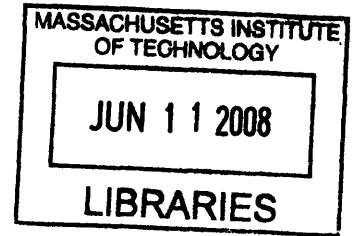


Essays in Macroeconomics and Experiments

by

Olga Shurchkov

B.A., Economics, Wellesley College (2001)



Submitted to the Department of Economics
in partial fulfillment of the requirements for the degree of

Doctor of Philosophy

at the

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

June 2008

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Abstract

This dissertation consists of four chapters on empirical and experimental macroeconomics and other experimental topics.

Chapter 1 uses a laboratory experiment to test the predictions of a dynamic global game designed to capture the role of information and coordination in speculative attacks. The game has a large number of heterogeneously informed agents deciding whether to attack a status quo; the status quo in turn collapses if enough agents choose to attack. The theory predicts that the equilibrium size of the attack is decreasing in both the underlying strength of the status quo and the agents' cost of attacking. Furthermore, the knowledge that the status quo has survived a past attack decreases the incentive to attack, implying that a new attack is possible only if agents receive new information. Our experimental evidence supports these theoretical predictions. We identify the agents' beliefs about the actions of others to be the main channel through which the relative strength of the status quo, the cost of attacking, and learning impact observed behavior. However, we also find that the subject's actions are overly aggressive relative to the theory's predictions. Once again, we find that the excess aggressiveness in actions stems from the aggressiveness of their beliefs about others' actions.

Chapter 2 studies gender inequality in performance. One explanation for this inequality is that the genders perform differently under competitive conditions, as previous experimental studies have found a significant gender gap in competitive tasks that are perceived to favor men. We use a verbal task that is perceived to favor women and find no gender difference under competition *per se*. We also reject the hypothesis that a "stereotype threat" explains the inability of women to improve performance under competition: even in verbal tasks, competition does not increase women's performance. We offer an alternative explanation for this finding: namely, that women and men respond differently to time pressure. With reduced time pressure, competition in verbal tasks greatly increases the performance of women, such that women significantly outperform men. This effect appears largely due to the fact that extra time in a competition improves the quality of women's work, leading them to make fewer mistakes. On the other hand, men use this extra time to increase the quantity of work, which results in a greater number of mistakes.

Chapter 3 studies the effects of institutions on development in post-Communist Russia. Even though Russia transitioned to a democratic institutional system in 1991, old Communist institutions persist in some of its regions. These "shadow institutions" have a significant effect on economic outcomes and, in particular, on small business development. We show that regions

run by old Communist elites have had lower levels of economic development than regions led by newcomers to the political arena.

Chapter 4 uses a laboratory experiment to investigate whether an uninformative announcement by an outsider can help us detect multiplicity in a dynamic global game setting. When theory predicts a unique equilibrium, the announcement should have no effect on behavior. In the presence of multiplicity, the announcement may serve as a coordination device. The experimental results suggest that the effect of the uninformative announcement is significant only in circumstances where information dynamics result in multiple equilibria. Moreover, the announcement seems to impact observed behavior through its effect on the subjects' beliefs about others' actions.

Thesis Supervisor: George-Marios Angeletos

Title: Professor of Economics

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Acknowledgments

This thesis is only possible due to the brilliant advice, constant support, and boundless patience of my primary advisor, Marios Angeletos. His passion for research, his keen and profound intuition for understanding the real world, and his dedication to intellectual excellence have been a true inspiration throughout graduate school and will continue to shape my life as an economist in the future. He has taught me the principles behind economic research: to approach questions in a rigorous manner, to organize ideas clearly and concisely, and to see the big picture. I cannot thank him enough for his generosity with his time and for his patience.

I am deeply indebted also to Ernst Fehr. His engaging teaching introduced me to the use of experiments in various fields of economics. Without his guidance and confidence in me, I would not have had the courage to combine macroeconomics and experiments in my research. Ernst was extremely generous, inviting me to Zurich and providing me with the financial support I required for my first experiment. I hope that our joint work will continue into the future.

I owe a debt of gratitude to Daron Acemoglu. He was my first mentor in the department, and I have learned a great deal from his enthusiasm for research, his ability to contribute to so many different areas of economics, and his creativity in the approach to answering real-world questions.

I am further grateful to Olivier Blanchard, Anna Mikusheva, Nancy Rose, Ivan Werning, Muhamet Yildiz, and the faculty members of the MIT economics department for their continued commitment to excellence in teaching, mentoring, and job market preparation. I benefited tremendously from the frequent interactions with these faculty over the years. I am also thankful to Anat Bracha, Boris Maciejovsky, and Daniel Mochon for valuable discussions that were very helpful in writing Chapter 2 of this thesis.

Finally, I would like to thank Chip Case and Akila Weerapana who were my undergraduate advisors at Wellesley College. They played an instrumental role in my development into an economist and in my decision to apply to graduate schools. More importantly, their support and friendship continue to this day – something that I cherish and value more than I can describe in words.

I gratefully acknowledge the financial support from the Institute for Empirical Research in

Economics, the National Science Foundation, and the MIT Shultz Fund. I thank the Harvard Business School Computer Laboratory for Experimental Research for providing the space for running experiments for Chapters 2 and 3 of this thesis.

My friends and colleagues, Miriam Bruhn, Filipa Sa, and Pierre Yared deserve a special mention for making my tenure at MIT enjoyable, for giving me invaluable suggestions and comments on my work, and for sharing in the joys and the sorrows of a graduate student's life. An incomplete list of my other classmates I would like to thank are Tatyana Deryugina, Moshe Cohen, Florian Ederer, Dimitris Papanikolau, Vasia Panousi, Jose Tessada, Patrick Warren, Tom Wilkening, and Ding Wu. Many thanks go to my friends outside of MIT for being such an integral part of my life: Emily Donegan, Sabrina De Melo, Anastasia Evdokunina, Inna Poliakova, Sabina Shtilman, and Jennifer Stack. I am also greatly indebted to computer programmer extraordinaire, Bobby Speaker, who helped me computerize and run the experiments for Chapter 2 of this thesis.

Finally, my time in graduate school and my research would not have been the same without the constant support and love of my husband, Mike. I thank him for his patience, for his constant confidence in my abilities, and for his unconditional love. He helped me get through my hardest times at MIT, and this thesis is dedicated to him. I am also extremely thankful to my wonderful parents, Igor and Luba Shurchkov, for pushing me to achieve more than I could ever imagine in life, for believing in my success and giving me the opportunities to attain it, and for providing me with great affection and support.

To my parents and Mike

Chapter 1

Coordination and Learning in Dynamic Global Games: Experimental Evidence

1.1 Introduction

Coordination amongst economic agents is an essential element in many macroeconomic events. The ability of individual participants to agree on a specific course of action such as an attack on a currency peg, a run on a bank, or a riot determines the ultimate outcome for the economy as a whole and can change the course of a country's history.

Currency crises are events involving coordination that have received particular attention in recent history. Within the affected country, a crisis can have an enormous negative impact on economic growth and can cause political change and turmoil.¹ Nevertheless, our ability to predict the outcome of such events and our understanding of the reasons behind their onset

¹For example, in Indonesia, the Asian economic crisis of 1997-1998 caused the growth rate of real GDP to fall from 8.2 percent in 1996 to 1.9 percent in 1997 to -14.2 percent in 1998. The figures are similar in other affected countries (IMF, 2000). Furthermore, in Indonesia, the crisis played a role in ending Suharto's long period of authoritarian rule when riots and demonstrations caused his political isolation, finally compelling him to resign. In Thailand and South Korea, democratic elections were held and opposition parties came to power for the first time since political liberalization (Freedman, 2004). The effects of these crises go beyond the borders of just one nation. More and more often, crises spread from the country of origin to other economies, threatening to cause worldwide contagion (Forbes, 2000, Boston, 2003).

and timing remain limited.

In particular, previous empirical work has failed to establish a clear relationship between the state of the macroeconomic fundamentals and the probability of a crisis (Glick and Rose, 1998, Rose 2001, Meese and Rogoff, 1983). This relationship is important because, if stronger fundamentals reduce the likelihood of a crisis, policies geared toward improving them would be advisable in times when the currency seems vulnerable to a potential attack. Moreover, the strength of the fundamentals determines the need for potentially costly policy intervention such as an interest-rate hike. However, once again, we lack clear empirical evidence on the impact of policy interventions on the occurrence of currency crises (Kraay, 2003). Furthermore, while it seems reasonable that a failed speculative attack may signal that the fundamentals are not too weak, it is not clear whether costly defense policies actually promise a prolonged period of tranquility or whether the threat of a new attack is imminent.

This thesis chapter uses an experimental approach to address the aforementioned issues. Our starting point is a dynamic global game that captures the features of currency crises that seem to be essential for understanding these issues: (1) the coordination element of currency crises that arises due to strategic complementarities in agents' actions, (2) the heterogeneity of expectations about the underlying economic fundamentals among the agents, and (3) the fact that the agents' beliefs about their ability to induce a regime change may vary over time. Unlike the common-knowledge models of crises that capture the first feature but abstract away from the second and the third features, our model delivers concrete testable predictions regarding the impact of fundamentals, policy, and information on the equilibrium outcomes. Thus, its predictions can shed light on the aforementioned issues.

While it would be desirable to test the predictions of the model using field data, several problems would arise with this approach. Field data contain additional forces not captured by the model that can limit our ability of testing, per se, the particular forces that the model focuses on. Furthermore, field data are riddled with endogeneity problems that are difficult to avoid since natural experiments are not readily available in the context of crises.² By contrast,

²For example, suppose we were to use cross-country panel data to test whether interest rate hikes have an effect on the probability of a currency crisis. For a proper test, we would need a source of exogenous variation in the interest rate (i.e., a natural experiment). However, countries that raise interest rates may do it because they are already in a crisis situation, which means that causality may go both ways.

in the laboratory, we can perfectly measure all the relevant payoff variables (“fundamentals”) and can exogenously vary the relevant “policy variables.” Furthermore, one of the goals of this study is to understand the structure of individual agents’ strategies and beliefs. However, field data on crises are typically at the aggregate level and contain no information about individual behavior and expectations. Thus, using field data would prevent us from investigating whether fundamentals, policies, and information affect outcomes and individual behavior also through expectations about the actions of others, as the theory predicts. By contrast, the beauty of a laboratory experiment is in the experimenter’s ability to elicit subjects’ beliefs.

Model. Our experiment is based on a two-period variant of the dynamic global game developed by Angeletos, Hellwig, and Pavan (2007). The model consists of a large number of agents and two possible regimes, the status quo and an alternative. The game continues into the second period as long as the status quo is in place. In each period, each agent can either attack the status quo (i.e., take an action that favors regime change), or not attack. The net payoff from attacking is positive if the status quo is abandoned in that period and negative otherwise. Regime change, in turn, occurs if and only if the percentage of agents attacking exceeds a threshold $\theta \in \mathbb{R}$ that parameterizes the strength of the status quo. The parameter θ captures the component of the payoff structure (the “fundamentals”) that is never common knowledge. In the first period, each agent receives a private signal about θ . If the game continues into the second period, agents may or may not receive more private information about θ .

Model Interpretation. Within the context of currency crises, the fundamental θ represents the strength of the currency peg or the ability of the central bank to defend the peg. The agents are the speculators deciding whether to attack the currency. The cost of attacking can be interpreted as the interest rate.

Model Predictions. The model makes the following predictions that are directly related to the aforementioned issues.

1. In the first stage, the size of the attack is monotonically decreasing in the strength of the economic fundamentals, θ ; equivalently, the agents’ strategies are decreasing in their individual private signals.
2. An individual’s propensity to attack, and, by implication, the aggregate size of the attack,

decrease in the cost of attacking.

3. In the second stage, if the agents do not receive an additional more precise private signal in the second stage, then not attacking is the unique equilibrium. This result arises from the fact that agents have learned that the regime survived a past attack, which along the equilibrium means that the fundamental θ must be good enough. We henceforth refer to this type of learning as “endogenous learning.”
4. On the other hand, under some parameter restrictions, a new attack becomes possible in the second stage if agents receive sufficiently precise new information. We henceforth refer to the arrival of new information as “exogenous learning.” While “endogenous learning” reduces the incentive to attack in the second period, “exogenous learning” can make a new attack possible.

Experimental Results. To address the policy issues discussed above, we conduct several treatments of a laboratory experiment where we vary the strength of the fundamentals, the cost of attacking, and the availability of information in the second stage.

In the first stage of the experiment, we find that the size of the attack is monotonically decreasing in the strength of the fundamentals and that subjects’ strategies are monotonic in their private signals which is consistent with the theory prediction of a unique equilibrium in monotone strategies. However, we observe that the subjects behave more aggressively relative to the theory, at least for treatments in which the cost of attacking is set to be relatively high. Furthermore, we find that the agents’ behavior is less responsive to the changes in the cost of attacking than in the theory. Thus, the experimental results show that costly policy interventions may not be as effective in preventing speculative attacks as the theory suggests.

In the second stage of the experiment, we detect the effects of the interaction between learning and coordination. In order to distinguish between the effects of endogenous and exogenous learning, we first run a treatment where the subjects do not receive any new private information. In this case, the only additional information that the subjects receive in stage two is that the game has not ended. Along the equilibrium, this means that the fundamentals are sufficiently good and serves as a source of “endogenous learning.” We find that, in the second stage of this treatment, the subjects are in fact learning from the outcome of the first

stage since the probability of attack is greatly reduced.

Next, to examine the impact of exogenous learning, we run a treatment where the subjects receive an additional precise private signal in the second stage of the experiment. In this case, the subjects are still able to learn endogenously through their observation that the experiment proceeded into stage two, but in addition, they can now learn exogenously by incorporating this more precise information into their decision to attack the status quo. We find that the probability of attack in the second stage now increases significantly relative to the treatment with endogenous learning only. Together, the second-stage findings imply that a policy-maker, having previously successfully defended the regime, cannot be assured that the crisis is averted. The endogenous learning, induced by the observation of a failed attack, alone makes the speculators relatively less aggressive, but a new attack may become possible as the agents accumulate new information about the strength of the regime.

While the theory predictions regarding the monotonicity of strategies and the importance of learning are supported by the experimental evidence, as mentioned above, we find that the subject's actions are overly aggressive relative to the theory's predictions. Therefore, we test two hypotheses about the aggressiveness of subjects' behavior. The first hypothesis postulates that the subjects are "irrational" in the sense that they are intrinsically biased toward attacking, i.e., they simply enjoy attacking. An alternative hypothesis is that the agents act rationally given their beliefs of what others will do, but it is their beliefs that are more aggressive than the theory predicts. We find evidence that leads us to reject the first hypothesis: given the subjects' aggressive expectations relative to the model predictions, their actions are mostly consistent with best-response strategies. The subjects' aggressive behavior stems from their aggressive beliefs and not from intrinsic aggressiveness. In terms of applications, this finding suggests that, even if the theory moves away from multiple equilibria in describing speculative attacks, the aggressiveness of expectations about others' actions may remain an essential determinant of equilibrium outcomes – a determinant that is currently not captured by the theory.

Related Literature. The theoretical literature on coordination games applied to currency crises starts with a seminal paper by Obstfeld (1996). Obstfeld's model is a coordination game with perfect information that yields multiple equilibria. While these so-called "second generation" models capture the self-fulfilling aspect of currency crises that can happen without any

apparent change in macroeconomic fundamentals, they can also be viewed as incomplete theory which is particularly weak in delivering useful predictions regarding the role of fundamentals and policies. Seeking to resolve this indeterminacy, Carlsson and van Damme (1993a, 1993b) and later Morris and Shin (1998) relax Obstfeld's assumption of common knowledge. They show that, under certain restrictions on the information structure, multiplicity of equilibria can be eliminated by assuming that agents receive heterogenous private information about the state of the fundamentals. This result has already been applied to several macroeconomic phenomena: see Goldstein and Pauzner (2001) and Rochet and Vives (2004) for bank runs; Corsetti, Guimaraes and Roubini (2003) and Morris and Shin (2004) for debt crises; Atkeson (2000) for riots; Chamley (1999) for regime switches; and Edmond (2005) for political change.

To capture the interaction between the two types of learning we mentioned earlier, Angeletos, Hellwig, and Pavan (2007) consider a class of dynamic global games in which agents can take actions in multiple periods and accumulate information over time. This extension emphasizes the idea that speculators have the option to take multiple shots against the currency peg and may also accumulate information over time and learn from past outcomes. The authors show that the interaction between "endogenous" and "exogenous" learning can sustain multiple equilibria, while at the same time delivering interesting predictions for the dynamics of speculative attacks – predictions that we test in this study.

The experimental literature that tests the predictions of the above models begins with studies that are based on coordination games with perfect information, in which equilibria are Pareto-rankable (as is the case in Obstfeld's model). Cooper et al. (1990) find that the observed pattern of play is accurately predicted by the Nash equilibrium concept and that coordination failures can emerge in which the outcome is a Pareto-inferior Nash equilibrium. Van Huyck, Battalio, and Beil (1990) find similar results when they study a class of tacit pure-coordination games with multiple equilibria. In particular, their experimental results suggest that the Pareto-dominant outcome is extremely unlikely either initially or in repeated play and that coordination failures arise due to strategic uncertainty. In a follow-up experiment, Cooper et al. (1992) study coordination games with nonbinding, pre-play communication. They find that in coordination games with a cooperative strategy, one-way communication increases the prevalence of the Pareto-dominant equilibrium relative to the no-communication baseline.

Several papers explore the effects of dynamics in a coordination environment. Cheung and Friedman (2006) maintain the common-knowledge assumption and examine speculative attacks with varying amounts of public information, focusing on size asymmetries (i.e., the effect of a large player on behavior and outcomes). They find that weaker (or more rapidly deteriorating) fundamentals increase the likelihood of successful speculative attacks and hasten their onset, and that public access to information about either the net speculative position or the fundamentals also enhances success. The presence of a larger speculator further enhances success. Other studies explore coordination dynamics with herding. Brunnermeier and Morgan (2006) examine “clock games” that end when the third of six players exits, and those three players receive a payoff that increases continuously in the exit time. The authors report that, consistent with the unique symmetric pure strategy Nash equilibrium, players exit sooner when they have better information about other players’ choices and clock settings.³

Experimental studies that are most closely related to the present study involve laboratory tests of the predictions of static coordination games with private information. Cabrales, Nagel, and Armenter (2003) test the *global* coordination game theory in two-person games with random matching inspired by Carlsson and van Damme (1993a). They find that, with private information about the payoffs, the subjects’ behavior converges to the theoretical prediction after enough experience has been gained. In a recent paper, Heinemann, Nagel, and Ockenfels (2004) test the predictions of the static speculative-attack model of Morris and Shin in a laboratory experiment. The authors compare sessions with private and public information, and conclude that in all sessions subjects used threshold strategies, i.e., attacked whenever the state of the fundamentals or the signal was beyond some critical state or signal, respectively. The authors point to these results as evidence in support of the theory which predicts a unique equilibrium under certain parameter restrictions. To our knowledge, this study is the first to test the predictions of *dynamic* global games in a laboratory experiment.

Finally, several studies have used field data to test the predictions of static global games. We view these studies as complementary to the experimental approach taken in this chapter. Chen, Goldstein, and Jiang (2007) find evidence that strategic complementarities generate

³Other recent studies examining effects of dynamics and coordination include Costain, Heinemann, and Ockenfels (2007) and Schotter and Yorulmazer (2003).

financial fragility using field data on mutual fund returns. Prati and Sbracia (2002) estimate the effect of the precision of information on the propensity for speculative attacks using reduced form specifications motivated by static global games, and find indirect empirical support for the theory. This evidence complements our findings of the impact of exogenous learning on outcomes. Danielsson and Peñaranda (2007) use structural estimation with data on the yen-dollar exchange rate to examine the relationship between fundamentals and liquidity that derives from a static global game. To our knowledge, there are no empirical studies that examine learning in a dynamic context.

The rest of the chapter is organized as follows. Section 1.2 introduces the model and derives the theoretical predictions to be tested. Section 1.3 describes the experimental procedures and treatments. Section 1.4 describes the experimental results. Section 1.5 provides an extension designed to capture the agents' excess aggressiveness and the heterogeneity in their strategies. Section 1.6 concludes and discusses possible extensions.

1.2 The Model

Our model is a simple two-period version of the model developed by Angeletos, Hellwig, and Pavan (2007). There are two regimes, the status quo and the alternative. The agents, indexed by i , decide simultaneously between two possible courses of action. Agent i can either choose action A (“attack”), an action that favors regime change, or choose action B (“not attack”), an action that favors the status quo. The status quo collapses if the mass of agents choosing action A (“aggregate size of the attack”), exceeds θ , which parametrizes the strength of economic fundamentals. A low value of θ thus represents a relatively weak state of the fundamentals, and a high value of θ represents a relatively strong state of the fundamentals. We will denote the regime outcome by $R_{t+1} \in \{0, 1\}$ where $R_{t+1} = 0$ refers to the survival of the status quo, while $R_{t+1} = 1$ refers to the collapse of the status quo. Action A is associated with an opportunity cost c . If action A is successful (i.e., the status quo is abandoned), each agent choosing action A earns an income of $y > c$. If not (i.e., the status quo prevails), then the agent choosing action A earns 0. Action B yields no payoff and has no cost. The payoff of an individual agent

can be written as

$$u_{it} = U(a_{it}, A_t, \theta) = \begin{cases} a_{it}(y - c) & \text{if } A_t \geq \theta \\ -a_{it}c & \text{if } A_t < \theta \end{cases} .$$

where $a_{it} \in \{0, 1\}$ denotes the action chosen by agent i at time t ($a_{it} = 1$ represents attacking and $a_{it} = 0$ represents not attacking) and A_t denotes the aggregate size of the attack at time t .

Consider for a moment the case when the state of the fundamentals, θ , is common knowledge among the agents. In that case, each agent's best response function is

$$g(A_t, \theta) = \arg \max_{a_{it} \in \{0, 1\}} U(\cdot) = \begin{cases} 1 & \text{if } A_t \geq \theta \\ 0 & \text{if } A_t < \theta \end{cases} .$$

It follows that, for all $\theta \in [0, 1]$, there are two pure-strategy Nash equilibria in this game, namely that either all agents choose action A or all agents choose action B; $[0, 1]$ is the region where multiplicity is possible. This is the type of multiplicity underlying all “second-generation” models of crises.

However, in our setup (as in any global game), agents have heterogeneous information about the strength of the status quo. Nature draws θ from a normal distribution $N(z, 1/\alpha)$ which defines the initial common prior about θ .⁴ Each agent then receives a private signal $x_{it} = \theta + \xi_{it}$, where $\xi_{it} \sim N(0, 1/\beta_t)$ is i.i.d. across agents and independent of θ and β_t is the precision of private information. We further assume that $\beta_1 \geq \frac{\alpha^2}{2\pi}$, which is a standard condition that guarantees uniqueness, as we will see in section 1.2.1.⁵ The status quo is in turn abandoned if and only if the measure of agents choosing action A, which is denoted by A_t , is greater than or equal to θ .

1.2.1 First-Period Predictions

Let us first focus on the equilibrium outcomes in the first period of the game. Note that it is strictly dominant to choose action A for sufficiently low signals, namely for $x_1 < \underline{x}$, where

⁴Note that z can also be thought of as a public signal that all agents receive. In particular, in the case when the prior is uniform, z represents a public signal and the rest of the model remains identical to the one we present here.

⁵The information structure is parameterized by $\beta_t = \sigma_{x,t}^{-2}$ and $\alpha = \sigma_z^{-2}$, the precisions of private and public information, respectively, or equivalently by the standard deviations, $\sigma_{x,t}$ and σ_z . The agents know the values of z , α , and β_t .

\underline{x} solves $Pr(\theta \leq 0|\underline{x}) = c/y$. Similarly, it is strictly dominant to choose B for sufficiently high signals, namely for $x_1 > \bar{x}$, where \bar{x} solves $Pr(\theta \leq 1|\bar{x}) = c/y$.⁶ This suggests that we should look for monotone Bayesian Nash equilibria in which the agents' strategy is non-increasing in x_1 .

Thus, suppose that there is a threshold, x_1^* , such that an agent will choose action A if and only if he gets a private signal below this cutoff ($x_1 \leq x_1^*$). The measure of agents choosing action A is then decreasing in θ and is given by $A_1(\theta) = Pr(x_1 \leq x_1^*|\theta) = \Phi(\sqrt{\beta_1}(x_1^* - \theta))$, where Φ is the c.d.f. of the Standard Normal distribution. It follows that the status quo is abandoned if and only if $\theta \leq \theta_1^*$, where θ_1^* solves $\theta_1^* = A_1(\theta_1^*)$, or equivalently

$$\theta_1^* = \Phi(\sqrt{\beta_1}(x_1^* - \theta_1^*)). \quad (1.1)$$

The posterior probability of regime change for an agent with signal x_1 is then simply $Pr(R_1 = 1|x_1) = Pr(\theta \leq \theta_1^*|x_1)$. Since the latter is decreasing in x_1 , each agent finds it optimal to choose action A if and only if $x_1 \leq x_1^*$, where x_1^* solves $Pr(\theta \leq \theta_1^*|x_1^*) = c/y$. Since posteriors about θ are normally distributed with mean $\frac{\beta_1}{\beta_1 + \alpha}x_1 + \frac{\alpha}{\beta_1 + \alpha}z$ and variance $\frac{1}{\beta_1 + \alpha}$ (precision $\beta_1 + \alpha$), this condition is equivalent to

$$\Phi\left(\sqrt{\beta_1 + \alpha}\left(\theta_1^* - \frac{\beta_1}{\beta_1 + \alpha}x_1^* - \frac{\alpha}{\beta_1 + \alpha}z\right)\right) = c/y. \quad (1.2)$$

It follows that any monotone equilibrium corresponds to a pair (x_1^*, θ_1^*) that solves the system of the two equations (1.1) and (1.2). Such a solution always exists and is unique for all z if and only if $\beta_1 \geq \frac{\alpha^2}{2\pi}$, which we have assumed to be the case. (See Appendix A for the proof.) Moreover, iterated elimination of strictly dominated strategies ensures that, when the monotone equilibrium is unique, there is no other equilibrium. This gives us the first prediction that we will later test.

Prediction 1. *There exists a unique x_1^* , such that in any equilibrium of the dynamic game, an agent chooses action A (“attack”) in the first period if and only if $x_1 < x_1^*$. By implication, $A_1(\theta)$ is decreasing in θ , and there exists a unique θ_1^* such that the status quo is abandoned in the first period if and only if $\theta < \theta_1^*$.*

⁶Note that, for notational tractability, we suppress the individual subscript, i , from now on.

Figure 1.1 illustrates the equilibrium in the first period. It plots the aggregate size of the attack, A_t , against θ .

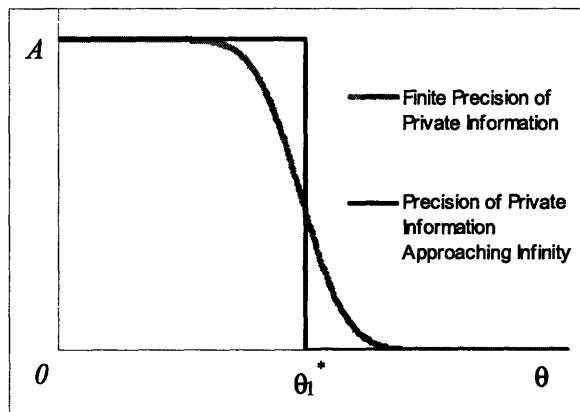


Figure 1.1: Size of the Attack, A_t , vs. θ for Finite β_1 and β_1 Approaching Infinity

As the precision of private information approaches infinity, the theory predicts that everyone should choose action A for all $\theta \leq \theta_1^*$, and no one should choose action A for all $\theta > \theta_1^*$, which is represented by the black line in Figure 1.1. However, for finite precision of private information, the theory predicts that $A_1(\theta)$ is monotonically decreasing in θ , resulting in the grey line in Figure 1.1.

An additional implication of the equilibrium in the first period is that an increase in the cost of attacking, c , reduces each agent's incentive to attack. Moreover, the higher cost also causes every agent to expect others to attack less. This provides us with the second testable prediction.

Prediction 2. *The thresholds θ_1^* and x_1^* are decreasing in c , the cost of choosing action A.*

Note that in a coordination game with complete information, such as Obstfeld (1996), the cost of attacking plays no role in equilibrium play: either everyone attacks the status quo or no one attacks the status quo, regardless of cost. Moreover, agents' strategies need not be monotonic in θ .

1.2.2 Second-Period Predictions

Recall that the game continues into the second period if and only if the status quo is in place. We will consider two possibilities for the information structure in the second period. First,

suppose that the agents receive no additional information outside the game. In this case, when agents arrive at the second period, they observe that the status quo has survived the first-period attack. From this observation and by using equilibrium reasoning, the agents can infer that the state of the fundamentals is not too weak, because otherwise it would have collapsed under the first attack. In particular, they now know that θ is above θ_1^* . This knowledge causes a first-order-stochastic-dominance shift of beliefs upwards, causing agents' behavior to become less aggressive. This effect, in turn, guarantees that no agent is willing to attack in the second period.⁷

Prediction 3. *If no new information arrives in the second period, then choosing action B (“not attack”) for all x is the unique continuation equilibrium.*⁸

Testing this prediction allows us to isolate the effects of endogenous learning on agents' behavior. However, we would also like to examine the effects of the interaction between endogenous and exogenous learning. This can be accomplished by changing the information structure in period two, such that agents receive an additional signal that is sufficiently precise. That is, $x_{i2} = \theta + \xi_{i2}$, where $\xi_{i2} \sim N(0, 1/\beta_2)$ and β_2 is sufficiently high.

The size of the attack in period two is given by $A_2(\theta) = \Pr(x_2 \leq x_2^*|\theta)$, which is decreasing in θ , and the probability of regime change for an agent with signal x_2 is $\Pr(R_2 = 1|x_2, R_1 = 0) = \Pr(\theta \leq \theta_2^*|x_2, \theta > \theta_1^*)$, which is decreasing in x_2 if $\theta_2^* > \theta_1^*$. Therefore, in any equilibrium in which an attack occurs in the second period, θ_2^* and x_2^* solve

$$\theta_2^* = \Phi(\sqrt{\beta_2}(x_2^* - \theta_2^*)) \quad (1.3)$$

$$1 - \frac{\Phi(\sqrt{\beta_2 + \alpha}(\frac{\beta_2}{\beta_2 + \alpha}x_2^* + \frac{\alpha}{\beta_2 + \alpha}z - \theta_2^*))}{\Phi(\sqrt{\beta_2 + \alpha}(\frac{\beta_2}{\beta_2 + \alpha}x_2^* + \frac{\alpha}{\beta_2 + \alpha}z - \theta_1^*))} = c/y. \quad (1.4)$$

Equations (1.3) and (1.4) are the second-period analogues of equations (1.1) and (1.2): it

⁷See Lemma 2 in Angeletos, Hellwig, and Pavan (2007) for proof of uniqueness in any monotone equilibrium. Overall uniqueness can be shown by iterated deletion of strictly dominated strategies, along the same lines as the iterated dominance argument in Appendix A.

⁸The robust prediction of the model is that the observation that the status quo has survived a first-period attack creates a large drop in the size of the attack in the second period. The prediction that the size of the attack drops exactly to zero is an artifact of the simple model. For example, this would not be the case if we were to allow θ to vary slightly across periods (see Angeletos, Hellwig, and Pavan (2007)). The extension in section 1.5 that introduces excess aggressiveness is another example of a slight change to the model that produces a non-zero size of the attack with no new information in the second stage.

is easy to check that (1.3) and (1.4) admit a solution if and only if z is sufficiently high.⁹ The intuition comes from a combination of two effects. First, the arrival of new information causes the agents to discount endogenous learning. This effect alone makes them relatively more aggressive. However, the second effect of new information is that agents now also discount the prior, z . If z is low (an “aggressive prior”), discounting it makes the agents relatively less aggressive. If z is high (“lenient prior”), discounting it makes the agents relatively more aggressive and may eventually offset the incentive not to attack induced by the knowledge that the regime survived past attacks. Thus, not surprisingly, the prediction with new information in the second period depends on the prior.

Prediction 4. *If the agents receive a new private signal in the second period such that β_2 is sufficiently large and z is sufficiently high, then there exists an x_2^* and an equilibrium in which an agent chooses action A (“attack”) if and only if $x_2 < x_2^*$.¹⁰*

In summary, we intend to test the following predictions in a laboratory experiment. In the first period, the aggregate size of attack and the individual incentive to attack are decreasing in both, the fundamental, θ , and the cost of attacking, c . In the second period, a failed attack causes a reduction in the size of attack with no new information. However, the arrival of new information increases the incentive to attack.

1.3 Overview of the Experiment

1.3.1 Procedures

We conducted six sessions of the experiment at the experimental laboratory at the Institute for Empirical Research in Economics at the University of Zurich, with the first four sessions held in June of 2006 and the following two sessions held in October of 2006. The subjects were all students at the University of Zurich. The procedure was kept the same throughout all six sessions, except that the order of the treatments was reversed to test whether the order of the

⁹For a special case where the cost of attacking equals $1/2$, “high z ” means $z > 1/2$. Under this condition, $\theta_1^*(z) < 1/2$, because $\theta_1^*(z)$ is monotonically decreasing in z and $\theta_1^*(1/2) = 1/2$. (See Appendix A for proof.)

¹⁰What matters for existence of an attack equilibrium in the second period is not z per se, but rather θ_1^* and a critical threshold θ_∞ . θ_∞ is a threshold, such that, if $\theta_1^* < \theta_\infty$, then sufficiently precise new information makes an attack possible in the second period. Because θ_1^* is a decreasing function of z , a new attack is possible if z is sufficiently high.

treatments mattered for the results. All sessions were computerized using the program z-Tree (Fischbacher, 2007). The subjects were first asked to read through and sign informed consent forms for non-biomedical research.¹¹ Paper copies of the instructions¹² were distributed to the participants prior to the beginning of the experiment. The subjects were asked to answer several control questions that tested their understanding of statistics, as well as the experimental procedures. Questions were answered in private. The subjects could not see or communicate with one another. At the end of the experiment, each participant filled out a computerized questionnaire.¹³ The questionnaire asked the subjects about their strategies, as well as their understanding of statistics and probability. At the very end, each subject was paid in cash a show-up fee equal to 15 Swiss Francs (CHF) and his or her earnings over the course of the session. Final income of each subject was first given in points and then converted to Swiss Francs at the rate of 10 points = 50 centimes for Sessions 1-2 and at the rate 10 points = 25 centimes for Sessions 3-6. Average income (including the show-up fee) was 83.8 CHF, 45.5 CHF, and 25.5 CHF for Sessions 1-2, 3-4, and 5-6, respectively.¹⁴

Each of the six experimental sessions had 30 participants divided randomly into two groups of fifteen people.¹⁵ Each session consisted of 40 independent rounds of play, with each round corresponding to a new random number θ drawn from a normal distribution $N(z, 1/\alpha)$.¹⁶ Thus, one can interpret each round as a new economy parametrized by the state of fundamentals, θ . The subjects were informed of the mean and the standard deviation of this distribution in the instructions. In addition, at the beginning of the round, each subject received a hint number (private signal, x_1) about the random number θ . In the instructions, the subjects

¹¹Copies of the informed consent forms in German or English are available upon request.

¹²A copy of instructions is available in Appendix C. Full copies of the instructions in German and English are available upon request for all treatments.

¹³Copies of the questionnaire questions in German or English are available upon request.

¹⁴Note that the differences in average incomes across sessions 3-4 and 5-6 arise due to the differences in the cost of choosing action A.

¹⁵Note that the model assumes a continuum of agents, while the experiment uses a finite number. However, this is not essential for the predictions of the model.

¹⁶This experiment relies on the use of the normal distribution, as opposed to the uniform distribution used by Heinemann, Nagel, and Ockenfels (2004). The ideas are essentially identical. The benefit of running the experiment using the normal is the tractability of the analysis. The theory involved in the dynamic case is significantly more complicated as compared to the static benchmark, which necessitates the use of the normal. The normal distribution is also relatively simple to grasp for the test subjects, since it is fully parametrized by the mean and the precision. To ensure the subjects' full understanding during the experiment, we provided them with several examples and conducted a quiz to familiarize them with the normal distribution.

were informed that the hint number is centered around the true value of θ and were given its standard deviation ($1/\beta_1$).

Each round consisted of one or two stages (periods) of decision-making. In stage 1 of each round, each subject had to decide between actions A or B as described in section 1.2. Once all the subjects chose their actions in each stage of every round, they were asked a follow-up question: “How many other members of your group do you think chose action A?” Next, each subject received the following information: if the game ended after stage one, he or she found out that action A was successful, learned the value of the unknown number, how many other subjects chose action A, and his or her payoff in the round. If the game continued into the second stage, two scenarios were possible. In the treatment without new information, the subject did not get an additional hint number (private signal) after the first stage, but only got a reminder of his or her first-stage hint number and received notification that action A was not successful. In the treatment with new information, the subjects received a new hint number if the game continued into stage 2. Analogously to the first stage, the subjects were informed that the second-stage hint number is centered around the true value of θ and were given its standard deviation ($1/\beta_2$) in the instructions.

1.3.2 Parameterization

We re-scaled all numbers by a factor of 100, so that the subjects did not have to deal with fractions. We chose the gross payoff, y , of a successful attack to be 100 and the gross payoff of an unsuccessful attack to be 0. This payoff scheme was chosen for its simplicity for the theory, as well as for the experiment participants.

Table 1.1 records the remaining parameters by session.

Table 1.1.

Parameterization.

Session	$z, 1/\alpha$	$1/\beta_1$	$1/\beta_2$
1-4	65, 50	7	N/A
5-6	75, 55	10	1

In all cases, z , the prior about θ was chosen to be high enough that a new attack becomes possible with the arrival of new information in the second stage. At the same time, in order

to get a reasonable number of random draws within the critical interval of $[0, 100]$, we kept z sufficiently high and α sufficiently low. The standard deviation, β_t , was chosen based on satisfying the criterion for stage-one uniqueness, namely $\beta_1 \geq \frac{\alpha^2}{2\pi}$.

We ran different treatment conditions based on the cost of action A and on the information provided to the participants in the second stage of the experiment. The various treatment conditions are summarized in Table 1.2.

Table 1.2.

Treatment Summary.

Session	Cost of Action A		Information in Stage 2	
	First 20	Last 20	First 20	Last 20
	Rounds	Rounds	Rounds	Rounds
1,2	20	50	No	No
3,4	50	20	No	No
5	60	60	No	Yes
6	60	60	Yes	No

1.4 Data Analysis

1.4.1 Variables and Summary Statistics

In our analysis, the main dependent variables are the aggregate size of the attack (measured as the fraction of subjects choosing action A), and the individual decision whether to attack (a binary choice variable, with 1 representing action A and 0 representing action B). We also consider the effects of the explanatory variables on the reported expectation of the number of other subjects choosing action A (*belief* variable). This allows us to study the extent to which the observed outcomes are driven by “equilibrium reasoning.” Henceforth, whenever we refer to the evidence of equilibrium reasoning in the data, we mean that actions are driven by the reported expectation of others’ actions.

The main explanatory variables are the random number, θ , on the aggregate level and the subject-specific hint number (private signal, x) on the individual level. We also look at several other variables which can have an effect on outcomes. One of these controls is the *cost* of action

A (attacking), which varies across treatments and sessions. When we explore the impact of endogenous learning, we look at the effect of *stage* on actions, where stage takes on values of 1 or 2. In order to understand the effects of exogenous learning, we introduce a new-information dummy (*NI dummy*), which takes on a value of 1 in the treatment where subjects receive a more precise private signal in the second stage and a value of 0 otherwise. Finally, we look at subjects' expectations about the size of the attack by creating a *belief* variable.

Table 1.B1 in Appendix B provides descriptive statistics for the experiment.

1.4.2 First-Period Predictions

Armed with the model predictions provided in section 1.2, we now revisit the four questions that motivated this thesis chapter and test whether the theory's answers to these questions are consistent with the experimental data.

Question 1: Are outcomes monotonic in the fundamentals?

In the first stage of each independent round, the subjects chose between actions A and B. Both the aggregate and the individual-level data confirm that observed behavior exhibits monotonicity of outcomes in the fundamentals. On the aggregate level, Figure 1.2 plots the size of the attack (i.e., the fraction of subjects choosing action A) against θ for the cost-50 treatment. It shows that A is strictly decreasing in θ , just as the theory predicts. Moreover, Figure 1.2 demonstrates that, for low states, almost everyone always chose action A, while for high states, almost everyone always chose action B. There is an intermediate range of fundamentals for which the size of the attack is decreasing in θ . We test this nonparametrically by carrying out a locally weighted regression of the number of attackers on the value of θ which is represented by the black monotonically decreasing line in Figure 1.2.¹⁷ The monotonicity of the fitted line confirms the hypothesis.¹⁸

¹⁷The fact that we have very few negative draws of θ is the reason behind the slightly positive slope of the fitted line in that range. That is, the positive slope is driven by "mistakes." The fitted line is otherwise monotonic over the critical range of fundamentals.

¹⁸See Figures 1.B1 and 1.B2 in Appendix B for similar plots for the other two cost treatment conditions.

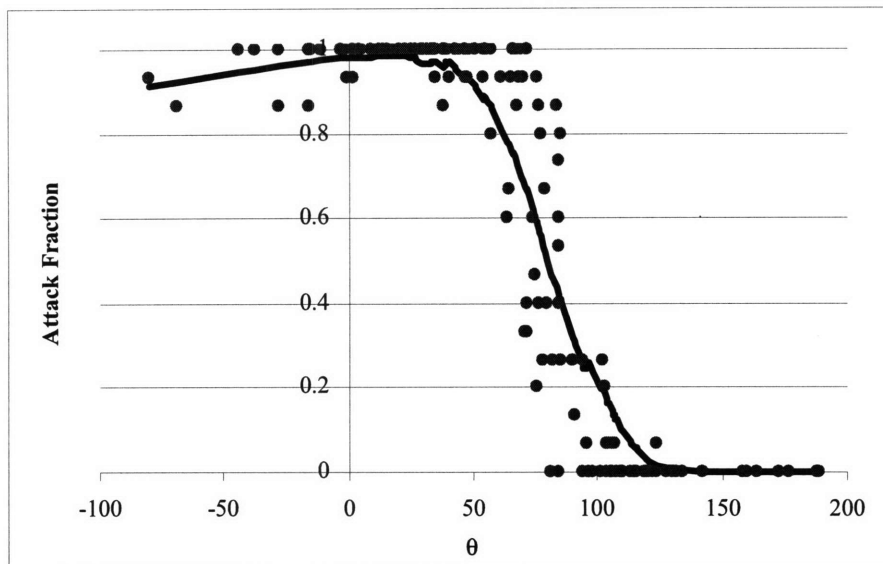


Figure 1.2: Aggregate Size of Attack vs. θ (Cost-50 Treatment)

The analysis of the individual-level behavior in stage 1 also supports the theoretical predictions. Figure 1.3 was constructed by creating discrete bins for x and calculating the probability of choosing action A for each bin. The figure demonstrates that the probability of choosing action A is decreasing in the hint number (private signal) over almost the entire range of x .¹⁹

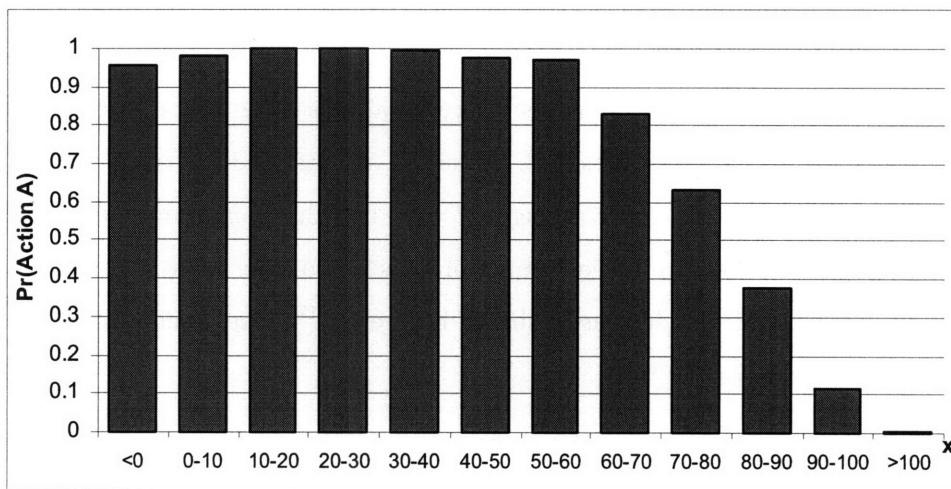


Figure 1.3: Probability of Attack vs. x (Cost-50 Treatment)

We also ran subject-level regressions of each individual’s action on her private signal, x ,

¹⁹See Figures 1.B3 and 1.B4 in Appendix B for similar plots for the other two cost treatment conditions.

and various controls. We use ordinary least squares to estimate the effects.²⁰ The results are reported in Table 1.3.

²⁰While a logit regression would be more appropriate given the binary nature of the dependent variable, we use a linear probability model since the logistic approach would result in biased estimates from a fixed-effects regression due to the panel structure of the dataset. Let p_{it} represent the probability that subject i attacks in period t , then $E[a_{it}] = 1 \cdot p_{it} + 0 \cdot (1 - p_{it}) = p_{it}$. This is modeled as $p_{it} = \Pr[a_{it} = 1] = F(x'_{it}\beta + \mu_i)$. For a linear probability model, $F(x'_{it}\beta) = x'_{it}\beta + \mu_i$, and the usual panel data methods apply. That is, β can be consistently estimated by eliminating μ_i using the within transformation (demeaning the data). This is possible because the MLE of μ_i and β are asymptotically independent. The only issue is that \hat{a}_{it} is not guaranteed to lie in the unit interval.

In order to use the logistic approach, we need to define a threshold a_{it}^* such that

$$\begin{aligned} a_{it} &= 1 \text{ if } a_{it}^* > 0 \\ &= 0 \text{ if } a_{it}^* \leq 0, \end{aligned}$$

where $a_{it}^* = x'_{it}\beta + \mu_i + \nu_{it}$ with $\Pr[a_{it} = 1] = \Pr[a_{it}^* > 0] = \Pr[\nu_{it} > -x'_{it}\beta - \mu_i] = F(x'_{it}\beta + \mu_i)$, where the last inequality holds as long as the density function for F is symmetric around zero. In this case, μ_i and β are unknown parameters and as $N \rightarrow \infty$, for a fixed T , the number of parameters μ_i increases with N . This means that μ_i cannot be consistently estimated for a fixed T . This is known as the incidental parameters problem.

The MLE of μ_i and β are no longer asymptotically independent with a qualitative limited dependent variable model with fixed T (like logit) as demonstrated by Chamberlain (1980). In his paper, Chamberlain proposes using the conditional logit approach to correct for this problem. For estimates using the conditional logit technique, see Table 1.B2 in Appendix B.

Table 1.3.

Stage 1 Individual Level Regressions (Action on x)

Variable	Dependent Variable: Action		
	1	2	3
Private signal, x	-0.007*** (0.0001)	-0.007*** (0.0001)	-0.001*** (0.0001)
Cost of Attacking		-0.003*** (0.0003)	-0.0004** (0.0002)
Belief			0.067*** (0.0010)
R^2	0.58	0.59	0.84
No. of observations	6000	6000	6000

Note: Robust standard errors in parentheses. Regressions include subject, group, and round fixed effects. For sessions 5-6, only the no-new-info treatment data are used. Significance levels: ** 5%, ***1%

According to Column 1, the effect of x on the choice of action is negative, as the theory predicts, and statistically significant at the 1 percent confidence level. This effect remains strong after controlling for the cost of attack.

The last column of Table 1.3 shows further shows that a subject's expectation of others' actions has a significant effect on her own action. This validates a key feature of best-response behavior predicted by the theory. At the same time, it is important to examine the determinants of the subject's reported expectation of others' actions in the data. Table 1.4 explores this.

Table 1.4.

Stage 1 Individual Level Regressions (Belief on x)		
Variable	Dependent Variable: Belief	
	1	2
Private signal, x	-0.083*** (0.0012)	-0.084*** (0.0010)
Cost of Attacking		-0.032*** (0.0037)
R^2	0.63	0.63
No. of observations	6000	6000

Note: Robust standard errors in parentheses.

Regressions include subject, group, and round fixed effects. For sessions 5-6, only the no-new-info treatment data are used. Significance levels: ** 5%, ***1%.

Table 1.4 suggests that subjects with lower signals expect others to attack less, as the theory predicts. The fact that the private signal affects the subjects' decisions not only directly (Table 1.3), but also through their expectations about others' strategies (Table 1.4) serves as evidence that subjects exhibit "equilibrium reasoning" in the first stage of the experiment.

Question 2: Does the cost of Action A (attacking) affect the probability of attack?

Table 1.3 reports that an increase in the cost of attacking seems to reduce the probability of attack significantly. Moreover, Column 2 of Table 1.4 shows that, in addition to reducing one's own probability of attack, higher cost also lowers the subject's expectation of others attacking, once again providing evidence in support of equilibrium reasoning.

While we can already say with confidence that outcomes are indeed monotonic in the fundamentals and that higher cost of attacking lowers the likelihood of an attack, we can provide more quantitative answers to our questions by estimating subjects' average threshold strategies and comparing them to the theoretical predictions. For every session of the experiment, we run a logit regression of a subject's action on her private signal x to estimate the threshold \hat{x}_s that

agents appear to use in the data.²¹ We use a similar method to estimate the aggregate threshold, $\hat{\theta}_s$, for every session. Table 1.5 reports the stage-one threshold estimates and compares them to the theoretical thresholds θ^* and x^* . Column 5 of the table reports the percentage of subjects who wrote that they followed a threshold strategy in their post-experiment questionnaire.

Table 1.5.
Threshold Summary.

Session	Cost	θ^*	$\hat{\theta}$	x^*	\hat{x}	Percent Using Thresholds
1,2	20	81.5	88.8	87.8	86.8	93%
1,2	50	48.1	82.8	47.8	81.1	93%
3,4	20	81.5	89.0	87.8	87.3	95%
3,4	50	48.1	77.3	47.8	76.6	95%
5,6	60	34.8	71.6	30.9	73.4	92%

While the theoretical thresholds are highly sensitive to the cost of attacking, the estimated thresholds decrease only slightly with cost, even though we ensure that the subjects are well aware of the change in costs from treatment to treatment. This finding suggests that an increase in the cost, such as an interest-rate hike by a policy-maker, must be relatively large for it to have an effect on the speculators' decision to attack which can be quite costly for the economy. The observed insensitivity to the cost of attacking also implies that the subjects are much more aggressive in their behavior than the theory predicts, at least in the high-cost treatments.

Next, we investigate whether thresholds change over the rounds. For example, if we were to find an upward trend in thresholds over the rounds, we would conclude that subjects are learning to coordinate better over time. To address this question, Figure 1.4 plots the estimated thresholds, $\hat{\theta}$, across rounds.

²¹ $\hat{x}_s = -\frac{\beta_0}{\beta_1}$, where subscript s represents the session, β_0 is the constant, and β_1 is the coefficient on x in the logit regression.

Note that, for now, we are assuming that there is a unique individual threshold among all subjects.

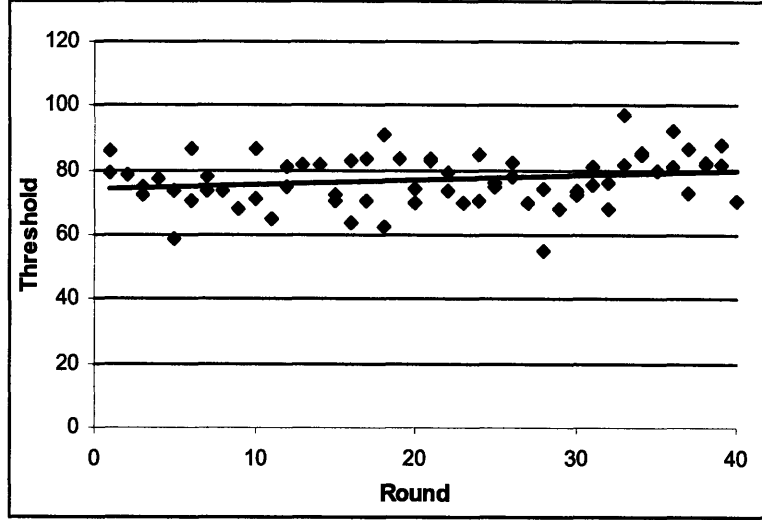


Figure 1.4: Estimated Thresholds, $\hat{\theta}$, vs. Rounds (Pooled Data for Sessions 1-4)

The estimated aggregate thresholds do not vary significantly with round, since the fitted black line exhibits only a slight upward trend. We can also see that the variance of thresholds does not decrease with rounds, which implies that subjects are not learning from round to round.

Another important question is whether the observed excess aggressiveness relative to theory arises due to the subjects' failure to follow a best-response strategy (i.e., due to "mistakes"). This question can be answered by estimating the number of subjects who did not follow a best-response strategy given their private signal. In order to determine each subject's best response, we assume that each individual knows that others are using an average threshold $\hat{x} > x^*$ in each round of the experiment. Assuming that each subject expects that others' follow the threshold, \hat{x} , we next ask whether individual behavior is consistent with the theoretical best response strategy to this \hat{x} . In order to answer this question, we compute the threshold $\hat{\hat{x}}$ that is the best response to \hat{x} .²² Given this best-response threshold, $\hat{\hat{x}}$, an agent will attack

²²The best-response threshold, $\hat{\hat{x}}$, is the individual threshold that sets the posterior probability of regime change $Pr(\theta \leq \hat{\theta} | \hat{x})$ equal to the cost of attacking, c . That is,

$$\Phi \left(\sqrt{\beta_1 + \alpha} \left(\hat{\theta} - \frac{\beta_1}{\beta_1 + \alpha} \hat{x} - \frac{\alpha}{\beta_1 + \alpha} z \right) \right) = c$$

where $\hat{\theta}$ is the aggregate threshold that solves

$$\hat{\theta} = \Phi(\sqrt{\beta_2}(\hat{x} - \hat{\theta}))$$

if and only if her private signal, x , is below \hat{x} and refrain from attacking otherwise. We call observed actions that fail to be consistent with this best-response behavior “mistakes.” That is, a “mistake” would be an instance when a subject attacks when $x > \hat{x}$ or does not attack when $x < \hat{x}$.

We find that, across all rounds and treatments for sessions 1-4, in approximately 91 percent of cases subjects followed a strategy that was a best response to the estimated threshold, \hat{x} . Figures 1.5 and 1.6 show the number of “mistakes” by round for sessions 1-2 and 3-4, respectively.

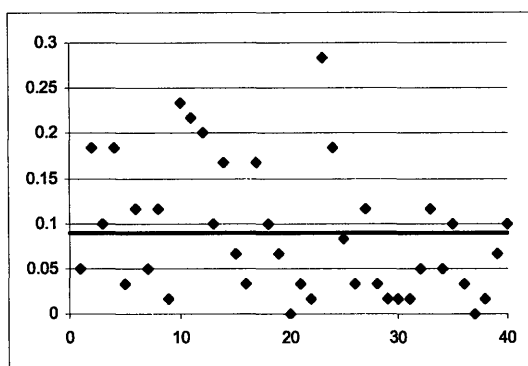


Figure 1.5: Proportion of “Mistakes”
vs. Rounds (Sessions 1-2)

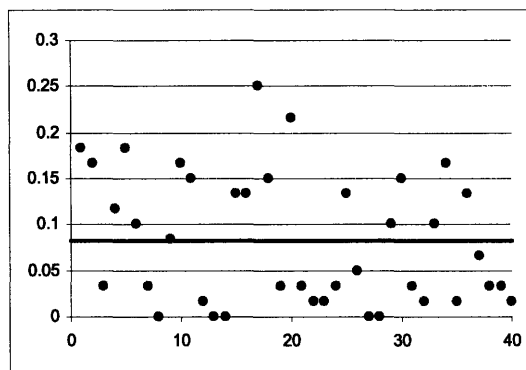


Figure 1.6: Proportion of “Mistakes”
vs. Rounds (Sessions 3-4)

The figures above show that the number of “mistakes” relative to best-response does not change significantly with round. The dispersion also does not seem to decrease across rounds.²³ The solid black line represents the fraction of “mistakes” averaged across rounds. One explanation of this lack of learning over rounds is that the overall average payoff loss due to “mistakes” is relatively low (see column 4 of Table 1.6).

(Recall that \hat{x} is the estimated threshold. Thus, the right-hand side of the latter equation represents the aggregate size of the attack, $A(\theta) = Pr(x \leq \hat{x}|\theta)$.)

²³Using standard OLS techniques to fit a regression line through these data produces slope coefficients that are not statistically significantly different from zero. See Table 1.B7 in Appendix B for regression results.

Table 1.6.

"Mistake" Descriptive Statistics				
Session	Cost	Total Share of "Mistakes"	% "Mistakes" w/in 2 Standard Deviations of \hat{x}	Average % of Payoff Lost Due to "Mistakes"
1,2	20	11.1%	75.9%	1.8%
1,2	50	7.6%	83.5%	5.8%
3,4	20	6.0%	86.1%	1.3%
3,4	50	10.7%	74.2%	10.8%
5,6	60	6.6%	79.7%	12.0%

Note also that the last column of Table 1.6 suggests that subjects are making "mistakes" primarily in rounds where their private signals are close to the estimated threshold (within two standard deviations of \hat{x}). That is, on average, only 20% of all actions that are not consistent with best response happen in rounds with the realization of x far away from the threshold. One can therefore interpret "mistake" episodes as cases of minor miscoordination. The extension of the model in section 1.5 will rationalize these "mistakes" with the notion of agents' excessively aggressive beliefs about others' actions.

Another way to interpret "mistakes" is to realize that experimental subjects are unlikely to follow a single threshold. In fact, our logit regressions on the individual-level data suggest that thresholds are heterogeneous in the experiment. Using the same logistic estimation we used to compute the average threshold, \hat{x}_s , we can derive the expression for the distribution of these heterogeneous thresholds.²⁴

Figure 1.7 shows this distribution of individual thresholds for the cost 50 treatment.²⁵ The extension of the model in section 1.5 will capture this heterogeneity.

²⁴This expression is simply the minus the derivative of the logistic, given by the following equation.

$$P(x) = -\frac{\beta_1 e^{-(\beta_0 + \beta_1 x)}}{[1 + e^{-(\beta_0 + \beta_1 x)}]^2}$$

²⁵See Appendix B, Figures 1.B5 and 1.B6, for similar figures for Cost 20 and Cost 60 Treatments.

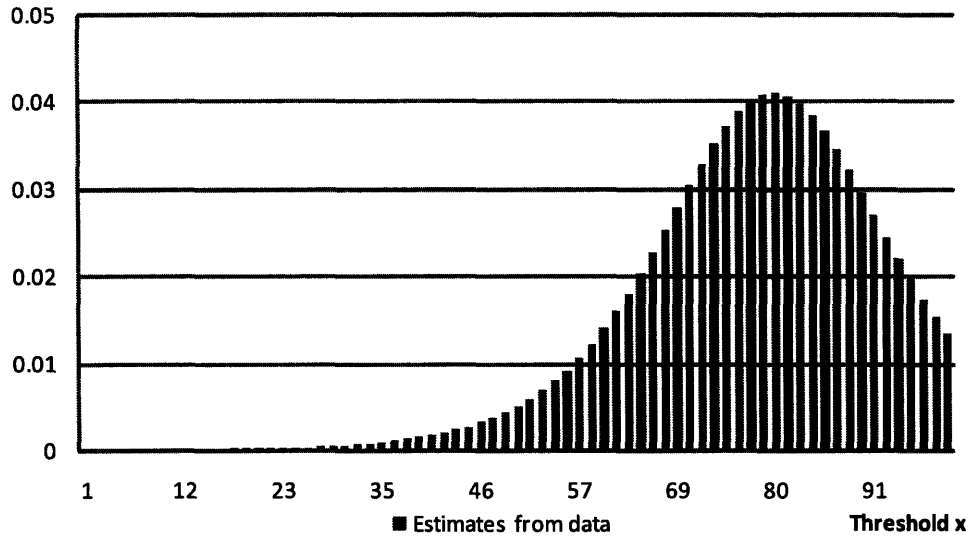


Figure 1.7: The P.D.F. of Individual Thresholds $\hat{x}_{i,s}$ for the Cost-50 Treatment

1.4.3 Second-Period Predictions

Endogenous Learning

So far, we have been analyzing the data from the first stage of the experiment. In this section, we will discuss the effects of learning across stages under the condition that subjects do not receive an additional signal in the second stage. That is, we use the data to answer our next question.

Question 3: Does a failed speculative attack reduce the probability of a new attack without arrival of new information?

We find that the knowledge that the experiment has not ended in the first stage bears a strong effect on subjects' behavior in the second stage. In particular, we find that the average probability of attack in the second stage of the no-new-information treatments is much lower than the average probability of attack in stage one, as is shown in Figure 1.8.

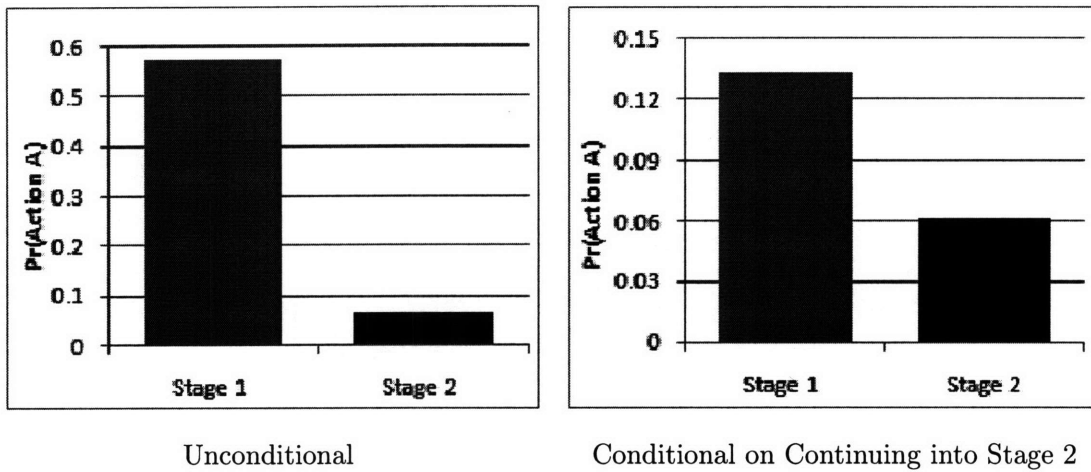


Figure 1.8: Average Probability of Action A for the No-New-Information Treatments

Figures 1.9 and 1.10 document the reduction in aggressiveness in the strategy space of the agents for cost-50 and cost-20 treatments, respectively.

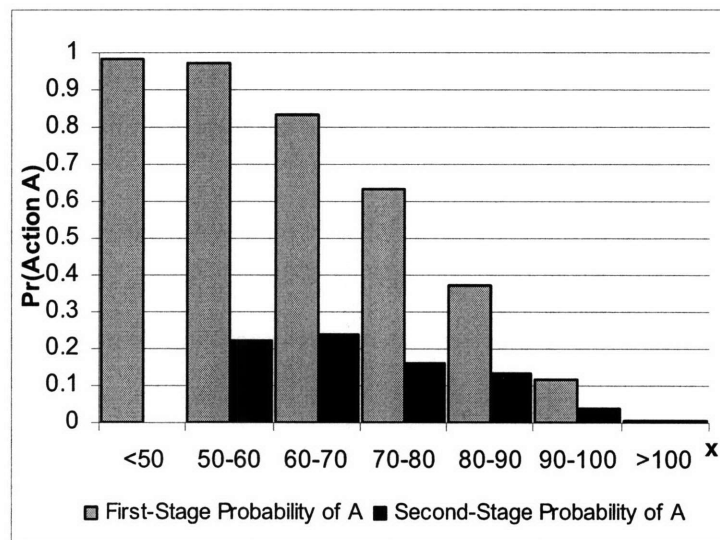


Figure 1.9: Probability of Attack vs. x by Stage for Cost-50 Treatments

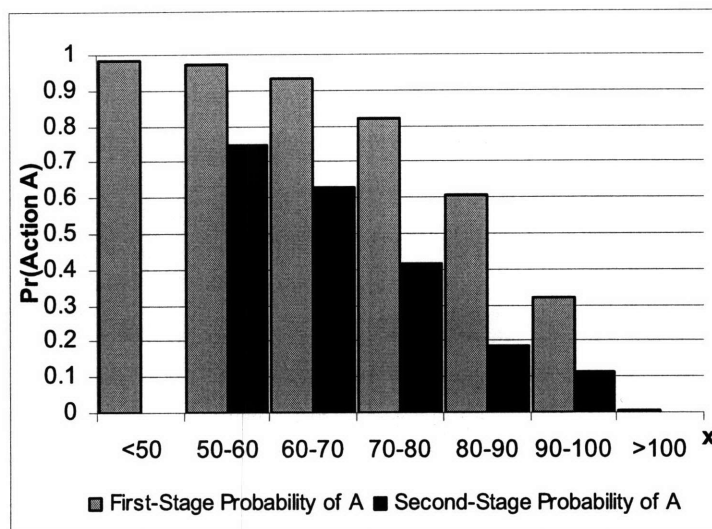


Figure 1.10: Probability of Attack vs. x by Stage for Cost-20 Treatments

In both figures, the probability of action A is lower in the second stage relative to the first stage. This evidence is consistent with the theory's prediction regarding the impact of endogenous learning. However, the agents continue to act overly aggressively in the second stage of the experiment. Note also that the reduction in the probability of action A is substantially larger in the high-cost treatment.

The reduced likelihood of an attack without new information in the second stage is statistically significant, as Table 1.7 demonstrates. Table 1.7 reports results of individual-level regressions of action on the private signal, x , that now include pooled data for stages one and two and add *stage* as a control variable. The variable *stage* has a negative and highly statistically significant effect on action, which means that the probability of subjects choosing action A is reduced as we go from stage one to stage two.²⁶

²⁶Again, see Table 1.B3 in Appendix B for estimation using conditional logit.

Table 1.7.

Pooled Individual Level Regressions (Action on x)			
Variable	Dependent Variable: Action		
	1	2	3
Private signal, x	-0.006*** (0.0001)	-0.006*** (0.0001)	-0.0008*** (0.0001)
Cost of Attack		-0.0026*** (0.0003)	-0.0005** (0.0002)
Belief			0.0655*** (0.0008)
Stage	-0.2307*** (0.0073)	-0.2272*** (0.0073)	-0.0399*** (0.0054)
R^2	0.58	0.59	0.84
No. of observations	6000	6000	6000

Note: Robust standard errors in parentheses. Regressions include subject, group, and round fixed effects. For sessions 5-6, only the no-new-info treatment data are used. Significance levels: ** 5%, ***1%

Table 1.7 shows that the observation that the experiment continued into the second stage has a significant negative effect on the subject's own decision to attack. Experimental behavior is consistent with endogenous learning.

Next, we wish to understand whether this learning effect takes place at the game-theoretic level, or only at the single-agent decision level. That is, we wish to examine whether the subjects expect *others* to be learning as well. Table 1.8 reports these findings.

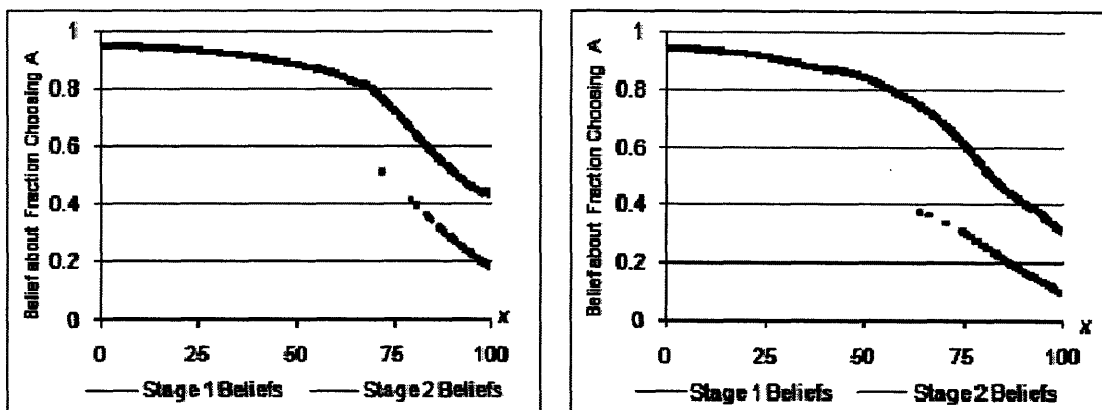
Table 1.8.

Pooled Individual Level Regressions (Belief on x)		
Variable	Dependent Variable: Belief	
	1	2
Private signal, x	-0.078*** (0.0010)	-0.079*** (0.0010)
Cost of Action A		-0.032*** (0.0030)
Stage	-2.901*** (0.0901)	-2.858*** (0.0895)
R^2	0.66	0.67
No. of observations	8820	8820

Note: Robust standard errors in parentheses.

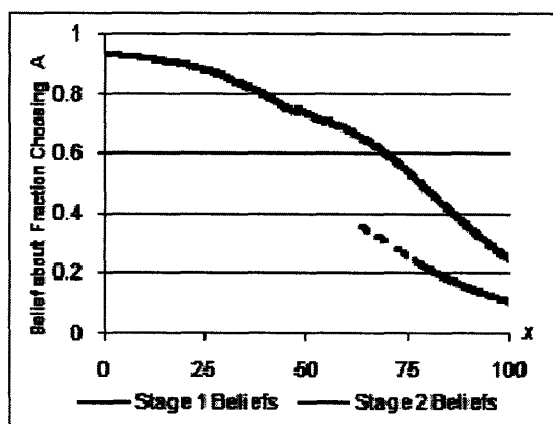
Regressions include subject, group, and round fixed effects. For sessions 5-6, only the no-new-info treatment data are used. Significance levels: ** 5%, ***1%

While Table 1.7 shows that subjects exhibit endogenous learning going from the first to the second stage, Table 1.8 demonstrates that they also expect other subjects to be learning. In fact, we can see that the subjects' beliefs are indeed the source of this learning by examining Figure 1.11. It plots reported individual beliefs about the size of the attack in stage one (black line) and stage two (grey line) against the private signal for all the no-new-information treatments. The first-stage beliefs are everywhere above and to the right of the second-stage beliefs, which documents a reduction in aggressiveness of beliefs going from the first into the second stage.



Cost 20 No New Information

Cost 50 No New Information



Cost 60 No New Information

Figure 1.11: Beliefs about Fraction of Agents Choosing Action A in Stages 1 and 2

The evidence presented in Table 1.8 and Figure 11 suggests that, in the second stage of the treatments with no new information, subjects exhibit a form of equilibrium reasoning consistent with that in the theory.

New Information

With sufficiently precise new information in the second stage, the theory predicts that an attack becomes possible again, given that the parameters have been chosen appropriately. Figure 1.12 contrasts the average probability of action A in the two stages of the experiment for the no-new-information (NNI) and the new-information (NI) treatments. Note that the figure was constructed using only the rounds that continued into the second stage and for which the random number drawn was below 100. This allows us to make the clearest possible comparison

between treatments.

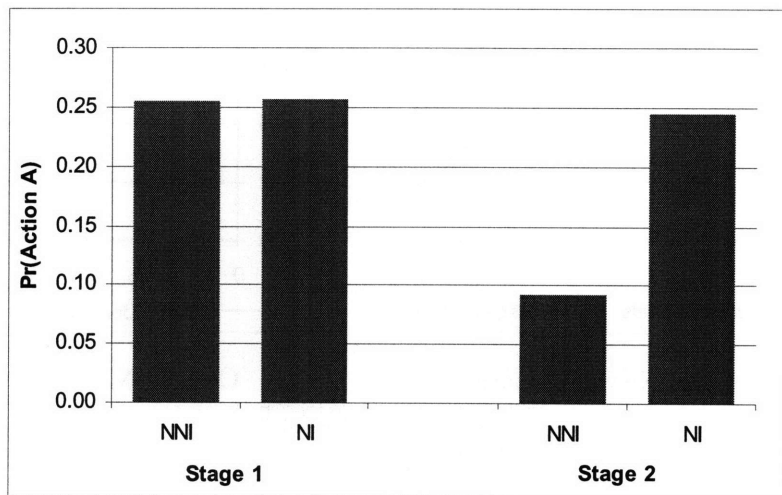


Figure 1.12: Average Probability of Action A for the Treatments with No-New-Information (NNI) and New-Information (NI)

First, consider the average probability of action A for the two treatment conditions in the first stage. The probabilities are very close in magnitude: 0.25 for the treatment with no new information in stage two and 0.26 for the one with new information in stage two. We can therefore conclude that the anticipation in stage one that new information will arrive in stage two does not alter behavior in stage one. Secondly, note that the probability of action A is reduced dramatically in the no-new-information treatment, as the experiment continues into the second stage. Both facts are consistent with the theory.

Figure 1.13 shows the probability of action A in the strategy space of the agents for the no-new-information (NNI) and the new-information (NI) treatments with cost of action A of 60. For most bins, the probability of action A in the second stage for the new-information treatment exceeds the probability of action A for the no-new-information treatment. Moreover, for x above 80, the probability of action A in the second stage in the new-information treatment exceeds the probability of action A in the first stage.

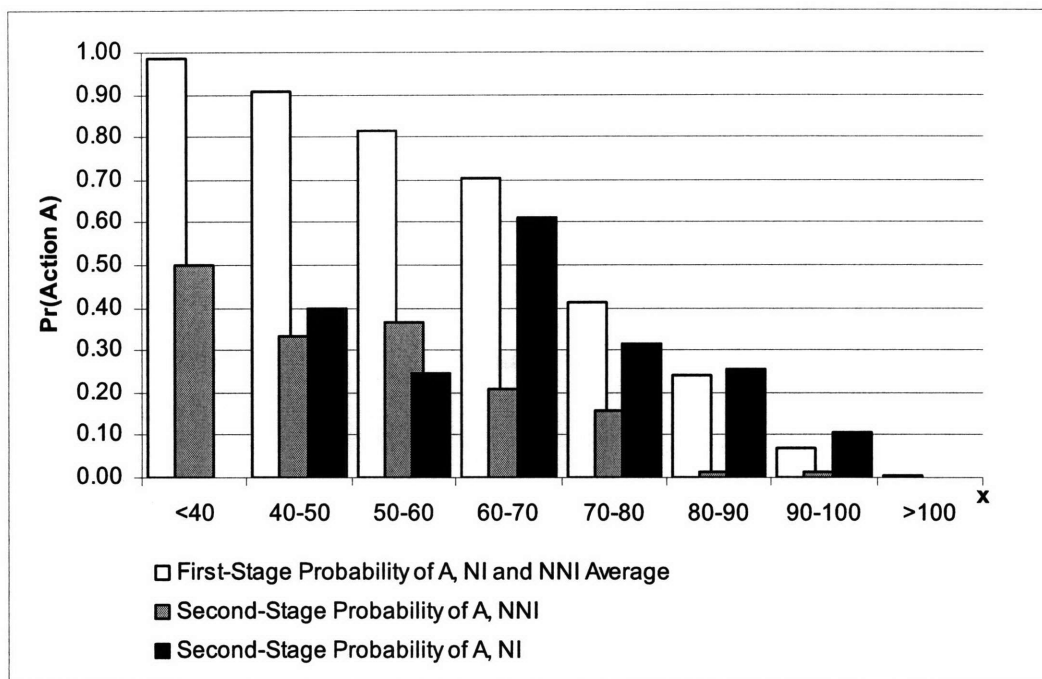


Figure 1.13: Probability of Action A vs. x by Stage for the No-New-Information (NNI) and New-Information (NI) Treatments

We confirm this result by running an individual-level regression of action in the second stage on the private signal, x , and the new-information treatment dummy. The results of this regression are reported in Table 1.9.²⁷ The statistically significant coefficient on the new-information (NI) dummy tells us that subjects are more likely to choose action A in stage two of the new-information treatment than in stage two of the no-new-information treatment, which is consistent with the theoretical prediction.

²⁷For estimates using the conditional logit technique, see Table B-IV in Appendix B.

Table 1.9.

Stage 2 Individual Level Regressions (Action on x)

Variable	Dependent Variable:
	Action
Private signal, x	-0.0021*** (0.0002)
New Information (NI) Dummy	0.0880*** (0.0153)
R^2	0.22
No. of observations	1395

Note: Robust standard errors in parentheses.

Regressions include subject, group, and round fixed

effects. Data for Sessions 5-6. Significance levels: ***1%

Table 1.10 confirms that new information affects not only subjects' own decision-making process, but also affects their beliefs that new information makes others relatively more aggressive in the second stage.

Table 1.10.

Stage 2 Individual Level Regressions (Belief on x)

Variable	Dependent Variable: Belief
Private signal, x	-0.0380*** (0.0025)
New Information Dummy	1.354*** (0.1820)
R^2	0.37
No. of observations	1395

Note: Robust standard errors in parentheses.

Regressions include subject, group, and round fixed effects. Data for Sessions 5-6. Significance levels: ***1%

Furthermore, Figure 1.14 shows that, in the treatment with new information, agents expect others to be more aggressive than in the treatment with no new information for almost the entire range of x .²⁸ That is, the beliefs in the new-information (NI) treatment, represented by the black curve, are above the beliefs in the no-new-information (NNI) treatment, represented by the grey curve.²⁹

²⁸Note that there are relatively few rounds where the experiment continues into the second stage for values of x below 60.

²⁹Note that the black line in last panel of Figure 13 represents stage-two beliefs plotted against the realizations of the signal x in the NI treatment, while the grey line represents stage-two beliefs plotted againsts the realizations of the private signal in the NNI treatment.

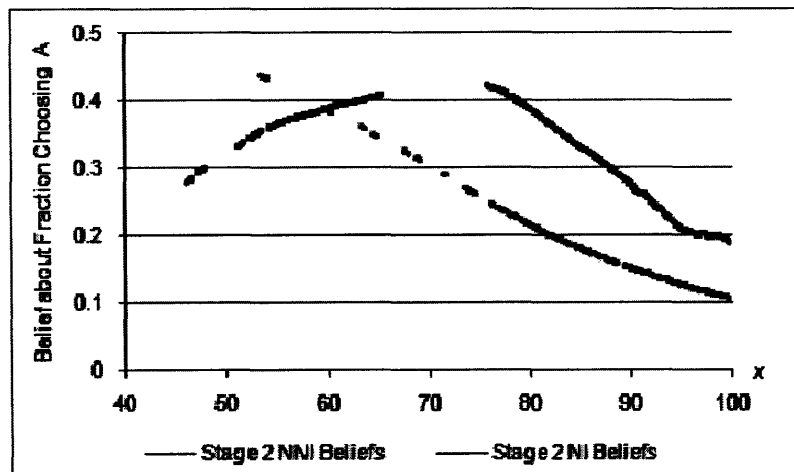


Figure 1.14: Beliefs about Fraction of Agents Choosing Action A in Stage 2 in the No-New-Info (NNI) and New-Info (NI) Treatments

In summary, our findings suggest that the probability of a crisis declines with an improvement in the fundamentals and with an increase in the cost of attacking. Furthermore, subjects attack significantly less having learned that the regime has survived a past attack. Finally, arrival of new information makes the agents more aggressive, increasing the probability of a new attack. We identify the agents' beliefs about the actions of others to be the main channel through which the relative strength of the status quo, the cost of attacking, and learning impact observed behavior.

1.4.4 Rationality and Consistency of Beliefs

Rationality

In order to address the four issues mentioned in the introduction, we have been using a theoretical model whose qualitative predictions seem to hold up in the laboratory. However, throughout the study, we have encountered a systematic deviation from these predictions. In particular, we see that the behavior of experimental subjects is more aggressive than predicted in both the static and dynamic frameworks. In this section, we seek to understand whether the aggressiveness results from behavior that is irrational given the agents' beliefs, and whether the agents' actions are consistent with their expectations of the actions of others.

First, we address the question of rationality by testing two hypotheses. The first hypothesis

postulates that subjects are “irrational” in the sense of being intrinsically biased toward attacking: they simply enjoy attacking. An alternative hypothesis is that subjects are “rational” in a sense that they follow a best-response strategy given their beliefs of what others will do, and it is their beliefs that are more aggressive than the theory predicts.

Recall that before revealing the outcome of the experiment at a particular stage, we ask each subject about his or her belief as to the number of other group members choosing action A. The actual reported beliefs are a function $\hat{g}(x_i)$, which we estimate from the actual data using a kernel regression approach. Figure 1.15 plots these beliefs about the fraction of agents choosing action A (the attack fraction) and the theoretical benchmark for these beliefs over x_i in the range $[0, 100]$ for Cost-20, Cost-50, and Cost-60 treatments. The theoretical benchmark is calculated in the following way. Since the theory predicts that the equilibrium size of the attack is given by $A(\theta) = \Phi(\sqrt{\beta}(x^* - \theta))$, the theory also predicts that the expected size of the attack for an agent with signal x_i who expects all other agents to use the threshold x^* is given by the following expression:

$$E \left[\Phi \left(\sqrt{\beta}(x^* - \theta) \right) | x_i \right] \equiv g(x_i; x^*) \quad (1.5)$$

where x^* is the theoretical threshold derived in section 1.2.1.

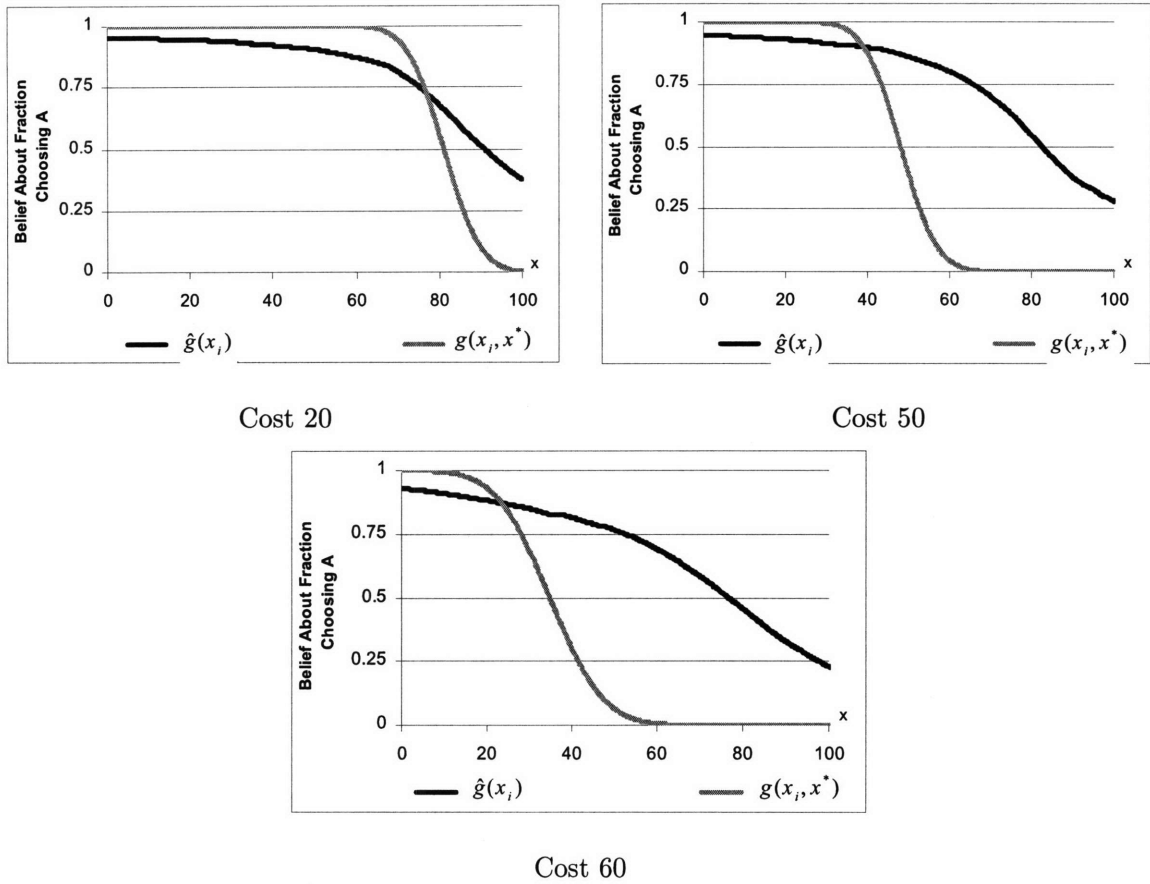


Figure 1.15: Beliefs about Fraction of Agents Choosing Action A vs. Theory Prediction

In sections 1.4.2 and 1.4.3, we found that the agents' actions are more aggressive than the theory predicts. Figure 1.15 shows that the subjects' *beliefs* are more aggressive than predicted by the theory in the cost-50 and cost-60 treatments. (The agents' expectations of the size of the attack lie above/to the right of the theoretical expectation for higher values of x_i .) These are exactly the cost treatments for which we find more aggressive actions relative to theory. However, the beliefs in the cost-20 treatment do not seem to be much more aggressive than the theoretical prediction. Looking back at Table 1.5, agents' actions are not more aggressive than the theoretical prediction in the cost-20 treatment. Note also that the plots in Figure 1.15 demonstrates that the observed beliefs are less sensitive to the individual signal than the theory predicts, since $\hat{g}(x_i)$ is flatter than $g(x_i, x^*)$. This feature of the observed beliefs is present in all cost treatments.

Figure 1.15 provides visual support for the view that the subjects of these experiments do

not act aggressively merely due to irrationality, but rather due to a rational response to their overly aggressive expectations. To provide more concrete evidence on the role of each force, we construct a measure of rationality based on our data of the subjects' beliefs. A rational agent will choose action A if and only if the expected payoff from choosing it is greater than the cost.³⁰

First, we compute \bar{x}_i such that $\hat{g}(x_i) = g(x_i; \bar{x}_i)$. \bar{x}_i is the threshold for others' strategies that rationalizes subject i 's belief about the size of the attack. Note that the threshold \bar{x}_i is different from the threshold \hat{x} that we refer to in section 1.4.2. While \hat{x} directly rationalizes individual behavior, \bar{x}_i rationalizes the subjects' beliefs about the behavior of others. The values of \bar{x}_i are plotted across x_i for different costs of attacking in Figure 1.16.

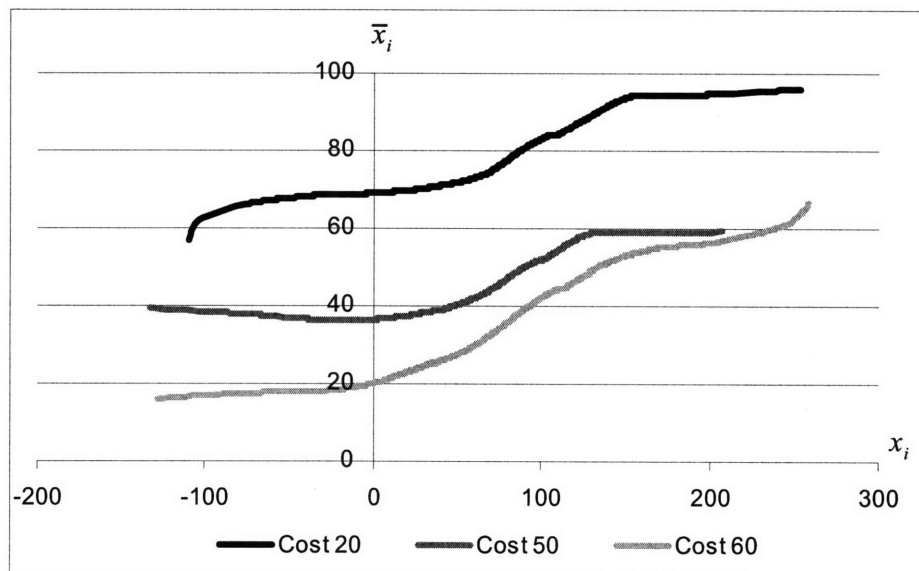


Figure 1.16: Thresholds \bar{x}_i for Different Cost Treatments

Across the three cost treatments, the values of \bar{x}_i decrease with the cost of attacking. This once again indicates that agents expect others to attack less when the cost of attacking is higher. Within each cost treatment, \bar{x}_i increases with the individual private signal, x_i . This suggests that there is a positive relationship between the individual agent's error in beliefs about the

³⁰Note that for the sake of this comparison, we would actually need to know the subject's belief about the probability of a successful attack, not his or her belief about the size of the attack. That is, the relevant condition for rationality is not actually to choose action A if and only if $E[A(\theta)|x_i] - (\frac{\alpha}{\alpha+\beta}x_i + \frac{\beta}{\alpha+\beta}z) > c$. Rather, instead of $E[A(\theta)|x_i]$ in expression, the proper term is the expectation of $I_{A(\theta)>\theta}$, where $I_{A(\theta)>\theta} = I_{\theta < \theta^*} = \begin{cases} 1 & \text{if } \theta < \theta^* \\ 0 & \text{if } \theta > \theta^* \end{cases}$. This, however, is not a question a typical subject is capable of answering.

actions of others and the agent’s expectation about θ . Agents with a high private signal x_i expect others to use less aggressive strategies (i.e., have a higher \bar{x}_i).

Given this \bar{x}_i , a rational agent would always choose action A if and only if the following inequality holds:

$$\Phi \left(\sqrt{\beta_1 + \alpha}(\bar{\theta}_i - \frac{\beta_1}{\beta_1 + \alpha}\bar{x}_i - \frac{\alpha}{\beta_1 + \alpha}z) \right) > c$$

where $\bar{\theta}_i$ is the solution to the equation $\bar{\theta}_i = \Phi(\sqrt{\beta_1}(\bar{x}_i - \bar{\theta}_i))$. Using this criterion, we find that subjects are rational in 76.98 percent of cases in the cost-20 treatment, 90.79 percent of cases in the cost-50 treatment, and 89.44 percent of cases in the cost-60 treatment.

The above evidence leads us to reject the hypothesis that subjects are being “irrational” or acting “too aggressively” given their beliefs. The data suggest that it is the subjects’ beliefs that are overly aggressive. However, given these beliefs, agents act “rationally” in 86 percent of cases, on average.

Consistency

Secondly, we address the issue of consistency by asking whether the subjects’ actions are consistent with their beliefs about the size of the attack. Ideally, we would like to compare each individual’s expectation of the fraction of agents attacking given his or her individual signal, x , to the actual fraction of agents attacking given x . However, the latter metric is not available (that is, we can only plot the aggregate attack against θ , not x). However, we can employ the law of iterated expectations, in order to provide a measure of consistency. In particular, the theory tells us that the following must hold:

$$E[A(\theta)] = E[E[A(\theta)|x]] \tag{1.6}$$

Thus, we can test whether actions are consistent with individual beliefs by estimating the right- and the left-hand sides of equation (1.6). The left-hand side of equation (1.6) is the expectation of the actual fraction of agents choosing action A, which we compute by integrating numerically the size of the attack that we get from the data, weighted by the distribution of θ . On the right-hand side of (1.6), $E[A(\theta)|x]$ is the belief about the fraction of agents choosing action A, which we have directly solicited from the subjects. To compute $E[E[A(\theta)|x]]$, we average

beliefs across x , weighing each x by its density. That is, we integrate numerically the function $\widehat{g}(x_i)$, weighted by the distribution of x . Table 1.11 presents the results from this analysis.

Table 1.11.

Test of Consistency in Stage 1.

Cost	Average Realized	Beliefs Averaged
Treatment	Attack Fraction	Across x
20	0.5857	0.5354
50	0.5799	0.5347
60	0.5071	0.4615

To construct the above table, we use stage-one data for the three cost treatments. Note that the values reported in Columns 2 and 3 of Table 1.11 are close to one another, which serves as evidence that actual outcomes are consistent with beliefs.

Next, we perform a consistency test for the second stage of the experiment (Table 1.12).

Table 1.12.

Test of Consistency in Stage 2.

Cost & Info	Average Realized	Beliefs Averaged
Treatment	Attack Fraction	Across x
20 NNI	0.0227	0.0610
50 NNI	0.0769	0.2101
60 NNI	0.0461	0.1505
60 NI	0.1248	0.1484

Note that there is a reversal in aggressiveness: in the first stage, agents' beliefs are slightly less aggressive than their actions, while in the second stage, agents' actions are less aggressive relative to their beliefs, especially in the high-cost treatments with no new information. The subjects seem to think that others will be daring and attempt another attack, while they themselves behave cautiously. There is also a difference between the new-information and the no-new-information treatments within the cost-60 treatment. The subjects beliefs are approximately the same, but actions are vastly different. Agents act much more aggressively upon arrival of new information than without it, just as the theory predicts.

1.5 Extension

In this section, we extend the benchmark model in order to match better certain aspects of our experimental findings. In particular, this extension is designed to capture the notion that the agents' excess aggressiveness in actions stems from the excess aggressiveness in their beliefs about the actions of others. It will further generate heterogeneity in agents' individual strategies induced by the heterogeneity in their beliefs (see Figure 1.8).

We modify the benchmark game as follows. Every agent i believes that all other agents j derive a benefit from attacking, B_{jt} . This captures the idea that each agent thinks that others are enjoying attacking more than herself. Another interpretation of this extension is that each agent thinks that others are more risk-loving than herself. Thus, in the mind of agent i , the payoffs of agent j are given by the following table.

	$A_t > \theta$	$A_t < \theta$
Choose action A	$1 + B_{jt} - c$	$B_{jt} - c$
Choose action B	0	0

where A_t denotes the aggregate size of the attack at time t , θ parametrizes the strength of the status quo, and c represents the cost of attacking. Clearly, if all agents believe that all other agents have, at least on average, a positive B_j , this model will generate excess aggressiveness in equilibrium. In addition, we can account for the possibility that different agents have different beliefs about the distribution of benefits in the population. This will generate heterogeneity in individual strategies of agents.

In particular, agent i thinks that B_j is distributed according to a cumulative density function, $F_i(B)$. In order to make quantitative statements about the equilibrium of this model, we further assume that $F_i(B)$ is a normal distribution with mean, μ_i , and standard deviation, σ_i . For simplicity, we assume that $\sigma_i = \sigma$ is the same across agents. However, μ_i differs across agents, which serves as the source of heterogeneity. Specifically, we assume that $\mu_i \sim N(\bar{\mu}, \bar{\sigma})$. Thus, this model is parameterized by three additional parameters, $\bar{\mu}$, $\bar{\sigma}$, and σ . For simplicity, we further assume that $\bar{\sigma} = \sigma$.

The equilibrium is then characterized as follows. An agent with belief μ_i expects that other

agents, j , follow a strategy profile $x^\#(B_j; \mu_i)$, such that agent i believes that agent j will attack the status quo if and only if $x_j < x^\#(B_j; \mu_i)$. Agent i then follows her best-response strategy to the schedule $x^\#(B_j; \mu_i)$, according to which she thinks others are playing. The best-response is described by the threshold $x_B^*(\mu_i)$, such that agent i attacks the status quo if and only if $x_i < x_B^*(\mu_i)$. In equilibrium, each agent has a unique strategy profile for others' actions in her mind ($x^\#(B_j; \mu_i)$) and follows a unique best-response strategy ($x_B^*(\mu_i)$). Thus, if we look at the entire population of agents, we can use a simple Monte-Carlo simulation to produce a histogram of individual best-response thresholds, which will now be heterogeneous across agents. (For a full equilibrium characterization, see Appendix A.)

We can solve for this equilibrium numerically. The mean of the distribution of μ_i 's, $\bar{\mu}$, and the standard deviation, $\bar{\sigma}(= \sigma)$, are determined using the method of moments such that the model produces the distribution of individual thresholds, $x_B^*(\mu_i)$, with mean threshold and the standard deviation of thresholds that most closely match the mean and the standard deviation of the distribution of individual strategies in the data. The rest of the parameters of the model are chosen to match our experimental parameters (see section 1.3.2). Figure 1.17 displays the distribution of individual thresholds that this model produces, represented by the light grey bars in the histogram. The dark grey lines represent the distribution of individual thresholds estimated from the experimental data.³¹

³¹See Appendix B, Figures 1.B7 and 1.B8, for similar figures for cost 50 and cost 60.

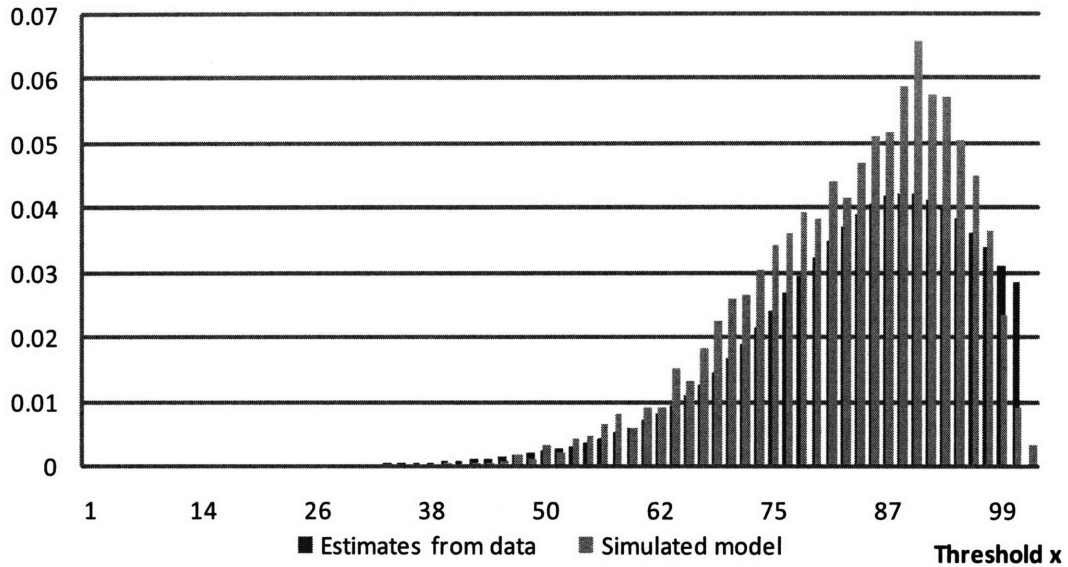


Figure 1.17: The Distribution of Individual Thresholds in the Model and in the Data (Cost 20)

The histogram shows that the extension of the model produces a much better fit to the experimental data than the baseline model. In particular, we now explicitly model the excess aggressiveness that comes from agents' believing that others are deriving an additional benefit from attacking. Thus, by construction, the mean of the individual thresholds is higher in the extension than in the baseline model without the benefit. Furthermore, the extension captures an additional feature of the experimental findings, namely the heterogeneity of individual thresholds in the population.

1.6 Conclusion

This study was motivated by certain open questions associated with events that require coordination, such as currency crises. Recent theoretical work in global games looks promising in providing new insightful answers to these questions.

We test several of these predictions in an experimental setting. As the theory predicts, we find that the size of the attack is decreasing in both the underlying strength of the status quo and the agents' cost of attacking. Following a failed attack, the knowledge that the status quo

has survived greatly decreases the probability of a new attack. However, as agents receive new private information over time, the probability of a new attack significantly increases.

Furthermore, we find that agents take a truly game-theoretic approach to deciding whether or not to attack the status quo. This means that the relative strength of the status quo, the cost of attacking, and learning impact observed behavior mainly through agents' beliefs about the actions of others.

Although the experimental evidence provides qualitative support for our theoretical predictions, we also find that the subject's actions are overly aggressive relative to the theory's predictions. Once again, the excess aggressiveness in actions stems from the aggressiveness of their beliefs about others' actions.

To capture this excess aggressiveness and the heterogeneity of agents' strategies that we observe in the laboratory, we extend the model to allow each agent to think that others are deriving an additional benefit from attacking and that the distribution of these benefits is not common knowledge in the population. The resulting model produces a distribution of predicted strategies that closely fits our experimental findings.

This study can be extended along several dimensions. Experimentally, the framework can be extended to have more than just two periods, in order to shed some light on the timing of crises. As another extension of this experimental framework, it would be interesting to see if communication plays a significant role for equilibrium selection in the dynamic global game setting, and multiplicity detection in particular. Finally, we find evidence that agents' expectations about the actions of others are overly aggressive relative to theory. Thus, a natural extension of the theory seems to involve exploring theoretical reasons for this excess aggressiveness and incorporating them into our models. Extending the literature in these directions may provide us with a better understanding of the inner-workings of a currency crisis and may therefore prove to be a fruitful line of research.

1.7 Appendix A: Derivations

1.7.1 Derivations for Section 1.2

Let us solve for some of these equilibrium values and do some comparative statics that will help with the experiment. First, we solve for the equilibrium threshold in period 1, $\theta_1^*(z)$, for the special case where cost of attacking, c , equals $\frac{1}{2}$ (as was the case in some of the experimental treatments)

$$\theta_1^* = \Phi(\sqrt{\beta_1}(x_1^* - \theta_1^*)). \quad (1.7)$$

Solving (1.7) for x_1^* , we get

$$x_1^* = \theta_1^* + \beta_1^{-1/2}\Phi^{-1}(\theta_1^*).$$

Now we substitute x_1^* into the other equilibrium equation, namely

$$\begin{aligned} \Phi\left(\sqrt{\beta_1 + \alpha}\left(\theta_1^* - \frac{\beta_1}{\beta_1 + \alpha}x_1^* - \frac{\alpha}{\beta_1 + \alpha}z\right)\right) &= 1/2 \\ \Phi\left(\sqrt{\beta_1 + \alpha}\left(\theta_1^* - \frac{\beta_1}{\beta_1 + \alpha}\left[\theta_1^* + \beta_1^{-1/2}\Phi^{-1}(\theta_1^*)\right] - \frac{\alpha}{\beta_1 + \alpha}z\right)\right) &= 1/2. \end{aligned}$$

Putting the terms that contain θ_1^* on the left-hand side of the expression, we get

$$\frac{\alpha}{\beta_1 + \alpha}\theta_1^* - \frac{\beta_1^{1/2}\Phi^{-1}(\theta_1^*)}{\beta_1 + \alpha} = \frac{\Phi^{-1}(1/2)}{\sqrt{\beta_1 + \alpha}} + \frac{\alpha}{\beta_1 + \alpha}z. \quad (1.8)$$

We can simplify this expression further, using the fact that $\Phi^{-1}(1/2) = 0$ and multiplying both sides by $\frac{\beta_1 + \alpha}{\alpha}$

$$\theta_1^* - \frac{\beta_1^{1/2}}{\alpha}\Phi^{-1}(\theta_1^*) = z. \quad (1.9)$$

Solving for θ_1^* gives us the equilibrium threshold in the first round, $\theta_1^*(z)$, such that the regime will collapse in the first round ($R_1 = 1$) if and only if $\theta \leq \theta_1^*$. Such a solution exists and is unique if and only if the following relationship holds for the precisions: $\beta_1 \geq \alpha^2/(2\pi)$. To see this, we define

$$G(\theta_1^*(z), z) \equiv z - \theta_1^* + \frac{\beta_1^{1/2}}{\alpha}\Phi^{-1}(\theta_1^*) = 0.$$

Note that $G(\theta_1^*(z), \cdot)$ is continuous and differentiable in $\theta \in (0, 1)$, and that $G(0, z) = -\infty$ and

$G(1, z) = \infty$, which implies that there necessarily exists a solution and any solution satisfies $\theta_1^*(z) \in (0, 1)$. This establishes existence. To prove uniqueness, note that

$$\frac{\partial G(\theta_1^*(z), z)}{\partial \theta_1^*} = \frac{\beta_1^{1/2}}{\alpha} (\Phi^{-1})'(\theta_1^*) - 1.$$

We can re-write this using the formula for the derivative of an inverse function:

$$(\Phi^{-1})'(\theta_1^*) = \frac{1}{\Phi'(\Phi^{-1}(\theta_1^*))} = \frac{1}{\phi(\Phi^{-1}(\theta_1^*))},$$

where $\phi(\cdot)$ is the p.d.f. of the standard normal distribution and is bounded by $\frac{1}{\sqrt{2\pi}}$ (i.e. $\max_{\omega \in \mathbb{R}} \phi(\omega) = \frac{1}{\sqrt{2\pi}}$). Therefore,

$$\frac{\partial G(\theta_1^*(z), z)}{\partial \theta_1^*} = \frac{\beta_1^{1/2}}{\alpha} \frac{1}{\phi(\Phi^{-1}(\theta_1^*))} - 1 > \frac{\beta_1^{1/2}}{\alpha} \sqrt{2\pi} - 1.$$

Then if $\frac{\beta_1^{1/2}}{\alpha} \sqrt{2\pi} - 1 \geq 0$, or if $\frac{\beta_1}{\alpha^2} \geq \frac{1}{2\pi}$, the function G is strictly increasing in θ_1^* ($\frac{\partial G(\theta_1^*(z), z)}{\partial \theta_1^*} > 0$), which implies a unique solution to (1.9).

We can use the Implicit Function Theorem to demonstrate that the threshold $\theta_1^*(z)$ is monotonically decreasing in z . Let $F(\theta_1^*(z), z)$ be defined as

$$F(\theta_1^*(z), z) \equiv \frac{\alpha}{\beta_1 + \alpha} \theta_1^* - \frac{\beta_1^{1/2} \Phi^{-1}(\theta_1^*)}{\beta_1 + \alpha} - \frac{\alpha}{\beta_1 + \alpha} z = 0$$

$$\frac{\partial \theta_1^*}{\partial z} = - \frac{\partial F / \partial z}{\partial F / \partial \theta_1^*} = - \frac{-\frac{\alpha}{\beta_1 + \alpha}}{\frac{\alpha}{\beta_1 + \alpha} - \frac{\beta_1^{1/2}}{\beta_1 + \alpha} (\Phi^{-1})'(\theta_1^*)} = \frac{1}{1 - \frac{\beta_1^{1/2}}{\alpha} (\Phi^{-1})'(\theta_1^*)}. \quad (1.10)$$

As we have shown above, the derivative of the inverse of the c.d.f. of the standard normal is positive and reaches its minimum at $\sqrt{2\pi}$. We also know that the relationship between the precisions is $\frac{\beta_1^{1/2}}{\alpha} \geq \frac{1}{\sqrt{2\pi}}$. Thus, the whole fraction in (1.10) is negative (i.e., $\frac{\partial \theta_1^*}{\partial z} \leq 0$). Intuitively, θ_1^* is decreasing in z because when the public signal (z) has a high mean, the fundamentals are relatively good. So, the region where the attack will be successful in the first period is relatively small. Thus, the threshold theta is low. In other words, when the mean of the prior is high, the agents are initially pessimistic about their ability to overthrow the regime. So, in the first period, the size of the attack is relatively small. Then, in the second period, if

the agents get a sufficiently precise private signal, an attack becomes possible. (That is, agents can become optimistic about their ability to change the status quo.) This is why this scenario can lead to multiplicity.

We can also verify that $\theta_1^*(1/2) = 1/2$ (but only if the public signal is completely uninformative relative to the private signal, that is if $\alpha/\beta_1 \rightarrow 0$). Let us substitute $1/2$ into equation (1.8):

$$\begin{aligned} \frac{\alpha}{\beta_1 + \alpha} \left(\frac{1}{2} \right) - \frac{\beta_1^{1/2} \Phi^{-1}(1/2)}{\beta_1 + \alpha} &= \frac{\Phi^{-1}(1/2)}{\sqrt{\beta_1 + \alpha}} + \frac{\alpha}{\beta_1 + \alpha} \left(\frac{1}{2} \right) \\ -\frac{\beta_1^{1/2}}{\beta_1 + \alpha} &= \frac{1}{\sqrt{\beta_1 + \alpha}}. \end{aligned}$$

Squaring both sides

$$\begin{aligned} \frac{\beta_1}{(\beta_1 + \beta)^2} &= \frac{1}{\beta_1 + \beta} \\ \frac{1}{1 + \beta/\beta_1} &= 1. \end{aligned}$$

which is true for all β_1 if $\alpha/\beta_1 \rightarrow 0$ or equivalently if $\beta_1/\alpha \rightarrow \infty$. To put this differently, we just found the Morris-Shin limit threshold. The Morris-Shin limit is the limit as the ratio of precisions of private and public information approaches infinity, or in other words private information becomes infinitely precise relative to public information. It is

$$\lim_{\frac{\beta_1}{\alpha} \rightarrow \infty} \theta_1^*(z) = \frac{1}{2} = 1 - c \equiv \theta_\infty.$$

Finally, we employ the Implicit Function Theorem again to show that $\theta_1^*(z)$ is monotonic in β_1 . Define

$$\begin{aligned} H(\theta_1^*(z), z) &\equiv \theta_1^* - \frac{\beta_1^{1/2}}{\alpha} \Phi^{-1}(\theta_1^*) - z = 0 \\ \frac{\partial \theta_1^*}{\partial \beta_1} &= -\frac{\partial H / \partial \beta_1}{\partial H / \partial \theta_1^*} = \frac{\frac{1}{2\alpha} \beta_1^{-1/2} \Phi^{-1}(\theta_1^*)}{1 - \frac{\beta_1^{1/2}}{\alpha} (\Phi^{-1})'(\theta_1^*)}. \end{aligned} \tag{1.11}$$

We already know that the denominator of (1.11) is always negative. Let us focus on the

numerator. The inverse c.d.f. of the normal has the following property:

$$\begin{aligned}\Phi^{-1}(\theta_1^*) &< 0 \text{ if } \theta_1^* < 1/2 \\ \Phi^{-1}(\theta_1^*) &> 0 \text{ if } \theta_1^* > 1/2.\end{aligned}$$

Therefore, to sign this fraction, we need to consider our two cases. In Case 2', z is low ($z < 1/2$), which implies that $\theta_1^*(z) > 1/2$ because we have proved above that $\theta_1^*(z)$ is monotonically decreasing in z and that $\theta_1^*(1/2) = 1/2$. This implies that $\Phi^{-1}(\theta_1^*) > 0$, and the numerator of (1.11) is positive (i.e., $\theta_1^*(z)$ is decreasing in β_1). In Case 2'', z is high, that is $z > 1/2$, so we know that $\theta_1^*(z) < 1/2$. In this case, $\Phi^{-1}(\theta_1^*) < 0$, the numerator of (1.11) is negative (i.e., $\theta_1^*(z)$ is increasing in β_1). This shows that $\theta_1^*(z)$ is monotonic in β_1 .

Iterated Dominance Argument.

We have established there exists a unique monotone equilibrium in the first stage whenever the noise in private information is small enough. This result, however, leaves open the possibility that there are other non-monotone equilibria. We now show that there is no other equilibrium and, what is more, that the equilibrium is dominance solvable.

For simplicity, consider the special case where $\alpha = 0$. This allows us to eliminate the dependence on z and denote the strategy by $a(x_1)$ and the aggregate size of the attack by $A(\theta)$.

For any $\tilde{x}_1 \in [-\infty, +\infty]$, let $A_{\tilde{x}_1}(\theta)$ denote the mass of agents attacking (choosing action A) when (almost every) agent chooses action A if and only if $x_1 \leq \tilde{x}_1$. Next, we define the function

$$V(x_1, \tilde{x}_1) = E[U(1, A_{\tilde{x}_1}(\theta), \theta) - U(0, A_{\tilde{x}_1}(\theta), \theta) | x_1],$$

which represents the utility difference between choosing action A and choosing action B for an agent who has a private signal x_1 and expects the other agents to attack if and only if their signals fall below \tilde{x}_1 . From the model,

$$A_{\tilde{x}_1}(\theta) = \Phi(\sqrt{\beta_1}(\tilde{x}_1 - \theta))$$

and

$$V(x_1, \tilde{x}_1) = y - y\Phi(\sqrt{\beta_1}(x_1 - \tilde{\theta}_1)) - c,$$

where $\theta = \tilde{\theta}_1(\tilde{x}_1)$ is the unique solution to $A_{\tilde{x}_1}(\tilde{\theta}_1) = \tilde{\theta}_1$, or equivalently the inverse of

$$\tilde{x}_1 = \tilde{\theta}_1 + \beta_1^{-1/2} \Phi^{-1}(\tilde{\theta}_1).$$

Note that $\tilde{\theta}_1$ is increasing in \tilde{x}_1 , which implies that $V(x_1, \tilde{x}_1)$ is increasing in \tilde{x}_1 . That is, the more aggressive the other agents are, the higher the payoff from choosing action A. Furthermore, $V(x_1, \tilde{x}_1)$ is decreasing in x_1 : the higher the private signal, the lower the expected payoff from choosing action A.

Next, note that $V(x_1, \tilde{x}_1)$ is continuous in x_1 and satisfies $V(x_1, \tilde{x}_1) \rightarrow y - c > 0$ as $x_1 \rightarrow -\infty$ and $V(x_1, \tilde{x}_1) \rightarrow -c < 0$ as $x_1 \rightarrow +\infty$. We can therefore define a function $h(\cdot)$ such that $x_1 = h(\tilde{x}_1)$ is the unique solution to $V(x_1, \tilde{x}_1) = 0$ with respect to x_1 . That is, when agents $j \neq i$ choose action A if and only if $x_{j1} \leq \tilde{x}_1$, agent i finds it optimal to choose action A if and only if $x_{i1} = h(\tilde{x}_1)$. Since $V(x_1, \tilde{x}_1)$ is continuous in both arguments, decreasing in x_1 and increasing in \tilde{x}_1 , the function $h(\tilde{x}_1)$ is continuous and increasing in \tilde{x}_1 . Finally, note that $h(\tilde{x}_1)$ has a unique fixed point $x_1^* = h(x_1^*)$ and this fixed point is indeed the threshold x_1^* of the unique monotone equilibrium that we constructed in section 1.2.1.

Now, construct a sequence $\{\underline{x}_{1,k}\}_{k=0}^{\infty}$ with $\underline{x}_{1,0} = -\infty$ and $\underline{x}_{1,k} = h(\underline{x}_{1,k-1})$ for all $k \geq 1$. In particular, letting $\underline{\theta}_{1,k-1}$ be the solution to

$$\underline{x}_{1,k-1} = \underline{\theta}_{1,k-1} + \beta_1^{-1/2} \Phi^{-1}(\underline{\theta}_{1,k-1}),$$

we have

$$V(x_1, \underline{x}_{1,k-1}) = y - y\Phi(\sqrt{\beta_1}(x_1 - \underline{\theta}_{1,k-1})) - c,$$

and thus

$$\underline{x}_{1,k} = \underline{\theta}_{1,k-1} + \beta_1^{-1/2} \Phi^{-1}(1 - c/y).$$

Thus, with $\underline{x}_{1,0} = -\infty$, we have $\underline{\theta}_{1,0} = 0$, $\underline{x}_{1,1} = \beta_1^{-1/2} \Phi^{-1}(1 - c/y)$, and so on. Clearly, the sequence $\{\underline{x}_{1,k}\}_{k=0}^{\infty}$ is increasing and is bounded above by x_1^* . Hence, it converges to some \underline{x}_1 . By continuity of $h(\cdot)$, the limit \underline{x}_1 must be a fixed point of h . But we have already proved that $h(\cdot)$ has a unique fixed point, and therefore $\underline{x}_1 = x_1^*$.

Next, construct a sequence $\{\bar{x}_{1,k}\}_{k=0}^{\infty}$ with $\bar{x}_{1,0} = -\infty$ and $\bar{x}_{1,k} = h(\bar{x}_{1,k-1})$ for all $k \geq 1$.

Note that the sequence is decreasing and is bounded below by x_1^* . Hence, sequence $\{\bar{x}_{1,k}\}_{k=0}^\infty$ converges to some \bar{x}_1 . By continuity of $h(\cdot)$, the limit \bar{x}_1 must be a fixed point of h . But we have already proved that $h(\cdot)$ has a unique fixed point, and therefore $\bar{x}_1 = x_1^*$. ■

1.7.2 Derivations for Section 1.5

We start by considering an agent i with a specific belief about the average benefit of other agents, μ_i . First, we determine the strategy that agent i thinks other agents, j , are following in equilibrium by solving a fixed point problem. In particular, we look for monotone Bayesian equilibria such that the other agents' strategy is non-increasing in x . Note that the posterior probability of regime change given x and z is now given by $\Pr(A(\theta) \geq \theta|x, z)$, where $A(\theta)$ is the average aggregate size of the attack given by

$$A(\theta; x^\#(B_j; \mu_i), F) = E[\Pr(x < x^\#(B_j; \mu_i)|\theta, B_j)] = \int \Phi\left(\frac{x^\#(B_j; \mu_i) - \theta}{\sigma_{x_1}}\right) dF(B_j)$$

where $\Phi(\cdot)$ is a standard normal distribution and $F(B_j)$ is the distribution function for the others' benefit of attacking, B_j , that exists in the mind of agent i . Note that $A(\theta; x^\#(B_j; \mu_i), F)$ is a monotonically decreasing function of θ . It follows then that, in the mind of agent i , there exists a unique $\theta_B^*(\mu_i)$ such that $A(\theta; x^\#(B_j; \mu_i), F) \geq \theta$ if and only if $\theta < \theta_B^*(\mu_i)$, where θ_B^* solves the following equation $\int \Phi\left(\frac{x^\#(B_j; \mu_i) - \theta_B^*(\mu_i)}{\sigma_{x_1}}\right) dF(B_j) = \theta_B^*(\mu_i)$. In the mind of agent i , the best response for agent j for a given realization of B_j is then to attack if and only if $x_j \leq x^{\#\#}(B_j; \mu_i)$ where $x^{\#\#}(B_j; \mu_i)$ solves $\Phi\left(\sqrt{\beta_1 + \alpha}\left(\theta_B^*(\mu_i) - \frac{\beta_1}{\beta_1 + \alpha}x^{\#\#}(B_j; \mu_i) - \frac{\alpha}{\beta_1 + \alpha}z\right)\right) = c - B_j$. In equilibrium, $x^\#(B_j; \mu_i) = x^{\#\#}(B_j; \mu_i)$. Thus, for given values of $z, F(B_j), \alpha$, and β , the following two equations characterize the excess-aggressiveness equilibrium in the mind of agent i .

$$\theta_B^*(\mu_i) = \int_{c-1}^c \Phi\left(\frac{x^\#(B_j; \mu_i) - \theta_B^*(\mu_i)}{\sigma_{x_1}}\right) dF(B).$$

$$\Phi\left(\sqrt{\beta_1 + \alpha}\left(\theta_B^*(\mu_i) - \frac{\beta_1}{\beta_1 + \alpha}x^\#(B_j; \mu_i) - \frac{\alpha}{\beta_1 + \alpha}z\right)\right) = c - B_j.$$

The system of two equations above can be solved numerically to obtain the thresholds

$(x^\#(B_j; \mu_i), \theta_B^*(\mu_i))$ that agent i thinks other agents are following. The bounds of integration have to be set to be $[c - 1, c]$ in order to satisfy the constraint $0 \leq c - B \leq 1$, since the solution requires us to take an inverse-cdf of $(c - B)$. The distribution function $F(B_j)$ is then re-normalized so that the p.d.f. adds up to 1.

The second step is to compute a threshold $x_B^*(\mu_i)$ that is agent i 's best response to the average $x^\#(B_j; \mu_i)$, which is simply the individual threshold that sets the posterior probability of regime change $Pr(\theta \leq \theta_B^*(\mu_i) | x^\#(B_j; \mu_i))$ equal to the correct cost of attacking, c .

$$\Phi \left(\sqrt{\beta_1 + \alpha} \left(\theta_B^*(\mu_i) - \frac{\beta_1}{\beta_1 + \alpha} x_B^*(\mu_i) - \frac{\alpha}{\beta_1 + \alpha} z \right) \right) = c.$$

1.8 Appendix B: Tables and Figures

Table 1.B1.

Descriptive Statistics.

	Sessions 1-4	Sessions 1-4	Sessions 5-6	Sessions 5-6
	Cost 20	Cost 50	No New Info	New Info
Min θ	-73.83	-92.73	-107.48	-49.06
Max θ	208.46	188.30	256.30	256.68
Mean θ	73.48	64.40	77.78	74.10
Min x (Stage 1)	-82.55	-106.92	-128.23	-71.84
Max x (Stage 1)	220.85	201.84	277.64	281.59
Mean x (Stage 1)	73.37	64.45	77.80	74.25
Mean # Attackers (Stage 1)	9.16	8.89	6.88	7.18
Mean # Attackers (Stage 2)	1.28	0.89	0.50	1.94
Mean Belief (Stage 1)	8.57	8.35	6.62	7.03
Mean Belief (Stage 2)	1.76	1.58	1.08	2.71
% Successful Attacks (Stage 1)	57%	54%	43%	41%
% Successful Attacks (Stage 2)	1.9%	0%	0%	2.1%
Median Comfort Level with Stats and Probability (out of 5)	4	4	4	4
Number of subjects	120	120	60	60

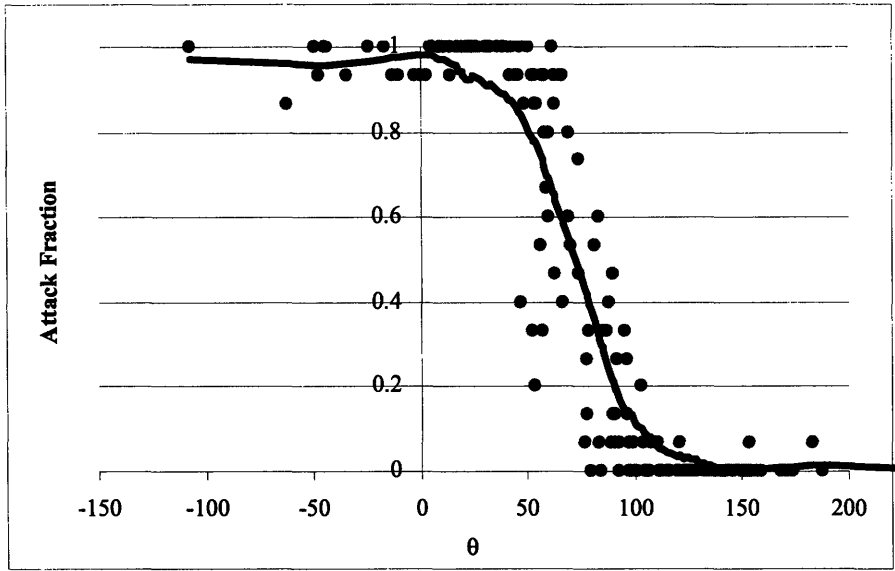


Figure 1.B1: Aggregate Size of Attack vs. θ (Cost-60 Treatment)

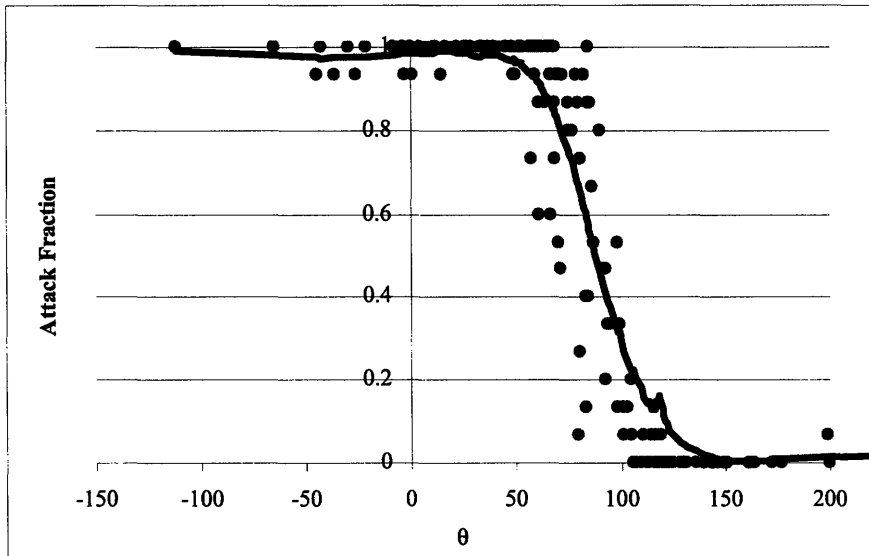


Figure 1.B2: Aggregate Size of Attack vs. θ (Cost-20 Treatment)

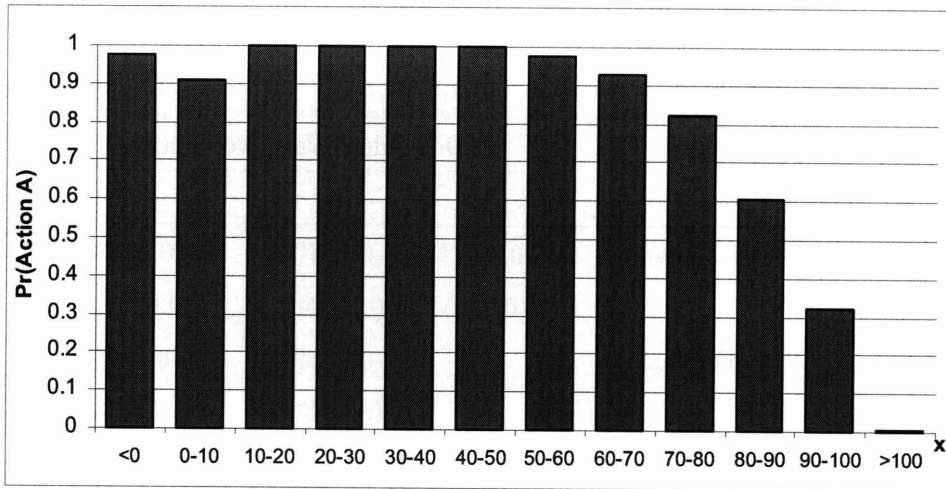


Figure 1.B3: Probability of Attack vs. x (Cost-20 Treatment)

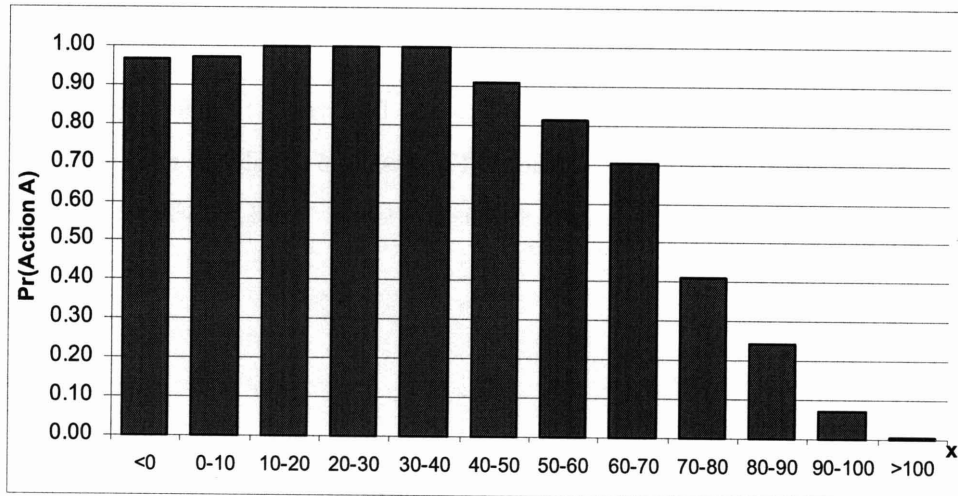


Figure 1.B4: Probability of Attack vs. x (Cost-60 Treatment)

Table 1.B2.

Stage 1 Individual Level CLogit Regressions (Pooled Data for All Sessions)

Variable	Dependent Variable: Action		
	1	2	3
Private signal, x	-0.105*** (0.010)	-0.106*** (0.0104)	-0.06*** (0.0091)
Cost of Attacking		-0.031*** (0.0046)	-0.012* (0.0065)
Belief			0.684*** (0.0511)
Pseudo R^2	0.74	0.75	0.91
No. of observations	6000	6000	6000

Note: Standard errors in parentheses. Regressions include subject, group, and round fixed effects. For sessions 5-6, only the no-new-info treatment data are used. Significance levels: **10%, * 5%, ***1%

Table 1.B3.

Individual Level CLogit Regressions (Pooled Data for All Sessions)

Variable	Dependent Variable: Action		
	1	2	3
Private signal, x	-0.105*** (0.0090)	-0.108*** (0.0096)	-0.061*** (0.0097)
Cost of Attacking		-0.034*** (0.0044)	-0.015** (0.0064)
Belief			0.708*** (0.0485)
Stage	-1.634*** (0.1157)	-1.596*** (0.1145)	-0.844*** (0.1785)
Pseudo R^2	0.75	0.76	0.92
No. of observations	8820	8820	8820

Note: Robust standard errors in parentheses. Regressions include subject, group, and round fixed effects. For sessions 5-6, only the no-new-info treatment data are used. Significance levels: ** 5%, *** 1%

Table 1.B4.
 Stage 2 Individual Level CLogit Regressions Using (Sessions 5 and 6)

	Dependent Variable: Action
Private Signal, x	-0.1428*** (0.0199)
NI Dummy	0.7783* (0.4518)
R ²	0.67
No. of observations	1395

Note: Standard errors in parentheses.

Regressions include group, subject, and round fixed

effects. Significance level: *10%, *** 1%.

Table 1.B5.
 Aggregate-Level OLS Regressions of Average Fraction of
 Mistakes on Rounds

	Dependent Variable: Average Fraction of Mistakes	
	Sessions 1-2	Sessions 3-4
Round*Cost	-0.0024 (0.0019)	-0.0011 (0.0021)
Constant	0.1148** (0.0261)	-0.071** (0.0044)
R ²	0.04	0.01
No. of observations	40	40

Note: Robust standard errors in parentheses.

Significance levels: **5%, *** 1%.

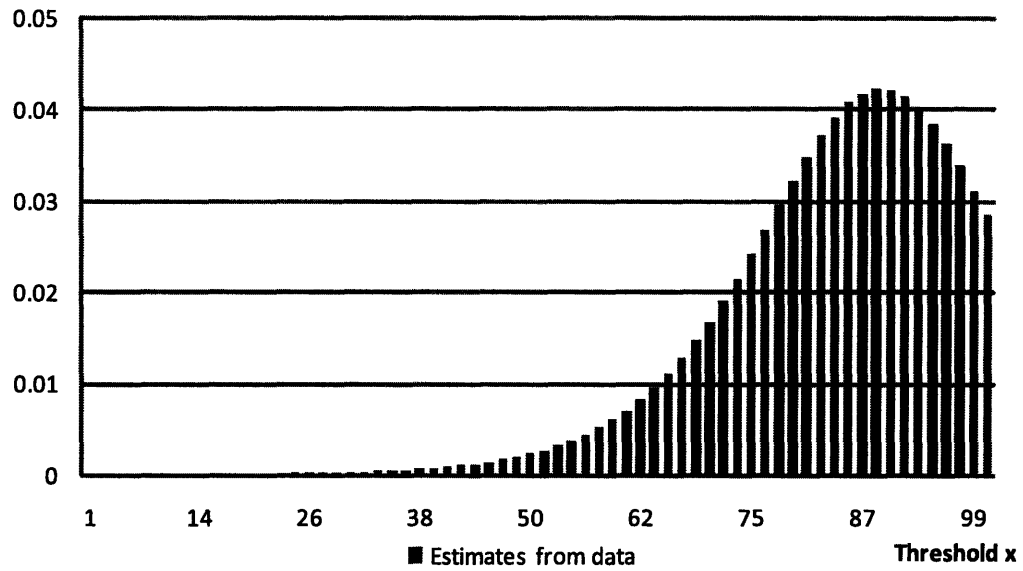


Figure 1.B5: The PDF of Individual Thresholds \hat{x}_{is} for the Cost-20 Treatment

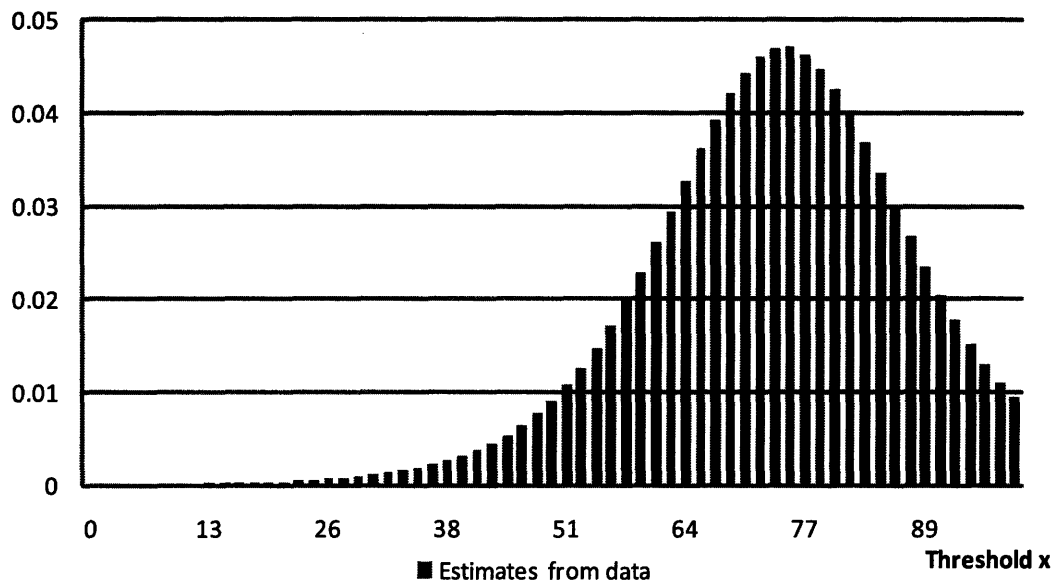


Figure 1.B6: The PDF of Individual Thresholds \hat{x}_{is} for the Cost-60 Treatment

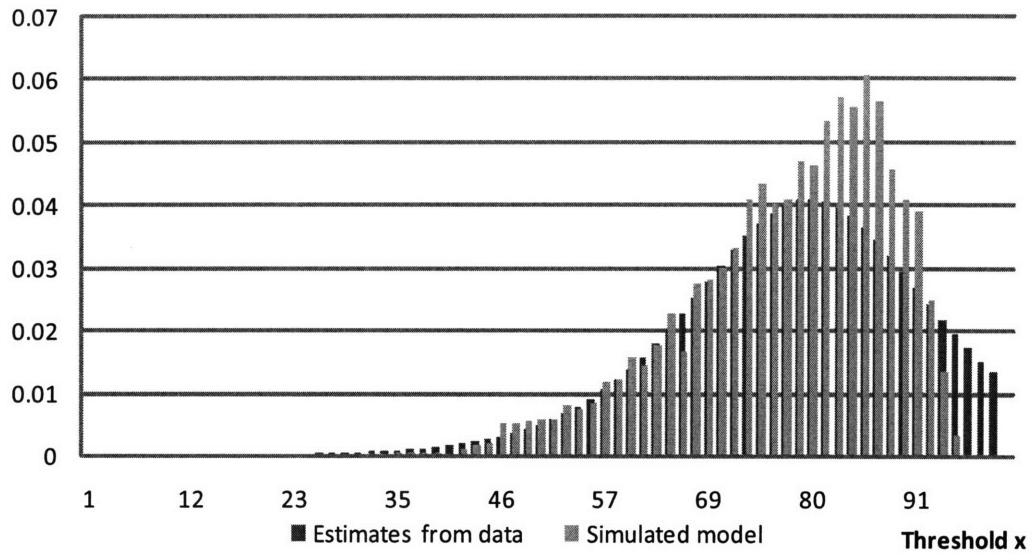


Figure 1.B7: The Distribution of Individual Thresholds in the Model and in the Data (Cost 50)

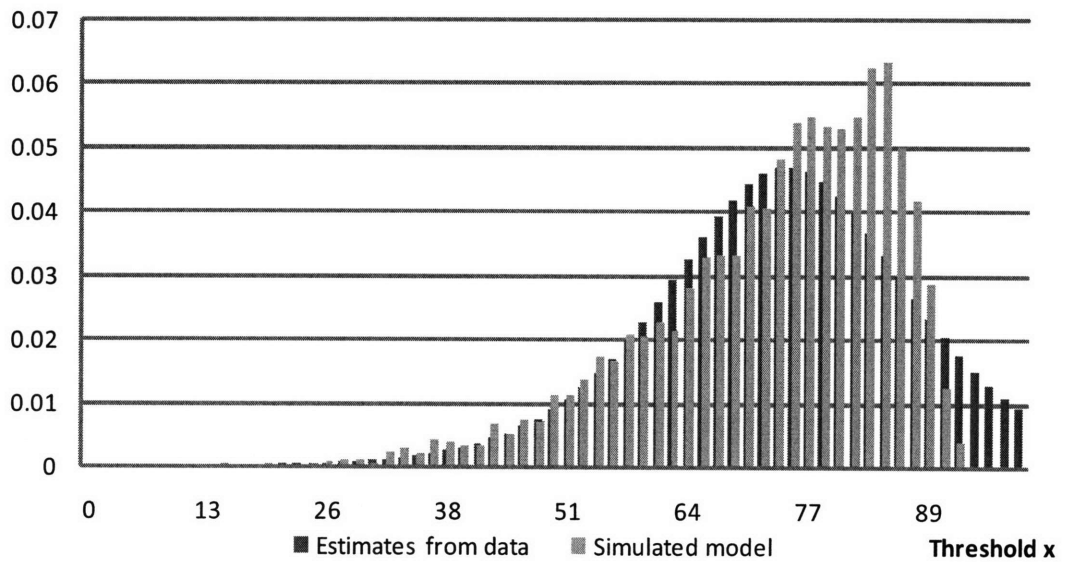


Figure 1.B8: The Distribution of Individual Thresholds in the Model and in the Data (Cost 60)

1.9 Appendix C: Sample Instructions

The following instructions are translated from original German for the No-New-Information/
New Information Session.

Instructions for the experiment
--

You are now participating in an economic experiment. Please read the following instructions carefully, paying attention to details. You will receive all the information you require for participation in the experiment. If you do not understand something, please raise your hand. We will answer your question at your desk.

Communication between participants is absolutely forbidden during the experiment! Not obeying this rule will lead to immediate exclusion from the experiment and all payments. If you have a question during the experiment, please raise your hand.

Your payment in this experiment will be calculated in points at first. The total point score you earn during the experiment will be converted to US Dollars at the end of the experiment. The following exchange rate applies in this case:

$$10 \text{ points} = 25 \text{ centimes}^{32}$$

You will receive the amount of points you earned during the experiment plus 15 Swiss Francs for appearing in **cash**.

The experiment consists of **20 rounds**; each round consists of one or two decision stages.

You are one of **15 people** who interact with each other during the experiment. In the first stage of each round, you must choose either action A or B, based on the information available to you. If there is a second stage in a round, you will again have to decide choose either action A or B.

³²Translator's note: Exchange rate in June 2006: 1 Swiss Franc = 0.81 U.S. Dollars

Income

You must choose either action A and action B in every stage of every round.

- If you choose action A, you will incur a cost of 60 points and you will earn a gross income of either 100 or 0 points, depending whether action A is successful or not.
 - If action A is successful, you will earn an income of $100 - 60 = 40$ points
 - If action A is not successful, you will incur a loss of $0 - 60 = -60$ points.

Again, whether action A is successful or not depends on whether more than $Y\%$ of the 15 people chose action A.

- If you choose action B, you will neither incur costs nor earn an income, independent of what others have chosen. Your income from B is thus always 0.

If action A is successful in the first stage (i.e., enough of the 15 other people chose it), the round will end and the next one will begin. If action A was not successful in the first stage (i.e., less than $Y\%$ of the other 15 people selected it), the round will continue into the second stage. In the second stage, each of the 15 participants must again choose either action A or B. The round always ends after stage 2, at which point the next round begins (20 rounds in total). Losses, which may occur if action A is unsuccessful, will be financed by the income from other rounds or, if necessary, from the show up-fee of 15 Swiss Francs.

What determines whether action A is successful or not?

The number Y dictates the minimum percentage of people that need to choose action A for action A to be successful. **The number Y is randomly determined in each round and remains fixed for the duration of each round.** If, for example, Y is 60, then at least 60% of the 15 people (i.e. at least 9 people) must choose action A for it to be successful. In this

case, all who choose action A earn an income amounting to $100 - 50 = 50$ points. If fewer than 60% select A (8 people or fewer), A will be unsuccessful. In this case, all who choose A will incur a loss amounting to 50 points ($0 - 50 = -50$).

The computer selects the number Y at the beginning of each round from a normal distribution with an **average value** of 65 and a standard deviation of 50. This means that the average value of Y is 65, but the number Y drawn may deviate from the average value in a round. Positive and negative deviations from the number 75 are equally probable. The distribution (standard deviation) of the number Y was chosen in such a way that there is approximately a 33% probability that Y lies between 20 and 75 and an equal probability of approximately 33% that Y lies between 75 and 120. For reasons of simplicity, the number Y selected is rounded to one decimal point.

Please note that Y can also take on a negative value. In this case, a single individual suffices to make action A successful. Y can also exceed 100. In this case, action A is never successful, even if all 15 people (i.e. 100%) choose action A. The attached information sheet shows the minimum number of people needed to choose A in order for A to be successful.

Information

Your Private Hint Number

You do not know how large Y is when you make your decision. You only know that Y has an average value of 75 and a distribution of 55.

Instead, you will receive a **private hint number** x that gives you information about the value of Y. The private hint number x is given in the form of $x = Y + z$, where z is a normally distributed random variable with an average value of 0 and a standard deviation of 10. **On average, the private hint number x accurately reflects the value of Y, because the value of the random variable z is zero on average.** However, in any given situation, the hint number can differ from Y. **In particular, there is approximately 67% probability that Y lies within ± 10 of your hint number x.**

Example 1: You receive a private hint number of 24.5. There is thus a probability of approximately 67% that the unknown random number Y lies within ± 10 of your private hint

number (i.e., there is 67% probability that Y lies between 14.5 and 34.5).

Example 2: You receive a private hint number of 62.8. There is a 67% probability that Y lies between 52.8 and 72.8.

Exact procedure in stage 1 of a round

The computer first draws the random number Y . The random number is the same for all participants. Next, a private hint number is given to each of the other 15 participants. Since the private hint numbers vary around the true value of Y , each of the 15 participants usually receives a different private hint number. However, there is always a 67% probability that the true value of Y lies within the interval of ± 10 of the private hint number.

Each of the 15 participants then decides whether to choose action A or B. The decision is entered on the decision screen (see the next page for the example). When you have made your decision, please press the OK button. You can revise your decision until you press the OK button.

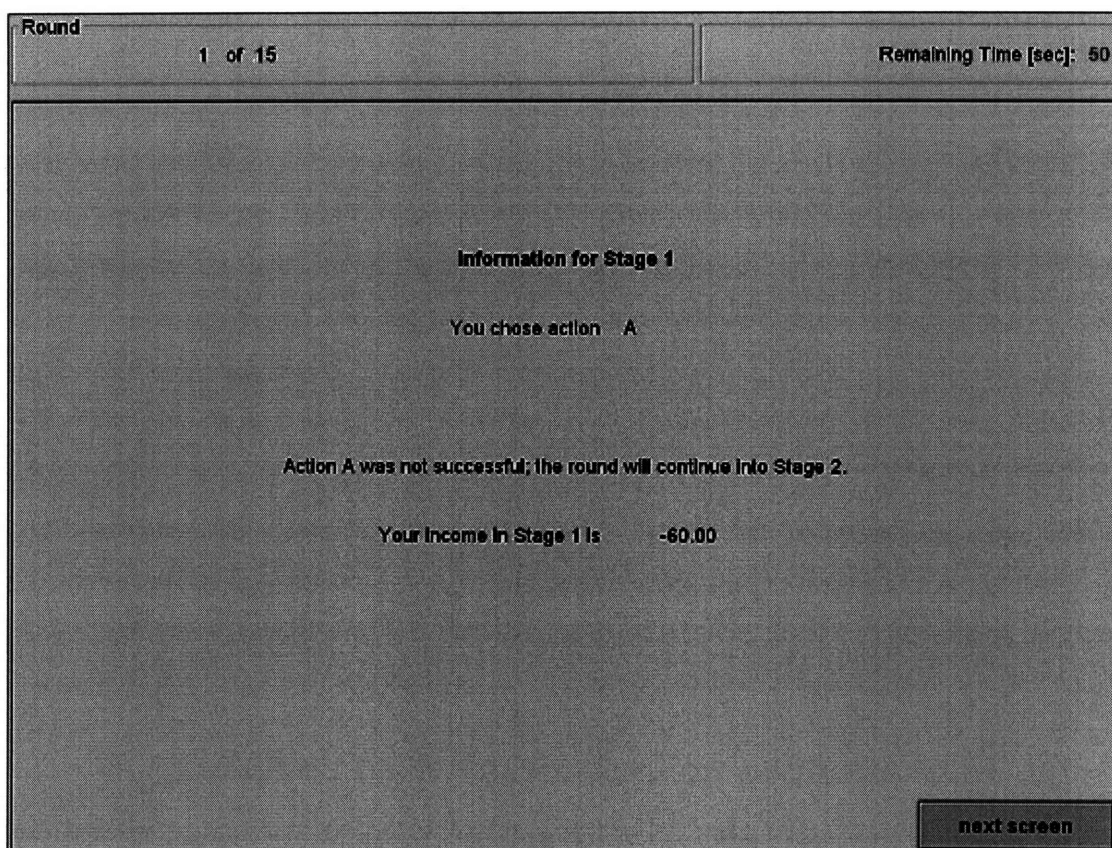
The first screen you see:

The screenshot shows a game interface with a dark background and white text. At the top, there are two boxes: the left one says "Round 1 of 15" and the right one says "Remaining Time [sec]: 54". Below this, the text reads "You are in currently Stage 1." followed by "THE ANNOUNCER CHOSE ACTION B". Then, "Your hint number is 88.5". At the bottom, it says "Please, choose either action A or B" and "I choose:" with two radio button options, "A" and "B". In the bottom right corner, there is a black button with the text "OK" in white.

After all the 15 people have decided, each is asked on a new screen about his or her assessment of the frequency of action A.

The next screen will contain relevant information. If action A is successful, each of the 15 participants will be informed about how many people chose action A, the fact that action A was successful, the actual value of the random number Y , and the income in the round. If action A was not successful, each of the 15 participants will be informed that action A was not successful, that the round will continue into the second stage, and each will learn the income in the first stage.

The information screen in Stage 1:



The exact procedure in stage 2 of a round

If action A was not successful in the first stage, the 15 participants will not be informed of actual value of Y , but will be reminded of the hint number they received in the first stage. Then, the 15 participants must again decide between actions A and B. The rules of this stage are otherwise identical to those in the first stage.

The calculation of income in stage 2 is exactly the same as that in stage 1. Individuals who opt for action B neither earn an income nor incur a cost. Those who opt for action A earn an income of $100 - 60 = 40$ points, if action A is successful, and an income of $0 - 60 = -60$ points, if action A is not successful.

Another information screen appears at the end of stage 2 that informs you about the following (please, refer to the screen on the next page):

- Your income in stage 1
- Number of people who chose action A in stage 2
- The random number Y in this round
- Whether action A was successful in stage 2
- Your income in stage 2
- Your total income in this round

The information screen in Stage 2:

Round 1 of 15 Remaining Time [sec]: 47

Complete Information after Stage 2

You chose action A in Stage 2

Your Income in Stage 1 was	-60.00
The total number of people who chose action A in Stage 1 was	10
The total number of people who chose action A in Stage 2 was	12
The random number was	76.2

In Stage 2, action A was successful.

Your Income in Stage 2 is	40.00
Your total income in this round is	-20.00

next screen

A new round then begins; the computer first draws a new random number Y. You will then receive a new private hint number based on the new random number Y, which will give you

information about Y.

The incomes stemming from each round will be added up at the end of the experiment. In addition to the show-up fee, this will constitute your entire income for the experiment. Losses that might result from individual rounds will be funded by means of income from other rounds or, if necessary, from the show-up fee of 15 Swiss Francs.

Control questions

Please answer all of the following questions. If you have any questions, please raise your hand!

1. The random number Y has the value of -3.4 . How many people must choose action A for A to be successful?

At least _____ people must choose A.

2. The random number Y has the value of 34.2 . How many people must choose action A for A to be successful?

At least _____ people must choose A.

3. The random number Y has the value of 105.0 . How many people must choose action A for A to be successful?

At least _____ people must choose A.

4. Your private hint number is 16.4 . Find the interval around your private hint number within which the random number Y lies with a probability of 67% .

The interval around my private hint number is _____

5. Your private hint number is 48.1 . Find the interval around your private hint number where the random number Y lies with a probability of 67% .

The interval around my private hint number is _____

6. The random number Y is 63.1 , and your private hint number is 56.4 . Assume you choose action A at stage 1 of this round. Four other people also select action A.

a) How much do you earn at stage 1 of this round? I earn _____ points.

b) You again choose action A at stage 2 and 9 other people also choose action A. How much do you earn at stage 2 of this round?

I earn _____ points in stage 2.

7. The random number Y is 63.1, and your private hint number is 56.4. Assume you choose action B at stage 1 of this round. Four other people select action A.

a) How much do you earn at stage 1 of this round? I earn _____ points.

b) You then choose action B at stage 2 and 10 other people choose action A. How much do you earn at stage 2 of this round?

I earn _____ points in stage 2.

Information sheet

The random number Y indicates the minimum percentage of people who must choose action A in order for action A to be successful. The following table shows how many people must choose action A in order for action A to be successful, if the random number assumes certain values.

In order to understand the table, you should keep in mind that 1 of 15 participants represents 6.6%, 2 of 15 participants represent $2 \times 6.6\% = 13.3\%$, etc.

The right column shows the minimum number of participants who must choose action A for action A to be successful. The left column shows the corresponding intervals for the random number Y .

IF THE UNKNOWN NUMBER Y LIES IN THE FOLLOWING INTERVAL:	THE FOLLOWING MINIMUM NUMBER OF THE 15 PEOPLE MUST CHOOSE A FOR ACTION A TO BE SUCCESSFUL.
<0	0
0 – 6.6	1
6.7 – 13.3	2
13.4 – 20.0	3
20.1 – 26.6	4
26.7 – 33.3	5
33.4 – 40.0	6
40.1 – 46.6	7
46.7 – 53.3	8
53.4 – 60.0	9
60.1 – 66.6	10
66.7– 73.3	11
73.4 – 80.0	12
80.1 – 86.6	13
86.7 – 93.3	14
93.4 – 100	15
>100	>15 (i.e. impossible)

Instructions for a second experiment

A second experiment will now take place, after which the entire experiment will conclude and you will receive the following amount in cash:

Your show up fee of 15 Swiss Francs
+ Your income from experiment 1
+ Your income from experiment 2
= Total Income

The previous conversion rate applies for the calculation of the income from the second experiment, namely

10 points = 25 centimes

Information on the procedure of the second experiment

The new experiment is almost identical to the previous one and also consists of 20 rounds. The only difference from the previous experiment is as follows. If the experiment proceeds into stage 2 of a round, each of the 15 participants will receive an additional, **much more precise, private hint number**. The additional private hint number is a lot more precise (100 times more precise) such that there is approximately 67% probability that **Y now lies within ± 1 of your hint number**. Please, note that the value of the random number Y is the same in stage 1 and stage 2. However, because of **the additional, more precise hint number, you can better gauge the value of Y in stage 2**.

Example 1: You receive a private hint number of 20.2 in stage 2. There is thus a probability of approximately 67% that the unknown random number Y lies within ± 1 of your private hint number (i.e., there is 67% probability that Y lies between 19.2 and 21.2).

Example 2: You receive a private hint number of 63.1 in stage 2. There is a 67% probability that Y lies between 62.1 and 64.1.

Chapter 2

Performance in Competitive Environments: Are Women Really Different?

2.1 Introduction

The study of gender differences in performance has a long history in the field of economics. Despite a recent policy push toward the equalization of men and women in the workplace and in society, considerable inequality persists, especially in high profile jobs. The possible explanations put forth in the literature can be sorted into three categories. The first explanation relies on gender differences in skills and preferences that lead to occupational self-selection (Polachek 1981; Macpherson and Hirsch 1995). The second explanation points to discrimination in the workplace which results in differential treatment of men and women of identical abilities and preferences (Black and Strahan 2001). The final and most recent class of explanations for the gender gap rests on the experimental evidence that women may be less effective than men in certain competitive environments (Gneezy, Niederle, and Rustichini 2003; Niederle and Vesterlund 2007). In this study, we would like to explore the latter explanation further and ask whether the nature of the competitive task at hand influences the outcome.

Society generally perceives men to be better than women at following directions and reading

maps, while women supposedly tend to follow landmarks when driving (Bhattacharya 2005). When it comes to solving mazes, men are found to be overwhelmingly superior to women (Pease and Pease 2000, p. 107). Similarly, men are perceived to have higher math abilities relative to women, while women are perceived to have superior verbal skills. In particular, Pajares and Valiante (2001) note that differences in achievement of middle school students lie in the stereotyped beliefs about gender differences rather than gender itself. Girls report stronger motivation and confidence in writing and receive higher grades in language arts. Boys report stronger performance-approach goals (Pajares and Valiante 2001).

Previous experimental studies on gender differences under competition have focused primarily on tasks that are typically perceived to be better suited for men (Gneezy, Niederle, and Rustichini 2003; Niederle and Vesterlund 2007). In fact, these studies cite the so-called “stereotype threat” as a possible explanation for why women tend to “shy away from competition” in their experiments. The idea of a stereotype threat first appeared in the field of psychology. It describes the fear that certain behavior would confirm an existing stereotype of a group with which one identifies (Steele and Aronson 1995; Spencer, Steele, and Quinn 1999). Presumably, competition against men can bring out this stereotype threat in women, hindering their performance in historically male-dominated tasks.

Both Gneezy, Niederle, and Rustichini (2003) and Niederle and Vesterlund (2007) motivate their experimental research by the fact that gender gaps in income and social position are widespread. Bertrand and Hallock (2001) document this fact by gathering data on the five highest-paid executives of a large group of U.S. firms over the period of 1992–1997, where they find that only 2.5 percent of the executives in the sample are women.¹ Several authors have argued that this inequality is due to the innate inability of women to compete (see Baron-Cohen (2003), Lawrence (2006), and the citations in Barres (2006)). Gneezy, Niederle, and Rustichini (2003) and Niederle and Vesterlund (2007) argue that, in mathematical tasks, women fail to compete against men, but not against other women. The authors claim that these results help explain gender inequality in the labor force. However, this conclusion deserves further

¹A similar underrepresentation of women is found among CEOs at Fortune 500 companies (<http://money.cnn.com/magazines/fortune/fortune500/womenceos>), tenured faculty at leading research institutions (MIT 1999), and or top surgeons in New York City according to New York magazine (<http://nymag.com/bestdoctors/>).

thought, because the workplace is a complex environment. During the competition to reach the top in almost all spheres of business and politics, one's success is rarely measured solely based on performance in mathematical tasks that end in a matter of minutes. Women and men compete also on the basis of verbal and communication abilities, which are *not* associated with a stereotype threat against women. On the contrary, the aforementioned studies suggest that, in verbal tasks, the stereotype threat might actually negatively affect men, not women.

This study uses a verbal task in a competitive environment in order to shed light on the following two hypotheses. The first hypothesis is that competition hinders women's performance relative to the performance of men in *any* environment. In order to test this hypothesis, we run a controlled experiment where groups of men and women solve "Word-in-a-Word" puzzles. In the benchmark treatment, the subjects are paid according to their own performance in the task. Each group member is paid per valid word found within a larger word over a period of several minutes. We do not find statistically significant differences between men and women in this treatment.

To study the effects of competition, we use a tournament, where only the participant with the highest score is paid proportionally to his or her output. In contrast to previous studies which use tasks that stereotypically favor men, we find no significant gender differences in performance with the competitive payment scheme, which leads us to reject the first hypothesis.

The rejection of the first hypothesis leads us to consider the second hypothesis: gender differences stem not from competition *per se*, but rather from the effect of stereotype threat made salient in a competitive setting. In other words, stereotype threat has a symmetric effect on both genders. If the second hypothesis holds, just as women's performance suffers relative to men's when the competitive task is of a mathematical nature, symmetrically, men's performance should also suffer relative to women's when the competitive task is verbal. This can be explained by a rational inclination for the stereotyped-against group to compete less due to the presence of seemingly superior competitors (this stereotype-threat explanation is proposed, for example, by Gneezy, Niederle, and Rustichini 2003).

We observe that neither women nor men increase their performance in competition with verbal tasks. First, it seems that competition affects men in a manner that is consistent with the second hypothesis: they compete more in tasks that stereotypically favor men (math,

mazes, etc.) and not in tasks that stereotypically favor women (verbal tasks). However, a true test of the hypothesis involves comparing the performance of men under piece-rate and under tournament in a single-sex environment which eliminates any potential effect of stereotype threat. We find that competition does not improve the performance of men in a single-sex environment, which implies that the second hypothesis can be rejected.

There is another potential reason for women not outperforming men in a tournament, even with tasks that favor them. Note that in all previous experiments, including those described in this chapter so far, men and women have been solving various tasks under intense time pressure. Deadlines in the workplace, though typically strict, allow the workers an opportunity to think deeply about their decisions. Time constraints are rarely as pressing as they appear in previous laboratory experiments on the topic. At the same time, recent evidence by Paserman (2008) leads us to believe that pressure has a larger detrimental effect on women's performance under competition as compared to men's performance.

In order to shed light on the impact of time pressure in competitive verbal tasks, we run the piece-rate and tournament treatments giving the subjects ample time to find as many words as they possibly can within a larger word. We also now provide them with an opportunity to give up before the time runs out, which allows us to test whether men and women have different attitudes regarding effort in these tasks.

Under this scenario, we find that extra time does not create a large difference between male and female performance in the piece rate condition. However, extended time in a competitive environment greatly increases the performance of women – so much so that they significantly outperform the men. A large portion of women's improvement over the men comes from the quality of output they produce. Women seem to use the extra time to ensure that their words are correct, which results in a smaller relative number of mistakes. On the other hand, men tend to enter more invalid words, which results in a higher frequency of mistakes. In addition, men also give up slightly more than women in the competitive round. This evidence is consistent with the literature in evolutionary biology that suggests that men, as “hunters,” tend to have lower attention spans. On the other hand, women, as “gatherers,” tend to pay attention to detail and can stay focused on a singular task for a prolonged period of time. For example, men often flick through TV channels and do not have the patience to watch commercials, while

women are not as averse to sitting through the boring breaks (Sullivan 2001, Pease and Pease 2000). Similarly, our findings are consistent with the notion that higher levels of the hormone testosterone in men are associated with a lower attention span (Sullivan 2001). This study therefore supports the research that “shows that we are more products of our biology than the victims of social stereotypes” (Pease and Pease 2000).

Related Literature. Gneezy, Niederle, and Rustichini (2003) test whether men and women differ in their propensity to perform in competitive environments using an experiment where groups of three men and three women perform the task of solving computerized mazes. In the piece-rate treatment, men and women do not differ significantly in terms of performance. Under competition, men significantly outperform the women, which leads to a large gender gap in performance. This gap disappears in a single-sex environment: women’s scores increase significantly in a tournament against other women. This finding is supported by a recent field study conducted by Gneezy, Leonard, and List (2008), who compare the effects of competition on gender performance in two distinct societies: a patriarchal society and a matrilineal society. Men compete at about twice the rate as the women in the former, while the opposite is true in the latter.² Gneezy and Rustichini (2004) obtain similar results as Gneezy, Niederle, and Rustichini (2003), when they analyze the performance of young boys and girls in a race over a short distance. Note that, just as with reading maps or following directions, men can be perceived to be more skilled at solving mazes or running. The benchmark treatment shows that this perception is most likely false. However, in a competitive setting, the social perceptions become more salient, which may inhibit female performance.

Niederle and Vesterlund (2007) conduct a laboratory experiment where men and women add up five two-digit numbers with and without competition. Although there are no gender differences in performance under either of these compensation schemes, there is a substantial gender difference when participants subsequently choose the scheme they want to apply to their next performance. Twice as many men as women choose the tournament over the piece rate. This gender gap in tournament entry is not explained by performance either before or after the entry decision. However, as in the previous experiment, the task of adding up numbers,

²Note that Gneezy, Leonard, and List (2008) use a task of tossing a ball into a bucket placed 3 meters away from the subject, which is a task that is likely to be perceived as better-suited for men.

though very simple, is of mathematical nature and therefore perceived to be biased toward men. Again, the piece-rate treatment demonstrates the falsehood of this perception, since there is no difference in performance between men and women. On the other hand, the tournament treatment forces subjects to think more in terms of social norms and points out the idea that women are not supposed to be good at math.

Field data has also been used to provide evidence for a gender gap in performance in competitive settings. Paserman (2008) uses data from nine tennis Grand Slam tournaments played between 2005 and 2007 to assess whether men and women respond differently to competitive pressure in a setting with large monetary rewards. The author's detailed point-by-point analysis reveals that, relative to men, women are substantially more likely to make unforced errors at crucial junctures of the match.

In all of the above studies, differences in performance between men and women may be explained by the salience of stereotype threat brought about by competition. The importance of non-gender stereotype threat for performance is documented in the broader literature. For example, Hoff and Pandey (2004) conduct a series of field experiments where low-caste male junior high school student volunteers in rural India performed the task of solving mazes under economic incentives. The authors find no caste differences in performance when caste is not publicly revealed, but making caste salient created a large and robust caste gap. Furthermore, when the link between performance and payoff was purely mechanical, the caste gap disappeared.

Bracha et al. (2008) show that stereotype threat can also be made salient through more subtle channels than a public announcement. In this recent study, the authors investigate the effects of affirmative action on performance in mathematical tasks under a piece-rate incentive scheme. The preliminary findings suggest that women score lower and attempt fewer questions in the rounds where they expect affirmative action to take place.

This chapter is the first to document the effects of competition in a non-mathematical task on gender performance and to show that stereotype threat is not the predominant reason for gender differences. In addition, this chapter demonstrates that women do not shy away from competition against men per se. In fact, women outperform men in a tournament when time pressure is reduced. Another contribution of this study is the finding that, in a low-pressure

tournament, women benefit from an increase in both the quality and the quantity of their work. On the other hand, men's quality of work suffers due to an increase the quantity of output.

The rest of the chapter is organized as follows. Section 2.2 describes the experimental design, procedures, and treatments. Section 2.3 presents the results of the data analysis. Section 2.4 concludes and discusses potential future research in this area.

2.2 Overview of the Experiment

In order to gauge the effects of different payment schemes on performance of men and women, we keep with the pervious literature and conduct a laboratory experiment in which the subjects solve a real verbal task.

2.2.1 The Task

The subjects were told in the instructions that their objective in this experiment was to work on a series of Word-in-a-Word puzzles.³ In particular, in each round, subjects would have three or fifteen minutes (depending on the treatment) to find as many sub-words that can be formed out of the letters of a big word as possible. In order to control for the level of difficulty across the various treatments within the same experiment, we picked puzzles that contain a similar number of sub-words and that have a similar maximum possible score. At the beginning of the experiment, each participant was given three minutes to solve a practice Word-in-a-Word puzzle in order to get familiar with the task.

The following are the general rules for all the puzzles.

- The sub-words must be 4-letters long or longer.
- Acceptable characters are the letters A-Z only. Any other symbol like a number or another symbol is automatically discarded.
- Proper nouns are not allowed.

³For a complete set of instructions, see Appendix A. The puzzles were computerized using the programming language Python and were based on the games provided by the website www.wordplays.com.

- Plurals are allowed.
- Each letter in the puzzle word can only be used once within each new sub-word.

The following are the scoring rules for all the puzzles.

- Valid words add $(N - 3)$ points to the score, where N is the total number of letters in the word.
- Invalid words subtract $(N - 3)$ points from the score, where N is the total number of letters in the word.
- If the word is a duplicate, no points are subtracted and no points are added.
- If the word is too short, 1 point is subtracted from the total score (regardless of whether the word is 1, 2, or 3 letters long).

The time (three minutes in part 1 and fifteen minutes in part 2) ran out automatically. In the first part of the experiment, the subjects did not have the option of finishing the task earlier than three minutes. Once the time ran out, the subjects were presented with their score (in points) and the maximum possible score in a given puzzle. Since the program records the scores automatically, the subjects did not need to keep track of their winnings from round to round.⁴

Finally, changing the puzzle from round to round may prove problematic if the difficulty of the task varies dramatically. If the words were not similar, the results would not be directly comparable across treatments.⁵ Our main strategy is to carefully choose puzzles with the number of correct sub-words and the maximum number of points that is as similar as possible

⁴Gneezy, Niederle, and Rustichini (2003) leave it to the subjects to record the number of correctly solved mazes, because they use the internet for the experiment. Although the authors claim to have been monitoring the subjects, in order to ensure that they did not lie about their performance, it is possible that some of them were left unwatched at some points during the task. If men tend to lie and overestimate their performance more in the tournament than in the piece-rate treatment, then this would potentially distort the results. That is the reason for why we did not use online word puzzles for this experiment, but rather programmed our own version of the game.

⁵This problem does not arise to the same extent in studies that use mathematical tasks. For example, Gneezy, Niederle, and Rustichini (2003) simply restrict their pool of mazes to a certain level of difficulty. Because every word is different, it is difficult to find truly identical tasks.

in all the treatments. In particular, the range for the number of sub-words is 77-85, and the range for the maximum possible number of points is 132-137. We also check how difficult it is to find the sub-words in any given puzzle. In particular, we count the number of permutations of letters needed to arrive at any one sub-word (the complexity factor). In particular, the average complexity factors for each of the big words (i.e., puzzles) are 2.51 for *carriageway*, 2.60 for *ordination*, 2.39 for *memorable*, 2.12 for *allopathy*, and 2.45 for *equitable*. The practice word is *infusate*.⁶

2.2.2 The Procedure

The experiment was conducted at the Computer Lab for Experimental Research (CLER) at Harvard Business School (HBS). All sessions were held in February-March of 2008. The total number of individual participants was 76 people (16 groups of two men and two women and three groups of all men).⁷ Most subjects were students at Harvard University (undergraduates and graduates), although students from other Boston-area universities, such as MIT and Boston University, also participated. Because all these institutions are highly competitive in terms of academics, we do not expect women or men to be particularly intimidated by competition against the opposite gender.

CLER recruits subjects via an online registration procedure. Subjects first register for the CLER subject pool. Then, they can sign up for certain studies of their choosing. At any point, a subject could remove him- or herself from the study if he or she is unable to attend. When the subjects arrived at the lab, we separated them into groups of four. Even though gender was not emphasized at any point during the study and explicit communication was not allowed, the subjects were encouraged to look around to see the composition of their group.

Upon entering the lab, the subjects were first asked to read through and sign informed consent forms for non-biomedical research.⁸ Paper copies of the instructions⁹ were distributed

⁶Since the practice round is never used for any of the analysis, we choose an easier word (with 183 sub-words and 342 maximum number of points).

⁷We do not use the data generated by two subjects. One of the subjects (a woman) closed the puzzle program by accident and had to redo the task, which may have increased performance artificially. Another subject's data for the competition round are missing since they were mistakenly erased when the results were compiled for final payment.

⁸Copies of the informed consent forms are available upon request.

⁹Full copies of the instructions can be found in Appendix A.

to the participants prior to the beginning of the experiment. In the instructions, the subjects could answer several control questions in order to familiarize themselves with the payment schemes and the experimental procedures. All questions were answered in private by the experimenter. At the end of the experiment, each participant filled out a brief questionnaire.¹⁰ The questionnaire asked the subjects some standard demographic questions and inquired about their strategies and beliefs throughout the experiment. At the very end, each subject was paid in cash a show-up fee equal to 10 US \$ and his or her earnings over the course of the session. Final income of each subject was first given in points and then converted to US \$ at the different rates according to the payment scheme detailed below. Average income (including the show-up fee) was \$17.84 across all groups and maximum income (including the show-up fee) was \$46.05. The approximate average duration of the sessions was 1 hour 10 minutes.

2.2.3 High Time Pressure Treatments

Our first goal is to establish whether men and women exhibit different levels of performance under competition in verbal tasks. As a benchmark measure of performance in these tasks, we use a non-competitive piece-rate payment scheme. To test whether there is a gender-specific effect of competition on performance, we introduce a competitive treatment in the following round. Note that the expected payoff in this competitive treatment is set to be identical to the expected payoff in the noncompetitive treatment.¹¹ However, payment is now uncertain. In order to disentangle the effects of competition and uncertainty on gender differences in performance, we also conduct a “random winner” treatment. In this treatment, payment is uncertain, yet independent of the performance of others.

Piece Rate Treatment

In this benchmark (non-competitive) treatment, the subjects have three minutes to solve one word puzzle (puzzle word: *carriageway*). Each subject receives 10 cents for every point earned in this round. No winner is announced, and everyone earns income according to one’s own performance.

¹⁰A copy of the questionnaire is available in Appendix A.

¹¹The expected payoff is equal to $\frac{1}{4}(Y_1+Y_2+Y_3+Y_4)$, where Y is the number of points of each group member i .

Competitive Treatment

Once again, each subject is presented one word puzzle (puzzle word: *ordination*). However, the scoring is now different. The total score in this round is compared to the scores of the other three members in the group. The person with the highest score (“the winner”) in this round then receives 40 cents for every point earned. The other three members of the group receive 0 points. In case of a tie, the winner is determined randomly out of the top performers.

Random Winner Treatment

In this treatment, subjects also solve one word puzzle (puzzle word: *memorable*). As in the previous treatment, there is only one “winner.” However, “the winner” is chosen at random out of the 4 people in the group. This person receives 40 cents for every point earned. The other three members of the group receive 0 points.

2.2.4 Reduced Time Pressure Treatments

The second goal of this experiment is to understand the impact of time constraints on male and female performance with and without competition. For this purpose, we run a second experiment that closely resembles the above design, but differs from it along one dimension. In particular, the subjects now have ample amounts of time (five times the length of time in the previous experiment) to try to find as many sub-words within the larger word as possible. We now also give the subjects the opportunity to finish each treatment before the period of fifteen minutes allotted for each round elapses.

We conduct two treatments in this reduced time-pressure environment: noncompetitive (puzzle word: *allopathy*) and competitive (puzzle word: *equitable*). Because the design of these treatments is otherwise identical to that of the high-time-pressure treatments above, we can conduct parallel comparisons within the two piece-rate and the two tournament scenarios, as well as compare between the piece-rate and the tournament schemes under the lowered time constraint.

Table 2.1 summarizes the various treatments for this experiment.

Table 2.1.

Treatment Summary.

Payment Type	Time Pressure	
	High	Low
Practice	X	
Piece-Rate	X	X
Tournament	X	X
Random	X	

2.3 Data Analysis

2.3.1 Variables and Summary Statistics

In our analysis, the main dependent variable is *score* measured as the total number of points accumulated in any round.¹²

The main explanatory variables are dummy variables, *gender*, with 1 denoting a female subject, and *competition*, with 1 denoting a tournament payment scheme. We also include standard demographic control variables in our regressions. In particular, we control for *age*, whether the subject is a native English *language* speaker (with 1 denoting a non-native speaker), and whether the subject's field of study is broadly categorized into a mathematical concentration or a humanities-related *concentration* (with 1 denoting humanities). Finally, during the experiment we attempt to gauge whether the subjects' confidence changes across the different treatments. We measure *gender confidence* by asking each subject to report his or her belief about who would be better in these tasks on average, men or women (1 denotes the perception that women are better at the verbal task).

Table 2.2 provides descriptive statistics for all the experiments. First, note that women, achieve a slightly higher average score (16.9) than men (15.7) in the verbal task even in the piece-rate treatments (although the difference in those treatments is not statistically significant).

¹²Note that we could also create an adjusted score variable by dividing the score by the maximum possible number of points or by the total number of sub-words in the puzzle. However, because we have already chosen the puzzle words to have the closest possible number of subwords, all the results using the score and the adjusted score variables would be qualitatively identical.

Second, competition seems to have very different effects in low-time-pressure and high-time-pressure environments. Average performance of both men and women is enhanced by competition in the former (for men, the score increases from 14.8 in the piece-rate to 20.7 in the tournament, and for women, the score increases from 16.2 in the piece-rate to 27.5 in the tournament, on average). In a high-time-pressure treatment, on the other hand, average performance of both genders drops when competition is introduced (men's scores decline from 15.7 in the piece-rate to 13.2 in the tournament, and women's scores decline from 16.9 to 13.8 in the tournament, on average).

Third, the gender confidence variable confirms the findings of previous studies that suggest that women are stereotyped to be better at verbal tasks than men: 81.9% of male subjects and 90% of female subjects report that they think that women would be, on average, better than men in these puzzles. Thus, it is fair to say, that in our experiment there is a stereotype threat against men, rather than against women. Note that the piece-rate treatments below suggest that the perception that women are better at verbal tasks than men is likely false.

Table 2.2.
Descriptive Statistics.

	Mixed Groups			Men
	Men	Women	Total	Only
Mean Score in Piece-Rate (High Time Pressure)	15.7	16.9	16.3	17.8
Mean Score in Competition (High Time Pressure)	13.2	13.8	13.5	14.2
Mean Score in Random Winner (High Time Pressure)	15.3	15.7	15.5	16.5
Mean Score in Piece-Rate (Low Time Pressure)	14.8	16.2	15.5	17.6
Mean Score in Competition (Low Time Pressure)	20.7	27.5	24.0	23.7
Max Score in Piece-Rate (High Time Pressure)	28	34	34	30
Max Score in Competition (High Time Pressure)	24	29	29	25
Max Score in Piece Rate (Low Time Pressure)	30	30	30	29
Max Score in Competition (Low Time Pressure)	36	42	42	32
Max Score in Random Winner (High Time Pressure)	31	35	35	32
Mean Time Spent in Piece-Rate (Low Time Pressure, Secs)	839	792	815	773
Mean Time Spent in Competition (Low Time Pressure, Secs)	741	862	801	860
Mean Age (Years)	22.3	21.0	21.7	21.5
Gender Confidence (% think own gender is better at task)	28.1	90.0	—	36.4
Percent Studying Humanities (%)	30	32	31	33
Percent Native English Speakers (%)	87.1	90.0	88.5	66.7
Number of Subjects	32	30	62	12

2.3.2 Performance in High Time Pressure Environment

Figure 2.1 and Figure 2.2 present the number of points (score) achieved by men and women in the piece rate and in the tournament treatments, respectively. The height of the bars in both figures corresponds to the share of male and female participants, respectively, who achieved the score in a given range.

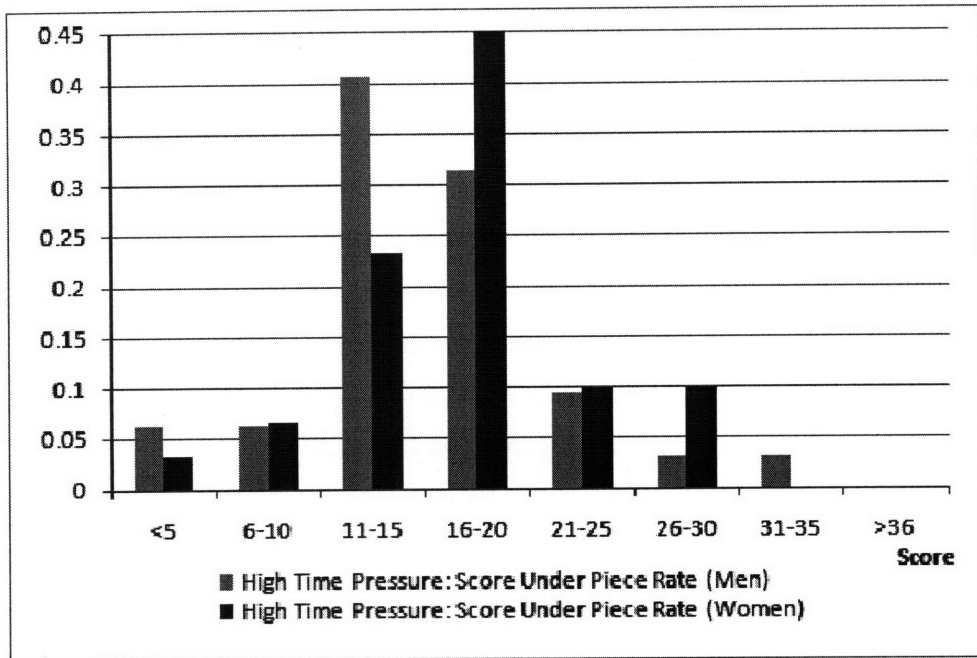


Figure 2.1: Score Under Piece Rate in High Time Pressure Environment

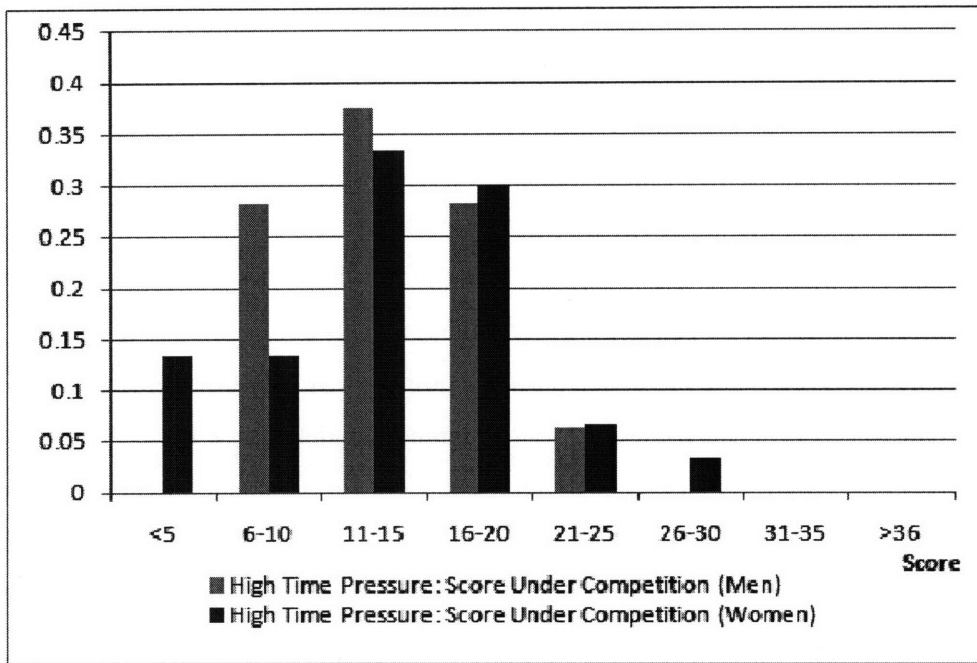


Figure 2.2: Score Under Competition in High Time Pressure Environment

The side-by-side comparison of the distributions of scores under piece rate and under mixed tournament (the competitive environment) leads to two observations. First, women and men

do not appear to differ in performance within the two treatments. Second, both genders seem to do slightly worse in the competitive environment. In order to test the first observation, we follow the previous literature and perform the two-sided Mann-Whitney U test, which compares distributions. The p -value of the Mann-Whitney test is 0.3476 in the piece-rate condition and 0.5723 in the tournament condition, which implies that there are no statistically significant differences in performance between men and women in either case.

Furthermore, in this high-pressure environment, competition seems to reduce the performance of men (with the p -value of the Mann-Whitney test of 0.073) as well as the performance of women (with the p -value of the Mann-Whitney test of 0.047).

Note that the tournament scheme differs from the piece rate condition in two ways: the payment depends on the performance of others and it is uncertain. For example, Dohmen and Falk (2006) attribute part of the gender difference in preferences for the competitive environment to differences in the degree of risk aversion. We therefore need to check that the results are truly driven by the effects of competition rather than risk aversion. In order to introduce uncertainty without competition, we run another treatment, where once again only one group member is paid, but that person is chosen at random. The results are similar in the random-winner treatment condition, where the p -value of the Mann-Whitney test is 0.7892.

We conclude that, unlike in mathematical tasks, in verbal tasks there is no significant difference between men and women when competition is involved. Competition does not boost performance of men. This allows us to reject the first hypothesis: competition alone does not seem to be the driving force behind gender differences in performance.

Next, we consider the second hypothesis proposed in the introduction: namely that the feelings of stereotype threat create gender differences under competition. Note that the above results for men could to be in line with the stereotype threat explanation: a man knows that in competition he is facing one person drawn from the same skill distribution as himself and two people who have higher verbal ability. Therefore, a man has a lower expectation of winning the tournament, which may hinder his performance. However, this reasoning would suggest that women should perform better under competition in verbal tasks, which we do not find. In order to resolve the question of the role of stereotype threat under competition, we compare the piece-rate and competition scores of men in a single-sex environment. We find that men do not

perform significantly better relative to piece-rate when competing only against other men (the p -value of the Mann-Whitney test is 0.148).¹³ Therefore, we can reject the second hypothesis that stereotype threat is a major cause of gender differences in a competitive environment. Figure 2.3 provides a visual summary of the results. It shows that men and women do not perform differently in any of the treatments, and that men in a single-sex environment also do not perform significantly better than men (or women) in the mixed tournament.

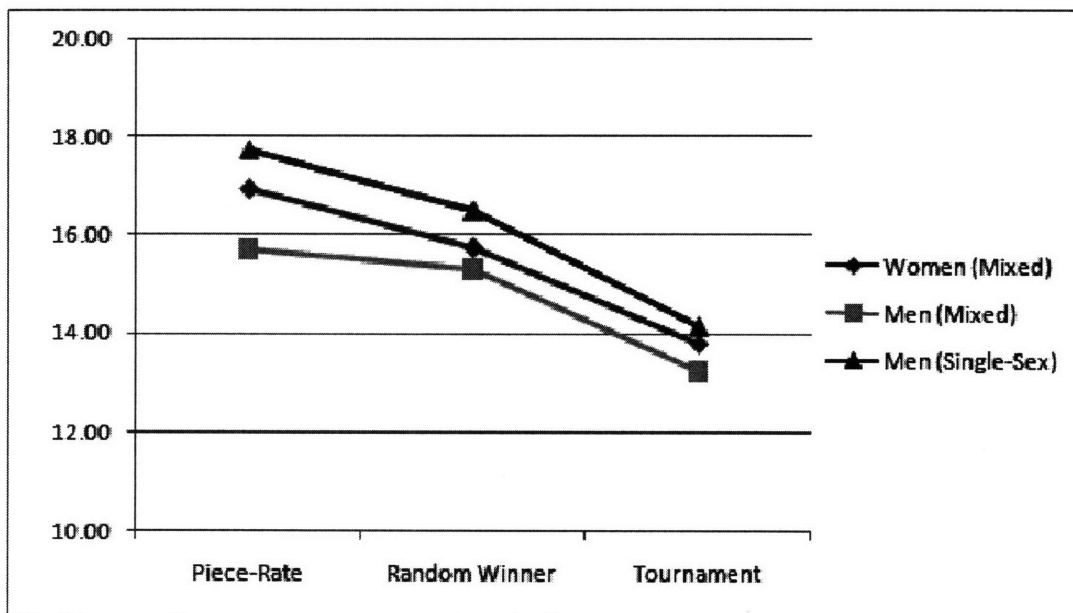


Figure 2.3: Average Score in the High-Time-Pressure Treatments

2.3.3 Performance in Low Time Pressure Environment

One potential reason behind women’s lack of improvement in performance under competition can be competition itself: women never compete.¹⁴ But another explanation is that time constraints and pressure affect men and women differently. In order to shed light on the latter question, we conduct an extended-time treatment, keeping the difficulty of the tasks and all other factors the same. In this low-time-pressure environment, we have two rounds: one with a piece-rate payment scheme and one with a tournament payment scheme. Figure 2.4 presents

¹³Note that so far we only have data for three single-sex groups. In the future, we plan to collect more data in this treatment in order to increase robustness of results.

¹⁴We know from previous studies (Gneezy, Niederle, and Rustichini 2003) that women do not always shy away from competition. For example, women do perform better when facing only other women in a tournament.

average scores of men and women for all the treatments in the experiment.

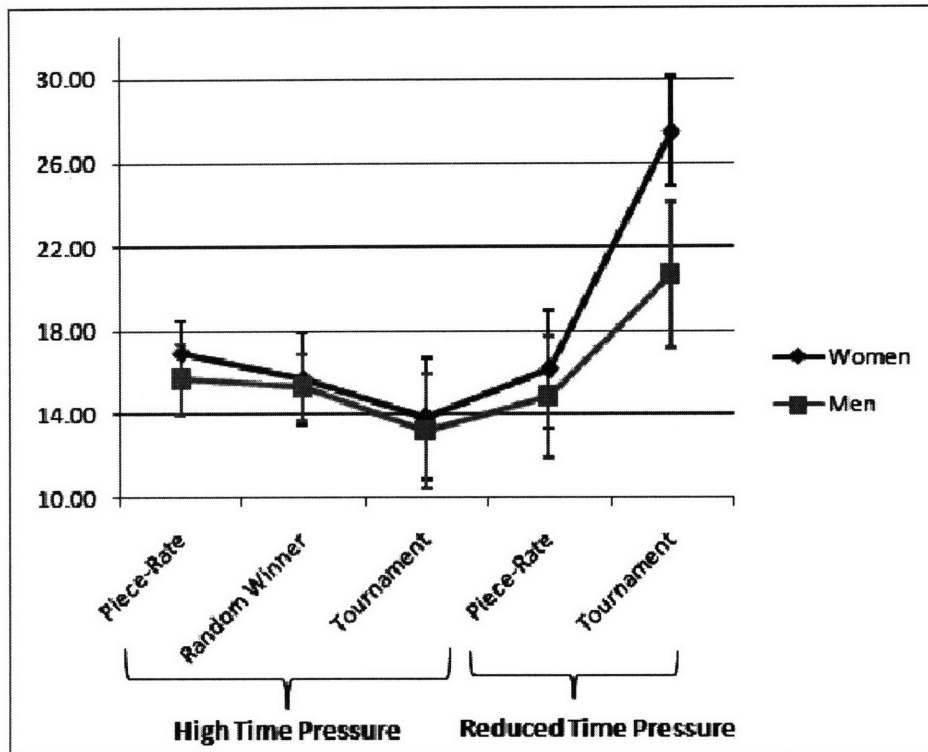


Figure 2.4: Average Score for All Treatments
(Confidence Intervals at the 90% Confidence Level)

Figure 2.5 and Figure 2.6 present the number of points (score) achieved by men and women under low time pressure in the piece rate and in the tournament treatment, respectively.

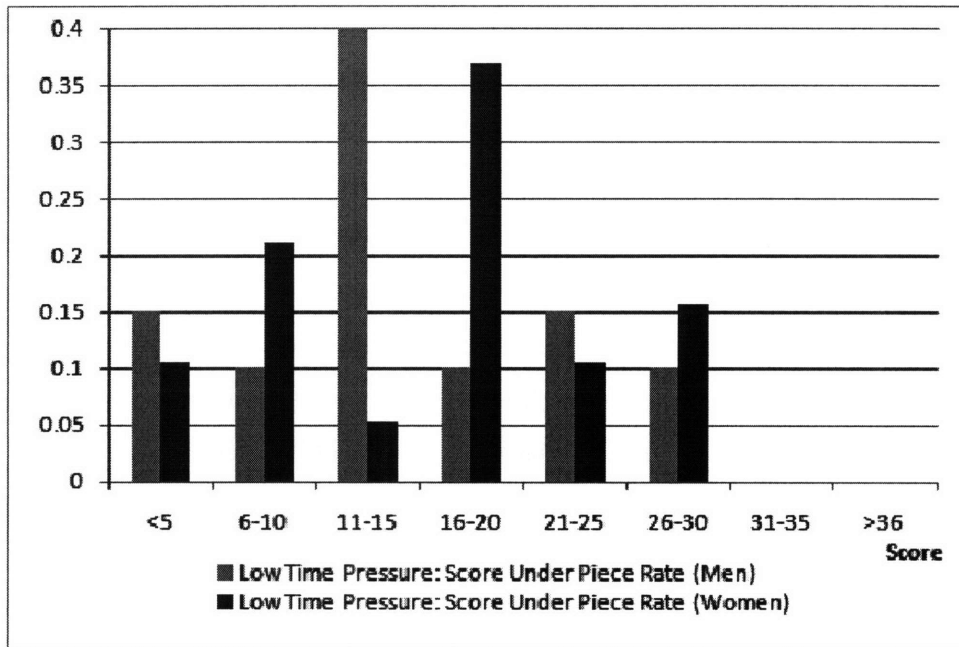


Figure 2.5: Score Under Piece Rate in Low Time Pressure Environment

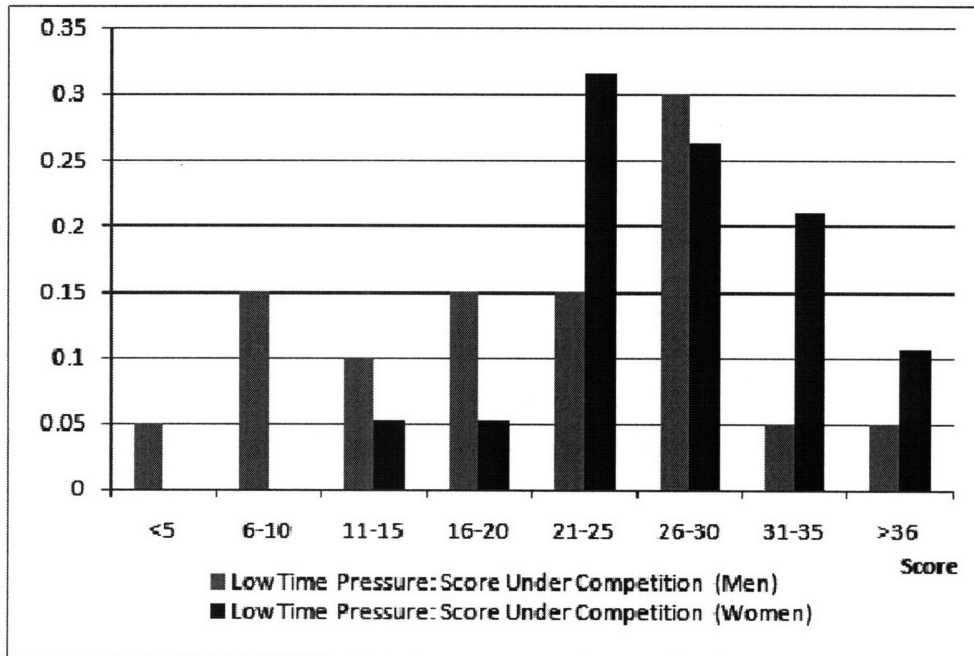


Figure 2.6: Score Under Competition in Low Time Pressure Environment

Comparing the distributions of scores for women and men in Figure 2.5 and Figure 2.6, we note that the availability of extra time significantly changes the outcomes. First, we observe

that extra time increases the performance in the competitive round for both, men and women. Comparing the piece-rate treatment to the tournament, men increase performance with the p -value of the Mann-Whitney test of 0.0422 which is statistically significant at the 5 percent confidence level. The women increase performance with the p -value of the Mann-Whitney test of 0.0001 which is statistically significant at the 1 percent confidence level. Second, we find that competition with extended time boosts female performance beyond the performance of men. In order to test whether the apparent gender difference under competition is statistically significant, we again perform the Mann-Whitney U Test in both treatments. The p -value of the Mann-Whitney test is 0.673 in the piece-rate condition which implies that there is no statistically significant difference between the scores of men and women here. However, the p -value is 0.043 in the low-pressure tournament condition: women perform significantly better than men in this competitive environment. ¹⁵

2.3.4 Women and Men: Quality vs. Quantity

In this section we look deeper into the mixed-group data in order to shed light on the origins of the differential performance of men and women in extended-time competitive environments. In particular, we focus on a measure of mistakes in various treatment conditions. This measure is simply the number of points lost due to entering invalid words.

First, we compare mistakes made under piece-rate and tournament in the high pressure environment. The Mann-Whitney U Test p -values of 0.5474 and 0.4292 in piece-rate and competition, respectively, suggest that the numbers of mistakes made by women and men do not differ significantly in either treatment when time pressure is relatively high. With reduced time pressure, the p -value in the piece rate condition is 0.9653, which once again implies that men and women make the same number of mistakes. However, in the low-time-pressure tournament, the p -value is 0.0275, which means that, at the 5 percent confidence level, we can

¹⁵Note that it is possible that some of the positive effect of competition on both genders stems from the usage of a different puzzle word in the two treatments. However, first of all, according to the complexity factor, the tournament word is actually slightly more difficult than the piece-rate word. Secondly, the effect of competition and reduced time pressure is strong enough in magnitude to dominate any of these word effects. In future work, we plan to test this hypothesis by switching the order of the words in piece-rate and competition rounds.

Most importantly, the significant gender gap that we find here arises independently of word choice and order, since all subjects face the same puzzle in a particular round.

reject the null hypothesis that men and women make the same number of mistakes.

Figure 2.7 documents the average number of mistakes made by men and women in all treatments.

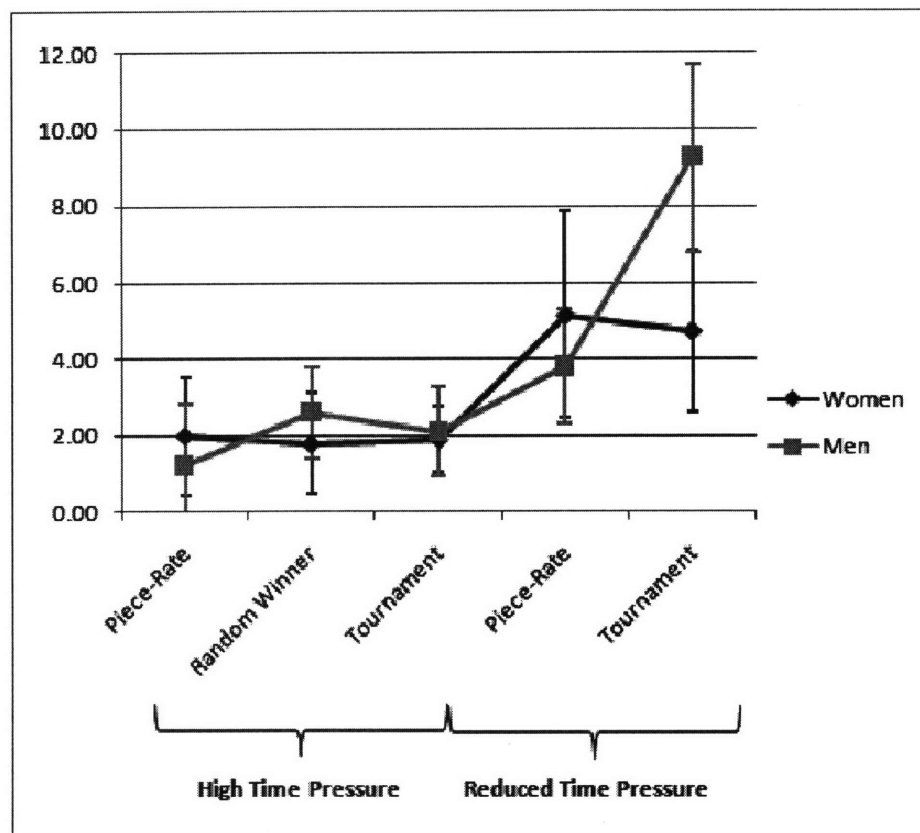


Figure 2.7: Average Mistakes (in terms of Points) for All Treatments
(Confidence Intervals at the 90% Confidence Level)

In the low pressure environment, competition slightly reduces the number of mistakes made by women from the average of 5.2 points to 4.7 points. However, the important factor contributing to the men falling behind the women in this treatment condition is that reduced time pressure increases the number of mistakes made by men significantly from the average of 3.8 points in the piece-rate condition to the average of 9.3 points in the tournament. This is an average increase of 5.5 points, which is a large effect, given that the mean number of points earned by men in the entire experiment is approximately 16 points. (The Mann-Whitney U Test rejects the null hypothesis that men do not make more mistakes significantly at the 1 percent confidence level with a p -value of 0.0084.)

The results suggest that while women might see the increase in time they can spend on the task as a way to increase the quality of their words, men seem to view the extended time only as a way to come up with more words. Table 2.3 documents the ratio of points lost due to invalid words (the quality measure) to the total number of points that the subject could have achieved if all the words were valid (the quantity measure). In the reduced-time-pressure tournament, men’s percentage of mistake points out of all possible points is almost double that of women (30.6 percent vs. 16.2 percent).

Table 2.3.
The Percentage of “Mistake” Points Relative to the Total Possible
Points Across Treatments (Quality-to-Quantity Ratio, %).

	Men	Women
High Time Pressure Piece-Rate	6.1	10.1
High Time Pressure Tournament	12.5	12.1
Low Time Pressure Piece-Rate	19.5	28.0
Low Time Pressure Piece-Rate	30.6	16.2

On average, in the reduced time-pressure environment, men can potentially achieve a slightly higher score than women (30.2 vs. 29.2) because they enter more words. Unfortunately for men, this increase in quantity seems to greatly reduce quality which results in an overall reduced performance.

Recall also that in the part of the experiment with reduced time pressure, the subjects are able to withdraw from the game at any point during the round by clicking a “finish” button. The instructions clearly explained to the participants that they may click this button after they had come up with all the sub-words they possibly can. Looking back at the descriptive statistics (Table 2.2), we note that women tend to spend more time (give up less) in the competitive round than in the piece-rate. The opposite is true for men: they seem to give up more in the competitive round. The effect is statistically significant at the 13% confidence level for women, according to the Mann-Whitney *U* Test and may also add to the explanation of the gender difference between men and women.

2.4 Conclusion

This thesis chapter uses controlled experiments to address two major issues in the literature regarding gender differences. First, we explore further a hypothesis put forth in previous studies that men outperform women in *all* competitive environments. Previous studies used tasks of mathematical nature, which are stereotyped to be better suited for men. In this chapter, we use verbal tasks, which are typically thought to favor women. We find no gender differences in performance in either non-competitive or competitive payment conditions. Neither gender shows improvement in performance when the payment scheme changes from piece-rate to tournament. These findings lead to a rejection of the first hypothesis.

Second, we test the hypothesis that stereotype threat affects both men and women in a symmetric fashion, favoring the gender for which the task is perceived to be most well-suited. Our study finds that with verbal tasks, men do not improve performance in a single-sex tournament relative to men in a mixed tournament. This evidence implies a rejection of the second hypothesis.

Next, we propose an alternative explanation for the lack of a positive effect of competition on women. This explanation relies on the differential effect of time pressure on the performance of men and women. Our main finding is that women perform significantly better than men in competitive verbal tasks once the time constraint is relaxed. An important factor contributing to this gender difference is that men and women respond differently to reduced time pressure. While women seem to use the extra time to increase the quality of their work (effectively reducing the number of mistakes), men use the time to increase the quantity, producing a higher volume of work, but also increasing the share of mistakes.

We conclude that competition is not the cause of the gender gap in performance per se. Its effect depends greatly on the type of the task at hand. In the workplace, women and men face competition not only in terms of their ability to perform jobs of mathematical nature, but also in terms of their verbal abilities, such as writing reports, creating presentations, and talking to clients. According to previous studies, competition favors men in the former. We find that, in the latter, competition favors women, at least when they are given ample time to complete the task. Using a more creative task in this study, such as word puzzles, also allows us to show that women tend to do a more thorough job than men when given the opportunity. For

example, with a task like solving mazes, quantity is all that matters, since an incorrectly solved maze is not penalized. With a verbal task, on the other hand, quality matters as well, which is what eventually separates women from men in competition.

The evidence documented in this thesis chapter suggests that gender inequality may be explained by the inherent differences in the responses of men and women to time pressure, rather than by societal conditioning, such as stereotype threat. This result implies that policies seeking to alleviate gender gaps in the workforce may want to subsidize a work culture that promotes a more relaxed atmosphere and lowered time pressure.

This study provides several directions for future research. In addition to the two hypotheses proposed and tested in this chapter, we consider a third hypothesis that stems from the rejection of the first two. In particular, the hypothesis claims that stereotype threat only affects women and not men in a competitive setting. Note that we observe that competition in verbal tasks does not seem to help women. Previous studies (for example, see Gneezy, Niederle, and Rustichini (2003)) suggest that women may do worse relative to men in competition precisely because they expect men to have superior mathematical abilities. If men were affected by stereotype threat in our study, then the finding that women do not improve performance under competition would lead to a rejection of the third hypothesis. However, in our study, stereotype threat does not have an effect on men, and therefore, we cannot test the third hypothesis with available data. In other words, we cannot distinguish that women do not do better under competition because they are affected by male presence even in verbal tasks or because they do not change their behavior for strategic reasons, anticipating that stereotype threat would not have an effect on men. However, the extended-time experimental framework provides an opportunity for testing this third hypothesis in the future. The new treatment that would provide such a test is one where men and women perform tasks of a mathematical nature in a reduced-time-pressure environment. Comparing performance of men and women in the piece-rate and tournament conditions in this treatment would allow for a direct comparison of the responses of men and women to stereotype threat.

Another important robustness check that has yet to be conducted involves running more sessions of the experiment and changing the order in which puzzle words appear from treatment to treatment. Currently, we control for puzzle-word order effects by choosing words of similar

difficulty and documenting their complexity factors. Conducting additional sessions would allow us to collect more data and make changing the order of puzzle words possible. Extra single-sex sessions are also required in order to add data to our currently small sample.

2.5 Appendix A: Experimental Documents

Instructions for the experiment (Part 1)

Thank you very much for participating in this experiment which involves solving word puzzles. Please read the following instructions very carefully. You will receive all the information you require for participation in the experiment. If you follow these instructions, you will have an opportunity to earn real money that will be paid to you, privately and in cash, at the end. If you do not understand something, please raise your hand and wait for the experimenter to come to your place and answer your question privately.

Communication between participants is absolutely forbidden during the experiment! Not obeying this rule will lead to immediate exclusion from the experiment and all payments.

Groups:

Throughout the experiment, you will be a member of a **group of 4 people** (two men and two women). The other three members of your group are sitting in the same row of desks as you.

Puzzles:

You will be solving **Word-in-a-Word** puzzles. You will have 3 minutes to find as many smaller sub-words that can be formed out of the letters of the big word as you possibly can. Everyone will be working on the same puzzle at the same time.

General rules:

- The words must be 4-letters long or longer
- Acceptable characters are letters A-Z only. Any other symbol like a number or another symbol will be automatically discarded
- Proper nouns are not allowed

- Plurals are allowed
- Each letter in the word can only be used once

Scoring rules:

- Valid words add $(N - 3)$ points to your score, where N is the total number of letters in the word
- Invalid words subtract $(N - 3)$ points from your score, where N is the total number of letters in the word
- If the word is a duplicate, no points are subtracted, no points are added
- If the word is too short, 1 point is subtracted from the total score (regardless of whether the word is 1, 2, or 3 letters long)

Example: Big word PERSUASIVELY. You enter: persuasive (+7), live (+1), live (0), lyve (-1), lyver (-2), sap (-1), as (-1). The total number of points is 3.

Rounds and Payment:

The first part of the experiment consists of **4 rounds**. In each round, you will have a chance to work on a different word puzzle. You will have 3 minutes to work on each puzzle (**3 minutes per puzzle**).

Round 1: In this round, you will be given 3 minutes to solve one word puzzle for **practice**. This puzzle will not add or subtract from your total score.

Round 2: In this round, you will be asked to solve one word puzzle. Each of you will receive **10 cents** for every point you earn in this round.

Round 3: In this round, you will again solve one word puzzle. However, the scoring will be different. Your total score in this round will be compared to the scores of the other three members in your group. The person with the highest score (“the winner”) in this round will receive **40 cents** for every point earned. The other three members of the group will receive 0

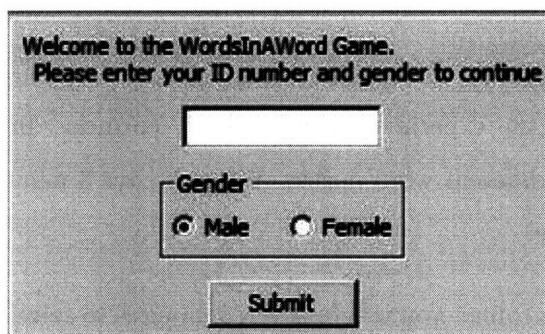
points. Note that, in case of a tie, the winner will be determined randomly out of the two top performers.

Round 4: In this round, you will also solve one word puzzle. As in Round 3, there will only be one “winner” in this round. However, “the winner” will be chosen at random out of the 4 people in the group and will receive **40** cents for every point earned. The other three members of the group will receive 0 points.

Exact Procedure

At the beginning of the experiment, you will receive an ID number. For example, members of the same group might receive ID numbers A1, A2, A3, and A4. Members of another group might receive ID numbers B1, B2, B3, and B4.

On the first screen you see, you will enter your ID number. You will also be prompted to enter your gender. Next, you will click “Submit.”

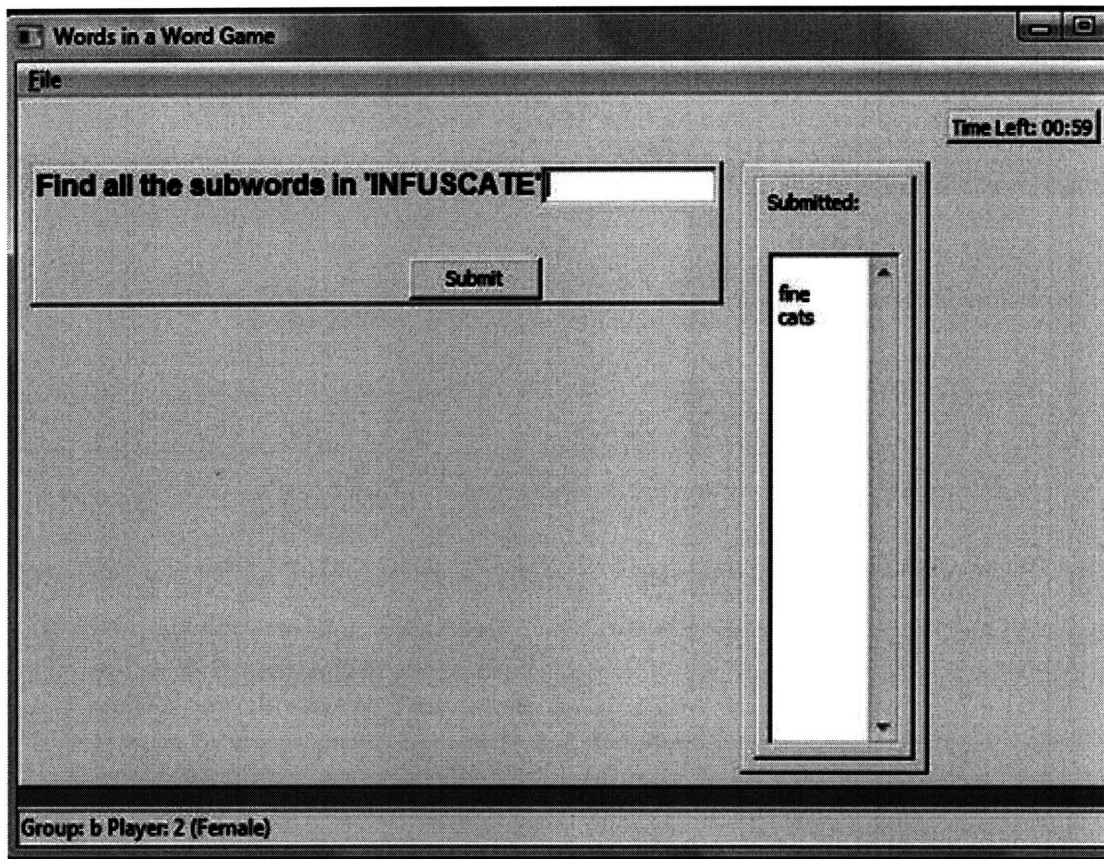


The screenshot shows a registration screen for the 'WordsInAWord Game'. At the top, it says 'Welcome to the WordsInAWord Game. Please enter your ID number and gender to continue'. Below this is a text input field for the ID number. Underneath is a 'Gender' section with two radio buttons: 'Male' (which is selected) and 'Female'. At the bottom of the form is a 'Submit' button.

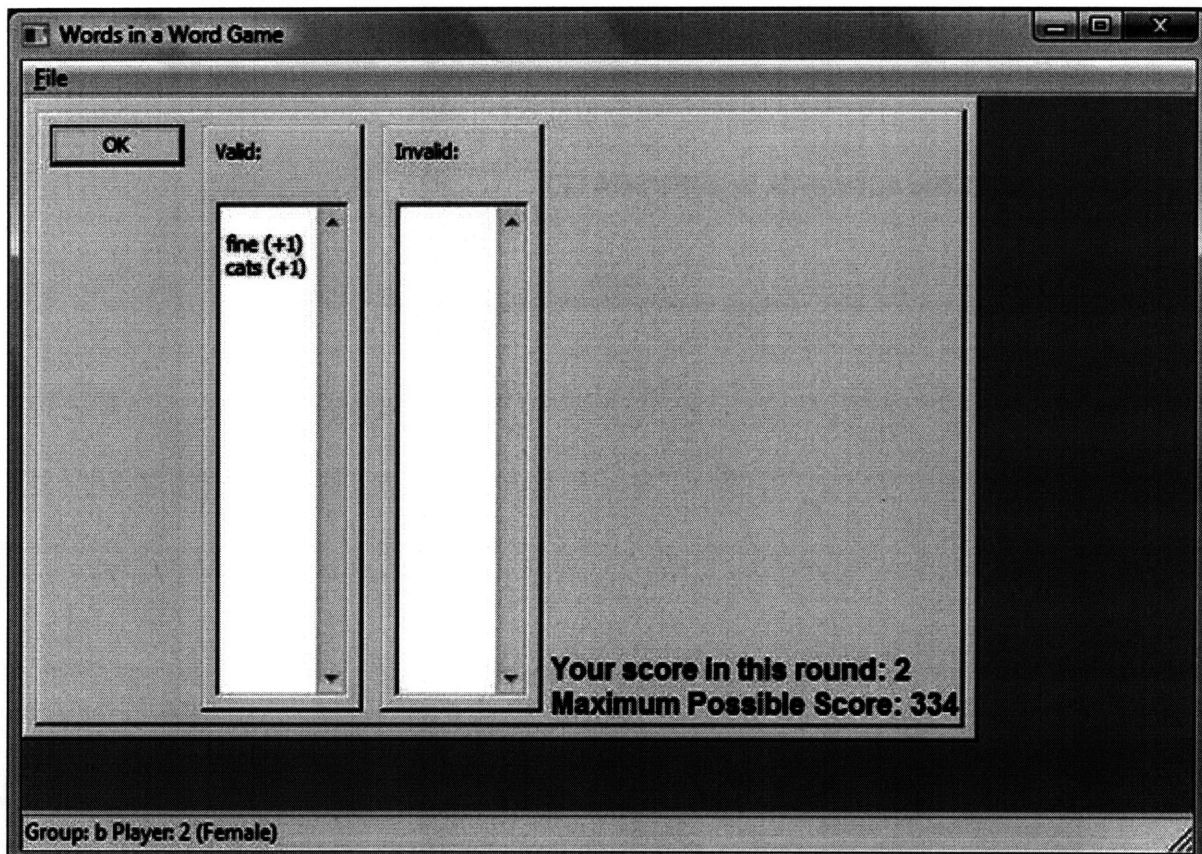
To start the first round, press “Start” on the screen that follows.

On the next screen, you will be presented with the practice puzzle (Round 1). Remember that you will have 3 minutes to come up with as many valid words as you can. After typing in your sub-words, you can either press the “Enter” key on the keyboard or click “Submit.” Make sure you check the spelling of your sub-words carefully, since you will NOT be able to go back

and edit them once they have been submitted.



The time will run out automatically. Once it does, you will see your score (the number of points you earned in this practice round) and the total possible number of points you could have earned in this puzzle. Please, click "OK" to proceed.



Please, be patient. You might need to wait 30 seconds to 2 minutes in between rounds!

Clicking “OK” will bring you to an information screen. Please, click the button labeled with “Proceed to next Round,” at which point the second round will start. The procedures for Rounds 2, 3, and 4 are identical to the procedure in Round 1.

You will receive your payment at the end of the experiment. Payment calculation for Part 1:

- Your income from Round 1 [= \$0]
- + Your income from Round 2
- + Your income from Round 3

+ Your income from Round 4

= Total income for Part 1

Control Questions

1. You are in Round 2. Your big word is CREATIVITY. You find words Reactivity, Race, Rat, Creative, and Crate.

- (a) Your score in points is _____ points. (Answer: 4 Points)
- (b) Your income made on this word puzzle is \$ _____ (or _____ cents)

2. You are in Round 3. Your score is 30 points. The other members of your group scored 14 points, 25 points, and 29 points.

- (a) Your income made on this word puzzle is \$ _____ (or _____ cents)
- (b) The income of the person with the score of 25 points is \$ _____ (or _____ cents)

3. You are in Round 3. Your score is 30 points. The other members of your group scored 14 points, 25 points, and 30 points.

- (a) Your income is \$ _____ (or _____ cents) with probability of $\frac{1}{2}$ and \$ _____ (or _____ cents) with probability of $\frac{1}{2}$.
- (b) The income of the person with the score of 25 points is \$ _____ (or _____ cents)

4. You are in Round 4. Your score is 30 points. The other members of your group scored 14 points, 25 points, and 39 points.

- (a) Your income is \$ _____ (or _____ cents) with probability of $\frac{1}{4}$ and \$ _____ (or _____ cents) with probability of $\frac{3}{4}$.

Instructions for the experiment (Part 2)

The second part of this experiment is identical to the first part with the exception that you will now have 15 minutes to solve each word puzzle. At any point during the 15 minutes, you will be able to finish the round by clicking the button labeled “Finish.” **Please, DO NOT CLICK “Finish”** until you are absolutely sure that you cannot come up with any more words.

Rounds and Payment:

Round 1: In this round, you will be asked to solve a word puzzle. You should have as much time as you need to come up with as many words as you can (up to 15 minutes). Each of you will receive 5 cents for every point you earn in this round.

Round 2: In this round, you will again get a new word puzzle, and you will again have as much time as you need to come up with as many words as you can (up to 15 minutes). However, the scoring will be different. Your total score in this round will be compared to the scores of the other three members in your group. The person with the highest score (“the winner”) in this round will receive 20 cents for every point earned. The other three members of the group will receive 0 points. Note that, in case of a tie, the winner will be determined randomly out of the two top performers.

Exact Procedure

The exact procedure is identical to the procedure described in the instructions for Part 1. The payment calculation for Part 2:

$$\begin{array}{r} + \quad \text{Your income from Round 1} \\ + \quad \text{Your income from Round 2} \\ \hline = \quad \text{Total income for Part 2} \end{array}$$

Total Income in the Experiment

$$\begin{array}{r} \text{Show-up fee [= \$10]} \\ + \quad \text{Total income for Part 1} \\ + \quad \text{Total income for 2} \\ \hline = \quad \text{Total income in the Experiment} \end{array}$$

Questionnaire

We ask you now to fill out a brief questionnaire. Please, answer as honestly and completely, as you possibly can, since your answers will help us tremendously!

1. What is your ID number (i.e. A1, B3, etc.)? _____
2. What is your gender? _____
3. What is your age? _____
4. Are you a native English speaker (i.e., is English your first language)? _____
5. Are you currently a student? _____

And if yes, what is your field of study? _____

6. In your opinion, who would be better in these tasks on average, men or women (circle one)?

Men

Women

7. How did changing the rules from non-competitive to competition change your effort and performance in the puzzles? Why?

8. Circle one answer:

- | | | |
|------------------|-------------------------------|-----------------------------------|
| Competition: | a. helped me. | b. hurt me |
| I tried more in: | a. non-competitive rounds | b. competitive rounds |
| I gave up: | a. never | b. more in non-competitive rounds |
| | c. more in competitive rounds | d. in both type of rounds |

THANK YOU VERY MUCH FOR YOUR PARTICIPATION IN THIS STUDY!!!

Chapter 3

Heroes of Our Time: Political Elites in Russia and the Effect of Their Background on Economic Outcomes

“To arouse <...> devotion and awe towards oneself – is that not the first sign, and the greatest triumph, of power?” - M. Lermontov, *A Hero of Our Time*

3.1 Introduction

The issues of development and growth have been on the mind of economists since economics first began to emerge as a social science. Why are some countries wealthier than others? Why do some countries grow faster than others? Similar questions can also be asked about regions within countries. These questions are still at the heart of economic research today.

Although there is a vast body of literature that seeks to answer the above questions, we will focus on only one of its branches. This branch of research examines the persistence of institutions, and how institutions can affect the development of an economy. Since all too often the term “institutions” is used rather loosely, note that when we talk about “good institutions,” we have in mind institutions that protect private property rights and are known as “democratic.” By “bad institutions,” one typically means extractive institutions or those that persist under a non-democratic regime.

Empirical studies suggest that institutions play an important role for modern-day development and that institutions can be persistent over time (Acemoglu, Johnson, and Robinson 2001, 2002). It is not too surprising that “good” institutions are persistent. It makes sense to introduce constraints on rulers, if such constraints will be in place in the future. However, more surprisingly, “bad institutions” appear at least as persistent as institutions of private property. There are many reasons for this persistence. First of all, institutional change may be extremely costly. It can potentially create political unrest and temporary economic instability. Thus, such a change would only occur if its benefits outweighed the costs. Secondly, the gains to a non-democratic strategy may depend on the size of the ruling elite. When the elite is small, each member would have a larger share of revenues, so the elite may have a greater incentive to promote extractive institutions. Moreover, the elites are more likely to opt for institutions of private property when they have good investment opportunities themselves (Bates 1981; Acemoglu and Robinson 2000). The elites that come to power with extractive or non-democratic institutions, however, often lack a comparative advantage in productive activities, and have less to gain from enforcing property rights, even though they are socially beneficial. Thirdly, institutional change may not be in the interest of those in power, because it may bring about new economic and social norms and thereby displace the elites from power. Therefore, these elites may try to prevent this change. It is this last aspect of persistence of institutions that will be of most interest to us as we look at political elites in one particular country.

We investigate Russia in the 1990s, which provides us with a natural experiment to test persistence of institutions, as well as their influence on various economic variables. We will show that old institutions in Russia continue to exist even after the transition period. In the late 1980s, Russia underwent a dramatic change in regime, from a centrist, Communist government to a “democracy.” The latter is written in quotes because the transition period is far from over. Privatization of property seemed to be just an excuse for powerful and corrupt elites to seize more power and resources at the expense of the citizens who still abide by the socialist paradigm of “the government is always right” (Goldman 2003). The electoral process in Russia is far from democratic, with the powerful groups rigging the polls and coercing people to vote one way or the other. Books have been, and will be, written about the nature of Russian pseudo-democracy by historians and political scientists. Economists are more interested in the

effects that this transition has had on the welfare of the country.

In this thesis chapter, we investigate the ways in which institutional change and persistence affects development. In particular, we examine whether differential speed of institutional change in different Russian regions has a effect on economic outcomes, measured by small business development. We choose to focus on small business development as our main economic outcome variable as it arguably the best proxy for the development of a free-market economic system in the region. In particular, we will look at the number of small businesses per 1000 people in the Russian regions, called oblasts, and ask whether “shadow institutions,” i.e., the old power structure that still prevails in certain areas, have any effects on small business development. The proxy for the old power structure or persistent Communist institutions is a leader dummy variable. If the governor of a particular region is someone who was an important Communist leader prior to democratization, then we dub him or her “old elite.” On the other hand, if the leader was never a party boss under Communism, we consider him or her a “new elite.” The hypothesis is the following: in an oblast governed by a new elite, the economic outcome should be statistically significantly different from (and presumably better than) the outcome in an oblast ruled by old elite. This hypothesis tests for the persistence of non-democratic institutions, proxied by the elite variable, in post-transition Russia and helps us shed light on the relationship between this persistence and economic welfare in the Russian regions.

Using panel-data regressions, we find a significant correlation between the identity of the region’s leader and the number of small businesses in the region. In particular, an “old elite” in power has a significant negative effect on small business development. A region with a new elite in power has, on average, 420 more small businesses than a region whose leader in an old elite. This finding is consistent with the cross-country literature on the effects of democracy on growth (Barro 1996).

The main problem here is one of clearly establishing causation. First, there is the issue of reverse causality which stems measuring institutional change contemporaneously with economic development. That is, it is possible that regions that develop more slowly are more likely to maintain a Communist old elite leader in power, and vice versa, regions that develop faster may be more likely to seek out new elites as leaders. Second, there is also the potential for omitted variable bias. While our hypothesis is that the power structure affects economic outcomes,

economic outcomes may also affect the elite structure in the region. The latter relationship is less obvious, yet reasonable. For example, most of the oblasts' governors were originally appointed by the first Russian president, Boris Yeltsin, and democratically elected in 1994 or later. Since Yeltsin himself was a high official in the Communist party, those he favored were most likely his party friends. In his appointments, he probably ranked the regions by GDP, resources, and other economic variables, and gave the ones with more potential to his friends. Thus, the economic outcomes may have served, at least in part, in determining whether the oblast' governor turned out to be old or new elite.

In order to resolve the issue of potential reverse causality, we forecast economic outcomes using variables that have been predetermined by the time of transition in 1991. These variables are the percent of total votes received by the Communist Party candidate, G. Zyuganov, in the 1996 presidential election, urbanization in year 1913, and the year when a certain region was annexed to the Russian Federation. We find that if the oblast's leader is "new elite," the number of small businesses increases by 2470, holding everything else constant. The issue of omitted variable bias is more difficult to address and will be discussed in the future research section of this chapter.

The rest of the chapter is organized as follows. The next section discusses the construction of the dataset, providing brief descriptions of our dependent variable, regressors, and potential instruments. Section 3.3 provides a look at relevant economic trends and descriptive statistics. Section 3.4 documents the results of our regressions which prove that the regional leader's old elite status has a negative effect on certain economic outcomes. Section 3.5 concludes.

3.2 Data

We begin the discussion of empirical results with an explanation of generation of data. The dataset used here was constructed using a multitude of sources, ranging from statistical year-books to biographies of leaders downloaded from the internet. The full list of sources can be found in the appendix to this chapter (section 3.6). In the following paragraphs, we will give a brief description of the variables.

The cross-section dimension of the dataset consists of 72 out of 89 Russian regions. We

exclude several regions that are small and are included in the data for the larger regions in all available sources.¹ We also omit those oblasts whose data are sparse and imprecise due to on-going ethnic conflict, such as Chechnya.² Finally, cities of Moscow and St. Petersburg, as well as the Moscow and Leningrad oblasts, are excluded because many data sources include the two cities in the data for the oblasts which creates an outlier problem and biases the results.

The potential outcome variables are, the number of small businesses per thousand people, available for years 1994 – 2000, weighted by population in the oblast, the unemployment rate, available for 1993 – 2000, and the logarithm of GDP, available for 1991 – 1998, weighted by the consumer price index in order to take care of purchasing power differences across regions. We choose to focus on small business development and on GDP. First, small business development is the best proxy for the emergence of a free-market economic system in the region. It is also a proxy for investment – a variable that we do not have access to for all Russian regions at this time. Secondly, institutional change may have a delayed on unemployment and potentially on GDP growth. In fact, it is actually not clear that we should expect a positive effect on employment in a region that gets a new elite as a leader, at least at first. On the other hand, the “new elite” effect on small businesses should be direct and more immediate. Note also that it would be ideal to look at outcomes before and after the transition in order to get a complete picture of the effect of regime change on the regional economies. However, this proves to be a rather difficult task when Russian data are concerned. For example, since all businesses were owned by the Communist government prior to democratization, one can safely assume that the number of small private businesses in the Soviet Union was zero. Similarly, since unemployment was considered a vice unbecoming of a “flourishing socialist society,” statistical agencies prior to the transition recorded unemployment as being zero. Though probably untrue, this prevents any attempts of comparing unemployment figures before and after 1989.

The main regressor is a dummy variable labeled “elite,” which was generated by reading biographies of current and previous leaders of a region and assessing their position prior to year 1991. If the leader did not hold a relatively high position in the Communist Party prior

¹These regions are Agin-Buryatskiy Autonomous Okrug (AO), Chukotskiy AO, Evenkiy AO, Khanty-Mansiyskiy AO, Komi-Permyatskiy AO, Koryakskiy AO, Nenetskiy AO, Taymyrskiy AO, Ust'-Ordynskiy Buryatskiy AO, Yamalo-Nenetskiy AO, and Yevreyskaya AO.

²Other such regions are Ingushskaya Respublika, Respublika Dagestan, and Respublika Severnaya Osetiya.

to democratization, s/he is a “new elite” and is assigned a value of 1. An example of a high regional political position is first secretary of obispolkom (i.e. regional executive committee of the party). Since almost everyone was a member of the party before 1991, we assume an arbitrary, but not unreasonable, criterion for the cutoff “high position” in the party to be secretary of such party executive committee. If the leader of a region changed over the period 1991-2001, we look at the successor beginning with the year of his/her coming to office and assign the status of old or new elite according to the above criteria.

In many of the regressions, we will use several control variables. These are: population density in the oblast; an index of natural resource potential; distance from Moscow in kilometers; current party affiliation of the regional leader, a variable that assigns numbers on the scale of one through five to the governor, with one indicating a democratic leader and five indicating a Communist leader; average temperature in the month of January which proxies for severity of the region’s climate; percent of heavy industry in the oblast; percent of the region’s total population who are of Russian ethnicity; and a dummy variable for whether or not the region was temporarily occupied by the invading German army during the Second World War, with 1 signifying occupancy.

In order to correct the endogeneity problem, we must find a valid instrument for our elite variable. To address the issue of reverse causality, we consider variables that have been pre-determined by the time of transition in 1991. Note that the data for the Communist period is unavailable or highly unreliable due to the party’s efforts to keep the actual state of the economy a secret. Thus, we need to come up with alternatives. The first such variable the percent of total votes received by the Communist Party candidate, G. Zyuganov, in the 1996 presidential election. This was the first democratic election to feature a Communist nominee, since the party reemerged in Russia in 1993. Assuming that the governor does not have direct influence on the ways people vote in his/her region, this variable helps us distinguish which regions are pro-Communist and which are not. It is reasonable to assume that the regions that showed greater support for the Communist candidate in 1996 were also more “red” before democratization.

Another variable we consider is the year a particular oblast became part of the Russian Empire. Note that many of the regions were part of the Russian Empire since before any written

historical accounts were made. These oblasts are given a year when it was first mentioned in the archives or when its most prominent city was founded. Furthermore, annexation can mean one of the following: founding due to exploration (most regions in the Far East, for instance) or due to imperialistic expansion. The final potential instrument is the percent of the population in the region living in an urban setting before the revolution of 1917, in particular in year 1913. The difficulty in obtaining this variable lies in the fact that the oblast borders changed dramatically after 1917, and therefore one must translate the data from the old to the new borders. In this thesis chapter, we compare the pre-Revolution and the current regions by overlaying transparencies of large-scale maps. Making the simplifying assumption that there was no drastic change in urban centers over the course of the twentieth century, we scale the urbanization variable by the ratio of new area and the old area of the region.³ Note that because the latter two instruments are far back in time, we are likely to run into weak instruments problems.

3.3 A Brief Look at Trends and Descriptive Statistics

Before we begin a more in-depth econometric analysis, we point out some of its trends and features. First, consider small business development – our main outcome variable. Looking at the data for two regions, Stavropolskiy Krai and Respublika Mariy-El, provided in Figure 3.1, we see that while in the former the number of small businesses per thousand people started out relatively high, at around 5.1 businesses, it decreased to 2.67 in 2000. The trend is opposite Marii-El, where the number of small businesses per thousand people increased from 2.88 in 1994 to 5.4 in 2000.

³For a more precise translation, one can use for example a mapping computer system, such as GIS.

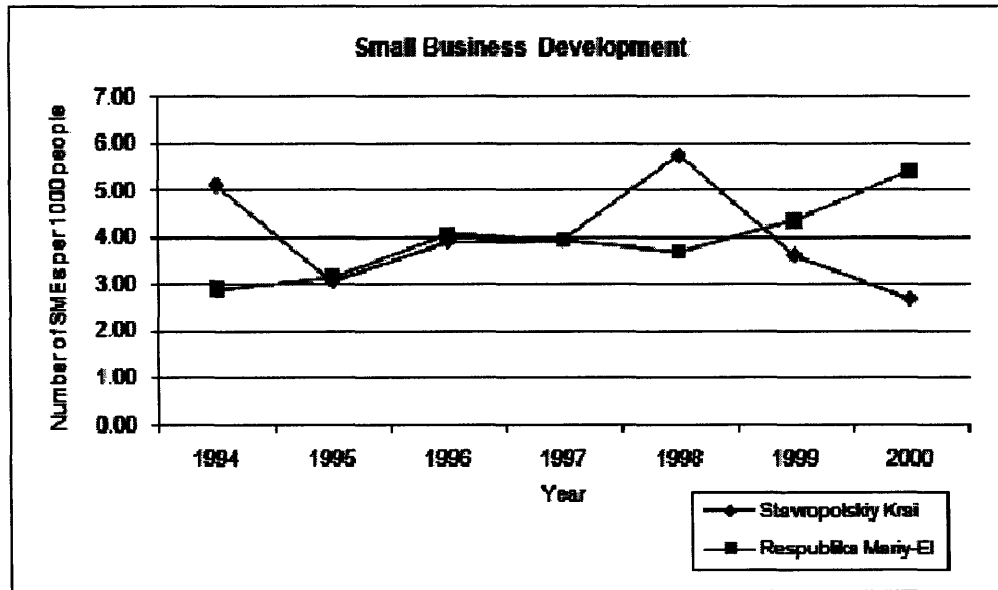


Figure 3.1: Number of Small Bussinesses per 1000 People Across Time

Note that the overall trend of small businesses was downward over the period, as shown in Table 3.1, although the decrease was not very dramatic. What might have caused the difference between these two regions? There are many factors that can potentially contribute to the explanation, but the one we are most interested in is the relative political power of the two governors prior to democratization. The governor of Stavropolskiy Krai was an “old elite” over the entire period. By contrast, the governor of Respublika Mariy-El who ruled until 1996 was an “old elite”, while his successor who came to power in 1996 was “new elite.”

Table 3.1.

Small Bussiness Descriptive Statistics

Number of Small Bussiness per 1000			
Year	Average	Minimum	Maximum
1994	5.07	2.79	10.86
1995	4.69	2.19	10.59
1996	4.16	1.56	10.32
1997	4.08	1.64	10.79
1998	4.22	1.55	9.76
1999	4.41	1.74	13.01
2000	4.36	2.13	12.13

If we consider the unemployment figures for the same two regions, presented in Figure 3.2, we no longer get the same story. Unemployment in both areas grew steadily from 1993 to 1998. Then it declined in Stavropolskiy Krai, while staying on the upward trend in Mariy-El. Note that unemployment was on the rise over the years 1993-2000 (Figure 3.3).

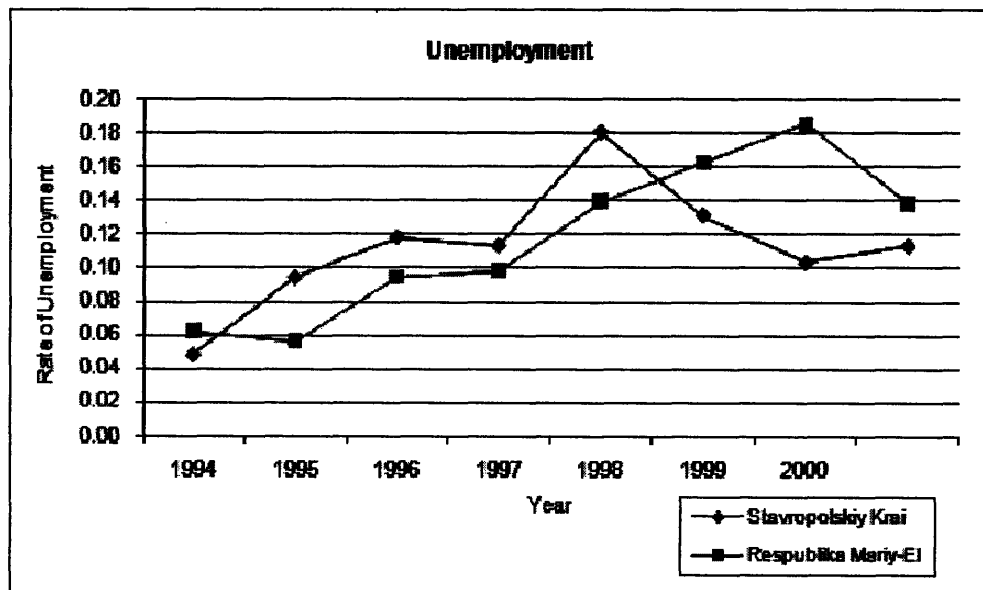


Figure 3.2: Unemployment Rate in Stavropolskiy Krai and Res. Mariy-El Across Time

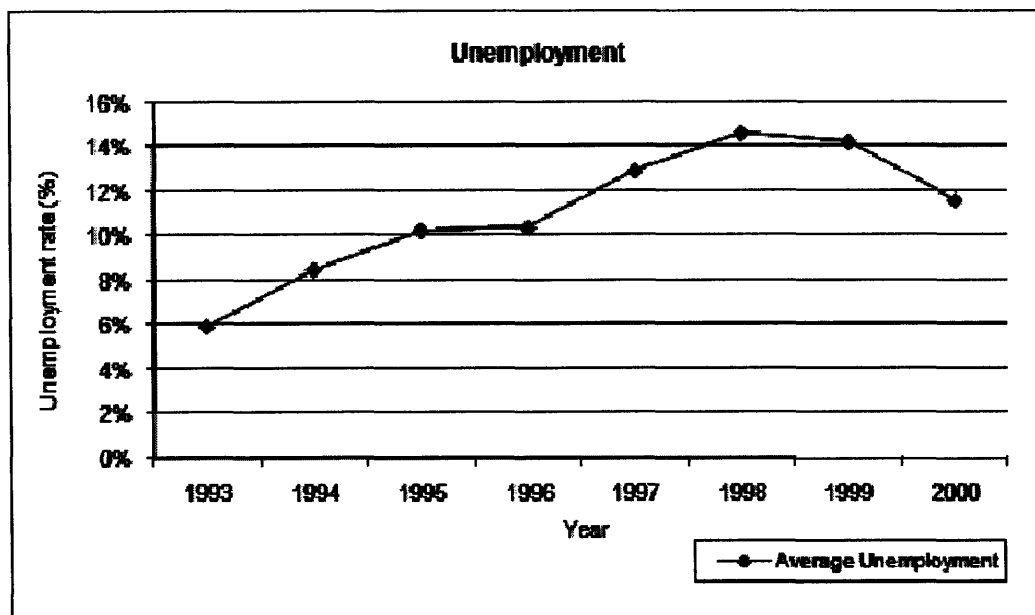


Figure 3.3: Unemployment Rate Across Time

We can also look at various relationships by year instead of by oblast. Figure 3.4 plots the number of the small businesses per thousand people in 2000 against the average elite variable, which is constructed by taking an arithmetic average over time of the elite variable in each region.

where SME_{i2000} is the number of small business per 1000 people in year 2000 in region i , $AveElite$ is the average new elite dummy over the period 1991 – 2001, X_i is the vector of various controls, and ε_i is the random error term.

Table 3.2.
Simple OLS Regressions Using Cross-Section Data

Dependent Variables	Number of Small Businesses per 1000 People in Year 2000				Change in Unemp. Rate 1993-2000	
	Basic OLS			Robust SE	Basic OLS	
	(1)	(2)	(3)	(4)	(5)	(6)
Average Elite	1.85**	1.69**	1.32*	1.32*	-0.006	0.0004
1991-2000	(0.61)	(0.60)	(0.69)	(0.74)	(0.01)	(0.01)
Distance from Moscow		0.02** (0.008)	0.02* (0.01)	0.02* (0.01)		0.0004** (0.0002)
Natural Resource			-0.46	-0.46		0.01
Potential Index			(0.53)	(0.59)		(0.01)
Percent Russian of Total Population			0.81 (1.30)	0.81 (1.03)		-0.06** (0.02)
Percent Heavy Industry			1.53 (1.83)	1.53 (1.94)		-0.04* (0.03)
Average Temp. in January			-0.01 (0.05)	-0.01 (0.07)		0.00001 (0.0007)
Average Party Affiliation 1991-2000			-0.27 (0.26)	-0.27 (0.22)		0.002 (0.004)
Nazi Dummy			-0.05 (0.63)	-0.05 (0.60)		0.009 (0.009)
R ²	0.12	0.18	0.21	0.21		0.34
N	72	72	72	72	72	72

Standard errors in parentheses. Significance levels: *10%, **5%, ***1%.

The results tell us that if the leader belonged to the new elite category in a particular region (i.e., the average over 9 years for the elite dummy variable is close to one), then that region had more small businesses per thousand people in year 2000, controlling for various regional conditions. The effect is statistically significant at the 10 percent confidence level. Note that the only statistically significant control variable is distance from Moscow in this specification. As distance from Moscow increases by one hundred kilometers, the number of small businesses per thousand people in the oblast increases by 0.02. Note that the magnitude of the coefficient is very small, however, and therefore we can conclude that distance only has a very small effect on the number of small businesses in a region.

As a sensitivity specification, we run the same regression with robust standard errors to account for potential heteroskedasticity issues. The results are unaffected by this procedure. The last two columns of Table 3.2 provide OLS regression results of the effects of the same right-hand side variables as before on the change in unemployment rate 1993-2000. The elite dummy variable has no statistically significant effects on unemployment. Distance from Moscow has a statistically significant positive effect on the change in unemployment, although the magnitude of the coefficient is very small once again. The coefficient on the percent of people of Russian nationality in the total regional population has the expected sign and is statistically significant at the 5 percent confidence level: in a region with one percent more Russians, the increase in unemployment over the period 1993 to 2000 is smaller by 0.06 percent. Finally, unemployment seems to be affected by percentage of oblasts' industry that is allocated to heavy machinery production. In a region with one percent higher heavy industry allocation, the increase in unemployment over the period 1993 to 2000 is smaller by 0.04 percent. This coefficient is statistically significant at the 10 percent confidence level.

3.4.2 Simple Panel Regressions

The preceding analysis was based on cross-section data and focused on the number of small businesses in a specific year, using average values for the elite variable. This preliminary analysis allowed us to see certain trends in the data, but it did not take advantage of the time dimension of the panel. Table 3.3 presents results of simple OLS regressions using this richer structure of the data.

Table 3.3.
OLS and FGLS Regressions Using Panel Data

Dependent Variables	Number of Small Businesses per 1000 People in Year 2000				Unemp.	Log
	Basic OLS		FGLS		Rate	GDP
	(1)	(2)	(3)	(4)	(5)	(6)
Elite Dummy	0.69*** (0.18)	0.42*** (0.17)	0.69*** (0.16)	0.42*** (0.17)	-0.002 (0.003)	0.09 (0.06)
Distance from Moscow		0.02*** (0.003)		0.02*** (0.004)	0.04*** (0.01)	0.03* (0.01)
Natural Resource Potential Index		-0.22 (0.24)		-0.22 (0.22)	-0.01** (0.005)	0.12 (0.08)
Percent Russian of Total Population		0.34 (0.33)		0.34 (0.43)	-0.07*** (0.01)	0.31** (0.15)
Percent Heavy Industry		0.16 (0.62)		0.16 (0.65)	-0.05*** (0.01)	-0.44* (0.23)
Average Temp. in January		0.02 (0.02)		0.02 (0.02)	0.001*** (0.0004)	-0.01* (0.006)
Party Affiliation		-0.11* (0.06)		-0.11* (0.06)	0.001 (0.001)	
Nazi Dummy		-0.28 (0.21)		-0.28 (0.23)	-0.001** (0.004)	0.06 (0.07)
Population		-0.03*** (0.01)		-0.03*** (0.01)	-0.04*** (0.02)	0.01*** (0.002)
R ²	0.03	0.12				0.10

Robust standard errors in parentheses. Significance levels: *10%, **5%, ***1%.

Columns 1 and 2 of Table 3.3 show the effects of the elite variable on the number of small businesses over the period 1994-2000. In all of the cases, there is a statistically significant correlation between the leader's status prior to democratization and small business development.

Also, once again, distance has a statistically significant and positive impact on small businesses, although it is still small in magnitude. In order to perform a specification test, we run a cross-sectional time-series FGLS regression and compare the ordinary and generalized least squares estimates using a Hausman test. With a chi-squared value of nearly zero, we fail to reject the null hypothesis that both estimators are consistent. We also see that the GLS regression has not changed the coefficients dramatically. In the rest of the chapter, we continue to use our OLS estimator as the baseline.

The last two columns of Table 3.3 show that there is no direct statistically significant correlation between the elite variable and the rate of unemployment and log GDP. However, the OLS regressions give us a reduced-form look at the relationship between our variables of interest. Due to endogeneity between political status and economic outcomes, we seek an instrument to get a consistent estimate of the coefficient on the elite variable. As an instrument, we need a variable that determines (i.e., is correlated with) the elite dummy, but is uncorrelated with the error term in the regression. Our main candidates for an instrument are variables that had been predetermined prior to the transition year 1991: the share of votes going to the Communist party, urbanization in 1913, and the year of the region's annexation. Note that the exclusion restriction implied by the instrumental variables strategy is that, conditional on the other controls, the potential instruments have no effect on small businesses today, other than their effects through institutional development.

3.4.3 Determinants of the “New Elite” Dummy

Table 3.4 presents the determinants of the elite dummy. Note that these are chosen because they had presumably been predetermined prior to the year 1991. Since we have a binary dependent variable, we use logit regressions instead of simple OLS. The following is the general equation for these regressions

$$\Pr[Elite_{it} = 1|X_{it}] = F[\mu + Z'_{it}\delta + X'_{it}\phi] \quad , \quad (3.2)$$

where $Elite_{it}$ is the new elite dummy in region i and in year t , Z_{it} is the vector of predetermined variables, X_{it} is the vector of other covariates, and F is the distribution function of the logistic (Logit) distribution.

Table 3.4.

Potential Determinants of the Elite Variable Using Logit and Probit Regressions

Dependent Variables	Elite Dummy			
	(1)	(2)	(3)	(4)
<i>Potential Instruments:</i>				
Com. Party Votes	-3.15***			-2.85***
in '96 Pres. Election	(1.02)			(1.09)
Urban Population		-6.75***		-6.66***
in 1913		(1.80)		(2.01)
Year Annexed to			-0.13	-0.04
Russian Empire/USSR			(0.10)	(0.1)
<i>Controls:</i>				
Distance from	0.03***	0.05***	0.04***	0.05***
Moscow	(0.007)	(0.01)	(0.01)	(0.01)
Natural Resource	-0.76**	-0.69**	-0.66**	-0.73**
Potential Index	(0.32)	(0.34)	(0.32)	(0.34)
Percent Russian	2.45***	2.85***	2.57***	2.53***
of Total Population	(0.67)	(0.69)	(0.69)	(0.69)
Percent Heavy	1.02	0.98	0.42	1.47*
Industry	(0.87)	(0.88)	(0.87)	(0.90)
Average Temp.	0.06***	0.11***	0.08***	0.11***
in January	(0.02)	(0.02)	(0.02)	(0.03)
Party Affiliation	-0.40***	-0.47***	-0.47***	-0.40***
	(0.10)	(0.10)	(0.10)	(0.10)
Nazi Dummy	0.14	-0.13	-0.08	-0.04
	(0.32)	(0.32)	(0.34)	(0.35)
Population	0.002	0.001	0.003	0.005
	(0.007)	(0.007)	(0.007)	(0.007)
R ²	0.13	0.13	0.12	0.15

Robust standard errors in parentheses. Significance levels: *10%, **5%, ***1%.

The first two columns of Table 3.4 present the effects of the share of votes Zyuganov received in the 1996 presidential election on the elite variable. Though we cannot directly interpret the coefficient in the case of a logit regression, we see that the election had a significant effect on the elite variable at a 5 percent confidence level. The negative relationship is expected: in a very pro-Communist region, the leader is more likely to have been a powerful party boss prior to democratization (with the elite value of zero). Distance from Moscow is statistically significant at the 1 percent confidence level. One possible interpretation of the positive relationship is the following. Since the far-away regions are arguably less desirable than the regions closer to the capital, Yeltsin appointed his friends, members of “old elite,” to govern the near-by regions, while the remote regions fell into the hands of “new elites.” Similar logic can be used to explain the negative relationship between elite and the natural resource index. The negative coefficient on the party variable is also intuitive. A reformist governor (with a low value for the party affiliation variable), is not likely to have been a powerful leader in the Communist party, and hence his/her elite value is more likely to be 1 rather than 0. Regions that have a higher average temperature in January have leaders who tend to be “new elite.” Finally, it is rather surprising that there is a positive statistically significant relationship between the percentage of Russians in an oblast and the elite value.

The third column provides results of a logit regression using urbanization in 1913 as the main independent variable. Regions that were more urbanized in 1913 tend to have leaders who are “old elite” today. This relationship is statistically significant at the one percent confidence level. The coefficients on the control variables can be interpreted as in the previous paragraph. Finally, the effect of the year a particular region was annexed to the Russian Empire on the elite dummy variable is negative and statistically significant at the 10 percent confidence level. Regions that were annexed earlier are more likely to have a “new elite” leader. This might seem counterintuitive, but if we look at the dprobit results, we see that the probability is very small in magnitude. Thus, the relationship between the year of annexation and the elite status is virtually nonexistent. The last two columns of Table 3.4 are dedicated to documenting the effect of all three of the above predetermined variables on the elite dummy.

3.4.4 Regressions Using Predicted Values as Instruments

Finally, we can look at the results of our main analysis: the IV regressions using the three aforementioned instruments. Since the variable that needs to be instrumented is binary, we cannot perform the standard IV procedure that uses the instruments directly. Instead, we use the methodology similar to that in a paper by Dubin and McFadden (1984). In particular, we run the logit regressions discussed earlier (equation 3.2) and obtain predicted values. Then, we use these values as instruments for the elite variable in our IV regressions. A further important specification issue is one of usage of random versus fixed effects in our panel IV regression. First of all, based on a Breusch and Pagan Lagrange Multiplier test for random effects with a chi-squared value of 590.47 which greatly exceeds the critical value of approximately 3, we conclude that there are individual effects in the data, and the random effects model is appropriate. Secondly, a Hausman specification test, which returns a chi-squared value close to zero, suggests that after weighing the two alternatives, the random effects model should be used.

Tables 3.5 and 3.6 present the main results of the study. Table 3.5 presents the effect of the leader's status on small business development, while Table 3.6 presents the effect of the elite dummy on log GDP adjusted for CPI. As we have seen from our previous discussion, we can focus mainly on the specification that uses the predicted values from the logit regression of the share of Communist votes in the 1996 election on the elite dummy as an instrument (first two columns of each table), since the urbanization and annexation variables suffer from numerous problems, the greatest of which is weak instruments. This is reinforced by the fact that the coefficients on the elite variable in the rest of the columns are quite unstable, while the coefficients in the first two columns seem to make sense.

Table 3.5-Panel A.
 Regressions of Number of Small Businesses per 1000 People
 on the Elite Dummy Using Various Instruments: 2SLS

Dependent Variables	Number of Small Businesses Per 1000 People					
	Predicted Value		Predicted Value		Predicted Value Using	
	Using Com Party		Using Urbanization		Com Party Votes	
	Votes in 1996		in 1913		& Urbanization 1913	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Two-Stage Least Squares</i>						
Predicted Elite Dummy	2.80**	2.47**	2.56*	0.58	2.67**	2.25
	(1.45)	(1.20)	(1.51)	(2.44)	(1.27)	(1.75)
Distance from		0.0001		0.0003		0.0002
Moscow		(0.0001)		(0.0002)		(0.0002)
Natural Resource		0.07		-0.15		0.07
Potential Index		(0.38)		(0.50)		(0.62)
Percent Russian		-0.61		0.21		-0.49
of Total Population		(0.87)		(1.30)		(0.62)
Percent Heavy		0.13		0.13		0.17
Industry		(1.08)		(1.20)		(1.75)
Average Temp.		-0.01		0.02		-0.003
in January		(0.03)		(0.04)		(0.05)
Nazi Dummy		-0.21		-0.33		-0.22
		(0.38)		(0.44)		(0.61)
Population		0.02*		0.03*		0.02
		(0.01)		(0.02)		(0.02)

Robust standard errors in parentheses. Significance levels: *10%, **5%, ***1%.

Table 3.5 - Panel B.

First Stage for the Elite Dummy Variable

Dependent Variables	Number of Small Businesses Per 1000 People					
	Predicted Value		Predicted Value		Predicted Value Using	
	Using Com Party		Using Urbanization		Com Party Votes	
	Votes in 1996		in 1913		& Urbanization 1913	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel B: First Stage for the Elite Dummy Variable</i>						
Instrument	1.00***	1.00***	0.95***	0.78	0.97***	0.98***
	(0.34)	(0.29)	(0.35)	(0.38)	(0.30)	(0.44)
Distance from Moscow		0.0003	(0.29)	0.00001		0.0002
		(0.002)		(0.00004)		(0.003)
Natural Resource		-0.002		-0.03		-0.01
Potential Index		(0.09)		(0.14)		(0.16)
Percent Russian of Total Population		-0.0002		0.09		0.003
		(0.21)		(0.34)		(0.34)
Percent Heavy Industry		-0.003		-0.01		-0.1
		(0.26)		(0.36)		(0.34)
Average Temp. in January		0.00003		0.002		0.0002
		(0.008)		(0.01)		(0.01)
Nazi Dummy		-0.0003		-0.01		-0.002
		(0.09)		(0.13)		(0.01)
Population		0.003		0.001		0.0006
		(0.03)		(0.04)		(0.005)
Overall R ²	0.03	0.09	0.03	0.15	0.03	0.09
Hausman Test $Pr > \chi^2$	0.3362		0.297		0.353	

Robust standard errors in parentheses. Significance levels: *10%, **5%, ***1%.

Panel B of Table 3.5 and Table 3.6 presents the first-stage results, which are statistically significant at the one percent confidence level for almost all the specifications. Consider the

second stage (Panel A) for the regressions with the number of small businesses per 1000 people as the dependent variable. The elite variable has a statistically significant effect on small business development at the 5 percent confidence level. According to the second column, the specification that includes all the relevant control variables, an oblast with a new elite leader has on average 2470 more small businesses than a region with an old elite leader, holding everything else constant. This is a large effect since the average number of small businesses across all Russian regions over the relevant time period is 4427.

Panel A of Table 3.6 documents the effect of the elite variable on log GDP. According to the regressions that include only the share of votes received by the Communist party in 1996 and the regressions that include both predetermined variables (Communist votes and annexation year), a region with a “new elite” leader has a statistically significantly higher CPI-adjusted GDP, holding everything else constant. This effect is statistically significant at the 1 percent confidence level.

Table 3.6-Panel A.
 Regressions of CPI-Adjusted Log GDP
 on the Elite Dummy Using Various Instruments: 2SLS

Dependent Variables	Log GDP (CPI Adjusted)					
	Predicted Value		Predicted Value		Predicted Value Using	
	Using Com Party		Using Year of		Com Party Votes	
	Votes in 1996		Annexation		& Annex Year	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: Two-Stage Least Squares</i>						
Predicted Elite Dummy	0.64***	1.36***	0.47***	0.85	0.64***	1.30***
	(0.15)	(0.40)	(0.15)	(0.65)	(0.15)	(0.36)
Distance from Moscow		-0.01*		-0.002		-0.005
		(0.003)		(0.005)		(0.003)
Natural Resource Potential Index		0.32***		0.23		0.31***
		(0.11)		(0.15)		(0.11)
Percent Russian of Total Population		-0.41		-0.12		-0.38
		(0.30)		(0.43)		(0.28)
Percent Heavy Industry		-0.61**		-0.55		-0.60**
		(0.31)		(0.35)		(0.30)
Average Temp. in January		-0.03***		-0.02*		-0.02***
		(0.01)		(0.01)		(0.01)
Nazi Dummy		0.08		0.07		0.08
		(0.10)		(0.11)		(0.10)
Population		-0.002		0.01		-0.002
		(0.004)		(0.01)		(0.004)

Robust standard errors in parentheses. Significance levels: *10%, **5%, ***1%.

Table 3.6 - Panel B.

First Stage for the Elite Dummy Variable

Dependent Variables	Log GDP (CPI Adjusted)					
	Predicted Value Using Com Party Votes in 1996		Predicted Value Using Year of Annexation		Predicted Value Using Com Party Votes & Annex Year	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel B: First Stage for the Elite Dummy Variable</i>						
Instrument	0.99*** (0.09)	0.97*** (0.19)	1.00*** (0.19)	1.04*** (0.38)	0.99*** (0.09)	0.96*** (0.18)
Distance from Moscow		0.0001 (0.001)	(0.29)	-0.003 (0.02)		0.0003 (0.002)
Natural Resource Potential Index		-0.004 (0.06)		0.08 (0.08)		-0.01 (0.06)
Percent Russian of Total Population		0.02 (0.15)		-0.02 (0.25)		0.03 (0.14)
Percent Heavy Industry		0.004 (0.16)		-0.005 (0.20)		0.01 (0.16)
Average Temp. in January		0.0003 (0.004)		-0.001 (0.01)		0.0005 (0.004)
Nazi Dummy		-0.001 (0.004)		0.001 (0.01)		-0.001 (0.05)
Population		0.0001 (0.02)		-0.004 (0.03)		0.0002 (0.002)
Overall R ²	0.01	0.03	0.01	0.05	0.01	0.03
Hausman Test $Pr > \chi^2$	0.3362		0.894		0.353	

Robust standard errors in parentheses. Significance levels: *10%, **5%, ***1%.

These results strongly suggest that Communist institutions do, in fact, persist in Russia even after its transition to democracy. After accounting for regional differences such as resources, geography, etc. and after correcting the reverse causality problem, we still see a significant

difference between regions that are governed by people who were powerful before 1991 and regions whose leaders are political newcomers.

3.5 Conclusion

The analysis in this chapter indicates that the status of a regional leader prior to democratization is important for the development of that region even after transition. The first part of this chapter uses simple OLS regressions to show that there is a strong correlation between the identity of an oblast's governor as an "old elite" and a lack of small business development in that oblast. In order to show that there is an actual causal relationship between political elites and economic outcomes, we seek to resolve the issues of endogeneity. Part of the problem is reverse causality, which we address with the use of variables that had been predetermined by the time of transition as potential instruments for the elite variable. The analysis suggests that a causal relationship can be established: regions with new elites in power have, on average, 2470 more small businesses than regions with old elites in power. Future research is required in order to resolve the issue of omitted variable bias. One solution would be to use another instrument. In particular, we can look at leadership change within the regions that is unrelated to Yeltsin's decisions and appointments. When a leader passes away due to natural causes, he or she must be replaced by another person. Presumably, there is no time for official appointments that come from above, and therefore the issue of endogeneity disappears.

We see that certain aspects of old institutions persist even beyond a seeming change to a new institutional system and become "shadow institutions." This is a new and exciting area of macroeconomic effects of institutional development which demands further research. In the future, we purport to look gather data over a longer time period in order to build a richer panel dataset. We plan to explore other potential dependent variables, other than small business development. In particular, GDP per capita growth is another variable that can be used, once we have a longer time horizon. Expanding the definition of the elite variable to include not only the identities of the governors of various regions, but also members of their cabinets, can also be a promising venue of further research.

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- Kirsan Ilyumzhinov

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Republic of Karachaevo-Cherkessia (RUS-KCH)

- Vladimir Khubiyev

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- Vladimir Semyonov

RR

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- Aslan Dzharimov

RR

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- Martaza Rakhimov

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Altai Krai (RUS-ALT)

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Khabarovsk Krai (RUS-KHA)

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- Vasily Dyakonov

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Kemerovo Oblast (RUS-KEM)

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- Valentin Sobolev

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RR

Kursk Oblast (RUS-KRS)

- Aleksandr Mikhailov

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Leningrad Oblast (RUS-SPT)

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RR

Lipetsk Oblast (RUS-LPT)

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- Oleg Korolev

RR

Magadan Oblast (RUS-MAG)

- Viktor Mikhailov

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- Valentin Tsvetkov (b. 1948 - d. 2002)

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RR

Moscow Oblast (RUS-MSK)

- Anatoly Tyazhlov

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RR

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Murmansk Oblast (RUS-MRM)

- Yevgenii Komarov

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RR

Nizhnii Novgorod Oblast (RUS-NZN)

- Boris Nemtsov

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- Ivan Sklyarov

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RR

Novgorod Oblast (RUS-NVG)

- Mikhail Prusak

RR

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Novosibirsk Oblast (RUS-NOV)

- Vitaly Mukha

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Omsk Oblast (RUS-OMS)

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Pskov Oblast (RUS-PSK)

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RR

Rostov Oblast (RUS-RSV)

- Vladimir Chub

RR

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Ryazan Oblast (RUS-RYZ)

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RR

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RR

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Saratov Oblast (RUS-SRT)

- Yuri Belyh

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RR

Smolensk Oblast (RUS-SML)

- Anatoly Glushenkov

http://niiss.ru/sf_glushenkov.shtml

- Aleksandr Prokhorov

RR

<http://www.panorama.ru/info/demo/TEXTS/36912.html>

Sverdlovsk Oblast (RUS-SVD)

- Eduard Rossel

<http://www.whoiswho.ru/russian/Password/journals/31999/rossel.htm>

RR

- Aleksei Strakhov

http://www.garant.ru/files/duma_htm/deputats/185-1.htm

Tambov Oblast (RUS-TMB)

- Vladimir Babenko (b.1931 – d. 1996)

http://www.geocities.com/CapitolHill/2768/p38_tamb.html

- Oleg Betin

RR

http://www.grankin.ru/dosye/ru_bio350.htm

- Aleksandr Ryabov
http://www.grankin.ru/dosye/ru_bio182.htm
 Tomsk Oblast (RUS-TOM)
- Viktor Kress
 RR
<http://kress.tomsk.net/bio.htm>
 Tula Oblast (RUS-TUL)
- Nikolai Sevryugin
<http://www.vgd.ru/S/sevstjnv.htm#Sevryugin>
- Vasilii Starodubtsev
 RR
 Tver Oblast (RUS-TVR)
- Vladimir Suslov
<http://www.roiip.ru/regions/83/07.htm>
<http://www.vgd.ru/S/suhanov.htm#Suslin>
- Vladimir Platov
<http://www.prazdniki.ru/person?isvip=1&c=3992>
 RR
 Tyumen Oblast (RUS-TYU)
- Yurii Shafranik is appointed in 1991.
<http://nefte.ru/person/ch.htm>
<http://www.nns.ru/restricted/persons/shafran6.html>
- Leonid Roketskii is elected governor in 1993.
 RR
 Ulyanovsk Oblast (RUS-ULY)
- Yury Goryachev
 RR
http://www.grankin.ru/dosye/ru_bio193.htm
 Vladimir Oblast (RUS-VLD)
- Yury Vlasov

<http://www.vgd.ru/V/vlasov.htm>

<http://www.whoiswho.ru/russian/Password/journals/11999/regionr.htm>

- Nikolay Vinogradov

RR

http://www.grankin.ru/dosye/ru_bio123.htm

Volgograd Oblast (RUS-VGG)

- Ivan Shabunin

http://www.grankin.ru/dosye/ru_bio125.htm

- Nikolai Maksyuta

<http://www.roiip.ru/regions/48/07.htm>

http://www.dosye.ru/dosye/ru_bio321.htm

RR

Vologda Oblast (RUS-VLG)

- Nikolai Podgornov

<http://www.whoiswho.ru/russian/Password/journals/61999/region.htm>

- Vyacheslav Pozgalev

<http://www.roiip.ru/regions/49/07.htm>

RR

Voronezh Oblast (RUS-VRN)

- Aleksandr Kovalev

<http://www.voronezh.ru/iv/bio/ak.html>

<http://www.nns.ru/regiony/voron5.html>

- Ivan Shabanov

RR

Yaroslavl Oblast (RUS-YRS)

- Anatoly Lisitsyn

RR

http://i1600.100mb.ru/~grankin/dosye/ru_bio199.htm

Moscow City

- <http://www.nupi.no/russland/database/start.htm>

St. Petersburg

- <http://www.nupi.no/russland/database/start.htm>

Year of Annexation to the Russian Empire or to USSR and the Nazi Occupancy Dummy:

Many of the historical accounts for the regions were found on the following website:

<http://www.hf.uib.no/Andre/vesti/>

For the oblasts not listed on the above website,

Orel Oblast

http://oriol.school.edu.ru/default.asp?wci=doc&tmpl=text&d_no=136

<http://oriol.school.edu.ru/>

Tula Oblast

<http://www.tula.intergrad.ru/istoria.htm>

Kaluga Oblast

<http://www.admobl.kaluga.ru/New/History/history.htm>

Bryansk Oblast

<http://www.bryanskobl.ru/~press/history/index.shtml>

Smolensk Oblast

<http://www.russiancity.ru/text/smo.htm>

Krasnodar Krai

<http://www.kuban.su/history.html>

Gorno-Altai Republic

<http://www.gorn-altai.intergrad.ru/istoria.html>

Buryatia Republic

http://www.buryatia.org/history/buryat_hist.html

Irkutsk Oblast

http://www.admirk.ru/Obl_hist.htm

Kaliningrad Oblast

“Wikipedia, The Free Encyclopedia”

Krasnoyarsk Oblast

<http://region.krasu.ru/history/krasnoyarsk/kr1.php>

Kurgan Oblast

<http://kurganregion.ru/kurgan/common/history1.html>

Khakasia Republic

<http://www.khakasia.ru/khakasia/history/>

Kemerovo Oblast

<http://www.kemerovo.intergrad.ru/histori.html>

Leningrad Oblast

<http://home.comset.net/freshspb/history/>

Chelyabinsk Oblast

<http://www.74.ru/town/history.php>

Magadan Oblast

<http://magadan.school.edu.ru/>

Murmansk Oblast

<http://www.murmanchanin.ru/mrmfoto>

Chuvashia Republic

<http://www.pti.ru/org/lito/prayslist/demo/gn/istor/3/glava3.htm>

Moscow City

<http://pubs.carnegie.ru/books/1998/11np/PDF/01moskva.pdf>

St. Petersburg

<http://pubs.carnegie.ru/books/1998/11np/PDF/02stpeter.pdf>

Chapter 4

Equilibrium Selection in Global Games: an Experiment

4.1 Introduction

Interactions between economic agents often depend on their ability to coordinate on a course of action. Examples range from applications in industrial organization, such as deciding on the timing of commercial breaks by contemporary music radio stations (Sweeting, 2006), to macroeconomic events, such as speculative attacks on a currency (Obstfeld 1996). However, coordination attempts are often based solely on agents' expectations that others would act in unison, which can lead to miscoordination.

Several papers propose the use of focal points and framing as a way to resolve indeterminacy in coordination games. The best-known example was offered by Shelling (1980) where two strangers are unable to communicate, but need to meet somewhere in New York City without having set a location or time for the meeting. In his informal experiments conducted in the 1950s, Shelling found that people overwhelmingly chose Grand Central Station at noon. Thus, Grand Central Station was the focal point in the experiment. Frames relate abstract strategies available to the players to the context of the game, providing the players with extra information about which equilibrium might be chosen by others (Bacharach and Stahl, 2000; Casajus, 2000). In fact, experimental evidence shows that even a coordination device such as a non-equilibrium salient payoff may attract the players' choices although it is Pareto dominated by all Nash

equilibria (Bosch-domènech and Vriend, 2007).

Focal points and framing facilitate equilibrium selection by conveying additional information to the players. Are there situations where a completely uninformative public announcement plays a role in determining an outcome of an economic event? While other coordination devices alter the nature of the game from the beginning, such “cheap talk” communication serves as a generic sunspot that can help agents settle on an equilibrium while preserving the game in its original state. This type of sunspot can also serve a different purpose. While pure coordination games of complete information always deliver the prediction of multiple equilibrium outcomes that are easily detected in the laboratory (Cooper et al. 1990, 1992), coordination games of incomplete information (Morris and Shin, 1998; Angeletos, Hellwig, and Pavan, 2007) provide a new challenge for the experimenter seeking to test this theory.

Model. We choose the framework used by Angeletos, Hellwig, and Pavan (2007) for testing whether agents coordinate on multiple courses of action in an environment where multiple equilibria are possible. The model consists of a large number of agents and two possible regimes, the status quo and an alternative. The game continues into the second period as long as the status quo is in place. In each period, each agent can either attack the status quo (i.e., take an action that favors regime change), or not attack. The net payoff from attacking is positive if the status quo is abandoned in that period and negative otherwise. Regime change, in turn, occurs if and only if the percentage of agents attacking exceeds a threshold $\theta \in \mathbb{R}$ that parameterizes the strength of the status quo. The parameter θ captures the component of the payoff structure (the “fundamentals”) that is never common knowledge. In the first period, each agent receives a private signal about θ . If the game continues into the second period, agents receive additional sufficiently precise private information about θ . In addition to the private signals, the agents also receive an announcement of “attack” or “do not attack” either in the first or in the second period. The announcer does not receive any private information about θ . The announcer’s payoff depends on whether sufficiently many of the non-announcers followed suit, but her choice does not influence the outcome of the game for a given value of θ and size of the attack.

While it would be more straightforward to use a static coordination game for this study, we find the dynamic setting more appealing and natural. First, allowing agents to take actions

over multiple periods and learn over time brings this study closer to applications. Applications include self-fulfilling bank runs, currency crises, or political change where the announcement can be interpreted as any piece of news in the media that is not coming from an informed party directly involved in the economic crisis. Second, the theory suggests that the multiplicity emerges endogenously in this dynamic model, which is a more natural outcome than introducing multiplicity exogenously by changing the parameters of a static model. Finally, when one changes parameter values in order to introduce regions with and without multiplicity in the static model, these changes are continuous and therefore subtle from the perspective of the experimental subjects. In this model, on the other hand, the change in information going from the first to the second stage is discrete and dramatic enough, so that it may be easier for the subject to notice and understand it.

Model Predictions. In the first period (stage) of the game, agents should put no weight on the announcement and make decisions based solely on their private signals in accordance with the uniqueness prediction of the theory.

Second-period predictions depend on the information structure. Without new information, there is a unique equilibrium in the second stage, namely that no agent attacks the regime and the regime survives. In this case, the announcement should have no significant effect on behavior. With new information and under the assumption of a relatively “lenient” initial prior about the state of the fundamentals, in addition to the no-attack equilibrium, a new attack becomes possible. In this case, the announcement may serve as a coordination device.

Experimental Results. To test the predictions of the model, we conduct several treatments of a laboratory experiment where we vary the strength of the fundamentals, information in the second stage, and the nature and timing of the uninformative announcement.

In the first stage of the experiment, we find some evidence that agents respond to the announcement, although the effect is only marginally statistically significant. Previous research finds that beliefs play a major role in subjects’ behavior, and in particular overly aggressive beliefs are responsible for excessive aggressiveness in agents’ strategies. We find evidence for the same pattern in this experiment. Therefore, we explore whether the announcement affects actions in the first stage through its effect on beliefs and find that this is indeed the case.

In the second stage of the experiment, we first examine the treatment with new informa-

tion, where the theory predicts the possibility of a new attack. Here, we seek to detect a differential effect of the announcement on equilibrium outcome selection. In treatments with the announcement in the first and in the second stage, the announcement acts as a coordination device. Comparing rounds with the announcement of “attack” to the rounds with the announcement of “no attack,” we find that the probability of attack is significantly greater in the former than in the latter.

So far, it has been unclear whether the effect of the announcement in the second stage is due to the presence of multiple equilibria or due to the fact that the game continued into the second stage. In order to distinguish between these two alternatives, we conduct a control treatment where the subjects do not receive new information in the second stage. Recall that the announcement should not play an important role in this case, according to the theory. We confirm that, without new information, the announcement does not have a statistically significant effect on subjects’ decisions, which is consistent with the theory.

The strong effect of the announcement on agents’ actions in the second stage with new information points to the presence of multiple equilibria in this environment and allows us to conclude that even such a weak coordination device can serve as a tool for equilibrium selection. Furthermore, we find evidence of equilibrium reasoning employed by the subjects. That is, the announcement seems to affect subjects’ actions through its effect on their beliefs about the actions of others.

Related Literature. This study contributes to several branches of literature. The first branch tests the theory of global coordination games which starts with seminal papers by Carlsson and van Damme (1993a), (1993b) and later Morris and Shin (1998). These papers abandon the assumption of common knowledge, introducing incomplete information. They show that, under certain restrictions on the information structure, multiplicity of equilibria can be eliminated by assuming that agents receive heterogeneous private information about the state of the fundamentals. This result has already been applied to several macroeconomic phenomena: see Goldstein and Pauzner (2001) and Rochet and Vives (2004) for bank runs; Corsetti, Guimaraes and Roubini (2003) and Morris and Shin (2004) for debt crises; Atkeson (2000) for riots; Chamley (1999) for regime switches; and Edmond (2005) for political change. Another line of work has emphasized how the determinacy of equilibria and the outcomes of these games may criti-

cally depend on the endogeneity of information (see Angeletos and Werning (2006) for learning through prices, Angeletos, Hellwig, and Pavan (2007) for learning through past outcomes).

Global games have been tested both with field data (Cheng, Goldstein, and Jiang, 2007) and by conducting laboratory experiments (Cabrales, Nagel, and Armenter (2003); Heinemann, Nagel, and Ockenfels (2004)). More recent experiments extend the framework to multiple periods in order to explore the effects of learning on coordination. Costain, Heinemann, and Ockenfels (2007) analyze the evolution of herding behavior in the context of a dynamic global game by making decisions sequential and allowing some previous actions to be observed. Chapter 1 of this thesis finds that agents exhibit learning from new information and from past outcomes. Furthermore, the study documents that agents use equilibrium reasoning, basing their actions on their beliefs about others actions.¹ While this chapter uses the same framework and experimental design as the first chapter, its goals and ultimate contributions are new and different. While the previous study focused on learning effects over time and on the evidence of equilibrium reasoning, this chapter addresses the technical issue of multiplicity that can be detected and characterized in the laboratory using a particular coordination device.

The second branch of literature that is related to this study deals with the effects of coordination devices, communication in particular, on coordination. There are numerous papers that provide experimental evidence that focal points promoting coordination (for example, see Mehta, Starmer, and Sugden, 1994; Ochs, 1990). Cooper et al. (1992) study non-binding pre-play communication in pure coordination games as a particular coordination device. They find that in coordination games with a cooperative strategy, one-way communication increases play of the Pareto-dominant equilibrium relative to the no-communication baseline. One can even view existence of a large player in a coordination game as a type of coordination device that allows the agents to agree on a payoff-dominant course of action (Cheung and Friedman, 2006). Finally, Duffy and Fisher (2005) provide direct evidence of sunspot equilibria in the laboratory.

Finally, this study adds to the literature that focuses on testing for the presence of multiple equilibria. Sweeting (2005) finds empirical evidence for the presence of multiple equilibria in

¹Other papers on dynamics of coordination include Cheung and Friedman (2006) (though this experiment only involves public information), Cornand (2006), Schotter and Yorulmazer (2003).

radio markets. Davis and Weinstein (2004) test empirically whether the location of production in eight manufacturing industries is subject to multiple equilibria and find that data prefer a model with a unique stable equilibrium.

The rest of the chapter is organized as follows. Section 4.2 describes the dynamic two-period model of a speculative attack with an uninformative announcement and discusses theoretical predictions to be tested. Section 4.3 describes the experimental procedures and treatments. Section 4.4 describes the results of the data analysis. Section 4.5 concludes and discusses potential future research in this area.

4.2 The Model

Actions and Payoffs: Our model is a simple two-period version of the model developed by Angeletos, Hellwig, and Pavan (2006) in which there are two regimes, the status quo and the alternative. There are two types of agent: a single announcer, indexed by j , and a large number of non-announcers (hereafter, “agents”), indexed by i . The announcer moves decides between two possible courses of action, A and B. The other agents also decide simultaneously whether to “attack” (choose action A that favors regime change) or abstain from “attacking” (choose action B that favors the status quo). The status quo collapses if the mass of agents choosing action A (“aggregate size of the attack”), exceeds θ , which parametrizes the strength of economic fundamentals. A low value of θ thus represents a relatively weak state of the fundamentals, and a high value of θ represents a relatively strong state of the fundamentals. Note that the announcer’s decision does not play a role for the outcome of the game. We will denote the regime outcome by $R_{t+1} \in \{0, 1\}$ where $R_{t+1} = 0$ refers to the survival of the status quo, while $R_{t+1} = 1$ refers to the collapse of the status quo.

For the agents, action A is associated with an opportunity cost c . If action A is successful (i.e., the status quo is abandoned), each agent choosing action A earns an income of $y_i > c$. If not (i.e., the status quo prevails), then the agent choosing action A earns 0. Action B yields

no payoff and has no cost.² The payoff of an individual agent can be written as

$$u_{it} = U_i(a_{it}, N_t, \theta) = \begin{cases} a_{it}(y_i - c) & \text{if } N_t \geq \theta \\ -a_{it}c & \text{if } N_t < \theta \end{cases} .$$

where $a_{it} \in \{0, 1\}$ denotes the action chosen by agent i at time t ($a_{it} = 1$ represents attacking and $a_{it} = 0$ represents not attacking) and N_t denotes the aggregate size of the attack at time t .

The payoff of the announcer depends on her ability to forecast the outcome of the final stage of the game. If her announcement is A and action A is successful, the announcer receives an income of y_j , but if action A is not successful, the announcer loses y_j . On the other hand, if her announcement is B and action A is successful, the announcer loses y_j , but if action A is not successful, the announcer gains y_j . The payoff of an announcer j is summarized by

$$u_j = U_j(a_j, N_t, \theta) = \begin{cases} a_j y_j & \text{if } N_t \geq \theta \\ -a_j y_j & \text{if } N_t < \theta \end{cases} .$$

where $a_j \in \{1, -1\}$ denotes the action chosen by announcer j ³ ($a_{it} = 1$ represents an announcement of attacking and $a_{it} = -1$ represents not attacking) and N_t denotes the aggregate size of the attack at time t .

Timing and Information: The first period of the model consists of two sub-periods. The announcer moves first, deciding between A and B. Next, the agents receive information and decide between A and B as well. The announcer does not get to act again in the second period. Therefore, only agents get to make decisions in the second period.

Since the status quo is abandoned when $N_t \geq \theta$ and maintained when $N_t < \theta$, multiple equilibria exist for $\theta \in [0, 1]$, when θ is common knowledge. However, following Morris and Shin (1998), in this model, agents get heterogeneous information about the strength of the status quo. Nature draws θ from a normal distribution $N(z, 1/\alpha)$ which defines the initial common prior about θ . Note that z can be thought of as the public signal that all agents

²Note that the payoff to the agent does not depend on θ . Or it only depends on it in the following way: if θ is so low that the regime collapses, the payoff to the agent choosing action A is y , but if the regime survives, the payoff is always the same (0).

³Recall that the announcer decides at the beginning of the game and never again. That is why announcer's action a_j is not indexed by the time subscript t .

receive. Each agent also gets to observe the announcement (either in the first or in the second period). Note that the announcement is a public signal that is completely uninformative, since the announcer only knows z and α , but does not get any further information about the strength of the fundamentals. The announcement can come either in the first or in the second stage of the game. In addition to receiving these public signals, each non-announcer then receives a private signal $x_{it} = \theta + \xi_{it}$, where $\xi_{it} \sim N(0, 1/\beta_t)$ is i.i.d. across agents and independent of θ and β_t is the precision of private information.⁴ The status quo is in turn abandoned if and only if the measure of agents choosing action A, which is denoted by N_t , is greater than or equal to θ .

4.2.1 First-Period Predictions

Let us first focus on the equilibrium in the first period of the game.

Agents: Even if the agents receive the announcer's signal in the first period, they only take into account their own private signals x_1 , combining it with the information on the distribution of θ . This is because they know that the announcement is completely uninformative. Note that for the agents it is strictly dominant to choose action A for sufficiently low signals – namely for $x_1 < \underline{x}$, where \underline{x} solves $Pr(\theta \leq 0|\underline{x}) = c/y$ – and to choose B for sufficiently high signals – namely for $x_1 > \bar{x}$, where \bar{x} solves $Pr(\theta \leq 1|\bar{x}) = c/y$.⁵ This suggests that we should look for monotone Bayesian Nash equilibria such that, for a given realization of z , an agent chooses A if and only if the realization x of her private signal is below a certain threshold x_1^* (and in which the agents' strategy is non-increasing in x_1). In such an equilibrium, the aggregate size of the attack is given by $N_1(\theta) = Pr(x_1 \leq x_1^*|\theta) = \Phi(\sqrt{\beta_1}(x_1^* - \theta))$, where Φ is the c.d.f. of the Standard Normal distribution. The status quo is abandoned if and only if $\theta \leq \theta_1^*$, where θ_1^* solves $\theta_1^* = N_1(\theta_1^*)$, or equivalently

$$x_1^* = \theta_1^* + \beta_1^{-1/2}\Phi^{-1}(\theta_1^*). \quad (4.1)$$

⁴The information structure is parameterized by $\beta_t = \sigma_{x,t}^{-2}$ and $\alpha = \sigma_z^{-2}$, the precisions of private and public information, respectively, or equivalently by the standard deviations, $\sigma_{x,t}$ and σ_z . The agents know the values of z , α , and β_t .

⁵Note that, for notational tractability, we suppress the individual subscript, i , from now on.

It follows that the expected payoff from attacking is $Pr(\theta \leq \theta_1^* | x_1) - c/y$ and hence threshold x_1^* solves $Pr(\theta \leq \theta_1^* | x_1^*) = c/y$. Since posteriors about θ are normally distributed with mean $\frac{\beta_1}{\beta_1 + \alpha} x_1 + \frac{\alpha}{\beta_1 + \alpha} z$ and variance $\frac{1}{\beta_1 + \alpha}$ (precision $\beta_1 + \alpha$), this condition is equivalent to

$$\Phi \left(\sqrt{\beta_1 + \alpha} (\theta_1^* - \frac{\beta_1}{\beta_1 + \alpha} x_1^* - \frac{\alpha}{\beta_1 + \alpha} z) \right) = c/y. \quad (4.2)$$

If we substitute (4.1) into (4.2), we get a single equation in θ^* :

$$-\frac{\alpha}{\sqrt{\beta_1}} \theta_1^* + \Phi^{-1}(\theta_1^*) = \sqrt{1 + \frac{\alpha}{\beta_1}} \Phi^{-1}(1 - c/y) - \frac{\alpha}{\sqrt{\beta_1}} z \quad (4.3)$$

One can easily check that a solution to equation (4.3) always exists and is unique for all z if and only if $\beta_1 \geq \frac{\alpha^2}{2\pi}$.

Announcer: The announcer who needs to make an announcement decision in the first stage maximizes her utility given the prior z , anticipating that the agents do not take into account the announcement because of its completely uninformative nature. In a fully rational equilibrium, the announcer should announce action A if and only if

$$Pr[\theta < \theta_1^*(z) | z] \geq 0.5 \quad (4.4)$$

For derivation of equation (4.4), see Appendix A.

Note that $\theta_1^*(z)$ is decreasing in z , $Pr[\theta < \theta_1^*(z) | z]$ is also decreasing in z . This gives us the following result:

Proposition 1 *There exists a threshold z_1^* , such that the announcer will choose the following equilibrium strategy in the first stage*

$$a_j = \begin{cases} 1 & \text{if } z \leq z_1^* \\ -1 & \text{if } z > z_1^* \end{cases} .$$

The following prediction is implied by the first-period equilibrium of the model.

Prediction 1. *There exists a unique x_1^* , such that in any equilibrium of the dynamic game, an agent chooses action A (“attack”) in the first period if and only if $x_1 < x_1^*$. By implication,*

$A_1(\theta)$ is decreasing in θ , and there exists a unique θ_1^* such that the status quo is abandoned in the first period if and only if $\theta < \theta_1^*$. There also exists a unique threshold z_1^* , such that in any equilibrium of the game, an announcer chooses an announcement of A in the first period if and only if $z \leq z_1^*$.

In this chapter, we focus on the part of the above prediction that deals with the role of the announcement. Note that the equilibrium is unique and does not vary with the announcement. In terms of the implication for the experiment, this means that we should not observe an effect of the announcement on the thresholds x_1^* and θ_1^* .

4.2.2 Second-Period Predictions

The game continues into the second period as long as the status quo is in place, and the game ends if the status quo is abandoned in the first period. We will consider two possibilities for the information structure in the second period. First, suppose that the agents receive no additional private signal.

Agents: In this case, when the agents arrive at the second period, they observe that the status quo must have survived the first-period attack. From the observation that the status quo is still in place, the agents learn that the state of the fundamentals is not too weak, because otherwise it would have collapsed under the first attack. In fact, they now know that θ must be above θ_1^* . The knowledge that $\theta > \theta_1^*$ causes a first-order-stochastic-dominance shift of beliefs upwards, causing agents' behavior to become less aggressive. It turns out that this effect is strong enough to imply that no agent is willing to take action A in the second period, delivering a unique equilibrium. Therefore, the fact that the agents also receive an uninformative announcement should play no role in this framework.

Announcer: The announcer knows that the other agents do not receive more information in the second stage. Therefore, the announcer making a decision in the second stage always chooses an announcement of B.

The following prediction is implied by the second-period equilibrium of the model with no new information.

Prediction 2. *If no new information arrives in the second period, then choosing action B ("not attack") for all x is the unique continuation equilibrium for the agents regardless of*

the announcement. Similarly, the equilibrium strategy for the announcer is to always choose an announcement of B .

Next, we consider the information structure in the second period where agents receive an additional private signal that is sufficiently precise. That is, $x_{i2} = \theta + \xi_{i2}$, where $\xi_{i2} \sim N(0, 1/\beta_2)$ and β_2 is sufficiently high. Recall that agents also get the announcement.⁶

Agents: The size of the attack in period two is given by $A_2(\theta) = \Pr(x_2 \leq x_2^*|\theta)$, which is decreasing in θ , and the probability of regime change for an agent with signal x_2 is $\Pr(R_2 = 1|x_2, R_1 = 0) = \Pr(\theta \leq \theta_2^*|x_2, \theta > \theta_1^*)$, which is decreasing in x_2 if $\theta_2^* > \theta_1^*$. Therefore, in any equilibrium in which an attack occurs in the second period, θ_2^* and x_2^* solve

$$\theta_2^* = \Phi(\sqrt{\beta_2}(x_2^* - \theta_2^*)) \quad (4.5)$$

$$1 - \frac{\Phi(\sqrt{\beta_2 + \alpha}(\frac{\beta_2}{\beta_2 + \alpha}x_2^* + \frac{\alpha}{\beta_2 + \alpha}z - \theta_2^*))}{\Phi(\sqrt{\beta_2 + \alpha}(\frac{\beta_2}{\beta_2 + \alpha}x_2^* + \frac{\alpha}{\beta_2 + \alpha}z - \theta_1^*))} = c/y. \quad (4.6)$$

Equations (4.5) and (4.6) are the second-period equivalents of equations (4.1) and (4.2) in the dynamic setting. We can solve them for θ_2^* and x_2^* , which will tell us under which conditions we can have an attack in the second period. In particular, the public information revealed to all subjects before the first period must be such that z is sufficiently high.⁷ Intuitively, when z is high (“lenient prior”), arrival of new more precise private information makes the marginal agent more aggressive and may eventually offset the incentive not to choose action A induced by the knowledge that the regime survived past attacks. Indeed, if z is sufficiently high, so that $\theta_1^* < \theta_\infty$, then a second attack necessarily becomes possible once β_2 is sufficiently high (i.e., the second signal is sufficiently precise). Note that θ_∞ is the limit of the equilibrium threshold of the static game as the precision of private information becomes infinite (in particular, $\theta_\infty = 1 - c/y$). In this case, the theory predicts that, in addition to the no-attack equilibrium (which is *always* an equilibrium), there can also be an attack equilibrium (for proofs and derivations see chapter 1 of this thesis). The effect brought about by the introduction of exogenous information, making the marginal agent more aggressive, counteracts the effect of endogenous learning that makes

⁶In the model, we consider cases where agents receive the announcement in the first stage, in the second stage, or in both.

⁷For a special case where the cost of attacking equals 1/2, “high z ” means $z > 1/2$. Under this condition, $\theta_1^*(z) < 1/2$, because $\theta_1^*(z)$ is monotonically decreasing in z and $\theta_1^*(1/2) = 1/2$. (See Shurchkov (2007) for proof.)

the agent relatively less aggressive. In fact, the effect of more precise information can offset the incentive not to choose action A induced by the knowledge that the regime has survived past attacks, thus making new attacks possible.

Announcer: The announcer who needs to make an announcement decision in the second stage maximizes her utility given the prior z and the first-period threshold θ_1^* . The announcer also knows that the agents have received new information in this stage of the game. Therefore, the equilibrium characterization for the announcer is analogous to the above characterization for the agents. In particular, in the no-attack equilibrium, where the agents always choose action B, the announcer always chooses the announcement of B. However, in addition to the no-attack equilibrium, there is now a possibility of a new attack. In this case, the announcer should announce action A if and only if

$$\Pr[\theta < \theta_2^*(z)|z, \theta_1^*(z)] \geq 0.5 \quad (4.7)$$

For derivation of equation (4.7), see Appendix A.

Note that $\theta_2^*(z)$ is decreasing in z , $\Pr[\theta < \theta_2^*(z)|z, \theta_1^*(z)]$ is also decreasing in z , which gives us the following result:

Proposition 2 *There exists a threshold z_2^* , such that the announcer will choose the following equilibrium strategy in the “attack equilibrium” with new information in the second stage*

$$a_j = \begin{cases} 1 & \text{if } z \leq z_2^* \\ -1 & \text{if } z > z_2^* \end{cases} .$$

The following prediction is implied by the second-period equilibrium of the model with new information.

Prediction 3. (a) *There is always an equilibrium in which agents choose action B (“not attack”) for all x and, equivalently, the announcer chooses an announcement of B in the second period.* (b) *In addition to the no-attack equilibrium, there also exist an x_2^* , z_2^* and an equilibrium in which an agent chooses action A (“attack”) if and only if $x_2 < x_2^*$ and the announcer chooses an announcement of A if and only if $z < z_2^*$.*

Because we have multiple equilibria in the second stage with new information, we expect the announcement to play a role as a coordination device, adding no additional information about the strength of the status quo but helping us detect multiplicity. If the announcement is action A, the agents are more likely to coordinate on the attack equilibrium; if the announcement is action B, the agents are more likely to coordinate on the no-attack equilibrium.

In the following sections, we proceed to test the above predictions in a laboratory experiment.

4.3 Overview of the Experiment

Procedures:

We conducted four sessions of the experiment at the Computer Lab for Experimental Research (CLER) at Harvard Business School. All four sessions were held in Spring of 2007. The overall procedures were kept the same throughout all sessions. All sessions were computerized using the program z-Tree (Fischbacher, 2007). The subjects were first asked to read through and sign informed consent forms for non-biomedical research.⁸ Paper copies of the instructions were distributed to the participants prior to the beginning of the experiment.⁹ The subjects were asked to answer several control questions that tested their understanding of statistics, as well as the experimental procedures. Questions were answered in private. The subjects could not see or communicate with one another. At the end of the experiment, each participant filled out a computerized questionnaire.¹⁰ The questionnaire asked the subjects about their strategies, their gender, and their level of understanding of statistics and probability. At the very end, each subject was paid in cash a show-up fee equal to 10 US \$ and his or her earnings over the course of the session. Final income of each subject was first given in points and then converted to US \$ at the rate of 10 points = 20 cents in all sessions. Average income (including the show-up fee) was \$21.84.

Session 4 constituted the control treatment where a group of 15 subjects participated in 20 independent rounds of play. Each round corresponded to a new random number θ drawn

⁸Copies of the informed consent forms are available upon request.

⁹Full copies of the instructions are available upon request.

¹⁰Copies of the questionnaire questions in German or English are available upon request.

from a normal distribution $N(z, 1/\alpha)$ ¹¹. Thus, one can interpret each round as a new economy parametrized by the state of fundamentals, θ . The subjects were informed of the mean and the standard deviation of this distribution in the instructions. In addition, at the beginning of the round, each subject received a private signal (“hint number” x_1) about the random number θ . The subjects were given information about the distribution of this hint number in the instructions.

Each round consisted of one or two periods (“stages”) of decision-making. In stage 1 of each round, each subject had to decide between actions A or B as described in section 4.2. Once all the subjects chose their actions in each stage of every round, they were asked a follow-up question, namely: “How many other members of your group do you think chose action A?” Next, each subject received the following information. If the game ended after stage one, he or she found out that action A was successful, learned the value of the unknown number, how many other subjects chose action A, and his or her payoff in the round. If the game continued into the second stage, the subjects in some of the sessions (1 and 2) received a new more precise signal (“hint number” x_2) about the random number θ .

Sessions 1-3 differed from session 4 in that, in addition to the 15 regular players, each group contained one announcer (chosen according to a pre-determined random order in every round). Each of the 16 participants knew that the random number θ drawn from a normal distribution $N(z, 1/\alpha)$. However, this was the extent of the information available to the announcer who picked between actions A and B without receiving a hint number. In session 1, which had 32 participants divided randomly into two groups of sixteen people, the non-announcers received the announcement along with hint number (x_1) in the first stage. The rest of the experimental design was identical to one in session 4. In sessions 2 and 3, each consisting of one group of 16 participants, the non-announcers received the announcement in the second stage. Subjects in sessions 1 and 2 received a more precise hint number (x_2), while the subjects in session 3 did not.

¹¹This experiment relies on the use of the normal distribution, as opposed to the uniform distribution used by Heinemann, Nagel, and Ockenfels (2004). The ideas are essentially identical. The benefit of running the experiment using the normal is the tractability of the analysis. The theory involved in the dynamic case is significantly more complicated as compared to the static benchmark, which necessitates the use of the normal. The normal distribution is also relatively simple to grasp for the test subjects, since it is fully parametrized by the mean and the precision. To ensure the subjects’ full understanding during the experiment, we provided them with several examples and conducted a quiz to familiarize them with the normal distribution.

The non-announcer payoffs are summarized in the table below:

	$A > \theta$	$A < \theta$
Choose action A	100 - 60	-60
Choose action B	0	0

The announcer's income depended on whether or not she could match the choices that other 15 people made in stage 2. The announcer payoffs are summarized in the table below:

	$A > \theta$	$A < \theta$
Announce action A	60	-60
Announce action B	-60	60

We ran different treatment conditions based on the timing of the announcement. The various treatment conditions are summarized in Table 4.1.

Table 4.1.

Treatment Summary.

Session	Announcement in	Information in Stage 2
1	Stage 1	Yes
2	Stage 2	Yes
3	Stage 2	No
4	Never	Yes

Parameterization:

We re-scaled all numbers by a factor of 100, so that the subjects did not have to deal with fractions. We chose the gross payoff, y , of a successful attack to be 100 and the gross payoff of an unsuccessful attack to be 0.

Table 4.2 records the remaining parameters for all sessions.

Table 4.2.

Parameterization.

Session	$z, 1/\alpha$	$1/\beta_1$	$1/\beta_2$
1, 2, 4	75, 55	10	1
3	75, 55	10	-

Note that the mean of the normal distribution was chosen to be high enough, in order to ensure that we can test the scenario where a new attack becomes possible with the arrival of new information in the second stage. In order to get a reasonable number of random draws within the interval of $[0, 100]$, we kept the mean “not too high.” The standard deviation was chosen based on satisfying the criterion for stage-one uniqueness, which necessitates that the precision of public information must be sufficiently lower than the precision of private information. We also, however, needed to keep the precision from being too low, again in order to get enough random draws within the interval $[0, 100]$.

4.4 Data Analysis

4.4.1 Variables and Summary Statistics

In our analysis, the main dependent variables are the size of the attack (measured as the fraction of subjects choosing action A) on the aggregate level, and the action (a binary choice variable, with 1 representing action A and 0 representing action B) on the individual level.

The main explanatory variables are the announcement dummy variable with 1 corresponding to an announcement of “attack” and 0 otherwise, the random number, θ , on the aggregate level and the subject-specific hint number, x , on the individual level. We also look at several other variables which can have an effect on outcomes. We consider subjects’ expectations about the size of the attack by creating a *belief* variable.

Table 4.B1 in the Appendix provides descriptive statistics for the experiment.

4.4.2 First Period Predictions

In the first stage of each independent round, the subjects chose between actions A and B. Before we move on to our main results of the effect of the announcement on actions, we confirm that the experiment conducted at Harvard Business School using potentially different subjects delivered similar results as our previous experimental study conducted at the University of Zurich (see chapter 1 of this thesis). Figure 4.1 below plots the aggregate size of attack (the total number of players out of 15 choosing action A) against θ and confirms that behavior observed using the new subject pool is still consistent with monotone strategies. In all sessions,

the size of the attack is strictly decreasing in θ , just as the theory predicts. Moreover, the figure below shows that, for low states, almost everyone always chose action A, while for high states, almost everyone always chose action B. There is an intermediate range of fundamentals for which the size of the attack is decreasing in θ . We test this nonparametrically by carrying out a locally weighted regression of the number of attackers on the value of θ which is represented by the black monotonically decreasing line in Figure 4.1. The monotonicity of the fitted line confirms the hypothesis.

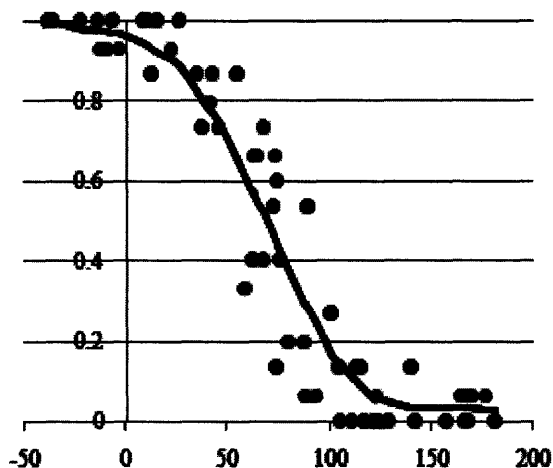


Figure 4.1: Kernel Regression for Session 1-3

We next turn to the main prediction in this section that deals with the effects of the announcement. First of all, note that the announcer does not follow equilibrium strategy described in section 4.2. In particular, in all sessions, announcers actually chose action A more frequently than action B, even though in this experiment $z > 50$ (see Table 4.B1 in Appendix B). The probability of announcement A being correct (average over all sessions and stages) is 43 percent, while the average probability of announcement B being correct is 68 percent.

We ask whether an announcement of “attack” or “A” significantly increases the probability of attacking in the first stage. Recall that, theoretically, the prediction of unique equilibrium dictates that the announcement should have no effect on actions in the first stage. Figure 4.2 shows the average probability of attack in the first stage with an announcement of A and with an announcement of B.

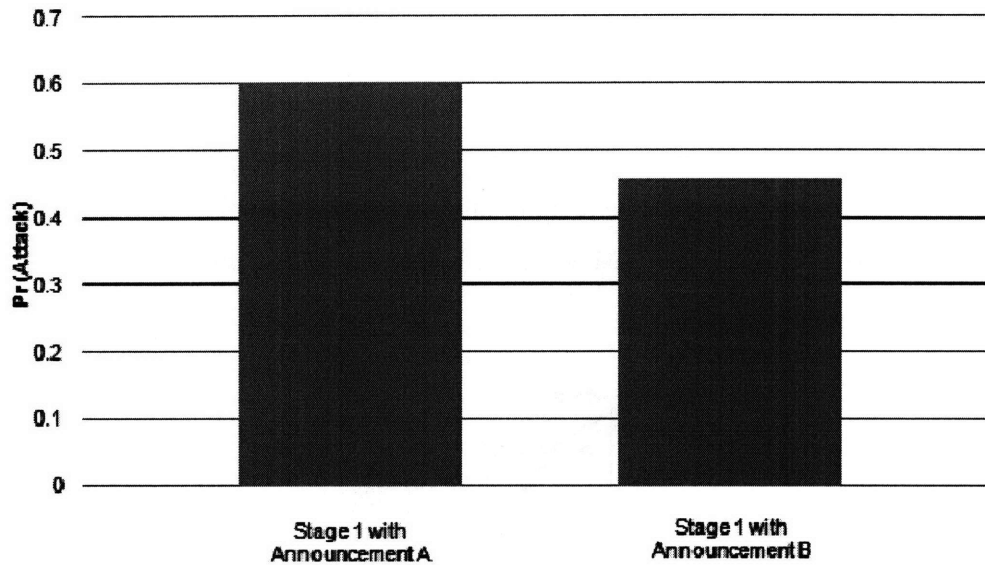


Figure 4.2: Average Probability of Attack in Stage 1 (Session 1)

The above figure shows that there is a slight decrease in aggressiveness in the rounds where the announcement is B rather than A. Note that some of the difference results from the average realization of θ in the rounds with the announcement of A being slightly lower than the average θ with the announcement of B (60 vs. 68, respectively). In order to see whether the announcement has a statistically significant effect on the probability of attacking, we ran subject-level regressions of each individual's action on the announcement dummy and the private signal, x . Since we are seeking to analyze the effects of the announcement on stage-one actions, we only use data from sessions where the announcement was revealed in the first stage. We use ordinary least squares to estimate the effects. The results are reported in Table 4.3.¹²

¹²In all the regressions, we use non-announcer data only, since the announcers' actions do not influence the outcome of the coordination game.

Table 4.3.

Stage 1 Individual Level Regressions for Probability of Attack (Data for Session 1)

Variable	Dependent Variable: Action		
	1	2	3
Announcement	0.0787 (0.0545)	0.0873* (0.0486)	0.0417 (0.0421)
Private signal, x		-0.0050*** (0.0003)	-0.003*** (0.0004)
Belief			0.0389*** (0.0045)
R^2	0.44	0.62	0.71
No. of observations	450	450	450

Note: Subject and round fixed effects included, Robust standard errors in parentheses.

Significance levels: *10%, ** 5%, ***1%.

In the specification in column 1, the announcement dummy has no statistically significant effect on the probability of attack. When we add the other main explanatory variable, private signal x , we see that the effect of x on the choice of action is negative, as the theory predicts, and statistically significant at the 1 percent confidence level. The inclusion of the private signal also makes the effect of announcement on the probability of attack marginally statistically significant: an announcement of “attack” increases the probability of attack by approximately 8.7% on average at the 10% significance level.

However, once we add the subjects’ beliefs about the size of the attack into the regression, the significance of the announcement dummy disappears while the belief variable has the expected effect on the probability of attack. If the subject believes that more other participants are going to be choosing action A, she is also more likely to choose action A – the definition of strategic complementarity.

We can see therefore that the announcement impacts observed behavior in the first stage only through its effect on the beliefs of subjects about others’ actions. Table 4.4 further reinforces the idea that subjects exhibit this type of equilibrium reasoning.

Table 4.4.
Stage 1 Individual Level Regressions for Beliefs (Data for Session 1)

Variable	Dependent Variable:	
	Belief	
	1	2
Announcement	1.0847* (0.5794)	1.1740** (0.5357)
Private signal, x		-0.0523*** (0.0042)
R^2	0.39	0.55
No. of observations	450	450

Note: Subject and round fixed effects included,
Robust standard errors in parentheses.
Significance levels: *10%, ** 5%, ***1%.

Note that this finding that the announcement has an effect on the beliefs of the subjects is not consistent with the theoretical prediction of unique equilibrium in the first stage of the model. One can think of explaining this result with a model that incorporates optimism in agents' beliefs about others' actions (see for example, Izmalkov and Yildiz 2007).

4.4.3 Second Period Predictions

So far, we have analyzed the data from the first stage of the experiment. In this section, we will focus on the effect of the announcement on second-stage behavior. There are two informational treatments. In the main treatment, the subjects are provided with a new more precise private signal in the second stage.

Effect of Announcement with New Information

Here, the theory predicts that a new attack becomes possible, given that the parameters have been chosen appropriately. In the presense of multiplicity, the announcement allows the agents to coordinate of a different course of action depending on the announcement. Recall that the model predicts that, if the second-stage environment were characterized by a unique equilibrium, the announcement would have no significant effect on subjects' actions, just as we

saw in the first stage. Figure 4.3 contrasts the average probability of action A in the two stages of the experiment for the cases where the announcer chose action A and where the announcer chose action B. Note that the figure was constructed using only the rounds that continued into the second stage. This allows us to make the clearest possible comparison across treatments.

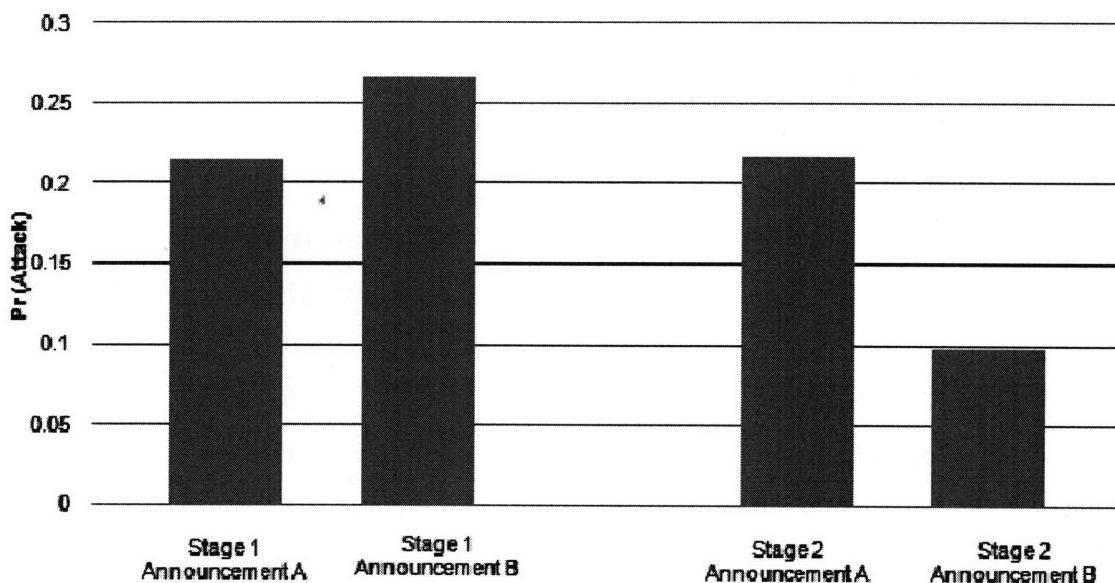


Figure 4.3: Average Probability of Action A for Rounds with Announcement of A and with Announcement of B, New Information Treatments

First, note that the average probabilities of action A in the first stage are close in magnitude: 0.215 for the situations where the announcer chose A and 0.267 for the situations where the announcement was B. The announcement does not play a big role in the first stage where the theory predicts a unique equilibrium. However, the story is quite different in the second stage. We find that the average probability of action A in the second stage under an announcement of A is significantly higher (0.218) than the probability of action A under an announcement of B (0.098). In fact, the probability of attack is more than doubled by an announcement of A as compared to the announcement of B.

We confirm this result by running an individual-level regression of action in the second stage on the announcement and other controls. Table 4.5 reports results using only the data

for sessions with announcement in the second stage.

Table 4.5.
Stage 2 Individual Level Regressions (NI Sessions)

Variable	Dependent Variable	
	Action	
	1	2
Announcement	0.0885** (0.0383)	-0.0315 (0.0275)
Private signal, x	-0.0042*** (0.0005)	-0.0004 (0.0005)
Belief		0.0572*** (0.0046)
R^2	0.45	0.68
No. of observations	540	540

Note: Subject fixed effects included, Robust standard errors in parentheses.

Significance levels: *10%, ** 5%, ***1%.

Consider the first column in Table 4.5. The coefficient on the announcement dummy is positive and statistically significant which implies that the announcement does allow agents to coordinate on different courses of action. That is, an announcement of action A makes it more likely that subjects choose action A in the second stage, which supports the model's prediction 3. Introducing the belief variable into the regression equation suggests that the effect of the announcement in the second stage goes through the subjects' expectations of others' actions. In particular, the subjects believe that the announcement will act as a coordination device for other subjects, and this why they pay attention to it themselves. Table 4.6 confirms that this equilibrium reasoning argument.

Table 4.6.

Stage 2 Individual Level Regressions for Beliefs (Data for NNI Sessions)

Variable	Dependent Variable:	
	Belief	
	1	2
Announcement	1.077** (0.4430)	2.098** (0.3888)
Private signal, x		-0.0675*** (0.0053)
R^2	0.31	0.57
No. of observations	540	540

Note: Subject and round fixed effects included,

Robust standard errors in parentheses.

Significance levels: *10%, ** 5%, ***1%.

An announcement of A increases the expected size of the attack in the subject's mind by 2 people. This effect is statistically significant.

Effect of Announcement with No New Information

We now need to verify that the announcement plays an important role only in the environments where information creates a possibility of a new attack and therefore where there is a need for a coordination mechanism. It is possible that, instead, the announcement *always* matters in the second stage, regardless of information. In order to explore this issue, we conduct a control treatment where subjects do not receive a new signal in the second stage. Here once again the theory predicts a unique equilibrium with no one attacking. Hence the announcement should not have a significant effect on the probability of attack in this case. In fact, without new information, we should still see a great reduction in aggressiveness going from the first into the second stage. Furthermore, we should see no difference between rounds with and without the announcement. Figure 4.4 demonstrates that in the control treatment with no new information in the second stage, the announcement has no effect on the average probability of attack. The reduction in aggressiveness across the two stages is consistent with the model's prediction 2.

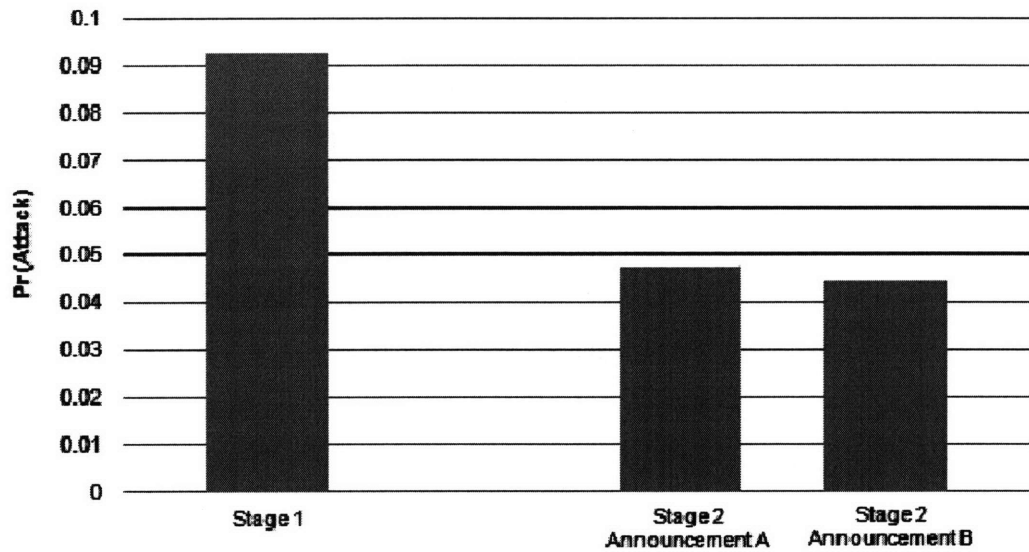


Figure 4.4: Average Probability of Action A with No New Information in the Two Stages,
No New Information Treatments

Table 4.7 below presents the results of an individual-level regression that are consistent with the observation we made in Figure 4.4. Namely, the announcement does not have a statistically significant differential effect on actions in the second stage without new information.

Table 4.7.

Stage 2 Individual Level Regressions (NNI Session)

Variable	Dependent Variable: Action		
	1	2	3
Announcement	-0.0003 (0.0235)	-0.2218 (0.2225)	-0.1906 (0.1754)
Private signal, x		-0.0005 (0.0015)	-0.0003 (0.0012)
Belief			0.0563*** (0.0113)
R^2	0.28	0.29	0.57
No. of observations	195	195	195

Note: Subject and round fixed effects included, Robust standard errors in parentheses.

Significance levels: *10%, ** 5%, ***1%.

In all specifications, the announcement dummy has no statistically significant effect on the probability of attack. In fact, the only variable that matters for the willingness of subjects to attack again in the second stage seems to be the belief variable. This result is consistent with previous findings that suggest that excess aggressiveness in actions stems from excess aggressiveness in beliefs about others' actions (see chapter 1 of this thesis).

4.5 Conclusion

This chapter uses an uninformative announcement to detect multiplicity and to see whether coordination on multiple courses of action is reached in a laboratory experiment. We find that the announcement has a small effect in terms of significance and magnitude in the situations where the theory predicts a unique equilibrium. On the other hand, the announcement seems to play an important role for equilibrium selection in the situations where multiple courses of action are theoretically possible. In all cases, the announcement affects individual behavior through its effect on the beliefs of the players about others' actions. This result confirms the

importance of equilibrium reasoning in collective action situations.

4.6 Appendix A: Derivations for Section 4.2

Derivations and proofs for the agents' strategies can be found in chapter 1 of this thesis.

To derive equation (4.4), recall that the announcer will only choose an announcement of A in the first stage if the payoff from that announcement exceeds the payoff from an announcement of B. That is, if

$$\begin{aligned} \Pr[\theta < \theta_1^*(z)|z]y + (1 - \Pr[\theta < \theta_1^*(z)|z])(-y) &\geq \\ \Pr[\theta < \theta_1^*(z)|z](-y) + (1 - \Pr[\theta < \theta_1^*(z)|z])y & \end{aligned}$$

where the left-hand side represents the payoff from choosing an announcement of A in the first stage.

In order to find the optimal strategy, we set the two sides of the above expression equal and rearrange terms producing the condition in equation (4.4).

The derivation of equation (4.7) is completely analogous. The announcer will only choose an announcement of A in the second stage if the payoff from that announcement exceeds the payoff from an announcement of B. That is, if

$$\begin{aligned} \Pr[\theta < \theta_2^*(z)|z, \theta_1^*(z)]y + (1 - \Pr[\theta < \theta_2^*(z)|z, \theta_1^*(z)])(-y) &\geq \\ \Pr[\theta < \theta_2^*(z)|z, \theta_1^*(z)](-y) + (1 - \Pr[\theta < \theta_2^*(z)|z, \theta_1^*(z)])y & \end{aligned}$$

where the left-hand side represents the payoff from choosing an announcement of A in the second stage.

In order to find the optimal strategy, we set the two sides of the above expression equal and rearrange terms producing the condition in equation (4.7).

4.7 Appendix B: Tables and Figures

Table 1.B1.
Descriptive Statistics.

	Session 1	Session 2	Session 3
% Announcements of Action A	67%	53%	50%
% Successful Announcements of Action A	55%	38%	30%
% Successful Announcements of Action B	70%	71%	60%
Min θ	-39.23	-14.30	2.64
Max θ	170.15	181.54	180.33
Mean θ	62.76	88.02	88.57
Min x (Stage 1)	-63.99	-35.50	-15.89
Max x (Stage 1)	186.46	203.45	203.67
Mean x (Stage 1)	62.80	88.08	88.84
Mean # Attackers (Stage 1)	8.33	5.53	5.55
Mean # Attackers (Stage 2)	3	1.95	0.69
Mean Belief (Stage 1)	7.05	4.83	4.80
Mean Belief (Stage 2)	4.35	2.11	1.63
% Successful Attacks (Stage 1)	47%	33%	35%
% Successful Attacks (Stage 2)	0%	5%	0%
% Female	44%	41%	44%
Median Comfort Level with Stats and Probability (out of 5)	4	3.5	4
Number of subjects	32	32	16

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