment 2) perspective-taking.

Chapter 5

Lifting the Curse of Knowing:
How Feedback Improves Perspective-Taking

This chapter is based on:

Damen, D., Van Amelsvoort, M., Van der Wijst, P., Pollmann, M., & Krahmer, E. (2019). Lifting the curse of knowing: How feedback improves perspective-taking. *Submitted for journal publication.* The preregistration and the anonymized data are accessible via osf.io/kpw6u

Abstract

People are likely to use their own knowledge as a frame of reference when they try to assess another person's perspective. Due to this egocentric anchoring, people often overestimate the extent to which others share their point of view. This study investigated which type of feedback (if any) stimulates perceivers to make estimations of another person's perspective that are less biased by egocentric knowledge. We allocated participants to one of three feedback conditions (no feedback, explicit feedback, implicit feedback). Findings showed that participants who were given the opportunity to learn through feedback not only adjusted their perspective-judgment more than those who did not receive feedback, they also showed less egocentric projection on future assessments. Participants adjusted their perspective within the same trial to the same degree for both feedback types. However, participants' egocentricity bias was only reduced when they received individuated information about another person (implicit feedback) and not when they received explicit feedback about their performance. Implications of these findings for theories of perspective-taking are discussed.

Keywords: perspective-taking; egocentricity bias, privileged information, egocentric projection, feedback.

Introduction

In our everyday life, we try to infer other people's inner motives, thoughts and feelings by observing their behavior. We can possibly interpret a helping hand as a genuine display of kindness, or a sad face of a child opening a present as a signal of disappointment. When we try to interpret other people's mental states, we engage in so-called *perspective-taking*. During this perspective-taking, we often rely on our own thought processes in order to understand those of others. This reliance on our own knowledge, however, might lead to an overreliance on the self-perspective, unintentionally stimulating perceivers to overestimate the similarity between their own and the other's point of view (e.g., Epley & Gilovich, 2001, 2006; Epley, Keysar, Van Boven, & Gilovich, 2004; Keysar, 1994). In this study, we examine what perceivers need in order to overcome this overreliance on the self-perspective and form perspective-taking judgments that reflect another person's perspective.

Perceivers' overestimation of the similarity between their own and other people's perspectives has been documented in many different contexts, including decision-making (Bazerman & Neale, 1982; Camerer, Loewenstein, & Weber, 1989; Garcia, 2002; Van Boven, Dunning, & Loewenstein, 2000), language production (e.g., Damen, Van der Wijst, Van Amelsvoort, & Krahmer, 2019, **Chapter 3**; Wardlow Lane, Groisman, & Ferreira, 2006; Wardlow Lane & Liersch, 2012) and language comprehension processes (e.g., Epley et al., 2004; Gerrig, Ohaeri, & Brennan, 2000; Keysar, 1994; Weingartner & Klin, 2005; Weingartner, Klin, & Weingartner, 2009). In the latter case, studies have shown that readers are likely to overestimate the extent to which their own knowledge about a story character's intention is accessible to a less informed protagonist. For example, in their "Sarcastic Messages" experiment, Epley et al. (2004) asked participants to read stories in which a speaker protagonist left an ambiguous voicemail message (e.g., "About that dancing class: I can't think of better ways to spend my Tuesday evenings") on the answering machine of an addressee protagonist. The voicemail was ambiguous in the sense that it could be interpreted as either sarcastic or sincere. Participants (the readers) learned how to solve the message's ambiguity by the clarifying event information they received before they actually listened to the voicemail. For example, they could learn that the speaker referred to an event that had resulted in a negative experience (e.g., "The dance class had been dull"), causing the speaker's message to be interpreted as sarcastic by readers themselves. The addressee protagonist – the person to whom the message was addressed –, however, did not receive this disambiguating information (they only received the voicemail) and, hence, had no reason to believe that the speaker intended his voicemail to be sarcastic. Epley et al. (2004) showed that readers indeed used the additional clarifying information to interpret the speaker's intention to be sarcastic.

Strikingly, Epley et al. (2004) further showed that, when asked to interpret the addressee's interpretation of the voicemail, readers assumed the speaker's communicative intention was also transparent to addressees. More specifically, when readers' privileged information suggested that the speaker was being sarcastic, readers were more likely to assume that addressees would also perceive the speaker's sarcasm. Once readers had resolved the message's ambiguity, they were unable to appreciate that the speaker's intention was less clear to the addressee protagonists than it was to readers themselves (see also the illusory transparency of intention in Keysar, 1994, 2000, and curse of knowledge effect in Camerer, Loewenstein, & Weber, 1989).

Epley et al. (2004) argued that readers' overestimation of the similarity between their own and the addressee protagonists' perspectives was the result of a perspective-taking process that followed two sequential phases: an egocentric anchoring phase and an adjustment phase (see also Barr, & Keysar, 2005; Epley, 2008; Epley & Gilovich, 2004; Jacowitz & Kahneman, 1995; Kahneman, 2011; Tversky & Kahneman, 1974). During the first phase, readers use their own interpretation of the speaker's voicemail as a frame of reference (egocentric anchor) when they try to determine the addressees' interpretation. Subsequently, readers adjust away from this egocentric anchor to account for possible informational differences between their own and the addressees' perception. These perspective-adjustments are thus predicted to be performed in sequential steps and to terminate when readers are satisfied with the resulting judgment (Epley et al., 2004). However, because readers know the speaker's actual intention, adjustments away from this knowledge are often insufficient (see also Epley & Gilovich, 2004, 2006; Jacowitz & Kahneman, 1995; Tversky & Kahneman, 1974). The resulting judgment, therefore, is likely to reflect too much of the readers' own interpretation of the speaker's message. In this sense, readers do not sufficiently take into account that addressees do not have access to readers' privileged information.

By comparing two groups of readers who were given different task instructions, Epley et al. (2004) found evidence for this egocentric anchoring and adjustment during perspective-taking. One group of readers was instructed only to indicate their own perception of the speaker's sarcasm, whereas the other group only judged the addressees' perception of the speaker's sarcasm. Findings showed that the perception of sarcasm was more moderate when readers had to take the addressees' perspective than when readers indicated their own interpretation of the voicemail. Based on these more moderate perceived sarcasm scores in the perspective-taking condition, Epley et al. (2004) concluded that readers did acknowledge that the messages were likely to be perceived as more ambiguous by the uninformed addressees than by themselves. Specifically, the authors argued that, even though readers used their own knowledge about the speaker's

intention as a frame of reference to assess how the addressee would perceive the message (egocentric anchoring), readers *did* slightly modify their egocentric interpretation into a more moderate judgment (adjustment). However, since readers still believed that addressees would perceive the speaker's sarcasm, these perspective-adjustments were not sufficient enough to reflect the addressees' true perspectives. In other words, readers' knowledge about the speaker's communicative intention "cursed" (Birch & Bloom, 2007; see also Camerer, Loewenstein & Weber, 1989) their ability to appreciate a less informed perspective. This raises the question what readers might need in order to adjust away from an egocentric interpretation and to form a judgment that more accurately reflects the perspective of a less informed protagonist. In this study, we set out to investigate this question. More specifically, we will examine the extent to which readers learn to make estimations of another person's perspective that are more accurate due to the feedback they receive about their perspective-taking performance.

Adjusting the Self-Perspective

Various studies have investigated what contributes to perceivers' egocentric reasoning, with few of them proposing, for example, that it might be caused by perceivers' inability to inhibit salient knowledge (e.g., Bayen, Pohl, Erdfelder, & Auer, 2007; Lagattuta, Sayfan, & Blattman, 2010), or their inability to disregard information that comes easily to mind (e.g., Harley, Carlsen, & Loftus, 2004; see Birch, Brosseau-Liard, Haddock, & Ghrear, 2017, for a review). Building on these mechanisms, some studies have proposed that various factors might decrease egocentric projection, such as perceivers' ability to inhibit their own perspective (e.g., Samuel, Roehr-Brackin, Jelbert, & Clayton, 2019), their ability to exert motivational resources (e.g., Epley & Gilovich, 2004, 2006; Epley et al., 2004; see also Simmons, LeBoeuf, & Nelson, 2010), perceivers' counterfactual reasoning (e.g., Pohl, 1998; see also Naylor, Lamberton, & Norton, 2011), or perceivers' awareness of their biased interpretations (e.g., Bazerman & Neale, 1982; Epley & Gilovich, 2006). Following this line of reasoning, Damen et al. (2020, **Chapter 4**; see also Damen, Van Amelsvoort et al., 2019; Damen, Van der Wijst et al., 2019; **Chapter 2** and **3**) investigated whether perceivers could be stimulated to rely less on egocentric knowledge during perspective-taking when they were stimulated to attend to information that was accessible to another person. They investigated this assumption in a direct replication and extension of Epley et al.'s (2004) "Sarcastic Messages" experiment. In particular, Damen et al. (2020, **Chapter 4**) tested whether readers' egocentric anchoring and adjustment during perspective-taking would benefit from explicit instructions to focus their attention on the story characters' (different) perspectives. Damen et al. (2020, **Chapter 4**) tried to accomplish this by asking readers to

indicate which information about the speaker's experience was known to addressees before they estimated the addressees' interpretation of the voicemails. Importantly, when readers (incorrectly) answered that addressees knew about the speaker's experience, readers were informed that their answer was wrong and that addressees did not have access to this clarifying event information[1]. In addition to replicating the general findings of Epley et al. (2004), Damen et al.'s (2020, **Chapter 4**) findings showed that the explicit instructions to focus on addressees' knowledge and attentional status did not stimulate readers to consider this information when assessing the addressees' interpretation of the message. Readers still overestimated the extent to which addressees shared readers' privileged information about the speaker's communicative intention, regardless of an explicit focus on addressees' perspective. In this sense, explicit instructions to focus their attention on another person's perspective did not help perceivers to inhibit their privileged perspective in order to increase their perspective-taking accuracy. These findings by Damen et al. (2020, **Chapter 4**) are in line with related studies (Damen, Van Amelsvoort et al., 2019; Damen, Van der Wijst et al., 2019; **Chapter 2** and **3**) that showed that an explicit focus on or an explicit awareness of another person's different perspective does not suffice to reduce perceivers' tendency to overrely on their own knowledge and attentional status during perspective-taking. The question, therefore, remains if and how perceivers can be stimulated to adjust the self-perspective into an accurate presentation of the other person's perspective. In Damen et al. (2020, **Chapter 4**), readers never learned that they misattributed their perception of the speaker's sarcasm onto addressees. However, information about the inaccuracy of their judgments might actually help readers to improve their perspective-taking.

The Role of Feedback in Debiasing Social Judgments

It is reasonable to assume that providing perceivers with feedback might help them to engage in more accurate perspective-taking the second time around. After all, feedback might help perceivers to update their mental representation and to engage in more elaborative thinking to reduce possible biases (e.g., Creyer & Ross, 1993; Petty, Cacioppo, & Goldman, 1981). A large body of research has shown, for instance, that feedback helps interlocutors to produce and understand language on the basis of their interlocutor's perspective (e.g., Horton & Gerrig, 2002; Krauss & Fussell, 1991; Krauss, Garlock, Bricker, & McMahon, 1977; Krauss & Weinheimer, 1966; Kraut, Lewis, & Swezey, 1982; Matthews, Lieven, & Tomasello, 2007; Wardlow & Heyman, 2016). Here, we raise the question whether feedback also stimulates individuals to adopt a more successful strategy to inferring other

[1] Details of the perspective-focus instructions are accessible via osf.io/kv5mu.

people's perspectives. In other words, we ask whether feedback helps perceivers to debiase their social judgments in subsequent perspective-taking.

The literature is largely equivocal concerning the effectiveness of feedback in reducing cognitive biases. To identify their commonalities and differences, it is important to regard the content of the feedback that is provided to perceivers. In the psychological literature, three main classes of feedback are identified: outcome feedback, task properties feedback, and cognitive or process feedback (e.g., Balzer et al., 1989, 1992, 1994; Kluger & DeNisi, 1996). Outcome or process feedback communicates the correctness of the judgments to individuals immediately after they have made a mistake (e.g., Balzer et al., 1989). This type of feedback is proven most effective in predictable, simple tasks in which judgers are able to trace back what resulted in them making this mistake (e.g., Hirst, Luckett, & Trotman, 1999). Task properties feedback indicates to individuals the best course of action and tells them how the task should be performed. This type of feedback is often combined with cognitive or process feedback, in which individuals receive an important insight into their judgment strategy. The intent of cognitive or process feedback is to highlight how perceivers' judgment is biased by their own preconceptions or false-beliefs (e.g., Harmon & Rohrbaugh, 1990; Thompson & DeHarpport, 1994; see also Leung & Trotman, 2005). It is, therefore, logical that this type of feedback is often applied when trying to debiase social judgment.

Pohl and Hell (1996), for instance, studied whether performance feedback could diminish individuals' tendency to overestimate their ability to reconstruct their knowledge in hindsight (see *hindsight bias* in Fischhoff, 1975; Slovic & Fischhoff, 1977; Hawkins & Hastie, 1990; Kahneman, 2011; Christensen-Szalanski & Willham, 1991). Poll and Hell (1996) provided participants with detailed feedback about their ability to recall previous answers to difficult questions, and the extent to which their recall estimations were biased by questions' solutions they received beforehand. In contrast to their expectations, however, this explicit feedback about the extent to which their judgments were inaccurate, and how they could improve it, did not help participants to reduce the number of biased recollections. Regardless of the feedback, participants still estimated an answer that was closer to the solution than to their original belief.

The ineffectiveness of performance feedback in helping to debiase social judgment has also been demonstrated by Camerer, Loewenstein and Weber (1989), who revealed that providing perceivers with explicit feedback about their perspective-taking performance did not diminish their tendency to project their knowledge of a company's true value onto others who did not have access to this information. Interestingly, however, the authors did show that individuals were less likely to attribute their knowledge of a company's true value when they received this feedback in a more implicit manner. That is, participants'

estimations of others' beliefs were more accurate when they learned to exchange a company's assets for dividend that was based on what others believed the company was worth. Apparently, the amount of dividend earned disclosed more information about other people's beliefs than the actual performance feedback did. This experience allowed participants to make predictions of others' perspectives that were more accurate and less biased by privileged information.

Comparable findings have been described by Thompson and De Harpport (1994), who aimed to improve negotiators' performance during integrative negotiations. The authors built on the assumption that most negotiators enter a negotiation with a biased perception of their counterpart's incompatible interests (e.g., Thompson & Hastie, 1990). This is compounded by that fact that negotiators do not exchange information to update these false-beliefs in order to improve their interpersonal understanding (e.g., Thompson, 1991), and their chances of reaching an integrative agreement (e.g., Thompson & Hastie, 1990; see also Thompson & Loewenstein, 1992). Thompson and DeHarport (1994) found that receiving detailed information about both compatible and incompatible interests of one's opponent helped negotiators to understand their opponent's interests to a better degree, and to form an agreement that was more beneficial for all parties involved. This in comparison to negotiators who were explicitly informed about their negotiation performance and the negotiators who did not receive any feedback at all. Negotiators who received individuated information about their opponent and, thereby, implicit feedback about their misconceptions, were less likely to hold on to their biased judgment of their opponent's incompatible interests.

Related findings were presented by a more recent study focused on improving perceivers' interpersonal understanding by stimulating them to *get* this individuated information from their interlocutor. In their final experiment, Eyal, Steffel and Epley (2018) instructed one group of romantic couples to converse about their attitudes and interests, before they estimated their partner's preferences in a subsequent task. Eyal et al. (2018) showed that couples who directly experienced their partner's interests (through conversation) made better predictions about their partner's preferences than the couples who were not able to converse in advance. More specifically, couples who engaged in conversation were less likely to project their own preferences onto their partner than the couples who had not had the opportunity to require more insight into their partner's preferences. The more information individuals received, the less likely they were to use privileged information as a frame of reference while understanding their partner's perspective. In a similar vein, West (1996) also showed that this accumulated experience of another person's preferences helps to reduce perceivers' tendency to project their own preferences on this person. In West (1996), participants learned to predict a target's

preference for quilt patterns through the feedback they received from the target. The more perceivers learned about the target's preferences, the less likely they were to project their egocentric preferences onto the target on subsequent perspective-taking trials.

Combined, these findings raise the question whether feedback might stimulate interpersonal accuracy in a communicative context that is by definition more ambiguous. Findings of a study by Weingartner and Klin (2005) do suggest that presenting perceivers with accurate information about another person's perspective allows them to recognize that the other person's perspective is different from one's self-perspective. Weingartner and Klin (2005) tracked participants' reading time while they read a description of a story character's perspective that was different from participants' egocentric perspective. To illustrate this, imagine that participants read a story in which a protagonist Tom (the speaker) attended a dance lesson that had been tedious and boring. Participants subsequently read that Tom leaves a note for his friend Eileen (the addressee) in which he refers to his dance lesson experience (i.e., "I cannot think of better ways to spend my Tuesday evenings"). Since participants previously learned that Tom had not enjoyed the dance lesson, they are expected to interpret Tom's message to be sarcastic. However, because Eileen does not know whether Tom had liked the dance lesson, she has no reason to share participants' sarcastic uptake of Tom's message. After reading Tom's message, Weingartner and Klin (2005) subsequently presented participants with Eileen's sincere interpretation of Tom's note (e.g., the target line: "Eileen could not wait to join Tom in the dance lesson"). Weingartner and Klin (2005) showed that the reading times on this target line were longer when participants' privileged information suggested that Tom was being sarcastic than when participants' privileged information suggested Tom was being sincere. The authors argued that this slowdown in reading demonstrated that participants realized that Eileen's knowledge was different from their own perspective. Unfortunately, Weingartner and Klin (2005) did not examine whether this online processing of another person's (different) perspective stimulated participants to *adjust* their judgment of Eileen's perspective after receiving insight about her actual uptake of the message. Therefore, it remains unexplored whether feedback actually helps perceivers to acknowledge this information in their perspective-judgment, and whether this feedback decreases perceivers' egocentric projection on future perspective-taking attempts.

Explicit and Implicit Feedback

In this study, we explore the extent to which performance feedback helps perceivers to adjust their judgment in next perspective-taking attempts. We further explore the manner in which this performance feedback is presented to perceivers. As may have

become apparent in our previous discussion of the literature, performance feedback can differ in terms of its explicitness. That is, perceivers can be explicitly informed about the (in)accuracy of their judgment and how they can improve it (as in Camerer et al., 1989), or they can receive this information in a more implicit manner (as in Thompson & DeHarport, 1994, and Eyal et al., 2018). To point out these differences more clearly, we build on the distinction that is made in language learning research. In this research domain, feedback that signals to learners that they have made an error and how this error can be addressed is termed as "corrective feedback" (e.g., Ellis, Loewen, & Erlam, 2006). This corrective feedback can take either an explicit form ("No, she did not goed – she went") or an implicit form (e.g., "She went"; Ellis et al., 2006; see also indirect versus direct feedback in Van Beuningen, De Jong, & Kuiken, 2008). When learners receive the feedback explicitly, the error is explicitly identified and learners are provided with explicit information about how to correct their mistake. In this way, learners are stimulated to make direct comparisons, stimulating the acquisition of explicit and implicit knowledge (Ellis et al., 2006). By contrast, implicit feedback often takes the form of recasting the utterance of the learner in a correct format, without explicitly signaling the error. In this case, the nature of the error often remains unclear and learners have to infer by themselves that an error has been made. Even though receiving implicit feedback might stimulate a more reflective learning process because learners need to exert more effort in self-editing their errors (e.g., Ferris, 1995), the danger of receiving this type of feedback lies in the fact that learners might not be aware of its corrective intent (e.g., Nicholas, Lightbown, & Spada, 2001), or that learners still do not know how to correct their error (e.g., Chandler, 2003; Van Beuningen et al., 2008). Recent studies indeed evidenced the advantage of explicit feedback over implicit feedback, with regard to both short-term (Ellis et al., 2006) and long-term learning effects (Van Beuningen et al., 2008; Chandler, 2003). In addition, studies have even shown that learners are more likely to perceive the feedback to be corrective when it is explicit rather than implicit (Ellis et al., 2006). In sum, it can thus be assumed that, when given explicit feedback, learners are more aware that they need to improve their learning.

Building on this line of reasoning, the explicitness of performance feedback might affect whether and how well perceivers learn to take a (less informed) perspective. Perceivers are expected to attend the most to information that is unexpected, because it violates earlier held beliefs and is, thus, most informative (West, 1996). In turn, perceivers exert more cognitive effort to process this informative information, because they try to reconcile it with prior held beliefs (e.g., Jerónimo, Volpert, & Bartholow, 2017; Macrae, Bodenhausen, Schloerscheidt, & Milne, 1999; see also Bartholow, Fabiani, Gratton, & Bettencourt, 2001). In this way, we might expect that explicit feedback will receive more attention than implicit feedback, also because the corrective intent and the applicability

of the latter feedback type needs to be inferred by perceivers themselves. Hence, implicit feedback might be processed more slowly and less elaborately than explicit feedback, affecting perceivers' ability to adopt a less informed perspective. To our knowledge, the influence of explicit and implicit feedback on perceivers' perspective-taking has not yet been examined. In this study, we investigate whether and how these different feedback types affect perceivers' egocentric projection during perspective-taking.

The Current Study

This study examines the role of performance feedback as a mean to gain accurate insight into another person's perspective. To this end, we replicate and extend Damen et al. (2020, **Chapter 4**), investigating how well readers are able take a perspective of an addressee protagonist who is less informed. In two experiments, we investigate whether confronting readers with feedback helps them to better re-assess addressees' perspective, decreasing their tendency to attribute their perception of a speaker's sarcasm onto these addressees. In the first experiment, we aim to explore whether readers adjust their perspective differently depending on how they gain this perspective-insight, by contrasting two approaches. For the first approach, we rely on readers' bottom-up inferencing, through which readers gain interpersonal insight by reading addressees' reactions to the speaker's message and, thus, receiving individuated feedback about the addressees. Since this strategy implicitly communicates to readers whether their first prediction of addressees' perspective was correct, we will refer to this approach as implicit feedback. We contrast this approach to a strategy through which readers receive explicit feedback about their performance. In this approach, readers are explicitly informed about the extent to which their prediction was inaccurate. We will refer to this as explicit feedback. We will use this term to refer to the situation in which readers are made explicitly aware that they have made an error, and how they can improve their predictive accuracy. We assess perceivers' perspective-adjustments by asking them to judge addressees' perception before and after feedback. In the second experiment, we aim to replicate our findings of Experiment 1, while controlling for this second time measurement. In particular, we examine whether the perspective-adjustments in the feedback conditions reported in Experiment 1 were actually due to the feedback participants received and not due to participants judging addressees' perception twice for each experimental item. The findings of both experiments provide insight into the role feedback in reducing perceivers' egocentric projection strategy during perspective-taking.

EXPERIMENT 1

This study replicates and extends Damen et al.'s (2020, **Chapter 4**) study in which readers judged addressees' interpretation of voicemails sent by a speaker protagonist. We extend the experimental design by adding a feedback manipulation and a subsequent second measurement of readers' judgment of addressees' interpretation of the voicemail. In line with previous findings (Epley et al., 2004; Damen et al., 2020, **Chapter 4**), we expect that readers will initially overestimate the extent to which their privileged information about the speaker's sarcastic intention is accessible to uninformed addressees. This hypothesis is supported when we find that readers are more likely to attribute their perception of a speaker's sarcasm onto addressees before (Time 1) than after (Time 2) feedback. We further expect that feedback allows readers to track successfully which information is accessible to the uninformed addressee. In particular, compared to the baseline condition in which readers do not receive feedback, we expect that both explicit and implicit feedback will lead to predictions that are more accurate on Time 2 than on Time 1. Finally, we expect that this adjustment effect is stronger when readers receive explicit rather than implicit feedback about their performance. In addition to testing these hypotheses, we anticipate possible individual differences in readers' propensity to engage in perspective-taking (see Damen, Van Amelsvoort, Van der Wijst, & Krahmer, 2019, **Chapter 2**) by exploring the extent to which participants' self-reported propensity to engage in perspective-taking predicts their perspective-taking accuracy. The preregistration of our hypotheses and analyses can be accessed via the Open Science Framework (Damen, Van Amelsvoort, Van der Wijst, Pollmann, & Krahmer, 2018, November 20).

Method

Participants and Sample Size

In this study, we extended the experimental design of Damen et al. (2020, **Chapter 4**) who replicated and extended Epley et al. (2004). Damen et al. (2020) found medium to large effect sizes. For a medium effect size, the G*Power calculation (version 3.1.9.2) indicated that we would require a sample size of 22 participants per experimental condition to obtain an alpha err probability of .05 and a power of .95. In our preregistration, we described our intention to recruit at least 30 participants per experimental condition. Since Damen et al. (2020, **Chapter 4**) recruited around 50 participants per experimental condition, we chose to approximate this number. After a period of three months, we reached a total sample size of 149 undergraduates. Seven participants were excluded either because

they recognized the voice-actor (*N* = 5) or because they were non-native speakers of the language of the experiment (Dutch, *N* = 2). The remaining 142 participants (105 women, 37 men, M_{age} = 21.57, age-range 18-38) were randomly allocated to the control (*N* = 48), explicit feedback (*N* = 47), or implicit feedback (*N* = 47) conditions.

Design

In each condition, participants read 12 scenarios in which a speaker protagonist (Tom) left a voicemail-message on the answering machine of an addressee protagonist. After hearing this voicemail, participants were asked to judge to what extent they thought the addressee would perceive the speaker (Tom) as being sarcastic (1 = definitely as sincere, 7 = definitely as sarcastic), both before (Time 1) and after (Time 2) they received feedback about their first perspective-judgment. This resulted in a 3 (*Condition*: control, explicit feedback, implicit feedback) x 2 (*Time*: Time 1, Time 2) mixed design in which *Condition* was treated as a between-subjects factor and *Time* as a within-subjects factor.

Procedure and Materials

The Dutch materials of this experiment are accessible via osf.io/kpw6u. We replicated and extended the experimental materials and procedure of Damen et al.'s (2020, **Chapter 4**) "interpretation" condition. Participants were invited to the lab and were asked to take place in soundproof cubicles. All participants gave their consent before participating in the experiment. Participants read 12 stories (and a practice item) that were presented in Qualtrics^{XM}. This software was also used to collect participants' responses. In this study, we re-used the stories of Damen et al. (2020, **Chapter 4**). Damen et al. (in press) obtained the original scenarios used in Epley et al. (2004) from Keysar (p.c.) and translated them into Dutch. All stories described an event in the life of Tom. For instance, in the story "The Dance Class", participants read the following story (English translation of Dutch original):

> Tom was on his way to the first night of his ballroom dancing class when he saw Eileen, an old friend from his dorm last year. When he told her that he was on his way to a ballroom dancing class, she excitedly replied, "I'm thinking of taking that class, but I can't make it to tonight's class--I am having dinner with friends. Could you call me when you get back and tell me how it is?"

Subsequently, participants learned that Tom's experience had either been negative (e.g., "(…) the instructor spent the entire time taking attendance and filling out lengthy

forms and questionnaires.") or positive (e.g., "(…) the instructor spent the entire time teaching the class fun, new dances."). Both experiences followed with Tom leaving a voicemail on the answering machine of his friend. In "The Dance Class" story, Tom left the following voicemail message:

> Eileen, this is Tom. Hope you enjoyed your dinner. About that ballroom dancing class: Judging from tonight's class, I can't think of better ways to spend my Tuesday evenings. Anyways, give me back a call and I'll fill you in on the details. Bye.

We re-used the 12 voicemails from Damen et al. (2020, **Chapter 4**) who demonstrated the validity of the voicemails. That is, in a separate rating experiment, Damen et al. (2020, **Chapter 4**) asked listeners to rate the voicemails (narrated by the fifth author) in the absence of clarifying (positive, negative) event information (1 = as very sincere, 7 = as very sarcastic). This rating experiment showed that the voicemails sounded truly ambiguous to the uninformed listeners as participants rated the voicemails to sound neither as very sarcastic nor as very sincere (M = 3.73, SD = 0.83).

Immediately after listening to Tom's voicemail, we asked participants to indicate how the addressee protagonist (Tom's friend) would perceive the voicemail message (1 = definitely as sincere, 7 = definitely as sarcastic). This constituted the first measurement of participants' judgment of the addressee's perception of sarcasm (Time 1). All stories were presented to participants on a computer screen. Stories were grouped together in booklets, with half the stories describing a positive event, whereas the other half described a negative event. We created four versions of these booklets: The first booklet contained a random order of negative versus positive events (booklet 1), and another one contained its mirror image (booklet 2). Additionally, for each booklet, we created a version that contained a reversed order of the events. We were interested in the extent to which readers accurately learned to engage in perspective-taking. Therefore, in contrast to Damen et al. (2020, **Chapter 4**), we only focused on those instances in which readers' egocentric perspective *diverged* from the addressee protagonist. This was only the case for the stories in which readers' privileged information suggested that Tom was being sarcastic (negative events). In these instances, readers could engage in egocentric projection by wrongly assuming that their perception of Tom's sarcasm was shared by the uninformed addressee protagonist. Since readers' interpretation of Tom's message corresponded to the addressee's perspective when readers' privileged information suggested that Tom was being sincere (positive events), we treated these trials as fillers.

Feedback Manipulation

We manipulated which feedback (explicit, implicit, none) participants received after their first judgment about the addressee's perception of sarcasm. Participants received the feedback in Qualtrics^{XM}. In the explicit feedback condition, participants received (performance) feedback about the accuracy of their judgments. This feedback was tailor-made in the sense that participants were informed about the extent to which their judgments were inaccurate. For all stories, the addressee protagonist had no reason to interpret the speaker's message as sarcastic. This means that only the readers who predicted that addressees would interpret the messages as definitely sincere (1) made accurate perspective-taking attempts. Therefore, the explicit feedback ranged from "You are completely right!" (1) to "You are completely wrong!" (7), depending on participants' answer on the 7-point scale (1 = definitely as sincere, 7 = definitely as sarcastic). For example, those who selected "2" on the 7-point scale almost made an accurate prediction of the addressee's perspective and were, therefore, informed that their answer was almost accurate. An example of the explicit feedback for "The Dance Class" story is presented in Figure 1. By tailoring the explicit feedback to participants' predictions, we aimed to reduce participants' insecurity about the ambiguossness of the voicemails, consequently stimulating participants to make accurate adjustments away from their own sarcastic interpretation of the voicemails.

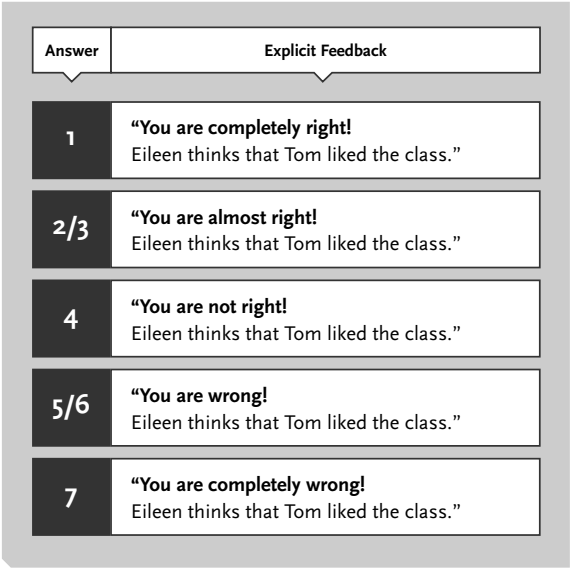

Figure 1. Example of the explicit feedback participants received for the scenario "The Dance Class". The type of feedback depended on participants' choice on the 7-point scale (1 = definitely as sincere, 7 = definitely as sarcastic).

Participants in the implicit feedback condition received feedback about the inaccuracy of their perspective-judgment regardless of their choice on the 7-point scale. This feedback constituted a follow-up text that described the addressee's sincere interpretation of Tom's voicemail. For instance in "The Dance Class" story, participants could derive from Eileen's behavioral response to Tom's voicemail that she thought that Tom had enjoyed attending the class:

> After saying goodbye to her friends, Eileen cycled home. She decided she was going to search for her dancing shoes the minute she would arrive at home. She could hardly wait to join Tom in the dance class. If Tom liked the dance class, she definitely would like it too.

In contrast to the two feedback conditions, participants in the control condition did not receive feedback about their first assessment of the addressee's perception of sarcasm. Subsequently to their first judgment, these participants read a follow-up text that described the addressee's thoughts and actions that did not target her interpretation of the voicemail:

> After saying goodbye to her friends, Eileen cycled home. She and her friends enjoyed dinner. They had known each other since high school and had built up a close friendship. Although they only saw each other a few times a year, it was always as if they never had been apart.

To examine the uptake of the feedback, participants in all three conditions subsequently re-judged the addressee's interpretation of the voicemail (1 = definitely as sincere, 7 = definitely as sarcastic). After this second assessment, we measured the extent to which participants were seriously engaged in the task by asking participants to answer a comprehension question about the story they just read (e.g., "What was the story about?") with three answer options (e.g., "A. Break dance lesson, B. Street dance lesson, C. Ballroom dance lesson). We made sure that all 12 comprehension questions did not target participants' privileged information and that the level of difficulty varied across all questions. Participants who answered the comprehension question incorrectly were informed to read the stories and to listen to the voicemails more carefully. Participants answered on average 11 out of 12 questions correctly (M = 10.52, SD = 1.07). We examined the extent to which participants' accuracy scores on the comprehension questions differed between the experimental conditions. Since the accuracy scores were non-normally distributed in all three conditions[2],

[2] Explicit feedback ($Z_{skewness}$ = .02; $Z_{kurtosis}$ = -1.35), implicit feedback ($Z_{skewness}$ = -2.07; $Z_{kurtosis}$ = 0.77), and control condition ($Z_{skewness}$ = -2.20, $Z_{kurtosis}$ = 0.70).

we performed a non-parametric Kruskal-Wallis test to investigate this relationship. This test showed that the number of correct responses differed between conditions, $H(3) = 9.73$, $p < .01$. Pairwise comparisons with adjusted p-values indicated that participants answered more comprehension questions correctly in the implicit feedback condition ($M = 10.81$, $SD = 0.95$) than in the explicit feedback condition ($M = 10.13$, $SD = 1.15$), ($p < .01$). The accuracy scores did not differ between the control ($M = 10.63$, $SD = 1.00$) and the two feedback conditions ($p > .05$). Arguably, this difference is not so surprising, given the nature of the different feedback types. It can be argued that readers in the implicit feedback condition spent more time processing the information, because they had to infer the mismatch in perspectives from a description of addressees' true perspective (see also Weingartner & Klin, 2005). This in contrast to the readers who were not (control condition) or were explicitly informed (explicit feedback condition) about the inaccuracy of their judgment and, thereby, could have spent less time on the information that was presented in the stories.

Participants' Perspective-Taking Tendency

As a final step in the experimental procedure, we asked participants to fill out a questionnaire that measured the extent to which participants themselves thought they had acknowledged the addressees' perspective. The questionnaire contained 8 items (e.g., "I was aware that Tom's friends could interpret the voicemail messages differently than me") that were alternated by 7 fillers questions (e.g., "I liked to read the stories") and measured on a 7-point scale (1 = totally disagree, 7 = totally agree). The individual items are presented in Table 1. The scale had a moderate reliability (Cronbach's $\alpha = .64$). Factor analysis showed that the scale could not be divided into meaningful subsets or into

Table 1. Items of Participants' Perspective-Taking Tendency Scale

While reading the stories, listening to the voicemails and while answering the questions that followed the voicemails:	
1.	I especially took into account what I knew about Tom's experience (R)
2.	I found it difficult to imagine how Tom's friends would interpret the voicemails (R)
3.	I especially took into account what Tom's friends knew about Tom's experience
4.	I could easily imagine how Tom's friends would interpret the voicemails
5.	I was especially aware of what Tom's friends knew about Tom's experience
6.	I tried to imagine how Tom's friends would understand the voicemails as much as possible
7.	I was especially aware of what I knew about Tom's experience (R)
8.	I was aware that Tom's friends could interpret the voicemail messages differently from me

Note: Items (R) were recoded before the analysis.

a subset that improved the reliability of the scale. Hence, we treated it as a single scale and report below on the average score of the 8 items. After filling out the questionnaire, we collected participants' demographics, debriefed participants about the purpose of the experiment and thanked them for their participation.

Results

Feedback and Perspective Adjustments

The anonymized dataset of this experiment is accessible via osf.io/kpw6u, and the preregistered analyses are accessible via osf.io/vbsyz (Damen, Van Amelsvoort, Van der Wijst, Pollmann, & Krahmer, 2018, November 20). Following our preregistration, we computed a mean perceived sarcasm score of participants' first (Time 1) and second (Time 2) judgment of addressee's perception of the speaker's sarcasm for the scenarios in which participants' privileged information suggested that the speaker was being sarcastic (negative events). In our preregistration, we reported that we would exclude outliers to improve deviations from normality. Exploratory analyses that included the difference score between the two mean ratings revealed three outliers in the control condition and one outlier in the explicit feedback condition (the deviance ranged from -4.78 to 2.99). When these outliers were excluded, normality improved and the data in the explicit feedback ($Z_{skewness}$ = -0.07, $Z_{kurtosis}$ = -0.89), implicit feedback ($Z_{skewness}$ = 0.94, $Z_{kurtosis}$ = -0.70), and control condition ($Z_{skewness}$ = 0.96, $Z_{kurtosis}$ = -1.02) were normally distributed. To adhere to our preregistration, we will report the findings of our analyses that did not include these three outliers. To examine whether the outliers affected our findings, we re-ran our analyses on the complete dataset. Below, we report whether the findings are different when the outliers were included in the dataset.

We submitted the two mean scores (Time 1, Time 2) to a mixed analysis of variance in which *Condition* (control, explicit feedback, implicit feedback) was treated as a between-subjects factor and participants' judgment of the addressee's perception of sarcasm (*Time*; Time 1, Time 2) as a within-subjects factor. The means of participants' judgment of addressee's perception of sarcasm as a function of *Time* and *Condition* are presented in Figure 2.

In line with our first hypothesis, participants thought addressees would perceive the speaker's sarcasm more at their first ($M_{Time\ 1}$ = 3.64, *SD* = 1.17) than at their second judgment ($M_{Time\ 2}$ = 2.40, *SD* = 1.27), $F(1, 134) = 249.48$, $p < .001$, $\eta_p^2 = .65$. Furthermore, participants' overall perspective-taking accuracy differed as a function of Condition, $F(2, 134) = 81.21$, $p < .001$, $\eta_p^2 = .95$. More specifically, compared to the control condition (*M* = 4.09, *SD* = 0.11), participants were less likely to judge that addressees would also perceive the speaker's

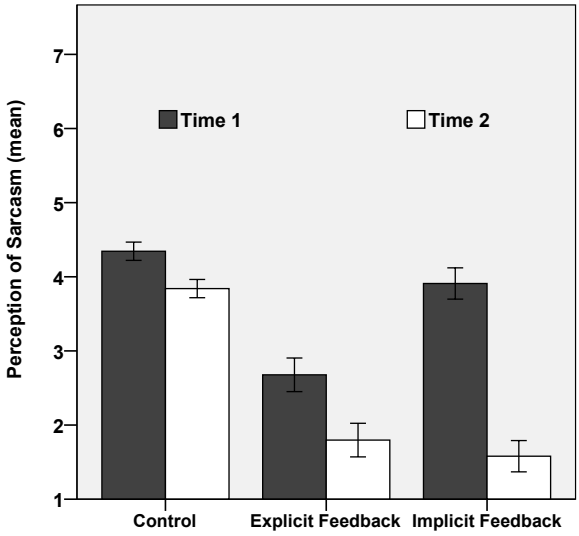

Figure 2. Mean scores of participants' judgments of addressees' perception of sarcasm (1 = definitely as sincere, 7 = definitely as sarcastic) as a function of *Time* (Time 1, Time 2) and *Condition* (control, explicit feedback, implicit feedback). Error bars represent 95% confidence intervals.

sarcasm in both the explicit (M = 2.24, SD = 0.11) and implicit feedback (M = 2.75, SD = 0.11) conditions (both $p < .001$). In addition, participants receiving explicit feedback made more accurate predictions about the addressee's perspective than participants receiving this feedback implicitly ($p < .01$).

In addition, we expected an interaction effect between the type of feedback and time point, in the sense that we predicted no difference between participants' first and second judgment in the control condition, but an improvement between participants' first and second judgment in the feedback conditions. Results indeed showed that the main effect of *Time* was qualified by a significant interaction with *Condition*, $F(2, 134) = 50.49$, $p < .001$, $\eta_p^2 = .43$. Bonferroni corrected pairwise comparisons that compared participants' perspective-taking accuracy of their second perspective-judgment showed that participants adjusted their first prediction into a more accurate second prediction after they had received both explicit (M = 1.80, SD = 0.85) and implicit (M = 1.58, SD = 0.68) feedback, compared to the control condition in which this feedback was absent (M = 3.84, SD = 0.74), $p < .001$. The accuracy of participants' second prediction did not differ between the two feedback conditions ($p = .515$).

Interestingly, results also showed that participants' perspective-taking accuracy of their first prediction differed as a function of *Condition*. Pairwise comparisons revealed that participants in the explicit feedback condition (M = 2.68, SD = 1.02) thought addressees

would perceive less sarcasm at Time 1 than the participants in the implicit feedback (M = 3.91, SD = 1.00) and control (M = 4.34, SD = 0.77) conditions (both $p < .001$). At Time 1, the perceived sarcasm scores did not differ between the implicit feedback and control condition ($p = .084$).

In addition to our preregistered analyses, we explored the degree to which participants adjusted their judgments from Time 1 to Time 2 as a function of *Condition*. This analysis showed that the effect of *Time* (Time 1, Time 2) was significant in the control, $F(1, 134) = 13.57$, $p < .001$, $\eta_p^2 = .09$, explicit feedback, $F(1, 134) = 42.38$, $p < .001$, $\eta_p^2 = .24$, and implicit feedback condition, $F(1, 134) = 296.70$, $p < .001$, $\eta_p^2 = .69$. Moreover, we computed a mean difference score between participants' first and second judgment of addressees' perception of the speaker's sarcasm and submitted this difference-score to a one-way analysis of variance. This follow-up analysis showed that participants' perspective-adjustments differed between conditions, $F(2, 134) = 50.49$, $p < .001$. Helmert contrasts revealed that participants adjusted their perspective more after receiving either explicit ($M_{difference} = 0.88$, $SE = 0.16$) or implicit feedback ($M_{difference} = 2.33$, $SE = 0.15$), compared to the control condition in which this feedback was absent ($M_{difference} = 0.50$, $SE = 0.09$), $t(134) = -6.60$, $p < .001$. Moreover, participants who received the feedback implicitly adjusted their perspective more than the participants who received the feedback explicitly, $t(134) = 7.58$, $p < .001$.

We explored the extent to which participants' *final judgment* (for the last story) reflected the addressee's actual perspective and whether this final score differed as a function of condition. Recall that participants predicted addressees' perception of the speaker's sarcasm on a 7-point scale, whereby a score of 1 (i.e., definitely as sincere) reflected the addressees' perspective. We computed a mean score of participants' final judgment made on Time 2 and submitted this score to a one-way analysis of variance. The accuracy of participants' final judgments differed as a function of *Condition*, *Brown-Forsythe* $F(2, 95.56) = 36.01$, $p < .001$. Planned contrast showed that participants' final judgments in the explicit (M = 1.46, SD = 1.21) and implicit (M = 1.38, SD = 0.49) conditions was more accurate than participants' final judgments in the control condition (M = 3.18, SD = 1.48), $t(60.54) = -7.30$, $p < .001$. In addition, the accuracy of participants' final judgments did not differ between the explicit and implicit condition, $t(59.29) = -0.38$, $p = .703$.

As a final step in our exploratory analyses, we plotted participants' first and second judgment of the addressees' perspective over time, ranging from the first story participants read till the last (Figure 3). Visually inspection of the plots indicates that participants' overall perspective-taking accuracy does not improve over time in the control condition. This learning effect, however, does seem to occur in the feedback conditions, especially in the condition in which participants received explicit feedback about their perspective-taking accuracy. Participants' first predictions of the addressees' perspective seem to have benefitted the

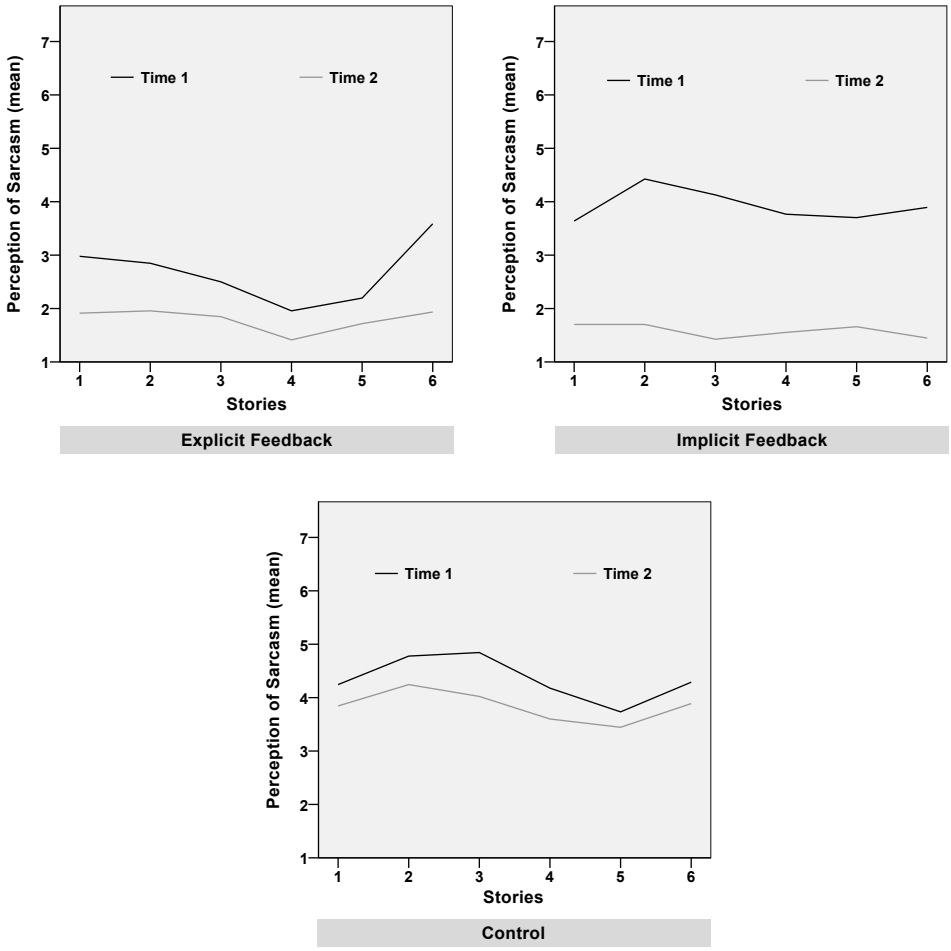

Figure 3. Participants' first (Time 1) and second judgment (Time 2) of addressees' perception of sarcasm (1 = definitely as sincere, 7 = definitely as sarcastic; mean score) plotted over time for the 6 stories communicating a negative event for the explicit feedback, implicit feedback and control conditions.

most from the explicit feedback, allowing participants to increase their accuracy over time. This in contrast to the implicit feedback condition, in which participants relied most on privileged information when *first* predicting addressees' perspective to the same degree over time, and only adjusted their predictions after they received addressees' uptake of the speaker's message. Interestingly, this uptake did not seem to influence participants' learning effect over time, as their accuracy on first predictions did not improve over time.

All findings remain unchanged when the outliers were included into the analyses and when we controlled for the order in which the scenarios appeared in the booklets (normal,

reversed). Considering the gender imbalance in our design, we also controlled for possible gender differences. Results showed that all findings remained unchanged and that the main effect of *Gender*, $F(1, 133) = 0.03$, $p = .860$, as well as the *Gender*Time* interaction, $F(1, 133) = 0.27$, $p = .604$, on perspective-taking accuracy were both non-significant.

In addition to our mixed analysis of variance, we preregistered that we would perform linear mixed effects analyses to control for random item (scenarios) and subject effects (e.g., Baayen, Davidson, & Bates, 2008; Baayen & Milin, 2010). For this analysis, we used the LMER function from the lme4 package in R (version 3.5.1, CRAN project; R Core Team, 2017). We construed four models to obtain the comparisons appertaining to our hypotheses and used a Bonferroni correction (i.e., $\alpha \leq .013$) to correct for multiple comparisons. We started the analyses by construing the models with a maximal random effects structure (Barr, Levy, Scheepers, & Tily, 2013). These maximal models included *Condition* (control, explicit feedback, implicit feedback), *Time* (Time 1, Time 2), and *Condition*Time* interaction as fixed factors, and random intercepts and slopes for both subjects and items. When the model did not converge, we excluded random slopes with the lowest variance until the model did converge (Barr et al., 2013). For all models, only the model containing random intercepts for both subjects and items reached convergence. For these intercept-only models, we estimated the confidence intervals and *p*-values by parametric bootstrapping over 100 iterations (Bates, Mächler, Bolker, & Walker, 2015). The estimated coefficients, standard errors and the structure of the models are presented in Table 2 to 5.

The linear mixed effect analyses replicated the results from our mixed analysis of variance. All three conditions showed a main effect of *Time*, indicating that readers' second predictions of the addressee protagonist's perspective were more accurate than their first predictions. In addition, the accuracy of these second predictions differed as a function of

Table 2. Estimated coefficients and standard errors for the mixed model (M1) fitted to readers' judgments of addressees' perception of sarcasm as a function of Condition and Time

	b	SE_b	*t*	*99% CI*
Intercept (Control, Time 1)	4.35	0.15	27.49	3.96, 4.72
Explicit feedback (Time 1)	**-1.68**	**0.15**	**-9.65**	**-2.07, -1.29**
Implicit feedback (Time 1)	-0.42	0.16	-2.44	-0.84, 0.01*
Time 2 (Control)	**-0.50**	**0.13**	**-3.88**	**-0.83, -0.14**
Explicit feedback * Time 2	-0.38	0.17	-2.06	-0.84, 0.04**
Implicit feedback * Time 2	**-1.84**	**0.18**	**-10.13**	**-2.33, -1.40**

Note: The control condition and Time 1 were treated as reference categories. Significant results are presented in bold. A Likelihood Ratio Test (LRT) showed that adding the order in which the scenarios appeared in the booklets did not improve the models' fit, $\chi^2(6) = 8.44$, $p = .207$.*This comparison reached significance at the 95% CI [-0.74, -0.09]; **This comparison reached significance at the 95% CI [-0.74, -0.07].

Table 3. Estimated coefficients and standard errors for the mixed model (M2) fitted to readers' judgments of addressees' perception of sarcasm as a function of Condition and Time

	b	SE_b	*t*	*99% CI*
Intercept (Explicit feedback, Time 1)	2.67	0.15	16.98	2.31, 3.06
Implicit feedback (Time 1)	**1.26**	**0.17**	**7.30**	**0.85, 1.71**
Control (Time 1)	**1.68**	**0.15**	**9.65**	**1.30, 2.09**
Time 2	**-0.88**	**0.11**	**-6.86**	**-1.18, -0.61**
Implicit feedback * Time 2	**-1.46**	**0.16**	**-8.10**	**-1.87, -1.02**
Control * Time 2	0.38	0.16	2.06	-0.04, 0.80*

Note: The explicit feedback condition and Time 1 were treated as reference categories. Significant results are presented in bold. A Likelihood Ratio Test (LRT) showed that adding the order in which the scenarios appeared in the booklets did not improve the models' fit, $\chi^2(6) = 8.44$, $p = .207$. *This comparison reached significance at the 95% CI [0.06, 0.70].

Table 4. Estimated coefficients and standard errors for the mixed model (M3) fitted to readers' judgments of addressees' perception of sarcasm as a function of Condition and Time

	b	SE_b	*t*	*99% CI*
Intercept (Implicit feedback, Time 2)	1.58	0.15	10.14	1.30, 1.89
Explicit feedback (Time 2)	0.21	0.18	1.20	-0.23, 0.67
Control (Time 2)	**2.26**	**0.18**	**13.07**	**1.83, 2.76**
Time 1	**2.34**	**0.13**	**18.45**	**2.02, 2.70**
Explicit feedback * Time 1	**-1.46**	**0.20**	**-8.10**	**-1.99, -0.98**
Control * Time 1	**-1.84**	**0.18**	**-10.13**	**-2.35, -1.42**

Note: The implicit feedback condition and Time 2 were treated as reference categories. Significant results are presented in bold. A Likelihood Ratio Test (LRT) showed that adding the order in which the scenarios appeared in the booklets did not improve the models' fit, $\chi^2(6) = 8.44$, $p = .207$.

Table 5. Estimated coefficients and standard errors for the mixed model (M4) fitted to readers' judgments of addressees' perception of sarcasm as a function of Condition and Time

	b	SE_b	*t*	*99% CI*
Intercept (Control, Time 2)	3.85	0.16	24.31	3.44, 4.26
Explicit feedback (Time 2)	**-2.06**	**0.15**	**-11.81**	**-2.44, -1.65**
Implicit feedback (Time 2)	**-2.26**	**0.17**	**-13.07**	**-2.68, -1.79**
Time 1	**0.50**	**0.12**	**3.88**	**0.20, 0.83**
Explicit feedback * Time 1	0.38	0.18	2.06	-0.10, 0.83*
Implicit feedback * Time 1	**1.84**	**0.18**	**10.13**	**1.35, 2.27**

Note: The control condition and Time 2 were treated as reference categories. Significant results are presented in bold. A Likelihood Ratio Test (LRT) showed that adding the order in which the scenarios appeared in the booklets did not improve the models' fit, $\chi^2(6) = 8.44$, $p = .207$. *This comparison reached significance at the 95% CI [0.02, 0.72].

Condition. Readers who received either explicit or implicit feedback about the accuracy of their first predictions were more accurate the second time around than readers who did not receive such feedback (control). Moreover, the accuracy of readers' second predictions did not significantly differ between the implicit and explicit feedback conditions. Furthermore, readers who received the feedback explicitly made more accurate first predictions as opposed to the readers in both the implicit feedback and control condition. The linear mixed effect analysis showed, however, a slightly different pattern when examining the influence of *Condition* on the degree to which readers adjusted their perspective. As in the mixed analysis of variance, the degree to which readers adjusted their judgments significantly differed between the control and implicit feedback condition, and between the implicit feedback and explicit feedback condition. The mixed analysis of variance showed, however, that the difference between the explicit feedback and control condition only tended to reach significance (i.e., this difference reached significance at the 95% CI).

Curse of Knowledge Effect and Feedback

We specified in our preregistration that we would explore the replicability of perceivers' curse of knowledge effect, as it has been documented in Damen et al. (2020, **Chapter 4**) and Epley et al. (2004)[3]. If participants' privileged information negatively impacts their ability to take a less informed perspective, then participants in the control condition – who did not receive feedback – should still attribute their perception of a speaker's sarcasm to addressees when their privileged information suggested that the speaker was being sarcastic (negative events) as opposed to being sincere (positive events). In addition to testing this preregistered assumption, we explored the extent to which feedback reduces this curse of knowledge effect on perspective-taking. As we have seen in the previous paragraph, both explicit and implicit performance feedback allowed participants to adjust their estimation on Time 1 to a more accurate prediction of the addressees' perspective on Time 2. Apart from improving perceivers' accuracy, we do not know, however, whether participants' egocentricity bias (their curse of knowledge) is diminished after feedback. If this is the case, then we do not expect to see any differences in participants' judgments of addressees' perception of sarcasm when participants' privileged information suggests the speaker intended his voicemail to sound sarcastic rather than sincere. In order to investigate this, we computed mean scores for participants' *second* (Time 2) judgment of addressees' perception of the speaker's sarcasm for the six stories that communicated a negative event

[3] In our preregistration, we specified that we would test participants' curse of knowledge effect on perspective-taking in our one-shot control condition (see Experiment 2). However, with regard to its integrity, we chose to analyze this effect also in Experiment 1.

and the six stories that communicated a positive event (*Event Information*). After excluding two outliers in the explicit feedback condition (deviance 1.98 and 2.15), and seven outliers in the implicit feedback condition (deviance ranged from -0.93 to 1.90), the difference score between the negative and positive events at Time 2 was normally distributed in the control ($Z_{skewness}$ = -0.08, $Z_{kurtosis}$ = -0.34), explicit feedback ($Z_{skewness}$ = 1.60, $Z_{kurtosis}$ = -0.22), and implicit feedback conditions ($Z_{skewness}$ = 0.63, $Z_{kurtosis}$ = 0.36). To examine whether the outliers affected our findings, we re-ran our analyses on the complete dataset and report whether the findings are different when the outliers are included into the analyses.

We submitted the two mean scores to a mixed analysis of variance in which *Condition* (control, explicit feedback, implicit feedback) was treated as a between-subjects factor and participants' judgments of addressees' perception of sarcasm at Time 2 (*Event Information*: positive, negative) as a within-subjects factor. The analysis revealed a significant main effect of *Event Information*, $F(1, 130) = 31.71$, $p < .001$, $\eta_p^2 = .20$. At Time 2, participants still thought addressees would perceive the speaker's sarcasm more when their privileged information suggested the speaker was being sarcastic (*negative event*; $M = 2.39$, $SD = 1.29$) than when their privileged information suggested the speaker was being sincere (*positive event*; $M = 2.02$, $SD = 1.06$). The analysis further revealed a significant main effect of *Condition*, $F(2, 130) = 144.89$, $p < .001$, $\eta_p^2 = .69$. Bonferroni corrected pairwise comparisons revealed that, at Time 2, participants attributed the perception of sarcasm onto addressees more in the control condition ($M = 3.43$, $SE = 0.09$), than in the explicit feedback ($M = 1.58$, $SE = 0.09$) and implicit feedback ($M = 1.43$, $SE = 0.10$) conditions (both $p < .001$). The mean difference between the explicit and implicit feedback conditions was not significant ($p = .852$). More importantly, the main effect of *Event Information* was qualified by a significant interaction with *Condition*, $F(1, 130) = 8.65$, $p < .001$, $\eta_p^2 = .12$ (see Figure 4). Simple effect analysis revealed that the effect of *Event Information* was significant in the control, $F(1, 130) = 37.31$, $p < .001$, $r = 0.47$, and explicit feedback condition, $F(1, 130) = 15.55$, $p < .001$, $r = 0.33$, but not in the implicit feedback condition, $F(1, 130) = 0.00$, $p = 1.00$). These findings suggest that – in both the control and explicit feedback condition – participants' privileged information still cursed their ability to estimate a less informed perspective at Time 2. Participants in these two conditions were still more likely to attribute their perception of sarcasm onto addressees when their privileged information suggested the speaker was being sarcastic (negative events; $M_{control} = 3.75$, $SD = 0.80$; $M_{explicit\ feedback} = 1.79$, $SD = 0.97$) than when their privileged information suggested the speaker was being sincere (positive events; $M_{control} = 3.11$, $SD = 0.85$; $M_{explicit\ feedback} = 1.37$, $SD = 0.61$). In contrast, participants receiving implicit feedback were not cursed by their privileged information when they estimated the addressees' perspective at Time 2. These participants thought addressees would perceive the speaker's voicemail to the same degree for both positive ($M = 1.43$,

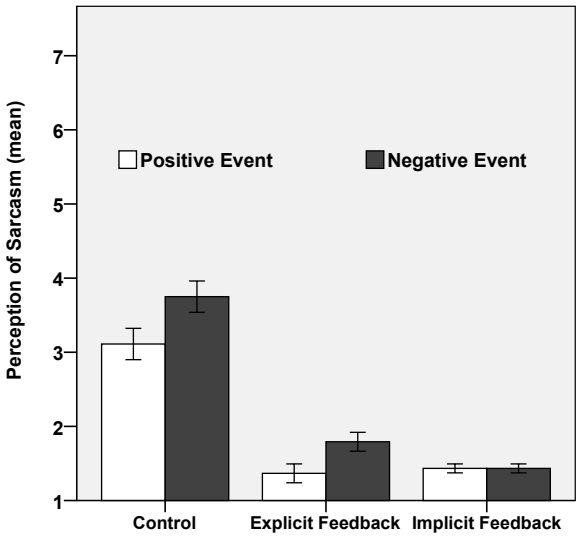

Figure 4. Mean scores of participants' estimations of addressees' perception of sarcasm (1 = definitely as sincere, 7 = definitely as sarcastic) at Time 2 as a function of *Event Information* (positive event, negative event) and *Condition* (control, explicit feedback and implicit feedback). Error bars represent 95% confidence intervals.

SD = 0.44) and negative events (*M* = 1.43, *SD* = 0.40). All findings remained unchanged when the outliers were included in the analyses, and when we controlled for the order in which the scenarios (items) were presented to participants.

Participants' Perspective-Taking Tendency

We explored the relationship between participants' self-reported perspective-taking tendency and their actual perspective-taking behavior. As part of this exploratory analysis, we first investigated whether participants' propensity to engage in perspective-taking (self-report) differed as a function of condition. Subsequently, we examined the extent to which this self-reported propensity predicted participants' perspective-taking accuracy during the experiment. Exploratory analyses revealed three additional outliers in the implicit feedback condition and one additional outlier in the control condition (the deviance ranged from 1.87 to 2.3). After excluding these outliers, normality improved and participants' self-reported perspective-taking tendency was normally distributed in the explicit feedback ($Z_{skewness}$ = -0.97, $Z_{kurtosis}$ = -1.21), implicit feedback ($Z_{skewness}$ = -0.33, $Z_{kurtosis}$ = -0.47), and control condition ($Z_{skewness}$ = 1.44, $Z_{kurtosis}$ = -0.70). A one-way analysis revealed that participants' perspective-taking tendency did not differ between conditions, *Welch's* $F(2, 85.69) = 0.33$, $p = .717$. Participants in the explicit feedback ($M = 4.18$, $SD = .87$), implicit

feedback (M = 4.30, SD = .67), and control (M = 4.22, SD = 0.61) condition reported to have regarded the addressee protagonists' perspectives to the same degree. Findings remained unchanged when the outliers were included into the analysis.

In a follow-up linear mixed effect analysis, we investigated whether participants' self-reported perspective-taking predicted the accuracy of their second perspective-judgments. We created a full model that included participants' self-report as a fixed effect and random intercepts for both subjects and items. We obtained the *p*-values using the Likelihood Ratio Test (LRT). In this test, we compared the full model with the intercept only model. This LRT test revealed that participants' self-report did not predict their perspective-taking accuracy, $\chi^2(1) = 0.87$, $p = .352$; $b = -0.01$, $SE_b = 0.02$, $t = -0.93$. Findings remained unchanged when the outliers were included into the analysis.

Intermediate Discussion

Experiment 1 showed that perceivers improved the accuracy of their social predictions when they received feedback about their perspective-taking performance. In line with our expectations, perceivers initially overestimated the extent to which their perception of a speaker's sarcastic intention was accessible to addressees who did not share perceivers' privileged knowledge. When perceivers re-judged addressees' perspective, however, this overestimation reduced due to the feedback perceivers received. Perceivers receiving feedback after their first prediction of addressees' uptake of the speaker's message made more accurate second predictions than the perceivers who did not receive this performance feedback. In contrast to our expectation, however, the extent to which perceivers adjusted their first predictions into more accurate second predictions did not depend on the type of feedback they received. Irrespective of the manner in which the feedback was disclosed to perceivers, perceivers' second judgments of the addressees' perspective were more accurate after feedback than when this feedback was absent. Although the adjustments perceivers made were more accurate due to the feedback, participants were not aware of their improvement. That is, participants' (self-reported) perspective-taking tendency did not differ between those who did or did not receive performance feedback, nor did participants' self-report predict their perspective-taking accuracy during the reading task.

The results of Experiment 1 also replicated perceivers' curse of knowledge effect (Damen et al., 2020, **Chapter 4**; see also Epley et al., 2004; Keysar, 1994; Weingartner & Klin, 2005, 2009). Perceivers were more likely to assume that an uninformed addressee would perceive a speaker's sarcasm when perceivers' privileged information suggested that the speaker was being sarcastic as opposed to being sincere. Interestingly, our findings

revealed that this egocentricity bias was only eliminated after implicit feedback, but not after explicit feedback or no feedback. More precisely, even though perceivers improved the accuracy of their predictions for both feedback types compared to the control, perceivers' adjusted predictions in the explicit feedback condition were still biased by perceivers' privileged information.

Although perceivers in the control condition did not receive feedback about the accuracy of their predictions, our findings indicated that they also adjusted their first prediction into a more accurate second prediction of the addressees' perspective. This 'positive' adjustment could have been the result of perceivers reflecting on their earlier assessment and subsequently coming to a more accurate conclusion (although these adjustments were still less accurate compared to the adjustments made by the perceivers who received feedback). We aim to rule out this alternative explanation in Experiment 2, in which we ask perceivers to judge the addressee's perspective only once for each story.

EXPERIMENT 2

The aim of Experiment 2 is twofold. First, we examine whether the perspective-adjustments in the feedback conditions reported in Experiment 1 were due to the experimental manipulation and not due to participants judging the addressee's perception twice for each experimental item. In order to investigate this, we allocated participants to two additional experimental conditions in which participants judged the addressee's perspective only once for each scenario. In particular, we allocated participants either to a one-shot control or to a one-shot implicit feedback condition. These additional experimental conditions were exact replications of the control and implicit feedback conditions reported in Experiment 1. However, participants in the one-shot control and the one-shot implicit feedback condition judged the addressee's interpretation of the voicemail only once for each scenario (item). We expect that the accurate perspective-taking responses on Time 2 in Experiment 1 were due to participants receiving accurate information about the addressee's perspective (feedback manipulation) and not due to participants benefiting from having to re-think about their first assessment. More specifically, we hypothesize that participants' judgments of the addressee's perception of sarcasm when participants' privileged information suggests that the speaker was being sarcastic (negative events) will not differ between the two-shot control (Experiment 1) and the new one-shot control condition, nor between the two-shot implicit feedback condition (Experiment 1) and the new one-shot implicit feedback condition.

Furthermore, we aim to replicate the feedback effect that was reported in Experiment 1. We expect that participants will be less likely to misattribute their perception of

the speaker's sarcasm onto addressees when participants receive information about addressees' (sincere) interpretation of the voicemail (one-shot implicit feedback) than when this feedback is absent (one-shot control).

Method

Participants and Sample Size

As in Experiment 1, we aimed to recruit 50 participants per experimental condition. After three months of data collection, 93 undergraduates were recruited. Three participants were excluded because they recognized the voice-actor (the fifth author). The remaining participants were randomly allocated to either the one-shot control (N = 44) or to the one-shot implicit feedback condition (N = 46). For the two-shot control (N = 48) and two-shot implicit feedback (N = 47) conditions, we used the data from Experiment 1. Our analyses are thus based on a total of 185 undergraduates (125 women, 60 men, M_{age} = 21.57, age-range 17-32). All participants gave their consent before participating in the experiment and received course credits for their participation.

Design, Materials and Procedure

We replicated the experimental design of Experiment 1. Hence, detailed information about the study's design, the materials and procedure can be found there. The only difference with the design of Experiment 1 is that we now measured participants' judgments of the addressee's perception of the speaker's sarcasm only once for each experimental item (1 = definitely as sincere, 7 = definitely as sarcastic). Participants in the one-shot control condition judged the addressee's perception immediately after listening to the speaker's voicemail. In contrast, participants in the one-shot implicit feedback condition read an additional text after listening to the voicemail. This text described the addressee's reaction to the speaker's message and were exact replications from the texts used in the two-shot implicit feedback condition (Experiment 1). Participants could infer from the addressees' behavioral responses that – for both positive and negative experiences – addressees thought that Tom had been sincere about his past experience. We contrasted participants' judgments of addressees' perception of sarcasm collected in the one-shot control and the one-shot implicit feedback condition against participants' sarcasm scores that were collected during the second time measurement (Time 2) in the two-shot control and two-shot implicit feedback conditions. Hence, our analyses will be based on a between-subjects design in which participants were randomly allocated to one

of the four conditions (*Condition*: one-shot control, one-shot implicit feedback, two-shot control, two-shot implicit feedback).

In both the one-shot conditions, each story ended with a comprehension question measuring participants' attentiveness while reading the stories and listening to the voicemails. Participants in the one-shot control and one-shot implicit feedback condition answered almost all questions correctly (M = 10.61, SD = 0.92) and the number of correct responses did not differ between experimental conditions, $H(1) = 0.21$, $p = .646$. Subsequently, participants filled out the perspective-taking tendency scale from Experiment 1 (Cronbach's $\alpha = .63$.), noted down their demographics, were debriefed about the purpose of the experiment, and thanked for their participation.

Results

Feedback and Perspective Adjustment

The anonymized dataset of this additional study is accessible via osf.io/kpw6u, and our preregistered analyses are accessible via osf.io/vbsyz (Damen et al., 2018, November 20). Following the statistical procedures of Experiment 1, we computed a mean sarcasm score of participants' judgments of addressees' perception of the speaker's sarcasm for the scenarios in which participants' privileged information suggested that the speaker was being sarcastic (negative events). For the two-shot control and two-shot implicit feedback conditions, we used participants' responses that were measured on Time 2. Exploratory analyses revealed one outlier (the deviance was 3.25) in the two-shot implicit feedback condition. After excluding this outlier, the data in the two-shot implicit feedback ($Z_{skewness}$ = 2.01, $Z_{kurtosis}$ = -0.32) and one-shot implicit feedback ($Z_{skewness}$ = 3.02, $Z_{kurtosis}$ = -0.01) conditions were still positively skewed. The data in the one-shot control ($Z_{skewness}$ = -1.45, $Z_{kurtosis}$ = 0.65) and two-shot control ($Z_{skewness}$ = -0.47, $Z_{kurtosis}$ = -1.29) conditions were normally distributed. Since the assumption of normality was violated for the implicit feedback conditions, we examined our hypotheses employing both parametric and non-parametric analyses.

For our parametric analysis, we submitted the average sarcasm score to a one-way analysis of variance in which we examined the influence of the between-subjects factor *Condition* on participants' judgment of addressees' perception of sarcasm. In addition, we performed three planned contrast that were in line with the hypotheses we wanted to investigate. The first contrast compared the difference in sarcasm scores between the one-shot control and two-shot control condition (H1), the second between the one-shot and two-shot implicit feedback condition (H2), and the third between the one-shot control and the one-shot implicit feedback condition (H3).

The analysis first revealed a significant effect of *Condition* on participants' judgment of the addressees' perception of sarcasm, *Welch's F*(3, 96.03) = 150.07, $p < .001$. Confirming our first two hypotheses, planned contrast revealed that the sarcasm scores did not differ between the one-shot control ($M = 4.09$, $SD = 1.05$) and the two-shot control condition ($M = 3.75$, $SD = 0.90$), $t(80.16) = -1.72$, $p = .089$, nor between the one-shot implicit feedback ($M = 1.59$, $SD = 0.68$) and the two-shot implicit feedback ($M = 1.51$, $SD = 0.48$) conditions, $t(80.77) = -0.62$, $p = .536$. Furthermore, the last planned contrast also confirmed our third hypothesis, and showed that the perception of sarcasm score was significantly higher in the one-shot control than in the one-shot implicit feedback condition, $t(73.11) = -13.36$, $p < .001$.

For the non-parametric analysis, we performed a Kruskal-Wallis Test. This test confirmed the findings from the one-way analysis of variance, $H(3) = 126.64$, $p < .001$. Pairwise comparisons with adjusted *p*-values showed that the sarcasm scores significantly differed between the one-shot implicit feedback condition and the one-shot control, $H(3) = 92.19$, $p < .001$, but not between the one-shot control and the two-shot control condition, $H(3) = 9.39$, $p = 1.00$, nor between the one-shot implicit feedback and the two-shot implicit feedback condition, $H(3) = 1.19$, $p = 1.00$.

Curse of Knowledge Effect and Feedback

We examined whether we could replicate participants' curse of knowledge effect from Experiment 1. We hypothesized that participants would project their perception of a speaker's sarcasm onto addressees more when their privileged information suggested the speaker was being sarcastic (negative events) than when their privileged information suggested the speaker was being sincere (positive events). We further expected that this effect would be qualified by our feedback manipulation. In particular, we expected that the difference in participants' attribution of a speaker's sarcasm for positive and negative events would remain in the two control conditions, but would disappear in the two implicit feedback conditions. To examine these assumptions, we computed a mean perceived sarcasm score for participants' estimation of the addressees' perception of the speaker's sarcasm for both negative and positive events. Exploratory analysis that included the difference score of negative and positive events revealed twelve outliers in the one-shot implicit feedback condition (deviance ranged from -0.76 to 1.41) and two outliers in the two-shot implicit conditions (deviance 1.23 and 1.90). In compliance to our preregistration and Experiment 1, we excluded outliers to improve the normal distribution of the data. After excluding the outliers, the data was normally distributed in the one-shot control condition ($Z_{skewness} = -0.91$, $Z_{kurtosis} = 0.46$), the two-shot control condition ($Z_{skewness} = -0.08$,

$Z_{kurtosis}$ = -0.34), the one-shot implicit condition ($Z_{skewness}$ = 0.03, $Z_{kurtosis}$ = 0.75), and in the two-shot implicit feedback condition ($Z_{skewness}$ = 1.16, $Z_{kurtosis}$ = 1.45). To inspect whether the exclusion of the outliers affected our findings, we re-ran our analyses on the complete dataset and report whether including the outliers impact our findings differently.

We submitted the two mean scores to a mixed analysis of variance in which Condition (one-shot control, two-shot control, one-shot implicit feedback, and two-shot implicit feedback) was treated as a between-subjects factor, and participants' judgment of addressees' perception of sarcasm (*Event Information*: positive, negative) as a within-subjects factor. Results revealed a main effect of *Event Information*, $F(1, 167) = 24.45$, $p < .001$, $\eta_p^2 = 0.13$, that was qualified by a significant interaction with *Condition*, $F(1, 167) = 7.44$, $p < .001$, $\eta_p^2 = 0.12$. Participants thought addressees would perceive the speaker's sarcasm more when their privileged information suggested the speaker was being sarcastic ($M = 2.78$, $SD = 1.45$) rather than being sincere ($M = 2.37$, $SD = 1.14$). Simple effect analysis revealed that this difference remained significant in both the one-shot control, $F(1, 167) = 31.18$, $p < .001$, $r = 0.40$, and the two-shot control condition, $F(1, 167) = 19.28$, $p < .001$, $r = 0.32$, but that this difference was not significant in both the one-shot implicit feedback, $F(1, 167) = 0.02$, $p = .887$, and the two-shot implicit feedback condition, $F(1, 167) = 0.03$, $p = .863$. All findings remained unchanged when the outliers were included into the analyses, and when we controlled for the order in which the items (scenarios) appeared to participants.

Participants' Perspective-Taking Tendency

As in Experiment 1, we explored the relationship between participants' self-reported perspective-taking tendency and their actual perspective-taking behavior. First, we examined whether participants' propensity to engage in perspective-taking (self-report) differed as a function of condition. In particular, we tested whether participants' perspective-taking tendency differed between the two new one-shot conditions (one-shot control, one-shot implicit feedback), and whether the two new one-shot conditions differed from the two-shot conditions (one-shot control vs. two-shot control, one-shot implicit feedback vs. two-shot implicit feedback). Following our preregistration, we excluded additional outliers to improve the normal distribution of the data. Participants' perspective-taking tendency was normally distributed after excluding one outlier in the one-shot control condition (deviance = 2.01), two outliers in the two-shot implicit feedback condition (deviance = 1.95, 2.32) and one outlier in the two-shot control condition (deviance = 1.86)[4]. A one-way

[4] Participants' self-reported perspective-taking tendency was normally distributed in the one-shot control ($Z_{skewness}$ = 0.72, $Z_{kurtosis}$ = 0.54), one-shot implicit feedback ($Z_{skewness}$ = 1.40, $Z_{kurtosis}$ = -0.10), the two-shot control ($Z_{skewness}$ = 1.46, $Z_{kurtosis}$ = -0.83), and the two-shot implicit feedback condition ($Z_{skewness}$ = -0.20, $Z_{kurtosis}$ = -0.59).

analysis revealed that participants' tendency to engage in perspective-taking significantly differed between conditions, $F(3, 176) = 8.38$, $p < .001$. Planned contrasts revealed that participants in the one-shot implicit feedback condition ($M = 4.90$, $SD = 0.87$) reported a higher perspective-taking tendency than participants in the one-shot control condition ($M = 4.32$, $SD = 0.74$), $t(176) = -3.72$, $p < .001$. The second planned contrast showed that there were no significant differences between the one-shot control and the two-shot control conditions ($M = 4.23$, $SD = 0.62$), $t(176) = -0.59$, $p = .555$. However, the last planned contrast showed that participants in the one-shot implicit feedback condition did report a higher perspective-taking tendency than the participants in the two-shot implicit feedback condition ($M = 4.28$, $SD = 0.67$), $t(176) = -3.99$, $p < .001$. All findings remained unchanged when the outliers were included into the analyses.

In a follow-up linear mixed effect analysis, we investigated whether participants' self-reported perspective-taking predicted the accuracy of their judgment. We created a full model that included participants' self-report as a fixed effect and random intercepts for both subjects and items. We obtained the *p*-values using the Likelihood Ratio Test (LRT). In this test, we compared the full model with the intercept only model. This LRT test revealed that participants' self-report predicted their perspective-taking accuracy, $\chi^2(1) = 15.08$, $p < .001$; $b = -0.05$, $SE_b = 0.01$, $t = -3.97$. Findings remained unchanged when the outliers were included into the analysis.

General Discussion

This study examined the extent to which performance feedback stimulates perceivers to make accurate perspective-taking predictions. In particular, we set out to investigate whether and how feedback affects perceivers' ability to take a perspective of a person who is less informed than they are. Additionally, we examined the extent to which different feedback types (explicit, implicit) affect perceivers' perspective-taking performance. We investigated this question by replicating and extending the experimental design of Damen et al. (2020, **Chapter 4**) in which perceivers judged addressees' perception of a speaker's message. Findings indicated that perceivers learned from the feedback they received. After feedback, perceivers made predictions that were more accurate, meaning that they were less likely to assume that addressees would also perceive the speaker's sarcasm.

Experiment 2 further evidenced the effectiveness of feedback to improve perceivers' social judgment, by ruling out the possibility that improvements in perspective-taking were due to task demands. In Experiment 2, perceivers judged addressees' perspective only once after they did (one-shot implicit feedback) or did not (one-shot control) received insight

into the addressees' uptake of the speaker's message. Results showed that perceivers' perspective-taking accuracy did not differ between the one-shot control condition and the two-shot control condition from Experiment 1, nor between the one-shot implicit feedback condition and the two-shot implicit feedback condition from Experiment 1. Importantly, perceivers made less accurate predictions about the addressees' perspective in the absence of feedback, thereby replicating the results from Experiment 1.

This study also examined whether the type of performance feedback (explicit, implicit) affects the extent to which perceivers improve the accuracy of their predictions. We reasoned that the corrective intent of implicit feedback might be less clear to perceivers, because they need to infer counterfactual information from a description of addressees' true perspective (e.g., Chandler, 2003; Ellis et al., 2009; Nicholas et al., 2001; Van Beuningen et al., 2008). Consequently, this might affect perceivers' processing of counterfactual information and their ability to override an automatic egocentric response. We therefore assumed that perceivers' adjustments made to the self-perspective would be less accurate after implicit rather than explicit feedback. In contrast to this expectation, however, perceivers' adjustments did not depend on the type of feedback they received. More specifically, perceivers who were informed about the extent to which their first prediction was inaccurate (explicit feedback) were just as accurate the second time around as the perceivers who had to infer their perspective-taking accuracy from a description of addressees' true perspective (implicit feedback).

We further showed that perceivers were less likely to project their perspective onto others on *first* predictions when they were explicitly informed about addressees' perception than when perceivers had to infer this information from a description of addressees' behavioral responses. This reduction in egocentric projection also translated to people improving their accuracy over time. In this sense, perceivers' egocentric anchoring improved more after explicit feedback than after implicit feedback. It could be argued that these results were caused by perceivers learning to uphold a different strategy to infer the addressee protagonist's perspective based on the type of feedback they received. Recall that perceivers in the explicit feedback condition received tailor-made feedback about the inaccuracy of their judgment based on their answer on the 7-point scale. Hence, these perceivers were explicitly informed about the extent to which their egocentric anchoring was inaccurate. Perceivers in the explicit feedback condition made better perspective-taking deductions on first trials, decreasing the overall level of egocentrism (over time). In contrast, perceivers in the implicit feedback condition had to deduce the incorrectness of their judgment from a description of the addressee's behavioral response to the message. Perceivers receiving the feedback implicitly, therefore, could have been more cautious to assume the addressee's sincere interpretation until they had actually inferred the

addressee's uptake of the messages. Given the ambiguity of the judgments, it is reasonable to assume that the readers in the implicit feedback condition kept assessing the target's feelings based on an (incorrect) self-anchor. Especially in ambiguous situations, readers are expected to judge other people's perspectives using their own knowledge as a frame of reference (e.g., Epley, Morewedge, & Keysar, 2004; Krueger, 2003; see also "extratarget strategies" such as egocentric projection and stereotyping in Ames, 2005). This reliance on self-knowledge is argued to decrease in light of behavioral counterevidence (for a review see Ames, 2005). Our findings from the implicit feedback condition confirm this suggested pattern. Perceivers receiving implicit feedback only adjusted their egocentric anchor for the second measurement of that same scenario, but they did not transfer this adjustment to later scenarios. Examining which perspective-taking strategy readers upheld to infer addressees' perspective is beyond the scope of this research. Future research might examine this question by using more qualitative methods to capture readers' perspective-taking strategy at each step during the task.

This study replicated the curse of knowledge effect on perspective-taking (e.g., Damen et al., 2020, **Chapter 4**; Epley et al., 2004, Keysar, 1994; Weingartner & Klin, 2005, 2009). Perceivers were more likely to attribute their perception of the speaker's sarcasm onto addressees when their privileged information suggested the speaker was being sarcastic than when their privileged information suggested the speaker was being sincere. More importantly, we further showed that this curse of knowledge effect was eliminated when perceivers received insight into another person's mental state. That is, when perceivers experienced the addressee's uptake of the speaker's message, they reported they were more aware of this difference in perspectives, and this awareness was reflected in their behavior as they were less likely to overestimate the similarity in beliefs. In this way, our findings extend those by Weingartner and Klin (2005), who found that perceivers took more time to process addressees' perspective because it presented them with counterfactual information they needed to reconcile. We have shown that this online processing of counterfactual information translated to perceivers explicitly acknowledging addressees' perspective when judging their viewpoint. Important to note here is that perceivers' bias was only eliminated after they experienced the addressee's uptake of the speaker's message (implicit feedback), but not after they received explicit feedback about their performance. Hence, even though perspective-taking accuracy improved for both feedback types, perceivers' bias was not eliminated after explicit feedback (see also discussion in Eyal et al., 2018). These findings correspond to previous studies by Camerer et al. (1989), Thompson and DeHarport (1994), and Eyal et al. (2018), who showed that receiving or *getting* (Eyal et al., 2018) information that allows perceivers to make accurate inferences can reduce – or in this study eliminate – perceivers' egocentric bias during social judgment.

In this way, this study provides promising directions for future studies aimed to eliminate related cognitive biases (see Pronin, 2006, for a review).

An intriguing question that follows from this line of argumentation is whether feedback helps perceivers to learn more about their overall perspective-taking proficiency. Findings from perceivers' self-reported perspective-taking tendency in Experiment 1 suggest that perceivers receiving feedback did not feel that they took the addressee's perspective more into account than the participants who did not receive such feedback. A tentative explanation can be that perceivers' confidence in their perspective-taking proficiency was lowered due to the feedback they received (e.g., see Ryback, 1967). Results from perceivers' self-reported perspective-taking tendency in Experiment 2 seem to support this tentative explanation. We showed that, although perceivers experiencing addressees' uptake of the messages made equally accurate predictions about addressees' perspective in both implicit feedback conditions, those who received information about addressees' uptake *after* making a first prediction (two-shot implicit feedback) reported a lower perspective-taking tendency than the perceivers who received this information *before* making any prediction at all (one-shot implicit feedback condition). In other words, it seems that the perceivers in the two-shot condition were less confident about their perspective-taking performance due to the information (feedback) they received, compared to the perceivers in the one-shot condition. The actual perspective-taking performance, however, did not differ between the two conditions. This reduction in confidence can be especially beneficial in terms of increasing perspective-taking accuracy. That is, perceivers' overconfidence in their ability to predict another person's mental state reduces the likelihood of perceivers making an accurate prediction of another person's perspective (Eyal et al., 2018; Savitsky, Keysar, Epley, Carter, & Swanson, 2011; Swann & Gill, 1997). Future research might further examine the relation between feedback and perceivers' confidence, and how this confidence is related to perspective-taking accuracy.

Findings of this research are in line with the egocentric anchoring and adjustment process of perspective-taking by showing that perceivers who were informed about another person's true perspective were more likely to *adjust* away from an egocentric interpretation in order to take into account the differences in perspectives than those who did not receive such feedback. Interestingly, even though explicit performance feedback helped perceivers to improve their perspective-taking accuracy, our findings further showed that these accurate adjustments after feedback were, however, still "insufficient" (e.g., Barr, & Keysar, 2005; Epley et al., 2004; Epley, 2008; Epley & Gilovich, 2004, 2006; Jacowitz & Kahneman, 1995; Tversky & Kahneman, 1974). That is, perceivers' receiving explicit feedback were still biased by their own perception of the speaker's message. Only perceivers receiving implicit feedback about their performance were able to inhibit their own uptake of the

message in order to appreciate the less informed perspective of addressees. This study further identified an interesting phenomenon with regard to people's tendency to improve the accuracy of their perspective-taking judgments. In line with previous research (e.g., Hanna, Tanenhaus, & Trueswell, 2003; Keysar, Barr, Balin, & Brauner, 2000), results from our study showed that perceivers only improved their perspective-taking when they are prompted to do so. However, we have further shown that directly experiencing another person's perspective not only improves the accuracy of perceivers' judgments, but also eliminates any bias that might affect this accuracy. We encourage future research to examine whether this performance feedback helps perceivers to improve the accuracy of their social predictions over time, and whether this improvement will transfer to other perspective-taking activities by helping perceivers to select a more accurate strategy to gain insight into another person's viewpoint.

Conclusion

This study showed that perceivers made predictions about another person's perspective that were more accurate after they received insight into their perspective-taking performance. Perceivers learned to improve their estimations of another person's viewpoint when they were informed about the extent to which their predictions had been accurate (explicit feedback) and when they received a description of another person's true perspective (implicit feedback). Although both feedback types improved perceivers' perspective-taking accuracy in comparison to the baseline (no feedback), perceivers' egocentricity bias was only attenuated when they directly experienced another person's true perspective.

Chapter 6

General Discussion and Conclusion

Four years of academic research definitely enhances one's view on a phenomenon. This also happened to acclaimed writer, Naema Tahir, who radically changed her view on arranged marriages after defending her dissertation on the topic at the end of 2019 (Riemersma, 2019). Having refused to enter an arranged marriage herself, she actively engaged in the public debate about women's individualistic role in a multicultural society. During her research, however, she learned that she had projected her own beliefs onto the topic, thereby underappreciating the cultural values of the traditional marriage. She now advocates that people should come to an unbiased understanding of arranged marriages, something that is only possible if they are aware of how the lens through which they approach the matter at hand frames their conception and understanding of it. Although this sounds desirable, the research in this thesis suggests that an unbiased understanding may be difficult to achieve.

As we noted at the onset of this dissertation, the earlier literature is largely equivocal regarding perceivers' ability to engage in accurate perspective-taking. On the one hand, some studies evidenced perceivers' propensity to represent another person's point of view spontaneously and accurately (e.g., Brown-Schmidt & Hanna, 2011; Brown-Schmidt, Gunlogson, & Tanenhaus, 2008; Elekes, Varga, & Kiraly, 2016; Heller, Gorman, & Tanenhaus, 2012; Schneider, Slaughter, Dux, 2017; Qureshi, Apperly, & Samson, 2010), whereas others showed that perceivers often find it difficult to suppress the automatic representation of their own point of view (e.g., Apperly, Back, Samson, & France, 2008; Epley, Morewedge, & Keysar, 2004; Ferguson, Apperly, & Cane, 2017; Keysar, Lin, & Barr, 2003). The latter studies point to an egocentricity bias, stating that the accessibility or ease by which perceivers' own perceptions are retrievable biases their ability to form a social judgment that truly reflects another person's perspective (e.g., Barr & Keysar, 2005; Birch & Bloom, 2007; Epley, Keysar, Van Boven, & Gilovich, 2004; Keysar, Barr, & Horton, 1998; Krueger & Clement, 1994). In this dissertation, we tested this egocentricity bias, and examined whether we could attenuate this egocentricity bias by stimulating perceivers to focus their attention on another person's point of view before judging this person's perception. We tested whether an explicit focus on what another person knows or is attending to would help perceivers to prioritize this other perspective before their own, evidenced by perceivers representing this other perspective in communication.

We studied this question in four empirical studies, each building on influential earlier studies in the field. Each chapter replicated the design of a seminal previous study (either directly or conceptually), and subsequently extended the research design to fit the specific research question of this thesis. Given that we already discussed each of the studies in detail in each of the individual chapters, we shall be comparatively brief here. In this chapter, we will only briefly summarize and discuss the main findings of our four studies, and address the implications as well as the directions for future research.

Chapter 2: To what extent does an explicit focus on another person's point of view promote perspective-taking?

In our first study, we addressed two main questions. First, we examined perceivers' propensity to take another person's (visual) perspective. Subsequently, we questioned whether we could stimulate this perspective-taking, by instructing perceivers to focus on another person's point of view. We investigated these questions in two different experiments. Both experiments adopted Todd, Hanko, Galinsky and Mussweiler's (2011) spatial perspective-taking task in which speakers described an object's location that could be located on the basis of their own predominant point of view (egocentric frame of reference) or from the perspective of another person (altercentric frame of reference). This task tapped into perceivers' spontaneous perspective-taking, because we measured speakers' tendency to adopt another person's frame of reference without being explicitly instructed to do so. We examined whether speakers would show to appreciate another person's unique vantage point and to prioritize this viewpoint over their predominant egocentric frame of reference by locating the object from this person's perspective and not from their own point of view.

In Experiment 1, we tested the assumption that perceivers focusing on self-other differences are more likely to adopt another person's point of view spontaneously than perceivers focusing on self-other similarities. To this end, we directly replicated Todd et al.'s (2011) experimental paradigm and primed speakers to focus on either self-other differences or self-other similarities before they were asked to locate an object in a visual scene. As in Todd et al. (2011), we contrasted speakers' descriptions of the object's location to the descriptions that were provided by speakers who were not primed with a specific mind-set. Our findings showed that we were unable to replicate Todd et al.'s (2011) findings. In the setting in which the prime was absent, we found that the majority of speakers (61%) located the object from their egocentric perspective. This egocentricity bias during spatial perspective-taking was also apparent when speakers were focusing on self-other differences (73%) or on self-other similarities (69%). In contrast to Todd et al. (2011), a mind-set focusing on self-other differences did not attenuate speakers' egocentricity bias. Speakers focusing on self-other differences were just as likely to describe an object's location from their egocentric perspective as speakers focusing on self-other similarities were.

In Experiment 2, we tried to intensify speakers' awareness of perspectives and addressed the question whether explicit focus instructions would stimulate spontaneous perspective-taking. Instead of priming speakers beforehand with a particular mind-set, we explicitly instructed speakers to focus on either their own (self-focus) or on another person's (other-focus) point of view during the spatial perspective-taking task. We achieved this by asking speakers to indicate how objects appeared to themselves (self-focus) or to

another person (other-focus) before they located the object in the visual scene. Findings revealed that other-focused speakers were more likely to adopt another's perspective than self-focused speakers were. More specifically, only 16% of the self-focused speakers described the object's location from the other person's perspective, compared to 51% of the other-focused speakers. Interestingly, however, these percentages did not significantly differ when we compared them to the percentage of altercentric responses (39%) in the setting in which these explicit focus instructions were absent. In other words, compared to the baseline, explicit other-focus instructions did not increase perspective-taking, nor did explicit self-focus instruction reduce perspective-taking. The findings of these two experiments support the existence of perceivers' egocentricity bias and confirm its robustness. They further suggest that it is difficult to change perspectives in communication in a subtle way.

Chapter 3: To what extent does speakers' referential communication benefit from an explicit focus on addressees' perspective?

In our previous study, we tested speakers' tendency to adopt another person's point of view *spontaneously*, and examined whether we could stimulate this propensity by explicit other-focus instructions. In this sense, the spatial perspective-taking task in **Chapter 2** differed from tasks that measure perceivers' perspective-taking *accuracy*, such as false-belief (e.g., Schneider, Bayliss, Becker, & Dux, 2012) and Director Tasks (e.g., Epley, Morewedge, & Keysar, 2004) in which egocentric responses are termed to be inaccurate. In **Chapter 3**, we addressed the question whether explicit other-focus instructions stimulate perspective-taking in a task that measures perceivers' perspective-taking accuracy. In particular, we set out to investigate the influence of explicit other-focus instructions in a communicative setting in which speakers (and addressees) are expected to take their interlocutors' perspective. To this end, we tested speakers' tendency to regard their addressee's perspective in a referential communication setting, and examined the extent to which speakers refer to information that is not known by their addressee. We further tested whether an explicit and repeated focus on the information the addressee is attending to would help speakers to refrain from leaking information about their privileged perspective and, consequently, design references that are more optimal from their addressee's point of view.

Inspired by the design of Kaland, Krahmer and Swerts (2011, 2014), we invited dyads to take part in a referential communication game. Speakers were instructed to refer to a target object (e.g., a circle) in such a way that their addressee would be able to select the intended target out of a set of four objects. Out of these four objects, three were visible

to both addressees and speakers, and one object was privileged to speakers and, thus, hidden from addressees' view. The three mutually visible objects were differently shaped (e.g., circle, triangle, square) and could, therefore, be distinguished by mentioning their shape. The fourth object was blocked from the addressees' perspective and privileged to speakers. This occluded figure showed a contrast in either size or color to one of the three mutually visible objects (e.g., large circle). Research has shown that speakers not only regularly overspecify their references, but that they are also likely to *leak* information about their privileged perspective (Wardlow Lane et al., 2006; Wardlow Lane & Ferreira, 2008; Wardlow Lane & Liersch, 2012). Speakers leak privileged information when their reference to the target object includes information (in the form of an adjective) about the contrast that is presented to them in privileged ground ("the *small* circle"). In two experiments, we tested whether we could attenuate speakers' informational leakage by explicitly instructing them to focus on addressees' perspective *before* they construed their referential message. Speakers allocated to this addressee-focus setting pointed out the three objects their addressee was seeing before they identified the target object. We contrasted these referential messages to a communicative setting in which speakers were asked to point out the four figures that were visible to themselves (stimulating a speaker-focus), and to a setting in which speakers did not receive explicit focus-instructions (baseline). Our findings showed that speakers were very likely to refer to information that was privileged to them, regardless of an explicit addressee- or speaker-focus. Compared to the baseline, addressee-focused speakers did not leak less privileged information, nor did speaker-focused speakers increase their informational leakage. We replicated these findings in a subsequent experiment in which we intensified the focus manipulation. We concluded that, even during a relatively easy task and with explicit instructions to take addressees' knowledge and attentional status into account, speakers were very likely to refer to information that was not known to addressees. In sum, the findings of this study evidenced speakers' egocentric bias during referential communication, and showed this bias is not attenuated by an explicit focus on another person's perspective.

On could ask whether speakers relied on addressees' feedback to infer whether their referential communication had been successful or not. If speakers relied on addressees' contribution to make communication successful (e.g., Clark & Brennan, 1991; Clark & Krych, 2004), they might not have felt the need to correct for perspective mistakes themselves. If speakers are able to rely on addressees' feedback in deciding whether a message was formulated correctly, they do not have to rely on their own cognitive judgment whether their message adheres to addressees' perspective. In this sense, self-generated feedback—by constantly monitoring whether the to be disclosed information corresponds to addressees' knowledge and attentional status—is more cognitively taxing than being

able to rely on others to detect perspectives mistakes. Speakers are therefore expected to only rely on self-generated cues when other-generated feedback is not available to them (Gann & Barr, 2014). This raises the question whether our speakers would have been more attentive to the elicited other-perspective if they were not able to rely on addressees' collaborative contribution, but were designated to their own judgments. We addressed this question in **Chapter 4**.

Chapter 4: To what extent does an explicit focus on another person's perspective influence readers' perspective-taking accuracy?

Two experiments in **Chapter 4** tested whether an explicit focus on another person's knowledge and belief would attenuate perceivers' egocentricity bias during reading, thereby stimulating the accuracy of perceivers' perspective-taking. Experiment 1 replicated Keysar's (1994) illusory transparency of intention paradigm, which we termed as the "curse of knowledge effect". In this paradigm, readers judge an addressee protagonist's perception of a speaker's sarcasm. Readers read a story about a speaker protagonist sending an ambiguous message (e.g., "You wanted to know about the restaurant, well, marvelous, just marvelous.") to one of his friends. In this message, the speaker refers to a past experience. Only readers learn whether this experience had been positive ("The food was indeed delicious and the service impeccable.") or negative ("The food was unimpressive and the service was mediocre."). Hence, information about the speaker's experience is privileged to readers and not known to the addressee protagonist. Earlier studies suggest that readers not only use their privileged information to perceive the speaker's message as sincere (positive experience) or sarcastic (negative experience), but that they also use this information to guide their interpretation of the addressee's uptake of the message (Keysar, 1994; see also Epley, Keysar, Van Boven, and Gilovich's, 2004). That is, when readers perceive the speaker's message as sarcastic, they wrongly assume that the addressee will also perceive the speaker's sarcasm. Readers' knowledge about the speaker's communicative intention thus curses their ability to appreciate a less informed perspective.

The aim of this study was twofold. First, we aimed to replicate Keysar's (1994) curse of knowledge effect in a non-student population, using a sample that is more representative for the population at large (e.g., Peterson, 2001). Second, we examined the extent to which an explicit focus on the addressee's perspective would attenuate readers' egocentricity bias during reading. We tested the assumption that readers focused on the addressee's perspective would be less likely to think that this addressee shares readers' privileged perspective than the readers who do not receive such explicit addressee-focus

instructions. We investigated this question by randomly allocating readers either to an addressee-focus or to a baseline setting. In the addressee-focus setting, readers read an introductory scenario that introduced the addressee's character and her preferences. This information could later on be used when readers needed to make choices on the basis of the addressee's perspective. This addressee-focus task should make all information associated with the addressee accessible (see also Garrod & Sanford, 1990; Sanford, Clegg, Majid, 1998; Weingartner & Klin, 2005), including the information that is known by or accessible to this addressee. Findings of this study showed that we replicated Keysar's (1994) curse of knowledge effect in a non-student sample. Readers thought the speaker's intention was clearest to the addressee to the extent it was clear to themselves. We also showed that readers' egocentricity bias did not attenuate by the explicit addressee-focus instructions. Regardless of an explicit focus on the addressee's perspective, readers still overestimated the similarity between their and the addressee's perception of the speaker's sarcasm.

In our first experiment, the addressee-focus instruction was unrelated the addressee's interpretation of the speaker's message. This means that any false-belief readers might have had about the addressee's uptake of the speaker's message was not addressed by the focus manipulation. Our second experiment, therefore, confronted readers with the focus instructions during the reading task, and explicitly instructed readers to focus on which information was accessible to addressees before they judged their interpretation of the speaker's messages. To this end, we replicated and extended Epley, Keysar, Van Boven, and Gilovich's (2004) "Sarcastic Messages" experiment, in which readers judged addressee protagonists' interpretation of a speaker's voicemail. In addition to replicating their egocentric anchoring and adjustment paradigm, we manipulated the extent to which readers were explicitly instructed to acknowledge their own knowledge of the speaker's perspective (speaker-focus) or the extent to which they were explicitly instructed to acknowledge the uninformed perspectives of the addressees (addressee-focus) before judging the addressees' uptake of the messages. Our findings confirmed perceivers' egocentric anchoring and (insufficient) adjustment during perspective-taking. Although perceivers' judgments of the addressees' perception of sarcasm were more moderate when they judged addressees' perspective than when they judged their own interpretation of the messages, readers still thought addressees would perceive the speaker's sarcasm. Interestingly, we further showed that enhancing perceivers' attention to addressee' knowledge and attentional status during the reading and perspective-taking task did not diminish perceivers' egocentric projection.

Chapter 5: To what extent does feedback improve perceivers' perspective-taking accuracy?

As demonstrated in **Chapter 2** to **4**, trying to shift perceivers' attention to an alternative perspective prior or during perspective-taking does not stimulate spontaneous (**Chapter 2**) nor more accurate (**Chapter 3** and **4**) perspective-taking. Perceivers' egocentric perspective is apparently too accessible and, therefore, too predominant to stimulate an accurate monitoring and adjustment process (see Epley et al., 2004; Epley, 2008; Keysar & Barr, 2002). One thing that could have contributed to perceivers not sufficiently adjusting their judgment away from an egocentrically biased interpretation is the fact that they were never confronted with the consequence of their inaccurate predictions. In **Chapter 4**, for instance, readers did not learn that the speaker's intention was less transparent to addressees than it was to themselves. We questioned whether informing perceivers about their perspective-taking performance (i.e., performance feedback) would help perceivers to improve their perspective-taking. In **Chapter 5**, we investigated this question by replicating the experimental design of **Chapter 4**, and extending the paradigm by allocating readers to one of the three feedback conditions (none, explicit, implicit). In the two feedback conditions, readers received either explicit or implicit performance feedback about their initial predictions of the addressees' interpretation of the speaker's messages. In the explicit feedback condition, readers were informed about the extent to which their prediction had been inaccurate. In the implicit feedback condition, readers received insight into the addressees' uptake of the message by reading about their thoughts and actions after the event. After this explicit or implicit performance feedback, readers re-judged the addressees' perspective. Findings showed that participants who were given the opportunity to learn through feedback not only adjusted their perspective-judgment more than those who did not receive feedback, they also showed less egocentric projection on future assessments. Readers adjusted their perspective within the same trial to the same degree for both feedback types. Interestingly, however, readers' egocentricity bias was only attenuated after receiving individuated information about their addressees. More precisely, whereas readers receiving explicit feedback or no feedback at all still judged that addressees' would perceive the speaker's sarcasm more when privileged information suggested the speaker was being sarcastic rather than sincere, the judgments of readers receiving implicit feedback did not show this biased pattern. On the contrary, those who experienced the addressees' perspective by reading about their thoughts and behavior in response to the speaker's message were able to acknowledge that these addressees had no reason to perceive the speaker's sarcasm, even though readers' egocentric perspective suggested otherwise.

In sum, the four empirical studies in this dissertation examined the role of explicit attention to another person's perspective on perceivers' tendency to engage in spontaneous

(**Chapter 2**) and accurate (**Chapter 3** to **5**) perspective-taking. We showed that an explicit attention to another person's perspective does not necessarily stimulate perceivers to adopt another person's visual perspective (**Chapter 2**), to improve their audience design during referential communication (**Chapter 3**), or to improve the accuracy of their predictions during reading (**Chapter 4**). We concluded that interpersonal accuracy only seems to improve when perceivers directly experience another person's perspective, and learn to use this insight during perspective-taking (**Chapter 5**).

Implications and Suggestions for Future Research

The findings presented in this dissertation lent support for the robustness of perceivers' egocentricity bias during both language production and language comprehension. We have shown that this bias continues to affect perceivers' perspective-taking, even when they are focused on information that would allow them to make more accurate estimations of other people's perspectives. In our four empirical studies, we did not just ask perceivers to hypothesize about opposite perspectives (see for instance Lord, Lepper, and Preston, 1984), but we *showed* perceivers this opposite side before they engaged in perspective-taking. Regardless of this explicit focus on more reliable perspective-information, perceivers still overly relied on their own perceptions to judge those of others. In this way, our findings contribute to the anchoring and adjustment model that explains how perceivers engage in perspective-taking (Epley et al., 2004; see also Barr & Keysar, 2005; Tamir & Mitchell, 2013). We have shown that perceivers are very likely to anchor in their predominant egocentric viewpoint and (insufficiently) move away from this egocentric interpretation when predicting another person's perspective (e.g., Epley et al., 2004; Epley & Gilovich, 2004, 2006; Keysar, Barr & Horton, 1998; see also Nickerson, 1999). More importantly, we have shown that helping perceivers to adjust their egocentric interpretation is difficult and requires more than explicit nudges. Stimulating perceivers to focus on another person's knowledge, beliefs and attentional focus did not help them to prioritize this alternative information before their own perceptions, resulting in egocentrically biased judgments.

The findings of our empirical studies indicate that perceivers find it hard to reach interpersonal accuracy, because their (unconscious or automatic) reliance on their own perceptions biases their ability to do so (see also Eyal, Steffel, & Epley, 2018). This implies that perceivers' egocentricity bias – like any other cognitive bias affecting the formation or attribution of beliefs and decision-making (for a review see Pronin, 2006) – is difficult to attenuate. Nudging perceivers to focus on information that would be more reliable for perspective-taking did not help them to inhibit their privileged perceptions during perspective-taking. We have seen that the way perceivers produce and understand

language was largely influenced by how they viewed the world themselves, regardless of their attention to another person's different point of view. This caused perceivers to project their own point of view onto others, attenuating interpersonal accuracy.

In our final study, however, we presented a promising finding with regard to increasing perceivers' interpersonal accuracy and attenuating egocentric biases during social judgment. In **Chapter 5**, perceivers' egocentricity bias was attenuated when they learned to rely on other strategies than egocentric projection. More specifically, when perceivers directly experienced the other person's perspective in response to their judgment (see implicit performance feedback in **Chapter 5**), they were less likely to attribute their egocentric perspective onto others in their subsequent judgments. Similar findings have been found by Eyal, Steffel and Epley (2018), who showed that romantic couples who *asked* rather than *took* their partner's perspective made subsequent predictions of their partner's preferences that were more accurate. As Eyal et al. (2018) mentioned, asking others about their perspectives is, of course, intuitively a more reliable method than simply guessing these perspectives on the basis of existing knowledge. Interestingly, however, the couples who acquired knowledge about their partner's preferences through conversation were not aware of the benefit of this strategy. In fact, couples who got their partner's perspective were equally confident about their ability to assess their partner's preferences than those who relied on other (ineffective) strategies to predict the other's point of view (Eyal et al., 2018). This interesting pattern was also reflected in our perceivers' self-reported perspective-taking performance in **Chapter 5**. Although our perceivers learned the most through the information they received from the other person, they did not report higher levels of perspective-taking proficiency. Apparently, even though individuated information about the other person enabled perceivers to inhibit their egocentric perspective and stimulated them to predict the other's perspective more accurately, perceivers were not aware that this strategy would also result in the best outcome.

We argued that the discrepancy in perceivers' introspection and their actual perspective-taking performance could be the result of a reduction in perceivers' confidence. In **Chapter 5**, perceivers were repeatedly confronted with feedback about their perspective-taking performance, which could have reduced their confidence in their ability to understand other people's perspectives. This reduction in confidence, however, can be especially beneficial in terms of increasing interpersonal accuracy. In fact, overconfidence is one of the biases that reduces the likelihood of perceivers making an accurate prediction of another person's point of view (e.g., Ames, Kammrath, Suppes & Bolger, 2010; Eyal et al., 2018; Savitsky, Keysar, Epley, Carter, & Swanson, 2011; Swann & Gill, 1997). Unfortunately, individuals often fail to see their own susceptibility to cognitive biases and, hence, fail to see their own limitations in this (e.g., Ames & Kammrath, 2004; Dunning,

Johnson, Ehrlinger, & Kruger, 2003; Pronin, 2006). This low self-awareness is detrimental for perceivers' perspective-taking improvement, with research showing that those low in self-awareness do not learn from the feedback they receive (Sheldon, Dunning, Ames, 2014), as they perceive any effort to improve this competence as irrelevant (Frantz, 2006). Thus, perceivers' low self-awareness about their perspective-taking proficiency – whether it is triggered due to them over- or underestimating their abilities – can stand in their way to selecting a strategy that helps them to reach accurate insight into their interlocutors' perspectives.

In this dissertation, we focused on instances in which perceivers' egocentric projection compromised their ability to acknowledge differences in perspectives. There are, however, ample situations in which egocentric projection might actually help perceivers to see similarities in dispositions and viewpoints. In instances such as these, egocentric projection might actually lead to more accurate insights than, for instance, stereotyping (Ames, 2005). It thus boils down to the question whether people can learn to select the right strategy to gain interpersonal insight at the correct time (see Ames, 2004ab, 2005, for an overview of strategies). In our final study, as well as in Eyal et al. (2018), perceivers were confronted with the use of one strategy above the other. The question remains, however, whether perceivers will also *get* the other's perspective if they are offered the opportunity to do so. Will perceivers' privileged knowledge stand in the way of them acquiring the knowledge they need? Will repeated experience through, for instance, performance feedback stimulate perceivers to exert effort in selecting a strategy that increases their interpersonal understanding? Future studies can explore the extent to which perceivers' cognitive biases and their beliefs in the effectiveness of each strategy used affect their ability to understand people's perspectives in communication.

Furthermore, our findings offer promising implications for interventions that aim to increase interpersonal understanding. In conflict resolution settings, for example, negotiators often find themselves corroded onto their own misconceptions of the situation, meaning that they enter negotiations with a biased understanding of their interlocutor's interests (e.g., Thompson & Hastie, 1990; see also Thompson, Nadler, & Lount Jr., 2006). This is compounded by the fact that most negotiators do not exchange information that is needed to update their false-beliefs (e.g., Thompson, 1991), thereby negatively affecting their trust and understanding, and their chances of finding a solution for their conflict (e.g., Thompson & Loewenstein, 1992; Thompson et al., 2006). Helping negotiators to exchange information, and to acquire knowledge and experience might help them to debiase their judgment and to increase interpersonal understanding. Future studies can explore which communicative strategies employed by third-party interventions are most effective in helping negotiators to increase interpersonal understanding.

To conclude, this dissertation shows the value of replication studies to further our understanding of the phenomenon being examined. By replicating seminal studies in the field, we were able to verify and strengthen the reliability of the original and reproduced findings. This was reinforced by preregistering our hypotheses and analyses before data collection. This whole process made us realize how important it is that studies are described in sufficient detail and that, preferably, the experimental materials and the data are openly available to aid the success of any replication attempt. While we were setting up our replication studies, all original authors were available for clarifications and answered our questions in detail. Therefore, we want to express our utmost gratitude to Constantijn Kaland, Boaz Keysar, and Andrew Todd for helping us to stand – firmly – on the shoulder of giants.

Conclusion

In this dissertation, we examined the role of an explicit attention to another person's point of view on perceivers' ability to regard this other perspective during language processes. The findings of our four empirical studies showed that perceivers' egocentricity bias during language production and comprehension is robust. Regardless of an explicit attention to information accessible to another person, perceivers were still likely to describe spatial relations from their own point of view (**Chapter 2**), to refer to information not known to their interlocutor (**Chapter 3**), and to overestimate the similarity in beliefs (**Chapter 4** and **5**). Findings from our final study suggest that directly experiencing another person's viewpoint might help perceivers to inhibit egocentric projection during mental state inference. A valuable lesson we draw from this discussion is that we – as perceivers of other minds – should give our deductions a helping hand by acquiring actual insight that will help us to understand others in a less biased manner. Presumably, this will remain difficult and effortful for many people to achieve. Perhaps we will have to come to terms with the fact that we will not be like Sherlock Holmes any time soon.

Summary

An integral part of our social life is our propensity and ability to deduce what other people desire, know, believe and feel. This perspective-taking should help us to adjust our communication accordingly. We often aim to approach other minds in a Sherlockian manner: perceiving and deducing what other people know or experience without letting our egocentric perceptions bias our better judgment. An abundant amount of research has shown, however, that taking other people's perspective, like Doyle's consulting detective, is not that easy to achieve. In fact, when communicating with others, we often do not acknowledge that they might pay attention to different things or evaluate things differently than we do. As perceivers of other minds, we often find it difficult to appreciate this difference in perspectives because our own perceptions bias our ability to do so. In this dissertation, we set out to investigate this curious case of the perspective taker. We questioned whether the perspective taker could learn to set aside their egocentric perspective when estimating those of others. In particular, we investigated whether perceivers of other minds would be less likely to produce and understand language based on their egocentric perspectives if they are explicitly attending to the perspective of their interlocutor.

We examined perceivers' propensity and ability to engage in perspective-taking through four empirical studies. In each of these studies, we explicitly and repeatedly stimulated perceivers to pay attention to their interlocutor's perspective either before (**Chapter 2** and **3**), during (**Chapter 3** and **4**) or after (**Chapter 5**) they assessed their interlocutors' perspectives. We investigated whether these instructions would help perceivers to spontaneously (**Chapter 2**) or accurately (**Chapter 3** to **5**) acknowledge their interlocutors' perspectives in communication. We examined our research questions by independently replicating the experimental designs of seminal studies in the field. We subsequently adapted and extended these designs to fit the purpose of our research. This approach allowed us to investigate the replicability of earlier results and enabled us to compare the current findings with past conclusions more effectively.

Overview of Studies

Study 1

In our first study presented in **Chapter 2**, we examined perceivers' propensity to adopt another person's visual perspective. In particular, we questioned whether we could stimulate this perspective-taking by instructing speakers to focus on another person's point of view before they located an object in a visual scene. We adopted Todd, Hanko, Galinsky and Mussweiler's (2011) spatial perspective-taking task in which speakers described an

object's location. As a first response, speakers could either describe the object's location from their own point of view (egocentric anchoring), or locate the object from the perspective of the person who was co-present in the visual scene. In the first experiment, we tested the replicability of Todd et al.'s (2011) finding, namely that speakers focusing on self-other differences are more likely to describe the object's location from another person's visual perspective than speakers focusing on self-other similarities. To this end, we directly replicated Todd et al.'s (2011) experimental paradigm by priming perceivers with a particular mind-set that focused either on self-other differences or self-other similarities before they located an object in a visual scene. However, we were unable to replicate earlier results. Speakers focusing on self-other differences were just as likely to describe an object's location from their egocentric perspective as speakers focusing on self-other similarities.

We questioned whether the speakers' egocentric anchoring and lack of perspective taking was due to the priming method being unrelated to the spatial perspective-taking task that followed it. Hence, we hypothesized that raising speakers' awareness of spatial perspective differences would incite them to adopt another person's viewpoint more spontaneously, especially when their awareness of spatial differences was raised during the perspective-taking task itself. We tested this question in our second experiment. Instead of priming speakers beforehand with a particular mind-set, we now explicitly instructed speakers to either focus on their own perspective or on another person's point of view during the spatial perspective-taking task. We achieved this by asking speakers to indicate how objects appeared either to themselves, or to another person, before they located the object in the visual scene. Speakers allocated to a baseline did not receive these explicit focus instructions. We found that speakers who were focused on another person's point of view were more likely to adopt this person's perspective spontaneously than the speakers who were focused on their own perspective. However, compared to the speakers in the baseline, we found that explicit instructions to focus on another perspective did not increase perspective-taking, nor did explicit instructions to focus on one's own perspective reduce perspective-taking. In sum, this study supports the existence of perceivers' egocentric bias during communication, and confirms its robustness. We have shown that perceivers anchor their communication on egocentric information, and that this egocentric anchoring is difficult to adjust into an alternative perspective using only subtle nudges.

Study 2

We have already shown that it is very difficult to stimulate speakers to prioritize another's viewpoint by shifting their focus to an alternative perspective. We questioned whether the effects observed in our first study would also arise in situations in which

speakers need to attend to their interlocutor's perspective to facilitate successful communication. We set out to investigate this question in our second study, presented in **Chapter 3**, by examining the ability of speakers to acknowledge their interlocutor's perspective during referential communication. Previous studies have repeatedly shown that speakers often leak information from their own point of view when referencing, thereby disclosing information that is not relevant from the perspective of their addressees. It was unknown, however, whether stimulating speakers to focus on their addressee's perspective before engaging in referential communication would attenuate this informational leakage and, consequently, help speakers to design more optimal references from their addressee's perspective.

Inspired by Kaland, Krahmer and Swerts' (2011, 2014) experimental design, we invited dyads to take part in a collaborative referential communication game in which speakers referred to objects in such a way that their addressee could identify the intended figure. We allocated the dyads to one of the three experimental settings. Each of these settings elicited a different perspective-focus (baseline, self-focus or other-focus). In the two perspective settings, speakers were explicitly instructed to regard either their addressee's point of view or their own perspective before they referred to the target object. We tested the assumption that explicit attention to their addressee's perspective will help speakers to inhibit leaking privileged information, thereby stimulating them to construe a message that is optimally tailored to the perspective of their addressee. We found that speakers were very likely to refer to information that was privileged to them, regardless of an explicit focus on their addressee's knowledge and attentional status. Compared to the baseline, the leakage of privileged information did not decrease among speakers focused on their addressee's point of view, nor did it increase among speakers focused on their own viewpoint. We replicated these findings in a subsequent experiment in which we intensified the focus manipulation. We concluded that, even during a relatively easy task and with explicit instructions to take their addressee's knowledge and attentional status into account, speakers were very likely to refer to information that was not known to their addressee. The findings of this study demonstrated speakers' egocentric bias during referential communication, and showed that this bias is not attenuated by an explicit focus on another person's perspective.

Study 3

Even though our second study showed that an explicit focus on an alternative perspective did not affect speakers' communication, we did not know whether these effects arose due to the collaborative nature of the task. That is, speakers could have relied on their addressee's contribution to single out any comprehension or perspective-taking mistakes.

We thus questioned whether speakers would have been more attentive to their addressee's perspective if they were not able to rely on their addressee's collaborative contribution, but were solely dependent on their own judgments. In our third study, we set out to investigate this question. In two experiments, we explored whether an explicit focus affects perspective-taking accuracy during a task that involves reading. Our first experiment replicated Keysar's (1994) illusory transparency of intention paradigm. In this paradigm, readers judged an addressee protagonist's perception of a speaker's sarcasm. Readers read two stories in which a speaker protagonist sent an ambiguous message to a friend that, depending on information privileged to the readers, could be interpreted as either sincere or sarcastic. Readers were subsequently asked to judge how the addressee protagonist would interpret the speaker's message. Previously, it had been found that readers' privileged information guides their interpretation of the addressee's uptake of the speaker's message. More specifically, readers are more likely to assume that addressees will also perceive the speaker's sarcasm when readers interpret the message as being more sarcastic as opposed to sincere. Readers' privileged knowledge about the speaker's intention thus curses their ability to appreciate a less informed perspective. In our first experiment, we tested whether we could replicate Keysar's (1994) curse of knowledge effect in a non-student population, using a sample that is more representative for the population at large. Secondly, we examined the extent to which an explicit focus on the addressee's perspective would attenuate this curse of knowledge effect. We randomly allocated readers to either an addressee-focus or to a baseline setting. Readers in the addressee-focus setting read an introductory scenario that introduced the addressee protagonist's character and her preferences, and were subsequently asked to use this information to make choices on the basis of the addressee's perspective. This addressee-focus task should make all information associated with the addressee accessible, including the information that is known by or accessible to this addressee. We tested the assumption that readers focused on the addressee's perspective would be less likely to attribute their perception of the speaker's sarcasm to the addressee, as opposed to the readers focused on their own perspective. We were able to replicate Keysar's (1994) curse of knowledge effect in a non-student sample. Readers attributed their perception of the speaker's intention onto the uninformed addressee. We also showed that this egocentric projection was not attenuated by the explicit addressee-focus instruction. Regardless of an explicit focus on the addressee's perspective, readers still overestimated the similarity between their and the addressee's perception of the speaker's sarcasm.

In our first experiment, the addressee-focus instruction was unrelated to the addressee's interpretation of the speaker's message. This means that any false beliefs readers might have had about the addressee's uptake of the speaker's message was not addressed by the focus manipulation. Our second experiment, therefore, confronted

readers with the focus instructions during the reading task. We explicitly instructed readers to focus on what information was accessible to addressees before they judged the addressees' interpretation of the speaker's message. To this end, we replicated and extended Epley, Keysar, Van Boven, and Gilovich's (2004) "Sarcastic Messages" experiment, in which readers judged addressee protagonists' interpretation of a speaker's voicemail. This enabled us to investigate the replicability of the egocentric anchoring and adjustment paradigm, which suggests that perceivers adjust their perspective-judgments away from an egocentric frame of reference to account for any perspective-differences that might exist. Previously, it had been found that these adjustments are often insufficient, because perceivers' egocentric perspective biases this adjustment process. Whether an explicit focus on an alternative perspective could help perceivers to adjust accordingly, however, was yet to be explored. We set out to investigate this question in our second experiment. In addition to replicating Epley et al.'s (2004) egocentric anchoring and adjustment paradigm, we manipulated the extent to which readers were explicitly instructed to acknowledge their own knowledge of the speaker's perspective (speaker-focus) or the extent to which they were explicitly instructed to acknowledge the uninformed perspectives of the addressees (addressee-focus) before judging the addressees' uptake of the speaker's voicemail. Our findings confirmed perceivers' egocentric anchoring and (insufficient) adjustment during perspective-taking. Although readers' judgments of the addressees' perception of sarcasm were more moderate when they judged the addressees' perspective of the message as opposed to their own, readers still thought addressees would perceive the speaker's sarcasm. We further showed that enhancing readers' attention to the addressees' perspective did not diminish perceivers' egocentric projection.

Study 4

As has been demonstrated in the previous studies in this dissertation, trying to shift perceivers' attention to an alternative perspective prior or during perspective-taking does not stimulate more spontaneous or accurate perspective-taking. Perceivers' egocentric perspective is apparently too accessible and, therefore too predominant, that it biases social judgment. What was unexplored, however, is whether previous findings were the result of perceivers never being confronted with the consequences of their perspective-taking mistakes. For instance in our third study, readers did not learn that the speaker's intention was less transparent to addressees than it was to themselves. We set out to investigate this question in our fourth and final study. In this study, presented in **Chapter 5**, we tested whether informing perceivers about the success of their perspective-taking performance would help perceivers to improve the accuracy of their judgments.

We replicated the experimental design of our fourth study and extended the paradigm by allocating readers to one of the three feedback conditions (none, explicit or implicit). In the two feedback conditions, readers received either explicit or implicit feedback about their initial predictions of the addressees' interpretation of the speaker's message. In the explicit feedback condition, readers were informed about the extent to which their predictions had been inaccurate. In the implicit feedback condition, readers received insight into the addressees' uptake of the messages by reading about their thoughts and actions after receiving the speaker's voicemails. After this explicit or implicit feedback, readers re-judged the addressees' perspective. Findings showed that readers who were given the opportunity to learn through feedback – both implicit and explicit – improved their perspective-taking, compared to the readers who did not receive feedback. Within the same trial, readers adjusted their perspective to the same degree for both feedback types. Interestingly, however, readers' egocentric bias was only attenuated after receiving individuated information about their addressees. Readers receiving either no or explicit feedback judged that the addressees would perceive the speaker's sarcasm more, when privileged information suggested the speaker was being sarcastic rather than sincere. Readers receiving implicit feedback, however, did not show this biased pattern in their judgments. On the contrary, those who were able to experience the addressees' perspective by reading about their thoughts and behavior, were able to acknowledge that these addressees had no reason to perceive the speaker's sarcasm, even though the readers' egocentric perspective suggested otherwise.

Implications

Our findings lend support for the robustness of perceivers' egocentric bias during both language production and language comprehension. We have shown that this bias continues to affect perceivers' perspective-taking, even when they are focused on information that would allow them to make more accurate estimations of other peoples' perspectives. In this way, our findings contribute to the anchoring and adjustment model that explains how perceivers engage in perspective-taking. We have shown that perceivers are very likely to anchor their predominant egocentric viewpoint and (insufficiently) move away from this egocentric interpretation when they predict another person's perspective. More importantly, we have shown that helping perceivers to adjust their egocentric interpretation is difficult and requires more than explicit nudges. In particular, stimulating perceivers to focus on another person's knowledge, beliefs and attentional focus did not help them prioritize this information over their own perceptions, resulting in biased judgments. In fact, we have shown that perceivers' egocentrism can be attenuated, only when they learn

to rely on other strategies than egocentric anchoring and adjustment. That is, perceivers who directly experienced what their interlocutor thought, desired or felt were able to accurately acknowledge this different perspective during social judgment. Hence, our findings offer promising implications for interventions that aim to increase interpersonal understanding. Being open for discussion, and for exchanging and acquiring perspective-information, might help perceivers to accurately assess and acknowledge what truly goes around in the mind of those around them.

Conclusion

The findings of our four empirical studies showed that perceivers find it difficult to disregard their egocentric perspective when producing and understanding language. We have particularly shown that perceivers' egocentric bias is robust and difficult to attenuate. Irrespective of an explicit and repeated focus on interlocutors' perspectives, perceivers are still very likely to describe spatial relations from their own point of view (**Chapter 2**), to refer to information not known to their interlocutors (**Chapter 3**), and to overestimate the similarity in beliefs (**Chapter 4** and **5**). We have shown that perceivers' egocentrism can be attenuated when they directly experience another person's point of view (**Chapter 5**). A valuable lesson we thus draw from our findings is that we – as perceivers of other minds – should give our deductions a helping hand by acquiring actual insight that will help us to understand others in a less biased manner. Presumably, this will remain difficult and laborious for many people to achieve. Solving the curious case of the perspective taker may not quite be that *elementary*.

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Publication List

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Damen, **D**., Van der Wijst, P., Van Amelsvoort, M., & Krahmer, E. (2019). Perspective-taking in referential communication: Does stimulated attention to addressees' perspective influence speakers' reference production? *Journal of Psycholinguistic Research*, *48*(2), 257-288.

Damen, **D**., Van der Wijst, Van Amelsvoort, M., & Krahmer, E. (2020). Can the curse of knowing be lifted? The influence of explicit perspective-focus instructions on readers' perspective-taking. *Journal of Experimental Psychology: Learning, Memory and Cognition.* Advance online publication: doi.org/10.1037/xlm0000830

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Book Chapters

Van der Wijst, P., Hong, A., & **Damen**, **D**. (in press). Context and environment in negotiation. In D. M. Kilgour & C. Eden (Eds.), *Handbook of Group Decision and Negotiation*.

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Working Papers

Damen, **D**., Van Amelsvoort, M., Van der Wijst, P., Pollmann, M., & Krahmer, E. (2019). Lifting the curse of knowing: Feedback improves perspective-adjustments. *Submitted for publication*.

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Papers in Conference Proceedings (peer reviewed)

Damen, **D**., Van Amelsvoort, M., Van der Wijst, P., & Krahmer (2019). Lifting the curse of knowing: How feedback improves readers' perspective-taking. In A.K. Goel, C.M. Seifert, & C. Freksa (Eds.), *Proceedings of the 41st Annual Conference of the Cognitive Science Society* (pp. 1586-1591). Montreal, QB: Cognitive Science Society.

Damen, **D**., Van Amelsvoort, M., Van der Wijst, P., & Krahmer, E. (2018). Changing minds: The effect of stimulated attention to another's different point of view on visual perspective-taking. In T. T. Rogers, M. Rau, X. Zhu, & C. W. Kalish (Eds.), *Proceedings of the 40th Annual Conference of the Cognitive Science Society* (pp. 1572-1577). Austin, TX: Cognitive Science Society.

Damen, **D**., Van der Wijst, P., Van Amelsvoort, M., & Krahmer, E. (2018). The curse of knowing: The influence of explicit perspective-awareness instructions on perceivers' perspective-taking. In T. T. Rogers, M. Rau, X. Zhu, & C. W. Kalish (Eds.), *Proceedings of the 40th Annual Conference of the Cognitive Science Society* (pp. 1578-1583). Austin, TX: Cognitive Science Society.

Damen, **D**., Van der Wijst, P., Van Amelsvoort, M., & Krahmer, E. (2017). Fostering perspective-taking in social interaction. In M. Schoop, & D. M. Kilgour (Eds.), *Doctoral Consortium of the 17th International Conference on Group Decision and Negotiation* (pp. 1-9). Stuttgart, Germany.

Damen, **D**., Van der Wijst, P., Van Amelsvoort, M., & Krahmer, E. (2017). Perspective-taking in referential communication: Does stimulated attention to addressee's perspective influence speakers' reference production? In G. Gunzelmann, A. Howes, T. Tenbrink, & E. J. Davelaar (Eds.), *Proceedings of the 39th Annual Conference of the Cognitive Science Society* (pp. 1866-1871). Austin, TX: Cognitive Science Society.

Damen, **D**., Van der Wijst, P., Van der Toorn, Y., & Van Amelsvoort, M. (2014). Circular questions, trust and understanding in mediations. In P. Zaraté, G. Camilleri, D. Kamissoko, & F. Amblard (Eds.), *Group Decision and Negociation 2014: Proceedings of the Joint International Conference of the INFORMS GDN Section and the EURO Working Group on DSS* (pp. 262-267). Toulouse University.

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Camilleri, G., Kamissoko, D. & Amblard, F. (Eds.), *Group Decision and Negociation, 2014: Proceedings of the Joint International Conference of the INFORMS GDN Section and the EURO Working Group on DSS* (pp. 106-111). Toulouse University.

Papers of Conference Presentations (peer reviewed)

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in communication

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