ABSTRACT

Nudging Towards Socially Efficient Norms

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Within groups, members must coordinate on similar levels of effort in order to finish their tasks at the same time. In group coordination games with multiple Nash equilibria such as the minimum effort game (MEG), which equilibrium will prevail is difficult to predict. The MEG models group coordination by rewarding all players for higher group minimum effort levels, while punishing players for expending effort above the minimum effort of all group members. In most laboratory experiments involving the MEG, participants fail to coordinate on the Pareto-efficient Nash equilibrium. Here I design an experiment to show that 'nudge' periods in which coordination is facilitated are effective at reducing the amount of coordination failure. I include in the design a method to show that structuring these 'nudge' periods in continuous time (rather than discrete time) increases the persistence of successful coordination as continuous time provides subjects the opportunity to change strategies mid-period and thus reduce uncertainty. I advance previous research into group coordination by providing a design to test 'nudges' in continuous time games. My experiment is structured so that a period of induced efficient coordination will develop a focal equilibrium that persists even when the incentives to coordinate are weakened. I conduct power analyses to estimate the necessary group size.

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CHAPTER ONE

Introduction

A vital element of any successful group effort is coordination. Whether two friends who have both forgotten where they are meeting up for lunch must decide which restaurant to go to, or two drivers must decide to pass each other on the right side or the left side of the road, coordination is key for a successful interaction. Many of these problems may easily be solved by communication, as a quick text message between the two friends would settle which restaurant they should meet at (Cooper, 1989). In the absence of communication, however, other methods may be used to solve coordination issues. Established social norms may solve many of the coordination problems. However, an issue that arises from reliance on norms is that the optimal outcome need not coincide with the norm, as in the case of coordinated amounts of effort (Huyck, 1990). Norms may be established that result in outcomes that are less than socially optimal.

I study the effects of a 'nudge' on the equilibrium that is reached in a game involving group coordination. Potentially, if coordination can be artificially jump-started, repetition may cement a more efficient equilibrium into the social norm, rather than a less efficient equilibrium. To study this, I use the minimum effort game (MEG), which is a coordination game that captures the relationship between team members in a task where each member may contribute some amount of effort (at a cost to themselves), and the

group is rewarded in proportion to the minimum effort that was contributed in the group. Coordination in this game is difficult, as the option that reduces the risk of loss also produces the least amount of social welfare. Specifically, I conduct lab-based experiments with human subjects participating in a variation of the MEG. My aim is to determine whether 'nudges' in the form of focal points and/or a set of easier conditions for cooperation will have an effect on the long-run equilibrium that is reached.

Beyond the use of nudges in the laboratory, my study also examines the concept of time and risk. I implement the MEG in both discrete and continuous time in the laboratory. Continuous time games are games in which players interact continuously with other players, immediately observing the choices of other players and responding to them. Players payoffs are based on the entirety of the time in which they interact with other players, and thus their final decision is weighted the same as all other decisions made during the time period. I use continuous time in addition to discrete time to test the effectiveness of nudges based on group learning once the nudge is removed.

1.1 The Minimum Effort Game (MEG)

The minimum effort game (MEG) is a coordination game that models coordination when players must choose how much effort they wish to expend (which is costly) on a group task. As the success of the task is only dependent upon the lowest effort contributed by any player, there is a cost to expending more effort than that put forth by the person expending the lowest effort. Put in a concrete setting, consider six workers whose job is to clean the inside of a passenger plane between flights. As a quick turnaround time is valued by the airline company, the six workers jointly receive a bonus

based upon the time required to prepare the entire plane. Each worker may take between one minute and seven minutes to clean his or her section of the plane. All six workers are best off if they each perform their tasks in the shortest amount of time. However, if one worker takes two minutes to clean the plane, and the others take six minutes, the former worker is worse off than if he or she had taken six minutes to clean the plane, since there is no bonus unless the entire plane is cleaned quickly. The best off any worker can be is if that worker cleans the plane in the same amount of time as the slowest worker, and all the workers are better off if they clean the plane in the shortest amount of time, since the bonus is worth the extra amount of effort. In this type of interaction, nudges may be a solution to the coordination problem.

In a typical MEG in the laboratory, players are divided into groups of between four and sixteen players, and there are between ten and thirty turns during which each player chooses an effort level between one and seven (Hyuck, 1990; Brandts, 2006). Each turn, all players make some monetary payment based on how much effort they exerted, and receive a monetary payoff based upon the minimum effort exerted. There are seven Nash equilibria (where each player expends the same amount of effort), and they are Pareto-ranked from least socially efficient to most socially efficient (Hyuck, 1990). The least socially efficient Nash equilibrium occurs when all players expend the lowest possible effort; the most socially efficient Nash equilibrium occurs when all players expend the highest possible effort. Group interactions that involve a group benefit from the weakest link, the slowest player, or the minimum in some category may be modeled with the MEG.

For most studies, subjects begin with a low minimum effort and a high average effort, but the average quickly drops to the minimum possible effort after only a few periods, where it stays until the end of the game (Hyuck, 1990; Leng, 2018; Riedl, 2015). As a reflection of human ability to coordinate, this is not a promising signal that socially efficient equilibria will be reached through natural processes. Many researchers have found variations to the MEG in which efficient coordination may be reached. Previous research on the minimum effort game has focused on group size (Hyuck, 1990), the relative cost of effort (Goeree and Charles, 2005), group composition (Engelmann and Normann, 2010), and information available to group members (Leng, 2018), among other topics.

Hyuck (1990) is the first to address the MEG in the laboratory. Hyuck's treatment is thorough, and addresses questions of group size, increased information, and elimination of the cost of effort. Hyuck finds that groups struggle to coordinate on the efficient outcome while pairs of players usually succeed. By increasing the information available to subjects concerning the actions of other participants, Hyuck also induces higher minimum efforts. Lastly, Hyuck modifies the MEG in one treatment such that the cost of expending effort is zero, fundamentally changing the nature of the game. Even though expending the maximum effort becomes the dominant strategy, Hyuck finds that some subjects fail to follow standard game theoretic predictions. Following in Hyuck's steps, other scholars use the MEG to address other coordination problems. I build upon the basic structure laid out by Hyuck, but improve upon the foundation by changing the underlying time structure of the game and the payoff structure.

Goeree and Charles (2005) address problems of differing costs of effort, and their research is of particular relevance to this experiment. Whereas Hyuck fundamentally changed the Nash equilibria of the game in one of the treatments by removing any cost to exerting effort, Goeree and Charles maintain the structure of the MEG but change the magnitude of the cost of expending effort. They find that the lower the cost of expending effort, the higher effort levels are on average. Following in this area of inquiry, I vary the cost of expending effort as well in an attempt to increase effort levels, but I do so during the MEG rather than between MEG's. Although they use randomized pairing between partners, their findings indicate that altering the cost parameters may have an effect on MEG for groups.

The research of Engelmann and Norman (2010), although narrow in its focus, points to the effects of the composition of the subject pool. Their experiment was conducted in Denmark, and they found a connection between the percent of Danes in a group and the minimum effort. Namely, they conclude, Danish egalitarianism drives higher levels of coordination and cooperation in their experiments. In my pilot data I see similarly high levels of coordination on socially efficient outcomes, but as my experiment is run in a different social climate at a different time, I add to existing research by examining other social reasons for observing high levels of coordination.

Many other researchers use the MEG to study a variety of topics. Le Lec (2015) employs the MEG to examine the power of voluntary sanctions on increasing social welfare. Riedl (2015) adds a neighborhood choice phase to the MEG to examine the effects of allowing individuals to choose with whom they interact. In 2018 Leng studied

the effects of the time structure of the game and how information is used by players. The MEG is a flexible tool for studying a variety of social coordination problems.

In this paper I present my experimental design and predictions for a method of increasing efficient coordination in the MEG. This method is based on the concept of a 'nudge' presented by Thaler and Sunstein (2008). Our experiment implements a nudge in the minimum effort game by allowing subjects to practice coordination during several paid periods with parameters more conducive to coordination.

Other researchers have studied a similar nudge (although they do not label it as a nudge). Devetag (2005) examined the use of historical precedent to create efficient focal equilibria. Devetag finds that historical precedent from a different game with a related structure effectively reduced coordination failure and inefficient coordination in the MEG. The game that Devetag uses to establish precedent is not a MEG, reducing the applicability of Devetag's results to situations in which the structure cannot be altered. Hyuck (1990) uses a different game to establish precedent, and thus suffers from the same application problems as Devetag. Brandts (2006) has similar findings, except that Brandts uses a MEG with different parameters to develop an efficient precedent among subjects. Additionally, Brandts and Hyuck use a treatment in which the nudge period comes between two standard periods, whereas Devetag only examines the use of a nudge period before other periods of the MEG. In my experimental design, I choose to focus on nudges as interventions in a MEG, and thus follow a design similar to Brandts' design. However, I improve upon their design by increasing the number of periods after the nudge periods relative to the number of nudge periods; beyond this, I introduce nudges to

the continuous time environment, which is a step forward in the research related to nudges.

1.2 Nudges

Thaler and Sunstein study the effects of nudges on the behaviors of individuals, typically examining the format in which institutions present choices to individuals (Thaler and Sunstein, 2008). Thaler and Sunstein define a nudge to be a policy that leads people to a certain, predictable decision without reducing their set of choices. To understand what a nudge is, an example is helpful. Consider organ donor programs: in all states, organ donation is optional, but in some states residents must opt-in to the program, while in other states residents must opt-out of the program. Although the options are exactly the same, either to be an organ donor or not, and it is essentially costless to do either (there is no fine for not being an organ donor), states that have the opt-out program for organ donation have a significantly higher rate of organ donation than states that have an opt-in program (Thaler and Sunstein, 2008). Changing from an opt-in to an opt-out program is implementing a nudge, because in both cases the options are the same, but the people involved make different choices. Or consider an escalator which has footprints painted on each of the steps, two side by side on the right side of the step to indicate standing, and one footprint on the left side, alternating between the right foot and the left foot each step to indicate walking. These footprints in no way require that those who stand on the escalator stand on the right and those who walk up the escalator use the left side, but in practice people consciously (or subconsciously) obey the suggestion of these footprints. These are a nudge because the choices available to the

persons involved remain unchanged, yet their behavior is modified. A shove is a policy that brings about a change in behavior by making other behaviors more costly, like placing a high tax on non-organ donors or requiring a series of lengthy forms to be completed before opting-out is possible. Thaler and Sunstein support the use of nudges to promote social welfare, not the use of shoves.

It may be supposed that nudges work because people cannot "think like Albert Einstein, store as much memory as IBM's Big Blue, and exercise the willpower of Mahatma Gandhi" (Thaler and Sunstein, 2008). People are not perfect economic agents, and thus are often lazy when it comes to decision making. Whereas a perfect economic agent makes choices upon the merits of the various options, considering opportunity costs and marginal gains, real people often use heuristics in the decision making process and are therefore influenced by the order and type of choices presented to them (Kjær et al., 2006). A choice architect or policy maker may make use of these heuristics in designing choice structures and policies, and may help others by doing so.

In general, situations in which people do not have the opportunity to learn from their mistakes are prime targets for nudges. Although the MEG is a repeated game, the periods are not independent as coordination may be based upon previous equilibria. After only a few periods, participants may be committed to strategies and may be unable to reverse the direction of the game. Thus, unless a high level of coordination is present at the start of the game, coordination failure is likely to plague the entire duration. Any learning that takes place during the MEG will be too late to be helpful.

I implement a learning nudge in the MEG. In my design, there are 16 periods in a MEG. During periods 5 through 8, I change the cost of expending effort to one-tenth of the original cost, and increase the bonus from the group minimum effort by 5%. This nudge is focused on allowing players to learn to coordinate in an environment in which the cost of over-expending effort is minimized. Our goal with implementing this nudge is that players will learn to coordinate on socially optimal equilibria, and that this coordination will persist when the nudge period is terminated.

1.3 Continuous time

One powerful new tool that has recently been developed for studying human interaction in economic games in a laboratory setting is the implementation of continuous time interactions, rather than merely discrete time interactions. Conventionally, most games (including the MEG) are played in discrete time on a computer that is linked with the computers of other subjects playing in discrete time. Discrete time means that players independently make their decisions, submit them, and then wait for other players to make their decisions. Players either make decisions simultaneously (like in a game of Rock-Paper-Scissors) or in turns (like in a game of Chess). Time is discrete because all choices are considered to be made at the same time, or in sequence, and there are a finite number of turns. In continuous time games, however, there are no turns, but rather intervals of time. The players choose between their options, but can freely switch between options whenever they want during the period of play. Continuous time is similar to a 'king of the hill' contest in that the final state may not be important, but rather how long each player was on top of the hill. The game is always being played, and

flow payoffs are given. Flow payoffs are based on the total amount of time that a particular position exists, rather than upon choices at the end of the period.

For example, if Rock-Paper-Scissors was in continuous time, each player could toggle between Rock, Paper, or Scissors over some time period, say twenty seconds. Instead of choosing a winner based on the final condition of the game, a 'flow' payoff would give one point for each second that a player was in the winning position (i.e. playing Rock while the opponent played Scissors). The winner would then be determined based on who had the greater number of points. Fundamentally different models are needed to analyze behavior in this environment (Simon and Stinchcombe, 1989). Although the theory behind continuous time has been around for some time, the experimental implementation was difficult without sophisticated computer programs. Continuous time computer experiments began to appear in the literature around the mid 2000's, and in 2009 Daniel Friedman and Ryan Oprea received an NSF grant to design a program to easily allow experimenters to run continuous time experiments.[1]

Continuous time environments produce outcomes that are difficult to predict with standard theory. For the MEG, previous research has focused on the information available to players in a continuous time environment, and how that information affects the minimum level of effort observed (Deck, 2012; Leng, 2018). The research of Leng (2018) found the minimum level of effort significantly increased when continuous time and complete information of other players' choices was applied to the MEG. Continuous time allows participants to respond quickly to changes made by other players, and to act

[1] They received NSF grant SES-0925039. Although their software, ConG, is not widely used, the quantity of their work moved continuous time experiments from the fringe of experimental economics to a common laboratory tool.

on the additional information. Continuous time has other interactions than just with information, as in games where players only make one choice, such as in the war of attrition games of Oprea, Wilson, and Zillante (2013), or in the continuous prisoners' dilemma of Friedman and Oprea (2012). These interactions make predicting the effect of nudges on a continuous game difficult, and to my knowledge this is the first such experiment. As many situations in real life are more accurately described using continuous time, this method is appropriate to study many real-life phenomena.

What makes the MEG a particularly good candidate for a nudge in continuous time is that there is an added element of risk in the interaction. Whereas other coordination issues involve risk because coordination may not take place, the minimum effort game involves an additional risk because higher levels of effort may be undercut by players choosing to expend less effort (Anderson, 2001). This means that players would prefer to coordinate to the highest level of effort, but a concern for safety may cause them to expend lower levels of effort. In continuous time the risks incurred from not lowering effort levels are reduced, since players may respond to any changes nearly instantaneously (Friedman, 2012). Nudges have been studied in discrete time games, while the exploration of continuous time nudges, as I do here, is a new area of research.

CHAPTER TWO

Experimental Design

My experimental design closely resembles the full information treatments of Leng (2018). I modified the code provided by Leng to implement a nudge in the MEG. Adding to Leng's full information treatments, I implemented a nudge version of the MEG by changing the parameters associated with the cost of effort and the bonus from the minimum effort.

There are four treatments in the experiment. The first is the control, which is a discrete time MEG, similar to other MEGs in the literature. The second is a nudge treatment in which a nudge table replaces the standard table during some of the periods in the middle of the MEG; this MEG is also in discrete time. I apply continuous time to both of the prior treatments to obtain two more treatments, a standard continuous time MEG and a nudge continuous time MEG. I use a between subject design so that each subject only participates in one treatment.

2.1 Mathematical Presentation of MEG

In my discrete time minimum effort game, players are placed in groups of four. Players privately and simultaneously choose to expend integer effort levels between 1 and 7. The higher the effort that a player chooses to expend, the lower his or her payoff for the period. The incentive for expending a higher level of effort comes from the bonus based on the minimum effort of the group (including one's own effort). This bonus is

conventionally twice the magnitude of the cost of expending effort. This means that there are seven Nash equilibria, each Nash equilibrium corresponding to an effort that all players choose. These are ranked from the least Pareto-efficient (where all players choose to expend 1 effort), to most efficient (where all players choose to expend 7 effort).

Formally, the discrete MEG is a game played in groups of N participants over R periods, where each participant $i \in \{1, 2, \ldots, N\}$ simultaneously chooses some effort level $\sigma_{i,j} \in \{1, 2,...,7\}$, where $i \in \{1, 2, \ldots, N\}$ is an index to indicate the participant choosing the effort, and $j \in \{1, 2, \ldots, R\}$ is an index to indicate the period the effort was chosen in. The parameter α is a constant sufficiently large to prevent negative payoffs, β is the cost of expending effort, and μ is the bonus from the group minimum effort. Payoffs for any individual i in any period j, denoted $\pi_{i,j}$, are given by the function:

$$\pi_{i,j}(\sigma_{1,j},...,\sigma_{N,j}) = \alpha - \beta * \sigma_{i,j} + \mu * \min\{\sigma_{1,j},...,\sigma_{N,j}\} \quad (1)$$

and the total payoff for an individual i, denoted π_i, is given by:

$$\pi_i = \sum_{j=1}^{R} \pi_{i,j}(\sigma_{1,j},...,\sigma_{N,j}) \quad (2)$$

Participants remain in the same group all R periods. After each period j, participants are informed of the effort $\sigma_{i,j}$ for every participant in the group, and of their own payoffs for that period and all previous periods. Participants have x seconds to decide on an effort level.

2.2 Parameters and payoffs

A screenshot of the payoff table used in my experiment is given in Figure 2.1; on the vertical axis participants choose their own effort, and on the horizontal axis the

minimum effort of the group is displayed. One's payoff is found by looking at the number in the same row as one's effort and the same column as the minimum effort of the group (including one's own). For example, if a participant in a group of four chooses an effort of 6 and the other three participants choose efforts of 7, 4, and 2, that participant would earn a payoff of 40 for that period. Of importance is that, if a participant knew *a priori* the efforts of the other participants, then that participant would be best off expending the same amount of effort as the minimum effort chosen by the rest of the group members. Expending more than this effort is costly, and expending less would lower the group minimum and decrease that participant's payoff.

A common set of parameters for the game are $\alpha = 70$, $\beta = 10$, and $\mu = 20$. The payoff table derived from these parameters is displayed in Figure 2.1, and is standard in many studies involving the MEG (Leng et al., 2018; Hyuck et al., 1990). For the control treatment, these parameters are used. As mentioned earlier, this study explores the effects of nudges on the MEG. To implement a nudge phase, I have chosen an alternate set of parameters to use. In this experiment, a nudge period is a period in which $\alpha = -10$, $\beta = 1$, and $\mu = 21$. Our nudge parameters have a larger difference from the base parameters used by Brandts (2006); I chose these parameters to create a payoff table with period numbers along the diagonal and an easy-to-understand progression up any column. As Brandts (2006) finds that the size of the incentive did not make a significant difference in the behavior induced, I am confident that these nudge parameters will induce the desired behavior. Participants are not informed of these alternate parameters prior to the start of the experiment, but are informed of the possibility that the parameters could change. In

all nudge periods, the numbers on the payoff table presented to the subjects are switched to reflect this new payoff scheme. To help participants recognize this, the coloring palette of the payoff table is also switched (in a manner such that individuals with Deuteranopia and Protanopia can tell the difference). A screenshot of the payoff table during the nudge period is presented in Figure 2.2.

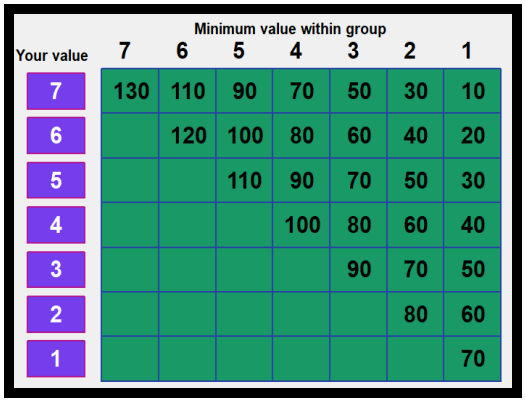


Figure 2.1 This is the standard payoff table and is used for the majority of the experiment. Individual payoffs are calculated by finding the intersection of the row that corresponds to the individual's chosen value, and the column that corresponds to the group minimum effort.

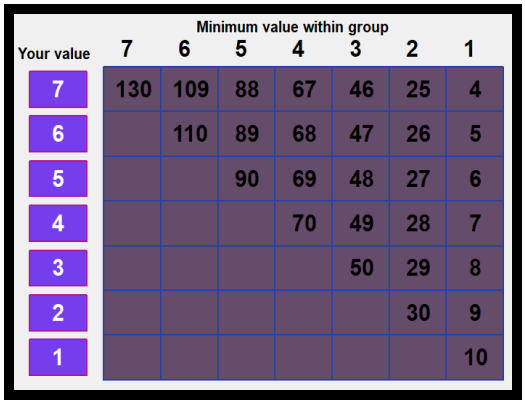


Figure 2.2 This is the nudge payoff table that is only used to calculate payoffs in nudge periods (periods 5 through 8 of treatments with a nudge). The color is different from the standard payoff table to signal to subjects that the payoffs have changed from their original values.

The nudge payoff table has an important difference from the standard payoff table in that in any given column, every payoff in that column is strictly greater than every

payoff in the column to the right, and strictly less than every payoff in the column to the left. Although the rankings of the payoffs in any column are the same as in the standard table, the relative rankings of the payoffs between columns are different. For example, compare the payoffs under the standard table of an effort of 6 with a minimum effort of 4, which is 80, and an effort of 3 with a minimum effort 3, which is 90. Thus, a value in a column of lower minimum effort is greater than a value in a column of higher minimum effort. But under the nudge table, these payoffs are 68 and 50 respectively, and so the nudge table maintains that payoffs in a higher minimum effort column are always greater than payoffs in a lower minimum effort column. The nudge table maintains the basic structure of the minimum effort game, but reduces the risk involved with over-expending effort. This means that the cost for over-expending effort is small, and may serve to increase the minimum effort level.

Our nudge table is fundamentally different from the "B" table of Hyuck (1990), as it maintains the basic structure of the MEG. The table that Hyuck used did not include an incentive to match the minimum effort of other group members. Therefore, an effort level of 7 is a strictly dominant strategy in Hyuck's nudge period, and it is not in my nudge period.

In order to effectively model a nudge to a system already in existence, I choose to implement the nudge table only after 4 periods of the MEG have been played under the standard payoff table. This means that groups will already be developing a trend when the nudge is implemented. I have chosen a mid-game implementation because many

nudges are policy decisions concerning systems that are already in place, and to model these policies I need subjects to have a history of coordination failure as a group.

2.3 Continuous Time

As described earlier, continuous time can be applied to the discrete time MEG to create the continuous time MEG. The continuous time version has the same parameters and formulation[1], except that $\Sigma_{i,j}$ is a function of t with domain $t \in [0,1]$, and range $\{1, 2,..., 7\}$, that is $\Sigma_{i,j} : [0,1] \rightarrow \{1, 2,..., 7\}$. As the time t progresses from 0 to 1 at some rate of $t = x$ seconds, participants may change their effort levels and may observe the present effort levels of other participants. This means that $\Sigma_{i,j}(t)$ is not chosen *ex ante* but rather as the participant observes the choices of other participants. Thus, the payoff function is now:

$$\Pi_{i,j}(\Sigma_{1,j},...,\Sigma_{N,j}) = \int_0^1 (\alpha - \beta * \Sigma_{i,j}(t) + \mu * \min\{\Sigma_{1,j}(t),...,\Sigma_{N,j}(t)\})\, dt \qquad (3)$$

Note that $\Pi_{i,j}$ is a function of the $\Sigma_{i,j}$'s, not a function of t. That is, $\Pi_{i,j}(\Sigma_{1,j},...,\Sigma_{N,j})$ is not evaluated for constants $\Sigma_{i,j}(t)$, but for functions $\Sigma_{i,j} : [0,1] \rightarrow \{1, 2,..., 7\}$. The $\Sigma_{i,j}$'s are functions that map t into the space of possible efforts, so each $\Sigma_{i,j}$ can be evaluated at a time t. The function $\Pi_{i,j}(\Sigma_{1,j},...,\Sigma_{N,j})$ is not evaluated at a time t but rather is an integral over the $\pi_{i,j}$ presented in equation (1). Intuitively, $\Pi_{i,j}$ considers the game by looking at the choices of each player during the entire time, and cannot be evaluated at any particular time.

[1] To clarify the differences between the discrete and continuous time versions of the MEG, I denote the effort and payoff objects in discrete time with lowercase Greek letters (σ and π) and in continuous time with uppercase Greek letter (Σ and Π), respectively.

2.4 Treatments

We implement a 2 by 2 factorial design (see Table 2.1), with discrete or continuous time as one dimension, and whether a nudge period is implemented or not as the other dimension. For the time variable, the treatments are either conducted entirely in discrete time or entirely in continuous time. The payoffs are scaled such that if all players in the continuous time game maintained constant effort levels for the entire 45 second period, then they would earn the same payoffs as if they had chosen those effort levels in a discrete time period. Between periods subjects in all treatments are shown a history of their earnings, the average effort of each subject in each period, and the average minimum effort of each period. The next period only begins once all subjects indicate that they are ready.

Table 2.1 The row is the type of time used in the minimum effort game, and the column is whether a nudge period is implemented.

	No Nudge	Nudge
Discrete	*Standard Discrete*	*Nudge Discrete*
Continuous	*Standard Continuous*	*Nudge Continuous*

For the nudge variable, the nudge payoffs are implemented in periods 5 through 8 (inclusive). Thus, participants play the first 4 periods as in a regular minimum effort game, the next 4 with the nudge payoffs, and the final 8 with normal payoffs. This design is chosen so that the effects of the nudge may be evident. The first 4 periods with the standard payoff table are necessary so that the appearance of the nudge table in period

5 is a change from the status quo. The nudge lasts for 4 periods, which I hypothesize is enough time for players to develop an equilibrium at a minimum effort above the minimum effort in the corresponding treatment with no nudge. Finally, the last 8 periods are designed to see if this equilibrium persists, as I am testing for the lasting effects of the nudge.

These two variables give rise to four treatments:

1. Standard Discrete Participants play a 16 period MEG with the standard payoff table. The game is played in discrete time. This is the control group.

2. Nudge Discrete This is the same as above, except that in periods 5 through 8 the nudge table is used to determine payoffs rather than the standard table. This game is also played in discrete time.

3. Standard Continuous Participants play a 16 period MEG in continuous time using the standard payoff table.

4. Nudge Continuous Continuous time is used for all 16 periods, and in periods 5 through 8 the nudge payoff table is also implemented.

2.5 Hypotheses

For my hypotheses, I choose to focus on the average minimum effort rather than the average effort because I am interested in successful and efficient coordination rather than in attempts at efficient coordination. For example, if a group has an average minimum effort of 3 but an average effort of 5, this indicates that the group made

attempts at coordinating at a high level, but at least one member of the group contributed significantly less than the others. From a policy perspective, this is not a success. As nudges are often policy tools, I will focus on policy-related results.

While the definition of the minimum effort in a period for a group in discrete time is intuitively the minimum of the values chosen, the minimum effort over a period of time for a group in continuous time is not as straightforward. Two methods that are equally direct are measuring the minimum effort at the end of the period and measuring the minimum effort during the entire period via an integral. I choose the latter as this takes full advantage of the continuous time environment and may allow more of the subtleties to be examined.

In the statistical tests presented to test the first two hypotheses, I choose to use one-sided tests rather than two-sided tests. I choose this because Brandts (2006) found clear and positive effects from implementing a similar nudge in discrete time, and as this nudge is designed to increase the minimum effort, I believe it will at the very least not do the opposite. Particular reasons for the use of a one-sided test are presented along with both hypotheses. For the third hypothesis, I choose to use a two-sided test for reasons given in that section. For all tests I use the conventional significance level of 0.05.

2.5.1 Hypothesis 1

The first question that must be addressed is whether the nudge payoff table is effective in easing the difficulties of coordination. Are participants more successful at coordinating at higher minimums when the nudge payoff table is implemented? As discussed earlier, the nudge payoff table reduces the cost of over-expending effort, and

increases the benefit from coordinating on a higher group minimum effort. Brandts (2006) implements a minimum effort game over 30 periods and finds that the group minimum is significantly higher when the bonus from the group minimum is increased. Although Brandts' parameters vary from ours, Brandts finds that the size of the parameter change does not play a significant role in increasing the minimum effort level. For this reason, I predict that minimum effort will be higher in the periods with the nudge payoff table than in periods with the standard payoff table.

> *Hypothesis 1:* The average minimum effort will be higher in periods 5 through 8 of the *Nudge* treatments than in periods 5 through 8 of the *Standard* treatments.

To test the hypothesis that minimum efforts are higher in periods 5 through 8 in *Nudge* treatments than in *Standard* treatments, I use the one-sided Wilcoxon rank-sum test. I choose the one-sided Wilcoxon rank-sum test because I have no theory indicating that these observations are normally distributed. The test is one-sided since I expect the minimum effort in *Standard* treatments to average around 2, so a decrease would be difficult. Additionally, I use an independent test because the data comes from two entirely different populations since subjects only participate in one treatment. Data from both *Nudge* treatments is pooled, and data from both *Standard* treatments is pooled. Using the method described in 3.2, I average the values from four minimum values per group, and I conduct the Wilcoxon rank-sum test between the pooled *Nudge* data set and the pooled *Standard* data set. The null hypothesis is that the two pools have the same or

lower mean minimum effort, and the alternative is that the mean minimum effort is greater in the *Nudge* treatments.

2.5.2 Hypothesis 2

Our primary aim is to test the effectiveness of this particular nudge on coordination games. As this nudge is designed to help participants develop patterns of coordination through learning, the effectiveness of the nudge will be judged not by the immediate effects on behavior, but by the long term group coordination success or failure. I predict that the *Nudge* treatments will maintain higher minimum efforts than the *Standard* treatments. To test this, I examine the average minimum effort in period 15 in all four treatments. I choose to examine the average minimum effort in period 15 rather than in period 16 to make my results robust to end-of-game effects.

> *Hypothesis 2:* The average minimum effort in period 15 will be higher in the *Nudge* treatments than in the *Standard* treatments.

This hypothesis is tested in the same manner, except this time the test would be conducted between the mean minimum effort per group in period 15 (rather than periods 5 through 8). The one-sided Wilcoxon rank-sum test would again be used for the same reasons. The null hypothesis is that the two pools have the same or lower mean minimum effort, and the alternative is that the minimum effort is greater in the *Nudge* treatments.

2.5.3 Hypothesis 3

Communication between participants in the minimum effort game can only be achieved through chosen effort levels, as these are the only actions that other participants can observe. Continuous time adds a richness to the communication space of the MEG. Whereas in discrete time players are only able to communicate (through their actions) once per period, in continuous time participants can communicate a multitude of times in any given period. Leng (2018) has shown that when information regarding other players' actions is significantly rich, the minimum effort in continuous time is significantly higher than in discrete time. As I expect the nudge periods to introduce some change into the dynamics of the MEG (hypothesis 1), and as I theorize that players' ability to communicate their intentions to raise the minimum effort is vital to successful coordination, a richer communication space is expected to improve the effectiveness of the nudge. I predict that the *Nudge* treatments will be more successful in raising the group minimum effort in the *Continuous* treatments than in the *Discrete* treatments.

> *Hypothesis 3:* The average minimum effort in period 15 increases more in the *Nudge Continuous* treatment than in the *Nudge Discrete* treatment.

For the final hypothesis, I am concerned about the relative effect of the nudge periods in the continuous time setting compared to the discrete time setting, and wish to see whether the nudge is more effective in continuous time than in discrete time. To quantitatively test this, I first calculate the relative increase in minimum effort. This is done by subtracting the mean minimum effort in a *Standard* treatment from the

corresponding *Nudge* treatment. That is, the mean minimum effort in period 15 of *Standard Discrete* is subtracted from each of the minimum efforts in period 15 of *Nudge Discrete*, and the same for the *Standard Continuous* and *Nudge Continuous*. The increase in minimum effort is then a set of observations (one for each group in *Nudge* treatments) With these groups I run a two-sided Wilcoxon rank-sum test to test whether the *Continuous* treatment is greater than the *Discrete* treatment as the improvement in the *Continuous* case may not have a larger magnitude than the improvement in the *Discrete* case. The null hypothesis is that the increases in the *Discrete* case have the same mean as in the *Continuous* case, and the alternative is that increases in the *Continuous* case have a different mean.

CHAPTER THREE

Results

Due to the COVID-19 pandemic, only data on the *Standard Discrete* treatment could be gathered. Such data functions as a pilot for the rest of the experiment. In this section I discuss the procedures used in the experiment, describe the participants, and present the pilot data.

3.1 Procedures

Two pilot experiments were run in February and March of 2020 in the experimental economics laboratory at Baylor University. I am grateful for the generosity of the Baylor Undergraduate Research and Scholarly Achievement initiative for funding this research by providing the cash to pay participants.

The experiment is programmed in z-Tree (Fischbacher, 2007). Thanks to the z-Tree code provided by Leng (2018), I am able to implement the MEG in continuous time. I modified the code provided by Leng to allow for the *Nudge* treatments mentioned above. Subjects were able to change their decisions every 0.3 seconds, which is close enough to continuous time that I consider the experiment to be run in continuous time, as did Leng (2018). To minimize differences between the continuous time and discrete time treatments, subjects in the discrete time treatments had 45 seconds (the same as in the continuous time treatments) to modify their choices after all subjects had submitted their initial choices. The subjects could not see the choices of other subjects or the minimum

effort during this time. After the experiment concluded, at least one subject verbally expressed confusion over the purpose of this, yet as this is important to maintaining similarity between the treatments, I decided to keep it as a feature of the game.

Subjects entered the laboratory, and after all subjects arrived, subjects were each randomly assigned a computer. If there were more subjects than group size permitted, then some subjects were randomly selected to receive the participation fee and were dismissed. After paper instructions were handed out to subjects, the instructions were read by the experimenter. Following a quiz to check understanding, the treatment began. All subjects in a session were a part of the same treatment group. No deception was used, and for *Nudge* treatments subjects were not informed of the learning period. Subjects in *Standard* treatments received the same instructions as their *Nudge* counterparts would have, and all subjects faced the ambiguous possibility of a change in the parameters. Subjects were paid their show-up fee of $10, and their earnings. I expected earnings to average $10 in addition to the show-up fee. After the experimental portion of the MEG (but before the survey and compensation), subjects participated in an unrelated labor market task. After the conclusion of that task, one of the two games was randomly selected for payment (this was emphasized to subjects after reading the opening instructions); this was common information to participants. In both sessions, the unrelated labor market task was chosen to be paid. The mean earnings from the MEG (if it had been paid) in addition to the show-up fee would have been $15.62.

3.2 Participants

There were two sessions, the first with two groups of four, and the second with one group of six. (Due to the results of the two groups of four, group size was increased to six after the first session). I intended to recruit a total of 96 participants, but due to the COVID-19 pandemic, in-person classes at Baylor University were canceled after only two sessions had been conducted. I successfully gathered pilot data for the *Standard Discrete* treatment. Thus a total of 14 subjects participated in the experiment. Subjects were recruited from a pool of participants maintained by the university. Prior to the experiment, additional individuals were recruited into the subject pool by solicitation outside of on-campus dining halls. As nearly all freshmen at Baylor are required to have a meal plan, I do not suspect this to be a source of participation bias. No subjects had participated in similar experiments, and subjects could only participate in one treatment. Subjects were all between the ages of 18 and 22, 57% (8 of 14) were female, and 64% (9 of 14) were white. 4 reported attending religious services at least once a week, 6 at least once a month, and 4 a few times a year or never. These are characteristic of Baylor students as reported by the university[1].

3.3 Pilot data

According to my hypotheses, I anticipate the data from the *Standard Discrete* treatment to have low levels of efficient coordination. However, in my data, groups quickly converge to efficient outcomes. A basic overview of the choices made by the subjects is presented in Table 3.1

[1] https://www.baylor.edu/irt/index.php?id=75614 This is a report published by Baylor University about the composition of the undergraduate student body for Fall 2018 and Fall 2019

Table 3.1 Summary statistics on effort and minimum throughout the experiment for all groups.

Periods	Mean Effort (Standard Deviation)	Mean Minimum Effort (Standard Deviation)
1 - 4	5.93 (1.58)	3.68 (1.98)
5 - 8	5.45 (2.28)	4.14 (2.77)
9 - 12	5.55 (2.31)	4.79 (2.66)
13 - 16	6.95 (0.23)	6.86 (0.35)

The data gathered from the first session (consisting of two groups of four) is presented first, followed by the data from the second session. The groups are labeled 1.1 and 1.2 for the first session, and 2 for the second session.

3.3.1 Session 1

A graph of the efforts of both groups is presented in Figure 3.1. In Figure 3.2 I present a graph of the minimum effort for both groups. Both groups show a rapid increase in coordination, and group 1.2 even shows successful improvement in coordination after an equilibrium that lasted 12 periods. This is roughly in line with previous work, particularly that of Engelmann and Norman (2010). They found that, in settings with full information about the choices of other group members and 10 periods of play, the mean final minimum effort level was 4.125. Of the 2 groups of 4, the mean minimum effort at period 10 was 6.5, and the mean final minimum effort level was 7 (perfect coordination). This indicates that the groups coordinated with similar or even higher levels of efficiency.

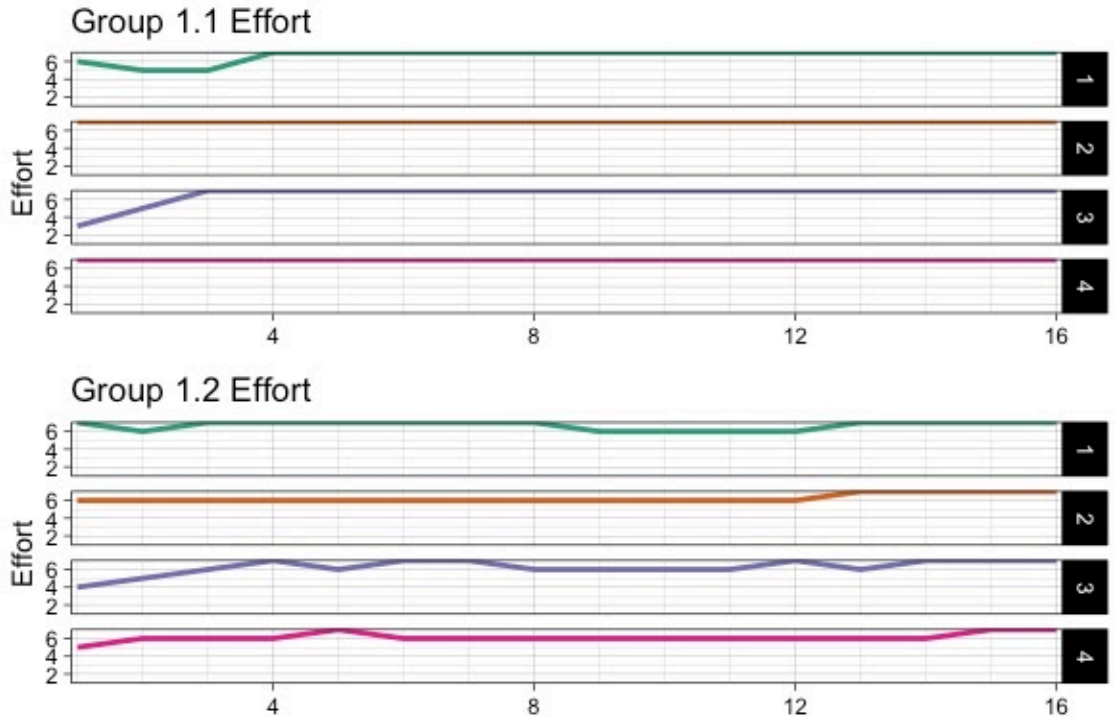


Figure 3.1 The effort in each period for each subject in the first session is displayed. 1 is the minimum possible effort, and 7 is the maximum effort. Note: effort is only measured at discrete period numbers (1, 2, etc.), and the slope between periods is only graphed to make seeing the changes easier.

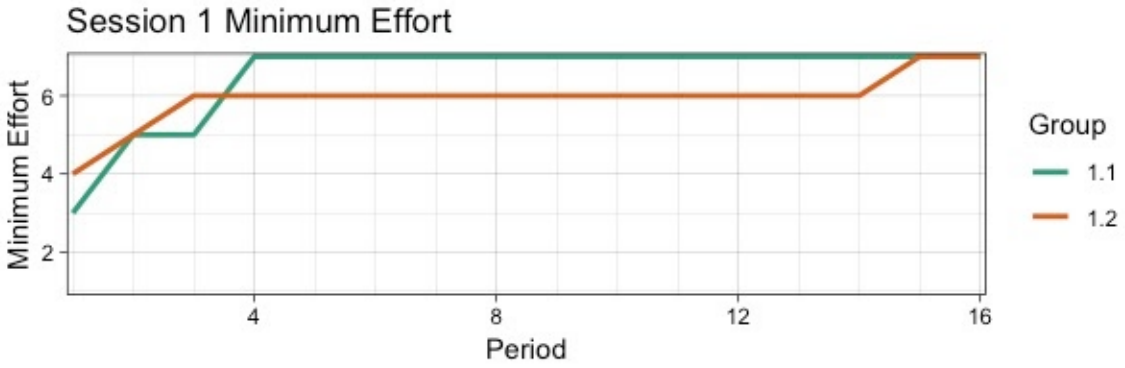


Figure 3.2 The minimum effort in each period for each group in the first session is displayed.

After observing this level of coordination, I decided to increase the group size from four to six out of a concern that the efforts too quickly converged to an efficient equilibrium. If most groups converged before the nudge was implemented in period 5,

then a treatment effect would be difficult to detect. In the literature, groups of size four often exhibit this level of coordination, but after observing this in the laboratory with both groups, I decided to increase the group size to six in an attempt to make coordination more difficult.

3.3.2 Session 2

The graph of the data from group 2 is presented in Figure 3.3. The minimum effort for the group is displayed in Figure 3.4. Considering only periods 1 through 10, the group follows the pattern other researchers have observed in MEGs of this size. However, in period 10 a change begins to occur, and by period 12 the minimum has spiked from 1 to 7, marking a significant change in the equilibrium. This continues to the close of the game. Upon inspection, this appears to be driven primarily by the actions of subjects 1 and 2. In period 9, all subjects other than subject 2 had chosen an effort of 1. In period 10, subject 1 joins subject 2 in expending maximum effort. Subjects 4 and 5 follow suit in period 11, and in period 12 all subjects expend maximum effort. This equilibrium persists to the end of the game. This observation shows the value of increasing the number of periods in experimental games; some behaviors are not observed until a significant number of periods have been completed. This matches the results found by Berninghaus (1998), where increasing the number of periods increases the probability of coordinating on a Pareto efficient outcome.

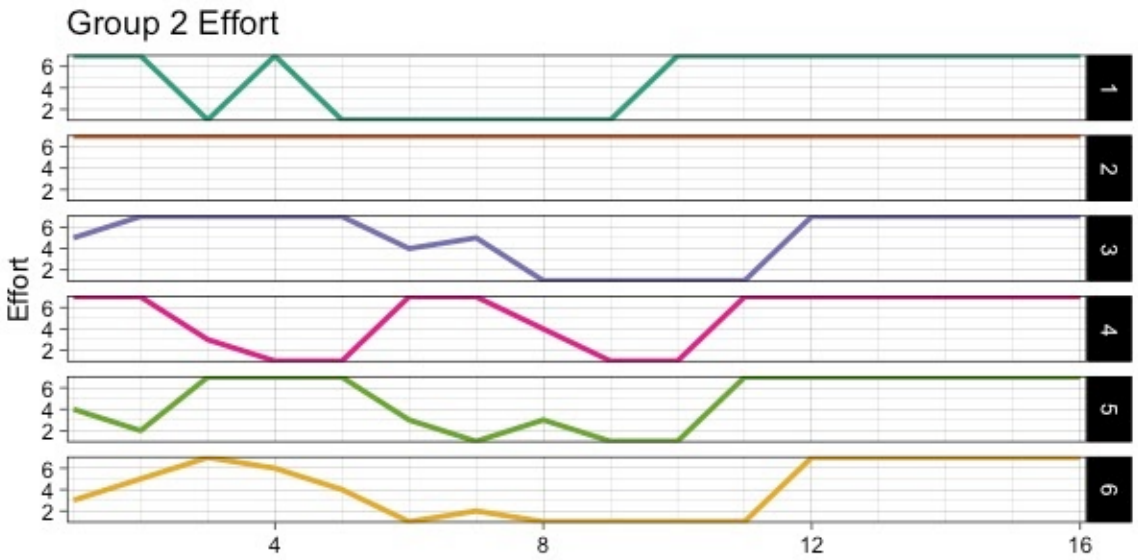


Figure 3.3 The effort in each period for each subject in the second session is displayed.

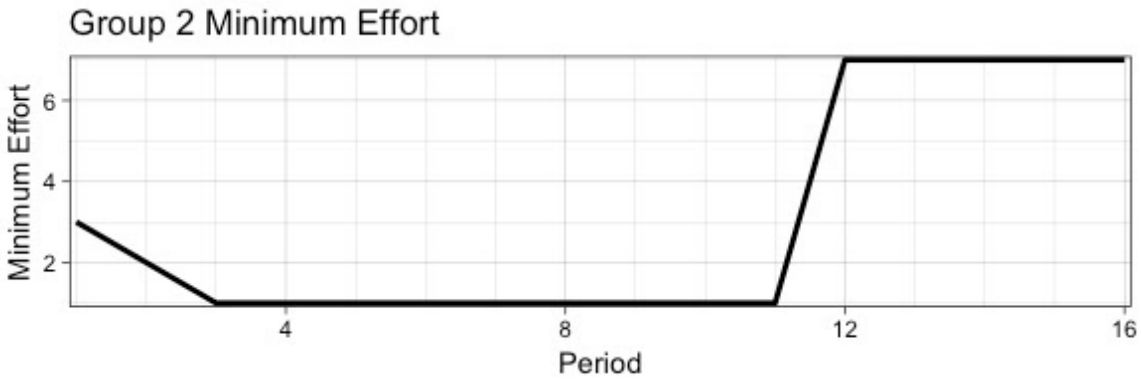


Figure 3.4. The minimum effort for group 2, consisting of six participants.

3.3.3 Combined Sessions

For the remainder of this section, I combine the data from both sessions. Examining both sessions together, I can draw a few other conclusions from the pilot data. First, I can examine the frequency of each effort level chosen during the MEG. 224 individual effort choices were selected, and a bar graph of the percent frequency of these choices is presented in Figure 3.5 along with a bar graph of the percent frequency of the

48 group minimum effort levels. From these graphs I see that 7 is a strong focal point for individual effort choices, as most players attempt to coordinate on the Pareto dominant Nash equilibrium. However, the frequency of the minimum effort graph shows how a minimum effort of 1 is a frequent occurrence.

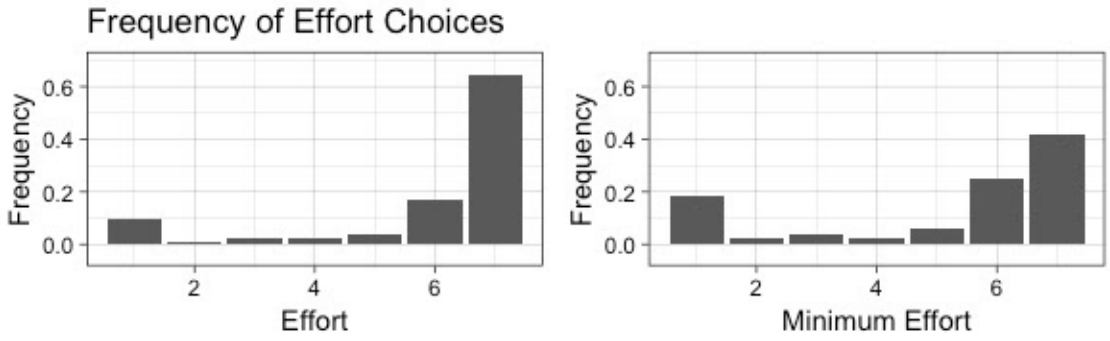


Figure 3.5 The frequency is the percent, across all periods and all subjects, that the given effort is expended.

Another observation comes from examining effort at the beginning of the MEG with effort at the end of the MEG. Although I had originally anticipated end-game effects, none of the groups exhibited different behavior between periods 15 and 16. I run a Wilcoxon signed-rank test to see whether there is a difference in the mean effort between period 1 and period 16. The null hypothesis is that the difference is 0, and the alternative hypothesis is that the difference is not 0. The null is rejected in favor of the alternative hypothesis with a p-value of 0.014, so I find a significant difference in the effort levels in period 1 compared with effort levels in period 16.

Of interest with this data is the high level of efficient coordination present. Baylor students seem to coordinate on efficient outcomes more than students at other universities, which makes detecting a treatment effect in this case difficult. If the

majority of *Standard Discrete* groups coordinate on the most efficient effort choice by the end of the MEG, then a nudge is not going to do much to help them out. Even in the group of size six this behavior is observed. Although this may be a fluke, it seems reasonable to redesign the experiment with either harder parameters (increasing the cost of effort relative to the gain from increasing the minimum effort) or an increased group size. Although reducing information about other players usually decreases coordination, I do not want to make such a change as it would undermine the gain from continuous time. Continuous time, in general, is more effective at increasing coordination when there is full information because subjects can cheaply signal by quickly changing their choices.

3.4 Connecting Data to Hypotheses

Since only data on the *Standard Discrete* treatment has been gathered, statistical tests on the hypotheses cannot be conducted. In section 2.5, following each hypothesis I described the necessary statistical tests to conduct to provide evidence for or against the hypotheses. In this section, I describe how this data may support the hypotheses, or may reveal difficulties in the design that may be improved upon. I only consider the data from group 2.

3.4.1 Hypothesis 1

The data suggest that, for groups of size six, verifying the first hypothesis should be quite possible. As the minimum effort for group 2 is at the lowest possible state, increasing it through a nudge should be fairly straightforward, and I would expect this to

work. There is significant variation in the choices made by individual subjects in group 2 during periods 5 through 8, which suggests that a nudge may work by reducing confusion and providing a focal point.

3.4.2 Hypothesis 2

For the second hypothesis, it seems that the parameters of the *Standard Discrete* treatment are too favorable to subjects, as every group reached maximum efficiency by period 15. However, such behavior with these parameters is unusual in the literature. Baylor students may be too cooperative for groups of size six to fail to coordinate on the efficient outcome with consistency. If this is the case, then group size would need to be increased to even have the opportunity to observe data to verify this hypothesis. Even when a treatment effect exists, the current conditions may not allow it to be observed.

3.4.3 Hypothesis 3

The same problems occur in this data concerning the third hypothesis as occur with the second. Without room to improve the minimum effort in period 15, no treatment effect may be observed. My pilot data indicates that a change in the experimental design may be necessary to detect a change due to the continuous time environment.

3.5 Regression Analysis Using Survey Data

Beyond my primary hypotheses, I intend to study group demographics to look for other effects due to the particular composition of the group. In particular, I am interested in studying the effects of the level of religiosity present in group members. As Baylor is a Baptist university that requires two semesters of chapel for undergraduate students, the

common Christian background of many of the students may influence the amount of efficient coordination in the laboratory. Since I observe higher than expected levels of coordination in the laboratory, I will examine this peculiar feature of Baylor students.

For the statistical examination, I use the demographic data gathered after the experiment. The two features I want to examine with regression are whether religious students initially expend more effort, and whether religious students are more likely to expend higher than the minimum effort. One of the questions asked subjects how often he or she attends religious services. I will provide a rank-ordering for the religious service attendance responses to use as the independent variable, and will conduct two regressions: one with effort in period 1 (or average effort, in the *Continuous* treatments) as the response variable, and one with the average effort a subject expends above the minimum effort as the dependent variable.

CHAPTER FOUR

Analysis and Conclusion

In this section I conduct several power analyses to estimate the sample size needed to obtain reasonable statistical power. Following the power analyses, I conclude with possible follow up studies.

4.1 Power Analyses

As mentioned earlier, after the first session, I decided that a group size of 6 is necessary. The following analyses are performed on groups of size six. I use the common significance level of $p = 0.05$ for the power analysis, and seek to find a sample size that would give a power of 0.8 or higher. Additionally, I assume that I collect samples of identical sizes: each treatment has the same number of groups. As I have reason to expect an increase due to the nudges, I perform one-sided tests for the first two hypotheses, and a two-sided test for the third hypothesis (see section 2.5). In order to conduct a power analysis, I must first specify population distributions for the minimum effort value. Because my data is limited, I provide fairly uninformative estimates.

Based on my method for quantifying the minimum effort in a period, the minimum effort in a period in the *Discrete* treatment is a discrete is a variable i.e. $\sigma_{i,j} \in \{1, 2,..., 7\}$. The minimum effort in a period in either *Continuous* treatment, or in the average of several *Discrete* treatment periods (i.e. hypothesis 1), however, is a

continuous variable i.e. $\int_0^1 \min\{\sigma_{1,j}(t),...,\sigma_{N,j}(t)\}\, dt \in [1, 7]$. For hypothesis 1, all data used is continuous, so I treat the population distribution as continuous. For hypothesis 2, I pool data from the *Discrete* and *Continuous* treatments, so I choose to treat my sample as coming from a continuous population with minimum 1 and maximum 7. For hypothesis 3, I subtract the predicted mean minimum effort in period 15 of the *Discrete* treatment from the specified population minimum effort distribution of the corresponding *Continuous* treatment. This again produces a continuous variable, but the value is in the interval [-6, 6] instead. As all samples may be considered from a continuous distribution, a Wilcoxon rank-sum test is applicable.

I am able to compute power estimates of the Wilcoxon rank-sum test from these continuous distributions[1]. I choose to parameterize the population distributions in the *Continuous* treatments with beta distributions for two reasons. First, beta distributions are well studied in statistics, and second, my software required that a canonical distribution be used (the Beta being the closest match from the provided distributions). This works well in general, but as minimum efforts of 1 and 7 may be focal (even in continuous time), the beta distribution cannot capture this behavior well. Accepting this difficulty, I minimize the problem by choosing fairly diffuse distributions.

As the Beta distribution only ranges from 0 to 1, I perform a transformation to change the range. I multiply the value drawn from the standard Beta by 6, and then add 1. This is a simple rescaling so that the nontrivial domain of the Beta matches the domain of the distribution of the minimum efforts. For sections 4.1.1 and 4.1.2, when I

[1] I used the R statistical software program and the 'wmwpow' package

refer to Beta distributions, I refer to the rescaled version. In section 4.1.3 a different rescaling will be necessary.

4.1.1 Power Test for Hypothesis 1

In Figure 4.1 I display the specified population distributions for minimum efforts periods 5 through 8. For the *Standard* treatments, I choose to model the population with a Beta (2, 3). I choose these parameters because they are both diffuse and produce a mean of 3.4. Additionally, these reflect the data I gathered (which had an average minimum effort of 1 for periods 5 through 8). For the *Nudge* treatments, I choose to model the population with a Beta (2, 1). These parameters are diffuse and produce a mean of 5.

Using these distributions, I constructed a power curve in Figure 4.1. I find that both samples need to consist of at least 11 groups to obtain a power of greater than 0.8. Thus, the experimental design would need to include at least 6 groups per treatment for a total of 144 subjects.

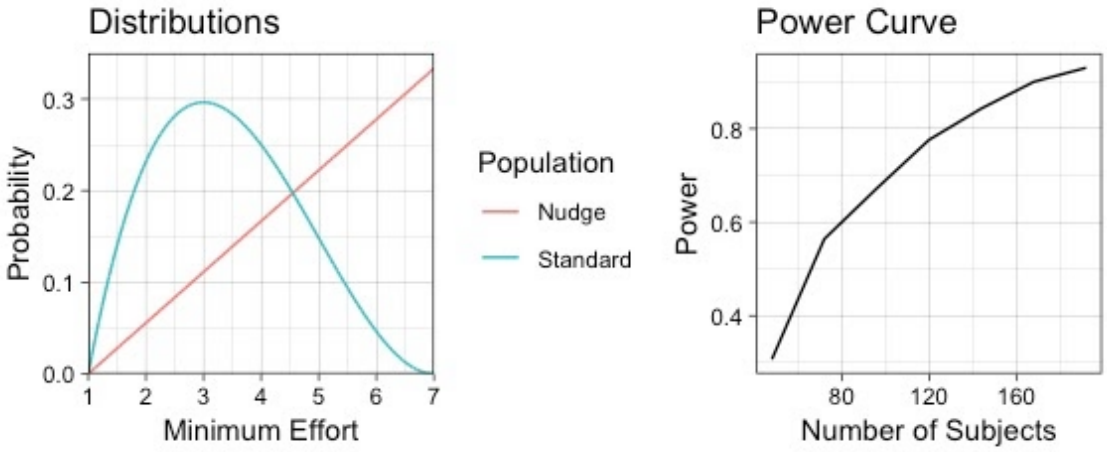


Figure 4.1 These are the PDFs of the distributions for hypothesis 1. The number of subjects is calculated under the assumption that groups consist of 6 subjects, and is the total number of subjects needed in the experiment to obtain that power level.

4.1.2 Power Test for Hypothesis 2

In Figure 4.2 I display the specified population distributions for minimum efforts in period 15. For the *Standard* treatments, I choose to use a Beta (2, 4) for a mean of 3. My data seems to come from a better population than this, but that is mostly due to the efforts of 1 subject and I do not expect this to hold in general. For the *Nudge* treatments, I choose to use a Beta (4, 2) for a mean of 5.

Using these distributions, I constructed a power curve in Figure 4.2. I find that both samples need to consist of at least 6 groups to obtain a power of greater than 0.8. Thus, the experimental design would need to include at least 3 groups per treatment, for a total of 72 subjects.

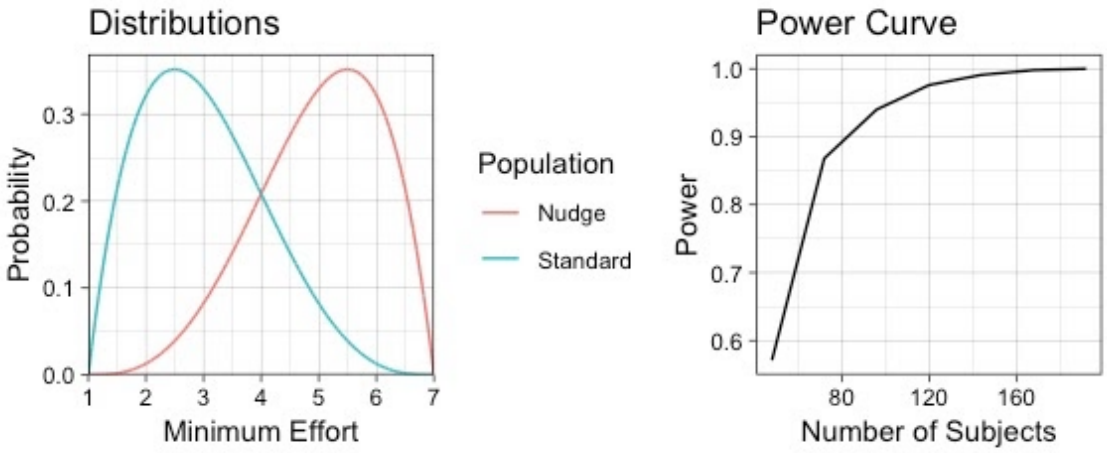


Figure 4.2 These are the PDFs of the distributions for hypothesis 2. The number of subjects is calculated under the assumption that groups consist of 6 subjects, and is the total number of subjects needed in the experiment to obtain that power level.

4.1.3 Power Test for Hypothesis 3

Rescaling the Beta distributions for this power analysis is different as the range is from -6 to 6. To do so I multiply the value drawn from the standard Beta by 12 and then

subtract 6. Using this rescaling, I transform the nontrivial domain of the Beta to [-6, 6]. For the *Discrete* treatments, I choose to use a Beta (2.2, 1.3) for a mean of ≈ 1.54. This is in line with Leng (2018) for the difference between full information discrete and continuous MEGs. For the *Continuous* treatments, I choose to use a Beta (5.5, 3) for a mean of ≈ 1.76 The limiting factor on the Beta parameters for the *Continuous* treatment is the high level of coordination already present in the *Continuous* treatments observed by Leng. After attempting to find the correct sample size to obtain a power of 0.8, I stopped after I came to the conclusion that the sample size would be impractical to reach even a small power. This is not promising for finding results concerning the effects of nudges in discrete versus continuous time.

However, if I assume that the nudge is almost always effective in the *Continuous* treatment, and that I have a tighter distribution in the *Discrete* treatments, I may change the distribution for the *Continuous* treatment to Beta (33, 18) and the distribution of the *Discrete* treatments to Beta (30, 24). These are presented in Figure 4.3.

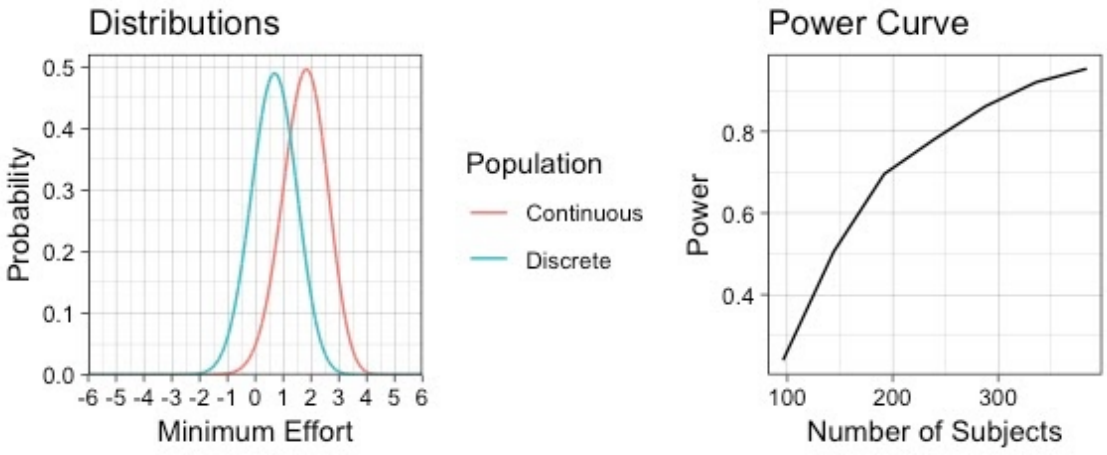


Figure 4.3 These are the PDFs of the distributions for hypothesis 3. The number of subjects is calculated under the assumption that groups consist of 6 subjects, and is the total number of subjects needed in the experiment to obtain that power level.

Using these distributions, I constructed a power curve in Figure 4.3. I find that both samples need to consist of at least 12 groups to obtain a power of greater than 0.8.

As each data set is based on the difference of a *Standard* group and a *Nudge* group, I would need at least 12 groups per treatment to obtain the desired power, for a total of 288 subjects. My original design would only have had a power of roughly 0.1 under these assumptions. Under less informative assumptions about the underlying distributions, the power would have been significantly less.

4.2 Conclusion

I conduct a pilot experiment to test the effectiveness of a nudge in discrete and continuous time. Similar research indicates that my nudge would be effective in the discrete time case, and I would expect to see some treatment effect. My pilot data indicates some level of egalitarianism among Baylor students, as indicated by the high levels of coordination on the socially efficient equilibrium. Power analyses indicate that a small sample size would be adequate to detect treatment effects.

The main goal of my experiment is to understand the effects of the time environment upon decision making under nudges and learning. If I had collected a full data set that supported the hypotheses, then I would have concluded that the time context (either discrete or continuous) in which nudges are applied is critical to their effectiveness. A key feature of my nudge is that it is only temporary; when it is removed, there is uncertainty among the subjects as to which equilibria will prevail. I would have concluded that the structure of time in a game determines how players act in uncertain circumstances. As subjects do not need to commit to a strategy until they have seen the

initial moves of other subjects, they have the liberty to use an almost costless signal that they desire to continue the high levels of effort even when the nudge is removed. Whenever the environment changes in a rapid way, continuous time interactions allow group members to quickly signal their preferences without committing to them, thus reducing the risk and allowing efficient group coordination to arise.

As my second hypothesis depends on the first hypothesis, and the third hypothesis depends on the second hypothesis, I will consider the implications if successive hypotheses are not supported after collecting a full data set. If my first hypothesis is not supported by the data, then two conclusions can be drawn. The first is that the nudge table did not have the desired effect of increasing effort levels. This seems unlikely, but it would indicate that subjects are either risk averse (even in the face of the increased gains from making risky choices) or are pessimistic about the actions of other subjects. The second is that the nudge is not effective because subjects already coordinate high effort equilibria without the nudge, so any improvement would not be detectable. Given the high minimum effort levels present in the pilot data, this second conclusion would be reasonable.

If my second hypothesis is not supported by the data (but the first had been), then I can conclude either that the effect of the nudge is short lived, or that the subjects in the *Standard* treatments succeeded at increasing their effort expenditure. Given that increases in minimum effort are rare in the literature, the former is likely the explanation. Although not the desired conclusion, I can then know that such a direct application of a

learning nudge may not have lasting impact. Attempting to jump-start efficient coordination is ineffective and a waste of effort.

In the event that only the final conclusion is not supported by the data, I can conclude that group members are willing to and successfully maintain more efficient coordination regardless of the time structure. This indicates that the timing is not an important part of the effectiveness of the nudge, and that this learning nudge can be effectively applied in both discrete and continuous time interactions.

Based on my power analysis, I expect my first and second hypotheses to be supported by the data, but that the third hypothesis to remain unsupported without either a significantly larger sample size or a different subject pool. My pilot data indicates that Baylor students are simply too cooperative and community focused to allow for a subtle effect as seen in the pilot data. In group of size six, the efforts of one subject turned around the course of the group from the least efficient coordination to the most efficient coordination (see Figure 3.4, subject 2). I base the power analysis on the assumption that the *Continuous* treatments will see a jump of about 1.7 in the minimum effort between *Standard* and *Nudge* treatments; as Leng (2018) observed an average minimum effort of 5.5 for a treatment similar to *Standard Continuous*, even this small increase would seem improbable. If Baylor students are more community focused than Leng's subject pool, then the possible increase may be even less. Given that the *Nudge Discrete* is expected to have an average minimum effort of 3.5 (Brandts, 2006), there is significantly more room for the *Discrete* time treatments to increase even after an increase due to the community focus of Baylor students.

Future research could focus on improving the power for the test of hypothesis 3. Specifically, there is difficulty in detecting treatment effects if the continuous time treatment without a nudge already has a high level of coordination. Perhaps a way of increasing the difficulty of coordination by increasing group size could be a profitable route to take. In my research, I have come to the obstacle that coordination improvement can only take place where coordination failure is present.

In conclusion, I expect to find that establishing historical precedent is a key factor in developing efficient coordination in groups. The reduction of risk inherent to continuous time interactions serves to protect these newly established norms from decaying. This is applicable to policy decisions because governments and firms may seek to foster coordination between group members. An effective nudge period will serve to promote coordination in the long term.

APPENDICES

APPENDIX A

Discrete Time Experimental Instructions

Images of the instructions for the *Discrete Standard* and *Discrete Nudge* treatments are included in the following pages.

Game One Instructions

General

Thank you for agreeing to take part in this study. Please read the instructions we give you today carefully. A clear understanding of the instructions will help you make better decisions and increase your earnings. After reading the instructions, there will be six questions to test your understanding. To guarantee everyone understands the experiment, you cannot move on to the next question if you do not enter the correct answer.

If you have any questions during the experiment, please raise your hand. Please do not consult with other participants in this room or look at the computer screens of other participants. Please turn your phone off or to silent mode now and place it on the floor, along with any other materials you have brought into the room.

At the beginning of the experiment, you will be randomly assigned into a group consisting of six people, you and five others. The other people in your group are sitting in this room. You and the other five group members will remain in the same group throughout today's experiment. You and any group members will remain anonymous. Please do not attempt to communicate with other participants in the room during the experiment.

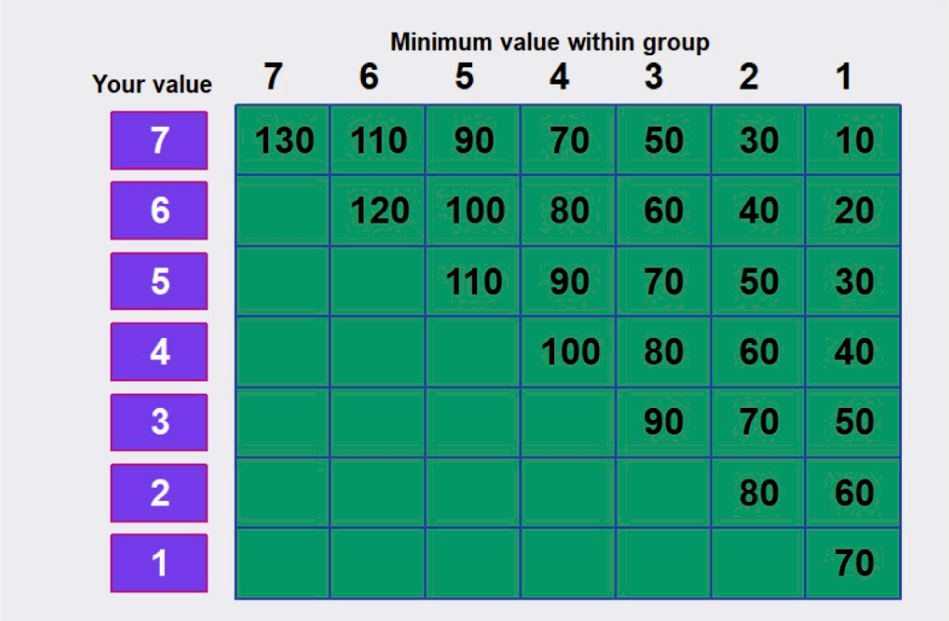
Payoff

The amount of money each participant gets paid is different. It depends on your choice and the other five group members' choices. Details are as following.

This experiment has sixteen periods altogether. At the beginning of each period, you will be asked to pick a value of X. The values of X you may choose are 1, 2, 3, 4, 5, 6 or 7. The value you pick for X and the minimum value picked for X by any participant in your group, including your choice of X, will determine the payoff you receive. Figure 1 on the next page is an example of what this payoff matrix may look like.

The earnings in each period may be found by looking across from the value you choose on the left-hand side of the matrix and down from the minimum value chosen within group from the top of the matrix. For example, suppose the payoff matrix in Figure 1 is the current payoff matrix. If you choose a 4 and values chosen by five other members are 5, 5, 5, 4, and 3, then the smallest value chosen in your group is 3, and your current payoff is 80 points. In Figure 1, the payoff can be found at the cell in the fourth row and fifth column. The points will be converted into cash at the end of the experiment. 100 points equals 1 USD. Thus, the more points you earn, the more you will get paid.

Figure 1



You can change your value every 0.3 seconds. In other words, you or other participants can change the choice three or four times in one second. Periods will each last 45 seconds. During the 45 seconds, you can switch your chosen value as many times as you wish. Your choice for this period is the value you pick at the last second. For instance (assuming the matrix in Figure

1 is the current payoff matrix), if you choose value 6 at the beginning of the period and change to 5 when 20 seconds are left, but switch to 7 when 10 seconds are left and then stayed there till the period end, your choice for this period is 7. Your payoff is only determined by your last choice and the group members' (including yours) minimum last choice.

Values can be picked by clicking the purple button. Your chosen value will be highlighted in a different color. Before the 45 seconds have ended, you cannot observe the group minimum or values picked by any participants.

During the game, the payoff matrix could potentially change from the initial payoff matrix. Any change will be indicated by a significant change in the coloring scheme of the payoff matrix, as well as an announcement on your computer monitor.

After the 45 seconds have ended, you are provided with a table recording your choice and group minimum value, along with your period payoff and its converted money amount. Please see Figure 2.

Figure 2

Values chosen and payoffs from all periods				
Period	**Your value**	**Group minimum value**	**Your payoff**	**Converted to USD**
1	7.00	5.00	90	0.90
2	3.00	2.00	70	0.70
3	3.00	1.00	50	0.50
4	1.00	1.00	70	0.70
5	3.00	2.00	29	0.29

Additionally, you are provided a table displaying the values chosen by all players in each previous round, along with your own value. Please see Figure 3. After you have finished reviewing the table, the next period will start.

Figure 3

Values chosen by all members from all periods					
Subject 1	**Subject 2**	**Subject 3**	**Subject 4**	**Subject 5**	**Subject 6**
1.00	4.00	4.00	1.00	3.00	5.00
1.00	5.00	1.00	3.00	6.00	2.00
4.00	5.00	6.00	6.00	4.00	6.00
5.00	5.00	6.00	4.00	4.00	4.00

Earnings

You will be paid at the end of the experiment in cash based on the sum of point earnings throughout the experiment. The conversion rate from points to USD is 100. That is

100 points = 1 USD

Apart from the converted point earnings, you are paid another $10 participation fee.

A quiz consisting of six questions will start before the experiment. Feel free to ask the experimenter questions during the quiz if you have any confusion. A full understanding of the experiment is important.

APPENDIX B

Continuous Time Experimental Instructions

Images of the instructions for the *Continuous Standard* and *Continuous Nudge* treatments are included in the following pages.

Game One Instructions

General

Thank you for agreeing to take part in this study. Please read the instructions we give you today carefully. A clear understanding of the instructions will help you make better decisions and increase your earnings. After reading the instructions, there will be ten questions to test your understanding. To guarantee everyone understands the experiment, you cannot move on to the next question if you do not enter the correct answer.

If you have any questions during the experiment, please raise your hand. Please do not consult with other participants in this room or look at the computer screens of other participants. Please turn your phone off or to silent mode now and place it on the floor, along with any other materials you have brought into the room.

At the beginning of the experiment, you will be randomly assigned into a group consisting of six people, you and five others. The other people in your group are sitting in this room. You and the other five group members will remain in the same group throughout today's experiment. You and any group members will remain anonymous. Please do not attempt to communicate with other participants in the room during the experiment.

Payoff

The amount of money each participant gets paid is different. It depends on your choice and the other five group members' choices. Details are as following.

This experiment has sixteen periods altogether. At the beginning of each period, you will be asked to pick a value of X. The values of X you may choose are 1, 2, 3, 4, 5, 6 or 7. The value you pick for X and the minimum value picked for X by any participant in your group, including your choice of X, will determine the payoff you receive. Figure 1 on the next page is an example of what this payoff matrix may look like.

The earnings in each period may be found by looking across from the value you choose on the left-hand side of the matrix and down from the minimum value chosen within group from the top of the matrix. For example, suppose the payoff matrix in Figure 1 is the current payoff matrix. If you choose a 4 and values chosen by five other members are 5, 5, 5, 4, and 3, then the

smallest value chosen in your group is 3, your current payoff is 80 points. In Figure 1, the payoff can be found at the cell in the fourth row and fifth column. The points will be converted into cash at the end of the experiment. 100 points equals 1 USD. Thus, the more points you earn, the more you will get paid.

Figure 1

Your value	Minimum value within group 7	6	5	4	3	2	1
7	130	110	90	70	50	30	10
6		120	100	80	60	40	20
5			110	90	70	50	30
4				100	80	60	40
3					90	70	50
2						80	60
1							70

You can change your value every 0.3 seconds. In other words, you or other participants can change the choice three or four times in one second. Periods will each last 45 seconds. During the 45 seconds, you can switch your chosen value as many times as you wish. The numbers in the payoff matrix are the period payoffs you would earn if you maintained the same action throughout the period. For instance (assuming the matrix in Figure 1 is the current payoff matrix), if you chose value 6 for the entire period and the minimum value chosen in your group stays at 2, you would earn 40 points.

Apart from your own value and the smallest value chosen within your group, your payoff in a period also depends on how much time is spent in each cell of the payoff matrix. For example, if you choose value 7 for the first 15 seconds and value 4 for the later 30 seconds while the minimum group value stays at value 2 for the whole period, then period accumulated earnings are calculated as follows. In the first 15 seconds you stayed at the 1st row and 6th column cell of the payoff matrix getting payoff 30, you accumulated (15 / 45) * 30 = 10 points. In the later 30 seconds, you stayed in the 4th row and 6th column cell getting payoff of 60, you accumulated (30 / 45) * 60 = 40 points. Your period accumulated payoff would be:
10 points + 40 points = 50 points.

In summary, the final payoff is accumulated by the time spent in each cell. The more time you spend in any one cell, the closer final payoff will be to the payoff in that cell. As in the above example, 50 points is closer to 60 than to 30. This is because you spent longer staying at the cell with payoff 60 than staying at the cell with payoff 30.

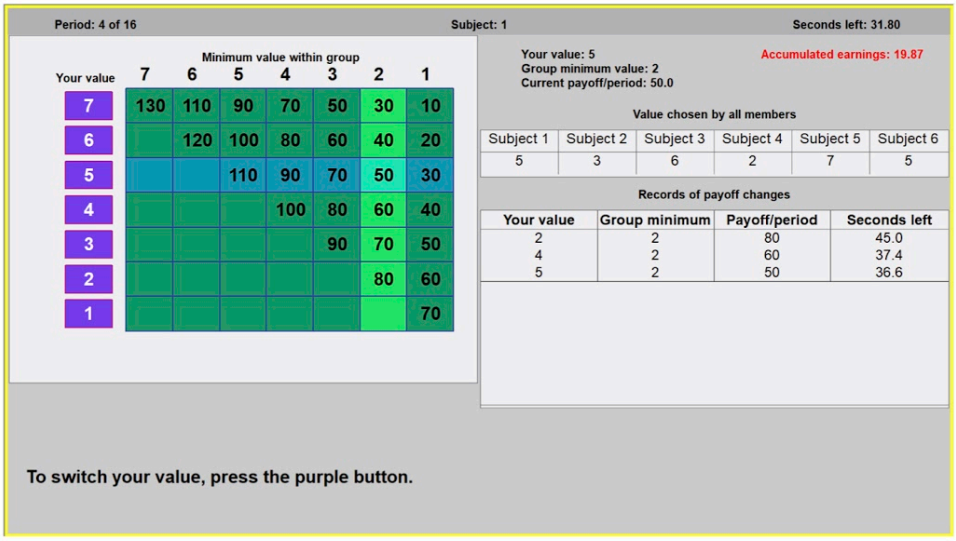
During the game, the payoff matrix could potentially change from the initial payoff matrix. Any change will be indicated by a significant change in the coloring scheme of the payoff matrix, as well as an announcement on your computer monitor.

Details on How to Use the Software

At the beginning of each period, each participant has to choose a starting value for X. After the starting values are picked by all participants in the room, the 45 seconds for the period begins and payoffs start to accumulate.

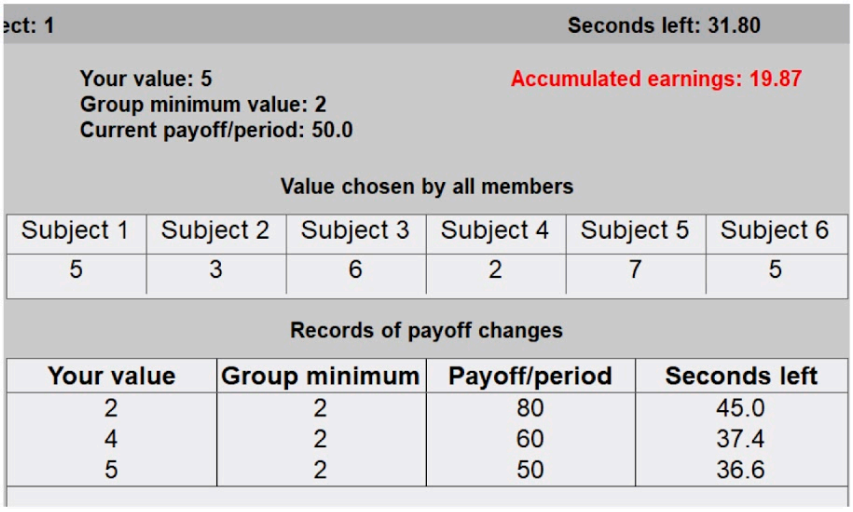
Figure 2 (on the next page) shows a screenshot from the experiment. The clock at the right top corner of the screen shows how much time is left in the period. The left side of the screen shows same payoff matrix as Figure 1. At any moment during the period, you will be able to change your action by clicking the purple buttons at the left side of screen. The row corresponding to your chosen action and the column corresponding to the group minimum will both be highlighted in different colors. For example, Figure 2 currently shows that you have chosen a value of 6 (the row highlighted in blue), while the group minimum is also 6 (the column highlighted in bright green).

Figure 2



The right-hand side of the screen displays your earnings during the period. A closer look at the right side of the screen is shown in Figure 3 on the next page. Your current accumulated earnings are displayed in red at the top. The accumulated earnings are updated every 0.3 seconds. The next three lines display your current chosen value, group minimum value, and current payoff per 45 seconds. The table below displays the value chosen by each participant. You can easily find your subject number at the top middle of the screen.

Figure 3



The table in the middle displays records of changed payoff and the time that change is made. For example, the first row shows that your starting value is 2 (or you chose 2 when 45 seconds are left) and the starting group minimum is 2 giving a current payoff of 80. You changed your value from 2 to 4 when 37.4 seconds were left in the period, and the group minimum remained the same. In other words, you spent 45.0 - 37.4 = 7.6 seconds playing the starting value 2 with the group minimum equals 2. The second row shows that you spent 37.4 - 36.6 = 0.8 seconds playing value 4 with the minimum equals 2, and so on.

At the end of each period, you are provided with another two tables. The first table lists your average value and group average minimum value, along with your period payoff and its converted money amount. Please see Figure 4. The second table records the average chosen value for all group members. Please see Figure 5. After you have finished reviewing the table, the next period will start.

Figure 4

Values chosen and payoffs from all periods				
Period	**Your average value**	**Group average minimum value**	**Your payoff**	**Converted to USD**
1	6.00	2.63	53	0.53
2	5.27	1.98	47	0.47
3	6.03	3.81	76	0.76
4	6.04	4.32	86	0.86
5	4.62	2.91	47	0.47
6	4.69	2.95	47	0.47

Figure 5

Average values chosen by all members from all periods					
Subject 1	**Subject 2**	**Subject 3**	**Subject 4**	**Subject 5**	**Subject 6**
3.97	3.00	3.00	2.00	6.00	5.00
5.00	4.95	6.38	3.90	6.00	4.05

Earnings

You will be paid at the end of the experiment in cash based on the sum of point earnings throughout the experiment. The conversion rate from points to USD is 100. That is

100 points = 1 USD

Apart from the converted point earnings, you are paid another $10 participation fee.

A quiz consisting of ten questions will start before the experiment. Feel free to ask the experiment administrator questions during the quiz if you have any confusion. A full understanding of the experiment is important.

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