One Step at a Time: Does Gradualism Build Coordination?*

Maoliang Ye†, Jie Zheng‡, Plamen Nikolov§, Sam Asher**

Updated: December 31, 2017

* We would like to thank Alberto Alesina, Jim Alt, Nejat Anbarci, Abhijit Banerjee, Max Bazerman, Iris Bohnet, Hannah Bowles, Gary Charness, Raj Chetty, Lu Dong, Paolo Epifani, Ernst Fehr, Daniel Friedman, John Friedman, Roland Fryer, Francis Fukuyama, Sen Geng, Edward Glaeser, Francesca Gino, Brett Graham, Torben Iversen, Zhi Li, Garett Jones, Yuichiro Kamada, Larry Katz, Judd Kessler, David Laibson, Jeffrey Liebman, Jaimie Lien, Tracy Xiao Liu, Erzo F. P. Luttmer, Brigitte Madrian, Juanjuan Meng, Louis Putterman, Alvin Roth, Jason Shachat, Kenneth Shepsle, Andrei Shleifer, Monica Singhal, Dustin Tingley, Yun Wang, Roberto Weber, Xi Weng, Chunlei Yang, Tristan Zajonc, Richard Zeckhauser, Yao Zeng, and participants at the ESA meetings in Tucson, Zurich and Prague, the 2012 Chinese Meeting of the Econometric Society in Beijing, the Xiamen University Experimental Economics International Workshop in 2012 and 2013, the Princeton Graduate Conference on Psychology and Policy Making in 2012, the RES 2012 Conference in Cambridge, the SEA 2012 Conference in D.C., the APSA 2011 Conference in Seattle, the IEA 2011 Congress in Beijing, the CES 2011 Conference in Beijing, the PCS 2011 Meeting in San Antonio, the 2017 Shanghai Workshop on Experimental and Behavioral Economics, and seminars at Harvard, HBS, MIT, Peking University, Tsinghua University, Fudan University, SHUFE, WISE at Xiamen University, Shanghai Jiaotong University, Zhejiang University, SWUFE, UIBE, Southeastern University, Sun Yat-sen Univesrity, UNNC, and Renmin University of China for valuable comments at different stages of this project. We also thank Lorenzo Casaburi for his input in the early stage of our study. This study was financially supported by the Youth Grant for Humanities and Social Sciences Research, Chinese Ministry of Education [grant number 14YJC790156], the Fundamental Research Funds for the Central Universities [grant number 20720161075], and by the Institute for Quantitative Social Sciences at Harvard University. The usual disclaimer applies. The School of Economics at Renmin University of China for generously provided the laboratory. Xuelin Li and Yuqi Zheng provided excellent research assistance. The usual disclaimer applies.

† Corresponding author. Department of Public Finance, School of Economics, Wang Yanan Institute for Studies in Economics, MOE Key Laboratory of Econometrics, and Fujian Key Laboratory of Statistical Science, Xiamen University, Xiamen 361005, Fujian, China. E-mail: maoliang.ye@post.harvard.edu. Tel: (+86) 592-2188827.

‡ Department of Economics, School of Economics and Management, Tsinghua University, Beijing, China 100084. Email: jie.academic@gmail.com.

§ Department of Economics, State University of New York, Binghamton, NY 13902. Email: pnikolov@binghamton.edu.

** World Bank, 1818 H Street, NW, Washington, DC 20433, sasher@worldbank.org.

One Step at a Time:
Does Gradualism Build Coordination?

Abstract

This study investigates a potential mechanism to promote coordination. With theoretical guidance using a belief-based learning model, we conduct a multiple-period, binary-choice, and weakest-link coordination experiment in the laboratory to study the effect of gradualism – increasing the required levels ("stakes") of contributions slowly over time rather than requiring a high level of contribution immediately – on group coordination performances in high-stake projects. We randomly assign subjects to three treatments: starting and continuing at a high stake, starting at a low stake but jumping to a high stake after a few periods, as well as starting at a low stake and gradually increasing the stakes over time (the Gradualism treatment). We find that groups coordinate most successfully with high stakes in the Gradualism treatment relative to the other two treatments. We also find evidence that supports the belief-based learning model. These findings point to a simple mechanism for promoting successful voluntary coordination.

JEL Classifications: C91; C92; D03; D71; D81; H41

Keywords: gradualism, coordination, laboratory experiment, belief-based learning

1. Introduction

Coordination[1] is at the core of a wide variety of economic activities and organizational management (Schelling, 1960; Arrow, 1974). Although some large groups manage to coordinate successfully in the real world, the experimental literature shows that simultaneous large-group tacit coordination often fails; and repetition alone does not solve the problem.[2] For example, each individual has seven effort choices for actions in each period in many coordination games (e.g., Van Huyck, Battalio & Beil, 1990; Knez & Camerer, 1994, 2000; Cachon & Camerer, 1996; Weber, 2006; Chaudhuri, Schotter & Sopher, 2009). In a large group, members that have exerted a large effort in the first period reduce their effort levels once they observe that the low efforts of their partners harm their payoffs. After just several periods, a Pareto-inefficient outcome is attained, and the groups are then trapped in inefficient outcomes.

This study explores a potential mechanism to promote coordination: gradually increasing the stake of a coordination project within a fixed group.[3] We refer to gradualism as the hypothesis that allowing agents to coordinate first on small and easy-to-achieve goals (projects) and slowly increasing the level of goals facilitates subsequent coordination on otherwise hard-to-achieve outcomes. We observe many real-world examples of gradual coordination in settings within and between organizations. First, intra-organization team building often adopts a gradual method: New employees are given small or easier initial tasks to help build coordination, which ensures that they can coordinate well in larger or harder tasks later. Second, organizations often start small in collaborative projects with other organizations and if successful they build up to larger joint projects over time.

In this study, we propose theoretical predictions regarding the role of gradualism using a belief-based learning model and test the predictions using a computer-based laboratory experiment with repeated interactions. In each period, each subject is endowed with certain points (monetary units in the laboratory) and is asked to participate in a group project with a certain stake. Each subject has two options: (1) to contribute the exact pre-set amount (i.e., the stake) or (2) to contribute nothing. In each period, each subject realizes an extra return only when all group members contribute to the project; otherwise, each ends up with the initial endowment minus his contribution. This set-up is generally referred to as the minimum-effort or weakest-link coordination game: The payoff depends on the effort of the individual and the minimum effort of all group members. The stage game in each period is a multiple-player stag hunt because of the binary choice feature available to each player.

[1] In this study, we focus on those coordination games with Pareto-ranked equilibria.
[2] Devetag and Ortmann (2007) provide a survey on the disparity between the real world and the experimental literature.
[3] The present study focuses on a fixed-size group or a short horizon when the group size and group composition have not yet changed. For gradual organizational growth, see Weber (2006). In Section 2, we compare and contrast Weber's study to the present study.

We assign the subjects to three main treatments of stake patterns, which differ in the first six periods but feature an identical high stake for the next six periods. The first treatment, labeled Big Bang, features a constant high stake for all 12 periods. The second treatment, labeled Semi-Gradualism, features a constant low stake for the first six periods and then a high stake for the next six periods. In our third and key treatment, termed Gradualism, we increase the stake in each of the first six periods in small amounts until the highest stake is reached in Period 7 (See Figure 1 for a graphical illustration). We exploit this design to address how the pattern of varying stake levels influences group coordination at high-stake levels. Specifically, we test the effect of gradualism (i.e., starting with a low stake and slowly increasing the stake) on coordination performances in the high-stake periods.

[Figure 1 about here]

When a person interacts with others in a group, he/she not only learns about group members but may also develop beliefs about the contributing tendencies of an average person from the general population. Alternatively, the successful (unsuccessful) coordination history may simply encourage (discourage) one to contribute in a new group without learning about the population. In either case, the coordination history in a previous group may affect the contribution decision of an individual in a new group. To explore this potential transmission of coordination behavior from one group to another, we introduce a second stage to the experiment. The subjects from various treatments in the first stage were randomly reshuffled into new groups when they entered the second stage of the game, and played the same game as that at the end of the first stage.

Our first main finding is that the Gradualism treatment attains significantly more successful coordination at a high level of stake in the first stage. In our laboratory experiment, subjects in the Gradualism treatment are more likely to contribute in the final high-stake periods than in the other two main treatments. In terms of magnitude, the effects are large. For example, 61.1% of the Gradualism groups successfully coordinate in the end period, whereas only 16.7% and 33.3% of Big Bang and Semi-Gradualism groups do so, respectively. The Semi-Gradualism treatment fails to foster high-stake coordination compared with the Gradualism treatment. Our findings suggest that for a group to establish successful coordination at a high stake level, it is better to begin at a low-stake level and, equally important, to increase the stake level slowly.

Our second main finding is that subjects in the Gradualism treatment, who are more likely to experience successful coordination at the end of the first stage than subjects in the other treatments, are 12.2 percentage points more likely to contribute upon entering a new group when we reshuffle subjects from all treatments into new groups in the second stage. However, these subjects become less likely to contribute when they find that their contributions are not rewarded in the new environment because the new group members may have different coordination outcomes previously. This result shows the externality of coordination building (or collapse) across social groups.

To explore the potential channel that gradualism fosters high-stake coordination, we conduct a supplementary experiment where we have the same design with the main experiment except that we explicitly elicit the belief of the subjects regarding the probability that his group members contribute in each period. The results support the belief-based learning model we use to derive our theoretical predictions: Subjects form their initial beliefs based on the initial stake, make their contribution decisions based on their beliefs, and update their beliefs after observing the coordination outcome in each period.

The present study is the first that clearly tests the role of exogenous gradualism in coordination within a given group. The present study adopts an exogenous setting of stake paths from a normative perspective and answers whether gradualism works better rather than whether players choose gradualism. Through randomizing subjects into various treatments, we can also avoid the self-selection problems when gradualism is endogenously chosen. We find that the coordination success rate is indeed higher under the gradualism mechanism than under the competing mechanisms, and this may help explain why gradualism is a popular practice in many real-world settings.

The present study has two key contributions. First, our study contributes to the literature on coordination mechanism. Economists have addressed ways to promote successful coordination via various mechanisms, such as communications (Cooper, DeJong, Forsythe & Ross, 1992; Charness, 2000; Weber, Camerer, Rottenstreich & Knez, 2001; Duffy & Feltovich, 2002, 2006; Chaudhuri et al., 2009), teams (Feri, Irlenbusch & Sutter, 2010), between-group competitions (Bornstein, Gneezy & Nagel, 2002; Riechmann & Weimann, 2008), voluntary group formations (Yang, Xu, Meng, & Tang, 2017), gradual organization growths (Weber, 2006), social identities (Chen & Chen, 2011; Chen, Li, Liu & Shih, 2014), full information feedback about others' previous round choices (Berninghaus & Ehrhart, 2001; Devetag, 2003; Brandts & Cooper, 2006a), and precedent transfer (Devetag, 2005). The present study examines how exogenous stake paths foster successful group coordination, thus proposing an alternative mechanism, i.e., gradualism. To the best of our knowledge, the only other studies that examine the effect of exogenous path dependence on group coordination are Weber (2006) and Romero (2015).[4] Second, our study contributes to the more general literature on dynamic simultaneous games with a gradualist feature, such as in public goods (Dorsey, 1992; Marx & Matthews, 2000; Kurzban, McCabe, Smith & Wilson, 2001; Duffy, Ochs & Vesterlund, 2007; Offerman & van der Veen, 2013; Oprea, Charness & Friedman, 2014), and prisoners' dilemmas

[4] Riedl, Rohde and Strobel (2011), Salmon and Weber (2011), and Yang et al. (2013) can be considered a general path dependence approach, but with endogenous paths. They all focus on the group size issue. However, stake path change in our design is novel.

(Andreoni & Samuelson, 2006). We provide detailed comparisons between the present study and the literature on dynamic simultaneous games in Section 2.

2. Relation with the Literature on Dynamic Simultaneous Games

In order to better understand our study and its contributions, below we compare it to the theoretical and experimental literature in coordination games with varying paths, and in other dynamic simultaneous games (e.g., public goods games, prisoners' dilemma games) with a gradualist feature.

In a dynamic laboratory weakest-link coordination experiment, Weber (2006) studies the dynamics of organizational growth and finds that gradually growing group size leads to more successful coordination in a large group versus starting with a large group. The present study differs from Weber (2006) in four major ways: (1) We vary the stake path and explore gradualism in coordination within a given fixed-size group, whereas Weber studies the path of group size; (2) the choice set in each period is binary, and the payoff structure is simpler; (3) a third main treatment Semi-Gradualism is used, which explores whether a sudden stake increase negatively affects coordination; (4) and our theoretical model is based on the general belief-based learning framework, whereas Weber (2006) adopts a linear adaptive dynamics.

In a concurrent study, Romero (2015) finds that groups coordinate better with a certain cost when the cost is increasing than when the cost is decreasing to that level. Our setting differs from his in two ways: (1) We change the stake level, which indicates not only the cost but also the benefit;[5] (2) and we compare a slow increase of the stake with a sudden increase and a start at a high stake, whereas Romero alternatively compares an increasing path of the cost with a decreasing one. In addition, the theoretical models are also different: We employ a belief-based learning model with supporting experimental evidence.

Battalio, Samuelson and Van Huyck (2001) also explore stake size effects in coordination games in a laboratory environment. They focus on how various levels of optimization premium (defined as the difference between the payoff of the best response to an opponent's strategy and the inferior response) influence players' behavior differently, in which the stake size is the optimization premium. In their study, the stake size is fixed within a treatment. By contrast, we allow the stake size, interpreted as the participation cost (as well as net profit of success) of a stag-hunt coordination project, to vary over time within a treatment. In particular, we examine the role of gradually increasing the stakes in building large-stake coordination, which is not examined by Battalio, Samuelson and Van Huyck (2001).

[5] Other studies change either benefit (Brandts & Cooper, 2006b) or cost (Goeree & Holt, 2005), but not both. Our setup reflects those studies (Van Huyck et al., 1990; Knez & Camerer, 1994, 2000; Cachon & Camerer 1996; Weber, 2006; Chaudhuri et al., 2009) that fixes the benefit-cost ratio at 2:1 at equilibrium, but we simplify the decision choice to be binary and change the scale of the group project simultaneously through varying the stake. Yang et al. (2017) have a similar setup where the benefit-cost ratio is constant at 2:1 at equilibrium, but the scale increases with group size.

The gradualist approach in the present study differs from those in previous studies of Dorsey (1992), Marx and Matthews (2000), Kurzban et al. (2001), Duffy et al. (2007), and Oprea et al. (2014) on dynamic real time public goods provision. Other than the binary weakest-link structure that differs from these studies, another unique feature of our setup is that the public projects in our game are independent from one period to the next: Contributions cannot accumulate over periods, and each project features its own target (stake). In the aforementioned studies on dynamic voluntary contribution to a single public project, players are allowed to contribute whenever and as much as they wish and to accumulate their contributions over the course of the project (before the end of the game, each period has no objective). Our study determines whether working on smaller tasks first facilitates a group in accomplishing a large collective task, rather than whether dividing the call for contribution into multiple periods and allowing contributions to accumulate over time improves collective contributions. Although the aforementioned studies relate to certain real-world examples (e.g., long-term fund drives), the present study is better aligned with other important real-world cases that were mentioned earlier. In the examples mentioned in the introduction, the duration of the final high-stake project is relatively short and is not divisible into subperiods to accumulate effort. Moreover, regular feedback about what other participants contribute to the final project is not provided. Instead, players face an independent project with a clear and smaller objective in each period other than the final high objective, and players assess how they performed on these small tasks after each period.

In another concurrent study, Offerman and van der Veen (2013) explore whether governmental subsidies geared toward promoting public good provisions should be abruptly introduced or gradually increased; in other words, given the benefit of the public good, whether the individual cost of providing the public good should be decreased sharply or gradually. The results favor an immediate increase of subsidy: When the final subsidy level is substantial, the effect of a quick increase is stronger than that of a gradual increase. The present study differs from Offerman and van der Veen (2013) in three key ways. First, these authors focus on how the use of subsidies can stimulate cooperation after an unsuccessful cooperation at the start of a game. The mechanism used in the present study is distinct from their subsidy mechanism because our mechanism focuses on the variation of stake patterns instead of governmental subsidies. Second, our study manipulates the stake level that decides both the cost and the benefit of the public good, whereas in the set up of Offerman and van der Veen, only the cost changes. A third distinction relates to the fact that the stake paths of the present study are non-decreasing, whereas paths of the other are non-increasing.

We outline the efficiency gains of gradualism more clearly than Andreoni and Samuelson (2006). Andreoni and Samuelson (2006) examine a twice-played prisoners' dilemma, in which the total stakes in two periods are fixed, whereas the distribution of these stakes across periods can be varied. Both their theoretical and experimental results show that the best way is to "start small" with bigger stakes in the second period. However, cooperation is low for the period with a high stake in their experiment. One potential explanation of the advantage of our setup in promoting efficiency over that of Andreoni and Samuelson (2006) is that our weakest-link structure does not allow free riding, unlike the other setup.

Several theoretical studies examine monotone games, which are multi-period games in which players are constrained to choose strategies that are non-decreasing over time (i.e., players must increase their respective contributions over time) (Gale, 1995, 2001; Lockwood & Thomas, 2002; Choi, Gale & Kariv, 2008). In contrast to these studies, our game employs a different feature – we enable the stake to be non-decreasing, rather than the contribution.

Watson (1999, 2002) examines theoretically how "starting small and increasing interactions over time" is an equilibrium for dynamic cooperation with the option to break up unilaterally, which forces "starting small" in equilibrium endogenously. Through determining the stake path exogenously, we address whether gradualism promotes coordination at high-stake levels among fixed partners with no chance of breaking up (each player in a group can choose not to contribute in each period, but not to leave the group before the game ends), rather than whether players themselves choose to adopt a gradualist approach. In addition, our setup focuses on weakest-link coordination problems with no chance of free riding.

3. Experimental Settings

3.1 Game Composition, Information Structure, and Payoff Structure

We conducted the laboratory experiment at the Renmin University of China in Beijing, China, in July 2010 with 256 subjects that were recruited through the bulletin board system and posters. The majority of the subjects were students from Renmin University and nearby universities.

The experiment consisted of 18 sessions, which were all computerized using the z-Tree software package (Fischbacher, 2007). Both the instructions (see supplemental material S1) and the game information shown on the computer screen were in Chinese. In each session, we randomly assigned subjects to groups of four; our sample consisted of 64 groups in total.

The experiment included two stages: The first stage comprised twelve periods, whereas the second one comprised eight periods. Group members did not change within each stage but subjects were randomly reshuffled into groups of four after the first stage; this rule was made to be common information. The subjects were not told the exact number of periods in each stage. Instead, the subjects were told that the experiment would last from 30 minutes to one hour, including the time for sign-up, reading of instructions, taking a quiz designed to ensure that subjects understood the experimental rule, and final payment. Such a design reflects many real-world cases when people do not know the exact number of coordination opportunities.

At the beginning of each period, subjects knew the stake of the current period but not those of future periods. This condition replicates the circumstances of many real-world cases, in which people do not know what is at stake in future interactions.

In each period, we endowed each subject with 20 points and asked each to give a certain number of points to the common pool of his assigned group. The required number could vary across periods, and each subject could only choose either "to give the *exact* points required" (we use the natural term "give" rather than "contribute" in the instruction), which we refer to as *stake*, or "not to give" at all. If all members in a group contributed, then each member not only received the stake back, but also gained an extra return, which equaled the stake. If not all group members contributed, then each member finished the period with only his remaining points (i.e., the initial endowment in each period minus the contribution of the subject).

In summary, players earned according to the following function in each period, which is conditional on the actions of all players in the same group:

$$Earning_{i,t} = \begin{cases} 20 + S_t, & if\ A_{i,\,t} = C\ \ and\ A_{j,\,t} = C, \forall j \neq i \\ 20, & if\ A_{i,\,t} = NC \\ 20 - S_t, & if\ A_{i,\,t} = C\ \ and\ \exists j \neq i,\ s.t.\ A_{j,\,t} = NC \end{cases}$$

where $Earning_{i,t}$ is i's earning in period t, S_t is the stake at t. $A_{i,\,t}$ and $A_{j,\,t}$ are the actions of i and j at t, respectively (i and j are in the same group.) C represents "contribute," whereas NC represents "not contribute." Thus, each period featured a four-player stag hunt game, in which two pure-strategy Nash equilibria exist: when all players contribute and when no player contributes. For all values of S_t, the secure equilibrium (all players choosing NC) is risk-dominant according to the Harsany-Selten definition. Except for the binary choice set in each period, our setup is consistent with the standard of the minimum effort coordination literature that has a 2:1 benefit–cost ratio at each pure-strategy equilibrium.[6]

We did not allow communication across subjects for two reasons. First, communication among heterogeneous groups, which could benefit from coordination, is often impossible or restricted in the real world. Second, a design that precludes communication makes coordination among players difficult. We study how gradualism may help solve coordination difficulties in the absence of communication mechanisms.

At the end of each period, each subject knew whether all four group members (including himself/herself) contributed the stake for that period but did not know the total number of group members

---

[6] For example, in Van Huyck et al. (1990), player *i*'s payoff is $p_i = .20(\min x_k) - .10x_i + .60$, where $\min x_k$ is the minimum effort of all group members including *i*. In each pure-strategy equilibrium, $x_k = x_i$ for all *k* and $(\min x_k) = x_i$, thus the benefit–cost ratio is .20/.10=2 and the net return (benefit–cost) at equilibrium equals the effort cost, as in our case.

who contributed (in case fewer than four members contributed). This design is consistent with the type of minimum-effort coordination games with limited information feedback, in which the only commonly available historical data to players is the minimum contribution of group members. [7] By adopting this design we can also increase the difficulty of coordination given other aspects of the experiment, and study whether gradualism can help overcome such a difficulty.

The final total payment to each player equaled the accumulated earnings over all periods plus a show-up fee. The exchange rate was 40 points per CNY 1. An average subject earned CNY 21–22 (around USD 3) including the show-up fee for the whole experiment, which covered ordinary meals for one to two days on campus. With regard to the purchasing power, this payment was comparable to those experiments conducted in other countries.

3.2 Treatment Group Assignments

Our experiment comprised three main treatments: (1) Big Bang, (2) Semi-Gradualism, (3) Gradualism, as well as a fourth High Show-up Fee treatment, which is a variant of the Big Bang treatment. All groups in the three main treatments faced the same stake in the second half (Periods 7–12) of the first stage, but stake paths differed for each treatment in the first half (Periods 1–6). The first half of the first stage featured different stake paths for each treatment. The different stake paths might yield potential earning differences and could potentially lead to a wealth effect across treatments. To isolate the wealth effect on the contribution of participants from the effect of the three main treatments in the second half of the first stage, we introduced the High Show-up Fee treatment, which is identical to the Big Bang treatment except that we give subjects high show-up fees. We describe the High Show-up Fee treatment in detail later in this Section. We randomly assigned 12 subjects (three groups) into the three main treatments for eight of the 18 sessions. In the remaining 10 sessions, we randomly assigned 16 subjects (four groups) into the four treatments (three main treatments and the High Show-up Fee treatment). In total, we had 18, 18, 18, and 10 groups (or 72, 72, 72 and 40 subjects) in Big Bang, Semi-Gradualism, Gradualism, and High Show-up Fee treatments, respectively. Unreported randomization checks show that the randomization of treatment assignment worked well (available upon request).

Figure 1 shows the game stakes across periods. For the Big Bang treatment, the stakes were always kept at the highest level, which was 14.[8] For the Semi-Gradualism treatment, we set the stakes at two for the first six periods and then we set them at the highest stake for the next six periods. Finally, for the

[7] This feature is popular in contract theory literature, in which an imperfect observation of efforts is common.

[8] We calibrated the highest stake level using 12 and 14, and finally opted for 12 in two sessions and 14 in 16 sessions. To make full use of the samples, we pooled all 18 sessions together in the analysis.

Gradualism treatment, we gradually increased the stakes from 2 to 12 with a step of 2 for the first six periods, and we kept them fixed at the highest stake for the next six periods.

To isolate the potential wealth effect, we varied the show-up fee for treatments. The show-up fee provided to each subject for the three main treatments was 400 points, whereas that for the High Show-up Fee treatment was 480 points. The extra 80 points sufficiently captured the potential earning differences accumulated over Periods 1–6 (we discuss this in detail in Section 6); thus, this treatment enabled us to isolate the wealth effect by comparing the High Show-up Fee to the Big Bang treatment.[9]

This experiment has a second stage, as mentioned. This stage aims to explore the potential transference of the coordination behavior of an individual from an old social group into a new one when he/she interacts with new members that have a different coordination history. The subjects from various treatments were randomly reshuffled into groups of four when they entered the second stage of the game. New group members did not necessarily come from the same group in the first stage; this rule was made to be common information. Within the second stage of the game, group compositions were fixed, and stakes were all set at the highest stake (14) for all periods and all groups (i.e., those treated in different treatments in the first stage faced the same stake in each period of the second stage).

At the start of the second stage, we notified each player that he/she would enter a new random group. At the end of each stage, we notified each player of the number of points he/she had accumulated to date.

At the end of the experiment, we asked subjects to complete a brief survey that collected information on age, gender, nationality, education level, concentration at school, working status, income, as well as their risk preferences over lotteries adopted from Holt and Laury (2002).[10]

Table 1 and Figure 1 illustrate the main experimental design.

[Table 1 about here]

---

[9] The method to isolate the wealth effect in the present study differs from the popular method used in the experimental literature, which randomly selects one or several periods for payment. Although no consensus is reached on whether the wealth effect is a serious problem and whether this effect should be isolated in repeated games, we are concerned that adopting random payments may change the behaviors of some subjects: Some may play more cautiously because the period(s) selected for pay fully determine their performance payments, and others may play randomly or less seriously because not all periods count for payments. Our alternative method, which varies the show-up fee, avoids this concern: Comparing the performances under different show-up fees can test the wealth effect associated with the show-up fee. Of course, such a method has its limitation: The potential wealth effect of show-up fee, which subjects may consider to be a "sunk benefit," may differ from the wealth effect of the performance earning accumulated across the earlier periods. To partially address this limitation, we denominate the show-up fee in points (versus in CNY), which has the same term and unit with the stake and accumulated performance earnings in the experiment.

[10] We code the risk aversion attitude as the number of option A chosen in questionnaire by Holt and Laury (2002), ranging from 0 to 10. Thus the larger the value, the more averse the subject is to risk. Simple regression of contribution decision on risk aversion shows the average marginal effect of risk aversion on contribution decision is -0.01, which suggests that if risk aversion increases from 0 (smallest possible value) to 10 (largest possible value), on average the contribution rate will fall by 10 percentage points. However, this effect is statistically insignificant when standard errors are clustered at either the group level (p=0.44) or at the subject level (p=0.45). Regression tables are available upon request.

4. A Simple Belief-based Learning Model with Theoretical Predictions

The coordination problem in each period involves multiple equilibria. When multiple equilibria exist, the beliefs of the players are central in deciding which equilibrium outcome will be selected. Gradualism and alternative stake paths may matter in the coordination dynamics because they affect beliefs: The stake level at the start of the game influences how players form their initial beliefs about the actions of others, whereas the stake path influences how players subsequently update these beliefs. Thus, we develop a simple belief-based learning model for theoretical predictions, although we do not rule out the possibility that other theoretical models may also be consistent with our experimental results. Another reason for the adoption of belief-based learning model rather than reinforcement learning model (e.g., Roth & Erev, 1995) is that the salient change in the stakes between Periods 6 and 7 in the Semi-Gradualism treatment may make players less likely to naively follow previous reward-generating actions.

Before we go into the full details of our theoretical model, we present the basic intuition as follows. In the belief-based learning framework, rational players have prior beliefs about the actions of others before the game starts, and update these beliefs based on the outcome of each period. The lower the stake at the start of a game, the cheaper for players to attempt coordination in the face of uncertainty, and the stronger their beliefs are that others will contribute to the group project. Therefore, under the weakest-link payoff structure, the lower the stake at the start of a game, the higher the contribution and success rates of group coordination are at the start of a game (we define coordination success as the case that all members in a group contribute.) When groups successfully coordinate at a given stake level, players reinforce their beliefs about the likelihood that others will contribute at the same stake level. Alternatively, coordination failure at a given stake level causes players to doubt that other players in their group will contribute later at the same or a higher stake level. Finally, when stakes increase in two consecutive periods, previous successful coordination at the low stakes may (or may not) largely influence posterior beliefs of the players regarding the actions of the other players at slightly (or substantially) higher stake levels. Thus, successful coordination at a low-stake level may not imply an immediate successful coordination at a high-stake level if the stake increases dramatically. For this reason, slowly increasing the stake may better maintain coordination success.

The main aspects of the model are belief-based learning, myopia,[11] and standard self-interested preference. These assumptions allow us to focus on the belief updating process, which is an important

[11] We adopt the myopia (i.e., backward looking) assumption for two reasons. First, this assumption is often used in learning models, such as reinforcement learning (e.g., Roth & Erev, 1995), belief-based learning (e.g., Fudenberg & Levine, 1998), experience-weighted attraction learning (e.g, Camerer & Ho, 1998, 1999), and adaptive dynamics (e.g, Crawford, 1995; Van Huyck et al., 1997; Weber, 2006). Second, myopia allows us to focus on the key process of belief updating. In our experiment players do not know the future stake and the number of periods, which may limit

feature in dynamic coordination games. A more general model of dynamic games is beyond the scope of the present study.

The game structure and payoff rule in each period are as follows. There are *N* periods. In each period, a group of *I* risk-neutral players conduct a binary coordination task: Each player can choose to contribute ("*C*") or not to contribute ("*NC*"). The endowment per person in each period is *E*. The stake of the coordination task, $S_t (0 < S_t < E)$, may vary across periods. Thus, each player can choose to contribute either zero or exactly S_t in each period, but no other amount of efforts to the group project. We adopt a minimum-effort (weakest-link, or more specifically, stag-hunt) payoff structure: The value of the project output for everyone is αS_t ($\alpha > 1$; we adopt $\alpha = 2$ in our experiment) if all *I* players contribute S_t, and zero otherwise. Thus, the payoff of player *i* in period *t* is as follows:

$$Earning_{i,t} = \begin{cases} E + (\alpha - 1)S_t, & if\ A_{i,t} = C\ \ and\ A_{j,t} = C, \forall j \neq i \\ E, & if\ A_{i,t} = NC \\ E - S_t, & if\ A_{i,t} = C\ \ and\ \exists j \neq i,\ s.t.\ A_{j,t} = NC \end{cases}$$

Players do not know the actions of others when they make decisions. After each period, each player knows whether all members in his/her group (including himself/herself) have contributed in that period.

The stage game has two pure-strategy Nash equilibria, one payoff-dominant equilibrium with all players choosing *C* and one risk-dominant equilibrium with all choosing *NC*, as well as a mixed-strategy Nash equilibrium in which all players choose *C* with probability $\alpha^{-\frac{1}{I-1}}$. In contrast to the one-shot stage game, we consider a multi-period interaction between players where players can update their subjective beliefs regarding the behavior of other players over time. In a typical player *i*'s mind, player *j*'s type ($j \neq i$) is characterized by the highest level of contribution player *j* (unconditionally) chooses, denoted by X_j, where $X_j \geq 0$.[12] Thus, player *i*'s belief about player *j*'s type is a probability distribution of X_j, which can be characterized by a continuous and weakly increasing cumulative distribution function, $F_j^i(\cdot)$.[13]

A player's beliefs on the type of another player can be updated over time through observing the outcomes of the stage game in previous periods. According to the information feedback feature in the setting, after each period *t*, player *i* only knows whether or not all group members (including *i*) have contributed at *t*. There are in total three cases:

---

the potential of forward looking and more strategic play. Thus, the backward looking assumption is a reasonable simplification.

[12] A player of type X is assumed to contribute S_t for all $S_t \leq X$.

[13] In the earlier version of this paper, a level-k thinking (Nagel, 1995; Stahl & Wilson, 1995; Ho et al., 1998; Costa-Gomes et al., 2001; Costa-Gomes & Crawford, 2006; Costa-Gomes et al., 2009) is assumed in order to derive the theoretical predictions of the model. We thank the editors and the referees for suggesting the direction for a simpler belief-updating learning process.

Case 1: *i* and all other group members contribute at *t* under stake level S_t, thus coordination succeeds. Then, *i* gets the feedback that all other group members have contributed. *i* updates belief and believes that the contribution thresholds of all other group members are not smaller than S_t. Specifically, suppose at the beginning of period *t* player *i*'s belief about player *j*'s type was $F^i_{j,t}(\cdot)$ over $[0,+\infty)$ and at period *t* all players contributed at the stake level S_t, then at the beginning of period *t+1* player *i*'s belief about player *j*'s type will become $F^i_{j,t+1}(\cdot)=1-(1-F^i_{j,t}(\cdot))/(1-F^i_{j,t}(S_t))$ over $[S_t,+\infty)$ according to the Bayesian rule.

Case 2: *i* contributes, but not all other group members have contributed at *t*, thus coordination fails. Then, *i* gets the feedback that not all other group members have contributed. *i* updates belief and believes that at least one group member's contribution threshold is smaller than S_t. Therefore, at *t*+1 *i* believes that not all other group members will contribute for any $S_{t+1}\geq S_t$.

Case 3: *i* does not contribute at *t*, thus coordination fails. Then, *i* only gets the feedback that the group fails to coordinate but cannot know whether all other group members have contributed. *i* cannot update the minimum contribution threshold of all other group members.

Since the game is played anonymously and each player's strategy is not made known to other players, it is reasonable to assume that a player's belief is the same across all other players, and therefore we simplify $F^i_{j,t}(\cdot)$ as $F^i_t(\cdot)$.[14]

Assuming that every player forms a rational belief regarding the type of other players described as above, the equilibrium for the stage game critically depends on players' beliefs and can be characterized in the following lemma.

Lemma 1. For a given period *t* with stake level S_t, player *i* chooses *C* if and only if his belief satisfies $F^i_t(S_t)\leq 1-\alpha^{-\frac{1}{I-1}}$, and in the symmetric belief case we have the following equilibrium:

$$Equilibrium_t=\begin{cases} A_{i,t}=C,\forall i & if\ F^i_t(S_t)\leq 1-\alpha^{-\frac{1}{I-1}},\forall i \\ A_{i,t}=NC,\forall i & if\ F^i_t(S_t)> 1-\alpha^{-\frac{1}{I-1}},\forall i \end{cases}$$

Proof: See supplementary material S2.

The intuition of Lemma 1 is very simple: The more confident a player is about his group members contributing at the current stake level, the more likely this player is going to contribute, and the more likely the coordination will succeed.

In the following, we investigate how different conditions on stake levels across periods will affect the likelihood of successful coordination. First, we show that coordination is easier when the stake size is small.

[14] Our main results hold even if we allow for heterogeneous beliefs across other players or under the assumption of risk averse behavior. The proofs are available upon request.

Proposition 1 (Initial Stake). The lower the stake at period 1, S_1, the higher the probabilities that each player will contribute and that the coordination will succeed at period 1.

Proof: Suppose $S_1' < S_1$. By the weak monotonicity feature of c.d.f, we have $F_1^i(S_1') \leq F_1^i(S_1)$, for all player i. By Lemma 1, it is straightforward that if player i chooses to contribute under the stake level S_1 (requiring $F_1^i(S_1) \leq 1-\alpha^{-\frac{1}{I-1}}$), he will always do so under the stake level S_1' (since $F_1^i(S_1') \leq F_1^i(S_1) \leq 1-\alpha^{-\frac{1}{I-1}}$). Therefore, the probability of each player contributing and that of successful coordination are higher under S_1' than under S_1. *Q.E.D.*

Then we show that fixing the size of stakes across different periods, the dynamic pattern of coordination is path-dependent and persistent.

Proposition 2 (Persistency of Success/Failure). In two consecutive periods with the same stake $S_{t+1} = S_t$, a group will succeed in coordination at period $t+1$ when it succeeds at period t and will fail in coordination at period $t+1$ when it fails at period t.

Proof: Suppose that a group succeeds at period t, by Lemma 1 it must be the case that $F_t^i(S_t) \leq 1-\alpha^{-\frac{1}{I-1}}$ for all player i. At the beginning of period $t+1$, every player updates his belief such that $F_{t+1}^i(S_t) = 1-(1-F_t^i(S_t))/(1-F_t^i(S_t)) = 0 \leq 1-\alpha^{-\frac{1}{I-1}}$, and by Lemma 1 every player will contribute resulting in successful coordination. Suppose instead that a group fails at period t, by Lemma 1 it must be the case that $F_t^i(S_t) > 1-\alpha^{-\frac{1}{I-1}}$ for some player i. Given that at the next period this player i will not update his belief and will still not choose to contribute, the coordination will fail at period $t+1$ as well. *Q.E.D.*

We can also show that a slow increase in stake is always not worse than a quick increase for maintaining successful coordination, as shown below.

Proposition 3 (Quick vs. Slow Increase). Conditional on successful coordination at period t with a stake S_t, the lower the next stake $S_{t+1} (> S_t)$, the higher the probability that the coordination at period $t+1$ will succeed.

Proof: Suppose $S_t < S_{t+1}' < S_{t+1}$ and it suffices to show that conditional on successful coordination at period t with stake level S_t, if the coordination succeeds at period $t+1$ with S_{t+1}, it will also succeed with S_{t+1}'. Given the successful coordination at period t, by Lemma 1 it must be the case that $F_t^i(S_t) \leq 1-\alpha^{-\frac{1}{I-1}}$ for all player i. At the beginning of period $t+1$, every player updates his belief such that $F_{t+1}^i(S_{t+1}) = 1-(1-F_t^i(S_{t+1}))/(1-F_t^i(S_t))$ and $F_{t+1}^i(S_{t+1}') = 1-(1-F_t^i(S_{t+1}'))/(1-F_t^i(S_t))$. Since $S_{t+1}' < S_{t+1}$, we have $F_{t+1}^i(S_{t+1}') \leq F_{t+1}^i(S_{t+1})$ for all player i. Therefore, if the coordination succeeds at period $t+1$ with S_{t+1} (by Lemma 1 $F_{t+1}^i(S_{t+1}) \leq 1-\alpha^{-\frac{1}{I-1}}$), we must have $F_{t+1}^i(S_{t+1}') \leq F_{t+1}^i(S_{t+1}) \leq 1-\alpha^{-\frac{1}{I-1}}$, implying that the coordination will also succeed with S_{t+1}'. *Q.E.D.*

Based on the results described above, we would like to theoretically compare the coordination success rate of the three mechanisms in question (namely the Big-Bang mechanism, Semi-Gradualism mechanism, and the Gradualism mechanism). These mechanisms vary only in terms of the stake levels $S_t (t = 1, \cdots, N)$ over periods, specified below, and we assume $S_1 > 0$, $l > 0$, $1 < N_1 < N$, where $N_1 + 1$ is the first period for the stake to reach the highest level.

Big-Bang mechanism: $S_t^{BB} = S_1 + N_1 l, t = 1, \cdots, N$.

Semi-Gradualism mechanism: $S_t^{SG} = \begin{cases} S_1, & t = 1, \cdots, N_1 \\ S_1 + N_1 l, & t = N_1 + 1, \cdots N \end{cases}$

Gradualism mechanism: $S_t^{G} = \begin{cases} S_1 + (t-1)l, & t = 1, \cdots, N_1 \\ S_1 + N_1 l, & t = N_1 + 1, \cdots N \end{cases}$

Obviously, we have $S_1^{BB} = S_1^{SG} = S_1^{G} = S_1$ and $S_t^{BB} = S_t^{SG} = S_t^{G} = S_t \equiv S_1 + N_1 l$ for all $t = N_1 + 1, \cdots N$.

Theorem 1 (Performance Comparison). For any number of players ($I \geq 2$), for any multiplier ($\alpha > 1$), for any continuous and weakly increasing belief functions ($F_1^i(\cdot), i = 1, \cdots, I$),

(a) Gradualism outperforms Semi-Gradualism;

(b) Semi-Gradualism outperforms Big-Bang,

where mechanism A outperforms mechanism B in the sense that for period $t = N_1 + 1, \cdots N$, A succeeds in coordination whenever B succeeds in coordination.

Proof: First we show that Gradualism outperforms Semi-Gradualism. It suffices to show that Gradualism succeeds in coordination at period $t = N_1 + 1$ whenever Semi-Gradualism succeeds at period $t = N_1 + 1$. Suppose that Semi-Gradualism succeeds at period $t = N_1 + 1$, it must be the case by the previous analysis that Semi-Gradualism succeeds in coordination for all periods $t = 1, \cdots, N_1 + 1$. This implies (1) $F_{N_1+1}^i(S_{N_1+1}) = 1 - (1 - F_1^i(S_{N_1+1})) / (1 - F_1^i(S_1)) \leq 1 - \alpha^{-\frac{1}{I-1}}$ and (2) $F_1^i(S_1) \leq 1 - \alpha^{-\frac{1}{I-1}}$, for all player *i*. Note that (2) implies that Gradualism succeeds in coordination at period *t=1*. Given this, at the beginning of period *t=2*, every player updates his belief such that $F_2^i(S_2^G) = 1 - (1 - F_1^i(S_2^G)) / (1 - F_1^i(S_1))$. Since $S_2^G < S_{N_1+1}$ implies $1 - (1 - F_1^i(S_2^G)) / (1 - F_1^i(S_1)) \leq 1 - (1 - F_1^i(S_{N_1+1})) / (1 - F_1^i(S_1))$, we have $F_2^i(S_2^G) \leq F_{N_1+1}^i(S_{N_1+1}) \leq 1 - \alpha^{-\frac{1}{I-1}}$, indicating that Gradualism succeeds in coordination at period *t=2*. Given this, at the beginning of period *t=3*, every player further updates his belief such that $F_3^i(S_3^G) = 1 - (1 - F_1^i(S_3^G)) / (1 - F_1^i(S_2^G))$. Note that $S_3^G < S_{N_1+1}$ and $S_1 < S_2^G$, so we have $F_3^i(S_3^G) \leq F_N^i(S_{N_1+1}) \leq 1 - \alpha^{-\frac{1}{I-1}}$, again indicating that Gradualism succeeds in coordination at period *t=3*.

A simple mathematical induction can show that given Gradualism succeeds in coordination at period *t*, it will also succeed in coordination at period *t+1*, therefore at period $t = N_1 + 1$, Gradualism will succeed in coordination.

Second we show that Semi-Gradualism outperforms Big-Bang. It suffices to show that Semi-Gradualism succeeds in coordination at period $t = N_1 + 1$ whenever Big-Bang succeeds at period $t = N_1 + 1$. Suppose that Big-Bang succeeds at period $t = N_1 + 1$, it must be the case that Big-Bang succeeds in coordination for all period $t = 1, \cdots, N_1 + 1$. This implies $F_1^i(S_{N_1+1}) \le 1 - \alpha^{-\frac{1}{I-1}}$, for all player *i*.

Note that $F_1^i(S_1) \le F_1^i(S_{N_1+1}) \le 1 - \alpha^{-\frac{1}{I-1}}$ implies that Semi-Gradualism succeeds in coordination for all periods $t = 1, \cdots, N_1$. Given this, at the beginning of period $t = N_1 + 1$, every player updates his belief such that $F_{N_1+1}^i(S_{N_1+1}^{SG}) = 1 - (1 - F_1^i(S_{N_1+1})) / (1 - F_1^i(S_1))$. Note that $F_{N_1+1}^i(S_{N_1+1}^{SG}) = 1 - (1 - F_1^i(S_{N_1+1})) / (1 - F_1^i(S_1)) \le 1 - (1 - F_1^i(S_{N_1+1})) = F_1^i(S_{N_1+1})$ and $F_1^i(S_{N_1+1}) \le 1 - \alpha^{-\frac{1}{I-1}}$, we have $F_{N_1+1}^i(S_{N_1+1}^{SG}) \le 1 - \alpha^{-\frac{1}{I-1}}$, indicating that Semi-Gradualism succeeds at period $t = N_1 + 1$.

Q.E.D.

Theorem 1 provides a complete ranking of the coordination success rates among the three mechanisms of our interest, and shows that the Gradualism mechanism is superior than the other two in terms of promoting successful coordination at the final high-stake periods.[15]

5. Main Hypotheses in Both Stages

Based on theoretical predictions in Theorem 1, we would like to directly test how these mechanisms perform in the laboratory environment, which is formerly stated in the following hypothesis.

Hypothesis 1: In Stage 1, the Gradualism treatment outperforms the Semi-Gradualism treatment and the Semi-Gradualism treatment outperforms the Big Bang treatments in the high-stake projects.

When subjects enter the second stage, they may have learned about group members via coordination outcomes in the first stage and form their beliefs about the general population regarding the contribution tendency. The coordination performances in the first stage of the game can influence how they believe about new group members and how they play in the second stage.[16] Given that the Gradualism treatment

[15] We also conducted comparative statics analysis for the impacts of the number of players, the size of multiplier, and the type of belief functions on the coordination success rate. We were also able to show that given strictly increasing belief functions, there always exists a gradualism mechanism where the stake (not necessarily evenly) increases overtime such that the success of coordination is achieved with certainty. All these results are included in the supplementary material S3.

[16] Other studies provide evidence that history can influence subsequent behavior. In a two-stage trust game, Bohnet and Huck (2004) find that once players get to experience a cooperative environment in the first stage of a game, they

may promote more successful group coordination (relative to other treatments) in the first stage, we propose that conditional on being placed in the Gradualism treatment during the first stage, players will be more likely to contribute (relative to players from other treatments) when they enter the second stage of the game. Alternatively, even players may not necessarily form their beliefs about new group members based on their coordination history with old members, reinforcement learning (e.g., Roth & Erev, 1995) can induce those with a coordination success (or failure) at the end of Stage 1 to tend to (or not to) contribute when they enter the first period of Stage 2. Thus, we have the second hypothesis below:

Hypothesis 2: Those treated in the Gradualism treatment in Stage 1 are more likely to contribute in the first period of Stage 2 than those treated in other treatments. The higher success rate in coordination of Gradualism treatment (relative to other treatments) at the end of Stage 1 drives this result.

6. Results: Impact of Gradualism on Coordination

This section presents our results of coordination outcomes. We begin by focusing our analysis on the following three outcome variables per period: (1) whether a group coordinates successfully (defined as whether all four group members contribute) or not, (2) whether an individual contributes or not, and (3) payoff of each individual.

6.1 First Stage Result Highlights

To test Hypothesis 1, we examine the performances of various treatments in Periods 7–12 of the first stage, when all treatments face the same high stake, which is the main interest of this study.

Main Result 1: The Gradualism treatment significantly outperforms alternative treatments: Starting at a low stake and growing slowly lead to more successful coordination and higher earnings in the high-stake periods.

Figure 2 shows the success rate by treatment. In Period 7, 66.7% of Gradualism groups coordinate successfully (i.e., all four group members contribute), whereas the success rates of Big Bang, Semi-Gradualism, and High Show-up Fee groups are only 16.7%, 33.3%, and 30%, respectively. Figure 2 illustrates that the success rates for all the treatments remain stable from Period 7 to Period 12. We use the average success rate over Periods 7–12 for each group as one observation, so each group has one observation. The differences in average success rates over Periods 7–12 between the Gradualism treatment and Bing Bang, Semi-Gradualism, and High Show-up Fee treatments are all statistically significant

become more trusting (of others) in a new environment in the second stage.

(Wilcoxon-Mann-Whitney two-sided test: p<0.01, p=0.06, and p=0.09, respectively; observations are at the group level given that coordination success is a group-level outcome; N=64).[17]

[Figure 2 about here]

Figure 3 shows the average individual earning by treatment. Subjects in the Big Bang and High Show-up Fee groups have higher earning potentials (i.e., higher stakes) in Periods 1–6. However, on average, they earn less than the subjects in the Gradualism treatment because of the high success rates in the Gradualism treatment. The Semi-Gradualism groups earn less than the Gradualism treatment in Periods 2–6 because of the lower earning potential. These earning differences persist over Periods 7–12, when all the treatment groups experience the same high stakes. The differences in accumulative individual earnings over Periods 7–12 between Gradualism and Big Bang, Semi-Gradualism and High Show-up Fee treatments are all highly statistically significant (Wilcoxon-Mann-Whitney two-sided test for Period 7: p<0.0001, p<0.0001, and p=0.02, respectively when observations are at the individual level, N=256; p-value becomes 0.05, 0.04 and 0.38, respectively when observations are at the group level, N=64).[18] Clearly, from the perspective of social welfare, gradualism also works best.

[Figure 3 about here]

6.2 Wealth Effect Does Not Drive Main Result 1

To address the concern that diverse performances for Periods 7–12 may be caused by wealth effect (i.e., those treated in the Gradualism treatment may earn more in Periods 1–6, thus, they are more likely to contribute in Periods 7–12). Table 1 provides the summary of individual earnings accumulated over Periods 1–6 (excluding the show-up fee) for each treatment. On average, subjects in the Gradualism treatment earn the most through the first six periods, and the average (median) accumulated earnings from Period 1 to Period 6 are 112.42 (106) points for the Big Bang, 126.31 (130) points for the Semi-Gradualism, and 143.94 (162) points for the Gradualism treatment. However, the differences in means (and medians) across treatments are dramatically smaller than 80 points (the difference in the show-up fee between the High Show-up Fee treatment and the other three treatments). This result demonstrates that a show-up fee difference of 80 points between the Big Bang and High Show-up Fee treatments is sufficient to capture the potential income differences at the beginning of Period 7 among Big Bang, Semi-Gradualism, and Gradualism treatments. In fact, when we add the show-up fee, the subjects in the Gradualism treatment, on average, earn less than the subjects in the High Show-up Fee treatment by the end of Period 6. Thus, because

[17] Success rate in the Big Bang treatment is insignificantly different from that in the Semi-Gradualism and High Show-up Fee treatments (Wilcoxon-Mann-Whitney two-sided test: p=0.18, p=0.42, respectively).

[18] Earning in the Big Bang treatment is insignificantly different from that in the Semi-Gradualism and High Show-up Fee treatments (Wilcoxon-Mann-Whitney two-sided test: p=0.72, p=0.37, respectively).

the Gradualism treatment results in better performance than the High Show-up Fee treatment in Periods 7–12 of Stage 1, a wealth effect from the first six periods cannot account for the difference in performance of the subsequent periods. Moreover, by randomly assigning subjects into treatment groups, thus balancing wealth levels (outside the laboratory) across treatments, we rule out the possibility that the differences in performance are due to the differences in individual wealth levels from the real world.

6.3 Coordination Dynamics in the First Stage

To identify why the Gradualism treatment performs best in Periods 7–12 than other treatments, we examine the coordination dynamics in Figures 2 and 4 and Figure S.1 in the supplementary materials. The three patterns presented below support Propositions 1–3 in the theoretical model, respectively.

Pattern 1: The lower the stake size, the higher the average contribution and success rates in Period 1.

Figure 4 displays contribution rates for Period 1. The average contribution rate is above 90% for Semi-Gradualism and Gradualism treatments with a low stake, which is much higher than the contribution rate of 60% for Big Bang and High Show-up Fee treatments with a high stake (Wilcoxon-Mann-Whitney two-sided test between these two categories: $p<0.0001$; observations are at the individual level).

[Figure 4 about here]

As shown in Figure 2, the differences in success rates are more evident. Over two-thirds of Semi-Gradualism and Gradualism groups coordinate successfully at the low initial stake, whereas only 16.6% (or 30%) of the Big Bang (or High Show-up Fee) groups succeed at the high initial stake (Wilcoxon-Mann-Whitney two-sided test between these two categories: $p<0.0001$; observations are at the group level). A weakest-link structure requires that all four group members contribute at the same time to make coordination a success, thus a difference in contribution rate may result in an even larger difference in success rate.[19]

We do not detect a wealth effect induced by different show-up fees: No difference is observed in the contribution rate between the Big Bang and High Show-up Fee treatments. The difference in success rate in Period 1 between Big Bang and High Show-up Fee treatments is completely attributed to random factors in group assignment rather than a wealth effect.[20]

[19] Assuming the probability of contributing is independent across members in a group (which is plausible in Period 1 because players are randomly assigned to groups and have not interacted with each other), the success rate should be the biquadrate of the contribution rate. As long as the contribution rates are sufficiently high, the difference in the success rate exceeds that in the contribution rate.

[20] As mentioned in Section 3, since the effect of show-up fee could be different from that of the performance earning, the lack of a wealth effect from the show-up fee cannot completely rule out the possibility that a wealth effect remains associated with performance earnings accumulated across Periods 1–6.

Pattern 2: Conditional on having failed coordination in Period *t*, most groups fail at the same or a higher stake in Period *t*+1; Conditional on successfully coordinating in Period *t*, most groups succeed at the same stake in Period *t*+1.

Figure S.1 in the supplementary materials details coordination for groups across periods by treatment. A group in a given period succeeds in coordination if and only if the number of contributors (i.e., the vertical axis) equals four. Figure S.1 shows that once a group fails to coordinate, it rarely becomes successful thereafter.[21] This pattern is likely due to players obtaining limited information feedback regarding the group outcome each period: Each member does not know how many group members contribute or not if the group fails in coordination. The above findings are consistent with the coordination literature, which has discovered that a group reaching an inefficient outcome is not likely to subsequently achieve a more efficient outcome with limited information feedback and without further mechanisms.

Similarly, once a group succeeds, it almost always remains successful at the same stake.[22]

Pattern 3: Conditional on successfully coordinating in Period *t*, most groups succeed at a slightly higher stake in Period *t*+1. However, fewer groups remain successful at a much higher stake in Period *t*+1.

Figure S.1 in the supplementary materials shows most successful groups of the Gradualism treatment in the first period keep successful when the stake increases slowly before it reaches the highest stake in Period 7. We note a large gap in success rates between groups in the Gradualism treatment and those in the Semi-Gradualism treatment in Period 7, when the stake jumps from 2 to 14 for the latter treatment. Both treatments exhibit high success rates of approximately 70% for the first six periods. However, the success rate of the Semi-Gradualism treatment falls dramatically to only 33.3% in Period 7, whereas that of the Gradualism treatment remains at a high level of 66.7% (Wilcoxon-Mann-Whitney two-sided test between these two treatments in Period 7: $p<0.05$; observations are at the group level).

The results of Semi-Gradualism and Gradualism treatments in Periods 6 and 7 confirm the importance of increasing the stake slowly and avoiding shocks to the stake size. In fact, in Period 7 the contribution rate of the Gradualism treatment is only about five percentage points higher than that of the Semi-Gradualism treatment (see Figure 4). Why does the success rate of the Semi-Gradualism treatment drops sharply from Period 6 to Period 7, whereas that of the Gradualism treatment remains high? Notably, a high contribution rate does not guarantee a high success rate because the latter requires that most groups have all four group members contributing at the same time. For the Semi-Gradualism treatment, a moderate 15

[21] Only six exceptions (one Big Bang group: group No.101; five Semi-Gradualism groups: groups No. 112, 132, 142, 172, 182) out of 64 groups exist.

[22] Only seven exceptions (one Big Bang group: group No.101; four Semi-Gradualism groups: groups No. 82, 112, 132, 142; and two Gradualism groups: groups No. 33, 173) exist.

percentage point decrease in the contribution rate from Period 6 to Period 7 translates to a sharp 40 percentage point drop in the success rate, suggesting that a large portion of subjects who give up contributing in Period 7 comes from previously successful groups: An unanticipated big jump in the stake causes some of the subjects in the previously successful groups to be unwilling to continue contributing. Previously successful coordination established at low-stake periods is sabotaged even if only one of the four group members stops contributing. Figure S.1 confirms this.[23] Thus, avoiding shocks can help overcome the coordination difficulty and remain coordination success, which is exactly why the Gradualism treatment outperforms the Semi-Gradualism treatment.

Driven by Patterns 2 and 3, the success rates are quite stable over periods except a drop from Period 6 to Period 7 for the Semi-Gradualism treatment. Conditional on successfully coordinating in Period 1, Big Bang, Gradualism, and High Show-up Fee groups usually remain successful in subsequent periods. Groups in the Big Bang and High Show-up Fee treatments exhibit lower success rates in Period 1 than groups in the Gradualism treatment and therefore, on average, they perform worse than groups in Gradualism treatment at a high-stake level.

6.4 Second Stage Result Highlights

In Table 2, we test Hypothesis 2 and examine whether the treatment type in the first stage influences individual behavior and outcomes in the second stage when subjects are placed in a new group.

[Table 2 about here]

Main Result 2: Subjects exposed to the Gradualism treatment in Stage 1 are more likely to contribute upon entering a new group in Stage 2 than subjects who are previously exposed to other treatments. However, this difference quickly disappears.

We pool all the three non-Gradualism treatments together (Big Bang, Semi-Gradualism, and High Show-up Fee) and define them as the default category. Therefore, the coefficients on the Gradualism variable indicate the difference between the Gradualism treatment and the other three treatments lumped together.[24]

Interestingly, Column 1 shows that the subjects in the Gradualism treatment are 12.2 percentage points (86.1 vs. 73.9; $p<0.05$) more likely to contribute in the first period of Stage 2.

Considering that the Gradualism treatment has a higher success rate in the last period of the first stage, in the second part of Hypothesis 2 we propose that the higher contribution rate at the beginning of Stage 2

[23] Among the eight Semi-Gradualism groups in which the number of contributors decreases from Period 6 to 7, seven groups (groups No. 32, 52, 92, 112, 142, 172 and 182) are successful in Period 6 but fail in Period 7 due to one or two "betrayers" in each group, whereas only one group (group No. 82) already fails in Period 6.

[24] When we separate the other three treatments, the results (available upon request) are similar, and we do not detect significant differences among the other three treatments.

of the subjects exposed to this treatment is the result of the higher success rate of the previous period (i.e., the last period of Stage 1). To test this, we regress the contribution decision in the first period of Stage 2 on the coordination result in the last period of Stage 1 in Column 2. The result confirms our hypothesis: For subjects who fail to coordinate in the last period of Stage 1, about two-thirds contribute in the first period of Stage 2, whereas all the subjects belonging to a successful group in the last period of Stage 1 contribute in the first period of Stage 2. The difference in the contribution rate in Period 1 of Stage 2 by success type in the last period of Stage 1 is 35.4 percentage points and highly significant. In Column 3, we control for the treatment dummy. The coefficient on the variable "success in previous period" does not change, whereas the coefficient on the treatment dummy becomes almost zero and statistically insignificant. This result suggests that the treatment regime in the first stage of the experiment influences the contribution rate in the first period of Stage 2, mostly through the success rate in the last period of Stage 1.

However, the higher contribution rate at the beginning of Stage 2 of those Gradualism subjects translates into neither a higher success rate (Column 4) nor higher average earnings (Column 5). On average, Gradualism subjects earn 1.5 points less than subjects from other treatments (the difference is statistically insignificant.) As a direct consequence, the contribution rate of Gradualism subjects decreases faster than that of other subjects from Period 1 to Period 2 of Stage 2, and becomes comparable to that of other subjects in Period 2 and throughout Stage 2 (shown in Columns 6 and 7). This convergence of behavior suggests a possible learning process that after observing the coordination outcome in Period 1 of Stage 2, the subjects from the Gradualism treatment realize that their new group partners are not as likely to contribute as the ones they have partnered with in the first stage of the game; therefore, they become less willing to contribute in subsequent periods.[25] This learning behavior suggests an externality of coordination building (or collapse) across different social groups.

7. Belief Elicitation Experiment: Evidence of Belief-based Learning

In order to further explore whether the belief-based learning model in Section 4 is indeed one underlying reason why gradualism works, we conducted a supplementary experiment at Xiamen University in May 2013, in which we adopted an identical design as the main experiment except that we elicited the belief of subjects about the action of others in each period (detailed instructions are available upon request.)[26]

[25] Our design suggests the role of individual learning when groups are reshuffled, which differs from the transfer of "shared history" across different games when fixed groups having experienced coordination success in a game play a different coordination game.

[26] Direct incentive-compatible belief elicitation has become increasingly popular in experimental economics (e.g., Offerman, Sonnemans & Schram, 1996; Nyarko & Schotter, 2002; Costa-Gomes & Weizsäcker, 2008; Rey-Biel, 2009; also see Schotter & Trevino, 2014 for a recent survey). We ended up not eliciting beliefs in the main experiment because we were concerned that belief elicitation would have contaminated our results. Such a concern is verified by Rutström and Wilcox (2009), and Gächter and Renner (2010).

We have 24 subjects in the elicitation experiment (two groups each for Big Bang, Semi-Gradualism and Gradualism treatments). These subjects are different from subjects in the aforementioned main experiment. After each period, we ask the belief of each subject about the number of contributors among the other three group members, N_C, in his group (note that the group size is four). We require each subject to report four probability values for the cases that N_C equals 0, 1, 2, and 3, respectively. These four probability values should range from 0 to 100 (included) and sum to 100. To provide incentives for the subjects to report their true beliefs, we reward a subject 5 points for that period if he/she assigns his highest reported probability value to N_C.[27,28] In case of a tie (i.e., a subject assigns the same probability to n different numbers from 0 to 3 and one of the n numbers equals N_C), he/she receives the reward with a probability of $1/n$.[29] Among the four probability values, the probability that N_C equals three is the most important and relevant because of the weakest-link payoff structure; we divide this probability by 100 and code it as the belief that all other three group members contribute, which ranges from 0 to 1.

We check whether the decisions of the subjects are consistent with their beliefs in each period. Then, we examine whether they have updated their beliefs based on the feedback after each period. We present the results in Tables 3 and 4, respectively. Considering that estimating the treatment effect is not the purpose of the supplementary experiment, we pool all the treatments and all periods in both stages.[30]

Table 3 shows how the belief that all other three group members contribute in a period affects the contribution decision of the subjects in that period. Standard errors are clustered at the individual level. We pool all the 24 subjects in three treatments and all the 20 periods in two stages, and gain a total of 480 observations. We report OLS results for easier interpretation. We also conduct probit and logit regressions given that the contribution variable is binary, which leads to similar results (available upon request). Column 1 shows that both the belief and the stake matter, which are consistent with our theoretical predictions: The higher the belief that all other three contribute in a period, the higher the probability that

[27] For instance, a subject reports that the probabilities that N_C equals 0, 1, 2, and 3 are 10%, 20%, 40%, and 30%, respectively. This suggests that this subject believes that N_C is most likely to be 2 because 40%>30%>20%>10%. Then, if and only if it turns out that N_C equals 2, his guess is considered "right" and we reward him/her 5 points for that period.

[28] Among the four probability values a subject reports, only the highest probability determines rewards and is thus incentive-compatible. We choose to elicit all four probabilities because a full set of probabilities provide richer information. At the least, subjects have the incentive to report the most possible number of contributors. Thus, our method can elicit the same belief as the way of asking the subjects what is the most possible number of contributors. Although the other three probabilities we elicited is not incentive compatible and could be noisy, it provides more information. Thus, we have more freedom for data analysis and can decide how to use the incentive-compatible and incentive-incompatible data.

[29] For instance, if a subject reports that the probabilities that N_C equals 0, 1, 2 and 3 are all 25%, then this subject receives the reward with a probability of 1/4 no matter how many group members contribute. In another example, a subject reports that the probabilities that N_C equals 0, 1, 2 and 3 are 10%, 30%, 30% and 30%, respectively; then he/she receives the reward with a probability of 1/3 if and only if N_C equals 1, 2 or 3.

[30] Since the groups are reshuffled when subjects enter the second stage, we also separately estimate the regression results for the two stages and have similar results (available upon request).

the subject will contribute; the higher the stake, the lower the probability that the subject will contribute. The coefficient on belief is close to 1, which indicates a dominant role of belief in contribution decisions. Controlling for subject fixed effects in Column 2 and period fixed effects in Column 3 does not change the substantive results in Column 1, although the coefficient on stake becomes almost zero and insignificant when period fixed effects are controlled. In Column 4, we consider the potential impact of the contribution decision of the subjects in the previous period. In Column 5, we further consider the potential impact of the coordination outcome (i.e., success or failure) of the subject's group in the previous period, although its effect may have been captured by the updated belief that all the other three group members will contribute in current period. Considering that these two specifications use lagged variables, we have a slightly small number of observations. The results with these two lagged controls do not change the substantive results as regards the effect of the belief. Thus, we can conclude that the contribution decisions of the subjects in each period are indeed largely affected by their beliefs that all the other three group members contribute in that period.

Table 4 shows the belief formation and updating process. Column 1 examines the initial belief that all the other three group members contribute in Period 1. A higher stake in Period 1 leads to a smaller belief that all other three group members contribute, although the coefficient is statistically insignificant (note that we only have 24 observations). In an unreported regression, we replace the stake with a Big Bang dummy as the dependent variable and have obtained similar results. Columns 2–5 examine the belief updating process. In Column 2 we include the belief and the coordination result in the previous period, and both have a large effect on current belief, which shows that belief indeed updates based on the previous belief and the coordination outcome. When we further include the increase in stake from the previous period to the current period in Column 3, the coefficients on lagged belief and lagged coordination outcome are basically unchanged. In Columns 4, we control for the subject fixed effects. In Column 5, we further control for period fixed effects. The inclusion of these fixed effects does not change the substantive results of the belief updating process.

To conclude, the supplementary experiment with belief elicitation shows that the subjects indeed make contribution decisions according to their beliefs, and belief updating involves the previous belief and the coordination outcome.[31] These results provide suggestive evidence for the belief-based learning model, although it is unnecessarily the only or best-fitted model. In addition, our evidence is only suggestive because the actual decision is possibly independent of beliefs, and that the beliefs are ex-post rationalizations when they are elicited.

[31] We also applied an Arellano-Bond linear, dynamic panel-data estimation method (Arellano & Bond, 1991). Results are qualitatively similar and available upon request.

8. Conclusion and Discussion

The current study analyzes the effect of gradualism, defined as increasing step-by-step the stake level required for group coordination projects, on successful group coordination using data from a randomized laboratory experiment. No previous study has identified what pattern of successively and exogenously set stake levels yields coordination most successfully among individuals. Via a laboratory experiment, we find strong evidence that gradualism can serve as a powerful mechanism in achieving socially optimal outcomes in group coordination. Gradualism significantly outperforms alternative paths, which shows that starting at a low stake level and slowly growing the stake size are both important for coordination at later periods and at a substantial stake level.

We propose a belief-based learning model as one underlying channel that explains why gradualism works. The results from a supplementary experiment with belief elicitation support this explanation although we have not ruled out the possibility that other reasons, together with belief-based learning, may also lead to the results.

The two key features of the gradualism mechanism in the current study are as follows: the binary choice set in each period and the gradual increase in the stake of projects. First, the subjects are restricted to two choices in each period (but without mandatory or semi-mandatory institutions, such as sanctions and social pressures): to contribute the exact stake size or not to contribute at all. The binary choice set prevents players from contributing too little to harm others and from contributing too much to get hurt, thus serving as an effective coordination device. Second, a small initial stake encourages players to contribute at the beginning, whereas a gradual path of the stake maintains the high willingness of the players to contribute even when the stake becomes substantial.

We also find an externality of coordination building (or collapse) across different social groups. Subjects treated in the gradualism setting are more likely to contribute upon entering a new group than subjects treated differently. However, when these gradualism subjects find that their contributions have not been rewarded in the new groups, they tend to contribute less as well. Thus, the contribution behavior of the subjects treated in the gradualism regime and alternative regimes converges quickly. This result may have policy implications for ensuring efficient coordination outcomes when society members from diverse cultures and institutions merge.

The results of the current study may have policy implications beyond the particular case of building coordination within small groups. Given that we quantify large positive effects of gradualism on successful coordination, our results can be used to inform policies on how to promote coordination among individuals, organizations, regions, and countries.

References

Arellano, M., & Bond, S. (1991). Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations. *Review of Economic Studies, 58(2)*, 277-297.

Andreoni, J., & Samuelson, L. (2006). Building rational cooperation. *Journal of Economic Theory, 127(1),* 117-154.

Arrow, K. J. (1974). *The limits of organization.* New York: W. W. Norton & Company.

Battalio, R., Samuelson, L., Huyck, J. V. (2001). Optimization Incentives and coordination failure in laboratory stag hunt games. *Econometrica, 69*(3), 749-764.

Berninghaus, S. K., & Ehrhart, K. M. (1998). Time horizon and equilibrium selection in tacit coordination games: Experimental results. *Journal of Economic Behavior and Organization, 37(2),* 231-248.

Berninghaus, S. K., & Ehrhart, K. M. (2001). Coordination and information: Recent experimental evidence. *Economics Letters, 73(3),* 345-351.

Bohnet, I., & Huck, S. (2004). Repetition and reputation: Implications for trust and trustworthiness when institutions change. *American Economic Review, 94(2),* 362-366.

Bornstein, G., Gneezy, U., & Nagel, R. (2002). The effect of intergroup competition on group coordination: An experimental study. *Games and Economic Behavior, 41(1),* 1-25.

Brandts, J., & Cooper, D. J. (2006a). Observability and overcoming coordination failure in organizations. *Experimental Economics, 9(4),* 407–423.

Brandts, J., & Cooper, D. J. (2006b). A change would do you good.... An experimental study on how to overcome coordination failure in organizations. *American Economic Review, 96(3),* 669-693.

Cachon, G. P., & Camerer, C. F. (1996). Loss-avoidance and forward induction in experimental coordination games. *Quarterly Journal of Economics, 111(1),* 165-194.

Camerer, C., & Ho, T. H. (1998). Experience-weighted attraction learning in coordination games: Probability rules, heterogeneity, and time-variation. *Journal of Mathematical Psychology*, *42*(2), 305-326.

Camerer, C., & Ho, T. H. (1999). Experience weighted attraction learning in normal form Games. *Econometrica, 67(4),* 827-874.

Charness, G. (2000). Self-serving cheap talk: A test of Aumann's conjecture. *Games and Economic Behavior, 33(2),* 177-194.

Chaudhuri, A., Schotter, A., & Sopher, B. (2009). Talking ourselves to efficiency: Coordination in inter-generational minimum effort games with private, almost common and common knowledge of advice. *Economic Journal, 119(534),* 91-122.

Chen, R., & Chen, Y. (2011). The potential of social identity for equilibrium selection. *American Economic Review*, *101 (6),* 2562-2589.

Chen, Y., Li, S. X., Liu, T. X., & Shih, M. (2014). Which hat to wear? Impact of natural identities on coordination and cooperation. *Games and Economic Behavior*, *84*, 58-86.

Choi, S., Gale, D., & Kariv, S. (2008). Sequential equilibrium in monotone games: A theory-based analysis of experimental data. *Journal of Economic Theory, 143(1),* 302-330.

Cooper, R., DeJong, D. V., Forsythe, R., & Ross, T. W. (1992). Communication in coordination games. *Quarterly Journal of Economics, 107(2),* 739–771.

Costa-Gomes, M. A., Crawford, V. P., & Broseta, B. (2001). Cognition and behavior in normal-form games: An experimental study. *Econometrica, 69(5),* 1193-1235.

Costa-Gomes, M. A., & Crawford, V. P. (2006). Cognition and behavior in two-person guessing games: An experimental study. *American Economic Review, 96(5),* 1737-1768.

Costa-Gomes, M. A., & Weizsäcker, G. (2008). Stated beliefs and play in normal-form games. *Review of Economic Studies, 75(3),* 729-762.

Costa-Gomes, M. A., Crawford, V. P., & Iriberri, N. (2009). Comparing models of strategic thinking in Van Huyck, Battalio, and Beil's coordination games. *Journal of European Economic Association*, *7*(2-3), 365-376.

Crawford, V. P. (1995). Adaptive dynamics in coordination games. *Econometrica, 63(1),* 103-143.

Crawford, V. P., & Iriberri, N. (2007). Level-k auctions: Can a non-equilibrium model of strategic thinking explain the winner's curse and overbidding in private-value auctions? *Econometrica*, *75(6),* 1721-1770.

Devetag, G. (2003). Coordination and information in critical mass games: An experimental study. *Experimental Economics, 6*(1), 53–73.

Devetag, G. (2005). Precedent transfer in coordination games: An experiment. *Economics Letters, 89*(2), 227-232.

Devetag, G., & Ortmann, A. (2007). When and why? A critical survey on coordination failure in the laboratory. *Experimental Economics*, *10*(3), 331-344.

Dorsey, R. (1992). The voluntary contributions mechanism with real time revisions. *Public Choice, 73(3),* 261-282.

Duffy, J., & Feltovich, N. (2002). Do actions speak louder than words? Observation vs. cheap talk as coordination devices. *Games and Economic Behavior, 39(1),* 1–27.

Duffy, J., & Feltovich, N. (2006). Words, deeds, and lies: Strategic behavior in games with multiple signals. *Review of Economic Studies*, *73*(3), 669-688.

Duffy, J., Ochs, J., & Vesterlund, L. (2007). Giving little by little: Dynamic voluntary contribution games. *Journal of Public Economics, 91(9),* 1708-1730.

Feltovich, N. (2012). Belief-based learning models. In N. Seel (Eds.), *Encyclopedia of the sciences of learning*. Springer, U.S.A.

Feri, F., Irlenbusch, B., & Sutter, M. (2010). Efficiency gains from team-based coordination—Large-scale experimental evidence. *American Economic Review*, *100(4)*, 1892-1912.

Fischbacher, U. (2007). z–Tree: Zurich toolbox for ready–made economic experiments. *Experimental Economics, 10(2),* 171–178.

Fudenberg, D., & Levine, D. (1998). *Theory of learning in games*. Cambridge, MA: MIT Press.

Gächter, S., & Renner, E. (2010). The effects of (incentivized) belief elicitation in public goods experiments. *Experimental Economics*, *13*(3), 364-377.

Gale, D. (1995). Dynamic coordination games. *Economic Theory, 5(1),* 1-18.

Gale, D. (2001). Monotone games with positive spillovers. *Games and Economic Behavior, 37(2),* 295-320.

Goeree, J. K., & Holt, C. A. (2005). An experimental study of costly coordination. *Games and Economic Behavior*, *51*(2), 349-364.

Ho, T. H., Camerer, C., & Weigelt, K. (1998). Iterated dominance and iterated best response in experimental "p-beauty contests". *American Economic Review*, *88(4)*, 947-969.

Holt, C. A., & Laury, S. K. (2002). Risk aversion and incentive effects. *American Economic Review, 92(5),* 1644-1655.

Knez, M., & Camerer, C. F. (1994). Creating expectational assets in the laboratory: Coordination in weakest-link games. *Strategic Management Journal, 15(8),* 101-119.

Knez, M., & Camerer, C. F. (2000). Increasing cooperation in prisoner's dilemmas by establishing a precedent of efficiency in coordination games. *Organizational Behavior and Human Decision Processes, 82(2),* 194-216.

Kurzban, R., McCabe, K., Smith, V. L., & Wilson, B. (2001). Incremental commitment and reciprocity in a real-time public goods game. *Personality & Social Psychology Bulletin, 27(12),* 1662-1673.

Lockwood, B., & Thomas, J. P. (2002). Gradualism and irreversibility. *Review of Economic Studies, 69(2),* 339-356.

Marx, L., & Matthews, S. (2000). A dynamic voluntary contribution to a pubic project. *Review of Economic Studies, 67(2),* 327-358.

Nagel, R. (1995). Unraveling in guessing games: An experimental study. *American Economic Review*, *85(5)*, 1313-1326.

Nyarko, Y., & Schotter, A. (2002). An experimental study of belief learning using elicited beliefs. *Econometrica, 70(3),* 971-1005.

Offerman, T., Sonnemans, J., & Schram, A. (1996). Value orientations, expectations and voluntary contributions in public goods. *Economic Journal, 106 (437), 817*-845.

Offerman, T., & van der Veen, A. (2015). How to subsidize contributions to public goods: Does the frog jump out of the boiling water? *European Economic Review, 74*, 96-108.

Oprea, R., Charness, G., & Friedman, D. (2014). Continuous time and communication in a public-goods experiment. *Journal of Economic Behavior & Organization, 108*, 212-223.

Rey-Biel, P. (2009). Equilibrium play and best response to (stated) beliefs in normal form games. *Games and Economic Behavior, 65(2),* 572-585.

Riechmann, T., & Weimann J. (2008). Competition as a coordination device: Experimental evidence from a minimum effort coordination game. *European Journal of Political Economy, 24(2),* 437-454.

Riedl, A., Rohde, I., & Strobel, M. (2016). Efficient coordination in weakest-link games. *Review of Economic Studies, 83*(2), 737-767.

Romero, J. (2015). The effect of hysteresis on equilibrium selection in coordination games. *Journal of Economic Behavior & Organization, 111*, 88-105.

Roth, A. E., & Erev, I. (1995). Learning in extensive-form games: Experimental data and simple dynamic models in the intermediate term. *Games and Economic Behavior*, *8(1),* 164-212.

Rutström, E. E., & Wilcox, N. T. (2009). Stated beliefs versus inferred beliefs: A methodological inquiry and experimental test. *Games and Economic Behavior, 67(2),* 616-632.

Salmon, T. C., & Weber, R. A. (2017) Maintaining efficiency while integrating entrants from lower performing groups: an experimental study. *Economic Journal, 127*(600), 417–444.

Schelling, T. C. (1960). *The strategy of conflict.* Cambridge, MA: Harvard University Press.

Schotter, A., & Trevino, I. (2014). Belief elicitation in the laboratory. *Annual Review of Economics*, *6*, 103-128.

Stahl, D. O., & Wilson, P. W. (1995). On players' models of other players: Theory and experimental evidence. *Games and Economic Behavior*, *10*(1), 218-254.

Van Huyck, J. B., Battalio, R. C., & Beil, R. O. (1990). Tacit coordination games, strategic uncertainty, and coordination failure. *American Economic Review, 80(1),* 234-248.

Watson, J. (1999). Starting small and renegotiation. *Journal of Economic Theory, 85(1),* 52-90.

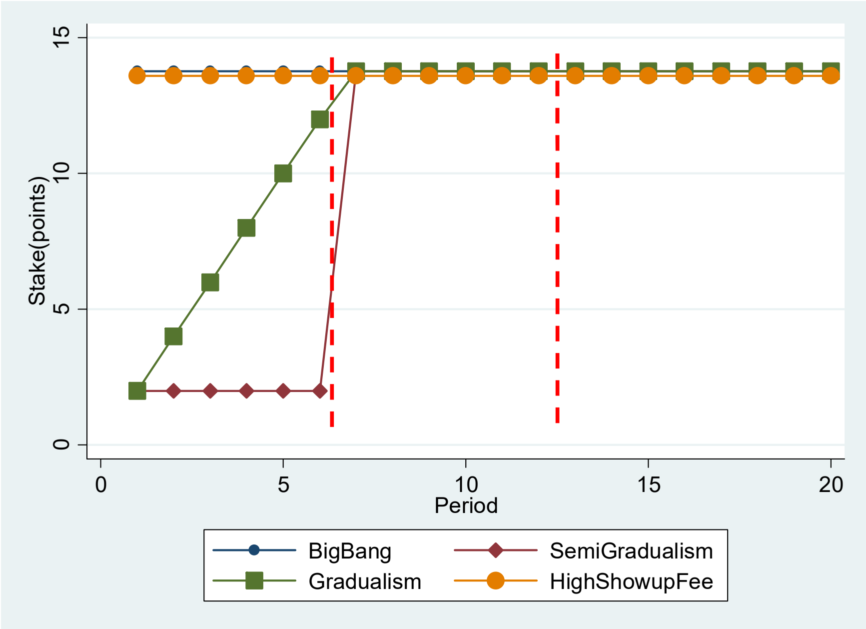
Watson, J. (2002). Starting small and commitment. *Games and Economic Behavior, 38(1),* 176-199.

Weber, R. A., Camerer, C., Rottenstreich, Y., & Knez, M. (2001). The illusion of leadership: Misattribution of cause in coordination games. *Organization Science, 12(5),* 582-598.

Weber, R. A. (2006). Managing growth to achieve efficient coordination in large groups. *American Economic Review, 96(1),* 114-126.

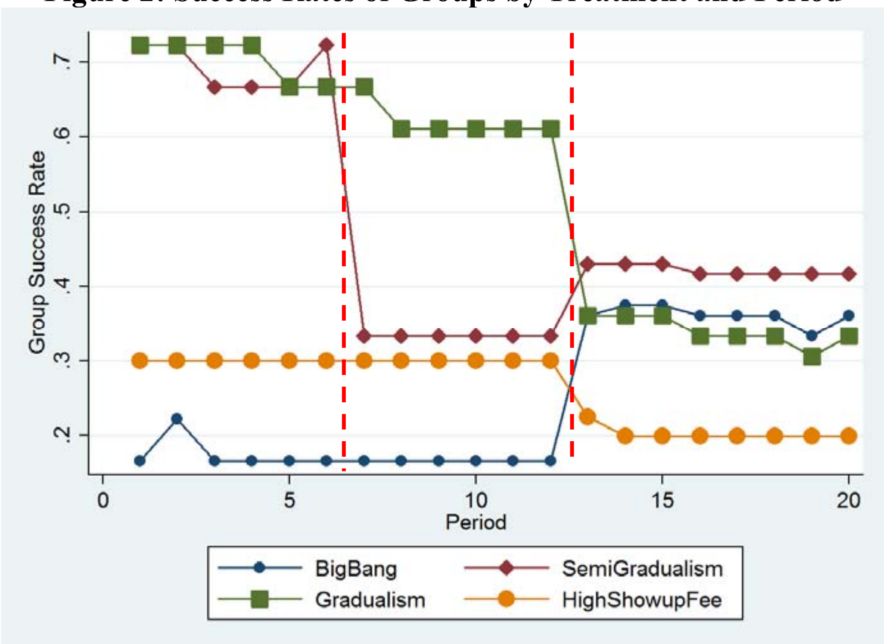
Yang, C., Xu, M., M, J., & Tang, F. (2017). Efficient large-size coordination via voluntary group formation: An experiment. *International Economic Review, 58*(2), 651–668.

Figure 1: Stake Patterns of the Treatments



Note: The first and second vertical dotted lines separate the two halves (Periods 1–6 and 7–12) of the first stage, and the two stages (Periods 1–12 and 13–20), respectively. Coordination performances of different treatments in the second half (Periods 7-12) of the first stage are the main interests of this study. The High Show-up Fee treatment is identical to the Big Bang treatment except for a higher show-up fee (see Table 1 for details). Group members are fixed within each stage, whereas subjects from various treatments are reshuffled when they enter the second stage.

Figure 2: Success Rates of Groups by Treatment and Period



Note: A group coordinates successfully if all four members contribute the stake in that period.

Figure 3: Average Individual Earning by Treatment and Period

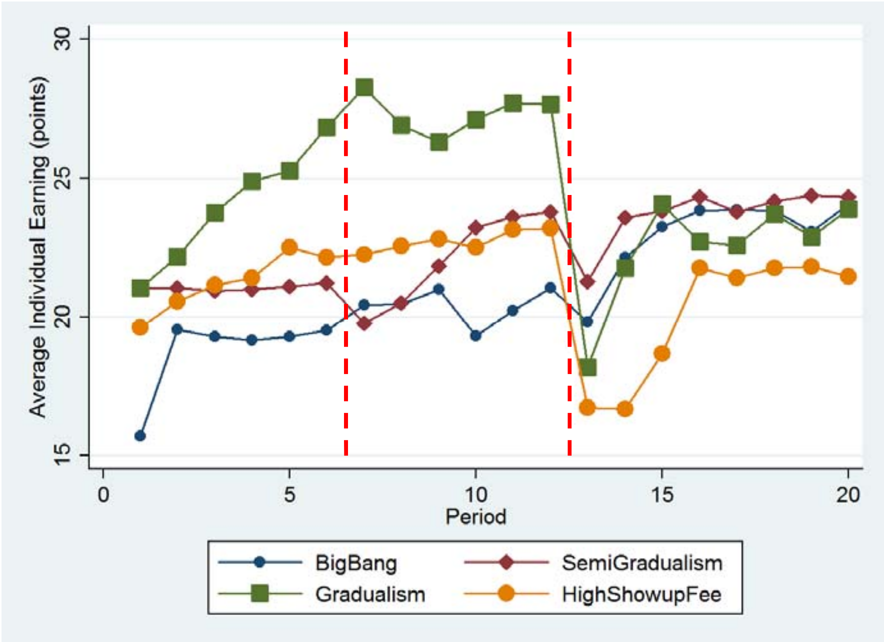


Figure 4: Contribution Rate by Treatment and Period

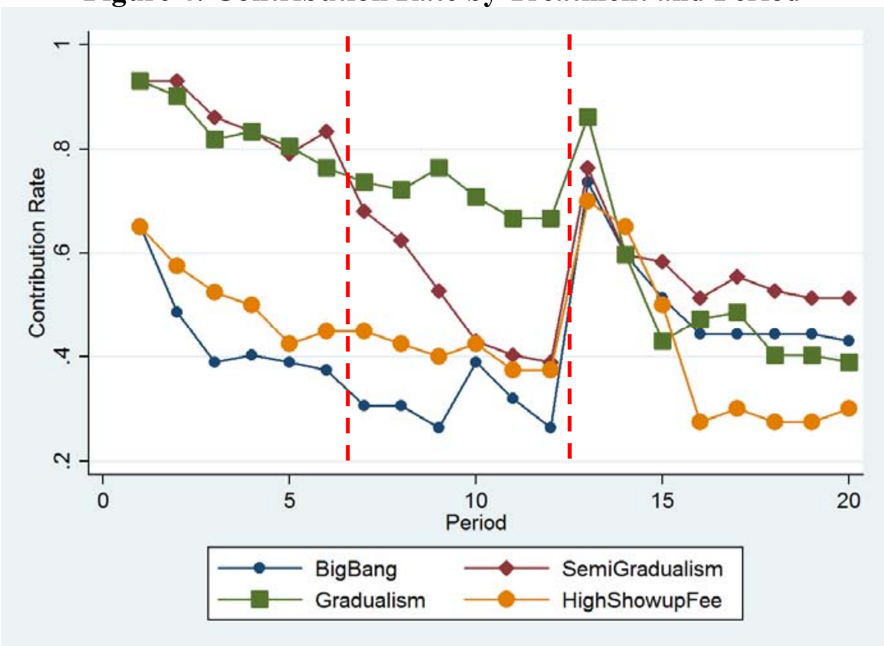


Table 1: Summary of Treatments in the First Stage

Treatment	Big Bang	Semi-Gradualism	Gradualism	High Show-up Fee
Endowment in each period (points)	20	20	20	20
Show up Fee (points)	400	400	400	480
Exchange Rate (points/CNY)	40	40	40	40
Stake in Period 1 (points)	14	2	2	14
Stake in Period 6 (points)	14	2	12	14
Stake in Period 7-12 (points)	14	14	14	14
Number of groups	18	18	18	10
Number of subjects	72	72	72	40
Average earning up to Period 6 (points; excluding show-up fee)	112.42	126.31	143.94	127.35
Median earning up to Period 6 (points; excluding show-up fee)	106	130	162	106

Note: We have 18 sessions in total. Eight sessions have 16 subjects for each session who are randomized into the above four treatments, and the other 10 sessions have 12 subjects for each session who are randomized into the Big Bang, Semi-Gradualism, and Gradualism treatments.

Table 2: Contribution and Earnings in Each Period of the Second Stage by Treatment

	Period 1					Period 2	the Whole Second Stage
	Contribution	Contribution	Contribution	Success	Earning	Contribution	Contribution
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Gradualism	0.122**		-0.002	0.002	-1.540	-0.012	-0.006
	(0.052)		(0.026)	(0.043)	(1.420)	(0.060)	(0.029)
Success in previous period		0.354***	0.354***				
		(0.035)	(0.034)				
Constant	0.739***	0.646***	0.647***	0.359***	19.710***	0.609***	0.511***
	(0.034)	(0.035)	(0.036)	(0.068)	(1.568)	(0.049)	(0.054)
Observations	256	256	256	256	256	256	2,048
R-squared	0.017	0.164	0.164	0.000	0.003	0.000	0.000

Note: OLS regression results are reported. The default category is three non-Gradualism treatments (Big Bang, Semi-Gradualism and High Show-up Fee) all together; when we separate these three treatments, the results (available upon request) are similar and we do not detect significant differences among these three treatments. Robust standard errors in parentheses. Standard errors are all clustered at the session level. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: The Effect of Belief on Contribution Decisions in Belief Elicitation Sessions (OLS)

	Contribution Dummy				
	(1)	(2)	(3)	(4)	(5)
Belief that all other three contribute	0.916***	0.847***	0.841***	0.723***	0.518***
	(0.050)	(0.069)	(0.067)	(0.082)	(0.106)
Stake	-0.012**	-0.011**	0.001	0.001	0.006
	(0.005)	(0.005)	(0.007)	(0.007)	(0.006)
Subject fixed effects	N	Y	Y	Y	Y
Period fixed effects	N	N	Y	Y	Y
Lagged contribution dummy				0.213**	0.106
				(0.096)	(0.080)
Lagged success dummy					0.318**
					(0.116)
Constant	0.291***	0.0871	0.131*	-0.0704	-0.0755
	(0.0729)	(0.0698)	(0.0758)	(0.0893)	(0.0817)
Observations	480	480	480	456	456
R-squared	0.669	0.731	0.762	0.786	0.801

Note: Robust standard errors in parentheses are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1. Contribution is a dummy indicating whether the subject contributes in current period. Belief ranges from 0 to 1 and represents the subject's belief that all other three group members contribute in current period. Stake refers to the stake in current period. Unreported probit and logit regressions have similar results.

Table 4: Belief Formation and Updating in Belief Elicitation Sessions (OLS)

	Belief that all other three group members contribute in current period				
	Period 1	Period 2-20 (Both Stages Together)			
	(1)	(2)	(3)	(4)	(5)
Lagged belief		0.372***	0.373***	0.260***	0.269***
		(0.070)	(0.0683)	(0.061)	(0.057)
Lagged success dummy		0.490***	0.488***	0.566***	0.561***
		(0.064)	(0.0623)	(0.066)	(0.062)
Stake	-0.011				
	(0.014)				
△Stake			0.002	0.001	0.000
			(0.015)	(0.015)	(0.016)
Subject fixed effects	N	N	N	Y	Y
Period fixed effects	N	N	N	N	Y
Constant	0.693***	0.061***	0.060***	0.102***	0.143***
	(0.094)	(0.016)	(0.014)	(0.008)	(0.042)
Observations	24	456	456	456	456
R-squared	0.032	0.777	0.777	0.808	0.818

Note: Robust standard errors in parentheses are clustered at the individual level. *** p<0.01, ** p<0.05, * p<0.1. Belief ranges from 0 to 1. Stake refers to the stake in current period. △Stake is the difference in stake between current period and the previous period.