Communication and Channel Use in Virtual Work: Experimental Insights

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Abstract

Recent decades have seen an ongoing shift toward virtual work. A major consequence of this shift is the reliance on communication channels, such as text, audio, and video, for communication, collaboration, and information exchange. Since extensive research in various fields has highlighted the positive impact of communication across different workplace situations, it is essential to understand the implications of this change in communication. Virtual communication channels differ in terms of their media richness and capacity to facilitate social presence, raising critical questions about whether the benefits of communication transfer to virtual work settings.

An initial literature review summarized existing experimental research findings on whether using different communication channels impacts behavioral outcomes and outlined important avenues for future research. Based on this review, a controlled experiment investigated people's preferences for text or video messages when being dishonest. Thereafter, an online study focused on nonverbal information in communication and explored whether enriching virtual pre-play communication with nonverbal cues improves team coordination and cooperation. Beyond studies on communication channels, an online field experiment examined virtual work on crowdworking platforms and how preference-personalized casual microtasking systems influence the performance of crowdworkers.

Insights from the literature review revealed that the shift from face-to-face to virtual communication can significantly impact human behavior in workplace settings. While richer communication channels, such as video channels, can mitigate some negative effects, more nonverbal information in one-way communication, such as messages, poses risks. Notably, individuals have preferences for different communication channels, because subjects were significantly more likely to choose text over video messages when lying. Additionally, enriching audio communication with static nonverbal cues in the form of profile pictures significantly reduced team cooperation compared to audio-only and video communication. In the context of crowdwork, the results revealed no positive effects of preference-based personalization on performance. Overall, this dissertation contributes to the fields of economics and information systems by providing experimental insights into how different communication channels shape behavior. These findings offer valuable implications for researchers and practitioners, particularly in the context of ongoing discussions about office-based, remote, and hybrid work arrangements.

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List of Abbreviations

CLT	Charismatic Leadership Tactic
DSR	Design Science Research
ECU	Experimental Currency Units
ICT	Information and Communication Technology
IOS	Inclusion of Other in the Self Scale
IS	Information Systems
KD^2Lab	Karlsruhe Decision and Design Lab
LDA	Latent Dirichlet Allocation
OLS	Ordinary Least Squares
P-E	Person-Environment
P-J	Person-Job
RQ	Research Question
WLG	Weakest-Link Game

1 Introduction*

1.1 Motivation

With the continuous advancement of technologies, recent decades have seen a growing shift toward greater engagement in virtual work. By 2025, about two-thirds of employees who can work remotely work either fully remotely or in hybrid arrangements. However, a sizable gap remains between employee preferences and employer expectations, with desired and planned work-from-home days diverging by approximately 0.6 days per week (Barrero et al., 2021). Public statements by large companies, such as Amazon, AT&T, or JPMorgan Chase, calling for return-to-office mandates (Wong, 2025) demonstrate the debate on the future of work. On the one hand, employees value the benefits of less commuting and flexible work schedules in remote work arrangements (Barrero et al., 2021). Moreover, recent studies have emphasized the advantages of remote and hybrid work, highlighting improvements in job satisfaction and reduced attrition rates without compromising performance (Bloom et al., 2022, 2024; Chatterjee et al., 2022). On the other hand, the shift toward virtual work comes with a range of challenges as it can negatively impact employee well-being (Charalampous et al., 2019; Denzer & Grunau, 2024; Hill et al., 2024; Yang et al., 2023), reduce innovative performance (Brucks & Levav, 2022; Gibbs et al., 2024), and hamper information sharing (Yang et al., 2022). Employees, in particular, recognize social interaction and face-to-face collaboration as key benefits of in-person work (Barrero et al., 2021).

One key consequence of the trend toward virtual work is the changing nature of communication. Traditional office-based work naturally facilitates spontaneous, face-to-face interactions. In contrast, employees increasingly rely on technologies in virtual work to communicate, collaborate, and exchange information (Bloom et al., 2022; Yang et al., 2022). Understanding the potential impact of this change is crucial, as a large body of research across different fields has revealed significant positive effects of communication on behavior in various workplace situations. Prior studies have demonstrated that communication significantly enhances cooperation (e.g., Dawes et al., 1977; Isaac & Walker, 1988; Isaac et al., 1985; Sally, 1995), fosters trust and trustworthiness (e.g., Ben-Ner & Putterman, 2009; Charness & Dufwenberg, 2006), influ-

*This chapter is based on the following studies: Haug et al. (n.d.), Hörrmann et al. (n.d.), Nieken and Walther (2024), and Walther (2025).

ences bargaining behavior and fairness concerns (e.g., Bohnet & Frey, 1999a, 1999b; Roth et al., 1995), or improves coordination and efficiency (e.g., Charness, 2000; Cooper et al., 1992; Duffy & Feltovich, 2006; Van Huyck et al., 1993). Given these wide-ranging benefits, effective communication is key to successful work in companies and organizations. Hence, it is crucial to understand whether the positive effects of communication also extend to virtual work.

In virtual work, individuals depend on various virtual communication channels, which enable communication through different technologies without necessitating physical presence. Among these, text, audio, and video channels are particularly prominent, each varying in its specific characteristics. These differences may influence the effectiveness of communication and are captured by highly influential theories: in particular, media richness theory (Daft & Lengel, 1986) and social presence theory (Short et al., 1976). Media richness theory suggests that communication channels vary in their ability to transmit rich information (Daft & Lengel, 1986). Video channels can convey paraverbal and nonverbal information that is not available in text channels, and such information can impact behavior (see, e.g., Andreoni & Petrie, 2004; Bohnet & Frey, 1999a, 1999b). Social presence theory focuses on the extent to which a communication channel allows individuals to feel the presence of others (Short et al., 1976), which has been shown to play a key role in enhancing honesty (Cohn et al., 2022; Nieken & Walther, 2024). Over the past few decades, experimental research has examined how the use of different communication channels influences cooperation (Bochet et al., 2006; Brosig et al., 2003; Frohlich & Oppenheimer, 1998), honesty (Abeler et al., 2014; Cohn et al., 2022; Conrads & Lotz, 2015), creativity (Brucks & Levav, 2022; Grözinger et al., 2020), or performance (Nieken, 2023). However, a synthesis of the existing body of literature is still missing, and current insights are limited, highlighting the need for further research. To address and better understand communication-related challenges and opportunities in virtual work, this dissertation investigates how communication, and particularly the use of different virtual communication channels, affects human behavior.

Regarding virtual work, numerous terms, such as remote work, distributed work, mobile work, or telework, are often used interchangeably or to describe closely related concepts. This thesis adopts a broader definition of virtual work. It refers to any form of work in which communication and interactions are mediated by technology instead of being in-person (Hill et al., 2024; Makarius & Larson, 2017; Raghuram et al., 2019). Thus, virtual work arrangements include remote work, in which individuals work from home or any other location, as well as work and research conducted on online platforms such as Upwork, MTurk, or Prolific, which are mediated via technologies.

Within the information systems (IS) literature, such platforms are referred to as crowdworking platforms, as they enable a crowd of workers to complete tasks in exchange for financial compensation (Durward et al., 2016; Jäger et al., 2019). Research has already investigated how the workers' performance on such platforms can be enhanced (Wang et al., 2017). One opportunity in virtual work to further improve performance is work-related personalization. While research on personalization has been particularly prominent in the domains of marketing and consumer behavior (Chandra et al., 2022; Murthi & Sarkar, 2003; Thirumalai & Sinha, 2013; Weidig et al., 2024; Zhang, 2011), this thesis investigates personalization in the context of crowdwork as the process of modifying a system's functionality or content to increase its relevance for individuals. Studies have demonstrated that personalizing task recommendations according to crowdworkers' characteristics, such as approval rates or nationality, helps to improve crowdworkers' outcomes (Amer-Yahia et al., 2016; Difallah et al., 2013; Paulino et al., 2022), underscoring both the potential and the increasing interest in personalized crowdwork (Paulino et al., 2023a). However, this body of research remains limited in scope. Beyond task recommendations, Paulino et al. (2022) highlighted the role of task design in influencing crowdwork. As technologies increasingly facilitate flexibility in virtual work, they offer opportunities to personalize task designs according to individual preferences, such as polychronicity or altruism. Personalizing the design of tasks can be achieved through strategic variations in the communication content of instructions. For example, Nieken (2023) showed that modifying instructions to include charismatic leadership tactics significantly influenced workers' performance. Thus, personalizing task instructions that prioritize different goals, such as financial rewards or altruistic outcomes, may represent a valuable approach to optimizing workers' performance. However, whether preference-based personalization improves the performance of crowdworkers remains to be explored.

1.2 Research Objective

This thesis aims to synthesize and extend findings from experimental research to examine how communication, and in particular the use of different virtual communication channels, can mitigate challenges and leverage opportunities in virtual work. Specifically, it explores the impact of various communication channels on human behavior, emphasizing honesty and cooperation. Moreover, the impact of preference-based personalization on crowdworker performance is investigated. The research questions (RQs) addressed in this paper are explained below.

Communication plays a crucial role in various contexts of daily life. It is multifaceted and involves more than just spoken or written words, encompassing three dimensions—verbal, paraverbal, and nonverbal—whose interaction can influence the impact of communication. (Nieken, 2023). Verbal communication involves the explicit content of messages and specific words. Paraverbal communication includes aspects such as tone of voice or intonation, reflecting how words are spoken. Nonverbal communication, also known as body language, describes gestures, facial expressions, or eye contact, providing additional context beyond words (Driskell et al., 2003; Warkentin et al., 1997). Understanding these dimensions is particularly important in virtual work, where communication depends on channels that vary in the richness of cues they can transmit (Daft & Lengel, 1986). While text channels can only convey verbal information, audio channels offer additional paraverbal information, and nonverbal information is only available via face-to-face and video channels. Previous research revealed that using different communication channels can significantly affect cooperation, performance, or creativity (see, e.g., Brosig et al., 2003; Brucks & Levav, 2022; Nieken, 2023). However, the existing literature lacks a systematic review that provides comprehensive insights into whether the effect of communication is impacted when transitioning from in-person to virtual communication and whether using different virtual communication channels yields different outcomes. Therefore, the first RQ addressed in this dissertation is:

> **RQ1:** *How does the use of face-to-face and different virtual communication channels affect human behavior?*

Study I addresses this RQ with a literature review comprising 21 experimental studies. To provide comprehensive insights, findings were structured according to whether communication was one-way or two-way. These insights revealed that, particularly in two-way communication, using less rich communication channels, such as text, can negatively affect cooperation (Bochet et al., 2006; Brosig et al., 2003; Frohlich & Oppenheimer, 1998; Rockmann & Northcraft, 2008) and creative performance (Brucks & Levav, 2022; Grözinger et al., 2020; Grund et al., 2025). However, in one-way communication, such as messages, more nonverbal information is not always advantageous (Nieken, 2023; Zylbersztejn et al., 2020). Beyond synthesizing existing knowledge, the literature review suggests a need for further research, as studies directly comparing virtual communication channels (text, audio and video) remain limited (see Figure 2.2b). Moreover, behavior in some economic settings, such as coordination or dishonesty between subjects, has not been studied extensively. The review helped to identify critical research gaps that formed the basis for the second and third RQ, contributing to a deeper understanding of the effects of using different communication channels.

An area more specifically explored in this thesis is dishonest behavior. Previous research primarily focused on face-to-face, audio, and text communication to examine how honesty differs across communication channels (Abeler et al., 2014; Cohn et al., 2022; Conrads & Lotz, 2015). These studies have shown that while including paraverbal cues in audio communication compared to text communication did not significantly affect honesty, face-to-face interaction promoted greater honesty than text-based communication (Conrads & Lotz, 2015). However, the current state of research does not provide a detailed understanding of the role of video communication. Moreover, prior studies have primarily examined dishonesty directed toward the experimenter rather than between subjects. This gap is particularly relevant given the growing reliance on video channels to enable communication between employees, even in hybrid work arrangements where employees work physically co-located in the office on some days (Bloom et al., 2022). Video communication introduces nonverbal cues and the possibility of identification that can enhance perceived human presence[1], a factor known to encourage honest behavior (Cohn et al., 2022). As individuals frequently can choose among various communication channels in everyday and workplace settings, it is crucial to understand whether some channels facilitate dishonest behavior. This highlights the importance of investigating the following RQ:

RQ2: *When given the choice between text and video messages, do people prefer text or video messages when being dishonest?*

In **Study II**, we conducted a controlled experiment ($n = 181$) to address this question and used a modified sender-receiver deception game (Gneezy et al., 2013). In our experimental design, senders could choose between sending a text message or a video message to the receivers. Senders were financially incentivized to lie, and we implemented two treatments: one in which senders could only send an honest message and another in which they could choose between an honest or a dishonest message. Our findings revealed that when senders had the opportunity

[1]This thesis follows the definition of Cohn et al. (2022), who define "human presence" as the "feeling of closeness in terms of socially interacting with another person." In terms of communication channels, we interpret this definition closely to the concept of social presence (see, e.g., Short et al., 1976, for social presence theory)

to lie, they significantly preferred text over video messages when being dishonest to receivers. A possible mechanism for this behavior is that senders perceived a significantly lower level of human presence when sending a text compared to a video message, which might have facilitated the decision to lie.

In the next step, we investigate the role of nonverbal cues in communication and whether these cues influence cooperation and coordination within teams. Findings from the literature review in **Study I** underscore the importance of media richness in fostering cooperative behavior. Specifically, richer communication channels have been shown to enhance cooperation compared to less rich channels (Bochet et al., 2006; Brosig et al., 2003; Frohlich & Oppenheimer, 1998; Rockmann & Northcraft, 2008). While these studies offer valuable insights, the underlying mechanisms driving these effects remain unclear. One plausible explanation relates to the role of nonverbal cues, which vary depending on the communication channel. This issue accurately reflects real-world scenarios in which individuals can adjust the level of nonverbal cues during virtual meetings by deciding whether to turn on their cameras or display profile pictures. This choice affects if and how individuals can be identified during communication, and the ability to identify others has been shown to increase prosocial behavior (Bohnet & Frey, 1999b; Charness & Gneezy, 2008), cooperation (Andreoni & Petrie, 2004), trustworthiness (Eckel & Petrie, 2011), and efficiency (Bohnet & Frey, 1999a). To contribute to this literature, we examine the following RQ:

RQ3: *Does enriching virtual pre-play communication with nonverbal cues increase team coordination and cooperation?*

We conducted a controlled online experiment with two parts ($n = 612$) in **Study III** to answer this question. Part 1 of this study, which was treatment-independent, served primarily to recruit subjects for Part 2. In Part 2, fixed teams of three subjects completed two stages: a pre-play communication stage and a team decision stage. Teams participated in a virtual meeting during the communication stage, in which the availability of nonverbal cues varied depending on the treatment. In the *Audio* treatment, subjects could only hear each other, and nonverbal cues were not available. In the *Photo* treatment, we included static nonverbal cues via subjects' selfies. In the *Video* treatment, turned-on cameras enabled dynamic nonverbal cues via live video. Communication was not related to the team decision stage, as instructions for the task were provided only after the virtual meeting. In the team decision stage, subjects played a repeated weakest-link game (WLG) (Brandts & Cooper, 2006a; Van Huyck et al., 1990) for ten rounds.

Our results showed that enriching virtual pre-play communication with more nonverbal cues did not significantly affect coordination. However, we observed significant treatment effects on cooperation in the first round of the WLG: (i) Adding selfies in the *Photo* treatment significantly reduced cooperation at the 10% level compared to *Audio*, while (ii) turning on cameras in the *Video* treatment significantly increased cooperation relative to the *Photo* treatment. Notably, cooperation levels in *Audio* and *Video* did not differ significantly.

In the final step, this thesis explores personalization within virtual work on crowdworking platforms. Specifically, **Study IV** examines casual microtasking systems, which integrate small tasks into the daily routines of crowdworkers. Technologies that enable the completion of such microtasks offer opportunities for personalizing the design of tasks to enhance crowdworker outcomes. The person-environment (P-E) fit theory (Caplan, 1987; Edwards et al., 1998) suggests that crowdworkers' characteristics are relevant and suitable for personalization. According to this theory, individuals should experience greater satisfaction and perform better when the work environment aligns with their preferences. First, we focus on polychronicity, the preference for multitasking, as a basis for personalization. Prior research has demonstrated that aligning task demands with workers' multitasking preferences can enhance job satisfaction, well-being, and performance (Hecht & Allen, 2005; Kirchberg et al., 2015; König & Waller, 2010). Second, we examine altruism as a social preference for designing personalized tasks, building on a large body of evidence that individuals are not purely selfish and often consider the welfare and outcomes of others (Charness & Rabin, 2002; Fehr & Fischbacher, 2002; Levitt & List, 2007). With this in mind, we investigate the following RQ:

> **RQ4:** *How does a preference-personalized casual microtasking system affect crowdworker performance?*

In **Study IV,** we conducted two large-scale experimental studies on Prolific ($n = 102$ and $n = 116$) to investigate RQ4. We derived design requirements for a casual microtasking system to personalize task designs according to individual preferences, specifically, polychronicity and altruism. In the first study, we examined task designs that enabled either multitasking or monotasking. In the second study, we analyzed task designs that either addressed crowdworkers' altruistic or selfish preferences. By adjusting the communication content of the task instructions without changing the underlying incentive structure, we could emphasize the altruistic element of the task to a greater or lesser extent, thereby personalizing the task according to the differing degrees of altruistic preference among the crowdworkers. Our results indicated that personalization based on polychronicity and altruism did not enhance crowdworker performance. Notably, the findings related to altruism-based personalization highlighted a complex interaction between intrinsic and extrinsic motivations. For crowdworkers with strong altruistic preferences, personalization based on altruism appeared to backfire, resulting in decreased performance. Thus, personalized task designs must be selected cautiously.

1.3 Thesis Structure

Table 1.1 provides an overview of the structure of this dissertation. **Chapter 1** introduces the scientific and practical motivation, outlines the research objective, and derives the RQs. **Chapter 2** focuses on the literature review from **Study I** to examine the impact of using face-to-face and various virtual communication channels on human behavior (RQ1). These insights emphasized key research gaps that inform the subsequent experimental studies. **Chapter 3** presents the findings from a controlled experiment (**Study II**) to explore whether individuals prefer text or video messages when lying to others (RQ2). **Chapter 4** focuses on RQ3 and reports results from an online experiment (**Study III**) examining the impact of nonverbal cues in virtual pre-play communication on team coordination and cooperation. **Chapter 5** covers **Study IV** and presents the results from two online experiments to examine preference-based personalization on crowdworker performance. **Chapter 6** synthesizes the results of this dissertation, discusses contributions and practical implications, and outlines limitations and directions for future research. **Chapter 7** concludes the dissertation.

Chapter	**Content**	**Research Question**
Chapter 1	Introduction	
Chapter 2	Study I: The Effect of Virtual Communication Channels on Human Behavior: A Review	RQ1
Chapter 3	Study II: Honesty in Virtual Communication	RQ2
Chapter 4	Study III: Beyond Words – Nonverbal Cues in Virtual Collaborations	RQ3
Chapter 5	Study IV: Preference-based Personalization of Casual Microtasking Systems – Design and Empirical Findings	RQ4
Chapter 6	Discussion	
Chapter 7	Conclusion	

Table 1.1: Thesis Overview

2 Study I: The Effect of Virtual Communication Channels on Human Behavior: A Review*

Abstract: As workplaces and social interactions continue shifting toward virtual settings, virtual communication channels like video and text have become fundamental for collaboration and information exchange. However, these channels differ in their capacity to convey information, potentially shaping the impact of communication on behavior through various mechanisms, such as anonymity. This review examines 21 studies to assess how the use of face-to-face and different virtual communication channels affects human behavior. The results suggest that relying exclusively on virtual communication can impair behavior in some situations, such as cooperation or creativity. This issue can be mitigated using richer communication channels such as video or audio. However, especially in one-way messages, using video communication channels that convey nonverbal information is not always advantageous and involves risks. This illustrates the complexity of using different communication channels and highlights the need for future research. The insights from this review have important implications for organizations and individuals, emphasizing the crucial role of using appropriate communication channels in virtual settings. Effective channel selection can enhance cooperation and innovation, thereby contributing to the broader discussion on the future of work in remote and hybrid settings.

Keywords: virtual communication; communication channel; behavior; future of work

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2.1 Introduction

Remote work represents a fundamental transformation of the modern workplace, with an increasing number of individuals shifting from in-person toward virtual work arrangements (Barrero et al., 2023; Gallup, 2024), which impacts work in organizations (Bloom et al., 2022, 2024; Gibbs et al., 2024). One key consequence of this shift is the changing nature of communication (Yang et al., 2022), such as moving from spontaneous, in-person communication to increased reliance on technologies to interact, collaborate, or exchange information virtually.

In various workplace situations, including contract negotiations, team collaboration, or product innovation, communication plays a crucial role, and a substantial body of literature underscores the significant impact of communication on human behavior and decision-making (see, e.g., Bohnet & Frey, 1999a; Charness & Dufwenberg, 2006; Cooper et al., 1992; Dawes et al., 1977; Isaac & Walker, 1988; Roth et al., 1995). The question is whether this positive effect of communication transfers from face-to-face to virtual settings, as different virtual communication channels, such as text, audio, and video, can differ in their characteristics. Nonverbal cues, which are available in face-to-face and video channels compared to text channels, influence anonymity and identification in interactions, thereby affecting subjects' behavior in strategic settings (see, e.g., Andreoni & Petrie, 2004; Bohnet & Frey, 1999a, 1999b). Moreover, different communication channels vary in their ability to convey human presence, which is vital in fostering honesty (Cohn et al., 2022; Nieken & Walther, 2024). Such differences between communication channels are emphasized in the literature according to media richness (Daft & Lengel, 1986), social presence (Short et al., 1976), and media synchronicity (Dennis et al., 2008).

Thus, it is crucial to understand (i) how the shift from face-to-face to virtual communication impacts human behavior and decision-making and (ii) how the use of different virtual communication channels shapes these outcomes.

This paper serves as a first step towards a better understanding by summarizing and discussing the results of 21 experimental studies (see Table 2.1). I followed a structured methodology to address this interdisciplinary research topic (Kitchenham & Charters, 2007; Webster & Watson, 2002). To assure comparability across studies, the selection criteria prioritized depth over breadth. I focused on studies published in the field of economics that either relied on standard economic paradigms, such as the trust game (Berg et al., 1995) or public goods games, or used financial incentives to influence subjects' behavior (see Section 2.3 for further details on the methodology of this literature review).

Overall, the findings of this review emphasize that the transition from face-to-face to virtual communication can reduce the positive effect of communication on behavior and decision-making. Furthermore, studies suggest that the impact of communication can depend on the specific virtual communication channel used. A generalized conclusion would be too simplistic, as communication can be one-way, for example, as a message, or two-way, such as live video meetings between subjects. Moreover, communication serves multiple functions, including facilitating problem comprehension, coordinating decisions, and strategically shaping expectations, which can vary based on the study design. Thus, to provide a more precise examination, the findings from experimental studies were structured according to the following four categories: i) two-way communication in interdependent strategic interactions, where subjects' payoffs directly depend on their own and others' decisions, ii) two-way communication in creativity tasks, iii) nonverbal information in one-way communication, and iv) honesty and promises in one-way communication.

Regarding two-way communication in interdependent strategic interactions, previous studies revealed that the transition from face-to-face to virtual communication can reduce the positive impact of communication, in particular when text communication is used, as this can minimize cooperation (Bochet et al., 2006; Frohlich & Oppenheimer, 1998; Rockmann & Northcraft, 2008) and impair reciprocity (Bicchieri et al., 2010; Lev-On et al., 2010). However, this negative impact can be mitigated using richer video communication channels (Brosig et al., 2003; Rockmann & Northcraft, 2008), which underlines the importance of using appropriate channels. As for two-way communication in creativity tasks, rich video communication is important (Grözinger et al., 2020). Yet it seems that video channels cannot fully compensate for the advantages of face-to-face interactions when innovative ideas are crucial (Brucks & Levav, 2022; Grund et al., 2025). To explore nonverbal information in one-way communication, experimental studies have utilized standardized messages to isolate the role of nonverbal information by keeping the communication content constant across different communication channels. In contrast to the findings on two-way communication, where richer channels tended to increase the positive impact of communication, higher media richness was not always beneficial. For example, video messages used to motivate online employees or encourage donations were found to be equally or sometimes even less effective than text or audio messages (Nieken, 2023; Zylbersztejn et al., 2024b), suggesting that an increase in nonverbal information may also introduce certain risks. Regarding honesty in one-way communication, research has provided evidence that anonymous text-based channels may encourage dishonest behavior compared to face-to-face communication (Abeler et al., 2014; Conrads & Lotz, 2015). However, differences diminished when richer audio communication was used (Cohn et al., 2022; Conrads & Lotz, 2015).

Overall, this review is an essential first step to a better understanding of how different communication channels affect human behavior and decision-making. It also points out that research is limited so far, and it has only been partially successful in identifying mechanisms to explain how different communication channels affect the outcomes in various experimental settings. This highlights the need for future research to gain more precise insights.

This review makes a twofold contribution. First, I provide a comprehensive overview of experimental studies examining how communication channels influence human behavior and decision-making. The insights from several studies were structured based on whether communication was one-way or two-way and according to the task or respective behavior investigated. They show that communication effectiveness can vary depending on the channels used. This aspect should not be overlooked in research when designing experiments or conducting literature reviews in which communication is crucial. It points out that selecting appropriate communication channels is essential for individuals in companies, research, and daily life. Therefore, further research is necessary to provide a more detailed picture. Second, I relate empirical findings to the ongoing public debate on the future of work, particularly the shift from traditional office setups to more remote work. In the context of communication, I emphasize using hybrid work arrangements that combine the benefits of remote and office-based work. While certain risks are associated with exclusively utilizing virtual communication channels, these can be mitigated by using proper communication channels.

The remainder of this paper is structured as follows: Section 2.2 outlines the necessity and importance of this review based on existing findings. Section 2.3 describes the methodological approach of this literature review. Section 2.4 examines experimental studies exploring how using different communication channels affects human behavior and decision-making across various settings. Section 2.5 discusses implications, limitations, and future research opportunities, and Section 2.6 concludes the paper.

2.2 Theory

Communication is a fundamental aspect that allows individuals to exchange information in various settings effectively. Modern technologies enable people in organizations, companies, and their daily lives to rely on video conferencing, audio calls, or text chats to interact with others, and researchers are increasingly shifting communication in studies from face-to-face to virtual communication. Thus, there is a growing need to better understand whether and how the use of different communication channels affects human behavior and decision-making. Beyond the practical relevance, I highlight the relevance of this question based on existing findings in the literature.

2.2.1 The Effect of Communication

The role of communication in economics and various research fields has been extensively studied for decades (Charness & Dufwenberg, 2006; Cooper et al., 1992; Dawes et al., 1977; Isaac & Walker, 1988). Many studies point out the significant influence of communication on behavior and decision-making, which is reflected in a wide range of situations. Meta-analyses confirmed a positive significant effect of communication on cooperation (Balliet, 2010; Sally, 1995). Dawes et al. (1977), Isaac et al. (1985), and Isaac and Walker (1988) revealed that communication facilitated subjects to coordinate, which significantly increased cooperation in public goods games and social dilemmas. Other studies found positive effects of communication on trust and trustworthiness (see, e.g., Ben-Ner & Putterman, 2009; Charness & Dufwenberg, 2006), bargaining and fairness concerns (see, e.g., Bohnet & Frey, 1999a, 1999b; Nieken & Schmitz, 2023; Roth et al., 1995), or coordination and efficiency (see, e.g., Charness, 2000; Cooper et al., 1992; Duffy & Feltovich, 2006; Van Huyck et al., 1993). These results suggest that the impact of communication on behavior and decision-making stems not only from its ability to facilitate coordination but also from its ability to shape expectations or influence behavior via promises. Thus, the impact of communication can vary depending on the type of interactions and the specific research context. While there are many possible causes for the positive effect of communication, and a comprehensive examination of these causes falls beyond the scope of this paper, many studies have focused on face-to-face communication to investigate communication. However, the transition from face-to-face to virtual settings necessitates the use of virtual channels, which fundamentally change the way of communication (Yang et al., 2022). Researchers adapted to these new possibilities by incorporating virtual communication channels, such as text chats, into experimental studies, demonstrating that virtual communication can also significantly influence behavior and decision-making (see, e.g., Ben-Ner & Putterman, 2009; Ben-Ner et al., 2011; Brosig et al., 2003; Cason et al., 2012; Nieken & Schmitz, 2023). Still, it is unclear whether and how the transition from face-to-face to virtual communication influences the effect of communication on behavior and decision-making.

2.2.2 Virtual Communication Channels

Virtual communication channels allow communication between individuals through various technologies without requiring physical presence. In this paper, three categories of virtual communication channels—text, audio, and video—alongside face-to-face (Brandts et al., 2019) are highlighted.[1] These channels differ in terms of multiple characteristics, but there can also be differences within one category of communication channel. For example, a text channel can be a live chat, enabling more real-time interaction than e-mails.

There are three widely recognized theories to highlight possible differences between communication channels: media richness theory (Daft & Lengel, 1986), social presence theory (Short et al., 1976), and media synchronicity theory (Dennis et al., 2008). The media richness theory states that different communication channels can vary in the degree to which they transmit different information. Richer channels, such as face-to-face or video, enable the transmission of nonverbal cues and signals[2] like body language, facial expressions, gestures, or tone of voice, which are absent in text channels (Daft & Lengel, 1986). These cues influence factors such as the level of anonymity or the availability of information during communication. According to Short et al. (1976), the social presence theory describes the extent to which different communication channels enable individuals to perceive the presence of others and foster a sense of connection (as cited in Oh et al., 2018). In contrast, media synchronicity theory explains the degree to which the features of a communication channel support synchronous communication between individuals (Dennis et al., 2008). For example, while modern video communication channels enable high synchronicity via real-time interaction, e-mail communication is rela-

[1] Brandts et al. (2019) also mention Paper and Pencil, which I do not examine in this study.

[2] I consider cues to be unintentionally conveyed, while I consider signals to be strategically and intentionally conveyed (Spence, 1973).

tively asynchronous and, thus, potentially alters behavior in settings that require collaboration.

2.2.3 Understanding the Impact of Different Communication Channels

Based on the theories discussed, several studies provide implications on why different communication channels can impact behavior, such as cooperation, prosocial behavior, or trust. In the context of the media richness theory, video communication allows individuals to see one another, unlike audio or text communication, thereby affecting anonymity and identification. This, in turn, can influence human behavior and decision-making, such as fairness concerns in dictator games. Dictators made significantly higher offers when they and recipients could visually identify each other (Bohnet & Frey, 1999b), when facial pictures were shared (Burnham, 2003), or when family names were revealed (Charness & Gneezy, 2008). Similarly, reducing anonymity through identification decreased free-riding in a public goods game (Andreoni & Petrie, 2004) and significantly enhanced cooperation in a prisoner's dilemma (Bohnet & Frey, 1999a). Moreover, in trust games, subjects valued pictures of their counterparts as they were willing to pay for these, and recipients were significantly more trustworthy when they saw a picture of senders' faces (Eckel & Petrie, 2011). Zylbersztejn et al. (2020) showed that subjects in a hidden action game likewise valued being able to view photos and videos of their counterparts. However, only strategically relevant content significantly improved subjects' ability to predict the trustworthiness of their counterparts (Zylbersztejn et al., 2020, 2021).

Beyond possible effects of anonymity, nonverbal cues or signals, such as gestures, facial expressions, or eye movements, can impact interactions (Argyle, 2013). Face-to-face or video channels allow the conveyance of smiles, winks, or handshakes, which can help to increase trust (Scharlemann et al., 2001) or can act as a coordination device by establishing trust in groups (Manzini et al., 2009). Gaze information, such as where individuals look or for how long (Hessels, 2020), is a form of nonverbal communication either absent or only partially available in virtual communication channels (Bohannon et al., 2013). Kurzban (2001) showed that mutual eye gaze between group members can increase cooperation.

Additionally, subjects' attractiveness can be important when communication channels with visual information are used. Expectations of others' cooperativeness increased with physical attractiveness (Andreoni & Petrie, 2008; Zylbersztejn et al., 2024a), and the perceived trustworthiness was higher for more attractive trustees (Wilson & Eckel, 2006). A similar beauty premium has also been observed in hiring decisions, where physical attractiveness benefited men but not women (Ruffle & Shtudiner, 2015).

In the context of the social presence theory, subjects were significantly less honest when reporting their results on their own, without interacting with the experimenter, compared to when they reported them verbally (Pascual-Ezama et al., 2015). Additionally, Cohn et al. (2022) found that human presence was crucial in reducing dishonesty, as individuals were significantly more dishonest when interacting with a machine rather than with a human being. Regarding media synchronicity, studies within the economic discipline that explicitly examined the influence of differences in communication synchronicity (e.g., Conrads and Reggiani, 2017) are limited.

While these empirical findings may seem distinct and were not necessarily designed to provide insights into the use of different communication channels, they underscore the importance of a more in-depth investigation into how various communication channels can influence behavior and decision-making.

2.3 Method

I conducted a literature review focusing on experimental studies within the economic research field. The review process followed a structured methodology (Kitchenham & Charters, 2007; Webster & Watson, 2002) to ensure comprehensiveness and relevance through an in-depth understanding of the addressed interdisciplinary research topic.

Based on prior knowledge and a broad search on Google Scholar, I identified key papers investigating the effect of communication channels on human behavior and decision-making. These preliminary findings helped me to develop a search string to expand the scope of relevant studies. The final search string was: "communicat* AND (channel OR media OR mode OR form) AND experiment*." The search string was designed to capture a wide range of studies by incorporating different terminologies.[3] To get a broad picture of the relevant studies, I used Web of Science and Scopus as digital databases for my search. I included the Web of Science categories "Business," "Economics," and "Management" in my search in Web of Science (1082 hits) and the subject areas "Business, Management and Accounting" and "Economics, Econo-

[3]There are several alternative terminologies for "channel" in the context of communication across various research fields: communication media (see, e.g., Brosig et al., 2003; Sussman & Sproull, 1999), communication mode (see, e.g., Frohlich & Oppenheimer, 1998), communication form (see, e.g., Bochet et al., 2006).

metrics and Finance" in my search in Scopus (2222 hits). In the first step, I scanned the title, abstract, and keywords (and full text when needed) and used two inclusion criteria to select appropriate studies to address my research questions. Due to the focus of this review, the first criterion was to include only studies that compared two or more of the following communication channels: text, audio, video, and face-to-face. The second criterion was to only include experimental studies to increase comparability. After this initial search, it became apparent that the research topic is inherently interdisciplinary and encompasses several research fields, such as economics, IS, human-computer interaction, psychology, marketing, accounting, and decision sciences. I intended to focus on studies published in the field of economics, so I decided to develop two additional criteria. Both criteria were used to ensure that differences in methodological approaches did not overly constrain the comparability of results across the selected studies. Thus, I included papers that used standard economic paradigms (e.g., the trust game (Berg et al., 1995), public goods games, the coin toss task, the ultimatum game (Güth et al., 1982)) to investigate human behavior and decision-making. These paradigms are extensively employed in research, making them a well-established foundation for analysis (see e.g., Abeler et al., 2019; Engel, 2011; Johnson & Mislin, 2011; Oosterbeek et al., 2004; Zelmer, 2003, for meta-studies of the coin toss task, the dictator game, the trust game, the ultimatum game, or public goods games). In addition, they provide a common framework for benchmarking insights across diverse studies, facilitating meaningful comparisons. Moreover, I applied a fourth criterion, regardless of whether a standard economic paradigm was used or not, and included studies in which subjects' behavior was financially incentivized, as is common in experimental economics (Azrieli et al., 2018; Burke et al., 1996; Charness et al., 2016; Harrison, 1994; Loewenstein, 1999; Voslinsky & Azar, 2021). Lastly, I applied a backward and forward search based on the selected studies to ensure all relevant studies were included. This left me with a final list of 21 relevant papers.

2.4 Results

This section summarizes and discusses experimental findings on how face-to-face and different virtual communication channels impact human behavior and decision-making, which is becoming increasingly important in economic research (see Figure 2.1). Table 2.1 presents a comprehensive overview of key papers investigating this question. Moreover, Figure 2.2 illustrates the frequency with which different communication channels are compared across these papers.

As discussed before, the benefits of communication may depend on the nature of the interaction and the respective research context. To achieve a more comprehensive understanding, the experimental findings discussed in this review were structured into four categories: i) two-way communication in interdependent strategic interactions, ii) two-way communication in creativity tasks, iii) nonverbal information in one-way communication, and iv) honesty and promises in one-way communication.

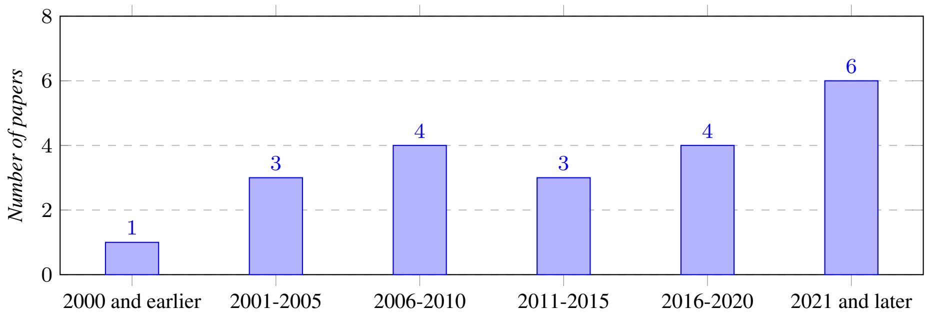
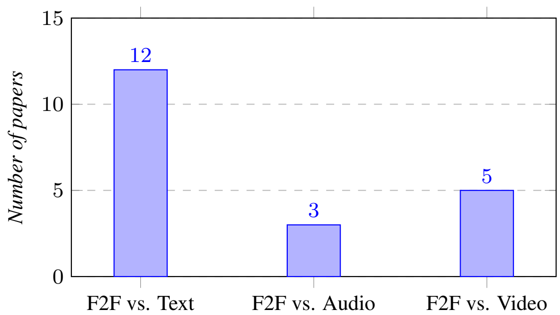
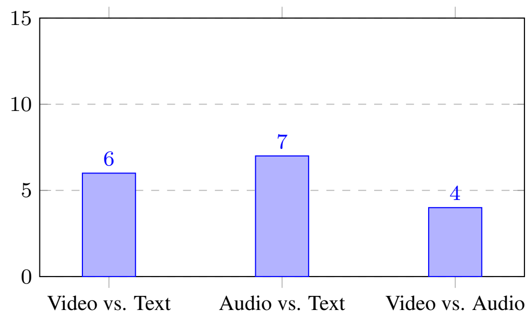


Figure 2.1: Number of papers that compare different communication channels over time



(a) Face-to-face vs. virtual channels



(b) Only virtual channels

Figure 2.2: Number of papers by compared communication channels

2.4.1 Two-way Communication in Interdependent Strategic Interactions

Standard economic paradigms, such as public goods games, trust games, or bargaining games, represent interactions between two or more individuals in which individuals' payoffs depend not only on their own decisions but also on the decisions of others. Such interactions are common in companies and organizations, for example, when buyers and suppliers negotiate prices or team members collaborate on large company-wide projects.

A common challenge in teamwork is the phenomenon of free-riding, where some team members contribute minimal effort while still benefiting from the efforts of others, leading to an unequal distribution of rewards. This problem is also crucial in providing public goods, where individuals tend to contribute less, and others must bear the costs. Communication can decrease such uncooperative behavior (Isaac & Walker, 1988; Isaac et al., 1985). However, studies that allowed game-relevant pre-play communication showed that face-to-face yielded significantly higher or at least equal group cooperation compared to text communication in public goods games (Abatayo et al., 2018; Bochet et al., 2006) but also in a prisoner's or social dilemma (Frohlich & Oppenheimer, 1998; Rockmann & Northcraft, 2008). One possible explanation is that face-to-face communication fosters higher levels of trust, mediating the relationship between the communication channel and cooperation (Rockmann & Northcraft, 2008). This would be consistent with previous findings, highlighting the crucial role of trust in facilitating cooperation (Balliet & Van Lange, 2013). Still, studies have not identified a clear mechanism explaining the superiority of face-to-face communication. Face-to-face and text communication seem equally effective in ensuring an understanding of the game. Using an impartial prisoner's dilemma compared to a standard prisoner's dilemma, in which the conflict between group and self-interest was absent, resulted in no differences between both channels (Frohlich & Oppenheimer, 1998). Similarly, when subjects could punish other subjects' behavior, such as free-riding, differences in cooperation were no longer significant (Bochet et al., 2006). Thus, only when subjects had the possibility of free-riding with no risk of punishment did face-to-face communication led to significant higher cooperation than text. Since face-to-face and text differ in anonymity and Bochet et al. (2006) explicitly forbid subjects to reveal their identity, a higher anonymity might have hampered the impact of communication in text communication. This would be in line with findings showing that identification decreased free-riding in a public goods game (Andreoni & Petrie, 2004) and significantly enhanced cooperation in a prisoner's dilemma (Bohnet & Frey, 1999a). Findings by Brosig et al. (2003) and Rockmann and Northcraft (2008) indicate that this difference in cooperation between face-to-face and virtual communication can be overcome using richer communication channels, such as video channels, which reduce anonymity in virtual communication. In an adapted version of a "disarmament exercise," cooperation was similar via video compared to face-to-face communication and improved significantly compared to communication through text (Rockmann & Northcraft, 2008). Again, differences in trust mediated this effect.

Authors	Communication channels	Between-subject	Groups	Group Size	Game-related content	Restricted content	Same verbal content	One-way/ Two-way	Pre- Play	Duration	Focus of Task/Game	Findings	Additional information
Abatayo et al. (2018)	F2F & Text	✓	✓	4	✓	✗	✗	Two-way	✓	10 min	Cooperation	F2F $\approx$ Text	
Abatayo et al. (2020)	F2F & Text	✓	✓	4*	✓	✗	✗	Two-way	✓	10 min	Trust and Reciprocity	F2F $\approx$ Text	*Communication in groups of four and decisions in groups of two
Abeler et al. (2014)	Audio & Text	✓	✗	-	✓	✓	✓	One-way*	✗	-	Honesty	Audio $\approx$ Text	*Communication of outcome to experimenter
Babutsidze et al. (2021)	Video, Audio & Text	✓	✓	2	✓	✗	✓*	One-way**	✓	-	Trust	Video $\gtrapprox$ Audio Video $>$ Text Audio $\gtrapprox$ Text	*Verbal content homogenized **Pre-play messages from agents to principals
Bicchieri et al. (2010)	F2F & Text	Within	✓	2	✓ / ✗*	✓**	✗	Two-way	✓	F2F: 2 min Text: 5 min	Trust and Reciprocity	F2F $\gtrapprox$ Text	*Varied between treatments **No information on identities
Bochet et al. (2006)	F2F & Text	✓	✓	4	✓	✓*	✗	Two-way	✓	5 min	Cooperation	F2F $>$ Text	*Text: no information on identities
Brosig et al. (2003)	F2F, Video & Audio	✓	✓	4	✓	✗	✗	Two-way	✓	10 min	Cooperation	F2F $\approx$ Video F2F $>$ Audio Video $>$ Audio	
Brosig et al. (2004)	Video & Text	✓	✓	2	✓	✓*	✗	Two-way	✓	10 min (Video), 15 min (Text)	Bargaining	Video $\gtrapprox$ Text	*Text: no information on identities
Brucks and Levav (2022)	F2F & Video	✓	✓	2	✓	✗	✗	Two-way	✗	6 min (Lab), 60 min (Field)	Creativity	F2F $\gtrapprox$ Video	
Cohn et al. (2022)	Audio & Text	✓	✗	-	✓	✓	✓	One-way*	✗	-	Honesty	Audio $\approx$ Text	*Communication of outcome to experimenter
Conrads and Lotz (2015)	F2F, Audio & Text	✓	✗	-	✓	✓	✓	One-way*	✗	-	Honesty	F2F $\approx$ Audio F2F $\gtrapprox$ Text Audio $\approx$ Text	*Communication of outcome to experimenter
Conrads and Reggiani (2017)	F2F, Audio & Text	✓	✗	-	✓	✓	✓	One-way*	✗	-	Promise-making/ Promise-keeping	F2F $\approx$ Audio F2F $\approx$ Text Audio $\approx$ Text	*Communication of decision to experimenter
Frohlich and Oppenheimer (1998)	F2F & Text	✓	✓	5	✓	✗	✗	Two-way	✓*	n/a	Cooperation	F2F $\gtrapprox$ Text	*Communication before rounds 1 to 8
Greiner et al. (2014)	F2F & Text	✓	✓	2	✗	✓*	✗	Two-way	✓	5 min	Bargaining	F2F $\gtrapprox$ Text	*No information on identities
Grözinger et al. (2020)	F2F, Video & Text	✓	✓	3	✓	✗	✗	Two-way	✗	30 min	Creativity	F2F $\approx$ Video F2F $>$ Text Video $>$ Text	
Grund et al. (2025)	F2F & Video*	✓**	✓	2	✓	✗	✗	Two-way	✗	8 min	Creativity	F2F $\gtrapprox$ Video	*2-phase experiment **Also within-subject with F2F and video
Lev-On et al. (2010)	F2F & Text	Within	✓	2	✓	✓*	✗	Two-way	✓	2-10 min**	Trust and Reciprocity	F2F $\gtrapprox$ Text	*No information on identities
McGinn et al. (2003)	F2F & Text	Within	✓	2	✓	✗	✗	Two-way	✓	F2F: 6 min Text: 13 min	Bargaining	F2F $\approx$ Text	
Nieken (2023)	Video, Audio & Text	✓	✗	-	✓	✓	✓**	One-way***	✓	-	Real Effort	*Video $<$ Audio Video $<$ Text Audio $\gtrapprox$ Text	*Findings in the neutral set-up **Varied between treatments ***Task instructions
Rockmann and Northcraft (2008)	F2F, Video & Text	✓	✓	2, 4 or 6	✓	✗	✗	Two-way	✓	5 or 10 min	Cooperation	F2F $\gtrapprox$ Video F2F $>$ Text Video $\gtrapprox$ Text	
Zylbersztejn et al. (2024b)	Video, Audio & Text	✓	✗	-	✓	✓	✓	One-way*	✓	-	Donations	Video $\approx$ Audio Video $\approx$ Text Audio $>$ Text	*Pre-recorded and standardized message addressed to potential donors

Note: F2F = Face-to-face

Table 2.1: Overview of experimental studies investigating how using different communication channels impacts human behavior and decision-making

In contrast, in a business-oriented dilemma, group cooperation was similar in video and text communication but significantly lower than in face-to-face communication. Yet, in both studies, the video technology did not allow for real-time visual interaction, as images were refreshed only every three to four seconds. This affected the synchronicity of communication and the ability to convey nonverbal information, which likely influenced group cooperation. Brosig et al. (2003) further supports the implication that video communication channels can be an effective alternative to face-to-face for group cooperation. In a ten-round public goods game, groups achieved similar levels of cooperation with video and face-to-face communication. Both yielded significantly higher cooperation and greater stability compared to audio communication. Further analyses by the authors revealed that differences in communication content could not explain the higher group cooperation in the video compared to the audio channel. Moreover, the authors concluded that differences in social distance were not a possible mechanism. They used an identification treatment in which group members could visually identify each other but could not communicate. This did not result in higher cooperation compared to the baseline without communication, indicating that identification without the possibility for communication was not effective in increasing cooperation (Brosig et al., 2003), which is in contrast to other studies (Andreoni & Petrie, 2004; Bohnet & Frey, 1999a). Still, it is possible that (visual) identification and the presence of nonverbal cues, combined with strategically discussing the game, enhanced cooperation, which may explain the superiority of video over audio communication. Although there is no clear consensus on which features of communication channels are most important in cooperative situations, findings suggest that while text-based communication can decrease cooperation, video communication tends to mitigate this effect and achieve cooperation levels similar to in-person interaction.

Another challenge besides free-riding is establishing trust, as individuals must decide whether to rely on others without assured reciprocity. Trust fosters cooperation (Balliet & Van Lange, 2013), yet uncertainty about others' intentions can lead to hesitation. At the same time, a lack of trustworthiness, where individuals prioritize self-interest, can hinder collective actions and the enforcement of social norms (Fehr & Gächter, 2000). Communication is essential to trust and reciprocity as it shapes expectations and influences decisions through promises and agreements (Ben-Ner & Putterman, 2009; Charness & Dufwenberg, 2006). When comparing the effect of communication between face-to-face and text in the trust game (Berg et al., 1995), trust levels did not significantly differ. These results were observed irrespective of i) the use of a between-subject or within-subject design, ii) group size, or iii) the relevance of the communication content to the game (Abatayo et al., 2020; Bicchieri et al., 2010; Lev-On et al., 2010). Results from a two-stage sequential bargaining game support the idea that text can similarly establish trust as richer communication channels. Subjects primarily used pre-play communication to reach an agreement on the efficient equal split outcome, and the behavior of the first movers did not significantly differ between video and text communication (Brosig et al., 2004). Overall, the results suggest that although face-to-face or video communication is richer, more synchronous, and allows higher social presence compared to text communication, trusting behavior was not affected.

Second movers' behavior on reciprocity in such interactions, however, can be impacted when using different communication channels. While in a between-subjects design, Abatayo et al. (2020) found no significant difference in the amount sent back between face-to-face and text communication, in within-subjects designs, receivers' reciprocity decreased in text compared to face-to-face communication (Bicchieri et al., 2010; Lev-On et al., 2010). Again, findings from a two-stage sequential bargaining game support the superiority of richer communication channels (Brosig et al., 2004). Although the first movers' behavior was similar between video and text communication, fairness concerns were higher in video communication because significantly more equal-split outcomes were achieved, indicating an increased prosociality. This negative effect of text communication was primarily caused by the second movers, who tended to break their agreement four times more often than agreeing via video, indicating lower reciprocity. Again, there is no clear evidence as to why reciprocity was affected by using different communication channels. In all three studies, subjects were not allowed to reveal their identities, emphasizing the higher anonymity in text communication as a possible explanation. Findings from Eckel and Petrie (2011) support the importance of anonymity, as reciprocity significantly increased when receivers could see a picture of senders' faces. Furthermore, face-to-face and video channels might be better for establishing trustworthiness, as they allow for facial expressions such as a genuine smile, which can signal higher trustworthiness (Centorrino et al., 2015).

Moreover, in an ultimatum game, the effect of communication was significantly reduced in text compared to face-to-face interaction, even when the pre-play communication content was irrelevant to the game. Proposers' offers and responders' payoffs were marginally significantly higher in face-to-face than in text chat communication (Greiner et al., 2014). Thus, even when communication could not be used to discuss strategies, face-to-face communication positively impacted prosocial behavior in a bargaining setting. However, this positive effect depends on the context of bargaining. In a one-stage, simultaneous bargaining game that included a buyer-seller pair with private information, the likelihood of a trade and bidding strategies after game-relevant pre-play communication did not significantly differ between face-to-face and text communication (McGinn et al., 2003). Note that compared to other studies discussed above, the degree of anonymity was not different between the communication treatments in the study by McGinn et al. (2003) because identities were disclosed in all treatments. Still, the higher richness in face-to-face did not yield a higher efficiency in this bargaining context.

In summary, while text communication may have similar effects on human behavior and decision-making in some settings as face-to-face communication, e.g., in terms of trust, several studies suggest that the shift from face-to-face to virtual communication can have negative behavioral implications. Text communication can facilitate free-riding, hinder reciprocity, or reduce prosociality. High levels of anonymity and the limited richness of text communication are possible drivers of these differences. However, the decline in virtual communication can be overcome using richer video communication channels that can achieve group cooperation similar to face-to-face interactions. Given that only Brosig et al. (2003), Greiner et al. (2014), and Rockmann and Northcraft (2008) compared virtual communication channels (text, audio, and video), insights into how the use of different virtual communication channels affects behavior and decision-making in strategic interactions are limited, indicating opportunities for future research.

2.4.2 Two-way Communication in Creativity Tasks

In the following, I investigate communication in team creativity tasks.[4] Three studies that explicitly compared different communication channels emphasized the importance of media richness to increase creative output (Brucks & Levav, 2022; Grözinger et al., 2020; Grund et al., 2025).

Grözinger et al. (2020) used a highly visual, collaborative task in which teams of three subjects communicated via text, video, or face-to-face. While video and face-to-face communication

[4] Team creativity tasks share similarities with public goods games discussed earlier, as both involve collective contributions toward a shared outcome. This review distinguishes them, noting that creative tasks rely more on idea-sharing, making behavior less strategic due to the absence of explicit incentives and quantifiable contributions.

resulted in similar levels of creativity, creative performance was significantly lower when using a less rich text-based chat. This difference was primarily due to variations in the usefulness of ideas and less so based on uniqueness and aesthetic values. Interestingly, differences in combined measures that included uniqueness, aesthetic value and usefulness were only significant when comparing video and chat (e.g., for excellent ideas) and not when comparing face-to-face and chat.

Brucks and Levav (2022) and Grund et al. (2025) point out the importance of face-to-face interaction compared to rich video communication. In a two-phase experiment, Grund et al. (2025) used the *Unusual Uses Task* (Torrance, 1966) to examine the effect of different work settings (face-to-face in the lab vs. video communication from home) on creative performance in dyadic teams. Results revealed that at least one in-person phase was crucial for high creative output. While creativity in the first phase did not differ significantly between teams that worked solely face-to-face or via video, qualitative creativity in the second phase was significantly higher when both phases were conducted face-to-face. Similarly, teams in hybrid settings, which switched between video and face-to-face interaction, also yielded significantly higher creativity in the second phase than fully video teams, highlighting the importance of at least one face-to-face meeting. This effect was primarily driven by differences in very rare answers as an indicator of unusual, innovative thoughts. Further analysis ruled out trust and sympathy as moderating factors. However, self-selection partially mitigated the negative impact of working via video. Teams that chose a fully work-from-home setting exhibited higher qualitative creativity than those assigned to it exogenously (Grund et al., 2025).[5]

A lab and a field study employing idea generation tasks support differences between face-to-face and video communication (Brucks & Levav, 2022). Additionally, they highlight an underlying mechanism explaining why video communication can impede creative output. In the lab study, pairs were instructed to generate creative uses for a product and thereafter select the most creative idea. The number of creative ideas in the video treatment was significantly lower than in the face-to-face treatment. In contrast to the creative performance, the decision quality was better for video pairs as they selected a significantly higher-scoring idea than face-to-face pairs. The authors analyzed why creativity was impacted and examined several alternative explanations, such as verbal and nonverbal behaviors, mimicry, or eye gaze. They ruled out trust

[5]Notably, the experimental design also varied peer presence, as face-to-face teams worked simultaneously with other teams in the lab, while video teams worked alone from home, potentially influencing results beyond the communication channel.

as a possible mechanism because the level of trust measured in an economic trust game did not vary depending on the communication channel used. Interestingly, the authors suggest that the physical nature of video, compared to in-person communication, narrowed the visual focus of the subjects, hindering idea generation. This insight was achieved using state-of-the-art technology to measure the subjects' eye movements. To assess whether the findings in the lab are generalizable, Brucks and Levav (2022) conducted a field study in a large telecommunications infrastructure company in five countries. The setting was similar. Teams of two engineers participated in an ideation workshop and worked on this task via video or in person. They had to generate product ideas and then select and submit one idea as a future product innovation for the company. In all five countries, pairs generated significantly fewer creative ideas when collaborating via video than face-to-face.

Given the limited number of studies, current findings can only offer initial insights into how creativity may be influenced by virtual compared to face-to-face communication. While richer video communication can serve as an alternative to mitigate this difference, face-to-face interactions are crucial to encourage high creativity in teams.

2.4.3 Nonverbal Information in One-way Communication

Findings in previous subsections focused on scenarios involving interactive, two-way communication between two or more individuals. However, communication can be one-way, such as sending messages to motivate employees. Research on leadership revealed that simple text messages aimed to motivate employees actually decreased performance (Fest et al., 2021), and regardless of the communication content, charismatic speeches had a higher motivational effect on performance than neutral speeches (Antonakis et al., 2022). Both findings emphasize the relevance of one-way communication. In strategic situations, simple one-way messages can be used to influence the expectations and behavior of others by making promises, which can help to increase reciprocity (Charness & Dufwenberg, 2006; Charness & Gneezy, 2008). Such messages can be transmitted via text, audio, or video, whereby the different channels differ in their ability to transmit nonverbal and paraverbal information. The studies by Babutsidze et al. (2021), Nieken (2023), and Zylbersztejn et al. (2024b) have in common that they isolated the effects of nonverbal information on human behavior and decision-making by keeping the communication content constant, thus indicating causal insights into using different virtual communication channels.

Although the findings on trust behavior in two-way communication showed no differences between face-to-face and text communication (Abatayo et al., 2020; Bicchieri et al., 2010; Lev-On et al., 2010), richer messages had a positive impact on increasing trust (Babutsidze et al., 2021). The authors used a hidden action game (Charness & Dufwenberg, 2006), and before deciding on trust, principals received a text, audio, or video message from agents. Note that only the principals' trust decisions were considered, and these decisions did not influence agents' payoffs. The results revealed that the significance of the positive impact of auditory cues on trust varied according to whether or not the messages entailed a promise. Trust rates were significantly higher for audio messages than text messages, only when the messages contained a promise to act trustworthy. Without a promise, auditory cues were not enough to significantly increase the trust of the principals. Adding nonverbal cues had similar positive effects as audio messages. Interestingly, video messages led to significantly higher trust than text messages, regardless of whether the messages included an agent's promise to act trustworthy, pointing out the importance of visual cues, especially when messages conveyed a promise. Given that the verbal content was homogenized in this experimental setting, the authors could draw a causal inference that auditory and visual cues helped increase trust.

In contrast to these findings, Nieken (2023) and Zylbersztejn et al. (2024b) suggest that video messages can pose some risks. Zylbersztejn et al. (2024b) presents insights into how different virtual communication channels influence prosociality in the context of donations. They investigated how a pre-recorded and standardized message with varying amounts of paraverbal and nonverbal information in the form of a text, audio, or video message affected the prosocial behavior of potential donors. Adding auditory cues in an audio message compared to a simple text message significantly increased average donations by nearly 40%. This positive impact decreased when using a richer video message, including visual cues. There was no significant difference between a video and an audio message. Compared to a text message, a video message increased donations by only 20%, but this difference was insignificant. Further analysis revealed that ratings on individual characteristics of the charity members in the messages could not explain the differences in donation behavior. Thus, the authors suggest that the reduced anonymity in the audio messages promoted prosocial behavior.

In the gig economy, Nieken (2023) investigates whether instructions presented via text, audio, or video impact subjects' performance in a subsequent text transcription task. In addition to the virtual communication channel, the author varied whether instructions were neutral or enriched with charismatic leadership tactics (CLTs). While all charisma instructions contained verbal CLTs, such as metaphors, nonverbal CLTs, such as animated tone of voice, body gestures, or facial expressions, were conveyed depending on the richness of the channel. Although one might expect the video instructions to be superior, richer communication, including nonverbal cues and signals, backfired in the neutral setting compared to text and audio instructions. The quantitative output significantly decreased when variables for moderating effects and control variables were added. Auditory cues and signals in audio instructions helped to marginally significantly increase the quantitative performance compared to standard text instructions. In contrast, qualitative performance was similar across the different instructions in the neutral settings. If instructions included CLTs, quantitative and qualitative output did not significantly vary between text, audio, and video. When comparing the effect of CLTs within a communication channel, performance did not differ after text or audio instructions, depending on whether CLTs were present. However, the output significantly increased after the charisma video instructions compared to the neutral video instructions. Based on these findings, Nieken (2023) suggests that, especially when communication is transmitted via video messages, verbal and nonverbal signals must be aligned and balanced to convey a congruent and holistic message. Otherwise, it can be disadvantageous.

2.4.4 Honesty and Promises in One-way Communication

Communication is intended to transmit information in various day-to-day situations where honesty is crucial. However, incentives for dishonesty can also be present. Policyholders may have financial incentives encouraging dishonesty when reporting insurance claims. Job applicants may exaggerate their experience, skills, or qualifications in application documents to enhance their chances of getting hired or obtaining a higher salary. Such scenarios align with economic models of dishonest behavior, exemplified by experimental paradigms like the die roll task (Fischbacher & Föllmi-Heusi, 2013) or the coin toss task. Research has shown that dishonest behavior is influenced by various factors, including observability, the magnitude of the lie, or potential monetary gains or losses (Abeler et al., 2019; Dufwenberg & Dufwenberg, 2018; Gneezy et al., 2018). In the context of communication, individuals also exhibit a preference for not being seen as liars (Abeler et al., 2014; Khalmetski & Sliwka, 2019), and such image concerns might be impacted when communication channels vary the degree of anonymity.

Conrads and Lotz (2015) examine how using different communication channels (text, audio, and face-to-face) affects dishonesty when subjects report the results of a coin toss task to the experimenter. They revealed that payoff-maximizing lying increased when communication channels allowed for higher anonymity and social distance. However, pairwise comparisons on maximum lying were only significant when anonymity and distance were greatest, with subjects reporting their results via text from home compared to face-to-face in the lab. This is in line with research showing that human presence is vital in honest behavior. Dishonesty was significantly less prevalent when subjects verbally reported their outcomes compared to self-reporting without interaction with the experimenter (Pascual-Ezama et al., 2015), and subjects lied significantly less when interacting with a human compared to a machine (Cohn et al., 2022).[6] Abeler et al. (2014) and Cohn et al. (2022) support the findings that there are no large differences in honesty between audio and text communication. Abeler et al. (2014) showed that only extreme lying behavior was significantly more frequent when outcomes were reported to the experimenter by text-based selection than via phone. The overall results on dishonesty did not differ significantly. Furthermore, subjects were similarly honest when reporting the result of a coin toss task to the experimenter via chat as when they reported it via a Skype call (Cohn et al., 2022). Overall, the transition from face-to-face communication to highly anonymous text communication may influence honest reporting. However, this effect can be diminished by using less anonymous and richer communication channels, such as audio.

There is little evidence about promising and keeping promises, but it does shed light on the importance of media synchronicity (Conrads & Reggiani, 2017). In a non-binding set-up without formal obligations (taking part in a short online survey),[7] different communication channels that varied according to media richness and synchronicity did not influence behavior regarding keeping a promise. But, making a promise was affected. More synchronous face-to-face, audio, and chat channels yielded similar promise-making rates, indicating that media richness had no impact. However, communication channels with higher media synchronicity compared to asynchronous text channels (online survey in the lab or from home), in which subjects had more time to think about making a promise, resulted in significantly higher promise-making

[6]The results by Nieken and Walther (2024) support the importance of human presence. They showed that subjects chose text messages significantly more often than video messages when being dishonest compared to being honest, and suggest a lower perceived human presence in text messages as a possible mechanism.

[7]The authors intentionally did not financially incentivize promise-making and promise-keeping since they were interested in genuine promises as a pure commitment to help.

rates.

In summary, anonymous text channels that allow asynchronous communication can increase the risk of dishonest behavior and decrease promise-making. However, using less anonymous and more synchronous communication methods, such as audio, especially when virtual communication is necessary, may help diminish these risks.

2.5 Discussion

The insights from this review suggest that shifting from face-to-face to virtual communication can influence human behavior and decision-making. However, there is some evidence that richer communication channels, such as video or audio, can mitigate these effects and achieve effectiveness comparable to face-to-face communication, especially in two-way communication. In the following, I discuss the findings presented in Section 2.4 in the context of the ongoing debate about the trend from in-person to virtual work in companies, organizations, and research. Thereafter, I address limitations and point out future research opportunities.

In recent years, remote and hybrid work arrangements have gained significant prominence (Barrero et al., 2023; Gallup, 2024). Beyond traditional employment, online labor platforms like Upwork and Amazon Mechanical Turk have also experienced substantial growth (Kässi et al., 2021). Studies suggest benefits such as increased job satisfaction and reduced attrition without compromising performance (Bloom et al., 2022, 2024). Yet, many firms aim to enforce full-time office returns, reflecting a divide between employer and employee expectations (Barrero et al., 2021). This ongoing debate on the future of work is shaped by evolving communication dynamics and reliance on virtual communication channels (Yang et al., 2022). The experimental findings examined contribute to this discussion.

In two-way interdependent strategic interactions, selecting appropriate communication channels is crucial. Relying solely on text communication can be risky and decrease group cooperation (Bochet et al., 2006; Frohlich & Oppenheimer, 1998; Rockmann & Northcraft, 2008), prosocial behavior (Greiner et al., 2014), and reciprocity (Bicchieri et al., 2010; Lev-On et al., 2010). Although evidence is still scarce, video communication can close this gap compared to face-to-face communication, particularly when cooperation is essential (Brosig et al., 2003; Rockmann & Northcraft, 2008). In settings where creativity and innovation are vital, evidence suggests that fully remote work arrangements may pose challenges. While video communication channels can be just as effective as face-to-face interactions in a highly visual creative task that requires collaboration (Grözinger et al., 2020), research highlights the advantages of face-to-face communication in fostering innovative ideas (Brucks & Levav, 2022; Grund et al., 2025). This is consistent with findings when comparing office-based, hybrid, and remote work arrangements (Gibbs et al., 2024). However, these caveats can be mitigated by switching from fully remote to hybrid settings (Grund et al., 2025), which supports adopting hybrid work as an effective working arrangement (Choudhury et al., 2024). For dishonest behavior in one-way communication, there seem to be no large differences when transitioning to virtual communication, nor when comparing audio and text communication (Abeler et al., 2014; Cohn et al., 2022; Conrads & Lotz, 2015). But, when aiming to shape the behaviors of others, e.g., to motivate online workers or to increase donations via messages, findings are more mixed. In such contexts, for instance, when CEOs address employees, politicians communicate with the public, or researchers provide instructions to subjects, more cues or signals are not always better. Video messages that include nonverbal cues and signals are not always beneficial and can be less effective than less rich messages via text or audio (Nieken, 2023; Zylbersztejn et al., 2024b). It is crucial to ensure that verbal and nonverbal information is aligned to convey a consistent message (Nieken, 2023). In summary, this review shows that relying solely on virtual communication channels when working remotely presents challenges. However, these challenges can be mitigated by adopting proper communication strategies, suggesting that traditional office-based work is not necessarily required. Hybrid working arrangements that leverage the benefits of remote and office-based work can be an effective approach to exploit the full potential of communication.

Beyond its implications for the workplace, this review offers valuable suggestions for experimental research involving communication. The shift from physical laboratories to online environments has been accelerated by platforms such as Prolific and Amazon Mechanical Turk (Douglas et al., 2023). In 2022 alone, over 150,000 studies were published on Prolific (Tomczak et al., 2023), illustrating the growing reliance of researchers on virtual communication for conducting research. During the design phase of a study, selecting a particular communication channel, whether for interaction between subjects or for transmitting information from experimenters to subjects, is a critical decision that should not be neglected. This choice can significantly influence how subjects behave, potentially introducing unintended effects. Furthermore, when comparing and discussing findings across different studies in literature reviews or meta-analyses, the used communication channel might impact the comparability of results (e.g., as demonstrated in Balliet (2010)). Consequently, overlooking this factor, where communication plays an important role, may lead to overly simplistic or inaccurate conclusions.

It is important to emphasize that this review is only a first step towards understanding how face-to-face and various virtual communication channels influence human behavior and decision-making. One limitation is that this review synthesized findings from only 21 experimental studies, implying that the current state of research limits the conclusions and implications. The impact of communication channels in cooperative settings has been studied more extensively, revealing some mixed findings. While Abatayo et al. (2018) found no significant differences between face-to-face and text communication in fostering cooperation within a public goods game, several other studies have demonstrated the superiority of face-to-face communication and richer communication channels in enhancing group cooperation (Bochet et al., 2006; Brosig et al., 2003; Frohlich & Oppenheimer, 1998; Rockmann & Northcraft, 2008). Although the experimental settings differed to a greater or lesser extent, such as in the experimental task or group size, the more recent findings by Abatayo et al. (2018) may suggest that individuals have become increasingly accustomed to virtual communication, potentially diminishing the differences observed in earlier research. However, further studies are needed to gain a more comprehensive understanding of this possible phenomenon. Besides cooperation, there is still a lack of research on trust, reciprocity, negotiation, and creativity. Moreover, behavior in some key economic settings has yet to be studied in the context of using different communication channels, including coordination (Brandts & Cooper, 2006a; Van Huyck et al., 1990), lying between subjects (Gneezy, 2005; Gneezy et al., 2013), or competitive behavior in contests (Konrad, 2009). Therefore, this paper emphasizes that expanding research across different settings and communication channels is essential in order to achieve a more thorough understanding.

Another limitation is identifying mechanisms that explain why the use of different communication channels influences behavior and decision-making in certain situations. Communication channels can vary in several characteristics. This makes it difficult to draw causal conclusions or derive mechanisms, especially when comparing rich face-to-face or video channels with text-based channels, which are compared in almost half of the experimental studies reviewed in this paper (see Figure 2.2). Some studies addressed this challenge by using standardized messages to isolate the effects of paraverbal and nonverbal information, thus allowing a more distinct understanding (Babutsidze et al., 2021; Nieken, 2023; Zylbersztejn et al., 2024b). They reveal that it is possible to identify mechanisms when thoroughly designing experiments. For instance, future research could compare audio communication without photos of subjects' faces with audio communication including subjects' faces to isolate the effect of anonymity and identification. In addition, Brucks and Levav (2022) conducted in-depth analyses using state-of-the-art eye-tracking technology to show that the physical nature of video communication was a mechanism that hindered creativity in teams. Such technologies might also reveal interesting outcomes when comparing text, audio, and video communication channels. Moreover, using methods such as Latent Dirichlet Allocation (LDA) (Blei et al., 2003) or other communication content analysis tools allows the analysis of the communication content in interactive settings without restricting communication or employing standardized one-way messages.

2.6 Conclusion

This paper presents a comprehensive review examining current experimental findings on how face-to-face and virtual communication channels affect human behavior and decision-making. I focused on three virtual communication channels—text, audio, and video—alongside face-to-face. The experimental findings revealed that in two-way communication, shifting communication from face-to-face to virtual communication channels can reduce the positive impact of communication. This can be reduced by utilizing richer video communication channels. In one-way messages, higher media richness is not always beneficial. In particular, video messages aimed at motivating employees or encouraging donations can be equally effective or even less effective than text or audio messages, indicating potential risks associated with increased nonverbal information. The findings presented in this paper are particularly relevant given the ongoing shift toward virtual work. They underscore the importance of selecting appropriate communication channels to increase effectiveness in various settings. Thus, these insights inform decision-makers in companies and organizations that rely on remote and hybrid work arrangements and researchers who conduct experiments on online platforms. However, this review serves only as an initial step toward a more comprehensive understanding of the topic, highlighting the need for further research to refine and expand these insights.

3 Study II: Honesty in Virtual Communication*

Abstract: Remote work arrangements and increased virtual communication are commonplace. Particularly in organizations, virtual communication has become an essential tool for collaboration and exchanging information. Virtual communication channels, such as text or video messages, provide different levels of human presence compared to face-to-face communication. Given that human presence is known to impact moral behavior, this raises the question if different communication channels are used when being dishonest. To investigate this question, we conducted a controlled experiment using a sender-receiver deception game where the senders could choose between a text or a video message. In the baseline condition, the senders had to be honest and were not allowed to lie. In the treatment condition, the senders had the option of sending an honest or a dishonest message to the receivers. Even though we observe no differences in channel choice if we compare the two treatments, our results, however, show that in the treatment condition, the senders chose the text communication channel significantly more often when being dishonest compared to being honest. We discuss different potential mechanisms, such as differences in perceived human presence between text and video communication, for our findings. Our findings have important implications for various contexts and for strategies to prevent dishonest behavior.

Keywords: digitization; virtual communication; communication channel; honesty; human presence

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3.1 Introduction

Does virtual communication facilitate dishonest behavior? The emergence of fake news and misinformation suggests that the possibility of communicating anonymously online provokes these types of immoral behavior. In addition, employees and companies are concerned that remote work arrangements reduce social ties and opportunities for small talk among workers, impeding social relationships. Given that working from home and remote work arrangements are commonplace (Barrero et al., 2021), employees have to rely on virtual communication within organizations. Therefore, it is vital to understand if and how the choice of communication channels impacts information exchange and honest communication. In this paper, we investigate the relationship between the choice of the communication channel and dishonest behavior in a controlled experiment. The results provide a first insight into this complex topic.

Although virtual communication channels simplify the interaction between employees, they replace face-to-face interaction to some extent and, thus, diminish human presence in daily communications. This issue depends on the communication channel used, as they differ in their degree of allowing human presence (see, e.g., Short et al., 1976, for social presence theory), but also in their degree of allowing nonverbal communication cues (see, e.g., Daft & Lengel, 1986, for media richness theory). In this paper, we concentrate on two common communication channels, text and video messages, which have become increasingly important, especially in hybrid work arrangements (Bloom et al., 2022). While text messages are widely used, one-way video communication is also becoming increasingly popular in both professional and everyday contexts, e.g., as video clips in MS Teams (Microsoft, 2025), as video notes in WhatsApp (WhatsApp, 2025), or as video résumés in job applications (Indeed, 2025; TikTok, 2021). We investigate if and how the chosen channel relates to the communication content. Previous research indicates that human presence can impact moral behavior in general (Abeler et al., 2014; Cohn et al., 2022; Conrads & Lotz, 2015), and in particular dishonesty, which has already been extensively studied (Abeler et al., 2019; Gneezy et al., 2018; Khalmetski & Sliwka, 2019).

Our primary focus is to shed light on the question of whether people prefer text or video messages when being dishonest. We are also interested in whether the option to send dishonest messages impacts the choice of the communication channel compared to a situation where the message has to be honest. Third, we investigate the signaling value of the chosen communication channels.

To answer these questions, we conducted a controlled experiment using a modified sender-receiver deception game (Gneezy et al., 2013) consisting of two parts. In Part 1, the senders privately observed a random integer $1 \leq s \leq 6$ and had to send a pre-written message, "The assigned number is r", with $1 \leq r \leq 6$, to the receivers. In contrast to the classic deception game, the senders had the choice between sending a text or video message to the receivers. Afterward, the receivers decided whether to follow the message or not. The sender's payoff increased linearly with the reported number r and did not depend on the receiver's reaction. This payment structure reduced strategic concerns for the senders when choosing a channel. In our two treatments, we varied whether or not the senders were allowed to lie to the receivers. In the *NoChoice* treatment, the senders had no choice to be dishonest and revealed the true observed integer. This treatment serves as a baseline to control for differences in the channel choice irrespective of dishonest behavior. In the *Choice* treatment, the senders could choose to send an honest or a dishonest message to the receivers, and our payment structure incentivized the senders to lie. To rule out gender effects, we formed pairs of senders and receivers with the same self-chosen gender. Given that the focus of the paper is not on gender differences, we opted for a pure male sample and discuss potential limitations in Section 3.5.

Our results reveal no significant differences regarding the channel choice between our two treatments. However, we observe that the senders in the *Choice* treatment chose significantly more often text messages when being dishonest. A potential mechanism for this result is that the senders perceived significantly less human presence in text messages compared to video messages, which might have reduced lying costs. In addition, some senders preferred the video message in the *Choice* treatment to signal honesty. Since the receivers' follow decisions did not significantly vary between text and video messages in the *Choice* treatment, the senders' signals were not crucial for the receivers.

Our paper relates to two strands of literature: (i) Literature studying moral behavior in economic decision-making and (ii) literature on virtual communication.

Our findings contribute to the literature studying moral behavior in economic decision-making and will help to understand the cognitive and behavioral factors that drive dishonest behavior better (e.g., see Abeler et al., 2019; Dufwenberg & Dufwenberg, 2018; Gneezy et al., 2018; Khalmetski & Sliwka, 2019; Lundquist et al., 2009; Pascual-Ezama et al., 2015). Particularly relevant to our paper is that human presence is crucial in reducing dishonesty because individuals are more likely to behave dishonestly when interacting with a machine rather than with a human being (Cohn et al., 2022). We contribute to a better understanding how different levels of human presence arise in virtual settings without face-to-face interaction. Our results suggest that human presence increases when people unveil their faces and voices in video messages compared to text messages. This difference in human presence between the different communication channels may allow people to self-select into the channel with less human presence to decrease their cost of lying.

Furthermore, our paper broadly links to an extensive body of research that studies the effect of communication on economic behavior (e.g., Bicchieri and Lev-On, 2007; Charness and Dufwenberg, 2006; Cooper et al., 1992; Crawford, 1998; He et al., 2017; Isaac and Walker, 1988; Lundquist et al., 2009). We contribute to the literature on virtual communication that examines how different communication channels affect economic behavior, including but not limited to cooperation and coordination (Bochet et al., 2006; Brosig et al., 2003), charisma and performance (Nieken, 2023), creativity (Grözinger et al., 2020), trust and trustworthiness (Babutsidze et al., 2021; Zylbersztejn et al., 2020, 2021), or bargaining (Valley et al., 2002). This research is of great importance because it demonstrates that using more appropriate communication channels in certain situations can increase, among other things, efficiency or effectiveness in several day-to-day situations. Even more connected to our research Abeler et al. (2014), Cohn et al. (2022), and Conrads and Lotz (2015) studied the effect of different communication channels on dishonest behavior and revealed, among other things, that there are no significant differences between text and audio communication channels. However, Conrads and Lotz (2015) showed that there are differences in dishonesty between text and face-to-face communication. The impact of video communication channels, which are the closest to face-to-face communication, is still unknown in the current state of research, and our paper aims to close this research gap. Moreover, most studies in this research area focused on subjects' behavior when preselected into specific communication channels. People in everyday situations, however, often have the choice to choose their preferred communication channel and, therefore, could choose communication channels that simplify lying. Our study is intended to be a starting point in this relatively new area of research to investigate people's preferences for different communication channels.

Our study provides important implications since our results suggest a connection between dishonest behavior and the chosen communication channel. Institutions, organizations, and individuals should be aware of this and prioritize video communication channels in situations where honest behavior is decisive. More generally, it is crucial to understand that human presence is higher when communicating via video communication channels, and human presence is an essential factor for moral behavior, such as dishonest behavior (Cohn et al., 2022). Furthermore, our results suggest that, especially in situations where higher information asymmetries exist, the communication channel is not just a tool to transmit information, but also the choice of the communication channel might be used as a signal. Some people perceive video communication channels as more credible, trustworthy, or honest and try to signal their honest intentions by choosing this channel. Hence, decision-makers and organizations should be aware that selecting a communication channel could convey information about their intentions.

The remaining paper is structured as follows: In Section 3.2, we describe the experimental design, the procedures and derive our hypotheses. In Section 3.3, we explain our main variables of interest, sample selection, and empirical strategy. We present our results in Section 3.4 and discuss them in light of a series of behavioral mechanisms that may drive them in Section 3.5. Section 3.6 concludes our paper.

3.2 Experimental Design and Hypotheses

In the following, we first describe our general experimental setup, including the modified sender-receiver deception game (Gneezy et al., 2013). Then we provide details on our two treatments and the procedures before explaining our hypotheses.[1]

3.2.1 Design Overview

We conducted a controlled experiment and used a modified two-player sender-receiver deception game (Gneezy et al., 2013) consisting of six rounds. Each round had two parts. In both parts, two players were randomly matched to form a pair in each round. One player was in the role of the sender, and one player was in the role of the receiver. In Part 1, each pair was randomly assigned an integer $1 \leq s \leq 6$. Each integer was equally likely. We will refer to s as the "assigned number." Only the sender was informed about the assigned number. Afterward, the sender was asked to record a pre-written message, "The assigned number is r" with $1 \leq r \leq 6$. We will refer to the message content as the "reported number." The sender could use either a

[1]We preregistered our study before data collection at https://aspredicted.org/blind.php?x=7R5_71C

text or video communication channel to record the message. Note that the sender had to spend at least 40 seconds on this page, irrespective of the chosen channel. This ensured that choosing a text message did not allow the sender to save time. The sender could only choose between pre-written messages for the text communication channel. When the sender chose the video communication channel, he[2] was only allowed to record the sentence, "The assigned number is r", to keep the verbal content identical between both channels. In Part 2, the receiver received the text or video message from the sender. He then had to decide whether or not to trust the message and follow it.

The sender's payoff increased linearly with the reported number r and neither depended on the assigned number s, the chosen communication channel, nor the receiver's reaction. The sender's payoff π_s in experimental currency units (ECU) was:

$$\pi_s = 10 + 3 \cdot r$$

The receiver had two options. He could follow the sender's message, or he could not follow the message. If the receiver followed the message and it contained the assigned number ($r = s$), he received 10 ECUs. If he followed and the message did not contain the assigned number ($r \neq s$), he received 0 ECU. If he did not follow the message, his payoff was 3 ECU. Hence, the sender's decision to lie affected the receiver's payoff. The receiver's payoff was:

$$\pi_r = \begin{cases} 10 & \text{if the receiver followed and } r = s \\ 0 & \text{if the receiver followed and } r \neq s \\ 3 & \text{if the receiver did not follow} \end{cases}$$

Both payoff structures and the sequence of events were common knowledge. In total, subjects played six rounds, and sender-receiver pairs changed every round following a perfect-stranger matching to avoid moral balancing (Ploner & Regner, 2013). In the first round, subjects played the standard deception game using text messages. The purpose of the first round was to understand the sender's general honesty behavior better. We used the strategy method (Selten, 1967) to elicit the senders' behavior. Payoffs in this round depended on the assigned number s and the respective decisions from the senders and the receivers. Afterward, the senders and the receivers played five consecutive rounds (Rounds 2 to 6) of our modified deception game

[2] We use the masculine form because we only hired male subjects in this study.

with the channel choice. A computer randomly selected two payoff-relevant rounds with equal probability to mute potential income effects. No subject received any information about the game outcomes, the other subjects' actions, the assigned number in Round 1, or the payoff-relevant rounds in Rounds 2 to 6 until the end of the experiment. Next, subjects had to answer a brief questionnaire containing questions on perceived human presence and social image concerns for both communication channels (Cohn et al., 2022), on experience and usage of text and video communication channels, on the competence self-concept related to the use of information and communication technology (ICT), on online and mobile communication, and on personality and general interpersonal trust. We also collected demographic information, including the subjects' age, field of study, education, past participation in experiments, and self-reported attention. Lastly, we included an optional text field in which we asked if subjects had any comments on the experiment. See Section 3.3.1 and Appendix A.1 for further details.

3.2.2 Treatments

We conducted two treatments, which varied only with regard to Rounds 2 to 6, in which the senders and the receivers played our modified deception game. In the *NoChoice* treatment, the sender had no choice about the communication content because they had to send an honest message to the receiver. It was common knowledge for both subjects that the message had to be honest. In the *Choice* treatment, the sender had the choice of whether to send an honest or a dishonest message. The sender knew that the receiver had no information about whether the message was honest or dishonest. Hence, compared to the *NoChoice* treatment, information asymmetries existed between the sender and the receiver only in the *Choice* treatment. In the *NoChoice* treatment, the messages in Rounds 2 to 6, from the senders to the receivers, were only an information transmission. Therefore, the *NoChoice* treatment serves as a baseline for the senders' channel preferences compared to the *Choice* treatment, where the senders could lie and information asymmetries existed.

3.2.3 Procedures

We first describe the general procedures before addressing the specific procedures for the senders and the receivers.

Recall that our experiment consisted of two parts, Part 1 and Part 2. To minimize the potential confound that the senders and the receivers knew each other, we conducted Part 1 and Part 2 using different subject pools. Part 1 elicited the behavior of the senders and was conducted in the Karlsruhe Decision and Design Lab (KD^{2}Lab).[3] We used hroot (Bock et al., 2014) to recruit the senders. The game in Part 1 was programmed in oTree (Chen et al., 2016), and the online questionnaire to elicit personal data, such as demographics, was generated using SoSci Survey. In Part 2, we elicited the behavior of the receivers. Part 2 was conducted online via Prolific (www.prolific.co), and we used SoSci Survey to generate the online questionnaire. We hired Prolific subjects located in Germany, Austria, or Switzerland. We only hired male subjects for both parts to exclude gender effects (see Section 3.5 for a brief discussion). We only hired German-speaking subjects to mitigate confounds due to a lack of language proficiency. We first collected all observations from the senders in Part 1, and a few weeks thereafter, we conducted Part 2 with all the receivers. The general procedures were identical for the senders and the receivers. Before starting Round 1 and Round 2, subjects had to answer control questions to understand the game rules. In the end, subjects answered a questionnaire on attitudes and demographics (see Section 3.3.1). In addition to the payoff for Rounds 1 to 6, subjects received a show-up fee. In Part 1, we implemented an exchange rate of 1 ECU = €0.10, and we ran 15 sessions with six to seven senders on average. Their average completion time was approximately 25 minutes, and they earned €9.21 on average. This results in average hourly earnings for the senders of around €13.82.[4] In Part 2, we implemented an exchange rate of 1 ECU = £0.09. The receivers' average completion time was approximately 20 minutes, and they earned £3.37 (approximately €3.82) on average. This results in average hourly earnings for the receivers of around €11.46. We now describe the specific procedures for the senders in Part 1 (for a graphical illustration, see Figure A.1 in Appendix A.2). The senders came to the KD^{2}Lab and were randomly assigned to one of 40 air-conditioned and soundproofed one-person cubicles (see Figure A.2 in Appendix A.2 for an exemplary picture of the setup in the cubicle). Before the start of Round 2, we described in detail how the video recording worked, and the senders could ask the experimenters throughout the experiment if they encountered problems. As described above, we executed all sessions of Part 1 before conducting Part 2 on Prolific.

[3]The KD^{2}Lab has been funded by the DFG and the Karlsruhe Institute of Technology (INST-12138411-1FUGG).

[4]As is common practice for laboratory experiments, the completion time does not include the time for traveling to and from the lab as well as the waiting time before the start of the experiment. We assume that these additional times are, on average, around 15 minutes because subjects often live close by or participate in a break between lectures on campus.

We implemented a technical check at the beginning of Part 2 to ensure that the audio and video were working. The receivers could only continue the experiment if they passed this test (for a graphical illustration of the procedures for the receivers, see Figure A.3 in Appendix A.2).

3.2.4 Hypotheses

Our main focus is studying (dis)honesty and the choice of the communication channel. Therefore, our primary interest lies in the senders' behavior. In the second step, we also study the receivers' reaction. Recall that messages in the *NoChoice* treatment were only an information transmission because the senders were not allowed to lie and, thus, had no choice over the content. This was common knowledge. Therefore, the *NoChoice* treatment serves as a baseline to understand the senders' general channel preferences (e.g., some senders might prefer text over video messages because they have an aversion to showing themselves in a video). The *Choice* treatment adds other possible motives to this baseline. Here, the senders have the choice to send an honest or a dishonest message to the receivers. Another motive we expect in the *Choice* treatment is that subjects prefer text messages when lying to the receiver. The findings from Cohn et al. (2022) suggest that human presence is key to mitigating dishonest behavior. In their study, subjects who were more prone to dishonest behavior preferred to avoid human interactions. Furthermore, Abeler et al. (2014) and Conrads and Lotz (2015) revealed that subjects' extremely dishonest behavior increased for a more anonymous text communication channel compared to audio or face-to-face communication. Given that the senders were not allowed to lie in the *NoChoice* treatment, we expect that the additional motive to prefer text messages in the *Choice* treatment leads to differences between both treatments in the chosen communication channel. We therefore formulate the following hypothesis:

Hypothesis 1

The senders' share of video messages is lower (share of text messages is higher) if the senders have the option to be dishonest (*Choice* treatment) compared to a setup in which they have to be honest (*NoChoice* treatment).

The additional motive to prefer text over video messages in the *Choice* treatment should only impact the channel choice if subjects intend to send a dishonest message. As we compare senders' behavior within one treatment, we expect that there is a positive correlation between sending a text message and lying. Thus, we formulate the following hypothesis:

Hypothesis 2

Within the *Choice* treatment, the senders' share of video messages is lower (share of text messages is higher) if the senders are dishonest than if the senders are honest.

The chosen channel has no informational value for the senders in the *NoChoice* treatment because the message is always truthful. This was common knowledge. In contrast, the chosen channel might be perceived as a signal in the *Choice* treatment. Eckel and Petrie (2011) showed that receivers in the trust game (Berg et al., 1995) are more trustworthy if they have the opportunity to see a photo of the senders. Thus, in our modified deception game, sending a video message and revealing one's face and identity might be perceived as a signal of trustworthiness. Additionally, senders trust less if the setup between both subjects is more anonymous (Barmettler et al., 2012; Johnson & Mislin, 2011). Therefore, in our setup, we expect that a receiver's decision to trust relates positively to receiving a less anonymous video message. We thus formulate the following hypothesis:

Hypothesis 3

Within the *Choice* treatment, the receivers' share of follow decisions is higher when receiving video messages compared to text messages.

3.3 Data and Estimation Strategy

In the following, we describe the main variables used in the analyses and provide information on the sample and our estimation strategy.[5]

3.3.1 Variables of Interest

Our main variables of interest concern the senders' behavior in Rounds 2 to 6, in which they played five consecutive rounds of our modified deception game with the communication channel choice. In particular, we are interested in whether the senders chose the text or the video channel. Second, we are interested in whether the channel preferences differed between the *NoChoice* treatment and the *Choice* treatment. We use an indicator variable *Choice Treatment* to analyze the differences between the two treatments, which is one if a subject was part of the *Choice* treatment and zero otherwise. To analyze the channel preferences, we use an indicator

[5] All control variables used in the analyses in Section 3.4 and Appendix A.2 are described in Appendix A.1.

variable *Video*, which is one if a sender decided to send a video message and zero otherwise. The variable *Share Video* refers to the share of the five rounds, in which a sender chose the video message, ranging from zero (only text messages) to one (only video messages). For the *Choice* treatment, we are also interested in the senders' decision to send an honest or a dishonest message and how this decision interacts with the channel choice. To analyze the content of the message, we use an indicator variable *Dishonest*, which is one if a sender's message was dishonest ($r \neq s$) and zero otherwise.

To study potential mechanisms, we followed Cohn et al. (2022) and elicited the senders' perceived human presence from the receivers. For each communication channel, we used three items in which the senders self-stated their perceived human presence on a 7-point Likert scale (see Table A.1 in Appendix A.2). Δ *Human Presence*$_{\textit{Video - Text}}$ indicates the difference in perceived human presence between video and text messages. Positive values of Δ *Human Presence*$_{\textit{Video - Text}}$ indicate that the senders perceived a higher human presence in video messages than in text messages. Negative values indicate that human presence was lower in video messages than in text messages.

Our main variable of interest for the receivers' behavior is the decision to follow the senders' messages in Rounds 2 to 6. The indicator variable *Follow* is one if a receiver followed a sender's message and zero otherwise. The variable *Share Follow* refers to the share of the five rounds, in which a receiver followed the message, ranging from zero (no follow decisions) to one (only follow decisions). To analyze the communication channel of the message, we use an indicator variable *Video Message*, which is one when receiving a video message and zero otherwise.

3.3.2 Sample Selection

For Part 1, we gathered observations from 100 subjects. We conducted two attention checks in our post-experimental questionnaire, in which one subject failed one of the two attention checks.[6] We excluded observations from two senders due to technical issues (no sound, no video storage). Four senders used nonverbal cues, such as shaking their heads, to indicate that they were honest or dishonest in their message. We excluded observations from these senders. This leaves us with observations from 94 senders in our analytical sample (30 senders in the *NoChoice* treatment and 64 senders in the *Choice* treatment). Due to the excluded observations

[6] In line with our preregistration, this subject was not excluded because we only excluded subjects that failed both attention checks.

in Part 1, for Part 2, we gathered observations from 94 subjects. Again, we conducted two attention checks in our post-experimental questionnaire, which seven subjects failed and were excluded from the data set. This leaves us with observations from 87 receivers in our analytical sample (27 receivers in the *NoChoice* treatment and 60 receivers in the *Choice* treatment). See Appendix A.2 for demographic information of the senders in Table A.3 and in Table A.4 for the receivers.

3.3.3 Empirical Strategy

Our empirical strategy is as follows. We analyzed the senders' and the receivers' behavior separately. We started our analyses with non-parametric tests. Next, we estimated a series of regressions to answer our questions. Since we are interested in the subjects' behavior over five rounds and for all hypotheses the dependent variables (*Video* and *Follow*) are binary, we estimated a series of panel probit regressions where i indexes subjects and t indexes rounds.[7]

To investigate Hypothesis 1, we analyzed the sender behavior and first introduced the variable *Choice Treatment* to show the pure treatment effect. In the second specification, we added the variable *Round* and the variable *Assigned Number*. To test further potential mechanisms, we also included Δ *Human Presence*$_{\textit{Video - Text}}$ (Δ *Human Presence* in the regression equations below). In the third specification, we include the interaction between *Choice Treatment* and Δ *Human Presence*$_{\textit{Video - Text}}$. In the last specification, we added controls for demographic information.[8] This results in the following regression equation:

$$P(video_{it} = 1|X_{it}, \alpha_i) = \phi(\beta_0 + \beta_1 choicetreatment_i + \beta_2 round_{it} + \beta_3 assignednumber_{it} \\ + \beta_4 \Delta humanpresence_i + \beta_5 choicetreatment_i \times \Delta humanpresence_i + \beta_6 controls_i + \alpha_i)$$

We use a similar strategy when investigating Hypothesis 2, but restrict the sample to the sender behavior in the *Choice* treatment. First, we introduced the variable *Dishonest* to show the pure effect of whether the senders sent an honest or a dishonest message. The second specification is enriched with controls for the *Round* and the *Assigned Number*, and again includes Δ *Human*

[7] $video_{it}$ and $follow_{it}$ are the binary dependent variables for subject i in round t; X_{it} is the collection of independent variables for subject i in round t and are more detailed defined in the sum of $\phi(\cdot)$; α_i is the unobserved subject-specific effect (random effect); $\phi(\cdot)$ is the cumulative distribution function of the standard normal distribution

[8] In Appendix A.1, we provide a further specification of all other control variables used in the analyses in Appendix A.2 that are not included in the equations below.

Presence$_{\textit{Video - Text}}$. The third specification also contains the interaction with *Dishonest* to test for potential mechanisms. The fourth specification includes demographic controls. This leads to the following regression equation:

$$P(video_{it} = 1|X_{it}, \alpha_i) = \phi(\beta_0 + \beta_1 dishonest_{it} + \beta_2 round_{it} + \beta_3 assignednumber_{it}$$
$$+ \beta_4 \Delta humanpresence_i + \beta_5 dishonest_{it} \times \Delta humanpresence_i + \beta_6 controls_i + \alpha_i)$$

For Hypothesis 3, we analyze the receiver behavior. We first introduced the variable *Video Message* to show the pure effect of whether the receivers received a text or a video message. In the second specification, we added the variable *Round* and the content of the message using the variable *Reported Number*. In the third and last specification, we again added demographics, which results in the following regression equation:

$$P(follow_{it} = 1|X_{it}, \alpha_i) = \phi(\beta_0 + \beta_1 videomessage_{it} + \beta_2 round_{it} + \beta_3 reportednumber_{it}$$
$$+ \beta_4 controls_i + \alpha_i)$$

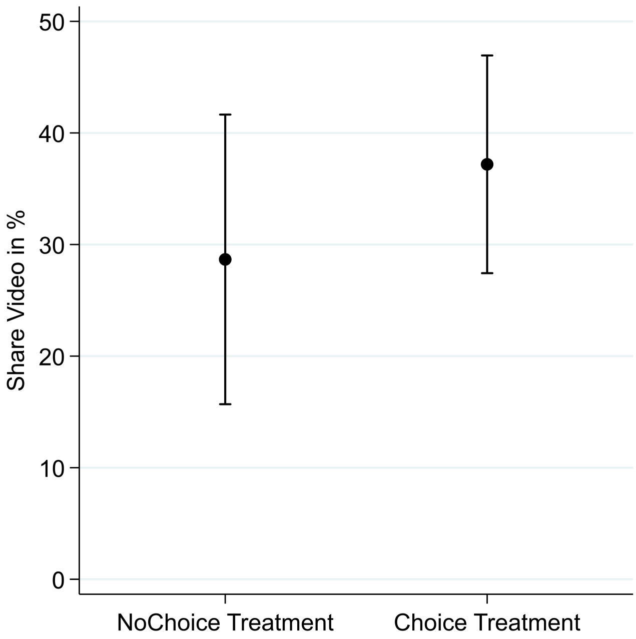
3.4 Results

In the following, we first analyze the senders' behavior and, afterward, the receivers' behavior.

3.4.1 Sender Behavior

We start with descriptive information about the senders' behavior in both treatments. *Share Video* was, on average, 28.67% in the *NoChoice* treatment and 37.19% in the *Choice* treatment (see Figure 3.1). Even though we observe a higher share of video messages in the *Choice* treatment, the difference is not statistically significant (two-sided Mann-Whitney U test, $p = 0.326$). When we look at behavior across rounds, 53.33% of senders in the *NoChoice* treatment and 43.75% of senders in the *Choice* treatment had a strong preference for the text channel and chose text messages in all five rounds. Whereas, 10.00% of senders in the *NoChoice* treatment and 18.75% of senders in the *Choice* treatment preferred video messages in all rounds.

A series of panel probit regressions reported in Table 3.1 support the initial impression that there is no significant difference in the channel choice between our treatments. Models (1) and



Note: Dots indicate averages and whiskers indicate 95% confidence intervals.

Figure 3.1: Share of video messages over all rounds by treatment

(2) list the main effects of our treatment variation and show that the coefficient for the *Choice* treatment is positive but not significant. Interestingly, a higher assigned number increased the likelihood that the senders preferred the video channel, whereas Δ *Human Presence*$_{\textit{Video - Text}}$ had a negative impact on the likelihood of sending a video. Recall that positive values of Δ *Human Presence*$_{\textit{Video - Text}}$ indicate that the senders perceived a higher human presence in video messages than in text messages. Thus, the negative coefficient in our regressions indicates that the senders preferred a text message if they perceived a higher difference in human presence.

In Model (3), we added the interaction effect *Choice Treatment* $\times$ Δ *Human Presence*$_{\textit{Video - Text}}$ to test if a higher perceived human presence in video messages than in text messages interacts with our treatment manipulation. We observe no significant effect ($p = 0.272$) between Δ *Human Presence*$_{\textit{Video - Text}}$ and *Choice Treatment*. The coefficient for Δ *Human Presence*$_{\textit{Video - Text}}$ shows that the overall effect on *Video* in the *NoChoice* treatment is negative (-0.848) and significant ($p = 0.010$). Using a two-sided F-test, we analyzed the overall effect of Δ *Human Presence*$_{\textit{Video - Text}}$ on *Video* in the *Choice* treatment revealing a negative (-0.431) significant effect ($p = 0.038$). Thus, Δ *Human Presence*$_{\textit{Video - Text}}$ has a significant effect on the channel choice in both treatments.

Dep. Var.: Video	(1)	(2)	(3)	(4)
Choice Treatment	0.569	0.519	-0.005	0.128
	(0.529)	(0.536)	(0.776)	(0.768)
Round		-0.003	-0.003	-0.003
		(0.059)	(0.059)	(0.059)
Assigned Number		0.200^{***}	0.200^{***}	0.202^{***}
		(0.072)	(0.072)	(0.072)
Δ Human Presence$_{\text{Video - Text}}$		-0.529^{***}	-0.848^{**}	-0.792^{**}
		(0.183)	(0.331)	(0.330)
Choice Treatment $\times$ Δ Human Presence$_{\text{Video - Text}}$			0.418	0.401
			(0.380)	(0.378)
Age				0.087
				(0.095)
Business and Economics				-0.422
				(0.511)
Constant	-1.342^{***}	-1.401^{**}	-0.977	-3.041
	(0.480)	(0.631)	(0.767)	(2.566)
Observations	470	470	470	470
Log pseudolikelihood	-215.578	-205.492	-205.039	-204.103
Δ *Human Presence*$_{\textit{Video - Text}}$ + *Choice Treatment* $\times$ Δ *Human Presence*$_{\textit{Video - Text}}$ = 0			0.038	0.054

Robust standard errors adjusted for 94 clusters (subjects) in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: The figures in the row "Δ *Human Presence*$_{\textit{Video - Text}}$ + *Choice Treatment* $\times$ Δ *Human Presence*$_{\textit{Video - Text}}$ = 0" are the p-values for the overall effect of Δ *Human Presence*$_{\textit{Video - Text}}$ on *Video* in the *Choice* treatment. Since Δ *Human Presence*$_{\textit{Video - Text}}$ is quasi-continuous, we used the sample mean of Δ *Human Presence*$_{\textit{Video - Text}}$ in the *Choice* treatment (1.193). For the complete table with all control variables, see Table A.5 in Appendix A.2.

Table 3.1: Panel probit regression (random effects) with *Video* as dependent variable and *Choice Treatment* as independent variable

Result 1 There are no statistically significant differences regarding the chosen channel between the *NoChoice* treatment and the *Choice* treatment.

Next, we investigate if there are differences between the chosen channels in the *Choice* treatment when the senders were honest or dishonest, and thus, whether there is a relation between the channel choice and the choice to lie. On average, the senders' share of dishonest messages in the *Choice* treatment was 48.13%. 15.63% of senders never lied over the five rounds, 10.94% always lied, and 73.44% switched between honest and dishonest messages. On average, the senders' share of video messages when lying to the receivers was 27.13%. In contrast, the senders' average share of video messages when being honest was 44.74%. A two-sided Wilcoxon signed-rank test comparing each sender's share of video messages when being dishonest to when being honest at the subject level shows that the sender's share of video messages was significantly lower when being dishonest compared to being honest ($p = 0.005$). Thus, the data suggest a correlation between the chosen channel and the decision to lie at the subject level. Figure 3.2 depicts the share of video messages over the rounds and reveals that the senders' average share of videos was lower when lying in each of the five rounds.

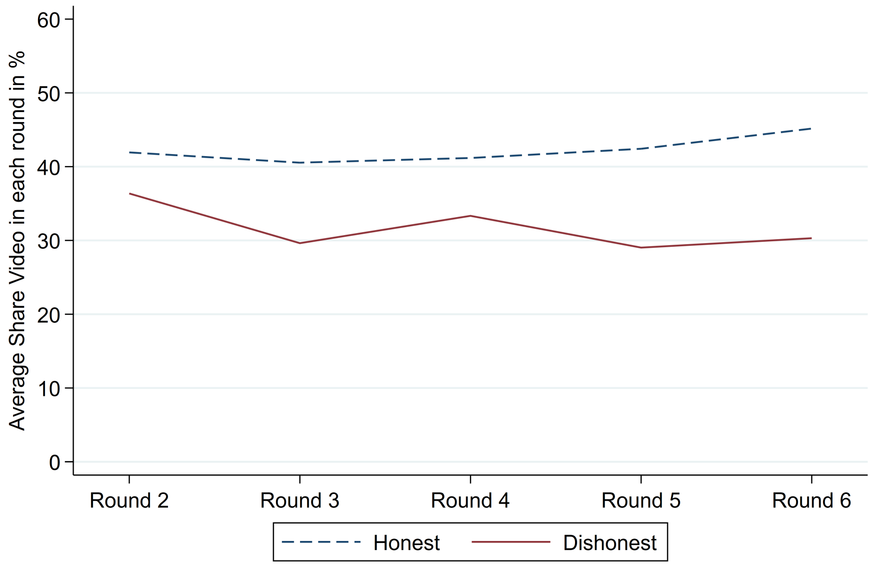


Figure 3.2: Share of video messages in each round by message content in the *Choice* treatment

To investigate if the impression from Figure 3.2 is reflected in an econometric analysis, we conducted another series of panel probit regressions reported in Table 3.2.[9] In Models (1) and (2), the main effects of *Dishonest* are negative, statistically significant ($p < 0.05$), and robust to *Round*, *Assigned Number* and Δ *Human Presence*$_{\textit{Video - Text}}$ as controls. The results support our hypothesis that the senders chose text messages more often when lying to the receivers. Similar to our previous findings, a higher perceived human presence in video messages than in text messages relates to a higher likelihood that the senders preferred text messages.

In Model (3), we added the interaction effect *Dishonest* $\times$ Δ *Human Presence*$_{\textit{Video - Text}}$ to test if a higher perceived human presence in video messages than in text messages interacted with the content of the message. The coefficient of the interaction effect *Dishonest* $\times$ Δ *Human Presence*$_{\textit{Video - Text}}$ is also negative (-0.388) but not significant ($p = 0.181$). Thus, there is a tendency for the senders' likelihood to send a text message to be negatively related to a dishonest message and a higher perceived difference in human presence between both channels. While the overall effect of Δ *Human Presence*$_{\textit{Video - Text}}$ on *Video* is negative (-0.377) but not significant ($p = 0.222$) when the senders were honest, the significance changes when the senders were

[9]We also conducted a series of panel logit regressions with fixed effects, but 62.50% of senders in the *Choice* treatment were excluded because they had chosen either text messages in all five rounds or video messages in all five rounds.

Dep. Var.: Video	(1)	(2)	(3)	(4)
Dishonest	-1.329***	-1.411**	-1.100*	-1.117*
	(0.401)	(0.581)	(0.604)	(0.608)
Round		-0.002	0.014	0.016
		(0.072)	(0.072)	(0.072)
Assigned Number		-0.032	-0.019	-0.020
		(0.122)	(0.124)	(0.124)
Δ Human Presence$_{\text{Video - Text}}$		-0.505**	-0.377	-0.326
		(0.243)	(0.308)	(0.303)
Dishonest $\times$ Δ Human Presence$_{\text{Video - Text}}$			-0.388	-0.391
			(0.290)	(0.290)
Age				0.190
				(0.174)
Business and Economics				-0.285
				(0.829)
Constant	-0.340	0.402	0.126	-4.351
	(0.467)	(0.726)	(0.772)	(4.355)
Observations	314	314	314	314
Log pseudolikelihood	-136.348	-134.276	-132.997	-131.960
Δ Human Presence$_{\text{Video - Text}}$ + Dishonest $\times$ Δ Human Presence$_{\text{Video - Text}}$ = 0			0.020	0.024

Robust standard errors adjusted for 64 clusters (subjects) in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: The figures in the row "Δ *Human Presence*$_{\textit{Video - Text}}$ + *Dishonest* $\times$ Δ *Human Presence*$_{\textit{Video - Text}}$ = 0" are the p-values for the overall effect of Δ *Human Presence*$_{\textit{Video - Text}}$ on *Video* when the senders were dishonest. Since Δ *Human Presence*$_{\textit{Video - Text}}$ is quasi-continuous, we used the sample mean of Δ *Human Presence*$_{\textit{Video - Text}}$ in the *Choice* treatment (1.193). We excluded six observations due to downward lying ($r < s$). For the complete table with all control variables, see Table A.6 in Appendix A.2.

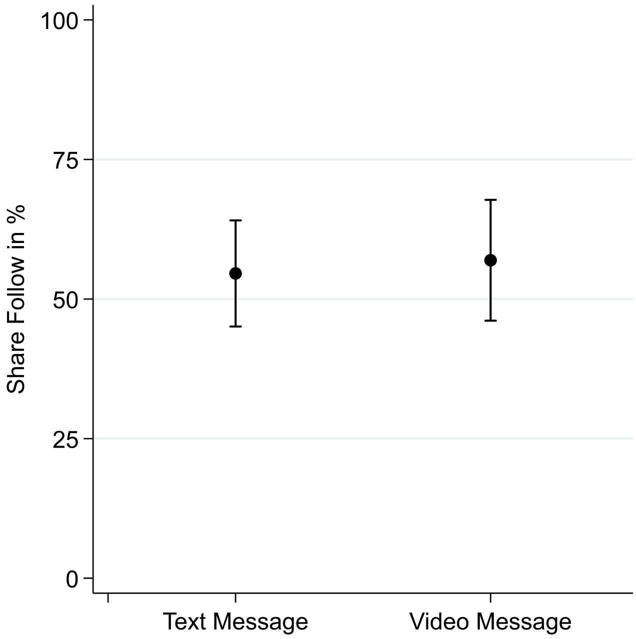
Table 3.2: Panel probit regression (random effects) with *Video* as dependent variable and *Dishonest* as independent variable

dishonest. A two-sided F-test reveals that the overall effect of Δ *Human Presence*$_{\textit{Video - Text}}$ on *Video* is negative (-0.764) and significant ($p = 0.020$) when the senders were dishonest. Model (4) shows that the results are robust to demographic controls.

Result 2 There is a positive and significant correlation between the senders' decisions to choose a text message and to send a dishonest message.

3.4.2 Receiver Behavior

For the receiver behavior, our focus lies on whether the decision to follow or not differed depending on the communication channel in the *Choice* treatment. The receiver's average share of follow decisions was 54.58% when receiving text messages and 56.94% when receiving video messages (see Figure 3.3). A subject-level comparison using a two-sided Wilcoxon signed-rank test shows no statistically significant difference ($p = 0.948$).



Note: Dots indicate averages and whiskers indicate 95% confidence intervals.

Figure 3.3: Share of follow decisions by communication channel in the *Choice* treatment

We report a series of panel probit regressions in Table 3.3. The coefficients for *Video Message* are positive but not significant in all three specifications. Thus, despite differences in sender behavior, there seems to be no signaling value regarding the chosen channel in the *Choice* treatment. As expected from previous studies, a higher reported number decreased the likelihood that the receivers follow the message.

Result 3 We find no significant differences in receivers' follow behavior depending on whether they received text or video messages.

3.5 Discussion

The results of our study show that channel decisions did not differ depending on our treatment manipulations. However, we observe a correlation between the chosen channel and the truthfulness of the message if the senders had the opportunity to lie. Below, we discuss potential mechanisms that may explain our findings. Even though we do not observe treatment differences in general, behavioral differences might be masked by the fact that the senders could be dishonest in the *Choice* treatment. In the following, we first focus on the results for

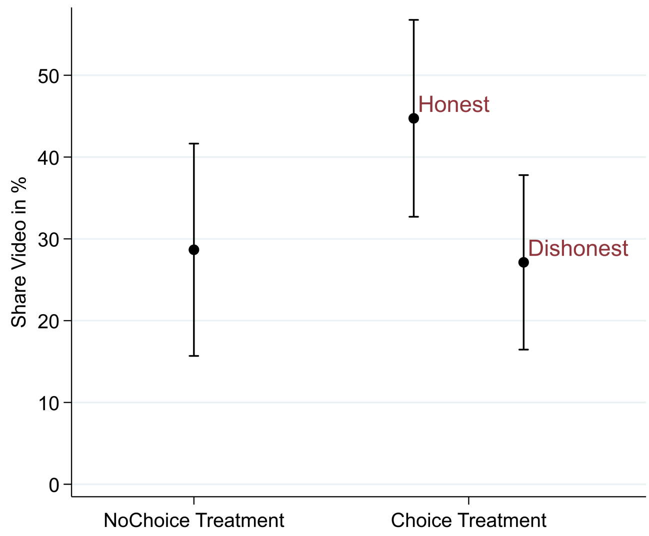
Dep. Var.: Follow	(1)	(2)	(3)
Video Message	0.041	0.018	0.015
	(0.175)	(0.180)	(0.179)
Round		0.068	0.068
		(0.052)	(0.052)
Reported Number		-0.201**	-0.203**
		(0.095)	(0.095)
Age			-0.018
			(0.020)
University Degree			-0.143
			(0.338)
Constant	0.164	0.894	1.508*
	(0.184)	(0.545)	(0.897)
Observations	300	300	300
Log pseudolikelihood	-182.276	-177.750	-177.284

Robust standard errors adjusted for 60 clusters (subjects) in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: For the complete table with all control variables, see Table A.7 in Appendix A.2.

Table 3.3: Panel probit regression (random effects) with *Follow* as dependent variable and *Video Message* as independent variable

senders in both treatments. We discuss potential reasons for not observing any significant treatment differences in channel choices and explain some indications that point to a general video aversion. Second, we focus on senders within the *Choice* treatment. We discuss Δ *Human Presence*$_{\textit{Video - Text}}$ as a possible mechanism for our second hypothesis and outline alternative explanations. Third, we briefly discuss the results for receivers. Last, we explain limitations and highlight avenues for future research.

Sender behavior in both treatments On average, the senders' share of video messages in the *Choice* treatment was 44.74% when being honest, 27.13% when being dishonest, and 28.67% in the *NoChoice* treatment (see Figure 3.4). Recall that information asymmetries existed only in the *Choice* treatment, and the receivers had no information about whether the senders' messages were honest. In contrast, the senders' messages in the *NoChoice* treatment were only an information transmission. Table 3.2 reveals that sending a dishonest message significantly decreased the senders' likelihood to send a video message. In other words, being honest increased the senders' likelihood to send a video message. Qualitative responses further indicate why some senders preferred the video message when sending an honest message. After Round 6, we asked all senders why they chose the text or video messages in Round 6. Of all senders



Note: Dots indicate averages and whiskers indicate 95% confidence intervals.

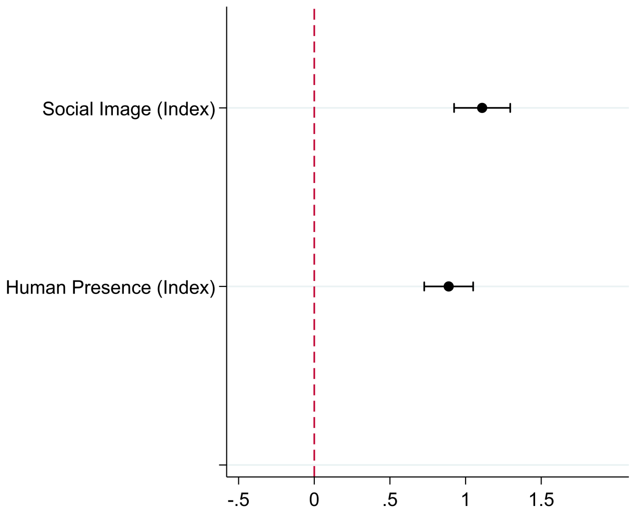
Figure 3.4: *Share Video* by treatment and communication content

in the *Choice* treatment who sent a video message and were honest, 64.29% stated that they chose this channel because it is more credible, personal, honest, or trustworthy (see Table A.8 in Appendix A.2). This indicates that these senders chose video messages to signal to the receivers that the messages were honest. According to Spence (1973), signals need to be costly to effectively convey valuable information. Video messages are, among other things, less anonymous and allow a higher human presence (we will examine this further in the next paragraph). Thus, we suggest that sending a video message instead of a text message might lead to costs and, therefore, also be a valuable signal. There was no such signal value regarding the channel choice in the *NoChoice* treatment because the receivers knew that the senders had to be honest. For further analyses, we compared the senders' average share of video messages when being honest in the *Choice* treatment (44.74%) and the senders' average share of videos (all messages were honest) in the *NoChoice* treatment (28.67%). Although this difference is not significant (two-sided Mann-Whitney U test, $p = 0.139$), there is a tendency for the senders who sent an honest message to have a higher preference for video messages in the *Choice* treatment. Additionally, the senders in the *Choice* treatment tended to prefer text messages when being dishonest (see Table 3.2). This might be due to lower lying costs when the senders chose a text instead of a video message. Compared to the *NoChoice* treatment (28.67%), the share of video messages when the senders were dishonest in the *Choice* treatment (27.13%) was only slightly smaller. The non-significant difference might be due to limited statistical power or other possible alternative explanations like spiteful preferences that could have increased the likelihood of video messages when being dishonest in the *Choice* treatment. The two effects mentioned above, i) more video when being honest as a signal, and ii) more text when being dishonest, have had an impact in different directions (see Figure 3.4), and, therefore, might explain why we do not observe significant treatment differences regarding the channel choice.

Recall that the senders had a higher preference for text messages, the higher they perceived the difference in human presence between video and text messages, irrespective of the treatment. Model (2) in Table 3.1 reveals that Δ *Human Presence*$_{\textit{Video - Text}}$ has a statistically significant effect on *Video*. In addition, Model (3) shows that the overall effects (main and interaction) of Δ *Human Presence*$_{\textit{Video - Text}}$ on *Video* are negative and significant in the *NoChoice* treatment (p = 0.010) as well as in the *Choice* treatment ($p = 0.038$) (see Section 3.4). Given that the interaction effect *Choice Treatment* $\times$ Δ *Human Presence*$_{\textit{Video - Text}}$ is not significant, the treatment manipulation did not interact with the difference in perceived human presence. We interpret this finding as an indicator of a general video aversion. Some subjects preferred text over video channels because they did not want to show themselves in a video. This impacted the channel choice for the senders in both treatments.

Sender behavior in the *Choice* treatment Regarding our second hypothesis, we explore whether the perceived difference in human presence impacted the channel choice differently depending on the truthfulness of the message in the *Choice* treatment. Cohn et al. (2022) state that human presence can reduce dishonest behavior. Our results suggest that the impact of human presence on dishonest behavior is also important in the context of using different communication channels. In general, dishonest behavior can be explained by a variety of intrinsic lying costs and reputational costs associated with inference about peoples' honesty (see Abeler et al., 2019, for a meta-study). Thus, the channel choice might offer senders the opportunity to reduce costs associated with their lie. A lower perceived human presence in text compared to video messages might account for reduced costs when lying. The results of our survey show that the perceived human presence was about 0.9 standard deviations lower for text compared to video messages (see Figure 3.5), and this difference is statistically significant (two-sided Wilcoxon signed-rank test, $p < 0.01$).[10] In Table 3.2, the interaction between *Dishonest* and

[10]For each communication channel, we used three items in which the senders self-stated their perceived human presence and three items in which the senders self-stated their social image concerns on a 7-point Likert scale (see Table A.1 in Appendix A.2). All responses were standardized using the mean and standard deviation for



Note: Dots indicate averages and whiskers indicate 95% confidence intervals.

Figure 3.5: Difference in perceived human presence and social image concerns between video and text messages (standardized)

Δ *Human Presence*$_{\textit{Video - Text}}$ has a negative coefficient but is not statistically significant (e.g., $p = 0.177$ in Model (4)). If we look at the overall (main and interaction) effect of Δ *Human Presence*$_{\textit{Video - Text}}$ on *Video*, it is only statistically significant if the senders lied but not if the senders were honest. For a further analysis, we excluded observations in which the senders had no incentive to send a dishonest message because the assigned number s was six (see Table 3.4). Now, the interaction effects *Dishonest* $\times$ Δ *Human Presence*$_{\textit{Video - Text}}$ in Models (3) and (4) are significant (Model (3) $p = 0.044$ and Model (4) $p = 0.040$). Thus, we suggest that the choice to send a text instead of a video message facilitated the senders to behave more dishonestly because the lower perceived human presence in text messages reduced lying costs.

Besides our potential mechanism, one might speculate that there are alternative reasons why people might prefer to send text or video messages in our setup. With our design and experimental procedures, we mitigated some of these. We ensured that the channel choice was not affected by the fact that sending text messages can be faster in terms of time compared to recording videos. For both channels, the senders could only proceed with the experiment after at least 40 seconds had expired. Sending text messages might also be easier regarding the cognitive load than recording video messages. However, our data indicate that cognitive load did not depend on whether the senders sent an honest or a dishonest message because being

video message responses. We then created a human presence index and a social image concerns index using the unweighted average of the standardized responses. At last, we subtracted the index for text from the index for video to measure the difference in human presence and social image concerns.

Dep. Var.: Video	(1)	(2)	(3)	(4)
Dishonest	-1.055**	-1.089**	-0.504	-0.543
	(0.425)	(0.539)	(0.578)	(0.586)
Round		0.017	0.058	0.060
		(0.076)	(0.077)	(0.077)
Assigned Number		-0.019	-0.006	-0.009
		(0.125)	(0.131)	(0.131)
Δ Human Presence$_{\text{Video - Text}}$		-0.388*	-0.071	-0.039
		(0.221)	(0.320)	(0.321)
Dishonest $\times$ Δ Human Presence$_{\text{Video - Text}}$			-0.632**	-0.640**
			(0.313)	(0.312)
Age				0.162
				(0.168)
Business and Economics				-0.084
				(0.769)
Constant	-0.396	0.065	-0.533	-4.388
	(0.420)	(0.687)	(0.772)	(4.238)
Observations	266	266	266	266
Log pseudolikelihood	-120.448	-119.004	-116.225	-115.470

Robust standard errors adjusted for 64 clusters (subjects) in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: We excluded two observations due to downward lying ($r < s$). For the complete table with all control variables, see Table A.9 in Appendix A.2.

Table 3.4: Panel probit regression (random effects) with *Video* as dependent variable, *Dishonest* as independent variable and only observations without assigned number $s = 6$

dishonest did not significantly increase the time the senders took to complete the video message recording page (*Completion time video*) (see Table A.10 in Appendix A.2). Furthermore, our payment structure, in which the senders' payoff is independent of the receivers' action, aimed to reduce some strategic concerns when choosing a channel. Compared to other payment structures, such as the one used in Gneezy (2005), the senders had no own monetary incentives to convince the receivers of their lies.

Receiver behavior To sum up, from our sender decisions in Part 1, we found that channel preferences did not differ depending on our treatment manipulation but on whether the senders were honest. Some senders tried to use the channel choice as a signal, but when looking at Part 2, this signal was not relevant to the receivers. Our results in Table 3.3 reveal that follow decisions did not significantly vary between text and video messages in the *Choice* treatment. The coefficient for the reported number in Table 3.3 is negative and marginally statistically significant. Thus, the content of the messages was of some relevance to the receivers, but not the chosen channel. This might be due to the fact that the receivers may not have perceived the senders' choice to send a video message as costly enough in order for it to serve as a reliable signal.

Limitations and future research opportunities In the following, we discuss the limitations of our study and highlight opportunities for future research. First, our study serves as a starting point to investigate preferences for different communication channels, focusing on dishonest behavior. We provided evidence that this research field is highly relevant because senders' channel preferences can vary, and we shed light on potential mechanisms. Our goal was to use common communication channels. Thus, the senders had the choice between text and video messages. However, these channels differ in several aspects (e.g., media richness, anonymity, or perceived human presence), and we leave it to future research projects to disentangle these aspects. A more nuanced variation of treatments, including text messages with images of subjects' faces, could be one potential avenue to disentangle anonymity. Furthermore, some senders stated that they chose a text message when lying to the receivers because video messages contain nonverbal cues that might be useful for the receivers to detect a lie. Comparing audio and video messages would be an important extension of our experiment. It would provide a more precise investigation into the impact of nonverbal cues, such as facial expressions or gestures, on economic decision-making. Similarly, comparing text and audio messages would make it possible to identify the influence of paraverbal cues.

A second limitation refers to our potential mechanism that the difference in perceived human presence between text and video messages impacted the channel decision when the senders decided to lie in the *Choice* treatment. With our findings, we cannot clearly distinguish how this mechanism affected the costs of lying and, thus, the senders' preference for text messages. Cohn et al. (2022) suggest that higher social image concerns could be a reason. Similar to perceived human presence, senders' social image concerns were about 1.1 standard deviations higher for video compared to text messages (see Figure 3.5), and this difference is statistically significant (two-sided Wilcoxon signed-rank, $p < 0.01$). In addition, a statistically significant correlation of 0.532 shows that the difference in social image concerns can be partly attributed to differences in perceived human presence. Recent studies indicate that social image concerns increase honesty (Abeler et al., 2019; Dufwenberg & Dufwenberg, 2018; Gneezy et al., 2018; Khalmetski & Sliwka, 2019). Further analyses of our results in Table 3.5 show that the coefficients related to Δ *Social Image*$_{\textit{Video - Text}}$ are negative and, thus, increased the preference for text messages. However, at least in our setup, these concerns had no significant effect on the channel choice, possibly due to limited statistical power. Recall that we used different subject pools to hire the senders and the receivers. This high anonymity between the senders and the receivers could be a reason why social image concerns did not play a major role in our setup.

Dep. Var.: Video	(1)	(2)	(3)	(4)
Dishonest	-1.329***	-1.410**	-1.141	-1.144
	(0.401)	(0.584)	(0.716)	(0.717)
Round		-0.001	0.002	0.005
		(0.071)	(0.069)	(0.069)
Assigned Number		-0.033	-0.034	-0.033
		(0.123)	(0.123)	(0.123)
Δ Social Image$_{\text{Video - Text}}$		-0.207	-0.142	-0.107
		(0.232)	(0.263)	(0.255)
Dishonest $\times$ Δ Social Image$_{\text{Video - Text}}$			-0.139	-0.146
			(0.231)	(0.233)
Age				0.197
				(0.168)
Business and Economics				-0.398
				(0.808)
Constant	-0.340	0.200	0.066	-4.534
	(0.467)	(0.838)	(0.859)	(4.242)
Observations	314	314	314	314
Log pseudolikelihood	-136.348	-135.821	-135.544	-134.277

Robust standard errors adjusted for 64 clusters (subjects) in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: We excluded six observations due to downward lying ($r < s$). For the complete table with all control variables, see Table A.11 in Appendix A.2.

Table 3.5: Panel probit regression (random effects) with *Video* as dependent variable and *Dishonest* and Δ *Social Image*$_{\textit{Video - Text}}$ as independent variables

Finally, we decided to hire only male subjects primarily for two reasons. First, we thereby excluded gender effects as a potential driving factor for the channel choices, which could have a stronger effect in our sender-receiver task than in other tasks without interaction between subjects, such as those used in (Cohn et al., 2022; Conrads & Lotz, 2015). Due to the reason that we included video messages, the gender of the senders was recognizable to the receivers. Studies revealed that there can be gender differences in trust and trustworthiness (see, e.g., Croson & Gneezy, 2009; Van Den Akker et al., 2020). Therefore, gender can serve as a signal about the trustworthiness of a person, and a variation in gender could have interfered with the effect of the chosen communication channel. In addition, studies showed that there are gender differences in lying (see Capraro, 2018; Gerlach et al., 2019, for meta-analyses), and thus, these gender effects could have affected our results. Second, our study serves as a starting point. Hiring either male or female subjects ensured a more homogeneous sample and enabled us to focus more on investigating subjects' channel preferences. We decided on male subjects because dishonest behavior is more prevalent among men than among women, in particular in sender-receiver games (see Capraro, 2018; Gerlach et al., 2019, for meta-analyses). To conclude, we point out that the results of our study are not necessarily relevant for non-male subjects. However, our results show that including female and non-binary subjects would be a valuable extension of our experiment.

3.6 Conclusion

Our study presents a controlled experiment using a sender-receiver game to study whether the choice of the communication channel varies with the truthfulness of the message content. We concentrated on comparing text and video channels because these are two common ways of communication in companies and organizations, as well as in day-to-day conversations in private settings. We conducted two controlled treatments to study whether the opportunity to lie impacts the chosen communication channel. Overall, we observed no significant impact of the opportunity to lie on the channel choice. However, our data revealed that the video channel was chosen more often when sending a truthful message, and the text channel was preferred if the message contained a lie. This suggests that the senders used the channel choice as a signal if they had the opportunity to lie. Our results indicate that the choice of communication channels can convey information about intentions and that text communication channels can facilitate dishonest behavior. Thus, carefully selecting available communication channels might be an option to prevent dishonest communication. Interestingly, the receivers did not adapt their behavior to the chosen channel. While our study used a simple sender-receiver game, the results provide a first step towards a better understanding of the interplay between communication channels and truthful communication. Further research is needed to disentangle potential mechanisms and shed more light on the causal relationship between (dis)honest reporting and channel choices in different situations.

4 Study III: Beyond Words: Nonverbal Cues in Virtual Collaborations*

Abstract: Virtual communication has become commonplace, especially for geographically dispersed teams collaborating on joint projects despite lacking prior relationships. These teams will likely encounter coordination failures, resulting in low overall cooperation rates. However, nonverbal cues transmitted during communication might help to mitigate this problem. Our study investigates whether and how nonverbal cues in virtual team meetings can foster collaboration. We implemented a weakest-link game in a controlled, large-scale online experiment on Prolific. Before playing the weakest-link game, team members participated in a non-game-related virtual team meeting to get acquainted. We systematically varied the availability and amount of nonverbal cues between our treatments during this meeting phase. We expected these cues to be crucial in overcoming strategic uncertainty and fostering group cohesion. Depending on the treatment, the virtual team meeting was an audio-only meeting, an audio meeting with selfies, or a video meeting. We observe significantly higher cooperation in the audio-only and the video meeting than in the audio meeting enriched with selfies. Static nonverbal cues, thus, had a negative effect on team cooperation. We find that the audio-only and the video meeting led to similar team cooperation and that coordination did not differ between treatments. Our findings shed light on how virtual teams can enhance their collaboration by leveraging the power of nonverbal cues. By understanding the impact of nonverbal cues, organizations can foster stronger connections and achieve more successful outcomes.

Keywords: communication channels; weakest-link; cooperation; virtual collaboration

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4.1 Introduction

"Please turn on your camera during the meeting" is a common phrase at the start of virtual meetings to encourage engagement and connection via video. 92% of U.S. executives even go as far as stating that employees who frequently turn off their cameras (or mute themselves) will not have a long-term future with the company (WakefieldResearch, 2022), but still, many virtual meetings keep relying on audio and therefore verbal and paraverbal cues only. Since the Covid-19 pandemic, virtual meetings have become common for many at work or in their daily lives and are expected to persist in the future (Barrero et al., 2023; Tolliver & Sass, 2024). While virtual meetings facilitate collaboration among geographically dispersed teams, they are also increasingly used in hybrid work arrangements, even when employees are co-located on office days (Bloom et al., 2022). Together with the ongoing globalization, this leads to new teams meeting virtually for the first time. Therefore, it is important to understand how enriching virtual pre-play communication with nonverbal cues affects team collaboration.

Encouraging new teams to turn on their cameras can have several underlying reasons. On the one hand, it enables teams to gain more information about others through the added nonverbal cues as explained by the media richness theory (Daft & Lengel, 1986). The availability of nonverbal cues simultaneously makes the communication more similar to well-known face-to-face meetings. It provides all meeting participants with more information to judge their team members better, e.g., to gain an understanding of their intentions and their type (Eckel & Petrie, 2011; He et al., 2017). Furthermore, pre-play video communication has been shown to significantly increase cooperation compared to audio communication and to levels similar to those in face-to-face communication (Brosig et al., 2003), proposing video communication as a good substitute for in-person communication.

However, an increase in nonverbal cues through a video can also have negative impacts on virtual meetings, such as nonverbal overload, leading to zoom fatigue or less social connectedness of meeting participants (Fauville et al., 2023; Queiroz et al., 2023). Thus, another real-world example of how to enrich virtual meetings with nonverbal cues is static photos similar to profile pictures in MS Teams or Zoom. This could lead to a reduction in overload while simultaneously allowing for the visual identification of others. Being able to identify others (e.g., by name or photo) reduces anonymity, is useful for promoting prosocial behavior (Andreoni & Petrie, 2004; Charness & Gneezy, 2008), and has an effect on trust and efficiency

(Bohnet & Frey, 1999b; Eckel & Petrie, 2011). To our knowledge, it has not been investigated whether adding static photos to audio communication leads to similar results on team cooperation and coordination compared to audio-only or video communication. In addition, the effect of communication on coordination and cooperation has previously usually been studied with game-related communication including promises and commitment (see e.g., Avoyan and Ramos (2023), Brosig et al. (2003), and He et al. (2017)), while we add to it by having a communication stage that is not game-related to focus on team building and the effect of nonverbal cues. This study intends to fill these gaps by addressing the question: Does enriching virtual pre-play communication with nonverbal cues increase team coordination and cooperation?

We conducted a controlled online experiment in two parts. While the treatment-independent Part 1 was mainly used to recruit subjects for Part 2 and to collect selfies of subjects, the main experimental manipulation took place in Part 2. Part 2 was completed in fixed teams of three subjects and consisted of two main stages: a communication stage and a team decision stage. In the communication stage, subjects took part in a virtual meeting, the format of which varied depending on the treatment. In the baseline (*Audio* treatment) with no nonverbal cues, subjects could only hear each other, in the *Photo* treatment they could additionally see selfies of all team members', and in the *Video* treatment they saw the live videos of each other. The main change between the *Audio* and *Photo* treatment was thereby the possibility of visual identification through static nonverbal cues (selfies). From the *Photo* to the *Video* treatment, turned-on cameras further increased the amount of available nonverbal cues by adding dynamic components, such as facial expressions and gestures. Notably, the communication stage was the only treatment-dependent stage and not game-related, as subjects only received the instructions for the team decision task afterwards. In the team decision stage, subjects played the weakest-link game (WLG) (Brandts & Cooper, 2006a; Van Huyck et al., 1990) with limited feedback for ten rounds. Note that only decisions in Round 1 were relevant to our hypotheses, as they capture immediate treatment differences.

The data show that enriching virtual pre-play communication to transmit more nonverbal cues had no significant effect on coordination in the first round of the WLG. For cooperation, however, we identified significant treatment differences: (i) adding static nonverbal cues with a selfie marginally significantly decreased team cooperation compared to the baseline (*Audio*), and (ii) compared to a static selfie, live interaction via video significantly increased cooperation. Nevertheless, there was no difference in cooperation between the *Audio* and *Video* treatment.

Our paper contributes to three areas of research: (i) Research on team coordination and cooperation, in particular in virtual settings, (ii) literature on different communication channels, and (iii) literature relating to identification and anonymity.

The positive effect of communication on economic behavior and decision-making has already been demonstrated in general (Charness & Dufwenberg, 2006; Cooper et al., 1992; Crawford, 1998; He et al., 2017), as well as its role in enhancing coordination and cooperation, particularly in the WLG (Avoyan & Ramos, 2023; Chen & Chen, 2011; Dong et al., 2018; Toku et al., 2022). Our study contributes to that literature by providing an in-depth analysis of the role of nonverbal communication. A main difference between our study and the existing literature is that communication was not game-related and, therefore, did not allow for promises and commitments, but instead focused on the nonverbal parts of communication, which had not yet been extensively investigated in this context. The results do not show any positive effects on cooperation or coordination when adding static or dynamic nonverbal cues to an audio-only meeting. In contrast, static nonverbal cues in the form of selfies, which provided visual identification, decreased team cooperation compared to no nonverbal cues and dynamic nonverbal cues in the form of a live video. Similarly, social identity (Chen & Chen, 2011) and group cohesion (Gächter et al., 2025) had both been studied in relation to coordination and cooperation. Our results extend previous research that found beliefs and group cohesion crucial for promoting cooperation (Gächter et al., 2025). While Gächter et al. (2025) manipulated group cohesion exogenously by putting either friends or strangers in groups, we show that varying the availability of nonverbal cues in virtual communication can also significantly influence the emergence of cohesion within groups.

Furthermore, our findings contribute to the literature studying virtual communication with a focus on how different communication channels affect economic behavior such as charisma and performance (Nieken, 2023), creativity (Grözinger et al., 2020), trust and trustworthiness (Babutsidze et al., 2021; Zylbersztejn et al., 2020, 2021), affiliation with communication partners (Sprecher, 2014), or charitable giving (Zylbersztejn et al., 2024b). More related to our paper, Brosig et al. (2003), Bochet et al. (2006), and Frohlich and Oppenheimer (1998) studied the effect of different communication channels on cooperation. While the study by Brosig et al. (2003) with game-related communication revealed a significantly higher rate of cooperation when using video compared to audio communication, we found no evidence that this is also the case for game-unrelated communication. Furthermore, our findings contribute to the understanding of how different degrees of human presence emerge in virtual communication, which is particularly relevant for reducing dishonest behavior (Cohn et al., 2022; Nieken & Walther, 2024).

Our *Photo* treatment allowed us to isolate the effect of visual identification and, thus, our paper links to an extensive body of research related to identification and anonymity. While other studies found positive effects of identification (e.g., Andreoni and Petrie (2004), Charness and Gneezy (2008), and Eckel and Petrie (2011)), visual identification did not appear to have the same effect in our study and hampered cooperation. We used the variable *Homophily* to measure how similar teams were in terms of age, gender, and ethnicity. Interestingly, *Homphily* only had an indirect significant effect on cooperation in the *Photo* treatment, which allowed visual identification. This suggests that in very diverse teams, including selfies of people in virtual meetings, can have adverse effects on cooperation. Moreover, our results reveal that enriching virtual communication with more nonverbal cues by adding selfies of the subjects or turning on video during the communication significantly reduced the perceived sense of anonymity. However, anonymity was not relevant for team behavior regarding coordination and collaboration in any treatment. Instead, perceived human presence, which was significantly lower at the 10% level with static nonverbal cues, indirectly affected team cooperation.

The remainder of the paper is organized as follows. Section 4.2 presents our study design, as well as our treatments and hypothesis. In Section 4.3, we explain our main variables, our sample, and our empirical strategy. We describe and discuss our results in the following Sections 4.4 and 4.5 and then conclude with Section 4.6.

4.2 Experimental Design and Hypotheses

In this section, we present our general study design, procedure, treatments, and hypotheses.

4.2.1 General Description

We conducted a controlled online experiment on Prolific, consisting of two parts, which were conducted with several days difference.[1] Part 1 was elicited with SoSci Survey (Leiner, 2024),

[1]The study has been pre-registered at OSF and approved by the ethics committee of the Karlsruhe Institute of Technology.

while in Part 2 we used the experimental software oTree (Chen et al., 2016). Subjects were recruited in the first part of the study, where they completed a brief survey and took a selfie as a necessary preparation for the second part. Part 2 then contained the main experimental manipulation, with three subjects randomly forming a team. Each team went through two main stages. First, a communication stage, in which they met their team members in a virtual meeting,[2] and thereafter, a team decision stage. The teams played a WLG for ten rounds to measure team coordination and cooperation. Lastly, subjects individually answered a brief survey.

4.2.2 Study Part 1

In Part 1, subjects were recruited via Prolific, and all subjects were located in the United States.[3] This part of the study was identical for all treatments and served two primary purposes: (i) to collect subjects' selfies and audio recordings, and (ii) to select study time slots for participation in Part 2. First, the subjects received general instructions and made some technical checks to test their audio and video. Then, subjects took a selfie and an audio recording of their voice with a pre-specified sentence.[4] These were necessary for the *Photo* treatment as well as for identification in Part 2 of the study. Since some Part 2 questions had to be answered for each team member individually, we provided subjects with the audio recordings and, in the *Photo* and *Video* treatment, with all team members' selfies to facilitate identification. Next, subjects got a list of possible time slots to participate in Part 2 and answered a brief survey.[5] The average duration of Part 1 was about 11.2 minutes, and subjects received a fixed payment of £2.00 independent of whether they participated in Part 2. This resulted in subjects' average

[2]Virtual meetings were facilitated using the open-source platform Jitsi (https://jitsi.github.io/handbook/) integrated into oTree via its iFrame API. We used Jitsi for two reasons. First, we were able to host it locally on servers hosted by our university to comply with data protection rules. Second, the setting allowed us to customize the meeting software to our requirements using the iFrame API (e.g., turning off the camera function or integrating subjects' selfies).

[3]In addition, we used Prolific prescreeners for age (18 to 65), fluent languages (English), approval rate (min. 95%), number of previous submissions (min. 100), and record video (Yes). Due to the age restriction and the language screener, we had a sample of working-age subjects who had no language barrier during the virtual meeting. In addition, they must have previously agreed to record a video of themselves to reduce return rates because they had to upload a selfie, and we recorded the audio during the communication stage. As the resulting Prolific sub-sample was female-dominated (as of September 2024), we used a quota sample on gender to ensure similar numbers of male and female subjects in our study.

[4]"This is a test for the virtual team communication study."

[5]This survey captured demographic data, the Inclusion of Other in the Self Scale (IOS) (Aron et al., 1992), the We Scale (Cialdini et al., 1997), risk and social preferences (reciprocity, altruism) (Falk et al., 2023), interpersonal trust (Nießen et al., 2020), and collective orientation (Driskell et al., 2010; Hagemann, 2020).

hourly earnings of £10.81 in Part 1.

Study Part 1 and Part 2 were always held on different days. The advantage of splitting our study into two parts was that we could elicit specific measures before the treatment manipulation to eliminate some potential endogeneity problems. In particular, the time gap between Part 1 and Part 2 should mitigate potential influences of these measures on the WLG outcomes. In addition, we needed to review subjects' selfies and audio recordings before Part 2 to exclude subjects that did not complete Part 1 successfully (e.g., if subjects' faces were not displayed in the selfies or subjects did not record the pre-specified sentence). Furthermore, the backgrounds of the uploaded selfies were edited before Part 2. We grayed out the photo backgrounds to exclude any influences from the subject's surroundings (see Figure 4.1).

4.2.3 Study Part 2

The second part of the study consisted of two main stages: (i) a communication stage, and (ii) a team decision stage. First, we describe the treatment manipulation, the WLG and the team decision stage, before providing a detailed description of the Part 2 procedure.

Treatments Our experiment encompassed three treatments (*Audio*, *Photo*, and *Video*). The treatments only differed in the communication stage, i.e., the virtual meeting.[6] In all treatments, subjects were able to communicate via audio. The treatments differed in the amount and type of nonverbal cues that were available in the virtual meeting. The *Audio* treatment served as our baseline compared to the *Photo* and *Video* treatments. Subjects could only hear each other as cameras were exogenously turned off. Therefore, only verbal and paraverbal information were possible, but no nonverbal cues.

In the *Photo* treatment, cameras were also exogenously turned off.[7] Compared to the *Audio* treatment, in which subjects could only see images of subjects' generic IDs ("team member E", "team member O", "team member I") (see Figure 4.1), in the *Photo* treatment we added static nonverbal cues. This was done by replacing the images of generic IDs with subjects'

[6]The identification of individual team members in the questionnaires posed an exception. In the *Photo* and *Video* treatment, the selfie and audio recording from Part 1 were shown with the subject's generic ID, while only the audio recording was provided in the *Audio* treatment. A second exception was the attractiveness rating, in which, depending on the treatment, only the attractiveness of the voice (*Audio*) or also the physical attractiveness of the other team members (*Photo* and *Video*) was captured.

[7]In the *Audio* and *Photo* treatment, subjects were not able to turn on the camera function in Jitsi.

selfies that they took in Part 1 of the study. Therefore, the *Photo* treatment eliminated visual anonymity by including static nonverbal facial cues.

We included dynamic nonverbal cues in the *Video* treatment. Subjects' cameras were turned on, and thus, they could see themselves and their team members. This live video interaction further increased the amount of nonverbal cues, e.g., mimics, gestures, and posture.

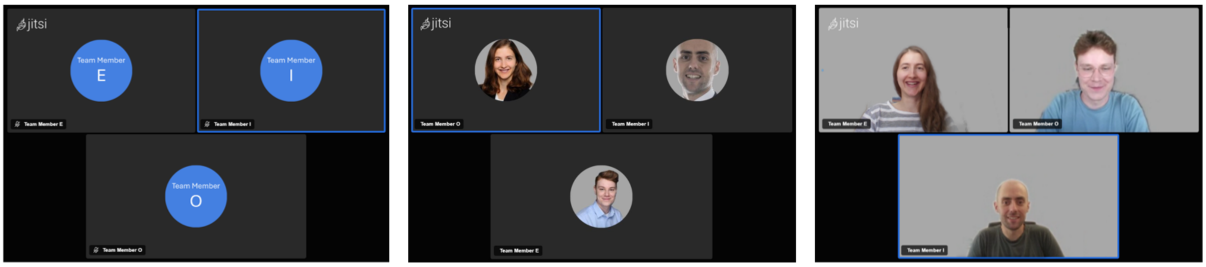


Figure 4.1: Virtual meeting setup (from left to right): *Audio*, *Photo*, *Video*

Weakest-link Game In the team decision stage, we used the WLG, a standard economic game that allowed us to measure coordination and cooperation (see Section 4.3.1 for details). We chose the naturalistic WLG framing by Brandts and Cooper (2006a) in which team members acted as employees and were asked to allocate work hours (the term used for effort levels) between an individual project P_B and a team project P_A. In each round, subjects had to choose one of the following effort levels h_A to allocate to the team project: $h_A = \{0, 10, 20, 30, 40\}$. The payoff in the WLG depended on the own effort h_A as well as the minimum effort in the team h_{min}. The own effort was costly with $c = 5$ leading to costs of $5 \times h_A$ and the minimum effort was rewarded with a bonus. We set $B = 10$, which is one of the bonus rates used by Brandts and Cooper (2006a). The payoff function in our setting, therefore, was as follows:

$$\pi = 200 - 5 \times h_A + 10 \times h_{min}$$

Each time subjects made a decision in the WLG, they saw the resulting payoff table (see Figure 4.2), which showed their possible payoffs for all combinations of their own possible effort and the possible minimum effort of all other team members.

Following Dong et al. (2018), coordination on the highest effort level is expected if $n < B/c$ and coordination on the lowest effort level for $n > B/c$. Therefore, for the parameters $n = 3$ (team size), $c = 5$, and $B = 10$, coordination on the lowest effort level was expected. The resulting WLG had five pareto-ranked pure Nash equilibria, which were the strategy profiles in

Payoff table		Minimum number of hours contributed to Project A by any team member other than yourself				
		0	**10**	**20**	**30**	**40**
Your number of hours contributed to Project A (h_A)	**0**	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

Figure 4.2: Payoff Table for the Weakest-Link Game

which all team members chose the same effort level. The risk-dominant equilibrium in which all team members chose an effort of zero was at the same time ranked lowest in terms of pareto efficiency. Contrarily, all team members choosing the highest effort level led to the pareto- and payoff-dominant equilibrium (Riedl et al., 2016; Van Huyck et al., 1990).

Procedure Part 2 started with information on the different study stages and the payoff. Next, subjects had to complete some technical checks and affirm that the selfies and audio recording they saw were the ones they uploaded in Part 1.

As stated in Section 4.2.1, in the communication stage, teams of three subjects had the opportunity to communicate in a virtual meeting. Subjects were grouped into these teams based on their arrival time and then received instructions for the virtual meeting. Immediately before the virtual meeting, subjects received instructions for a brief icebreaker task following Kaiser et al. (2022) and Koudenburg et al. (2013). They individually rated different types of vacations[8] on a 7-point Likert scale (see Figure B.1 in Appendix B.1). Next, we asked them to discuss these vacation types during the virtual meeting, a method which has been established as an icebreaker to promote conversational flow (Kaiser et al., 2022; Koudenburg et al., 2013). In addition, it allowed us to control for similarities within teams. The virtual meeting started as soon as all team members completed this task. Beyond that, subjects received no further instructions regarding the communication content and were free to communicate as they wanted. However, it is important to note that subjects did not receive any information about the task in the subsequent stage, i.e., the WLG. Subjects were only informed that they would, after the meeting, be grouped again with the same team. Thus, the communication content was not game-related. Communication was used in our study to get to know other team members and

[8]sun, sea and beach vacation; party vacation; winter sports vacation; city vacation; educational vacation; camping vacation; cruise vacation

not for promises, or to discuss other content related to the game. The virtual meeting was the only treatment-dependent stage of the experiment. It entailed seven minutes of unrestricted communication and ended automatically. Since we grayed out the photo backgrounds for the *Photo* treatment to exclude any influences from the subject's surroundings, we asked subjects to use a gray background in the *Video* treatment. After a successful virtual meeting (see Section 4.3.2 for our exclusion criteria), the teams proceeded with the team decision stage.

At this point, subjects received the instructions for the WLG and played ten rounds in their fixed teams of three. After the first and the last round of the WLG, we elicited subjects' beliefs about the effort levels of their other two team members. After every round of the WLG, subjects received limited feedback (Brandts & Cooper, 2006b) about the minimum effort in the previous round but not about the individual effort of their team members.

After the ten rounds of the WLG, subjects individually answered a brief survey to elicit team-specific information. We used the Inclusion of Other in the Self Scale (IOS) (Aron et al., 1992), the We scale (Cialdini et al., 1997), and an attractiveness rating[9] (He et al., 2017) for each team member. In addition, perceived human presence (Cohn et al., 2022) and perceived anonymity (Hite et al., 2014) were elicited. To conclude the study, subjects were asked if they knew any of the other team members prior to the meeting, and in the *Video* treatment, if the other team members had selected the gray background during the meeting.

As Part 2 was carried out mainly in a team and dropouts and waiting times could be problematic, we followed the recommendations of Arechar et al. (2018). As soon as subjects were grouped at the beginning of the communication stage, each study page was equipped with on-screen timers. If subjects did not complete a page in time, they were counted as dropouts, and their team members were notified. If a dropout occurred at any point during the communication stage, the other team members could not complete the study and were paid in part, compensating them for their time. During the team decision stage, dropouts did not immediately terminate the study for the whole team. Instead, the remaining team members could, with the knowledge that a team member dropped out, continue with the WLG, [10] finish the study and were paid in full.

Subjects received a fixed payment of £6.00 for participation and a bonus payment. This bonus

[9] In the *Audio* treatment, subjects rated attractiveness of the voice only, in the *Photo* and *Video* treatment, physical attractiveness was rated additionally.

[10] Only the decisions of the remaining team members were relevant to determine the payoff, and this was communicated to the remaining subjects.

payment depended on the outcome of the WLG, and a computer randomly selected one of the ten rounds (with equal probability to mute potential income effects) as payoff-relevant. The subjects received, on average, 240.44 ECU as a bonus, which corresponds to an average of £4.81. In total, this results in subjects' average earnings of £10.81 in Part 2. Part 2 had an average duration of about 32.1 minutes, which results in average hourly earnings for subjects of around £20.17.

4.2.4 Hypotheses

The main objective of our paper is to study how different nonverbal cues impact cooperation and coordination. As we are interested in the immediate treatment effects on coordination and cooperation, we focus our hypotheses on the first round of the WLG. As the WLG was played in a limited feedback setting (Brandts & Cooper, 2006b), subjects received information on the team minimum after each round. Therefore, treatment effects might be diminished after the first round. Recall that there were no nonverbal cues in the *Audio* treatment. Therefore, this treatment served as a baseline compared to the *Photo* and *Video* treatments. In the *Photo* treatment, subjects received additional static nonverbal cues, as they could see team members' selfies. In the *Video* treatment, we increased the amount of nonverbal cues in the virtual meeting even further. In addition to team members' faces, subjects received dynamic nonverbal cues in a virtual video meeting with turned-on cameras.

Based on literature on communication channels (e.g., Bochet et al., 2006; Brosig et al., 2003), group cohesion (Gächter et al., 2025), identification (Andreoni & Petrie, 2004; Bohnet & Frey, 1999b; Eckel & Petrie, 2011), and media richness (Balliet, 2010; Daft & Lengel, 1986), we expected that increasing the amount of nonverbal cues in a virtual meeting would have positive effects on coordination and cooperation. In particular, in the *Photo* treatment compared to the *Audio* treatment, anonymity was reduced because visual identification of team members was added. The identification of others has been shown to increase cooperation in a public goods game (Andreoni & Petrie, 2004) and in a prisoner's dilemma (Bohnet & Frey, 1999b), to enhance trustworthiness in a trust game (Eckel & Petrie, 2011) and to facilitate the detection of whether responders accepted or rejected offers in a ultimatum game (Van Leeuwen et al., 2018). When comparing the *Video* treatment with the other two treatments, following the media richness theory from Daft and Lengel (1986), we expected that video would lead to higher coordination and cooperation. In a public goods game, video communication significantly increased cooperation compared to audio communication (Brosig et al., 2003). Moreover, non-verbal signals, such as smiles, winks, or handshakes, that were in our setting only available in the *Video* treatment, facilitated group cooperation in a WLG (Manzini et al., 2009). This leads us to the following hypotheses:

H1: Enriching virtual pre-play communication to transmit more nonverbal cues leads to higher levels of coordination in teams in the first round of a WLG.

H2: Enriching virtual pre-play communication to transmit more nonverbal cues leads to higher levels of cooperation in teams in the first round of a WLG.

In particular, we expect that (i) coordination/cooperation in the *Photo* treatment is higher than in the *Audio* treatment, and (ii) coordination/cooperation in the *Video* treatment is higher than in the *Photo* treatment.

4.3 Data and Estimation Strategy

In the following, we describe the main variables used in the analyses and provide information on the sample and our estimation strategy.

4.3.1 Variables of Interest

We use two main variables to test our hypotheses to measure coordination and cooperation. These variables are used based on Gächter et al. (2025), who define coordination as matching the effort level of other team members (reaching any Nash equilibrium) and cooperation as achieving pareto-superior equilibria. Therefore, if coordination is high, subjects will select the same effort level, resulting in a low wasted effort. Besides, high cooperation requires a high minimum effort in the team. In line with this definition, we choose the *Team Minimum Effort in round r* and the *Team Wasted Effort in round r* as our main variables.

Team Wasted Effort in round r

To test our first hypothesis (coordination), we use the wasted effort in round $r = 1$ at the team level. The team's wasted effort is the total sum of the individual efforts in a team minus the total sum of the minimum efforts in a team: *Team Wasted Effort* = $\sum_{i=1}^{n}(x_i - x_{min})$.

Team Minimum Effort in round r

Similarly, to test our second hypothesis (cooperation) we use the minimum effort in round $r = 1$ at the team level: *Team Minimum Effort* = $\min_i(x_i)$. As subjects select from the possible effort choices {0, 10, 20, 30, 40}, the *Team Minimum Effort* in round 1 will also be one of these values.

We use the indicator variable *Photo* (*Video*) to analyze pairwise differences between our treatments, which is one if a subject or team was part of the *Photo* (*Video*) treatment and zero otherwise.

4.3.2 Sample Selection

In total, 1775 subjects completed Part 1. We excluded 12 subjects because they failed both attention checks. In addition, we excluded 89 subjects because their uploaded selfie or audio recording did not fulfill the requirements explained in the upload instructions (e.g., subjects' selfie did not display their face or subjects did not record their voice). As a result, 1674 subjects who met our requirements were invited to participate in Part 2. From these subjects, 963 fulfilled our requirements to start the communication stage in Part 2 and were grouped into teams of three.[11] Thus, 321 teams were able to start the communication stage, 105 teams in the *Audio* treatment, 93 teams in the *Photo* treatment, and 123 in the *Video* treatment. Of these 321 teams, 204 teams were able to finish the study, forming our analytical sample (67 teams in the *Audio* treatment, 68 teams in the *Photo* treatment, and 69 teams in the *Video* treatment). All teams with technical issues during the virtual meeting or dropouts were directly excluded from the study or excluded from our analytical sample.

4.3.3 Empirical Strategy

We started the analysis of our hypotheses with non-parametric tests. First, we investigated the differences between all three treatments and, subsequently, examined all pairwise differences. Then, we estimated a series of ordinary least squares (OLS) regressions where i indexes teams.

To investigate Hypothesis 1, we analyzed the teams' coordination behavior and first introduced

[11] Before the communication stage, subjects had to pass a browser, audio, and video check and had to confirm their uploaded selfie and audio recording from Part 1. Furthermore, we could not form a team of three for some subjects because not enough subjects were available during the time slot.

the variables *Photo* and *Video* to show the pure treatment effects. In the second specification, we added the control variables *Homophily*, *Team Cohesion Part 1*, and *Team Risk Aversion*. *Homophily* refers to an index that increases with the similarity of team members in terms of age, gender, and ethnicity. *Team Cohesion Part 1* and *Team Risk Aversion* control for team average preferences on team cohesion and risk aversion that we elicited before the communication stage in Part 1 of our study. We followed the same strategy to investigate Hypothesis 2. This results in the following regression equation:

$$Y_i = \beta_0 + \beta_1 photo_i + \beta_2 video_i + \beta_3 controls_i + \epsilon_i$$

Y refers to the *Team Wasted Effort* and *Team Minimum* in Round 1.

4.4 Results

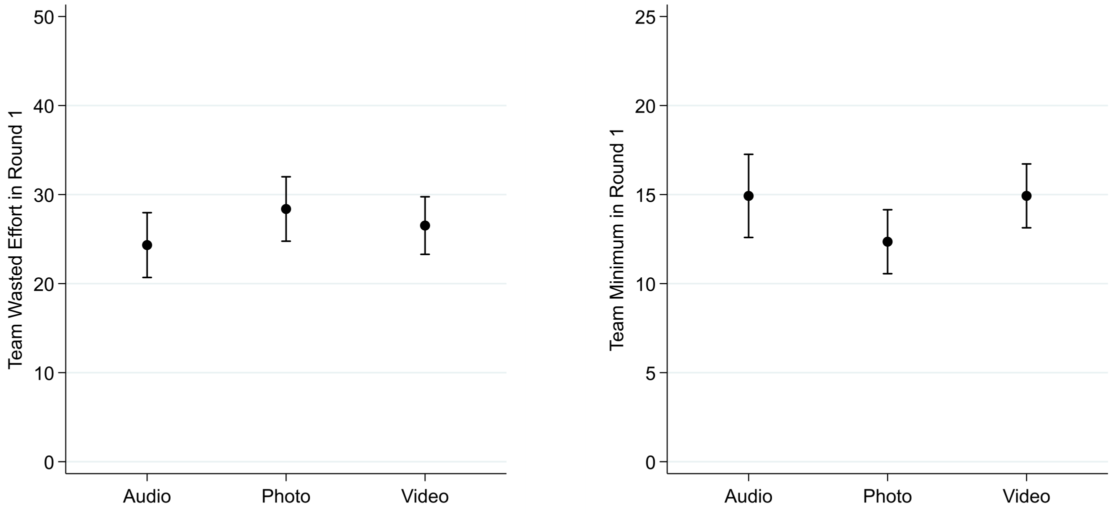
In the following, we analyze coordination in the WLG and, afterward, cooperation. As pre-registered, our main analysis covers non-parametric tests and regressions investigating coordination and cooperation in Round 1, then we conduct some exploratory analyses to investigate individual behavior, behavior over rounds, and potential mechanisms.[12]

4.4.1 Coordination

We start with descriptive information about the teams' coordination behavior in Round 1 in all three treatments. Recall that we measure coordination with *Team Wasted Effort*. Thus, higher levels of coordination are measured by lower levels of *Team Wasted Effort*. In the baseline (*Audio* treatment) with no nonverbal cues, *Team Wasted Effort* was, on average, 24.33 (SD = 15.20). We observed higher levels of 28.38 in the *Photo* treatment (SD = 15.22) and 26.52 in the *Video* treatment (SD = 13.70) (see Figure 4.3a and Table 4.2). A Kruskal–Wallis test showed that the treatments are not statistically significant from each other ($p = 0.217$). Likewise, pairwise comparisons between our treatments revealed no significant differences (see Table

[12]Randomization checks showed no significant differences in demographics (age, gender, ethnicity, education) and social preferences (in particular *Team Risk Aversion*) between the treatments. Overall, *Team Cohesion Part 1* scores were also not significantly different between treatments (Kruskal–Wallis test, $p = 0.146$). Only a pairwise comparison between the *Photo* and *Video* treatment revealed a significant difference in *Team Cohesion Part 1* at the 10% level (Mann-Whitney U test, $p = 0.069$).

4.2).[13] Thus, more nonverbal cues during the virtual meeting did not facilitate higher team coordination in Round 1.



(a) Team Wasted Effort

(b) Team Minimum Effort

Note: Dots indicate means and whiskers indicate 95% confidence intervals.

Figure 4.3: *Team Wasted Effort* (Coordination) and *Team Minimum* (Cooperation) in Round 1 by treatment

A series of OLS regressions reported in Table 4.1 support the initial impression that enriching virtual communication with nonverbal cues did not decrease the wasted effort in teams. The coefficients for the *Photo* and *Video* treatments are positive in Models (1) and (2), but the differences are not significant. Two-sided F-tests of the linear hypothesis *Photo* = *Video* also showed that the difference between the *Photo* and the *Video* treatment is not statistically significant (Model (1) $p = 0.453$; Model (2) $p = 0.451$).

Result 1 Enriching virtual pre-play communication to transmit more nonverbal cues did not lead to higher levels of coordination (lower levels of wasted effort) in teams in the first round of a WLG.

4.4.2 Cooperation

Descriptive information about the teams' cooperative behavior in Round 1 reveals mixed results. Recall that we measure cooperation with *Team Minimum Effort*. Thus, higher levels of cooperation are measured by higher levels of *Team Minimum Effort*. *Team Minimum Effort*

[13]Only the pairwise comparison between the *Audio* and *Photo* treatment showed a significant difference at the 10% level ($p = 0.095$).

Dep. Var.:	*Team Wasted Effort*		*Team Minimum Effort*	
	(1)	(2)	(3)	(4)
Photo Treatment	4.054	4.252	-2.572*	-2.734*
	(2.618)	(2.666)	(1.503)	(1.505)
Video Treatment	2.193	2.350	0.002	-0.123
	(2.484)	(2.535)	(1.502)	(1.522)
Homophily		7.829		3.101
		(9.635)		(5.160)
Team Cohesion Part 1		-0.467		0.217
		(1.391)		(0.795)
Team Risk Aversion		0.289		-0.513
		(0.793)		(0.444)
Constant	24.328***	18.922*	14.925***	15.492***
	(1.857)	(10.021)	(1.191)	(5.573)
Observations	204	204	204	204
Photo Treatment = Video Treatment	0.453	0.451	0.048	0.046

Robust standard errors clustered at the team level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: As pre-registered, we also conducted ordered probit regressions. The regression results are similar and can be found in Table B.1 in Appendix B.1.

Table 4.1: OLS regressions with *Team Wasted Effort* and *Team Minimum Effort* in Round 1 as dependent variables

	(1) Audio	(2) Photo	(3) Video	(1) vs (2)	(1) vs (3)	(2) vs (3)
Team Wasted Effort Round 1	24.33 (15.20)	28.38 (15.22)	26.52 (13.70)	$p = 0.095$	$p = 0.251$	$p = 0.487$
Team Minimum Effort Round 1	14.93 (9.75)	12.35 (7.55)	14.93 (7.60)	$p = 0.197$	$p = 0.596$	$p = 0.046$

Standard deviations are in parentheses. The last three columns report results from pairwise statistical comparisons based on two-sided Mann-Whitney U tests.

Table 4.2: Summary statistics and pairwise treatment comparisons based on non-parametric tests

was, on average, 14.93 in the *Audio* treatment (SD = 9.75), 12.35 in the *Photo* treatment (SD = 7.55) and 14.93 in the *Video* treatment (SD = 7.60). While a Kruskal–Wallis test showed that these treatment differences are not statistically significant ($p = 0.137$), the *Photo* treatment yielded the worst results (see Figure 4.3b and Table 4.2).[14]

A series of OLS regressions reported in Table 4.1 support the initial impression of a U-shaped form of how nonverbal cues affected team cooperation. The coefficient for the *Photo* treatment is negative in Models (3) and (4) and significant at the 10% level. In our naturalistic "weakest link" game, adding subjects' selfies in an audio meeting reduced the minimum hours allocated

[14]The pairwise comparison between the *Photo* and *Video* treatment showed a significant difference at the 5% level ($p = 0.046$).

to the team project by an average of more than 2.5 hours per week (approx. -17%). In contrast, the coefficient for the *Video* treatment is close to zero in Models (3) and (4) and not significant. Two-sided F-tests of the linear hypothesis *Photo* = *Video* showed that the difference between the *Photo* and the *Video* treatment is statistically significant (Model (3) $p = 0.048$; Model (4) $p = 0.046$). Overall, the *Team Minimum Effort* was higher in both the *Audio* and the *Video* treatment compared to the *Photo* treatment. Thus, the *Audio* and the *Video* treatment led to similar results. While static nonverbal cues decreased cooperation, allowing dynamic nonverbal cues appears to have mitigated the backfiring compared to the *Audio* treatment.

Result 2 Enriching virtual pre-play communication to transmit more nonverbal cues revealed a U-shaped impact on cooperation in teams in the first round of a WLG. Adding subjects' selfies as static nonverbal cues had a negative impact, whereas dynamic nonverbal cues led to similar results as audio communication.

4.4.3 Exploratory Analysis

In Sections 4.4.1 and 4.4.2, we analyzed decisions at the team level in Round 1 of the WLG. In this section, we provide exploratory analyses to assess decisions at the subject level, decisions over all 10 rounds at the team level, and potential mechanisms for the results on cooperation in Section 4.4.2.

To investigate cooperation and coordination behavior at the subject level, we followed the literature (e.g., Avoyan & Ramos, 2023) and calculated the normalized efficiency of subjects for each round in the WLG.

Efficiency in round r

$$\text{Efficiency} = \frac{\text{actual payoff} - \text{minimum possible payoff}}{\text{maximum possible payoff} - \text{minimum possible payoff}} = \frac{\text{actual payoff}}{400}$$

In our study, the minimum possible payoff in the WLG was 0 ECU and the maximum possible payoff was 400 ECU. In Round 1, *Efficiency* was, on average, 0.585 in the *Audio* treatment, 0.536 in the *Photo* treatment, and 0.576 in the *Video* treatment (see Figure B.2 in Appendix B.1). Similar to the results on team cooperation in Round 1, *Efficiency* was statistically significantly higher in *Audio* and *Video* compared to *Photo* (see Table 4.4).

Again, we conducted a series of OLS regressions reported in Table 4.3 to investigate the robustness of this initial impression. The coefficient for *Photo* is negative and statistically significant at the 10% level, also when controlling for demographic characteristics as well as individual risk and group preferences.[15] Two-sided F-tests of the linear hypothesis *Photo* = *Video* showed similar results, but differences are no longer significant at the 10% level when including control variables (Model (1) $p = 0.090$; Model (2) $p = 0.106$). Thus, similar to the results on cooperation at the team level, the performance at the subject level, measured in terms of *Efficiency*, was reduced by including static cues compared to no nonverbal cues. Dynamic cues in *Video* enhanced subjects' performance again to the level of *Audio*.

Dep. Var.: *Efficiency*	(1)	(2)
Photo Treatment	-0.049*	-0.049*
	(0.027)	(0.027)
Video Treatment	-0.009	-0.010
	(0.026)	(0.026)
Female		0.016
		(0.015)
Age		-0.000
		(0.001)
Ethnicity White		0.019
		(0.017)
Team Cohesion Part 1		-0.003
		(0.006)
Risk Aversion		-0.002
		(0.003)
Constant	0.585***	0.592***
	(0.021)	(0.048)
Observations	612	612
Photo Treatment = *Video Treatment*	0.090	0.106

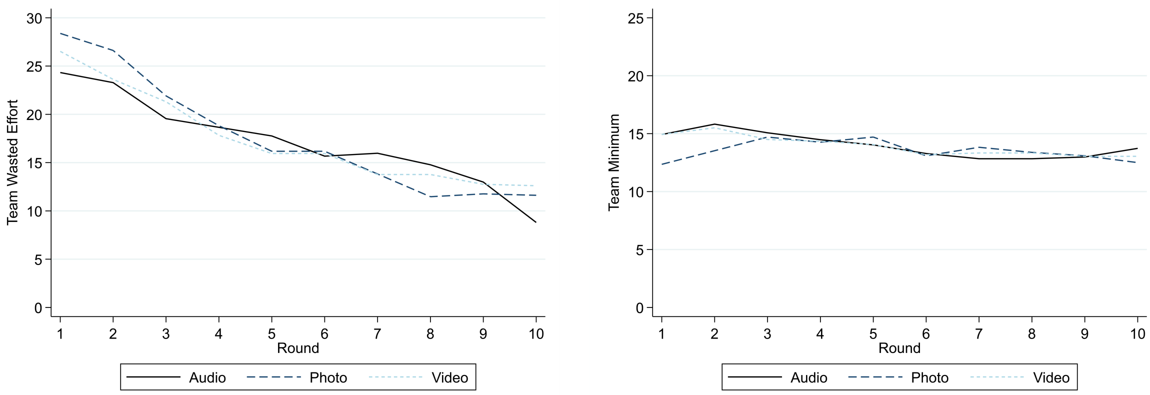
Robust standard errors clustered at the team level in parentheses,
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.3: OLS regressions at the subject level with *Efficiency* in Round 1 as dependent variable

Following this subject-level analysis, we analyzed the team behavior over all ten rounds of the WLG. For *Team Wasted Effort* over all rounds, no distinct differences between the treatments were observed (see Figure 4.4a). While *Team Wasted Effort* in the *Photo* treatment stayed highest until Round 3 and the order of the other two treatments also remained the same in the first three rounds, the treatment differences declined and, starting with Round 4, no clear order or treatment differences were visible. Over the rounds, the average *Team Wasted Effort* in all treatments decreased significantly (see Table B.2 in Appendix B.1).

[15]Remember that the pairwise comparison between the *Photo* and *Video* treatment revealed a significant difference in *Team Cohesion Part 1* at the 10% level (Mann-Whitney U test, $p = 0.069$).

Figure 4.4b shows that the treatment differences in *Team Minimum Effort* were largest in the first round and disappeared in Round 3 of the WLG. From then on, there was no longer a distinct difference between treatments. In Round 2, the treatment difference described in Section 4.4.2 between *Photo* and *Video* was no longer statistically significant (two-sided Mann-Whitney U test, $p = 0.297$). The average *Team Minimum Effort* across all rounds was 14 in *Audio*, 13.54 in *Photo*, and 13.93 in *Video* and all pairwise comparisons based on two-sided Mann-Whitney U tests are not statistically significant (see Table 4.4). Overall, cooperative behavior was stable, and the *Team Minimum Effort* did not increase or decrease significantly over the rounds.



(a) Coordination

(b) Cooperation

Figure 4.4: *Team Wasted Effort* and *Team Minimum* over all rounds by treatment

	(1) Audio	(2) Photo	(3) Video	(1) vs (2)	(1) vs (3)	(2) vs (3)
Individual Efficiency Round 1	0.585 (0.196)	0.536 (0.184)	0.576 (0.171)	$p = 0.032$	$p = 0.840$	$p = 0.037$
Team Minimum Effort Round 1-10	14 (9.69)	13.54 (8.92)	13.93 (9.33)	$p = 0.952$	$p = 0.954$	$p = 0.956$
Team Belief Minimum Round 1	19.75 (6.87)	19.36 (6.82)	20.48 (6.65)	$p = 0.946$	$p = 0.395$	$p = 0.385$
Team Cohesion Part 2	3.92 (0.97)	3.59 (1.01)	3.88 (1.03)	$p = 0.054$	$p = 0.951$	$p = 0.064$
Team Anonymity	5.68 (0.69)	5.41 (0.74)	5.17 (0.84)	$p = 0.038$	$p = 0.000$	$p = 0.054$
Team Human Presence	4.74 (0.92)	4.47 (0.90)	4.74 (0.80)	$p = 0.066$	$p = 0.851$	$p = 0.070$

Standard deviations are in parentheses. The last three columns report results from pairwise statistical comparisons based on two-sided Mann-Whitney U tests.

Table 4.4: Summary statistics for exploratory analyses and pairwise treatment comparisons based on non-parametric tests

Next, we discuss potential mechanisms that may explain our findings on team cooperation in Round 1. The results from Gächter et al. (2025) showed that beliefs and team cohesion affect team cooperation in a WLG. Thus, as described in Section 4.2.3, we elicited subjects' beliefs about the effort levels of their other two team members after the first round and team cohesion in the post-experimental survey. To study beliefs and team cohesion as potential mechanisms at the team level, we followed (Gächter et al., 2025) and calculated a belief regarding the team minimum (*Team Belief Minimum Round 1*) and constructed a team cohesion index (*Team Cohesion Part 2*). In addition, we elicited subjects' perceived anonymity (Hite et al., 2014) and their perceived human presence (Cohn et al., 2022), because our treatments varied subjects' visual identification within the communication stage. *Team Anonymity* describes the team average of the subjects' perceived anonymity, and *Team Human Presence* describes the team average of subjects' perceived human presence. Values are higher if subjects perceived a higher anonymity or human presence.

Note that these four measures were not exogenous because our treatment variations influenced them. While *Team Belief Minimum Round 1* did not differ significantly between treatments (Kruskal-Wallis test, $p = 0.606$) and also pairwise comparisons are not significant (see Table 4.4), team cohesion varied. *Team Cohesion Part 2* was, on average, 3.92 in the baseline, 3.59 in the *Photo* treatment, and 3.88 in the *Video* treatment.[16] A Kruskal–Wallis test showed that these treatment differences are significant at the 10% level ($p = 0.092$). Pairwise comparisons yield similar differences between the treatments compared to differences in *Team Minimum Effort* in the first round and indicate that static photos tended to mute the development of team cohesion (see Table 4.4 and Figure B.3). Although a Kruskal-Wallis test revealed no significant difference ($p = 0.107$), the treatment differences for perceived human presence show similar patterns. Data on *Team Anonymity* reveal that our treatment variation on visual anonymity had the expected effect on perceived anonymity. *Team Anonymity* was, on average, 5.68 in the baseline, 5.41 in the *Photo* treatment, and 5.17 in the *Video* treatment. Thus, the perceived anonymity of the subjects decreased significantly when more nonverbal cues for visual identification of other team members were available during the communication phase (Jonckheere–Terpstra test, $p < 0.001$).

Despite these results showing how nonverbal cues in virtual communication can influence team

[16]Interestingly, the average *Team Cohesion Part 2* was $\in (2, 4]$ in all three treatments. Thus, although the subjects did not know any other team members and the teams varied concerning demographic characteristics, they achieved a medium level of team cohesion based on the categorization following Gächter et al. (2025).

cohesion, human presence, and anonymity, we did not include these variables in the regressions in Table 4.1 due to endogeneity concerns. Hence, we conducted a decomposition analysis for each treatment to investigate potential mechanisms of how our treatments affected the *Team Minimum Effort*. An analysis of correlation independent of our treatments shows that there is a positive significant correlation between *Team Minimum Effort* and *Team Belief Minimum Round 1* ($r = 0.648$) and between *Team Minimum Effort* and *Team Cohesion Part 2* ($r = 0.214$) (see Table B.3 in Appendix B.1). Therefore, the decomposition analysis in each treatment was based on the following simultaneous equation model, where i indexes teams:

$$\begin{aligned}\text{Team Minimum Effort}_i = {} & \beta_0 + \beta_1 \cdot \text{Team Belief Minimum Round } 1_i \\ & + \beta_2 \cdot \text{Team Cohesion Part } 2_i + \beta_3 \cdot \text{Team Anonymity}_i \\ & + \beta_4 \cdot \text{Team Human Presence}_i + \beta_5 \cdot \text{Homophily}_i \\ & + \beta_6 \cdot \text{Team Risk Aversion}_i + \epsilon_i \qquad (1)\end{aligned}$$

$$\begin{aligned}\text{Team Belief Minimum Round } 1_i = {} & \beta_0 + \beta_1 \cdot \text{Team Cohesion Part } 2_i + \beta_2 \cdot \text{Team Anonymity}_i \\ & + \beta_3 \cdot \text{Team Human Presence}_i + \beta_4 \cdot \text{Homophily}_i \\ & + \beta_5 \cdot \text{Team Risk Aversion}_i + \epsilon_i \qquad (2)\end{aligned}$$

$$\begin{aligned}\text{Team Cohesion Part } 2_i = {} & \beta_0 + \beta_1 \cdot \text{Team Anonymity}_i + \beta_2 \cdot \text{Team Human Presence}_i \\ & + \beta 3 \cdot \text{Homophily}_i + \beta 4 \cdot \text{Team Risk Aversion}_i + \epsilon_i \qquad (3)\end{aligned}$$

The results in Table 4.5 reveal that in all treatments only the *Team Belief Minimum Round 1* had a positive significant effect on the team minimum in Round 1, while all other control variables, including *Team Cohesion Part 2*, had no significant direct effect on team cooperation in the first round (equation (1)). However, *Team Cohesion Part 2* indirectly affected team cooperation via beliefs but only in the *Audio* and *Photo* treatment. In Models (1) and (2), it had a positive significant effect on *Team Belief Minimum Round 1*, whereas in the *Video* treatment, team cohesion did not significantly affect teams' beliefs (equation (2)). When looking at the effect of *Homophily* on beliefs, we see differences between the treatments. While in the *Audio* and *Video* treatments, *Homophily* had no significant effect on the minimum belief within teams, it had a positive and strongly significant effect in the *Photo* treatment. Compared to the baseline, static

	(1) *Audio*	(2) *Photo*	(3) *Video*
Equation 1			
Dep. Var.: Team Minimum Effort			
Team Belief Minimum Round 1	1.083***	0.637***	0.726***
	(0.140)	(0.106)	(0.104)
Team Cohesion Part 2	-0.779	-0.653	0.577
	(1.135)	(1.007)	(0.966)
Team Anonymity	-1.324	-1.424	0.428
	(1.286)	(0.949)	(0.830)
Team Human Presence	0.784	1.138	0.138
	(1.108)	(1.141)	(1.214)
Homophily	5.229	-7.719	4.558
	(6.505)	(8.075)	(7.087)
Team Risk Aversion	-0.788	-0.002	0.065
	(0.644)	(0.554)	(0.550)
Constant	2.322	10.231	-8.547
	(10.902)	(9.350)	(8.939)
Equation 2			
Dep. Var.: Team Belief Minimum Round 1			
Team Cohesion Part 2	3.213***	2.432***	1.623
	(1.199)	(0.939)	(1.220)
Team Anonymity	0.756	0.336	-0.151
	(1.198)	(1.170)	(1.069)
Team Human Presence	-1.175	-1.294	-0.322
	(1.111)	(0.981)	(1.517)
Homophily	1.054	18.972***	-10.088
	(5.609)	(6.388)	(7.479)
Team Risk Aversion	-0.332	-0.315	-0.434
	(0.626)	(0.647)	(0.469)
Constant	10.051	3.872	26.162***
	(8.982)	(8.175)	(8.858)
Equation 3			
Dep. Var.: Team Cohesion Part 2			
Team Anonymity	0.018	0.005	0.074
	(0.141)	(0.126)	(0.101)
Team Human Presence	0.751***	0.689***	0.986***
	(0.097)	(0.111)	(0.101)
Homophily	-0.215	-0.568	-0.078
	(0.592)	(0.878)	(0.705)
Team Risk Aversion	-0.047	0.134	0.072
	(0.051)	(0.090)	(0.052)
Constant	0.723	-0.041	-1.602*
	(0.979)	(1.141)	(0.921)
Log pseudolikelihood	-734.5	-735.0	-738.1
Observations	67	68	69

Robust standard errors clustered at the team level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.5: Mediation analysis: Estimating simultaneous linear equations

selfies made demographics more salient in a team, which might have increased the relevance of homophily on behavior within a team. In the *Video* treatment compared to the *Photo* treatment, the demographics of team members should be equally salient in a team. Still, the coefficient of *Homophily* in the *Video* treatment is not significant and, interestingly, negative and not positive compared to the *Photo* treatment. Thus, the dynamic nonverbal cues influenced the effect of *Homophily* on *Team Belief Minimum Round 1*. The third equation in Table 4.5 shows that in all three treatments, only *Team Human Presence* positively and significantly affected team cohesion. It, therefore, indirectly affected team cooperation in the *Audio* and *Photo* treatment.

4.5 Discussion

The results of our study show that nonverbal cues affected team cooperation but not coordination. Against our expectation that an increased amount of nonverbal cues in general, as well as the possibility of identification alone, would lead to higher levels of coordination and cooperation, we even observed negative effects of visual identification through static nonverbal cues in the form of a selfie on team cooperation. In the following, we first discuss the robustness and possible mechanisms for our results on cooperation. Second, we explain limitations and highlight avenues for future research.

Previous research showed that high perceived attractiveness of the person displayed can affect the behavior of the other team members (Andreoni & Petrie, 2008; Ruffle & Shtudiner, 2015; Wilson & Eckel, 2006), e.g., through an increase in perceived cooperativeness (Zylbersztejn et al., 2024a). While we have subjective ratings of team members' physical attractiveness in the *Photo* and *Video* treatment, the usage of these scores comes with endogeneity problems. Therefore, we have conducted a short follow-up study to elicit exogenous ratings of subjects' perceived attractiveness (see Appendix B.2). *Team Attractiveness* describes the team average of the exogenous ratings on subjects' attractiveness, *Team Variance Attractiveness* the variance of team members' attractiveness scores.[17]. The exogenous attractiveness ratings of our follow-up study and the endogenous ratings in Part 2 are significantly correlated (Pearson correlations, $p < 0.01$). This holds true for individual attractiveness ratings, as well as for the *Team Attractiveness* and there are exogenous treatment differences for individual attractiveness ratings between *Photo* and *Video* (see Table B.4 in Appendix B.2). We included *Team Attractiveness* as

[17]The discussion of *Team Variance Attractiveness* can be found in Appendix B.2.

a control in the OLS regressions (see Table B.6 in Appendix B.2).[18] It has no significant impact on the *Team Minimum Effort* in Round 1, and the coefficient for the *Video* treatment is positive in Model (5) and significant at the 10% level. Thus, *Team Attractiveness* cannot explain the differences between the *Photo* and *Video* treatment on cooperation.

A subgroup analysis revealed that the significant treatment differences observed in the overall sample persisted only in teams with below-median *Team Attractiveness* (two-sided Mann-Whitney U test, $p = 0.043$). In contrast, no significant treatment differences are found in teams with above-median *Team Attractiveness* ($p = 0.412$). These findings were also confirmed with OLS regressions (see Table B.7 in Appendix B.2). In teams that were below-median in terms of *Team Attractiveness*, treatment differences are significant at the 5% level. Including control variables, treatment differences remain, but only at the 10% level. In addition, *Homophily* has a significant positive effect on team cooperation (at the 10% level), which might be responsible for the decrease in difference between *Photo* and *Video* treatments. Therefore, the selfies particularly seem to have an effect in teams that are rated lower in terms of attractiveness. In these teams, a higher similarity in terms of gender, age, and ethnicity can benefit cooperation.

In the past, it has been shown that the cooperation of others can be detected more accurately when seeing them in a video rather than a photo (Bonnefon et al., 2017). This could explain why the *Video* does better than the *Photo* treatment. Furthermore, some literature provides possible explanations for why cooperation in the *Photo* treatment was lower than in *Audio*. The presence of photos has been shown to be disadvantageous when inferring cooperation behavior in a trust game (Efferson & Vogt, 2013). Another possible explanation for why identification through selfies led to significantly lower cooperation is a potential distraction from the verbal communication content. Through the static nature of the photos, the communication stage could be altered, e.g., through a change in communication content or dynamics. This could be related to the lower perceived human presence in the *Photo* treatment. We analyzed the verbal content using the collected audio recordings to investigate possible treatment differences in the communication stage (see Appendix B.3 for details). First, we look at *Character Count*, which describes the number of characters used in a team (communication length). Figure B.4a in Appendix B.3 shows that the *Character Count* increased from the *Audio* to the *Photo* and further to the *Video* treatment. In particular, the teams in the *Video* treatment talked more than

[18]We only included *Team Attractiveness* as control variable in the regressions as pairwise correlations between the constructs elicited in the follow-up study (see Appendix B.2) are significant (see Table B.5).

in the other two treatments, and these differences are significant (see Table B.9 in Appendix B.3). Similar to *Team Cohesion Part 2*, *Team Human Presence*, and *Team Anonymity*, we could not include *Character Count* in the regressions in Table 4.1 due to endogeneity concerns. However, in Table B.10 in Appendix B.3 we observe that independent of our treatments, *Character Count* has a significant positive effect on *Team Minimum Effort* in Round 1 at the 5% level. Since *Character Count* was significantly higher in the *Video* treatment, the communication length might explain some treatment differences in cooperation between the *Photo* and the *Video* treatment. In addition, we used the *Standard Deviation Speech Share* to examine whether unequal speaking times, e.g., when one team member spoke for the majority of the seven-minute virtual meeting, influenced the subsequent results. While the positive effect of *Character Count* is robust when controlling for *Standard Deviation Speech Share* and other variables, *Standard Deviation Speech Share* has no significant impact on the *Team Minimum Effort* in Round 1. Moreover, *Standard Deviation Speech Share* did not significantly differ between treatments (see Figure B.9 in Appendix B.3). Additionally, the communication content was compared between teams and treatments, using clustering and topic-modeling approaches (see Appendix B.3 for details on the analyses). As no treatment differences in communication content could be observed, only the communication length, but not the actual content, could help understand some of the treatment differences.

Next, we outline some limitations of our study and propose directions for future research. First, a key limitation of our study is that the photos used are simple selfies shot by the subjects themselves and not professional photos, which are usually used in these circumstances. Therefore, results may not be directly transferable to practical applications. Moreover, we did not elicit or account for subjects' preferences and prior experiences with the specific meeting formats. The subjects may have been more accustomed to audio-only meetings, as many employees in small team settings typically keep their cameras off during meetings (Tolliver & Sass, 2024). Consequently, the unfamiliarity of the meeting format could have impacted their decisions. Furthermore, removing the usual option to turn the camera on or off might have led to reactance effects (Brehm, 1966). These reactance effects may have been particularly pronounced in cases where subjects felt that their personal comfort was restricted. Therefore, understanding and accounting for individual preferences and past experiences with these meeting formats could influence the results and help explain some of the observed behavior. This potential limitation relates explicitly to the *Photo* treatment. The lower cooperation observed in this treatment might be partially explained by subjects' lack of experience with audio meetings enriched with selfies. Given that many individuals may be more familiar with purely audio-based meetings or those with live video, this unfamiliar format could have hindered their engagement or sense of comfort, thus impacting their cooperative behavior. This effect might decline with increasing familiarity. Since existing research showed that the choice of a communication channel can matter for individuals (Nieken & Walther, 2024), future research could examine whether individuals prefer different meeting formats and whether such preferences enhance team outcomes. Expanding on the previous limitation, our study did not consider the potential effects of repeated communication within the same meeting format. It is plausible that repeated exposure to a specific format could alter subjects' behavior over time. For example, with repeated communication, subjects might become more accustomed to the *Photo* treatment, reducing initial discomfort and potentially leading to more collaboration. Future research could expand on this by incorporating repeated communication and decision stages to explore how ongoing interaction in a consistent meeting format influences cooperation and coordination. This could provide valuable insights into how nonverbal cues, such as mimics and gestures, play a role in long-term collaboration. Additionally, future studies could focus on analyzing the nonverbal cues in the video meeting. As we only have audio recordings available, our study cannot investigate how and which nonverbal cues actually affected and potentially improved cooperation. Another possibility for future research involves investigating the impact of adding photos to audio meetings when communication is game-related, and subjects are more focused on achieving specific goals. Thereby adding to the existing literature on communication with promises and commitment (e.g., Brosig et al., 2003; Charness and Dufwenberg, 2006).

4.6 Conclusion

Our study introduces a controlled online experiment investigating the impact of nonverbal cues in virtual communication on cooperation and coordination. Specifically, we focused on comparing audio communication, audio communication enhanced with selfies, and video communication. Overall, we did not find a significant effect of nonverbal cues on coordination in the first round of a WLG. However, cooperation was significantly higher when communication did not include any nonverbal cues (*Audio*) or was enriched with dynamic nonverbal cues (*Video*) than when static nonverbal cues allowed visual identification (*Photo*). Similar results hold for the individual efficiency of subjects' outcomes. Looking at potential mechanisms, our results suggest that the belief on the team minimum had a significant effect on the team minimum, while perceived team cohesion indirectly affected the team minimum via beliefs only in the *Audio* and *Photo* treatment and not when cameras were turned on.

Our results suggest that team cooperation was dampened through static nonverbal cues, even though they enabled visual identification and thereby significantly decreased anonymity. A possible explanation for why static nonverbal cues did not have the expected effect on cooperation is the perceived human presence, which was marginally significantly lower in the *Photo* treatment compared to the other treatments and indirectly affected team cooperation. Thus, to increase cooperation in newly formed teams in a one-shot setting, engaging team members to turn on their cameras was not necessary, as audio-only worked similarly well. Instead, encouraging teams in audio meetings to talk more can be effective, as more verbal communication positively affected cooperation in our study. Our insights reveal some practical implications for practitioners and policy-makers. To increase team cooperation as well as coordination, it should not matter if new virtual teams get to know each other in audio or video meetings. Still, based on our results, both of these meeting formats should be preferred over audio meetings enriched with selfies. This is particularly relevant for organizations and companies with very diverse teams (in relation to age, gender, and ethnicity), as cooperation in such teams might suffer from highlighting these differences with selfies. Further research could study repeated interaction settings and extend the research on the effect of photos when communication is game-related.

5 Study IV: Preference-based Personalization of Casual Microtasking Systems: Design and Empirical Findings*

Abstract: Casual microtasking systems integrate microtasks into the daily routines of crowdworkers, leveraging spare moments and, at the same time, increasing quality by situating tasks within real-world contexts. However, ensuring high-quality results remains challenging, with issues like multitasking and lack of motivation negatively affecting outcomes. Despite existing research showing that task design in general has a significant impact on outcomes, research on the design of casual microtasking systems is limited, particularly regarding personalization to crowdworkers' preferences. This study addresses this gap by following a crowdworker-centered design approach for personalized casual microtasking systems, building on the person-environment fit theory. We derive four design requirements and instantiate them in the personalized microtasking system *MyCrowdSurfer*. We conducted two large-scale experimental field studies on Prolific to investigate the impact of different design variants. Our results show that while designs allowing crowdworkers to multitask improved performance compared to sequential monotasking, preference-based personalization had no positive effect. Our study contributes a nuanced understanding and highlights the need to carefully select preference-based personalization design variants, as personalization can also backfire. Our findings provide insights into how to design casual microtasking systems by enhancing their efficacy and addressing real-world challenges faced by crowdworkers.

Keywords: crowdsourcing; microtasking; personalization; preferences; design science

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5.1 Introduction

Crowdsourcing describes the provision of tasks that are executed by a crowd of people working on a task contributing to a common goal (Jäger et al., 2019). When the crowd receives a monetary reward for their contribution, it is called crowdworking (Durward et al., 2016). Crowdworking platforms such as Amazon Mechanical Turk (MTurk), Prolific, or Upwork have become very popular and important in recent years. These crowdworking platforms are used intensively for so-called microtasks, i.e., short and simple tasks such as tagging images, categorizing text elements, or verifying information (Gadiraju et al., 2014). An innovative approach for crowdworking is embedding tasks into crowdworkers' daily lives so that they are available when it is convenient for the crowdworker, so-called casual microtasking. For example, the work by Hahn et al. (2019) shows that integrating short writing tasks into the Facebook feed allows users to leverage spare micromoments during brief breaks from their primary work. Another advantage is that crowdworkers can be in a specific context when working on a task. This can be helpful, for example, for feedback tasks on interface design (Haug et al., 2023). Integrating these feedback tasks into websites ensures situatedness, which can increase participation rates and engagement (Hettiachchi et al., 2020b). Moreover, in a crowdworking context, this can lower crowdworkers' perceived effort for executing a microtask as they can skip searching for and emerging in a task (Haug et al., 2023).

However, similar to traditional crowdworking tasks, ensuring high-quality results is a persistent challenge for casual microtasking. The outcome quality may even be worse than in traditional crowdworking (Haug et al., 2023). Known reasons for low-quality results in crowdworking are malicious workers (Gadiraju et al., 2015) or a lack of intrinsic motivation (Rogstadius et al., 2011). An extensive body of research has already focused on improving the outcome quality in microtask execution (Wang et al., 2017). We argue that, especially for an intrusive approach like casual microtasking, it is crucial to follow a crowdworker-centric approach to develop a design to positively influence crowdworkers' job performance. Previous research implemented approaches like filtering for specific characteristics of crowdworkers like approval rate, the number of completed tasks, or their nationality (e.g., Kittur et al., 2013) to match the requirements of the task with the characteristics of crowdworkers. Such a filtering approach is an initial attempt to provide a better "fit" between the crowdworker and the microtask. One theory that describes this is the person-environment (P-E) fit theory (Cable & Edwards, 2004; Caplan, 1987). It states that creating a fit between a person's abilities, needs, preferences, and values and a job's supplies, demands, and values should positively affect the person's job satisfaction and job performance. Following the rationale of the P-E fit theory and extending this initial filtering approach, personalized task recommendations based on crowdworkers' characteristics can further improve outcomes (Amer-Yahia et al., 2016; Difallah et al., 2013; Paulino et al., 2022).

Existing research has shown that the task design in casual microtasking systems can have a huge impact on task outcomes (Paulino et al., 2022). However, research is scarce on the analysis of the fit between the environment of crowdworkers and their individual preferences for how and when they execute microtasks and how this affects the task outcome. Concluding, there is a lack of research on the design of casual microtasking systems in general and on the personalization of the design according to crowdworkers' preferences in particular.

This paper addresses this research gap and tackles a real-world challenge that crowdworkers face in casual microtasking. We propose preference-based designs for casual microtasking systems and empirically evaluate their impact on crowdworkers' performance. According to the P-E fit theory, humans are more satisfied with a job and perform better when their abilities meet the demands of their environment or when the supplies of the environment address their needs (Cable & Edwards, 2004; Caplan, 1987). The P-E fit theory is frequently applied in IS research to analyze antecedents of job performance and satisfaction (Ayyagari et al., 2011; Tams et al., 2018; Venkatesh et al., 2017). Leveraging the P-E fit theory as kernel theory, we investigate the impact of personalizing the designs according to crowdworkers' preferences. We focus on the fit between the crowdworkers' preferences and their job, the so-called person-job (P-J) fit, as the key aspect of the P-E fit theory. Thus, we assume that casual microtasking systems that consider the preferences of crowdworkers should impact crowdworkers' performance positively.

Although casual microtasking offers the advantage of integrating small tasks into the primary activities of crowdworkers, it presents unique challenges. Casual microtasking is more intrusive and can compromise task quality, as crowdworkers need to multitask and may not focus entirely on these secondary tasks (Haug et al., 2023). Imagine that you are working on a software development task and eventually go on YouTube to search for a tutorial on how to code a feature. You receive a pop-up asking you for your opinion on the YouTube recommendations. Even though you are willing to answer this request, would you be able to give it your full attention and write a long paragraph about what could be improved in the YouTube recommendation algorithm? Maybe yes, maybe no, depending on your preference to switch between different tasks. This example demonstrates the necessity of understanding individual crowdworker's preferences regarding required multitasking. Furthermore, it could even be beneficial to introduce a design variant that is personalized to the individual preference of each crowdworker for monotasking or multitasking.

Social preferences such as altruism, fairness preferences, positive and negative reciprocity, as well as trust (Fehr & Fischbacher, 2002; Fehr & Schmidt, 2001; Levitt & List, 2007) represent another important category of preferences that can have an impact on crowdworker performance. For general participation in crowdsourcing activities, altruism is recognized as one of individuals' most frequently mentioned motives (Buettner, 2015). Altruism describes people's behavior to consider the interests of others without having only selfish ulterior motives (Andreoni et al., 2010). To the best of our knowledge, the role of altruism in financially incentivized crowdworking using personalized casual microtasking systems has not yet been researched. Overall, we focus on two prominent preferences of crowdworkers. Polychronicity as a preference for or against multitasking and altruism as a social preference, and seek to answer the following research questions:

RQ1: How to design a personalized casual microtasking system that takes crowdworkers' polychronicity and altruism preferences into account?

RQ2a/b: How does a polychronicity/altruism-personalized casual microtasking system affect crowdworker performance under consideration of individual polychronicity/altrusim perferences?

To answer these questions, we combine a design science study with two large-scale experimental field studies with crowdworkers actively engaging on the crowdsourcing platform Prolific. First, following a theory-guided design science research (DSR) approach, we propose a crowdworker-centric personalized design of a casual microtasking system called *MyCrowdSurfer*. Building on existing knowledge described by the P-E fit theory and literature on the two preferences (polychronicity and altruism), we derive design requirements for a casual microtasking system that offers different personalized design variants with respect to crowdworkers' preferences. We implement a running software prototype as a browser extension that can integrate microtasks into crowdworkers' daily internet surfing. This extension can adapt its design to crowdworkers' polychronicity and altruism to provide a personalized experience. Second, we use the *MyCrowdSurfer* system to analyze the effects of different personalization design variants on the job performance of crowdworkers. In the first experimental field study, we focus on design variants that address either a multitasking or monotasking preference. Subsequently, in the second experimental field study, we analyze design variants that either address crowdworkers' altruistic or selfish preferences. In both studies, we first analyze the design variants' general effects on crowdworkers' job performance and then investigate how a personalized design variant according to crowdworkers' individual preferences affects their performance. In both studies, crowdworkers need to complete an artificial task as a primary task that asks them to find information on Wikipedia. In parallel, they are invited to work on a bonus task as a secondary task. This involves providing alt-tags for images on the corresponding Wikipedia pages to increase the accessibility of Wikipedia. The participants use the *MyCrowdSurfer* browser extension for seven days to work on both microtasks. After seven days, they participate in a questionnaire.

Our paper extends the literature on crowdsourcing by introducing preference-based personalization of casual microtasking systems. We contribute with descriptive and prescriptive knowledge by suggesting an innovative design and investigating the effects of different personalized task designs on crowdworkers' job performance. Applying P-E fit theory to crowdworking suggests that aligning tasks with crowdworkers' preferences can enhance job performance. In our study, personalized task designs based on crowdworkers' preferences had no positive impact on performance, while task designs allowing crowdworkers to multitask improved performance compared to sequential monotasking. Thus, our study contributes to a more nuanced understanding. Especially for altruism-based personalization, our findings reveal the complex interaction of intrinsic and extrinsic incentives. When looking at preference-based casual microtasking systems, we find that different task design variants can influence job performance. This is a simple and low-cost tool that is easy to apply. However, our results also suggest that these design variants must be selected cautiously because they might backfire. Practically, platform designers can leverage the insights on casual microtasking systems to create more engaging and efficient task environments. By embedding tasks into platforms that users frequently visit, such as social media, designers can increase participation and reduce perceived effort. Further research is needed to explore the fit between crowdworkers' environment and their preferences. Finally, our study provides actionable knowledge for practitioners to design casual microtasking systems for increasing crowdworker performance.

The remainder of this paper is organized as follows. First, we introduce the key underlying concepts of our study, focusing on crowdworking systems and personalization to define the relevant constructs of interest in our research and provide a short overview of related work. Subsequently, we present our kernel theory and derive four design requirements for designing a preference-based personalization of casual microtasking systems. Based on that, we present the design and implementation of *MyCrowdSurfer*. Next, we describe two studies in which we applied the instantiated design variants to investigate the effect of personalization, as introduced above, on crowdworkers' performance. Finally, we summarize our findings, discuss the theoretical and practical contributions, and critically reflect on the limitations. Based on that, we provide ideas for future research.

5.2 Conceptual Foundations & Related Work

5.2.1 Crowdsourcing and Casual Microtasking Systems

Crowdsourcing is a form of digital work that uses a large, undefined group of people to solve tasks (Durward et al., 2016; Howe, 2008). Paid crowdsourcing, called crowdworking (Durward et al., 2016), can be done via online platforms like Prolific and MTurk. These platforms serve as connections between job providers and crowdworkers across the world and usually offer short tasks, so-called microtasks, that only take a few minutes or even seconds to be completed (Kittur et al., 2013). Usually, these tasks do not require specific skills and are repetitive, like labeling tasks, transcriptions, or surveys (Deng et al., 2016). The tasks can be conducted directly via the platform, using additional survey platforms, or via dedicated crowdsourcing systems. Crowdsourcing systems facilitate the outsourcing of tasks to a broad online community, offering a versatile approach to task completion.

A new crowdsourcing concept is casual microtasking, which was introduced by Hahn et al. (2019). Casual microtasking is a type of crowdsourcing where microtasks are seamlessly integrated into other online activities that users are primarily engaged in. This integration allows to leverage spare micromoments and also enables to offer tasks to crowdworkers when they are already in the right context for the task. Goncalves et al. (2015) show the potential of context to motivate participation in ubiquitous crowdsourcing tasks. Therefore, situatedness and context have the potential to increase participation rates and engagement. Context can also lead to less perceived effort for crowdworkers and can improve certain parts of task outcomes, such as making design feedback more real (Haug et al., 2023). Haug et al. (2023) developed the *CrowdSurfer*, a browser extension to integrate microtasks into crowdworkers' everyday internet surfing to leverage the context for specific tasks where context could be beneficial. In this study, we build upon their system, which is publicly available on GitHub.[1]

5.2.2 Personalization

We build on the generic definition of personalization as a process that alters a system's functionality, interface, information access, content, or distinctiveness to enhance its relevance for an individual (Fan & Poole, 2006). Personalization of the system can focus on different dimensions, e.g. the system's design (interaction), the system's content, or the task.

While personalization is known to provide a competitive advantage in online retail, its application in other contexts is rather scarce. The study by Eichler and Dostál (2012) is one of the exceptions. The study investigates how a personalized adaptation of the user interface based on users' activity influences user experience and productivity. The existing body of research in the domain of personalization in IS primarily focuses on consumer-centric applications (Ho & Bodoff, 2014; Thirumalai & Sinha, 2013). However, there is a notable gap in exploring personalization in work-related contexts. While the positive impacts observed in consumer settings are promising, they cannot be directly translated to professional environments.

5.2.3 Personalized Crowdsourcing Systems

Naudet and Lykourentzou (2014) discuss the use of personalization in crowdsourcing and provide a foundational overview. Moreover, Table 5.1 provides an overview of related research in personalized crowdsourcing. Personalized crowdsourcing systems exploit user models to filter, recommend, or adapt crowdsourcing tasks (Naudet & Lykourentzou, 2014). For personalization, the most used characteristics of crowdworkers are interests (Alsayasneh et al., 2017; Amer-Yahia et al., 2016; Wang et al., 2022), skills (Alsayasneh et al., 2017; Kurup & Sajeev, 2018; Wang et al., 2022; Wecker et al., 2019, 2020), and cognitive abilities or cognitive styles (Hettiachchi et al., 2020a; Paulino et al., 2023b, 2023a). The user model that is used for the personalization can be generated via tests, existing user profiles, or task fingerprinting, thus

[1] https://git.scc.kit.edu/h-lab/research/haug_saskia_crowdsurfer

analyzing crowdworkers' behavior in previous tasks (Paulino et al., 2023b). For example, Alsayasneh et al. (2017) ask crowdworkers to select what combination of tasks they would prefer to work on. They use the results to assign crowdworkers to personalized task compositions. Paulino et al. (2023b) use tests and task fingerprinting to assess crowdworkers' cognitive abilities, specifically executive functions like cognitive flexibility.

Regarding the personalization dimension, recent research has shown that personalized task composition and recommending tasks according to crowdworkers' skills, interests, and abilities can improve crowdworkers' experience, task throughput, and task results (Amer-Yahia et al., 2016; Difallah et al., 2013). Geiger and Schader (2014) provide an overview of personalized task recommendations in crowdsourcing and provide a conceptual foundation for designing personalized task recommendation mechanisms. Personalization, in particular, according to cognitive abilities and preferences, has received much attention recently. Hettiachchi et al. (2020a) developed a system that recommends and assigns tasks according to crowdworkers' results in fast cognitive tasks. Thereby, they have been shown to increase job performance.

Besides personalizing the task selection phase, Wecker et al. (2019, 2020) propose further ideas for personalization in different phases of crowdsourcing according to crowdworkers' characteristics like motivational messages, tutorial material, and feedback on crowdworkers' progress. Paulino et al. (2022, p. 484) state that "task design is one of the core aspects of the crowdsourcing process and its optimization is a priority for many requesters that want to have their tasks solved in short times and with high levels of accuracy." Therefore, they explore the adaptation of task designs according to information processing preferences. Their results show that user interface adaptations can improve outcomes and acceptance rates of crowdworkers.

5.2.4 Research Gap

We address a significant gap in the field of crowdworking, focusing on the promising approach of preference-based personalization. While casual microtasking offers unique benefits compared to traditional crowdsourcing tasks, it also presents new challenges. Unlike traditional crowdsourcing where tasks are the primary focus of the crowdworker, in casual microtasking, the task is often only the secondary task, which can potentially lead to reduced focus and decreased quality of results. Therefore, there is a need for a crowdworker-centric design of such tasks.

Study	Type of Study	Assessment	Personalization Characteristic	Personalization Dimension	Results
Alsayasneh et al. (2017)	Algorithm and experimental user study	User selection	Interests (type of task) and skills	Task assignment	Personalization enhances worker experience
Difallah et al. (2013)	Artifact (crowdsourcing system) and experimental user study	Social network profile	Interests (e.g., likes on social media)	Task assignment	Personalization leads to higher task accuracy
Hettiachchi et al. (2019)	Experimental user study	Cognitive test performance	Cognitive abilities	Task assignment and recommendation	Personalization improves task accuracy
Hettiachchi et al. (2020a)	Artifact (crowdsourcing system) and experimental user study	Cognitive test performance	Cognitive abilities	Task assignment and recommendation	Personalization improves job performance
Kurup and Sajeev (2018)	Algorithm and experimental study (evaluation of algorithm performance)	Skill taxonomy mapping	Skills	Task recommendation	Expected: personalization allows new workers to find matching tasks faster
Organisciak et al. (2015)	Experimental user study	Profiling tasks	Taste	Task recommendation	Personalization improves subjective task outcomes
Paulino et al. (2022)	Case study	Cognitive test performance	Cognitive styles	Task design	Personalization improves task matching for better task outcomes
Paulino et al. (2023b)	Case study	Cognitive test and task fingerprinting	Cognitive abilities	Task design	Personalization improves task accuracy effectively
Wang et al. (2022)	Algorithm and experimental study (evaluation of algorithm performance)	Historical repositories of platform	Interest, skills (preferences and technical abilities)	Task recommendation	Personalization leads to less effort for crowdworkers and more efficient matching
This study	Design study and field experiment	Providing alt-tags and questionnaire	Polychronicity and altruism	Task design	Personalization does not improve job performance

Table 5.1: Overview of personalized crowdsourcing research

At the same time, the personalization of task recommendation, assignment, and design in crowd work is evolving. While it can improve task outcomes, there is a lack of a deeper understanding of the effects of personalized crowdsourcing. Further, most personalization approaches focus on skills and interests as workers' characteristics and adapt task recommendations and assignments, but not the task design itself. We want to tackle this research gap by designing and investigating crowdworker-centric personalized casual microtasking systems. We argue that casual microtasking is an intrusive form of crowdworking and, therefore, has a special need for a crowdworker-centric design. It must be seamlessly integrated into crowdworkers' primary tasks and respect their preferred working style. Consequently, personalization according to their polychronicity could be useful. Further, previous research has shown that people exhibit altruism and are thus motivated by non-monetary factors like having an impact and contributing to something good (Andreoni & Miller, 2002; Deng et al., 2016; Deng & Joshi, 2016; Rogstadius et al., 2011). Following the P-J fit theory, we argue that considering crowdworkers' altruistic preferences in the task design and personalizing the task design according to their individual preferences can also improve job performance.

5.3 MyCrowdSurfer - Designing Preference-based Personalized Casual Microtasking Systems

We follow the DSR paradigm to answer *RQ1* and propose a theory-driven design for preference-based personalized casual microtasking systems. In particular, we draw on the P-E fit theory as the kernel theory for our design (Caplan, 1987). We exploit this theory by deriving design requirements, instantiating them in design variants that are personalizable to two contrary preferences. We then rigorously evaluate the design variants of our requirements in two controlled experimental field studies (Venable et al., 2016).

5.3.1 Kernel Theory

Person-Environment Fit Theory The P-E fit theory explores the interplay between individuals and their work environments (Edwards et al., 1998). It posits that a fit between the characteristics of a person and the characteristics of the work environment influences job satisfaction and job performance. Therefore, individuals seek P-E fit, broadly defined as the "congruence, match, similarity, or correspondence between the person and the environment" (Edwards & Shipp, 2007, p. 212). Previous research presented two types of P-E fit: While supplementary fit describes the similarity of characteristics between the human and the work environment, complementary fit describes how the characteristics of the human complement the characteristics of the environment. Supplementary fit is achieved when humans perceive a value congruence with the environment. Complementary fit can further be operationalized as need-supply fit or demand-ability fit (Kristof, 1996). Both are often used to measure employees' perceived fit with their jobs rather than their workgroups or organizations (Guan et al., 2011; Piasentin & Chapman, 2007). There are three levels of P-E fit that individuals might search for in their workplace: person-organization fit, P-J fit, and person-group fit (Kristof, 1996). Person-organization fit deals with the fit between the person's values, beliefs, and goals and the organization's culture. Person-group fit deals with the match between the person and the workgroup. A high fit leads to fewer conflicts and better collaboration (Kristof, 1996). P-J fit deals with the fit between the person's abilities, preferences, skills, and needs with the requirements and offerings of the job (Sekiguchi, 2004). P-J fit is a well-researched concept, especially in the context of recruiting and job engagement (Chen et al., 2014; Sekiguchi, 2004; Warr & Inceoglu, 2012). In our study, we focus on the P-J fit as this fit is the most important aspect of crowd work.

Polychronicity Polychronicity is considered the preference for handling multiple tasks at once, also called multitasking (König & Waller, 2010). It encompasses their inclination to engage in concurrent activities, such as performing two or more tasks simultaneously or switching attention among multiple tasks. The term "polychronicity" describes people's preferences for multitasking, while the actual behaviors, rather than attitudes, should be termed "multitasking" (König & Waller, 2010). Those with higher polychronic tendencies, often referred to as "polychrons", prefer multitasking and are more comfortable with interruptions and switching activities. Those with lower polychronic tendencies, known as "monochrons", lean towards monotasking, where tasks are executed sequentially. Monochrons are known for strict planning, concentrating on and prioritizing tasks (Kaufman-Scarborough & Lindquist, 1999). There are two types of multitasking: Dual-tasking refers to performing two activities simultaneously, such as driving a car and listening to music (Huxhold et al., 2006). Task-switching means that the attention is allocated among multiple tasks before completing any task compared to completing the tasks sequentially (Koch et al., 2010, 2018; Monsell, 2003). Polychrons are humans who have a preference for task-switching, dual-tasking, or both. However, most research on polychronicity considers polychronicity mainly as the preference for task-switching, which can also be seen by the focus on task-switching in existing scales to measure polychronicity (Bluedorn et al., 1999; Kaufman et al., 1991; Lindquist & Kaufman-Scarborough, 2007). In this study, we also only consider task-switching when talking about multitasking. Polychronicity is considered to be a relatively stable individual preference (Howard & Cogswell, 2023).

Lascau et al. (2019) investigate the multitasking behavior of crowdworkers. Their study provides recommendations for crowdworking platform owners and task designers on how to design for crowdworkers' preferences. Their recommendations for task designers are rather broad (e.g., "Pay well") and mainly aim not to force crowdworkers into a multitasking behavior when they prefer monotasking. We argue that there is a need to better understand how task designs can respect and even be personalized according to crowdworkers' preferences, like polychronicity.

There is also empirical support that investigated the P-E fit perspective of polychronicity. Hecht and Allen (2005) and Kirchberg et al. (2015) identify a connection between polychronicity scores and workers' well-being and job satisfaction following the P-E fit theory. Asghar et al. (2020) and Asghar et al. (2021) utilize the P-E fit theory to research the effect of polychronicity on turnover intentions and job performance. The studies were conducted in the context of service jobs, where polychronic workers might experience a greater fit due to the required multitasking. König and Waller (2010) suggest that the effect of polychronicity on job performance depends on the fit between the demands of the task and the abilities of the worker, calling for more empirical research on P-E fit and polychronicity.

Polychronicity is a preference that is beneficial in many jobs nowadays. Crowdworkers tend to multitask, although research showed that they do not all prefer multitasking (Lascau et al., 2019). However, multitasking also has its downsides. When switching from a primary task to a secondary task and back, workers always need some time to immerse in the task again. This time depends on multiple factors (McFarlane & Latorella, 2002). Also, work performance will decrease when monotaskers are forced into a multitasker setting and the other way around.

Altruism An important factor that can impact the performance of crowdworkers are social preferences. While the term preference, especially in the crowdsourcing context, is often used to describe crowdworkers' interests or types of tasks they like to do (Amer-Yahia et al., 2016), social preferences in economics relate to the fact that humans typically not only care about their own outcomes (the "homo economicus") but also take other people's outcomes and intentions into account. There is ample evidence that many people exhibit social preferences and care about the well-being and profit of others in various domains (Charness & Rabin, 2002; Fehr & Fischbacher, 2002; Levitt & List, 2007). Prominent examples of social preferences include altruism, fairness, or reciprocity (Fehr & Fischbacher, 2002; Fehr & Schmidt, 2001; Levitt & List, 2007). Prosocial preferences such as altruism and positive reciprocity also play a role in the workplace. Recent studies reveal that prosocial workers respond with extra effort and are willing to accept lower wages if they receive information about the company's social responsibility (see, e.g., Burbano, 2016 for online labor markets and Cassar, 2019). In addition, multiple studies investigated the motives of crowdworkers to accept crowdworking tasks, how they can be incentivized, and how intrinsic and extrinsic motivation are related (Deng & Joshi, 2016; Fest et al., 2021; Law et al., 2016; Nieken, 2023). Buettner (2015) listed altruism as one of the most frequently mentioned motives of individuals participating in crowdsourcing activities. Taken together, social preferences impact behavior in the workplace and can be used to design jobs and work environments. We argue that personalization based on social preferences, particularly prosocial preferences such as altruism, is a promising approach to improve crowdworker performance and well-being.

5.3.2 Design Requirements

Previous research on personalized crowdsourcing focused mainly on apparent crowdworker characteristics like skills that can easily be matched with task requirements. In our work, we focus on crowdworkers' preferences. While it is not always easy for crowdworkers to identify tasks that match their preferences, working on tasks that do not fit crowdworkers' preferences might reduce their satisfaction and performance. In contrast to previous work, which has focused on the fit between the task content and the worker's skills or interests, we focus on the design of the task. While the content of the task might be hard to change for employers, there is typically some leeway in design choices. We assume that changing the task design will affect crowdworkers' performance in general. Further, we argue that although personalized recommendations and assignment of tasks to workers have been proven to be beneficial, it is also worth investigating personalized task designs. By making small adaptations to the task design, the same task might fit different shapes of preferences and thereby lead to higher satisfaction and better task outcomes for all workers. Paulino et al. (2022) and Paulino et al. (2023b) showed that adapting the task design to crowdworkers' cognitive styles can lead to better task outcomes and higher acceptance rates. Casual microtasking is a very intrusive form of crowdsourcing as it integrates crowdsourcing tasks into crowdworkers' everyday internet surfing. Therefore, these crowdsourcing tasks can potentially interrupt or distract crowdworkers during other primary tasks. Consequently, we articulate the first design requirement for our casual microtasking system as follows:

Design Requirement 1 (REQ1): Casual microtasking systems should support crowdworkers in performing microtasks in parallel to their primary tasks.

While some crowdworkers enjoy working on microtasks during other primary tasks, others feel interrupted and would prefer to work only when it is convenient to them (Haug et al., 2023). Multitasking behavior and polychronicity are known to impact factors like well-being and job performance. Existing research applying P-E fit theory has shown that polychronicity directly affects job performance (Asghar et al., 2020). We need contrary task designs to achieve a P-J fit for both polychrons and monochrons. As casual microtasking is a task type that usually promotes multitasking behavior, we see great potential when personalizing the task design to the polychronicity of the current user. Consequently, we argue that crowdworkers should perform better when the casual microtasking system fosters their polychronicity preference. Therefore, we articulate the following second design requirement for the design of casual microtasking systems:

Design Requirement 2 (REQ2): Casual microtasking systems should be personalized to crowdworkers' polychronicity in order to increase the fit between crowdworkers' polychronicity and the microtasks' required behavior.

As discussed previously, crowdworkers often mention aspects like "making an impact" or "doing something good" as motivational factors (Deng et al., 2016). For example, Deng and Joshi (2016) show that task significance and meaningfulness are important motivators for crowdworkers. Thus, prosocial preferences, and in particular altruism, are promising candidates for task design. Therefore, we articulate our third design requirement:

Design Requirement 3 (REQ3): Casual microtasking systems should encourage crowdworkers to care not only about themselves but also about the well-being and profit of others.

As described in the P-E fit theory, to perceive a fit between an environment and yourself, the characteristics of the environment must be visible. Consequently, we argue that without changing the microtask itself or its purpose, the design of the task can make certain aspects more salient to address different levels of prosocial preferences. For example, the instructions of a simple labeling task could highlight how these labels help to make the resulting algorithm fairer to trigger prosocial preferences. In contrast, the description could focus on the monetary benefits of the crowdworkers and leave the prosocial aspects unmentioned. Thus, we assume that personalizing the saliency of prosocial task aspects to the crowdworkers' prosocial preferences can increase the perceived fit between the task and the crowdworkers' preferences. Therefore, we articulate our fourth design requirement:

Design Requirement 4 (REQ4): Casual microtasking systems should be personalized to crowdworkers' prosocial preferences in order to increase the fit between crowdworkers' preferences and the saliency of the microtasks' attributes.

5.3.3 Design Instantiation

To test our design, we build on the existing casual microtasking open source system *CrowdSurfer* (Haug et al., 2023). We adapted the existing system design to instantiate the four design requirements. Further, we needed an artificial microtask that we could use as context for our experimental studies. For addressing prosocial preferences, finding an appropriate context is not trivial because we needed a context where we could make the prosocial mission of the task more or less salient.

Context As casual microtasking is especially beneficial for tasks that profit from the user being in a specific natural context while working on the task, we were looking for a context that allowed us to make certain aspects of the task's context more or less salient. At the same time, we needed a task that has the potential to trigger prosocial preferences. We decided on the task of providing alt-tags for images on Wikipedia. Alt-tags (also called alternative texts) are descriptions of images that make them accessible (e.g., for people with visual impairments) as they can be read by screen readers. The lack of high-quality alt-tags is a persistent problem in web accessibility and cannot fully be solved by solutions relying on artificial intelligence (Stangl et al., 2020, 2021). For writing good alt-tags, the context of the image must be considered (Kreiss et al., 2022). Therefore, this task is highly suitable for demonstrating the benefits of casual microtasking. At the same time, the task allows us to make the prosocial aspect of alt-tags more or less salient, making the task more or less appealing for prosocial individuals.

The MyCrowdSurfer System The *CrowdSurfer* system is developed as a Google Chrome extension that can integrate microtasks into crowdworkers' everyday internet surfing and, thereby, leverage spare micromoments or take advantage of crowdworkers being in a specific situation when performing tasks. The *CrowdSurfer* design was developed and evaluated in a previous study on the collection of crowd feedback on website designs (Haug et al., 2023). The *CrowdSurfer* recruits and pays participants via existing crowdworking platforms like Prolific or MTurk. The system itself consists of two main elements: The panel to manage the task (Figure 5.1) and pop-ups that appear whenever a task is available on a website (Figure 5.2). The panel shown in Figure 5.1 opens when clicking on the icon in the extensions bar (1). It can, for example, show how many tasks were already conducted by the crowdworkers (4) and give general instructions. The panel also provides a link to redo the tutorial (3) and a button to turn the extension off whenever crowdworkers want to switch to private mode (2). This feature is highly appreciated by crowdworkers and helps them to overcome privacy concerns (Haug et al., 2023). The core functionality of the *CrowdSurfer*, including the pop-ups, enables submitting answers to microtasks (Figure 5.2). Microtasks are attached to an element on a website and can be minimized or rejected. When minimized, small icons still show the availability of tasks.

We call our personalized version of the system *MyCrowdSurfer*. We extended the system to our specific context of collecting alt-tags for images on Wikipedia. In practice, pop-ups would appear for all images requiring an alt-tag on Wikipedia. In a real-world scenario, where the extension would be available for the long term and offers various tasks, crowdworkers would accidentally find the tasks when searching for something on Wikipedia. When they enter a Wikipedia page, they usually have a primary task in mind, e.g., finding specific information. When they see a task pop-up, they are interrupted and tempted to multitask. For monochrons, it would probably be more convenient when they could first finish their primary task and then work on the casual microtask and provide alt-tags for images.

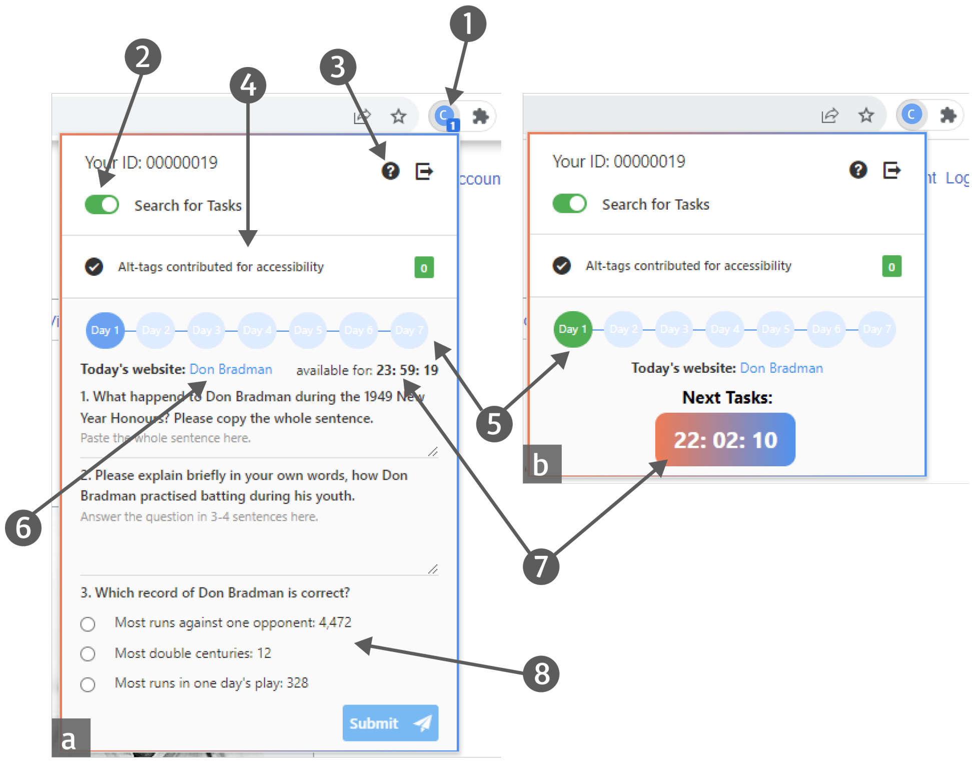


Figure 5.1: Screenshot of the panel of our casual microtasking system

Design personalization for experimental study Conducting a controlled experimental study has the benefit of allowing a proper identification of causal effects. On the other hand, it requires making some design choices that limit the external validity. In the following, we explain our choices in detail. To control the multitasking behavior of crowdworkers and counteract the active search for tasks, we created an artificial main task. The goal of the main task was to direct participants to websites that offer alt-tag tasks. Providing alt-tags is then only presented as a voluntary bonus task. This has the benefit that we can limit the number of available alt-tag tasks to the webpages that are included in our main task. Further, as we control the primary task, we can also track participants' interaction with the primary task to better understand their task-switching behavior. The main task provides a link to a specific topic (see Figure 5.1 (6)) and asks crowdworkers three questions about this topic that can be answered by exploring the Wikipedia page (8). The goal of this task is to mimic a real-life scenario where users visit Wikipedia to search for some specific information and then see the alt-tag tasks. We randomized the order of the tasks for each crowdworker and decided that participants had 24 hours to complete each task. Because the study duration is limited to seven days, we included seven different main tasks in the system. Thus, crowdworkers could do one task a day on average. In the panel, we show an overview of the main tasks in the form of a timeline (5). This timeline shall provide transparency to crowdworkers about how many main tasks they have solved and how many are left. The panel also includes a timer that always shows how much time is left until the current task will have to be finished, and a new main task will appear (7). Finally, to make results more comparable between crowdworkers and to collect enough data points, we show tasks for all images on the webpages, whether or not these images already have a real alt-tag on the Wikipedia webpage.

Polychronicity-based Personalization Design Casual microtasking, in general, enforces multitasking behavior, which might not be appreciated by all crowdworkers as studies show that there is also a vast amount of crowdworkers who tend to prefer monotasking (Lascau et al., 2019). As explained in Section 5.3.1, we focus on the task-switching aspect of multitasking. In our set-up, multitasking implies that crowdworkers must interrupt their primary task (searching for specific information on Wikipedia) to work on the secondary task (providing alt-tags for images). A task design that supports monotasking behavior implies that crowdworkers finish the primary task before working on or even thinking about the secondary task. This means the secondary task should, in the best case, only appear after the crowdworker has finished the primary task. While in a real-world scenario, it would be a task for itself to automatically detect when someone has found the desired information on Wikipedia and has the capacity for a new task, our setup with the artificial main tasks facilitates this.

Altruism-based Personalization Design Furthermore, we focus on altruism as a prosocial preference that describes people's preference to consider the interests of others without having only selfish ulterior motives (Andreoni et al., 2010). As already outlined above, previous research demonstrates that the salience of the prosocial aspects of the work task and environment can affect worker behavior (Cassar, 2019). Clearly, the creation of alt-tags can be perceived as a prosocial act because it makes Wikipedia accessible for visually impaired users. Therefore, we chose to make the prosocial mission of our alt-tag bonus task more salient to appeal to people with strong altruism preferences. By changing the framing and presentation, we vary whether the prosocial mission of the crowdworkers' job is highlighted and made more salient or not. As crowdworkers differ in their altruism, there could also be a need to personalize this framing to match their altruism preference.

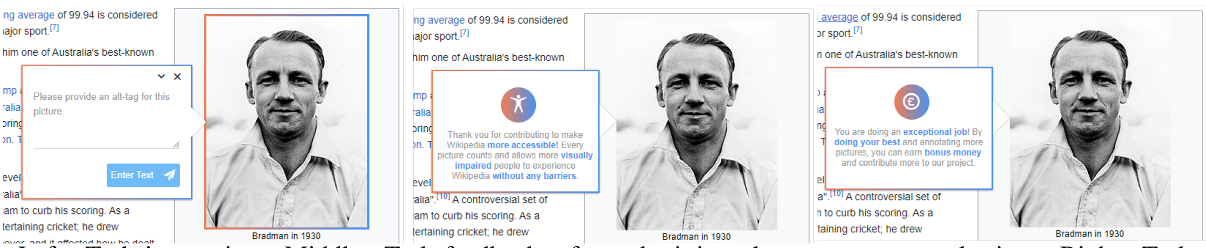
The only difference between our treatment manipulations concerning altruism is that the framing of the task differed. While the task, the payment, and the structure of events remained unchanged, we varied whether the prosocial mission of the task was salient or not. Thus, we only changed the framing and emphasized the prosocial mission or the option to earn money. In the following, we explain how we adapted our task design.

Task instructions and setup:

After agreeing to work on a task, participants are often redirected to another platform, like a separate crowdsourcing system or a survey platform. There, they receive instructions for a task, and sometimes they need to complete comprehension checks to show that they understood the instructions before they can start the task. In the altruistic design variant, the introduction emphasizes that the crowdworker is contributing to a more inclusive world and making Wikipedia more accessible so that everyone can experience it. In the selfish design variant, the focus is on the users themselves, emphasizing that they can earn money by doing an exceptional job and that their skills are needed.

Task management and feedback:

While working on the task, participants can get feedback on their performance. These feedback messages are also an element where the altruistic framing of the task could differ. We provide feedback in two ways: First, in the panel of the extension, the number of alt-tags (altruistic) (see Figure 5.1 (4)) or the bonus money earned (selfish) is displayed to give feedback on the amount of bonus work participants already did. Second, after submitting an alt-tag, the design variants display different messages. While in the altruistic design variant, the message thanks the user for helping to make Wikipedia more accessible, the selfish design variant tells the user that they are doing an exceptional job and are earning extra money. We ensure that both the introduction and the feedback messages are of similar length in both systems. The messages are displayed in Figure 5.2. In Table C.1 in Appendix C.1, we provide an overview of the exact differences between the messages.



Left: Task instruction. Middle: Task feedback, after submitting alt-tags to present altruism. Right: Task feedback, after submitting alt-tags to present selfishness.

Figure 5.2: Screenshot of the pop-ups of our casual microtasking system

5.4 Experimental Studies

5.4.1 Study Setup

We conducted both experimental studies using the same procedure and measurements, and, therefore, use this section to present our hypotheses and introduce the general study setup.

Hypotheses

Our main hypothesis is that a better fit between crowdworker preferences and the casual microtasking system will result in increased job performance.[2] Thereby, we will assess whether the instantiation of our proposed designs for preference-based personalization of casual microtasking systems fulfills their purpose, i.e., to increase the performance of crowdworkers who interact with a system that fits their individual preferences, like polychronicity and altruism. As outlined above, we argue that the positive effect on crowdworkers' performance results from the microtask matching crowdworkers' preferences. Also, other studies found positive effects of systems that account for a higher P-J fit (Edwards et al., 1998). Chilton et al. (2005) showed that a better fit between software developers' cognitive styles and the demands of the job leads to less strain and better job outcomes. Thus, there is empirical evidence of the positive effects of systems accounting for P-J fit on the performance of workers. In the context of accessibility feedback, we are specifically interested in positively impacting crowdworkers' behavior to contribute a large number and a higher quality of alt-tags. Drawing on existing research, we argue that personalized casual microtasking systems will positively affect the job performance of crowdworkers. We articulate the following four hypotheses:[3]

First, we argue that casual microtasking systems that are designed to support crowdworkers in performing microtasks in parallel to their primary tasks lead to higher job performance (REQ1). We articulate the following first hypothesis:

[2] We preregistered the study, including the study design and the hypotheses at https://aspredicted.org/tg7c-4tkg.pdf.

[3] In accordance with our pre-registration, our primary focus is on Hypotheses 2 and 4. Hypotheses 1 and 3 were not part of our pre-registration as they emerged later in the research process. While these hypotheses were not included in the pre-registered framework, we present them here to offer a more comprehensive analysis of our study's findings.

Hypothesis 1: Crowdworkers who interact with a multitasking casual microtasking system achieve higher job performance than crowdworkers who interact with a monotasking casual microtasking system.

At the same time, however, individual preferences of crowdworkers regarding their polychronicity must be taken into account in the design of the system (REQ2). Therefore, we articulate a second hypothesis:

Hypothesis 2: Crowdworkers who interact with a preference-based personalized casual microtasking system that fits their individual polychronicity preference achieve higher job performance than crowdworkers who interact with a system that does not fit their polychronicity preference.

Another important design dimension of casual microtasking systems is to encourage crowdworkers to care not only about themselves but also about the well-being and profit of others (REQ3). On this basis, we articulate a third hypothesis:

Hypothesis 3: Crowdworkers who interact with a casual microtasking system highlighting altruistic motives achieve higher job performance than crowdworkers who interact with a casual microtasking system highlighting selfish motives.

Finally, we argue that considering prosocial preferences of crowdworkers should be taken into account in the design of the system (REQ4). Therefore, we articulate the following hypothesis:

Hypothesis 4: Crowdworkers who interact with an altruism-personalized casual microtasking system that fits their individual altruism preference achieve higher job performance than crowdworkers who interact with a system that does not fit their altruism preference.

Procedure

We conducted two separate studies via the platform Prolific, one for each preference. In study 1, we investigate the polychronicity-based design variants, while in study 2, we analyze the altruism-based design variants. Besides the different *MyCrowdSurfer* designs, both studies follow the same procedure as depicted in Figure 5.3. Our two studies were conducted as longitudinal field studies with three parts: pre-screening, main task, and post-task questionnaire. In all three parts, we used LimeSurvey for the instructions and the survey. The first part was a pre-screening survey in which participants answered questions about demographics, social preferences (incl. altruism), and polychronicity preferences. We also asked the participants about their English level according to the Common European Framework of Reference for Languages (CEFR), nationality, and primary browser. We then analyzed the results and excluded participants who did not fit our predefined requirements regarding their primary browser (Google Chrome), nationality (US, UK, South Africa), and English level (B2 or higher).

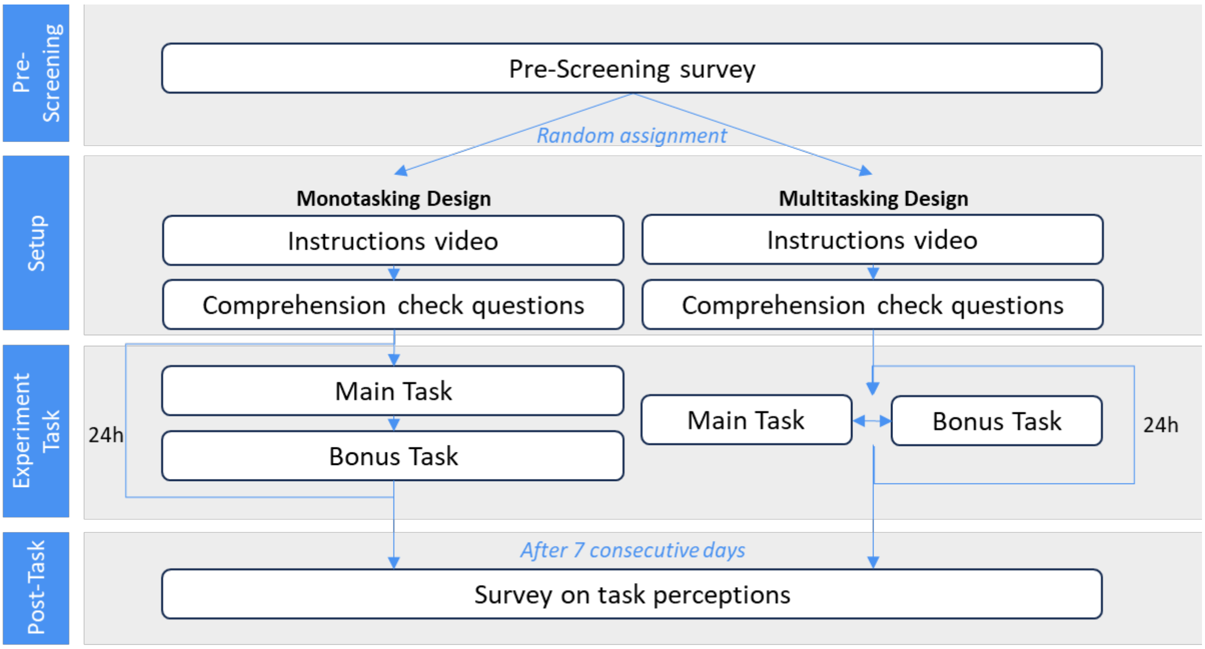


Figure 5.3: Study procedure for the monotasking design variant (left) and the multitasking design variant (right).

We analyzed the data with respect to the participants' polychronicity preferences (study 1) or altruism preferences (study 2). We only included participants with strong preferences in our study. For participants with no strong preference, we did not expect a significant effect in a personalization treatment. Therefore, we excluded participants who scored between the 40% and 60% quantile of the standardized polychronicity (study 1) or altruism (study 2) preference scores. The remaining participants were randomly assigned to the multitasking or monotasking design variant (study 1) or the altruistic or selfish design variant (study 2). By doing so, we received a 2x2 matrix for each study. We could distinguish our participants by low or high preference scores for polychronicity (study 1) or altruism (study 2) and a fit or no fit between their preference and the *MyCrowdSurfer* design variants (see Figure 5.4). For study 1, we used the selfish frames in both treatments, as they represent the baseline design. For study 2, we used the multitasking design variant as this is how casual microtasks are typically integrated into the daily internet surfing of crowdworkers (see Figure C.1 in Appendix C.1). The second part is the main part of our study, in which participants had to use the extension over seven consecutive days. The experimental task is explained in more detail in the following section. After seven days, the participants were invited to complete our post-task questionnaire. In this questionnaire, they were asked about their perceptions of the *MyCrowdSurfer* design in general, and the main task and the alt-tag task separately.

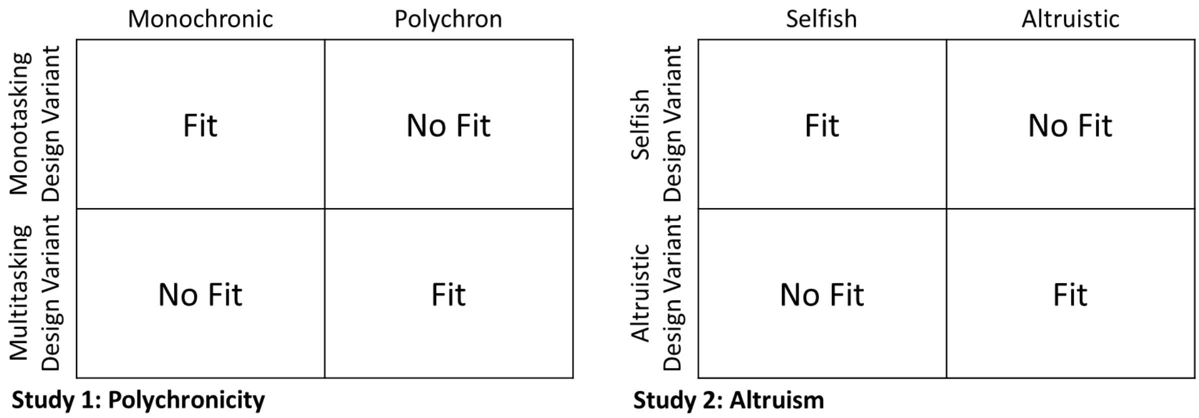


Figure 5.4: 2x2 matrices for study 1 (left) and study 2 (right)

Experimental Task

The experimental task description instructed participants to use the Chrome extension for seven consecutive days. On each day, participants received a new main task via the extension that led them to a specific Wikipedia page. On these websites, the alt-tag bonus tasks were then either available immediately (multitasking design variant) or only after submitting the main tasks (monotasking design variant). The minimum requirements to successfully complete the second part of the study were to install the extension, complete the setup including watching a video with the instructions for the tasks, submit six of the seven main tasks, and have at least 50% of answers correct. For the seven tasks, we chose topics on which we expected the participants to have similar prior knowledge and which are not related to one of the nationalities of our participants (US, UK, South Africa). We also avoided topics that some participants might be emotional about, like politics, sports clubs, or celebrities. Finally, we aimed to cover different types of images like photos, graphs, charts, illustrations, and pages with a varying number of images. Therefore, the topics for our tasks were *Adidas*, *United Nations*, *Palomino*, *Don Bradman*, *Brazilian Carnival*, *Tyrol* and *Marketing Strategy*. Each participant received the seven tasks in a random order. Each task consisted of three questions. For the first question, participants must search the answer on the Wikipedia page and copy the respective sentence.

Thereby, we could ensure that participants used the English Wikipedia page to answer the questions and see the available alt-tag tasks instead of using other websites and missing the alt-tag tasks. The second question asked them to summarize a paragraph or compare two aspects that were presented in the article in three to four sentences. Thereby, we wanted the participants to engage with the content of the Wikipedia article to understand the context of the images. The last question was a multiple-choice question with three answer options. With all three types of tasks, we wanted to mimic a real interaction with Wikipedia as it could happen when participants search for something on Wikipedia in real life.

Measures

Controls, Attention, and Comprehension Checks In the pre-screening, we collected demographic variables such as gender, age, and education, and included one attention check. In the main task, we included one comprehension check in which participants had to answer six questions about their task. We included three to four comprehension checks and two to three attention checks in the third part.[4] These checks ensured that our participants were attentive while participating in our study.

Preferences For measuring participants' polychronicity, we applied the 14-item Multitasking Preference Inventory (MPI) (Poposki & Oswald, 2010). Compared to other measures for polychronicity, this inventory measures the polychronicity on the individual level and not the cultural level. For measuring participants' social preferences, including altruism, we relied on the established Global Preference Survey (Falk et al., 2018, 2023). For altruism, participants had to answer two questions, one on a scale from 0 to 10 and one where they had to enter a value between \$0 and \$1600. A list with all items is attached in Appendix C.1 (Table C.2).

Manipulation Checks The post-task questionnaire contained two manipulation check questions measured on a seven-point Likert scale. They tested whether our design variants resulted in the desired effects. On a seven-point Likert scale ranging from 1 (strongly disagree) to 7 (strongly agree), participants were asked (1) whether "the microtask served an altruistic goal" to assess altruism and (2) whether "[the participant] had to handle multiple tasks at once" to

[4] As preregistered, participants who failed the attention check in the first part were excluded from the analysis. We also excluded participants who failed two or more attention checks in the third part.

assess polychronicity.

Dependent Variables Our main dependent variable is the job performance. We measured the job performance in two steps. First, we analyzed the number of provided alt-tags per participant and called this construct *quantity*. As all participants received the same payment for the alt-tag bonus task and all alt-tag submissions were completely voluntary, more provided alt-tags mean a higher job performance. For a deeper analysis, we also consider the alt-tags' quality (length and relevance). We define the construct *quantity*$_{adjusted}$ as follows:

$$\text{quantity}_{adjusted} = \text{quantity} \times \text{quality} \tag{5.1}$$

For each participant, we multiplied the quantity with a distinct quality factor. This quality factor for the participant is the average of the quality score for all alt-tags that the participant provided and is defined as follows:

$$\text{quality} = \frac{\sum(\text{relevance}_{normalized} \times 0.66 + \text{length}_{normalized} \times 0.33)}{\text{quantity}} \tag{5.2}$$

The quality of an alt-tag is defined by adding the relevance of the alt-tags and their length with different weights. We argue that both the relevance and the length of the alt-tag are indicators of crowdworkers' job performance, but we assume the relevance is twice as important as the length.[5] A very short alt-tag, for example for a logo, might be better than a very long alt-tag for the same logo. However, providing longer alt-tags shows more effort and also means that participants decided to work on more complex images that require more text. To assess each alt-tag's relevance, we applied the scale of Williams et al. (2022) that provides four categories for alt-tags. As in their scale, zero means that no alt-tag was provided, and we do not need to include this category in our assessment. Consequently, all alt-tags will receive scores from one to four depending on their relevance and specificity. To allow for an unbiased assessment, we used ChatGPT (3.5) to assess each alt-tag. To do so, we provided ChatGPT the image, the Wikipedia page, the textual context of the image on the Wikipedia page, and a definition for each category. Finally, we needed to normalize the resulting relevance score so that 0.25 means

[5] We tested the robustness of this assumption by computing the quality and thus also the *quantity*$_{adjusted}$ based on two alternative assumptions: (1) relevance and length are equally important, and (2) length is twice as important as relevance. In both cases, the results presented in Section 5.4 remain qualitatively unchanged and are available upon request.

the alt-tag is of very low relevance for the image, and 1 means that the alt-tag is very relevant and includes all necessary information. We did this by dividing the resulting scores by four.

To normalize the length of our alt-tags, we first counted the number of words for each alt-tag. We determined the 0.99 quantile of the maximum number of words for one alt-tag to exclude outliers. We then calculated the normalized length for each alt-tag by dividing the number of words by the number of words of the 0.99 quantile.

Recruitment

We recruited our participants on Prolific. Prolific is known to have many part-time crowdworkers for whom casual microtasking might be more convenient than for full-time crowdworkers (Oppenlaender et al., 2020). We recruited participants from the UK, US, and South Africa in similar proportions to get a diverse set of altruism preferences (Falk et al., 2018, 2023). We assumed a loss of 20% due to our exclusion criteria. We also excluded an additional 20% of participants who achieved average scores for polychronicity (study 1) or altruism (study 2) as we only included participants with scores below or equal to the 40% quantile or above or equal to the 60% quantile. Further, we assumed an attrition rate of 20% of participants during or after the main task as they conducted too few main tasks, decided not to participate in the post-task questionnaire, or failed attention checks. As we aimed for a minimum of 120 complete participants in each study, we decided to recruit 250 participants per study in the beginning. As we had more drop-outs during the study than expected, we recruited another 120 participants later. We treated them identically to our first 250 participants and used the same cut-off values to determine their preferences.[6] Participants received £1.20 for participating in the pre-screening. When successfully completing the main task, consisting of submitting six of the seven tasks and having at least 50% of answers correct, they received a base payment of £6.50. They additionally received £0.10 for each correct answer in the main task as an incentive to answer the questions thoughtfully. For the alt-tag bonus tasks, they also received £0.10 per high-quality alt-tag. For participating in the post-task questionnaire, the crowdworkers received an additional bonus payment of £1.50.

[6]In both studies, we recruited all participants over two waves. We calculated the 40% and 60% quantiles of the samples from the first wave and used these quantile values to characterize the second wave as well. These same cut-off values ensured that all participants were characterized using the same quantile values. However, the true 40% and 60% values of the second wave sample were not used, and thus, the number of participants we characterized as monochrons and polychrons in study 1 and as selfish and altruistic in study 2 differs slightly.

5.4.2 Study 1: The Impact of Polychronicity-based Personalization

Pre-Screening We invited 370 participants to the pre-screening for study 1. We excluded 63 participants due to our predefined criteria (attention checks, nationalities, primary browser, English level). Additionally, we had to exclude one participant who did not enter a valid Prolific ID. We used the remaining 306 participants to standardize the mean polychronicity scores. Additionally, we added all single scores (Poposki & Oswald, 2010). Our participants covered almost the full range of possible answers for polychronicity (14 - 66) and showed, with a mean of 38.91 and 43.10% of participants tending towards polychronicity (53.45% tending towards preferring monotasking), similar characteristics as other samples (Lascau et al., 2019). We characterized the 140 participants with polychronicity scores below or equal to the 40% quantile as monochrons and the 115 participants with polychronicity scores above or equal to the 60% quantile as polychrons.[7] 51 participants were excluded from the next steps because their polychronicity scores were between the 40% and 60% quantile. The remaining 255 participants were invited to the next step, the main task.

Sample characteristics Out of the 255 participants invited to the main task, 139 did not fulfill the minimum requirements to successfully complete the second part of the study. These participants did not start the second task, did not complete the setup, or submitted less than six of the seven main tasks.[8] This leaves us with responses from 116 participants for our analysis (52% female, M_{age} = 35.2), including 51 in the *No Fit* treatment and 65 in the *Fit* treatment. 61 participants were part of the monotasking design variant, and 51 were part of the multitasking design variant. We provide more detailed sample characteristics about participants' demographics and polychronicity in Tables C.3, C.4, C.5, and C.6 in Appendix C.1. Except for gender between *NoFit* and *Fit*, the demographics and preferences of participants did not differ significantly between *Fit* and *No Fit* nor between our two design variants in study 1.

Manipulation Check We conducted a manipulation check to evaluate the effectiveness of our two design variants (multitasking and monotasking). On a seven-point Likert scale, we asked crowdworkers whether they *"had to handle multiple tasks at once"*. Crowdworkers who inter-

[7] The number of participants we characterized as monochrons and polychrons differs slightly as we used the same cut-off (40% and 60% quantile) values in both waves.

[8] No participant failed two or more attention checks in the third part.

acted with the multitasking design variant responded on average with 3.91, and crowdworkers who interacted with the monotasking design variant responded on average with 3.80. This difference is not statistically significant (two-sided Mann-Whitney U test, $p = 0.734$). Based on the survey question, the manipulation was not salient enough to be perceived as significantly different.

Results

General Effects of Polychronicity-based Design Variants on Crowdworkers' Performance
To test Hypothesis 1, we use non-parametric tests and regressions to compare crowdworkers' performance in the alt-tag bonus task (*quantity* and $quantity_{adjusted}$) depending on whether crowdworkers interacted with the monotasking or multitasking design variant. We use an indicator variable *Multitasking Design*, which is one if crowdworkers interacted with the multitasking design variant and zero if crowdworkers interacted with the monotasking design variant. Comparing the quantitative performance between the two design variants, we find that *quantity*, thus the average number of submitted alt-tags per participant, increased by 45.51% from 25.18 in the monotasking design variant to 36.64 in the multitasking design variant. The difference is statistically significant (see Table 5.2). When including the quality of the performance, taking into account the length and relevance of the alt-tags, we find similar results and, again, the difference is statistically significant.

	(1) *Monotasking Design*	(2) *Multitasking Design*	(1) vs (2)
quantity	25.18	36.64	$p = 0.022$
$quantity_{adjusted}$	13.07	19.01	$p = 0.038$
	(1) *No Fit*	(2) *Fit*	(1) vs (2)
quantity	31.55	29.88	$p = 0.782$
$quantity_{adjusted}$	15.51	16.17	$p = 0.987$

The last column report results from pairwise statistical comparison based on two-sided Mann-Whitney U tests.

Table 5.2: Summary statistics and pairwise treatment comparisons for study 1

A series of OLS regressions[9] reported in Table 5.3 and Table 5.4 support the initial impression that there is at least a significant difference at the 10% level in job performance between the

[9] In Model (2), (4), and (6), we control for gender, age, and nationality. Two participants answered "non-binary" when asked about their gender. In the regressions, non-female participants include all male participants and two non-binary participants.

design variants. Models (1) and (2) list the main effects of *Multitasking Design*, and all coefficients are positive and significant at the 10% level. Thus, our findings reveal that crowdworkers had a higher job performance when they interacted with the multitasking design variant compared to the monotasking design variant.

Dep. Var.: *quantity*	(1)	(2)	(3)	(4)	(5)	(6)
Multitasking Design	11.456*	11.888*			2.254	2.593
	(6.370)	(6.356)			(9.967)	(10.283)
Fit			-1.672	-2.354	-8.844	-9.325
			(6.487)	(6.513)	(8.809)	(8.923)
Fit × Multitasking Design					16.397	16.631
					(13.103)	(13.843)
Constant	25.180***	35.765**	31.549***	40.912***	30.400***	41.470**
	(4.115)	(15.020)	(4.930)	(15.135)	(7.489)	(16.183)
Demographics	✗	✓	✗	✓	✗	✓
R^2	0.028	0.043	0.001	0.014	0.042	0.057
Observations	116	116	116	116	116	116

Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. For the complete table with all coefficients, see Table C.7 in Appendix C.1.

Table 5.3: Study 1: OLS regressions with *quantity* as dependent variable

Individual Preference-dependent Effects of Polychronicty-based Personalization on Crowdworkers' Performance Next, we compare crowdworkers' performance in the bonus task depending on whether they interacted with a design variant that fitted their individual polychronicity or not, to test our Hypothesis 2. We use an indicator variable *Fit*, which is one if crowdworkers interacted with a design variant that fitted their individual polychronicity and zero otherwise. Comparing the quantitative performance between *Fit* and *No Fit*, we find that *quantity* was 29.88 in the *Fit* treatments and 31.55 in the *No Fit* treatments, and the difference is not statistically significant (see Table 5.2). When taking the quality of the performance ($quantity_{adjusted}$) into account, the results are similar, and the difference is not statistically significant either.

The OLS regressions reported in Table 5.3 and Table 5.4 support the initial impression that there is no significant difference in job performance between *Fit* and *No Fit*. Models (3) and (4) list the main effects of our *Fit* treatments, and the coefficients are not significant.

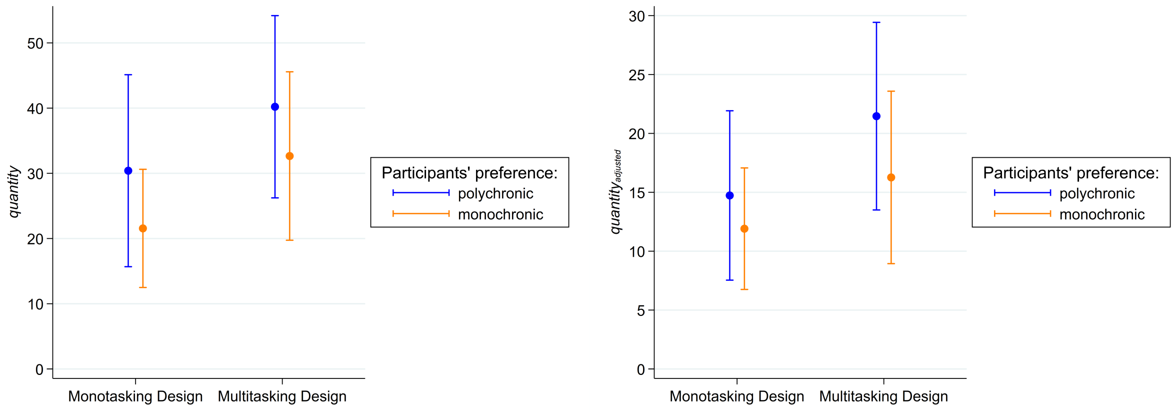
Additional Analyses We find that job performance in the bonus task increased when crowdworkers interacted with the multitasking design variant, but did not differ depending on the

Dep. Var.: $quantity_{adjusted}$	(1)	(2)	(3)	(4)	(5)	(6)
Multitasking Design	5.939*	5.934*			1.532	2.031
	(3.509)	(3.480)			(5.223)	(5.453)
Fit			0.660	-0.115	-2.821	-3.023
			(3.522)	(3.514)	(4.512)	(4.688)
Fit × Multitasking Design					8.022	7.100
					(7.125)	(7.511)
Constant	13.066***	12.765	15.512***	14.997*	14.731***	14.872
	(2.151)	(8.984)	(2.592)	(8.892)	(3.658)	(9.288)
Demographics	✗	✓	✗	✓	✗	✓
R^2	0.025	0.039	0.000	0.014	0.037	0.047
Observations	116	116	116	116	116	116

Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. For the complete table with all coefficients, see Table C.8 in Appendix C.1.

Table 5.4: Study 1: OLS regressions with $quantity_{adjusted}$ as dependent variable

personalization considering polychronicity preferences. To better understand our results, we conducted additional analyses to assess the crowdworkers' performance dependent on their preference (polychronic vs. monochronic) (see Figure 5.5a and 5.5b). In both design variants, we observe that polychronic crowdworkers tended to have a higher performance for *quantity* and $quantity_{adjusted}$ compared to monochronic crowdworkers. For example, *quantity* increased by 23.15% for polychronic crowdworkers (*Fit*) compared to monochronic crowdworkers (*No Fit*) in the multitasking design variant. However, a two-sided Mann-Whitney U test shows that this difference is not statistically significant ($p = 0.679$). Our regression results in Table 5.3 Model (5) support this impression. A two-sided F-test of the linear hypothesis *Multitasking Design + Fit + Fit × Multitasking Design = Multitasking Design* also shows that the difference is not statistically significant ($p = 0.438$). Considering only the monotasking design variant, *quantity* increased by 41.00% for polychronic crowdworkers (*No Fit*) compared to monochronic crowdworkers (*Fit*). Again, a two-sided Mann-Whitney U test ($p = 0.716$) and a two-sided F-test of the linear hypothesis *Fit* = 0 (Table 5.3 Model (5)) show that the difference is not statistically significant ($p = 0.318$). The results are similar if we compare $quantity_{adjusted}$. Furthermore, Figure 5.5a and 5.5b suggest that both monochronic and polychronic crowdworkers had a higher performance in the multitasking compared to the monotasking design variant. Similar to the performance increases in Table 5.2, *quantity* increased by 32.27% for polychronic crowdworkers and by 51.44% for monochronic crowdworkers in the multitasking compared to the monotasking design variant. Likewise, $quantity_{adjusted}$ increased by 45.69% for polychronic crowdworkers and by 36.52% for monochronic crowdworkers. However, two-sided Mann-Whitney U tests and two-sided F-tests of our regression results in Table 5.3 and Table 5.4 reveal that these differences are not significantly different.



(a) *quantity* (b) $quantity_{adjusted}$

Note: Dots indicate means and whiskers indicate 95% confidence intervals.

Figure 5.5: Study 1: *quantity* and $quantity_{adjusted}$ over Monotasking vs. Multitasking design variant and polychronic vs. monochronic crowdworkers

5.4.3 Study 2: The Impact of Altruism-based Personalization

Pre-Screening We also invited 370 participants to the pre-screening for study 2. We excluded 60 participants due to our predefined criteria (attention checks, nationalities, primary browser, and English level). We used the remaining 310 participants to standardize and weigh the altruism scores according to Falk et al. (2023). We characterize the 131 participants with altruism scores below or equal to the 40% quantile as selfish and the 125 participants with altruism scores above or equal to the 60% quantile as altruistic.[10] 54 participants were excluded from the next steps because their altruism scores were between the 40% and 60% quantile. The remaining 256 participants were invited to the next step, the main task.

Sample Characteristics Out of the 256 participants invited to the main task of the second study, 154 did not fulfill the minimum requirements to successfully complete the second part of the study. These participants did not start the second task, did not complete the setup, or submitted less than six of the seven tasks.[11] This leaves us with responses from 102 participants for our analysis (51% female, M_{age} = 33.5), including 51 in the *No Fit* treatments and 51 in the

[10]The number of participants we characterized as selfish and altruistic differs slightly as we used the same cut-off values (40% and 60% quantile) in both waves.

[11]No participant failed two or more attention checks in the third part.

Fit treatments. 55 participants were part of the selfish design variant, and 47 were part of the altruistic design variant. We provide more detailed sample characteristics about participants' demographics and altruism preference in Tables C.9, C.10, C.11, and C.12 in Appendix C.1. Similar to study 1, except for gender between the altruistic and the selfish design variants, the demographics and preferences of participants did not differ significantly between *Fit* and *No Fit* nor between our two design variants in study 2.

Manipulation Check We conducted a manipulation check to evaluate the effectiveness of our two design variants. On a seven-point Likert scale, we asked participants whether "the microtask served an altruistic goal". Crowdworkers who interacted with the altruistic design variant responded on average with 5.55, and crowdworkers who interacted with the selfish design variant responded on average with 5.15. This difference is statistically significant at the 10% level (two-sided Mann-Whitney U test, $p = 0.074$), and thus, our experimental manipulation tended to be perceived as intended.

Results

General Effects of Altruism-based Design Variants on Crowdworkers' Performance To test Hypothesis 3, we proceed similarly to the analysis of Hypothesis 1. We compare crowdworkers' performance in the alt-tag bonus task (*quantity* and *quantity*$_{adjusted}$) depending on whether crowdworkers interacted with the altruistic or selfish design variant. We use an indicator variable *Altruistic Design*, which is one if crowdworkers interacted with the altruistic design variant and zero if crowdworkers interacted with the altruistic design variant. Comparing the quantitative performance of the two design variants, we find that *quantity* even decreased by 46.81% from 35.93 in the selfish design variant to 19.11 in the altruistic design variant. The difference is not statistically significant (see Table 5.5). On average, *quantity*$_{adjusted}$ decreased by 43.95% from 18.50 in the selfish design variant to 10.37 in the altruistic design variant and the difference is statistically significant at the 10% level. Thus, the results do not support our hypothesis and even reveal that there is a tendency that crowdworkers had a higher performance in the selfish design variant when the focus of the instructions was more on earning money.

We conducted a series of OLS regressions reported in Table 5.6 and Table 5.7 to further analyze the performance between our two altruism-based design variants. Models (1) and (2) list the main effects of *Altruistic Design* with and without controlling for gender, age, and nationality.

	(1) *Altruistic Design*	(2) *Selfish Design*	(1) vs (2)
quantity	19.11	35.93	$p = 0.135$
$quantity_{adjusted}$	10.37	18.50	$p = 0.092$
	(1) *No Fit*	(2) *Fit*	(1) vs (2)
quantity	30.59	25.76	$p = 0.663$
$quantity_{adjusted}$	15.80	13.71	$p = 0.611$

The last column report results from pairwise statistical comparison based on two-sided Mann-Whitney U tests.

Table 5.5: Summary statistics and pairwise treatment comparisons for study 2

All four coefficients for *quantity* and $quantity_{adjusted}$ are negative and significant ($p < 0.05$). The results suggest that crowdworkers had a significantly lower job performance in the bonus task when they interacted with the altruistic compared to the selfish design variant.

Dep. Var.: *quantity*	(1)	(2)	(3)	(4)	(5)	(6)
Altruistic Design	-16.821***	-13.731**			-14.624*	-7.998
	(5.994)	(5.889)			(8.499)	(9.392)
Fit			-4.824	-3.030	-2.050	2.602
			(6.447)	(6.330)	(10.541)	(9.661)
Fit x Altruistic Design					-3.902	-11.534
					(12.160)	(13.231)
Constant	35.927***	28.738**	30.588***	19.271*	36.897***	25.470*
	(5.195)	(11.416)	(4.565)	(10.861)	(7.017)	(13.421)
Demographics	✗	✓	✗	✓	✗	✓
R^2	0.067	0.194	0.006	0.155	0.072	0.202
Observations	102	102	102	102	102	102

Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. For the complete table with all coefficients, see Table C.13 in Appendix C.1.

Table 5.6: Study 2: OLS regressions with *quantity* as dependent variable

Individual Preference-dependent Effects of Altruism-based Personalization on Crowdworkers' Performance To reveal whether altruism-based personalization affected the crowdworkers' job performance, similar to study 1, we use an indicator variable *Fit*, which is one if crowdworkers interacted with a design variant that fitted their individual altruism preference and zero otherwise. We compare the job performance in the bonus task depending on the variable *Fit*. On average, *quantity* even decreased by 15.79% from 30.59 in the *No Fit* treatments to 25.76 in the *Fit* treatments. The difference is not statistically significant (see Table 5.5). We also find no significant differences when including the quality of the performance.

The OLS regressions reported in Table 5.6 and 5.7 support this impression. The coefficients

Dep. Var.: $quantity_{adjusted}$	(1)	(2)	(3)	(4)	(5)	(6)
Altruistic Design	-8.127**	-6.902**			-4.894	-1.703
	(3.236)	(3.423)			(4.481)	(4.959)
Fit			-2.091	-1.372	1.244	3.540
			(3.426)	(3.399)	(5.463)	(5.324)
Fit × Altruistic Design					-6.216	-10.567
					(6.597)	(7.455)
Constant	18.501***	15.776**	15.802***	10.963*	17.913***	12.406*
	(2.669)	(6.732)	(2.289)	(6.471)	(3.294)	(7.432)
Demographics	✗	✓	✗	✓	✗	✓
R^2	0.056	0.137	0.004	0.102	0.066	0.158
Observations	102	102	102	102	102	102

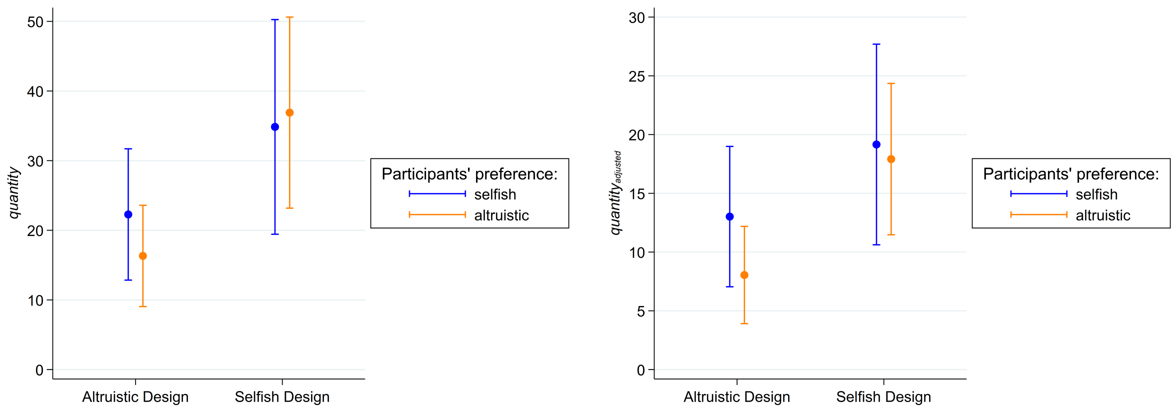
Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. For the complete table with all coefficients, see Table C.14 in Appendix C.1.

Table 5.7: Study 2: OLS regressions with $quantity_{adjusted}$ as dependent variable

for *Fit* in Models (1) and (2) are negative and not significant. Thus, the results reveal that crowdworkers with a fit between their used design variant and altruism preference had no higher performance in the bonus task.

Additional Analyses Similar to study 1, performance in the bonus task differed depending on the used design variant. However, altruism-based personalization did not have a statistically significant effect on job performance and even tended to lower crowdworkers' job performance. Again, we conducted additional analyses to investigate crowdworkers' job performance dependent on their altruism preference (selfish vs. altruistic). First, Figure 5.6a and 5.6b indicate that both altruistic and selfish crowdworkers had a lower job performance in the altruistic compared to the selfish design variant. Considering only selfish crowdworkers, performance decreased by 36.10%, and $quantity_{adjusted}$ decreased by 32.05% in the altruistic compared to the selfish design variant. Two-sided Mann-Whitney U tests and two-sided F-tests of our regression results in Table 5.6 and 5.7 do not show that these differences are statistically significant.

In the next step, we examine altruistic crowdworkers in more detail. *quantity* decreased by 55.77% from 36.90 in the selfish design variant (*No Fit*) to 16.32 in the altruistic design variant (*Fit*). Similarly, $quantity_{adjusted}$ decreased by 55.05% from 17.91 in the selfish design variant (*No Fit*) to 8.05 in the altruistic design variant (*Fit*). Thus, if we consider only the crowdworkers with strong altruism preferences, the job performance in the *Fit* treatment decreased by more than 50% compared to the *No Fit* treatment. Two-sided Mann-Whitney U tests indicate that the difference for *quantity* ($p = 0.193$) and $quantity_{adjusted}$ ($p = 0.115$) are not statistically



(a) *quantity* (b) $quantity_{adjusted}$

Note: Dots indicate means and whiskers indicate 95% confidence intervals.

Figure 5.6: Study 2: *quantity* and $quantity_{adjusted}$ over Altruistic vs. Selfish design variants and selfish vs. altruistic participants

significant. However, the regression results in Table 5.6 and 5.7 suggest that the differences are statistically significant. Using Model (5) and (6) in Table 5.6, two-sided F-tests of the linear hypothesis *Altruistic Design* + *Fit* + *Fit* × *Altruistic Design* = 0 indicate that *quantity* significantly decreased in the altruistic compared to the selfish design variant (Model (5), $p = 0.011$; Model (6), $p = 0.032$). The same applies if we adjust the job performance for quality in Table 5.7 Model (5) and (6). Two-sided F-tests of the linear hypothesis *Altruistic Design* + *Fit* + *Fit* × *Altruistic Design* = 0 for $quantity_{adjusted}$ also reveal that the differences in job performance are significant (Model (5), $p = 0.013$; Model (6), $p = 0.033$). Therefore, the results suggest that for crowdworkers with strong altruism preferences, altruism-based personalization tended to backfire by decreasing the job performance in the bonus task.

5.5 Discussion

Personalized microtask designs that adapt to crowdworkers' preferences are an innovative approach to impact crowdworkers' job performance. While research on personalized crowdworking in IS is rather scarce, there are promising results in other contexts like web personalization for affecting consumer behavior (Ho & Bodoff, 2014; Ho et al., 2011; Zhang et al., 2011). Also, personalizing task assignments and recommendations in crowdworking and personalization according to cognitive abilities have been explored. However, there is a research gap on personalization according to crowdworkers' preferences that go beyond their interests for specific topics or their preference for particular types of tasks. Against this backdrop, we first designed a personalized casual microtasking system following the DSR paradigm. We applied the P-E fit theory as our kernel theory to derive requirements and justify our design variants. In the second step, we investigated the impact of preference-based personalization on job performance in casual microtasking systems in the context of collecting alt-tags on Wikipedia. In two field studies, we analyzed the impact of preference-based designs and personalization according to the crowdworkers' polychronicity (study 1) and altruism (study 2). In the following, we discuss the implications of our results for theory, design, and practice.

5.5.1 Theoretical Contributions

First, our research contributes to the descriptive knowledge base by establishing an evidence-based connection between personalized task designs and crowdworkers' job performance. With our longitudinal field study, we extend existing research that investigated the effect of P-J fit on job performance in traditional work settings and research on personalized crowdsourcing. Furthermore, we contribute to the literature on how different task designs impact performance.

In our first study, we show that a multitasking design variant led to higher job performance. However, polychronicity-based personalization did not increase job performance. We assume this is caused by additional factors besides personalization that impact job performance and wash out the effect of the personalization. For example, in the multitasking design variant, participants had more time to provide alt-tags and were more flexible and autonomous. They could submit alt-tags before, during, and after completing the main task, while in the monotasking treatment, participants could only provide alt-tags after completing the main task. Also, some research suggests that the effect of polychronicity on job performance depends on the P-E fit (König & Waller, 2010), while other studies show that polychronicity in general is positively related to job performance (Kantrowitz et al., 2012). This could explain why we do not see a significant difference between participants in the *Fit* and *No Fit* treatment and polychrons providing slightly more alt-tags than monochrons (see Figure 5.5).

Our second study shows that an altruistic compared to a selfish design variant backfired, leading to lower job performance. Similar to study 1, we do not observe significant effects of altruism-based personalization on the outcome. Given that the crowdworking labor market is highly anonymous, the workers might concentrate on the pure transactional value of the contract. Thus, their focus lies more on the monetary compensation than on the requester's motives or the task's nature. If the requester makes the altruistic component more salient, this might be perceived as a signal that the requester wants to trick workers into working hard for a low amount of money. As Fest et al. (2021) have shown, positive framing of a job description can backfire in online labor markets. Interestingly, while selfish crowdworkers performed similarly in both design variants, altruistic crowdworkers provided significantly fewer alt-tags when using the altruistic design. Our manipulation checks show that our designs work, and participants thought that the altruistic design served more of an altruistic goal than the selfish design. Although the altruistic goal was provided externally by us as the task requester, we assume that workers internalized the altruistic motive (referred to as internalized extrinsic motivation (Ryan & Deci, 2000)) and were, therefore, both intrinsically and extrinsically motivated to provide alt-tags. However, the interplay of intrinsic and extrinsic motivators (in our case bonus payments) is complex. The introduction of monetary incentives has the potential to strengthen, but also to reduce intrinsic motivation, as explained by the Motivation Crowding Theory (Frey & Jegen, 2001). Monetary incentives can be counterproductive for prosocial activities due to the image motivation being crowded out. In a public scenario, individuals would not have the feeling of being perceived positively by others for their prosocial behavior due to the additional financial incentive (Ariely et al., 2009). Also, they are concerned about seeming to be greedy instead of prosocial (Exley, 2017). Monetary benefits for prosocial behavior can also create doubt about the true motive of the altruistic activity and make individuals lose their intrinsic motivation. This is called the "overjustification effect" (Bénabou & Tirole, 2006). Although the core driver for the overjustification effect is social reputation that can only suffer in a public scenario, the effect can also happen in a private setting where individuals then start questioning their own motives, leading to a reduced intrinsic motivation (Bénabou & Tirole, 2006). This effect is also related to the "tainted altruism" phenomenon that refers to situations where altruistic acts are perceived as having selfish motivations (Newman & Cain, 2014). In our case, the monetary reward makes the act of providing alt-tags less pure as it is mixed with self-interest. Additionally, as shown by Cassar and Meier (2021), our altruistic participants may have seen the prosocial incentive as a strategic move by us in the experimental design rather than a genuine act of kindness, resulting in the altruistic design backfiring.

5.5.2 Design Contributions

We contribute four design requirements and an artifact instantiation to the prescriptive knowledge base. We complement prior research to improve job performance in crowdsourcing tasks based on specific design variants and personalization (Pagano & Bruegge, 2013). We rigorously analyzed the design variants in a longitudinal field study to demonstrate the feasibility of personalized casual microtasking.

Our theory-driven design requirements, exploiting the P-E fit theory, provide context-specific design knowledge to guide the design of preference-based personalization in casual microtasking. We show that personalization according to polychronicity (REQ2) does not improve overall job performance. In our polychronicity-based design variant (REQ1), we show how the design of casual microtasking can also respect the preferences of monochrons, not forcing them into a multitasking behavior as it would happen in a traditional task design (Haug et al., 2023). However, participants had a higher job performance when our casual microtasking system supported multitasking. Both requirements provide high generalizability to other crowdworking tasks. While the polychronicity-based personalization of the timing of feedback tasks is rather specific to casual microtasking, personalized task recommendations according to polychronicity scores for other, more traditional crowdworking tasks might be a fruitful avenue for future research. While some crowdworking tasks require participants to switch between tabs, like verifying information, others require more focus, like transcriptions. Consequently, some tasks might fit better to polychrons while others fit the preferences of monochrons.

For our third and fourth requirements, we focused on altruism as a social preference, as this fit the context of our task. Of course, the requirements could also be implemented by addressing other social preferences or task types. Based on our results regarding personalization according to altruism (REQ4), we provide evidence that only for altruistic participants altruism-based personalization can lead to contrary effects. In addition, regarding altruism-based design variants (REQ3), we observed a backfiring of a positive altruistic frame, even though the underlying monetary incentive structure did not change. In accordance with existing research (Nieken, 2023; Wu & Quinn, 2017), we demonstrate that changing the wording in task instructions and task management can impact task outcomes. Thus, we recommend that task requesters should be cautious when designing tasks and combining intrinsic and extrinsic motivators in crowdworking.

5.5.3 Practical Contributions

Our study supports practitioners with actionable knowledge to improve distinct aspects of job performance in crowdworking. Low-quality crowdworking results are a persistent problem for crowdworking platforms. While past research often focused on quality measures or the effects of varying financial incentives in crowd work (Daniel et al., 2018; Ho et al., 2015), we followed another approach, accounting for and leveraging the individual differences of crowdworkers. Also, existing research on personalized crowdsourcing mainly focused on task assignments, not adapting the tasks themselves. Therefore, our research makes two practical contributions to personalized crowdworking.

First, our proposed design guides how to drive job performance by utilizing different personalization mechanisms and design variants. Practitioners and researchers can use this design knowledge for their task design. They can build upon our results to make a grounded decision for or against preference-based personalization in their tasks. Moreover, they can also use the design knowledge to inform their task design, even if deciding against personalization. Our first design variants demonstrate how to instantiate crowdworking tasks that require multitasking or monotasking behavior. Our second design variant shows practitioners how to display or hide a task's goal or prosocial component. We contribute three general components of crowdworking tasks that can be adapted to display different motives. This design can easily be transferred to all kinds of crowdworking tasks. However, as our results show, personalization according to altruism must be treated carefully. It is important to understand that appealing to the participant's social preferences might also result in lower performance. Hence, the framing of the task is crucial and the potential effects need to be taken into account. More generally, our study shows that the interplay of intrinsic and extrinsic motivators in crowd work is complex. It is not always better to show crowdworkers that they can make an impact with their submissions. Moreover, related research also raised concerns regarding the ethical implications of introducing intrinsic motivation to crowdworking tasks (Law et al., 2016). Additional intrinsic motivations might lead to crowdworkers unknowingly contributing more without getting paid more. However, as our study shows, this is not always the case. When task requesters still decide to add an intrinsic motivator, they need to ensure a fair balance between monetary rewards and performance.

Second, we contribute to practice with two artifacts that provide exemplary design variants for two different types of preference-based personalization of casual microtasking. These artifacts exemplify how practitioners and researchers can apply the proposed design knowledge in real-world crowdworking contexts. We demonstrate the application of these systems in two real-world casual microtasking scenarios using the crowdworking platform Prolific. In doing so, we address potential reasons for the low job performance of crowdworkers in casual microtasking. This enables practitioners to successfully build further personalized casual microtasking systems and learn from our results. Therefore, our research is highly relevant to practice since it enables a better understanding of personalized task designs in crowdworking as an approach to increase the job performance of crowdworkers.

5.5.4 Limitations and Future Research Opportunities

Our work comes with limitations that provide opportunities for future research. First, for our study, we needed to introduce an artificial main task to replace a real task. This main task was necessary to distinguish between a monotasking and multitasking design by knowing when participants finished their primary task without having to rely on behavioral data like click data, which is difficult to obtain in a field study. In the multitasking design variant, participants could provide alt-tags before, during, or after answering the questions of the main task about the Wikipedia page. However, in the monotasking design variant, participants could only work on the alt-tags after they submitted their answers for the main task. Thus, they were forced into monotasking. Future research could investigate features that allow for a monotasking design in a real-world scenario.

Second, we only investigated personalization according to one social preference, namely altruism. While this is a well-researched preference that also fits our task context, there are more social preferences, and we cannot necessarily transfer our results to all other social preferences. Each social preference, such as fairness or reciprocity, might have specific effects and must be addressed differently. Consequently, our study only serves as a starting point to investigate more social preferences in future studies.

Third, we expect a potential self-selection bias in our studies. From the beginning, we explained to participants that they would need to install a Google Chrome extension to participate in the task to minimize costly dropouts. Participants who were unwilling to do so, as they do not prefer such kinds of tasks or have data privacy concerns, might have refrained from participating. There is no way in research to completely avoid self-selection bias. Also, in a real-world scenario, we would experience the same selection bias as crowdworkers who did not want to install browser extensions would not participate in casual microtasks on Prolific. Therefore, we argue that our results are still generalizable to all potential users of casual microtasking systems.

Fourth, we conducted a rigorous field study, using an innovative artifact. We decided on a field study to guarantee the empirical validity of our results. Although the artifact was extensively tested before the study, minor technical issues might have existed. As we expect these issues not to be specific to one design variant or one group of participants, we suppose they would not affect our results. Future research could investigate the detected effects in a more controlled setting, such as a lab study. This would allow for a deeper understanding of the robustness and generalizability of our results.

5.6 Conclusion

Our research addresses important challenges of improving the job performance of crowdworkers in casual microtasking. We propose a theory-driven design for a preference-based personalization of casual microtasking systems by building on the P-E fit theory and instantiating the four derived requirements in a software artifact. Our empirical results reveal that while designs allowing crowdworkers to multitask improved performance compared to sequential monotasking, preference-based personalization had no positive effect. We demonstrate that the altruism-based design variant can negatively affect job performance. Our study contributes a nuanced understanding and highlights the need to carefully select preference-based personalization design variants, as personalization can also backfire. Thus, we contribute to the descriptive knowledge base by providing insights into the complex effects of preference-based personalization on job performance in casual microtasking.

6 Discussion*

6.1 Summary and Contribution

This thesis investigated how communication, particularly the use of different communication channels, affects behavior in virtual work. In light of the continued expansion of remote and hybrid work arrangements and the growing relevance of online crowdworking platforms, the topic has become increasingly important in academic research and practice. The dissertation contributes to economics and IS research by providing experimental insights into behavioral dynamics. Four RQs were introduced and outlined in Section 1.2 and addressed through findings derived from four distinct studies. First, a literature review summarized existing research on whether the impact of communication is affected when shifting from in-person to virtual communication and whether using different virtual communication channels leads to different behavioral results. Building on this foundation, two controlled experimental studies were conducted to investigate whether people prefer text or video messages when lying and how nonverbal cues in virtual communication impact team coordination and cooperation. Finally, a third study contributed to the literature on personalization in crowdwork by examining whether preference-based task designs can enhance performance in crowdworking settings. In the following, the contribution of each study to the outlined RQs is discussed.

Study I presented a literature review synthesizing findings from 21 experimental studies. Given the wide range of experimental settings that were used to examine the impact of using different communication channels (see Table 2.1), insights were structured according to four categories: (i) two-way communication in interdependent strategic interactions, (ii) two-way communication in creativity tasks, (iii) nonverbal information in one-way communication, and (iv) honesty and promises in one-way communication. The review showed that outcomes can vary across different settings in one-way and two-way communication. In two-way communication transitioning from face-to-face to virtual communication can negatively affect cooperation (Bochet et al., 2006; Brosig et al., 2003; Frohlich & Oppenheimer, 1998; Rockmann & Northcraft, 2008), reciprocity (Bicchieri et al., 2010; Lev-On et al., 2010), and creativity (Brucks & Levav, 2022; Grözinger et al., 2020; Grund et al., 2025). However, richer virtual communication chan-

*This chapter is based on the following studies: Haug et al. (n.d.), Hörrmann et al. (n.d.), Nieken and Walther (2024), and Walther (2025).

nels, such as video, can mitigate some of these negative effects (Brosig et al., 2003; Grözinger et al., 2020). In one-way communication, conveying messages via richer channels, such as video, is not always superior to text messages (Nieken, 2023; Zylbersztejn et al., 2024b), which demonstrates the complex interaction of verbal and nonverbal communication. These results contribute to the literature on communication and underscore the importance of carefully considering the choice of communication channel in experimental designs, as this could unintentionally impact the outcomes. This can also be relevant when comparing results from existing research if different communication channels are utilized in the studies (e.g., as demonstrated in Balliet (2010)). Last, the review suggests that while the current experimental findings are an important first step, they do not yet provide a comprehensive understanding of the mechanisms that drive differences when using various communication channels. Possible reasons that make investigating mechanisms challenging are that communication channels can differ in several characteristics, such as media richness or social presence. Furthermore, variations in communication content can influence the experimental results if not controlled for. However, recent advances, such as eye-tracking or communication content analysis, offer promising tools for uncovering causal relationships in future research.

Study II addressed whether people prefer to send text or video messages when lying and provided new insights into the role of video communication and human presence in dishonesty. Our experimental findings revealed that senders were significantly more likely to choose text rather than video messages when being dishonest compared to when being honest. A potential underlying mechanism for this result is that senders perceived a significantly lower human presence in text compared to video messages. The insights advance the theoretical understanding of moral behavior by highlighting the importance of perceived human presence as a factor influencing dishonesty (Cohn et al., 2022). Moreover, our findings demonstrated how different degrees of human presence emerge in virtual communication, suggesting that disclosing face and voice in video communication can significantly increase perceived human presence compared to text communication. Thus, individuals may choose communication channels with lower perceived human presence to minimize their lying costs. In addition to the human presence, we also showed that subjects experienced significantly greater social image concerns in video compared to text messages. While these findings contribute to studies suggesting that social image concerns increase honesty (Abeler et al., 2014; Dufwenberg & Dufwenberg, 2018; Gneezy et al., 2018; Khalmetski & Sliwka, 2019), in our experimental setup, these concerns did not significantly influence the choice of channel. Furthermore, this dissertation extends the existing literature on how different communication channels impact dishonesty. While previous studies (Abeler et al., 2014; Cohn et al., 2022; Conrads & Lotz, 2015) identified no significant differences between text and audio communication regarding honesty, our results underscore the critical role of video communication in preventing dishonest behavior, emphasizing the need for further research on the impact of human presence and video communication on dishonesty. Beyond dishonesty, the results of the *Choice* treatment revealed that some senders chose video messages to signal honesty when given the choice of lying or telling the truth. However, since the receivers' follow decisions did not differ significantly between text and video messages in this treatment, the senders' intended signals did not play a crucial role for the receivers.

Study III employed a two-part controlled online experiment to address RQ3, investigating whether enriching virtual pre-play communication with nonverbal cues enhances team coordination and cooperation. Drawing on a practical example (turning on cameras or inserting profile pictures into virtual meetings), our experimental design expands existing research by outlining insights on the effects of nonverbal cues in game-unrelated communication on team outcomes. Results indicated that coordination in the first round of the WLG was unaffected by the presence or type of nonverbal cues. However, enriching virtual communication with nonverbal cues affected cooperation in the first round. Incorporating static nonverbal cues in the form of selfies decreased cooperation by approx. 17% compared to audio-only and video communication. Dynamic nonverbal cues via video resulted in cooperation levels similar to those observed with audio-only communication. We could not identify a clear mechanism for why subjects' selfies hampered cooperation due to endogeneity issues. However, group cohesion and human presence decreased similarly to cooperation in the *Photo* treatment compared to the *Audio* and *Video* treatment and might explain some of our treatment differences. Moreover, further analyses revealed that an increase in the length of communication (*Character Count*) during the virtual meeting increased cooperation within teams. Since turned-on cameras in the *Video* treatment encouraged teams to talk more, this could explain some of the differences in cooperation between the *Video* and *Photo* treatment. We contribute to research investigating how communication, especially nonverbal communication, affects coordination and cooperation (see e.g. Avoyan & Ramos, 2023; Chen & Chen, 2011; Cooper et al., 1992; He et al., 2017). Furthermore, our findings extend the literature on the use of different communication channels. For game-related communication, previous studies have emphasized the use of richer communication channels to enhance cooperation (Bochet et al., 2006; Frohlich & Oppenheimer, 1998). Furthermore, Brosig et al. (2003) observed significantly higher cooperation for video compared to audio communication. We expand this research by showing that video communication is no longer superior when communication is not game-related. With our *Photo* treatment, we contribute to research investigating how identification and anonymity affect economic behavior. In contrast to existing research emphasizing positive effects of identification on trustworthiness (Eckel & Petrie, 2011) or fairness concerns (Charness & Gneezy, 2008), in our study, visual identification via selfies had no positive impact on coordination and even decreased cooperation in our experimental setting. In addition, our results showed that visual identification during a virtual meeting hampered the development of group cohesion, an important factor for cooperation (Gächter et al., 2025), and reduced perceived human presence, which can facilitate dishonest behavior (Cohn et al., 2022).

In **Study IV**, two large-scale experimental online studies on Prolific addressed RQ4 and answered whether preference-based personalized casual microtasking systems affect crowdworker performance. Our experimental setting provided new insights into how task designs can be personalized according to the polychronic and altruistic preferences of crowdworkers and whether this approach enhances performance. The results showed that personalizing the task design according to polychronicity by varying whether or not the crowdworker could multitask had no significant effect on crowdworker performance. We also found no positive impact when personalizing the communication content of instructions by emphasizing the financial or altruistic goal to a greater or lesser extent to address altruistic preferences. We even found that performance decreased for altruism-based personalization when the altruistic goal of the task was more salient for crowdworkers with higher altruistic preferences. Our results contribute to the growing literature on crowdwork and the design of casual microtasking systems. The results of both studies suggest that the task design can significantly affect performance, extending the findings of Paulino et al. (2022), and underscoring the crucial role of task design in crowdwork. Furthermore, we advance research on personalization in crowdwork (Amer-Yahia et al., 2016; Difallah et al., 2013; Paulino et al., 2022, 2023a) and point out that personalization is not always beneficial and can even be counterproductive in some cases. Our findings on altruism-based personalization underscore the importance of carefully designing instructions, which is in line with previous work (Nieken, 2023; Wu & Quinn, 2017). Furthermore, our insights contribute to a large body of research on motivation crowding theory (Frey & Jegen, 2001) and extend studies that examine the nuanced relation between intrinsic and extrinsic incentives (Ariely et al., 2009; Bénabou & Tirole, 2006; Cassar & Meier, 2021; Exley, 2017; Fest et al., 2021).

6.2 Practical Implications

The empirical insights derived from the literature review and experimental studies offer valuable implications for practitioners and decision-makers in virtual work. First, this thesis outlines implications regarding the ongoing discourse on remote and hybrid arrangements. While employees recognize possible benefits and limitations of work-from-home, tensions persist between employee preferences and employer expectations regarding the frequency of work-from-home days. Furthermore, CEOs of large companies continue to express differing views on the ideal balance between office-based, hybrid, and fully remote work arrangements (Barrero et al., 2021). Empirical evidence from **Studies I–III** contributes to this debate by examining whether the shift from face-to-face to virtual communication impacts communication effectiveness and whether such effects depend on the virtual communication channel used. Thereafter, insights from **Study IV** provide guidance for the personalization of tasks when working on crowdworking platforms.

In two-way communication situations, insights from **Study I** underscore the importance of using appropriate communication channels. While research has shown that virtual communication is not necessarily disadvantageous compared to in-person communication, for example, when it comes to building trust (Abatayo et al., 2020; Bicchieri et al., 2010; Lev-On et al., 2010), the use of less rich channels, such as text, can have some negative effects in situations where cooperation or reciprocity is crucial (Bicchieri et al., 2010; Bochet et al., 2006; Brosig et al., 2003; Frohlich & Oppenheimer, 1998; Lev-On et al., 2010). Therefore, this thesis advises practitioners not to underestimate the choice of a communication channel, as it can significantly impact workers' behavior in different settings. In fully remote work, where employees completely rely on virtual communication, using rich communication channels in situations where communication is not just about exchanging information should be actively encouraged, as text channels can impair the effectiveness of communication. However, even in hybrid work arrangements, richer communication channels are emphasized, as research indicates that even when employees work in the office, they increasingly use virtual communication channels instead of meeting in person (Bloom et al., 2022). In line with current research (Bloom et al., 2022, 2024; Chatterjee et al., 2022), this thesis highlights the importance of hybrid work arrangements. These arrangements combine the work flexibility of remote work with the possibility of collaborating face-to-face. Findings from **Study I** emphasize the value of face-to-face communication, as the studies summarized in the literature review did not reveal any significant disadvantages of face-to-face communication compared to virtual communication in terms of behavioral outcomes. Moreover, the review points out innovation-oriented work as one workplace setting where face-to-face communication can offer significant benefits compared to virtual communication. Although video communication could significantly outperform text chats in a visual team creativity task (Grözinger et al., 2020), it was less effective in generating ideas than face-to-face communication (Brucks & Levav, 2022; Grund et al., 2025). Hence, practitioners and decision-makers should encourage employees in hybrid work to schedule dedicated days at the office. During these days, in-person meetings should be prioritized to leverage the benefits of face-to-face communication rather than relying on virtual communication when employees are in the same location.

Study III offers more distinct implications for two-way communication in newly formed teams who get to know each other in a virtual meeting. In such contexts, it is recommended that practitioners highlight either audio-only or video communication rather than audio communication combined with static profile pictures. Our study found that enriching audio communication with profile pictures, which allow for identification within the team, reduced cooperation in the subsequent task. These results indicate some risks of identification when teams virtually meet for the first time. In particular, in settings with high demographic diversity, profile pictures may draw more attention to demographic characteristics such as age, gender, and ethnicity, possibly increasing perceived differences within the team. Our results showed that the increased awareness of diversity within teams via subjects' selfies significantly impacted team cooperation in our setting, while *Homophily* had no significant impact on cooperation in the *Audio* and *Video* treatment. This finding is particularly relevant for organizations with diverse teams, as it suggests that the visual emphasis on demographic diversity during early team interactions may hinder cooperation the more diverse the teams are. These effects can be mitigated when profile pictures are removed or team members interact via live video, where dynamic interaction may reduce the salience of static demographic cues. Accordingly, organizations should be careful about the identifiability of team members in virtual meetings, especially when teams are newly formed.

In contrast to two-way communication, the implications for one-way communication, such as written or recorded messages, vary. One-way communication is commonly used in research and organizations, for example, to provide instructions in experimental studies or to motivate employees through announcements. A prominent example is the New Year's address by the German Federal Chancellor, which is publicly available as a written text (Scholz, 2024b) and as a video message on YouTube (Scholz, 2024a). Moreover, video messages are increasingly used in everyday situations, e.g., as video clips in MS Teams (Microsoft, 2025) or as video notes in WhatsApp (WhatsApp, 2025), emphasizing the use of different channels in one-way communication. Unlike text messages, video messages contain not only verbal but also paraverbal and nonverbal information, and the interplay between these different aspects of communication is complex. **Study I** revealed that more information in communication does not necessarily lead to better outcomes. Particularly in persuasive contexts, such as when it comes to enhancing motivation or prosocial behavior, additional nonverbal information can sometimes be counterproductive (Nieken, 2023; Zylbersztejn et al., 2024b). Practitioners should carefully consider the alignment and balance of verbal and nonverbal information to ensure that the message is coherent (Nieken, 2023). In some cases, simpler text or audio messages may be more effective than more complex video messages.

Other real-life contexts where one-way communication is relevant are situations in which honest reporting is crucial, such as insurance claims or job candidate selection (Conrads & Lotz, 2015). While résumés are traditionally sent in written form, the emergence of video résumés represents a new approach in job search (Indeed, 2025; McKinley, 2023; TikTok, 2021), emphasizing the importance of honesty in messages across different virtual communication channels. **Study I** and **Study II** provide some practical implications in this regard. Previous research found no significant differences in honesty between audio and text communication (Abeler et al., 2014; Cohn et al., 2022; Conrads & Lotz, 2015), suggesting that adding the voice alone might not be effective in fostering honest behavior. However, the presence of visual cues, such as the ability to see others during communication, has been shown to play a crucial role in reducing dishonest behavior (Conrads & Lotz, 2015), likely due to an increased perception of human presence. Therefore, in situations where face-to-face interactions are not feasible or too costly, promoting or requiring video communication can be an effective strategy to prevent dishonesty. Beyond that, the results from **Study II** suggest that, especially in situations with high information asymmetry, the communication channel can be more than just a way to share information. It can also be a potential signal. Some subjects in our experiment perceived video messages as more credible, trustworthy, or honest, and chose this channel to show that they were sincere. Therefore, practitioners should know that the choice of communication channel itself can convey information about the sender's intentions.

Given the increasing relevance of crowdworking platforms such as Prolific and MTurk (Douglas et al., 2023; Tomczak et al., 2023), the findings from **Study IV** offer practical implications regarding preference-based personalization in crowdworking contexts. Researchers and practitioners can use our findings to design tasks personalized to individual preferences, such as polychronicity and altruism. Our insights further support the development of personalized task designs according to other crowdworkers' preferences. The results of our field studies help researchers to assess the opportunities and challenges of personalization in order to decide whether or not to adopt it. Specifically, we found that personalization based on polychronicity did not negatively affect crowdworkers' performance. In contrast, caution is advised when personalizing based on altruism, since highlighting the prosocial goals of a task can unintentionally lead to a reduction in performance for crowdworkers with strong altruistic preferences. Consequently, this study underscores the complex interaction between intrinsic and extrinsic incentives in crowdworking and suggests that combining both incentives is not always beneficial.

6.3 Limitations and Future Research

Although the studies presented in this dissertation were carefully designed and conducted according to scientific standards, certain limitations must be taken into account. The explicit acknowledgment and discussion of these limitations ensures transparency and strengthens the overall credibility of the research. Furthermore, identifying these limitations provides a basis for pointing out promising directions for future research.

Causality and Mechanisms Although **Study II-IV** provided valuable insights into using different communication channels and preference-based personalization in virtual work, they did not fully uncover the underlying mechanisms that may be driving these effects. In **Study II**, we observed that senders preferred text messages over video messages when being dishonest, identifying lower perceived human presence as a potential mechanism. However, we could only establish correlation rather than causality as we examined subjects' behavior within one treatment (*Choice*). Moreover, the *Choice* treatment was designed so that senders had to make two decisions: whether to lie and whether to send a text or video message. However, after the first few rounds, the senders were likely familiar with the procedure, making it difficult to distinguish whether they had first decided to lie and then to choose the communication channel, or vice versa. That ambiguity concerning the order of decisions complicates causal inferences. Future studies employing between-subject designs, where subjects cannot choose their communication channel, could help to draw causal conclusions about whether text messages facilitate dishonesty. Furthermore, text and video messages differ in several aspects, such as perceived human presence, anonymity, and the availability of paraverbal and nonverbal cues, suggesting that mechanisms other than perceived human presence may also explain our findings. Future research can disentangle these aspects by, for example, incorporating text messages accompanied by facial images to isolate the role of anonymity.

In **Study III**, we measured group cohesion and perceived human presence after the team decision stage (ten rounds of the WLG). While the results of Gächter et al. (2025) suggest that the timing of these measurements, before or after the team decision stage, is not crucial, our data showed that our experimental treatment variations significantly influenced group cohesion and perceived human presence. This raised endogeneity issues that limited the appropriateness of these variables for the regression analyses presented in Table 4.1 and complicated our efforts to analyze the underlying mechanisms. We considered using our treatments as an instrumental variable within a two-stage least squares estimation to address these endogeneity issues. However, valid instruments must satisfy the exclusion restriction. They must influence the dependent variable only through the endogenous variable, not directly or through alternative channels (see e.g. Angrist & Pischke, 2009; Sajons, 2020; Wooldridge, 2016). However, the treatments significantly influenced cooperation in the WLG and significantly impacted group cohesion, perceived human presence, and the communication length (*Character Count*). As a result, the exclusion restriction was not satisfied, making our treatments unsuitable as an instrument. While this underscores the need for further research, particularly because the availability of nonverbal cues and the use of different communication channels can have various intended and unintended effects, it also highlights the underlying complexity of uncovering mechanisms.

Therefore, this thesis emphasizes that, for future research, carefully designed studies are essential to address these challenges.

In **Study IV**, our results showed that altruism-based personalization can involve risks, as demonstrated by a decline in performance among crowdworkers with higher altruistic preferences when the altruistic goal of the task was emphasized. We discussed possible explanations for this result concerning the interaction between intrinsic and extrinsic incentives, as described in the motivation crowding theory (Frey & Jegen, 2001). However, we could not draw any clear conclusions about the specific mechanisms responsible for the negative effect of personalization on the performance of altruistic crowdworkers. Since the monetary incentives remained constant in our experimental setup, future studies could systematically vary the salience of an altruistic goal and the amount of financial incentives. This approach would allow for a more comprehensive investigation of the interactions between intrinsic and extrinsic motivational factors in crowdwork.

Generalizability Our literature review in **Study I** focused on experimental studies within the field of economics and included only studies that either used standard economic paradigms or in which subjects' behavior was financially incentivized. This ensured that the results of the selected studies were comparable. However, other studies across different research fields were not included due to the criteria. Still, they might provide some interesting findings on the use of different communication channels (see e.g. Dennis & Kinney, 1998; Mangus et al., 2024; Sheffield, 1995; Sprecher, 2014; Valley et al., 1998). This limits the generalizability of the insights gained from **Study I** and underscores the need for a more comprehensive review across different research fields.

In **Study II**, we aimed to investigate subjects' channel choices, rather than gender-related differences. Given existing evidence highlighting gender differences in dishonest behavior (see Capraro, 2018; Gerlach et al., 2019, for meta-analyses), we limited our sample exclusively to male subjects. Consequently, the generalizability of our findings is limited to male subjects. Future research should, therefore, extend our experimental design to include female and non-binary subjects to achieve a more nuanced understanding of preferences for different communication channels. Moreover, our experimental design involved a payment structure in which senders' payoffs were independent of receivers' decisions (Gneezy et al., 2013). Thus, our findings cannot necessarily be generalized to dishonest behavior in other contexts. Alternative experimental paradigms, such as those proposed by Gneezy (2005), where senders have monetary incentives to persuade receivers of their lies successfully, may yield different results regarding subjects' preferences for various communication channels. Investigating such variations in incentive structures represents a promising direction for future research.

In **Study III,** we utilized a diverse subject pool recruited via Prolific, which allowed us to get insights across age groups, genders, and ethnicities. This diversity strengthens the generalizability of our experimental findings. Our study focused on newly formed teams engaging in a one-time virtual meeting and demonstrated that static nonverbal cues can decrease team cooperation. While such one-time interactions occur in practice, teams within organizations typically interact repeatedly over longer periods. Therefore, future research should employ longitudinal studies involving multiple virtual meetings to investigate how enriching virtual communication with nonverbal cues affects team outcomes when team members are familiar with each other and engage repeatedly. Furthermore, communication in our experimental setup was not directly related to the subsequent task, as subjects were only informed about the WLG after completing the virtual meeting. Another avenue for future research would be to investigate scenarios in which virtual communication, enriched with nonverbal cues, is explicitly game-related. This would help figure out the role of nonverbal cues when communication is more about promises and commitments between team members.

In **Study IV**, we focused on two preferences, polychronic and altruism, to examine how preference-based task designs can increase crowdworker performance. These insights provide important contributions and implications on personalizing task designs and whether personalization is a promising avenue for future research. However, we cannot transfer our insights to other social preferences, such as fairness or reciprocity. As social preferences are well-researched (Falk et al., 2018), our study only serves as a starting point to investigate personalization according to other social preferences in future studies.

The Role of Nonverbal Information **Studies I–III** provide valuable insights into how different communication channels, which differ in their ability to transmit paraverbal and nonverbal information, affect human behavior. However, since nonverbal communication encompasses different elements such as gestures, facial expressions, or eye contact (Driskell et al., 2003; Warkentin et al., 1997), the results could not draw any clear conclusions about which specific nonverbal information is critical in video and face-to-face communication. **Study I** pointed out that only Brucks and Levav (2022) used eye-tracking technology to compare face-to-face and video communication, highlighting the potential of advanced technologies for drawing causal conclusions. In addition, the data from **Study II** could be further evaluated by examining nonverbal information in the recorded video messages to investigate its influence on the receivers' behavior. In **Study III**, technical limitations restricted data collection to audio recordings, making it impossible to assess the influence of facial expressions such as smiles on cooperation in the *Video* treatment. Prior research indicates that nonverbal information significantly influences behavior, for example, gestures such as smiles or winks can increase trust (Manzini et al., 2009; Scharlemann et al., 2001), and mutual eye gaze can play a key role in cooperative behavior (Kurzban, 2001; Nieken & Reuscher, 2023; Reuscher, 2025). Therefore, future studies comparing richer communication channels should not neglect the investigation of nonverbal information. Such analyses can deepen our understanding of nonverbal information in communication and its influence on human behavior.

7 Conclusion

The increasing prevalence of virtual work, in which communication and interactions no longer occur in person but are mediated by technology, represents a fundamental change in the organization and execution of work. While a large number of previous studies have emphasized the importance of communication in the workplace, the increasing reliance on virtual communication channels introduces new communication-related challenges. Yet, the implications of this change are not well understood. This dissertation aimed to deepen the understanding of virtual communication by examining how the use of different communication channels affects behavior in virtual work settings.

This dissertation comprised four different studies. **Study I** presented a comprehensive literature review summarizing experimental research findings on the effects of using different communication channels on human behavior. This synthesis demonstrated that the shift from face-to-face to virtual communication can significantly affect behavior in the workplace. It also identified avenues for future research, which served as the basis for our second and third studies. **Study II** reported on a controlled experiment investigating preferences for communication channels in the context of dishonest behavior. The results revealed that video messages were preferred over text messages when subjects lied. **Study III** investigated the role of nonverbal cues in virtual team communication. A controlled online experiment showed that enriching audio communication with profile pictures negatively affected subsequent team cooperation, highlighting the nuanced effects of visual cues in team interactions. **Study IV** focused on personalization in virtual work, particularly within crowdworking platforms. An online experiment examined the effects of personalizing the task design to crowdworkers' preferences. The results demonstrated that personalization did not improve performance and even negatively affected performance when altruistic motives were emphasized.

Overall, this dissertation contributes to research on communication by emphasizing that the choice of communication channels plays a key role in workplace effectiveness and should not be neglected. Furthermore, we expand the literature on crowdwork by examining the opportunities and challenges of preference-based personalization in virtual work. The findings offer practical implications for improving communication and collaboration in virtual work settings. Finally, this study contributes to the current debate on the future of work, particularly with regard to the adoption and implications of office-based, remote, and hybrid work arrangements.

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A Appendix for Study II

A.1 Control Variables

This section describes all control variables used in the analyses in Section 3.4. Concerning demographics, *Age* measures subjects' age.[1] As the senders in Part 1 were students recruited from the KD^{2}Lab subject pool, we asked them for their current major in their studies. We grouped those in majors related to *Business and Economics*, and *Other majors*, with the latter category serving as a baseline unless otherwise mentioned. In contrast to the senders, the receivers in Part 2 were hired from Prolific. Therefore, we asked the receivers about their highest education level. The indicator variable *University Degree* is one if the receiver's highest education level is a university degree and zero otherwise. For subjects' behavior in Rounds 2 to 6, we use the variable *Round* to control for the respective round. Due to the fact that only the senders were informed about the assigned number, we use the variable *Assigned Number* to control for this. To get some qualitative data on why the senders preferred a text or a video message, we included a text field and asked the senders after Round 6 why they chose a text or video message in Round 6. For the receivers' behavior, we use the variable *Reported Number* to control for the content of the senders' messages.

For additional analyses in Section 3.5, similar to Δ *Human Presence*$_{\textit{Video - Text}}$, we elicited the senders' social image concerns. For each communication channel, we used three items in which the senders self-stated their social image concerns on a 7-point Likert scale (see Table A.1). Δ *Social Image*$_{\textit{Video - Text}}$ indicates the difference in social image concerns between video and text messages. Positive values of Δ *Social Image*$_{\textit{Video - Text}}$ indicate that the senders had higher social image concerns in video messages than in text messages. Negative values indicate that social image concerns were lower in video than in text messages.

Next, we describe all additional control variables used in the analyses in Appendix A.2. *Participation* describes an indicator variable, which is one if a subject already participated in at least one experiment and zero otherwise.[2] Remember that subjects played the standard deception game using the strategy method in Round 1. Thus, every sender had to send six messages and, thus, had six decisions to send an honest or a dishonest message. To control whether the senders' general honesty behavior explains the channel decisions in Rounds 2 to 6, the variable *Share Dishonest Round 1* refers to the share of dishonest messages, ranging from zero (only honest messages) to one (only dishonest messages). Similar to *Share Dishonest Round 1*, the variable *Share Follow Round 1* refers to the receivers' share of follow decisions in Round 1, ranging from zero (no follow decisions) to one (only follow decisions). Because the senders had in Rounds 2 to 6 the option of sending video messages, we asked them on an 11-point scale the likelihood that a receiver would recognize them from their voice or face when he saw the video message (*Recognition*). We elicited this information from the receivers to control for the unlikely event that the senders and the receivers knew each other. The indicator variable *Identification* is one if a receiver stated that they knew a sender and zero otherwise. We asked one question each using a 5-point Likert scale to measure subjects' experience with text and video communication channels. The variable *Diff. in Experience* describes the difference in experience between video and text messages. On a 6-point scale, we asked subjects how regularly they use text and video communication channels. The variable *Diff. in Usage* describes

[1] In Part 1, we asked subjects for their year of birth and "translated" these into their age. In Part 2, we elicited subjects' age using age groups.

[2] The senders were asked whether they already participated in at least one experiment at the KD^{2}Lab.

the difference in usage between video and text messages. We used the ICT Self-Concept Scale on a 6-point Likert scale according to (Schauffel et al., 2021) to assess subjects' general (*ICT General*) and communication-specific (*ICT Communication*) competence self-concept related to the use of information and communication technology. In addition, we measured subjects' perceptions about online and mobile communication (*Online Comm.*) on an 11-point scale according to ESS Round 10, 2023. We used five questions that measure different aspects of online and mobile communication (see Table A.2 for more details). To control for personality, we used the BFI-10 scale according to the five-factor model (Rammstedt et al., 2014) using a 5-point Likert scale. The BFI-10 scale classifies personality into five constructs (*Extraversion*, *Agreeableness*, *Conscientiousness*, *Neuroticism*, and *Openness*). *Trust* refers to the interpersonal trust of subjects measured with three items using a 5-point Likert scale according to Nießen et al. (2021). After the general instructions and before Round 1 and Round 2, subjects had to answer control questions on understanding the game rules. *Failed Attempts* is the sum of failed attempts across all control questions.

A.2 Tables and Figures

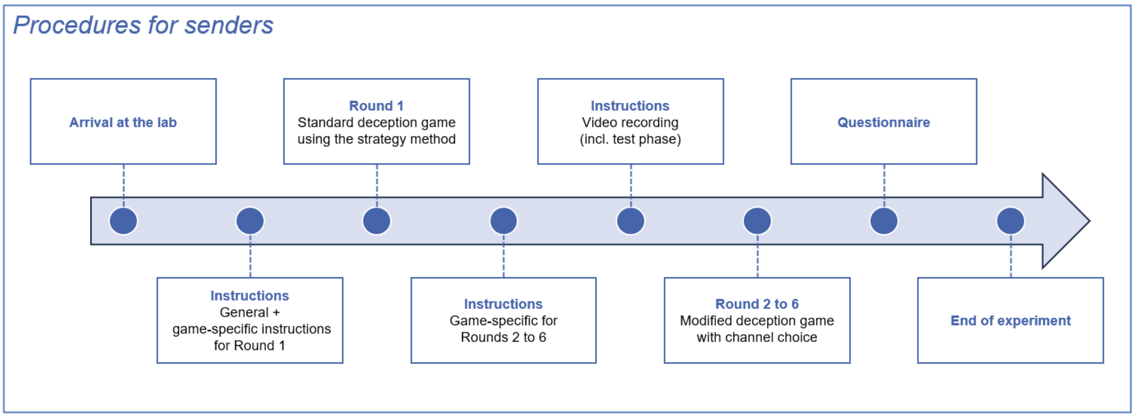


Figure A.1: Procedures for the senders in Part 1

Construct	**Items for senders (Participant A)**	**Reference**
Perceived Human Presence	I would feel [have felt] very close to Participant B. ("close" in the sense of emotional rather than physical proximity) I would feel [have felt] the presence of Participant B very strongly. I would feel [have felt] very connected to Participant B.	(Cohn et al., 2022)
Social Image Concers	I would be [have been] very concerned about what Participant B thinks about me. I would care [cared] very much about leaving a good impression on Participant B. It would be [was] very important for me that Participant B thinks I am honest.	(Cohn et al., 2022)

Note: We elicited both constructs for text and video messages. The form in brackets [ ] was used only if senders had sent at least one text message or at least one video message.

Table A.1: Overview of items to elicit perceived human presence and social image concerns.

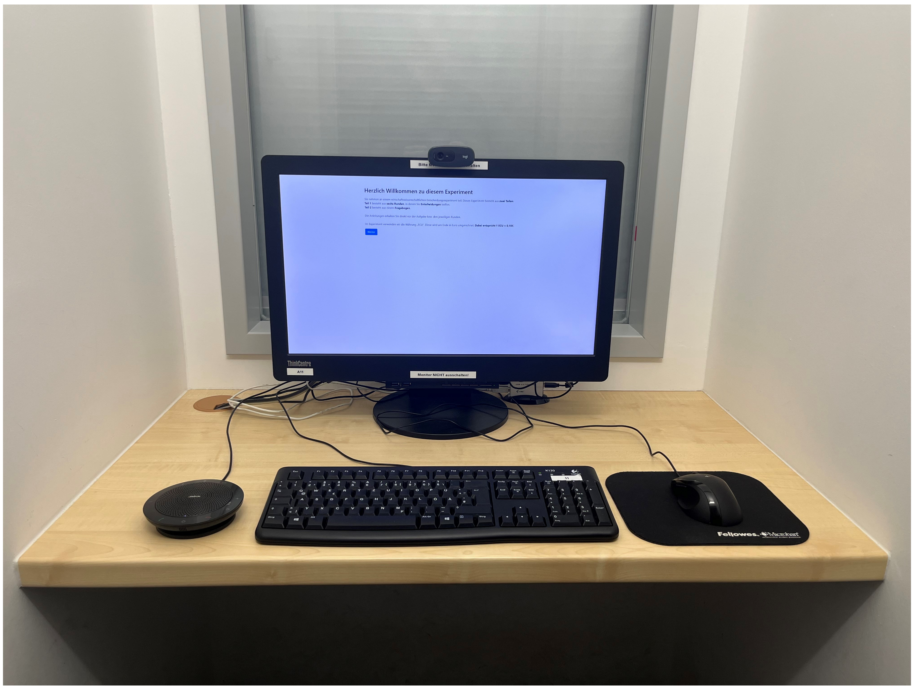


Figure A.2: Setup in each cubicle consisting of a computer, an external webcam, and an external conference speaker

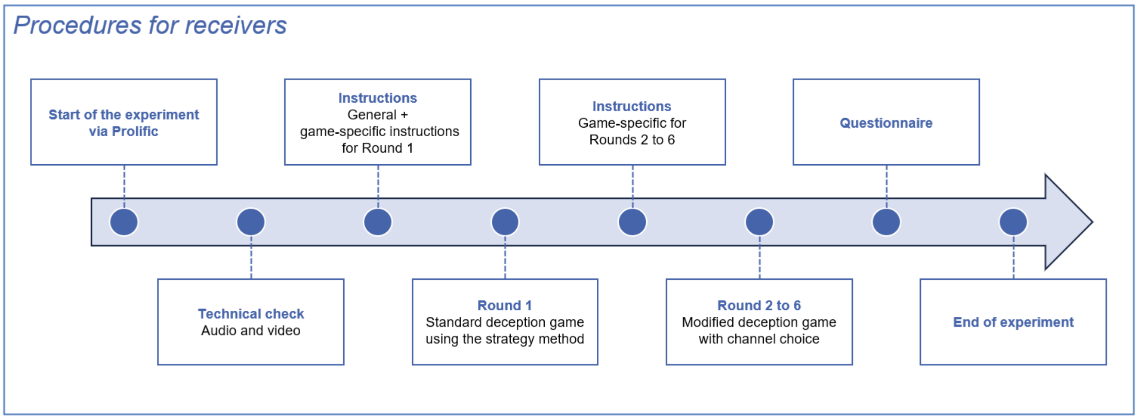


Figure A.3: Procedures for the receivers in Part 2

Measure	To what extent would you say that online and mobile communication ...
Online Comm. 1	... makes people feel closer to one another?
Online Comm. 2	... makes work and personal life interrupt each other?
Online Comm. 3	... makes it easy to coordinate and manage activities?
Online Comm. 4	... undermines personal privacy?
Online Comm. 5	... exposes people to misinformation?

Table A.2: Five questions about online and mobile communication according to ESS Round 10, 2023

	NoChoice	*Choice*	Total
Age	24.20	23.95	24.03
	(2.797)	(2.687)	(2.710)
Business and Economics	0.333	0.531	0.468
	(0.479)	(0.503)	(0.502)
Other majors	0.667	0.469	0.532
	(0.479)	(0.503)	(0.502)

Table A.3: Means of key demographics of the senders across treatments.

	NoChoice	*Choice*	Total
Age	30.35	30.15	30.21
	(8.880)	(7.902)	(8.166)
University Degree	0.519	0.483	0.494
	(0.509)	(0.504)	(0.503)
No University Degree	0.481	0.517	0.506
	(0.509)	(0.504)	(0.503)

Table A.4: Means of key demographics of the receivers across treatments.

Dep. Var.: Video	(1)	(2)	(3)	(4)	(5)
Choice Treatment	0.569	0.519	-0.005	0.128	0.113
	(0.529)	(0.536)	(0.776)	(0.768)	(0.664)
Round		-0.003	-0.003	-0.003	-0.002
		(0.059)	(0.059)	(0.059)	(0.059)
Assigned Number		0.200^{***}	0.200^{***}	0.202^{***}	0.200^{***}
		(0.072)	(0.072)	(0.072)	(0.070)
Δ Human Presence$_{\text{Video - Text}}$		-0.529^{***}	-0.848^{**}	-0.792^{**}	-0.958^{***}
		(0.183)	(0.331)	(0.330)	(0.320)
Choice Treatment $\times$ Δ Human Presence$_{\text{Video - Text}}$			0.418	0.401	0.697^{*}
			(0.380)	(0.378)	(0.391)
Age				0.087	0.088
				(0.095)	(0.087)
Business and Economics				-0.422	-0.568
				(0.511)	(0.502)
Share Dishonest Round 1					-0.655
					(0.729)
Diff. in Usage					-0.118
					(0.270)
Diff. in Experience					0.480^{*}
					(0.281)
ICT General					-0.335
					(0.425)
ICT Communication					0.304
					(0.390)
Online Comm. 1					-0.042
					(0.085)
Online Comm. 2					-0.098
					(0.102)
Online Comm. 3					0.130
					(0.173)
Online Comm. 4					0.054
					(0.106)
Online Comm. 5					-0.059
					(0.118)
Participation					-0.243
					(0.599)
Failed Attempts					-0.712^{**}
					(0.337)
Trust					0.936^{*}
					(0.509)
Extraversion					-0.014
					(0.294)
Agreeableness					-0.097
					(0.313)
Conscientiousness					-0.253
					(0.295)
Neuroticism					-0.252
					(0.309)
Openness					-0.189
					(0.282)
Constant	-1.342^{***}	-1.401^{**}	-0.977	-3.041	-3.032
	(0.480)	(0.631)	(0.767)	(2.566)	(5.424)
Observations	470	470	470	470	470
Log pseudolikelihood	-215.578	-205.492	-205.039	-204.103	-192.853
Δ *Human Presence*$_{\textit{Video - Text}}$ + *Choice Treatment* $\times$ Δ *Human Presence*$_{\textit{Video - Text}}$ = 0			0.038	0.054	0.217

Robust standard errors adjusted for 94 clusters (subjects) in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: The figures in the row "Δ *Human Presence*$_{\textit{Video - Text}}$ + *Choice Treatment* $\times$ Δ *Human Presence*$_{\textit{Video - Text}}$ = 0" are the p-values for the overall effect of Δ *Human Presence*$_{\textit{Video - Text}}$ on *Video* in the *Choice* treatment. Since Δ *Human Presence*$_{\textit{Video - Text}}$ is quasi-continuous, we used the sample mean of Δ *Human Presence*$_{\textit{Video - Text}}$ in the *Choice* treatment (1.193).

Table A.5: Panel probit regression (random effects) with *Video* as dependent variable, *Choice Treatment* as independent variable and all control variables

Dep. Var.: Video	(1)	(2)	(3)	(4)	(5)
Dishonest	-1.329***	-1.411**	-1.100*	-1.117*	-1.192*
	(0.401)	(0.581)	(0.604)	(0.608)	(0.614)
Round		-0.002	0.014	0.016	0.012
		(0.072)	(0.072)	(0.072)	(0.072)
Assigned Number		-0.032	-0.019	-0.020	-0.037
		(0.122)	(0.124)	(0.124)	(0.127)
Δ Human Presence$_{\text{Video - Text}}$		-0.505**	-0.377	-0.326	-0.291
		(0.243)	(0.308)	(0.303)	(0.304)
Dishonest $\times$ Δ Human Presence$_{\text{Video - Text}}$			-0.388	-0.391	-0.335
			(0.290)	(0.290)	(0.275)
Age				0.190	0.187
				(0.174)	(0.145)
Business and Economics				-0.285	-0.536
				(0.829)	(0.736)
Share Dishonest Round 1					0.931
					(1.206)
Diff. in Usage					-0.936*
					(0.499)
Diff. in Experience					0.362
					(0.415)
ICT General					-0.569
					(0.812)
ICT Communication					0.030
					(0.667)
Online Comm. 1					0.036
					(0.142)
Online Comm. 2					-0.039
					(0.154)
Online Comm. 3					-0.116
					(0.320)
Online Comm. 4					0.174
					(0.148)
Online Comm. 5					-0.160
					(0.149)
Participation					-0.902
					(1.015)
Failed Attempts					-0.123
					(0.441)
Trust					1.167
					(0.746)
Extraversion					0.076
					(0.445)
Agreeableness					-0.662
					(0.495)
Conscientiousness					-0.073
					(0.408)
Neuroticism					-0.561
					(0.403)
Openness					-0.275
					(0.447)
Constant	-0.340	0.402	0.126	-4.351	-1.102
	(0.467)	(0.726)	(0.772)	(4.355)	(7.566)
Observations	314	314	314	314	314
Log pseudolikelihood	-136.348	-134.276	-132.997	-131.960	-125.513
Δ Human Presence$_{\text{Video - Text}}$ + Dishonest $\times$ Δ Human Presence$_{\text{Video - Text}}$ = 0			0.020	0.024	0.043

Robust standard errors adjusted for 64 clusters (subjects) in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: The figures in the row "Δ *Human Presence*$_{\textit{Video - Text}}$ + *Dishonest* $\times$ Δ *Human Presence*$_{\textit{Video - Text}}$ = 0" are the p-values for the overall effect of Δ *Human Presence*$_{\textit{Video - Text}}$ on *Video* when the senders were dishonest. Since Δ *Human Presence*$_{\textit{Video - Text}}$ is quasi-continuous, we used the sample mean of Δ *Human Presence*$_{\textit{Video - Text}}$ in the *Choice* treatment (1.193). We excluded six observations due to downward lying ($r < s$).

Table A.6: Panel probit regression (random effects) with *Video* as dependent variable, *Dishonest* as independent variable and all control variables

Dep. Var.: Follow	(1)	(2)	(3)	(4)
Video Message	0.041	0.018	0.015	0.015
	(0.175)	(0.180)	(0.179)	(0.183)
Round		0.068	0.068	0.069
		(0.052)	(0.052)	(0.052)
Reported Number		-0.201**	-0.203**	-0.207**
		(0.095)	(0.095)	(0.092)
Age			-0.018	-0.002
			(0.020)	(0.024)
University Degree			-0.143	-0.273
			(0.338)	(0.309)
Δ Human Presence$_{\text{Video - Text}}$				-0.108
				(0.128)
Share Follow Round 1				2.510***
				(0.620)
Diff. in Usage				0.186
				(0.151)
Diff. in Experience				-0.299*
				(0.182)
ICT General				-0.290
				(0.449)
ICT Communication				0.742*
				(0.388)
Online Comm. 1				0.256***
				(0.072)
Online Comm. 2				0.047
				(0.079)
Online Comm. 3				-0.111
				(0.096)
Online Comm. 4				0.042
				(0.080)
Online Comm. 5				0.161
				(0.102)
Participation				0.293
				(0.288)
Failed Attempts				0.036
				(0.141)
Trust				0.483**
				(0.244)
Extraversion				0.184
				(0.215)
Agreeableness				-0.414**
				(0.192)
Conscientiousness				-0.196
				(0.177)
Neuroticism				-0.084
				(0.237)
Openness				0.340*
				(0.196)
Constant	0.164	0.894	1.508*	-6.572**
	(0.184)	(0.545)	(0.897)	(2.833)
Observations	300	300	300	300
Log pseudolikelihood	-182.276	-177.750	-177.284	-154.670

Robust standard errors adjusted for 60 clusters (subjects) in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Panel probit regression (random effects) with *Follow* as dependent variable, *Video Message* as independent variable and all control variables

Channel choice	Dishonest or honest message	Reason for channel choice
Video	Honest	Sollte ehrlicher wirken, als Text und ich hatte die höchste Mögliche Zahl.
Video	Honest	Persönlicher und damit glaubwürdiger
Video	Honest	Da ich versuche, Teilnehmer B die Wahrheit über die Zahl mitzuteilen, glaube ich, dass eine Videonachricht mit direktem Blick in die Kamera die Intention eher mitteilen kann, ähnlich wie Blickkontakt in einem persönlichen Gespräch.
Video	Honest	Da visueller & akustischer Kontakt zu mehr Vertrauen führen kann und alle meine Nachrichten der Wahrheit entsprechen.
Video	Honest	Es wirkt authentischer eine Person zu hören, als nur eine Computernachricht zu erhalten. Ich wollte, dass die Echtheit mir abgenommen wird.
Video	Honest	Meiner Meinung nach ist das Vertrauen beim Versenden einer Videonachricht höher als bei einer geschriebenen Nachricht.
Video	Honest	Persönlicher, ich habe die korrekte Zahl übergeben. Ein Video lässt Teilnehmer B vllt. besser vertrauen.
Video	Honest	Eine Videonachricht ist persönlicher als eine eine Textnachricht. Ich wollte, dass Teilnehmer B mich vertrauen könnte.
Video	Honest	Weil eine Videonachricht vertrauenswürdiger vorkommt als eine Textnachricht und ich glaube, dass mein Gegenüber bei hohen Zahlen (wie der 5) skeptisch ist ob sie der Wahrheit entsprechen
Video	Honest	Ich wollte konstant nur Videos verschicken
Video	Honest	Hat mehr Spaß gemacht.
Video	Honest	Videonachricht
Video	Honest	Weil ich davor schon genug Textnachrichten versendet habe und etwas Abwechslung wollte.
Video	Honest	Weil ich davor eine Textnachricht gesendet habe und abwechseln wollte.

Note: In this table, we only included the reasons for the chosen channel given by senders who chose to send an honest video message in the *Choice* treatment. The reasons are the original responses of the senders in the German language.

Table A.8: Reasons given by senders for sending an honest video message in Round 6

Dep. Var.: Video	(1)	(2)	(3)	(4)	(5)
Dishonest	-1.055**	-1.089**	-0.504	-0.543	-0.591
	(0.425)	(0.539)	(0.578)	(0.586)	(0.594)
Round		0.017	0.058	0.060	0.061
		(0.076)	(0.077)	(0.077)	(0.079)
Assigned Number		-0.019	-0.006	-0.009	-0.015
		(0.125)	(0.131)	(0.131)	(0.133)
Δ Human Presence$_{\text{Video - Text}}$		-0.388*	-0.071	-0.039	0.014
		(0.221)	(0.320)	(0.321)	(0.343)
Dishonest $\times$ Δ Human Presence$_{\text{Video - Text}}$			-0.632**	-0.640**	-0.688**
			(0.313)	(0.312)	(0.312)
Age				0.162	0.152
				(0.168)	(0.142)
Business and Economics				-0.084	-0.549
				(0.769)	(0.731)
Share Dishonest Round 1					0.804
					(1.182)
Diff. in Usage					-0.817*
					(0.459)
Diff. in Experience					0.424
					(0.422)
ICT General					-0.687
					(0.808)
ICT Communication					0.238
					(0.653)
Online Comm. 1					0.115
					(0.138)
Online Comm. 2					-0.038
					(0.141)
Online Comm. 3					-0.188
					(0.300)
Online Comm. 4					0.217
					(0.145)
Online Comm. 5					-0.210
					(0.141)
Participation					-1.115
					(0.930)
Failed Attempts					-0.143
					(0.460)
Trust					0.808
					(0.716)
Extraversion					0.326
					(0.411)
Agreeableness					-0.566
					(0.481)
Conscientiousness					0.001
					(0.406)
Neuroticism					-0.324
					(0.376)
Openness					-0.256
					(0.430)
Constant	-0.396	0.065	-0.533	-4.388	-1.042
	(0.420)	(0.687)	(0.772)	(4.238)	(7.518)
Observations	266	266	266	266	266
Log pseudolikelihood	-120.448	-119.004	-116.225	-115.470	-108.784

Robust standard errors adjusted for 64 clusters (subjects) in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: We excluded two observations due to downward lying ($r < s$).

Table A.9: Panel probit regression (random effects) with *Video* as dependent variable, *Dishonest* as independent variable, only observations without assigned number $s = 6$ and all control variables

Dep. Var.: Completion time video	(1)	(2)	(3)	(4)
Dishonest	4.379	3.924	2.721	3.299
	(3.585)	(3.216)	(2.845)	(2.769)
Round		-1.015	-0.979	-0.923
		(0.932)	(0.937)	(0.935)
Assigned Number		-0.032	-0.060	0.027
		(0.458)	(0.470)	(0.465)
Δ Human Presence$_{\text{Video - Text}}$		0.343	-0.260	-0.104
		(1.231)	(1.370)	(1.554)
Dishonest $\times$ Δ Human Presence$_{\text{Video - Text}}$			2.230	2.672
			(2.473)	(2.577)
Age				-0.315
				(0.653)
Business and Economics				-5.260
				(4.130)
Constant	47.144***	51.065***	51.520***	60.561***
	(2.520)	(4.690)	(4.651)	(16.830)
Observations	119	119	119	119

Robust standard errors adjusted for 36 clusters (subjects) in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: We only included senders in the *Choice* treatment in these regressions. 43.75% of senders (28 senders) in the *Choice* treatment had a strong preference for the text channel and chose text messages in all five rounds.

Table A.10: Linear panel regressions (random effects) with *Completion time video* as dependent variable and *Dishonest* as independent variable

Dep. Var.: Video	(1)	(2)	(3)	(4)	(5)
Dishonest	-1.329***	-1.410**	-1.141	-1.144	-1.251*
	(0.401)	(0.584)	(0.716)	(0.717)	(0.697)
Round		-0.001	0.002	0.005	0.002
		(0.071)	(0.069)	(0.069)	(0.069)
Assigned Number		-0.033	-0.034	-0.033	-0.057
		(0.123)	(0.123)	(0.123)	(0.125)
Δ Social Image$_{\text{Video - Text}}$		-0.207	-0.142	-0.107	0.012
		(0.232)	(0.263)	(0.255)	(0.240)
Dishonest $\times$ Δ Social Image$_{\text{Video - Text}}$			-0.139	-0.146	-0.127
			(0.231)	(0.233)	(0.215)
Age				0.197	0.215
				(0.168)	(0.143)
Business and Economics				-0.398	-0.520
				(0.808)	(0.693)
Share Dishonest Round 1					1.161
					(1.196)
Diff. in Usage					-0.999**
					(0.501)
Diff. in Experience					0.479
					(0.404)
ICT General					-0.721
					(0.784)
ICT Communication					-0.137
					(0.649)
Online Comm. 1					0.011
					(0.142)
Online Comm. 2					-0.037
					(0.151)
Online Comm. 3					-0.020
					(0.303)
Online Comm. 4					0.123
					(0.141)
Online Comm. 5					-0.153
					(0.154)
Participation					-0.829
					(1.004)
Failed Attempts					-0.341
					(0.452)
Trust					1.067
					(0.763)
Extraversion					0.210
					(0.398)
Agreeableness					-0.500
					(0.461)
Conscientiousness					-0.193
					(0.393)
Neuroticism					-0.690*
					(0.411)
Openness					-0.015
					(0.382)
Constant	-0.340	0.200	0.066	-4.534	-1.505
	(0.467)	(0.838)	(0.859)	(4.242)	(7.512)
Observations	314	314	314	314	314
Log pseudolikelihood	-136.348	-135.821	-135.544	-134.277	-127.177

Robust standard errors adjusted for 64 clusters (subjects) in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Note: We excluded six observations due to downward lying ($r < s$).

Table A.11: Panel probit regression (random effects) with *Video* as dependent variable, *Dishonest* and Δ *Social Image*$_{\textit{Video - Text}}$ as independent variables and all control variables

A.3 Instructions for Senders

Welcome to this experiment

You take part in an economic decision-making experiment. This experiment consists of **two parts:**

Part 1 consists of **six rounds** in which you make **decisions.**

Part 2 consists of a **questionnaire.**

You will receive the instructions directly before the task or the respective rounds.

In the experiment, we use the currency "ECU". This is converted into euros at the end of the experiment. **Here, 1 ECU = €0.10**.

Your payoff

Your decisions in **Part 1 are relevant** for your **payoff. You will receive more detailed information before the start of each round**. **In addition,** you will receive **20 ECU** for carefully completing the **questionnaire in Part 2.**

Immediately after the experiment, you will receive a link to an **encrypted website of the KD^{2}Lab**, where you can deposit your **bank details** to receive your payoff for the experiment. The bank data will be stored separately from the experimental data. Please deposit your bank details there immediately after the experiment so that the money you have earned in the experiment can be transferred to your account soon. Therefore, please do not close the experiment browser window until you are asked to do so.

Please note:

The payoff **is anonymous,** i.e., no participant is informed of the total payoff of another participant.

Contact for questions

During the entire experiment, no communication is allowed except via the experimental software. If you have a question, please open the cubicle door wide. We will then come to you.

Part 1: Instructions Round 1

In Part 1, two participants (Participant A and Participant B) are each assigned to a group. You will **be in the role of Participant A** throughout Part 1.

Participants in the role of Participant B will **participate in a separate session with a time delay**, are male, and will **not** be **recruited via the KD^{2}Lab panel**.

Part 1 consists of **six rounds,** and each round is divided into **two stages**. First, we describe both stages of Round 1.

Stage 1

In Stage 1, the computer randomly assigns **each group (Participant A and Participant B)** an **integer between 1 and 6**. Each number 1, 2, 3, 4, 5, or 6 is **equally likely**.

Only **at the end of the experiment** you receive the information as to which random number was **actually** assigned by the computer.

For **each possible assigned number (1, 2, 3, 4, 5, and 6),** you must write a pre-written **message to Participant B** about the assigned number (see Table 1). This message **does not have to** contain the actually assigned number. Participant **B does not receive any information about the actually assigned number** before his decision at Stage 2.

Table 1: Your decisions in Round 1

Assigned number	1	2	3	4	5	6
Message to Participant B: “The assigned number is...”						

Stage 2

Participant B must decide whether to **follow your message.** To do this, Participant B decides for **all possible messages whether** or not to follow this message.

At the end of the experiment, you send the corresponding message to Participant B according to the **actually assigned number,** and Participant B makes the corresponding decision.

Payoff

The payoff for both participants is determined as follows:

Your payoff (see Table 2):

You receive a payoff of **10 ECU plus three times the number sent in the message in ECU.** That is, 13 ECU if you send the message that the assigned number is 1, 16 ECU if you send the message that the assigned number is 2, etc.

Table 2: Your payoff table in Round 1

Number sent	1	2	3	4	5	6
Your payoff (in ECU)	13	16	19	22	25	28

Payoff Participant B (see Table 3):

If Participant B **follows your message**, Participant B receives a **payoff of 10 ECU** if the **message contains the actual number**. If your message **does not contain the actually assigned number**, Participant B receives a payoff of **0 ECU.**

If Participant B **does not follow** your message, Participant B receives a **payoff of 3 ECU.**

Table 3: Payoff table for Participant B in Round 1

Decision	**Payoff Participant B (in ECU)**
Participant B **follows** your message, and the message **contains** the actually assigned number	10
Participant B **follows** your message, and the message **does not** contain the actually assigned number	0
Participant B **does not** follow your message	3

Participant B is informed about the procedures and the resulting payoff for participants A and B.

Your decision based on the **actually assigned number** will be decisive for the payoff in **Round 1**. You will receive the information about the actually assigned number at the **end of the experiment**.

Comprehension questions Round 1

You ...

- ... must always write a message to Participant B.
- ... are free to decide whether to write a message to Participant B.

If the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is "... must always write a message to Participant B."

Participant B ...

- ... must follow your message.
- ... is free to decide whether to follow your message.

If the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is"... is free to decide whether to follow your message."

Your payoff is...

- ... 10 ECU plus three times the number sent in the message in ECU, irrespective of the decision of Participant B.
- ... 10 ECU plus three times the number sent in the message in ECU if Participant B follows the message, otherwise 0 ECU.
- ... 10 ECU plus three times the number sent in the message in ECU if Participant B does not follow the message, otherwise 0 ECU.

If the wrong answer was given: Unfortunately, your answer is incorrect. Please review the summary of instructions at the bottom of the page and try.

After two incorrect answers: Unfortunately, your answer is incorrect. The correct answer is: "... 10 ECU plus three times the number sent in the message in ECU, irrespective of the decision of Participant B."

Decisions Round 1

As described in the instructions, you are in the role of **Participant A** in Stage 1.

For **each possible assigned number (1, 2, 3, 4, 5, and 6),** decide which pre-written **message you** would like to write **to Participant B.**

Assigned number	**Message to Participant B: "The assigned number is ..."**					
1	◯ 1	◯ 2	◯ 3	◯ 4	◯ 5	◯ 6
2	◯ 1	◯ 2	◯ 3	◯ 4	◯ 5	◯ 6
3	◯ 1	◯ 2	◯ 3	◯ 4	◯ 5	◯ 6
4	◯ 1	◯ 2	◯ 3	◯ 4	◯ 5	◯ 6
5	◯ 1	◯ 2	◯ 3	◯ 4	◯ 5	◯ 6
6	◯ 1	◯ 2	◯ 3	◯ 4	◯ 5	◯ 6

Part 1: Instructions Round 2 - 6

Rounds 2 - 6 are identical and are described below. In **each round,** the groups are **reassigned,** and you work on the task with a **different participant in the role of Participant B**.

The task is similar to Round 1. We will now explain the differences.

Stage 1

In Stage 1, the computer randomly assigns **each group (Participant A and Participant B)** an **integer between 1 and 6**. Each number 1, 2, 3, 4, 5, or 6 is **equally likely**.

In Rounds 2 - 6, you will be **informed** about **the assigned number.** You then send **a pre-written message** to Participant B about the assigned number.

[Choice Treatment]

As in Round 1, this message **does not** have to contain the actually assigned number. Participant B **does not** receive **any information about the actually assigned number** before making his decision at Stage 2.

In contrast to Round 1, you now have **two options** in Rounds 2 - 6 to send the message to Participant B:

[NoChoice Treatment]

In contrast to Round 1, **this message must contain the actually assigned number**. **Participant B does not** receive **any information about the actually assigned number** before his decision on Stage 2, but is informed that your message must contain the actually assigned number.

In addition, you now have **two options** in Rounds 2 - 6 to send the message to Participant B:

[Both Treatments]

Option 1: **A video message**.

Option 2: **A text message** ("The assigned number is ...")

Below, you can see an **example of** a **video message**.

[Choice Treatment]

After you have been informed of the assigned number, you are **free to decide** in **Rounds 2 - 6** whether you want to send Participant B **a text message or a video message**. You then decide **which message** you want to send to Participant B in the text or video message (see Table 4).

[NoChoice Treatment]

After you have been informed of the assigned number, you are **free to decide** in **Rounds 2 - 6** whether you want to send Participant B **a text message or a video message** (see Table 4).

Table 4: Your decisions in Rounds 2 - 6

Do you want to send Participant B a text message or a video message?
◯ Text message ◯ Video message

[Choice Treatment]

Which message do you want to send to Participant B in the text or video message? **"The assigned number is ..."**
◯ 1 ◯ 2 ◯ 3 ◯ 4 ◯ 5 ◯ 6

If you decide to send Participant B **a video message**, you then record a video message about the assigned number. The video message **may only** contain **the sentence "The assigned number is ...", i.e.** the video message may not contain any other words.

[NoChoice Treatment]

If you decide to send Participant B **a video message**, you then record a video message about the assigned number. The video message **must** contain **the actually assigned number** and **may only** contain **the sentence "The assigned number is ...", i.e.** the video message may not contain any other words.

[Both Treatments]

You have at least **40 seconds** in each round to record the video message. After these 40 seconds have expired, **the "Next" button appears,** and you can move to the next page.

If you decide to send Participant B **a text message** and therefore do not record a video message, the **"Next" button will** only appear after **40 seconds have expired**.

Stage 2

As in Round 1, Stage 2 of Rounds 2 - 6 takes place after Stage 1 in a separate session with a time delay. Participant B receives a text message or a video message from you about the assigned number and decides whether or not to follow the message.

Part 1: Payoff Round 2 - 6

The payoff for both participants **does not change in Rounds 2 - 6** and is **identical to the payoff in Round 1:**

Your payoff (see Table 5):

Table 5: Your payoff table in Rounds 2 - 6

Number sent	1	2	3	4	5	6
Your payoff (in ECU)	13	16	19	22	25	28

Payoff Participant B (see Table 6):

Table 6: Payoff table for Participant B in Rounds 2 - 6

Decision	Payoff Participant B (in ECU)
Participant B **follows** your message, and the message **contains** the actually assigned number	10
Participant B **follows** your message, and the message **does not** contain the actually assigned number	0
Participant B **does not** follow your message	3

As in Round 1, Participant B is informed about the procedures and the resulting payoff for participants A and B. In addition, in Rounds 2 - 6, Participant B is informed that you have **two options (text message or video message)** for the message to Participant B.

At the end of the experiment, **two of the five rounds** are **randomly drawn**. The decisions made by you and Participant B in these two Rounds are used to **determine the payoff**.

Note:

The random numbers (actually assigned number and payoff-relevant rounds) are independent of each other.

Comprehension question Round 2 - 6

You ...

- ... must always send a video message to Participant B.
- ... must always send a text message to Participant B.
- ... are free to decide whether to send a text message or a video message to Participant B.

If the wrong answer was given: Sorry, your answer to this question is incorrect. The correct answer is: "... are free to decide whether to send a text message or a video message to Participant B."

Note on the video messages

As described in the privacy policy, depending on your decisions in Rounds 2 - 6, you **might** record video messages that will be shown to other participants in a separate session with a time delay. At no time will the other participants know your name or the total amount of your payoff. Other participants might be able to recognize you by your language or appearance. However, these will **not** be recruited via the KD^{2}Lab panel, but throughout Germany, Austria, and Switzerland.

Instructions for recording a video message

Before you start with Round 2, we will explain how to record a video message, and you can test the video recording.

In the middle of the top edge of the monitor in front of you, you will find a **camera** with which the frames of the video message are recorded. The **microphone** for recording your voice is located **on the table in front of you**.

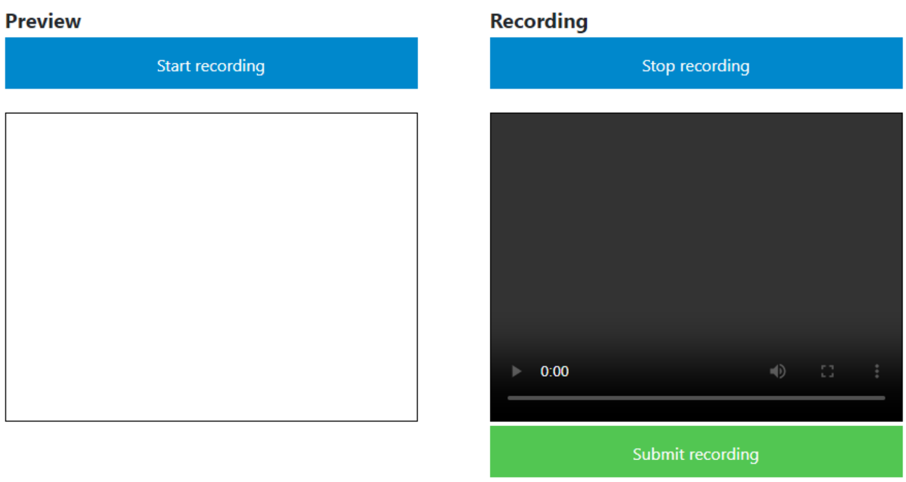
If you click on the **blue "Start recording"** button, you start recording a video message, and the color of the button **changes to red**. All sounds are now recorded via the microphone and frames via the camera. You can see the video recorded in real-time below the "Start recording" button.

Recording stops automatically after a maximum of **5 seconds.** You can also stop the recording **manually earlier** by clicking on the **blue "Stop recording"** button. The color of the **"Start recording"** button then changes back to **blue,** and the recorded video message with frames and sound is displayed below the "Stop recording" button.

As standard with a video player, you can listen to and watch the recorded video message again using the **Play (triangle)** button.

If you click on the **green "Submit recording"** button, you submit the current video message, and it is **downloaded and saved locally on your computer**. The downloaded files are only stored **internally by the university and not on external servers.** In addition, the video message is only downloaded via the **"Submit recording" button and not automatically** when the recording is stopped.

If you are not satisfied with the current video message or have not met the video message requirements, **you can start a new recording using the "Start recording" button**. In this case, the current video message will be deleted and overwritten with the new video message.



Please test the recording of a video message now. You can **test** the video recording **as often as you want** to familiarize yourself with how it works. These test videos have **no impact** on your payoff and will not be shown to Participant B.

Please do **not** wear **a face mask** during video recording. If your face is not fully visible in the videos, you can adjust the camera vertically upwards or downwards.

If you have any **questions or problems with recording a video message**, please open the cubicle door wide. We will then come to you.

Trial round

Before you start Round 2, a trial round will be conducted. Your decisions in this trial round are **not relevant for your payoff, and your text message or video message will not be sent to other participants B**. Apart from this, the trial round is **identical** to Rounds 2 - 6.

Trial round: Decision text or video message

Your group has been assigned the following number by a computer in the **trial round:**

Number X

[Choice Treatment]

Do you want to send Participant B a text message or a video message in the trial round?
◯ Text message ◯ Video message

Which message do you want to send to Participant B in the text or video message? "The assigned number is ..."
◯ 1 ◯ 2 ◯ 3 ◯ 4 ◯ 5 ◯ 6

[NoChoice Treatment]

Do you want to send Participant B the message "The assigned number is X." as a text message or as a video message in the trial round?
◯ Text message ◯ Video message

[Choice Treatment]

If you decide to send Participant B **a video message**, you then record a video message about the assigned number. The video message **may only** contain **the sentence "The assigned number is ...", i.e.** the video message may not contain any other words.

[NoChoice Treatment]

If you decide to send Participant B **a video message**, you then record a video message about the assigned number. The video message **must** contain **the actually assigned number** and

may only contain **the sentence "The assigned number is ...", i.e.** the video message may not contain any other words.

[Both Treatments]

You have at least **40 seconds** in each round to record the video message. After these 40 seconds have expired, **the "Next" button appears,** and you can move to the next page.

If you decide to send Participant B **a text message** and therefore do not record a video message, the **"Next" button will** only appear after **40 seconds have expired**.

Trial round: Please wait ...

If the participant has decided for the text message

[Choice Treatment]

In the **trial round,** you were assigned the **number X** and decided to send the message **"The assigned number is Y"** as a **text message**.

[NoChoice Treatment]

In the **trial round,** you were assigned the **number X** and decided to send the message **"The assigned number is X"** as a **text message.**

[Both Treatment]

You now need to wait **40 seconds until** the "Next" button appears, and you can continue with the task.

Note:

At the bottom of the page, you will see the **remaining time** until the "Next" button appears. Please **do not** reopen the page, as this will restart the timer for the remaining time.

Trial round: Recording the video message

If the participant has decided for the video message

[Choice Treatment]

In the **trial round,** you were assigned the **number X** and decided to send the message **"The assigned number is Y"** as a **video message**.

[NoChoice Treatment]

In the **trial round,** you were assigned the **number X** and decided to send the message **"The assigned number is X"** as a **video message.**

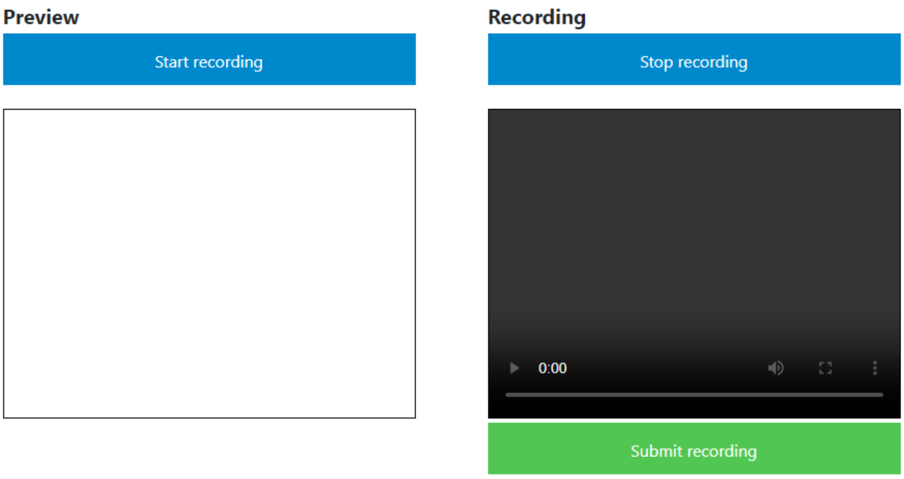
[Both Treatments]

Please record this video message to Participant B.

You can submit the recorded video message using the **“Submit recording”** button. Look **into the camera** while recording, and **do not wear a face mask.**

Please submit only **one** video message. If you submit several video messages, the **last submission will be** used. **The video message may not** contain any **words other than the sentence “The assigned number is ...”**.

You can open the instructions for recording a video message again below.



Note:

At the bottom of the page, you will see the **remaining time** until the “Next” button appears. Please **do not** reopen the page, as this will restart the timer for the remaining time.

Trial round: Result

[Choice Treatment]

	Number assigned by the computer	**Your message to Participant B**	**Text message or video message**	**Your payoff**
Trial round	X	The assigned number is Y	...	aa ECU

[NoChoice Treatment]

	Number assigned by the computer	**Your message to Participant B**	**Text message or video message**	**Your payoff**
Trial round	X	The assigned number is X	...	aa ECU

[Both Treatments]

Round 2: Decision text or video message

Your group has been assigned the following number by a computer in **Round 2:**

Number X

[Choice Treatment]

Do you want to send Participant B a text message or a video message in Round 2?
◯ Text message ◯ Video message

Which message do you want to send to Participant B in the text or video message? "The assigned number is ..."
◯ 1 ◯ 2 ◯ 3 ◯ 4 ◯ 5 ◯ 6

[NoChoice Treatment]

Do you want to send Participant B the message "The assigned number is X." as a text message or as a video message in Round 2?
◯ Text message ◯ Video message

[Choice Treatment]

If you decide to send Participant B **a video message**, you then record a video message about the assigned number. The video message **may only** contain **the sentence "The assigned number is ...", i.e.** the video message may not contain any other words.

[NoChoice Treatment]

If you decide to send Participant B **a video message**, you then record a video message about the assigned number. The video message **must** contain **the actually assigned number** and **may only** contain **the sentence "The assigned number is ...", i.e.** the video message may not contain any other words.

[Both Treatments]

You have at least **40 seconds** in each round to record the video message. After these 40 seconds have expired, **the "Next" button appears,** and you can move to the next page.

If you decide to send Participant B **a text message** and therefore do not record a video message, the **"Next" button will** only appear after **40 seconds have expired**.

Round 2: Please wait ...

If the participant has decided for the text message

[Choice Treatment]

In **Round 2**, you were assigned the **number X** and decided to send the message **“The assigned number is Y”** as a **text message**.

[NoChoice Treatment]

In **Round 2**, you were assigned the **number X** and decided to send the message **“The assigned number is X”** as a **text message.**

[Both Treatment]

You now need to wait **40 seconds until** the “Next” button appears and you can continue with the task.

<u>Note:</u>

At the bottom of the page, you will see the **remaining time** until the “Next” button appears. Please **do not** reopen the page, as this will restart the timer for the remaining time.

Round 2: Recording the video message

If the participant has decided for the video message

[Choice Treatment]

In **Round 2**, you were assigned the **number X** and decided to send the message **“The assigned number is Y”** as a **video message**.

[NoChoice Treatment]

In **Round 2**, you were assigned the **number X** and decided to send the message **“The assigned number is X”** as a **video message.**

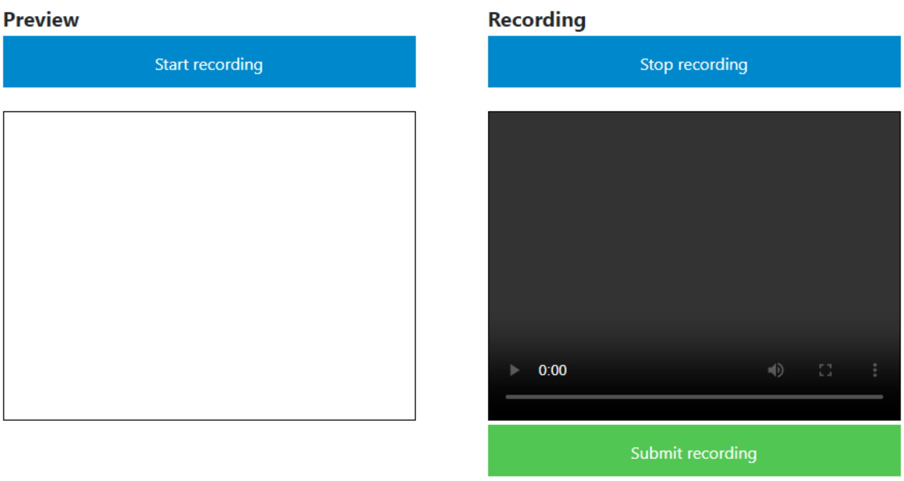
[Both Treatments]

Please record this video message to Participant B.

You can submit the recorded video message using the **“Submit recording”** button. Look **into the camera** while recording, and **do not wear a face mask.**

Please submit only **one** video message. If you submit several video messages, the **last submission will be** used. **The video message may not** contain any **words other than the sentence “The assigned number is ...”**.

You can open the instructions for recording a video message again below.



<u>Note:</u>

At the bottom of the page, you will see the **remaining time** until the “Next” button appears. Please **do not** reopen the page, as this will restart the timer for the remaining time.

Pages now repeat for Rounds 3 - 6

Part 1: End

Please briefly state why you decided to send a text message / video message in Round 6.

[open text field]

You have completed Part 1 of the study. Part 2 begins for you on the next page.

Part 2: Questionnaire Page 1

In Part 2 of the study, you will answer a questionnaire.

The first questions refer to **Rounds 2 - 6 in Part 1**. In these rounds, you had two options to send the message to Participant B:

Option 1: **A video message**.

Option 2: **A text message** (“The assigned number is ...”)

Please rate the extent to which you agree with the following statements if you would send [have sent] Participant B a video message. [If Participant A sent at least one video message]	Do not agree at all 1	2	3	4	5	6	Fully agree 7
I would feel [have felt] very close to Participant B. (“close” in the sense of emotional rather than physical proximity)							
I would feel [have felt] the presence of Participant B very strongly.							
I would feel [have felt] very connected to Participant B.							
I would be [have been] very concerned about what Participant B thinks about me.							
I would care [cared] very much about leaving a good impression on Participant B.							
It would be [was] very important for me that Participant B thinks I am honest.							

Please rate the extent to which you agree with the following statements if you would send [have sent] Participant B a text message. [If Participant A sent at least one text message]	Do not agree at all 1	2	3	4	5	6	Fully agree 7
I would feel [have felt] very close to Participant B. (“close” in the sense of emotional rather than physical proximity)							
I would feel [have felt] the presence of Participant B very strongly.							
I would feel [have felt] very connected to Participant B.							
I would be [have been] very concerned about what Participant B thinks about me.							
I would care [cared] very much about leaving a good impression on Participant B.							
It would be [was] very important for me that Participant B thinks I am honest.							

Please answer using a scale.

The value **1** means: **Very unlikely**, the value **11** means: **Very likely**.

How high do you estimate the **probability that** a participant in the role of Participant B will **recognize** you **by your language or your face** if they see a video message from you?

1: Very unlikely										11: Very likely
○	○	○	○	○	○	○	○	○	○	○

Questionnaire Page 2

Before we ask you the next questions, we would first like to explain the **two categories of communication channels** in more detail. In this context, communication channels are defined as media that can be used to transfer information between two or more people.

Written Communication Channels	This refers to communication channels, such as email, SMS, or WhatsApp (without voice messages, video calls, etc.), with which **messages** are **only** transmitted **using text and emojis.** Sounds and images cannot be transmitted using these communication channels.
Audiovisual Communication Channels	This refers to communication channels, such as video conferencing, Facetime, or video calls, with which **messages** are transmitted **via sounds and images.** These communication channels can be used to transmit information about voice pitch, pronunciation, intonation, pace of speech, posture, gestures, and facial expressions.

We will now ask you questions about your **experience and use of the two different communication channels**. Please note that the questions refer to your **entire life, i.e.** your private life as well as your studies, work, etc.

How often do you use written communication channels?

- Never
- At least once a year
- At least once a month
- At least once a week
- At least once a day
- Several times a day

How often do you use audiovisual communication channels?

- Never
- At least once a year
- At least once a month
- At least once a week
- At least once a day
- Several times a day

Questionnaire Page 3

Please rate the extent to which you agree with the following statements.	Do not agree at all	Rather disagree	Somewhat agree	Rather agree	Fully agree
I have a lot of experience with written communication channels					
I have a lot of experience with audiovisual communication channels					

Questionnaire Page 4

In the following, you will be asked questions about the **handling of digital systems**. Digital systems are all **digital applications (e.g., software or apps)** and **all digital devices (e.g., computers or smartphones)**.

Please rate the extent to which you agree with the following statements.	Strongly disagree	Disagree	Slightly disagree	Slightly agree	Agree	Strongly agree
I can operate digital systems.						
I am good at using digital systems.						
I quickly learn when it comes to using digital systems.						
It is easy for me to get familiar with new digital systems.						
I have always been good at using digital systems.						
I can communicate information through various media formats (text, image, video, sound ...).						

Please rate the extent to which you agree with the following statements.	Strongly disagree	Disagree	Slightly disagree	Slightly agree	Agree	Strongly agree
Careful processing is important. Please click on “Slightly agree” for this question.						
I am good at collaborating with others through digital systems.						
I quickly learn which communication medium (text, audio, video, sound ...) has to be used for editing a task.						
It is easy for me to spread information through digital systems.						

Questionnaire Page 5

The next questions are about **online and mobile communication**. This refers to **communication** taking place over the **Internet or mobile networks**, using **mobile phones, computers, tablets or other digital devices**.

To what extent would you say that online and mobile communication …	Not at all	Completely
... makes people feel closer to one another? (“closer” in the sense of emotional rather than physical proximity)		
... makes work and personal life interrupt each other?		
... makes it easy to coordinate and manage activities?		
... undermines personal privacy?		
... exposes people to misinformation?		

Questionnaire Page 6

How well do the following statements describe your personality? I see myself as someone who …	Disagree strongly	Disagree a little	Neither agree nor disagree	Agree a little	Agree strongly
… is reserved.					
… is generally trusting					
… tends to be lazy					
… is relaxed, handles stress well					
… has few artistic interests					
… is outgoing, sociable					
… tends to find fault with others					
… does a thorough job					
… gets nervous easily.					
… has an active imagination					

Questionnaire Page 7

The following statements may apply more or less to you. To what extent do you think each statement applies to you personally?	Does not apply at all	Applies a little	Applies somewhat	Applies mostly	Applies completely
I am convinced that most people have good intentions.					
Careful processing is important. Please click on "Applies mostly" here.					
You can't rely on anyone these days.					
In general, people can be trusted.					

Result Part 1 and payoff

Result Round 2 - 6

The following table gives you an overview of your results in Rounds 2 - 6.

Round	Number assigned by the computer	Your message to Participant B	Text message or video message	Your payoff
Round 2	X	The assigned number is Y	Text message	xx ECU
Round 3	X	The assigned number is Y	Video message	xx ECU
Round 4	X	The assigned number is Y	Video message	xx ECU
Round 5	X	The assigned number is Y	Video message	xx ECU
Round 6	X	The assigned number is Y	Text message	xx ECU

Your expected payoff

In **Round 1 of Part 1,** the computer assigned the **number X to** your group and you earned **xx ECU.**

For **Rounds 2 - 6 of Part 1, Rounds a and b** have been drawn for your payoff.

Please note that in Rounds 2 - 6 only your decisions in the video messages are relevant for your payoff and we will check your video messages first. Therefore, your final payoff may differ from the expected payoff.

Your total payoff in **Part 1 of the study is xx ECU**.

In addition, you will receive **20 ECU** for **Part 2 of the study**.

Your expected payoff for the entire study is, therefore, xx ECU.

Transition to SoSci Survey

We will now ask you for socio-demographic data (age and course of study). As this data is personal and we take the issue of data protection very seriously, this part will be carried out on the **SoSci Survey software hosted by the university**.

Click **here** to go to SoSci Survey.

Part 2: Socio-demographic data

Please give us a little more information about you before we conclude the experiment.

In which year were you born?

Which of the following subjects are you studying? (multiple answers possible)

- Industrial Engineering and Management
- Mechanical engineering
- Informatics
- Information Systems
- Information Engineering and Management
- Business Administration
- Mathematics
- Other [Open text field]

Have you already taken part in other experiments in the KD^{2}Lab?

- Yes
- No

Please indicate how much **attention** you have paid to this survey. You will receive your **payoff regardless of your answer to this question**. We appreciate your honesty!

	very little	little	some	much	very much
I have paid ____ attention to this survey.					

Do you have any comments on the experiment? [Open text field]

Payoff

Thank you for your participation!

Before payoff is made, we will check whether you have complied with the video message requirements in Part 1 and answered the questionnaire fully and carefully. Participants who fail to do so will not receive a payoff.

In order to receive the amount by bank transfer, you must send your bank details to the KD²Lab. Personal data that you transmit to the KD²Lab for payoff will not be associated with your decisions in the study. To deposit your bank details, you will need your access key. You will receive this access key from the experiment management.

Please click on "Complete study" to deposit your bank details. Please enter your details now - it will not be possible to enter them later.

Complete study

A.4 Instructions for Receivers

Please enter your (24-digit) Prolific ID first.

[Text field]

Technology check

In this study, you will **listen to** and **watch** videos. Therefore, we will first check your **technical requirements** to ensure that audio and video will work for you.

Please watch **and listen to** the following video and then answer the **two questions**.

Audio check

What color did the person in the video name?

- Red
- Blue
- Green
- Yellow

Video check

How many fingers did the person in the video show?

- One finger
- Two fingers
- Three fingers
- Four fingers
- Five fingers

If one of the two questions is answered incorrectly, a participant cannot continue with the questionnaire.

Welcome to this study

You take part in an economic decision-making experiment. This experiment consists of **two parts:**

Part 1 consists of **six rounds** in which you make **decisions.**

Part 2 consists of a **questionnaire.**

You will receive the instructions directly before the task or the respective rounds.

In the experiment, we use the currency "ECU". This is converted into pounds at the end of the experiment. **Here, 1 ECU = £0.09**.

Please note:

NO DECEPTION POLICY. In this study, **all information in the instructions is true**. We will inform you in detail about the procedures and the information available to all participants.

Your payoff

Your decisions in **Part 1 are relevant** for your **payoff. You will receive more detailed information before the start of each round**. **In addition,** you will receive **20 ECU** for carefully completing the **questionnaire in Part 2.**

At the end of the experiment, you will receive your **completion code** to confirm that you have completed the study. You will receive your payoff no later than four working days after your participation after we have evaluated your decisions.

Please note:

The **payoff is anonymous**, i.e. no participant is informed of the total payoff of another participant.

Part 1: Instructions Round 1

In Part 1, two participants (Participant A and Participant B) are each assigned to a group. You will **be in the role of Participant B** throughout Part 1.

Participants in the role of Pparticipant A **took part in a separate session with a time delay**, are male and were **not recruited via Prolific**.

Part 1 consists of **six rounds**, and each round is divided into **two stages**. First, we describe both stages of Round 1.

Stage 1

In Stage 1, the computer randomly assigned **each group (Participant A and Participant B)** an **integer between 1 and 6**. Each number 1, 2, 3, 4, 5, or 6 was **equally likely**.

Only **at the end of the experiment** you receive the information as to which random number was **actually** assigned by the computer.

For **each possible assigned number (1, 2, 3, 4, 5, and 6),** Participant A had to write a **prewritten message to you about the assigned number.** This message **did not have to** contain the actually number assigned. **You will not** receive **any information about the actually assigned number** before your decision at Stage 2.

Stage 2

You must decide whether to **follow the message from Participant A**. To do this, you decide for **all possible messages whether** or not to follow this message (see Table 1).

Table 1: Your decisions in Round 1

Message from Participant A	**Your decision**	
"The assigned number is 1"	follow ◯	do not follow ◯
"The assigned number is 2"	follow ◯	do not follow ◯
"The assigned number is 3"	follow ◯	do not follow ◯
"The assigned number is 4"	follow ◯	do not follow ◯
"The assigned number is 5"	follow ◯	do not follow ◯
"The assigned number is 6"	follow ◯	do not follow ◯

At the end of the experiment, the corresponding message is sent to you by Participant A according to the **actually assigned number**, and your corresponding decision is executed.

Payoff

The payoff for both participants is determined as follows:

Payoff Participant A (see Table 2):

Participant A has received a payoff of **10 ECU plus three times the number sent in the message in ECU.** That is, 13 ECU if Participant A has sent the message that the assigned number is 1, 16 ECU if Participant A has sent the message that the assigned number is 2, etc.

Table 2: Payoff table for Participant A in Round 1

Number sent	1	2	3	4	5	6
Payoff Participant A (in ECU)	13	16	19	22	25	28

Your payoff (see Table 3):

If you **follow the message from Participant A**, you will receive a payoff **of 10 ECU** if the **message contains the actual number**. If the message from Participant A **does not contain the actually assigned number**, you will receive a payoff of **0 ECU.**

If you **do not follow** the message from Participant A, you will receive a **payoff of 3 ECU.**

Table 3: Your payoff table in Round 1

Decision	**Your payoff (in ECU)**
You **follow** the message from Participant A and the message **contains** the actually assigned number	10
You **follow** the message from Participant A and the message **does not** contain the actually assigned number	0
You **do not** follow the message from Participant A	3

Participant A was informed about the procedures and the resulting payoff for participants A and B.

Your decision based on the **actually assigned number** will be decisive for the payoff in **Round 1**. You will receive information about the actually assigned number at the **end of the experiment**.

Examples of the payoff

In the following table, you will find 4 examples that illustrate your payoff and the payoff of Participant A.

Assigned number by the computer	**Message from Participant A**	**Decision by Participant B**	**Payoff for Participant A**	**Payoff for Participant B**
2	The assigned number is 2	Follow	16 ECU	10 ECU
2	The assigned number is 2	Do not follow	16 ECU	3 ECU
2	The assigned number is 6	Follow	28 ECU	0 ECU
2	The assigned number is 6	Do not follow	28 ECU	3 ECU

On the following pages, we describe **2 exemplary scenarios**. Please read through them and **then answer the questions about the payoff**.

Scenario 1:

The assigned **number** by the computer **is 4**. If the assigned number is 4, Participant A has decided to send you the message “**The assigned number is 4**”

Question 1:

What payoff in ECU would Participant A have received if you do not follow the message?

- 13
- 16
- 19
- 22
- 25
- 28

If the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is: Participant A would have received a payoff off 22 ECU. At the bottom of the page, you will find the instructions for the payoff.

Question 2:

What payoff in ECU would you receive if you follow the message?

- 10
- 0
- 3

If the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is: You would receive a payoff of 10 ECU because you followed the message and the message contained the actually assigned number. At the bottom of the page, you will find the instructions for the payoff.

Scenario 2:

The assigned **number** by the computer **is 1**. If the assigned number is 1, Participant A has decided to send you the message "**The assigned number is 4**"

Question 1:

What payoff in ECU would Participant A have received if you do not follow the message?

- 13
- 16
- 19
- 22
- 25
- 28

If the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is: Participant A would have received a payoff off 22 ECU. At the bottom of the page, you will find the instructions for the payoff.

Question 2:

What payoff in ECU would you receive if you follow the message?

- 10
- 0
- 3

If the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is: You would receive a payoff of 0 ECU, because you followed the message and the message did not contain the actually assigned number. At the bottom of the page you will find the instructions for the payoff.

Control question 1:

Before you make your decisions in Round 1, we have two control questions below.

Participant A ...

- ... always had to write a message to you.
- ... was free to decide whether to write a message to you.

If the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is: "Participant A always had to write a message to you".

Control question 2:

You ...

- ... must follow the message from Participant A.
- ... are free to decide whether to follow the message from Participant A.

If the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is: "You are free to decide whether to follow the message from Participant A".

Decisions Round 1

As described in the instructions, you are in the role of **Participant B** in Stage 2.

For **all possible messages from Participant A**, decide whether you **follow** the message **or not.**

Message from Participant A	**Your decision**	
"The assigned number is 1"	follow ◯	do not follow ◯
"The assigned number is 2"	follow ◯	do not follow ◯
"The assigned number is 3"	follow ◯	do not follow ◯
"The assigned number is 4"	follow ◯	do not follow ◯
"The assigned number is 5"	follow ◯	do not follow ◯
"The assigned number is 6"	follow ◯	do not follow ◯

Part 1: Instructions Round 2 - 6

Rounds 2 - 6 are identical and are described below. In **each round**, the groups are **reassigned**, and you work on the task with a **different participant in the role of Participant A**.

The task is similar to Round 1 and we will now explain the differences.

Stage 1

In Stage 1, the computer randomly assigned **each group (Participant A and Participant B)** an **integer between 1 and 6**. Each number 1, 2, 3, 4, 5, or 6 was **equally likely**.

In Rounds 2 - 6, participant A was informed about **the assigned number.** Participant A then sent you **a pre-written message** about the assigned number.

[Choice Treatment]

As in Round 1, the message from Participant A **did not** have to contain the actually assigned number. You will **not** receive **any information about the actually assigned number** before you make your decision at Stage 2.

In contrast to Round 1, Participant A now had **two options** in Rounds 2 - 6 to send the message to you:

[NoChoice Treatment]

In contrast to Round 1, **the message from Participant A had to contain the actually assigned number**. You will **not** receive **any information about the actually assigned number** before you make your decision at Stage 2.

In addition, Participant A now had **two options** in Rounds 2 - 6 to send the message to you:

[Both Treatments]

Option 1: **a video message**.

Option 2: **a text message** ("The assigned number is ...")

Below you can see an **example of** a **video message**.

Stage 2

As in Round 1, Stage 1 took place before Stage 2 was conducted in a separate session with a time delay.

Each round you receive a video message or a text message from a Participant A about the assigned number and decide whether to **follow** the message **or not**.

Please note:

In this study, **two treatment groups were** used, which differed **in Rounds 2 to 6** in terms of **whether or not the message from Participant A had to contain the actually assigned number.**

[Choice Treatment]

You are in the treatment group in which the message from Participant A did not have to contain the actually assigned number.

[NoChoice Treatment]

You are in the treatment group in which the message from Participant A always had to contain the actually assigned number.

Part 1: Payoff Round 2 - 6

The payoff of both participants **does not change in Rounds 2 - 6** and is **identical to the payoff in Round 1:**

Payoff Participant A (see Table 4):

Table 4: Payoff table for Participant A in Rounds 2 - 6

Number sent	1	2	3	4	5	6
Payoff Participant A (in ECU)	13	16	19	22	25	28

Your payoff (see Table 5):

Table 5: Your payoff table in Rounds 2 - 6

Decision	**Your payoff (in ECU)**
You **follow** the message from Participant A and the message **contains** the actually assigned number	10
You **follow** the message from Participant A and the message **does not** contain the actually assigned number	0
You **do not** follow the message from Participant A	3

As in Round 1, Participant A was informed about the procedures and the resulting payoff for participants A and B.

At the end of the experiment, **two of the five rounds** are **randomly drawn**. The decisions made by you and Participant A in these two rounds are used to **determine the payoff.**

Note:

The random numbers (actually assigned number and payoff-relevant rounds) are independent of each other.

Control question 1: Round 2 - 6

Please answer the following control questions.

In Rounds 2 to 6 ...

- ... the message from Participant A had to contain the actually assigned number.
- ... the message from Participant A did not have to contain the actually assigned number.

[Choice Treatment]

If the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is: "In Rounds 2 to 6, the message from Participant A <u>*did not*</u> *have to contain the actually assigned number."*

[NoChoice Treatment]

If the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is: "In Rounds 2 to 6, the message from Participant A had to contain the actually assigned number."

[Both Treatments]

Control question 2: Round 2 - 6

Participant A ...

- ... always had to send you a video message.
- ... was free to choose whether to send you a video message or a text message.
- ... always had to send you a text message.

If the wrong answer was given: Unfortunately, your answer is not correct. Please take another look at the summary of the instructions at the bottom of the page and try again. If again the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is: "Participant A was free to choose whether to send you a video message or a text message."

Control question 3: Round 2 - 6

In a round, a Participant B receives the message from a Participant A: "The assigned number is 3".

What payoff will Participant B receive if he follows the message?

- 0 ECU
- 10 ECU
- 3 ECU
- I cannot say because I do not know the actually assigned number.

[Choice Treatment]

If the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is: "I cannot say because I do not know the actually assigned number."

[NoChoice Treatment]

If the wrong answer was given: Unfortunately, your answer to this question is incorrect. The correct answer is: "Participant B receives 10 ECU because the message from Participant A in Rounds 2 to 6 <u>*had*</u> *to contain the actually assigned number."*

[Both Treatments]

<u>Please note:</u>

[Choice Treatment]

In this round, the message from Participant A <u>did not</u> have to contain the actually assigned number.

[NoChoice Treatment]

In this round, the message from Participant A <u>always</u> had to contain the actually assigned number.

The messages have been checked to ensure that they always contain the actually assigned number.

Your payoff:

- 10 ECU: Follow & message contains the number
- 0 ECU: Follow & message does not contain the number
- 3 ECU: Do not follow

Round 2 (3, 4, 5, 6): Decision

Participant B receives a text message

(The Next button on this page appears after 10 seconds)

You have received a text message from Participant A. The text message from Participant A is:

“The assigned number is X.“

What number did Participant A send you in the text message?

- Number 1
- Number 2
- Number 3
- Number 4
- Number 5
- Number 6

Decide whether to follow the message or not	follow ◯	do not follow ◯

Participant B receives a video message

(The Next button on this page appears after 10 seconds)

You have received **a video message** from Participant A.

Listen to and watch the video message and then decide whether you want to **follow** the message **or not**.

You can **listen to and watch** the **video message several times**.

What number did Participant A send you in the video message?

- Number 1
- Number 2
- Number 3
- Number 4
- Number 5
- Number 6

Decide whether to follow the message or not	follow ◯	do not follow ◯

End Part 1

You have completed Part 1 of the study. Part 2 begins for you on the next page.

Part 2: Questionnaire Page 1

In Part 2 of the study, you will answer a questionnaire.

The first questions refer to **Rounds 2 - 6 in Part 1**. In these rounds, Participant A had two options to send a message to you:

Option 1: **A video message**.

Option 2: **A text message** ("The assigned number is ...")

Please rate the extent to which you agree with the following statements if Participant A has sent you a video message.	Do not agree at all 1	2	3	4	5	6	Fully agree 7
I felt very close to Participant A. ("close" in the sense of emotional rather than physical proximity)							
I felt the presence of Participant A very strongly.							
I felt very connected to Participant A.							
I was very concerned about what Participant A thinks about me.							
I cared very much about leaving a good impression on Participant A.							
It was very important for me that Participant A thinks I am honest.							

Please rate the extent to which you agree with the following statements if Participant A has sent you a text message.	Do not agree at all 1	2	3	4	5	6	Fully agree 7
I felt very close to Participant A. ("close" in the sense of emotional rather than physical proximity)							
I felt the presence of Participant A very strongly.							
I felt very connected to Participant A.							
I was very concerned about what Participant A thinks about me.							
I cared very much about leaving a good impression on Participant A.							
It was very important for me that Participant A thinks I am honest.							

Questionnaire Page 2

Did you know a Participant A in a video message?

- Yes
- No

If yes was selected

In which round(s) did you know a Participant A?

- 2
- 3
- 4
- 5
- 6
- I do not remember

Questionnaire Page 3

Before we ask you the next questions, we would first like to explain the **two categories of communication channels** in more detail. In this context, communication channels are defined as media that can be used to transfer information between two or more people.

We will now ask you questions about your **experience and use of the two different communication channels**. Please note that the questions refer to your **entire life, i.e.** your private life as well as your studies, work, etc.

Written Communication Channels	This refers to communication channels, such as email, SMS, WhatsApp (without voice messages, video calls, etc.), with which **messages** are **only** transmitted **using text and emojis.** Sounds and images cannot be transmitted using these communication channels.
Audiovisual Communication Channels	This refers to communication channels, such as video conferencing, Facetime, or video calls, with which **messages** are transmitted **via sounds and images.** These communication channels can be used to transmit information about voice pitch, pronunciation, intonation, pace of speech, posture, gestures, and facial expressions.

How often do you use written communication channels?

- Never
- At least once a year
- At least once a month
- At least once a week
- At least once a day
- Several times a day

How often do you use audiovisual communication channels?

- Never
- At least once a year
- At least once a month
- At least once a week
- At least once a day
- Several times a day

Questionnaire Page 4

Please rate the extent to which you agree with the following statements.	Do not agree at all	Rather disagree	Somewhat agree	Rather agree	Fully agree
I have a lot of experience with written communication channels					
I have a lot of experience with audiovisual communication channels					

Questionnaire Page 5

In the following, you will be asked questions about the **handling of digital systems**. Digital systems are all **digital applications (e.g., software or apps)** and **all digital devices (e.g., computers or smartphones)**.

Please rate the extent to which you agree with the following statements.	Strongly disagree	Disagree	Slightly disagree	Slightly agree	Agree	Strongly agree
I can operate digital systems.						
I am good at using digital systems.						
I quickly learn when it comes to using digital systems.						
It is easy for me to get familiar with new digital systems.						
I have always been good at using digital systems.						
I can communicate information through various media formats (text, image, video, sound ...).						
Careful processing is important. Please click on “Slightly agree” for this question.						
I am good at collaborating with others through digital systems.						
I quickly learn which communication medium (text, audio, video, sound ...) has to be used for editing a task.						
It is easy for me to spread information through digital systems.						

Questionnaire Page 6

The next questions are about **online and mobile communication**. This refers to **communication** taking place over the **Internet or mobile networks**, using **mobile phones, computers, tablets or other digital devices**.

To what extent would you say that online and mobile communication …	Not at all	Completely
... makes people feel closer to one another? ("closer" in the sense of emotional rather than physical proximity)		
... makes work and personal life interrupt each other?		
... makes it easy to coordinate and manage activities?		
... undermines personal privacy?		
... exposes people to misinformation?		

Questionnaire Page 7

How well do the following statements describe your personality? I see myself as someone who …	Disagree strongly	Disagree a little	Neither agree nor disagree	Agree a little	Agree strongly
… is reserved.					
… is generally trusting					
… tends to be lazy					
… is relaxed, handles stress well					
… has few artistic interests					
… is outgoing, sociable					
… tends to find fault with others					
… does a thorough job					
… gets nervous easily.					
… has an active imagination					

Questionnaire Page 8

The following statements may apply more or less to you. To what extent do you think each statement applies to you personally?	Does not apply at all	Applies a little	Applies somewhat	Applies mostly	Applies completely
I am convinced that most people have good intentions.					
Careful processing is important. Please click on "Applies mostly" here.					
You can't rely on anyone these days.					
In general, people can be trusted.					

Questionnaire Page 9

Please give us a little more information about you before we conclude the experiment.

Please enter your gender.

- Male
- Female
- Miscellaneous
- Not specified

How old are you?

- 18 - 20
- 21 - 25
- 26 - 30
- 31 - 35
- 36 - 40
- 41 - 50
- 51 - 60
- 61 - 70
- Not specified

What is your highest educational qualification?

- Without a general school-leaving certificate
- Secondary school leaving certificate
- Advanced secondary school leaving certificate
- Advanced technical college or university entrance qualification
- University degree (Bachelor, ...)
- University degree (Master, Diplom, Magister, ...)
- Doctor/PhD
- Other [Open text field]

Have you already taken part in other experiments?

- Yes
- No

Questionnaire Page 10

Please indicate how much **attention** you have paid to this survey. You will receive your **payoff regardless of your answer to this question**. We appreciate your honesty!

	very little	little	some	much	very much
I have paid ____ attention to this survey.					

Questionnaire Page 11

Thank you for your participation! If you have any suggestions or would like to tell us something, please write to us here. Please do not enter any personal data (including third-party data) in this field:

[Text field]

Result Part 1

Before the experiment is finished, we would like to give you some information about your results in rounds 1 - 6 in Part 1.

The following table gives you an overview.

Round	Number assigned by the computer	Your message to Participant B	Text message or video message
Round 1	X	The assigned number is Y	
Round 2	X	The assigned number is Y	Text message
Round 3	X	The assigned number is Y	Video message
Round 4	X	The assigned number is Y	Video message
Round 5	X	The assigned number is Y	Video message
Round 6	X	The assigned number is Y	Text message

Thank you for your participation!

We would like to thank you very much for your help. Your answers have been saved.

You will receive £1.80 for carefully completing the questionnaire in Part 2. For the experimental task in Part 1, you will receive a payoff depending on your decisions and the decisions of Participant A. After we have evaluated your decisions, you will receive this payoff as a bonus.

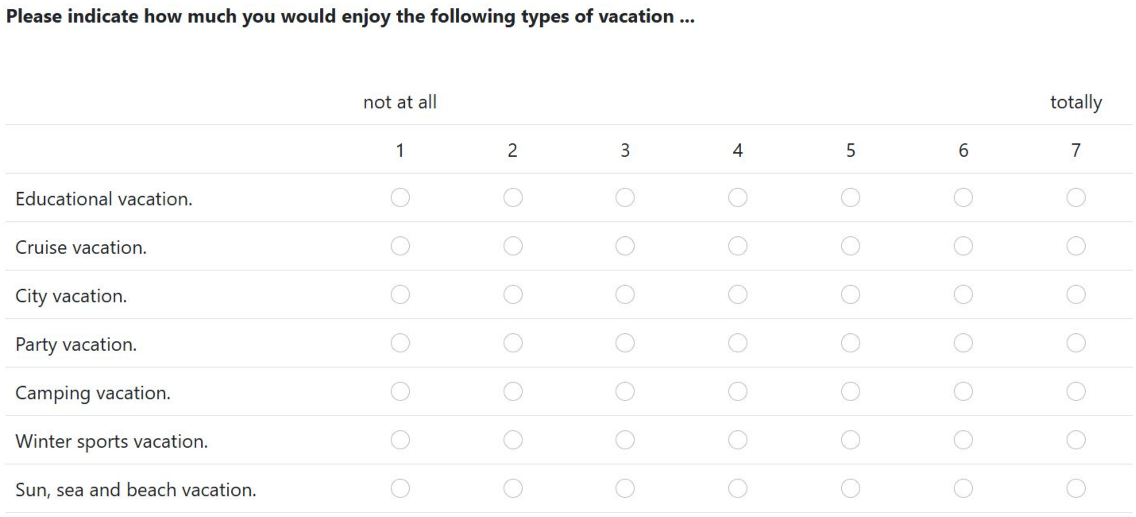
Before the payoff is made, we will check whether you have answered the questions completely and carefully. Participants who fail to do so will not receive a payoff.

Please click on the following link to complete the study.

Complete study

B Appendix for Study III

B.1 Tables and Figures


Please indicate how much you would enjoy the following types of vacation ...

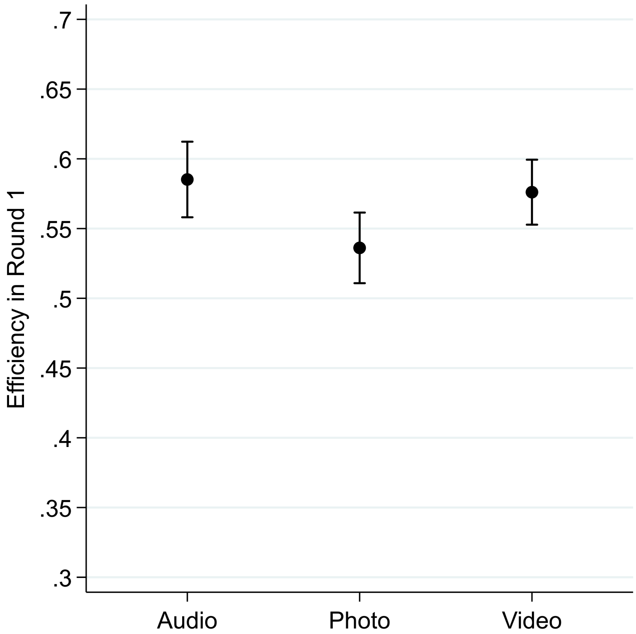
	not at all						totally
	1	2	3	4	5	6	7
Educational vacation.	○	○	○	○	○	○	○
Cruise vacation.	○	○	○	○	○	○	○
City vacation.	○	○	○	○	○	○	○
Party vacation.	○	○	○	○	○	○	○
Camping vacation.	○	○	○	○	○	○	○
Winter sports vacation.	○	○	○	○	○	○	○
Sun, sea and beach vacation.	○	○	○	○	○	○	○

Figure B.1: Rating of different vacations

Dep. Var.:	*Team Wasted Effort*		*Team Minimum Effort*	
	(1)	(2)	(3)	(4)
Photo Treatment	0.291	0.304	-0.321*	-0.344*
	(0.183)	(0.185)	(0.188)	(0.188)
Video Treatment	0.164	0.175	0.021	0.005
	(0.175)	(0.179)	(0.190)	(0.192)
Homophily		0.507		0.377
		(0.658)		(0.660)
Team Cohesion Part 1		-0.029		0.034
		(0.096)		(0.103)
Team Risk Aversion		0.022		-0.065
		(0.054)		(0.057)
Observations	204	204	204	204
Photo Treatment = Video Treatment	0.465	0.463	0.048	0.044

Robust standard errors clustered at the team level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.1: Ordered probit regressions with *Team Wasted Effort* and *Team Minimum Effort* in Round 1 as dependent variables



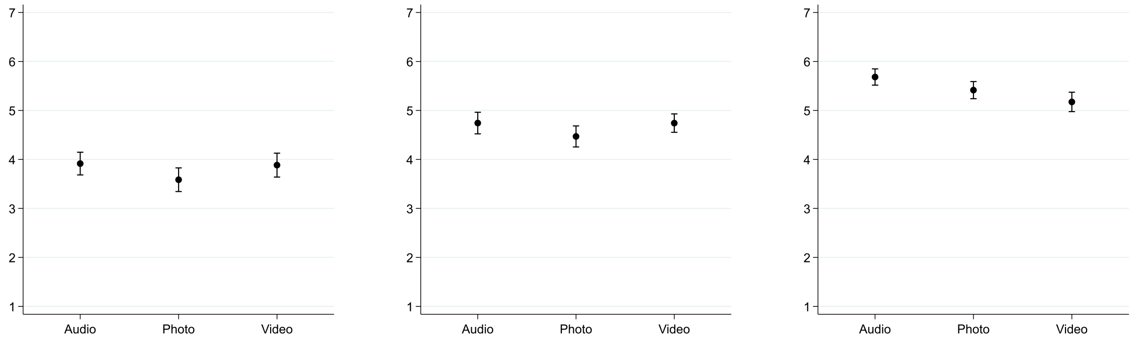
Note: Dots indicate averages and whiskers indicate 95% confidence intervals.

Figure B.2: *Efficiency* in Round 1 by treatment

Dep. Var.: *Team Wasted Effort*	(1)	(2)
Photo Treatment	0.497	0.919
	(1.569)	(1.557)
Video Treatment	0.227	0.461
	(1.679)	(1.669)
Round	-1.655***	-1.655***
	(0.133)	(0.133)
Homophily		7.846
		(5.470)
Team Cohesion Part 1		-1.159
		(0.849)
Team Risk Aversion		0.603
		(0.493)
Constant	26.280***	21.562***
	(1.395)	(6.228)
Observations	2040	2040

Robust standard errors clustered at the team level in parentheses,
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2: Linear panel regressions (random effects) with *Team Wasted Effort* as dependent variable



(a) Team Cohesion Part 2 (b) Team Human Presence (c) Team Anonymity

Note: Dots indicate means and whiskers indicate 95% confidence intervals.

Figure B.3: *Team Cohesion Part 2*, *Team Human Presence* and *Team Anonymity* by treatment

Variable	(1)	(2)	(3)	(4)
(1) Team Minimum Effort Round 1				
(2) Team Belief Minimum Round 1	0.648***			
(3) Team Cohesion Part 2	0.214***	0.267***		
(4) Team Anonymity	-0.059	-0.034	-0.083	
(5) Team Human Presence	0.155**	0.132*	0.689***	-0.186***

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Pairwise Pearson's correlations at the team level ($n = 204$)

B.2 Follow-up Study to Elicit Exogenous Ratings of Subjects' Selfies

In the follow-up study, we asked subjects who had not participated in any part of this study before to rate the selfies (taken in Part 1) of subjects in the *Photo* and *Video* treatment. The raters were recruited from the same Prolific subject pool as our main study subjects, using the same screeners.[1] We randomly allocated the 411 selfies into sets of ten, while ensuring a 50-50 split between the men's and women's selfies (44 sets). To ensure an equal gender split, some selfies were used in two sets. Four random orders of the selfies were generated for each set (176 subsets) (Todorov et al., 2005); thus, the effects of the order of selfies within a set should be limited.[2] To achieve a reliable rating database, we ensured that each of the 44 unique sets (of 10 selfies) was rated by a minimum of 24 different raters (12 men, 12 women), which results in a required sample of 1056 subjects.[3]

Each rater was randomly assigned to one set and asked to rate the subjects displayed in the selfies according to their attractiveness (1: Attractive - 7: Not attractive), trustworthiness (1: Trustworthy - 7: Not trustworthy), intelligence (1: Not intelligent - 7: Intelligent), confidence (1: Not self-confident - 7:Self-confident), masculinity (1: Very feminine - 7: Very masculine), competence (1: Not competent - 7: Competent), and predictability (1: Predictable - 7: Not predictable) on a 7-point Likert scale, following Zylbersztejn et al. (2024a). After rating the ten selfies on all seven dimensions, we asked subjects whether they knew any of the persons they were shown and elicited demographic data. Subjects received a fixed payment of £2.00. Overall, 1062 raters (530 men, 528 women, 4 other) successfully completed the follow-up study,[4] resulting in 24 to 49 raters per selfie.

For the analysis, ratings for attractiveness, trustworthiness, and predictability were reverse-coded. For each subject in the *Photo* and *Video* treatment, the mean attractiveness rating is calculated and used for subject-level analysis. For team-level analysis, the *Team Attractiveness* is the team average of the ratings on subjects' attractiveness. The same approach was used to compute *Team Trustworthiness*, *Team Intelligence*, *Team Confidence*, *Team Masculinity*, *Team Competence*, and *Team Predictability*. In addition, *Team Variance Attractiveness* describes the variance of team members' attractiveness scores to better capture the heterogeneity of team members. While the analysis of *Team Attractiveness* is discussed in Section 4.5, *Team Variance Attractiveness* is investigated in this appendix.

We included the *Team Variance Attractiveness* as a control in the OLS regressions (see Table B.6 in Appendix B.2). The treatment differences between *Photo* and *Video* on cooperation remained significant at the 5% level (see Model (6)). Therefore, *Team Variance Attractiveness* cannot explain the treatment differences. Still, *Team Variance Attractiveness* has a significant negative effect on cooperation in the *Photo* treatment at the 10% level. A sub-group analysis of teams with a *Team Variance Attractiveness* above or below the median revealed that only for teams with a *Team Variance Attractiveness* above the median, treatment differences are

[1] With the exception of the agreement to record videos.

[2] The only restriction was that an order should not start with all five selfies of the same gender.

[3] Selfies included in two sets, therefore had a minimum of 48 ratings, and each subset was rated by at least three men and three women.

[4] A total of 1099 subjects completed the study, but 37 subjects had to be excluded because they reported technical problems (30), knew at least one of the persons they were shown (4), or failed our attention check (3).

significant (two-sided Mann-Whitney U test, $p = 0.047$). OLS regressions in these sub-groups confirmed the results. Significant treatment differences are only observed in the sub-group with teams with an above-median variance in team members' attractiveness scores (see Table B.8).

	(1) Photo	(2) Video	(1) vs (2)
Attractiveness	3.75	3.87	$p = 0.044$
	(0.61)	(0.60)	
Trustworthiness	4.09	4.19	$p = 0.033$
	(0.46)	(0.48)	
Intelligence	4.54	4.63	$p = 0.148$
	(0.52)	(0.50)	
Confidence	4.34	4.56	$p = 0.001$
	(0.67)	(0.64)	
Masculinity	4.19	4.06	$p = 0.267$
	(1.32)	(1.41)	
Competence	4.61	4.71	$p = 0.040$
	(0.48)	(0.46)	
Predictability	4.01	4.09	$p = 0.029$
	(0.37)	(0.37)	

Standard deviations are in parentheses. The last column reports results from pairwise statistical comparisons based on two-sided Mann-Whitney U tests.

Table B.4: Summary statistics of individual averages for exogenous selfie ratings and pairwise treatment comparisons based on non-parametric tests

Variable	(1)	(2)	(3)	(4)	(5)	(6)
(1) Team Attractiveness						
(2) Team Trustworthiness	0.74***					
(3) Team Intelligence	0.62***	0.61***				
(4) Team Confidence	0.57***	0.48***	0.54***			
(5) Team Masculinity	-0.40***	-0.53***	-0.17**	-0.16*		
(6) Team Competence	0.64***	0.65***	0.90***	0.69***	-0.15*	
(7) Team Predictability	0.37***	0.59***	0.43***	0.19**	-0.37***	0.42***

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: Pairwise Pearson's correlation coefficients at the team level ($n = 137$) of exogenous selfie ratings

Dep. Var. by Column:	*Team Wasted Effort*			*Team Minimum Effort*		
	(1)	(2)	(3)	(4)	(5)	(6)
Video Treatment	-1.861	-1.571	-2.078	2.575**	2.517*	2.655**
	(2.475)	(2.533)	(2.537)	(1.294)	(1.283)	(1.292)
Homophily		16.754	17.294		2.375	1.429
		(11.328)	(11.662)		(6.398)	(6.318)
Team Cohesion Part 1		-1.733	-1.463		0.470	0.488
		(1.572)	(1.580)		(0.842)	(0.822)
Team Risk Aversion		0.391	0.197		-0.303	-0.211
		(0.873)	(0.871)		(0.477)	(0.475)
Team Attractiveness		-4.867			1.411	
		(3.308)			(1.749)	
Team Variance Attractiveness			1.388			-3.773*
			(4.483)			(2.239)
Constant	28.382***	40.236**	21.249*	12.353***	5.429	12.035*
	(1.846)	(17.434)	(12.304)	(0.916)	(9.035)	(6.307)
Observations	137	137	137	137	137	137

Robust standard errors clustered at the team level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: OLS regressions with *Team Wasted Effort* and *Team Minimum Effort* in Round 1 as dependent variables, the *Team Attractiveness* or *Team Variance Attractiveness* as controls and *Photo* as baseline

Dep. Var.: Team Minimum Effort	*below-median*		*above-median*	
	(1)	(2)	(3)	(4)
Video Treatment	3.754**	3.574*	1.282	1.001
	(1.720)	(1.794)	(1.885)	(1.830)
Homophily		14.049*		-7.774
		(7.800)		(9.511)
Team Cohesion Part 1		1.345		0.259
		(1.188)		(1.194)
Team Risk Aversion		-0.506		-0.295
		(0.668)		(0.749)
Team Attractiveness		-2.436		3.784
		(4.081)		(3.614)
Constant	11.579***	8.066	13.333***	4.129
	(1.283)	(14.409)	(1.295)	(20.840)
Observations	68	68	69	69

Robust standard errors clustered at the team level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.7: OLS regression of sub-groups (below- and above-median in *Team Attractiveness*) with *Team Minimum Effort* in Round 1 as dependent variables, the *Team Attractiveness* as control and *Photo* as baseline

Dep. Var.: Team Minimum Effort	*below-median*		*above-median*	
	(1)	(2)	(3)	(4)
Video Treatment	1.458	1.984	4.012**	3.960**
	(1.711)	(1.705)	(1.909)	(1.955)
Homophily		-0.991		2.819
		(8.268)		(10.584)
Team Cohesion Part 1		1.584		-0.461
		(1.049)		(1.216)
Team Risk Aversion		-0.596		0.198
		(0.613)		(0.738)
Team Variance Attractiveness		-3.417		-0.207
		(11.027)		(3.493)
Constant	14.167***	12.286	10.312***	9.205
	(1.154)	(8.975)	(1.381)	(9.831)
Observations	68	68	69	69

Robust standard errors clustered at the team level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.8: OLS regression of sub-groups (below- and above-median in *Team Variance Attractiveness*) with *Team Minimum Effort* in Round 1 as dependent variables, the *Team Variance Attractiveness* as control and *Photo* as baseline

B.3 Communication Content Preparation and Analysis

Teams' audios of the communication stage were recorded during the main study, and we informed subjects about this in the instructions. Immediately after the communication stage ended, a time window of a maximum of two minutes was available to upload the recordings to our server. Meanwhile, subjects could start reading the instructions for the team decision stage. Of the 204 teams that finished the study, we obtained complete audio recordings from 201 teams (*Audio* treatment: 65, *Photo*: 67, *Video*: 69). For three teams, the connection speed of all team members did not allow for the upload to complete within two minutes, so they had to be excluded from the communication analysis.

The audio recordings were transcribed using a local version of OpenAI's Whisper (Radford et al., 2023), and speaker segmentation was done with Pyannote speaker-diarization (Bredin & Laurent, 2021). Afterwards, the transcripts were manually corrected.

For the analysis of the communication content, we used several variables related to the total amount spoken in a team, the share of speech within a team, as well as the similarity of words used between teams. For the variable *Character Count*, which was used for the total communication length, we removed punctuation (as well as the speaker assignment) and counted the total number of characters used (excluding spaces). For the share of speech, we proceeded similarly by stripping the text for each speaker of its punctuation and counting the characters. The speech share of each team member was then calculated as the individual team members' character count divided by the total character count of the team. At the team level, we used the standard deviation of the speech share within a team (*Standard Deviation Speech Share*) for our analysis.

In addition, we applied natural language processing to compare the communication content between teams and treatments. We followed data preparation and analysis recommendations by Ash and Hansen (2023), Gentzkow et al. (2019), and Werner and Andres (2024). Therefore, we first used standard approaches to clean texts by deleting double spaces, lowercasing, removing stopwords such as "the" or "is", and lemmatizing to reduce words to their dictionary form (Ash & Hansen, 2023; Gentzkow et al., 2019). The resulting texts were, therefore, more comparable, while the relevant content was preserved. When analyzing data at the team level, the speaker diarization was additionally deleted. As a first step, we looked at the 20 most frequent words for each treatment separately (see Figure B.5 in Appendix B.3). On first impression, the content seemed to be similar in all treatments, as in particular, the most frequent words were the same, and vacation-related topics were frequently discussed in all treatments. To go deeper into the content analysis, we conducted a cluster analysis and topic-modeling. Compared to topic-modeling, the cluster analysis works on a vector representation of our transcripts and provides a stricter separation of transcripts, as each is assigned to one specific cluster only (Ash & Hansen, 2023).

For the cluster analysis, we followed Werner and Andres (2024) and used agglomerative clustering with an euclidean metric, a type of Hierarchical Clustering for which clusters do not need to have a specific shape. The cleaned data was transformed into numerical form using the term-frequency-inverse-document-frequency (TF-IDF), handling each transcript as a document. Next, we excluded terms present in at least 99% of documents (as they have little meaning when comparing transcripts), as well as very rare terms that are not present in at least 10% of transcripts (sparsity threshold of 90%). We compared the mean silhouette width (Rousseeuw, 1987) for different sparsity thresholds and number of clusters and used the elbow criteria to choose parameters. This led us to use the 90% threshold in combination with three clusters (mean silhouette width = 0.11).[5] We found that all but three transcripts (two from the *Audio* and one from the *Video* treatment) were assigned to the same cluster, confirming our first impression that the transcripts were very similar in content.[6]

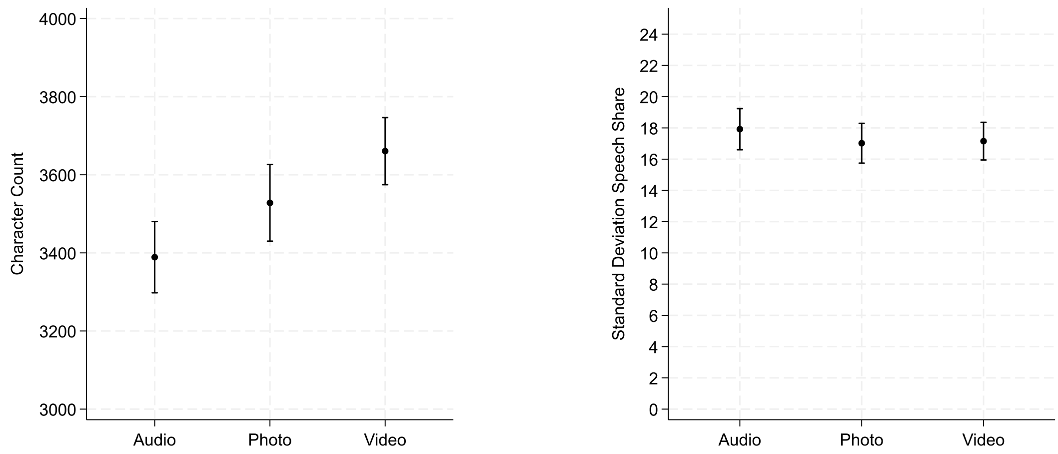
For topic-modeling we chose two different approaches. The first approach uses Latent Dirichlet Allocation (LDA) (with Gensim (Řehůřek & Sojka, 2010)). First, a dictionary of tokens was built and further enriched by bigrams. Similarly to clustering, we removed tokens that were not used in at least 10% of the transcripts or were in more than 99% of the transcripts. This dictionary and the corresponding bag-of-words model (see Ash and Hansen, 2023) were used to train the LDA model. A comparison of C_v coherence scores (Röder et al., 2015) of different topic numbers was performed. The highest coherence score was reached for choosing two topics. The topics were then assigned to the individual transcripts, which showed that both topics were present in all of the transcripts. Looking at the most important words in each topic, we found that both topics were related to vacation. Therefore, based on this topic-modeling, we again could not detect any major differences between transcripts and, thereby, treatments.

As the transcripts were manually cleaned before analysis, we had an idea of the topics that were actually discussed. Most importantly, the discussion was resolved around vacations, and this icebreaker topic was mentioned in most of the teams. Still, we also identified other topics that we then used in our second topic-modeling approach.[7] We used the BART model (Lewis et al., 2019), specifically the pre-trained bart-large-mnli version, to perform text classification on the transcripts (not the cleaned data). For each speaker segment, we implemented the zero-shot classification (Yin et al., 2019) to check whether it matched any of the topics in our predefined topic list. We first looked at the overall *Topic Number* discussed in a team and then had a closer look at whether there are differences between treatments in the topic occurrence. The average *Topic Number* in a team is 8.17 in the *Audio*, 8.18 in the *Photo*, and 9.09 in the *Video* treatment. Pairwise comparisons with the *Video* treatment showed that the average *Topic Number* discussed in a team was significantly higher in the *Video* treatment (Mann-Whitney U test: *Audio* vs. *Video*: $p = 0.004$, *Photo* vs. *Video*: $p = 0.002$). This is in line with the higher *Character Count* in the *Video* treatment and over all treatments the correlation between *Topic Number* and *Character Count* was positive and significant ($r = 0.21$***). While most of the pre-defined topics occurred similarly often in all treatments (pairwise comparisons with Mann-Whitney U tests), there were some significant differences in the frequencies of occurrence of the topics "family" (*Video* $>$ *Photo*: $p = 0.03$), "work" (*Photo* $>$ *Audio*: $p = 0.05$; *Video* $>$ *Audio*: $p = 0.003$), "study" (*Video* $>$ *Audio*: $p = 0.085$; *Video* $>$ *Photo*: $p = 0.006$), and "coordination" (*Video* $>$ *Audio*: $p = 0.02$; *Video* $>$ *Photo*: $p = 0.002$). These significant differences were in line with the overall higher number of topics discussed in the *Video* treatment. Overall, there was no clear pattern that could explain the observed differences in cooperation.

[5] A different number of clusters, lower sparsity thresholds, or different clustering methods did lower the mean silhouette width and were therefore discarded.

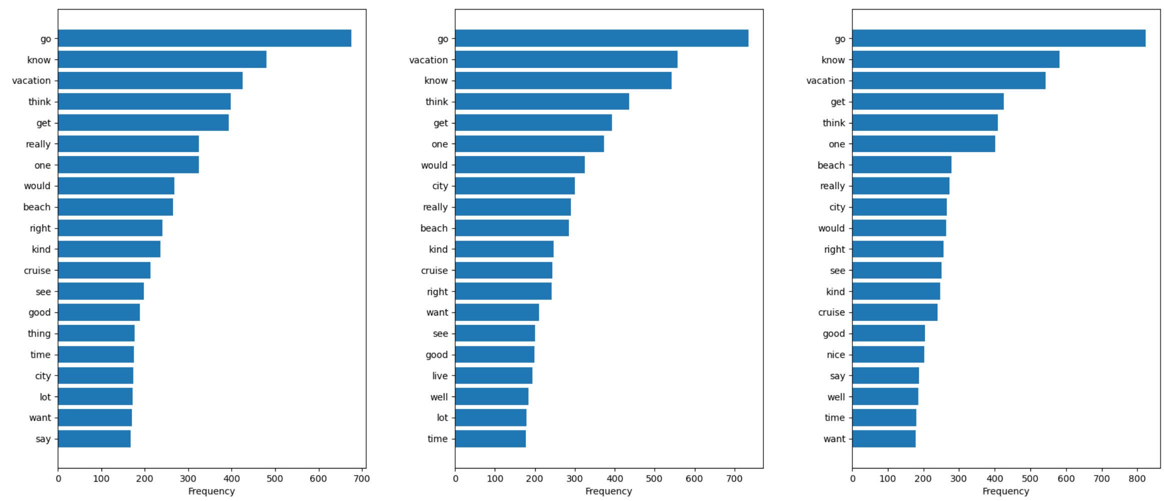
[6] A similarly high difference in cluster sizes persisted also when increasing the number of clusters and changing the sparsity threshold.

[7] Full list of topics used: vacation, politics, family, friends, career, work, sport, greeting, study, coordination, team, audio, economy, home.



(a) Character Count (b) Standard Deviation Speech Share

Figure B.4: Character Count and Standard Deviation Speech Share by treatment



(a) Audio (b) Photo (c) Video

Figure B.5: Top 20 words: Frequency by treatment

	(1) Audio	(2) Photo	(3) Video	(1) vs (2)	(1) vs (3)	(2) vs (3)
Character Count	3389	3528.15	3660.55	$p = 0.233$	$p = 0.007$	$p = 0.095$
	(653.44)	(713.35)	(633.68)			
Standard Deviation	17.92	17.02	17.15	$p = 0.44$	$p = 0.702$	$p = 0.661$
Speech Share	(9.43)	(9.26)	(8.89)			

Standard deviations are in parentheses. The last three columns report results from pairwise statistical comparisons based on two-sided Mann-Whitney U tests.

Table B.9: Summary statistics of team averages for character count and speech share and pairwise treatment comparisons based on non-parametric tests

Dep. Var.: *Team Minimum Effort*	(1)	(2)	(3)	(4)
Character Count	0.002**		0.002**	0.002**
	(0.001)		(0.001)	(0.001)
Standard Deviation Speech Share		0.061		0.057
		(0.072)		(0.075)
Homophily			3.890	4.016
			(5.102)	(5.144)
Team Cohesion Part 1			0.020	-0.010
			(0.783)	(0.784)
Team Risk Aversion			-0.362	-0.338
			(0.437)	(0.435)
Constant	6.670**	13.067***	6.636	5.577
	(3.121)	(1.270)	(6.360)	(6.362)
Observations	201	201	201	201

Robust standard errors clustered at the team level in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.10: OLS regressions with *Team Minimum Effort* in Round 1 as dependent variable, *Character Count* and Standard *Deviation Speech Share* as controls

B.4 Instructions for Part 1

Eligibility Check

Before we start, kindly note that you can only participate in Part 2 of this study if you are able and willing to use a Chrome, Edge or Opera browser. If you are not able or willing to use one of these three browsers, you cannot proceed with Part 1 of this study.

Are you able and willing to use a Chrome, Edge or Opera browser in Part 2 of this study?

- Yes
- No

Welcome to Part 1 of the Study "Virtual Team Communication"!

Thank you for your interest in our study. The goal of the study is to investigate communication in virtual teams.

This study consists of **two parts. Your participation in both parts is essential for the success of the study.**

Attention: Part 2 will be a separate study. We will invite eligible participants from part 1 (this study) for part 2. Part 2 will take place at a specific time slot which you will select during part 1. Kindly note that we cannot guarantee that you will be able to participate in part 2.

Part 1 (current study):

- Fixed compensation: GBP (£) **2.00**
- Duration: approx. **15 minutes**
- You will be asked to answer a short questionnaire and to upload a photo, and an audio recording which might be shown to other participants in part 2.

Part 2 (follow-up study at a different time slot):

- Fixed compensation: GBP (£) **6.00**
- Potential bonus: up to GBP (£) **8.00**
- Duration: approx. **35 minutes**

To take part in this study we ask for your consent to the storage of personal data (audio data and photos) in a private project on the online repository Open Science Framework (OSF), as explained in the general study information.

Important: Participation in part 1 and part 2 is only possible if this consent is given.

Information about the study and data protection

Before we start, we want to give you some information about the study and inform you about data protection.

Principal Investigators: Michelle Hörrmann, Petra Nieken, Sven Walther (Karlsruhe Institute of Technology)

Description: This is the first part of a study investigating communication in virtual teams. This first part will take approximately 15 minutes of your time.

Eligibility criteria: You are eligible to participate in the first and second part of the study if you previously agreed to share photos of your face, video, and audio with other participants and are technically and physically able to take part in a virtual conference. Additionally, you are between 18 and 65 years old, speak English fluently, have at least 100 previous submissions, and have an approval rate of at least 95%. Furthermore, your residence is in the U.S. and you have not participated in this study before.

Risk and benefits: There are no physical or emotional risks associated with the content of the study that would go beyond the risks of daily life. Your participation may improve remote work and benefit society.

Confidentiality: The information collected in this study may be published in a report or a journal article and presented to interested parties, including possibly, but not exclusively, members of editorial boards or scientific committees. We do not collect your IP address and will delete the Prolific ID from the study dataset immediately after the final payment is completed. Other information (e.g., survey responses, and time of the survey) will be kept by the researchers and may be used for future studies. To participate in this study, we require your consent to the storage of personal data (audio data and photo) in a private project on the online repository OSF for a period of ten years after study completion. During the review process, personal data (audio and photo) will be made available to journal editors and reviewers to verify our statements. An anonymized transcript of the audio recording and the anonymized research data will be made publicly available on OSF for an unlimited period of time.

Your right as a participant: Participation is entirely voluntary. You may leave the study and withdraw your consent at any time without any penalty and prejudice.

Ethics Approval: This research has been reviewed according to the ethics procedures involving human subjects by the Karlsruhe Institute of Technology (IRB number: A2024-037).

If you want further information about data protection, please click **here**. If you want to re-read or download the general study information, please click **here**.

Consent

I have understood the general information on the "Virtual Team Communication" research project as well as the instructions and consent to my participation in the project. I understand that my Prolific ID and the individual participation code will be used to verify my participation, link the data from the pre and main study, and send my payment on Prolific. I have been informed of and provided with the information on the collection of personal data, including audio data and a photo, in the "Virtual Team Communication" research project as well as the processing, usage and storage as described in the available documents. **My consent explicitly refers to the storage of personal data (audio data and photos) in a private project on the online repository OSF**, as described in the **general study information** under point 2.2.

☐ I consent to the collection, processing, storage, usage and disclosure of my personal data in the research project "Virtual Team Communication" as described above and in the general study information.

If you have consented to all of the data processing, the study will start after you have clicked yes.

- Yes, I want to participate.
- No, I do not want to participate.

Please enter your Prolific ID so that we can pay you.

Part 1: Procedure

In Part 2, you will **participate in a virtual team meeting**. We need to ensure that you **meet the technical requirements** for this part. We might also **show a photo of your face and an audio of your voice to other participants**. Note that you can only **participate if you upload a photo of your face and an audio of your voice. Otherwise, please return the study!**

General procedure:

1. Technical requirements check
2. Upload photo and audio recording
3. Pick a time slot for Part 2
4. Answer a short survey

More information on the procedure:

- For the virtual team meeting in Part 2, we need to check your technical requirements. You cannot proceed with the study and will not get any payment if you fail the technical tests.
- For the virtual team meeting in Part 2, we need a **photo of your face and an audio recording of your voice**. You cannot proceed with the study and will not get any payment if you are not taking a picture of your face and record an audio of your voice.
- You choose a time slot in which you can take part in Part 2. **Note, that we can only conduct Part 2 if we have full teams**. Thus, we **overbook time slots** in order to reduce the risk that we cannot conduct the sessions in Part 2 because participants will not attend Part 2 at the stated time slot.
- Next, you will answer survey questions for approximately 5 minutes. We collect some demographic information. In addition, we will ask you to state your opinion and preferences regarding different economic situations and decisions (there are no right or wrong answers). **It is important to note that your answers will not affect whether or not you can participate in Part 2**.
- When you have finished the study, you will receive a completion code that you will submit on the Prolific platform. The code ensures that we can send the payment to you via Prolific.
- The total time commitment is approximately 15 minutes.

Technical Test: Page 1

In Part 2, you will take part in a **virtual team meeting**. We will therefore check your technical requirements to ensure that the **audio, microphone, and video work properly.**

Audio test: To do this, first please watch and listen to the following video and then answer the question below.

Which color was mentioned in the video?

You cannot proceed with the study and will not get any payment if you fail the audio test.

- Red
- Blue
- Green
- Yellow

Technical Test: Page 2

Video test: Please watch and listen to the following video and answer the question below.

Which number was shown in the video?

You cannot proceed with the study and will not get any payment if you fail the video test.

- 1
- 2
- 3
- 4
- 5

Technical Test: Page 3

<u>Microphone test</u>

We also want to ensure that your microphone is working properly. To test your microphone, please record the sentence below **and use your own voice**.

If you participate in Part 2 of this study, the audio recording you submit might be shown to other participants in Part 2. If you do not participate in Part 2, we will **delete the audio recording**.

Please press the "START" button to start the recording. To stop the recording, please press the "STOP" button. This audio recording will then be displayed under the buttons and **you can listen to it again**.

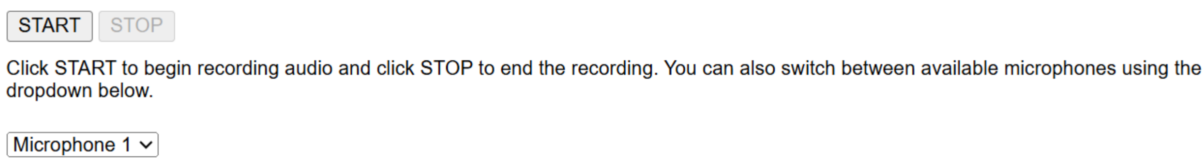
If you want to make a new audio recording, simply press the "START" button again. The last audio recording will then be overwritten by the new recording.

When you are satisfied with a recording, **submit it by clicking on the "Next" button**.

We will **review the submitted audio recording** and only if the **recording is sufficient**, you **may participate in Part 2 of the study**.

Please now record <u>yourself</u> and read the following sentence:

"This is a test for the virtual team communication study."



Please select "Yes" to confirm that you recorded an audio of yourself and will submit this audio recording by clicking on the "Next" button.

If you do not want to submit an audio recording, select "No". In this case you cannot proceed with this study and will not get any payment. Even if you already recorded an audio, we are going to delete it within the next few days.

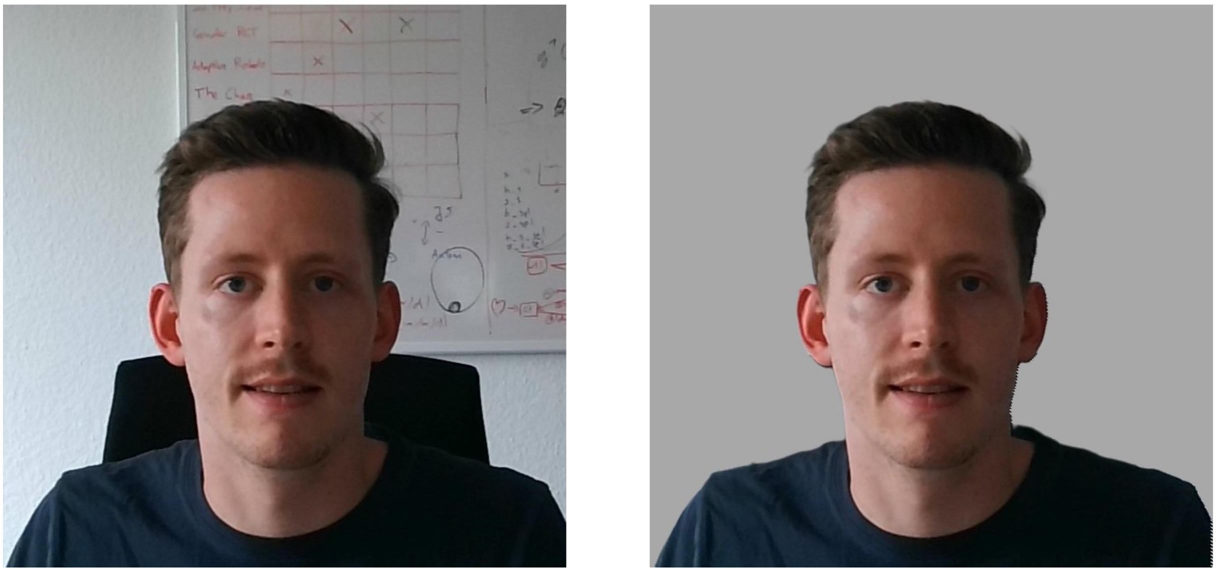
- Yes, I want to participate.
- No, I do not want to participate.

Photo Instructions

We will now explain how to take a photo and how we will edit the photo you take.

The photo you take, **might be shown to other participants in Part 2** of the study. Before it might be used in Part 2, we will **edit the background of the photo. We will only replace the background with a gray background**. This ensures, that objects or other things in the background will not be visible in the photo you take. See below an example on how we edit the photo. If necessary, we will also edit the photo to a square format.

Original Photo | Edited Photo



<u>Brief instructions to take a photo:</u>

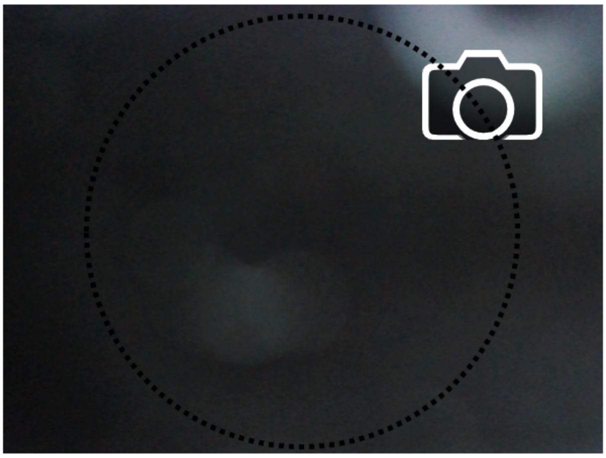
Below you see a **live image**. If not please allow your browser to use the camera. **To take a photo, you just need to click on any part of the live image**. Then you will see the photo to the right of the live image.

If you click on the **"Next" button**, you submit the **latest photo** that you see to the right of the live image, and it is downloaded. The **downloaded files are only stored internally on a KIT server and not on an external server**.

Note that the photo is **only downloaded via the "Next" button** and not automatically when you take a photo. Thus, you can test it as often as you want and **only the latest photo will be downloaded when you click on the "Next" button**.

The background of this photo is then edited and the edited photo might then be used in Part 2. **The original photo is deleted immediately after editing**.

Please now take a photo and place your face in the center of the dotted circle.



Click on the video or the camera icon to take a snapshot. You can also switch between available cameras using the dropdown below.

Camera 1

Please select "Yes" to confirm that you took a photo of yourself and will submit this photo by clicking on the "Next" button

If you do not want to submit a photo, select "No". In this case you cannot proceed with this study and will not get any payment. Even if you already took a photo, we are going to delete it within the next few days.

- Yes, I want to participate.
- No, I do not want to participate.

Time slots for Part 2

As stated in the beginning, **the main goal of this study is to recruit participants for Part 2 and let them sign up for one specific time slot in which Part 2 will be conducted**. Part 2 will last approximately **35 minutes and you can earn up to £ 14.00**.

Part 2 with the name "Virtual Communication – Part 2 (£ 6.00 + a potential bonus of up to £ 8.00)" will open directly at the beginning of the time slots below. For example: If you decide to choose a time slot "10:00 AM – 10:35 AM", we activate Part 2 at 10:00 AM.

In order for you to start the study immediately, we will also send you a **Prolific message with a participation link!**

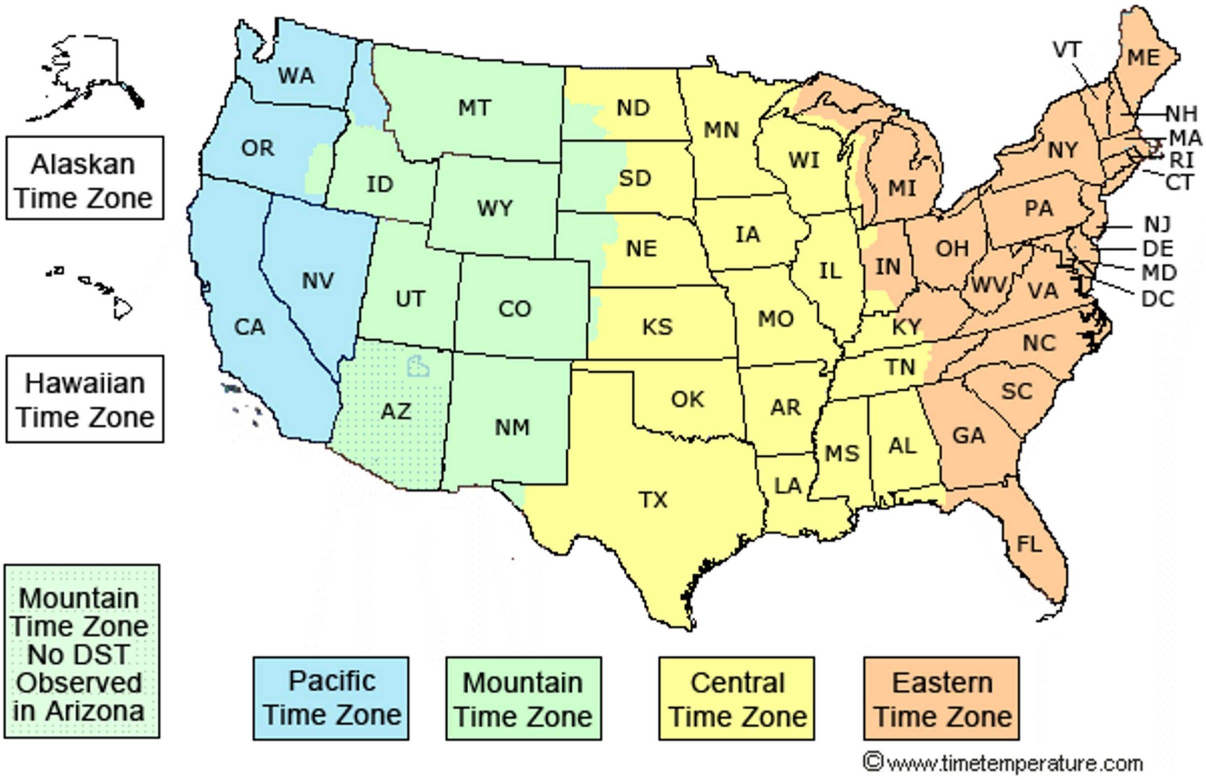
As we need full teams to conduct the sessions, we will overbook the time slots. Thus, even if you decide to choose a time slot below, it is possible that you cannot participate in Part 2 because other participants started Part 2 earlier and the slots are already taken. However, it can happen that participants drop out early during Part 2. In that case, a place will be open for the participants we overbooked.

Please only state time slots for which you are absolutely sure that you can participate.

If you are not absolutely sure, please select **"No time slot is possible"**.

In this case, you cannot participate in Part 2, however, **Part 1 will proceed for you and you will get the fix payment for this study independent of this question**. We might send you small studies later, **to give you more possible time slots to participate in Part 2**.

Note: In the USA there are several time zones. **All sessions that you can choose below are based on the Eastern Time Zone**. Below you can find a **picture with all time zones in the USA**, if you are not living in the Eastern Time Zone.



	Hawaiian Time Zone	Alaskan Time Zone	Pacific Time Zone	Mountain Time Zone	Central Time Zone	Eastern Time Zone
Difference to Eastern Time Zone	- 5:00	- 4:00	- 3:00	- 2:00	- 1:00	- 0:00
Example	12:00 PM	1:00 PM	2:00 PM	3:00 PM	4:00 PM	5:00 PM

Which session would you like to attend?

Please only state time slots for which you are absolutely sure that you can participate.

If you are not absolutely sure, please select “No time slot possible”.

- September xx, 2024: 12:00 PM - 12:35 PM (Eastern Time Zone)
- No time slot possible

It is very important for our research that you participate in Part 2 if you have chosen a time slot. Therefore, please confirm that you wrote down the time slot in which you would like to participate in Part 2.

- Yes, I confirm that I wrote down the time slot.
- No, I do not confirm because no time slot was possible.

Survey: Page 1

In the following, you will answer a short questionnaire.

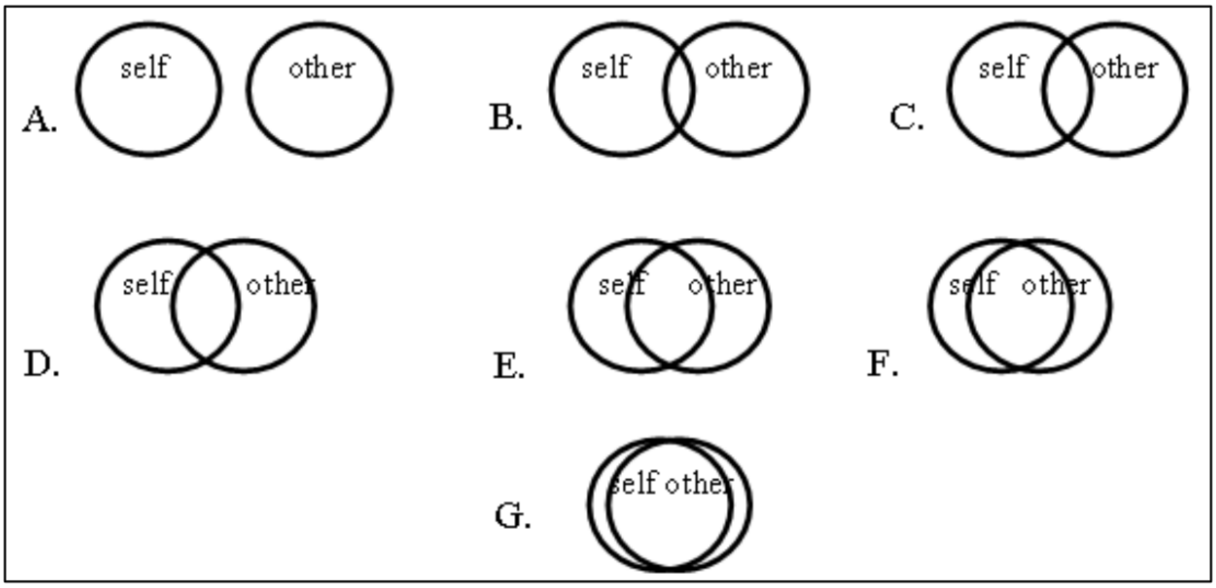
All participants for this pre-study and the main study will be recruited over the Prolific platform and we used the following criteria:

- Between 18 and 65 years old
- Fluent in English
- Approval rate of at least 95
- At least 100 previously submitted tasks on the platform
- Living in the United States

Assume now that you form a **random team with two other Prolific participants**. Please **think of one** of these two Prolific participants in your team and **answer the following two questions**.

Please, look at the circles diagram provided below. Then, consider which of these pairs of circles would best represent your connection with this participant from your team. By selecting the appropriate letter below, please indicate to what extent you and this participant would be connected.

A.	B.	C.	D.	E.	F.	G.



Please, select the appropriate number below to indicate to what extent you would use the term "WE" to characterize you and this participant from your team.

1: Not at all	2	3	4	5	6	7: Very much so

Survey: Page 2

In general, how willing or unwilling are you to take risks?

Use a scale from 0 to 10, where 0 means "completely unwilling to take risks" and a 10 means you are "very willing to take risks". You can also use any numbers between 0 and 10 to indicate where you fall on the scale, like 0, 1, 2, 3, 4, 5, 6, 7, 8, 9, 10.

0: completely unwilling to take risks	1	2	3	4	5	6	7	8	9	10: very willing to take risks

Survey: Page 3

How well does each of the following statements describe you as a person?

Indicate your answer on a scale from 0 to 10. A 0 means "does not describe me at all," and a 10 means "describes me perfectly."

When someone does me a favor I am willing to return it.

0: does not describe me at all	1	2	3	4	5	6	7	8	9	10: describes me perfectly

If I am treated very unjustly, I will take revenge at the first occasion, even if there is a cost to do so.

0: does not describe me at all	1	2	3	4	5	6	7	8	9	10: describes me perfectly

Survey: Page 4

We now ask you for your willingness to act in a certain way.

Indicate your answer on a scale from 0 to 10. A 0 means "completely unwilling to do so," and a 10 means "very willing to do so."

How willing are you to punish someone who treats <u>you</u> unfairly, even if there may be costs for you?

0: completely unwilling to do so	1	2	3	4	5	6	7	8	9	10: very willing to do so

How willing are you to punish someone who treats <u>others</u> unfairly, even if there may be costs for you?

0: completely unwilling to do so	1	2	3	4	5	6	7	8	9	10: very willing to do so

How willing are you to give to good causes without expecting anything in return?

0: completely unwilling to do so	1	2	3	4	5	6	7	8	9	10: very willing to do so

Survey: Page 5

Please think about what you would do in the following situation.

You are in an area you are not familiar with, and you realize you lost your way. You ask a stranger for directions. The stranger offers to take you to your destination. Helping you costs the stranger about 20 US dollar in total. However, the stranger says he or she does not want any money from you. You have six presents with you. The cheapest present costs 5 US dollar, the most expensive one costs 30 US dollar.

Do you give one of the presents to the stranger as a "thank-you"- gift?

If so, which present do you give to the stranger?

- No present
- The present worth 5 US dollar
- The present worth 10 US dollar
- The present worth 15 US dollar
- The present worth 20 US dollar
- The present worth 25 US dollar
- The present worth 30 US dollar

Survey: Page 6

Imagine the following situation: Today you unexpectedly received 1,000 US dollar.

How much of this amount would you donate to a good cause? (State an integer between 0 and 1,000.) ______

Survey: Page 7

The following statements may apply more or less to you. To what extent do you think each statement applies to you personally?

	does not apply at all	applies a bit	applies somewhat	applies mostly	applies completely
I am convinced that most people have good intentions.					
It is important to process the survey carefully. Please click on "applies a bit" here.					
You cannot rely on anyone these days.					
In general, people can be trusted.					

Survey: Page 8

Below you will find a series of statements. This is about your own opinion. There are therefore no “right” or “wrong” answers. Answer in the way that best applies to you. Please answer the statements with regard to your personal attitude. Think either about your education, your studies or your work.

	disagree	somewhat disagree	neither	somewhat agree	strongly agree
I find working on team projects to be very satisfying.					
I would rather take action on my own than to wait around for others’ input.					
I prefer to complete a task from beginning to end with no assistance from others.					
Teams usually work very effectively.					
I think it is usually better to take the bull by the horns and do something yourself, rather than wait to get input from others.					
For most tasks, I would rather work alone than as part of a group.					
I can usually perform better when I work on my own.					
I find that it is often more productive to work on my own than with others.					
I like working with others.					
I do not like having to rely on other team members..					
When I disagree with other team members, I tend to go with my own gut feelings.					
Careful processing is important. Please click on “strongly agree” here.					

	disagree	somewhat disagree	neither	somewhat agree	strongly agree
When I have a different opinion than another group member, I usually try to stick with my own opinion.					
It is important to stick to your own decisions, even when others around you are trying to get you to change.					
When others disagree, it is important to hold one's own ground and not give in.					
I think that even when working in a team, one should always do what one think is right.					
When I am convinced of something, I stick to my opinion, whatever other team members say.					

Survey: Page 9

How many days per month do you typically use Prolific?

On the days when you use Prolific, how many hours do you typically spend on it?

Is Prolific your primary source of income?

- Yes
- No
- Other, please specify

Survey: Page 10

Which U.S. state do you currently live in?

Please Choose

How do you identify?

- Male
- Female
- Other

How old are you?

What is the highest level of education you have completed?

- less than High School
- High School/GED
- Some College
- 2-year College degree
- 4-year College degree
- Master's degree
- Doctoral degree or Professional Degree (JD, MD)

What ethnic group do you belong to?

- White
- Black
- Asian
- Mixed
- Other
- Prefer not to say

Thank you for participating in our study!

We would like to thank you very much for helping us.

Please paste the participation code below on the Prolific website: xx, or click here to finish the study and be redirected to Prolific.

If you have chosen a time slot during which you would like to take part in Part 2, **it is very important for our research that you take part in Part 2.**

In order for you to start the study immediately, we will send you **a Prolific message with a participation link!**

B.5 Instructions for Part 2

Welcome to Part 2 of the Study "Virtual Team Communication"!

You participated in part 1 of the study "Virtual Team Communication" and now take part in part 2.

Thank you for your interest in our study and for coming back to take part in the second part of study. The goal of the study is to investigate communication in virtual teams.

The study consists of **two parts.**

In the second part, which you are about to start, you will be paid a fixed compensation of **GBP (£) 6.00** for participating and receive a potential bonus of up to **£8.00**. This part will take approximately **35 minutes**.

To take part in this study we ask for your consent to the storage of personal data (audio data and photos) in a private project on the online repository Open Science Framework (OSF), as explained in the general study information.

Important: Participation in this study is only possible if this consent is given. To participate in the second part of the study, please, on the next pages, confirm again below that you consent to this usage of your data.

Information about the study and data protection

Before we start, we want to give you some information about the study and inform you about data protection.

Principal Investigators: Michelle Hörrmann, Petra Nieken, Sven Walther (Karlsruhe Institute of Technology)

Description: We study communication in virtual teams. This second part of the study will take approximately 35 minutes of your time.

Eligibility criteria: You are eligible to participate if you participated in the first part of our study and have not participated in the second part of this study before.

Risk and benefits: There are no physical or emotional risks associated with the content of the study that would go beyond the risks of daily life. Your participation may improve remote work and benefit society.

Confidentiality: The information collected in this study may be published in a report or a journal article and presented to interested parties, including possibly, but not exclusively, members of editorial boards or scientific committees. We do not collect your IP address and will delete the Prolific ID from the study dataset immediately after the final payment is completed. Other information (e.g., survey responses, and time of the survey) will be kept by the researchers and may be used for future studies. To participate in this study, we require your consent to the storage of personal data (audio data and photo) in a private project on the online repository OSF for a period of ten years after study completion. During the review process, personal data (audio and photo) will be made available to journal editors and reviewers to verify our statements. An anonymized transcript of the audio recording and the anonymized research data will be made publicly available on OSF for an unlimited period of time.

Your right as a participant: Participation is entirely voluntary. You may leave the study and withdraw your consent at any time without any penalty and prejudice.

Ethics Approval: This research has been reviewed according to the ethics procedures involving human subjects by the Karlsruhe Institute of Technology (IRB number: A2024-037).

If you want further information about data protection, please click **here**. If you want to re-read or download the general study information, please click **here**.

To start with the study please now confirm again that you consent to the previously described usage of your data.

Consent

I have understood the general information on the "Virtual Team Communication" research project as well as the instructions and consent to my participation in the project. I understand that my Prolific ID and the individual participation code will be used to verify my participation, link the data from the pre- and main study, and send my payment on Prolific. I have been informed of and provided with the information on the collection of personal data, including audio data and a photo, in the "Virtual Team Communication" research project as well as the processing, usage and storage as described in the available documents. **My consent explicitly refers to the storage of personal data (audio data and photos) in a private project on the online repository OSF**, as described in the **general study information** under point 2.2.

☐ I consent to the collection, processing, storage, usage and disclosure of my personal data in the research project "Virtual Team Communication" as described above and in the general study information.

If you have given your consent on the last page, the study will start after you have clicked yes.

- Yes, I want to participate.
- No, I do not want to participate.

Please enter your Prolific ID so that we can pay you.

General information and procedure

In Part 2 of this study, we will be investigating how people make decisions in different situations.

The study consists of **three stages and a questionnaire** and will take about **35 minutes**.

In this study, you **will form a team with two other participants. Your team will remain the same throughout the study**. This means that your team will always consist of the same members.

In Stage 1, you will be asked to answer various questions and to carry out technical tests in order to **check whether you can proceed with the study**. Therefore, within Part 1, **you might not be able to proceed with the study and will have to return your submission**.

In Stage 2, you will get to know your team members in a **virtual team meeting**. You will have 7 minutes to get to know each other and exchange ideas.

In Stage 3, you will receive detailed information about the **task and the payoff** from this stage onwards. As part of this task, you will form a team again with your team members from Stage 2.

Payoff

We use the "ECU" currency for the study. It will be converted into £(GBP) for payoff at the end of the study. **In this study, 10 ECU = £0.20.**

If you are not able to proceed with the study during Stage 1, you will not get any payoff and need to return your submission.

You will receive a **fixed payment of £6.00 for your participation in the study**.

You may also receive an additional bonus payment in Stage 3. You will receive more detailed information before the start of Stage 3.

Note: None of the participants will know how much another participant has been paid.

Stage 1

Study check: Page 1

On the following pages, we will check whether you can participate in Stage 2.

Before you can continue, please confirm the following: (If you do not confirm all three statements, you cannot proceed with this study.)

☐ **I AM WILLING TO INTERACT WITH OTHER PARTICIPANTS IN A VIRTUAL TEAM MEETING.**

☐ **I UNDERSTAND THAT I WILL ONLY BE PAID IF I COMPLETE THE WHOLE STUDY.**

☐ **MY CAMERA AND MICROPHONE SEEM TO WORK FINE.**

Study check: Page 2 - Browser check

You can only participate in Stage 2 of this study if you use **a Chrome, an Edge or an Opera browser**.

If you are currently not using a Chrome, an Edge or an Opera browser, **please copy the link of the current page and open it in a Chrome, an Edge or an Opera browser**. Otherwise, you cannot proceed with this study.

Afterward, please then click on the "Next" button to proceed with the study.

Study check: Page 3 - Technical check

<u>Audio and video test</u>

Before taking part in the audio and video test, please watch and listen first of all to the following video and then answer the two questions.
(You cannot proceed with this study, if you do not pass the audio and video test.)

Which color was mentioned in the video?

- Red
- Blue
- Green
- Yellow

Which number was shown in the video?

- 1
- 2
- 3
- 4
- 5

Study check: Page 4 - Photo check

Before we start with the virtual team meeting, look at the photo below and answer the question below the picture.

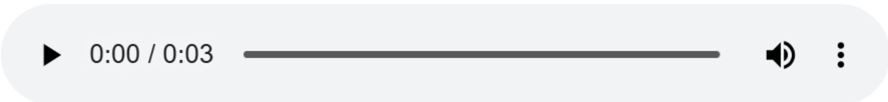
Picture

Is the above picture a photo of your face that you submitted in Part 1 of this study?

- Yes, that is a photo of my face.
- No, that is not a photo of my face.

Study check: Page 5 - Audio recording check

Before we start with the virtual team meeting, listen to the audio recording below and answer the question below the audio.



Is the above audio recording the one you recorded and submitted in Part 1 of this study?

- Yes, that is an audio recording of me which I recorded and submitted in Part 1 of this study.
- No, that is not an audio recording of me which I recorded and submitted in Part 1 of this study.

Please wait

There are currently not enough participants in order to create a team of three participants.

Number of participants currently needed to create a team: x

While waiting, you can start reading the instructions of Stage 2 below.

This wait page will automatically refresh every 5 seconds to ensure that you proceed to the next page as soon as all team members are ready.

Stage 2: Instructions

In this stage, you will have the chance to **get to know the other team members** in a virtual team meeting. The virtual team meeting will last **7 minutes** and end automatically.

To give you some input for starting the conversation, we ask each of you to evaluate different types of vacation before the meeting starts. Please feel free to discuss your evaluation with the others during the meeting. You are of course welcome to chat about other topics as well.

On the next page you should indicate how much you would enjoy the following types of vacation on a scale from 1 (not at all) to 7 (totally):

- Sun, sea and beach vacation.
- Party vacation.
- Winter sports vacation.
- City vacation.
- Educational vacation.
- Camping vacation.
- Cruise vacation.

Stage 2 and 3:

On-screen timer

Time left to complete this page: **5:00**

Note:
As this is a team experiment, and teams proceed at the pace of the slowest team member, on the following pages we have **on-screen timers** at the top of the pages (see above).

If you **do not proceed to the next page or reach a decision within the given time**, you will be tagged as a dropout and will not be paid for this study.

On **instructions pages**, the on-screen timers are **five minutes** and on **decision pages**, the on-screen timers are **three minutes**.

Stage 2: Start

Time left to complete this page: **3:00**

In this stage, you will have the chance to **get to know the other team members** in a virtual team meeting. The virtual team meeting will last **7 minutes** and end automatically.

To give you some input for starting the conversation, we ask each of you to evaluate different types of vacation before the meeting starts. Please feel free to discuss your evaluation with the others during the meeting. You are of course welcome to chat about other topics as well.

Please indicate how much you would enjoy the following types of vacation ...

	not at all 1	2	3	4	5	6	totally 7
City vacation.							
Cruise vacation.							
Camping vacation.							
Winter sports vacation.							
Educational vacation.							
Sun, sea and beach vacation.							
Party vacation.							

Virtual Team Meeting: General Instructions

Time left to complete this page: **5:00**

For this study we will be using the video meeting software Jitsi. It is a common meeting software and works like other similar tools such as Zoom, MS Teams, or others.
For this study, Jitsi is hosted on an internal KIT server. Thus, no data is stored on external servers.

On this page, are some brief instructions for the meeting software Jitsi.

[Audio and Photo Treatment]

General Instructions:

- Please give the meeting software **all permissions** that are asked for.
- In total, the virtual team meeting will last **7 minutes** and end automatically. First, please **test your technical setup and whether you can hear the other team members**.

<u>**Instructions for the technical setup:**</u>

- You can click on ˆ **above the “Mute / Unmute” (microphone) icon** to select the microphone and speaker you want to use. This might help if your currently selected microphone or speaker do not work. “Mute / Unmute” (microphone) icon:

[Video Treatment]

<u>**General Instructions:**</u>

- Please give the meeting software **all permissions** that are asked for.
- In total, the virtual team meeting will last **7 minutes** and end automatically. First, please **test your technical setup and whether you can hear the other team members**.

<u>**Instructions for the technical setup:**</u>

- You can click on ˆ **above the “Mute / Unmute” (microphone) icon** to select the microphone and speaker you want to use. This might help if your currently selected microphone or speaker do not work. “Mute / Unmute” (microphone) icon:
- If you have several cameras, you can choose which camera is used by clicking on ˆ above the “Start/Stop camera” icon. It is mandatory to keep the camera turned on during the meeting. “Start/Stop camera” icon:
- Optionally, you can click on the “Select background” icon and select one of the gray images (they are all the same) as your virtual background. “Select background” icon:

[All treatments]

Before you can continue, please confirm the following:
(If you do not confirm the statement, you cannot proceed with this study.)

☐ **I HAVE READ AND UNDERSTOOD THE INSTRUCTIONS FOR THE MEETING SOFTWARE.**

Virtual Team Meeting: Recording Instructions

Time left to complete this page: **5:00**

The virtual team meeting will start on the following page.

Note:
We will record the audio of the virtual team meeting (ONLY the audio and NOT the video). When the virtual meeting begins, you will see a **pop-up window asking for your permission to allow access to your <u>current tab</u>.**
Please click "Allow" to grant permission so that we can record the audio of the virtual team meeting.

Note that you will only get paid if you allow the recording of the audio as this is a necessary requirement of this study.

As already mentioned, remember that you can only proceed with this study if you use **a Chrome, an Edge or an Opera browser**. If you are currently using a different browser, **please copy the link of the current page and open it in a Chrome, an Edge or an Opera browser**. Otherwise, you cannot proceed with this study.

Please wait

Not all of your team members are ready for the virtual team meeting. **Please wait until your team members are ready**.

This wait page will automatically refresh every 5 seconds to ensure that you proceed to the next page as soon as all team members are ready.

Virtual Team Meeting

The virtual team meeting has started and you have a total of 7 **minutes to discuss the vacation types. In the Jitsi meeting, you can see a timer at the top center**. The virtual team meeting then ends automatically and you will proceed with the next page.

[Audio Treatment Meeting Screen]

[Audio and Photo Treatment]

Note:
You can click on ˆ **above the "Mute / Unmute" (microphone) icon** to select the microphone and speaker you want to use. This might help if your currently **selected microphone or speaker do not work**.
"Mute / Unmute" (microphone) icon:

Each team member is given a name during **Stage 2 and Stage 3**. Your name is **Team Member E** and the names of the other team members are: **Team Member I and O**. The names were assigned **randomly** and there is **no hierarchy** based on the names.

[Audio Treatment]

In the Jitsi meeting, you can see the names of each team member in the **middle of each screen**.

[Photo Treatment]

In the Jitsi meeting, you can see the names of each team member in the **bottom left corner of each screen**.

[Video Treatment]

- You can click on ˆ **above the "Mute / Unmute" (microphone) icon** to select the microphone and speaker you want to use. This might help if your currently selected microphone or speaker do not work. "Mute / Unmute" (microphone) icon:
- If you have several cameras, you can choose which camera is used by clicking on ˆ above the "Start/Stop camera" icon. It is mandatory to keep the camera turned on during the meeting. "Start/Stop camera" icon:
- Optionally, you can click on the "Select background" icon and select one of the **gray images (they are all the same)** as your virtual background. "Select background" icon:

Each team member is given a name during **Stage 2 and Stage 3**. Your name is **Team Member E** and the names of the other team members are: **Team Member I and O**. The names were assigned **randomly** and there is **no hierarchy** based on the names. In the Jitsi meeting, you can see the names of each team member in the **bottom left corner of each screen**.

[All treatments]

Questionnaire Page

Time left to complete this page: **3:00**

[Audio Treatment]

During the virtual team meeting, could you **hear both of the other two team members**?

- Yes, I could hear both of the other two team members.
- No, I could not hear both of the other two team members.

[Photo Treatment]

During the virtual team meeting, could you **hear and see a photo of both of the other two team members**?

- Yes, I could hear and see a photo of both of the other two team members.
- No, I could not hear and see a photo both of the other two team members.

[Video Treatment]

During the virtual team meeting, could you **hear and see both of the other two team members?**

- Yes, I could hear and see both of the other two team members.
- No, I could not hear and see both of the other two team members.

[All treatments]

Please wait

Please wait until your team members are ready.

This wait page will automatically refresh every 5 seconds to ensure that you proceed to the next page as soon as all team members are ready.

Stage 3:

Instructions

Time left to complete this page: **5:00**

The decision-making task consists of **10 identical, consecutive rounds**.

Imagine that you and the other two members of your team belong to a virtual team of a firm. You can think of a round as being a workweek. In each week, each of the virtual team members spends 40 hours working for the firm. You must choose how to allocate your time between two projects, Project A and Project B.

Project A: This project is a **team project**. The success of this project depends on all members in your virtual team and is determined by the contribution of the team member who contributes the least.

Project B: Think of this project as your **individual project**. It has been assigned to you by the firm and you are solely responsible for the success.

In each round, we will ask you how many hours you want to contribute to the Team Project A. The available choices are **0 hours, 10 hours, 20 hours, 30 hours, and 40 hours. Your remaining hours will be put towards your individual Project B**. For example, if you contribute 30 hours to Project A, this means that 10 hours will be put towards Project B.

For your decision you have to consider the following conditions: Working on the Team Project A generates higher costs for you than working on your individual Project B. Specifically, each hour that you contribute to Project A instead of Project B generates additional costs of 5 ECU, which will be deducted from your personal payoff in the respective round. Note further, that we will also ask the other two members of your virtual team how they want to allocate their time to your Team Project A and their own individual Project B.

You will find detailed information on individual payoff on the next page.

Your Payoff

Time left to complete this page: **5:00**

In each of the 10 rounds, you will receive an individual payoff that consists of a **fixed wage**, a **bonus**, and **individual costs** for allocating working hours to Project A.

Fixed Wage:
The Fixed Wage is 200 ECU which you will receive each time you start a new round.

Bonus:
The bonus increases your fixed wage and depends on the "success" of Project A. The success is determined by the minimum number of hours contributed to Project A by **any team member** (denoted by h_{min}). In other words, the project's success is determined by the team member who contributes the least amount of time to the project. To calculate your bonus, we will multiply the minimum number of hours contributed to Project A by 10 ECU, i.e., $Bonus = 10ECU \times h_{min}$

Individual Costs:
Since working on the Team Project A generates higher costs than working on your own individual Project B, we will deduct 5 ECU for each hour you contribute to Project A (denoted by h_A) from your payoff, i.e., $Costs = 5ECU \times h_A$

$$Payoff = 200ECU + (10ECU \times h_{min}) - (5 \times h_A)$$

Overall, your individual payoff consists of the **fixed wage** plus the **bonus** minus the **individual costs** from working on the Team Project A. You do not need to memorize this formula – the computer program will give you payoff tables at any point in the rounds where you need to make a decision.

This yields the following payoff table:

Payoff table	**Minimum number of hours contributed to Project A by any team member other than yourself**					
		0	**10**	**20**	**30**	**40**
Your number of hours contributed to Project A (h_A)	**0**	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

Procedure for a round

Time left to complete this page: **5:00**

In each of the 10 rounds, you will decide how many hours you contribute to the Team Project A. The other two team members will make the same decision.

Note that the payoff structure is the same for all of you.

Note that, when making your decision, you will not know what decision the other team members have made in this round. We will not reveal the choices of individual team members. **At the end of each round**, we will inform you about your payoff and the **minimum number of hours contributed to Project A by any of all team members.**

Your payoff for Stage 3: The computer will **randomly select one of the 10 rounds. This round will determine the payoff you will receive for taking part in Stage 3 of this study.**

Comprehension Question 1

Time left to complete this page: **3:00**

Before the task starts, we ask you some comprehension questions.

Payoff table	Minimum number of hours contributed to Project A by any team member other than yourself					
Your number of hours contributed to Project A (h_A)		**0**	**10**	**20**	**30**	**40**
	0	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

What would your payoff be if you contribute 0 hours to Project A and the minimum number of hours contributed to Project A by any team member other than yourself is 10 hours?

Comprehension Question 2

Time left to complete this page: **3:00**

Payoff table	Minimum number of hours contributed to Project A by any team member other than yourself					
Your number of hours contributed to Project A (h_A)		**0**	**10**	**20**	**30**	**40**
	0	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

What would your payoff be if you contribute 20 hours to Project A and the minimum number of hours contributed to Project A by any team member other than yourself is 10 hours?

Comprehension Question 3

Time left to complete this page: **3:00**

Payoff table	Minimum number of hours contributed to Project A by any team member other than yourself					
Your number of hours contributed to Project A (h_A)		**0**	**10**	**20**	**30**	**40**
	0	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

What would your payoff be if you contribute 40 hours to Project A and the minimum number of hours contributed to Project A by any team member other than yourself is 30 hours?

Comprehension Question 4

Time left to complete this page: **3:00**

Payoff table	Minimum number of hours contributed to Project A by any team member other than yourself					
		0	**10**	**20**	**30**	**40**
Your number of hours contributed to Project A (h_A)	**0**	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

What does your payoff depend on? Please select all options that apply.

☐ Number of hours you contribute to Project A

☐ Sum of all hours contributed to Project A in the team

☐ Minimum number of hours contributed to Project A by any team member (including yourself)

Thank you for reading the instructions and answering the comprehension questions

Time left to complete this page: **5:00**

Please click the "Next" button to start the task. The first round starts automatically as soon as all team members are ready.

Please wait

Not all of your team members are finished with reading the instructions and answering the comprehension questions. **Please wait until your team members are ready.**

This wait page will automatically refresh every 5 seconds to ensure that you proceed to the next page as soon as all team members are ready.

Decision - Round 1 (- 10)

Time left to complete this page: **3:00**

Please choose the number of hours you want to contribute to Project A (h_A). The remaining hours will be contributed to Project B.

- 0
- 10
- 20
- 30
- 40

Payoff table	Minimum number of hours contributed to Project A by any team member other than yourself					
		0	**10**	**20**	**30**	**40**
Your number of hours contributed to Project A (h_A)	**0**	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

Your best guess - Round 1 (& 10)

Time left to complete this page: **3:00**

[Audio Treatment]

Note:
You will be asked some questions about **each team member** on this page. Thus, you need to remember each team member. If you do not recall your team members, below you can find **audio recordings of all team members**.

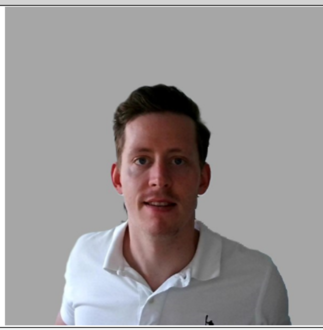
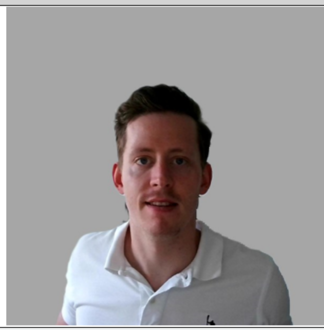
Audio recordings of all team members

Team Member E	You	Team Member O
0:00 / 0:03	0:00 / 0:03	0:00 / 0:03

[Photo and Video Treatment]

Note:
You will be asked some questions about **each team member** on this page. Thus, you need to remember each team member. If you do not recall your team members, below you can find **photos and audio recordings of all team members**.

<u>Photos and audio recordings of all team members</u>

Team Member E	You	Team Member O
		
0:00 / 0:03	0:00 / 0:03	0:00 / 0:03

[All treatments]

We now would like you to **guess what each team member contributed to Project A in this round**. Please tell us how confident you are about each of your guesses.

Payoff table		Minimum number of hours contributed to Project A by any team member other than yourself				
		0	**10**	**20**	**30**	**40**
Your number of hours contributed to Project A (h_A)	**0**	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

What is your best guess how many hours Team Member ... contributed to Project A in this round?

- 0
- 10
- 20
- 30
- 40

How confident are you in your guess for Team Member ...?

not at all 1	2	3	4	5	6	7 *very much so*

What is your best guess how many hours Team Member ... contributed to Project A in this round?

- 0
- 10
- 20
- 30
- 40

How confident are you in your guess for Team Member ...?

not at all 1	2	3	4	5	6	7 *very much so*

Please wait

Not all of your team members are finished with their decisions. Please wait until your team members are ready.

This wait page will automatically refresh every 5 seconds to ensure that you proceed to the next page as soon as all team members are ready.

Results - Round 1 (- 10)

Time left to complete this page: **3:00**

You contributed **xx hours** to Project A.
In this round, the minimum number of hours allocated to Project A in your team was **xx hours.**
Thus, your payoff in this round is **xxx ECU.**

Questionnaire Page 1

Finally, we ask you to complete the individual questionnaire. Please read the questions carefully and thoroughly before selecting an answer option.

Questionnaire Page 2

[Audio Treatment]

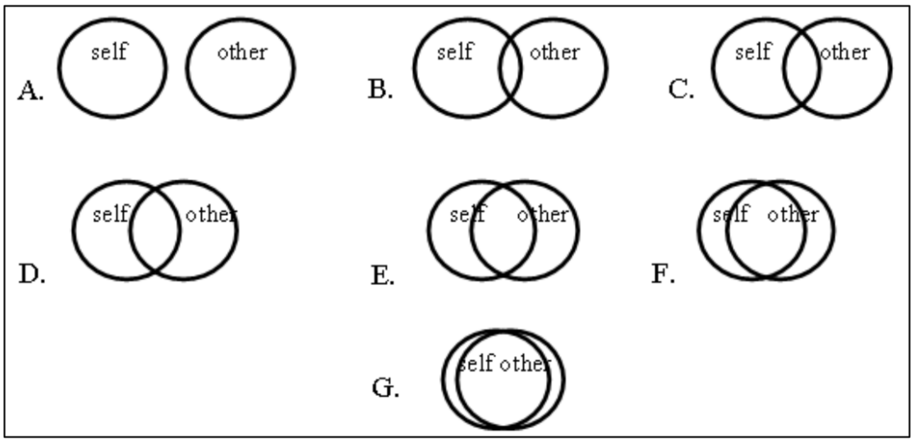
Note:
You will be asked some questions about **each team member** on this page. Thus, you need to remember each team member. If you do not recall your team members, below you can find **audio recordings of all team members**.

[Photo and Video Treatment]

Note:
You will be asked some questions about **each team member** on this page. Thus, you need to remember each team member. If you do not recall your team members, below you can find **photos and audio recordings of all team members**.

[All treatments]

Please, look at the circles diagram provided below. Then, consider which of these pairs of circles would best represent your connection with each team member from your team.



By selecting the appropriate letter below, please indicate to what extent you ...

... and **Team Member ...** would be connected.

A.	B.	C.	D.	E.	F.	G.

... and **Team Member ...** would be connected.

A.	B.	C.	D.	E.	F.	G.

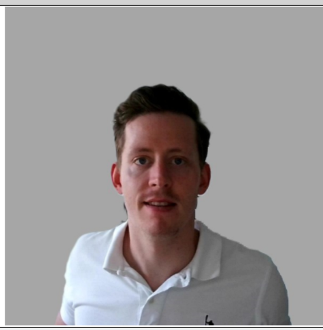
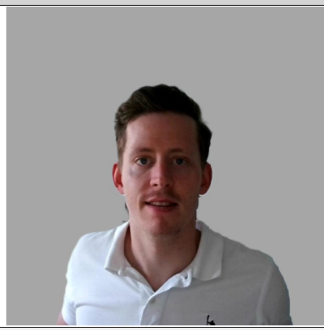
[Audio Treatment]

Audio recordings of all team members

Team Member E	You	Team Member O
0:00 / 0:03	0:00 / 0:03	0:00 / 0:03

[Photo and Video Treatment]

Photos and audio recordings of all team members

Team Member E	You	Team Member O
		
0:00 / 0:03	0:00 / 0:03	0:00 / 0:03

[All treatments]

Questionnaire Page 3

[Audio Treatment]

Note:
You will be asked some questions about **each team member** on this page. Thus, you need to remember each team member. If you do not recall your team members, below you can find **audio recordings of all team members**.

[Photo and Video Treatment]

Note:
You will be asked some questions about **each team member** on this page. Thus, you need to

remember each team member. If you do not recall your team members, below you can find **photos and audio recordings of all team members**.

[All treatments]

Please, select the appropriate number below to indicate to what extent you would use the term "WE" to characterize you ...

... and **Team Member ....**

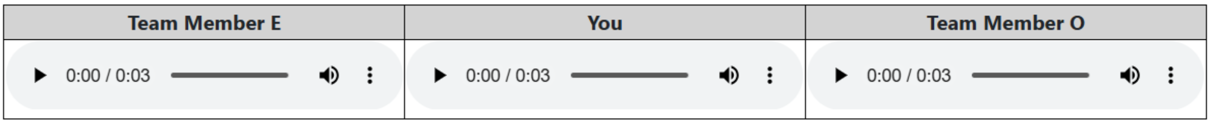
not at all 1	2	3	4	5	6	7 *very much so*

... and **Team Member ....**

not at all 1	2	3	4	5	6	7 *very much so*

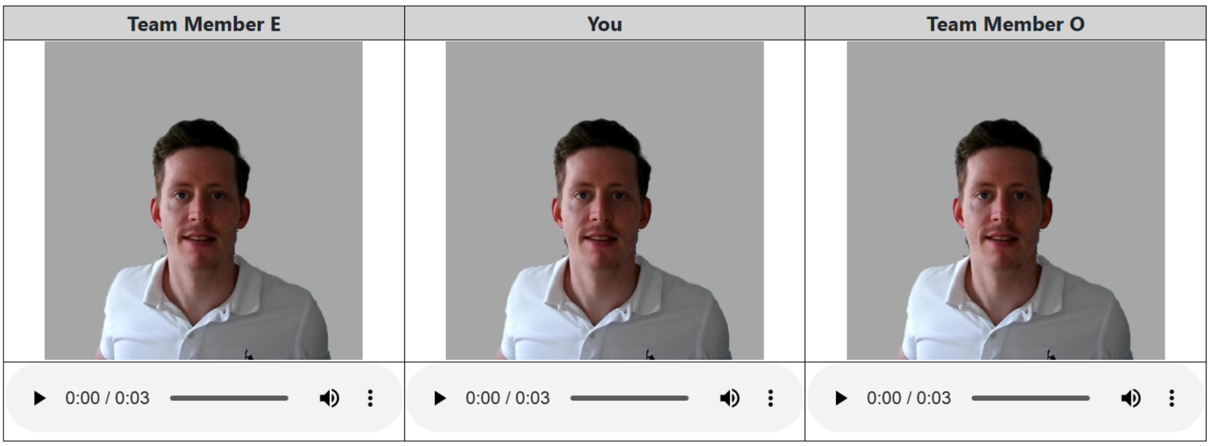
[Audio Treatment]

Audio recordings of all team members



[Photo and Video Treatment]

Photos and audio recordings of all team members



[All treatments]

Questionnaire Page 4

[Audio Treatment]

Note:
You will be asked some questions about **each team member** on this page. Thus, you need to remember each team member. If you do not recall your team members, below you can find **audio recordings of all team members**.

[Photo and Video Treatment]

Note:
You will be asked some questions about **each team member** on this page. Thus, you need to remember each team member. If you do not recall your team members, below you can find **photos and audio recordings of all team members**.

[Photo and Video Treatment]

How would you rate the **physical attractiveness** of the other team members on a 1-7 scale? (1 is very unattractive; 4 is average compared to the general US population; 7 very attractive)

How would you rate the ...	very unattractive 1	2	3	4	5	6	very attractive 7
... physical attractiveness of Team Member ...							
... physical attractiveness of Team Member ...							

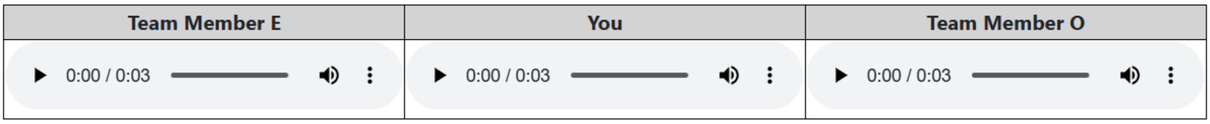
[All treatments]

How would you rate the **attractiveness of the voice** of the other team members on a 1-7 scale? (1 is very unattractive; 4 is average compared to the general US population; 7 very attractive)

How would you rate the ...	very unattractive 1	2	3	4	5	6	very attractive 7
... attractiveness of the voice of Team Member ...							
... attractiveness of the voice of Team Member ...							

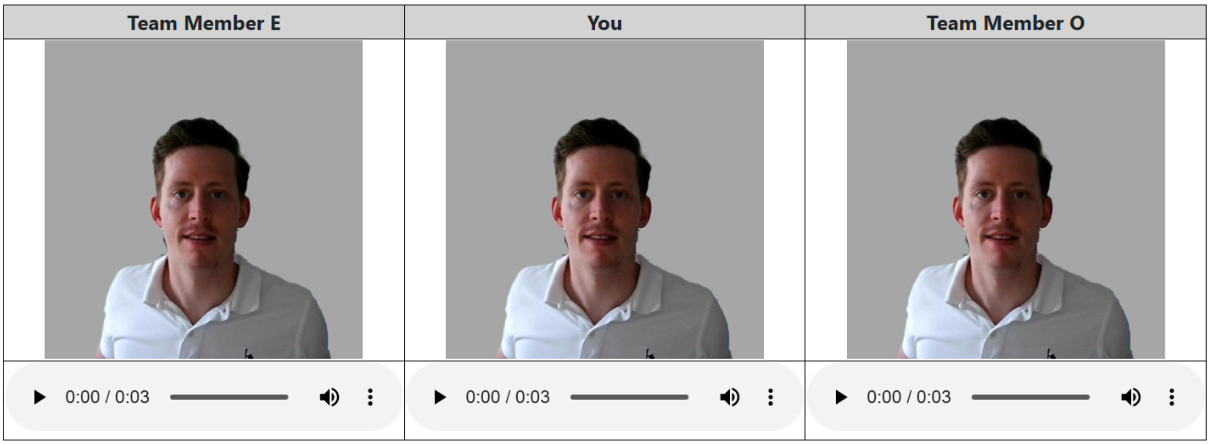
[Audio Treatment]

Audio recordings of all team members



[Photo and Video Treatment]

Photos and audio recordings of all team members



[All treatments]

Questionnaire Page 5

Please indicate your level of agreement or disagreement with each of the following statements.	strongly disagree	strongly agree
I am confident that other team members do not know who I am.		
I believe that my personal identity remains unknown to other team members.		
Careful editing is important, click on the box on the far left “strongly disagree”.		
I am easily identified as an individual by other team members.		
Other team members are likely to know who I am.		
My personal identity is known to other team members.		

Questionnaire Page 6

During the virtual team meeting with your team members, ...	Not at all						Very much
... how close did you feel to the other team members?							
... how strongly did you feel the presence of the other team members?							
... careful editing is important, click on the box on the far right “very much”.							
... how connected did you feel to the other team members?							
... to what extent did you feel that you are alone?							

Questionnaire Page 7

[Audio Treatment]

Note:
You will be asked some questions about **each team member** on this page. Thus, you need to remember each team member. If you do not recall your team members, below you can find **audio recordings of all team members**.

[Photo and Video Treatment]

Note:
You will be asked some questions about **each team member** on this page. Thus, you need to remember each team member. If you do not recall your team members, below you can find **photos and audio recordings of all team members**.

[All treatments]

Please indicate whether you knew **Team Member ...** before this study.

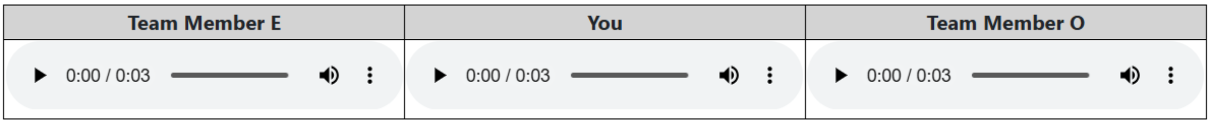
- I did not know Team Member ... before this study
- I knew Team Member ... before this study

Please indicate whether you knew **Team Member ...** before this study.

- I did not know Team Member ... before this study
- I knew Team Member ... before this study

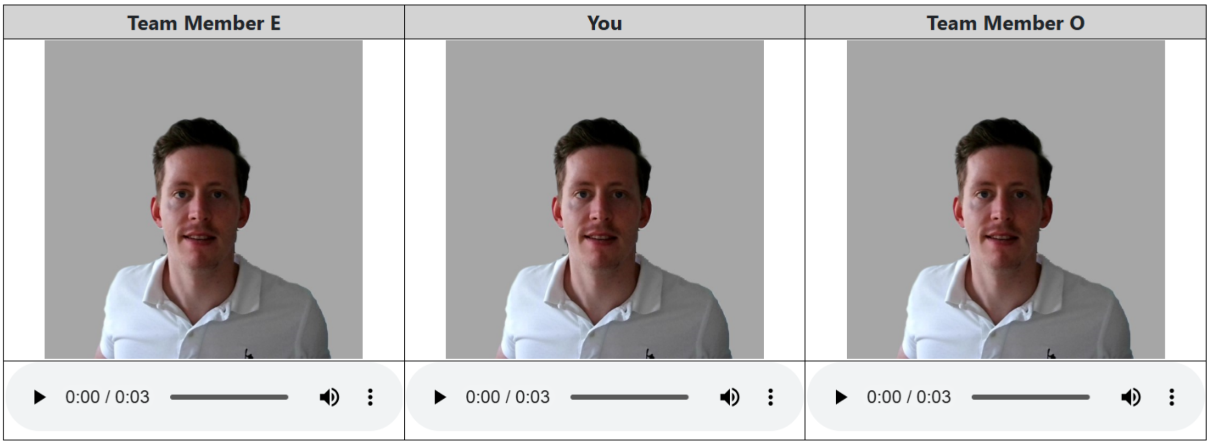
[Audio Treatment]

Audio recordings of all team members



[Photo and Video Treatment]

Photos and audio recordings of all team members



[Video Treatment]

Questionnaire Page 8

Note:
You will be asked some questions about **each team member** on this page. Thus, you need to remember each team member. If you do not recall your team members, below you can find **photos and audio recordings of all team members**.

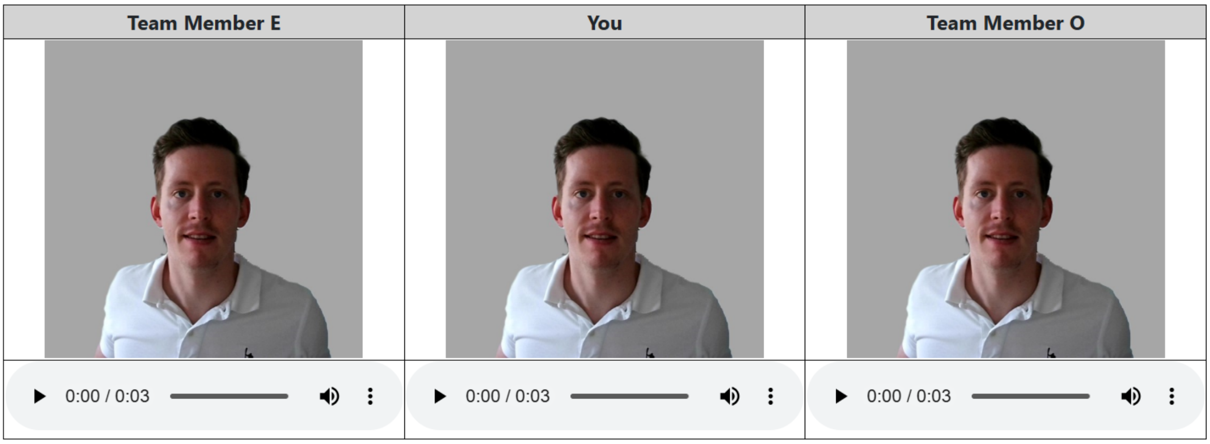
Had **Team Member ...** selected a video background during the virtual team meeting?

- Yes.
- No.
- I do not remember.

Had **Team Member ...** selected a video background during the virtual team meeting?

- Yes.
- No.
- I do not remember.

Photos and audio recordings of all team members



[All treatments]

Questionnaire Page 8/9

We will now ask you to provide some **feedback regarding the setup and comprehensibility**. **Please give your answers in the fields below. Thank you!**

Did you encounter any technical problems?

Where there any problems with comprehensibility of the instructions?

Thank you for participating in our study!

Result and Payoff:

Your fixed payment in this study is **£6.00.**

In Stage 3, **Round ...** was payoff-relevant. Thus, your **bonus payment for Stage 3 is xxx ECU**.

This corresponds to a bonus payment of **£x.xx** and a total payment amount of **£x.xx.**

Please paste the participation code on the Prolific website: xx, or click here to finish the study and be redirected to Prolific.

C Appendix for Study IV

C.1 Tables and Figures

	Selfish Design Variant	**Altruistic Design Variant**
Bonus task instructions	Thank you for also working on the bonus task to make Wikipedia more accessible! Of course, this task allows you to earn some extra money. At the same time, your work will make pictures accessible. Every picture will add one more data point for the project. We ask you to do at least the mandatory main tasks. However, we expect you to do your best and contribute picture annotations to the project. The more pictures you manage to annotate, the better for the project and the more bonus money you will earn. This is your chance to earn extra money by doing an exceptional job! Thank you for participating in the project and working on the bonus task to annotate pictures!	Thank you for also being part of our mission to make Wikipedia more accessible! So, why should you engage in our bonus task? You might think, "to earn some extra money", but in reality, you are doing something special. Your efforts will help to make pictures accessible for visually impaired readers. Every picture count and brings us closer to the goal of tearing down the barriers for visually impaired readers. We ask you to do at least the mandatory main tasks. However, we also invite you to do your very best and contribute as much alt-tags as you can. The more pictures you annotate, the better! Thank you for "being the eyes" of others and helping out others in need!
Info on submitted bonus tasks	Bonus earned via alt-tags	Alt-tags contributed for accessibility
Feedback message	You are doing an **exceptional job**! By **doing your best** and annotating more pictures, you can earn **bonus money** and contribute more to our project.	Thank you for contributing to make Wikipedia **more accessible**! Every picture counts and allows more **visually impaired** people to experience Wikipedia **without any barriers**.

Table C.1: Overview of differences between the selfish and altruistic design variant

Construct	Items	Reference
Polychronicity	I prefer to work on several projects in a day, rather than completing one project and then switching to another. I would like to work in a job where I was constantly shifting from one task to another, like a receptionist or an air traffic controller. I lose interest in what I am doing if I have to focus on the same task for long periods of time, without thinking about or doing something else. When doing a number of assignments, I like to switch back and forth between them rather than do one at a time. I like to finish one task completely before focusing on anything else. (R) It makes me uncomfortable when I am not able to finish one task completely before focusing on another task. (R) I am much more engaged in what I am doing if I am able to switch between several different tasks. I do not like having to shift my attention between multiple tasks. (R) I would rather switch back and forth between several projects than concentrate my efforts on just one. I would prefer to work in an environment where I can finish one task before starting the next. (R) I don't like when I have to stop in the middle of a task to work on something else. (R) When I have a task to complete, I like to break it up by switching to other tasks intermittently. I have a "one-track" mind. (R) I prefer not to be interrupted when working on a task. (R)	Multitasking Preference Inventory (MPI) (Poposki & Oswald, 2010)
Altruism	Imagine the following situation: Today you unexpectedly received 1,600 U.S. dollars. How much of this amount would you donate to a good cause? ($0 - $1,600) How willing are you to give to good causes without expecting anything in return? (0 - 10)	Falk et al. (2018, 2023)
Person-Job Fit	I fitted right in to the job. Taking everything into account, the job was a complete fit for me. The job provided a total fit for me.	Venkatesh et al. (2017)
Job Satisfaction	Overall, I was satisfied with my job. I would have prefered another, more ideal job. (R) I was satisfied with the important aspects of my job.	Sykes (2020)
Job Performance	I managed to plan my work so that it was done on time. My planning was optimal. I kept in mind the results that I had to achieve in my work. I was able to separate main issues from side issues at work. I was able to perform my work well with minimal time and effort. I took on extra responsibilities. I started new tasks myself, when my old ones were finished. I took on challenging work tasks, when available. I worked at keeping my job knowledge up-to-date. I worked at keeping my job skills up-to-date. I came up with creative solutions to new problems. I kept looking for new challenges in my job.	Koopmans et al. (2014)

Table C.2: Overview of items

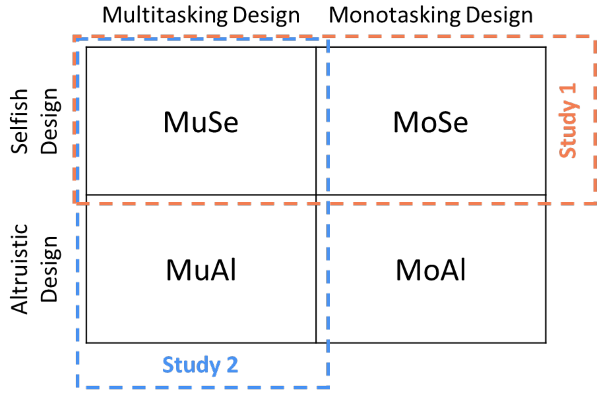


Figure C.1: Combinations of design options for the study artifacts of study 1 and 2

Variable	Mono (n = 61)	Multi (n = 55)	Analysis results
Age	34.33 (12.02)	36.16 (12.04)	Not sign., p = 0.38
Female	0.508 (0.504)	0.509 (0.505)	Not sign., p = 1.00
US	0.295 (0.460)	0.309 (0.466)	Not sign., p = 1.00
UK	0.344 (0.479)	0.364 (0.485)	Not sign., p = 0.98
ZA	0.361 (0.484)	0.327 (0.474)	Not sign., p = 0.86

Note: p-values are based on two-sided Mann-Whitney U tests. Two participants were not included because they answered “diverse” when asked about their gender.

Table C.3: Study 1: Means of key demographics over Monotasking vs Multitasking design variant

Variable	NoFit (n = 51)	Fit (n = 65)	Analysis results
Age	34.31 (10.56)	35.89 (13.08)	Not sign., p = 0.83
Female	0.392 (0.493)	0.600 (0.494)	Sign., p = 0.04
US	0.294 (0.460)	0.308 (0.465)	Not sign., p = 1.00
UK	0.412 (0.497)	0.308 (0.465)	Not sign., p = 0.33
ZA	0.294 (0.460)	0.385 (0.490)	Not sign., p = 0.41

Note: p-values are based on two-sided Mann-Whitney U tests. Two participants were not included because they answered “diverse” when asked about their gender.

Table C.4: Study 1: Means of key demographics over NoFit vs Fit

Variable	Mono (n = 61)	Multi (n = 55)	Analysis results
Polychronicity	37.51 (12.37)	40.47 (11.67)	Not sign., p = 0.19

Note: p-values are based on two-sided Mann-Whitney U tests.

Table C.5: Study 1: Means of preferences over Monotasking vs Multitasking design variant

Variable	NoFit (n = 51)	Fit (n = 65)	Analysis results
Polychronicity	40.08 (11.79)	38 (12.32)	Not sign., p = 0.26

Note: p-values are based on two-sided Mann-Whitney U tests.

Table C.6: Study 1: Means of preferences over NoFit vs Fit

Dep. Var.: *quantity*	(1)	(2)	(3)	(4)	(5)	(6)
Multitasking Design	11.456*	11.888*			2.254	2.593
	(6.370)	(6.356)			(9.967)	(10.283)
Female		4.591		5.070		3.817
		(6.342)		(6.408)		(6.483)
Age		-0.286		-0.236		-0.295
		(0.317)		(0.326)		(0.323)
UK		-3.855		-4.327		-3.786
		(7.779)		(7.840)		(7.831)
ZA		-4.900		-4.966		-3.574
		(9.126)		(9.233)		(9.340)
Fit			-1.672	-2.354	-8.844	-9.325
			(6.487)	(6.513)	(8.809)	(8.923)
Fit × Multitasking Design					16.397	16.631
					(13.103)	(13.843)
Constant	25.180***	35.765**	31.549***	40.912***	30.400***	41.470**
	(4.115)	(15.020)	(4.930)	(15.135)	(7.489)	(16.183)
R^2	0.028	0.043	0.001	0.014	0.042	0.057
Observations	116	116	116	116	116	116

Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.7: Study 1: OLS regressions with *quantity* as the dependent variable and all coefficients

Dep. Var.: $quantity_{adjusted}$	(1)	(2)	(3)	(4)	(5)	(6)
Multitasking Design	5.939*	5.934*			1.532	2.031
	(3.509)	(3.480)			(5.223)	(5.453)
Female		4.503		4.539		3.988
		(3.457)		(3.554)		(3.587)
Age		-0.033		-0.013		-0.042
		(0.204)		(0.207)		(0.204)
UK		-0.343		-0.440		-0.187
		(4.419)		(4.427)		(4.440)
ZA		-2.057		-2.129		-1.525
		(4.953)		(4.974)		(5.078)
Fit			0.660	-0.115	-2.821	-3.023
			(3.522)	(3.514)	(4.512)	(4.688)
Fit × Multitasking Design					8.022	7.100
					(7.125)	(7.511)
Constant	13.066***	12.765	15.512***	14.997*	14.731***	14.872
	(2.151)	(8.984)	(2.592)	(8.892)	(3.658)	(9.288)
R^2	0.025	0.039	0.000	0.014	0.037	0.047
Observations	116	116	116	116	116	116

Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.8: Study 1: OLS regressions with $quantity_{adjusted}$ as dependent variable and all coefficients

Variable	NoFit (n = 51)	Fit (n = 51)	Analysis results
Age	32.53	34.49	Not sign., p = 0.72
	(8.907)	(11.96)	
Female	0.451	0.569	Not sign., p = 0.32
	(0.503)	(0.500)	
US	0.216	0.294	Not sign., p = 0.50
	(0.415)	(0.460)	
UK	0.353	0.235	Not sign., p = 0.28
	(0.483)	(0.428)	
ZA	0.431	0.471	Not sign., p = 0.84
	(0.500)	(0.504)	

Note: p-values are based on two-sided Mann-Whitney U tests.

Table C.9: Study 2: Means of key demographics over NoFit vs Fit

Variable	Altruistic (n = 47)	Selfish (n = 55)	Analysis results
Age	31.64	35.11	Not sign., p = 0.11
	(9.128)	(11.45)	
Female	0.617	0.418	Sign., p = 0.07
	(0.491)	(0.498)	
US	0.255	0.255	Not sign., p = 1.00
	(0.441)	(0.440)	
UK	0.255	0.327	Not sign., p = 0.57
	(0.441)	(0.474)	
ZA	0.489	0.418	Not sign., p = 0.60
	(0.505)	(0.498)	

Note: p-values are based on two-sided Mann-Whitney U tests.

Table C.10: Study 2: Means of key demographics over Altruistic vs Selfish design variant

Variable	NoFit (n = 51)	Fit (n = 51)	Analysis results
Altruism	0.0526	0.0036	Not sign., p = 0.51
	(1.060)	(0.878)	

Note: p-values are based on two-sided Mann-Whitney U tests.

Table C.11: Study 2: Means of preferences over NoFit vs Fit

Variable	Altruistic (n = 47)	Selfish (n = 55)	Analysis results
Altruism	-0.0579	0.102	Not sign., p = 0.54
	(1.029)	(0.917)	

Note: p-values are based on two-sided Mann-Whitney U tests.

Table C.12: Study 2: Means of preferences over Altruistic vs Selfish design variant

Dep. Var.: *quantity*	(1)	(2)	(3)	(4)	(5)	(6)
Altruistic Design	-16.821***	-13.731**			-14.624*	-7.998
	(5.994)	(5.889)			(8.499)	(9.392)
Female		-19.453***		-22.522***		-18.511***
		(6.554)		(6.894)		(6.676)
Age		0.132		0.290		0.173
		(0.264)		(0.269)		(0.266)
UK		4.544		5.389		3.269
		(7.494)		(7.693)		(7.815)
ZA		21.976***		23.511***		23.514***
		(7.840)		(8.310)		(8.583)
Fit			-4.824	-3.030	-2.050	2.602
			(6.447)	(6.330)	(10.541)	(9.661)
Fit × Altruistic Design					-3.902	-11.534
					(12.160)	(13.231)
Constant	35.927***	28.738**	30.588***	19.271*	36.897***	25.470*
	(5.195)	(11.416)	(4.565)	(10.861)	(7.017)	(13.421)
R^2	0.067	0.194	0.006	0.155	0.072	0.202
Observations	102	102	102	102	102	102

Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.13: Study 2: OLS regressions with *quantity* as dependent variable and all coefficients

Dep. Var.: $quantity_{adjusted}$	(1)	(2)	(3)	(4)	(5)	(6)
Altruistic Design	-8.127**	-6.902**			-4.894	-1.703
	(3.236)	(3.423)			(4.481)	(4.959)
Female		-8.081**		-9.641***		-7.338**
		(3.519)		(3.512)		(3.561)
Age		0.049		0.128		0.080
		(0.157)		(0.157)		(0.157)
UK		1.501		1.957		0.560
		(4.059)		(4.055)		(4.208)
ZA		9.280**		10.060**		10.745**
		(4.068)		(4.332)		(4.493)
Fit			-2.091	-1.372	1.244	3.540
			(3.426)	(3.399)	(5.463)	(5.324)
Fit × Altruistic Design					-6.216	-10.567
					(6.597)	(7.455)
Constant	18.501***	15.776**	15.802***	10.963*	17.913***	12.406*
	(2.669)	(6.732)	(2.289)	(6.471)	(3.294)	(7.432)
R^2	0.056	0.137	0.004	0.102	0.066	0.158
Observations	102	102	102	102	102	102

Robust standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.14: Study 2: OLS regressions with $quantity_{adjusted}$ as dependent variable and all coefficients