Experimental Studies on Market Entry under Uncertainty and on Coordination

Lan Yao

Supervisor: Jordi Brandts

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Extracto

Esta tesis es dividida en tres capítulos que se refieren a dos temas diferentes. El segundo capítulo se concentra en los efectos incentivos de bajar salarios llanos y su papel en la ayuda del fracaso de coordinación vencido. Los resultados débilmente apoyan los efectos positivos de bajar salarios llanos. Los dos papeles en los terceros y cuartos capítulos relacionan los estudios de la incertidumbre de información de riesgo y ambigüedad en juegos de entrada de mercado. Estudiamos experimentalmente decisiones bajo la incertidumbre de riesgo y ambigüedad en juegos de entrada de mercado, que captura los rasgos básicos de los fenómenos sobre la entrada en el mercado. La tarea importante es averiguar si la participación excesiva está relacionada con los tipos de información de riesgo y ambigüedad, y si las decisiones son diferentes en riesgo y ambigüedad en ambientes estratégicos. Encontramos la ambigüedad que busca en un ajuste de mercado de un ambiente relativo del mercado de información arriesgado y ambiguo en la correspondencia fija. Sin embargo, en un ambiente no relativo del mercado de información arriesgado y ambiguo, la busca de ambigüedad es saliente en la correspondencia arbitraria, pero no en la correspondencia fija. Encontramos que los efectos de ambigüedad en juegos estratégicos no dependen de si el riesgo y la ambigüedad son puestos en contextos relativos o no relativos, pero en la complejidad estratégica en los juegos. Más fuerte la complejidad estratégica es, más saliente la ambigüedad efectúa.

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Contents

Chapter 1

Introduction

This thesis is divided into three chapters that refer to two different topics, all of them analyzed through laboratory experiments. One topic is on the coordination problem in the organization, the other is on the market entry under information uncertainty of risk and ambiguity. Although the two topics are different, they are both motivated by "searching for facts", where the experiments study the effects of variables about which existing theory may have little to say, and by "Whispering in the ears of Princes", where the experiments resemble the naturally occurring environments that is the focus of interest for the policy purposes at hand.[1] Although the two topics have attracted a lot of attention over the past years, inefficiency taken from coordination failure and business failure resulting from overentry keep as problems which are difficult to solve in the filed. We really think such studies are hard to be explained only with economic theory. Laboratory experiments provide another tool to understand the puzzles happening in economic environments.

The second chapter contains the paper "Flat wage effects and loss avoidance in overcoming coordination failure". The problem of coordination failure, particularly in “team production” situations, is central to a large number of corporations and organizations. Several previous experiments using minimum effort coordination games have tried to find ways to overcome coordination failure in organizations. In the payoff equation used by previous research, where the payoff of an employee results from the sum of a flat wage and a bonus subtracting his effort cost, flat wages and bonus rates are two important financial incentives deciding payoffs in the coordination. Bonus rates have been proved to be an efficient incentive. However, different from bonus, flat wage is a constant which is not influenced by effort input and is ignored in the

[1] Kagel, John H., and Roth, Alvin E., (1995).

studies of coordination games. Our experiments are set in a corporate environment considering incentive changes after the coordination failure in the initial phases. The focus is on the incentive effects of lowering flat wages and its role in helping overcome coordination failure. Incentive effects of flat wages could be positive, negative or zero predicted by game theoretic solution concepts, decision theory or psychological evidence separately. The experimental results show that lowering the flat wage has small, statistically significant, positive effects on effort choices when coordination failure occurs. It plays similar roles as the incentive of raising bonus rates. However, both incentive changes are not strong enough in helping overcome coordination failure. When lowering flat wages and raising bonus rates are added together, it can do the best in coordinating on more efficient equilibria. The results weakly support the reference point effects of lowering flat wages. It seems that the flat wage can be considered as a reference point in a framed condition. Under the situation which subjects fail to coordinate and keep earning the flat wage (zero effort chosen), this initial flat wage is reinforced to be a reference point and the payoffs less than it are valued as “loss”. As a result, loss avoidance to the lowered flat wages may make them to choose higher efforts and draw them out of inefficiencies. What is more, we also find that experience of coordination failure is very important for later coordination. Compared with the groups without incentive change and in bad coordination, the groups with incentive changes may also stays in the bad coordination, but a simple restart of the game can help them improve coordination. The finding suggests that even if the incentive given in the former periods can not reverse the situation of groups stuck at inefficient equilibria, subjects learn from each other in the process and have the preference for coordination, later a simple “restart” can help overcome coordination failure. It does not happen with groups without incentive changes.

The two papers in the third and fourth chapters relate the studies of information uncertainty of risk and ambiguity in market entry games. Both are joint work with my advisor Professor Jordi Brandts. Over the past fifty years in the academic studies, decision making under uncertainty was mostly viewed as choice over a number of outcomes with known probabilities. However, it is not obvious at all why decision makers should know probability. Knight (1921) is the first to distinguish ambiguity from risk, where probabilities are unknown or imperfectly known as opposed to a situation under risk where probabilities are known. In a comprehensive survey of the literature on experimental studies of decision-making under uncertainty, Camerer and Weber (1992) view ambiguity as "uncertainty about probability, created by missing information that is relevant and could be known" (Camerer and Weber (1992), page 330). Experimental studies confirm a preference for betting on events with information about probabilities and ambiguity aversion is found in most experiments on individual choice problems. However, in many situations of decisions under uncertainty in strategic interaction, excessive participation is salient without the guidance of available information about probabilities. A class of examples in the field of industrial organization describe the decisions of firms on whether to enter a market. Evidence of "Excess entry" and the high rate of business failure has been reported by many empirical studies. In another example of the freeway congestion problem, many commuters face the daily dilemma of taking a predictable, but slower route or risking hours of gridlock on a potentially faster freeway. However, there is no sign of less crowded on a faster freeway. Such evidence is not consistent with the findings of aversion to choices with higher uncertainties in individual decision problems.

We study experimentally decisions under uncertainty of risk and ambiguity in market entry games, which captures the basic features of the phenomena mentioned above. The important task is to find out whether the excessive participation is related to the information types of risk and ambiguity, and whether decisions are different in risk and ambiguity in strategic environments. Payoffs are decided by both the variance of the number of entrants and uncertainty (risk or ambiguity) of market capacities. In the risky information situation there are two possible market capacities, both known to occur with probability 1/2. In the ambiguous information situation the two possible market capacities effectively occur with probability 1/2 but participants are only told that there is uncertainty about capacities. The design of information types of risk or ambiguity is uniform in the experiments in both chapters.

The third chapter contains the paper "Excess entry and ambiguity seeking: an experimental study in two-market entry games". In the strategic game treatment, subjects must choose among three choices, staying out with a fixed payoff, entering a risky information market or entering an ambiguous information market. In each of the two latter choices, payoffs are decided by both the variance of the number of entrants and uncertainty (risk or ambiguity) of market capacities. We also do a control treatment to see whether decisions are different in an individual choice problem. In the individual choice game, payoffs in each of the two latter choices are decided only by the uncertainty (risk or ambiguity) of market capacities but not the number of entrants. Our results somehow prove the internal relation between excessive participation and uncertainty about information. We find ambiguity seeking behavior when players face the risky market and the ambiguous market simultaneously, where average entry is higher in the market with ambiguous information than the one with risky information. They suggest that when potential entrepreneurs face alternative opportunities of different levels of uncertainties, they are able to control their entry when knowing probability while not without probability information. In the individual choice treatment, the average entry is similar in the two markets. It seems that controlling behavior appears only when subjects are situated in a strategic environment. The possible explanation of ambiguity seeking found in strategic environments is that without information about probability, people may be competent and overconfident in a competitive environment, but they may try to behave rationally in the entry decisions in the choices with known probability.

In the fourth chapter entitled "Ambiguity effects and strategic complexity in market entry games", the main motivation stems from the potential connections between ambiguity effects and the complexity of the strategic environments. Chapter three implies that ambiguity seeking is driven by a market setting of a comparative environment of risky and ambiguous information market and high strategic complexity. What if entrepreneurs face a simple situation of a strategic environment, such as a single market? How do they evaluate an uncertain event with risky or ambiguous information in isolation? In the experiments, subjects must choose between two choices, staying out with a fixed payoff, or entering either an risky information market or ambiguous information market. We run the game in both fixed matching and random matching over 50 rounds to represent the different level of strategic complexity of coordinated entry and one-time entry separately. We find that average entry is higher under ambiguous information treatment than under risky information treatment in random matching, but it is similar in both treatments in fixed matching. Combining the results in chapter three, which studies it in comparative contexts in fixed matching, we find that ambiguity effects in strategic games do not depend on whether risk and ambiguity are put in comparative or noncooperative contexts, but on the strategic complexity in the games. The stronger the strategic complexity is, the more salient the ambiguity effects.

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Chapter 2

Flat Wage Effects and Loss Avoidance in Overcoming Coordination Failure

2.1 Introduction

The problem of coordination failure, particularly in "team production" situations, is central to a large number of corporations and organizations. As a good example, Ichniowski, Shaw and Prennushi (1997) study the steel production in an assembly line setting with productivity largely determined by unscheduled downtime, which implies that the steel production efficiency is decided by the coordinated works of all the employees in the assembly line. Coordination failure happens when one of those employees does a poor job (leading to breakdowns on his part of the assembly line) and destroys the efficiency of the entire line.

Even if the benefits from improving coordination are obvious, coordination failure happens frequently and has been a substantial obstacle to efficiency. The typical description of such situations is a version of the "minimum-effort" (or "weak link") coordination game, which was first studied experimentally by Van Huyck et al. (1990). In such multi-equilibrium games, all the members can be better off by choosing more efficient equilibria, but the inefficient equilibrium is often chosen. Considerable studies of such weak link games have found that groups end up coordinating on inefficient outcomes that are hard to reverse.

It is important to find the way out of inefficient coordination. A good understanding of the payment mechanism in such minimum effort coordination games can help explain the reasons of coordination failures and clarify the roles of different incentives. The minimum-effort coordination game studied in the experiment is a stylized model of a firm. The firm consists of a number of employees who choose among different effort levels. The firm gives payment as a reward according to the minimum effort level chosen by employees. The payoff of each employee "i" in the group is decided by the following formula:

Payoff of Employee $i = W + B$*minimum effort - C* individual effort

where each employee's payoff is a function of his own effort ("individual effort" in the equation) and the "minimum effort" of all the employees. The magnitude of the payoff also depends on three parameters: "W", the flat wage, "B", the bonus rate and "C", the unit effort cost under certain technology situation. Both the flat wage and the bonus rate are the payment scheme decided by the firm. The flat wage W governs the secure payoff when zero effort is chosen and the bonus rate B governs the benefit to the employees of coordinating at a higher effort level. The unit effort cost C is an employee's individual cost for exerting one unit effort. The importance of the bonus rate and the cost of unit effort in improving coordination have been well studied by many experiments. Van Huyck et al. (1990) show that the decision on effort level is positively related to the bonus rate and negatively related to the unit effort cost.[1] Goeree and Holt (2005) study a random matching coordination game and aim at finding the relation between coordination level and the unit effort cost. They find that the lower the unit effort cost subjects face, the higher the coordination level is. Brandts and Cooper (2006) study the relation between the coordination level and the bonus rate in groups stuck in a coordination trap. They find that an increase of the bonus rate aiming at coordination indeed leads to improved coordination.

The above experiments have shown that either lowering the unit effort cost or raising the bonus rate can improve coordination level. To our knowledge, the effect of flat wages on coordination decisions has never been studied. Incentive effects of flat wages could be positive, negative or zero. One may think that the flat wage doesn't play any role in decision making, because the flat wage appears in individual profits as an added constant term. However, as one of the two payment mechanisms, flat wages may influence individual effort choices once the magnitude of the flat wage and the bonus are compared. Intuitively, a higher flat wage may give individuals less incentive to choose high efforts because the higher secure payoff makes the bonus incentive less attractive. Correspondingly, a lower flat wage may be considered as an indirect incentive for choosing a more efficient equilibrium. On the other hand, moving down the flat wage may hurt intrinsic incentives. One may be encouraged by the high flat wage given by the entrepreneur

[1] Van Huyck et al. (1990) do not define flat wages and bonus rates but use similar equation in their experiment.

while may respond passively to a low flat wage. It is interesting to investigate whether the flat wage can be an instrument for improving coordination or not and how effective it would be.

All the conjectures above can be explained by standard theories. Game theoretic solution concepts imply that adding or subtracting the same constant to all payoffs does not affect a player's rank-ordering of outcomes and should have no effect on behavior, which supports the ineffectiveness of flat wages.[2]

Decision theoretic research holds the view that flat wages may be treated as individual "reference point" and any payoff less than the flat wage is regarded as a kind of "loss". The desire to avoid a "loss" has been used in helping overcome coordination failure. It is firstly considered by Cachon and Camerer (1996) in coordination games. The experimental results show that the certain loss of the entry fee can induce subjects to choose a high number in an effort to alter the signs of the payoffs. Both Rydval and Ortmann (2005) and Feltovich, Iwasaki and Oda (2006) investigate experimentally the conjecture that "possible-loss avoidance" (the decision between a possible negative payoff and a certain positive payoff) solves the tension between choosing efficiency and inefficient equilibria in one shot and repeated stag-hunt games separately. However, these studies define loss avoidance as a preference to avoid negative payoffs corresponding to a zero reference point. If the definition of loss avoidance can be extended to its broad sense, where to avoid a positive payoff below a reference point, flat wages can be considered as a reference point and the lowered flat wage as a loss, and then the incentive effects of flat wage can be positive in helping reach higher efforts.

Psychological studies on causal attribution theory reveal that behavior is sensitive to the perceived cause of a given event or situation which is regarded as the basis for an action. In a gift exchange experiment Brandts and Charness (2004) finds that employees respond with lower effort to low wages when they originate from a self-interested party whose payoff depend on their choice compared to when a low wage originates from a random source. In the present experiment, the flat wage is intentionally lowered down by the experimenter, which could be considered as a passive incentive.

The present experiments are based on Brandts and Cooper (2006), in which the repeated corporate turnaround game between "employees" and a "firm" is introduced. More specifically, in the three ten block periods in our experiment, the incentive changes happen at the beginning of the second one, and the same information will be informed at the beginning of the third one. In an initial phase of the first block of 10 periods, employees face a situation in which coordination failure is supposed to occur, and then a kind of incentive change is introduced in

[2] Erev, Bereby-Meyer and Roth (1999).

an attempt to induce employees to coordinate on a more efficient equilibrium. In the present paper, the focus is on the incentive effect of lowering flat wages and its role in helping overcome coordination failure. I design different treatments to explore the effects of lowering the flat wage (hereafter, Treatment LW), its comparison with that of raising the bonus rate (hereafter, Treatment RB) and the combined use of lowering the flat wage and raising the bonus at the same time (hereafter, Treatment LWRB).

The experimental results show that lowering the flat wage has a small, statistically significant, positive effect on effort choices when coordination failure occurs. It plays similar role as the incentive of raising bonus. However, both incentive changes are not strong enough in helping overcome coordination failure. When lowering flat wages and raising bonus rates are added together, LWRB can do the best in coordinating on more efficient equilibria. The results weakly support the reference point effects of lowering flat wages. It seems that the flat wage can be considered as a reference point in a framed condition. Under the situation which subjects fail to coordinate and keep earning the flat wage (zero effort chosen), this initial flat wage is reinforced to be a reference point and the payoffs less than it are valued as “loss”. As a result, loss avoidance to the lowered flat wages may make them choosing higher efforts and draw them out of inefficiencies.

The experimental results also reveal the importance of experience of coordination failure which happens in the third 10 block periods. Different from experimental results in several other studies (Brandts and Cooper, 2006; Weber et al., 2007), in the present paper incentive changes can not immediately reverse the coordination failure situation. The responses to incentive changes are not persistent and the inefficient outcomes are chosen again in the end of the second ten block periods. However, it happens to provide a unique chance to investigate how a pause, which is the stop separating the second and third blocks, plays roles when incentive changes can not reverse coordination failure in the second 10 block periods. We find that although both the groups given incentives and those without given incentives coordinate inefficiently in the second block, more efficient coordination equilibria turn out to be possible for the former but not for the latter when we simply stop the repeating coordination game and start the process again. The finding suggests that even if the incentive given in the former periods can not reverse the situation of groups stuck at inefficient equilibria, subjects learn from each other in the process and have the preference on coordination, later a simple “restart” can help overcome coordination failure. It does not happen with groups without incentive changes.

This paper is organized as follows. Section 2.2 describes the experimental design and the hypotheses. Section 2.3 gives the results. Section 2.4 summarizes our results and adds some concluding remarks on the limitations of our results. Appendix A contains tables that are not included in the paper. Appendix B contains the instruction for Treatment LWRB used in the experiments.

2.2 Experimental Design

2.2.1 Description of the experiment

The minimum effort coordination game in our experiments consists of a manager and 4 employees. For all the experimental sessions reported below, the experimenter plays the role of the firm manager while subjects fill the roles of the four employees. The payoff of each subject i is given by the equation below,

Payoff of Employee $i = W + B$*minimum effort - C* individual effort

For all of the sessions reported below, $C = 5$, W and B are the financial incentive parameters we focus on throughout the experiments. We restrict an employee's effort choice to the integers: 0, 10, 20, 30, 40, and 50.

At the beginning of each session subjects read the instructions directly from their computer screens[3]. Subjects then took a quiz to ensure that they understood the basic features of the instructions.

The experiment was divided into 3 parts with a total of 30 periods. Part 1 consists of 10 periods of the game with the payoff structure displayed in Table 1. Subjects played in fixed groups. In each period, subjects made choices by clicking on one of the effort levels and then the screen displayed the choices of all four members, the minimum choice in the group, the subject's payoff in the current period and his accumulated payoffs. Subjects never received any information on outcomes in other groups.

[3] The experiment was programmed and conducted with the software z-tree (Fischbacher, 2007)

Table 1. Employee i's payoff table for $W = 400$, $B = 7$

Effort by Employee i	Minimum Effort by Other Employees					
	0	10	20	30	40	50
0	400	400	400	400	400	400
10	350	420	420	420	420	420
20	300	370	440	440	440	440
30	250	320	390	460	460	460
40	200	270	340	410	480	480
50	150	220	290	360	430	500

At the conclusion of Part 1, the screen informed about the value W and B for period 11 to 20. Subjects knew the value of W and B could be changed, but they did not know the magnitude of the incentive change in subsequent ten-period blocks. These new instructions corresponded to one of the different incentive treatments listed in Table 2. Following 10 periods in Part 2, subjects were told that the same W and B as in part 2 would be given for the final 10 periods. In the Control treatment, the value W and B was kept constant for 30 periods. However, subjects were informed about the value of W and B at the beginning of every 10 periods. At the end of the session, subjects were paid on an individual and private basis.

Table 2. Treatment list

	Control	LW	RB	LWRB
groups	5	15	15	15
Period $1-10$	$W = 400, B = 7$	$W = 400, B = 7$	$W = 400, B = 7$	$W = 400, B = 7$
Period 11 to 30	$W = 400, B = 7$	$W = 300, B = 7$	$W = 400, B = 9$	$W = 300, B = 9$

The incentives given at the beginning of period 11 changed payoff structure in the interim of repeated games, and is related to the study of the static equilibrium selection in the dynamic selection process[4]. Since we aim at comparing the effectiveness of different incentive, we prefer to control the magnitude of incentives in each treatment to be similar[5]. We use the concepts of

[4] The static equilibrium selection criterion usually refers to "deductive selection", the selection based on reasoning and coordination on focal points; correspondently, "inductive selection" is the selection based on adaptive dynamic environment. See also Haruvy and Stahl (2004).

[5] The evidence on whether the magnitude of incentives matters in coordination games is mixed. Brandts and Cooper (2006) shows that the size of incentives has almost no influence in helping groups coordinate on more efficient equilibria, but other researches conclude that behavior in closely related coordination games is responsive to the magnitude of payoff incentives (e.g. Battalio et al., 2001; Goeree and Holt, 2005; Weber, Rick and Hamman, 2006).

"payoff dominance" and "risk dominance", two selection principles by Harsanyi and Selten (1988) and later by Selten (1995). They are concerned with pair-wise comparisons of Nash equilibria. In the present experiments, payoff dominance is defined as the ratio of the most efficient payoff (the wage when the highest efforts chosen by all subjects) to the least efficient payoff (flat wage). Theoretically, a higher payoff dominance ratio favors more efficient equilibrium outcome. As for the definition of risk dominance, one choice is risk-dominant and the other neighbor choice is risk-dominated if deviation loss from equilibrium associated with the former are greater than deviation losses with the latter. I take one part of table 1 here as an example:

Table 3: An example

Effort by Employee i	Minimum Effort by Other Employees	
	0	10
0	400	400
10	350	420

The Nash Equilibrium $(0, 0)$ risk-dominates $(10, 10)$ because the deviation loss 50 $(= 400 - 350)$ is higher than 20 $(= 420 - 400)$. In the present paper, I use the ratio between the deviation losses (i.e. 50/20) to control the magnitude of the payoff structure after incentive changes. The ratio is the same for treatments with the same bonus rate B. Based on the research of Selton (1995), higher B implies more chances for efficient coordination. Table 4 lists the defined value of payoff dominance and risk dominance in the treatments.

Table 4: The values of risk dominance and payoff dominance

	Control	LW	RB	LWRB
Payoff dominance	1.25	1.33	1.5	1.67
Risk dominance	2.5	2.5	1.25	1.25

We recruited 200 subjects from the undergraduate population of Universitat Autònoma de Barcelona of Spain using posters and classroom announcements. We conducted 1 session of 20 subjects of the control treatment and 3 sessions of 60 subjects in each of the other 3 treatments. Subjects were only allowed to participate in a single session. The payoffs are represented experimental currency units (ECU), which were converted into euros at the end of the experiment at a rate of 1/1000 ECU. Each subject received a 5 euros show-up fee in addition to money accumulated playing the game. The average payoff of the experimental participants was 17.5 euros.

2.2.2 Questions

Since my experiments focus on the question how financial incentives help reverse coordination failures in the second and third block of ten rounds, all the analysis is based on one important hypothesis that in periods 1-10 average minimum effort will be close to zero. Then we can now formulate the following questions focusing on the effectiveness of lowering flat wages:

Question 1: Does lowering flat wages in period 11 cause the average effort to increase, decrease or stay at the same level?

The role of lowering flat wages can be observed in the two pair-wise comparisons (LW vs. Control, RB vs. LWRB) in which situation the flat wage falls from 400 to 300 units in period 11. If the hypothesis on coordination failure in the first 10 periods is correct, where individuals' behavior converges to zero effort level, lowering flat wages in period 11 may lead to positive, negative or zero effects based on the reasoning of, decision theory, psychology or game theory separately.

Question 2: Which can provide the best incentive in overcoming coordination failure, LW, RB or LWRB?

The question of which incentive is more effective, a reward or punishment, is controversial. If LW, considered as a punishment, is confirmed to be a positive incentive in helping improve coordination, the comparison among LW, RB and LWRB would be interesting.

2.3 Experimental results

2.3.1 Overview of results

My overview of the data begins by examining results in the first ten periods when subjects play based on Table 1, with $W = 400$ and $B = 7$. As predicted, the majority of groups converge to the inefficient outcome. The minimum effort is zero in 41 of the 50 groups (82%) at the end of period 10. Since my experiments aim at finding how an incentive change can improve coordination in groups stuck at bad equilibria, the focus is on the groups with initial failure (group minimum of zero in period 10). In the 41 groups, the average effort choice is 25.53 and 3.31 in period 1 and 10 respectively. The average group minimum changes from 7.37 in period 1 downward to zero in period 10.

The main part of my study deals with the changes (LW, RB and LWRB) in period 11 and focuses on the effects of lowering wages in improving coordination for the next 20 periods. We find that for all groups with initial failure in period 10, motivated by the incentives given in period 11, both average individual choices and the average group minimum respond to it. The average individual choice increases to 17.15 and average group minima to 3.74, while for the Control groups it is 10.5 and 0 respectively.

Detailed information of how average individual choices and group minima for groups with initial failure evolve over time are given by Figure 1 and 2.

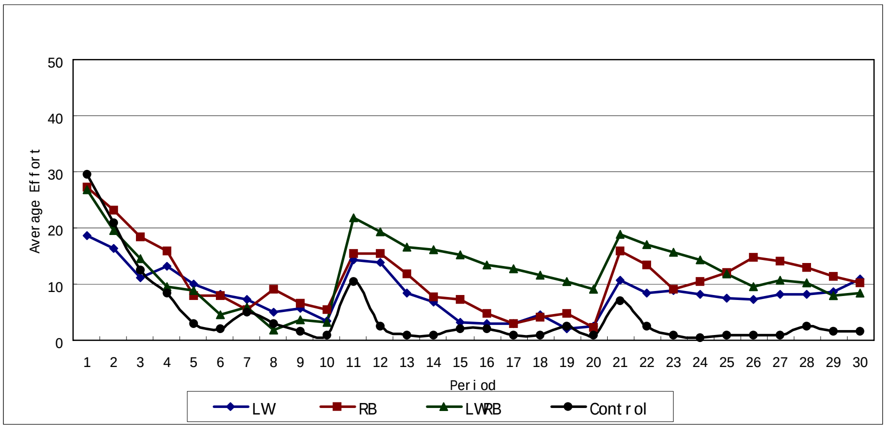

Figure 1: Comparison of Treatments on individual level

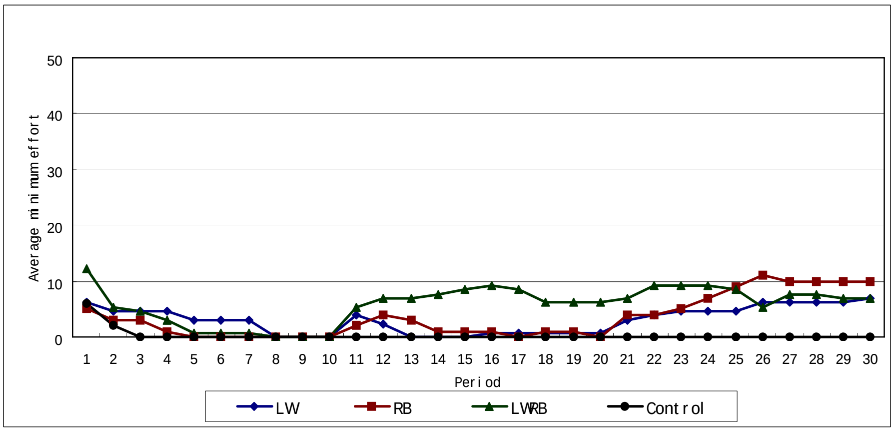

Figure 2: Comparison of treatments on group level

In Figure 1, the average individual choices jump in all four treatments following the changes of B or W. In period 11, the increase in Treatment LWRB is strongest and the increase in Control is weakest. However, they keep decreasing just after period 11. The decrease in Treatment

LWRB is moderate while the decrease in Control is sharp. The effects of Treatment LW or RB are in between. In period 20, besides weak coordination in Treatment LWRB, coordination failure happens in all other three treatments. After the incentive change of LWRB in period 11, average choice reaches its peak 21.73 in that period and gradually goes down to 9.04 in period 20. Average minima stably increase in periods 11-16 to 9.23 and then go down to 6.15 in period 20.

Figure 3 displays the distribution of individual choices in period 11 on groups with initial failure in period 10. Responding to the incentive change, the majority of choices in the Control are on zeros, while under LWRB choices are more equally distributed in effort 0, 10, 20, 30, 40, and 50. In Treatment LW or RB, there are more 0s in LW while more 20s in RB.

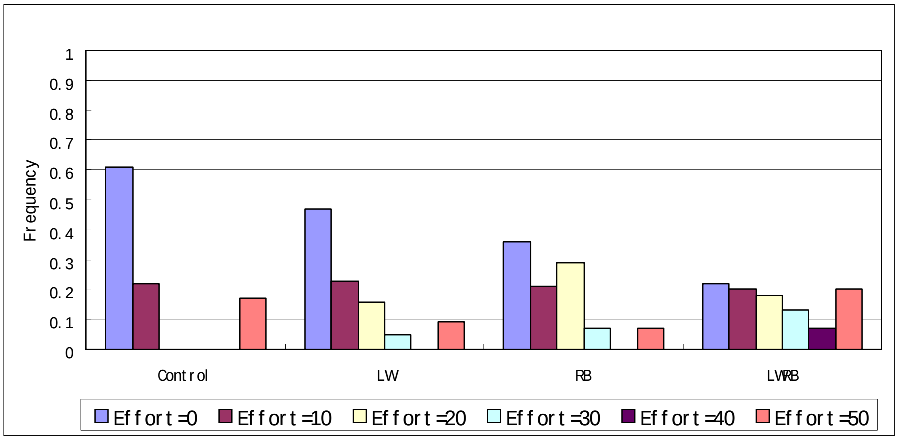

Figure 3: Distribution of individual efforts, period 11

There is a restart in period 21 caused by the pause to announce that W or B would continue to be the same. With the restart the average choices in each treatment almost jump to the same level as these in period 11, with Treatment LWRB strongest and Control weakest. Interestingly, in the periods 21-30, the choices in LWRB goes down gradually to the level similar to period 20 while the choices in LW and RB keep themselves rising stably. In the end, the choices in the incentive treatments (LWRB\LW\ RB) converge to the same level while the choices in Control go down to zero.

The group level data displayed in Figure 2 shows a similar pattern, and it gives a clearer view of several points discussed above. First, the average group minima in Control are zero after period 3. The pauses at the end of period 10 and period 20 play no roles to Control treatment in improving coordination. Second, the difference between Treatment LWRB and Treatment LW and RB are significant in periods 11-20 but insignificant in periods 21-30. Third, the choices are similar in Treatments LW and RB both in the second ten block period and in the third ten block period.

2.3.2 The effects of lowering flat wages

In order to have a clear view of the effects of different incentives and clarify the role of lowering flat wages in the coordination game, we examine the statistical validity of the impressions obtained from the graph. Table 5 reports regressions of comparison between Treatment LW and Control on both the group level and individual level.

Table 5. Effect of LW on subject choices and group minima

	Dependent variable: choice or average choice (standard errors clustered by group)			Dependent variable: minimum or average minimum		
	Period 11	Periods 11-15	Periods 16-20	Period 11	Periods 11-15	Periods 16-20
Period 10 minimum	0.55**	0.675***	0.86***	0.77***	0.93***	0.95***
	(0.20)	(0.13)	(0.08)	(0.14)	(0.05)	(0.05)
LW	3.74	5.85***	1.48	3.79*	1.21	0.76
(vs. Control)	(3.96)	(1.99)	(1.59)	(2.19)	(0.72)	(0.79)
Constant	10.5***	3.4***	1.5	*Dropped*	$-2.66e-15$	*Dropped*
	(2.25)	(0.96)	(0.88)			
N	80	80	80	20	20	20
R2	0.04	0.24	0.39	0.34	0.84	0.82

*** Significant at 1% level; ** Significant at 5% level; * Significant at 10% level

The regression analysis includes all the groups in each treatment. The primary independent variables are the group minima in period 10 and the dummy for Treatment LW (other than Treatment Control). It reveals that coefficients of dummy variables are always positive. Compared with the Control, in the periods after the introduction of LW (period 11), the effectiveness of LW is clear but not very strong. The group minima of LW in period 11 is marginally significant, and average individual choices in periods 11-15 is strongly significant. All other differences are statistically insignificant.

The regression in Table 6 reveals the effects of LWRB in periods 11-20. Compared with LW, the effect is significant and strongly significant in periods 11-15; and compared with RB, the effect on average choice in period 11 is almost insignificant and only marginally significant.

Table 6. Effect of LWRB on subject choices and group minima

	Dependent variable: choice or average choice (standard errors clustered by group)						Dependent variable: minimum or average minimum					
	Period 11		Periods 11-15		Periods 16-20		Period 11		Periods 11-15		Periods 16-20	
Period 10 minimum	0.62***	0.46***	0.71***	0.56***	0.85***	0.68***	0.81***	0.51***	0.88***	0.71***	0.92***	0.75***
	(0.06)	(0.05)	(0.06)	(0.10)	(0.07)	(0.16)	(0.12)	(0.06)	(0.07)	(0.12)	(0.08)	(0.17)
LWRB (vs. RB)	3.80*		3.79		4.26		−0.36		2.02		3.82	
	(2.20)		(3.37)		(4.15)		(2.66)		(2.56)		(3.88)	
LWRB (vs. LW)		7.11**		7.69**		7.10		0.65		4.52**		4.94
		(3.22)		(3.17)		(4.21)		(2.56)		(2.06)		(3.87)
Constant	16.97***	14.42***	12.69***	9.47***	5.39***	3.35**	3.93**	4.32**	3.35**	1.66**	1.51	1.17
	(1.55)	(2.89)	(2.20)	(1.57)	(1.58)	(1.23)	(1.62)	(1.94)	(1.37)	(0.73)	(1.03)	(0.81)
N	120	120	120	120	120	120	30	30	30	30	30	30
R^2	0.27	0.13	0.50	0.32	0.55	0.33	0.79	0.38	0.82	0.67	0.66	0.41

*** Significant at 1% level; ** Significant at 5% level; * Significant at 10% level

The findings suggest that choices in periods 11-20 are significantly positive on the group minimum effort in period 10 for all treatments, but the difference among treatments are minute. A closer inspection of my experimental results reveals that there are always groups with coordination on period 10 (initial coordination success), some even with very successful coordination on highest choice[6]. As a result, when the groups with successful coordination are counted in and aggregate choices are analyzed, the significant level of the dummy variable on incentives LWRB and RB weakens.

I also use t test to explore the mean value of the distribution, where I exclude the groups with group minimum effort above zero in period 10. The data used in t test are the same as those described by Figure 1 and Figure 2. Table 7 provides the pair-wise comparison of treatments, in which t values are the difference between the mean value of the former and the latter treatments.

The results of the t test for LW &Control are similar as those of the regression analysis. There is no significant difference in the mean choices between the incentive RB and LW in the periods after the incentive change, which reflects the fact that the incentive of LW can behave similar as that of RB. However, different from the result of regression analysis, it shows that LWRB performs significantly better than LW or RB.

[6] We can take the comparison LWRB&RB as a good example. 2 out of 15 groups in the LWRB treatment reach coordination in period 10. While 5 out of 15 groups in the RB treatment reach coordination in period 10, of which 2 groups even coordinate successfully on choice 50.

Table 7. Two-sample t test on periods 11-20 (Groups with initial failure)

	LW / Control	LW / RB	LWRB / LW	LWRB / RB
Period 11	$t = 0.840$	$t = -0.366$	$t = 2.222^{**}$	$t = 1.713^{**}$
Periods 11-15	$t = 2.727^{***}$	$t = -1.072$	$t = 3.864^{***}$	$t = 2.467^{***}$
Periods 16-20	$t = 0.993$	$t = -0.555$	$t = 3.641^{***}$	$t = 2.911^{***}$
Observations	13/5 groups	13/10 groups	13/13 groups	13/10 groups

***Significant at 1% level; ** Significant at 5% level; * Significant at 10% level

Why are the statistical results strongly significant in the t-test but weak in the regression? First, the two analyses serve as different purposes. The regression analysis in Table 5 and 6 explains the dynamic process that how the choices are explained by the group minimum and includes all groups. While the t test in Table 7 helps confirm the significance in the difference of the second block periods in the observations in Figure 1 and 2, where only groups with coordination failure are included.

Based on the econometric analyses, I am ready to answer Question 1 and Question 2 by the following regularity.

Regularity 1: Lowering flat wage is a weak positive incentive in improving coordination.

(a) Lowering flat wage is effective to help raise choice level in period 11 but is not enough keeping the effects till period 20.

(b) Among incentives, lowering flat wage and raising bonus at the same time perform best in overcoming coordination failure, where weak coordination is reached in the periods 11-20.

2.3.3 Explanation to the results

The experimental results confirm the difference between the treatment pairs (LW & Control, LWRB & RB). However, the coordination is extremely hard and only weak coordination is formed in the end of the game (period 30). By contrast, both Brandts and Cooper (2006) and Weber, Rick and Hamman (2007) find that an increase in the bonus rate leads to an considerable increase in the minimum effort for all different bonus rate increases used in rounds 11-20.

We don't think that employees in firms experiencing coordination failure are unable to read the payoff table or fail to realize that everyone could be better off if all choose high effort level. It may be because of the following reasons.

First, we believe the incentives given in our experiment is still not so attractive compared with the secure payoffs to some of the subjects in the experiment and lead to the coordination collapse in some of the groups. However, it proves again the magnitude of the incentives matters in individual decisions. It would be interesting to try a bigger incentive changes based on the same experimental design and find the way to reach higher level coordination.

Second, both Brandts and Cooper (2006) and Weber, Rick and Hamman (2007) restrict an employee's effort choices the integers 0, 10, 20, 30 and 40, but the available choices in my experiment are 0, 10, 20, 30 , 40 and 50. One more choice added may leads to quite different result. It is easier for subjects to "climb" their way out of the inefficient outcome by choosing similar effort gradually from lower to higher level. Therefore it may be the case that the more choices available for subjects, the harder for them to coordinate on similar effort level and then to higher level coordination.

Fortunately, we find the weak evidence on the incentive differences. The important issue is to explain how lowering flat wages effectively influence effort choices. In a coordination game with multiple equilibria, when those equilibria can be Pareto ranked, the game is an example for the type of games showing a tension between efficiency and security (Foster and Young, 1990; Young, 1993; Kandori et al., 1993.). The 'safest' equilibrium is the most inefficient one, while the most efficient equilibrium is the most 'insecure' outcome (Straub, 1995; Crawford, 1991). We know from the experiments by Van Huyck et al. (1990, 2001) that large groups approach the play of the inefficient equilibrium and receive security payoffs over time, thus display the problem of coordination failure.

In Table 1, instead of taking risk for 500 by choosing effort 50, one can receive security payoff 400 by choosing effort zero. In my experiment, in period 10 most groups choose zero effort and receive security payoff 400. Later the security payoff is lowered from 400 to 300 in both the incentive change of LW (vs. the Control) and that of LWRB (vs. RB). We find that under similar historical decisions and similar payoff structures, effort choices in the incentive changes with lowered flat wage are higher than that without it. The possible explanation on the role of lowering flat wages is loss aversion theory. One implication of loss aversion is that individuals have a strong tendency to remain at or above the reference point, such as status quo (Kahneman and Tversky, 1991), and try to avoid choices with payoffs below reference point. Instead of zero, reference point can be historically formed by individuals, so it can be bigger than zero. In our experiment, after repeated playing the first ten periods, the majority of subjects are paid the security payoff, which is equal to the flat wage. Then they act as if past wages serve as the reference point, in order to avoid possible loss, they prefer to choose effort levels with payoffs higher than their reference point. Once the flat wage is lowered, the preference of loss avoidance induces decisions on higher choices with payoffs higher than the reference point.

2.3.4 Learning from coordination failure

Now I move the study to the third block of ten periods. The effort choices on pure restart effects are observed. In period 21, both the incentives and the Control treatments respond to the restart effects, all of which lead to higher average choices compared with period 20. However the responding magnitudes are quite different.

It is not surprising that the restart plays roles in leading to higher choices. It is also not surprising to see the different degree of responses in the incentives and the Control treatments. However, since the average choices (2.25, 2.5, and 1) and the average minima (0, 0.77, and 0) of RB, LW and the Control are similar in period 20, their difference in the last 10 periods on the restart is really puzzling.

Table 8. Effects of incentives in periods 11-20 on the last ten periods

	Dependent variable: choice or average choice (standard errors clustered by group)			Dependent variable: minimum or average minimum		
	Period 21	Periods 21-25	Periods 26-30	Period 21	Periods 21-25	Periods 26-30
Average Minimum	0.80***	0.88***	0.91***	0.98***	0.96***	0.94***
	(0.05)	(0.07)	(0.12)	(0.03)	(0.06)	(0.12)
Incentives (vs. control)	4.52	6.69***	7.82**	2.34*	3.67*	6.74*
	(3.27)	(2.42)	(3.48)	(1.19)	(1.88)	(3.39)
Constant	7**	2.4***	1.5	$3.55e-15$	0	$-8.88e-15$
	(2.65)	(0.73)	(1.06)	$(2.51e-15)$	$(4.25e-15)$	$(6.15e-15)$
N	140	140	140	35	35	35
R^2	0.39	0.57	0.43	0.87	0.74	0.47

*** Significant at 1% level; ** Significant at 5% level; * Significant at 10% level

It may be the case that although the results in period 20 are quite similar for the incentive treatments (LW / RB) and the Control, the process of converging to it is different. The average choices and the average minima of the incentive treatments (LW/ RB) are diminishing in the second ten periods (11-20), but the average minima of the Control are zero for all the ten periods (11-20). It is quite possible that the incentives in period 11 can be an impact on learning how to coordinate for groups in periods 11-20, which does not happen in the Control.

Table 8 describes the regression analysis of the comparison between two incentives (RB and LW) and the Control. It confirms my conjecture. The influence of the group minima in periods 11-20 is strongly significant not only in period 21 but the process of the last ten periods (21-30). Besides the average choice in period 21, the coefficient on incentive is always positive and significant for choices. It indicates that choices in period 21 and the last ten periods were positively affected by the introduction of incentives in period 11 and the interaction among group members in period 11-20. As a result, the restart effects and the incentives given in the past jointly play roles in leading to higher choices.

Regularity 2: The restart effects taken by the pause in the end of period 20 make subjects choose more effort choices to the incentive treatments than to Control. It does help stable coordination to the incentive treatments but does not to the control.

2.4 Conclusion

The present paper focuses on the flat wage effects to the incentive systems and aims at finding better ways to overcome coordination failure. Issues surrounding flat wages have been discussed in many topics in economic theory and in practice but have never been studied in the coordination problem.

In all the treatments of our experiment, subjects' reaction to the incentives is very weak and the coordination is extremely hard. The main difference from other research appears in the second 10 block periods. In Brandts and Cooper (2006), there is a steady increase in the group minimum following the introduction of incentives; in Weber, Rick and Hamman (2007), the immediate response to incentives is much larger than that those in Brandts and Cooper (2006), however, groups either make it to a more efficient equilibrium immediately or do not. In our experiment, subjects neither response considerably to incentives nor try to climb their way out of the inefficient equilibrium. The differences suggest that coordination behaviors are sensitive to the experimental design and subject sample. I believe the incentives given in our experiment is still not so attractive compared with the secure payoffs to some of the subjects in the experiment and lead to the coordination collapse in some of the groups. However, it proves again the magnitude of the incentives matters in individual decisions. It would be interesting to try a bigger incentive changes based on the same experimental design and find the way to reach higher level coordination.

Although the response to the incentive changes is not very big, the figures show clear difference among different incentives. The findings suggest weak and positive effects of lowering the flat wage in coordination games. The results are likely to prove useful in practice. Reducing flat wages and increasing bonus rates would lead to the most efficient coordination outcomes.

Our experimental results on lowering flat wage can be explained by loss avoidance. In the experiments of several researches (Rydval and Ortmann 2005; Feltovich, Iwasaki and Oda 2006), individuals seek to avoid "pure" loss (i.e. negative payoffs), which affects action choices in stag hunt games. With respect to coordination games, our result reveals that individuals show loss avoidance to any payoff lower than the flat wage. Individuals situated in framed environments can generate the reference point above zero. Reference point formed in the dynamic process and the loss avoidance can be good reasoning when firms consider the incentive change of flat wages.

We also find the importance of learning from coordination failures in the restart effects. A simple restart can help overcome coordination failure if the incentive given in the former periods can not reverse the situation of groups stuck at inefficient equilibria. It seems that even if the incentive changes can not lead group members to successful coordination, people are still learning in the process. The restart can help them recover history and the accumulated experience can do benefits to future interaction. It teaches us that if the incentive change can not reverse the coordination failure in the organization, it is possible to have a break and start again.[7]

Our work embarks on the study of flat wage effects in coordination games. Loss avoidance and reference point can help understanding how people learn in the dynamic environment and how people evaluate losses and gains when faced with coordination in organizations. The effects of flat wage should be taken into accounted when we design a mechanism where coordination is involved.

[7] The effects of restart is also found in Weber (2006).

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2.5 Appendix A

Table A1. Payoffs in Treatment LW with $W = 300$, $B = 7$

Effort by Employee i	Minimum Effort by Other Employees					
	0	10	20	30	40	50
0	300	300	300	300	300	300
10	250	320	320	320	320	320
20	200	270	340	340	340	340
30	150	220	290	360	360	360
40	100	170	240	310	380	380
50	50	120	190	260	330	400

Table A2. Payoffs in Treatment RB with $W = 400$, $B = 9$

Effort by Employee i	Minimum Effort by Other Employees					
	0	10	20	30	40	50
0	400	400	400	400	400	400
10	350	440	440	440	440	440
20	300	390	480	480	480	480
30	250	340	430	520	520	520
40	200	290	380	470	560	560
50	150	240	330	420	510	600

Table A3. Payoffs in Treatment LWRB with $W = 300$, $B = 9$

Effort by Employee i	Minimum Effort by Other Employees					
	0	10	20	30	40	50
0	300	300	300	300	300	300
10	250	340	340	340	340	340
20	200	290	380	380	380	380
30	150	240	330	420	420	420
40	100	190	280	370	460	460
50	50	140	230	320	410	500

2.6 Appendix B: Instruction for Treatment LWRB

General information: The purpose of this experiment is to study how people make decisions in a particular situation. From now on and till the end of the experiment any communication with other participants is not permitted. If you have a question, please raise your hand and one of us will come to your desk to answer it.

You will receive 5 euros for showing up on time for the experiment. In addition, you will make money during the experiment. The payoffs in the experiment are represented by experimental Currency Units. Upon completion of the experiment the amount that you make will be paid to you in euro at a rate of €1/1000. Payments are confidential; no other participant will be told the amount you make.

Parts, Rounds and Groups: This experiment will have several parts. In Part 1 there will be 10 rounds. After these ten rounds have finished, we will give you instructions for the next part of the experiment. In each round you will be in a group with 3 other participants. The participants you are grouped with will be the same in all rounds.

Description of the Decision Task(s) in Part 1 of the Experiment: You and the other members of your group are employees of a firm. You can think of a round of the experiment as being a workweek. In each week, each of the employees in each firm spends 50 hours at the firm. You have to choose how to allocate your time between two activities, Activity A and Activity B. Specifically, you will be asked to choose how much time to devote to Activity A. The available choices are 0 hours, 10 hours, 20 hours, 30 hours, 40 hours and 50 hours. Your remaining hours will be put towards Activity B. For example, if you devote 30 hours to Activity A, this means that 20 hours will be put towards Activity B.

For each round of the experiment you will receive a flat wage and a bonus that depends on the minimum number of hours spent on Activity A by a member of your group. For all rounds of this experiment, the flat wage W and the bonus rate B may vary between rounds. They are selected by the firm manager. In this experiment, the firm manager is being played by the computer. We will always let you know the flat wage and the bonus rate before you choose how many hours to devote to Activity A.

Payoffs: The payoff that an employee receives in a round depends on the number of hours he chooses to spend on Activity A, the number of hours chosen by the others in his firm to spend on Activity A, the flat wage W and the bonus rate B selected by the firm manager. The payoff for the *ith* employee of the firm is given by the formula below where Hi is the number of hours spent by the *ith* employee of the firm on Activity A and min(HA) is the smallest number of hours an employee of the firm spends on Activity A. You do not need to memorize this formula – the computer program will give you payoff tables at any point where you need to make a decision.

$P_i = W + (B \times \min(H_A)) - (5 \times Hi)$

Playing a Round: For each round of the experiment, the computer will display a screen like the one shown below. The payoffs shown in the payoff table will be adjusted for the changing values of W and B. For the example below, we set W=250 and B = 8. Notice that this is displayed above the payoff table.

Each employee will choose a number of hours to spend on Activity A using the buttons on the right hand side of the screen. You may change your choices as often as you like, but once you click on "Enter" your choice is final. Note that when you make your decision you will not know what the other employees in your firm are doing in the round.

At no point in time will we identify the other employees in your firm. In other words, the actions you take in this experiment will remain confidential.

Table B1. Flat wage $W = 250$ and Bonus rate $B = 8$

My Hours on Activity A	Minimum Effort Spent on Activity A by Other Employees					
	0	10	20	30	40	50
0	250	250	250	250	250	250
10	200	280	280	280	280	280
20	150	230	310	310	310	310
30	100	180	260	340	340	340
40	50	130	210	290	370	370
50	0	80	160	240	320	400

Information that you will receive: After each round you will be informed about the number of hours you have spent on Activity A, the lowest number chosen by all of the employees in your firm, the firm's payoff, your payoff for the latest round, and your accumulated payoffs through the current round. You will also be shown the decisions by you and the decisions of all the other employees of your group from the current and previous rounds.

Payment: At the end of the experiment you will be paid, in cash, the sum of the payoffs that you will have earned in the rounds of the experiment at a rate of €1/1000. As noted previously, you will be paid privately and we will not disclose any information about your actions or your payoff to the other participants in the experiment.

Payoff Quiz

Before we begin the experiment, please answer the following questions. For all of these questions, assume that W=250 and B = 8. This gives employees the payoff table shown below. We will go through the answers to a sample problem before you do the rest of the quiz. Please raise your hand if you are having trouble answering one of the questions.

Table B2. Flat wage $W = 250$ and Bonus rate $B = 8$

My Hours on Activity A	Minimum Effort Spent on Activity A by Other Employees					
	0	10	20	30	40	50
0	250	250	250	250	250	250
10	200	280	280	280	280	280
20	150	230	310	310	310	310
30	100	180	260	340	340	340
40	50	130	210	290	370	370
50	0	80	160	240	320	400

Sample Question: Suppose you choose to spend 10 hours on Activity A. The other employees choose to spend 30, 20, and 50 hours on Activity A.

The minimum number of hours an employee of the firm spends on Activity A is __10____

Your payoff is ___280_____ pesetas.

1) Suppose you choose to spend 20 hours on Activity A. The other employees choose to spend 30, 0, and 10 hours on Activity A.

The smallest number of hours an employee of the firm spends on Activity A is______

Your payoff is ________ .

2) Suppose you choose to spend 0 hours on Activity A. The other employees choose to spend 20, 30, and 10 hours on Activity A.

The smallest number of hours an employee of the firm spends on Activity A is ______

Your payoff is ________ .

3) I am grouped with the same three individuals for all thirty rounds of the experiment (True/False)? ________

4) My actions and payoffs will be confidential (True/False)? ________

Chapter 3

Excess Entry and Ambiguity Seeking: an Experimental Study in Two-market Entry Games (Jointly with Jordi Brandts)

3.1 Introduction

Most economic decisions are made under uncertainty. Decision makers are often faced with very complicated situations out of their control, both related to the noise coming from other participants and the uncertain political, environmental and technological developments around. Over the past fifty years in the academic studies, decision making under uncertainty was mostly viewed as choice over a number of outcomes with known probabilities. However, it is not obvious at all why decision makers should know probability. Recently, there are growing interests in studying uncertainty in strategic games again and mainly on decision making with unknown probabilities.

It is a long story on the discussion of uncertainty about probabilities. Knight (1921) is the first to distinguish ambiguity from risk, where probabilities are unknown or imperfectly known as opposed to a situation under risk where probabilities are known. At the same time, Keynes (1921) also considers it "a very confusing problem" that how to use available information to evaluate unknown probability. It is Ellsberg (1961) who gives clear evidence to distinguish risk from ambiguity and illustrates how ambiguity can affect decision making in important ways. The

Ellsberg experiments seem to suggest that subjects avoid the options with unknown probability. Camerer and Weber (1992) provide a comprehensive survey of the literature on experimental studies of decision-making under uncertainty with unknown probabilities of events. Based on this literature, Camerer and Weber (1992) view ambiguity as "uncertainty about probability, created by missing information that is relevant and could be known" (Camerer and Weber (1992), page 330). Experimental studies confirm a preference for betting on events with information about probabilities and ambiguity aversion is found in most experiments on individual decisions.

Most decisions in empirical life are without the guidance of available information about probabilities, but people are not always averse to such situations. A class of examples in the field of industrial organization describes the decisions of firms on whether to enter a market. Evidence of "Excess entry" and the high rate of business failure has been reported by many empirical studies. Timothy Dunne et al. (1988) estimate that 61.5 percent of all entrants exited within five years and 79.6 percent exited within ten years. Most of these exits were failures. (see also Daniel Shapiro and R.S. Khemani, 1987; Dunne et al., 1989a,b; Paul A. Geroski, 1991; John R. Baldwin, 1995). Urban and Hauser (1993) report an average failure rate of 35% across different industries. Entrepreneurs can only have vague information about the market demand but are not able to know the probability. However, they behave aggressively in entering the market. In another example of freeway congestion problem, many commuters face the daily dilemma of taking a predictable, but slower route or risking hours of gridlock on a potentially faster freeway. However, there is no sign of less crowded on a faster freeway. Less seriously, a scene from the movie about John Nash, "A Beautiful Mind", describes the decisions of a group of men whether to pursue a blonde woman and several brunettes. Nash suggests that the men ignore the blonde and each concentrates on a (different) brunette because of the high uncertainty in pursuing the blonde woman. However, as a social phenomenon, you may see all pursue the blonde and each thinks he will win.

All these phenomena have the characteristics in common in which each player chooses whether to participate in an activity and payoffs depend on the number of players who do so and decrease with the number of participants. All these decisions are related to great uncertainties, but excessive participation is salient in such situations. Such evidence of decisions in strategic games is not consistent with the findings of aversion to choices with higher uncertainties in individual decision problems.

In the present paper, we will study experimentally decisions under uncertainty of risk and ambiguity in a kind of strategic game, a market entry game, which captures the basic features of the phenomena mentioned above. The important task is to find out whether the excessive participation is related to the information types of risk and ambiguity, and whether decisions are different in risk and ambiguity. In the market entry game, subjects must choose among three choices, staying out with a fixed payoff, entering a risky information market or entering an ambiguous information market. In each of the two latter choices, payoffs are decided by both the variance of the number of entrants and uncertainty (risk or ambiguity) of market capacities. We also do a control treatment to see whether decisions are different in a strategic game and an individual choice problem. In the individual choice experiment, payoffs in each of the two latter choices are decided only by the uncertainty (risk or ambiguity) of market capacities but not the number of entrants.

The phenomenon of ambiguity aversion—or the preference for gambles with known as opposed to unknown probabilities—has been well documented in the literature on individual decision making in psychology. However, there are only a few studies of ambiguity in strategic games, and they find mixed results. For example, Sarin and Weber (1993) study ambiguity in an experimental asset market using auctions and find that the market price for the unambiguous bet is considerably larger than the market price of the ambiguous bet, which can be explained by ambiguity aversion. Chen et al. (2006) study ambiguity in the first and second sealed bid auctions and find that in first price auctions, bids are lower with the presence of ambiguity, which can be explained as ambiguity loving. Ambiguity about missing information is defined in many different ways. Two most noted ways are used in the above papers separately. In Sarin and Weber (1993), ambiguity is defined as compound lotteries, a sophisticated expression of probability, such as second-order probability. Chen et al. (2006) study risk and ambiguity, where probability in ambiguity is totally unknown, and the probability effectively occurs with asymmetric probabilities (say $p = 0.3/0.7$ instead of $p = 0.5/0.5$) in the first and second sealed bid auctions.

In our experiment, we treat the settings in the simplest way. In the risky information market, there are two possible market capacities, both known to occur with probability 1/2. In the ambiguous information market the two possible market capacities (the same as those used in risk) effectively occur with probability 1/2 but participants are only told that there is uncertainty about capacities.

A market entry game with the basic features of business entry situations is first studied in the experiments by Daniel Kahneman (1988), and then is explored more thoroughly by Amnon Rapoport and his colleagues (Sundali et. al, 1995; Rapoport et al. 1998; Rapoport, Seale and Winter, 2000; Rapoport, Seale and Winter, 2002 (thereafter RSW), Rapoport, Seale and Parco, 2002). One common characteristic of all the market entry experiments above is that the entry decision is made under a given market capacity. Although researches find that the subjects enter a bit too frequently at first, interactive decisions of agents are accounted for surprisingly well on the average frequencies of entry by the equilibrium solution after the game being iterated for a large number of periods. To Kahneman, the behavioral regularities found in this game looked "like magic" (Kahneman, 1988, pp. 12).

Our main departure from previous experimental literature on this game is that players do not know the exact value of market capacity. This change simulates the empirical life in a closer way. It may help explain many empirical puzzles, especially the excess entry and high rate of business failure in the field. Our results somehow prove the internal relation between excessive participation and uncertainty about information. We observe more entrants in the ambiguous information market than in the risky information market. Especially overentry is salient in the ambiguous market. While in the individual choice treatment which involves only uncertainty of risk and ambiguity, decisions of entry is similar in the two markets. We find that in the strategic environment, individuals control their entry not too frequently when they know the probability, while they don't do so when they do not know it. Such controlling behavior appears only when subjects are situated in a strategic environment.

The paper is organized as follows. The next section introduces the market entry game and characterizes its equilibria. Section 3.3 presents the experimental results and carries out the analyses on the results. Section 3.4 concludes the paper. The instruction used in the strategic game is attached in Appendix.

3.2 Experimental design

3.2.1 A two-market entry game with fixed capacity

A two-market entry game is played by a group of N symmetric players facing two independent markets. They must make decisions simultaneously and independently on one choice, whether to enter one of the markets or to stay out. One very simple and frequently used formulation is where payoffs are linear in the number of entrants or players. The payoff to player i's strategy of staying out ($S^i = X$), entering one market ($S^i = Y$) or entering the other market ($S^i = Z$) is computed from the following formula, which is common knowledge:

$$\pi_i = \begin{cases} v, & \text{if } S^i = X \\ v + r(c_Y - m_Y), & \text{if } S^i = Y \\ v + r(c_Z - m_Z), & \text{if } S^i = Z \end{cases}$$

Where v, r are positive constants. The values of m_Y and m_Z denote separately the number of agents that choose entry in market Y and market Z, and the sum of both values always satisfies $0 \leq m_Y + m_Z \leq N$. The values of c_Y and c_Z are interpreted separately as the capacities of market Y and market Z and are publicly known. The constraint $1 \leq c_Y + c_Z < N$ can make it possible that the payoff of entering is not always higher than that of staying out. In this noncooperative N-person game, the return to entry exceeds the return to staying out, a fixed v, if and only if $m_j < c_j$ $(j = Y, Z)$.

In characterizing the pure-strategy equilibria, we denote equilibrium entrant numbers in each market m_Y^* and m_Z^* and those choosing to stay out equal $m_X^* = N - m_Y^* - m_Z^*$, and distinguish between two cases, depending on whether $v = v + r(c_j - m_j^*)$ or $v \neq v + r(c_j - m_j^*)$. If c_j is not an integer, there are $N!/m_Y^*!m_Z^*!m_X^*!$ pure strategy equilibria with $m_j^* = |c_j|$, where $|c_j|$ is the largest integer smaller than c_j. If c_j is an integer, then there exist $N!/m_Y^*!m_Z^*!m_N^*!$ pure-strategy equilibria with $m_j^* = c_j$ and, in addition, $N!/m_Y^*!m_Z^*!m_N^*!$ pure-strategy equilibria with $m_j^* = c_j - 1$.

There also exists a symmetric mixed-strategy equilibrium, where each player i either enters into market Y, enters into market Z, or stays out with respective probabilities E_Y^i, E_Z^i, or $1 - E_Y^i - E_Z^i$, where E_j^i is the probability of entry by player i in market j. For $c_j > 1$, the symmetric mixed strategy equilibrium in a market is given by

$E_j^i = \frac{c_j - 1}{N-1}$ for $i = 1, ..., N$ and $j = Y, Z$

Note that the expected number of entrants in the symmetric mixed equilibrium is $N * E_j^i$, which is different from but can be very close to the pure strategy equilibria value m_j^*.

3.2.2 A two-market entry game with uncertain capacity

In the two-market entry game studied in our experiments, instead of being a fixed capacity definitely announced, c_j is not publicly announced and two possible values $\overline{c}_j$ and $\underline{c}_j$ are provided and occur with probability p and $1-p$ separately, which are the given information when decisions are made.

Based on the assumption of the expected utility (EU) theory and risk neutrality, and in view of the fact that payoffs are linear in the parameter c_j, we can carry on the equilibrium analysis easily by using similar method mentioned above. In view of the pure strategy equilibrium, the payoff of entry is in a form of expected value $U(m_j^*) = p(v + r(\overline{c}_j - m_j^*)) + (1-p)(v + r(\underline{c}_j - m_j^*))$, in brief, $U(m_j^*) = v + r\left\{\left[p\overline{c}_j + (1-p)\underline{c}_j\right] - m_j^*\right\}$. We distinguish between two cases depending on whether $v = U(m_j^*)$ or $v \neq U(m_j^*)$. If $p\overline{c}_j + (1-p)\underline{c}_j$ is an integer, then pure equilibrium entrant number is $m_j^* = p\overline{c}_j + (1-p)\underline{c}_j$ or $m_j^* = p\overline{c}_j + (1-p)\underline{c}_j - 1$; If $p\overline{c}_j + (1-p)\underline{c}_j$ is not an integer, then $m^* = \lfloor p\bar{c}_j + (1-p)\underline{c}_j \rfloor$ where $\lfloor p\bar{c}_j + (1-p)\underline{c}_j \rfloor$ is the largest integer smaller than $p\bar{c}_j + (1-p)\underline{c}_j$.

The symmetric mixed Nash equilibrium is of the form $E^i_j = \frac{\left(p\bar{c}_j + (1-p)\underline{c}_j\right)-1}{N-1}$ for $i = 1, ..., N$. Then the expected number of entrants in mixed strategy is $N \cdot E^i_j$.

3.2.3 Description of the experiment

Our experiments involve repeated play of the two-market entry game with uncertain capacity over 50 periods. There are 2 treatments in our experiments. Treatment 1 simulates the market entry game described above by a group of 7 people with defined values of parameter v and r in the payoff functions. Treatment 2 is a revised version of market entry game used in Treatment 1, where it is an individual choice experiment, and hence payoffs of each individual are independent of other players, thereafter, besides the values v and r, m_j is given a fixed value. We first explain the experimental design of the strategic environment in Treatment 1, and then explain the individual choice problem in Treatment 2, and clarify the procedures of the experiments in the end.

Treatment 1: strategic game.

We begin by discussing the parameterization of the payoff function. We choose to set $v = 12$ and $r = 2$ resulting in the following payoff functions (in experimental points):

$$\pi_i(\partial) = \begin{cases} 12, & \text{if } S^i = X \\ 12 + 2(c_Y - m_Y), & \text{if } S^i = Y \\ 12 + 2(c_Z - m_Z), & \text{if } S^i = Z \end{cases}$$

Where we use three actions X, Y and Z to denote the choices of "stay out", "enter into market Y" and "enter into market Z". $0 \leq m_Y + m_Z \leq 7$ is the number of subjects (including subject i) choosing entry. c_j $(j = Y, Z)$ is the actual market capacity occurring in one period in market j.

In our experiments, we set two values $\underline{c}_j = 1.1$ or $\bar{c}_j = 3.1$, which occurs with probability $p = \frac{1}{2}$ separately in one period. Instead of randomizing the appearance of $\underline{c}_j$ and $\bar{c}_j$ with probability $\frac{1}{2}$ in the process of each session, we use a computer to generate one realization for each market before the experiments. Such a method can keep the two realizations uniform in various sessions and can avoid the noise in the realization-generated process[1]. In half of the sessions, we set realization 1 for market Y and realization 2 for market Z, in the other half of

[1] Although we set the command $p = \frac{1}{2}$ in 50 periods in the computer, among various realizations generated, one of the two values ($\underline{c}_j$ and $\bar{c}_j$) often appears in more than 30 periods. As a result, we choose two realizations in which two values appears quite evenly around 25 periods out of 50 periods separately.

the sessions, we switch the two realizations for the two markets. Since our focus is to compare entry decisions in the two markets, We try to avoid results distorted by the sequence of $\overline{c}$ and $\underline{c}$ values in a realization. As a result, to switch the two realizations in the two markets can solve the problem.

We choose $\underline{c}_j$ and $\overline{c}_j$ to be non-integers so that there exists only one pure equilibrium number of entrants [2]. At the same time, their values are close to an integer so that in equilibrium the difference in payoff to those entering one market and those staying out remains quite small. At the same time, our design avoids any possible negative payoffs generated from the equation.

Our choice of $N = 7$ is based on the following consideration. Most previous studies of market entry game with fixed capacity use a group of 20 players. Since we add the difficulties to subjects in understanding c_j and making entry decisions, we prefer a simple payoff structure and easy calculations. All these favor our choice of a smaller number of subjects.

In our experiments, group members knew they were playing with fixed partners $N = 7$ in all periods. Instead of random matching, where subjects change partners in every period, we use fixed matching based on the following considerations. On one hand, it can provide many independent observations in the unit of groups and makes it easy for the statistic analysis. On the other hand, fixed matching is used in most studies of market entry games, and it is good to see how differently individuals behave under uncertain capacity.

The tasks described in the instructions are not framed in a market situation. Since the two-market entry game can reflect various aspects in empirical life, such as investment in stock markets and commuter decisions in avoiding traffic jams, without framing the game, observations can reflect general views in such strategic games while will not be distorted by a specific environment.

Subjects were told they were faced with 3 choices. By choosing choice X, one can always earn a fixed amount 12.

In choice Y and choice Z, payoffs are decided by both capacity c_j and the number of entrants m_j. In other words, subjects were faced with two uncertainties, uncertainty about capacity and uncertainty about others' strategy. Especially in our experimental design, we differentiate two different uncertainties about capacity in each market.

In choice Y, subjects were told that capacity c_j was from one of the two values $\underline{c}_j = 1.1$ and $\overline{c}_j = 3.1$ each occurring with probability $\frac{1}{2}$, an uncertain capacity with known probability (thereafter we call it Risk). In choice Z, subjects only knew that capacity c_j was from one of the two values $\underline{c}_j = 1.1$ and $\overline{c}_j = 3.1$ and the probability for each to appear kept the same in all

[2] If $\underline{c}_j$ and $\overline{c}_j$ are integers, there exists two pure equilibria entrant numbers.

periods, but they were not told the probability, an uncertain capacity with unknown probability (thereafter we call it Ambiguity). In principle, the true probability about $\underline{c}_j$ and $\overline{c}_j$ in choice Z is the same as that in choice Y, $\underline{c}_j = 1.1$ and $\overline{c}_j = 3.1$ each occurring with probability $\frac{1}{2}$.

In order to ensure they understand their payoffs from choosing Y and Z clearly, the instructions also include the following four tables revealing all possible payoff values from choosing action Y and action Z. In the first two tables about the payoffs in choice Y, each lists all possible payoffs in the conditions of different number of entrants in $c = 1.1$ and $c = 3.1$ separately, where it clearly denotes that each capacity occurs with probability $\frac{1}{2}$. In the last two tables about payoffs in choice Z, the content in the tables is exactly the same, while the tables clearly denote that probability is unknown in each capacity and the same in all periods.

Payoffs in choice Y

Payoffs in low capacity, $c = 1.1$ with probability $\frac{1}{2}$							
Fraction of players who choose action Y	1/7	2/7	3/7	4/7	5/7	6/7	7/7
Payoff each earns from choosing action Y	12.2	10.2	8.2	6.2	4.2	2.2	0.2

Payoffs in high capacity, $c = 3.1$ with probability $\frac{1}{2}$							
Fraction of players who choose action Y	1/7	2/7	3/7	4/7	5/7	6/7	7/7
Payoff each earns from choosing action Y	16.2	14.2	12.2	10.2	8.2	6.2	4.2

Payoffs in choice Z

Payoffs in low capacity, $c = 1.1$ with unknown probability but uniform over periods							
Fraction of players who choose action Z	1/7	2/7	3/7	4/7	5/7	6/7	7/7
Payoff each earns from choosing action Z	12.2	10.2	8.2	6.2	4.2	2.2	0.2

Payoffs in high capacity $c = 3.1$ with unknown probability but uniform over periods							
Fraction of players who choose action Z	1/7	2/7	3/7	4/7	5/7	6/7	7/7
Payoff each earns from choosing action Z	16.2	14.2	12.2	10.2	8.2	6.2	4.2

Treatment 2: individual choice game.

Compared with Treatment 1, where subjects make choices under both uncertainty about others' strategies and uncertain situations of risk and ambiguity, Treatment 2 studies individual choices when subjects only face the uncertainty about capacities. In order to form a good comparison of individual attitude in risk and ambiguity situations between a strategic game and an individual choice game, we use similar payoff functions and instruction in Treatment 2. The only difference from the parameterization used in Treatment 1 is that instead of being a variable decided endogenous in the experiments, the values of m_Y and m_Z are fixed, where we set $m_Y = 2.1$ and $m_Z = 2.1$[3] separately.

The payoff functions (in experimental points) used in the experiments are following:

$$\pi_i(\partial) = \begin{cases} 12, & \text{if } S^i = X \\ 12 + 2(c_Y - 2.1), & \text{if } S^i = Y \\ 12 + 2(c_Z - 2.1), & \text{if } S^i = Z \end{cases}$$

The design of the uncertainty about capacity c_j is exactly the same as those in Treatment 1.

In order to ensure they understand their payoffs from choosing Y and Z clearly, the instructions also include the following two tables revealing all possible payoff values from choosing choice Y and choice Z. The first table below denotes the two possible payoffs in choice Y, the payoff is 10 in low capacity situation and it is 14 in high capacity situation occurring with probability $\frac{1}{2}$ separately. The second table provides the same information about payoffs, but different information about probability in choice Z, where low or high capacity occurs with unknown probability and the same in all periods. You may find that the payoffs structure in Treatment 2 is very simple, and our complicated way of telling the choices by showing the equations firstly is just to make comparable instructions between Treatment 1 and Treatment 2.

Payoffs in choice Y

Capacities in choice Y	Low capacity $c = 1.1$ (with probability $\frac{1}{2}$)	High capacity $c = 3.1$ (with probability $\frac{1}{2}$)
Payoff from choosing action Y	10	14

Payoffs in choice Z

Capacities in choice Z	Low capacity $c = 1.1$ (with unknown probability but uniform over periods)	High capacity $c = 3.1$ (with unknown probability but uniform over periods)
Payoff from choosing action Z	10	14

Procedures:

Treatment 1 and Treatment 2 follow the same steps in the experiments. We use Z-tree[4] to program our experiments. After the paper instructions were read aloud, subjects completed a set of Review Questions on the computer terminals to test their understanding of the instructions. They could not finish this part until they had answered all the questions correctly. Subjects

[3]The value 2.1 is quite close to the pure strategy eqilibrium number of entrants in the strategic game in Treatment 1.

[4]Fischbacher, U., (2007), z-Tree: Zurich Toolbox for Ready-made Economic Experiments.

were told in the instructions that they would make one choice among choice X, choice Y and choice Z in each period, and repeatedly play the same game in 50 periods.

Each period took place in the following way:

1. All the subjects made decisions simultaneously between three actions X, Y or Z without communication among them,

2. The capacity was not known to subjects at the beginning of one period when they made decisions,

3. At the end of one period, the only information they received is their own payoffs and their payoff history.

We only provide payoff informations to subjects because of the following reasons. First, we try to simulate the situation in the field. In a strategic environment described in Treatment 1, such as in a market, where entrepreneurs face with both competitors and market situations, they are only able to know the final result of their decisions, their payoffs, but not how their payoffs are decided by the two factors. Hence their information about how their payoffs are realized is not complete. While the situation is different in individual choice game described in Treatment 2, where payoff information can reflect market situations, so their information about how the payoffs are realized is complete. As a result, in our settings, the information feedback is not complete in Treatment 1 and it is complete in Treatment 2. We accept the natural difference occurring in a strategic environment and an individual choice environment. We would not like to manipulate a setting of complete information feedback in a strategic environment in Treatment 1.

Second, we try to provide as little information as possible to subjects and make it harder for subjects to learn probabilities and other's strategies in ambiguity situations in Treatment 1. Once we release information more than their payoffs, for example, number of entrants or the occurrence of $\underline{c}$ or $\overline{c}$, individuals may use it to adjust their conjecture on probability or on others's strategies. Although we use fixed matching in repeated periods, our focus is not on coordination or learning issues. We want to collect enough observations of decisions in one-short game to study individual attitude to risk and ambiguity situations. The way of providing least information can help subjects learn about the game instead of learning about probability or other's strategy.

109 students from the Universitat Autónoma de Barcelona of Spain participated in our experiments. They were recruited through E-mail invitations and reading notices on the experimental recruiting website using ORSEE system[5]. Each subject was only allowed to participate

[5] Greiner, B., (2004). An Online Recruitment System ORSEE.

in a single session that lasted around 45 minutes. 84 students participated in Treatment 1 in 12 groups of seven, and we ran four sessions[6] with 21 subjects seated in each session. 25 students participated in Treatment 2, and we ran two sessions[7] with 12 subjects and 13 subjects in each session.

3.2.4 Equilibrium predictions of Treatment 1

In the risky choice Y, where $c = 1.1/3.1$ with known probability $\frac{1}{2}$, pure strategy Nash equilibria have 2 players always entering each of the two markets, while the symmetric mixed strategy Nash equilibrium predicts an entry probability 0.183 and the expected number of entrants 1.281 in choice Y.

Although the parameterization in the ambiguous choice Z is exactly the same as that in the risky choice Y, in view of the minimum information (only payoff information) given in the end of each period, the ambiguous information about probability is hard to be revealed even after many periods. Therefore it is hard to predict how players hold their views on an ambiguous state and react to the opponent's strategy. It is rational to think that subjects should expect equal probabilities on high and low states. We will take the equilibria in the risky information market as a benchmark to carry out our analysis in the ambiguous information market.

In most of the literature on fixed matching playing market entry games, coordination and learning in repeated interaction are the main research focus, where individuals' coordination on the behavior of others lead to an asymmetric equilibrium, and learning model predicts convergence to a pure equilibrium even though such play may take a long time to emerge.

In the present experiments, in Treatment 1 with least information feedback, uncertain information on probability may disturb individual's preference to one choice, at the same time without knowing the number of entrants in each market clearly, coordination becomes very hard. The two factors may make coordination and learning impossible even among partners over 50 periods. Given the assumption of identical incentives among players, one might think that the mixed symmetric equilibrium is particularly salient.

3.2.5 Questions and Hypotheses

Our experimental design is intended to address two specific research questions.

[6] As have been mentioned above about the realizations of capacities $\underline{c}$ and $\overline{c}$, in two sessions we use realization 1 in choice Y, and realization 2 in choice Z; in the other two sessions, we switch the settings of two realizations in the program in choice Y and Z.

[7] Here in one session we use realization 1 in choice Y, and realization 2 in choice Z; in the other session, we switch the settings of two realizations in the program in choice Y and Z.

Question 1: Do individuals enter more or less into the market with ambiguous information?

Ambiguous information can be understood in different ways. Among them, Camerer and Weber (1992) construct a pragmatic definition of ambiguity, where "Ambiguity is uncertainty about probability, created by missing information that is relevant and could be known". Although "ambiguity aversion" is never been clearly defined, such a notion is used in variants of Ellsberg problems, in which decision makers prefer the choice with known probabilities. Prior studies have widely accepted that individuals are averse to ambiguous situation of lacking probability information in one-shot individual choice experiments.

We intend to know individual preference to risk and ambiguity in strategic games. As a result, the notion of ambiguity aversion in present paper is not exactly the same as ambiguity aversion mentioned in the above individual choice experiments. In their experiments, ambiguity aversion is defined as the aversion to an individual choice environment with unknown probability. Comparatively, we define ambiguity aversion as the aversion to a strategic environment with unknown probability. However, they have the essence of the notion, that is, aversion to an environment with unknown probability. In our experimental settings, it is ambiguity aversion if there are less entrants in Ambiguity market than in Risk market, and vise versa.

Economic analysis is based on full rationality assumption and hence hold the conjecture of ambiguity aversion assumption in explaining behavior, while psychologists find out many ambiguity seeking phenomena occurring in certain conditions, which may result from two kinds of "mistakes". One is "competitive blind spots", that is, agents fail to appreciate how many competitors they will face. The second is overconfidence, a phenomenon that has been documented in many contexts. Camerer and Lovallo's (1999) participants in a kind of market entry game exhibit ambiguity seeking when they are influenced by their confidence in evaluating their skills. Additionally, Heath and Tversky's (1991) use "competence hypothesis" to explain ambiguity seeking found in their research. In their discussion, firms are born of feelings of competence in running a business.

It seems that although ambiguity aversion is widely accepted in individual choice experiments, studies reveal that such preference can be reversed if they feel competent and make certain mistakes, such as overconfidence, in an ambiguous environment, hence one may expect to see more optimistic behavior in a situation of Ambiguity. In Ellsberg-type decision problems, lack of information cannot be overcome by personal confidence, but in a strategic environment involving competition, lack of information can be the source of optimism and overconfidence.

Treatment 1 studies risk and ambiguity in a strategic game. It simulates a situation where both competition and cooperation are needed. A subject may be cooperative to choose to stay out to release the possible tension in the market, while he may be competent to try to enter a market for extra payoffs. A subject may neglect the bad situation and is overconfident in winning in the market. As a result, we hold the conjecture of ambiguity seeking in such situations, where there are more entrants in the ambiguous information market than in the risky information market. Comparatively, Treatment 2 is a good control, where strategic interactions and emotional factors do not play roles, to find out how differently subjects treat risk and ambiguity in individual choice experiments.

If we do find subjects behavioral difference in the risky information market and the ambiguous information market, we are also interested in the following question.

Question 2: Do any effects of ambiguity persist in the face of experience and feedback?

Prior studies on the attitude to risk and ambiguity are mainly in one-shot games. In our studies, we do not intentionally study how subjects learn from the history, but we also do not refuse the possible sequential dependencies of decisions over periods. Quite possibly, such sequential dependencies may reveal the reasons of subjects preference. In Treatment 2, there may exist entry preference in one of the two markets at the beginning of the periods, however, evidence about payoff can provide exact information about the capacity, hence missing information about probability is able to be revealed in the ambiguous market, and in the end individuals may treat the two markets equally. We simulate the information feedback of a strategic environment in Treatment 1, the feedback given at the end of one period is not enough to reveal the capacity in that period and to help predict probability correctly in the ambiguous information market. It is interesting to investigate that with least information, whether the difference between risk and ambiguity is persistent, diminishes or disappears, and how the least information about payoffs will introduce difficulty for subjects to understand probability.

Chen et al. (2006) study ambiguity effects in repeated play of the first and second sealed bid auctions lasting 30 periods. Ambiguity loving was found only in Rounds 1–5. In the other research (Sarin and Weber, 1993) on the ambiguity effects in an asset market bidding experiment finds that aversion to ambiguity does not vanish in the face of market incentives and feedback but with short repeated periods of 8.

The question is related to the concept of the *weight of evidence,* an old theory raised by Keynes (1921). It distinguishes the probability of an event from the evidence supporting it, and appears closely related to the notion of ambiguity arising from known-to-be-missing information (Camerer (1995), pp. 645). Generally, the weight of evidence can be defined as the amount of available information relative to the amount of conceivable information. The gap is the amount of missing information. In our settings in Treatment 1, the implication of the evidence may be vague in judging probability but it is interesting to know how people evaluate available information and how they use it to predict probability. Since economic decisions in empirical life is an continuous process, to study this question may provide instructions to behavior in the practice.

3.3 Experimental results

Our experimental results are separated in two parts. In the first part, we will report the experimental results of the strategic game described in Treatment 1. In the second part, we will report the experimental results of the individual choice game described in Treatment 2.

3.3.1 Results of the strategic game in Treatment 1

We first explore general entry behaviors into the two markets by observing group behavior and aggregate data of all subjects over periods, and then individual entry behaviors are analyzed to account for the findings from aggregate data. Finally, we go to individual decision process to explore the reasons of ambiguity effects, where people's switching behavior and coordination are studied.

Aggregate results on the number of entrants

We get into the heart of the matter by comparing entrant numbers in the risky information and ambiguous information markets. We present a general view of entry situations in each of the 12 groups separately. Table 1 reports the observed mean number of entrants into the two markets in each group in the first 25 and the last 25 periods separately. By comparing the mean number of entrants in the risky and ambiguous information markets in each group and calculating number of groups with persistent preference to a market, we can make statistic analysis on how significantly ambiguity aversion or ambiguity seeking appears. By comparing group members' mean number of entrants in one market in the first 25 and the last 25 periods, we can find out whether group members behavior keeps the same pattern or converge to certain choice.

Table 1 lists the mean numbers of entrants of each group in the risky information market, the ambiguous information market in periods 1-25, and the risky information market, the ambiguous information in periods 26-50 from left to right in each row. The values in the squares are the variance of mean entrants number in 25 periods. The last two rows are the simple statistics on the number of groups with more mean entrants in the risky choice, where it is 0 in periods 1-25 and 2 in periods 26-50, and the simple binomial distribution on the assumption of probability 0.5 for binomial choices (more entrants in Risk or more entrants in Ambiguity), where it is 0.00 (calculated by $\binom{12}{0}0.5^0 0.5^{12}$) in periods 1-25 and 0.054 (calculated by $\binom{12}{2}0.5^2 0.5^{10}$) in periods 26-50. The observations have the following suggestions.

Table 1. Observed mean number of entrants by group

	Periods 1-25		Periods 26-50	
	Risk	Ambiguity	Risk	Ambiguity
Gr.1	1. 8	2. 52	2. 24	1. 92
	(1. 135)	(1. 174)	(1. 145)	(1. 132)
Gr.2	1. 96	2. 48	2. 44	2. 24
	(1. 219)	(1. 174)	(1. 239)	(0. 909)
Gr.3	1. 56	2. 6	1. 8	2. 08
	(1. 102)	(0. 851)	(0. 983)	(0. 893)
Gr.4	1. 84	2. 16	2	2. 28
	(1. 010)	(1. 049)	(0. 695)	(0. 920)
Gr.5	1. 76	2. 36	1. 96	2. 36
	(1. 179)	(1. 057)	(0. 826)	(1. 130)
Gr.6	1. 8	2. 2	1. 72	2. 6
	(1. 023)	(1. 061)	(0. 920)	(1. 135)
Gr.7	1. 8	2. 48	1. 76	1. 92
	(1. 061)	(0. 902)	(1. 072)	(0. 629)
Gr.8	1. 6	1. 76	1. 28	1. 32
	(0. 983)	(1. 369)	(0. 920)	(0. 971)
Gr.9	2. 12	2. 12	2. 12	1. 8
	(1. 073)	(1. 180)	(1. 180)	(0. 983)
Gr.10	1. 72	1. 72	1. 48	1. 64
	(1. 253)	(0. 963)	(1. 103)	(0. 845)
Gr.11	1. 44	1. 68	1. 56	1. 88
	(0. 806)	(0. 884)	(0. 755)	(0. 713)
Gr.12	1. 88	2. 32	2. 28	2. 56
	(1. 110)	(1. 520)	(0. 963)	(0. 986)
Average	1. 773	2. 2	1. 887	2. 05
	(1. 097)	(1. 155)	(1. 049)	(1. 014)
Num. of Gr. (*Risk* $\geq$ *Ambiguity*)	0		2	
Pro. (*Risk* $\geq$ *Ambiguity*)	0. 00		0. 054	

In the first 25 periods, the entrant number in ambiguity is higher than that in risk in all 12 groups. In the last 25 periods, besides group 1 and group 2, the preference of entry for ambiguity keeps the same in other groups, where we can accept the difference with significantly high probability 0.946. The observations from aggregate data by group reveal that individuals prefer to choose the ambiguous information market, and such a preference becomes weaker but does not disappear over time.

Table 1 provides us the first impression on the difference between the entry decisions into the two markets. We do find ambiguity seeking, and especially it seemly does not disappear after iteration of 50 periods. In order to acquire a general view on how the number of entrants changes over periods, The two graphs in Figure 1 report the aggregate average number of entrants by period and by every 5 periods separately. The two lines in each graph describe the numbers of entrants in the risky and the ambiguous information markets over all the subjects and how they change over 50 periods. In the up graph, besides the large difference between the ambiguous market (with the highest value around 3) and the risky market (with the lowest value around 1) in the first 5 periods, the numbers in both markets fluctuate between 1.5 and 2.5. It seems that the number in the ambiguous market is higher than the number in the risky market in most periods, but the difference is not very clear in the end of the periods. The down graph in Figure 1 reports the average number of entrants of all individuals in every 5 periods. We can clearly see that the number in the risky market is always closely below the pure strategy equilibrium value 2 and is always above the mixed strategy equilibrium value 1.281. Comparatively, the number in the ambiguous market is always above 2 except for the point denoting the value in periods 36-40. The findings indicate a clear preference of entering into the ambiguous market to the risky market over the first 30 periods. Combining the statistic analysis of the last 25 periods in Table 1 and the numbers in the last 20 periods in Figure 1, we think the difference of entrant number into the two markets gets smaller but does not disappear.

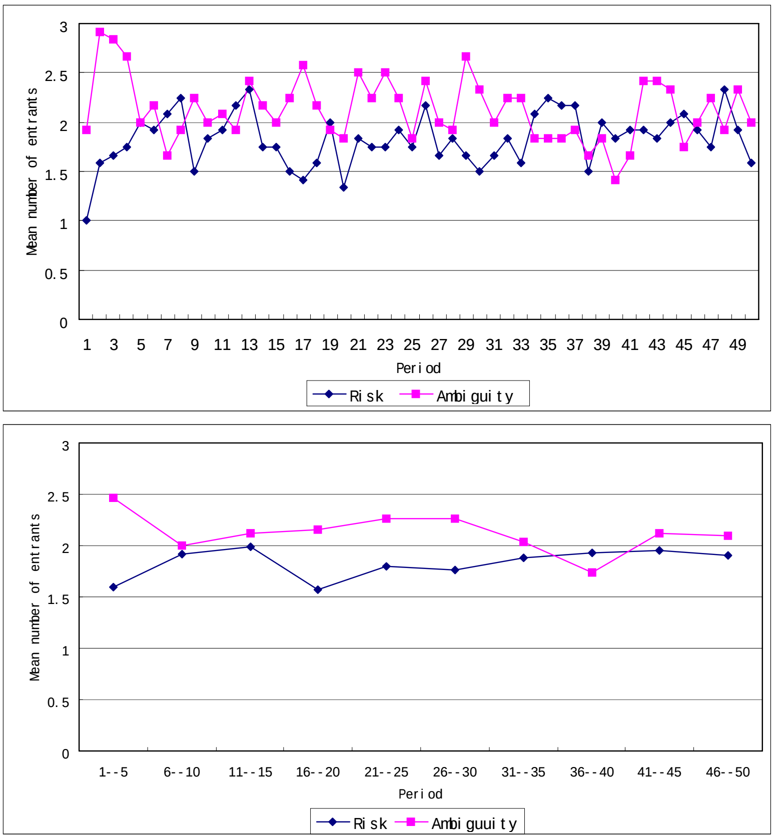
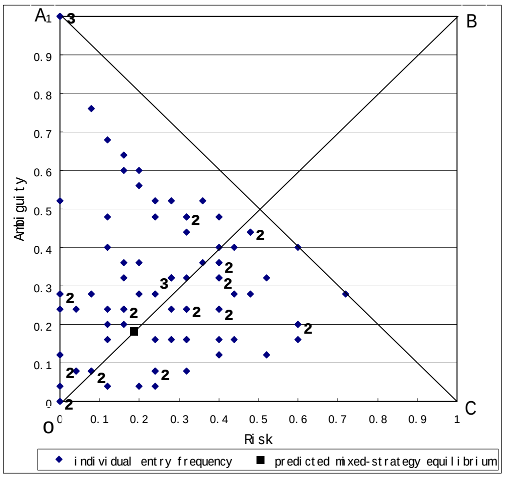


Figure 1: Mean number of entrants by period (up) and by every 5 periods (down) in Treatment 1.

Till now, we have provided preliminary answers to Question 1 and Question 2. Since the two-market entry game is played by 7 fixed members repeatedly in 50 periods, observations by group hide some very important informations: whether ambiguity seeking results from a few subjects' extreme behavior in entry decisions in each group, or represents majorities choice preference? whether persistent ambiguity seeking results from consistent behavior of all the individuals in a group or from the balance of different types of subjects convergence to different choices?

Individual results of entry frequency

In this part, all the analyses focus on individual choices in the risky and ambiguous information markets. Our first attempt to observe individual differences is by plotting individuals' proportions of entry to each market. Here we also separate the observations over time in two parts, the first 25 periods and the last 25 periods. The two graphs in Figure 2 show individual entry frequencies in the first 25 and the last 25 periods separately. Both graphs plot the observed proportion of an individual's entry in the risky information market, denoted on X axis, against his proportion of entry in the ambiguous information market, denoted on Y axis. Each data point is based on 25 observations by individual. For example, the points on the diagonal line OB indicate individuals who have symmetric entry frequencies in the risky and the ambiguous information markets, while points on the diagonal line AC indicate individuals whose sum of entry frequencies in the two markets equals 1, in other words, those who never choose Out. In another example, the points on the Y axis represent individuals who have zero entry frequency in the risky information market. In each of the graphs, there is one benchmark point indicating the mixed-strategy equilibrium[8].



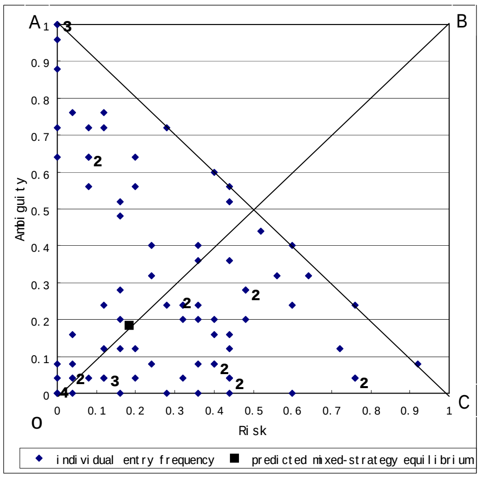


Figure 2: Observed individual entry frequency in periods 1-25 (left) and in periods 26-50 (right)

We find that individual differences are quite large, where the points are scattered without clear concentrated area in both graphs. Individuals clearly mix their strategy but in ways which are hard to account for by the symmetric mixed-strategy equilibrium. By comparing the observed entry frequencies in the first 25 and the last 25 periods separately in the two graphs, it appears that the points are more loosely distributed in the down graph in Figure 2. In particular, many points move to the edge of the graph (X axis, Y axis and the diagonal line AC).

We test the observations of individual level entry frequencies of the two graphs in Figure 2

[8] Predicted entry frequency in the risky information market is 0.183, and that in the ambiguous information market is also 0.183 on the assumption of neutral attitude of probability $\frac{1}{2}$ under unknown probability.

separately with Wilcoxon signed-rank test. Table 2 reports the results. The null hypothesis is that the distribution of individual entry frequencies is the same in the risky and the ambiguous markets. Z denotes the statistic difference between the two distributions. In periods 1-25, the test rejects the null hypothesis with probability 1.0000 and in periods 26-50, the test rejects the null hypothesis with probability 0.0776. The results suggest that the distributions of entry frequencies in the two markets are significantly different in periods 1-25, but are not in periods 26-50. They are consistent with the aggregate data of number of entrants over periods, where the difference between the two markets is small in the last 20 periods.

Table 2. Wilcoxon signed-rank test
on entry frequencies of Risk and Ambiguity

	Periods 1-25	Periods 26-50
Z value	0.000	−1.765
$Prob > \|Z\|$	1.0000	0.0776
Observations	168	168

The test on the first and the last 25 periods leaves us a question, that is, how does one individual change his choice over time, and how do the changes explain the diminishing difference between the two markets? At the same time, we are also interested in the decision of mixed strategies in repeated games. Following O'Neill (1987), studies of mixed strategies conducted in the last 10 years have mostly focused on finitely iterated two-person zerosum games with no pure-strategy equilibria, and mixed strategy was also found in market entry games with fixed capacities by Rapoport, Seale and Winter (2000) and Zwick and Rapoport (2002). The common characteristic in these studies is that there are significant departures from mixed strategy equilibrium play on the individual levels, where there are many subjects who either enter too frequently or too infrequently, and most importantly there may exist sequential dependencies that constitute adaptation and the repetition bias. So in what follows, we will study whether individuals randomize over 3 choices and how their mixed strategy changes in the later periods.

Figure 3 intends to trace one individual's behavioral changes in the first 25 periods and the last 25 periods. Each point represents the observed change of the proportion of entry in the risky information market, denoted on X axis, against the change of the proportion of entry in the ambiguous information market, denoted on Y axis, where the sign indicates the increase (positive) or the decrease (negative) of entry frequencies in one market in the latter periods. For example, points in area A correspond to a negative value for risk and a positive value for ambiguity, which corresponds to individuals who decrease their entry frequencies in the risky market and increase their entry frequencies in the ambiguous market in the latter part of the game. Comparatively, points in area D indicate individuals who did the opposite in the two markets.

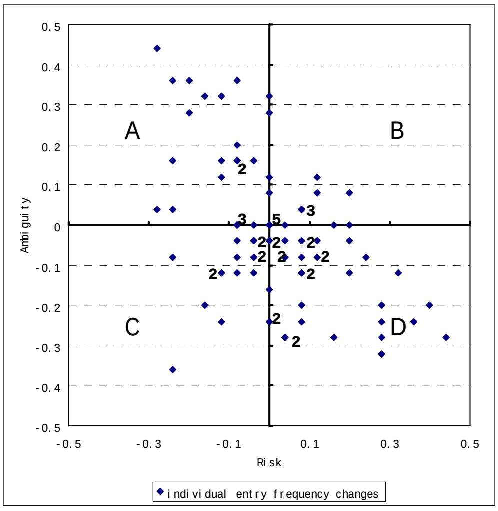


Figure 3: Observed changes of individual entry frequencies from periods 1-25 to periods 26-50.

Firstly we read the observations in Figure 3. by observing the distribution of points in the coordinates. Among the 84 points, there are 19%, 7%, 17% and 32% of points in the 4 areas A, B, C and D separately and there are 11% and 8% of points located in X axis and Y axis, and 6% of points located at point $(0, 0)$. Most points are located in Area D (27 points). It seems that many individuals increase entry to risk and decrease entry to ambiguity at the same time. We also observe a quite high percentage of points in Area A (16 points), where individuals decrease entry to Risk and increase entry to Ambiguity at the same time. We go further to look carefully at the points in the graph by evaluating how far they are from the point (0, 0). we do find in area A and D many points are far away from the point (0, 0). It seems that individuals move largely from Risk to Ambiguity and from Ambiguity to Risk. The diminishing difference between Risk and Ambiguity in the later periods may not result from the increase of all people's entry into risk but from more people who change from Ambiguity to Risk than from Risk to Ambiguity.

Further more, we find that there are quite a lot of points located around point (0, 0). There are 33.3% of points located in the square ranging between X $[-0.1, 0.1]$ and Y $[-0.1, 0.1]$, which indicates individuals with little changes of entry into the two markets. There are 61.9%

of points located in the square ranging between X $[-0.2, 0.2]$ and Y $[-0.2, 0.2]$, which can help explain majority's behavior. The findings indicate that there exist quite many people who have no incentives to increase or decrease entry to both markets simultaneously in the latter part of the game.

The findings provide us very important hints on individual choices in the risky and ambiguous information markets. We do find many subjects randomize their choices in the two markets and keep stable entry frequencies over the first 25 and the last 25 periods. It seems that payoff feedback and experience do not change their choice preference in the ambiguous choice over time. We also find many subjects change largely the entry proportions to each market, but the changes may go in opposite directions, increasing preference to Risk or increasing preference to Ambiguity. It seems that for these people, experience and payoff evidence does influence their decisions. Our next step is to get into individuals' decision process to see how they respond to the evidence of received payoffs and try to figure out the possible reasons of ambiguity seeking found in strategic games.

Switches in decision

A simple way of observing the influence of payoffs and experience is to study the switching behavior between decisions. An analysis of the transition matrix between period $t-1$ and period t allows us to find out how payoff information in period $t-1$ influences decisions in period t. In particular, we are interested in the decision difference in period t between the case of having entered into the risky market or ambiguous market in period $t-1$. Importantly, payoff information from previous risky choice and ambiguous choice may provide quite different signals. Since probability is known in risky choice, payoff information from previous risky choice will provide hints on others' behavior in the markets. Comparatively probability is unknown in ambiguous choice but is the same in all periods, besides its implication on others' strategy, payoff information from previous ambiguous choice gives subjects the chance to finding out the probability. As a result, by checking how differently one evaluate payoff information from previous risky choice and previous ambiguous choice, we can take the decision to previous risky choice as a good benchmark to study the impact of payoffs in the ambiguous information market.

The investigation of switches in decisions is based on the aggregate observations by group over 50 periods in the data. Table 3 reports the observed proportions of decisions in choices Out, Risk and Ambiguity in each group in period t in response to choices Out, Risk and Ambiguity in period $t-1$ separately.

Table 3. Proportions of entry choice in period t by group

			Period $t-1$		
			Risk	Ambiguity	Out
Period t	Risk	*Gr.* 1	0.485	0.236	0.179
		Gr. 2	0.430	0.256	0.277
		Gr. 3	0.354	0.113	0.274
		Gr. 4	0.479	0.309	0.122
		Gr. 5	0.626	0.164	0.125
		Gr. 6	0.437	0.129	0.243
		Gr. 7	0.517	0.234	0.116
		Gr. 8	0.338	0.132	0.194
		Gr. 9	0.311	0.240	0.348
		Gr. 10	0.375	0.160	0.198
		Gr. 11	0.438	0.149	0.164
		Gr. 12	0.53	0.15	0.252
	Ambiguity	*Gr.* 1	0.253	0.582	0.149
		Gr. 2	0.262	0.479	0.261
		Gr. 3	0.244	0.670	0.123
		Gr. 4	0.372	0.473	0.158
		Gr. 5	0.187	0.612	0.199
		Gr. 6	0.253	0.603	0.193
		Gr. 7	0.303	0.617	0.095
		Gr. 8	0.169	0.421	0.163
		Gr. 9	0.311	0.417	0.163
		Gr. 10	0.2	0.580	0.109
		Gr. 11	0.219	0.552	0.126
		Gr. 12	0.17	0.533	0.325
	Out	*Gr.* 1	0.263	0.182	0.672
		Gr. 2	0.308	0.265	0.462
		Gr. 3	0.402	0.217	0.603
		Gr. 4	0.149	0.218	0.719
		Gr. 5	0.187	0.224	0.676
		Gr. 6	0.310	0.267	0.564
		Gr. 7	0.180	0.150	0.789
		Gr. 8	0.493	0.447	0.643
		Gr. 9	0.377	0.343	0.489
		Gr. 10	0.425	0.259	0.692
		Gr. 11	0.342	0.299	0.710
		Gr. 12	0.3	0.317	0.423

We read the observations in two different ways. First, we compare the preference for staying in the same choices. In other words, we compare the two observations, proportion of staying with Risk and the proportion of staying with Ambiguity in two consecutive periods in each group. We find that except group 4 and group 5, the proportions of Ambiguity are higher than those of Risk. We can accept the significant preference of staying in Ambiguity for staying in Risk with probability 0.98 by using the binomial distribution $\binom{12}{2}0.5^2 0.5^{10}$. It seems that compared with the risky information market, individuals are more likely to stay in the ambiguous information market.

Second, we compare subjects' behavior when they switch from one market (Risk /Ambiguity) to the other (Ambiguity /Risk) market. We compare the proportion of Ambiguity when previous period's choice was Risk and the proportion of Risk when previous period's choice was Ambiguity in each group and we surprisingly find out that the former value is always higher than the latter in all the groups. Individuals are more likely to switch from Risk to Ambiguity than from Ambiguity to Risk.

According to the decision process observed above, the results are consistent with ambiguity seeking found in the present study, and provide the observations in details among groups. However, it seems that observations of switches in decisions are not enough to explain the reasons of ambiguity seeking in the game, and especially how come it persists strongly over the first 25 periods. We think the detailed payoff information can help us understand individuals' attitude to ambiguity in the decision process.

We have the following questions. First, in view of the reason of keeping in Ambiguity, whether previous payoff information provides a positive signal to stay, or such information does not influence their initial preference to ambiguity? Second, in view of the reason of switching to Ambiguity, since one is not able to know the information more than his own choice, what payoff information in Risk induce the switches to Ambiguity?

We will try to answer these questions by relating choices in period t to choices in period $t-1$ and to gains or losses in the previous choice. Based on the aggregate observations across all subjects over 50 periods. Table 4 reports the observed proportions of decisions in choices Out, Risk and Ambiguity in period t in response to the payoffs 2.2, 4.2, 6.2, 8.2, 10.2, 12, 12.2, 14.2 and 16.2 in period $t-1$, where we separate the observations in three blocks depending on the sources (Out/Risk/Ambiguity) of the payoffs.

The upper block in the table presents proportions in every choice in response to various previous payoffs received in choosing Risk. The middle block presents proportions in every choice in response to various previous period payoffs received in choosing Ambiguity.

Table 4. Proportions of entry choice in period *t* in response to the choice and payoffs

	Period $t-1$								
	Payoffs (Obs.) in Risk market								
Period t	2.2	4.2	6.2	8.2	10.2	12.2	14.2	16.2	*Allpayoffs*
		(5)	(72)	(195)	(264)	(258)	(186)	(99)	(1079)
Out		0.6	0.472	0.405	0.307	0.244	0.258	0.222	0.306
Risk		0.2	0.361	0.354	0.443	0.484	0.538	0.434	0.446
Ambiguity		0.2	0.167	0.241	0.25	0.271	0.204	0.343	0.248
$\frac{Ambiguity}{Out+Ambiguity}$		0.25	0.261	0.373	0.449	0.526	0.442	0.607	0.448
	Payoffs (Obs.) in Ambiguity market								
	2.2	4.2	6.2	8.2	10.2	12.2	14.2	16.2	*Allpayoffs*
	(12)	(25)	(126)	(226)	(274)	(283)	(228)	(77)	(1251)
Out	0.583	0.64	0.397	0.345	0.270	0.184	0.149	0.182	0.260
Risk	0	0.16	0.214	0.181	0.226	0.180	0.202	0.104	0.191
Ambiguity	0.417	0.2	0.389	0.473	0.504	0.636	0.649	0.714	0.549
$\frac{Risk}{Out+Risk}$	0	0.2	0.350	0.344	0.456	0.495	0.575	0.364	0.424
	Payoffs (Obs.) in staying out								
	12								
	(1786)								
Out	0.629								
Risk	0.205								
Ambiguity	0.166								
$\frac{Risk}{Risk+Ambiguity}$	0.553								

Comparing the proportions of Risk in the first block and the proportions of Ambiguity in the second block, we find that the former is always equal or lower than the latter one in response to the same payoff level in the previous period, and the difference becomes larger for high payoff levels, for example, the values are 0.484 and 0.636 in response to payoff 12.2, and the values are 0.434 and 0.714 in response to payoff 16.2 in Risk and Ambiguity separately. Comparing the proportions of Ambiguity choice in the first block and the proportions of Risk choice in the second block, we find that the latter is always equal or lower than the former one in response to the same payoff level in the previous period, and especially the latter is quite similar for various previous payoffs, while the former increases as the amount of previous period payoff increases.

The observations in the first two blocks have some important characteristics in common and provide us some hints on how information influences the Risk and Ambiguity choices in different ways. It seems that in Risk high payoff in previous period does not provide strong incentives for subjects to stay, while they do in Ambiguity.

This suggests that when facing choice Risk, subjects in Risk choice control their own behavior in avoiding entering too frequently in a specific way. They may take the probability information $\frac{1}{2}$ as a standard in evaluating received payoffs and they may not overweight the appearance of high payoffs. As a result, a high payoff may not be a positive signal to enter, and oppositely they choose Out or Ambiguity to avoid the possibility of low payoffs. It seems that they try to match their decisions with the appearance of probability information, but they may do it in a naive way without considering too much on others' strategy. By contrast, without the guide of a certain probability, subjects loose their control into Ambiguity market, and such phenomena become strong especially when they receive high payoffs.

Previous studies on individual decisions claim that people prefer choices with known probability to choices with unknown probability. The findings in our games provide us some clues in understanding known and unknown probabilities in a different way. Since people try to behave rationally in entry decisions in the risky information market, whether the available information about probability can help improve coordination in one group? Figure 4 reports the number of entrants into the two markets in each group over 50 periods. In view of the pure strategy equilibrium of 2 subjects in each market, we can not observe clear coordination in the risky information market among groups, and the numbers fluctuate even in the ending periods. It seems that two markets settings with uncertain capacities increase the complexity of the game, and make coordination extremely hard to reach.

We have found clear ambiguity seeking in the two-market entry game. Although the effects diminish over time, the significant level keeps robust in the first 30 periods. Since interaction among players is one important characteristic in differentiating our game and other studies, we hold the conjecture that competition rising from interaction may be an important reason of overentry, and such competition becomes drastic when probability is unknown. In order to check it, we use the same instruction pattern to study a kind of individual choice experiment.

Figure 4: Mean number of entrants by period by group

3.3.2 Results of the individual choice game in Treatment 2

We will report the results of the individual choice game described in Treatment 2 but in a simpler way. We will first explore ambiguity effects in the individual choice game itself, and then discuss the difference of the results in the strategic game and the individual choice game.

The two graphs in Figure 5 report the aggregate proportions of entry by period and by every 5 periods separately. The two lines in each graph describe the entry proportions of all individuals into each market and the changes over 50 periods. In each graph, the two lines overlap each other in all the 50 periods. We can not observe any difference between entry decisions in the two markets. The entry frequencies in both markets in the beginning periods are a little higher than those in later periods. Especially the entry frequencies in both markets seem very stable in the interval $[0.2, 0.3]$ in the down graph. We also go to individual data to compare one's entry frequencies in both markets. Table 5 reports the proportions of entry into the two markets in the first 25 and the last 25 periods separately by individual in the game, totally 25 subjects included. The last three rows in the table show the statistics on the number of subjects who enter more in Risk, those who enter in Risk and Ambiguity with equal frequencies, and those who enter more in Ambiguity, which are 12, 3 and 10 separately in periods 1-25 and are 10, 7 and 8 separately in periods 26-50. The observations do not imply the preference of majority to any market neither in the first nor in the last 25 periods. It provides consistent results as in Figure 5.

Figure 5: Mean number of entrants by period (up) and by every 5 periods (down) in Treatment 2

Before we accept the similar entry behavior in the two markets, we also go to the decision process. Table 6 reports the observed proportions of decisions in choices Out, Risk and Ambiguity in period t in response to the payoffs 10, 12 and 14 in period $t-1$, where we separate the observations in three blocks depending on the sources (Out/Risk/Ambiguity) of the payoffs. We read the observations the same way as has been done in analyzing observations in Treatment 1.

First, we compare the preference for staying in the same choices. Comparing the proportions of Risk in response to previous Risk choice and the proportions of Ambiguity choices to previous Ambiguity choice, we do not find clear difference between entry frequencies in response to the same payoff level in the previous period. For example, the values are 0.530 and 0.554 in response to payoff 10, and the values are 0.554 and 0.558 in response to payoff 14 in Risk (in response to previous Risk) and Ambiguity (in response to previous Ambiguity) separately. Second, we compare subjects' behavior when they switch from one market (Risk/Ambiguity) to the other (Ambiguity/Risk) market. Comparing the proportions of Ambiguity in response to previous Risk and the proportions of Risk in response to previous Ambiguity, we also do not find clear difference.

Table 5. Observed proportions of entry by individual

	Periods 1-25		Periods 26-50	
Subject	Risk	Ambiguity	Risk	Ambiguity
1	0.52	0.44	0.64	0.36
2	0.36	0.36	0.04	0.56
3	0.2	0	0	0
4	0.4	0.28	0.12	0
5	0.16	0.84	0.68	0.24
6	0.32	0.6	0	0.8
7	0.16	0.08	0.16	0
8	0.48	0.04	0.2	0.68
9	0.44	0.08	0.76	0
10	0.68	0.16	0.56	0.04
11	0.68	0.2	0.2	0.24
12	0	0	0	0
13	0.44	0.4	0.36	0.52
14	0	0.48	0	0
15	0.28	0.36	0.64	0.16
16	0.4	0.44	0.6	0.28
17	0.2	0.56	0.44	0.44
18	0.16	0.56	0	0.52
19	0.28	0.36	0.16	0.4
20	0.04	0.08	0	0
21	0.4	0.36	0.4	0.44
22	0.16	0.08	0.04	0.04
23	0.6	0.24	0.28	0.2
24	0.44	0.44	0.28	0.56
25	0	0.76	0	0
Average	0.312	0.328	0.262	0.259
Number ($Risk > Ambiguity$)	12		10	
Number ($Risk =]Ambiguity$)	3		7	
Number ($Risk < Ambiguity$)	10		8	

Table 6. Proportions of entry choice in period t in Treatment 2

	Period $t-1$				
	Risk (351 obs.)		Ambiguity (358 obs.)		Out (516)
Period t	10	14	10	14	12
Out	0.238	0.145	0.207	0.218	0.725
Risk	0.530	0.554	0.238	0.224	0.149
Ambiguity	0.232	0.301	0.554	0.558	0.126

Most of the previous studies of risk and ambiguity find clear evidence of ambiguity aversion in individual choice experiments. However, we find neither ambiguity aversion nor ambiguity seeking in the individual choice experiments described in Treatment 2. The indifference between risk and ambiguity may result from experimental settings in our experiment. First, repeated play may help subjects understand the probability occurring in the ambiguity market and help reveal the similarity in the two markets. Second, instead of a binary choice experiment between risk and ambiguity situations, we have three choices in the game. An extra choice Out with a secure payoff may influence decisions into the two markets.

In principle, our experimental settings of the individual choice game in Treatment 2 serve for the studies of the strategic game described in Treatment 1 and help clarify the explanation for the ambiguity seeking phenomenon.

First, Treatment 2 helps solve confusions and doubts on whether ambiguity seeking results from repeated play. One may doubt that more entrants in the ambiguous market are not related to the preference to a choice with unknown probability. It may be explained as the curiosity for finding the probability by repeated entering into the ambiguous market. It seems that we can not refuse such a conjecture if we observe the results in Treatment 1 in isolation. Results from Treatment 2 clearly show that choices in Ambiguity do not differ from those in Risk even in the beginning periods. As a result, we can firmly say that curiosity about probability can not be the reason of ambiguity seeking.

Second and most importantly, the individual choice game in Treatment 2 leaves aside the strategy effects occurring in Treatment 1, and help explain the reasons of ambiguity seeking in the strategic game. By observing individual decision process, we conjecture that ambiguity seeking may result from individuals' controlling behavior when information about probability is available. Treatment 2 provides us a good benchmark and it suggests that controlling behavior does not happen in an individual choice game. It seems that information about probability can help control entry behavior only when subjects are situated in a strategic environment.

As has been discussed when we raise the questions of the present paper, uncertainty about probability in a strategic environment is quite different from that in an individual choice environment. We do believe that interaction among people may arouse overconfidence or competence. Such feelings weaken when the information about probability is known, but strengthen otherwise. As a result, we observe the controlling behavior in Risk and ambiguity seeking happens in a strategic game.

3.4 Conclusion

Our experiments find ambiguity seeking in the strategic game of two-market entry situations but no ambiguity effects in the individual choice problem. It seems that information about probability plays quite different roles in strategic environments. Without information about probability, people may be competent and overconfident in a competitive environment, but they may constrain themselves in the choices of known probability and try to behave rationally in the entry decisions.

Relating our findings to the field, why do most manufacturers and managers behave aggressively in market entry decisions? High uncertainty about market demand could be one of the most important reason. In the U.S. ready-to-eat breakfast cereal industry, for example, close to 70% of all new products are crapped within two years after their introduction.

The idea of overconfidence and competence in economic decisions goes back at least as far as Adam Smith (1776). He argues in his book *the wealth of nations* that people systematically overestimate their chances of success in any venture because of their inability to control those factors that can be controlled and the factors beyond their control.

Camerer and Lovallo (1999) are the only one aiming at using market entry games to explain overentry in the field. They include a potentially potent psychological variable — relative skill perception—in market entry games. They created a paradigm in which entrants' payoffs depend on their skill to measure business entry decisions and personal overconfidence simultaneously. They find that overconfidence about relative ability can trigger excess entry. Grieco and Hogarth (2006) is the only research which uses ambiguity to explain excess entry. They find that entrepreneurial entry decisions are better explained by ambiguity seeking influenced by feelings of competence. However, instead of using strategic games with interaction among players, individuals receive their own private ambiguous information and make choices in isolation.

Our experimental result may provide a possible explanation: the more uncertain the market demand is, the stronger preference entrepreneurs holds in entering the market, and in the end more failure entries.

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3.5 Appendix: Instruction of the strategic game in Treatment 1

General Information

The purpose of this experiment is to study how people make decisions in a particular situation. From now on and till the end of the experiment any communication with other participants is not permitted. If you have a question, please raise your hand and one of us will come to your desk to answer it.

You will receive 4 euros for showing up on time for the experiment. In addition, you will make money during the experiment. Upon completion of the experiment the amount that you make will be paid to you in cash. Payments are confidential; no other participant will be told the amount you make.

Rounds and Groups:

This experiment will have 50 rounds. In each round you will be in a group with 6 other participants, totally 7 people. The members in your group will be fixed in all rounds. You will not be informed of the identity of people who you are playing with neither during the experiment nor in the end of the experiment.

Description of the Decision Task(s) in the Experiment:

In each round, you are asked to make a choice between one of three possible actions, action "X", action "Y." or action "Z". If you choose action X, you will receive a fixed amount of money. If you choose Y, your payoff will depend on the state of the world and the choice of other participants in your group. Given certain state of the world, the less the number of Y chosen by your group, the higher your payoffs is in choosing action Y. If you choose Z, your payoff will depend on the state of the world and the choice of other participants in your group. Given certain state of the world, the less the number of Z chosen by your group, the higher your payoffs is in choosing action Z.

The state of the world in action Y will be high or low. When you make your decision you do not know it is high or low. However, all of you know the probabilities of high or low.

The state of the world in action Z will be high or low. When you make your decision you do not know it is high or low, and you also do NOT know the probabilities of high or low. However, you know that the probabilities of high and low are uniform in every round.

How payoffs are determined

Payoffs in every round of this game are determined as follows.

- If you choose action X, your payoff for the round is 12.

• If you choose action Y, your payoff for the round depends on the state of the world and the total number of players, including yourself, who choose action Y.

Suppose that n =1, 2, 3, 4, 5, 6 and 7 represent the number of players in your group who choose action Y. If you are one of these n players, your payoff for the round is given by:

Your points in one round $= 12 + 2(c - n)$

The value of c depends on the state of the world for choice Y. In every round it will be $c = 1.1$ with probability $\frac{1}{2}$ or $c = 3.1$ with probability $\frac{1}{2}$.

For example, if $c = 3.1$ and $n = 1$, that is, the state of world is high and you are the only player out of the group of 7 (1/7) who chooses action Y, then your payoff from choosing action Y would be $12 + 2(3.1 - 1) = 12 + 4.2 = 16.2$

For another example, if $c = 1.1$ and $n = 7$, that is, the state of the world happens and all five players (7/7) choose action Y, then each player's payoff from choosing action Y would be $12 + 2(1.1 - 7) = 12 - 11.80 = 0.2$

The complete set of possible payoffs you can earn from choosing action Y in each round are provided in the following table which you may refer to at any time during the experiment.

Payoffs in the low state of the world, $c = 1.1$
(with probability $\frac{1}{2}$)

Fraction of 7 players who choose action Y	1/7	2/7	3/7	4/7	5/7	6/7	7/7
Payoff each earns from choosing action Y	12.2	10.2	8.2	6.2	4.2	2.2	0.2

Payoffs in the high state of the world, $c = 3.1$
(with probability $\frac{1}{2}$)

Fraction of 7 players who choose action Y	1/7	2/7	3/7	4/7	5/7	6/7	7/7
Payoff each earns from choosing action Y	16.2	14.2	12.2	10.2	8.2	6.2	4.2

If you choose action Z, your payoff for the round depends on the state of the world and the total number of players, including yourself, who choose action Z.

Suppose that n =1, 2, 3, 4, 5, 6 and 7 represent the number of players in your group who choose action Z. If you are one of these n players, your payoff for the round is given by:

Your points in one round $= 12 + 2(c - n)$

The value of c depends on the state of the world for choice Y. In every round it will be $c = 1.1$ or $c = 3.1$ with unknown probability, but the probability keeps uniform in every round.

For example, if $c = 3.1$ and $n = 1$, that is, the state of world is high and you are the only player out of the group of 7 (1/7) who chooses action Z, then your payoff from choosing action Z would be $12 + 2(3.1 - 1) = 12 + 4.2 = 16.2$

For another example, if $c = 1.1$ and $n = 7$, that is, the state of the world happens and all five players (7/7) choose action Z, then each player's payoff from choosing action Z would be $12 + 2(1.1 - 7) = 12 - 11.80 = 0.2$

The complete set of possible payoffs you can earn from choosing action Z in each round are provided in the following table which you may refer to at any time during the experiment.

Payoffs in the low state of the world, $c = 1.1$
(with unknown probability, but uniform over periods)

Fraction of 7 players who choose action Z	1/7	2/7	3/7	4/7	5/7	6/7	7/7
Payoff each earns from choosing action Z	12.2	10.2	8.2	6.2	4.2	2.2	0.2

Payoffs in the high state of the world, $c = 3.1$
(with unknown probability, but uniform in over periods)

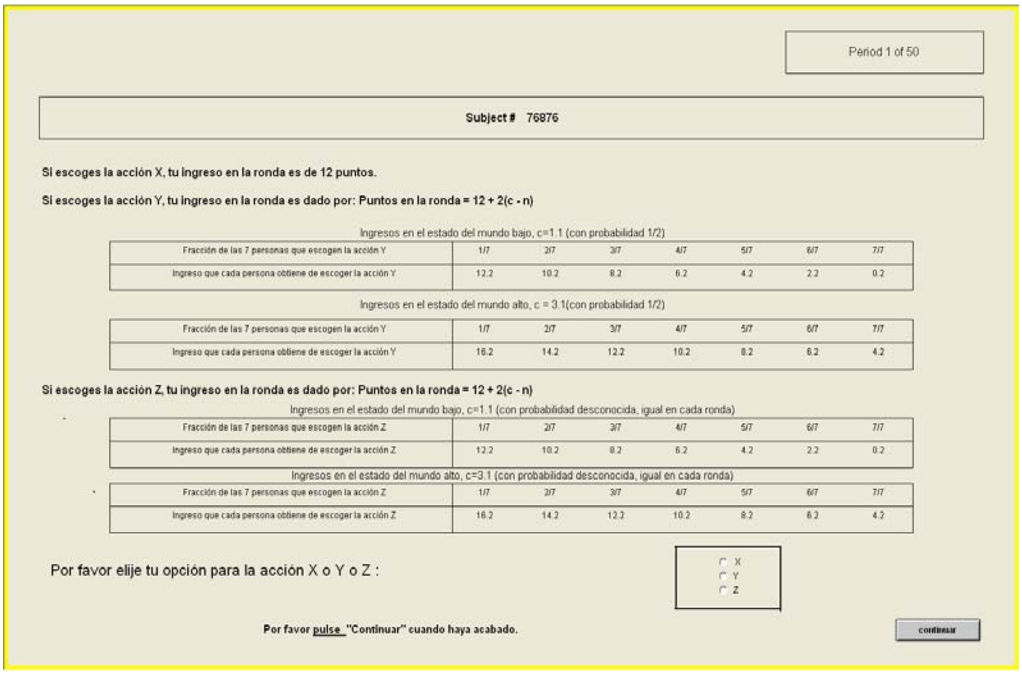
Fraction of 7 players who choose action Z	1/7	2/7	3/7	4/7	5/7	6/7	7/7
Payoff each earns from choosing action Z	16.2	14.2	12.2	10.2	8.2	6.2	4.2

These payoff possibilities from playing action X, action Y or action Z will remain the same over all rounds. Are there any questions about how choices determine payoffs?

Playing a round:

Note that in each round, when you make your decision you will not know what the other participants in your group are doing in the round. You will also not know the state of the world.

First, you need to make your choice on action X, action Y or action Z. The computer will display a screen like the one shown below. Please press the button besides your choice. You may change your choices as often as you like, but once you click on "Enter" your choice is final.



Meanwhile, the computer will "roll the die" to decide the state of the world of action Y, $c = 1.1$ or $c = 3.1$, and the state of the world of action Z, $c = 1.1$ or $c = 3.1$.

Then, the computer helps calculate the result, and you will be informed of your payoff in this round, your accumulated payoff in the past rounds, and the decision you have made.

Payoffs

At the end of the experiment you will be paid, in cash, the sum of the payoffs that you will have earned in the 50 rounds of the experiment plus show up fee 4 euros. The ratio between the experimental points and euros is 1 point = 0.02 euros. As noted previously, you will be paid privately and we will not disclose any information about your actions or your payoff to the other participants in the experiment.

Payoff quiz

Before we begin the experiment, please answer the following questions. The following questions aim at helping you understand how the payoffs are realized. We will go through the answers to a sample problem before you do the rest of the quiz. Please raise your hand if you are having trouble answering one of the questions.

Sample Question: If you made a choice of action X, and the state of the world $c = 1.1$ and the number of Y in your group is 1 and the number of Z in your group is 3, as a result, your payoff is ___12___.

Question 1: will the participants I am grouped with be the same in all rounds? _____

Question 2: Do you know the probability of high or low state of the world in action Y ? ____

Question 3: Do you know the probability of high or low state of the world in action Z ? ____

Question 3: If you made a choice of action Z, and the state of the world in action Z is $c = 3.1$ and the number of Z in your group is 2, as a result, your payoff is ______

Chapter 4

Ambiguity Effects and Strategic Complexity in Market Entry Games (Jointly with Jordi Brandts)

4.1 Introduction

A long history of entrepreneurship literature has asserted that entrepreneurs are ambitious in entry decisions. High rate of business failure in the industry and overentry phenomena remains a puzzle for decades. In finding the reasons behind such a phenomenon, Camerer and Lovallo (1999) study experimentally market entry games to explain overentry in the field, they argue that over-optimism is a key component in explaining excess entry. Grieco and Hogarth (2006) frame business entry as facing a lottery of risk, where information about probabilities are known, and ambiguity, where information about probabilities are unknown or imperfectly known. They find that entrepreneurial entry decisions are better explained by a preference for ambiguous information situations, a notion of ambiguity seeking, influenced by the feelings of competence.

Recently, Brandts and Yao (2008a) study experimentally how entry into two markets with uncertain capacity is affected by the type of information of risk and ambiguity potential entrants have available in the market interaction. They find ambiguity seeking behavior when players face the risky market and the ambiguous market simultaneously, where average entry is higher in the market with ambiguous information than the one with risky information. They suggest that when potential entrepreneurs face alternative opportunities of different levels of uncertainties, they are able to control their entry when knowing probability while not without probability information. It seems that ambiguity seeking is driven by a market setting of a comparative environment and high strategic complexity. What if entrepreneurs face a simple situation of a strategic environment, such as a single market? How do they evaluate an uncertain event with risky or ambiguous information in isolation?

The main motivation for our work stems from the potential connections between ambiguity effects and the complexity of the strategic environments. In other words, our concern is whether ambiguity seeking is a consistent phenomenon when a risky information market and an ambiguous information market are evaluated separately with strategic interaction among people.

This question relates to the evaluation of risk and ambiguity in comparative and non-comparative contexts. Ellsberg (1961) is the first to distinguish risk and ambiguity with experimental evidence in comparative contexts. Ellsberg's two-color problem uses two urns, one containing 50 red and 50 black balls called the known urn (or the risky urn), and one containing 100 balls in an unknown combination of red and black called the unknown urn (or the ambiguous urn). In Ellsberg's experiments, a majority of subjects indicated a preference for the risky urn over the ambiguous urn whenever betting on red or black balls, a result known as the "Ellsberg paradox". Many experiments were conducted by varying the Ellsberg's problem to study the ambiguity behavior and find consistent ambiguity aversion results.

Heath and Tversky (1991) firstly demonstrate the importance of a comparative context, where people prefer to bet on events more knowledgeable or competent for them. It is Fox and Tversky (1995) who clearly compare ambiguity effects in comparative and non-comparative contexts separately. They find that ambiguity aversion disappears in a non-comparative context. They propose the Comparative Ignorance Hypothesis, according to which ambiguity aversion is produced by a comparison with less ambiguous events. Chow and Sarin (2001) studies experimentally the same issue, and although their results do not support the strong conclusion of Fox and Tversky (1995), they do find a diminishing effect in separate evaluations in a non-comparative context.

Sarin and Weber (1993) study ambiguity effects in a market setting given incentives and immediate feedback. They compared subjects' bid for risky and ambiguous bets in several experimental markets in a comparative and non-comparative context separately and provide a positive answer to ambiguity aversion and the Comparative Ignorance Hypothesis. It seems that market setting is not sufficient to eliminate the effects of ambiguity and comparative ignorance.

The present paper also intends to study whether decisions on risky and ambiguous bets are influenced by positioning in comparative and non-comparative contexts. However, It is quite different from testing Comparative Ignorance Hypothesis proposed by Fox and Tversky (1995).

First, we study ambiguity effects in strategic environments. Second, we extend the ambiguity study by Brandts and Yao (2008a) in market entry games, where instead of ambiguity aversion, ambiguity seeking is salient in comparative contexts.

In our experiments, instead of facing a risky and an ambiguous information market simultaneously, subjects make entry decisions to one uncertain market of risk or ambiguity. Especially we study ambiguity effects in both repeated play of both fixed matching (in which subjects interact with the same partners in all periods) and random matching (in which subjects change partners in every period) separately.

In natural environments, both fixed matching and random matching make economic sense. Fixed matching simulates the long-run behavior in a group, for example, in the oligopoly market, entrepreneurs of different firms in the same industry often make decisions simultaneously to invest in a new product, use a new technology, which relates strategic interaction among partners. Random matching simulates one shot decision such as driving on a road, entering a bar. Zwick and Rapoport (2002) studies market entry games with fixed capacity both with fixed matching and random matching. They find less entrants in fixed matching than in random matching. The results imply that matching ways may influence entry decisions. Players are possible to know the strategy of other group members in fixed matching but they are not able to predict the behavior of new partners in random matching. Hence, it seems that fixed matching and random matching represent different levels of strategic complexity. It is harder to figure out the complexity in random matching than in fixed matching.

We find ambiguity seeking in random matching and no ambiguity effects in fixed matching. Our results show that ambiguity effects do not necessarily disappear in a non-comparative context in strategic environments. Instead of relating to a comparative or a non-comparative context, we hold the conjecture that ambiguity effects in strategic games depend on the strategic complexity in the games. The stronger the strategic complexity is, the more salient the ambiguity effects.

The next section describes the market entry game and characterizes its equilibria. Section 4.3 presents the experimental results and carries out the analyses on the results. We draw conclusions in Section 4.4. The instruction used in the fixed matching is attached in Appendix.

4.2 Experimental Design

4.2.1 A market entry game with fixed capacity

The market entry game is played by a group of N symmetric players who must decide simultaneously and independently whether to enter a market or to stay out. One very simple and frequently used formulation is where payoffs are linear in the number of entrants or players. The payoff to player i's strategy of staying out ($S^i = X$) or entering ($S^i = Y$) is computed from the following formula, which is common knowledge:

$$\pi_i = \begin{cases} v, & \text{if } S^i = X \\ v + r(c - m), & \text{if } S^i = Y \end{cases}$$

where v, r are positive constants and $0 \leq m \leq N$ is the number of agents that choose entry. The value of c is interpreted as the market capacity and publicly known. The constraint $1 \leq c < N$ can make it possible that the payoff of entering is not always higher than that of staying out. In this noncooperative N-person game, the return to entry exceeds the return to staying out, a fixed v, if and only if $m < c$.

There are $\binom{N}{m^*}$ pure strategy Nash Equilibria for this class of games with equilibrium entrants number m^*. We distinguish between two cases, depending on whether $v = v + r(c - m^*)$ or $v \neq k + r(c - m^*)$. If c is an integer, then $m^* = c$ or $m^* = c - 1$; If c is not an integer, then $m^* = |c|$ where $|c|$ is the largest integer smaller than c.

Additionally, for $c > 1$, denoting the (symmetric) probability of entry by E^i, the symmetric mixed strategy equilibrium is given by

$E^i = \frac{c-1}{N-1}$ for $i = 1, ..., N$

where y^i is the probability of entry by the ith player. Note that the expected number of entrants in the symmetric mixed equilibrium is $N * E^i$, which is different from but can be very close to the pure strategy equilibria value m^*.

4.2.2 A market entry game with uncertain capacity

In the market entry game studied in our experiments, instead of being a fixed capacity definitely announced, c is not publicly announced and two possible values $\overline{c}$ and $\underline{c}$ are provided and occur with probability p and $1 - p$ separately. In other words, subjects do not know the exact value of market capacity c when they make entry decisions in one period.

Based on the assumption of the expected utility (EU) theory and risk neutrality, and in view of the fact that payoffs are linear in the parameter c, we can carry on the equilibrium analysis easily by using similar method mentioned above. In view of the pure strategy equilibrium, the payoff of entry is in a form of expected value $U(m^e) = p(v + r(\overline{c} - m^*)) + (1-p)(v + r(\underline{c} - m^*))$, in brief, $U(m^*) = v + r\{[p\overline{c} + (1-p)\underline{c}] - m^*\}$. We distinguish between two cases depending on whether $v = U(m^*)$ or $v \neq U(m^*)$. If $p\overline{c} + (1-p)\underline{c}$ is an integer, then $m^* = p\overline{c} + (1-p)\underline{c}$ or $m^* = p\overline{c} + (1-p)\underline{c} - 1$; If $p\overline{c} + (1-p)\underline{c}$ is not an integer, then $m^* = |p\overline{c} + (1-p)\underline{c}|$ where $|p\overline{c} + (1-p)\underline{c}|$ is the largest integer smaller than $p\overline{c} + (1-p)\underline{c}$. There are also $\binom{N}{m^*}$ pure strategy Nash equilibria.

The symmetric mixed Nash equilibrium is of the form $E^i = \frac{(p\overline{c} + (1-p)\underline{c}) - 1}{N-1}$ for $i = 1, ..., N$. Accordingly the expected number of entrants in mixed strategy is $N * E^i$.

4.2.3 Description of the experiment

Our experimental design involves repeated play of the market entry game by a group of $N = 5$ people. We begin by discussing the parameterization of the payoff function and treatments. We then explain the procedures of the experiments.

We choose to set $v = 6$ and $r = 2$ resulting in the following payoff function (in experimental points):

$$\pi_i(\partial) = \begin{cases} 6, & \text{if } S^i = X \\ 6 + 2(c - m), & \text{if } S^i = Y \end{cases}$$

Where we use two actions X and Y to denote the choices of "stay out" and "enter". $0 \leq m \leq 5$ is the number of subjects (including subject i) choosing Y. c is the actual market capacity occurring in a certain period.

In our experiments, we set two values $\underline{c} = 2.1$ or $\overline{c} = 4.1$, which occurs with probability $p = \frac{1}{2}$ separately in one period. Instead of randomizing the appearance of $\underline{c}$ and $\overline{c}$ with probability $\frac{1}{2}$ in the process of each session, we use a computer to generate a realization before the experiments. Such a method can keep the realization uniform in various sessions and can avoid the noise in the realization-generated process[1]. Since our focus is to compare entry decisions in the risky market treatment and the ambiguous market treatment, we try to avoid results distorted by the sequence of $\overline{c}$ and $\underline{c}$ values in a realization. As a result, we prepare two realizations. For each treatment, we use realization 1 and realization 2 in half of the sessions separately.

We choose $\underline{c}$ and $\overline{c}$ to be non-integers so that there exists only one pure equilibrium number of entrants [2]. At the same time, their values are close to an integer so that in equilibrium the

[1] Although we set the command $p = \frac{1}{2}$ in 50 periods in the computer, among various realizations generated, one of the two values ($\underline{c}$ and $\overline{c}$) often appears in more than 30 periods. As a result, we choose two realizations in which two values appears quite evenly around 25 periods out of 50 periods separately.

[2] If $\underline{c}$ and $\overline{c}$ are integers, there exists two pure equilibria entrant numbers.

difference in payoff to those entering one market and those staying out remains quite small. At the same time, our design avoids any possible negative payoffs generated from the equation.

Our choice of $N = 5$ is based on the following consideration. Most previous studies of market entry game with fixed capacity use a group of 20 players. Since we add the difficulties to subjects in understanding c and making entry decisions, we prefer a simple payoff structure and easy calculations. All these favor our choice of a smaller number of subjects.

We use a 2×2 design to set up the treatment structure. In the information dimension, we include treatments with and without the information of the probability of $\underline{c}$ and $\overline{c}$ in the uncertain market, which are simply termed "Risk" and "Ambiguity" separately. In the matching dimension, we include treatments by whether or not group members are fixed in all periods, which are termed "Fixed matching" and "Random matching".

Table 1 summarizes the relevant features of the experimental sessions, including matching ways, information conditions and total number of subjects in each treatment. We use four abbreviations FR, FA, RR and RA to describe the four treatments in our experiments. 155 students from the Universitat Autónoma de Barcelona of Spain participated in our experiments. They were recruited through E-mail invitations and reading notices on the experimental recruiting website using ORSEE system[3]. Each subject was only allowed to participate in a single session that lasted around 45 minutes. 75 students participated in treatments FR and FA in 8 groups of five and 7 groups of five separately, and we ran two sessions[4] for each treatment. 80 students participated in treatments RR and RA, and we ran two sessions[5] of 20 subjects for each treatment.

Table 1: Features of Experimental Sessions

Matching ways	Information Conditions	Treatment Abbreviation	No. Subjects Per Session	Total No. Subjects
Fixed matching	Risk	FR	20	40
	Ambiguity	FA	20 or 15	35
Random matching	Risk	RR	20	40
	Ambiguity	RA	20	40

The tasks described in the instructions are not framed in a market situation[6]. Since the mar-

[3] Greiner, B., (2004). An Online Recruitment System ORSEE.

[4] As have been mentioned about the realizations of capacities $\underline{c}$ and $\overline{c}$, we use realization 1 and realization 2 in two sessions separately.

[5] In each session, a group of 5 player are randomized in a pool of 20 subjects. We use realization 1 and realization 2 in each session.

[6] We use the same style instruction as in Brandts and Yao (2008a)

ket entry game can reflect various aspects in empirical life, such as investment in stock markets, commuter decisions in avoiding traffic jams, without framing the game, observations can reflect general views in such strategic games while will not be distorted by a specific environment.

Subjects were told they were faced with 2 choices. By choosing choice X, one can always earn a fixed amount 6. In choice Y, payoffs are decided by both capacity c and the number of entrants m. In other words, subjects were faced with two uncertainties, uncertainty about capacity and uncertainty about others' strategy. In treatments FR and RR, subjects were told that capacity c was from one of the two values $\underline{c} = 2.1$ and $\overline{c} = 4.1$ each occurring with probability $\frac{1}{2}$, an uncertain capacity with known probability. In treatments FA and RA, subjects only knew that capacity c was from one of the two values $\underline{c} = 2.1$ and $\overline{c} = 4.1$ and the probability for each to appear kept the same in all periods, but they were not told the probability, an uncertain capacity with unknown probability. In principle, the true probability about $\underline{c}$ and $\overline{c}$ is the same in all treatments, $\underline{c} = 2.1$ and $\overline{c} = 4.1$ each occurring with probability $\frac{1}{2}$, and the difference in Risk and Ambiguity is whether it is known to subjects.

In order to ensure they understand their payoffs from choosing Y clearly, the instructions also include the following two tables revealing all possible payoff values from choosing action Y. Each table lists all possible payoffs in the conditions of different number of entrants in $c = 2.1$ and $c = 4.1$ separately, where it clearly denotes in treatments FR and RR that each capacity occurs with probability $\frac{1}{2}$, while in treatments FA and RA that probability is unknown to subjects and the same in all periods.

Payoffs in low capacity, $c = 2.1$

{In Treatments FR and RR}with probability $\frac{1}{2}$

{In Treatments FR and RR} with unknown probability but uniform over periods

Fraction of players who choose action Y	1/5	2/5	3/5	4/5	5/5
Payoff each earns from choosing action Y	8.2	6.2	4.2	2.2	0.2

Payoffs in high capacity, $c = 4.1$

{In Treatments FA and RA}with probability $\frac{1}{2}$

{In Treatments FA and RA} with unknown probability but uniform over periods

Fraction of players who choose action Y	1/5	2/5	3/5	4/5	5/5
Payoff each earns from choosing action Y	12.2	10.2	8.2	6.2	4.2

4.2.4 Procedures

Subjects in all treatments follow the same steps in the experiments. We use Z-tree[7] to program our experiments. After the paper instructions were read aloud, subjects completed a set of Review Questions on the computer terminals to test their understanding of the instructions. They could not finish this part until they had answered all the questions correctly. Subjects were told in the instructions that they would make one choice between choice X and choice Y in each period, and repeatedly play the same game in 50 periods.

Each round took place in the following way:

1. All the subjects made decisions simultaneously between two actions X or Y without communication among them.

2. The market capacity in the period was not known to subjects neither at the beginning nor in the end of one period;

3. At the end of one period, the only information they received is their own payoffs and their payoff history.

Two comments about the procedure of the experiments are in order.

First is about the design of the repeated play of the game. Most experimental studies of ambiguity effects in individual choice games use one shot design. Our design of repeatedly play of 50 periods results from the following reasons. On one hand, we are studying ambiguity effects in strategic environments, which is more complicated than in individual choice game. It may take some periods for players to know how to play the game. On the other hand, repetition makes it possible to see how people learn about the entry behavior of others and the equilibria when players face ambiguity in strategic environments.

However, repetition may induce noise in studying ambiguity effects. The sampling effects resulting from repeated play may confuse us whether one choice implies the preference to risk/ambiguity or the belief on the probability distribution formed in the repeated play.

Our second comment on the design of information feedback may help partly solving the above problem. In the end of one period, we only provide payoff informations to subjects. We try to provide as little information as possible to subjects and make it harder for subjects to learn probabilities and other's strategies in the ambiguous situations in Treatments FA and RA. Once we release information more than their payoffs, for example, number of entrants or the occurrence of $\underline{c}$ or $\overline{c}$, individuals may use it to adjust their conjecture on probability or on others's strategies. The way of providing least information can help subjects learn about the

[7] Fischbacher, U., (2007), z-Tree: Zurich Toolbox for Ready-made Economic Experiments.

game instead of learning about probability or other's strategy.

What is more, such a design keeps the same information feedback as that in Brandts and Yao (2008a). It is reasonable for the following reasons. First, we try to simulate the situation in the field. In strategic environments, such as in a market, where entrepreneurs face both competitors and market situations, they are only able to know the final result of their decisions, their payoffs, but not how their payoffs are decided by the two factors. Hence their information about how their payoffs are realized is not complete. As a result, we accept the natural feedback of incomplete information, and would not like to manipulate a setting of complete information feedback in a strategic environment.

4.2.5 Equilibrium predictions and hypotheses

In the treatments of $c = 2.1/4.1$ with known probability $\frac{1}{2}$, pure strategy Nash equilibria have 3 players always entering and 2 players always staying out. Symmetric mixed strategy Nash equilibria predict an entry probability 0.525 and the expected number of entrants 2.625.

Although the parameterization in the market with ambiguity is exactly the same as that in the market with risk, in view of the minimum information (only payoff information) given in the end of each period, the ambiguous information about probability is hard to be revealed even after many periods. Therefore it is hard to predict how players hold their views on an ambiguous state and react to the opponent's strategy. It is rational to think that subjects should expect equal probabilities on high and low states. We will take the equilibria in the treatment Risk as a benchmark[8] to carry out our analysis in the treatment Ambiguity.

Random matching can be taken as participating in a series of one-shot plays of such games, given that the players have identical incentives, one might think the mixed symmetric equilibria is particularly salient. In contrast, as has been studied in most of the literature on fixed matching in market entry games, learning and evolution is natural in repeated interaction. Individuals will learn to condition their behavior on the behavior of others and hence converge to an asymmetric equilibrium. In particular, reinforcement learning model predicts convergence to a pure equilibrium even though such play may take a long time to emerge.

4.2.6 Questions and Hypotheses

Our experimental design is intended to address two specific research questions.

[8] If subjects randomize their decisions in the Ambiguity treatment, their entry probability should be around $\frac{1}{2}$, which is close to the predicted entry probability 0.525 in the treatment Risk.

Question 1: Do individuals enter more or less into the ambiguous market in fixed matching?

Brandts and Yao (2008a) find that individuals enter more into the ambiguous market when they face the choices of the risky and ambiguous markets simultaneously, a within-subjects design, in the fixed matching. They observe ambiguity seeking in strategic environments in a comparative context. In our experiment, we keep all other settings similar, and study individuals' entry decisions in one (the risky market or the ambiguous market) of the uncertain markets in isolation in a non-comparative context (a between-subjects design). Here we follow the same way of defining ambiguity preference as in Brandts and Yao (2008a), where it is ambiguity seeking if there are more entrants in the ambiguous market than in the risky market, and vise versa.

Although Comparative Ignorance Hypothesis is defined basing the disappearance of ambiguity aversion in non-comparative contexts in individual choice experiments (Fox and Tversky, 1995; Chow and Sarin, 2001). Their explanation of comparative ignorance may help us understand our study in the strategic environments. First, the information advantage of the known probability over the unknown probability is made vivid in a comparative context. It is also explained that discrimination between two options will be more pronounced in a joint evaluation than in separate evaluations when one option serves as an easy reference point to evaluate the other option. In our research, different from the content of Comparative Ignorance Hypothesis, we are testing whether ambiguity seeking disappears in a non-comparative context. The reasons mentioned above can help set up the hypothesis of ambiguity effects in our market entry games, that is, ambiguity seeking may disappear in a strategic game. Till now, in the two experimental papers relating the study of ambiguity effects in strategic environments, Comparative Ignorance Hypothesis is supported by the results of Sarin and Weber (1993), but not by Chen et al. (2006), where ambiguity seeking is found in the first and second price sealed bid auctions in conditions of unknown vs. known distribution of bidder valuations in a non-comparative context.

In practice, since ambiguity aversion is proved by most experiments and became a widely accepted assumption in theoretic research, to test whether ambiguity seeking found in Brandts and Yao (2008a) is robust is meaningful for the following two reasons. If no ambiguity effects are found, it seems that whether subjects are positioned in a comparative and non-comparative environments does matter in strategic environments; and if ambiguity seeking is found, we can not only confirm the finding of Brandts and Yao (2008a) in a non-comparative context, but provide a good implication that ambiguity effects can be different from ambiguity aversion in strategic environments.

Question 2: Does ambiguity affect behavior consistently in fixed and random matching?

In our experiment, since subjects face both uncertainty about probability and uncertainty about others' strategy, and are given the least information in the end of one period, fixed matching and random matching may induce either similar or different decisions to subjects.

On one hand, subjects in the fixed matching are not able to acquire information on the number of entrants, hence subjects are hard to figure out strategy of group members, which make coordination extremely hard. As a result, decisions result from these two matching ways may become minute.

On the other hand, subjects in fixed matching may behave more sophisticated than those in random matching. In random matching, although subjects played the game repeatedly over all 50 periods, in each period, they were grouped with different subjects who are drawn randomly from the fixed population. Each of these subjects takes a random sample of previous plays and react accordingly. Actions in earlier periods may have a feedback effect on actions by subjects in later periods. However, their actions should have nothing to do with learning at the individual level. We can assume that after an agent plays the game once, he will consider new players in his group as agents of the same type (from the same sample size). As a result, uncertainty about capacities is emphasized. In the fixed matching, although subjects only receive payoff information, subjects may be sophisticated in making decisions. How one subject plays is highly related to his beliefs of other members in the group. Hence, subjects' decisions in the interaction may be influenced by both uncertainty about capacity and uncertainty about others' strategy. As a result, decisions result from these two matching ways may become salient.

In view of the complexity of strategic environments, decision making in empirical life may be influenced by many factors. For example, strategic consideration of other players and the attitude to the type of information of risk and ambiguity may interact with each other when decisions are made. Repeated play of the game in fixed matching and random matching represent two situations of different level of strategic complexity in practice, where it is more complex in the latter. It is interesting to know how subjects treat ambiguity in different strategic complexity, and in what conditions information type is more important than strategic consideration?

4.3 Experimental Results

We first explore general entry behaviors into the market with risky information and the one with ambiguous information by observing aggregate data of all subjects over periods, and by group and session behavior in both fixed matching and random matching. Then individual entry behaviors are analyzed. Finally we go to individual decision process to explore the reasons of ambiguity effects. In the discussion below, for convenience, we always use RR and RA to denote the risky market treatment and ambiguous market treatment in the random matching, and FR and FA to denote the risky market treatment and ambiguous market treatment in fixed matching.

4.3.1 Aggregate results on the number of entrants

We get into the heart of the matter by comparing entrant numbers in the risky market treatment and the ambiguous market treatment. Figure 1 reports the results in fixed matching. The up and down graphs describe the aggregate average numbers of entrants by period and by every 5 periods separately. The two lines in each graph describe the numbers of entrants in the risky market treatment FR and the ambiguous market treatment FA in a group of 5 people and how they change over 50 periods. We find that the two lines indicating FR and FA overlap in the interim of all 50 periods. The mean numbers of entrants change little in FR and FA and the lines keep quite flat over all 50 periods but increase slightly in the last periods. In down graph describing the mean number over every 5 periods, the mean entrant numbers in FR and FA are almost always higher than the predicted mixed strategy equilibrium number 2.625 and the pure strategy equilibrium number 3 except for the point denoting the value in periods 21-25.

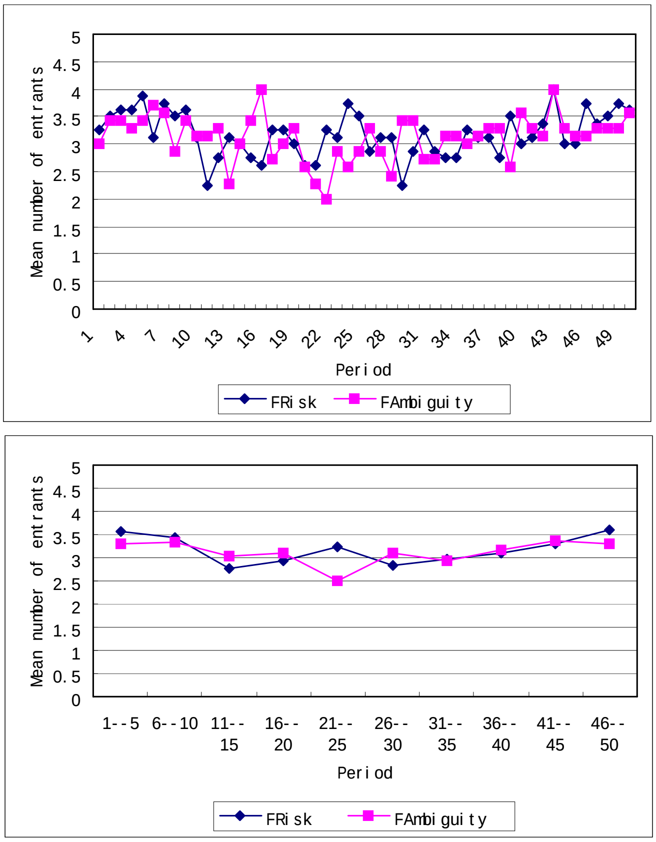


Figure 1: Mean number of entrants by period (up) and by every 5 periods (down) in fixed matching.

Figure 2 reports the results in random matching and the descriptions are following the same structure as those shown in Figure 1. We find a difference between the numbers of entrants in the risky market treatment RR and the ambiguous market treatment RA in the random matching. There are two important observations in the graph. First, the mean number of entrants in RA is almost always above that in RR and the difference diminishes in the last ten periods. The down graph provides a clear view about it. We observe that the distance between the mean number of entrants in RA and RR is similar over all 50 periods with the exception of one intersection point of periods $41 - 45$. It seems that the difference is stable especially in the first 40 periods. Second, in both treatments, the mean number of entrants changes little and the lines keep quite flat over all 50 periods. Mean number of entrants in RR fluctuates around the predicted mixed strategy equilibrium number 2.625, and mean number of entrants in RA is around and almost always higher than the pure strategy equilibrium value 3.

As has mentioned in the design, we use two realizations for half of the sessions in a treatment to avoid the influence of specific realization. Hence we will present the session-level result below. Table 2 shows session-level mean and standard deviations of the number of entrants over all 50 periods. The realization in session i $(i = 1, 2)$ is the same in the risky market treatment and the ambiguous market treatment. The observed means in session 1 and session 2 over all 50 periods are 3.08 and 3.285 in RA and they are 2.705 and 2.79 in RR, where the numbers of entrants are higher in both sessions $(3.08 > 2.705;\ 3.285 > 2.79)$. In the fixed matching, the observed means in session 1 and session 2 over all 50 periods in FA are 3.13 and 3.11 and they are 2.935 and 3.425 in FR, where the comparison of the two values in session 1 and in session 2 are not consistent $(3.13 > 2.935;\ 3.11 < 3.425)$.

As a result, we find that in the aggregate data there are more entrants in the ambiguous situation than in the risky situation in the number of entrants in the random matching; while there are no differences in the fixed matching.

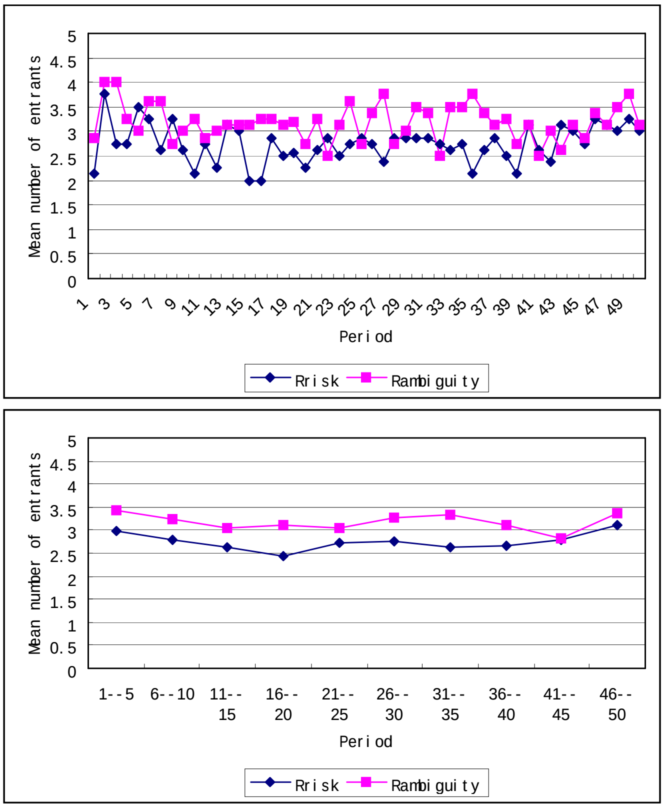


Figure 2: Mean number of entrants by period (up) and by every 5 periods (down) in random matching.

Table 2. Observed means of the total number of entry

	Observed mean	St. Dev.	Obs.
	Fixed matching		
$RA(session1)$	3.08	1.065	200
$RA(session2)$	3.285	1.017	200
$RR(session1)$	2.705	1.053	200
$RR(session2)$	2.79	1.108	200
	Random matching		
$FA(session1)$	3.13	0.978	150
$FA(session2)$	3.11	1.004	200
$FR(session1)$	2.935	0.991	200
$FR(session2)$	3.425	0.998	200

Table 3 provides the regression analysis to test the findings above. In every binary logit regression on the entry decision of a period, the dependent variable is the binary decision of entering and staying out, and the independent variable is the dummy variable checking whether it is the risky market treatment or the ambiguous market treatment. We run the regressions considering both random and clustered effects. To be concrete, Since the formation of a group of 5 people are randomized drawn from the sample of 20 people, the clustering analyses in random matching is based on the session level. We are prudent in considering they are dependent observations and cluster by session. In the fixed matching, the clustered unit is the group. We find that in the random matching, decisions between risky and ambiguous situations are always significantly different, which is denoted by 0.362 at the 1% level in both random and clustering effects. However,they are always insignificant in fixed matching.

Table 3. Binary logit regression in entry decisions

	Random matching		Fixed matching	
	Random	Session Clustered	Random	Group Clustered
Ambiguity				
(vs. Risk)	0.362***	0.362***	−0.051	−0.051
	(0.065)	(0.076)	(0.068)	(0.123)
Constant	0.199***	0.199***	0.558***	0.558***
	(0.045)	(0.028)	(0.046)	(0.111)
Observations	4000	4000	3750	3750
R^2	0.01	0.01	0.00	0.00

***significantly different from zero at the 1% level; **5% Idem.; *10% Idem.

Since aggregate data can not tell the group heterogeneity in fixed matching and the entry situation of a group in one period in random matching, we go to the groups to see it.

Figure 3 reports the mean number of entrants and standard deviation of individual entry frequency in the groups in the fixed matching. It yields two interesting observations. Consider first the mean number of entrants by group. There are two groups of bars indicating mean entrants by group in treatments FA and FR separately. In treatment FA (FR), the 7 groups (8 groups) indicated by 7 bars (8 bars) are listed in a sequence of increasing mean entrants. We find that in FA, where subjects do not know the probability, the difference of mean entrant number among groups is minute with the lowest 2.86 and the highest 3.3. By contrast, In FR where subjects know the probability of the state, the difference among groups is bigger with the lowest mean entrant number 2.58 and the highest 3.74. It suggests that group variance in FR is larger than that in FA. A second observation is on the heterogeneity of individual entry frequency in one group. The number written in each bar indicates the standard deviation of entry frequency of 5 members in a group, where the smaller the value is, the similar the players' entry behavior in a group. We find that in both treatments FA and FR, groups with higher mean number of entrants have lower standard deviation of entry frequency. The finding suggests that competition in the groups with similar players is stronger and then leads to higher entrants.

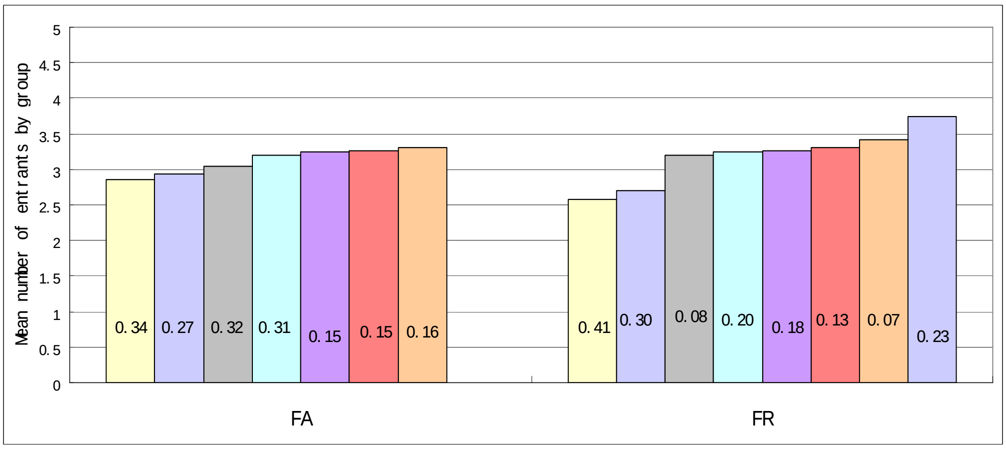


Figure 3: Mean number of entrants by group in fixed matching.

Figure 4 reports the proportion of number of entrants by group over all 50 rounds in the random matching. There are two groups of bars which indicate the risky and ambiguous information treatments separately. In each group, the 3 bars with different colors indicate 3 cases: groups with the number of entrants smaller than 3 (including groups with 0, 1 and 2 entrants), equal to 3 and bigger than 3 (including groups with 4 and 5 entrants).[9] The height of the bars indicates the fraction of observations for that treatment falling into that bin. We find that in treatment RA, 25% of the groups with entrant number smaller than 3, 41% bigger than 3. By contrast, in treatment RR, there are more groups (43%) with entrant number smaller than 3 than groups (26%) with entrant number bigger than 3. It suggests that subjects of ambiguity treatment are faced more frequently with overentry situation and suffer more from coordination failure. It concludes that when subjects do not know probability, they do enter more frequently, correspondingly, they are more probably faced with drastic situation of over-crowd in the market. Although they are not told the number of entry clearly, they do suffer payoff losses whenever the capacity is high or low.

[9] We take the number 3 as the benchmark to separate the 3 cases because the number of entrants is 3 in pure strategy equilibrium.

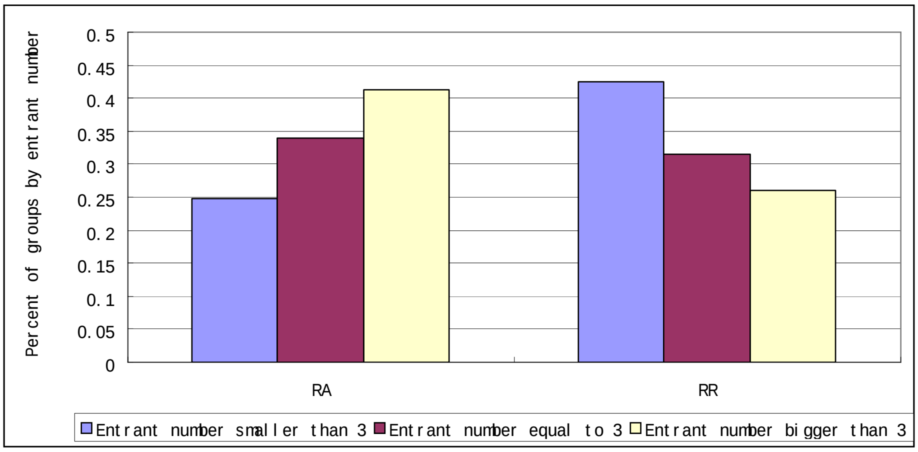


Figure 4: Distribution of groups in random matching

It seems that we have provided preliminary answers to Question 1, where comparative ignorance exists when players make entry decisions in a non-comparative context in fixed matching, and Question 2, where comparative ignorance does disappear in a non-comparative context in random matching.

Our results confirm the existence of comparative ignorance, where ambiguity seeking disappears in the market entry game studied in Brandts and Yao (2008a) in a non-comparative context. It may be quite possibly related to the discussion (Fox and Tversky, 1995; Chow and Sarin, 2001) that ambiguity effects are salient only in a contrast between risk and ambiguity. However, it is not clear enough to explain ambiguity seeking phenomenon when our game is positioned in random matching. In view of the consistent ambiguity seeking phenomenon, it seems that decisions in random matching in our non-comparative contexts may have some common characteristics as those in fixed matching in comparative contexts in Brandts and Yao (2008a).

It is very confusing why ambiguity seeking happens in random matching but not in fixed matching. It may be quite related to different individual behavior in the game. Aggregate observations may hide some important informations of individual behaviors. As a result, we will go to individual data in the next section.

4.3.2 Individual results of entry frequency

The results presented above are averaged across subjects and provide no information on individual differences. In this part, we present individual level observations over 50 periods and how subjects of different types are distributed in the risky and ambiguous situations.

Figure 5 and 6 report the distribution of individuals with different entry frequencies in the fixed matching and random matching separately. The individual entry frequency, which is calculated by the proportions of entry of an individual over all 50 periods, is put in one of the 5 intervals, $[0, 0.2]$, $[0.21, 0.40]$, $[0.41, 0.60]$, $[0.61, 0.80]$, and $[0.81, 1.0]$. The two bars with different colors located in each interval separate the observations of the risky market treatment and the ambiguous market treatment. The height of each bar indicates the fraction of individuals from a treatment with the entry frequency falling in the interval.

Figure 5 reports the observations in the fixed matching. We find that the fractions of individuals are lower in both low interval $[0, 0.2]$ and high interval $[0.81, 1]$, and most of the observations are located in the intervals $[0.41, 0.6]$ and $[0.61, 0.80]$, which can explain 60% of observations in FA and more than 70% of observations in FR. The finding suggests that instead of using pure strategy, most subjects use mixed strategy in making entry decisions.

Figure 6 reports the observations in the random matching. For both treatments, the observed individual entry frequency varies considerably across interval $[0, 1]$ with the lowest fraction 0.05 and 0.15 in the interval $[0, 0.2]$ and the highest fraction 0.375 and 0.275 in the interval $[0.61-0.80]$ in RA and RR situations separately. In the intervals $[0, 0.2]$, $[0.21, 0.4]$, $[0.41, 0.6]$, the fractions of individuals in RA are lower than those in RR; while in the intervals $[0.61, 0.80]$ and $[0.81, 1]$, the fractions in RR are higher than those in RR.

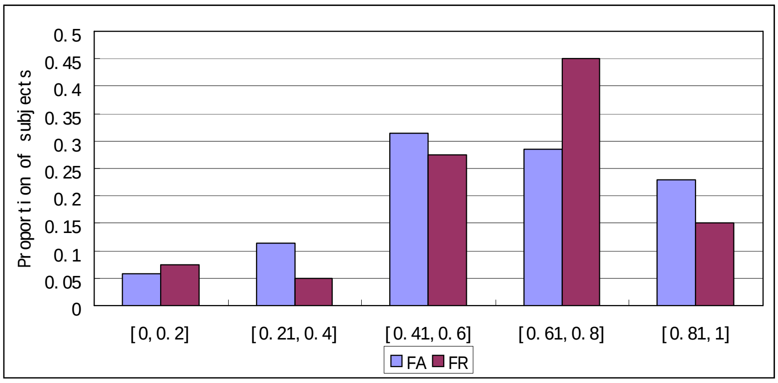


Figure 5: Distribution of individuals entry frequency in fixed matching

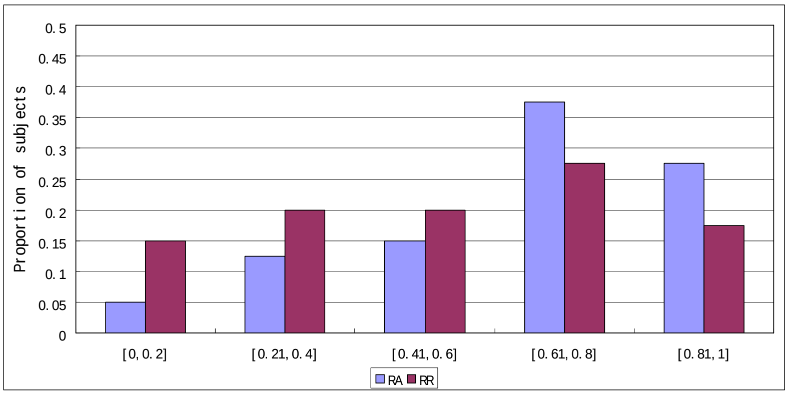


Figure 6: Distribution of individuals entry frequency in random matching

In view of the individual data in both matching ways, the majority of subjects mixes their strategy over all 50 periods but in quite different ways. The finding suggests the failure of the symmetric mixed-strategy equilibrium[10] in explaining the results. In previous researches of market entry game under certain market capacity, Rapoport and his colleges also observed heterogeneity in individual decisions although the average frequencies of entry in market entry games look remarkably like those generated by Nash equilibrium play. Duffy and Hopkins (2006) attempt to find the long-run predictions of certain type equilibrium in repeated playing 100 periods market entry game. They do find that play does seem to approach a pure strategy equilibrium.

The discussion on which type of equilibrium are agents likely to coordinate on is not the main issue in the present study. However, it may help understand why comparative ignorance exists in the random matching and disappear in the fixed matching. Theoretically in the random matching of repeated one-shot play of such games, symmetric mixed strategy equilibrium is more salient than asymmetric pure strategy equilibrium, while in the fixed matching, pure strategy equilibrium is predicted. In view of imprecise evidence regarding the closeness of play by each group or a session by observing individual frequencies, following the method used by Duffy and Hopkins (2006), we make a use of the Gini index of inequality, which may test observations in a more strict way, to check the trend of the possible equilibrium type.

Let P_i be the percentage of all decisions to enter ($\delta^i = 1$) made by player i over R rounds, (e.g. the last 25 rounds): $P_i = N_i/N$, where $N_i = \sum_{t=1}^{R}(\delta_t^i = 1)$ and $N = \sum_{i=1}^{5} N_i$). K is the

[10] Since random matching over periods is more like a repeated one-shot game, subjects are more possible using mixed-strategy than pure one.

number of players in one group. Gini coefficient weights members entering decisions in one group and doe not require a determination of which players are playing certain pure or mixed strategies, which is described by the equation below,

$$G = \frac{1}{2}\sum_{i=1}^{K}\sum_{j=1}^{K}|(1/K)P_i - (1/K)P_j|$$

In the fixed matching, in pure strategy equilibrium, 3 players should always enter and the remainder always stays out over the R-round interval, therefore the vector of P_i values, sorted from the last to the most is $P = \{0, 0, 1/3, 1/3, 1/3\}$, and the Gini coefficient is around 0.4. By contrast, if all the subjects in a group play symmetric mixed strategy equilibrium, the vector is $P = \{1/5, 1/5, 1/5, 1/5, 1/5\}$, and the Gini coefficient is equal to 0.

In the random matching, we assume $K = 20$, in which way we include the possible influence taken from the source of the same session. In pure strategy equilibrium, 12 players should always enter, where $P = \{0, 0, ...1/12, 1/12\}$ including eight 0 and twelve 1/12, and the Gini coefficient is around 0.4. If all the subjects in a group play symmetric mixed strategy equilibrium, the vector is $P = \{1/20, 1/20, ...1/20\}$, and the Gini coefficient is equal to 0.

To determine how close and the speed of subjects' convergence to a particular equilibrium in one group or one session, we calculated the Gini coefficients by group or by session in all 50 periods, last 25 periods and last 10 periods. The results are listed in Table 4.

In the fixed matching, in each group or each session, the values for over all 50 periods, last 25 periods and last 10 periods change little in all four treatments. The coefficients of 7 groups ($Gr - 1, 4, 5, 7, 9, 13, 15$) decrease slightly in the last 10 periods, and those of 7 groups (Gr_$2, 3, 6, 8, 10, 11, 12$) increase slightly. The coefficients of session 1 of RR and RA increase slightly in the last 10 periods, and those of session 2 of RR and RA decrease slightly. We find the values of Gini coefficients is ranged between those of pure strategy equilibrium and the symmetric mixed strategy equilibrium. It is hard to be explained by any equilibrium.

However, in the average observations of every treatment, the values in the random matching are higher and the convergence are quicker than those in the fixed matching, and are closer to the mixed strategy equilibrium coefficients 0.4.

Table 4. Gini Coefficient

		Periods		
		All 50	Last 25	Last 10
	Gr_1	0.238	0.205	0.183
	Gr_2	0.113	0.125	0.137
	Gr_3	0.220	0.241	0.262
FA	Gr_4	0.124	0.176	0.103
	Gr_5	0.258	0.294	0.274
	Gr_6	0.114	0.076	0.142
	Gr_7	0.288	0.286	0.211
	Average	0.194	0.200	0.187
	Gr_8	0.391	0.388	0.429
	Gr_9	0.276	0.326	0.271
	Gr_10	0.142	0.081	0.146
FR	Gr_11	0.065	0.1	0.146
	Gr_12	0.049	0.061	0.11
	Gr_13	0.136	0.187	0.133
	Gr_14	0.152	0.176	0.173
	Gr_15	0.102	0.122	0.053
	Average	0.164	0.180	0.183
RA	Session1	0.186	0.213	0.298
	Session2	0.181	0.209	0.208
	Average	0.184	0.211	0.253
RR	Session1	0.254	0.301	0.324
	Session2	0.308	0.336	0.331
	Average	0.282	0.319	0.328

Both symmetric mixed strategy and pure strategy equilibrium fail to explain individual behaviors clearly in both the fixed matching and the random matching. However, individual decisions in the random matching seem more prone to mixed strategies than those in the fixed matching. It is possible that individuals in the fixed matching manage to coordinate with other players' strategy in the group but could not in view of the complex uncertainties and without long enough periods.

4.3.3 Switches in decision

In view of the same experimental design in the fixed and random matching, it appears that the matching way does affect behavior. The main difference in the two matching ways is how subjects treat the interaction with others. Our next step is to get into individuals' decision process. With least information of payoff feedback in the end of one period, subjects may respond differently in the two settings, which may help us understand why comparative ignorance exists in one setting but not in the other and whether ambiguity seeking results from different levels of strategic complexity embodied in the two matching ways.

Since payoffs are the only information subjects received and are the most important information they use in updating their behavior, a simple way of observing the decision process is to study the switching behavior between decisions. An analysis of the transition matrix between period $t-1$ and period t allows us to find out how payoff information in period $t-1$ influences decisions in period t. In particular, we are interested in the decision difference in the risky market treatment and ambiguous market treatment. Importantly, payoff information from previous choice in the risky information market or the ambiguous information market may provide quite different signals. Since probability is known in the former, payoff information from previous choice will provide hints on others' behavior in the markets. Comparatively probability is unknown in the latter treatment, but is the same in all periods, besides its implication on others' strategy, payoff information from previous choice gives subjects the chance to finding out the probability. As a result, by checking how differently one evaluates payoff informations from previous choice from the risky market or ambiguous market, we can take the decision to previous choice in the risky information treatment as a good benchmark to study the impact of payoffs in the ambiguous information treatment.

Table 5 presents the proportion of entry in period t conditional on payoff levels in period $t-1$, using data of all subjects over 50 periods in one treatment. In the first column of the table, the 7 possible payoffs resulting from entering decisions in period $t-1$ are listed by the sequence of the increasing magnitude, followed by the payoff 6 from the decision of staying out.

We first analyze the results in the random matching. Treatments RR and RA yield two groups of interesting observations. In RR the proportions of entry rises as the increase of payoffs, where they are 0.578 and 0.913 when faced with the lowest payoffs 0.2 and the highest 12.2 separately in previous period; while in RA, they do not change much as the increase of payoffs, where the entry frequency reacts evenly (mostly above 0.70) to various payoffs from entering.

In the fixed matching, we find that the proportions of entry in period t change little in response to various payoffs in period $t-1$ in treatment FR, and the proportions of entry rise a little as the increase of previous payoff levels in treatment FA.

Table 5. Entry frequencies conditional on payoff levels of previous period

	Entry Proportion in Period t							
	RR		RA		FR		FA	
	Mean	Obs.	Mean	Obs.	Mean	Obs.	Mean	Obs.
IN								
$P_{t-1,\ 0.2}$	0.578	45	0.700	100	0.7	80	0.65	60
$P_{t-1,\ 2.2}$	0.551	156	0.708	260	0.736	212	0.648	216
$P_{t-1,\ 4.2}$	0.633	229	0.677	269	0.709	347	0.702	248
$P_{t-1,\ 6.2}$	0.742	298	0.737	316	0.760	312	0.756	234
$P_{t-1,\ 8.2}$	0.712	208	0.720	211	0.743	202	0.842	234
$P_{t-1,\ 10.2}$	0.690	116	0.750	80	0.782	78	0.824	68
$P_{t-1,\ 12.2}$	0.913	23	0.833	12	1	12	0.714	7
OUT								
6	0.401	885	0.504	712	0.457	717	0.437	648

Next, we will use a binary logit regression to test the observations in Table 5 and attempt to answer how entry decisions are influenced by various payoff levels in previous period among different treatments. In the regression, the dependent variable is the action chosen in period t, $E \in \{0,1\}$, where 1 denotes entry, and the independent variables are a set of binary variables implying different payoff levels. The regression we used is of the following form:

$$Pr(E_t^i = 1) = \frac{\exp(\beta_0 + \sum_{j=0.2,\ldots,12.2} \beta_j P_{t-1,\ j})}{1+\exp(\beta_0 + \sum_{j=0.2,\ldots,12.2} \beta_j P_{t-1,\ j})}$$

Here β_0 is the fixed effect specific to a treatment, and $P_{t-1,\ j}$ is the dummy variable which equals to 1 if payoff j (j can be 0.2, 2.2, 4.2, 6.2, 8.2, 10.2 or 12.2) appears in period $t-1$, and β_j is the coefficient for the regressor. We do four such regressions for treatments RR, RA, FR and FA separately. Each regression includes observations of all the subjects over all 50 periods in one treatment.

Table 6 reports the regression results. To avoid the dummy variable trap, we drop the dummy for payoff 6.2[11], which means that any of the payoff 6.2 occurred in period $t-1$ can be

[11] Since the payoff 6.2 is the closest to the value of staying out (6), compared with other values, it is better to be considered as the base payoff.

used as the base payoff. In the regression, the coefficient of payoff 6.2 therefore is considered as zero.

Table 6. Binary logit model of the probability of entry

	Entry(*t*)			
	RR	*RA*	*FR*	*FA*
$P_{t-1,\,0.2}$	$-.741^{***}$	$-.185^{***}$	−0.303	−0.514
	(.008)	(.067)	(0.329)	(0.471)
$P_{t-1,\,2.2}$	$-.849^{***}$	−.148	−0.126	−0.522
	(.083)	(.421)	(0.319)	(0.352)
$P_{t-1,\,4.2}$	$-.508^{***}$	−.294	−0.260	−0.278
	(.087)	(.211)	(0.299)	(0.204)
$P_{t-1,\,8.2}$	−.151	$-.086^{*}$	−0.091	0.539
	(.213)	(.049)	(0.264)	(0.377)
$P_{t-1,\,10.2}$	$-.256^{***}$	.066	0.127	0.407
	(.068)	(.324)	(0.399)	(0.346)
$P_{t-1,\,12.2}$	1.297	.577		−0.217
	(1.272)	(.543)		(1.124)
constant	1.054	1.032	1.151^{***}	1.133
	(.132)	(.257)	(0.280)	(0.283)
Observations	1075	1248	1231	1067
R^2	.02	.003	0.003	0.025

***significantly different from zero at the 1% level; **5% Idem.; *10% Idem.
Clustered by session or group

First, we compare the regressions RR and RA in the random matching. In RR, the coefficients of payoffs 0.2, 2.2 and 4.2 are strongly significantly negative[12], and those of payoffs 8.2 and 12.2 are insignificant. The regression suggests that entry probability increases as the payoff values get bigger in 0.2, 2.2 and 4.2, and entry probability responds indifferently to payoffs 6.2, 8.2, 10.2 and 12.2. While In RA, only the coefficients of payoffs 0.2 and 8.2 are significant, but the values of the coefficients are very close to 0. It suggests that the coefficients of various

[12] The coefficient on payoffs 12.2 is significantly negative, which may be the bias resulting from low observations.

payoffs are all close to 0, and hence entry probability responds to various payoffs indifferently. In view of similar values of constant β_0 in the regressions for RR and RA, the logit regressions confirm our observations in Table 5., where decisions of entry are low in respond to previous low payoffs in the risky information and they are uniform to various payoffs in previous period in the ambiguous information.

Second, we compare the regressions FR and FA in the fixed matching. In both the regressions for FR and FA, all the coefficients are insignificant, in other words, the values of all the coefficients are not different from 0. We do not observe the difference in the decision process from the results of two regressions.

The regression analysis provides us a proof on how differently (similarly) subjects treat risk and ambiguity in the random matching (the fixed matching). In the random matching, in risky information, the proportion of entry is greater when the previous payoff is higher and it is weaker when the previous payoff is lower, and in the ambiguous information with unknown probability, the proportion of entry keeps uniformly high. In contrast, in the fixed matching, entry behavior does not change for various payoffs in both risky and ambiguous information situations.

In the analysis below, we will change a way to observe the decision process by using a contrast of two matching ways under one information type. We begin by comparing subjects' behavior in the two matching dimensions under a certain information pattern (known probability or unknown probability). In other words, we will try to compare the decisions in the fixed matching with those in the random matching under the risky information market and the ambiguous information market separately.

We use a binary logit regression to test how entry decisions are influenced by various payoff levels in previous period and the matching way. In the regression, the dependent variable is the action chosen in period t, $E \in \{0,1\}$, where 1 denotes entry, and the independent variables are a set of binary variables implying different payoff levels and the matching way. The regression we used is of the following form:

$$Pr(E_t^i = 1) = \frac{\exp(\beta_0 + \sum\limits_{j=0.2,\ldots,12.2} \beta_j P_{t-1,\,j} + \phi M_{t-1})}{1+\exp(\beta_0 + \sum\limits_{j=0.2,\ldots,12.2} \beta_j P_{t-1,\,j} + \phi M_{t-1})}$$

Here β_0 is the fixed effect specific to a treatment, and $P_{t-1,\,j}$ is the dummy variable which equals to 1 if payoff j (j can be 0.2, 2.2, 4.2, 6.2, 8.2, 10.2 or 12.2) appears in period $t-1$, and M_{t-1} is the dummy variable which equals to 1 if the observation is from the fixed matching, and β_j and ϕ are the coefficients for the regressors. In the two pair of regressions discussed below, one pair includes observations in the treatments RR and FR, and the other pair includes observations in the treatments RA and FA. Each pair includes two binary regressions on both clustered effects and random effects indicating the different restriction levels on individual correlations. In the fixed matching, we cluster the observations by group, and in the random matching, we cluster the observations by session.

Table 7 reports the results. In the pair of regressions for the risky information treatments RR and FR, the coefficients of payoffs 0.2, 2.2, 4.2 are strongly significant in both random effects and clustered effects, and the coefficient of dummy variable M_{t-1} is strongly significant in random effects and weakly significant in clustered effects. In the regressions for the ambiguous information treatments RA and FA, all the coefficients of $P_{t-1,\,j}$ and M_{t-1} are insignificant. Besides, in the two pairs of regressions, the coefficients of dummy variable M_{t-1} are always positive, which are 0.359 and 0.112 separately. The finding suggests that subjects in the fixed matching entered the market significantly more frequently than subjects in the random matching.

Table 7, together with Table 6, provides us valuable information and possible explanations on ambiguity seeking in the random matching and no ambiguity effects in the fixed matching. To conclude, there are two important points.

First, information about probability is important for people to control their entry decisions, which make less entry in risk than in ambiguity. However, such controlling behavior happens only in the random matching but not in the fixed matching.

In the risky market treatment of the random matching, subjects control their own decisions in avoiding to enter too frequently in a specific way. They decrease their entry possibility when they receive low payoffs and increase their entry when they receive high payoffs. It seems that they try to matching their decisions with the appearance of low and high state. By contrast, without the guide of certain probability, subjects may loose their control on it.

Second, in the fixed matching, strategic coordination and competition are prior to the consideration on the uncertain information in making decisions.

In the decision process of the fixed matching, we do find people behave similarly to the information type of risk and ambiguity. The entry frequencies keep high to various payoffs received in previous period. It may be the reason that subjects focus more on the endogenous uncertainties resulting from individual interactions than on the exogenous uncertainties of market capacities. In other words, they neglect the fact that whether the information about probability is known or unknown. By contrast, in the random matching, without specific partners, subjects consider the information about probabilities as the most important information in decisions.

Table 7. Binary logit model on entry decision

	Entry(t)			
	Risky information(RR&FR)		Ambiguous information(RA&FA)	
	Random	Clustered	Random	Clustered
$P_{t-1,\,0.2}$	-0.571^{**}	-0.507^{**}	-0.310	-0.310
	(0.211)	(0.211)	(0.196)	(0.201)
$P_{t-1,\,2.2}$	-0.476^{***}	-0.476^{**}	-0.321^{**}	-0.321
	(0.145)	(0.225)	(0.139)	(0.246)
$P_{t-1,\,4.2}$	-0.390^{***}	-0.390^{**}	-0.287^{**}	-0.287^{**}
	(0.130)	(0.175)	(0.137)	(0.124)
$P_{t-1,\,8.2}$	-0.118	-0.118	0.205	0.205
	(0.146)	(0.154)	(0.152)	(0.197)
$P_{t-1,\,10.2}$	-0.086	-0.086	0.210	0.210
	(0.187)	(0.179)	(0.222)	(0.213)
$P_{t-1,\,12.2}$	1.765^{*}	1.765^{*}	0.554	0.554
	(0.735)	(0.952)	(0.571)	(0.493)
M_{t-1}	0.359^{***}	0.359^{*}	0.112	0.112
	(0.104)	(0.192)	(0.094)	(0.140)
constant	0.927^{***}	0.927^{***}	1.028^{***}	1.028^{***}
	(0.093)	(0.149)	(0.105)	(0.163)
Observations	2318	2318	2315	2315
R^2	0.01	0.02	0.01	0.01

***significantly different from zero at the 1% level; **5% Idem.; *10% Idem.

4.3.4 Comparison with Brandts and Yao

Compared with experimental results of Brandts and Yao (2008a), our experiments find that ambiguity effects disappear in a non-comparative context in the fixed matching. However, ambiguity seeking still exist when we change the matching way to the random matching. In order to understand ambiguity effects clearly in the strategic environments, it is important to understand how differently people make decisions in the three similar games.

Brandts and Yao (2008a) find ambiguity seeking and provide the following suggestions on the behavioral difference in risky and ambiguous situations. It seems that in the risky information market, high payoffs in previous period do not provide strong incentives for subjects to stay, while they do in the ambiguous information market. They suggest that subjects in the risky choice control their own behavior in avoiding entering too frequently in a specific way. They may take the probability information $\frac{1}{2}$ as a standard in evaluating received payoffs and they may not overweight the appearance of high payoffs. As a result, a high payoff may not be a positive signal to enter, and oppositely they choose to stay out or to enter into ambiguous market to avoid the possibility of low payoffs. It seems that they try to matching their decisions with the appearance of probability information, but they may do it in a naive way without considering too much on others' strategy. By contrast, without the guide of a certain probability, subjects loose their control into the ambiguous information market, and such phenomena become strong especially when they receive high payoffs.

Our experiments find ambiguity seeking in the random matching. Subjects in our experiments have some common characteristics in treating risky information but not exactly the same. Subjects in the risky market treatment also control their behavior in a specific way. They only control their entry in respond to low payoffs, but not to high payoffs in previous period. Compared with Brandts and Yao (2008a), the difference in respond to high payoffs in previous period may influenced by whether alternative market is available or not[13].

Brandts and Yao (2008a) and our fixed matching game have the same fixed partners settings, and theirs and our random matching game have the least similarity in the game settings. However, by studying the decision process carefully, we find that compared with our fixed matching game, subjects in theirs and our random matching game face more complex situations. Although subjects in Brandts and Yao (2008a) are in fixed groups, two alternative markets make knowing others' strategy or coordination very hard. Similar situation happens in our random matching game.

We conjecture that ambiguity effects are salient in a strategic game with great complexity. In such situations, subjects are not able to know or to interfere in others' strategy, and then overemphasize the uncertain information in the game. comparatively, when the strategic environments are simple, strategic interaction is prior to consideration of uncertain information in their decision, and the importance of information type become minute.

4.4 Conclusion

The present paper extends the study of Brandts and Yao (2008a) of ambiguity seeking in strategic market entry games. We find ambiguity seeking in random matching and no ambiguity effects in fixed matching. It proves that ambiguity effects do not necessarily disappear in a non-

[13] In the game of Brandts and Yao (2008), subjects control their entry to risk in respond to previous high payoffs from risk, and mostely switch their choice to the ambiguous market but not to staying out.

comparative context in strategic environments. Instead of relating to a comparative or in a non-comparative context, ambiguity effects in strategic games depend on the strategic complexity in the games. The stronger the strategic complexity is, the more salient the ambiguity effects.

Relating our findings to the empirical world, why do most manufacturers and managers behave aggressively in market entry decisions? Excessive entry may result from high complexity in the market situations, where there is both demand uncertainty and drastic competitions.

We embark on the study of ambiguity effects in a kind of strategic game of market settings. Some may think that our findings of more entrants in the ambiguous market in random matching is related to decisions of the high level thinking. Camerer, Ho and Chong (2004) study the hierarchy in thinking steps in such strategic games. However, they find high level thinking may happen in members of the fixed group in simple strategic games. We conjecture that high level thinking is hard in random matching. As a result, high level thinking may help explain no ambiguity effects in fixed matching, but can not help explain ambiguity seeking found in random matching.

We do believe the importance of decision making to ambiguous situations in strategic environment, and it is far from fully understanding it. It is important to test ambiguity effects in other kind of strategic games and to see whether our results turn out to be robust.

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4.5 Appendix: Instruction for fixed matching treatments

General Information

The purpose of this experiment is to study how people make decisions in a particular situation. From now on and till the end of the experiment any communication with other participants is not permitted. If you have a question, please raise your hand and one of us will come to your desk to answer it.

You will receive 4 euros for showing up on time for the experiment. In addition, you will make money during the experiment. Upon completion of the experiment the amount that you make will be paid to you in cash. Payments are confidential; no other participant will be told the amount you make.

Rounds and Groups:

This experiment will have 50 rounds. In each round you will be in a group with 4 other participants, totally 5 people. The 5 people in a group are fixed In each round. You will not be informed of the identity of people in your group neither during the experiment nor in the end of the experiment.

Description of the Decision Task(s) in the Experiment:

In each round, you are asked to make a choice between one of two possible actions, action "X" or action "Y." If you choose action X, you will receive a fixed amount of money. If you choose Y, your payoff will depend on the state of the world and the choice of other participants in your group. Given certain state of the world, the less the number of Y chosen by your group, the higher your payoffs is in choosing action Y.

{Treatment RISK: The state of the world will be high or low. When you make your decision, you do not know it is high or low, but you know the probabilities of high and low.}

{Treatment AMBIGUITY: The state of the world will be high or low. When you make your decision, you do not know it is high or low, and you also do NOT know the probabilities of high and low. However, you know the probability is uniform in all rounds. }

How payoffs are determined

Payoffs in every round of this game are determined as follows.

• If you choose action X, your payoff for the round is 6.

• If you choose action Y, your payoff for the round depends on the state of the world and the total number of players, including yourself, who choose action Y.

Suppose that n =1, 2, 3, 4, 5 represent the number of players in your group who choose action Y. If you are one of these n players, your payoff for the round is given by:

Experimental points $= 6 + 2(c - n)$

{Treatment RISK, The value of c depends on the state of the world. In each round, it will be $c = 2.1$ with probability 1/2 and $c = 4.1$ with probability 1/2. }

{Treatment AMBIGUITY, The value of c depends on the state of the world. In each round, it will be $c = 2.1$ or $c = 4.1$ with unknown probability, but the probability keeps uniform in all rounds.}

For example, if $c = 4.1$ and $n = 1$, that is, the high state of world happens and you are the only player out of the group of 5 (1/5) who chooses action Y, then your payoff from choosing action Y would be $6 + 2(4.1 - 1) = 6 + 6.2 = 12.2$.

For another example, if $c = 2.1$ and $n = 5$, that is, the low state of the world happens and all five players (5/5) choose action Y, then each player's payoff from choosing action Y will be $6 + 2(2.1 - 5) = 6 - 5.80 = 0.2$

The complete set of possible payoffs you can earn from choosing action Y in each round are provided in the following table which you may refer to at any time during the experiment.

Payoffs in the low state of the world, $c = 2.1$

{Treatment RISK, (with probability $\frac{1}{2}$)}

{Treatment AMBIGUITY, (unknown probability but uniform over periods)}

Fraction of players who choose action Y	1/5	2/5	3/5	4/5	5/5
Payoff each earns from choosing action Y	8.2	6.2	4.2	2.2	0.2

Payoffs in the high market capacity situation, $c = 4.1$

{Treatment RISK, (with probability $\frac{1}{2}$)}

{Treatment AMBIGUITY, (unknown probability but uniform over periods)}

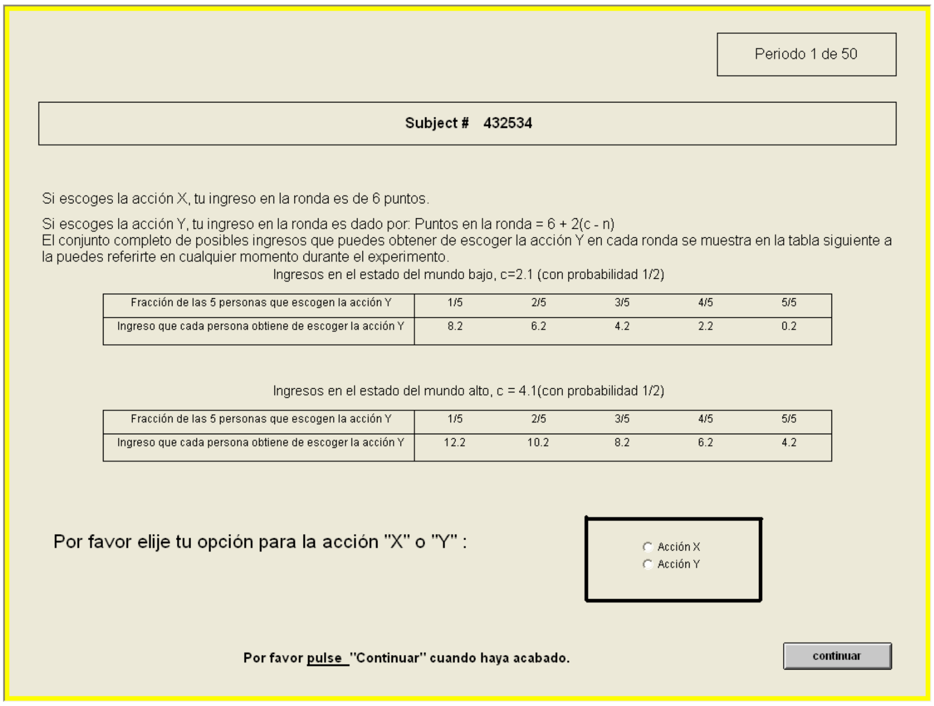
Fraction of players who choose action Y	1/5	2/5	3/5	4/5	5/5
Payoff each earns from choosing action Y	12.2	10.2	8.2	6.2	4.2

These payoff possibilities from playing action X or action Y will remain the same over all rounds. Are there any questions about how action choices determine payoffs?

Playing a round:

Note that in each round, when you make your decision you will not know other participants' choice in your group in the round and you will also not know the state of the world.

First, you need to make your choice on action X or action Y. The computer will display a screen like the one shown below. Please press the button besides your choice. You may change your choices as often as you like, but once you click on "Enter" your choice is final.



Periodo 1 de 50

Subject # 432534

Si escoges la acción X, tu ingreso en la ronda es de 6 puntos.

Si escoges la acción Y, tu ingreso en la ronda es dado por: Puntos en la ronda = 6 + 2(c - n)
El conjunto completo de posibles ingresos que puedes obtener de escoger la acción Y en cada ronda se muestra en la tabla siguiente a la puedes referirte en cualquier momento durante el experimento.

Ingresos en el estado del mundo bajo, c=2.1 (con probabilidad 1/2)

Fracción de las 5 personas que escogen la acción Y	1/5	2/5	3/5	4/5	5/5
Ingreso que cada persona obtiene de escoger la acción Y	8.2	6.2	4.2	2.2	0.2

Ingresos en el estado del mundo alto, c = 4.1(con probabilidad 1/2)

Fracción de las 5 personas que escogen la acción Y	1/5	2/5	3/5	4/5	5/5
Ingreso que cada persona obtiene de escoger la acción Y	12.2	10.2	8.2	6.2	4.2

Por favor elije tu opción para la acción "X" o "Y" :

Acción X
Acción Y

Por favor pulse "Continuar" cuando haya acabado.

continuar

{Treatment RISK, Meanwhile, the computer will "roll the die" to decide the state of the world, $c = 2.1$ or $c = 4.1$ with probability 1/2}

{Treatment AMBIGUITY, Meanwhile, the computer will "roll the die" to decide the state of the world, $c = 2.1$ or $c = 4.1$ with certain probability, and the probability is uniform for all the rounds}

Then, the computer helps calculate the result, and you will be informed of your payoff in this round, your accumulated payoff in the past rounds, and the decision you have made.

Payoffs

At the end of the experiment you will be paid, in cash, the sum of the payoffs that you will have earned in the rounds of the experiment plus show up fee 4 euros. The ratio between experimental points and euros is 1 point= 0.035 euros. As noted previously, you will be paid privately and we will not disclose any information about your actions or your payoff to the other participants in the experiment.

Payoff quiz

Before we begin the experiment, please answer the following questions. The following questions aim at helping you understand how the payoffs are realized. We will go through the answers to a sample problem before you do the rest of the quiz. Please raise your hand if you are having trouble answering one of the questions.

Sample Question: If you made a choice of action X, and the state of the world $c = 2.1$ and the number of Y in your group is 1, as a result, your payoff is 6.

Question 1: will the participants I am grouped with be the same in all rounds? _____

Question 2: Do you know the probability of high or low state of the world? ____

Question 3: If you made a choice of action Y, and the state of the world $c = 4.1$ and the number of Y in your group is 2, as a result, your payoff is ______