Bank Runs as Pure Coordination Failures: Experimental Evidence and Endogenous Evolutionary Learning*

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Abstract

This paper provides an experimental investigation of how the difficulty of coordination – measured by a *coordination parameter* – affects the occurrence of bank runs as pure coordination failures. We find that there is a critical value of the coordination parameter that serves as the watershed for coordination. When the parameter is below (above) the critical value, experimental economies stay close to or converge to the non-run (run) equilibrium. When the parameter is equal to the critical value, experimental economies' outcomes vary widely. We introduce an '*endogenous*' evolutionary algorithm to account for the behavior of human subjects observed in the laboratory.

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1 Introduction

One important function of a bank is to pool depositors' resources and invest in profitable (illiquid) long-term assets. At the same time, a bank issues short-term interest-bearing demand deposits to meet depositors' liquidity needs. The demand deposit contract improves social welfare by providing a type of insurance which allows depositors with liquidity needs to earn interest on their deposits and share the proceeds from long-term investment.

An unappealing feature of the demand deposit contract is that it is associated with multiple self-fulfilling equilibria and opens the gate to bank runs in which a large number of depositors 'run' to the bank to withdraw money even in the absence of liquidity needs. Whether optimal risk sharing can be achieved hinges critically on the depositor's expectations about other depositors' actions. In the 'good' equilibrium, only those with liquidity needs (*impatient consumers*) withdraw early, earning returns higher than what liquidating the long-term asset entitles them to. Those who do not need liquidity (*patient consumers*), expecting other patient consumers to do the same, wait until the long-term asset matures, earning returns lower than the rate of return of the long-term asset (but higher than the return to impatient consumers). In this equilibrium, optimal risk sharing is achieved by a transfer of consumption from patient consumers to impatient consumers which improves the *ex ante* welfare of depositors. In the 'bad' equilibrium, however, expecting other patient consumers to do the same, every patient consumer 'runs' to the bank to withdraw money, and the bank is forced to liquidate its profitable long-term investment at fire sale prices in order to honor the demand deposit contract. In this case, profitable long-term projects are interrupted, risk sharing is destroyed, and the welfare outcome is worse than that in the autarky where the bank does not exist.

Existing theoretical literature on bank runs can be broadly classified into two categories. The first view is that bank runs are the result of pure coordination failures when the only source of uncertainty is strategic uncertainty, or uncertainty about the choices of other depositors. In particular, bank runs may occur when depositors coordinate their withdrawing decisions on the realization of a commonly observed 'sunspot' variable unrelated to fundamentals. This view is initially formalized by Diamond and Dybvig (1983, hereafter DD), and is substantiated later by Waldo (1985), Loewy (1991), Cooper and Ross (1998), and Peck and Shell (2003). The second view is that bank runs occur only when there is uncertainty about fundamentals and are triggered by unfavorable noisy signals about the fundamentals. For example, according to Chari and Jagannathan (1988), Jacklin and Bhattacharya (1988), Alonso (1996), Allen and Gale (1998), Loewy (1998), Chen (1999), Morris and Shin (2000), Yorulmazer (2003), Goldstein and Pauzner (2005) and Gu (2007), bank runs occur when depositors receive adverse (noisy) news about the quality of the bank's long-term assets. Champ, Smith and Williamson (1996), Smith (2003), Loewy (2003), and Gomis-Porqueras and Smith (2006) attribute bank runs to uncertainty in aggregate liquidity needs.

Empirical investigation of the likely causes of bank runs is hampered by the lack of data on the expectation and behavior of individual depositors in the real-world bank runs. To circumvent the problem of lack of field data, we carry out an experimental study about whether bank runs can occur as the result of pure coordination failures. Instead of suggesting 'sunspots' as the coordination device, we hypothesize that whether or not bank runs occur depends upon the *coordination parameter*, defined as the percentage of depositors required to leave money in the bank until assets mature so that the strategy achieves a better payoff than withdrawing early.[1] More difficult coordination is represented by a higher value of the coordination parameter. To conduct the experiments, we recruit human subjects to play a repeated one-shot 10×2 (there are 10 subjects and 2 actions, withdrawing money or leaving money in the bank) game as described by a demand deposit contract. The game has two symmetric Nash equilibria: the good or non-run equilibrium, and the bad or run equilibrium. The experimental results demonstrate that the coordination parameter acts as a coordination device, and bank runs occur as a result of pure coordination failures if and only if coordination is difficult. In particular, there is a critical value of the coordination parameter that defines the watershed for coordination. When the coordination parameter is less than 0.7, most subjects perceive that it is easy to coordinate and choose to leave money in the bank, and the experimental economies stay close to or converge to the non-run equilibrium. When the coordination parameter exceeds 0.7, most subjects perceive coordination to be too difficult and choose to withdraw early, and the experimental economies stay close to or converge to the run equilibrium. However, when the coordination parameter is equal to 0.7, the consensus breaks down and we observe wide variation in subjects' choices across different sessions. Our experimental results thus suggest that bank runs may occur as a result of pure coordination failures when depositors perceive difficult coordination.

The importance of the coordination parameter is discussed by Temzelides (1997). He introduces the evolutionary algorithm specified in Young (1993) and Kandori, Mailath and Rob (1993) into a repeated version of the DD model. Depositors play myopic best response with inertia and experiment with a certain probability to 'flip' their strategies. For a given experimentation rate, the model yields a stochastic dynamical system that defines a Markov chain on the finite state space with a unique invariant distribution. Temzelides (1997) proves that when the probability of experimentation approaches zero, the economy stays at the non-run equilibrium with probability one if and only if withdrawing late is risk dominant, or when the coordination parameter is less than 0.5. Although Temzelides (1997) notes the importance of the coordination parameter, the evolutionary algorithm adopted in the paper fails to capture the correct threshold value of the coordination parameter. The paper predicts 0.5 as the critical point, while our experimental results suggest 0.7 as the watershed for coordination. This is because the parameters of the algorithm in Temzelides (1997) are exogenously given. Every period, a fixed fraction of depositors play myopic best response, following the strategy that worked best in the previous period. The experimentation rate is also exogenously given. However, we know in general that subjects in experiments do not respond to the environment in a mechanical way. For example, whether they choose to play the best response may depend on whether or not they have enough information to determine what the best strategy in the previous period was. The probability of experimentation may depend on the current state of play and the potential for obtaining higher payoffs in case of experimentation, etc. In order to address these issues, we introduce what we call the '*endogenous*' evolutionary algorithm in which updating depends upon agents' information set. Whether

[1]We do not investigate the sunspot explanation of bank runs because sunspot behavior is rarely observed in controlled experiments with human subjects. To the best of our knowledge, there has been no laboratory evidence of 'sunspots' in games with multiple equilibria that can be Pareto ranked. Duffy and Fisher (2005) provide the first direct evidence of sunspots in the laboratory. However, they purposefully make the two equilibria not Pareto comparable because they suspect that if one equilibrium were Pareto dominant, subjects might coordinate on it as a focal point for their expectations. In a bank-run model, the two equilibria are clearly Pareto rankable.

or not an agent plays best response and the probability of experimentation are endogenously determined by the coordination parameter and what the agent can infer about the number of late withdrawals in the previous period. We estimate the experimentation rates from the experimental data. Simulations using the estimated experimentation rate successfully capture 0.7 as the watershed for coordination and generate results similar to those observed in the laboratory.

Our paper is a new addition to the experimental literature on bank runs. Madiès (2006) focuses on the solution to preventing bank runs. Klos and Sträter (2007) test the prediction of the global game theory of bank runs based on the model in Goldstein and Pauzner (2005). Garrat and Keister (2009) study how depositors' decisions are affected by uncertainty about the fundamental withdrawal demand and by changing the number of opportunities subjects have to withdraw. Schotter and Yorulmazer (2009) investigate bank runs in a dynamic context and examine the factors that affect the speed of withdrawals. We study bank runs from a different angle and focus on the effect of difficulty of coordination.

At the same time, the study in the paper offers additional insight into the experimental literature on coordination games which feature multiple Nash equilibria that can be Pareto ranked. In particular, we investigate how the outcomes of such games are affected by the difficulty of coordination. This insight complements previous work which studies the effects of the number of subjects (Battalio, Beil and Van Huyck, 1990, 1991), and the payoff differential between different equilibria (Battalio, Beil and Van Huyck, 2001; Cabrales, Nagel and Armenter, 2007).[2]

The paper proceeds as follows. Section two outlines the model that underlies the experimental studies. Section three describes the experimental design. Section four presents and discusses the experimental results. The 'endogenous' evolutionary algorithm is introduced in section five. In section six, we estimate the probability of experimentation and use the evolutionary algorithm to simulate the time path of the number of late withdrawals. We then compare the simulation outcomes with the experimental results. Section seven concludes.

2 The Theoretical Framework

The theoretical framework that underlies our experimental studies is the baseline DD model, which is in essence a pure coordination game where the only uncertainty is strategic uncertainty, or uncertainty concerning other depositors' actions. We provide a brief discussion of the model in the following.

There are three dates (indexed by 0,1, and 2) and a single homogeneous good. There are D *ex ante* identical agents in the economy. At date 0 (planning period), each agent is endowed with 1 unit of good and faces a preference/liquidity shock that determines their types. Liquidity shocks are realized at the beginning

[2] Heinemann, Nagel and Ockenfels (2004), and Duffy and Ochs (2009) test the prediction of the global game theory where agents receive noisy private information about a payoff relevant variable, but they both include an investigation of the counterpart economies where the information is public and multiple symmetric Nash equilibria exist. As in our study, subjects are able to choose one of the equilibria, and the payoff relevant variable serves as a coordination device to resolve the multiplicity. The papers differ in ours in that a change of the payoff relevant variable causes both the difficulty of coordination and the payoff differential between two equilibria to change. This simultaneous movement makes it hard to disentangle the effect of the two factors. Our experimental design keeps the payoff differential fixed and allows us to focus on the effect of the difficulty of coordination.

of date 1. Among the D agents, N of them become patient agents who are indifferent between consumption at date 1 and date 2, and $D-N$ agents become impatient agents who care about only consumption at date 1. Realization of liquidity shocks is private information. Preferences are described by the state-dependent utility function

$$U(c_1, c_2) = \begin{cases} u(c_1) & \text{if impatient,} \\ u(c_1 + c_2) & \text{if patient,} \end{cases}$$

where c_1 and c_2 denote the consumption at date 1 and date 2, respectively. The function $u(\cdot)$ satisfies $u'' < 0 < u'$, $\lim_{c\to\infty} u'(c) = 0$, and $\lim_{c\to 0} u'(c) = \infty$. The relative risk aversion coefficient $-cu''(c)/u'(c) > 1$ everywhere. There is a productive technology that transforms 1 unit of date 0 output into 1 unit of date 1 output or $R > 1$ units of date 2 output.

At the socially optimal allocation, impatient agents consume only at date 1 and patient agents consume only at date 2. Let c_i and c_p denote the consumption by impatient consumers and patient consumers, respectively. The optimal choice of (c_i, c_p) maximizes the *ex ante* welfare of a representative agent and solves the problem

$$\max_{c_i, c_p} \left(1 - \frac{N}{D}\right) u(c_i) + \frac{N}{D} u(c_p)$$

subject to

$$\left(1 - \frac{N}{D}\right) c_i + \frac{N}{D}\frac{c_p}{R} = 1.$$

The solution is $1 < c_i^* < c_p^* < R$. A bank, by offering demand deposit contracts, can support the optimal risk-sharing allocation. The contract requires agents to deposit their endowment with the bank at date 0. In return, agents receive a bank security which can be used to demand consumption at either date 1 or 2. The bank promises to pay $r > 1$ to agents who demand consumption at date 1. If the bank does not have enough money to fulfill its promise, it divides the available resource evenly among depositors who demand consumption.[3] Investment left after paying early withdrawers generates a rate of return $R > r$ and the proceeds are shared by all late withdrawers at date 2. The deposit contract can thus be described by

$$\begin{aligned} c_e &= \min\left\{r, \frac{D}{D-z}\right\} \\ c_\ell &= \max\left\{0, \frac{D - r(D-z)}{z} R\right\} \end{aligned}$$

where c_e and c_ℓ are the payoffs to early and late withdrawers, respectively, and z is the number of depositors who choose to withdraw late.

There are two symmetric Nash equilibria with the demand deposit contract. One is the non-run equilibrium, which occurs when each impatient agent withdraws at date 1, while each patient agent anticipates all other patient agents will withdraw at date 2, and thus also waits until date 2. At this equilibrium, the optimal

[3]The baseline DD model has a sequential service constraint (SSC). The SSC is not essential to generating multiple equilibria; the fact that $r > 1$ is sufficient to generate a payoff externality and panic-based runs even in the absence of the SSC. We abstract from SSC for simplification.

risk sharing allocation is achieved if r is set to c_i^*. There is also another equilibrium (the run equilibrium) when each patient agent assumes all other patient agents withdraw at date 1, and thus also finds it optimal to withdraw at date 1. In this case, all consumers end up with 1 unit of consumption. and the allocation is even worse than that in the autarky where the bank does not exist. The expectation or belief of the depositors thus plays a critical role in determining the equilibrium outcome.

For the experiments, we keep the important features of the baseline DD model. Depositors play a pure coordination game where the only uncertainty is strategic uncertainty. The game has two symmetric pure strategy Nash equilibria: the run equilibrium and the non-run equilibrium. There is a payoff externality in the sense that each player's action affects the payoff of other players. To facilitate the experimental design, we deviate from the original DD model along two dimensions. First, in the baseline DD model, there are both patient and impatient agents. Impatient agents always withdraw early and only patient agents are "strategic" players. Here we will focus on "strategic" players so we let $D = N$. Second, instead of calculating the optimal short-term interest rate r (as a function of depositors' preferences, the fraction of depositors who need liquidity at date 1, and the rate of return of the bank's long-term investment R), we simply set r to be a value that is greater than 1. In this paper, we intend to study how the difficulty of coordination affects the outcome of the coordination game implied by the demand deposit contract. Using r as a controlled parameter allows us to capture the difficulty of coordination in a simple way (see the next section).[4] With the simplification, the demand deposit contract can be characterized by

$$c_e = \min\left\{r, \frac{N}{N-z}\right\}; \tag{1}$$

$$c_\ell = \max\left\{0, \frac{N - r(N-z)}{z} R\right\}. \tag{2}$$

3 Experimental Design

The experiments are conducted in a laboratory environment where human subjects play a coordination game as described in the previous section. Our main objective is to examine whether or not bank runs can be the result of pure coordination failures when strategic uncertainty is the only source of uncertainty. The hypothesis is that agents' expectations and actions will depend on how difficult the coordination task is, and bank runs occur if and only if coordination becomes difficult. We can determine the difficulty of coordination by asking the following question: what fraction of depositors should withdraw late (at date 2 when the bank's long-term asset matures), so that the strategy achieves a better payoff than withdrawing early (at date 1)? We call this fraction the *coordination parameter* and denote it by η. A higher value of η represents more difficult coordination. We can solve η in two steps. First, solve the value of z, the number

[4] For optimal contracting in the DD framework, please refer to Green and Lin (2000, 2003), Andolfatto, Nosal and Wallace (2007), Andolfatto and Nosal (2008), and Ennis and Keister (2009a, 2009b, 2009c). The first few papers show that the multiple equilibria result goes away if more complicated contingent contracts – as compared with the simple demand deposit contracts in DD – are used. The three papers by Ennis and Keister show that the multiple equilibrium result is restored if the banking authority cannot commit to not intervene in the event of a crisis, or the consumption needs of agents are correlated.

of depositors who choose to withdraw late, that equalizes the payoffs to early and late withdrawers:

$$r = \frac{N-(N-z)r}{z}R;$$

and denote it by z^*. Thus, z^* is given by:

$$z^* = \frac{R(r-1)}{r(R-1)}N.$$

Second, divide z^* by N to get η, the fraction of depositors who withdraw at the second date, that equalizes the payoffs to early and late withdrawers:

$$\eta = \frac{z^*}{N} = \frac{R(r-1)}{r(R-1)}.$$

In the experiments, we set $N = 10$ and $R = 2$. Variation in the coordination parameter η is achieved by changing r. Fixing R allows us to fix the payoff differential between the two symmetric Nash equilibria to $(R-1)$, and concentrate on the effect of the coordination parameter. Note that we could follow the DD model more strictly to have liquidity shocks and set r optimally. In that case, η would be defined as the fraction of patient consumers who need to withdraw late to equalize the payoffs to early and late withdrawers; it can be calculated as $\frac{R(r-1)}{r(R-1)}\lambda$ where λ is the fraction of depositors who need liquidity at date 1, and r is determined endogenously by R, λ and depositors' preferences. Doing so, however, would incur two complications. First, we would need to make an implicit strong assumption that we know the preferences of depositors so that we can set r in an optimal way. Second, to generate different values of η while maintaining the optimality of the demand deposit contract would require changing R and r at the same time. This would change the payoff differential between the two Nash equilibria and introduce another factor that may affect the coordination results (see Battalio, Beil and Van Huyck, 2001; Cabrales, Nagel and Armenter, 2007). Then we need to disentangle the effects of difficulty of coordination and the payoff differential. To avoid these complications, we fix R and let r vary.

The program used conduct the experiments is written in z-Tree (Fischbacher 2007). In each session, 10 subjects (enrolled from graduate and upper level undergraduate economics classes) are each assigned a computer terminal through which they can input their decisions (see the Appendix for the experimental instructions). Communication among subjects is not allowed during the experiments.

Each experimental session consists of 8 phases, each phase is characterized by a different r or η, and each phase lasts for 10 periods or rounds (see table 1). Every subject begins each period with 1 experimental dollar (ED) in the bank and then makes a decision to withdraw or to leave money in the bank. Payoff tables for all 8 phases are provided to list the payoff that an individual will receive if he/she chooses to withdraw early or to leave money in the bank given that $n = 1 \sim 9$ of the other 9 subjects choose to withdraw early. The payoff tables help to reduce the calculation burden of the subjects so that they can focus on playing the coordination game and forming expectation about what other subjects will do. Once all subjects make their decisions, the total number of late withdrawers is calculated. Each subject's payoff is then determined by equations (1) and (2). Each subject is presented with the history of his/her own actions, payoffs, and cumulative payoffs for the current period and all previous periods (measured in EDs). A message is broadcast on the subjects' computer screens each time r is changed. The first 10 periods (phase 0) are for trial purposes so that the subjects can familiarize themselves with the task to be performed. Subjects are paid only for the 7 formal phases. After the experiment, the total payoff that each subject earns is converted into cash.

A total of 8 experimental sessions are conducted. For robustness check, we run experiments at different places and with different ordering of the coordination parameter. The experiments are run at three locations: Simon Fraser University (SFU), Burnaby, Canada; University of Manitoba (UofM), Winnipeg, Canada; and University of International Business and Economics (UIBE), Beijing, China. Among the 8 sessions of experiments, four have the coordination parameter increasing over time and four have it decreasing. The conversion rate used to pay subjects is 1 $ED = 0.2\ CAD$ at SFU and UofM, and 1 $ED = 0.8\ RMB$ at UIBE. The conversion rates from ED to local currency are set such that participants on average earn about 1.5 times as much as they earn as tutors. All experiments are run in English. To ensure that the subjects in Beijing have sufficient English reading and listening skills, we restrict our subject pool to those who have passed the College English Test Grade IV, a standardized national English language test for college students in China. We also ensure that the subjects understand the experimental instructions by giving them 10 trial periods and several opportunities to ask questions before the formal experiments begin.

4 Experimental Results

In this section, we present and discuss the experimental results. Figure 1 plots the path of z, the number of late withdrawals, for the 8 sessions of experiments. Table 2 lists the starting, terminal, and mean values of the number of late withdrawals (denoted as S, T, and M respectively) for each value of η (or each phase of a session). To characterize the performance of the experimental economies, we define eight performance categories as described in table 3. Table 4 categorizes the performance of the 8 sessions of experiments. In SFU1, UIBE1, UIBE3, and UofM1, η increases from 0.1 to 0.9; in SFU2, UIBE2, UIBE4, and UofM2, η decreases from 0.9 to 0.1. Two observations stand out.

Finding 1. *There is more coordination at late withdrawal when coordination is easier.*

Note (in figure 1) the downward trend in the four experiments (left panel) with increasing η and the upward trend in the four experiments (right panel) with decreasing η. As shown in table 2, the average number of late withdrawals tends to decrease with η.

Finding 2. *When coordination is easy (hard), subjects tend to coordinate at the non-run (run) equilibrium. The consensus breaks down when η is equal to* 0.7.

As shown in table 4, when coordination is easy (when η is 0.1, 0.2, 0.3, and 0.5), the economy stays close to (or fairly close to) or converges to the non-run equilibrium; when coordination is difficult (when η is 0.8 and 0.9), the economy stays close to (or fairly close to) or converges to the run equilibrium.[5] When

[5]When $\eta \leq 0.5$, all economies are marked by NN, FN or CN. When $\eta \geq 0.8$, all economies are marked by RR, FR or CR.

$\eta = 0.7$, the experimental results are very different across the 8 sessions. UIBE1 starts with 6 subjects who coordinate on late withdrawals. This number proceeds up to 8 and then back to 6. This session is marked by "H". SFU1 begins with a high level of coordination with 8 subjects withdrawing late at first, but the number of late withdrawals goes down to 2. UIBE3 and UofM1 start with 7 subjects who coordinate on late withdrawals, but the number of late withdrawals eventually goes down to 0. These three sessions are marked by "CR". UIBE2, UIBE4, and UofM2 begin with very low coordination and stay there; they are marked by "RR". SFU2 starts with 6 subjects who coordinate on late withdrawal. This number goes up to 8. This session is marked by "CN". The experimental results thus suggest $\eta = 0.7$ as a watershed for coordination. When $\eta < 0.7$, most subjects perceive that it is easy to coordinate and choose to leave money with the bank. When $\eta > 0.7$, most subjects perceive coordination to be too difficult and choose to withdraw early. When $\eta = 0.7$, the consensus breaks down and various outcomes are possible.

The result is similar to the global game literature in that we observe a shift from one equilibrium to another for a certain threshold of a payoff relevant state variable (here r or η). What is different is that we study the case of complete information where the payoff function is common knowledge, and the only source of uncertainty is the strategic uncertainty. In this case, multiple equilibria continue to exist according to the logic of Morris and Shin (1998) and DD. In the experiments, however, we do not see multiple equilibria and the subjects are able to consistently resolve the multiplicity in favor of one of the two equilibria with the coordination parameter serving as the coordination device. Similar results are found by Heinemann, Nagel and Ockenfels (2004) and Duffy and Ochs (2009).[6] In these two papers, the payoff relevant variable is comparable to R in our model. A change in R induces a change in both the payoff differential and the difficulty of coordination, and makes it difficult to separate the effects of the two factors. Our experimental design keeps the first factor fixed, and allows us to concentrate on the effect of the difficulty of coordination.

Besides the two major findings above, the experimental results also show that the coordination parameter affects the pattern of intraphase learning: the learning effect is stronger for intermediate values of η. As shown in table 4, for intermediate values of η (0.5, 0.7, and 0.8), it takes time for some economies to reach the equilibria as subjects learn from plays in the previous rounds. For example, when η is 0.5, two sessions are marked by "CN" (please refer to table 3 for definition of the performance categories); when η is 0.7, four sessions are marked by either "CN" or "CR"; when η is 0.8, three sessions are marked by "CR". When η takes extreme values (0.1, 0.2, 0.3, and 0.9), the learning effect is weak in the sense that the experimental economies reach either the run or non-run equilibrium instantly. When η is 0.1, 0.2, or 0.3, all sessions are marked by "NN"; when η is 0.9, seven sessions are marked by "RR" or "FR".

To ensure that the experimental results are robust, we try different ordering of the coordination parameter and run the experiment in two countries. A quick examination of figure 1 and table 4 shows that the experimental results are robust to the change of ordering of η and locations. When $\eta = 0.1$, 0.2, and 0.3, all eight sessions stay close to the non-run equilibrium and are marked by "NN" in table 4. When $\eta = 0.5$, all session settle down to the non-run equilibrium (marked by "NN", "FN" or "CN"). When $\eta = 0.8$ and 0.9,

[6]Both papers compare the performance of experimental economies under two information scenarios. In the first scenario, agents receive private noisy signals about the payoff relevant variable. In the second scenario, the information is public. They find that the information structure does not have significant effect on the performance of the economy.

all sessions settle down to the run equilibrium (marked by "RR" or "CR"). When $\eta = 0.7$, for the increasing order, we have one session with moderate high coordination (marked by "H" and conducted in China) and three sessions marked settling down to the run equilibrium (marked by "CR"). When η takes the decreasing order, we have one session settling down to non-run equilibrium (marked by "CN" and conducted in Canada) and three sessions staying close to the run equilibrium (marked by "RR").

Finally, we would like to point out here that although we study the role of the difficulty of coordination in the context of a bank run model, the findings should be applicable to more general ‘entry’ games (games with complete information with strategic complementarity). Our experimental investigation thus complements previous research that studies how the number of subjects (Battalio, Beil and Van Huyck, 1990, 1991) and the payoff differential between different equilibria (Battalio, Beil and Van Huyck, 2001; Cabrales, Nagel and Armenter, 2007) affect the outcomes of entry games.

5 The Evolutionary Algorithm

Evolutionary algorithms have been introduced as an equilibrium selection mechanism into many rational expectation models with multiple equilibria and have greatly contributed to the understanding of these models. Temzelides (1997) introduces the evolutionary algorithm (developed by Young, 1993; and Kandori, Mailath and Rob, 1993) into a repeated version of the DD model.[7]

The algorithm has two main components. The first component is myopic best response with inertia. Myopic best response means that when agents react to the environment, they respond by choosing the strategy that performed better in the previous period. In the context of the DD model, the myopic best response is to withdraw early if $z_{t-1} \leq z^*$ and to withdraw late otherwise. There is inertia in the sense that not all agents react instantaneously to the environment. The second component is experimentation. There is a probability δ that agents change their strategies at random. In the context of the DD model, experimentation involves flipping strategies from early to late withdrawal or vice versa.

In the standard version of the algorithm, the probabilities of engaging in best response and experimentation are governed by some exogenous process. For example, in the algorithm used in Temzelides (1997), agents play myopic best response and carry out experimentation with fixed probabilities which are iid across subjects and time. Temzelides (1997) proves that as the (fixed) probability of experimentation approaches zero, the economy stays at the non-run equilibrium with probability one if and only if withdrawing late is risk dominant, or when less than half of patient depositors are required to withdraw late so that withdrawing late offers a higher return than withdrawing early, or when $\eta < 0.5$. In other words, the standard algorithm predicts $\eta = 0.5$ as the watershed for coordination. Our experimental results, however, suggest 0.7 as the critical value. To resolve this discrepancy, we need to modify the standard evolutionary algorithm.

We know in general that agents in experiments do not respond to the environment in a mechanical way.

[7] One may question the appropriateness of using learning or evolutionary algorithms for bank runs since bank runs are nowadays rare events (due to the existence of explicit or implicit deposit insurance). However, there are some historical episodes of frequent bank runs. For example, the 1932-33 banking panic in the United States was so severe that it induced president Roosevelt to enact the federal deposit insurance program. A wave of runs on financial firms was also observed during the 2007-2010 financial crisis.

For instance, whether or not they choose to play the best response may depend on whether or not they have enough information to tell what a better strategy in the previous period was. The probability that they carry out experimentation may depend on the current state of play and the potential for obtaining higher payoffs in case of experimentation, etc. In order to address these issues, we introduce what we call the '*endogenous*' evolutionary algorithm. We let both components of the standard evolutionary algorithm depend on agents' information sets. Throughout the experiments that we conducted, all subjects can infer the value of the coordination parameter η. In addition, subjects are shown their individual payoffs for each period and they can use this information to try to infer the number of late withdrawals in the previous period. Introducing this 'endogeneity' may enable the algorithm to better capture the features of actual experiments. In particular, we are interested in examining whether the new algorithm generates dynamics in which $\eta = 0.7$ turns out to be the critical value.

Next we describe the new algorithm in detail. Let s_{bt}^k ($k = 1, 2, ..., N$) be agent k's strategy choice following myopic best response with inertia, and s_t^k be the strategy following experimentation at time t (which is the strategy played at time t). Let e and l represent early withdrawal and late withdrawal, respectively. Let $\hat{z}$ be the minimum value of z that ensures the bank has a positive amount of money after paying early withdrawers.[8] Note that $\hat{z} < z^*$. Let π_t^k be the payoff received by agent k at time t. The two components of the evolutionary algorithm are modified as below.

(1) Myopic best response with inertia. Instead of using an exogenous process to determine whether or not an agent plays the best response (as in Temzelides, 1997), we let the decision be based on the agent's information set, i.e., whether he/she can infer what the 'best response' was. In the context of our experiments, only subjects who can infer whether $z_{t-1} > z^*$ can determine whether withdrawing late was the better strategy, and only these subjects play the myopic best response. Those who do not have the information play 'inertia' or do not respond. The strategy choice following the myopic best response with inertia s_{bt}^k is determined as follows.

(1a) If $s_{t-1}^k = e$, then $s_{bt}^k = e$. During the experiments, depending on their payoffs, subjects who withdrew early in the previous period (*early withdrawers*) may or may not be able to infer whether $z_{t-1} > z^*$. The information difference, however, does not affect their strategy choice following best response with inertia. If they received r, they can infer that $z_{t-1} > \hat{z}$. However, since $\hat{z} < z^*$, they cannot infer whether $z_{t-1} > z^*$. In this case, they play inertia and do not update their strategy. If early withdrawers were paid less than r, they can infer that $z_{t-1} < \hat{z} < z^*$ and that withdrawing early was the better strategy. In either case, early withdrawers choose to withdraw early as a result of myopic best response with inertia.

(1b) If $s_{t-1}^k = l$, then $s_{bt}^k = \begin{cases} l \text{ if } \pi_{t-1}^k > r; \\ e \text{ otherwise.} \end{cases}$ During the experiments, those who withdrew late in the previous period (*late withdrawers*) can always infer whether $z_{t-1} > z^*$. If they had payoff higher than r, they can infer that $z_{t-1} > z^*$ and that withdrawing late was the better strategy. As a result, they choose l as the strategy. If their payoff was less than r, they can infer that $z_{t-1} < z^*$ and they choose e.

[8] We can calcuate $\hat{z}$ as $N(r-1)/r$. Note that $\hat{z}$ is also the minimum value of z that ensures late withdrawers receive positive payoffs, and early withdrawers receive the promised short-term payoff r.

To sum up, as a result of the myopic best response play with inertia, only late withdrawers who received payoffs higher than r choose to withdraw late; all other subjects choose to withdraw early.

(2) Experimentation. During the experiments, subjects can infer the value of η all the time. They can also try to infer z_{t-1}, the number of late withdrawals in the previous period, by looking at the payoff tables (see the experimental instructions in the Appendix) and their individual payoffs. Remember that η measures how difficult the coordination task is. The number of late withdrawals in the previous period shows how the group coordinated in the past. Both η and z_{t-1} may influence a subject's belief about what other subjects will do in the next period, which in turn may affect the subject's strategy choice. We modify the standard algorithm to account for this possibility. For those who cannot infer the exact value of z_{t-1} (*uninformed subjects*), the experimentation rate depends only on η. For those who can infer the exact value of z_{t-1} (*informed subjects*), the experimentation rate depends on both η and z_{t-1}. Early withdrawers can infer the value of z_{t-1} only when they were paid less than r in the previous period.[9] Late withdrawers can infer z_{t-1} only when they received positive payoffs in the previous period.[10] Let δ^i_{el} (δ^u_{el}) be the probability of changing from withdrawing early to withdrawing late for informed subjects (uninformed subjects), and δ_{le} be the probability of flipping in the reverse direction. Experimentation proceeds as follows. [11]

(2a) If $s^k_{bt} = e$, then $s^k_t = l$ with probability

$$\begin{cases} \delta^i_{el}(z_{t-1}, \eta) \text{ if } (s^i_{t-1} = e \text{ and } \pi^i_{t-1} < r) \text{ or } (s^i_{t-1} = l \text{ and } \pi^j_{t-1} > 0); \\ \delta^u_{el}(\eta) \text{ otherwise.} \end{cases}$$

(2b) If $s^k_{bt} = l$, then $s^k_t = e$ with probability $\delta_{le}(z_{t-1}, \eta)$. Note that those who choose to withdraw late as a result of best response with inertia can infer the value of z_{t-1}.

6 Simulations

In this section, we compare the behavior of the endogenous evolutionary algorithm with that observed in the experiments. We first use logit regressions to estimate the probability of experimentation from the experimental data.[12] The estimated probabilities are then used to simulate time paths of the number of late withdrawals. Finally, we compare the simulation outcomes with the results from experiments with human subjects..

In the logit regressions, a change of strategy is coded as 1 and no change of strategy is coded as 0. We run three logit regressions to estimate δ^i_{el}, δ^u_{el} and δ_{le}, respectively. Table 5 lists the number of observations for each regression. The number and rate of observed experimentation are also listed. According to feedback from subjects, the difference between z_{t-1} and z^* played an important role in determining the probability

[9] This occurs when $z_{t-1} < \hat{z}$.

[10] This occurs when $z_{t-1} > \hat{z}$.

[11] In Temzelides (1997), experimentation is carried out in a way such that $\delta_{el} = \delta_{le} = \delta$, where δ is a fixed exogenous probability.

[12] We have a total of 5040 observations. There are 8 experimental sessions, each session has 10 players and 7 phases, each phase is run for 10 periods generating 9 observations for each player if the initial actions are taken as given. The total number of observations is thus given by $8 \times 10 \times 7 \times 9 = 5040$.

that subjects experiment with a strategy, even though they know that the other strategy gave a higher payoff in the previous period. In view of this, we use the term $(z_{t-1} - z^*)$ as the regressor to estimate δ^i_{el} and δ_{le}, the experimentation rates for informed agents. Note that $z^* = \eta N$ so z^* contains information about η. The expected sign of $(z_{t-1} - z^*)$ is positive in the case of δ^i_{el}, meaning that agents are more likely to change strategies to late withdrawal as z_{t-1} moves closer to z^* from below. The expected sign of $(z_{t-1} - z^*)$ is negative for δ_{le}, meaning that the probability of experimentation from late to early withdrawal increases as z_{t-1} moves closer to z^* from above. For uninformed agents, we use η as the regressor. In the estimation of δ^u_{el}, the expected sign of η is negative, meaning that uninformed subjects are less likely to flip strategy from early to late withdrawal as coordination becomes more difficult.

The first logit regression investigates the rate at which informed agents change strategy from early to late withdrawal. The rate δ^i_{el} is modelled as

$$\text{logit }(\delta^i_{el}) = log\left(\frac{\delta^i_{el}}{1-\delta^i_{el}}\right) = \alpha_0 + \alpha_1(z_{t-1} - z^*).$$

The regression result is presented in table 6. The coefficient of $(z_{t-1} - z^*)$ is very significant (the p-value is 0) and has the expected sign. The probability of experimentation from early to late withdrawal for informed subjects increases as z_{t-1} moves closer to z^* from below. This fact means that even though informed subjects know withdrawing early was the better strategy (which gives payoff r) in the previous period, they may be willing to try withdrawing late, hoping that a better coordination will occur in the next period and that they can receive a higher payoff than r. The estimated probability of experimentation from early to late withdrawal is

$$\hat{\delta}^i_{el}(z_{t-1}, \eta) = \frac{1}{1 + e^{-0.74-0.51(z_{t-1}-z^*)}}. \tag{3}$$

The second logit regression examines the rate of experimentation from early to late withdrawal for uninformed agents. The experimentation rate δ^u_{el} depends only on η and is modelled as

$$\text{logit }(\delta^u_{el}) = log\left(\frac{\delta^u_{el}}{1-\delta^u_{el}}\right) = \beta_0 + \beta_1\eta.$$

The result of the regression is presented in table 7. The coefficient of η is significantly negative (the p-value is 0) meaning that uninformed agents are less likely to change from early to late withdrawal if η is higher, or as coordination becomes more difficult. The estimated probability of experimentation from early to late withdrawal is given by

$$\hat{\delta}^u_{el}(\eta) = \frac{1}{1 + e^{-1.03+2.74\,\eta}}. \tag{4}$$

The final logit regression estimates the rate of experimentation from late to early withdrawal. As mentioned earlier, those who choose to withdraw late as a result of myopic best response with inertia are late withdrawers who are informed about z_{t-1}. The term $(z_{t-1} - z^*)$ is used as the regressor to estimate δ_{le}:

$$\text{logit }(\delta_{le}) = log\left(\frac{\delta_{le}}{1-\delta_{le}}\right) = \gamma_0 + \gamma_1(z_{t-1} - z^*).$$

Table 8 shows the regression result. The coefficient of $(z_{t-1} - z^*)$ is negative and significant (the p-value is 0) meaning that subjects are less likely to change actions from late to early withdrawal if $(z_{t-1} - z^*)$ increases or the number of late withdrawals in the previous period moves further above z^*. The expected experimentation rate from late to early withdrawal is given by

$$\hat{\delta}_{le}(z_{t-1}, \eta) = \frac{1}{1 + e^{3.04+0.24(z_{t-1}-z^*)}}. \tag{5}$$

We now apply the endogenous evolutionary algorithm and use the estimated probability of experimentation to simulate the path of the number of late withdrawals. We use the same model parameters that were used in experiments with subjects: there are 10 simulation subjects, 7 phases or values of η, and 10 rounds in each phase. Each simulation adopts the starting values (S) in one of the eight experimental sessions.

In the following, we illustrate the simulation process using the starting values of SFU1. The starting values of z in the 7 phases are respectively 10, 9, 10, 10, 8, 2 and 1. In each phase, strategies in the first round are set to match the starting value of z. For example, in phase 1, the simulation economy starts with all 10 agents withdrawing late so that z is equal to 10 in the first round. From round 2 on, each simulation agent updates its strategy according to the rules of the endogenous evolutionary algorithm. First, the agent plays best myopic response with inertia updating strategy if it can infer whether or not $z_{t-1} > z^*$. Strategy updating is such that the agent chooses to withdraw late if and only if $z_{t-1} > z^*$. Second, after the myopic best response with inertia, the agent experiments by flipping strategies. The probability of experimentation depends on the agent's information set and is calculated from equation (3), (4) or (5). For example, if the agent chooses early withdrawal as the strategy after the myopic best response play and knows the value of z_{t-1}, the probability of changing strategy to late withdrawal is given by $\hat{\delta}^i_{el}(z_{t-1}, \eta)$. After each agent's strategy is determined, we calculate the number of late withdrawals. Each simulation is run for 70 periods, generating 70 values of z.

We run 100 simulations for each set of starting values. In table 9, we list the minimum, maximum, and average (across the 100 simulations) of the terminal (T) and mean value (M) of z for each phase or η. For example, the 100 simulations using SFU1 starting values generate the following result. For $\eta = 0.1$, the minimum, maximum, and average value of the 100 terminal values are 9, 10, and 9.9, respectively. Table 10 lists the frequencies at which the simulated economies fall into each of the eight performance categories: "NN", "FN", "CN", "H", "RR", "FR", "CR", and "L" as defined in table 3. An investigation of tables 9 and 10 shows that the endogenous evolutionary algorithm captures the main features of the experimental data. For instance, the economies stay close to or converge to the non-run equilibrium when η is small. For $\eta = 0.1$, 0.2 and 0.3, all simulated economies stay close to the non-run equilibrium, spending 100% of the time at "NN" or "FN". When $\eta = 0.5$, all economies spend more than 98% of the time at "NN", "FN" or "CN" (and very occasionally at "H"). When $\eta = 0.8$ and 0.9, all simulated economies spend more than 99% of the time at "RR", "FR" or "CR" (and very occasionally at "L"). When $\eta = 0.7$, as in the actual experiments, the simulated economies produce very different results, spending a positive amount of time in each of the eight performance categories. We also provide a plot of a sample simulated time path of z in figure 2, which looks very similar to the time path of z from experiments with subjects. Compared with the standard evolutionary algorithm in Temzelides (1997), the endogenous algorithm replicates the experimental data more closely, and successfully captures 0.7 as the watershed for coordination.

7 Conclusion

In this paper, we take an experimental approach to studying the possibility and conditions under which bank runs occur purely as a result of coordination failures. To do that, we enroll human subjects to act as depositors and play a repeated version of the game defined by a demand deposit contract. We find that bank runs can happen as the result of pure coordination failures, but only when coordination is difficult. A critical value of the coordination parameter serves as the watershed for coordination. When the coordination parameter is less than 0.7, most subjects perceive that it is easy to coordinate and therefore choose to leave money with the bank so that that the experimental economies stay close to or converge to the non-run equilibrium. When the coordination parameter exceeds 0.7, most subjects perceive coordination to be too difficult and therefore choose to withdraw early so that the experimental economies stay close to or converge to the run equilibrium. When the coordination parameter is equal to 0.7, the consensus breaks down and the performance of the experimental economies becomes unpredictable. We observe huge differences in subjects' choices across different experimental sessions. The experimental results thus suggest a third alternative explanation of bank runs: depositors' expectations and actions depend critically on a parameter which measures the difficulty in coordination; bank runs occur when depositors perceive difficult coordination.

In order to capture the observed features of the experimental data, we use an evolutionary algorithm with myopic best response with inertia and experimentation. Unlike the standard algorithm used in Young (1993), Kandori, Mailath and Rob (1993) and Temzelides (1997), where the probabilities of playing best response and experimentation are exogenously given, we introduce an 'endogenous' algorithm where the probabilities are endogenously determined by subjects' information sets: the coordination parameter and possibly the history of the number of late withdrawals. Compared with the standard algorithm, the new algorithm offers a better replication of the experimental results. For example, it successfully captures 0.7 as the watershed for coordination (Temzelides, 1997, finds 0.5 as the critical value). The endogenous evolutionary algorithm thus represents a reasonable explanation for the behavior of human subjects in the laboratory.

Finally, we would like to point out that although we study the role of the difficulty of coordination in the context of a bank run model, the result is applicable to more general 'entry' games. The findings in this paper complement previous studies on how the number of subjects (Battalio, Beil and Van Huyck, 1990, 1991) and the payoff differential between different equilibria (Battalio, Beil and Van Huyck, 2001; Cabrales, Nagel and Armenter, 2007) affect the outcomes of entry games.

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Table 1: Parameters Used in the Experiment

Phase	0	1	2	3	4	5	6	7
r	1.43	1.05	1.11	1.18	1.33	1.54	1.67	1.82
η	0.60	0.10	0.20	0.30	0.50	0.70	0.80	0.90
Period (increasing η)	-9-0	1-10	11-20	21-30	31-40	41-50	51-60	61-70
Period (decreasing η)	-9-0	61-70	51-60	41-50	31-40	21-30	11-20	1-10

Table 2: Mean, Starting and Terminal Value of the Number of Late Withdrawals

	0.1			0.2			0.3			0.5			0.7			0.8			0.9		
	S	T	M	S	T	M	S	T	M	S	T	M	S	T	M	S	T	M	S	T	M
SFU1	10	9	9.8	9	9	9.7	10	9	9.7	10	9	9.6	8	2	1.1	2	0	3.1	1	0	0.2
UIBE1	10	10	10	9	10	9.9	10	9	9.7	8	10	9.3	6	6	1.2	2	0	7.5	1	0	0.2
UIBE3	9	10	9.8	8	10	9.5	10	10	10	9	10	9.6	7	0	0.4	3	0	2.1	0	0	0
UofM1	8	10	9.7	10	10	9.9	9	10	9.8	9	10	9.9	7	0	0.7	1	0	2.2	0	0	0.1
SFU2	10	10	10	10	10	10	10	10	10	10	10	9.8	6	9	1.9	6	0	7.9	4	2	2.2
UIBE2	10	10	10	10	10	9.9	8	10	9.7	4	8	7.2	1	0	0.5	1	0	0.4	1	1	0.6
UIBE4	10	10	10	10	10	10	10	10	10	6	9	8.7	0	0	0.7	2	0	0.5	1	0	0.5
UofM2	10	9	9.7	10	10	10	8	9	9.6	4	9	7.8	1	0	0.6	1	1	1	3	3	2

Table 3: Performance Classification

Category	Label	Criterion
Very close to the non-run equilibrium	NN	$\mathrm{M} \geq 9$
Fairly close to the non-run equilibrium	FN	$8 \leq \mathrm{M} < 9$
Converging to the non-run equilibrium	CN	$5 < \mathrm{M} < 8$ and $\mathrm{T} \geq 8$
Moderate high coordination	H	$5 < \mathrm{M} < 8$ and $\mathrm{T} < 8$
Very close to the run equilibrium	RR	$\mathrm{M} \leq 1$
Fairly close to the run equilibrium	FR	$1 < \mathrm{M} \leq 2$
Converging to the run equilibrium	CR	$2 < \mathrm{M} < 5$ and $\mathrm{T} \leq 2$
Moderate low coordination	L	$2 < \mathrm{M} < 5$ and $\mathrm{T} > 2$

Table 4: Performance of the Experimental Economies

η	0.1	0.2	0.3	0.5	0.7	0.8	0.9
SFU1	NN	NN	NN	NN	CR	CR	RR
UIBE1	NN	NN	NN	NN	H	CR	RR
UIBE3	NN	NN	NN	NN	CR	RR	RR
UofM1	NN	NN	NN	NN	CR	RR	RR
SFU2	NN	NN	NN	NN	CN	CR	CR
UIBE2	NN	NN	NN	CN	RR	RR	RR
UIBE4	NN	NN	NN	FN	RR	RR	RR
UofM2	NN	NN	NN	CN	RR	RR	FR

Table 5: Observations for Logit Regression and Overall Experimentation Rates

		Number of Obs.	Number of Experimentation	Experimentation Rate (%)
$s_b = e$	Informed	1824	149	8.17
	Uninformed	336	108	32.1
$s_b = l$	Informed	2880	31	1.08

Table 6: Logit Regression of Experimentation from Early to Late Withdrawal (Informed Subjects)

Experimentation from Early to Late Withdrawal	Coefficient	Standard Error.	t-statistic	p-value
$z_{t-1} - z^*$	0.51	0.04	13.93	0.00
Constant	0.74	0.22	3.30	0.00

Table 7: Logit Regression of Experimentation from Early to Late Withdrawal (Uninformed Subjects)

Experimentation from Early to Late Withdrawal	Coefficient	Standard Error.	t-statistic	p-value
η	-2.74	0.62	-4.43	0.00
Constant	1.03	0.41	2.49	0.01

Table 8: Logit Regression of Experimentation from Late to Early Withdrawal

Experimentation from Late to Early Withdrawal	Coefficient	Standard Error.	t-statistic	p-value
$z_{t-1} - z^*$	-0.24	0.07	-3.40	0.00
Constant	-3.04	0.04	-7.10	0.00

Table 9: Starting, Terminal and Mean Value of the Number of Late Withdrawals in Simulated Economies

		0.1			0.2			0.3			0.5			0.7			0.8			0.9		
		S	T	M	S	T	M	S	T	M	S	T	M	S	T	M	S	T	M	S	T	M
	Min		9	9.5		9	9.1		8	9.2		7	8.6		0	1.8		0	0.2		0	0.1
SFU1	Max	10	10	10	9	10	9.9	10	10	10	10	10	10	8	10	9.8	2	5	2.8	1	3	1.1
	Ave		9.9	9.9		9.9	9.7		9.8	9.8		9.7	9.7		6.5	7.5		0.5	0.9		0.3	0.4
	Min		9	9.5		9	9.1		8	9.2		7	7.9		0	1		0	0.2		0	0.1
UIBE1	Max	10	10	10	9	10	9.9	10	10	10	8	10	9.8	6	10	9	2	5	2.8	1	3	1.1
	Ave		9.9	9.9		9.9	9.7		9.8	9.8		9.6	9.2		1.9	3		0.5	0.9		0.3	0.4
	Min		9	9.4		9	8.7		8	9.2		7	8.3		0	1.5		0	0.3		0	0
UIBE3	Max	9	10	9.9	8	10	9.8	10	10	10	9	10	9.9	7	10	9.2	3	5	2.9	0	3	1
	Ave		9.9	9.8		9.9	9.6		9.8	9.8		9.7	9.5		2.1	3.6		0.5	1.1		0.3	0.3
	Min		9	9.2		9	9.4		8	8.8		7	8.3		0	1.5		0	0.1		0	0
UofM1	Max	8	10	9.8	10	10	10	9	10	9.9	9	10	9.9	7	10	9.2	1	5	2.4	0	3	1
	Ave		9.9	9.6		9.9	9.9		9.8	9.7		9.7	9.5		2.1	3.6		0.5	0.7		0.3	0.3
	Min		9	9.5		9	9.4		8	9.2		7	8.6		0	1		0	0.7		0	0.4
SFU2	Max	10	10	10	10	10	10	10	10	10	10	10	10	6	10	9	6	5	3.6	4	3	1.7
	Ave		9.9	9.9		9.9	9.9		9.8	9.8		9.7	9.7		1.9	3		0.5	1.7		0.3	0.9
	Min		9	9.5		9	9.4		8	8.8		7	3		0	0.2		0	0.1		0	0.1
UIBE2	Max	10	10	10	10	10	10	8	10	9.8	4	10	9.1	1	10	7.4	1	5	2.4	1	3	1.1
	Ave		9.9	9.9		9.9	9.9		9.8	9.5		9.5	7.5		1.6	1.3		0.5	0.7		0.3	0.4
	Min		9	9.5		9	9.4		8	9.2		7	7		0	0.1		0	0.2		0	0.1
UIBE4	Max	10	10	10	10	10	10	10	10	10	6	10	9.6	0	6	3	2	5	2.8	1	3	1.1
	Ave		9.9	9.9		9.9	9.9		9.8	9.8		9.6	8.7		1.5	1.1		0.5	0.9		0.3	0.4
	Min		9	9.5		9	9.4		8	8.8		7	3		0	0.2		0	0.1		0	0.3
UofM2	Max	10	10	10	10	10	10	8	10	9.8	4	10	9.1	1	10	7.4	1	5	2.4	3	3	1.5
	Ave		9.9	9.9		9.9	9.9		9.8	9.5		9.5	7.5		1.6	1.3		0.5	0.7		0.3	0.7

Table 10: Performance of Simulated Economies

		0.1	0.2	0.3	0.5	0.7	0.8	0.9			0.1	0.2	0.3	0.5	0.7	0.8	0.9
	NN	100	100	100	98	41				NN	100	100	100	98	1		
	FN				2	26				FN				2	3		
	CN									CN					2		
SFU1	H					10			SFU2	H					3		
	RR						75	99		RR					1	85	100
	FR					1	21	1		FR					25	13	
	CR					19	4			CR					53	2	
	L					3				L					12		
	NN	100	100	100	70	1				NN	100	100	97	3			
	FN				29	3				FN			3	36			
	CN				1	2				CN				59	1		
UIBE1	H					3			UIBE2	H				1			
	RR					1	75	99		RR					42	85	99
	FR					25	21	1		FR					42	13	1
	CR					53	4			CR					6	2	
	L					12				L					9		
	NN	100	99	100	90	3				NN	100	100	100	29			
	FN		1		10	6				FN				65			
	CN									CN				5			
UIBE3	H					6			UIBE4	H				1			
	RR						63	100		RR					61	75	99
	FR					11	30			FR					29	21	1
	CR					60	6			CR					6	4	
	L					14	1			L					4		
	NN	100	100	98	90	3				NN	100	100	97	3			
	FN			2	10	6				FN			3	36			
	CN									CN				59	1		
UofM1	H					6			UofM2	H				1			
	RR						85	100		RR					42	85	90
	FR					11	13			FR					42	13	10
	CR					60	2			CR					6	2	
	L					14				L					9		

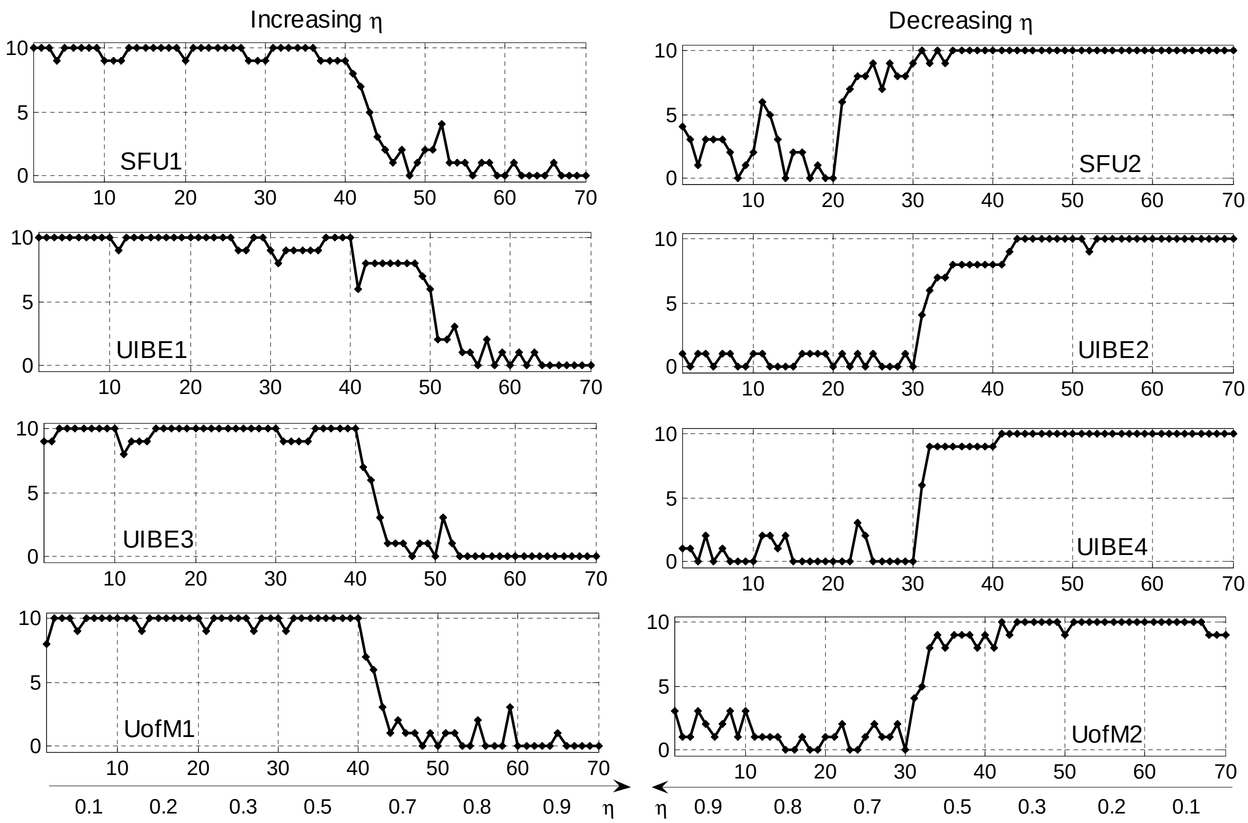


Figure 1: Experimental Results

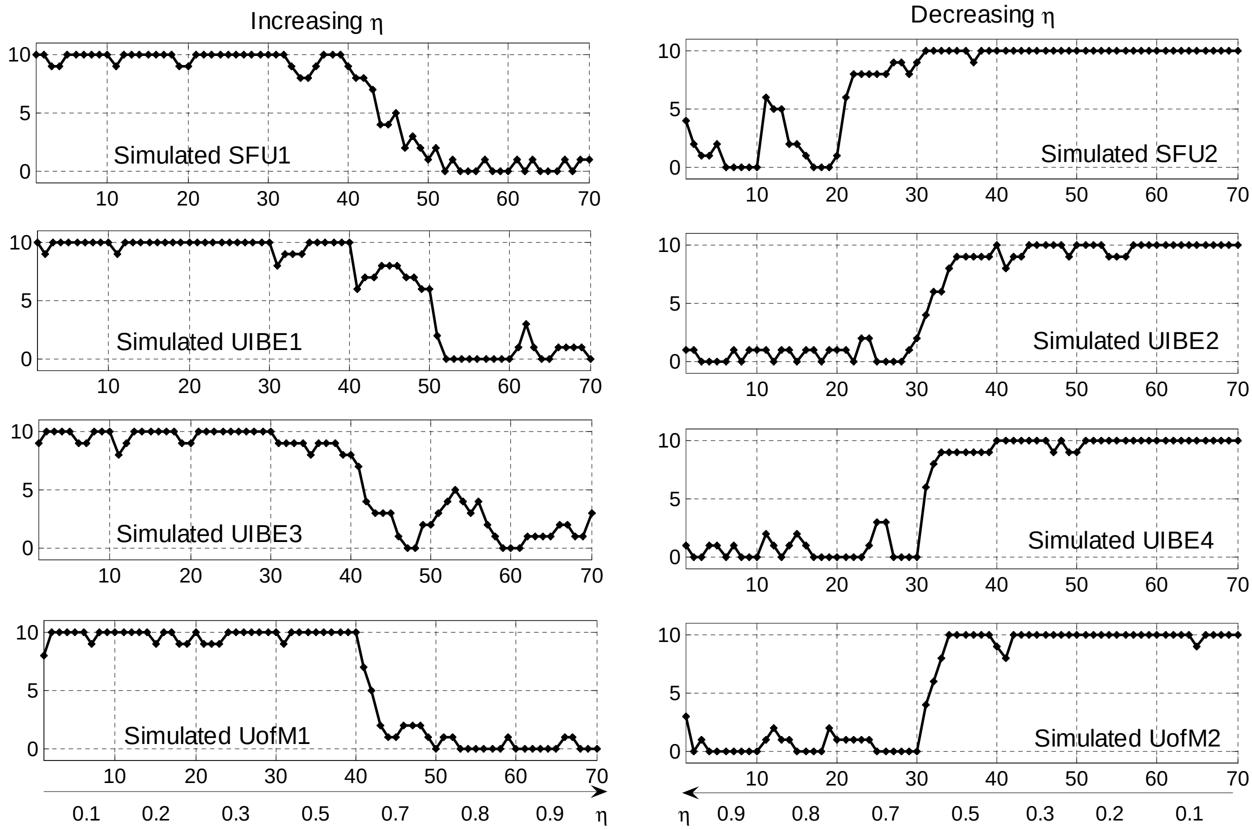


Figure 2: Sample Simulated Path of Number of Late Withdrawals

Appendix (not for publication)

Experimental Instructions - Increasing Order of the Coordination Parameter

This experiment has been designed to study decision-making behavior in groups. During today's session, you will earn income in an experimental currency called experimental dollars or for short ED. At the end of the session, the currency will be converted into dollars. 1 ED corresponds to 0.2 dollars. You will be paid in cash. The participants may earn different amounts of money in this experiments because each participant's earnings are based partly on his/her decisions and partly on the decisions of the other group members. If you follow the instructions carefully and make good decisions, you may earn a considerable amount of money. Therefore, it is important that you do your best.

Please read these instructions carefully. You will be required to complete a quiz, in order to demonstrate that you have a complete and accurate understanding of these instructions. After you have completed the quiz, the administrator will check your answers and discuss with you any questions that have been answered incorrectly.

Description of the task

You and 9 **other people** (there are 10 of you altogether) each have 1 ED deposited in an experimental bank. You must decide whether to withdraw your 1 ED or leave it deposited in the bank. The bank promises to pay $r > 1$ EDs to each withdrawer. The money that remains in the bank will earn interest rate $R > r$. At the end, the bank will divide what it has evenly among people who choose to leave money in the bank. Note that if too many people desire to withdraw, the bank may not be able to fulfill the promise to pay r to each withdrawer (remember that $r > 1$). In that case, the bank will divide the 10 EDs evenly among the withdrawers and those who choose to wait will get nothing.

Your payoffs depend on your own decision and the decisions of the other 9 people in the group. Specifically, how much you receive if you make a withdrawal request or how much you earn by leaving money deposited depends on how many people in the group place withdrawing requests.

Let e be the number of people who request withdrawals.

The payoff (in ED) to those who withdraw will be:

$$\min\{r, \tfrac{10}{e}\} \text{ or the minimum of } r \text{ and } \tfrac{10}{e}.$$

The payoff (in ED) to those who leave money in the bank will be:

$$\max\{0, \tfrac{10-er}{10-e}R\} \text{ or the maximum of } 0 \text{ and } \tfrac{10-er}{10-e}R.$$

In the experiment, R will be fixed at 2.0. There are 8 values of r: 1.43, 1.05, 1.11, 1.18, 1.33, 1.54, 1.67 and 1.82. To facilitate your decision, the payoff tables $0 \sim 7$ list the payoffs if n **of the other** 9 **subjects** request to withdraw (n is unknown at the time when you make the withdrawing decision – it can be any integer from 1 to 9 – and you have to guess it) for each of the 8 situations. Table 0 will be used for practice, and table $1 \sim 7$ will be used for the formal experiment. Let's look at two examples:

Example 1. Use table 0 where $r = 1.43$. Suppose that 3 other subjects request withdrawals. Your payoff will be 1.43 if you request to withdraw (the number of withdrawing requests e will be $3 + 1 = 4$, and your payoff is $\min\{r = 1.43, \frac{10}{e} = \frac{10}{4} = 2.5\} = 1.43$). If you choose to leave money in the bank, your payoff will be 1.63 (the number of withdrawing requests $e = 3$, and your payoff is $\max\{0, \frac{10-er}{10-e} R = \frac{10-3\times1.43}{10-3} 2.0 = 1.63\} = 1.63$).

Example 2. Use table 7 where $r = 1.82$. Suppose that 6 of other subjects request withdrawals. Your payoff from withdrawing will be $\min\{1.82, \frac{10}{e} = \frac{10}{7} = 1.43\} = 1.43$ and your payoff for leaving money in the bank will be $\max\{0, \frac{10-6\times1.82}{10-6} 2.0 = -0.46\} = 0$.

Now let us take a closer look at the tables. Note the following features of tables:

- The payoff to withdrawing is more stable, it is fixed at r for most of the time and is bounded below by 1.
- The payoff to leaving money in the bank is more volatile. When n – the number of other people requesting withdrawals – is small, leaving money in the bank offers higher payoff than withdrawing. The opposite happens when n is large enough. For your convenience, bold face is used to identify the threshold value of n at which withdrawing starts to pay equal to or higher than leaving money in the bank.
- The threshold values of n varies from table to table. The general pattern is that it is smaller when r is bigger.

Note that you are not allowed to ask people what they will do and you will not be informed about the other people's decisions. You must guess what other people will do – how many of the other 9 people will withdraw – and act accordingly.

Procedure

You will perform the task described above for 70 times. Each time is called a period. Each period is completely separate; i.e., you start each period with 1 in the experimental bank. You will keep the money you earn in every period. At the end of each round, the computer screen will show you your decision and payment for that period. Information about earlier periods and your cumulative payment is also provided.

Note that the payment scheme changes every 10 periods, so please use the **correct** payoff table:

Table 1 for period 1-10,

Table 2 for period 11-20,

Table 3 for period 21-30,

Table 4 for period 31-40,

Table 5 for period 41-50,

Table 6 for period 51-60, and

Table 7 for period 61-70.

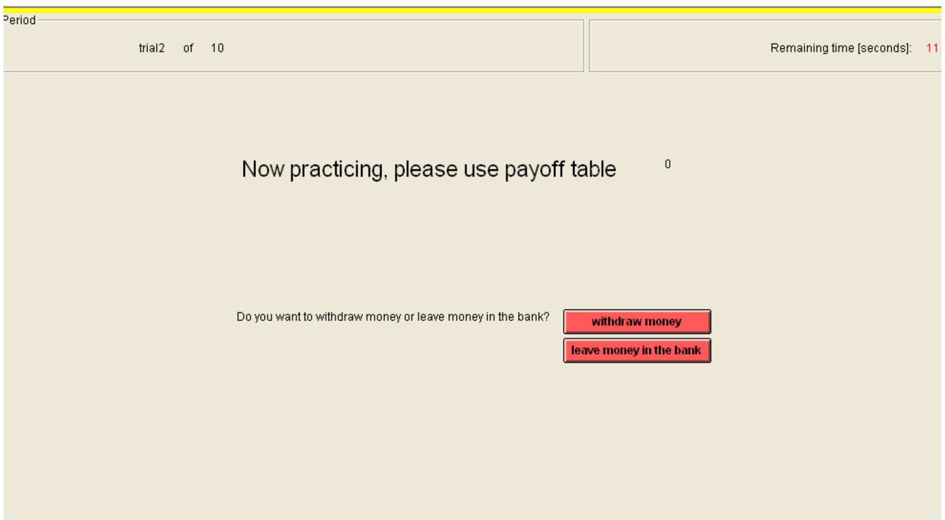
You will be reminded when you need to change to a new table; pay attention to the message.

Beside the 70 paid periods, you will also be given 10 practice periods get familiar with your task. You will not be paid for the practice periods. Please use table 0 for practice. After the practice periods, you will have a chance to ask more questions before the experiment formally starts. You will be paid for each formal period.

Computer instructions

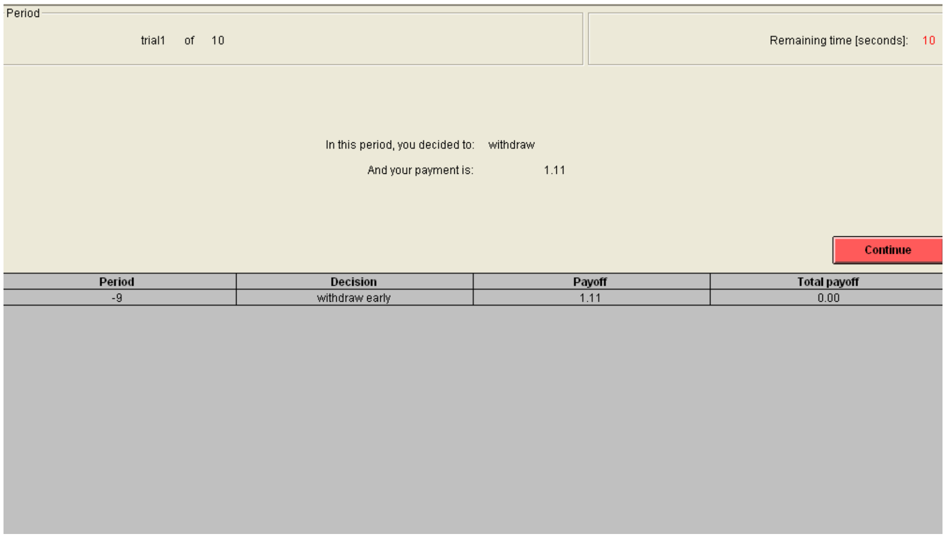
You will see three types of screens: the decision screen, the payoff screen and the waiting screen.

Your withdrawing decisions will be made on the decision screen as shown below. You can choose to withdraw money or leave money in the bank by pressing one of the two buttons. **Note** that your decision will be final once you press the buttons, so be careful when you make the move. The header provides information about what period you are in and the time remaining to make a decision. After the time limit is reached, you will be given a flashing reminder 'please reach a decision!'. The screen also shows which table you should look at.



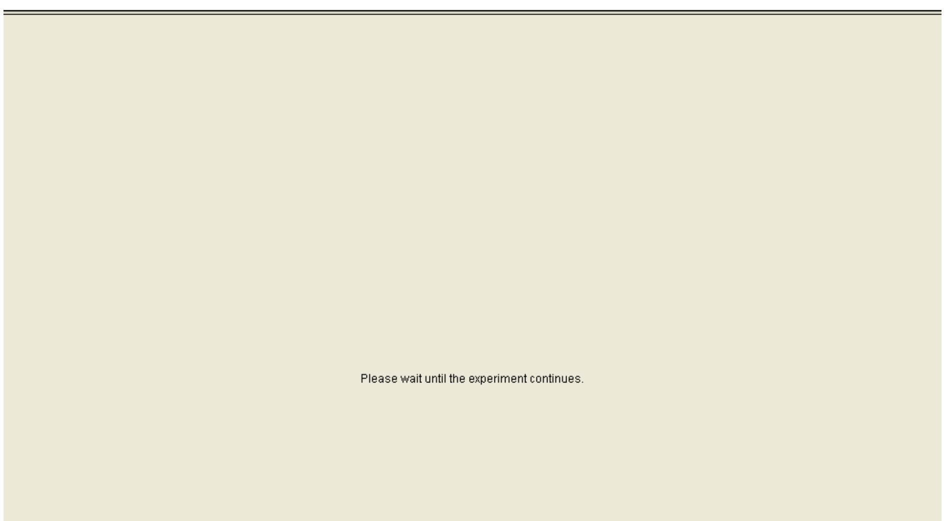
The Decision Screen

After all subjects input their decisions, a payoff screen will appear as shown below. You will see your decision and payoff in the current period. The history of your decisions and payoffs as well as your cumulative payoff are also provided. After finishing reading the information, push the 'continue' button to go to the next period. You will have 30 seconds to review the information before a new period starts.



The Payoff Screen

You may see a waiting screen following the decision or payoff screens – this means that other people are still making decisions or reading the information, and you will need to wait until they finish to go to the next step.



The Waiting Screen

Payment

At the end of the entire experiment, the supervisor will pay you in cash. Your earnings in dollars will be:

$$\text{total payoff in } ED \times 0.2.$$

Table 0 (for practice): payoff if n of other 9 subjects withdraw

$r = 1.43$

n	payoff if withdraw	payoff if leave money in the bank
0	1.43	2.00
1	1.43	1.90
2	1.43	1.79
3	1.43	1.63
4	1.43	1.43
5	1.43	1.14
6	1.43	0.71
7	1.25	0.00
8	1.11	0.00
9	1.00	0.00

Table 1: payoff if n of other 9 subjects withdraw

$r = 1.05$

n	payoff if withdraw	payoff if leave money in the bank
0	1.05	2.00
1	1.05	1.99
2	1.05	1.98
3	1.05	1.96
4	1.05	1.93
5	1.05	1.90
6	1.05	1.85
7	1.05	1.77
8	1.05	1.60
9	1.00	1.10

Table 2: payoff if n of other 9 subjects withdraw

$r = 1.11$

n	payoff if withdraw	payoff if leave money in the bank
0	1.11	2.00
1	1.11	1.98
2	1.11	1.94
3	1.11	1.91
4	1.11	1.85
5	1.11	1.78
6	1.11	1.67
7	1.11	1.49
8	1.11	1.12
9	1.00	0.02

Table 3: payoff if n of other 9 subjects withdraw

$r = 1.18$

n	payoff if withdraw	payoff if leave money in the bank
0	1.18	2.00
1	1.18	1.96
2	1.18	1.91
3	1.18	1.85
4	1.18	1.76
5	1.18	1.64
6	1.18	1.46
7	1.18	1.16
8	1.11	0.56
9	1.00	0.00

Table 4: payoff if n of other 9 subjects withdraw

$r = 1.33$

n	payoff if withdraw	payoff if leave money in the bank
0	1.33	2.00
1	1.33	1.93
2	1.33	1.84
3	1.33	1.72
4	1.33	1.56
5	1.33	1.34
6	1.33	1.01
7	1.25	0.46
8	1.11	0.00
9	1.00	0.00

Table 5: payoff if n of other 9 subjects withdraw

$r = 1.54$

n	payoff if withdraw	payoff if leave money in the bank
0	1.54	2.00
1	1.54	1.88
2	1.54	1.73
3	1.54	1.54
4	1.54	1.28
5	1.54	0.92
6	1.43	0.38
7	1.25	0.00
8	1.11	0.00
9	1.00	0.00

Table 6: payoff if n of other 9 subjects withdraw

$r = 1.67$

n	payoff if withdraw	payoff if leave money in the bank
0	1.67	2.00
1	1.67	1.85
2	1.67	1.67
3	1.67	1.43
4	1.67	1.11
5	1.67	0.66
6	1.43	0.00
7	1.25	0.00
8	1.11	0.00
9	1.00	0.00

Table 7: payoff if n of other 9 subjects withdraw

$r = 1.82$

n	payoff if withdraw	payoff if leave money in the bank
0	1.82	2.00
1	1.82	1.82
2	1.82	1.59
3	1.82	1.30
4	1.82	0.91
5	1.67	0.36
6	1.43	0.00
7	1.25	0.00
8	1.11	0.00
9	1.00	0.00