Experiments on the hold-up problem and delegation

Yang, Yadi

DOI:
10.26116/center-lis-2108

Publication date:
2021

Document Version
Publisher's PDF, also known as Version of record

CentER

Experiments on the hold-up problem and delegation

YADI YANG

Tilburg University

Experiments on the hold-up problem and delegation

Proefschrift ter verkrijging van de graad van doctor aan Tilburg University

op gezag van de rector magnificus, prof. dr. W.B.H.J. van de Donk, in het

openbaar te verdedigen ten overstaan van een door het college voor

promoties aangewezen commissie in de Portrettenzaal van de Universiteit op

dinsdag 25 mei 2021 om 10.00 uur

door

Yadi Yang,

geboren te Hunan, China

Promotor: prof. dr. J.J.M. Potters (Tilburg University)

Copromotor: dr. C. Argenton (Tilburg University)

leden promotiecommissie: prof. dr. S. Suetens (Tilburg University)
prof. dr. R. Sloof (Universiteit van Amsterdam)
prof. dr. W. Müller (Tilburg University)
dr. A. Possajennikov (University of Nothingham)

<If the thesis has been made (partly) possible (financially or otherwise) by an external organization/authority, this should be included here.>

ISBN <optional to mention ISBN code>

Naam drukkerij <name of print-shop>

Experiments on the hold-up problem and delegation

Yadi Yang

April 6, 2021

Acknowledgements

This dissertation would not have been possible had I not been blessed with the kind help and support from many people.

First and foremost, I wish to express my sincere gratitude to my supervisors Jan Potters and Cédric Argenton. They have provided constant support and invaluable guidance throughout all stages of my PhD period. Jan has always been a great mentor and a role model who inspired me to find my way of becoming a researcher in experimental economics. He never hesitates to share his experience on conducing effective research, academic writing, giving interesting presentations, as well as striking a balance between various challenges of completing a PhD. I still have to learn how to remain passionate and naturally insert witty comments when giving a presentation like how Jan does. Jan is always patient to listen to my every preliminary research ideas and never hesitates to ask insightful questions that motivates me to push my boundaries to search for answers. Cédric constantly encourages me to think from different perspectives. He always provides precise, prompt, and detailed feedback on my work, from the motivation of research questions through details of experimental design to correction of my grammar mistakes. Both Jan and Cédric have provided tremendous support to me both academically and emotionally. I would like to thank them for always trusting me and guiding me to ask new questions and search for new answers, as well as encouraging me when I felt pessimistic about my PhD career. It has been a lucky and pleasant experience having them as my supervisors.

I would also like to express my gratitude to the members of my dissertation committee: Alex Possajennikov, Randolph Sloof, Sigrid Suetens, and Wieland Müller. I appreciate their time and effort spent in carefully reading this dissertation and their constructive comments and suggestions. Their generous suggestions have greatly contributed to the improvement of this dissertation, and will continue to have substantial impacts on the future development of the papers. In particular, I am thankful to Sigrid who shared her insights and suggestions on job market paper.

I have benefited from insightful meetings and discussions with colleagues from the Economics Department of Tilburg University, especially to the Behavioral and

Experimental Group. They include but are not limited to: Jan Boone, Elena Cettolin, Patricio Dalton, Eric van Damme, Mery Ferrando, Reyer Gerlagh, Eline van der Heijden, Gijs van de Kuilen, Madina Kurmangaliyeva, Boris van Leeuwen, Jens Prüfer, David Schindler, Florian Schütt, Ben Vollaard, and Bert Willems. They have provided helpful comments on my papers. Great thanks also go to Korine Bor, Cecile de Bruijn, Aislinn Callahan-Brandt, Ella Munoz-Baan, and all the previous and current secretaries of the Department of Economics for their strong administrative support, especially during my job applications, and academic meeting reservations.

I am grateful to have gained friendships during my journey of completing the PhD. Many thanks go to Manwei Liu and Wanqing Zhang, for being my closest companions on this journey, for all discussions about research and all the chit-chats over coffee that distract us from work, for all late nights in the office and all the afternoon walks in the forest, and for the laughter and cries we shared. I want to thank Chen Sun and Xue Xu for generously sharing their experience of being a fellow researcher in experimental economics. I have benefited greatly from their advice on conducting experiments and searching for information on conferences and training courses. I am also grateful to Shuai Chen, Clemens Fiedler, Ruonan Fu, Tao Han, Chen He, Lei Lei, Zihao Liu, Marie le Mouel, Zilong Niu, Yi Sheng, Xiaoyu Wang, Mingjia Xie, Yuanyuan Xu, Yi Zhang, Nan Zhao, Yeqiu Zheng and many others.

I am grateful that I have found dancing as an interest and emotional support. Whenever I feel stuck or discouraged in my research, dancing always help me recover from negative emotions. Many thanks to all my dance teachers at the Sports Center of Tilburg University: Anna, Annette, Helena, Kevin, Marleen, Mechteld, Nadia, Sammy, and Theresa for helping me come to terms with all my negative thoughts through dancing. I am thankful to my dancing friends Jessie and Jingze for trying out new dance classes with me. I am grateful to have met all the teammates during my three years in my dance team *Infini-T*. It has been a great pleasure training with them for all the competitions and showcases.

I am specially thankful to my dear friend Remy, who always cheers me up whenever I feel like procrastinating and showers me with encouraging words, warm hugs, and witty humor whenever I feel down. Many thanks to Minghao and Mingyu, for constantly supporting me through my most depressive times.

I also thank Qualcomm for providing financial support for my PhD.

Last but not least, I owe my deepest gratitude to my parents for their unconditional love and constant support through the years.

Contents

Chapter 1

Introduction

The PhD dissertation consists of a series of four papers on strategic decisions in the hold-up problem and delegation in the field of experimental economics: a review of papers using the experimental method to examine the hold-up problem (Chapter 1), an experimental study on the relationship between delegation and the nature of strategic interaction (Chapter 2), an experimental study examining the effectiveness of strategic delegation as a remedy for the hold-up problem (Chapter 3), and an experimental study investigating the credit-shifting effect of delegation (Chapter 4).

Chapter 1 provides an overview of 26 experimental studies published in 1993–2020 on the hold-up problem. The hold-up problem refers to underinvestment in relationship-specific assets when multiple parties divide the gains from investment via *ex post* bargaining with incomplete contracts. The papers reviewed in this chapter conduct experiments that take the form of a two-stage game with one prior investment stage and one subsequent bargaining stage to divide the surplus from investment. The papers are classified based on two essential features of experimental design: 1) whether only one party invests or both parties invest and 2) whether one party has full bargaining power or parties have balanced power. The experimental results all show various degrees of underinvestment, but to a smaller extent than standard predictions. The common findings indicate the role of social preference concerns in mitigating the hold-up problem. This chapter also summarizes and compares the various methods to alleviate the hold-up problem as investigated in the papers reviewed. It shows advantages of using the experimental method to examine remedies for the hold-up problem, especially the remedies that build on informal controls, enhance the effect of social preferences, and do not impose strict changes on the institutional environment.

Chapter 2 studies delegation with strategic complements and strategic substitutes. As argued by Schelling (1960), strategic delegation can serve as a commitment device if the principal sets appropriate incentives for the delegate. The shape of the incentives is determined by the nature of the strategic interaction. This chapter explores this relationship in a model and a corresponding experiment with two principals and two agents. Each principal has to decide whether to give its agent cooperative (altruistic) incentives or competitive (spiteful) incentives. The important prediction of the model is that principals will set cooperative incentives for their agents in case the game is characterized by strategic complements and competitive incentives in case the game is characterized by strategic substitutes. The main result of the experiment is that principals indeed set more competitive incentives with strategic substitutes than with strategic complements, but that the incentives are competitive (rather than cooperative) even in the complements case. This latter result is in line with findings in the literature that players already tend to be cooperative by themselves in games with strategic complements, so that principals do not want to, or need to, further incentivize the agents in that direction.

One important implication of Chapter 1 is that methods to mitigate the hold-up problem build on the establishment of commitment. Chapter 2 investigates strategic delegation as a commitment device. Chapter 3 extends the implications from the previous two chapters. In this chapter, strategic delegation is applied to a prototypical hold-up problem to examine its effectiveness as a mitigation remedy for the hold-up problem in an experiment. This chapter examines a canonical hold-up problem Ellingsen and Johannesson (2004b), in which an investing player can be exploited (held-up) by a non-investing player who offers the investing player a share of the surplus which is below the investment costs. It is shown theoretically that delegation can mitigate this hold-up problem by providing the delegate with the appropriate incentives. The investing player can incentivize a delegate to only accept offers which are non-exploiting, and the non-investing player can incentivize a delegate to not make exploiting offers. A corresponding experiment is conducted to test whether such contracts do in fact emerge. The results indicate that the theoretically predicted types of contracts occur in only about forty percent of the interactions. However, if they do, they lead to significantly higher investment rates and less frequent hold-up. The results indicate the potential efficiency-enhancing effect of strategic delegation.

Chapter 4 studies a different aspect of delegation: the credit-alleviating and blame-shifting effect of delegation. Evidence from existing studies indicate that blame for unkind decisions can be effectively shifted by delegating to a third party. However, there is not sufficient evidence indicating whether delegation also reduces credit for kind decisions. Chapter 4 aims to explore the answer for this question in a delegated dictator-game experiment (Bartling and Fischbacher, 2012) with one principal, one delegate, and two receivers. The principal decides between an unfair allocation and a fair allocation. The unfair allocation assigns higher payoffs to the principal and the delegate but lower payoffs to the two recipients, while the fair allocation evenly divides the same total amount among the four players. The principal can also delegate the decision. Chapter 4 compares one treatment where the receivers can implement punishment and three treatments where the receivers can assign rewards. The results provide an affirmative answer to the research question. Delegation reduces the reward for "good" behaviors as well as punishment for "bad" behaviors.

Chapter 2

A survey of the hold-up problem in the experimental economics literature[1]

2.1 Introduction

When multiple parties make non-recoverable relationship-specific investments that generate a joint surplus to be divided through ex-post bargaining, underinvestment may occur. Since the final allocation is determined by the interplay of ex-post bargaining power of all participating parties, each agent is unlikely to fully appropriate the return from his investment. Therefore, agents refrain from investing at the efficient level for fear of being held-up by their counterparts. This underinvestment is referred to as the "hold-up problem" in the economic literature (Che and Sákovics, 2008). It is a common phenomenon in bilateral transactions that rely on incomplete contracts. Common examples of the hold-up problem can be found in procurement contracts where the manufacturer needs to make product-specific investment beforehand, employment contracts where the employee needs to invest in firm-specific skills, etc. In these cases, the specific investments are non-contractable, and cannot be appropriated by suppliers or employees if being held-up. As a result, manufacturers and employees may make insufficient investments in the investment stage.

Early theoretical analyses of the hold-up problem can be found in Williamson (1971) and Klein et al. (1978). Klein (1998) summarizes three main features of the hold-up problem as "specific investments", "incomplete contracts", and "renegotiation". Grout (1984) develops a model for the hold-up problem in an intra-firm

[1]This chapter is adapted from Yang, Y. (2021). A survey of the hold-up problem in the experimental economics literature. *Journal of Economic Surveys*, 35(1):227—249.

employment contract setting,[2] while Tirole (1986) develops a similar model in inter-firm transactions.[3] A simplified version of their two-stage model with one investor (seller) and one proposer (buyer) is summarized by Che and Sákovics (2008). A buyer and a seller decide upon a contract to trade a positive quantity q at price t. Before the trade takes place, the seller makes a binary investment decision with fixed and non-recoverable costs to increase the joint surplus. The investment choice is observable but not verifiable, thus cannot be contracted upon. The price and quantity to be traded in the second stage are the only contractable variables in this case. The socially optimal solution that maximizes the overall payoff yields the seller to invest. However, Nash bargaining solution at the negotiation stage yields an equal split of the gross surplus between the seller and the buyer. When the seller decides whether to invest in the first stage, he anticipates that he bears the whole investment cost but receives only half of the investment return. There are conditions under which a rational seller refrains from investing in the first stage, despite it being socially optimal. A more general two-agent model is analysed by Grossman and Hart (1986). In the first stage, each agent independently makes an investment decision that contributes to a joint-surplus. Agent i invests I_i with per-unit cost c. Investments I_1 and I_2 together generate a joint surplus $R(I_1, I_2)$, with $\partial R(I_1, I_2)/\partial I_i \geqslant 0$, and $\partial^2 R(I_1, I_2)/\partial I_i^2 \leqslant 0$. In the second stage, the two agents negotiate over the division of the surplus. The (Nash) cooperative game solution gives a net payoff of $\frac{1}{2}R(I_1, I_2) - cI_i$ to agent i. Using backwards induction, anticipating the bargaining result, agent i chooses the first stage investment level I_i to maximize the net payoff, resulting in the selection of $\hat{I}_i$ that satisfies the first order condition $\frac{1}{2}\partial R/\partial \hat{I}_i = c$. However, the first-best investment levels that maximize the total payoff $R(I_1, I_2) - cI_1 - cI_2$ are given by (I_1^*, I_2^*) that satisfies the first order condition $\partial R/\partial I_i^* = c$. With the assumptions about the first and second order derivatives of $R(.)$, it can be shown that $\hat{I}_i \leqslant I_i^*$. In the absence of a contract on ex-ante investments, undesirable underinvestment occurs.

In recent years, conducting laboratory experiments has become a popular data collection method among economists. It allows researchers to implement the institutional environment that aligns best with theory, to insert strict control that reduces various confounding factors, and to create counterfactuals that establish causality. These features are usually difficult to obtain with field data. The experimental method has been frequently applied in examining people's behaviors in strategic interactions. The typical setting of an experiment on the hold-up

[2] Malcomson (1997) provides an overview of the hold-up problem in the labor market.

[3] See Schmitz (2001) for a survey of the hold-up problem using the incomplete contracts approach.

problem takes the form of a “nested bargaining game”, as phrased by Sonnemans et al. (2001). Two subjects are paired up to participate in a two-stage game. In the first stage, they make non-contractable investments with fixed and non-recoverable costs. In the second stage, they bargain over the surplus generated from previous investment decisions. The detailed set-up varies across different experiments. Investment may come from both subjects or only one subject, and could be either a continuous decision where the subject selects a level to invest or a binary decision where the subject selects whether to invest or not. The bargaining mechanism also differs across studies, with ultimatum game, dictator game, and alternating-offer Rubinstein (1982) bargaining game as the most common forms. Being a bargaining game with a preceding investment stage, the hold-up problem refers to a very specific scenario. As a consequence, the number of experimental studies that specifically focus on “hold-up” games is limited. The rest of this paper reviews experimental papers on the hold-up problem. By comparing their experimental settings, the typical results, and manipulations they employ to solve the hold-up problem, it is found that social preferences play an important role in mitigating the hold-up problem, the effectiveness of which is found to largely depend on various aspects of the game structure.

To avoid inefficiency, economists have been looking into ways of diminishing the prevalence of hold-up and restore investment incentives. Miller (2011) provides a comprehensive summary of remedies for the hold-up problem. Conventional remedies (classified as “formal controls” by Miller (2011)) can be categorized into two major types: organizational remedies and contractual remedies (Che and Sákovics, 2008). Laboratory experiment provides a convenient testbed to examine the treatment effect of a certain policy under strictly controlled conditions. Experiments testing the two types of strategic remedies are limited in numbers. Joint ownership and option contracts are found to effectively mitigate the hold-up problem. In addition to these conventional remedies, behavioral remedies (classified as “informal controls” by Miller (2011)) such as observable investment, costly punishment, communication, and the provision of social history are found to have a significant effect through the channel of social preferences.

The rest of the paper is organized as follows. The next section summarizes the experimental studies on the hold-up problem. Experiments are classified based on their experimental design. Section 2.3 reviews the different remedies for the hold-up problem, which includes conventional remedies as well as behavioral remedies. The main findings and results are summarized in Section 2.4. Section 2.5 discusses the generalisability of laboratory results and implications for further research.

2.2 Experiments about hold-up

Experimental studies on the hold-up problem differ in various aspects of their design. Nevertheless, they share common features in the basic set-up. A prototypical hold-up experiment consists of two stages: a production stage requiring sunk investments and a subsequent bargaining stage to divide the surplus earned from the joint-production. Following the theoretical model by Grossman and Hart (1986) and Tirole (1986), most experiments pair up two endowed subjects who each decide individually how much of their endowment to be invested in a joint production. In some experiments, the two subjects are explicitly framed as "buyers" and "sellers" (Hackett, 1993, 1994), representing the original incomplete contract problem. Some other experimenters frame the roles as "investor" and "trading partner" (Ellingsen and Johannesson, 2004a,b). In most other studies, a neutral framing that labels different roles by different letters is employed. Early experiments allow both parties to make the investment decisions. In more recent experiments, a more common setting is to allow only one investor. Subjects in all studies are undergraduate students with business and economics majors. A comparison of the general features of experimental design is shown in Table 2.1.

2.2.1 Comparison of experimental settings

Despite variation in details, an experiment on the hold-up problem essentially consists of an investment stage and a subsequent bargaining stage. For each of the two stages, experiments differ in whether both subjects or only the investor is allowed to take actions and whether the decisions are dichotomous or continuous. In the bargaining stage, the specific bargaining mechanism employed in each study also differs. A summary of the hold-up experiments reviewed in this paper by their setup is shown in Table 2.2.

The earliest experiment on the hold-up problem dates back to Hackett (1993, 1994), the design of which closely resembles the model by Grossman and Hart (1986). Hackett conducts ten periods of a two-sided nested bargaining game, using stranger matching, so that each subject will not encounter another subject that he had interacted with twice, to avoid reputation effects. The roles of buyer or seller are randomly assigned to subjects at the beginning of every period. Subjects simultaneously select a value that induces an unrecoverable quadratic cost. The values chosen by both players generate a joint surplus with production cost, the value of which is known to both subjects. Both the surplus and the production cost can be either high or low. The value selected by the buyer corresponds to the probability of a high surplus, and the value selected by the seller corresponds to

Table 2.1: Experimental design of the hold-up problem (I)

	Periods	Matching	Framing	Fixed role	Intervention
Hackett (1993)	10	Stranger	Buyer-Seller	No	Relative investment incentives
Hackett (1994)	10	Stranger	Buyer-Seller	No	Discount rate, investment observability
Berg et al. (1995)	1	One-shot	Neutral	-	Social history
Gantner et al. (2001)	2	Random	Neutral	Yes	
Sonnemans et al. (2001)	18	Block structure	Neutral	Within blocks	Level of outside option
Oosterbeek et al. (2003)	10	Block structure	Neutral	Within blocks	Level of outside option
Ellingsen and Johannesson (2004a)	1	One-shot	Investor-Partner	-	Communication (Promise and threat)
Ellingsen and Johannesson (2004b)	1	One-shot	Investor-Partner	-	Communication (Promise and threat)
Sloof et al. (2004)	20	Block structure	Neutral	Within blocks	Level of outside option
Charness and Dufwenberg (2006)	1	One-shot	Neutral	-	Communication (Promise)
Sloof et al. (2007)	36	Block structure	Neutral	Within blocks	Investment costs and observability
Fehr et al. (2008)	10	Random	Neutral	Yes	Ownership structure
Vanberg (2008)	1	One-shot	Neutral	-	Communication (Promise)
Charness and Dufwenberg (2010)	1	One-shot	Neutral	-	Communication (Promise)
Charness et al. (2011)	36	Random	Neutral	No	History information
Hoppe and Schmitz (2011)	1	One-shot	Neutral	-	Contractual changes
Huck et al. (2012)	30	Random/Endogenous	Neutral	Yes	History, competition
Dufwenberg et al. (2013)	5	Random	Neutral	Yes	Level of outside option
Morita and Servátka (2013)	1	One-shot	Neutral	-	Group identity
Eisenkopf and Nüesch (2016)	10	Stranger	Neutral	Yes	Third party
Ismayilov and Potters (2016)	1	One-shot	Neutral	-	Communication (Promise)
Eisenkopf and Nüesch (2017)	10	Stranger	Neutral	Yes	Third party
Davis and Leider (2018)	10	Random	Supplier-Retailer	Yes	Contractual changes
Morita and Servátka (2018)	1	One-shot	Neutral	-	Outside option
Haruvy et al. (2019)	100	Random	Neutral	Yes	Uncertainty, reputation
Zheng et al. (2020)	1	One-shot	Neutral	-	Reciprocity, veto power

Table 2.2: Experimental design of the hold-up problem (II)

	Investment Stage		
	Both invest	*Only one invests*	
	Continuous	Continuous	Binary
Both propose			
One-shot bargaining			
Nash demand	Gantner et al. (2001)		Ellingsen and Johannesson (2004a)
Dynamic bargaining			
Rubinstein bargaining	Hackett (1993, 1994)	Sonnemans et al. (2001)	Oosterbeek et al. (2003), Sloof et al. (2004)
Dynamic with feedback			Davis and Leider (2018)
One subject proposes			
Dictator game			
Binary Offer			Charness and Dufwenberg (2006, 2010), Vanberg (2008) Charness et al. (2011), Huck et al. (2012) Ismayilov and Potters (2016)
Level Offer		Berg et al. (1995) Eisenkopf and Nüesch (2016, 2017)	Sloof et al. (2007)
Ultimatum game			
Binary Offer			Dufwenberg et al. (2013)
Level Offer	Gantner et al. (2001)		Ellingsen and Johannesson (2004b), Hoppe and Schmitz (2011), Morita and Servátka (2013, 2018), Haruvy et al. (2019), Zheng et al. (2020)

the probability of low production cost. The second-stage bargaining is a modified version of the Rubinstein (1982) procedure. In his implementation, Hackett fixed the maximum number of allowed bargaining rounds without informing the subjects, following the setting of Binmore et al. (1991), so that the subjects only know that there is a positive probability of a forced breakdown after each bargaining round. Before the bargaining stage, subjects each can decide whether to enter the bargaining stage or not. One or both subjects opting out terminates the game, and allocates all the surplus to the buyer and all the costs to the seller. Bargaining only proceeds when both parties agree to bargain. Subjects alternate in making offers, with the buyer proposing first in all odd-numbered periods and the seller proposing first in all even-numbered periods. In each bargaining round, the responder chooses whether to accept or to reject and comes up with a counter-offer in the next bargaining round. The disagreement payoff is the same as the opting out results. After the allocation of the surplus is finalized, each subject has an independent option to veto the results. Selection of veto by at least one subject eradicates all surplus and production cost, leaving both subjects zero payoffs.

In retrospect, there are many unique features in the design of Hackett (1993, 1994). Both the production function and the sunk cost function are non-linear, while most later studies use either a linear function or a binary choice for these two decisions. Roles are reassigned at the beginning of every period, indicating that it is possible for the same subject to play both roles, while most later studies have fixed roles for the subjects for the whole session. Different from one-round simple bargaining mechanisms as employed in many later experiments, Rubinstein alternating offer procedure is implemented in Hackett (1993, 1994), allowing him to gather more information on the bargaining behavior and examine the effect of discount rate on the results by manipulating the maximum allowed bargaining rounds. In his “high discount rate” treatment, the maximum number of allowed bargaining round for some periods is fixed to be one, which is equivalent to a one-round ultimatum game as in many other studies.

Apart from the above three aspects, the most distinctive feature of Hackett (1993, 1994) is the inclusion of a veto option. The hold-up behavior on the buyer’s side could take two different forms: a disadvantageous allocation in the bargaining or choosing to opt-out before the bargaining stage starts. The veto option also increases the bargaining power of the seller.

A later experiment by Gantner et al. (2001) has a more standard experimental setting. Similar to Hackett (1993, 1994), two randomly matched subjects bargain over a joint surplus generated from their preceding investments. The joint production function is a linear combination of investments from both subjects. Subjects paired together differ in marginal productivity. This manipulation alters the bargaining balance between the two partners and makes room for different equity standards. The bargaining stage takes two forms: an ultimatum game and a Nash demand game. Each subject participates in both games with different partners. The two possible orders of combination are both included to exclude order effect. In the bargaining stage, subjects simultaneously select a demand and a lowest acceptance bound. The roles of a proposer and a responder are assigned to each subject after the values are chosen. In the ultimatum game setting, the demand of the proposer is matched with the lowest acceptance level of the responder. An agreement is reached if and only if the sum of the two values is less than the total surplus, resulting in an allocation that gives the demanded amount to the proposer and the residual to the responder. In the Nash demand game, the demand of both subjects is matched first. If the sum does not exceed the total gain, an agreement is reached and subjects receive their respective demand, with the efficiency loss of any unclaimed amount. Otherwise, their least acceptance bounds are matched and the surplus is distributed by the same method in the case of an agreement. Under both schemes, the disagreement payoff is zero for both subjects.

In the two experiments discussed above, both subjects invest and have the chance to propose an allocation, therefore, they both have an incentive to hold-up the partner and the possibility to become a potential victim to hold-up. In a number of more recent experiments, only one party makes the investment decision, while the other party proposes an allocation. Under this design, the incentive to hold up his counterpart only falls on the non-investor. This design fits the model by Hart (1995). Under this design, it is easier to distinguish the motives and behaviors of the two parties. A number of experiments with such unilateral investment stage still adopt bargaining mechanisms which assign symmetric bargaining power to both parties, *e.g.* the one-shot Nash demand game (Ellingsen and Johannesson, 2004a) and the multi-round alternating offer game (Sonnemans et al., 2001; Oosterbeek et al., 2003; Sloof et al., 2004; Davis and Leider, 2018). The bargaining procedure of Sonnemans et al. (2001), Oosterbeek et al. (2003), and Sloof et al. (2004) is similar to the Rubinstein procedure as applied by Hackett (1993, 1994), except that here, the maximum number of allowed rounds is fixed to ten and is known to all subjects. In each bargaining round, the responder decides whether to accept the offer and end the bargaining, or to reject with a counteroffer in the following round (if applicable) at the cost of receiving the disagreement payoff. In some treatments of Sonnemans et al. (2001) and Sloof et al. (2004), subjects also have the option to opt-out and end the bargaining stage in each round. The outside option of Sonnemans et al. (2001) leaves a positive amount to the investor and zero to the non-investor, while the contrary is the case for Sloof et al. (2004). Sonnemans et al. (2001) and Sloof et al. (2004) employ a special "block" structure in their experiments. They divide their multiple periods into blocks: Sonnemans et al. (2001) had two blocks of nine periods and Sloof et al. (2007) had four blocks of five periods. This "block" structure allows the experimenters to adopt perfect stranger matching within each block to avoid reputation effect and gives them room for testing within-subject treatments. Oosterbeek et al. (2003) adopt perfect stranger matching in all ten of their experimental periods. However, subjects participated as both roles: they were assigned one role for the first five periods and the other role for the remaining five periods. Davis and Leider (2018) adopt a unique dynamic bargaining mechanism, where both parties are allowed to make offers and provide limited feedback with few restrictions on the order and the number of offers can be made by either player.

The ultimatum game, where one player proposes an allocation and the other player decides whether to accept or to reject, can be regarded as an extreme case of the Rubinstein alternating offer bargain with only one round of offer. It is common in most recent hold-up experiments (Ellingsen and Johannesson, 2004b; Sloof et al., 2007; Hoppe and Schmitz, 2011; Morita and Servátka, 2013, 2018; Haruvy et al., 2019; Zheng et al., 2020) to adopt a unilateral investment with a binary investment choice followed by an ultimatum bargaining stage where the non-investor proposes an allocation and the investor decides whether to accept or to reject. Rejection leads to zero payoffs to both parties. Dufwenberg et al. (2013) use binary choices in the bargaining stage as well: instead of having to come up with his own allocation scheme, the non-investor chooses between an equal split and an option to "hold up" the investor by exploiting his payoff to almost zero. The investor then chooses between two allocations which are equivalent to accepting an unfair offer and punishing the unfair non-investor at his own cost in a standard ultimatum game.

Some experimental studies reduce the bargaining power of the investor by adopting a dictator game in the bargaining stage, so that the investor no longer has the possibility to reject an offer made by the non-investor. Such hold-up games with a dictator bargaining stage following the investment stage can be regarded as the trust game. The typical trust game was designed and conducted by Berg et al. (1995). The investor first decides a proportion of his show-up fee to be transferred to the non-investor, the amount of which will then be tripled by the experimenter. The non-investor then decides how much of the tripled amount to be sent back to the investor. Since the "investment stage" is explicitly framed as sending money to the partner, there is a stronger focus on willingness to trust and reciprocity in trust games. In addition, the incentive to hold up is stronger in this case, since the non-investor can propose any allocation without the fear of being rejected. Despite these differences, the trust game still constructs a hold-up situation. This experiment has been replicated by a large number of researchers as summarized in the meta study of Johnson and Mislin (2011). These subsequent studies closely follow the basic design of the Berg et al. (1995) experiment, with variations in minor setting details such as rate of return, location, subjects' demographic heterogeneity, etc. It is beyond the scope of this paper to provide an exhaustive overview of all trust game experiments. A number of typical studies that explore different mechanisms to increase the level of investment and transfer (Berg et al., 1995; Sloof et al., 2007; Charness and Dufwenberg, 2006, 2010; Charness et al., 2011; Huck et al., 2012; Eisenkopf and Nüesch, 2016; Ismayilov and Potters, 2016; Eisenkopf and Nüesch, 2017) are selected to compare different potential remedies for the hold-up problem.

2.2.2 Stylized findings

The hold-up experiments discussed above adopt different experimental settings. Some experimenters also introduce variations to test for a solution. Therefore, it is difficult to quantitatively compare the results. However, a qualitative comparison of the simple hold-up game in the baseline treatment of these experiments yield some general results that hold across different studies.

2.2.2.1 Evidence of hold-up

The surveyed studies all find evidence showing that the hold-up problem does exist. The most straightforward representation of hold-up lies in proposals made in the bargaining stage. In experiments where only one subject is allowed to propose an allocation, cases of exploitation are found in a number of studies. Dufwenberg et al. (2013) find that more than half of non-investors choose to exploit the investors by choosing the allocation that leaves only a minimum amount to the investor. Ellingsen and Johannesson (2004b), Berg et al. (1995), and Sloof et al. (2007) also find quite a few cases where the non-investor extracts all payoffs from the investor. Fehr and List (2004) find that on average non-investors only offer a small proportion of the total surplus to the investors. In experiments with multi-round bargaining, Oosterbeek et al. (2003) find that a majority of the first proposals by the non-investor leaves a less than the equal-split of the surplus to the investor, which indicates that non-investors take the chance to hold up the sunk investment of the investors. In experiments where both parties can propose an allocation, evidence of hold-up is also found on both sides. Ellingsen and Johannesson (2004a) find a number of cases where the claims from both parties add up to more than the total surplus to be divided. Gantner et al. (2001) also find a few allocations that fit the game-theoretic predictions.

For fear of being held-up by their partners, underinvestment on the investors' side occurs accordingly. In the studies reviewed, different degrees of underinvestment is found on the investors' side. Hackett (1993, 1994) finds that a substantial amount of subjects choose investment amounts below the efficient level. Gantner et al. (2001) also discover a few occurrences of low investment or even zero investment. Sonnemans et al. (2001) and Sloof et al. (2004) find that the average investment levels in all treatments are below the socially efficient level. In Sonnemans et al. (2001), the majority of individual investments fall below the efficient level. In experiments with binary investment choices, a substantial amount of investors refrain from investing in the first stage of the game (Dufwenberg et al., 2013; Oosterbeek et al., 2003; Ellingsen and Johannesson, 2004a,b; Sloof et al., 2007). In most trust games following Berg et al. (1995), quite a number of investors invest below the efficient level.

2.2.2.2 Discrepancy with standard self-interest predictions

In spite of the individual occurrences found in a number of experiments, on the aggregate level, the hold-up problem is found to be less of a concern than in theoretical models under standard self-interest assumptions. Underinvestment, although present, is found less severe than what standard self-interest theory predicts. Hackett (1993, 1994) finds that the average investment levels in the investing stage by both parties lie between the self-interest predictions and the socially optimal level, with a substantial portion of cases above the self-interest predictions. Gantner et al. (2001) find that the efficient investment level is selected in most cases. In experiments with binary investment choices (Oosterbeek et al., 2003; Ellingsen and Johannesson, 2004a,b; Sloof et al., 2007; Morita and Servátka, 2018; Zheng et al., 2020), underinvestment occurs less frequently than the self-interest predictions. Berg et al. (1995) and Fehr and List (2004) also find that a large proportion of investors trust their partners with the majority of their endowment.

In the bargaining stage, evidence also shows that exploitation of the investors is less severe than the standard self-interest prediction. In experiments where only one party is allowed to make an offer in the bargaining stage, namely the experiments using an ultimatum game (Gantner et al., 2001; Ellingsen and Johannesson, 2004b; Hoppe and Schmitz, 2011; Morita and Servátka, 2013, 2018; Haruvy et al., 2019; Zheng et al., 2020) or a dictator game (Berg et al., 1995; Fehr and List, 2004; Charness and Dufwenberg, 2006; Sloof et al., 2007; Charness and Dufwenberg, 2010; Charness et al., 2011; Huck et al., 2012; Eisenkopf and Nüesch, 2016; Ismayilov and Potters, 2016; Eisenkopf and Nüesch, 2017) as the bargaining mechanism, the proposer offers a positive amount to the partner under most circumstances. In the experiment by Ellingsen and Johannesson (2004b), the most common offer by the proposer is one that allocates equal net profit to both parties, chosen by almost 50 percent of the proposers. Consistent with the common robust results in pure ultimatum game or pure dictator game (Fehr and Schmidt, 2006), the majority of bargaining outcomes in these experiments exhibit a deviation from the standard self-interest prediction of full exploitation. In addition, similar results are also found in experiments with repeated alternate offers. Hackett (1993) finds an equal split of the total surplus to be the most frequent among all successfully negotiated final allocations, with other allocations clustered around it. Sonnemans et al. (2001) also find the finally agreed allocations to be different from the game-theoretic predictions with standard self-interest assumptions. Instead of an equal split of the total surplus, they find the average value of the final allocations closer to the "split-the-difference" result, which is defined as "both players receiving their no-trade pay-offs plus 50% of the remaining surplus".

A third discrepancy is found in the link between investment behavior and bargaining results. Various experimental results exhibit a close positive correlation between the investment level and the allocation results (Hackett, 1993, 1994; Gantner et al., 2001; Oosterbeek et al., 2003; Ellingsen and Johannesson, 2004a). Hackett (1993) finds that the difference in the sunk investment cost has a significant and positive effect on the bargaining outcomes. The bargaining outcome suggests the existence of an equity rule where the party investing the greater share receives the larger proportion of the surplus. Hackett (1994) discovers strong support for subjects adjusting their investment decisions after anticipating a linkage between observable investment levels and resulting allocations. In experiments with a Nash demand bargaining stage, the Nash cooperative bargaining solution predicts the unique equilibrium outcome of an equal split of the surplus, regardless of what happens in the investment stage. However, Gantner et al. (2001) find the share of surplus that subjects claim in the bargaining stage to be positively correlated with their share of input in the investment stage. The results of Ellingsen and Johannesson (2004a) also indicate that subjects take into consideration the sunk cost of the investors during the bargaining stage.

The discrepancy with standard self-interest predictions in many of the experiments above indicates that subjects' decisions in hold-up games are influenced by more than just pure strategic concerns. Various theories taking into account social preferences of the subjects can offer a better explanation for the observations. Researchers fit their experimental results with predictions from standard models and social preference models. They typically find that various selected social preference models better explain the results (Berg et al., 1995; Gantner et al., 2001; Fehr and List, 2004; Sloof et al., 2004; Dufwenberg et al., 2013; Morita and Servátka, 2018).

Some of the patterns discovered in the bargaining stage of hold-up experiments are similar to findings in respective simple bargaining games. In studies of simple ultimatum games, there is a common trend that responders reject strictly positive offers, and offers made by the proposers are clustered around the equal split and skewed to the left (Fehr and Schmidt, 1999), which is against standard theoretical predictions based on material payoffs. Similar results are found in hold-up experiments with an ultimatum bargaining stage. Ellingsen and Johannesson (2004b) and Sloof et al. (2007) both find that subjects reject unfair positive material offers. On the other hand, some findings are particular to hold-up experiments due to the distinct settings. For instance, Ellingsen and Johannesson (2004b) show that instead of an equal split of total surplus, the most common offer is an equal split of the net surplus, indicating that the proposer takes into account not only the final payoff but also the sunk investment cost of the investor.

Though predictions from models of social preferences fit many experimental findings, the specific mechanism through which social preferences work still remains to be identified. Various models are selected to fit the data from different experiments. Dufwenberg et al. (2013) use their intention-based reciprocity model (Dufwenberg and Kirchsteiger, 2004) to explain investors selecting an allocation to punish the non-investor at their own costs. Gantner et al. (2001), Ellingsen and Johannesson (2004b), and Sloof et al. (2004) show that different variations from the inequality aversion model (Fehr and Schmidt, 1999) can explain the behaviors observed in their experiments. Fehr et al. (2008) find that their results largely depend on the individual heterogeneity in the degree of fair-mindedness among subjects. They claim that the behaviors they observed are results of the interaction between self-interested and fair subjects. Zheng et al. (2020) decompose the hold-up game and provide supporting evidence for the effect of reciprocity. These experimental results provide further support for the advantage of various social preference models over self-interest material payoff models in explaining people's behavior in hold-up situations.

In addition to individual heterogeneity in the level of fair-mindedness, differences also arise in equity standards that individuals apply to their decisions. Individuals may have different understandings of what constitutes a fair allocation. Ellingsen and Johannesson (2004a,b) find that despite an equal split of the net surplus being the most common allocation, some subjects are found to demand an equal split of the total surplus, which leads to disagreement in a number of cases. By introducing asymmetric marginal contribution rate, Gantner et al. (2001) find three different equity standards among subjects whose decisions could be regarded as "equitable". In addition to whether it is the net surplus or the total surplus to be divided, subjects also disagree on whether their contribution should be in line with their productivity, whether the surplus should be divided in proportion to their contribution, etc. Differences in equity standards create additional obstacles to reach an agreement in the bargaining stage, thus undermining the effect of social preferences on mitigating the hold-up problem. Furthermore, it also creates some moral wiggle room which a self-interested subject can exploit by choosing the equity standard to his own advantage.

2.2.2.3 Summary

Results from various hold-up experiments provide evidence for the existence of the hold-up problem, as well as a general pattern indicating that the problem is less severe than standard theoretical predictions with pure monetary payoff concerns. The few cases of complete hold-up occur only on the individual level in most experimental studies, while on the aggregate level, the investment level lies in between the self-interest strategic prediction and the socially optimal level. Most subjects take into account the link between investment decisions of both subjects and the allocation results. Models of social preferences explain the findings better than models of self-interest. Social preferences open a new channel that mitigates the hold-up problem. However, the detailed mechanism still remains to be understood. Individual heterogeneity in social preferences and equity standards may undermine the effect of social preferences.

2.3 Remedies for the hold-up problem

Miller (2011) classifies remedies for the hold-up problem into two categories: formal remedies and informal remedies,[4] depending on whether it requires formal changes in the institutional environment or not. Che and Sákovics (2008) further classifies the formal remedies into two categories: organizational remedies, such as vertical integration as proposed by Klein et al. (1978), and contractual remedies, such as contracting on the allocation before the investment decision is made. A small number of experimental studies examine the effectiveness of various formal remedies (Fehr et al., 2008; Hoppe and Schmitz, 2011; Eisenkopf and Nüesch, 2016, 2017; Davis and Leider, 2018). On the other hand, a growing number of experiments provide evidence for how various behavioral methods, such as changing the level of the outside option (Sonnemans et al., 2001; Oosterbeek et al., 2003; Sloof et al., 2004; Morita and Servátka, 2018), the observability of investment decisions (Hackett, 1993; Sloof et al., 2007), veto power and punishment possibilities (Hackett, 1993, 1994; Dufwenberg et al., 2013; Fehr and List, 2004; Zheng et al., 2020),

[4]Miller (2011) adopts the terminology from the management literature and uses terms such as “formal controls” and “informal controls”.

communication (Ellingsen and Johannesson, 2004a,b; Charness and Dufwenberg, 2006, 2010; Vanberg, 2008; Ismayilov and Potters, 2016), information of past behaviors (Berg et al., 1995; Charness et al., 2011; Huck et al., 2012; Haruvy et al., 2019; Zheng et al., 2020), group identity (Morita and Servátka, 2013), etc., play an important role in mitigating the hold-up problem.

2.3.1 Formal remedies

2.3.1.1 Organizational remedies

As argued by Hart (1995) and Grossman and Hart (1986), changes in the organizational structure can help mitigate the hold-up problem. They argue that joint-ownership gives most room for underinvestment incentives to take effect, since both parties have the chance to hold up their partners in post-production renegotiations. On the contrary, single-party ownership solves this problem since the incentive is not eroded by allocating the ownership to only one party.

Fehr et al. (2008) conduct several experiments to examine the effect of different ownership schemes on subjects' investment behaviors. Their basic experimental setting adds an additional ownership-setting stage prior to the two-party joint-investment hold-up game. The ownership scheme determines how the joint surplus from investment is allocated. Joint ownership is associated with an equal split of the surplus, while with single ownership, the owner receives all the surplus at the cost of a fixed fee which is paid to his partner in order to obtain ownership. Subjects start with an initial ownership scheme, where one subject has the option to change the ownership scheme by selling his share of the surplus in exchange for a fixed fee. Two different ownership scenarios are tested in the experiment: 1) Subjects start with single ownership and the owner decides whether to give half of the ownership to his partner or to retain single ownership and pay a fixed fee to his partner. 2) Subjects start with joint ownership and one subject decides whether to sell his share for a fixed price or to retain joint ownership. In the investment stage, subjects sequentially choose the investment level, with the subject who does not have the option to switch ownership schemes moves first. The final surplus generated by the investments is then divided according to their agreed ownership scheme in the first stage.

Fehr et al. (2008) find that joint ownership is most frequently selected by the subjects, even though they start from different initial ownership schemes. In the joint initial ownership treatment, the majority of the subjects choose to maintain the initial ownership scheme. Among those who offer to purchase the ownership of their partners, the offer is rejected in 30% of the cases. In the single initial ownership treatment, the majority of initial owners offer joint ownership to their partners. Under joint ownership, the investment behaviors are the same no matter what initial ownership scheme they start from. There is a strong positive relationship between the investment level of the first mover and the second mover, indicating a reciprocal pattern. The efficient investment level is chosen by most of the first movers, and the second mover responds by choosing the efficient level as well, while only a small proportion of second movers exploit the first mover by choosing the minimum investment level. Fehr et al. (2008) find that in spite of different initial conditions, joint-ownership prevails as the most selected ownership structure and efficient investment is achieved in most cases under joint ownership, which is contrary to the predictions by Hart (1995) and Grossman and Hart (1986). Nevertheless, they provide evidence that changing the ownership structure indeed affects investment incentives and thus provides a solution to the hold-up problem. However, it is far from sufficient to make a decisive claim of joint ownership being the most efficient ownership structure. Fehr et al. (2008)'s findings are only valid when the number of partners is small, or if the free-rider problem is not too severe, and if there is no other way of contracting on the relationship-specific investment. The effectiveness of joint ownership can be partially attributed to different fairness concerns among subjects, but the effect is only limited to cases with less strong free-rider incentives.

Eisenkopf and Nüesch (2016, 2017) conduct experiments to test whether third-party ownership can mitigate the hold-up problem. In a Berg et al. (1995) trust game, they introduce a third party after the investment stage to decide the allocation of the investment gains instead of the non-investor. They compare various selection mechanisms of the third party, varying the degree of independence of the third party: 1) The computer randomly selects a third party who receives a fixed payment (Eisenkopf and Nüesch, 2016, 2017). 2) The non-investor chooses a third party whose payment increases in the number of times being selected among all unidentified third parties (Eisenkopf and Nüesch, 2017). 3) The non-investor chooses a third party who receives a fixed payment based on non-binding messages sent by third parties prior to the trust game (Eisenkopf and Nüesch, 2016). 4) The non-investor chooses a third party whose payment increases in the number of times being selected, based on non-binding messages sent by the third parties prior to the trust game (Eisenkopf and Nüesch, 2016). 5) The non-investor chooses a third party whose payment increases in the number of times being selected with a fixed identification number to induce a one-sided reputation (Eisenkopf and Nüesch, 2017). Among all selection mechanisms, the selection of the third party is exogenous to the investor, while the non-investor has various degrees of information about the third party.

Compared with random selection, the selection mechanisms in the latter four treatments may induce various degrees of potential bias towards the non-investor and thus render the third party less independent. This in turn affects the investor's trust of the third party. This is reflected in the proportion of back transfers and the level of investment. Randomly selected third parties are found to transfer back the highest amount for a given level of investment and induce the highest level of investment from the investors in both studies (Eisenkopf and Nüesch, 2016, 2017). When the non-investor selects the third party without identifying information, the proportion of back transfers is slightly lower than that with random selection, while the level of investment is similar (Eisenkopf and Nüesch, 2017). When the third party can communicate with the non-investor via a non-binding pre-play message but receives a fixed payment, the proportion of back transfers is only marginally higher than that in the baseline two-party hold-up treatment, while the investment level is no improvement from the baseline. However, if the payment of the third party depends on whether the third party is selected, the proportion of back transfers is similar to that in the two-party baseline, while the level of investment is lower than the two-party baseline (Eisenkopf and Nüesch, 2016). When the third-party can build a one-sided reputation with the non-investor, both the proportion of back transfers and the investment level are lower than that in the two-party baseline (Eisenkopf and Nüesch, 2017). These results indicate that transferring the allocation right to a third party with the appropriate level of independence can mitigate the hold-up problem, although the effect only takes place after some positive experience. On the other hand, competition among the third parties for the chance of being selected and one-sided reputation with the non-investor offsets this effect and exacerbates the hold-up problem. Eisenkopf and Nüesch (2017) further investigate the selection mechanism of the third party by conducting an additional experiment where the non-investor is given the option to endogenously choose whether to delegate the allocation right to a third party. The results do not differ when the delegation choice is made endogenously and exogenously.

2.3.1.2 Contractual remedies

The source of the hold-up problem lies in the fact that investments are non-contractable in the first place, and thus both parties may have insufficient incentives to invest the efficient level. A natural solution would be to contract on the allocation of the surplus prior to the investment decision. Whether contracts can effectively mitigate the hold-up problem is widely debated in the economic literature. Maskin and Moore (1999) initiate the argument that contracts can solve the hold-up problem. Nöldeke and Schmidt (1995) discuss the possibility of using an option contract to solve the hold-up problem. Rogerson (1992) also discusses the contractual solution to the hold-up problem. According to Hart (1995), contractual solution is effective only in two limited cases: either when the widget type can be described in advance, or when the investment can be verified.

Hoppe and Schmitz (2011) add an additional contracting stage before a typical one-shot hold-up experiment with a single-party investing stage and an ultimatum bargaining stage to compare the effectiveness of different contractual arrangements. The results from the baseline treatment fit the general stylized findings of hold-up experiments: around 40% of investors already choose the high investment. Hoppe and Schmitz (2011) examines three different contractual treatments: a fixed-price contract where the non-investor pays a fixed price to the investor, an option contract where the non-investor has the option to pay the same fixed price but can decide whether to exercise the contract or not, and an option contract with renegotiation where the non-investor can make another offer should he choose not to exercise the option contract. According to theoretical predictions, only the option contract can ensure sufficient investment incentives. The findings of Hoppe and Schmitz (2011) are consistent with the predictions. The fixed-price contract does not induce higher investments than the baseline no-contract treatment. The option contract significantly increases the investment incentive. In the option contract treatment, all contracts were accepted and around 90% of investors choose the high investment. Allowing renegotiation undermines the effect of an option contract. In the treatment of renegotiable option contract, the frequency of high investment is lower than that in non-renegotiable option contract treatment. However, the investment level is still higher than that in both fixed-price contract treatment and the baseline treatment. Hoppe and Schmitz (2011)'s findings indicate that an option contract significantly improves investment incentives and can effectively mitigate the hold-up problem. Allowing for renegotiation undermines the effect, but not as severely as theory predicts.

Davis and Leider (2018) also provide supporting evidence for the effectiveness of an option contract. They conduct a similar experiment to compare a wholesale price contract, a quantity premium contract, an option contract, and a service-level agreement in a hold-up game with random demand and a sophisticated dynamic bargaining procedure. An option contract and a service-level agreement are found to be the most efficient in increasing the investment level. They also find an indication of "superficial fairness" in their unique bargaining procedure consisting of an unstructured offer process and a structured communication process. The negotiated price often falls in the middle of the contracting space.

2.3.1.3 Summary

The number of experimental studies examining the effectiveness of formal remedies is limited. Contrary to standard theoretical predictions, joint ownership is found to be the most efficient ownership structure that significantly mitigates the hold-up problem. An independent third party can help increase both the investment level and transfers to the investor. An option contract on the post-investment allocation most effectively solves the hold-up problem. Renegotiation undermines the effectiveness of that contractual agreement but does not cancel it altogether.

2.3.2 Behavioral remedies

2.3.2.1 Outside options

Che and Sákovics (2008) remark that the effects of organizational remedies may depend on the bargaining solution. Changes in the outside options of a hold-up game can have a large effect on the incentives for the investor and the non-investor, and thus alter the bargaining positions. The Outside Option Principle (Binmore et al., 1989) indicates that a binding outside option has a stronger effect on the incentive of the investor, and can even induce the efficient level of investment. When one party is made residual claimant of the surplus, he then has the incentive to invest. On the other hand, the hold-up problem occurs when the non-investor's outside option is non-binding. In a series of experiments, Sonnemans et al. (2001), Sloof et al. (2004), and Oosterbeek et al. (2003) examine different levels of the outside option. Sonnemans et al. (2001) find that the relationship between the level of the outside option and the investment level depends on whether opting out is possible. When opting out is allowed, investment levels decrease as the value of the outside option increases; while when opting out is not available, the average investment level increases with the outside option. Sloof et al. (2004) and Oosterbeek et al. (2003) find little effect of different levels of the outside option. The overall investment level is constant over different values of the non-investors' outside option. More opportunism and lower investments are found when the non-investors' outside option is high and binding, while the hold-up problem is less severe when the non-investor's outside option is low and non-binding.

Morita and Servátka (2018) complements previous studies on the outside option by examining the effect of investing in outside options as a form of ex-post opportunistic behavior (Klein et al., 1978). They find that when the investor invests in outside options, the size of the outside option decreases the non-investor's offers; when the investor does not invest in outside options, the non-investor's offers increase with the size of the outside option. When the outside option is high, investing in outside options is regarded as opportunistic by the non-investor and can thus crowd out the non-investor's other-regarding preferences.

2.3.2.2 Observability of investment

Standard theory with self-interest assumptions predicts that the hold-up problem can be alleviated by making specific investment unobservable, since private information can create an informational rent that boosts investment incentives. Sloof et al. (2007) conduct an experiment altering the observability of the first-stage investment to examine this. They also include changes in the cost of investment to test for the interplay of the two effects. They show that the effect of investment observability depends on the level of the sunk investment cost. Making investment unobservable can mitigate the hold-up problem when the cost of investment is high, while observability does not play a significant role when the cost is relatively low. When investment costs are high or intermediate, unobservable investment leads to higher investment levels. When investment costs are low, information condition does not significantly alter investment levels. The average investment levels are close to the predictions of standard economic theory with self-interest assumptions. When investment is observable, after observing the investment choice not selected, non-investors always demand full exploitation. After observing investment, non-investors leave room to cover the investment cost of the investor. The results suggest that private information may partially crowd out the positive investment incentive effect of fairness and reciprocity motivations. Making investments unobservable also makes it difficult to determine whether the investor is being fair or not. As a result, unobservable investments interfere with the social preference effects, and undermines the effect of social preferences. Unobservable investments boost investments only when the costs of investment are relatively high, and thus there is insufficient scope for social preferences, while making investments observable might be a better remedy for the hold-up problem under stronger effect of social preferences.

In one treatment of Hackett (1993), he explicitly tests whether making observable investment is indeed an effective solution. Both players are informed of the investment decision by himself as well as by the partner. He finds that final bargaining allocation tends to be in line with ex-ante investment, especially in the treatment where information about investment is made observable to the players. Hackett (1994) provides more supporting evidence that subjects in the treatment when investment is observable invest ten to eight percentage points more than subjects in treatment when investment is unobservable. With observable investments, subjects observe whether their counterparts have made a sufficiently "fair" investment, and reciprocate with respective "fair" allocations. Hackett (1993, 1994) also provide evidence that the treatment effect is larger when the investment cost is high, which is consistent with the findings of Sloof et al. (2007). The above experimental evidence indicates that the effectiveness of making investments common knowledge as a remedy for the hold-up problem largely depends on whether social preference is strong enough. When the investment cost is low, it is more likely for social preferences to take effect. Therefore, making investments observable reinforces the effect of social preferences and can effectively increase investment incentives. On the other hand, when the investment cost is high, there is limited room for social preferences. In this case, making the investments unobservable can better alleviate the hold-up problem.

2.3.2.3 Veto power and punishment

The possibility for the responding party to reject a disadvantageous proposal can work as a tool of punishment to prevent the proposing party from exploiting with a low offer. The possibility to veto greatly changes the bargaining structure. The trust game can be regarded as a hold-up game with a veto-free bargaining stage. Zheng et al. (2020) isolate the hold-up game into a trust game which captures the effect of reciprocity and an ultimatum game which captures the effect of veto power. They separate the subjects and let them play one of these two games prior to playing the complete hold-up game with an ultimatum bargaining stage. The investors are given information of their paired non-investors' choices in the first game before they make the investment decision in the hold-up game. They find strong evidence for the effect of reciprocity. On the contrary, they did not find sufficient supporting evidence for the effect of veto power.

Hackett (1993, 1994) adds an additional veto option after the bargaining process, where subjects can choose whether to veto the bargaining results and receive zero payoffs instead. In his experiment, both players have the incentive to hold up. The incentive for the buyer is stronger since the no trade payoff of the bargain allocates all the gain from investment to the buyer and all the cost to the seller. Therefore, the buyer has an incentive to reject the proposals of the seller and receive the no-trade payoff. Veto on the sellers' side gives credible threat when no agreement is reached or when the buyers choose not to bargain, but also acts as a non-binding threat when an agreement is reached.

Experiments on social preferences show that people are willing to punish others by sacrificing their own payoffs (Fehr and Schmidt, 2006). In one treatment of Dufwenberg et al. (2013), after the non-investor not choosing the equal allocation, the investor is given an option to reduce the payoff of the non-investor to a large extent at the cost of losing all of his own payoffs. The possibility of costly punishment largely increases the number of investors who choose to invest in the first stage, as compared to that in a control treatment without punishment opportunity. Compared with a 100% hold-up rate in the no-punishment control treatment, punishment significantly reduces the hold-up rate to 50%. Costly punishment effectively stops a proportion of hold-up behaviors and as a result restores investment incentives. Similarly, Fehr and List (2004) allow the investor to impose a fixed fine on the non-investor if the payback amount is lower than a "desired back-transfer" amount as announced by the investor. They also find that non-investors transfer back a higher amount of money to the investors when the punishment option is available. Moreover, within the punishment treatment, the amount sent back is higher when the punishment option is not enacted. Their results suggest that the existence of a costly punishment option can prevent hold-up behaviors, even though it is a non-binding threat and the investor chooses not to exercise it.

2.3.2.4 Communication

Ellingsen and Johannesson (2004a,b) conduct a series of experiments to test the effectiveness of cheap talk communication in mitigating the hold-up problem. They find that non-binding messages are indeed effective in mitigating the hold-up problem. Ellingsen and Johannesson (2004b) use an ultimatum game in the bargaining stage, where communication works as a non-binding promise or threat, while Ellingsen and Johannesson (2004a) use the Nash demand game as the bargaining stage, where communication works as a coordination device. In both studies, they allow either the investor or the non-investor to send a free-form message. The investor sends a message to the non-investor simultaneously as he makes the investment decision, which can be viewed as a threat. The non-investor sends a message before the investor makes the investment decision, which can be viewed as a promise. Ellingsen and Johannesson (2004b) find that both communication treatments increase the offers made by the non-investors to the investors, though the difference is insignificant, partly due to the small number of observations. They also find that the treatment with non-investor communication leads to the highest offers to the investors than the treatment with investor communication. Comparing the consistency between the message being sent and the actions taken by the party who sends the message, promises are shown to be more credible than threats, since none of the promises were violated, while less than half of the threats that explicitly states that any offers less than the denoted amount will be rejected are executed. Applying the inequity aversion model by Fehr and Schmidt (1999), they show that inequity aversion makes promises by the non-investors more credible and threats by the investors less credible. Ellingsen and Johannesson (2004a) find similar results. Communication increase the investment rate and the investor's profit. The rate of bargaining breakdowns is much lower when communication is allowed. Comparing the two studies, the increase in the rate of investment caused by communication is found to be higher in the Nash demand game (Ellingsen and Johannesson, 2004a) than the increase in Ultimatum game (Ellingsen and Johannesson, 2004b). Communication works better as a device to improve coordination. However, Ellingsen and Johannesson (2004a) also show that communication cannot fully eliminate the hold-up problem caused by coordination failure. There are still cases of investors refraining from investing in the investor-communication treatment. Together, these two experiments emphasise the role of communication in mitigating the hold-up problem.

A growing strand of experimental literature examines the mechanism through which communication, especially promises, increases the level of back transfer and investment in trust games.[5] In a modified trust game with risk following the setup of Charness and Dufwenberg (2006), the non-investor is allowed to send a pre-play free-form message to the investor. Ellingsen and Johannesson (2004b) argue that people have a preference for promise-keeping, which fosters commitment to fulfill promises. Charness and Dufwenberg (2006) suggest an alternative mechanism of guilt aversion. People fulfill promises to avoid the guilt of deviating from others' payoff expectations. Studies aiming to distinguish between the two different mechanisms provide conflicting results. Vanberg (2008) finds supporting evidence for the commitment-based preference for promise-keeping, instead of the expectation-based guilt aversion. Charness and Dufwenberg (2010), however, find limited evidence for both mechanisms. In a treatment where the message of the non-investor is not delivered, Ismayilov and Potters (2016) find non-investors who make a promise are more likely to practice trustworthy behaviors than those who do not make a promise, which is in line with the commitment-based "internal consistency" explanation. However, Ismayilov and Potters (2016) fail to establish causality between promises and trustworthiness, and suggest a self-selection effect of communication such that more trustworthy non-investors are also more likely to send promises. In a trust game without communication, Ellingsen et al. (2010) fail to establish correlation between subjects' elicited beliefs and more trusting/trustworthy behaviors, and Kawagoe and Narita (2014) find similar results in a trust game with pre-play communication, providing evidence against the expectation-based guilt aversion hypothesis. On the other hand, by exogenously varying the probability of the trustor keeping the promise, Ederer

[5] The level of back transfer is often used as a proxy for trustworthiness while the level of investment for trust in this literature.

and Stremitzer (2017) provide supporting evidence for the expectation-based guilt aversion argument. In a survey paper, Cartwright (2019) compares the experimental evidence from various studies on the expectation-based guilt aversion with those on the commitment-based argument.[6] He argues that the difference in the second-order beliefs elicitation approaches adopted in different studies renders it difficult to disentangle the two models. In a recent study, Di Bartolomeo et al. (2019) exogenously vary both promises and beliefs to compare the two approaches. Their results provide little support for the expectation-based argument.

2.3.2.5 Past behaviors

Reputation of past behaviors in the game can affect subjects' expectation of what decisions the partner will make, and thus is believed to have an important effect in various strategic interactions (Schelling, 1960). Charness et al. (2011) investigate the effect of two types of reputation schemes of the non-investor: information of past behaviors in the same role (non-investor) or in the opposite role (investor). Even though only reputation in the same role is found to effectively induce the non-investor to select the trustworthy allocation, both reputation schemes have a similar positive effect on investment decisions of the investor. The reputation effect of the non-investor acting as an investor in the past indicates the role of "indirect reciprocity". Huck et al. (2012) examine the interplay of competition and different degree of reputation on mitigating the hold-up problem. They find limited effect of reputation: reputation of identifiable non-investors can partially increase investment decisions compared with a no-reputation baseline, but the investment rate is still below the first-best; offering information of all past decisions of all non-investors does not lead to more investment decisions than limited information. However, after introducing competition, only the reputation of identifiable non-investors is sufficient to foster efficient first-best investment. Haruvy et al. (2019) finding supporting evidence for reputation of the non-investor leads to higher investment in a hold-up game with uncertainty and information asymmetry. Zheng et al. (2020) provide the investor with information of the non-investor' decision in a prior trust game or ultimatum game. They find strong supporting evidence for information of non-investor's previous trust game decisions leading to lower underinvestment, indicating the effect of reciprocity.

Economic models have shown that people get disutility from deviation from the behaviors of most other people (Bénabou and Tirole, 2006). Berg et al. (1995) show that a social norm of trust and cooperation can help initiate more trusting and cooperative behaviors from individuals, and thus mitigates the hold-up problem. In

[6] Cartwright (2019) refers to the expectation-based guilt aversion as "belief-based model of guilt aversion", and the commitment-based model as "reference-based model of guilt aversion".

one treatment, subjects are given a summary of behaviors from subjects that have participated in previous sessions of the experiment without the social treatment. For each possible amount to be sent, the report details the frequency of every amount, as well as the average payback amount and net return. They find that after the social history treatment is introduced, there is a slight increase in the frequency of equal amount sent, as well as an increase in the frequency of higher amounts to be sent back by the non-investor. Although isolated, their findings suggest that social history tends to reinforce the effects of trust and reciprocity in mitigating the hold-up problem.

2.3.2.6 Group Identity

Morita and Servátka (2013) argue that group identity reinforces each member's altruism towards other group members, and thus can lead to higher back transfers and higher investments. In their experiment, subjects are randomly divided into two teams, with team uniforms and a trivia question task to strengthen group identity. Each subject plays the hold-up game with either a member of his own team or of the other team. They find a higher investment rate and average back transfers in the same-team treatment than in the different-team treatment.

2.3.3 Summary

Experimental evidence has shown that the hold-up problem is less serious than standard theoretical predictions with self-regarding preferences of only monetary payoffs, mostly due to the fact that individuals take into consideration the payoff of their counterparts and will reciprocate in response to their counterparts' behaviors. Methods that enhance this channel can effectively alleviate the hold-up problem. Making investments observable, giving subjects an option to veto "unfair" behaviors, allowing pre-play communication before any decision is made, providing information of past behaviors, and fostering a sense of group identity all prove to significantly improve investment incentives. Individuals' behaviors in hold-up situations are also influenced by social history and social norms.

2.4 Conclusion

Being a nested bargaining game, experiments on the hold-up problem are more complicated and less commonly conducted than other simple bargaining games. This paper reviews the experimental literature on hold-up. Experiments on the hold-up problem take various forms, resulting in different representations of the problem. Though they differ in various aspects, these experiments share the same essential game structure as well as some common patterns in their results. Individual occurrences of hold-up and underinvestment have been discovered, whereas on the aggregate level the problem is less severe than what standard self-interest theory predicts. The overall behavior lies in between the social optimum and the self-interest strategic equilibrium. Subjects' behaviors are believed to be strongly influenced by other-regarding preferences and reciprocity. A positive correlation between investment decision and bargaining results is discovered in many experiments. Individuals are found to take into consideration the investment level of their counterparts and incorporate it in their bargaining behaviors. On average, the experimental results on the hold-up problem are strongly influenced by social preferences and thus exhibits an overall pattern that is less bleak than predicted by theory.

Different variations are introduced in the experiments to examine how different types of remedies can mitigate the hold-up problem. Experimental results provide some evidence for contractual solutions and organizational solutions. Joint ownership is shown to be the most efficient ownership structure, which is contrary to theoretical results. Ownership of an independent third party is found to effectively mitigate the hold-up problem. An option contract is found to significantly boost investment incentives both when renegotiation is forbidden and allowed. However, due to the limited number of studies, further research needs to be done to study the effectiveness of these conventional remedies as well as their interplay with social preferences.

On the other hand, a number of experiments focus on behavioral remedies. The hold-up game is essentially a bargaining game embedded in an investment game. Results from other simple bargaining games can also be applied to the bargaining stage of the hold-up game. Methods that are shown to instigate or to reinforce a "fair" allocation in other experiments on ultimatum games, dictator games, trust games, etc. can also be applied to the hold-up game. Changes in experimental settings such as making investment decision observable, the potential threat of veto power, cheap talk, reputation, and group identity are found to effectively mitigate the hold-up problem. These remedies work through various aspects of social preferences. However, further study is needed to better understand how social preferences can affect individuals' behaviors in the hold-up problem. For example, conflicting evidence has been found regarding the mechanisms of how promises lead to higher back transfers in trust games. Various models of social preferences have been fitted with experimental data, but there is not conclusive evidence about the mechanism through which social preferences actually alters individuals' behavior. In addition, individual heterogeneity in the degree of social preference and the judgment of fair allocations makes it more complicated to examine the social preference channel. The interplay of these individual heterogeneities creates a moral wiggle room which less prosocial individuals can exploit. Further research on the effectiveness of social preferences in hold-up situations can focus more on these heterogeneities.

2.5 Discussion

Laboratory experiments have many obvious advantages in collecting data for research on the hold-up problem. They enable researchers to easily implement the institutional environment that is aligned with theory, and to insert strict controls that reduce confounding factors. Though experiments on the hold-up problem are limited in numbers, they provide important insights about individuals' behaviors in a hold-up situation. Most importantly, laboratory experiments make it easier to examine the treatment effect of a remedy for the hold-up problem. Various experimental results have provided important information to better understand the hold-up problem.

This being said, a few aspects of laboratory experiments raise concerns about the external validity of their results. The generalisability of applying laboratory results to the field is one of the most common criticisms of laboratory experiments. Laboratory experiments are mostly conducted with student subjects in a controlled environment, whose behaviors may differ from other economic players in real-world transactions from naturally occurring environments (Levitt and List, 2007). As a consequence, there have been questions about whether the results of laboratory experiments also pervade to similar situations in the real world. Among the hold-up experiments reviewed, Fehr and List (2004) address this issue by conducting the same trust game with both student subjects and CEOs from the coffee mill sector in Costa Rica. By comparing the results from the two different subject pools, they find that CEO subjects exert more trust and exhibit more trustworthiness than student subjects. On average, CEO investors invest a larger share than students, and CEO non-investors offer back a larger share than students for any given investment level. This suggests that the hold-up problem could be less serious among CEOs. Though the difference could be partly due to the fact that the stakes used in the experiment may be considered too small to the CEOs but not to the students, the result provides an implication that there may be a discrepancy between lab results and real-world observations for hold-up games. Therefore, it is worth taking extra caution when attempting to draw implications from laboratory experiment results.

In addition to different subjects, laboratory experiments differ from real life situations in various ways. The hold-up problem can take on many different forms under different settings. The complications in institution and environment may not be fully abstracted in the current simple laboratory experiment. One example where the hold-up problem has been alleged to be present can be found in the standardization process of Standard Setting Organizations (SSOs). When the candidate technology to be incorporated in the standard is patent protected, the hold-up problem takes the form of patent hold-up, where patent owners fail to impose royalty fees according to the FRAND terms and charge high royalty fees; or it can also take the form of so-called "patent ambush", in which patent owners withhold information about the patent in the standard setting process. Some results from the hold-up experiments in the laboratory could offer implications to the functioning of SSOs. The fair, reasonable, and non-discriminatory terms (FRAND) as is often imposed by most SSOs on members is an example. Members participating in the standard-setting process typically commit to charging license fees for their patents according to FRAND terms. The enforcement of FRAND terms reflects fairness concerns. Punishment of violating the FRAND terms can also work as a non-executed threat for the patent-holding parties to enforce FRAND terms. However, due to the heterogeneity of equity standards (Gantner et al., 2001), there could be different interpretations of the FRAND standard, which leads to difficulty in implementation. Different parties may have different interpretations of the FRAND pricing, which may still lead to some degree of patent hold-up. On the other hand, apart from different backgrounds of subjects, there are many more differences between actual SSOs and general laboratory hold-up experiments. First, in all laboratory experiments discussed above, each subject makes a decision individually in his or her own interest, while in the standard-setting process, or many other real-life firm decisions, choices are decided collectively by a group within a firm. In the standard-setting example, the decision right is often delegated to a certain group of officers. There is already a large amount of literature studying the difference between individual and group decision making in non-strategic individual decisions such as risk and ambiguity attitudes. They seem to suggest that individuals act as if less risk-averse and more ambiguity averse when they are part of a group than when they act individually (Brunette et al., 2015). Another common finding is that the degree of difference depends on the group decision rule. Studies comparing individual and group decision making on strategic social interaction games are quite limited in numbers. A recent study (Ambrus et al., 2009) also suggests that in a gift exchange game individuals act differently depending on the group decision rule. Whether social preferences prevail under group decision is essential to mitigating the hold-up problem. To study the individual-group decision difference in the hold-up game can offer implications for solving real-life problems such as patent hold-up. Similarly, the literature on delegation (Hamman et al., 2010; Bartling and Fischbacher, 2012) shows that due to responsibility shifting, "unfair" and "immoral" decisions are chosen more frequently under delegation. These studies focus on bargaining behavior in a dictator game. It is possible that delegation can also affect investment behavior. As the behavioral remedies for the hold-up problem work through the channel of social preferences, studying how delegation affects behaviors in both stages of a hold-up game can have an essential impact on finding a solution for the hold-up problem.

Another important difference lies in the decision process. In all experiments reviewed in this paper, subjects make decisions facing a computer screen or a paper questionnaire, without direct face-to-face interaction with their partners. Anonymity is strictly enforced in the laboratory. In addition, most experiments use a stranger setting, so that subjects interact with the same partner only once. A typical laboratory experiment usually takes no more than two hours, which limits the time span of the experiment. In the example of real business interactions, most interactions are conducted face-to-face through frequent meetings in as long as several months or even a few years. With face-to-face repeated interaction, it is possible for reputation or "self-image" effect to kick in, which may in turn strengthen the social preference channel. Therefore, whether hold-up in real-life interactions is similar to that in laboratory experiments remains an open issue.

The laboratory experiment is a simplified prototype of the real problem. This simplification is a double-edged sword that makes it possible for all the advantages of laboratory studies to be achieved, but on the other hand, also ignores some complications that could potentially induce different results. Current laboratory experiments provide important empirical evidence on subjects' behaviors and possible remedies of the hold-up problem with the basic structure. It is promising to continue adopting experimental methods for further understanding of the underlying mechanism behind the experimental evidence. Future experimental studies can gradually expand the scope by including variations that more closely resemble the real situation. For example, group decision and delegation could be introduced to the experimental process. In addition, a competitive environment could be introduced. Incorporating these variations will provide new evidence on the hold-up problem and its remedies under more "realistic" environments.

Chapter 3

Delegation with strategic complements and substitutes[1]

3.1 Introduction

Thomas Schelling was the first to point out that players may have incentives to use delegation as a strategic commitment device: "Just as it would be rational for a player to destroy his own rationality in certain game situations, ..., it may also be rational for a rational player to select irrational agents" (Schelling, 1960, p. 143). Other players' equilibrium strategies depend on a player's own best response function. A player can modify its best response function by delegating decisions to an agent and setting the agent's incentives. By appropriately doing so, a player can move other players' equilibrium strategies in a given direction.

This insight has been widely applied in theoretical models. A textbook example is quantity-setting oligopoly where owners have an incentive to make the managers' compensation depend, not just on profits, but also on revenues or sales (Vickers, 1985; Fershtman and Judd, 1987; Sklivas, 1987). Doing so induces the managers to compete more aggressively and set higher quantities than they would if compensation were based on profits only. Other applications of strategic delegation include R&D decisions (Kopel and Riegler, 2006), mergers (Ziss, 2001), location choice (Liang et al., 2011), corporate finance (Brander and Lewis, 1986), resource extraction (Ritz, 2008), organizational design (Vroom, 2006), political competition (Harstad, 2010), and climate policy (Habla and Winkler, 2018).[2] In all of these settings, principals have a strategic motive to provide agents with incentives that differ from the principals' own incentives.

An overarching insight emerging from this literature is that the direction in

[1]This chapter is adapted from joint work with Jan Potters.

[2]For a review of the literature on strategic delegation in industrial organization, see Kopel and Pezzino (2018); for applications in management, see Sengul et al. (2012).

which the agents' incentives are distorted depends on the type of strategic interaction. If a game is characterized by strategic complements principals are predicted to endow their agents with more cooperative payoffs than the principals' own payoffs; in the case the game is characterized by strategic substitutes principals will give their agents more competitive payoffs.[3] Miller and Pazgal (2001) provide an illustrative example. In a quantity-setting duopoly with substitutable products (strategic substitutes) a firm-owner benefits if the other firm chooses a low quantity and this may be achieved by giving her manager aggressive incentives, inducing the manager to choose a higher quantity than the firm-owner would have incentives to do herself. Conversely, in a price-setting duopoly with substitutable products (strategic complements) the firm-owner benefits if the other firm chooses a higher price and this can be achieved by giving her agent cooperative incentives, stimulating the agent to choose a higher price than the firm-owner would choose herself. Although the logic behind the predictions is compelling, it is certainly not trivial.

We explore these predictions by means of a laboratory experiment. We implement a four-player game between two principal-agent pairs. Each agent makes the decisions on behalf of his principal. Each principal determines the payoff function of her own agent by assigning a certain weight to the other principal's payoffs, as in Miller and Pazgal (2001). A positive weight implies cooperative (altruistic) incentives; a negative weight implies competitive (spiteful) incentives. We implement two treatments: one with strategic complements and one with strategic substitutes. This allows us to examine whether the principals distort the agents' payoffs away from the principals' own payoffs and whether, as predicted, the direction depends on the nature of the strategic interaction.

Empirical studies on strategic delegation are scarce. Aggarwal and Samwick (1999) find that managers' bonuses are more positively correlated with rivals' profits when the degree of competition is higher. This is consistent with the use of strategic delegation to soften competition in a price-setting oligopoly when products are closer substitutes. Kedia (2006) classifies industries into complement or substitute industries depending on whether firms' marginal profits are decreasing or increasing in rival firms' sales levels. She finds that executive compensation is less closely related to profits and more to sales in substitute than in complement industries. This is consistent with the prediction that executive delegation leads to more aggressive competition with substitutes than with complements. Bloomfield (2018) uses data on the executive compensation contracts and finds that the

[3] A closely related insight emerges from the literature on the evolution of preferences. Bester and Güth (1998) and Possajennikov (2000) show that altruistic (spiteful) preferences are evolutionary stable when the material payoff functions are characterized by strategic complements (substitutes).

prevalence of revenue-based incentives increases with industry concentration in Cournot industries. This effect arises only after the introduction of an executive compensation disclosure mandate and does not occur in Bertrand industries, which is consistent with the gist of the theoretical literature about strategic delegation. However, studies based on field data often face issues caused by the difficulty to measure the shape of the compensation contracts and the type of strategic interaction. There is no widely accepted method. Kedia (2006) measures strategic interaction using data on the change in profits and sales in relation to rival firms' profits and sales. Bloomfield (2018) uses four different measures. Moreover, empirical measures of strategic interaction and compensation are prone to potential endogeneity issues: executive compensation may be affected by the type of market interaction, but in turn may also shape this interaction.

An important advantage of experiments is that the nature of the interaction can be varied exogenously. Another advantage is that incentive contracts can be observed without noise. Huck et al. (2004) were the first to study strategic delegation in the laboratory. They implement quantity-setting duopoly experiments and find that owner-principals typically align the payoffs of the manager-agents with the principals' own payoffs; that is, principals do not choose more competitive (aggressive) incentives for their agents, as would be predicted by the models of Vickers (1985), Fershtman and Judd (1987), and Sklivas (1987). On the other hand, evidence supporting these theoretical predictions is reported in the experimental study by Barreda-Tarrazona et al. (2016) who find that principals induce more aggressive behavior by inversely relating their agents' compensation to competitors' profits. A major difference between our study and Huck et al. (2004) and Barreda-Tarrazona et al. (2016) is that we do not only implement a setting with strategic substitutes but also one with strategic complements.[4]

Our results indicate that in a majority of the cases principals distort their agents' incentives, both with strategic complements and with strategic substitutes. As predicted, distortions in the direction of competitiveness are more frequent with strategic substitutes (73.6%) than with strategic complements (59.4%). Contrary to the prediction, however, with strategic complements principals also set competitive incentives more frequently than cooperative incentives. Upon closer inspection we find that this may be explained by the behavior of the agents which is broadly in line with subgame-perfect equilibrium predictions in the Substitutes treatment

[4]There also exists a small strand of experimental literature on delegation in allocation games. Fershtman and Gneezy (2001) study strategic delegation in an ultimatum game and find that both the proposer and the responder can benefit from using a delegate. Studies on delegated dictator games (Hamman et al., 2010; Coffman, 2011; Bartling and Fischbacher, 2012; Choy et al., 2016; Gawn and Innes, 2019b) show that principals may use a delegate to make unfair decisions on their behalf without feeling morally responsible for such unfairness. Responsibility for making unfair offers can be effectively shifted to a delegate, allowing punishment to be avoided.

but more cooperative than predicted in the Complements treatment. Given that agents behave cooperatively with complements the principals have no incentive to induce the agents to behave cooperatively. Principals even have an incentive to set slightly competitive incentives since the strategic effect of the incentives is weaker than predicted. Taken together, our results support the relevance of strategic delegation models, but also indicate that this support is more compelling for strategic substitutes than for strategic complements.

The rest of the paper is organized as follows. In Section 3.2 we introduce the theoretical model on which our experiment is based. Section 3.3 describes the experimental design. Section 3.4 presents the results. Section 3.5 discusses these results. Section 3.6 concludes the paper.

3.2 The Model

The model on which our experiments is based follows the basic set-up of Miller and Pazgal (2002) and Eaton (2004). We consider a two-stage game with four players: two principals (principal i and j) and two agents (agent i and j).[5] The principal i's payoff function takes the following form:

$$\pi_i = ax_i - bx_i^2 + cx_ix_j \tag{3.1}$$

with $x_i, x_j \geq 0$, $i, j = 1, 2$, and $i \neq j$. We impose the following restrictions: $\frac{\partial \pi_i}{\partial x_i} = a - 2bx_i + cx_j > 0$ for all x_i, x_j, $\frac{\partial^2 \pi_i}{\partial x_i^2} = -2b < 0$, $a > 0$, and $b > |c|$. The strategic environment of the game is represented by the sign of c. In case $c > 0$, we have $\frac{\partial^2 \pi_i}{\partial x_i \partial x_j} > 0$, indicating that x_i and x_j are strategic complements. In case $c < 0$, we have $\frac{\partial^2 \pi_i}{\partial x_i \partial x_j} < 0$, indicating that x_i and x_j are strategic substitutes.

Principals delegate the choice of x_i to their respective agents. Each agent's payoff is a weighted sum of the payoff of his own principal and the payoff of the other principal:

$$G_i = \lambda_i \pi_i + (1 - \lambda_i)\pi_j \tag{3.2}$$

The specification of the agent's payoff function follows Miller and Pazgal (2002). This captures the idea that the agent takes into consideration of the payoff of both the own principal and the rival principal.[6] The weight λ_i is set by principal i. It is essentially a decision to select an agent with specific (social) preferences over the payoffs of the two principals. If a principal sets $\lambda_i = 1$, she selects an agent whose

[5]For ease of distinction, we will use feminine pronouns for the principals, and masculine pronouns for the agents.

[6]Although we don't expect to observe managerial compensation contracts that literally correspond to (2), there is some evidence that firms in imperfectly competitive markets get close to them. See Aggarwal and Samwick (1999).

payoff is perfectly aligned with her own payoff. With $\lambda_i > 1$, a principal selects an agent who places a high weight on her own payoff and a negative weight on the other principal's payoff. We call such preferences "competitive". If the principal sets $\lambda_i < 1$, she selects an agent who places a positive weight on both her own payoffs and the other principal's payoff. We call such preferences "cooperative". As in Huck et al. (2004), we assume $\lambda \in [0, 2]$. Following previous theoretical literature on strategic delegation (Vickers, 1985; Fershtman and Judd, 1987; Sklivas, 1987; Miller and Pazgal, 2002), the agent's payoff is assumed not to be paid out of the principal's payoff, in order to focus on principal's incentive-setting motivations without possible cost considerations.

The game consists of two stages. In the first stage, the principals, simultaneously and independently, set λ_i and λ_j. In the second stage, being informed about λ_i and λ_j, the agents set x_i and x_j, simultaneously and independently. We use backward induction to solve for the subgame perfect equilibrium. In the second stage, given λ_i and λ_j, each agent i chooses x_i to maximize his payoff G_i. It is straightforward to show that this yields the following equilibrium:

$$x_i^*(\lambda_i, \lambda_j) = \frac{ac\lambda_j + 2ab\lambda_i\lambda_j}{4b^2\lambda_i\lambda_j - c^2}, \tag{3.3}$$

with $i, j = 1, 2$, $i \neq j$, and $4b^2\lambda_i\lambda_j - c^2 \neq 0$.

Substituting the equilibrium values for x_i and x_j, into the principals' payoff functions, yields the following unique subgame perfect equilibrium:[7]

$$\lambda_i^* = \lambda_j^* = 1 - \frac{c}{2b}. \tag{3.4}$$

Detailed derivations are provided in Appendix 3.7.1.

In case $c > 0$ (x_i and x_j are strategic complements), we have $\lambda^* < 1$; principals set cooperative incentives for their agents, assigning positive weight to the payoff of the other principal. In case $c < 0$ (x_i and x_j are strategic substitutes), we find $\lambda^* > 1$; principals set competitive incentives for their agents, assigning negative weight to the payoff of the other principal. It is these basic predictions that we aim to test in our experiment

The parameters we used for the experiment are: $a_{comp} = 8, b_{comp} = 1, c_{comp} = 0.8$ for the Complements treatment, and $a_{subs} = 40, b_{subs} = \frac{25}{9}, c_{subs} = -\frac{20}{9}$ for the Substitutes treatment.[8] These parameters satisfy a number of conditions which we deem desirable for a balanced comparison between the two treatments:

[7] $4b^2\lambda_i\lambda_j - c^2 \neq 0$, $b \neq 0$, $2b \neq c$ need to be satisfied for the existence of a unique pure strategy SPE.

[8] With these parameters, the restrictions specified in Footnote 7 are satisfied for $\lambda \in [0, 1]$ in the Complements treatment and $\lambda \in [1, 2]$ in the Substitutes treatment.

1. The equilibrium weights in the two treatments are equidistant from the neutral (no-delegation) case: $|\lambda^*_{comp} - 1| = |\lambda^*_{subs} - 1|$. Specifically, with our parameters we have $\lambda^*_{comp} = 0.6$ and $\lambda^*_{subs} = 1.4$.
2. The equilibrium payoffs for the principals are the same in the two treatments. With our parameters we have $\pi^*_{comp} = 67.2 = \pi^*_{subs}$.
3. The equilibrium payoffs for the agents are the same in the two treatments. With our parameters we have $G^*_{comp} = G^*_{subs} = 67.2$.[9]

3.3 Experimental design

The experiment was conducted in February and March 2017 at CentERlab, Tilburg University. We held five sessions with strategic complements (the Complements treatment) and five sessions with strategic substitutes (the Substitutes treatment). The number of participants in each session ranged between 12 to 24. The total number of subjects was 180. Each session lasted around 150 min. The average payment for each subject was 18.62 Euro in the Complements treatment and 19.75 Euro in the Substitutes treatment, including a 3-Euro show-up fee. The experiment was programmed using zTree (Fischbacher, 2007).

At the beginning of each session, instructions (see Appendix 3.7.3) were given to the subjects. The game was presented in a neutral frame. The λ choice of the principal was labeled as a "weight", and the x choice of the agent was called an "input"; the payoff to the principal was labeled as "earnings" and the payoff to the agent was labeled as "compensation". Subjects were randomly assigned to the roles of principals and agents after reading the instructions. The roles were fixed for the entire session.

Participants were informed about the payoffs in three different ways. They were informed about the payoff function, they were provided with a payoff matrix, and they were given access to a payoff calculator in which they could input possible values for the weights (λ_i, λ_j) and the inputs (x_i, x_j) and see the corresponding payoffs ("earnings" and "compensation"). The payoff matrix exhibited six possible values for the inputs. These six values corresponded to six benchmark outcomes of the two treatments.[10]

[9]Our set of parameters is not unique in satisfying these four conditions. To reduce arbitrariness we used the following procedure by Pazgal and Miller (2001) to relate the two treatments. Given a linear demand function (we use $q_i = 8 - p_i + 0.8p_j$) a game with strategic complements arises by taking prices as strategic variables (i.e., $x_i = p_i$, i = 1,2) and an equivalent game with strategic substitutes arises by taking quantities as strategic variables (i.e., $x_i = q_i$, i = 1,2).

[10]Let $x_i(\lambda_i, \lambda_j)$ denote the equilibrium value of x_i in the subgame with (λ_i, λ_j). The six benchmarks can then be defined as (1) $x_i(\lambda^*, \lambda^*)$, (2) $x_i(1, 1)$, (3) $x_i(\lambda^*, 1)$, (4) $x_i(1, \lambda^*)$, (5) $x_i(2 - \lambda^*, \lambda^*)$, and (6) $x_i(\lambda^*, 2 - \lambda^*)$. The latter two benchmarks refer to cases in which one of the principals set a value of λ that fits the other treatment. The actual values used in the matrix were slightly adjusted to retain similar distance between each other and were rounded to

The matching protocol was aimed at retaining the one-shot character of the game, while at the same time giving the subjects the possibility to learn. At the beginning of a session, each principal was randomly matched with an agent. A principal-agent pair remained together for three rounds. At the beginning of the three rounds, a principal chose λ from the interval $[0, 2]$. This λ was kept fixed for three rounds. This allowed the agent to gain some experience with a specific value of λ. After three rounds, a principal was re-matched with another agent, but such that a principal was not matched with the same agent more than once.[11]

In each round, a principal-agent pair was randomly matched with another principal-agent pair (with replacement). All four players were informed of the λ-pair set by the two principals. In a round, an agent then chose a value for x_i from $[0, 15]$ in the Complements treatment, and from $[0, 10]$ in the Substitutes treatment. Specifically, for each treatment we wanted the equilibrium values of x that correspond to the no-delegation benchmark ($\lambda = 1$) to be roughly in the middle of the strategy space.

At the end of each round, all four decision variables (weights and inputs) and each player's own corresponding payoff (earning or compensation) were revealed to the each player. In addition, subjects had access to a history table with the same information from previous rounds. A session consisted of 24 rounds, where the first three rounds were trial rounds which did not count for the final earnings. After all 24 rounds were completed, subjects were asked to fill in a survey which collected demographic information: age, gender, country of residence, education level, number of courses in economics, and whether they have some knowledge of game theory.

At the end of the session one round was randomly selected for payment. The conversion rate of "points" into money earnings was $3 : 1$ in the Complements treatment, and $4 : 1$ in the Substitutes treatment. This was to done to make average earnings (at the theoretical predictions) similar across the two treatments.[12] Subjects also received a show-up fee of 3 Euro.

one decimal place.

[11]One session in the Substitutes treatment had only 12 participants, and a principal was matched with two agents twice.

[12]Even though payoffs are the same in the subgame perfect equilibria of the two treatments, they are different in the no-delegation benchmark ($\lambda = 1$) which, based on the results of Huck et al. (2004), we anticipated to be reached often as well.

3.4 Results

3.4.1 Principal's choice of λ

Our main hypothesis is that principals set cooperate incentives with strategic complements and competitive incentives with strategic substitutes. Table 3.1 presents basic statistics about the weights set by the principals. Table 3.2 presents p-values of sign tests comparing the weights set by the principals to the SPE benchmark and the no-delegation equivalent benchmark.[13] The results in the Complements treatment do not support the theoretical prediction. The principals on average set a λ of 1.186, much higher than the predicted value of 0.6. A sign test rejects the hypothesis that the value of λ is equally likely to be above than below 1 in favor of the alternative hypothesis that principals are more likely to choose competitive incentives ($\lambda > 1$). In fact, in each of the five sessions, the average value of λ was above 1.

Table 3.1: Principals' choice of λ

Treatment	SPE predicted λ^*	Average λ
Complements	0.6	1.186 (0.078)
Substitutes	1.4	1.382 (0.147)

Notes: The unit of observation is one independent session. Standard deviations are shown in brackets.

In the Substitutes treatment (second row), we observe an average value for λ of 1.382, which is close to the theoretically predicted value of 1.4. A sign test cannot reject the hypothesis that the median of λ is 1.4. At the same time a sign test rejects the hypothesis that the median value of λ is 1. These findings support the hypothesis that principals set competitive incentives with strategic substitutes.

Table 3.2: p-values from tests of λ

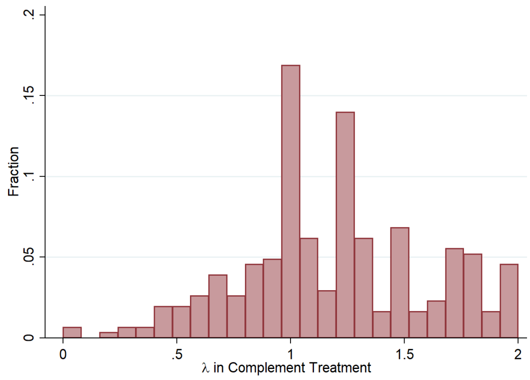
Treatment	Sign test $H_1 : \lambda \neq \lambda^*$	Sign test $H_1 : \lambda > 1$	Mann-Whitney u test $H_1 : \lambda_{\text{Comp}} < \lambda_{\text{Subs}}$
Complements	0.063*	0.031**	0.024**
Substitutes	1.000	0.031**	

Notes: The unit of observation is one independent session. Number of independent observations is 5 per treatment. Stars represent the level of significance, with *** p<0.01, ** p<0.05, and * p<0.1.

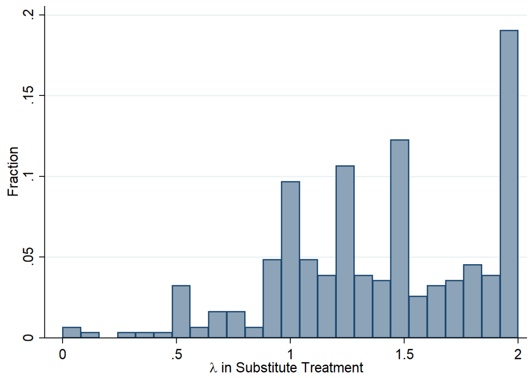
Comparing the two treatments, we find that principals on average set less competitive incentives in the Complements treatment ($\lambda = 1.186$) than in the

[13] For all tests we treat each session as one observation, implying that we have 5 independent observations in each treatment.

Substitutes treatment ($\lambda = 1.382$). A one-sided Mann-Whitney u test reveals that this difference is significant. The direction of this difference is in line with the main theoretical prediction.



(a) Complements



(b) Substitutes

Figure 3.1: Histogram of λ in both treatments
Notes: The histograms are based in all decisions by all principals over the 21 rounds. The horizontal axis uses a bin width of 0.08.

Figure 3.1 displays the distribution of the values of λ for each of the two treatments. In the Complements treatment, the modal value of λ is 1, the "no-delegation"-equivalent. Other frequently selected values are 1.25 and 1.5. In 59.6% of the cases a value $\lambda > 1$ is chosen. The overall distribution is also skewed to the right, but less so than in the Substitutes treatment. In the Substitutes treatment, the distribution of λ is skewed to the right, where most of the λ choices are higher than 1. The modal value of λ is 2, the competitive extreme, where the principal induces the agent to care only about the difference in payoffs of the two principals. Other frequently chosen values are 1, 1.25 and 1.5. Overall, a value $\lambda > 1$ is chosen in 73.6% of the cases.

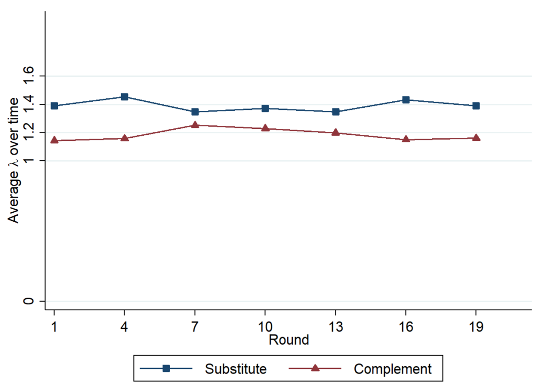


Figure 3.2: Average λ over time

Figure 3.2 presents the development of λ over time. It turns out that the average in the Substitutes treatment is always above the average in the Complements treatment. Moreover, the average values are rather stable in both treatments. Most importantly, there is no evidence that the value of λ in the Complements treatment displays a downward trend and tends toward the equilibrium value over time.[14]

3.4.2 Agents' choice of x

Descriptive statistics of the agents' choice of x are presented in Table 3.3. To evaluate the agents' choice of x, we use two benchmark values: the predicted equilibrium value ($x_i(\lambda^*, \lambda^*)$) assuming that the principals choose the equilibrium values for λ, and the predicted equilibrium value of the inputs ($x_i(\lambda_i, \lambda_j)$) in the subgame corresponding to the values for λ_i and λ_j that the principals actually choose in the round (see equation 3.3). The latter benchmark varies from one round to the next. The third column in Table 3.3 is based on the average value across all rounds within each session.

Table 3.3: Agent's choice of x

Treatment	Prediction		Average x
	$x(\lambda^*, \lambda^*)$	Average $x(\lambda_i, \lambda_j)$	
Complements	12	6.573 (0.290)	7.329 (0.429)
Substitutes	5.6	5.410 (0.182)	5.566 (0.152)

Notes: The unit of observation is one independent session. Standard deviations are shown in brackets.

We observe that in the Complements treatment agents set a much lower average value x (7.329) than would be predicted by SPE (12). Much of this difference can be explained by the earlier observation that the principals on average set a more competitive (i.e., higher) λ than predicted by SPE. Taken this into account, when investigating agents' responses, the SPE predicted x is a less relevant benchmark than the NE predictions with the specific (λ_i, λ_j) pair each agent faces in each subgame. We observe that the average value of x (7.329) is actually higher than the average equilibrium value of the inputs (6.573) in the corresponding subgames ($x_i(\lambda_i, \lambda_j)$). In this sense, the behavior of the agents in the Complements treatment is more cooperative than the incentives by the principals would induce them to be.[15]

[14] Similar patterns are discovered at the session level. See Appendix 3.7.2.2 for details.

[15] Recall that due to the difference in the strategic nature of the interaction, the value of x has a different interpretation in the two treatments. In the Complements treatment a higher x indicates more cooperative behavior, whereas in the Substitutes treatment a higher x indicates more competitive behavior.

For the Substitutes treatment, the results are quite different. The average value of x (5.566) is very close to the value (5.6) predicted by SPE. Statistically, the two values are indistinguishable ($p = 1$ with a sign test). As was seen in the previous subsection, the average values for λ set by the principals are also close to the SPE prediction. So, unlike the Complements treatment, the incentives set by the principals provide no reason for the inputs chosen by the agents to deviate from SPE. Still, taking the actual values of the λ's into account, the average equilibrium inputs (5.410) are somewhat lower than the average observed inputs (5.566), and the difference is significant with a sign test. This implies that agents behave more competitively than the incentives give them reason to. However, the difference is small in magnitude, and it is fair to say that the behavior of the agents accords quite well with SPE.[16]

Table 3.4: p-values from tests of x

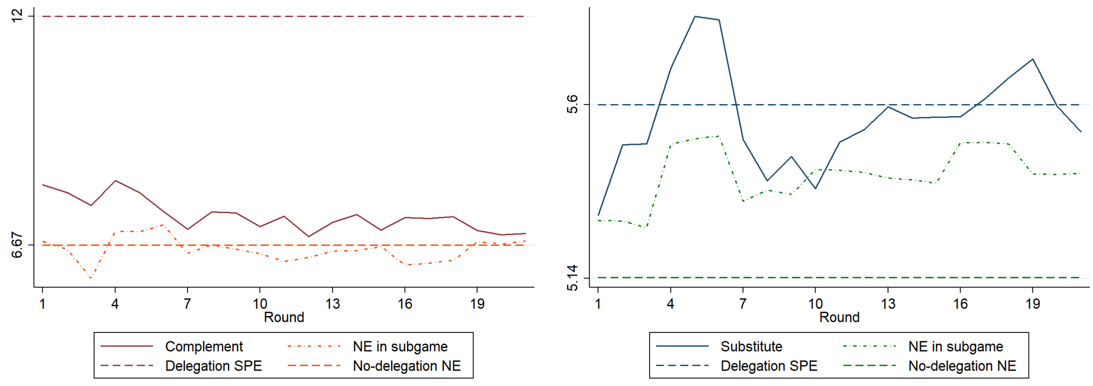
Treatment	$H_1 : x \neq x(\lambda^*, \lambda^*)$		$H_1 : x \neq x(\lambda_i, \lambda_j)$	
	Sign test	Signed rank test	Sign test	Signed rank test
Complements	0.063*	0.043**	0.063*	0.043**
Substitutes	1.000	0.893	0.063*	0.043**

Notes: The unit of observation is one independent session. Number of independent observations is 5 per treatment. Stars represent the level of significance, with *** p<0.01, ** p<0.05, and * p<0.1.

The dynamics of the average x are depicted in Figure 3.3. The two static benchmarks in the figure are the inputs that correspond to the delegation subgame perfect equilibrium ($x(\lambda^*, \lambda^*)$) and the no-delegation equilibrium ($x(\lambda = 1, \lambda = 1)$), respectively. The dynamic benchmark ($x(\lambda_i, \lambda_j)$) is based on the equilibrium inputs corresponding to the actual weights chosen by the principals.

In the Complements treatment, the average x starts at a relatively high level and approaches the no-delegation equilibrium towards the last round. In the Substitutes treatment, the average x starts at a relatively low level and approaches the SPE towards the last round. This implies that in both treatments the values of x start at a relatively cooperative level, and that cooperation decreases over time (i.e., x increases over time in Substitutes and decreases over time in Complements).

[16] We also looked at the observations when both principals set $\lambda = 1$, which can be regarded as equivalent to the case without delegation, as the payoff function of the agents are the same as the payoff function of their respective principals. There were altogether 40 (28) observations in 3 (1) sessions of the Complements (Substitutes) treatment. In the Complements treatment, the average value of x in these no-delegation equivalent cases (7.778) is also more cooperative than the no-delegation NE (6.67). In the Substitutes treatment, the average value of x in the no-delegation equivalent cases (4.786) is also slightly more cooperative than the no-delegation NE (5.14). Due to the small number of observations, we are unable to statistically test the differences.



(a) Complements (b) Substitutes

Figure 3.3: Agents' x decisions over time

We also see that in the Substitutes treatment the average input traces the dynamic benchmark (equilibrium in the subgame) quite well. The two move up and down more or less in parallel, indicating that the agents are responsive to the incentives set by the principals. In the Complements treatment, the inputs display a downward trend over time, but are above the theoretical benchmark in almost all rounds. As we already noted, agents' choices in this treatment are more cooperative than their incentives would predict. The last three rounds exhibit a narrowing of the gap between agents' choice of inputs and the dynamic benchmark. However, there is no clear trend indicating whether sufficiently long play would result in the convergence to the dynamic benchmark.

To further examine how agents respond to the incentives set by the principals, we estimate the following relationship between agents' input choices in a round (x_{it}) and the weights set by the own principal (λ_{it}) and the other principal (λ_{jt}):

$$x_{it} = \alpha_0 + \alpha_1 \lambda_{it} + \alpha_2 \lambda_{jt} + \epsilon_{it} \tag{3.5}$$

This equation can be interpreted as an empirical first-order Taylor approximation of the non-linear equilibrium equation (3.3). The equation is estimated using a random effect panel regression with AR(1).[17] The estimated coefficients are presented in Table 3.5. We observe that the agents' input choices are significantly affected by the principals' weights. Moreover, for each treatment the signs of the effects of both the own and the other principals' weight are in line with the

[17] A Hausman test ($\chi^2 = 0.01$ for Complements and $\chi^2 = 5.32$ for Substitutes) cannot reject the null hypothesis that the difference in coefficients is not systematic. The hypothesis of no serial correlation (F=5.17 for the Complements treatment and F=35.71 for the Substitutes treatment) is rejected for both treatments. Since subjects are randomly re-matched within the same session, we cannot rule out the fact that standard errors may be correlated within session. Therefore, a Prais and Winstein panel regression with AR(1) disturbance and session-level clustered standard errors is estimated.

Table 3.5: Agents' inputs in response to principals' weights

Variables	Complements	Substitutes
λ_{it}	-2.543***	1.404***
	(0.195)	(0.196)
λ_{jt}	-0.479**	-0.107***
	(0.157)	(0.00843)
Constant	10.95***	3.750***
	(0.559)	(0.261)
Observations	924	930
Number of subjects	46	44

Notes: A Prais-Winsten panel regression model with AR(1) disturbance is estimated. Standard errors are clustered at Session level and shown in parentheses. Stars represent the level of significance, with *** p<0.01, ** p<0.05, and * p<0.1.

equilibrium predictions that follow from equation (3.3).[18] This indicates that the agents respond to the incentives set by the principals in the direction predicted by the equilibria of the corresponding subgames. We calculate the theoretically predicted coefficients α_1 and α_2 of Equation 3.5 around each observed (λ_i, λ_j) pairs. In the Complements treatment, the average predicted α_1 is -19.778 and the average predicted α_2 is -10.322, while the estimated α_1 and α_2 in the regression as reported in Table 3.5 are -2.543 and -0.479 respectively. In the Substitutes treatment, the average predicted α_1 and α_2 are 7.028 and -1.781 respectively, while the estimated coefficients are 1.404 and -0.107. The average theoretically predicted coefficients are higher than the estimated ones in absolute values. Even though the agents' responses to the incentives in the subgames are in the predicted directions, the agents largely underreact to the incentives set by their own principal and the other principal.

3.4.3 Payoffs

In the game with strategic complements, the model predicts that both principals set cooperative incentives, resulting in both principals and agents being better off than in the case without delegation. With our parameterization, the delegation SPE yields a payoff of 67.2 for both principals and agents, while the Nash equilibrium payoff is 44.4 without delegation for both roles. In the game with strategic substitutes, the model predicts that principals set competitive incentives

[18]Specifically, we have $\frac{\partial x_i}{\partial \lambda_i} = -\frac{2abc\lambda_j(2b\lambda_j+c)}{(4b^2\lambda_i\lambda_j-c^2)^2}$ and $\frac{\partial x_i}{\partial \lambda_j} = -\frac{ac^2(2b\lambda_i+c)}{(4b^2\lambda_i\lambda_j-c^2)^2}$. With our parameterization, this gives $\frac{\partial x_i}{\partial \lambda_i} < 0, \frac{\partial x_i}{\partial \lambda_j} < 0$ for Complements, and $\frac{\partial x_i}{\partial \lambda_i} > 0, \frac{\partial x_i}{\partial \lambda_j} < 0$ for Substitutes.

to induce their agents to act more competitively, resulting in both principals and agents being worse off than without delegation. With our parameterization, the payoff for both principals and agents is 67.2 in the SPE, which is lower than the equilibrium payoff of 73.5 without delegation (or, equivalently, with $\lambda_i = \lambda_j = 1$) for both roles.

Table 3.6: Average payoffs for principals and agents by treatment

Treatment	Delegation SPE	No-delegation NE	Principals' payoff	Agents' payoff
Complements	67.2	44.4	44.592	47.828
			(2.110)	(2.755)
Substitutes	67.2	73.5	64.540	68.191
			(0.848)	(2.565)

Notes: The unit of observation is one independent session. Standard deviations are shown in brackets. The payoffs of the principals and the agents are the same in the delegation SPE, as well as in the no-delegation NE.

The average payoffs of the principals and agents in each treatment are shown in Table 3.6. In the Complements treatment, both principals and agents are worse off than the SPE prediction. Their realized payoffs are much closer to the no-delegation prediction, which is consistent with the fact that principals set more competitive incentives than those in the SPE. As was seen in the previous section, agents' actions in the Complements treatment are more cooperative than predicted. As a result, even though the principals' average incentives are slightly more competitive than the no-delegation equivalent level, both principals and agents are slightly better off than in the no-delegation equilibrium. In the Substitutes treatment, the average payoffs for both the principals and the agents are similar to the SPE prediction. This result, of course, is consistent, with the fact that both the principals' and the agents' decisions are close to the SPE prediction.

3.5 Discussion

The behaviors of both the principals and the agents in the Substitutes treatment in our experiment accord well with the theoretical predictions. Principals set competitive incentives, which are responded to with competitive actions by the agents, although agents are less reactive to incentives than theoretically predicted. The results of the Complements treatment, however, differ substantially from the theoretical predictions. Principals set competitive incentives whereas they are predicted to set cooperative incentives, and agents act more cooperatively than predicted given these incentives. How can we explain this?

First, we should note that the finding that agents in our Complements treatment act more cooperatively than predicted is in line with the results in various experimental studies of oligopoly without delegation (Engel, 2007; Suetens and Potters, 2007; Potters and Suetens, 2009; Barreda-Tarrazona et al., 2016). They report significantly more cooperation when actions are strategic complements than in the case of strategic substitutes. In price-setting oligopoly experiments it is often found that outcomes are more collusive than predicted by equilibrium, whereas in quantity-setting experiments they are typically more competitive.

It is possible that the principals in the Complements treatment set more competitive incentives than predicted *because* the agents behave more cooperatively than predicted. The delegation SPE predicts that principals set cooperative incentives for their agents in order to induce them to behave more cooperatively than they are predicted to do without such incentives. But if the agents already behave cooperatively without explicitly being induced to do so, and if the principals anticipate or learn this, then the principals may have an incentive to set less cooperative incentives in the first place.

To further explore this possibility we examine how the principals' incentives change if they anticipate that the agents will respond in accordance with equation (3.5). It is straightforward to show that the equilibrium weights would then become:

$$\tilde{\lambda}_i = \tilde{\lambda}_j = \tilde{\lambda} = \frac{a\alpha_1 + (c - 2b)\alpha_0\alpha_1 + c\alpha_0\alpha_2}{(\alpha_1 + \alpha_2)(2b\alpha_1 - c(\alpha_1 + \alpha_2))} \tag{3.6}$$

If we insert the values for the parameters (a, b, c) of our experiment and the estimated coefficient (α_0, α_1, α_2) from Table 3.5, the predicted equilibrium weights are $\tilde{\lambda} = 1.100$ in the Complements treatment, and $\tilde{\lambda} = 1.162$ in the Substitutes treatment.

The principals now have an incentive to set competitive incentives also in the Complements treatment. The reason is that agents act more cooperatively than equilibrium predicts. To compensate for this the principals may want to stimulate the agents to act more competitively. This may explain why the predicted value of $\tilde{\lambda} = 1.100$ is much closer to the average value of $\lambda = 1.186$ in the experiment than the SPE prediction of $\lambda^* = 0.6$. In the Substitutes treatment, the value $\tilde{\lambda} = 1.162$ is lower than the SPE prediction of $\lambda^* = 1.4$ and also lower than the average observed value of $\lambda = 1.382$. The reason is that, as we have seen in Section 3.4.2, the agents are more competitive than the equilibrium in the subgame predicts. This gives principals an incentive to set less competitive incentives than in the SPE. Still, the size of this adjustment is smaller than in the Complements treatment.

Our finding that principals in the Substitutes treatment set competitive incentives is different from the results in Huck et al. (2004), who find strong evidence for principals choosing neutral (non-distorted) incentives in an oligopoly setting with strategic substitutes.[19] The main reason for their result is the agents' behavior in asymmetric subgames where one principal sets competitive incentives and the other sets neutral incentives. In their experiment, the agents with "neutral" incentives find themselves in strategically weaker positions and punish the agents with competitive incentives. This destroys the strategic advantage of setting competitive incentives, making it dominated by setting neutral incentives. We also observe similar patterns in our Substitutes treatment. In our Substitutes treatment, agents with strategically weaker positions in asymmetric subgames punish their counterpart agents by acting more competitively than predicted. However, the agents do not punish enough to make it a clearly dominated strategy for principals to set competitive incentives. This may explain why we still observe principals setting competitive incentives in our Substitutes treatment.

3.6 Conclusion

In this paper we provide experimental evidence on strategic delegation. We find that principals tend to endow their agents with payoffs which differ from their own payoffs. In line with prediction, we find that on average the principals set competitive incentives for their agents in case the underlying game is characterized by strategic substitutes. Contrary to prediction, however, the principals also set competitive incentives for their agents in case the game is characterized by strategic complements, even though less so than with strategic substitutes.

Our paper underscores the relevance of the literature, inspired by Schelling (1960), suggesting that players may use delegation for strategic reasons. Principals distort their agents' payoffs. Moreover, the degree to which they do so, if not the direction, depends on the nature of strategic interaction. Theoretically, delegation is predicted to lead to more competitive outcomes in games with strategic substitutes and to more cooperative outcomes in games with strategic complements. The former prediction is borne out by our experimental results, whereas the latter is not. In this sense the results point toward an important asymmetry. The competition-enhancing effect of delegation under strategic substitutes seems to be more compelling behaviorally than the cooperation-enhancing effect under strategic complements. A possible explanation is that strategic complementar-

[19] In Huck et al. (2004), principals only choose between two incentive schemes: a competitive incentive which is the delegation SPE prediction and a neutral incentive which is equivalent to not delegating.

ity by itself already embodies a cooperation-enhancing effect without delegation (Bester and Güth, 1998; Potters and Suetens, 2009). Given that agents' behavior is more cooperative than predicted, principals' incentive to further encourage cooperation are weakened if not reversed.

Interestingly, our finding that principals set more competitive incentives for their agents with substitutes than with complements is broadly consistent with the empirical literature that relates executive compensation to strategic interaction (Kedia, 2006; Bloomfield, 2018). In fact, this literature reveals little evidence that executives are endowed with cooperative incentives in the case of complement industries. For instance, Bloomfield (2018) indicates that he does not have reliable data to test the prediction that executive compensation in Bertrand industries encourages collusive behavior. So, while the empirical evidence for Cournot industries is in line with strategic delegation, for Bertrand industries the evidence is less convincing.

Our experiment invites several paths for further inquiry. One is the question whether the distortion of incentives relies on the observability of the incentives that the principals set for the agents. The essence of strategic delegation is to change one's own best response function and to induce the other player to respond to this change in the desired direction. It would be interesting to examine to what extent the observability of the agents' contracts is key here. Another important question relates to the cost of delegation. In our current setup the agent's incentives bear no direct cost to the principal. It would be interesting to examine whether different incentives would be set in case the principal would have to pay for the agents' payoffs. We leave these issues for future studies.

3.7 Appendix

3.7.1 Mathematical derivation of the model

Using backwards induction, in the second stage of the game, agent i maximizes his payoff $G_i = \lambda_i \pi_i + (1 - \lambda_i)\pi_j$, knowing that the principal's payoff is $\pi_i = ax_i - bx_i^2 + cx_i x_j$. Agent i selects x_i to maximize G_i

$$G_i = -\lambda_i b x_i^2 + (\lambda_i a + cx_j)x_i + (1 - \lambda_i)(ax_j - bx_j^2) \tag{3.7}$$

The F.O.C of Equation (3.7) yields:

$$x_i = \frac{\lambda_i a + cx_j}{2\lambda_i b} \tag{3.8}$$

$$x_j = \frac{\lambda_j a + cx_i}{2\lambda_i b} \tag{3.9}$$

Solving the equation system gives agents' best response function to (λ_i, λ_i):

$$x_i^*(\lambda_i, \lambda_j) = \frac{ac\lambda_j + 2ab\lambda_i\lambda_j}{4b^2\lambda_i\lambda_j - c^2} \tag{3.10}$$

with $4b^2\lambda_i\lambda_j - c^2 \neq 0$.

Anticipating that agents' best response to the λ pairs in the second stage takes the above form, principal i selects λ_i in the first stage to maximize:

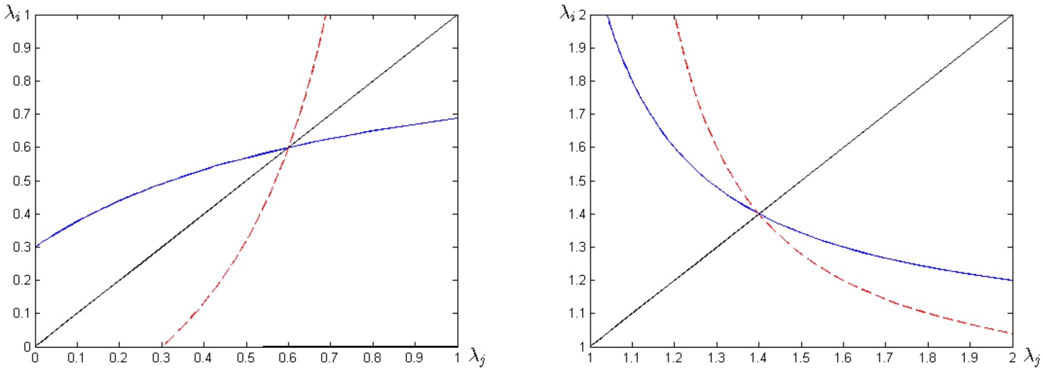
$$\pi_i\big(x_i^*(\lambda_i, \lambda_j), x_j^*(\lambda_j, \lambda_i)\big) \tag{3.11}$$

Derivation of the F.O.C gives us a system of best response functions $\lambda_i = f(\lambda_j), \lambda_j = f(\lambda_i)$ showing how principal i set λ_i in response to λ_j set by the other principal. Solving the system of equations, we have the subgame perfect equilibrium:

$$\lambda_i^* = \lambda_j^* = 1 - \frac{c}{2b}. \tag{3.12}$$

with $4b^2\lambda_i\lambda_j - c^2 \neq 0$, $b \neq 0$, and $2b \neq c$.

With the parameters we used in our experiment ($a_{comp} = 8, b_{comp} = 1, c_{comp} = 0.8$ for the Complements treatment, and $a_{subs} = 40, b_{subs} = \frac{25}{9}, c_{subs} = -\frac{20}{9}$ in the substitute treatment), the best response functions $\lambda_i = f(\lambda_j), \lambda_j = f(\lambda_i)$ and the SPE λ^* in each treatment can be plotted as in Figure 3.4



(a) Strategic complement (b) Strategic substitute

Figure 3.4: Best-response functions of λ in two treatments

Note: The blue solid line represents the best-response function of λ_i and the red dashed line represents the best-response function of λ_j.

3.7.2 Additional Tables

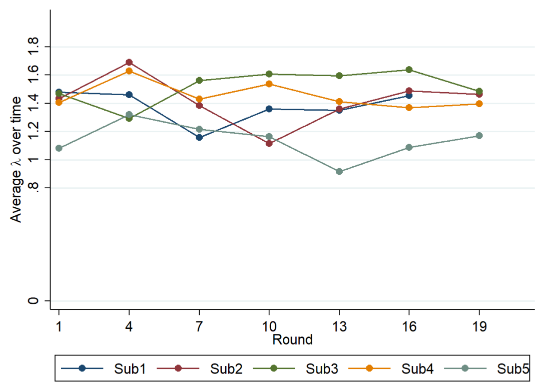
3.7.2.1 Principals' choice of λ

Table 3.1: Principals' choice of λ in each session

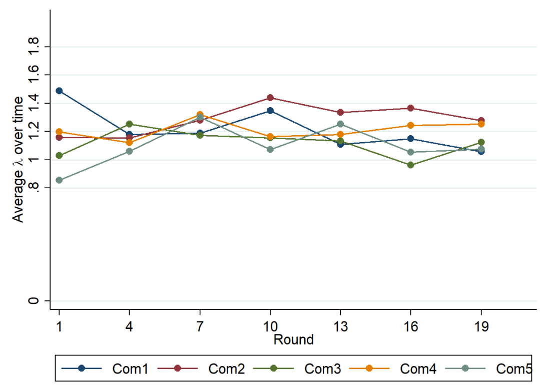
Treatment	Subjects	Average λ	$\lambda > 1$	$\lambda = 1$
Complements				
Com1	16	1.217 (0.435)	68.07%	5.36%
Com2	16	1.287 (0.400)	69.64%	3.57%
Com3	20	1.119 (0.400)	51.43%	22.86%
Com4	20	1.211 (0.426)	67.14%	12.86%
Com5	16	1.096 (0.378)	44.64%	21.43%
Substitutes				
Sub1	24	1.378 (0.501)	66.66%	22.22%
Sub2	16	1.419 (0.556)	71.43%	7.14%
Sub3	24	1.521 (0.424)	84.52%	3.57%
Sub4	12	1.454 (0.236)	97.62%	0%
Sub5	16	1.137 (0.337)	64.29%	5.36%

Notes: Standard deviations are shown in brackets.

3.7.2.2 Development of λ over time in each session



(a) Strategic substitute



(b) Strategic complement

Figure 3.5: Average λ over decision intervals in two treatments

3.7.3 Instructions

3.7.3.1 Instructions in the Complements treatment

Welcome to the experiment. We will first go over the instructions together. After that, you will be given some time to read the instructions at your own pace and ask questions. Please do not write on the instructions. If you need to take notes, you can use the extra blank paper.

During the experiment, you will interact with other participants in this room and make some decisions. The earnings that you make during the experiment are denoted in points. The number of points you earn depends on your decisions, the decisions of other participants, and chance. At the end of the experiment, we will exchange your points into Euro according to a conversion rate of **3 points = 1 Euro**. In addition, you will receive a participation fee of **3 Euro**. The payment shall be transferred to your bank account within one working day.

Please be quiet during the experiment and do not talk with any other participants. If you have a question, please raise your hand and an experimenter will come to you.

The task

There will be two roles: **Principal** (denoted by **P**), and **Agent** (denoted by **A**). You will either be a principal or an agent. A principal is matched with one other principal, let's call them Principal 1 (P1) and Principal 2 (P2). Each principal has to select an input level, $Input_1$ for P1 and $Input_2$ for P2. These input levels determine the earning of each principal. Specifically, the earnings of P1 are given by the following equation:

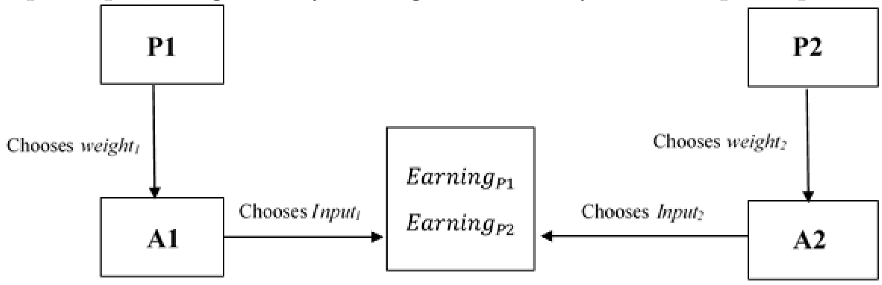
$$Earning_{P1} = 8 \times Input_1 - {Input_1}^2 + 0.8 \times Input_1 \times Input_2$$

The earning of P2 is determined in a similar way.

However, the input decisions will not be made by the principals themselves. Every principal is matched with an agent. The input decision is made by the agent to whom the principal is matched. That is, Agent 1 (A1) chooses the input level ($Input_1$) for P1, and Agent 2 (A2) chooses the input level ($Input_2$) for P2. The only decision a principal makes is how her agent is compensated. The compensation of A1 depends on the earning of P1 and the earning of P2, with a weight set by P1. That is, the compensation of A1 is given by the following equation:

$$Compensation_{A1} = weight_1 \times Earning_{P1} + (1 - weight_1) \times Earning_{P2}$$

where $weight_1$ is selected by P1. Similarly, the compensation of A2 is determined by the weight ($weight_2$) chosen by P2. In other words, the compensation of each agent is determined by the earning of his own principal and the earning of the other principal, weighted by a weight chosen by his own principal.



The graph on the previous page can help you understand the task. Four participants will interact together: two principals and two agents. The principals P1 and P2 will select their weights simultaneously and independently. After that, the two agents, A1 and A2, will be informed about these weights, and each agent chooses an input for his principal simultaneously and independently. These inputs determine the earnings of each principal. These earnings in turn determine the compensation (earnings) of the agents.

Timing

As soon as the experiment starts, you will be randomly assigned a role of a principal or an agent. Then each principal is randomly matched with an agent to form one principal-agent pair. Each principal selects a weight **from 0 to 2** for her agent's compensation (as explained above). Every principal-agent pair remain together for three rounds, and so does the weight selected by the principal. After three rounds, each principal will be randomly matched with another agent, and must select a weight for her new agent's compensation.

At the beginning of each round, each principal-agent pair will be randomly matched with another principal-agent pair to form a four-person group. The weights selected by the two principals will be revealed to both principals and agents. After learning the weights, each agent selects an input **from 0 to 15** for his principal. At the end of every round, you will be informed of your own earning/compensation, as well as the decisions of all four participants in the same group as you. You will also see a history table of the four decisions (two weights and two inputs) and your earning/compensation of all previous rounds. After each round, each principal-agent pair will be randomly matched with another pair.

Before the experiment, there will be three trial rounds. These trial rounds are for you to get familiar with the experiment and will not be counted towards your payment. After the three trial rounds, there will be 21 rounds in total. After all the 21 rounds, you will answer a short questionnaire. At the end of the experiment, one of the 21 rounds will be randomly selected for your payment. Each round has an equal chance of being selected for payment. Please treat your decision in every round with care. Your points earned in the selected round will be exchanged into Euro according to a conversion rate of **3 points = 1 Euro**.

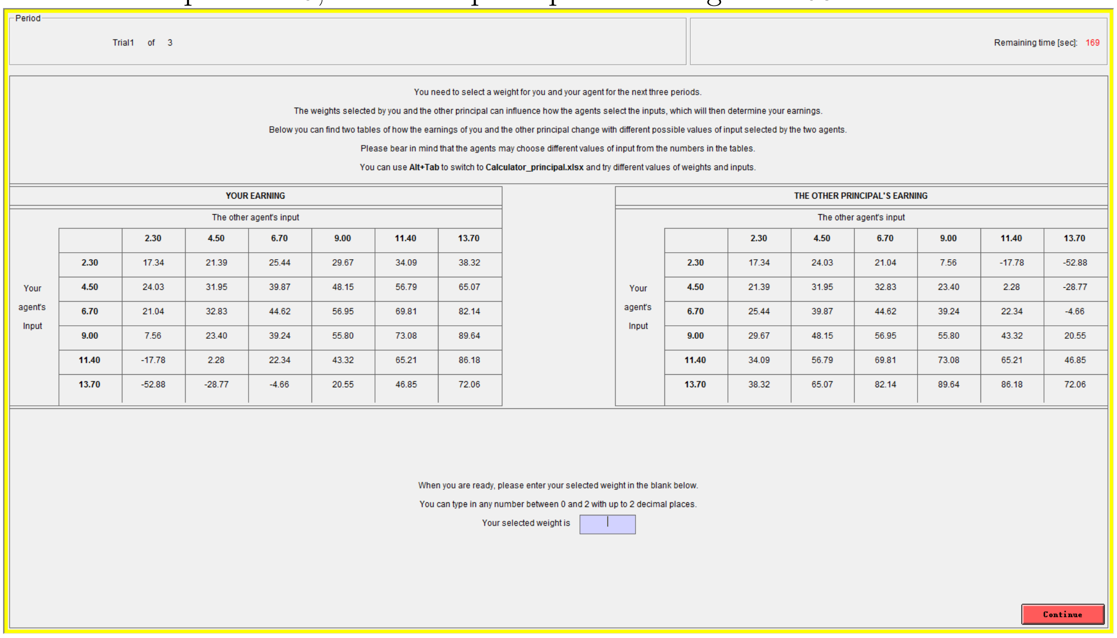
Your decisions in the three trial rounds will not be timed. In the 21 rounds that follow, you have three minutes to make up your mind for each decision. When the time is up, you will be given 10 seconds more. If still no decision is made after 10 seconds, the experiment moves on to the next stage and take your decision as the default level 0.

Information

Some information will be provided to help you understand how your earning or compensation is determined and to make better decisions.

Information for the principal

If you are a principal, when you need to choose a weight, your screen will look like the following graph. You will see two tables on the screen, the one on the left showing how your earning depends on the input choices of both agents, and the one on the right showing how the earning of the other principal depends on the input choices of both agents. In both tables, the first column includes some possible values for input from which your agent may choose, and the first row includes some possible values for input from which the other agent may choose. The numbers in other cells of the tables represent the earnings of you (left table) or the other principal (right table) for a specific combination of inputs. For example, the number 17.34 in the second row and second column of the left table indicates that your earning is 17.34 points, when both your agent and the other agent choose an input of 2.3; the number 24.03 in the second row and third column of the right table indicates that when your agent chooses an input of 2.3, and the other agent chooses an input of 4.5, the other principal's earning is 24.03.



Period

Trial1 of 3

Remaining time [sec]: 169

You need to select a weight for you and your agent for the next three periods.
The weights selected by you and the other principal can influence how the agents select the inputs, which will then determine your earnings.
Below you can find two tables of how the earnings of you and the other principal change with different possible values of input selected by the two agents.
Please bear in mind that the agents may choose different values of input from the numbers in the tables.
You can use **Alt+Tab** to switch to **Calculator_principal.xlsx** and try different values of weights and inputs.

YOUR EARNING

Your agent's Input \ The other agent's input	**2.30**	**4.50**	**6.70**	**9.00**	**11.40**	**13.70**
2.30	17.34	21.39	25.44	29.67	34.09	38.32
4.50	24.03	31.95	39.87	48.15	56.79	65.07
6.70	21.04	32.83	44.62	56.95	69.81	82.14
9.00	7.56	23.40	39.24	55.80	73.08	89.64
11.40	-17.78	2.28	22.34	43.32	65.21	86.18
13.70	-52.88	-28.77	-4.66	20.55	46.85	72.06

THE OTHER PRINCIPAL'S EARNING

Your agent's Input \ The other agent's input	**2.30**	**4.50**	**6.70**	**9.00**	**11.40**	**13.70**
2.30	17.34	24.03	21.04	7.56	-17.78	-52.88
4.50	21.39	31.95	32.83	23.40	2.28	-28.77
6.70	25.44	39.87	44.62	39.24	22.34	-4.66
9.00	29.67	48.15	56.95	55.80	43.32	20.55
11.40	34.09	56.79	69.81	73.08	65.21	46.85
13.70	38.32	65.07	82.14	89.64	86.18	72.06

When you are ready, please enter your selected weight in the blank below.
You can type in any number between 0 and 2 with up to 2 decimal places.
Your selected weight is

Continue

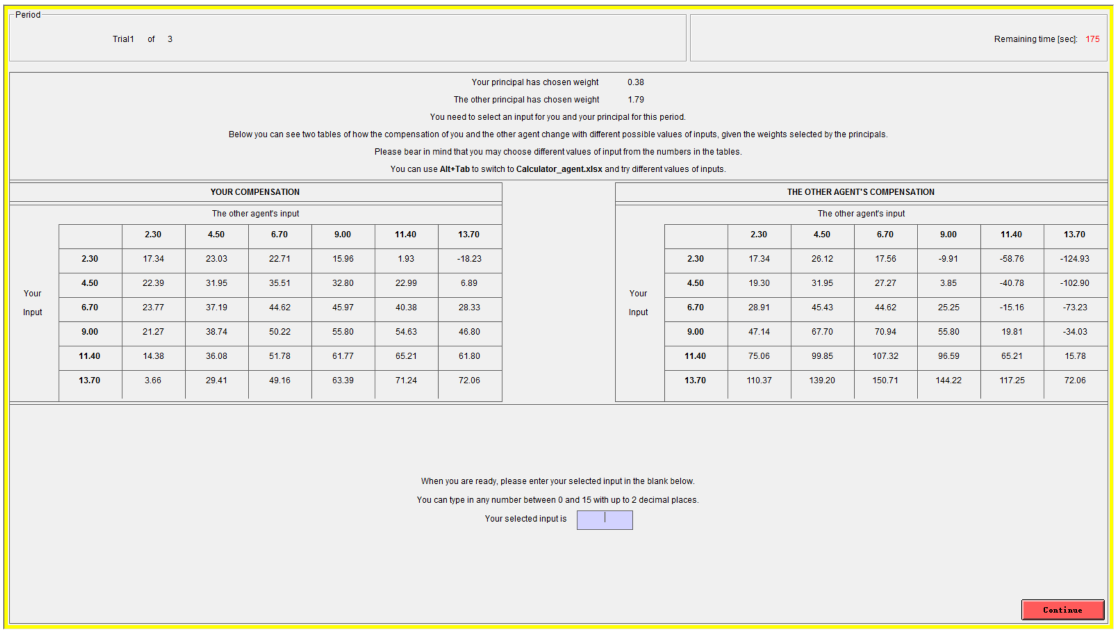
The tables you see here in the instructions are only to help you understand the experiment. Please note that the numbers may be different in the actual experiment. The values for the inputs in the rows and columns chosen are for illustration only. Agents can also select other values than those in the tables, as long as they are in between 0 to 15.

When you are assigned your roles, an experimenter will come and help you open a calculator file "Calculator_principal.xlsx". You can use "**Alt+Tab**" to switch to the calculator file and try different possible values for the weights and inputs of you and the other pair. You can move the scrollbars in the calculator file to try different value combinations, and you will see the earnings and compensations for that specific combination you try. You will also see two similar tables showing how each agent's compensation depends on different possible values of inputs selected by them. As you move the scrollbars, the numbers in the two tables will change accordingly.

When you are ready, you can use "**Alt+Tab"** to switch back to the experiment interface and type in your choice of weight in the blank on screen. Please pay attention to the time limit.

Information for the agent

If you are an agent, when you need to choose an input, your screen will look like the graph below. You will first be reminded of the weight chosen by your principal and the other principal. You will then see two tables, the one on the left showing how your compensation depends on the input choices of both agents, the one on the right showing how the compensation of the other agent depends on the input choices of both agents, given the compensation weights chosen by the principals. In the two tables, the first column includes some possible values of input you can choose, and the first row includes some possible values of input from which the other agent can choose. The numbers in other cells of the tables represent the compensations of you (left table) or the other agent (right table) for each specific combination of inputs. For example, the number 17.34 in the second row and second column of the left table indicates that given the weight (0.38) chosen by your principal, your compensation is 17.34 points, when both you and the other agent choose an input of 2.3; similarly the number 19.3 in the third row and second column of the right table indicates that given the weight (1.79) chosen by the other principal, when you choose an input of 4.5, and the other agent chooses an input of 2.3, the other agent's compensation is 19.3.



Period

Trial1 of 3

Remaining time [sec]: 175

Your principal has chosen weight 0.38

The other principal has chosen weight 1.79

You need to select an input for you and your principal for this period.

Below you can see two tables of how the compensation of you and the other agent change with different possible values of inputs, given the weights selected by the principals.

Please bear in mind that you may choose different values of input from the numbers in the tables.

You can use **Alt+Tab** to switch to **Calculator_agent.xlsx** and try different values of inputs.

YOUR COMPENSATION

The other agent's input

Your Input	2.30	4.50	6.70	9.00	11.40	13.70
2.30	17.34	23.03	22.71	15.96	1.93	-18.23
4.50	22.39	31.95	35.51	32.80	22.99	6.89
6.70	23.77	37.19	44.62	45.97	40.38	28.33
9.00	21.27	38.74	50.22	55.80	54.63	46.80
11.40	14.38	36.08	51.78	61.77	65.21	61.80
13.70	3.66	29.41	49.16	63.39	71.24	72.06

THE OTHER AGENT'S COMPENSATION

The other agent's input

Your Input	2.30	4.50	6.70	9.00	11.40	13.70
2.30	17.34	26.12	17.56	-9.91	-58.76	-124.93
4.50	19.30	31.95	27.27	3.85	-40.78	-102.90
6.70	28.91	45.43	44.62	25.25	-15.16	-73.23
9.00	47.14	67.70	70.94	55.80	19.81	-34.03
11.40	75.06	99.85	107.32	96.59	65.21	15.78
13.70	110.37	139.20	150.71	144.22	117.25	72.06

When you are ready, please enter your selected input in the blank below.

You can type in any number between 0 and 15 with up to 2 decimal places.

Your selected input is

Continue

The table you see here is only to help you understand the experiment. Please note that the numbers may be different in the actual experiment. The values for the inputs in the rows and columns chosen are for illustration only. You and the other agent are free to choose other values from 0 to 15.

When you are assigned your roles, an experimenter will come and help you open a calculator file "Calculator_agent.xlsx". You can use "**Alt+Tab**" to switch to the calculator file and try different possible values of inputs. You first need to type in the weights selected by the two principals, and then you can use the two scrollbars to try different possible values for inputs. As you move the scrollbars, you can see how the compensations change with different combinations of inputs you try.

When you are ready, you can use "**Alt+Tab"** to switch back to the experiment interface and type in your choice of input in the blank on screen. Please pay attention to the time limit.

Summary

1. You are assigned a role of a principal or an agent.
2. The experimenter opens the calculator file for you.
3. A principal and an agent form a principal-agent pair for 3 rounds.
4. Each principal selects a weight which determines how the compensation of her agent depends on her own earning and the earning of the other principal. The weight is fixed for 3 rounds.
5. In each round, the principal-agent pair are randomly matched to another pair. The weights are revealed to all four participants matched together.
6. Given the weights set by the principals, each of the two agents selects a level of input between 0 and 15. The inputs are chosen anew in each round.

7. The two input levels determine the earnings of the two principals.
8. The earnings of the principals, together with the weights, determine the compensation of the agents.
9. After 3 rounds, new principal-agent pairs are randomly formed.
10. In total there are 3 trial rounds and 21 rounds that count towards your payment.
11. After the experiment one of the 21 rounds will be randomly chosen for payment, with an exchange rate of 3 points for 1 Euro.

You can now go over the instructions on your own and ask clarifying questions (if any). When you are ready, you can answer the practice questions on your screen to check if you have understood the instructions. Please raise a hand if you have a question.

Please be reminded that you are not allowed to communicate with other participants throughout the experiment.

Practice questions

Please answer the practice questions below:

1. You are a principal. In one round, your screen is exactly like the graph on page 3. After you and the other principal have selected your weights, your agent selects an input of 11.4, and the other agent selects an input of 6.7. Your earning will be __________ points. The other principal's earning will be __________ points. If this round is selected for payment at the end of the session, your points equal __________ Euro.
2. You are an agent. In one round, your screen is exactly like the graph on page 4. After knowing the weights selected by the two principals, you choose an input of 9, and the other agent choose an input of 11.4. Your compensation will be __________ points. The other agent's compensation will be __________ points. If this round is selected for payment at the end of the session, your points equal __________ Euro.

Please raise a hand if you have finished or if you have a question.

Please be reminded that you are not allowed to communicate with other participants throughout the experiment.

3.7.3.2 Screenshots of the external profit calculators in Complements treatment

Figure 3.6: External profit calculator for the principal in Complements treatment

You can move the scrollbars below to try different values of weight that can be chosen by you and the other principal
You will need to choose a weight for your agent in the experiment interface.
Please move the scrollbar to try different values. As you move the scrollbars, the corresponding value will show up in the cell below. Please DO NOT change the numbers in the cells.
Please DO NOT type in the cells.

TRY: Your choice of weight 0.55 | **TRY:** The other principal's choice of weight 1.80

You can move the scrollbars below to try different values of input that can be chosen by your agent and the other agent
Please move the scrollbar to try different values. As you move the scrollbars, the corresponding value will show up in the cell below.
Please DO NOT type in the cells.

TRY: Your agent's choice of input 0 | **TRY:** The other agent's choice of input 0

For every combination of weights and inputs you try above, you can see below the earnings and compensations of you, the other principal, your agent and the other agent with the above selected values.

Your earning 0.00 | **The other principal's earning** 0.00

Your agent's compensation 0.00 | **The other agent's compensation** 0.00

Below you can see how the compensation tables of your agent and the other agent changes as you try different values of weight

YOUR AGENT'S COMPENSATION

		The other agent's input					
		5.60	**6.70**	**7.50**	**9.00**	**12.00**	**14.00**
	5.60	38.53	41.33	42.68	43.66	39.55	32.31
Your agent's	**6.70**	40.85	44.62	46.68	48.98	47.51	42.03
Input	**7.50**	41.71	46.18	48.75	52.01	52.46	48.26
	9.00	41.42	47.21	50.74	55.80	59.85	58.05
	12.00	33.41	41.84	47.29	55.95	67.20	70.20
	14.00	22.57	32.76	39.49	50.55	66.60	72.80

THE OTHER AGENT'S COMPENSATION

		The other agent's input					
		5.60	**6.70**	**7.50**	**9.00**	**12.00**	**14.00**
	5.60	38.53	34.94	29.60	13.37	-43.39	-99.23
Your agent's	**6.70**	47.24	44.62	39.98	25.07	-29.05	-83.13
Input	**7.50**	54.79	52.88	48.75	34.80	-17.40	-70.20
	9.00	71.71	71.12	67.95	55.80	7.20	-43.20
	12.00	116.35	118.40	117.15	108.60	67.20	21.60
	14.00	154.11	157.92	157.95	151.80	115.20	72.80

Figure 3.7: External profit calculator for the agent in Complements treatment

This calculator is ony for you to get a better idea of the experiment. The numbers you try here will not be recorded.
When you are ready, please switch back to the experiment page and enter your decisions there.

Please enter below the weights selected by your principal and the other principal in this period.
Please enter the weights you see on the experiment interface.

Your principal's weight 0.00 | The other principal's weight 0.00

You can move the scrollbars below to try different values of input that can be chosen by you and the other agent
Please move the scrollbar to try different values. As you move the scrollbars, the corresponding value will show up in the cell below.
Please DO NOT type in the cells.

TRY: Your's choice of input 0 | **TRY:** The other agent's choice of input 0

You can see below the compensations of you and the other agent with the above selected values

Your compensation 0.00 | **The other agent's compensation** 0.00

3.7.3.3 Instructions in the Substitutes treatment

Welcome to the experiment. We will first go over the instructions together. After that, you will be given some time to read the instructions at your own pace and ask questions. Please do not write on the instructions. If you need to take notes, you can use the extra blank paper.

During the experiment, you will interact with other participants in this room and make some decisions. The earnings that you make during the experiment are denoted in points. The number of points you earn depends on your decisions, the decisions of other participants, and chance. At the end of the experiment, we will exchange your points into Euro according to a conversion rate of **4 points = 1 Euro**. In addition, you will receive a participation fee of **3 Euro**. The payment shall be transferred to your bank account within one working day.

Please be quiet during the experiment and do not talk with any other participants. If you have a question, please raise your hand and an experimenter will come to you.

The task

There will be two roles: **Principal** (denoted by **P**), and **Agent** (denoted by **A**). You will either be a principal or an agent. A principal is matched with one other principal, let's call them Principal 1 (P1) and Principal 2 (P2). Each principal has to select an input level, $Input_1$ for P1 and $Input_2$ for P2. These input levels determine the earning of each principal. Specifically, the earnings of P1 are given by the following equation:

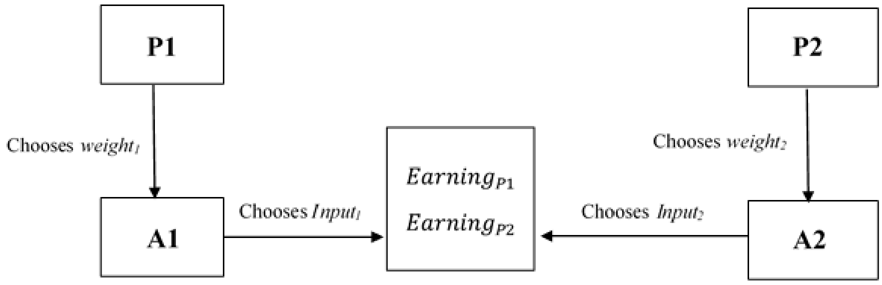
$$Earning_{P1} = 40 \times Input_1 - \frac{25}{9} \times Input_1^2 - \frac{20}{9} \times Input_1 \times Input_2$$

The earnings of P2 are determined in a similar way.

However, the input decisions will not be made by the principals themselves. Every principal is matched with an agent. The input decision is made by the agent to whom the principal is matched. That is, Agent 1 (A1) chooses the input level ($Input_1$) for P1, and Agent 2 (A2) chooses the input level ($Input_2$) for P2. The only decision a principal makes is how her agent is compensated. The compensation of A1 depends on the earning of P1 and the earning of P2, with a weight set by P1. That is, the compensation of A1 is given by the following equation:

$$Compensation_{A1} = weight_1 \times Earning_{P1} + (1 - weight_1) \times Earning_{P2}$$

where $weight_1$ is selected by P1. Similarly, the compensation of A2 is determined by the weight ($weight_2$) chosen by P2. In other words, the compensation of each agent is determined by the earning of his own principal and the earning of the other principal, weighted by a weight chosen by his own principal.



The graph on the previous page can help you understand the task. Four participants will interact together: two principals and two agents. The principals P1 and P2 will select their weights simultaneously and independently. After that, the two agents, A1 and A2, will be informed about these weights, and each agent chooses an input for his principal simultaneously and independently. These inputs determine the earnings of each principal. These earnings in turn determine the compensation (earnings) of the agents.

Timing

As soon as the experiment starts, you will be randomly assigned a role of a principal or an agent. Then each principal is randomly matched with an agent to form one principal-agent pair. Each principal selects a weight **from 0 to 2** for her agent's compensation (as explained above). Every principal-agent pair remain together for three rounds, and so does the weight selected by the principal. After three rounds, each principal will be randomly matched with another agent, and must select a weight for her new agent's compensation.

At the beginning of each round, each principal-agent pair will be randomly matched with another principal-agent pair to form a four-person group. The weights selected by the two principals will be revealed to both principals and agents. After learning the weights, each agent selects an input **from 0 to 10** for his principal. At the end of every round, you will be informed of your own earning/compensation, as well as the decisions of all four participants in the same group as you. You will also see a history table of the four decisions (two weights and two inputs) and your earning/compensation of all previous rounds. After each round, each principal-agent pair will be randomly matched with another pair.

Before the experiment, there will be three trial rounds. These trial rounds are for you to get familiar with the experiment and will not be counted towards your payment. After the three trial rounds, there will be 21 rounds in total. After all the 21 rounds, you will answer a short questionnaire. At the end of the experiment, one of the 21 rounds will be randomly selected for your payment. Each round has an equal chance of being selected for payment. Please treat your decision in every round with care. Your points earned in the selected round will be exchanged into

Euro according to a conversion rate of **4 points = 1 Euro**.

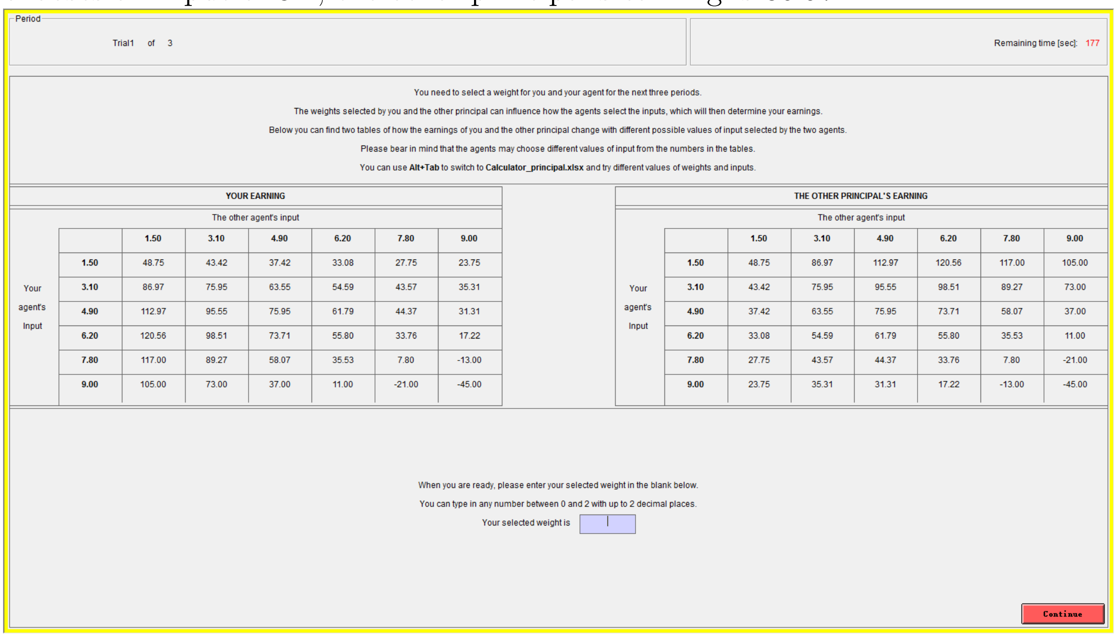
Your decisions in the three trial rounds will not be timed. In the 21 rounds that follow, you have three minutes to make up your mind for each decision. When the time is up, you will be given 10 seconds more. If still no decision is made after 10 seconds, the experiment moves on to the next stage and take your decision as the default level 0.

Information

Some information will be provided to help you understand how your earning or compensation is determined and to make better decisions.

Information for the principal

If you are a principal, when you need to choose a weight, your screen will look like the graph on the next page. You will see two tables on the screen, the one on the left showing how your earning depends on the input choices of both agents, and the one on the right showing how the earning of the other principal depends on the input choices of both agents. In both tables, the first column includes some possible values for input from which your agent may choose, and the first row includes some possible values for input from which the other agent may choose. The numbers in other cells of the tables represent the earnings of you (left table) or the other principal (right table) for a specific combination of inputs. For example, the number 48.75 in the second row and second column of the left table indicates that your earning is 48.75 points, when both your agent and the other agent choose an input of 1.5; the number 86.97 in the second row and third column of the right table indicates that when your agent chooses an input of 1.5, and the other agent chooses an input of 3.1, the other principal's earning is 86.97.



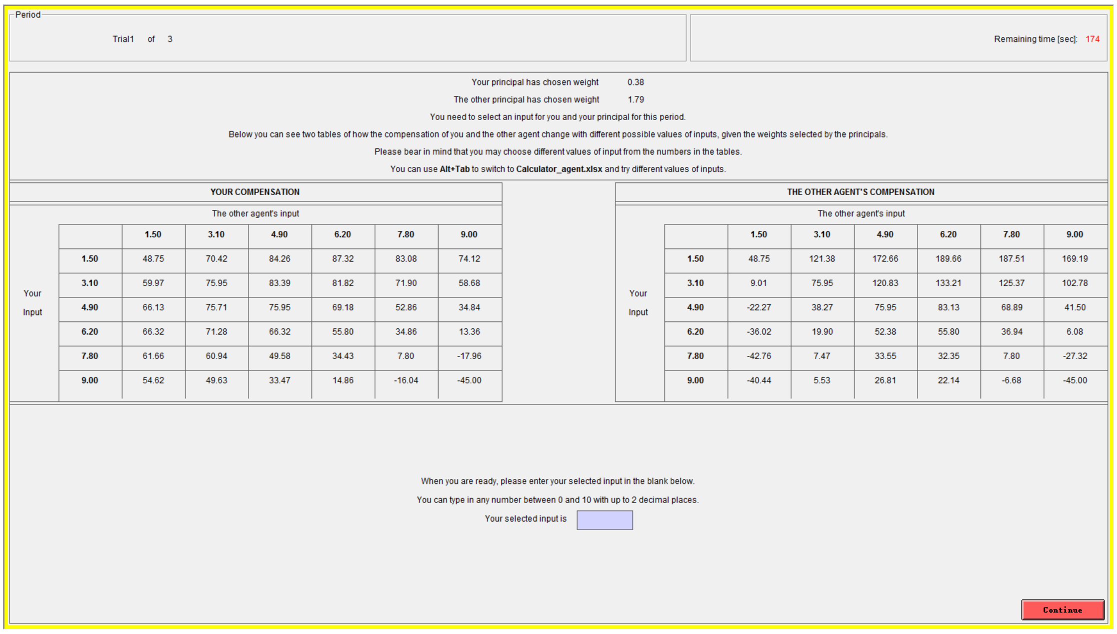
The tables you see here in the instructions are only to help you understand the experiment. Please note that the numbers may be different in the actual experiment. The values for the inputs in the rows and columns chosen are for illustration only. Agents can also select other values than those in the tables, as long as they are in between 0 to 10.

When you are assigned your roles, an experimenter will come and help you open a calculator file "Calculator_principal.xlsx". You can use "**Alt+Tab**" to switch to the calculator file and try different possible values for the weights and inputs of you and the other pair. You can move the scrollbars in the calculator file to try different value combinations, and you will see the earnings and compensations for that specific combination you try. You will also see two similar tables showing how each agent's compensation depends on different possible values of inputs selected by them. As you move the scrollbars, the numbers in the two tables will change accordingly.

When you are ready, you can use "**Alt+Tab"** to switch back to the experiment interface and type in your choice of weight in the blank on screen. Please pay attention to the time limit.

Information for the agent

If you are an agent, when you need to choose an input, your screen will look like the graph below. You will first be reminded of the weights chosen by your principal and the other principal. You will then see two tables, the one on the left showing how your compensation depends on the input choices of both agents, the one on the right showing how the compensation of the other agent depends on the input choices of both agents, given the compensation weights chosen by the principals. In the two tables, the first column includes some possible values of input you can choose, and the first row includes some possible values of input from which the other agent can choose. The numbers in other cells of the tables represent the compensation of you (left table) or the other agent (right table) for each specific combination of inputs. For example, the number 48.75 in the second row and second column of the left table indicates that given the weight (0.38) chosen by your principal, your compensation is 48.75 points, when both you and the other agent choose an input of 1.5; similarly the number 9.01 in the third row and second column of the right table indicates that given the weight (1.79) chosen by the other principal, when you choose an input of 3.1, and the other agent chooses an input of 1.5, the other agent's compensation is 9.01.



YOUR COMPENSATION

Your Input \ The other agent's input	**1.50**	**3.10**	**4.90**	**6.20**	**7.80**	**9.00**
1.50	48.75	70.42	84.26	87.32	83.08	74.12
3.10	59.97	75.95	83.39	81.82	71.90	58.68
4.90	66.13	75.71	75.95	69.18	52.86	34.84
6.20	66.32	71.28	66.32	55.80	34.86	13.36
7.80	61.66	60.94	49.58	34.43	7.80	-17.96
9.00	54.62	49.63	33.47	14.86	-16.04	-45.00

THE OTHER AGENT'S COMPENSATION

Your Input \ The other agent's input	**1.50**	**3.10**	**4.90**	**6.20**	**7.80**	**9.00**
1.50	48.75	121.38	172.66	189.66	187.51	169.19
3.10	9.01	75.95	120.83	133.21	125.37	102.78
4.90	-22.27	38.27	75.95	83.13	68.89	41.50
6.20	-36.02	19.90	52.38	55.80	36.94	6.08
7.80	-42.76	7.47	33.55	32.35	7.80	-27.32
9.00	-40.44	5.53	26.81	22.14	-6.68	-45.00

The table you see here is only to help you understand the experiment. Please note that the numbers may be different in the actual experiment. The values for the inputs in the rows and columns chosen are for illustration only. You and the other agent are free to choose other values from 0 to 10.

When you are assigned your roles, an experimenter will come and help you open a calculator file "Calculator_agent.xlsx". You can use "**Alt+Tab**" to switch to the calculator file and try different possible values of inputs. You first need to type in the weights selected by the two principals, and then you can use the two scrollbars to try different possible values for inputs. As you move the scrollbars, you can see how the compensations change with different combinations of inputs you try.

When you are ready, you can use "**Alt+Tab**" to switch back to the experiment interface and type in your choice of input in the blank on screen. Please pay attention to the time limit.

Summary

1. You are assigned a role of a principal or an agent.
2. The experimenter opens the calculator file for you.
3. A principal and an agent form a principal-agent pair for 3 rounds.
4. Each principal selects a weight which determines how the compensation of her agent depends on her own earning and the earning of the other principal. The weight is fixed for 3 rounds.
5. In each round, the principal-agent pair are randomly matched to another pair. The weights are revealed to all four participants matched together.
6. Given the weights set by the principals, each of the two agents selects a level of input between 0 and 10. The inputs are chosen anew in each round.

7. The two input levels determine the earnings of the two principals.
8. The earnings of the principals, together with the weights, determine the compensation of the agents.
9. After 3 rounds, new principal-agent pairs are randomly formed.
10. In total there are 3 trial rounds and 21 rounds that count towards your payment.
11. After the experiment one of the 21 rounds will be randomly chosen for payment, with an exchange rate of 4 points for 1 Euro.

You can now go over the instructions on your own and ask clarifying questions (if any). When you are ready, you can answer the practice questions on your screen to check if you have understood the instructions. Please raise a hand if you have a question.
Please be reminded that you are not allowed to communicate with other participants throughout the experiment.

Practice questions

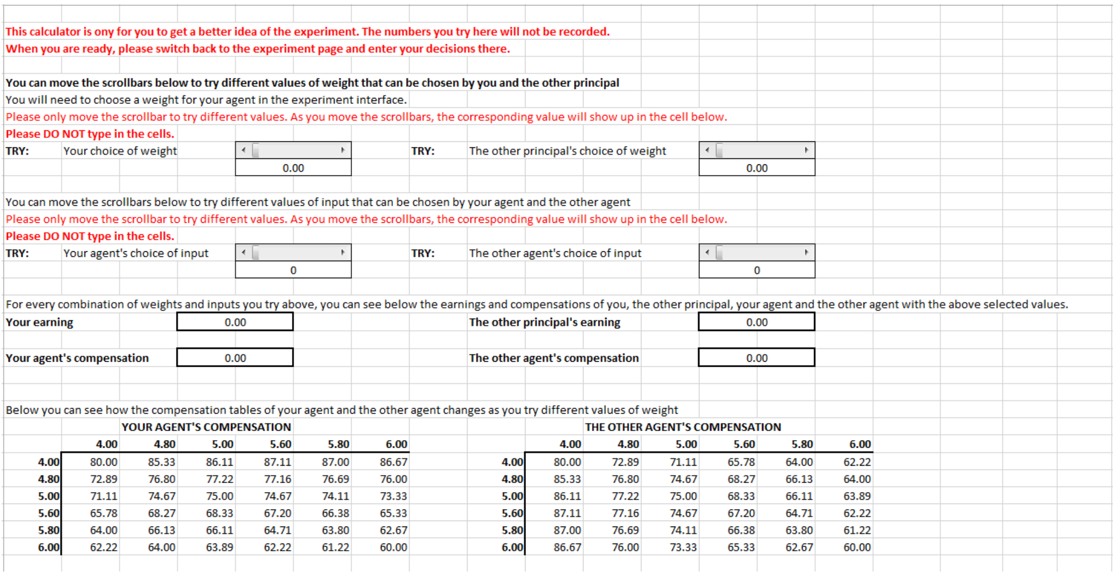
Please answer the practice questions below:

1. You are a principal. In one round, your screen is exactly like the graph on page 3. After you and the other principal have selected your weights, your agent selects an input of 6.2, and the other agent selects an input of 7.8. Your earning will be __________ points. The other principal's earning will be __________ points. If this round is selected for payment at the end of the session, your points equal __________ Euro.
2. You are an agent. In one round, your screen is exactly like the graph on page 4. After knowing the weights selected by the two principals, you choose an input of 9, and the other agent choose an input of 11.4. Your compensation will be __________ points. The other agent's compensation will be __________ points. If this round is selected for payment at the end of the session, your points equal __________ Euro.

Please raise a hand if you have finished or if you have a question.
Please be reminded that you are not allowed to communicate with other participants throughout the experiment.

3.7.3.4 Screenshots of the external profit calculators in Substitutes treatment

Figure 3.8: External profit calculator for the principal in Substitutes treatment



This calculator is ony for you to get a better idea of the experiment. The numbers you try here will not be recorded.
When you are ready, please switch back to the experiment page and enter your decisions there.

You can move the scrollbars below to try different values of weight that can be chosen by you and the other principal
You will need to choose a weight for your agent in the experiment interface.
Please only move the scrollbar to try different values. As you move the scrollbars, the corresponding value will show up in the cell below.
Please DO NOT type in the cells.

TRY: Your choice of weight 0.00 **TRY:** The other principal's choice of weight 0.00

You can move the scrollbars below to try different values of input that can be chosen by your agent and the other agent
Please only move the scrollbar to try different values. As you move the scrollbars, the corresponding value will show up in the cell below.
Please DO NOT type in the cells.

TRY: Your agent's choice of input 0 **TRY:** The other agent's choice of input 0

For every combination of weights and inputs you try above, you can see below the earnings and compensations of you, the other principal, your agent and the other agent with the above selected values.

Your earning 0.00 **The other principal's earning** 0.00

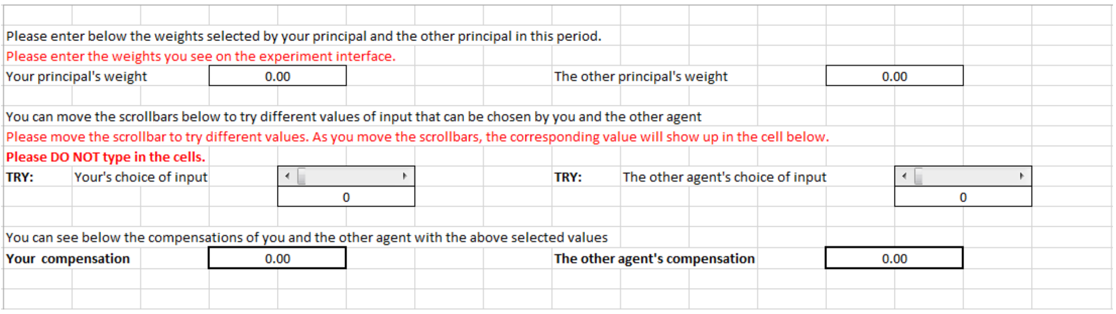
Your agent's compensation 0.00 **The other agent's compensation** 0.00

Below you can see how the compensation tables of your agent and the other agent changes as you try different values of weight

	YOUR AGENT'S COMPENSATION					
	4.00	**4.80**	**5.00**	**5.60**	**5.80**	**6.00**
4.00	80.00	85.33	86.11	87.11	87.00	86.67
4.80	72.89	76.80	77.22	77.16	76.69	76.00
5.00	71.11	74.67	75.00	74.67	74.11	73.33
5.60	65.78	68.27	68.33	67.20	66.38	65.33
5.80	64.00	66.13	66.11	64.71	63.80	62.67
6.00	62.22	64.00	63.89	62.22	61.22	60.00

	THE OTHER AGENT'S COMPENSATION					
	4.00	**4.80**	**5.00**	**5.60**	**5.80**	**6.00**
4.00	80.00	72.89	71.11	65.78	64.00	62.22
4.80	85.33	76.80	74.67	68.27	66.13	64.00
5.00	86.11	77.22	75.00	68.33	66.11	63.89
5.60	87.11	77.16	74.67	67.20	64.71	62.22
5.80	87.00	76.69	74.11	66.38	63.80	61.22
6.00	86.67	76.00	73.33	65.33	62.67	60.00

Figure 3.9: External profit calculator for the agent in Substitutes treatment



Please enter below the weights selected by your principal and the other principal in this period.
Please enter the weights you see on the experiment interface.

Your principal's weight 0.00 The other principal's weight 0.00

You can move the scrollbars below to try different values of input that can be chosen by you and the other agent
Please move the scrollbar to try different values. As you move the scrollbars, the corresponding value will show up in the cell below.
Please DO NOT type in the cells.

TRY: Your's choice of input 0 **TRY:** The other agent's choice of input 0

You can see below the compensations of you and the other agent with the above selected values

Your compensation 0.00 **The other agent's compensation** 0.00

Chapter 4

Can strategic delegation solve the hold-up problem?

4.1 Introduction

Underinvestment in relationship-specific assets is a prevalent problem in bilateral transactions with incomplete contracts. Due to unverifiable efforts or stochastic uncertainty, the allocation of investment returns cannot be contracted upon before the investment decision, and can only be determined through *ex post* bargaining. This gives rise to possible opportunistic behaviors to appropriate investment returns. For fear of not being able to extract a sufficient share of gains to cover sunk investment, investors[1] refrain from choosing the efficient investment level. This underinvestment is often referred to as the "hold-up problem" (Klein, 1998). The hold-up problem is prevalent in vertical relationships such as manufacturer-retailer relationships (Klein et al., 1978). When a manufacturer builds customized prototypes or develops product innovations for a specific retailer, the profitability of the prototype or the technology is affected by *ex post* stochasticity and thus cannot be contracted upon when the manufacturer makes the investment decision. Fearing that the retailer may offer a low price, the manufacturer refrains from making efficient investments.[2]

One of the essential elements that give rise to the hold-up problem is *ex post*

[1]The current paper considers the hold-up problem with a one-sided investment decision. The player who makes the investment decision is referred to as the investor, and the other player the non-investor. This is equivalent to the "seller-buyer" notation in other studies of the hold-up problem in vertical transactions.

[2]The hold-up problem occurs also in many other circumstances with incomplete contracts and *ex post* renegotiation, *e.g.* in firm-employee relationships with specific skills investment (MacLeod and Malcomson, 1993; Malcomson, 1997), standard-essential technology licensing (Farrell et al., 2007; Ganglmair et al., 2012; Li and Shuai, 2019), international climate agreements (Harstad, 2012, 2016), the market for academic journals (McCabe and Snyder, 2018), and international trade (Carnegie, 2014).

opportunistic behavior due to the lack of commitment (Klein, 1998). Existing studies on remedies for the hold-up problem highlight the use of various methods to increase the cost for opportunism and restore commitment (Miller, 2011). The current paper studies whether such commitment can be achieved by delegation in a setting with the hold-up problem. Strategic delegation can serve as a commitment device in some strategic environments under appropriate conditions (Schelling, 1960, 2006). A player can delegate the decision to an agent with known behavioral traits or setting an observable incentive scheme in advance. In this way, the principal commits to a strategy, which alters the opposing player's beliefs and responding strategies. It has been theoretically proved that principals can gain strategic advantages by credible commitment in the form of strategic delegation in oligopolistic competitions (Vickers, 1985; Fershtman and Judd, 1987; Sklivas, 1987) and various bargaining situations (Jones, 1989; Burtraw, 1992). The current paper implements strategic delegation in a hold-up game in the laboratory and presents experimental evidence for how delegating the allocation decision in the bargaining stage to an agent with appropriate incentive schemes can help constrain opportunism and improve investment decisions.

The set-up in this paper is similar to the canonical model (see Che and Sákovics (2008) for a stripped-down version of the model). It is a two-player game consisting of an investor and a non-investor. The game consists of two stages: a unilateral investment stage where the investor faces a binary decision of whether or not to make a costly investment, and a subsequent bargaining stage where both players divide the gains from investment. The allocation of the investment gains cannot be contracted before the investor makes the investment decision. In the bargaining stage, the non-investor has the incentive to opportunistically extract a large share of the surplus, thus leaving an amount insufficient to cover the investment cost to the investor. Anticipating this, the investor refrains from investing in the first stage, thus creating the hold-up problem. In this paper, the bargaining stage is modeled as an ultimatum game where the non-investor makes a take-it-or-leave-it offer to divide the surplus and the investor decides whether to accept or reject.

This paper considers a baseline scenario without delegation and two delegation scenarios. In the baseline scenario, the investor and the non-investor play the basic hold-up game and make all decisions by themselves. In one delegation scenario, an agent makes the decision in the bargaining stage on behalf of the investor. The investor sets the incentive scheme for the agent prior to the investment stage of the hold-up game by setting the agent's payoff conditional on the principal's payoffs. In the other delegation scenario, the agent makes the offer on behalf of the non-investor in the bargaining stage. The non-investor sets the incentive scheme for the agent for the agent in a similar way before the investment stage. Strategic delegation allows the principal to effectively commit to strategies that later place them in an advantageous position in bargaining by setting an appropriate incentive scheme. In the investor-delegation scenario, the investor can induce the agent to reject any offer below the investment cost, thus making a credible threat to not settle for any exploitative offers. In the non-investor-delegation scenario, the non-investor can induce the agent to offer at least as much as the investment cost, thus making a credible promise to not exploit the investor so as to convince him to invest. In each of the two delegation scenarios, by giving the agent incentives which differ from the principal's own preferences, the principal are able to establish commitments that would otherwise not be credible if the strategies were to be chosen by herself[3]. With such commitments, there will be less hold-up risk and an improvement in investment decisions compared to the no-delegation baseline.

This paper shows theoretically how strategic delegation can mitigate the hold-up problem and provides an experimental test of this potential in the laboratory. Empirical evidence on strategic delegation in bargaining is scarce. Laboratory experiments can complement the lack of empirical data with the important advantage of exogenously implementing the different institutional environments with strict control, thus the difference in investment can be compared without noise. This paper conducts an experiment with three treatments, corresponding to the baseline no-delegation scenario and the two delegation scenarios respectively.

The severity of the hold-up problem is captured by the frequency of investment, which reflects the investor's concern for potential hold-up risk, and the ultimatum offer of the non-investor (or the agent), which measures the degree of opportunism. The experimental results provide evidence for the hold-up problem. Investment decisions in all three treatments are nowhere near the efficient level. There is no significant variation in investment rates across treatments. However, conditional on investment taking place, the non-investor-delegation treatment exhibit significantly less severe hold-up behaviors compared with the other two treatments. Offers no less than the investment cost are observed significantly more frequently than the other two treatments. There is also a substantial amount of offers above the investment cost in this treatment, resulting in an average ultimatum offer significantly higher than the other two treatments. In the two delegation treatments, the investment decisions and the ultimatum offers are affected by the incentive scheme of the agent. In the investor-delegation treatment, the average investor induces the agent to reject opportunistic offers in a little more than half of the cases. Those cases are associated with more frequent investments and higher ultimatum offers than when the incentive schemes take other forms. In the non-investor-

[3]For ease of distinction, this paper adopts the arbitrary convention of using feminine pronouns for the principal, and masculine pronouns for the agent.

delegation treatment, the non-investor induces the agent to a sufficient amount to cover the investment cost in almost half of the cases. Those cases are also associated with fewer opportunistic offers, compared to cases with other incentive schemes. Therefore, the null result about investment rates reflects a composition effect: when the principals set the appropriate incentive schemes, investment happens more often and opportunism is hindered; when the principals do not manage efficaciously to use delegation, investment happens even less often than in the Control treatment. Strategic delegation can mitigate the hold-up problem, but in this experiment, this only applies to the subset of contracting pairs with the appropriate incentive schemes.

This paper builds its theoretical foundation upon the notion of using strategic delegation as a commitment device in bargaining (Schelling, 1960). By implementing delegation in a hold-up game setting, this paper provides evidence that the principal can gain strategic advantages by setting the incentives of the agent to differ from the principal's own payoffs. This paper contributes to the literature exploring remedies for the hold-up problem. It is in line with the growing strand of literature that investigates remedies which do not require strict institutional changes. The hold-up problem is mitigated by establishing commitment via an incentive scheme that makes the investment cost relevant for the agent. Yoon (2018) discusses the theoretical possibility of solving the hold-up problem through strategic delegation by the investor. In his paper, this is possible by making the investment cost relevant for the agent via the stock options, which raises the threat point of the agent in bargaining and can result in a higher share allocated to the investor. The scenario with delegation by the investor in the current paper has a similar mechanism. By inducing the agent to reject offers below the investment cost, the investor incorporates the sunk investment cost to the payoffs of the agent and induces the agent to be in a stronger position in the bargaining stage. This paper is also related to the few experimental studies on the trust game with a third party making the allocation decisions (Fershtman, 2007; Eisenkopf and Nüesch, 2016, 2017). These studies differ from the current paper since the agent receives a fixed payment and the principal cannot effectively commit to making or accepting certain offers. In the current paper, the principal sets an incentive scheme for the agent that links the agent's payoff to the payoff of the principal in the bargaining stage. If the agent responds to such incentives, this enables *ex ante* commitment to strategies in the bargaining stage.

The rest of the paper is organized as follows. Section 4.2 summarizes related literature. Section 4.3 explains the model for the delegated hold-up game and the SPE predictions. Section 4.4 describes the experiment procedure and Section 4.5 presents the experimental results. Section 4.6 takes a closer look at the incentive schemes set by the principals for the agents and their key role in mitigating the hole-up problem. Section 4.7 concludes the paper.

4.2 Related literature

This paper is related to two major strands on literature: the theoretical foundation is built upon studies of strategic delegation as a commitment device; it contributes to the literature on remedies for the hold-up problem. In particular, it is related to prior literature which examines the role of a third player to solve the hold-up problem.

4.2.1 Strategic delegation

Schelling (1960, 2006) proposes the idea that strategic delegation can work as a commitment device. "The delegation of part or all of one's interest, or part or all of one's initiative for decision, to some agent who becomes...another player in the game" (Schelling, 1960, p. 142) allows the principal to commit to certain strategies, which affects the opponents' beliefs and thus decisions. If chosen appropriately, strategic delegation can help the principal gain an advantageous position in subsequent transactions.[4] By setting the incentives of the agent in a different direction of herself, the principal can induce the agent to choose strategies that would not be possible if the decisions were to be made by herself.

Strategic delegation was first formally modeled in settings with oligopolistic competition as a two-stage game: in the first stage, firm owners set compensation schemes of the managers; in the second stage, managers select quantities or prices (Vickers, 1985; Fershtman and Judd, 1987; Sklivas, 1987; Miller and Pazgal, 2001, 2002). Firm owners can set the managers' compensation as a weighted average of profit and revenues, sales, or relative profits, which gives the original Cournot or Bertrand game features of a Stackelberg game, where the delegating firm is placed in a pseudo-Stackelberg leader position. Firm owners set aggressive compensations (a higher weight on revenues, sales, or the firm owners' own profit) in quantity competitions and induce the managers to set higher quantities than in cases without delegation; firm owners set cooperative compensations (a lower weight on revenues and sales, or a higher weight on the rival firm's profit) in price competitions and induce the managers to choose higher prices than in cases without delegation. A few experimental studies (Huck et al., 2004; Barreda-Tarrazona et al., 2016) implement strategic delegation in a Cournot oligopoly setting. While

[4] Sengul et al. (2012) provides a review of strategic delegation from the perspective of strategic management and organization theory and compares different incentive schemes. Kopel and Riegler (2008) summarize strategic delegation in oligopolistic competition.

Barreda-Tarrazona et al. (2016) find confirming evidence for principals setting the incentives for agents to deviate from strict profit maximization, Huck et al. (2004) find that principals rarely choose the aggressive contracts with sales bonus. Potters and Yang (2021) examine delegation with strategic complements and strategic substitutes respectively. While they find supporting evidence for competitive delegation incentives with strategic substitutes, they do not find sufficient evidence for cooperative incentives in delegation with strategic complements.

A different strand of literature investigates commitment by delegation in bargaining situations over the division of a sum of money (Jones, 1989; Burtraw, 1992, 1993) or the provision of public goods (Segendorff, 1998). Like delegation games with oligopolistic competition, these delegation games with bargaining also consist of two stages. In the first stage, the principal writes an incentive contract for the agent. The incentive contract maps the agent's payoffs to second-stage bargaining outcomes. The principal can thus induce the agent to commit to advantageous bargaining strategies via the contract. In the second stage, the agent bargains with the opponent on behalf of the principal via Nash bargaining. A common finding of these papers is the existence of a non-cooperative Nash equilibrium where the principals set the agents' incentive contracts in directions different from the principals' own preferences to gain an advantageous bargaining position. The setting of the incentive contract can also be regarded as selecting agents with certain types of preferences (Lammers, 2010). A growing experimental economics literature implements this kind of delegation in ultimatum bargaining (Fershtman and Gneezy, 2001) and face-to-face bargaining (Schotter et al., 2000). Fershtman and Gneezy (2001) find confirming evidence for strategic delegation: when the decision of the responder in the ultimatum game is made by an agent, the responder induces the agent to be tough by setting incentives such that the agent only receives the highest level of compensation if he accepts offers that are sufficiently high.

Other experiments of delegated bargaining focus on a different advantage of delegation from commitment. The principal can "shift the blame" by delegating unfavorable decisions to an agent, which allows the principal to extract a larger share of the surplus in ultimatum bargaining (Fershtman and Gneezy, 2001) and dictator games (Hamman et al., 2010). Unlike the current paper, in these experiments, delegation does not serve as a commitment device, but as a "scapegoat" who is less likely to be held responsible for unkind actions. Coffman (2011), Bartling and Fischbacher (2012), and Oexl and Grossman (2013) show that unkind offers made by an agent in dictator games are punished less often than if the unkind offers were made by the principals themselves, providing supporting evidence for the "blame-shifting" conjecture.

4.2.2 The hold-up problem

The hold-up problem is a classical example of problems associated with incomplete contracts. Klein et al. (1978) introduce the hold-up problem as the underinvestment in exclusive dealership contracts caused by post-contractual opportunism. It is featured with specific investments, incomplete contracts, and renegotiation (Klein, 1998). The hold-up problem in inter-firm transactions is formally modeled in the setting with procurement transactions by Tirole (1986),[5] while Grout (1984) models the hold-up problem with a firm-employee setting.[6] Che and Sákovics (2008) summarize a stripped-down version of the canonical model of the hold-up problem with bilateral trade of a buyer and a seller. The seller makes a binary costly investment decision that is unverifiable and thus cannot be contracted upon. The investment generates a surplus to be divided through Nash bargaining between the seller and the buyer, which yields an equal split of the surplus in equilibrium. If half the surplus is insufficient to cover the investment cost, the seller refrains from making the investment decision, hence the hold-up problem. Empirical evidence of hold-up can be found in various inter-firm transactions (*e.g.* procurement contracts and supply chains)[7] and intra-firm transactions (*e.g.* employer-employee relationships)[8].

Remedies to mitigate the inefficiency in investment caused by the hold-up problem are examined both empirically and experimentally.[9] Early investigations of remedies for the hold-up problem implement changes in the institutional environment. Miller (2011) summarizes this type of remedies as remedies using formal controls. Che and Sákovics (2008) classifies these remedies into organizational remedies which change ownership rights via vertical integration or joint venture (Grossman and Hart, 1986; Hart, 1995), and contractual remedies which rely on contract enforcement (Rogerson, 1992; Nöldeke and Schmidt, 1995; Malcomson,

[5]See Schmitz (2001) for a review of the hold-up problem with the incomplete contracts approach. See Coase (2006) for an overview of the hold-up problem with exclusive dealership.

[6]See Malcomson (1997) for a review of the hold-up problem in the labor market.

[7]See Rindfleisch and Heide (1997) for a review of some early empirical investigations of how asset specificity, environmental and behavioral uncertainty affect investment decisions and opportunism. Evidence for the hold-up problem can be found in various industries, *e.g.* the aerospace industry (Masten, 1984), the franchise markets (Beales III and Muris, 1995), the broiler industry (Vukina and Leegomonchai, 2006), textiles and opium exporting in colonial India (Kranton and Swamy, 2008), Kenyan rose exporting (Macchiavello and Morjaria, 2015), standard-essential patents (Galetovic et al., 2015), and various US manufacturing industries (Martin and Otto, 2017).

[8]Card et al. (2014) provide evidence for hold-up in wage bargaining using matched employer-employee data in Italy. Other empirical studies on hold-up in the labor markets focus on the effect of unionization on firm-specific investments. There has been mixed effects, with Addison et al. (2007) find no impact of work councils or unions, Hirsch (1991), Cavanaugh (1998), and Cardullo et al. (2015) find evidence for a negative impact.

[9]See Miller (2011) for a general of survey of various forms of remedy for the hold-up problem. See Yang (2021) for a review of experimental studies on the hold-up problem.

1997; Maskin and Moore, 1999). Shelanski and Klein (1995) review some early empirical evidence of how hold-up threat due to asset specificity can lead to organizational and contractual changes. Both empirical and experimental studies have examined the effectiveness of these two types of remedies. Experimental evidence has exhibited improvement in investment efficiency from joint ownership (Fehr et al., 2008) and option contracts (Hoppe and Schmitz, 2011). Vertical integration and joint ventures are shown to be associated with more healthcare services (Ciliberto, 2006) and higher patent investment (Geng et al., 2016). Stronger contract enforcement is found to lead to higher investment in golf courses (Cookson, 2018). (Dubois and Vukina, 2016) show how switching from short-term to long-term contracts results in an increase in effort and faster adoption of productivity-enhancing technologies in the broiler industry.

Miller (2011) classifies an additional group of remedies as informal substitutes when strict institutional changes are not possible. Most of this type of studies involve behavioral devices such as the observability of investment decisions (Hackett, 1994; Sloof et al., 2007), the size of the outside option (Dufwenberg and Gneezy, 2000; Oosterbeek et al., 2003; Sloof et al., 2004), information asymmetry (Drake and Haka, 2008; Miller, 2007; Nguyen and Tan, 2019), the opportunity to punish (Hackett, 1994; Dufwenberg et al., 2013), group identity (Morita and Servátka, 2013, 2018), and reputation (Haruvy et al., 2019), *etc*, and provides evidence via laboratory experiments. In particular, a small strand of literature studies the effect of strengthening commitment via pre-game communication. Similar to the current paper, these studies also adopts the design of the hold-up game with a preceding stage that allows the investor or the non-investor to make commitments regarding decisions in the bargaining stage. While in the current paper this is done by setting the incentive schemes for an agent, these papers establish commitment by allowing the non-investor to make a promise (Ellingsen and Johannesson, 2004a,b; Charness and Dufwenberg, 2006, 2010) or the investor to make a threat (Ellingsen and Johannesson, 2004a,b). They provide evidence of communication mitigating the hold-up problem. Specifically, Ellingsen and Johannesson (2004b) investigate promises and threats in a similar setting to the current paper with unilateral investment and ultimatum bargaining. They find that both threats and promises strengthen credibility, but promises are more credible. This result corresponds to the finding in the current paper since investors inducing agents to reject low offers and only accept sufficiently high offers have similar effects of a threat, and non-investors inducing agents to not exploit have similar effects of a promise.

4.2.3 Third player in the hold-up problem

Using strategic delegation to mitigate the hold-up problem involves a third player. Some studies have also explored the possibility of employing a third party to mitigate the hold-up problem. Baliga and Sjöström (2009) theoretically discuss how introducing a third party as a "budget breaker" (Holmstrom, 1982) can mitigate the hold-up problem. Fines paid to the third party can restore incentives for investment and reach the first-best outcome. Arya et al. (2015) show that linear cost-based contracts can be implemented via a third party as the middleman to coordinate between a manufacturer and a retailer. In these papers, the third party works as an intermediary. The investor and the non-investor have no control over the third party except for making transfer. This is essentially different from strategic delegation which links the agent's payoffs to the payoffs of the principal.

Consisting of a unilateral investing stage and a subsequent stage where the non-investor allocates the gain from investment via a dictator game, the trust game by Berg et al. (1995) has similar structures as the hold-up game. The investor faces potential hold-up risk of the non-investor not offering sufficient amount and thus may refrain from investing. It can be regarded as a special form of hold-up game. Fershtman (2007), Eisenkopf and Nüesch (2016, 2017) conduct delegated trust game experiments which introduce a third party to allocate the gain from investment. In these experiments, the third party can be implemented as a neutral intermediary, as well as exgenously appointed or endogenously selected on behalf of one of the two transacting parties. Fershtman (2007) finds no improvement in investment level when the third party is exogenously appointed and labeled as a neutral intermediary, representative of the investor, or representative of the non-investor. On the contrary, Eisenkopf and Nüesch (2016, 2017) find that only when the third party is neutral is there an improvement in investment. When the agent is selected by the non-investor competition between potential agents (Eisenkopf and Nüesch, 2016) and one-sided reputation (Eisenkopf and Nüesch, 2017), investment level is even lower than when the third party is absent. In these cases, the agent decides the allocation by himself without intervention from the principal. In other words, the principal cannot commit to certain strategies in the later stage to alter the belief of the counterpart. The results in these papers indicate that giving the allocation right to a third party alone without commitment is not sufficient to restore trust from the investor. The current paper differs from these studies by allowing the principal to have some control over the agent's decision in the bargaining stage via the incentive scheme. The difference implies that the improvement in investment is mainly due to the commitment effect.

One paper that discusses the possibility of solving the hold-up problem through strategic delegation is Yoon (2018). This is possible when the manager of the investor is compensated with stock options which make the sunk investment cost relevant from the managers' perspective. The relevant sunk cost raises the threat point of the manager in the bargaining stage, and thus leads to higher share allocated to the investor, which may induce the first-best investment level. In the current paper, the incentive scheme that maps the payoff of the agent to the payoff of the principal essentially also has the effect of relating the sunk investment cost to the agent. The current paper measures the incentive schemes from a general perspective. In addition, the current paper also explores the additional scenario when the delegation is on the non-investor's side.

4.3 The model

A typical hold-up game is with two players and consists of two stages: an investment stage which generates a joint surplus, and a bargaining stage where the two players negotiate to divide the surplus. In the current paper, the investment stage consists of a binary decision of whether to invest with a fixed cost or not. The player who makes the investment decision is referred to as the investor, and the other player the non-investor. The bargaining stage takes the form of an ultimatum bargaining game where the non-investor makes a take-it-or-leave-it offer about how to divide the gain from investment, and the investor decides whether to accept or to reject.[10] The current paper examines the effect of commitment in two delegation scenarios: (1) delegation by the investor and (2) delegation by the non-investor. In both scenarios, the principal sets an observable compensation scheme for the agent before the hold-up game starts. In order to focus on the commitment perspective of delegation without complications of additional cost-benefit analysis, delegation is assumed to be costless. The agent is compensated from an additional budget without affecting the payoff of the principal.[11]

4.3.1 The baseline hold-up game

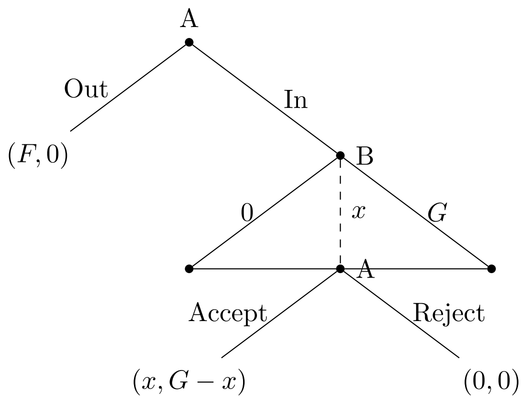
The baseline model is a two-player hold-up game. The two players in the game are denoted by Player A and Player B. π_i denotes the payoff for each player $i = A, B$. The game consists of an investment stage (henceforth Stage 1) and a subsequent ultimatum bargaining stage (henceforth Stage 2) to divide the gain

[10] The similar setting is adopted by Ellingsen and Johannesson (2004b), Hoppe and Schmitz (2011), Dufwenberg et al. (2013), Morita and Servátka (2013, 2018), and Haruvy et al. (2019).

[11] Fershtman and Gneezy (2001), Fershtman (2007), Eisenkopf and Nüesch (2016, 2017) adopt similar settings of costless delegation.

from investment. In Stage 1, Player A makes an investment decision, represented by the indicator variable I, which equals 1 if Player A invests and 0 otherwise. The investment has a fixed opportunity cost of F. If $I = 0$, the game ends, and Player A keeps the investment cost F, *i.e.* $\pi_A = F$, $\pi_B = 0$. If $I = 1$, the investment generates a gain of G. Similar to other models on the hold-up problem, it is assumed that $G > F$, indicating that it is more efficient to invest since it generates a higher surplus. If $I = 1$, the game proceeds to Stage 2 where the gain from investment G is divided through ultimatum bargaining. Player B makes a proposal of $(x, G - x)$ where x is the amount to be allocated to Player A, an integer in $[0, G]$, and $G - x$ is the amount to be allocated to Player B. Player A can either accept or reject the proposal. Player A's acceptance decision is represented by $a(x) \in \{Accept, Reject\}$. If Player A accepts, the proposed allocation is implemented, $\pi_A = x$, and $\pi_B = G - x$. If Player A rejects, both players end up with 0, $\pi_A = \pi_B = 0$. Figure 4.1 shows the game tree representation of the baseline hold-up game.

Figure 4.1: Game tree representation of the baseline hold-up game



In Stage 3, player A accepts any proposal with $x > 0$. Anticipating this, player B proposes to keep almost all of G for himself and offers $x = \epsilon$, with ϵ being a minimum positive amount. Anticipating this in Stage 1, player A does not invest in the first place. The unique subgame perfect equilibrium of the game is therefore $I = 0$, $x = \epsilon$, $a = Accept$ if $x > 0$ and $a = Reject$ if $x = 0$.[12]

Proposition 1. *Without delegation, Player A does not invest and Player B offers 0 in the ultimatum game.*

[12] In case of equality, it is assumed that the player takes the action on the right branch, *i.e.* $I = 1$ if $x = F$ and $a = Reject$ if $x = 0$.

4.3.2 Delegation by Player A

In one delegation scenario, a third player, Player C, is included in the game to serve as an agent for Player A. Player A sets an observable incentive scheme for Player C in Stage 0 by assigning a value to $\pi_C = f(\pi_A)$ from $[0, G]$ for each possible value of π_A. Then the game follows as in the baseline game. However, if Player A chooses $I = 1$ in Stage 1 and the game continues to Stage 2, it is Player C who decides whether to accept or to reject Player B's offer on behalf of Player A. The payoffs for Player A and Player B are determined by the actions in the game in the same way as in the baseline game. The payoff for Player C is determined by the incentive scheme $\pi_C = f(\pi_A)$, which is set by Player A in Stage 0. The incentive scheme $f(\pi_A)$ is assumed to be costless. In this way, the principal's incentive setting decision is not complicated by potential trade-offs between her own payoffs and the cost of the incentives. In addition, the size of the pie to be divided between Player A and B is not affected by the cost of the incentive scheme and remains G whether there is delegation or not, which maintains reasonable comparability with or without delegation. Such costless delegation is also implemented by Fershtman and Gneezy (2001).

In Stage 3 Player C's best response action is $a = Accept$, if $f(x) > f(0)$; $a = Reject$, if $f(x) \leq f(0)$. In Stage 2, Player B makes the proposal $\hat{x} = argmax(G - x)$, such that $f(\hat{x}) > f(0)$. In Stage 1, Player A's decision is $I = 0$ if $\hat{x} < F$; $I = 1$ if $\hat{x} \geq F$. How Player C and Player B act in the hold up game therefore determines how Player A sets $f(x)$ in Stage 0. In order to make sure that she gets at least F if she chooses $I = 1$, $f(x)$ needs to be set such that for $x \in [0, F)$, $f(x) < f(0) < f(F)$; for $x \in [F, G]$, $f(x) \geq f(F) > f(0)$. In other words, Player A needs to induce Player C to be sufficiently tough to reject any offers below the investment cost. In this way, Player A makes sure she receives a sufficient share in the bargaining stage to cover the sunk investment cost and thus eliminates the hold-up risk.

The game has a unique subgame perfect equilibrium: for any $x \in [0, G)$, $f(x) \leq f(0) < f(G)$; $I = 1$, $x = G$, $a = Accept$ if $f(x) > f(0)$; $a = Reject$ if $f(x) \leq f(0)$. Player A makes a pregame commitment via the incentive scheme of Player C that only offers equal the full investment gain G is accepted.

Proposition 2. *With delegation by Player A, Player A rewards Player C to reject offers below the investment cost and accept only the full surplus. Player A invests and Player B offers the full share in the ultimatum game.*

4.3.3 Delegation by Player B

In the other delegation scenario, a third player, Player C, is included in the game to serve as an agent for Player B. Player B sets an observable incentive scheme for Player C in Stage 0 by assigning a value to $\pi_C = f(\pi_B)$ from $[0, G]$ for each possible value of π_B. Then the game follows as in the baseline game, with the proposal $(x, G - x)$ made by player C on behalf of player B in Stage 2. The payoffs for Player A and Player B are determined by the actions in the game in the same way as in the baseline hold-up game. The payoff of Player C is determined by the incentive scheme $\pi_C = f(\pi_B)$, which is set by Player B in Stage 0. Similar as delegation by Player A, $f(\pi_B)$ is also assumed to be costless.

In Stage 3 Player A accepts any $x > 0$. In Stage 2, Player C proposes $x = \bar{x}$, such that $\bar{x} = argmax\ f(G - x)$. In Stage 1, Player A's action is $I = 0$ if $\bar{x} < F$, $I = 1$ if $\bar{x} \geq F$. In Stage 0, anticipating how Player A and Player C respond to $f(\pi_B)$, Player B needs set $f(\pi_B)$ such that $\bar{x} \geq F$ in order to convince Player A to invest. This means that $f(G - F) > f(G - x)$ for any $x < F$. In other words, Player B needs to induce Player C to be sufficiently "fair" to not offer any amount below the investment cost, which also eliminates the hold-up risk and restores Player A's investment incentive.

The game has a unique subgame perfect equilibrium: for any $x \neq F$, $f(0) < f(G - x) < f(G - F)$; $I = 1$, $x = F$, $a = Accept$. Player B makes a pregame promise via the incentive scheme of Player C to offer an amount equal to the investment cost.

Proposition 3. *With delegation by Player B, Player B rewards Player C to offer the investment cost. Player A invests and Player C offers the said amount in the ultimatum game.*

4.4 The experiment

4.4.1 Experimental design and procedure

A laboratory experiment is designed to test the theoretical predictions. There are three treatments: a baseline treatment without delegation (henceforth Control treatment), and two treatments with delegation: one with delegation by Player A (henceforth Treatment A), and the other with delegation by Player B (henceforth Treatment B). In the experiment, $F = 6$ and $G = 10$. x in the proposal $(x, 10 - x)$ made by Player B (or the agent Player C in Treatment B) is an integer number from $[0, 10]$. In the two delegation treatments, Player is set the incentive scheme $f(\pi_i)$ in a similar way to the one implemented by Fershtman and Gneezy (2001).

The principal fills in a form by assigning a value $f(\pi_i)$ from $[0, 10]$ to each possible value of π_i from $[0, 10]$.

Table 4.1: Incentive scheme for the agent

π_i	0	1	2	3	4	5	6	7	8	9	10
$f(\pi_i)$											

In the Control treatment, after reading the instructions, subjects are randomly assigned roles of Player A and Player B. Two players assigned with different roles are randomly matched together. The baseline hold-up game as explained in Section 4.3.1 is played out step-by-step. Player A first chooses between In and Out. If Player A chooses Out, the game stops and the two players end up with $(6, 0)$. If Player A chooses In, the game continues. Player B makes a proposal $(x, 10 - x)$ with x being an integer number from $[0, 10]$. Player A decides whether to accept or to reject the proposal. If Player A accepts, the proposal is implemented. If Player A rejects, both players end up with nothing.

In the two delegation treatments, there is an additional Player C who works as the agent. Subjects are randomly assigned one of the three roles, and three players with different roles are randomly matched together. There is a stage where the principal first sets the incentive scheme for the agent. In Treatment A (B), Player A (B) first fills in the above form to set the compensation scheme for Player C. The form is then announced to all three players, so that the incentive scheme is observable. After that, Player A chooses between In and Out. If Player A chooses Out, the game stops and the two players A and B end up with the same payoff as in the Control treatment, and the agent Player C receives the payoff according to the incentive scheme set by Player A (B) previously. If Player A chooses In and the game proceeds to the ultimatum bargaining stage, the agent Player C makes the decisions on behalf of Player A (B) in this stage. Player A and Player B's payoffs are determined in the same way as in the Control treatment, while Player C's payoff is the corresponding amount that Player A (B) assigns in the incentive scheme.

In all three treatments, there is one practice round and after that, the game is played for 20 rounds. At the beginning of each round, subjects are randomly re-matched within a matching group. The minimum size of a matching group is 6 in the Control treatment and 9 in the two delegation treatments. Repeated game with random matching is implemented in this paper, taking into account the complexity of the delegated hold-up game, which is different from Fershtman and Gneezy (2001) and Ellingsen and Johannesson (2004b) who let their subjects play the game only once. Compared with the delegated ultimatum game of Fershtman and Gneezy (2001), there is an additional investment stage in the delegated holdup game. Compared with the hold-up game of Ellingsen and Johannesson (2004b), there is an additional incentive setting stage. The game is played repeatedly so that the subjects can be sufficiently familiar with the underlying payoff structure of the game, so as to reduce noise caused by confusion. To minimize reputation effects, subjects are randomly re-matched in every round. One of the 20 rounds is randomly selected at the end of the session for payment. The instructions for the three treatments are given in Appendix 4.8.2.

The experiment was conducted in June and September 2018 in CentER lab at Tilburg University. The experiment was programmed using zTree (Fischbacher, 2007). There are six sessions of the baseline treatment, and five sessions in each of the two delegation treatments. The number of participants in each session ranged between 6 to 10 in the baseline treatment, and 9 to 21 in the two delegation treatments.[13] There are 168 participants in total, with 48 in the Control treatment, 60 in Treatment A, and 60 in Treatment B. Subjects are bachelor and master students from various disciplines at Tilburg University. Each session lasted around 40 minutes in the baseline treatment, and around 75 minutes in the two delegation treatments. The payment unit was in Euros. In addition to the randomly selected payoff out of the 20 rounds, subjects also received a show-up fee of 3 Euro. The average payment for each subject was 7.7 Euro, with the minimum being 3 Euro, and the maximum being 13 Euro.

4.4.2 Behavioral predictions

The severity of the hold-up problem can be measured by (1) the investment decisions of Player A, *i.e.* whether Player A invests when the investment is efficient; and (2) the ultimatum offer made by Player B/C in the bargaining stage, *i.e.* whether Player B/C offers enough for Player A to cover the investment cost. The current paper focuses on two related variables: the investment rate and the hold-up rate. The investment rate is defined as the frequency of Player A choosing In. "Hold-up" is defined as the case in which an ultimatum offer insufficient to cover the sunk investment cost. In this experiment, "hold-up" occurs when the ultimatum offer is lower than 6. Accordingly, the hold-up rate is defined as the proportion of ultimatum offers below 6.

Theoretical analysis in Section 4.3 indicates that there will be higher investment rates and lower hold-up rates in the two delegation treatments compared with the Control treatment. In Treatment A, Player A induces Player C to reject any offers below the investment cost via the incentive scheme, which creates a credible

[13]There were two matching groups of 9 in one session in Treatment A with 18 subjects in that session, and a matching group of 9 and one of 12 in one session in Treatment B with 21 subjects in that session. In all other sessions, each session constitutes one matching group.

threat that hinders Player B from extracting too much surplus, and thus restores investment incentive for Player A. In Treatment B, Player B induces Player C to offer at least as much as the sunk investment cost, which creates a credible promise to not exploit Player A, and thus also encourages Player A to invest. In both delegation cases, a reduction in hold-up behaviors and an increase in investment decisions are predicted.

Hypothesis 1. *The investment rate is higher in the two delegation treatments than in the Control treatment. The investment rates in Treatment A and Treatment B are similar.*

Hypothesis 2. *The hold-up rate is lower in the two delegation treatments than in the Control treatment. The hold-up rates in Treatment A and Treatment B are similar.*

The level of ultimatum offers also provides information about the hold-up behavior. A higher level of ultimatum offer is associated with a lower hold-up rate. Therefore, the ultimatum offers in the two delegation treatments are predicted to be higher than those in the Control treatment. Contrary to the investment rate and the hold-up rate, which are predicted to exhibit no difference between Treatment A and Treatment B, ultimatum offers are predicted to be higher in Treatment A than in Treatment B. In Treatment A, delegation gives Player A a pseudo "first-mover advantage" to commit to asking for as large a share as possible. SPE predicts the extreme case of Player A asking for the whole gain from investment. This will be hard to observe in the experiment, but generally, it is reasonable to predict that Player A will incentivize Player C to start accepting at an amount above the investment cost. In Treatment B, Player B has the pseudo "first-mover advantage" and can promise to offer no more than the amount just enough to convince Player A to invest, *i.e.* the same amount as the investment cost.

Hypothesis 3. *The ultimatum offer is higher in the two delegation treatments than in the Control treatment. The ultimatum offer is higher in Treatment A than in Treatment B.*

The propositions in Section 4.3 are derived under the assumption that the players are self-interested agents who only care about their own material payoffs. Violation of this assumption may result in possible behavioral deviations. Previous experimental studies of the baseline hold-up problem show that social preferences can mitigate the hold-up problem to some extent (Gantner et al., 2001; Ellingsen and Johannesson, 2004b,a). Non-investors rarely fully exploit and usually offer a positive amount to the investor. In some cases, an equal, 6:4, or 4:6 split of the investment gain is sometimes observed and accepted by the investor. This indicates that the hold-up problem in the baseline game may be less severe than SPE predictions with more investment decisions and higher bargaining offers to the investors. This potential concern for fairness can also affect behaviors in the two delegation treatments of the current experiment. In addition to using the investment cost as the benchmark, principals set incentive schemes for the agents according to their own interpretations of a "fair" allocation. For example, an equal split that allocates 5 each to the investor and the non-investor can also restore investment despite being lower than the sunk investment cost.

Another possible behavioral deviation may arise due to individual heterogeneity in social preferences of the players. Individuals may have different interpretations with regards to what allocation is "fair" or equitable. In the hold-up game with fixed investment cost, some individuals may regard the equal split of the investment gain as "fair", while some may regard the equal split of the net return as "fair" (Gantner et al., 2001; Ellingsen and Johannesson, 2004a,b). The discrepancy in equity standards may intensify the hold-up problem. If Player A and Player B have different equity standards, in Treatment B, when Player B incentivizes the agent according to her own equity standard, it may be insufficient to convince Player A to invest.

Another associated issue is the trust of the rationality of the agent. In the two delegation treatments, one key assumption is that the agent is rational and follows through the incentive scheme set by the principals. In the experiment, a number of agents' actions may deviate from what the incentive schemes induce them to do. This consequently brings about trust issues from the principal. Especially in Treatment A, adding a third-player gives rise to the risk of the agent not behaving according to the principals' instructions, in addition to the original hold-up risk. This may prevent Player A from investing, which even intensifies the hold-up problem.

4.5 Results

Table 4.2 presents an overview of the investment rate, hold-up rate, and ultimatum offer in all three treatments. All variables are presented as the treatment average of observations from six independent matching groups. For each treatment, the first column reports data from all 20 rounds, while the second column reports data from the last four rounds.

Both the overall data and data from the last four rounds indicate similar levels of investment rate across all three treatments. The investment rate is a little bit below 50%. A Kruskal-Wallis test does not show any significant difference across

Table 4.2: Summary of results across treatments

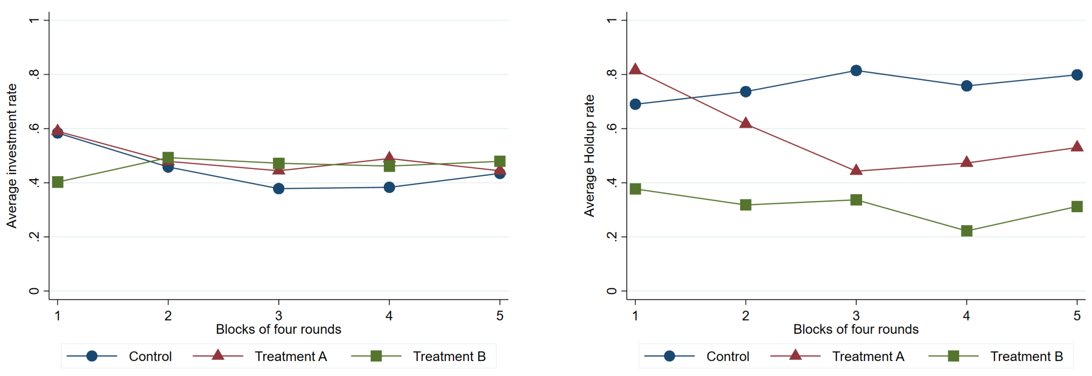
	Control		Treatment A		Treatment B	
	Overall	Last 4	Overall	Last 4	Overall	Last 4
Invest. Rate	0.45	0.43	0.49	0.44	0.46	0.49
	(0.117)	(0.126)	(0.279)	(0.137)	(0.127)	(0.128)
Hold-up Rate	0.73	0.80	0.64	0.53	0.24	0.30
	(0.148)	(0.218)	(0.193)	(0.232)	(0.252)	(0.315)
Ult. Offer	4.82	4.79	4.24	5.24	6.03	6.34
	(0.433)	(0.500)	(1.468)	(1.231)	(0.617)	(0.900)
Accept. Rate	0.90	0.93	0.75	0.79	0.97	1
	(0.046)	(0.059)	(0.120)	(0.109)	(0.030)	(0)

Note: The unit of observation is one independent matching group. The table reports the treatment average (and standard deviation in parentheses) over six independent matching groups of the proportion of “In” decisions, the proportion of ultimatum offers that are below 6, the level of ultimatum offer, and the proportion of ultimatum offers that are accepted for each treatment in all 20 rounds and in the last four rounds.

the three treatments. In terms of (under-)investment decisions, the result does not indicate any significant treatment effect of delegation on the hold-up problem. Figure 4.2a divides all 20 decision rounds in five blocks of four rounds and plots the average investment rate in each treatment across the five blocks. The average investment rate remains between 0.4 and 0.6 across all five blocks in all three treatments. The difference in the pattern of the investment rate is also small across the three treatments.

Result 1. *The investment rate is similar in all three treatments*

Figure 4.2: Investment rate and hold-up rate across five blocks of four rounds



(a) Average investment rate (b) Average hold-up rate

Note: The unit of observation is one independent matching group. The graph presents the development of (a) average investment rate and (b) average hold-up rate in blocks of four rounds for each treatment.

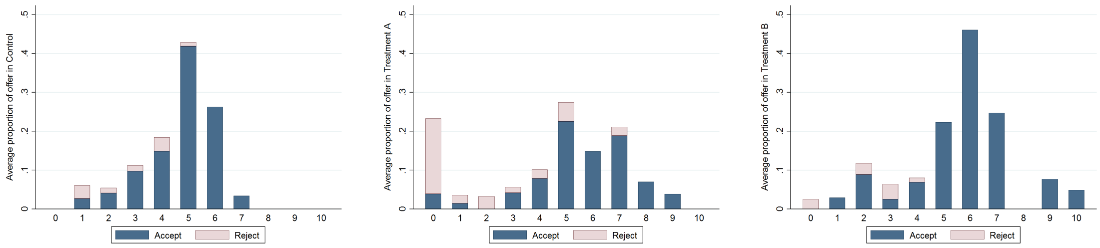
The average hold-up rate measures the actual hold-up behavior from the non-investor's side after the investor has chosen to invest. Looking at the overall data of all 20 rounds, in the Control treatment, the non-investor makes an offer below the outside option of the investor around 73% of the times when the game moves to the ultimatum stage. In Treatment A, the proportion of such offers is around 64%, slightly lower than in the Control treatment. In Treatment B, the proportion of offers below 6 is only 24%. A Kruskal-Wallis test and a Dunn's test indicate that the hold-up rate in Treatment B is significantly lower than both the Control treatment ($p < 0.01$) and Treatment A ($p < 0.05$), while the difference between the Control treatment and Treatment A is not significant. In the last four rounds, the hold-up rate is 80% in the Control treatment, 53% in Treatment A, and 30% in Treatment B. A Dunn's test following a Kruskal-Wallis test indicates a significant difference between Treatment A and the Control treatment ($p < 0.05$), as well as between Treatment B and Control ($p < 0.01$), which is also present in the overall average data. However, the difference is not significant between Treatment A and B. Figure 4.2b shows that the pattern of the average hold-up rate in both the Control treatment and Treatment B is fairly stable across the five blocks, with the hold-up rate remaining between 0.6 and 0.8 in the Control Treatment and between 0.2 and 0.4 in Treatment B. In Treatment A, the average hold-up rate starts at around 0.8 in the first block, and then drops to between 0.4 and 0.6 in the following four blocks. The dynamics indicate a stable significant treatment effect in Treatment B over time, while the treatment effect in Treatment A only becomes significant towards the later rounds of the experiment.

Result 2. *The hold-up rate is similar in the Control treatment and Treatment A, but significantly lower in Treatment B than in the other two treatments. Towards the end of the game, the hold-up rate decreases in Treatment A.*

The comparison of the hold-up rate across the three treatments indicates that delegation by the non-investor can effectively reduce the actual hold-up behavior from the non-investor's side. A similar conclusion can also be drawn from the comparison of the average ultimatum offer across treatments in the fifth and fourth rows of Table 4.2. When the game has progressed to the ultimatum stage, the average offer is higher in Treatment B than in the other two treatments, which is supported by a Dunn's test following a Kruskal-Wallis test ($p < 0.01$ when comparing Treatment B with either Control or Treatment A). The overall average ultimatum offers are similar in all 20 rounds as in the last four rounds. The only exception is found in Treatment A, where there is a slight increase to 5.24 in the last four rounds, but it does not affect the treatment effect.

The last two rows of Table 4.2 report the overall conditional acceptance rate

Figure 4.3: Distribution of ultimatum offers and the frequency of acceptance



(a) Control (b) Treatment A (c) Treatment B

Note: The unit of observation is one single ultimatum offer decision. The graph presents the average frequency each possible value of ultimatum offer is offered and the respective frequency of acceptance and rejection decisions in (a) Control, (b) Treatment A, and (c) Treatment B.

of ultimatum offers in each treatment. The findings in all 20 rounds are similar as in the last four rounds. In the Control treatment, most ultimatum offers are accepted at a rate around 90%. In Treatment B, the (conditional) acceptance rate is close to 100%, with all offers accepted in the last four periods towards the end of the game. However, there appears to be more rejections in Treatment A. The (conditional) acceptance rate in Treatment A is lower than the other two treatments. A Dunn's test following a Kruskal-Wallis test indicate that the acceptance rate in Treatment A is significantly lower than in Control ($p < 0.05$) and in Treatment B ($p < 0.01$), and the acceptance rate in Treatment B is also significantly higher than in Control ($p < 0.05$). Figure 4.3 presents the distribution of ultimatum offers and the acceptance rate conditional on the level of offers in all three treatments. Higher ultimatum offers are offered more frequently in Treatment B and also accepted more often than in the other two treatments. In Treatment A, lower offers are rejected more often than in the other two treatments. In addition, a small proportion of high offers is also rejected in Treatment A, while the same level of offer has a 100% acceptance rate if offered in the other two treatments.

Result 3. *The ultimatum offer is similar in the Control treatment and Treatment A, but significantly higher in Treatment B than in the other two treatments.*

Table 4.3 compares the average payoffs for each player over all 20 rounds across treatments. The overall payoff for Player A is 5.94 in Treatment B, slightly higher than in other two treatments. This difference is statistically significant, supported by a Dunn's test following a Kruskal-Wallis test ($p < 0.05$ when comparing with Control, and $p < 0.01$ when comparing with Treatment A). The overall payoffs for Player B and Player C are similar across treatments. The second panel of Table 4.3 presents the payoffs of each player conditional on Player A choosing "In" and

Table 4.3: Summary of payoffs across treatments

	Control	Treatment A	Treatment B
Overall payoff			
A's payoff	5.31	5.11	5.94
	(0.296)	(0.404)	(0.275)
B's payoff	2.02	1.64	1.81
	(0.556)	(1.056)	(0.705)
C's payoff	-	5.53	5.43
		(0.219)	(0.439)
Payoff if A chooses In			
A's payoff	4.50	3.48	5.95
	(0.438)	(1.761)	(0.580)
B's payoff	4.50	2.75	3.74
	(0.367)	(1.520)	(0.712)
C's payoff	-	8.09	5.36
		(0.973)	(0.323)

Note: The unit of observation is one independent matching group. The table reports the average payoffs (and standard deviation in parentheses) for each type of player in six independent matching groups over all 20 rounds in each treatment.

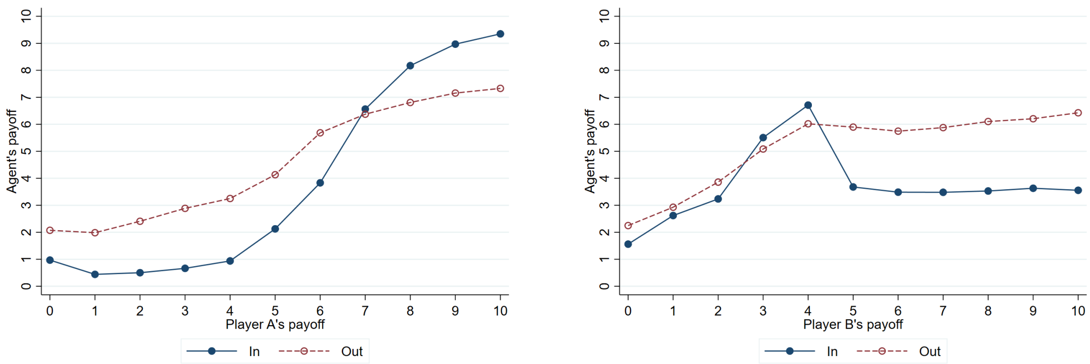
the game proceeding to the ultimatum stage. It can be calculated that the joint payoff of A and B in the ultimatum stage is as high as 9.02 in Control. The highest level of joint payoff is 9.69 as observed in Treatment B ($p < 0.05$ when comparing with Control and $p < 0.01$ when comparing with Treatment A in a Dunn's test following a Kruskal-Wallis test). This is consistent with the frequently proposed high offers and high acceptance rate in the treatment. Similarly, the joint payoff is lowest in Treatment A ($p < 0.05$ when comparing with Control and $p < 0.01$ when comparing with Treatment B in a Dunn's test following a Kruskal-Wallis test), being only 6.44, which is also consistent with the distribution of ultimatum offers and the relatively high rate of rejections in Treatment A.

4.6 Incentive Schemes

The incentive scheme set by the principal plays a key role in enabling strategic delegation to work as a commitment device. Theoretical predictions in Section 4.3 indicate that the principal distorts the incentives of the agent, *i.e.* incentives of the agent do not increase with the principal's own payoff for some values, in order to make a credible commitment. In Treatment A, this is represented by setting $f(\pi_A) \leq f(0) < f(6)$ for all $\pi_A \in [0, 6)$. The delegate is rewarded for rejecting any offer below the investment cost 6. In Treatment B, this is represented by setting

$f(\pi_B) \leq f(4)$ for $\pi_B \in [4, 10]$. The delegate is rewarded for keeping at most 4 for Player B and thus offering at least 6 to Player A. This property of the incentive scheme is defined as *Commit*. In addition, a self-interest principal also aligns the incentives of the agent with her own payoffs for some values. In Treatment A, this is represented by setting $f(10) \geq f(\pi_A)$ for $\pi_A \in [0, 10]$, so that accepting only the whole pie is the agent's most preferred action. In Treatment B, this is represented by setting $f(\pi_B) \leq f(4)$ for $\pi_B \in [0, 4)$, so that it is in the delegate's best interest to keep at least 4 for Player B and thus to offer no more than the investment cost 6 to Player A. This property of the incentive scheme is defined as *Align*.

Figure 4.4: Average incentive scheme for the delegation treatments



(a) Treatment A (b) Treatment B

Note: The unit of observation is one independent matching group. The graph presents the average incentive for each possible value of the principal's payoff in (a) Treatment A and (b) Treatment B.

The average incentive schemes in each of the two delegation treatments are shown in the two panels of Figure 4.4 respectively. In each treatment, the average incentive schemes for those whose Player A has chosen "In" and for those whose Player A has chosen "Out" are presented separately. In both treatments, the *Align* property is represented for both types: the reward for the agent when the principal's payoff is 10 (4) in Treatment A (B) is higher than the reward for any amount below it. On the other hand, the *Commit* property is only represented among those whose Player A has chosen "In". In Treatment A, the reward for the agent when Player A's payoff is 0 is higher than the reward for any amount below 4. In Treatment B, the reward for the agent when Player B's payoff is 4 is higher than the reward for any amount above it. On the contrary, the incentive schemes for those whose Player A has chosen "Out" rarely exhibit any trend of the *Commit* property in both treatments.

The incentive schemes in the two delegation treatments can be classified according to the representation of these two properties. Incentive schemes that satisfy both the *Commit* property and the *Align* property are classified as *Commit & Align*; incentive schemes that satisfy only the *Commit* property but not the *Align* property are classified as *Commit Only*; incentive schemes that satisfy only the *Align* property but not the *Commit* property are classified as *Align Only*; whereas all other incentive schemes that satisfy neither of the two properties are classified as *Other*.

Table 4.4: Comparison across different incentive schemes in Treatment A

	Commit & Align	Commit Only	Align Only	Other
Share	0.38	0.15	0.37	0.10
	(0.116)	(0.184)	(0.190)	(0.057)
Investment rate	0.77	0.10	0.34	0.25
	(0.313)	(0.203)	(0.308)	(0.332)
Hold-up rate	0.42	0.67	0.94	1
	(0.308)	(0.577)	(0.100)	(0)
Ultimatum offer	4.35	3.58	4.61	4.50
	(2.175)	(2.003)	(0.266)	(0.500)

Note: The unit of observation is one independent matching group. The table reports the treatment average of all 20 rounds (and standard deviation in parentheses) for each type of incentive scheme in Treatment A.

Table 4.4 shows the average investment rate, hold-up rate, and ultimatum offer for each type of incentive scheme In Treatment A. Around 38% incentive schemes as shown in Column 1 satisfy both properties. They exhibit an average investment rate of around 77% and an average hold-up rate of around 42%. The investment rate is higher than that of incentive schemes that satisfy only one or neither of the properties, and the hold-up rate is lower than that of all other incentive schemes. Compared with the overall investment rate and hold-up rate in Treatment A and the Control treatment as shown in Table 4.2, these *Commit & Align* incentive schemes also exhibit a higher average investment rate and a lower hold-up rate. This result indicates that when Player A induces the agent to reject any offers below the investment cost as well as encourages the agent to accept as high an offer as possible, Player A is more confident to invest and Player B holds up less often. Theoretical predictions indicate that the *Commit* property is essential to hinder Player B from exploiting, regardless of whether the *Align* property is present or not. However, the *Commit Only* incentive schemes as shown in Column 2 indicate otherwise. The hold-up rate is higher and the ultimatum offer is lower than that of the *Commit & Align* incentive schemes. In addition, the ultimatum offer is even lower than that in Control. On the other hand, the hold-up rate is the highest for the two types of incentive schemes that do not satisfy the *Commit* property. This indicates that inducing the agent to reject any offers below the investment cost is a necessary but not sufficient condition to hinder hold-up. Player A also needs to induce the agent to be sufficiently tough to accept only high offers.

In Treatment A, both the investment decision and the incentive setting decision are made by Player A, which generates potential endogeneity bias in explaining the difference in investment rates among different types of incentive schemes. The three types of incentive schemes that do not satisfy the two properties simultaneously are all associated with lower investment rates than that in Control. One possible explanation for this low investment rate is the lack of trust in the agent when delegation is mandatory. Because of the behavioral uncertainties associated with delegating the decision right to a third party, investors who do not trust the agent may choose to not invest and thus may not spend efforts in setting the proper incentive scheme.

Result 4. *With delegation by Player A, when Player A induces the agent to reject low offers and accept sufficiently high offers, there is a higher investment rate and a lower hold-up rate.*

Table 4.5: Comparison across different incentive schemes in Treatment B

	Commit & Align	Commit Only	Align Only	Other
Share	0.44	0.05	0.34	0.17
	(0.188)	(0.069)	(0.107)	(0.131)
Investment rate	0.53	0.81	0.25	0.46
	(0.232)	(0.270)	(0.189)	(0.149)
Hold-up rate	0.26	0.06	0.72	0.38
	(0.182)	(0.115)	(0.298)	(0.256)
Ultimatum offer	6.17	6.95	4.72	6.08
	(0.118)	(1.078)	(1.168)	(0.715)

Note: The unit of observation is one independent matching group. The table reports the treatment average of all 20 rounds (and standard deviation in parentheses) for each type of incentive scheme in Treatment B.

Table 4.5 shows the average investment rate, hold-up rate, and ultimatum offer for each type of incentive scheme In Treatment B. Around 44% incentive schemes as shown in Column 1 satisfy both properties. They are associated with an average investment rate of around 53%, an average hold-up rate of around 26% and an average ultimatum offer of 6.17, which is similar to the overall results in Treatment B as shown in Table 4.2. The small share (5%) of *Commit Only* incentive schemes exhibit the highest investment rate (81%), lowest hold-up rate (6%) and highest ultimatum offer (6.95) in Treatment B. Theoretical predictions indicate that the *Commit* property is essential to hinder hold-up from Player B/C and restore investment from Player A, regardless whether the *Align* property is present or not. The low hold-up rate and high ultimatum offer associated with the two types of incentive schemes that satisfy the *Commit* property is consistent with the prediction. When Player B induces the agent to offer at least as much as the investment cost, the agent indeed follows through. However, promising to offer just the investment cost is not sufficient to convince Player A to invest. An even stronger promise is needed. A mixed-effect logistic model of observations in the Control treatment and Treatment B[14] with the binary variable of investment decision being the dependent variable is estimated. The estimated coefficients are presented in Column (1) of Table 4.6. The *Commit* property has a positive effect on investment, while the *Align* property has a negative effect of approximately similar size. Both effects are significant.

Result 5. *With delegation by Player B, when Player B induces the agent to offer at least as much as the investment cost, there is a lower hold-up rate and a higher ultimatum offer. There is a higher investment rate only when Player B induces the agent to offer more than the investment cost.*

Column (2) of Table 4.6 shows the estimated coefficients of a mixed-effect logistic model[15] of observations from all three treatments with the binary variable of whether the ultimatum offer is considered as hold-up being the dependent variable. The results are consistent with Result 4 and Result 5. The *Commit* property significantly reduces the hold-up rate in both delegation treatments in comparison to Control. The coefficients are larger in Treatment A than in Treatment B. In addition, in Treatment B, the *Align* property offsets the effect of *Commit* and raises the hold-up rate. Column (3) of Table 4.6 shows the estimated coefficients of a mixed-effect Tobit model[16] of observations from all three treatments with the level of ultimatum offer being the dependent variable. Similar to the results of the hold-up rate, *Commit* and *Align* have significant and opposites effect on the ultimatum offer in Treatment B, with the effect of *Commit* being positive, and *Align* negative. In Treatment A, *Align* increases the level of ultimatum offer. *Commit* also has a slight positive effect, but it is hardly significant.

In both regression (2) and regression (3), it is worth noting that after controlling for incentive scheme types, Treatment B exhibit significantly lower hold-up rates and higher ultimatum offers than the Control treatment. This comes from the *Other* incentive schemes as shown in Column (4) of Table 4.5. A closer look at these incentive schemes shows that on average the non-investor sets a high reward

[14]Observations from Treatment A are not included because of the endogeneity bias in the investment decision and the incentive scheme types. Since subjects are randomly re-matched within each independent matching group, therefore the random effects are clustered by each matching group.

[15]Random effects are clustered by each independent matching group.

[16]Random effects are clustered by each independent matching group. Observations are left-censored at 0 and right-censored at 10.

for the agent to offer the equal split. This fairness concern reduces opportunistic behaviors and mildly improves investment compared to the Control treatment.

Table 4.6: Regression table dependent on incentive scheme properties

	(1) Investment	(2) Hold-up	(3) Ultimatum offer
Treatment A		20.40	-1.360***
		(783.3)	(0.482)
Treatment B	0.887*	-2.964***	1.448***
	(0.500)	(1.000)	(0.415)
A × *Commit*		-4.617***	0.109
		(0.715)	(0.253)
B × *Commit*	1.755***	-2.885***	0.970***
	(0.265)	(0.608)	(0.348)
A × *Align*		-17.79	1.141***
		(783.3)	(0.406)
B × *Align*	-1.584***	1.697**	-1.015***
	(0.329)	(0.623)	(0.366)
Constant	2.379**	2.628	3.709***
	(1.131)	(1.732)	(1.382)
Observations	880	596	596
Number of groups	12	18	18
Number of subject	25	62	62

Note: This table presents regression results using mixed-effect models. Random effects are clustered by each independent matching group. Time fixed-effects are included. Column (1) and (2) present average estimated coefficients from a mixed-effect logistic model with the binary variables of Investment and Hold-up as the dependent variable respectively. Column (3) presents coefficient estimates from a mixed-effect Tobit model with the ultimatum offer as the independent variable, left censored at 0 and right censored at 10. Standard errors are in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

The results of all observations in the three treatments pooling the incentive scheme types as shown in Section 4.5 seem to indicate that the only significant difference is the lower hold-up rate and higher ultimatum offer in Treatment B than the other two treatments. However, how the principals set the incentive scheme plays an important role in establishing commitment. With delegation by the investor, only when the investor induces the agent to reject potential hold-up offers and be sufficiently "tough" to accept high offers can hold-up be hindered and investment increase. With delegation by the non-investor, the non-investor inducing the agent to take into account the investment cost can reduce hold-up and encourage investment. However, when the non-investor does not induce the agent to be “fair” enough, there may be a counter-effect.

4.7 Conclusion

This paper examines strategic delegation as a commitment device in a hold-up game. In the baseline game with unilateral investment and subsequent bilateral bargaining, underinvestment occurs because the non-investor does not make sufficient offers for the investor to cover the sunk investment cost. Delegation is implemented in two treatments: one with the agent making decisions in the bargaining stage on behalf of the investor, and the other the non-investor. The principal has some control over the agent’s decisions by first setting an observable incentive scheme that links the agent’s payoff to her own payoff before the hold-up game starts. The effectiveness of delegation as a remedy for opportunistic hold-up largely depends on the type of incentive scheme set by the principals. With delegation by the investor, when the investor induces the agent to reject offers below the investment cost and encourages the agent to accept sufficiently high offers, the investor is more prone to invest and this also hinders opportunism from the non-investor. With delegation by the non-investor, when the non-investor induces the agent to offer more than the investment cost, the agent follows through by exploiting the investor less often and the investor is also more prone to invest; however, if the non-investor limits the agent’s offer with the investment cost as the upper bound, it has an offsetting effect on both hold-up behaviors and investment decisions. Overall, delegation by the non-investor is more effective in reducing hold-up and encouraging investment.

Both the theoretical predictions and experimental results indicate that the incentive scheme set by the principal plays an essential role in establishing commitment. In order to establish commitment, the principal needs to distort the incentives of the agent. In the baseline hold-up game, the non-investor can refrain from making exploitative offers if the investor makes it credible to reject low offers in the bargaining stage; the investor will invest if the non-investor makes it credible to offer a sufficient amount. Both are not possible without effective commitment devices. With delegation, the investor can set the incentive scheme such that the agent prefers the investor getting a payoff of 0 to any positive payoff below the investment cost, or the non-investor can set the incentive scheme such that the agent prefers making an offer above the investment cost to any smaller amount. In this way, the agent decides on rejection or makes an offer on behalf of the principal. In practice, this is possible by setting instructions for the agent to act “tough” or “fair” or by employing the agent with “tough” or “fair” preferences or personality traits.

The current paper provides evidence for strategic delegation as a remedy to mitigate the hold-up problem. Most of the existing literature on contractual and organizational remedies for the hold-up problem requires the implementation of strict controls through changes in the institutional environment. This paper explores a novel remedy that establishes commitment via an already existing institution, *i.e.* the separation of ownership and management. It provides confirming evidence for the strategic advantage of observable managerial incentives that differ from the principal's payoff function. This paper sheds light on strategic delegation in bargaining, the potential advantage of which has not been explored enough in the existing literature.

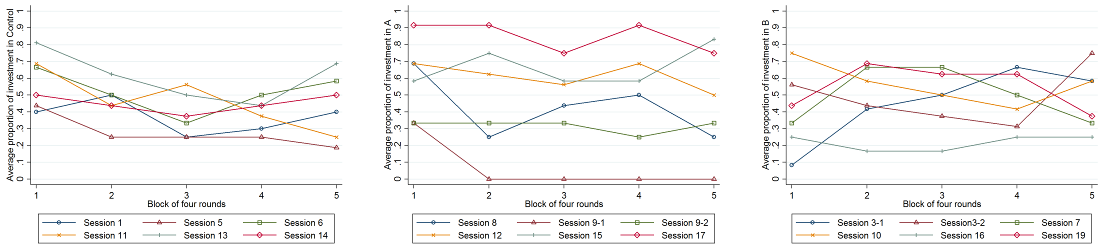
The investor inducing the agent to reject offers below the investment cost is essentially similar to making a credible threat. In the same way, the non-investor inducing the agent to offer an amount above the investment cost is similar to making a credible promise, which is also related to using hostages as credible commitment as suggested by Williamson (1983). In a hold-up game similar to the baseline game in the current paper, Ellingsen and Johannesson (2004b) find stronger credibility in promises than in threats. They claim that inequity aversion makes it harder for investors to follow through threats that may generate unequal outcomes, but helps non-investors keep fair promises. In the current paper, credibility is not a concern since the incentive scheme is set before the investment decision and is not renegotiable. The comparison between delegation by the investor and by the non-investor in the current paper echos the finding of Ellingsen and Johannesson (2004b). Taking into account the fairness concerns, credible promises by the non-investor also have a larger effect than credible threats by the investor in hindering opportunism.

The focus of this paper is on the commitment effect of strategic delegation. In order to single out this effect, some real-world complications were abstracted away in the experimental design. In particular, delegation is exogenously imposed and is assumed to be costless. When the principal needs to pay the agent out of her own budget, commitment is at the cost of the principal's own payoff. The principal faces the trade-off between incentivizing the agent to increase her own payoff and to gain strategic advantages in the bargaining stage. As a result, the share of principal reluctant to distort the agent's incentives away from her own payoff may increase. This opens room for future research.

4.8 Appendix

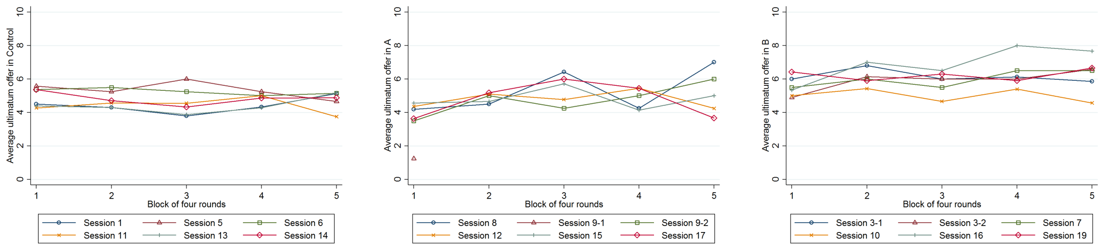
4.8.1 Average investment rate and ultimatum offer over time

Figure 4.5: Development of average investment rate over time



(a) Control (b) Treatment A (c) Treatment B

Figure 4.6: Development of average ultimatum offer over time



(a) Control (b) Treatment A (c) Treatment B

4.8.2 Instructions

4.8.2.1 Instructions in Treatment A

Welcome to the experiment. We will first go over the instructions together. After that, you will be given some time to read the instructions at your own pace and ask questions. Please do not write on the instructions. If you need to take notes, you can use the extra blank paper.

During the experiment, you will interact with other participants in this room and make some decisions. The decisions are anonymous and will not be linked to your identity. Your payment from the experiment depends on your decisions, the decisions of other participants, and chance. In addition, there is a participation fee of **3 Euro**. You will receive your final payment at the end of the experiment in cash.

Please be quiet during the experiment and do not talk with any other participants. If you have a question, please raise your hand and an experimenter will come to you.

The task

In the experiment, there are three types of roles, Player A, Player B and Player C. You will be randomly assigned as one of the roles with equal chances. Your role will be fixed throughout the experiment.

The game

This experiment consists of a basic game where the decisions of Player A and Player B determines the payment of each other. The details of the basic game is as follows.

Step 1: Player A chooses between **In** and **Out**.

If Player A chooses **Out**, then Player A gets 6, Player B gets 0, and that's the end of the game.

If Player A chooses **In**, the game continues to Step 2.

Step 2: After Player A has chosen **In**, Player B makes a proposal to divide 10 euro between Player A and Player B.

Player B can make a proposal to give X to Player A, and keep 10-X for himself.

Step 3: Player A decides whether to **accept** or to **reject** Player B's proposal.

If Player A **accepts**, Player A gets X, Player B gets 10-X.

If Player A **rejects**, both Player A and B gets 0.

Player A and Player C

However, in this experiment, Player A **CANNOT** make the decision by himself in Step 3. Instead, Player A must hire a Player C to decide whether to accept or to reject Player B's proposal on his behalf. Before Step 1, there is a Step 0 in which Player A will set a payment scheme determining how Player C will be paid

(The details of how Player A should set the payment scheme is explained below). The payment scheme is announced to all three players. Player A will then choose between **In** and **Out**. If Player A chooses **In**, Player B makes a proposal to divide 10 between Player A and Player B. Then Player C decides on behalf of Player A whether to **accept** or **reject** the proposal.

Step 0: Player A sets the payment scheme for Player C by filling in the following form on the screen. In each column, Player A must fill in a value from 0 to

If you get	0	1	2	3	4	5	6	7	8	9	10
Player C gets											

10 of how much to pay Player C if in the end Player C gets for Player A the corresponding amount. For example, in the grid below 0, Player A must fill in how much he wants Player C to get if Player C gets for him 0 in the game, etc.

The money used to pay Player C comes from an extra budget of 10 euro. It shall only be used to pay Player C. If Player A fills in less than 10 in the form, the unused amount is lost. How much Player A fills in the form will not affect Player A's own payment.

The payment scheme is announced to Player A, Player B, and Player C after it is set.

Procedure

There will be one practice round and 20 rounds that count towards your final payment. As the experiment begins, you will be randomly assigned one of the three roles. Your role will be kept fixed throughout the experiment. At the beginning of each round, you will be randomly matched with two other players to play the game step by step. At the end of each round, you will be informed of the other players' decisions and your payment in that round. After all 20 rounds, you will fill in a questionnaire.

Payment

At the end of the experiment, one of the 20 rounds will be randomly selected for your payment. In addition, you will receive a participation fee of **3 Euro**. The payments will be made in cash to you at the end of the experiment.

Summary

- There are one practice round and 20 rounds that count towards your payment.
- You will be randomly assigned a role of Player A, Player B, or Player C. The role is kept fixed throughout the experiment
- You will be randomly matched with two other players at the beginning of each round.

- In each round, Player A first sets a payment scheme for Player C. The payment scheme is announced to all three players.
- Player A chooses between In and Out
- If Player A chooses Out, Player A gets 6, Player B gets 0, and Player C gets the corresponding amount for if Player A gets 6 according to the payment scheme. The game ends for that round.
- If Player A chooses In, the game continues. Player B makes a proposal to divide 10 between Player A and himself.
- Player C decides whether to accept or to reject the proposal.
- If Player C accepts, Player A and Player B each get according to the proposal by Player B, and Player C gets the corresponding amount for Player A's actual payment according to the payment scheme.
- If Player C rejects, both Player A and Player B get 0, and Player C gets the corresponding amount for if Player A gets 0 according to the payment scheme.
- You will learn other players' decisions and your payment in that round at the end of each round.
- At the end of the experiment, one of the 20 rounds is randomly selected for payment.

You can now go over the instructions on your own and ask clarifying questions (if any). Please raise a hand if you have a question.
Please be reminded that you are not allowed to communicate with other participants throughout the experiment.

4.8.2.2 Instruction for Treatment B

Welcome to the experiment. We will first go over the instructions together. After that, you will be given some time to read the instructions at your own pace and ask questions. Please do not write on the instructions. If you need to take notes, you can use the extra blank paper.
During the experiment, you will interact with other participants in this room and make some decisions. The decisions are anonymous and will not be linked to your identity. Your payment from the experiment depends on your decisions, the decisions of other participants, and chance. In addition, there is a participation fee of **3 Euro**. You will receive your final payment at the end of the experiment in cash.
Please be quiet during the experiment and do not talk with any other participants. If you have a question, please raise your hand and an experimenter will come to you.

The task

In the experiment, there are three types of roles, Player A, Player B and Player C. You will be randomly assigned as one of the roles with equal chances. Your role will be fixed throughout the experiment.

The game

"This experiment consists of a basic game where the decisions of Player A and Player B determines the payment of each other. The details of the basic game is as follows.

Step 1: Player A chooses between In and Out.

If Player A chooses Out, then Player A gets 6, Player B gets 0, and that's the end of the game.

If Player A chooses In, the game continues to Step 2.

Step 2: After Player A has chosen In, Player B makes a proposal to divide 10 euro between Player A and Player B.

Player B can make a proposal to give X to Player A, and keep 10-X for himself.

Step 3: Player A decides whether to accept or to reject Player B's proposal.

If Player A accepts, Player A gets X, Player B gets 10-X.

If Player A rejects, both Player A and B gets 0.

Player B and Player C

However, in this experiment, Player B **CANNOT** make the decisions himself. Instead, Player B must hire a Player C to make all the decisions on his behalf. Before Step 1, there is a Step 0 in which Player B will set a payment scheme determining how Player C will be paid (The details of how Player B should set the payment scheme is explained below). The payment scheme is announced to all three players. Player A will then choose between **In** and **Out**. If Player A chooses **In**, Player C makes a proposal to divide 10 between Player A and Player B on behalf of Player B. Then Player A decides whether to **accept** or **reject** the proposal.

Step 0: Player B sets the payment scheme for Player C by filling in the following form on the screen. In each column, Player B must fill in a value from 0 to 10 of

If you get	0	1	2	3	4	5	6	7	8	9	10
Player C gets											

Table 4.7: Incentive scheme for the agent

how much to pay Player C if in the end Player C gets for Player B the corresponding amount. For example, in the grid below 0, Player B must fill in how much he wants Player C to get if the Player C gets for him 0 in the game, etc.

The money used to pay Player C comes from an extra budget of 10 euro. It shall

only be used to pay Player C. If Player B fills in less than 10 in the form, the unused amount is lost. How much Player B fills in the form will not affect Player B's own payment.
The payment scheme is announced to Player A, Player B, and Player C after it is set.

Procedure
There will be one practice round and 20 rounds that count towards your final payment. As the experiment begins, you will be randomly assigned one of the three roles. Your role will be kept fixed throughout the experiment. At the beginning of each round, you will be randomly matched with two other players to play the game step by step. At the end of each round, you will be informed of the other players' decisions and your payment in that round. After all 20 rounds, you will fill in a questionnaire.

Payment
At the end of the experiment, one of the 20 rounds will be randomly selected for your payment. In addition, you will receive a participation fee of **3 Euro**. The payments will be made in cash to you at the end of the experiment.

Summary

- There are one practice round and 20 rounds that count towards your payment.
- You will be randomly assigned a role of Player A, Player B, or Player C. The role is kept fixed throughout the experiment
- You will be randomly matched with two other players at the beginning of each round.
- In each round, Player B first sets a payment scheme for Player C. The payment scheme is announced to all three players.
- Player A chooses between In and Out
- If Player A chooses Out, Player A gets 6, Player B gets 0, and Player C gets the corresponding amount for if Player B gets 0 according to the payment scheme. The game ends for that round.
- If Player A chooses In, the game continues. Player C makes a proposal to divide 10 between Player A and Player B on behalf of Player B.
- Player A decides whether to accept or to reject the proposal.
- If Player A accepts, Player A and Player B each get according to the proposal by Player C, and Player C gets the corresponding amount for Player B's actual payment according to the payment scheme.
- If Player A rejects, both Player A and Player B get 0, and Player C gets

the corresponding amount for if Player B gets 0 according to the payment scheme.

- You will learn other players' decisions and your payment in that round at the end of each round.
- At the end of the experiment, one of the 20 rounds is randomly selected for payment.

You can now go over the instructions on your own and ask clarifying questions (if any). Please raise a hand if you have a question.
Please be reminded that you are not allowed to communicate with other participants throughout the experiment.

Chapter 5

Receiving credit: On delegation and responsibility[1]

5.1 Introduction

The notion that the responsibility for unattractive decisions can be shifted to others has a long tradition. "Princes ought to leave affairs of reproach to the management of others," Machiavelli (1532) wrote in The Prince (Chapter XIX). The effectiveness of blame-shifting is illustrated in economic experiments by Bartling and Fischbacher (2012). They show that decision-makers can avoid being punished for an unfair decision by delegating the decision to another person. Machiavelli (1532) also proposed that favorable decisions should not be delegated: "Princes ought to keep affairs of grace in their own hands". In the current paper, we study the effectiveness of this latter strategy. Do decision-makers receive more credits when making favorable decisions themselves rather than delegating them to others?

To address this question we conduct economic experiments that directly build on the game implemented by Bartling and Fischbacher (2012). The first player (the dictator) can decide between an equal (fair) and an unequal (unfair) allocation that determines the payoffs of four players. Instead of making the decision herself, she can also pass the decision to the second player (the delegate) who then chooses between the two allocations. The monetary payoffs of the first and second players are perfectly aligned. The fair allocation divides the total amount equally among all four players, whereas the unfair allocation gives higher payoffs to the first and the second players but lower payoffs to the other two players (the receivers). In Bartling and Fischbacher (2012) the receivers can decide to punish the dictator or the delegate, or both. For the sake of replication, we copy this design in one of

[1]This chapter is adapted from joint work with Jan Potters and Cédric Argenton.

our treatments. Our contribution is to add treatments in which the receivers can decide to reward rather than punish the dictator or the delegate, or both. This allows us to explore whether credit taking has similar effects as blame avoidance.

Bartling and Fischbacher (2012) propose a model of responsibility attribution to explain how the receivers will punish the dictator, the delegate, or both, in case the unfair allocation is chosen and how this depends on who made the final decision. We show that the model extends naturally to the case of rewards. If the fair allocation is chosen, responsibility is attributed in proportion to which player—dictator or delegate—contributed more to this outcome occurring. The model predicts that the dictator will be rewarded more when choosing the fair allocation herself as compared to the case that the fair allocation is chosen by the delegate. The model also predicts that in the latter case the delegate is rewarded more than the dictator. One can say that the responsibility model predicts that the rewards in response to fair allocations are a mirror image of the punishments in response to unfair allocations.

Whether this symmetry (or mirroring) of rewards and punishments will hold empirically is less obvious than it may seem. There is evidence that punishment in response to "bad" behavior is stronger and more prevalent than rewarding in response to "good" behavior (Offerman, 2002; Croson and Konow, 2009; Kube et al., 2013). It cannot be ruled out that responsibility attribution also exhibits asymmetric patterns and causes the incentives to shift blame to be different from those to take credit. In fact, in political science, it is often argued that politicians and legislators have much stronger incentives to avoid blame than to claim credit. This is sometimes attributed to a "negativity bias" on the part of voters and public opinion which is rooted in the asymmetric treatment of gains and losses (Weaver, 1986). Our experiment allows us to explore any asymmetric patterns of responsibility attribution.

We find evidence in support of the responsibility attribution model in the presence of either punishments or rewards. In our treatment with punishment, the dictator's punishment decreases if the dictator delegates and the delegate chooses the unfair allocation instead of the dictator directly choosing the unfair allocation. However, the dictator is still punished for indirectly contributing to the unfair outcome. In our main treatment with rewards, the dictator receives a lower reward if the dictator delegates and the delegate chooses the fair allocation instead of the dictator directly choosing the fair allocation. The main departure from this model is that in our reward treatments, both the dictator and the delegate get a positive reward, independently of the outcome and of whether they make a decision or not. We show, however, that once a "baseline" level of reward is accounted for, departures from that level, which can then be interpreted as either rewards or punishments, are again in line with responsibility attribution.

In view of the observed patterns of rewards and punishments in the experiment, one would expect that the presence of rewards makes it more attractive for dictators to choose the fair allocation and less attractive to delegate and that the presence of punishments makes it less attractive for dictators choose the unfair allocation and more attractive to delegate. Interestingly, we find no support for these predictions when comparing the treatments with rewards or punishments to a treatment in which receivers have no sanction possibilities. The rate at which dictators delegate decisions is almost constant across the treatments. The same holds for the rate at which dictators choose the fair allocation. This rate is also higher than in Bartling and Fischbacher (2012). The dictators, as well as the delegates, in our experiment are rather fair-minded, and they do not respond much to (or do not anticipate) the decisions by the receivers.

Various experimental studies provide evidence that people choose to delegate decisions to an agent when they want to reach an unfair outcome. When delegating the decision-making to another player is provided as an option, the delegation rate ranges from 17% to 73% in experiments with allocation games such as the dictator game (Hamman et al., 2010; Coffman, 2011; Bartling and Fischbacher, 2012; Choy et al., 2016; Gawn and Innes, 2019b) or the ultimatum game (Fershtman and Gneezy, 2001). Delegation is also observed for other types of unfavorable tasks such as communicating bad information (Garofalo and Rott, 2018), lying (Erat, 2013; Kandul and Kirchkamp, 2018; Gawn and Innes, 2019a), or bribery (Drugov et al., 2014). One common observation in the various delegation games is that delegation leads to less punishment for the principal when the unfavorable decision is made by the delegate than when it is made directly by the principal. In ultimatum games, unfair offers are found to be accepted more frequently when they are made by a delegate than when they are directly made by the principal (Fershtman and Gneezy, 2001; Choy et al., 2016). In delegated dictator games, the principal receives lower punishment when the unfair decision is chosen by a delegate (Coffman, 2011; Bartling and Fischbacher, 2012; Oexl and Grossman, 2013). Bartling and Fischbacher (2012) compared outcome, intention, and responsibility as motives for punishment, and find changes in responsibility attribution as an explanation for the punishment assignment outcomes. Delegation can shift the responsibility from the principal to the delegate and thus leads to less punishment for the principal. This blame-shifting pattern is also observed when the delegate is powerless and have zero influence on the probability of the fair allocation being chosen (Oexl and Grossman, 2013). On the other hand, Coffman (2011) fails to establish a causal link between the decrease in punishment with delegation and diffusion of responsibility, but argues that it is the distance caused by indirect interaction that reduces punishment.

Our paper contributes to the literature by providing evidence for rewarding patterns with the delegation of decisions, and making a comparison between rewarding patterns and punishment patterns under comparable designs. Existing studies of reward and positive delegation are limited. Coffman (2011) conducts a donation experiment where the donation choice is framed as either direct donation to the recipient or delegated donation via an NGO and finds that donations that are framed as NGO-mediated are rewarded much less. However, the difference is only in framing. Coffman (2011) cannot distinguish between the effect of responsibility diffusion and distance. Eisenkopf and Fischbacher (2015) conduct a delegated trust game allowing for variations of delegation's influence on the final outcome. In their design, delegation generates an efficiency gain, the size of which differs across treatments. They find that while the direct decision-maker is rewarded more than the indirect decision-maker, trustees do not take into account the efficiency gains of delegation when rewarding the principal. They vary the size of the efficiency gain for delegation, but do not vary the probability that the delegate can influence whether the fair allocation will be selected. As a result, their results do not provide a contrast with previous studies on the delegation of unfavorable decisions. Our paper compares punishment and reward under the same design as Bartling and Fischbacher (2012) with variations in the degree of delegation's influence on the probability of the fair outcome being chosen. It thus provides a crisp comparison of responsibility attribution under punishments and rewards.

Our paper is also related to experimental papers studying responsibility attribution in gift-exchange games. These studies investigate the worker's effort provision in gift-exchange games when the wage-setting decision is made by the employer or delegated to either a neutral agent, a random device, or an agent whose payoff is related to the employer's. Charness (2000) finds that workers in a gift-exchange game respond to identical levels of wages with lower efforts when the wages are set by a neutral agent than when the wages are determined by a random device, suggesting a possible "responsibility-alleviation" effect. Charness (2004) and Maximiano et al. (2013) find that workers respond with lower efforts when a low wage is "intentionally" set by the employer than when it is delegated to either a random device or a neutral agent, which is similar to "punishing" unfavorable wages when they are directly made by the employer than when they are delegated. They also find that workers' effort provision in response to high levels of wages is higher when the wage is set by the employer than when it is delegated. Similar results are observed when the agent's payoff is proportional to the employer's payoff instead of being purely neutral (Maximiano et al., 2013).

The rest of the paper is organized as follows. Section 5.2 explains the experimental design and procedural details. Section 5.3 predicts reward and punishment patterns based on the responsibility attribution model. Section 5.4 reports the behavioral patterns in reward and punishment as well as the delegation and allocation decisions. Section 5.5 provides explanations for the observed deviations from predictions in treatments with reward. Section 5.6 provides concluding remarks.

5.2 Experimental Design

In the experiment, we conduct a delegated dictator game with a similar design as Bartling and Fischbacher (2012). There are three roles in the game: dictator (Player A), delegate (Player B), and recipient (Player C). Four players are matched together to form one group comprising one A, one B, and two Cs. A decides the allocation of 20 points among the four players by choosing between two different allocation options[2]: 1) assigning 5 points to each player (the fair allocation) and 2) assigning 9 points each to A and B and 1 point each to both Cs (the unfair allocation). A has the option to delegate the allocation decision to B. If A chooses to delegate, B makes the allocation decision on behalf of A. The material interests of A and B are aligned and opposite to Cs'.

The main variation of treatment is whether the recipients can reward or punish the dictator and the (potential) delegate. In the baseline treatment (*Baseline*), we implement the delegated dictator game without reward or punishment. Cs cannot make any decisions once A or B has chosen the allocation. In one treatment (*D&P*), we follow the design of Bartling and Fischbacher (2012) by allowing the punishment option. Cs are given the option to reduce the payoffs of other players at a fixed cost and one of their plans is randomly selected to be implemented. C can choose to reduce the payoffs of either A, B, the other C, or all three players. If the selected C chose to reduce payoffs, one point is deducted from his own payoff. The total number of points to be deducted from the three players must not exceed seven. The final payoff of any player should not be below zero.

In order to study responsibility attribution in the positive direction, we run a treatment with the option for Cs to reward A and/or B in the same delegated dictator game (*D&R*). The design of the *D&R* treatment mirrors that of the *D&P* treatment. Cs are given the option to increase the payoffs of A and B by assigning extra points. They can choose to assign up to a total of seven points to either A, B, or both. One of the two reward plans is randomly selected to be implemented.

[2]The two options are referred to as the fair option and the unfair option respectively by Bartling and Fischbacher (2012).

There are two differences from the way punishment is implemented in the *D&P* treatment. First, rewarding is costless. Choosing to assign points does not reduce the payoff of the selected C. Second, the selected C cannot choose to assign extra points to the other C. The reasons for those design choices are as follows. In the presence of costly rewards and social preferences, the selected C might have refrained from giving rewards in the case of a favorable outcome, as this would take the final allocation away from equality (In *D&P*, punishing decision-makers after observing an unfair outcome moved the allocation toward equality). In the case of an unfair outcome, the selected C may want to use her reward points to re-balance the allocation in favor of the other C. (In *D&P*, punishing the other C only distorted the allocation further.) In addition, costless reward can better represent psychological credit attribution, which also does not incur any additional monetary costs.

In order to compare the reward of delegated decisions with that of pure allocation decisions, we include a treatment with the reward option but no delegation (*NoD&R*). In this treatment, A chooses between the two allocations, while B does not make any decision. One of the two Cs is randomly selected with an option to assign extra points to either A, B, or both, using the same rewarding mechanism as in the *D&R* treatment.

We conduct two additional variants of the *D&R* treatment: *Asymmetric* and *Random*, as Bartling and Fischbacher (2012) did with their *D&P* treatment. In the *Asymmetric* treatment, if A chooses not to delegate, she can only choose the unfair allocation. If A delegates the decision, B can choose between the fair and the unfair allocations. In this treatment, delegation by A can unambiguously be interpreted as a 'kind' intent by A. In the *Random* treatment, instead of delegating to B, A can choose to delegate to a die which selects between the two allocations with the same probability as the Bs do in the *D&R* treatment. The goal of these two treatments is to 1) identify the impact of different reward motives, and 2) offer a parallel comparison with the corresponding treatments with punishment in Bartling and Fischbacher (2012). The details are discussed in Section 5.3.

A summary of all six treatments is shown in Table 5.1.

Table 5.1: Summary of treatments

	A delegates	C can punish	C can reward	A can be fair	Subjects
Baseline	To B	No	No	Yes	202
D&P	To B	Yes	No	Yes	202
D&R	To B	No	Yes	Yes	201
NoD&R	No	No	Yes	Yes	201
Asymmetric	To B	No	Yes	No	200
Random	To die	No	Yes	Yes	204

The experiment was conducted using the strategy method. In each treatment, Bs have to decide which allocation to choose before they know whether A has chosen to delegate or not. Both Cs have to decide on how many punishment or reward points to assign for all possible scenarios before they know the decisions of A or B and before they know whether their decisions are selected to be implemented. In the *NoD&R* treatment, there are two possible scenarios: A choosing the fair allocation or A choosing the unfair allocation. In treatments with delegation (the *D&R*, *D&P*, *asymmetric*, and *random* treatments), there are four possible scenarios: A choosing the fair allocation and not delegating, A choosing the unfair allocation and not delegating, A choosing to delegate and B choosing the fair allocation, and A choosing to delegate and B choosing the unfair allocation.

The experiment was approved by the TiSEM Institution Review Board (IRB EXE 2020-011) and pre-registered at the AEA RCT Registry (AEARCTR-0006036). The experiment was conducted as an online experiment on the online platform Prolific (Palan and Schitter, 2018) in June 2020. The *Baseline*, *D&R*, *D&P*, and *NoD&R* treatments were conducted first. The *asymmetric* and *random* treatments were conducted after we finished collecting data for the above four treatments, so that the average probability of Bs choosing between the fair and the unfair allocation in the *D&R* treatment could be implemented for the die in the *Random* treatment. In order to keep the demographic features of subjects as similar as possible to those of Bartling and Fischbacher (2012) (who dealt with university students), we imposed the following restrictions on the subject pool: 1) subjects' age should be between 18 and 30; 2) subjects should be students; 3) the highest level of education completed should be at least high school; 4) subjects should be US residents. We took measures to minimize the gap in control between the online experiment and the lab experiment. We implemented an additional recruitment criterion that the subject's past submissions on the Prolific platform should receive an approval rate of at least 90%.[3] We kept our instructions as close to those of Bartling and Fischbacher (2012), with modifications based on design differences and adjustments for the online survey. Subjects were requested to complete the same set of practice questions as in Bartling and Fischbacher (2012) to improve their understanding of the instructions. The questions were changed into multiple-choice questions to adapt to online submissions. Subjects were only allowed to proceed if they chose the correct answer at the first attempt. The instructions for the three roles in the *D&R* treatment are shown in Appendix 5.7.1. The experiment took around 15 minutes on average. The experimental points were

[3]Submissions on the Prolific platform are rejected if the researcher has valid reasons indicating the subject was being negligent, *e.g.* the completion time was exceptionally short, crucial questions were skipped, or the subject failed attention checks.

converted to US dollars using the exchange rate of 5 points = \$1.[4] In addition, each subject also received a fixed participation fee of \$1.1. For the *Baseline*, *D&R*, *D&P*, and *NoD&R* treatments, the average payment was around \$1.96. For the *asymmetric* and *random* treatments, the average payment was around \$2.03.

5.3 Predictions

Our primary interest lies in the reward and punishment behavior of the C players. Different motives for reward and punishment can give rise to different behavioral patterns. Several theories have been proposed to explain why and how agents would use punishments or rewards following the observation of decisions made by others. First, they could be averse to unequal or unfair outcomes and use punishments or rewards as a way to reach final payoff allocations that are more in line with their (social) preferences (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). Second, they could reciprocate "good" behavior by rewarding "kind" actions, and punishing "unkind" actions, taken by others (Rabin, 1993; Levine, 1998; Charness and Rabin, 2002; Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006). Third, they could use rewards and punishments to meet their assessment of whoever is responsible for the "fair" or "unfair" outcome they observe (Coffman, 2011; Bartling and Fischbacher, 2012). Bartling and Fischbacher (2012) propose responsibility attribution as a motive to impose punishment and find supporting evidence in their experiment. We apply their argument on responsibility attribution to the realm of favorable tasks delegation and rewards to establish our hypotheses, while keeping in mind that other motives, such as intention-based reciprocity or outcome-based social preferences may play a role.

We extend the model of Bartling and Fischbacher (2012) to include both positive and negative responsibility. Positive responsibility corresponds to a reward motive that is attributed to a player if her actions increased the probability of a favorable outcome. Negative responsibility corresponds to a punishment motive and is attributed to the player whose action increased the probability of an unfavorable outcome. In the context of the delegated dictator game in our experiment, in the four treatments with reward (*D&R*, *NoD&R*, *Asymmetric*, and *Random*), the player (A or B) who increases the probability of the fair allocation being chosen should be held responsible and receive a reward. On the other hand, in the *D&P* treatment, the player (A or B) who increases the probability of the unfair allocation should receive some punishment. The stronger the impact a player has on the probability, the higher the level of reward/punishment she receives.

[4] The payment was transferred to the subjects via the Prolific system. The final payment was in British pounds using the exchange rate automatically adopted by the system.

Formally, in the *D&P* treatment, following Bartling and Fischbacher (2012)'s annotation, let C's belief about the probability of A and B choosing the unfair allocation be denoted by α^- and β^- respectively, and the probability of A delegating the decision by δ. Thus, the *ex ante* probability of the unfair allocation is $\alpha^- + \delta\beta^-$. If the fair allocation is chosen, nobody increases the probability of the unfair allocation. In the case of punishments, responsibility is only attributed if the unfair allocation is selected. If A does not delegate and directly chooses the unfair allocation, A is the only player taking an action and responsible for all the probability increase. If A delegates and B chooses the unfair allocation, the post-decision probability of the unfair allocation is β^-. If $\alpha^- + \delta\beta^- < \beta^-$, both players contribute to the probability increase. A's share of the probability increase is $R_A^- = (\beta^- - \alpha^- - \delta\beta^-)/(1 - \alpha^- - \delta\beta^-)$, and B's share of the probability increase is $R_B^- = (1 - \beta^-)/(1 - \alpha^- - \delta\beta^-)$. If $\beta^- < (1 + \alpha^-)/(2 - \delta)$, B is more responsible than A, and *vice versa*. If $\alpha^- + \delta\beta^- > \beta^-$, by delegating, A reduces the probability of the unfair allocation and should not be held responsible, while B is fully responsible. The predicted responsibility of A and B in the *D&P* treatment as represented by their share of influence on the probability of the unfair outcome is shown in Table 5.2.

Table 5.2: Theoretical responsibility of A and B in the *D&P* treatment

	A unfair	A delegates B unfair	
		$\alpha^- + \delta\beta^- < \beta^-$	$\alpha^- + \delta\beta^- > \beta^-$
Responsibility of A	1	R_A^-	0
Responsibility of B	0	R_B^-	1

Note: $R_B^- > R_A^-$ if $\beta^- < (1 + \alpha^-)/(2 - \delta)$

Let $p_i(j, x)$ denote the punishment for i when the direct decision-maker who chooses the allocation is j and the chosen allocation is x, with $i, j = A, B$, $x = f$ for the fair allocation, and $x = u$ for the unfair allocation. We derive the following predictions regarding the punishment in the *D&P* treatment based on the responsibility attribution.

Hypothesis P 1. $p_A(\cdot, f) = p_B(\cdot, f) = 0$. *Punishment for both players is zero when the outcome is fair, independently of who made the decision.*

Hypothesis P 2. $p_A(A, u) > p_A(B, u)$, $p_B(B, u) > p_B(A, u)$. *When the chosen allocation is unfair, player i receives higher punishment when she is the direct decision-maker than when she is not.*

Hypothesis P 3. *1.* $p_A(A, u) > p_B(A, u) = 0$. *If A does not delegate and directly selects the unfair allocation, only A is punished.*

2. *$p_A(B,u) > p_B(B,u)$ if $\alpha^- + \delta\beta^- < \beta^-$ and $\beta^- > (1+\alpha^-)/(2-\delta)$, $p_B(B,u) >= p_A(B,u)$ otherwise. If A delegates and B selects the unfair allocation, B is punished more severely than A for some combinations of probabilities.*
3. *$p_A(B,u) \geq p_B(A,u)$, equality holds if $\alpha^- + \delta\beta^- \geq \beta^-$. Whether delegation completely reduces the punishment for A depends on the specific combinations of probabilities.*

Responsibility attribution in the treatments with reward takes on a mirroring pattern. Responsibility is only attributed when the chosen allocation is fair. Let C's belief of the probability of A and B choosing the fair allocation be denoted by α^+ and β^+, and the probability of A delegating by δ. In the *D&R* treatment, the *ex ante* probability of the fair allocation is $\alpha^+ + \delta\beta^+$. If A delegates, the *ex post* probability of the fair allocation is β^+. If $\alpha^+ + \delta\beta^+ < \beta^+$, both A and B are responsible for increasing the probability. A's share of the probability increase is $R^+_{A1} = (\beta^+ - \alpha^+ - \delta\beta^+)/(1-\alpha^+ - \delta\beta^+)$ and B's share $R^+_{B1} = (1-\beta^+)/(1-\alpha^+ - \delta\beta^+)$. If $\beta^+ < (1+\alpha^+)/(2-\delta)$, B has a larger impact on the probability increase than A, and *vice versa*. If $\alpha^+ + \delta\beta^+ > \beta^+$, A reduces the probability of the fair allocation by delegating, while B is fully responsible.

In the *Asymmetric* treatment, since A can only choose the unfair allocation if she does not delegate, the *ex ante* probability of the fair outcome is $\delta\beta^+$. If A delegates and B chooses the fair allocation, A's share in the probability increase is $R^+_{A2} = (\beta^+ - \delta\beta^+)/(1-\delta\beta^+)$ and B's responsibility is $R^+_{B2} = (1-\beta^+)/(1-\delta\beta^+)$. If $\beta^+ < 1/(2-\delta)$, B is more responsible for the probability increase than A, and *vice versa*. A's responsibility is always no less than the corresponding case in the *D&R* treatment, while B's responsibility is always no more than the corresponding case in the *D&R* treatment.

In the *Random* treatment, following Bartling and Fischbacher (2012)'s assumption that an individual's responsibility is not affected by moves of nature, if A delegates to the die which results in the fair allocation, only A takes full responsibility. The predicted responsibility of A and B as represented by their influence on the probability of the fair outcome is shown in Table 5.3.

Let $r_i(j,x)$ denote the reward for i in the *D&R* treatment when the direct decision-maker who chooses the allocation is j and the chosen allocation is x, with $i,j = A,B$, $x = f,u$. We have the following predictions about the reward pattern.

Hypothesis R 1. $r_A(\cdot,u) = r_B(\cdot,u) = 0$. *Reward for both players is zero when the outcome is unfair, independently of who made the decision.*

Hypothesis R 2. $r_A(A,f) > r_A(B,f)$, $r_B(B,f) > r_B(A,f)$. *When the chosen allocation is fair, player i a higher reward when she is the direct decision-maker than when she is not.*

Table 5.3: Theoretical responsibility of A and B in treatments with rewards

	A fair	A delegates B fair	
		$\alpha^+ + \delta\beta^+ < \beta^+$	$\alpha^+ + \delta\beta^+ > \beta^+$
D&R			
Responsibility of A	1	R^+_{A1}	0
Responsibility of B	0	R^+_{B1}	1
Asymmetric			
Responsibility of A	-	R^+_{A2}	0
Responsibility of B	-	R^+_{B2}	1
Random			
Responsibility of A	1	1	
Responsibility of B	0	0	

Note: $R^+_{B1} > R^+_{A1}$ if $\beta^+ < (1+\alpha^+)/(2-\delta)$; $R^+_{B2} > R^+_{A2}$ if $\beta^+ < 1/(2-\delta)$;
$R^+_{A2} > R^+_{A1}$; $R^+_{B2} < R^+_{B1}$

Hypothesis R 3. *1. $r_A(A,f) > r_B(A,f) = 0$. If A does not delegate and directly selects the fair allocation, only A is rewarded.*

2. $r_A(B,f) \geq 0$, $r_B(B,f) > 0$; $r_A(B,f) > r_B(B,f)$ if $\alpha^+ + \delta\beta^+ < \beta^+$ and $\beta^+ > (1+\alpha^+)/(2-\delta)$, $r_B(B,f) > r_A(B,f)$ otherwise. If A delegates and B selects the fair allocation, B receives a higher reward than A for some combinations of probabilities.

3. $r_A(B,f) \geq r_B(A,f)$, equality holds if $\alpha^+ + \delta\beta^+ \geq \beta^+$. Whether delegation completely reduces the reward for A depends on the specific combinations of probabilities.

Let $r_i^{AS}(\cdot,\cdot)$ denote the reward for player i in the *Asymmetric* treatment, and $r_i^{RD}(\cdot,\cdot)$ denote the reward for player i in the *Random* treatment. We have the following additional predictions comparing the three treatments.

Hypothesis R 4. $r_A^{RD}(A,f) = r_A^{RD}(die,f) > 0$, $r_B^{RD}(A,f) = r_B^{RD}(die,f) = 0$. *In the random treatment, delegating to a die does not affect the reward for any of the players.*

The responsibility attribution model predicts that, conditional on the outcome, A receives the same reward when she delegates to a random device as when she does not delegate. A pure "distance" effect would dilute A's responsibility in the event when the die would lead to a fair outcome. Intention-based reciprocity would not reward A for delegating while she could have chosen the fair allocation, while outcome-based social preferences would call for rewarding A and B in no circumstances (as it always takes the final payoff allocation away from equality).

Hypothesis R 5. $r_A(A,f) = r_A^{RD}(die,f) > r_A^{AS}(B,f) \geq r_A(B,f) \geq 0$, *the last two equality hold if* $\alpha^+ + \delta\beta^+ \geq \beta^+$; $r_B(B,f) \geq r_B^{AS}(B,f) > r_B^{RD}(B,f) = 0$, *the* *first equality holds if $\alpha^+ + \delta\beta^+ \geq \beta^+$. If A delegates and the outcome is fair, A's reward in the Random treatment is the highest among all three treatments. A's reward in the Asymmetric treatment is no less than in the D&R treatment. B's reward in the D&R treatment is no less than in the Asymmetric treatment.*

Delegating to another player decreases A's reward, but the reward for A in case of a fair outcome in the *Asymmetric* treatment is higher than that in the *D&R* treatment. The intention-based reciprocity model predicts the same comparison, whether the outcome is fair or not (as A's decision to delegate can unambiguously be interpreted as 'kind'). In contrast, in the case of a pure distance effect, delegation would lead to the dilution of A's responsibility. Again, outcome-based social preferences would call for rewarding A or B in no circumstances (as it always takes the final payoff allocation away from equality).

5.4 Results

We focus first on the punishment and reward decisions of Cs. We will compare our observations of C's punishment behavior in the *D&P* treatment to that in the corresponding treatment of Bartling and Fischbacher (2012). In our treatments with reward, we will compare results to the theoretical predictions. Next, we will look at delegation decisions by comparing treatments with punishment or rewards with our *Baseline* treatment.

5.4.1 Punishment decisions

The average punishment for each player under all four possible scenarios in the *D&P* treatment are shown in Table 5.4. The punishment patterns provide supporting evidence for our predictions based on the responsibility attribution model. Players are only substantially punished when the outcome is unfair. When punishment occurs, the direct decision-maker is most severely punished. Delegation effectively shifts responsibility and decreases A's punishment. The main findings of punishment patterns in the corresponding treatment of Bartling and Fischbacher (2012) are successfully replicated in our experiment.

Table 5.4: Average punishment in the *D&P* treatment

	A unfair	A delegate B unfair	A fair	A delegate B fair
A	1.96 (2.70)	1 (1.74)	0.14 (0.57)	0.13 (0.55)
B	0.64 (1.24)	1.69 (2.44)	0.19 (0.70)	0.13 (0.55)
Other C	0.05 (0.22)	0.05 (0.22)	0.12 (0.50)	0.05 (0.05)

Note: Standard errors are shown in parenthesis.

In our *D&P* treatment, punishment for all three players is close to zero when the outcome is fair. The other C, who has no impact on the final allocation, receives almost zero punishment regardless of the outcome. Consistent with our Hypothesis P1, the punishment for both A and B whenever the unfair allocation is selected is substantially above zero.

Result P 1. $p_A(\cdot, f) = p_B(\cdot, f) = 0$. *Both A and B only receives substantial punishment when the final outcome is unfair.*

When the outcome is unfair, we find that a given player receives higher punishment when she is the direct decision-maker who selects the allocation than when she is not, consistent with Hypothesis P2. A receives an average punishment of 1.96 points if she directly selects the unfair allocation, but receives only around 1 point when she delegates and B chooses the unfair allocation. Similarly, B receives an average punishment of 0.64 points if A directly selects the unfair allocation, but the punishment for B increases to 1.69 if the allocation is selected by B. Both differences are statistically significant, supported by a two-sided Wilcoxon signed-rank test with $p < 0.01$.

Result P 2. $p_A(A, u) > p_A(B, u)$, $p_B(B, u) > p_B(A, u)$. *When the outcome is unfair, player i's punishment is higher when she is the direct decision-maker than when she is not.*

Comparisons of the punishment levels between A and B exhibit a pattern that is consistent with Hypothesis P3. If A directly selects the unfair allocation, A's punishment (1.96) is significantly higher than B's punishment (0.64); if A delegates and B selects the unfair allocation, B's punishment (1.69) is significantly higher than A's punishment (1) ($p < 0.01$ in a two-sided Wilcoxon signed-rank test for both comparisons). We also find that the punishment for A (1) if A delegates and B chooses the unfair allocation is significantly higher than the punishment for B (0.64) if A directly chooses the unfair allocation ($p < 0.01$ in a two-sided Wilcoxon signed-rank test). Moreover, the punishment for B (1.69) if A delegates and B chooses the unfair allocation is not significantly different from the punishment for A (1.96) if A does not delegate and selects the unfair allocation himself ($p = 0.37$ in a two-sided Wilcoxon signed-rank test). Assuming that Cs anticipate the average decision rates, we can calculate the *ex ante* probabilities of the fair and unfair outcomes to determine the hypothesized responsibility attributed to A and B. With our observed delegation and allocation decisions of A and B as shown in Table 5.6 in Section 5.4.3, we estimate the probability of A being unfair to be $\hat{\alpha}^- = 0.5$, the probability of B being unfair to be $\hat{\beta}^- = 0.24$, and the probability of delegation to be $\hat{\delta} = 0.12$. The comparisons of the corresponding punishment between A and B provide confirming evidence for our Hypothesis P3.2 and P3.3, with these observed probabilities[5].

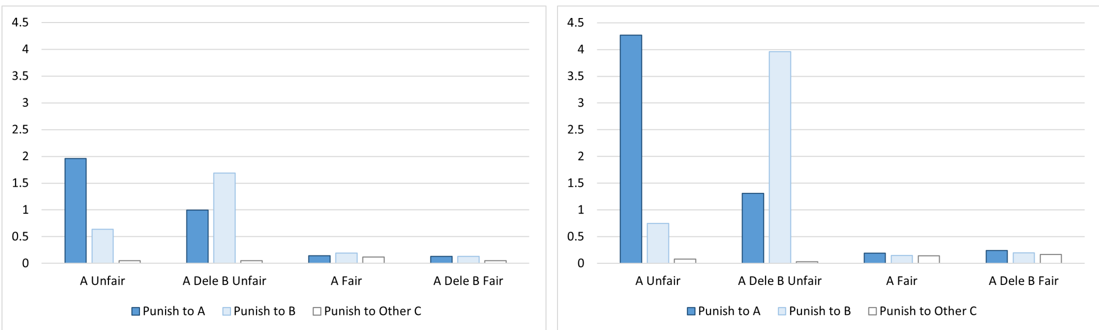
Result P 3. *1.* $p_A(A,u) > p_B(A,u) > 0$. *If A does not delegate and directly selects the unfair allocation, A receives more punishment than B.*

2. $p_B(B,u) > p_A(B,u)$. *If A delegates and B selects the unfair allocation, B receives more punishment than A.*

3. $p_A(B,u) > p_B(A,u) > 0$. *If A delegates and B selects the unfair allocation, A is still partly responsible for the unfair outcome.*

Our observations of punishment for A in the *D&P* treatment provide supporting evidence for the predictions based on responsibility attribution. Punishment for A decreases when she delegates compared to when she directly chooses the allocation. On the other hand, when A directly chooses the unfair allocation, B in our experiment still receives an average punishment of 0.64 point. This positive punishment for B when A directly chooses the unfair allocation cannot be explained by the responsibility attribution model, since B does not take any action and thus should not be attributed any responsibility. With the same logic, intention-based reciprocity does not provide an explanation either. This punishment can be partly explained by a desire to rectify the unfair outcome.

Figure 5.1: Average punishment in the *D&P* treatment



(a) Our experiment (b) Bartling and Fischbacher (2012)

Figure 5.1 shows the comparison between our observations and Bartling and Fischbacher (2012). The punishment pattern in our experiment as summarized above mimics the one in the corresponding treatment of Bartling and Fischbacher (2012). The only difference is that the level of punishment in our *D&P* treatment is generally lower than that in the corresponding treatment of Bartling and Fischbacher (2012). The highest level of average punishment is below 2 points in our experiment, while that in their corresponding treatment is almost as high as 4 points. Fewer Cs are willing to incur a cost to assign punishment points in our

[5] $\hat{\alpha}^- + \hat{\delta}\hat{\beta}^- = 0.59 < \hat{\beta}^-$, $(1+\hat{\alpha}^-)/(2-\hat{\delta}) = 0.8 > \hat{\beta}^-$.

experiment, and those who choose to punish also assign lower punishment on average. As mentioned in Section 5.2, our subjects are 18-30 year-old US nationals who are currently students and who have completed at least high school. We imposed these restrictions with the goal to approach the demographic characteristics of the university undergraduate subjects of Bartling and Fischbacher (2012) as closely as possible. However, two big differences still remain: 1) our subjects are US nationals currently residing in the US, while subjects of Bartling and Fischbacher (2012) resided in Switzerland; 2) Our experiment was conducted online while Bartling and Fischbacher (2012) ran the experiment in the laboratory. This difference in the level of average punishment could be a result of these differences.[6]

5.4.2 Reward decisions

We summarize the average reward for each player under all four possible scenarios in each treatment with reward in Table 5.5. Figure 5.2 shows the average reward for A and B in all possible scenarios in the *D&R* treatment (Figure 5.2a) and in the no-delegation benchmark *NoD&R* treatment (Figure 5.2b). Figure 5.3 shows the average reward for A and B in all possible scenarios in the *Asymmetric* treatment (Figure 5.3a) and the *Random* treatment (Figure 5.3b).

Table 5.5: Average reward for A and B in treatments with reward

	A unfair	A delegates B unfair	A fair	A delegates B fair
D&R				
A	0.53 (1.19)	1.67 (2.21)	3.24 (1.90)	2.13 (1.36)
B	1.72 (2.34)	0.62 (1.25)	2.1 (1.45)	3.21 (1.84)
NoD&R				
A	0.63 (1.23)	-	3.36 (1.52)	-
B	2.45 (2.78)	-	2.28 (1.24)	-
Asymmetric				
A	0.59 (1.22)	2.36 (2.65)	-	2.78 (1.26)
B	2.01 (2.51)	0.81 (1.45)	-	3.1 (1.36)
Random				
A	1.01 (1.71)	1.24 (1.71)	3.01 (1.95)	2.76 (1.66)
B	1.87 (2.42)	1.79 (2.18)	1.94 (1.41)	2.61 (1.56)

Note: In Column 2 and Column 4 of the last two rows, average reward when A delegates and the die gives the unfair/fair outcome is shown respectively. Standard errors are shown in parenthesis.

In all four treatments with the reward option, contrary to Prediction R1, both A and B receive a positive reward both when the selected allocation is fair and unfair, regardless of the direct decision-maker. In most treatments, given the direct

[6] Our experiment was conducted in June 2020, amid the Covid-19 pandemic, which could have impacted subjects' behavior.

decision-maker, the reward for each player is significantly higher with fair outcomes than with the corresponding unfair outcome, except for the reward for B in the *NoD&R* treatment and the *Random* treatment. These differences are statistically significant, supported by two-sided Wilcoxon signed-rank tests.[7] This cannot be explained by the responsibility attribution model which predicts reward to occur only when the outcome is fair and when a player has affected the likelihood of the fair outcome. It is worth noticing that B receives a positive reward even when B does not make any decision, and both when A directly selects the fair and the unfair allocation. None of the existing theories provide a reasonable explanation, either. Outcome-based social preferences do not explain why C would make the final payoff allocation when the outcome is unfair, while intention-based reciprocity does not call for rewarding B, who has not made any decision. We try to provide an explanation for this in Section 5.5.

Result R 1. $r_A(\cdot, u) > 0$, $r_B(\cdot, u) > 0$. *Both players receive a positive reward if the chosen allocation is unfair, regardless of who is the direct decision-maker.*

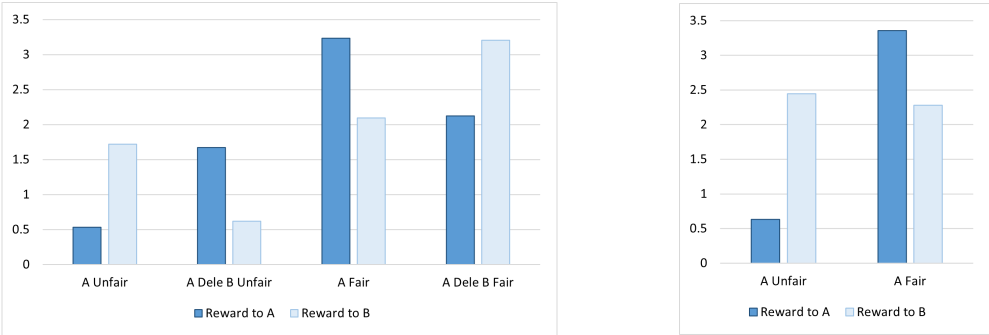
This being said, our *D&R* treatment shows a reward pattern symmetric to the punishment pattern observed in the *D&P* treatment. As predicted by Hypothesis R2, when the outcome is fair, a given player receives a significantly higher reward when she is the direct decision-maker than when she is not. As shown in the last two columns of the first panel in Table 5.5, A receives a reward of 3.24 when A directly selects the fair allocation, while she only receives a reward of 2.13 when she delegates and the fair allocation is chosen by B. Similarly, B's reward is 2.1 when A directly selects the fair allocation, while it is as high as 3.21 when A delegates and B chooses the fair allocation. Both pairwise comparisons are statistically significant, supported by a two-sided Wilcoxon signed-rank tests with $p < 0.01$

Result R 2. $r_A(A, f) > r_A(B, f)$, $r_B(B, f) > r_B(A, f)$. *When the outcome is fair, player i receives higher reward when she is the direct decision-maker than when she is not.*

The comparisons of reward between A and B in the *D&R* treatment also exhibit a pattern consistent with Hypothesis R3. If A directly selects the fair allocation, A's reward (3.24) is significantly higher than B's reward (2.1); if A delegates and B selects the fair allocation, B's reward (3.21) is also significantly higher than A's reward (2.13)($p < 0.01$ in a two-sided Wilcoxon signed-rank test for both comparisons). With our observed delegation and allocation decisions of A and B

[7] $p < 0.1$ when comparing A's reward if B chooses the fair allocation and if B chooses the unfair allocation after A delegates in the *Asymmetric* treatment; $p < 0.05$ when comparing B's reward if A directly chooses the fair allocation and if A chooses the unfair allocation; $p < 0.01$ for all other comparisons.

Figure 5.2: Average reward in *NoD&R* and *D&R* treatment



(a) *D&R* treatment (b) *NoD&R* treatment

as shown in Table 5.6 in Section 5.4.3, we estimate the probability of A being fair to be $\hat{\alpha}^+ = 0.63$, the probability of B being fair to be $\hat{\beta}^+ = 0.66$, and the probability of delegation to be $\hat{\delta} = 0.16$. Since $\hat{\alpha}^+ + \hat{\delta}\hat{\beta}^+ = 0.73 > \hat{\beta}^+$, the comparisons of the corresponding punishment between A and B are consistent with Hypothesis R3.2.

We find that A's average reward (2.13) when A delegates and B selects the fair allocation is not significantly different from B's average reward (2.1) when A directly selects the fair allocation ($p = 0.94$ in a two-sided Wilcoxon signed-rank test). Similarly, A's average reward (3.24) when A directly selects the fair allocation is also similar with the reward for B (3.21) when B is the direct decision-maker for the fair allocation. This pattern can be seen more clearly in Figure 5.2a. The reward patterns for A and B when A direct selects the fair allocation mirror the reward patterns for B and A when A delegates and B selects the fair allocation. With our estimated $\hat{\alpha}^+$, $\hat{\beta}^+$, and $\hat{\delta}$, since $\hat{\alpha}^+ + \hat{\delta}\hat{\beta}^+ = 0.73 > \hat{\beta}^+$, these comparisons are consistent with Hypothesis R3.3.

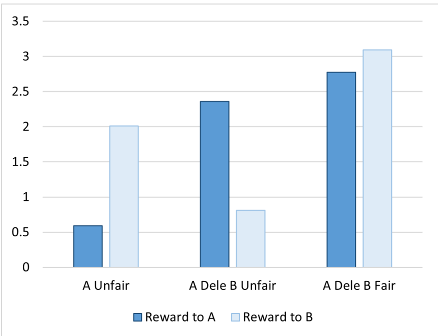
Result R 3.

1. $r_A(A, f) > r_B(A, f)$. *If A does not delegate and directly selects the fair allocation, A receives more reward than B.*
2. $r_B(B, f) > r_A(B, f)$. *If A delegates and B selects the fair allocation, B receives more reward than A. The mirrored result is also found when B selects the unfair allocation.*
3. $r_A(B, f) = r_B(A, f)$. *Given the allocation result, the indirect decision-maker in the two different scenarios receives the same level of reward.*

To summarize our findings in the *D&R* treatment, when looking at pairwise comparisons, we find confirming evidence for our predictions based on responsibility attribution. Rewarding patterns in the *D&R* treatment exhibit a mirror pattern to the punishment pattern in the *D&P* treatment. However, in that treatment, A and B subjects receive a positive reward even when the outcome is unfair, and B receives a positive reward when A does not delegate and B does not make any (consequential) decision.

An important deviation from our responsibility-based Hypothesis R1 is that subjects also receive a positive reward when the outcome is unfair. In the *D&R* treatment, these rewards when the outcome is unfair also exhibit a few patterns: 1) $r_A(A,u) < r_A(B,u)$, $r_B(B,u) < r_B(A,u)$. When the outcome is unfair, a player receives a significantly lower reward when she is the direct decision-maker than when she is not. 2) $r_A(A,u) < r_B(A,u)$. If A does not delegate and directly selects the unfair allocation, A receives less reward than B. 3)$r_B(B,u) < r_A(B,u)$. If A delegates and B selects the unfair allocation, B receives less reward than A. 4)$r_A(B,u) = r_B(A,u)$. When the outcome is unfair, we also found that the indirect decision-maker receives similar levels of reward. The rewards when the outcome is unfair exhibit a mirror image to the rewarding patterns when the outcome is fair as summarized in Results R2 and R3.

Figure 5.3: Average reward in *Asymmetric* and *Random* treatment



(a) *Asymmetric* treatment

(b) *Random* treatment

In the *Asymmetric* (Figure 5.3a) treatment and the *Random* treatment (Figure 5.3b), we observe a pattern reminiscent of the one in *D&R* treatment, as well as some new findings. In the *Asymmetric* treatment, each player receives a significantly lower reward when she directly selects the unfair allocation than when she is not the direct decision-maker ($p < 0.01$ in one-sided Wilcoson signed-rank tests). The reward for A and B when A directly selects the unfair allocation mirrors the reward for B and A when A delegates and B selects the unfair allocation. A's reward when A delegates and B selects the unfair allocation (2.36) is slightly higher than the reward for B when A directly selects the unfair allocation (2.01), but the difference is not statistically significant ($p = 0.18$ in a Wilcoxon signed-rank test). Comparing the *Asymmetric* treatment with the *D&R* treatment, A receives 2.78 points of reward when A delegates and B selects the fair allocation in the *Asymmetric* treatment, which is significantly higher than in the *D&R* treatment (2.13) ($p < 0.01$ in a one-sided Mann Whitney u test). A's reward (2.36) when A delegates and B selects the unfair allocation in the *Asymmetric* treatment is also mildly significantly higher ($p < 0.1$ in a one-sided Mann Whitney u test) than in the *D&R* treatment (1.67). This points to the fact that A's action space tends to affect the reward she received. Cs take into account the fact that delegating is the only possibility to reach the fair outcome and raise the reward for A if A delegates. This pattern is predicted by the responsibility attribution model. It is also compatible with intention-based reciprocity. However, it clearly violates any outcome-based theory of retribution.

Result R 4. $r_A^{AS}(B, \cdot) > r_A^{AS}(A, u)$. $r_A^{AS}(B, \cdot) > r_A(B, \cdot)$. *In the Asymmetric treatment, delegation increases A's reward, compared with the case when A does not delegate.*

In the *Random* treatment, A receives a significantly higher reward when the outcome is fair than when the outcome is unfair, both when A directly makes the allocation decision and when A delegates ($p < 0.01$ in one-sided Wilcoson signed-rank tests). Being the direct decision-maker does not have a significant impact on the level of rewards in the *Random* treatment. When the final allocation is unfair, the reward for A does not differ significantly if A directly makes the decision (1.01) or if A delegates the decision (1.24) ($p = 0.16$ in a two-sided Wilcoxon signed-rank test). When the final allocation is fair, the reward for A if A delegates and B selects the fair option is as high as 2.76, also not significantly different from A's reward (3.01) if A directly selects the fair option ($p = 0.22$ in a two-sided Wilcoxon signed-rank test). In addition, when A delegates and the fair allocation is subsequently selected, A receives a significantly higher reward in the *Random* treatment (2.76) than in the *D&R* treatment (2.13) ($p < 0.01$ in a one-sided Mann-Whitney u test). This provides confirming evidence for our predictions, indicating that delegating to a random device does not dilute A's responsibility.

Result R 5. $r_A^{RD}(A, x) = r_A^{RD}(die, x), x = u, f$. *Given the allocation result, delegating to a random device does not have a significant impact of the level of reward for A.*

5.4.3 Delegation decisions

Table 5.6 summarizes the delegation and allocation decisions of A and B in all treatments. A large proportion of both As and Bs choose the fair allocation, which is consistent with the common finding from previous dictator games (Engel, 2011). Compared with previous binary choice dictator games, our subjects exhibit a higher degree of generosity. In the treatments where B is asked to choose between the two allocations, the fraction of Bs who choose the fair allocation ranges between 66% and 76%. In the treatments where As are asked to choose among the two allocations and delegation, the fraction of A who selects the fair allocation ranges between 44% and 60%.

We do not find any significant effect of adding a punishment option or a reward option on the delegation and the allocation decisions. In the *D&P* treatment, there is a slight decrease of 13 percentage points in the fraction of As who select the unfair allocation and a slight increase of 10 percentage points in the fraction of Bs who select the fair allocation compared with the *Baseline* treatment. Around 10% of As choose to delegate in the *Baseline* treatment, while the fraction slightly increases to 16% in the *D&R* treatment and 12% in the *D&P* treatment. However, neither of the difference is significant ($p = 0.62$ in a Fisher's exact test comparing *D&R* with *Baseline*; $p = 0.29$ in a Fisher's exact test comparing *D&P* with *Baseline.*). Compared with the delegation rate of 17% in the no punishment treatment and 55% in the *D&P* treatment of Bartling and Fischbacher (2012), the rate of delegation is relatively low in our experiment. It is also much lower compared with other studies on delegated dictator games (38% in Oexl and Grossman (2013), 40% in Hamman et al. (2010), and 22.2% in Gawn and Innes (2019b)) and delegated ultimatum games (73% in Fershtman and Gneezy (2001) and 38% in Choy et al. (2016)).

Table 5.6: Delegation and allocation decisions of A and B

	Unfair (%)	Fair (%)	Delegate (%)	Observations
Baseline				
A	31	60	10	52
B	34	66	-	50
D&P				
A	44	44	12	52
B	24	76	-	50
D&R				
A	31	53	16	51
B	34	66	-	50
NoD&R				
A	28	72	-	50
Asymmetric				
A	28	-	72	50
B	33	67	-	50
Random				
A	28	56	16	50

It is worth asking whether those low delegation rates align with expected payoff maximization for A. On the basis of treatment averages, A does not profitably shift the blame in the *D&P* treatment. She can expect to pocket 7.04 if she directly chooses the unfair allocation, against 5.79 if she delegates. In our experiment, given the lower levels of punishment and the large fraction of Bs who choose the fair allocation, it simply does not pay off for As to delegate in many cases.

In our treatments with reward, the low level of delegation is consistent with our finding that delegation decreases A's reward when the outcome is fair and that the majority of A and B choose the fair allocation. On the basis of treatment averages, in *D&R*, A pockets 8.24 if she directly chooses the fair allocation, while she can expect about 8.31 if she delegates. The same applies to the *Random* treatment (A's expected payoff is 8.58 if she delegates and 8.01 if she directly chooses the fair allocation). However, in all treatments, directly selecting the unfair allocation gives the highest expected payoff for A (9.53 in *D&R*, 9.59 in *Asymmetric*, 10.01 in *Random*). A majority of subjects are not maximizing the expected payoff and choose the fair allocation in our experiment.[8]

In the *NoD&R* treatment where there is no delegation option and A can only choose between the two allocations, around 72% of A chooses the fair allocation. In the *Asymmetric* treatment where A can only choose between directly selecting the unfair allocation and delegating, around 72% of A chooses the delegate. This amount is similar to the sum of the fraction of A who directly chooses the fair allocation and who chooses to delegate in the *Baseline* treatment, the *D&R* treatment, and the *Random* treatment. A similar pattern is also observed when comparing a direct dictator game and a delegated dictator game in Gawn and Innes (2019b). This finding implies that the dictators who delegate may come from the ones who would otherwise choose the fair allocation if the delegation option were not available.

5.5 Discussion

Our main observations of punishment behaviors in the *D&P* treatment provide confirming evidence for the predictions based on the responsibility-attribution model. Punishments are only (substantially) assigned when the unfair allocation is selected. The direct decision-maker receives more punishment. Delegation shifts some responsibility from A to B, but A is still held partially responsible for delegating to a B who selects the unfair final outcome. The only deviation from the responsibility-attribution model is that B gets punished when A directly chooses the unfair allocation. Such a deviation is also observed in Bartling and Fischbacher (2012). Neither responsibility attribution nor intention-based reciprocity provides

[8]It is also possible the fact that our subjects come from the Prolific subject pool may play a role. Peer et al. (2021) compare Prolific, MTurk, and other online panels, and find considerable differences in terms of subjects' comprehension, attention, and honesty across different platforms. However, there is not enough evidence exploring the difference of online experiments on Prolific and real-life lab experiments.

a reasonable explanation for this pattern. One possible explanation is that this is due to outcome-based social preferences, as punishing B may reduce the inequality of the payoff allocation. However, in our experiment as in Bartling and Fischbacher (2012), the payoff of C goes down by 1 upon punishing, whereas B's payoff is reduced by less than 1 on average. All in all, the findings in the *D&P* treatment provide supporting evidence for the responsibility attribution model, but also point to some possible residual role for outcome considerations. Other possible explanations include an "angry-at-the-world" state of mind on the receivers' side, which may create a lower bound of punishment when Cs are put in an unfavorable situation, *i.e.* receive the lower payoff in the unfair allocation. However, this deviation does not affect our main observations which are based on the comparison of punishment between A and B.

Our findings in the treatments with reward also offer considerable support for the predictions of the responsibility-attribution model. In the *D&R* treatment, the direct decision-maker receives more reward for a fair outcome. Delegation followed by a fair choice by B leads to a higher reward for B, but A is still held partially responsible for the outcome. The fact that the reward pattern is affected by the change in A's strategy set in our *Asymmetric* treatment and, in particular, the fact that A gets a higher reward when delegating to an unfair B in that treatment than in the *D&R* treatment, is consistent with both responsibility attribution and intention-based reciprocity. However, our observation that reward for A is similar whether A makes the decision herself or delegates to a random die in the *Random* treatment provides supporting evidence for the responsibility-attribution argument.

One important deviation from predictions in our treatments with reward is that both players receive a positive reward regardless of whether the outcome is fair or unfair. Outcome-based inequality aversion does not provide sufficient motives for reward, as any reward to A and/or B distorts the allocation further away from equality with both the fair or unfair outcome. Neither the responsibility attribution model nor the intention-based reciprocity model provides an explanation for this. Positive rewards for both players when A directly selects the fair outcome can be considered as an indication of C's preference for fairness. However, this cannot explain why both A and B are also rewarded even when the outcome is unfair.

An interesting observation in particular is that B receives a significantly higher reward than A when A directly selects the unfair allocation. When A does not delegate, B is a pure bystander who does not make any decision. This seems to indicate that B is rewarded as a bystander for doing nothing when A selects the unfair allocation. This cannot be explained by intention-based reciprocity (B does not do anything) or outcome-based social preferences (B already enjoys a high payoff when the unfair allocation is chosen and rewarding him makes the allocation even more unequal). It seems like B is rewarded for *not* being held responsible for an unfavorable outcome.

A possible interpretation is that Cs use the different levels of reward to indicate both reward and punishment. If we take a benchmark level of reward as a reference point, any reward below this level can be regarded as a punishment while any reward above this level can be regarded as an actual reward. In our treatments with reward, A and B are punished with a lower reward when the outcome is unfair and rewarded with a higher reward when the outcome is fair. One reasonable example of such a baseline is the average reward for B when A does not delegate both when the outcome is fair and unfair in the *D&R* treatment, *i.e.* 1.91.[9] Figure 5.4 shows the adjusted "punishment" with the unfair outcome and the adjusted "reward" with the fair outcome for each player, subtracting the reference value 1.91 from the average reward as shown in Figure 5.2a. One can see that the direct decision-maker receives a higher "reward" than the other player when the outcome is fair and is more severely "punished" than the other player when the outcome is unfair. The "reward" patterns with the fair outcome exhibit an exact mirror image with the "punishment" patterns with the unfair outcome. These results indicate that when using the different levels of reward for both punishing and rewarding, delegation reduces both punishment and reward in line with responsibility attribution.

We stress, however, that this interpretation is speculative at this point. Although it is appealing to think of C-subjects as using a baseline level of reward to be able to administer both (extra) rewards and punishments, our experiment was not designed specifically to test that hypothesis.

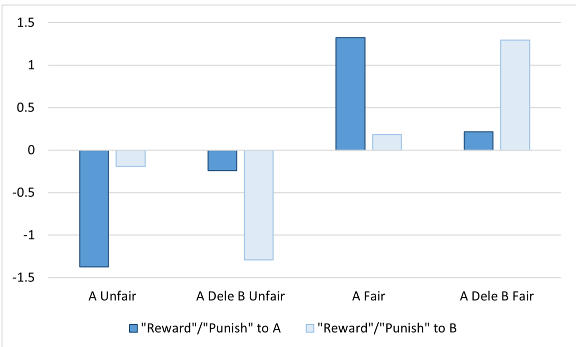


Figure 5.4: Adjusted "reward" and "punishment" in the *D&R* treatment

[9]The selection of this baseline level is speculative with the data we collected from our current experiment. We do not rule out the possibility of choosing other baseline levels. Other reasonable examples of such a level include the average reward for B in the *NoD&R* treatment, 2.36. Choosing other values of the reference point does not alter the main directions of our exercise.

5.6 Conclusion

We conduct an experiment of a delegated dictator game that allows a recipient to punish or reward the principal or the delegate. Our treatment with punishment replicates the main behavioral patterns of Bartling and Fischbacher (2012). We find confirming evidence for delegation effectively shifting the responsibility associated with negative tasks. We also find evidence for outcome-based punishment. In our treatments with reward, we find that delegation reduces both reward and punishment (in the form of lower levels of punishment compared with a benchmark level). On the other hand, recipients take into account the action space of the principal when assigning rewards. Delegating to a random device does not produce a distancing effect. We also find that subjects who delegate are those who would be fair without the delegation option.

Our results indicate that delegation reduces the credit associated with positive outcomes and the blame associated with negative outcomes by shifting the responsibility from the indirect decision-maker to the direct decision-maker. However, the credit-alleviation and the blame-shifting effect of delegation do not make it more cost-effective than directly choosing the allocation. In our experiment, we observe a lower proportion of delegation decisions compared with previous studies with delegated allocation games. The high proportion of subjects directly choosing the fair allocation may play a role.

5.7 Appendix

5.7.1 Instructions in the *D&R* treatment

Instructions for player A, B, and C in the *D&R* treatment, our main treatment are shown below. For each role, the instructions include five sections: Introduction, Instructions, Practice questions, Decision, and End of experiment. The Introduction, Practice questions, and End of experiment sections for all three roles are the same, and thus we only report them in Appendix 5.7.1.1. In Appendix 5.7.1.2 and 5.7.1.3, only the respective Instructions and Decision sections are reported.

5.7.1.1 Instructions for A

Introduction

Welcome!

The aim of this study is to understand people's decision making.

The estimated time to complete this study is 10 minutes. You will be asked to make a number of choices regarding a scenario on the next page. You will receive $1.1 for completing this study. In addition, you will receive a bonus payment that will be determined by your choices and other participants' choices in the study, within the range of $0 to $3.2, depending on the scenario.

Your responses will remain CONFIDENTIAL and will be used for scientific purposes only. If you have any questions regarding this study, please contact the researcher using the message function on Prolific. Thank you!

You will be matched with three other participants in this study. You will never learn of the identity of the three participants matched with you, nor will the three participants matched with you learn of your identity.

There are three types of participants in this study: participants A, B, and C. ONE participant A, ONE participant B, and TWO participants C will be matched together. You will be randomly assigned as one of the three roles. Your bonus payment depends on your decisions and the decisions of the other three participants matched with you in this study.

In the study, your payment will be calculated in points. The total number of points you earn during the study will be converted to dollars when we calculate your bonus payments. The following conversion rate applies: 5 Points = $1.

_____ I understand and agree with these instructions and would like to participate in this study.

_____ I do not agree with these instructions and would not like to participate.

Instructions

Please read the following instructions carefully. You can earn a bonus payment, depending on your decisions and those of the other participants, in addition to the $1.1 you receive for completing this study. It is thus very important that you read these instructions carefully.

You are a **participant A**.

The three other persons assigned to you are one participant B and two participants C.

In this study, either participant A or participant B decides how 20 points will be distributed among the four participants.

In distributing the points, participant A or B must decide between two possible allocations:

- Allocation 1: Participants A and B receive 9 points each and the two participants C receive 1 point each.
- Allocation 2: Participant A, participant B, and both participants C receive 5 points each.

As a participant A, you can either choose between allocations 1 and 2 yourself or to delegate the decision to participant B. If you choose to not delegate, your decision between allocations 1 and 2 will be implemented and relevant for the final points. If you choose to delegate, you cannot choose between the two allocations. In this case, participant B's decision between allocations 1 and 2 will be implemented and relevant for the final points.

The table below provides an additional summary of the two allocations which either you or—if you delegate the decision—participant B must decide.

	Your points	B's points	One C's points	Other C's points
Allocation 1	9	9	1	1
Allocation 2	5	5	5	5

After you or—if you decide to delegate the decision—participant B has decided on the allocation of the 20 points, both participants C learn the following:

- whether participant A delegated the decision to participant B or not, and
- the implemented allocation.

Following this, one of the two participants C will be chosen randomly. The randomly chosen participant C has the possibility of assigning a total of up to 7 extra points at her discretion to you and/or participant B. The chosen participant C can also decide to not give the extra points or assign less than 7 points in total.

Example 1

Allocation 1 is chosen (by you or participant B). The randomly chosen participant C decides to assign 3 points to you and 4 points to participant B. The following payments then result:

Your points	B's points	One C's points	Other C's points
9+3=12	9+4=13	1	1

Example 2

Allocation 2 is chosen (by you or participant B). The randomly chosen participant C decides to assign 3 points to you and 2 points to participant B. Note that the chosen participant C does not opt to assign all 7 extra points. The following payments result:

Your points	B's points	One C's points	Other C's points
5+3=8	5+2=7	5	5

Example 3

The randomly chosen participant C does not choose to assign extra points. The points shown in the following table will then result, depending on the chosen allocations.

	Your points	B's points	One C's points	Other C's points
Allocation 1	9	9	1	1
Allocation 2	5	5	5	5

Practice questions

Before you proceed to make the actual decision, please answer eight practice questions. They serve to make you more acquainted with the study. You can go back to the previous page to refer to the instructions. If you fail to give correct answers to practice question 2 or practice question 8, the study terminates for you. You will not proceed to the decision stage and thus will not be able to earn the bonus payment. For other practice questions, you will receive a warning message if you answer incorrectly. You should correct them before you proceed.

The decisions and numerical values in the practice questions are chosen on a purely random basis and are not to be considered as a hint or suggestion as to how you should decide in the decision stage.

Practice Question 1

Participant A chooses to delegate. Whose decision is relevant for the bonus payment at the end of the study?

- Participant A

- Participant B

Practice Question 2

Participant A chooses to NOT delegate. Whose decision is relevant for the bonus payment at the end of the study?

- Participant A
- Participant B

Practice Question 3

Allocation 1 is implemented. The randomly chosen participant C decides to assign the points according to the table below.

	A's points	B's points	One C's points	Other C's points
Allocation 1	9	9	1	1
Assigned points	0	3		
Final points				

What are the respective final points of each participant to be filled in the last row of the table?

- 5, 5, 5, 5
- 9, 12, 1, 1
- 9, 9, 1, 1

Practice Question 4

Allocation 2 is implemented. The randomly chosen participant C decides to assign the points according to the table below.

	A's points	B's points	One C's points	Other C's points
Allocation 2	5	5	5	5
Assigned points	1	2		
Final points				

What are the respective final points of each participant to be filled in the last row of the table?

- 5, 5, 5, 5
- 9, 9, 1, 1
- 6, 7, 5, 5

Practice Question 5

Allocation 2 is implemented. The randomly chosen participant C decides to assign the points according to the table below.

	A's points	B's points	One C's points	Other C's points
Allocation 2	5	5	5	5
Assigned points	6	3		
Final points				

Is this possible?

- No, it is not possible.
- Yes, the resulting points are 11, 8, 5, 5.

Practice Question 6

Allocation 2 is implemented. The randomly chosen participant C decides to assign the points according to the table below.

	A's points	B's points	One C's points	Other C's points
Allocation 2	5	5	5	5
Assigned points	7	0		
Final points				

What are the respective final points of each participant to be filled in the last row of the table?

- 5, 5, 5, 5
- 12, 5, 5, 5
- 9, 9, 1, 1

Practice Question 7

Allocation 1 is implemented. The randomly chosen participant C decides to assign the points according to the table below.

	A's points	B's points	One C's points	Other C's points
Allocation 1	9	9	1	1
Assigned points	0	0		
Final points				

What are the respective final points of each participant to be filled in the last row of the table?

- 5, 5, 5, 5
- 9, 9, 1, 1
- 8, 13, 1, 1

Practice Question 8

Allocation 2 is implemented. The randomly chosen participant C decides to assign the points according to the table below.

	A's points	B's points	One C's points	Other C's points
Allocation 2	5	5	5	5
Assigned points	0	5		
Final points				

What are the respective final points of each participant to be filled in the last row of the table?

- 9, 9, 1, 1
- 5, 5, 5, 5
- 5, 10, 5, 5

Decision

It is now time to make the decision. Once you have made the decision and clicked the button in the lower right corner to continue, the study will be over and your decision will be recorded.

You decision, together with the decisions of the other three participants matched with you, determine your final bonus payment. Please make the decision carefully.

You, as a participant A, are matched with one participant B and two participants C. Either you or participant B must decide between the following two allocations:

	Your points	B's points	One C's points	Other C's points
Allocation 1	9	9	1	1
Allocation 2	5	5	5	5

You can decide whether you would like to choose allocation 1 or allocation 2 yourself, or if you would like to delegate this decision to participant B.

If you choose one of the two allocations yourself, your decision will be relevant for the final points. If you choose to delegate, the decision of participant B will be implemented for the final points. Both participants C learn whether you have chosen to delegate and which allocation is implemented.

Following this, one of the participants C is chosen randomly. The chosen participant C can choose to assign a total of up to 7 extra points to you and/or participant B.

What is your decision?

- Do not delegate and choose Allocation 1 yourself
- Do not delegate and choose Allocation 2 yourself
- Delegate the decision to participant B

End of experiment

This is the end of the study. Thank you for your participation.

Your decision has been recorded. If you are eligible for bonus payment, your final points will be calculated based on your decision and the decisions of the other three participants matched with you in this study. It will be transformed into dollar with the conversion rate 5 points = $1. The bonus payment will be transferred to your Prolific account within 2 weeks.

Please click the button in the lower right corner to finish the study and proceed back to Prolific.

5.7.1.2 Instructions for B

The Introduction, Practice questions, and End of experiment sections for Participants B are the same as those for Participant A. Only the Instructions and Decision sections are different.

Instructions

Please read the following instructions carefully. You can earn a bonus payment, depending on your decisions and those of the other participants, in addition to the $1.1 you receive for completing this study. It is thus very important that you read these instructions carefully.

You are a **participant B**.

The three other persons assigned to you are one participant A and two participants C.

In this study, either participant A or participant B decides how 20 points will be distributed among the four participants.

In distributing the points, participant A or B must decide between two possible allocations:

- Allocation 1: Participants A and B receive 9 points each and the two participants C receive 1 point each.
- Allocation 2: Participant A, participant B, and both participants C receive 5 points each.

Participant A can either choose between allocations 1 and 2 herself or to delegate the decision to you. If participant A chooses to not delegate, participant A's decision between allocations 1 and 2 will be implemented and relevant for the final points. In this case, your decision will not be relevant for the final points. If participant A chooses to delegate, your decision between the two allocations will be implemented and relevant for the final points.

The table below provides an additional summary of the two allocations which either participant A or—if she delegates the decision—you must decide.

After participant A or—if she decides to delegate the decision—you have decided on the allocation of the 20 points, both participants C learn the following:

	A's points	Your points	One C's points	Other C's points
Allocation 1	9	9	1	1
Allocation 2	5	5	5	5

- whether participant A delegated the decision to participant B or not, and
- the implemented allocation.

Following this, one of the two participants C will be chosen randomly. The randomly chosen participant C has the possibility of assigning a total of up to 7 extra points at her discretion to participant A and/or you. The chosen participant C can also decide to not assign the extra points or assign less than 7 points in total.

Example 1

Allocation 1 is chosen (by you or participant B). The randomly chosen participant C decides to assign 3 points to you and 4 points to participant B. The following payments then result:

A's points	Your points	One C's points	Other C's points
9+3=12	9+4=13	1	1

Example 2

Allocation 2 is chosen (by you or participant B). The randomly chosen participant C decides to assign 3 points to you and 2 points to participant B. Note that the chosen participant C does not opt to assign all 7 extra points. The following payments result:

A's points	Your points	One C's points	Other C's points
5+3=8	5+2=7	5	5

Example 3

The randomly chosen participant C does not choose to assign extra points. The points shown in the following table will then result, depending on the chosen allocations.

	A's points	Your points	One C's points	Other C's points
Allocation 1	9	9	1	1
Allocation 2	5	5	5	5

Decision

It is now time to make the decision. Once you have made the decision and clicked the button in the lower right corner to continue, the study will be over and your decision will be recorded.

You decision, together with the decisions of the other three participants matched with you, determine your final bonus payment. Please make the decision carefully.

You, as a participant B, are matched with one participant A and two participants C. Either participant A or you must decide between the following two allocations:

	A's points	Your points	One C's points	Other C's points
Allocation 1	9	9	1	1
Allocation 2	5	5	5	5

Participant A can decide whether she would like to choose allocation 1 or allocation 2 herself, or if she would like to delegate this decision to you.

If participant A chooses to not delegate, her own decision will be relevant for the final points. If participant A chooses to delegate, your decision will be implemented for the final points. Both participants C learn whether participant A has chosen to delegate and which allocation is implemented.

Following this, one of the participants C is randomly chosen. The chosen participant C can choose to assign a total of up to 7 extra points to participant A and/or you.

Which allocation will you choose?

- Allocation 1
- Allocation 2

5.7.1.3 Instructions for C

The Introduction, Practice questions, and End of experiment sections for Participants C are the same as those for Participant A. Only the Instructions and Decision sections are different.

Instructions

Please read the following instructions carefully. You can earn a bonus payment, depending on your decisions and those of the other participants, in addition to the $1.1 you receive for completing this study. It is thus very important that you read these instructions carefully.

You are a **participant C**.

The three other persons assigned to you are one participant A, one participant B,and one other participant C.

In this study, either participant A or participant B decides how 20 points will be distributed among the four participants.

In distributing the points, participant A or B must decide between two possible allocations:

- Allocation 1: Participants A and B receive 9 points each and the two participants C receive 1 point each.
- Allocation 2: Participant A, participant B, and both participants C receive 5 points each.

Participant A can either choose between allocations 1 and 2 herself or to delegate the decision to participant B. If participant A chooses to not delegate, participant A's decision between allocations 1 and 2 will be implemented and relevant for the final points. If participant A chooses to delegate, participant B's decision between the two allocations will be implemented and relevant for the final points.

The table below provides an additional summary of the two allocations which either participant A or—if she delegates the decision—participant B must decide.

	A's points	B's points	Your points	Other C's points
Allocation 1	9	9	1	1
Allocation 2	5	5	5	5

After participant A or—if she decides to delegate the decision—participant B has decided on the allocation of the 20 points, both participants C learn the following:

- whether participant A delegated the decision to participant B or not, and
- the implemented allocation.

Following this, one of the two participants C will be chosen randomly. The randomly chosen participant C has the possibility of assigning a total of up to 7 extra points at her discretion to participant A and/or participant B. The chosen participant C can also decide to not assign extra points or to assign less than 7 points in total.

Example 1

Allocation 1 is chosen (by you or participant B). The randomly chosen participant C decides to assign 3 points to you and 4 points to participant B. The following payments then result:

A's points	B's points	Chosen C's points	Other C's points
9+3=12	9+4=13	1	1

Example 2

Allocation 2 is chosen (by you or participant B). The randomly chosen participant C decides to assign 3 points to you and 2 points to participant B. Note that the chosen participant C does not opt to assign all 7 extra points. The following payments result:

A's points	B's points	Chosen C's points	Other C's points
5+3=8	5+2=7	5	5

Example 3

The randomly chosen participant C does not choose to assign extra points. The points shown in the following table will then result, depending on the chosen allocations.

	A's points	B's points	Chosen C's points	Other C's points
Allocation 1	9	9	1	1
Allocation 2	5	5	5	5

Decision

It is now time to make the decision. Once you have made the decision and clicked the button in the lower right corner to continue, the study will be over and your decision will be recorded.

You decision, together with the decisions of the other three participants matched with you, determine your final bonus payment. Please make the decision carefully.

You, as a participant C, are matched with one participant A, one participant B, and one other participant C. Either participant A or participant B must decide between the following two allocations:

	A's points	B's points	One C's points	Other C's points
Allocation 1	9	9	1	1
Allocation 2	5	5	5	5

Participant A can choose to implement her own decision or to delegate the decision to participant B.

If participant A chooses to not delegate, her own decision will be relevant for the final points. If participant A chooses to delegate, participant B's decision will be implemented for the final points Both participants C learn whether participant A has chosen to delegate and which allocation is implemented.

Following this, one of the participants C is chosen randomly. The chosen participant C can choose to give a total of up to 7 extra points to participant A and/or participant B.

We therefore ask you to make your decision for each of the following four cases:

- Participant A does not delegate and decides herself for allocation 1 (9, 9, 1, 1)

- Participant A does not delegate and decides herself for allocation 2 (5, 5, 5, 5)
- Participant A delegates and participant B decides for allocation 1 (9, 9, 1, 1)
- Participant A delegates and participant B decides for allocation 2 (5, 5, 5, 5)

Participant A and/or participant B make their decisions without knowing what you or the other participant C would do in the four cases.

If you are randomly chosen, your decision for that case which actually arises from participant A's decision will be implemented.

Each of your two decisions can therefore be relevant for your payment.

Possible case 1

Participant A delegates the decision, and participant B chooses the following allocation:

A receives 5 point.
B receives 5 points.
Each C receives 5 points.

How much extra points are you willing to assign to participant A and participant B?

(You can also choose to fill in 0 for both blanks. The total points assigned to participant A and participant B must not be higher than 7. Your response will only be approved if the sum is less than or equal to 7.)

_____ Extra points for participant A

_____ Extra points for participant B

Please fill in the sum of the extra points you have filled in above.

_____ Total extra points

Possible case 2

Participant A delegates the decision, and participant B chooses the following allocation:

A receives 9 point.
B receives 9 points.
Each C receives 1 points.

How much extra points are you willing to assign to participant A and participant B?

(You can also choose to fill in 0 for both blanks. The total points assigned to participant A and participant B must not be higher than 7. Your response will only be approved if the sum is less than or equal to 7.)

_____ Extra points for participant A

_____ Extra points for participant B

Please fill in the sum of the extra points you have filled in above.

_____ Total extra points

Possible case 3

Participant A does not delegate the decision, and she chooses the following allocation:

A receives 9 point.
B receives 9 points.
Each C receives 1 points.

How much extra points are you willing to assign to participant A and participant B?

(You can also choose to fill in 0 for both blanks. The total points assigned to participant A and participant B must not be higher than 7. Your response will only be approved if the sum is less than or equal to 7.)

_____ Extra points for participant A

_____ Extra points for participant B

Please fill in the sum of the extra points you have filled in above.

_____ Total extra points

Possible case 4

Participant A does not delegate the decision, and she chooses the following allocation:

A receives 5 point.
B receives 5 points.
Each C receives 5 points.

How much extra points are you willing to assign to participant A and participant B?

(You can also choose to fill in 0 for both blanks. The total points assigned to participant A and participant B must not be higher than 7. Your response will only be approved if the sum is less than or equal to 7.)

_____ Extra points for participant A

_____ Extra points for participant B

Please fill in the sum of the extra points you have filled in above.

_____ Total extra points

Bibliography

Addison, J. T., Schank, T., Schnabel, C., and Wagner, J. (2007). Do works councils inhibit investment? *ILR Review*, 60(2):187–203.

Aggarwal, R. K. and Samwick, A. A. (1999). Executive compensation, strategic competition, and relative performance evaluation: Theory and evidence. *The Journal of Finance*, 54(6):1999–2043.

Ambrus, A., Greiner, B., Pathak, P., et al. (2009). Group versus individual decision-making: Is there a shift. *Institute for Advanced Study, School of Social Science Economics Working Paper*, 91.

Anderhub, V., Königstein, M., and Kübler, D. (2003). Long-term work contracts versus sequential spot markets: experimental evidence on firm-specific investment. *Labour Economics*, 10(4):407–425.

Antràs, P. (2003). Firms, contracts, and trade structure. *The Quarterly Journal of Economics*, 118(4):1375–1418.

Arya, A., Löffler, C., Mittendorf, B., and Pfeiffer, T. (2015). The middleman as a panacea for supply chain coordination problems. *European Journal of Operational Research*, 240(2):393–400.

Baik, K. H. and Kim, I.-G. (1997). Delegation in contests. *European Journal of Political Economy*, 13(2):281–298.

Baldenius, T. (2006). Ownership, incentives, and the hold-up problem. *The RAND Journal of Economics*, 37(2):276–299.

Baliga, S. and Sjöström, T. (2009). Contracting with third parties. *American Economic Journal: Microeconomics*, 1(1):75–100.

Barreda-Tarrazona, I., Georgantzís, N., Manasakis, C., Mitrokostas, E., and Petrakis, E. (2016). Endogenous managerial compensation contracts in experimental quantity-setting duopolies. *Economic Modelling*, 54:205–217.

Bartling, B. and Fischbacher, U. (2012). Shifting the blame: On delegation and responsibility. *The Review of Economic Studies*, 79(1):67–87.

Beales III, J. H. and Muris, T. J. (1995). The foundations of franchise regulation: Issues and evidence. *Journal of Corporate Finance*, 2(1-2):157–197.

Bénabou, R. and Tirole, J. (2006). Incentives and prosocial behavior. *The American Economic Review*, 96(5):1652–1678.

Bénabou, R. and Tirole, J. (2010). Individual and corporate social responsibility. *Economica*, 77(305):1–19.

Berg, J., Dickhaut, J., and McCabe, K. (1995). Trust, reciprocity, and social history. *Games and Economic Behavior*, 10(1):122–142.

Bester, H. and Güth, W. (1998). Is altruism evolutionarily stable? *Journal of Economic Behavior & Organization*, 34(2):193–209.

Bester, H. and Sákovics, J. (2001). Delegated bargaining and renegotiation. *Journal of Economic Behavior & Organization*, 45(4):459–473.

Binmore, K., Morgan, P., Shaked, A., and Sutton, J. (1991). Do people exploit their bargaining power? an experimental study. *Games and Economic Behavior*, 3(3):295–322.

Binmore, K., Shaked, A., and Sutton, J. (1989). An outside option experiment. *The Quarterly Journal of Economics*, 104(4):753–770.

Bloomfield, M. (2018). Compensation disclosures and the weaponization of executive pay: Evidence from revenue-based performance evaluation. *mimeo. Wharton School, University of Pennsylvania.*

Bolton, G. E. and Ockenfels, A. (2000). Erc: A theory of equity, reciprocity, and competition. *American Economic Review*, 90(1):166–193.

Bonfim, M. P. and Silva, C. A. T. (2019). Inhibitors mechanisms of group and individuals dishonesty. *Journal of Accounting, Management and Governance*, 22(2):205–226.

Brander, J. A. and Lewis, T. R. (1986). Oligopoly and financial structure: The limited liability effect. *The American Economic Review*, 76(5):956–970.

Brown, J. R., Cookson, J. A., and Heimer, R. Z. (2016). Law and finance matter: Lessons from externally imposed courts. *The Review of Financial Studies*, 30(3):1019–1051.

Brunette, M., Cabantous, L., and Couture, S. (2015). Are individuals more risk and ambiguity averse in a group environment or alone? results from an experimental study. *Theory and Decision*, 78(3):357–376.

Bulow, J. I., Geanakoplos, J. D., and Klemperer, P. D. (1985). Multimarket oligopoly: Strategic substitutes and complements. *Journal of Political economy*, 93(3):488–511.

Burtraw, D. (1992). Strategic delegation in bargaining. *Economics Letters*, 38(2):181–185.

Burtraw, D. (1993). Bargaining with noisy delegation. *The RAND Journal of Economics*, 24(1):40–57.

Butera, L. and Houser, D. (2018). Delegating altruism: Toward an understanding of agency in charitable giving. *Journal of Economic Behavior & Organization*, 155:99–109.

Cadigan, J., Schmitt, P., Shupp, R., and Swope, K. (2009). An experimental study of the holdout problem in a multilateral bargaining game. *Southern Economic Journal*, 76(2):444–457.

Cai, H. and Cont, W. (2004). Agency problems and commitment in delegated bargaining. *Journal of Economics & Management Strategy*, 13(4):703–729.

Caillaud, B., Jullien, B., and Picard, P. (1995). Competing vertical structures: Precommitment and renegotiation. *Econometrica*, 63(3):621–646.

Camerer, C. F. and Thaler, R. H. (1995). Anomalies: Ultimatums, dictators and manners. *Journal of Economic perspectives*, 9(2):209–219.

Card, D., Devicienti, N. F., Revelli-CCA, L., and Maida, A. (2014). Rent-sharing, hold-up, and wages: Evidence from matched panel data. *The Review of Economic Studies*, 81:84–111.

Cardullo, G., Conti, M., and Sulis, G. (2015). Sunk capital, unions and the hold-up problem: Theory and evidence from cross-country sectoral data. *European Economic Review*, 76:253–274.

Carlsson, F., Kataria, M., Lampi, E., and Levati, M. V. (2017). Doing good with other people's money: An experiment on people's (un)willingness to grant others the freedom to choose. *Journal of Behavioral and Experimental Economics*, 71:79–87.

Carnegie, A. (2014). States held hostage: Political hold-up problems and the effects of international institutions. *American Political Science Review*, 108(1):54–70.

Cartwright, E. (2019). A survey of belief-based guilt aversion in trust and dictator games. *Journal of Economic Behavior & Organization*, 167:430–444.

Cavanaugh, J. K. (1998). Asset-specific investment and unionized labor. *Industrial Relations*, 37(1):35–50.

Cellini, R., Lambertini, L., and Ottaviano, G. I. (2020). Strategic inattention, delegation and endogenous market structure. *European Economic Review*, 121:103324.

Chakravorty, S. (2019). Representation in multi-issue delegated bargaining. *The B.E. Journal of Theoretical Economics*, 19(1).

Charness, G. (2000). Responsibility and effort in an experimental labor market. *Journal of Economic Behavior & Organization*, 42(3):375–384.

Charness, G. (2004). Attribution and reciprocity in an experimental labor market. *Journal of labor Economics*, 22(3):665–688.

Charness, G., Cobo-Reyes, R., Jiménez, N., Lacomba, J. A., and Lagos, F. (2012). The hidden advantage of delegation: Pareto improvements in a gift exchange game. *The American Economic Review*, 102(5):2358–79.

Charness, G., Du, N., and Yang, C.-L. (2011). Trust and trustworthiness reputations in an investment game. *Games and Economic Behavior*, 72(2):361–375.

Charness, G. and Dufwenberg, M. (2006). Promises and partnership. *Econometrica*, 74(6):1579–1601.

Charness, G. and Dufwenberg, M. (2010). Bare promises: An experiment. *Economics letters*, 107(2):281–283.

Charness, G. and Kuhn, P. (2011). Lab labor: What can labor economists learn from the lab? In *Handbook of labor economics*, volume 4, pages 229–330. Elsevier.

Charness, G., Naef, M., and Sontuoso, A. (2019). Opportunistic conformism. *Journal of Economic Theory*, 180:100–134.

Charness, G. and Rabin, M. (2002). Understanding social preferences with simple tests. *The Quarterly Journal of Economics*, 117(3):817–869.

Che, Y.-K. and Sákovics, J. (2004). A dynamic theory of holdup. *Econometrica*, 72(4):1063–1103.

Che, Y.-K. and Sákovics, J. (2008). Hold-up problem. In Durlauf, S. N. and Blume, L. E., editors, *The New Palgrave Dictionary of Economics*. Palgrave Macmillan, Basingstoke.

Chen, X., Hong, F., and Zhao, X. (2019). Concentration and variability of forecasts in artificial investment games: an online experiment on wechat. *Experimental Economics*, pages 1–33.

Chirco, A., Colombo, C., and Scrimitore, M. (2013). Quantity competition, endogenous motives and behavioral heterogeneity. *Theory and decision*, 74(1):55–74.

Choy, A. K., Hamman, J. R., King, R. R., and Weber, R. A. (2016). Delegated bargaining in a competitive agent market: an experimental study. *Journal of the Economic Science Association*, 2(1):22–35.

Ciliberto, F. (2006). Does organizational form affect investment decisions? *The Journal of Industrial Economics*, 54(1):63–93.

Coase, R. (2006). The conduct of economics: The example of fisher body and general motors. *Journal of Economics & Management Strategy*, 15(2):255–278.

Coffman, L. C. (2011). Intermediation reduces punishment (and reward). *American Economic Journal: Microeconomics*, 3(4):77–106.

Condorelli, D. and Szentes, B. (Forthcoming). Information design in the hold-up problem. *Journal of Political Economy.*

Cookson, J. A. (2018). Does contract enforcement mitigate holdup? *Review of Corporate Finance Studies*, 7(2):245–275.

Corts, K. S. and Neher, D. V. (2003). Credible delegation. *European Economic Review*, 47(3):395–407.

Croson, R. and Konow, J. (2009). Social preferences and moral biases. *Journal of Economic Behavior and Organization*, 69(3):201–212.

Davis, A. M. and Leider, S. (2018). Contracts and capacity investment in supply chains. *Manufacturing & Service Operations Management*, 20(3):403–421.

Dawid, H., MacLeod, W. B., et al. (2008). Hold-up and the evolution of investment and bargaining norms. *Games and Economic Behavior*, 62(1):26–52.

Di Bartolomeo, G., Dufwenberg, M., Papa, S., and Passarelli, F. (2019). Promises, expectations & causation. *Games and Economic Behavior*, 113:137–146.

Dijk, O. and Holmén, M. (2017). Charity, incentives, and performance. *Journal of behavioral and experimental economics*, 66:119–128.

Drake, A. R. and Haka, S. F. (2008). Does abc information exacerbate hold-up problems in buyer-supplier negotiations? *The Accounting Review*, 83(1):29–60.

Drugov, M., Hamman, J., and Serra, D. (2014). Intermediaries in corruption: an experiment. *Experimental Economics*, 17(1):78–99.

Du, N., Heywood, J. S., and Ye, G. (2013). Strategic delegation in an experimental mixed duopoly. *Journal of Economic Behavior & Organization*, 87:91–100.

Dubois, P. and Vukina, T. (2016). Incentives to invest in short-term vs long-term contracts: Theory and evidence. *The BE Journal of Economic Analysis & Policy*, 16(3):1239–1272.

Dufwenberg, M. and Gneezy, U. (2000). Measuring beliefs in an experimental lost wallet game. *Games and Economic Behavior*, 30(2):163–182.

Dufwenberg, M. and Güth, W. (1999). Indirect evolution vs. strategic delegation: A comparison of two approaches to explaining economic institutions. *European Journal of Political Economy*, 15(2):281–295.

Dufwenberg, M. and Kirchsteiger, G. (2004). A theory of sequential reciprocity. *Games and Economic Behavior*, 47(2):268–298.

Dufwenberg, M., Smith, A., and Van Essen, M. (2013). Hold-up: With a vengeance. *Economic Inquiry*, 51(1):896–908.

Eaton, B. C. (2004). The elementary economics of social dilemmas. *Canadian Journal of Economics*, 37(4):805–829.

Ederer, F. and Stremitzer, A. (2017). Promises and expectations. *Games and Economic Behavior*, 106:161–178.

Eisenkopf, G. and Fischbacher, U. (2015). Naïve responses to kind delegation. *Managerial and Decision Economics*, 36(7):487–498.

Eisenkopf, G. and Nüesch, S. (2016). Third parties and specific investments. *Schmalenbach Business Review*, 17(2):151–172.

Eisenkopf, G. and Nüesch, S. (2017). Trust in third parties. *Journal of Economic Behavior & Organization*, 137:410–427.

Ellingsen, T. and Johannesson, M. (2004a). Is there a hold-up problem? *The Scandinavian Journal of Economics*, 106(3):475–494.

Ellingsen, T. and Johannesson, M. (2004b). Promises, threats and fairness. *The Economic Journal*, 114:397–420.

Ellingsen, T., Johannesson, M., Tjøtta, S., and Torsvik, G. (2010). Testing guilt aversion. *Games and Economic Behavior*, 68(1):95–107.

Ellingsen, T. and Miettinen, T. (2008). Commitment and conflict in bilateral bargaining. *The American Economic Review*, 98(4):1629–35.

Ellingsen, T. and Miettinen, T. (2014). Tough negotiations: Bilateral bargaining with durable commitments. *Games and Economic Behavior*, 87:353–366.

Ellingsen, T. and Robles, J. (2002). Does evolution solve the hold-up problem? *Games and Economic Behavior*, 39(1):28–53.

Embrey, M., Mengel, F., and Peeters, R. (2018). Strategy revision opportunities and collusion. *Experimental Economics*, pages 1–23.

Engel, C. (2007). How much collusion? a meta-analysis of oligopoly experiments. *Journal of Competition Law & Economics*, 3(4):491–549.

Engel, C. (2011). Dictator games: A meta study. *Experimental Economics*, 14(4):583–610.

Engelmann, D. and Normann, H.-T. (2007). An experimental test of strategic trade policy. *Journal of Economic Behavior & Organization*, 64(1):144–156.

Englmaier, F. (2010). Managerial optimism and investment choice. *Managerial and Decision Economics*, 31(4):303–310.

Erat, S. (2013). Avoiding lying: The case of delegated deception. *Journal of Economic Behavior & Organization*, 93:273–278.

Falk, A. and Fischbacher, U. (2006). A theory of reciprocity. *Games and Economic Behavior*, 54(2):293–315.

Falk, A. and Heckman, J. J. (2009). Lab experiments are a major source of knowledge in the social sciences. *Science*, 326(5952):535–538.

Farrell, J., Hayes, J., Shapiro, C., and Sullivan, T. (2007). Standard setting, patents, and hold-up. *Antitrust Law Journal*, 74(3):603–670.

Farrell, J. and Rabin, M. (1996). Cheap talk. *Journal of Economic perspectives*, 10(3):103–118.

Fehr, E., Kremhelmer, S., and Schmidt, K. M. (2008). Fairness and the optimal allocation of ownership rights. *The Economic Journal*, 118(531):1262–1284.

Fehr, E. and List, J. A. (2004). The hidden costs and returns of incentives: Trust and trustworthiness among CEOs. *Journal of the European Economic Association*, 2(5):743–771.

Fehr, E. and Schmidt, K. M. (1999). A theory of fairness, competition, and cooperation. *Quarterly Journal of Economics*, 114(3):817–868.

Fehr, E. and Schmidt, K. M. (2006). The economics of fairness, reciprocity and altruism–Experimental evidence and new theories. *Handbook of the Economics of Giving, Altruism and Reciprocity*, 1:615–691.

Fehr, E. and Tyran, J.-R. (2008). Limited rationality and strategic interaction: The impact of the strategic environment on nominal inertia. *Econometrica*, 76(2):353–394.

Fershtman, C. (2007). Delegated trust: The role of agency in trust relationship. Discussion paper no. 3-2007, Pinhas Sapir Center for Development.

Fershtman, C. and Gneezy, U. (2001). Strategic delegation: An experiment. *The RAND Journal of Economics*, 32(2):352–368.

Fershtman, C. and Judd, K. L. (1987). Equilibrium incentives in oligopoly. *The American Economic Review*, 77(5):927–940.

Fershtman, C., Judd, K. L., and Kalai, E. (1991). Observable contracts: Strategic delegation and cooperation. *International Economic Review*, 32(3):551–559.

Fershtman, C. and Kalai, E. (1997). Unobserved delegation. *International Economic Review*, 38(4):763–774.

Fischbacher, U. (2007). z-tree: Zurich toolbox for ready-made economic experiments. *Experimental economics*, 10(2):171–178.

Fudenberg, D. and Tirole, J. (1984). The fat-cat effect, the puppy-dog ploy, and the lean and hungry look. *The American Economic Review*, 74(2):361–366.

Galetovic, A., Haber, S., and Levine, R. (2015). An empirical examination of patent holdup. *Journal of Competition Law & Economics*, 11(3):549–578.

Ganglmair, B., Froeb, L. M., and Werden, G. J. (2012). Patent hold-up and antitrust: How a well-intentioned rule could retard innovation. *The Journal of Industrial Economics*, 60(2):249–273.

Gantner, A., Güth, W., and Königstein, M. (2001). Equitable choices in bargaining games with joint production. *Journal of Economic Behavior & Organization*, 46(2):209–225.

Garofalo, O. and Rott, C. (2018). Shifting blame? Experimental evidence of delegating communication. *Management Science*, 64(8):3911–3925.

Gawn, G. and Innes, R. (2019a). Lying through others: Does delegation promote deception? *Journal of Economic Psychology*, 71:59–73.

Gawn, G. and Innes, R. (2019b). Who delegates? Evidence from dictator games. *Economics Letters*, 181:186–189.

Geng, H., Hau, H., and Lai, S. (2016). Technological progress and ownership structure. Discussion paper no. dp11064, CEPR.

Grossman, S. J. and Hart, O. D. (1986). The costs and benefits of ownership: A theory of vertical and lateral integration. *The Journal of Political Economy*, 94(4):691–719.

Grout, P. A. (1984). Investment and wages in the absence of binding contracts: A Nash bargaining approach. *Econometrica*, 52(2):449–460.

Habla, W. and Winkler, R. (2018). Strategic delegation and international permit markets: Why linking may fail. *Journal of Environmental Economics and Management*, 92:244–250.

Hackett, S. C. (1993). Incomplete contracting: A laboratory experimental analysis. *Economic Inquiry*, 31(2):274–297.

Hackett, S. C. (1994). Is relational exchange possible in the absence of reputations and repeated contact? *Journal of Law, Economics, & Organization*, 10(2):360–389.

Hamman, J. R., Loewenstein, G., and Weber, R. A. (2010). Self-interest through delegation: An additional rationale for the principal-agent relationship. *The American Economic Review*, 100(4):1826–1846.

Harstad, B. (2008). Do side payments help? collective decisions and strategic delegation. *Journal of the European Economic Association*, 6(2-3):468–477.

Harstad, B. (2010). Strategic delegation and voting rules. *Journal of Public Economics*, 94(1–2):102–113.

Harstad, B. (2012). Climate contracts: A game of emissions, investments, negotiations, and renegotiations. *The Review of Economic Studies*, 79(4):1527–1557.

Harstad, B. (2016). The dynamics of climate agreements. *Journal of the European Economic Association*, 14(3):719–752.

Hart, O. (1995). *Firms, contracts, and financial structure*. Oxford university press.

Hart, O. (2009). Hold-up, asset ownership, and reference points. *The Quarterly Journal of Economics*, 124(1):267–300.

Haruvy, E., Katok, E., Ma, Z., and Sethi, S. (2019). Relationship-specific investment and hold-up problems in supply chains: Theory and experiments. *Business Research*, 12(1):45–74.

Heywood, J. S. and Ye, G. (2009). Delegation in a mixed oligopoly: The case of multiple private firms. *Managerial and Decision Economics*, 30(2):71–82.

Hill, A. (2015). Does delegation undermine accountability? Experimental evidence on the relationship between blame shifting and control. *Journal of Empirical Legal Studies*, 12(2):311–339.

Hirsch, B. T. (1991). Union coverage and profitability among us firms. *The Review of Economics and Statistics*, 73(1):69–77.

Holmstrom, B. (1982). Moral hazard in teams. *The Bell Journal of Economics*, 13(2):324–340.

Holmstrom, B. (1984). On the theory of delegation. In Boyer, M. and Kihlstrom, R., editors, *Bayesian Models in Economic Theory*, pages 115–141. Springer.

Hoppe, E. I. and Schmitz, P. W. (2011). Can contracts solve the hold-up problem? experimental evidence. *Games and Economic Behavior*, 73(1):186–199.

Huck, S., Lünser, G. K., and Tyran, J.-R. (2012). Competition fosters trust. *Games and Economic Behavior*, 76(1):195–209.

Huck, S., Müller, W., and Normann, H.-T. (2004). Strategic delegation in experimental markets. *International Journal of Industrial Organization*, 22(4):561–574.

Immordino, G., Menichini, A. M. C., and Romano, M. G. (2015). Contracts with wishful thinkers. *Journal of Economics & Management Strategy*, 24(4):863–886.

Ismayilov, H. and Potters, J. (2016). Why do promises affect trustworthiness, or do they? *Experimental Economics*, 19(2):382–393.

Jackson, M. O. and Wilkie, S. (2005). Endogenous games and mechanisms: Side payments among players. *The Review of Economic Studies*, 72(2):543–566.

Jansen, T., van Lier, A., and van Witteloostuijn, A. (2007). A note on strategic delegation: The market share case. *International Journal of Industrial Organization*, 25(3):531–539.

Johnson, N. D. and Mislin, A. A. (2011). Trust games: A meta-analysis. *Journal of Economic Psychology*, 32(5):865–889.

Jones, S. R. (1989). Have your lawyer call my lawyer: Bilateral delegation in bargaining situations. *Journal of Economic Behavior & Organization*, 11(2):159–174.

Joskow, P. L. (1985). Vertical integration and long-term contracts: The case of coal-burning electric generating plants. *Journal of Law, Economics & Organization*, 1:33.

Joskow, P. L. (1987). Contract duration and relationship-specific investments: Empirical evidence from coal markets. *The American Economic Review*, 77(1):168–185.

Joskow, P. L. (1990). The performance of long-term contracts: Further evidence from coal markets. *The RAND Journal of Economics*, 21(2):251–274.

Kandul, S. and Kirchkamp, O. (2018). Do i care if others lie? current and future effects when lies can be delegated. *Journal of Behavioral and Experimental Economics*, 74:70–78.

Katz, M. L. (1991). Game-playing agents: Unobservable contracts as precommitments. *The RAND Journal of Economics*, 22(3):307–328.

Kawagoe, T. and Narita, Y. (2014). Guilt aversion revisited: An experimental test of a new model. *Journal of Economic Behavior & Organization*, 102:1–9.

Kedia, S. (2006). Estimating product market competition: Methodology and application. *Journal of Banking & Finance*, 30(3):875–894.

Klein, B. (1996). Why hold-ups occur: The self-enforcing range of contractual relationships. *Economic Inquiry*, 34(3):444–463.

Klein, B. (1998). The hold-up problem. In Newman, P., editor, *The New Palgrave Dictionary of Economics and the Law*. Palgrave Macmillan UK, London.

Klein, B., Crawford, R. G., and Alchian, A. A. (1978). Vertical integration, appropriable rents, and the competitive contracting process. *Journal of law and economics*, 21(2):297–326.

Kockesen, L. and Ok, E. A. (2004). Strategic delegation by unobservable incentive contracts. *The Review of Economic Studies*, 71(2):397–424.

Kopel, M. and Pezzino, M. (2018). Strategic delegation in oligopoly. In Corchón, L. C. and Marini, M. A., editors, *Handbook of Game Theory and Industrial Organization, Volume II: Applications*, chapter 10, pages 248–285. Edward Elgar Publishing, Cheltenham.

Kopel, M. and Riegler, C. (2006). R&D in a strategic delegation game revisited: A note. *Managerial and Decision Economics*, 27(7):605–612.

Kopel, M. and Riegler, C. (2008). Delegation in an R&D game with spillovers. *Contributions to Economic Analysis*, 286:177–213.

Koppel, H. and Regner, T. (2014). Corporate social responsibility in the work place. *Experimental Economics*, 17(3):347–370.

Kranton, R. and Swamy, A. V. (2008). Contracts, hold-up, and exports: Textiles and opium in colonial India. *The American Economic Review*, 98(3):967–89.

Kube, S., Marechal, M., and Puppe, C. (2013). Do wage cuts damage work morale? Evidence from a natural field experiment. *Journal of the European Economic Association*, 11(4):853–870.

Lambertini, L. (2016). Managerial delegation in a dynamic renewable resource oligopoly. In *Dynamic Perspectives on Managerial Decision Making*, pages 93–107. Springer.

Lambertini, L. and Trombetta, M. (2002). Delegation and firms' ability to collude. *Journal of Economic Behavior & Organization*, 47(4):359–373.

Lammers, F. (2010). Fairness in delegated bargaining. *Journal of economics & management strategy*, 19(1):169–183.

Levine, D. K. (1998). Modeling altruism and spitefulness in experiments. *Review of Economic Dynamics*, 1(3):593–622.

Levitt, S. D. and List, J. A. (2007). What do laboratory experiments measuring social preferences reveal about the real world? *The journal of economic perspectives*, 21(2):153–174.

Li, C. (2018). Supplier competition and cost reduction with endogenous information asymmetry. *University of Connecticut School of Business Research Paper No. 18-10*.

Li, C. and Wan, Z. (2016). Supplier competition and cost improvement. *Management Science*, 63(8):2460–2477.

Li, Y. and Shuai, J. (2019). Licensing essential patents: The non-discriminatory commitment and hold-up. *The Journal of Industrial Economics*, 67(1):37–55.

Liang, W.-J., Tseng, C.-C., and Wang, K.-C. A. (2011). Location choice with delegation: Bertrand vs. cournot competition. *Economic Modelling*, 28(4):1774–1781.

Luporini, A. (2006). Relative performance evaluation in a multi-plant firm. *Economic Theory*, 28(1):235–243.

Macchiavello, R. and Morjaria, A. (2015). The value of relationships: Evidence from a supply shock to kenyan rose exports. *The American Economic Review*, 105(9):2911–45.

Machiavelli, N. (1532). *The prince*. Indianapolis: Hackett Publishing, 2008.

MacLeod, W. B. and Malcomson, J. M. (1993). Investments, holdup, and the form of market contracts. *The American Economic Review*, 83(4):811–837.

Malcomson, J. M. (1997). Contracts, hold-up, and labor markets. *Journal of economic literature*, 35(4):1916–1957.

Martin, T. and Otto, C. A. (2017). The effect of hold-up problems on corporate investment: Evidence from import tariff reductions. Research paper no. fin-2017-1208, HEC Paris.

Maskin, E. and Moore, J. (1999). Implementation and renegotiation. *Review of Economic studies*, 66(1):39–56.

Masten, S. E. (1984). The organization of production: Evidence from the aerospace industry. *The Journal of Law and Economics*, 27(2):403–417.

Maximiano, S., Sloof, R., and Sonnemans, J. (2013). Gift exchange and the separation of ownership and control. *Games and Economic Behavior*, 77(1):41–60.

McCabe, M. J. and Snyder, C. M. (2018). Open access as a crude solution to a hold-up problem in the two-sided market for academic journals. *The Journal of Industrial Economics*, 66(2):301–349.

Miettinen, T. and Perea, A. (2015). Commitment in alternating offers bargaining. *Mathematical Social Sciences*, 76:12–18.

Miller, F. (2007). Information aggregation, strategy, and investment decision in buyer-supplier relationships. *SSRN Working Paper*.

Miller, F. (2011). Using formal and informal controls to limit opportunism: Review and classification of remedies to hold-ups. *Journal of Accounting Literature*, 30(1):1–39.

Miller, N. H. and Pazgal, A. I. (2001). The equivalence of price and quantity competition with delegation. *The RAND Journal of Economics*, 32(2):284–301.

Miller, N. and Pazgal, A. (2002). Relative performance as a strategic commitment mechanism. *Managerial and Decision Economics*, 23(2):51–68.

Monteverde, K. and Teece, D. J. (1982). Supplier switching costs and vertical integration in the automobile industry. *The Bell Journal of Economics*, 13(1):206–213.

Morita, H. and Servátka, M. (2013). Group identity and relation-specific investment: An experimental investigation. *European Economic Review*, 58:95–109.

Morita, H. and Servátka, M. (2018). Investment in outside options as opportunistic behavior: An experimental investigation. *Southern Economic Journal*, 85(2):457–484.

Nakamura, Y. (2019). The impact of the presence of biased managers as strategic commitment on endogenous market structures. *Managerial and Decision Economics*, 40(1):84–96.

Nguyen, A. and Tan, T. Y. (2019). Information control in the hold-up problem. *The RAND Journal of Economics*, 50(4):768–786.

Nöldeke, G. and Schmidt, K. M. (1995). Option contracts and renegotiation: A solution to the hold-up problem. *The RAND Journal of Economics*, 26(2):163–179.

Oexl, R. and Grossman, Z. J. (2013). Shifting the blame to a powerless intermediary. *Experimental Economics*, 16(3):306–312.

Offerman, T. (2002). Hurting hursts more than helping helps. *European Economic Review*, 46:1423–1437.

Oosterbeek, H., Sloof, R., and Van De Kuilen, G. (2004). Cultural differences in ultimatum game experiments: Evidence from a meta-analysis. *Experimental economics*, 7(2):171–188.

Oosterbeek, H., Sonnemans, J., and Van Velzen, S. (2003). The need for marriage contracts: An experimental study. *Journal of Population Economics*, 16(3):431–453.

Palan, S. and Schitter, C. (2018). Prolific. ac—a subject pool for online experiments. *Journal of Behavioral and Experimental Finance*, 17:22–27.

Patt, A. and Zeckhauser, R. (2000). Action bias and environmental decisions. *Journal of Risk and Uncertainty*, 21(1):45–72.

Peer, E., Rothschild, D. M., Evernden, Z., Gordon, A., and Damer, E. (2021). Mturk, prolific or panels? Choosing the right audience for online research. *SSRN Working Paper*.

Pei, H. D. (2018). A blame shifting theory of delegation. *SSRN Working Paper 3034656*.

Possajennikov, A. (2000). On the evolutionary stability of altruistic and spiteful preferences. *Journal of Economic Behavior & Organization*, 42(1):125–129.

Possajennikov, A. (2010). On delegation in contests and the survival of payoff maximizing behavior. *The B.E. Journal of Theoretical Economics*, 10(1).

Potters, J. and Suetens, S. (2009). Cooperation in experimental games of strategic complements and substitutes. *The Review of Economic Studies*, 76(3):1125–1147.

Potters, J. and Yang, Y. (2021). Delegation with strategic complements and substitutes. *Working Paper*.

Rabin, M. (1993). Incorporating fairness into game theory and economics. *The American Economic Review*, 83(5):1281–1302.

Rindfleisch, A. and Heide, J. B. (1997). Transaction cost analysis: Past, present, and future applications. *Journal of Marketing*, 61(4):30–54.

Ritz, R. A. (2008). Strategic incentives for market share. *International Journal of Industrial Organization*, 26(2):586–597.

Rogerson, W. P. (1992). Contractual solutions to the hold-up problem. *The Review of Economic Studies*, 59(4):777–793.

Rotemberg, J. J. (1994). Human relations in the workplace. *Journal of political Economy*, 102(4):684–717.

Rubinstein, A. (1982). Perfect equilibrium in a bargaining model. *Econometrica*, 50(1):97–109.

Schelling, T. C. (1960). *The strategy of conflict*. Harvard university press.

Schelling, T. C. (2006). *Strategies of commitment and other essays*. Harvard University Press, Cambridge, MA.

Schmitt, P., Shupp, R., Swope, K., and Mayer, J. (2008). Pre-commitment and personality: Behavioral explanations in ultimatum games. *Journal of Economic Behavior & Organization*, 66(3-4):597–605.

Schmitz, P. W. (2001). The hold-up problem and incomplete contracts: A survey of recent topics in contract theory. *Bulletin of economic research*, 53(1):1–17.

Schotter, A., Zheng, W., and Snyder, B. (2000). Bargaining through agents: An experimental study of delegation and commitment. *Games and Economic Behavior*, 30(2):248–292.

Sebald, A. (2010). Attribution and reciprocity. *Games and Economic Behavior*, 68(1):339–352.

Segendorff, B. (1998). Delegation and threat in bargaining. *Games and Economic Behavior*, 23(2):266–283.

Selten, R. (1998). Multistage game models and delay supergames. *Theory and decision*, 44(1):1–36.

Sengul, M., Gimeno, J., and Dial, J. (2012). Strategic delegation: A review, theoretical integration, and research agenda. *Journal of Management*, 38(1):375–414.

Shelanski, H. A. and Klein, P. G. (1995). Empirical research in transaction cost economics: A review and assessment. *Journal of Law, Economics, & Organization*, 11(2):335–361.

Sklivas, S. D. (1987). The strategic choice of managerial incentives. *The RAND Journal of Economics*, 18(3):452–458.

Sloof, R., Oosterbeek, H., and Sonnemans, J. (2007). Does making specific investments unobservable boost investment incentives? *Journal of Economics & Management Strategy*, 16(4):911–942.

Sloof, R., Sonnemans, J., and Oosterbeek, H. (2004). Specific investments, holdup, and the outside option principle. *European Economic Review*, 48(6):1399–1410.

Song, F. (2008). Trust and reciprocity behavior and behavioral forecasts: Individuals versus group-representatives. *Games and Economic Behavior*, 62(2):675–696.

Sonnemans, J., Oosterbeek, H., and Sloof, R. (2001). On the relation between asset ownership and specific investments. *The Economic Journal*, 111(474):791–820.

Spycher, S. and Winkler, R. (2019). Strategic delegation in the formation of international environmental agreements. *SSRN Working Paper 3361731*.

Suetens, S. and Potters, J. (2007). Bertrand colludes more than cournot. *Experimental Economics*, 10(1):71–77.

Sutan, A. and Vranceanu, R. (2016). Lying about delegation. *Journal of Economic Behavior & Organization*, 121:29–40.

Tirole, J. (1986). Procurement and renegotiation. *The Journal of Political Economy*, 94(2):235–259.

Trillas, F. (2019). Innovative behavioral regulatory agencies as second generation commitment devices. *Journal of Economic Policy Reform*, pages 1–17.

Vafaï, K. (2004). Delegation and opportunism. *Journal of Institutional and Theoretical Economics*, 160(3):498–521.

Van Reenen, J. and Menezes-Filho, N. (2003). Unions and innovation: A survey of the theory and empirical evidence. In Addison, J. T. and Schnabel, C., editors, *International Handbook of Trade Unions*. Edward Elgar, Northampton, Mass.

Vanberg, C. (2008). Why do people keep their promises? An experimental test of two explanation. *Econometrica*, 76(6):1467–1480.

Vickers, J. (1985). Delegation and the theory of the firm. *The Economic Journal*, 95:138–147.

Von Siemens, F. A. (2009). Bargaining under incomplete information, fairness, and the hold-up problem. *Journal of Economic Behavior & Organization*, 71(2):486–494.

Vroom, G. (2006). Organizational design and the intensity of rivalry. *Management Science*, 52(11):1689–1702.

Vukina, T. and Leegomonchai, P. (2006). Oligopsony power, asset specificity, and hold-up: Evidence from the broiler industry. *American Journal of Agricultural Economics*, 88(3):589–605.

Weaver, R. K. (1986). The politics of blame avoidance. *Journal of Public Policy*, 6(4):371–398.

Williamson, O. E. (1971). The vertical integration of production: Market failure considerations. *The American Economic Review*, 61(2):112–123.

Williamson, O. E. (1983). Credible commitments: Using hostages to support exchange. *The American Economic Review*, 73(4):519–540.

Yang, Y. (2021). A survey of the hold-up problem in the experimental economics literature. *Journal of Economic Surveys*, 35(1):227–249.

Yoon, D.-H. (2018). Strategic delegation, stock options, and investment hold-up problems. *Accounting, Organizations and Society*, 71:1–14.

Zhang, J. and Zhang, Z. (1997). R&d in a strategic delegation game. *Managerial and Decision Economics*, 18(5):391–398.

Zheng, K., Wang, X., Ni, D., and Yang, Y. (2020). Reciprocity and veto power in relation-specific investments: An experimental study. *Sustainability*, 12(10):4027.

Ziss, S. (2001). Horizontal mergers and delegation. *International Journal of Industrial Organization*, 19(3–4):471–492.

CENTER DISSERTATION SERIES

CentER for Economic Research, Tilburg University, the Netherlands

No.	Author	Title	ISBN	Published
617	Matjaz Maletic	Essays on international finance and empirical asset pricing	978 90 5668 618 5	January 2020
618	Zilong Niu	Essays on Asset Pricing and International Finance	978 90 5668 619 2	January 2020
619	Bjorn Lous	On free markets, income inequality, happiness and trust	978 90 5668 620 8	January 2020
620	Clemens Fiedler	Innovation in the Digital Age: Competition, Cooperation, and Standardization	978 90 5668 621 5	October 2020
621	Andreea Popescu	Essays in Asset Pricing and Auctions	978 90 5668 622 2	June 2020
622	Miranda Stienstra	The Determinants and Performance Implications of Alliance Partner Acquisition	978 90 5668 623 9	June 2020
623	Lei Lei	Essays on Labor and Family Economics in China	978 90 5668 624 6	May 2020
624	Farah Arshad	Performance Management Systems in Modern Organizations	978 90 5668 625 3	June 2020
625	Yi Zhang	Topics in Economics of Labor, Health, and Education	978 90 5668 626 0	June 2020
626	Emiel Jerphanion	Essays in Economic and Financial decisions of Households	978 90 5668 627 7	July 2020
627	Richard Heuver	Applications of liquidity risk discovery using financial market infrastructures transaction archives	978 90 5668 628 4	September 2020
628	Mohammad Nasir Nasiri	Essays on the Impact of Different Forms of Collaborative R&D on Innovation and Technological Change	978 90 5668 629 1	August 2020
629	Dorothee Hillrichs	On inequality and international trade	978 90 5668 630 7	September 2020
630	Roland van de Kerkhof	It’s about time: Managing implementation dynamics of condition-based maintenance	978 90 5668 631 4	October 2020

No.	Author	Title	ISBN	Published
631	Constant Pieters	Process Analysis for Marketing Research	978 90 5668 632 1	December 2020
632	Richard Jaimes	Essays in Macroeconomic Theory and Natural Resources	978 90 5668 633 8	November 2020
633	Olivier David Armand Zerbib	Asset pricing and impact investing with pro-environmental preferences	978 90 5668 634 5	November 2020
634	Laura Capera Romero	Essays on Competition, Regulation and Innovation in the Banking Industry	978 90 5668 635 2	December 2020
635	Elisabeth Beusch	Essays on the Self-Employed in the Netherlands and Europe	978 90 5668 636 9	December 2020
636	Sophie Zhou	Essays on the Self-Employed in the Netherlands and Europe	978 90 5668 637 6	November 2020
637	Vincent Peters	Turning modularity upside down: Patient-centered Down syndrome care from a service modularity perspective	978 90 5668 638 3	December 2020
638	Pranav Desai	Essays in Corporate Finance and Innovation	978 90 5668 639 0	January 2021
639	Kristy Jansen	Essays on Institutional Investors, Asset Allocation Decisions, and Asset Prices	978 90 5668 640 6	January 2021
640	Riley Badenbroek	Interior Point Methods and Simulated Annealing for Nonsymmetric Conic Optimization	978 90 5668 641 3	February 2021
641	Stephanie Koornneef	It's about time: Essays on temporal anchoring devices	978 90 5668 642 0	February 2021
642	Vilma Chila	Knowledge Dynamics in Employee Entrepreneurship: Implications for parents and offspring	978 90 5668 643 7	March 2021
643	Minke Remmerswaal	Essays on Financial Incentives in the Dutch Healthcare System	978 90 5668 644 4	July 2021
644	Tse-Min Wang	Voluntary Contributions to Public Goods: A multi-disciplinary examination of prosocial behavior and its antecedents	978 90 5668 645 1	March 2021

No.	Author	Title	ISBN	Published
645	Manwei Liu	Interdependent individuals: how aggregation, observation, and persuasion affect economic behavior and judgment	978 90 5668 646 8	March 2021
646	Nick Bombaij	Effectiveness of Loyalty Programs	978 90 5668 647 5	April 2021
647	Xiaoyu Wang	Essays in Microeconomics Theory	978 90 5668 648 2	April 2021
648	Thijs Brouwer	Essays on Behavioral Responses to Dishonest and Anti-Social Decision-Making	978 90 5668 649 9	May 2021
649	Yadi Yang	Experiments on hold-up problem and delegation	978 90 5668 650 5	May 2021

The four essays collected in this PhD thesis explore the hold-up problem and delegation using the experimental method. The first essay provides a summary of the experimental literature examining the hold-up problem. The second essay investigates the relationship between strategic delegation and the nature of the strategic interaction through a laboratory experiment. The third essay demonstrates strategic delegation can help mitigates the hold-up problem by setting the appropriate incentive schemes in a laboratory experiment. The fourth essay studies the credit-shifting effect of delegation with rewards in an online experiment.

Yadi Yang (1990) received her Bachelor's degree in Economics and Finance at the University of Hong Kong. She obtained her joint Master's degree in Economics at Uppsala University and the University of Siena in 2013. After completing the Research Master program in Economics, she started in 2015 as a PhD candidate at the department of economics at TiSEM, Tilburg University.

ISBN: 978 90 5668 650 5
DOI: 10.26116/center-lis-2108

TILBURG SCHOOL OF ECONOMICS AND MANAGEMENT

CentER Dissertation Series