ESSAYS ON COOPERATION AND COMPETITION IN STRATEGIC ENVIRONMENTS

by

Alecia Evans

A Dissertation

Submitted to the Faculty of Purdue University

In Partial Fulfillment of the Requirements for the degree of

Doctor of Philosophy

Department of Agricultural Economics

West Lafayette, Indiana

May 2022

THE PURDUE UNIVERSITY GRADUATE SCHOOL
STATEMENT OF COMMITTEE APPROVAL

Dr. Juan Pablo Sesmero, Chair
Department of Agricultural Economics

Dr. Joseph Balagtas
Department of Agricultural Economics

Dr. Jonathan Bauchet
Division of Consumer Science

Dr. Timothy Cason
Department of Economics

Dr. Steven Wu
Department of Agricultural Economics

Approved by:
Dr. Nicole Olynk Widmar

To Beth

ACKNOWLEDGMENTS

In my formative years, I had to memorize the following quote from Henry Wadsworth Longfellow, “the heights of great men reached and kept were not attained by sudden flight, but they, while their companions slept, were toiling upward in the night”. This became my mantra, the driving force that propelled me to this point in my life. However, the successful completion of my doctoral studies would not have been possible solely by this internal unrelenting drive. I owe much gratitude to my advisor, Professor Juan Pablo Sesmero. He encouraged my academic curiosity and helped me to reign it in when it ran wild. Also, he gave very detailed feedback in every dimension of the research process that provided the guidance I needed to develop as a researcher. I am also thankful to Professor Timothy Cason and Dr. Yaroslav Rosokha. Professor Cason offered a lot of guidance in the design and implementation of my laboratory experiment. He is very generous with his vast knowledge of experimental economics and his passion for it is contagious. Dr. Rosokha introduced me to agent-based modeling and was always willing and able to offer insights into improving the efficiency of my coding. I am also grateful to the other members of committee who have inspired and guided me in various ways. Dr. Bauchet for always being available. Dr. Wu’s reminders of keeping the research problem simple, and Dr. Balagtas for his excellent introduction to the modern agricultural markets.

Most importantly, this process would have been impossible without the unwavering emotional support of my family and friends. I am also thankful for all the assistance and guidance from the faculty and staff of the Agricultural Economics department at Purdue University and particularly, the Jim and Neta Hicks Small Grant.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

ABSTRACT

In many economic settings agents behave strategically. Understanding and, sometimes regulating, that behavior is often crucial to enhance the efficiency with which scarce resources are allocated. A peculiar feature of economics is that cooperation among agents sometimes boosts efficiency, and sometimes hinders it. Social dilemmas, highly ubiquitous in economics, are situations in which cooperation boosts efficiency. Highly concentrated markets where a few firms operate, are situations in which cooperation (also known as collusion) among firms hinders efficiency. In such markets competition, rather than cooperation, boosts efficiency. In this dissertation, I study how uncertainty affects cooperation in social dilemmas, and how the presence of cooperative firms affects competition in concentrated markets.

Both of the settings I study in this dissertation (social dilemmas with noisy payoffs and duopsony with endogenous location and pricing strategy) face a similar challenge. Their complexity compromises the tractability of conventional equilibrium concepts. In other words, Nash equilibria do not exist, or there is a multiplicity of equilibria. This, in turn, precludes comparative static analyses characterizing the effect of exogenous market forces (uncertainty and firm ownership structure) on market and welfare outcomes.

I address this key challenge through a combination of genetic algorithms and laboratory experiments. A genetic algorithm consists of a selection process that identifies strategies that perform better than others, on average. Therefore, surviving strategies constitute, in a sense, average best responses. More than one strategy may survive. This happens when none of the surviving strategies is weakly dominated by the other surviving strategies. An equilibrium is a combination of surviving strategies. In this context, a comparative static analysis consists of the change in equilibrium (combination of surviving strategies) due to a change in exogenous forces. These comparative static analyses generate testable hypotheses. In Essays 1 and 2, I implement laboratory experiments to test these hypotheses.

In Essay 1, I compare infinitely repeated social dilemmas with deterministic and noisy payoffs. I test whether noise in payoffs (where noisy payoffs are generated by a random shock and are uncorrelated amongst agents), which introduces imperfect monitoring, affects cooperation. Experimental evidence shows that imperfect monitoring reduces cooperation

because it hinders agents' ability to threaten defectors with a reciprocal defection. Therefore, noise reduces efficiency by unraveling cooperation in social dilemmas. In Essay 2, I study whether correlation among agents' noisy payoffs strengthens monitoring and restores cooperation. Experimental evidence shows that stronger (though still imperfect) monitoring due to correlation helps cooperation if and only if agents are prone to cooperate in the initial rounds of the repeated game. Therefore, correlation among shocks affecting agents' payoffs may or may not increase efficiency depending on the type of players participating in the social dilemma.

Finally, in Essay 3, I use a genetic algorithm to generate comparative statics characterizing the effect of a cooperative firm on market equilibrium and efficiency in a spatial duoposony. A Nash equilibrium in this setting does not exist when location, price, and the degree of spatial price discrimination are all endogenous in the seminal Hotelling's model. I use a genetic algorithm to identify a stable equilibrium in this setting. I find that a cooperative firm increases efficiency. But, counterintuitively, it does so when the cooperative does not directly compete with the privately owned firm. This is because the cooperative maximizes market share when its procurement region does not overlap with the privately owned firm's procurement region.

1. INTRODUCTION

The efficiency of many markets depends upon the extent to which agents can cooperate or compete. However, the extent of cooperation or competition depends upon key features of the trading environment. I focus on two of them. First, there is the existence of imperfect monitoring among agents which hinders the agents' ability to engage in conditional cooperation ("if you defect, I'll defect, but I will cooperate as long as you cooperate") and raise efficiency. Second, there is the ownership structure of the industry (especially, the degree of vertical differentiation) that may strengthen competition and efficiency. As such, my objective is to study the effect of imperfect monitoring and ownership structure on behavior and market efficiency. A key challenge to studying behavior in these settings is that Nash equilibria are either intractable or multiple equilibria exist. I circumvent this challenge by using agent-based modeling to generate testable hypotheses, and then conducting laboratory experiments to test them.

In essay 1 and 2, I study the effect of imperfect monitoring, that is generated from the presence of random shocks, on cooperation in social dilemmas. The well-being of economic agents is frequently affected by random shocks, such as weather events, epidemics, or financial shocks. Given that individuals exist in the same geographic space, or are engaged in complementary or competing economic activities, there is usually some degree of correlation between the shocks people face. The sign and degree of correlation across shocks are especially important for economic activities conducted in groups. In these activities, there is often a social dilemma, a situation in which there is a conflict between individual and collective interests. Examples include groups designed to facilitate informal risk-sharing (e.g. Fitzsimons, Malde, and Vera-Hernández (2018)), provide access to information, insurance and credit (e.g. Bloch, Genicot, and Ray (2008)), improve productivity and profitability of farmers in developing countries (Agarwal 2018), and prevent resource exhaustion (e.g. Ostrom et al. (1999)). In many of these groups, individuals repeatedly interact overtime. Repetition can enhance cooperation, but it does not necessarily guarantee it.

When payoffs are affected by random shocks, it is harder for individuals to know whether others are cooperating or not, introducing imperfect monitoring in a repeated game, thereby weaking cooperation. Despite the empirical pervasiveness of noisy payoffs, there is a limited understanding of how they affect cooperation in social dilemmas. In essay 1, I recognize that noisy payoffs introduce imperfect monitoring amongst agents which can cause them to incorrectly infer the actions of others and change their own behavior, perhaps affecting cooperation. I conduct a laboratory experiment to examine the effect of noisy payoffs on cooperation in an infinitely repeated prisoner's dilemma. I find that noise inhibits cooperation, relative to a deterministic setting. Experimental evidence suggests that this is because noise raises inferential error, which hinders agents' ability to engage in conditional defection strategies. In other words, imperfect monitoring renders threats of punishment in repeated interactions, less effective.

Essay 1 assumes that shocks affecting outcomes are uncorrelated across agents. If shocks were correlated, as they are in many empirical settings, players can use their own payoffs and knowledge of shock correlation to better infer their opponent's actions. A natural question arising from this study is: what, then, can restore cooperation in social dilemmas with noisy payoffs? I examine this issue in essay 2.

Many empirical situations display the structure of an indefinitely repeated social dilemma with correlated noisy payoffs. Microfinance (or some variation of group borrowing, lending, or saving) is touted as successfully providing credit where formal financial markets are underdeveloped or non-existent. Given that there is a preference for group members to be similar (e.g. geographically, culturally, religiously), individuals will have correlated shock exposure. This allows members to provide mutual insurance. But free-riding challenges these arrangements. The extended family, especially in developing countries, is also an important source of informal insurance. Where formal insurance markets are underdeveloped and economic well-being is very sensitive to random shocks, the extended family serves as a safety net for family members. However, there is evidence that family members do renege on these arrangements (Baland, Guirkinger, and Mali 2011; Jakiela and Ozier 2016). Agricultural cooperatives also fall prey to free-riding problems. For instance, Bonroy et al. (2019) documents how members free-ride on product quality, which is exacerbated by the monitoring challenges. Monitoring is costly. A similar situation can arise in the very formation of a cooperative organization (Giannakas, Fulton, and Sesmero 2016). Within each of these situations, the correlation structure could aid in monitoring because a member's well-being could offer insights into the well-being of others.

I develop a framework to study this environment and predict that correlation across shocks can restore cooperation by enhancing knowledge about past behavior. I then test this prediction in a laboratory experiment. On average, I fail to confirm our prediction. Nevertheless, I find that correlation across shocks fosters (inhibits) cooperation among subjects that choose to cooperate (defect) during the initial stages of the game. I complement our experiments with simulations based on a genetic algorithm and find that correlation makes conditional cooperation strategies more successful, prompting these strategies to survive the evolutionary process. As a result, in an evolutionary framework, correlation unambiguously enhances cooperation.

In Essay 3, I study the effect of a cooperative firm (where the firm's objective is to maximize the surplus of input suppliers) in an otherwise private market, on market equilibrium and efficiency. High cost of transporting farm products degrades the outside option of farmers (that is, farmers prefer to sell to nearby buyers so as to save in transportation cost and receive a higher price) and weakens competition among buyers. Agricultural cooperatives (COOP) can discipline investor-owned firms (IOF) because their objective is to maximize farmers' surplus. I study the effectiveness of a COOP in strengthening competition in the context of a spatial market, where firms choose both location and spatial-pricing strategies. I use a genetic algorithm to circumvent that inexistence of a Nash equilibrium in the seminal Hotelling model.

The presence of a COOP has the largest competitive effect when transportation cost is small. In this case, the COOP reduces distance between plants (that is, buyer differentiation), as well as input price markdown and the degree of spatial price discrimination by the IOF. As a result, efficiency rises. However, the presence of a COOP produces the largest efficiency gains when transportation cost is high, and the IOF is not affected by the presence of the COOP. This is because the COOP, in the absence of competition from the IOF (due to high transportation cost), can capture a larger share of the procurement market. Therefore, the presence of a COOP raises efficiency even if it does not compete directly with the IOF.

References

Agarwal, Bina. 2018. "Can group farms outperform individual family farms? Empirical insights from India." *World Development* 108:57–73.

Baland, Jean-Marie, Catherine Guirkinger, and Charlotte Mali. 2011. "Pretending to be poor: Borrowing to escape forced solidarity in Cameroon." *Economic development and cultural change* 60 (1): 1–16.

Bloch, Francis, Garance Genicot, and Debraj Ray. 2008. "Informal insurance in social networks." *Journal of Economic Theory* 143 (1): 36–58.

Bonroy, Olivier, Alexis Garapin, Stephen F Hamilton, and Diogo M Souza Monteiro. 2019. "Free-riding on Product Quality in Cooperatives: Lessons from an Experiment." *American Journal of Agricultural Economics* 101 (1): 89–108.

Fitzsimons, Emla, Bansi Malde, and Marcos Vera-Hernández. 2018. "Group size and the efficiency of informal risk sharing." *The Economic Journal* 128 (612): F575–F608.

Giannakas, Konstantinos, Murray Fulton, and Juan Sesmero. 2016. "Horizon and free-rider problems in cooperative organizations." *Journal of Agricultural and Resource Economics,* 372–392.

Jakiela, Pamela, and Owen Ozier. 2016. "Does Africa need a rotten kin theorem? Experimental evidence from village economies." *The Review of Economic Studies* 83 (1): 231–268.

Ostrom, Elinor, Joanna Burger, Christopher B Field, Richard B Norgaard, and David Policansky. 1999. "Revisiting the commons: local lessons, global challenges." *science* 284 (5412): 278–282.

2. NOISY PAYOFFS IN AN INFINITELY REPEATED PRISONER'S DILEMMA

In many settings, interactions amongst economic agents have the structure of a social dilemma – a situation in which agents fail to cooperate even when cooperation is mutually beneficial. When such interactions are repeated over time, cooperation is more likely. In repeated interactions, agents adjust their behavior to the past behavior of those they interact with. Sometimes the behavior of others is not directly observable, so agents use observed outcomes to infer others' behavior. When outcomes are affected by noise (that is, random shocks), an observed outcome may be consistent with multiple actions. This can introduce the possibility of inferential error – a situation where agents misperceive the past behavior of people they interact with. This may, in turn, impact their decisions and, consequently, cooperative outcomes. We study how noise affects inferential error, and by extension, behavior and cooperation in repeated social dilemmas.

The prisoner's dilemma (PD) is typically used to capture and study the social dilemma that is at the heart of many economic interactions. In social dilemmas, mutual cooperation achieves a first best. However, not cooperating is a dominant strategy for individual players, making no cooperation a Nash equilibrium of the stage game. In other words, individual actions, motivated by self-interest, often conflict with the collective interest of the entire group. Nevertheless, cooperation is still likely if interactions amongst players are infinitely repeated, and players are sufficiently patient. This is because players may engage in conditional defection, that is, players cooperate under the threat of future defection by others if they themselves defect. Ioannou (2014b) argues that cooperation is not a robust result, but it is rather driven by the assumption of an error-free environment, which facilitates conditional defection. However, most of the social dilemmas we observe in the real world are hardly error-free because outcomes are affected by random shocks.

When outcomes are affected by random shocks, conditional defection becomes harder because it is difficult for players to know when others have failed to cooperate. In other words, it is difficult for players to monitor others' actions. Given how ubiquitous random shocks are in real-world interactions, it is important to understand its impacts on monitoring and, consequently, cooperative behavior in social dilemmas.

For empirical context, consider an agricultural cooperative. Free-riding challenges the success of agricultural cooperatives. Members may free-ride on product quality (Bonroy et al. 2019) and even the very formation of cooperatives (Giannakas, Fulton, and Sesmero 2016). For instance, in marketing cooperatives, the ability to produce a certain amount of high-quality product depends on the coordinated actions and cooperation of all members. This is because members commit to investing in quality-enhancing inputs so that, as a group, they can achieve a certain volume at a minimum targeted quality.[1] However, actions by individual farmers are often unobserved and hard to infer from outcomes due to noise affecting them (e.g. weather)[2], thereby posing a challenge for monitoring. Bonroy et al. (2019) has offered some documentation of this phenomenon in winemaking cooperatives, among other situations. We examine how noise affecting output influences behavior and cooperation in this type of social dilemma settings.

We examine this in a laboratory experiment. We implement an infinitely repeated PD with a continuation probability of $\delta = 0.9$. There are two treatments. In the first treatment, we implement a PD with no noise. In the second treatment, the stage game of the PD is affected by a random shock that is independent across players. This shock can be positive or negative and can be interpreted as an individual experiencing good luck (positive shock) or bad luck (negative shock). The shock introduces imperfect monitoring, resulting in agents making errors in inferring the actions of others. For our main result we find that cooperation is higher in the treatment without noise (perfect monitoring) than the treatment with noise (imperfect monitoring). Experimental data shed light on the mechanisms underlying our main result.

Under perfect monitoring, subjects can sustain cooperation by credibly engaging in conditional defection – that is, they can threaten others with defection if they defect. In the

1. ↑In the case of the formation of cooperatives, farmers must commit to contribute a minimum amount for the cooperative to invest in projects (Giannakas, Fulton, and Sesmero 2016).

2. ↑In other words, it is hard to determine whether a farmer that does not deliver a certain level of quality has shirked responsibilities, or has simply been unlucky and faced adverse growing conditions outside of her control.

treatment with noise, subjects often made inferential errors. Imperfect monitoring resulting from inferential errors weakens the subjects' ability to credibly engage in conditional defection. It does so for two reasons. First, players are unsure about others' actions and think they may be failing to detect others' defection. In this case, they may preemptively defect to avoid the lowest payment (the "sucker" payment in a PD). In addition, they know it is also hard for others to detect defection, so they think they may be able to use imperfect monitoring to defect and avoid detection and punishment by others. In sum, random shocks affecting outcomes introduce imperfect monitoring, inhibiting the use of conditional defection strategies and, consequently, cooperation.

The strategies that the subjects played supported this belief. In the no noise treatment, subjects predominantly played Tit-for-Tat (TFT), a conditional defection strategy. This is not surprising given that defection is easily detected, and subjects can easily retaliate. However, in the treatment with noise, subjects leaned into more unconditional, uncooperative strategies such as Always Defect (AD). When they employed conditional strategies, these strategies were predominantly unforgiving, such as Grim Trigger (Grim). To summarize, imperfect monitoring pushed them to either unconditionally defect or defect when they first suspect the other player defected.

Our study contributes to the literature on cooperation in noisy in infinitely repeated PDs. Most of the literature have focused on the impact of implementation error (Fundenberg and Maskin 1990; Miller 1996; Fudenberg, Rand, and Dreber 2012; Imhof, Fudenberg, and Nowak 2007; Ioannou 2014a, 2014b; Zhang 2018). With implementation error, there is a probability that players' actions are implemented differently than they were intended. Implementation errors create an environment of imperfect information. Under these conditions, errors can alter strategies that dole out harsh punishment for defections. Overtime, such strategies may even be weeded out the environment (Fundenberg and Maskin 1990). However, the results on the impact of such errors are mixed. In some environments, agents do not tolerate defection, and in fact, are more likely to respond to defection with defection than to respond to cooperation with cooperation (Ioannou 2014b). In others, implementation errors can improve cooperation if the level of noise is low and the benefit to cooperation is high (Zhang 2018).

However, in an environment with noisy payoffs, actions are always implemented as intended. But subjects are often unsure about what action was actually implemented. Herein lies a critical difference of the two noisy environments. While implementation error creates imperfect information, the presence of inferential error creates an environment of imperfect monitoring.

We identify two ways in which an imperfect monitoring environment differs from an imperfect information environment. First, in an imperfect monitoring environment, an unconditional strategy such as AD will not accidentally cooperate. As matter of fact, Ioannou (2014b) identifies this as a reason for inferential errors being more devastating to cooperation than implementation errors. Second, conditional strategies are typically harder to implement, because of the likelihood of inferential error. Our results are consistent with both of these forces. In our treatment with noise, subjects do play more unconditional, uncooperative strategies.

Our setup is closest to Bendor, Kramer, and Stout (1991) and Bendor (1993). As a matter of fact, we experimentally test a framework very similar to theirs. Note that, while Ioannou (2014b) considers imperfect monitoring (perception errors) in their design, the inferential error rate was exogenously determined. However, our design has no such constraints. The error rate is endogenously determined and stems from the agents' ability to use the available information to update beliefs and make inference about others' actions. This environment mirrors many situations where the social dilemma is heightened by monitoring challenges.

The rest of the paper is organized as follows. In Section 2, we present the theoretical background. In Section 3, we give the details of the experimental design. In Sections 4, we outline the main results from our experiment. In Section 5, we conclude with a discussion of our main results.

2.1 Theoretical Background

We begin with a standard symmetric prisoner's dilemma where the stage game is given as $T > R > P > S$ and $2R > T + S$ (Table 2.1). Similar to Bendor (1993), we then introduce a random shock to the stage game payoffs. The players' realized payoffs become

$\hat{T}$, $\hat{R}$, $\hat{P}$ and $\hat{S}$. For example, let $\hat{X} = X + V$, where $X = \{T, R, P, S\}$. That is, the payoff is composed of a deterministic stage game payoff plus a random shock. Unlike Bendor where the shock is normally distributed, we implement shocks that are uniformly distributed between $V_{LB} \leq V \leq V_{UB}$. A uniformly distributed payoff makes it easier for subjects in the laboratory experiment to understand the game and the mapping from outcomes to actions. Furthermore, the shock is also identically and independently distributed across players.

Table 2.1. Deterministic payoff for the prisoner's dilemma

	C	**D**
C	R, R	S, T
D	T, S	P, P

This random shock introduces an environment of imperfect monitoring. Given the distribution of realized payoffs, there is an area in which a player cannot know for sure the actions of the other player. Imagine a situation where player 1 is cooperating. As long as $S + V_{UB} > R + V_{LB}$ there is region of uncertainty where she is unsure about the action of her opponent. If she is defecting, this is true for $P + V_{UB} > T + V_{LB}$. In this region of uncertainty, there is a positive probability that players will make inferential errors, in that, they can either incorrectly infer that the other player defected (Type 1 error) or incorrectly infer that they had cooperated (Type 2 error). To see how this happens, again, assume that player 1 is cooperating. If her realized payoff is within, but close to the lower bound of the region of uncertainty, one of two things could have occurred. The other player could have cooperated as well, and she received a large negative shock. But alternatively, she could have received the sucker payoff along with a large positive shock.

Given this region of uncertainty, players will face a trade-off between Type 1 and Type 2 errors. Each player selects a benchmark value and is informed if the other player's realized payoff lies above, below, or at this value. The benchmark values that a subject selects, serve as a private noisy signal about the other player's payoff.[3] In an environment of completely

3. ↑Given that the realized payoffs are independent across players, this signal is essentially uninformative. However, in other work, we used the same experimental framework but allowed for correlation in payoffs across payoffs. With greater degrees of correlation, the signal becomes more informative.

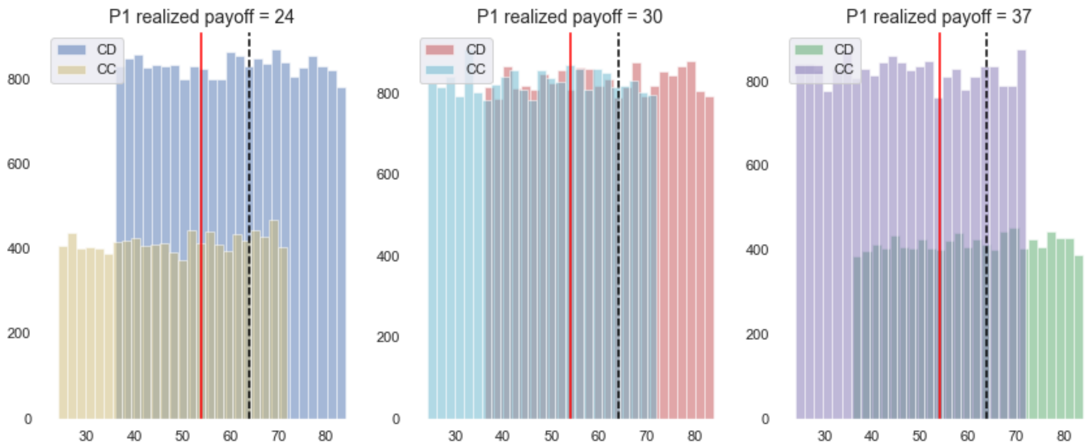
uncorrelated shocks, the value of the benchmark indicates the weight a player places on committing Type 1 or Type 2 error.

To see this, consider first a setting with the following deterministic payoffs: $R = 48$, $T = 60$, $S = 13$, and $P = 25$. Also, let us assume that shocks are uniformly distributed in the range $[-24, 24]$. In this setting, when a player cooperates, the region of uncertainty ranges from $[24, 37]$. This means that the player's payoff could, with a positive probability, fall within this range if the other players defects or cooperates. If the player's payoff is above 37, then the other player could not have defected. If the player's payoff is below 24, the other player could not have cooperated. When a player defects, the region of uncertainty ranges from $[36, 49]$.

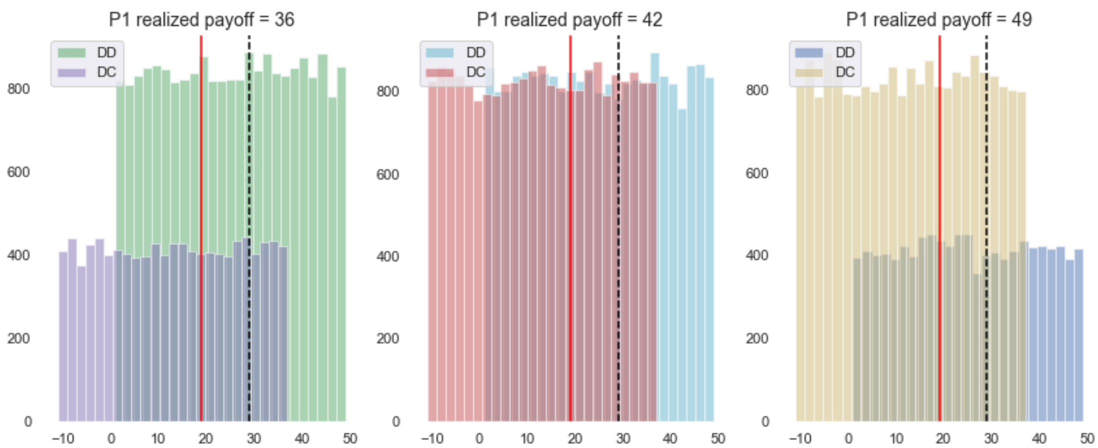
But how does the player infer the actions of her opponent within the region of uncertainty? In the absence of more information, the player simply makes a guess. And each time she guesses, there is a chance of inferential error. But if the player has some information about her opponent's payoff, they can use this information to make a more informed guess and reduce inferential error. Generally speaking, if the other player obtains a high (low) payoff, then they are, all else constant, more likely to have defected (cooperated). In practice, players often have some information, if noisy, about their opponent's well-being; whether they buy machinery, pay debt, purchase durables, make renovations on the house, and other similar expenses/investments.

To mimic this feature of many empirical settings, we introduce a noisy signal of the opponent's payoffs. Notice that, due to the symmetry across subjects, a player knows the probability distribution of their opponent's payoffs conditional on her own actions. And she can use this information and a noisy signal about her opponent's payoff to infer their actions. To illustrate this point, consider the payoff distributions portrayed in Figure 2. In this figure, we show the results of simulating player 2's payoff one million times, conditional on player 1's action and payoff. In each scenario, player 1 is cooperating and we show the distributions of player 2 cooperating and defecting given various realized payoffs of player 1.

Suppose that player 1's payoff is 24 as in the left figure of Figure 2.1a. If player 1 knew with accuracy that player 2's payoff is 30, then she would know that player 2 cooperated – no negative shock is large enough to lower player 2's payoff to 30 when player 2 defects.



(a) The distribution of realized payoffs for player 2 when player 1 cooperates



(b) The distribution of realized payoffs for player2 when player 1 defects

Figure 2.1. In Panel (a), player 1 is cooperating. We show the distributions for player 2, when player 2 cooperates as well (CC) and when player 2 defects (CD). The distribution of player 2's realized payoff for a realized payoff of 24, 30 and 37 for player 1. The possible realized payoffs for player 2 are in the range [24, 85]. As the benchmark increases from 54 (red solid line) to 64 (black dashed line), a greater importance is placed on not committing Type 1 errors (incorrectly inferring defection). In Panel (b), player 1 is defecting. We show the distributions for player 2, when player 2 cooperates (DC) and when player 2 defects as well (DD). We show the distribution of player 2's realized payoff, when player 1's realized payoff is 36, 42 and 49. The possible realized payoffs for player 2 are in the range [-11, 49]. As the benchmark increases from 19 (red solid line) to 29 (black dashed line), a greater importance is placed on not committing Type 1 errors (incorrectly inferring defection).

By the same logic, player 1 would know that player 2 defected if her payoff was 80. If player 2's payoff fell on the part of the domain where both distributions overlap (henceforth, the "overlapping region"), then she would be unsure about player 2's actions and could incorrectly infer it. Moreover, in reality, players seldom have access to accurate information. They typically have noisy information that, at best, tells a player whether their opponent's payoff falls within some "region" that the player considers informative.

We formalize this situation by letting the player establish a benchmark value, on the opponent's payoff domain, above which the player assumes the opponent defected and below which the player assumes the opponent cooperated. The noisy signal indicates whether the opponent is above or below the benchmark, but not her exact payoff.[4] The level of this benchmark not only affects inferential error but also the type of error incurred. Suppose player 1 sets a very high benchmark. This means that she will infer defection from player 2 only if player 2's payoff is very high. In other words, player 1 will mostly infer cooperation. This will probably lead to a high frequency of inferential error, mostly consisting of Type 1 errors. If the benchmark is very low, the frequency of inferential error will also tend to be high, but errors will mostly consist of Type 2. Finally, a benchmark located towards the middle of the "overlapping region" will result in lower frequency of inferential error and a balanced prevalence of Type 1 and Type 2 errors.

From the simulations in Figure 2.1, we can numerically show that Type 1 and Type 2 errors are equalized at a benchmark value of 54. If we increase the benchmark value to, say, 64, Type 1 error (incorrectly inferring defection) increases, while Type 2 error (incorrectly inferring cooperation) decreases. If we decrease the benchmark value below 54, Type 1 error increases and Type 2 error decreases.

Therefore, by changing the benchmark relative to which the player gets a noisy signal, she is changing not only the overall frequency of inferential error but also the implicit weight that is placed on committing Type 1 or Type 2 errors. If the player is particularly concerned about being the "sucker" (cooperating when the opponent is defecting), then she will set

4. ↑In an empirical situation this is similar to player 1 obtaining information regarding expenses/investments made by player 2 or other information of similar nature, that indicates the overall region in which player 2's payoff fell.

a low benchmark. But this can impact overall cooperation. If the player infers defection, she may want to retaliate and defect. In this case, the other player may eventually infer defection and defect themselves (if they were not already defecting). This situation may prompt cooperation to unravel. The opposite may happen if players set a low benchmark.

Another important insight from Figure 2.1 is that, the frequency of inferential error is also determined by the player's own payoff. For instance, if player 1 obtains a payoff of 30, at any point within the "overlapping region" defection and cooperation by player 2 are equally likely. But if the player obtains a high payoff of 37 then, within the "ovelapping region" it is more likely that player 2 cooperated – which is partly why player 1 obtained a high payoff to begin with. Therefore, if player 1's payoff is high, it is slightly easier to correctly infer the actions of player 2, thereby reducing inferential error. This also true of player 1 obtains a low payoff of 24. In this case, it is slightly easier for player 1 to correctly infer defection.

Ultimately, the frequency and composition of inferential error depends upon how players use the information available to them, regarding their own payoff and the payoff of their opponent. And, in turn, the frequency and composition of inferential error is likely to affect their willingness to cooperate and, overall cooperation in the repeated game with noise.

A key mechanism underlying cooperation in repeated PDs is the type of strategies followed by players. Players may engage in unconditional strategies (that is, always cooperate or always defect) or conditional ones. Conditional strategies are easy to follow (and, therefore, more credible and effective) under perfect monitoring. Both players can observe their opponent's behavior and condition their actions to them. And both players know the other players can observe their behavior, and so on.

In contrast, imperfect monitoring makes conditional strategies harder to implement. First, if the player's payoff falls in her region of uncertainty, then she cannot know with certainty her opponent's past behavior. It is, naturally, harder to condition a strategy on an uncertain event. Second, the threat of a conditional defection from another player may be perceived as weaker if the player knows that her opponent may not detect her defection with certainty. It is unclear, therefore, how imperfect monitoring may influence players' strategies. Will they use more or less conditional strategies? If they use conditional strategies, what are the conditions under which players will infer defection? And conditional on inferring defection, how quickly would they punish their opponent, and for how long?

Our discussion reveals a direct effect of noise on inferential error, and then two channels through which inferential error may affect cooperation. These mechanisms are portrayed in Figure 2.2. Noisy payoffs introduce uncertainty regarding the opponent's past behavior, which may induce subjects to incorrectly infer their opponents' past actions. This has a direct and an indirect effect on cooperation.

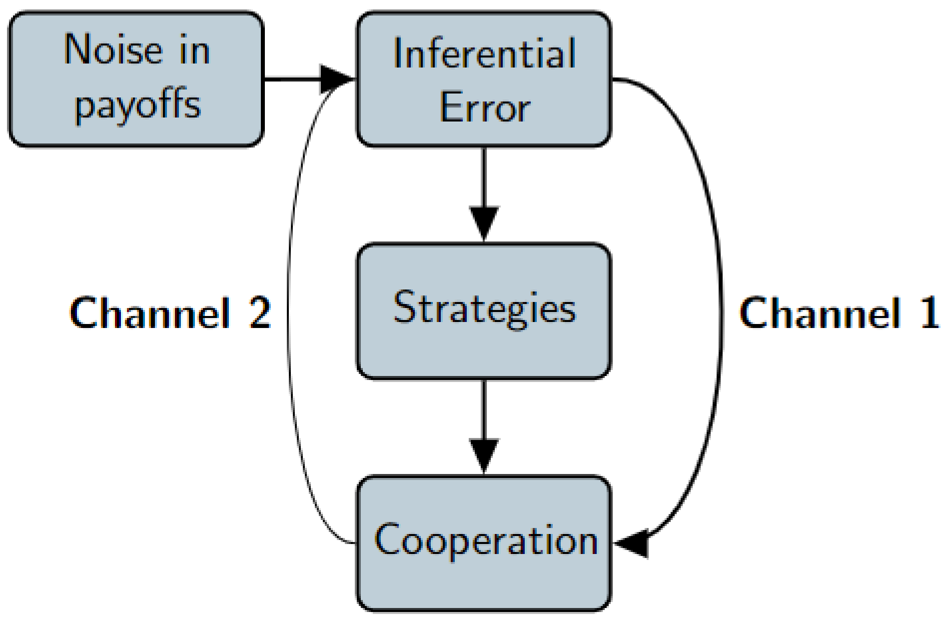


Figure 2.2. Causal channels and the effect of noisy payoffs on cooperation

We call the direct effect of inferential error on cooperation, channel 1 (Figure 2.2). A player that defects after she detects a defection by her opponent, may incorrectly infer defection, and punish the opponent unnecessarily. She may also be unnecessarily punished by her opponent, prompting the player to defect herself, even if she was not defecting before. A similar logic applies when a player incorrectly infers cooperation. In this case, players may fail to punish defection by their opponent, or may be able to hide defection from their opponents due to uncertainty.

We call the indirect effect of inferential error on cooperation, channel 2 (Figure 2.2). In the absence of uncertainty regarding the opponent's actions, a player may commit more credibly to conditional strategies. But when inferential errors emerge due to noisy payoffs, players may be prone to rely less on conditional strategies; or at the very least simplify conditional strategies to avoid switching every time the player infers (perhaps incorrectly) a change in her opponent's actions. In this channel, inferential error affects cooperation, but through a change in the type of strategies used by players.With these considerations in mind, our experimental design is aimed at answering the following question:

Question 1: *Is cooperation lower in the presence of noise?*

We predict that the presence of noise in outcomes will inhibit cooperation. We believe this will be mostly driven by two distinct mechanisms. First, the presence of noise may weaken monitoring, that is, increase inferential error. This prediction is not only motivated by our numerical simulations (illustrated in Figure 2.1), but also in line with theoretical discussions in Aoyagi, Bhaskar, and Fréchette (2019) and Ioannou (2014b). But, as previously discussed, overall frequency and nature of inferential errors will depend on players' ability to process information and their choice of benchmark. Therefore, we raise the following question about this mechanism:

Question 2: *How frequent are inferential errors with noisy outcomes?*

We predict that, in the presence of noisy outcomes, players will make inferential errors.

A second mechanism relates to the type of strategies used by players, conditional on their inference. A player may decide to play unconditional strategies, that is, defect or cooperate regardless of the opponent's actions. But often, players engage in conditional strategies (Dal Bó and Fréchette 2018). If a player infers that their opponent defected in the previous round, she can be tolerant or punish them. If she punishes them, she may do it for one round or multiple rounds. If noise affects the ability of players to infer their opponent's past actions, and/or the confidence with which players infer such actions, then noise may change the type of strategies used by the players. This motivates our next question:

Question 3: *How do the strategies that players use vary with noise?*

We predict that subjects will use less forgiving strategies in our treatment with noise, in comparison to the treatment with no noise. Our prediction is motivated by observations in

Ioannou (2014b). His automata are more likely to respond to defection with defection than to respond to cooperation with cooperation. We expect similar results with our subjects, where they are less forgiving and lenient with noise.

To obtain an answer to these questions we conduct a laboratory experiment where subjects play the game we discussed and simulated in our theoretical background. We now turn to the design of the laboratory experiment.

2.2 Experimental Design

The experiment was conducted at Purdue University's Vernon Smith Experimental Economics Laboratory (VSEEL). Two treatments were implemented, a treatment with noise and one without. A total of 80 subjects participated in seven sessions, with each session consisting of 10 or 12 subjects. Table 2.2 shows the treatment details. Subjects accumulated points throughout the session. At the end of the session, these were converted at an exchange rate of \$1 = 300 points. We implemented a between-subject design, where each subject participated in only one session. There was only one treatment per session. The sessions for the noise treatment lasted approximately 90 minutes, while the no noise sessions lasted on average 60 minutes.

Table 2.2. Treatment, sessions, and subjects in the experiment

Treatment	No. of Sessions	Total Subjects
Noise	3	36
No noise	4	44

In each session, players are matched in pairs. The pair then plays the prisoner's dilemma game repeatedly. In other words, after each round of the game there is a probability that the game continues onto another round (and, conversely, a probability that the game ends). We use a continuation probability of $\delta = 0.9$. The game in which the same pair plays multiple rounds until termination is called a supergame. For both treatments, subjects are randomly rematched before each supergame. We pre-drew these game lengths using a geometric distribution (Romero and Rosokha 2019).

Table 2.3 shows the payoff matrix of the stage game in the no noise treatment. The payoffs for subjects are denoted in points. In each round, subjects choose between cooperation and defection (in the experiment we used neutral language of A or B). At the end of each round, subjects received feedback on only their payoff. On each decision page, subjects had a history of all their previous actions.

Table 2.3. Payoff of the stage game

	C	**D**
C	48, 48	13, 60
D	60, 13	25, 25

The interface for the noise treatment had a few notable differences. For this treatment, we introduce a random shock to the stage game payoff in Table 2.3. This shock is independent across rounds and across players and uniformly distributed within the range [-24, 24]. Also, in the noise treatment, before each supergame, subjects select two benchmark values. One benchmark value is used if the subject opts to cooperate and the other if they defect. The allowable range of values for each corresponds with the range of possible realized payoff the opponent could receive. That is, subjects were restricted to the range [24, 84] if they choose cooperation and a range of [-11, 49] if they choose defection. After each round, subjects receive information about the other player's realized payoff relative to these benchmark values (in addition to exact information on their own payoffs, of course). Specifically, the signal indicates if the opponent's realized payoff is above, below, or equal to the benchmark value selected.

To understand decisions by the subjects, we elicit their beliefs about the likely action of their opponent by using a Binarized Scoring Rule (BSR). The BSR is incentive compatible in that, as long as subjects prefer getting a reward as opposed to no reward, to maximize the probability of getting the reward, they find it optimal to truthfully report their beliefs about the other subject's action (Hossain and Okui 2013). We incentivize this truth telling with 2 points per decision. That is, for the 84 decisions that each subject made, they could earn an additional 168 points for truthfully reporting their beliefs. In Appendix A.1, we describe the belief elicitation process. In the instructions, we did not give subjects the full details of the belief elicitation process. They were informed that the details were available after the session.[5] This design feature follows Danz, Vesterlund, and Wilson (2020), who, in an experiment using BSR, found that giving subjects very detailed information on the incentive structure of the BSR results in them making errors in excess of 40%, than if detailed information was not given.

For both treatments, all the details of the experiment were explained to the subjects in the instructions (a copy is presented in Appendix A.2). Subjects read the detailed instructions onscreen before each session. They also had a written copy throughout the entire session. The experiment was programmed in oTree (Chen, Schonger, and Wickens 2016).

2.3 Results

To recap, the no noise treatment is an environment of perfect monitoring. In the absence of noise, subjects are fully aware of the action of their opponent given their observed payoff. This is not true in the presence of noise. While subjects can correctly infer the action of their opponent outside of the region of uncertainty, within this region, it is possible to make incorrect inferences about the actions of others. This creates an environment of imperfect monitoring. We are interested in understanding how imperfect monitoring affects cooperation in an infinitely repeated PD. We examine this in three parts. First, we examine the impact of noise on cooperation (Question 1 in Section 2). Then we examine the underlying mechanisms: the impact of noise on inferential errors (Question 2 in Section 2), and the impact of noise on the strategies subjects play, conditional on inference (Question 3 in Section 2).

2.3.1 Cooperation Rates

Figure 2.3 shows the evolution of cooperation under the baseline of no noise and the treatment with noise. Average cooperation measures the proportion of rounds in which subjects cooperated in a supergame. Across all supergames, for both treatments, cooperation rate is statistically greater than zero. However, overall cooperation is significantly higher in

5. ↑No subject asked for this information after the session.

the no noise treatment (0.68 versus 0.40, *p-value* 0.00). Statistical significance is established using a probit regression clustered at the session level. No statistical significance refers to a *p-value* above 0.1. While cooperation increased over subsequent supergames played by subjects under the no noise treatment, it did not increase with noise. This suggests players learned to play conditional strategies and sustain higher cooperation in an environment of perfect monitoring, but that imperfect monitoring likely inhibited this learning process.

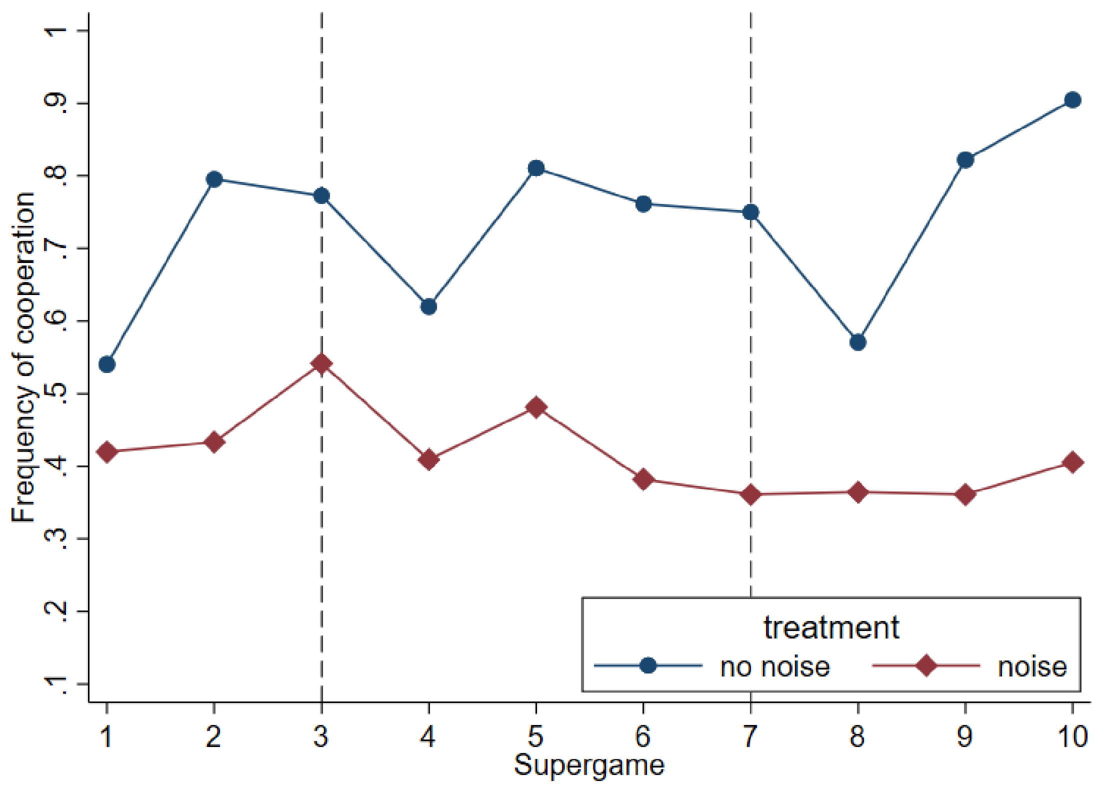


Figure 2.3. Frequency of cooperation across supergames for noise (red line) and no noise (blue line)

In Table 2.4, we further disaggregate these results by first round of the first supergame (column 1), early supergames (column 2), late supergames (column 3) and by all rounds of all supergames (column 4). We find that average cooperation in the very first round is not statistically different between the two treatments (*p-value* 0.53). For supergame 1-3, the difference between the two treatments is 0.21 (*p-value* 0.000), and this increases to 0.31 (*p-value* 0.001) in late supergames (that is, supergames 8-10).

A comparison of cooperation rates in the first round of the first supergame with later supergames suggests learning takes place under perfect and imperfect monitoring. But this learning process have opposite effects across environments. In a perfect monitoring environment, players learn to cooperate as revealed by an increase in cooperation rates from 0.57 to 0.68. In an imperfect monitoring environment, as players learn to play, cooperation unravels. As a result, cooperation rates decrease drastically from 0.61 to 0.37.

Table 2.4. Average Cooperation

Treatment	First round	Supergame 1-3	Supergame 8-10	All
Noise	0.61**	0.45***	0.37**	0.40***
	(0.07)	(0.04)	(0.06)	(0.04)
No noise	0.57***	0.66***	0.68***	0.68**
	(0.02)	(0.03)	(0.08)	(0.06)
p-value	0.53	0.00	0.00	0.00

Notes: Robust standard errors (in parenthesis) are clustered at the session level. * Indicates statistical significance at the 10% level (0.05 $<$ p-value $<$ 0.1). ** Indicates statistical significance at the 5% level (0.01 $<$ p-value $<$ 0.05). *** Indicates statistical significance at the 1% level (p-value $<$ 0.01)

We use values reported in Figure 2.3 and Table 2.4 to answer Question 1 in Section 2:

Result 1: *Cooperation is higher with perfect monitoring than imperfect monitoring.*

Result 1 supports our prediction that cooperation is higher under perfect monitoring. Our two treatments are parallel to the perfect monitoring and imperfect (noisy) private monitoring treatments of Aoyagi, Bhaskar, and Fréchette (2019). Our approach differs in an important way from the imperfect private monitoring of Aoyagi, Bhaskar, and Fréchette (2019). Inferential error is endogenous in our setting, and it depends on the agents' ability to use available information. In their study, inferential error is exogenous. Players do not directly observe their opponent's past action, but instead receive a private signal with a set known accuracy about such action. The signal is either good or bad, and a good signal is more likely to occur when their opponent is cooperative. Unlike our observations, they found no statistical difference in cooperation rates between imperfect private monitoring and perfect monitoring.

We now turn our attention to the mechanisms underlying the link between noise and cooperation.

2.3.2 Inferential Error and Cooperation

To measure inferential error, we use the subjects' beliefs regarding their opponent's past action elicited from subjects using the BSR and contrast those beliefs with actual actions taken by their opponents. In our framework, after each round, subjects selected the probability with which they believed that the other player had cooperated. If they indicated a probability greater than 0.5, we assigned inference to cooperation. For probabilities less than 0.5, we assigned inference to defection. When subjects assigned equal probability to cooperation and defection, we randomly assigned inference to cooperation or defection with a 0.5 probability.[6]

Subjects did commit inferential error outside of the region of uncertainty. However, this only occurred about 8% of the time. In contrast, inside of the region of uncertainty, subjects committed inferential error 33% of the time. For this reason, in our analysis, we examine inferential error both within the region of uncertainty (ROU) and across all decisions (All). Table 2.5 outlines both.

Table 2.5. Inferential Error Rates with Noise

	Supergame 1-3	Supergame 8-10	*p-value*
All	0.17**	0.15**	0.12
	(0.04)	(0.03)	
ROU	0.38**	0.31**	0.10
	(0.06)	(0.06)	

Notes: Robust standard errors (in parenthesis) are clustered at the session level. * Indicates statistical significance at the 10% level (0.05 < p-value < 0.1). ** Indicates statistical significance at the 5% level (0.01 < p-value < 0.05). *** Indicates statistical significance at the 1% level (p-value < 0.01)

6. ↑Of the 3024 observations for the noise treatment, there are 69 observations where subjects reported 0.5 probability of the other subject cooperating.

Regardless of the disaggregation across supergames, inferential error rate is statistically significantly greater than zero for both early supergames and later supergames. If we focus on errors within the region of uncertainty, the error rate slightly decreases from 0.38 (38%) in supergames 1-3 to 0.31 (31%) in supergames 8-10. This difference is at the margin of statistical significance *(p-value* 0.10). Likewise, the difference between earlier rounds (0.17) and later rounds (0.15) across all decisions is subjected to considerable noise (*p-value* 0.11).

Since, in the absence of noise, subjects know the opponent's past action with certainty, no inferential error takes place. In the presence of noise, however, inferential error emerges. The results in Table 2.5 deliver an answer to Question 2 in Section 2:

Result 2: *In the presence of noise, when the past action of the other player is uncertain, subjects commit inferential errors about a third of the time.*

Result 2 supports our prediction that inferential error raises with noise. We now turn our attention to the indirect channel through which noise affects cooperation; a behavioral channel, whereby noise prompts subjects to change the nature of the strategies they play.

2.3.3 Discussion of Strategies

We start this section by conducting an initial, crude analysis of how subjects respond to inferred past actions of their opponents. We then move on to a more elaborate scheme to elicit strategies (a set of contingent actions) played by subjects with and without noise in the infinitely repeated PD.

In Table 2.6 we report the frequency with which subjects defect in response to (inferred) defection and the frequency with which they cooperate in response to (inferred) cooperation. Like Ioannou (2014b), we see evidence that subjects are more likely to respond to defection with defection than to reciprocate with cooperation after cooperation when noise is present. As shown in Table 2.6, in the noise treatment, subjects respond to a perceived defection with defection roughly 47% of the time, while they defected in response to a perceived cooperation only 16% of the time. In contrast, in the absence of noise, players are more likely to cooperate in response to inferred cooperation (62% of the time) than they are likely to defect in response to defection (28% of the time).

Table 2.6. Player's Response to Other Player's Action

	CC	DC	CD	DD
No Noise	61.6%	6.23%	4.4%	27.8%
Noise	28.8%	16%	8.3%	46.9%

Notes: CC: a player cooperates after perceiving cooperation; DC: a player defects after perceiving cooperation; CD: a player cooperates after perceiving defection; DD: a player defects after perceiving defection

Interestingly, the incidence of cooperation after an inferred defection is much higher with noise than without. This perhaps indicates that subjects realize that their opponent is susceptible to making errors as well. Therefore, in these instances, they are giving their opponent the benefit of the doubt. Nevertheless, the incidence of defection after an inferred cooperation is also higher with noise than without. This seems to point that some subjects are simply going for the temptation payoff, perhaps conjecturing that their opponent will give them the benefit of the doubt. Therefore, after our crude analysis of actions, it remains unclear whether or not noise prompts subjects to use more conditional strategies and, if so, what these strategies are.

We use the Strategy Frequency Estimation Method (SFEM) from Dal Bó and Fréchette (2011) to elicit the frequency of strategies across treatments. SFEM uses a Maximum Likelihood Estimation (MLE) to estimate the frequency with which each strategy from a set of pre-determined set of strategies is found in the experimental data. This methodology has since been employed in Fudenberg, Rand, and Dreber (2012), Rand, Fudenberg, and Dreber (2015), Dal Bó and Fréchette (2018), Aoyagi, Bhaskar, and Fréchette (2019), Dal Bó and Fréchette (2019) and Romero and Rosokha (2019), for example. This method assumes that each subject uses the same strategy across supergames. However, they can make mistakes. These mistakes are not the errors that are generated from the experimental design, but rather, it is assumed that subjects can make mistakes when choosing their intended actions for the particular strategy they are following.

Using the notations of Dal Bó and Fréchette (2019), assume that the probability with which subject i makes mistakes is $1-\beta$ and the probability that her chosen actions correspond with a strategy k is β. The likelihood that her observed choices were actually generated by strategy k is $Pr_{\text{i}}(s^k) = \Pi_{M_{\text{i}}} \Pi_{R_{im}} (\beta)^{I^k_{\text{i}mr}} (1-\beta)^{1-I^k_{\text{i}mr}}$. In this expression, I^k_{imr} is an indicator function that takes the value 1 when the choice that was actually made in round r and supergame m is the same as what the subject would have made if she were following strategy k. It is coded 0 otherwise. M and R are the sets of supergames and rounds. The parameter β is estimated within the model. It can also be interpreted as the probability that an action is taken given that it is prescribed by a strategy k. Therefore β is the basis for evaluation of model fit, that is, as the model fit improves β approaches 1.

Therefore, the MLE process entails choosing both the probability of mistakes and the frequency of strategies that maximizes the likelihood of the sequences of choices. That is, the log-likelihood is $\Sigma_I ln(\Sigma_K \phi^k Pr_{\text{i}}(s^k))$, where K is the subset of strategies being considered and ϕ^k is a vector of parameter estimates that represent the frequency of strategies. We bootstrapped the standard errors in a way that respects the data generating process of our experimental data. We randomly draw the appropriate number of sessions, then for each session the appropriate number of subjects, then supergames. All with replacement. The bootstrapping process was done 1000 times. The standard deviation of the bootstrapped MLE estimates provide the standard errors.

We considered a subset of the ten strategies described in Appendix A.3. We first estimated the MLE using the twenty strategies described in Fudenberg, Rand, and Dreber (2012). Then we reduced this to ten strategies. All strategies that were statistically significantly identified from the larger set are included in this subset. We employed the additional step, given that Dal Bó and Fréchette (2019) found that the estimates of the MLE are robust to including additional strategies. However, excluding essential strategies could lead to misleading results. We will disaggregate results on the early stages of the game (supergames 1-3) and the late stages (supergames 8-10).

Results on strategy frequency are reported in Table 2.7. Subjects predominantly used memory-1 strategies. These are simple strategies in which subjects condition their actions only on the immediate past round. As shown in Table 2.7, for both treatments, the fraction of memory-1 strategies increased between supergames $1-3$ and supergames $8-10$. For the noise treatment this increased from 0.48 to 0.68, while for the no noise treatment this increased from 0.59 to 0.79. This indicates that players employ increasingly simple, though still conditional, strategies are they learn to play the game. Overall, the most employed strategies are AD, GRIM and TFT – all memory-1 strategies.

As expected, the strategies played are consistent with the cooperation rates observed across treatments. In the noise treatment, the fraction of more cooperative strategies decreased from 0.39 (17% TFT and 22% TF2T) in supergames 1 – 3 to 0.15 in supergames 8 – 10 (15% TFT). However, under perfect monitoring where noise is absent, the fraction of cooperative strategies increased from roughly 50% (35% TFT and 14% GRIM3) to about 54% (TFT). For the noise treatment, the most dominant strategy is AD, whereas the most dominant strategy without noise is TFT.

Our analysis indicates that players are more prone to use unconditional strategies under noise, while they rely more on conditional strategies without noise. Conditional strategies are played 40% of the time under noise and 80% of the time without noise. Also, the unconditional strategies used in a noisy environment, are predominantly non-cooperative. The results in Table 2.7 deliver an answer to Question 3 in Section 2:

Result 3: *Subjects used less cooperative strategies in the presence of noise.*

Result 3 supports our prediction that noise prompts subjects to play less forgiving and lenient strategies. The strategies played are consistent with the pattern of lower cooperation under the noise treatment relative to the treatment with perfect public monitoring.

Table 2.7. Estimation of Strategies Used

		Unfriendly			Provocable			Lenient				
Supergames	Treatment	AD	DTFT	DGRIM2	GRIM	2TFT	TFT	GRIM2	GRIM3	TF2T	AC	Beta
1-3	Noise	0.31***	0.01	0.03	0.02	0.13*	0.17**		0.08	0.22**	0.02	0.83***
		(0.09)	(0.05)	(0.03)	(0.10)	(0.08)	(0.09)		(0.10)	(0.10)	(0.05)	(0.03)
	No Noise	0.10**	0.02	0.05	0.14*	0.12	0.35***	0.06	0.14*	0.01		0.90***
		(0.05)	(0.04)	(0.04)	(0.10)	(0.15)	(0.14)	(0.10)	(0.10)	(0.08)		(0.02)
8-10	Noise	0.38***	0.05*		0.25***		0.15**	0.06		0.07	0.03	0.91***
		(0.12)	(0.04)		(0.10)		(0.09)	(0.7)		(0.07)	(0.05)	(0.02)
	No Noise	0.04	0.05	0.04	0.25**		0.54***		0.07			0.94***
		(0.04)	(0.04)	(0.04)	(0.12)		(0.12)		(0.07)			(0.02)

Notes: Bootstrapped standard errors are in parenthesis. Strategies that are 0.0 are dropped *** p<0.01, ** p<0.05, * p<0.1

2.4 Conclusion

The "Folk Theorem" for repeated games suggests that with repetition and sufficiently patient players, cooperation can arise as a sub-game perfect Nash equilibrium. However, when noise is introduced to social dilemma type settings, cooperation tends to be weaker than is expected. Previous studies have mostly focused on noise in the form of implementation error, where an action is accidentally change from what was intended. We examined the less explored inferential error, arising from noisy payoff structure. This noise creates imperfect monitoring, and region of uncertainty exists where players are prone to incorrectly infer the likely action of their opponent.

The primary focus of this paper is on the difference in cooperation rate between an environment with no noise and one without. Findings from Ioannou (2014b) suggest that inferential errors will erode cooperation, and to a greater extent than implementation error. We do find evidence of lower cooperation rate, but with respect to a baseline of no noise. We present evidence of this lower cooperation rate resulting from the prevalence of inferential error. With noise, subjects frequently incorrectly infer the previous action of their opponent. The noisy environment may have made them more comfortable to attempt to gain the temptation payoff. We saw a greater prevalence of subjects responding to cooperation with defection under the noise treatment, than under the no noise treatment.

This may also explain the prevalence of AD in the population of strategies employed. Subjects could have pre-emptively defected to avoid being a sucker. Dawes and Thaler (1988) observed that people tend to cooperate until they have evidence to show that they are being taken advantage of by who they are interacting with. Also, Dal Bó and Fréchette (2018) notes that cooperation is more likely to emerge as an equilibrium when the environment allows cooperation to be robust to strategic uncertainty. Subjects may have anticipated that the noisy environment would inhibit their ability to decipher their opponent's action. It could also be a case that players felt comfortable defecting under a veil of ignorance. Future work could examine if removing this veil, through improved monitoring, could recover cooperation.

References

Aoyagi, Masaki, V Bhaskar, and Guillaume R Fréchette. 2019. "The impact of monitoring in infinitely repeated games: Perfect, public, and private." *American Economic Journal: Microeconomics* 11 (1): 1–43.

Bendor, Jonathan. 1993. "Uncertainty and the evolution of cooperation." *Journal of Conflict resolution* 37 (4): 709–734.

Bendor, Jonathan, Roderick M Kramer, and Suzanne Stout. 1991. "When in doubt... Cooperation in a noisy prisoner's dilemma." *Journal of conflict resolution* 35 (4): 691–719.

Bonroy, Olivier, Alexis Garapin, Stephen F Hamilton, and Diogo M Souza Monteiro. 2019. "Free-riding on Product Quality in Cooperatives: Lessons from an Experiment." *American Journal of Agricultural Economics* 101 (1): 89–108.

Chen, Daniel L, Martin Schonger, and Chris Wickens. 2016. "oTree—An open-source platform for laboratory, online, and field experiments." *Journal of Behavioral and Experimental Finance* 9:88–97.

Dal Bó, Pedro, and Guillaume R Fréchette. 2011. "The evolution of cooperation in infinitely repeated games: Experimental evidence." *American Economic Review* 101 (1): 411–29.

———. 2018. "On the determinants of cooperation in infinitely repeated games: A survey." *Journal of Economic Literature* 56 (1): 60–114.

———. 2019. "Strategy choice in the infinitely repeated Prisoner's Dilemma." *American Economic Review* 109 (11): 3929–52.

Danz, David, Lise Vesterlund, and Alistair J Wilson. 2020. *Belief elicitation: Limiting truth telling with information on incentives.* Technical report. National Bureau of Economic Research.

Dawes, Robyn M, and Richard H Thaler. 1988. "Anomalies: cooperation." *Journal of economic perspectives* 2 (3): 187–197.

Fudenberg, Drew, David G Rand, and Anna Dreber. 2012. "Slow to anger and fast to forgive: Cooperation in an uncertain world." *American Economic Review* 102 (2): 720–49.

Fundenberg, Drew, and Eric Maskin. 1990. "Evolution and cooperation in noisy repeated games." *The American Economic Review* 80 (2): 274–279.

Giannakas, Konstantinos, Murray Fulton, and Juan Sesmero. 2016. "Horizon and free-rider problems in cooperative organizations." *Journal of Agricultural and Resource Economics,* 372–392.

Hossain, Tanjim, and Ryo Okui. 2013. "The binarized scoring rule." *Review of Economic Studies* 80 (3): 984–1001.

Imhof, Lorens A, Drew Fudenberg, and Martin A Nowak. 2007. “Tit-for-tat or win-stay, lose-shift?” *Journal of theoretical biology* 247 (3): 574–580.

Ioannou, Christos A. 2014a. “Asymptotic behavior of strategies in the repeated prisoner’s dilemma game in the presence of errors.” *Artif. Intell. Res.* 3 (4): 28–37.

———. 2014b. “Coevolution of finite automata with errors.” *Journal of Evolutionary Economics* 24 (3): 541–571.

Miller, John H. 1996. “The coevolution of automata in the repeated prisoner’s dilemma.” *Journal of Economic Behavior & Organization* 29 (1): 87–112.

Rand, David G, Drew Fudenberg, and Anna Dreber. 2015. “It’s the thought that counts: The role of intentions in noisy repeated games.” *Journal of Economic Behavior & Organization* 116:481–499.

Romero, Julian, and Yaroslav Rosokha. 2019. “The evolution of cooperation: The role of costly strategy adjustments.” *American Economic Journal: Microeconomics* 11 (1): 299–328.

Zhang, Huanren. 2018. “Errors can increase cooperation in finite populations.” *Games and Economic Behavior* 107:203–219.

3. COOPERATION IN SOCIAL DILEMMAS WITH CORRELATED NOISY PAYOFFS: THEORY AND EXPERIMENTAL EVIDENCE

Many important economic activities are carried out in groups where agents interact repeatedly over time. These groups are often formed to overcome market failures that inhibit socially desirable trading. For instance, agents organize in groups to facilitate informal risk-sharing (e.g. Fitzsimons, Malde, and Vera-Hernández (2018)), provide access to information, insurance and credit (e.g. Bloch, Genicot, and Ray (2008)), improve productivity and profitability of farmers (Agarwal 2018), and prevent resource exhaustion (e.g. Ostrom et al. (1999)). The success of these groups, however, crucially depends on the ability of members of the group to cooperate with each other.

But many of these settings have the structure of a social dilemma – a situation in which rational agents may fail to cooperate even when cooperation is mutually beneficial. This problem is exacerbated by uncertainty about past behavior – for example, arising from agents' inability to perfectly monitor each other – which may induce subjects to incorrectly infer others' past actions. Imperfect monitoring can be induced by random shocks that alter agents' payoffs (Bendor, Kramer, and Stout 1991; Bendor 1993). Such environments are empirically pervasive. As such, in this paper we focus on imperfect (private) monitoring and investigate how the structure of correlation across random shocks that alter players' payoffs affects the strength of monitoring and, ultimately, cooperation in infinitely repeated social dilemmas.

The outcome of infinitely repeated social dilemmas greatly depends on the strategic environment (see Dal Bó and Fréchette (2018) for a recent review). An important distinction between the structure of correlation across shocks and other features of the trading environment that can spur or hinder cooperation, is that correlation also affects risk-sharing; and risk-sharing is at the core of many economic activities that are carried out in groups. In developing countries, where formal insurance and credit markets are underdeveloped and economic well-being is very sensitive to random shocks (including income and health shocks), informal risk-sharing arrangements serve as a safety net. A prominent example of this environment is the extended family. In this context, risk-sharing takes the form of reciprocal credit systems where some siblings assist others who later reciprocate (Baland et al. 2016), or a buffer to smooth consumption in the event of crop losses (Fitzsimons, Malde, and Vera-Hernández 2018). In these settings, negative correlation across shocks facilitates risk-sharing while positive correlation inhibits risk-sharing (e.g. Fafchamps (2011)).

But in addition to the correlation structure facilitating risk-sharing, cooperation is necessary for the success of risk-sharing groups. Yet, cooperation often fails in these environments. Members of the family sometimes avoid sharing wealth by taking out loans to feign liquidity constraints (Baland, Guirkinger, and Mali 2011), or try to obscure their true endowments from others in the family (Jakiela and Ozier 2016). When agents interact repeatedly, cooperation is more likely, but hardly a foregone conclusion. While the effect of the correlation structure on risk-sharing is straightforward, its effect on cooperation remains unclear.

The primary question we raise in this study is whether the structure of the correlation across noisy payoffs affects cooperation among subjects. But we are also interested in understanding the mechanisms underlying this effect. One possibility is that correlation strengthens monitoring, thereby allowing subjects to lower inferential error. This lower inferential error could also prompt a change in the strategies used by players.

We investigate these issues in three steps. First, we develop a theoretical framework and generate testable predictions regarding the effect of correlation on inferential error and, ultimately, cooperation. We then test these predictions in a laboratory experiment and examine other mechanisms arising in the experimental setting. Finally, we complement the experiment with simulations based on a genetic algorithm. This allows us to examine which strategies observed in the experiment are likely to survive from an evolutionary point of view.

We build on Bendor, Kramer, and Stout (1991) and Bendor (1993) and develop a framework to formally model behavior in an infinitely repeated prisoner's dilemma with noisy payoffs. We extend this framework by 1) allowing players to choose the benchmark against which a private signal is defined, and 2) allowing shocks affecting subjects' payoffs to be (positively and negatively) correlated. Based on this framework we predict that correlation will strengthen monitoring (that is, will lower inferential errors by subjects) thereby enhancing cooperation.

We test the predictions generated from of our theoretical framework in a laboratory experiment. A laboratory experiment is appropriate because the private information needed to understand how monitoring impacts cooperation is not usually available from observational data. Additionally, and in contrast to field experiments, a laboratory experiment allows us to have full control of the strategic environment. We can exogenously manipulate the correlation structure and prevent communication outside of the strategic environment which allows us to establish clear causality and identify subjects' inferences and strategies within a large set of possible options.

In our experiment, subjects play an infinitely repeated prisoner's dilemma (PD) with a continuation probability of $\delta = 0.9$. In the stage game of the PD, an agent's payoff is affected by a random shock that is uniformly distributed with mean zero. Each player receives a private noisy signal about the realized payoff of the other player in relationship to a benchmark value set by the subject. The subject can combine this information, with information on her own payoff, to infer whether the other agent has deviated or defected. We then expand this by including correlated shocks. We implement four treatments that vary the correlation level from high ($\rho = 0.9, -0.9$) to moderate ($\rho = 0.4, -0.4$), to compare against a baseline case of no correlation ($\rho = 0$).

Our main result is that correlation, either positive or negative, does not improve cooperation relative to the baseline of $\rho = 0$, on average. A closer look at mechanisms clarifies this seemingly puzzling result. Stronger correlation does tend to lower inferential error. But this does not persuade "not nice" subjects (not nice subjects are those that defect in the very first round) to engage in cooperation, unconditional or otherwise. Therefore, in games where a "not nice" player is involved, correlation helps unveil defection which precipitates the unraveling of cooperation. This is confirmed by experimental results which show that, when both players are "not nice", correlation is associated with lower inferential error and lower cooperation. Conversely, and by the same mechanism, when both players are "nice", correlation is associated with lower inferential error and higher cooperation.

Across observations, most of the interactions involve "not nice" players. This fact underpins the muddled relationship we find between correlation and cooperation, on average. We complement our laboratory experiment with a computational experiment based on an evolutionary algorithm to provide further intuition on how cooperation can be maintained overtime under such environment. In this process, certain types of players, those implementing the most successful strategies, are more likely to survive (Axelrod 1980). Indeed, we find that higher degrees of correlation did result in high levels of cooperation.

The rest of the paper is organized as follows. In Section 2, we discuss the nature of our contribution in the context of the broader literature. In Section 3, we present the theoretical background. In Section 4, we give the details of the experimental design. In Sections 5 and 6, we outline the questions our analysis will answer and the main results from our experiment. In Section 7, we present a computational experiment that test behavioral aspects of our experimental design. In Section 8, we conclude with a discussion of our main results.

3.1 Related Literature

We contribute to the literature on cooperation in infinitely repeated PDs when knowledge about past behavior is limited, leading to inferential uncertainties about other players' actions (players are forced to guess their opponents' past behavior). A part of this literature introduces inferential uncertainty by considering noise in the form of implementation error, experimentally and theoretically (Fundenberg and Maskin 1990; Miller 1996; Fudenberg, Rand, and Dreber 2012; Imhof, Fudenberg, and Nowak 2007; Ioannou 2014a, 2014b; Zhang 2018). With implementation errors, there is a probability that the action the players implement is different from the one they intended. This obscures knowledge about past behavior in the sense that players know the action their opponent took but are unsure about their intentions. Furthermore, players are aware of this probability. We can consider this as a signal each player receives about the probability that their opponent actually intended the observed action. This signal delivers information, albeit incomplete. Consequently, players may incorrectly infer the intent of others. Papers in this strand of literature find that incomplete information regarding intent can, though not always , reduce cooperation.

A key feature of the literature on implementation error is that the environment is characterized by imperfect information (where past actions are observable, but the intention is unclear) rather than imperfect monitoring (where past actions are unobservable). This is a subtle, yet important distinction. Both frameworks are appropriate for distinct empirical settings; and they are not observationally equivalent, that is, one does not tend to mimic the other. As pointed out by Ioannou (2014b), imperfect monitoring (which causes individuals to draw incorrect inferences about others past actions) is more detrimental than imperfect information (where individuals are prone to errors in implementing their own actions). This is because implementation errors can introduce cooperative actions even in the presence of unconditional uncooperative strategies (for example, Always Defect), thereby facilitating cooperation. While this would also imply possible deviations from unconditional cooperation strategies (thereby hindering cooperation), these kinds of strategies are not as ubiquitous as their unconditional defection counterparts. In this study, we employ an imperfect monitoring framework because it better captures key features of the empirical settings that motivate our analysis.

We examine the literature on imperfect monitoring in two broad strands. One strand of the literature on imperfect monitoring studies deterministic PDs where the players do not directly observe their opponent's past action, but receive a private signal with a set accuracy about such action (Aoyagi, Bhaskar, and Fréchette 2019; Kayaba, Matsushima, and Toyama 2020). The signal is either good or bad, and a good signal is more likely to occur when their opponent is cooperative. The monitoring accuracy is the probability of receiving the correct signal, and a lower accuracy translates into a higher probability of inferential error (that is, a higher chance that a player will incorrectly guess their opponent's past action). Aoyagi, Bhaskar, and Fréchette (2019) vary the monitoring environment and find that subjects can sustain cooperation under imperfect private monitoring (at rates comparable to perfect monitoring but lower than imperfect public monitoring). Kayaba, Matsushima, and Toyama (2020) vary the accuracy of the signal and find that cooperation increases as monitoring strengthens, that is, as the signal becomes more accurate.

The empirical settings that motivate our study, such as the extended family, collective agrarian societies, and micro-finance groups, involve random shocks that affect subjects' payoffs (for example, weather events, unexpected health issues). Therefore, while the deterministic PD framework employed by Aoyagi, Bhaskar, and Fréchette (2019) and Kayaba, Matsushima, and Toyama (2020) captures the key issue of imperfect monitoring, it does not fit situations where random shocks affecting payoffs constitute the source of imperfect monitoring. The other strand of literature on imperfect monitoring introduces uncertainty regarding past behavior through noise in the form of random payoffs (Bendor, Kramer, and Stout 1991; Bendor 1993). We build on the framework developed by Bendor, Kramer, and Stout (1991) and Bendor (1993), but our analysis differs from those papers in important ways.

First, our primary objective is to understand how correlation across shocks affecting payoffs alters inferential error (monitoring strength) and, ultimately cooperation. We study this because in the empirical settings where payoffs are affected by random shocks, these shocks are often correlated. In many cases, shocks are positively correlated. For instance, in many microfinance institutions, in order to overcome moral hazard and adverse selection, groups are composed of agents living in the same geographic space and probably conducting similar economic activities such as farming. In other cases, shocks are negatively correlated. For instance, in many extended family settings (informal insurance), players engage in fundamentally different activities such as farming and urban employment; activities that are often negatively correlated, or uncorrelated. To better understand cooperation in these settings, we extend the framework in Bendor, Kramer, and Stout (1991) and Bendor (1993) to accommodate correlation across shocks. We then systematically vary the correlation structure and compare inferential errors and cooperation across structures.

Like in Bendor, Kramer, and Stout (1991) and Bendor (1993), players in our framework receive a signal about their opponent's payoff. The signal is defined in relationship to a benchmark value, that is, the signal indicates whether the opponent's payoff is above or below that benchmark. A salient feature of our framework is that we allow subjects in the lab (and automata in the genetic algorithm) to choose the benchmark. The informational value of the signal (the extent to which the signal helps players infer their opponent's action) depends upon where the benchmark is set. Moreover, the correlation structure affects the informational content of the signal, but the degree to which this information is exploited depends on, once more, where the benchmark is set. Therefore, in our framework, the accuracy of the signal is endogenous – it depends upon the subjects' ability to set the benchmark at a level that minimizes inferential error. We now turn to a more formal characterization of our framework.

3.2 Theoretical Background

We study a situation in which two players play an infinitely repeated prisoner's dilemma game. The deterministic payoff is that of a standard prisoner's dilemma (see Table 3.1) where, $T > R > P > S$ and $2R > T + S$. However, for the stage game, the payoff to each player is affected by a uniformly distributed shock. With this shock, the realized payoff (stage game payoff plus random shock) becomes $\hat{T}$, $\hat{R}$, $\hat{P}$ and $\hat{S}$. This payoff is $\hat{X} = X + V$, where $X = \{T, R, P, S\}$, and V is the random shock with mean zero, and it is uniformly distributed between a lower bound V_{LB} and an upper bound V_{UB}. That is, $V_{LB} \leq V \leq V_{UB}$. Two shocks are generated, one for each agent, which we denote by V_1 and V_2. The shocks are independent across rounds but can be positively or negatively correlated between them.

Table 3.1. Deterministic payoff for the prisoner's dilemma

	C	**D**
C	R, R	S, T
D	T, S	P, P

Similar to Bendor (1993), for a range of payoffs, the random shock introduces uncertainty and limits a player's ability to infer their opponent's actions. In this range of payoffs, which we call the region of uncertainty, if a player plays cooperate, it is possible to incorrectly infer that the other player defected when they had in fact cooperated (Type 1 error). Also, they could incorrectly infer that the other player had cooperated when they had in fact defected (Type 2 error). This region of uncertainty exists as long as $S + V_{UB} > R + V_{LB}$. [1]

Figure 3.1 gives the distribution of the realized payoff of player 1 when she cooperates. For any realized payoff to the left of the region of uncertainty, player 1 knows without a

1. ↑If the player plays defect, the region of uncertainty exists as long as $P + V_{UB} > T + V_{LB}$

doubt that they received the sucker payoff, $\hat{S}$ (the payoff is too low to be anything else). And for any region to the right, player 1 knows without a doubt that they received the reward payoff, $\hat{R}$ (conditional on the subject having cooperated, the payoff is too high to be anything else). Within the region of uncertainty, player 1 is unsure and there is a probability $p > 0$ that they will make an incorrect inference about the other player's action. We call this an inferential error. The reason for player 1's uncertainty within this range of payoffs is that two scenarios are probabilistically possible. It is possible that player 2 defected, but that player 1 received a large and positive shock, making the payoff that of a "lucky sucker". It is also possible that player 2 cooperated, but that player 1 received a large and negative shock, making the payoff that of an "unlucky reward".

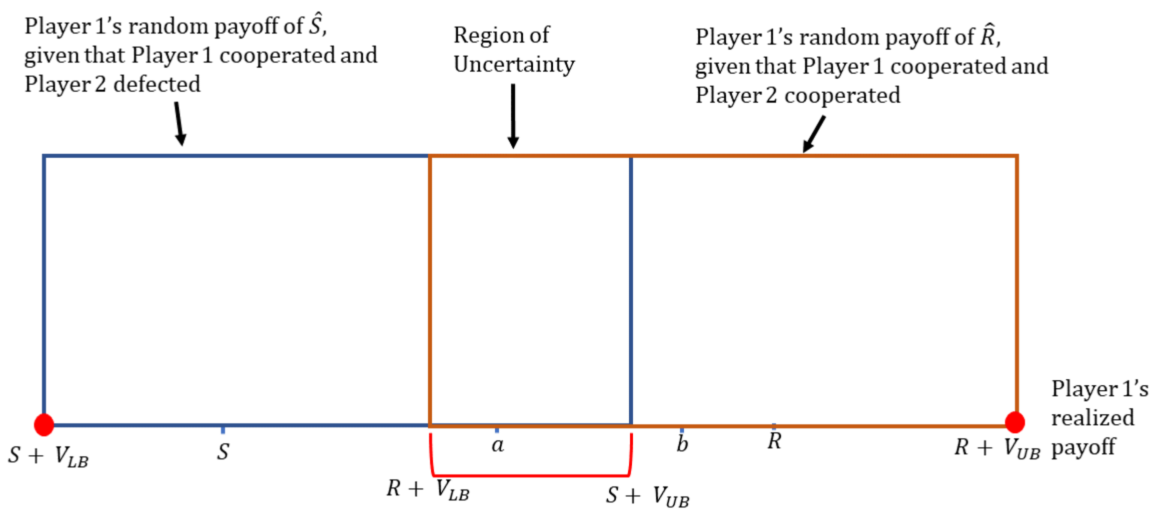


Figure 3.1. Player 1's realized payoff when cooperation is played. The height of the distribution is the probability of the realized payoff $\hat{X}$

We assume that shocks are uniformly distributed. This departs from the treatment in Bendor (1993) which assumed that shocks are normally distributed (see Appendix B.1). To better fit the empirical settings that are of primary interest to us, we introduce correlation across these shocks. This also departs from Bendor (1993) who assumed that shocks are independent across players. We do, however, maintain that shocks are uncorrelated over time. As previously established, when people are engaged in social dilemma type groups, correlation among shocks is the norm, rather than the exception. As such, we conjecture that knowledge of the correlation structure may help reduce a players' inferential error. In particular, we theorize that knowledge of the correlation structure can reduce inferential error by allowing for some degree of monitoring.

To see how this happens, imagine that player 1 cooperates and receives a payoff slightly above $R + V_{LB}$ (see Figure 3.1). That is, she receives a payoff that is low within the region of uncertainty. Because player 1 is in the region of uncertainty, without any other information, it is hard for player 1 to know whether player 2 cooperated and she got unlucky (received a bad shock), or whether player 2 defected and she got lucky (received a good shock). Now, let us assume that player 1 knows that player 2 received a payoff above some benchmark value (Bendor's critical cutoff value from Appendix B.1). For the sake of argument, let this benchmark value be greater than R in Figure 3.1. This indicates that player 2 did well in that they received a high payoff. This could have resulted from two likely situations: (1) player 2 defected or (2) player 2 cooperated and she simply got lucky while player 1 did not. But, if player 1 knows that shocks are positively correlated across players, then a scenario in which player 2 defected is more likely than one in which player 2 cooperated and received a bad shock, while player 1 received a good shock.

We formalize this as follows. Consider two states of nature $\theta \in \{C, D\}$, indicating that the other player has cooperated (C) or defected (D). Each player starts with an uninformed prior that these events are equally likely. Then, each player receives a signal, $s \in \{0, 1\}$ that allows them to update their belief using simple Bayesian techniques. The signal tells a player that the other player's payoff is above or equal to ($s = 0$) or below it ($s = 1$) a benchmark chosen by her. The signal is the imperfect information the player has regarding the payoff of the other.

We now formalize a decision rule using the noisy signal. Assume that player 1 gets a signal that tells her where player 2's realized payoff lies in relation to this benchmark value. She then uses this signal to update her belief on C and D. The simple Bayesian process is outlined in Appendix B.3. The probability that the opponent will be above the benchmark, conditional on the player having cooperated, $P(s = 0/\theta = C)$, is denoted by $\boldsymbol{\pi}_C$. Similarly, the probability that the opponent will be below the benchmark, conditional on the player having defected, $P(s = 1/\theta = D)$, is denoted by $\boldsymbol{\pi}_D$. The signal player 1 receives about player 2's realized payoff gives additional support about $\theta = C$ if $\pi_C > 1 - \pi_D$. Due to symmetry, a similar argument holds for player 2.

Notice that $s = 0$ is equivalent to $P > B$, where P is the other player's realized payoff and B is the benchmark chosen. Similarly, $s = 1$ is equivalent to $P < B$. Therefore, the expression $\pi_C > 1 - \pi_D$ is actually a function of the benchmark chosen by the player. If the player sets a very high benchmark (a value close to the upper bound of the possible realized payoffs of the other player), then $\pi_C < 1 - \pi_D$. In this case, the player will always infer defection regardless of the realized payoff of the other player. In turn, this implies high levels of Type I error and low levels of Type 2 error. Conversely, if the player sets a very low benchmark (a value close to the lower bound of the possible realized payoffs of the other player), then $\pi_C > 1 - \pi_D$ regardless of the other player's realized payoff. Therefore, she will always infer cooperation by her opponent, which in turn implies high levels of Type 2 error and low levels of Type I error. In both of these cases, inference will be incorrect close to half of the time. Therefore, if the benchmark is set close to the upper or lower bound of the other player's possible realized payoffs, the signal regarding the other player's realized payoff contains very little information and may not lead to better inference.

On the other hand, if the player sets a more intermediate level for the benchmark, then Type I and 2 errors will be balanced (similarly frequent), and overall errors will be minimized. But the level will depend on how much of an overlap there is with these two distributions. These distributions and consequently their overlaps are shaped by both the correlation between shocks and the actions of player 1. This is illustrated numerically in Appendix B.2 and B.3. In Figures B.1 and B.2 we simulated the realized payoff for player 2, for different combinations of player 1's realized payoff (within the region of uncertainty) and correlation between shocks, conditional on player 1 cooperating.

As shown by the numerical simulations in Figures B.1 and B.2 a higher correlation (either positive or negative) between shocks shifts the distributions apart, thereby reducing inferential errors, all else constant. This is a mechanical effect and is independent of where the player sets the benchmark based on which the signal is defined. Wherever the benchmark is set, if player 1 infers defection (cooperation) when player 2's payoff is above (below) the benchmark, higher correlation between shocks will make it more likely to that this is in fact true.

But the degree to which correlation translates into a reduction in inferential errors also depends upon where player 1 sets the benchmark. If player 1 sets the benchmark at the payoff where both distributions intersect and correlation is high, the signal will convey information highly indicative of player 2's actions which results in low inferential error and, moreover, a situation where Type I and 2 errors are equally likely. In other words, by choosing the correct benchmark, player 1 can, with a given signal, refine her Bayesian updating of the prior inference regarding player 2's actions. Therefore, there is also a behavioral channel through which higher correlation reduces inferential error. Higher correlation reduces inferential error the most when the agent has the ability to choose the right benchmark based on which the signal is defined.

We illustrate the effect of correlation on inferential error in Figures B.3 and B.4 in Appendix B.3, where we present the probability distributions of player 2's realized payoffs. We assume that, after the signal, Bayesian updating proceeds based on the benchmark value that equates Type 1 and Type 2 errors (from Player 2's distribution) when $\rho = 0$. In Table B.1 in Appendix B.2 we present Type 1 and Type 2 errors along with $\boldsymbol{\pi}_C$ and $\boldsymbol{\pi}_D$ using these benchmark values. Using Bayesian updating we put forward a simple decision rule for player 1. If $\rho \geq 0$ and player 2's realized payoff is above the benchmark value, assume that player 2's mostly likely action was defection. Likewise, if player 1 is signaled that player 2's realized payoff is below the benchmark value, assume that their most likely action was cooperation. The opposite holds for $\rho < 0$. Simulations reported in Table B.1 show that higher correlation, under this choice of benchmark, translates into a significant reduction of both type I and 2 errors, but the reduction is larger when correlation is positive.

We hypothesize that players in an infinitely repeated prisoner's dilemma will use the signal more effectively when correlation is higher, that this will strengthen monitoring between players (by reducing inferential error), and that this will in turn enhance cooperation. To test these hypotheses we implemented an experiment, which we now proceed to discuss.

3.3 Experimental Design

The experiment is designed to test if a stronger correlation between shocks affecting players' payoffs reduces inferential error and by extension fosters cooperation in an infinitely repeated prisoner's dilemma. To induce the infinitely repeated game, subjects were informed that after each round, there was 0.9 probability that a supergame will continue for another round. We pre-drew the random game length of each supergame to ensure that in each session, each supergame lasted for same number of rounds. For each treatment, subjects played 84 rounds over 10 supergames (See Table 3.2 for the treatment summary).[2]

Table 3.2. Treatment, sessions, and subjects in the experiment

Treatment	No. of Sessions	Total Subjects
$\rho = 0.9$	3	34
$\rho = 0.4$	3	36
$\rho = 0$	3	36
$\rho = -0.4$	3	36
$\rho = -0.9$	3	36

Table 3.3 shows the stage game of the prisoner's dilemma, denoted in points. In each round, each subject's payoff was affected by random, uniformly distributed shock in the range [-24, 24]. Subjects were only told their realized payoff (payoff inclusive of random shock faced) and the correlation level between their random shock and the random shock of the other player. All these details were included in the instructions that the subjects read on their computer monitors at the beginning of each session. The subjects also had access to the same instructions in written form throughout the entire session. An example of these instructions is included in the Appendix A.2. We implemented five treatments: a very high positive and negative correlation ($\rho = 0.9$ and $\rho = -0.9$), moderate positive and negative correlation ($\rho = 0.4$ and $\rho = -0.4$) and the baseline case of no correlation ($\rho = 0$).

Before each supergame, each subject selects two benchmark values, one to be used if they select cooperate and the other if they select defection. The benchmark values were restricted to the range of possible realized payoffs of their opponent. That is, subjects were

2. ↑Across all 5 treatments, subjects made a total of 14,952 decisions.

Table 3.3. Payoff of the stage game

	C	**D**
C	48, 48	13, 60
D	60, 13	25, 25

restricted to the range [24, 84] if they choose cooperation and a range of [-11, 49] if they choose defection. To assist subjects in understanding how the benchmark values work, we included an interactive feature in the instructions. This is a simulation in which the subject and the computer simultaneously make a choice, then the subject receives a feedback on their realized payoff. Also, there is a slider that allows subjects to play around with setting different benchmark values to see what feedback they will receive (above, below, or equal to) about the other player's realized payoff.

At the beginning of each round, subjects choose between cooperation and defection (in the experiment, we used neutral language of "A" and "B", instead of "Cooperate" and "Defect"). Immediately after this choice they receive feedback on the resulting realized payoff and also a private signal about the realized payoff of the other subject. This signal tells if the other subject's realized payoff is above, equal to, or below the benchmark value each subject selected at the beginning of each supergame.

In our design, we opted to allow subjects to select their own benchmark to mimic noisy signals in real-world group interactions. In real-world groups, for example the extended family or other similar settings, individuals use spending habits of others to determine their well-being (Baland, Guirkinger, and Mali 2011; Jakiela and Ozier 2016). Individuals may vary on the thresholds above which they infer defection. For example, an individual may consider that the other person's paying rent or debts is sufficient proof that they are shirking on risk-sharing agreements. In contrast, others may set a higher bar and consider traveling or similar bigger spending event as sufficient proof of defection.

After each round, we also used a Binarized Scoring Rule (BSR) to elicit incentivized beliefs from each player about the actions of the other player. This was necessary to help us estimate inferential errors and also to estimate the strategies that are being played by subjects across supergames. In each round, after subjects make a decision and their realized payoff is revealed, they were asked how likely they believed that the other subject cooperated. The BSR is incentive compatible in that, as long as subjects prefer getting a reward as opposed to no reward, to maximize the probability of getting the reward, the best action is for them to truthfully report their beliefs about the other subject's action (Hossain and Okui 2013). In our context, the reward was an additional 2 points. That is, over 84 rounds, a subject could earn a maximum of 164 additional points for truthfully reporting their beliefs about the likely action of the other player. The BSR is also independent of risk attitudes and whether the subject is an expected utility maximizer or not. In Appendix A.1, we describe the belief elicitation process. In the instructions, we did not give subjects the full details of the belief elicitation process. They were informed that the details were available after the session.[3] This design feature is consistent with the results of Danz, Vesterlund, and Wilson (2020), who, in an experiment using BSR, found that transparent information on incentives gave rise to error rates in excess of 40%.

On the decision screen for each round, each subject saw a summary of the decision and outcome from the previous round. Also, there was a brief summary of the correlation structure for the treatment. There was also a reminder of the two benchmark values that they selected. At the end of a supergame, before they are randomly rematched, subjects received a detailed account of the actions they made and their realized payoffs, as well as the actions and payoff of the other subject. See Appendix A.4 for a screenshot of this.

A total of 178 subjects participated in 15 sessions at Purdue University's Vernon Smith Experimental Economics Laboratory (VSEEL). Each treatment had either 10 or 12 subjects and lasted for 90 minutes.[4] Subjects accumulated points during each session, and these were converted at an exchange rate of \$1 = 300 points. On average, subjects earned \$21.58 including a show-up fee of \$5. We used a between subject design, where each subject participated in only one session. For the session, subjects first read the on-screen instructions, then they completed quiz. Following this, they played five unpaid practice rounds against the computer. For additional practice in setting the benchmark, in the practice rounds subjects

3. ↑No subject asked for this information after the session.
4. ↑COVID-19 regulations stipulated a maximum of 13 subjects per session. For some sessions, we had no-shows that resulted in only 10 subjects per session.

were allowed to select the benchmark after each round. But for the paid repeated games, the benchmark values were set at the beginning of each supergame. They were informed of all of this in the instructions at the beginning of each session. For the paid repeated prisoner's dilemma, after each supergame, subjects were randomly rematched with another subject in the room.[5] The experiment was programmed in oTree (Chen, Schonger, and Wickens 2016).

3.4 Questions and Predictions

Based on the theoretical analysis in Section 3, numerical simulations (reported in Appendix B and discussed in Section 3), and insights from previous literature, we predict that a stronger correlation between shocks that affect players' payoffs, fosters cooperation. We further hypothesize that correlation fosters cooperation by reducing inferential error, that is, by strengthening monitoring. With lower inferential error, we theorize that subjects will depend on more lenient strategies with stronger correlation. We start by stating our primary question:

Question 1: *Is cooperation higher with stronger correlation?*

Based on insights from previous studies (Kayaba, Matsushima, and Toyama 2020) we make the following prediction:

Prediction 1: Cooperation will increase with stronger positive correlation across shocks. The effect of stronger negative correlation is non-monotonic and, thereby, ambiguous. In other words, stronger positive correlation will foster cooperation, but stronger negative correlation may foster or hinder cooperation.

We now turn our attention to the mechanism underlying Prediction 1. First, we focus on the link between correlation and monitoring strength or, in other words, inferential error:

Question 2: *Is inference error lower with stronger correlation?*

We will focus on the degree of inferential error across the various correlation structures. This includes total error rate, as well as Type 1 and Type 2 errors. From our numerical simulations based on Section 3 (see Appendix B), we predict that the correlation structure will affect inferential errors through a mechanical channel and a behavioral channel. We first

5. ↑This was not a perfect random rematching. There was a possibility that a subject could be re-matched with someone they played with in a previous supergame.

focus on the mechanical channel, whereby a stronger correlation reduces inferential error even without the subjects using the signal to update their beliefs about the other player's action. Based on our theoretical analysis, we make the following predictions:

Prediction 2: Stronger positive correlation reduces inferential errors, thereby improving the ability of subjects to monitor each other. It does so by lowering Type 1 and 2 errors.

Prediction 3: The effect of negative correlation on inferential error is non-monotonic. Weak negative correlation raises inferential error (both Type 1 and 2), but strong negative correlation reduces it (both Type 1 and 2). Therefore, as negative correlation becomes stronger, it first impairs and then improves the ability of subjects to monitor each other.

Predictions 2 and 3 constitute one channel through which correlation affects cooperation in the way described by Prediction 1. This follows from previous studies, which suggest that stronger monitoring (lower inferential error) has a positive effect on cooperation (Kayaba, Matsushima, and Toyama 2020). These findings suggest that positive correlation is likely to foster cooperation because it lowers inferential error. Our simulations suggest that, in turn, negative correlation has an ambiguous effect on cooperation because it has an ambiguous effect on inferential error.

We now discuss the behavioral channel, whereby subjects can use a combination of the correlation structure, and a noisy private signal about their opponent's payoff to update their beliefs on the likely action of their opponent. The noisy signal is determined relative to a benchmark set by the player. The benchmark is some payoff on the domain of the opponent's payoffs. As portrayed in Figures B.1 and B.2, conditional on a player's action, payoff received, and a correlation structure, there are two payoff distributions for her opponent: one when the opponent cooperates, and one when she defects. These two distributions can overlap; the overlap becomes smaller as the correlation between shocks strengthens and eventually disappears when correlation is sufficiently strong. If the benchmark is set at the center of the overlapping region, Type I and II errors are roughly equalized, and the total inferential error is minimized. Based on the patterns of the overlapping distributions (e.g., Figures B.3 and B.4), when shocks are positively (negatively) correlated, the opponent is likely to have cooperated (defected) if her payoff is above the threshold. Therefore, we make the following predictions regarding the players' inferential process:

Prediction 4: Subjects will set a benchmark near the center of the overlapping region of the opponent's payoff distributions.

Then, if the player's payoff falls within her region of uncertainty, she will use the following decision rule: when $\rho \geq 0$, a signal that the other player is above the benchmark value supports the inference that the other player defected in the previous round. And, when $\rho < 0$ such a signal supports the inference that the other player cooperated.

In summary, we predict that positive (negative) correlation will reduce (have an ambiguous impact on) inferential errors regardless of the player's inferential process (Predictions 2 and 3). But we also predict that players will adjust their inferential process to minimize total inferential error and equalize Type I and Type II errors (Prediction 4). If Prediction 4 is correct, the players can more effectively use the information contained in the signal, conditional on the correlation. In other words, correlation is more likely to reduce inferential error if players follow the inferential process described in Prediction 4.

We now look at the effect of correlation on the type of strategies used by subjects, which will influence the prevalence of cooperation. We raise the following question:

Question 3: *Do subjects play more lenient strategies when random shocks are correlated?*

Drawing on conjectures in Aoyagi, Bhaskar, and Fréchette (2019), we make the following prediction about dominant strategies under alternative correlation structures:

Prediction 5: If a player thinks, with more confidence, that his opponent cooperated, they will be more lenient. This is to reduce the likelihood of retaliation. Combining Predictions 1-4, we expect that strong positive correlation will lead subjects to employ more lenient strategies.

3.5 Results

To analyze the results, we first examine the link between the correlation structure and cooperation. This allows us to test Prediction 1 and, ultimately, offer an answer to Question 1 in Section 5. We then turn to the mechanisms through which correlation affects cooperation. First we examine the relationship between the structure of correlation and inferential error rates, which allows us to test Predictions 2-4 and answer Question 2 in Section 5. Finally, we test Prediction 5 regarding the link between the structure of correlation and strategies played by subjects, which allows us to address Question 3 in Section 5.

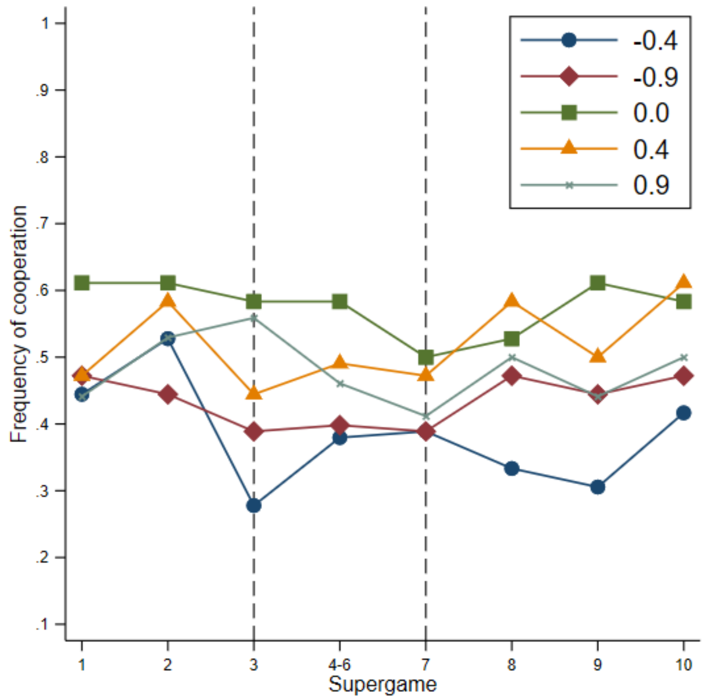
3.5.1 Correlation and Cooperation

Figure 3.2 shows the evolution of cooperation for all shock correlation structures: uncorrelated ($\rho = 0$), strong negative correlation ($\rho = -0.9$), moderate negative correlation ($\rho = -0.4$), moderate positive correlation ($\rho = 0.4$), and strong positive correlation ($\rho = 0.9$). Average cooperation measures the proportion of rounds in which subjects cooperated in a supergame. We report results on cooperation for different correlation structures over subsequent supergames. We compare cooperation in the first stage of each supergame (Figure 3.2a) with cooperation in over all stages of those supergames (Figure 3.2b).

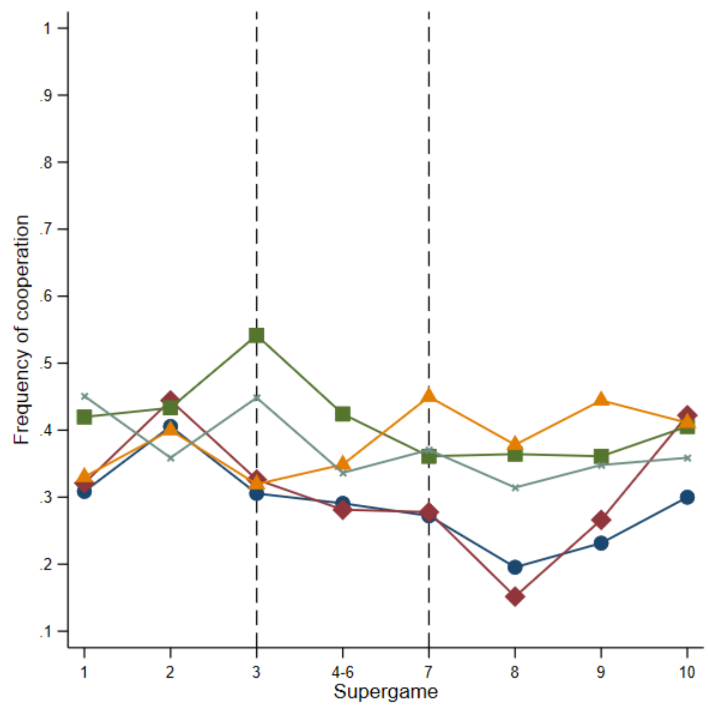
There is some level of cooperation for all correlation structures. A comparison between the top and bottom panels shows that cooperation seems higher in the first round than in subsequent rounds of a supergame, indicating that cooperation tends to unravel towards later rounds of the supergame. The evolution across supergames when all rounds are considered (Figure 3.2b) show a decline, albeit weak, in cooperation. Results in Figure 3.2 show that strong positive or negative correlation does not seem to induce more cooperation, relative to the baseline of no correlation. A pattern that contradicts our Prediction 1.

We report more formal measures of cooperation patterns across treatments (correlation structures) in Table 3.4. Once more, to better understand the evolution of cooperation, we examine cooperation in supergame 1, early stage supergames (1-3) and late stage supergames (7-10). For each block of supergames, we further disaggregate the results by first round and all rounds. Results in Table 3.4 confirm that, on average, cooperation is lower under strong correlation (both positive and negative) than when shocks are uncorrelated. They also confirm that cooperation generally unravels across rounds within a supergame and over subsequent supergames. The statistical significance reported in Table 3.4 refers to whether cooperation is statistically significantly different from zero.[6]

6. ↑Unless otherwise stated, statistical significant is established by using a probit regression where errors are clustered at the session level.



(a) Round 1 Only



(b) All Rounds

Figure 3.2. Frequency of cooperation across supergames for all correlation structures

Table 3.4. Average Cooperation

	Supergame 1		Supergame 1-3		Supergame 7 - 10	
Rounds:	First	All	First	All	First	All
-0.9	0.472**	0.321***	0.435**	0.356***	0.444**	0.222**
	(0.056)	(0.003)	(0.061)	(0.02)	(0.062)	(0.043)
-0.4	0.444***	0.309***	0.417**	0.335**	0.361	0.223**
	(0.028)	(0.027)	(0.085)	(0.05)	(0.135)	(0.046)
0	0.611**	0.420**	0.602***	0.451***	0.556**	0.367**
	(0.073)	(0.064)	(0.037)	(0.040)	(0.092)	(0.057)
0.4	0.472***	0.330*	0.500***	0.347**	0.542**	0.406***
	(0.028)	(0.086)	(0.042)	(0.038)	(0.064)	(0.019)
0.9	0.441***	0.451***	0.510**	0.425***	0.463*	0.333
	(0.031)	(0.023)	(0.068)	(0.036)	(0.143)	(0.156)

Notes: Robust standard errors (in parenthesis) are clustered at the session level. * Indicates statistical significance at the 10% level (0.05 < p-value < 0.1). ** Indicates statistical significance at the 5% level (0.01 < p-value < 0.05). *** Indicates statistical significance at the 1% level (p-value < 0.01)

Results in Table 3.4 do not offer information on the difference in cooperation rates across correlation structures. We present such information in Table 3.5. Table 3.5 shows the difference between cooperation with correlation and cooperation without correlation, as well as whether such differences are statistically significant. We report differences in cooperation rates for all rounds. Results in Table 3.5 show that negative correlation hinders cooperation, while positive correlation neither fosters nor hinders cooperation. The results are in stark contrast to Prediction 1.

Given that our results indicate some learning over subsequent supergames, we focus on results from the last four supergames. These figures yield the main result of this paper, which answers Question 1 in Section 5:

Result 1: *On average, correlation does not improve cooperation.*

The average results conceal substantial heterogeneity across supergames. As discussed by Ostrom in her review of the literature (Ostrom 2000) a key force influencing cooperation

Table 3.5. Difference in Average Cooperation (All Rounds)

	Supergame 1	Supergame 1-3	Supergame 7 - 10
-0.9	-0.099*	-0.094**	-0.146**
	(0.054)	(0.038)	(0.060)
-0.4	-0.111*	-0.116**	-0.144**
	(0.059)	(0.055)	(0.062)
0.4	-0.089	-0.103**	0.038
	(0.091)	(0.047)	(0.051)
0.9	0.031	-0.026	-0.034
	(0.058)	(0.046)	(0.1403)

Notes: This table gives the difference in average cooperation for each treatment from no correlation. Robust standard errors are in parentheses and are clustered at the session level

in social dilemmas is the type of players involved in them and, specifically, the players' willingness to engage in reciprocity that would lead to conditional cooperation. To investigate this in the context of our experiment, we tagged players in a way that is indicative of their willingness to initiate cooperation: whether they cooperated in the first round of each supergame or not. If a player cooperated in round 1, we labeled them as "nice" and they were labeled as "not nice" otherwise.[7]

Using our classification, we calculated the cooperation rate across supergames $7-10$ for interactions between: (1) two nice players ('nice-nice'), (2) two not nice players ('not-not'), and (3) a nice player and a not nice player ('nice-not'). Results are reported in Table 3.6. From these results we can see that when players are nice, positive correlation fosters cooperation from the baseline of $\rho = 0$ ($\rho = 0.9$ $p-value = 0$; $\rho = 0.4$ $p-value > 0.1$). This is consistent with Prediction 1. This is not true when at least one not-nice player is present. And when both players are not-nice, correlation hinders cooperation ($\rho = -0.9$ $p-value > 0.1$; $\rho = -0.4$ $p-value > 0.05$).

7. ↑We borrow this language from Bendor, Kramer, and Stout (1991), but our definition differs slightly from his. Bendor classified subjects as not-nice if they defected without a cause, i.e., without believing that their partner defected. However, we define not-nice as defecting in the very first round.

Table 3.6. Cooperation by Niceness for Supergames 7 - 10

Treatment	Nice-Nice	Not-Not	Nice-Not
-0.9	0.581**	0.079*	0.175
	(0.097)	(0.020)	(0.069)
	N = 308	N=458	N=962
-0.4	0.497*	0.154***	0.186**
	(0.066)	(0.005)	(0.020)
	N = 296	N=846	N=586
0	0.696**	0.095*	0.282***
	(0.112)	(0.028)	(0.027)
	N = 504	N=326	N=898
0.4	0.726**	0.145	0.178*
	(0.074)	(0.068)	(0.042)
	N = 752	N=566	N=410
0.9	0.955***	0.024**	0.244
	(0.009)	(0.005)	(0.112)
	N = 332	N = 410	N=890

Notes: Robust standard errors are in parentheses and are clustered at the session level

The results reported in Table 3.6 warrant a more qualified characterization of the relationship between correlation across shocks and cooperation. We provide such qualification in the following statement:

Result 1': *When agents are nice (i.e., when agents are prone to cooperation in the initial rounds of the supergame), positive correlation fosters cooperation while negative correlation does not. When agents are not nice, strong correlation (either positive or negative), hinders cooperation.*

We now turn to the mechanisms underlying the link between correlation and cooperation to further elucidate the reasons why our Prediction 1 only seems to hold when players are nice.

3.5.2 Correlation and Inferential Error

In the previous section we reported evidence that, on average, higher levels of correlation does not foster higher levels of cooperation. We now further investigate the channel through which we anticipated the correlation structure influencing cooperation rates.

We used beliefs elicited from subjects using the BSR to estimate how inference error evolves from early supergames to later supergames. In our framework, after each round, subjects selected the probability with which they believed that the other player had cooperated. If they indicated a probability greater then 0.5, we assigned their belief to cooperation (we assume they infered cooperation). For probabilities less than 0.5, we assigned their belief to defection. We interpret a probability of 0.5 as the subject giving equal weighting to the probability to cooperation and defection. As such, we randomly assigned the subject's belief to cooperattion or defection with a 0.5 probability.[8] An inferential error occurs when there is a mismatch between the subject's belief about their opponent's action and the actual action taken by their opponent.

Table 3.7 shows the inferential error rates across correlation structures. Given that subjects do make errors outside of the region of uncertainty, we present the error rate across all actions ("All errors") and the error rate when the player's payoff falls within the region of uncertainty ("ROU errors"). We further disaggreated "All error" into Type 1 (incorrectly infering defection) and Type 2 error (incorrectly infering cooperation), and across early supergames (1-3) and late supergames (7-10).

As indicated in Table 3.7, subjects incur inferential errors across treatments and supergames (virtually all error rates are statistically significantly different from zero). They also incur errors when their payoff falls outside of the region of uncertainty. Given that subjects did incur errors outside of the region of uncertainty, albeit small relative to the region of uncertainty on average (8% outside versus 32% within the region of uncertainty), we focus on the total inferential error; that is, both within and outside of the region of uncertainty. Overall, across all treatments, inferential error is statistically greater than zero. However,

8. ↑There were 439 such observations (out of a total of 14,952). After the random assignment, 50.57% of these were assigned as defection. Also, 27.3% of the observations fall within the region of uncertainty.

Table 3.7. Inferential Error Rates

	Supergame 1-3				Supergame 7-10			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	All Errors	ROU Errors	All Type 1	All Type 2	All Errors	ROU Errors	All Type 1	All Type 2
-0.9	0.20**	0.41***	0.08**	0.12**	0.13*	0.28*	0.04*	0.09*
	(0.03)	(0.006)	(0.009)	(0.023)	(0.032)	(0.067)	(0.010)	(0.023)
-0.4	0.22***	0.44***	0.07***	0.15***	0.16**	0.34**	0.05**	0.11**
	(0.004)	(0.025)	(0.003)	(0.007)	(0.030)	(0.044)	(0.010)	(0.025)
0	0.17**	0.38**	0.097	0.07***	0.15**	0.32**	0.04**	0.11*
	(0.039)	(0.061)	(0.039)	(0.005)	(0.029)	(0.054)	(0.005)	(0.026)
0.4	0.20***	0.36**	0.07**	0.12**	0.15***	0.33***	0.06**	0.10**
	(0.011)	(0.038)	(0.012)	(0.020)	(0.015)	(0.019)	(0.008)	(0.021)
0.9	0.16*	0.30**	0.07*	0.08	0.06**	0.17**	0.02**	0.04**
	(0.051)	(0.036)	(0.020)	(0.035)	(0.010)	(0.025)	(0.004)	(0.007)

Notes: Robust standard errors are in parentheses and are clustered at the session level inferential error is lower in late supergames than in early ones, which suggests learning by the subjects. Consequently, we base our discussion on results from later stage supergames where subjects have gained some experience.

Inferential error was smallest under $\rho = 0.9$ (column 5 of Table 3.7), which is consistent with Prediction 2. The link between inferential error and negative correlation is non-monotonic. Moderate negative correlation raises inferential error (second row of column 5) and strong negative correlation reduces it (first row of column 5) relative to the baseline of no correlation. This is also consistent with Prediction 3. Therefore, strong correlation (either positive or negative, but especially the former) lowers inferential error.

To examine whether these differences in inferential error rates across treatments are distinguishable from chance, we calculated the difference between inferential error under each treatment (positive and negative correlation) and inferential error under the baseline (no correlation), as well as the standard deviation and statistical significance of these differences. Results are reported in Table 3.8. The results indicate that the effect of (strong) positive correlation on inferential error is distinguishable from chance, while the effect of negative correlation is not.

Table 3.8. Differences in Total Inferential Error (Supergames 7-10)

	Difference in error (SD)
-0.9	-0.020
	(0.037)
-0.4	0.015
	(0.037)
0.4	0.004
	(0.278)
0.9	-0.084***
	(0.026)

Notes: This table gives the difference in total error rates for each treatment relative to no correlation. Robust standard errors are in parentheses and are clustered at the session level

Negative correlation is less effective in strengthening monitoring (that is, reducing inferential error) than positive correlation because it expands the opponent's domain of possible payoffs conditional on their actions, making it harder for a player to infer those actions (see distributions of player 2's realized payoffs in Appendix B.3). To illustrate this, consider the following situation. If two players' payoffs are positively correlated, and player 1 received a low payoff while player 2 received a high payoff, player 1 will have more confidence that the most likely action of player 2 was defection than if these payoffs were negatively correlated. If the payoffs are negatively correlated, this same situation could arise for a variety of reasons. It could be that player 1 received the sucker payoff. Or it could be that player 2 had in fact cooperated but received a positive shock, while player 1 received a negative shock – a likely combination given that shocks are negatively correlated. This makes inference harder and raises the likelihood of inferential error. This is encapsulated in the following statement:

Result 2: *Stronger positive correlation reduces inferential error.*

The strength of monitoring itself, as measured by total inferential error, matters for cooperation. But this is not the only dimension of inferential error that matters. The composition of total inferential error is also crucial. As discussed in the development of Prediction 4, conditional on a player's action, payoff received, and a correlation structure, there are two payoff distributions for her opponent: one when the opponent cooperates, and one when she defects. If these distributions overlap, the location of the benchmark within the overlapping region will determine the prevalence of Type I and II errors. This matters because if the benchmark is set at a level where most errors are Type I (Type II) errors, then the player will be less (more) likely to cooperate, perhaps inducing her opponent to defect (cooperate) more often. A high prevalence of Type I (Type II) errors will hinder (foster) cooperation. Given its importance for the level and composition of inferential errors, we now examine the extent to which players use the inferential process outlined in Prediction 4.

When a player cooperates, the center of the overlapping region of her opponent's payoff distributions is around 54. And when she defects, it is around 19. Therefore, in line with Prediction 4, we expect that those are the payoffs the player will choose as benchmark values. While roughly in line with our Prediction (especially when considering variation across players), players tend to choose a lower value (on average) when they cooperate (46 with a standard deviation of 14) and a higher value (on average) when they defect (25 with a standard deviation of 12.5).

As also stated in Prediction 4, under the Bayesian updating rule outlined in Section 3, with positive (negative) values of ρ, subjects will more likely assume that their opponent defected (cooperated) if they are signaled that their opponent's payoff is above their benchmark value. For negative ρs, the opposite applies. We find little evidence that subjects are updating their beliefs based on their private noisy signal. This rule explains subjects' actions around 50% of the time, on average, across all treatments. The performance of this rule does not improve between supergames 1-3 and 7-10.

These results suggest a very limited role for the behavioral channel (agents use the signal to refine their inference) in the link between correlation and inferential error. Consequently, our results indicate that the reduction in inferential errors under stronger correlation is mostly driven by the mechanical channel, whereby correlation reduces inferential error without the subjects using the signal to update their beliefs. We summarize this in the following statement:

Result 2: *Subjects appear not to use their private signal to update their beliefs.*

Having found little evidence in favor of our predicted Bayesian updating process, we explore alternative ways in which subjects could be updating their beliefs. We explored a simple rule where subjects are more likely to assume cooperation if their own payoff is high enough and defection otherwise. Table 3.9 shows the average payoff within the region of uncertainty when subjects assume that the other player is defecting or cooperating.

Table 3.9. Average Payoff when Assuming Cooperation or Defection

		Assumption	
		C	D
Action	C	31.2/31 (3.56)	42.9/43 (3.72)
	D	29.6/29 (3.68)	41.9/42 (3.76)

Notes: This table shows the mean/median payoff of player when they assume cooperation or defection of the other player. Standard deviation is in parenthesis.

The payoffs in Table 3.9 approximately coincide with the midpoint of the region of uncertainty of the player making the inference, as opposed to the center of the overlapping region of the opponent's payoff distributions. The midpoints are 30.5 when cooperation is played and 42.5 when defection is played. This suggests the following inference rule. A subject will most likely assume cooperation if their payoff is substantially (more than 1 standard deviation) above the midpoint of their own region of uncertainty. Alternatively, a subject will most likely assume defection if their payoff is substantially (more than 1 standard deviation) below the midpoint of their own region of uncertainty.

However, if the player's payoff is within 1 standard deviation (above or below) of the midpoint of their own region of uncertainty, they will most likely make the same assumption about the other player's action as they did in the previous period. That is, there is a grey area where players will make inference based on how the history of play (or propensity to give others the benefit of the doubt if there is no history of play) shaped their perception of the other player's type (either good or bad). We examine the extent to which this inferential rule (as opposed to the one advanced in Prediction 4) can rationalize inference drawn by subjects in the laboratory. Results of our analysis are reported in Table 3.10. This simple decision rule matches the data, on average, 88% of the time. For supergames 1-3, the match is 85%, and this increases to 90% in supergames 7-10. The match is high across treatments, which lends credence to this inferential process.

Table 3.10. Percentage Match of Simple Updating Rule

	All		Supergame 1-3		Supergame 7-10	
	Mean	SD	Mean	SD	Mean	SD
-0.9	0.885	0.319	0.858	0.349	0.903	0.296
-0.4	0.865	0.342	0.824	0.381	0.881	0.324
0	0.891	0.311	0.886	0.318	0.890	0.313
0.4	0.867	0.340	0.830	0.376	0.891	0.311
0.9	0.908	0.290	0.859	0.348	0.927	0.260

3.5.3 Correlation and Strategies Used by Subjects

In this section, we examine the relationship between correlated shocks and the nature of strategies used by players. Note that this mechanism can also be distinct from the effect of correlation on the inferential process. This is because a change in the correlation structure may prompt players to follow a more (or less) lenient strategy, conditional on whatever they infer about their opponent's actions.

In Section 5, we predicted that a stronger positive correlation would raise the confidence with which players make inference and make them prone to employ more lenient strategies (Prediction 5). To elicit the most likely strategies played by subjects in the laboratory, we used the Strategy Frequency Estimation Method (SFEM) of Dal Bó and Fréchette (2011). In other words, we use a maximum likelihood estimation (MLE) approach to calculate the frequency of each strategy under consideration along with β that gives the model fit (see Appendix A.5 for a detailed discussion). For this, we considered a subset of eight strategies from the twenty outlined in Fudenberg, Rand, and Dreber (2012). In Appendix A.3, we give a description of each. The strategies we consider consist of the eight top strategies identified by SFEM from data in Dal Bó and Fréchette (2018) (see Gill and Rosokha (2020)). The description of each strategy is similar to Fudenberg, Rand, and Dreber (2012).

We classified strategies into three categories: lenient, provocable and unfriendly. A lenient strategy (TF2T, AC, GRIM2) starts with cooperation and does not defect immediately when the opponent first defects. Provocable strategies (GRIM, 2TFT, TFT) start with cooperation but immediately defect when the opponent first defects. Unfriendly strategies (AD, DTFT) defect in the first round. The MLE estimation identifies that most prominent strategies and results are reported in Table 3.11. We focus our analysis on supergames 8-10, where subjects would have gained experience. Subjects predominantly used simple, memory-1 strategies. With memory-1 strategies, subjects only respond to their opponent's action from the very last round. Memory-1 strategies include AD, DTFT, GRIM, TFT and AC.

Across all treatments, and on average, AD was the most frequent strategy, accounting for about half of the strategies used. Note that, the sum of the two most prominent provocable strategies, TFT and GRIM, is actually higher than AD when $\rho = 0$. Yet, AD becomes

Table 3.11. Estimation of Strategies Used

	Unfriendly		Provocable			Lenient			
Correlation	AD	DTFT	GRIM	2TFT	TFT	GRIM2	TF2T	AC	Beta
0	0.38***	0.05	0.25***		0.16**	0.06	0.07	0.03	0.91***
	(0.119)	(0.042)	(0.101)		(0.088)	(0.07)	(0.075)	(0.05)	(0.021)
-0.4	0.57***	0.09	0.10*		0.10	0.13*	0.02		0.87***
	(0.131)	(0.079)	(0.068)		(0.084)	(0.087)	(0.028)		(0.028)
-0.9	0.52***	0.02		0.12*	0.15*	0.12*	0.05	0.03	0.91***
	(0.087)	(0.029)		(0.074)	(0.095)	(0.082)	(0.06)	(0.045)	(0.026)
0.4	0.38***	0.03	0.19**	0.02	0.08	0.06	0.24***		0.90***
	(0.099)	(0.048)	(0.086)	(0.051)	(0.072)	(0.098)	(0.093)		(0.026)
0.9	0.47***	0.04	0.11**		0.15**	0.08	0.08	0.07	0.92***
	(0.147)	(0.061)	(0.061)		(0.081)	(0.094)	(0.111)	(0.057)	(0.023)

Notes: Bootstrapped standard errors are in parenthesis. Strategies that are 0.0 are dropped. *** p<0.01, ** p<0.05, * p<0.1

more prominent (even more than the sum of provocable strategies) when correlation got stronger. This rejects our Prediction 5 that subjects will play more lenient strategies as positive correlation becomes stronger. This result, however, is in line with the fact that cooperation was, on average, lower under stronger correlation. Also, in line with cooperation patterns, AD is more prominent under negative correlation. These results are summarized in the following insight.

Result 4: *Subjects played mostly unfriendly strategies, regardless of the correlation structure.*

Results 1-4 offer mixed evidence regarding our predictions. Predictions 2 and 3 are supported by the data, while Prediction 4 is not. And Prediction 1 is rejected, but a qualified version of it is supported by the data (Result 1'). In the next section, we combine these pieces of evidence to better elucidate the nature of the relationship between correlation and cooperation.

3.5.4 Mechanisms and the Effect of Correlation on Cooperation

A crucial aspect of our results is that a strong positive correlation leads to enhanced monitoring on average, as revealed by lower inferential errors (Table 3.8). And this does not translate into more cooperation, on average (Table 3.5). However, it decidedly fosters cooperation when both agents are nice (that is, when they cooperate in the early stages of the game) and hinders it when both agents are not nice (Table 3.6). One possible explanation for this result is that, once we disaggregate by type of players, strong positive correlation lowers inferential error only when players are nice, thereby strengthening monitoring and, consequently, cooperation. Another possible explanation is that enhanced monitoring only fosters cooperation if players are nice.

To explore the first possible explanation, we examine inferential error under alternative correlation structures; but this time we disaggregate results by the type of players involved in the game. The results are reported in Table 3.12. When we compare our results with those for the average situation (Table 3.8), we can see that virtually the same patterns emerge – strong (but not moderate) positive correlation lowers inferential errors when players are both nice and when they are both not-nice. It also lowers inferential error but to a lesser extent when one player is nice and the other is not. In contrast, negative correlation (either moderate or strong) does not lower inferential error in a way that is distinguishable from chance. This is with the exception of high negative correlation when both players are not nice.

Table 3.12. Error Rates by Niceness for Supergames 7 – 10

Treatment	Nice-Nice	Not-Not	Nice-Not
-0.9	0.107**	0.107	0.146**
	(0.015)	(0.05)	(0.032)
-0.4	0.118**	0.199***	0.135*
	(0.005)	(0.012)	(0.032)
0	0.113*	0.239**	0.135***
	(0.030)	(0.04)	(0.012)
0.4	0.122*	0.208*	0.127***
	(0.031)	(0.056)	(0.007)
0.9	0.018	0.059**	0.084**
	(0.012)	(0.008)	(0.017)
Observations	N = 332	N = 410	N = 890

Robust standard errors are in parentheses and are clustered at the session level

Results in Table 3.12, leaves us with the second possible explanation – that enhanced monitoring only fosters cooperation if players are nice. The results seem to strongly support this explanation. This follows from the observation that the reduction in inferential error is largest when correlation is strong and positive. In fact, inferential errors when correlation is 0.9 is almost indistinguishable from zero. Coincidentally, cooperation is also most prevalent (in fact, highly prevalent at 95%) when correlation is 0.9. This strongly suggests that players' ability to monitor their opponent (and players' knowledge about their opponents' ability to monitor them) disciplines the subjects into cooperation. But this insight is much more nuanced that may seem at first glance. As correlation raises from 0 to 0.9, cooperation decidedly drops from 10% to 2.5% (Table 3.6), despite inferential error dropping from 24% to 6% (Table 3.12).

Our results suggest that a strong positive correlation improves monitoring, like we predicted. But in contrast to our prediction, improved monitoring does not necessarily foster cooperation – it simply better reveals to players the actions of their opponents. If those actions happen to be non-cooperative, as it tends to be the case at first if their opponent is not nice, then they move more quickly towards defection to avoid being the sucker. On the other hand, if the actions revealed by improved monitoring happen to be cooperative, as it tends to be the case at first if their opponent is nice, then they move more decisively towards cooperation.

In summary, our results indicate that a strong positive correlation removes the veil of ignorance regarding the actions of the opponent; but its effect on cooperation depends on whether that reveals a cooperative or non-cooperative opponent. If stronger monitoring reveals an opponent that is reluctant to cooperate, then it will prompt cooperation to unravel. Of course, a player should know that improved monitoring will make her actions more visible as well, and that consistent defection will induce her opponent to retaliate, thereby condemning her to a low payoff. So, why are "not nice" players not anticipating this? If the player thinks her opponent will not give her the opportunity to build trust and cooperation, she may swiftly move to defect in anticipation of a bad equilibrium. As long as "not nice" players are in the mix and they are revealed by improved monitoring, there is a chance that players will defect to avoid a sucker payoff.

3.6 The Computational Experiment

In Section 6.2. we conjectured that, as long as not nice players are in the mix, improved monitoring (from strong positive correlation) may lower cooperation. This is because players may be pessimistic about the prospects of cooperation when improved monitoring reveals a not nice opponent, and defect preemptively to avoid becoming the sucker in the prisoner's dilemma game. We are not the first ones to identify the composition (that is, types) of players involved in a repeated game as an important force underlying the prevalence of cooperation. The presence of not nice players may increase the prevalence of a bad (defection) equilibrium. Ostrom (2000) made this point in her review of the literature on cooperation in social dilemmas. But she, along with Axelrod (1980), also surmised that repeated interactions may stimulate an evolutionary process by which not nice players are selected out of the pool. To investigate this further, we complement our laboratory experiment with a computational simulation of such evolutionary process.

We use a genetic algorithm (Holland 1975) for the evolutionary process. The genetic algorithm starts with a pool of candidate strategies, called automata. The interaction among automata mirrors the experimental framework closely, but there are some notable differences. First, instead of playing ten supergames, automata interact over hundreds of generations. In one generation each automaton is matched to every other automaton, including itself, and they play a supergame. Second, from one generation to the next, the environment dynamically changes given that only strategies with the best performance survive (that is, advance to the next generation).

Strategies also undergo a process of mutation. This process is not removed from reality. One can consider this as a process of exploration and learning, where the agents interacting in the environment try new strategies. If successful, then these strategies are mimicked by other players. If a strategy is unsuccessful, it is abandoned and no longer part of the strategy pool in subsequent generations. Lastly, the automata update their beliefs on the likely action of their opponent according to the decision rule we characterized in Section 3. This is another way in which automata differ from subjects in the laboratory since, from the experimental results, we found that subjects did not seem to follow this rule closely.

Despite these differences, we belief that this is an appropriate framework to gain insights into how cooperation can evolve over many repeated interactions under various correlation structures. Genetic algorithms have been widely used to show how strategies evolve under various environments. We use the tournament style pioneered by (Axelrod 1980) that have since then been used to examine the evolution of strategies in repeated noisy PDs. Similar computational exercises, by and large, have focused on implementation and perception errors (Ioannou 2014b; Miller 1996; Zhang 2018). Other repeated PD applications include examining the impact of costly strategy adjustments (Romero and Rosokha 2019).

3.6.1 The Computational Experiment

Our evolutionary process is similar to Miller (1996) and Ioannou (2014b). Strategies are implemented as finite state automata, with a fixed number of states. Each internal state is accorded cooperation (C) or defection (D). Conditioned on what the other player does, each state has transition rules that dictate what state the automaton should next transition to. Given that subjects mostly used memory-1 strategies, and to reduce computational time, we focus only on finite automata with at most two states. Memory-1 strategies only consider the immediate past move of its opponent. In our environment, each automaton carries information about not only the strategies, but also, the benchmark values relative to which the signal is defined. The two benchmark values that are needed if the automaton is in a cooperation or defection state are endogenously determined; that is, the benchmark values must survive the evolutionary process just like the strategy must. As such, each automaton is represented by a 21-bit binary string. The first 7-bit translates into an integer in the range [24, 84] and the second 7-bit an integer in the range [-11, 49]. The final 7-bits represent the strategy. For this last 7-bit, the first bit represents the starting state, and the rest gives the transitional states. While there are 128 representations of 7-bit strings, these map into 26 unique strategies. We provide more details on the representation of the automata in Appendix B.4.

For the evolutionary process (summarized in Table 3.13), we begin with a population of thirty randomly generated 21-bit binary strings. In every generation, an automaton is paired with every other automaton, including a copy of itself, to play the repeated PD for eighty rounds on average (we use a continuation probability of 0.9875). In the first generation, the automaton also randomly selects two benchmark values, one is used when it cooperates and the other when it defects. At the end, a performance score is calculated based on average payoff across all interactions. If there are ties, one is randomly selected. To populate the next generation, the top twenty performers are selected (the parents). These parents create ten children through a process of mutation. A pair is randomly selected from the parents, and the top performer of this pair undergoes mutation. Mutation involves changing every bit of each string with a probability of 0.04. Mutation occurs on both the strategy and the two benchmark values. Therefore, the automaton updates both its strategy and its benchmark values. This method of mutation does not guarantee that all the best of the best strategies will be selected, or that all the worst of the best will be eliminated. The evolutionary process is simulated for 1000 generations. Then the entire process is repeated 100 times.

Table 3.13. The Evolutionary Process of Genetic Algorithm

Step 1:	Initialize Generation T0 of 30 randomly generated automata
Step 2:	Initiate round-robin tournament
Step 3:	For t = 0 to T:
	For each automaton:
	For round 1 to length of round:
	Play all other automata and self
	Determine average payoff using PD matrix
Step 4:	Select 20 automata with highest payoff
Step 5:	Create 10 - pairs:
	Select from each pair, automata with highest payoff
Step 6:	t = t + 1 Mutate every bit with probability of 0.04

3.6.2 Results of Computational Experiment

Across all levels of correlation, cooperation begins at approximately 50% (Figure 3.3). This is expected given that the automata are randomly selected in the first generation. In fact, out of the 126 possible strategies, 40 are AC and 40 are AD. Immediately, cooperation suffers a sharp reduction. But as generations progress, cooperation rises and stabilizes at higher levels. Higher levels of correlation, both positive and negative, induce higher levels of cooperation. Initially, and for a large number of generations, cooperation under $\rho = -0.9$ is lower than under $\rho = 0.9$. However, cooperation levels under $\rho = -0.9$ and $\rho = 0.9$ converge by generation 1000. Notably, all levels of ρ yield higher cooperation rates than $\rho = 0$, except for $\rho = -0.4$. These results are consistent with our Predictions 1-4. Correlation structures that lower inferential error (both moderate and strong positive correlation, as well as strong negative correlation), also foster cooperation, albeit in the long run, and as a result of the evolutionary process. Negative moderate correlation, which we both predicted and found evidence to support that it raises inferential error, hinders cooperation.

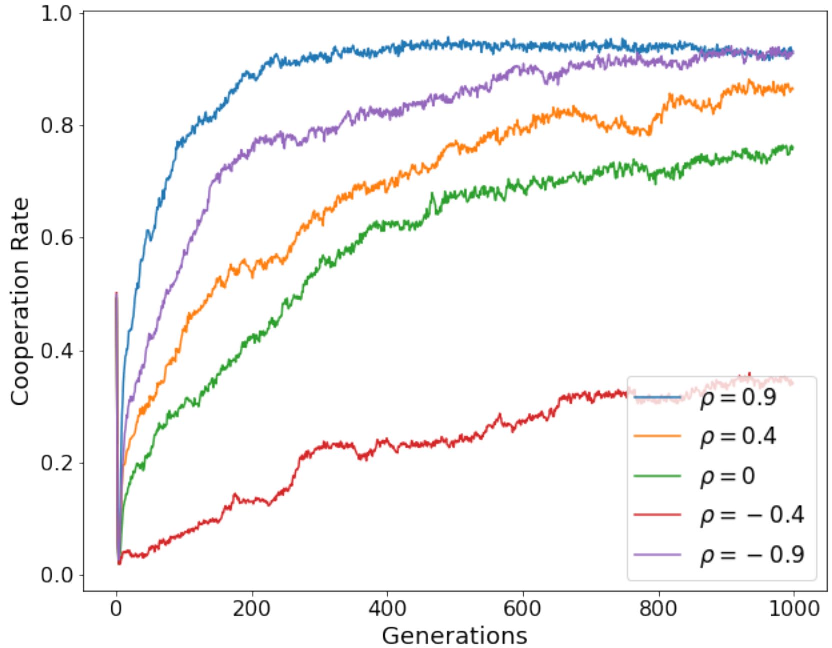


Figure 3.3. Evolution of cooperation over 1000 generations

As discussed before, the correlation structure may affect cooperation through the inferential error, and subsequently influence strategies employed by players. Figure 3.4 shows the evolution of strategies for each correlation structure. We display the memory-1 strategies that the MLE indicated best matched the subjects' data. Mostly two strategies, TFT and AD, explain the differences in cooperation rates observed. For $\rho = 0.9$, where the highest cooperation rate was observed, AD very quickly dies out in the population, leaving TFT as the most frequent strategy. The lower the cooperation rate, the slower AD disappears from the population. This suggests that with sustained interaction, cooperation can be maintained. For high levels of ρ, agents will converge to high levels of cooperation quickly. This process is slower for moderate levels of correlation and $\rho = 0$.

The evolution of the benchmarks offers additional insight into the effect of correlation on cooperation. We report those results in Figure 3.5. The benchmark value ascribed to cooperation under positive correlation and under the baseline increased to around 70 by generation 500. The benchmark value falls quickly to 35 under strong negative correlation,

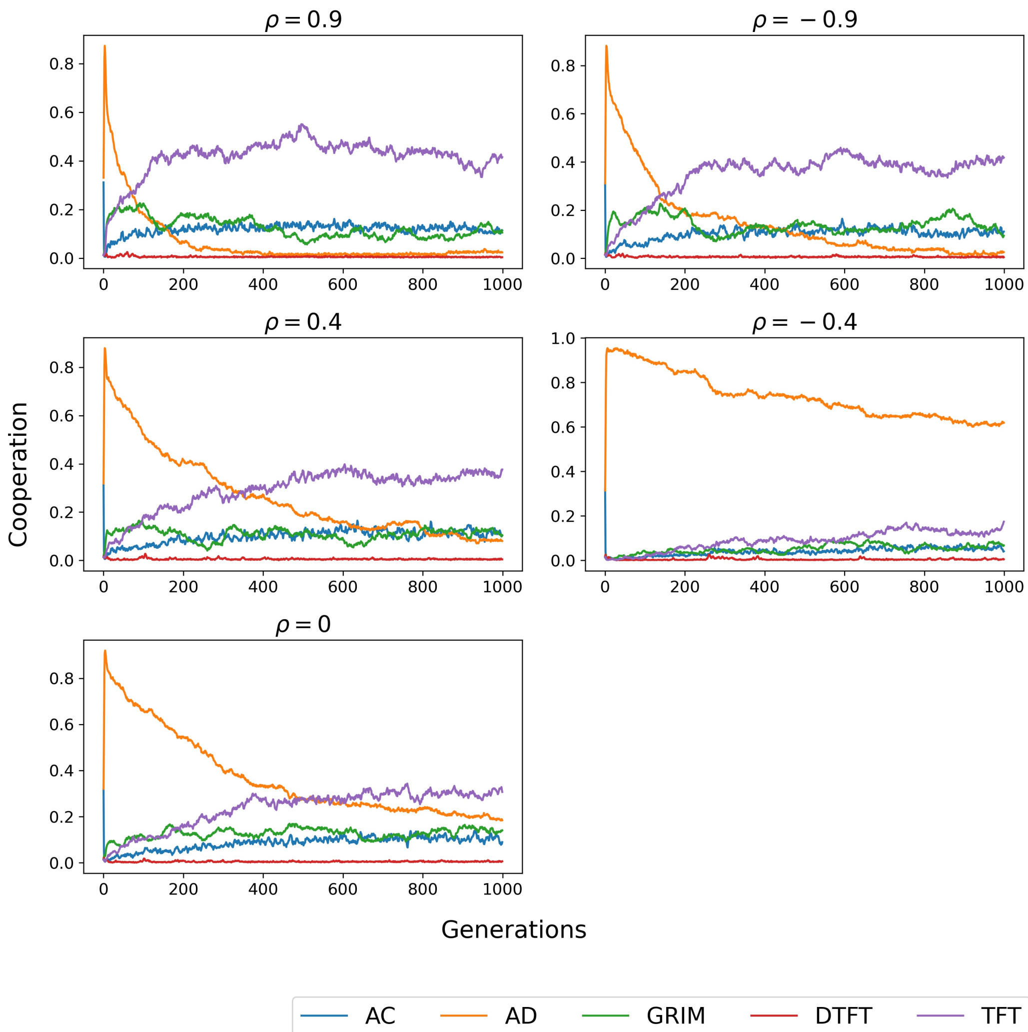


Figure 3.4. Evolution of strategies under each correlation

but only to 45 under moderate negative correlation. Recall that the decision rule prescribes cooperation if there is a signal that the other player's realized payoff is below (above) the benchmark value for $\rho \geq 0$ ($\rho < 0$). Therefore, a (low) high benchmark value under positive (negative) correlation implies, all else constant, that it is more likely the automaton will cooperate in error, rather than defect in error.

The pattern of the rate at which the average benchmark converges to its long-term value, matches the cooperation rate patterns. Under a positive (negative) and strong correlation the benchmark converges more quickly and to a higher (lower) long-term value, and this coincides with a quicker convergence to cooperation (Figure 3.3).

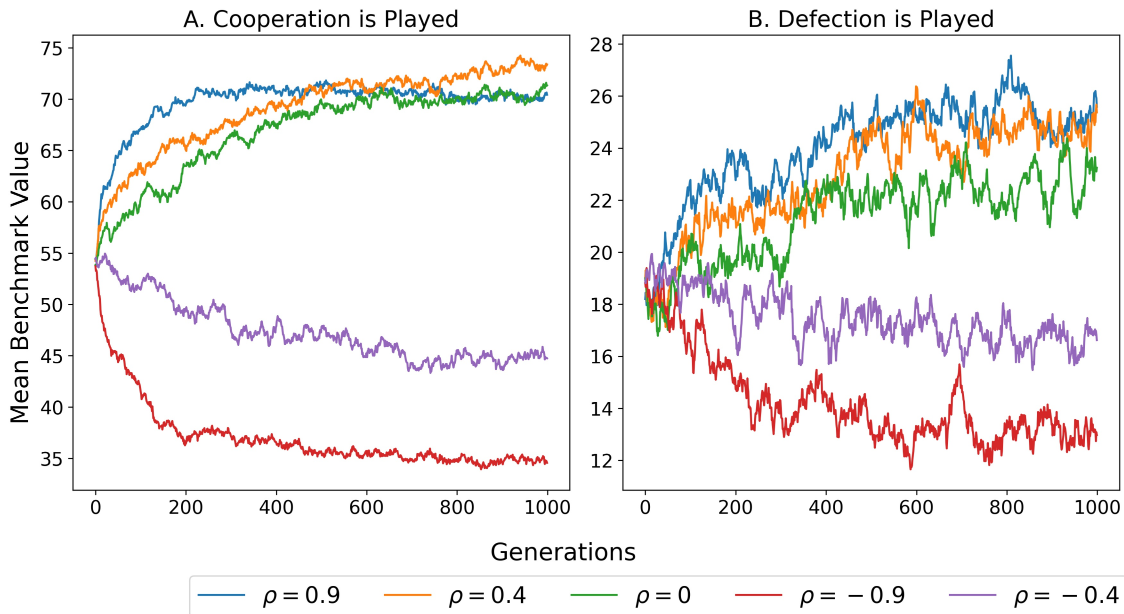


Figure 3.5. Evolution of benchmark values under each correlation

A closer look at results in Figures 3.4 and 3.5 points to an important interaction between both mechanisms underlying the effect of correlation on cooperation: the strategy mechanism and the inferential error mechanism. At the long-term benchmark values observed in Figure 3.5, conditional strategies become somewhat unresponsive to inferential error. When an agent's realized payoff falls within the region of uncertainty, they are more likely to cooperate than defect under the long-term benchmark values. This is only reinforced when the dominant strategy is TFT. If the automaton employs a TFT strategy and selects a high (low) benchmark under positive (negative) correlation, it is more likely to cooperate in error, than to defect in error. This fosters cooperation even further and offers an additional explanation to the high level of cooperation in the computational experiment relative to the laboratory experiment.

3.7 Discussion

In this study, we examined the impact of correlation on cooperation in an infinitely repeated prisoner's dilemma. In the stage game, players' payoffs are affected by a random shock that is uniformly distributed. This shock is independent across rounds but correlated across players. We explored five correlation structures, two positive correlation levels (moderate and high), two negative correlation levels (moderate and high), and a baseline of no correlation. We found that, on average, correlation does not enhance cooperation. While negative correlation weakly lowers cooperation, positive correlation has no impact on cooperation.

We offered two explanations for this observation. First, we anticipated that higher levels of correlation would improve cooperation through two mechanisms: a reduction in inferential error when correlation is positive or negative and strong (including a purely mechanical channel and a behavioral channel), and a change in strategies towards more lenient alternatives. We found evidence supporting the first mechanism, albeit weaker than predicted because subjects in the lab failed to fully exploit the behavioral channel. Second, and also on average, more lenient strategies did not become more prevalent under stronger correlation.

At first glance, it seems puzzling that improved monitoring delivered by positive and strong negative correlation structures does not translate into more cooperation on average. However, the reason for this becomes apparent when we explore the heterogeneity concealed in the average effect. We found that improved monitoring associated with certain correlation structures may have simply revealed the uncooperative nature of many players in the subject pool. In sessions where players were prone to cooperate (defect) at the beginning of the supergame, positive and strong negative correlation structures led to lower inferential error and higher (lower) cooperation. This observation lends support to the argument that cooperation in social dilemmas depends greatly on the environment. In our case, we see that allowing for better monitoring does not automatically lead to cooperation when monitoring is imperfect.

Our results give preliminary insights into the prospects of cooperation in groups with correlated outcomes. Noisy payoffs that are uncorrelated introduce an environment of imperfect monitoring. This imperfect monitoring environment seems to create a veil of uncertainty that encourages free-riding. It appears that individuals are inclined to act in their self-interest if they believe that their uncooperative behavior can go unnoticed. As better monitoring partially removes this veil of uncertainty, cooperation strengthens (unravels) if players are prone (reluctant) to cooperate at initial stages of the game. As pointed out before, there are many environments where the correlation structure of shocks is important to effectiveness of economic groups – perhaps more prominently environments of mutual insurance. Our results suggest that risk-sharing and cooperation can interact in complicated ways depending on the composition of the subject pool.

We also found some evidence that negative correlation weakens cooperation in comparison to the baseline of no correlation. This is true even in the presence of an evolutionary process that tends to select uncooperative players out of the pool. This seems problematic for economic groups that provide mutual insurance. Negatively correlated shocks mean that whenever someone in the group has had a bad shock, another member has had a good shock to offset this. This sounds ideal for risk-sharing. However, we found that moderate negative correlation introduces additional noise, as players found it more difficult to unravel the information contained in the correlation structure, relative to positively correlated shocks. In these circumstances, there appears to be a trade-off between cooperation and risk-sharing. In light of this tradeoff, it seems preferable to include agents with uncorrelated payoffs into the group, instead of agents with negatively correlated payoffs. Future work should explicitly consider the impact of correlated shocks in a risk-sharing environment.

References

Agarwal, Bina. 2018. "Can group farms outperform individual family farms? Empirical insights from India." *World Development* 108:57–73.

Aoyagi, Masaki, V Bhaskar, and Guillaume R Fréchette. 2019. "The impact of monitoring in infinitely repeated games: Perfect, public, and private." *American Economic Journal: Microeconomics* 11 (1): 1–43.

Axelrod, Robert. 1980. "Effective choice in the prisoner's dilemma." *Journal of conflict resolution* 24 (1): 3–25.

Baland, Jean-Marie, Isabelle Bonjean, Catherine Guirkinger, and Roberta Ziparo. 2016. "The economic consequences of mutual help in extended families." *Journal of Development Economics* 123:38–56.

Baland, Jean-Marie, Catherine Guirkinger, and Charlotte Mali. 2011. "Pretending to be poor: Borrowing to escape forced solidarity in Cameroon." *Economic development and cultural change* 60 (1): 1–16.

Bendor, Jonathan. 1993. “Uncertainty and the evolution of cooperation.” *Journal of Conflict resolution* 37 (4): 709–734.

Bendor, Jonathan, Roderick M Kramer, and Suzanne Stout. 1991. “When in doubt... Cooperation in a noisy prisoner’s dilemma.” *Journal of conflict resolution* 35 (4): 691–719.

Bloch, Francis, Garance Genicot, and Debraj Ray. 2008. “Informal insurance in social networks.” *Journal of Economic Theory* 143 (1): 36–58.

Chen, Daniel L, Martin Schonger, and Chris Wickens. 2016. “oTree—An open-source platform for laboratory, online, and field experiments.” *Journal of Behavioral and Experimental Finance* 9:88–97.

Dal Bó, Pedro, and Guillaume R Fréchette. 2011. “The evolution of cooperation in infinitely repeated games: Experimental evidence.” *American Economic Review* 101 (1): 411–29.

———. 2018. “On the determinants of cooperation in infinitely repeated games: A survey.” *Journal of Economic Literature* 56 (1): 60–114.

Danz, David, Lise Vesterlund, and Alistair J Wilson. 2020. *Belief elicitation: Limiting truth telling with information on incentives.* Technical report. National Bureau of Economic Research.

Fafchamps, Marcel. 2011. “Risk sharing between households.” *Handbook of social economics* 1:1255–1279.

Fitzsimons, Emla, Bansi Malde, and Marcos Vera-Hernández. 2018. “Group size and the efficiency of informal risk sharing.” *The Economic Journal* 128 (612): F575–F608.

Fudenberg, Drew, David G Rand, and Anna Dreber. 2012. “Slow to anger and fast to forgive: Cooperation in an uncertain world.” *American Economic Review* 102 (2): 720–49.

Fundenberg, Drew, and Eric Maskin. 1990. “Evolution and cooperation in noisy repeated games.” *The American Economic Review* 80 (2): 274–279.

Gill, David, and Yaroslav Rosokha. 2020. “Beliefs, learning, and personality in the indefinitely repeated prisoner’s dilemma.” *Available at SSRN 3652318.*

Holland, John H. 1975. *Adaptation in natural and artificial systems: An introductory analysis with applications to biology, control, and artificial intelligence.* Adaptation in natural and artificial systems: An introductory analysis with applications to biology, control, and artificial intelligence. Pages: viii, 183. Oxford, England: U Michigan Press. ISBN: 978-0-472-08460-9.

Hossain, Tanjim, and Ryo Okui. 2013. “The binarized scoring rule.” *Review of Economic Studies* 80 (3): 984–1001.

Imhof, Lorens A, Drew Fudenberg, and Martin A Nowak. 2007. “Tit-for-tat or win-stay, lose-shift?” *Journal of theoretical biology* 247 (3): 574–580.

Ioannou, Christos A. 2014a. “Asymptotic behavior of strategies in the repeated prisoner’s dilemma game in the presence of errors.” *Artif. Intell. Res.* 3 (4): 28–37.

———. 2014b. “Coevolution of finite automata with errors.” *Journal of Evolutionary Economics* 24 (3): 541–571.

Jakiela, Pamela, and Owen Ozier. 2016. “Does Africa need a rotten kin theorem? Experimental evidence from village economies.” *The Review of Economic Studies* 83 (1): 231–268.

Kayaba, Yutaka, Hitoshi Matsushima, and Tomohisa Toyama. 2020. “Accuracy and retaliation in repeated games with imperfect private monitoring: Experiments.” *Games and Economic Behavior* 120:193–208.

Miller, John H. 1996. “The coevolution of automata in the repeated prisoner’s dilemma.” *Journal of Economic Behavior & Organization* 29 (1): 87–112.

Ostrom, Elinor. 2000. “Collective action and the evolution of social norms.” *Journal of economic perspectives* 14 (3): 137–158.

Ostrom, Elinor, Joanna Burger, Christopher B Field, Richard B Norgaard, and David Policansky. 1999. “Revisiting the commons: local lessons, global challenges.” *science* 284 (5412): 278–282.

Romero, Julian, and Yaroslav Rosokha. 2019. “The evolution of cooperation: The role of costly strategy adjustments.” *American Economic Journal: Microeconomics* 11 (1): 299–328.

Zhang, Huanren. 2018. “Errors can increase cooperation in finite populations.” *Games and Economic Behavior* 107:203–219.

4. JOINT CHOICE OF LOCATION AND SPATIAL PRICING POLICY IN A MIXED MARKET

Spatial markets (spatial dispersion of processing plants and relatively high cost of transporting goods from the farm to the buyer) can protect buyers of farm products from competition (Sexton 2013). In some settings, weak competition resulting from spatial markets has allowed buyers to mark the price of farm products down, that is, has allowed them to pay a price below marginal value product. Recent headlines, such as the NYT's "Record beef prices, but ranchers aren't cashing in" (Goodman and Schaff 2021), illustrate the importance of market structure and its market and welfare implications. Entry of new firms is, of course, a key force to discipline market power. But many barriers persist that inhibit this force. In agricultural procurement markets, the existence of cooperatives (farmer-owned processing firms) is another important force that can unleash competition. In this paper, we study the effects of agricultural cooperatives on competition and welfare in the context of spatial markets.

Agricultural cooperatives (COOP) are key players along the agricultural supply chain, and have significant market shares in Western countries (Candemir, Duvaleix, and Latruffe 2021), but they coexist with investor-owned firms (IOF) in what is dubbed as mixed markets. The presence of COOPs may affect market equilibrium through location (or, more generally, product differentiation) and/or pricing. Pricing is a complex issue in spatial markets because transportation cost introduces heterogeneity among sellers and, hence, it introduces the possibility of spatial price discrimination. Therefore, a spatial-pricing strategy refers to the markdown chosen by the buyer (that is, difference between the input's marginal value product and its price), and whether the buyer changes markdown by distance to the seller (that is, whether the buyer engages in spatial price discrimination).

Two key contributions to the literature on the effects of cooperatives on competition and welfare in spatial markets are Fousekis (2011) and Fousekis (2015). Fousekis (2011) considered a mixed duopsony (one IOF and one COOP). In this model, firms can choose the spatial-pricing strategy, but not locations. In addition, firms are not allowed to choose the degree of spatial price discrimination; they can either not discriminate (free-on-board pricing, FOB) or fully discriminate (uniform delivered pricing, UD). These assumptions deliver tractability in their spatial model. Fousekis (2015) endogenizes location and pricing but assumes away spatial price discrimination to attain tractability. In this paper, we extend the analyses of Fousekis (2011) and Fousekis (2015) to examine the joint choice of location and spatial-pricing strategy (allowing for spatial price discrimination) in a mixed market.

Examining these decisions jointly often introduces complexities that render the model intractable, namely the models are plagued by the lack of equilibrium or the presence of multiple equilibria. To circumvent the issues of model intractability, we use an agent-based modelling framework, specifically a genetic algorithm. This method has been previously used to study spatial competition in agricultural markets (Graubner, Balmann, and Sexton 2011; Graubner and Sexton, n.d.). Our objective is to compare welfare and market outcomes in a mixed market (IOF and agricultural cooperative) to that of a pure market (two IOFs like the one recently considered in Graubner and Sexton (n.d.), where firms jointly choose location and spatial-pricing strategy. We also strive to understand the mechanisms (location and pricing) through which COOPs affect welfare, and their relative importance.

We found that market and welfare outcome do change under a mixed market. Our key finding is that the interaction of transportation policies and ownership structure affect welfare and market outcome in agricultural procurement markets. The disciplinary effect of the COOP is most profound in a highly competitive market. when transportation cost is low, the COOP aggressively moves closer to the market center and the IOF responds by moving closer to the endpoint of the market. The IOF is able to remain some competitiveness by adjusting its spatial-pricing strategy rather than engaging in excessive differentiation as previously observed in the literature. So overall, the presence of the COOP triggers shorter distance between the firms, lower markdowns and weaker spatial price discrimination. In terms of welfare, the overall market efficiency improves. This is mostly generated from the increase in surplus that goes to producers. Also this increase in surplus is greater than decline in the surplus that goes to buyers, leaving overall well-being greater.

In the next section, we give a brief overview of the literature relating to spatial competition in agricultural markets. In Section 3, we present the model framework of a mixed market consisting of one IOF and an open membership cooperative. The computational experiment, using a genetic algorithm, is detailed in Section 4. The results comparing market and welfare outcome of a pure market versus mixed market is detailed in Section 5. Then, we conclude in Section 6.

4.1 Related Literature

A recent review of spatial competition within agricultural economics by Graubner, Salhofer, and Tribl (2021), highlights that many aspects of the oligopsonistic nature of agricultural buyers' markets remain under-researched. Factors contributing to this oligopsonistic structure include the fact that processors and producers are distributed over geographical space (Graubner, Salhofer, and Tribl 2021), and that transportation costs can be high owing to bulky or perishable products, and this limits producers to accessing the processers that are in their close geographical proximity (Rogers and Sexton 1994).

The Hotelling (1929) model has become the bedrock upon which studies examine location and pricing choices of agricultural buyers. There is a plethora of studies that have examined the spatial pricing strategies given the fixed location of firms at the endpoints of Hotelling's Main Street. The general approach assumes that pricing choice is exogenous, and firms choose either full free-on-board (FOB) or full uniform delivered (UD) or firms endogenize their pricing strategy through optimal discriminatory (OD) pricing. With FOB, producers are responsible for paying the full transportation cost to deliver their products to the processor and the processor offers a constant farm-gate price. In contrast, with UD pricing, the processor bears the whole transportation cost. As such, UD is an extreme form of price discrimination against neighboring producers. All processors receive the same farm-gate price regardless of their distance from the plant. This is in contrast to FOB where the difference in farm-gate price exactly reflects transportation cost from the producer locations. OD, on the other hand, entails partial absorption of transportation cost.

Spatial pricing strategies are examined in pure markets with only IOFs (Zhang and Sexton 2001; Graubner, Balmann, and Sexton 2011; Graubner and Sexton, n.d.) or mixed markets with an IOF and a cooperative (Fousekis 2011, 2015). This usually entails a two-stage game where processors select their price strategy (whether to employ FOB or UD) in the first period , and conditioned on this, engage in price competition in the second period.

In Hotelling's model, the relative importance of space is expressed in terms of the ratio of transportation rate times the distance of the market to the marginal value product of the finished product. For low transportation rates, the procurement market tends towards the Bertrand competitive equilibrium. For sufficiently high transportation cost, firms act as isolated monopsonists where procurement regions of buyers do not overlap. Zhang and Sexton (2001), a seminal piece considering spatial input procurement markets, found that mutual FOB pricing strategy for both processors is a Nash equilibrium when markets are competitive. Under very high transportation cost, Zhang and Sexton (2001) found that both processors employ UD pricing. For intermediate levels of transportation cost, one firm uses UD while the other uses FOB. They note that in the case of UD pricing, no pure Nash equilibrium exists, only mixed. In terms of welfare, in almost all cases UD yields higher welfare than FOB.

FOB is not a Nash equilibrium outcome under the specifications considered by Graubner, Balmann, and Sexton (2011). They used a genetic algorithm to circumvent the analytical challenges faced by Zhang and Sexton (2001), namely, discontinuities in the profit function. Additionally, they considered a linear price-distance function, that allowed processors to choose FOB, UD or partial absorption of transportation cost in the range (0,1). When competition is intense, UD pricing is an equilibrium. While firms act as isolated monopsonists, they use OD pricing.

These results are premised on processors choosing their spatial pricing strategies, while keeping location fixed. Graubner and Sexton (n.d.) show that when this is relaxed, market outcomes do change. Using a genetic algorithm, they model a duopsony where location and pricing strategy are made jointly. They found neither maximum nor minimum differentiation. This implies that if firms are allowed to choose location along with pricing strategies, it may result in better economic efficiency and competition. With high transportation costs, firms do not act like isolated monopsonists, but rather locate at the market quartiles while adopting almost optimal monopsony price discrimination. As transportation cost tends towards zero, the price offered by a processor at a producer's location converges to the input's marginal value product and firms are still located at the market quartiles. Therefore, they can avoid the Bertrand paradox of competing away their profits by locating in the market's center.

These studies of spatial pricing with fixed location, all assume a symmetric market with two IOFs that are maximizing profit. However, real world procurement markets are populated with processors with different objectives. Cooperatives are significant players in agricultural markets. They usually have different objectives than IOFs, that may include maximizing member welfare, maximizing total producers' returns or maximizing the price that producers receive for their products (Fousekis 2015). Sexton (1990), first applied the "yardstick of competition" to a mixed market. That is, the ability of cooperatives to discipline IOFs. He showed that cooperatives temper the market power of IOFs by forcing them to have a lower farm-processor price spread. Fousekis (2011) found evidence of this disciplining effect. He extended the framework of Zhang and Sexton (2001), where both processors are able to choose between FOB or UD pricing strategy in two-stage game with location fixed at the endpoint of the Hotelling line.

Unlike the pure market in Zhang and Sexton (2001), Fousekis (2011) found that when the mixed market is competitive, processors (IOF and cooperative) employ mutual UD strategies. In a weakly competitive environment, FOB is a strictly dominant strategy for the cooperative while the IOF employs either FOB or UD. In the pure markets, processors employ mutual UD. In mixed market, under all levels of competition the cooperative is an aggressive opponent in its pricing strategies. Even under competitive conditions, instead of fostering some sort of collusion, both firms became more aggressive.

The competitive yardstick effect remains if processors are allowed to compete along prices and location in a mixed market (Fousekis 2015). As the market gets competitive, the cooperative moves closer to both the center of the market and the IOF. This market outcome is still inefficient, but less so than the outcome of a pure market. According to Fousekis (2015), the inefficiency comes from limited differentiation in location. But this limited differentiation is less costly than the excessive differentiation of a pure market.

Interestingly, Graubner and Sexton (n.d.) did not find excessive differentiation in the pure market when two IOFs compete on pricing and location. Fousekis (2015) had some noticeable, and perhaps, critical differences. Their theoretical model assumed a convex transportation cost function to circumvent the issues of demand function discontinuities and non-quasi-concave profit functions. Additionally, both processors (IOF and cooperative) were assumed to follow FOB pricing. That is, they do not engage in spatial price discrimination. In this paper we attempt to reconcile the differences in these two outcomes. What are the market and welfare outcomes when duopsony IOF and cooperative choose both location and pricing policy simultaneously? We go back to the seminal Hotelling model with linear transportation cost, to allow for comparison with Graubner and Sexton (n.d.). Unlike Fousekis (2015), a FOB pricing strategy is not given. Rather, processors are allowed to engage in spatial price discrimination. The literature has established that endogenizing location and spatial price discrimination with linear transportation cost, results in a lack of pure strategy subgame perfect Nash equilibrium. This happens when processors are close to each other. Then, it is in the interest of each processor to undercut the other to capture the entire market as a result of the quasi-concave profit functions (Economides 1984). To circumvent this, we use an agent-based model, specifically a genetic algorithm (Holland 1975).

4.2 The Model

Consider an agricultural procurement market where homogenous producers are uniformly distributed along a line with interval [0,1], in a fashion akin to Hotelling's Main Street. For each producer, the amount that is supplied is determined by the price, p, that is being offered at their location x. That is:

$$q(x) = max\{p_{\mathrm{i}}(x), 0\} \tag{4.1}$$

where i represents two processors. At each location x, there is exactly one producer.[1]

Along this market, there are only two processors of agricultural input. Furthermore, one processor is an open membership producers' cooperative (COOP), and the other is an investor-owned firm (IOF). Each choose to locate their plant at any one of the equidistant points within [0,1] such that $x_{COOP} = [0, \frac{1}{2}]$ and $x_{IOF} = [0, \frac{1}{2}]$.

1. ↑We use notations very similar to Graubner and Sexton (n.d.) throughout this paper.

In keeping with Graubner and Sexton (n.d.), our baseline, we use a linear transportation cost. This precludes us from making direct comparisons to Fousekis (2011) and Fousekis (2015), that are closest to this paper in considering a mixed market. They used quadratic transportation cost to arrive at analytical solutions. A producer incurs a cost of $\alpha_{\mathrm{i}} tR$ to sell to either the COOP or the IOF, where t is the transportation rate to ship 1 unit of product from the farm gate to the processor across a distance of R, that is $|x - x_{\mathrm{i}}|$. α_{i} is the fraction of the transportation cost that is borne by the producer and is in the range [0,1]. In models where location is endogenous, the distance between the two processors times t, determines the intrinsic competitiveness of the market (Graubner and Sexton, n.d.).

Both the IOF and the COOP sell the output they produce in a competitive market for a constant price Φ. As such, the marginal value product of the input it receives from the producers is $\phi = \Phi - c$. We normalize ϕ to 1, therefore, $\phi = \Phi - c = 1$. Each processor decides on their location and spatial pricing strategy simultaneously. That is, they both decide on a spatial price and location strategy (m_{i}, α_{i}, x_{i}), where m_{i} is the constant mill price at the gate of the processor and is within the range [0,1].

An IOF and a COOP diverge in their objectives and, therefore, employ fundamentally different pricing and location strategies, all else constant. Nevertheless, whatever their objectives, they must employ a strategy that constitutes a best response to the strategy used by their competitor. Therefore, these strategies are intertwined in a spatial market. We now proceed to describe the objectives and the subsequent problem to be solved by each type of firm.

4.2.1 The COOP

The cooperative we model is detailed in Fousekis (2011) and Fousekis (2015). We consider a producers' cooperative with open membership, whose objective is to maximize members' surplus. As such, the COOP prices in a way that it offers the maximum price it can without incurring negative profits. That is, $m_{COOP} = \Phi - c = 1$. Additionally, since the COOP does not extract rents from farmers, it has no incentives to price-discriminate against farmers according to their locations. Therefore, the COOP employs an FOB strategy. In other words, $\alpha_{COOP} = 1$, as the COOP members pay the full cost of delivering products to the COOP. Therefore, for the COOP, the farmgate price $p_{\text{i}}(x)$, the price offered by the COOP to the producer at location x is:

$$p_{COOP}(x) = 1 - tR \tag{4.2}$$

Because the COOP prices at its net average revenue product, it makes zero profit from processing the input it procures from its members.

The COOP's objective is to maximize surplus amongst its members. Using (4.1) and (4.2), surplus, S_{i}, per farmer is:

$$S_{COOP} = \frac{1}{2}[1 - tR]^2 \tag{4.3}$$

The market boundary for the COOP is dictated by the local price being zero, that is, $1 - tR = 0$. This is so because the COOP employs FOB, where $\alpha_{COOP} = 1$, therefore there is no unique location where profit is zero. As such, the boundary defined by $\frac{1}{t}$.

4.2.2 The IOF

The IOF's objective is to maximize profit. The farmgate price offered by the IOF is:

$$p_{IOF}(x) = m_{IOF} - \alpha_{IOF}tR \tag{4.4}$$

Given the supply function from (4.1), profit for the IOF at each processor location x is:

$$\pi_{IOF}(x) = [\ b_{IOF}(x) - p_{IOF}(x)]\ p_{IOF}(x) \tag{4.5}$$

where $b_{IOF}(x)$ is the price at each location that returns zero profit to the IOF, that is:

$$b_{IOF}(x) = 1 - tR \tag{4.6}$$

The IOF will not engage with processors outside of its procurement region. This market boundary for the IOF is determined by two factors, the farmgate price is zero, $0 = m_{IOF} -$

$\alpha_{IOF}tB_1$ or profit is zero, $0 = 1 - m_{IOF} - (1-\alpha_{IOF})tB_2$. The effective boundary is the lesser of the two conditions, that is:

$$B = min\left\{ \frac{m}{\alpha t}\bigg|_{\alpha>0}, \frac{1-m}{(1-\alpha)t}\ \bigg|_{0\leq\alpha<0} \right\} \tag{4.7}$$

If the effective boundary is $B = \frac{m}{\alpha t}$, then farmers located beyond the boundary are not willing to sell to the processor, because they can never obtain a positive farm-gate price. However, the effective boundary in (4.7) opens up the possibility of arbitrage by sellers. If the effective boundary is $B = \frac{(1-m)}{(1-\alpha)t}$, the processor is not willing to buy from distances beyond B. However, the farmer can deliver their product to the boundary of the processor (if profitable) at their own expense. To understand this, we illustrate this situation in Figure 4.1.

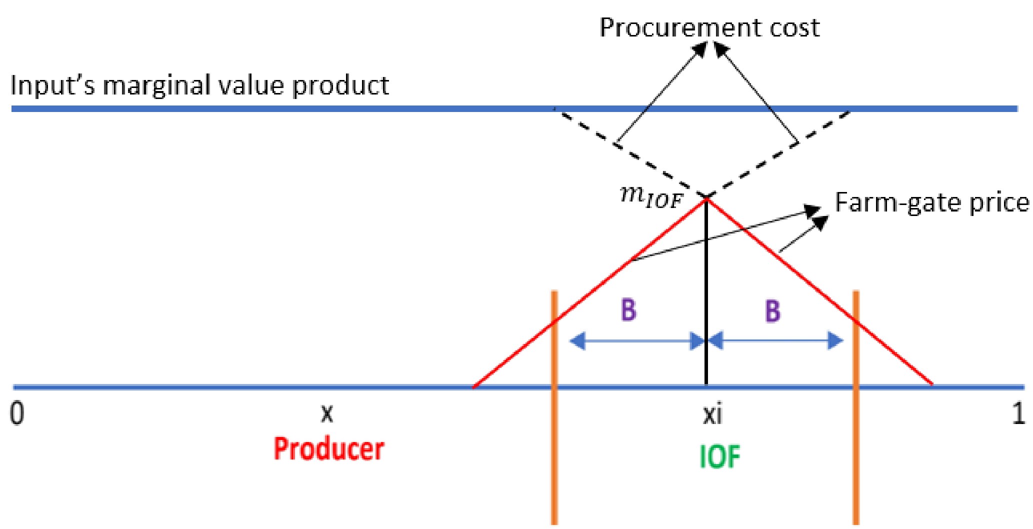


Figure 4.1. The producer at location x is outside of the service area of the IOF

In Figure 4.1, we portray an IOF that chooses a location x_{IOF}, and a spatial pricing strategy pair (m_{IOF}, α_{IOF}). Under these choices, procurement cost at the plant gate is m_{IOF}, and grows with distance at a rate of $(1-\alpha_{IOF})t$. As a result, procurement cost reaches marginal value product at distance B, and exceeds marginal value product beyond that distance. This means that the firm will obtain negative profits on inputs bought from farmers located beyond a distance B from the plant. However, also notice that, at distance B, farm-gate price is positive. In fact, farmgate price from selling to the IOF at B is:

$$p_{IOF}(x)^B = m_{IOF} - \alpha_{IOF} t B \tag{4.8}$$

Therefore, a producer located at x can opt to deliver their product to the border of the IOF, if that price minus the cost of transporting the product from x to B is positive (provided the seller does not get a positive price from the COOP) or lower than the price she would get if she sells to the COOP. The cost to the producer to take their product to the boundary is:

$$C^B = \begin{cases} t(x_{IOF} - B - x) & \text{if } x < x_{IOF} \\ t(x - (x_{IOF} + B)) & \text{if } x > x_{IOF} \end{cases} \tag{4.9}$$

The net farmgate price to the producer at the boundary is:

$$\text{net price} = p_{IOF}(x)^B - C^B \tag{4.10}$$

We can combine equations (4.8) - (4.10) to formally characterize the arbitrage condition for a hypothetical supplier located at x as follows:

$$m_{IOF} - \alpha_{IOF} t - t|x - B| > max\{0, 1 - |x - x_{COOP}|t\} \tag{4.11}$$

where $|x - x_{COOP}|$ is the distance between the farmer and the coop (not portrayed in Figure 4.1).

4.3 Computational Experiment

We adopt the evolutionary process outlined by Graubner and Sexton (n.d.). There are 400 equally spaced producers in market of distance normalized to 1. There is only one producer at each location and the two processors, the COOP and the IOF, can locate at any of these locations. A producer will sell their product to the processor that offers the higher price at their location.

A processor's spatial price and location strategy is $\gamma_{\mathrm{i}} = (m_{\mathrm{i}}, \alpha_{\mathrm{i}}, x_{\mathrm{i}})$. We refer to this as a chromosome. In our genetic algorithm, the information that the chromosome carries are represented as a binary string. The first generation of the algorithm is initialized with each processor randomly selecting 25 chromosomes. That is, the IOF selects 25 random triplets of m_{i}, α_{i} and x_{i}. While the COOP randomly selects x_{i}, given that $m_{\mathrm{i}} = \alpha_{\mathrm{i}} = 1$. Next, the fitness of each chromosome is determined.

Fitness is determined in an Axelrod (1980) type round-robin tournament. Each chromosome of a processor plays every other chromosome of the other processor. When a chromosome from one processor is pitted against a chromosome from the other processor, we calculate the sum of local profits for the IOF (equation 4.5) and local surplus for the COOP (4.3) over all producer locations, x_{j}. Recall that producer will sell to only one processor, the one that offers the higher price at its location. For the IOF, that is,

$$\boldsymbol{\pi}_{IOF}(\gamma_{IOF}, \gamma_{COOP}) = \sum_{\mathrm{j}=1}^{n} \boldsymbol{\pi}(x_{\mathrm{j}}) \quad \forall x_{\mathrm{j}} \in [0,1] : p_{IOF}(x_{\mathrm{j}}) > p_{COOP}(x_{\mathrm{j}}) \tag{4.12}$$

and the COOP:

$$TS_{COOP}(\gamma_{COOP}, \gamma_{IOF}) = \sum_{\mathrm{j}=1}^{n} S(x_{\mathrm{j}}) \quad \forall x_{\mathrm{j}} \in [0,1] : p_{COOP}(x_{\mathrm{j}}) > p_{IOF}(x_{\mathrm{j}}) \tag{4.13}$$

After a chromosome from a processor plays every chromosome from the other processor, an average fitness is calculated that indicates it's performance. After estimating the average fitness for each γ_{COOP} and γ_{IOF}, we identify chromosomes to move on to the next generation. We use three standard genetic algorithm operators to get these candidates. This includes a process of selection, crossover, and mutation.

The process of selection is based on average fitness. For each processor, the chromosomes in the top 80% are selected into the next generation. This method of selection ensures that the best traits move on to the next generation. To fill the remaining spots of the next generation, we randomly select from the already selected chromosomes. The random selection is done in proportion to the average fitness, in that, the higher the fitness, the more likely a chromosome will be selected.

We use crossover and mutation to add new traits to the population. Because the genetic algorithm is randomly initialized, this ensures that the algorithm does not get stuck at a local solution based on the values it was initialized with. After selection, we perform crossover with a probability of 10%. In the crossover process, we use one randomly selected crossover point to split up two parent chromosome and rejoin them. For example, two parents $\gamma^1 = (m^1, \alpha^1, x^1)$ and $\gamma^2 = (m^2, \alpha^2, x^2)$ could become $\gamma^1 = (m^2, \alpha^1, x^1)$ and $\gamma^2 = (m^1, \alpha^2, x^2)$ or it could be $\gamma^1 = (m^2, \alpha^2, x^1)$ and $\gamma^2 = (m^1, \alpha^1, x^2)$.

After crossover, the chromosomes go through a process of mutation. Every bit of the binary string that represents the chromosome is altered with a probability of 4%. After selection, crossover and mutation, the new population of chromosomes move on to the next generation. This process occurs for a total of 2500 generations. Next, we repeat the entire process 20 times.

4.4 Results

We first replicate the results of the pure market in Graubner and Sexton (n.d.). This serves the dual purpose of validating our genetic algorithm and providing the baseline case for our analysis. Next, we compare these results to a mixed market with a profit maximizing cooperative and a welfare maximizing COOP. We do so in two parts. First, we analyze the market effects of processors jointly choosing location and spatial pricing. Next, we compare the welfare effects. We implement two treatments: high level of transportation cost, $t = 4$, and a low level, $t = 1.5$.

Our choice of t is influenced by the literature. As $t \to 0$, the Bertrand-Nash equilibrium emerges, where m equals the gross marginal revenue product. When $t \to 4$, processors act like spatial monopsonists. As a matter of fact, Graubner and Sexton (n.d.) found that this occurred at $t \in [3, 4)$. This is shown in Figure 4.2 (from Graubner and Sexton (n.d.)).

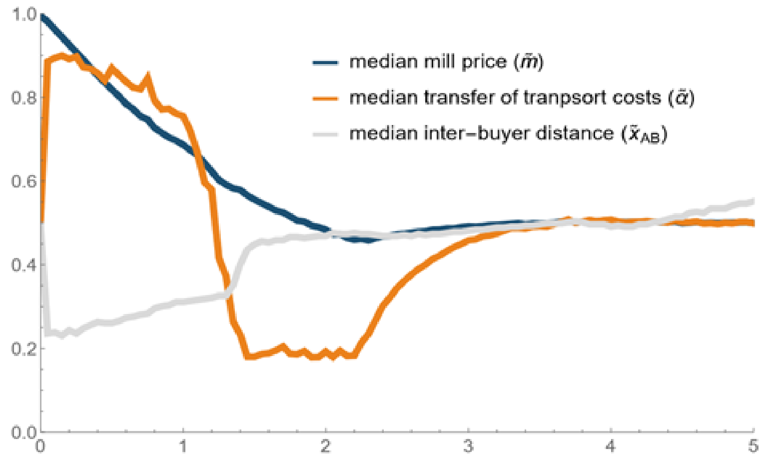


Figure 4.2. Pricing strategy and location, given t, in the generalized Hotelling model, source: Graubner and Sexton (n.d.)

4.4.1 Market Effects

Our main comparative statics of interest is the effect of the presence of a COOP on competition and welfare. And we also want to examine the robustness of those effects across transportation cost, a force that underlies the degree of competitiveness of the spatial market. We start by validating our genetic algorithm. We do so by replicating results from Graubner and Sexton (n.d.). We then run our own simulations that characterize our comparative statics of interest. Even though the entire genetic algorithm was carried out for 2500 generations, we focus our analysis only on the final 5%. Given that the simulation was repeated 20 times, we have a total of 2500 games per treatment.

We begin by replicating the results of Graubner and Sexton (n.d.) for $t = 4$ and $t = 1.5$ for a pure market with two IOFs (see Figure 4.2). Their results confirmed the touching equilibrium of Economides (1984). With high transportation cost, the processors act as monopsonist and engage in OD pricing with $m = 0.5$ and $\alpha = 0.5$. Processors are located near the market quartiles, with median distance between firms of $\hat{x} = 0.49$, and engage in weak competition at the market center. At $t = 1.5$, the median value of α falls to 0.18 and m increases to 0.56.

In Figure 4.3 we present results characterizing the effect of a COOP on market equilibrium and welfare. The genetic algorithm selects strategies with the highest average fitness scores. We then take random draws from those selected strategies and present the cumulative distributions for m, α and $\hat{x}$ for $t = 4$ and $t = 1.5$. These random draws can be interpreted as combinations of surviving strategies that happen to be matched for a specific game.

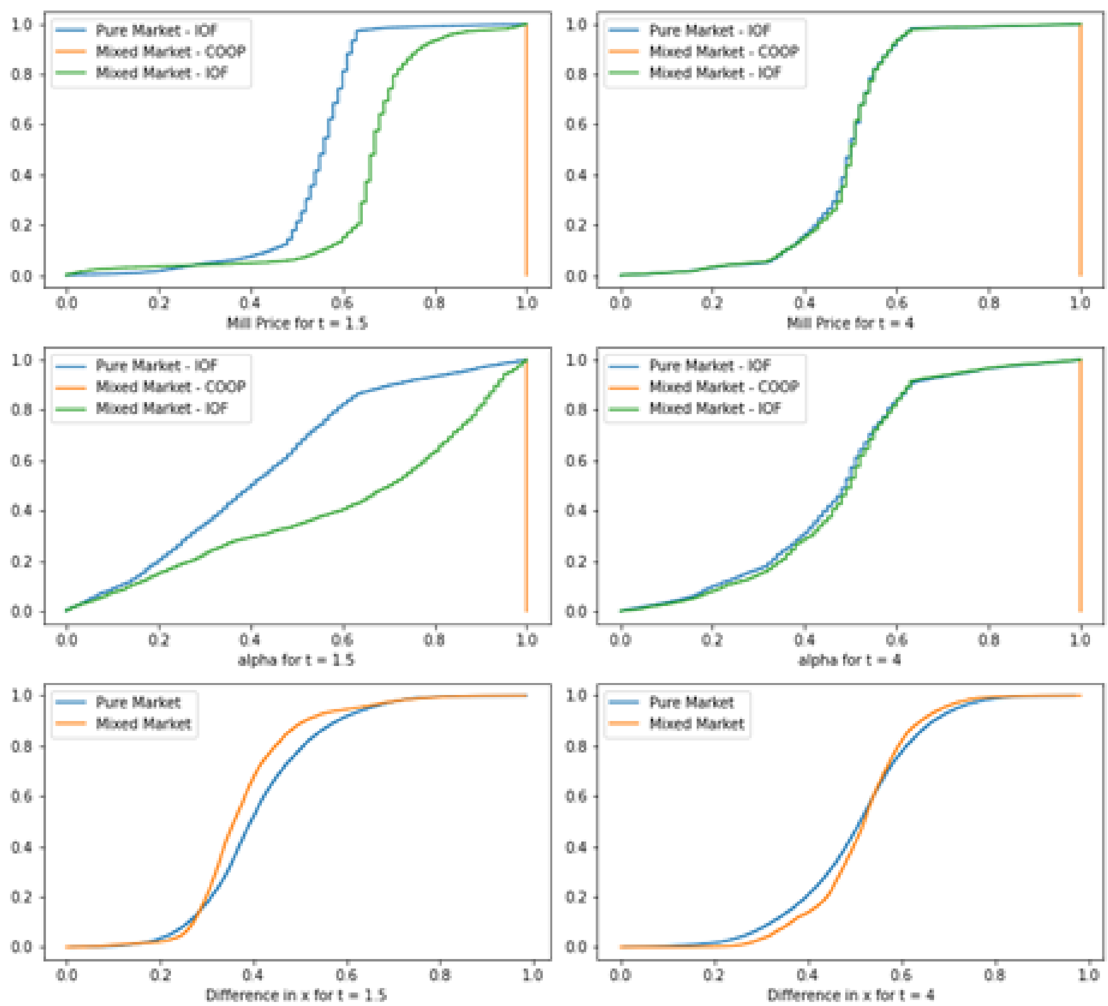


Figure 4.3. Cumulative distribution of for $t = 1.5$ and $t = 4$

Our results confirmed the touching equilibrium. The median values of $\hat{m}$ and $\hat{\alpha}$ at t=4 are 0.5 and 0.49 respectively (see Appendix C for the full set of results from the simulations). The processors are located at the quartiles, with the median location of one processor at $\hat{x}_A = 0.24$ and the other at $\hat{x}_B = 0.76$. This results in a median distance of 0.5.

For more intense competition, t=1.5, the processors move closer together but still away from the market center. The median distance between processors is $\hat{x} = 0.4$. In response to the competitive pressure, processors increase m and lower the share of transportation cost that is borne by the producers, α. In other words, they reduce markdown and increase spatial price discrimination. Graubner and Sexton (n.d.) point out that this near-UD pricing strategy plus locating close to the market quartile enables processors to access farm products and limit competition from their opponent.

A key insight emerging from Figure 4.3 is that the presence of a COOP strengthens competition if transportation cost is low enough to generate overlap in procurement regions. The CDFs of variables m, α, and $\hat{x}$ is clearly altered by the presence of a COOP with $t = 1.5$, but not with $t = 4$. For $t = 4$, the mixed market conforms to the touching equilibrium. Both the IOF and COOP are located near the market quartiles, with the IOF employing OD pricing ($\alpha \cong 0.5$). As competitiveness of the market increases due to a reduction in transportation cost, the COOP becomes a very aggressive player in the market. The COOP moves away from the market quartile to locate close to the market center ($\hat{x}_{COOP} = 0.43$). To soften competition from the COOP, the IOF responds by moving closer to the endpoint of the market ($\hat{x}_{IOF} = 0.79$) and away from the COOP. The IOF also responds to the aggressive pricing strategy of the COOP increasing m and curbing spatial price discrimination by increasing α.

In Fousekis (2015), when the market is sufficiently competitive, the COOP locates at the market center and controls $\frac{5}{6}^{th}$ of the market share, and leaving only $\frac{1}{6}^{th}$ of the market share to the IOF (the distance between the two processors is $\frac{1}{3}$). The IOF cannot compete with the COOP and can only procure from producers in its backyard. We see a similar effect in our framework. However, the COOP does not locate as close to the center and the IOF does not locate as close to the extreme. Therefore, the IOF keeps $\frac{1}{5}^{th}$ of the market rather than $\frac{1}{6}^{th}$. The median distance between the two is $\hat{x} = 0.36$; a smaller distance than that observed between two IOFs.

While Fousekis (2015) endogenized locations, he assumed that both IOF and COOP adopt FOB pricing. In this study, we find that the presence of a COOP does reduce the degree of spatial price discrimination, but that it does not eliminate it. The IOF exploits rents on its own portion of the market by engaging in spatial price discrimination. This pushes the IOF to defend its market share more strongly (because the margin obtained from additional suppliers is largest than it would be in the absence of spatial price discrimination), which means locating slightly closer to the center than it would have located had it not engaged in spatial price discrimination.

To summarize, results in Figure 4.3 show that stronger competition triggered by the presence of a COOP translates into more competitive behavior along all aspects of the firm's strategy: shorter distance (lower product differentiation), lower markdowns (that is, a higher m), and weaker spatial price discrimination (that is, a higher α). However, much of the behavioral response of the IOF to a COOP is on the spatial-pricing strategy, rather than on product differentiation. There are two reasons behind this result. First, the IOF's ability to still engage in spatial price discrimination weakens its incentives to differentiation. And second, the move towards the center of the COOP limits the IOF's ability to differentiate.

4.4.2 Welfare Effects

Next, we examine the welfare effects of a mixed market relative to a pure market. In Figure 4.4, we illustrate the total surplus to processors/IOFs (TBS), and the total surplus to farmers/producers (TFS). In the pure market

$$TBS = \pi_A + \pi_B \tag{4.14}$$

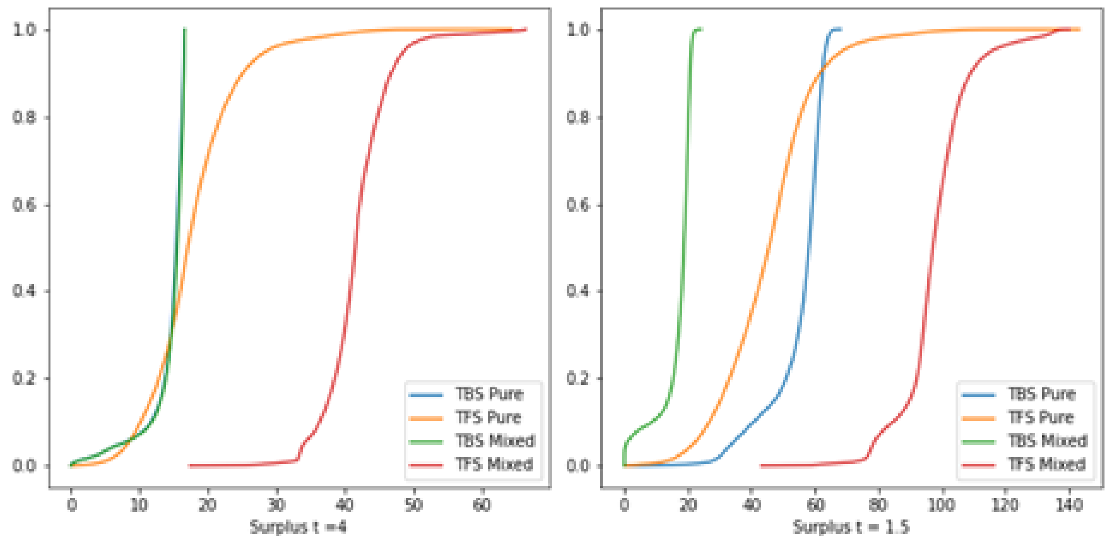
$$TFS = \frac{1}{2} p_{\mathrm{i}}(x)(q(x) \tag{4.15}$$

In the mixed market

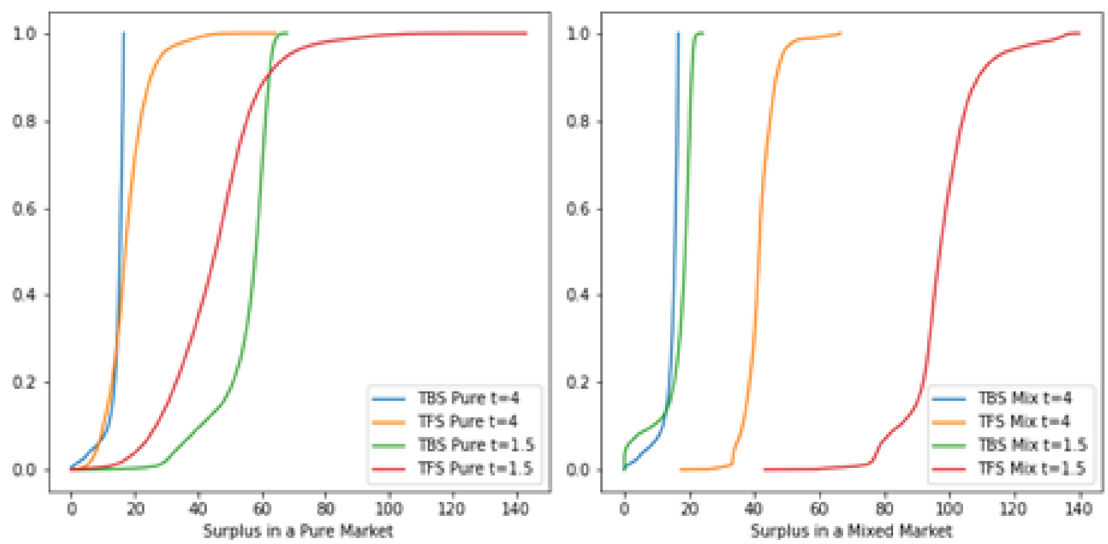
$$TBS = \pi_{IOF} \tag{4.16}$$

In the mixed market, TFS remains the same as equation (4.15).

In the pure market, higher transportation cost erodes market surplus in general. Total market surplus is lower under higher transportation cost, due to lower surplus being received by both the processors and the producers (Figure 4.4b). For $t = 1.5$, while total market surplus increases, a larger share of this is captured by the processors. Graubner and Sexton



(a) Market surplus by t



(b) Market surplus by market structure

Figure 4.4. Cumulative distribution of total surplus accrued to buyer/processor (TBS), total surplus accrued to farmers/producers (TFS) for $t = 1.5$ and $t = 4$ in a pure market (IOF + IOF) and mixed market (IOF + COOP)

(n.d.) show that producers capture a larger portion of market surplus only for a range of $t \in [0, 1.1]$.

In a mixed market, the presence of the COOP increases total surplus, and therefore market efficiency. In Figure 4.4, we see that the presence of the COOP greatly increases TFS and decreases TBS. This reduction in TBS is not so much through eroding the surplus of the remaining IOF but rather from eliminating the surplus of the firm that is operating as a COOP and not an IOF. The outcome of the processor does not improve as the market becomes more competitive in a mixed market. While the producers are better off under $t = 1.5$ or $t = 4$ (Figure 4.4b), the effects are stronger for smaller t. When $t = 1.5$, farmers command a lion share of the total market surplus. As a matter of fact, the outcome of the processor does not improve much as the market becomes more competitive in a mixed market. Overall, producers' welfare is at its highest in an intrinsically competitive mixed market, while processors' welfare is greatest in a competitive pure market (Figure 4.4a).

Despite total surplus being largest in the mixed market when $t = 1.5$, the largest efficiency gains occur under $t = 4$. To see this, compare total surplus in the mixed to that of the pure market when $t = 1.5$ and for $t = 4$. Total surplus increases by 78% (32 to 57) under $t = 4$, but only by 12% (104 t0 116) under $t = 1.5$. That is, when we move from a pure market to a mixed market, the change in the COOP procurement region is bigger under $t = 4$ than under $t = 1.5$

4.5 Conclusion

The oligopsonistic nature of agricultural buyers' market do have important welfare implications. In recent headlines, we have seen renewed focus on the deleterious effect that this market structure can have on farmers' well-being. Yet, this market structure is largely under-examined in the literature. In the cases where it is, most studies have failed to consider firms jointly choosing location and spatial-pricing strategy. And, when this joint choice is considered, it is under the confines of a pure market. In this paper, we recognize the importance of agricultural cooperatives as key players in the agricultural procurement market. As such, we examine the mechanisms through which a cooperative may affect welfare and market outcomes, when firms in a mixed market jointly choose location and spatial-pricing strategy. To circumvent the problems of model intractability, we do so using a genetic algorithm.

We identified two interacting forces that drive market and welfare outcome. The cooperative can discipline an investor-owned firm, but mostly so when the market is competitive enough to generate an overlap in the procurement region. That is, when transportation cost is low. With low transportation cost, the cooperative is an aggressive player in the market, and this is materialized along all dimensions of the firms' strategies. The market outcome results in shorter distance between firms (this can also be interpreted as lower product differentiation), lower markdowns and weaker spatial price discrimination. Unlike in previous studies where the IOF is relegated to a serve a mere $\frac{1}{6}^{th}$ of the market, the IOF uses spatial-pricing strategy, rather than excessive differentiation, to respond to the encroachment of the cooperative.

In terms of welfare, market efficiency improves under a mixed market.Total surplus is higher for both $t = 1.5$ and $t = 4$. Interestingly, the highest gains in efficiency are achieved with high transportation cost, while the cooperative does not compete directly with the IOF.

Moving forward we wish to further examine how these market and welfare outcome would respond to various government policies.

References

Axelrod, Robert. 1980. "Effective choice in the prisoner's dilemma." *Journal of conflict resolution* 24 (1): 3–25.

Candemir, Ahmet, Sabine Duvaleix, and Laure Latruffe. 2021. "Agricultural cooperatives and farm sustainability–A literature review." *Journal of Economic Surveys* 35 (4): 1118–1144.

Economides, Nicholas. 1984. "The principle of minimum differentiation revisited." *European Economic Review* 24 (3): 345–368.

Fousekis, Panos. 2011. "Free-on-board and uniform delivery pricing strategies in a mixed duopsony." *European Review of Agricultural Economics* 38 (1): 119–139.

———. 2015. "Location equilibria in a mixed duopsony with a cooperative." *Australian Journal of Agricultural and Resource Economics* 59 (4): 518–532.

Goodman, Peter S., and Erin Schaff. 2021. "Record Beef Prices, but Ranchers Aren't Cashing In." *The New York Times* (December 27, 2021). ISSN: 0362-4331. https://www.nytimes.com/2021/12/27/business/beef-prices-cattle-ranchers.html.

Graubner, Marten, Alfons Balmann, and Richard J Sexton. 2011. "Spatial price discrimination in agricultural product procurement markets: a computational economics approach." *American Journal of Agricultural Economics* 93 (4): 949–967.

Graubner, Marten, Klaus Salhofer, and Christoph Tribl. 2021. "A Line in Space: Pricing, Location, and Market Power in Agricultural Product Markets." *Annual Review of Resource Economics* 13:85–107.

Graubner, Marten, and Richard J Sexton. n.d. "More Competitive Than You Think? Pricing and Location of Processing Firms in Agricultural Markets."

Holland, John H. 1975. *Adaptation in natural and artificial systems: An introductory analysis with applications to biology, control, and artificial intelligence.* Adaptation in natural and artificial systems: An introductory analysis with applications to biology, control, and artificial intelligence. Pages: viii, 183. Oxford, England: U Michigan Press. ISBN: 978-0-472-08460-9.

Hotelling, Harold. 1929. "Stability in Competition." Publisher: [Royal Economic Society, Wiley], *The Economic Journal* 39 (153): 41–57. ISSN: 0013-0133. https://doi.org/10.2307/2224214. http://www.jstor.org/stable/2224214.

Rogers, Richard T, and Richard J Sexton. 1994. "Assessing the importance of oligopsony power in agricultural markets." *American Journal of Agricultural Economics* 76 (5): 1143–1150.

Sexton, Richard J. 1990. "Imperfect competition in agricultural markets and the role of cooperatives: A spatial analysis." *American Journal of Agricultural Economics* 72 (3): 709–720.

———. 2013. "Market power, misconceptions, and modern agricultural markets." *American journal of agricultural economics* 95 (2): 209–219.

Zhang, Mingxia, and Richard J Sexton. 2001. "FOB or uniform delivered prices: Strategic choice and welfare effects." *The Journal of Industrial Economics* 49 (2): 197–221.

A. EXPERIMENTAL DETAILS

A.1 Description of the Belief Elicitation strategy

For this, you will receive either 0 points or 2 points. Your chance to win 2 points depends on both your guess and if the other participant invested in Project A. Specifically, your chance of receiving 2 points is determined in the following way:

1. First, you will guess the probability that the other participant invested in Project A. You will guess a number from 0 to 100, that we convert to a decimal.
2. If the other participant invested in Project A, your chance-to-win 2 points will be:

$$2z - z^2$$

   where z is the probability you selected, that the other participant selected Project A
3. If the other participant invested in Project B, your chance-to-win 2 points will be:

$$1 - z^2$$

4. To determine whether you receive 2 points, the computer will randomly draw a number between 0 and 100. Each number between 0 and 100 is equally likely to be picked
5. If the number drawn by the computer is less than or equal to your chance-to-win, then you will receive 2 points. Otherwise, you receive 0 points

A.2 Experimental Instructions

Instructions for the Noise Treatment

Welcome!

Today's experiment will last about 60 minutes. You will be paid a show-up fee of $5 together with any money you accumulate during this experiment. The money you accumulate will **depend partly on your actions, partly on the actions of others, and partly on**

chance. Therefore, please read the instructions carefully. This money will be paid at the end of the experiment in private and in cash.

Your returns will be recorded in points. At the end of the session, the total number of points in your account will be converted into cash at an exchange rate of 300 unit = \$1. It is possible for you to get negative points in a round. If at the end of the session you have negative units in your account, you will be paid the show-up fee.

It is important that during the experiment you remain silent. If you have any questions or need assistance of any kind please raise your hand, but do not speak, and an experiment administrator will come to you and you may then whisper your question.

In addition, please turn off your cell phones and put them way now. Please do not look into anyone's booth at any time.

Please read the following instructions carefully. You will be given a quiz at the end to test your understanding and you earn \$0.50 for each correct answer.

Agenda:

- Experimental instructions
- Quiz
- Experiment

How a match works

This session is made up of 10 matches between you and other participants in the room. In each match, you play a random number of rounds with another participant. The length of a match is randomly determined in that, after each round, there is a 90% chance that the match will continue for at least another round. Once the match ends, you will be randomly re-grouped with another participant to play another match. Whenever a match ends, you will be informed of this before you are re-grouped.

Decisions and Payoffs (Before Random Draw)

In each match, you will make a series of investments in a project with the same participant. For each round of a match, you can invest in either Project A or Project B. The participant you are playing with has the same two options. You each choose your project at the same time. The returns on investment depends on the project you choose, the project the other participant chooses and a random draw. That is, the returns you get depend on:

- the investment you made (Project A or Project B)
- the investment made by the other participant
- a random draw

The following table summarizes the return you get based on your decision and the other participant's decision:

		Other participant's choice	
		A	*B*
Your choice	*A*	**48**,**48**	**13**,**60**
	B	**60**,**13**	**25**,**25**

The first red bolded entry in each cell represents your returns before accounting for the random draw, while the second entry in blue represents the returns of the participant you are grouped with (how the random draw affects you and the other participant's returns will be explained below).Ignoring the random draw, if:

- You invest in Project A and the other participant invests in Project A, your both earn **48 points**
- You invest in Project A and the other participant invests in Project B, you earn **13 points** and the other participant earns **60 points**
- You invest in Project B and the other participant invests in Project B, you both earn **25 points**
- You invest in Project B and the other participant invests in Project A, you earn **60 points** and the other participant earns **13 points**

Your project returns may change depending on a random draw

In each round, after you have invested in a project, your return may change by a random draw. Let's call this random draw v_1. This means that, your return may increase, decrease, or stay the same by an amount v_1. The computer will randomly select this number in each round. This random draw does not depend on the project that you or the other participant choose or the random draw in previous rounds. This draw is completely random.

This random draw will always be a number from -24 to 24. Each number, of the 49 integer values between -24 and 24, is equally likely to occur.

The other participant's return, after they have invested, will also change by a random draw. Let's call this amount v_2. This means that, the returns from their project may increase, decrease, or stay the same by an amount v_2. The computer will generate these integers for you both. **These integers will be completely independent. This means that your random draw is completely unrelated to the random draw of the other participant.**

In the diagram below, there are 500 examples of random draws for you and the other participant, where each dot represents a random draw. If you hover your cursor over one of these dots, you will see a pair of numbers where the first integer (labelled 'yours') represents your random draw and the second integer (labelled 'theirs') is the random draw for the other participant.

Now, the total return you receive is dependent on your random draw AND the choices made by the other participant. The other participant's random draw does not affect your return. With the random draw, the rule for your investment returns now becomes:

		Other participant's choice	
		A	B
Your choice	A	$\mathbf{48} + v_1$,$\mathbf{48} + v_2$	$\mathbf{13} + v_1$,$\mathbf{60} + v_2$
	B	$\mathbf{60} + v_1$,$\mathbf{13} + v_2$	$\mathbf{25} + v_1$,$\mathbf{25} + v_2$

Pay close attention to the following information.

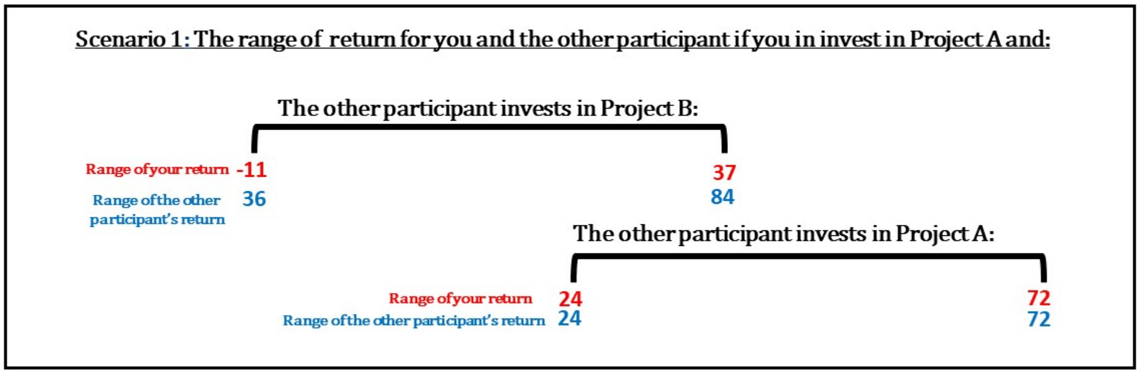
For both you and the other participant, taking into account the return and the random draw:

- the minimum return that can be received is **-11** (the lowest possible return 13 plus minimum possible random draw of -24)
- and the maximum return that can be received is **84** (the highest possible return 60 plus maximum possible random draw of +24).

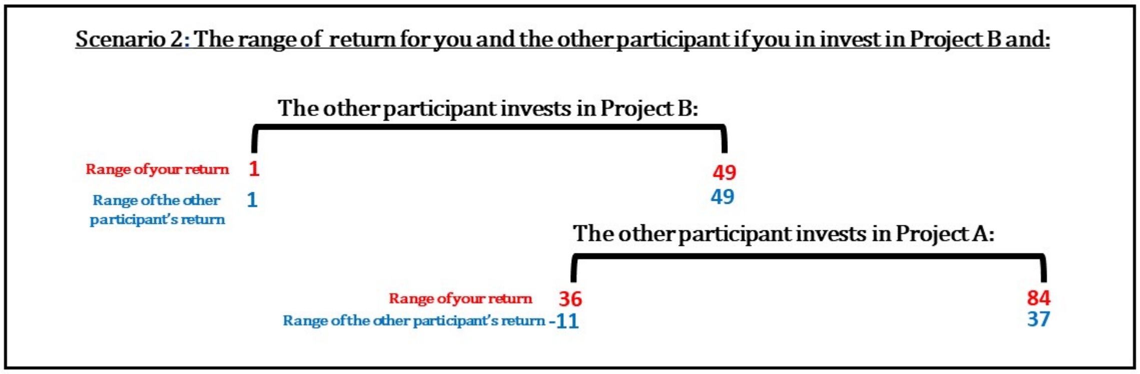
After you have made an investment choice in A or B, the range of the returns after accounting for the random draw is:

1. **If you invest in Project A and the other participant invests in Project A:**
   - Your return will range from **24** (48 plus worst random draw -24) to **72** (48 plus best random draw +24)
   - The other participant's return will range from **24** (48 plus worst random draw -24) to **72** (48 plus best random draw +24)
2. **If you invest in Project A and the other participant invests in Project B:**
   - Your return will range from **-11** (13 plus worst random draw -24) to **37** (13 plus best random draw 24)
   - The other participant's return will range from **36** (60 plus worst random draw -24) to **84** (60 plus best random draw +24)
3. **Note that if YOUR return falls between 24 and 37, you CANNOT know for sure whether the other participant invested in Project A or B. If YOUR**

return is less than 24 or greater than 37 you CAN know for sure what project the other participant invested in. (see Scenario 1 in the diagram below).**



1. **If you invest in Project B and the other participant invests in Project B:**
   - Your return will range **1** to **49**
   - The other participant's return will range from **1** to **49**
2. **If you invest in Project B and the other participant invests in Project A:**
   - Your return will range **36** to **84**
   - The other participant's return will range from **-11** to **37**
3. **Note that if YOUR return falls between 36 and 49, you CANNOT know for sure whether the other participant invested in Project A or B. If YOUR return is less than 36 or greater than 49 you CAN know for sure what project the other participant invested in. (see Scenario 2 in the diagram below).**

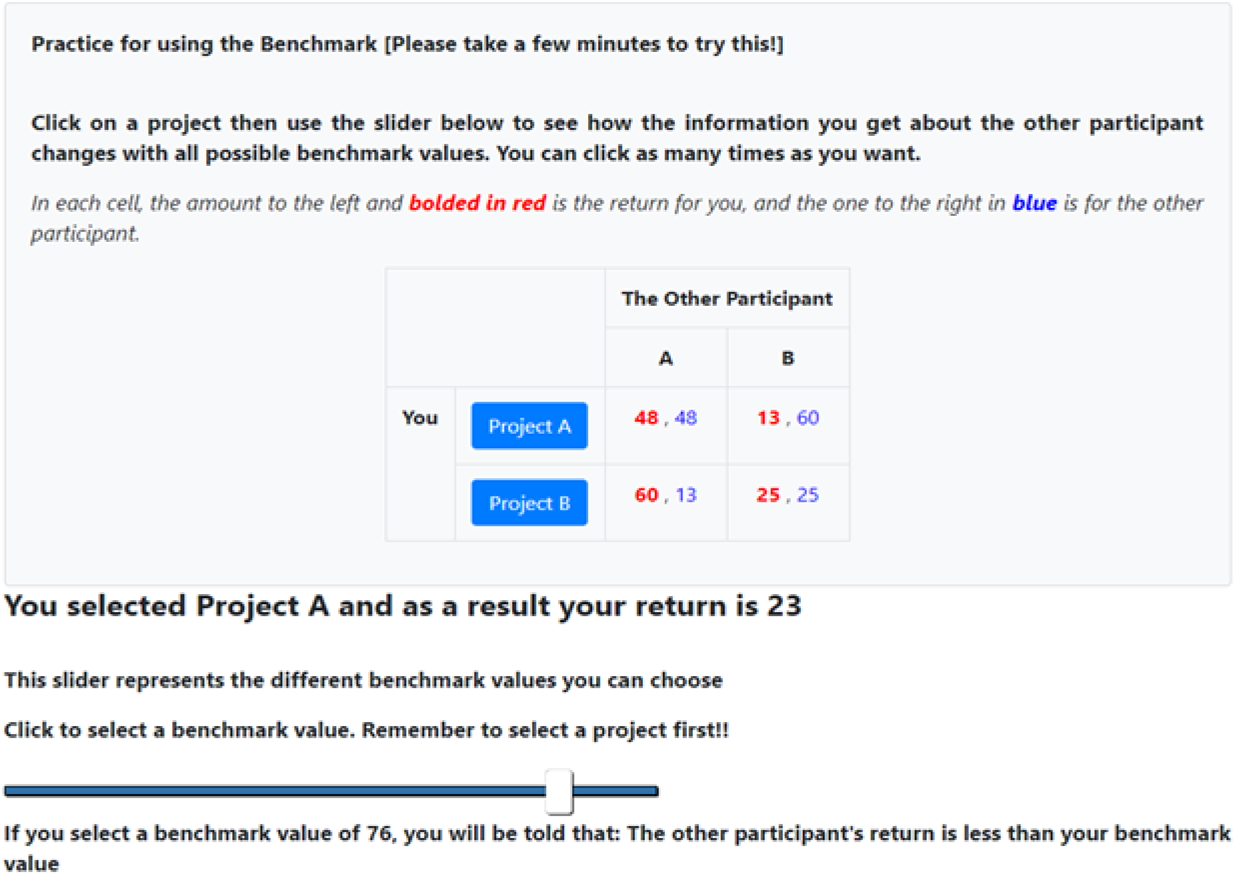


You will get a signal

You will not be told the return of the other participant, but you will always get a signal about their return. This signal will tell if the other participant's return is above, below, or equal to a benchmark value. This benchmark value can help you rule out ranges of values of the other participant's return. You will set one benchmark value that will be used if you select Project A and another if you select Project B. These benchmark values have to be within the range of possible return for both projects for the other participant. That is, 24 to 84 for Project A and -11 to 49 for Project B.

At the beginning of each match, you will be prompted to select these benchmark values. For example, if you set a benchmark value of 50 for Project A, you will be signaled that the other participant's return is above, below, and equal to 50. You only set this benchmark value at the beginning of each match. The same benchmark will be used for all the rounds in a match. When a new match begins, you will be prompted to set the benchmark value again.

Here you can simulate how the benchmark can be used. First select a project, then the computer will randomly select a project as well. Move the slider to see what information you receive about the other participant based on the benchmark you select.



Practice for using the Benchmark [Please take a few minutes to try this!]

Click on a project then use the slider below to see how the information you get about the other participant changes with all possible benchmark values. You can click as many times as you want.

In each cell, the amount to the left and ***bolded in red*** *is the return for you, and the one to the right in* ***blue*** *is for the other participant.*

		The Other Participant	
		A	B
You	Project A	**48** , 48	**13** , 60
	Project B	**60** , 13	**25** , 25

You selected Project A and as a result your return is 23

This slider represents the different benchmark values you can choose

Click to select a benchmark value. Remember to select a project first!!

If you select a benchmark value of 76, you will be told that: The other participant's return is less than your benchmark value

After Each Round

On the result page, **we will ask you what you think the chances are that the other participant chose Project A.**

Depending on your guess, you can earn 2 points or 0 points. We are interested in learning about your best and honest guesses. **You will be paid according to a formula which is specifically designed to maximize the chances that you will win the 2 points if you submit your best guess.**

Your guess will be converted into a chance-to-win. This is calculated by the computer according to a formula that is explained on separate page that you can request after the experiment. On the computer interface, you will be able to see the chance-to-win for each outcome directly below your guess.

You will not be paid for your answer until the end of the experiment. Your answer will not be shown to any other participant. Your answer will not affect the experiment in any way.

The Interface of the Experiment

Before each match, your computer screen will look like this:

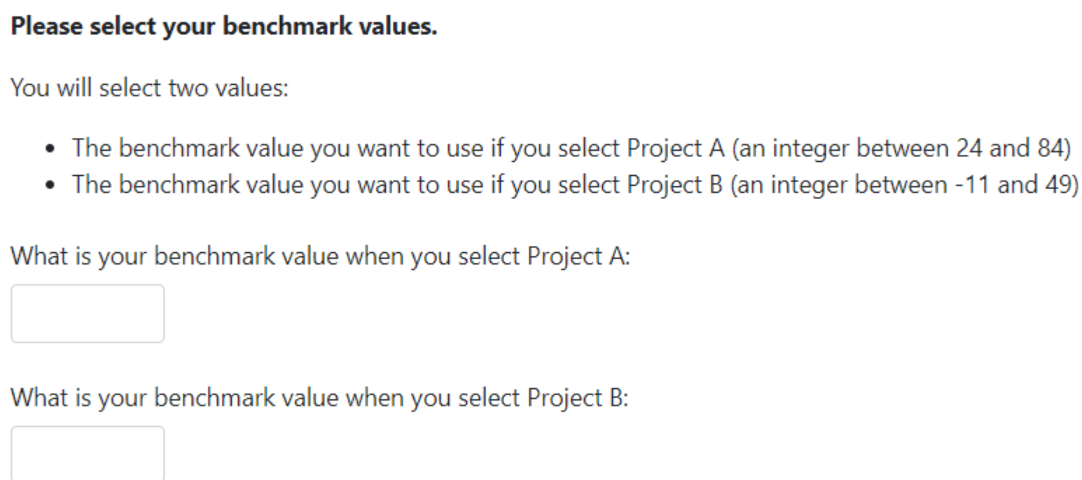


Figure A.1. Set Your Benchmark Values

After you select these benchmark values, the round will begin. In each round, the screen to select a project for you and the other participant looks like this:

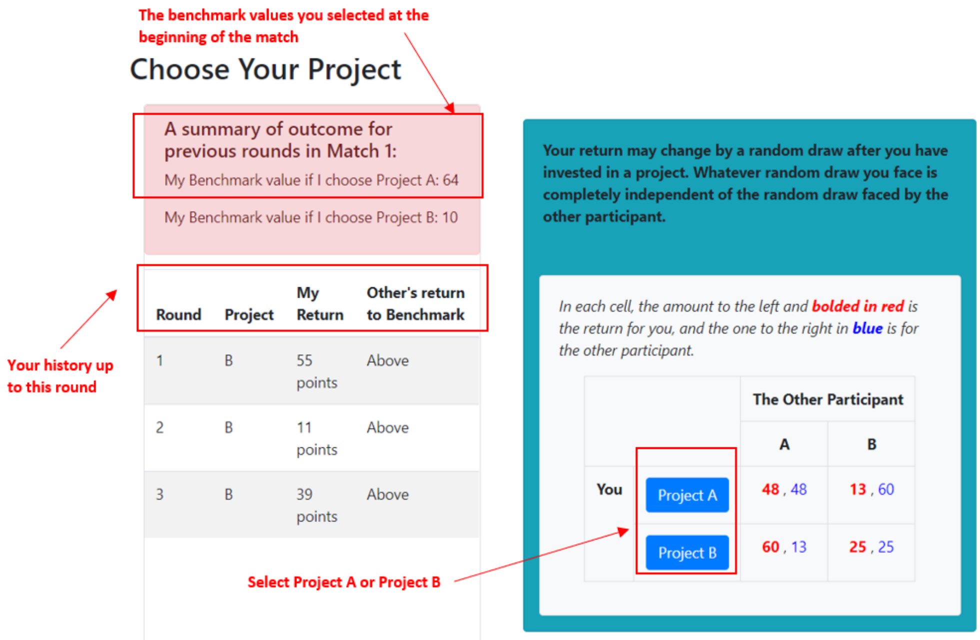


Figure A.2. Your Selection Screen

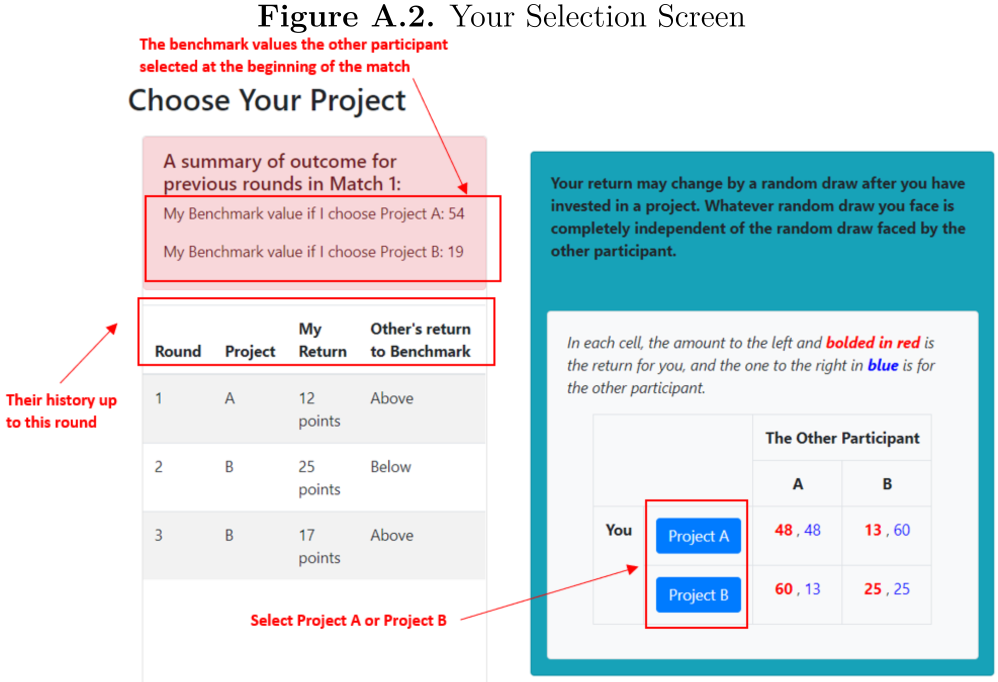


Figure A.3. The Other Participant's

This screen also displays a summary of the outcome of all previous rounds including: the project you chose; your return (inclusive of the random draw you faced); and if the other participant's return is above, below or equal to the benchmark value you set at the beginning of the match.

After you and the other participant have made a decision, your result screen will display:

- The decision made BY YOU
- YOUR realized returns (inclusive of your random draw).
- If the other participant is above, below or equal to the benchmark
- A slider for you to guess the probability that the other participant selected Project A

This is an example of what the computer screen may look like after you have made your choice:

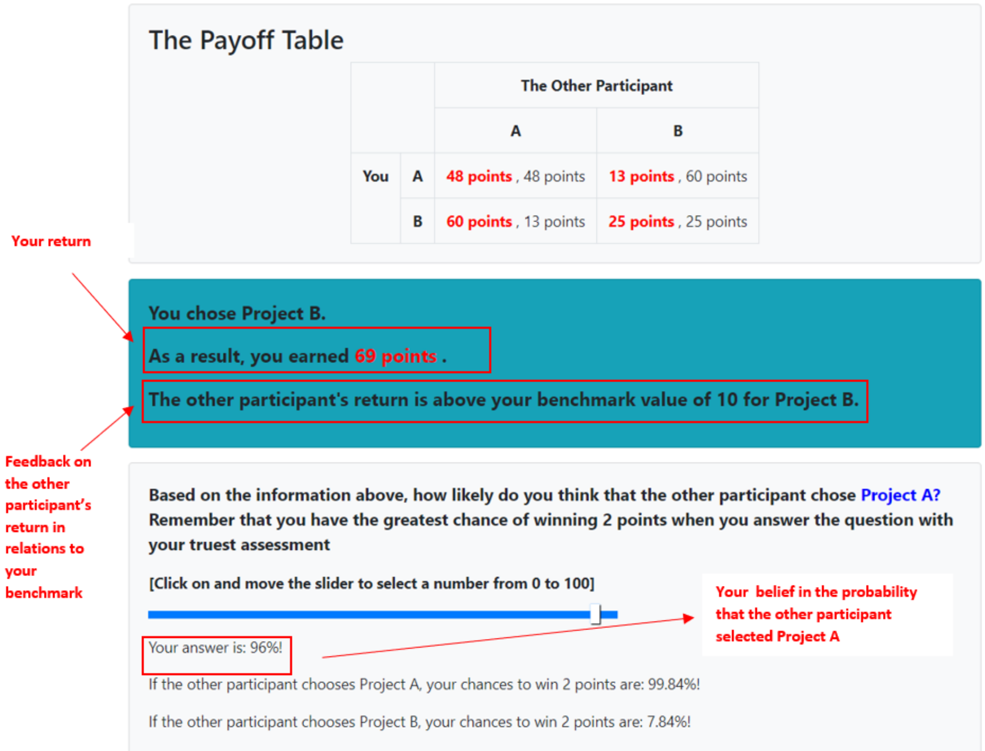


Figure A.4. Your Result Screen

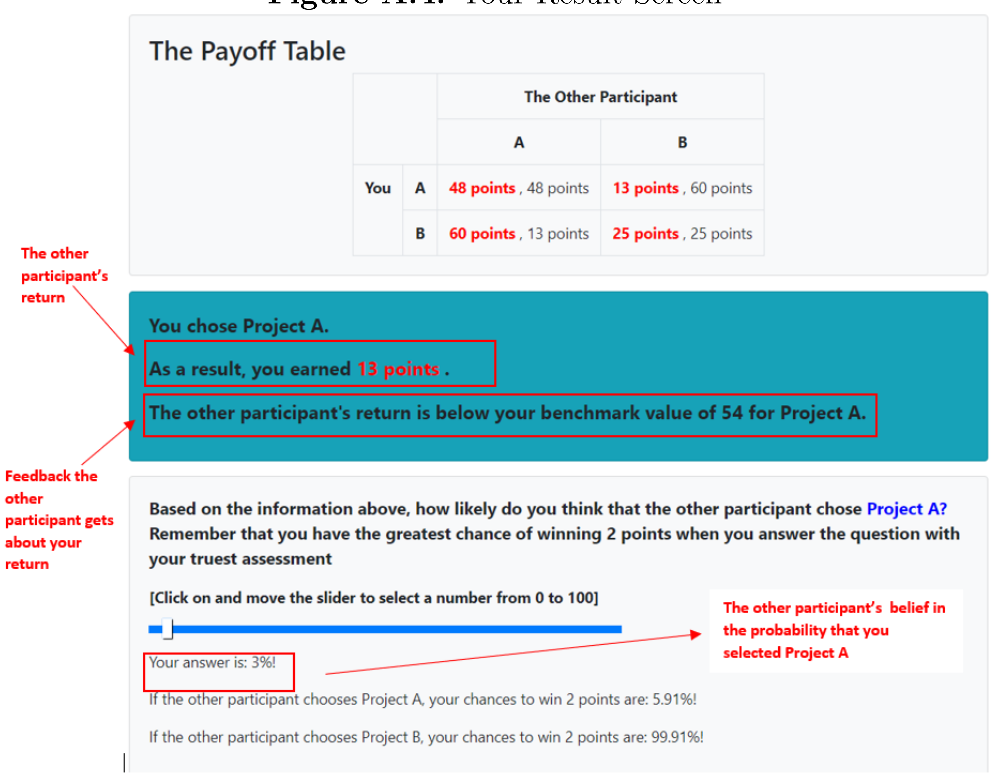


Figure A.5. The Other Participant's Result

Once a match ends, you will be randomly re-grouped with a different participant in the room for another match. Each match has the same setup. You will play a number of such matches with different people in the room.

Reminders

To summarize, the number of rounds in a match is randomly determined. After each round, there is a 90% chance that the match will continue for at least another round. You and the other participant will get a random draw. Whatever random draw you get is completely independent of the random draw faced by the other participant. This means that your random draw is completely unrelated to the random draw of the other participant.

You will not know the return of the other participant, but you will be able to select a benchmark value to signal to you the possible range of returns for the other participant. After you both have invested in a project, you will be told if the other participant's return was above, below or equal to this benchmark value. At the end of this session, you will receive $1 for every 300 point in your account. You will now take a very short quiz to make sure you understand the setup. You will earn $0.50 for each correct answer.

After the quiz, you will play 4 practice rounds to get you familiarized with the game. For the practice rounds, you will play against the computer and NOT the other participants. The computer will randomly select responses. Also, you will be able to select benchmark values for each round. This ONLY happens for the practice rounds. After the practice rounds, you will begin playing with the other participants in the room.

A.3 Description of Strategies

Strategy	Description
AD	Always defect
DTFT	Defect in the first round, then play TFT
DGRIM2	Defect in the first round, then play GRIM2
GRIM	Cooperate until the other player defects, then defect forever
TFT	Cooperate unless other player played defection in the last round
2TFT	Cooperate unless other player defected in either of the last two rounds
GRIM2	Cooperate until the other player defects in 2 consecutive rounds, then defect forever
GRIM3	Cooperate until the other player defects in 3 consecutive rounds, then defect forever
TF2T	Cooperate unless other player defected in both of the last two rounds
AC	Always cooperate

A.4 Feedback After Supergame

Match 1 has ended. Your cumulative decision and payoff for this match is:

All periods:

Rounds	Your Decision	Your Return	Other Decision	Other Return
1	A	-8 points	B	39 points
2	B	38 points	A	-10 points
3	A	35 points	B	82 points
4	B	66 points	A	20 points
5	B	51 points	A	-5 points
6	A	50 points	A	52 points
7	A	41 points	A	39 points
8	B	53 points	A	17 points
9	A	65 points	A	60 points

A.5 The Maximum Likelihood Estimation Method

We use the Strategy Frequency Estimation Method (SFEM) from Dal Bó and Fréchette (2011) to estimate the fraction of strategies employed in each treatment. This methodology uses a Maximum Likelihood Estimation (MLE) to estimate the frequency with which each strategy from a set of pre-determined set of strategies is found experimental data. This methodology has since been employed Fudenberg, Rand, and Dreber (2012), Rand, Fudenberg, and Dreber (2015), Dal Bó and Fréchette (2018), Aoyagi, Bhaskar, and Fréchette (2019), Dal Bó and Fréchette (2019) and Romero and Rosokha (2019), for example. This method assumes that each subject uses the same strategy across supergames. However, they can make mistakes. These mistakes are not the errors that are generated from the experimental design, but rather, it is assumed that subjects can make mistakes when choosing their intended actions for the particular strategy they are following.

Using the notations of Dal Bó and Fréchette (2011), assume that the probability with which subject i makes mistakes is $1-\beta$ and the probability that her chosen actions correspond with a strategy k is β. The likelihood that her observed choices were actually generated by strategy k is $Pr_{\mathrm{i}}(s^k) = \Pi_{M_{\mathrm{i}}}\Pi_{R_{\mathrm{i}m}}(\beta)^{I^k_{\mathrm{i}mr}}(1-\beta)^{1-I^k_{\mathrm{i}mr}}$. $I^k_{\mathrm{i}mr}$ is an indicator function that takes the value 1 when the choice that was actually made in round r and supergame m is the same as what the subject would have made if she were following strategy k. It is coded 0 otherwise. M and R are the sets of supergames and rounds. β is estimated within the model. It can also be interpreted as the probability that an action is taken given that it is prescribed by a strategy k. Therefore β is the basis for evaluation of model fit, that is, as the model fit improves β approaches 1.

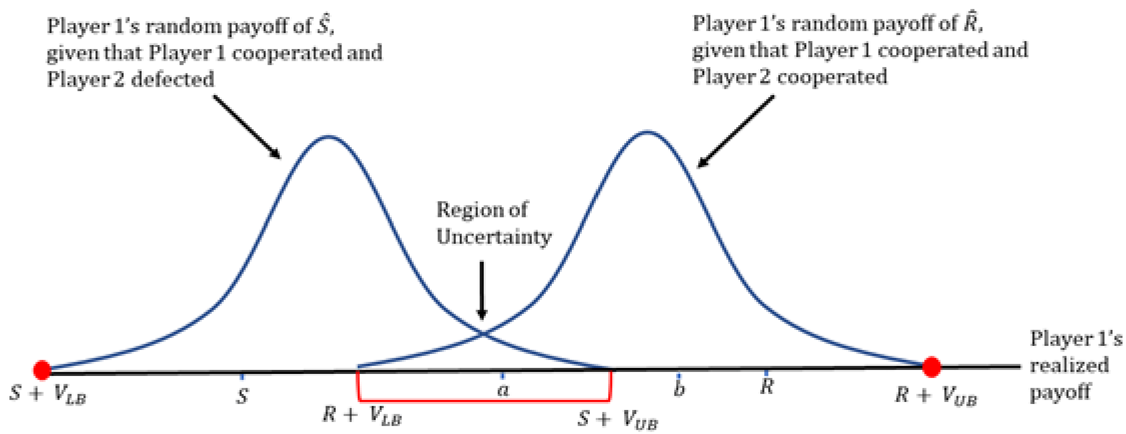
Therefore, the MLE process entails choosing both the probability of mistakes and the frequency of strategies that maximizes the likelihood of the sequences of choices. That is, the log-likelihood is $\Sigma_I ln(\Sigma_K \phi^k Pr_{\mathrm{i}}(s^k))$, where K is the subset of strategies being considered and ϕ^k is a vector of parameter estimates that represent the frequency of strategies.

We bootstrapped the standard errors in a way that respects the data generating process of our experimental data. We randomly draw the appropriate number of sessions, then for each session the appropriate number of subjects, then supergames. All with replacement. The bootstrapping process was done 1000 times. The standard deviation of the bootstrapped MLE estimates provide the standard errors.

B. NUMERICAL SIMULATION

B.1 The Player's Own Payoff Distributions

This is the distribution of realized payoff for player when she cooperates according to Bendor (1993). The shocks faced by each player is independent. The size of the region of uncertainty is determined by the variance of the shock faced by the player. In Bendor (1993), each player sets critical cutoff values such that Type 1 and Type 2 errors have the same probability p of occurring, where $\frac{1}{2} > p > 0$. Also, each player knows where the realized payoff of their opponent lies in relation to the critical cutoff value.



B.2 Bayesian Updating Process

		Signal	
		$s = 0$	$s = 1$
State of Nature	$\theta = C$	π_c	$1 - \pi_c$
	$\theta = D$	$1 - \pi_D$	π_D

Let $s = 0$ be that player 1 received a signal that player 2's realized payoff is above the benchmark value and $s = 1$ be that it is below.

Using a Bayesian approach, player 1 will update her prior after she receives a signal according to:

$$P(\theta = C/s = 0) = \frac{P(\theta = C \cap s = 0)}{P(s = 0)}$$

$$P(\theta = C/s = 0) = \frac{P(s = 0/\theta = C)P(\theta = C)}{P(s = 0)}$$

Directly from Bayes' rule:

$$P(\theta = C/s = 0) = \frac{\pi_C P(\theta = C)}{\pi_C P(\theta = C) + (1 - \pi_C)(1 - P(\theta = C))}$$

Where $P(\theta = C) = 0.5$. This is player 1's prior belief on the probability of cooperation. π_C ($P(s = 0/\theta = C)$) can be thought of as player 1's belief of the type of signal that is possible when the other player cooperates. With these information, player 1 can calculate $P(\theta = C/s = 0)$, the probability that the true state is cooperation given that there is signal that player 2 is above the benchmark value.

The decision rule:

A signal is informative, or support your belief about the state, if and only if $P(\theta = C/s = 0) > P(\theta = D/s = 0)$ and $P(\theta = C/s = 1) < P(\theta = D/s = 1)$. That is $\pi_C > 1 - \pi_D$ and $1 - \pi_C < \pi_D$. Using the former expression, a signal is informative about a state if it is more likely to occur in the cooperation state and not very likely to occur in the defection state.

If player 1 is cooperating and gets a signal that player 2 is above a benchmark value, Table B.1 shows Player 1's beliefs about Player 2's most likely action.

ρ	RP	$T1$	$T2$	π_C	$1 - \pi_C$	π_D	$1 - \pi_D$	Error	P2's most likely action
0.9	24	0.00	0.03	0.00	1.00	0.03	0.97	0.03	D
0.4	24	0.06	0.25	0.06	0.94	0.25	0.75	0.31	D
0.0	24	0.36	0.35	0.36	0.64	0.35	0.65	0.71	D
-0.4	24	0.23	0.55	0.77	0.23	0.45	0.55	0.78	C
-0.9	24	0.00	0.34	1.00	0.00	0.66	0.34	0.34	C
0.9	25	0.00	0.02	0.00	1.00	0.02	0.98	0.02	D
0.4	25	0.10	0.25	0.10	0.90	0.25	0.75	0.35	D

0.0	25	0.36	0.36	0.36	0.64	0.36	0.64	0.72	D
-0.4	25	0.31	0.55	0.69	0.31	0.45	0.55	0.86	C
-0.9	25	0.00	0.26	1.00	0.00	0.74	0.26	0.26	C
0.9	26	0.00	0.01	0.00	1.00	0.01	0.99	0.01	D
0.4	26	0.13	0.24	0.13	0.87	0.24	0.76	0.37	D
0.0	26	0.36	0.36	0.36	0.64	0.36	0.64	0.72	D
-0.4	26	0.36	0.52	0.64	0.36	0.48	0.52	0.88	C
-0.9	26	0.00	0.21	1.00	0.00	0.79	0.21	0.21	C
0.9	27	0.00	0.01	0.00	1.00	0.01	0.99	0.01	D
0.4	27	0.14	0.23	0.14	0.86	0.23	0.77	0.37	D
0.0	27	0.36	0.36	0.36	0.64	0.36	0.64	0.72	D
-0.4	27	0.40	0.51	0.60	0.40	0.49	0.51	0.91	C
-0.9	27	0.01	0.18	0.99	0.01	0.82	0.18	0.19	C
0.9	28	0.00	0.00	0.00	1.00	0.00	1.00	0.00	D
0.4	28	0.16	0.21	0.16	0.84	0.21	0.79	0.37	D
0.0	28	0.35	0.36	0.35	0.65	0.36	0.64	0.71	D
-0.4	28	0.41	0.50	0.59	0.41	0.50	0.50	0.91	C
-0.9	28	0.02	0.16	0.98	0.02	0.84	0.16	0.18	C
0.9	29	0.00	0.00	0.00	1.00	0.00	1.00	0.00	D
0.4	29	0.17	0.21	0.17	0.83	0.21	0.79	0.38	D
0.0	29	0.36	0.36	0.36	0.64	0.36	0.64	0.72	D
-0.4	29	0.44	0.49	0.56	0.44	0.51	0.49	0.93	C
-0.9	29	0.03	0.10	0.97	0.03	0.90	0.10	0.13	C
0.9	30	0.00	0.00	0.00	1.00	0.00	1.00	0.00	D
0.4	30	0.19	0.20	0.19	0.81	0.20	0.80	0.39	D
0.0	30	0.36	0.36	0.36	0.64	0.36	0.64	0.72	D
-0.4	30	0.46	0.48	0.54	0.46	0.52	0.48	0.94	C
-0.9	30	0.05	0.06	0.95	0.05	0.94	0.06	0.11	C
0.9	31	0.00	0.00	0.00	1.00	0.00	1.00	0.00	D

0.4	31	0.20	0.19	0.20	0.80	0.19	0.81	0.39	D
0.0	31	0.36	0.36	0.36	0.64	0.36	0.64	0.72	D
-0.4	31	0.47	0.45	0.53	0.47	0.55	0.45	0.92	C
-0.9	31	0.07	0.05	0.93	0.07	0.95	0.05	0.12	C
0.9	32	0.00	0.00	0.00	1.00	0.00	1.00	0.00	D
0.4	32	0.21	0.17	0.21	0.79	0.17	0.83	0.38	D
0.0	32	0.36	0.36	0.36	0.64	0.36	0.64	0.72	D
-0.4	32	0.49	0.44	0.51	0.49	0.56	0.44	0.93	C
-0.9	32	0.09	0.03	0.91	0.09	0.97	0.03	0.12	C
0.9	33	0.01	0.00	0.01	0.99	0.00	1.00	0.01	D
0.4	33	0.22	0.16	0.22	0.78	0.16	0.84	0.38	D
0.0	33	0.36	0.36	0.36	0.64	0.36	0.64	0.72	D
-0.4	33	0.50	0.41	0.50	0.50	0.59	0.41	0.91	C
-0.9	33	0.14	0.02	0.86	0.14	0.98	0.02	0.16	C
0.9	34	0.01	0.00	0.01	0.99	0.00	1.00	0.01	D
0.4	34	0.23	0.15	0.23	0.77	0.15	0.85	0.38	D
0.0	34	0.36	0.36	0.36	0.64	0.36	0.64	0.72	D
-0.4	34	0.51	0.39	0.49	0.51	0.61	0.39	0.90	C
-0.9	34	0.18	0.01	0.82	0.18	0.99	0.01	0.19	C
0.9	35	0.01	0.00	0.01	0.99	0.00	1.00	0.01	D
0.4	35	0.23	0.13	0.23	0.77	0.13	0.87	0.36	D
0.0	35	0.36	0.36	0.36	0.64	0.36	0.64	0.72	D
-0.4	35	0.53	0.36	0.47	0.53	0.64	0.36	0.89	C
-0.9	35	0.21	0.00	0.79	0.21	1.00	0.00	0.21	C
0.9	36	0.02	0.00	0.02	0.98	0.00	1.00	0.02	D
0.4	36	0.25	0.10	0.25	0.75	0.10	0.90	0.35	D
0.0	36	0.36	0.36	0.36	0.64	0.36	0.64	0.72	D
-0.4	36	0.53	0.31	0.47	0.53	0.69	0.31	0.84	C
-0.9	36	0.25	0.00	0.75	0.25	1.00	0.00	0.25	C

0.9	37	0.03	0.00	0.03	0.97	0.00	1.00	0.03	D
0.4	37	0.25	0.06	0.25	0.75	0.06	0.94	0.31	D
0.0	37	0.36	0.35	0.36	0.64	0.35	0.65	0.71	D
-0.4	37	0.55	0.20	0.45	0.55	0.80	0.20	0.75	C
-0.9	37	0.29	0.00	0.71	0.29	1.00	0.00	0.29	C

Table B.1. RP: realized payoff of Player 1; T1: Type 1 Error; T2: Type 2: Error; Error: Inferential Error; P2: Player 2

B.3 Opponent's Distribution

Figures B.1 and B.2 give the distribution of payoffs for player 2 for different values of realized payoff for player 1 under different correlation structures. For each scenario, two distributions are generated assuming that player 1 is cooperating: (1) the distribution of player 2 realized payoffs when she cooperates as well (CC), and (2) the distribution of player 2 realized payoffs when she defects (CD). One million simulations are done for each. For each simulation, all possible realized payoff for player 2 for a specific realized payoff for player 1 is plotted. A simulation is done for each distribution under every scenario. While we only display realized payoff of 29, 30, 31, 32, 33 (Figure B.1) (we used the corresponding region for player 1 defecting: 41, 42, 43, 44, 45 (Figure B.2) for player 1, we simulated all possible payoffs in the region of uncertainty including the boundaries.

Figure B.3 gives an example of the signal player 1 receives about the realized payoff for player 2. Player 1 sets a benchmark value (the red vertical line) such that Type 1 and Type 2 errors are the same when $\rho = 0$ This benchmark value will also be used for every other values of ρ. Player 1 will receive a signal that tells if player 2's realized payoff is above or below this benchmark value. If we focus on the scenario where player 1 has a realized payoff of 31, if shocks are positively highly correlated, the mass of the CC distribution lies below the benchmark value, and the mass of the of the CD distribution lies above the benchmark value. If player 1 is told that player 2's realized payoff is above the benchmark value (in other words, player 2 got a relatively big realized payoff), if they assumed player 2 defected, the probability of being correct is very large (area of the CD distribution above the benchmark value) and the probability of being wrong (area of the CC distribution above the benchmark value) is very small. The probability of being incorrect increases as $|\rho|$ decreases. The analysis is similar if player 1 is defecting. Figure B.4 shows the distributions when player 1 defects.

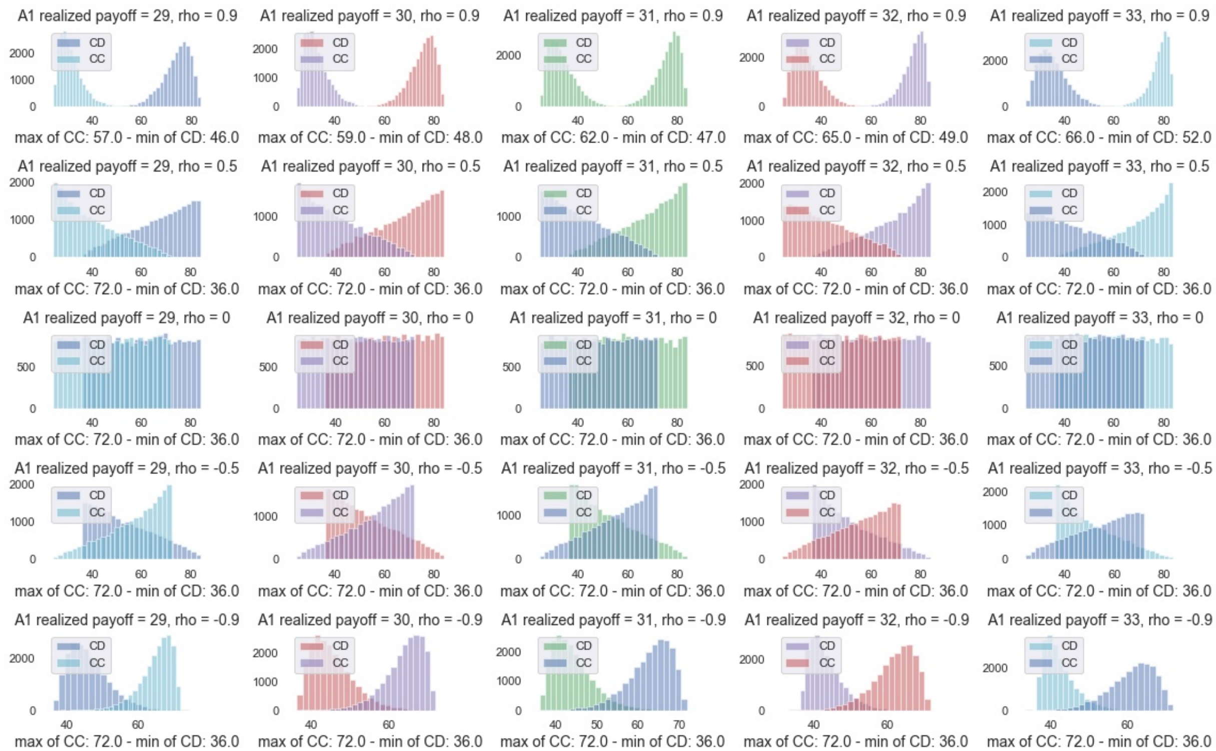


Figure B.1. The distribution of player 2's realized payoff for various values of ρ and realized payoff of player 1, when player 1 plays cooperate.

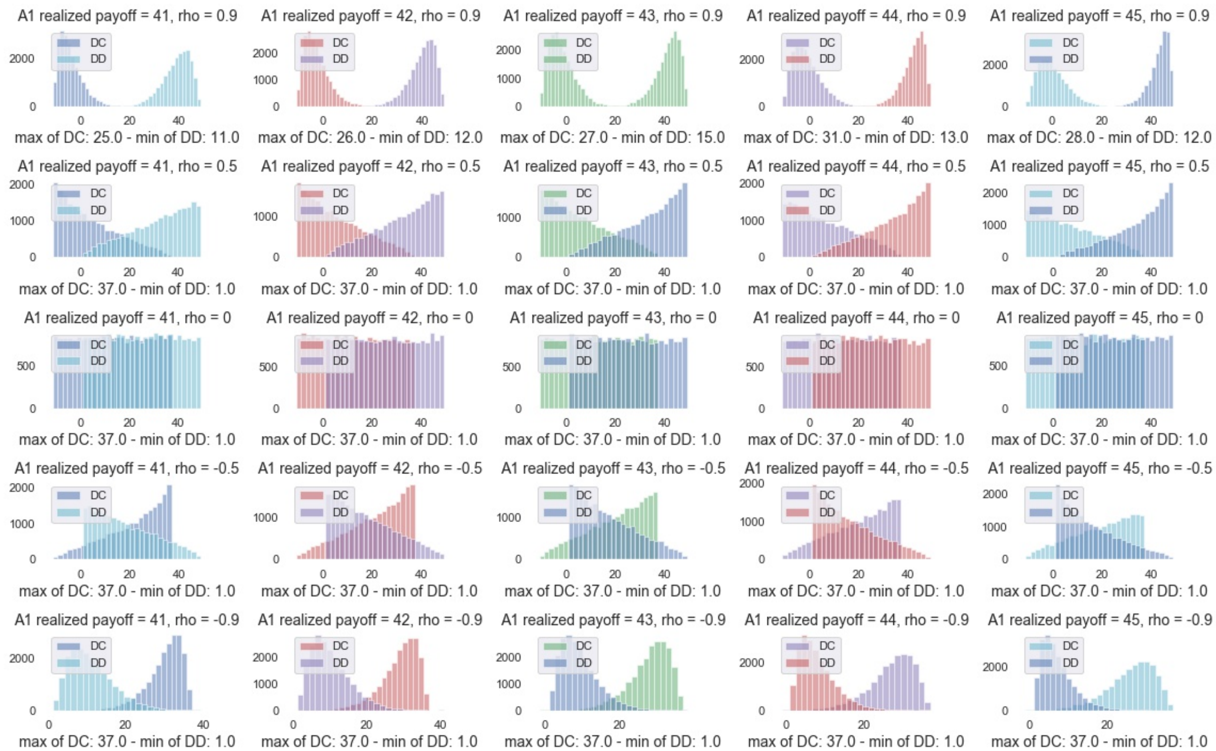


Figure B.2. The distribution of player 2's realized payoff for various values of ρ and realized payoff of player 1, when player 1 plays defect.

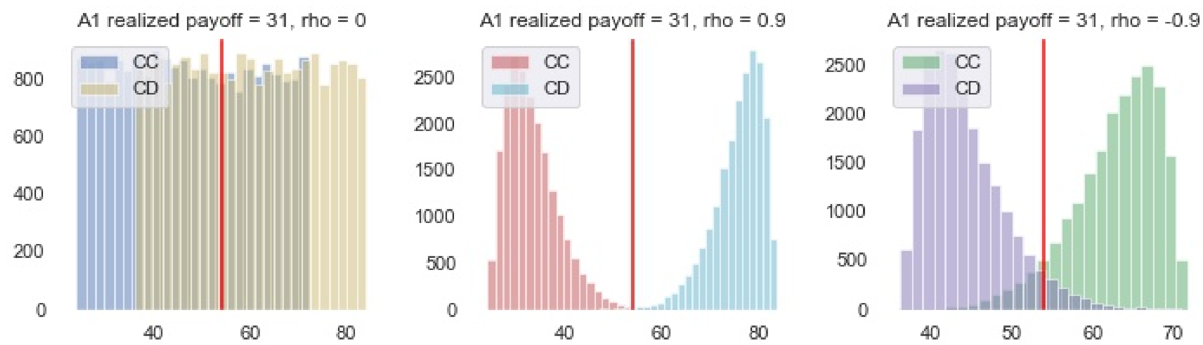


Figure B.3. The distribution of player 2's realized payoff for a realized payoff of 31 for player 1, along with the benchmark value set by player 1. Player 1 will always get a signal that tells if player 2's realized payoff lies above or below this benchmark value. Player 1 is cooperating.

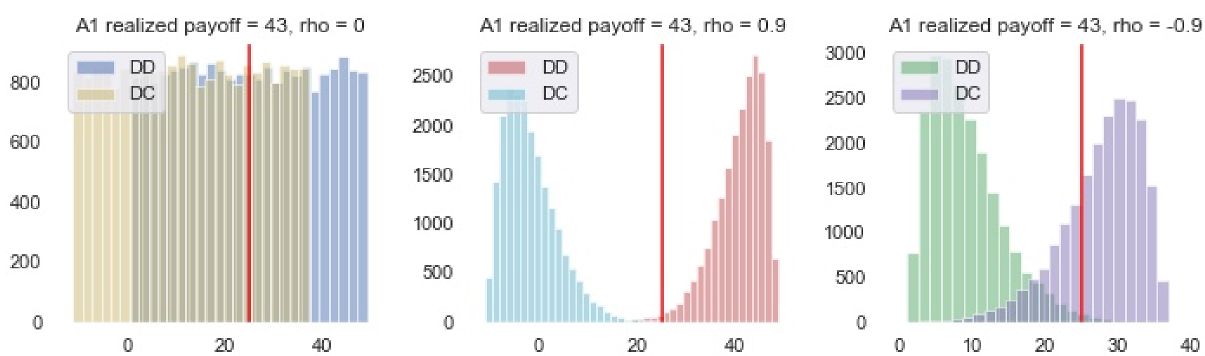


Figure B.4. The distribution of player 2's realized payoff for a realized payoff of 43 for player 1, along with the benchmark value set by player 1. Player 1 will always get a signal that tells if player 2's realized payoff lies above or below this benchmark value. Player 1 is defecting.

B.4 Details on the Automata

There are 128 combinations of automata using 7-bit strings (2^7). However, different automata can represent the same strategy. As such, there are 26 unique strategies. Of the 128 strategies, forty of them represents AD and forty represents AC. The other 24 unique strategies have two different 7-bit string representation.

To facilitate the noisy signal – two benchmark values – like subjects in the lab, strategies select these at the beginning of each generation. While we cannot theorize the process through which subjects select these benchmark values, the automata are programmed to randomly select these. With is method, we can see what benchmark values survive the evolutionary process. To accommodate the strategy and benchmark values, a 21-bit string is generated for each strategy. In Figure B.5, we show an example of a representation for TFT. The first 7-bit string translate into a benchmark value of 44 if C is played. The second 7-bit string translated into 15 if D is played. The third 7-bit string represents the strategy.

Figure B.5. TFT Representation

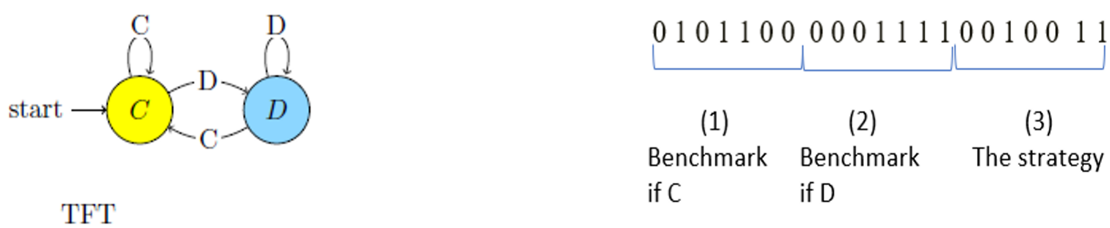


Figure B.6 shows how the strategies are coded along with the representation for TFT and DTFT. The strategies are coded as having an initial state and internal states. The first bit indicates that the automaton begins in state 0. The other two bits (first bit for state 0 and second bit for state 1) prescribes the action for each state. The next two gives the transition rule if cooperation is observed in each state and the final two bits give the transition for each state if defection is observed. For example, for TFT, the final 6 bits suggest the following action. The first two [0 1] says play C in state 0 and play D in state 1. The second two [0 0] prescribes the action in each state if the other player is observed to have cooperated. It says that if you are in state 0, transition to state 0. And, if you are in state 1, transition to state 0. The final 2 bits [1 1] prescribes the action in each state if the other player is observed to have defected. If you are in state 0 transition to state 1 and if you are in state 1 transition to state 1. Note that, in the diagrammatic representation, the transition states are represented by the labelled arcs and a vortex shows the internal state. The player's prescribed action is represented by the letter in the middle.

Figure B.6. Coding of the Strategies

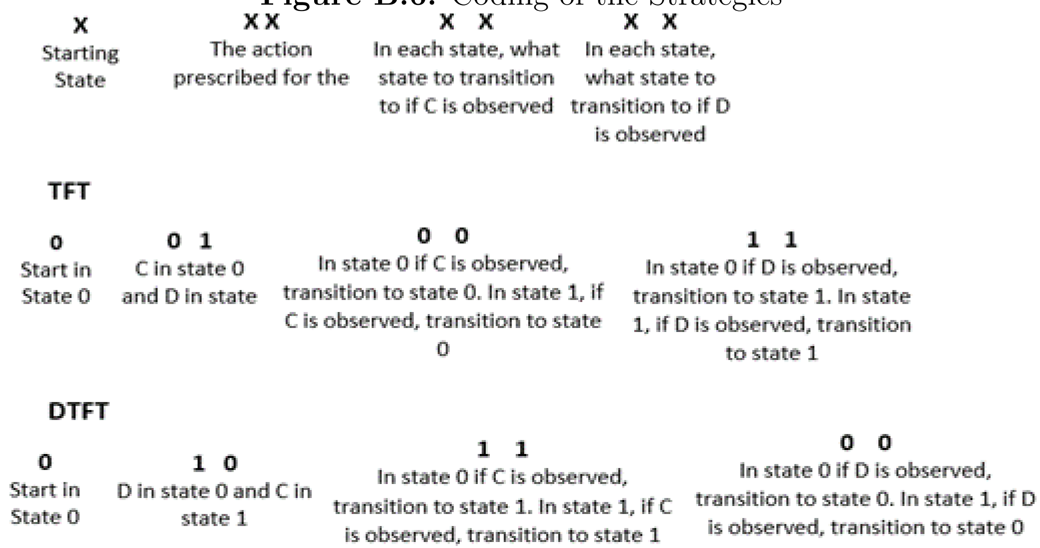
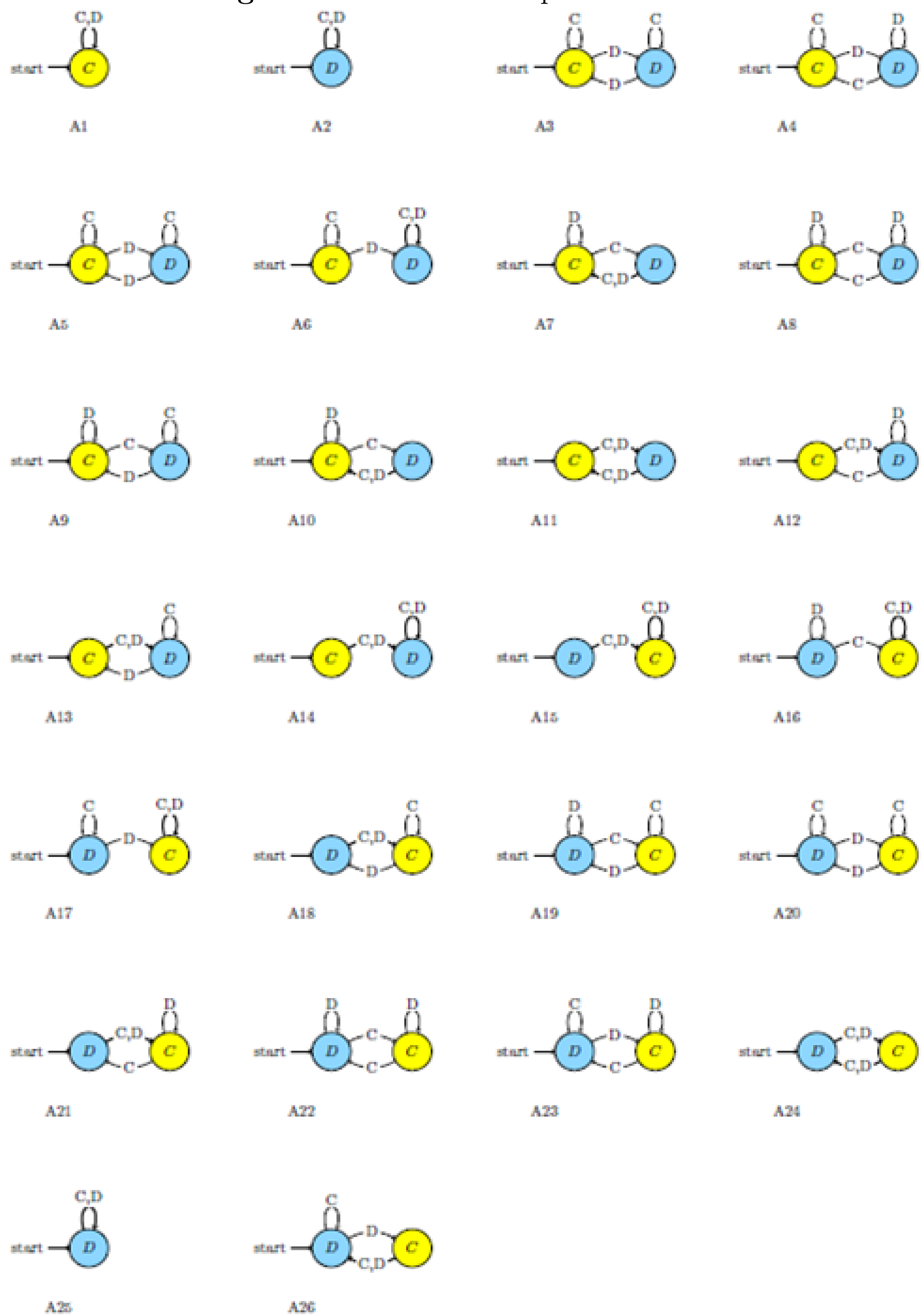


Figure B.7. The 26 Unique Automata



C. RESULTS OF THE SPATIAL MODEL

Table C.1. Results of the spatial-pricing and location model

	t=1.5	t=4
$\hat{m}_p$	0.56	0.5
	(0.04)	(0.04)
$\hat{\alpha}_p$	0.4	0.49
	(0.16)	(0.09)
$\hat{x}_p$	0.39	0.51
	(0.05)	(0.05)
$\hat{m}_{m,c}$	1	1
	(0.00)	(0.00)
$\hat{\alpha}_{m,c}$	1	1
	(0.00)	(0.00)
$\hat{m}_{m,I}$	0.67	0.5
	(0.03)	(0.04)
$\hat{\alpha}_{m,I}$	0.7	0.5
	(0.21)	(0.09)
$\hat{x}_m$	0.36	0.52
	(0.02)	(0.01)

This table shows the median values of $\hat{m}_\mathrm{i}$, $\hat{\alpha}_\mathrm{i}$, and the distance between the two processors $\hat{x}$. Where i is pure market (p), mixed market (m), cooperative (c) and IOF (I). Standard deviation is in parenthesis.