From Conversation to Connection: The Pragmatics of Conversational Listening

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From Conversation to Connection: The Pragmatics of Conversational Listening

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FROM CONVERSATION TO CONNECTION: THE PRAGMATICS OF CONVERSATIONAL LISTENING

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ABSTRACT

Clicking, vibing, sharing a wavelength, feeling in sync. People have extensive lay beliefs (and phrases) capturing the importance of social connection. For decades, academics and practitioners have advanced the idea that listening is a powerful process for building these connections. But what defines high-quality listening? Using a multi-disciplinary and multi-method approach, my dissertation research identifies several gaps in our understanding of this ubiquitous social process and seeks to develop a deeper understanding of the *pragmatics of conversational listening*. First, I develop a novel model of *conversational listening*, which I conceptualize as a dynamic process enacted by two or more people through behavior in conversation (verbal, non-verbal, and paralinguistic cues). Critically, this model involves a three-stage process of (1) *attention*—directing one's attention to conversational content, (2) *processing*—interpreting and evaluating conversation content cognitively, (3) *expression*—enacting observable behaviors that convey one's successful execution of the first two stages. In this way, high-quality conversational listening requires that people are both *being* and *feeling* heard by their conversation partners.

Next, I empirically tested the extent to which *being* heard and *feeling* heard align during live conversation. Using a combination of correlational and experimental methods, results revealed that people struggle to decipher attentive from inattentive listening. In fact, people tended to *overestimate* the extent to which their conversation partners were listening to them—

often believing their partners were listening when they were not. Results revealed that this was due (at least in part) to a lack of diagnostic behavioral cues of attentive listening—people can (and do) successfully feign attentiveness through various non-verbal (e.g., nodding, eye-contact) and paralinguistic (e.g., back-channels, laughter) behaviors in conversation.

Finally, I explore these processes amid interpersonal conflict—a context riddled with poor listening. Results show that people consistently underestimate the extent to which their disagreeing counterparts are willing to listen to and learn about their views. These beliefs predict derogation of counterparts and negative experiences of conflictual conversations. Overall, this work begins to shed light on the uniquely complex and important social process of conversational listening and reveals that the best listening may be spoken.

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INTRODUCTION

In May 2023, a new epidemic was declared in the United States. One that the U.S. Surgeon General declared represents a "profound threat to our health and well-being" (Office of the Surgeon General, 2023). This epidemic affects approximately one in two U.S. adults (Bruce et al., 2019; Shovestul et al., 2020) and increases the risk of pre-mature death as much as smoking 15 cigarettes per day (Holt-Lunstad et al., 2017). This is an epidemic of *loneliness*.

Social Determinants of Well-Being

For many, this declaration begs the question: is a *social* life a *happy* and *healthy* life? Decades of research investigating the social determinants of well-being would suggest that the answer to this question is an emphatic 'yes.' This work reveals that one key to happiness is a rich social life and meaningful connections. A substantial literature has demonstrated a strong positive link between social connection and well-being, while loneliness is related to depression and diminished emotional well-being (Heisel & Duberstein, 2005; Kahneman & Deaton, 2010). This is true at the individual level: the happiest people tend to spend more time with friends and family (Collins et al., 2022; Diener & Seligman, 2002; Mehl et al., 2010). Indeed, a series of recent experimental interventions designed to increase social engagement led to increased reports of happiness (Epley & Schroeder, 2014; Rohrer et al., 2018; Sandstrom & Dunn, 2014; Zelenski et al., 2012). And this relationship persists moment-to-moment as well: people report greater positive affect when they are engaged in social interactions than when they are alone (Kahneman et al., 2004; Larson et al., 1986; Methot et al., 2021; Pavot et al., 1990).

However, it's not only the *quantity* of our social interactions that matter, but also the *quality*. Indeed, while people do report feeling happier when they engage in a higher number of social interactions, the *features* of these interactions (e.g., in terms of the conversational depth,

amount of self-disclosure, and knowing or liking one's interaction partners) have also been found to be an important predictor of positive affect and feelings of connection (Sun et al., 2020). The "need to belong" has been deemed a universal basic need (Baumeister & Leary, 1995)—one that can be satisfied with *pleasant* social interaction and *strong* interpersonal relationships. Consistent with this idea, research has repeatedly demonstrated a critical link between social connection quality and well-being: people experience greater positive affect when engaging with relationship partners to which they feel closer (Vittengl & Holt, 1998); more intimate interactions have been found to decrease feelings of loneliness (Wheeler et al., 1983); happier people tend to have more meaningful conversations (Mehl et al., 2010; Reis et al., 2000). It is clear that reaping the benefits of a rich social life requires not only recurrent social interaction, but also fostering connections defined by feelings of closeness, meaning, and intimacy. So, how do we foster these types of social connections? Important insights into this question may come from the study of *conversation*.

The Psychology of Conversation

Conversation forms the foundation of our social connections. For the most part, our relationships are sparked, fostered, and maintained through our conversations. Indeed, one could even conceptualize social relationships as a series of conversations occurring over time. A stranger who we talk to only a single time could be represented as a single point in time:

[----------•----------]

An acquaintance who we bump into every few months may be depicted as a series of sporadic points:

[-------•----------•----------•----------•----]

A spouse or child could be represented as a dense and repeated pattern of points:

[•-•-•--•-••-•-•-•-•-•-•-••-•-•-•-•-•-•-•--•-•-•-•-•-•••-•-•-•-•-•••-•-•-•-•-•-•-•-•-•--]

If conversation is the basis of our relationships in this way, then it follows that a better understanding of how to foster meaningful social connections requires a better understanding of conversation itself.

Conversation is defined as a verbal interaction between two or more people (Yeomans et al., 2023). The verbal content that defines conversation refers to the exchange of words—these words can be spoken, written, typed, or signed. But, often, our conversations involve more than just words—paralinguistic and non-verbal content are critical aspects of conversation as well. Paralinguistic content refers to the aspects of verbal communication that do not involve words—such as pitch, tone, volume, punctuation—and serve to modify or add nuance (or often emotion) to the meaning of verbal communication. Non-verbal content captures everything we do in a conversation beyond the words—such as eye contact, body language, and facial expressions. These three categories capture the broad range of content that may be involved in a conversation, but the extent to which these different cues are present varies across modality. Face-to-face communication involves the full gambit of conversational content: verbal, paralinguistic and non-verbal. Email communication involves verbal content and limited paralinguistic content but lacks non-verbal content. Phone calls involve verbal and paralinguistic content but provide restricted access to non-verbal content. But, importantly, each of these types of communication involves the verbal content that defines conversation.

For decades, conversation has been studied widely across a variety of disciplines (e.g., Garfinkel, 1956; Goffman, 1981; Heritage, 2008; Pomerantz, 1990; Sacks et al., 1974; Schegloff, 1968; Schegloff & Sacks, 1973; Stivers et al., 2010; Stivers & Sidnell, 2012), but more recent technological advances allow researchers to study conversations in context, more directly, and at

larger scales than ever before. This has given rise to a growing field of work on the psychology of conversation (see Yeomans et al., 2023 for a review). Researchers have sought to describe important patterns in naturally occurring conversations across a variety of contexts from work emails to customer service encounters to police officer body camera footage (e.g., Doyle et al., 2017; Packard et al., 2023; Voigt et al., 2017). Some of this emerging work has taken a more prescriptive approach, combining correlational and experimental methods to explore the relationship between specific conversational behaviors (what people say and do in their conversations) and a variety of important conversational outcomes (e.g., Curhan et al., 2022; Huang et al., 2017; Templeton et al., 2022). It is this latter approach that describes my research. Throughout my research, I use this approach to develop a theoretical and empirical understanding of the conversation behaviors associated with developing, fostering and maintaining meaningful social connections—and in particular, in my dissertation work, I focus on the *pragmatics of conversational listening.*

The Importance of Conversational Listening

A large and diverse body of research has shown that feeling listened to (i.e., "feeling heard") is critical to human thriving. Employees who report feeling heard are less emotionally exhausted (Lloyd et al., 2015), more creative (Castro et al., 2018), seek more feedback (Qian et al., 2019), and overall, show improved work performance (e.g., Bergeron & Laroche, 2009; for a review, see Kluger & Itzchakov, 2022). Patients are more satisfied with the care they receive and adhere more closely to medication regimens when they feel listened to by healthcare providers (Indovina et al., 2016; Shafran-Tikva, & Kluger, 2018; Wanzer et al., 2004). Romantic couples who display more signals of listening are better able to navigate stressors and report greater relationship satisfaction (Bodenmann, 2005; Kuhn et al., 2018; Pasupathi et al., 1999). When

individuals feel heard they experience less anxiety (Itzchakov et al., 2017; Itzchakov & Weinstein, 2021) and report positive boosts in their mood (Hale et al., 1998). Being a good listener is something many, if not most, of us aspire to—and for good reason. A large body of research speaks to the pervasive importance of *conversational listening* for fostering meaningful relationships and promoting individual well-being. But, what is conversational listening? In its most basic form, listening is often thought of as the absence of speaking—the times in conversations when our partner(s) are talking (and we aren't). But conversational listening is much more than silence.

Previous definitions of listening have primarily taken one of two forms. A large body of research in cognitive psychology and psycholinguistics has investigated the *intra*personal aspects of listening. This work defines listening as a cognitive process of "selecting, organizing, and integrating information" (see Imhof, 2010 for a review). Unlike the automatic perceptual process of hearing, *listening* is an intentional process that requires cognitive effort as one selects which information to attend to (Anderson, 2004; Atkinson & Shiffrin, 1968) and commit to working memory for further organization and integration (Baddeley, 2006; Cain, 2006). In conversation, this is what it means to *be heard* by one's partner(s)—a critical aspect of effective communication as it facilitates the exchange of information (e.g., Cain et al., 2004; Cherry & Kruger, 1983; Gilakjani & Sabouri, 2016; Just & Carpenter, 1992; Lundsteen, 1979; Rost, 2009; Strother, 1987).

On the other hand, a wealth of research has investigated the *inter*personal aspects of listening. In conversation, this is what it means to *feel heard* by our conversation partners (regardless of their true cognitive processes; e.g., Itzchakov et al., 2016; Lloyd et al., 2015; Wanzer et al., 2004). These perceptions are critical for successful communication—they drive

positive interpersonal experiences such as trust and relationship satisfaction (e.g., Ramsey & Sohi, 1997; Weger et al., 2014). While is it clear that there are robust and varied benefits of perceiving that others are listening to us, it is less clear what actually constitutes good listening in our conversation partners. Drawing from people's lay beliefs, research has conceptualized "good listening" as a miscellaneous constellation of cognitive (e.g., "to not judge," "to consider"), affective (e.g., "to empathize," "to listen with the heart") and behavioral processes (e.g., "to ask for clarification," "make eye contact"; Halone & Pecchioni, 2001). However, only this last group of processes is directly observable to others and thus, these behavioral processes play a key role in the interpersonal perception of listening (Brunswik, 1952). This idea dates back to Carl Rogers' seminal work on active listening in therapeutic relationships which formalized the idea that our individual behavior (such as nodding, smiling, or paraphrasing) plays a key role in making others feel heard (or not; e.g., Rogers & Farson, 1957). However, to date, there is limited empirical work studying the relationship between discrete conversation behaviors and counterpart experiences of *feeling heard*. This is, in part, due to the lack of a comprehensive and cohesive definition of *conversational listening*.

The disparate nature of the previous work on listening points to a fundamental puzzle in our understanding of conversational listening: it is performed *cognitively* and expressed (and perceived) *behaviorally* (Janusik, 2007; Witkin, 1990)—but people's cognitions and behaviors are not always aligned. For conversational listening to truly be a source of meaningful social connection, individuals must both *be* and *feel* heard by their conversation partners—one without the other falls short of high-quality conversational listening. This leaves many open questions about the *expression* and *perception* of conversational listening that I explore in my dissertation. How does listening unfold in conversation? How closely does *feeling* heard map onto *being*

heard? How can people effectively communicate their engagement and willingness to listen through their behavior in conversation?

Summary of Chapters

Social connection is critical to human flourishing. One theme that has emerged from decades of research in this domain is the power of feeling heard for cultivating myriad positive outcomes for both individual and relational well-being. Indeed, high-quality listening may be key to a happy and healthy life. But what is high-quality listening?

In my first dissertation chapter, solo-authored, entitled *When Listening is Spoken*[1], I develop a novel theoretical model of *conversational listening*, which I conceptualize as a dynamic three-stage process enacted by two or more people through observable behavior in conversation (verbal, non-verbal, and paralinguistic cues). The first two stages are *intra*personal, unfolding in the mind of the listener: (1) *attention*—directing one's attention to conversational content; and (2) *processing*—interpreting and evaluating conversation content cognitively. However, these two stages are imperceptible to others. In the model, I posit a third stage of listening that is unique to dialogue: (3) *expression*—enacting observable behaviors that convey one's successful execution of the first two stages. In the expression stage, a listener communicates the cognitive work they've done to engage with the mind of their counterpart, creating the possibility for the counterpart to "feel heard" and reap the relevant benefits.

While *attention* and *processing* can be sufficient to achieve some informational goals, and the *expression* of listening can be sufficient to achieve some relational goals, I argue that all three stages of listening are needed to foster enduring trust and interpersonal rapport. Together, they

[1] This work was originally published as: Collins, H. K. (2022). When listening is spoken. *Current Opinion in Psychology*, *47*, 101402. https://doi.org/10.1016/j.copsyc.2022.101402

ensure that people will both *be* and *feel* heard. Critically, I argue that verbal behaviors (rather than paralinguistic or non-verbal behaviors) may be the most *valid* signals of conversational listening because these behaviors rely directly on conversation content and thus, cannot be effectively feigned in moments of inattentiveness. In this way, the best listening may be spoken.

This three-stage model of listening makes two important contributions. First, understanding conversational listening as a multi-stage process allows us to integrate previous (and seemingly distinct) work from across fields into a single model describing a sequence of behaviors. This perspective enables a view into the relationship between the various stages of the process. Of particular interest is the nature of the relationship between *being* heard (the *intra*personal aspects) and *feeling* heard (the *inter*personal aspects) and circumstances when they align versus diverge. Second, this model grounds listening as a construct that is enacted with agency—a process that is performed and maintained through people's behavior in conversation—and thus, people can make concrete choices to improve both their listening and listening detection. Examining this model of conversational listening, my empirical research has produced several key insights, some of which I explore in Chapters 2 & 3 of my dissertation.

In my second dissertation chapter, co-authored with Julia A. Minson, Ariella S. Kristal, and Alison W. Brooks, entitled *Conveying and Detecting Listening in Live Conversation*[2], I sought to empirically study the extent to which *being* heard and *feeling* heard align during live conversation. Do perceptions of listening align with listeners' cognitive engagement? Using a combination of correlational and experimental methods, we find that perceivers struggle to distinguish between attentive and inattentive listening—in fact, they tend to *overestimate* the

[2] This work was originally published as: Collins, H. K., Minson, J. A., Kristal, A., & Brooks, A. W. (2024). Conveying and detecting listening during live conversation. *Journal of Experimental Psychology: General, 153*(2), 473–494. https://doi.org/10.1037/xge0001454

extent to which their conversation partners are listening to them. First, we find this phenomenon in the context of natural conversations during which we collected yoked in-the-moment reports of listeners' cognitive engagement and their partners' perceptions of their attentiveness. In 31% of instances, peoples' perceptions did not align with their partners' self-reports—19% of the time perceivers thought their partner was listening attentively when their partner reported mind-wandering, and 12% of the time perceivers believed their partner was not listening when in fact they were. In three follow-up experiments, we manipulated listening during natural conversations in various ways—first by asking participants to complete a separate memorization task during their conversation, second by asking participants to listen to a different audio input in the room other than their partner, and third, using technological software to garble audio so that people could not hear portions of what their partner was saying. In each experiment, we find that our manipulations successfully adjusted people's listening but that their conversation partners' perceptions of their listening did not vary. Further, behavioral coding results suggest that this error is (at least in part) due to a lack of diagnostic cues of attentive listening—people can (and do) successfully feign non-verbal and paralinguistic signals of attentive listening in natural conversations. These findings underscore a key difference between *being* heard and *feeling* heard and call for further investigations of behaviors that effectively and authentically communicate listening—in particular, the importance of verbal expressions of listening.

One of the most common contexts in which people feel as though they are not being heard is during interpersonal conflict. Yet, the ability to communicate effectively in moments of disagreement is critical for many interpersonal processes. In my third dissertation chapter, co-authored with Charles A. Dorison, Francesca Gino, and Julia A. Minson entitled *Underestimating*

Counterparts' Learning Goals Impairs Conflictual Conversations[3], we found across seven studies (N = 2,614) that people consistently underestimate the extent to which their disagreeing counterparts are willing to listen to and learn about their views—a belief that predicts derogation of counterparts and negative experiences of conflictual conversations. Critically, when individuals received information revealing that a counterpart who disagreed with them on a hotly debated issue was interested in listening to and learning about their views, they evaluated that individual more positively and were more willing to engage with them in the future. In the context of the Israeli-Palestinian conflict, Israelis evaluated their Palestinian counterparts more positively when they were told that their partner intended to listen to and learn about their views. Increasing the extent to which individuals believe they will feel heard and understood by disagreeing counterparts may be a powerful conflict resolution tool.

[3] This work was originally published as: Collins, H. K., Dorison, C. A., Gino, F., & Minson, J. A. (2022). Underestimating counterparts' learning goals impairs conflictual conversations. *Psychological Science*, *33*(10), 1732-1752. https://doi.org/10.1177/09567976221085494

CHAPTER 1: WHEN LISTENING IS SPOKEN

Conversational *listening* is critical to human flourishing across domains. At work, employees who feel that their supervisor listens to them report reduced emotional exhaustion, are more committed to their jobs, and show greater internal motivation (among other benefits; Bergeron & Laroche, 2009; Castro et al., 2018; Kluger & Itzchakov, 2022; Lloyd et al., 2015; Qian et al., 2019). In the healthcare domain, individuals are more satisfied with their medical care when they feel heard by their healthcare providers (Indovina et al., 2016; Wanzer et al., 2004) and show greater medication adherence (Shafran-Tikva & Kluger, 2018). In romantic relationships, signals of listening are associated with improved dyadic coping and overall relationship satisfaction (Bodenmann, 2005; Kuhn et al., 2018; Pasupathu et al., 1999; Ramsey & Sohi, 1997; Weger et al., 2014)—even responsive strangers are better liked in get-to-know-you conversations (Huand et al., 2017). Individual well-being is also enhanced when people feel heard—they report less anxiety (Hale et al., 1998; Itzchakov et al., 2017; Itzchakov & Weinstein, 2021), greater self-awareness (Pasupathi & Rich, 2005), and reduced loneliness (Itzchakov et al., 2023). At the most basic level, feeling listened to activates the reward centers in the brain (Kawamichi et al., 2015).

Clearly, good listening is critical to our social lives. However, to be good interpersonal listeners our conversation partners must both *be* and *feel* heard—one without the other falls short of high-quality listening. Emerging work reveals that perceptions of conversational listening are often inaccurate (Collins et al., 2023; see Chapter 2)—dishonest portrayals of listening are common, and often go undetected (a target is *feeling* heard without *being* heard); and, in some cases, honest portrayals of listening are dismissed (a target is *being* heard without *feeling* heard). Conversational listening presents a challenge of deception detection—how can honest listeners

be credited as such, and how can dishonest listeners be discovered? In this chapter, I will review work on listening, arguing that to effectively convey (and detect) honest high-quality listening, people must rely on valid behavioral cues that cannot be feigned: *verbal expressions of listening*. Counterintuitively, the very best listening is spoken.

Cognitive Listening: A Two-Stage Model

Listening is often conceptualized as a two-stage process: (1) *attention*, directing one's attention to conversational content, and (2) *processing*, interpreting and evaluating conversational content cognitively. A long history of research in cognitive psychology and psycholinguistics has defined listening as a process of "selecting, organizing, and integrating information" (Imhof, 2010). Distinct from hearing and seeing, which are the automatic perception of sound waves and visual stimuli, listening is an intentional process that requires attentional capacity and cognitive effort as one selects which conversational content to attend to and commit to working memory for further processing (Anderson, 2004; Baddeley, 2006; Cain, 2006). The cognitive processes of listening facilitate information exchange and learning (Cain et al., 2004; Strother, 1987), and as such, are required for successful interpersonal communication (Cherry & Kruger, 1983; Gilakjani & Sabouri, 2016; Rost, 2009; Gorawara-Bhat & Cook, 2011). However, this two-stage model describes cognitive processes that occur in the mind of the listener, and as such, are unobservable to others (Janusik, 2007; Witkin, 1990). This *intra*personal model does not consider the complex interpersonal dynamics that occur when listening is performed interpersonally, amidst conversation.

Conversational Listening: A Three-Stage Model

Conversational listening unfolds over time as two (or more) individuals take turns speaking and not speaking across several "turns" within a conversation (Figures 1-2).

Conversations occur across modalities, from synchronous face-to-face conversation to asynchronous written conversation (e.g., email). Conversational content always includes verbal information (written or spoken text) and may include nonverbal and paralinguistic content as well. Thus, depending on modality, listeners must utilize their auditory and/or visual attention to monitor behavior across channels of expression—*attending to* and *processing* verbal, non-verbal, and paralinguistic content (Table 1).

Table 1. Examples of verbal, nonverbal, and paralinguistic cues that may (or may not) receive auditory and visual attention from listeners during conversation.

		Listener Attention	
		Auditory Attention	**Visual Attention**
Conversational Content	**Verbal**	spoken words	written text
	Non-verbal	non-spoken sounds (e.g., sniffing, clapping, tapping, footsteps)	body language, nodding, facial expressions, eye gaze
	Paralinguistic	voice pitch, back-channels, laughter, pauses, stutters	written response speed, capitalizations

The interpersonal benefits of listening relate to *perceived* listening in and across conversations (e.g., Bergeron & Laroche, 2009; Castro et al., 2018; Kluger & Itzchakov, 2022; Lloyd et al., 2015; Qian et al., 2019)—they occur when people sense that others are listening to them. Feeling heard cannot directly follow from this two-stage cognitive model of listening, as the processes involved are imperceptible. This suggests that there must be a critical third stage of listening unique to conversation: (3) *expression*, enacting observable behaviors that convey attention and processing of antecedent conversational content (Figure 1 & Figure 2). In this stage, a listener communicates the cognitive work they are doing in order to *show* that they're

listening to their conversation partner(s) (Rogers & Farson, 1957). This third stage is critical to conversational listening—without it, no one will feel heard.

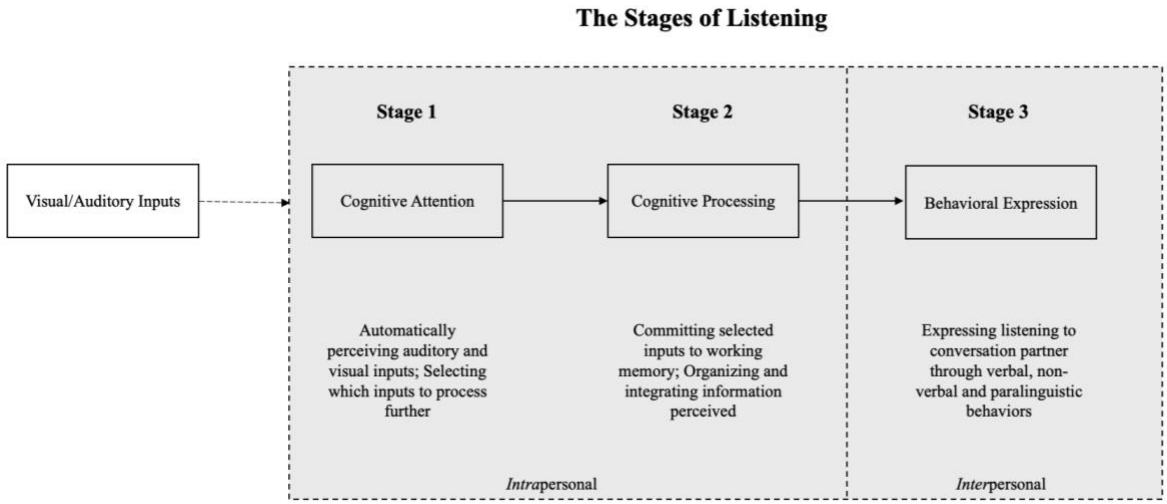

Figure 1. Three-Stage Model of Conversational Listening

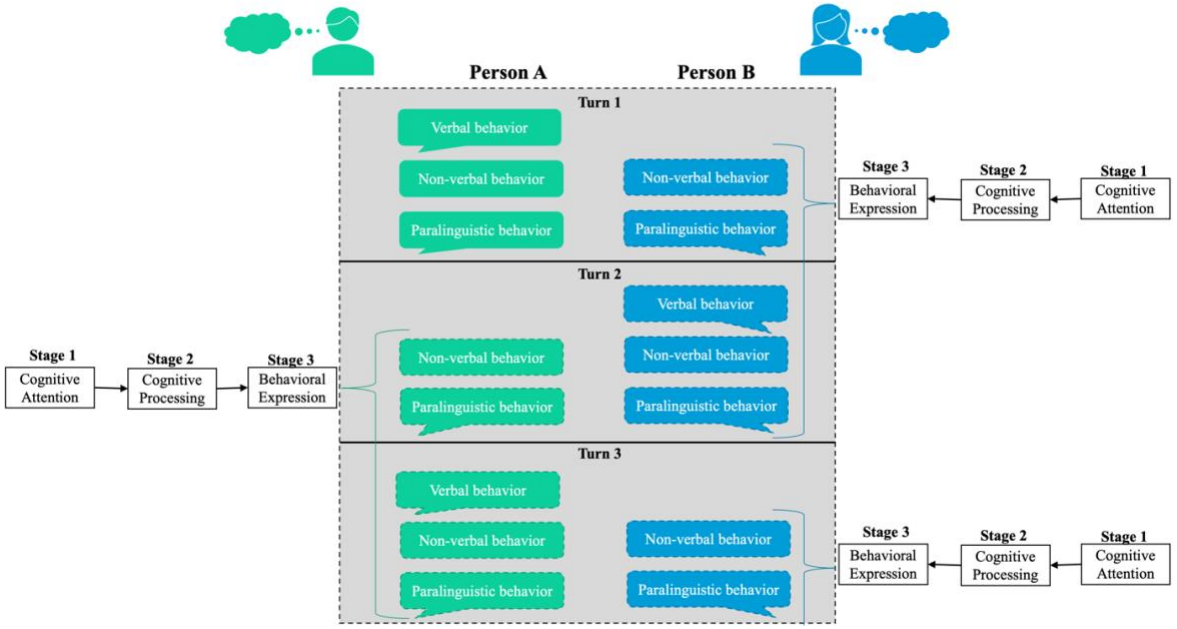

Figure 2. Conversational listening enacted over multiple turns of a conversation.

Listening Expression: Non-verbal, Paralinguistic, and Verbal Cues

Though it is a key step in the process of conversational listening, this third stage also introduces the possibility of deception. Perceivers must infer listener's cognitive engagement (the truth of which cannot be directly observed) from their expressed behavior (which can be misrepresented). Research suggests that the expression of listening can take many forms across the different channels of conversational content—there are nonverbal (e.g., facial expressions, body language), paralinguistic (e.g., pauses, laughter, tone of voice), and verbal (e.g., words, grammar, syntax) signals of attentiveness.

Listening can be expressed through various nonverbal cues of good listening (Bodie & Jones, 2012; Collins et al., 2023, see Chapter 2; Halone & Pecchioni, 2001), such eye contact (Gorawara-Bhat & Cook, 2011; Wohltjen & Wheatley, 2021; Kelly Jr. & True, 1980), head nodding (Hale et al., 2020; Osugi & Kawahara, 2018), smiling and frowning (Wang & Gratch, 2009), and forward trunk lean or close physical distance (Haase & Tepper, 1972)—indeed, these non-verbal behaviors are the primary focus of converants' lay beliefs about what good listening looks like. Additionally, there are paralinguistic cues of listening. Back-channels are short verbal utterances such as "uh-huh," "mhmm," and "um," produced by one participant in a conversation while another is talking. Such utterances are extremely common in conversation—approximately 19% of utterances constitute back-channel feedback (Jurafsky et al., 1997)—and serve to communicate attention and establish rapport (Kawahara et al., 2015). Indeed, back-channels as well as vocal entrainment (i.e., the mirroring of a conversation partner's vocal characteristics such as pitch) have been shown to communicate attention and understanding in conversation (Xiao et al., 2013).

But the expression of listening does not end with non-verbal and paralinguistic behaviors—conversational listening can be expressed verbally as well (Rogers & Farson, 1957). This can include verbal behaviors such as paraphrasing (Snell, 2021; Seehausen et al., 2012; Weger Jr. et al., 2010), asking questions (especially follow-up questions) (Huang et al., 2017), and conversational uptake behaviors—when one acknowledges, repeats, or reformulates what someone else has said, which predicts satisfaction and learning (Demszky et al., 2021). There are many verbal cues of attentiveness, such as verbal affirmations (Yeomans et al., 2020), requests for clarification (e.g., repair questions) (Jurafsky et al., 2009), providing solicited advice (Bodie & Jones, 2012; Halone & Pecchioni, 2001), and calling-back to previous topics, that have yet to be empirically linked to the experience of listening.

Given the myriad cues that signal cognitive engagement in a conversation, which should people rely on to transparently express (and detect) honest high-quality listening? After all, the informational (e.g., learning, information exchange) and interpersonal (e.g., trust, relationship satisfaction) benefits of high-quality listening require that counterparts not just *be* heard but *feel* heard.

Deceptive Cues in Conversational Listening

Prior work has uncovered which behaviors are associated with *perceptions* of good listening (Huang et al., 2017; Gorawara-Bhat & Cook, 2011; Kelly Jr. & True, 1980; Hale et al., 2020; Osugi & Kawahara, 2018; Wang & Gratch, 2009; Haase & Tepper, 1972; Jurafsky et al., 1997; Xiao et al., 2013; Snell, 2021; Seehausen et al., 2012; Weger Jr. et al., 2010), and has delineated lay beliefs about good listening by asking people to describe its cognitive (e.g., “to not judge”), affective (e.g., “to empathize”), and behavioral correlates (e.g., “to make eye contact”) (Bodie & Jones, 2012; Halone & Pecchioni, 2001). This work reveals what people *believe* good

listening looks like. Unfortunately, recent work shows that deception is common in conversational listening—listeners' expressive cues are often misrepresented in conversation (intentionally and unintentionally), and, consequently, dishonest portrayals of listening often go undetected by conversation partners (and honest portrayals can be mistakenly dismissed) (Clark & Schaefer, 1989 Collins et al., 2023, see Chapter 2).

The internal cognitive experience and external behavioral expression of listening misalign to a staggering degree, similar to phenomena such as emotional and verbal deception (Levine & Walk, 2020; Porter & Ten Brinke, 2008; Bond Jr. & DePaulo, 2006). Most conversation partners err in the direction of believing more attention and processing have occurred than is actually the case (Collins et al., 2023, see Chapter 2). Emerging evidence suggests that this over-attribution of listening may be a two-sided problem. Similar to findings in the lie detection literature, behavioral differences between attentive and inattentive listeners in conversation are minimal, making it difficult to decipher another's true level of cognitive engagement (Collins et al., 2023, see Chapter 2; DePaulo et al., 2003). Indeed, attentive listeners neglect to display behaviors that effectively signal listening (even when they are), while inattentive listeners engage in behaviors that signal listening (even when they're not) (Collins et al., 2023, see Chapter 2). This work points to a critical distinction between our current understanding of the behavioral cues that inform perceptions of listening and the cues that *truthfully* represent listeners' cognitive engagement. Though the illusion of attentive listening (i.e., dishonest expressions of listening) may be sufficient to achieve short-term relational motives, such as enjoyment or avoiding awkwardness, such deception likely impedes the pursuit of high-informational motives in conversation, such as achieving mutual understanding—especially over time, across multiple conversations (Yeomans et al., 2022). Therefore, the

effectiveness of high-quality conversational listening requires that listeners engage in (and perceivers look for) behavioral cues that effectively (and not deceptively) make the true cognitive effort of listening transparent. What are these cues?

Verbal Cues: Transparent Expressions of Listening

Bublitz (1988) expressed concerns about deceptiveness of backchannels like "yea" and "uh huh," noting that such simple, short utterances that occur frequently may be the perfect device for "pretending to listen." This concern seems relevant for many non-verbal (e.g., nodding) and paralinguistic cues (e.g., laughing) as well. For example, sustained eye contact is considered by lay people to be one of the key signals of good listening (Abi-Esber et al., 2024). However, eye contact follows established coordination patterns during conversation, wherein eye gaze is directed to whoever is speaking regardless of the content (Ho et al., 2015). Indeed, eye contact can be used to deceptively signal listening even when one's mind wanders far beyond the content of the conversation. Critically, many of the non-verbal and paralinguistic behaviors associated with perceptions of listening are only loosely related to the verbal content of the conversation, which allows them to be performed even in the absence of attention and processing. Therefore, the most commonly relied upon cues of "good listening" (e.g., nodding, eye contact) (Collins et al., 2023, see Chapter 2; Bodie & Jones, 2012; Halone & Pecchioni, 2001) can be misleading—they may not be honest representations of the internal cognitive process of listening.

To transparently express (or detect) listening, interlocutors must learn to engage in (or focus on) behaviors that cannot be faked. In contrast to nonverbal and paralinguistic signals of listening, which may be easily faked, most (if not all) verbal cues of listening respond directly to a partner's verbal content. Thus, they cannot be effectively enacted if one has not *attended* to and

processed the information communicated. For example, paraphrasing requires that a listener both attend to and process what someone else is communicating so that they can then restate it in their own words. Follow-up questions are similarly contingent on the cognitive act of listening, since these questions build from what has previously been communicated. Verbal expressions of listening may be the most effective way to express (and detect) honest conversational listening. Though speakers should not abandon the use of nonverbal and prosodic cues—these are important signals of engagement that form a core part of the lay schema of good listening—content-dependent verbal cues are more direct and reliable indicators of the cognitive process of listening. If someone puts in the cognitive effort to listen attentively, they should claim the rewards of their work by expressing it clearly and transparently.

Another powerful advantage of verbal expressions of listening is that they may actually change the cognitive process of listening for the better. People's intentions guide the information that they attend to, filter, and process (Anderson & Pichert, 1978). Thus, when a person engages in a conversation with the intent to express their listening with content-dependent verbal cues, they will inevitably look for conversation content that they can later paraphrase, acknowledge, ask follow-up questions about or call-back to. In doing so, they must actively *attend* to and *process* the content that is being communicated, thus improving their underlying cognitive engagement in the process. Verbal expressions of listening may not only increase the extent to which people *feel* heard, but also the extent to which they are *being* heard, in positive feedback loops amidst live conversation.

Listening unfolds not only within a single conversation (inter-turn listening), but also across multiple conversations within a relationship (relational listening). Listening can be expressed in the moment that it occurs (e.g., providing back-channel feedback while someone is

speaking), during the next turn (e.g., paraphrasing what someone has said), several turns later (e.g., calling back to something mentioned earlier), or in a separate conversation (e.g., asking a follow-up question about something discussed yesterday). Non-verbal and prosodic cues that do not depend on conversation content can only be used to signal listening in the moment that it is occurring (e.g., nodding can signal that one is listening *right now*, but not that one was listening yesterday). However, since verbal cues are content-dependent, they can signal listening beyond a single conversational turn. Consider a colleague who, in a separate email chain days later, acknowledges a point you made in your last team meeting. In this example, the cognitive act of listening that occurred during an earlier conversation is being expressed later. Just as inter-turn listening may be fundamental to an effective conversation, relational listening may be fundamental to a successful relationship (Reis, 2012)—and verbal expressions of listening are integral to both.

Finally, such transparent expressions of listening may also promote reciprocal honesty. Indeed, preliminary research suggests that people are more willing to disclose information to high-quality listeners (Weinstein et al., 2021)—understandably, honest disclosures may not be beneficial if people are not certain they are truly *being* heard. This seems especially likely over the course of multiple conversations, as transparent expressions of listening build trust and promote relationship satisfaction. Future work should seek to better understand the role of high-quality listening—and its various expressions—in eliciting honesty from others.

Conclusion

Making others feel heard is a critical interpersonal skill that improves relationships and enhances well-being across domains. However, *feeling* heard is not always the same as *being* heard. Conversational listening is a three-stage process, consisting of *attention* and *processing*

which occur *intra*personally and cannot be observed, and finally, *expressio*n, makes the cognitive act of listening observable to others. Without *expression*, nobody would feel heard. However, this final stage of the listening process introduces the possibility of deception. Indeed, conversants tend to focus on non-verbal and paralinguistic cues as signals of listening, but these behaviors are not always honest signals of the underlying cognitive processes of listening. Verbal expressions of listening may be the most effective signals of listening, in part because these content-dependent behaviors cannot be effectively faked in the absence of cognitive listening. Thus, although it sounds counterintuitive, it's possible that the best listening is spoken.

CHAPTER 2: CONVEYING AND DETECTING LISTENING DURING LIVE CONVERSATION

You're in a coffee shop, meeting with a new acquaintance over steaming lattes. While you're talking, your conversation partner seems engaged: they hold your gaze, smile at the funny parts, and nod warmly. You think they're a good listener, and you're excited to see them again soon. But were they *really* listening? If you probed their mind during the conversation, what were they actually attending to and thinking about? Were they really listening attentively, or just creating the impression of good listening?

From business to medicine to romance, being seen as a "good listener" is widely advised and highly desired. And for good reason—perceptions of listening are associated with many beneficial outcomes. In the workplace, employees who feel that their supervisor listens to them report lower emotional exhaustion, lower turnover intentions, greater internal motivation (Lloyd et al., 2015) and seek more feedback (Qian et al., 2019). In romantic relationships, signals of listening are associated with an improved ability to respond to and cope with stressors as well as overall relationship satisfaction (Bodenmann, 2005; Kuhn et al., 2018). In healthcare, patients who feel that their healthcare provider listens to them show higher levels of medication adherence (Shafran-Tikva & Kluger, 2016) and are more satisfied with their care during a hospital stay (Wanzer et al., 2004). Perceptions of listening even play a critical role during first encounters among strangers: perceptions of call center employees as good (or bad) listeners drive customer satisfaction ratings (De Ruyter & Wetzels, 2000; Min et al., 2021), and responsive strangers are better liked in get-to-know-you conversations—they even receive more second-date offers on first dates (Huang et al., 2017). However, though perceptions of listening are

consequential across a wide range of domains, research has not examined the extent to which perceptions of listening are *accurate*.

Previous Research on Listening – A Conundrum for Determining Accuracy

The psychological process of conversational listening is much more complex than simply hearing sounds (i.e., auditory processing). To capture the cognitive experience of listening, and account for the temporally unfolding nature of conversation, which requires participants to listen and respond dynamically and recursively, we define listening as: *attending to and processing another person's verbal, nonverbal, and paralinguistic cues amidst conversation.* This definition reflects the separable cognitive processes of *attending to* and *processing* content from a conversation partner (Collins, 2022), including their verbal cues (e.g., words, grammar, syntax), nonverbal cues perceived visually (e.g., facial expressions, body language, hand gesticulation), and paralinguistic (i.e., prosodic) cues perceived auditorily (e.g., pauses, interruptions, back-channel utterances like "mmhm," "yea," laughter, tone, accent, and volume of voice; Yeomans et al., 2024).

In conversation, the private cognition involved in attending to and processing a partners' cues is happening in a profoundly interpersonal, co-constructed system—in tandem with at least one other human mind. Seminal work in psycholinguistics and conversation analysis suggests that "language use is fundamentally a joint activity" (p. 244, Clark 1994; Clark & Schaefer, 1989; Clak & Wilkes-Gibbs, 1986; Schegloff et al., 2977): while speaking and listening are individual actions, they contribute toward joint "problems"—errors in achieving shared understanding—and dealing with these "problems" requires joint management, strategies that can help both prevent and repair glitches in shared understanding (Clark, 1994). Taken together, individuals use different verbal, nonverbal, and paralinguistic cues to coordinate their shared

understanding on a turn-by-turn basis, and doing so is a uniquely human ability (Dingemanse et al., 2021).

Importantly, though, this coordination process is fraught. While the private act of listening may be *signaled* through responsive verbal cues (e.g., words of affirmation, paraphrasing, follow-up questions), nonverbal cues (e.g., nodding, eye gaze, facial expression), and paralinguistic cues (e.g., back-channels like "uh huh," silence, laughter), those cues may (or may not) represent the underlying cognitive process of listening (Collins, 2022). Furthermore, those cues may (or may not) be accurately perceived. Janusik (2007) describes this conundrum aptly: "Listening research is a challenge, as listening is performed cognitively and perceived behaviorally, but listening cognitions and behaviors are not always congruent (Witkin, 1990)." Accordingly, prior psychological research on listening has focused on the *intrapersonal* (cognitive) experience of listening (and its consequences) and, separately, on the *interpersonal* perception of listening (and its consequences).

Early listening scholars attempted to measure listening using hearing, comprehension, and recall measures. However, more recent scientific consensus has concluded that these measures of listening cannot meaningfully distinguish listening from memory capacity (Thomas & Levine, 1994)—if one is attentively listening in the moment, they may not necessarily remember the content later. Thus, researchers to date have not uncovered a robust measure to capture listening as a unique cognitive activity, distinct from other related cognitive processes.

Separately, prior interpersonal models of listening describe how people *perceive* others' listening. This work has largely relied on two types of measures: 1) the listener's behavioral cues, such as eye contact and nodding, which researchers have assumed are indicative of whether or not someone is paying attention (based on lay beliefs about what good listening looks like),

and 2) their partners' self-reported perceptions of listening (e.g., "How well do you think your partner listened?"; Itzchakov et al., 2016; Wanzer et al., 2004).

Much of the attention in this area has focused on "active listening," a construct initially advocated by humanistic psychologist Carl Rogers (e.g., Rogers & Dymond, 1954; Rogers & Farson, 1957) and considered desirable by many fields (Cheon & Grant, 2009; Hafen & Crane, 2003; Kubota et al., 2004; McNaughton et al., 2008; Mishima et al., 2000; Rautalinko & Lisper, 2004). Active listening embraces the benefits of listening, as well as the importance of communicating that one is doing so (i.e., ensuring that listening is *perceived*). According to this literature, by engaging in behaviors that people associate with listening (see Bodie et al., 2012), the listener can signal to their partner that they are, which will improve the interaction and, ultimately, the relationship. Indeed, active listeners are better liked, and people find interacting with them more satisfying (e.g., Weger et al., 2010; Weger et al., 2014). However, research on active listening has focused on *perceptions* of listening, usually by surveying the perceiver. Thus, it remains unclear whether high-performing active listeners also perform the cognitive work of listening well, or whether they are simply better at conveying this impression.

The current research seeks to address this puzzle by manipulating people's ability to listen during live conversation, as well as employing measures of actual listening, to triangulate on an understanding of the congruence between people's *cognitive* experience of listening and their partners' *perceptions* of it. Given the mismatch between how listening is performed (cognitively), conveyed (behaviorally), and perceived (interpersonally), we predict that perceptions of listening may not always align with listeners' private cognitions:

Hypothesis 1: Perceptions of conversational listening are often inaccurate (i.e., perceiver ratings of listening do not align with target self-reported listening).

A Lens Model Approach

Why might people struggle to perceive others' conversational listening? The classic Brunswik lens model offers a framework to understand the process (and potential failures) of interpersonal perception (Brunswik, 1956; Karelaia & Hogarth, 2008; Nestler & Back, 2013). According to the lens model, perceptual accuracy requires three stages: (1) a latent trait (in our case, "listening") is expressed through observable cues by a target, (2) perceivers must attend to these observable cues, and then (3) perceivers must use this information to inform their perceptions of the target (in our case, their listening). If there is a breakdown at any of these stages—target expression, perceiver attention, or perceiver judgment—then perceptual accuracy will suffer.

The lens framework has been applied to many forms of interpersonal perception. For example, research on lie detection finds that people are, on average, no better than chance at detecting when a person is lying versus telling the truth (see reviews by DePaulo et al., 1985; Kraut, 1980; Zuckerman et al., 1981). One meta-analysis found an average accuracy rate of 54%—and found no difference in performance between lay people and experts such as law enforcement personnel (Bond & DePaulo, 2006). Following from the lens model, two explanations have been offered for this finding: (1) a lack of valid observable cues that reveal deception, and/or (2) the idea that people rely on the wrong behavioral cues to form their judgments. Research suggests that the former explanation plays a larger role—liars behave similarly to truth tellers, resulting in a dearth of valid cues that reveal deception (Hartwig & Bond, 2011; Wiseman et al., 2012).

Importantly, "valid cues" have a particular meaning when people are motivated to mislead their counterparts. In order to accurately diagnose deception, observers must seek cues

that cannot be faked. We expect that the same is true for perceptions of listening. That is, the social value of being perceived as a "good listener" means that people are highly motivated to feign listening even in instances of inattentiveness—and are likely well practiced in doing so. Just as liars seek to conceal cues of their deception, listeners are likely to conceal cues of their inattentive listening. Thus, it may be the case that just as with lying, there are few *valid* cues of good listening that exist—cues that cannot be feigned when one is motivated to do so (Collins, 2022), leading to our second prediction:

> *Hypothesis 2:* Inaccuracies in perceptions of conversational listening are (at least partly) due to a lack of diagnostic behavioral cues conveyed by the listener (i.e., the cues that perceivers focus on are successfully feigned by motivated actors).

Overly Optimistic Perceptions of Listening

We consider two types of perceptual errors in listening detection, sometimes referred to as Type I and Type II errors. First, what happens when people believe that their conversation partners are listening when they are not (Type I error)? For example, imagine you believe a work colleague is listening to you in a meeting, but they are in fact, mentally preparing their grocery list or ruminating about a different project. While you might leave with the impression that you effectively communicated important information, your colleague may be just as uninformed as before the meeting began.

However, the opposite perceptual error might also occur. What happens when people believe that their conversation partners aren't listening when they actually are (Type II error)? From marriage counselors to customer service employees to international mediators, conflict

resolution professionals are frequently tasked with repairing the relational damage resulting from one party accusing the other of failing to listen (e.g., Agne, 2018).

Type I errors (someone perceiving that you are listening when you aren't) may prevent attainment of information exchange goals. However, these errors do allow people to "feel heard," an emotional state known to be extremely positive. Meanwhile, Type II errors (someone perceiving that you aren't listening when you are) likely result in relational costs such as negative emotional reactions and accusations of inattentiveness—though they might make information transmission more likely. Overall, it is hard to know which of these errors is more costly. The costs and benefits of each type is likely to vary across contexts. To make specific predictions about which error is more common in the case of conversational listening, we turn to the theory underlying "want-should conflicts," common situations in which people make choices between behaviors that are pleasant in the moment (e.g., eating ice cream) versus beneficial in the long-run (e.g., eating broccoli; see Bitterly et al., 2015 for a review).

In the case of conversation (as in other want-should conflicts), the "want-self" is myopic and prioritizes instant gratification (i.e., being perceived as a good listener in this moment) while the "should-self" prioritizes long-term benefits (i.e., ensuring that one leaves a conversation well-informed, even at the cost of some immediate awkwardness). Amid conversation—as in battles between ice cream and broccoli—we hypothesize that the "want-self" will win out. In moments of inattentiveness, individuals will mask their wandering mind to maintain the impression that they are listening. This will be especially true when relational goals are or seem more important than informational goals (Yeomans et al., 2021). Just as liars want to be perceived as truthful, listeners may be motivated to cover up moments of inattention to make a good impression—because, as an abundance of research has demonstrated, being perceived as a

good listener confers benefits. Of course, some conversations require a greater emphasis on informational goals (e.g., a doctor explaining how to properly take a medication), which may encourage the "should-self" to allow more Type II errors—or to use more preventative and repair strategies to reveal glitches in shared understanding (Clark, 1994).

This interpersonal want-should conflict as experienced by the target, is largely in line with the one being experienced by a perceiver. Calling out a counterpart for inattentiveness when they are in fact listening—a false accusation—carries an immediate relational penalty. In contrast, giving the counterpart the benefit of the doubt when they are not attending to your words may or may not lead to costs down the road. Thus, prior theorizing suggests that both the target and the perceiver have reason to downplay the occurrence of cues that suggest attentional lapses. This analysis also aligns with error management theory (EMT; Haselton & Buss, 2000), which argues that people are systematically more likely to make a particular error when the costs and benefits of Type I versus Type II errors are asymmetric.

Taken together, we predict that the social desirability of being perceived as a good listener will drive listeners to mask inattentiveness in conversations to serve immediate relational goals (such as those highly salient in our studies). As a result, conversationalists may often be left with the impression that information exchange and social connection occurred, even when those impressions are erroneous (Yeomans et al., 2021). This leads to our third prediction:

Hypothesis 3: Perceptions of conversational listening are over-optimistic (i.e., perceivers believe targets are listening more than they are).

The Current Work

Across five studies, we investigate the extent to which people convey and detect listening in conversation. Our core prediction is that speakers frequently make errors in their perceptions

of whether their counterpart is listening to them. We theorize that these errors occur because 1) people's minds wander more often than we realize amidst live conversation, and yet 2) people are motivated to come across as good listeners, and 3) there are few behavioral cues that allow attentive versus inattentive listening to be detected.

Previous investigations of interpersonal listening have focused on a sense of feeling heard or understood—which incorporate concepts like empathy, care, and support into their measures, for example, by surveying the perceiver with scale items like "X understands how I feel" (Bodie, 2011) or "X cares about me" (Lloyd et al., 2015). We advance this work by simultaneously examining listeners' cognitive engagement during live conversation (via recalled and contemporaneous self-report, as well as a variety of experimental manipulations), allowing us to measure whether they are actually listening, and comparing these measures to whether the perceiver *feels* heard, understood, or validated (during, and after, the conversation).

In an exploratory study, we investigate this phenomenon by assessing the relationship between ratings of self-reported and perceived listening during a live conversation. Then, in a series of experiments (Experiments 1-4), we ask dyads to engage in a live conversation, with individuals later asked to assess the extent to which a target was listening attentively. Using a variety of paradigms, we manipulate actual listening between experimental conditions by introducing distractions, adjusting incentives, or using technology to limit participants' physical ability to hear their partner—and then measure interpersonal perceptions of listening.

Transparency and Openness

We report how we determined our sample size, all data exclusions (if any), all manipulations, and all measures in the study (as suggested by Simmons et al., 2012). All data and

materials are available here:

https://osf.io/w4nf9/?view_only=db83cc05213c4964951ab88b1f9e5de0.

Exploratory Study

In an exploratory study, we sought to investigate our phenomenon of interest: the association between perceived and actual listening during live conversation. In our study design and analysis, we follow in the tradition of interpersonal perception research, which captures perceptual accuracy as the correlation of perceivers' inferences and targets' self-ratings (e.g., Back & Nestler, 2016; Brunswik, 1952, 1956; Zaki et al., 2009). We sought to investigate this phenomenon as directly as possible during live conversation. Thus, rather than asking participants to recall their own and/or their partner's listening after the conversation, we collected a series of moment-to-moment assessments during conversation, following an approach used in mind-wandering research (Smallwood & Schooler, 2006). All procedures and analyses were pre-registered: https://aspredicted.org/blind.php?x=LB5_3GQ.

Exploratory Study Method

Participants

We recruited 141 dyads (made up of two people who were previously unacquainted) from the participant pool at a university in the northeastern United States to take part in a 45-minute study about conversation for which they were paid $18. Our sample consisted of university students, staff, and members of the local community. As per our pre-registration, data from 41 dyads were excluded from analyses.[4] We analyzed data from the remaining 100 dyads (N = 200 participants). Participants reported their gender ("What is your gender? Male, Female, Non-

[4] 27 dyads had technological problems (e.g., poor audio/video quality or dropping off the call completely); 13 dyads failed to follow instructions (e.g., not responding to the private chat messages from the experimenter, or not engaging in the full 25-minute conversation); 1 dyad did not complete the post-conversation survey

binary/Other"; 32% Male, 65% Female, 3% Non-Binary/Other) and their age ("What is your age?" [Open-ended numeric response]; M_{age} = 28 years, SD_{age} = 12 years).

Procedure

This study took place over the Zoom video conferencing platform. All participants were randomly assigned to role of "target" or "perceiver" and paired with a partner assigned to the opposite role for a 25 min conversation. When participants logged onto the online platform, they were randomly assigned by the experimenter to take on the role of 'target' or 'perceiver,' and read detailed instructions about the study procedures. Participants were told they would have a 25-minute "get-to-know-you" conversation. To make the conversation easier, we provided participants with a list of five topics they could potentially discuss (i.e., favorite food, hobby, book or TV show, place to visit, and animal).

We told participants that at various times throughout the conversation, the researcher would send them a private message asking them to report their own (in the 'target' condition) or their partner's (in the 'perceiver' condition) listening at that moment. Specifically, every 5 minutes, the participants were asked to respond to the question: "Think about the last time [your partner was/you were] talking, right before you received this message. In that moment, [were you/was your partner] attentively listening to [your partner/you]?" (1: *Yes, [I/my partner] was fully attentive*, 2: *[My/my partner's] mind was wandering*). Finally, to encourage honest responding, and following the mind-wandering literature (Smallwood & Schooler, 2006), we told all participants that natural fluctuations in attentive listening are normal and commonly occur during conversation. Thus, for each dyad, we obtained five yoked measurements of one participant's self-reported listening and their conversation partner's contemporaneous perception of their listening.

Exploratory Study Results

Targets (who were asked about their own listening) reported listening attentively during 76% of all measurement occurrences, and reported mind wandering during 24% of all measurement occurrences. To the best of our knowledge, this is the first-ever measure of the extent to which listeners' minds wander during live conversation. Our primary measure of interest was the perceivers' detection of their partner's attentiveness versus inattentiveness. To that end, 69% of perceiver guesses of (attentive versus inattentive) listening matched targets' self-reports of listening (attentive versus inattentive). Perceptions aligned 64% of the time when the partner reported listening, and 5% of the time when the partner reported *not* listening. On the other hand, approximately one third of perceivers' guesses (31%) did not align with targets' self-reports. Perceptions diverged from self-reports 19% of the time when the partner reported attentive listening, and 12% of the time when the partner reported *not* listening.

Signal Detection of Attentive Listening

To investigate the direction of this misalignment, we drew on signal detection theory, treating target self-reports of listening as the signal (see Table 2; Stanislaw & Todorov, 1999). Here we see that when the signal was present (i.e., the target reported attentive listening), perceivers recognized it as such in 84% of occurrences. However, when the stimulus was absent (i.e., the target reported not listening), perceivers incorrectly believed their partner was listening in 78% of occurrences (Type I error). These results suggest a strong bias toward believing that the target was listening, regardless of whether they actually were (or were not). We summarize these signal detection results in Table 2 and depict Type I errors across the 5-min measurement increments in Figure 3.

Table 2. Signal Detection Analysis of Exploratory Study Results.

	Perceiver thought target was not listening	**Perceiver thought target was listening**
Target reported not listening	22% CORRECT	78% TYPE I ERROR
Target reported listening	16% TYPE II ERROR	84% CORRECT

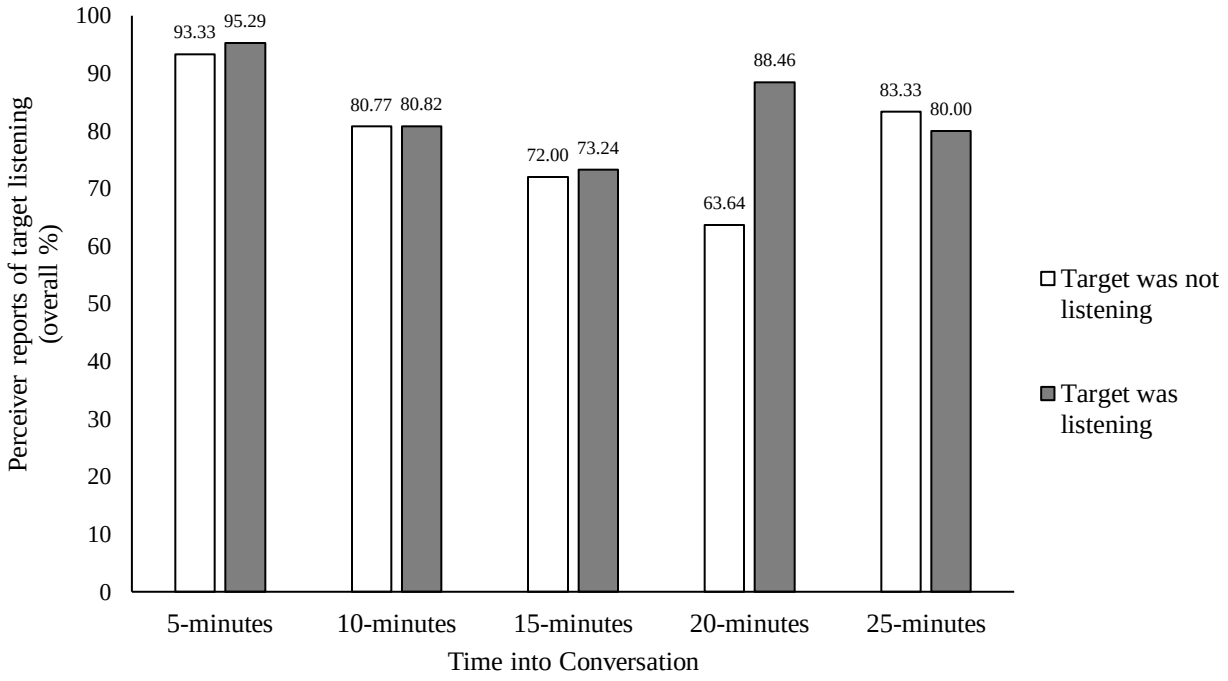

Figure 3. This figure depicts the percentage of perceivers who reported that their partner was attentively listening at each timepoint. Perfect accuracy would be depicted by all white bars at 100% and all gray bars at 0%. Instead, with an exception at the 20-minute mark, the percentage of perceivers who reported that their partner was listening is nearly identical when their partners self-reported listening attentively (gray bars) and when their partners self-reported not listening attentively (white bars).

Perceptions vs. Experience of Listening During Conversation

Moving beyond signal detection, per our pre-registered analysis plan, we analyzed the extent to which targets' self-reported listening during the conversation corresponded to perceivers' ratings of their listening on average. When we compared the average number of times

participants self-reported listening throughout the conversation (i.e., the mean for *targets*) to the average number of times participants perceived their partner to be listening throughout the conversation (i.e., the mean for *perceivers*) using a paired samples t-test, we found that perceived listening (M = 4.14 out of 5, SD = 1.02) was significantly higher than self-reported listening (M = 3.82 out of 5, SD = 1.06), $t(95) = -2.05$, $p = .04$, *Cohen's d* = 0.30, *95% CI* [0.01, 0.59].[5]

Exploratory Study Discussion

Our initial study allowed us to explore our phenomenon of interest—the extent to which perceptions of listening align with listeners' self-reports during a live conversation. Results provide preliminary evidence for two insights: (1) in approximately one-third of instances, perceptions of conversational listening did not match target self-reports (Hypothesis 1), and (2) perceivers largely over-attributed attentive listening, often believing their conversation partners were listening to them when they were not (Hypothesis 3).

The results of this exploratory study reveal that perceptions of listening in natural conversation do not always match reality. Instead, people's minds seem to naturally wander away from the conversation without their counterparts noticing. However, this study was correlational and utilized a binary response scale that may have biased our results—there are many attentive states in between "fully attentive" and "mind wandering" that are not captured here. Furthermore, it is impossible to establish "ground truth": were perceivers over-attributing listening or were targets under-reporting it? Thus, in Experiment 1, we sought to examine this

[5] Five dyads were dropped due to a single missing observation. We repeated this analysis predicting listening rating from a fixed-effect for condition (target vs. perceiver), and a random-effect for dyad to account for repeated observations (Bates et al., 2015). This allowed us to drop only missing responses instead of entire dyads. Results confirmed that perceiver rated listening was higher than target self-reported listening, $b = 0.07$, $p = .009$, *95%CI*[0.02, 0.11]. Additional analyses are included in the Supplemental Material in Appendix A.

phenomenon further, by experimentally inducing variation in participants' motivation to listen (by using distraction and financial incentives) at various levels of attentiveness.

Experiment 1

In Experiment 1, we investigate people's tendency to convey and detect different levels of listening in live conversation by experimentally manipulating the listener's motivation to attend to the conversation. In order to induce variation in listening, we instructed one participant in each dyad to pay careful attention to their partner (*Listening* condition), direct their attention elsewhere (*Distracted* condition), or direct their attention elsewhere while *pretending* to listen to their partner (*Feigned Listening* condition), all during live conversation.

Our three-condition design allowed us to investigate whether participants who are explicitly incentivized to feign listening are perceived differently than those who are simply asked to direct their attention elsewhere. Though a large body of research establishes listening as a desirable social behavior, our exploratory study suggests that conversation partners frequently suffer from moments of inattention, even in short conversations with relatively few distractions. Yet, social desirability may motivate people to maintain the appearance of consistent listening. We thus predict that perceivers will struggle to detect listening differences across these three conditions—attentive and inattentive listeners alike will be given credit for listening attentively.

Experiment 1 Method

Participants

We recruited 162 pairs of strangers from a participant pool at a university in the northeastern United States consisting of university students, staff, and members of the local community. Dyads came into the lab to participate in a 10-minute study on everyday conversations for which they were paid a flat rate of $15, with potential to earn up to an

additional $5 in bonus payments. Data from 11 dyads were excluded from analysis,[6] and thus our analyses are based on the remaining 151 dyads (N = 302). Participants reported their gender ("What is your gender? Male, Female, Non-Binary"; 34% Male, 55% Female, 1% Non-Binary, 10% non-response) and their age ("What is your age" [Open-ended numeric response]; M_{age} = 23 years, SD_{age} = 5 years).

Procedure

When they arrived at the lab, participants were paired with a partner they had not met before and were told that they would spend five minutes in conversation. Specifically, we instructed dyad members to get to know each other and determine whether or not they would make good roommates. We also told participants that there would be a series of videos playing on a screen in the room. The instructions regarding these videos varied by experimental condition as follows.

Listening Manipulation. Within each dyad, one participant sat with their back to the video screen and was instructed to ignore the videos playing behind them. This was the "unmanipulated partner." The other participant (the "manipulated partner") was seated in full view of the video screen and was randomly assigned to one of three conditions: (1) *Listening* condition (n = 50), (2) *Distracted* condition (n = 49), or (3) *Feigned Listening* condition (n = 52). The unmanipulated partner did not know the manipulated partner's private listening instructions. This approach follows the methods of recent conversation research, whereby one conversationalist enacts an experimental intervention unbeknownst to their partner, and researchers observe their partner's natural reactions (e.g., Huang et al., 2017; Yeomans et al., 2020). The manipulated participants were video recorded throughout the interaction.

[6] 2 dyads knew each other; 2 dyads did not complete the questionnaires; 4 dyads experienced a technical malfunction; 3 dyads received a questionnaire that did not match their condition assignment.

We instructed participants in the *Listening* condition to ignore the videos and listen attentively to their partner. Participants learned that they could earn a bonus based on how well they remembered what their partner said.

Participants in the *Distracted* condition were instructed to pay attention to the video playing on the screen behind their partner. They learned that the video consisted of a series of muted commercials and that they could earn a bonus for each commercial that they recalled.

Participants in the *Feigned Listening* condition were instructed to pay attention to the commercials playing on the screen while *pretending* to listen attentively to their partner. These participants were offered a bonus for each commercial they recalled *only if* their partner reported thinking they had been listening throughout the conversation (we did not provide specific criteria for how the partner would make this assessment).

After both participants in a dyad read their instructions independently, they were brought into the conversation room and seated face-to-face.

Measures

Our primary measure of interest was whether the listening manipulation influenced the impressions that the unmanipulated participants formed of their manipulated partners. To this end, we asked the unmanipulated participants to evaluate the quality of their partner's listening, the conversation overall, as well as their partner on several dimensions.

Perceptions of Listening. Unmanipulated participants reported the extent to which they thought their partner was "a good listener," was "interested in what I had to say," and was "engaged in this conversation" on a scale from 1: "Extremely disagree" to 6: "Extremely agree." Responses to these three items were averaged to create an overall measure of the extent to which participants thought their partner was listening ($\alpha = 0.91$).

Enjoyment. Unmanipulated participants also reported the extent to which they agreed (1: "Extremely disagree" to 6: "Extremely agree") with five statements assessing their enjoyment of their partner as well as their conversation (e.g., "I liked my partner" and "I found the conversation with my partner interesting"; $\alpha = 0.91$).

Warmth, Competence, and Status. Unmanipulated participants also rated their partner's warmth (4-items; e.g., "I think my partner is tolerant"; $\alpha = 0.78$; Fiske et al., 2002), competence (5-items; e.g., "I think my partner is independent"; $\alpha = 0.64$; Fiske et al., 2002), and power (3-items; e.g., "I think my partner is dominant"; $\alpha = 0.78$; Smith et al., 2008) on a scale from 1: "Extremely disagree" to 6: "Extremely agree." Warmth, competence, and status are dimensions on which individuals readily evaluate each other and which have important effects for subsequent interactions (Fiske et al., 2007). We were interested in testing whether perceptions of listening, even inaccurate ones, would affect such interpersonal inferences, further supporting our argument that good listening is a highly desired, but often misperceived, behavior. Importantly, both participants were told that their interpersonal ratings would remain private to mitigate potential impression management concerns.

Manipulation Check. To assess whether participants followed our instructions, manipulated participants were asked to recall as many of the commercials as they could. We tallied the number of commercials participants correctly recalled (out of nine). Manipulated participants also predicted the extent to which their partner would report that they were a good listener on a scale from 1: "Extremely disagree" to 6: "Extremely agree."

In an attempt to more directly measure the extent to which participants listened to their partner using a recall measure, unmanipulated participants completed the Activities Preferences Questionnaire (APQ; Surra & Longstreth, 1990; Swann & Gill, 1997) prior to the conversation,

and manipulated participants predicted their responses to the questionnaire after the conversation. In reviewing the conversation transcripts, the vast majority of the conversation pairs did not discuss the activities included in the APQ. Because of this, we do not include this measure in our main analyses.

Experiment 1 Results

Self-Perceptions

Our manipulation successfully shifted participants' attention toward the videos playing in the room, as evidenced by the number of commercials the manipulated participants recalled, $F(2, 148) = 90.11$, $p < .001$. Participants in the *Distracted* condition correctly recalled the greatest number of commercials ($M = 5.61$, $SD = 1.93$). Conducting Tukey's test for post-hoc comparisons, participants in the *Feigned Listening* condition recalled significantly fewer commercials than those in the *Distracted* condition ($M = 4.46$, $SD = 2.36$; $b = -1.15$, *95% CI* [-2.02, -0.28], $SE = 0.37$, $p = .006$), but recalled significantly more than those in the *Listening* condition ($M = 0.84$, $SD = 0.93$; $b = 3.62$, *95% CI* [2.75, 4.49], $SE = 0.37$, $p < .001$; see Figure 4). These results suggest that participants in the *Distracted* and *Feigned Listening* conditions were indeed devoting a substantial amount of attention to a stimulus other than their partner.

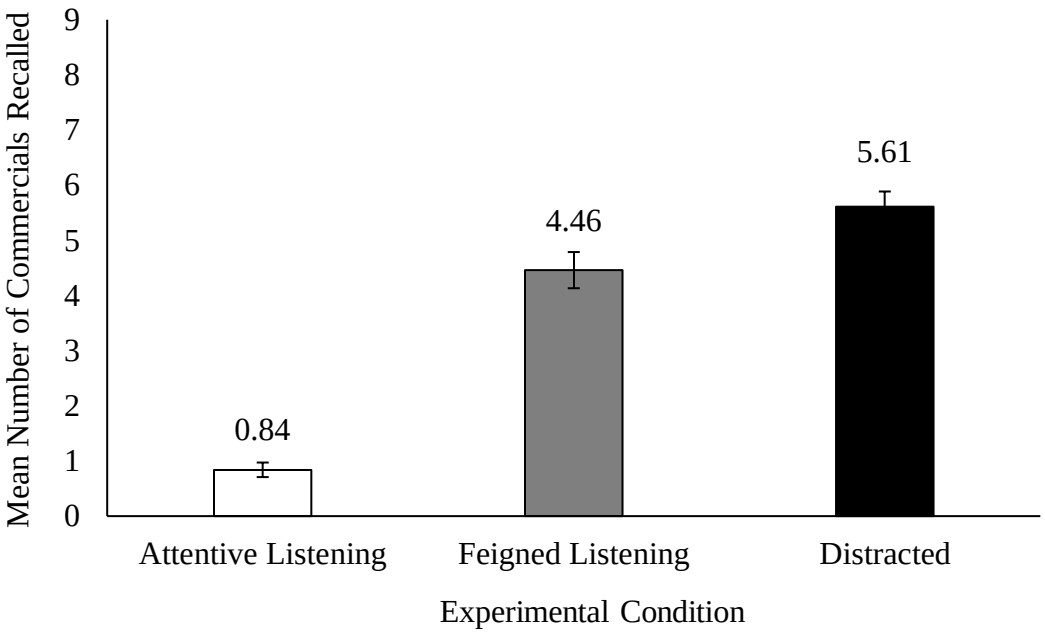

Figure 4. Mean number of commercials correctly recalled by participants across conditions (Experiment 1), with error bars representing standard errors.

Furthermore, manipulated participants believed that the quality of their listening would be easily detected, $F(2, 148) = 5.79$, $p = .004$. Again, using Tukey's test for post-hoc comparisons, participants in the *Distracted* condition thought that their partner would rate them as a significantly worse listener ($M = 4.78$, $SD = 1.18$) than participants in the *Listening* ($M = 5.32$, $SD = 0.77$; $b = -0.54$, *95%CI* [-0.96, -0.13], $SE = 0.17$, $p = 0.006$) and *Feigned Listening* ($M = 5.25$, $SD = 0.56$; $b = -0.47$, *95% CI*[-0.88, -0.07], $SE = 0.17$, $p = 0.02$) conditions. The two latter conditions did not differ from each other ($b = -0.07$, *95%CI* [-0.48, 0.34], $SE = 0.17$, $p = 0.91$), suggesting that participants in the *Feigned Listening* condition felt confident in their ability to convince their partner that they were listening attentively (see Figure 5).

Partner Perceptions

We find no differences between the three conditions in the partners' evaluations of listening quality ($M_{Listening} = 5.36$, $SD_{Listening} = 0.91$; $M_{Distracted} = 5.18$, $SD_{Distracted} = 1.01$; $M_{Feigned}$

$_{Listening}$= 5.42, $SD_{Feigned\ Listening}$ = 0.82; $F(2, 147) = 0.84$, $p = .43$; Listening vs. Distracted: $b = -0.18$, *95% CI* [-0.61, 0.26], $SE = 0.19$, $p = 0.61$; Listening vs. Feigned Listening: $b = 0.06$, *95% CI* [-0.37, 0.48], $SE = 0.18$, $p = 0.95$; Distracted vs. Feigned Listening: $b = 0.23$, *95% CI* [-0.20, 0.67], $SE = 0.18$, $p = 0.42$). Participants who conversed with a partner who was secretly memorizing commercials rated their partner's listening quality similarly to those interacting with a fully attentive partner (see Figure 5).

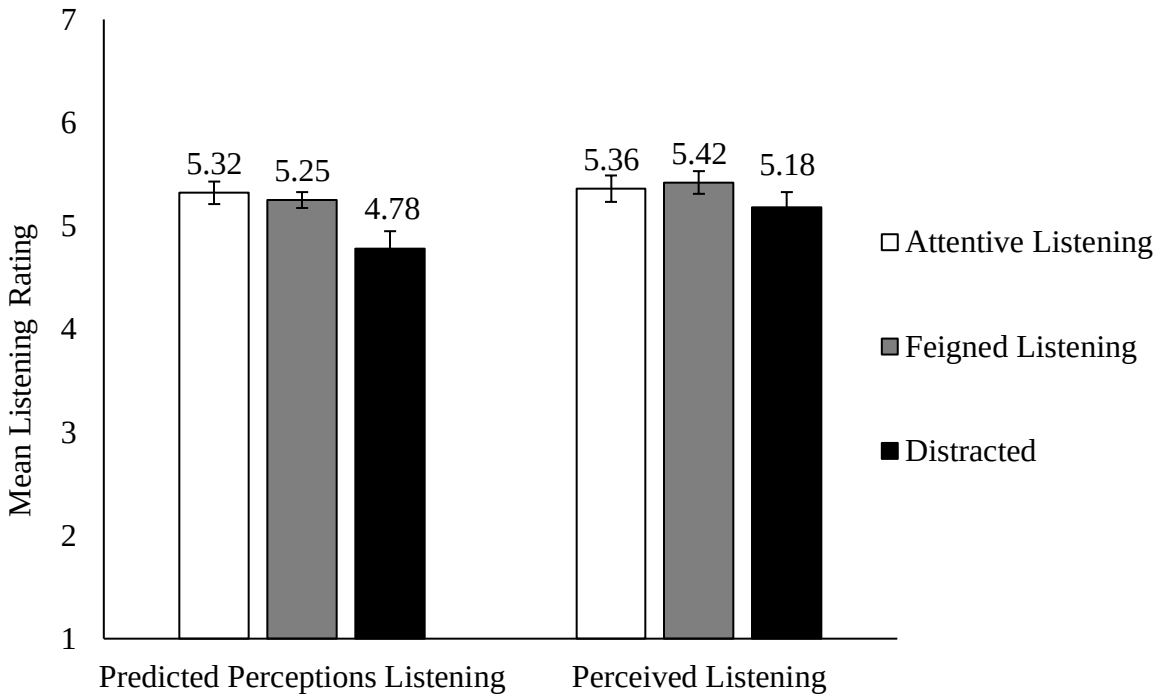

Figure 5. "Perceived Listening" bars show ratings of listening by the unmanipulated partners, while "Predicted Perceptions of Listening" bars show how manipulated partners believed they would be rated by their naïve partners (Experiment 1). Though participants in the distracted condition predicted that their partner would rate them as a poorer listener, there were no differences in perceived listening ratings across conditions. Error bars represent standard errors.

Similarly, there were no significant differences between the three conditions in ratings of warmth, competence, or status (see Table 3). However, conversation partners in the *Feigned Listening* condition reported marginally greater enjoyment of the conversation than partners in the *Listening* condition (with no differences between the *Distracted* condition and the *Listening* or *Feigned Listening* conditions; see Table 3). One possible explanation for this result is that listeners incentivized to "fake it" may have been more emphatic in their attempts to convey their listening.

Table 3. Evaluations of Listener Warmth, Competence, Power, and Conversational Enjoyment Across Conditions.

	Omnibus F-Value	***p***	**Attentive Listening** ***M (SE)***	**Feigned Listening** ***M (SE)***	**Distracted** ***M (SE)***
Warmth	0.37	.69	5.22_{a} (0.11)	5.20_{a} (0.11)	5.10_{a} (0.11)
Competence	0.12	.89	4.83_{a} (0.09)	4.88_{a} (0.09)	4.88_{a} (0.09)
Power	0.69	.50	3.50_{a} (0.14)	3.54_{a} (0.14)	3.71_{a} (0.14)
Enjoyment	2.56	.09	4.91_{a} (0.12)	5.28_{b} (0.12)	5.08_{ab} (0.12)

Note: Means in each row with different subscripts were significantly different at the $p < .01$ level. For example, a mean with subscript 'a' differs from a mean with subscript 'b'.

Experiment 1 Discussion

The results of Experiment 1 suggest that people's ability to detect others' listening is limited, providing further evidence in support of Hypothesis 1. While some partners listened attentively, others pretended to listen, and still others focused on an external stimulus, their counterparts did not discern these differences. These results also offer additional evidence in support of Hypothesis 2: the error appeared to be driven by the over-attribution of attentive listening—distracted and feigned listeners were rated similarly to those who were attentively listening (with all condition-level means above 5 on a 1-7 scale). We believe that the lack of difference in perceptions between distracted and feigned listeners was driven by the social desirability of appearing like a good listener, leading participants in the *Distracted* condition to feign listening to hide their inattentiveness, even without explicit experimental instructions to do so. While it may also be possible that manipulated partners were able to effectively divide their attention between multiple stimuli, in which case their attentiveness may not have differed across the three conditions, previous research shows that directing cognitive attention toward an additional stimulus interferes with the attention paid to the original stimulus (e.g., Cohen & Gordon-Salant, 2017; McKnight & McKnight, 1993; Patten et al., 2014). Thus, we do not

believe that this explains our results. However, we address this alternative explanation further in the design of Experiment 4.

Applying a lens model approach (Brunswik, 1952), we propose that the inaccuracies in interpersonal perception found in our Exploratory Study and Experiment 1 could stem from two possible explanations: (1) people rely on invalid cues when judging listening, and (2) listeners convey limited or invalid cues of their listening. The former explanation would suggest that people who are listening behave differently than those who are not, but perceivers are relying on the wrong cues when making their judgments. Conversely, the latter explanation suggests that observable behavioral differences between those who are listening and those who are not are limited (or absent). We examine these two explanations in Experiment 2.

Experiment 2

In Experiment 2, we investigated behavioral differences between people who are attentively listening and those who are not. In doing so, we aim to clarify why perceptions of conversational listening do not align with actual listening: do targets fail to convey cues of inattentive and/or attentive listening? Or are perceivers simply missing diagnostic, observable cues? In Experiment 2, we ask third-party observers to code the behavior of the listeners (i.e., targets) from Experiment 1 and assess their listening throughout the conversation (by guessing their assigned condition, and separately, by coding potential behavioral cues of attentive and inattentive listening). This design also allowed us to test whether removing the cognitive demands of live conversation might improve peoples' ability to accurately detect listening quality.

Experiment 2 Method

Participants

A total of 650 participants were recruited through Amazon's Mechanical Turk (mTurk) to complete a 15-minute survey about conversations in exchange for $1.20. After data exclusions due to technical difficulties and failed attention checks,[7] we obtained data from 398 participants. Participants reported their gender ("What is your gender? Male, Female, Non-binary/Other; 62% Male, 38% Female) and their age ("How old are you (in years)?" [Open-ended numeric entry]; M_{age} = 35 years, SD_{age} = 10 years).

Procedure

When we recorded the interaction in Experiment 1, we positioned the camera facing the "manipulated" partner in each conversation, offering the viewer an unobstructed frontal view of this participant. In Experiment 2, participants ("observers") watched and evaluated the videos of the conversations we collected in Experiment 1. Specifically, they watched a recording of one of the interactions and were asked to report the extent to which the participant in the video engaged in several behaviors. To gather fine-grained coding of the videos, we asked observers to watch each video divided into 1-minute segments. After each 1-minute segment, the video paused, and the observer reported how frequently the individual in the video engaged in various behaviors. Then the video would resume playing and pause again after another minute. This process repeated until the video ended. Overall, due to random assignment and our exclusions, 10 videos were rated by a single observer, 24 were rated by two observers, 129 were rated by three observers, and 224 videos were rated by four observers.

[7] We excluded data from 252 participants (19 reported they would be unable to watch a video as part of the study; 61 participants failed an attention check at the start of the survey; 61 participants reported they would be unable to listen to audio during the study; 104 participants failed our second attention check which occurred in the middle of the video coding task; 7 participants failed to complete the full survey).

Measures

Based on prior listening theory (e.g., Bodie et al., 2012) and the open-ended responses of participants from a pilot study[8], we generated a list of verbal and non-verbal behaviors that social scientists and laypeople have suggested might represent valid cues of good and bad listening. For each video segment, observers rated the extent to which the manipulated conversation partner engaged in each of those behaviors (detailed below) on a scale from 1: "Not at all" to 4: "A lot."

Specifically, observers were asked to report the extent to which the manipulated conversation partner deployed verbal cues—*verbal interruptions* (cutting the partner off, talking over the partner, etc.) and *verbal affirmations* (defined as 'mhmm', 'uh-huh', 'yes', etc.)—and nonverbal cues: *nodding*, *eye contact*, *looking away* (behind the partner's head, up, down, or to the side), *smiling*, *fidgeting*, *leaning forward*, and *leaning backward*.

Predicting Listener Condition. After watching a full video, observers received an explanation of the lab paradigm that we used to collect the video—including an explanation of the experimental manipulation. We asked observers to guess which condition the manipulated conversation partner had been assigned to: attentive, distracted, or feigned listening.

Own and Partner Perceptions. Observers also reported whether they thought the individual was a good listener, and whether they were likable using the same items as in Experiment 1. Further, we asked observers to guess the evaluations that the Experiment 1 conversation partners provided of the manipulated participant in the video.

[8] In this pilot study, a sample of participants (N = 829) were asked what they look for when they are trying to detect whether someone is "listening attentively to you, pretending to listen, or tuning out altogether." Participants listed "3 qualities, behaviors, and/or tendencies" using an open-ended response format. These responses were read by the authors to derive a list of behaviors commonly thought to signal listening.

Experiment 2 Results

In the analyses that follow, we assess differences in observers' evaluations of the target individuals across conditions. To do so, we used mixed effects models with a fixed-effect for the condition and a random-effect for the dyad number to account for multiple ratings of each conversation.

Behavioral Coding

Conducting Tukey's test for post-hoc comparisons, we find that target participants (our manipulated participants from Experiment 1) were more likely to interrupt their partner when they were in the *Feigned Listening* compared to the *Listening* condition (b = 0.2, *95% CI* [0.04, 0.49], p = .02), and marginally more than in the *Distracted* condition (b = 0.20, *95% CI* [-0.02, 0.43], p = .09; see Table 4). However, beyond this behavioral difference, observers did not detect any significant differences across the three conditions in terms of the frequency of other verbal or non-verbal behaviors that both lay people and psychologists consider to be pervasive, observable, and diagnostic cues of attentive versus inattentive listening (see Table 4). In other words, conversation partners who were instructed to listen attentively were no more likely to nod, affirm, lean forward, lean backward, maintain eye contact, or look away than those explicitly assigned to direct their attention elsewhere.

Table 4. Frequency of Various Behavioral Listening Cues Across Conditions.

	Omnibus F-Value	Attentive Listening *M (SE)*	Feigned Listening *M (SE)*	Distracted *M (SE)*
Behavior				
Verbal interruptions	4.33*	1.72_{a} (0.07)	1.99_{b} (0.07)	1.78_{ab} (0.07)
Verbal affirmations	1.12	3.00_{a} (0.06)	2.87_{a} (0.06)	2.91_{a} (0.07)
Nodding	2.17	3.05_{a} (0.07)	2.85_{a} (0.07)	2.92_{a} (0.07)
Eye Contact	0.03	3.43_{a} (0.06)	3.45_{a} (0.06)	3.45_{a} (0.06)
Looking Away	0.76	2.01_{a} (0.07)	2.09_{a} (0.08)	2.14_{a} (0.08)
Smiling	1.94	3.05_{a} (0.08)	3.01_{a} (0.08)	3.21_{a} (0.08)
Fidgeting	1.54	2.58_{a} (0.08)	2.72_{a} (0.09)	2.51_{a} (0.09)
Leaning Forward	0.65	1.89_{a} (0.09)	2.03_{a} (0.09)	1.91_{a} (0.10)
Leaning backwards	1.42	1.60_{a} (0.08)	1.77_{a} (0.08)	1.61_{a} (0.08)
Predicted Partner Perceptions				
Listening	0.23	5.12_{a} (0.08)	5.06_{a} (0.08)	5.12_{a} (0.08)
Liking	0.09	4.82_{a} (0.08)	4.78_{a} (0.08)	4.80_{a} (0.08)
Enjoyment	0.27	4.96_{a} (0.08)	4.88_{a} (0.08)	4.95_{a} (0.08)
Own Perceptions				
Listening	0.76	4.91_{a} (0.09)	4.75_{a} (0.09)	4.81_{a} (0.09)
Liking	1.43	4.83_{a} (0.08)	4.77_{a} (0.08)	4.64_{a} (0.08)

Note: Omnibus F-values for condition effects, $*p < .05$, $**p < .01$, $***p < .001$. Means in each row with different subscripts were significantly different at the $p < .05$ level. For example, a mean with subscript 'a' differs from a mean with subscript 'b'.

Condition Guess

Next, we parsed the data based on methods from signal detection theory, where the signal was the target's listening (signal is considered present in the *Listening* condition; signal is considered absent in the *Distracted* and *Feigned Listening* conditions; see Table 5). When the signal was present (i.e., the target was in the *Listening* condition), observers recognized this to be the case 80% of the time. However, when the signal was absent (i.e., the target was in either the *Distracted* or *Feigned Listening* conditions), observers still incorrectly believed they were in the *Listening* condition 74% of the time. As in Experiment 1, these results show a strong bias toward reporting that the target was listening, regardless of whether this was actually the case. In other

words, observers were insensitive to actual variations in listening, such that they believed *distracted* or *feigned listening* to be attentive listening in three-quarters of cases.

Table 5. Signal Detection Analysis of Experiment 2 Results

	Observer guessed target *not listening*	**Observer guessed target *listening***
Target *not listening*	26% CORRECT	74% TYPE I ERROR
Target *listening*	20% TYPE II ERROR	80% CORRECT

Our inferential analyses are in line with the descriptive pattern. Overall, 37% of observers correctly guessed the listener's condition assignment, but accuracy differed by video condition. Using a binary logistic regression mixed-effects model, with Tukey-method for pairwise comparisons, we found that observers were significantly less likely to correctly guess the condition (0/1) when the target was in the *Distracted* (14% correct; b = -3.16, *95% CI* [-3.96, -2.36], SE = 0.34, $p < .001$) or *Feigned Listening* conditions (15% correct; b = -3.13, 95% *CI* [-3.92, -2.34], SE = 0.34, $p < .001$) compared to the *Listening* condition (80% correct). We found no differences between the *Distracted* and *Feigned Listening* conditions (b = 0.03, 95% *CI* [-0.83, 0.89], p = .997). This difference was driven by an omnibus tendency for observers to over-ascribe listening irrespective of its actual presence: 76% of observers guessed that the target individual in the video was in the *Listening* condition.

Own and Partner Perceptions

Observer evaluations of the target's listening and likability did not differ significantly between conditions. Similarly, there were no differences in observers' predictions of how the

manipulated conversationalist was perceived by their partner during the live conversation (see Table 5).

Finally, we linked the perceptions of third-party observers to those made by targets' original conversation partner from Experiment 1. There was no relationship between perceptions of listening as rated by third-party observers and by conversation partners from Experiment 1, $b = 0.17$, *95% CI* [-0.13, 0.47], $SE = 0.15$, $p = .26$. Thus, there was little to no consensus on the targets' level of listening between conversation partners and observers. Combined with the overall lack of behavioral differences between attentive and inattentive listeners, this result suggests that there was no clear signal of listening to be detected.

Experiment 2 Discussion

The results of Experiment 2 were consistent with those of our Exploratory Study and Experiment 1: even when people did not bear the cognitive load of active conversation (but simply observed others conversing), they were unable to detect whether conversation partners were listening or merely pretending to. As before, third-party observers showed something like the "truth bias" (Bond & DePaulo, 2006; Levine et al., 1999; Vrij, 2008) such that they believed a target was attentively listening even when that individual was incentivized to direct their attention elsewhere.

Behavioral coding revealed one observable behavioral difference between conditions: participants in the *Feigned Listening* condition were more likely to verbally interrupt their partner. It may be the case that because these participants were attending to the videos, and *pretending* to attentively listen to their partner, their spoken participation in the conversation was poorly timed. Prior work suggests that even minor disruptions or distractions can disrupt conversational flow and turn-taking (e.g., Boland et al., 2022; Truong et al., 2020), so it is no

wonder that attending to a simultaneous distractor stimulus changed interruptive conversational behavior. What may be more surprising is that targets' observable behavior did not show differences on any other verbal or non-verbal cues.

These results clarify our understanding of Experiment 1. Namely, it does not appear that the unmanipulated partners simply missed obvious cues of listening because they were too cognitively taxed with participating in the conversation. Rather, drawing on the lens model (Brunswik, 1952), it appears that inaccuracies in listening perception are, at least in part, due to a dearth of diagnostic observable cues of attentive listening (Hypothesis 2).

Thus far, our results suggest that people have a strong baseline assumption that counterparts are listening, and that inaccuracies in listening detection are largely driven by the over-attribution of listening when it is not taking place. In Experiment 3, we investigate whether correcting this baseline assumption improves accuracy in listening detection. Namely, we examine whether people can accurately detect listening even when they have perfect insight into the mind of the listener—when they know *exactly* how much time the speaker spent listening (and not).

Experiment 3

We have found that people commonly believe a conversation partner is listening when they aren't—this was true for individuals actively engaged in a conversation as well as dispassionate, cognitively unencumbered third-party observers. Across these studies, however, participants assessed someone else's listening—someone whose mind they could not access. Thus, these results may represent a strong baseline assumption about attentiveness (on average) which may lead participants to err purely because they don't have insight into the *true* amount of attentive listening that has occurred. If participants were aware of the amount of inattentive

listening their partner engaged in overall, would they be able to detect the rise and fall of it with greater accuracy? In Experiment 3, we test whether people can accurately detect listening when they are perfectly aware of the underlying base rate of attentiveness, by asking them to diagnose *their own* listening after the fact.

Method

Participants

130 individuals were recruited from the participant pool at a university in the northeastern United States consisting of university students, staff, and members of the local community. Participants came to the behavioral lab to participate in a 30-minute study about conversation for which they were paid a flat rate of $10, with the potential to earn bonus payments of up to $21. Data from a total of 40 participants were excluded,[9] leaving a final sample of data from 90 participants. Participants reported their gender ("What is your gender?"; 51% Male, 49% Female), their age ("What is your age (in years)? [Open-ended numeric entry]; M_{age} = 37 years, SD_{age} = 16 years), their ethnicity ("Which choice most accurately describes your ethnicity?" 52% White, 9% Black or African American, 1% American Indian or Alaska Native, 21% Asian, 17% Other), and their employment status ("What statement best describes your current employment situation?" 40% Working as paid employee, 17% Self-employed, 32% Student, 11% Other).

Procedure

[9] 16 participants experienced technological issues (e.g., the videos didn't record, or the song did not play properly through the speakers); 14 participants did not follow the instructions (e.g., telling the researcher they didn't realize they had to listen to the story or song when they were explicitly instructed to do so); 10 participants compromised the experimental procedure so that they could more easily win the bonus (e.g., wearing a hat or sunglasses during one part of the procedure but not during the other).

In this study, we used a within-subjects design. Participants were seated in a room with an experimenter, who read two stories out loud to each participant, with story order counterbalanced, while music was playing in the room. We selected two songs ("I Am My Own Grandpa," and "Big Rock Candy Mountain") that were obscure enough that participants were unlikely to be familiar with them, but used plain language that would be easy to understand and remember. Participants received different instructions about how to direct their attention and behave with respect to the story and the background music while each story was read, according to the experimental condition to which they were assigned. Specifically, all participants were told to listen attentively to one story and were randomly assigned to one of two conditions for the other story (described below; order randomized). Participants were video recorded as they listened. To ensure this procedure was similar to the social interaction procedures in our other studies, we told the participant that the experimenter (who was reading the story to them while sitting a few feet away in the same room) would be evaluating their listening quality. Additionally, while reading the story, we instructed the experimenter to look up and make eye contact with the participant at regular intervals to ensure that the experience felt interactive. These elements of the design were a purposeful effort to introduce the real-world social pressures of being perceived as a good listener.

Attentive Listening Instructions. During one of the two stories that the experimenter read to the participant during the study, participants were asked to "listen as attentively as possible to the story." They were told that they would answer comprehension questions about the story and would receive a $1.00 bonus for each question they answered correctly.

Inattentive-Listening Instructions. During the other story (counterbalanced), all participants were incentivized to experience one of two levels of cognitive distraction, which we

manipulated between participants. In the *Inattentive-Listening* condition (n = 45), participants were asked to "listen as attentively as possible to the song playing in the room" and told that they would receive a $1.00 bonus for each line of lyrics that they correctly recalled. In the *Semi-Attentive Listening* condition (n = 45), participants were asked to "listen as attentively as possible to the story AND the song playing in the room" and that they would receive a $1.00 bonus for each comprehension question they answered about the story *and* each line of song lyrics they correctly recalled.

Manipulation Check

Participants completed seven comprehension questions about the story their partner read to them, as well as seven fill-in-the-blank questions about the lyrics of the song playing in the room. Further, after completing each listening task, participants reported how "attentively" they listened to the story, and how "distracted" they were while their partner was reading the story (1: "*Not at all"*; 5: "*Extremely"*). This new measure addresses a limitation in the design of Experiment 2: we did not have a measure of how much of the conversation participants could recall of the conversation, but instead focused on their recall of the commercials (i.e., the distractor task). Thus, in our Experiment 3 design, we include recall measures for both the interpersonal listening content (story) as well as the distractor task (song).

Detecting their Own Listening

After participants finished the listening task, they were shown ten separate 5-second, muted video clips of themselves listening (five clips from each story), a methodology commonly used in "thin-slice" research (e.g., Ambady et al., 2000; Ambady & Rosenthal, 1992). For each video clip, participants were asked to guess which set of instructions they were following during the recorded time. We then asked participants "How confident are you in your answer?" (1 = Not

at all confident; 7 = Extremely confident). Finally, participants guessed the number of clips (of the ten) for which they thought they had correctly identified the condition assignment.

Results

Manipulation Check

Our listening manipulation appears to have been successful: Participants in both the *Inattentive* and *Semi-Attentive Listening* conditions reported listening more attentively to the story when they were instructed to do so ($M_{Inattentive}$ = 5.69, $SD_{Inattentive}$ = 1.12; $M_{Semi\_Attentive}$ = 5.91, $SD_{Semi\_Attentive}$ = 1.00) than when they were instructed to listen to the song ($M_{Inattentive}$ = 3.18, $SD_{Inattentive}$ = 1.85; $M_{Semi\_Attentive}$ = 5.16, $SD_{Semi\_Attentive}$ = 1.28; *Inattentive*: $t(44) = 9.03$, $p < .001$, *Cohen's d* = 1.60, 95% *CI* [1.07, 2.14]; *Semi-Attentive*: $t(44) = 4.19$, $p < .001$, *Cohen's d* = 0.65, 95% *CI* [0.31, 0.99]). Additionally, participants in the *Inattentive* and *Semi-Attentive Listening* conditions reported feeling more distracted from the story when they were instructed to listen to the song ($M_{Inattentive}$ = 4.73, $SD_{Inattentive}$ = 1.74; $M_{Semi\_Attentive}$ = 4.87, $SD_{Semi\_Attentive}$ = 1.70) than when they were instructed to listen to the story ($M_{Inattentive}$ = 3.67, $SD_{Inattentive}$ = 1.49; $M_{Semi\_Attentive}$ = 3.91, $SD_{Semi\_Attentive}$ = 1.87; *Inattentive*: $t(44) = 4.28$, $p < .001$, *Cohen's d* = 0.66, 95% *CI* [0.32, 0.99]; *Semi-Attentive*: $t(44) = 4.17$, $p < .001$), *Cohen's d* = 0.53, 95% *CI* [0.26, 0.80]).

When we examine the responses to the story comprehension questions, we see that participants in the *Inattentive Listening* condition answered more questions correctly when they were asked to listen to the story (M = 3.78, SD = 1.61) than when they were asked to listen to the song (M = 2.60, SD = 1.37, $t(44) = 3.70$, $p < .001$, *Cohen's d* = 0.79, 95% *CI* [0.30, 1.27]). However, those in the *Semi-Attentive Listening* condition answered a similar number of story comprehension questions correctly when they were asked to listen to the story (M = 3.56, SD =

1.37) as when they were asked to listen to both the story *and* the song (M = 3.04, SD = 1.43, $t(44) = 1.70$, $p = 0.10$, *Cohen's d* = 0.36, 95% *CI* [-0.08, 0.80]).

Further, participants in both the *Inattentive* and *Semi-Attentive Listening* conditions recalled more song lyrics correctly when they were asked to listen to the song ($M_{Inattentive}$ = 2.11, $SD_{Inattentive}$ = 1.70; $M_{Semi\_Attentive}$ = 1.31, $SD_{Semi\_Attentive}$ = 1.41) than when they were asked to listen to the story ($M_{Inattentive}$ = 0.33, $SD_{Inattentive}$ = 0.56; $M_{Semi\_Attentive}$ = 0.69, $SD_{Semi\_Attentive}$ = 1.00; *Inattentive:* $t(44) = 6.74$, $p < .001$, *Cohen's d* = 1.40, 95% *CI* [0.82, 1.98]; *Semi-Attentive:* $t(44) = 2.85$, $p = .007$, *Cohen's d* = 0.50, 95% *CI* [0.13, 0.88]).

In sum, these results suggest that our manipulations were effective. Both self-report and behavioral measures show that participants paid less attention to the story when instructed to do so.

Signal Detection of Attentive Listening

To test our key hypotheses, we first examined participants' accuracy in detecting their own level of listening, leveraging both their perception of their own behavioral cues and their recall of how much they were actually listening during the experimental task.

Overall, participants correctly guessed their listening on 64% of trials (31% when listening attentively to the story; 33% when listening inattentively to the story). Thus, in over one third of trials (36%), participants did not correctly guess their own listening in a task they completed minutes prior (19% when listening attentively to the story; 17% when inattentively listening to the story).[10] As before, we draw from signal detection theory to investigate the direction of this error—treating attentive listening (whether the participant had been instructed to

[10] Accuracy rates were slightly higher for participants in the *Inattentive* (69% correct overall; 34% when listening attentively to the story; 35% when inattentively listening to the story) than the *Semi-Attentive Listening* (58% correct overall; 28% when listening attentively to the story; 30% when inattentively listening to the story) condition.

attend to the story) as the signal (see Table 6; Stanislaw & Todorov, 1999). Here we see that when the signal was present (i.e., the participant had been instructed to listen to the story), participants judged it to be so in 61% of trials (66% *Inattentive* condition; 56% *Semi-Attentive* condition). However, when the signal was absent (i.e., the participant had been instructed to listen to the song), participants incorrectly believed they were listening attentively in 49% of trials (49% *Inattentive* condition; 48% *Semi-Attentive* condition). Unlike in Studies 1 and 2, we did not observe a bias for participants to report that they were listening—having completed the listening task just minutes prior, participants appeared to know exactly how much they were (and were not) listening throughout the task. However, this did not increase their accuracy in detecting their own listening—in fact, hit rates decreased in this study compared to the earlier studies, with participants correctly guessing they were listening in less than two-thirds of trials. Thus, even when we corrected participants' baseline over-assumption of listening, they were still relatively unimpressive in their ability to decipher the rise and fall of attentive listening at specific times during the interaction.

Table 6. Signal Detection Analysis of Experiment 3 Results

	Clip identified as *inattentive or semi-attentive listening*	**Clip identified as *attentive listening***
***Inattentive Listening* Condition**		
Inattentive listening trial	52% CORRECT	48% TYPE I ERROR
Attentive-listening trial	34% TYPE II ERROR	66% CORRECT
***Semi-Attentive Listening* Condition**		
Semi-attentive listening trial	51% CORRECT	49% TYPE I ERROR
Attentive-listening trial	44% TYPE II ERROR	56% CORRECT

Perceptual Accuracy in Listening Detection

Moving beyond descriptive measures of accuracy, we investigate whether participants were more likely to guess they were listening on trials in which they were instructed to listen attentively to the story. Thus, we conducted a binary logistic regression predicting participant guesses of listening (values recoded such that 0 = *Guess not listening*, 1 = *Guess listening*) from a fixed-effect for trial type (whether the participant had been instructed to listen attentively to the story) and trial number (one out of ten guesses), and a random effect for participant to account for repeated observations (Bates et al., 2015). We found a positive association between trial type and participant guess of listening, $b = 0.57$, *95% CI* [0.19, 0.95], $p = .003$, and no effect of trial number, $b = 0.008$, *95% CI* [-0.06, 0.07], $p = .80$.[11] Translating this result into an odds ratio, participants were 1.77 times more likely to guess they were listening attentively when they had

[11] We find no interaction between trial type and condition (*Inattentive* vs. *Semi-Attentive*), $b = -0.44$, *95% CI* [-0.99, 0.10], $p = .11$.

received instructions to do so than when they had not. Although participants were significantly more likely to guess that they were listening during a given clip when they were actually listening, the results are not encouraging regarding the extent of participants' discernment—they correctly guessed their listening on only 61% of trials.

Confidence in Listening Perceptions

Next, we assess participants' self-reported confidence in their guesses. We conducted a mixed-effects regression predicting participant confidence ratings in their guesses from a fixed-effect for whether they correctly guessed their listening (0 = incorrect guess; 1 = correct guess) and trial number, and a random effect for participant to account for repeated observations (Bates et al., 2015). We found a positive relationship between guess correctness and confidence, b = 0.30, *95% CI* [0.14, 0.46], $p < .001$, and a positive relationship between trial number and guess confidence, $b = 0.05$, *95% CI* [0.03, 0.08], $p < .001$. On average, participants reported that they thought they had guessed approximately six clips correctly ($SD = 2.10$). Further, there was a significant positive relationship between the number of clips that participants thought they had correctly identified and the number of clips they had actually correctly identified, $b = 0.46$, *95% CI* [0.25, 0.67], $p < .001$. Overall, 39% of participants thought they correctly identified more clips than was the case (44% underestimated; 17% correctly estimated). These results suggest that participants had some insight into the limitations of their listening detection accuracy, perhaps because they knew their base rate of inattention and recognized that they couldn't tell by watching when those moments occurred.

Experiment 3 Discussion

The results of Experiment 3 suggest that people do not detect listening with high accuracy, even when they observe their *own* nonverbal cues immediately after engaging in

attentive, inattentive, or semi-attentive listening. In particular, mirroring the results of Experiments 1-2, people overestimated (their own) attentive listening, even just minutes after experiencing the rise and fall of their attentiveness firsthand during an interaction.

One interpretation of the results of Experiment 3 is that when people have an accurate baseline for the amount of attentive vs. inattentive listening—in this case, because they have just engaged in the task themselves—they still do not achieve anywhere near perfect accuracy in listening detection. Participants guessed incorrectly on 36% of the trials. Thus, even with insight into the mind of the listener, listening detection is still poor, likely because listeners do not give off high-fidelity, observable cues of their attentive versus inattentive minds (as was found in Experiment 2).

As at a dinner party or work meeting, our participants were under pressure to seem attentive to the experimenter sharing a story. The pressure to appear attentive and interested even when other tasks draw one's attention away is shared by most psychology experiments and real social settings alike.

Still, it remains unclear whether people were able to effectively divide their attention (i.e., multi-task): perhaps distracted listeners were still listening to their partners to a substantial extent. In our final study, we disentangle multi-tasking from feigned attentive listening by limiting listeners' physical ability to hear (rendering multi-tasking impossible).

Experiment 4

In Experiments 1 and 3, we guided participants' listening behavior with instructions and incentives (to listen attentively, inattentively, or semi-attentively). However, it's possible that the human mind is highly capable of dividing its attention between multiple stimuli, including live conversation, and that all participants in Experiments 1 and 3—even those whose attention was

divided—were able to process their partner's words and respond accordingly (even while simultaneously attending to video advertisements or music lyrics).

In Experiment 4, we tested this explanation by strictly limiting participants' *ability to hear* their conversation partner's words—by garbling portions of the conversation. At the same time, we incentivized "listeners" to act as if the conversation was proceeding without disruption. Specifically, we told listeners that their payment depended on maintaining their partner's ignorance about the sporadically garbled content. Although individuals in the real world are not explicitly financially incentivized to appear like good listeners, we hoped to emulate the social incentives that often lead people to feign attentive listening.

While we define listening as attending to and processing another person's verbal, nonverbal, and prosodic cues during conversation, we chose to limit targets' access to their partner's verbal cues in this study, rather than nonverbal or prosodic cues. The exchange of verbal content (words) between two or more people is what defines conversation (Yeomans et al., 2021). By limiting verbal content, this study provides a stringent test of listening perception, while still allowing participants to carry on a responsive, live interaction. Though people are well-practiced in conversing with limited or no access to their partner's prosodic or nonverbal cues (e.g., via text-based media like email or text messaging), conversing without verbal content presents a more stringent test of the ability to feign attentive listening while maintaining a responsive interaction.

Between participants, we varied how much of the conversation was garbled. This design helped us answer two important questions. First, to what extent are our earlier results due to the fact that people are excellent at dividing their attention between listening and a distractor task? Secondly, how pervasive can lapses in hearing and listening become before conversation partners

begin to notice them? As previously noted, recall (which was used in the previous studies) is an imperfect measure of listening (Thomas & Levine, 1994). We circumvent this challenge in Experiment 4 by limiting auditory input itself. All procedures and analyses were pre-registered: https://aspredicted.org/blind.php?x=sf5nz6.

Experiment 4 Method

Participants

A total of 242 participants[12] were recruited from the participant pool at a university in the northeastern United States, consisting of university students, staff, and members of the local community. Participants came to our behavioral lab in groups of 4-6 to participate in a 60-minute study about conversation in which they would engage in a series of one-on-one conversations with the other participants in the session. Participants were paid a flat rate of $20 with the potential to earn up to $20 in additional bonus payments. After excluding data from conversations in which a participant expressed confusion about the instructions or disclosed the listening manipulation to their partner (n = 9 conversations), we analyzed data from 305 conversations (N = 235 unique participants; 66% engaged in three conversations, 27% engaged in two conversations, 7% engaged in a single conversation).

Participants reported their gender ("What is your gender? Male, Female, Non-binary/Other"; 44% Male, 52% Female, 1% Non-binary/Other, 3% Non-response) and their age ("What is your age?" [Open-ended numeric entry]; M_{age} = 32 years, SD_{age} = 14 years).

Protocol

Participants completed ten minute, one-on-one, round-robin video chats with 2-3 different partners. Conducting these conversations over video chat (instead of face-to-face)

[12] We were unable to reach our pre-registered sample size due to the onset of the COVID-19 pandemic. This study was actively running in the lab when local shelter-in-place restrictions required data collection to stop.

allowed us to asymmetrically manipulate audio for one participant in each dyad. The number of participants that arrived for each experimental session determined the number of conversations each participant completed (we maximized the number of unique round-robin dyads possible in each session). We instructed participants to "get to know their conversation partners," and provided them with a list of seven conversation topics that they could (but did not have to) use (e.g., Where did you grow up? What did you do last summer? Do you have any pets? Have you watched any good TV shows lately?). Participants also received private instructions: half of the participants in each session were randomly assigned to the role of *target*, the other half to the role of *perceiver*.

Target. Those participants assigned to the role of *target* learned that there may be times during the conversation when they might not understand their partner—instead of hearing their partner's words, they would hear garbled sounds, and this garbled sound was an *intentional* part of the study, not a technical glitch. By design, we used a computer program to obscure what the targets (but not the perceivers) could hear during the conversation. Unbeknownst to the targets, we randomly assigned them to one of four conditions corresponding to the amount of time the ten-minute conversation would be garbled from their perspective only: 0% (0 seconds garbled), 25% (150 seconds garbled), 50% (300 seconds garbled), or 75% (450 seconds garbled). We configured the computer program to intermittently turn on and off a voice filter that obscured what the targets heard from their partner at specified intervals (25% garbled condition: 30 seconds filter off, 10 seconds filter on; 50% garbled condition: 10 seconds filter off, 10 seconds filter on; 75% garbled condition: 3 seconds filter off; 10 seconds filter on), which repeated throughout the ten-minute conversation. Random assignment to the level of garbling (0%, 25%,

50%, 75%) was performed at the session level (n_0 = 80 conversations; n_{25} = 87 conversations; n_{50} = 70 conversations; n_{75} = 68 conversations).

Our manipulation allowed us to test whether people can give the impression that they are listening, even when they cannot actually hear the words that their partner is saying. The results of our earlier studies might be explained by the idea that targets who were rated as good listeners despite distractions were simply able to effectively divide their attention. However, this explanation cannot apply to the current study where participants could not hear parts of the conversation. If participants exposed to largely garbled speech manage to create the impression of good listening, we can conclude with greater confidence that feigned listening is very difficult to detect. Importantly, we incentivized targets to act as if nothing was amiss:

> *"Your primary goal is to be (or at least appear to be) a GREAT LISTENER. After each conversation, your partner will rate how well they think you listened to them. If your partner reports that you were a good listener (i.e., one of the 40 best listeners in the study)…then you will earn a $20 bonus…In order to earn the "good listener" bonus, your partner should think the conversation has played out naturally and smoothly."*

Perceivers. Those assigned as *perceivers* were completely ignorant of the targets' instructions and the conversational garbling. But for fairness in payment potential, the perceivers were also incentivized:

> *"Your primary goal is to be likeable. After each conversation, your partner will rate how much they liked you. If your partner reports that you were highly likeable (i.e., one of the 40 most likeable people in the study)…then you will earn a $20 bonus."*

In this manner, both targets and perceivers were financially incentivized to make a positive impression on each other.

After each ten-minute conversation, participants completed a post-conversation survey, which included self-reported items about the conversation and their partner. At the end of the lab session, participants completed a final demographic survey and received payment.

Measures completed by perceivers

After each conversation, the perceivers (who were blind to the manipulation) reported their perceptions of their (manipulated) counterpart's listening ("My partner was a good listener," "My partner was engaged in the conversation") and responsiveness ("My partner made me feel heard," "My partner made me feel validated," "I felt that my partner cared about me") on a scale from 1: "*Strongly Disagree"* to 7: "*Strongly Agree*." They also reported the extent to which they agreed or disagreed that their partner "worked hard to listen to me," "was attentive to what I was saying," and "understood what I was saying" (1: "*Strongly Disagree"*; 7: "*Strongly Agree"*). These evaluations represent our key dependent variables in this study.

Perceivers also estimated the percentage of the things they said during each conversation that they believe the target heard (1-100%), and their general assessments of their (manipulated) counterpart, including likeability ("My partner is likable," "I liked my partner," "I would enjoy spending time with my partner," "I disliked my partner (R)"), intelligence ("My partner is smart"), and interestingness ("My partner is interesting") on a scale from 1: "*Strongly Disagree"* to 7: "*Strongly Agree*."

Measures completed by targets

Targets made predictions about their partners' perceptions of them. Each target predicted their partner's perceptions of their listening ("My partner thought I was a good listener," and "My partner thought I was engaged in the conversation") and responsiveness ("My partner felt heard," "My partner felt validated," and "My partner felt that I cared about them") on a scale

from 1: "*Strongly Disagree"* to 7: "*Strongly Agree*." Targets also reported whether they "worked hard to listen to my partner," "was attentive to what my partner was saying," and "understood what my partner was saying" on a scale from 1: "*Strongly Disagree*" to 7: "*Strongly Agree*." Targets also reported whether they thought their partner would say that they could hear them (yes/no) and guessed "My partner would say that I could hear ___% of what they said" (1-100%). These measures served as manipulation checks.

Finally, targets predicted how likeable their partner would rate them ("My partner thinks I'm likable," "My partner liked me," "My partner would enjoy spending time with me," "My partner dislikes me (R)," 1: "*Strongly Disagree"* to 7: "*Strongly Agree"*).

We told all targets and perceivers that their ratings of each other would remain private to assuage impression management concerns.

Experiment 4 Results

Since each participant engaged in several conversations, we conducted mixed-model regressions, clustering at the participant level and controlling for order effects (Bates et al., 2015). We report specific results for each dependent variable, specifying the results of each pairwise comparison (conducting Tukey-tests for multiple comparisons), as well as the overall omnibus F-test. All results are presented in aggregate in Table 7.

Table 7. Perceiver and Target Ratings Across Conditions.

		Experimental Condition: Percentage of Garbled Conversational Content			
	Omnibus F-Value	**0%** ***M (SE)***	**25%** ***M (SE)***	**50%** ***M (SE)***	**75%** ***M (SE)***
Perceiver Ratings					
Listening	0.36	6.24_{a} (0.16)	6.17_{a} (0.15)	6.19_{a} (0.17)	6.00_{a} (0.17)
Responsiveness	1.95	6.00_{a} (0.18)	5.72_{a} (0.17)	5.99_{a} (0.19)	5.44_{a} (0.19)
Worked Hard	1.13	5.79_{a} (0.19)	5.96_{a} (0.17)	6.13_{a} (0.19)	5.66_{a} (0.20)
Attentive	0.90	6.18_{a} (0.17)	6.13_{a} (0.16)	6.24_{a} (0.18)	5.85_{a} (0.18)
Understood	3.88*	6.18_{a} (0.19)	6.12_{a} (0.17)	6.13_{a} (0.19)	5.39_{b} (0.19)
Guess % Heard	5.77**	88.40_{a} (2.49)	85.70_{a} (2.35)	81.90_{ab} (2.61)	74.20_{b} (2.63)
Liking	0.81	6.03_{a} (0.17)	6.06_{a} (0.16)	5.99_{a} (0.18)	5.71_{a} (0.18)
Intelligence	1.54	6.35_{a} (0.17)	6.16_{a} (0.16)	6.08_{a} (0.18)	5.83_{a} (0.18)
Interestingness	1.05	6.00_{a} (0.20)	5.94_{a} (0.19)	5.93_{a} (0.21)	5.54_{a} (0.21)
Target Ratings					
Predicted Listening	3.37*	6.22_{a} (0.16)	6.23_{a} (0.16)	5.94_{ab} (0.17)	5.57_{b} (0.17)
Predicted Responsiveness	3.02*	6.04_{a} (0.16)	6.02_{a} (0.15)	5.88_{ab} (0.17)	5.42_{b} (0.17)
Predicted Liking	4.65**	5.56_{a} (0.14)	6.18_{b} (0.14)	5.98_{ab} (0.15)	5.56_{a} (0.15)
Worked Hard	3.74*	5.94_{a} (0.19)	6.61_{ab} (0.18)	6.77_{b} (0.20)	6.63_{ab} (0.20)
Attentive	0.39	6.54_{a} (0.15)	6.64_{a} (0.14)	6.59_{a} (0.16)	6.42_{a} (0.16)
Understood	31.51***	6.59_{a} (0.24)	5.52_{b} (0.23)	4.64_{c} (0.25)	3.34_{d} (0.25)
Percent Heard	109.10***	94.20_{a} (2.64)	68.90_{b} (2.52)	50.40_{c} (2.80)	27.00_{d} (2.82)
Guess Partner % Heard	4.90**	78.70_{a} (9.57)	78.00_{ab} (5.66)	67.20_{a} (6.31)	50.50_{c} (5.35)

Note: Omnibus F-values for condition effects, *$p < .05$, **$p < .01$, ***$p < .001$. Means in each row with different subscripts were significantly different at the $p < .05$ level. For example, a mean with subscript 'a' differs from a mean with subscript 'b'.

Was the listening manipulation successful?

Several results suggest that our listening manipulation was successful. First, targets' reports of the percentage of the conversation that they heard decreased significantly with each increase in garbling (see Figure 6; 0 vs. 25%: b = -25.20, *95% CI* [-34.70, -15.73], $p < .001$; 25 vs. 50%: b = -18.60, *95% CI* [-28.40, -8.74], $p < .001$; 50 vs. 75%: b = -23.30, *95% CI* [-33.70, -12.99], $p < .001$). Further, targets' self-reported understanding during the conversation diminished with each increase in garbling (see Figure 7; 0 vs. 25%: b = -1.07, *95% CI* [-1.92, -

0.21], p = .008; 25 vs. 50%: b = -0.88, *95% CI* [-1.76, 0.001], p = .05; 50 vs. 75%: b = -1.30, *95% CI* [-2.23, -0.37], p = .002).

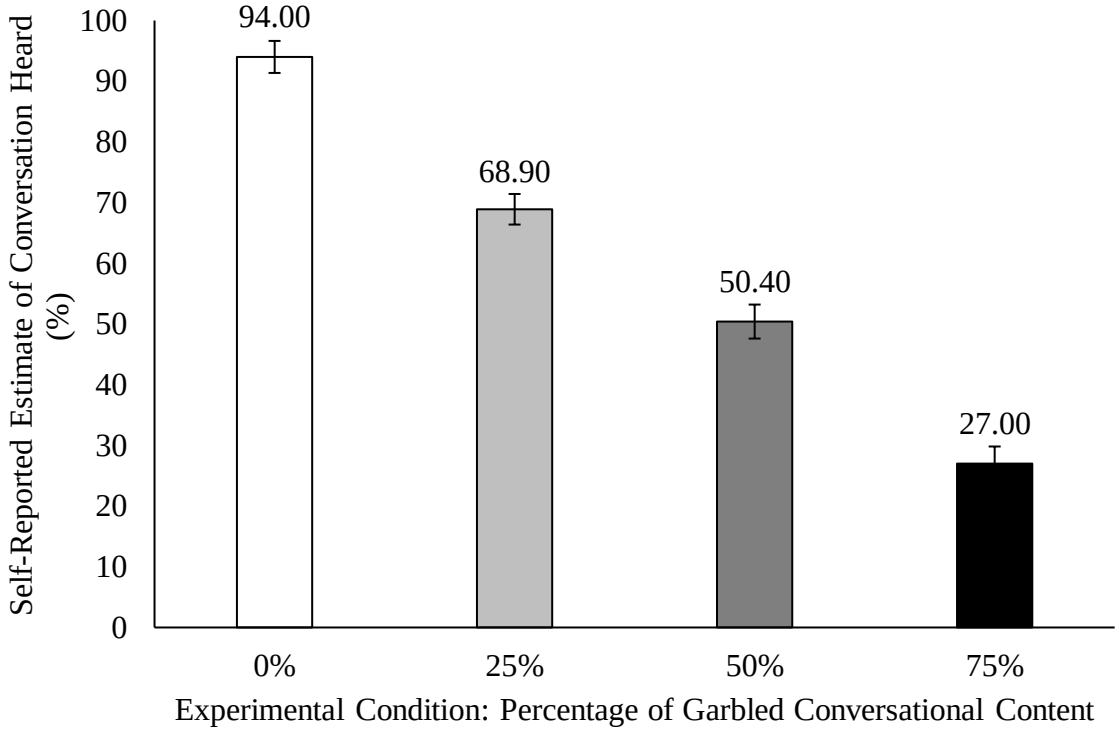

Figure 6. Targets estimated the percentage of conversational content they heard, with error bars representing standard errors (Experiment 4). Participants' self-reported estimates were quite accurate. Those who heard 100% of the content estimated they heard 94%, those who heard 75% estimated 69%, those who heard 50% estimated 50%, and those who heard 25% estimated 27%. Each condition significantly differed from all others.

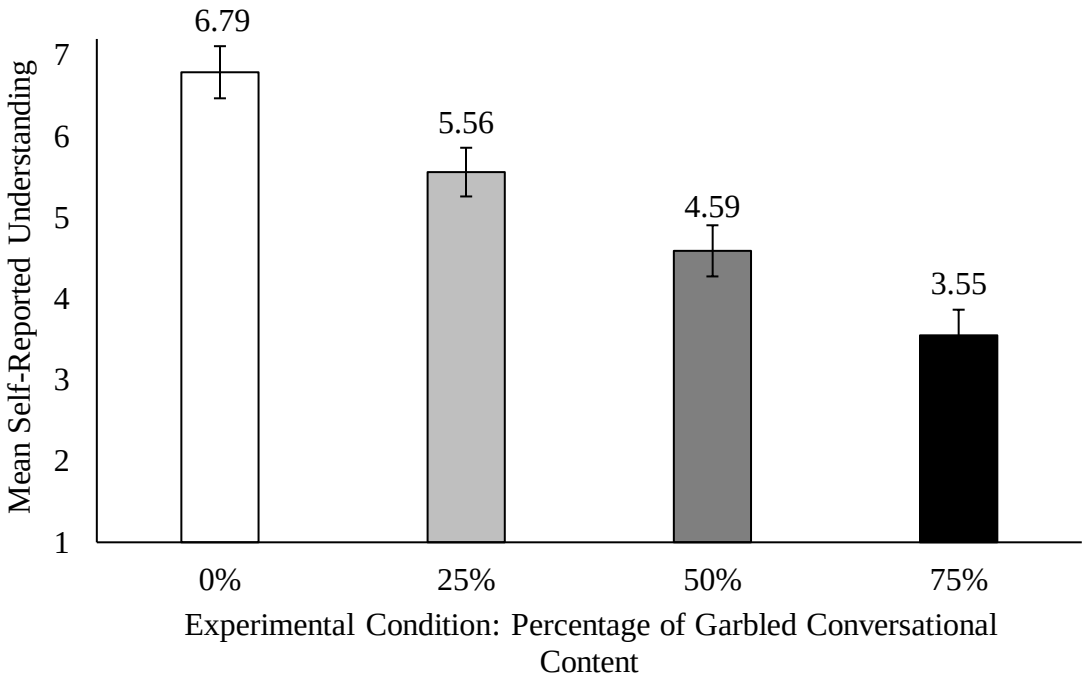

Figure 7. Targets' mean self-reported understanding during the conversation, with error bars representing standard errors (Experiment 4). Each condition significantly differed from all others.

Additionally, targets reported working harder to listen to their partner in the 50% compared to the 0% condition (0 vs. 50%: $b = 0.83$, *95% CI* [0.11, 1.55], $p = .003$)–and marginally harder in the 25% and 75% compared to the 0% condition (0 vs. 25%: $b = 0.67$, *95% CI* [-0.01, 1.35], $p = .06$; 0 vs. 75%: $b = 0.69$, *95% CI* [-0.03, 1.41], $p = .07$). Interestingly, there were no significant differences on this measure between the obscured conditions (25 vs. 50%: $b = 0.16$, *95% CI* [-0.55, 0.86], $p = .94$; 25 vs. 75%: $b = 0.02$, *95% CI* [-0.68, 0.73], $p = .99$; 50 vs. 75%: $b = -0.13$, *95% CI* [-0.88, 0.60], $p = .96$).

Importantly, targets reported no differences in their attentiveness during the conversation across the conditions, suggesting they were indeed incentivized to appear as though they were listening to their partner (0 vs. 25%: $b = 0.11$, *95% CI* [-0.42, 0.64], $p = .95$; 25 vs. 50%: $b =$ -

0.05, *95% CI* [-0.60, 0.50], $p = .99$; 50 vs. 75%: $b = -0.17$, *95% CI* [-0.75, 0.41], $p = .87$; all other ps > .72).

Did targets think they could feign listening?

Comparing targets' predictions of their partners' perceptions of their listening and responsiveness across conditions, we find that participants in the 75% garbled condition predicted that their partner would rate them as being poorer listeners than those in the 0% ($b = -0.65$, *95% CI*[-1.27, -0.02], $p = .04$) and 25% conditions ($b = -0.66$, *95% CI*[-1.27, -0.05], $p = .03$), and as less responsive than in the 0% ($b = -0.62$, *95% CI* [-1.23, -0.01], $p = .04$) and 25% conditions ($b = -0.60$, *95% CI* [-1.20, -0.005], $p = .047$), but show no differences from the 50% condition (listening: $b = -0.37$, *95% CI* [-1.01, 0.27], $p = .44$; responsiveness: $b = -0.45$, *95% CI* [-1.08, 0.17], $p = .24$). Additionally, when asked to predict how likable their partner would find them, targets in the 0% condition actually predicted their partner would find them less likable than those in the 25% condition ($b = 0.62$, *95% CI* [0.10, 1.13], $p = .01$), would be similarly likable to those in the 50% ($b = 0.42$, *95% CI* [-0.13, 0.96], $p = 0.20$) and 75% conditions ($b = 0.00007$, *95% CI* [-0.55, 0.55], $p > .99$). Further, those in the 75% condition also felt they would be seen as less likable than those in the 25% condition ($b = -0.62$, *95% CI* [-1.15, -0.08], $p = .02$), but no different than those in the 50% ($b = -0.42$, *95% CI* [-0.98, 0.15], $p = .22$) condition. Thus, targets did feel that the manipulation would affect their partners' impression of them.

Finally, when comparing targets' predictions of how much of the conversation their partner thought they heard, targets in the 75% condition reported that their partner would think they heard significantly less than in 25% condition ($b = -27.52$, *95% CI* [-48.70, -6.36], $p = .006$), and marginally less than in the 0% and 50% conditions (0 vs. 75%: $b = -28.20$, *95% CI* [-57.80, 1.41], $p = .07$; 50 vs. 75%: $b = -16.71$, *95% CI* [-39.10, 5.65], $p = .20$), with no other

between-condition differences reaching significance (*p*s > .58). These results suggest that, except for those whose conversations were 75% garbled, most targets felt they were able to convince their partner of their listening.

Did perceivers notice?

As in Experiments 1-3, we find no differences across conditions in (unmanipulated) perceivers' perceptions of the targets' (manipulated) listening (see Figure 8; 0 vs. 25%: b = -0.07, *95% CI* [-0.65, 0.52], p = .99; 0 vs. 50%: b = -0.04, *95% CI* [-0.66, 0.57], p = .99; 0 vs. 75%: b = -0.23, *95% CI* [-0.85, 0.39], p = .78; all other *p*s > .86).

Further, when reporting how responsive their partner was in the conversation, perceivers rated targets in the 0%, 25%, 50%, and 75% conditions as similarly responsive (0 vs. 25%: b = -0.28, *95% CI* [-0.94, 0.38], p = .69; 0 vs. 50%: b = -0.005, *95% CI* [-0.70, 0.69], p = .99; 0 vs. 75%: b = -0.56, *95% CI* [-1.27, 0.16], p = .18; all other *p*s >.69; see Figure 8).

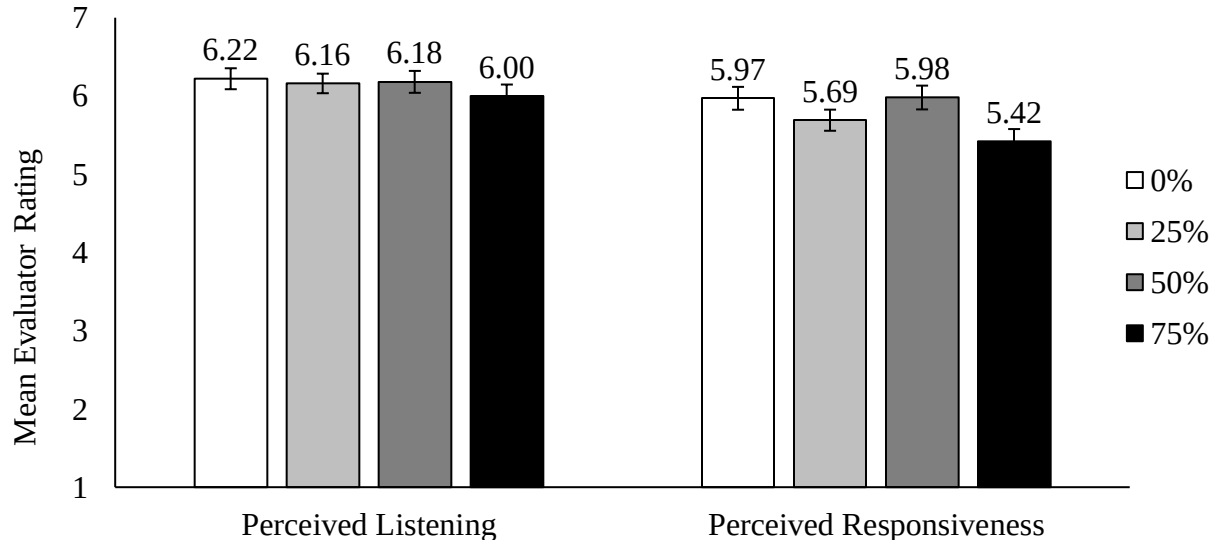

Figure 8. Perceivers' mean ratings of target listening and responsiveness across conditions, with error bars representing standard errors. There were no significant differences in perceived listening across conditions. Perceived responsiveness was significantly lower in the 75% garbled condition, compared to the 0% and 50% (but not 25% garbled) conditions.

Perceivers' ratings of targets' effort during the conversation also did not differ across conditions (0 vs. 25%: b = 0.17, *95% CI* [-0.49, 0.84], p = .91; 0 vs. 50%: b = 0.35, *95% CI* [-0.35, 1.05], p = .57; 0 vs. 75%: b = -0.13, *95% CI* [-0.83, 0.58], p = .97; all other ps > .09) and attentiveness (0 vs. 25%: b = -0.05, *95% CI* [-0.66, 0.56], p = .99; 0 vs. 50%: b = 0.06, *95% CI* [-0.58, 0.71], p = .99; 0 vs. 75%: b = -0.32, *95% CI* [-0.97, 0.33], p = .56; all other ps > .43). Like ratings of responsiveness, we found no differences between the 0%, 25% and 50% conditions on perceptions of the targets' understanding (0 vs. 25%: b = -0.07, *95% CI* [-0.72, 0.59], p = .99; 0 vs. 50%: b = -0.06, *95% CI* [-0.75, 0.63], p = .99; 25 vs. 50%: b = 0.008, *95% CI* [-0.66, 0.68], p = .99). Only when targets heard 75% of the conversation garbled did perceivers notice that the targets' understanding suffered (0 vs. 75%: b = -0.79, *95% CI* [-1.48, -0.10], p = .02; 25 vs. 75%: b = -0.72, *95% CI* [-1.40, -0.05], p = .03; 50 vs. 75%: b = -0.73, 95%*CI* [-1.44, -0.03], p = .04).

When directly asked to estimate the percentage of the conversation their partner heard, perceivers made statistically equivalent estimates across the 0%, 25% and 50% garbled conditions (0 vs. 25%: b = -2.73, *95% CI* [-11.70, 6.21], p = .86; 0 vs. 50%: b = -6.49, *95% CI* [-15.90, 2.93], p = .28; 25 vs. 50%: b = -3.76, *95% CI* [12.90, 5.40], p = .71). Though perceivers guessed that targets heard less in the 75% condition compared to the 0% and 25% conditions (0 vs. 75%: b = -14.18, *95% CI* [-23.60, -4.74], p = .001; 25 vs. 75%: b = -11.45, *95% CI* [-20.70, -2.26], p = .008), they reported no differences between the 50% and 75% conditions (b = -7.70, *95% CI* [-17.40, 1.96], p = .17). Even in that condition, perceivers overestimated targets' ability to hear, guessing that they *could* hear 75% of the conversation, when they could only hear 25%.

Finally, when asked to make person-level judgments about the targets, the perceivers reported no differences in liking (0 vs. 25%: b = -0.03, *95% CI* [-0.58, 0.64], p = .99; 0 vs. 50%: b = -0.04, *95% CI* [-0.68, 0.61], p = .99; 0 vs. 75%: b = -0.31, *95% CI* [-0.96, 0.33], p = .59; all

other *p*s > .69), judgments of interestingness (0 vs. 25%: *b* = -0.06, *95% CI* [-0.77, 0.66], *p* = .99; 0 vs. 50%: *b* = -0.07, *95% CI* [-0.83, 0.69], *p* = .99; 0 vs. 75%: *b* = -0.47, *95% CI* [-1.23, 0.30], *p* = .39 all other *p*s > .47), or judgments of intelligence (0 vs. 25%: *b* = -0.20, *95% CI* [-0.81, 0.42], *p* = .84; 0 vs. 50%: *b* = -0.28, *95% CI* [-0.92, 0.37], *p* = .68; 0 vs. 75%: *b* = -0.53, *95% CI* [-1.17, 0.12], *p* = .15; all other *p*s > .52).

These results present a fairly extreme example of listening perception inaccuracy. Even when targets could only hear 25% of their partner's spoken words in the conversation, they were rated as high-quality listeners, appearing engaged and responsive to their partner. Interestingly, at this extreme, evaluations of understanding suffered—suggesting that the inability to hear one's partner did have some effect on perceivers' impressions. In support of results from Studies 1-3, these findings reveal the insensitivity of listening perceptions to the actual cognitive experience of listeners.

Experiment 4 Discussion

The results of Experiment 4 suggest that perceptions of listening are largely impervious to a listener's inability to actually hear their partner's words. It was only when 75% of the verbal content was garbled that perceivers noticed that their conversation partners understood less. But even in this extreme case, perceiver ratings of listening, responsiveness, effort, attentiveness, interestingness, likeability, and intelligence did not differ (both compared to targets who could hear more and those who could hear everything).

Even though these results are contingent on targets being incentivized to feign their listening, it is striking that people can portray attentive listening even when they cannot hear most of what is being said. This suggests that perceptions of listening are largely determined by behaviors that are surprisingly untethered from the informational meaning of the conversation.

While one would hope such extreme situations are uncommon outside the laboratory, these results reveal that listening perceptions can be divergent from reality—especially when incentives to deceive are present. These incensitves are likely common in many naturalistic conversations given the social desirability of being perceived as a "good listener."

While these these findings do not rule out the possibility of listener multi-tasking in Experiments 1 and 3, they do rule *in* the highly skilled ability of listeners to convey attentiveness and understanding, even in the absence of it. Overall, these results reinforce the findings from our previous studies that perceptions of listening are often inaccurate: there seems to be a substantial gap between *feeling* heard and actually *being* heard.

General Discussion

Conversational listening is a key building block of human social functioning. Information transmission, interpersonal connection, conflict management, happiness—the key foundations of human flourishing—hinge critically on our ability to hear, understand, and respond to others (e.g., Schiller, 1996; Yeomans et al., 2021). A large body of work finds that speakers and listeners alike experience myriad benefits when people are perceived as "good listeners" (e.g., Bodenmann, 2005; Huang et al., 2017; Kuhn et al., 2018; Lloyd et al., 2015; Qian et al., 2019; Shafran-Tikva & Kluger, 2018; Wanzer et al., 2004; Yeomans et al., 2020). At the same time, a rich literature on the failings of mind perception (Epley, 2008; Eyal et al., 2018) and the inability to detect lies from truth (Bond & DePaulo, 2006) calls into question whether perceptions of listening accurately reflect the internal experience of listening (i.e., *being heard*) or merely reflect an illusory subjective experience in the mind of the speaker (i.e., *feeling heard*). Although the subjective experience matters immensely, it may not represent the construct it is understood

to represent: *attending to and processing of another person's verbal, nonverbal, and paralinguistic cues during conversation*.

Across five studies, we find that there is a difference between *being* and *feeling* heard. In live conversation, people's perceptions of their conversation partners' listening were only moderately related to the partners' internal cognitive experiences of listening (Exploratory Study). Though people's listening fluctuated naturally throughout their conversations (with mind wandering reported 24% of the time), they were also able to nimbly adjust their listening in line with instructions—by either listening attentively, inattentively, or dividing their attention when they were told to do so (Experiment 1)—and their conversation partners were scarcely able to detect the rise and fall of their partner's attentiveness, whether via natural fluctuation or via our experimental intervention. This phenomenon extended to third-party observers who were not immersed in the conversation (Experiment 2), listeners who looked back on their own listening (Experiment 3), and people interacting with listeners who could not physically hear what their partner was saying (Experiment 4). Thus, across a diverse set of studies, we find support for our three primary hypotheses: that (1) perceptions of conversational listening often do not align with listeners' internal cognitive experiences; (2) they are often inaccurate due to a lack of diagnostic behavioral cues displayed by the listener—attentive listeners behave similarly to inattentive listeners; and consequently (3) perceivers primarily *overestimate* the extent to which their conversation partners are listening to them.

Theoretical Contributions

Our work makes several contributions that advance our understanding of interpersonal perception, listening, and the psychology of conversation more broadly. Across studies, we find a consistent pattern of *overestimation*: perceivers were biased towards over-attributing listening,

frequently believing their conversation partners were listening attentively to them when they were not. These results are similar to the truth bias, in which people assume others are telling the truth more often than they are (Bond & DePaulo, 2006; Levine et al., 1999; Vrij, 2008). In fact, the direction of this error may be socially adaptive for perceivers—perhaps it is less costly to mistakenly assume someone is listening when they aren't than to erroneously accuse them of inattentiveness. This is in line with prior work on "want-should conflict" (Bitterly et al., 2015) because an accusation of inattentiveness carries immediate social costs, whereas the risks of being misunderstood are probabilistic and temporally distant.

Inaccurate perceptions of listening make sense, as we find a dearth of behavioral differences between attentive listeners and those pretending to listen attentively—with both engaging in a range of verbal and non-verbal behaviors to the same extent. A similar pattern has been found to contribute to inaccuracy in lie detection, such that truth tellers and liars behave very similarly across a range of observable behaviors (Hartwig & Bond, 2011; Wiseman et al., 2012). It may be that conversationalists engage in a specific *type* of deception—people may feign their listening when their attention is drawn away from the speaker, perhaps due to the social desirability of appearing as a "good listener." In fact, it may be helpful for conversationalists to think of feigned cues of listening as a specific type of deception, even if these moments of deception are often prosocial (e.g., Levine & Schweitzer, 2015).

Our findings raise important questions: When are communicators better or worse off erring in the direction of over-optimism about their partners' attentiveness? And when are listeners better or worse off covering up moments of inattentiveness? Though people seem well practiced in these maneuvers, their optimality depends on interlocutors' informational and

relational goals, such as whether they care more about accurate information exchange, politeness, enjoyment, or the smoothness of the conversation (Yeomans et al., 2021).

Further, these findings contribute to an emerging literature shedding light on people's inattentiveness to others during conversation. Recent work has shown that people are insensitive to conversational coherence and perspective-taking—in both their lack of reaction to nonsensical turns of phrase (e.g., "colorless green ideas sleep furiously"), and even to whom their conversational partners are (e.g., Galantucci & Roberts, 2014; Galantucci et al., 2018; Roberts et al., 2016; Yeomans & Brooks, 2021). Together with previous work, our findings build on a growing literature that highlights the risks of miscoordination inherent in live conversation (see Yeomans et al., 2021 for a review).

Recent research on the importance of establishing and sustaining shared reality—the perceived commonality of internal states with other people—for the development and maintenance of relationships suggests that our findings may be particularly consequential (Higgins et al., 2021; Rossignac-Milon et al., 2021; Rossignac-Milon & Higgins, 2018). Inaccuracy in listening perception is almost by definition a roadblock to shared reality—it may feel polite to feign listening in the moment, but this deception, if left unnoticed or unrepaired, will likely erode shared understanding and may jeopardize relationships over time.

Limitations and Future Directions

Our methods and findings are qualified by several limitations that offer fruitful avenues for future research. First, we primarily observed conversations between strangers. Future work should explore the listening behaviors between people who know each other. Perhaps close pairs are better able to detect idiosyncratic cues of poor listening (or perhaps they believe they're better, but aren't); perhaps people are particularly good at fooling close others that they're

listening; perhaps close pairs are less likely to devote the effort needed to feign good listening; and so on—with meaningful consequences for relational harmony and information exchange beyond one-time conversations.

Additional work is also required to understand how these processes play out across different *types* of conversations (Yeomans et al., 2021). Every conversation is wildly different—in fact, every aspect of conversational context can change between conversations, and within them, from one turn to the next (who, what, where, when, why, and how they're occurring). We focus here on interactions primarily driven by relational goals, however, perceptual errors about others' attentiveness—and the ways in which conversationalists manage them—may be different in contexts where high-informational goals (e.g. learning, brainstorming, making decisions, persuading, exchanging accurate information) loom large.

Additionally, by design, we conducted our studies in a controlled lab setting. Future research should investigate more naturalistic contexts, especially contexts in which it may be easier (or harder) to detect feigned or inattentive listening—for example by considering the role of communication medium or modality (e.g., Boland et al., 2022; Schroeder et al., 2017; Lieberman et al., 2022). Different types of communication media often constrain the cues that are available to listeners (e.g., video-conferences might only show people from the shoulders up; phone calls provide no nonverbal cues at all); change the reliability of certain cues of attention (e.g., one cannot be sure of directed eye contact over video-conferencing, Abi-Esber et al., forthcoming); and therefore alter people's overall ability to accurately detect listening in an increasingly digital world.

Second, future work should examine how misperceptions of listening influence important downstream consequences, such as learning, productivity, decision-making, trust, liking, and

other indicators of relationship quality. As suggested above, this work should specifically investigate the role of repeated interaction—while feigned listening may provide benefits within a single conversation, the costs of such deceptions may reveal themselves over time and may influence both relational and informational outcomes across many conversations in a relationship (Yeomans et al., 2021).

Third, additional research is required to understand the relative role of perceived attention and perceived information processing. Prior research has identified multiple stages in attitude formation, distinguishing between attention to information versus information processing (Kunda, 1990; McGuire, 1968; Minson et al., 2020). Whereas attention has some external markers (eye gaze, absence of interruption), information processing is internal. Thus, it may be the case that individuals use cues of attention (which are easy to feign) to infer others' internal processing. Future work could investigate whether reminding people that signals of *attention* are not reliable signals of *processing* might improve the accuracy of perceived listening overall.

Finally, additional research is required to understand the relative roles of the speaker and listener in promoting these misperceptions. Are speakers insensitive to their partners'—and perhaps their own—lapses in listening? Do we all underestimate the extent to which the human mind wanders? Are listeners skilled at feigning attention when their attention is divided—and when their minds predictably wander? All of the above? We find preliminary evidence that listeners feign attentiveness in live conversation through various non-verbal behaviors—nodding and smiling when their attention is elsewhere. But we suspect speakers may also be to blame by relying too heavily on low-fidelity signals of their attentiveness. In fact, it's likely that these effects are recursively reinforcing: speakers hold overly optimistic beliefs about partner attentiveness, which creates unrealistic norms and expectations of attentiveness, which puts

pressure on people to feign attentive listening to cover frequent moments of inattentiveness, which, unnoticed and unrepaired, reinforce overly optimistic beliefs.

On the other hand, recognizing this perceptual error may present an opportunity for growth. We call for future work to investigate explicit conversational strategies that may disrupt this fallacious listening loop. For example, with minimal intervention, listeners may be nudged to more explicitly admit their lapses in listening, which would allow for more immediate repair of glitches in information exchange, or learn to use more verbal signals of attentive listening that cannot be faked, such as making call-backs to earlier topics, paraphrasing, or asking follow-up questions—behaviors recently described as powerful signals of "conversational uptake" (Collins, 2022; Demszky et al., 2021; Huang et al., 2017; McQuaid et al., 2015). At the same time, speakers, too, may be able to adjust their expectations to anticipate the cognitive demands of attentive listening, to become more forgiving of their partners' lapses in listening, and to remain open to repair strategies and attuned to high-fidelity signals of attentive listening, like verbal uptake.

Conclusion

Recent work suggests that people are blind to major disruptions in logical coherence during interpersonal encounters (Galantucci et al., 2018; Galantucci & Roberts, 2014; Roberts et al., 2016). On the one hand, this is unsurprising: conversation is a complex, overwhelming decision environment that requires relentless thinking, perceiving, monitoring, and deciding—the human mind is bound to make mistakes. On the other hand, the misalignment between perceptions of listening and listeners' underlying cognitive experiences that we document here is a radically different approach than one that has been taken by the prior literature which has largely focused on developing, conveying, and identifying "good" and "active" listening skills.

Taken together, our findings suggest that people often misjudge whether their partners are listening (or not) and call for a re-examination of this important and pervasive behavior for which cognitive and social experiences seem misaligned. Though both are important, it seems there is a noticeable (but navigable) gap between *feeling* heard and *being* heard in conversation.

CHAPTER 3: UNDERESTIMATING COUNTERARTS' LEARNING GOALS IMPAIRS CONFLICTUAL CONVERSATIONS

Dislike, disrespect, and distrust toward holders of opposing ideological views, or "affective polarization," have recently surpassed previously documented levels (Finkel et al., 2020; Iyengar et al., 2019). From policy arguments to workplace disagreements, to scientific debates, engagement with opposing ideas devolves into attitude conflict, often causing relational harm (Kennedy & Pronin, 2008; Schroeder et al., 2017; for review, see Minson & Dorison, 2021).

Given that communication between opponents is a precondition for solving important social problems, researchers have sought to improve conflictual dialogue by fostering a focus on learning. Thus, partisans have been encouraged to "consider the opposite," (Lord et al., 1984), ask elaboration questions (Chen et al., 2010), take the perspective of out-group members (Todd & Galinsky, 2014; Bruneau & Saxe, 2012), use open-minded thinking (Baron, 2019), and be receptive to opposing views (Minson et al., 2020). Here, we suggest that a complementary focus on individuals' *beliefs about their counterpart's learning goals* can prove useful.

Goals in Conflictual Conversations

Goals are desired endpoints that guide behavior (Fischbach & Ferguson, 2007; Gollwitzer & Oettingen, 1998; Dweck & Elliot, 1983). Only recently have scholars considered the *interpersonal* nature of goals (see Fitzsimons & van Dellen, 2015), and little is known about their role in conflictual conversations.

Prior work proposed two broad categories of goals in disagreement. Judd (1978) suggested that parties strive either to demonstrate that their attitude is correct ("competitive orientation") or to learn more about the issue ("cooperative orientation"). Relatedly, mediation

practitioners distinguish between "advocacy" (arguing for one's views) versus "inquiry" (soliciting additional input) mindsets (Garvin & Roberto, 2001; Lee, 2018). Here, we refer to these as persuasion and learning goals, respectively.

Prior research suggests that beliefs about counterparts' learning goals in particular may powerfully shape conversations. People want to be understood (Swann, 2011) and validated (Reis & Patrick, 1996). "Active listening"—a communication style conveying a desire to learn from the speaker—is a key therapeutic skill (Rogers & Farson, 2021). When discussing difficult topics, feeling heard increases self-esteem and open-mindedness (Itzchakov et al., 2020; Voelkel et al., 2021). When others convey an interest in learning through engaged listening, speakers feel supported and accepted. By contrast, although people respond poorly to persuasion attempts (Friedstad & Wright, 1994; Koslow, 2000), there is no evidence that *the absence* of persuasion dramatically improves interpersonal outcomes. Thus, we predict that believing that a counterpart holds learning goals will lead to more positive evaluations of, and experiences with, that counterpart during attitude conflict.

(In-)Accuracy in Goal Perception

Prior work has demonstrated that parties in conflict regularly misjudge opponents (Ahler & Sood, 2018; Moore-Berg et al., 2020). Counterparts systematically over-estimate how much out-group members' views differ from their own (i.e., "false polarization"; Fernbach & Van Boven, 2021), and how negatively outgroup members view the in-group (Lees & Cikara, 2020; Ruggeri et al., 2021). Such misunderstandings are perhaps not surprising considering people's tendency to derogate out-group members' intelligence, motives, perspective taking, and even basic humanity (Brandt & Crawford, 2020; Minson et al., 2020; Schroeder et al., 2017).

Importantly, being willing to learn requires counterparts to have benevolent intent and sophisticated perspective-taking—the very qualities we refuse to acknowledge in opponents. Furthermore, learning goals are difficult to evaluate: if a counterpart is listening silently, how can we know whether they are learning about our perspective or generating counterarguments? By contrast, persuasion goals are more clearly signaled by the presence of counterarguments. Building on this work, we hypothesize that, on average, partisans believe their counterparts to be less willing to learn about their views than is actually the case. The same mis-estimation however, is not likely to extend to persuasion goals that are more easily perceived.

Intervening in Conflict by Re-Calibrating Goal Perceptions

Informational interventions can effectively reduce misperceptions during attitude conflict (Dorison et al., 2019; Lees & Cikara, 2020; Moore-Berg et al., 2020; Ruggeri et al., 2021). Furthermore, the benefits of shifting (mis-calibrated) perceptions have also been demonstrated in applied settings (Fishkin et al., 2019). For example, the non-profit organization Braver Angels brings together liberals and conservatives in a learning-focused environment designed for increasing partisans' insights about each other. An overwhelming majority of participants report high levels of mutual understanding and empathy after the workshops (Jacobs et al., 2019; see also Baron et al., 2021 for experimental evidence). Building on this prior work, we hypothesize that informing participants that their counterpart is open to learning about their views will improve both interpersonal evaluations and actual conversational experiences.

The Present Research

We test three inter-related hypotheses across seven pre-registered studies (N=2,614). Studies 1a-1c test whether disagreeing participants under-estimate the extent to which their counterpart is willing to learn about their perspective (across multiple domains). Study 2 tests

whether perceptions about counterpart learning goals drive affective polarization and evaluations of a conflictual conversation in the context of the 2020 US presidential election. Finally, Studies 3-5 test whether manipulating perceived learning goals improves conflict outcomes in American partisan politics and the Israeli-Palestinian conflict.

Study 1a

Method

In Studies 1a-c we investigated the goals people endorsed when interacting with holders of opposing views (across multiple domains of attitude conflict) and compared these to ones that individuals believed their counterparts endorsed. We theorized that while participants would systematically under-estimate the learning goals held by counterparts, this same pattern would not persist for persuasion goals.

Participants. To achieve 90% power based on effect size estimates from a pilot study, we recruited 600 participants through Amazon's Mechanical Turk (MTurk) to complete a 5-minute survey in which they provided open-ended descriptions of the goals that they and their counterparts pursue in conflictual conversations. First, participants reported their interest in several topics (e.g., Broadway musicals, Trivia Game Shows) on a scale from 1: "Not at all" to 5: "Extremely." As per our pre-registration, only participants who reported having a strong interest (reporting either 4: Very or 5: Extremely) in Political News, NBA Basketball, NFL Football, or MLB Baseball were eligible to complete the rest of the survey. While our main practical interest was in political disagreements, we chose to recruit sports fans to test whether our effects would generalize to another context in which individuals have strong attitudes. This resulted in a final sample of 201 participants (39% Female, M_{age} = 33). For this and all other studies, we report how we determined sample sizes, all data exclusions, all manipulations, and all measures

(Simmons et al., 2012). Data, materials, pre-registrations, and code for all studies are available here: https://researchbox.org/372.

Protocol. Depending on self-reported interest in each topic, participants imagined having a 5-minute conversation with someone from the opposite side of the political spectrum (n = 100) or someone they disagreed with about the best team in a professional sports league (n = 101). Participants were assigned to imagine a conversation on the topic in which they reported having a strong interest (reporting either 4: Very or 5: Extremely), and those who indicated a strong interest in multiple topics were randomly assigned to one. Participants in the politics group were told to imagine having a conversation with someone from the "opposite side of the political spectrum." Participants who considered a conversation about sports imagined talking to someone who disagreed with them on which team was the best in the league.

We then randomly assigned participants to report their goals (self-condition, n = 101) or their partner's goals (other-condition, n = 100) during this conversation. Participants wrote up to five goals that they (or their partner, depending on condition) would have during the conversation using open-ended text boxes. Participants generated a total of 960 goals. Finally, participants provided basic demographic information including their age and gender.

Coding. We coded participants' open-ended responses according to the following pre-registered process. First, one coder examined the data and removed any responses that were non-sensical or unrelated to having a conversation with a disagreeing other (e.g., "Buy an RV"; n = 104 goals excluded). Next, this coder was joined by a second coder to read and classify each goal as belonging to one of three categories according to a pre-determined coding rubric, based on prior research (Garvin & Roberto, 2001; Judd, 1978; Lee, 2018): learning goals, persuasion goals, or miscellaneous goals. We had substantial agreement between coders, as indicated by

80% (n = 767) agreement. In order to factor in the level of agreement due to chance, we calculated Cohen's unweighted *kappa* = 0.67, *95%CI*[0.63, 0.71], indicating a substantial level of agreement (Altman, 1990). A third coder then read and classified the remaining 193 goals where the first two coders did not agree. For 88% (n = 170) of these goals, the third coder provided a code that agreed with one of the first two coders, which was then retained as the final categorization for these goals. The remaining 23 goals were read and classified by a fourth coder, and the most common classification produced by the four coders was considered final. All coders were blind to hypotheses.

After each goal was coded as belonging to a unique category, we calculated three dependent variables for each participant: (1) proportion of learning goals, (2) proportion of persuasion goals, and (3) proportion of miscellaneous goals generated.

Results

Most goals reported in both conditions (79%) fell into the categories of "learning" and "persuasion." Figure 9 presents the proportion of different goal types generated by participants in both conditions.

In line with our theorizing, participants listed dramatically fewer learning goals when considering their counterpart's goals (M = 0.16, SD = 0.24), rather than their own goals (M =0.42, SD = 0.30), $t(190.88) = 6.74$, $p < .001$, Cohen's $d = -0.95$, *95%CI*[-1.25, -0.66]. Importantly, this pattern could not be attributed to a broader failure to vividly consider the goals of other people: participants listed a greater number of persuasion goals for their counterpart (M = 0.71, SD = 0.30) than for themselves (M = 0.38, SD = 0.31), $t(198) = 7.66$, $p < .001$, Cohen's $d = 1.08$, *95%CI*[0.78, 1.38]. While we did not predict this reversal for persuasion goals, we discuss it further in the General Discussion. Critical to our investigation, a 2x2 mixed ANOVA

with one between-subjects factor (other vs. self) and one within-subjects factor (proportion of goals classified as learning vs. persuasion) yielded a significant interaction, $F(1, 396) = 103.94$, $p < .001$. Participants reported a slightly greater proportion of miscellaneous goals for themselves (M = 0.20, SD = 0.21) than for their counterpart (M = 0.13, SD = 0.20), t(197.93) = 2.50, $p = .01$, Cohen's $d = -0.35$, *95%CI*[-0.63, -0.07].

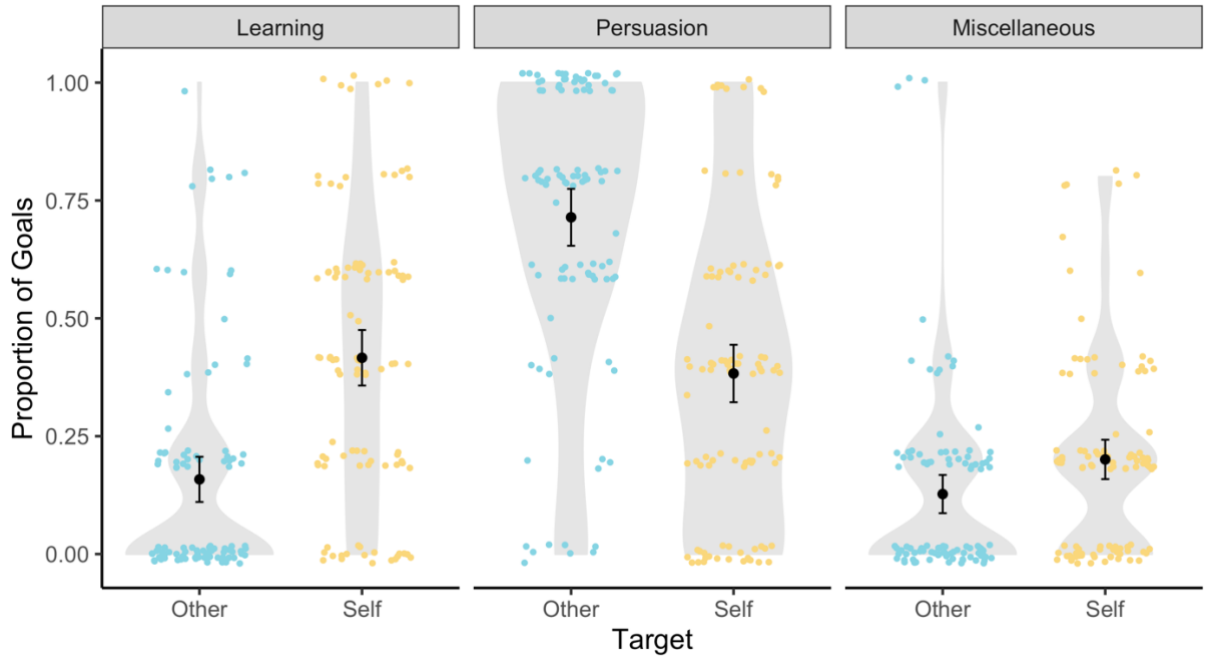

Figure 9. Comparison of the conversational goals reported for the self and those reported for a disagreeing other in Study 1a. Participants systematically under-estimated the learning goals reported by others, but this pattern did not generalize to (and reversed for) persuasion goals. Shaded plots display the distributions; error bars represent 95% CI around the mean; colored dots represent raw data.

Finally, we assessed whether the effects documented above depended on the specific context (politics vs. sports). A 2x2 mixed ANOVA found a significant interaction between condition (self vs. other) and goal type (learning vs. persuasion) for participants imagining a conversation about sports, $F(1, 198) = 23.20$, $p < .001$, and politics, $F(1, 194) = 108.53$, $p < .001$.

Of note, however, we found that the effects persisted in both contexts (Table 8). However, these differences were larger (approximately double in size) when participants imagined a conversation about politics.

Table 8. Comparison of Proportion of Goals in Each Category Reported for the Self vs. a Disagreeing-Other, Separated by Topic of Attitude Conflict.

	Self vs. Disagreeing-Other		
	Learning Goals	Persuasion Goals	Miscellaneous Goals
Politics	Cohen's d = -1.48, *95%CI*[-1.93, -1.03]	Cohen's d = 1.49, *95%CI*[1.04, 1.94]	Cohen's d = -0.23, *95%CI*[-0.63, 0.17]
Sports	Cohen's d = -0.55, *95%CI*[-0.95, -0.15]	Cohen's d = 0.79, *95%CI*[0.38, 1.20]	Cohen's d = -0.48, *95%CI*[-0.88, -0.08]

Discussion

Individuals under-estimated the number of learning-related, but not persuasion-related, goals for conflict counterparts. These effects were amplified for politics (vs. sports).

Study 1b

Method

Study 1b tested the same overarching hypotheses with a new sample population (Prolific Academic) and a different response format (Likert items). We again assessed whether, across domains, participants would underestimate counterparts' learning (but not persuasion) goals.

Participants. To achieve 90% power based on effect size estimates from a pilot study, we recruited 400 participants through Prolific Academic to complete a 3-minute survey. Participants again reported their interest in several topics (e.g., Broadway musicals, Trivia Game Shows) on a scale from 1: "Not at all" to 5: "Extremely." As per our pre-registration, only participants who reported having a strong interest (reporting a 4: "Very" or 5: "Extremely") in Political News,

NBA basketball, NFL football, or MLB baseball were eligible to complete the rest of the survey. Further, 82 participants who failed our attention check were excluded. This resulted in a final sample of 160 participants (36% Female, M_{age} = 32).

Protocol. As in Study 1a, participants imagined having a 5-minute conversation with someone they disagreed with about politics (n = 97) or sports (n = 63) based on their self-reported interest in each topic. Participants who indicated a strong interest in multiple topics were randomly assigned to one.

Own Goals. Participants reported how important various goals were to them in this conversation on a scale from 1: "Not at all important" to 5: "Extremely important." Most critically, participants evaluated three statements pertaining to learning goals: (1) Learning about your partner's perspective; (2) Understanding your partner's point of view; and (3) Hearing evidence for your partner's beliefs (α= .73). In addition, participants evaluated three statements pertaining to persuasion goals: (1) Persuading your partner of your point of view; (2) Convincing your partner that you're right; and (3) Presenting evidence for your point of view (α= .84). Order of all six items was randomized.

Beliefs About Partner's Goals. Participants were also asked to report "How important would each of these goals be for <u>your partner</u> in this conversation?" using the same items as above (α_{learn} = .51; $\alpha_{persuade}$ = .93). The order in which participants reported their own goals and their beliefs about their partner's goals during the conversation was counterbalanced, such that half of the participants first answered all questions about their own goals before answering about their partner, and the other half of participants completed the same two sets of questions in the opposite order.

Results

We again theorized that participants would under-estimate their counterparts' learning goals, but that this under-estimation would not extend to persuasion goals. This theorizing was supported: In line with Study 1a, participants evaluated learning goals as being less important to their counterparts (M = 3.20, SD = 1.17) than to themselves (M = 4.02, SD = 0.79), $t(159) = 9.69$, $p < .001$, Cohen's $d = -0.81$, *95%CI*[-0.99, -0.62]. Replicating Study 1a, we again saw that this pattern was not matched for persuasion goals: participants rated persuasion goals as being more important to counterparts (M = 3.92, SD = 0.78) than to themselves (M = 3.62, SD = 0.87), $t(159) = -4.30$, $p < .001$, Cohen's $d = 0.36$, *95%CI*[0.19, 0.53]. To put these results in perspective, we found that 66% of participants reported higher learning goals for themselves than a disagreeing counterpart (while only 12% reported lower learning goals for themselves and only 25% reported the same level for the self and other). Figure 10 presents these results.

To test our primary hypothesis, we tested a mixed effects model specifying a fixed effect for goal type (learning vs. persuasion) and target (self vs. other), and a random effect for participant to account for repeated measures. In line with our predictions we found a significant interaction between goal type and target, $b = -1.13$, *95%CI*[-1.38, -0.87], $p < .001$. These results again revealed that people under-estimated the importance of learning goals to their counterparts, but did not make the same error for persuasion goals.

An alternative account of our results may be that individuals have very little insight into other's goals and are thus responding randomly, around the midpoint of the scale. However, the fact that they reported significantly higher persuasion than learning goals for counterparts makes this explanation less credible.

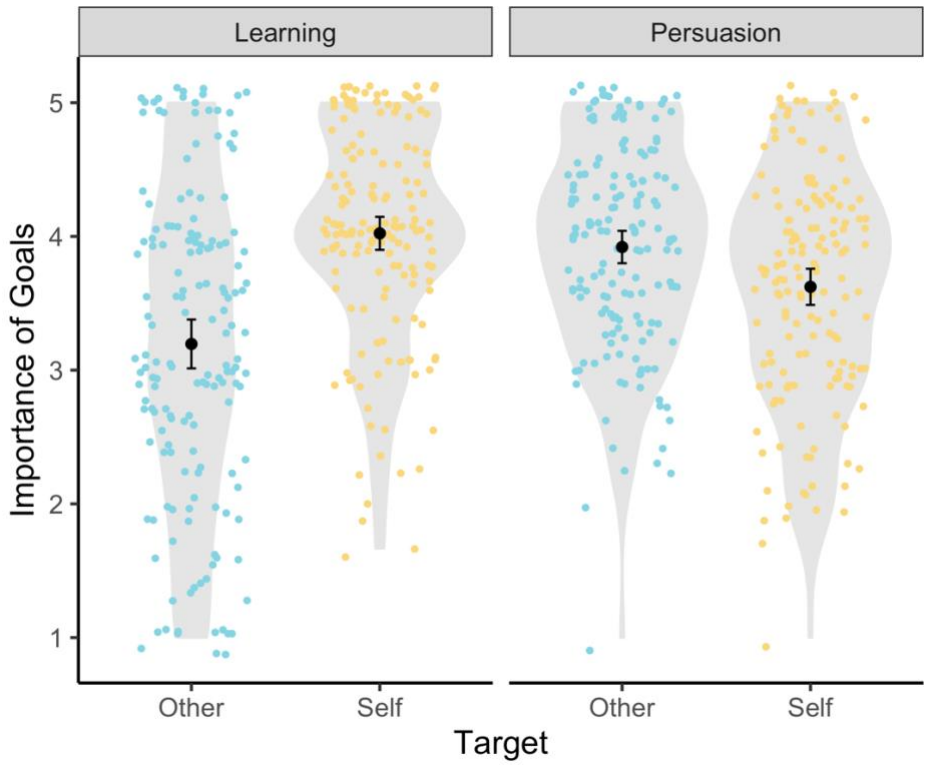

Figure 10. Comparison of the conversational learning and persuasion goals reported for the self and a disagreeing other in Study 1b. Participants systematically under-estimated the learning goals reported by others, but this pattern did not generalize to (and reversed for) persuasion goals. Shaded plots display the distributions; error bars represent 95% CI around the mean; colored dots represent raw data.

Finally, we assessed whether the documented effects above depended on the specific context (politics vs. sports). A mixed effects model including a random effect for participant found a significant interaction between condition (self vs. other) and goal type (learning vs. persuasion) for participants imagining a conversation about sports, b = -0.75, *95%CI*[-1.11, -0.40], $p < .001$, and politics b = -1.37, *95%CI*[-1.72, -1.01], $p < .001$. Of note, however, we found that the effects persisted in both contexts (Table 9). However, as in Study 1a, these differences were larger (approximately 1.5x larger) for participants imagining a conversation about politics.

Table 9. Comparison of Importance of Goals in Each Category Reported for the Self vs. a Disagreeing-Other, Separated by Topic of Attitude Conflict.

	Self vs. Disagreeing-Other	
	Learning Goals	**Persuasion Goals**
Politics	Cohen's *d* = -0.89, *95%CI*[-1.14, -0.65]	Cohen's *d* = 0.43, *95%CI*[0.19, 0.67]
Sports	Cohen's *d* = -0.65, *95%CI*[-0.93, -0.36]	Cohen's *d* = 0.25, *95%CI*[0.02, 0.48]

Discussion

Study 1b replicated Study 1a with a new response format and new participant sample.

Study 1c

Method

In Study 1c, we assessed whether the self-other difference observed in Studies 1a-b extends to (1) all other individuals (including both agreeing- and disagreeing-others), or (2) whether disagreement is required. Thus, we added a new condition in which participants imagined *observing* a conflictual conversation (rather than *engaging in* one) between someone who agreed with them on an issue and someone who disagreed with them.

Participants. To achieve 90% power based on effect size estimates from Studies 1a-b, we recruited 700 participants through Prolific Academic to participate in a 3-minute study. As before, participants reported the extent to which they cared about several topics. As per our pre-registration, we excluded 380 participants who did not have a strong interest in Political News, NBA basketball, NFL football, or MLB baseball. This resulted in a final sample of 320 participants, all of whom passed our attention check.

Protocol. All participants were asked to imagine a conflictual conversation on one of several topics: politics (n = 139) or sports (n = 181). Participants were randomly assigned to one

of three conditions, varying the target whose goals participants considered: self, disagreeing other, agreeing other. In the "self" and "disagreeing other" conditions, participants completed the same task as in Studies 1a-b: they imagined engaging in a conversation with someone who disagreed with them on their assigned topic and evaluated the importance of various goals either to themselves (in the "self" condition) or to the disagreeing other. In the "agreeing other" condition, participants were instead asked to imagine that they were watching this conversation between two people who disagreed with each other on their assigned topic. Participants were told that one of the individuals in the conversation agreed with their own point of view, while the other individual disagreed. In this condition, participants evaluated the importance of various goals to the agreeing other in this conversation.

Using items from Study 1b, participants evaluated the importance of three items pertaining to persuasion goals (α= .71) and three items pertaining to learning goals (α= .90), with all items presented in randomized order.

Results

We first assessed whether we replicated the pattern of results from Studies 1a-b. This was in fact the case: In line with our prior results, participants again under-estimated the importance of learning goals to a disagreeing other (M = 3.22, SD = 1.10), relative to themselves (M = 3.90, SD = 0.85), $t(204.48) = 5.09$, $p < .001$, Cohen's $d = 0.69$, *95%CI* [0.41, 0.97]. Additionally, participants over-estimated the importance of persuasion goals to a disagreeing other (M = 3.84, SD = 0.80) relative to themselves (M = 3.55, SD = 1.00), $t(198.60) = 2.34$, $p = .02$, Cohen's $d = -0.32$, *95%CI* [-0.59, -0.05]. We again observed a significant interaction effect between goal type (learning vs. persuasion) and target (self vs. disagreeing other), $b = -0.97$, *95%CI*[-1.33, -0.61], $p < .001$.

Next, we considered the relative importance of learning goals that participants attribute to the self, relative to an agreeing other. Our key question was whether agreeing counterparts would be perceived more similarly to the self (suggesting disagreement is required for the self-other difference to occur) or more similarly to disagreeing counterparts (suggesting that the self-other difference is robust across levels of agreement). We found relatively greater support for the former hypothesis. First, we found a small and only marginally significant difference in the importance of learning goals that participants report for themselves (M = 3.90, SD = 0.85) compared to an agreeing other (M = 3.68, SD = 0.96), $t(205.02) = 1.82$, $p = .07$, Cohen's $d = -0.25$, *95%CI*[-0.52, 0.02]. Thus, agreeing others were not seen as holding identical goals as the self. However, we also found that participants reported learning goals to be more important to an agreeing other (M = 3.68, SD = 0.96) compared to a disagreeing other (M = 3.22, SD = 1.10), $t(211.42) = 3.22$, $p = .001$, Cohen's $d = -0.44$, *95%CI*[-0.71, -0.17]. To put these results in perspective, agreeing others are perceived more similarly to the self ($M_{self} = 3.90$ vs. $M_{agreeing\ other} = 3.68$, mean difference = 0.22) than to disagreeing others ($M_{agreeing\ other} = 3.68$ vs. $M_{disagreeing\ other} = 3.22$, mean difference = 0.44).

When we turned to examining persuasion goals, we found that participants reported those to be less important to themselves (M = 3.55, SD = 1.00) compared to an agreeing-other (M = 4.07, SD = 0.66), $t(180.27) = 4.42$, $p < .001$, Cohen's $d = 0.61$, *95%CI* [0.33, 0.89]. Surprisingly, and in contrast to the pattern of results for learning goals above, participants reported persuasion goals to be *more* important to an agreeing-other than to a disagreeing-other (M = 3.84, SD = 0.80), $t(208.98) = 2.28$, Cohen's $d = 0.31$, *95%CI*[0.04, 0.58]. Figure 11 presents these data.

Taken together, while participants believed disagreeing others to be less willing to learn than agreeing others, they also believed them to place less importance on persuasion. Thus, the

self-other difference in learning goals revealed in Studies 1a-b appeared to be driven primarily by disagreement: disagreeing others were believed to be particularly unwilling to learn, even compared to agreeing others (though participants did believe themselves to be slightly more willing than even an agreeing other). Intriguingly, the self-other difference in persuasion goals appeared to be robust across levels of agreement: agreeing others were believed to be even more focused on persuasion than disagreeing others.

Finally, while we again found that while our effects persisted across domains, they were stronger for politics than sports (Table 10).

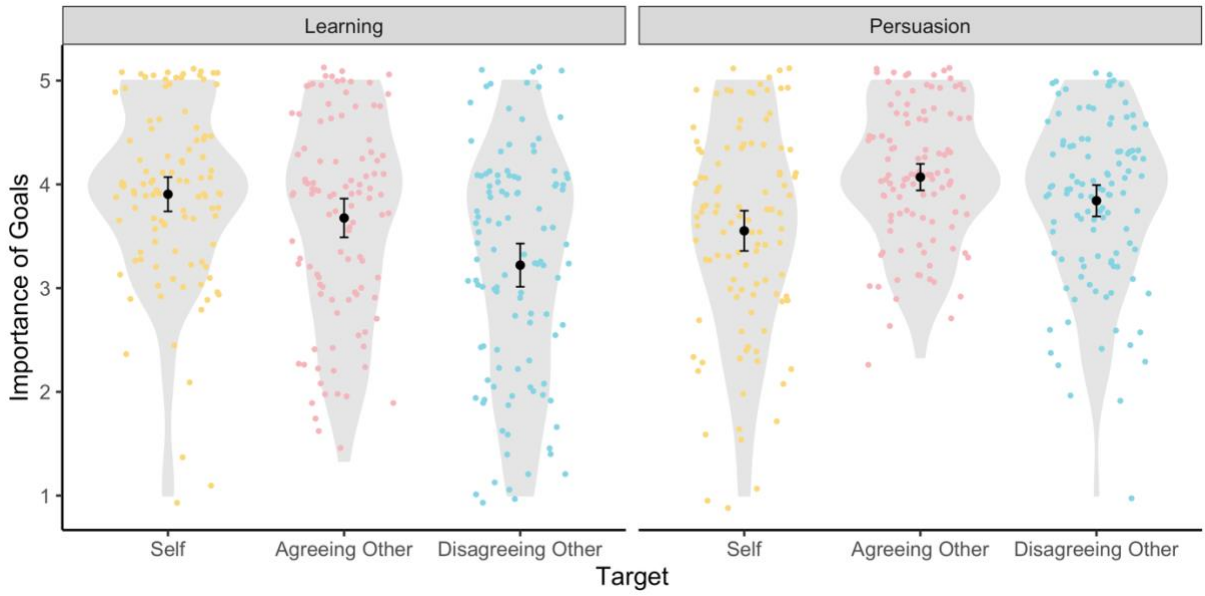

Figure 11. Comparison of the conversational learning and persuasion goals reported for the self, an agreeing other, and a disagreeing other in Study 1c. Participants systematically under-estimated the learning goals reported by disagreeing others compared to the self and an agreeing other, but this pattern did not generalize to persuasion goals. Shaded plots display the distributions; error bars represent 95% CI around the mean; colored dots represent raw data.

Table 10. Comparison of Importance of Goals in Each Category Reported for the Self, an Agreeing Other, and a Disagreeing-Other Separately by Topic of Discussion.

	Self vs. Disagreeing-Other	
	Learning Goals	**Persuasion Goals**
Politics	Cohen's *d* = -1.06, *95%CI*[-1.52, -0.60]	Cohen's *d* = 0.66, *95%CI*[0.22, 1.10]
Sports	Cohen's *d* = -0.57, *95%CI*[-0.98, -0.16]	Cohen's *d* = 0.09, *95%CI*[-0.31, 0.49]
	Self vs. Agreeing-Other	
	Learning Goals	**Persuasion Goals**
Politics	Cohen's *d* = -0.27, *95%CI*[-0.70, 0.16]	Cohen's *d* = 0.75, *95%CI*[0.31, 1.18]
Sports	Cohen's *d* = -0.37, *95%CI*[-0.79, 0.05]	Cohen's *d* = 0.49, *95%CI*[0.07, 0.92]
	Agreeing-Other vs. Disagree-Other	
	Learning Goals	**Persuasion Goals**
Politics	Cohen's *d* = -0.83, *95%CI*[-1.24, -0.41]	Cohen's *d* = -0.04, *95%CI*[-0.44, 0.35]
Sports	Cohen's *d* = -0.19, *95%CI*[-0.61, 0.23]	Cohen's *d* = -0.51, *95%CI*[-0.93, -0.09]

This pattern of results provided evidence that people systematically under-estimate the importance of learning goals to disagreeing counterparts—compared to both themselves and an agreeing counterpart—and do so particularly in the context of political disagreements.

Discussion

The under-recognition of disagreeing others' willingness to learn did not stem from a simple self-other difference, but rather was driven by disagreement—particularly in the political domain. We focus the remainder of our investigation on political attitude conflict.

Study 2

Method

Study 2 examined whether (1) the self-other difference in learning goals persisted in a more naturalistic setting (i.e., a synchronous conversation regarding a bitterly contested election), and (2) whether perceived learning goals predicted subsequent evaluations of the conversation and counterparts. We theorized that (1) the self-other difference would persist even after engaging in (rather than anticipating) a conflictual conversation and (2) perceptions of partner learning goals would robustly predict conflict outcomes, above and beyond other measures of both a focal participant (i.e., an actor) and their counterpart (i.e., their partner).

Recruitment Survey. We recruited participants through a third-party survey firm to participate in a study regarding the 2020 United States Presidential Election. The study consisted of a pre-survey (i.e., a recruitment survey) and a main survey one week later. Over a two-month period between September-October 2020, a total of 4,344 participants completed our pre-survey. Of these, 1,561 participants opened the main survey, and 636 participants were successfully matched with an opposing party supporter to have a conversation. While we pre-registered to collect at least 300 conversations (to achieve 90% power based on effect size estimates from Studies 1a-c, while attending to financial constraints), we ultimately collected 318 because data collection took place in weekly waves.

Interested participants completed an initial pre-survey to determine their eligibility. Participants reported who they were most likely to vote for in the upcoming presidential election, the strength of their support for their candidate, and their opposition to the other candidate. They were deemed eligible if they met two criteria: (1) they strongly supported their candidate (>=3 on a 0: “Not at all” to 5: “Extremely” scale), and (2) they strongly opposed the other candidate (>=3

on a 0: “Not at all” to 5: “Extremely” scale). Eligible participants were asked whether they were willing to complete a 20-minute study the following day in which they would have a 10-minute chat-based conversation with someone they disagreed with regarding the election. Eligible participants who indicated this willingness were invited to complete our main survey.

Main Survey. After reporting whether they were more likely to vote for the Republican candidate (Donald Trump) or Democratic candidate (Joe Biden), participants learned that they would spend the next 10-minutes talking to someone with *opposing voting intentions*. We instructed them to spend the time discussing their beliefs about who is the best candidate. Then, participants responded to three questions to test their comprehension of these instructions, asking them about the length of the conversation, who they would be talking to, and the topic. Participants who answered any of these questions incorrectly were provided with another opportunity to answer and were removed from the survey if they answered incorrectly a second time. Finally, we told participants that they would receive a bonus payment if they remained engaged and on topic for the full 10-minutes.

After the participants received these instructions, we paired each participant with a conversation partner who held opposing voting preferences. We did so via ChatPlat, an online platform that allows for real-time, synchronous chat-based conversation. Each voter was paired with an opposing-candidate voter on a first-come first-serve basis. Participants used text-based communication and they received a 60 second warning when the chat was about to end. After the conversation, participants responded to several measures regarding their counterpart and the conversation they just had.

Measures. First, participants reflected on their own and their counterpart’s goals during the conversation, with the target of consideration counterbalanced. Specifically, participants

reported the importance of the same persuasion (3-item; $\alpha = .83$) and learning (3-item; $\alpha = .73$) goals used in Studies 1b-c. This resulted in a total of eight goals (actor vs. partner; self-assessed vs. perceptions of counterpart; learning vs. persuasion). Additionally, participants reported how enjoyable, pleasant and aversive the conversation was for them (1: "Not at all"; 7: "A lot"; $\alpha = .77$), and made evaluations of how moral, objective, intelligent, trustworthy and likable their partner seemed during the conversation (1: "Extremely Immoral/Unintelligent/Biased/Unlikable/Untrustworthy"; 7: "Extremely Moral/Intelligent/Objective/Likable/Trustworthy"; $\alpha = .89$). Finally, participants reported how much, if at all, their position changed during the conversation (-3: "My position is further from my partner's", 0: "My position did not change," +3: "My position is closer to my partner's") and predicted the same for their conversation partner.

Results

After the conclusion of the conversations, 505 participants completed the entirety of our survey. As per our pre-registration, a research assistant blind to hypothesis reviewed all transcripts and identified 367 participants who remained on topic for the entire 10-minute conversation. This final sample of 367 participants served as our final dataset for analysis (n = 184 Republican supporters, n = 183 Democratic supporters; 34% male; $M_{age} = 54$).

Analytic plan. In the analyses that follow, we differentiate between "actors" (the participant whose responses are being considered) and "partners" (their conversation counterpart). Given the dyadic nature of this data, each participant served as both an "actor" (when they provided their evaluations) and a "partner" (when their conversation counterpart provided evaluations). Thus, we analyzed our data using mixed effects models specifying our key variables as fixed effects and including a random effect for group to account for multiple

observations of the same conversation (one from each conversation counterpart). Our results are robust to two additional approaches to modeling dyadic data: (1) modeling negative non-independence in lieu of random effects using the nlme package in R (Pinheiro et al., 2013) and (2) modeling fixed effects, clustering standard errors at the level of the dyad (Yeomans et al., 2019). Full details are available in our code file posted online.

Do actors under-estimate their partner's learning goals? Our first key question was whether we would replicate the self-other difference in learning goals in a live, synchronous conversation around a hotly contested current event. We did. Specifically, actors reported that learning goals were less important to their partners (M = 3.34, SD = 1.13) than to themselves (M = 4.13, SD = 0.82) during the conversation, $b = 0.78$, *95%CI*[0.65, 0.91], $p < .001$. To put these results in perspective, we found that 66% of participants reported higher learning goals for themselves than their conversation partner (while only 15% reported lower learning goals for themselves and only 19% reported the same level for themselves and their partner).

Surprisingly, and in contrast to Studies 1a-c, actors also under-estimated the importance of persuasion goals to their partner (M = 3.05, SD = 1.00) relative to themselves, (M = 3.34, SD = 0.97), $b = 0.29$, *95%CI*[0.16, 0.42], $p < .001$—51% of participants reported higher persuasion goals for themselves than their conversation partner (31% reported lower persuasion goals for themselves, 19% reported the same amount for themselves and their partner).

While this difference is small, it is nevertheless intriguing. This reversal may be due to the fact that these evaluations were provided after rather than before the conversation. In particular, looking at the means across the earlier studies, self-reported goals as well as perceptions of partner learning goals remained almost identical when rated before (Studies 1b-c) and after (Study 2) the conversation. However, perceptions of partner persuasion goals—which

received the highest ratings when anticipating such conversations—decreased when rated after the conversation. It may be the case that in line with research on "false polarization", participants expected their partners to be more extreme in their views and thus more aggressive in their persuasion attempts, a forecast that experienced proved to be erroneous. We consider this idea further in the General Discussion.

Critically, despite the fact that participants under-estimated the persuasion goals held by their conversation partners, we still found a significant interaction between target (other vs. self) and goal type (learning vs. persuasion), $b = 0.50$, $95\%CI[0.32, 0.67]$, $p < .001$. Figure 12 presents these data, which reveal that participants under-estimated their disagreeing counterpart's learning goals to a greater extent than their persuasion goals. Of note, this effect did not differ for Republicans versus Democrats (see Supplementary Materials in Appendix B for full results). Thus, even in the context of a synchronous conversation in which counterparts had already undertaken an entire interaction, they continued to under-estimate the learning goals of their partner, and did so to a greater extent than they under-estimated the persuasion goals of their partner.

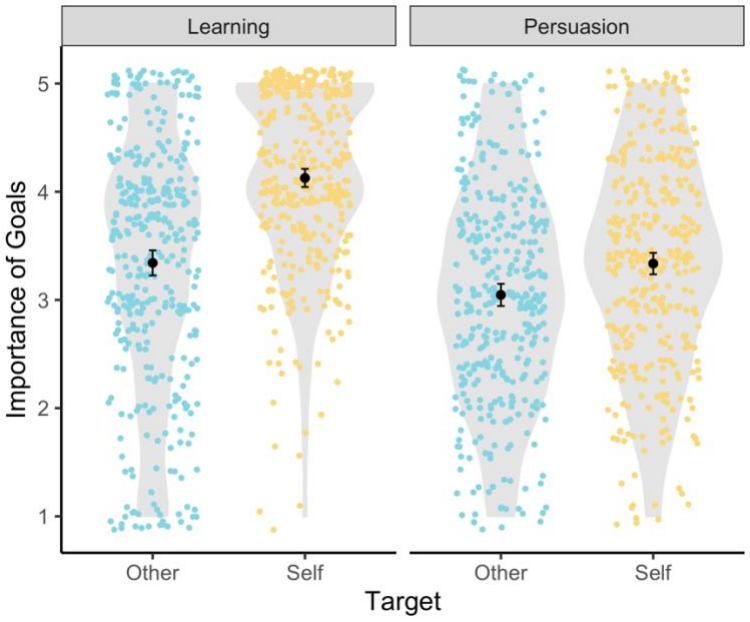

Figure 12. Comparison of the post-conversation goals reported for the self and a disagreeing counterpart in Study 2. Participants systematically under-estimated the learning goals reported by disagreeing others compared to the self even after engaging in a synchronous conversation, but this pattern did not generalize to persuasion goals. Shaded plots display the distributions; error bars represent 95% CI around the mean; colored dots represent raw data.

What are the consequences of perceived learning goals? Although participants dramatically under-estimated their counterpart's desire to learn about their perspective, there was also considerable variance in these perceptions—the standard deviation was 1.13 on a 5-point scale, and ratings spanned the entire length of the scale, with perceived learning goals ranging from 1 to 5.

As described in detail below, results revealed that actor perceptions of partner learning goals robustly predict two key conversational outcomes: actors' evaluations of their partners and actors' conversational enjoyment. To obtain a global measure of actors' evaluations of their partners, we averaged participant ratings of their partner's objectivity, intelligence, likability, morality and trustworthiness ($\alpha = .89$; all results replicate for each individual dimension of

person perception, see Supplemental Material in Appendix B for details). We similarly averaged participant ratings of enjoyment, pleasantness and aversiveness, to obtain a measure of conversational enjoyment ($\alpha = .77$). For each outcome, we took a three-step analytic approach (detailed in Figure 13), fitting mixed-effects models with a random effect for dyad, and various fixed-effects specified below. We detail all results below and in Table 11.

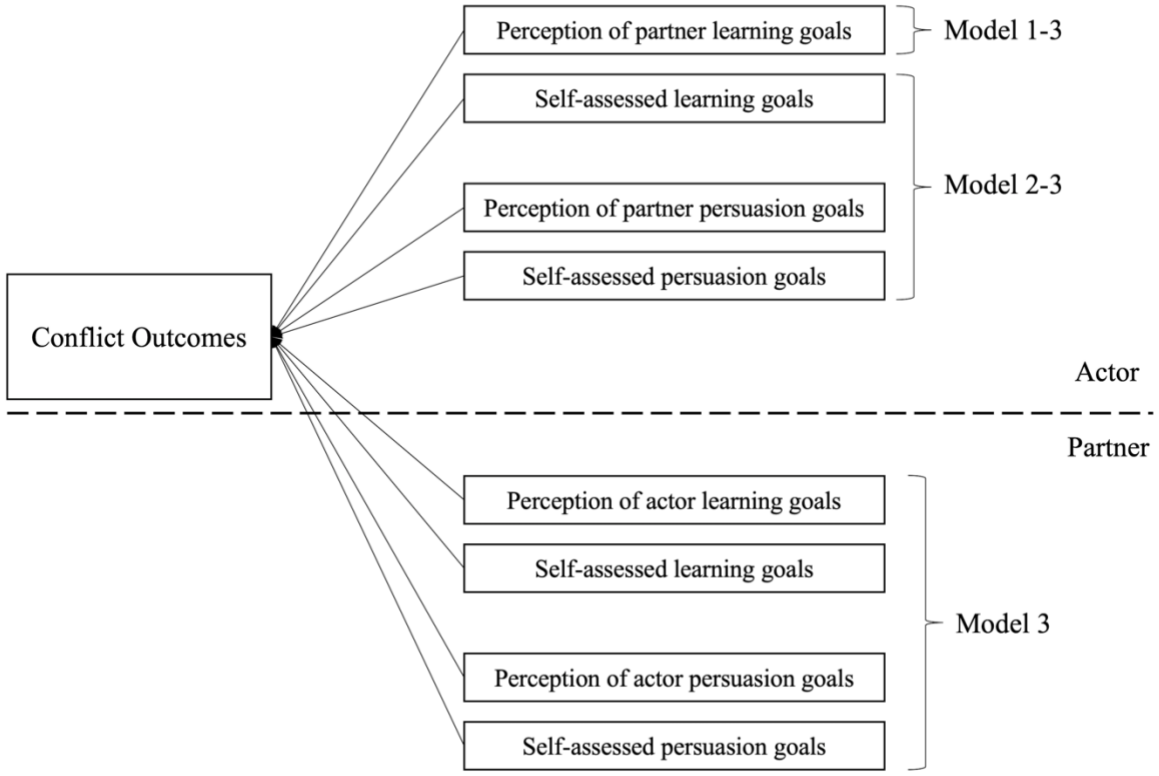

Figure 13. Three-step analytic approach used to investigate actor and partner effects in determining conversational enjoyment and partner evaluations.

Actor Perceptions of Partner Learning Goals. First, we assessed the relationship between perceived learning goals and conflict outcomes. Thus, in Model 1, we predicted conflict outcomes from *actor perceptions of partner learning goals*. Results showed a significant positive

relationship between actor perceptions of partner learning goals and actor's enjoyment of the conversation, $b = 0.70$, *95%CI*[0.58, 0.81], $p < .001$. Additionally, actors evaluated their conversation partners more positively when they perceived their partners had greater learning goals, $b = 0.61$, *95%CI*[0.51, 0.71], $p < .001$. Thus, perceptions of counterpart learning goals were positively associated with conflict outcomes.

Actor Effects. Second, we sought to understand the role of "actor" effects in determining conflict outcomes. Namely, we investigated how each of the various goals rated by *actors* related to their own experiences of the conversation. Thus, in Model 2, we predicted conflict outcomes from four variables: (1) *actor perceptions of partner learning goals*, (2) *actor self-assessed learning goals*, (3) *actor perceptions of partner persuasion goals*, and (4) *actor self-assessed persuasion goals*. Given that all regression results for all models are available in Table 11, we provide only a summary of the most important results in the main text.

Results were similar for both conversational enjoyment and evaluations of counterparts. When controlling for all of these covariates, we found that actor perceptions of partner learning goals were significantly positively related to conversation enjoyment, $b = 0.72$, *95%CI*[0.60, 0.84], $p < .001$. When we compared the magnitude of this fixed-effect coefficient to that of the other three predictors, we found that actor perceptions of partner learning goals were significantly more predictive—and over three times the magnitude—of conversational enjoyment than the other three actor effects (all p's $< .001$). A similar pattern arose for actor evaluations of counterparts. When controlling for all of these covariates, actor perceptions of partner persuasion goals were significantly positively related to actor evaluations of their counterpart, $b = 0.65$, *95%CI*[0.54, 0.75], $p < .001$. Last, when we again compared the magnitude of the fixed-effects coefficients, we found that actor perceptions of partner learning

goals were significantly more predictive—and over two times the magnitude—of counterpart evaluations than the other three actor effects (all *p*'s < .001).

Partner Effects. Finally, we wanted to understand the role of "partner" effects in determining outcomes. In other words, we investigated how each of the various goals rated by *partners* related to actor's experiences of the conversation. Thus, Model 3 was a fully saturated model in which we predicted conflict outcomes from the actor variables included in Model 2 as well as the following four additional variables: (5) *partner perceptions of actor learning goals*, (6) *partner self-assessed learning goals*, (7) *partner perceptions of actor persuasion goals*, and (8) *partner self-assessed persuasion goals*. Given that all regression results are available in Table 11, we again only summarize the key results below.

Results again looked similar for both conversational enjoyment and evaluations of counterparts. Even in this fully saturated model, considering all "actor" and "partner" effects, actor perceptions of partner learning goals were significantly positively related to conversational enjoyment, $b = 0.65$, *95%CI*[0.53, 0.77], $p < .001$. When we compared the magnitude of all fixed-effects coefficients, we found that actor perceptions of partner learning goals were significantly more predictive of enjoyment than all other goals (all *p*'s < .001). We want to highlight two additional points of interest. First, and perhaps most interesting, in this model we found that actor perceptions of partner persuasion goals showed a much stronger—approximately six times stronger—association with conversational enjoyment than partner's self-assessed learning goals (which showed no relationship, $b = 0.10$, *95%CI*[-0.06, 0.26], $p = .23$). These results emphasize the particular importance of *perceived learning goals*—regardless of whether their conversation counterparts endorsed the goal to learn about their views, it was actor *perceptions* of these goals that determined outcomes. Second, it is important to note that actor

perceptions of partner *persuasion* goals showed no relationship with their conversational enjoyment, b = -0.09, *95%CI*[-0.23, 0.05], p = .19—highlighting that perceived *learning* (and not persuasion) goals play a unique role in determining conflict outcomes.

Similarly, actor perceptions of partner learning goals were significantly positively related to counterpart evaluations, b = 0.59, *95%CI*[0.48, 0.69], $p < .001$. As before, when we compared the magnitude of all of these fixed-effects coefficients, we found that actor perceptions of partner learning goals were significantly more predictive of enjoyment than all other goals (all p's < .001). Again, we highlight two points of interest. First, we again found that partner self-assessed learning goals showed no relationship with partner evaluations, b = 0.11, *95%CI*[-0.03, 0.26], p = .13—while actor perceptions of partner learning goals showed a significantly stronger relationship (approximately six times larger) with counterpart evaluations. Again, this finding emphasizes the particular importance of *perceived learning goals* in conflict outcomes—above and beyond the goals that conflict counterparts self-report. Second, we again found that actor perceptions of partner *persuasion* goals showed no relationship with partner evaluations, b = 0.0009, *95%CI*[-0.13, 0.13], p = .99—once again emphasizing that these effects do not extend to persuasion goals.

Overall, these results point to the unique importance of *perceptions of a counterpart's willingness to learn* for conflict outcomes—actor perceptions of partner learning goals were the single most important predictor of conflict outcomes (while partner *self-assessed* learning goals, and actor perceptions of partner *persuasion* goals showed no relationship).

Table 11. Actor and Partner Effects in Predicting Conversational Enjoyment and Counterpart Evaluations.

	MODEL 1	**MODEL 2**	**MODEL 3**
		Enjoyment	
Actor perceptions of partner learning goals	*b* = 0.70, 9*5%CI[*0.58, 0.81]***	*b* = 0.72, *95%CI*[0.60, 0.84]***	*b* = 0.65, *95%CI*[0.53, 0.77]***
Actor self-assessed learning goals		*b* = 0.22, *95%CI*[0.06, 0.38]**	*b* = 0.27, *95%CI*[0.11, 0.43]**
Actor perceptions of partner persuasion goals		*b* = -0.13, *95%CI*[-0.26, 0.005]†	*b* = -0.09, *95%CI*[-0.23, 0.05]
Actor self-assessed persuasion goals		*b* = -0.22, *95%CI*[-0.36, -0.08]**	*b* = -0.15, *95%CI*[-0.29, -0.006]*
Partner perceptions of actor learning goals			*b* = 0.19, *95%CI*[0.07, 0.31]**
Partner self-assessed learning goals			*b* = 0.10, *95%CI*[-0.06, 0.26]
Partner perceptions of actor persuasion goals			*b* = -0.07, *95%CI*[-0.21, 0.07]
Partner self-assessed persuasion goals			*b* = -0.19, *95%CI*[-0.33, -0.05]**

Table 11 (Continued). Actor and Partner Effects in Predicting Conversational Enjoyment and Counterpart Evaluations.

	MODEL 1	MODEL 2	MODEL 3
Partner Evaluations			
Actor perceptions of partner learning goals	*b* = 0.61, *95%CI*[0.51, 0.71]***	*b* = 0.65, *95%CI*[0.54, 0.75]***	*b* = 0.59, *95%CI*[0.48, 0.69]***
Actor self-assessed learning goals		*b* = 0.09, *95%CI*[-0.05, 0.24]	*b* = 0.12, *95%CI*[-0.03, 0.26]
Actor perceptions of partner persuasion goals		*b* = -0.02, *95%CI*[-0.14, 0.10]	*b* = 0.0009, *95%CI*[-0.13, 0.13]
Actor self-assessed persuasion goals		*b* = -0.27, *95%CI*[-0.39, -0.15]***	*b* = -0.24, *95%CI*[-0.37, -0.11]***
Partner perceptions of actor learning goals			*b* = 0.19, *95%CI*[0.08, 0.30]***
Partner self-assessed learning goals			*b* = 0.11, *95%CI*[-0.03, 0.26]
Partner perceptions of actor persuasion goals			*b* = -0.16, *95%CI*[-0.29, -0.03]*
Partner self-assessed persuasion goals			*b* = -0.16, *95%CI*[-0.29, -0.03]*

A Note on Political Affiliation. The three models reported above do not distinguish between actors who supported Joe Biden vs. Donald Trump. However, it could have been the case that the benefits of perceived learning goals differed across political affiliation. To test this hypothesis, we re-ran the above analyses including a term for the interaction between actor political affiliation (whether they supported the Democratic or Republican candidate) and actor perceptions of partner learning goals. We found no interaction for either conversational enjoyment or partner evaluations (see Supplementary Materials in Appendix B for details),

suggesting that perceived learning goals were an important determinant of conflict outcomes for both Democrats and Republicans.

Linguistic Markers of Learning Goals. Finally, on an exploratory basis, we analyzed the conversation transcripts to investigate the linguistic cues associated with learning goals (both self-reported and perceived). To do so, we analyzed the conversation transcripts using a combination of strategies. First, we used the politeness R package (Yeomans et al., 2018) to generate a count of the various linguistic features used by each participant in their conversation. This package uses pre-trained natural language processing (NLP) models to calculate a set of syntactic and social markers from natural language (e.g., gratitude, apologies, acknowledgment). Second, looking beyond the use of specific linguistic features, we also calculated each participant's level of conversational receptiveness (Yeomans et al., 2020) using a pre-trained natural language processing algorithm.

We present two sets of key results here, but present more in-depth analyses in the SOM. First, we were interested in understanding the linguistic cues associated with *actor perceptions of partner learning goals*. In other words, what linguistic features used by partners are associated with actors' perceptions of their learning goals? Partners perceived to place the greatest importance on learning goals (highest 33% compared to the lowest 33%) expressed significantly less negative emotion. Further, these partners scored higher in conversational receptiveness (Yeomans et al., 2020), perhaps helping to explain one possible mechanism through which the use of this conversational style has been shown to improve conflict outcomes.

Second, we were interested in the linguistic features associated with participants' self-assessed learning goals. Indeed, participants who reported the greatest learning goals (highest 33% compared to lowest 33%) asked significantly more questions. However, these individuals

did not differ in their level of conversational receptiveness (Yeomans et al., 2020), which supports previous results showing that people struggle to enact a receptive conversation style without explicit instructions on how to do so.

We did not make any specific predictions about these results, so we hesitate to make any claims about their decisiveness. However, it is interesting to note that the linguistic features associated with perceived learning goals are limited. Perhaps these goals are not effectively enacted in conversation—instead, these perceptions may be primarily based on actor internal cognitions rather that observation of partner behavior. Further, we note that the linguistic cues associated with self-reported and partner-perceived learning goals do not overlap. Thus, there is a disconnect between the linguistic features that individuals used to enact their learning goals in conversation, and those that their partners associated with a willingness to learn. Overall, it seems that counterparts struggle to effectively signal their learning goals in conversation, which may explain the persistent under-estimation of counterpart learning goals, even after a 10-minute conversation.

Discussion

After a 10-minute conflictual conversation, participants under-estimated counterparts' learning goals. Further, perceived learning goals were the single most important predictor of conversational enjoyment and partner evaluations.

Study 3

Method

Studies 1-2 provided evidence that (1) participants in conflictual conversations systematically under-estimate their counterpart's learning (but not persuasion) goals, and (2) perceptions of counterpart's learning goals are a key predictor of conversational outcomes. In

Study 3, we *manipulated* perceptions of a disagreeing partner's learning goals and asked participants to evaluate this partner. To examine the practical significance of this effect, we compared the effect of information about a partner's learning goals to the effect of belonging to the same (vs. opposing) political party.

Participants. To achieve 90% power based on effect size estimates from Studies 1a-2, we recruited 902 participants through Prolific Academic to complete a 3-minute survey. As per our pre-registration, we excluded participants who failed our attention check or reported their political orientation to be neither liberal nor conservative, leaving a total of 666 participants (47% male; M_{age} = 34.0, SD = 12.2).

Protocol. We told participants that we were planning a future study in which they would be paired up with another participant to have a 10-minute discussion about their perspectives on current hot-button issues over an online chat platform. Participants first reported their political orientation on a scale from 1: Extremely liberal, 7: Extremely conservative with 4: Neither as the mid-point (American National Election Studies, 2010). They also reported how important learning and persuasion goals would be for them in the upcoming conversation using the same 6-items as in previous studies. Then, participants were presented with information about a potential discussion partner who they were told had completed this survey in the last few days. At this point, participants were randomly assigned to one of four conditions that varied in the information they received about the potential partner.

Participants in the "agreement" condition were told they would be paired with a partner who reported the same political orientation as the participant (e.g., self-reported liberals were told this potential discussion partner was also a liberal). By contrast, participants in the "disagreement" condition, were told that their potential partner had reported the opposite

political orientation. In both of these conditions, the participants were given no information about the potential partner's goals for the conversation.

In two additional "disagreement" conditions, we also showed participants a screenshot of the potential discussion partner's self-reported learning and persuasion goals (using the same 6-items that the participants had themselves completed). Thus, in the "disagreement, high learning goals" condition, participants were told the potential discussion partner reported the opposite political orientation, but also that in the upcoming conversation they reported learning goals to be extremely important and persuasion goals to be moderately important to them. By contrast, in the "disagreement, low learning goals" condition, the potential discussion partner was presented as having the opposite political orientation, placing a minimal value on learning goals, while considering persuasion goals to be moderately important.

Our first two conditions allowed us to compare anticipated outcomes of conversations with ideologically aligned versus unaligned partners. An extensive prior literature led us to predict that participants would hold negative expectations regarding a conversation with an opposite-party partner (e.g., Dorison et al., 2019). Our third condition allowed us to evaluate how much of this antipathy could be overcome by signaling to participants that the opposite-party counterpart is interested in understanding their perspective. Finally, the fourth condition enabled us to rule out the possibility that the mere mention of learning goals improves conversational expectations.

Partner Evaluations. Participants reported how moral, objective, intelligent, trustworthy and likable they expected their partner to be during the upcoming conversation (using the same response options as in Study 2; $\alpha = .90$). We took the average of these 5-items to represent a measure of partner evaluations.

Results

First, we compared partner evaluations between participants who were paired with an agreeing vs. disagreeing counterpart, but did not receive any information about that counterpart's conversational goals. As in previous work on affective polarization (Boxell et al., 2024; Iyengar et al., 2019; Minson et al., 2020), participants derogated holders of opposing views (M = 3.95, SD = 0.97) relative to holders of aligned views (M = 4.37, SD = 0.96), $t(318.43) = 3.89$, $p < .001$, Cohen's $d = 0.44$, 95% *CI*[0.21, 0.66].

Critically, this effect was entirely reversed when participants learned about an ideological opponent who had reported high learning goals (M = 4.75, SD = 1.05). Indeed, the disagreeing counterpart with high learning goals was evaluated significantly more positively than an agreeing counterpart with no goal-related information $t(329.99) = 3.46$, $p < .001$, Cohen's $d = 0.38$, 95%*CI*[0.16, 0.60]. Thus, participants' aversion to engaging with a disagreeing other (compared to an agreeing other) appeared to be at least partially driven by their belief that they would be unwilling to learn about their views.

Finally, and in line with our predictions, participants made more negative evaluations of disagreeing others with low learning goals (M = 2.81, SD = 0.94) than agreeing counterparts, $t(316.10) = 14.71$, $p < .001$, Cohen's $d = 1.64$, *95%CI*[1.39, 1.90]—an effect size almost four times larger than the difference between agreeing and disagreeing others with no information about goals. Figure 14 presents these data. Providing information about conflict counterparts' learning goals had a significant effect on participants' evaluations of them—and could even overcome the robust effect of shared political ideology.

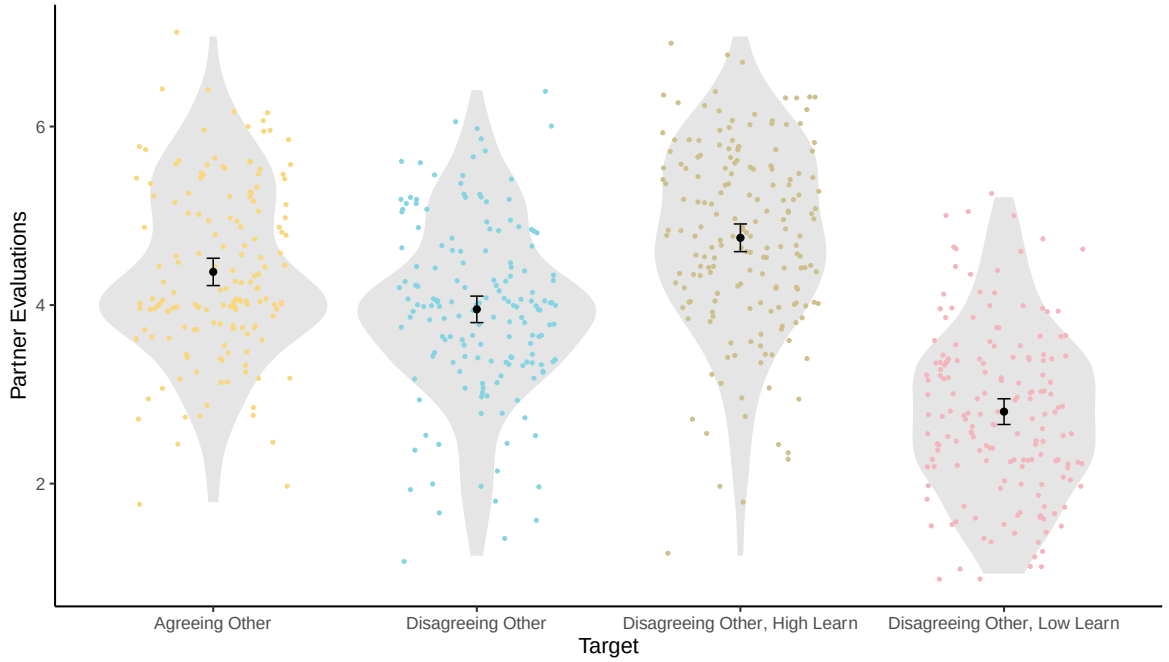

Figure 14. Evaluations of a conversation counterpart as moral, objective, intelligent, likable, and trustworthy across conditions in Study 3. Participants evaluated an agreeing other more positively than a disagreeing other when no goal relevant information was provided. However, evaluations of a disagreeing other with high learning goals was more positively than even an agreeing other (with no goal information). Shaded plots display the distributions; error bars represent 95% CI around the mean; colored dots represent raw data.

Discussion

Believing that an ideologically opposing conversation partner was willing to learn about one's perspective improved expectations for a conflictual conversation, and had a greater effect than political ideology.

Study 4

Method

Study 3 provided initial evidence that correcting people's mis-estimation of opponents' learning goals can reduce affective polarization. Could this intervention yield dividends once counterparts actually engage with each other's beliefs? Study 4 examines this possibility.

Participants. To achieve 90% power based on effect size estimates from a pilot study, we recruited 506 participants through Prolific Academic to complete a 15-minute survey. As per our pre-registration, a total of 406 participants passed both of our attention check questions and completed the full survey. Due to a coding error, we excluded data from an additional 75 participants who were matched with agreeing (rather than disagreeing) partners (final N = 331; 51% male; M_{age} = 35.3, SD = 12.6).

Protocol. We informed participants that in a future study, they would have an opportunity to discuss their views on current hot-button issues using an online chat platform with another person from today's study. Their responses to the current survey would be used to match them with discussion partners. We showed participants screen shots from ChatPlat, a popular research tool for conducting online interactions, to increase the believability of our cover story.

Participants then reported their attitudes on two policy issues—one concerning preferential hiring for women in STEM fields and the other concerning investment by the United States in fighting international terrorist organizations. They then selected the issue that they felt most strongly about and were told that this was the topic they would discuss in the upcoming study. We then asked participants about their goals for the upcoming conversation (using the same 6-items from Studies 1b-3).

Next, we presented participants with information about two potential conversation partners who "took this survey a few days ago." Participants saw the purported partner's participant number and their view on the focal policy issue (which was always the opposite of that reported by the participant). Importantly, participants also saw a screenshot of the earlier questionnaire responses of their potential partner. This served as our manipulation. Specifically, the two partners were presented as having reported either high or low learning goals with respect

to the upcoming conversation. Both partners were presented as having reported identical moderate levels of persuasion goals.

Participants viewed this information about one potential discussion partner and made several evaluations (described below). They then read a paragraph-long argument purportedly written by their future partner explaining the partner's view on the focal issue. In reality, participants were randomly assigned to view one of five opinion statements collected in a previous study written by a different sample of online participants. To collect these seed texts, we asked a sample of participants in a previous study to write a paragraph "to support your opinion on this issue. Please incorporate all the reasons and evidence you can think of to explain your point of view." Participants then evaluated the content and tone of the statement they believed to have been produced by the first partner they were evaluating. They then repeated the entire process for the second partner under consideration. Between participants, we counterbalanced the order of presenting the partner with the high versus low learning goals. Finally, participants chose which of the two partners they were more interested in talking to during the future study.

Partner Evaluations. First, participants evaluated each potential discussion partner in terms of how moral, objective, intelligent, trustworthy and likable their partner would likely to be during the conversation (using the same response options as in previous studies; $\alpha = .94$).

Content Evaluations. Next, participants read each individual's opinion statement on the assigned issue and evaluated the content of their argument. Specifically, participants reported how persuasive, relevant, true, thoughtful, and evenhanded the statement was (1: "Not at all"; 7: "Extremely"; $\alpha = .93$).

Tone Evaluations. Finally, participants evaluated the tone of the argument, reporting how warm, confrontational and respectful the argument was (1: "Not at all" to 7: "Extremely"; α = .51).

Partner Choice. After having reviewed the information about the two potential discussion partners, participants were asked to select which one they would prefer to be paired with for the upcoming study.

Results

We tested four hypotheses related to our perceived learning goals intervention: effects on partner evaluations, evaluation of the tone of the written argument, evaluation of the content of the written argument, and willingness to interact again in the future. We found beneficial effects of perceived learning goals on all four outcomes. First, we found that participants provided more positive evaluations (calculated as the average of morality, objectivity, intelligence, trustworthiness, and likability) when evaluating a counterpart who had reported high (M = 5.00, SD = 1.04) versus low (M = 3.17, SD = 1.29) learning goals, $t(330) = 22.31$, $p < .001$, Cohen's $d = 1.57$, *95%CI*[1.39, 1.74] (Figure 15, Panel (a)). Second, when evaluating identical counter-attitudinal arguments, participants provided more positive content evaluations (the average of persuasiveness, relevance, truthfulness, thoughtfulness, and evenhandedness) when they believed the person who wrote the argument reported high (M = 3.38, SD = 1.01) rather than low (M = 3.13, SD = 0.88) learning goals, $t(330) = 6.70$, $p < .001$, Cohen's $d = 0.26$, *95%CI*[0.11, 0.42] (Figure 16, Panel (a)). Third, participants evaluated the tone of the argument more positively (warm, confrontational (R), respectful) when they believed the statement was written by someone with high (M = 3.46, SD = 0.83) rather than low (M = 2.88, SD = 0.85) learning goals, $t(330) = 9.95$, $p < .001$, Cohen's $d = 0.69$, *95%CI*[0.53, 0.85] (Figure 17, Panel (a)). These

results held for each of the individual measures and were not moderated by topic of discussion or attitude strength. Finally, participants were overwhelmingly more likely to choose to have a discussion with the disagreeing counterpart who endorsed high (78%) rather than low (22%) learning goals, $\chi^2(1) = 102.59$, $p < .001$.

Taken together, these results provided evidence that explicitly providing individuals with information about a conflict counterpart's willingness to learn about them is a simple, scalable intervention that reduces affective polarization and increases people's willingness to engage with opposing views.

Discussion

Informing participants that their counterpart was interested in learning about their perspective again enhanced their counterpart evaluations, as well as their evaluations of the counterpart's arguments (though the arguments themselves were identical).

Study 5

Method

The earlier studies document a robust self-other difference in people's beliefs about willingness to learn about opposing views, and tested a simple intervention to correct this mis-understanding, improving evaluations of both disagreeing others and their arguments. Next, we investigate whether such effects generalize outside of American partisan politics by testing our intervention in the context of the long-standing and bloody Israeli-Palestinian conflict.

Participants. We recruited 632 Hebrew-speaking Israeli citizens. We pre-registered a sample of 600 respondents (in order to achieve 90% power based on effect size estimates from Study 4), but ongoing recruitment through snowball sampling led to a final sample of 632 responses. Participants were recruited through online platforms (Prolific, n = 398; Cloud

Research Panel, n = 140) and 94 volunteer participants belonging to the professional and personal networks of the authors. As per our pre-registration, a total of 356 participants passed our attention check and completed the full survey (45% male; M_{age} = 31.4, SD = 11.4). This survey was conducted in Hebrew.

Protocol. As in Study 4, participants were informed that the survey would be used to pair them up with someone for a potential future study, in which they would discuss a controversial issue over an online chat platform. Specifically, participants were asked to state their agreement with the following statement in Hebrew: "To achieve a lasting peace agreement between Israel and the Palestinians, it will be necessary to address the Palestinian refugees' claims by internationally funded compensation and by their resettlement outside Israel's borders." Participants then reported their opinion on the issue and their goals for the upcoming conversation (using the same 6-items from Studies 1b-4).

To reduce the burden on our participants and test methodological generalizability, we used a between-subjects design (rather than the within-subjects design used in Study 4) in which participants considered a single Palestinian discussion partner who reported either high or low learning goals. Thus, we presented participants with information about one potential conversation partner who "took this survey a few days ago." They viewed this purported partner's name ("Ziad El Hamid") and their view on the issue ("Strongly Disagree"). Importantly, participants also saw a screenshot of the earlier questionnaire responses of their potential partner indicating either high or low learning goals for the upcoming conversation. All partners were again presented as having identical moderately high persuasion goals.

As in Study 4, participants evaluated their discussion partner on several dimensions. Specifically, they rated how moral, objective, intelligent, trustworthy and likable their partner

was likely to be during the conversation (using the same response options as in previous studies; $\alpha = .87$). We again combined these items into a single measure of partner evaluations.

Participants then read a paragraph-long argument purportedly written by this future discussion partner explaining their view on the issue. In reality, we used opinion statements that had been collected in a previous study. To collect these seed texts, we asked a sample of Palestinian residents of the West Bank to write a paragraph to "explain your reasons for your opinion" such that another person could "understand your reasoning and your beliefs." All the statements came from Palestinian respondents who strongly disagreed with the focal statement. Across both conditions, the Israeli participants in the current study were randomly assigned to view one of five such statements. Participants then evaluated the content (persuasive, relevant, true, thoughtful, and evenhanded on a scale from 1: "Not at all" to 7: "Extremely"; $\alpha = .88$) and tone (warm, confrontational, and respectful on a scale from 1: "Not at all" to 7: "Extremely"; $\alpha = .70$) of the argument purportedly written by their potential future conversation partner.

Results

Although Study 5 was conducted in a different language, cultural context, and in midst of a long-standing and bloody conflict, we found results that were largely in line with those of Study 4. Participants evaluated a potential conflict counterpart as more moral, objective, intelligent, trustworthy, and likable when they reported high (M = 4.4,0 SD = 0.95) rather than low (M = 2.90, SD = 0.96) learning goals, $t(350.66) = 14.77$, $p < .001$, Cohen's $d = 1.57$, *95%CI*[1.33, 1.81] (Figure 15, Panel (b)).

Further, when evaluating identical arguments about one of the most painful issues fueling the conflict, participants provided more positive evaluations of the argument tone (warm, confrontational (R), respectful) when they believed the author had high (M = 2.71, SD = 0.92)

versus low (M = 2.43, SD = 0.80) learning goals, $t(335.02) = 3.01$, $p = 0.003$, Cohen's $d = 0.32$, *95% CI*[0.11, 0.53] (Figure 16, Panel (b)).

Although differences in our measure of argument content did not reach traditional levels of significance, the difference was directionally in line with our predictions (high learning goals: M = 2.47, SD = 0.97; low learning goals: M = 2.30, SD = 0.99; $t(351.38) = 1.64$, $p = 0.10$, Cohen's $d = 0.17$, *95% CI*[-0.04, 0.38]; Figure 17, Panel (b)). Specifically, participants rated the argument as significantly more thoughtful, but not significantly more persuasive (trending), relevant, true, or evenhanded.

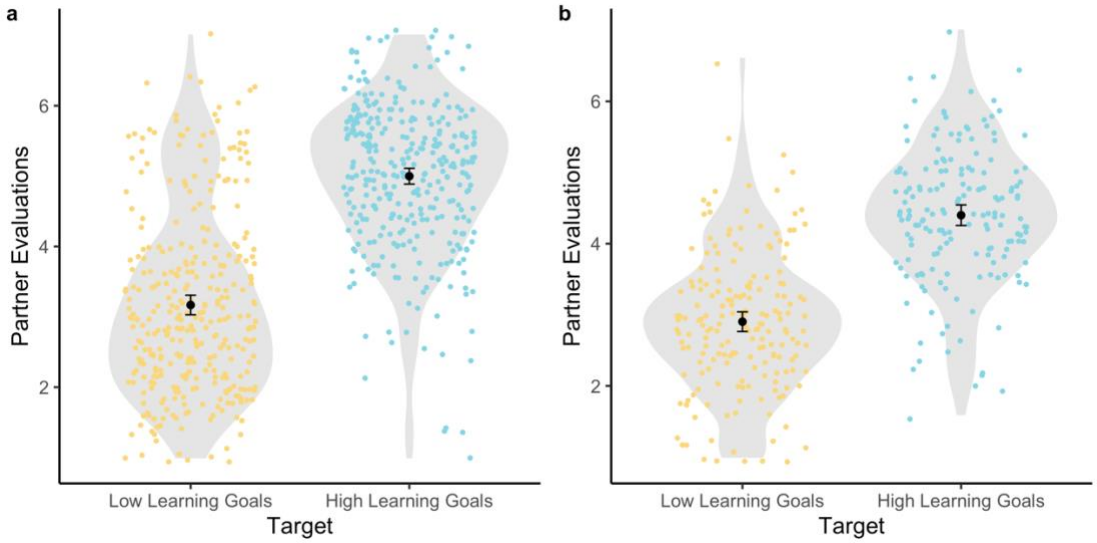

Figure 15. Evaluations of a conversation counterpart as moral, objective, intelligent, likable, and trustworthy depending on perceived learning goals in (a) Study 4 and (b) Study 5. Participants evaluated a disagreeing other more positively when they were told they had high (vs. low) learning goals. Shaded plots display the distributions; error bars represent 95% CI around the mean; colored dots represent raw data.

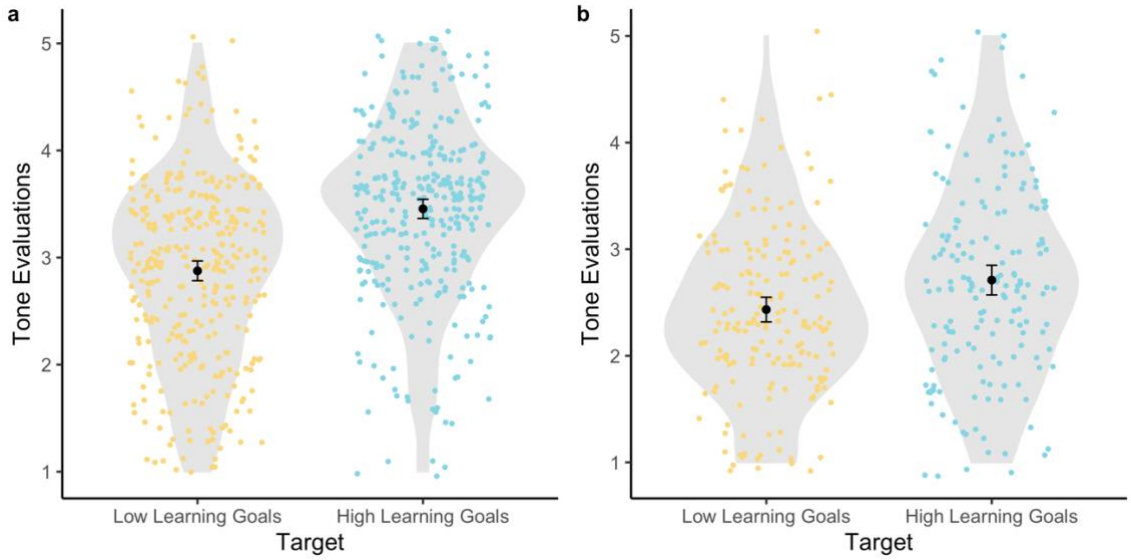

Figure 16. Evaluations of a conversation counterpart's argument as warm, confrontational, and respectful depending on perceived learning goals in (a) Study 4 and (b) Study 5. Participants evaluated the tone of a disagreeing other's argument more positively when they were told they had high (vs. low) learning goals. Shaded plots display the distributions; error bars represent 95% CI around the mean; colored dots represent raw data.

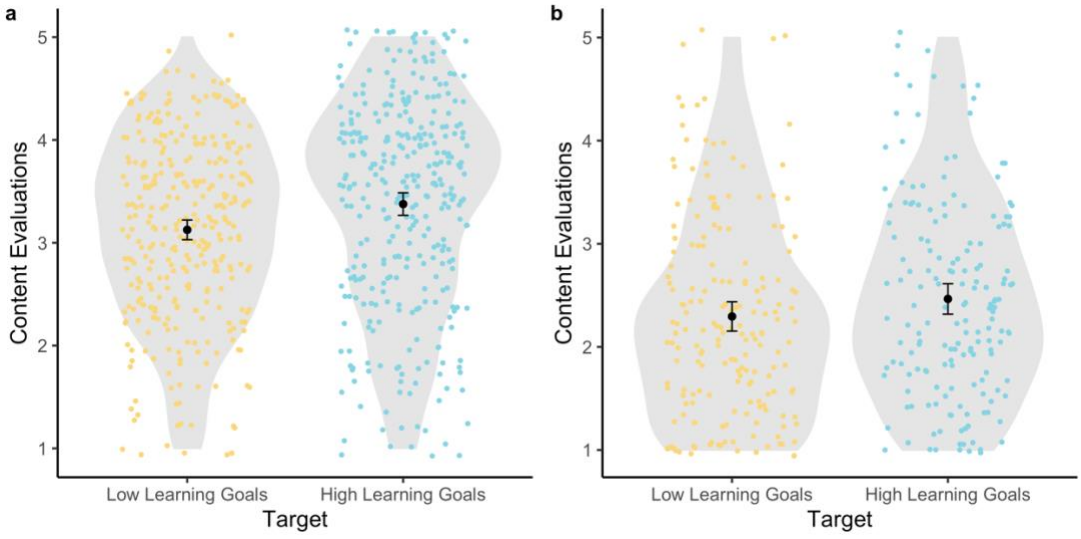

Figure 17. Evaluations of a conversation counterpart's argument as persuasive, relevant, true, thoughtful, and evenhanded depending on perceived learning goals in (a) Study 4 and (b) Study 5. Participants did not evaluate the content of a disagreeing other's argument differently depending on whether they had high (vs. low) learning goals. Shaded plots display the distributions; error bars represent 95% CI around the mean; colored dots represent raw data.

General Discussion

Across seven pre-registered studies, we document three findings. First, we identify a robust self-other difference, wherein conflict participants believe that counterparts are less willing to learn about their views than vice versa. Second, these beliefs predict how people evaluate counterparts, and their experiences with them. Third, manipulating beliefs about counterpart's learning goals improves conflict outcomes. In both American partisan politics and the Israeli-Palestinian conflict, counterparts and their arguments were evaluated more positively when participants believed that their counterpart was eager to learn about their perspective.

Contribution

Social psychology has a rich history of highlighting the role of situational forces in determining human behavior. In dyadic conflict, the social situation has one overwhelmingly salient feature: the other person. We build on the tradition of recognizing the power of the situation (Ross & Nisbett, 2011) and individual construal in shaping behavior.

Complementing prior work on the importance of *individual* attributes in determining conflict outcomes—e.g., receptiveness (Minson et al., 2020)—our results highlight the importance of individuals' *beliefs about others.* This shift in focus provides a new lens for conflict research. Indeed, the results of Studies 4-5 suggest that clearly signaling learning goals (e.g., "I would be interested to learn what you think about…") could lead to more productive dialogue.

Limitations and Future Directions

Our paper has limitations that offer avenues for future research. Our work relied primarily on online samples (Mturk and Prolific). Additional research should test the generalizability of these results to various samples, across various domains of attitude conflict—

perhaps even in conversations when no conflict is present. In doing so, future work could investigate *why* these effects are particularly robust in political disagreements.

Second, research should investigate *how* learning goals can be communicated. Individuals reported higher learning goals than their partners attributed to them even after a conversation, suggesting a breakdown in communication. Interestingly, our results find preliminary evidence that the linguistic cues that people associate with learning goals in others are not the same as the ones they employ themselves. Why does this mismatch occur? Could one's goals be communicated more directly? Future research could examine these questions.

Third, we rely on self-report measures to assess goals. People may be over-estimating their own willingness to learn (and accurately assessing the willingness of disagreeing others), rather than under-estimating their counterpart's willingness to learn. Future research should seek to precisely identify the source of the self-other difference.

Finally, given the critical importance of perceived learning goals for conflict outcomes, we focused primarily on learning goals in conflict. However, future research should further investigate persuasion goals—especially given that we observed inconsistencies in the direction of this self-other difference *before* (i.e., Study 1a-1c) versus *after* (i.e., Study 2) a conflictual conversation. Perhaps attitude conflict is less aversive than anticipated (Dorison et al., 2019) because people are faced with fewer persuasion attempts than they expect. Additionally, future research should further examine the correlation between one's level of learning and persuasion goals.

Conclusion

We shed light on a novel misprediction—the belief that disagreeing others are unwilling to learn about our views—with important consequences for conflict outcomes. In doing so, we

shift away from a focus on individual behavior and cognitions that determine conflict outcomes and towards a greater focus on person perception. Practically, these results also suggest a simple intervention to improving disagreeing conversations that calls for broader testing and potential implementation.

GENERAL DISCUSSION

My dissertation introduces a novel conceptualization of *conversational listening* as a three-stage process that includes both *intra*personal cognitive processes and *inter*personal behavioral processes—both of which are critical to high-quality listening. This model not only integrates previous work across fields, but also grounds conversational listening as a multi-stage process that is enacted through people's behavior in conversation. Critically, this means that people can meaningfully alter their behavior to improve the extent to which their conversation partners are both *being* and *feeling* heard.

Next, I present empirical evidence that interpersonal perceptions of listening in conversation (the extent to which people are *feeling* heard) do not always align with listeners' cognitive engagement (the extent to which people are *being* heard). It turns out there are many times in our conversations when we are *feeling* heard and not *being* heard (and vice versa). Drawing from my theoretical model of conversational listening, I propose (and find evidence) that these misperceptions are due, in part, to a lack of diagnostic behavioral signals of listening: most behavioral cues that past theory and peoples' lay beliefs suggest are indicative of good listening are effectively feigned by inattentive listeners. The behavioral cues that people seem rely on are primarily non-verbal (e.g., eye contact, nodding) and paralinguistic (e.g., back-channels, pitch)—which, for the most part, do not rely on conversation content and thus are often invalid signals of attentiveness. For this reason, verbal signals of listening, such as call-backs or follow-up questions (which draw directly on conversation content), may indeed be the most *valid* cues of attentive listening.

Finally, I explore these themes in the context of interpersonal attitude conflict. I find that people consistently under-estimate the extent to which disagreeing others are willing to listen to

and learn about their views in conflictual conversations. When anticipating a conflictual conversation, people expect their counterpart to be more focused on persuading them than listening to and learning about their views. These beliefs are negatively associated with people's evaluations of a disagreeing other and their views on the issue at hand. However, when we intervene to correct these misperceptions—by telling individuals that their counterpart expressed an interest in listening to and learning about their views on the issue—we improved their evaluations of this individual as well as the content and tone of their written arguments and increased their willingness to engage with them in the future.

Overall, the arguments and empirical evidence presented in my dissertation suggest that conversational listening is an important and uniquely complex social process. Listening begins in the privacy of our minds, but the benefits of good conversational listening require that the cognitive effort we expend to *attend* to and *process* what our conversation partners say and do is effectively *expressed* to our interaction partners. Luckily, this can be done through a variety of conversation behaviors (non-verbal, paralinguistic, and verbal cues)—but some of these cues (primarily non-verbal and paralinguistic cues) can (and are) effectively feigned by inattentive listeners to appear as though they are listening. Thus, more effectively conveying and detecting high-quality conversational listening may require that we refocus our attention and effort on the use of *verbal* expressions of listening (such as paraphrasing, follow-up questions, and call-backs). Doing so may prove to be a powerful tool for building meaningful social connections as well as for navigating interpersonal attitude conflict.

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APPENDIX A

Chapter 2 Supplemental Material

Exploratory Study

Supplemental Method

Post-Conversation Measures. After their 25-minute conversation, participants completed a brief survey in which they reflected on their conversation. Specifically, they responded to measures of listening, liking, and conversational enjoyment.

Listening. Participants provided post-hoc evaluations of their own (in the 'target' condition; $\alpha = 0.89$) or their partner's (in the 'perceiver' condition; $\alpha = 0.91$) listening. They responded to five items: (1) [I/My partner] was a good listener, (2) [I/My partner] was engaged in the conversation, (3) [I/My partner] made [my partner/me] feel heard, (4) [I/My partner] made [my partner/me] feel validated, (5) [My partner/I] felt that [I/my partner] cared about [them/me] (1: *Strongly Disagree*, 4: *Neither agree nor disagree*, 7: *Strongly Agree*).

Liking. Participants reported how much they liked their partner using a 4-item scale (Boothby et al., 2018; 1: *Strongly Disagree*, 4: *Neither agree nor disagree*, 7: *Strongly Agree*). The items included: (1) I generally liked my partner, (2) I would be interested in getting to know my partner better, (3) If given the chance, I would like to interact with the other participant again, (4) I could see myself becoming friends with the other participant ($\alpha = 0.93$).

Enjoyment. Finally, participants reported their enjoyment of the conversation with their partner using three items (Huang et al., 2017; 1: *Strongly Disagree*, 4: *Neither agree nor disagree*, 7: *Strongly Agree*). The items included: (1) I enjoyed this conversation, (2) I thought this conversation was engaging, (3) I had an interesting conversation with this person ($\alpha = 0.95$).

Supplemental Results

In our second pre-registered analysis, we conducted a binary logistic regression to predict target responses (actual listening) from a fixed effect for perceiver guesses (values recoded such that 0=*[My/my partner's] mind was inattentive*, 1= *Yes, [I/my partner] was fully attentive*) and time-point (which of the 5 ratings), and a random effect for dyad to account for repeated observations (Bates et al., 2015). We found a positive association between perceiver ratings and target self-reports, b = 0.49, *95% CI* [0.03, 0.96], SE = 0.49, p = .04, and a negative effect of time, b = -0.16, *95% CI* [-0.28, -0.05], SE = 0.06, p = .006 (see Figure 2). Translating these into an odds ratio, these results suggest that perceivers were 1.64 times more likely to report that the target was listening to them than the target was to report they were indeed listening.

To assess the correlation between perceiver and target reports of listening, we conducted a linear regression predicting target self-reports of listening from a fixed-effect for perceiver ratings, and a random effect for dyad to account for repeated observations (Bates et al., 2015).[13] Results showed a small positive correlation between perceiver and target ratings of listening, b = 0.09, *95% CI* [0.02, 0.16], SE = 0.04, p = .02, but perceiver ratings only accounted for 0.6% of the variance In target ratings (marginal R^2 = 0.0059; Vonesh et al., 1996).

Post-Conversation Measures. Though ratings of listening diverged *during* the conversation, differences in post-conversation ratings of listening (a composite of our five items) were not as dramatic, perhaps reflecting the targets' motivation to self-present as a good listener (M_{target} = 5.93, SE_{target} = 0.09; $M_{perceiver}$ = 6.03, $SE_{perceiver}$ = 0.09; b = 0.10, *95% CI* [-0.14, 0.35], SE = 0.12, p = 0.40). This remained true when we looked at single-items measures that may more directly measure cognitive attention: "good listener" (M_{target} = 6.05, SE_{target} = 0.10; $M_{perceiver}$ = 6.27, $SE_{perceiver}$ = 0.10; b = 0.22, *95% CI* [-0.04, 0.48], SE = 0.13, p = 0.10) and "engaged in the

[13] Recent work has found that linear regression yields unbiased estimates of effects on binary outcomes (Gomila, 2021).

conversation" (M_{target} = 6.29, SE_{target} = 0.10; $M_{perceiver}$ = 6.27, $SE_{perceiver}$ = 0.10; b = -0.02, *95% CI* [-0.27, 0.23], SE = 0.13, p = 0.88). We found no significant interaction with gender, b = -0.24, *95% CI* [-0.78, 0.30], SE = 0.28, p = 0.38 (women vs. men).

We also found a strong positive relationship between post-conversation ratings of listening (self-reports for *targets*; perceptions for *perceivers*) and liking (b = 0.73, *95% CI* [0.56, 0.91], SE = 0.09, p < .001)—though this was qualified by a significant interaction with role such that the relationship was stronger between perceived listening and liking of one's partner than between self-reported listening and liking of one's partner (b = 0.42, *95% CI* [0.07, 0.76], SE = 0.18, p = .02). Similarly, we find a strong positive relationship between post-conversation ratings of listening and enjoyment for targets and perceivers alike (b = 0.98, *95% CI* [0.81, 1.16], SE = 0.09, p < .001; and a non-significant interaction with role: b = 0.25, *95% CI* [-0.08, 0.58], SE = 0.17, p = .13).

Gender Differences for Main Effects

On an exploratory basis, we investigated gender differences in our main effects (comparing men and women because the number of participants identifying as "Non-binary/Other" was too small to power our statistical analysis, n = 6). Gender effects could influence our results either at the level of the listener or the perceiver—however, we find no evidence that accuracy of listening perceptions differed for men and women at either level. Indeed, perceiver guesses matched targets' self-reports of listening 70% of the time when listeners were women, and 69% of the time when listeners were men. Additionally, when perceivers were women, their guesses matched targets' self-reports 70% of the time while when the perceiver was a man, their guesses matched 69% of the time.

The misalignment between perceptions and target self-reports of listening were directionally biased in the same direction—there were significantly more Type I errors (perceiver thought target was listening when target reported mind-wandering) than Type II errors (perceiver reported target was not listening when target reported listening attentively) when the listener was a woman (77% Type I error; 14% Type II error) vs. a man (80% Type I error; 18% Type II error), and when the perceiver was a woman (76% Type I error; 17% Type II error) vs. a man (80% Type I error; 13% Type II error).

Experiment 1

Gender Differences for Main Effects

We explored whether there were gender differences in our main effect (again, comparing men and women because we did not have a large enough sample size to compare to individuals who identified as "Non-binary/Other", n = 3). First, we investigated whether perceptions of listening across the three experimental conditions varied as a factor of whether the *perceiver* was a man vs. a woman. We found no significant interaction between condition and the gender of the unmanipulated participant for perceptions of listening (Listening vs. Distracted*:* $b_{interaction}$ = -0.42, *95% CI* [-1.30, 0.46], *SE* = 0.44, p = .34; Listening vs. Feigned Listening: $b_{interaction}$ = -0.65, *95% CI* [-1.45, 0.16], *SE* = 0.41, p = .12; Distracted vs. Feigned Listening: $b_{interaction}$ = -0.22, *95% CI* [-1.10, 0.65], *SE* = 0.44, p = .61).

Further, we investigated whether perceptions of listening across the three experimental conditions were affected by the gender of the *listener*. Again, we found no significant interaction between condition and the gender of the manipulated participant for perceptions of listening (Listening vs. Distracted*:* $b_{interaction}$ = 0.46, *95% CI* [-0.38, 1.30], *SE* = 0.43, p = .28; Listening vs.

Feigned Listening: $b_{interaction}$ = -0.04, *95% CI* [-0.82, 0.75], *SE* = 0.40, *p* = .93; Distracted vs. Feigned Listening: $b_{interaction}$ = -0.50, *95% CI* [-1.32, 0.33], *SE* = 0.42, *p* = .24).

Experiment 2

Gender Differences for Main Effects

We explored whether there were differences in the behaviors that manipulated listeners in Experiment 1 enacted during their conversations based on their gender across the three conditions. Interaction analyses revealed differences between men and women across conditions for verbal interruptions (increases in verbal interruptions in the feigned listening vs. listening condition was greater for men than women), looking away (increases in looking away in the feigned listening vs. listening condition was greater for men than women), leaning forward (increases in leaning forward in the distracted vs. listening condition was greater for men than women), and leaning backward (increases in leaning backward in the feigned listening vs listening condition was larger for men than women). There were no differences on any of the other behaviors. All results are included in the table below.

Finally, the results showed no differences in how accurately third-party observers were able to identify listeners' assigned condition for men vs. women: 16% were correct for men and 20% for women, b_{gender} = -0.18, SE = 0.45, *95%CI* [-1.10, 0.71], *p* = .68.

Table A1. Frequency of Various Behavioral Listening Cues Across Conditions by Gender.

	Attentive Listening		Feigned Listening		Distracted	
	M (SE)		*M (SE)*		*M (SE)*	
Behavior	Men	Women	Men	Women	Men	Women
Verbal interruptions	**1.56 (0.10)**	***1.86 (0.10)***	**2.05 (0.09)**	***1.89 (0.10)***	1.74 (0.13)	1.71 (0.08)
Verbal affirmations	2.98 (0.10)	3.05 (0.08)	2.87 (0.09)	2.84 (0.10)	2.66 (0.13)	2.97 (0.08)
Nodding	3.14 (0.11)	3.04 (0.09)	2.88 (0.10)	2.81 (0.11)	2.72 (0.14)	2.95 (0.09)
Eye Contact	3.46 (0.09)	3.43 (0.08)	3.46 (0.08)	3.49 (0.09)	3.50 (0.12)	3.46 (0.07)
Looking Away	**1.84 (0.11)**	***2.15 (0.10)***	**2.22 (0.10)**	***1.93 (0.12)***	2.19 (0.15)	2.09 (0.10)
Smiling	3.04 (0.12)	3.14 (0.10)	2.92 (0.11)	3.14 (0.12)	3.15 (0.16)	3.25 (0.10)
Fidgeting	2.56 (0.14)	2.61 (0.12)	2.79 (0.12)	2.63 (0.14)	2.50 (0.18)	2.46 (0.11)
Leaning forward	**1.68 (0.14)**	***2.06 (0.12)***	1.97 (0.12)	2.10 (0.14)	**2.03 (0.19)**	***1.76 (0.11)***
Leaning backwards	**1.41 (0.12)**	***1.75 (0.10)***	**1.84 (0.10)**	***1.67 (0.12)***	1.59 (0.16)	1.53 (0.10)

Table A1 Note. Significant interaction effects are identified with a combination of bold and italics. Differences between bolded means in the same row are significantly different from the differences between bolded and italicized numbers.

Experiment 3

Gender Differences for Main Effects

We explored whether there were differences between men and women (no other gender-identities were present in our demographic data) in their ability to identify their own listening. Results showed that men and women did not differ in their accuracy. Men correctly guessed their listening on 63% of trials (30% when listening attentively to the story; 33% when listening

inattentively to the story), while women correctly guessed their listening on 63% of trials (31% when listening attentively to the story; 32% when listening inattentively to the story).

Experiment 4

Gender Differences for Main Effects

We investigated whether gender influenced our main effects (focusing on comparisons between men and women due to sample constraints, $n = 2$ participants identified as "Non-binary/Other"). Across conditions, women were perceived to be better listeners (controlling for condition and round), $b = 0.29$, SE = 0.13, *95%CI [*0.03, 0.55], $p = 0.03$, and were perceived to be significantly more responsive, $b = 0.37$, SE = 0.14, *95%CI* [0.10, 0.65], $p = .009$.

Additionally, while we find no significant interaction between listener gender and condition for ratings of perceived listening (all p's>.15), we do find a significant interaction for ratings of perceived responsiveness. Specifically, we find that the reduction in perceived responsiveness for participants in the 75% condition compared to the 0% ($b_{interaction} = -0.85$, SE = 0.40, 95%CI[-1.64, -0.05], p = 0.04) and 25% conditions ($b_{interaction} = -0.95$, SE = 0.45, 95%CI[-1.84, -0.06], p = 0.04) was greater for women than for men.

We also investigated the effect of perceiver gender on our main results. Results showed no main effect (p's>.87), or significant interaction with condition (p's>.06), for ratings of perceived listening or responsiveness.

APPENDIX B

Chapter 3 Supplemental Material

Study 2

Effects of Political Affiliation

On an exploratory basis, we investigated whether any of our results differed by participant political affiliation—namely whether they intended to vote for the Republican candidate (Donald Trump) or the Democratic candidate (Joe Biden).

Do actors under-estimate their partner's learning goals? Overall, actors reported that learning goals were less important to their partners than to themselves. We find no interaction with political affiliation (Republican vs. Democratic voters), $b = 0.04$, *95%CI*[-0.22, 0.30], $p = .74$, suggesting that both Republican and Democratic voters under-estimated their conflict partner's learning goals to the same extent (main effect of target: $b = 0.76$, *95%CI*[0.58, 0.95], $p < .001$).

Further, actors reported that persuasion goals were less important to their partners than to themselves. Again, we find no interaction with political affiliation (Republican vs. Democratic voters), $b = 0.03$, *95%CI*[-0.23, 0.28], $p = .85$, and thus, both Republican and Democratic voters under-estimated their conflict partner's persuasion goals to the same extent (main effect of target: $b = 0.28$, *95%CI*[0.09, 0.46], $p = .003$).

Finally, the under-estimation of learning goals was greater than the under-estimation of persuasion goals. Again, this effect did not interact with political affiliation (Republican vs. Democratic voters), $b = 0.02$, *95%CI*[-0.33, 0.37], $p = .92$, suggesting that both Republican and Democratic voters were especially unwilling to believe their conflict counterparts intended to

learn about their views (interaction between target and goal type: $b = 0.49$, *95%CI*[0.24, 0.73], $p < .001$).

What are the consequences of perceived learning goals? Overall, we found that actor perceptions of their partners learning goals were a key predictor of both their enjoyment in the conversation and the evaluations they made of their partner. We tested this across three models—initially with this single predictor, then adding in both "actor" and "partner" effects. We investigated the role of political affiliation in two ways. First, we included actor political affiliation (Republican or Democratic voter) as a covariate in each of these models, allowing us to investigate whether our main effect holds when we distinguish between political affiliations. Second, we investigated the interaction between actor political affiliation (Republican or Democratic voter) and actor perceptions of partner learning goals in order to investigate whether our key effect is the same across both Republican and Democratic voters.

Actor Perceptions of Partner Learning Goals. In Model 1, we predicted conflict outcomes from *actor perceptions of partner learning goals*. First, we controlled for actor political affiliation. While we found a main effect of actor political affiliation on conversational enjoyment wherein Democratic voters enjoyed the conversation significantly less than Republican voters, $b = -0.25$, *95%CI*[-0.48, -0.02], $p = .03$, our main effect remained the same. Namely, actor perceptions of partner learning goals remained a significant positive predictor of enjoyment, $b = 0.79$, *95%CI*[0.58, 0.81], $p < .001$, when controlling for actor political affiliation. Similarly, though Democratic voters made less positive evaluations of their partners than Republican votes, $b = -0.31$, *95%CI*[-0.50, -0.12], $p = .002$, actor perceptions of partner learning goals remained a significant positive predictor of partner evaluations, $b = 0.60$, *95%CI*[0.50, 0.70], $p < .001$, when controlling for political affiliation.

Next, we interacted actor political affiliation with actor perceptions of partner learning goals. We found no interaction for enjoyment of the conversation, $b = 0.02$, *95%CI*[-0.21, 0.23], $p = .89$, or partner evaluations, $b = 0.15$, *95%CI*[-0.05, 0.34], $p = .13$. Thus, the positive relationship between actor perceptions of partner learning goals and conflict outcomes was the same across both Republican and Democratic voters.

Actor Effects. In Model 2, we predicted conflict outcomes from four variables: (1) *actor perceptions of partner learning goals*, (2) *actor self-assessed learning goals*, (3) *actor perceptions of partner persuasion goals*, and (4) *actor self-assessed persuasion goals*. First, we controlled for actor political affiliation. Again, even though Democratic voters enjoyed the conversation less, $b = -0.30$, *95%CI*[-0.53, -0.07], $p = .01$, actor perceptions of partner learning goals remained a significant positive predictor of conversational enjoyment, $b = 0.72$, *95%CI*[0.60, 0.83], $p < .001$. We find this same pattern of results for partner evaluations—despite a main effect wherein Democratic actors made more negative evaluations of their partners, $b = -0.35$, *95%CI*[-0.55, -0.15], $p < .001$, actor perceptions of partner learning goals were positively related to the evaluates they made of their partners, $b = 0.64$, *95%CI*[0.54, 0.75], $p < .001$.

Next, we interacted actor political affiliation with actor perceptions of partner learning goals. We found no interaction for enjoyment of the conversation, $b = 0.05$, *95%CI*[-0.17, 0.26], $p = .68$, or partner evaluations, $b = 0.16$, *95%CI*[-0.03, 0.34], $p = .10$. Again, the positive effects of perceived learning goals on conflict outcomes persisted across different political affiliations.

Partner Effects. Model 3 was a fully saturated model in which we predicted conflict outcomes from the actor variables included in Model 2 as well as the following four additional variables: (5) *partner perceptions of actor learning goals*, (6) *partner self-assessed learning*

goals, (7) *partner perceptions of actor persuasion goals*, and (8) *partner self-assessed persuasion goals*. First, we controlled for actor political affiliation. In this fully saturated model, we no longer find a main effect of actor political affiliation on conversational enjoyment, b = -0.21, *95%CI*[-0.44, 0.02], p = .08. However, there remained a strong positive relationship between actor perception of partner learning goals and the extent to which they enjoyed the conversation, b = 0.65, *95%CI*[0.53, 0.77], p < .001, even when controlling for actor political affiliation. Democratic actors still made more negative evaluations of their partners, b = -0.26, *95%CI*[-0.45, -0.06]. Further, even when controlling for actor political affiliation, actor perceptions of partner learning goals were significantly positively related to the evaluations they made of their partner, b = 0.59, *95%CI*[0.48, 0.70], p < .001. Thus, even when taking into account differences between Democratic and Republican voters, perceived learning goals were positively related to conflict outcomes.

Next, we interacted actor political affiliation with actor perceptions of partner learning goals. We found no interaction for conversational enjoyment, b = 0.11, *95%CI*[-0.11, 0.33], p = .34, or partner evaluations, b = 0.16, *95%CI*[-0.04, 0.38], p = .11. Therefore, even when taking into account both actor and partner effects, the positive relationship between perceptions of partner learning goals and conflict outcomes remained the same across Democratic and Republican voters.

Effects Across Different Dimensions of Person Perception

When evaluating their partner, actors were asked to evaluate them on five different dimensions: moral, objective, intelligent, trustworthy, and likable (α = .90). In our main analyses, we take the average of these 5-items. However, previous research would suggest that such dimensions may be distinct from each other. Thus, here we report the results of analyses

investigating our main effect for each dimension separately. As a conservative test, we conducted analyses predicting each different dimension from our fully saturated Model 3 above.

Overall, we find that our main effect persisted: actor perceptions of partner learning goals were positively associated with each distinct dimension of person perception. Actors rated their conversation partners to be more moral $b = 0.44$, *95%CI*[0.30, 0.58], $p < .001$, more objective, $b = 0.72$, *95%CI*[0.56, 0.88], $p < .001$, more intelligent, $b = 0.52$, *95%CI*[0.38, 0.67], $p < .001$, more trustworthy, $b = 0.56$, *95%CI*[0.42, 0.70], $p < .001$, and more likable, $b = 0.69$, *95%CI*[0.55, 0.82], $p < .001$, when they perceived them to be willing to learn. Further, for each dimension of person perception, the effect of actor perceived learning goals was approximately twice as large as the next largest effect in the model.

Full Linguistic Analyses

Here we present the full results of our linguistic analyses of the conversation transcripts. We analyzed the conversation transcripts using the politeness R package, (Yeomans et al., 2018), which calculates a set of syntactic and social markers from natural language (e.g., gratitude, apologies, acknowledgment), to generate a count of the various linguistic features used by each participant in the conversation. Further, we calculated each participant's level of conversational receptiveness (Yeomans et al., 2020) using a pre-trained natural language processing algorithm.

First, we investigated the linguistic features used by partners that are associated with actors' perceptions of their learning goals. These results are presented in Fig. S1. Namely, in addition to the results presented in the main manuscript, participants perceived to place the greatest importance on learning goals (top 33%) also used significantly more first-person singular pronouns (e.g., "I" and "me") compared to those perceived to have place the least importance on goals (bottom 33%).

Next, we investigated the linguistic features associated with participants' self-assessed learning goals. These results are presented in Fig. S2. Specifically, in addition to the results presented in the main manuscript, participants who placed the greatest importance on learning goals (top 33%) use more impersonal pronouns (e.g., "that"), first person single ("I"), and second person pronouns (e.g., "you") compared to those who placed the least importance on leaning goals (bottom 33%).

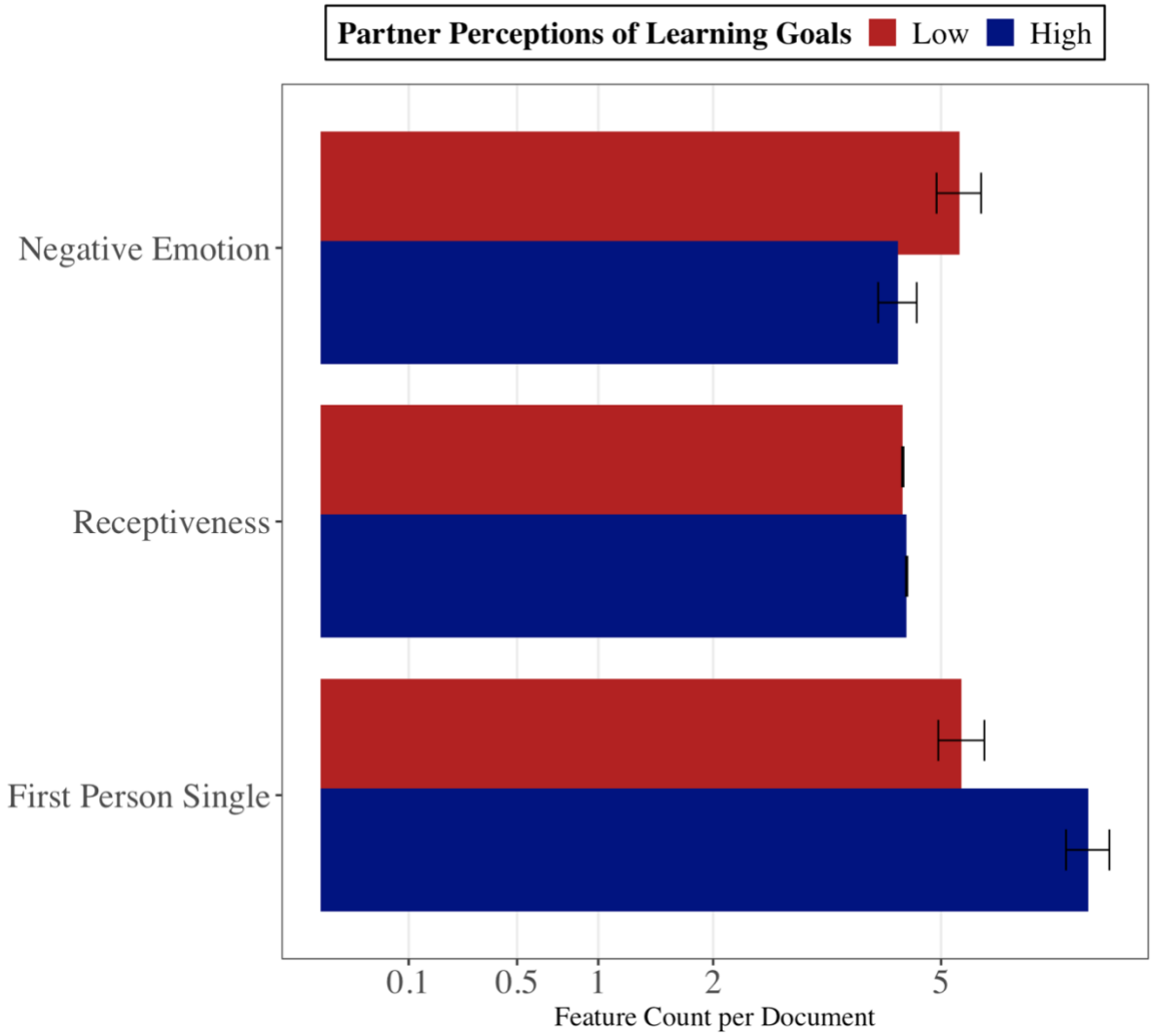

Figure B1. Relationship between actor perceptions of partner learning goals and partner linguistic features. Linguistic features that differed in frequency of use between participants perceived to place the greatest (top 33%) and least (bottom 33%) importance on learning goals.

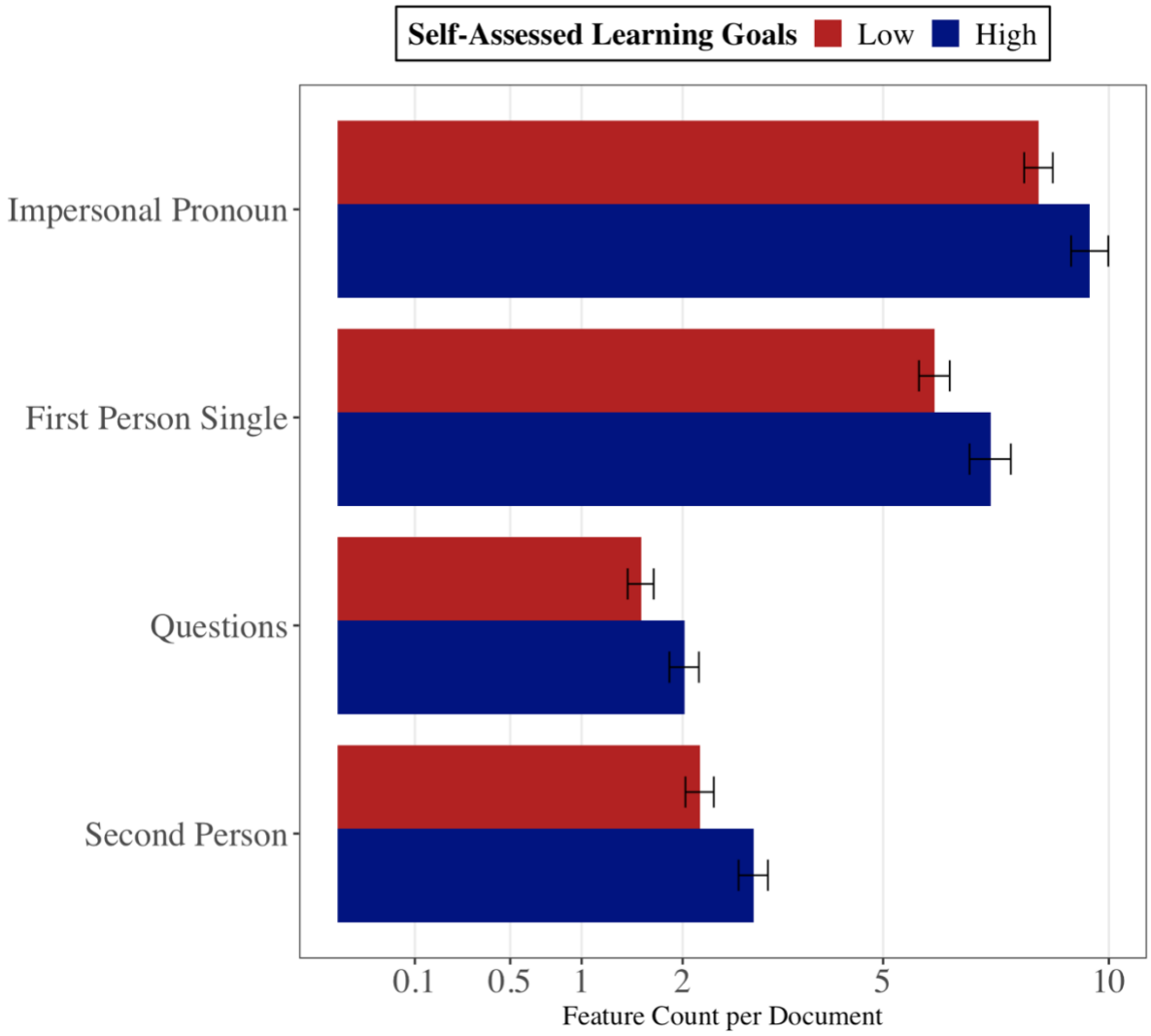

Figure B2. Relationship between participant self-assessed learning goals and participant linguistic features. Linguistic features that differed in frequency of use between participants who self-reported placing the greatest (top 33%) and least (bottom 33%) importance on learning goals.