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Persuading an audience: Testing information design in the laboratory

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Abstract

Governments, central banks, and private organizations frequently face the challenge of convincing their audience to take a specific action. One key choice is whether to send a public message that can coordinate the audience's actions or to rely instead on private messages that may differ across audience members and thereby miscoordinate actions. This paper uses a laboratory experiment to test whether public or private messages are more persuasive and how this depends on the audience's strategic environment. In the experiment, public signals are most persuasive. The results match the theoretical prediction that public persuasion works best when the receivers' strategic environment features strategic complements. However, contrary to theory, public signals are equally persuasive as private ones under strategic substitutes. Senders respond to this pattern by engaging more frequently in public communication, especially when the receivers' environment features strategic complements.

Keywords: information design, Bayesian persuasion, laboratory experiment, Bayes correlated equilibria, obedience, recommendations
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1 Introduction

Senders frequently speak to an audience of multiple receivers. For example, governments communicate with their citizens, or the leadership in private organizations addresses their employees or customer base. I focus on the sender's key choice of communication channel. The sender may employ *public* announcements, in which information is jointly revealed to all receivers. Alternatively, the sender may rely on *private* messages to individual receivers. In practice, senders often employ public communication strategies to convince their audiences to take a desired action. For instance, governments hold public press conferences, and central banks ensure that market participants can access their communication.[1] In other settings, private messages can be advantageous—for example, when route-planning services such as Google Maps or Waze recommend routes to their customers (Das, Kamenica, and Mirka, 2017). Miscoordinated routes minimize average travel times by reducing congestion; to ensure that drivers stay on their designated paths, recommendations to others are kept private.

Using a laboratory experiment, I provide the first empirical evidence on whether choosing the right communication channel helps a sender persuade her audience and what role the audience members' strategic interaction plays in that decision. As in Bayesian persuasion (Kamenica and Gentzkow, 2011), the sender can reveal superior information about the state of the world. As a key feature, the sender communicates with an audience of multiple receivers. The presence of other receivers in the audience may affect how persuasive different communication channels are. I test whether a sender benefits from tailoring her communication to the receivers' strategic interaction. In practice, as in the examples above, a receiver's optimal action frequently depends on other receivers' actions.[2] Theoretically, the receivers' strategic interaction determines whether private signals or public announcements are a more effective tool of persuasion, a prediction from the literature on information design (for example, Bergemann and Morris, 2019).

In particular, I introduce coordination and miscoordination motives into the audience members' strategic interaction, which encompasses many real-world interactions within audiences. To capture coordination motives, the receivers' strategic environment features strategic complementarities. Each receiver's incentive to choose an action increases in the number of other receivers choosing that action. With these complementarities, public messages are predicted to improve persuasion. A public message encourages all receivers to choose an identical action. Common actions reinforce incentives to select that action, and observing everyone's recommended actions increases incentives to choose the favored action by minimizing strategic uncertainty. To capture miscoordination motives, the receivers' environment features strategic substitutes; that is, each receiver's incentive decreases in others' choice of the same action. In this environment, private messages are predicted to perform better. Each receiver is encouraged to take a potentially different action and does not observe other receivers' messages.[3] By miscoordinating actions

[1] For example, central banks may want to tailor their communication to be commonly understood by the general public in order, for example, to anchor inflation expectations (Haldane and McMahon, 2018; Binder, 2017; Bholat, Broughton, Parker, Ter Meer, and Walczak, 2018; Haldane, Macaulay, and McMahon, 2021).

[2] Next to classical examples of interactions such as in financial markets or in teams in organizations, many political economy models introduce interacting receivers. Empirically, Cantoni, Yang, Yuchtman, and Zhang (2019) provide evidence for protest movements to feature strategic substitutes.

[3] The strategic tension between the sender's and the receivers' interests means that private signals cannot be revealed publicly. If both receivers have access to the private information revealed to each other, the sender can

and withholding information about the state from some receivers, persuasion can induce the favored action more frequently.

To create exogenous variation in the communication channel and the strategic environment, I study persuasion in a laboratory experiment. The experiment is designed to test the theoretical rationale, and to disentangle the mechanisms why either channel is more persuasive. In part, the theory correctly predicts the interdependence between channel choice and the receivers' strategic environment. However, public signals turn out to be more persuasive than predicted, compared to private signals, for reasons not yet captured theoretically. Public communication results in less noisy behavior because it has a simpler, symmetric structure, and it is advantageous given receivers' aversion to differential treatment through private signals.

The laboratory evidence is key to being able to arrive at these findings, and to shed light on one explanation why public persuasion features so prominently in e.g. governmental communication. The laboratory setting allows me to vary the communication strategies and the receivers' strategic interaction exogenously while holding constant other features that affect a sender's persuasiveness, such as her reputation. In contrast, in the field, researchers only observe the receivers' response to the communication channels that the senders select, which are often public, or cannot vary or establish the effect of the strategic environment on persuasion.[4]

I employ two experiments that build on an investment game introduced by Bergemann and Morris (2019). In that investment game, the receivers choose whether to invest without knowing whether the state of the world is good or bad. A receiver wants to match the state by investing only in the good state. In addition, a receiver's payoff depends on the choice of the other receiver, creating room for strategic complements or substitutes. Without information beyond the prior, investment is not profitable for receivers. Investment is attractive in the good state, yet receivers, on average, make a loss when investing without additional information about the state. This creates scope for persuasion. I assume that the sender wants to persuade receivers to invest, irrespective of the state. As in Bayesian persuasion (Kamenica and Gentzkow, 2011), the sender reveals information by committing to an information structure. The signals are action recommendations that are informative about the state and others' signals. When judging whether they can trust a sender's recommendation, the receivers need to consider not only their own inference but also their beliefs about others' information processing and decisions, which I elicit in the laboratory.

In the first experiment, I focus on receiver behavior. Computerized senders recommend actions to two participants in the role of receivers. I vary three treatment dimensions. First, I vary whether the game features strategic complements or substitutes. Second, I vary whether the information structure uses public or private signals. Third, I vary how aggressively the sender persuades the receivers by varying how often they receive a recommendation to invest in the bad state. Higher probabilities of this recommendation decrease expected gains from following recommendations. Formally, this varies whether an information structure satisfies obedience constraints, which measure whether a receiver can best respond by following recommendations.

no longer exploit her information about the state. I discuss this feature in more detail in Section 3.

[4]In addition, in the laboratory, I can measure the importance of the channel choice. This allows me to evaluate whether any practical constraints on the channel choice, such as a legal requirement to use public communication or an inability to ensure public dissemination of signals, limit a sender's persuasiveness.

I test three levels of aggressiveness, where expected payoffs from following are held constant at each level, and two levels satisfy obedience constraints for risk-neutral receivers. By comparing following rates in the three levels, I test whether obedience is predictive of behavior. These constraints are widely used theoretically, but, to the best of my knowledge, this paper is the first to test them empirically.

Comparing public and private communication, I find that a channel's persuasiveness depends on the strategic environment, but I also observe surprising deviations from the predictions. In particular, I find that public structures perform well in a broader sense than expected. I observe the theoretically predicted advantage of public structures in settings with strategic complements. The empirical benefit even exceeds the theoretically predicted wedge. With strategic substitutes—a setting in which private signals are predicted to enhance receivers' persuasion—both public and private platforms perform equally well. Empirically, receivers are less willing to follow private than public recommendations. Interestingly, they anticipate this effect, as they believe other receivers follow public recommendations more frequently than private ones. Senders thus benefit in ways not captured by existing theory from using public signals, providing a justification for the frequent use of public communication in practice.

Two mechanisms drive the empirical superiority of public signals. First, the receivers' behavior exhibits more variance than predicted in response to private signals. Therefore, there is less additional unintended variation with public signals. The noise specific to private structures adds uncertainty about others' behavior beyond what is deliberately introduced by the sender and beyond what is optimal to persuade receivers. Hence, the receivers' best response is to follow private recommendations less often, which decreases persuasion. The additional noise with private signals is consistent with their complexity. Only with private signals do the receivers have to reason through the uncertainty about which recommendations others have received. As a second mechanism, I show that whether the signals are public or private affects the receivers' reaction to experiencing *bad advice*. Here, bad advice is defined as the recommendation to invest in the bad state against the receivers' interest. With private signals, bad advice is sent to only one receiver, while the sender recommends that the other receiver not invest, a form of (ex-post) differential treatment. In contrast, both receivers receive a common (bad) recommendation with public signals. I find that only receivers who receive bad advice with private signals subsequently reduce their investment. This pattern is consistent with receivers disliking this differential treatment.

While not capturing the benefits of public persuasion, theory otherwise predicts behavior well. Depending on the information structure's aggressiveness, 78% to 90% of recommendations that theory predicts will be followed are indeed followed. In contrast, when not all recommendations are predicted to be followed in equilibrium, they are followed empirically only in 66% of periods. Therefore, the obedience constraints organize receivers' behavior reasonably well. Using data on beliefs, I show that the decisions to follow are consistent with the theoretically predicted mechanism: Receivers update their beliefs well, and signals are processed close to the theoretically predicted way. Beliefs about the state show some conservative updating but evolve in line with Bayesian predictions. Furthermore, participants have a good understanding of the average response of other receivers to different signals. Even more striking is that given receivers' beliefs, their decisions are close to their best response, especially so with public persuasion.

In a second experiment, computerized senders are replaced by human senders. The senders are incentivized to maximize receivers' investment and choose among the same information structures that were exogenously assigned in the first experiment. They choose between different levels of aggressiveness in persuasion and between public or private signals. Between subjects, I vary whether the receivers' game features strategic substitutes or complements.

This experiment allows me to study how participants in the role of senders persuade. This is important for three reasons. First, it means I can test whether senders adapt their choice to the receiver's strategic environment: do senders use public signals more frequently with strategic complements and private ones with substitutes? Second, it means I can assess whether senders foresee and react to the empirical superiority of persuasion with public signals. Third, it allows me to replicate receiver behavior in a setting where receivers interact with a human sender, instead of the computerized senders in the first experiment.

Behaviorally, it is plausible that receiver behavior changes in response to endogenous choices by sender-participants. This is an important distinction, and matches the senders' deliberate choice of communication strategies in practice. Receivers may expect human senders to share surplus fairly, as captured in classical games with models of social preferences Fehr and Schmidt (1999), or respond to the senders' intentions to deceive them, as captured in models of reciprocity Falk and Fischbacher (2006). Additionally, the experimental literature on cheap-talk games typically finds that receivers are typically more credulous than predicted, and senders more truthful than if they were motivated purely by self-interest (Blume, Lai, and Lim, 2020; Abeler, Nosenzo, and Raymond, 2019). These motives may, in turn, affect the sender's optimal communication strategy. Empirically, I find little evidence for changes in receiver behavior across the two experiments. Recommendations are followed slightly less often, but this change is similar across both games and all information structures.

When choosing between either type of communication channel, I find that senders employ public signals in 55% of periods. Crucially, they respond to the receivers' strategic interaction: they use public signals more frequently in games with strategic complements than in games with strategic substitutes. Senders apparently exploit both the theoretically predicted benefit specific to each game (as they use public signals more frequently in settings with strategic complements) and the empirical advantage of public signals (as they use public signals more frequently when pooling data across the two settings). Senders' beliefs indicate that they anticipate that receivers respond to a change in communication strategies. However, they underestimate how strongly receivers react to changes in communication strategies, which leads them to not fully capitalize on the potential gains from public signals.

In the experiment, senders persuade quite forcefully. The senders' median choice is the sender-optimal structure, which maximizes their own self-interested payoffs at the receivers' expense; it is just obedient for risk-neutral receivers to trust these signals. If anything, senders err by being even more aggressive than what theory predicts will maximize their self-interested payoffs. While senders believe that more aggressive persuasion leads receivers to implement the sender's desired action less frequently, they do not fully account for the strength of the receivers' response. This aggressiveness in this complex environment, in which senders communicate by committing to an information structure, contrasts with findings from settings with more direct communication, such as cheap-talk games.

In sum, I provide the first empirical evidence on the persuasion of audiences as modeled in the theoretical literature on information design. Along many dimensions, the behavior in the laboratory is consistent with the theoretical predictions. For example, in my empirical test of the theoretical concept of obedience constraints, choices are close to the predictions. Crucially, I find empirically that public messages help senders to persuade their audience in ways not yet captured theoretically. The messages' persuasiveness can be attributed to their simplicity, leading to less noisy behavior, and their equal treatment of receivers. Senders take advantage of the superiority of public signals.

In the following, I start by positioning the paper in relation to the literature. Section 3 describes the theoretical background, the theoretically motivated hypotheses, and the experimental design. Sections 4 and 5 present the results from the first and second experiment, respectively. Section 6 concludes.

2 Relation to the literature

Theory. This study builds on a setup introduced by Bergemann and Morris (2019) within the literature on information design. Information design generalizes Bayesian persuasion (Kamenica and Gentzkow, 2011) to multiple receivers. In the laboratory, I test whether a sender can leverage strategic uncertainty by choosing an appropriate communication channel to enhance persuasion. Bergemann and Morris derive this insight on the channel choice in the investment game used in this experiment. Relatedly, a large theoretical literature compares public and private signals as well as different types of strategic interaction. For example, Angeletos and Pavan (2007) study welfare, Ely (2017) bank runs, Arieli and Babichenko (2019) information disclosure as in advertising, and Inostroza and Pavan (2021) stress tests. Taneva (2019) studies designer-optimal information design. Mathevet, Perego, and Taneva (2020) study adversarial equilibrium selection and introduce an investment game similar to the one used in this paper. As a limitation to the sender-preferred equilibria studied in Bayesian persuasion, Tsakas, Tsakas, and Xefteris (2021) and Taneva and Wiseman (2022) consider strategically ignorant receivers.

More abstractly, Bergemann and Morris (2016) introduce Bayes correlated equilibria.[5] These equilibria are widely used theoretically—for example, for informationally robust auction design (Bergemann, Brooks, and Morris, 2019; Brooks and Du, 2021). They build on obedience constraints, which require that receivers' best response is to follow recommendations. I am the first to study whether these constraints capture receiver behavior empirically. I focus on the question whether receivers' empirical response depends on specific information structures—for example, whether their response depends on the publicness of a signal.

[5] Bayes correlated equilibria generalize correlated equilibria (Aumann, 1987) to games of incomplete information, see Forges (1993) for similar generalizations. Correlated equilibria have been tested in the laboratory—for example, by Van Huyck, Gillette, and Battalio (1992); Brandts and Holt (1992); Moreno and Wooders (1998); Cason and Sharma (2007); Duffy and Feltovich (2010); Bone, Drouvelis, and Ray (2013); Anbarci, Feltovich, and Gürdal (2018); Kurz, Orland, and Posadzy (2018); Friedman, Rabanal, Rud, and Zhao (2022); Anufriev, Duffy, Panchenko, and Young (2023). A connected line studies information transmission through mediators in the laboratory (Casella, Friedman, and Archila, 2020; Blume, Lai, and Lim, 2023). Unlike this literature, I study a sender that can not only correlate agents' play, but crucially has access to information about the uncertain state of the world, which she can use to persuade.

Experimental literature

Several strands of experimental literature are related to this study. First, single-receiver Bayesian persuasion has been recently studied in the laboratory (Frechette, Lizzeri, and Perego, 2022; Aristidou, Coricelli, and Vostroknutov, 2019; Au, Kwon, and Li, 2023). These papers test setups with a single receiver, whereas I focus on games with multiple interacting receivers.

Cheap talk with multiple receivers. The first more closely related literature studies other models of strategic information transmission experimentally, usually using cheap-talk games (Crawford and Sobel, 1982). This literature focuses on when information about the state of the world is transmitted to and trusted by receivers. It typically finds overcommunication, see Blume et al. (2020) for a recent survey.

In contrast to this large literature, I study the understudied setting with multiple interacting receivers, and I am first to show that this strategic interaction matters for a sender's optimal communication.[6] Theoretical work on communication with audiences began with Farrell and Gibbons (1989). This literature focuses on receivers that differ in their degree of preference misalignment, instead of modeling the receivers' strategic interaction. The presence of multiple receivers may lead the sender to communicate more truthfully using public or private messages than in cheap-talk games with a single receiver. In experimental tests of this work, communication is more truthful with public signals (Battaglini and Makarov, 2014; Drugov, Hernán-González, Kujal, and Troya-Martinez, 2021).[7] A recent literature on microtargetting studies messages that target heterogeneity between receivers, compared to public messages common to all voters (van Gils, Müller, and Prüfer, 2022; Tappin, Wittenberg, Hewitt, Berinsky, and Rand, 2023).

Within this literature, more closely related are two papers that capture some elements of audiences that interact strategically. However, neither one captures how a sender can enhance persuasion by choosing channels optimally, nor do they systematically vary the audience members' strategic interaction. Agranov and Schotter (2013) study an announcement game in which a player in the role of the government can choose to reveal information about the state to its citizen-players. The authors focus both on what information about the state is revealed when the preference misalignment between the government and its receivers varies and on which natural language is used.[8] Cooper, Hamman, and Weber (2020) consider a cheap-talk game in which a leader encourages followers to choose an action. Both papers fix the strategic interaction of the audience members. In contrast, I show that both anticipating the receivers' interaction and communicating publicly can be beneficial to a sender. I contribute empirical evidence on why public messages are prevalent in practice, whereas theoretically the benefits of these public messages are limited to games of strategic complements.

[6]A related literature compares behavior between games of strategic complements and substitutes (Fehr and Tyran, 2008; Potters and Suetens, 2009; Embrey, Mengel, and Peeters, 2019; Mermer, Müller, and Suetens, 2021).

[7]Kapoor and Magesan (2014) investigates public signals in the field. They find that when public information generated from traffic light countdowns is observable by all participants, it increases accidents.

[8]Conceptually related is work on language barriers. Introducing uncertainty about others' ability to understand messages may impede the efficiency of communication (Blume and Board, 2013; Blume, 2018; Giovannoni and Xiong, 2019), mirroring the importance of common knowledge about others' signals to enhance persuasion with strategic complementarities.

Global games. The second closely related strand experimentally studies strategic interactions global games (Carlsson and van Damme, 1993; Morris and Shin, 1998, 2002), where players in a game of strategic complements can receive private or common signals about the state of the world.[9] In contrast, I study a sender that attempts to persuade by coordinating agents' actions. Explicit coordination is a feature of many sender-audience interactions, such as governments' rhetorical interactions with their citizens, where I ask whether a sender can exploit the audience members' interaction to persuade them.

In experiments, behavior in the two types of information structures is more similar than theoretically predicted (Heinemann, Nagel, and Ockenfels, 2004, 2009; Cabrales, Nagel, and Armenter, 2007).[10] Trevino (2020) studies financial contagion between linked financial markets and finds that biases enhance contagion through traders' social learning, compared to contagion based purely on fundamentals. Avoyan (2022) allows agents in a global game to communicate, Szkup and Trevino (2021) study information acquisition in global games, and Mahmood (2023) studies global games with strategic substitutes.

3 Theoretical setup and experiment

In the laboratory experiment, I use an investment game devised by Bergemann and Morris (2019).[11] Here, I summarize key aspects of the theory underlying the experiment.

In this game, two firms simultaneously choose an action: to invest or not invest. Payoffs depend on both firms' actions. In addition, payoffs depend on the state of the world: $\theta \in \{\text{good}, \text{bad}\}$. Firms share the common prior of $Pr(\theta = \text{good}) = \frac{1}{2}$. Table 1 summarizes payoffs in the symmetric game, in which firm 1 is the row player and firm 2 the column player.

Table 1: Investment game

θ = good		Firm 2		θ = bad		Firm 2	
		invest	not invest			invest	not invest
Firm 1	invest	x+ϵ, x+ϵ	x, 0	Firm 1	invest	-1+ϵ, -1+ϵ	-1, 0
	not invest	0, x	0, 0		not invest	0, -1	0, 0

Here, x captures the payoff from investment in the good state, with $0 < x < 1$. ϵ characterizes the strategic interaction of the firms. When $\epsilon > 0$, the firms face strategic complements: their payoffs from investing compared to not investing are increasing if the second firm also invests. $\epsilon < 0$ implies strategic substitutes: payoffs from one firm's investment are decreasing in the second firm's investment.

In the experiment, I compare firms' behavior in a game with strategic complements to a game with strategic substitutes. Section 3.1 describes the parameterization and other details of how the game is implemented in the experiment.

[9] Related to this is the literature on sunspot equilibria, in which a sunspot realization serves as a correlation device. Coordination rates are higher than in the literature on correlated equilibria (Duffy and Fisher, 2005). Contrary to what theory predicts, both public and sufficiently correlated private signals generate sunspot equilibria (Fehr, Heinemann, and Llorente-Saguer, 2019).

[10] Cornand and Heinemann (2008) study theoretically to what extent signals in global games are optimally public. Experimentally, participants place a larger weight on a public signal over a private signal with stronger coordination incentives (Cornand and Heinemann, 2014).

[11] See Taneva (2019) on how to solve information design problems with common priors, as in this paper.

Sender. In addition to the two firms, this setup includes a sender (or information designer) who commits to an information structure. Conditional on the state realization, she sends a signal—in particular, a recommendation to firms to either invest or not invest. The probability that she makes a particular recommendation may depend on the state, as in typical persuasion games. Additionally, it can depend on the recommendation the other firm receives. This allows the information designer to (mis)coordinate the firms' actions.

To study persuasion setups, I study senders that maximize receivers' investment across all states. In doing so, and in assuming that the sender commits to an information structure, I connect to the literature on Bayesian persuasion (Kamenica and Gentzkow, 2011) and information design (Bergemann and Morris, 2019; Taneva, 2019).

In the first experiment, the sender is computerized and the choice of information structure is a treatment variable. Receivers have no information on the sender's intentions. In the second experiment, participants in the role of senders are explicitly incentivized to maximize investment. They receive a payoff for each receiver that chooses to invest. The goal and payoff structure are known to the receivers.

The (computerized) sender can persuade the receivers to invest by committing to an information structure. In both experiments, this allows me to reveal the exogenously or endogenously determined information structure to the receivers.[12] This feature is essential, as it fixes receivers' beliefs about how persuasion will unfold, which allows me to cleanly attribute changes in receivers' behavior to a change in public or private communication. My main interest is in the receivers' strategic interaction and how this interaction affects the sender's optimal choice of channel; these are strategic elements that are also present with other communication protocols.

Information structures. Table 2 presents the notation for general information structures in this setup. Each cell gives the probability that, conditional on a given state, the row-column combination of action recommendations is sent to the firms. $p_\theta - r_\theta$ is the probability that each firm receives a separate recommendation to invest in state θ, and r_θ is the probability that both firms receive a simultaneous recommendation to invest in state θ.

Table 2: General information structures

$\theta = \text{good}$	invest	not invest
invest	r_{good}	$p_{\text{good}} - r_{\text{good}}$
not invest	$p_{\text{good}} - r_{\text{good}}$	$1 + r_{\text{good}} - 2p_{\text{good}}$

$\theta = \text{bad}$	invest	not invest
invest	r_{bad}	$p_{\text{bad}} - r_{\text{bad}}$
not invest	$p_{\text{bad}} - r_{\text{bad}}$	$1 + r_{\text{bad}} - 2p_{\text{bad}}$

For a sender, it is optimal to always recommend investment to both firms in the good state and thus to set $r_{\text{good}} = p_{\text{good}} = 1$. Investment is always profitable in the good state. By maximizing investment in this state, the sender generates positive expected payoffs for receivers. This enables her to also sometimes recommend investment in the bad state, counterbalancing the gains in the good state with some expected losses in the bad state. This increases expected investment, as with the persuasion trade-off in Kamenica and Gentzkow (2011).

My focus, however, is on how the information structure's publicness affects persuasion. The information designer may use a public information structure by setting $r_{\text{bad}} = p_{\text{bad}}$ and

[12] Experimentally, whether senders exploit the power of commitment in Bayesian persuasion is the focus of Frechette et al. (2022). Theoretical sources for commitment are verifiability (Titova, 2022) or repeated interaction and public summaries (Best and Quigley, 2022).

$r_{\text{good}} = p_{\text{good}}$. In doing so, all firms always receive identical recommendations; messages are perfectly coordinated. Perfectly coordinating the signals generates common knowledge in the sense that both receivers know that they have received identical recommendations and have identical knowledge about the state. In the experiment, the receivers use the information structure to infer this perfect correlation. In practice, when persuading receivers to take an action, revealing information in a public announcement generates exactly the required common knowledge: all receivers are aware that this action has been recommended to each receiver.

Alternatively, the designer may use a private information structure. For example, she can set $r_{\text{bad}} = 0$ and $p_{\text{bad}} > 0$ in the bad state. Based only on the recommendation one firm received, this firm cannot infer with certainty what recommendation the other firm received. With a private information structure, firms' actions can be miscoordinated when the firms follow recommendations, as sometimes one firm invests while the other firm does not. Private signals feature two components: firms receive different signals and do not observe the other firm's signal. The definition of the private signals considered in this experiment, in which $r_{\text{good}} = 1$ and $p_{\text{bad}} - r_{\text{bad}} > 0$, clarifies why each receiver's private signal cannot be revealed to both receivers. Conditional on the state being bad, each firm receives the recommendation to invest with probability $p_{\text{bad}} - r_{\text{bad}}$. In that case, the other firm then receives the recommendation not to invest. If these two recommendations were revealed to both receivers, they would learn that the state is bad. In the bad state, the receiver can no longer best respond by investing. Therefore, when private signals are publicly revealed, the sender can no longer persuade receivers to invest in the bad state. The misaligned interests in the bad state between sender and receiver require that private signals remain private.[13]

Besides coordinating or miscoordinating firms' actions, a signal also transmits information about the state of the world, which a receiver can use to form a Bayesian posterior. Assume that a sender always recommends investment in the good state ($r_{\text{good}} = 1 = p_{\text{good}}$) and uses public signals that recommend investment with a probability of 50% in the bad state ($r_{\text{bad}} = p_{\text{bad}} = 0.5$). Conditional on receiving the recommendation to invest, the sender believes that the state is good with $Pr(\theta = \text{good}|\text{invest}) = \frac{Pr(\text{invest}|\theta=\text{good})Pr(\theta=\text{good})}{Pr(\text{invest}|\theta=\text{good})Pr(\theta=\text{good})+Pr(\text{invest}|\theta=\text{bad})Pr(\theta=\text{bad})} = \frac{.5}{.5+.25} = \frac{2}{3}$. Therefore, the firm learns that the state is more likely good than it believed before receiving the recommendation to invest. Given the new posterior, investment may now be profitable.

Obedience. Obedience constraints capture the degree to which a firm can trust an information designer and implement the recommended action.[14] Consider a firm receiving the recommendation to invest. It can use this recommendation to infer information about the state and about the action recommended to the second firm. By choosing the probabilities for each action recommendation appropriately, the information designer can ensure that firms' best response is to follow her recommendations. Following a recommendation is obedient if taking the recommended action is a best response; in that case, the Bayes Nash equilibrium is for both firms to follow. Knowing what is obedient allows the information designer to anticipate receivers' responses to different information structures. Then she can optimize over structures knowing firms' responses.

[13]It might also not be in the receivers' own interest to reveal signals truthfully. Conditional on considering investing, a receiver wants the second receiver not to invest in games of strategic substitutes and wants the second receiver to always invest in games of strategic complements. In the experiment, information cannot be shared.

[14]For a formal definition following Bergemann and Morris (2016), see Appendix Section A.

When a risk-neutral firm receives the recommendation to invest, obedience holds iff

$$\frac{1}{2}\underbrace{\left(r_{\text{bad}}\left(-1+\epsilon\right)+\left(p_{\text{bad}}-r_{\text{bad}}\right)\left(-1\right)\right)}_{\text{Investment in the bad state}}+\frac{1}{2}\underbrace{\left(r_{\text{good}}\left(x+\epsilon\right)+\left(p_{\text{good}}-r_{\text{good}}\right)x\right)}_{\text{Investment in the good state}}\geq 0 \tag{1}$$

To verify obedience, receivers first use Bayes' rule (for compactness, I cancel out common terms in Equation 1). The right-hand side equals 0, as the payoffs from no investment are normalized to zero.

Theoretically, all obedient information structures capture the set of Bayes correlated equilibria (Bergemann and Morris, 2016). In this experiment, I determine whether this representation corresponds to game play in the laboratory or whether some equilibria are easier or more difficult to induce than others.

For each information structure, games of strategic substitutes feature a unique equilibrium, while games of strategic complements generally feature two equilibria. I discuss equilibria for the parameters and information structures in the experiment in Section 3.2.

3.1 Experimental implementation of the investment game

In the laboratory experiment, players face either strategic complements or substitutes. In addition, they face (i) either private or public information structures, and (ii) different information structures, which vary their expected payoffs from following recommendations. In the first experiment, these two characteristics are varied exogenously. In the second experiment, they are chosen by another participant in the role of the sender.

The games are parameterized and normalized such that all payoffs are non-negative. All payoffs are denoted in points, which are exchanged at a rate of one point for five cents.

Table 3 presents the payoffs when the firms face strategic complements. As in the general example, investing is profitable only when the good state materializes. Firms face strategic complements, as the firms receive higher payoffs when both firms simultaneously invest. For example, if firm 1 invests in the good state, its payoff increases from 180 points to 210 points if firm 2 switches from not investing to investing.

Table 3: Game with strategic complements

$\theta =$ good		Firm 2	
		invest	not invest
Firm 1	invest	210, 210	180, 170
	not invest	170, 180	170, 170

$\theta =$ bad		Firm 2	
		invest	not invest
Firm 1	invest	100, 100	70, 170
	not invest	170, 70	170, 170

Table 4 presents the payoffs when the firms face strategic substitutes. As in the game with strategic complements, investment is only profitable in the good state. In contrast to that game, firms prefer that the other firm does not invest: firm 1's payoff from investing decreases when Firm 2 switches from not investing to investing.

Table 4: Game with strategic substitutes

θ = good		Firm 2 invest	Firm 2 not invest
Firm 1	invest	210, 210	260, 170
	not invest	170, 260	170, 170

θ = bad		Firm 2 invest	Firm 2 not invest
Firm 1	invest	20, 20	70, 170
	not invest	170, 70	170, 170

Both states are equally likely ($Pr(\theta = \text{good}) = 0.5$). Without information beyond this prior, firms would not be willing to invest in this game, as expected profits from investing are negative. The information designer can persuade firms to invest by conditioning signals on the state.

This experiment's primary interest is in understanding how players respond to different information structures. To this end, players face different exogenously designed information structures in the first experiment. Here, the role of the information designer is computerized. The structures themselves are revealed to participants. Across all information structures, all players always receive the recommendation to invest in the good state ($r_{\text{good}} = p_{\text{good}} = 1$). Players then either face private ($r_{\text{bad}} = 0$) or public information structures ($r_{\text{bad}} = p_{\text{bad}}$). In the first experiment, this is varied between subject.

For each class of information structures (private or public), each player faces three different information structures. They vary players' expected payoffs from following recommendations. Two of the information structures are obedient for risk-neutral players. *Optimal* structures yield close to the highest possible investment frequencies and thus are optimal for an information designer maximizing investment. If both firms follow the recommendations, their expected gains are barely positive, with fewer than five points for each firm. *Low* structures feature a less frequent recommendation to invest in the bad state. This decrease in frequency increases expected gains from following the recommendations to at least 22 points per firm and leads to a comparatively low level of investment. Unlike the *optimal* structures, *low* structures are also obedient for moderately risk-averse receivers.

Finally, *high* structures frequently feature the recommendation to invest in the bad state. These structures are not obedient, as they too frequently feature the recommendation to invest. If both firms follow these recommendations, they expect to lose more than five points.

Table 5 presents parameters and the receivers' probabilities of investing in the Bayes Nash equilibrium with maximal investment.

Table 5: Treatment table: Information structures

	Complements				Substitutes			
	Public		Private		Public		Private	
	r_{bad}	Pr(invest)	$p_{\text{bad}} - r_{\text{bad}}$	Pr(invest)	r_{bad}	Pr(invest)	$p_{\text{bad}} - r_{\text{bad}}$	Pr(invest)
High	71%	0%	48%	0%	32%	58%	48%	62%
Optimal	48%	74%	34%	67%	23%	62%	34%	67%
Low	19%	60%	14%	57%	10%	55%	14%	57%

Notes: Treatment parameters within the information structures (r_{bad}, $p_{\text{bad}} - r_{\text{bad}}$) and the probability each firm will invest in the equilibrium with maximal following (Pr(invest)). The left panel shows parameters for games of strategic complements, the right panel for games of strategic substitutes. Across all information structures, firms always receive the recommendation to invest in the good state ($r_{\text{good}} = p_{\text{good}} = 1$). r_{bad} is the probability that firms receive the joint recommendation to invest in the bad state. $p_{\text{bad}} - r_{\text{bad}}$ is the probability that only one firm receives the recommendation to invest, while the other receives the recommendation not to invest, in the bad state. With public structures, only common signals are used: $r_{\text{bad}} > 0$, while $p_{\text{bad}} - r_{\text{bad}} = 0$. With private structures, firms never receive the common recommendation to invest in the bad state: $r_{\text{bad}} = 0$, while $p_{\text{bad}} - r_{\text{bad}} > 0$. Within each level of obedience—*high*, *optimal*, and *low*—I fix the expected profits from following recommendations, assuming that the other receiver follows. *Optimal* and *low* are obedient for risk-neutral receivers.

Fixing the level of obedience, I set parameters such that the private information structures are identical between games of strategic complements and substitutes. For example, at the *optimal* level, each firm receives the private recommendation to invest in the bad state with a probability of 34% in both games. When following, this leads to identical expected profits across the two games.

The strategic advantage of public structures in games of complements and the advantage of private structures in games of substitutes become evident in the difference between public and private structures within each level for each game. Within each level of obedience, I fix expected profits from following the recommendations and then calculate the implied probability of recommending joint investment to both firms. In games of complements, this is a higher probability than was the case with private structures. For example, at the *optimal* level, both firms receive the recommendation to invest in the bad state with a probability of 48%, instead of the 34% with private structures. Crucially, in both public and private structures at the *optimal* level, firms expect to gain about five points if both firms follow. In games of substitutes, the probability of investment with public signals is lower than the probability with private signals. Again at the *optimal* level, firms receive the public signal to invest in the bad state with a probability of 23%, while they receive a private signal to invest in the bad state with a probability of 34%.

By fixing expected payoffs from following within each level (*low*, *optimal*, or *high*), play across the different structures (public versus private) becomes comparable. Signals are not equally informative across public and private signals, as the probability of the recommendation to invest in the bad state is changing.

In the second experiment, participants take on the role of the information designer. They receive a fixed payoff of 90 points each period and earn an additional 100 points for each receiver that chooses to invest. The senders choose among the six information structures that are used in the first experiment. Their choice thus entails two dimensions: Should they use a public or private information structure to persuade receivers? And which of the three levels of obedience should they use to maximize investment? After choosing a structure, the choice is revealed to participants jointly with the computer-generated signal.

3.2 Equilibria: Characterization and multiplicity

Conditional on choosing a particular information structure, these games generally feature two equilibria for games of strategic complements and one equilibrium for the games of strategic substitutes.

In the case of strategic substitutes, following an obedient information structure (*low* or *optimal*) constitutes the unique Bayes Nash equilibrium for risk-neutral receivers. If a structure is not obedient, only a mixed-strategy equilibrium survives, in which both receivers only probabilistically follow the recommendation to invest.

In the case of strategic complements, one Bayes Nash equilibrium for obedient structures is to follow recommendations. Therefore, as with strategic substitutes, *low* and potentially *optimal* information structures feature an equilibrium with following receivers. In the second Bayes Nash equilibrium, both receivers never invest, thus do not follow recommendations to invest. If one receiver does not follow the recommendation with sufficient likelihood, the equilibrium with full following is not attainable with complements. This is the case because only simultaneous investment by both receivers generates the complementary payoffs, $\epsilon = 30$ points. Crucially, this payoff is anticipated by the sender in calculating obedience, and receivers might no longer expect to gain from following recommendation if this payoff is not realized. This introduces another reason to potentially choose *low* structures: if receivers believe that others' best respond only noisily, it may no longer be a best response to follow in *optimal* information structures even for risk-neutral receivers. It is of theoretical interest in the literature on information design which of these equilibria prevails; for example, Mathevet et al. (2020) discuss sender-adversarial equilibrium selection. In the case of non-obedient information structures, the games of strategic complements feature only the equilibrium of not following.

When analyzing the experimental data, I use the equilibrium with the highest investment as a benchmark and compare data to this benchmark. This is the sender-preferred equilibrium and the unique equilibrium in games of substitutes. This equilibrium turns out to be a closer fit to the data than the alternative equilibrium with no investment in games of complements.

3.3 Theoretical predictions

In the first experiment, I test two dimensions central to the theory. First, I study the strategic advantage of public (private) structures in cases with strategic complements (substitutes).

Prediction 1. *Private structures induce more investment than public structures with strategic substitutes. Public structures induce more investment than private structures with strategic complements.*

The setup in this experiment captures the above predictions, which are typical in the information design literature. Table 5 illustrates the advantage of either public or private structures with the parameters of this experiment, within each level of obedience. With strategic complements, investments can be maximized with public signals; with strategic substitutes, private signals induce more investments than public signals.

Second, I test whether obedience captures empirical responses to information structures. Based on the expected profits, following is expected to be strongest in *low* levels. Following in *optimal* levels is equal to or lower as in *low* levels. The ranking of *low* and *optimal* depends on receivers' risk aversion: risk-neutral receivers follow in *optimal* structures; however, sufficiently risk-averse receivers follow only in *low* structures. The least amount of following is expected in *high*, levels, in which the choice to always follow does not constitute a best response.

Prediction 2. *The frequency of following recommendations is characterized by the following ranking:*

$$\textit{low} \geq \textit{optimal} > \textit{high}$$

Theoretically, the information designer anticipates the receivers' responses across different information structures. She can use these responses to choose structures advantageous to herself. However, empirically, play may differ. As a first step, players need to update their beliefs and comprehend that the information released in the recommendation is valuable. As a second step, players must choose accordingly and understand that following obedient information structures is profitable. What makes this setup particularly interesting is the inferences players make about others' behavior. Obedience relies on the common knowledge of players following recommendations.

In the second experiment, I focus on the information designers' choices. To maximize their own expected payoffs, if senders assume that the receivers are risk neutral, they can choose the information structure that maximizes receivers' expected investment. The first way they can do so is by exploiting the channel that theoretically enhances persuasion in each game.

Prediction 3. *In games of strategic complements, information designers choose public structures more often than they do in games of strategic substitutes.*

Second, payoff-maximizing senders choose the level of obedience that maximizes the level of investment conditional on receivers following:

Prediction 4. *Information designers choose structures according to the following ranking:*

$$\textit{optimal} > \textit{low} > \textit{high}$$

3.4 Experimental design

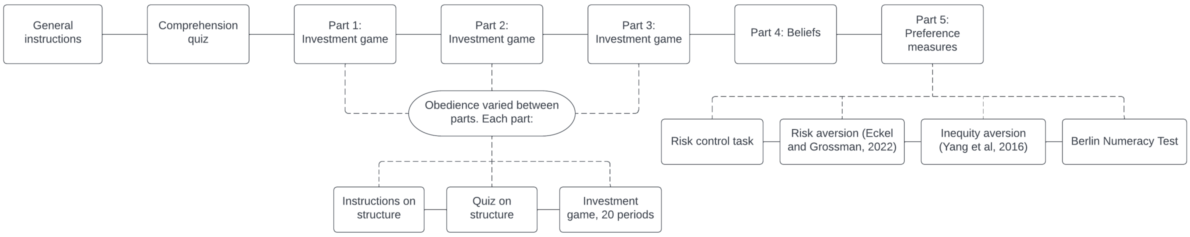
The experiment closely follows the theoretical setup, except that in the laboratory, the game is framed as two workers' decision to work or not work, not two firms' decision whether to invest. Each player's payoffs depend on their own decision and the decision of their coworker. In the first experiment, a computerized manager recommends actions, while in the second experiment this role is played by a participant. Information structures are implemented as a recommendation plan, according to which the workers receive recommendations. The state in the investment game is implemented as the randomly determined difficulty of the project, which is called difficult or easy.

At the moment that receivers decide, the screen summarizes the recommendation they received, the game, and the recommendation plan. After their decision, the state and the recommendations are revealed, participants learn their and their coworker's payoff and, in the second experiment, the manager's payoff. In addition, they learn what payoff they would have received if they had chosen the alternative action. In the second experiment, the sender's decision screen summarizes, for each available information structure, how frequently receivers in their matching group invested and followed recommendations in earlier periods.

First experiment. In the first experiment, I vary two between-subject treatment dimensions: (i) whether the strategic interaction of the receivers features complements or substitutes and (ii) whether the information structure that receivers face uses public or private signals.

Participants first receive general instructions on the investment game and have to pass a comprehension quiz. The investment game is played in three parts, with 20 periods per part. In each of these parts, players face one of the three levels *low*, *optimal*, and *high*. This treatment dimension, the third, varies within subject and with a counterbalanced order. At the beginning of each part, players first receive specific instructions for the new information structure and a comprehension quiz. Figure 1 shows the timeline of this experiment. Participants are allocated to matching groups of six participants, with random rematching every period.

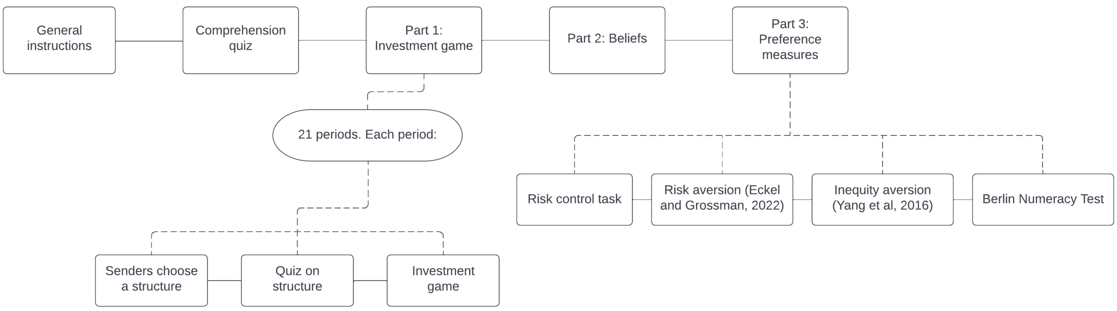
Figure 1: Timeline in the first experiment



Second experiment. In the second experiment, participants again first receive general instructions. For receivers, these are instructions similar to the first experiment, but they include some additional instructions on the senders' choice set and incentives. For senders, these instructions fully describe their own and receivers' decisions. Both senders and receivers have to pass a comprehension quiz afterward. During the experiment, senders also receive information about the receivers' responses to the information structures the senders in their matching group chose earlier. In this experiment, I vary only one between-subject treatment dimension: whether the receivers' strategic interaction features strategic complements or substitutes. To persuade receivers, the senders choose among the six different information structures that are varied exogenously in the first experiment. As in the first experiment, the information structure is revealed to the receivers.

This investment game is played only in one part, with 21 periods. Each period, receivers also have to answer one randomly selected question from a comprehension quiz similar to the quiz in the first experiment. Figure 2 shows the timeline of this experiment. Participants are allocated to matching groups of nine participants, with three senders and six receivers, with random rematching every period.

Figure 2: Timeline in the second experiment



Additional elicitations. The experiment concludes with measurements of beliefs and participants' characteristics to investigate mechanisms. In both experiments, I elicit participants' beliefs induced by the information structures. I elicit beliefs about whether the state is good and whether the other participant decides to invest—once for other participants that receive the recommendation to invest, and once for other participants that receive the recommendation not to invest. Participants predict in how many of 10 randomly drawn decisions the state was good and in how many decisions others invested, conditional on those participants having received the recommendation to invest or not invest. In the first experiment, this generates a set of 12 reports, 4 for each of the three levels of obedience. Out of the 12 reports, 1 is randomly drawn to be paid out. In the second experiment, beliefs for all six structures (combinations of public versus private and the three levels of obedience) are elicited and again 1 report is randomly paid out. If their report matches the actual value for 10 randomly selected instances, they receive a payment of 40 points in both experiments.

Second, I elicit participants' choices in an individual decision-making transformation of the investment game. The transformation strips away the strategic aspect of the game. By comparing choices between the two environments, we learn about the importance of these strategic aspects. Within each level of obedience, all structures and games generate equal expected payoffs. However, structures and games differ in their riskiness. In particular, the payoffs from investment in the bad state differ between public and private structures as well as between games. The probabilities of the bad state, conditional on receiving the recommendation to invest, counterbalance the difference in the payoffs. This preserves expected payoffs but affects the variances of payoffs. For example, joint investment with complements pays 100 points with public signals, while separate investment with private signals only pays 70 points. This payoff difference is offset by recommending investment in the bad state with a probability of 48% with public signals but only 34% with private signals. To measure whether individuals change their behavior in individual decision-making in agreement with the patterns I observe in the investment game, I introduce an individual control task. To generate this task, I use the investment game and associated information structure. Then, I assume that the second receiver follows recommendations, which removes the strategic element of the game. I compute expected payoffs from following a recommendation to invest for the game and for all information structures that each participant faces in the experiment, once conditional on the bad state materializing and once conditional on the good state materializing. The required probabilities of either state occurring are defined by the Bayesian posterior for the good and bad state materializing, conditional on the recommendation to invest. With the implied posterior probability, the good state materializes or the bad state materializes. In the experiment, the decision is framed as a lottery choice. The participants can choose a safe payoff, calibrated to match the payoff from no investment in the investment game. Alternatively, they can choose a risky payoff. This leads to a gain corresponding to the expected profit from investment in the good state, with the Bayesian posterior of the good state occurring when investment is recommended. With the remaining probability, this leads to a loss corresponding to the expected loss from investment in the bad state.

Third, I elicit risk preferences using the Eckel and Grossman (2002) task. Fourth, I elicit the parameters of the Fehr and Schmidt (1999) model for inequity aversion using the task in Yang, Onderstal, and Schram (2016). Fifth, participants' skills in understanding statistical information and risk are measured using the Berlin numeracy test (Cokely, Galesic, Schulz, Ghazal, and Garcia-Retamero, 2012).[15] Screenshots of all instructions are presented in Appendix Section C.

Experimental procedures. Both experiments, hypotheses and all analyses are preregistered at the AEA RCT registry (Ziegler, 2021, 2022). Experimental payments are exchanged at a rate of one point for five cents. In Appendix Section B.1, I provide balancing tables for both experiments. Treatments across all experiments are balanced, apart from Aheadness aversion in

[15]In the second experiment, only the first and third questions are used.

the second experiment (p-value=0.097). Controlling for this measure does not affect the results.

The first experiment was conducted in March 2021. Due to COVID-19 restrictions, the experiment was conducted online using a standard laboratory sample. The participants were recruited from the traditional subject pools of CREED at the University of Amsterdam in the Netherlands and MELESSA at LMU Munich in Germany, with the participants at MELESSA using ORSEE (Greiner, 2015). Both laboratories frequently conducted online experiments at that time, and protocols for running them online were in place. Besides the computerized experiment, participants were required to join a Zoom meeting with the experimenter. Participants were anonymized in the meeting and could only chat with the experimenter. This allowed close monitoring of potential problems, and participants could ask questions as in regular laboratory sessions. To verify their identity, participants either received a personalized link (at MELESSA) or had to verify their identity by taking pictures of themselves and their student ID using their webcams. Images were stored separately and deleted immediately after the sessions. Payments were implemented using bank transfers. Participants recorded their IBAN (and never their names or any other personal information) either in separate surveys (LimeSurvey at MELESSA) or in separate parts of the experimental software (at CREED). Almost all participants finished the experiment: out of 432 participants, only 1 participant dropped out (because of technical problems). This participant made 48 out of 60 decisions in the first three parts.

In the first experiment, payments were given for two randomly selected periods, each from a different randomly selected part. In total, 432 participants joined for 1 of 18 sessions, 288 of them being registered at CREED. Each session consisted of three to five matching groups, with six participants per matching group. The average age was 22.7 years. 249 out of the 432 participants were women; average earnings were 26.3 euros; and sessions took on average 82 minutes.

The second experiment was computerized and conducted in person in August and September 2022, in the laboratories of CREED in Amsterdam and MELESSA in Munich. In total, 360 participants joined for 1 of 22 sessions, 225 of them being registered at CREED. Participants received payments from two randomly selected periods. They were paid out in cash in all sessions apart from three sessions at MELESSA, which used the same payment procedure as the first experiment. Each session consisted of one to four matching groups, with nine participants per matching group. The average age was 22.6 years. 202 out of the 360 participants were women; average earnings were 26.9 Euros; and sessions took on average 99 minutes.

4 Results: Experiment 1

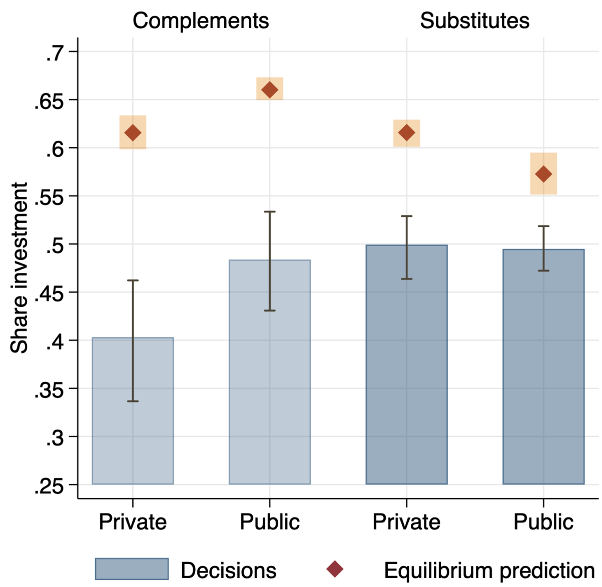
This section presents the results of the first experiment. The data from that experiment allows me to study receivers' behavior in different games and in response to exogenously assigned information structures.

4.1 Investments

The experiment was set up to measure whether receivers can be persuaded to invest. The measure of investment share is shown in Figure 3. Unless otherwise noted, all figures compare data on the two obedient levels (*low* and *optimal*) to ease interpretation, as this holds constant the existence of an equilibrium with full following. For regressions, I pool all data. Results are robust to using either approach.

The red diamonds illustrate equilibrium predictions. For strategic complements, theory predicts higher investment in public than in private structures. For strategic substitutes, theory predicts higher investment in private structures than in public ones.[16]

Figure 3: Investment decisions



Notes: Average frequency of investment by treatment, bars indicate observed choices and red diamonds choices in the Bayes Nash equilibria with the highest investment. The figure only uses data from *low* and *optimal* structures. Bars and shaded areas indicate 95% bootstrapped confidence intervals.

Overall, investment rates are substantial, with an average investment of 47% across all treatments. Absent information beyond the prior, for both separate and joint investment, investing would not be profitable, as participants would expect to lose between 5 and 55 points. Therefore, the appropriate benchmark to evaluate whether persuasion succeeded is no investment. This benchmark is also consistent with the individual risk measurement discussed in Section 4.4.

Participants frequently invest when receiving the recommendation to do so. The high investment rate suggests that the participants trust the signals they receive and trust their fellow participants to make the same inference as themselves. This can be interpreted as a mark of successful information design, as persuasion frequently succeeds.

Trusting others to follow is most crucial in games of strategic complements. In these environments, investing is only profitable if other receivers are also investing. Empirically, receivers invest in 44% of these cases. In contrast, in games of strategic substitutes, others' non-following reinforces the incentives to invest. Consistent with this difference in strategic incentives, average investment frequencies increase to 50% in games of substitutes.

[16]The theoretical treatment effects shown in Figure 3 are comparatively small because these data are averaged across obedience levels (*low* and *optimal*). For the *low* level, theoretical differences are relatively small, while I chose parameters to generate large treatment differences for *optimal* information structures. For example, the theoretically predicted interaction effect of public versus private signals interacted with the game is 14.9 percentage points with *optimal* structures (see Appendix Section B.2). I discuss parameter choices in more detail in Appendix Section A.1.

These data are also informative about equilibrium selection in games of strategic complements. For the two obedient structures in these games, investment is predicted in 64% of cases in the equilibrium of maximal following. Thus, empirically, investment frequencies come closer to the equilibrium with maximal investment, and inducing this equilibrium is frequently successful.[17]

Nevertheless, even though always following is an equilibrium for risk-neutral receivers, overall investment is still below the predicted investment. Two forces contribute to this finding. First, participants' beliefs exhibit some conservatism in updating about the probability that the state is good when receiving a recommendation to invest, which decreases expected profits from investment. This feature is discussed in more details in Section 4.3. In addition, these predictions assume risk neutral receivers. However, empirically, many participants exhibit risk aversion in the two control tasks at the end of the experiment. Using estimates of risk aversion from these tasks in the equilibrium prediction captures that empirically, investment rates are lower, and partially even predict lower investment than observed. I discuss this exercise in Appendix Section B.3.

Table 6 presents estimation results of the treatment effect. All columns compare investment behavior in the data (columns (1), (3), (5), and (7)) to the predicted behavior in the Bayes Nash equilibria with maximal investment (columns (2), (4), and (6)). To generate the equilibrium data, I use the recommendation draws from the experiment, and impose equilibrium following from the equilibrium with maximal following. Columns (1) and (2) compare data only within games of strategic substitutes, columns (3) and (4) only within games of complements. The key specifications are columns (5) and (6), which pool all data.[18] These specifications allow for a difference-in-differences interpretation between games and information structures. Column (7) only uses data from obedient information structures, as in Figure 3 (r_{bad} and $p_{\text{bad}} - r_{\text{bad}}$ at *low* and *optimal* levels).

Strikingly, the comparative statics for public and private information structures reveal a surprising pattern and an advantage of public information structures in the data across all strategic environments. Private structures perform no better with strategic substitutes than public ones (coefficient of -0.009 on Public; p-value=0.643; column (1)). This contrasts with the equilibrium prediction of higher investment with private signals (coefficient of -0.043; column (2)). In games of strategic complements, public structures increase investments by 9 percentage points (p-value=0.034; column (3)). This is in line with the theoretical prediction that public signals perform well with strategic complements. However, the empirical treatment effect exceeds

[17]The difference between predicted and observed investment is to a large extent driven by the fact that only some receivers within each group are not willing to invest when they receive the recommendation to invest. If instead the equilibrium without investment drove the behavior of some groups and thereby explained the difference between predicted and observed investment, we would expect to see some groups with very low average investment and some with high average investment. However, even at the *optimal* level, we observe low investment, coded as average investment in at most 3 of the 20 periods, for only 4% of groups. This rareness is inconsistent with the possibility that a non-investment equilibrium is prevalent for some groups. While the alternative equilibrium without investment exists, this does not appear to limit the sender-optimal equilibrium's attainability.

[18]The negative coefficient in column (6) on Complements is driven by *high* information structures. In that case, following one's recommendation does not constitute a Bayes Nash equilibrium. With complements, this implies no investment. A mixed-strategy equilibrium with partial investment arises with substitutes, where recommendations are followed only probabilistically. The coefficient is not significant in *optimal* and *high* structures, as the private structures across these two games are designed to be identical and recommend investment equally often. The maximal-investment Bayes Nash equilibria have both players always following these recommendations. Therefore, they induce equal investment.

the theoretically predicted benefit of just 3 percentage points (column (4)).

Column (5) documents the interaction effect—moving from private to public signals and from games of substitutes to games of complements—which is the main effect of theoretical interest. Investment increases by 10 percentage points (coefficient on Public × Complements; p-value=0.035; column (5)) when using public compared to private signals and when moving between games. Again, this slightly exceeds the theoretically predicted increase of 7 percentage points (column (6)).

To show that investments increase with public structures compared to the theoretical predictions, I interact models (5) and (6) and show estimates in Appendix Section B.2. Across both strategic environments, the empirical advantage exceeds the predicted advantage by 3 percentage points (p-value=0.080). This effect does not differ between strategic environments (p-value=0.604). At the *optimal* level, at which parameters are chosen to maximize power, the difference between the empirical and the predicted effect of public structures increases to 6 percentage points (p-value=0.024), while it is again similar between environments (p-value=0.421).

Summarizing, I find both evidence for the game-specific advantage of public signals in games of strategic complements and evidence for the general advantage of public signals. For the latter, I find that public structures do not perform worse than private structures even with strategic substitutes. This suggests that, in practice, public messages appear to possess inherent advantages when persuading receivers.

In Appendix Section B.2, I reproduce Figure 3 separately for all levels of obedience. As preregistered, I show that the analysis of Table 6 is robust to including controls, to using logistic regressions, and is similar over time in Appendix Section B.4. This also holds when only studying part-one data, where all treatment dimensions, including the level of obedience, were assigned between-subject.

Result 1. *Public information structures induce higher investments than private structures with strategic complements, more than theoretically predicted. In contrast, private information structures do not induce higher investment than public structures with strategic substitutes, contrary to theoretical predictions.*

The regression results in Table 6 also reveal how investment changes in *high* and *optimal* information structures compare to those in *low* structures. Consistent with the theoretical prediction that *high* information structures are not obedient, we observe less investment in this treatment. This effect is, however, smaller than theoretically predicted, especially for games of strategic complements. This implies that receivers partially trust recommendations they should not trust in equilibrium. Investment decreases by 4 percentage points (p-value=0.071; column (1)) when receivers face a *high* structure with strategic substitutes. For this game, investments are even predicted to increase in equilibrium for *high* structures (coefficient of 6% for *high* structures; column (2)), which highlights the empirical importance of persuading not too aggressively. With strategic complements, investment decreases by 7 percentage points (p-value<0.001; column (3)) when receivers face a *high* structure, consistent with the conjecture that when others do not follow, it reduces the incentive for own investment.

In addition, *optimal* structures do not increase investment compared to *low* structures. This

Table 6: Treatment effects: Investment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Substitutes		Complements			Diff-in-Diff	
	Data	NE	Data	NE	Data	NE	Data
Public	-0.009	-0.043***	0.087**	0.030***	-0.009	-0.043***	-0.004
	(0.019)	(0.011)	(0.039)	(0.007)	(0.020)	(0.011)	(0.022)
Complements					-0.108***	-0.211***	-0.096***
					(0.032)	(0.009)	(0.034)
Public × Complements					0.096**	0.073***	0.085*
					(0.045)	(0.013)	(0.045)
(1 if level=*optimal*)	-0.009	0.082***	-0.040*	0.122***	-0.025*	0.102***	-0.024*
	(0.018)	(0.014)	(0.021)	(0.016)	(0.014)	(0.011)	(0.014)
(1 if level=*high*)	-0.038*	0.058***	-0.073***	-0.577***	-0.055***	-0.260***	
	(0.020)	(0.013)	(0.020)	(0.011)	(0.014)	(0.039)	
Constant	0.514***	0.574***	0.445***	0.562***	0.533***	0.674***	0.508***
	(0.022)	(0.010)	(0.038)	(0.013)	(0.022)	(0.014)	(0.022)
Period trend, part and lab FE	Yes	No	Yes	No	Yes	No	Yes
Only obedient signals	No	No	No	No	No	No	Yes
Observations	12960	12960	12948	12948	25908	25908	17268
# clusters	36	36	36	36	72	72	72
# participants	216		216		432		432

Notes: The table reports OLS estimates and includes all data. The dependent variable is a dummy variable equal to 1 if the participant decided to invest (Data) or was predicted to invest in the Bayes Nash equilibrium with maximal investment (NE). Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

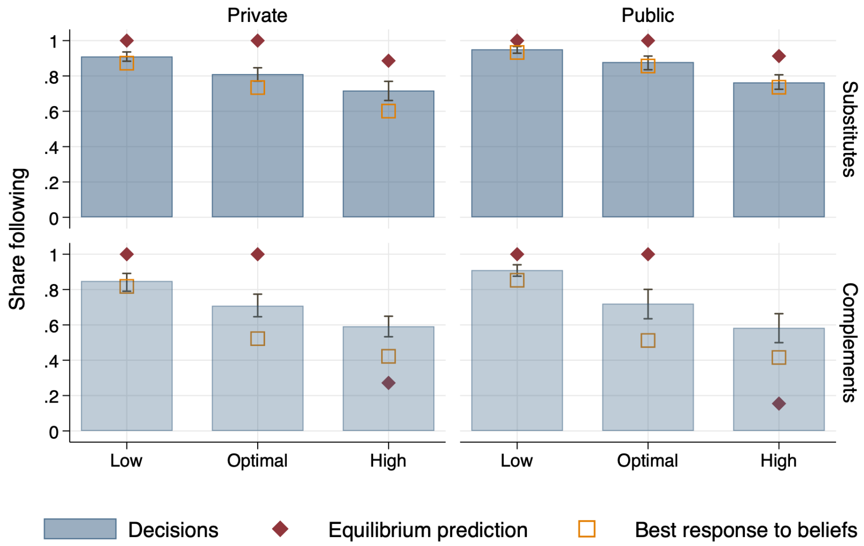
runs contrary to theoretical predictions when assuming risk-neutral receivers, as we expected an increase in investment (coefficient on *optimal* levels in columns (2) and (4)). Empirically, however, there is no significant effect for strategic substitutes (p-value=0.625; column (1)). For strategic complements, investment even decreases by 4 percentage points (p-value=0.068; column (3)). Some receivers are only willing to invest when substantial informational rents from following are available, consistent with some receivers' risk averseness. The next section discusses the following frequencies and obedience in more detail.

4.2 Following behavior

Participants face the critical decision of whether to trust and follow a recommendation. The investment behavior presented in Section 4.1 compounds two factors. First, how often is a recommendation to invest sent to receivers? Second, how often is this recommendation followed? As the former factor varies between information structures, focusing on the following behavior allows for a clean measure of receivers' responses to information structures.

Figure 4 presents average following behavior, differentiated by game, publicness, and information structure level. Following behavior is coded such that it is equal to 1 whenever a recommendation is followed (investing after the recommendation to invest, not investing after the recommendation not to invest), and zero otherwise. Table 7 reports accompanying regressions. Columns (1) and (3) use data, while columns (2) and (4) repeat the same analysis for predicted behavior in the equilibrium with maximal following. Columns (1) and (2) use data only from the obedient information structures (*low* and *optimal*), while columns (3) and (4) also use data from *high* structures. Column (2) reflects the equilibrium feature that all recommendations are followed in the equilibrium with maximal investment for obedient structures, as the estimate on the constant is one and there are no changes across treatment conditions.

Figure 4: Following rates



Notes: Average frequency of following a recommendation by treatment and by the level of the information structure. The variable is a dummy equal to 1 if a recommendation was followed (investment after the recommendation to invest, or no investment after the recommendation not to invest). Bars indicate observed choices, diamonds indicate the following rate in the equilibrium with the highest following, and squares are empirical best responses based on participants' separately elicited beliefs. Error bars indicate 95% bootstrapped confidence intervals.

Five facts emerge. First, receivers respond to the level of the information structure precisely as expected. Most following occurs with the strongest incentive to follow in *low* structures. The constant of 93% in column (1) indicates that in the baseline level (*low*), following is very prevalent and is close to the full following predicted in equilibrium in column (2). We observe intermediate levels of following for intermediate incentives in *optimal* structures. Compared to the omitted category *low*, following decreases by 13 percentage points in *optimal* structures (p-value<0.001; column (1)). Risk-neutral receivers are expected to respond equally to *optimal* and *low* structures; see column (2). Behavior in the laboratory is more nuanced, consistent with at least some risk-averse receivers. Last, there is the least following with the weakest incentives in *high* structures, with following rates 24 percentage points lower (p-value<0.001; column (3)).

Second, across most treatments, observed following is lower than in the Bayes Nash equilibria with the highest investment. For example, and not surprisingly, we can reject the null that there is full following for obedient structures (H_0: Constant=1; p-value=0.003; column (1)). Nevertheless, behavior is frequently in line with the sender-preferred equilibrium instead of the equilibrium with no investment, so we can reject the null that following in games of complements is canceling out the high baseline following in games of substitutes (H_0: Complements=Constant; p-value<0.001; column (1)). In addition, we observe more following than predicted in *high* levels. While following is predicted to decrease following by 44 percentage points with *high* levels (column (4)), following is observed to decrease by only 24 percentage points (p-value<0.001; column (3)). Therefore, some receivers continue to follow the recommendation to invest in *high* levels.

Third, participants are more likely to follow recommendations from public information

Table 7: Treatment effects: Following

	(1) Data	(2) NE	(3) Data	(4) NE
Public	0.054***	0.000	0.052**	0.009***
	(0.018)	(.)	(0.021)	(0.003)
Complements	-0.082***	0.000	-0.096***	-0.205***
	(0.029)	(.)	(0.030)	(0.004)
Public × Complements	-0.018	0.000	-0.030	-0.048***
	(0.039)	(.)	(0.043)	(0.005)
(1 if level=*optimal*)	-0.125***	0.000	-0.125***	-0.000
	(0.015)	(.)	(0.015)	(0.000)
(1 if level=*high*)			-0.241***	-0.444***
			(0.015)	(0.041)
Constant	0.934***	1.000	0.956***	1.110***
	(0.022)	(.)	(0.024)	(0.014)
Level of obedience	*low* & *optimal*		*low*, *optimal* & *high*	
Period trend, part and lab FE	Yes	No	Yes	No
Observations	17268	17268	25908	25908
# clusters	72	72	72	72
# participants	432		432	

Notes: The table reports OLS estimates and includes all data. The dependent variable is a dummy variable equal to 1 if the participant decided to follow a recommendation (invest after recommended to invest, or not invest after recommended not to invest) (Data) or was predicted to follow in the Bayes Nash equilibrium with maximal investment (NE). Columns (1) and (2) use data only from obedient structures, while columns (3) and (4) pool all data. Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

structures. Theoretically, this is surprising. The structures were designed to induce equal following in equilibrium for the obedient levels; see column (2). However, empirically, participants appear to trust private recommendations less than public ones, as following increases by 5 percentage points in public structures (p-value=0.004; column (1)). This feature drives the two key deviations from predicted investments reported in Section 4.1. First, with games of strategic substitutes, the higher following of public signals leads to similar investment rates across private and public signals. While private structures are more likely to recommend investment in the bad state, receivers' decreased following almost exactly cancels out this advantage. Second, with games of strategic complements, the increased following of public signals leads to the higher-than-predicted investment with public signals. Theoretically predicted effects are slightly different when including *high* structures in columns (3) and (4).[19] Nevertheless, the same pattern arises, as public recommendations are followed more frequently than theoretically predicted. In Section 4.4, I disentangle potential drivers of this effect.

Fourth, games of strategic substitutes generate higher following behavior than games of strategic complements. Following frequencies decrease by 8 percentage points with complements (p-value=0.006; column (1)). This is in line with the conjecture that receivers anticipate the noisy behavior of fellow receivers which decreases receivers' incentives to follow only in these games; as in equilibrium there is no change between games (column (2)). In games of strategic substitutes, when other receivers do not invest when they receive the recommendation to invest,

[19]This arises because the mixed equilibria for *high* levels in games of strategic substitutes feature slightly different following probabilities across public and private structures. In addition, recommendations not to invest are predicted to be followed in games of strategic complements but are sent at different frequencies for public and private structures.

it increases incentives to follow recommendations to invest. When the other receiver does not invest, it generates larger payoffs for receivers driven by the gains from investing in the good state. In contrast, receivers in games of strategic complements need the other receiver to invest to make their own investment profitable, especially in the *optimal* structure. Given that other receivers are not always following recommendations, following all recommendations is no longer a best response for receivers with complements.

Fifth, and most strikingly, behavior overall is remarkably close to the behavior in a best response to participants' beliefs. For this best response, I use beliefs about the state and about others' behavior conditional on each recommendation, described in Section 4.3. These beliefs were elicited only at the end of the experiment, so they represent the beliefs of experienced participants. Based on these beliefs, I predict which recommendations should be followed by payoff-maximizing risk-neutral receivers. To do so, I predict expected profits of following recommendations given each receiver's beliefs, and I predict they follow recommendations if the expected profit exceeds the no-investment payoff of 170 points. Since behavior is close to this best response, participants apparently understand this game well. When accounting for their beliefs about the play of others, which may differ from behavior in the Nash-equilibrium benchmark, as well as when accounting for their potential non-Bayesian inference about the state, participants behave close to what standard theory would predict. In addition, behavior is closer to the best response in public structures, as reported in Appendix Section B.5. This indicates that play is particularly sophisticated when participants face public signals, but less so when facing private signals. The closeness of behavior to the best responses is a mark of success of information design: we can use standard models to predict behavior. The next step is to investigate the induced beliefs.

Result 2. *Receivers respond to incentives to follow recommendations as theoretically predicted, and behavior is close to best responses to beliefs. In contrast to theoretical predictions, public information structures generate more following than equivalent private information structures. Consistent with theoretical predictions and moderate risk aversion, the frequency of following recommendations is characterized by the following ranking:*

$$\textit{low} > \textit{optimal} > \textit{high}$$

Appendix Section B.5 reports additional analyses. The results reported in Table 7 are similar when estimating the models using data only from recommendations to invest. In addition, they are robust to including additional controls. As expected, more risk-averse participants follow recommendations less. No characteristics other than gender correlate with following behavior.

4.3 Beliefs

In the first experiment, a computerized sender attempts to persuade receivers to invest by changing their beliefs. So far, we have observed that receivers' behavior changes. In the following, I present data on elicited beliefs for each between-subject treatment to measure whether the change in behavior is consistent with the changes in beliefs.

Theoretically, information on the state is inferred using Bayes' rule. In addition, in the equilibrium with maximal following, others are predicted to follow recommendations if they are obedient. In the experiment, participants reported beliefs at the very end after making all choices in the investment game. All beliefs presented here are conditional on having received the recommendation to invest.[20]

In the left panel of Figure 5, I show the average belief about the response of other participants to the recommendation to invest. The red diamond represents the observed following behavior. We observe that participants predict others' following behavior remarkably well.

In Table 8, I regress errors and squared errors in beliefs on treatments. The squared errors are informative about the presence of a prediction error. The errors are informative about the direction of this error, if present. Column (1) use the distance between the target and the reported belief about others' following a recommendation to invest and column (2) the squared distance. Prediction errors are larger for games of complements, in which receivers overestimate others' investment by 9 percentage points (p-value=0.025 in column (1); p-value=0.014 in column (2)). Errors also increase for *high* structures: receivers overestimate that others will invest by 14 percentage points (p-value<0.001 in column (1); p-value=0.002 in column (2)). Crucially, receivers predict others' following in public and private structures equally well (p-value=0.138; column (2)). The main deviation of behavior from theoretical predictions, the advantage of public signals, is also present in this belief channel.

In the right panel of Figure 5, I show the average belief that the state is good conditional on receiving the recommendation to invest. The red diamonds indicate the Bayesian posterior. In Table 8, I again regress errors and squared errors in beliefs on treatments, where column (3) uses the distance between the Bayesian posterior and the reported belief that the state is good after receiving the recommendation to invest, and column (4) uses the squared distance. Participants are generally slightly more pessimistic than predicted, so they under-respond to good news. This is reflected in the constant, in which they underestimate the odds that the state is good by 8 percentage points (p-value<0.001 in columns (3) and (4)). Otherwise, they only overestimate how likely the state is to be good in *high* structures, compared to *low* structures, by 3 percentage points (p-value<0.001 in column (3); p-value=0.009 in column (4)).

Result 3. *Beliefs evolve in line with Bayesian updating about the state and about the play of other receivers. Participants predict others' following behavior well and expect public structures to induce higher following.*

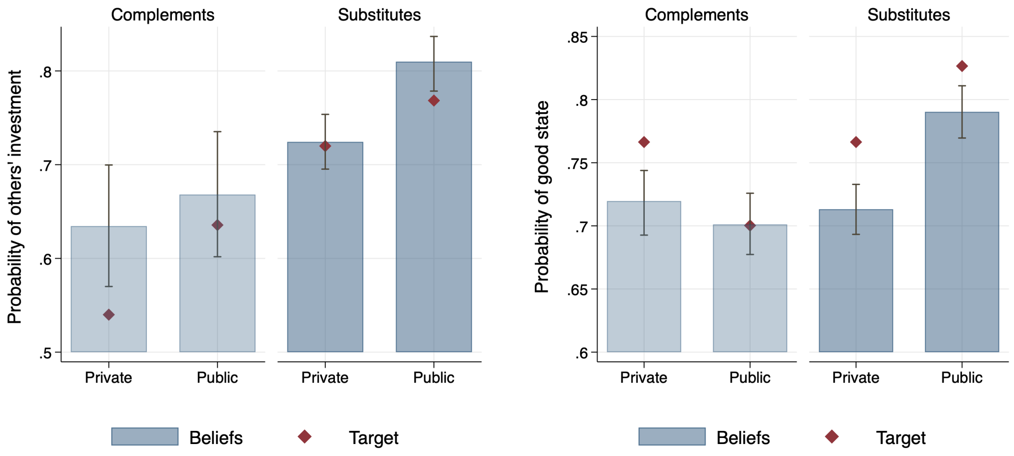
4.4 Mechanisms: Explaining the advantage of public structures

Contrary to theoretical predictions, participants are more willing to follow public signals. In addition, participants correctly believe that others do the same. In this section, I investigate mechanisms that may explain this advantage of public structures.

First, I study whether the advantage of public structures is still present in an individual control task that mirrors the game but removes the strategic interaction. The advantage of public structures can stem from two sources. First, it may result from the strategic uncertainty in the interaction with the other receiver. Second, different information structures may differ in their riskiness, even when stripped from the game.

[20] In Appendix Section B.7, I present averages for each level and for the recommendation not to invest.

Figure 5: Beliefs about the state and others' following behavior



Notes: Left panel: average reported belief that other participants invest, conditional on receiving the recommendation to invest, by treatment. Right panel: average reported belief that the state is good, conditional on receiving the recommendation to invest by treatment. This figure pools data from all levels of obedience. Bars indicate observed choices, diamonds indicate the observed target in the data, and error bars indicate 95% bootstrapped confidence intervals.

I first investigate the second possibility: differences in riskiness. Within the investment game, all structures are calibrated such that a risk-neutral receiver is equally willing to follow within each level of obedience (*low*, *optimal*, or *high*), but differences in riskiness may contribute to differences in choices observed in the investment game.

To obtain an individual control task for each structure and game, I remove strategic uncertainty about others' behavior by assuming that others follow their recommendations. Participants choose to either take a risky lottery, corresponding to following a recommendation to invest, or take the safe payoff, corresponding to not investing. The risky lottery is calibrated to match the expected payoffs and probabilities of the investment game and the associated information structure. Section 3.4 explains the task in more details.

Each participant makes three choices in this task, corresponding to the three information structures they face in the main parts of the experiment. Risk-neutral participants would accept the lotteries associated with the *low* and *optimal* structures and reject the lottery associated with the *high* structures. In Figure 6, I present the average share of participants who accept the risky choice. The red diamonds indicate the choices a risk-neutral participant makes. Table 9 presents the corresponding regressions of the decision to accept the risky lottery on treatment indicators.

The data indicate that the majority of participants are risk averse: while 86% accept the lottery corresponding to the *low* structures (coefficient on the constant, because *low* is the omitted category; p-value<0.001), as expected gains decrease, take-up of the lottery decreases: by 42% for the *optimal* lottery (p-value<0.001), which has an expected value just above the safe payoff, and by 74% for the *high* lottery (p-value<0.001) compared to the *low* lottery's take-up.

Crucially, there are no systematic differences between treatments (Public: p-value=0.528; Complements: p-value=0.279; Public×Complements: p-value=0.679). While behavior in the game indicates that participants are more likely to follow public signals, this increase in following

Table 8: Errors in beliefs

	(1)	(2)	(3)	(4)
	Others' following		State is good	
	Error	Error^2	Error	Error^2
Public	-0.038	-0.014	-0.018	-0.002
	(0.032)	(0.010)	(0.015)	(0.005)
Complements	-0.091**	0.032**	-0.011	0.004
	(0.040)	(0.013)	(0.016)	(0.004)
Public × Complements	0.102*	0.004	-0.027	0.001
	(0.054)	(0.016)	(0.023)	(0.006)
(1 if level=*optimal*)	-0.048**	0.003	-0.029***	-0.008***
	(0.020)	(0.008)	(0.007)	(0.003)
(1 if level=*high*)	-0.135***	0.029***	-0.054***	-0.004
	(0.022)	(0.009)	(0.009)	(0.003)
Constant	0.030	0.063***	0.084***	0.025***
	(0.033)	(0.009)	(0.012)	(0.004)
Part and lab FE	Yes	Yes	Yes	Yes
Observations	1293	1293	1293	1293

Notes: The table reports OLS estimates and includes all data. The dependent variables are errors in beliefs (target - belief) in columns (1) and (3) and squared errors in beliefs ((target - belief)^2) in columns (2) and (4). Columns (1) and (2) use the belief about others' investment after they receive the recommendation to invest. Columns (3) and (4) use the belief about the state being good after others receive the recommendation to invest. Public and Complements are dummy variables equal to 1 if the belief was reported for facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Beliefs were not elicited for one participant that dropped out earlier. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

is not present in this individual control task. Any change in behavior we see between these treatments is driven by the strategic interaction in the game and not by any differences in the riskiness of the structures.

Consistent with this finding, I do not detect significant correlations between following and a standard risk-preference measure (Eckel and Grossman, 2002), the treatment variables and their interactions (see Appendix Section B.8).

Table 9: Control lottery choice

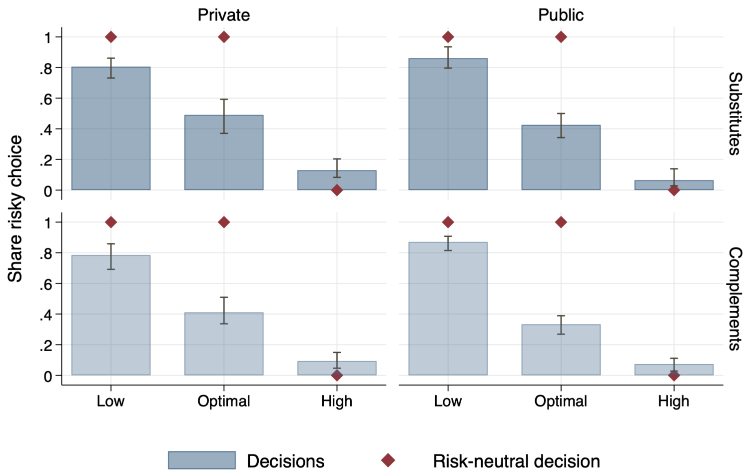
Public	-0.025	(0.039)
Complements	-0.045	(0.042)
Public × Complements	0.021	(0.050)
(1 if level=*optimal*)	-0.415***	(0.025)
(1 if level=*high*)	-0.740***	(0.024)
Constant	0.860***	(0.032)
Observations: 1293, # clusters: 72, # participants: 431		

Notes: The table reports OLS estimates and includes all data. The dependent variable is a dummy variable equal to 1 if a participant chose to take up the risky lottery, corresponding to following a recommendation to invest. Public and Complements are dummy variables equal to 1 if the lottery decision was made capturing a public information structure, with the omitted category being a private structure, or capturing a game with strategic complements, with the omitted category being a game of strategic substitutes. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Choices were not elicited for one participant that dropped out earlier. Standard errors in parentheses, clustered at the matching-group level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Result 4. *Differences in riskiness cannot explain the higher following in public information structures. Such structures' advantage only arises when receivers strategically interact.*

I also measure inequity aversion (Fehr and Schmidt, 1999), and I show in Appendix Section B.9 that it does not explain the higher following in public information structures. To summarize, I find that differences in play are not driven by features unrelated to the strategic nature of the game—namely, the game's inherent riskiness and inequalities in payoffs. In the following, I

Figure 6: Control lottery task



Notes: Bars indicate observed choices, diamonds indicate the risk-neutral choice, and error bars indicate 95% bootstrapped confidence intervals.

investigate two mechanisms that take the game's strategic component into account.[21]

As a first mechanism, I study whether private and public structures produce differences in the noisiness of behavior. Higher additional uncertainty about others' actions is detrimental to investment, as participants can no longer best respond by following recommendations. If public or private structures induce different degrees of noisiness, it may be desirable for a sender to rely more frequently on the less noisy environment to persuade receivers.

There are good reasons to expect that private structures generate more noisy behavior. One reason is that they require more complex strategic reasoning. Public signals generate common knowledge about others' signals. The symmetric decision structure with public signals may help receivers arrive at their best response and lead them to expect that others do so as well. In contrast, by design, private signals introduce uncertainty about others' signals. A corresponding increase in difficulty is consistent with Martínez-Marquina, Niederle, and Vespa (2019) finding that uncertainty—in this case about others' signals—contributes to failures of contingent reasoning. Similarly, Oprea (2020) finds that having to consider additional states—in this case the potential state of miscoordinated action recommendation, with one recommendation to invest and one not to invest—is perceived as complex and costly to process. In line with these findings, the number of errors in the quizzes associated with the information structures is significantly lower for public structures.[22] These quizzes directly measure their understanding for example, of what signals the second participant would receive if they themselves received a particular signal.

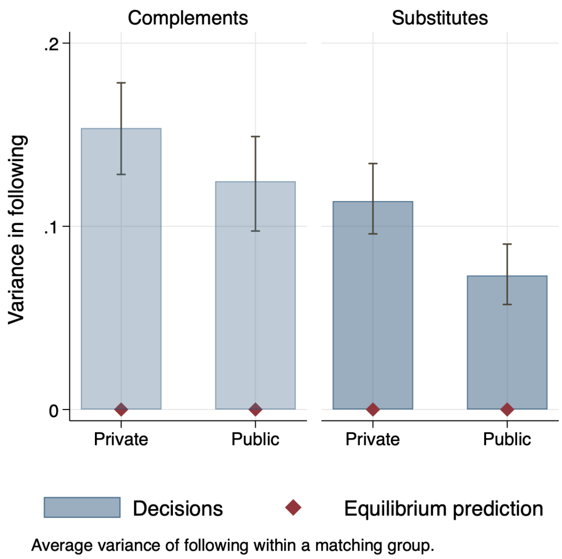
To document this mechanism, I begin by studying differences in the variance in the behavior between treatments. In Figure 7, I plot the average variance in the following behavior for *low* and *optimal* levels, calculated for each group and part separately. This provides a measure of how uncertain a participant is about the following decisions of participants within their matching

[21]The following analysis in this section is exploratory and was not preregistered.

[22]In a regression of the number of errors on treatment dummies, the coefficient on public is negative (-1.18, compared to a control average of 6.68) and significant (p-value=0.012, 431 observations, clustering standard errors on the matching-group level; all other coefficients are not significant at conventional levels).

group. Theoretically, there is zero variance in following behavior, as all obedient signals are always followed in equilibrium. Empirically, however, the more complex private signals generate noisier behavior than public signals: public signals decrease the standard deviation in following by 0.055 (coefficient on Public; p-value=0.009; Table 10; column (1)).[23]

Figure 7: Variance in following behavior



Notes: Average variance of following behavior, calculated on a matching group-part level. The figure only uses data from *low* and *optimal* structures. Bars indicate observed choices; red diamonds indicate the equilibrium predictions; error bars indicate 95% bootstrapped confidence intervals.

As explained, this increase in the variance in following behavior is detrimental to receivers' incentives to follow recommendations. Higher uncertainty about others' play implies that following is less frequently a best response. This was already documented in Section 4.2, as Figure 4 showed that the best response to receivers' beliefs implies lower following rates for private than for public structures. Even receivers' beliefs reflect the noisier behavior: there is more variance in beliefs about others' following a recommendation to invest for private than for public signals (see Appendix Section B.10).

Next, I show how this variance in behavior correlates with the treatment effects I find. Within each treatment, I split groups into those showing above- and below-median variance. I interact treatment indicators with a dummy variable capturing whether a group has above-median variance within each treatment in columns (2) and (3) in Table 10. In column (2), I focus on the decision to invest. I find the theoretically predicted advantage of private structures with strategic substitutes for the low variance groups (coefficient of 5 percentage points on Public; p-value<0.001). This effect, however, reverses for the high-variance groups, for which public structures induce higher investment than private ones (coefficient of 8 percentage points on Public $\times$ High variance; p-value=0.003). These two counteracting effects produce the nonsignificant treatment effect of public structures documented in Table 6. In column (3), we see that high-variance groups follow recommendations less frequently (coefficient of 12 percentage points on High variance; p-value<0.001). Here, public structures prove beneficial, as they generate

[23] In line with this analysis, estimated rationality parameters of quantal response equilibria suggest that play is closer to rationality in public than in private structures; see Appendix Section B.6 for details.

higher following rates for highly noisy groups (coefficient of 6 percentage points on Public × High variance; p-value=0.031). The noisy response to private signals thus indeed explains the superiority of public signals.

Table 10: Variance and heterogeneous effects

	(1) SD(following)	(2) Investment	(3) Following
Public	-0.055***	-0.049***	0.022
	(0.020)	(0.014)	(0.016)
Complements	0.053**	-0.074***	-0.073***
	(0.020)	(0.027)	(0.023)
Public × Complements	0.012	0.118***	-0.007
	(0.031)	(0.043)	(0.041)
High variance		-0.112***	-0.119***
		(0.022)	(0.020)
Public × High variance		0.081***	0.059**
		(0.026)	(0.027)
Complements × High variance		-0.050	-0.031
		(0.046)	(0.039)
Public × Complements × High variance		-0.062	-0.063
		(0.067)	(0.061)
(1 if level=*optimal*)		-0.025*	-0.126***
		(0.014)	(0.015)
(1 if level=*high*)		-0.055***	-0.241***
		(0.014)	(0.015)
Period		-0.003***	-0.003***
		(0.001)	(0.001)
Constant	0.378***	0.597***	1.024***
	(0.018)	(0.021)	(0.021)
Part and lab FE	Yes	Yes	Yes
Observations	216	25908	25908
# clusters	72	72	72
# participants	-	432	432

Notes: The table reports OLS estimates and includes all data. In column (1), the dependent variable is the standard deviation of the following behavior, calculated for each group and part separately. There are 72 groups making decisions across three parts each, which results in 216 observations. The dependent variables in columns (2) and (3) are the decision to invest and to follow a recommendation, respectively. High variance is a dummy variable equal to 1 if the average standard deviation of the matching group (calculated as in (1)) is above the median within each treatment. Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Result 5. *Private signals induce noisier behavior than public signals. The increased uncertainty lowers receivers' incentives to follow private signals, which decreases the sginals' persuasiveness.*

As a second mechanism, I show that participants' behavior is consistent with them disliking the differential treatment private structures produce. With private structures, at most one of the participants receives bad advice at any moment. Here, bad advice is the recommendation to invest even though the bad state materialized. If followed, this advice generates a loss for the receiver. In contrast, with public signals, both participants receive such a recommendation and simultaneously suffer losses when following it. Therefore, only participants with private structures can experience being the sole receiver losing out after trusting the sender. I show that participants' behavior is affected by being the only loss-making participant in games with private signals; when both make a loss with public signals, it does not change their behavior.

To study this mechanism, I focus on participants' response to having received bad advice in the past and how the response depends on whether they face public or private signals. I split participants into those who receive bad advice in the first period in which they face a new information structure and those who do not receive such advice. Then, I assess whether the behavior of these two groups differs in all subsequent periods when they face this structure.

In Table 11, I regress the decision to invest or to follow a recommendation on treatment dummies, a dummy for having received bad advice, and the interaction of the two. Participants who received bad advice are less likely to invest or follow in all future periods. Having received bad advice reduces investments by 12 percentage points (p-value<0.001; column (1)). Bad advice also decreases following by 13 percentage points (p-value<0.001; column (2)). However, this is solely driven by those participants who face private signals, as the interaction effect for public signals with bad advice almost exactly cancels out this baseline effect. Investment increases for public signals by exactly the 11 percentage-point loss measured for those having received bad advice (p-value=0.020; column (1)). Following increases by 11 percentage points (p-value=0.007; column (2)) for participants with public signals with bad advice, compared to those with private signals and bad advice. Column (3) shows that the effects on following are robust to including additional controls.[24]

Result 6. *Bad advice in private structures, but not in public structures, decreases investment in later periods after receivers experienced differential treatment.*

In Appendix Section B.12, I perform a back-of-the-envelope calculation of the relative contributions of the two mechanisms to the superiority of public signals. On average, public signals lead to 4 percentage points higher investment, which is an effect that is not predicted theoretically. When decomposing this effect, about 61% can be attributed to the complexity of private signals, with the remainder attributed to participants that had received bad advice decreasing their following.

5 Results: Experiment 2

I now present results from the second experiment, where human senders interact with receivers. In the second experiment, I explore whether receivers respond differently to human senders and how participants approach the sender's problem.

Participants in the role of sender are incentivized to maximize receivers' investment, a fact that receivers are aware of. This may change receiver behavior compared to the first experiment. If receivers' care about the senders' payoffs, potentially as captured by the receivers' social preferences or their concern for the senders' intentions, receivers may no longer be willing to follow recommendations as with computerized senders. Similarly, the receivers may expect that a sender communicates truthfully, as is typically found in experiments on cheap talk. Upon observing that a sender attempts to deceive them into investing too frequently in the bad state

[24]In Appendix Section B.11, I show that the pattern is similar when using other ways of measuring whether participants received bad advice, such as how often a participant overall received bad advice when facing an information structure. In addition, I show that the pattern is driven by those participants that receive bad advice, and not by participants that receive different recommendation than their matched participant, so not by participants that receive good advice while their matched participant receives bad advice. This finding is not consistent with alternative explanations for this pattern, such as conformism or a preference to always receive the same recommendations.

Table 11: Bad advice and future following

	(1) Investment	(2) Following	(3) Following
Public	-0.027	0.035	0.046**
	(0.021)	(0.021)	(0.022)
Complements	-0.110***	-0.098***	-0.090***
	(0.033)	(0.031)	(0.031)
Public × Complements	0.109**	-0.018	-0.031
	(0.047)	(0.045)	(0.045)
Bad advice	-0.122***	-0.128***	-0.123***
	(0.035)	(0.036)	(0.036)
Public × Bad advice	0.111**	0.125***	0.113**
	(0.047)	(0.045)	(0.045)
Complements × Bad advice	-0.009	0.009	-0.010
	(0.051)	(0.057)	(0.054)
Public × Complements × Bad advice	-0.033	-0.055	-0.028
	(0.066)	(0.069)	(0.067)
Constant	0.535***	0.963***	0.948***
	(0.022)	(0.024)	(0.081)
Period trend, part & lab FE	Yes	Yes	Yes
Additional controls	No	No	Yes
Observations	24612	24612	24510

Notes: The table reports OLS estimates and includes all data after period one in each part. Column (3) uses fewer observations, as some additional controls are not available for all participants. The dependent variables are the decision to invest or the decision to follow a recommendation. Bad advice is a dummy variable equal to 1 if a participant received a recommendation to invest when the state was bad in period 1 of the corresponding information structure. Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. The additional controls are participants' Fehr and Schmidt (1999) preferences, risk aversion, numeracy score, and demographic variables. Standard errors in parentheses, clustered at the matching-group level,* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

of the world, they may reduce their investment. These receivers may thus exhibit an aversion to being deceived beyond what is justified by the strategic skepticism in the game.

Furthermore, while the first experiment provides a good indication of how to persuade audiences, it is unclear whether real senders are capable of optimally adjusting their persuasion to their audience.

Apart from introducing human senders, the second experiment mirrors the first as closely as possible. The senders can choose among the six information structures exogenously assigned in the first experiment: either using public or private signals, and using one of the three levels of obedience (*low*, *optimal*, or *high*).

5.1 Differences in receivers' behavior

I begin by comparing receivers' behavior between the first and second experiment using data on beliefs.[25] Direct choice data in the second experiment is less informative for two reasons. First, data on receivers' choices are only available for the structures senders choose.[26] Second, the senders likely particularly rely on structures that are successful for their group of receivers, but these structures may be heterogeneous across groups. This means that we observe receivers'

[25] Nevertheless, Appendix Table A19 shows that the main result is also present in the choice data: public structures increase investment. In the second experiment, this effect is similar in both strategic environments.

[26] For example, 6 out of the 40 groups in the experiment did not encounter all structures, as none of the senders in these groups exploited their whole choice set during the entire experiment.

choices in a selected distribution of structures.[27] To account for the selection in choice data, at the end of the experiment I elicit beliefs for the full set of potential structures. As I elicited the same beliefs in both experiments, I can compare data from experiments with and without participants in the role of senders. Within the second experiment, I can also compare senders' and receivers' beliefs separately.

Figure 8 shows receivers' belief, across the two experiments, about other receivers' following behavior after they received the recommendation to invest. The red diamonds represent the observed following behavior within each experiment. The left panel reproduces data from Figure 5 on the receivers' beliefs in the first experiment. The middle panel shows the receivers' beliefs elicited in the second experiment, and the right panel the senders' beliefs. Table 12 presents estimation results of the corresponding effects. I regress the belief that others invest after receiving the recommendation to invest on features of the information structure (public versus private, information-structure level) for three samples. In column (1), I use receivers' beliefs from the first experiment. In column (2), I use receivers' beliefs from the second experiment. Column (3) uses beliefs of the senders from the second experiment. Column (4) pools data from both experiments and both roles to estimate interaction effects.

Most behavioral patterns are robust across both experiments and roles. Between the first and second experiments, receivers believe that following behavior decreases somewhat: from 80% (coefficient on the constant; p-value<0.001; column (1)) to 73% (coefficient on the constant; p-value<0.001; column (2); interaction effect in column (4): p-value<0.001). Senders, in turn, predict following rates 21 percentage points lower (coefficient on Second exp., senders; p-value<0.001).

In addition, senders predict that receivers' changes in behavior in response to different structures are smaller than the response predicted by receivers. In that sense, senders underestimate receivers' responses. For example, they believe that receivers respond to higher levels less than receivers believe other receivers respond. For *high* compared to *low* levels, receivers predict a decrease in following rates of 17 percentage points (p-value<0.001; column (2)), while senders only predict a decrease of 6 percentage points (p-value=0.026; column (3); interaction effect in column (4): p-value<0.001). Senders do not anticipate that receivers expect more following with public signals with strategic substitutes (p-value=0.400; column (3)), while receivers predict a decrease in the following rate of 11 percentage points (p-value<0.001; column (2); interaction effect in column (4): p-value=0.093). Thus, while senders partially anticipate the advantage of public structures, they underappreciate that receivers believe that public structures increase following behavior.

In Appendix Table A17, I show that beliefs about the state are also comparably updated across both experiments and roles. Across both experiments, receivers update as expected by becoming more pessimistic about the state with *optimal* and *high* structures. Again, senders underestimate the extent to which receivers believe others are more pessimistic.

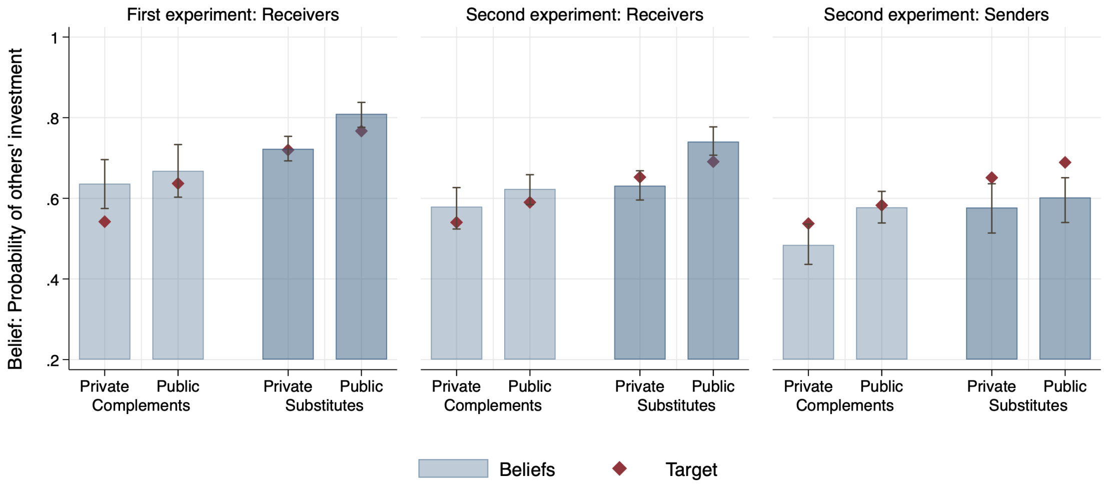
[27] Consistent with this form of selection, the distribution of chosen structures is quite imbalanced. Of the 63 total possible choices of information structures for each matching group (three senders per matching group, 21 periods), 32 of the 40 groups faced at least one information structure fewer than five times. Simultaneously, in 17 of the 40 groups just one information structure accounted for more than half of the receivers' choices (so, for more than 32 sender choices).

Table 12: Beliefs about others' following across all experiments

	(1)	(2)	(3)	(4)
	Belief: Probability others invest			
Public	0.087***	0.109***	0.025	0.087***
	(0.023)	(0.016)	(0.029)	(0.022)
Complements	-0.087**	-0.052	-0.091**	-0.086**
	(0.034)	(0.032)	(0.042)	(0.034)
Public × Complements	-0.055	-0.066***	0.068*	-0.055
	(0.050)	(0.023)	(0.035)	(0.050)
(1 if level=*optimal*)	-0.117***	-0.122***	-0.035*	-0.117***
	(0.010)	(0.013)	(0.018)	(0.010)
(1 if level=*high*)	-0.175***	-0.166***	-0.056**	-0.175***
	(0.014)	(0.015)	(0.024)	(0.014)
Second exp., receivers				-0.095***
				(0.026)
Second exp., senders				-0.214***
				(0.038)
Public × Second exp., receivers				0.023
				(0.027)
Public × Second exp., senders				-0.062*
				(0.036)
Complements × Second exp., receivers				0.036
				(0.047)
Complements × Second exp., senders				-0.005
				(0.054)
Public × Complements × Second exp., receivers				-0.011
				(0.055)
Public × Complements × Second exp., senders				0.123**
				(0.061)
(1 if level=*optimal*) × Second exp., receivers				-0.006
				(0.016)
(1 if level=*optimal*) × Second exp., senders				0.081***
				(0.021)
(1 if level=*high*) × Second exp., receivers				0.010
				(0.020)
(1 if level=*high*) × Second exp., senders				0.119***
				(0.028)
Constant	0.803***	0.726***	0.592***	0.811***
	(0.018)	(0.024)	(0.042)	(0.017)
Experiment	First	Second	Second	Both
Role	Receivers	Receivers	Senders	Both
Lab FE	Yes	Yes	Yes	Yes
Observations	1293	1440	720	3453
# clusters	72	40	40	112
# participants	431	240	120	791

Notes: The table reports OLS estimates. The dependent variable is the reported belief that other receivers invest after receiving the recommendation to invest. Column (1) uses data from the first experiment, with only receivers. Columns (2) and (3) use data from the second experiment. (2) are the receivers, (3) the senders. Column (4) pools data from both experiments and both roles. Public and Complements are the treatment indicators. Public and Complements are dummy variables equal to 1 if the belief was reported for facing a public information, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Second exp., receivers and Second exp., senders are dummies equal one if the belief is measured in the second experiment, for receivers and senders, respectively. The omitted category is the receivers in the first experiment. In column (1), beliefs were not elicited for one participant that dropped out earlier. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 8: Beliefs across all experiments



Notes: Average reported belief that other receivers invest, conditional on them receiving the recommendation to invest, by treatment and role. Left panel: receivers in the first experiment. Middle panel: receivers in the second experiment. Right panel: senders in the second experiment. Bars indicate observed beliefs, diamonds indicate the observed target in the data, and error bars indicate 95% bootstrapped confidence intervals.

5.2 Senders' choice of information structures

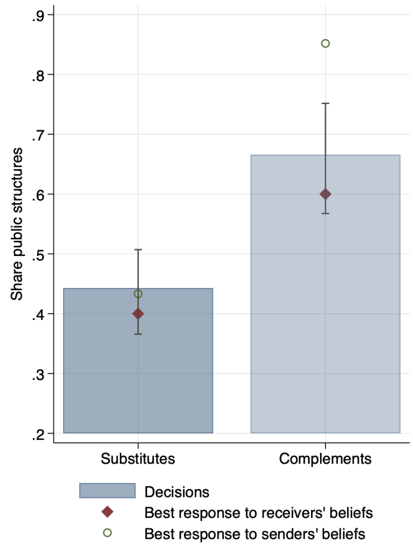
Now I turn to how senders' choose to persuade the receivers. I begin by discussing whether senders choose private or public communication.

Senders choosing public or private information structures. Figure 9 shows the share of public structures used. Table 13 presents estimation results of the corresponding treatment effect. In column (1), I regress the decision to use a public structure on a treatment indicator. Senders on average choose public structures slightly more often than private ones, in 55% of periods. Importantly, they respond to the receivers' interaction in making their own choice. They choose public structures 53% more frequently with strategic complements compared to substitutes (p-value<0.001; column (1)), consistently with the theoretical prediction.

In addition to senders' choices, I show best responses to receivers' and senders' beliefs in Figure 9. The best responses indicate what share of public structures would have maximized senders' payoffs when using beliefs to predict receivers' behavior.[28] The preceding analysis in this section revealed that compared to receivers' beliefs, senders believe that receivers do not respond strongly to changes in the information structure. Therefore, best responses to either senders' or receivers' beliefs may differ. Receivers may understand their own decision situation reasonably well. Senders, in contrast, are required to predict receivers' responses while simultaneously deciding on an optimal structure. A difference in the best responses to senders' and receivers' beliefs reveals the extent to which differences in beliefs affect the best response.

[28]For the best response to receivers' beliefs, I first calculate each receiver's best response to recommendations, based on each receiver's own beliefs about the state and others' following behavior. I aggregate these best responses by calculating the average best response within a matching group. Using this exercise, I obtain predicted investments for each of the possible information structures. I define a sender's best response to receivers' beliefs to be the information structure that maximizes investment, given predicted receiver behavior. The best responses to receivers' beliefs always exist. However, they do not exist for 40 of 120 best responses to senders' beliefs, as these senders hold beliefs that do not generate investment under any information structure.

Figure 9: Senders' choices of public and private signals



Notes: Share of public structures chosen by senders. Bars indicate observed choices; error bars indicate 95% bootstrapped confidence intervals. Red diamonds indicate the average best response to receivers' beliefs, green circles the best response to senders' own beliefs about receiver behavior.

Senders' decisions match a best response to receivers' beliefs quite closely. This indicates that senders' choices are reasonably close to choosing structures that maximize their own payoffs, and they are optimal based on expected receiver behavior. Here, the best response to receivers' beliefs is likely the most informative, as choices and beliefs in the first experiment revealed that participants' beliefs are reasonably accurate; thus, these beliefs give a good indication what investment behavior senders could have expected.

The best response to senders' own beliefs indicates that, if anything, they use public structures less frequently than expected. Clearly, senders anticipate both that public signals are generally more persuasive and that they are particularly valuable in games of strategic complements.

Result 7. *Senders on average use public signals slightly more often than private signals, and as predicted they use public signals more often when the receivers' strategic environment features strategic complements.*

Senders' choice of level. In Figure 10, I show how frequently senders choose each possible level of information structure. Senders are relatively aggressive in persuading receivers to invest frequently; the median choice in both games is the *optimal* structure. This structure recommends investment as often as possible while ensuring that risk-neutral receivers continue to best respond by following. However, this level also means that receivers' payoffs are quite low, while senders' payoffs are high if these recommendations are followed. In addition, senders surprisingly frequently employ *high* structures. In columns (2) and (4) in Table 13, I compare how much more frequently senders choose *optimal* instead of *low* structures. We can see that at the beginning of the experiment, senders on average are 18 percentage points more likely to choose *optimal* structures (coefficient on the constant; p-value=0.004; column (2)). However, senders over time learn to choose *low* structures more often (-1.5 percentage points per period, p-value=0.001; column (2)), which encourages investment. There is no significant difference in baseline choices between games of complements and substitutes (p-value=0.758; column (4)). However, in games of substitutes, senders are 17 percentage points more likely to choose *high* instead of *optimal* structures (coefficient on the constant; p-value=0.021; column (5)). In games of complements, senders are equally likely to choose either level (coefficient of -17% on Complements; p-value=0.082; column (5)). In Appendix Table A16, I repeat this analysis separately for the first third and last two-thirds of the data to study learning. Senders use public structures more frequently across both games as they gain experience and learn to avoid high levels in games of strategic complements.

A large majority of senders apparently understand that a too high level is not optimal, as receivers are no longer incentivized to follow. Yet, on average, they choose *high* levels, which reduce receivers' expected profits from following but increase their own profits if receivers do follow. Somewhat surprisingly, they are more aggressive than the best response to receivers' beliefs indicates. The senders would have generated higher investment by reducing their aggressiveness, as receivers would be more likely to follow recommendations. In addition, their own beliefs indicate that senders again underestimate the degree to which choosing a more aggressive persuasion strategy will affect receivers' choices, judged by the gap between the best responses to receivers' and senders' beliefs.

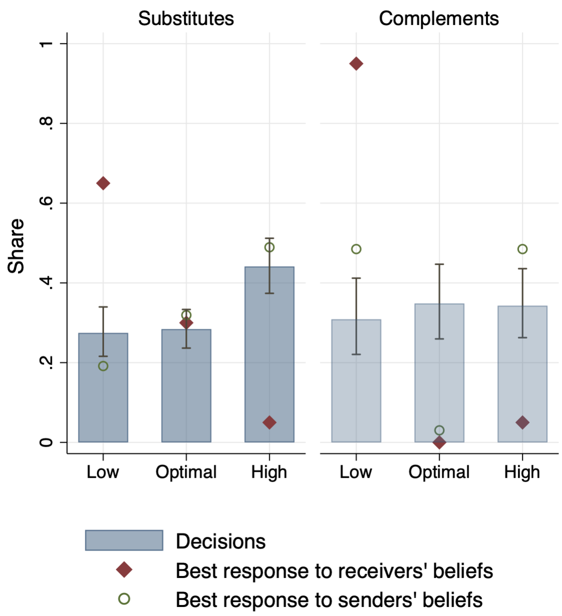
The aggressiveness in communication contrasts with typical findings in the earlier literature on cheap-talk experiments, in which senders typically overcommunicate relative to equilibrium predictions (Blume et al., 2020). Instead, communicating by committing to an information structure moves predictions closer to self-interested behavior. One reason may be that senders only deceive their receivers probabilistically, as uncertainty remains about which signals participants receive even conditional on the bad state materializing. This is in line with the literature on how uncertainty in choices diffuses participants' perceived responsibility for selfish choices (Falk and Szech, 2014; Exley, 2016).

Table 13: Senders: Treatment effects

	(1) Public	(2) *Optimal* vs. *low*	(3) *High* vs. *optimal*	(4) *Optimal* vs. *low*	(5) *High* vs. *optimal*
Complements	0.222***			0.031	-0.166*
	(0.062)			(0.099)	(0.093)
Period	0.003	-0.015***	0.001	-0.015***	0.001
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)
Constant	0.415***	0.184***	0.085	0.168**	0.171**
	(0.060)	(0.061)	(0.056)	(0.076)	(0.071)
Lab FE	Yes	Yes	Yes	Yes	Yes
Observations	2520	2520	2520	2520	2520
# clusters	40	40	40	40	40
# participants	120	120	120	120	120

Notes: The table reports OLS estimates. In column (1), the dependent variable is a dummy equal to 1 if the sender chose a public structure. In columns (2) and (4), the dependent variable is a difference in level shares: the share of *optimal* structures minus the share of *low* structures. In columns (3) and (5), the dependent variable is a difference in level shares: the share of *high* structures minus the share of *optimal* structures. Complements is the treatment indicator. This is a dummy variable equal to 1 if the decision was made facing a game with strategic complements, with the omitted category being a game of strategic substitutes. Period is a linear period trend. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 10: Senders' choice of level



Notes: Share of periods in which senders choose *low*, *optimal*, or *high* information structures. Bars indicate observed choices; error bars indicate 95% bootstrapped confidence intervals. Red diamonds indicate the average best response to receivers' beliefs, green circles the best response to senders' own beliefs about receiver behavior.

Result 8. *Senders persuade aggressively. In games of substitutes, they choose structures according to the following ranking:*

$$\textit{high} > \textit{optimal} = \textit{low}$$

In games of complements, they choose structures according to the following ranking:

$$\textit{high} = \textit{optimal} = \textit{low}$$

6 Conclusion

In this paper, I studied the optimal persuasion of an audience of interacting receivers. In a laboratory experiment, I showed that senders benefit from tailoring their communication strategy to the strategic interaction of their audience. In particular, when the audience faces a game of strategic complements, public signals enhance a sender's capability to persuade. In addition, I found that public signals are more persuasive than private signals across environments. This force has not been incorporated in theoretical models so far, yet it is strong enough to offset the potential strategic gains from private signals in games of strategic substitutes.

I ruled out two standard mechanisms that may be driving the superiority of public signals. Neither differences in riskiness nor inequalities can explain why public structures enhance persuasion. Instead, I found evidence for the following two mechanisms. First, receivers struggle with the more complex nature of private signals, as they understand less well what they can learn from them. This increases the noise in behavior. This unpredictability, in turn, reduces how often trusting private signals is a best response. Public signals solve this by relying on common knowledge and common actions, and this symmetry apparently makes them easier to understand and to optimally respond to these signals. Second, receivers exhibit a distaste for differential treatment with private structures if they have experienced unfavorable recommendations early on. Public signals solve this by recommending the same action to all receivers.

This study provides novel evidence on the strength of adapting the communication channel to the strategic environment of the receivers. As even students in a laboratory experiment can capitalize on these gains, it stands to reason that sophisticated players in practice can take advantage of appropriate communication channels to enhance persuasion. However, the senders in the laboratory still underestimate what they can gain from broadly employing public signals.

In practice, senders in these types of setups often use public communication. For example, governments are held accountable with transparent decision-making. Equal treatment is an important cornerstone of democratic governments. The results of this experiment provide an additional, purely strategic, rationale for using public communication. They enhance a government's persuasiveness, particularly strongly in games of strategic complements.

These results can help senders who communicate with strategically interacting audiences in many real-world settings. For example, close to the framing in the experiment, a manager may want to encourage effort on the part of her workers, whose rewards may feature complementarities or substitutabilities. This paper highlights that besides exploiting her knowledge about a project's difficulty, she can maximize effort by (mis)coordinating workers' actions by using private or public signals. In particular, I showed that public signals are a valuable tool for this manager, as they are more persuasive than private signals. Closer to the investment-game framing, a government may want to encourage investments into COVID-19 vaccine-production facilities while holding private information about future waves' severity or planned vaccination campaigns. The interaction of firms may feature strategic substitutes, as stiffer price competition ensues if both firms increase capacity. Alternatively, strategic complements can be introduced by increased public acceptance and subsequent sales of a more widely established vaccine technology, from a better understanding of this new technology with resulting improved production capabilities, or from other network effects on an industry level. This paper provides empirical evidence that the sender should carefully choose the channel in response to the prevailing interaction. Other examples include speculative attacks with strategic complementarities between market participants, which central banks or regulators try to prevent by strategically releasing information publicly.

There is still much to be learned about communication with an audience, with a small empirical and experimental literature. In this paper, I study small audiences, but results for larger audiences are crucial to understand how these strategic forces change with more receivers. In practice, many audiences are large, which increases both the difficulty in reasoning through optimal responses to signals but also the potential gains from optimal persuasion. Similarly, I give the theoretical predictions a good shot by revealing the sender's information structure. Data from an experiment in which this is not revealed, but sender and receivers interact repeatedly to allow them to learn these elements, would move the setup closer to some real-world settings. From a theoretical point of view it would also be interesting to study the benefits of public and private signals in settings without preference misalignment between sender and receivers. For example, Bergemann and Morris (2016) derive similar insights by incorporating payoff externalities between receivers and a sender maximizing receivers' average payoff.

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A Appendix: Theory

More formally, Bergemann and Morris (2016) consider decision rules σ which for each type t_i and state θ recommend an action to the player. Types t_i in this context capture information about the state revealed to player i. For game G and information structure S, σ is obedient if for all i, t_i and a_i the following inequality holds for all a_i':

$$\sum_{a_i,t_i,\theta} \pi\left((a_i,a_{-i})|(t_i,t_{-i})\right)\frac{1}{2}(\theta)\sigma\left((a_i,a_{-i})|(t_i,t_{-i}),\theta\right)u_i\left((a_i,a_{-i}),\theta\right)\right)$$
$$\geq \sum_{a_i,t_i,\theta} \pi\left((a_i,a_{-i})|(t_i,t_{-i})\right)\frac{1}{2}(\theta)\sigma\left((a_i,a_{-i})|(t_i,t_{-i}),\theta\right)u_i\left((a_i',a_{-i}),\theta\right)\Big)$$

That is, the recommended action a_i yields a payoff at least as high as any other action a_i'. Then, a player best responds by implementing the recommendation as long as the other players implement the recommended action. If a decision rule satisfies obedience, it is a Bayes correlated equilibrium (Bergemann and Morris, 2016), and there exists an expansion of the information structure in which following the decision rule constitutes a Bayes Nash equilibrium.

A.1 Parameter choice

In Table A4, I reproduce estimations using only *optimal* information structures, which are just obedient for risk-neutral receivers. These are the information structures for which I chose the parameters to yield the largest treatment differences, e.g., the interaction effect (public (vs. private) $\times$ complements (vs. substitutes)) is predicted to be 14.9 percentage points.

In addition, I chose parameters that yield are reasonably large treatment effect, compared to other potential choices. In the notation of Table 1, the parameters used in the experiment correspond to $x_{\text{com}} = 0.1$ and $\epsilon_{\text{com}} = 0.3$ for the game of strategic complements, and $x_{\text{sub}} = 0.9$ and $\epsilon_{\text{sub}} = -0.5$ for the game of strategic substitutes. To obtain the payoffs displayed in Tables 3 and 4, payoffs are multiplied by 100 and then a constant payoff of 170 is added. This ensures that payoffs are positive round numbers, to minimize loss aversion and mental effort of processing payoffs.

In the parameter choice, I measure the predicted treatment effect for exactly obedient structures. This choice is partially restricted. As they are probabilities, we need that $1 > p_{\text{bad}} \geq r_{\text{bad}} \geq 0$, as well as to keep signals private. There are two additional considerations. First, I chose parameters such that with private signals, no joint investment arises in the bad state, formally $p_{\text{bad}} - r_{\text{bad}} < .5$ Second, there are three levels of obedience, where the *high* structures require higher probabilities of investment recommendations than the *optimal* structures I compare here. Taken together, this implies that the highest probability of private signals in the bad state needs to be sufficiently lower than .5, $p_{\text{bad}} - r_{\text{bad}} << .5$.

For a selection of parameters, I show the predicted treatment effects in Table A1. Optimally, private structures set $p_{\text{bad}} = \epsilon + x, r_{\text{bad}} = 0$, and public structures set $p_{\text{bad}} = r_{\text{bad}} = \frac{\epsilon+x}{1-\epsilon}$. The first row is the *optimal* information structure, which is close to the exactly obedient information structure in the experiment, in the second row. Treatment effects are lower when increasing x

while holding $p_{\text{bad}} - r_{\text{bad}}$ constant, see the third and fourth row. When reducing the probabilities to invest, treatment differences again decrease, independent of the x and ϵ chosen, see rows five to eight.

Table A1: Parameter choices and predicted treatment effects

Parameters	Complements		Substitutes		
$(x_{\text{com}}, \epsilon_{\text{com}}; x_{\text{sub}}, \epsilon_{\text{sub}})$	Public r_{bad}	Private $p_{\text{bad}} - r_{\text{bad}}$	Public r_{bad}	Private $p_{\text{bad}} - r_{\text{bad}}$	Diff-in-diff TE
(.1, .3; .9, -.5)	.48	.34	.34	.23	.25
(.1, .3; .9, -.5)	.57	.4	.27	.4	.30
(.3, .1; .9, -.5)	.44	.4	.27	.4	.17
(.1, .3; .6, -.2)	.57	.4	.33	.4	.24
(.2, .1; .8, -.5)	.33	.3	.2	.3	.13
(.1, .2; .8, -.5)	.38	.3	.25	.3	.13
(.2, .1; .5, -.2)	.33	.3	.25	.3	.08
(.1, .1; .7, -.5)	.22	.2	.13	.2	.09

Information structure parameters $(p_{\text{bad}}, r_{\text{bad}})$ when varying the parameters of the game (x, ϵ). The column Diff-in-diff TE gives the difference-in-differences treatment effect between games and private vs. public structures, which is the difference in probabilities that a recommendation to invest will be sent in the bad state.

B Appendix: Additional empirics

B.1 Balancing tables

In Tables A2 and A3, I show that participant characteristics are balanced across treatments. In the second experiment, Aheadness aversion is not perfectly balanced and significantly different between treatments with a p-value of 0.097. Controlling for this measure, and other characteristics, does not affect results.

Table A2: Balancing table: First experiment

	Complements		Substitutes		
	Private	Public	Private	Public	p-values
Age	22.7	22.6	22.8	22.6	0.962
% women	54.6	56.5	65.7	53.7	0.398
% Bachelor	69.4	70.4	70.4	63.9	0.815
Risk	3.0	3.4	3.3	3.1	0.174
Numeracy score	2.4	2.4	2.3	2.5	0.770
Behindness aversion	3.5	3.4	3.8	3.6	0.723
Aheadness aversion	5.5	5.0	5.5	5.3	0.513
Quiz attempts	2.7	2.8	2.9	2.1	0.256

Notes: Average characteristic by treatment. In the last column I report p-values of a Kruskal-Wallis test, comparing equality of ranks across all treatments. Risk is the lottery chosen in the Eckel and Grossman (2002)-task, numeracy score the number of correct answers in the Berlin numeracy test (Cokely et al., 2012), behindness and aheadness aversion the switching points in the multiple price list-elicitation of β and α-parameters in Fehr and Schmidt (1999)-preferences by Yang et al. (2016).

Table A3: Balancing table: Second experiment

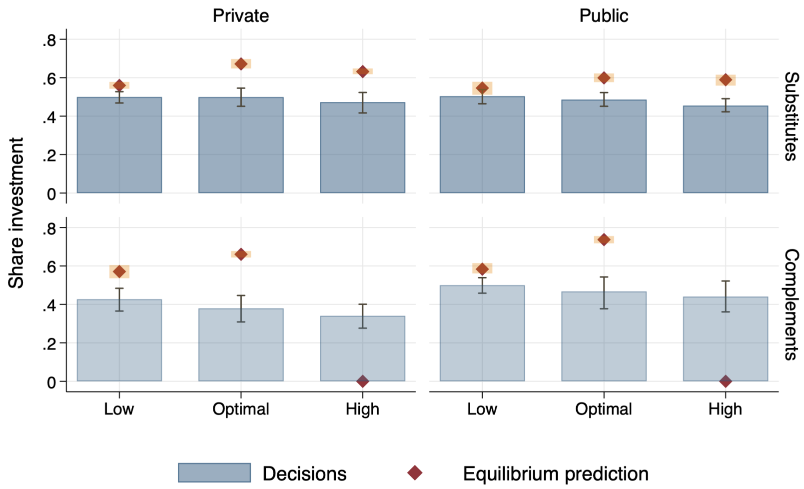
	Complements	Substitutes	p-values
Age	22.4	22.9	0.334
% women	55	57.2	0.672
% Bachelor	70.6	64.4	0.217
Risk	3.2	3.4	0.335
Numeracy score	1.1	1.0	0.548
Behindness aversion	3.7	3.8	0.533
Aheadness aversion	5.5	5.0	0.097
Quiz attempts	4.9	5.2	0.617

Notes: Average characteristic by treatment. In the last column I report p-values of a t-test, comparing equality of means across the two treatments. Risk is the lottery chosen in the Eckel and Grossman (2002)-task, numeracy score the number of correct answers in the Berlin numeracy test using only questions 1 and 3 (Cokely et al., 2012), behindness and aheadness aversion the switching points in the multiple price list-elicitation of β and α-parameters in Fehr and Schmidt (1999)-preferences by Yang et al. (2016).

B.2 Investment behavior

In Figure A1, I show investment rates separately for each level of obedience.

Figure A1: Investment decisions



Notes: Average frequency of investment by treatment, bars indicate observed choices and red diamonds choices in the Bayes Nash equilibria with the highest investment. Bars and shaded areas indicate 95% bootstrapped confidence intervals.

In Table A4, I reproduce the treatment effect table from the main text with additional controls, as preregistered. Columns (1), (3), (5) to (7) and (10) present decisions from the experiment, (2), (4), (8), and (11) repeat the regressions when participants use the Nash equilibrium strategy. (9) and (12) interact models (7) and (8) or (10) and (11), respectively.

Columns (1), (3), and (5) show estimates omitted from the table in the main text. Columns (6) and (7) show that results are robust to additional controls. Columns (10) to (12) only use data from *optimal* levels of information structures, which uses one-third of the entire data set. Column (10) shows this level's larger theoretically predicted treatment effects. Column (10) shows that treatment effects in the experiment are robust to only using this level for testing. Columns (10) and (12) show that public structures empirically increase investment compared to the Nash equilibrium prediction, and similar so for both games.

Table A5 reports logit estimates of the main treatment effects. Results are in line with the OLS results reported in the main text.

Table A4: Treatment effects with additional controls: Investment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Substitutes		Complements						Diff-in-Diff			
	Obs.	NE	Obs.	NE	Obs.	Obs.	Obs.	NE	NE vs. Obs.	Obs.	NE	NE vs. Obs.
Public	-0.009	-0.043***	0.087**	0.030***	-0.009	-0.001	0.004	-0.043***	-0.009	-0.013	-0.073***	-0.013
	(0.019)	(0.011)	(0.039)	(0.007)	(0.020)	(0.020)	(0.021)	(0.011)	(0.019)	(0.028)	(0.017)	(0.028)
Complements					-0.108***	-0.101***	-0.100***	-0.211***	-0.108***	-0.120***	-0.011	-0.120***
					(0.032)	(0.031)	(0.032)	(0.009)	(0.033)	(0.038)	(0.015)	(0.039)
Public × Complements					0.096**	0.087*	0.084*	0.073***	0.096**	0.101*	0.149***	0.101*
					(0.045)	(0.044)	(0.045)	(0.013)	(0.045)	(0.059)	(0.022)	(0.060)
(1 if level=*optimal*)	-0.009	0.082***	-0.040*	0.122***	-0.025*	-0.024*	-0.025*	0.102***	0.039***			
	(0.018)	(0.014)	(0.021)	(0.016)	(0.014)	(0.014)	(0.014)	(0.011)	(0.010)			
(1 if level=*high*)	-0.038*	0.058***	-0.073***	-0.577***	-0.055***	-0.056***	-0.056***	-0.260***	-0.157***			
	(0.020)	(0.013)	(0.020)	(0.011)	(0.014)	(0.014)	(0.014)	(0.039)	(0.022)			
Period	-0.002**		-0.004***		-0.003***	-0.003***	-0.003***		-0.001**	-0.003**		-0.000
	(0.001)		(0.001)		(0.001)	(0.001)	(0.001)		(0.001)	(0.001)		(0.001)
(1 if part=2)	0.017		-0.032*		-0.007	-0.008	-0.009		0.001	-0.052		-0.026
	(0.021)		(0.018)		(0.014)	(0.014)	(0.014)		(0.019)	(0.034)		(0.020)
(1 if part=3)	0.007		-0.023		-0.008	-0.008	-0.008		0.001	0.008		0.012
	(0.020)		(0.020)		(0.014)	(0.014)	(0.014)		(0.018)	(0.037)		(0.021)
(1 if session in Munich)	0.019		0.106**		0.063***	0.057**	0.053**		0.030**	0.069**		0.037**
	(0.020)		(0.040)		(0.023)	(0.024)	(0.024)		(0.014)	(0.029)		(0.017)
Behindness aversion						0.014**	0.011*					
						(0.006)	(0.006)					
Aheadness aversion						0.004	0.003					
						(0.003)	(0.003)					
Risk						0.012*	0.015**					
						(0.007)	(0.006)					
Numeracy						-0.016**	-0.015**					
						(0.007)	(0.007)					
Age							-0.002					
							(0.003)					
(1 if woman)							0.046***					
							(0.017)					
NE									0.131***			0.173***
									(0.016)			(0.019)
Public × NE									-0.034*			-0.060**
									(0.019)			(0.026)
Complements × NE									-0.103***			0.109***
									(0.031)			(0.038)
Public × Complements × NE									-0.023			0.048
									(0.044)			(0.059)
Constant	0.514***	0.574***	0.445***	0.562***	0.533***	0.461***	0.488***	0.674***	0.532***	0.518***	0.672***	0.496***
	(0.022)	(0.010)	(0.038)	(0.013)	(0.022)	(0.054)	(0.093)	(0.014)	(0.023)	(0.030)	(0.013)	(0.026)
Observations	12960	12960	12948	12948	25908	25860	25800	25908	51816	8628	8628	17256
# clusters	36	36	36	36	72	72	72	72	72	72	72	72
# participants	216	216	216	216	432	432	432	432	432	432	432	432
Levels	All	All	All	All	All	All	All	All	All	Only *optimal*	Only *optimal*	Only *optimal*

Notes: The table reports OLS estimates. The dependent variable is the choice to invest, either observed in the experiment (Obs.) or the investment choice in the equilibrium with the highest investment (NE). (1) and (2) use observed or predicted data from games of strategic substitutes, (3) and (4) from games of strategic complements and (5) to (12) pool all data. (10) to (12) use only data from *optimal* levels, all other models are estimated using all levels. Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. Aheadness aversion and behindness aversion are switching points in the choice lists to elicit α (behindness) and β-parameters (aheadness) of the Fehr and Schmidt (1999)-model, elicited using the task by Yang et al. (2016). Both measures range from 1 to 11, with mean 3.6, standard deviation 1.4 for behindness, and with mean 5.3, standard deviation 2.9 for aheadness aversion. Risk is the lottery chosen in the Eckel and Grossman (2002)-task, ranging from 1 to 6 with mean 3.2, standard deviation 1.5. Numeracy is the number of correct items in the Berlin numeracy test (Cokely et al., 2012), ranging from 0 to 4, mean 2.4, standard deviation 1.2. NE is a dummy variables equal one if this is a predicted choice in the Nash equilibrium with maximal investment, with the omitted category being an observed choice in the experiment. Standard errors in parentheses clustered on matching-group level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Logit estimates of the treatment effect: Investment

	(1) Substitutes	(2) Complements	(3) Diff-in-diff
Public	-0.035	0.364**	-0.035
	(0.076)	(0.165)	(0.081)
Complements			-0.445***
			(0.136)
Public × Complements			0.395**
			(0.184)
(1 if level=*optimal*)	-0.036	-0.166*	-0.100*
	(0.073)	(0.089)	(0.058)
(1 if level=*high*)	-0.151*	-0.304***	-0.226***
	(0.081)	(0.083)	(0.058)
Constant	0.054	-0.222	0.138
	(0.087)	(0.160)	(0.090)
Period trend, part and lab FE	Yes	Yes	Yes
Observations	12960	12948	25908
# clusters	36	36	72
# participants	216	216	432

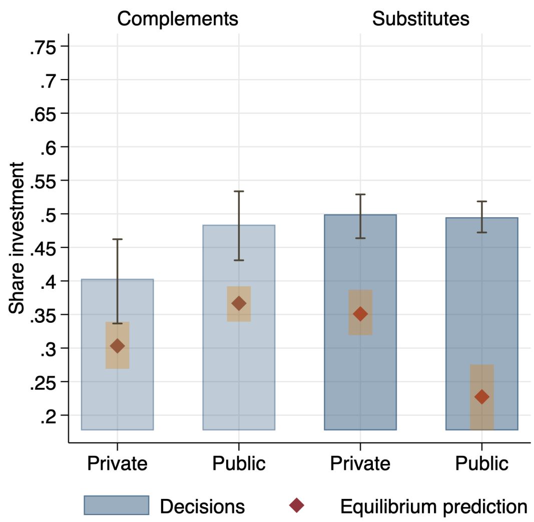
Notes: The table reports logit estimates and includes all data, also *high* structures. The dependent variable is a dummy variable equal to 1 if the participant decided to invest (Data) or was predicted to invest in the Bayes Nash equilibrium with maximal investment (NE). Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.3 Investment and risk aversion

Observed investment rates in the experiment are, on average, below the predictions. These predictions are based on risk-neutral receivers. Empirically, the two control task measuring risk aversion at the end of the experiment show that an overwhelming majority of participants are risk averse. Furthermore, adding the risk aversion measure introduced by Eckel and Grossman (2002) correlates significantly with investment choice, see Table A4, patterns are similar using the second control task.

Information from these task can also be used to adjust the equilibrium predictions for the level of risk aversion at the participant level. This is especially relevant for the optimal information structures, at expected profits are slim, while participants face risk. Even only slightly risk averse participants may not be willing to invest at this level. To account for this riskiness, I use the CRRA utility with the coefficients estimated from the lottery choice elicited in the Eckel and Grossman (2002) task, and, as a lower bound, calculate a best response to others' behaving as in equilibrium under risk neutrality. Figure A2 shows predicted choices, which, if anything, indicate that participants are willing to invest more often than predicted given their measured level of risk aversion.

Figure A2: Investment decisions



Notes: Average frequency of investment by treatment, bars indicate observed choices and red diamonds choices in the Bayes Nash equilibria with the highest investment when using participants' risk aversion elicited in the Eckel and Grossman (2002) task to calculate their expected utility. The figure only uses data from *low* and *optimal* structures. Bars and shaded areas indicate 95% bootstrapped confidence intervals.

B.4 Learning

In Table A6, I report regressions on learning effects for investment and following. (1) to (4) split data in the first 7 (in (1) and (3)) vs. the last 13 periods (in (2) and (4)). (5) to (7) repeat the investment regression for each part separately. Results are robust across periods and parts, except the no longer significant estimate on Public × Complements in (6) for part 2.

In Figure A3, I plot the average investment rate for the four between-subject treatments, separately for each part. Investment rates are similar over time across both dimensions of

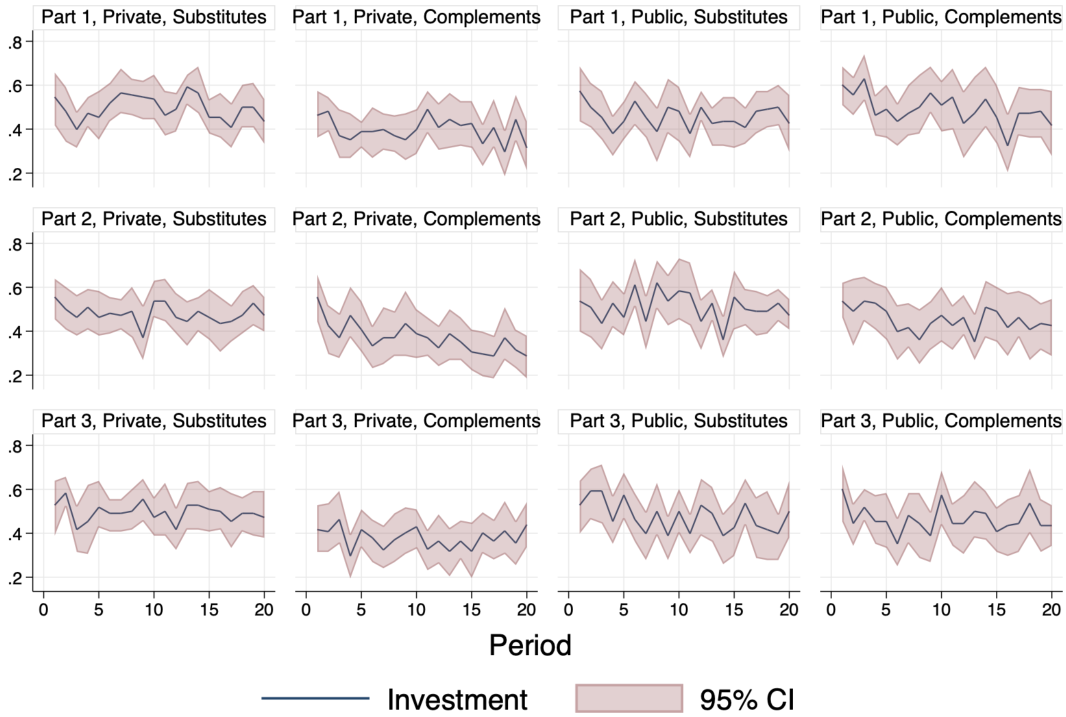
Table A6: Learning: Investment and following

	Investment		Following		Investment		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Public	0.004	-0.016	0.047**	0.054**	-0.038	0.031	-0.019
	(0.020)	(0.023)	(0.020)	(0.023)	(0.028)	(0.027)	(0.029)
Complements	-0.090***	-0.118***	-0.088***	-0.101***	-0.100***	-0.108***	-0.116***
	(0.032)	(0.034)	(0.028)	(0.032)	(0.033)	(0.040)	(0.036)
Public × Complements	0.085*	0.102**	-0.025	-0.033	0.132**	0.050	0.105*
	(0.043)	(0.049)	(0.039)	(0.046)	(0.050)	(0.054)	(0.056)
(1 if level=*optimal*)	-0.014	-0.030*	-0.109***	-0.134***	0.006	-0.089***	0.010
	(0.017)	(0.016)	(0.015)	(0.016)	(0.030)	(0.031)	(0.033)
(1 if level=*high*)	-0.020	-0.074***	-0.215***	-0.255***	-0.025	-0.079**	-0.062*
	(0.016)	(0.017)	(0.015)	(0.017)	(0.029)	(0.033)	(0.032)
Constant	0.540***	0.551***	0.971***	0.947***	0.522***	0.553***	0.510***
	(0.025)	(0.028)	(0.024)	(0.029)	(0.026)	(0.030)	(0.031)
Period	1-7	13-20	1-7	13-20	1-20	1-20	1-20
Part	1-3	1-3	1-3	1-3	1	2	3
Period trend, part, and lab FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9072	16836	9072	16836	8640	8640	8628
# clusters	72	72	72	72	72	72	72
# participants	432	432	432	432	432	432	432

Notes: The table reports OLS estimates. The dependent variable is a dummy variable equal to 1 if the participant decided to invest in (1), (2), and (5) to (7), or the participant followed the received recommendation in (3) and (4). Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

learning: between parts and within parts, over periods.

Figure A3: Learning

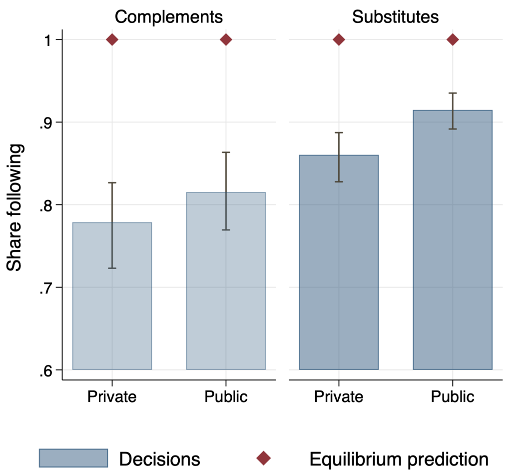


Notes: Average investment per period in the blue line, with 95%, bootstrapped confidence intervals (clustered on matching-group level) shaded in red. Separately by part (part 1, 2, and 3), public vs. private and substitutes vs. complements.

B.5 Following behavior

In this section, I present some additional statistics on the following behavior. In Figure A4, I show the average decision to follow averaged on a between-subject treatment level. In Table A7, I show regressions of the decisions to follow on treatment dummies with additional controls. In Table A8, I report estimates when repeating the analysis from the main text, but only using data when participants receive the recommendation to invest, which removes any variation in how often recommendations not to invest are being followed. Results are broadly in line with the analysis from the main text. In addition, I report estimates when regressing the squared distance between observed following decisions and the best response to beliefs in column (5). Empirical behavior is closer to the best response in public structures (estimate on Public, p-value=0.0247), but does not differ significantly in the other between-subject treatment dimensions.

Figure A4: Following rates



Notes: Average frequency of following a recommendation by treatment, bars indicate observed choices. Error bars indicate 95% bootstrapped confidence intervals.

Table A7: Treatment effects with additional controls: Following

	(1)	(2)	(3)
Public	0.052**	0.057***	0.061***
	(0.021)	(0.021)	(0.022)
Complements	-0.096***	-0.092***	-0.090***
	(0.030)	(0.029)	(0.030)
Public × Complements	-0.030	-0.036	-0.041
	(0.043)	(0.042)	(0.043)
(1 if level=*optimal*)	-0.125***	-0.125***	-0.126***
	(0.015)	(0.015)	(0.015)
(1 if level=*high*)	-0.241***	-0.242***	-0.242***
	(0.015)	(0.015)	(0.015)
(1 if part=2)	-0.009	-0.010	-0.010
	(0.014)	(0.014)	(0.014)
(1 if part=3)	-0.005	-0.005	-0.004
	(0.013)	(0.013)	(0.013)
Period	-0.003***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)
(1 if session in Munich)	0.056**	0.051**	0.050**
	(0.022)	(0.023)	(0.023)
Behindness aversion		0.007	0.004
		(0.005)	(0.005)
Aheadness aversion		0.004	0.004
		(0.003)	(0.003)
Risk		0.011*	0.014**
		(0.006)	(0.006)
Numeracy		-0.009	-0.008
		(0.007)	(0.007)
Age			-0.004
			(0.003)
(1 if woman)			0.047***
			(0.017)
Constant	0.956***	0.893***	0.962***
	(0.024)	(0.048)	(0.085)
Observations	25908	25860	25800

Notes: The table reports OLS estimates. The dependent variable is the decision to follow a recommendation (invest after recommended to invest, not invest after not invest). Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. Aheadness aversion and behindness aversion are switching points in the choice lists to elicit α (behindness) and β-parameters (aheadness) of the Fehr and Schmidt (1999)-model, elicited using the task by Yang et al. (2016). Both measures range from 1 to 11, with mean 3.6, standard deviation 1.4 for behindness, and with mean 5.3, standard deviation 2.9 for aheadness aversion. Risk is the lottery chosen in the Eckel and Grossman (2002)-task, ranging from 1 to 6 with mean 3.2, standard deviation 1.5. Numeracy is the number of correct items in the Berlin numeracy test (Cokely et al., 2012), ranging from 0 to 4, mean 2.4, standard deviation 1.2. Standard errors in parentheses clustered on matching-group level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Treatment effects: Following the recommendation to invest

	(1) Data	(2) NE	(3) Data	(4) NE	(5) (Data-NE)^2
Public	0.062**	0.000	0.052*	0.006	-0.086**
	(0.029)	(.)	(0.031)	(0.005)	(0.038)
Complements	-0.148***	0.000	-0.158***	-0.318***	-0.044
	(0.048)	(.)	(0.046)	(0.004)	(0.035)
Public × Complements	0.021	0.000	0.023	-0.014**	0.083
	(0.063)	(.)	(0.062)	(0.006)	(0.051)
(1 if level=*optimal*)	-0.164***	0.000	-0.163***	0.004	0.165***
	(0.020)	(.)	(0.020)	(0.003)	(0.022)
(1 if level=*high*)			-0.281***	-0.588***	0.187***
			(0.017)	(0.051)	(0.030)
Constant	0.897***	1.000	0.925***	1.163***	0.338***
	(0.032)	(.)	(0.033)	(0.019)	(0.033)
Level of obedience	*low* & *optimal*		*low*, *optimal* & *high*		
Period trend, part and lab FE	Yes	No	Yes	No	Yes
Observations	10638	10638	17110	17110	17110
# clusters	72	72	72	72	72
# participants	432		432	432	

Notes: The table reports OLS estimates and includes only data where participants received the recommendation to invest. In columns (1) to (4), the dependent variable is a dummy variable equal to 1 if the participant decided to follow a recommendation (invest after recommended to invest, or not invest after recommended not to invest) (Data) or was predicted to follow in the Bayes Nash equilibrium with maximal investment (NE). Columns (1) and (2) use data only from obedient structures, while columns (3) and (4) pool all data. In column (5), the dependent variable is the squared distance between decision to follow the recommendation to invest in the data and the predicted best response to beliefs ((Data-BR)^2). Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A5 repeats the best response analysis from Figure 4 using the empirical frequencies in the data instead of participants' beliefs. Differences can be attributed to errors in belief updating, either about the state or about others' actions. While broadly similar, especially in games of strategic substitutes receivers underinvest. Participants in games of strategic complements underreact to changes in obedience: they follow not often enough for *low* levels but follow too frequently for *optimal* and *high* levels.

B.6 Estimating quantal response equilibrium

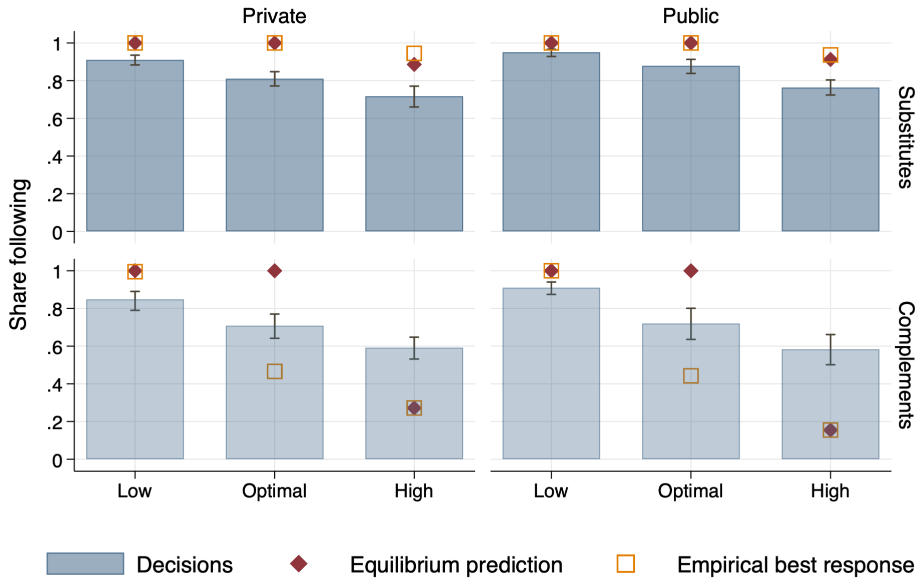
To account for noisy best responses, I estimated quantal response equilibria (McKelvey and Palfrey, 1995). I estimate the rationality-parameter λ in a logit-specification and focus on the decision to follow a recommendation to invest.[29]

To do so, I estimate λ to match empirically observed, aggregate probabilities to follow the recommendation to invest, imposing the following assumptions: (1) I calculate expected payoffs from investing and not investing, normalizing game-payoffs between 0 and 1; (2) Beliefs about the state are updated according to Bayes rule; (3) I estimate λ separately for each between-subject treatment, within each treatment I use only data from *low* structures.[30]

[29] Not following a recommendation not to invest entails investing when knowing with certainty that the state is bad. Participants appear to understand this feature, and invest in only 1.9% of periods in which they receive the recommendation not to invest. Therefore, I want to capture noisiness in the decision to invest when this is potentially profitable.

[30] These are the most interesting structures, as, especially with strategic complements, best replies involve never investing with *high* structures or with even minimal noise with *optimal* structures.

Figure A5: Following rates



Notes: Average frequency of following a recommendation by treatment and level of the information structure. The variable is a dummy equal to 1 if a recommendation was followed (investment after the recommendation to invest, no investment after the recommendation not to invest). Bars indicate observed choices, diamonds following rate in the equilibrium with the highest following, and squares are empirical best responses based on others' choices in the experiment. Error bars indicate 95% bootstrapped confidence intervals.

Estimated λ are, for games of strategic complements, 25.26 for public structures and 17.11 for private structures. For games of strategic substitutes, λ are estimated to be 23.09 for public structures and 16.91 for private structures.

This exercise is particularly interesting as it allows to compare a measure of rationality across games and structures. The estimates suggest that rationality is lower (closer to 0) with private compared to public structures for each game. This suggests that play is more sophisticated and closer to rationality with public structures.

B.7 Beliefs

In Table A9, I report data on all elicited beliefs for all treatments and levels. This now includes beliefs on what participants believed about the state and others' actions after receiving the recommendation not to invest. Beliefs are consistent with three key observations. First, across all treatments, on average, participants understand that the recommendation to invest is good news about the state. In contrast, the recommendation to invest is bad news, as beliefs about the state being good are higher after receiving the recommendation to invest. Second, they understand that others respond reasonably to recommendations, as they are more likely to invest after receiving this recommendation. Third, participants follow the expected pattern across levels, as they are less optimistic about the state and others' investment moving from *low* to *optimal* to *high* levels. Notable is also that participants' beliefs about the state across private structures (comparing complements and substitutes) are virtually identical. These structures were designed to induce identical beliefs, and participants between treatments responded identically. Last, note that beliefs after receiving the recommendation not to invest are likely also surprisingly

Table A9: Belief data

		Recommendation to invest		Recommendation not to invest	
Treatment	Level	State	Others invest	State	Others invest
Complements, Public	*Low*	.79	.77	.13	.14
	Optimal	.67	.64	.14	.17
	High	.65	.60	.13	.19
Complements, Private	*Low*	.80	.76	.10	.10
	Optimal	.71	.62	.11	.14
	High	.65	.54	.09	.13
Substitutes, Public	*Low*	.88	.90	.09	.17
	Optimal	.77	.81	.08	.17
	High	.72	.72	.08	.19
Substitutes, Private	*Low*	.79	.80	.11	.20
	Optimal	.69	.70	.13	.17
	High	.65	.67	.14	.21

Average beliefs of the state being good ("State") or others' decision to invest ("Others invest") in response to receiving the recommendation to invest or not to invest. Beliefs are coded as shares, with dummies equal to 1 if the state is good or others invest, respectively.

high because reports were measured for zero or higher; thus, noise in decision-making was only captured for positive errors. E.g., more than 75% of beliefs about the state are 0, as theoretically predicted; only a minority of participants report a positive probability of the state being good even though this is theoretically not possible.

B.8 Risk aversion and following behavior

As an additional measure of risk, I use the separately elicited risk aversion (Eckel and Grossman, 2002). In Table A10, I regress the decision to follow a recommendation on the risk measure, treatment dummies, and most importantly, their interaction, adding controls from (1) to (3). It does not appear to be the case that the risk measure captures differences in behavior specific to public information structures.

Table A10: Following and risk aversion

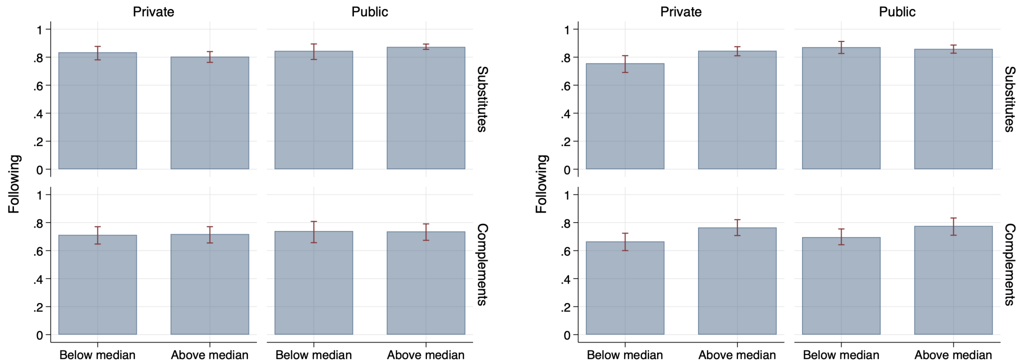
	(1)	(2)	(3)
Public	0.042	0.013	0.024
	(0.047)	(0.056)	(0.056)
Complements	-0.112***	-0.131*	-0.121*
	(0.022)	(0.067)	(0.067)
Public × Complements		0.059	0.042
		(0.095)	(0.096)
Risk	0.009	0.004	0.006
	(0.010)	(0.015)	(0.014)
Risk × Public	-0.002	0.012	0.009
	(0.013)	(0.017)	(0.017)
Risk × Complements		0.011	0.008
		(0.020)	(0.019)
Risk × Public × Complements		-0.028	-0.023
		(0.026)	(0.026)
Constant	0.792***	0.801***	0.900***
	(0.034)	(0.047)	(0.047)
Part, level and lab FE	No	No	Yes
Observations	25860	25860	25860
# clusters	72	72	72
# participants	431	431	431

Notes: The table reports OLS estimates. The dependent variable is the choice to follow a recommendation (investing after being recommended to invest, not investing after being recommended not to invest). Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. Risk is the lottery chosen in the Eckel and Grossman (2002)-task, where higher numbers indicate lower risk aversion. The index ranges from 1 to 6, with mean 2.3 and standard deviation 1.5. Standard errors in parentheses clustered on matching-group level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.9 Inequity aversion and following behavior

Another candidate to explain the superiority of public structures are social preferences. If followed, public structures minimize payoff inequality between participants. In contrast, following a private structure leads to unequal payoffs if the bad state realizes. To test this mechanism, I included an elicitation of the preference parameters of the Fehr and Schmidt (1999)-model, using the task by Yang et al. (2016). In Figure A6, I show the following rate when performing median splits by the aversion to being ahead in the left panel and by the aversion to being behind in the right panel. No clear pattern may explain higher following rates only in public information structures. Generally, the aversion to being behind appears to lead to more following.

Figure A6: Following and inequity aversion



Notes: Average following rate. Left panel: Median split by aversion to being ahead. Right panel: Median split by aversion to being behind. Bars indicate observed choices, diamonds the observed target in the data, and error bars indicate 95% bootstrapped confidence intervals.

Result 9. *Inequity aversion cannot explain the higher following in public information structures.*

In Table A11, I show how the decision to follow recommendations correlates with inequity aversion parameters (Fehr and Schmidt, 1999), especially for public information structures. There is no significant effect of either aversion to being ahead or behind.

Table A11: Following and inequity aversion

	(1)	(2)
Public	0.088	0.098
	(0.066)	(0.069)
Behindness aversion	0.009	0.008
	(0.007)	(0.007)
Public × Behindness aversion	-0.003	-0.003
	(0.010)	(0.010)
Aheadness aversion	0.009**	0.007
	(0.004)	(0.004)
Public × Aheadness aversion	-0.007	-0.006
	(0.006)	(0.006)
Complements	-0.111***	-0.097***
	(0.022)	(0.030)
Public × Complements		-0.029
		(0.043)
Constant	0.740***	0.851***
	(0.048)	(0.053)
Part, level and lab FE	No	Yes
Observations	25860	25860
Adjusted R^2	0.022	0.083
# clusters	72	72
# participants	431	431

Notes: The table reports OLS estimates. The dependent variable is the choice to follow a recommendation (investing after being recommended to invest, not investing after being recommended not to invest). Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. Aheadness aversion and behindness aversion are switching points in the choice lists to elicit α (behindness) and β-parameters (aheadness) of the Fehr and Schmidt (1999)-model, elicited using the task by Yang et al. (2016). Both measures range from 1 to 11, with mean 3.6, standard deviation 1.4 for behindness, and with mean 5.3, standard deviation 2.9 for aheadness aversion. Standard errors in parentheses clustered on matching-group level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.10 Noise in beliefs

Table A12 documents that beliefs are less noisy in public groups. I regress the standard deviation in beliefs within a matching group, at each level, on treatment dummies. Note that this standard deviation only captures variance within a group: Each participant reported beliefs only once for each level, thus any noise perceived by each participant within a level is not captured.

Table A12: Noise in beliefs

	(1) SD(beliefs)
Complements	0.163
	(0.238)
Public	-0.415*
	(0.219)
Complements × Public	0.358
	(0.329)
(1 if level=*optimal*)	0.103
	(0.122)
(1 if level=*high*)	0.261**
	(0.124)
Constant	1.910***
	(0.141)
Observations	216
# clusters	72

Notes: The table reports OLS estimates. The dependent variable is the standard deviation in beliefs about others' following a recommendation to invest. This is calculated on the matching group-level level, so one observation is the standard deviation within a matching group for each level (*low*, *optimal* or *high*). Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Standard errors in parentheses clustered on matching-group level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.11 Experiencing bad advice: Robustness

In Section 4.4, I show that only in private information structures, experiencing bad advice leads to lower investment and following in future periods. This section presents two robustness checks.

First, I show that the result is robust to different rules to capture who has received bad advice. I repeat the analysis presented in the main text, but count the number of times a participant has received bad advice within each information structure. In addition, I perform a median split of participants who received bad advice more often than the median facing the same information structure, which accounts for the fact that the frequency of receiving bad advice is correlated with the type of structure.

Results in Table A13 indicate that patterns are similar using the new measures. Columns (1) and (2) report estimates using the number of times bad advice was sent to a participant, columns (3) and (4) report estimates using the median split. Columns (1) and (3) use the decision to invest as dependent variables, (2) and (4) the decision to follow. Note that the bad advice-proxies are not significant in (1) and (3). Yet, across both specifications, public structures lead to higher investment of those participants that initially received bad advice, consistent with the analysis in the main text. Columns (2) and (4) show that those receiving bad advice more often follow less often, but this effect is not present in public structures, in line with the analysis in the main text.

Second, I show that this pattern is driven by participants that receive bad advice. An

Table A13: Robustness of bad advice

	(1) Investment	(2) Following	(3) Investment	(4) Following
Public	-0.048*	-0.002	-0.035	0.020
	(0.028)	(0.023)	(0.023)	(0.024)
Complements	-0.062*	-0.059*	-0.083**	-0.080**
	(0.036)	(0.031)	(0.034)	(0.031)
Public × Complements	0.086*	0.041	0.081*	-0.031
	(0.045)	(0.038)	(0.045)	(0.041)
# bad advice	-0.005	-0.020***		
	(0.007)	(0.006)		
Public × # bad advice	0.016*	0.015*		
	(0.009)	(0.009)		
Complements × # bad advice	-0.014*	-0.011		
	(0.008)	(0.008)		
Public × Complements × # bad advice	-0.000	-0.011		
	(0.011)	(0.012)		
Above median bad advice			-0.023	-0.071***
			(0.022)	(0.019)
Public × Above median bad advice			0.070**	0.077**
			(0.030)	(0.031)
Complements × Above median bad advice			-0.057*	-0.035
			(0.032)	(0.030)
Public × Complements × Above median bad advice			0.029	-0.009
			(0.046)	(0.046)
Constant	0.540***	0.982***	0.544***	0.989***
	(0.026)	(0.023)	(0.025)	(0.027)
Period trend, part & lab FE	Yes	Yes	Yes	Yes
Observations	25908	25908	25908	25908

Notes: The table reports OLS estimates and includes all data. The dependent variables are the decision to invest (1) and (3) or the decision to follow a recommendation (2) and (4). # bad advice is the number of times a participant received bad advice when facing an information structure. Above median bad advice is a dummy variable equal one if the participant received bad advice more often than the median times all participants facing that same structure received bad advice. Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. Standard errors in parentheses, clustered at the matching-group level,* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

alternative explanation may be a preference for conformism, or for always receiving the same recommendation. To test this alternative, I rerun the analysis presented in the main text, but instead compare participants that receive the recommendation not to invest to participants who receive the recommendation to invest in the good state in the first period of an information structure, which removes all participants that receive bad advice in the first period. Both remaining groups of participants receive good advice. However, participants that receive the recommendation not to invest with private information structure may experience miscoordinated advice, as their matched participant may receive the recommendation to invest. Instead, participants with public information structures always receive the same recommendation. The alternative explanations would predict that participants respond differently to experience the same or different recommendations. Conformity-driven explanations would imply that participants that experience different recommendation with private structures change their follow-up behavior in patterns similar to those participants who receive bad advice.

The results in Table A14 indicate that participants that receive the recommendation not to invest in the first period do not invest or follow differently in follow-up periods, irrespective of whether they face public or private information structure, compared to participants that receive the recommendation to invest in the good state. This indicates that the conformity is an unlikely explanation of the data. Instead, the data is consistent with participants disliking experiencing miscoordinated bad advice.

Table A14: Miscoordinated good advice and future following

	(1) Investment	(2) Following	(3) Following
Public	-0.033 (0.028)	0.019 (0.029)	0.031 (0.030)
Complements	-0.122*** (0.041)	-0.121*** (0.040)	-0.115*** (0.040)
Public × Complements	0.128** (0.056)	0.001 (0.054)	-0.012 (0.055)
Not invest	-0.013 (0.032)	-0.038 (0.031)	-0.043 (0.030)
Public × Not invest	0.015 (0.043)	0.038 (0.040)	0.034 (0.040)
Complements × Not invest	0.029 (0.046)	0.056 (0.046)	0.060 (0.045)
Public × Complements × Not invest	-0.055 (0.072)	-0.046 (0.069)	-0.045 (0.068)
Constant	0.548*** (0.028)	0.982*** (0.029)	0.974*** (0.087)
Period trend, part & lab FE	Yes	Yes	Yes
Additional controls	No	No	Yes
Observations	20615	20615	20558

Notes: The table reports OLS estimates and includes all data after period one in each part. I only use data where participants received good advice, so either the recommendation not to invest in the bad state or the recommendation to invest in the good state. Column (3) uses fewer observations, as some additional controls are not available for all participants. The dependent variables are the decision to invest or the decision to follow a recommendation. Not invest is a dummy variable equal to 1 if a participant received a recommendation not to invest in period 1 of the corresponding information structure. Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. The additional controls are participants' Fehr and Schmidt (1999) preferences, risk aversion, numeracy score, and demographic variables. Standard errors in parentheses, clustered at the matching-group level,* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.12 Benchmarking the importance of the two mechanisms

In this section, I provide a rough estimate of the relative contributions of the two mechanisms to advantage of public signals. Table A15 provides the needed estimates. Model (1) shows that public signals lead to 4 percentage points higher investment across all games. This is an advantage not predicted by theory: model (2) indicates that in the Bayes Nash equilibrium with maximal investment, no advantage of public structures would be expected.

First, I find that participants who receive bad advice reduce their follow-up investment. Model (3) indicates that in public structures, participants who receive bad advice invest 10 percentage points more than those with private structures. However, receiving such advice is probabilistic: on average, only 16% of participants received bad advice in period 1. This means that the effect on average behavior is only 1.6 percentage points.

Second, I find that in groups with above-median variance, public structures lead to 5 percentage points higher investment. As this effect is only present for half of the groups, those with above-median variance, the total effect is 2.5 percentage points.

Therefore, the total effect of 4 percentage points is can approximately be attributed to a 1.6 percentage point effect of bad advice, and a 2.5 percentage point effect of complexity and high variance. This means that the total effect of complexity is roughly 2.5/(1.6+2.5)=61%.

Table A15: Decomposing the effect of the two mechanisms

	(1) Investment	(2) NE investment	(3) Investment	(4) Investment
Public	0.039*	-0.007	0.024	0.010
	(0.023)	(0.008)	(0.023)	(0.024)
Complements	-0.060**	-0.174***	-0.061**	-0.056***
	(0.023)	(0.008)	(0.023)	(0.019)
Bad advice			-0.126***	
			(0.026)	
Public × Bad advice			0.102***	
			(0.033)	
High variance				-0.139***
				(0.024)
Public × High variance				0.052
				(0.037)
Constant	0.509***	0.643***	0.511***	0.589***
	(0.024)	(0.025)	(0.024)	(0.024)
Period trend; part, level and lab FE	Yes	Yes	Yes	Yes
Observations	25908	25908	24612	25908

Notes: The table reports OLS estimates. In (1), (3) and (4), the dependent variable is a dummy equal one if the participant chose to invest. In (3), the dependent variable is a dummy equal one if the participant would have been predicted to invest in the Bayes Nash equilibrium with maximal investment. Public and Complements are the treatment indicators. These are dummy variables equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure, or facing a game with strategic complements, with the omitted category being a game of strategic substitutes. Bad advice is a dummy variable equal to 1 if a participant received a recommendation to invest when the state was bad in period 1 of the corresponding information structure. High variance is a dummy variable equal to 1 if the average standard deviation of the matching group (calculated as in (1)) is above the median within each treatment. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.13 Additional analysis on the second experiment

Table A16 reports an analysis of the regressions of the second experiment, Table 13, separately for the first third and the last two-thirds, to study learning. Columns (1), (3) and (5) use data from periods 1 to 7, columns (2), (4) and (6) from periods 8 to 21, as preregistered.

There are clear indications for learning. Comparing columns (1) and (2), we observe that the average use of public signals across both games increases, as the constant increases from 38% to 47%. Columns (4) and (6) also show that receivers persuade less aggressively over time in games of complements. The coefficient on Complements is positive in (3), at the start, but no longer so with experience in (4). Similarly, the coefficient on Complements is not significant at the start in (5), but significant and negative in (6), with experience. For games of substitutes, if anything, receivers become more aggressive over time, as the coefficient on the constant increases in (6), compared to (5), thus senders are more likely to choose *high* instead of *optimal* structures.

Table A16: Senders: Treatment effects and learning

	(1)	(2)	(3)	(4)	(5)	(6)
	Public		*Optimal* vs. *low*		*High* vs. *optimal*	
Complements	0.244***	0.211***	0.156*	-0.032	-0.123	-0.188*
	(0.064)	(0.072)	(0.086)	(0.118)	(0.085)	(0.110)
Period	0.007	0.001	-0.009	-0.004	-0.014	-0.003
	(0.010)	(0.005)	(0.017)	(0.007)	(0.017)	(0.006)
Constant	0.382***	0.466***	0.133	0.017	0.173*	0.263**
	(0.059)	(0.098)	(0.090)	(0.127)	(0.090)	(0.123)
Period	1-7	8-21	1-7	8-21	1-7	8-21
Lab FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	840	1680	840	1680	840	1680
# clusters	40	40	40	40	40	40
# participants	120	120	120	120	120	120

Notes: The table reports OLS estimates. In (1) and (2), the dependent variable is a dummy equal one if the sender chose a public structure. In (3) and (4), the dependent variable is the difference in level shares, as the share of *optimal* minus the share of *low* structures. In (5) and (6), he dependent variable is the difference in level shares, as the share of *high* minus the share of *optimal* structures. (1), (3) and (5) use data from periods 1 to 7; (2), (4) and (6) from peridos 8 to 21. Complements is the treatment indicator, a dummy variable equal to 1 if the decision was made when receivers face a game with strategic complements, with a game of substitutes as the omitted category. Period is a linear period trend. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A17 complement the analysis on beliefs in the main text. I regress the belief that the state is good after receiving a recommendation to invest on characteristics of the information structure and the game. The estimates show that also beliefs about the state are updated very similarly for receivers in the first and second experiment. Again, senders underestimate the extent to which receivers' update, in response to *optimal* or *high* structures.

Table A17: Beliefs about the state across all experiments

	(1)	(2)	(3)	(4)
	Belief: Probability state is good			
Public	0.078***	0.081***	0.059**	0.078***
	(0.015)	(0.013)	(0.024)	(0.015)
Complements	0.011	0.024	0.008	0.011
	(0.016)	(0.020)	(0.027)	(0.016)
Public × Complements	-0.099***	-0.105***	-0.033	-0.099***
	(0.023)	(0.017)	(0.029)	(0.023)
(1 if level=*optimal*)	-0.102***	-0.107***	-0.025	-0.102***
	(0.006)	(0.009)	(0.015)	(0.006)
(1 if level=*high*)	-0.148***	-0.151***	-0.042**	-0.148***
	(0.008)	(0.010)	(0.016)	(0.008)
Second exp., receivers				-0.049**
				(0.020)
Second exp., senders				-0.201***
				(0.025)
Public × Second exp., receivers				0.003
				(0.019)
Public × Second exp., senders				-0.019
				(0.028)
Complements × Second exp., receivers				0.014
				(0.025)
Complements × Second exp., senders				-0.004
				(0.031)
Public × Complements × Second exp., receivers				-0.006
				(0.029)
Public × Complements × Second exp., senders				0.066*
				(0.037)
(1 if level=*optimal*) × Second exp., receivers				-0.006
				(0.011)
(1 if level=*optimal*) × Second exp., senders				0.077***
				(0.016)
(1 if level=*high*) × Second exp., receivers				-0.003
				(0.013)
(1 if level=*high*) × Second exp., senders				0.106***
				(0.018)
Constant	0.789***	0.742***	0.584***	0.789***
	(0.011)	(0.019)	(0.025)	(0.011)
Experiment	First	Second	Second	Both
Role	Receivers	Receivers	Senders	Both
Lab FE	Yes	Yes	Yes	Yes
Observations	1293	1440	720	3453
# clusters	72	40	40	112
# participants	431	240	120	791

Notes: The table reports OLS estimates. The dependent variable is the reported belief that the state is good after receiving the recommendation to invest. (1) uses data from the first experiment, with only receivers. (2) and (3) use data from the second experiment. (2) are the receivers, (3) the senders. (4) pools data from both experiments and both roles. Public and Complements are dummy variables equal to 1 if the beliefs was reported for facing a public rather than a private information structure, or facing a game with strategic complements rather than substitutes respectively. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Second exp., receivers and Second exp., senders are dummies equal one if the belief is measured in the second experiment, for receivers and senders, respectively. The omitted category are the receivers in the first experiment. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

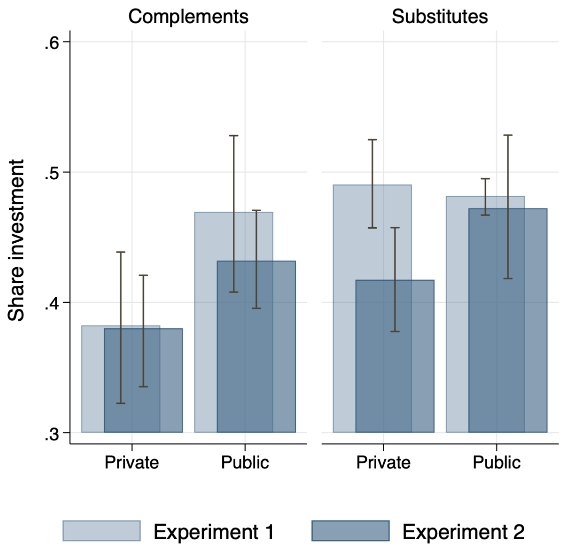
Table A18: Beliefs about the state across all experiments

	(1)	(2)	(3)
	Belief: Probability state is good		
Public	0.078***	0.081***	0.059**
	(0.015)	(0.013)	(0.024)
Complement	0.011	0.024	0.008
	(0.016)	(0.020)	(0.027)
Public × Complement	-0.099***	-0.105***	-0.033
	(0.023)	(0.017)	(0.029)
(1 if level=*optimal*)	-0.102***	-0.107***	-0.025
	(0.006)	(0.009)	(0.015)
(1 if level=*high*)	-0.148***	-0.151***	-0.042**
	(0.008)	(0.010)	(0.016)
Constant	0.789***	0.742***	0.584***
	(0.011)	(0.019)	(0.025)
Experiment	First	Second	Second
Role	Receivers	Receivers	Senders
Lab FE	Yes	Yes	Yes
Observations	1293	1440	720
# clusters	72	40	40
# participants	432	240	120

Notes: The table reports OLS estimates. The dependent variable is the reported belief that the state is good after receiving the recommendation to invest. (1) uses data from the first experiment, with only receivers. (2) and (3) use data from the second experiment. (2) are the receivers, (3) the senders. Public and Complements are dummy variables equal to 1 if the beliefs was reported for facing a public rather than a private information structure, or facing a game with strategic complements rather than substitutes respectively. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A7 and Table A19 presents data of receiver behavior similar to the first experiment, using data from the second experiment. Note that this is not directly comparable, as senders had chosen the information structure endogenously. This may now reflect that some matching groups responded heterogeneously to specific structures. Senders can anticipate this, so the regressions now compare data under selection, where those groups that respond particularly well, and potentially different from the average group, to a specific structure.

Figure A7: Investment decisions across the two experiments



Notes: Average frequency of investment by treatment, bars indicate observed choices, bars indicate 95% bootstrapped confidence intervals.

In columns (1), (3) and (5), I regress investment behavior on a treatment dummy for a game of strategic complements, as well as design features of the information structure (public vs. private, level). Columns (2), (4) and (6) repeat this for following decisions.

Table A19: Receiver behavior in the second experiment

	(1)	(2)	(3)	(4)	(5)	(6)
	Substitutes		Complements		Diff-in-Diff	
	Investment	Following	Investment	Following	Investment	Following
Public	0.048**	0.035*	0.052*	-0.016	0.051**	0.035*
	(0.021)	(0.020)	(0.029)	(0.024)	(0.022)	(0.020)
Complement					-0.039	-0.067***
					(0.029)	(0.022)
Complement × Public					0.006	-0.050*
					(0.037)	(0.029)
(1 if level=*optimal*)	-0.010	-0.121***	-0.036	-0.174***	-0.021	-0.146***
	(0.034)	(0.025)	(0.038)	(0.020)	(0.026)	(0.017)
(1 if level=*high*)	0.017	-0.177***	-0.033	-0.280***	0.001	-0.219***
	(0.027)	(0.025)	(0.047)	(0.029)	(0.027)	(0.020)
Constant	0.389***	0.838***	0.438***	0.896***	0.425***	0.892***
	(0.036)	(0.033)	(0.045)	(0.034)	(0.033)	(0.027)
Lab FE and period trend	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2520	2520	2520	2520	5040	5040
# clusters	20	20	20	20	40	40
# participants	120	120	120	120	240	240

Notes: The table reports OLS estimates. The dependent variable is a dummy variable equal to 1 if the participant decided to invest (Investment) or followed a recommendation (Following) by investing after receiving the recommendation to invest, or not investing after receiving the recommendation not to invest. Complements is a treatment indicator, a dummy variable equal to 1 if the decision was made facing a game with strategic complements, with the omitted category being a game of strategic substitutes. Public is a dummy variable equal to 1 if the decision was made facing a public information structure, with the omitted category being a private structure. (1 if level=*optimal*) and (1 if level=*high*) are dummy variables equal to 1 if the information structures used *optimal* or *high* probabilities to persuade receivers to invest, relative to the omitted category *low*, respectively. Note that the publicness and the level was an endogenous choice by senders in this experiment. Both the level and publicness are now chosen endogenously by senders. Standard errors in parentheses, clustered at the matching-group level, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Appendix: Instructions and screenshots

This section contains screenshots of the decision screens, receivers' instructions in the first experiment as well as screenshots of the senders' instructions in the second experiment. Receiver instructions in the second experiment were identical, apart from revealing how senders' payoffs depended on their choices.

In the first experiment, instructions were specific to the game (strategic substitutes vs. complements), all information structures one participant received were either public or private. Between parts, the level of the structure was varied.

In the second experiment, instructions were again specific to the game (strategic substitutes vs. complements). In addition, each role assignment (sender vs. receiver) had specific instructions.

C.1 Example decision screen

Below are screenshots of the senders' and receivers' decision screens from the second experiment.

Figure A8: Receivers' decision screen

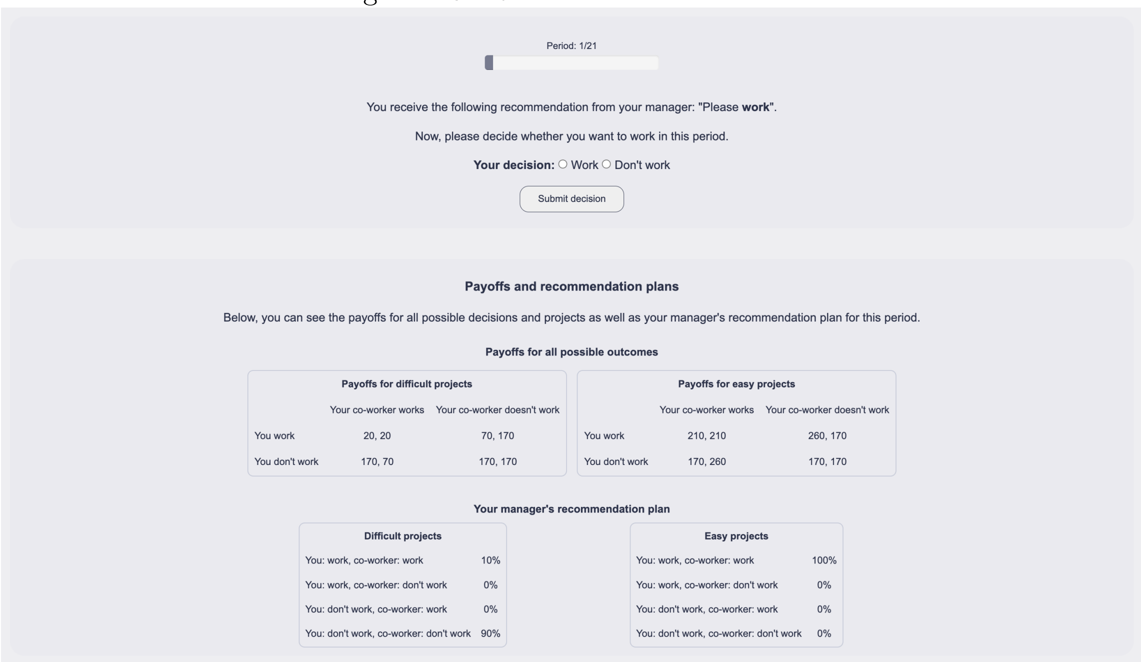
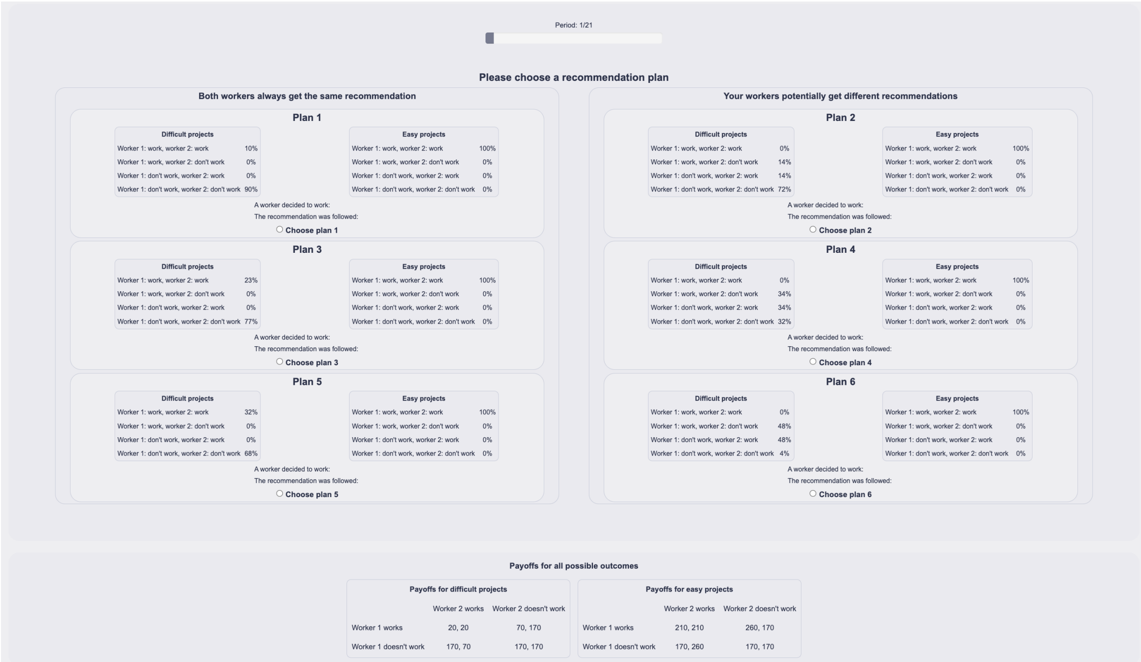
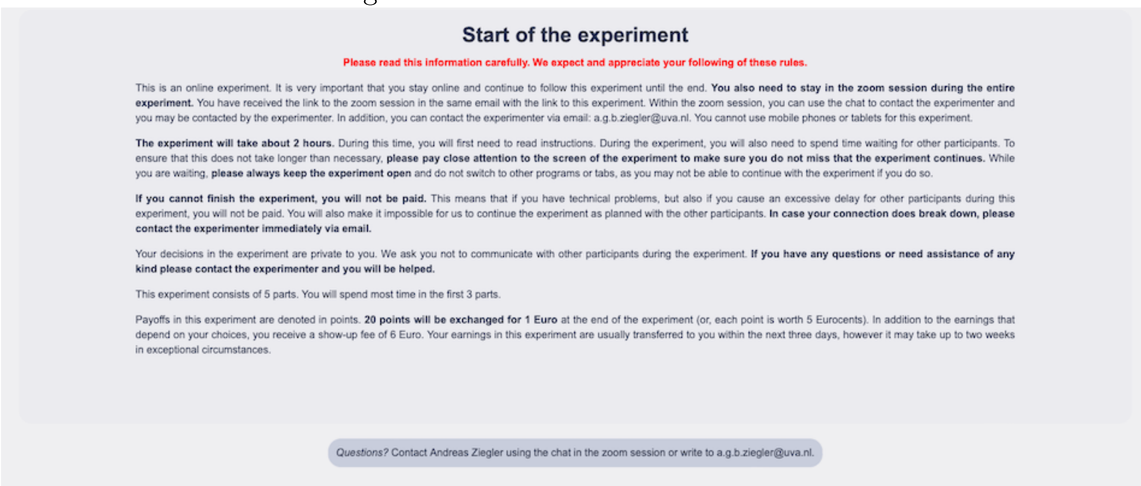


Figure A9: Senders' decision screen



C.2 Receivers' instructions in the first experiment

Figure A10: Receivers' instructions 1



Start of the experiment

Please read this information carefully. We expect and appreciate your following of these rules.

This is an online experiment. It is very important that you stay online and continue to follow this experiment until the end. **You also need to stay in the zoom session during the entire experiment.** You have received the link to the zoom session in the same email with the link to this experiment. Within the zoom session, you can use the chat to contact the experimenter and you may be contacted by the experimenter. In addition, you can contact the experimenter via email: a.g.b.ziegler@uva.nl. You cannot use mobile phones or tablets for this experiment.

The experiment will take about 2 hours. During this time, you will first need to read instructions. During the experiment, you will also need to spend time waiting for other participants. To ensure that this does not take longer than necessary, **please pay close attention to the screen of the experiment to make sure you do not miss that the experiment continues.** While you are waiting, **please always keep the experiment open** and do not switch to other programs or tabs, as you may not be able to continue with the experiment if you do so.

If you cannot finish the experiment, you will not be paid. This means that if you have technical problems, but also if you cause an excessive delay for other participants during this experiment, you will not be paid. You will also make it impossible for us to continue the experiment as planned with the other participants. **In case your connection does break down, please contact the experimenter immediately via email.**

Your decisions in the experiment are private to you. We ask you not to communicate with other participants during the experiment. **If you have any questions or need assistance of any kind please contact the experimenter and you will be helped.**

This experiment consists of 5 parts. You will spend most time in the first 3 parts.

Payoffs in this experiment are denoted in points. **20 points will be exchanged for 1 Euro** at the end of the experiment (or, each point is worth 5 Eurocents). In addition to the earnings that depend on your choices, you receive a show-up fee of 6 Euro. Your earnings in this experiment are usually transferred to you within the next three days, however it may take up to two weeks in exceptional circumstances.

Questions? Contact Andreas Ziegler using the chat in the zoom session or write to a.g.b.ziegler@uva.nl.

Figure A11: Receivers' instructions 2

Instructions page 1/6

The decision situation

In this experiment, you are in the role of a worker. You have the possibility to work on a project, together with a co-worker.

You decide whether you want to work, or whether you do not want to work. Your co-worker simultaneously makes the same decision. How much you earn depends on three factors:

1. whether you work,
2. whether your co-worker works,
3. whether the project you are working on is easy or difficult.

When you do not work, you receive a fixed wage of 170 points.

When you work, you can receive additional earnings, but you can also earn less. How much you earn depends on whether you work on an easy or difficult project. If you work on an easy project, you earn more compared to your fixed wage. If you work on a difficult project, you earn less, as you have to work hard and will not be rewarded sufficiently.

Your additional earnings also depend on whether your co-worker works or not. If you both work at the same time, your additional earnings are lower compared to only one of you working.

The payoff table below shows you how much you and your co-worker earn in each case. There is one table for difficult projects and one table for easy projects. The table for difficult projects is on the left, the table for easy projects is on the right.

In each table, you decide which row is selected ("You work" or "You don't work"). Your co-worker decides which column is selected ("Your co-worker works" or "Your co-worker doesn't work").

In each cell, you see the payoffs for each possible case. The first number is how much you earn, the second number is how much your co-worker earns. For example, if you work and your co-worker does not work on an easy project, the payoffs are 260, 170. This means that you get paid 260 points, and your co-worker gets paid 170 points.

Payoffs for difficult projects

	Your co-worker works	Your co-worker doesn't work
You work	20, 20	70, 170
You don't work	170, 70	170, 170

Payoffs for easy projects

	Your co-worker works	Your co-worker doesn't work
You work	210, 210	260, 170
You don't work	170, 260	170, 170

When you decide whether to work on a project, neither you nor your co-worker at first knows whether this particular project will be difficult or easy. In general, it is equally likely that a given project is difficult or easy.

Continue

Questions? Contact Andreas Ziegler using the chat in the zoom session or write to a.g.b.ziegler@uva.nl.

Figure A12: Receivers' instructions 3

Instructions page 2/6

Recommendations

Before you decide whether to work or not to work on a project, you will receive a recommendation from your manager. The recommendation will either be that you work on this project, or that you do not work. Your co-worker also receives a recommendation before he or she decides, but you do not see the recommendation your co-worker receives.

The manager is played by the computer and decides according to a pre-defined recommendation plan. During the experiment, you always see the recommendation plan the manager uses.

The manager knows whether a project is difficult or easy. In contrast, you and your co-worker will not be directly told whether a project is difficult or easy when you decide whether to work or not.

The manager will use this knowledge about the difficulty of the project to give you recommendations. The recommendation plan is different for difficult and easy projects. This means that you can learn more about the difficulty of the project from the manager's recommendation.

Go back Continue

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Figure A13: Receivers' instructions 4

Instructions page 3/6

Recommendation plans

You and your co-worker will receive recommendations according to the same recommendation plan. The manager always implements the plan you will see during the experiment.

In the tables below, you see an example recommendation plan. This is only an example to help you understand recommendation plans, you will see different plans in the experiment.

For both types of projects, you can see the probability that the manager sends each possible combination of recommendations to you and your co-worker.

Difficult projects	
You: work, co-worker: work	0%
You: work, co-worker: don't work	50%
You: don't work, co-worker: work	50%
You: don't work, co-worker: don't work	0%

Easy projects	
You: work, co-worker: work	100%
You: work, co-worker: don't work	0%
You: don't work, co-worker: work	0%
You: don't work, co-worker: don't work	0%

How to read recommendation plans

On the left, you see the example recommendation plan for difficult projects. You see that you can expect that in 50% of difficult projects, you would receive the recommendation to work, while your co-worker would receive the recommendation not to work. You can also expect that in the other 50% of difficult projects, you would receive the recommendation not to work, while your co-worker would receive the recommendation to work. In this example, you would never both receive the recommendation work at the same time (a joint recommendation to work) when the project is difficult. In addition, for difficult projects, you would also never receive the joint recommendation not to work.

On the right, you see the example recommendation plan for easy projects. In 100% of easy projects, both you and your co-worker would receive the recommendation to work at the same time. For easy projects, neither you nor your co-worker would ever receive the recommendation not to work.

Go back | Continue

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Figure A14: Receivers' instructions 5

Instructions page 4/6

The example recommendation plan

Difficult projects	
You: work, co-worker: work	0%
You: work, co-worker: don't work	50%
You: don't work, co-worker: work	50%
You: don't work, co-worker: don't work	0%

Easy projects	
You: work, co-worker: work	100%
You: work, co-worker: don't work	0%
You: don't work, co-worker: work	0%
You: don't work, co-worker: don't work	0%

What you can learn from recommendation plans about the project

Remember that you will not be directly told whether a project you work on is difficult or easy. At the start, it is equally likely that a project is difficult or easy. Then, you receive your recommendation from your manager. You can combine this recommendation with your manager's recommendation plan to learn more about your project.

Imagine that you receive the recommendation to work. In this example recommendation plan, you cannot tell whether a recommendation to work means for certain that a project is difficult or easy. However, it is more likely that a project is easy whenever you receive the recommendation to work in this example plan. This is the case as whenever your project is easy, you would always receive the recommendation to work. In contrast, you would receive the recommendation to work for only 50% of difficult projects.

What you can learn from recommendation plans about your co-worker's recommendation

Remember also that you will not see the recommendation your co-worker receives. However, you can combine your recommendation with your manager's recommendation plan to learn more about which recommendation your co-worker received.

Imagine that you receive the recommendation to work. In this example recommendation plan, what recommendation your co-worker received depends on whether a particular project is difficult or easy:

- For all difficult projects, your co-worker would receive the recommendation not to work *whenever you receive the recommendation to work.*
- For all easy projects, your co-worker would also receive the recommendation to work.

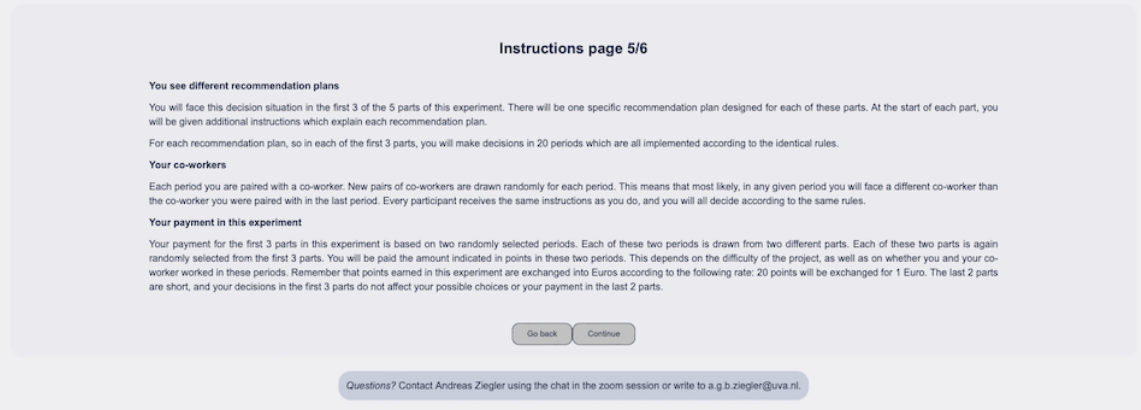
As just explained, a recommendation to work would also mean that it is more likely that the project is easy.

Now, you need to put the two pieces of information together. First, it is more likely that the project would be easy. Second, for easy projects, you would both receive the recommendation to work. Taken together, this means that if you would have received the recommendation to work, it is more likely that you would have both received the recommendation to work in this example plan.

Go back | Continue

Questions? Contact Andreas Ziegler using the chat in the zoom session or write to a.g.b.ziegler@uva.nl.

Figure A15: Receivers' instructions 6


Instructions page 5/6

You see different recommendation plans

You will face this decision situation in the first 3 of the 5 parts of this experiment. There will be one specific recommendation plan designed for each of these parts. At the start of each part, you will be given additional instructions which explain each recommendation plan.

For each recommendation plan, so in each of the first 3 parts, you will make decisions in 20 periods which are all implemented according to the identical rules.

Your co-workers

Each period you are paired with a co-worker. New pairs of co-workers are drawn randomly for each period. This means that most likely, in any given period you will face a different co-worker than the co-worker you were paired with in the last period. Every participant receives the same instructions as you do, and you will all decide according to the same rules.

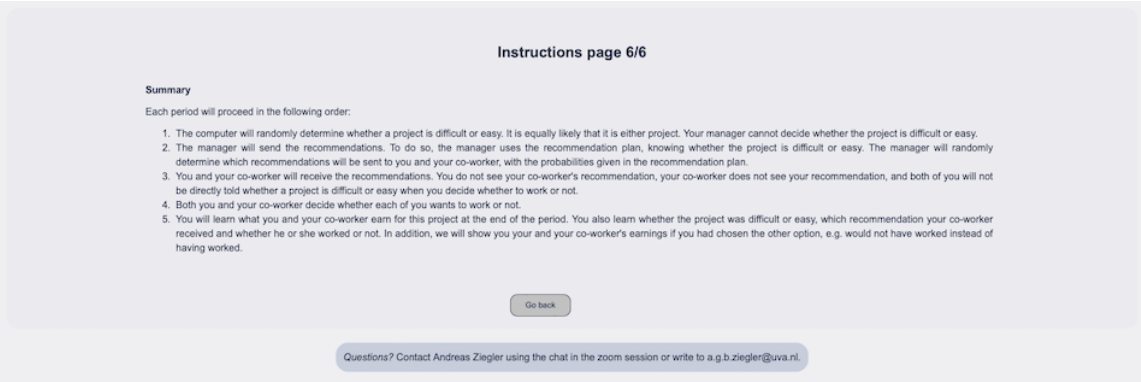
Your payment in this experiment

Your payment for the first 3 parts in this experiment is based on two randomly selected periods. Each of these two periods is drawn from two different parts. Each of these two parts is again randomly selected from the first 3 parts. You will be paid the amount indicated in points in these two periods. This depends on the difficulty of the project, as well as on whether you and your co-worker worked in these periods. Remember that points earned in this experiment are exchanged into Euros according to the following rate: 20 points will be exchanged for 1 Euro. The last 2 parts are short, and your decisions in the first 3 parts do not affect your possible choices or your payment in the last 2 parts.

Go back Continue

Questions? Contact Andreas Ziegler using the chat in the zoom session or write to a.g.b.ziegler@uva.nl.

Figure A16: Receivers' instructions 7


Instructions page 6/6

Summary

Each period will proceed in the following order:

1. The computer will randomly determine whether a project is difficult or easy. It is equally likely that it is either project. Your manager cannot decide whether the project is difficult or easy.
2. The manager will send the recommendations. To do so, the manager uses the recommendation plan, knowing whether the project is difficult or easy. The manager will randomly determine which recommendations will be sent to you and your co-worker, with the probabilities given in the recommendation plan.
3. You and your co-worker will receive the recommendations. You do not see your co-worker's recommendation, your co-worker does not see your recommendation, and both of you will not be directly told whether a project is difficult or easy when you decide whether to work or not.
4. Both you and your co-worker decide whether each of you wants to work or not.
5. You will learn what you and your co-worker earn for this project at the end of the period. You also learn whether the project was difficult or easy, which recommendation your co-worker received and whether he or she worked or not. In addition, we will show you your and your co-worker's earnings if you had chosen the other option, e.g. would not have worked instead of having worked.

Go back

Questions? Contact Andreas Ziegler using the chat in the zoom session or write to a.g.b.ziegler@uva.nl.

Figure A17: Receivers' instructions 8

C.3 Instructions for new information structures

In the first experiment, the level of the information structure was varied between parts. At the beginning of each part, participants received the following instructions.

Figure A18: Instructions for new information structure

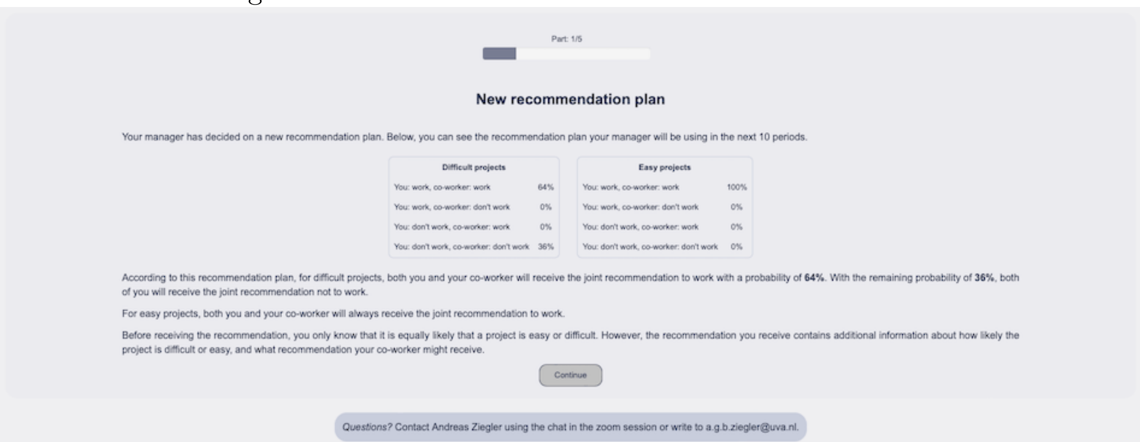
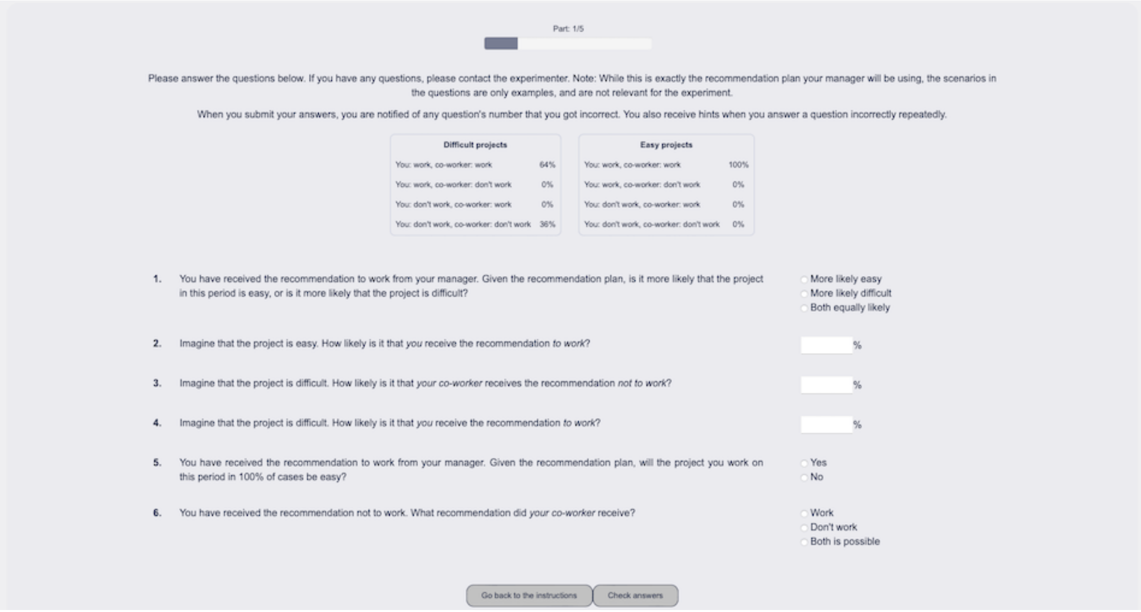


Figure A19: Quiz for new information structure



C.4 Senders' instructions in the second experiment

Figure A20: Senders' instructions 1

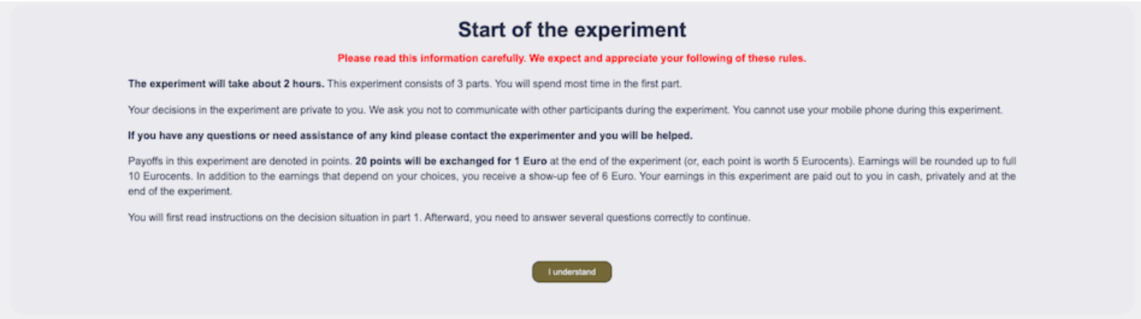


Figure A21: Senders' instructions 2

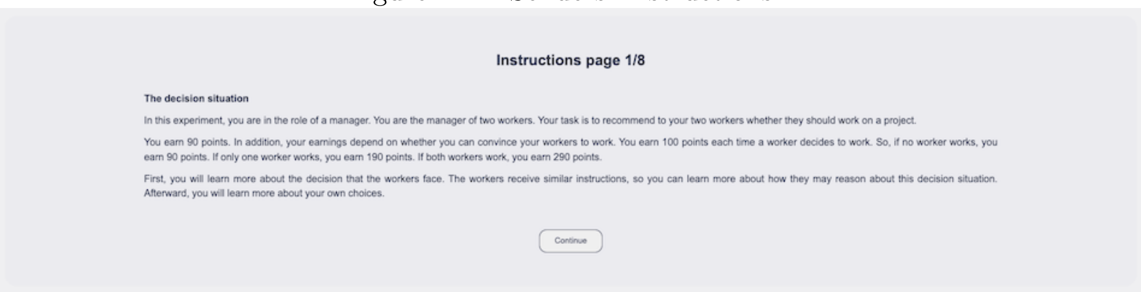


Figure A22: Senders' instructions 3

Instructions page 2/8

The decision situation of the workers

Each worker has to decide whether they want to work or do not want to work. The earnings of each worker depend on three factors:

1. whether they work,
2. whether their co-worker works,
3. whether the project they are working on is easy or difficult.

They receive a fixed wage of 170 points when they do not work.

When they work, they may receive additional earnings, but they may also earn less. How much they earn depends on whether they work on an easy or difficult project. If they work on an easy project, they earn more than their fixed wage. If they work on a difficult project, they earn less.

Their additional earnings also depend on whether their co-worker decides to work or not. If they both work simultaneously, their additional earnings are higher compared to only one of them working.

The payoff tables below show how much the workers earn in each case. The two workers' decision situations are identical; it is entirely random whether each worker is worker 1 or worker 2. There is one table for difficult projects and one table for easy projects. The table for difficult projects is on the left, and the table for easy projects is on the right.

In each table, worker 1 decides which row is selected ("Worker 1 works" or "Worker 1 doesn't work"). Their co-worker, worker 2, decides which column is selected ("Worker 2 works" or "Worker 2 doesn't work").

In each cell, you see the payoffs for each possible case. The first number is how much worker 1 earns; the second number is how much worker 2 earns. For example, if worker 1 works and worker 2 does not work on an easy project, the payoffs are 180, 170 (see the top right cell in the right table). This means that worker 1 gets paid 180 points, and worker 2 gets paid 170 points.

Payoffs for difficult projects

	Worker 2 works	Worker 2 doesn't work
Worker 1 works	100, 100	70, 170
Worker 1 doesn't work	170, 70	170, 170

Payoffs for easy projects

	Worker 2 works	Worker 2 doesn't work
Worker 1 works	210, 210	180, 170
Worker 1 doesn't work	170, 180	170, 170

Your workers know that you earn an additional 100 points each time a worker decides to work.

Recommendations

At first, your workers only know that it is equally likely that they face a difficult or an easy project. Before your workers decide whether to work or not to work on a project, they will receive a recommendation. The recommendation will either be that they work on this project or that they do not work. This can help your workers learn more about the project's difficulty and their decisions' consequences. You will decide what recommendations your workers will receive.

Go back | Continue

Figure A23: Senders' instructions 4

Instructions page 3/8

Sending recommendations

To send recommendations to your workers, you will rely on a recommendation plan. A recommendation plan defines the probabilities that your workers receive a particular combination of recommendations. These probabilities depend on the project's difficulty. In these plans, you choose how often different recommendations will be sent to your two workers. Later, you will learn how to read a recommendation plan.

Timing of recommendations

Decisions are made in the following order:

1. You will choose a recommendation plan.
2. The computer will randomly determine whether the project will be easy or difficult.
3. The computer will send recommendations to your workers in agreement with your chosen recommendation plan.

Before your workers decide whether to work or not, they see which recommendation plan you have chosen and receive their recommendation. Each worker receives only one recommendation from you as their manager, based on your chosen recommendation plan.

The menu of recommendation plans

In the experiment, you can choose from a fixed set of six recommendation plans. You see the menu of all six recommendation plans below. Next, you will learn more about reading recommendation plans.

Plan 1

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	19%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	0%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	0%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	81%	Worker 1: don't work, worker 2: don't work	0%

Plan 2

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	0%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	14%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	14%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	72%	Worker 1: don't work, worker 2: don't work	0%

Plan 3

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	48%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	0%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	0%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	52%	Worker 1: don't work, worker 2: don't work	0%

Plan 4

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	0%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	34%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	34%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	32%	Worker 1: don't work, worker 2: don't work	0%

Plan 5

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	71%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	0%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	0%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	29%	Worker 1: don't work, worker 2: don't work	0%

Plan 6

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	0%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	48%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	48%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	4%	Worker 1: don't work, worker 2: don't work	0%

Go back | Continue

Figure A24: Senders' instructions 5

Instructions page 4/8

Reading recommendation plans

You will now learn how to read recommendation plans, as will your workers. For this, we will describe two recommendation plans from the six recommendation plans available to you, Plan 1 and Plan 6. The other plans work similarly; we randomly chose these two plans to describe them in more detail.

Below, you see Plan 1.

Plan 1

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	19%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	0%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	0%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	81%	Worker 1: don't work, worker 2: don't work	0%

You can see the probability that your workers will receive each possible combination of recommendations for both difficult and easy projects. Again, worker 1 and worker 2 are in identical roles. For example, the first row in the left table tells you the probability that both your workers receive the recommendation to work for difficult projects.

On the left, you see how likely each recommendation will be sent whenever a project is difficult. You see that you can expect that in 19% of difficult projects, both worker 1 and worker 2 would receive the joint recommendation to work (left table, top row). You can also expect that in the other 81% of difficult projects, both worker 1 and worker 2 would receive the joint recommendation not to work (left table, bottom row). In this plan, a worker would never receive the recommendation to work for a difficult project at a point where the other worker received the recommendation not to work (left table, middle two rows).

On the right, you see how likely each recommendation will be sent whenever a project is easy. In 100% of easy projects, both workers would receive the recommendation to work simultaneously (right table, top row). Neither worker would ever receive the recommendation not to work for easy projects.

Below, you see Plan 6.

Plan 6

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	0%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	48%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	48%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	4%	Worker 1: don't work, worker 2: don't work	0%

On the left, you see that you can expect that in 48% of difficult projects, worker 1 receives the recommendation to work, while worker 2 receives the recommendation not to work. You can also expect that in other 48% of difficult projects, worker 2 receives the recommendation to work, while worker 1 receives the recommendation not to work. Lastly, you can expect that in the remaining 4% of difficult projects, both workers receive the joint recommendation not to work.

On the right, you see that in 100% of easy projects, both workers would receive the recommendation to work simultaneously, as in Plan 1.

Go back | Continue

Figure A25: Senders' instructions 6

Instructions page 5/8

All recommendation plans

Plan 1

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	19%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	0%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	0%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	81%	Worker 1: don't work, worker 2: don't work	0%

Plan 2

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	0%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	14%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	14%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	72%	Worker 1: don't work, worker 2: don't work	0%

Plan 3

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	48%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	0%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	0%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	52%	Worker 1: don't work, worker 2: don't work	0%

Plan 4

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	0%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	34%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	34%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	32%	Worker 1: don't work, worker 2: don't work	0%

Plan 5

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	71%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	0%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	0%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	29%	Worker 1: don't work, worker 2: don't work	0%

Plan 6

Difficult projects		Easy projects	
Worker 1: work, worker 2: work	0%	Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	48%	Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	48%	Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	4%	Worker 1: don't work, worker 2: don't work	0%

Above, you see that all recommendation plans always send, with a probability of 100%, the joint recommendation to work to your workers for easy projects. The plans only differ for difficult projects.

When the project is easy, the workers and you would agree that it is in the interest of everyone that both workers work. Your workers receive additional earnings when they work on an easy project, compared to not working. If the task is difficult, workers would prefer not to work while you prefer them to work, as they earn less if they work on a difficult project. For example, if the project turns out to be difficult and worker 2 does not work, worker 1 earns 70 points if they work, but they earn 170 points if they do not work.

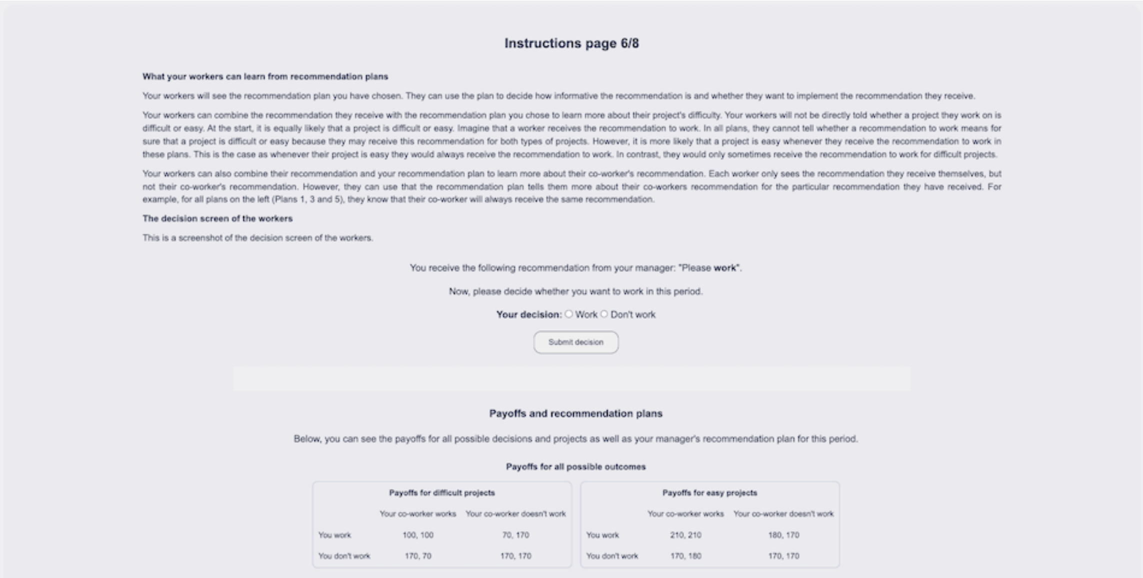
If you would always tell the workers to work, it would be in the best interest of each worker to ignore the recommendation and to never work. You can change this by choosing a plan, as they differ in how often they send the recommendation to work for difficult projects. Also, remember that if both of your workers work simultaneously, their earnings from working are higher than if only one of them works.

On the left are all plans which always send the same recommendations to your two workers: Plans 1, 3, and 5. They will only generate joint recommendations to work or joint recommendations not to work for difficult projects. However, they all differ in how likely your workers will receive these recommendations for difficult projects. Plan 1 sends the recommendation to work for difficult projects least frequently. Plan 3 uses this recommendation more often. Plan 5 sends this recommendation most frequently.

On the right are all plans which potentially send different recommendations to your workers: Plans 2, 4, and 6. For difficult projects, they can generate a recommendation to work for one worker, while another receives the recommendation not to work. Again, they all differ in how likely each recommendation is sent. Plan 2 sends the recommendation to work for difficult projects least frequently, Plan 4 more often, and Plan 6 most frequently.

Go back | Continue

Figure A26: Senders' instructions 7



Instructions page 6/8

What your workers can learn from recommendation plans

Your workers will see the recommendation plan you have chosen. They can use the plan to decide how informative the recommendation is and whether they want to implement the recommendation they receive.

Your workers can combine the recommendation they receive with the recommendation plan you chose to learn more about their project's difficulty. Your workers will not be directly told whether a project they work on is difficult or easy. At the start, it is equally likely that a project is difficult or easy. Imagine that a worker receives the recommendation to work. In all plans, they cannot tell whether a recommendation to work means for sure that a project is difficult or easy because they may receive this recommendation for both types of projects. However, it is more likely that a project is easy whenever they receive the recommendation to work in these plans. This is the case as whenever their project is easy they would always receive the recommendation to work. In contrast, they would only sometimes receive the recommendation to work for difficult projects.

Your workers can also combine their recommendation and your recommendation plan to learn more about their co-worker's recommendation. Each worker only sees the recommendation they receive themselves, but not their co-worker's recommendation. However, they can use that the recommendation plan tells them more about their co-workers recommendation for the particular recommendation they have received. For example, for all plans on the left (Plans 1, 3 and 5), they know that their co-worker will always receive the same recommendation.

The decision screen of the workers

This is a screenshot of the decision screen of the workers.

You receive the following recommendation from your manager: "Please **work**".

Now, please decide whether you want to work in this period.

Your decision: Work Don't work

Submit decision

Payoffs and recommendation plans

Below, you can see the payoffs for all possible decisions and projects as well as your manager's recommendation plan for this period.

Payoffs for all possible outcomes

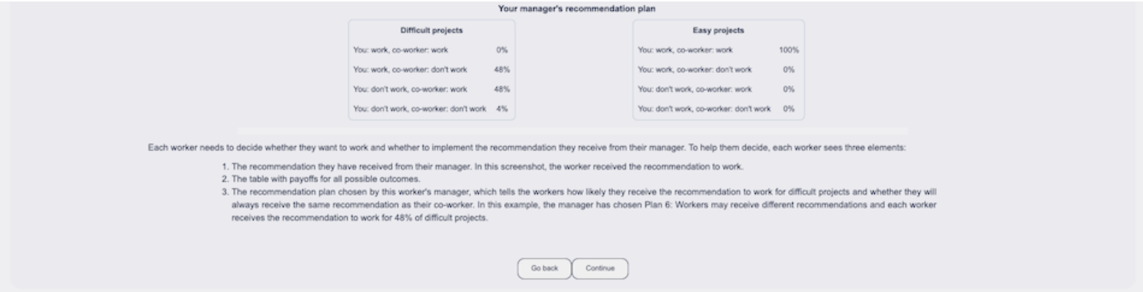
Payoffs for difficult projects

	Your co-worker works	Your co-worker doesn't work
You work	100, 100	70, 170
You don't work	170, 70	170, 170

Payoffs for easy projects

	Your co-worker works	Your co-worker doesn't work
You work	210, 210	180, 170
You don't work	170, 180	170, 170

Figure A27: Senders' instructions 8



Your manager's recommendation plan

Difficult projects

You: work, co-worker: work	0%
You: work, co-worker: don't work	48%
You: don't work, co-worker: work	48%
You: don't work, co-worker: don't work	4%

Easy projects

You: work, co-worker: work	100%
You: work, co-worker: don't work	0%
You: don't work, co-worker: work	0%
You: don't work, co-worker: don't work	0%

Each worker needs to decide whether they want to work and whether to implement the recommendation they receive from their manager. To help them decide, each worker sees three elements:

1. The recommendation they have received from their manager. In this screenshot, the worker received the recommendation to work.
2. The table with payoffs for all possible outcomes.
3. The recommendation plan chosen by this worker's manager, which tells the workers how likely they receive the recommendation to work for difficult projects and whether they will always receive the same recommendation as their co-worker. In this example, the manager has chosen Plan 6: Workers may receive different recommendations and each worker receives the recommendation to work for 48% of difficult projects.

Go back Continue

Figure A28: Senders' instructions 9

Instructions page 7/8

You choose recommendation plans repeatedly

You will face this decision situation in the first part of this experiment. You will choose a recommendation plan in 21 periods.

After you have chosen a recommendation plan, the plan is revealed to your workers. At the start of each period your workers have to answer one question on the plan you have chosen.

Over time, your decision screen will also summarize some key facts about previous choices for each recommendation plan. First, you will see how often workers in this session decided to work after you or other managers have chosen that plan. Second, you see how often a recommendation was followed by workers in this session when facing each plan. Following a recommendation means a worker decided to work after receiving the recommendation to work, or did not work after receiving the recommendation not to work.

Managers and workers

Each period you are paired with two workers. These two workers are paired with one manager, which is you. New groups of workers and managers are drawn randomly for each period. This means that in any given period you will most likely face a different pair of workers and your workers face a different manager than they were grouped with in the last period. Also, each worker will likely face a different co-worker than in the previous period. Every manager receives the same instructions as you, and you will all decide according to the same rules.

In each period, each worker is paired with only one manager. Each worker receives one recommendation, based on the recommendation plan their manager had chosen.

Your payment in this experiment

Your payment for the first part in this experiment is based on two periods, randomly selected from the total 21 periods. You will be paid 90 points plus 100 points for each decision to work by your workers in these two periods. Your earnings do not depend on the difficulty of the project or your chosen recommendation plan.

Remember that points earned in this experiment are exchanged into Euro according to the following rate: 20 points will be exchanged for 1 Euro. The last 2 parts are short, and your decisions in the first part do not affect your possible choices or your payment in the last 2 parts.

Go back | Continue

Figure A29: Senders' instructions 10

Instructions page 8/8

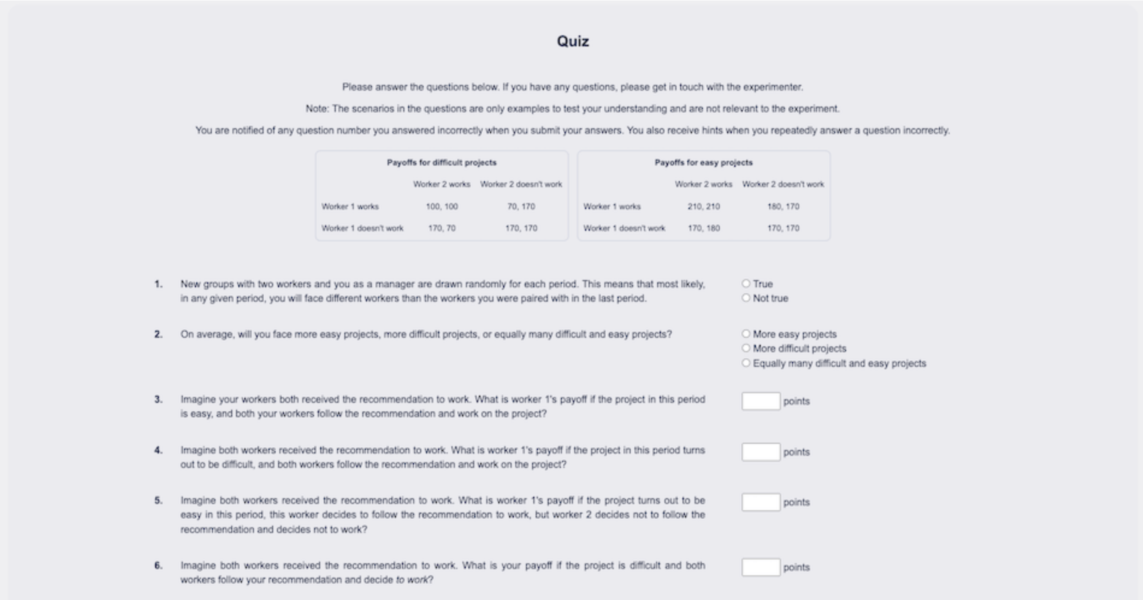
Summary

Each period will proceed in the following order:

1. You choose a recommendation plan.
2. The computer will randomly determine whether a project is difficult or easy. Both is equally likely. You cannot decide or influence whether the project is difficult or easy.
3. The computer will generate automated recommendations according to the recommendation plan you chose. The recommendations your workers receive depend on whether the project is easy or difficult. The computer will randomly determine which recommendations will be sent to your workers, with the probabilities given in the recommendation plan.
4. Your workers will see their recommendation plan and each worker receives their recommendation. They do not see each others' recommendations and will not be directly told whether a project is difficult or easy.
5. Both workers decide whether they want to work or not.
6. You will see what you and your workers earn for this project at the end of the period. You also learn whether the project was difficult or easy.

Go back | Continue

Figure A30: Senders' instructions 11



Quiz

Please answer the questions below. If you have any questions, please get in touch with the experimenter.

Note: The scenarios in the questions are only examples to test your understanding and are not relevant to the experiment.

You are notified of any question number you answered incorrectly when you submit your answers. You also receive hints when you repeatedly answer a question incorrectly.

Payoffs for difficult projects	Worker 2 works	Worker 2 doesn't work
Worker 1 works	100, 100	70, 170
Worker 1 doesn't work	170, 70	170, 170

Payoffs for easy projects	Worker 2 works	Worker 2 doesn't work
Worker 1 works	210, 210	180, 170
Worker 1 doesn't work	170, 180	170, 170

1. New groups with two workers and you as a manager are drawn randomly for each period. This means that most likely, in any given period, you will face different workers than the workers you were paired with in the last period.
   ○ True
   ○ Not true
2. On average, will you face more easy projects, more difficult projects, or equally many difficult and easy projects?
   ○ More easy projects
   ○ More difficult projects
   ○ Equally many difficult and easy projects
3. Imagine your workers both received the recommendation to work. What is worker 1's payoff if the project in this period is easy, and both your workers follow the recommendation and work on the project? ___ points
4. Imagine both workers received the recommendation to work. What is worker 1's payoff if the project in this period turns out to be difficult, and both workers follow the recommendation and work on the project? ___ points
5. Imagine both workers received the recommendation to work. What is worker 1's payoff if the project turns out to be easy in this period, this worker decides to follow the recommendation to work, but worker 2 decides not to follow the recommendation and decides not to work? ___ points
6. Imagine both workers received the recommendation to work. What is your payoff if the project is difficult and both workers follow your recommendation and decide *to work*? ___ points

Figure A31: Senders' instructions 12

7. Imagine both workers received the recommendation to work. What is your payoff if the project is difficult and both workers do not follow your recommendation and decide *not to work*? ___ points
8. If both worker 1 and worker 2 simultaneously work on a project, are worker 1's earning higher, lower or the same compared to when only worker 1 works?
   ○ Lower
   ○ Higher
   ○ The same
9. In Plan 3, see below, do your workers always get the same recommendation, or do they potentially get different recommendations?
   ○ Always the same
   ○ Potentially different
   ○ Both are possible

Plan 3

Difficult projects	
Worker 1: work, worker 2: work	48%
Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	52%

Easy projects	
Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	0%

10. In Plan 4, see below, how likely is it that worker 2 gets the recommendation not to work if the project turns out to be difficult? ___ %

Plan 4

Difficult projects	
Worker 1: work, worker 2: work	0%
Worker 1: work, worker 2: don't work	34%
Worker 1: don't work, worker 2: work	34%
Worker 1: don't work, worker 2: don't work	32%

Easy projects	
Worker 1: work, worker 2: work	100%
Worker 1: work, worker 2: don't work	0%
Worker 1: don't work, worker 2: work	0%
Worker 1: don't work, worker 2: don't work	0%

11. From the first part, how many periods are randomly selected to be paid out to you? ___ periods

Go back to the instructions | Check answers

C.5 Instructions for new information structures

In the second experiment, the level of the information structure was varied between period. At the beginning of each period, participants received a quiz question. The questions were randomized out of a set of questions similar to the questions in the first experiment.

Figure A32: Instructions for new information structure

Period: 1/21

New recommendation plan

Your manager has decided on a recommendation plan. Below, you can see the recommendation plan for this period.

Difficult projects		**Easy projects**	
You: work, co-worker: work	**0%**	You: work, co-worker: work	100%
You: work, co-worker: don't work	**34%**	You: work, co-worker: don't work	0%
You: don't work, co-worker: work	**34%**	You: don't work, co-worker: work	0%
You: don't work, co-worker: don't work	**32%**	You: don't work, co-worker: don't work	0%

Please answer the question below. If you have any questions, please contact the experimenter.

Note: While this is exactly the recommendation plan your manager will be using, the scenarios in the questions are only examples, and are not relevant for the experiment. The recommendations and project's difficulty in the scenarios are not connected to the project you will be working on.

You will receive your recommendation on the next page.

Imagine that the project is *difficult*. How likely is it that *your co-worker* receives the recommendation *not to work*? ____ %

Check answer

C.6 Instructions for tasks at the end of the experiment

After the game, I elicited participants' beliefs, for all structures they faced in the experiment.

Figure A33: Belief instructions

During the experiment, workers received recommendations from their managers. For different recommendations and all six recommendation plans, you now predict the decisions other participants in the role of workers in this experiment made and how often a project was easy.

Imagine that a worker in this experiment has received a recommendation from their manager. This recommendation was either to work, or not to work.

For each recommendation plan, we now ask you to predict how likely a project was easy and how likely participants in this experiment worked. You do this twice: once for the recommendation to work, and once for the recommendation not to work.

The computer will randomly pick 10 cases from the most recent 40 recommendations to work. These 40 recommendations contain no recommendation where you were involved in the first part, and have been made by groups which have finished the first part in this experiment.

Then, you predict in how many of these 10 randomly selected cases you think:

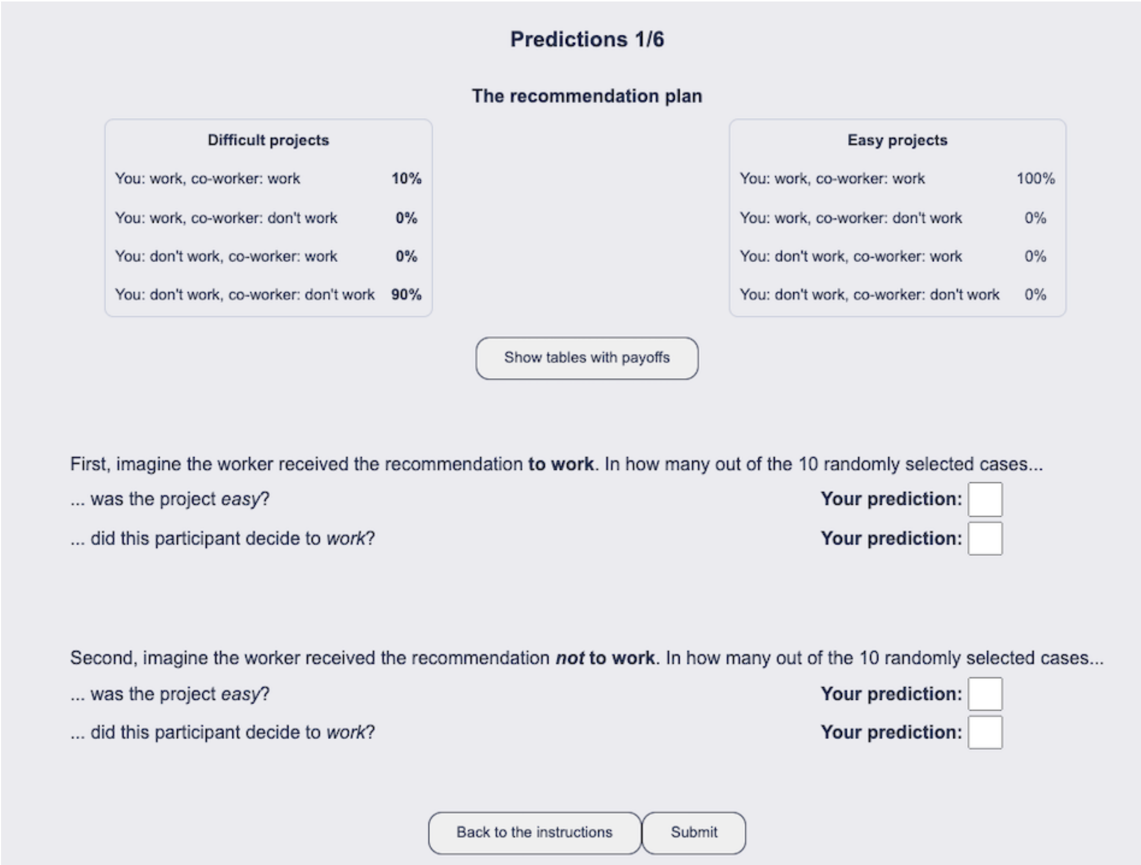
1. The project was easy.
2. The participants receiving this recommendation decided to work.

These will be numbers between 0 and 10. You will also make these predictions for the recommendation not to work. The same procedure will be followed for these recommendations.

From all your predictions in this part, one randomly chosen prediction will be paid out to you. First, we calculate the correct value for the 10 randomly drawn cases. If you correctly predict this value, you will be paid 40 points. If you have not predicted the correct value, you will be paid 0 points. In the unlikely case there are no such 40 recommendations in this experiment so far, you will be paid 40 points for your prediction.

Continue

Figure A34: Example belief decision screen



At the end, participants faced two different risk elicitations. In the first experiment, they saw only the lotteries associated with their treatment. In the second experiment, they saw lotteries for both public and private structures (as below).

Figure A35: Risk 1

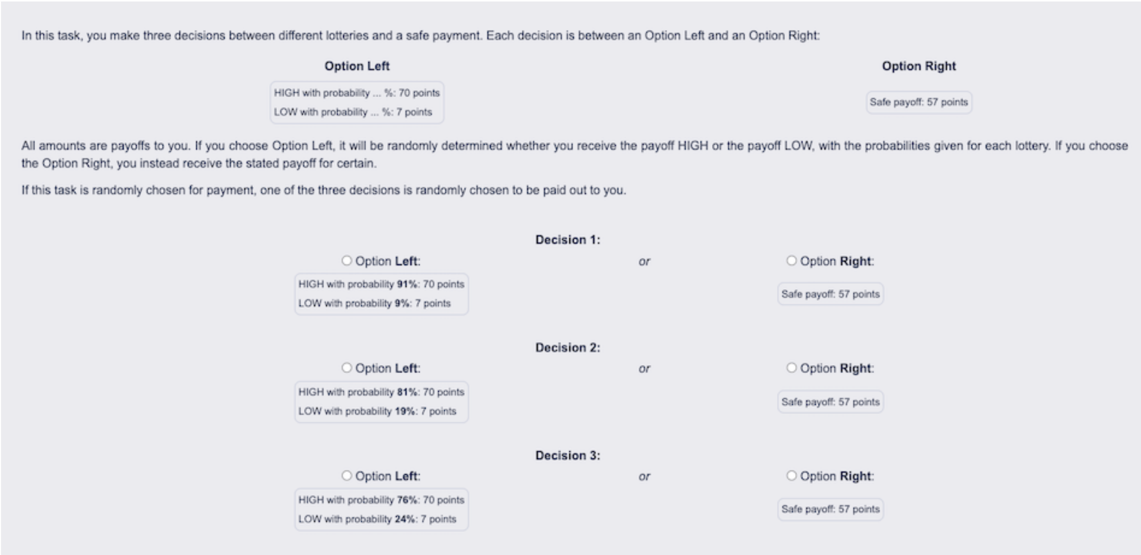


Figure A36: Risk 2

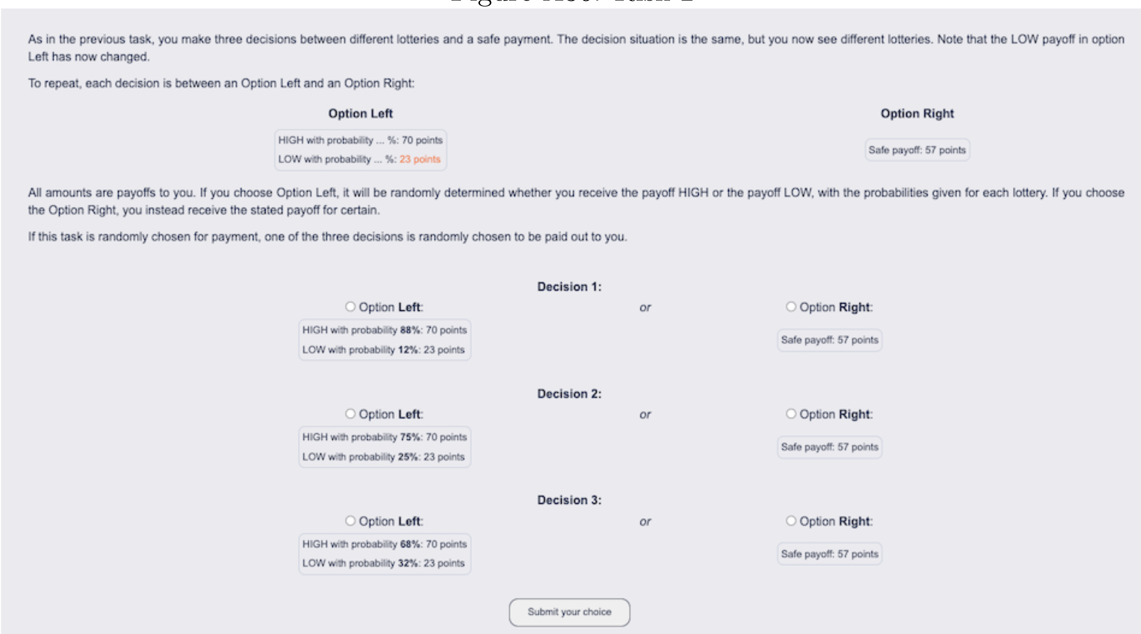


Figure A37: Risk 3

For this task, you choose one gamble you would like to play from six different gambles. The six different gambles are listed below. You must select one and only one of these gambles.

Each gamble has two possible outcomes (Roll Low or Roll High). For every gamble, each Roll has a 50% probability of occurring. At the end of the experiment, it will be randomly determined which event will occur.

For example, if you select Gamble 4 and Roll High occurs, you will be paid 130 points. If Roll Low occurs, you will be paid 40 points.

Your decision:

Gamble	Choice	Roll	Payoff	Probabilities
1	○	High Low	70 points 70 points	50% 50%
2	○	High Low	90 points 60 points	50% 50%
3	○	High Low	110 points 50 points	50% 50%
4	○	High Low	130 points 40 points	50% 50%
5	○	High Low	150 points 30 points	50% 50%
6	○	High Low	170 points 0 points	50% 50%

Submit your choice

Then, participants' social preferences were elicited.

Figure A38: Social preferences 1

In this task, you make 20 decisions, across two tables. Each decision involves a choice between an Option 1 and an Option 2:

Option 1	**Option 2**
Your payoff	Your payoff ....
Other's payoff	Other's payoff ....

The options refer to payments in points to you and one of the other participants in this experiment. For each option, two amounts will be displayed: one amount that you will receive yourself, and one amount that the other participant will receive.

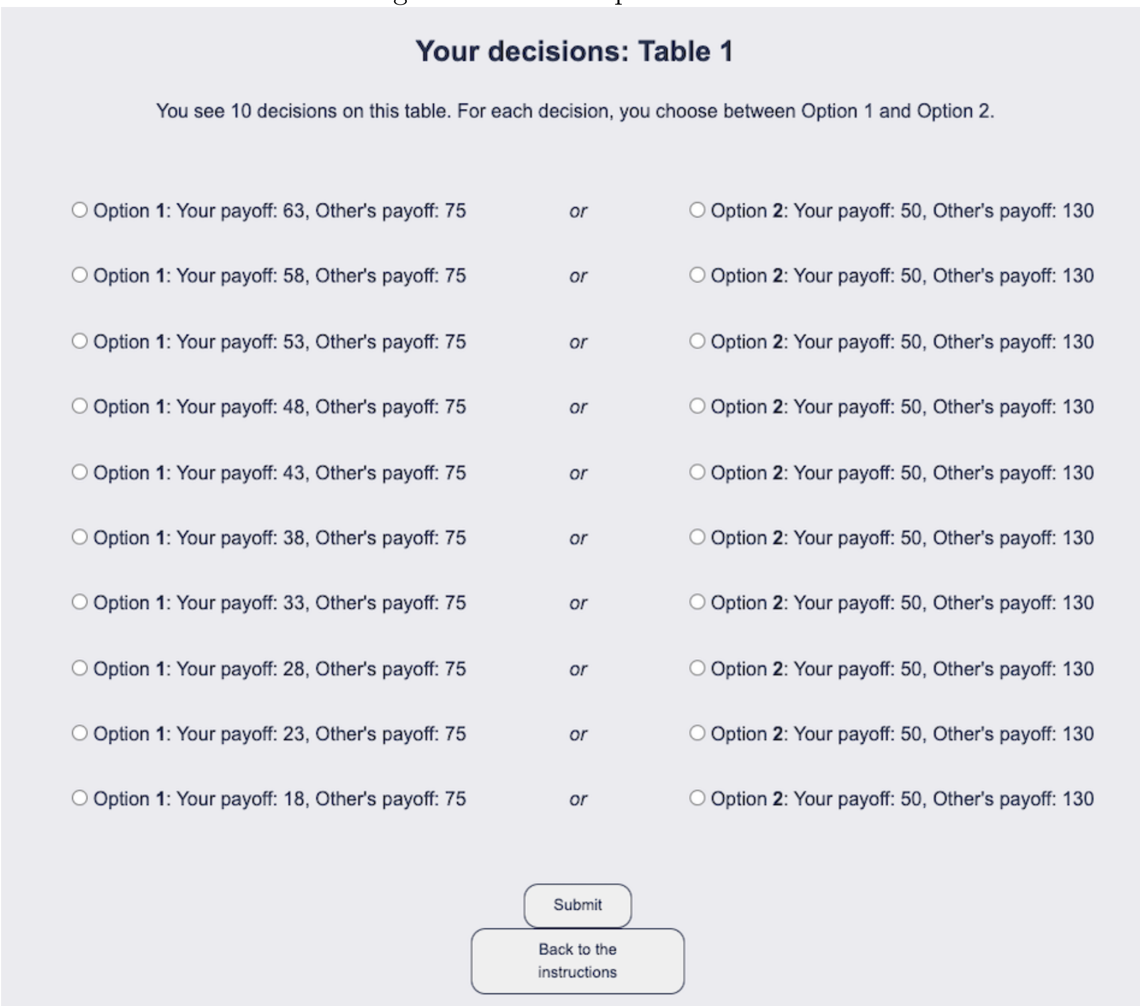
At the end of the experiment, all participants will be randomly matched into pairs. In each pair, one participant will be randomly chosen to be **Player A**, and the other will be **Player B**. If you are chosen to be **Player A**, one out of your 20 decisions made in this task will be randomly selected. For this randomly selected decision and the option you have chosen, you will receive *Your payoff* and your paired **Player B** will receive the *Other's payoff*. Otherwise, if you are chosen to be **Player B**, you will receive the *Other's payoff* as decided by your paired **Player A**. In this case that you are chosen to be **Player B**, your choices in this task do not affect anyone's payment.

If the number of the participants in this task is odd, we cannot combine all of them in pairs at the end of the experiment. In this case, one participant will receive a fixed payment of 130 points as his or her payoff in this task.

Within each table, only Option 1 changes between decisions. To simplify your choice, the computer will pre-fill choices as soon as you click on one decision. If you choose Option 1, all decision on the table above that choice will be pre-filled with Option 1; while all decision below a choice where you choose Option 2 will be pre-filled with Option 2. You can always change your decision until you click on "Submit".

Continue

Figure A39: Social preferences 2



Your decisions: Table 1

You see 10 decisions on this table. For each decision, you choose between Option 1 and Option 2.

○ Option **1**: Your payoff: 63, Other's payoff: 75	*or*	○ Option **2**: Your payoff: 50, Other's payoff: 130
○ Option **1**: Your payoff: 58, Other's payoff: 75	*or*	○ Option **2**: Your payoff: 50, Other's payoff: 130
○ Option **1**: Your payoff: 53, Other's payoff: 75	*or*	○ Option **2**: Your payoff: 50, Other's payoff: 130
○ Option **1**: Your payoff: 48, Other's payoff: 75	*or*	○ Option **2**: Your payoff: 50, Other's payoff: 130
○ Option **1**: Your payoff: 43, Other's payoff: 75	*or*	○ Option **2**: Your payoff: 50, Other's payoff: 130
○ Option **1**: Your payoff: 38, Other's payoff: 75	*or*	○ Option **2**: Your payoff: 50, Other's payoff: 130
○ Option **1**: Your payoff: 33, Other's payoff: 75	*or*	○ Option **2**: Your payoff: 50, Other's payoff: 130
○ Option **1**: Your payoff: 28, Other's payoff: 75	*or*	○ Option **2**: Your payoff: 50, Other's payoff: 130
○ Option **1**: Your payoff: 23, Other's payoff: 75	*or*	○ Option **2**: Your payoff: 50, Other's payoff: 130
○ Option **1**: Your payoff: 18, Other's payoff: 75	*or*	○ Option **2**: Your payoff: 50, Other's payoff: 130

Submit

Back to the instructions

Figure A40: Social preferences 3

Your decisions: Table 2

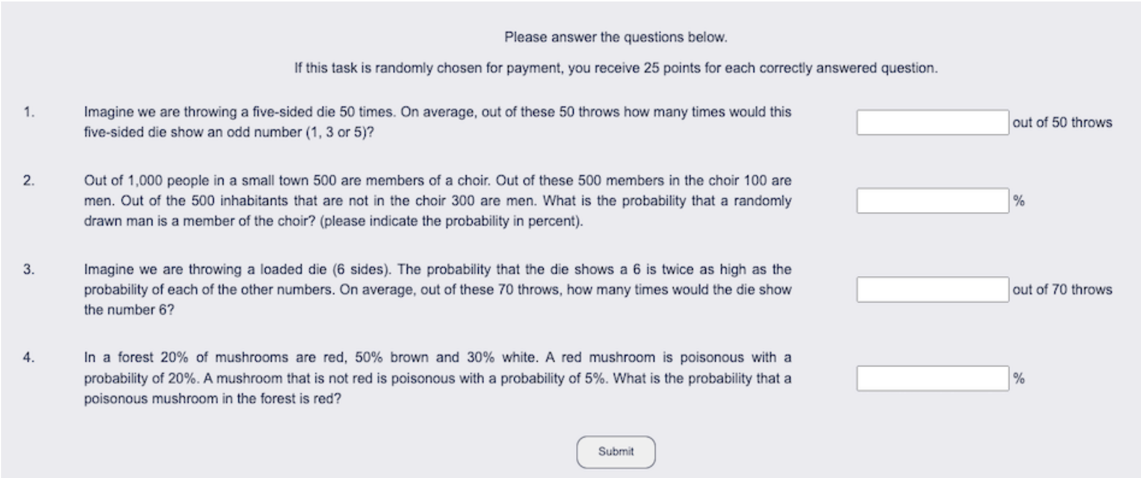
You see 10 decisions on this table. For each decision, you choose between Option 1 and Option 2.

○ Option **1**: Your payoff: 93, Other's payoff: 45	*or*	○ Option **2**: Your payoff: 85, Other's payoff: 25
○ Option **1**: Your payoff: 88, Other's payoff: 45	*or*	○ Option **2**: Your payoff: 85, Other's payoff: 25
○ Option **1**: Your payoff: 83, Other's payoff: 45	*or*	○ Option **2**: Your payoff: 85, Other's payoff: 25
○ Option **1**: Your payoff: 78, Other's payoff: 45	*or*	○ Option **2**: Your payoff: 85, Other's payoff: 25
○ Option **1**: Your payoff: 73, Other's payoff: 45	*or*	○ Option **2**: Your payoff: 85, Other's payoff: 25
○ Option **1**: Your payoff: 68, Other's payoff: 45	*or*	○ Option **2**: Your payoff: 85, Other's payoff: 25
○ Option **1**: Your payoff: 63, Other's payoff: 45	*or*	○ Option **2**: Your payoff: 85, Other's payoff: 25
○ Option **1**: Your payoff: 58, Other's payoff: 45	*or*	○ Option **2**: Your payoff: 85, Other's payoff: 25
○ Option **1**: Your payoff: 53, Other's payoff: 45	*or*	○ Option **2**: Your payoff: 85, Other's payoff: 25
○ Option **1**: Your payoff: 48, Other's payoff: 45	*or*	○ Option **2**: Your payoff: 85, Other's payoff: 25

Submit

Last, participants faced the Berlin numeracy test. In the second experiment, I used only two out of the four questions.

Figure A41: Numeracy task in the first experiment



Please answer the questions below.

If this task is randomly chosen for payment, you receive 25 points for each correctly answered question.

1. Imagine we are throwing a five-sided die 50 times. On average, out of these 50 throws how many times would this five-sided die show an odd number (1, 3 or 5)? [ ] out of 50 throws

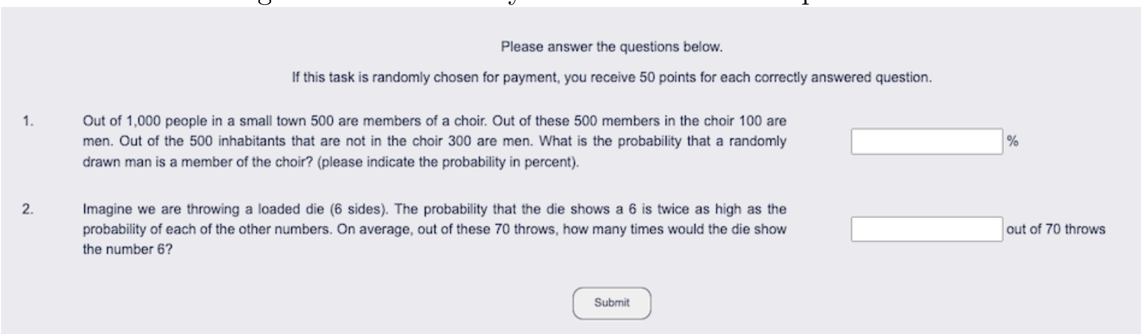
2. Out of 1,000 people in a small town 500 are members of a choir. Out of these 500 members in the choir 100 are men. Out of the 500 inhabitants that are not in the choir 300 are men. What is the probability that a randomly drawn man is a member of the choir? (please indicate the probability in percent). [ ] %

3. Imagine we are throwing a loaded die (6 sides). The probability that the die shows a 6 is twice as high as the probability of each of the other numbers. On average, out of these 70 throws, how many times would the die show the number 6? [ ] out of 70 throws

4. In a forest 20% of mushrooms are red, 50% brown and 30% white. A red mushroom is poisonous with a probability of 20%. A mushroom that is not red is poisonous with a probability of 5%. What is the probability that a poisonous mushroom in the forest is red? [ ] %

Submit

Figure A42: Numeracy task in the second experiment



Please answer the questions below.

If this task is randomly chosen for payment, you receive 50 points for each correctly answered question.

1. Out of 1,000 people in a small town 500 are members of a choir. Out of these 500 members in the choir 100 are men. Out of the 500 inhabitants that are not in the choir 300 are men. What is the probability that a randomly drawn man is a member of the choir? (please indicate the probability in percent). [ ] %

2. Imagine we are throwing a loaded die (6 sides). The probability that the die shows a 6 is twice as high as the probability of each of the other numbers. On average, out of these 70 throws, how many times would the die show the number 6? [ ] out of 70 throws

Submit