Universitat Autònoma de Barcelona

Coordination with Asymmetries in Highly Complex and Volatile Environments

Jessica L. Ellis

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Universitat Autònoma de Barcelona

Coordination with Asymmetries in Highly Complex and Volatile Environments

A dissertation presented

by

Jessica L. Ellis

to

the Business Department

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

Economics, Management, and Organization

Autonomous University of Barcelona

Barcelona, Spain

2020

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Abstract

Organizations with weak-link technologies are prone to inefficient productivity traps. Such coordination failure can be like a trap for organizations; change is difficult to achieve once firms get stuck in sub-optimal equilibria. Weak-link coordination games abstractly model organizational situations in which the lowest-performing component determines overall success or failure. While coordination problems have been widely analyzed, current studies offer little on the cognitive and behavioral determinants of coordination failure. This dissertation contributes to the experimental literature by examining the cognitive and behavioral elements of coordination outcomes when firms are characterized by complexity and uncertainty. Each of the three chapters contributes to a greater understanding of organizational coordination problems in which the effectiveness of various organizationally relevant variables is tested in laboratory experiments.

This research was motivated by military organizations in which previous studies on financial incentives and worker characteristics cannot be directly applied. While this dissertation comprises three stand-alone essays, each experimental environment was carefully constructed to model situations conducive to military organizations. In the first chapter, I present an experimental analysis of weak-link coordination games in which subjects face specific time constraints in a volatile environment. In a basic 2×2 design, one factor involves situations in which all subjects face decision time (seconds to select a strategy) constraints. Another factor varies feedback time (seconds to review the outcome of the round) constraints. Overall, I find that stringent decision constraints lead to lower minimum effort levels, but increased feedback time seems to improve decision-making in anticipating others' behaviors and aligning decisions with firm outcomes. Feedback constraints impact coordination outcomes more when decisions are made under time

pressure. This finding suggests that if decisions are made under time duress, adequate recovery time can mean the difference between efficient and inefficient coordination.

In the second chapter, I study the effect of heterogeneous groups on the efficiency of coordination in repeated weak-link coordination games. I develop an experimental environment to test compositional effects on organizational performance using peer effects to maximize productivity. Though previous experiments have shown that large group sizes result in inefficient outcomes, we demonstrate a situation in which collective efficiency can emerge through social preferences. Our experimental setup models a task environment with two different types of workers. In standard settings, subjects are given five effort choices: $E_i \in \{0, 10, 20, 30, 40\}$. I introduce a second type of worker restricted to the highest effort choices: $E_i \in \{30, 40\}$. I find that heterogeneous groups induce change to higher, more efficient equilibria. Despite increases in group size, firms with restricted workers report higher overall output. Several firms managed to coordinate at much higher levels, with two firms reaching the most efficient coordination level. Employees exert more effort when another employee(s) can be harmed in the firm. This paper provides a mechanism to alleviate coordination failure among large groups, demonstrating how social spillovers can overpower group-size effects.

In the third chapter, we test whether participants exert more considerable effort when payoff-equivalent incentives are framed as losses rather than gains. I also intersect loss aversion and social preferences to model real-world situations when employees' actions can cause another employee to bear a loss. I develop an experimental environment to test stake-size effects on organizational performance. Subjects are given an initial endowment at the beginning of the experiment, which allows us to assess decisions at different reference points and test the degree of loss aversion in each setting. We find significant treatment effects, such that employees in high-stake frames contribute significantly more effort than employees in low-stake frames. Loss aversion is efficiency-enhancing only in high-stake conditions. In other words, framing effects work if the stakes are high enough. We find that social preferences are not as influential in the loss domain compared to our previous findings in the gain domain. It appears that social concerns are influenced by the degree of loss framing. This paper contributes to the coordination literature on framing effects with different stakes and coordination outcomes when loss aversion intersects with social preferences. Indeed, the current COVID-19 pandemic underlines the importance of understanding how exposure to loss can shape decisions and whether this differs with social considerations when employees' decisions can cause another to suffer the consequences.

Dedication

This dissertation is dedicated to the memory of my Aunt Karen. During my Ph.D., she overcame breast cancer for the second time. In my final year, she fought an aggressive, non-curable brain cancer that took her life shortly before my defense. Her strength and unwavering courage over the past few years pushed me to be stronger, inspired me to do more, and changed my perspective and how I approached my struggles. She cared for people with such intensity and fought hard to make sure that her students received the best education and care. She always stressed the importance of education, and I think that this passion and respect for education unconsciously shaped my values and led to this academic pursuit.

I also dedicate this dissertation to my nieces and nephews: Kinsley, Charlotte, Quinn, Bennett, and Noah. You are my inspiration to achieve greatness and the motivation to keep going when I want to quit. I will try to pass on the values and commitment I learned from Aunt Karen, and hopefully do it with her spirit of compassion, love, and honesty.

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Introduction

Organizations with weak-link technologies are prone to inefficient productivity traps. Such "coordination failure" can be like a trap for organizations; change is difficult to achieve once firms get stuck in sub-optimal equilibria. Weak-link coordination games abstractly model coordination problems in organizations and other social contexts. While coordination problems have been widely analyzed, current studies offer little on the cognitive and behavioral determinants of coordination failure. This dissertation contributes to this body of literature by investigating the cognitive and behavioral elements of coordination outcomes when firms are characterized by volatility and uncertainty.

This dissertation was motivated by military organizations in which previous studies on financial incentives and worker characteristics cannot be directly applied. Each military branch has a unique role and specific mission requiring input from many different workers with different demands. Units require the synchronized efforts of members from numerous job-types ranging from finance to pararescue. Coordination is driven by the nature of the task rather than the worker's ability or motivation. The modern workforce faces similar complexities as jobs become more complex and specialized (Malone, Laubacher, and Johns 2011; Burke and Ng 2006; Parteka and Wolszczak-Derlacz 2017; Acemoglu and Autor 2011; Stromquist 2019). Firms face increased coordination requirements, competing in evolving industries characterized by rapid product turnaround, on-demand services, and new areas of expertise such as artificial intelligence (van Dam 2009). Work is characterized by increased volatility and hyper-connectivity (Faraj and Xiao 2006).

This dissertation is comprised of three essays that serve as stand-alone chapters. The first chapter analyzes the effect of time constraints on coordination levels when the worst performing agent determines economic output. We present an experimental analysis of weak-link coordination games in which subjects face specific time constraints in a volatile environment. In our basic 2×2 design, one of the factors involves situations in which all subjects face stringent decision time (seconds to select a strategy) constraints. Another factor varies what we call stringent feedback (seconds to review the outcome of the round) constraints. Overall, we find that stringent decision times lead to lower minimum effort contributions; however, providing higher feedback time improves firm minimum effort levels. Stringent decision constraints reduce subjects´ decision quality. Feedback time impacts coordination outcomes more when decisions are made under time pressure. Limiting subjects' ability to learn from each round and update their beliefs accordingly caused more volatile coordination outcomes resulting in coordination failure in which groups were not able to converge at any equilibria. Increased feedback time seems to improve decision-making in terms of anticipating the behaviors of others and aligning decisions with firm outcomes. This finding suggests that if decisions are made under time duress, adequate recovery time can mean the difference between efficient and inefficient coordination.

In the second chapter, we continue our examination of weak-link organizations with a focus on large, complex organizations in which success relies on many people with jobs that have very different requirements. In this experimental study, we aim to facilitate coordination among large groups by capturing social spillover effects associated with task heterogeneities. We develop an experimental environment to test compositional effects on organizational performance using peer effects to maximize productivity. Though previous experiments have shown that large group sizes result in inefficient outcomes, we demonstrate a situation in which collective efficiency can emerge through social preferences. Our experimental setup models a task environment with two different types of workers. In standard settings, subjects are given five effort choices: $E_i \in \{0, 10,$

20, 30, 40}. We introduce a second type of worker restricted to the highest effort choices: $E_i \in \{30, 40\}$. This has the potential to improve worker productivity and overall firm success if decision-makers are concerned with the welfare of others and believe that others will choose the same, thus introducing the notion of social preferences.

We find that heterogeneous groups induce change to higher, more efficient equilibria. Despite increases in group size, firms with restricted workers report higher overall output. Several firms managed to coordinate at much higher levels, with two firms reaching the most efficient coordination level. Regarding worker behavior, we find evidence of peer effects in effort levels. Employee average effort choices produced positive spillover effects in both experimental conditions. Employees exert more effort when another employee(s) can be harmed in the firm. This paper provides a mechanism to alleviate coordination failure among large groups, demonstrating how social spillovers can overpower group-size effects. These findings have important implications for optimal workplace organization.

In the third chapter, we examine real-world organizations in which employees need to coordinate on the reduction of adverse outcomes. In combat operations, a soldier's objective may be to safely and effectively accomplish the mission while preserving lives and precious resources. In homeland defense, disaster response teams at the state, local, and federal levels must work together to mitigate the effects of disastrous events to minimize human suffering and protect property. Moreover, employees may face situations with severe consequences if co-workers do not contribute the same level of effort. This chapter tests whether participants exert more considerable effort when payoff-equivalent incentives are framed as losses rather than gains. We also intersect loss aversion and social preferences to model real-world situations when employees' actions can cause another employee to bear a loss. We develop an experimental environment to test stake-size effects on organizational performance. Subjects are given an initial endowment at the beginning of the experiment, which allows us to assess decisions at different reference points and test the degree of loss aversion in each setting.

We find significant treatment effects, such that individuals in the high-stake treatments contribute significantly higher effort than subjects in low stake frames. Loss aversion is efficiency-enhancing only in high-stake conditions. In other words, framing effects work if the stakes are high enough. Loss framing with low stakes produced poor coordination outcomes; loss framing with high stakes significantly improves firm minimum effort. Coordination is markedly higher in high stake conditions compared to low stake conditions. Social concerns are influenced by the degree of loss framing. Average effort is higher in social treatments than the corresponding four-employee treatments, but average minimum effort decreases. Loss framing reduces social spillover effects. A closer investigation of firm dynamics and employee-level behavior reveals that peer effects in high stakes produce small positive spillovers. Merely having high stakes with homogenous worker groups is efficiency-enhancing. When the stakes are high, the saliency of consequences on restricted workers is not as important.

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Chapter I: Coordination in Complex Environments with Time Constraints

1 Introduction

Weak-link coordination games model organizations in which the lowest-performing individual determines group performance. Consider an airline, a prototypical example of an organization with weak-link production technology (Weber et al. 2001). An airplane cannot depart before all procedures (e.g., security screening, maintenance checks, fueling, baggage loaded by the ramp crew, catering and cleaning, boarding of passengers) have been completed. On-time departure depends upon the slowest operation. Knez and Simester's (2001) classic case study on Continental Airlines has been a hallmark in providing real-world parallels to weak-link laboratory game designs. The aviation industry is regarded as one of the most fast-paced, high-pressure environments (Bourne Jr and Yaroush 2003; Grote et al. 2010). Pilots consistently make decisions in operational environments, mostly under time pressure. Air traffic controllers must make decisions quickly, directing aircraft on the ground and through controlled airspace. Researchers have typically studied coordination problems in situations that allow the decision-maker unlimited time to make decisions. However, many workplace decisions are made under some time constraint (Kocher et al. 2019; Reid and Ramarajan 2016; Ordóñez and Benson 1997).

This paper aims to analyze the effect of time constraints on coordination levels when the worst performing agent determines economic output. This is relevant to project teams and task forces in which members have no previous experience with the others, and they must complete their tasks quickly. This is apparent in combat operations and disaster response with agencies from numerous jurisdictions. However, this extends beyond military operations and emergency response. Corporations often use temporary teams to solve a business need in a designated amount of time. Corporate task forces are frequently used in product development or to resolve issues

(Ancona and Caldwell 1992). Organizations are increasingly relying on action groups, which conduct complex, time-limited events in challenging environments. In action groups, people with different backgrounds, skills, and roles come together only for a specific task and must immediately coordinate their actions in intense and unpredictable situations (Schmutz et al. 2015; Edmondson 2003; Sundstrom et al. 2000). Examples include trauma teams, military units, special weapons and tactics (SWAT) teams, investment banking, film crews, and emergency responders (Bierly, Gallagher, and Spender 2008; Schulman 1993; Tolk, Cantu, and Beruvides 2015; Berthod, Grothe-Hammer, and Sydow 2015; Weick, Sutcliffe, and Obstfeld 2008; Weick et al. 1999; Meyerson et al. 1996).

Management and organizational scholars have given much attention to "high-reliability organizations" (HROs) in which substandard performance or deviations from standard practices can result in severe, adverse consequences (Rochlin 1996; Roberts 1989; Weick 2015).[1] Examples include air traffic control systems, naval aircraft carriers, firefighting brigades, and emergency medical teams. A large area of research exists on coordination in high-risk industries such as nuclear power operations (Waller, Gupta, and Giambatista 2004), military (Entin and Serfaty 1999), aviation (Grote et al. 2010), and healthcare (Burtscher et al. 2010; Schmutz et al. 2015; Madsen et al. 2006; Tolk, Cantu, and Beruvides 2015). Condensed time factors often characterize HROs – crucial activities are measured in seconds. This has, indeed, been manifested during the rapid escalation of the global COVID-19 pandemic. Governments have a small window of time to slow the spread of the virus. Researchers are making efforts to expedite a vaccine while maintaining safety and efficacy standards. Public health officials must operationalize containment efforts and prepare for the consequences and disruptions. Healthcare providers are on the front

[1] Camerer, Knez, and Weber (2004) use HROs as an example of weak-link games. See also Weber, Camerer, Rottenstreich, and Knez (2001) and Camerer and Knez (1996).

line, testing, triaging, and treating symptoms. This underlines the importance of understanding how time constraints can shape decisions in uncertain environments.

In this study, we are interested in how groups coordinate when participants are faced with different time allotments and whether time constraints play a more significant role in their ability to make a decision or process information on how others behaved in that period. We employ a weak-link game coordination game based on the "corporate turnaround game" by Brandts and Cooper (2006a). Their studies tackle the issue of coordination failure, predominantly when firms are stuck in a trap. Previous studies focus on group-based incentives (Brandts and Cooper 2006a), observability and leadership (Brandts and Cooper 2006b; Brandts et al. 2015; Brandts, Cooper, and Fatas 2007), and communication (Brandts and Cooper 2007; Brandts, Cooper, and Weber 2015). The corporate turnaround game is an experimental setting designed to represent a corporate environment where a group needs to escape from a sub-optimal performance trap. The game involves repeated play between four employees of a firm. In each period, the employees simultaneously choose an effort level. Employees initially face a low bonus rate, trapping groups into the worst possible outcome. The bonus is then increased, transforming the game into a weak-link game (Van Huyck, Battalio, and Beil 1990) with multiple Pareto-ranked equilibria.

We place time restrictions in the active decision-making stage when participants choose their effort level and the feedback stage, which reports the results of the period. We define these as decision constraints and feedback constraints. We are interested in coordination levels when subjects must respond rapidly and do not have the proper time to review all the information and reflect on their decision. Brandts and Cooper (2006b) illustrate the importance of feedback in achieving efficient coordination outcomes. Limited feedback makes it difficult to achieve coordination success in standard settings (see also Berninghaus and Ehrhart 2001). Providing full information feedback improves the ability of firms to overcome coordination failure. This efficiency-enhancing measure did not impact the power of successful organizations to avoid slipping into coordination failure. Time constraints in our experimental conditions are defined as mild (20 seconds) or stringent (5 seconds). Treatment designs follow a dual-process framework (Kahneman 2011). The dual-process theory posits that individuals have two different sets of thought processes by which they make decisions. System 1 thinking is automatic, fast, and non-conscious, whereas System 2 thinking is controlled, slow, and conscious (Kahneman 2011).

We find that time pressure in both decision and feedback stages have significant effects on coordination efficiency. Overall, we find that stringent decision times lead to lower minimum effort contributions; however, providing higher feedback time improves firm minimum effort levels. Stringent decision constraints reduce subjects' decision quality. Feedback time impacts coordination outcomes more when decisions are made under time pressure. Limiting subjects' ability to learn from each period and update their beliefs accordingly caused more volatile coordination outcomes resulting in coordination failure in which groups were not able to converge at any equilibria. Increased feedback time seems to improve decision-making in anticipating others' behaviors and aligning decisions with firm outcomes. This finding suggests that if decisions are made under time duress, adequate recovery time can mean the difference between efficient and inefficient coordination.

The remainder of this chapter is organized as follows—Section 2 of this chapter reviews relevant literature. Section 3 presents the experimental design and treatments, while Section 4 develops our hypotheses. Section 5 provides the procedures, and Section 6 contains the results. Section 7 concludes.

2 Related Literature

Experimental psychologists have extensively studied decision-making under time constraints, though these studies lack a strategic environment central to economic decisions. Experimental economists have recently given a considerable amount of attention to address these gaps (for a recent survey, Spiliopoulos and Ortmann 2018). The current literature on economic games with time constraints primarily focuses on the relationship between reaction times and social preferences. Researchers working with endogenous response time (RT) measure the time taken for a subject to reach a decision. Endogenous RT is a methodological tool often used to study individuals' decision processes. The most common type of exogenous time constraint is time pressure, in which subjects are forced to decide a limited amount of time (Sutter, Kocher, and Strauß 2003; Kocher and Sutter 2006). Time delay studies are also common in which subjects are forced to wait a minimum amount of time before they can respond (Rand, Greene, and Nowak 2012; Grimm and Mengel 2011; Neo et al. 2013). Most studies compare an endogenous RT treatment with exogenous time constraint treatments. These studies are most often viewed from the lens of dual-system models (Kahneman 2011). Dual-system (or dual-process) theories are increasingly applied to decision-making to classify actions as instinctive or deliberative.

Response time analysis has sparked a massive ongoing debate on the role of social preferences and RT. Several experimental studies have used individual's response time in economic games to argue that fair behavior is intuitive (Rubinstein 2007; Evans, Dillon, and Rand 2015b; Rand and Kraft-Todd 2014; Rand 2016; Schulz et al. 2014; Fischbacher, Hertwig, and Bruhin 2013; Di Guida and Devetag 2013; Lotito, Migheli, and Ortona 2013; Nielsen, Tyran, and Wengström 2014; Recalde, Riedl, and Vesterlund 2018). The 'Social Heuristics Hypothesis' (Rand et al. 2014) is the most prominent theory to explain the finding that cooperative decisions are made more quickly than selfish ones (Rand, Greene, and Nowak 2012; Evans, Dillon, and Rand 2014). Empirical support for this theory includes Lotito, Migheli, and Ortona (2013), who found that contributions and RT are negatively related in public goods games. Halali, Bereby-Meyer, and Meiran (2012) report that subjects reject unfair offers more quickly than they accept. Cappelen et al. (2016) found a strong association between short response times and fair behavior in the dictator game, inspiring the "fairness is intuitive" hypothesis. Similar conclusions are reached by (Cone and Rand 2014; Cornelissen, Dewitte, and Warlop 2011; Lotz 2015; Roch et al. 2000; Schulz et al. 2014; Nielsen, Tyran, and Wengström 2014).

This literature, however, is inconclusive, with experimental data revealing both positive and negative correlations. Dietz and Hartog (2006) examined behavior in the ultimatum game with time pressure and cognitive loads. They found that time pressure led to higher offers, but cognitive load did not appear to affect offers. Piovesan and Wengström (2009) found no effect on behavior in the ultimatum game. Tinghög et al. (2013) conducted a study across three countries to explore intuitive thinking on moral judgment and behavior. Using time pressure and altered cognitive load levels while participants made decisions in trolley type dilemmas and dictator games, they did not find an effect on moral judgments or increased giving. Verkoeijen and Bouwmeester (2014) were not able to find evidence of intuitive cooperation. Merkel and Lohse (2019) provide little support for the hypothesis that fairness is intuitive. This finding is in line with several papers (Fiedler et al. 2013; Tinghög et al. 2016; Martinsson, Myrseth, and Wollbrant 2014; Duffy and Smith 2014; Achtziger, Alós-Ferrer, and Wagner 2015; Kocher et al. 2017; Lohse 2016; Capraro and Cococcioni 2016). To explain the contradictory results, Evans, Dillon, and Rand (2015) argued that decision conflict, rather than the extent of intuitive versus reflective processing, drives reaction times in social dilemmas.[2]

These experiments analyze proposer and respondent behavior in public goods games, ultimatum games, and dictator games, not group coordination with multiple equilibria. Weak-link games are well suited to study group behavior. These games do not have the incentive problem of the prisoner's dilemma since all symmetric outcomes are equilibria. Individuals cannot contribute additional effort to compensate for a weak link in the group, nor can they free ride. We address these research gaps by analyzing how groups can attain high coordination levels when participants are faced with differences in time allotments in strategically complex situations. We contribute experimental results to the literature on economic games with time constraints. While coordination problems have been widely analyzed, current studies offer very little on the cognitive and behavioral determinants of coordination failure (Devetag and Ortmann 2007). This chapter examines the cognitive determinants of coordination failure when firms are characterized by complexity and time constraints. To our knowledge, these considerations are new in the economic literature on strategic interaction.

3 Experimental Design

An experimental firm consists of a fixed grouping of four employees who interact for 30 consecutive periods. The firm's productivity in each period is determined by employees' effort choices for the period. Each period is considered a workweek. Intuitively, employees spend 40

[2] Other studies contributing to the economic debate include Ponti and Rodriguez-Lara (2015) who find that impulsive subjects are more inequity inverse and reflective subjects are more selfish except for situations when there is nothing for them to gain. Corgnet, Espín, and Hernán-González (2015) argue that an in-depth analysis of the motivations underlying social behavior could offer an understanding of the contradictions. Their findings reveal that different motives can lead to identical choices in experiments studying social behavior and antisocial motivations can prompt behaviors that could appear as selfish or prosocial. Capraro et al. (2017) tested the debate in India and the U.S. and found that time pressure and low cognitive reflection scores were associated with subjects' concerns for relative shares and that time delay and high cognitive reflection scores are associated with subjects' concerns for social efficiency.

hours a week at work; therefore, we restrict employees' effort levels to ten-hour increments: $E_i \in \{0, 10, 20, 30, 40\}$. Participants begin with a fixed rate wage and a bonus rate, B, dependent upon the group contribution. Effort is costly, with C_i denoting the cost of a unit of effort for the *i*th employee. Each period contains a decision stage and a feedback stage. In the decision stage, players are shown a payoff table and asked to allocate their private resources (working time) between Activity X and Activity Y. After players simultaneously make their decision, they are informed of the minimum number of hours that the workers of the firm have spent on Activity X, their earnings in that period, and their accumulated earnings.

The turnaround game is traditionally broken up into three ten-period blocks with the goal of inducing coordination failure in the first block to pull firms out of performance traps in the following block. Our experiment does not use a block design. A key feature of our design is that we randomly distort the payoffs in each period. Each decision stage presents a new payoff table with changing values of the fixed-wage, cost of effort, and bonus. Additionally, we change the order that the effort levels are presented in the table. Hence, from the initial table, subjects must pay attention to the payoffs in each period. The objective is to avoid subjects selecting an option due to an automatic response. The per-period payoff to employee *i* is given by the following function:

$$\text{Employee } i\text{:} \quad \pi_e^i = 200 - C_i E_i + \left(B \times \min_{j \in \{1,2,3,4\}} (E_j)\right) \tag{1}$$

Table 1 corresponds to the case in which B = 10 and the cost of effort equals 5. This serves as the baseline payoff function of the main decision table. The payoff to each player depends on her choice, indicated by the row, and the minimum choice of all players in a group, indicated by the column. The number in each cell gives this payoff. Notice that if everyone picks the highest possible action, then the payoff-dominant equilibrium in the lower-right corner is selected, while if any of the players choose the lowest possible action, then the risk-dominant equilibrium in the upper-left corner is selected. Players incur a cost of 50 to increase their effort at each level. However, if matched, the net gain is also 50. These values were selected because past experiments have revealed that coordination failure is common, particularly when the amount of the bonus does not exceed the cost by a considerable amount. We know from other studies that lower costs of exerting effort are efficiency-enhancing (Goeree and Holt 2001; 2005; Brandts and Cooper 2006a; 2006b; Devetag and Ortmann 2007; Riedl, Rohde, and Strobel 2016). All payoffs during the experiment were denominated in an artificial currency, experimental currency units (ECU). Subjects have the potential to earn 12,000 ECUs at the end of the 30 periods. ECUs were converted at a rate of one euro per 500 ECUs.

Using Equation 1 as a benchmark, we established a range of values to generate 30 payoff tables, keeping the cost and potential net gain close to 50 in each period and potential net monetary payoffs close to 12,000 ECUs (this is the maximum amount a subject could make over 30 periods if Table 1 were used in all periods). We restrict the feasible fixed-wage rates to the integers $\in$ {195, 196, 197, 198, 199, 200, 201, 202, 203, 204, 205}. The bonus values were restricted to B $\in$ {9.5, 9.6, 9.7, 9.8, 9.9, 10.1, 10.2, 10.3, 10.4, 10.5}. C_i was equal to 4.8, 4.9, or 5. Given the other parameter values, these are the effort costs that do not allow subjects to lose money in a period (specifically if the fixed-wage is less than 200, the cost of effort must be less than 5 to avoid negative payoffs). Table 2 shows sample payoff tables when the fixed-wage is 205, B = 10.2, and C_i = 5. (See Appendix D for screenshots of each payoff table).

Table 1: Employee *i*'s payoff table

		Minimum Effort by Other Workers				
		0	**10**	**20**	**30**	**40**
Effort by Employee *i*	**0**	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

Table 2: Sample employee *i*'s payoff table

		Minimum Effort by Other Workers				
		0	**10**	**20**	**30**	**40**
Effort by Employee *i*	**0**	205	205	205	205	205
	10	155	257	257	257	257
	20	105	207	309	309	309
	30	55	157	259	361	361
	40	5	107	209	311	413

3.1 Time Allotments

Selecting the number of seconds assigned to treatments in exogenous time studies is crucial in the experimental design. We have discussed the literature that attempts to elicit behavior using two different systems in the brain; a matter of seconds could produce different responses. For instance, Nursimulu and Bossaerts (2014) found different risk behaviors in a matter of 2 seconds: subjects were risk-averse when given 1s and risk-neutral at the 3s and 5s mark. Researchers in previous studies use various methods to determine exogenous time allotments following a dual-systems framework.[3] To establish treatment times, we first referred to studies with repeated measures to account for temporal learning effects. In a repeated public goods game by Evans, Dillon, and Rand (2015), the average decision time was 1.3 seconds, and the average response time was 4.5 seconds. We used tools from the field of human-computer interaction to estimate how long a typical subject should spend on each screen.[4] We ran pilot sessions at 3s, 5s, and 10s to ensure that subjects could make choices while feeling pressure to do so. Our pilot sessions revealed changes in behavior at the 5s mark. Lastly, subjects reported how much pressure they felt after the game on a 5-point Likert scale.[5]

We define our treatments according to the level of constraint we place on subjects in each stage. Time constraints are limits of time to make a decision or perform a task. Time constraints in our experimental conditions are defined as mild (20 seconds) or stringent (5 seconds). Guided by a dual-system approach, we chose these time allotments to elicit different system processing. Though our goal was to create a feeling of time pressure in the 5s treatments, time pressure is subjective. Setting a time constraint is not enough to ensure that subjects feel time pressure (Svenson and Benson 1993). Time constraints exist whenever there is a time deadline, even if the person can complete the task in less time. Time pressure indicates that the time constraint induces feelings of stress. Thus, it is possible to have time constraints but no time pressure (Ordóñez and

[3] Subjects under time pressure were typically given 7-10 seconds to make their decision (Evans, Dillon, and Rand 2015b; Tinghög et al. 2013) (Tinghög et al. 2013; Evans, Dillon, and Rand 2015a; Cone and Rand 2014). In reflective treatments, subjects were forced to wait a minimum amount of time before making their decisions (Rand, Greene, and Nowak 2012; Cone and Rand 2014; Evans and van de Calseyde 2017).

[4] The keystroke-level model (KLM) introduced by Card, Moran and Newell (1980) is an appropriate model to use as it considers routine tasks and uses task execution time as the only measurement of performance. The execution times were found by adding the standard operators associated with each screen. KLM provides an execution time of 2.65 seconds for the decision task sequence and 1.3 seconds for the feedback screen.

[5] On average, subjects report feeling low pressure in treatments with 20s decision time (M = 1.89, SD = 1.05 in TC20×20 and M = 2.31, SD = 1.18 in TC20×5) and moderate pressure in treatments with 5s to make a decision (M = 3.07, SD = 1.05 in TC5X20 and M = 3.14, SD = 1.25 in TC5×5).

Benson 1997). We recorded response times for all subjects in all periods. In line with current literature, we measure the response time as the time elapsed from opening the decision screen until closing it again by submitting a decision on the screen.

3.2 Treatments

We have five experimental treatments along two dimensions: time allotment and the stage at which this occurs. The first treatment is a control group without exogenous time constraints, No Time Constraints (NoTC). In TC20×20, subjects have 20s to decide and 20s in the feedback stage to review the results. In TC20×5, subjects are given the same amount of time to decide but restricted to 5s in the feedback stage. In TC5×20, subjects have 5s in the active decision-making stage and 20s to review the results of the period. In the final treatment, TC5×5, subjects are given 5s in the decision stage and 5s in the feedback stage. Table 3 summarizes the main characteristics of each treatment.

Table 3: Summary of treatments

Treatment name	NoTC	TC20×20	TC20×5	TC5×20	TC5×5
Time allotment: Decision stage	None	20s	20s	5s	5s
Time allotment: Feedback stage	None	20s	5s	20s	5s
Number of firms	20	12	11	11	11
Number of employees	80	48	44	44	44

4 Behavioral Predictions

In the treatment without time constraints, we expect low average contribution levels as demonstrated through previous studies in which coordination outcomes often converge to an inefficient equilibrium (Brandts, Cooper, and Fatas 2007; Cartwright, Gillet, and Van Vugt 2009; Hamman, Rick, and Weber 2007; Brandts and Cooper 2006a). Predicting behavior in time-constrained environments is not as straightforward. We expect firm production levels and individual worker performance to decrease when we impose exogenous time constraints. This postulation is due to increased cognitive workload and the vast literature on behavior differences when subjects face time constraints.[6] Even the perception of time pressure, when none exists, can affect behavior (Svenson and Benson 1993; Dedonno and Demaree 2008; Maule, Hockey, and Bdzola 2000). Subjects are not aware of the time limits in other treatments; subjects with 20s may feel pressure unaware that subjects in other sessions have 5s.

The "Social Heuristics Hypothesis" (Rand et al. 2014) and the "Fairness is Intuitive Hypothesis" (Cappelen et al. 2016) suggest that intuitive decisions made in the stringent decision constraint treatments would favor fairness and cooperation. Between-subject heterogeneity is often high in strategic decision-making settings, and in this game, it only takes one player to pull the whole group to an inefficient level. Thus, the above hypotheses do not apply to this setting. Dual-system models imply that subjects use an automatic, intuitive response with 5s in the decision stage and a controlled, deliberate response with 20s. Judgment and decision-making research has demonstrated that cognitive processing is altered when decisions are made under time pressure (Rand, Greene, and Nowak 2012; Evans, Dillon, and Rand 2014; Lotito, Migheli, and Ortona 2013; Rand et al. 2014; Spiliopoulos, Ortmann, and Zhang 2018). Subjects use heuristic decision-making rules when their processing potential is low, and they are more likely to rely on pre-existing strategies. This provides the basis for our hypothesis on the effects of decision time constraints.

[6] Kocher and Sutter (2006) reported that time pressure reduces a subject's depth of reasoning. Kocher, Pahlke, and Trautmann (2013) found that time pressure minimizes an individual´s decision quality. In a repeated public goods game, Recalde, Riedl and Vesterlund (2018) found that the shorter RT was, the more likely errors were.

Hypothesis 1 *(Decision constraints)* Treatments with stringent decision constraints will result in inefficient coordination outcomes.

Our second hypothesis is on the impact of stringent feedback time on coordination outcomes. Psychological research reports that time pressure limits information processing (Maule, Hockey, and Bdzola 2000; Weenig and Maarleveld 2002). Limiting subjects' ability to learn from each period and update their beliefs accordingly could cause unstable coordination outcomes. If feedback time is low, it could be equivalent to a no-feedback setting. In that case, subjects may play as if it were a one-shot game. Contributions could be higher in this treatment, but this could imply a different type of coordination failure in which groups are not able to converge at any equilibria.[7]

Hypothesis 2 *(Feedback constraints)* Treatments with stringent feedback constraints will result in volatile coordination outcomes as subjects are unable to update their beliefs or adjust their effort choices as best responses.

Stringent decision constraints may cause participants to make their initial decisions using a simple heuristic. History-dependent behavior is not likely when subjects face high cognitive constraints; therefore, subjects will use this heuristic throughout the game. However, when given 20s in the feedback stage, they may have the ability to review the period's outcome and update their decision as the best response to the group. Therefore, feedback time may matter more when subjects are under stringent time constraints in the decision stage. Hypothesis 3 outlines the effects.

Hypothesis 3 *(Decision and feedback constraints)* Feedback constraints are more likely to impact coordination outcomes in treatments with stringent decision constraints.

[7] Van Huyck, Battalio, and Beil (1990) distinguish between two types of coordination failure. The first occurs when subjects coordinate at an inefficient equilibrium, called collective coordination problem. The second type happens when subjects fail to coordinate on any equilibrium, the individual coordination problem.

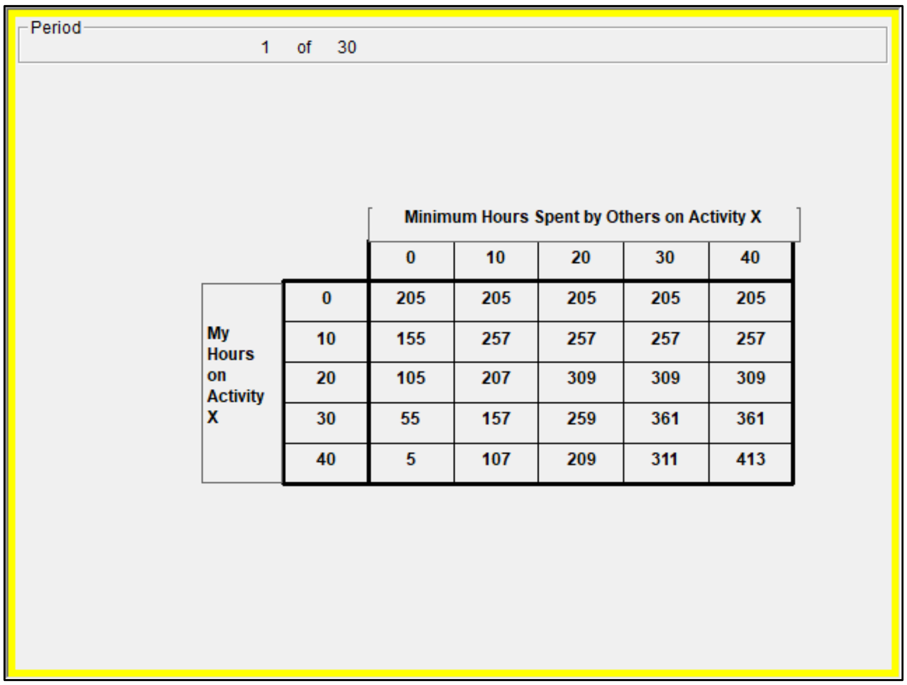
5 Procedures

All sessions were run in the business department at the Autonomous University of Barcelona (UAB) using the experimental software, z-Tree (Fischbacher 2007). We recruited 308 subjects (140 female, M_{age} = 24 years) through the Online Recruitment System for Economic Experiments, ORSEE (Greiner 2015). Subjects received five euros to arrive on time. Participants could only participate once. The average length of a session was 90 minutes from the time students checked in with the lab to the time they left the lab. The average session size was 12. The average payoff, including the five-euro show-up fee, was 17.00 euros.

At the beginning of each session, subjects were randomly seated and asked to read the consent form. Once subjects had signed and dated the consent form, the experimenter collected the forms and signed as a witness. The experimenter instructed subjects to refer to the paper instructions while she read aloud. At the end of the instruction set, participants were given a pre-quiz to ensure they understood the game's design. Subjects were asked to raise their hand and wait for an experimenter to come to their terminal if they had any questions about the game. The full text for the instructions and pre-quiz is given in Appendix A.

The instructions used a corporate context. Subjects were referred to as employees and were told they are working for a firm. To avoid asking subjects to choose a level of effort, we asked them to allocate time between Activity X and Activity Y, with Activity X playing the role of effort. Subjects were informed of how many employees were in each firm, which remained fixed throughout the 30 periods. Subjects were told that each period would present a new payoff table with changing values of the fixed-wage, cost of effort, and bonus. They were provided a range of values and given examples of different payoff tables.

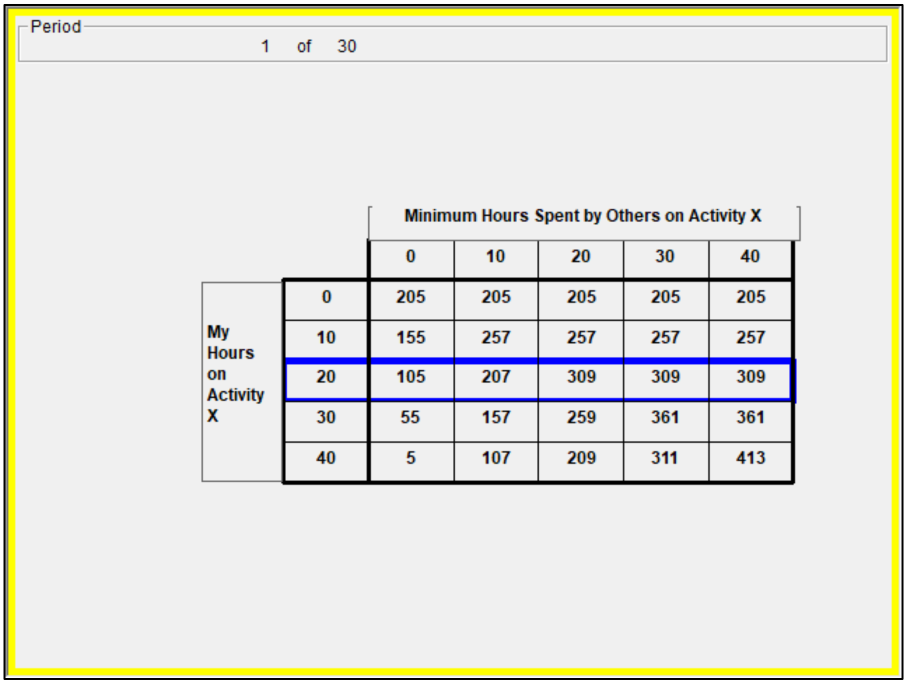
At the start of each period, subjects were shown a payoff table and asked to make their selection. Figure 1 provides an example of a decision screen. We designed the decision screen to be as clear and straightforward as possible, consisting solely of the header displaying the number of the period and the payoff table in which subjects clicked directly on the row corresponding to their decision. Once they selected the effort level, the entire row was highlighted, and subjects were immediately routed to a waiting screen. This is shown in Figure 2.



Period
1 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	0	205	205	205	205	205
	10	155	257	257	257	257
	20	105	207	309	309	309
	30	55	157	259	361	361
	40	5	107	209	311	413

Figure 1: Decision screen



	0	10	20	30	40
0	205	205	205	205	205
10	155	257	257	257	257
20	105	207	309	309	309
30	55	157	259	361	361
40	5	107	209	311	413

Figure 2: Decision screen with a highlighted choice

If a subject did not enter a decision in the allotted time, a default effort level of zero was selected, and this was considered a missing observation for that subject in that period. Altogether, 3.3 percent of the decisions were missing. Missing observations accounted for .9 percent of observations in TC20×20 and .5 percent of TC20×5 observations. Not surprisingly, subjects were more likely to violate the time constraint in conditions with 5s in the decision stage. Missing observations accounted for 8 percent of the observations in TC5×20 and 13 percent of the observations in TC5×5.[8] This could impact subjects' beliefs about the minimum played by others, particularly in the beginning. Subjects could also be more willing to continue choosing higher levels of effort and endure the costs of higher effort choices for longer durations, attributing low minimum levels to the default selection. In addition to the risk of someone in the group choosing

[8] One session alone accounted for 124 of missing values. There were 16 participants in that session. In rare cases, the blue box that highlighted the chosen row would not appear in time, even though the response was captured. Subjects clicked several times to ensure their response was captured, and this caused others to do the same. This can overload the software. For this reason, we limited the remaining sessions to 12 subjects.

a lower effort, subjects also face the risk that others cannot decide on time, pulling the group down to the lowest effort level. Subjects may anticipate themselves or someone in the group will violate the time constraint and choose the lowest effort to avoid the risk.

After each period, subjects were informed of the minimum effort of the group, their earnings in that period, and their accumulated earnings. Figure 3 provides an example feedback screen. Subjects in the control treatment did not have time restrictions. They could proceed to the feedback stage once all members of their group were ready. A new period began when all the groups in the session were ready. In the experimental treatments, each screen was displayed for the time allotted, and then subjects were automatically routed to the next stage. The running clock was not displayed in the header. The clock and default time triggers in z-Tree have approximately a 1.7-second delay.

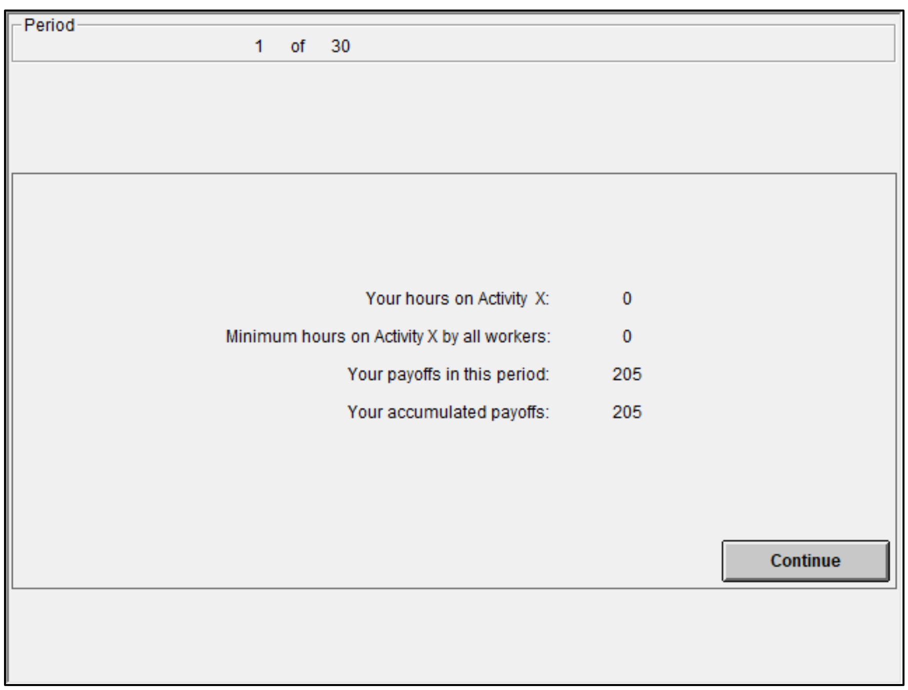


Figure 3: Feedback screen

Response times (RT) were recorded for all subjects in all periods. RT is a measure of the time a subject spends in each period. A period did not begin until all groups were ready. Subjects made their decision and were taken to a waiting screen until all members of their group decided. Each group could proceed to the feedback stage, but they were routed to a waiting screen until all groups were ready. Thus, capturing the time spent in a period per subject is not an accurate measure since it depends on everyone in the session. We addressed this by breaking down the response times at each stage. Decision time (DT) began when the payoff screen appeared, and the final time was recorded when the subject clicked the mouse, indicating her chosen effort level for that period. Feedback time (FT) began when the screen appeared for that subject, and the final time was recorded when she clicked the Continue button, indicating she was ready to proceed to the next period. These stage times were added together for the response time (RT), or total time spent in a period by a subject.

At the end of the game, subjects were asked to fill out a questionnaire. During this time, the experimenter calculated the earnings and prepared envelopes with cash earnings. The methods carried out in this experiment were approved by the ethical committee at UAB.

6 Results

This section investigates the impact of the experimental treatments on firm production levels and individual worker performance. We use three metrics to evaluate performance: effort, minimum effort, and waste. The term effort is used for the value chosen in period t by employee i. Minimum effort refers to the minimum contribution of all four employees in a firm in period t. Waste is defined as the difference between the employee's effort level and the firm's minimum effort in period t. Unless otherwise noted, treatment effects report results of Mann-Whitney tests using firm averages across all 30 periods.

6.1 Treatment Effects

Successful coordination requires groups to coordinate at the Pareto equilibrium; this was not achieved in any treatment. In the control treatment without exogenous time constraints, the minimum effort was above zero in the final period for 10 of the 20 firms. Four out of 12 firms attained coordination levels above zero in TC20×20; 4 out of 11 firms in TC20×5 finished the game above zero. In TC5×20, 2 out of 11 firms coordinated at levels above zero. In TC5×5, all firms converged to the least efficient effort level. Table 4 reports descriptive statistics at the firm level, across periods. Detailed pictures of how average minimum effort and average effort evolve over time are given by Figures 4 and 5.

Table 4: Summary of results

Treatment Name	NoTC	TC20×20	TC20×5	TC5×20	TC5×5
Avg Effort	13.28	12.09	12.52	9.90	12.02
Avg MinEffort	8.23	7.16	6.33	3.45	3.00
Avg Waste	5.03	4.93	6.18	6.45	9.02
Avg Earnings (ECUs)	6,535	6,358	6,038	5,566	4,943

This shows that average effort in the experimental treatments is not substantially different from the control treatment. At the firm level, we see similar output in NoTC and mild decision constraint treatments (pooling TC20×20 and TC20×5). The differences compared to NoTC, however, are not significant ($p = .320$; $p = .146$). Average minimum effort levels are substantially lower when decision constraints are high: 3.45 in TC5×20, and 3.00 in TC5×5 ($p = .014$; $p = .026$). Figure 4 illustrates distinct downward trends of average minimum effort levels in TC5×20 and TC5×5 compared to the other treatments, and this divide becomes more pronounced over time.

Trend tests report significantly decreasing minimum effort levels in TC5×5 (p =.002). In TC5×20, average minimum effort decreases over the rounds, though this is not a significant trend (p = .149).

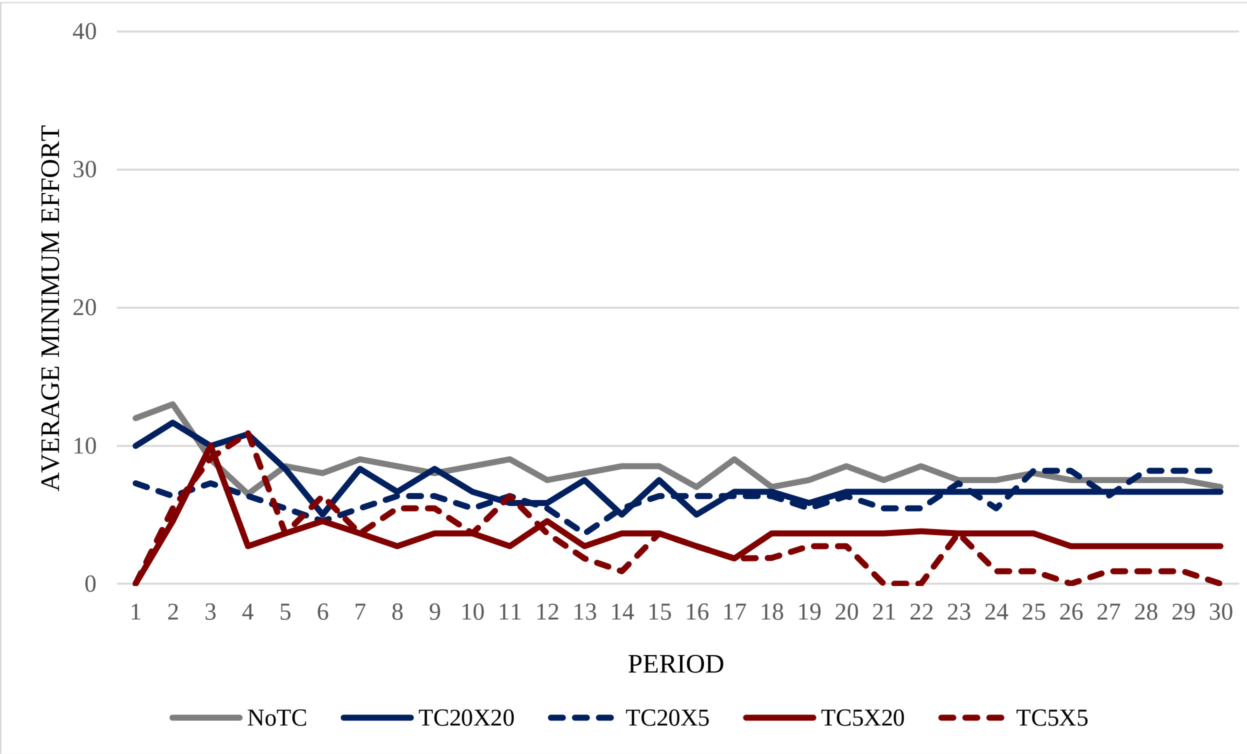


Figure 4: Average minimum effort by treatment

Comparing average effort at the employee level yields a different picture. Employee average effort in the time-constrained treatments is not substantially different from NoTC (13.28). The mean of average group effort is 12.09 in TC20×20 and 12.52 in TC20×5. Mann-Whitney tests do not show statistical difference among the treatments (p = .484 for TC20×20; p = .482 for TC20×5). Compared to NoTC, average effort in stringent decision constraint treatments (pooling TC5×20 and TC5×5) is lower. The differences, however, are not significant (p = .214; p = .676).[9]

[9] The average effort reported includes observations for which subject's did not enter a choice on time, which defaults to 0 hours. The average effort not including the default selections is 12.2 in TC20×20, 12.59 in TC20×5, 10.81 in TC5×20, and 13.89 in TC5×5. The change in average effort is not significant compared to NoTC (p = .552; p = .482; p = .476; p = .669).

Figure 5 presents the evolution of effort across 30 periods. We observe little difference between the control treatment and treatments with 20s decision time in the first period. Period one average effort is 23.60 in NoTC, 22.92 in TC20×20 (p = .678), and 22.95 in TC20×5 (p = .630). The first period average effort is 8.6 in TC5×20 (p = .001) and 6.6 in TC5×5 (p = .001). In the first period, choosing the lowest option of 0 hours is 5 percent in NoTC, 15 percent and 16 percent in TC20×20 and TC20×5, 64 percent TC5×20, and 79 percent in TC5×5. TC20×20 peaks in the second period with 11.67 before taking a steep drop. Average effort levels off by period 20 while TC20×5 is not steady until the final periods. While we observe TC5×20 and TC5×5 fall significantly below the other treatments in Figure 4, we see different patterns of effort choices in Figure 5. TC5×20 drops below the other treatments, while TC5×5 remains close to the other treatment levels. Trend tests report significantly decreasing effort levels in all treatments (p < .001).

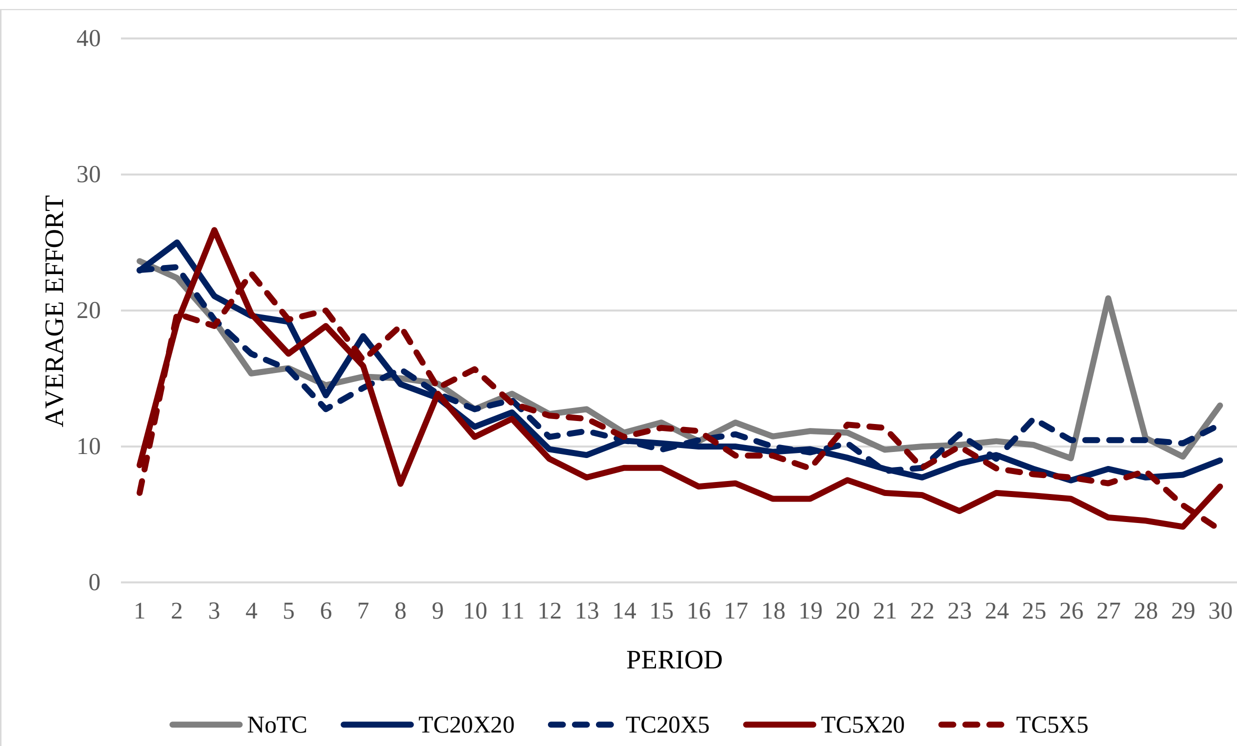


Figure 5: Average effort by treatment

We also look at the difference between the frequencies of choosing the risk-dominant and the Pareto-dominant outcomes. When subjects do not have any time constraints, 27 percent of selected effort levels are the lowest effort level of zero. This frequency nearly doubles to 51 percent in TC5×20 and 48 percent in TC5×5. Mild decision constraint conditions report similar safe option frequencies: 45 percent in TC20×20 and 46 percent in TC20×5. Choosing the Pareto-dominant option is highest in TC20×5 (11 percent) followed by TC5×5 (9 percent), TC5×20 (7 percent), and NoTC and TC20×20 (6 percent).

Across treatments, we observe substantial differences between the employee's average effort and the firm's average minimum effort. This is referred to as waste, which is a good measure of convergence. Low waste indicates strong convergence to equilibrium. If waste were zero, groups would fully coordinate at one of the equilibrium points. We consider waste a measure of decision quality. The reason being that effort above the minimum is a cost that subjects incur, and subjects learn this soon in the game. In the control treatment, average waste is 5.03. Using waste as a performance measure of decision-making ability, we should expect decision quality in mild decision-constrained treatments to be close to the control treatment. Average waste is lowest in TC20×20 (4.93) (p = .796) followed by TC20×5 (6.18) (p = .265).[10] This indicates that decision-making was the result of a controlled process. Thus, we can presume that decisions made under treatments with 20s align with expected decisions under System 2.

Heuristic decision-making manifests in stringent decision constraint treatments. Compared to the control treatment, waste levels are significantly higher in both TC5×5 (9.03) and TC5×20 (6.45) (p = .001; p = .058). Decision quality is lower in stringent decision constraint treatments.

[10] The average waste reported includes observations for which subjects did not enter a choice on time, which defaults to 0 hours. Average wasted effort did not change in mild decision constraints but increased to 7.04 in TC5×20 and 10.42 in TC5×5 (p = .005; $p < .001$).

This suggests that subjects exhibited intuitive decision-making as a result of System 1 processing. High levels of wasted effort suggest that earnings will also be lower. Taken over all periods, subjects in NoTC earn an average of 6,535 ECUs. Average earnings in TC20×20 are slightly lower (6,358 ECUs) (p = .182) and 6,038 ECUs in TC20×5 (p = .059). Earnings in the other time-constrained treatments are economically and statistically significantly less. Subjects earn an average of 5,566 ECUs in TC5×20 (p = .001) and 4,943 ECUs in TC5×5 (p = .001). Trend tests report significantly increasing profit over periods ($p < .001$) in TC5×20 and decreasing profit in TC5×5, though not significant (p = .917). This tells us that decisions are unstable in TC5×5. In TC5×20, subjects learn to overcome the individual coordination problem and coordinate on the same effort level.

We hypothesized that stringent decision constraints would result in inefficient coordination outcomes, and shorter time in the feedback stage would generate volatile outcomes. We summarize our findings on the effect of decision and feedback constraints in the following results:

Result 1 *(Decision constraints)* Stringent decision constraints reduce firm productivity, as measured by minimum effort. The data supports Hypothesis 1.

Result 2 *(Feedback constraints)* Short feedback time leads to a failure of subjects to coordinate their actions. Increased feedback time seems to improve decision-making in anticipating others' behaviors and aligning decisions with firm outcomes.

6.1.1 Decision Constraints

We now compare behavior in the two treatments with 20s decision time. Average minimum effort is 7.16 in TC20×20 and 6.33 in TC20×5 (p = .657). Average effort in TC20×20 is 12.09 and 12.52 in TC20×5 (p = .964). Waste in TC20×20 (4.93) is lower than waste in TC20×5 (6.18), though not significant (p = .324). As an alternative measure of stability, 37 percent of observations from

TC20×20 are Nash equilibria. The equivalent figure for TC20×5 is 32 percent. Figure 6 shows the side-by-side treatment comparisons.

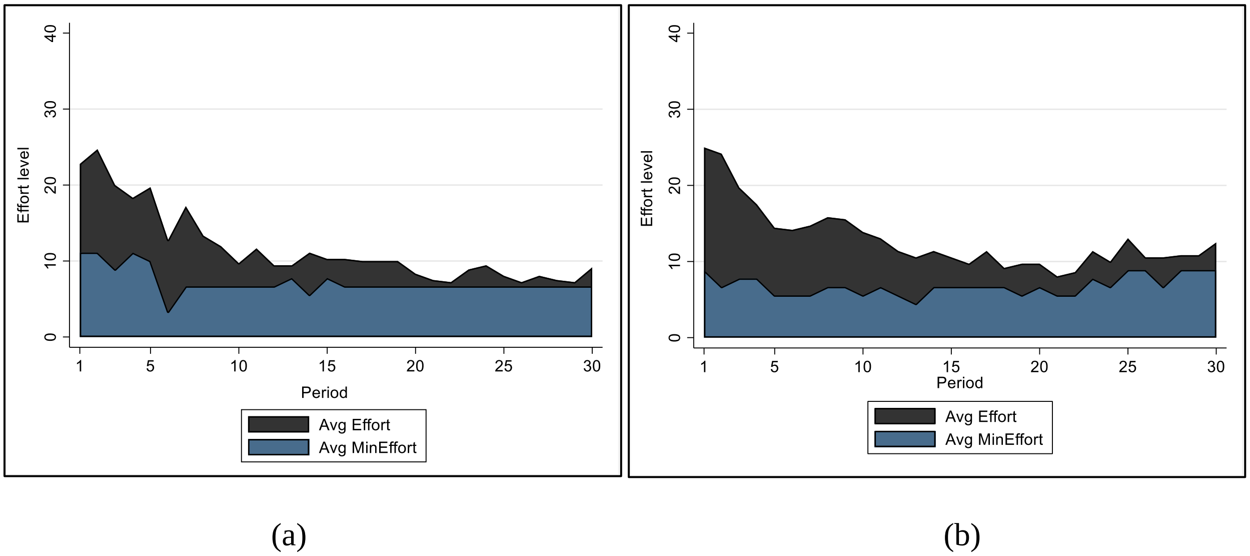
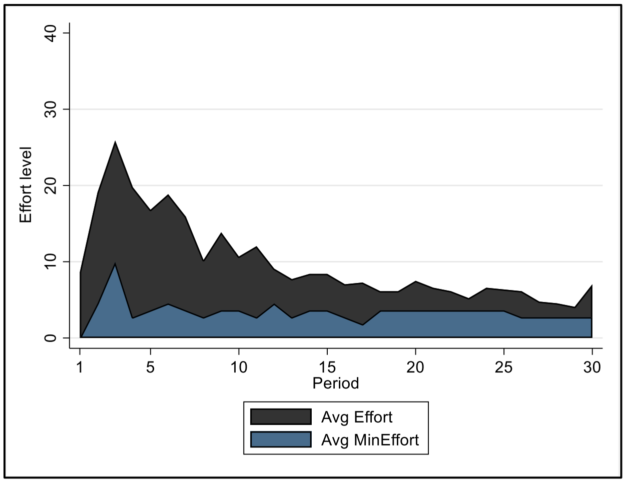
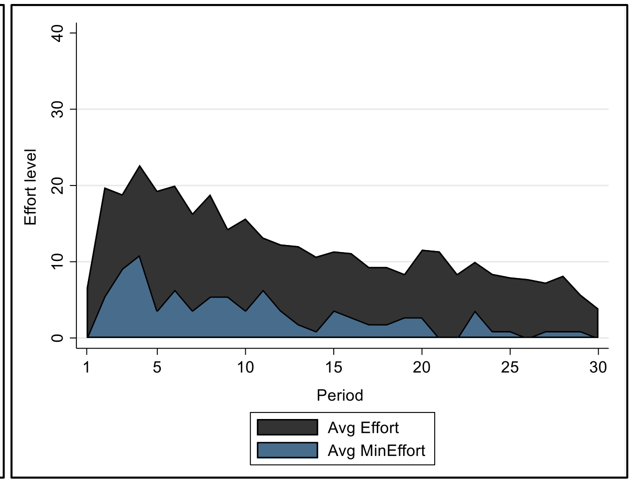


Figure 6: Average effort and minimum effort levels per treatment (a) TC20×20; (b) TC20×5

Comparing behavior in the two treatments with 5s decision time, we see that the distance between average effort and average minimum effort reduces significantly when additional feedback time is provided. Average minimum effort is 3.45 in TC5×20 and 3.00 hours in TC5×5 (p = .636). Average effort is 9.9 in TC5×20 and 12.02 in TC5×5 (p = .339). Waste in TC5×20 (6.45) is substantially lower than in TC5×5 (9.02) (p = .047). When subjects have 5s decision time, stringent feedback time generates volatile contributions indicating a failure of subjects to coordinate their actions. In TC5×5, 15 percent of observations are Nash equilibria. This nearly doubles when feedback time increases in TC5×20 (29 percent). This suggests that subjects can improve their decisions. Figure 7 reveals that increased feedback time in TC5×20 seems to enhance decision-making in anticipating others' behaviors and aligning decisions with firm outcomes. This suggests that if decisions are made under time pressure, adequate recovery time can mean the difference between inefficient and efficient coordination.



(a)



(b)

Figure 7: Average effort and minimum effort levels per treatment (a) TC5×20; (b) TC5×5

6.1.2 Feedback Constraints

In stringent feedback conditions, we predicted that subjects would use a simple heuristic, unable to react to group outcome. Waste is considerably lower in TC20×5 (6.18) than TC5×5 (9.02) (p = .056, n = 22). We created a dummy variable that measures the sum of the absolute differences in effort chosen in a period from the effort selected in the previous period.[11] The mean absolute value for this difference in lagged variables is 5.64 in TC20×5 and 9.26 in TC5×5. Turning to the conditions with 20s feedback time, we see much less variation. Wasted effort is 4.93 in TC20×20 and 6.45 in TC5×20 (p = .164). The mean absolute value for this difference in lagged variables is 5.04 in TC20×20 and 6.01 in TC5×20. When subjects have 20s review time, the difference between 20s decision time and 5s decision time is not as significant. We hypothesized that feedback constraints were more likely to impact coordination outcomes in treatments with stringent decision constraints. We summarize our findings on the effect of decision and feedback constraints in the following result:

[11] This measurement is similar to the Total Variation (TV) variable used by Oprea, Charness, and Friedman (2014).

Result 3 *(Decision constraints and feedback constraints)* In mild decision constraints, increased feedback time induces a small and statistically significant marginal improvement in coordination outcomes relative to stringent feedback time. In stringent decision constraints, increased feedback time leads to a substantial improvement in decision-making.

7 Conclusion

In this article, we examine the effect of time constraints on coordination outcomes in the weak-link game. We study how firms can attain high coordination levels when employees are faced with differences in time allotments in highly volatile and strategically complex situations. In our basic 2×2 design, one factor involves situations in which all subjects face stringent decision time (seconds to select a strategy) constraints. Another factor varies what we call stringent feedback (seconds to review the outcome of the period) constraints. Overall, we find that stringent decision constraints lead to lower minimum effort levels, but increased feedback time seems to improve decision-making in anticipating others' behaviors and aligning decisions with firm outcomes.

Average minimum effort is substantially lower when decision constraints are high. When subjects have 5s decision time, stringent feedback generates volatile contributions indicating a failure of subjects to coordinate their actions. Lagged effort variables and waste levels across time indicate that subjects were using strategic considerations in stringent decision-constrained treatments, but this was at a prolonged rate. Increased feedback time in TC5×20 improved decisions. Increased feedback time seems to improve decision-making in anticipating others' behaviors and aligning decisions with firm outcomes. This suggests that adequate recovery time can mean the difference between complete coordination failure and inefficient coordination for decisions made under severe time pressure.

In mild decision constraints, increased feedback time induced a small and statistically significant marginal improvement in coordination outcomes relative to stringent feedback time. In stringent decision constraints, increased feedback time led to a substantial improvement in decision-making. One of our research questions asks if there is a more significant impact when subjects face time constraints in the decision stage or the feedback stage. Our results indicate that there is not a clear answer to this question. In interrelated settings, aligning actions at the right time is critical. While stringent decision constraints had a clear impact on coordination outcomes, stringent feedback constraints limited subjects' ability to learn from each period.

In stringent feedback conditions, we predicted that subjects would use a simple heuristic, unable to react to group outcome. Feedback constraints impacted coordination outcomes in treatments with stringent decision constraints more than mild decision constraints. When subjects had 20s review time, the difference between 20s decision time and 5s decision time was not significant. This finding complements previous studies illustrating the importance of feedback in achieving efficient coordination outcomes. Limited feedback makes it difficult to achieve coordination success in standard settings (Berninghaus and Ehrhart 2001; Brandts and Cooper 2006b).

Our manipulations produced decision-making supported by a dual-systems framework. To assess individual decision-making quality, we used the variable waste. Low levels of waste indicated that decision-making was the result of a controlled process. Thus, we can presume that decisions made under treatments with 20s are in line with expected choices under System 2. There exists ample evidence in the psychology literature that increasing cognitive load leads to a higher probability that simple heuristics will be used (Harbaugh, Krause, and Vesterlund 2010; Betsch et al. 2004; De Neys 2006). Heuristic decision-making was evident in stringent decision constraint treatments. Decision quality was significantly lower. This suggests that subjects exhibited intuitive decision-making as a result of System 1 processing. Time pressure negatively impacted individual decision-making. The results align with psychological research that time pressure limits information processing (Maule, Hockey, and Bdzola 2000; Weenig and Maarleveld 2002). Kocher and Sutter (2006) reported that time pressure reduced a subject's depth of reasoning. Kocher, Pahlke, and Trautmann (2013) found that time pressure minimizes an individual's decision quality.

We contribute experimental results to the literature on economic games with time constraints. Our findings have significant implications for the understanding of heuristic decision-making in time-constrained, strategic interactions. Time pressure is a central facet of economic decision making these days (Kocher et al. 2019). Research on coordination in high-risk industries (HRIs) is essential since errors caused by poor coordination could result in severe consequences that harm or even kill people. This has been evident during the COVID-19 pandemic. The results have implications in practical contexts and can help set the stage for further research that recognizes job-specific adverse conditions in weak-link firms.

Our findings have applications in the management literature, particularly in organizations such as HROs. Firms that operate in time-constrained environments do not have the option to change the rapid tempo of operations. However, firms can improve decision-making and overall output by allowing employees adequate time to recover after high-stress periods. For instance, the U.S. Army trains members to function in the most stressful environments. Still, they take extensive measures to ensure that members properly learn from each experience and extract lessons from one event to apply it to others. An after-action review (AAR) is the primary method of reviewing individual performance, performance at the unit level, and the ultimate impact on the mission (Sawyer and Deering 2013; Baird, Holland, and Deacon 1999). AARs have become a popular business tool (Rodriguez and Talbot 2005), adopted by various organizations (Levy 2011; Sawyer and Deering 2013; Cronin and Andrews 2009; Scott et al. 2015). U.S. Air Force fighter pilots use a unique, disciplined, rigorous briefing and debriefing process to provide a more in-depth examination of particular actions and underlying cognitions (Ron, Lipshitz, and Popper 2006).

While this study provides an opportunity to advance the understanding of organizational social spillovers in coordination games, we acknowledge this study's limitations. There are potentially relevant variables that were not controlled that could have affected individual behavior in these interactions. For instance, we did not measure the subjects' cognitive abilities. The Cognitive Reflection Test (CRT) is a 3-item task commonly used to measure individual cognition (Frederick 2005). Cappelen et al. (2016) found significant heterogeneity in participants' cognitive abilities and show that these characteristics matter in response time studies.

While coordination problems have been widely analyzed, current studies offer very little on the cognitive determinants of coordination outcomes (Devetag and Ortmann 2007). Expanding our understanding of coordination behavior is an important direction for future research. Further studies in both the lab and the field can help shed light on this vital and underexplored question in the context of coordination under time constraints.

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Appendix A: Experimental Instructions

[**Remark:** In the following, we present the instructions for treatment TC20×20. Paragraphs with **[*]** were given in experimental treatments with exogenous time constraints (TC Treatments) but appropriately reformulated for TC20×5, TC5×20, and TC5×5. The instructions for the baseline treatment, NoTC, were identical except the paragraph marked with **[NoTC].** A complete set of instructions is available from the authors.]

INSTRUCTIONS

The purpose of this experiment is to study how individuals make decisions in certain contexts. In addition to the five-euro participation fee, you will be paid any additional money you accumulate at the end of today´s session. All payoffs during the experiment are denominated in an artificial currency, experimental currency units (ECU). At the end of the experiment, ECUs will be converted at a rate of one euro per 500 ECUs. Upon completion of the experiment, your earnings will be converted to euros, and you will be paid privately in cash. The exact amount you receive will be determined during the experiment and will depend on your decisions and the decisions of others. If you have any questions during the experiment, please raise your hand and wait for an experimenter to come to you. Please do not talk, exclaim, or try to communicate with other participants during the experiment. Participants intentionally violating the rules may be asked to leave the experiment and may not be paid.

Decision rounds: The experiment consists of 30 rounds. At the end of the game, we will ask you to answer some questions. In each round, you will be in a group with three other participants. The composition of these groups, which are called *firms,* will not vary during the experiment. Given that nobody will know the identity of the members of each group, all the actions you take during the experiment will be anonymous.

Task: You and the other members of your group are employees of a firm. You can think of a round of the experiment as being a workweek. Each of the four employees spends 40 hours per week at their firm. Your task will be to decide how to allocate your time between two activities: Activity X and Activity Y. Specifically, you will be asked to choose how much time to devote to Activity X. The available choices are 0 hours, 10 hours, 20 hours, 30 hours, or 40 hours. The remaining hours will be put towards Activity Y. For example, if you devote 30 hours to Activity X, this means that 10 hours will be put towards Activity Y.

Employee payoffs: An employee's payoffs will be determined by the number of hours that employee spends on Activity X, the minimum number of hours that employees in his or her firm spend on Activity X, the base salary, a bonus that depends on the minimum number of hours spent on Activity X by any of the members of your group, and the cost of effort. These payoffs are summarized by the formula below:

Employee Earnings = Base Salary − (Cost*Your hours on Activity X) + (Bonus*Minimum hours spent by other employees on Activity X).

The values of the Base Salary, Bonus, and Cost will not be the same in each round. Specifically, they will change randomly and with equal probability. The Base Salary will take values between 195 and 205: 195, 196, 197, ..., 205. The Bonus will take values between 9.5 and 10.5: 9.5, 9.6, 9.7, ..., 10.5; the cost will take the values of 4.8, 4.9, or 5. In addition, on your screen, you will always have information about the winnings for each possible decision in each round. If you don't understand the above formula, don't worry. It is provided to you as an additional way to understand your payoffs. The computer always shows your payoff table at any point where you need to make a decision. The tables include all of the information you need to make a decision.

Playing a Round as a Firm Employee: For each round of the experiment, the computer will display a table like the one shown below. Your earnings in each round can be found by looking at the hours you can choose to dedicate to Activity X (on the left side of the table) and the columns that indicate the minimum number of hours spent in Activity X by the other members of the group. This table will be the same for each member of the group. For the payoff table shown below, the Base Salary is equal to 200, the Bonus is equal to 10, and the Cost is equal to 5. Remember that when the game begins, the numbers that appear in the table may be different, as explained above.

Example:

Employee Earnings = 200 − (5∗Your hours on Activity X) + (10*Minimum hours spent by other employees on Activity X)

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours on Activity X	**0**	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

[NoTC TREATMENT]
To choose the number of hours to spend on Activity X, you can click any of the cells within a row that aligns horizontally with **My Hours on Activity X**. Doing so will highlight the row. You must stay within the boundaries of the row. In each round, the order of the rows will change, i.e., the 30-hour option may be in the first row or another position. Please be sure of your answer before you click on the row, as this will take you to the next screen. *You cannot change your answer once you select your number of hours.* Once you have chosen your option, the program will take you to another screen where you must click on the button labeled *Continue*. When you make your decision, you will not know what the other members of your group are selecting, nor will they know what you select.

Feedback as an employee: At the end of each round, you will receive a summary of what happened in that round including the number of hours that you spent on Activity X, the minimum number of hours spent by the other employees on Activity X, your earnings in that round, and your accumulated earnings through the current round.

[TC TREATMENTS]
To choose the number of hours to spend on Activity X, you can click any of the cells within a row that aligns horizontally with **My Hours on Activity X**. Doing so will highlight the row. You must stay within the boundaries of the row. In each round, the order of the rows will change, i.e., the 30-hour option may be in the first row or another position. Please be sure of your answer before you click on the row, as this will take you to the next screen. *You cannot change your answer once you select your number of hours.* When you make your decision, you will not know what the other members of your group are selecting, nor will they know what you select.

[*] You will be asked to make your choice within 20 seconds. If you haven't made a decision within this time, the program will automatically assign you 0 hours dedicated to Activity X. The same will happen to any member of the group.

[*] Feedback as an employee: At the end of each round, you will receive a summary of what happened in that round including the number of hours that you spent on Activity X, the minimum number of hours spent by the other employees on Activity X, your earnings in that round, and your accumulated earnings through the current round. This screen will also be available for 20 seconds, and then you will automatically be routed to the next round**.** Additionally, there is an option to select continue, and this will take you to a waiting screen until the 5 second waiting period is over.

Confidentiality and Payment: At the end of the experiment, you will be paid, in cash, the sum of the payoffs that you have earned in the rounds of the experiment along with the five-euro show-up fee. As noted previously, you will be paid privately, and we will not disclose any information about your actions or your payoff to the other participants in the experiment.

EXAMPLES AND QUESTIONS

Before we begin the experiment, we will have two short quizzes: one about your payoffs as an employee, and one testing general knowledge about the experiment.

For the following questions, use the information in the example table below. Please raise your hand if you are having trouble answering one of the questions.

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours Spent on Activity X	**20**	99	196	293	293	293
	0	197	197	197	197	197
	40	1	98	195	292	389
	30	50	147	244	341	341
	10	148	245	245	245	245

-Suppose that you choose to dedicate 10 hours to Activity X. The other employees in your firm decide to dedicate 30, 20, and 10 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

-Now suppose that you choose to dedicate 30 hours to Activity X. The other employees in your firm decide to dedicate 30, 20, and 40 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

-The same four people are in my firm for all 30 rounds of the experiment. □ True □ False

-The numbers in the payoff table are the same for all employees, they can change from round to round, and they have been obtained randomly. □ True □ False

Appendix B: Robustness Checks

B.1 Time Allotments

The Keystroke-Level model (KLM) was an appropriate model to use as it considers routine tasks and uses task execution time as the only measurement of performance. This model was initially introduced in 1980 by Card, Moran, and Newell, and it is still relevant in the field today. The execution times were found by adding the standard operators associated with each screen. KLM provides the execution time of 2.65 seconds for the decision task sequence and 1.3 seconds for the feedback screen.

Decision screen: 2.65 seconds

1.35s + 1.1s + 0.1s + 0.1s = 2.65s

1.) Find the area to select (M) 1.35s
2.) Point to the targeted area (P) 1.1s
3.) Press mouse button (B) 0.1s
4.) Release mouse button (B) 0.1s

Feedback screen: 1.3 seconds

1.1s + 0.1s + 0.1s = 1.3s

1.) Point to the button/area to select (P) 1.1s
2.) Press mouse button (B) 0.1s
3.) Release mouse button (B) 0.1s

B.2: Pressure

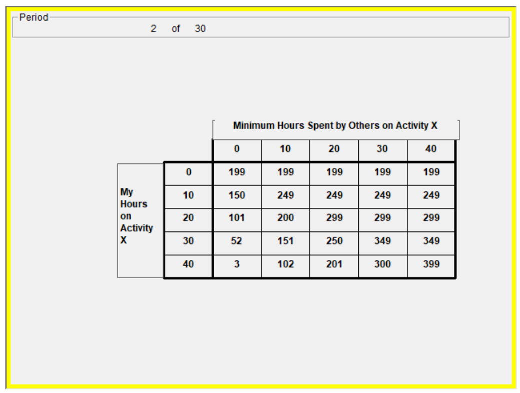
Table B.2 presents descriptive statistics of pressure reported by subjects in exogenous conditions on the questionnaire, including the minimum and maximum values chosen, means, and standard deviations.

Table B.2: Pressure – Descriptive Statistics

Variable name	Description	Treatment	Min Value	Max Value	Mean	S.D.
Pressure (subjects in exogenous conditions)	I felt under pressure when making my decisions. 1-5 Likert scale	TC20X20	1	4	1.89	1.05
		TC20X5	1	5	2.31	1.17
		TC5X20	1	5	3.06	1.05
		TC5X5	1	5	3.13	1.24

B.3 Additional Control Treatment- NoTCRC

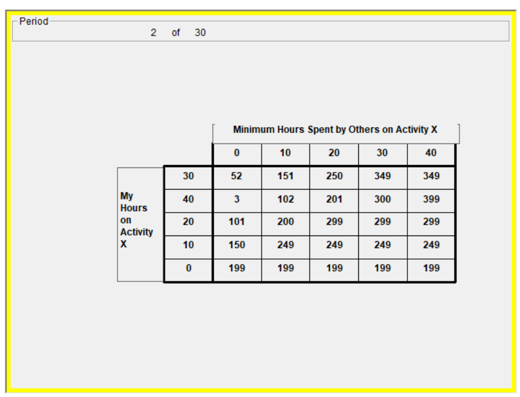
To examine the potential confound of cognitive load in decision tasks with changing rows, we ran an additional control treatment in which rows did not change, No Time Change Row Change (NoTCRC). Consider, for example, that subjects have already made their decision, but the time it takes to implement that decision, or look for the row corresponding to their decision, accounts for this time. Thus, the time to make their decision is minimized, which reduces the potential confound created by heterogeneity in cognitive ability. The figures below provide screenshots of the decision screen of both treatments.



Period 2 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	199	199	199	199	199
10	150	249	249	249	249
20	101	200	299	299	299
30	52	151	250	349	349
40	3	102	201	300	399

Figure B.3.1: Screenshot payoff matrix, NoTCRC



Period 2 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
30	52	151	250	349	349
40	3	102	201	300	399
20	101	200	299	299	299
10	150	249	249	249	249
0	199	199	199	199	199

Figure B.3.2: Screenshot payoff matrix, NoTC

Appendix C: Supplementary Analysis

C.1 Regressions

The regressions shown in Table C.1.1 provide formal statistical support to the conclusions in the text. Dummy treatment variables were created to assess treatment effects. NoTC serves as the baseline treatment for comparison. Since basic treatment tests rely on individual observations, clustering was done at the subject level for minimum effort and group level for effort and waste.

Table C.1.1: Random effects regressions on treatment effects

VARIABLES	(1) Minimum Effort	(2) Effort	(3) Waste
Unit of observation:	Firm	Employee	Employee
TC20×20	-1.08	-1.18	-.095
	(1.52)	(2.97)	(.770)
TC20×5	-1.92	-.756	1.16
	(1.53)	(3.41)	(1.02)
TC5×20	-4.80***	-3.38	1.42*
	(1.07)	(2.29)	(.842)
TC5×5	-5.25***	-1.26	3.99***
	(.878)	(2.29)	(1.09)
Constant	8.25***	13.28***	5.03***
	(.735)	(1.52)	(.548)
Observations	7,800	1,950	1,950
Number of Groups	260	65	65

Notes: Three (***), two (**), and one (*) star indicate statistical significance at the 1%, 5%, and 10%, respectively. Standard errors are denoted in parentheses.

The ordered probit regressions shown in Table C.1.2 provide formal statistical support to the conclusions in the text. Dummy treatment variables were created to assess treatment effects. NoTC serves as the baseline treatment for comparison. Since basic treatment tests rely on individual observations, clustering was done at the subject level for minimum effort, and group level for effort and waste.

Table C.1.2: Ordered probit regression on treatment effects

	Model 1	Model 2	Model 3
Dependent Variable	Minimum Effort	Effort	Waste
Unit of Observation	Firm	Employee	Employee
Base Treatment	NoTC		
TC20X20	-.190 (.194)	-.162 (.263)	-.026 (.113)
TC20X5	-.298 (.205)	-.133 (.301)	.131 (.129)
TC5X20	-.734*** (.205)	-.358 (.219)	.129 (.109)
TC5X5	-.869*** (.205)	-.181 (.202)	.414*** (.116)
Log-Likelihood	-9512.28	-13336.05	-9338.99
Number of Observations	9,240		
Number of Clusters	260	65	65

Notes: Standard errors are corrected for clustering at the level of observation. Three (***), two (**), and one (*) star indicate statistical significance at the 1%, 5%, and 10%, respectively. Standard errors are denoted in parentheses.

C.2 Response Time (RT) Analysis

Endogenous RT Analysis: In this subsection, we assess whether there is a relationship between decision time and effort levels. This analysis involves the two control treatments without time constraints. We observe a high degree of variance in the captured times, particularly in the decision stage. Variation of responses can indicate increased cognitive effort (C. F. Camerer and Hogarth 1999). In the first round, the average DT was 37 seconds. DT drops below 20 seconds by the seventh round and continues to decline as the rounds progress. By the end of the game, the average DT was 10 seconds. Tables C.2.1 and C.2.2 provide descriptive statistics for each treatment.

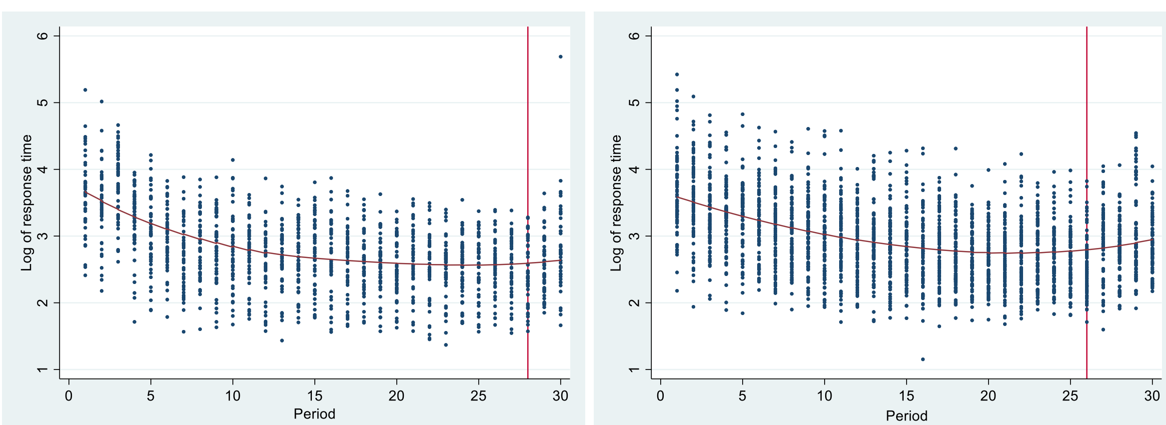
Table C.2.1: Descriptive statistics for NoTCRC

Variable	mean	median	s.d.	min	max	variance
RT	19.57972	15.6235	16.12579	3.391	295.699	260.041
DT	11.70949	8.525	11.08596	1.123	157.515	122.8985
FT	7.86953	5.663	10.11594	.562	284.732	102.3322

Table C.2.2: Descriptive statistics for NoTC

Variable	mean	median	s.d.	min	max	variance
RT	22.95853	18.3915	16.97514	3.167	226.471	288.1554
DT	15.62965	11.7075	14.48966	1.997	203.43	209.9502
FT	7.32888	5.6395	5.984248	1.05	74.74	35.81122

The tables above show the variance of response times for both endogenous response time treatments. With strong tails, logs were used for more accurate analyses (log_dt). The Lowess (locally weighted scatter-plot smoother) is a form of nonparametric regression, essentially providing the mean value of log_dt at each period. The xline shows the period in which the average response time was the lowest. In NoTCRC, RT was the lowest in round 28. In NoTC, RT was the lowest in period 26.



(a) (b)

Figure C.2.1: Logarithm of response times plotted against period (a) NoTCRC (b) NoTC

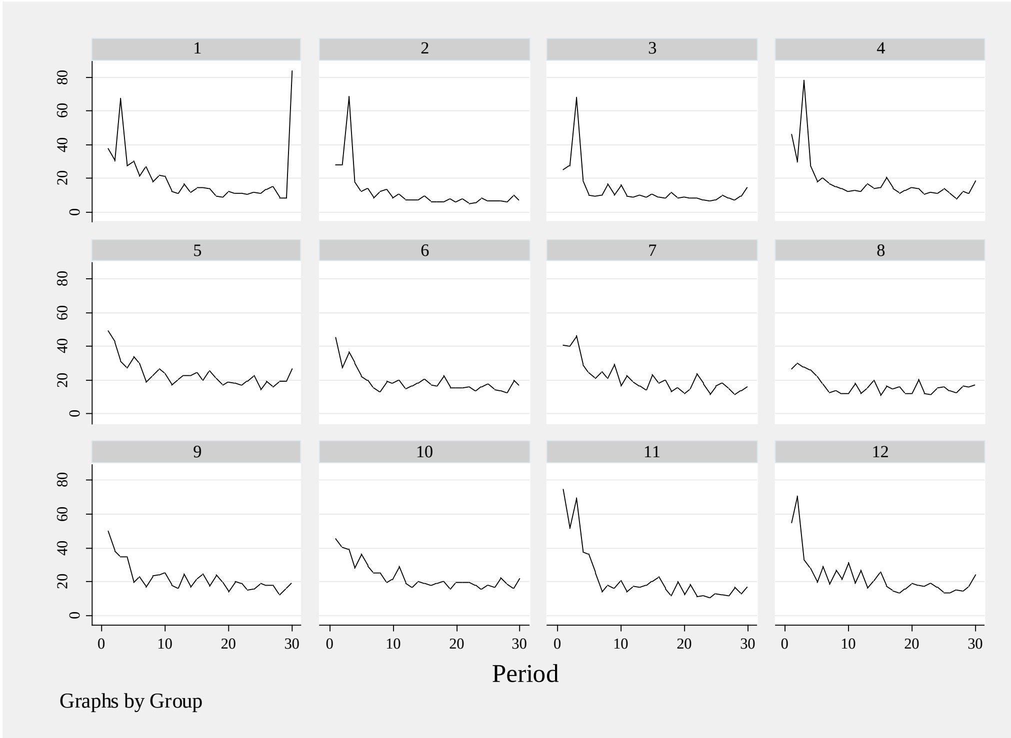


Figure C.2.2: Time series graph of response time (in seconds) by groups in NoTCRC

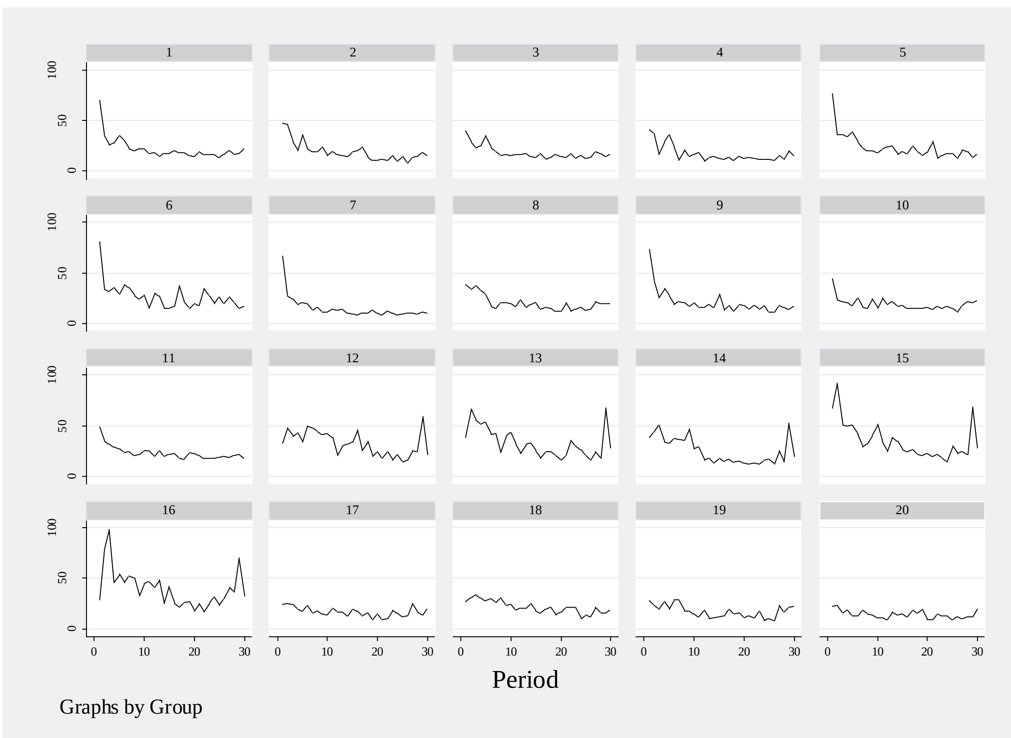


Figure C.2.3: Time series graph of response time (in seconds) by groups in NoTC

Appendix D: z-Tree Screenshots

D.1 NoTC Screenshots

Period
1 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
0	205	205	205	205	205
10	155	257	257	257	257
20	105	207	309	309	309
30	55	157	259	361	361
40	5	107	209	311	413

Period
2 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
30	52	151	250	349	349
40	3	102	201	300	399
20	101	200	299	299	299
10	150	249	249	249	249
0	199	199	199	199	199

Period
3 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
40	1	98	195	292	389
10	148	245	245	245	245
30	50	147	244	341	341
0	197	197	197	197	197
20	99	196	293	293	293

Period
4 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
0	203	203	203	203	203
30	53	158	263	368	368
20	103	208	313	313	313
40	3	108	213	318	423
10	153	258	258	258	258

Period
5 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
20	103	206	309	309	309
0	203	203	203	203	203
40	3	106	209	312	415
10	153	256	256	256	256
30	53	156	259	362	362

Period
6 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
30	54	151	248	345	345
10	154	251	251	251	251
40	4	101	198	295	392
0	204	204	204	204	204
20	104	201	298	298	298

Period
7 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
30	51	154	257	360	360
20	101	204	307	307	307
0	201	201	201	201	201
40	1	104	207	310	413
10	151	254	254	254	254

Period
8 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
10	153	256	256	256	256
30	53	156	259	362	362
40	3	106	209	312	415
20	103	206	309	309	309
0	203	203	203	203	203

Period

9 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
30	52	151	250	349	349
40	3	102	201	300	399
20	101	200	299	299	299
10	150	249	249	249	249
0	199	199	199	199	199

Period

10 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
40	1	96	191	286	381
0	201	201	201	201	201
20	101	196	291	291	291
10	151	246	246	246	246
30	51	146	241	336	336

Period

11 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
20	99	204	309	309	309
0	197	197	197	197	197
40	1	106	211	316	421
30	50	155	260	365	365
10	148	253	253	253	253

Period

12 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
20	104	202	300	300	300
10	154	252	252	252	252
30	54	152	250	348	348
40	4	102	200	298	396
0	204	204	204	204	204

Period

13 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
40	2	97	192	287	382
30	51	146	241	336	336
0	198	198	198	198	198
10	149	244	244	244	244
20	100	195	290	290	290

Period

14 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
20	104	202	300	300	300
10	154	252	252	252	252
40	4	102	200	298	396
0	204	204	204	204	204
30	54	152	250	348	348

Period

15 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
10	149	244	244	244	244
30	51	146	241	336	336
0	198	198	198	198	198
20	100	195	290	290	290
40	2	97	192	287	382

Period

16 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
30	54	158	262	366	366
0	204	204	204	204	204
10	154	258	258	258	258
20	104	208	312	312	312
40	4	108	212	316	420

Period
17 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
30	51	154	257	360	360
0	195	195	195	195	195
20	99	202	305	305	305
10	147	250	250	250	250
40	3	106	209	312	415

Period
18 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
0	202	202	202	202	202
20	102	198	294	294	294
40	2	98	194	290	386
30	52	148	244	340	340
10	152	248	248	248	248

Period
19 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
10	150	252	252	252	252
0	199	199	199	199	199
20	101	203	305	305	305
30	52	154	256	358	358
40	3	105	207	309	411

Period
20 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
40	4	105	206	307	408
30	54	155	256	357	357
10	154	255	255	255	255
20	104	205	306	306	306
0	204	204	204	204	204

Period
21 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
0	204	204	204	204	204
40	4	100	196	292	388
30	54	150	246	342	342
20	104	200	296	296	296
10	154	250	250	250	250

Period
22 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
0	200	200	200	200	200
20	100	204	308	308	308
30	50	154	258	362	362
10	150	254	254	254	254
40	0	104	208	312	416

Period
23 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
20	101	197	293	293	293
30	52	148	244	340	340
10	150	246	246	246	246
40	3	99	195	291	387
0	199	199	199	199	199

Period
24 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
10	147	252	252	252	252
0	196	196	196	196	196
30	49	154	259	364	364
20	98	203	308	308	308
40	0	105	210	315	420

Period

25 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	195	195	195	195	195
20	99	198	297	297	297
40	3	102	201	300	399
30	51	150	249	348	348
10	147	246	246	246	246

Period

26 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
10	149	248	248	248	248
40	2	101	200	299	398
20	100	199	298	298	298
0	198	198	198	198	198
30	51	150	249	348	348

Period

27 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
10	199	199	199	199	199
0	199	199	199	199	199
30	199	199	199	199	199
20	199	199	199	199	199
40	199	199	199	199	199

Period

28 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
30	51	154	257	360	360
20	99	202	305	305	305
10	147	250	250	250	250
0	195	195	195	195	195
40	3	106	209	312	415

Period

29 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
20	98	196	294	294	294
0	196	196	196	196	196
40	0	98	196	294	392
30	49	147	245	343	343
10	147	245	245	245	245

Period

30 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
40	1	103	205	307	409
10	148	250	250	250	250
20	99	201	303	303	303
30	50	152	254	356	356
0	197	197	197	197	197

D.2 NoTCRC Screenshots

Period

1 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
0	205	205	205	205	205
10	155	257	257	257	257
20	105	207	309	309	309
30	55	157	259	361	361
40	5	107	209	311	413

Period

2 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
0	199	199	199	199	199
10	150	249	249	249	249
20	101	200	299	299	299
30	52	151	250	349	349
40	3	102	201	300	399

Period

3 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
0	197	197	197	197	197
10	148	245	245	245	245
20	99	196	293	293	293
30	50	147	244	341	341
40	1	98	195	292	389

Period

4 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
0	203	203	203	203	203
10	153	258	258	258	258
20	103	208	313	313	313
30	53	158	263	368	368
40	3	108	213	318	423

Period

5 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
0	203	203	203	203	203
10	153	256	256	256	256
20	103	206	309	309	309
30	53	156	259	362	362
40	3	106	209	312	415

Period

6 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
0	204	204	204	204	204
10	154	251	251	251	251
20	104	201	298	298	298
30	54	151	248	345	345
40	4	101	198	295	392

Period

7 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
0	201	201	201	201	201
10	151	254	254	254	254
20	101	204	307	307	307
30	51	154	257	360	360
40	1	104	207	310	413

Period

8 of 30

	Minimum Hours Spent by Others on Activity X				
My Hours on Activity X	0	10	20	30	40
0	203	203	203	203	203
10	153	256	256	256	256
20	103	206	309	309	309
30	53	156	259	362	362
40	3	106	209	312	415

Period
9 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	0	199	199	199	199	199
	10	150	249	249	249	249
	20	101	200	299	299	299
	30	52	151	250	349	349
	40	3	102	201	300	399

Period
10 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	0	201	201	201	201	201
	10	151	246	246	246	246
	20	101	196	291	291	291
	30	51	146	241	336	336
	40	1	96	191	286	381

Period
11 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	0	197	197	197	197	197
	10	148	253	253	253	253
	20	99	204	309	309	309
	30	50	155	260	365	365
	40	1	106	211	316	421

Period
12 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	0	204	204	204	204	204
	10	154	252	252	252	252
	20	104	202	300	300	300
	30	54	152	250	348	348
	40	4	102	200	298	396

Period
13 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	0	198	198	198	198	198
	10	149	244	244	244	244
	20	100	195	290	290	290
	30	51	146	241	336	336
	40	2	97	192	287	382

Period
14 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	0	204	204	204	204	204
	10	154	252	252	252	252
	20	104	202	300	300	300
	30	54	152	250	348	348
	40	4	102	200	298	396

Period
15 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	0	198	198	198	198	198
	10	149	244	244	244	244
	20	100	195	290	290	290
	30	51	146	241	336	336
	40	2	97	192	287	382

Period
16 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	0	204	204	204	204	204
	10	154	258	258	258	258
	20	104	208	312	312	312
	30	54	158	262	366	366
	40	4	108	212	316	420

Period
17 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	195	195	195	195	195
10	147	250	250	250	250
20	99	202	305	305	305
30	51	154	257	360	360
40	3	106	209	312	415

Period
18 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	202	202	202	202	202
10	152	248	248	248	248
20	102	198	294	294	294
30	52	148	244	340	340
40	2	98	194	290	386

Period
19 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	199	199	199	199	199
10	150	252	252	252	252
20	101	203	305	305	305
30	52	154	256	358	358
40	3	105	207	309	411

Period
20 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	204	204	204	204	204
10	154	255	255	255	255
20	104	205	306	306	306
30	54	155	256	357	357
40	4	105	206	307	408

Period
21 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	204	204	204	204	204
10	154	250	250	250	250
20	104	200	296	296	296
30	54	150	246	342	342
40	4	100	196	292	388

Period
22 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	200	200	200	200	200
10	150	254	254	254	254
20	100	204	308	308	308
30	50	154	258	362	362
40	0	104	208	312	416

Period
23 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	199	199	199	199	199
10	150	246	246	246	246
20	101	197	293	293	293
30	52	148	244	340	340
40	3	99	195	291	387

Period
24 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	196	196	196	196	196
10	147	252	252	252	252
20	98	203	308	308	308
30	49	154	259	364	364
40	0	105	210	315	420

Period

25 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	195	195	195	195	195
10	147	246	246	246	246
20	99	198	297	297	297
30	51	150	249	348	348
40	3	102	201	300	399

Period

26 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	198	198	198	198	198
10	149	248	248	248	248
20	100	199	298	298	298
30	51	150	249	348	348
40	2	101	200	299	398

Period

27 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	199	199	199	199	199
10	150	252	252	252	252
20	101	203	305	305	305
30	52	154	256	358	358
40	3	105	207	309	411

Period

28 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	195	195	195	195	195
10	147	250	250	250	250
20	99	202	305	305	305
30	51	154	257	360	360
40	3	106	209	312	415

Period

29 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	196	196	196	196	196
10	147	245	245	245	245
20	98	196	294	294	294
30	49	147	245	343	343
40	0	98	196	294	392

Period

30 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	197	197	197	197	197
10	148	250	250	250	250
20	99	201	303	303	303
30	50	152	254	356	356
40	1	103	205	307	409

Chapter II: Introducing Weak Members to Improve Coordination in Weak-Link Coordination Games

1 Introduction

For organizations with weak-link technologies, output is a function not of a single worker's effort but the combined effort of all workers. Due to their interrelated structure, the actions of a single worker can lead to success or failure. In the workplace, there are many ways in which a worker may affect his or her co-workers. The dynamics of worker interaction are critical in determining individual productivity and firm performance (Chan, Li, and Pierce 2014). Even organizations with the most motivated employees cannot avoid coordination failure; employees may not even be aware of the effects of their choices. When employees are unaware of the effects of their choices, catastrophic failure can be the result. Such "coordination failure" can be like a trap for organizations; change is difficult to achieve once firms get stuck in sub-optimal equilibria (Rumelt 1995; Camerer and Knez 1996; Brandts and Cooper 2006a; Brandts et al. 2015). Change can only be successfully executed if everyone in the organization pulls in the same direction and works together toward the goal of change. This calls for organizational alignment. Organizations must structure the internal forces or conditions to serve as a mechanism to trigger behavioral change. Industrial economists and organizational psychologists have given much attention to firms with task heterogeneity and how to compose varying complexity tasks to maximize performance (Postrel 2009; Mathieu et al. 2008; Cason, Savikhin, and Sheremeta 2012; Cooper and Sutter 2018). Their insights have broad applications in several industries, yet they cannot explain systematic behavior in firm settings.

In this experiment, we consider coordination in organizations when success relies on many people with jobs that have vastly different requirements. This study was motivated by military organizations in which previous studies on financial incentives and worker characteristics cannot be directly applied. Each military branch has a unique role and specific mission requiring input from many different workers with different demands. Units require the synchronized efforts of members from numerous job-types ranging from finance to pararescue. Coordination is driven by the nature of the task rather than the worker's ability or motivation. The modern workforce faces similar complexities as jobs become more complex and specialized (Malone, Laubacher, and Johns 2011; Burke and Ng 2006; Parteka and Wolszczak-Derlacz 2017; Acemoglu and Autor 2011; Stromquist 2019). Firms face increased coordination requirements, competing in evolving industries characterized by rapid product turnaround, on-demand services, and new areas of expertise such as artificial intelligence (van Dam 2009). Work is characterized by increased volatility and hyper-connectivity (Faraj and Xiao 2006). Firms may even use this adaptability as a competitive advantage in evolving industries (Reeves and Deimler 2011; Gittell, Seidner, and Wimbush 2010; O'Neill and Salas 2018; Owen et al. 2001; Wei and Lau 2010).

In this study, we are interested in the dynamics of intra-firm task heterogeneity as a mechanism to achieve efficient coordination levels. More specifically, we aim to facilitate coordination among large groups by capturing social spillover effects associated with task heterogeneities. Spillover effects occur when an agent's actions or behaviors affect other agents' outcomes.[12] Social spillovers can be generated by the presence of other employees, the characteristics of the workgroup members, or the nature of the interaction with them (Martínez-Carrasco 2016). Social spillovers may activate based on the mere presence of someone else. Charness and Kuhn (2011) label this phenomenon as peer effects. In social psychology, this is referred to as social facilitation (Zajonc 1965; Markus 1978; Platania and Moran 2001). Since the

[12] The term behavioral spillover has been applied to a wide variety of phenomena but commonly used in economics to report laboratory experiments when outcomes in one game have an impact on subsequent behavior (see e.g., Bednar et al. 2012; Cason, Savikhin, and Sheremeta 2012; Savikhin and Sheremeta 2013; Falk, Fischbacher, and Gächter 2013; Angelovski et al. 2018). Our focus is not on behavioral spillovers across games, but spillovers in the workplace.

possibility of observing employees or receiving information about them gives rise to social spillovers, organizations may change their structure or firm composition to maximize productivity using these spillovers. That is, a firm can take advantage of the social preferences of its workers (Martínez-Carrasco 2016).

We study a weak-link game coordination game based on the "corporate turnaround game" by Brandts and Cooper (2006a). Their studies tackle the issue of coordination failure, predominantly when firms are stuck in a trap. Previous studies focus on group-based incentives (Brandts and Cooper 2006a), observability and leadership (Brandts and Cooper 2006b; Brandts et al. 2015; Brandts, Cooper, and Fatas 2007), and communication (Brandts and Cooper 2007; Brandts, Cooper, and Weber 2015). The corporate turnaround game is an experimental setting designed to represent a corporate environment where a group needs to escape from a sub-optimal performance trap. The game involves repeated play between four employees of a firm. The four employees simultaneously choose how much costly effort to expend with firm output and profits determined by the minimum effort in each round. Employees initially face a low bonus rate, trapping groups into the worst possible outcome. The bonus is then increased, transforming the game into a weak-link game (Van Huyck, Battalio, and Beil 1990) with multiple Pareto ranked equilibria.

In the past, we have studied situations when workers have the option to choose 0, 10, 20, 30, or 40 hours. Now we consider an organization with workers who do not have the opportunity to put forth little to no effort. Some professions do not allow employees to contribute less than the maximum effort. Employees must adopt an all-in mentality—for example, air traffic controllers make countless real-time decisions daily as they direct aircraft on the ground and in the air. Paratroopers must follow all procedures to safely deploy their parachute and land on a battlefield during air assault operations. Bioenvironmental engineers cannot cross an access control point to enter a contaminated zone without donning appropriately certified personal protective equipment (PPE). Other examples include military explosive ordinance disposal (EOD) teams, firefighters, and surgeons. Even jobs within the same profession can differ significantly depending on the specific role requirements. For instance, surgical nurses have different task demands than nurses in outpatient education services. The consequences of public health practitioners giving moderate or low effort can range from overlooked minor health code violations during a restaurant inspection to a large-scale infectious disease outbreak.

Experimental economics places little emphasis on firms' internal organization when workers perform various tasks within the firm. Yet organizations in most environments are comprised of employees in various roles, completing different tasks with varying levels of difficulty (Cooper and Sutter 2018). The experimental literature has mostly focused on employee productivity and worker ability, classifying workers as skilled or unskilled.[13] In economics, this has been predominantly studied at the macro-level (e.g., Goos, Manning and Salomons, 2014; Parteka and Wolszczak-Derlacz, 2017). A vast literature stresses the importance of distinguishing between skills and tasks (Acemoglu and Autor, 2011; Autor, 2013; Autor and Handel, 2013). Prendergast (2002) describes a task environment comprised of two distinct types of tasks. Routine workers account for most jobs, such as customer service roles or support roles. The second worker

[13] Worker heterogeneity is often studied in experimental economics. Workers can be classified by their ability or productivity relative to the firm. Experimenters assign subjects various roles within a firm to study the dynamics of these interactions. Brandts, Ortiz, and Solá Belda (2019) study distributional concerns when owner-managers compensate themselves and workers of different productivities and effort costs, as well as their relations to various equity principles. Güth et al. (2017) design an experimental setting in which workers have different productivity levels, and the employer decides how much to pay each worker. They find that employers pay higher wages to productive workers; however, this does not affect workers' performance throughout the game. Brandts et al. (2015) studied an experimental firm with different worker ability types to test whether help from high ability workers could pull a firm out of coordination failure. Charness and Kuhn (2007) examine high-productivity and low-productivity workers when a worker's effort could depend on colleagues' wages. Workers' effort choices were highly sensitive to their own wages, but indifferent to co-workers' wages.

task is complex, non-routine. Workers possess a unique skill set or provide specialist knowledge. Performance demands require coordinated individual performance in real-time, though worker tasks have widely varying temporal requirements and constraints. We model this task environment in our game design.[14]

In our baseline condition, four subjects are grouped as employees of a firm. Employees make a series of decisions on how much effort they will allocate, given five effort choices $E_i \in \{0, 10, 20, 30, 40\}$. These workers represent routine workers. Routine workers account for most jobs, such as customer service roles or support roles. In our experimental conditions, we introduce a second type of worker, restricted to two effort choices, $E_i \in \{30, 40\}$. These workers represent non-routine workers described above. Subjects are exogenously given a role within each firm with two types of employees, which we label type-1 and type-2. We implement two experimental treatments that vary only the number of type-2 employees in each firm. Our first experimental treatment adds one type-2 worker, increasing the firm size to five employees. Our other experimental firm comprises eight workers: four type-1 workers and four type-2 workers. *Explain why these numbers and keep a cohort of 4 members.

This illustrates a situation in which the fate of an agent's payoff is reliant upon all group members' matching contributions; some of the agents can indeed be worse off. All workers prefer coordination at the highest level; restricted workers need high coordination levels, or they end up in a worse fate. This has the potential to improve worker productivity and overall firm success if decision-makers are concerned with the welfare of others and believe that others will choose the

[14] Other examples of classifications include the "task approach" by Autor, Levy, and Murnane (2003) in which they use three broad task groups: routine cognitive and manual tasks, analytical and managerial tasks, and non-routine manual tasks; Baron and Kreps (1999) provide a framework to classify workers by the nature of the task they perform: soldiers, stars and guardians; Ellis and colleagues (2005) classify worker tasks on a measure of how critical the tasks are to organization success.

same, thus introducing the notion of social preferences (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002; Falk and Fischbacher, 2006; Battigalli and Dufwenberg 2007; 2009). Social preference theories have generated vast experimental literature, prominently tested in gift exchange games, dictator, trust, and ultimatum games.[15] These theories assume that players consider not only their payoffs but also the payoffs of others. The relationship between social preferences and coordination, however, has received limited attention in situations with Pareto-ranked equilibria (Gueye, Quérou, and Soubeyran 2020). Weak-link coordination games are well suited to study group behavior. These games do not have the incentive problem of the prisoner's dilemma since all symmetric outcomes are equilibria. Individuals cannot contribute additional effort to compensate for a weak link in the group, nor can they free ride.

We find that mixed-worker firms induce change to higher, more efficient equilibria. Despite increases in group size, firms with restricted workers report higher overall output. Several firms manage to coordinate at much higher levels, with two firms reaching the most efficient coordination level. Employees exert more effort when another employee(s) can be harmed in the firm. This is in line with evidence that transparency leads to more efficient outcomes (Mohnen, Pokorny, and Sliwka 2008), and peer effects generate positive spillovers in the workplace (Herbst and Mas 2015; Bonein 2018; Kane, Ransbotham, and Boynton 2012; Mas and Moretti 2009). Regarding worker behavior, we find evidence of peer effects in effort levels. In the second part of our analysis, we isolate type-1 workers' decisions to determine when social spillovers are highest. Employee average effort choices produced positive spillover effects in both experimental conditions. However, firms had difficulty raising minimum effort. Thus the introduction of

[15] Social preference models have been tested in various contexts in which players can impact the earnings of others. For a review of how games are used to measure social preferences, see Camerer and Fehr (2005); see Charness and Kuhn (2011) for a summary of contributions from laboratory experiments on the understanding of social preferences; see Cooper and Kagel (2016) for an additional survey of experimental results relating to other-regarding preferences.

restricted workers certainly reduces the extent of coordination failure but does not eliminate sub-optimal outcomes.

This chapter contributes to the coordination literature by providing a mechanism to alleviate coordination failure in large groups. This finding is remarkable, considering the prevalence of large organizations that continually face coordination demands. The last 30 years of laboratory studies have demonstrated over and over that large groups are doomed to fail. Not even traditional coordination mechanisms, such as leadership and communication, have been powerful enough to overcome coordination failure in large groups (Weber et al. 2001). This is discouraging to military organizations in which leadership and communication are paramount to success. Previous studies have stressed the vital role social membership and group identity play in military organizations (Akerlof and Kranton 2010; Goette, Huffman, and Meier 2006). There is evidence that coordination among individuals who share group identity is higher than those who do not (Charness, Rigotti, and Rustichini 2007; Chen and Chen 2011). This study demonstrates the powerful impact of peer effects in the absence of communication and leadership.

The remainder of this chapter is organized as follows—Section 2 reviews relevant literature. Section 3 presents the experimental design and treatments, while Section 4 develops our hypothesis. Section 5 provides the procedures, and Section 6 contains the results. Section 7 concludes.

2 Related Literature

The literature we review in this section concentrates on two aspects: group-size effects and organizational spillovers in weak-link coordination games. We highlight two revolutionary studies by Weber (2006) and Riedl, Rohde, and Strobel (2016) that exploit this game's social component as a mechanism to alleviate strategic uncertainty and improve coordination. Also relevant to this study is previous work examining the relationship between peer effects and workplace performance. We conclude with field studies that provide evidence of this phenomenon.

There exists a substantial amount of experimental research on coordination outcomes in small and large groups. Previous studies have demonstrated the vital role that group size plays on coordination outcomes (Van Huyck, Battalio, and Beil 1990; Chaudhuri, Schotter, and Sopher 2009; Knez and Camerer 1994; 2000; Weber et al. 2001)(Van Huyck, Battalio, and Beil 1990; 1991; Chaudhuri, Schotter, and Sopher 2009; Knez and Camerer 1994; 2000). The established precedent in lab studies is that large groups fail to coordinate due to strategic uncertainty, or the uncertainty surrounding players' beliefs about what the others will do. Uncertainty increases as people are added to the mix. In a pair, players consider themselves and their responses only to each other. Subjects in fixed pairs almost always coordinate on the most efficient level, but this drops drastically when group size reaches three (Van Huyck, Battalio, and Beil 1990; Knez and Camerer 1994; 2000). Groups of six are almost guaranteed to converge to the least efficient outcome. Coordination failure is a guarantee when group size reaches eight or more (Weber 2006; Knez and Camerer 1994; Cachon and Camerer 1996; Chaudhuri, Schotter, and Sopher 2009). Not even conventional strategies to overcome coordination failures such as leadership and communication have successfully solved large groups' coordination challenges (Weber et al. 2001; Chaudhuri, Schotter, and Sopher 2009).

Weber (2006) provided the first experimental evidence of efficient coordination in large "home-grown" groups. The experiment compares groups with 12 players to groups that start with two players and grow to this size, slowly adding new entrants. Home-grown groups began with two players, a setting proven to reach efficient coordination. These two players interacted over several rounds, allowing coordination success to become the social norm. Once this was achieved, the group size began to grow, adding new entrants to the group and allowing them to observe the group's history and established norms. As a result, groups of 12 regularly coordinated above the minimum, though efficient coordination was not reached. Riedl, Rohde, and Strobel (2016) provided a complementary mechanism to achieve efficient coordination in large groups, testing groups of 24, which is currently the largest group size tested in the laboratory. Using a "neighborhood" setup, players had the freedom to choose their interactions. Freedom of neighborhood choice overcame the coordination problem and led to efficient coordination. Together these studies provide valuable insights on overcoming coordination failure in large groups via social avenues.

Organizational spillovers are commonly studied in weak-link games. Martínez-Carrasco (2016) provides a classification of behavioral, organizational spillovers, splitting into technological and social. A technological spillover is when an agent responds to a cue in the workplace that does not rely on others' identity or characteristics in the firm. Social spillovers arise from an individual's social preferences or social norms established in the organization. The literature on weak-link games is predominantly in the area of technological spillovers, e.g., incentives, changes in payoff matrices that alter the attractiveness of the safe option and risky option, action spaces, the role of information, and the role of interaction structure (Brandts and Cooper 2006a; Hamman, Rick, and Weber 2007; Blume and Ortmann 2007). Social spillovers may activate based on the mere presence of someone else. Charness and Kuhn (2011) label this phenomenon as peer effects.

Studies assessing workforce performance have shown that introducing high performers into groups can increase others' production (Bandiera, Barankay, and Rasul 2013; Mas and Moretti 2009; Falk and Ichino 2006; Tan and Netessine 2019). Schultz, Schoenherr, and Nembhard (2010) show that workers on a production line adjust their speed to their co-workers' average rate because their workstations are interdependent. Even when there is no strategic independence between workers, their efforts may depend on their co-workers' efforts (Falk and Ichino 2006). Herbst and Mas (2015) review lab and field studies on the presence of peers, concluding that people work harder in the presence of their peers. However, it seems that the power of pure peer effects depends on the exact details of the work environment. Negative peer effects have been reported (Bellemare, Lepage, and Shearer 2010; Eriksson, Poulsen, and Villeval 2009), while other studies observe no correlation in output (Georganas, Tonin, and Vlassopoulos 2015; van Veldhuizen, Oosterbeek, and Sonnemans 2014).

In this experimental study, we test whether peer effects can lead large groups to efficient coordination in weak-link coordination games. We develop an experimental environment to test compositional effects on organizational performance using social spillovers to maximize productivity. This chapter contributes to the coordination literature by providing a mechanism to alleviate coordination failure among large groups, demonstrating that social spillovers overpower group-size effects.

3 Experimental Design

An experimental firm consists of a fixed grouping of *n* subjects (employees) who interact for 30 consecutive rounds. The firm's productivity in each round is determined by employees' effort choices for the round. Each round is considered a workweek. Intuitively employees spend 40 hours a week at work; therefore, we restrict employees' effort levels to ten-hour increments. Type 1 workers are routine workers and given five choices: $E_i \in \{0, 10, 20, 30, 40\}$. Type-2 workers are specialized, non-routine workers with limited effort choices: $E_i \in \{30, 40\}$. Participants begin with a fixed rate wage and a bonus rate, B, dependent upon the group contribution. Effort is costly, with

C_i representing the cost of a unit of effort for the *i*th employee. The per-period payoff to employee *i* is given by the following function:

$$\text{Employee } i\text{:} \quad \pi_e^i = 200 - C_i E_i + \left(B \times \min_{j \in \{1,2,3,4\}} (E_j) \right). \tag{2}$$

Table 5 corresponds to the case in which B = 10 and the cost of effort equals 5. This serves as the baseline payoff function of the main decision table (Table 5 is the standard payoff matrix for type-1 workers. Table 6 shows the payoff matrix for restricted type-2 workers). The table shows the payoffs to a worker who chooses a row action when the minimum action chosen by any worker in a firm, including the row worker, is shown in the column. Notice that if everyone picks the highest possible action, then the payoff-dominant equilibrium in the lower-right corner is selected, while if any of the players choose the lowest possible action, then the risk-dominant equilibrium in the upper-left corner is selected. Players incur a cost of 50 to increase their effort at each level. However, if matched, the net gain is also 50. These values were selected because past experiments have revealed that coordination failure is common, particularly when the amount of the bonus does not exceed the cost by a considerable amount. We know from other studies that lower costs of exerting effort are efficiency-enhancing (Goeree and Holt 2001; 2005; Brandts and Cooper 2006a; 2006b; Devetag and Ortmann 2007; Riedl, Rohde, and Strobel 2016). All payoffs during the experiment were denominated in an artificial currency, experimental currency units (ECU). Subjects have the potential to earn 12,000 ECUs at the end of the 30 rounds. ECUs were converted at a rate of one euro per 500 ECU.

The turnaround game is traditionally broken up into three ten-round blocks with the goal of inducing coordination failure in the first block to pull firms out of performance traps in the following block. Our experiment does not use a block design; subjects play 30 rounds. A key feature of our design is that we randomly distort the payoffs in each round. Each decision stage presents a new payoff table with changing values of the fixed-wage, cost of effort, and bonus. Using Equation 2 as a benchmark, we established a range of values to generate thirty new payoff tables, keeping the cost and potential net gain close to 50 in each round and the potential net monetary payoffs close to 12,000 ECUs (this is the maximum amount a subject could make over 30 rounds if Table 5 were used in all rounds). We restrict the feasible fixed-wage rates to the integers ∈ {195, 196, 197, 198, 199, 200, 201, 202, 203, 204, 205}. The values of the bonus were restricted to B ∈ {9.5, 9.6, 9.7, 9.8, 9.9, 10.1, 10.2, 10.3, 10.4, 10.5}. C_i was equal to 4.8, 4.9, or 5. Given the other parameter values, these are the effort costs that do not allow subjects to lose money in a round (specifically if the fixed-wage is less than 200, the cost of effort must be less than 5 to avoid negative payoffs). (See Appendix C for screenshots of each payoff table).

Table 5: Type-1 worker´s payoff table

		Minimum Effort by Other Workers				
		0	**10**	**20**	**30**	**40**
Effort by Worker *i*	**0**	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

Table 6: Type-2 worker´s payoff table

		Minimum Effort by Other Workers				
		0	**10**	**20**	**30**	**40**
Effort by Worker *i*	**30**	50	150	250	350	350
	40	0	100	200	300	400

3.1 Treatments

The primary treatment variable in our study is the number of restricted type-2 workers added to a cohort. Our baseline treatment is comprised of four type-1 workers. This cohort represents the traditional four-person firm studied in weak-link games. Maintaining a core group of four employees across conditions is essential to measure differences in behavior. Our first experimental treatment adds one type-2 worker, increasing the firm size to five employees. Four group members face the same action set as agents in the control treatments; one of the group members, however, is restricted. Our other experimental firm comprises eight workers: four type-1 workers and four type-2 workers. To demonstrate group-size effects, we ran a secondary treatment with eight type-1 subjects. This is a replicate of the baseline treatment except that groups are twice the size. The results from several studies mentioned earlier strongly suggest that small groups will converge to the efficient outcome, and large groups will converge to the inefficient outcome. Table 7 summarizes the features of each treatment and reports the number of groups and subjects that participated.[16]

Table 7: Features of treatments

Treatment name	T1	T2	T3	T4
Firm Size	4	5	8	8
Type-1 workers	4	4	4	8
Type-2 workers	0	1	4	0
Number of firms	9	18	12	3
Number of employees	36	90	96	24

[16] The number of groups was determined by a power analysis in conjunction with budget considerations and heterogeneity in data collection costs, following the optimal design standards given by List, Sadoff, and Wagner (2011). Fewer observations were collected in control treatments. Several studies have reported outcomes in standard 4-person and 8-person groups. Additionally, the authors report results from a preceding study of 32 control groups with the same parameters (Ellis and Sola 2020). We conducted a session with three groups of $n = 8$. Our interest is not merely on group-size effects and experience has shown in multiple repeated studies what happens in 8-person groups. Sampling costs are much higher for groups of eight subjects and sessions are very sensitive to logistic and recruiting issues such as a subject canceling at the last minute, unable to use groups of seven subjects or less.

Our experimental design addresses the following research questions:

- Can restricted workers improve (a) the performance of the other workers and (b) the overall firm minimum output?
- What is the optimal mix of workers to maximize social spillover effects?

4 Theoretical Predictions

In the baseline treatment, we expect low average contribution levels as demonstrated through previous studies in which coordination outcomes often converge to the least efficient equilibrium (Brandts, Cooper, and Fatas 2007; Cartwright, Gillet, and Van Vugt 2009; Brandts and Cooper 2006a; Hamman, Rick, and Weber 2007). We can expect minimum effort to be close to zero in the treatment with groups of eight subjects. Chaudhuri, Schotter, and Sopher (2009) replicated Van Huyck, Battalio, and Beil's (1990) experiment for $n = 8$. Minimum effort was not above 2 in any round (effort choices ranged from 1 to 7). Riedl, Rohde, and Strobel (2016) ran a baseline treatment with groups of eight players, reporting 2.93 average minimum effort (effort choices ranged from 1 to 7).

To make theoretical predictions regarding experimental firms, we must first make assumptions about how individuals perceive the game and anticipate others' actions. Recall that in each experimental firm, there are four workers with the same action set as workers in the baseline firm. Consider the strategic uncertainty underlying this cohort of four standard workers. Strategic uncertainty arises when subjects are unsure of what other players will choose. Subjects must decide whether they are willing to forgo a guaranteed payoff for the uncertain payoff from coordination at higher levels. In the baseline firm, players must consider their actions and the decisions of the other three players. Increasing one's effort at each level incurs a risk that someone else in the group will play an effort below this level. For example, consider an employee who would like to increase her effort from 0 to 10. If we assume that the other three employees are independent, this requires a 79 percent chance that each employee increases his effort.[17]

Type-1 workers in T2 and T3 face the same level of strategic certainty regarding the other three type-1 workers. Now, consider the additional strategic considerations when a type-2 worker is added to the group. Whereas before the uncertainty was present at each effort level, now the strategic risk lies only at the highest level, 40 hours. This is the only effort choice that type-2 workers can choose a lower effort. For the remaining values, no matter how many type-2 workers are added to the group, 30 hours remain the secure option for these workers. Type-1 workers face additional considerations regarding the consequences of their actions on type-2 workers, such as a potential decision conflict if one has to choose whether she is willing to sacrifice her earnings if the situation arises or the possible guilt one may feel for her actions.[18] A shared perception of consequences can create intense pressure for workers to do the right thing.

Given the vast body of empirical evidence on the relevance of social preferences when evaluating decisions and social spillover effects on performance, we conjecture that peer effects will arise in experimental treatments with type-2 workers. If peer effects exist, the behavior of experimental, mixed-worker firms will differ from the control treatment behavior. This provides the basis for Hypothesis 1.

Hypothesis 1: Average minimum effort will be higher in experimental treatments than in control treatments.

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[17] To derive this probability, solve for p such that 200 = 150*(1-p^3) + 250*p^3 (Brandts et al. 2015). If the probability that any one of N players will select a small number is equal to p, then the probability that a small number is selected by at least one player is $1-(1-p)^N$, which increases rapidly as N rises (Camerer and Knez 1996).

[18] We highlight a model that involves a trade-off between one's material payoff and one's degree of guilt from violating the expectations of another person (Battigalli and Dufwenberg 2007; 2009). This is called "disappointment aversion" in (Dufwenberg and Gneezy 2000) or "guilt aversion" (Charness and Dufwenberg 2006).

It is challenging to predict worker composition effects between group sizes of five and eight. We consider two potential behavioral responses that could cause a difference in output between the two conditions. The first is that distributional concerns may be proportional to the number of restricted workers. In T3, type-1 players' choices affect four times as many players as choices made by type-1 players in T2. The internal pressure, (Kandel and Lazear 1992) or a sense of increased responsibility, could lead to higher effort choices and increased firm output. Conversely, this could lead to higher levels of miscoordination (failure to coordinate at any equilibria) or wasted effort above the minimum. The second is that group members may feel a stronger sense of group identity when one restricted worker is outnumbered. They may feel a responsibility to protect this member. This view has been advanced in the social identity theory proposed by Tajfel and Turner (1979) (see Hogg and Terry 2000; Haslam 2004). When members of a group share a collective identity, coordination increases (Charness, Rigotti, and Rustichini 2007; Chen and Chen 2011).

5 Procedures

This experiment was conducted in the business department at the Autonomous University of Barcelona (UAB) using the software z-Tree (Fischbacher 2007). We recruited 246 undergraduate students (130 female; M_{age} = 22 years, SD = 3.82) from various faculties through ORSEE (Greiner 2015). We conducted 11 sessions from November 2018 to May 2019. Subjects could only participate once. Each session lasted approximately 1.5 hours, and the average payoff, including the five-euro show-up fee, was 19 euros.

At the beginning of each session, subjects were randomly seated and asked to read the consent form and sign in agreement. The experimenter collected the forms and signed as a witness. Subjects were given a set of written instructions and asked to follow along while the experimenter read the instructions aloud. At the end of the instruction set, subjects were given a pre-quiz to ensure they understood the payoffs and rules of the experiment. The full text for the instructions and pre-quiz is given in Appendix A.

The instructions used a corporate context. Subjects were referred to as employees and were told they are working for a firm. To avoid asking subjects to choose a level of effort, we asked them to allocate time between Activity X and Activity Y, with Activity X playing the role of effort. Subjects were informed of how many employees were in each firm, which remained fixed throughout the 30 rounds. Subjects were told that each round would present a new payoff table with changing values of the fixed-wage, cost of effort, and bonus. They were provided a range of values and given examples of different payoff tables. The instructions for mixed-worker treatments gave descriptions of both worker types and the number of type-1 and type-2 employees within each firm, but subjects did not receive their role assignment until the start of the game. We did not assign worker types in control treatments.

Once subjects answered all questions correctly, they were ready to proceed to the computer to begin the 30 rounds. In mixed-worker treatments, the first screen informed participants of their role in the firm, which remained fixed throughout the game. Once employees acknowledged their role assignment, the rounds began. In each round, employees simultaneously chose their effort levels. They were shown a payoff table similar to Tables 5 and 6 above. After each round, subjects were informed of the group's minimum effort, their earnings in that round, and their accumulated earnings.

At the end of the game, we asked subjects to fill out a questionnaire. During this time, the experimenter calculated the earnings and prepared envelopes with cash earnings. The methods carried out in this experiment were approved by the ethical committee at UAB.

6 Results

In this section, we investigate the impact of the experimental treatments on firm production levels and individual worker performance. We integrate elements of the management literature and organizational behavior literature on task heterogeneity throughout our analyses to propose relevant applications for organizations. Recall that an employee refers to an individual subject in the experiment, and a firm represents the fixed group of employees. The term effort is used for the value chosen in period *t* by employee *i*. Minimum effort refers to the minimum contribution of all employees in a firm in period *t*. In Section 6.1, we are interested in the dynamics between baseline treatments and mixed-worker treatments, testing our hypothesis. In Section 6.2, we narrow our focus to studyi employee behavior. We investigate the decisions of type-1 workers, interested in whether these workers change across treatments. Unless otherwise noted, treatment effects report results of Mann-Whitney tests using firm averages across all 30 rounds.

6.1 Treatment Effects

First, we report the results from T1 and T4. A direct comparison of choices and outcomes in T1 and T4 shows the effect of doubling the number of employees in a firm. This indicates the extent that strategic uncertainty affects employees' performance. Table 8 provides minimum effort frequencies in the final period as well as the average minimum effort in each treatment. T1 serves as our baseline measure of performance. This serves as a benchmark from which we can identify treatment effects and assess the magnitude of each treatment variable. In this treatment, employees exert an average of 17.75 hours; firm average minimum effort is 13.96. Over half of the firms finished at the least efficient level, while the others obtained mid-levels. These findings are in line with previous studies and accurately reflect the dynamics of a four-employee firm.

Table 8: Distribution of minimum effort levels in the final period

Treatment	Firm Size	Worker Composition		Distribution of (e_{min})					Average
		Type1	Type2	0	10	20	30	40	MinEffort
T1	n=4	4	0	5	1	3	0	0	13.96
T2	n=5	4	1	7	3	1	5	2	15.67
T3	n=8	4	4	4	2	4	2	0	15.39
T4	n=8	8	0	3	0	0	0	0	1.22

The magnitude of group-size effects is immediately apparent; firms sink to the inefficient equilibrium in which average minimum effort is close to zero (1.22) (p = .051, n = 12). Turning to the employee-level, average effort is much lower (5.44) in T4, though not significant (p = .145, n = 12). The distribution of first-period choices is similar for both treatments; average employee effort is 28.33 in T1 and 27.00 in T4 (p = .706, n = 12). This is in line with previous studies that show the distributions of first-period actions are almost the same in groups of 3, 14, and 16 (Camerer and Knez 1996; Van Huyck, Battalio, and Beil 1990; Weber 2006). Despite their similarity in the first period, T4 drops significantly throughout the remainder of the rounds. By the fifth period, average effort is below 10 hours producing a significant gap between the two treatments, which continues to grow until around period 15. Trend tests report significant decreasing effort levels (p < .001).[19] Subjects earn an average of 7,562 ECUs in T1 and 5,560 ECUs in T4 (p = .036, n = 12).

Turning now to mixed-worker firms, it appears that firm minimum output improves slightly, with certain firms reaching efficient coordination. Adding a restricted worker (T2) does not significantly increase firm production, measured by average minimum effort, relative to the baseline (p = .998, n = 27). Table 8 reveals that two firms were able to achieve efficient coordination in T2. Of the remaining groups, seven groups coordinated at the least efficient level

[19] Trends were calculated with the Stata command "nptrend" (Cuzick 1985; Altman 1991).

of zero, three obtained minima of 10, one a minimum of 20, and five minima of 30. Adding four restricted workers in T3 yields similar average firm production (15.39). Relative to the baseline, this is also not significant (p = .688, n = 21). Firms in T3 were not able to achieve efficient coordination, though 70 percent of the groups ended the game at levels above zero. Average minimum effort is higher, albeit not statistically significant, for both treatments as compared to the baseline. Thus, the introduction of restricted workers certainly reduces the extent of coordination failure but does not eliminate sub-optimal outcomes.

Result 1: Mixed worker firms do not significantly improve overall firm productivity, as measured by average minimum effort. Hypothesis 1 is not supported.

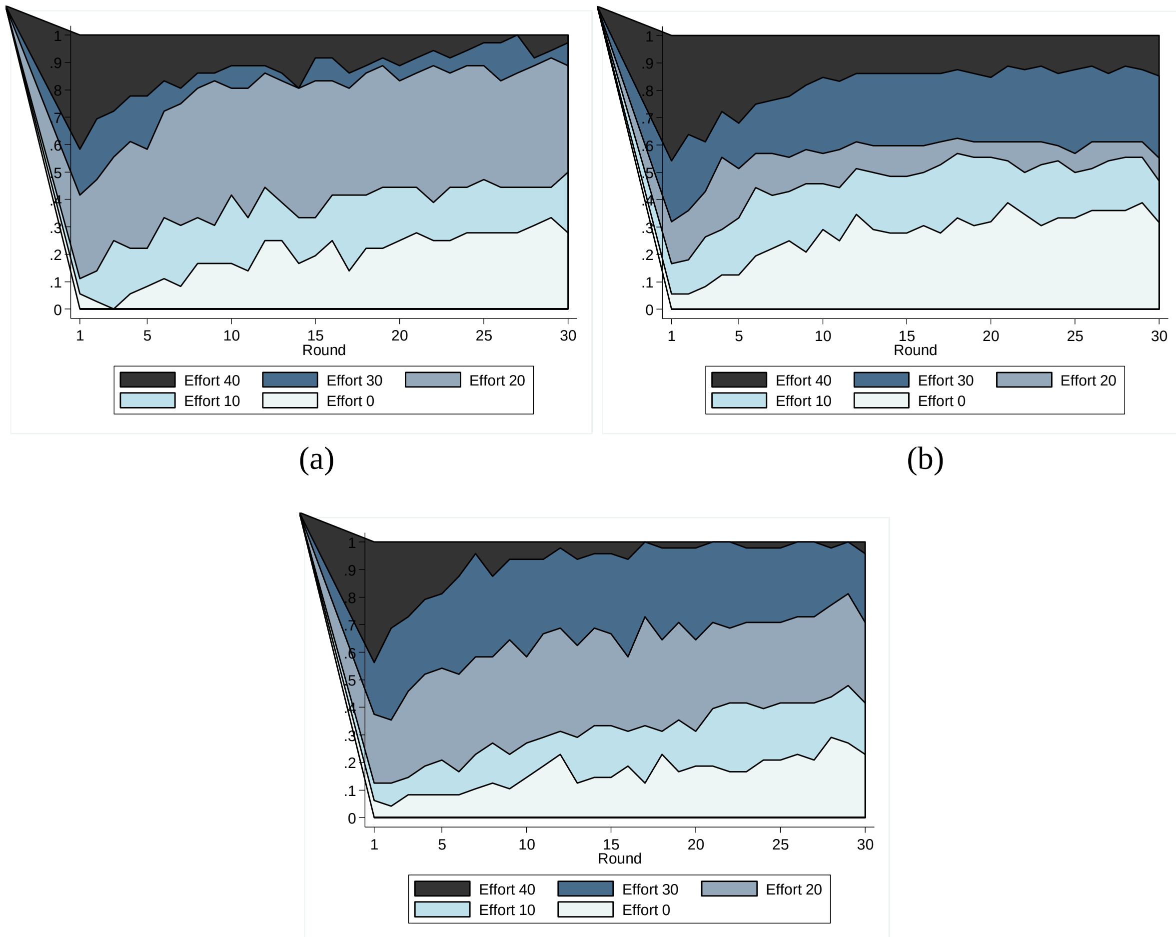
It is also interesting to compare the firm outcomes of the two treatments with groups of 8 players (T3 and T4) when only the group composition changes. The average minimum effort in T3 is significantly higher than T4 (p = .044; n = 15). Since four of the workers contribute at least 30 hours each period, we know that the average effort of each firm will be at least 15 hours. The average effort in T3 (25.15) is nearly five times more than T4 (5.44) (p = .009; n = 15) with type-1 workers contributing 19.69 hours on average.

6.2 Employee-level Behavior

In this subsection, we isolate the decisions of type-1 employees in T1, T2, and T3 and assess performance within this reference group. This analysis is essential because it allows us to assess employee behavior towards one restricted worker and four restricted workers and calculate the social effects when type-1 and type-2 worker ratios are 100 percent, 80:20, and 50:50. First, we look at the ratio of type-1 workers who match type-2 workers' minimum output of 30 hours. Peer effects are realized when individual choices are correlated with the choices of their peers (Cooper and Rege 2011). Duflo and Saez (2003) define spillover effects as the average difference in outcomes between units in treated groups and units in control groups. In the baseline treatment, 11 percent of workers average 30 hours or more; this jumps to 38 percent of type-1 workers in T2 and 23 percent of type-1 workers in T3.

We want to know when social effects are highest to determine the optimal mix of workers to maximize worker social preferences. Type-1 workers exert an average effort of 17.75 hours in T1, 18.85 hours in T2, and 19.69 hours in T3. Though type-1 workers in T3 report higher average effort levels, they earn an average of 225 ECUs less than type-1 workers in T2 (7,925 ECUs in T2 and 7,700 ECUs in T3) (p = .950, n = 30). This difference can be explained by the notion of waste, defined as the difference between the employee's effort level and minimum effort in a round. Waste is a good measure of convergence; low waste indicates strong convergence to equilibrium. If waste were zero, groups would fully coordinate at one of the equilibrium points. Type-1 workers in T3 waste slightly more effort (4.29) than type-1 workers in T1 (3.79) (p = .876, n = 21) and T2 (3.17) (p = .497, n = 30). This is interesting and can suggest that type-1 workers in T3 were more willing to take risks and select higher levels, sacrificing their earnings when their decisions impacted more people.

Figure 8 maps the distribution of effort levels chosen throughout the game. Comparing the choice patterns of type-1 employees in T1, T2, and T3, we observe different selection patterns. First, we look at the difference between the frequencies of choosing the risk-dominant and the Pareto-dominant outcomes. Choosing the risk-dominant option is lowest in T3 (16 percent), followed by T1 (19 percent) and T2 (27 percent). Pareto-dominant choices account for 18 percent of observations in T2, 13 percent of observations in T1, and 8 percent of observations in T3. A noticeable difference between the distributions is the proportion of subjects who invariably choose 20 hours in T1 (42 percent). In T2, this proportion shrinks (11 percent) and instead shifts to higher levels. In T2, we observe quite an even distribution at each effort level. In T3, moderate levels are chosen most often, with most subjects (61 percent) choosing 20 and 30 hours.



(a) (b) (c)

Figure 8: Type-1 worker cumulative distribution of efforts (a) T1; (b) T2; (c) T3

Looking at behavior in the first period can be informative of how subjects perceive the game and beliefs of how others will behave in different situations. Workers in T1 choose the Pareto-dominant choice 42 percent of the time. In the first period, average effort is 28.33, and average minimum effort is 16.67. In T2, the first-period average effort is 29.85, with nearly half of all choices at the highest level. However, minimum effort levels are substantially lower (15.00). More than half of type-1 workers in T3 select 40 hours in the first period; average effort is 28.75, and average minimum effort is 19.17 hours.

Now that we have a profile of type-1 worker characteristics, we turn our focus to type-2 workers. While the action set constraints do not allow as extensive behavioral analysis, it is important to understand the impact on restricted workers in mixed environments. Type-2 workers in T2 allocate an average of 31.75 hours each week, nearly 13 hours more than their type-1 counterparts (p = .040, n = 36). In T2, type-2 workers' average earnings are 5,900 ECUs compared to type-1 workers average earnings of 7,917 ECUs (a difference of 2,017 ECUs, approximately 4 euros) (p = .105, n = 36). Type-2 workers in T3 allot an average of 30.6 hours each week, 11 hours more than their type-1 co-workers. Type-2 workers' average earnings in T3 are 6,086 ECUs compared to type-1 workers' average earnings of 7,701 ECUs (a difference of 1,615 ECUs, approximately 3 euros) (p = .197, n = 24). In both conditions, type-2 workers work more hours per week than type-1 workers while earning less. This tells us to expect high levels of wasted effort by type-2 workers. Wasted effort by type-2 employees in T2 is 16.06, which is more than five times the amount of wasted effort by type-1 workers in T2 (p = .031, n = 36). Wasted effort by type-2 workers in T3 (15.21) is significantly higher than type-1 workers in T3 (p = .005, n = 24).

7 Conclusion

In this article, we study the effect of heterogeneous groups on the efficiency of coordination in repeated weak-link coordination games. We aim to facilitate efficient coordination among large groups by capturing social spillover effects associated with task heterogeneities. We develop an experimental environment to assess compositional effects on organizational performance using peer effects to maximize productivity. We test the hypothesis that social spillover effects produce firms with higher minima than firms in control treatments. Theoretically, increasing the group size increases the strategic uncertainty of the game and worsens the coordination problem. We examine the impact of social spillovers experimentally and find that it boosts efficiency in groups with five as well as eight subjects.

Despite increases in group size, firms with restricted workers report higher overall output. Several firms managed to coordinate at much higher levels, with two firms reaching the most efficient coordination level. Employees put forth more effort when another employee(s) can be harmed in the firm. Type-2 workers increased worker productivity by 21 percent in T2 and 42 percent in T3. These findings further support the idea that workers' productivity increases when they work alongside productive co-workers (Falk and Ichino 2006; Bandiera, Barankay, and Rasul 2011; Mas and Moretti 2009).

Regarding worker behavior, we find evidence of peer effects in effort levels resulting in positive social spillovers. While the average effort choices produce positive spillover effects in T2 and T3, some differences are noticeable based on the firm's composition. The impact of social preferences is not necessarily homogeneous across workers. Social spillover effects are highest in T3 if considering the average effort of type-1 employees. However, minimum effort is more difficult to increase. These results corroborate the findings of previous work that has examined positive spillovers in the workplace (Herbst and Mas 2015; Bonein 2018; Kane, Ransbotham, and Boynton 2012; Mas and Moretti 2009).

It is important to bear in mind the consequences arising from the extensive heterogeneity in individual production levels among the two different worker types. Type-2 workers in T2 contribute nearly 13 more hours each week and earn 2,000 fewer ECUs than their type-1 counterparts. The situation in T3 is slightly less bleak: type-2 workers exert 11 more hours of effort each week with earning differentials of 1,614 ECUs. On the basis of merit pay, type-2 workers should receive at least as much as less-productive co-workers (Gross, Guo, and Charness 2015).

Aguinis and O'Boyle (2014) argue that modern organizations have caused an emergence of star performers—select employees who contribute a disproportionate amount of output. Management practices that emphasize workers' homogeneity are unlikely to motivate star workers and encourage star turnover (Aguinis and O'Boyle 2014; Huselid 1995; Shaw and Gupta 2007).

This work contributes to the coordination literature by providing a mechanism to alleviate coordination failure among large groups, demonstrating that social spillovers overpower group-size effects. Our findings complement previous studies that have achieved successful coordination in large groups. Weber (2006) and Riedl, Rohde, and Strobel (2016) exploit the social component of the weak-link game as a mechanism to achieve efficient coordination in large groups. In our work, task heterogeneities induce change to higher, more efficient equilibria. Thus, we provide a new account of how to facilitate efficient coordination among large groups by capturing social spillover effects associated with task heterogeneities. This is consistent with larger companies facing the need to increase incentives and supervision as they grow in size. This chapter could also be interpreted not as heterogeneities because of differences in productivity, but how organizations could benefit by exerting limited supervision. That is, focusing supervision on a limited subset of employees guarantees a high level of effort for these restricted employees and expects others to follow.

These results apply to the management literature on modern-day organizational arrangements. Group composition has a substantial impact on productivity (Hamilton, Nickerson, and Owan 2003). The issue for organizations is not whether they have homogenous or heterogeneous workers, but instead how to align workers. The optimal mix will transpire when the social preferences of workers are maximized. From our results, it appears that firms should favor a setting in which the behavior of the most productive workers can be observed. Transparency leads to more efficient outcomes (Mohnen, Pokorny, and Sliwka 2008). Tan and Netessine (2019) recommend that organizations place high-ability workers in visible positions to maximize their positive spillovers and pay close attention to compensation schemes that reward these spillovers. Tasks most relevant to firm-level outcomes are those that yield the greatest star effects (Aguinis and O'Boyle 2014; Huselid, Beatty, and Becker 2005; Oldroyd and Morris 2012). Aguinis and O'Boyle (2014) assert that resource allocation that motivates star workers and maximizes their positive influence on others is crucial. For example, organizations should rotate stars in and out of work teams to introduce high-performance norms to a higher number of workers (Skilton and Dooley 2010). Moreover, because visibility intensifies spillover effects, managers may consider placing high performers where they can be observed by the maximum number of employees.

While this study provides an opportunity to advance the understanding of organizational social spillovers in coordination games, we acknowledge the limitations of this study—first, we change both the size of the group and the workers' action sets. A direct comparison would have included a control treatment with five homogeneous players since we also report a control treatment with groups of eight homogeneous players. Additionally, we could have studied treatments altering the ratio of workers in each firm. Second, there are potentially relevant variables that were not controlled that could have affected individual behavior in these interactions. We did not elicit social preferences (for example, Bartling et al. 2009). There are several methods in social psychology to measure one's Social Value Orientation (SVO), which could have contributed an explanation behavior in this setting (for overviews, see Kwong and Au 2004; Murphy and Ackermann 2012; 2014). We believe that our treatment effects have broad applications. Future replication studies should explore whether and to what extent our design choices influenced our empirical results.

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Appendix A: Experimental Instructions

A.1 Instructions for T1

[**Remark:** In the following, we present the instructions for treatment T1. Paragraphs with **[*]** were given in both control treatments but appropriately reformulated for T4. A complete set of instructions is available from the authors.]

INSTRUCTIONS

The purpose of this experiment is to study how individuals make decisions in certain contexts. In addition to the five-euro participation fee, you will be paid any additional money you accumulate at the end of today´s session. All payoffs during the experiment are denominated in an artificial currency, experimental currency units (ECU). At the end of the experiment, ECUs will be converted at a rate of one euro per 500 ECUs. Upon completion of the experiment, your earnings will be converted to euros, and you will be paid privately in cash. The exact amount you receive will be determined during the experiment and will depend on your decisions and the decisions of others. If you have any questions during the experiment, please raise your hand and wait for an experimenter to come to you. Please do not talk, exclaim, or try to communicate with other participants during the experiment. Participants intentionally violating the rules may be asked to leave the experiment and may not be paid.

[*] Decision rounds: The experiment consists of 30 rounds. At the end of the game, we will ask you to answer some questions. In each round, you will be in a group with three other participants. The composition of these groups, which are called *firms,* will not vary during the experiment. Given that nobody will know the identity of the members of each group, all the actions you take during the experiment will be anonymous.

Task: You and the other members of your group are employees of a firm. You can think of a round of the experiment as being a workweek. Each of the four employees spends 40 hours per week at their firm. Your task will be to decide how to allocate your time between two activities: Activity X and Activity Y. Specifically, you will be asked to choose how much time to devote to Activity X. The available choices are 0 hours, 10 hours, 20 hours, 30 hours, or 40 hours. The remaining hours will be put towards Activity Y. For example, if you devote 30 hours to Activity X, this means that 10 hours will be put towards Activity Y.

Employee payoffs: An employee's payoffs will be determined by the number of hours that employee spends on Activity X, the minimum number of hours that employees in his or her firm spend on Activity X, the base salary, a bonus that depends on the minimum number of hours spent on Activity X by any of the members of your group, and the cost of effort. These payoffs are summarized by the formula below:

Employee Earnings = Base Salary − (Cost*Your hours on Activity X) + (Bonus*Minimum hours spent by other employees on Activity X).

The values of the Base Salary, Bonus, and Cost will not be the same in each round. Specifically, they will change randomly and with equal probability. The Base Salary will take values between 195 and 205: 195, 196, 197, ..., 205. The Bonus will take values between 9.5 and 10.5: 9.5, 9.6, 9.7, ..., 10.5; the cost will take the values of 4.8, 4.9, or 5. In addition, on your screen, you will always have information about the payoffs for each possible decision in each round. If you don't understand the above formula, don't worry. It is provided to you as an additional way to understand your payoffs. The computer always shows your payoff table at any point where you need to make a decision. The tables include all of the information you need to make a decision.

Playing a Round as a Firm Employee: For each round of the experiment, the computer will display a table like the one shown below. Your earnings in each round can be found by looking at the hours you can choose to dedicate to Activity X (on the left side of the table) and the columns that indicate the minimum number of hours spent in Activity X by the other members of the group. This table will be the same for each member of the group. For the payoff table shown below, the Base Salary is equal to 200, the Bonus is equal to 10, and the Cost is equal to 5. Remember that when the game begins, the numbers that appear in the table may be different as explained above.

Example:

Employee Earnings = 200 – (5*Your hours on Activity X) + (10*Minimum hours spent by other employees on Activity X)

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours on Activity X	**0**	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

To choose the number of hours to spend on Activity X, use the buttons at the bottom of the screen. When you have made your final decision, click on the button labeled *Continue*. You may change your decision as often as you would like, but once you click *Continue,* your choice is final. Note that when you make your decision, you will not know what the other members of your group are selecting, nor will they know what you select.

Feedback as an employee: At the end of each round, you will receive a summary of what happened in that round including the number of hours that you spent on Activity X, the minimum number of hours spent by the other employees on Activity X, your earnings in that round, and your accumulated earnings through the current round.

Confidentiality and Payment: At the end of the experiment, you will be paid, in cash, the sum of the payoffs that you have earned in the rounds of the experiment along with the five-euro show-up fee. As noted previously, you will be paid privately, and we will not disclose any information about your actions or your payoff to the other participants in the experiment.

EXAMPLES AND QUESTIONS

For the following questions, use the information in the example table below. Please raise your hand if you are having trouble answering one of the questions.

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours Spent on Activity X	**0**	197	197	197	197	197
	10	148	245	245	245	245
	20	99	196	293	293	293
	30	50	147	244	341	341
	40	1	98	195	292	389

[*]-Suppose that you choose to dedicate 10 hours to Activity X. The other employees in your firm choose to dedicate 30, 20, and 10 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

[*]-Now suppose that you choose to dedicate 30 hours to Activity X. The other employees in your firm choose to dedicate 30, 20, and 40 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

[*]- The same four people are in my firm for all 30 rounds of the experiment. □ True □ False

- The numbers in the payoff table are the same for all employees, they can change from round to round, and they have been obtained randomly. □ True □ False

A.2 Instructions for T2

[**Remark:** In the following, we present the instructions for treatment T2. Paragraphs with **[*]** were given in both experimental treatments but appropriately reformulated for T3. A complete set of instructions is available from the authors.]

INSTRUCTIONS

The purpose of this experiment is to study how individuals make decisions in certain contexts. In addition to the five-euro participation fee, you will be paid any additional money you accumulate at the end of today´s session. All payoffs during the experiment are denominated in an artificial currency, experimental currency units (ECU). At the end of the experiment, ECUs will be converted at a rate of one euro per 500 ECUs. Upon completion of the experiment, your earnings will be converted to euros, and you will be paid privately in cash. The exact amount you receive will be determined during the experiment and will depend on your decisions and the decisions of others. If you have any questions during the experiment, please raise your hand and wait for an experimenter to come to you. Please do not talk, exclaim, or try to communicate with other participants during the experiment. Participants intentionally violating the rules may be asked to leave the experiment and may not be paid.

[*] Decision rounds: In each round, you will be in a group with four other participants. The composition of these groups, which are called *firms,* will not vary during the experiment. At the beginning of the experiment, each employee will be randomly assigned one of two roles within a firm: type-1 worker or type-2 worker. In each firm, there will be four type-1 workers and one type-2 worker. Given that nobody will know the identity of the members of each group, all the actions you take during the experiment will be anonymous.

Task: You and the other members of your group are employees of a firm. You can think of a round of the experiment as being a workweek. Each of the four employees spends 40 hours per week at their firm. Your task will be to decide how to allocate your time between two activities: Activity X and Activity Y. Specifically, you will be asked to choose how much time to devote to Activity X. The available choices are 0 hours, 10 hours, 20 hours, 30 hours, or 40 hours. The remaining hours will be put towards Activity Y. For example, if you devote 30 hours to Activity X, this means that 10 hours will be put towards Activity Y.

Employee payoffs: An employee's payoffs will be determined by the number of hours that employee spends on Activity X, the minimum number of hours employees in his or her firm spend on Activity X, the base salary, a bonus that depends on the minimum number of hours spent on Activity X by any of the members of your group, and the cost of effort. These payoffs are summarized by the formula below:

Employee Earnings = Base Salary − (Cost*Your hours on Activity X) + (Bonus*Minimum hours spent by other employees on Activity X).

The values of the Base Salary, Bonus, and Cost will not be the same in each round. Specifically, they will change randomly and with equal probability. The Base Salary will take values between 195 and 205: 195, 196, 197, ..., 205. The Bonus will take values between 9.5 and 10.5: 9.5, 9.6, 9.7, ..., 10.5; the cost will take the values of 4.8, 4.9, or 5. In addition, on your screen, you will always have information about the payoffs for each possible decision in each round. If you don't understand the above formula, don't worry. It is provided to you as an additional way to understand your payoffs. The computer always shows your payoff table at any point where you need to make a decision. The tables include all of the information you need to make a decision.

Playing a Round as a Firm Employee: For each round of the experiment, the computer will display a table like the one shown below. Your earnings in each round can be found by looking at the hours you can choose to dedicate to Activity X (on the left of the table) and the columns that indicate the minimum number of hours spent in Activity X by the other members of the group. This table will be the same for each member of the group. For the payoff table shown below, the Base Salary is equal to 200, the Bonus is equal to 10, and the Cost is equal to 5. Remember that when the game begins, the numbers that appear in the table may be different as explained above.

Example:

Employee Earnings = 200 − (5∗Your hours on Activity X) + (10*Minimum hours spent by other employees on Activity X)

Type-1 worker

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours on Activity X	**0**	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

Type-2 worker

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours on Activity X	**30**	50	150	250	350	350
	40	0	100	200	300	400

To choose the number of hours to spend on Activity X, use the buttons at the bottom of the screen. When you have made your final decision, click on the button labeled *Continue*. You may change your decision as often as you would like, but once you click *Continue,* your choice is final. Note that when you make your decision, you will not know what the other members of your group are selecting, nor will they know what you select.

Feedback as an employee: At the end of each round, you will receive a summary of what happened in that round including the number of hours that you spent on Activity X, the minimum number of hours spent by the other employees on Activity X, your earnings in that round, and your accumulated earnings through the current round.

Confidentiality and Payment: At the end of the experiment, you will be paid, in cash, the sum of the payoffs that you have earned in the rounds of the experiment along with the five-euro show-up fee. As noted previously, you will be paid privately, and we will not disclose any information about your actions or your payoff to the other participants in the experiment.

EXAMPLES AND QUESTIONS

For the following questions, use the information in the example tables provided below. Please raise your hand if you are having trouble answering one of the questions.

Imagine that you are a type-1 worker:

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours on Activity X	**0**	197	197	197	197	197
	10	148	245	245	245	245
	20	99	196	293	293	293
	30	50	147	244	341	341
	40	1	98	195	292	389

[*] -Suppose that you choose to dedicate 10 hours to Activity X. The other employees in your firm have chosen to dedicate 30, 20, 10, and 30 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

[*] -Now suppose that you choose to dedicate 30 hours to Activity X. The other employees in your firm choose to dedicate 30, 20, 20, and 40 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

Imagine that you are a type-2 worker:

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours on Activity X	**30**	50	147	244	341	341
	40	1	98	195	292	389

[*] -Suppose you choose to dedicate 30 hours to Activity X. The other employees in your firm have chosen to dedicate 30, 40, 0, and 20 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

[*] -Now suppose that you choose to dedicate 40 hours to Activity X. The other employees in your firm choose to dedicate 40, 30, 20, and 40 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

[*] -In each firm, there will be four type-1 workers and one type-2 worker. □ True □ False

[*] -The same five people are in my firm for all 30 rounds of the experiment. □ True □ False

-Worker types are randomly assigned by the computer. □ True □ False

Appendix B: Supplementary Analysis and Figures

Dummy treatment variables were created to assess treatment effects. T1 serves as the baseline treatment for comparison. Since basic treatment tests rely on individual observations, clustering was done at the subject level for minimum effort, and group level for effort and waste. Standard errors are corrected for clustering at the level of observation. The regressions shown below provide formal statistical support to the conclusions in the text.

Table B1: Random effects regressions on treatment effects

VARIABLES	(1) Minimum Effort	(2) Effort
Unit of observation:	Firm	Employee
T2	1.70	3.65
	(2.19)	(3.71)
T3	1.42	7.39*
	(2.17)	(4.01)
T4	-12.74***	-12.31**
	(2.93)	(6.06)
Constant	13.96***	17.76***
	(1.85)	(3.03)
Observations	7,380	1,260
Number of Groups	246	42

Notes: Three (***), two (**), and one (*) star indicate statistical significance at the 1%, 5%, and 10%, respectively. Standard errors are denoted in parentheses.

Appendix C: z-Tree Screenshots

Period
1 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
0	205	205	205	205	205
10	155	257	257	257	257
20	105	207	309	309	309
30	55	157	259	361	361
40	5	107	209	311	413

Period
2 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
30	52	151	250	349	349
40	3	102	201	300	399
20	101	200	299	299	299
10	150	249	249	249	249
0	199	199	199	199	199

Period
3 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
40	1	98	195	292	389
10	148	245	245	245	245
30	50	147	244	341	341
0	197	197	197	197	197
20	99	196	293	293	293

Period
4 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
0	203	203	203	203	203
30	53	158	263	368	368
20	103	208	313	313	313
40	3	108	213	318	423
10	153	258	258	258	258

Period
5 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
20	103	206	309	309	309
0	203	203	203	203	203
40	3	106	209	312	415
10	153	256	256	256	256
30	53	156	259	362	362

Period
6 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
30	54	151	248	345	345
10	154	251	251	251	251
40	4	101	198	295	392
0	204	204	204	204	204
20	104	201	298	298	298

Period
7 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
30	51	154	257	360	360
20	101	204	307	307	307
0	201	201	201	201	201
40	1	104	207	310	413
10	151	254	254	254	254

Period
8 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X				
	0	10	20	30	40
10	153	256	256	256	256
30	53	156	259	362	362
40	3	106	209	312	415
20	103	206	309	309	309
0	203	203	203	203	203

Period

9 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	30	52	151	250	349	349
	40	3	102	201	300	399
	20	101	200	299	299	299
	10	150	249	249	249	249
	0	199	199	199	199	199

Period

10 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	40	1	96	191	286	381
	0	201	201	201	201	201
	20	101	196	291	291	291
	10	151	246	246	246	246
	30	51	146	241	336	336

Period

11 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	20	99	204	309	309	309
	0	197	197	197	197	197
	40	1	106	211	316	421
	30	50	155	260	365	365
	10	148	253	253	253	253

Period

12 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	20	104	202	300	300	300
	10	154	252	252	252	252
	30	54	152	250	348	348
	40	4	102	200	298	396
	0	204	204	204	204	204

Period

13 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	40	2	97	192	287	382
	30	51	146	241	336	336
	0	198	198	198	198	198
	10	149	244	244	244	244
	20	100	195	290	290	290

Period

14 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	20	104	202	300	300	300
	10	154	252	252	252	252
	40	4	102	200	298	396
	0	204	204	204	204	204
	30	54	152	250	348	348

Period

15 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	10	149	244	244	244	244
	30	51	146	241	336	336
	0	198	198	198	198	198
	20	100	195	290	290	290
	40	2	97	192	287	382

Period

16 of 30

		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
My Hours on Activity X	30	54	158	262	366	366
	0	204	204	204	204	204
	10	154	258	258	258	258
	20	104	208	312	312	312
	40	4	108	212	316	420

Period 17 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
30	51	154	257	360	360
0	195	195	195	195	195
20	99	202	305	305	305
10	147	250	250	250	250
40	3	106	209	312	415

Period 18 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	202	202	202	202	202
20	102	198	294	294	294
40	2	98	194	290	386
30	52	148	244	340	340
10	152	248	248	248	248

Period 19 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
10	150	252	252	252	252
0	199	199	199	199	199
20	101	203	305	305	305
30	52	154	256	358	358
40	3	105	207	309	411

Period 20 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
40	4	105	206	307	408
30	54	155	256	357	357
10	154	255	255	255	255
20	104	205	306	306	306
0	204	204	204	204	204

Period 21 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	204	204	204	204	204
40	4	100	196	292	388
30	54	150	246	342	342
20	104	200	296	296	296
10	154	250	250	250	250

Period 22 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	200	200	200	200	200
20	100	204	308	308	308
30	50	154	258	362	362
10	150	254	254	254	254
40	0	104	208	312	416

Period 23 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
20	101	197	293	293	293
30	52	148	244	340	340
10	150	246	246	246	246
40	3	99	195	291	387
0	199	199	199	199	199

Period 24 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
10	147	252	252	252	252
0	196	196	196	196	196
30	49	154	259	364	364
20	98	203	308	308	308
40	0	105	210	315	420

Period
25 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	195	195	195	195	195
20	99	198	297	297	297
40	3	102	201	300	399
30	51	150	249	348	348
10	147	246	246	246	246

Period
26 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
10	149	248	248	248	248
40	2	101	200	299	398
20	100	199	298	298	298
0	198	198	198	198	198
30	51	150	249	348	348

Period
27 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
10	199	199	199	199	199
0	199	199	199	199	199
30	199	199	199	199	199
20	199	199	199	199	199
40	199	199	199	199	199

Period
28 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
30	51	154	257	360	360
20	99	202	305	305	305
10	147	250	250	250	250
0	195	195	195	195	195
40	3	106	209	312	415

Period
29 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
20	98	196	294	294	294
0	196	196	196	196	196
40	0	98	196	294	392
30	49	147	245	343	343
10	147	245	245	245	245

Period
30 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
40	1	103	205	307	409
10	148	250	250	250	250
20	99	201	303	303	303
30	50	152	254	356	356
0	197	197	197	197	197

Chapter III: Loss Aversion and Social Preferences in Weak-link Coordination Games

1 Introduction

Coordination failure can cause organizations to get trapped in situations that are unsatisfactory for all. When complementaries are important, employees must work together towards the goal of improvement. In many real-world organizations, however, employees need to coordinate on the reduction of adverse outcomes. In combat operations, a soldier's objective may be to safely and effectively accomplish the mission while preserving lives and precious resources. In homeland defense, disaster response teams at the state, local, and federal levels must work together to mitigate the effects of disastrous events to minimize human suffering and protect property. Moreover, employees may face situations with severe consequences if co-workers do not contribute the same level of effort. Labor economists have used experimental games to address circumstances in which a manager must make decisions that could cause employees to suffer a loss. A vital relationship to study is the relationship among employees when their actions can cause another employee to bear a loss. In this experiment, we test whether employees exert more effort when payoff-equivalent incentives are framed as losses rather than gains in both low and high stakes. We also study these settings when subjects can cause another player to lose earnings.

Management and organizational scholars have given much attention to 'high-reliability organizations' (HROs) in which substandard performance or deviations from standard practices can result in severe, adverse consequences (Rochlin 1996; Roberts 1989; K. E. Weick 2015). HROs have a high degree of accountability, unlike most organizations, where even the smallest of errors can lead to tragic results. Employees at all levels are accountable for adhering to safety protocols (Chassin and Loeb 2013; Hines et al. 2008). Examples include air traffic control systems, naval aircraft carriers, firefighting brigades, and medical emergency teams. A large area of research exists on coordination in high-risk industries such as nuclear power operations (Waller, Gupta, and Giambatista 2004) military (Entin and Serfaty 1999) aviation (Grote et al. 2010) and healthcare (Burtscher et al. 2010; Schmutz et al. 2015; Madsen et al. 2006; Tolk, Cantu, and Beruvides 2015). In these industries, an error caused by poor coordination may lead to severe consequences that harm or kill people. Decision-makers operate in a range of environments, from low-stakes training exercises to high-stakes catastrophes. Escalating circumstances can require decisions to shift from low-stakes to high-stakes. For instance, a structure fire can instantaneously become a high-stakes situation for a fire department if a hazardous materials (HAZMAT) incident is reported (Bigley and Roberts 2001).

Besides structural factors that affect coordination failure (e.g., incentive structure, group size, opportunities for feedback, for a review see Devetag and Ortmann 2007), merely framing the same situation in a different way can alter coordination behavior.[20] People will go to various lengths to avoid the pain of a loss, even when it does not necessarily make sense or align with their typical behavior (Camerer 2005). Most are willing to take higher risks if faced with a potential loss rather than a potential gain. Prospect theory (Kahneman and Tversky 1979) helps explain these behavioral tendencies with their framework of reference-dependent preferences and the notion of loss aversion. Loss aversion suggests that people evaluate outcomes based on certain reference situations, and perceived losses appear larger than perceived gains. Evidence indicates that losses have a greater psychological impact than comparable gains (Tversky and Kahneman 1991).

[20] A vast literature has looked at framing effects in public good games and found that subjects make higher contributions when the game is framed as a contribution rather than taking away from the public (Cox 2015; Khadjavi and Lange 2015; Gächter, Kölle, and Quercia 2017; Andreoni 2006; Willinger and Ziegelmeyer 2002; Madhuri 2012). However, some studies do not find an effect (e.g., Cox and Stoddard 2015; Cox et al. 2018), and others report reverse effects (Fosgaard, Hansen, and Wengström 2014).

Loss aversion has been studied extensively in the literature, though little is known about the effects when intersected with social preferences. In organizations, this interaction is highly relevant. Social preference theories assume that individuals consider not only their payoffs but also the payoffs of others (Fehr and Schmidt 1999; Bolton and Ockenfels 2000; Charness and Rabin 2002; Falk and Fischbacher 2006; Battigalli and Dufwenberg 2007, 2009). Social preference models have been tested in various contexts in which players can impact the earnings of others.[21] These two dimensions may intersect and affect decision-making simultaneously. In the following, we employ a 2×2 design to address the fundamental characteristics of loss aversion and social preferences in firm settings.

We use a variation of the weak-link coordination game designed by Brandts and Cooper (2006) called the "corporate turnaround game." Their studies tackle the issue of coordination failure, predominantly when firms are stuck in a trap. Previous studies focus on group-based incentives (Brandts and Cooper 2006a), observability and leadership (Brandts and Cooper 2006b; Brandts et al. 2015; Brandts, Cooper, and Fatas 2007), and communication (Brandts and Cooper 2007; Brandts, Cooper, and Weber 2015). The corporate turnaround game is an experimental setting designed to represent a corporate environment where a group needs to escape from a sub-optimal performance trap. The game involves repeated play between four employees of a firm. In each round, the employees simultaneously choose an effort level. Employees initially face a low bonus rate, trapping groups into the worst possible outcome. The bonus is then increased,

[21] Social preference theories have generated vast experimental literature, prominently tested in gift exchange games, dictator, trust, and ultimatum games. For a review of how games are used to measure social preferences, see Camerer and Fehr (2005); see Charness and Kuhn (2011) for a summary of contributions from laboratory experiments on the understanding of social preferences; see Cooper and Kagel (2016) for an additional survey of experimental results relating to other-regarding preferences.

transforming the game into a weak-link game (Van Huyck, Battalio, and Beil 1990) with multiple Pareto ranked equilibria.

Coordination games have demonstrated how equilibrium selection can be influenced by the desire to avoid a loss (Cachon and Camerer 1996; Rydval and Ortmann 2005). In this study, we investigate if coordination outcomes are affected by framing effects. In the “gain frame,” all payoffs are positive. We use the term “loss frame” to refer to payoff tables that contain negative payoffs. However, these situations do not reflect full loss-domain framing since not all choice alternatives result in a loss. The most common method of realizing a loss domain in the laboratory is to give subjects an adequate initial endowment from which they may incur a loss to guarantee a positive payoff at the end—the losses-from-an-initial-endowment approach (Sorensen 2018; Etchart-Vincent and l’Haridon 2011). We use data from the experiment in Chapter 2 as a benchmark, which allows us to study the relative efficiency of two alternative payment conditions incorporating losses.

Loss aversion is captured in prospect theory’s value function, which depicts a curvilinear relation between objective outcomes and subjective utility. The slope of this value function is steeper for losses than for gains (Kahneman and Tversky 1979), meaning that decisions resulting in involving potential losses or potential gains are determined by how much is at stake. We study the relationship between loss aversion and stake size.[22] Here, we refer to the size of the endowment given at the beginning of the game. We adopt the terminology used in a recent study by Poulsen

[22] Stake-size effects have been studied extensively in games, most commonly referring to the amount of monetary stakes. There is extensive literature on stake-size effects in economic games, though the results are inconclusive with wide variations in the findings (Camerer and Hogarth 1999). For example, Kocher, Martinsson, and Visser (2008) did not find stake size effects in public goods games. Johansson-Stenman, Mahmud, and Martinsson (2005) reported that the amount sent in trust games decreased significantly when the stake size was increased. Karagözoğlu and Urhan (2017) reported inconclusive stake size effects in a survey of bargaining and distribution games because of the wide variation in findings. Findings from a recent meta-analysis on stake-size effects in ultimatum and dictator games by Larney, Rotella, and Barclay (2019) suggest that people give less money in the dictator game when the stakes are high; the opposite occurs in the ultimatum game.

and Saral (2018), where “low-stakes” refer to a low initial endowment, and “high-stakes” refer to an increased initial endowment. Note that low and high stakes do not denote absolute levels but lower and higher levels in this study. To assess the stake-dependence of loss aversion, we manipulate the initial endowment that subjects receive at the beginning of the game. Subjects in low-stake conditions are given a low fixed amount and a payoff matrix in which we losses are associated with two of the choice alternatives. Subjects in the high-stake conditions receive a high fixed amount and a payoff matrix such that losses are possible with all choice alternatives above the risk-dominant option.

Additionally, we study framing effects when intersected with social preferences. We want to know how loss framing can shape decisions and whether these decisions change when employees can cause another to bear a loss. We use an experimental design from a previous experiment to model this organizational situation, reported in Chapter 2. In this experiment, subjects are exogenously given a role within each firm with two types of employees: non-restricted and restricted. Non-restricted workers make a series of decisions on how much effort they will allocate, given five effort choices: $E_i \in \{0, 10, 20, 30, 40\}$. These workers represent routine workers who account for most jobs, such as customer service roles or support roles. Restricted workers are limited to the highest effort choices: $E_i \in \{30, 40\}$. These workers are representative of employees who do not have the opportunity to put forth little to no effort, such as aviators, surgeons, firefighters. Employees must adopt an all-in mentality. This task environment reflects the characteristics of workers in HROs when worker tasks may have widely varying temporal demands and constraints.

As expected, there is a clear difference in behavior between the gain frame and loss frames. However, stake size played a significant role in how this behavior changed. In line with prospect theory, we find that individuals assigned to high-stakes treatment work harder than those assigned to the gain frame. Employees in the low-stake treatment do not improve performance in our case; loss aversion is efficiency-enhancing only in high-stake conditions. Low-stake conditions cause a decrease in coordination outcomes, while high-stake treatments attain higher coordination levels. Our research shows that framing effects can increase output, but only if the stakes are high enough. Otherwise, the outcome is worse. We find that social preferences are not as influential in the loss domain compared to our previous findings in the gain domain. It appears that social concerns are influenced by the degree of loss framing. Average minimum effort is lower in treatments with restricted workers; intersecting loss aversion and social preferences counter-intuitively leads to a drop in firm performance. In the realm of losses, the saliency of consequences on restricted workers is not as important. This finding indicates that social concerns are not as influential in decision-making when loss aversion is at play.

The remainder of this chapter is organized as follows — Section 2 reviews relevant literature. Section 3 presents the experimental design and treatments, while Section 4 develops our hypotheses. Section 5 provides the procedures, and Section 6 contains the results. Section 7 concludes.

2 Related Literature

The literature we provide in this section concentrates on framing effects in coordination settings and the stake-dependence of loss aversion in experimental games. First, we offer the existing literature on loss aversion in coordination and stag-hunt games. Next, we report experimental results on the impact of initial endowments on game behavior, focusing on the loss domain. We conclude with field experiments that use incentive contracts to exploit loss aversion, reporting that workers' performance was better under loss contracts than under gains contracts.

Loss aversion has been at the heart of laboratory and field experiments. Nevertheless, the experimental literature on loss aversion in coordination games is limited. Cachon and Camerer (1996) were the first to investigate loss avoidance in minimum and median effort coordination games. They have two conditions in the minimum effort game: the must-play condition in which subjects pay a fee to play the game and the opt-out condition in which subjects can choose to opt-out of the game, but they earn nothing. Behavior was vastly different in the two conditions. When players were forced to play the game, loss avoidance was not enough to induce efficient coordination; when players could opt out of the game, coordination improved. Their findings had significant implications for this field of research, encouraging experimentalists to explore behavioral changes when payoffs are framed as gains or losses.

Cachon and Camerer (1996) conjectured that altering the payoff structure with a fixed fee, in the beginning, would lead subjects to the payoff-dominant choice. Rydval and Ortmann (2005) tested this conjecture experimentally with three different stag-hunt game variations with negative payoffs. Their results did not confirm this conjecture. They found evidence of both certain and possible loss avoidance in stag-hunt games with high, medium, and low payoffs. Poulsen and Saral (2018) varied payoff framing and stake size in a coordination game. In the loss domain, subjects were given an initial endowment of 10 pounds in the low-stakes condition and 30 pounds in the high-stakes condition. Contrary to expectations, they found reduced coordination levels in the loss frames with the most substantial effect in the high-stakes condition. To our knowledge, no weak-link coordination studies exist that investigate the role of framing effects in a systematic way. Because of the tremendous practical relevance, it is crucial to identify behavioral factors that can explain and improve performance in organizations that require coordination of complex, interdependent activities to avoid catastrophic consequences.

Laboratory experiments have shown that the size of initial endowments has a significant effect on behavior. Andersen et al. (2011) reported that loss aversion increased substantially when the endowment on offer increased. Sorensen (2018) examined the effects of initial endowment size on individual behavior in loss domains. The results indicated that the endowment size might have a significant impact on behavior in early rounds. Etchart-Vincent and l'Haridon (2011) investigate risk aversion in a loss domain, studying three payment conditions: real losses, losses-from-an-initial-endowment, and hypothetical losses. They do not find significant differences in behavior across the loss domain's payment conditions, although they do find some differences in a gain domain. Studies have also shown that the origin of the endowment affects behavior, with unearned endowments leading to more generous and risky behavior. Individuals are more willing to take risks with money they have obtained previously, known as the house money effect (Johnson and Thaler 1990) observed, e.g., in Battalio, Kagel, and Jiranyakul (1990) and Keasey and Moon (1996). Cherry, Kroll, and Shogren (2005) studied the impact of heterogeneous endowments on contributions in a public good game. They reported significantly lower contribution levels when groups had heterogeneous rather than homogeneous endowments.

Loss aversion predicts people will work harder to avoid a loss than experience an equivalent gain (Imas, Sadoff, and Samek 2017). Recent studies have explored whether the design of incentive contracts can exploit this insight to improve employee performance in the workplace (Hong, Hossain, and List 2015; Hossain and List 2012; Fryer, Levitt, and Sadoff 2012; Fryer 2013; Stremitzer, Brooks, and Tontrup 2012). These studies found that framing incentives as losses (i.e., bonuses that workers could potentially lose) increased productivity relative to payoff-equivalent contracts where the same incentives were framed as gains. Imas, Sadoff, and Samek (2017) conducted lab experiments that measured both the impact of gain and loss contracts on productivity and ex-ante preferences for selecting into payoff-equivalent gain and loss contracts for the same task. They found that workers' performance was better under loss contracts than under gains contracts. Armantier and Boly (2015) conducted a lab and field experiment in which economically equivalent contracts were framed as menus of bonuses, penalties, or a combination of both. Subjects performed best when bonuses and penalties were combined.

While these findings demonstrate how incentive contracts can improve employee performance, it is important to understand the potential for adverse effects. For example, Cameron et al. (2010) argue that framing incentives as losses could encourage unethical behavior. Grolleau, Kocher, and Sutan (2016) show that ethical behavior can be susceptible to framing effects. The fear of a loss led to more dishonest behavior than the attractiveness of a gain. Furthermore, loss incentives do not have a universal effect; sometimes, they backfire in different environments. Hoffmann and Thommes (2020) found that loss framing backfires when it comes to motivating energy-efficient driving. List and Samek (2015) did not find evidence of framing effects on fostering healthy food choices. Charness et al. (2019) used different incentive schemes to study truth-telling and cheating behavior. They found no evidence of loss aversion when subjects were paid at the end of the session. When subjects were paid in advance, they found substantially less cheating in the loss frame.

In this experimental study, we test whether framing effects can lead groups to efficient coordination. We develop an experimental environment to test stake-size effects on organizational performance. We also intersect loss aversion and social preferences to model real-world situations when employees' actions can cause another employee to bear a loss. This paper contributes to the coordination literature on framing effects with different stakes and coordination outcomes when loss aversion intersects with social preferences. Indeed, in the wake of the current COVID-19 pandemic, healthcare workers place their lives on the line and trust their colleagues to practice the same precautions. Their actions are in the spotlight, attracting attention worldwide. This underlines the importance of understanding how exposure to loss can shape decisions and whether this differs with social considerations when employees' decisions can cause another to bear a loss.

3 Experimental Design

3.1 The Baseline Game

An experimental firm consists of a fixed grouping of four employees who interact for 30 consecutive rounds. The firm's productivity in each round is determined by employees' effort choices for the round. Each round is considered a workweek. Intuitively employees spend 40 hours a week at work; therefore, we restrict employees' effort levels to ten-hour increments: $E_i \in \{0, 10, 20, 30, 40\}$. Participants begin with a fixed wage, A, and a bonus rate, B, dependent upon the group contribution. Effort is costly, with Cx_i denoting the cost of a unit of effort for the *i*th employee. The payoff received by any player *i* can be represented by the following formula:

$$\text{Employee } i\text{:} \quad \pi_i = A + B\min(x) - Cx_i \tag{3}$$

Table 9 corresponds to the case in which A = 200, B = 10, and C = 5. The payoff to each player depends on her choice, indicated by the row, and all players' minimum choice, indicated by the column. The number in each cell gives this payoff. Notice that if everyone picks the highest possible action, then the payoff-dominant equilibrium in the lower-right corner is selected, while if any of the players choose the lowest possible action, then the risk-dominant equilibrium in the upper-left corner is selected. Players incur a cost of 50 to increase their effort at each level. However, if matched, the net gain is also 50. These values were selected because past experiments have revealed that coordination failure is common, particularly when the amount of the bonus does not exceed the cost by a considerable amount. We know from other studies that lower costs of exerting effort are efficiency-enhancing (Goeree and Holt 2001; 2005; Brandts and Cooper 2006a; 2006b; Devetag and Ortmann 2007; Riedl, Rohde, and Strobel 2016). All payoffs during the experiment were denominated in an artificial currency, experimental currency units (ECU). Subjects have the potential to earn 12,000 ECUs at the end of the 30 rounds. ECUs were converted at a rate of one euro per 500 ECU.

The turnaround game is traditionally broken up into three ten-round blocks with the goal of inducing coordination failure in the first block to pull firms out of performance traps in the following block. Our experiment does not use a block design; subjects play 30 rounds. A key feature of our design is that we introduce volatility in the experiment by randomly distorting the payoffs in each round. Each decision stage presents a new payoff table with changing values of the fixed-wage, cost of effort, and bonus. Using Equation 3 as a benchmark, we established a range of values to generate thirty new payoff tables, keeping the cost and potential net gain close to 50 in each round and the potential net monetary payoffs close to 12,000 ECUs (this is the maximum amount a subject could make over 30 rounds if Table 9 were used in all rounds). We restrict the feasible fixed-wage rates to the integers ∈ {195, 196, 197, 198, 199, 200, 201, 202, 203, 204, 205}. The values of the bonus were restricted to B ∈ {9.5, 9.6, 9.7, 9.8, 9.9, 10.1, 10.2, 10.3, 10.4, 10.5}. C_i was equal to 4.8, 4.9, or 5. Given the other parameter values, these are the effort costs that do not allow subjects to lose money in a round (specifically if the fixed-wage is less than 200, the cost of effort must be less than 5 to avoid negative payoffs). (See Appendix C for screenshots of each payoff table).

Table 9: Employee *i*'s payoff table

		Minimum Effort by Other Employees				
		0	**10**	**20**	**30**	**40**
Effort by Employee *i*	**0**	200	200	200	200	200
	10	150	250	250	250	250
	20	100	200	300	300	300
	30	50	150	250	350	350
	40	0	100	200	300	400

3.2 The Game in Loss Domains

To examine the effects of framing, the baseline game described above was transformed into a game with negative payoffs, keeping potential net monetary payoffs (12,000 ECUs) the same. This is the standard method used in loss frames in experimental games (Poulsen and Saral 2018; Sorensen 2018). Payoffs in the loss domain were derived simply by reducing the fixed-wage in each round and increasing the initial lump sum by this amount. This ensures that subjects will not go bankrupt. To assess stake-size effects, we manipulate the initial endowment that subjects receive at the beginning of the game. Hence, both frames are structurally equivalent in terms of payoffs. In the low-stakes frame, the initial endowment is 3,000 ECUs (1/4 of the potential net earnings in the baseline game), and the fixed-wage is 100. This produces the payoff matrix in Table 10. The high-stakes frame eliminates the fixed-wage in each round, providing this in the form of one lump sum payment of 6,000 ECUs (1/2 of the potential net earnings in the baseline game) at the beginning of the game. Table 11 shows the resulting payoff matrix.

Players in the standard game face risks in selecting effort above zero; now, they face the potential for loss. For example, a player with Table 9 payoffs will make 0 ECUs in a round if she chooses 40 hours, and one person in the group selects 0 hours. The same situation causes players with Table 10 payoffs to lose 100 ECUs in a round and players with Table 11 payoffs to lose 200

ECUs. Table 11 differs from the low stakes' payoff matrix in that any selection above 0 hours incurs the risk of negative earnings. Subjects do not earn additional ECUs if they select zero.

Table 10: Payoffs in low-stakes

		Minimum Effort by Other Employees				
		0	**10**	**20**	**30**	**40**
Effort by Employee *i*	**0**	100	100	100	100	100
	10	50	150	150	150	150
	20	0	100	200	200	200
	30	-50	50	150	250	250
	40	-100	0	100	200	300

Table 11: Payoffs in high-stakes

		Minimum Effort by Other Employees				
		0	**10**	**20**	**30**	**40**
Effort by Employee *i*	**0**	0	0	0	0	0
	10	-50	50	50	50	50
	20	-100	0	100	100	100
	30	-150	50	50	150	150
	40	-200	-100	0	100	200

3.3 Treatments

Our basic experiment design comprised a gain frame treatment (GF) and two payoff structures with losses: low-stakes frame treatment (LF) and high-stakes frame treatment (HF). The data presented for the gain frame treatments were collected in an earlier set of experiments, and the findings were reported in Chapter 2. Subjects in LF are given 3,000 ECUs at the beginning of the game and asked to make their decisions using Table 10. In this frame, selecting 20 hours increases earnings only if all other players choose 20 hours or above. If one player chooses zero, then subjects do not increase their profits in that round. Players lose ECUs if they select 30 or 40 hours, but another player in their group chooses zero. Subjects in the high-stakes frame (HF) receive 6,000 ECUs at the beginning of the game and make their decisions using Table 11.

To study framing effects when intersected with social preferences, we alter the firm composition by assigning a restricted worker to each firm. Each firm consists of four non-restricted employees and one restricted employee. We study three social conditions: gain frame with social concerns (GF-Social), low-stakes frame with social concerns (LF-Social), and high-stakes frame with social concerns (HF-Social). LF-Social uses the same payoff structure as LF above, and HF-Social uses the HF payoff structure. Table 12 provides descriptions of each treatment.

Table 12: Description of treatments

Treatment name	GF	GF-Social	LF	LF-Social	HF	HF-Social
Fixed wage	200	200	100	100	0	0
Initial endowment	0	0	3,000	3,000	6,000	6,000
Firm size	4	5	4	5	4	5
Number of firms	9	18	6	5	14	11
Number of employees	36	90	24	25	56	55

4 Theoretical Framework

According to prospect theory, people are more sensitive to losses compared to gains (Kahneman and Tversky 1979). The reflection effect of prospect theory demonstrates the reversing of risk aversion in which people tend to avoid risks under the gain domain and to seek risks under the loss domain (Kahneman and Tversky 1979; Tversky and Kahneman 1981; 1992). Meaning, no risk aversion is expected under the loss domain. Evidence of the reflection effect has been reported in many studies (Ert and Yechiam 2010; Weber, Shafir, and Blais 2004; Laury and Holt 2011; Abdellaoui, Bleichrodt, and Paraschiv 2007; Abdellaoui, Bleichrodt, and Kammoun 2013; Booij, Van Praag, and Van De Kuilen 2010; Baucells and Villasís 2010; Liu et al. 2014). In this case, we can expect subjects to choose high effort levels since these values offer the highest potential payment. If subjects take risks to move towards the payoff-dominant equilibria, we can assume that the two framing treatments (LF and HF) will lead to more efficient outcomes.

Hypothesis 1 (Framing effects) Minimum effort will be higher in the loss-frame treatments than the gain-frame treatment.

Our second hypothesis concerns the existence of stake-dependence in framing treatments. Although prospect theory does not empirically address stake-size effects, Kahneman and Tversky (1979) stated that "the aversiveness of symmetric fair bets generally increases with the size of the stake." A stake-size effect on risk aversion is often found in which risk aversion increases with the magnitude of the payoff. This phenomenon has been labeled as "relative risk aversion" (Holt and Laury 2002) and "the peanuts effect" (Weber and Chapman 2005). In the psychology literature, studies have shown that loss aversion increases under high stakes (e.g., Harinck et al. 2007; Mukherjee et al. 2017; Ert and Erev 2013). Andersen et al. (2011) reported that loss aversion increased substantially when the endowment on offer increased. If stake-size effects exist, behavior will differ in framing treatments as the magnitude of the initial endowment changes. We expect that individual worker performance and firm production levels will be higher in high-stake frames than low-stake frames.

Hypothesis 2 (Stake-size effects) High-stake frames will result in more efficient coordination outcomes than low-stake frames.

Our third hypothesis concerns the role of social preferences. We speculate that loss aversion and social preferences are complements such that the level of each enhances the effect of the other. Baron (1995) proposed that people follow a heuristic, called the 'do no harm' principle, whereby they avoid courses of action seen as actively harmful to others. Acts that could cause losses for others are perceived as more detrimental than comparable acts that could cause decreased gains (Royzman and Baron 2002). In our case, this means that causing someone else to lose money is considering more damaging than depriving them of a gain. Evans and van Beest (2017) and Van

Beest et al. (2005) provide support for the do-no-harm principle in social decision-making. Notably, the do-no-harm principle posits that the effects of framing on trust and reciprocity should be more robust when decisions have explicitly social consequences – losses should have more potent effects on behavior when they invoke the fear of harming another person. We anticipate that subjects will follow a 'do-no-harm' heuristic and contribute high effort levels to avoid causing another to lose money. As a result, we expect firm production and individual employee performance to be higher in social frames than the corresponding four-employee treatments.

Hypothesis 3 (Loss aversion and social preferences) Social frames will result in more efficient coordination outcomes than framing effects by itself.

5 Procedures

All sessions were run in the business department at the Autonomous University of Barcelona (UAB). A total of 160 undergraduate students (82 female; M_{age} = 22 years, SD = 4.54) were recruited through ORSEE (Greiner 2015) in six sessions during May 2019. Seventeen percent of the students were enrolled in business or economic programs, while the rest came from various faculties. Subjects received five euros to arrive on time. Participants could only participate once. Each session lasted approximately 1.5 hours, from the time students checked in with the lab to the time they left the lab. The average payoff, including the five-euro show-up fee, was 20.50 euros.

The experiment was computerized using the experimental software, z-Tree (Fischbacher 2007). To ensure anonymity, computers were situated within a partition-separated terminal. At the beginning of each session, subjects were randomly seated and asked to read the consent form, print their name, date, and provide a signature in agreement. The experimenter collected the signed consent forms, and subjects were given a written set of instructions and asked to follow along while the experimenter read aloud. At the end of the instruction set, participants were given a pre-quiz to ensure they understood the design of the game. Subjects were asked to raise their hand and wait for an experimenter to come to their terminal if they had any questions about the game. The full text for the instructions and pre-quiz is given in Appendix A.

The instructions used a corporate context. Subjects were referred to as employees and were told they are working for a firm. Instead of asking subjects to choose a level of effort, we asked them to allocate time between Activity X and Activity Y, with Activity X playing the role of effort. Subjects were informed of how many employees were in each firm, which remained fixed throughout the 30 rounds. Subjects were told that each round would present a new payoff table with changing values of the fixed-wage, cost of effort, and bonus. They were provided a range of possible values and given examples of payoff tables with different values. The instructions for treatments with restricted workers gave descriptions of both worker types and the number of non-restricted and restricted employees within each firm, but subjects did not receive their role assignment until the start of the game.

Once subjects answered all the questions correctly, they were ready to proceed to the computer to begin the 30 rounds. In treatments with restricted workers, the first screen informed participants of their role in the firm, which remained fixed throughout the game. Once employees acknowledged their role assignment, the rounds began. In each round, employees simultaneously chose their effort levels. After each round, subjects were informed of the group's minimum effort, their earnings in that round, and their accumulated earnings.

At the end of the game, subjects were asked to fill out a questionnaire. During this time, the experimenter calculated the earnings and prepared envelopes with cash earnings. The methods carried out in this experiment were approved by the ethical committee at UAB.

6 Results

This section investigates the impact of the experimental treatments on firm production levels and individual worker performance. The term effort is used for the value chosen in period *t* by employee *i*. Minimum effort refers to the minimum contribution of all four employees in a firm in period *t*. A large part of our analysis uses distributional comparative statics to study how individual decisions and equilibrium outcomes vary with changes in parameters. In Section 6.1, we analyze framing effects in the gain frame and the loss-frame conditions. Section 6.1.1 narrows the focus to assess whether stake-size effects are observed between the loss frames. Section 6.2 examines the interplay between loss aversion and social preferences by comparing treatments with non-restricted workers, both in the gain and loss frames. We study stake-size effects with the social loss-frame treatments in Section 6.2.1. We compare the efficacy between treatments to assess outcomes when loss aversion intersects with social preferences. Section 6.3 shifts our analysis to focus on the magnitude of the effects of firms with restricted workers. Unless otherwise noted, treatment effects report results of Mann-Whitney tests using firm averages across all 30 rounds.

6.1 Framing Effects

The first distinction we explore is between the gain frame treatment (GF), the low-stake loss frame treatment (LF), and the high-stake loss frame treatment (HF) to answer the broad research question of whether behavior changes under gain and loss domains. Using GF as the baseline reference, we report overall changes in minimum effort and effort. Figures 9 and 10 map the average minimum effort and average effort per treatment across all rounds. We anticipated outcomes to increase from the gain frame treatment to each loss frame. Instead, minimum effort decreased in LF while HF resulted in higher outcomes. Average minimum effort in GF is 13.96. Average minimum effort is 4.35 hours lower in LF (9.61) ($p = .402$, $n = 15$) and 6.30 hours higher in HF (20.26) ($p = .360$, n

= 23). In GF, firms were not able to reach efficient coordination. Five firms coordinated at the least efficient level of 0 hours, one a minimum of 10 hours, and three minima of 20 hours. Firms in LF report similar distributions in the final period: two firms finished the game at zero, three minima of 10, and one minimum of 20. One firm was able to achieve efficient coordination in HF. Of the remaining firms, five firms coordinated at the least efficient level of zero, one a minimum of 10, and seven minima of 30.

Looking at behavior in the first period can be informative of how subjects perceive the game and beliefs of how others will behave in different situations. Sorensen (2018) found that the size of the endowment had a significant impact on behavior in early rounds. Period one average minimum effort is 16.67 in GF, but Figure 9 shows a steady decline in firm contribution levels over the course of the game. LF began the game with low minimum effort levels (11.67) and was not able to recover or bounce back in any of the rounds; minimum effort dropped to 10.00 hours by period 10 and 8.33 from period 20 through the end. The average minimum effort in period one was 19.28 in HF, which was sustained throughout the game with the exception of a brief drop in period 17.

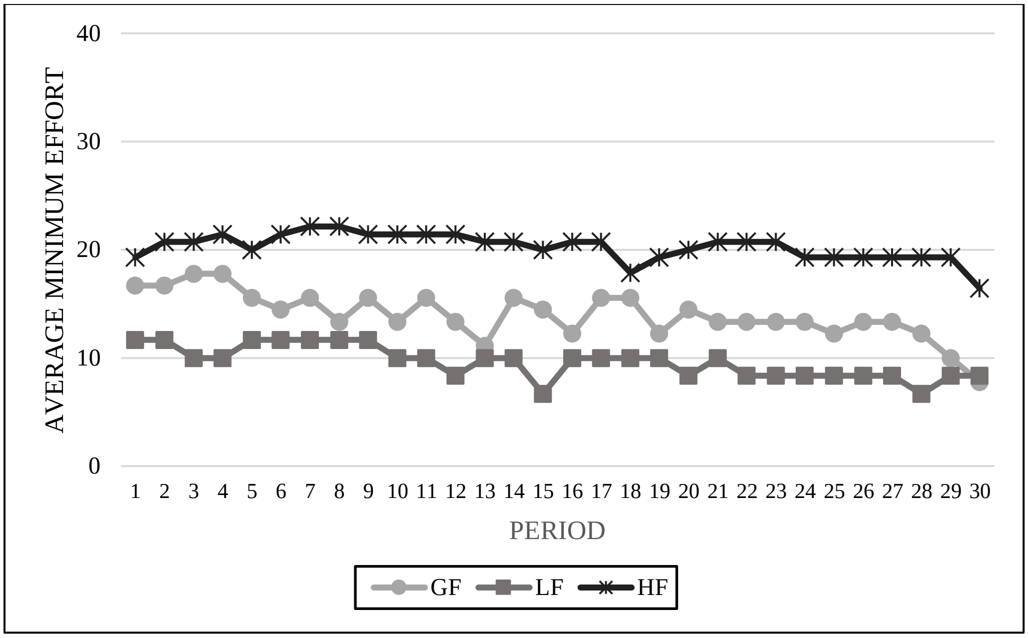


Figure 9: Average minimum effort across all rounds

Turning to the employee level, employees in the gain frame exert an average of 17.75 hours. Average effort is 4.70 hours lower in LF (13.03) (p = .475, n = 15) and 5.43 hours higher in HF (23.18) (p = .336, n = 23). In GF, average effort in period one is 26.67, but Figure 10 reveals that employees lower their effort significantly throughout the remaining periods, adjusting to the minimum output. Period one average effort is 25.00 in LF, which lasts only two rounds. By the midpoint of the game, average effort was 10.00 hours, persisting through the end. HF employees had the strongest start with average effort at 30.50 hours, but Figure 10 reveals a steady decline throughout the rounds. Lastly, we look at employee earnings across treatments. Compared to GF, employee earnings are significantly lower in LF (p = .001, n = 80) and higher in HF (p = .015, n = 92).

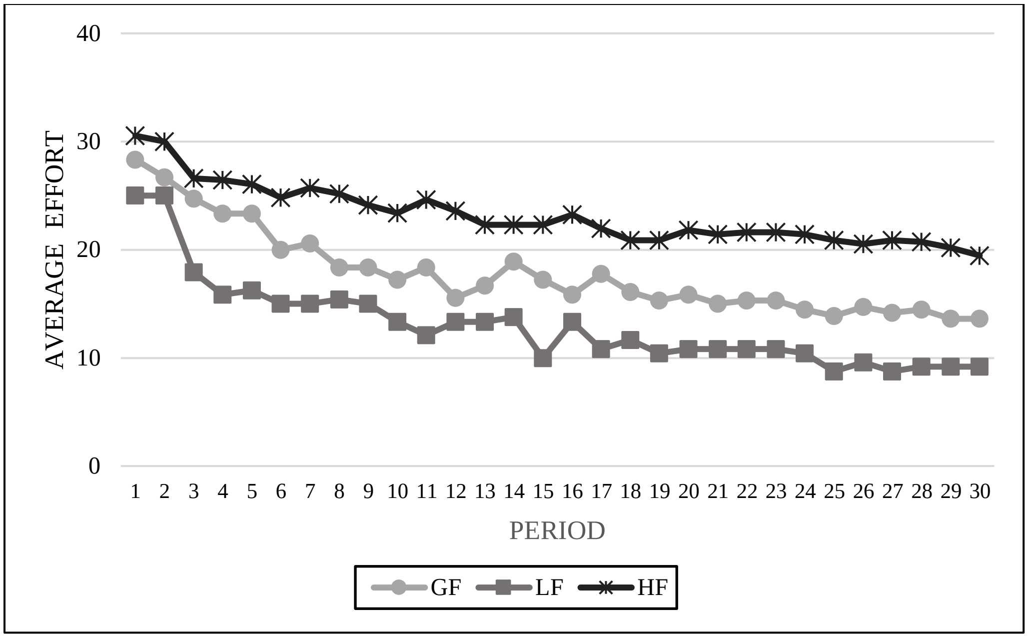


Figure 10: Treatment effects on effort

We summarize these findings in the result below:

Result 1 (Framing effects): Compared to the gain frame, the high-stake loss treatment improves firm productivity, while the low-stake loss treatment reduces minimum effort levels. Hypothesis 1 is partially supported.

This does not lend support for the reflection effect of prospect theory in which people tend to avoid risks under the gain domain and seek risks under the loss domain (Kahneman and Tversky 1979; Tversky and Kahneman 1981; 1992). This result differs from Poulsen and Saral (2018), who found lower coordination levels under loss frames with the most substantial impact in the high-stakes condition.

Section 6.1.1. Low-stake vs. High-stake

Next, we compare LF and HF to test whether we observe a stake-size effect. Figures 9 and 10 reveal the stark differences in behavior between low and high-stake treatments. Average minimum effort in HF is more than twice the average minimum output of LF (p = .094, n = 20). Employee average effort is 13.03 hours in LF and 23.18 hours in HF (p = .109, n = 20). Period one average effort is 25.00 in LF and 30.50 in HF (p = .008, n = 20). The difference in corresponding minima is even larger (11.67 vs. 19.29; p = .117, n = 20). The majority of subjects in LF choose effort levels below 20 hours (63%), while the majority of subjects in HF (60%) select effort levels above 20. In the first period, 75 percent of HF firms and 40 percent of LF firms attained minimum effort levels above 20 hours. In LF, .83 percent of choices resulted in a loss; in HF, 8.7 percent of choices incurred a loss. Recall that in LF, subjects face potential negative earnings only at 30 and 40 hours. This could be an explanation for low contributions in this frame. In HF, however, subjects face negative earnings at any level above 0 hours. We hypothesized that high-stake frames would result in higher coordination outcomes than low-stake frames. We summarize these findings in the result below:

Result 2 (Stake-size effects) Loss framing with high stakes improves firm minimum effort compared to the low-stakes treatment, though not significant. Hypothesis 2 is not supported.

This result is consistent with the observation that high stakes facilitate risk aversion (Holt and Laury 2002; Weber and Chapman 2005). LF does not reveal that loss aversion leads subjects to take risks and coordinate at the top level. This result supports previous findings with reversed loss-aversion at low magnitudes (Harinck et al. 2007; Mukherjee et al. 2017) and psychology studies in which loss aversion does not emerge for small to moderate losses (Yechiam and Hochman 2013; Gal and Rucker 2018).

6.2 Interplay Between Loss Aversion and Social Preferences

This subsection considers firms with an added restricted worker, using GF-Social as the baseline reference. We compare the efficacy between treatments to assess treatment outcomes when loss aversion intersects with social preferences. Since the employee level averages include restricted workers, we focus our analysis primarily on the non-restricted workers. Compared to GF-Social, average minimum effort is lower in LF-Social (p = .434, n = 23) and higher in HF-Social (p = .333, n = 29). Treatments in the social frames follow similar patterns in framing effects treatments above, indicating loss aversion impacts social frames in the same manner. In GF-Social, two firms were able to achieve efficient coordination. Of the remaining groups, seven groups coordinated at the least efficient level of zero, three obtained minima of 10, one a minimum of 20, and five minima of 30. In HF-Social, one firm achieved efficient coordination, two achieved minima 30, 20, and 10 hours; the remaining four ended the game at the least efficient level. Firms in LF-Social were not able to coordinate at efficient levels; two groups coordinated at the least efficient level, three groups obtained minima of 10, and one minimum of 20.

Figure 11 maps the average minimum effort per treatment across all rounds. Average minimum effort in GF-Social started at 15 hours, consistent throughout the game. LF-Social started slightly lower with average effort of 12 hours, which dropped to 8 by period three, eventually dropping to 6.00 hours in the final rounds. HF-Social had a stronger start at 20 hours, rising to 22 sporadically until the game's final ten rounds, dropping to 19.00 and finally 14.50 in the last period. At the employee level, average effort across all rounds in GF-Social is 21.40 hours, 6.35 hours more than LF-Social (15.05) (p = .363, n = 23) and 3.47 hours less than HF-Social (24.87) (p = .381, n = 29). Employees earned an average of 7,835 ECUs in GF-Social, 6,169 ECUs in LF-Social (p = .587, n = 23), and 8,302 ECUs in HF-Social (p = .363, n = 29).

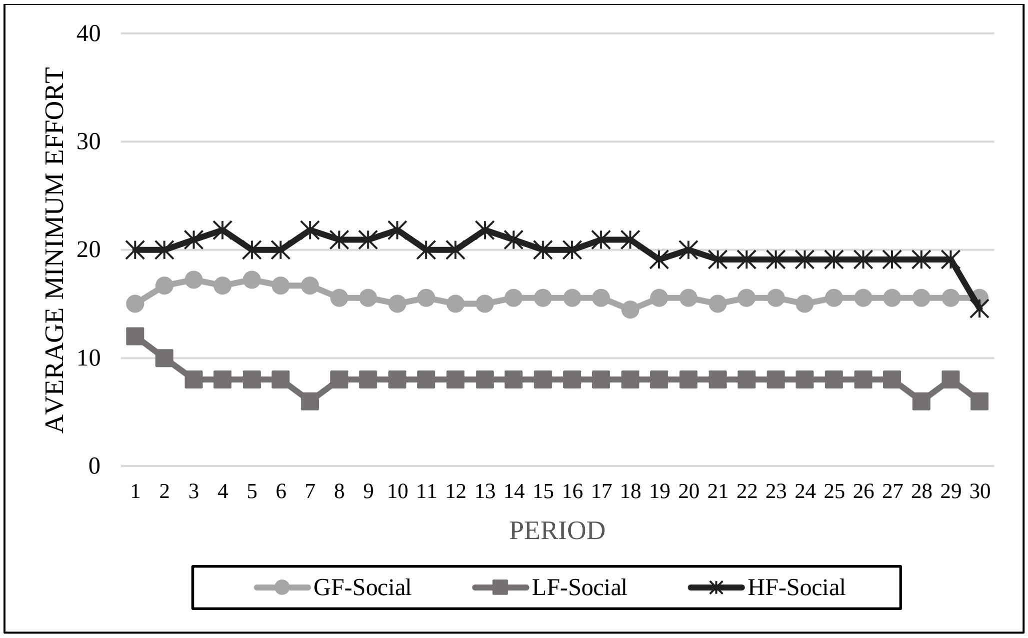


Figure 11: Average minimum effort across all rounds

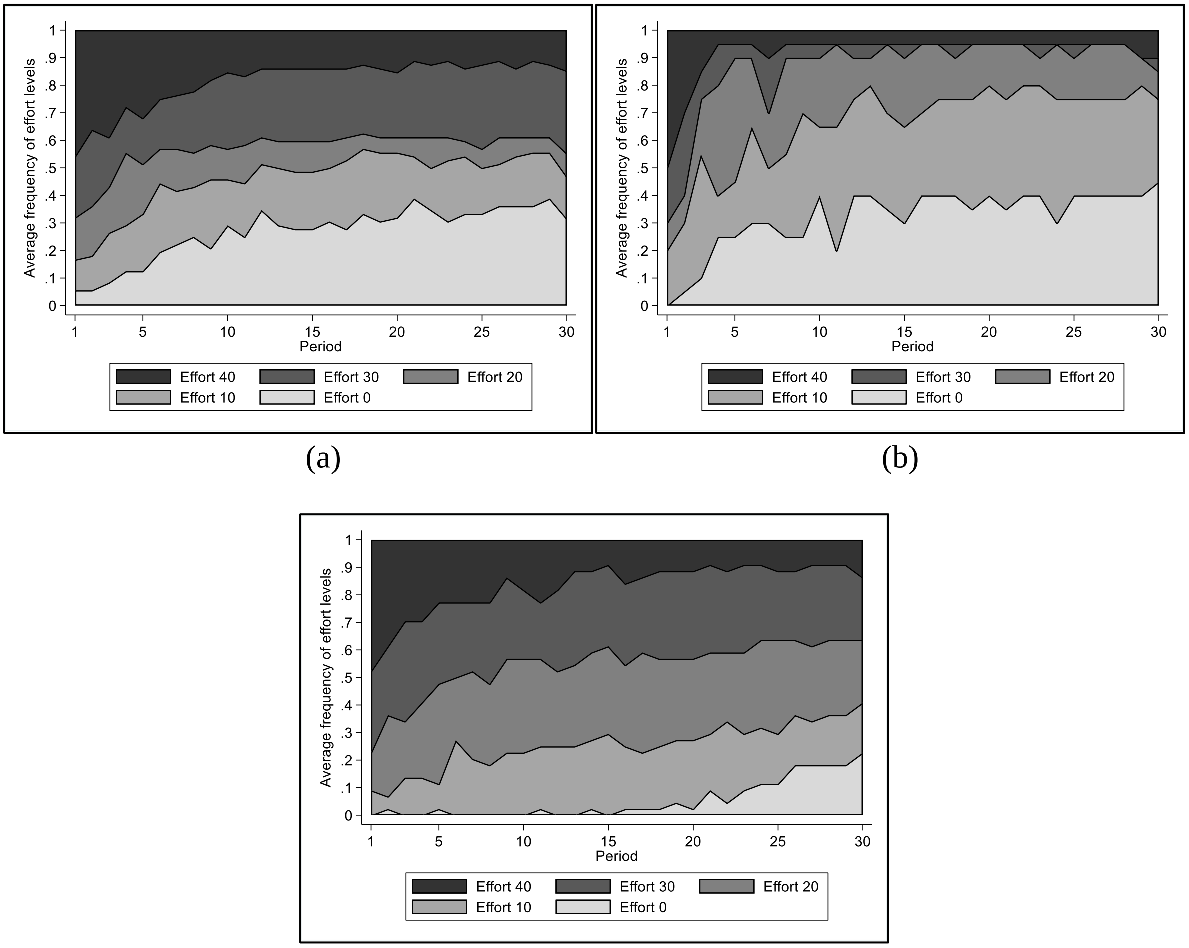
We consider choice behavior by agents in which they are responsible for the earnings of one restricted worker in their firm. Non-restricted workers exert an average effort of 18.85 hours in GF-Social, 12.30 hours in LF-Social, and 23.15 in HF-Social (p = .440, n = 23; p = .357, n = 29). In the first round, average effort by non-restricted employees was 29.17 in GF-Social, though this high level of input was not sustained throughout the game. Trend tests show that effort was significantly decreasing over rounds (p < .001, n = 30).[23] Average effort was 30.00 in LF-Social in period one, which dramatically drops in the first few rounds, falling to 10.00 hours by the midpoint, which was sustained throughout the game. Period one average effort in HF-Social is similar, 31.60 declining steadily over the rounds and finishing at 19.00 hours.

We continue our analysis with the presentation and comparison of effort choices. First, we look at the difference between the frequencies of choosing the risk-dominant option. If the minimum contribution in GF-Social is zero, the restricted worker earns less than the other

[23] Trends were calculated with the Stata command "nptrend" (Cuzick 1985; Altman 1991).

members, but she does not lose money in any of the rounds. In this setting, risk-dominant choices account for 27 percent of the observations. Turning to the loss frames, recall that the restricted worker can lose up to 100 ECUs per round in LF-Social and 200 ECUs each period in HF-Social. We imagined that subjects in the loss frames would make efficient choices, as acts that could cause losses for others can be perceived as more detrimental than comparable acts that could cause decreased gains (Royzman and Baron 2002). This seems to be the case in HF-Social, with risk-dominant choices constituting 5 percent of the observations. Conversely, risk-dominant choices make up 32 percent of all observations in LF-Social. It appears that the high-stakes conditions encourage non-restricted agents to adhere to the do-no-harm heuristic.

Next, we consider cumulative frequencies above 20 hours since 30 and 40 hours are the effort choices when everyone in the group can receive the same amount of money. The cumulative frequencies above 20 in GF-Social and HF-Social are similar: 43 percent of GF-Social observations and 46 percent of observations in HF-Social. The cumulative distribution is much lower in LF-Social, with 13 percent of observations at the top two effort levels. This finding indicates that social concerns are not as influential in decision-making when loss aversion is at play. Figure 12 provides the cumulative distribution of efforts for each treatment.



(a) (b)

(c)

Figure 12: Non-restricted worker cumulative distribution of efforts (a) GF-Social; (b) LF-Social; (c) HF-Social

6.2.1 Stakes and Social Preferences

In this subsection, we compare LF-Social and HF-Social to assess whether we observe a similar stake-size effect as above. Average minimum effort is considerably higher in HF-Social (19.94) than LF-Social (8.00) (p = .064, n = 16). Average effort by non-restricted workers is 12.30 in LF-Social and 23.15 hours in HF-Social, though this difference is not significant (p = .115, n = 16). Employee earnings are higher in HF-Social (8,302 ECUs) compared to LF-Social (6,169 ECUs) (p = .038, n = 16). Like LF and HF,1.5 percent of LF-Social choices resulted in a loss, and 9.7

percent of choices incurred a loss in HF-Social. This tells us that social concerns are influenced by the degree of loss framing.

6.3 Loss Aversion vs. Social Preferences

This phase of our analysis examines the relationship between loss framing treatments (LF and HF) and the corresponding treatment with both loss framing and social concerns (LF-Social and HF-Social). As a baseline reference, we refer to the changes between GF and GF-Social to measure how social preferences influence behavior when loss aversion does not exist. Average effort increases from GF to GF-Social by 3.65 hours and average minimum effort increases by 1.71 hours, though neither are statistically significant (p = .472, n = 126; p = .272, n = 126).

Turning to the first frame in the loss domain, we check for a similar pattern. From LF to LF-Social, average effort increases 2.00 hours from 13.03 to 15.05 (p = .662, n = 11) while average minimum effort decreases from 9.61 in LF to 8.00 in LF-Social (p = .706, n = 11). LF and LF-Social begin the game at the same minimum average effort level, close to 12 hours, and follow similar patterns throughout the game. LF average minimum effort falls to LF-Social values by period 15. Minimum effort in LF-Social drops to 8.00 in the third period, where it consistently stays despite a drop to 6.00 in period seven and again in final periods (periods 28 and 30). Average minimum effort in LF is 10.00 until period 22, at which point it drops to 8.00. Trend tests report decreasing minimum effort levels in LF (p = .025) and LF-Social (p = .393). Average effort in the first period is higher in LF-Social (30.40) compared to LF (25.00) (p = .078, n = 11). By the halfway point in the game, average effort dropped to 10.00, and in the final five periods, average effort is less than 5.00. Period one effort in LF-Social starts above 30.00, but average effort drops below 20.00 hours by the fifth period. By period 16, the average effort is 12.00, which was the first-period average minimum effort. Trend tests report significantly decreasing effort levels in LF and LF-Social ($p < .001$).

In the high stakes frame, average minimum effort is quite close: 20.26 in HF and 19.94 in HF-Social ($p = .998$, $n = 25$). Average minimum effort in HF-Social closely follows HF's dynamics, consistently averaging around two hours less than HF and only dropping below HF in a few instances. Average minimum effort is rather close in the first period, 19.28 in HF, and 20.00 in HF-Social ($p = .977$, $n = 25$). In both treatments, average minimum effort is 20.00 in period 5, period 15, and period 20. By the end of the game, average minimum effort is 16.42 in HF and 14.54 in HF-Social. Trend tests report decreasing minimum effort levels in HF and HF-Social, though not significant ($p = .598$ for HF; p = .239 for HF-Social). Average effort in HF increases from 23.18 to 24.87 in HF-Social ($p = .840$, $n = 25$). In the first period, HF average effort is 30.50 and HF-Social average effort is 32.36 ($p = .797$, $n = 25$). By the second period, average effort is 30 hours in both treatments. HF-Social average effort levels remain consistently above HF, following similar patterns as the low-stake frames. Trend tests report significantly decreasing effort levels in HF and HF-Social ($p < .001$). We summarize these findings in the result below:

Result 3 (Loss aversion and social preferences) Loss framing and social considerations do not improve firm productivity, as measured by minimum effort. The data does not support Hypothesis 3.

7 Conclusions

In this paper, we study framing effects in the classic problem of coordination, a problem that has important implications. Coordination in weak-link games was examined when outcomes were framed as gains and in situations when the same decision has the potential to lose money. Our loss frames differ with respect to the reference point, or the initial endowment subjects receive. We also intersect loss aversion and social preferences to model real-world situations when employees' actions can cause another employee to bear a loss.

In line with prospect theory, we find that individuals assigned to high-stakes treatment work harder than those assigned to the gain frame. Employees in the low-stakes treatment do not improve performance in our case. This does not lend support for the reflection effect of prospect theory in which people tend to avoid risks under the gain domain and to seek risks under the loss domain (Kahneman and Tversky 1979; Tversky and Kahneman 1981; 1992). This result differs from Poulsen and Saral (2018), who found lower coordination levels under loss frames with the most substantial impact in the high-stakes condition. One difference between our studies is that Poulsen and Saral (2018) gave subjects cash vouchers at the beginning of the experiment. In contrast, subjects in our experiment were informed of the initial endowment in the form of ECUs, but they did not have a physical voucher representative of this payment. Additionally, it may be problematic to compare the studies since the endowments differ between our studies.

We find a significant stake-size effect. Loss framing with low stakes produced low coordination outcomes; loss framing with high stakes improved employee effort and firm minimum effort. Loss aversion did not guide individuals to efficiency in low stake conditions. This result is in accordance with findings of reversed loss aversion at low magnitudes (Harinck et al. 2007; Mukherjee et al. 2017) and psychology studies in which loss aversion does not emerge for small to moderate losses (Yechiam and Hochman 2013; Gal and Rucker 2018). In a group of four standard workers, framing effects can increase output, but only if the stakes are high enough. Otherwise, the outcome is worse. This is in line with findings that loss aversion emerges only with high-stakes gambles (Ert and Erev 2007; 2013; Rabin and Weizsäcker 2009) and findings showing no behavioral indication of loss aversion in small stakes reported in description-based tasks

(Battalio, Kagel, and Jiranyakul 1990; Birnbaum and Bahra 2007; Ert and Erev 2013; Rieskamp 2008; Thaler and Johnson 1990).

When we examined the interplay between loss framing and social preferences, we found similar patterns of coordination behavior. Coordination was significantly higher in the social frame with high stakes than the social frame with low stakes. It appears that social concerns are influenced by the degree of loss framing. Average effort was higher in social treatments than corresponding four-employee treatments, but average minimum effort decreased. Intersecting loss aversion and social preferences counter-intuitively lead to a drop in firm performance. We considered choice behavior by agents across the two environments in which they are responsible for the earnings of one restricted worker in their firm. We expected to see a decrease in the number of subjects selecting 0 hours in social frames as they can see that restricted subjects will lose ECUs each round by doing so. This was observed in the high-stake condition, but not with low-stakes. Compared to the gain frame, there was no significant difference in the frequency of choices at the top effort levels in which everyone in the group can receive the same amount of money. This finding indicates that social concerns are not as influential in decision-making when loss aversion is at play.

This paper contributes to the coordination literature on framing effects with different stakes and coordination outcomes when loss aversion intersects with social preferences. Our results also inform theory as the interplay between loss aversion and social preferences has significant implications. These results are applicable to the management literature on modern-day organizational arrangements, particularly relevant to HROs. Our findings have applications for practice in areas such as how to frame contracts. Due to the contingent nature of loss aversion, our work suggests that organizations should practice caution if relying on loss frames as a performance-enhancing device. Additionally, our work demonstrates the importance for firms to understand how exposure to loss can shape employees' decisions and whether this differs when employees' decisions can cause another to bear a loss.

While this study provides an opportunity to advance the understanding of framing effects and social preferences in coordination games, a limitation of this study can be emphasized that deals with full loss-framing. In our experiment, there was no situation where all choices resulted in a loss. Implementing an investigation in a true domain of losses would be challenging since we cannot ask subjects to pay us at the end of the experiment. Further, there are potentially relevant variables that were not controlled that could have affected individual behavior in these interactions (Karagözoğlu and Urhan 2017). We did not collect information on subjects regarding their attitudes towards loss (Gächter, Johnson, and Herrmann 2007) or personality characteristics, such as the Big-Five personality traits (Ben-Ner, Kramer, and Levy 2008; Mccrae et al. 2016).

Considering the prevalence of organizations that face coordination demands in high-risk industries, further research on this topic should be undertaken. Because of the tremendous practical relevance, it is crucial to identify behavioral factors that can explain and improve performance in organizations that require coordination of complex, interdependent activities to avoid catastrophic consequences. The COVID-19 pandemic has demonstrated the importance of understanding behavior in loss domains. This underlines the importance of understanding how exposure to loss can shape decisions and whether this differs with social considerations when employees' decisions can cause another to bear a loss.

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Appendix A: Experimental Instructions

A.1 Instructions for LF

[**Remark:** In the following, we present the instructions for treatment LF. Paragraphs with **[*]** were given in both framing treatments, but appropriately reformulated for HF. A complete set of instructions is available from the authors.]

INSTRUCTIONS

The purpose of this experiment is to study how individuals make decisions in certain contexts. In addition to the five-euro participation fee, you will be paid any additional money you accumulate at the end of today´s session. All payoffs during the experiment are denominated in an artificial currency, experimental currency units (ECU). At the end of the experiment, ECUs will be converted at a rate of one euro per 500 ECUs. Upon completion of the experiment, your earnings will be converted to euros, and you will be paid privately in cash. The exact amount you receive will be determined during the experiment and will depend on your decisions and the decisions of others. If you have any questions during the experiment, please raise your hand and wait for an experimenter to come to you. Please do not talk, exclaim, or try to communicate with other participants during the experiment. Participants intentionally violating the rules may be asked to leave the experiment and may not be paid.

[*] Decision rounds: At the beginning of the experiment, you will receive 3,000 ECUs. The experiment consists of 30 rounds. In each round, you will be in a group with three other participants. The composition of these groups, which are called *firms,* will not vary during the experiment. Given that nobody will know the identity of the members of each group, all the actions you take during the experiment will be anonymous.

Task: You and the other members of your group are employees of a firm. You can think of a round of the experiment as being a workweek. Each of the four employees spends 40 hours per week at their firm. Your task will be to decide how to allocate your time between two activities: Activity X and Activity Y. Specifically, you will be asked to choose how much time to devote to Activity X. The available choices are 0 hours, 10 hours, 20 hours, 30 hours, or 40 hours. The remaining hours will be put towards Activity Y. For example, if you devote 30 hours to Activity X, this means that 10 hours will be put towards Activity Y.

Employee payoffs: An employee's payoffs will be determined by the number of hours that employee spends on Activity X, the minimum number of hours that employees in his or her firm spend on Activity X, the base salary, a bonus that depends on the minimum number of hours spent on Activity X by any of the members of your group, and the cost of effort. These payoffs are summarized by the formula below:

Employee Earnings = Base Salary − (Cost*Your hours on Activity X) + (Bonus*Minimum hours spent by other employees on Activity X).

The values of the Base Salary, Bonus, and Cost will not be the same in each round. Specifically, they will change randomly and with equal probability. The Base Salary will take values between 195 and 205: 195, 196, 197, ..., 205. The Bonus will take values between 9.5 and 10.5: 9.5, 9.6, 9.7, ..., 10.5; the cost will take the values of 4.8, 4.9, or 5. In addition, on your screen, you will always have information about the payoffs for each possible decision in each round. If you don't understand the above formula, don't worry. It is provided to you as an additional way to understand your payoffs. The computer always shows your payoff table at any point where you need to make a decision. The tables include all of the information you need to make a decision.

Playing a Round as a Firm Employee: For each round of the experiment, the computer will display a table like the one shown below. Your earnings in each round can be found by looking at the hours you can choose to dedicate to Activity X (on the left side of the table) and the columns that indicate the minimum number of hours spent on Activity X by the other members of the group. This table will be the same for each member of the group. For the payoff table shown below, the Base Salary is equal to 100, the Bonus is equal to 10, and the Cost is equal to 5. Remember that when the game begins, the numbers that appear in the table may be different, as explained above.

Example:

Employee Earnings = 100 − (5∗Your hours on Activity X) + (10*Minimum hours spent by other employees on Activity X)

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours on Activity X	**0**	100	100	100	100	100
	10	50	150	150	150	150
	20	0	100	200	200	200
	30	-50	50	150	250	250
	40	-100	0	100	200	300

Note that some of the payoffs in the table have a negative value. These values will cause you to lose ECUs. Throughout the experiment, you will be able to lose, at most, an amount equal to 3,000 ECUs, which is the amount you receive before starting the 30 decision rounds.

To choose the number of hours to spend on Activity X, use the buttons at the bottom of the screen. When you have made your final decision, click on the button labeled *Continue*. You may change your decision as often as you would like, but once you click *Continue,* your choice is final. Note that when you make your decision, you will not know what the other members of your group are selecting, nor will they know what you select.

Feedback as an employee: At the end of each round, you will receive a summary of what happened in that round including the number of hours that you spent on Activity X, the minimum number of hours spent by the other employees on Activity X, your earnings in that round, and your accumulated earnings through the current round.

Confidentiality and Payment: At the end of the experiment, you will be paid, in cash, the sum of the payoffs that you have earned in the rounds of the experiment along with the five-euro show-up fee. As noted previously, you will be paid privately, and we will not disclose any information about your actions or your payoff to the other participants in the experiment.

<u>EXAMPLES AND QUESTIONS</u>

For the following questions, use the information in the example table below. Please raise your hand if you are having trouble answering one of the questions.

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours Spent on Activity X	**0**	99	99	99	99	99
	10	50	149	149	149	149
	20	1	100	199	199	199
	30	-48	51	150	249	249
	40	-97	2	101	200	299

[*]-Suppose that you choose to dedicate 10 hours to Activity X. The other employees in your firm decide to dedicate 30, 20, and 10 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

[*]-Now suppose that you choose to dedicate 30 hours to Activity X. The other employees in your firm decide to dedicate 30, 20, and 40 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

- The same four people are in my firm for all 30 rounds of the experiment. □ True □ False

- The numbers in the payoff table are the same for all employees, they can change from round to round, and they have been obtained randomly. □ True □ False

A.2 Instructions for HF-Social

[**Remark:** In the following, we present the instructions for treatment HF-Social. Paragraphs with **[*]** were given in both social treatments, but appropriately reformulated for LF-Social. A complete set of instructions is available from the authors.]

INSTRUCTIONS

The purpose of this experiment is to study how individuals make decisions in certain contexts. In addition to the five-euro participation fee, you will be paid any additional money you accumulate at the end of today´s session. All payoffs during the experiment are denominated in an artificial currency, experimental currency units (ECU). At the end of the experiment, ECUs will be converted at a rate of one euro per 500 ECUs. Upon completion of the experiment, your earnings will be converted to euros, and you will be paid privately in cash. The exact amount you receive will be determined during the experiment and will depend on your decisions and the decisions of others. If you have any questions during the experiment, please raise your hand and wait for an experimenter to come to you. Please do not talk, exclaim, or try to communicate with other participants during the experiment. Participants intentionally violating the rules may be asked to leave the experiment and may not be paid.

[*] Decision rounds: At the beginning of the experiment, you will receive 6,000 ECUs. The experiment consists of 30 rounds. In each round, you will be in a group with four other participants. The composition of these groups, which are called *firms,* will not vary during the experiment. At the beginning of the experiment, each employee will be randomly assigned one of two roles within a firm: non-restricted worker or restricted worker. In each firm, there will be four non-restricted workers and one restricted worker. Given that nobody will know the identity of the members of each group, all the actions you take during the experiment will be anonymous.

Task: You and the other members of your group are employees of a firm. You can think of a round of the experiment as being a workweek. Each of the four employees spends 40 hours per week at their firm. Your task will be to decide how to allocate your time between two activities: Activity X and Activity Y. Specifically, you will be asked to choose how much time to devote to Activity X. The available choices are 0 hours, 10 hours, 20 hours, 30 hours, or 40 hours. The remaining hours will be put towards Activity Y. For example, if you devote 30 hours to Activity X, this means that 10 hours will be put towards Activity Y.

Employee payoffs: An employee's payoffs will be determined by the number of hours that employee spends on Activity X, the minimum number of hours employees in his or her firm spend on Activity X, the base salary, a bonus that depends on the minimum number of hours spent on Activity X by any of the members of your group, and the cost of effort. These payoffs are summarized by the formula below:

Employee Earnings = Base Salary − (Cost*Your hours on Activity X) + (Bonus*Minimum hours spent by other employees on Activity X).

The values of the Base Salary, Bonus, and Cost will not be the same in each round. Specifically, they will change randomly and with equal probability. The Base Salary will take values between 195 and 205: 195, 196, 197, ..., 205. The Bonus will take values between 9.5 and 10.5: 9.5, 9.6, 9.7, ..., 10.5; the cost will take the values of 4.8, 4.9, or 5. In addition, on your screen, you will always have information about the payoffs for each possible decision in each round. If you don't understand the above formula, don't worry. It is provided to you as an additional way to understand your payoffs. The computer always shows your payoff table at any point where you need to make a decision. The tables include all of the information you need to make a decision.

Playing a Round as a Firm Employee: For each round of the experiment, the computer will display a table like the ones shown below. Your earnings in each round can be found by looking at the hours you can choose to dedicate to Activity X (on the left of the table) and the columns that indicate the minimum number of hours spent in Activity X by the other members of the group. This table will be the same for each member of the group. For the payoff table shown below, the Bonus is equal to 10, and the Cost is equal to 5. Remember that when the game begins, the numbers that appear in the table may be different, as explained above.

Example:

Earnings = (10*Minimum hours spent by other employees on Activity X) − (5*Your hours on Activity X)

Non-restricted worker

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours on Activity X	**0**	0	0	0	0	0
	10	-50	50	50	50	50
	20	-100	0	100	100	100
	30	-150	-50	50	150	150
	40	-200	-100	0	100	200

Restricted worker

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours on Activity X	**30**	-150	-50	50	150	150
	40	-200	-100	0	100	200

Note that some of the payments in the table have a negative value. These values will cause you to lose ECUs. Throughout the experiment, you will be able to lose, at most, an amount equal to 6,000 ECUs, which is the amount you receive before starting the 30 decision rounds.

To choose the number of hours to spend on Activity X, use the buttons at the bottom of the screen. When you have made your final decision, click on the button labeled *Continue*. You may change your decision as often as you would like, but once you click *Continue,* your choice is final. Note that when you make your decision, you will not know what the other members of your group are selecting, nor will they know what you select.

Feedback as an employee: At the end of each round, you will receive a summary of what happened in that round including the number of hours that you spent on Activity X, the minimum number of hours spent by the other employees on Activity X, your earnings in that round, and your accumulated earnings through the current round.

Confidentiality and Payment: At the end of the experiment, you will be paid, in cash, the sum of the payoffs that you have earned in the rounds of the experiment along with the five-euro show-up fee. As noted previously, you will be paid privately, and we will not disclose any information about your actions or your payoff to the other participants in the experiment.

EXAMPLES AND QUESTIONS

For the following questions, use the information in the example tables provided below. Please raise your hand if you are having trouble answering one of the questions.

Imagine that you are a non-restricted worker:

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours on Activity X	**0**	0	0	0	0	0
	10	-49	50	50	50	50
	20	-98	1	100	100	100
	30	-147	-48	51	150	150
	40	-196	-97	2	101	200

[*] -Suppose that you choose to dedicate 10 hours to Activity X. The other employees in your firm decide to dedicate 30, 20, and 10 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

[*] -Now suppose that you choose to dedicate 30 hours to Activity X. The other employees in your firm decide to dedicate 30, 20, and 40 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

Imagine that you are a restricted worker:

		Minimum Hours Spent by Other Employees on Activity X				
		0	**10**	**20**	**30**	**40**
My Hours on Activity X	**30**	-147	-48	51	150	150
	40	-196	-97	2	101	200

[*] -Suppose that you choose to dedicate 30 hours to Activity X. The other employees in your firm have chosen to dedicate 30, 40, 0, and 20 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

[*] -Now suppose that you choose to dedicate 40 hours to Activity X. The other employees in your firm choose to dedicate 40, 30, 20, and 40 hours to Activity X.

The minimum number of hours that an employee dedicates to Activity X is _______.
My profit in ECUs is ________.

[*] -In each firm, there will be four non-restricted workers and one restricted worker. □ True □ False
[*] -The same five people are in my firm for all 30 rounds of the experiment. □ True □ False
-Worker types are randomly assigned by the computer. □ True □ False

Appendix B: Supplementary Analysis

The data presented for the gain frame treatments were collected in an earlier set of experiments and the findings were reported in Ellis and Sola (2020). For ease of comparison, we refer to the baseline treatments as gain frame (GF) and social gain frame (GF-Social). Gain and loss frames are structurally equivalent in terms of payoffs.

Table B1.1: Random effects regression on framing effects

VARIABLES	(1) Minimum Effort	(2) Effort
Unit of observation:	Firm	Employee
LF	-.435	-.473
	(.284)	(.559)
HF	.629***	.543
	(.231)	(.453)
Constant	.179	2.77
	(.212)	(.354)
Observations	3,480	870
Number of Groups	116	29

Notes: GF serves as the baseline treatment for comparison. Since basic treatment tests rely on individual observations, clustering was done at the subject level for minimum effort and group level for effort. Standard errors are corrected for clustering at the level of observation. Three (***), two (**), and one (*) star indicate statistical significance at the 1%, 5%, and 10%, respectively. Standard errors are denoted in parentheses.

Table B1.2: Random effects regression on social framing effects

VARIABLES	(1) Minimum Effort	(2) Effort
Unit of observation:	Firm	Employee
LF-Social	-.767***	-.635
	(.276)	(.501)
HF-Social	.427**	.345
	(.209)	(.379)
Constant	2.56	3.14
	(.128)	(.233)
Observations	5,100	1,020
Number of Groups	170	34

Notes: GF-Social serves as the baseline treatment for comparison. Since basic treatment tests rely on individual observations, clustering was done at the subject level for minimum effort and group level for effort. Standard errors are corrected for clustering at the level of observation. Three (***), two (**), and one (*) star indicate statistical significance at the 1%, 5%, and 10%, respectively. Standard errors are denoted in parentheses.

Appendix C: z-Tree Screenshots

C.1 Screenshots Low-stake Treatments

Period 1 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	105	105	105	105	105
10	55	157	157	157	157
20	5	107	209	209	209
30	-45	57	159	261	261
40	-95	7	109	211	313

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period 2 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	99	99	99	99	99
10	50	149	149	149	149
20	1	100	199	199	199
30	-48	51	150	249	249
40	-97	2	101	200	299

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period 3 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	97	97	97	97	97
10	48	145	145	145	145
20	-1	96	193	193	193
30	-50	47	144	241	241
40	-99	-2	95	192	289

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period 4 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	103	103	103	103	103
10	53	158	158	158	158
20	3	108	213	213	213
30	-47	58	163	268	268
40	-97	8	113	218	323

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period 5 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	103	103	103	103	103
10	53	156	156	156	156
20	3	106	209	209	209
30	-47	56	159	262	262
40	-97	6	109	212	315

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period 6 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	104	104	104	104	104
10	54	151	151	151	151
20	4	101	198	198	198
30	-46	51	148	245	245
40	-96	1	98	195	292

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period 7 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	101	101	101	101	101
10	51	154	154	154	154
20	1	104	207	207	207
30	-49	54	157	260	260
40	-99	4	107	210	313

Which option do you choose: 0 / 10 / 20 / 30 / 40

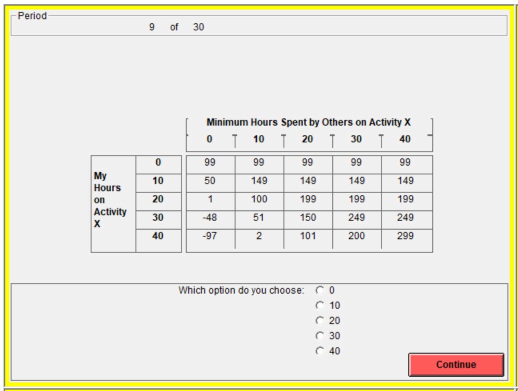
Continue

Period 8 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	103	103	103	103	103
10	53	156	156	156	156
20	3	106	209	209	209
30	-47	56	159	262	262
40	-97	6	109	212	315

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

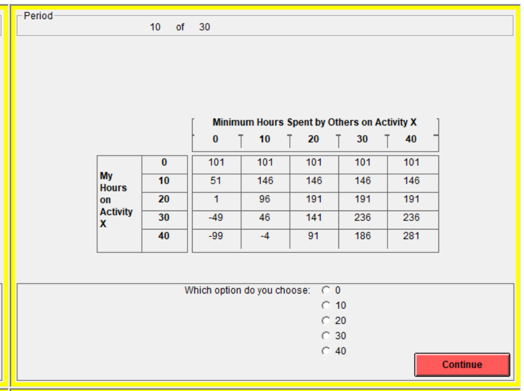


Period
9 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	99	99	99	99	99
10	50	149	149	149	149
20	1	100	199	199	199
30	-48	51	150	249	249
40	-97	2	101	200	299

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

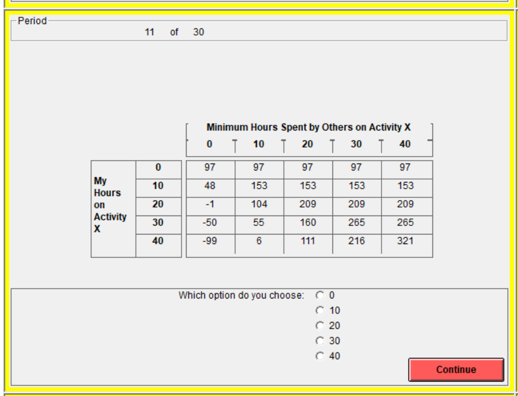


Period
10 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	101	101	101	101	101
10	51	146	146	146	146
20	1	96	191	191	191
30	-49	46	141	236	236
40	-99	-4	91	186	281

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

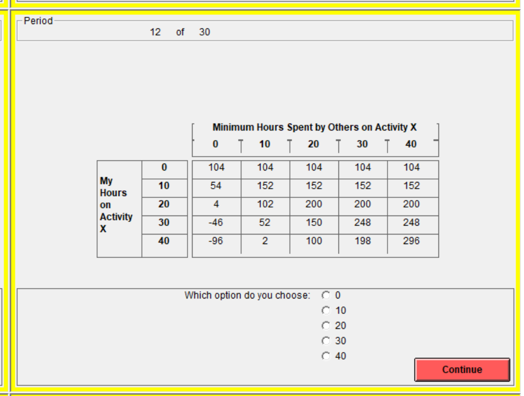


Period
11 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	97	97	97	97	97
10	48	153	153	153	153
20	-1	104	209	209	209
30	-50	55	160	265	265
40	-99	6	111	216	321

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

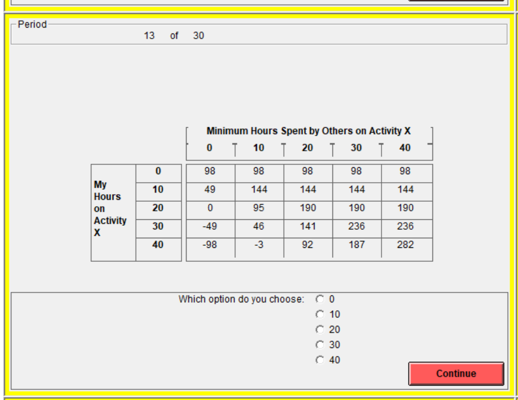


Period
12 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	104	104	104	104	104
10	54	152	152	152	152
20	4	102	200	200	200
30	-46	52	150	248	248
40	-96	2	100	198	296

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

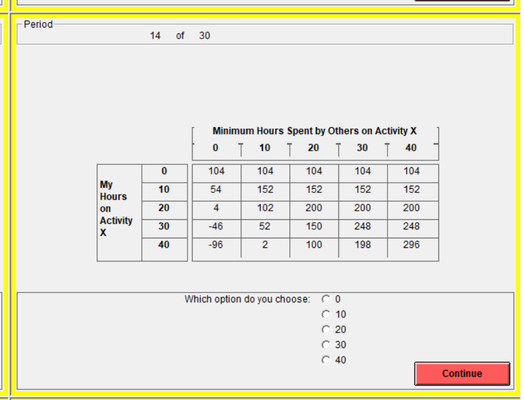


Period
13 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	98	98	98	98	98
10	49	144	144	144	144
20	0	95	190	190	190
30	-49	46	141	236	236
40	-98	-3	92	187	282

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

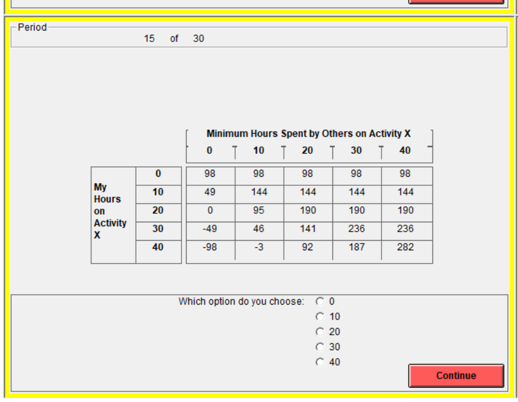


Period
14 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	104	104	104	104	104
10	54	152	152	152	152
20	4	102	200	200	200
30	-46	52	150	248	248
40	-96	2	100	198	296

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

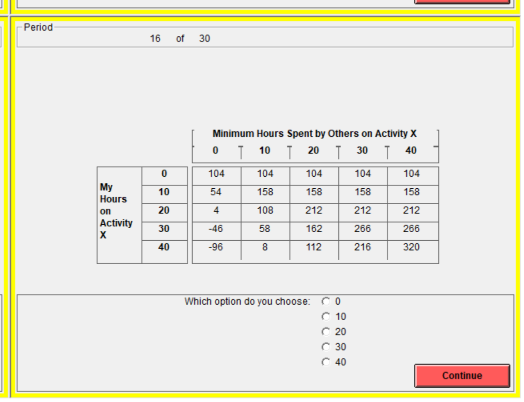


Period
15 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	98	98	98	98	98
10	49	144	144	144	144
20	0	95	190	190	190
30	-49	46	141	236	236
40	-98	-3	92	187	282

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue



Period
16 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	104	104	104	104	104
10	54	158	158	158	158
20	4	108	212	212	212
30	-46	58	162	266	266
40	-96	8	112	216	320

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period

17 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	95	95	95	95	95
	10	47	150	150	150	150
	20	-1	102	205	205	205
	30	-49	54	157	260	260
	40	-97	6	109	212	315

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period

18 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	102	102	102	102	102
	10	52	148	148	148	148
	20	2	98	194	194	194
	30	-48	48	144	240	240
	40	-98	-2	94	190	286

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period

19 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	99	99	99	99	99
	10	50	152	152	152	152
	20	1	103	205	205	205
	30	-48	54	156	258	258
	40	-97	5	107	209	311

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period

20 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	104	104	104	104	104
	10	54	155	155	155	155
	20	4	105	206	206	206
	30	-46	55	156	257	257
	40	-96	5	106	207	308

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period

21 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	104	104	104	104	104
	10	54	150	150	150	150
	20	4	100	196	196	196
	30	-46	50	146	242	242
	40	-96	0	96	192	288

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period

22 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	100	100	100	100	100
	10	50	154	154	154	154
	20	0	104	208	208	208
	30	-50	54	158	262	262
	40	-100	4	108	212	316

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period

23 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	99	99	99	99	99
	10	50	146	146	146	146
	20	1	97	193	193	193
	30	-48	48	144	240	240
	40	-97	-1	95	191	287

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period

24 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	96	96	96	96	96
	10	47	152	152	152	152
	20	-2	103	208	208	208
	30	-51	54	159	264	264
	40	-100	5	110	215	320

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
25 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	95	95	95	95	95
10	47	146	146	146	146
20	-1	98	197	197	197
30	-49	50	149	248	248
40	-97	2	101	200	299

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
26 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	98	98	98	98	98
10	49	148	148	148	148
20	0	99	198	198	198
30	-49	50	149	248	248
40	-98	1	100	199	298

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
27 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	99	99	99	99	99
10	50	152	152	152	152
20	1	103	205	205	205
30	-48	54	156	258	258
40	-97	5	107	209	311

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
28 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	95	95	95	95	95
10	47	150	150	150	150
20	-1	102	205	205	205
30	-49	54	157	260	260
40	-97	6	109	212	315

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
29 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	96	96	96	96	96
10	47	145	145	145	145
20	-2	96	194	194	194
30	-51	47	145	243	243
40	-100	-2	96	194	292

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

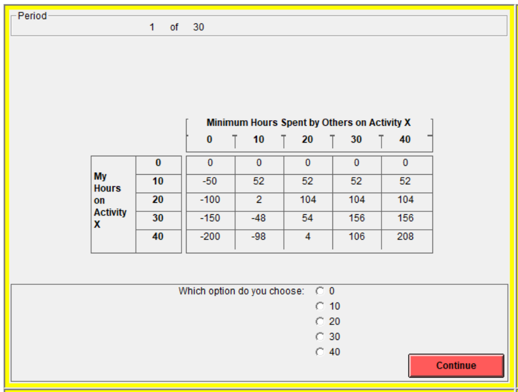
Period
30 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	97	97	97	97	97
10	48	150	150	150	150
20	-1	101	203	203	203
30	-50	52	154	256	256
40	-99	3	105	207	309

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

C.2 Screenshots High-stake Treatments

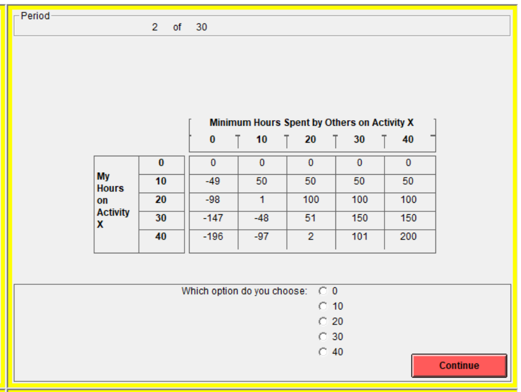


Period
1 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-50	52	52	52	52
20	-100	2	104	104	104
30	-150	-48	54	156	156
40	-200	-98	4	106	208

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

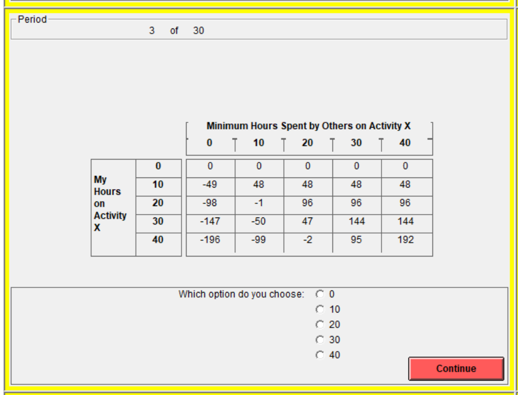


Period
2 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-49	50	50	50	50
20	-98	1	100	100	100
30	-147	-48	51	150	150
40	-196	-97	2	101	200

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

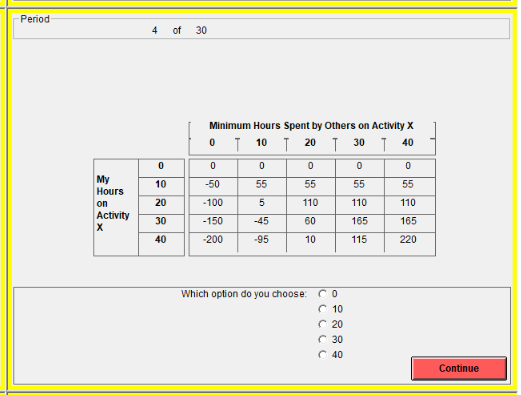


Period
3 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-49	48	48	48	48
20	-98	-1	96	96	96
30	-147	-50	47	144	144
40	-196	-99	-2	95	192

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

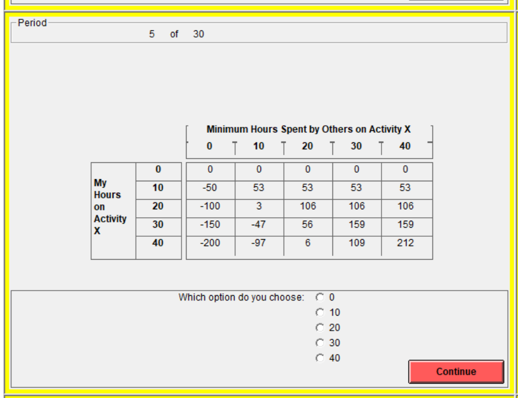


Period
4 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-50	55	55	55	55
20	-100	5	110	110	110
30	-150	-45	60	165	165
40	-200	-95	10	115	220

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

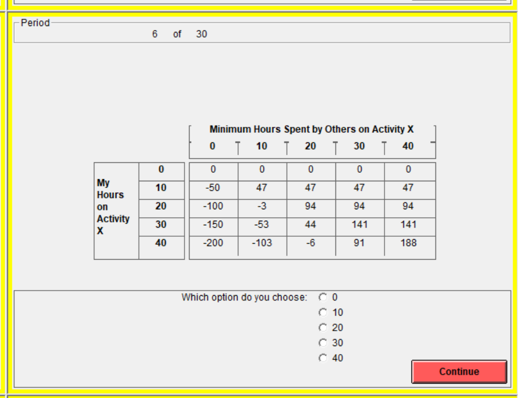


Period
5 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-50	53	53	53	53
20	-100	3	106	106	106
30	-150	-47	56	159	159
40	-200	-97	6	109	212

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

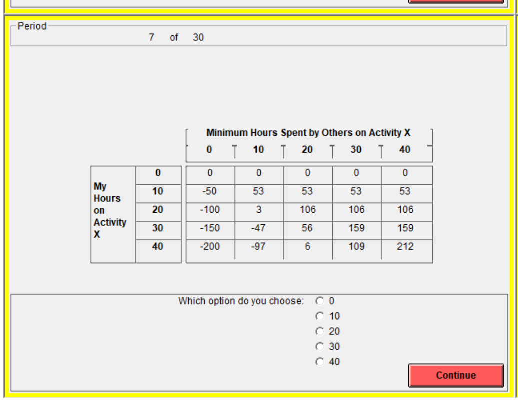


Period
6 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-50	47	47	47	47
20	-100	-3	94	94	94
30	-150	-53	44	141	141
40	-200	-103	-6	91	188

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

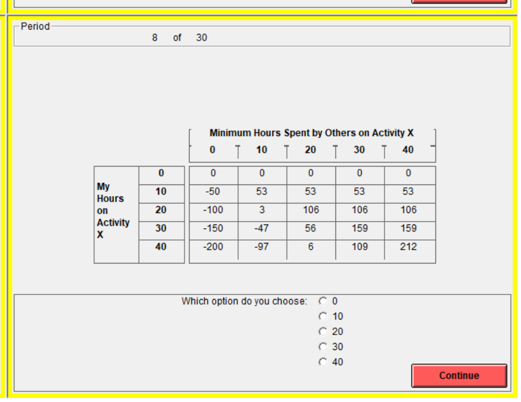


Period
7 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-50	53	53	53	53
20	-100	3	106	106	106
30	-150	-47	56	159	159
40	-200	-97	6	109	212

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue



Period
8 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-50	53	53	53	53
20	-100	3	106	106	106
30	-150	-47	56	159	159
40	-200	-97	6	109	212

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period

9 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	0	0	0	0	0
	10	-49	50	50	50	50
	20	-98	1	100	100	100
	30	-147	-48	51	150	150
	40	-196	-97	2	101	200

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period

10 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	0	0	0	0	0
	10	-50	45	45	45	45
	20	-100	-5	90	90	90
	30	-150	-55	40	135	135
	40	-200	-105	-10	85	180

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period

11 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	0	0	0	0	0
	10	-49	56	56	56	56
	20	-98	7	112	112	112
	30	-147	-42	63	168	168
	40	-196	-91	14	119	224

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period

12 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	0	0	0	0	0
	10	-50	48	48	48	48
	20	-100	-2	96	96	96
	30	-150	-52	46	144	144
	40	-200	-102	-4	94	192

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period

13 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	0	0	0	0	0
	10	-49	46	46	46	46
	20	-98	-3	92	92	92
	30	-147	-52	43	138	138
	40	-196	-101	-6	89	184

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period

14 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	0	0	0	0	0
	10	-50	48	48	48	48
	20	-100	-2	96	96	96
	30	-150	-52	46	144	144
	40	-200	-102	-4	94	192

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period

15 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	0	0	0	0	0
	10	-49	46	46	46	46
	20	-98	-3	92	92	92
	30	-147	-52	43	138	138
	40	-196	-101	-6	89	184

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period

16 of 30

My Hours on Activity X		Minimum Hours Spent by Others on Activity X				
		0	10	20	30	40
	0	0	0	0	0	0
	10	-50	54	54	54	54
	20	-100	4	108	108	108
	30	-150	-46	58	162	162
	40	-200	-96	8	112	216

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period
17 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-48	55	55	55	55
20	-96	7	110	110	110
30	-144	-41	62	165	165
40	-192	-89	14	117	220

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
18 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-50	46	46	46	46
20	-100	-4	92	92	92
30	-150	-54	42	138	138
40	-200	-104	-8	88	184

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
19 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-49	53	53	53	53
20	-98	4	106	106	106
30	-147	-45	57	159	159
40	-196	-94	8	110	212

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
20 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-50	51	51	51	51
20	-100	1	102	102	102
30	-150	-49	52	153	153
40	-200	-99	2	103	204

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
21 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-50	46	46	46	46
20	-100	-4	92	92	92
30	-150	-54	42	138	138
40	-200	-104	-8	88	184

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
22 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-50	54	54	54	54
20	-100	4	108	108	108
30	-150	-46	58	162	162
40	-200	-96	8	112	216

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
23 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-49	47	47	47	47
20	-98	-2	94	94	94
30	-147	-51	45	141	141
40	-196	-100	-4	92	188

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
24 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-49	56	56	56	56
20	-98	7	112	112	112
30	-147	-42	63	168	168
40	-196	-91	14	119	224

Which option do you choose: ○ 0 ○ 10 ○ 20 ○ 30 ○ 40

Continue

Period
25 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-48	51	51	51	51
20	-96	3	102	102	102
30	-144	-45	54	153	153
40	-192	-93	6	105	204

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period
26 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-49	50	50	50	50
20	-98	1	100	100	100
30	-147	-48	51	150	150
40	-196	-97	2	101	200

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period
27 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-49	53	53	53	53
20	-98	4	106	106	106
30	-147	-45	57	159	159
40	-196	-94	8	110	212

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period
28 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-48	55	55	55	55
20	-96	7	110	110	110
30	-144	-41	62	165	165
40	-192	-89	14	117	220

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period
29 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-49	49	49	49	49
20	-98	0	98	98	98
30	-147	-49	49	147	147
40	-196	-98	0	98	196

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Period
30 of 30

My Hours on Activity X	Minimum Hours Spent by Others on Activity X: 0	10	20	30	40
0	0	0	0	0	0
10	-49	53	53	53	53
20	-98	4	106	106	106
30	-147	-45	57	159	159
40	-196	-94	8	110	212

Which option do you choose: 0 / 10 / 20 / 30 / 40

Continue

Conclusion

Organizations with weak-link technologies are prone to inefficient productivity traps. Such "coordination failure" can be like a trap for organizations; change is difficult to achieve once firms get stuck in sub-optimal equilibria. Weak-link coordination games abstractly model coordination problems in organizations and other social contexts. While coordination problems have been widely analyzed, current studies offer little on the cognitive and behavioral determinants of coordination failure. This dissertation contributes to this body of literature by investigating the cognitive and behavioral elements of coordination outcomes when firms are characterized by volatility and uncertainty.

In the first experiment, we found that stringent decision times lead to lower minimum effort contributions; however, providing higher feedback time improves firm minimum effort levels. Stringent decision constraints reduce subjects´ decision quality. Feedback time impacts coordination outcomes more when decisions are made under time pressure. Limiting subjects' ability to learn from each round and update their beliefs accordingly caused more volatile coordination outcomes resulting in coordination failure in which groups were not able to converge at any equilibria. Increased feedback time seems to improve decision-making in terms of anticipating the behaviors of others and aligning decisions with firm outcomes. This finding suggests that if decisions are made under time duress, adequate recovery time can mean the difference between efficient and inefficient coordination.

The first chapter contributes experimental results to the literature on economic games with time constraints. Time pressure is a central aspect of economic decision making these days. Research on coordination in high-risk industries (HRIs) is essential since errors caused by poor coordination could result in severe consequences that harm or even kill people. This has been evident during the COVID-19 pandemic. The results have implications in practical contexts and can help set the stage for further research that recognizes job-specific adverse conditions in weak-link firms. Our findings have applications in the management literature, particularly in the organizations we mentioned earlier, such as HROs, task forces, and action groups. Firms that operate in time-constrained environments do not have the option to change the rapid tempo of operations. However, firms can improve decision-making and overall output by allowing employees adequate time to recover after high-stress periods.

In the second experiment, we studied the effect of heterogeneous groups on the efficiency of coordination in repeated weak-link coordination games. We aimed to facilitate efficient coordination among large groups by capturing social spillover effects associated with task heterogeneities. We tested the hypothesis that social spillover effects produce firms with higher minima than firms in control treatments. Theoretically, increasing the group size increases the strategic uncertainty of the game and worsens the coordination problem. We examined the impact of social spillovers experimentally and found that it boosts efficiency in groups with five as well as eight subjects. Whereas most sources of strategic uncertainty lower coordination output, this source was efficiency-enhancing. We found that heterogeneous groups induced change to higher, more efficient equilibria. Despite increases in group size, firms with restricted workers reported higher overall output. Several firms managed to coordinate at much higher levels, with two firms reaching the most efficient level of coordination. Regarding worker behavior, we found evidence of peer effects in effort levels. Employee average effort choices produced positive spillover effects in both experimental conditions. Employees exerted more effort when there is another employee(s) who can be harmed in the firm.

This work contributes to the coordination literature by providing a mechanism to alleviate coordination failure among large groups, demonstrating that social spillovers overpower group-size effects. Our findings complement previous studies that have achieved successful coordination in large groups. In our work, task heterogeneities induce change to higher, more efficient equilibria. Thus, we provide a new account, supported by empirical evidence, of how to facilitate efficient coordination among large groups by capturing social spillover effects associated with task heterogeneities. These results apply to the management literature on modern-day organizational arrangements. The issue for organizations is not whether they have homogenous or heterogeneous workers, but instead how to align workers. The optimal mix will transpire when the social preferences of workers are maximized. From our results, it appears that firms should favor a setting in which the behavior of the most productive workers can be observed.

In the final experiment, we tested whether participants exerted considerably more effort when payoff-equivalent incentives were framed as losses rather than gains. We also intersected loss aversion and social preferences to model real-world weak-link situations. A central behavioral prediction of loss aversion is that because individuals are more sensitive to losses, they will work harder to avoid a loss than to experience an equivalent gain. We found this to be valid only in high stakes conditions. Loss aversion was efficiency-enhancing only in high-stake conditions. Individuals in the high-stake treatments contributed significantly higher effort than subjects in low stake frames. Coordination was markedly higher in high stake conditions compared to low stake conditions. Social concerns were influenced by the degree of loss framing. Average effort was higher in social treatments than the corresponding four-employee treatments, but average minimum effort decreased. A closer investigation of firm dynamics and employee-level behavior revealed that peer effects in high stakes produce small positive spillovers, but overall loss framing reduced social spillover effects.

This experimental study contributes to the coordination literature on loss framing effects with different stakes and coordination outcomes when loss aversion intersects with social preferences. Our results also inform theory as the interplay between loss aversion and social preferences has significant implications. These results are applicable to the management literature on modern-day organizational arrangements, particularly relevant to HROs. When the stakes are high, the saliency of consequences on the type-2 worker is not as important. Merely having high stakes with homogenous worker groups is efficiency-enhancing. This finding is remarkable, considering the prevalence of large organizations that face coordination demands in high-risk industries. The COVID-19 pandemic has demonstrated the importance of understanding behavior in loss domains. These findings have applications for practice in areas such as how to frame contracts. Due to the contingent nature of loss aversion, our work suggests that organizations should practice caution if relying on loss frames as a performance-enhancing device.

Supporting Documentation

1. Ethical Compliance

All research involving human subjects in this thesis was approved by Committee of Ethics in Animal and Human Experimentation (CEEAH) of the Autonomous University of Barcelona under the Protocol # 3637, Project "Behavior in the Presence of Heterogeneities in Games/Coordination with Asymmetries in Highly Volatile and Complex Environments." The Air Force Human Research Protection Official (HRPO) concurred with the Institutional Review Board (IRB) determination for Air Force Protocol Number: FWR20170122Xe, Protocol Title: Behavior in the Presence of Heterogeneities in Games / Coordination with Asymmetries in Highly Volatile and Complex Environment.

2. Acknowledgements

This work was supported by a grant from the Air Force Office of Scientific Research, Award No. FA9550-18-1-002, Coordination with asymmetries in highly complex and volatile environments.

Appendix A: Ethical Protocol

1. Title of Research Procedure

Behavior in the Presence of Heterogeneities in Games / Coordination with asymmetries in highly volatile and complex environments

2. Brief Description of Project

This research project investigates how certain asymmetries subjects face in economic processes affect the ability to coordinate efficiently. We propose an analysis of collective trust in environments characterized by high volatility and strategic complexity. Though trust has been examined from different points of view, we highlight the collective nature of trust in coordination games, specifically the weakest-link game. In this setting, the standard prediction of multiplicity of equilibria typically results in poor, inefficient coordination outcomes. Hence, our research will show how, in this difficult environment, collective trust may emerge through the role of heuristics in the presence of volatility and complexity. We will induce these two characteristics by adding obstacles to coordination: our variables will imply that subjects face heterogeneities in decision time, risk, strategic complexity, and cognitive load. Our research will then enrich the analysis of strategic interactions in what we believe are more realistic contexts, thus allowing us to design organizational factors to help overcome coordination failure in these situations. This project includes three experiments: Experiment One: Time Constraints; Experiment Two: Strategic Complexity; Experiment Three: Loss Aversion. Each experiment will have different treatments, in which we test the dependent variables against control groups. However, all participants in all of the treatments have the ability to earn the same amount of money and will spend the same amount of time, no matter which treatment or role they are randomly assigned to. Each of these experiments will be described in detail below, as well as the recruiting process.

We develop this approach by using the experimental (economic) technique in a series of weak-link games. In the weak-link coordination game, players are asked to allocate their private resources (working time) between Activity A and Activity B. Participants begin with a fixed rate wage and a bonus rate, B, dependent upon the group contribution. The effort level, Ei, is presented in increments of 10.

With this base design, we add ¨volatility¨ using the term in an economic analysis sense. By this, we are referring to the degree of variation measured by the standard deviation. The volatility of the experiment is generated by small variations in the payoffs' table. Specifically, the parameters of the main decision table will have a small perturbation in each round. That is, for each payoff x in the table we generate a random number per round x_t generated by a uniform distribution such that $x_t \in [x-5, x+5]$. Hence, from the initial table, subjects will have to pay attention to the numbers in each round; the objective is to avoid subjects selecting an option by an automatic response. This change in the numbers does not alter the equilibria of the game and hence the strategic decision of the subjects. Examples of the payoff tables are illustrated below:

Payoff tables based on the model from Brandts and Cooper (2006):

$$\pi_e^i = 200 - 5E_i + \left(B \times \min_{j \in \{1,2,3,4\}}(E_j)\right)$$

B = 10

	0	10	20	30	40
0	200	200	200	200	200
10	150	250	250	250	250
15	125	225	275	275	275
30	50	150	250	350	350
40	0	100	200	300	400

B = 9.5

	0	10	20	30	40
0	200	200	200	200	200
10	150	245	245	245	245
20	100	195	290	385	290
30	50	145	240	335	335
40	0	95	190	285	380

Additionally, we create an environment prone to cognitive biases so that we can observe heuristics used in strategic situations and analyze whether these aid in judgments that lead to more efficient outcomes, or serve as biases that negatively impact decision-making. Creating an ¨environment prone to cognitive biases¨ pertains to the structure of the situation in which subjects are expected to apply heuristics in their choice rather than a rational decision process. The current literature on heuristics and biases is inconclusive, and it is unknown whether people use these rules of thumb to make better decisions or whether they bias our judgment and hinder the decision-making process.

We explore the game in various contexts, with the manipulation of three independent variables: time constraints, strategic complexities, and loss aversion. Therefore, we have separated them below with descriptions of each. Subjects are randomly assigned to all of these roles, and the amount of money earned in the experiment is dependent upon the decisions made by the entire group. The earning potential is precisely calculated and converted to an experimental exchange rate in each experiment, to ensure all participants have the earning potential deemed appropriate by the economic experiment protocol, and according to the region. This information will be known to subjects prior to starting the experiment.

EXPERIMENT 1: Subjects will have a limited time to make their decisions. This is between 2 and 30 seconds. Systematic changes in these variables will lead to several treatments. We chose these distinct numbers based on standards in the current literature on treatments with time pressure and reflection. Additionally, we have ran pilot sessions to test different time allotments and found a difference in subjects´ attitudes and behavior at the two-second time mark, thus confirming our design is in line with the cognitive load treatments in other experiments.

In every treatment we will announce at the beginning the specific characteristics of the treatment that subjects will face. We have made the layout of the screen as simple and clear for participants to view and make their decision; there is only one button that they must click. They must read all instructions, and pass the pre-quiz ensuring they understand the rules of the game. If they do not, we will review the instructions again until they fully understand. They are also given a practice round in the beginning to familiarize themselves with the layout since certain subjects will be asked to make their selection within two seconds.

EXPERIMENT 2: We plan to address in this part of the research the effects of changing strategic complexity: some subjects may choose between two options of input, rather than five options. In some other treatments the number of choices may increase. In every treatment we will announce the specific characteristics of the treatment that subjects will face. Below is an example.

	0	**10**	**20**	**30**	**40**
0	200	200	200	200	200
40	0	100	200	300	400

The payoff structure is the same, but we have eliminated three of the options. The earning potential is the same for all participants and is dependent upon all the choices of the group. We explain this in the consent form so that students do not feel the earning potential is unfair.

EXPERIMENT 3: In this case we study decision-making with ¨loss aversion¨, as Prospect Theory shows that people make different decisions when presented as a gain or loss. This implies losses; however, we do NOT refer to economic loss. Subjects do NOT go bankrupt nor will they ever be required to pay. They have the same earning potential as the other treatment; however, they have a higher lump sum or ¨base wage¨ in the start. In our context, they are small changes in the payoff table, but our experimental results can lend an understanding of decision-making, relating specifically to the theoretical literature on problems in organizations that face extreme changes or large losses, such as the military. An example is below.

	0	**10**	**20**	**30**	**40**
0	0	0	0	0	0
10	-50	50	50	50	50
15	-75	25	75	75	75
30	-150	-50	50	150	150
40	-200	-100	0	100	200

3. Principal Investigator
Name: Carles Sola Belda
NIF: 35096308X
Department/center: Department of Business
Telephone: (+34) 935811209
E-mail: Carles.Sola.Belda@uab.cat

The project team includes: Carles Sola Belda, Jordi Brandts, and Jessica Ellis. (CVs previously submitted).

4. Objectives of Human Experimentation Procedures

Our objective is to characterize trust and the resulting economic collective outcomes in coordination games characterized by volatility and complexity. We examine trust as a heuristic decision-making mechanism and means of uncertainty reduction. Finally, we want to design organizational factors that will help to overcome coordination failure in these situations. The research questions we will ask:

1.) How do coordination levels differ with respect to standard coordination games when subjects are faced

with heterogeneities in risk and complexity, in addition to strategic uncertainty?
2.) What heuristics do we observe in coordination efforts? Do they bias outcome or serve as a mechanism of judgment to make accurate decisions?
3.) What do our results reveal about trusting behavior and the act of trusting in a collective setting?
4.) Do the independent variables impact the propensity to trust in the beginning stages of the game? Or the final outcome?

In the current literature on heuristic decision-making, it is not known in which instances heuristics can lead to errors: cognitive biases. The original literature proposed by Kahneman and Tversky in 1979 provided documented instances of intuitive decision-making, and these have been shown to appear when people have limitations on time to process information or are faced with additional sensory inputs. Laboratory experiments have shown negative and positive correlations in regard to whether intuitive decision-making leads to better results under time pressure or cognitive load.

We use laboratory experiments to study behavior in situations that are difficult to test in the field. Our main objective is to provide experimental data on behavior and decision-making in these difficult environments for organizations to use.

5. Methodology of Experimental Procedure

The experiment will be conducted in the UAB computer laboratory, which is a secure environment that allows subjects to interact anonymously through individual computers. The observations obtained in this project will be individual and group data that participants enter through this platform, created in the z-Tree software (Fischbacher 2007). We will follow standard economic experiments protocol as provided in the handbooks of economics experiments (Fréchette and Schotter 2015; Roth and Kagel 1997; Davis and Holt 1993). This experimental methodology will allow us to obtain individual decision data in a known environment (weak-link game) that represents a relevant economic process. This kind of research is well known in the economic science, leading to publications in top journals and several Nobel Laureate in Reinhard Selten (1994), 2002 (Daniel Kahneman and Vernon Smith), 2009 (E. Ostrom) and 2012 (Alvin Roth).

RECRUITMENT PROCESS: The experiment will be listed on a web-based automated recruitment system Online Recruitment System for Economic Experiments (ORSEE) (Greiner 2004), where those registered can sign up. In ORSEE, students provide their name and email address; however, this is only to ensure they have not participated before and used for check-in. This information is retained only in ORSEE and will not be associated with their decisions or answers in the survey. Only the ORSEE administrator can register the experiments, which has restricted access so that other experimenters are not given access. The description will include the general information of the experiment, compensation, date and time, and approximate duration. (ORSEE Advertisement attached). ORSEE is a software tool that allows researchers to schedule experiment sessions and recruit participants. This is used to standardize procedures and depersonalize experimenter-subject interaction.

Eligibility or screening: Subjects must be 18 years of age or older and a student at UAB, must not have participated in our economic experiments prior, which ORSEE screens. Additionally, they cannot be a student of the researchers, which is stated in the description and verified by the administrator. Eligible students will be sent an invitation to our experiment with the dates and times and specific link to sign up. If interested, subjects may register for the preferred day and time. (copy of email attached).

(Full documentation on ORSEE procedures can be found at: DOI 10.1007/s40881-015-0004-4)

From a subject perspective:

1) Sign up through the participant registration form, agreeing to comply with the rules and privacy policy of the institution. Sign up via ORSEE recruitment website for preferred date and time.
2) Sign in on the day of the experiment
3) Receive random computer assignment
4) Read instructions and opportunity to ask questions
5) Sign informed consent form
6) Begin first round of experiment
7) Opportunity to ask questions again at the end of the round
7) Continue additional rounds
8) Check out and receive payment with proctor

DURATION OF THE PROCEDURE: Each session will last approximately one hour and a half. This includes check-in, reading and signing the consent form, the instructions and pre-quiz, game, post-experiment survey, and payment.

Subjects are asked to make their choice over 30 rounds. After the game, subjects are asked to fill out a questionnaire. This is coded in z-Tree, which is a standard procedure because this also allows time for the experimenter to finalize the results of the round and payoffs for each computer number. The questionnaire contains the following:

I. Demographics: Gender, Age, Primary language spoken in household, Faculty enrollment
This will be relevant for future experimental designs if we observe patterns of behavior or decision strategies. We are particularly interested in exploring gender differences.

II. Attitude towards risk in economic context: *Ten Paired Lottery-Choice Decisions with Low Payoffs by Holt and Laury (2002),* choice between hypothetical simple alternatives with losses
This will aid in our evaluation of the strategy selection by players. Do their attitudes towards risk reflect their decisions?

III. Attitude towards losses in economic context: *Individual-Level Loss Aversion in Riskless and Risky Choices by Gachter, Johnson, and Hermann (2007),* choice between hypothetical riskless and risky choice of the price to sell a toy car
This is relevant to compare behavior among Experiments One and Two with Experiment Three which we introduce loss.

IV. *Evaluation of a behavioral measure of risk taking: the Balloon Analogue Risk Task (BART). Lejuez CW, Read JP, Kahler CW, Richards JB, Ramsey SE, Stuart GL, Strong DR, Brown RA (2002) Journal of Experimental Psychology: Applied, 8, 75-84.*
This is an additional measure of risk-taking behavior.

V. Beliefs on trust and the trustworthiness of others: *General Trust Scale by Yamagishi & Yamigishi (1994),* 6-item questionnaire
Lastly, this aids in our working hypothesis on the role of swift trust in coordination games. If we can better understand participants´ beliefs on trust, we can begin to classify decision strategies as a level of trust. This data will be associated to the decisions taken in the experiment to control for some of these factors in the statistical analysis if necessary. Hence, the data is anonymous. The details can be found in the attachments.

RISKS: This experiment has no foreseeable risks for the subjects as defined by the common rule of minimal risk: "ordinarily encountered in daily life or during the performance of routine physical or psychological tests". It is conceivable that some subjects could feel frustrated with the decisions of others, with the amount of time in experiment one, the number of options in experiment two, or with the potential to have money deducted from their initial lump sum based on the decisions of others. However, we provide complete instructions and a practice stage to ensure all subjects understand their role in the game. We also allow students to leave at any time, which is further described in the consent form. We believe that is the case also in numerous occupations and the material consequences of errors in our case are very small. If any unanticipated problems involving risks to subjects or others occur, we will promptly report to the CEEAH and Air Force Program Officer, Chris McClernon.

INCLUSION CRITERIA: Subjects must be 18 years of age or older and a student at UAB. This is required to sign up for the experiment, and then students may select the date and time. Subjects will be included on a first-come basis. They can sign up through the ORSEE recruitment website. They cannot be a student of the researchers or have participated prior.

RIGHT TO WITHDRAW: Subjects may leave the experiment at any time without penalty. This is stated on the consent form: ¨You may choose not to participate or, if you agree to participate, you can withdraw your participation at any time without penalty or loss of benefits to which you are otherwise entitled.¨ Non-participation will not affect students' grades or academic standing. If a subject is uncomfortable, he or she may leave at any time. Questions or concerns will be answered by the experiment proctor if needed.

EARLY WITHDRAWAL: Subjects will only be removed if the experiment cannot fit them in due to capacity of the lab, or should an unexpected event occur (broken computer etc...) Subjects who are removed early will simply be paid a five euro show up fee on the spot. No further contact is needed.

CONSENT: Consent will be at the beginning of the experiment after the subjects have read the instructions and signed the consent form. Participants will be given a copy of this document if requested. We have attached the consent forms for each experiment.

6. Information to Participants

Subjects will receive detailed instructions at the beginning of the experiment with an explanation concerning the fact that in different rounds the numbers in the table may be different. Per economic protocol, we do not deceive subjects and we ensure they understand what is being asked prior to starting the experiment.

7. Compensation

Subjects will be paid five euro for simply showing up. At the end of the experiment, subjects will be paid additional money based on their performance in the game. Average earnings are 20-25 euro per hour. This procedure, standard in economic experiments, is intended to obtain data based on non-hypothetical trade-offs.

There is no economic burden to the students except for the opportunity cost of spending 1-2 hours of their time in the experiment. Subjects will act anonymously and will understand the payoff potential beforehand, so they will not be misled.

8. Management and Storage of Data

ACCESS TO DATA: The ORSEE administrator who oversees the recruitment of each experiment is able to see the names and email addresses of subjects who sign up. He or she will manage the arrival of subjects and direct them to a computer. From there, the experimenter will run the sessions. Only the experimenter will have access to the anonymous data. Each participant has a strictly confidential "Experiment Code" to guarantee that no participant can identify another one by his/her decisions nor earnings. Researchers may observe each participant's earnings at the end of the experiment but we will not associate decisions with any participants' names.

CONFIDENTIALITY: No identifiers will be used in the data. The experiment will compensate students based on the computer they are randomly sitting at, which is also referred to as an experiment code. Therefore, their connection to their performance ends when they leave the room. The experimentalist will write the amount of money with the computer number and give to the other assigned researcher in the next room. When finished, students will be given their payment and receipt. After all students are paid, the same researcher will produce the final paperwork on the amount paid for each session. This report, along with the receipts, is immediately hand-delivered directly to the Economic Manager who ensures they are filed in a secure, locked area to ensure confidentiality. The Economic Manager for our department is:

Marta San Jose Raja
Email: marta.sanjose@uab.cat
Phone: + 34935811209

PRIVACY: The research team does not keep any identifiable data. The only data kept is the number of the computer, or experimental code, associated with the decision and answers to the questionnaires. As mentioned above, we do not keep any copies of the receipts with identifiable information.

STORAGE OF DATA: All research data is anonymous. We keep this data in a shared file with the password from the security system at UAB.

Appendix B: Participant Consent Form

Consent to Participate in a Research Study

Study Title	Behavior in the Presence of Heterogeneities in Games
Principal Investigator	Carles Sola, Business Department
Contact Information	Carles.sola.belda@uab.cat

Introduction: You are being asked to participate in a research study on decision-making. Your participation is voluntary. You must be actively enrolled at UAB, and you must be 18 years of age or older. You may not participate if you are a student of the Principal Investigator or if you have any academic or professional relation to the Principal Investigator or anyone involved in this research (assistants in this session).

Purpose: The purpose of this experiment is to study how individuals make decisions in certain contexts. This is an economics experiment; therefore, we are not doing anything deceptive. We want to make sure the instructions are as clear as possible. We ask that you read this form and ask any questions that you may have before agreeing to be in the study. You will also be given printed, detailed instructions, and may ask any questions at any time.

Procedure: You will be asked to play a game, described below, and complete a survey when you finish the game. Everyone will finish the game at the same time, and the surveys will be answered at your own pace. During this time, the experimental staff will calculate your earnings so that you will be ready to check out as soon as you finish. Altogether, this will take no longer than two hours.

Description: As a worker of your firm you have to decide how to split your working week of 40 working hours between two activities: Activity X and Activity Y. An example of a question you will find is: How many hours do you want to invest in the project? This will be played over several rounds, and in each round you are part of the same group of four participants. You will be randomly assigned a role within the group that determines whether you have two options or more than two options. We will specify in the instructions how many options you will have in this session. After decisions are made in each round, you will be given time to review the results of the decisions in your group. This information will not reveal the identity of anyone in the experiment. After the game, you will be asked to answer some questions.

Payment: In addition to the five euro participation fee, you will be paid any additional money you accumulate at the end of today´s session. The payoff is determined in each round by the bonus rate, how many hours that employee spends on Activity X, and the minimum number of hours employees in his or her firm spend on Activity X. At the end of the session, your earnings will be calculated, and you will receive cash and a receipt.

Right to Withdraw: You may choose not to participate or, if you agree to participate, you can withdraw your participation at any time without penalty or loss of benefits to which you are otherwise entitled.

Confidentiality: The actions you take during the experiment will be absolutely anonymous. The research team will not be collecting or retaining any information about your identity. The records of this study will be kept strictly confidential; this includes the receipts. This receipt includes the name, DNI, the amount paid, and your signature. The paper receipt you will receive at the end is stored and managed by the Economic Management of the Faculty of Economics and Business. The sponsor of this research, the US Department of Defense, may review research records to ensure protection of the research participants.

Risk: The risk determined by the committee has been deemed minimal. It is possible that you may feel frustrated with the decisions of others. However, we provide complete instructions and a practice stage to ensure all subjects understand their role in the game. You may feel frustrated with the number of choices available to you, but the earning potential is the same for all participants and is dependent upon all the choices of the group.

Right to ask Questions and Report Concerns: You may ask the experiment proctor any questions or express any concerns you may have. If you have any questions about your rights as a research participant, you may contact the Ethics Committee in Human and Animal Research (CEEAH) office at: ceeah@uab.cat. This is a committee that oversees research at the university.

Consent: Your signature below indicates that you have decided to volunteer as a research participant for this study, and that you have read and understood the information provided above. You may request a signed and dated copy of this form to keep.

Subject	*(print name)*	**Date**	
Signature			

Witness/Translator	*(print name)*	**Date**	
Signature			

Appendix C: Translation Certificate English Documents

Servei de Llengües

Universitat Autònoma de Barcelona

Edifici B4
08193 Bellaterra (Barcelona). Spain
Tel. 93 581 13 25
Fax 93 581 27 36
E-mail: traduccions@uab.cat

UALT

Unitat d'Assessorament Lingüístic i Traduccions
Documents de Recerca

CERTIFICATE VALIDATING THE LANGUAGE REVISION OF THE SUBMITTED ARTICLE

March 15, 2017

To whom it may concern:

The Language Advisory & Translation Unit of the *Servei de Llengües* at the *Universitat Autònoma de Barcelona* hereby confirms that the documents *Consent Form English.docx*, *General Information English.docx*, *ORSEE Flyer Spanish.docx*, *Protocal English.docx* and *Recruitment Email English.docx*, written in English have been revised by a native English-speaking professional translator/corrector, and are now deemed to be acceptable, *from the point of view of its language*. The correction of these documents has specifically focused on the grammar, lexis, orthography and syntax of the language used, in accordance with the accepted rules and guidelines for Standard English.

It is evident, of course, that any text written by non-native users of the language is unlikely to achieve the same linguistic effect as a text of a similar nature written by native users, in terms not only of the complexity and variety of its grammar and lexis, but also with respect to the less identifiable component features of its overall style. However, whilst for a limited range of specific items the text may not conform to certain given publishing criteria (depending upon the journal in question), we nevertheless recognise that the general use of language in the article indicated in this certificate, as defined by broadly applied language-assessment standards such as those of the Council of Europe's Common European Framework of Reference for Languages, is in all other senses of native-like competence.

Lluc Potrony
Language Advisory & Translation Unit
Servei de Llengües
Universitat Autònoma de Barcelona

Document validated by the UAB Translation Service

Appendix D: UAB Ethics Committee (CEEAH) Approval

Universitat Autònoma de Barcelona

Vicerectorat d'Investigació

Comisión de Ética en la Experimentación Animal y Humana (CEEAH)

Universitat Autònoma de Barcelona
08193 Bellaterra (Cerdanyola del Vallès)

La Comisión de Ética en la Experimentación Animal y Humana (CEEAH) de la Universitat Autònoma de Barcelona, reunida el día **24-02-2017**, acuerda informar favorablemente el proyecto titulado **"Behavior in the Presence of Heterogeneities in Games/ Coordination with asymmetries in highly volatile and complex environments"** presentado por **Carles Sola Belda**

Elaborado:	**Aprovado:**
Nombre: Nuria Perez Pastor Cargo: Secretària de la CEEA de la UAB Fecha: NURIA PEREZ PASTO Firmado digitalmente por NURIA PEREZ PASTOR Nombre de reconocimiento (DN): c=ES, ou=Vegeu https://www.aoc.cat/CATCert/Regulacio, sn=PEREZ PASTOR, givenName=NURIA, serialNumber=35109638T,	Nombre: José Luis Molina González Cargo: President de la CEEAH de la UAB Fecha: MOLIN GONZALEZ, JOSE LUIS Firmado digitalmente MOLINA GONZALEZ, LUIS Fecha: 2017.02.28 11:00:42

Edifici A - Campus de la UAB - 08193 Bellaterra (Cerdanyola del Vallès) - Barcelona Spain
Tel.: 34 - 93 581 35 78 - Fax: 34 - 93 581 20 23
www.uab.cat / ceeah@uab.cat

The Commission on Ethics in Animal and Human Experimentation (CEEAH) of the Autonomous University of Barcelona addresses the items identified in the preliminary review to ensure Protocol # 3637, Project ¨Behavior in the Presence of Heterogeneities in Games/ Coordination with Asymmetries in Highly Volatile and Complex Environments¨ is pursuant to 32 CFR 219, DoDI 3216.02_AFI 40-402 and other applicable regulations.

Item I. The CEEAH verifies that the "Universitat Autonoma de Barcelona IRB #1" identified on the US Department of Health and Human Service's Office for Human Research Protections website is accurate and remains in an active status. These details are provided below:

Institutional Review Board Information:
FWA00022917

Parent Institution/Organization:
IORG0008447 - Universitat Auttónoma de Barcelona
Expires: 3/26/2018

IRB00010121- Universitat Autónoma de Barcelona IRB #1
Status: Active
Expires: 03/27/2020
Location: Bellaterra (Barcelona)

Item II. This activity has been determined exempt in accordance with 32 CFR 219.101(b)(2) (identical to 45 CFR 46.101(b)(2))?

We have verified that this project will only use adults, 18 years of age or older, in the study. This is screened in the original recruitment of participants, students must provide identification to enter the laboratory, and sign a consent form that they are 18 years of age or older.

MOLINA GONZALEZ, JOSE LUIS (FIRMA)

Firmado digitalmente por MOLINA GONZALEZ, JOSE LUIS (FIRMA)
Fecha: 2017.09.06 16:45:15 +02'00'

José Luis Molina
President of the Ethics Committee (CEEAH)
http://www.uab.cat/etica-recerca/

Edifici A - Rectorat
Campus de la UAB · 08193 Bellaterra
(Cerdanyola del Vallès) · Barcelona · Spain
+34 93 581 3578

Appendix E: Air Force Human Research Protection Official (HRPO) Concurrence

DEPARTMENT OF THE AIR FORCE
AIR FORCE RESEARCH LABORATORY
WRIGHT-PATTERSON AIR FORCE BASE OHIO 45433

15 September 2017

MEMORANDUM FOR JOSÉ LUÍS MOLINA GONZÁLEZ
ETHICS COMMITTEE PRESIDENT
UNIVERSITAT AUTÓNOMA DE BARCELONA

FROM: 711 HPW/IR
2245 Monahan Way, Bldg. 29, Rm 202B
Wright-Patterson AFB, OH 45433

SUBJECT: Human Research Protection Official (HRPO) Initial Review

References: (a) 32 Code of Federal Regulations 219, *Protection of Human Subjects.*
(b) Department of Defense (DoD) Instruction (DoDI) 3216.02_AFI 40-402, *Protection of Human Subjects and Adherence to Ethical Standards in Air Force Supported Research.*

1. The HRPO has reviewed and concurs with the Institutional Review Board (IRB) determination for the following protocol in accordance with the References and other applicable requirements:

Air Force Protocol Number: FWR20170122Xe
Protocol Title: Behavior in the Presence of Heterogeneities in Games / Coordination with Asymmetries in Highly Volatile and Complex Environments
Non-DoD Principal Investigator: Carles Sola Belda
IRB Name: Universitat Autonoma de Barcelona (UAB), Spain
Determination: Exempt in accordance with 32 CFR 219.101(b)(2)

2. Please contact my office to discuss any substantive change to this activity prior to implementation. This will ensure it does not impact the determination herein or compliance with the References.

3. Please contact me with any questions by phone: (703) 588-2163 or via email: jessica.candia@us.af.mil.

CANDIA.JESSICA.COX.1281728209

Digitally signed by CANDIA.JESSICA.COX.1281728209
DN: c=US, o=U.S. Government, ou=DoD, ou=PKI, ou=USAF, cn=CANDIA.JESSICA.COX.1281728209
Date: 2017.09.15 18:15:03 -04'00'

JESSICA CANDIA, JD
Human Research Protection Official

Attachment:
Terms of AF HRPO Approval

TERMS OF AIR FORCE HUMAN RESEARCH PROTECTION OFFICIAL (HRPO) APPROVAL

By virtue of the Air Force (AF) support (see definition in DoDI3216.02_AFI40-402) provided to the non- Department of Defense (DoD) institution performing the activity identified herein, this activity must comply with all applicable federal, DoD, and AF human research protection requirements. In addition to the requirements identified in the conducting non-DoD institution's Federalwide Assurance, compliance with the following laws, regulations, and guidance is required:

- Title 10 United States Code Section 980 (10 USC 980), "Limitation on Use of Humans as Experimental Subjects"
- Title 21 Code of Federal Regulations 50, 56, 312, and 812, Food and Drug Administration (FDA) Regulations
- Title 32 Code of Federal Regulations Part 219 (32 CFR 219), Department of Defense Regulations, "Protection of Human Subjects"
- Title 45 Code of Federal Regulations Part 46, (45 CFR 46) Department of Health and Human Services Regulations, "Protection of Human Subjects," Subparts B, C, and D as made applicable by DoDI3216.02_AFI40-402
- DoDI 3210.7, "Research Integrity and Misconduct"
- DoDI3216.02_AFI40-402, "Protection of Human Subjects and Adherence to Ethical Standards in Air Force Supported Research"
- DoDI 6200.02, "Application of Food and Drug Administration (FDA) Rules to Department of Defense Force Health Protection Programs"

Below is a select list of requirements from the regulations and guidance listed above (note: this list is not all inclusive).

- Submit for HRPO review substantive modifications prior to implementation these include but are not limited to: changes to the research protocol or consent, any modifications that could potentially increase risk to subjects, a change in Principal Investigator, change or addition of an institution, addition of any new engaged research staff, elimination or alteration of the consent process, change in the IRB of Record, change to the study population that has regulatory implications (e.g. adding children, active duty population, etc.), significant change in study design (i.e. would prompt additional scientific review), or a change that could potentially increase risks to subjects
- Conduct initial and continuing research ethics education for personnel who are engaged in the research
- Ensure IRB consideration of scientific merit of new research and any substantive amendments thereto
- Ensure additional protections for military research subjects to minimize undue influence
- Explain to subjects any provisions for medical care for research-related injury
- Report continuing review documentation, unanticipated problems, adverse events, research-related injury, and suspensions or terminations of research
- Appoint a research monitor, when necessary
- Safeguard for research conducted with international populations
- Protect pregnant women, prisoners, and children
- Comply with DoD limitations on research where consent by legally authorized representatives is proposed
- Comply with DoD limitation on exceptions from informed consent (e.g., 10 USC 980, 45 CFR 46, and 21 CFR 50)
- Comply with limitations on dual compensation for U. S. military personnel
- Follow DoD requirements for additional review for DoD-sponsored survey research or survey research within DoD
- Address and report allegations of non-compliance with human research protections
- Address and report allegations of research misconduct
- Follow procedures for addressing financial and other conflicts of interest
- Prohibit research with prisoners of war (POW)
- Comply with requirements for investigations of Food and Drug Administration regulated products (drugs, devices, and biologics)
- Follow recordkeeping requirements
- Support oversight by the supporting DoD Component (which may include DoD Component review of the research, requests for documentation such as Institutional Review Board (IRB) membership rosters, and site visits)

Please contact the supporting AF institution (e.g., via the Program Manager responsible for oversight of the relevant activity) with any questions for the AF HRPO.