Interdependence in Organizations and Laboratory Groups

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Dedication

To Mom, Dad, Stephanie, Abuelito, and Abuelita.

Everything I accomplish is because of you.

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Abstract

Interdependence arises in organizations when the appropriate action by an individual or group depends on what action others are taking. The following chapters examine cases of interdependence through the lenses of game theory and laboratory experiments. The research focuses on two games – the dirty faces game and the weak-link coordination game – that, while apparently very different, are in fact quite similar in key aspects. The most important of these is that (under some assumptions) in both games the efficient or "high effort" action is only optimal if it is the action being taken by everyone.

The dirty faces game is first presented, analyzed and tested experimentally to determine whether actual behavior conforms to the optimal, theoretically proposed outcome. This is shown not to be the case even in simple versions of the game. The latter chapters examine a possible means for inducing optimal behavior in the weak-link coordination game. This method involves starting with small group sizes, which are better suited for efficient coordination, and then growing large groups slowly. The effectiveness of this method is supported by an example from the field, theory, and experimental results.

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Chapter 1 Introduction

The study of interdependent goals in groups is of interest to both organizational researchers and economists. The organizations approach involves carefully examining and documenting the interdependence in behavior of individuals or groups of individuals within organizations (Barnard, 1938; March and Simon, 1958; Thompson, 1967; Heath, 1999). The focus of this research goes from operations on flight decks of aircraft carriers (Weick and Roberts, 1993) to the provision of health care (Gittell and Wimbush, 1998; Gittell and Fairfield, 1999). Another area of the organizational research looks at interdependence of goals between organizations or groups of firms (Khanna, Gulati, and Nohria, 1998; Human and Provan, 1997 & 1998). In economics, this type of interdependence is usually studied through coordination games (Camerer, in progress, Chapter 7; Cooper, 1999). Coordination games extend from simple matching games (Schelling, 1960; Bacharach and Bernasconi, 1997) to games like the battle-of-the-sexes (Cooper, DeJong, Forsythe and Ross, 1993) and stag hunt and weak-link games (Van Huyck, Battalio, and Beil, 1990; Battalio, Samuelson, and Van Huyck, 1997).

In both literatures, the problem is essentially the same. The optimal action by a member of a group depends on what everyone else is doing, and decision makers have an incentive to match the actions of others. The following chapters look at two different games where this type of coordination is important. In one of the games, the weak-link coordination game, the importance of solving this interdependence problem is obvious. In the other game, the dirty faces game, it is less so. Both games, however, share the key element that it is only optimal to take a particular action if everyone else is doing so as well.

The following chapter examines the dirty faces game. This game is derived from a popular puzzle present in the psychology and logic literatures. Starting from a description of the logic problem by Littlewood (1953), a formal description of the game is presented. The solution to the game is shown to make certain assumptions concerning the rationality of the players. Measuring the behavior of participants in the game allows a bound to be placed on the extent to which they are satisfying iterated dominance. This is because it is only optimal for players to behave "rationally" if they believe that the group of players playing the game will satisfy a certain number of steps of iterated dominance. Because of this, under certain assumptions the game can be shown to be a coordination game.[1] Experiments are reported that examine the actual behavior of experimental subjects in the game and compare it with the theoretical prediction.

Chapter 3 looks at a more commonly studied interdependence problem. While the main concern of this chapter is the interaction between coordination problems and growing groups (or organizations), it begins with a recognition of the fact that coordination is an important problem for firms and that it has been traditionally understudied by organizational researchers and economists. The weak-link coordination game is presented as a useful tool for examining problems of interdependence that arise frequently in organizations. Previous experimental studies on weak-link games have shown a strong group size effect: while small groups are able to solve the coordination problem almost always, large groups never do so. This result is tied in to the organizational literature on growing firms to argue that coordination failure is an important obstacle for organizations increasing in size. Possible solutions are discussed, with an emphasis on the possibility that the growth process itself can be manipulated to lead to successful growth. Motivated by a case study from the airline industry, a model is used to show that slow growth can lead to more efficiently coordinated large groups.

Chapter 4 proposes and conducts a test of the result that large successfully coordi-

[1] This is left for the concluding final chapter.

nated groups playing the weak-link game can be obtained through growth – starting off with a small group size and then adding players at a slow rate. The chapter reports the results of laboratory experiments intended to address this question.

In the conclusion, the results obtained from the above three chapters are summarized and directions for future research are proposed. A brief argument outlining why the dirty faces game can be thought of as a coordination game is also presented. Finally, some closing thoughts on the importance of and the best approach for studying interdependence are discussed.

Chapter 2 Uncommon Knowledge: An Experimental Test of the "Dirty Faces" Game

It is often the case that standard game theory makes unrealistic predictions concerning the behavior of players in a game. Support for this observation comes from the extensive experimental work on games. Instead of behaving according to theoretical predictions in these situations, it is frequently the case that subjects rely on other aspects of games or modify their selection principles according to unprescribed rules. For example, subjects' behavior may be affected by the way in which a game is framed or presented,[1] by other principles such as fairness or altruism,[2] or by a lack of common knowledge of complete rationality among the players. It is with the latter of these cases with which this chapter is primarily concerned.

Common knowledge of perfect rationality among the players in a game is important in much of equilibrium analysis. For example, iterated strict dominance, rationalizability, and backward induction rely on the assumption that it is commonly known that players are rational and will not play dominated strategies.[3]

This chapter studies the dirty faces game, a variant of an example originally developed by Littlewood (1953), in which players' knowledge and common knowledge of an event result in different equilibrium outcomes, similarly to Rubinstein's (1989)

[1]See, for example, Schotter, Weigelt and Wilson (1994) and Bacharach and Bernasconi (1997). Furthermore, Cooper, DeJong, Forsythe and Ross (1993); Güth, Huck, and Rapoport (1995a and 1995b); Rapoport and Fuller (1995); and Camerer, Knez and Weber (1996), all show that differences in timing of a game may affect play even without the usually assumed differences in information. These results are striking in that the timing of moves alone is generally considered inconsequential.

[2]McKelvey and Palfrey (1992) find support for this hypothesis in experimental tests of the centipede game. Furthermore, there is much evidence for this idea in the results of ultimatum and dictator games. The results of studies on these games are discussed extensively in Camerer and Thaler (1995).

[3]See Bernheim (1984), Pearce (1984), and Aumann (1995).

electronic mail game. Moreover, this equilibrium also relies on the presence of common knowledge of rationality among the players. There is one important difference between the dirty faces game and other games in which a player's optimal action depends on the number of steps of iterated rationality that others are satisfying such as the centipede game (McKelvey and Palfrey, 1992) and the p-beauty contest game (Nagel, 1995). In the dirty faces game the equilibrium resulting from common knowledge of rationality is Pareto-optimal and players can not increase their payoffs by having rational expectations about the bounded rationality of others. In the dirty faces game, everyone is best off when everyone is perfectly rational and this is commonly known.

How individuals will actually behave in this game is an important question for two reasons. The first reason is similar to the motivation for other experiments on iterated rationality. The number of iterations that individuals satisfy and that they believe others are satisfying have important implications for areas such as bargaining and mechanism design and implementation. The optimal behavior in a bargaining situation can be significantly changed if a party or parties are boundedly rational or believe others are. Similarly, the theoretical prediction about what behavior will result when a particular mechanism is implemented may be affected by relaxing the assumption of common knowledge of rationality. The second reason is unique to the dirty faces game and results from the fact that common knowledge of rationality results in the Pareto-optimal outcome in the general form of the game. This feature of the game is not shared by games used in previous experiments to study iterated rationality. In these other games, results indicating that subjects satisfy only a few steps of the process may be due to cognitive limitations, but they may also be due to the fact that Pareto-inferior outcomes arise when everyone is behaving as if common knowledge of rationality is satisfied. Therefore, previous experiments addressing the question of how many steps players can satisfy confound limits on iterated reasoning with a taste for improved social outcomes. In addition, if participants in experiments on the dirty faces game fail to satisfy many steps of iterated rationality even though it is in everyone's best interest to do so, future experiments can study how to improve players' ability to satisfy more steps and play the equilibrium, increasing efficiency.

The purpose of this study is to examine the behavior of actual participants in this game and compare it to the behavior predicted by the theory. The chapter is organized as follows. The next section discusses experiments similarly intended to address the question of whether or not common knowledge of rationality is satisfied in the laboratory. Immediately following, the dirty faces game will be discussed in detail and the experimental design used to test the game will be presented. The final sections will present the results of the experiments and provide a discussion of these as well as possibilities for future research.

2.1 Previous experiments on common knowledge of rationality

It is surprising that, in spite of the importance of common knowledge of rationality in game theory and the close relation between this field and experimental economics, more laboratory work has not been conducted to test this assumption. This section reviews the existing experimental work.

McKelvey and Page (1990) experimentally tested their previous result (McKelvey and Page, 1986) concerning the use of an aggregate statistic to convey information concerning individuals' posterior probabilities of an event and the convergence of these probabilities.[4] Their results show that, while participants in the experiment

[4]In the earlier paper, McKelvey and Page (1986) showed that results by Aumann (1976) and Geanakoplos and Polemarchakis (1982) concerning the equivalence of posterior probabilities under common knowledge can be maintained when only an aggregate statistic of the posteriors becomes common knowledge. This statistic need only satisfy stochastic regularity (which, for example, is satisfied by a statistic given by the mean of the posterior probabilities). Nielsen, Brandenburger, Geanakoplos, McKelvey and Page (1990) further extend this result to show that it holds not only for the conditional probabilities of an event, but equally for the conditional *expectations of a random*

used the publicly announced mean to update their beliefs, they did not do so in a manner consistent with the perfect Bayesian updating that the model suggests.[5]

Stahl and Wilson (1994 and 1995) used a series of games with varying properties to estimate the level of rationality for each of the participants, using their actions in the experiment. According to the models on which the experiments are based, players' types are determined by their action selection process and by their beliefs of the types of others. In one of these models, Level-0 type players simply randomize among their strategies, Level-1 type players assume that all other players are Level-0 types and respond accordingly, and Level-2 types believe that all other players are Level-0 and Level-1 types. A final type of players is described as behaving according to Nash equilibrium theory. Their results indicated that the number of Level-0 types in the experiment was negligible and that participants generally behaved as though they belonged to the higher types. They found support for the existence of a large percentage of sophisticated Nash types in the population.

In ongoing research, Costa-Gomes, Crawford, and Broseta (1998) use Mouselab technology[6] to record which payoff information subjects access when playing several two-player normal form games, and then use this information and subjects' decisions to determine their level of sophistication. While the analysis of the data generated by viewing patterns has not been completed, the strategy choices of subjects indicate that no more than two rounds of iterated dominance are being satisfied.

In perhaps the most clever design addressed at measuring the level of rationality

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variable. This result is also proven to hold when an aggregate statistic of the conditional expectations become common knowledge.

[5]Hanson (1996) points out flaws in McKelvey and Page's design, which invalidate the proof that the claimed Bayes-Nash equilibrium to the game is, in fact, such an equilibrium. Moreover, Hanson indicates that the fact that the Bayes-Nash equilibria to the game are not known invalidates the comparison between actual and predicted behavior.

[6]Mouselab is a computer interface in which payoff information is concealed from subjects until they use the mouse to reveal it. Thus, the experimenter can control the amount of information revealed to a subject at one time and, more importantly, can record the patterns in which subjects look up information. For more information on Mouselab, see Camerer, et al. (1993).

of players, as well as their beliefs concerning the rationality of others, Nagel (1995) studied a game previously discussed by Moulin (1986).[7] In these experiments, subjects were asked to select a number between 0 and 100. The average of these numbers was then computed, as well as a target number. The target number was the mean of the participants' choices multiplied by a constant, $0 < p < 1$, and the participant whose number was closest to this target number would then win the game and a predetermined prize. The unique Nash solution to this game, under the assumption that there is common knowledge between the participants of perfect rationality among the players, is for everyone to pick 0. The game is such, however, that if this necessary common knowledge assumption is not satisfied, it may no longer be optimal to behave according to the Nash prediction.[8] Nagel finds that it is never the case, for different values of p, that all subjects pick the equilibrium in the first play of the game. Instead, choices tend to be significantly higher, indicating that subjects are either not perfectly rational themselves, or are not certain that the infinite hierarchy of knowledge of rationality among the players is satisfied.

A further interesting aspect of Nagel's experiments, however, is that they make it possible to measure to what extent the hierarchy of subjects' beliefs over the rationality of other players is satisfied. For example, a choice greater than $100p$ indicates that a particular subject is not behaving rationally, since the target number can never be above this value. Furthermore, only a subject who does not believe that everyone else is rational would choose a number in the range $(100p^2, 100p)$, since this implies the belief that at least one of the other subjects will choose a value greater than $100p$. Hence, every choice greater than zero violates some form of the iterative process necessary to arrive at common knowledge of rationality. Nagel's results indicate that, while the majority of subjects do not exhibit violations of rationality in the first period, they only proceed through the above iterative process to two or three steps.

[7] A variant of this game has also been studied by Ho, Camerer, and Weigelt (1998).

[8] Note that it may be the case that all of the participants are aware of the unique equilibrium. However, if they are not sure that everyone else is aware of it, or that everyone else is aware that everyone is aware of it, then the failure of the Nash prediction may still occur.

Nonetheless, repeated play leads to adjustment towards the equilibrium in all cases.

One concern with interpreting this adjustment, however, is that a decrease in subjects' choices across periods may not represent an attainment of a higher level of iterative rationality. It may instead be the case that subjects are using a simple heuristic to guide their adjustment. For example, using a simple rule such as "choose p times the previous period's target number" will lead to convergence towards the Nash equilibrium in a manner similar to that observed in Nagel's experiments. However, it is hard to argue that such convergence indicates that more steps of the iterative process are being performed or that common knowledge of rationality is being approximated.

As the above experimental results indicate, certain aspects of the analysis underlying standard game theory may be unrealistic in that common knowledge of rationality among players may be not be present, at least initially, in an actual setting where games are played.

2.2 The dirty faces game

The game which this chapter concentrates on is one which is frequently present in the discussion of common knowledge and iterative reasoning. Littlewood (1953) presents the problem as follows:

> Three ladies, A, B, C, in a railway carriage all have dirty faces and are all laughing. It suddenly flashes on A: why doesn't B realize C is laughing at her?–Heavens! *I* must be laughable. (Formally: if I, A, am not laughable, B will be arguing: if I, B, am not laughable, C has nothing to laugh at. Since B does not so argue, I, A, must be laughable.)

> ... But further, what has not got into the books so far as I know, there is an extension, in principle, to n ladies, all dirty and all laughing. There is an induction: in the (n+1)-situation A argues: if I am not laughable, B, C, ... constitute an n-situation and B would stop laughing, but does not.

While this problem is present in much of the literature in several forms and under different names,[9] this chapter models it as a game in a manner similar to the way it is presented in Fudenberg and Tirole (1993).

The game can include any finite number of players, n. Each player's face is either "dirty" or "clean," as determined by nature. Players are aware of the states of the faces of other players, but not of their own face. In the situation, then, where all of the players have an incentive to choose a certain action when and only when they are certain that their face is dirty, an equilibrium can arise where, if all of the players' faces are dirty, they all act after exactly n periods, and no one acts before that time. As will be explained in more detail subsequently, however, this result relies significantly on the presence of common knowledge about the fact that at least one face is dirty and on common knowledge of rationality.

Formally, let $N = \{1, \cdots, n\}$. Assume that, for each player, $i \in N$, $x_i \in \{X, O\}$ represents that player's type as determined by chance according to a commonly known probability p. That is, with probability p, nature draws a player's type to be X, and with probability $1 - p$ the player is determined to be of type O. Each player's type is thus determined identically and independently of the types of the other players.

Assume next that the game consists of up to $T \geq n$ periods, in which each player chooses one of two actions, $\{U, D\}$, and that the game is over after any period in which any player chooses D. Further assume that players are faced with the following payoff table in each period of the game:

[9] See, for instance, Binmore and Brandenburger (1988) and Geanakoplos (1993).

		Type	
		X	O
Action	U	0	0
	D	α	$-\beta$

Table 2.1: Generic payoff table for dirty faces game

Hence, for α and β such that $p\alpha < (1-p)\beta$, a player will act when and only when she is certain that her type is X.[10]

In the game presented above, if at the end of each period all players observe the actions of all others, no player can learn anything about her own type in any period and should, therefore, never choose D, even when all the n players are of type X. This is because of the independence of player types and is true as long as the information each player receives at the beginning of the game is only the types of the other $n-1$ players. Thus, the equilibrium to the above game is for all players to select U in all periods.

Assume now, however, that a public announcement is made to the entire group of n players at the beginning of the game. In this announcement, all players are informed of whether there is at least one player whose type is X. Looking again at the case where all of the players are of type X, this announcement does not provide any of the players with new information, since they could already observe that everyone else is of type X and, therefore, that there is at least one individual whose type is X. What this announcement does accomplish, however, is to make this previously known fact common knowledge to all of the participants.[11]

[10] Assuming, not unreasonably, that she is either risk-neutral or risk-averse. This will be discussed in more detail later.

[11] In Littlewood's example, the announcement is replaced by the fact that all three women are laughing. Hence, since the laughter is observable by all of the women, who can in turn observe that they can all observe the laughter, it is commonly known that there is at least one person whose face is dirty when at least one person laughs out loud. In this example, the action D corresponds to ceasing to laugh, which any of the ladies would do immediately when she realized that her face was dirty.

Following the announcement, players now should be able to determine their own true type by the actions of the other players. Specifically, any players of type X will now be aware of their type – and will therefore choose D – in a finite number of periods.

Proposition 2.1 *Let N be a set of n risk-neutral or risk-averse players playing the dirty faces game with payoffs such that $p\alpha < (1-p)\beta$. Let k be the number of players of type X and let $K \subseteq N$ be the set of all players of type X. Then, for any n and k such that $0 < k \leq n$, in the unique Nash equilibrium to the game $s_i = U$ in all periods for which $t < k$ and, in period $t = k$, $s_i = U$ for all $i \notin K$ and $s_j = D$ for all $j \in K$. If $k = 0$, then $s_i = U$ for all $i \in N$.*

Proof. Remember that if $k \geq 1$, the announcement makes this common knowledge at the beginning of period 1. It is also true that, $\forall t$, if at the beginning of the period it is common knowledge that $k \geq t$, then if $k = t$, every player $i \in K$ will know that her type is X (since they see the $k-1$ other players of type X) and will select D while all the players not in K will choose U. If, on the other hand, $k > t$, then all the players will observe at least t other players of type X, everyone will choose U, and this will make it commonly known that $k > t$ at the end of the period. Thus, everyone will choose U in all periods t, such that $t < k$, and in period $t = k$, $s_i = D$ $\forall i \in K$ and $s_j = U$ $\forall j \notin K$. If $k = 0$, the announcement will make it common knowledge that there are no players of type X and all players will select U in every period.

To see why this is unique, note that a player will choose D if and only if she believes her type is X with probability greater than $\frac{\beta}{\alpha+\beta}$. Since there is only one way for updating to affect players' beliefs, the only way this can happen is if it is common knowledge that there are k players of type X and the player observes exactly $k-1$ other players of type X. This can only happen in period t and for players of type X.

Q.E.D.

An example will help make this clear. Assume that $n = \{1, 2, 3\}$ and refer to the three players as 1, 2, and 3. The state space and information structure are then as follows:[12]

$$\begin{aligned}
\Omega &= \{OOO, OOX, OXO, XOO, OXX, XXO, XOX, XXX\}; \\
p &= \{(1-p)^3, (1-p)^2p, (1-p)^2p, (1-p)^2p, (1-p)p^2, (1-p)p^2, (1-p)p^2, p^3\}; \\
\mathcal{P}_1 &= \{\{OOO, XOO\}, \{OXO, XXO\}, \{OOX, XOX\}, \{OXX, XXX\}\}; \\
\mathcal{P}_2 &= \{\{OOO, OXO\}, \{XOO, XXO\}, \{OOX, OXX\}, \{XOX, XXX\}\}; \\
\mathcal{P}_3 &= \{\{OOO, OOX\}, \{XOO, XOX\}, \{OXO, OXX\}, \{XXO, XXX\}\}; \\
\mathcal{P}_1 \vee \mathcal{P}_2 \vee \mathcal{P}_3 &= \{\{OOO\}, \{OOX\}, \{OXO\}, \{XOO\}, \{OXX\}, \{XXO\}, \{XOX\}, \{XXX\}\}; \\
\mathcal{P}_1 \wedge \mathcal{P}_2 \wedge \mathcal{P}_3 &= \{OOO, OOX, OXO, XOO, OXX, XXO, XOX, XXX\};
\end{aligned}$$

Now, assume that $\omega = XXX$ (all three players are of type X). Thus, $\mathcal{P}_1(\omega) = \{OXX, XXX\}$, $\mathcal{P}_2(\omega) = \{XOX, XXX\}$, and $\mathcal{P}_3(\omega) = \{XXO, XXX\}$, and only Ω can be common knowledge. This makes it impossible for players to learn anything from the behavior of others. Furthermore, no player is aware of the true state, even though all players know that there are at least two players of type X.

However, the inclusion of the announcement that there is at least one player whose type is X modifies the information structure in the following way:

[12]The state space and information structure (and the subsequent formal interpretation of common knowledge) are defined using the following characterization by Aumann (1976) of common knowledge, where $\mathcal{P}_1(\omega)$ represents the partition of the state space which Player 1 is aware of when the true state is ω:

Let $(\Omega, \mathcal{B}, p)$ be a probability space, $\mathcal{P}_1$ and $\mathcal{P}_2$ partitions of Ω whose join (the coarsest common refinement) $\mathcal{P}_1 \vee \mathcal{P}_2$ consists of non-null events. Given ω in Ω, an event A is called common knowledge at ω if A includes that member of the meet (the finest common coarsening) $\mathcal{P}_1 \wedge \mathcal{P}_2$ that contains ω.

$$\begin{aligned}
\mathcal{P}_1 &= \{\{OOO\},\{XOO\},\{OXO,XXO\},\{OOX,XOX\},\{OXX,XXX\}\};\\
\mathcal{P}_2 &= \{\{OOO\},\{OXO\},\{XOO,XXO\},\{OOX,OXX\},\{XOX,XXX\}\};\\
\mathcal{P}_3 &= \{\{OOO\},\{OOX\},\{XOO,XOX\},\{OXO,OXX\},\{XXO,XXX\}\};\\
\mathcal{P}_1 \wedge \mathcal{P}_2 \wedge \mathcal{P}_3 &= \{\{OOO\},\{OOX,OXO,XOO,OXX,XXO,XOX,XXX\}\};
\end{aligned}$$

and the event $\neg\{OOO\}$ is now common knowledge.[13]

Given that $\neg\{OOO\}$ is now commonly known, then all players know that if it is the case that the true state ω lies in the set $\{XOO, OXO, OOX\}$, then the one player who is of type X will be aware of this and will choose D in the first period. Hence, when no player does this, the events $\neg\{XOO\}, \neg\{OXO\}$, and $\neg\{OOX\}$ then become common knowledge as well and the information structure is as follows:

$$\begin{aligned}
\mathcal{P}_1 &= \{\{OOO\},\{XOO\},\{OXO\},\{OOX\},\{XXO\},\{XOX\},\{OXX,XXX\}\};\\
\mathcal{P}_2 &= \{\{OOO\},\{OXO\},\{XOO\},\{OOX\},\{XXO\},\{OXX\},\{XOX,XXX\}\};\\
\mathcal{P}_3 &= \{\{OOO\},\{OOX\},\{OXO\},\{XOO\},\{XOX\},\{OXX\},\{XXO,XXX\}\};\\
\mathcal{P}_1 \wedge \mathcal{P}_2 \wedge \mathcal{P}_3 &= \{\{OOO\},\{OOX\},\{OXO\},\{XOO\},\{OXX,XXO,XOX,XXX\}\};
\end{aligned}$$

Thus, at the beginning of Period 2, it is commonly known that there are at least two players of type X, or, equivalently, that the true state is in $\{OXX, XXO, XOX, XXX\}$. Now if it were the case that there are only two such players, both of them would know immediately because of the new information structure and would select D in the second period. When this does not happen, however, then all players become aware of the fact that it is not the case that there are only two players of type X. The information structure is now:

[13] It is also worthwhile to note that it is important that the information structure itself is common knowledge, since this is necessary for the players to be able to proceed through the following steps of reasoning.

$$\begin{aligned}
\mathcal{P}_1 &= \{\{OOO\},\{XOO\},\{OXO\},\{OOX\},\{XXO\},\{XOX\},\{OXX\},\{XXX\}\};\\
\mathcal{P}_2 &= \{\{OOO\},\{OXO\},\{XOO\},\{OOX\},\{XXO\},\{OXX\},\{XOX\},\{XXX\}\};\\
\mathcal{P}_3 &= \{\{OOO\},\{OOX\},\{OXO\},\{XOO\},\{XOX\},\{OXX\},\{XXO\},\{XXX\}\};\\
\mathcal{P}_1 \wedge \mathcal{P}_2 \wedge \mathcal{P}_3 &= \{\{OOO\},\{OOX\},\{OXO\},\{XOO\},\{OXX\},\{XXO\},\{XOX\},\{XXX\}\};
\end{aligned}$$

Therefore, in the third period, the event $\{XXX\}$ is now common knowledge and all players are aware of their own type. Thus, in Period 3, all players should select D, bringing the game to an end.

An important consideration, however, is that common knowledge of rationality among the players is overwhelmingly important in the above analysis. Otherwise, for example, the failure of any player to choose D in Period 1 might be attributed to a lack of rationality rather than to the fact that no player observes two players of type O. In this case, the next step in which it is commonly known that there are at least two players of type X is not reached.

As an illustration of this point, consider the case where $N = \{1, 2\}$ and let $\omega = XX$. Further, define the events

$$\begin{aligned}
\mathbf{R} &\equiv \text{Everyone is rational;}\\
\mathcal{K}^1(\mathbf{R}) &\equiv \text{Everyone knows that everyone is rational;}\\
\mathcal{K}^2(\mathbf{R}) &\equiv \text{Everyone knows that everyone knows,}\\
&\quad\ \text{that everyone is rational;}
\end{aligned}$$

and so on, so that $\mathcal{K}^l(\mathbf{R})$ corresponds to l iterations of the knowledge process. Now, if $\mathbf{R}$ holds, then it will be the case that each of the two players will choose D in Period 1 if and only if they observe the type of the other player to be O. However, this is not sufficient for the above equilibrium to hold. In order for it to then become common knowledge that both players are of type X, it must be the case that the event $\mathbf{R}\&\mathcal{K}^1(\mathbf{R})$ is true. Otherwise, the fact that the other player did not choose D might be attributed to a lack of rationality. However, if $\mathbf{R}\&\mathcal{K}^1(\mathbf{R})$ is satisfied, then this is sufficient for both players to become aware of the true state and choose D in Period 2.

The three player case proceeds similarly. Assume, as before, that the true state is $\omega = XXX$. Then, if $\mathbf{R}$ is true, players will choose D if and only if they observe two players of type O. As long as $\mathbf{R}\&\mathcal{K}^1(\mathbf{R})$ is also true, given that everyone chose U in Period 1, it will be known by everyone that there are at least two players whose type is X. Thus, since $\mathbf{R}$ holds, each player will choose D if and only if they observe one player of type X and one player of type O. Note, however, that in order for everyone to know that everyone knows that there are at least two players of type X, it is necessary that $\mathbf{R}\&\mathcal{K}^1(\mathbf{R})\&\mathcal{K}^2(\mathbf{R})$ be true. Therefore, this must also be the case if, after observing that everyone chose U, it is to be known by all players that the true state is XXX. It must be the case that $\mathbf{R}\&\mathcal{K}^1(\mathbf{R})\&\mathcal{K}^2(\mathbf{R})$ is satisfied in order for the predicted equilibrium to arise when the true state is XXX.

This result can be generalized, by induction, to the case where there are n players and all of them are of type X. In order for the correct equilibrium to arise in this case, it is sufficient that $\mathbf{R}\&\mathcal{K}^1(\mathbf{R})\&\cdots\&\mathcal{K}^{n-1}(\mathbf{R})$ is true.

Note, however, that the above statement need not be true in order for the equilibrium outcome to arise in other cases. For example, in the $n = 3$ case, all that is necessary when $\omega \in \{XOO, OXO, OOX\}$ is that $\mathbf{R}$ hold. Thus, if everyone is rational, the one player who observes two players of type O will know her own type and choose D in the first period and the other two players will select U, regardless of whether they think the other players are rational or not. Furthermore, if the true state is in the set $\{XXO, XOX, OXX\}$, then all that is necessary is that $\mathbf{R}\&\mathcal{K}^1(\mathbf{R})$ be true.

More generally, regardless of n, in the case where there are exactly k players whose type is X, what is sufficient for the correct equilibrium to arise is that $\mathbf{R}\&\mathcal{K}^1(\mathbf{R})\&\cdots\&\mathcal{K}^{k-1}(\mathbf{R})$.[14] Nonetheless, the complexity of the problem is not the same in all situations where k is equal. To see this, compare, using $n = 2$ and $n = 3$ as examples, the case where $k = 2$ and the true states are XX and XXO. In the two player game, the following are necessary: 1) 1 is rational, 2) 2 is rational, 3) 1 knows that 2 is rational, and 4) 2 knows that 1 is rational. However, in the case where there are three players, it must be true that: 1) 1 is rational, 2) 2 is rational, 3) 3 is rational, 4) 1 knows that 2 is rational, 5) 1 knows that 3 is rational, 6) 2 knows that 1 is rational, 7) 2 knows that 3 is rational, 8) 3 knows that 1 is rational, and 9) 3 knows that 2 is rational. In both cases, however, it is only the condition $\mathbf{R}\&\mathcal{K}^1(\mathbf{R})$ which is being satisfied. Therefore, the number of conditions which must be satisfied in the problem grows exponentially in the number of players.[15]

It is this reliance on the common knowledge of rationality among players that is the focus of this chapter. Moreover, as it is often the case that a laboratory setting provides an excellent environment for testing theoretical assumptions underlying normative theory, the remainder of this chapter will concern itself with an experimental test of the game and of these assumptions.

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[14]In the trivial case where $n = 1$, this is still true since all that is necessary is $\mathbf{R}$ so that when the announcement is made, the single player will immediately know her type and will act accordingly.

[15]However, if you consider the fact that players can ignore completely the actions (and rationality and knowledge) of players which they observe to be of type O, then the problems are identical in complexity. In the XXO case, for example, Players 1 and 2 can ignore Player 3 since it is only the assumptions about the other player of type X which are significant. If Player 3 is not rational, however, it could still be possible for her to choose D, leading to a non-equilibrium outcome.

2.3 Experimental Design

In order to test the situations where the necessary iterated levels of knowledge are the fewest, experiments were conducted using the $n = 2$ and $n = 3$ cases. The choice of parameters α, β, and p proved to be a more difficult decision. This was because the condition $p\alpha < (1-p)\beta$ had to be satisfied while it was also a goal to maximize p in order to produce the most instances of the case where all players are of type X and to minimize the occurrence of the trivial situation where no players are of type X.[16] Furthermore, α had to be made large enough so that subjects would stand to earn a significant, or at least reasonable, sum by choosing D once they knew that their type was X. However, increasing α also meant that β had to be increased so that the above inequality would hold. Since it was not desirable to have the possibility of negative earnings for the experiment for any subject,[17] the value of β could not be increased unboundedly.

These considerations resulted in the choice of parameters being $p = 0.8$, $\alpha = \$1.00$, and $\beta = \$5.00$, and the resulting payoff table presented in Table 2.2.[18] It was decided that the possibility of negative earnings in an experiment would be compensated for by a participation bonus.

The expected monetary value of an uniformed selection of D, therefore, was -$0.20, while choosing U would always yield $0.00. While this difference does not appear large, the belief that this gamble will not be taken by many participants finds further support in the extensive results indicating that subjects choose according to

[16]It could be argued that the case where only one player is of type X is also trivial, since this player, upon hearing the announcement is immediately aware of her type and should therefore choose D. This requires, however, that the player be rational, which is a testable assumption.

[17]Kahneman and Tversky (1979) show that the behavior of subjects' preferences over losses is substantially different from that of preferences over similar gains. Furthermore, implementing negative participant earnings in an experiment would have created additional design problems.

[18]The equilibrium to the game is unchanged by this choice of parameters since all that is necessary is that $p\alpha < (1-p)\beta$. Since a player's beliefs concerning her own type will only change if she becomes certain that her type is X (i.e., if it becomes common knowledge that there are k players of type X and she observes only $k-1$), then no player will act before then. Therefore, the equilibrium holds for these payoffs and this value of p.

a value function which places greater weight on losses than on gains, and which is concave for gains.[19]

Thus, since increasing the difference would have to be accompanied either by a decrease in p or by a higher participation bonus, the difference created by these parameters was judged to be sufficient.

The random determination of players' types according to p was implemented using a ten-sided die. A roll of the die resulting in 1 or 2 meant that a player was of type O, while the remaining faces corresponded to the type X. Thus, in the two player case, the probabilities of obtaining the situation where there are 0, 1, and 2 players of type X were 0.04, 0.32, and 0.64, respectively, while for the three player case, the probabilities of having 0, 1, 2, and 3 players of type X were 0.008, 0.096, 0.384, and 0.512, respectively. Hence, for both experiments, the desired outcome was the most likely and the expected occurrence of the trivial problem was minimized.

		Type	
		X	O
Action	U	0.00	0.00
	D	1.00	-5.00

Table 2.2: Payoffs for experiments

[19]See Kahneman and Tversky (1979). Moreover, in more recent work, Kahneman and Tversky (1992) construct the following value and weight functions for lotteries where a value x is awarded with probability p:

$$\begin{aligned} v(x) &= \begin{cases} x^\alpha & \text{if } x \geq 0 \\ -\lambda(-x)^\beta & \text{if } x < 0 \end{cases} \\ w^+(p) &= \frac{p^\gamma}{(p^\gamma + (1-p)^\gamma)^{1/\gamma}} \\ w^-(p) &= \frac{p^\delta}{(p^\delta + (1-p)^\delta)^{1/\delta}} \end{aligned}$$

and, using experimental data, they estimate values for the parameters: $\alpha = \beta = 0.88$, $\lambda = 2.25$, $\gamma = 0.61$, and $\delta = 0.69$. Using these values, the gamble under consideration in this experiment has a certainty equivalent of -\$1.78.

In order to convince the subjects that the process by which types were determined was not controlled by the experimenter and possibly predetermined, a monitor was randomly selected from among the participants. This was done by having each subject roll the same die which would later be used to determine the types of the participants and selecting the participant who rolled the highest number. In this way, subjects were also given the opportunity to observe rolls of the die.

Each experiment consisted of three sessions, where each session corresponded to a new game and a new draw of player types. Each session consisted of $n+1$ periods. The first session was labelled as a practice round, intended to familiarize the subjects with the procedure of the experiment. For this session, the payoffs were divided by 5 and no announcement was made regarding the players' types.

An interesting design problem arose in selecting the procedure by which to inform subjects of the types of the other players. The original problem as presented by Littlewood considers the situation where a player's face is either dirty or clean and, therefore, this state can be observed by everyone in the room other than the player herself. From an experimental standpoint, however, such a design implies that subjects, in observing each other, might be able to determine their own type by observing non-verbal cues obtained from other subjects, such as a look of astonishment. Therefore, the following design was used.

In order to prevent subjects from observing the identity of those they were playing with, each experiment consisted of at least two groups and the identity of subjects in a particular group was not revealed. Before the first session, subjects were randomly assigned a participant number which indicated to them their group, denoted by a letter, and their player number within the group. Thus, subjects were only made aware of the participant numbers of the other players in their group, and not of their identity.

At the beginning of each session, the monitor rolled the die to determine the type of each participant while hidden from the other participants behind a screen.[20] For each session the monitor was given a new Type Sheet, which contained all of the participant numbers along with a blank box next to each number. After each roll, the monitor would record either an "X" or an "O" in the box corresponding to that participant. The sheet would then be placed inside of a simple display box. The display box consisted of a cardboard box and a cardboard sheet with flaps. The Type Sheet was placed face up inside the box and the cardboard sheet was placed over it. In this manner, each of the flaps could then be raised to show only the type of any desired subject. In the two experimental sessions, the experimenter, at this point, made one of two possible announcements at the front of the room for each group. In the case where, say for Group A, all of the participants were of type O, the experimenter would announce, "There are no participants of type X in Group A." Otherwise, the announcement would be, "There is at least one participant of type X in Group A." Thus, common knowledge about this important piece of information was established for each group.

Following the announcements, the experimenter then proceeded to each subject and lifted the flaps so that they could observe the types of the other one or two participants in their group. Each subject then recorded this information on their Record Sheet for that session. Following that, the experiment proceeded to the first period.

In each period, every subject would select one of two actions, either "Up" or "Down." The subject recorded this choice on a Reporting Sheet. Each subject, following the determination of participant numbers, was given a stack of Reporting Sheets, each one of which contained the participant's number, as well as three boxes which could be checked. The first two boxes were for the action choices of Up or Down. The third box was labelled "Session Over" and subjects were instructed to only check this box once any of the participants in their group had chosen Down in a previous period. This was implemented to prevent participants from learning the

[20]Instructions are available in Appendix B.

identity of others in their group by their failure to mark on their Reporting Sheet in a period. The Reporting Sheets were then torn from the stack and collected by the experimenter.

Once all of the Reporting Sheets were collected and sorted, the entire set of actions was written on the board at the front of the room and read out loud twice by the experimenter. Subjects where then instructed to record the actions of the other players in their group on their session Record Sheet and proceed to the next period.

One more period than necessary was included in both experiments so that the number of periods would not serve as a cue for the desired behavior. Thus, both experiments were conducted with $n+1$ periods. Once the game arrived at Period $n+1$, behavior could no longer be consistent with the theoretical prediction.

At the end of the last period, the experimenter removed the Type Sheet from the display and proceeded to publicly display the entire sheet to all participants. At this time, subjects were instructed to record their own type as well as to verify that the information that they received at the beginning of the session, concerning the types of the other players in their group, was correct. After everyone calculated their earnings for that session, the experiment proceeded to the next session.

Upon the completion of the third session, participants were privately paid, in cash, their earnings in all three sessions plus an $11 participation bonus. This bonus guaranteed that no participant could finish the experiment with a negative total. Thus, the possible earnings in the experiment ranged from $0 (which would only occur if a subject was of type O in all three sessions and picked Down all three times) to $13.20. The experiment typically lasted about one hour in both the $n = 2$ and $n = 3$ cases.

The experiments were conducted in March and April 1996 using as subjects graduate and undergraduate students at the California Institute of Technology with little or no formal training in game theory. These subjects do not necessarily represent a typical population in terms of quantitative and reasoning ability. For this particular study, however, this does not present a drawback since the study is intended to analyze whether seemingly extreme predictions about the rationality within a population can be supported by laboratory results. Failure by subjects from this population to behave according to the theoretical prediction, therefore, would indicate that this failure of the normative prediction would apply equally, if not more strongly, to a majority of other populations.

2.4 Results

2.4.1 Group Behavior

The complete results for both the $n = 2$ and $n = 3$ experimental sessions are given in Tables 2.3 and 2.4. There are a total of 13 and 14 groups for the two and three player games, respectively. The first row for each session indicates the type of each player as determined by the roll of the die. Subsequent rows contain the actions selected by each player, U for Up and D for Down.[21]

The first, and perhaps most striking observation is the extent to which subjects over-play the strategy Down when they have no information, beyond their prior, indicating their type.[22] This uniformed action by the participants results in that, for

[21] A reporting error was committed by the experimenter in the second session for the second group in Set 4 of the 2 player game. At the end of the second period, the experimenter erroneously reported that one of the two participants had selected Down when, in fact, both subjects had chosen the action Up. Furthermore, neither of the two participants indicated that they realized this error had occurred and instead recorded that the other participant had chosen Down. Although the session was thus prematurely terminated for that group, both subjects had failed to behave according to the equilibrium prediction and, therefore, their third period actions would not have been informative. Furthermore, since the error occurred at the end of the second session and the experiment ended immediately following the conclusion of this session, there is no possibility that the reporting error affected subsequent actions.

[22] This is also true in the initial training session, the results of which are not reported in the tables. In these sessions, since no announcement was made, expected payoff maximizing subjects should never have chosen Down. However, 12 out of 26 subjects in the $n = 2$ condition and 20 out

		Exp. 1 (3 groups)						Exp. 2 (4 groups)							
Sess.	Player	A1	A2	B1	B2	C1	C2	A1	A2	B1	B2	C1	C2	D1	D2
I	Type	X	X	X	X	X	O	X	X	X	X	X	X	X	O
	1	U	U	U	U	D	U	U	D	U	U	U	U	D	U
	2	U	D	U	D					U	U	D	U		
	3									U	U				
II	Type	X	O	X	X	X	X	X	O	X	X	X	O	X	X
	1	D	U	U	U	U	U	U	U	U	U	D	U	U	U
	2			D	D	D	D	U	D	U	U			U	D
	3									D	U				

		Exp. 3 (2 groups)				Exp. 4 (4 groups)							
Sess.	Player	A1	A2	B1	B2	A1	A2	B1	B2	C1	C2	D1	D2
I	Type	X	X	X	O	X	X	X	X	X	X	X	X
	1	U	U	D	U	U	U	D	U	D	U	U	U
	2	U	D			D	D					D	U
	3												
II	Type	X	O	X	X	X	O	X	X	X	X	X	X
	1	D	U	U	U	D	U	U	U	D	U	U	U
	2			U	D			U	U			D	D
	3							*	*				

Table 2.3: Results for two player game

the $n = 2$ condition, only 14 of 18 cases where there are two players of type X reach the second period. Even more surprisingly, none of the three player groups reaches the third period, even though there are 12 cases where all players are of type X.

This result may be due to several reasons. First of all, it may be unlikely that subjects calculate the expected payoff, but instead focus on the prior probability of 0.8. That is, participants in the experiment may view the uniformed decision as having an 8 out of 10 chance of a "good" payoff and a 2 out of 10 chance of a "bad" payoff and, therefore, ignore the sizes of the payoffs. This may be particularly true since, if someone else in their group selects Down before they do, the participant will not be able to realize any payoffs. Additionally, it is likely the case that the subjects view the additional time involved with additional periods as having negative utility. There-

of 42 subjects in the $n = 3$ condition chose Down in some period. This result, stronger than in later sessions, can be in part attributed to the payoffs being considerably small in the first session and to the fact that subjects were able to end the session by choosing Down.

Sess.	Player	Exp. 1 (3 groups) A1	A2	A3	B1	B2	B3	C1	C2	C3	Exp. 2 (3 groups) A1	A2	A3	B1	B2	B3	C1	C2	C3
I	Type	X	X	X	O	X	X	X	X	X	X	X	X	X	X	X	X	X	O
	1	U	U	U	U	D	U	U	D	U	U	D	D	D	D	U	U	U	U
	2	D	U	U													U	D	D
	3																		
	4																		
II	Type	X	O	X	X	O	O	O	O	X	X	O	X	O	X	X	X	X	O
	1	U	U	U	D	U	U	U	U	D	U	D	U	U	D	U	U	D	D
	2	D	U	U															
	3																		
	4																		

Sess.	Player	Exp. 3 (3 groups) A1	A2	A3	B1	B2	B3	C1	C2	C3	Exp. 4 (2 groups) A1	A2	A3	B1	B2	B3
I	Type	X	X	X	O	O	X	X	O	X	X	X	X	O	X	X
	1	U	D	D	U	D	D	U	U	U	D	U	U	U	U	U
	2							D	U	D				U	D	U
	3															
	4															
II	Type	X	X	X	X	X	X	X	X	X	O	X	O	X	O	O
	1	U	U	D	U	U	U	U	U	U	D	D	U	D	U	U
	2				U	D	U	D	U	U						
	3															
	4															

Sess.	Player	Exp. 5 (3 groups) A1	A2	A3	B1	B2	B3	C1	C2	C3
I	Type	X	X	O	X	X	X	X	O	O
	1	U	U	U	D	U	U	D	D	U
	2	D	U	U						
	3									
	4									
II	Type	X	X	X	O	X	X	X	X	X
	1	U	D	U	U	U	U	U	D	U
	2				D	D	D			
	3									
	4									

Table 2.4: Results for three player game

fore, as long as at least one person in each group does so, choosing Down guarantees that the session will end more quickly. Moreover, it may be the case that subjects do not view the determination of types for each participant as being independent of the types of the other participants. There is some support for this in that, for the 3 player condition, over-playing Down is more frequent when subjects observe two players of type X as opposed to one player of each type. This provides weak evidence that participants are behaving as though the types within a group are positively correlated. Furthermore, over-playing the action Down is considerably more frequent in the $n = 3$ condition, where subjects play D when they have no information about their type 30 percent of the time, compared to 8 percent of the time in the $n = 2$ case. This perhaps implies that more subjects are randomizing in the 3 player game, where the solution is less transparent. Finally, there is weak evidence that the over-playing decreases across sessions. For $n = 2$ the decrease is from 12 percent in the first session to 4 percent in the second, while for $n = 3$ the decrease is from 33 percent to 26 percent. Similarly to the previous explanation, this observed decrease also implies that the cause of the over-playing may be confusion which leads more players to randomize both in earlier periods and in a more confusing situation (the three player game). While the above are all possible explanations for the observed result, it is most likely that a combination of these is responsible for the subjects' behavior.[23]

Tables 2.5 and 2.6 provide some summary results for the two conditions. The left side of each table reports the occurrence of each composition of types within a group. Since the game is symmetric, no distinction is made between participants in a group. Therefore, the case where subject A1 is of type X and subject A2 is of type O is identical to the situation where A1's type is O and A2's type is X. The frequencies of the different draws indicate that the results of the random process approximated the expected frequencies. Furthermore, the modal outcome in both conditions occurred, as expected, where all of the players are of type X. Also note that it was always the case that there was at least one player of type X in a group. Therefore, the announcement was always that there is at least one player of type X in the group for all groups.

Types	n	Predicted Behavior	Actual Behavior	
OO	0			
XO	8	(DU)	7	(0.88)
XX	18	(UU)(DD)	4	(0.22)
Total	26		11	(0.42)

Table 2.5: Summary of group results for $n = 2$

[23]An alternative design might have helped reduce the over-playing of down. For instance, not ending the game after any subject plays D and instead letting the game continue for $n + 1$ periods regardless of the players' actions would have eliminated the possibility that players might play D to end the game earlier. However, this benefit was not apparent until after the experiments were conducted and the over-playing of D was observed. In addition, using a design such as this would have allowed players to play strategies in which they alternated between choosing D and U, complicating the analysis.

Types	n	Predicted Behavior	Actual Behavior	
OOO	0			
XOO	6	(DUU)	3	(0.50)
XXO	10	(UUU)(DDU)	1	(0.10)
XXX	12	(UUU)(UUU)(DDD)	0	(0.00)
Total	28		4	(0.14)

Table 2.6: Summary of group results for $n = 3$

On the right-hand side of Tables 2.5 and 2.6, a summary of the behavior across groups is provided, aggregating between the two experimental sessions. The column *Predicted Behavior* refers to the expected behavior of the group given its composition. For example, in the XX case for $n = 2$, $((UU)(DD))$ indicates that both players should choose Up in the first period and Down in the second. The next column provides the actual number of groups which behaved according to this prediction, as well as the corresponding frequencies. Note that while the number of groups whose behavior corresponds to the theoretical prediction is low in both conditions, this is particularly true for the three player game. In fact, while for the two player case 42 percent of the groups behave according to what is predicted, this is true of only 14 percent of the groups when $n = 3$. Additionally, none of the 12 groups in the XXX situation behave according to the theory. More surprisingly, the predicted behavior occurs in only 50 percent of the groups with types XOO, where only one of the players knows her type and should choose Down (as long as she is rational).[24] The results in the tables indicate quite convincingly that group behavior does not conform to the predicted play in either of the games.

An interesting question is whether or not the failure of groups to behave according to theory differs systematically between sessions and across the two games. In the

[24]These figures could be said to be biased downward if playing Down without information can be classified as "rational" for any of the aforementioned reasons. However, it is not unreasonable for this analysis to treat this type of behavior as a deviation from the theory since it implies a violation of established properties of choice.

two player game, for instance, 31 percent of the groups in the first session behave as predicted, while for the second session, this is true of 54 percent of the groups. Similarly, for the three player game, 7 percent of the groups in Session I and 21 percent in Session II behave accordingly. These differences, however, are not significant. A Fisher Exact Test of the null hypothesis that behavior is the same across sessions produces p-values of 0.218 and 0.298 for the 2 and 3 player games, respectively. There is a difference, however, between groups in the two conditions. In the two player game, groups behave according to the theory 42 percent of the time, while this is true for only 14 percent of the observations in the 3 player game. This difference is significant in both a Fisher Exact Test ($p = 0.022$) and a Chi-Square Test ($p < 0.05$).[25] In summary, the results provide weak evidence for effects of both group size and experience on whether or not groups behave according to what is predicted.

Returning to the situation in the three player game where the types are XOO and the player of type X, therefore, knows her type, it is worthwhile to re-examine the result that the groups behaved according to the prediction only one-half of the time. Although this is surprising since the solution appears to be obvious, it is true that in each of these cases the player who knew her type did choose Down in the first period. The failure of the groups to behave according to the theory, then, is the result of the previously observed over-playing of the action Down by other members of the group, and does not imply that the player who knew her type behaved irrationally. For this reason, it seems appropriate to further examine behavior on an individual, rather than group, basis.

[25] However, the significance levels are possibly exaggerated since both tests rely on the independence of the observations, which is not present in this sample across two sessions. Despite this limitation, pooling data within groups is necessary for the tests to have power and, furthermore, common in practice.

2.4.2 Individual Behavior

In order to examine individual behavior, and whether it corresponds to predicted behavior, it is necessary to consider the information held by players when they make their choice. Thus, a player choosing an action at any time may or may not be behaving according to the theory, depending on what this player has previously observed with regard to the types and actions of other players. Tables 2.7 and 2.8 present the behavior of players conditional upon their information. The second column presents each action, Up or Down, along with the possible information a player received about the types of the other subjects in her group. Hence (D,X) in the two player case corresponds to the situation where a participant observed that the other player was of type X and, subsequently, chose Down. The next set of columns contain the number of participants with the given information who took that action, and the frequencies expected probabilities of that behavior. Behavior for each period is reported separately. The data is provided for both sessions, as well as for the aggregate of the two sessions.

Note that, at the individual level, adherence to the equilibrium prediction is considerably greater than for the groups. For example, looking at the first period in the two player case, 88 percent of subjects behave according to the equilibrium prediction in the first session, while 92 percent do so in the second session. In the second period, 50 percent of the subjects in Session I and 56 percent in Session II behave as predicted.

The results are similar for the three player case, where the first period percentages are 71 and 81, respectively, for Sessions I and II. In the second period, subjects behave according to the theory 67 percent of the time in both sessions.

Noticing again that the percentages differ by group size and by session, the tests from the previous section were repeated using individual data. The data used in these tests are reported in Table 2.9. This table again reports a player's observation of the types of the other players and the predicted behavior. In this case, however, the

		Period 1			Period 2*		
		n	freq.	pred.	n	freq.	pred.
Session I	(U,O)	0	0.00	0.00			
	(D,O)	3	1.00	1.00			
	(U,X)	20	0.87	1.00	7	0.50	0.00
	(D,X)	3	0.13	0.00	7	0.50	1.00
Total Agreements		23	0.88		7	0.50	
Total Violations		3	0.12		7	0.50	
Session II	(U,O)	1	0.20	0.00	1	1.00	0.00
	(D,O)	4	0.80	1.00	0	0.00	1.00
	(U,X)	20	0.95	1.00	6	0.40	0.00
	(D,X)	1	0.05	0.00	9	0.60	1.00
Total Agreements		24	0.92		9	0.56	
Total Violations		2	0.08		7	0.44	
Total	(U,O)	1	0.13	0.00	1	1.00	0.00
	(D,O)	7	0.87	1.00	0	0.00	1.00
	(U,X)	40	0.91	1.00	13	0.45	0.00
	(D,X)	4	0.09	0.00	16	0.55	1.00
Total Agreements		47	0.90		16	0.53	
Total Violations		5	0.10		14	0.47	

Table 2.7: Summary of individual behavior in two player game

* Both players chose U in Period 1.

behavior is not by periods but across periods. Hence, if a participant observed XO, the predicted behavior is that she would select Up in the first period and Down in the second. This table reports the numbers and frequencies of agreements and violations in each case, and for each session. Although the hypothesis that experience reduces the number of violations is supported for both games, that is, the second session produces a higher frequency of agreements for both games, this difference is not significant at any reasonable levels in a Chi-Square Test. The hypothesis that violations are more frequent in the $n = 3$ condition is also not supported by these data; neither of the differences is significant and, in fact, it is in the wrong direction for Session II. Thus, there is no support for this hypothesis and only weak support for the former one.

It is also possible to use individual behavior to test the validity of models which give predictions as to the behavior in these games. Tables 2.10 and 2.11 provide tests of and comparisons between alternate models for the two and three player games

		Period 1			Period 2*		
		n	freq.	pred.	n	freq.	pred.
Session I	(U,OO)	0	0.00	0.00			
	(D,OO)	2	1.00	1.00			
	(U,XO)	11	0.79	1.00	3	0.38	0.00
	(D,XO)	3	0.21	0.00	5	0.62	1.00
	(U,XX)	17	0.65	1.00	5	0.71	1.00
	(D,XX)	9	0.35	0.00	2	0.29	0.00
Total Agreements		30	0.71		10	0.67	
Total Violations		12	0.29		5	0.33	
Session II	(U,OO)	0	0.00	0.00			
	(D,OO)	4	1.00	1.00			
	(U,XO)	15	0.83	1.00	1	0.25	0.00
	(D,XO)	3	0.17	0.00	3	0.75	1.00
	(U,XX)	15	0.75	1.00	5	0.62	1.00
	(D,XX)	5	0.25	0.00	3	0.38	0.00
Total Agreements		34	0.81		8	0.67	
Total Violations		8	0.19		4	0.33	
Total	(U,OO)	0	0.00	0.00			
	(D,OO)	6	1.00	1.00			
	(U,XO)	26	0.81	1.00	4	0.33	0.00
	(D,XO)	6	0.19	0.00	8	0.67	1.00
	(U,XX)	32	0.70	1.00	10	0.67	1.00
	(D,XX)	14	0.30	0.00	5	0.33	0.00
Total Agreements		64	0.76		18	0.67	
Total Violations		20	0.24		9	0.33	

Table 2.8: Summary of individual behavior in three player game

* Both players chose U in Period 1.

respectively.[26] In both tables, the models are estimated separately for Sessions I and II as well as for the aggregate data of both sessions. For each model, the table presents the predicted probabilities of playing Up, given a player's information about the types of the other player or players. Hence, $P(U_1, X)$ represents the probability of playing Up in the first period after observing that the other player is of type X in the two player game and $P(U_2, XO)$ represents the probability of playing Up in the second period after observing one player of type X and one of type O in the three player game.[27] The probabilities are only given for the information sets for which there is

[26]The calculations for the results reported in this table were conducted using the GAMBIT Command Language (1996).

[27]In the latter case, the participant also has the information that both of the other players chose Up in the first period.

Number of Players	Session	Obs.	Pred.	Agreements n	freq.
2	I	O	D	3	1.00
		X	UD	13	0.57
		Total		16	0.62
	II	O	D	4	0.80
		X	UD	14	0.67
		Total		18	0.69
3	I	OO	D	2	1.00
		XO	UD	8	0.57
		XX	UUD	15	0.58
		Total		25	0.60
	II	OO	D	4	1.00
		XO	UD	14	0.78
		XX	UUD	12	0.60
		Total		30	0.71

Table 2.9: Individual behavior across periods

data and, hence, the models are only compared with respect to their predictions for these probabilities. In addition, the observed frequencies in each case are also given.

Some of the models include a parameter which determines the resulting predicted probabilities from a correspondence of probabilities. This parameter is given, where applicable, in the second to last column. Finally, the negative of the log-likelihood for each set of probabilities is also reported. Hence, in these tables, a higher likelihood corresponds to a lower number.

Five different predictive models are compared.[28] The first two of these are an approximation of the Nash equilibrium and a model where players act entirely in

[28] Another class of models that could have been tested are models that estimate the frequency of different types of players, where a player's type is a belief about how many steps of iterated rationality others are satisfying (see Stahl and Wilson, 1994 and 1995; Costa-Gomes, Crawford, and Broseta, 1998). However, there are two problems with fitting these models to this data. First, this type of model would classify any player choosing D prematurely as a "Level-0" or random type. Given the possible reasons outlined previously for why this behavior is frequent, these models would incorrectly classify these subjects as irrational types. Second, it would only be possible to estimate a very simplified version of these models because of the small number of actions by each participant.

Sess.	Model	$P(U_1,O)$	$P(U_1,X)$	$P(U_2,X)$	$P(U_2,O)$	param.	$-LL$
I	Nash*	0.000	1.000	0.000			∞
	Rand.	0.500	0.500	0.500			27.73
	NNM	0.250	0.750	0.250		0.500	22.49
	QRE	0.321	0.820	0.324		3.155	20.90
	PIM	0.170	0.830	0.394		0.660	19.62
	Actual	0.00	0.87	0.50			
II	Nash*	0.000	1.000	0.000	0.000		∞
	Rand.	0.500	0.500	0.500	0.500		29.11
	NNM	0.214	0.786	0.214	0.214	0.572	21.82
	QRE	0.340	0.862	0.303	0.070	3.677	20.77
	PIM	0.142	0.858	0.345	0.142	0.716	19.72
	Actual	0.20	0.95	0.40	1.00		
Agg.	Nash*	0.000	1.000	0.000	0.000		∞
	Rand.	0.500	0.500	0.500	0.500		56.84
	NNM	0.232	0.768	0.232	0.232	0.536	44.39
	QRE	0.328	0.838	0.315	0.088	3.360	41.74
	PIM	0.156	0.844	0.370	0.156	0.689	39.43
	Actual	0.13	0.91	0.45	1.00		

Table 2.10: Comparisons between predictions of behavior in two player game

error and, therefore, choose each action with equal probability in all cases.[29] We also estimate a simple model, labelled the Noisy Nash Model (NNM), which determines a choice probability correspondence as a function of a parameter, γ.[30] In this model players make probabilistic errors which are ignored by all players. The predicted probabilities in this model consist of all points which lie along the linear combination between the Nash equilibrium and random play. The parameter γ, therefore, is estimated from the interval [0,1] and, for a given value of γ, a player will choose correctly only with probability $0.5(1-\gamma)+1\gamma = 0.5+0.5\gamma$. Thus, when $\gamma = 0$, the model predicts random play, and for values of γ close to 1, the prediction approximates the Nash equilibrium.

Similarly to the above model, the logit specification of McKelvey and Palfrey's Quantal Response Equilibria (1998) produces, as a correspondence of a parameter λ,

[29] An approximation is used rather than the pure strategy equilibrium itself for the reason that, since the predictions are in pure strategies, the log-likelihood at the Nash equilibrium is undefined. Therefore, the reported value is the limit as the probabilities approach the equilibrium.

[30] This approach is similar to that of Smith and Walker (1993).

Sess.	Model	$P(U_1, OO)$	$P(U_1, XO)$	$P(U_1, XX)$	$P(U_2, XO)$	$P(U_2, XX)$	param.	$-LL$
I	Nash*	0.000	1.000	1.000	0.000	1.000		∞
	Rand.	0.500	0.500	0.500	0.500	0.500		39.51
	NNM	0.298	0.702	0.702	0.298	0.702	0.404	34.73
	QRE	0.220	0.691	0.652	0.324	0.803	2.199	34.54
	PIM	0.270	0.730	0.730	0.518	0.730	0.460	34.97
	Actual	0.00	0.79	0.65	0.38	0.71		
II	Nash*	0.000	1.000	1.000	0.000	1.000		∞
	Rand.	0.500	0.500	0.500	0.500	0.500		37.43
	NNM	0.222	0.778	0.778	0.222	0.778	0.556	28.60
	QRE	0.223	0.744	0.728	0.274	0.854	2.553	29.61
	PIM	0.206	0.794	0.794	0.448	0.794	0.558	28.96
	Actual	0.00	0.83	0.75	0.25	0.62		
Agg.	Nash*	0.000	1.000	1.000	0.000	1.000		∞
	Rand.	0.500	0.500	0.500	0.500	0.500		76.94
	NNM	0.261	0.739	0.739	0.261	0.739	0.478	63.76
	QRE	0.220	0.714	0.684	0.301	0.828	2.357	64.40
	PIM	0.237	0.763	0.763	0.487	0.763	0.525	64.22
	Actual	0.00	0.81	0.70	0.33	0.67		

Table 2.11: Comparisons between predictions of behavior in three player game

a progression from random play to the Nash Equilibrium. The QRE model incorporates error into the best response of players in a game, so that better responses are more likely to be played, but in this case the behavior of players is in equilibrium and they take into account the error in everyone's choices. The parameter λ, which is inversely related to error, measures precision in that when $\lambda = 0$ players are acting entirely in error while high values of λ correspond, in the limit, to the Nash equilibrium.[31]

Finally, a simple model called the Probabilistic Information Model (PIM), which incorporates the possibility that players probabilistically ignore the actions of other players is, included in the estimation. This model incorporates the principal aspect of the NNM, that players make probabilistic errors and fail to realize that such errors occur, and develops it one step further. In all cases where only **R**, or rationality, is necessary to make a decision, such as in the first period, behavior is identical to that

[31]For a formal and more detailed description of Quantal Response Equilibria for extensive form games, see McKelvey and Palfrey (1998).

under the NNM, with the parameter here labelled ρ. However, when $\mathbf{R}\&\mathcal{K}^1(\mathbf{R})$ is necessary, players may also ignore information they have received with probability ρ. Hence, in the two player case where both players are of type X, each player will select Up with probability $0.5 + 0.5\rho$ in the first period. Under the standard assumptions, however, if the game reaches the second period, then each player should now be aware of her own type. Using the same parameter, ρ, in this model, the player probabilistically either fails to realize or ignores this information, and, hence, selects Down in the second period only with probability $\rho(0.5+0.5\rho)+(1-\rho)(0.5-0.5\rho) = 0.5-0.5\rho+\rho^2$. This is similarly extended to the three person case when a player reaches the second period and observes types X and O. Here, the subject will again choose Down with probability $0.5 - 0.5\rho + \rho^2$, rather than 1.[32]

Looking at Table 2.10, it can be seen that, for the two player case, both the Nash approximation and randomness predictions can be strongly rejected in all three cases. The other three models all do a considerably better job of predicting the data. Using a likelihood ratio test, the NNM can be rejected in favor of both the QRE ($p < 0.1$) and the PIM ($p < 0.05$) in the first session. For the second session, the NNM can only be rejected in favor of the PIM ($p < 0.05$). Using the aggregate for both sessions, the NNM can again be rejected in favor of the QRE ($p < 0.05$) and the PIM ($p < 0.01$), although this pooling compromises the independence of the data and artificially decreases the values of p. Finally, while the PIM model does a better job of predicting the data than the QRE in all three cases, this difference is never significant at the 0.1 level.

In the three player case (Table 2.11), the Nash approximation and randomness predictions can again be rejected in favor of all three models. Interestingly, the NNM does a better job of predicting the data than does the PIM, though this difference is not significant at any reasonable levels. Similarly, the difference is not substantial in

[32] This model can be extended to the third period in the three person case, where the probability of misperception by another player in the second period is included in the decision probabilities. Since there is no data for the third period, however, this is irrelevant to this analysis.

a comparison between these two models and the QRE. While the QRE does slightly better in the first session than the other two models, this is reversed for the second session, and the difference is never significant.

An interesting observation is that, for all three models and in both games, the measure of precision (γ, λ, ρ) is always higher in the second session than in the first. While individual tests of the restrictions $\gamma_I = \gamma_{II}$, $\lambda_I = \lambda_{II}$, and $\rho_I = \rho_{II}$ fail to reject, at any reasonable levels, the constrained models where the parameter is the same for both sessions, the consistency of this observation again provides some support for the idea that learning, or at least a decrease in error, is taking place across sessions.

In sum, while all three models do a better job of predicting behavior than the Nash or randomness predictions, none convincingly outperforms any of the others in both games. While the PIM and QRE do a significantly better job of predicting the data in the two player game, this is not true of the three player game. Nonetheless, the increase in precision in all three models provides support for the hypothesis of learning.

2.5 Discussion

The equilibrium in the dirty faces game makes extreme predictions concerning both players' behavior and their assumptions about other players. As the above results and analysis indicate, the behavior of actual players in the game does not approximate the theoretical prediction. The results may be somewhat compromised by the fact that subjects routinely over-play the action Down, which, in order for the equilibrium to arise, should be played only when a participant is aware of her own type. Nonetheless, that a significant part of individual behavior is consistent, or at least fails to violate, the predicted behavior indicates that play is not entirely random.

Hence, models of play which specify and introduce some error perform fairly well in explaining the data. Nonetheless, the predictive ability of these models does not approach perfection, indicating that a better model can still be found. In addition, since there is some support for the hypothesis that behavior consistent with the standard theory increases with experience, such a model should include a learning, or precision parameter, to capture this effect.

The observation that error appears to be decreasing across sessions in both experiments indicates that subjects may be able to "learn" to play the correct equilibrium. While the data presented here does not allow for a strong test of this assumption, it does point towards the idea that including more sessions may lead to improved play relative to the prediction. Hence, future experiments should also include the possibility of more sessions in order to allow this adjustment process to take place and to properly measure it and incorporate it into a model. It is also important to note that it is difficult to explain this adjustment towards the equilibrium as the result of subjects' using a simple heuristic similar to the one discussed for Nagel's experiments.

A further question arises when the subject pool is considered. Using *Caltech* students, who are known for their quantitative ability (and, perhaps more importantly, know this of themselves), was intended to facilitate the equilibrium and provide an upper bound for its robustness in actual play. The fact that this group of subjects failed to behave as predicted indicates that the majority of populations may fare even worse. It might be interesting, then, to study the extent to which these results are robust across populations.[33] Moreover, if there exists a population effect, an interesting case to study might be where a subject from a highly quantitative population is matched with subjects from other populations. It does not seem obvious that the behavior of the more quantitative subject would be the same in this condition and

[33] Results from additional experiments with different populations (advanced high school students and UCLA undergraduates) point to some differences between populations. More specifically, the behavior of both the UCLA and high school students was less consistent with the theoretical prediction than that of Caltech students.

the one where she is matched with others she knows are like her.

The goal of this chapter is to address the question of whether predicted play is consistent with actual behavior in the dirty faces game. The results from the experiments indicate that subjects do not satisfy the common knowledge of rationality assumption necessary for equilibrium behavior to occur. In addition to this result, however, this study has increased the number of interesting questions remaining to be examined with this type of game.

Chapter 3 Weak-link coordination in growing organizations

This chapter is about coordination problems in firms, and the special difficulty that growth creates for efficient coordination. It begins with the recognition that while problems associated with obtaining cooperation have received much attention from economists and organizational researchers, coordination is as central a problem of organization and it is therefore surprising that coordination has received considerably less attention. The fact that a discussion of coordination problems has been around for some time in the organizational literature makes this even more surprising. Moreover, just as the prisoner's dilemma game models the problems associated with cooperation, coordination can also be represented in a simple game-theoretic way – as the game "stag hunt."

Experiments consistently show that large groups miscoordinate more often than small groups in a version of the stag hunt game known as the *weak-link coordination game*. If we believe that this game models problems present in organizations, this result suggests that larger firms may have more difficulty coordinating activity efficiently than small firms. This, in turn, implies that as firms grow larger, miscoordination tends to occur. However, no experiments have studied what happens to coordination when small groups grow into large groups by adding members. Perhaps the coordination problems large groups experience can be avoided if the process of growth is managed properly.

This chapter argues that the ability of large groups to coordinate successfully is critically affected by the growth process itself. Specifically, successfully coordinated small groups have established a set of rules or norms – either tacit or formalized –

governing what actions are appropriate. These norms allow the group to successfully coordinate activity. Therefore, it is crucial that as the organization grows adherence to the norms be maintained despite the entrance of new members. This is particularly true when the rules are not formal and are subject to interpretation. One way to achieve successful coordination through the growth process is to limit the number of new entrants at any time and to have significant socialization procedures in effect. Thus, successful growth is obtained by growing slowly and allowing sufficient time between growth periods for the rules not to be overwhelmed by new entrants. The result is a strong organizational culture that allows members to correctly anticipate what is the correct activity to be performed.

In this chapter, I provide two pieces of support for the above argument. First, I cite an example of a firm in the airline industry – Southwest Airlines – in which top management explicitly used this approach to growth to solve coordination problems. Southwest's emphasis on culture and slow expansion allowed it to grow successfully in an industry where coordination problems are important (see Knez and Simester (1997)) and avoid problems that plagued other airlines such as People Express. Second, using a formal model of behavior in weak-link coordination games (based on one developed by Crawford (1995)), I show how slow growth and exposure of new entrants to previous history of play can lead to more successfully coordinated large groups.

Finally, experiments reported in the final chapter investigate whether the result from the model holds in the laboratory. The experiments look at whether large groups playing the weak-link coordination game are more efficiently coordinated when they are "grown" – i.e., start off small and have players enter slowly – than when they begin playing at a large size. A second set of experiments examines the behavior of subjects placed in the role of "managers" and allowed to determine the group size. These experiments are conducted for two reasons. First, it is possible that the managers might discover growth paths that work better than those used in the first experiments. Second, evidence from previous experiments indicates that subjects are not aware of the difficulty in coordinating large groups (Weber, Camerer, Rottenstreich, and Knez, 1998). If this is true, then the managers might grow the groups too quickly, leading to coordination failure.

3.1 Coordination and cooperation in organizations: A game-theoretic view

In the standard economic theory, firms are organized to exploit gains from specialization which give rise to economies of scale and scope. In theory, spot market contracting for factors of production is replaced by hierarchical authority relations.

The new theory of the firm seeks to understand how firm structure solves the two basic problems of organization: cooperation and coordination. Cooperation refers to the problem of getting people to do what is not in their best interests but is in the best interests of the firm. Put in economic terms, this means internalizing externalities and limiting moral hazard (or opportunism). Monitoring, incentive contracts, persuasion and psychological forces can help do this.

3.1.1 Prisoner's dilemma vs. stag hunt games

A fundamentally different problem is coordination: Suppose there are two sets of activities, A and B, which could be performed simultaneously. Efficiency is only achieved if everyone performs the same activity. Thus, everyone wants to perform the same action that everyone else is performing, but they are unsure of what action others will take. This is the basic coordination problem made famous by Schelling (1960). To make the coordination problem more interesting, suppose that A is better for everyone but riskier. Therefore, if everyone performs activity A, then everyone is better off than if they all performed B, but anyone who does A when all others do not would have preferred to have done B. Then the problem of coordination is getting workers to believe that everyone else will do A, in which case they will as well. Coordination problems like this arise when there are complementarities in activities of different workers (or corporate divisions, etc.).

The contrast between these two fundamentally different problems can be sharply seen in two simple games: Prisoner's dilemma (PD) and stag hunt (or the assurance game), shown in Table 3.1.

	Cooperate	Defect
Cooperate	2,2	0,3
Defect	3,0	1,1

	High	Low
High effort	2,2	0,1
Low effort	1,0	1,1

Table 3.1: Prisoner's dilemma (top) and stag hunt (bottom) games

In the PD game players choose to either Cooperate or Defect. Defection is a dominant strategy because it gives a higher payoff regardless of what the other player does. In the stag hunt game players choose actions High or Low. Neither strategy is dominant because each can be better depending on what the other player does – the players wish to coordinate their choices because Low is the best response to another player choosing Low (then they both earn 1) but High is a best response if another player chooses High (then they both earn 2).[1]

[1] Stag hunt is also called an "assurance" game and is closely related to a threshold or step-level public goods game (in which a public good is provided if enough subjects contribute), also known as the "volunteer's dilemma" (Murnighan, Kim, and Metzger, 1993), and to infinitely-repeated prisoners' dilemma games where trigger strategies create multiple Pareto-ranked equilibria.

In both games, players could both be better off if they could coordinate their choices and earn (2,2) (from (Cooperate, Cooperate) in PD or (High, High) in stag hunt). The contrast between the two games lies in the reasons why coordination is difficult. In PD players prefer to not reciprocate cooperation, because if the other player cooperates defection pays 3 while cooperation pays 2. That is, it is not in the players' best interests to cooperate even though both players end up worse off as a result. In stag hunt, on the other hand, players do prefer to reciprocate High (which corresponds to cooperation), because reciprocating pays 2 while choosing Low pays 1. However, coordination is difficult because playing High results in the worst outcome if the other player plays Low, and players are unsure of what others will do when choosing their action.

Applying the games to behavior in organizations yields an insight into two – often confused – distinct problems faced by firms. Thus, "solving" the PD requires an organization to get players to act against their best interests. Defection imposes a negative externality on the other player and if players could somehow be prohibited from doing so, both would be better off. "Solving" stag hunt requires an organization to get players to believe that others will choose High; then they will prefer to choose High and reach the Pareto-optimal payoffs.[2]

Both games imply that rational players may choose strategies which lead to a collectively "irrational" outcome yielding Pareto-inferior payoffs. In PD, this collective inefficiency results because of the conflict between private and collective incentives. However, in stag hunt rational players may end up at the inefficient (Low, Low) equilibrium simply because of strategic uncertainty– they aren't sure what other players will do and there is therefore some risk associated with choosing High.

[2]Foss (1998) argues that the central role of leadership in organizations is to provide this type of solution. Having leaders make public announcements about which action everyone should take results in the preferred outcome being common knowledge and, therefore, salient. However, experimental work on coordination games by Van Huyck, Gillette and Battalio (1992) and by Weber, Camerer, Rottenstreich and Knez (1998) shows that the effects of this type of leadership are limited.

These two games capture two fundamental problems that organizations must overcome. As Williamson has pointed out at length (e.g., 1985), intrafirm contracting for production which uses relationship-specific assets creates a potential "holdup" problem due to moral hazard, of which the underlying problem is similar to the PD. A tremendous amount of literature has sprung up to study this and related problems as well as potential solutions.[3]

However, Williamson himself has also recognized the importance to firms of solving coordination problems arising from strategic uncertainty:

> Some disturbances . . . require coordinated responses, lest the individual parts operate at cross-purposes or otherwise suboptimize. Failures of coordination may arise because autonomous parties read and react to signals differently, even though their purpose is to achieve a timely and compatible combined response. (Williamson, 1991, p. 278)

Coordination problems arise when there are productive complementarities or interdependence between assets. Even in the absence of moral hazard, the firm must still find a way to coordinate use of the assets to achieve the optimal outcome. It is somewhat surprising, therefore, that the problem of achieving coordination has been relatively understudied.[4] This is even more surprising since problems arising from interdependence and strategic uncertainty have been discussed by prominent organizational researchers for some time. However, their attention to coordination problems – discussed in the next section – has not led to a subsequent emphasis on these problems.

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[3]See, for instance, Hart (1995), Milgrom and Roberts (1990), and Holmstrom and Tirole (1989).

[4]For instance, while there exists a veritable cornucopia of experimental studies on PD, the corresponding number for the stag hunt is considerably lower. See Battalio, Samuelson, and Van Huyck (1997) for a survey of recent experimental work on the stag hunt game.

3.1.2 Coordination in organizations

Many examples of coordination economies have been discussed in work on internal economics of the firm and business strategy. These problems all share the characteristic (present in the stag hunt game) that the optimal action of any individual or group depends on what others are doing and, therefore, it is only optimal to take a particular action if others are as well. For example, in Coase's (1937) original paper on the transactions-cost view of the firm, he emphasized how firm practices could economize on search in markets for price and quantity, and he did not much emphasize the hold-up and moral hazard problems later emphasized by Williamson (1985), Klein, Crawford and Alchian (1978) and others.[5] It is well-known in macroeconomics and other literatures that search often generates a coordination problem because having buyers and sellers searching in the same place is desirable since it results in markets with the most thickness and immediacy.[6] The buyers want to be where the sellers are, and vice versa. Therefore, Coase's original emphasis on firm structures as replacements for searching suggests a special kind of coordination motive for organizing, or put oppositely, the problem of coordinating search may be the problem the firm is an answer to. For example, think of a large firm with separate divisions, which can separately hire people from the outside (Outside strategy), or develop an expensive system for evaluating and tracking employees (Inside strategy). Suppose there are some economies of scale – that is, the quality-adjusted cost of getting good employees falls with the number of employees who are carefully tracked. Then the two divisions would like to adopt the same strategy: If one division uses the Inside strategy, then it may pay for the other to adopt it so they can pool their information, but if the other division uses the Outside strategy, then it doesn't pay to set up the expensive tracking system.[7]

Thompson (1967) discusses at great length coordination problems that arise when-

[5] In more recent work (Coase, 1988) he takes later researchers to task for overemphasizing hold-up at the expense of other problems like coordination.

[6] See, for instance, Cooper (1999).

[7] Becker and Murphy (1992) make a similar point.

ever there is interdependence between organizational units. He argues that there are three kinds of interdependence that arise in organizations. Pooled interdependence between organizational parts results whenever "unless each performs adequately, the total organization is jeopardized; failure of any one can threaten the whole and thus the other parts." (p. 54) Sequential interdependence is the result of a linked series of events where each task has to be performed before the next can be started. Finally, reciprocal interdependence between units involves a symmetric relation of sequential interdependence: each unit produces an output that serves as an input for the others. He argues that pooled interdependence is the most common kind and is present in all organizations. Note that pooled interdependence results in a problem similar to the one modeled by the stag hunt game. In addition, Thompson notes the importance of standardization – following a set of established rules that point to the "appropriate" action – in ensuring that segments of an organization are operating in "compatible ways." (p. 17)

In their study on the practices of several firms in different industries, Lawrence and Lorsch (1967) find that the ability to solve coordination problems plays an important role in determining success. They find that the ability to integrate the behavior of separate specialized units is crucial for the firms in their sample, particularly when these firms are in a dynamic environment. They argue that firms have a need to take advantage of the benefits of specialization, but that this specialization then creates a need for coordination. While firms in stable environments are more likely to solve coordination problems through formal and centralized decision making and planning, it is more difficult and undesirable for firms in changing environments to attempt to coordinate activity through hierarchical chains of command.[8] Instead, firms

[8]Experimental work by Shaw (1981) shows that centralized decision making is better suited to less complex tasks. Also using experiments, Leavitt (1962) shows that centralized structures are more efficient at simple tasks than decentralized ones. However, the decentralized groups perform better when the task becomes more complex because of noise in the environment and are better at introducing and implementing new ideas. Carley (1992) presents computational results indicating that formal, hierarchical organizational structures do better when employee turnover is high. Ghemawat and Ricart I Costa (1993) discuss the trade-off between static and dynamic efficiency in firms and argue that each may be an equilibrium, making it difficult for firms to change from one form of

in these industries tend to achieve successful coordination through separate interdepartmental units.[9] They find that better performing firms tend to be ones in which this task is successfully achieved by individuals or teams in the role of "integrators."[10]

The argument that as firms grow coordination costs increase and the hierarchical model is no longer a suitable solution is discussed by several other organizational researchers.[11] Scott (1998) states that coordinating activity is one of the main achievements of formal structure. He also argues that

> As information-processing needs increase up to a certain point, hierarchies can be of benefit, reducing transmission costs and ensuring coordination. But as information-processing needs continue to increase, hierarchies become overloaded. . . Such increased demands . . . encourage the creation of more decentralized structures. (Scott, 1998, p. 161)

Thus, centralized decision making is suitable for coordinating activity where communication and information-processing are manageable, but the ability of a centralized unit to achieve this is dramatically reduced with size.

Arrow (1974) also recognizes the importance of solving coordination problems in firms. He claims that one of the major roles of authority is to coordinate activity, particularly when interests coincide. He further argues that firms require internal languages, or codes, for information transmission if they are to successfully perform the function of aggregating information. Arriving at a successful code employed by everyone in the organization constitutes a coordination problem similar to the kind

efficiency to the other.

[9]Thompson (1967) argues that firms in changing environments need to devote resources to the informational aspect of change, figuring out not only what changes to make but how to re-design the organization so the changes will work.

[10]St. John and Harrison (1999) similarly find that high performing firms are significantly more likely to employ coordinating mechanisms.

[11]See, for instance, Starbuck (1965), Scott (1998).

studied by Schelling (1960). A successful code may exist even if it is not employed by other organizations, unless interaction with other firms require that they use the same language. The code employed by a particular organization may be the result of history and path dependence: conditions at the time at which the firm was created may have led to a particular code which then persisted because it produced successful internal coordination. However, a firm created at a different time or location may employ an equally successful but entirely different code.

A current example of where different codes have been adopted by different organizations – resulting in successful internal communication but increased problems with external interdependence – is evident in the argument for global accounting standards. With the increased international economic interdependence brought about by mergers and technological advances, there is a need for global accounting rules. However, there is a considerable debate concerning what these rules should be since countries have varied securities regulations.

Heath (1999) provides several additional examples of how firms face important coordination problems. Even though the success of the firms in several of his examples is linked directly to how well they solve these problems, Heath shows that they often neglect the importance of interdependence between units. Instead, they focus solely on improving the performance of individual units through even greater specialization and segmentation. This, in turn, often aggravates the coordination problem. Similarly, organizations fail to realize the importance of communication and translation between specialists to solve coordination problems. This failure to take an important problem into account by managers is termed "coordination neglect" by Heath.[12] An example of coordination neglect is provided by Chandler (1962) who writes about how in the early 1900's Ford tried to diversify and produce airplanes and tractors, as well as an accessory and parts business. The products were well-made and popular, but

[12]Heath also points to the neglect of the importance of coordination by organizational researchers. This point is also made by Lawrence and Lorsch (1967) who criticize early organizational scholars for not taking into account all the effects "of segmenting the organization into departments." (p. 9)

this venture was not successful because the organization as a whole was only suited to manufacturing and selling cars. Following the retirement and death of Henry Ford, the company implemented General Motor's decentralized approach to diversification (by hiring former GM executives) through reorganization and successfully re-started producing tractors and parts and accessories.

Similarly to Heath, March and Simon (1958) also find fault with the absence of coordination as a central problem of organizational theory. They argue that coordinating units play a crucial role in organizations and point to coordination problems arising when "the appropriateness of a particular activity may be conditional on what other activities are being performed in various parts of the organization" (p. 27) as important to firms. They argue that one example of this type of interdependence arises in the timing of activities, or problems of temporal synchronicity. As will be argued below, problems involving temporal interdependence can also be modeled generally as stag hunt games. Scheduling provides a way to solve this type of coordination problem.[13] March and Simon also argue that the tolerance for interdependence, or ease of coordination, is related to the stability of the environment and that communication plays an important role in solving coordination problems.

There are several examples of industries where coordinated activity between employees or separate units is crucial. One example is in software development (Kraut and Streeter, 1995; Kiesler, Wholey and Carley, 1994). It is usually the case that large software projects are developed simultaneously by independent teams where each team is responsible for a particular part of the code. However, while the parts are developed separately, there is a need for them to work as a unit in the final product. Thus, coordination regarding how the program will operate and how the separate parts will interrelate is critical.

[13]Lawrence and Lorsch (1967) and Scott (1998) also discuss the importance of scheduling for solving this kind coordination problem.

A second example is in the provision of health care (Gittell and Fairfield, 1999; Gittell and Wimbush, 1998). Patients undergoing diagnosis and treatment require interactions with separate and often geographically dispersed units of a health care organization. In order for the quality of care to be high and for the service to be provided at a high level of efficiency for the organization, the activities of each unit from radiology to billing must be well coordinated.

Another area of organizational behavior where coordination plays an important role is in the functioning of inter-organizational networks and alliances. In these, pairs of groups of firms form collaborative unions to achieve common goals. These goals are often only realized, however, if there is a high level of coordination between the organizations in the network. For instance, Khanna, Gulati and Nohria (1998) discuss learning alliances (alliances where the goal is for firms to learn from each other) in which "all firms must finish learning in order for any of them to derive the common benefits." (p. 197) Human and Provan (1997) studied two small to medium sized manufacturing networks and found that coordination and communication played a critical role in determining their success.

Milgrom and Roberts (1992) discuss examples from operations management like flexible manufacturing and CAD/CAM, or technology adoption. In these examples the returns to adopting one element of technology are high only if another element is adopted elsewhere in the firm (or another business practice is changed). Ideally, the firm should coordinate these choices so they pick the best bundle of technologies or practices. However, if the individual elements are chosen independently by different divisions or function managers, then a coordination problem similar to the one in stag hunt arises.[14]

There is also a body of evidence indicating that firms pursuing narrow business strategies perform better than firms that diversify into unrelated fields (Rotemberg

[14]See also Barnett and Freeman (1997).

and Saloner, 1994). One explanation is that focusing on a narrow range of activities avoids coordination problems that may arise when interdependent units within an organization are performing unrelated activities yet still face a need to communicate with each other. Teece, Rumelt, Dosi, and Winter (1994) show that diversification by firms usually involves adding activities that are related and complementary to activities already being performed by the firm. Similarly to the pursuit of narrow business strategies, this coherent diversification reduces the danger of coordination failure arising from the introduction of unrelated activities.

Baker, Gibbons and Murphy (1994) discuss subjective and objective measurements of performance, such as bosses' verbal ratings of an employee's success and some numerical productivity measure, in incentive contracts. They show that for some parameter ranges, using subjective measurements raises the marginal productivity of objective measurements, because the objective rating can serve as a discipline or check on the subjective measurement (e.g., it prevents a boss from giving good ratings to poor employees who they like or find personally useful). At the same time, using objective measurements raises the marginal productivity of subjective measurement because the subjective measure can correct for quirks or omissions in the objective rating. Thus, within some ranges both kinds of measurements are productive complements for each other, which creates a problem of coordinating the levels of the two kinds of measurements.

Thus, while the problem of cooperation in organizations has received more attention than coordination, the importance of the latter has been noted by several prominent organizational researchers. In addition, there are several examples of coordination problems that firms must regularly solve. Therefore, it is somewhat surprising that while these problems are more aptly modeled by the stag hunt game than by PD, it is PD that has been more frequently studied in experiments. Moreover, a variant of the stag hunt game – known as the weak-link coordination game – yields interesting insights into coordination problems in organizations.

3.2 Weak-link coordination games

3.2.1 Weak-link coordination problems in organizations

In the stag hunt game, a player's payoffs can be decomposed into two elements: A cost from choosing the strategy H, and a group payoff which depends on whether either player chose Low.[15] A natural n-person generalization of this game is one in which players choose numbers (or orderable strategies) and the group payoff depends on the lowest number selected by any player. These are sometimes called "weak-link," or minimum effort, coordination games (because everyone's payoff is a function of the lowest effort selected by any player, or the "weakest link") and are a special kind of order statistic game.[16] Processes in which the production function has the Leontief form (output depends on the minima of the factors of production) have the weak-link quality. Examples include production of chemical compounds, orchestra performances (one false note ruins the whole symphony), some kinds of cooking, and provision of safety in dangerous situations (see Camerer and Knez (1997)).

An example of weak-link coordination problems arising from safety requirements is provided by Weick and Roberts (1993). They discuss the critical need for "heedful interrelating" in preventing accidents in flight operations on aircraft carriers. They describe how even the most basic operations on flight decks involve several groups of people performing different tasks simultaneously and how a mistake in any task can lead to a catastrophic outcome.

Groups of firms engaged in illegal behavior are also often involved in weak-link

[15] So, for instance, the payoffs in the stag hunt game in Table 3.1 could be represented by a cost of 1 for choosing High and 0 for choosing Low and a payoff of either 1 to everyone if either player chose Low and 3 to everyone if both players chose High.

[16] See Van Huyck, Battalio and Beil (1990 & 1991), Crawford (1995), and Camerer (in progress, Chapter 7)

coordination problems. Suppose that the illegal activity is unlikely to be discovered or proven by regulatory agencies unless one of the firms reveals some information. However, none of the firms has an incentive to provide evidence unless it believes others will as well. Then the firms face a weak-link coordination problem in that they are all better off maintaining silence as long as all the other firms are. This was the case in the tobacco industry where, until recently, the top firms were able to maintain secrecy concerning their knowledge of the harmful effects of smoking, making it difficult for the government to take action against them. This changed, however, when the smallest of the big five firms, Liggett Group, provided evidence acknowledging that the industry had known of the effects all along. It is interesting that Liggett did not benefit from providing the evidence and instead the resulting settlements in several lawsuits forced the firm close to bankruptcy.[17]

Organizational change is another area where weak-link problems might arise. There is a large body of evidence indicating that firms face difficulties adapting to changes in their environment, as indicated by Hannan and Carroll (1995):

> History readily illustrates the difficulties of adaptation. Few organizations achieve either great longevity or great social power, and virtually none achieves both. In other words, few organizations succeed in solving their adaptive problems for very long in a turbulent world. (p. 24)

One explanation for this is that firms face weak-link problems in implementing many kinds of change. For instance, Hannan and Carroll write about how tacit agreements between members of a group concerning what actions are correct may then infuse these actions with social value. The result is that these actions become understood

[17]In fact, the anecdote often used to illustrate the prisoner's dilemma – of two suspects being questioned and urged to present evidence against the other – is actually often a stag hunt game. If the prosecutors lack sufficient evidence to convict either prisoner if neither confesses, then the best outcome for both is if neither defects and they are both released.

as the correct way of doing things and become more difficult to change when the need arises. A firm stuck at an inefficient equilibrium – that may once have corresponded to an efficient set of activities but may no longer do so because of changes in the firms' environment – may attempt to implement a change to an optimal set of activities. However, if employees only benefit by attempting to implement the change if everyone else attempts to do so as well, the problem is identical to a weak-link game (Nanda, 1996). The danger, then, is of ending up at an out of equilibrium outcome where some people are performing the old set of activities and others are attempting the new ones. This results in the worst outcome for the firm.[18] Hannan and Freeman (1984) discuss similar hazards involved with change. They argue that during change there is "a period of time during which existing rules and structures are being dismantled . . . and new ones are being created to replace them" and that "the presence of multiple rules and structures greatly complicates organizational action." (p. 158) Heath (1999) argues that an important danger of segmenting an organization into specialized units is that a change in the way products are produced may leave the old organizational form unprepared to handle the new production. When this happens, firms find themselves unable to change because they "have divided themselves into segments based on the technology of the previous generation" and "they are in a good position to improve the components produced within each segment, but not to coordinate the products that depend on a different kind of interaction." (p. 6)

Problems requiring temporal synchronicity also create weak-link coordination problems. In synchronicity problems, the key choice variable is the time at which one player's contribution to a group project is completed. For example, assume that workers would like to finish their portions of a project as late as possible – but the entire group would be better off if the project was finished sooner – and that the

[18]Levinthal (1997) shows that the implementation of change can be modeled for some organizations as jumping from one peak to another in a multi-peaked environment. In order for change to be successful, it is necessary for all critical units to change in the same manner, resulting in a coordination problem. An example of organizational change where several processes need to be implemented simultaneously is provided in the case of banks replicating practices between branches by Winter and Szulanski (1997).

project is not finished until all the portions are finished and assembled. Then each worker wants to finish just when the slowest worker does (no one wants to finish ahead of the slowest worker since this does not help complete the project any sooner) and everyone wants that time to be soon. This creates a coordination problem very similar to the one in the weak-link game experiments described below. Examples of this type of problem include assembly lines, projects with strict deadlines (e.g., preparing bids for contracts), assembling chapters of an edited book, or service activities in which customers are forced to wait until all parts of the service are completed. An example of the latter is airline departures – the plane can't leave until the gate crew, ground crew, baggage handlers, and flight crew have all finished their preparations (see Knez and Simester (1997)).

Weak-link coordination problems are a particularly punishing form of coordination. It only takes one person or factor failing to perform optimally for the outcome to the entire group to be negatively affected. Thus, every element must coordinate efficiently in order for efficient outcomes to be achieved.

It is not unreasonable, therefore, to expect that larger groups of people might have a more difficult time coordinating successfully in weak-link problems as well. Previous experimental research has established this phenomenon.

3.2.2 Previous work on weak-link coordination games

Weak-link coordination games were first studied experimentally by Van Huyck, Battalio, and Beil (1990).[19] Most early studies on weak-link games used an abstract version of the game in which players chose numbers and were given a payoff table indicating how their payoff depended on their own number and on the minimum num-

[19]This has also been studied experimentally by Cachon and Camerer (1996), Knez and Camerer (1994) and (1996), Camerer, Knez and Weber (1996), and Weber, Camerer, Rottenstreich and Knez (1998); and theoretically by Anderson, Goeree, and Holt (1996) and Crawford (1995).

ber chosen in their group.

Weber, Camerer, Rottenstreich and Knez (1998) used a decomposed payoff format in which players were told that their earnings were comprised of a group reward (which depended on the minimum number chosen) and a personal fee (which depended on their choice). To address concerns about how more natural contextual labels might affect behavior, Weber, et al., also included a condition in which players were told that the numbers they chose corresponded to the times at which they finished their portions of a group report.[20]

The instructions read:

> In this experiment, you are one of N members of a project team that is responsible for producing a series of reports. Each report that the team prepares consists of N sections, where each member of the team is responsible for contributing one of the sections. A report is considered complete only after all members of the team contributed their sections. Your team will be responsible for producing a total of T reports. Until a particular report is finished, no member of the team can work on his or her section of the next report.

Subjects then chose how early to contribute their section of the report, from 1 to 7 weeks early. Table 3.2 shows how their payoffs depended on their own choice and on the project completion time (the number of weeks ahead of schedule that the project was finished, which depended on the minimum number chosen by any subject.[21]).

[20] Behavior in this natural-label condition was indistinguishable from behavior in the more abstract conditions.

[21] The instructions to subjects actually had them choose a time at which they finished their portion of the report, so the completion time was the maximum of these numbers. The game is described in terms of minima in this chapter for comparability to prior research, and so that high numbers correspond to more efficient choices.

Player's Time	Report completion time 7 weeks early	6 weeks early	5 weeks early	4 weeks early	3 weeks early	2 weeks early	1 week early
7 weeks early	.90	.70	.50	.30	.10	-.10	-.30
6 weeks early		.80	.60	.40	.20	.00	-.20
5 weeks early			.70	.50	.30	.10	-.10
4 weeks early				.60	.40	.20	.00
3 weeks early					.50	.30	.10
2 weeks early						.40	.20
1 weeks early							.30

Table 3.2: Payoffs (in dollars) for weak-link game

The diagonal cells correspond to outcomes in which the player is choosing the same time as the minimum. These outcomes, in which everyone chooses the same time and receives the same payoff, are all Nash equilibria since no player would prefer to contribute either earlier (which would increase their personal fee but not affect the completion time or group reward) or later (which would lower their personal fee but would also affect the completion time and lower their reward by even more). Notice, however, that the equilibria are different because those with higher personal fees also yield higher payoffs. The Pareto-dominant (or "efficient") outcome arises when all of the participants select the earliest time, 7 weeks early, and receive \$.90. It is in the players' mutual interest to reach this outcome and the players realize this.

However, the efficient outcome may not be easy to achieve because players are faced with strategic uncertainty. Simply being unsure about what others will do may lead different players to take different actions, and when groups are large the minimum completion time may therefore be quite low.

Previous experiments with weak-link games have established clear regularities. Coordination on the efficient equilibrium is impossible for large groups. Of the seven sessions initially conducted by Van Huyck, Battalio, and Beil (1990) (VHBB) with groups of size 14 to 16, after the third period the minimum in all games was the lowest possible choice. For small groups (n = 2) coordination on the efficient equilibrium was much easier– it was reached in 12 of 14 (86%) of the groups studied (a result replicated by Knez and Camerer (1996)). Table 3.3 summarizes the distribution of fifth-period minima in several different experiments, all using the VHBB game in which subjects choose integers from 1 to 7 and choosing 7 is efficient.

1	2	3	4	5	6	7	Group Size	N	Source
9	0	0	0	0	0	91	2	28	VHBB, 1990; Knez & Camerer 1996
37	15	15	11	0	4	18	3	60	Knez & Camerer 1994, 1996
80	10	10	0	0	0	0	6	114	Knez & Camerer 1994
100	0	0	0	0	0	0	9	18	Cachon & Camerer 1996
100	0	0	0	0	0	0	14-16	104	VHBB, 1990

Table 3.3: Fifth period minimums (by %) in various 7-action weak-link studies (1 = inefficient; 7 = efficient)

The effect of group size could hardly be stronger. Subjects in a group of size 2 are almost assured to coordinate on the efficient outcome. Subjects in larger groups (six or more) are almost assured to converge to the least efficient outcome.[22] Thus, there is a strong negative relationship between the size of a group and the ability of its members to coordinate efficiently.

This increased difficulty of coordination associated with larger group sizes has also been recognized by organizational researchers. For instance, Thompson (1967) writes that because the costs of achieving tacit coordination, or coordination by mutual adjustment, are expensive and "increase as the number of positions involved increases, we would expect organizations facing reciprocal interdependence to fashion the smallest possible groups." [23] (p. 58) Lawrence and Lorsch (1967) argue that large

[22]Once these groups reached the inefficient outcome, they were not able to subsequently increase the minimum.

[23]Thompson also points to the importance of communication and repeated interaction of the same group members (or coordination by mutual adjustment) in solving problems of interdependence. He discusses as an example of a group where coordinated action is important small bomber crews where

organizations will be the ones that are able to solve the coordination problems arising from simultaneous differentiation and integration. If firms are not able to solve the coordination problem, on the other hand, it will be impossible for them to efficiently grow. In addition, it has also been argued that there is a connection between the size of inter-organizational networks and their effectiveness and longevity (Human and Provan, 1998; Phillips, 1960).

There exists a similar relation between size and successfully coordinated change in organizations. It is often argued that larger organizations are more likely to exhibit organizational inertia.[24] Recalling Nanda's (1996) argument that organizational change involves solving a problem essentially identical to the weak-link game, the significance of the above experimental result is obvious. Larger organizations are more subject to inertia because they find it more difficult to solve coordination problems associated with change.

Given the relationship between weak-link coordination games and coordination problems faced by firms, it becomes important to identify how large groups can coordinate successfully. This chapter is part of a larger general program of research concerned with identifying mechanisms for resolving large group coordination failure in weak-link problems, which the previous section argues correspond to a naturally-occurring organizational phenomenon.

Knez and Camerer (1994) investigated "mergers" – would two three-person groups coordinate better or worse when combined into one six-person group? Two opposite effects could occur: (1) The weak-link structure could easily harm efficiency in the larger merged group; (2) on the other hand, the merger could serve as a resetting mechanism which would allow an inefficient small group a chance to start over by emulating the more efficient group they were merged into. In fact, the merged groups

"communication had to be rapid, direct, and unambiguous" and "holding the same ten individuals as one team – crew integrity – was a high priority policy." (p. 62)

[24] See Hannan and Freeman (1984) for a discussion of this proposition.

always did worse (and in 80% of the cases converged to the least efficient outcome).

Weber, et al., (1997) showed that simple forms of leadership had little effect. Moreover, the paper predicted and found an "illusion of leadership." Some subjects were randomly chosen to be "leaders" who spoke briefly to their group about what they should choose, in either small (2-person) or large (10-person) groups. A large literature in social psychology on the "fundamental attribution error" shows that people often overestimate the causal effect of personal traits on behavior, and underestimate the effect of situational variables.[25] Weber, et al., predicted that this simple form of leadership (which amounts to one way non-binding communication) would not improve coordination in large groups and, further, that subjects would not fully appreciate how easily 2-person groups can coordinate and how difficult it is for 10-person groups to coordinate. Therefore, when evaluating the effectiveness of the leaders, the authors predicted that subjects' evaluations would be subject to the fundamental attribution error and that ratings would reflect the belief that leaders in the large groups did a bad job and leaders in the small groups did a good job. This is indeed what happened. Furthermore, in one experiment subjects who could cast a (costly) vote to "fire" the leader before continuing the experiment were more likely to fire the leaders in the large group condition than in the small group condition.

3.2.3 Weak-link coordination and growth

The weak-link game provides a way to test the connection between growth and coordination experimentally. While it is well-established in weak-link experiments that large groups coordinate poorly, and small groups coordinate well, no previous research has tied these phenomena together by exploring behavior in small groups as they grow larger by adding members. Interest in the effects of growth was motivated by two observations, one from the laboratory and one from the field.

[25] See, for instance, Ross and Nisbett (1991).

The laboratory observation motivating this work was that 3-person groups usually do not reach efficiency in weak-link games, unless they were created by adding a third person to a successful 2-person group, in which case they almost always reach efficiency. This phenomenon suggests that, in principle, arbitrarily large groups could be created which coordinate efficiently in the weak-link game, if they start small enough and grow slowly enough.

The field observation is the apparent contradiction between two firmly-held beliefs in organizations. The first belief is that firms must grow to achieve scale economies or critical mass. The second belief is that firms can grow too rapidly and end up suffering as a result.

Since experiments have established the presence of a large group coordination problem, a possible explanation for too rapid growth often resulting in failure is that it might be caused by increasing miscoordination. This possibility was previously stated by March and Simon (1958) who wrote: "As the size of organization increases . . . the coordination costs become larger." (p. 29) Similarly, Chandler (1962) wrote:

> . . . growth without structural adjustment can lead only to economic inefficiency. Unless new structures are developed to meet new administrative need which result from an expansion of a firm's activities into new areas, functions, or product lines, the technological, financial, and personnel economies of growth and size cannot be realized. (p. 176)

Thus, simply increasing the size of a successfully coordinated group may be enough to result in coordination failure. Taken to an extreme, the result that large, efficiently coordinated groups are never produced in the laboratory implies that growing organizations is a treacherous process which is almost certain to fail due to miscoordination. However, given that large, efficiently coordinated organizations are not uncommon, it must be that firms solve this problem somehow. One way they do this is to implement changes in the organization that alleviate the difficulty of solving the interdependence problem. These are the "structural adjustments" that Chandler writes about. Therefore, as a firm grows, it can create decentralized units with limited interdependence between them, increase communication between units that are interdependent, and create positions for specialized teams of integrators responsible for coordinating activity between units. An example of this can be found in Microsoft's recent reorganization into decentralized units. This change, resulting in separate divisions with unprecedented levels of autonomy, was implemented largely to solve problems arising from a large bureaucracy ill suited to cope with the firm's size and from the competitive pressures exerted by a rapidly changing environment. In Microsoft president Steven Ballmer's words, the change was implemented because "we needed to give people a beacon that they could follow when they were having a tough time with prioritization, leadership, where to go, what hills to take" (Moeller, Hamm and Mullaney, 1999, p. 106). Thus, solving coordination problems was a large part of the purpose of the reorganization.

While the above changes to a firm's structure can help alleviate coordination problems resulting from size, they are not the only way to accomplish successful growth. A second way can be described by an example.

3.2.4 Southwest Airlines

The approach to growth of Southwest Airlines provides an interesting case study in how the growth process itself can be used to grow a large, efficiently coordinated organization. Southwest began as a low-cost, no-frills airline flying short hauls from secondary airports (such as Love Field in Dallas).[26] The corporate cultures empha-

[26]Unless otherwise noted, the information on Southwest Airlines and People Express is from Harvard Business School cases.

sized friendly service by employees who were cross-utilized (performed different jobs), given a lot of freedom to determine their job responsibilities and expected to pitch in spontaneously (e.g., pilots sometimes checked in luggage), and paid less than at other major airlines. Success of the strategy depended on having lower costs than major carriers and attracting price-sensitive fliers who might have not flown at all, or taken an alternative method of transportation such as driving.

Southwest's basic strategy could be put plainly: 'We've always seen our competition as the car. We've got to offer better, more convenient service at a price that makes it worthwhile to leave your car at home and fly us instead.'. Their "luv service" and an emphasis on making a flight fun and simple were an important departure from the industry norms. This approach was successful as Southwest remained profitable for 24 consecutive years (through 1996) and was the only airline to earn a profit in 1992. The ability of Southwest to attract qualified employees willing to work for less was due largely to the team spirit that routinely led Southwest to be ranked as one of the best places to work.

What is particularly interesting about the Southwest case, however, is that the airline grew at a very slow rate and regularly passed over opportunities to enter profitable markets. Rather than expand rapidly to achieve a national base – an approach used regularly in the airline industry (for instance, by People Express) – Southwest grew very slowly by comparison and after a quarter of a century of operation had not yet entered markets in the Northeast.

This slow approach to growth was specifically intended to maintain the successful coordination – largely reflected in Southwest's culture – that the airline achieved as a small local carrier. This was accomplished by creating a unique culture in which employees were highly motivated, close to management, and shared a common objective of making customers happy and getting planes in and out on time at low cost. The airline business is an obstacle course of coordination problems, many of which have weak-link structure. This was particularly true for Southwest, which stressed on time service as one of its performance criteria.[27] In addition, on the short-haul routes usually used by Southwest, planes often make several trips a day, so if a plane is late on one haul, it affects the entire system and has a multiplier effect on disgruntled passengers all down the line. The culture worked well at solving this coordination problem when the airline was small. However, maintaining this culture which achieved coordination by stressing personal responsibility and a sense of community (rather than by command and control) could be easily strained by rapid growth.[28] In fact, Southwest's approach to growth was specifically motivated by a desire to maintain its culture.

Southwest was able to avoid the coordination failure often resulting from growth by growing more slowly and taking great pains to ensure that new employees were socialized into the culture. Rather than expand into new markets, Southwest stressed growth within the current route system and added new routes slowly. According to Southwest director of schedule planning, Peter McGlade, "controlled growth is essential" because "we want to make sure that the way we conduct business in a city we

[27] Recall that Knez and Simester (1997) argue that flight departures constitute an important weak-link coordination problem between several employees.

[28] As an example of where this happened, consider the case of People Express (PE). PE was initially very similar to Southwest in that it offered low-cost, no frills service over short hauls, using primarily secondary airports (such as Newark). PE founder Donald Burr also stressed a similar culture to Southwest's and initially this proved very successful. In fact, PE was widely held as a model of a successful management approach in the early 80's (Chen and Meindl, 1991).

However, PE's approach to growth differed significantly from Southwest's. After a brief period of initial success, PE grew at a remarkable rate. Between 1981 and 1984, the number of employees increased 12 times, making PE the fastest growing airline in history. This rapid growth created problems, however, as customer service suffered, flights were regularly delayed and overbooked, baggage was routinely mishandled, and employees complained of confusion – leading the airline to be renamed "People's Distress" by customers. In addition, the acquisition of Frontier Airlines in 1985 led to a sharp clash between PE's culture and that of Frontier, which operated more like a mainstream airline.

That PE's difficulties resulted from coordination problems is evident in the steps management took to solve the performance problems. After problems began to surface, operations were split up into small groups "intended to recreate the sense of being on a team, the direct communication, and the personal control that was being lost in the total company" and "where everyone knew everyone." Thus, by attempting to recreate small groups where coordination problems are easier to solve and communication is improved, management revealed their belief that coordination failure was largely responsible for the performance decline.

enter will be consistent with the way we conduct business throughout our system. . . We have to feel that we can hire Southwest-type people." McGlade also stressed that Southwest would "possibly pass over a city if it could not retain the Southwest 'luv' culture." For example, there were concerns about adding flights to Baltimore because "many feared that the airline would be unable to find Southwest-type employees on the East Coast."[29]

In addition, slower growth enabled the airline to choose "Southwest-types" more carefully, and to guarantee that new employees became immersed in the system more effectively. As additional means to maintain successful coordination on the "luv" culture, Southwest encouraged employee referrals of friends and family and required new employees to attend one of two "People Universities." In fact, this reliance on culture to create a large, efficiently operating organization is noted by Herb Kelleher, Southwest's founder and CEO. Colvin (1997) writes of a conversation he had with Kelleher:

> Once . . . I tried telling Herb that his culture wasn't all that important. "I can explain Southwest's success," I said. "You fly one type of aircraft, serve no meals, transfer no luggage, give no assigned seats, fly mostly on short hauls, and always charge the lowest fares on your routes. There's the formula. What's culture got to do with it?" Perhaps steam did not actually shoot out of his ears, but it looked as if it would. He slammed the table and said, "Culture has everything to do with it – because everything you said our competitors could copy tomorrow. But they can't copy the culture, and they know it." (p. 300)

Colvin goes on to note that some airlines, most notably United, have recently at-

[29]It is worth noting that Southwest dropped from first place in the annual Airline Quality Rating (a survey conducted of passengers' experiences with areas such as baggage handling, on-time arrivals, denied boardings and customer service) – a place it held for several years – to fifth in 1998. This coincides with a year in which Southwest grew rapidly and added service to several East Coast cities (Wichita State University News Release, April 19, 1999).

tempted to copy the things he mentioned, but have met with only limited success.

The above case study presents a convincing picture that Southwest's success throughout a period of growth is due largely to the approach to growth itself. By adding employees and markets slowly and by stressing the importance of maintaining the firm's culture for uniting action, the airline was able to reduce the coordination problems that are usually associated with growth.

An interesting question then is whether this approach to growth can yield similar results for other organizations. The following model of behavior in weak-link coordination games shows that in fact this is the case. By starting off with a small, efficiently coordinated group and then growing the group slowly enough and exposing new entrants to the group's history, large groups can be "grown" that are more efficiently coordinated than groups that begin at a large size.

3.2.5 A model of successful growth in weak-link games

This section presents a model that shows that growth can lead to large groups coordinated at higher levels of efficiency in the weak-link game. The intuition behind this result is simple. Players in a weak-link game are initially unsure of what action others will take (and, therefore, what the optimal response will be). Taking players first period choices as exogenous and independent of group size,[30] this uncertainty leads to choice error, represented by an exogenous mean-zero error term which is added to players choices in the first period of the game. Since the expected value of the minimum is determined by the distribution of first period strategies and by the variance of the error term, if this variance is positive and sufficiently small, the expected value of the minimum choice in small groups may be the action corresponding to the efficient

[30]In previous experiments using the weak-link game, the distribution of first period choices is similar across group sizes, indicating that players are not aware of the group size effect (see Weber, Camerer, Rottenstreich, and Knez, 1998).

equilibrium, while the expected value of the minimum in large enough groups may be considerably lower. If players adjust their choices towards the previous minimum and the variance of the error term decreases sufficiently rapidly with repeated play, small groups can remain efficiently coordinated while large groups collapse toward the inefficient equilibrium. Crawford (1995) uses this type of model to explain the laboratory result that small groups coordinate successfully while large groups do not.

What this chapter shows is that a growth process that starts off with two-player groups, allows them sufficient time to coordinate successfully, and then adds players at a slow enough rate can create large groups that are coordinated at higher levels of efficiency in expected value than groups that start off large. A key assumption underlying this result is that future entrants to the game who are allowed to watch the outcomes of the group actually playing the game will experience a similar decline in the variance of their error term to those actually playing the game. This is because observing outcomes reduces their uncertainty and leads to smaller trembles in their choice. While this may seem an unreasonable assumption to adherents of pure choice reinforcement learning models in which players only learn by experiencing outcomes, there are two reasons why this is not the case. First, the error term represents trembles due to uncertainty concerning the behavior of other players. If information about previous outcomes in the game is commonly known to both players and future entrants, then this uncertainty is unlikely to be as high as if it is not, and should be reduced for everyone. Second, there is experimental evidence that players' behavior in games is affected by observing the outcomes of other groups of players playing the game (Duffy and Feltovich, 1998) or by simply asking players to think about what other players might be doing (Weber, 1999). This implies that players and future entrants are more sophisticated than choice reinforcement models suggest and do, in fact, update their beliefs concerning the strategies of others based on more than just experienced outcomes.

The model is similar to work by Crawford (1995) and Kandori, Mailath, and Rob

(1993) in that it assumes equilibrium arises in repeated play out of a dynamic adjustment process rather than by arising initially through a refinement of the set of supergame equilibria. The advantage of this type of model is that it does not require players to be coordinated initially, but rather they begin by making choices under uncertainty and converge to one of the equilibria once the uncertainty is reduced. The goal of this model is not to provide a complete description of behavior in weak-link coordination games (for a much better example of that, see the paper by Crawford), but rather to formalize the above intuition about why growth works in a basic adaptive dynamics framework.

Assume that $N = \{1, \cdots, n\}$ is the set of $n \geq 2$ players playing the weak-link game represented in Table 3.2. Let the action taken by player $i \in N$ in period t be determined by a latent variable, x_{it}, where:

$$x_{it} = s_{it} + \epsilon_{it} \tag{3.1}$$

The terms x_{it}, s_{it}, and ϵ_{it} can be any real number. A player's choice, $a_{it} \in \{1, 2, \cdots, 7\}$, results from a function mapping x_{it} into the integer choice set. Let this function be weakly increasing such that if $x_{it} > x'_{it}$ then $a_{it} \geq a'_{it}$. Further, assume that if $x_{it} = m$, where m is an integer between 1 and 7, then $a_{it} = m$ as well. A function consisting of cutoff points satisfies these criteria.

The error term in the above equation, ϵ_{it}, represents noise in player i's choice due to uncertainty about what others will do. Let the error terms for all players be independent identically distributed normal random variables with mean zero and common variance σ_t^2. The term s_{it} represents player i's intended choice in the absence of uncertainty. Assume that in the first period every player intends to play the action corresponding to the efficient equilibrium ($s_{i1} = 7$ for all i) – since everyone wants to coordinate on the optimal equilibrium – but that the resulting actions may not all be seven because players' trembles due to uncertainty about what others will do, reflected in the error term.[31]

The n players' payoffs are then determined according to Table 3.2, which shows the payoff to every player resulting from a combination of his or her choice, a_{it}, and the minimum of all the choices. The following result has been shown previously by Crawford (1995) for a similar model of behavior in weak-link games.

Proposition 3.1 *Assume $\sigma_t^2 > 0$. Holding all else constant, then the expected value of the minimum choice in a period will be (strictly) lower if n is (strictly) larger. If $\sigma_t^2 = 0$, then the expected value will be unaffected by n.*

The proof is simple and is shown in Appendix A.

Thus, if players are not allowed to communicate prior to playing the game for the first time and there is therefore strategic uncertainty and σ_t^2 is greater than zero, then the expected value of the minimum will be higher when N consists of two players than when it is a large group. The intuition behind this result is clear and it should not come as a surprise. It is supported by the results of several experiments showing that while first period choices are similarly distributed, the first period minimum is lower when group size is higher.[32]

[31]The results below are not changed if players' initial choices are assumed to result from some common distribution, as long as the distribution is independent of n. However, this assumption is made for simplicity and to argue that with sufficient communication and decreased error, efficient coordination in the first round is possible.

[32]See, for instance, Van Huyck, Battalio, and Beil (1990) and Weber, Camerer, Rottenstreich, and Knez (1998).

In periods after the first, players' s_{it} converge toward the minimum in the previous period.[33] Assume this takes place according to the following linear adjustment process:[34]

$$s_{it} \;=\; (1-b)s_{it-1} + by_{t-1} \tag{3.2}$$

where y_t is the minimum of the a_{it} for $i \in N$ in period t. The parameter b represents the weight placed on the previous period's minimum. When $b = 1$, every players' s_{it} is exactly equal to the previous period's minimum (which is a best response). When $b = 0$, on the other hand, the s_{it} remain stationary across periods. Since it is unreasonable to believe that players will fail to react to the previous minimum entirely, assume that $0 < b \leq 1$.

Again, the x_{it} and the players' choices are determined by equation (1), with the uncertainty terms ϵ_{it} determined as before by n i.i.d. draws from a random variable distributed $N(0, \sigma_t^2)$. Since the error term represents uncertainty regarding other players, assume that its variance decreases with experience so that $\sigma_{t+1}^2 < \sigma_t^2$ for all t less than some $T^* > 1$, as long as outcomes are publicly announced.

The following proposition shows that for $t > 1$ the expected value of the minimum

[33]Crawford (1995) also includes a drift parameter that measures trends in players' adjustment of their beliefs. This additional parameter allows players' beliefs to change systematically in addition to reacting to previous outcomes. This parameter is not included because, as mentioned above, the goal is not to include a complete description of players' behavior in weak-link games, but rather to formally demonstrate the intuition behind why growth works. Also, Crawford's estimation of his model using experimental data showed that this drift parameter tended to be close to zero and statistically insignificant.

[34]This model assumes that the only feedback players obtain at the end of a period is the minimum choice (which they can then use to calculate their payoff). This is the case in many of the experiments.

y_t will be greater when the minimum in the previous period was higher.

Proposition 3.2 *Holding all else constant, then the expected value of the minimum choice in period t will be higher if the minimum choice in period $t-1$ was strictly higher.*

The proof is simple and is again left for Appendix A.

So far, we have assumed that N is constant across periods. However, given that the purpose of this chapter is to study growth and coordination together, it is necessary to relax this assumption. Therefore, replace N with $N_t = \{1, \cdots, n_t\}$ ($n_t \geq 2$, $\forall t$), which is the set of players playing the game in period t. Thus, the number of players can change from period to period.

We now need to define a growth path:

Definition 3.1 *Let $T > 1$. Then, define a* **growth path,** $\mathbf{G}$ *as a collection of ordered sets $\{N_1, \cdots, N_T\}$, where $N_t = \{1, \cdots, n_t\}$, such that $N_t \subseteq N_{t+1}$ for all t, $N_t \subset N_{t+1}$ for at least one t, and $N_t = N_{t-1}$ for all $t > T$.*

Therefore, a growth path is defined as a series of sets of players that are weakly increasing in size over time and where the number of players at period T is strictly greater than at period 1. Note also that the maximum number of players is attained by period T, after which the size of the group does not grow.

Given the definition of a growth path, there is some period t in which there are individuals who are not participating in the game but who will be playing the game in a future period (i.e., $i \in N_T \setminus N_t$). If these individuals are present in the room when the game is being played and the outcomes in every period are announced publicly, then these future entrants' uncertainty concerning the behavior of other players is reduced similarly to the uncertainty of those playing the game since they observe the same outcomes. Since the choice error is due to this kind of uncertainty, it is reasonable to assume that the variance of ϵ_{it} will decrease for these future players in the same way as for actual players. Therefore, let σ_t^2 be the same for all $i \in N_T$.

Recalling that a growth path G reaches its maximum number of players in period T, we are now ready to prove the first main result. Assume in both of the following propositions that σ_1 is always strictly positive.

Proposition 3.3 *Holding all else constant, the ex ante expected minimum choice for all periods will be higher for a growth path G ending at a group size of* n_T *than for a repeated game in which the group size is constant at* n_T.

Proof. Start by labelling the group following growth path G the "grown" group and the other group the "constant" group. Then, by the definition of a growth path, we know that $n_1^{grown} < n_1^{constant}$. Therefore, by Proposition 3.1, we know that the expected value of y_1^{grown} will be higher than the expected value of $y_1^{constant}$. Proceeding by induction, we know that there are only two ways in which the games will differ in period t. First, starting with $t = 2$, we know that (ex ante) y_{t-1}^{grown} will be higher than $y_{t-1}^{constant}$ and that by Proposition 3.2 this implies that in expectation $y_t^{grown} > y_t^{constant}$. The only other way in which the games will differ is that for $t < T$, $n_t^{grown} \leq n_t^{constant}$. By Proposition 3.1, we know this implies that in expectation $y_t^{grown} \geq y_t^{constant}$. In either case, the result holds. *Q.E.D.*

This shows that growing large groups – by starting off with smaller ones and adding players – will always lead to more efficiently coordinated groups (in expectation) than starting off at large group sizes. This implies that growth is a way to solve the large group coordination failure in weak-link games.

The following definition introduces the concept of outcome-equivalence, which indicates that two growth paths start and end with the same number of players.

Definition 3.2 *Two growth paths, G and G' are* ***outcome-equivalent*** *if and only if $N_1 = N_1'$ and $N_T = N_T'$.*

Note that this does not imply that they need to grow at the same speed or reach the maximum group size in the same period. In fact, this definition allows the possibility that one growth path jumps from size n_1 to n_T between the first and second periods and the other growth path takes any finite number of periods to do so.

If two growth paths are outcome-equivalent, then we can compare the speed at which they grow.

Definition 3.3 *If two growth paths, G and G', are outcome-equivalent, then G is* ***faster*** *than G' if $N_t' \subseteq N_t$ for all t and $N_t' \subset N_t$ for at least one t. Growth path G is* ***slower*** *than G' if $N_t \subseteq N_t'$ for all t and $N_t \subset N_t'$ for at least one t.*

Therefore, a faster growth path will always have at least as many players in a given period, and will have at least one more player in at least one period. The opposite will hold for a slower growth path.

In the next result, we compare two outcome-equivalent growth paths where one is faster.

Proposition 3.4 *If two growth paths, G and G' are outcome-equivalent and G is faster than G', then the ex ante expected value of y_t will be weakly lower than the ex ante expected value of y'_t for all t and will be strictly lower for at least one t.*

Proof. To prove this result, first look at the most extreme case where the definition of "faster" is minimally satisfied (i.e., $N_t = N'_t$ for all t except one (labeled t^*) at which $n_{t^*} = n'_{t^*} + 1$). Therefore, for all $t < t^*$, we know that $n_t = n'_t$ and, since everything else is also equal, the expected values of y_t and y'_t are equal as well. In period t^*, the only difference is that $n_{t^*} > n'_{t^*}$. Therefore, by Proposition 3.1 this implies that (in expectation) $y_{t^*} < y'_{t^*}$. Now, we know that for $t > t^*$, $n_t = n'_t$ and the only difference between the two games is in the previous period's minimum. We can then show by induction that, starting with $t = t^* + 1$, if the only difference at t is that $y_{t-1} < y'_{t-1}$, then by Proposition 3.2 we know that in expectation $y_t < y'_t$.

Having shown this, the remainder of the proof is simple. By definition, the only way in which the difference between the two growth paths can change for any t is for $n_t - n'_t$ to increase. By Proposition 3.1, we know that by increasing n_t relative to n'_t and holding all else constant, the result will be that the expected value of y'_t will increase relative to the expected value y_t. Since by Proposition 3.2, we know that this in turn will only lead to lower values (in expectation) of y_{t+1} relative to y'_{t+1}, the effects of moving the growth path away from the extreme case will only increase the difference between the expected values of the minima in the direction indicated in the proposition. *Q.E.D.*

The main result above (Proposition 3.3) shows that starting off small and growing a group leads to a more efficiently coordinated group (in expectation) than one that starts off at a large group size. However, it is perhaps unreasonable to assume that introducing new players will have no effect on the uncertainty that underlies error in all players' choices. Since the new entrants have not actually played the game before, it is likely that their introduction will increase the uncertainty and trembles in players' actions.[35] We can model this increase in uncertainty as a positive shock to σ_t^2 whenever $n_t > n_{t-1}$, meaning that the choice error increases with an increase in group size. In any period t, this shock is represented by the term z_t, where:

$$z_t = z(\tilde{n}_t, n_{t-1}, r) \tag{3.3}$$

The term $\tilde{n}$ is the increase in group size between periods $t-1$ and t and is therefore equal to $n_t - n_{t-1}$. The number of previous periods in which the group grew is given by the term r, which represents experience with growth.

The function z is increasing in $\tilde{n}_t$, decreasing in n_{t-1} and r, and always lies between 1 and some positive value $z^{MAX} < \infty$. Moreover, $z_t = 1$ whenever $\tilde{n}_t = 0$ and is greater than 1 otherwise. Letting σ_1^2 be exogenous, assume that in every period σ_t^2 decreases geometrically by a constant $k < 1$ so that

$$\sigma_t^2 = k\sigma_{t-1}^2 z_t \tag{3.4}$$

for all $t > 1$. As mentioned above, z_t is equal to 1 unless the group grows in period t and is greater than one otherwise. Since z is increasing in $\tilde{n}_t$, a larger increase in

[35] In addition, we may want to model growth when there is not common knowledge about outcomes.

group size will result in a larger increase in the variance of the choice error term. Conversely, since z is decreasing in n_{t-1} and r, the increase in the variance will be smaller when the size of the group about to grow is larger and when the group has had more experience with growth.

We can then prove the following result, which shows that a version of Proposition 3.3 holds even when σ_t^2 increases whenever the group grows.

Proposition 3.5 *For any function $z(\tilde{n}_t, n_{t-1}, r)$ such that $1 \leq z \leq z^{MAX}$ and such that z is increasing in $\tilde{n}_t$ and decreasing in n_{t-1} and r, it is possible to construct a growth path, G, ending at a group size of n_T, such that the expected value of the minimum for this grown group will be higher in all periods than for a repeated game in which the group size is constant at n_T.*

Proof. Start by again labelling the group following growth path G the "grown" group and the other group the "constant" group. For any final group size, $n_T \geq 2$, start the grown group at a group size of 2. This smallest group size means that the expected value of y_1^{grown} will be the highest, and that it will be higher than $y_1^{constant}$ (by Proposition 3.1). Also, by Proposition 3.2, the expected value of the minimum for the grown group will be higher for all periods before the group grows. Since $z(\tilde{n}_t, n_{t-1}, r)$ is increasing in $\tilde{n}_t$, let growth path G never grow the group by more than 1 player (i.e., $\tilde{n}_t = 1$ for all t in which $\tilde{n}_t \neq 0$). Therefore, since $z(\tilde{n}_t, n_{t-1}, r)$ is decreasing in its remaining two arguments, n_{t-1} and r, it is sufficient to show that there exists a period t' in which increasing the group size from 2 to 3 results in an expected value of $y_{t'}^{grown}$ that is greater than the expected value of $y_{t'}^{constant}$. As long as the result holds in this case, it will hold in subsequent growth episodes when $\tilde{n}_t$ is unchanged and n_{t-1} and r are both greater.

Note that $z_{t'} = z(1, 2, 0)$ is independent of t' and that, therefore, $\sigma_{t'}^2$ is equal to

$k^{t'-1}\sigma_1^2 z(1,2,0)$. From Proposition 3.3, we know that the expected value of y_t^{grown} is greater than the expected value of $y_t^{constant}$ for any value of t and for $\sigma_t^2 = k^{t-1}\sigma_1^2$. This implies that as long as there exists a period t' in which $k^{t'-t}z_{t'}$ is less than 1, then $y_{t'}^{grown}$ will be greater than $y_{t'}^{constant}$ in expectation. Since z is bounded from above by z^{MAX} and $k < 1$ we know that there exist large enough values of t' for which this is true. *Q.E.D.*

Note that the introduction of an increase in variance coincident with growth means that growth will have to be slower. If z is increased by less shared information between players, then the intuition that slower growth is required when there is lack of common knowledge about past outcomes is supported by the model.

The last three results show how the growth process itself can lead to more efficient coordination, first relative to a group that starts off at a large size and then relative to a group growing more quickly. This result holds when uncertainty and choice error are increased with growth. While the model is simple and does not capture all the elements of behavior in weak-link coordination games, the intuition behind the results is quite clear: by starting off small and then growing a group slowly, it is possible to create large, efficiently coordinated groups.

3.3 Discussion

Coordination is an important problem facing firms. Since there is both theoretical and experimental evidence that coordination becomes more difficult with size, this is particularly true for growing firms. It is a widely held belief that organizations can encounter problems when they grow and this chapter has argued that a large reason for these problems is due to coordination failure. Without implementing better communication mechanisms and adapting the organizational form and its practices to better deal with coordination problems, growing firms are almost certain to encounter these problems.

The example of Southwest Airlines provides an interesting counter-example, however. By growing slowly and taking great pains to expose new employees to the organizational culture, the airline was able to grow successfully and solve many of the coordination problems that plagued other airlines such as People Express. Thus, in addition to the above means, it may be possible to manage coordination problems arising during growth through the growth process itself.

This was in fact shown to be the case by applying a model of adaptive behavior with uncertainty to the weak-link game. The results of the model are interesting because they show that growth can generate higher levels of efficiency. A key assumption is that the uncertainty of players entering the game in later periods is reduced identically to that of actual players as long as outcomes are commonly observed by everyone. This assumption seems reasonable in light of existing experimental evidence and corresponds to the strong socialization process at Southwest and other firms, where new employees were exposed at length to how things were done at the airline.

Two interesting questions arise immediately concerning future work. First, it might be interesting to further explore the implications of varying the informational assumption. For instance, introducing some measure of shared history as an argument in z, might lead to further results supported by intuition. In addition, if it is commonly known that entrants are unaware of the history, then the uncertainty of players previously in the game might increase by a greater amount when these uninformed entrants are included in the game, meaning that z is not the same for everyone. In fact, there are several possible information manipulations which might have interesting effects on the results of the model.

Second, since it is often beneficial to discipline theory by testing its implications, it seems worthwhile to test the predictions of the model. One way to do so is with controlled laboratory experiments that look at whether or not slow growth produces more efficiently coordinated large groups. This is ideal since, while the theoretical results can be applied to coordination in actual growing organizations, the laboratory provides a controlled environment in which to directly test the model. In addition, once the theory is developed to include alternative information assumptions, then the changes to the results can be tested quite easily as well. While this is left as work to be done in the future, a direct test of the results in this chapter is performed in the next chapter.

Chapter 4 Experiments on growing efficient coordination

The difficulty faced by large groups in coordinating activity has been discussed both by organizational theorists (e.g., March and Simon, 1958; Chandler, 1962; Thompson, 1967; Nanda, 1996), and by game theorists and experimental economists (e.g., Van Huyck, Battalio, and Beil, 1990; Crawford, 1995; Anderson, Goeree, and Holt, 1996; Weber, Camerer, Rottenstreich, and Knez, 1998). The previous chapter explored solutions to the problem of large group coordination failure. Previously discussed approaches include improved communication and more suitable organizational forms. The key contribution of the last chapter, however, was to note that the growth process itself can be used to manage coordination problems. Motivated by a case study, an adaptive model of behavior in weak-link games was used to demonstrate that growing groups – starting off with a small group size and adding players – can lead to more efficiently coordinated large groups.

This chapter tests this result using experiments. The experiments are conducted similarly to previous experiments by Van Huyck, et al. (1990) and Weber, et al. (1998). The goal of these experiments is simply to test the main result from the previous chapter that "grown" groups are more efficiently coordinated than groups that start off at a large group size.

Two sets of experiments are reported. In experiment 1, the rate of growth was determined by the experimenter prior to the experiment. These experiments were intended to test whether growth paths that satisfy several desirable criteria can produce large groups coordinated at higher levels of efficiency than control groups. A second set of experiments was also conducted in which a participant not playing the weak-link game determined the size of the group. In these experiments, groups were again grown, but the rate of growth was endogenous and not determined until the experiment. Each experiment will be discussed in detail, and then the aggregate results will be used to determine the effectiveness of growth for solving large group coordination failure.

4.1 Experiment 1: Can controlled growth solve large group coordination failure?

4.1.1 Experimental Design

Since large groups of ten or more subjects have never consistently coordinated efficiently in previous experiments – in fact, they almost always end up at the least efficient outcome – this first set of experiments was designed to explore whether a slow, controlled growth rate determined by the experimenter could create large groups that coordinated efficiently. In the experiments, groups of 12 Stanford and UC Santa Cruz students were assembled in one room. The game was presented in the context of a report completion as in Weber, et al. (1998), and as described earlier. Instructions were read aloud and subjects answered several questions to check their comprehension of the instructions.[1]

In each experimental session, subjects were anonymously assigned participant numbers. Each session consisted of 22 periods. In the first several periods, only participants 1 and 2 played the weak-link game while the other subjects sat quietly.[2]

[1] Instructions are available in Appendix C.

[2] An important design challenge is how much to pay the non-participants for the periods in which they sat by waiting to be "hired." If they were paid nothing, they might be resentful at earning less than early participants which could, in turn, inflame or introduce social utilities. Announcing a fixed amount they earn per period could create a focal point which would influence the choices participants made. On the other hand, simply informing subjects that they would receive an unspecified fixed amount might lead them to believe that the amount would be determined by the experimenter as a result of behavior in the experiment. As a solution, the design allowed the experimenter to

In each period, participating subjects recorded a number from 1 to 7 (indicating the contribution time for their section of the report) on a piece of paper and handed it to the experimenter.[3]

At various preannounced and commonly known points, other participants joined the group of those actively playing the game. For each session, there was a schedule of such additions that was handed to all subjects at the beginning of the experiment. These schedules will be referred to as *growth paths*. For example, in one of the growth paths a third participant was added in period 7, joining the first two participants who continued to participate. Subjects all knew the predetermined growth path which explained when they began to participate, and they knew that earlier participants always continued to participate.[4] At the end of each period, the report completion time (minimum) was announced to all the subjects, including those who were not actively participating yet. In all growth paths, all 12 subjects were participating by the last few periods.

This design is intended to model behavior in a small firm which consists of only two "founders" and slowly adds employees who know the entire history of the firm's outcomes. There is no turnover (no employees "leave") because simultaneously studying the effects of turnover would complicate the conclusions that can be drawn concerning the effects of growth.

One possible criticism of the design is that the resulting "large" groups of 12 subjects are still relatively small by organizational standards. In a firm with 12 employees, for example, coordination would typically be achieved easily through direct communication and personal contact. However, by not allowing any communication

precommit to a per-period payment for non-participants by writing it in envelopes, which were handed to them, but were only opened at the end of the experiment.

[3]To prevent players from knowing which others were participating, all players handed in slips of paper; non-participants simply checked a box saying they were not participating.

[4]In addition, at the beginning of each period the experimenter announced which participants would actively participate in that round.

between subjects – other than through actions in the game – the complexity of the coordination problem faced by each group resembles the complexity of coordinating activity in much larger groups in real organizations. Therefore, the two-person game without communication might be more analogous to a small office with less than 20 employees who do communicate and meet regularly. The 12 person groups without communication, on the other hand, are more likely to resemble firms with several hundred employees that do have limited communication channels available than they are to resemble 12 person groups in real organizations – particularly given the difficulty previously observed with achieving coordination in laboratory groups larger than ten. This point is made by Weick (1965), who argues:

> As size increases, several things happen: subgroups form, communication is more difficult, and hierarchies grow taller and more explicit. To reproduce a "large" organization, one or more of these three characteristics can be added to the experimental situation. Although these three characteristics do not exhaust the accompaniments of size, they are representative, manipulable, and common in existing definitions of organization. Thus, they increase the correspondence between experimental and field conditions. (Weick, 1965, p.210)

Another reason for dismissing the above criticism is that the experiments are intended to address whether growth is a means for solving large group coordination failure. In order to answer this question, the experiments need to identify one group size where coordination is not difficult and one where it always fails. Then the experiments need only address whether efficient coordination is improved in the large groups when they are grown relative to when they start at a large size. The results reported in Table 3.3 show that the group sizes used in this experiment do, in fact, satisfy that criterion. Therefore, as long as there are parallels between the coordination problem in the experiments and problems faced by organizations, the success in groups that began at a size of 2 and were subsequently grown to a size of 12 in the experiments provides an insight into how large firms might solve their coordination problems through growth.

In addition, the experiments are deliberately oversimplified. The goal is not to create a rich lifelike replication of growing firms, but instead to strip away all the inessential details to focus on the strategic uncertainty facing employees in such firms when new members come in, and everyone desires to coordinate but choosing high effort actions is risky.[5] Moreover, the experiments are intended to build on previous research on weak-link games. In order to do so, it is necessary to change only a few design variables at a time – in this case, introducing growth. Finally, simple experiments provide a necessary baseline to which other features can be added in further research.

Another possible criticism of the design is that the only mechanism helping a group coordinate as it grows is the growth process itself. Therefore, the experiments ignore the usefulness of communication (March and Simon, 1958; Thompson, 1967; Heath, 1999), different organizational forms (Leavitt, 1962; Chandler, 1962; Scott, 1998), and specialized integrating units or employees (Lawrence and Lorsch, 1967) in improving coordination as a firm grows. There are several reasons why these alternative mechanisms were not included in the experiments and why they might not always work in real organizations.

First, as mentioned above, the experiments were intended to study only the effects of growth. In order to do this, it is necessary to hold as much possible constant except for the growth treatment variable. Therefore, while implementing several changes aimed at improving coordination at the same time might more realistically reflect the practices of growing firms, this would make it difficult to isolate the effects

[5] Weick (1965) provides similar and additional reasons for why experiments on organizations often require a very simplified version of the real situation.

of managing the growth process alone.

A second and more important reason is that some of the mechanisms may be too costly to attempt. For instance, it is difficult if not impossible to implement large changes in the structure of an existing organization. Similarly, it may be very costly to conduct a large-scale implementation of new forms of communication or to create entirely new positions aimed at coordinating activity. All of these changes would require extensive employee training and may present coordination problems themselves if some employees persist in doing things the old way.

A related reason why the alternative mechanisms might not be used is that they may not always be effective. For instance, there is experimental evidence that one-way communication only partially improves efficient coordination in stag hunt games (Cooper, DeJong, Forsythe and Ross, 1993) and that it does not improve coordination in large groups playing the weak-link game at all (Weber, Camerer, Rottenstreich and Knez, 1998). The benefits of communication can also be limited by problems of interpretation.[6] In order for communication to be effective, there has to be a well understood common language regarding what is meant by different statements. If this is not satisfied, then interpreting communication itself becomes a coordination problem. Communication is not the only one of these mechanisms that may prove ineffective. Having subjects play the weak-link game in a circular arrangement in which each subject interacts locally with only two other people rather than the whole group does not improve coordination on the efficient equilibrium (Keser, Berninghaus and Ehrhart, 1998). Therefore, while growing firms do often use some of these other mechanisms to reduce coordination problems, the fact that they are costly and do not always work makes it worthwhile to study growth alone.

In addition to the above design, control sessions were conducted to ensure replication of previous results. In these sessions, 12 groups of subjects played the game

[6] See Heath, 1999.

for 12 periods. The game was similarly framed in the context of a project completion and personal contribution times. However, no mention was made of growth or of participants not actively participating in any rounds.

4.1.2 Results

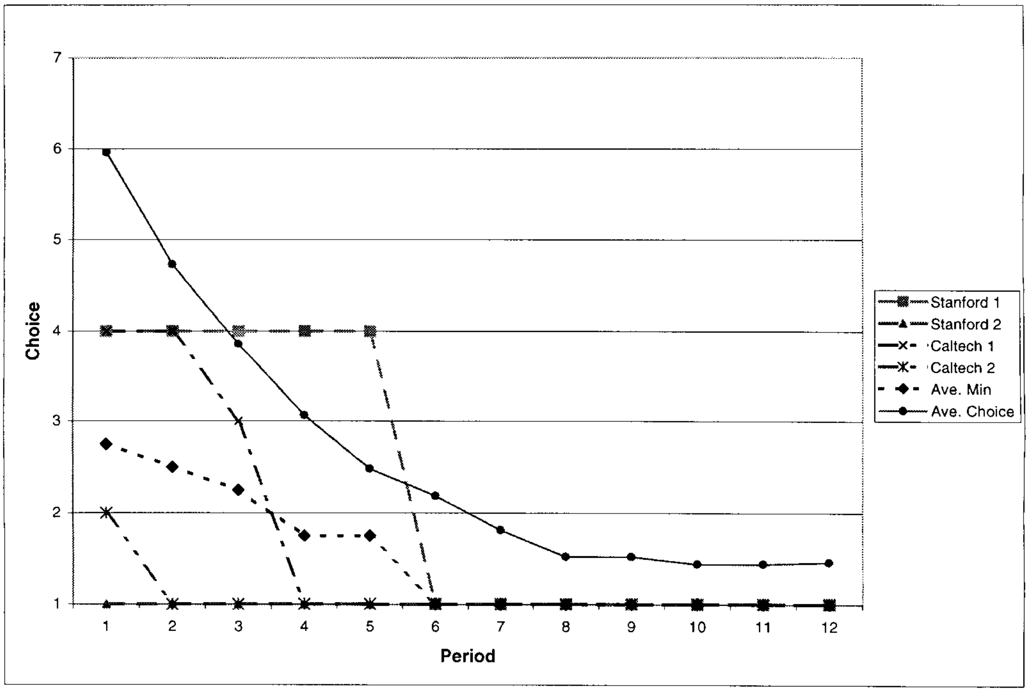
Four control sessions (n = 48) were conducted using undergraduates at both Stanford (2 sessions) and the California Institute of Technology (2 sessions) between February and December 1998.[7] The results of these experiments are reported in Figure 4.1, which presents the minimum choice across all 12 periods for each session. In addition, the solid line indicates the average of the minima in all four control sessions.

Overall, the results replicate previous experimental results on large groups playing weak-link coordination games. In two of the sessions, the minimum choice is initially 4. In the first Caltech session (Caltech 1), the minimum is again 4 in the next period but declines to 3 in the third period and then falls to 1 for the remaining periods. In the first Stanford session (Stanford 1), the minimum continues to be 4 through the fifth period and then falls to 1 for the final 7 periods.[8] In session Stanford 2, the minimum is initially 1 and continues to be so for the entire session. In the second Caltech session (Caltech 2), the minimum starts off at 2, but then goes down to 1 in the next period and remains there for the rest of the experiment. The solid line in Figure 4.1 indicates the average of the session minima. Finally, the average of all subjects' choices is also given. Note that both the average choice and the average of the minima consistently decrease and end up at or near one by the final periods. Note also that the average choice is initially high, indicating that many subjects are

[7]Some of the controls were conducted at the California Institute of Technology because this is where additional experiments on endogenous growth (experiment 2) were conducted.

[8]The results of session Stanford 1 are a bit perplexing. It is rarely the case that large groups are able to maintain a minimum greater than 1 for more than a few periods. Here, the minimum remained at 4 for longer than the few initial periods that it usually takes for coordination failure to occur and for the minimum to collapse to 1. The fact that the minimum finally falls to 1 is encouraging, as is the fact that the other control sessions exhibit behavior more consistent with previous results.

Figure 4.1: Choices in control treatment



initially selecting high effort levels but that the minimum is nonetheless low since it is sensitive to outliers.

The results of the control sessions indicate that, while behavior in one of the sessions is unusual and does not immediately converge to a minimum of 1, the expected result of coordination failure in large groups is obtained. Thus, if the growth sessions consistently establish more successful coordination (minima greater than one), the main hypothesis that controlled growth can lead to less large group coordination failure will be supported.

Seven growth sessions (n = 84) were conducted between January and March 1998. Four sessions were conducted at Stanford using graduate and undergraduate students with little or no formal training in game theory and three sessions were conducted at UC Santa Cruz using undergraduate students. It is important to note that each session of 12 subjects making 22 choices represents one data point, since the group either succeeds to coordinate efficiently or fails. It is therefore difficult to firmly establish strong results using such data, but there are clear regularities that can be observed by examining the individual session data and from which conclusions can be drawn. Thus, in this section the behavior in individual sessions will be analyzed. In a subsequent section, the aggregate results will be examined to establish more firm results.

The goal of these experiments was to explore the possibility that growth can lead to more efficiently coordinated large groups. Therefore, the first growth path studied (Figure 4.2) was selected with hopes that it would be slow enough to create a large group that behaved more efficiently than large groups had in earlier experiments and in the control sessions. The principles driving the choice of this path were to first establish repeated successful coordination in the group of size two (by allowing them to play several periods before adding more participants) and then to add players in a slow and regular manner. Thus, the growth path begins by only adding one player at a time – every two periods at first and then every period.

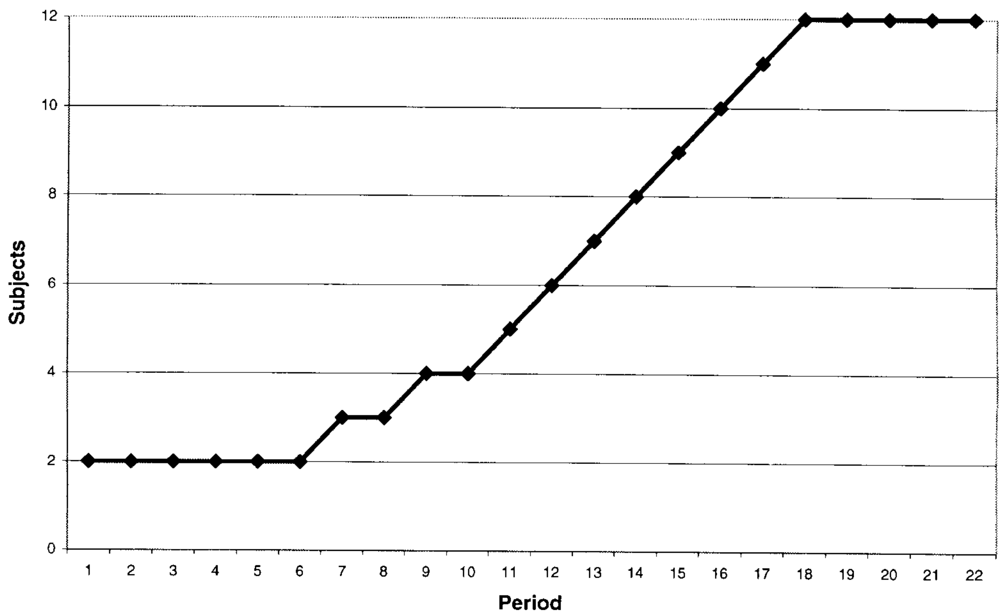
Sessions 1 and 2, presented in Figures 4.3 and 4.4 respectively, both used this growth path. The bottom part of each figure presents the minimum choice by any of the subjects participating in that period (project completion time). The top part of the figure presents the choices made by each of the participants across periods. Each segment of the bar for a given period corresponds to one individual's choice. Thus there are 2 segments in the first period and 12 in the final period.[9] The length of each segment indicates the number (weeks early) chosen by that individual – the longer the segment, the higher the choice. The shortest segment in a bar gives the minimum (completion time) for that round. The empty (non-shaded) segment at the top of each bar represents how far short the group is of the efficient equilibrium. This segment extends the bar to the length it would be if everyone were choosing 7, and therefore its length gives the difference in the sum of effort levels between the efficient outcome and the outcome actually obtained by the group. If this top segment is longer, then the average choice in that period is smaller. If this segment is not present for a particular period (as in periods 1 through 6 of session 1), then all of the participants are choosing 7 and the group is efficiently coordinated.

Both sessions 1 and 2 began with six periods in which only participants 1 and 2 played. As predicted based on earlier results, these two-person groups reached efficiency (in both sessions, the minimum was 7 in periods 5-6). However, when a third participant was added in period 7, the minimum dipped below 7 in both sessions.

In session 1, the minimum was 6 in periods 7-8. When a fourth participant was added, in period 9, the minimum fell further to 5 and stayed there in period 10.

[9] The choices in the final two periods (21 and 22) are not reported here because there is often a strong end of experiment effect in these games, in which subjects change their choices in the final period (perhaps to punish or do better that others, or perhaps because they believe that others will do so – in which case doing so is a best response). While this phenomenon is interesting, this chapter is not concerned with what occurs in the final rounds (after growth is completed), but rather with coordination during and immediately after growth.

Figure 4.2: Growth path 1



When a fifth participant was added, in period 11, the minimum fell to 4. This pattern suggests an interesting conjecture. Earlier research showed that precedents often matter dramatically, in the sense that a group expects the minimum in one period to be the minimum in an upcoming period. That is, the previous choice establishes a strong precedent that is reinforced by subsequent actions. In Figure 4.3, however, players seem to be inferring a precedent from the relation between changes in structure (group size) and changes in behavior. The fact that the minimum fell by one when group size increased from 2 to 3, and from 3 to 4, seems to create a precedent that "when we grow the minimum falls by 1," which is self-fulfilling in later periods.[10]

Figure 4.4, however, shows a different pattern. In that session the minimum was 7 in the two-person group in periods 5-6, but the minimum fell to 5 when a third player was added in round 7. The minimum rose to 6 in the next round, which indicates a possible recovery to an efficient minimum of 7, but a fourth player was added in round 9 and the minimum fell back to 5, where it remained – even when the group reached a size of 12. Note that while the minimum is not at the maximum of 7, a minimum of 5 is still much more efficient than the usually observed minimum of 1 in large groups. Thus, the fact that the minimum for a 12 person group is 5 in session 2 provides support for the hypothesis that growth can lead to successful coordination.

Because the first transition lowered the minimum from 7 to 5 in session 2, but then the minimum recovered to 6 in the next period, the question arose of whether full efficiency (a minimum of 7) could be reached by allowing the three-person group to recover by playing more periods before growing further. This was explored in growth path 2 (Figure 4.5), which begins with five periods of 2-person play, followed by four periods of 3-person play, to give the 3-person group more time to recover from any drop in the minimum after the 2-to-3 group size transition.

[10]Notice that while the minimum falls to 1 (inefficiency), the collapse is slow and regular. This contrasts with previous results and the control data where the collapse is typically much more rapid.

Figure 4.3: Session 1 data (Stanford)(Growth path 1)

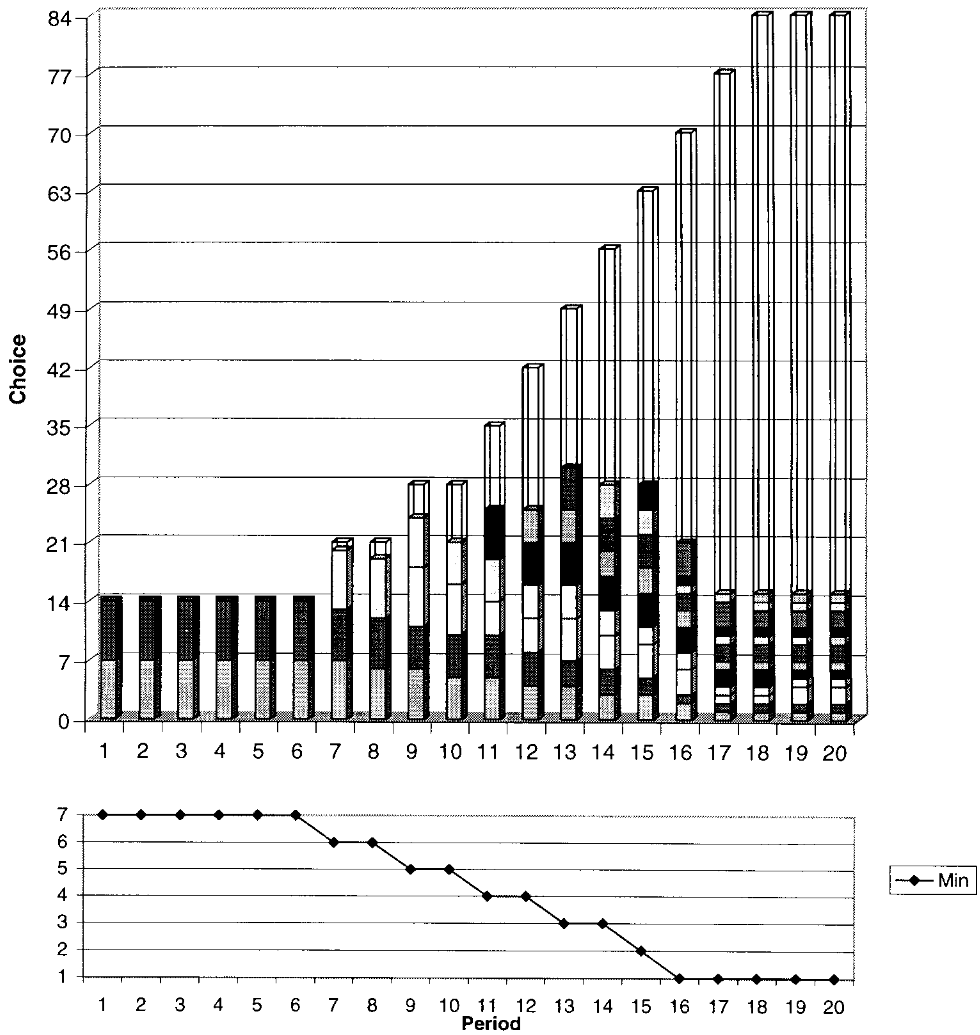


Figure 4.4: Session 2 data (Stanford)(Growth path 1)

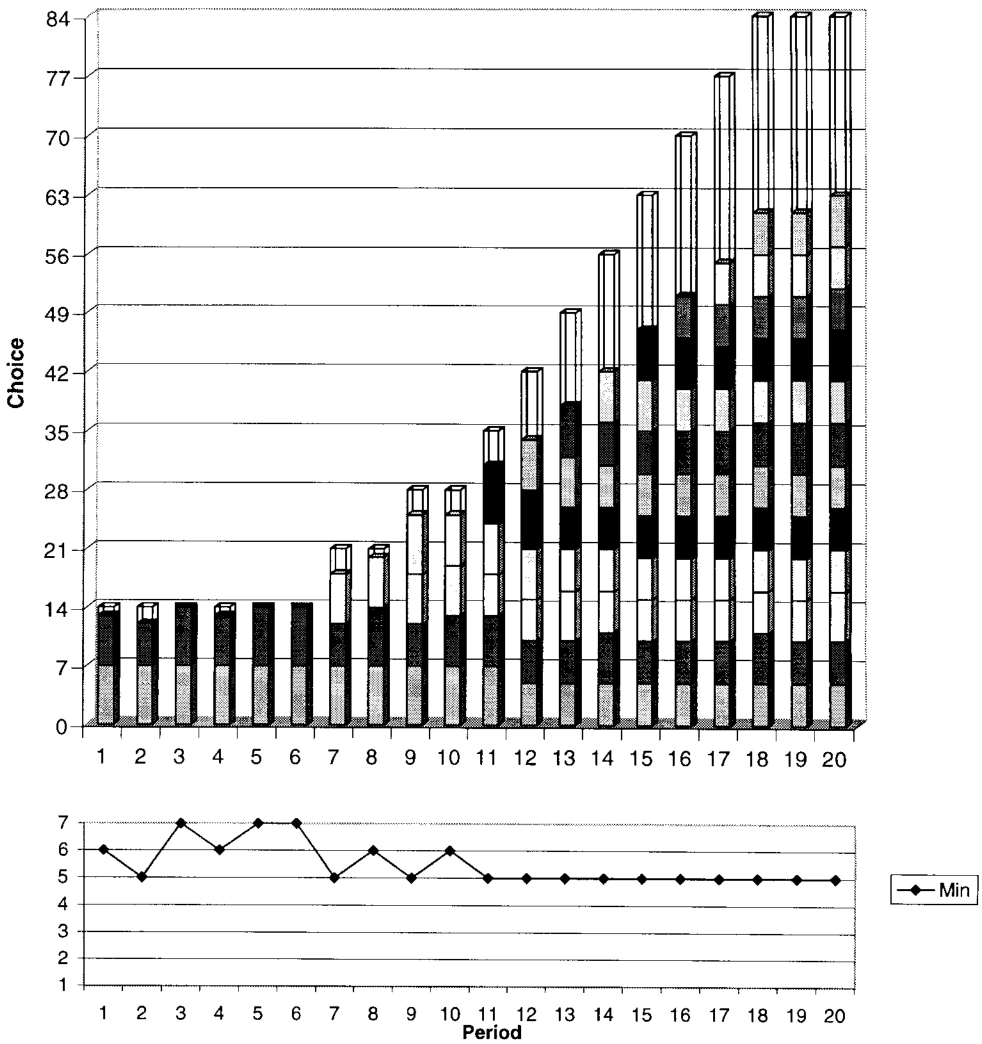


Figure 4.5: Growth path 2

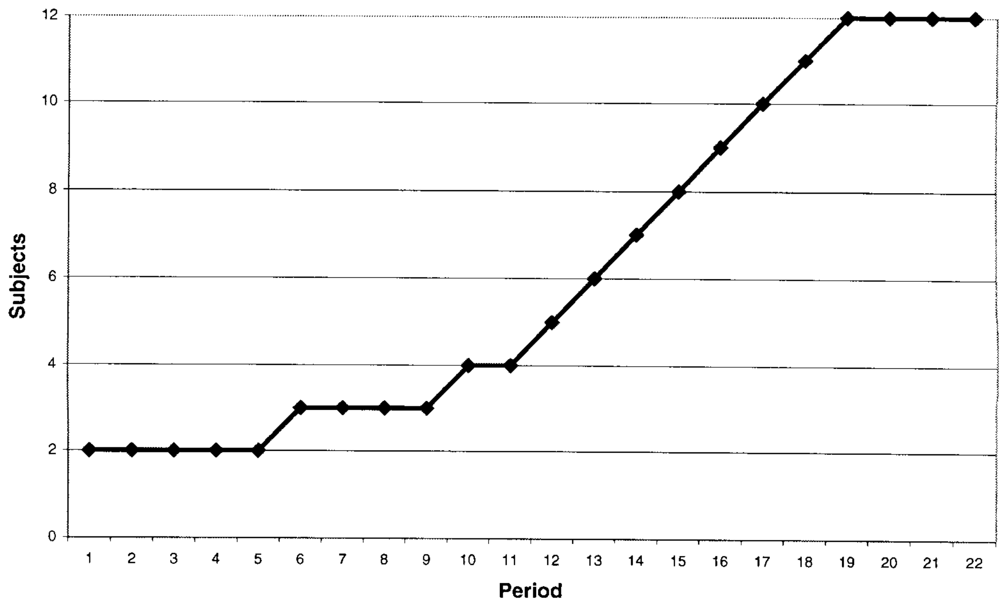


Figure 4.6: Session 3 data (Stanford)(Growth path 2)

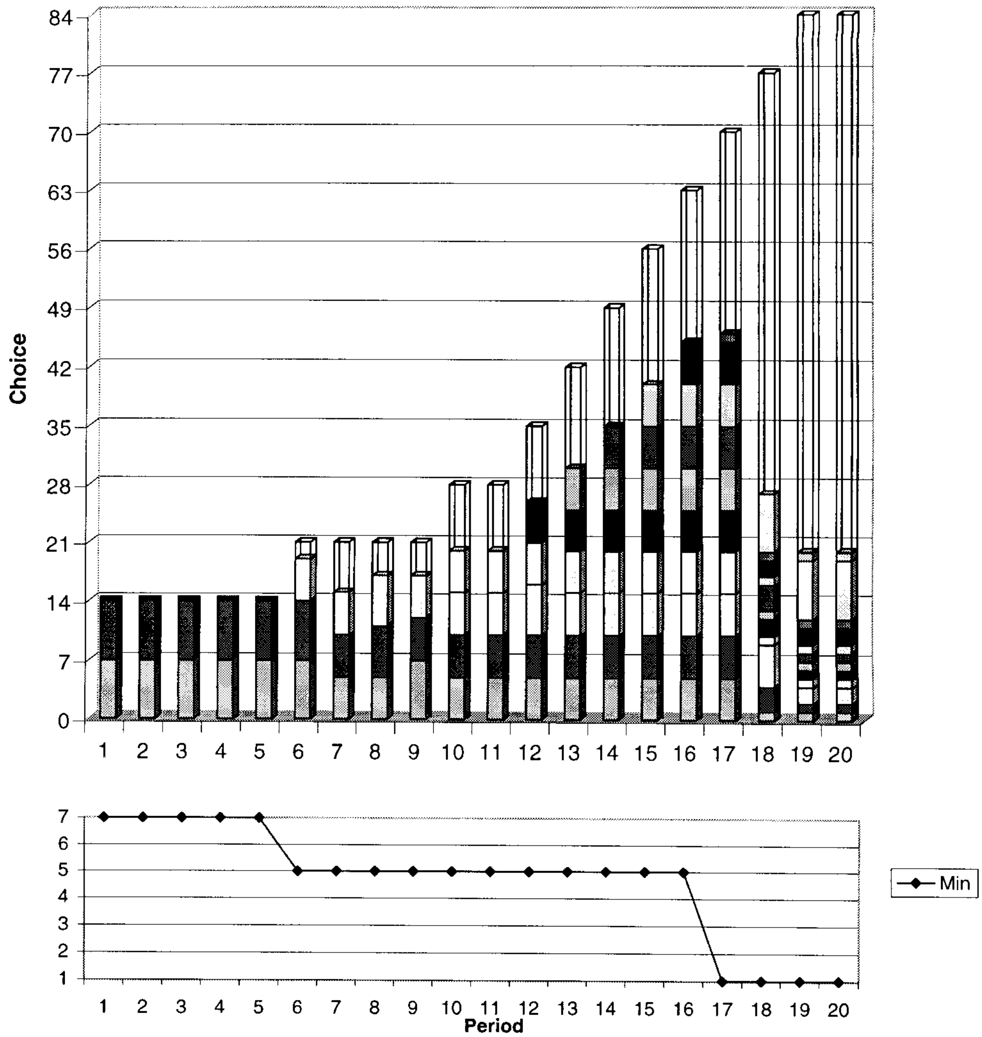
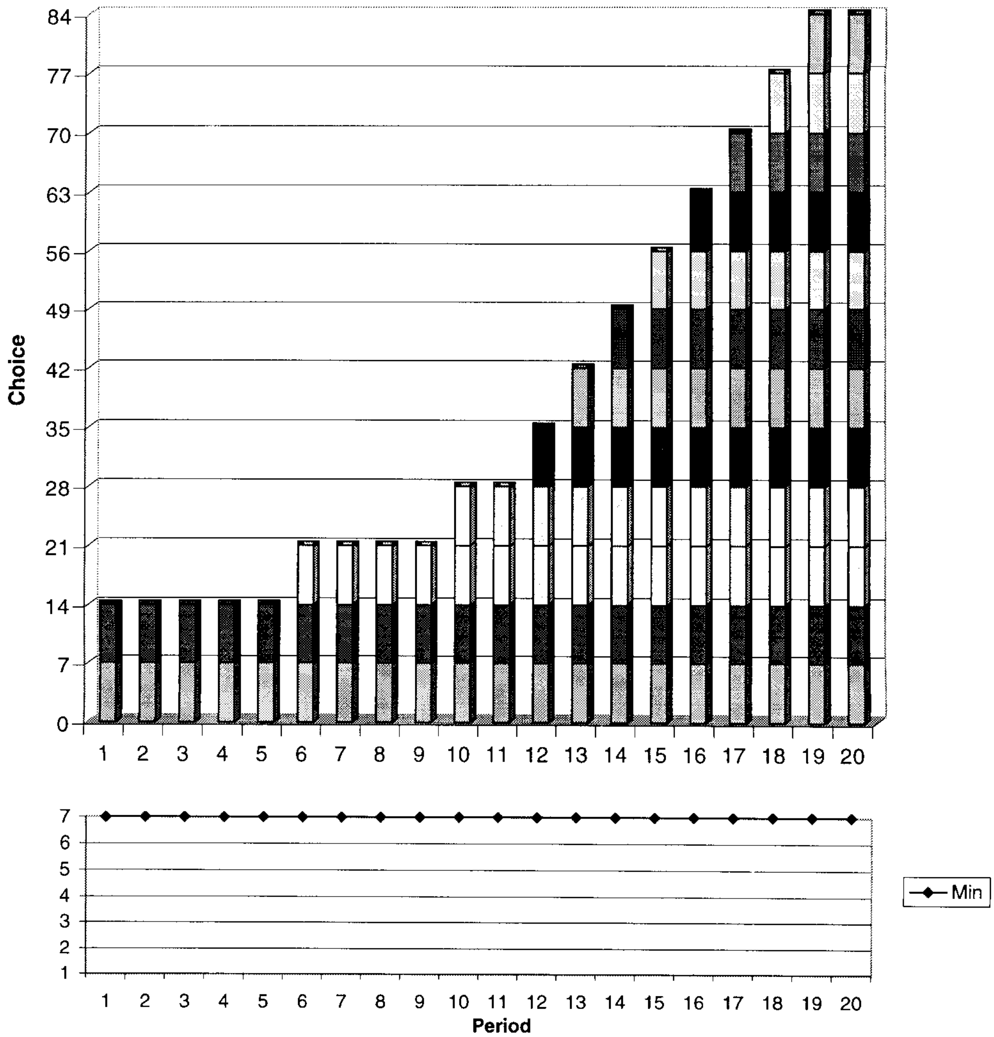


Figure 4.7: Session 4 data (Stanford)(Growth path 2)



Sessions 3 and 4 (Figures 4.6 and 4.7) used this growth path. In session 3, the minimum in the two-person group is reliably 7 for all five periods, then drops to 5 when the third person is added, and stays there. When more players are added, the minimum stays at 5 until period 17, when the 10th player enters and chooses 1. The minimum is 1 after that. While the minimum falls to 1 when the group reaches a size of 10, the fact that it remains at 5 until then provides some additional (though modest) support for the successful controlled growth hypothesis.

In session 4, the two-person group was again able to coordinate efficiently. In this session, however, when the third person was added the minimum continued at 7 and remained there through the entire growth path. Efficient coordination was obtained in all periods and an efficient group of size 12 was obtained. The results of this session provide strong evidence of successful growth.

Sessions 5 through 7 were conducted at UC Santa Cruz. These sessions used growth path 3 (Figure 4.8) which is identical to growth path 2 except that participant 12 enters at the same time as participant 11. This modification was made because of the concern that selecting one individual to be the last entrant might create greater incentives for this participant to want to punish other players.

In session 5 (Figure 4.9), the two-person group coordinated efficiently in periods 3-5. When the third person was added, the efficient equilibrium was again achieved. Efficient coordination continued until period 14, when the seventh participant entered and the minimum fell to 4. The minimum fell to 3 the next period, where it remained. While a drop in efficiency occurred when group size reached 7, the minimum did not fall the whole way to 1. This again provides some evidence that more efficient coordination in large groups can be obtained using growth.

Behavior in session 6 (Figure 4.10) was initially very similar. The minimum was 7 through period 13 when 6 subjects were participating. When the seventh participant

Figure 4.8: Growth path 3

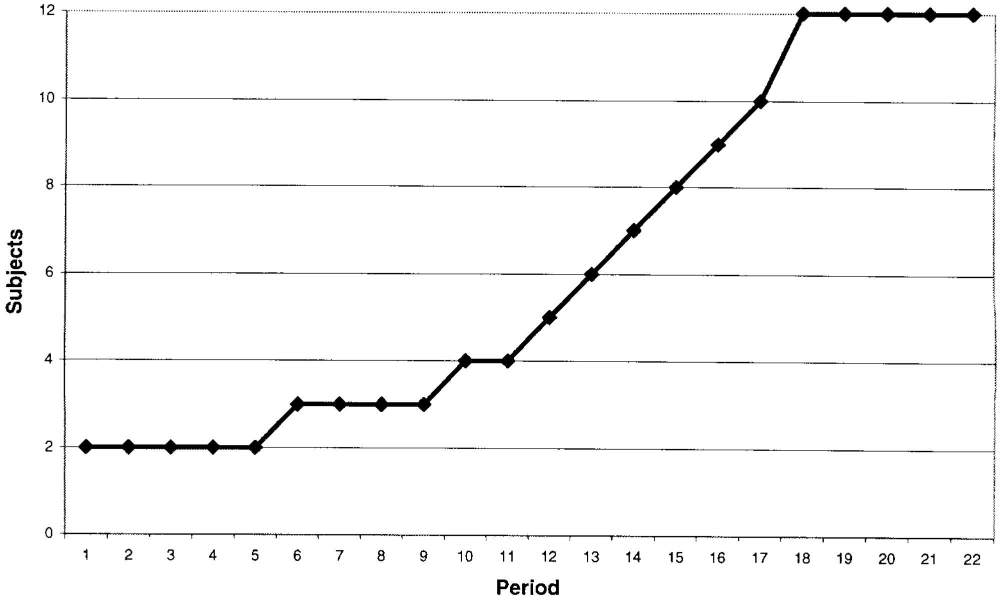


Figure 4.9: Session 5 data (UC Santa Cruz)(Growth path 3)

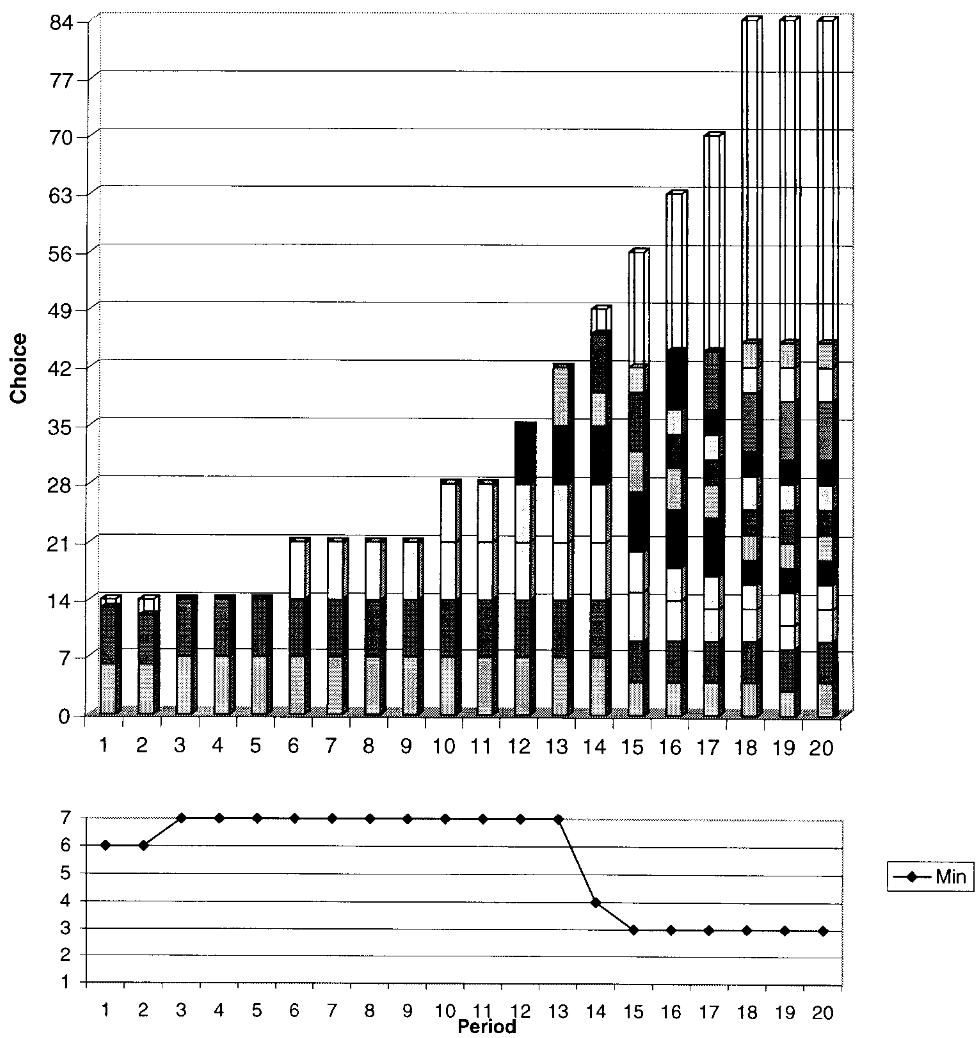


Figure 4.10: Session 6 data (UC Santa Cruz)(Growth path 3)

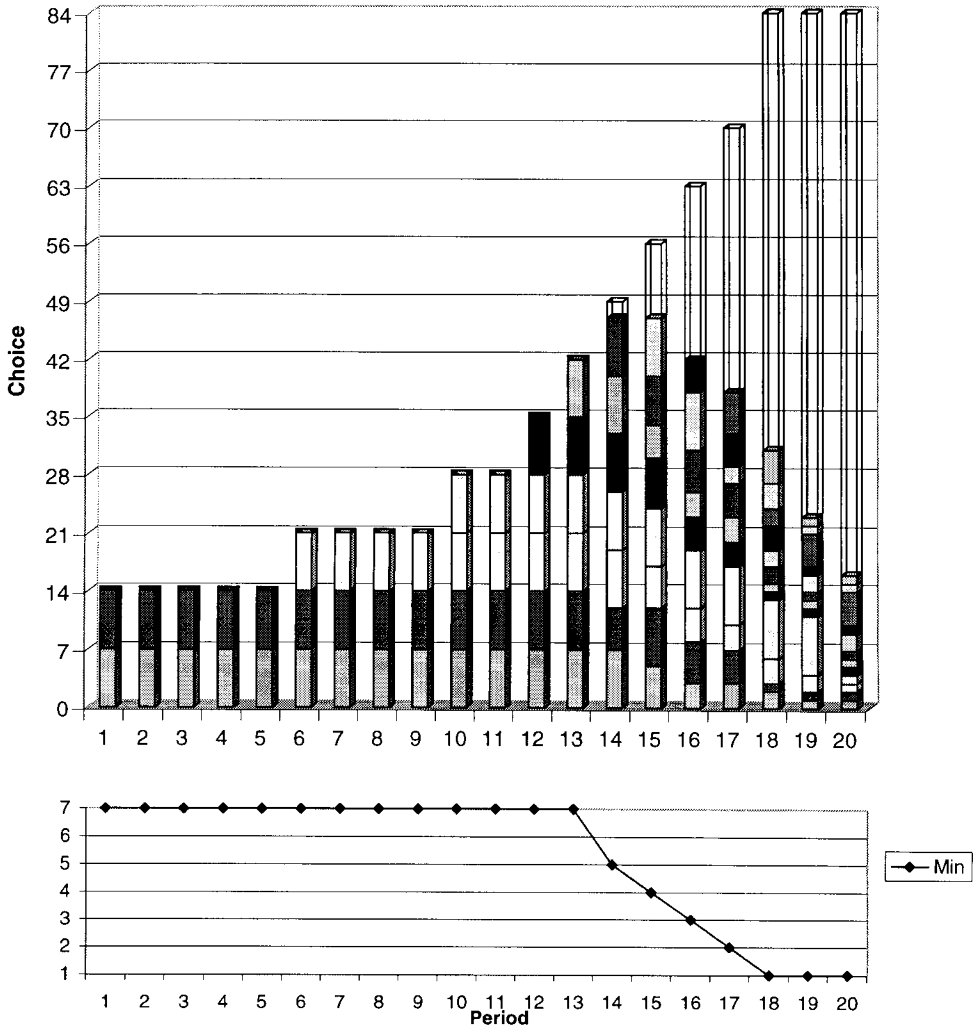
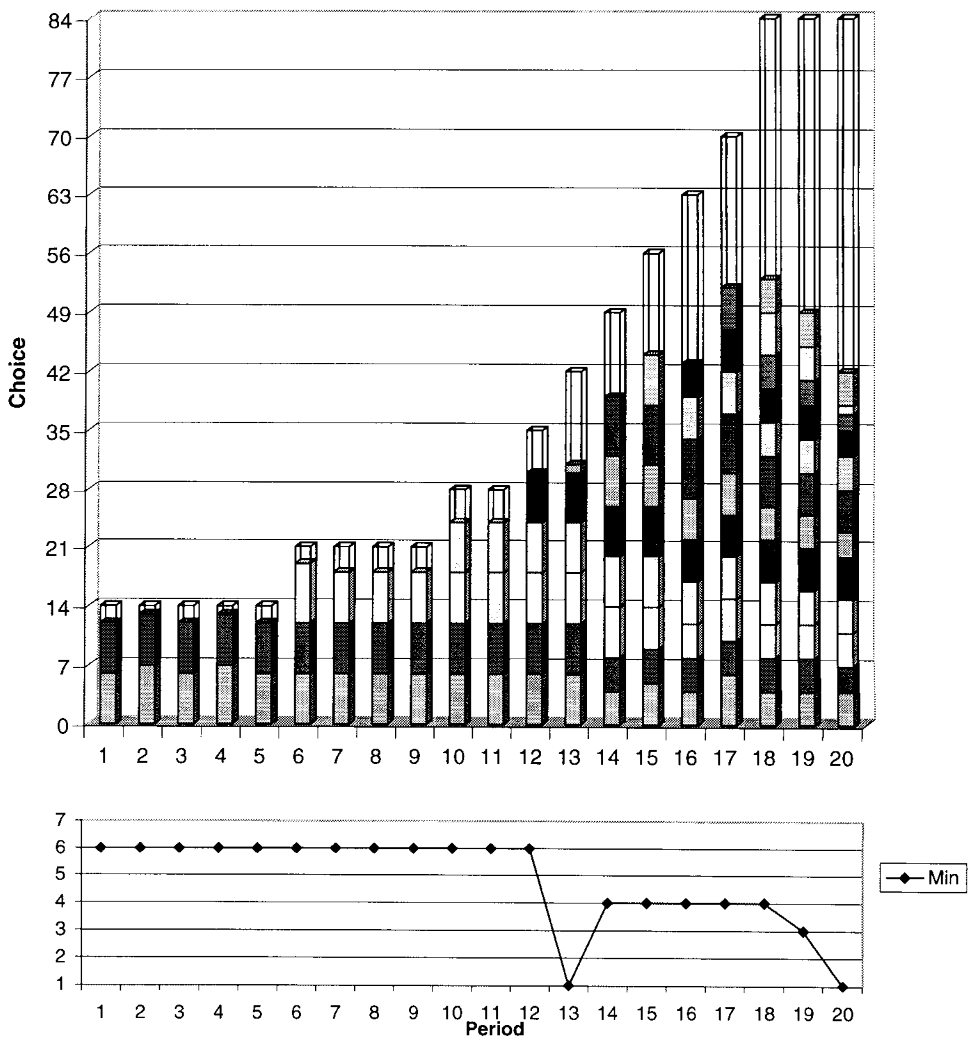


Figure 4.11: Session 7 data (UC Santa Cruz)(Growth path 3)



was added, the minimum fell to 5. It then continued to fall by one every period. While the minimum eventually reached 1, note that the decline was much more regular and gradual than in the control data.

In session 7 (Figure 4.11), the two person group was not able to coordinate on the efficient equilibrium. Instead, the minimum was 6 for all first five periods. The minimum continued to be 6 through period 12. In period 13, the sixth participant entered and selected 1. The minimum then went up to 4 and remained there through the 18th period, when the final two participants entered. It then dropped in the next two periods to 1. Thus, while efficient coordination was not maintained through all the periods, a large group with a minimum higher than one was obtained.

This analysis, though mainly a casual examination of behavior in individual sessions, nonetheless helps shed light on several behavioral regularities. First, while efficient coordination does not occur in all cases, efficiency (measured by the value of the minimum) is higher for large groups than in previous experiments. In only three of the seven sessions is the minimum initially 1 for the groups of size 12 – after having already played several periods in large groups (of size 11, 10, etc.). In the other four sessions the minima are 3, 4, 5 and 7. In the following period, efficiency declines in only one of the sessions (the minimum falls from 4 to 3 in session 7).

In addition, in all the sessions that end up at a minimum of 1, the minimum is higher at least through a group size of 9. This higher level of efficiency for groups of size 9 (the minima are 2, 5, 5, 7, 3, 3, and 4) is surprising in light of the fact that the minimum was always 1 for the large groups (nine or larger) in Table 3.3.[11] Thus, there is support for the hypothesis that starting with a 2-person group, which reliably reaches efficiency, and adding players slowly enough, enables much better coordination than starting with large groups.[12]

[11] While Table 3.3 reports the fifth period minima, the minima in the first period were not as high as in the sessions reported here and there was never a minimum of 7.

[12] In addition, the results of the experiments by Knez and Camerer (1994) which showed that

Second, it appears that early experience with growth is important in determining subsequent success. In sessions 2 through 7, the minimum did not drop when the fourth person was added compared to what it was when the third participant entered. In these groups, the minimum was 5 or greater through at least period 12, indicating that subjects may have learned that controlled growth was possible (at least for a while). In session 1, however, the minimum dropped both of the initial times the group grew and continued to drop with growth, indicating that the initial experience with growth led subjects to believe that the group could not grow successfully.

Third, the results of session 1, in which efficiency declined steadily, suggest that players may form "higher-order" precedents based on not just levels of previous play (e.g., expect the previous minimum to be the minimum again), but also on the relation between levels of previous play and group sizes or transitions. The fact that the minimum falls by one unit when a third person is added, and falls again by that same amount when a fourth person is added, seems to create a belief that adding a person leads the minimum to fall by one (which is self-fulfilled when the fifth, seventh, ninth and tenth people are added, though not when the sixth and eighth are added). This kind of behavior had not been observed in previous work because nobody had changed structural variables repeatedly from period to period, in a way which allows formation of higher-order relational precedents.[13]

The results of these sessions indicate that controlled growth can help solve the problem of large group coordination failure. However, the growth paths used do not always succeed in creating large, efficiently coordinated groups and, in fact, in only one of the sessions did the minimum remain at 7 throughout. Given the difficulty of obtaining successful growth, an interesting question is whether subjects are aware of

"merging" two three person groups leads to coordination failure (the minimum fell to 1 80 percent of the time) indicate that growth can be too rapid. This provides additional evidence that controlled growth can play an important role in obtaining successful coordination in large groups. Note that the minimum for a group of size 6 was 1 in only one of the seven sessions in experiment 1.

[13]There is even more convincing evidence of this in the results of experiment 2.

the need for slow, regular growth paths and, if so, whether they *can* discover more effective growth paths than those used in experiment 1. Experiment 2 addresses this question by endogenizing the growth path.

4.2 Experiment 2: Can subjects discover successful growth paths?

4.2.1 Experimental Design

In experiment 2, one participant was randomly selected to act as a "manager" and determine the growth path. This subject was placed in a separate room from the remaining subjects and an experimenter carried information between the two rooms. The game that the other participants would be playing was described to the manager (again framed in the context of a project completion), who was instructed that he or she would be responsible for selecting the size of the group for all periods after the first. In the first period, the group size was fixed at 2 and this was the smallest size that the manager was allowed to pick in any period. The experiment lasted 35 periods.

The manager was told that his or her earnings in each period would be determined by the number of active participants and by the group minimum (completion time). Table 4.1 describes the possible earnings for the manager.[14] Note that, for any group size, the manager is better off when the group coordinates efficiently. Also, the manager's payoff is higher when efficiently coordinated groups are larger, but the opposite is true for inefficient groups. Therefore, the manager has an incentive to

[14]The earnings are determined according to the following formula (rounded to the nearest cent if necessary):

$$\pi = \frac{n(min - 3.5)}{100} + 0.05$$

except for the payoff when the group size is 12 and the minimum is 7. Since the goal was for managers to attempt to reach this outcome, a large bonus was awarded for achieving it.

create a large group, but only if it is coordinated successfully.

Number of	Completion Time (weeks early)						
participants	1	2	3	4	5	6	7
2	0.00	0.02	0.04	0.06	0.08	0.10	0.12
3	-0.03	0.01	0.04	0.07	0.10	0.13	0.16
4	-0.05	-0.01	0.03	0.07	0.11	0.15	0.19
5	-0.08	-0.03	0.03	0.08	0.13	0.18	0.23
6	-0.10	-0.04	0.02	0.08	0.14	0.20	0.26
7	-0.13	-0.06	0.02	0.09	0.16	0.23	0.30
8	-0.15	-0.07	0.01	0.09	0.17	0.25	0.33
9	-0.18	-0.09	0.01	0.10	0.19	0.28	0.37
10	-0.20	-0.10	0.00	0.10	0.20	0.30	0.40
11	-0.23	-0.12	-0.01	0.11	0.22	0.33	0.44
12	-0.25	-0.13	-0.01	0.11	0.23	0.35	1.00

Table 4.1: Manager's payoffs (in dollars)

Following the manager's determination of the group size in each period, a group of up to 12 subjects played the game in the same format as in experiment 1. The instructions for these subjects were the same as before, except that they were now informed that the number of active participants would be determined at the beginning of each period by the manager. The manager would select a number, and then the participants whose numbers were 1 through that number would play the game.[15]

The experiments were conducted between June and October at the California Institute of Technology. Subjects were graduate and undergraduate students with little

[15]The one other difference with experiment 1 was that the manager was given the option, at the beginning of each period, to randomly reassign participant numbers. This was intended to allow the manager to "restart" the group in case the first few participants became stuck at a bad equilibrium. Previous results indicate that this does occasionally happen (though rarely) in small groups (see Table 3.3).

An important question has to do with how the game is changed by the modifications. The introduction of another player whose strategy choice consists of determining the group size and deciding whether to reassign participant numbers changes the game, in that there are now possibly punishment strategies available to both the manager and players in the game. However, pure strategy equilibria in which players coordinate efficiently if growth is sufficiently slow but more than one player decreases his or her choice if growth is too fast remain, as do multiple other pure strategy equilibria.

or no formal training in game theory. The experiments lasted about 2 hours.

These experiments allow us to test whether the subjects randomly assigned the role of managers are aware of the need for slow, controlled growth.[16] In addition, the growth paths generated by the managers allow us to further investigate the effectiveness of varying growth paths.

4.2.2 Results

Four sessions were conducted using 52 subjects.[17] While the number of sessions provides data on the choices of only four managers, the results provide interesting insights into the managers' cognition of the need for controlled growth as well as further evidence supporting the main growth hypothesis. Again, the results will first be examined by looking at behavior in individual sessions. In the next section, the aggregate results of experiments 1 and 2 will be analyzed more rigorously.

The results of the first session (E1) are presented in Figures 4.12 and 4.13. Figure 4.12 provides the same information as the figures for experiment 1. The number of segments for a period represent the number of participants playing the game, while the length of each segment represents a subject's choice. The bottom of the figure shows the minimum choice in each period. The number of active participants can also be seen in Figure 4.13, which presents the manager's choices of group size.

[16]Note that these experiments are also subject intensive and financially costly since 13 subjects are required to obtain one data point: a manager's success or failure. One possible solution to this is to study the managers separately, giving them feedback that is either artificially constructed by the experimenter or determined according to some model developed from the data from experiment 1. Economists are typically opposed to using deception in experiments, though, so the first option can be ruled out. The second option requires large amounts of data to be able to determine feedback in all contingencies. However, this is a possibility for future work. Particularly since several simulation programs already exist (usually used as part of the MBA curriculum) in which students "grow" companies (see, for instance, Graham, Morecroft, Senge, and Sterman, 1992).

[17]Due to the high concentration of male students at Caltech, the manager ended up being male in all four sessions.

Figure 4.12: Session E1 data (Caltech)(Endogenous growth)

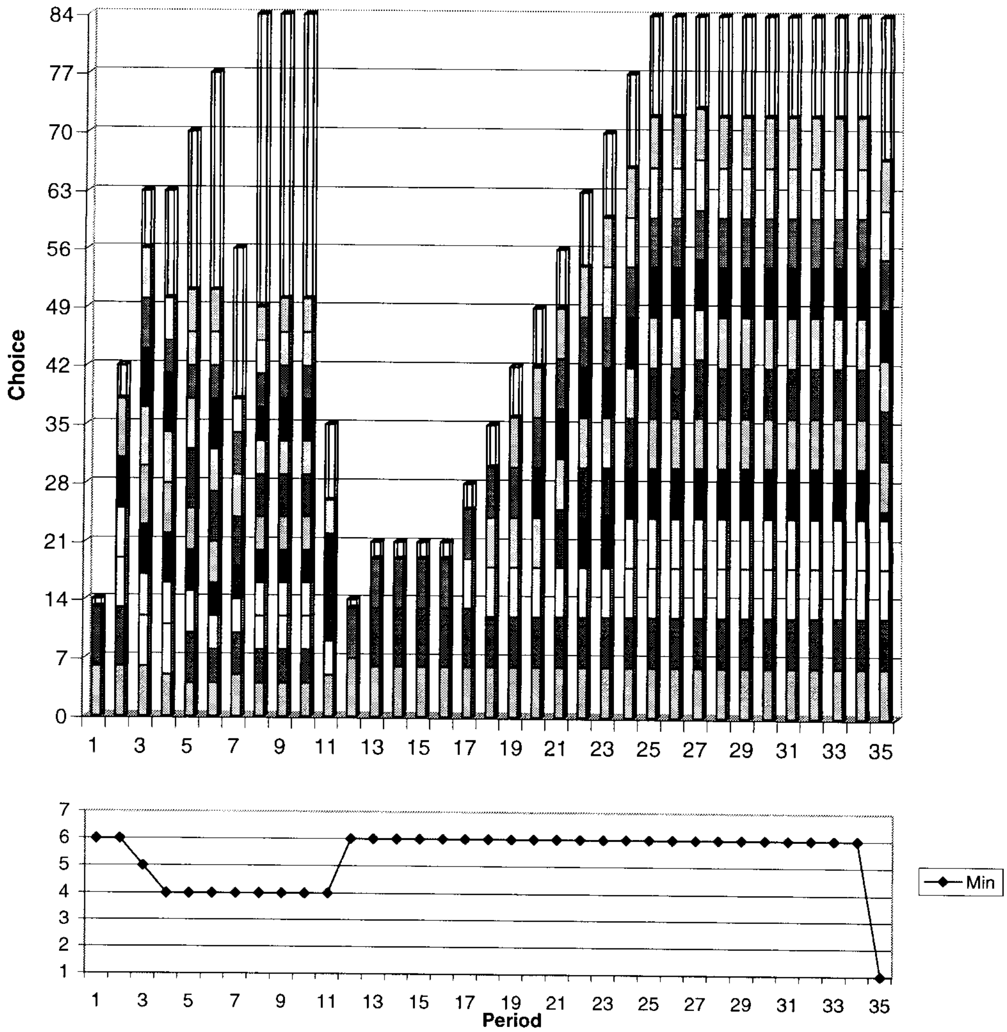
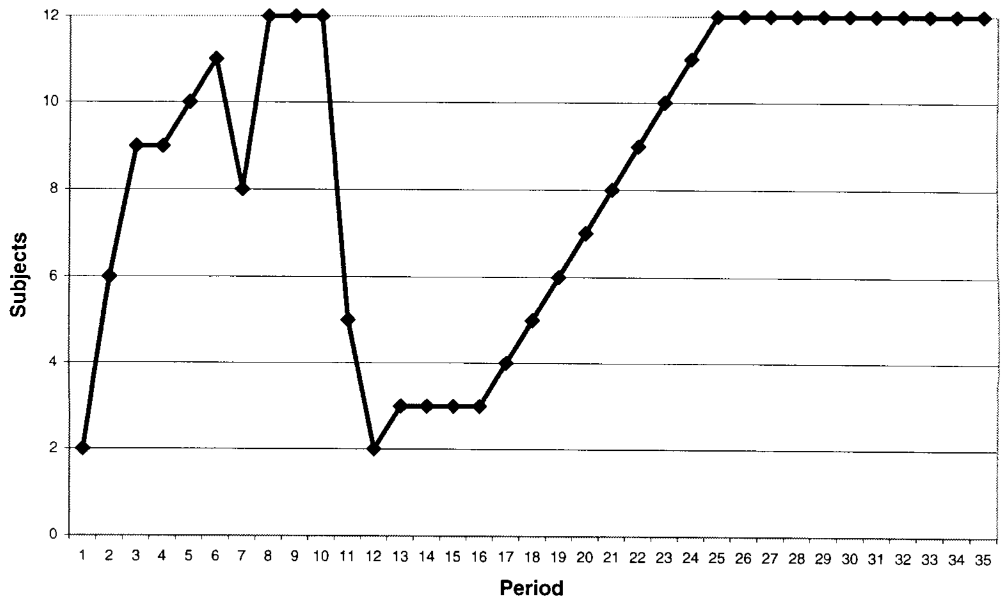


Figure 4.13: Endogenous growth path (Session E1)



The minimum choice in the first period was 6, which represents a high level of efficiency. In the second period, the first period in which the manager determined the group size, the manager raised the group size to 6. The minimum remained at 6, but then dropped to 5 when the manager increased group size to 9 in the next period. While group size remained at 9 in period 4, the minimum fell to 4 and remained there for several periods while the manager varied the group size between 8 and 12 and reassigned participant numbers a few times. At a minimum of 4, the manager earned $0.11 per period for a group of size 12, but this is less than he could earn by having a group of size 2 coordinating efficiently.

The manager then proceeded to "fire" several participants and start over with a group of size 2. This succeeded in raising the minimum back up to 6. The manager then added one more participant, which did not reduce the minimum, and remained at a group size of 3 for 4 periods. He then increased the group size by one every period until reaching a group size of 12 in period 25. The minimum remained at 6 through the remaining periods.[18] Thus, while the manager initially failed to realize the need for controlled growth and added participants too quickly, he started over with a group size of 2 and proceeded to add participants at a slow enough rate to create a large group playing a minimum of 6.

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[18]The minimum fell to 1 in the final period. This was the result of one participant's choice and is an example of the end game effect discussed earlier.

At least one subject lowered his or her choice below the previous minimum in either of the last two periods in two out of seven sessions in experiment 1 and two out of four sessions in experiment 2. Therefore, a better design might have been one that prevented subjects from unilaterally taking an action that could either prove very costly to others or affect the data so strongly. Such designs include using a stochastic stopping rule or a different order statistic. With a stochastic stopping rule, subjects would not know when the final period might come so it would be more costly to deviate from successful coordination and impossible to do so with certainty in the last period. With a less punishing order statistic, such as the second, one subject alone would not be able to affect the completion time or payoffs to others by unilaterally deviating from efficient coordination. Van Huyck, Battalio and Rankin (1996) conduct experiments using the second order statistic. While they find that some groups are able to coordinate efficiently, the game they study is substantially different in that there are 101 actions available instead of 7 and the cost to being one effort level away from the order statistic is much smaller. However, while these alternative designs would likely have reduced the phenomenon of players choosing lower numbers than the previous minimum, the goal was to study the most punishing coordination problems and to maintain a similar design to the one used in the majority of previous weak-link studies.

In session E2 (Figures 4.14 and 4.15), the first two participants coordinated efficiently on 7 in the first period. When the manager increased the group size to 5 in the next period the minimum remained at 7, but it then fell to 6 when the manager added 5 more participants in the next period. The manager then tried to raise the minimum by varying the group size (between 8 and 12) and reassigning participant numbers in the next few periods. However, while the minimum initially remained at 6, it fell to 1 in period 8 (when group size increased from 10 to 12). The manager then decreased the group size to 8 and the minimum went back up to 5.

During the remaining periods, the manager tried to increase the minimum by varying the group size. Although the smallest size he then selected was 4 (until the last period), when he did so the group almost always recovered to a minimum of 6. However, the manager did not let the group remain at a size of 4 for more than one period and tried to increase the group immediately by adding at least 2 more participants. This resulted in a drop in the minimum every time. The manager continued to vary the group size (with a resulting effect on the minimum) for the remaining periods, but was unable to find a growth path allowing him to create a large, efficiently coordinated group.

The results of session E3 (Figures 4.16 and 4.17) are similar. In this session, the minimum was 3 in the first period. Rather than allowing the two first participants time to coordinate, however, the manager immediately increased the group size to 7 and the minimum remained at 3. He decreased group size to 2 in period 4 – increasing the minimum to 6 – but then proceeded to add participants right away, leading to a drop in the minimum. Similarly to the manager in session E2, this manager varied the group size (between 3 and 10) in the remaining periods, increasing the group size too quickly after any increase in the minimum.

The behavior of the manager in session E4 was more like that of the manager

Figure 4.14: Session E2 data (Caltech)(Endogenous growth)

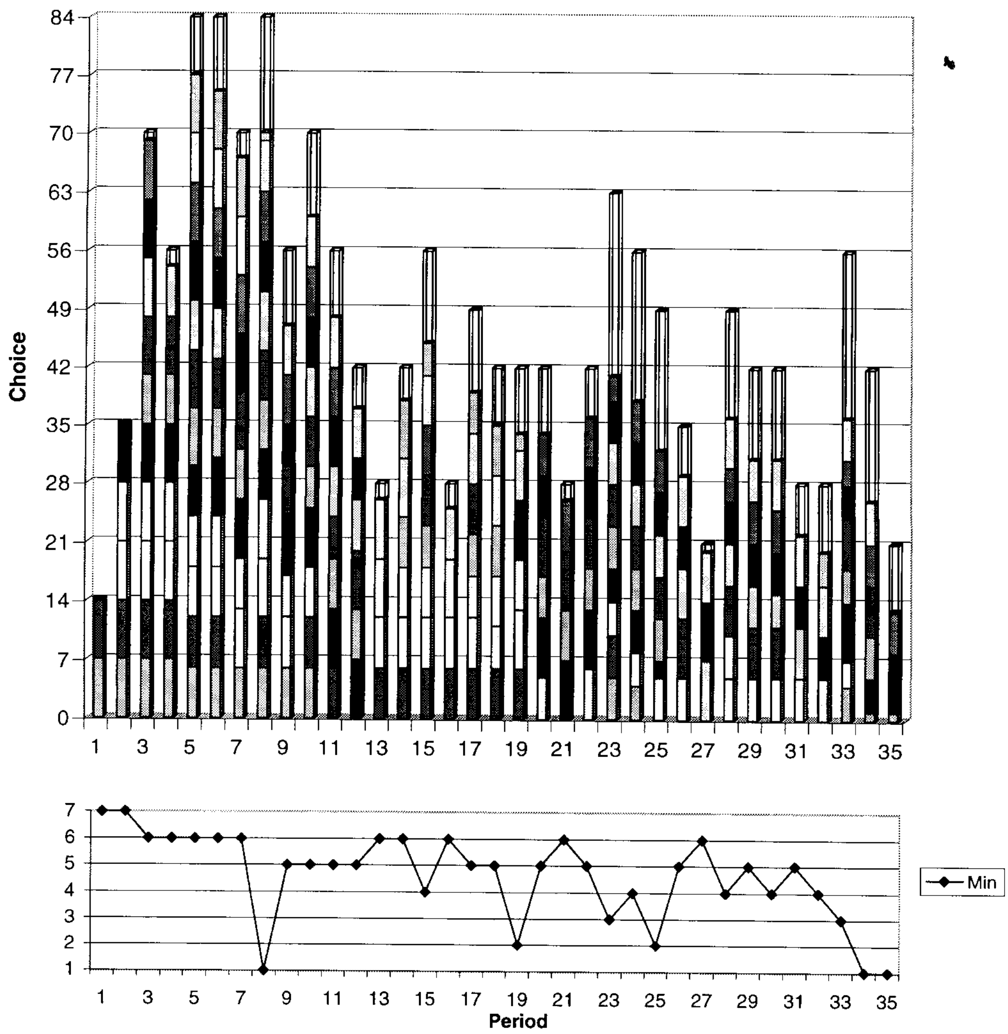


Figure 4.15: Endogenous growth path (Session E2)

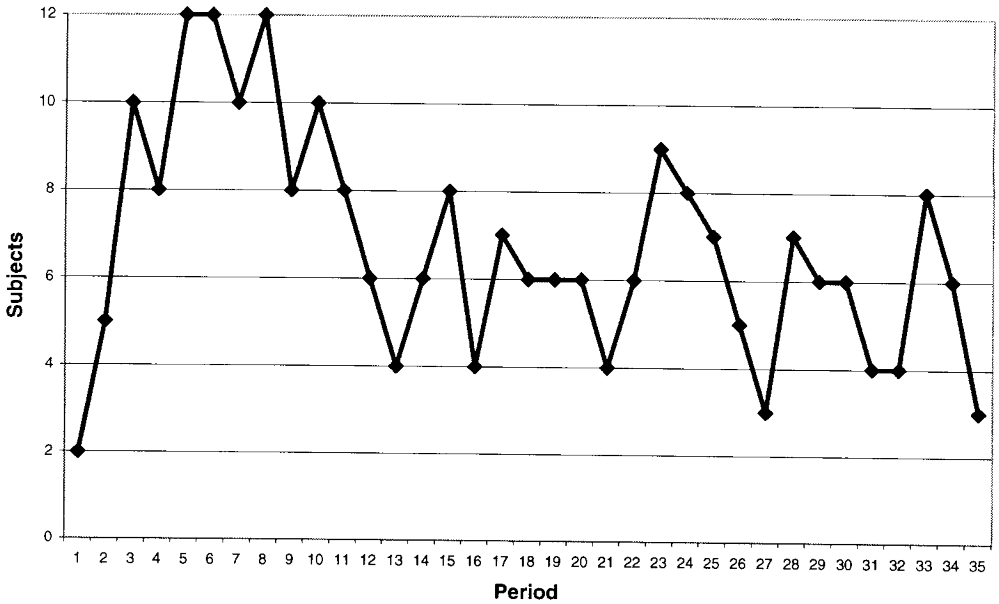


Figure 4.16: Session E3 data (Caltech)(Endogenous growth)

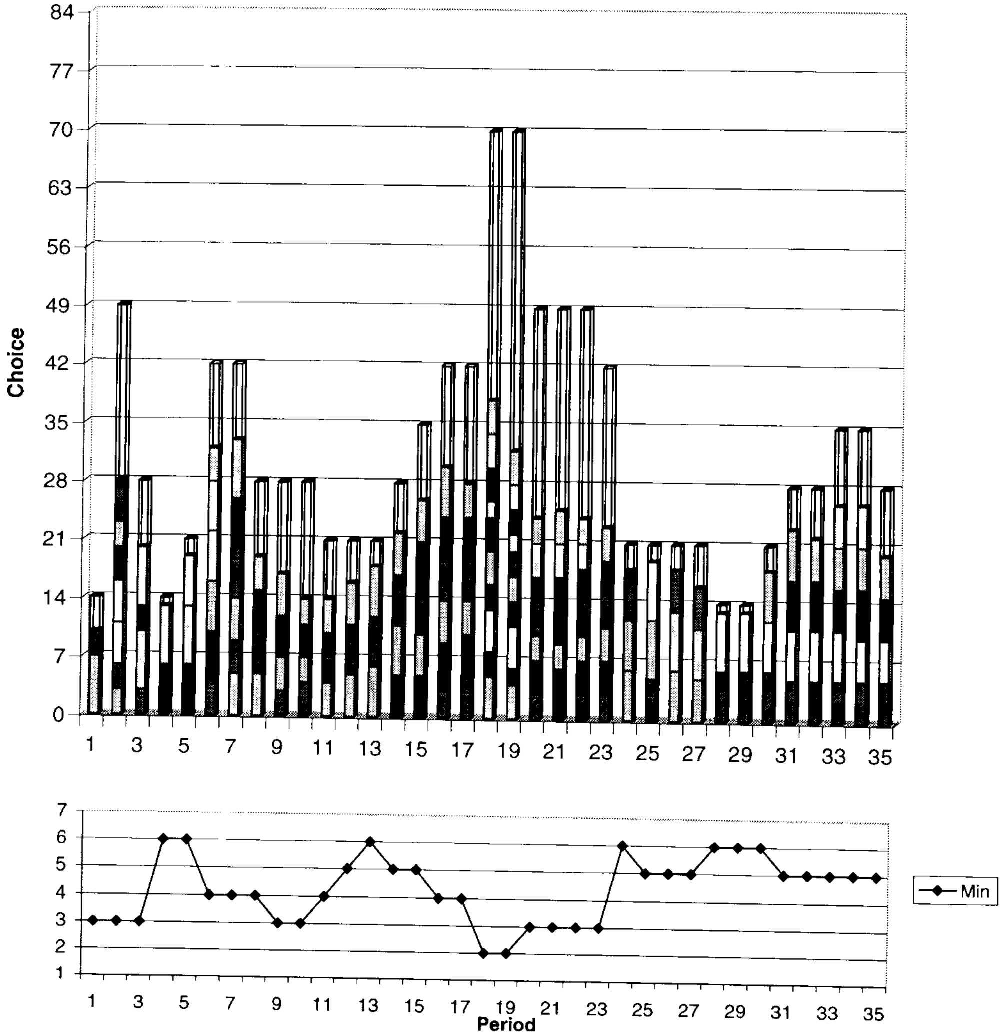
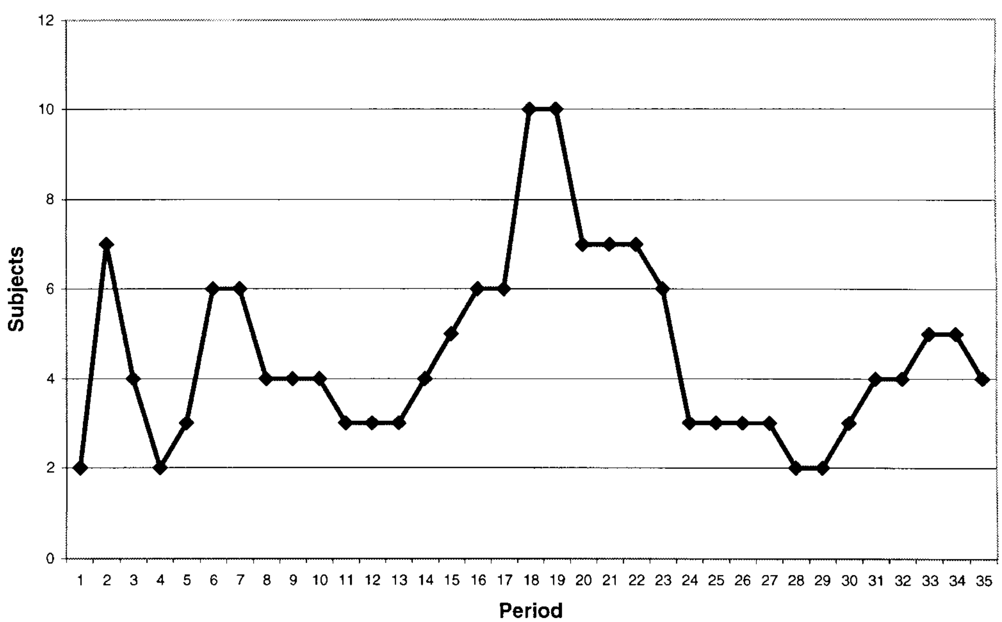


Figure 4.17: Endogenous growth path (Session E3)



in session E1. The minimum was 6 for the group of size 2 in the first period. The manager then tried to grow the group too quickly (increasing the size to 10) and the minimum fell to 1 in the next period. The minimum remained at 1 while the manager tried group sizes of 12, 7, and 5 unsuccessfully, and then moved up to 3 when he set group size at 3. The manager then increased the group size by one and allowed the group of size 4 enough time to raise the minimum to 6. When group size again increased by one, the minimum again fell to 3, but then rose to 7 as group size remained the same for several periods. For the remainder of the experiment, the manager always added only one or two participants at a time and the minimum fell to either 3, 4, or 5 every time the group grew. However, after the initial drop following growth, the minimum increased by exactly 1 in every period in which the group size remained the same. This continued until period 33, when the entire group of 12 participants coordinated successfully on 7. Thus, after initially growing too quickly, the manager in session E4 discovered a growth path slow enough to lead to efficient coordination.

The results of session E4 also point to an interesting phenomenon similar to the reaction to growth in session 1 of experiment 1, in which the minimum fell exactly by one when the group grew. In all periods after period 7, the group reacted in the same manner to growth. Every time the group grew, the minimum fell, but then increased by exactly one for every period that the group did not grow. Thus, similarly to session 1, a self-enforcing norm was established concerning how the group would react to growth.

While there are only four sessions, it is again possible to draw some conclusions based on an examination of the data. First, in all four sessions the subjects participating in the role of manager initially grew the groups too quickly, resulting in coordination failure. This points to a lack of cognition of both the difficulty of coordinating large groups – which is consistent with previous research (see Weber, et al.

Figure 4.18: Session E4 data (Caltech)(Endogenous growth)

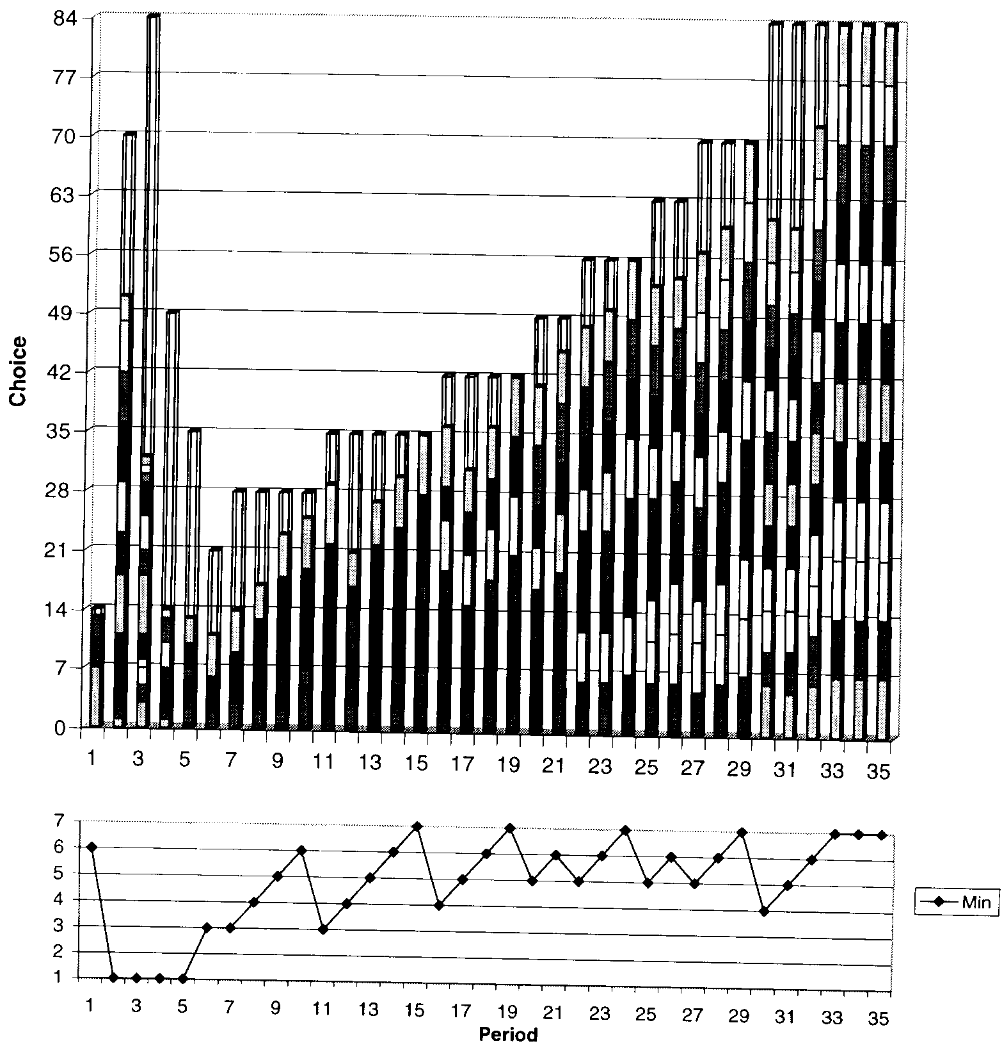
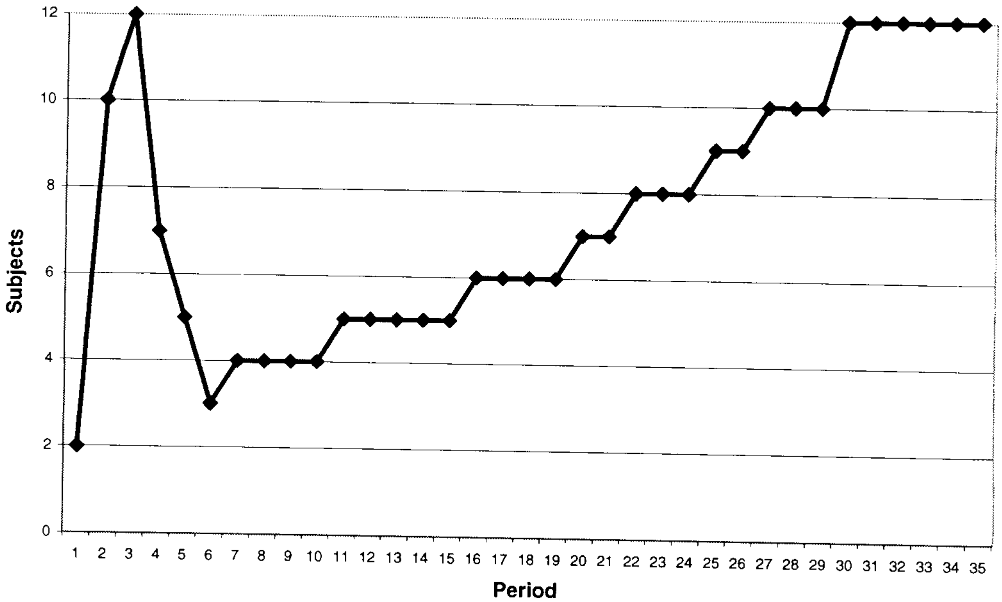


Figure 4.19: Endogenous growth path (Session E4)



(1998)) – and the need for controlled growth to solve coordination failure.[19]

Following this initial failure, however, there is evidence that some subjects learned to grow using a slower and more regular approach. While two of the managers failed to realize this and continued to grow too quickly, the other two started over with small groups and then grew slowly (never adding more than 2 participants at a time) to create large groups coordinated on minima of 6 and 7. The two managers that started over and grew slowly had higher earnings than the managers that did not (Managers in sessions E1 and E4 earned $7.39 and $7.37, respectively, while the managers in sessions E2 and E3 earned $4.28 and $2.54).[20] It is interesting to note that the growth path used by the manager in session E1 is very similar to the growth paths used in experiment 1.

There is also further support in these experiments for the hypothesis that slow, regular growth can lead to successful coordination in large groups. In the two sessions in which the managers started over at a small size and grew slowly (sessions E1 and E4), the result was large groups that coordinated on high minima. In addition, the failure to succeed of the other two managers indicates that the rate of growth is important in obtaining efficient large group coordination.

[19]It should be noted that though the managers in these experiments are students and not professional managers, students at Caltech are not a representative student population in that they are well above average in quantitative and analytical skills. In addition, many of them go on to fill important management positions and start high-tech firms shortly after leaving Caltech. Therefore, while it would be interesting to examine whether this result extends to a population of professional managers or MBA students (which is a possibility for future research – particularly if it is possible to conduct the one subject experiments discussed earlier), the use of this population allows for claims of at least some external validity.

[20]While the managers in sessions E2 and E3 did "poorly" in that they tried to grow the groups too quickly and therefore failed, they still made positive profits because they were always able to decrease the size of the group which usually led to improved coordination (or at least less negative earnings). In fact, while the average per period earnings of both the successful managers was $0.211, this number was $0.097 for the other two managers. The latter number is close to the average per period earnings ($0.087) that would have resulted if the payoffs in Table 4.1 had been applied to the results from experiment 1 (in which the experimenter served as manager). That these two numbers are close is surprising since in experiment 1 the group was constrained to grow even if the minimum fell to one and this was not true in experiment 2.

Finally, the results of session E4 provide strong additional support for the conjecture that experience with growth plays an important role in subsequent reactions to growth. In this session, as in session 1 of experiment 1, subjects responded not just to the previous minimum, but to what happened to the minimum as the group grew. Therefore, a precedent was established indicating that every time the group grew, the minimum fell, but that in every period that the group did not grow, the minimum went up by exactly one. The strength of this precedent and the extent to which it was a self-reinforcing belief held by all players is evident in the last few episodes of growth. When the group grew to a group size of 10, the minimum fell to 5. In the next two periods, all 10 players played first 6 and then 7. When the group grew again to a size of 12, the minimum fell to 4.[21] In the next three periods, all 12 players played first 5, then 6, and then 7. This points to a strong coordinating effect of previous experience with growth.

The above examination of behavior in the two experiments supports the notion that growth can lead to higher efficiency in large groups and points to additional interesting results. However, in order to establish more conclusively that growth works, the aggregate results must be examined more carefully.

4.3 Aggregate Results

In order to test the main hypothesis of the chapter – that groups that are grown slowly are more efficiently coordinated than groups that start off at a large group size – it is necessary to look at the aggregate data. The sessions in experiment 1 are all examples of 12 person groups that were grown since they started off at small group

[21] Since this was the first time that the group had grown successfully by more than one, there was noticeable agitation (e.g., fidgeting, longer response time) by several participants in the experiment. This points to the importance of the precedent since players were nervous because they had never experienced this kind of growth before and were therefore uncertain about what the outcome would be. As a result, the minimum fell below (to 4) what it had fallen the last few times the group grew (to 5).

sizes (2) and grew in size by only adding one or two players at a *time to a* large size (12). In addition to these sessions, however, two of the sessions in experiment 2 also provide data on groups that were grown slowly. In both sessions E1 and E4, following initial unsuccessful rapid growth, the managers started the groups over at small sizes (2 and 3, respectively) and then grew the groups slowly – never adding more than two players at a time – until reaching a group size of 12. Therefore, these two sessions are pooled together with the results of experiment 1 in Table 4.2.

Table 4.2 presents the growth paths (number of active participants) and minimum choices by period for each of the grown groups. For each of the five growth sessions used (two of which were determined by the managers in experiment 2), the results of groups growing at that rate are presented.[22] Of the resulting nine 12 person groups, two were successfully coordinated on 7 for more than one period (though the final two periods for session E4 in which all 12 players selected 7 are not included in the table). There was one 12-person group in which the minimum was 6, another in which it was 5, and another in which it was 3. In another group, the minimum at size 12 was initially 4, but it then fell to 3 and then 1. In the remaining three groups, the minimum was 1 by the time group size reached 12 and it remained there. While a majority of the groups are not efficiently coordinated, the fact that two are playing minima of 7 and another three are coordinated at levels of efficiency higher than 1 indicates that growth does work, even if only limitedly. In addition, as mentioned before, the majority of groups are also playing higher minima at intermediate group sizes (such as 9) than they did in previous experiments.

A better test of whether growth works or not can be obtained by comparing the distribution of choices between the grown groups and the control sessions, in which group size was constant at 12. In order to make this comparison, however, it is nec-

[22] All of the periods are not included in the table for sessions E1 and E4 since these experiments lasted longer and the focus is on what occurred when the groups reached larger sizes. The complete data for these sessions is available in Figures 4.12 and 4.18.

	Period																			
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Growth path 1	2	2	2	2	2	2	3	3	4	4	5	6	7	8	9	10	11	12	12	12
Session 1	7	7	7	7	7	7	6	6	5	5	4	4	3	3	2	1	1	1	1	1
Session 2	6	5	7	6	7	7	5	6	5	6	5	5	5	5	5	5	5	5	5	5
Growth path 2	2	2	2	2	2	3	3	3	3	4	4	5	6	7	8	9	10	11	12	12
Session 3	7	7	7	7	7	5	5	5	5	5	5	5	5	5	5	5	1	1	1	1
Session 4	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7	7
Growth path 3	2	2	2	2	2	3	3	3	3	4	4	5	6	7	8	9	10	12	12	12
Session 5	6	6	7	7	7	7	7	7	7	7	7	7	7	4	3	3	3	3	3	3
Session 6	7	7	7	7	7	7	7	7	7	7	7	7	7	5	4	3	2	1	1	1
Session 7	6	6	6	6	6	6	6	6	6	6	6	6	1	4	4	4	4	4	3	1
Growth path E1		2	3	3	3	3	4	5	6	7	8	9	10	11	12	12	12	12	12	12
Session E1		6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6	6
Growth path E4		5	6	6	6	6	7	7	8	8	8	9	9	10	10	10	12	12	12	12
Session E4		5	4	5	6	7	5	6	5	6	7	5	6	5	6	7	4	5	6	7

Table 4.2: Minimum choices by period for sessions 1 through 7

essary to decide on a valid comparison period. The control groups played as large groups for 12 periods. The grown groups all started off at small sizes and did not reach a group size of 12 for several periods. The earliest period in which a grown group reached the maximum size was period 18. Also, the grown groups did not all reach that size in the same period. The key question, then, is when the comparison should be made. A reasonable comparison is to compare the control groups in period t with the tth period in which the grown groups played as groups of size 12. In this case, subjects in both treatments have $t-1$ periods of play in 12-person groups and therefore share a similar history. This is still not a perfect comparison since the grown groups have a much longer history that includes the periods spent growing, and the question remains about when the comparison should be made (i.e., what the value of t should be). Should the first period of play in the control groups be compared to the first period in which the grown groups reached the maximum size? Another issue has to do with how many periods as 12-person groups there is data for in the grown groups. Since the maximum number of periods (after reaching a size of 12) in which there is data for all the grown groups is four, and since by period 4 the majority of 12-person groups have coordinated on some equilibrium, this was decided upon as the appropriate comparison point. Table 4.3, therefore, compares the distribution of subject choices in the four control sessions and the nine growth sessions in the fourth period in which participants played at a group size of 12.

		Control		Growth	
	7	2	(4.2 %)	20	(18.5 %)
	6	0	(0.0 %)	15	(13.9 %)
	5	5	(10.4 %)	13	(12.0 %)
Choice	4	16	(33.3 %)	8	(7.4 %)
	3	5	(10.4 %)	10	(9.3 %)
	2	9	(18.8 %)	9	(8.3 %)
	1	11	(22.9 %)	33	(30.6 %)
Total		48		108	

Table 4.3: Distribution of subject choice in fourth period as 12-person groups

The first thing to note is that the number of subjects choosing 1 is high in both conditions (11 of 48 in the control sessions and 33 of 108 in the growth sessions). While this number is higher for the growth sessions, this is not unreasonable since in three of the nine grown groups the minimum was 1 even before the group reached a size of 12. In these groups, therefore, subjects had more periods to coordinate on the inefficient equilibrium than in the control treatment, where only one session started off in at a minimum of 1 in period 1.

Just as interesting, however, is the difference in the distribution of high choices (6 or 7) between the two treatments. In the control sessions, only 2 of 48 subjects (4.2%) played either a 6 or a 7, while this is true of 35 of 108 subjects (32.4%). Therefore, the number of subjects playing the two highest strategies is much higher in the grown groups than in the control sessions.

The distributions in Table 4.3 are significantly different when compared using a Chi-Square test ($\chi^2_{(6)} = 29.97$, $p < 0.001$). In addition, when the cumulative choice frequencies (which can be derived from Table 4.3) are compared, the null hypothesis that the distributions are the same can be rejected in favor of higher choices in the grown groups at the $p < 0.01$ level (Kolmogorov-Smirnoff one-tailed, $\chi^2_{(2)} = 11.85$).[23]

There is also evidence that the rate of growth mattered. All four of the managers in experiment 2 encountered problems after growing the groups too quickly. The two successful managers (sessions E1 and E4) were able to grow efficiently coordinated groups only by starting over at a small size and then growing slowly. The other two managers did not attempt to do so – but instead continued to try to grow quickly – and subsequently failed. The correlation between change in group size and change in the minimum in experiment 2 is -0.497, indicating that the minimum decreased when group size increased by a large amount.[24]

The above tests lend support to the result that grown groups are more efficiently coordinated. Even without examining the aggregate results, however, the fact that two 12-person groups were able to successfully coordinate on the efficient equilibrium (a result never previously obtained) shows that growth <u>can</u> work to help solve large group coordination failure.

4.4 Discussion

The previous chapter argued that coordination, and not only cooperation, is an important problem faced by firms. The weak-link coordination game models the most

[23]Both of these tests, however, rely on the assumption that all the observations in each treatment are independent. In this case (as in much of experimental data) this assumption is not satisfied since the choices of all of the subjects in a session in a particular period are affected by the common history shared by these subjects. Therefore, the level of significance reported by the statistics is exaggerated.

[24]This correlation is -0.166 for experiment 1.

punishing forms of coordination – where the lowest level of any *input has* a strong effect on efficiency. In addition, the coordination problem modelled by this game is one frequently encountered by firms. Given the punishing nature of the game, it is not surprising that efficient coordination becomes much more difficult as the number of players grows. Previous experimental results indicate that it is impossible for large groups to coordinate on the efficient equilibrium.

Motivated by the observation from the business world that firms are often said to "grow too fast," by an example of a firm that grew slowly specifically to solve coordination problems, and by a model suggesting that slow growth may work to solve large group coordination failure, experiments were run testing whether growth was important in obtaining coordination in large groups. Evidence from the experiments indicates that growth may play an important role in determining the ability of large groups to coordinate efficiently. Starting with small groups that play efficiently and adding players slowly enough resulted in groups of size 12 that did not collapse to inefficiency. In fact, some of the minima in large groups reached the highest levels of efficiency. This was true for groups starting out small and growing slowly both by growth paths determined by the experimenter and by growth paths determined by a subject in the role of manager. The results of experiment 2 also showed that subjects are not initially aware of the need for slow growth, but that some of them may learn to grow slowly from experience. In addition, there is evidence that subjects' initial experiences with growth may be critical for determining subsequent success with growth and that previous experiences with growth may set precedents concerning what will happen the next time a group grows.

While the results of both sets of experiments indicate that growth plays an important role in determining success or failure in large group coordination problems, the generalization of these findings to organizations must be taken with caution. The laboratory is an artificial environment and subjects participating in simple experiments do not correspond directly to actors in complex organizations. In order to establish a connection, evidence must be found in the functioning of real organizations. The example of Southwest Airlines provides a convincing case in which the growth process itself appear to have been successfully manipulated to avoid coordination problems. However, this is just one case and a more exhaustive empirical study would greatly complement the results of this chapter. For instance, a more complete analysis might involve comparing the growth strategies of several firms across industries in which the importance of coordination varies. One conjecture is that growth paths would matter more (in determining success or failure) in industries where coordination plays a more central role, such as the airline industry.

It is also important to remember that controlled growth is just one of the means by which organizations avoid coordination problems due to size. Recall that in addition to growing slowly, Southwest also expended great effort on ensuring that new employees would fit well within the culture. Thus, socialization mechanisms for new entrants can play an important role in determining the ability of the organization to coordinate activity successfully. This is noted by Weick and Roberts (1993) who argue that "The quality of collective mind is heavily dependent on the way insiders interact with newcomers." (p. 368) In fact, in the experiments above, an important element of the design is that future entrants to the game are sitting in the same room as those currently playing and this is commonly known by all. If these entrants are not aware of the history of outcomes (or if the current players are not aware that the entrants know the history), the strength of the socialization process is weakened and growth is likely to be less successful.

Aside from socialization and training, better communication can help members of an organization remain successfully coordinated as the organization grows. Heath (1999) stresses the importance of direct physical communication in helping groups solve coordination problems. However, since the best way to achieve this is to place people in the same location, this is not always possible throughout the growth process. Therefore, it may also be necessary to implement better long-distance communication technology. Recent work has examined how decision making is affected by different forms of communication (Kiesler and Sproull, 1992; Hinds and Kiesler, 1995). Varying communication mechanisms in the above experiments, might also yield important insights into how the ability of an organization to coordinate successfully is affected by different modes of communication and information transmission. This becomes particularly important as firms expand geographically.

In spite of these caveats and the need to conduct additional studies to address the above questions, the contribution of this chapter is obvious: there exists at least one clear solution for large group coordination failure. This solution is a careful planning of the growth process itself. The example of Southwest Airlines provides an example of where this approach was used carefully and to great success.

Chapter 5 Conclusion

The preceding chapters have all discussed problems of interdependence. In the dirty faces game, the interdependence arises because players have to decide whether or not the number of steps of iterated rationality necessary for the equilibrium to arise are satisfied. The results of the experiments on this game show that subjects fail to satisfy even small numbers of such steps, a result consistent with other experimental work. However, the adherence of subjects to equilibrium behavior improves slightly when the game is repeated a second time, indicating that with enough experience subjects may be able to learn to play the equilibrium.

In the weak-link game studied in Chapters 3 and 4, the interdependence problem is more obvious and arises from the complementarity in players actions. Since this type of complementarity is ubiquitous in organizational behavior, a natural application of the game is as a tool for studying coordination problems in firms. As shown in Chapter 3, one important application is to the study of increased interdependence problems in growing firms. While other solutions to these problems are more frequently discussed, the growth process itself may be a possible tool for achieving successful coordination in large groups. This is supported by a case study, a simple model of behavior in weak-link games, and by the results of the experiments reported in Chapter 4.

While the two games appear to be unrelated, it is simple to show that they are in fact quite similar under certain assumptions. To see why, consider the situation faced by players being presented with the dirty faces game. Assume that as players read the instructions, they have to decide whether to invest mental effort in determining the solution to the game or simply choose not to (once they realize there is the secure action U). Since arriving at the equilibrium relies heavily on updating beliefs based on the behavior of others, the benefits to solving the game and playing equilibrium strategies are heavily interdependent with whether others do so as well. If we assume that players face a positive cognitive cost to solving the game, then it is easy to construct a case where it is only optimal to figure out the solution if all other players are also doing so. Therefore, players have to decide (under the key assumption that they must do so *ex ante*) whether to play a high effort – but risky – action and commit cognitive resources to solving for and playing the equilibrium, or simply to play the secure strategy which guarantees no losses. This is an identical problem to the one faced by players in stag hunt and weak link games. While the above argument rests on some possibly unrealistic assumptions, it nonetheless helps illustrate the similarity between the two games.[1]

Given this similarity, an interesting question is whether growth might help large groups play the equilibrium in the dirty faces game. The experiments in Chapter 1 indicate that 2 and 3 person groups might be able to "coordinate" on the equilibrium with repeated play. It might be possible to then create large groups of ten or more players by giving players experience with the simpler problem resulting from smaller group sizes first. However, this and the other future research outlined at the end of each chapter is beyond the scope of the current work.

A final important contribution of this collection of research is to tie together the work on interdependence by game theorists and organizational researchers. While both address problems that are very similar, there has been little dialogue between the two areas. One primary reason is that the two fields have employed entirely different approaches. Game theorists and economists have typically focused on abstract representations of interdependence problems, modelling them as coordination games without exhaustively searching for naturally occurring instances of where these games

[1]This similarity helps make sense of one aspect of the experimental results. In the experiments on both the dirty faces and weak-link games, subjects' early behavior is similar across group sizes and the reason for large observed differences in outcomes at the group level is that the structure of both games is such that a group is only "successful" if all players in that group behave optimally.

are played. Organizational researchers, on the other hand, tend to focus mainly on closely examining instances of interdependence in organizations on a case by case basis. While yielding a much richer picture of where these problems arise, why they matter, and how they actually operate, this approach results in an overwhelming body of knowledge that is fragmented and difficult to aggregate into a coherent theoretical framework.

This conflict between the two approaches extends beyond the study of interdependence. In attempting to answer the question of how groups of individuals interact, both economists and organizational researchers have adopted seemingly incongruous approaches. The approach of economics has been to use formal modelling to strip away everything but the basic structure of the interaction. This yields a powerful tool for analyzing how behavior should change given a change in the structure of the problem. Real behavior is then used only sometimes as a test of the validity of these predictions. Organizational researchers, on the other hand, have tended to adopt an approach in which the actual behavior of groups is the focus of study. This research typically involves much more of an emphasis on the observation and documentation of real, naturally occurring interactions. Therefore, this approach typically yields much more information and insight into how groups actually behave. However, theory is usually only a set of casually collected regularities that are seldom formalized or critically scrutinized and subjected to rigorous empirical testing. While both approaches have certainly led to a greater level of knowledge and comprehension of how groups interact, their joint application seems both necessary for a complete understanding and long overdue.

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Appendix A: Proofs

Proof of Proposition 3.1. In order to show that the proposition is true, assume without loss of generality that the choices of all participants are drawn from a common distribution (i.e., $s_{it} = s_{jt}$ for all i and j). If the result holds in this case, then it is simple to show that it holds when the s_{it} are not the same (as long as their choices are independent of group size). Therefore, we can now describe each choice by a common cumulative distribution function $F(x)$, which gives the probability that a player's x_{it} is smaller than the value x. It is then sufficient to show that the c.d.f. of the minimum for a smaller group first order stochastically dominates the c.d.f. of the minimum for a larger group. To see why this is true, first note that the c.d.f. of the minimum is given by $1 - (1 - F(x))^n$. This is the probability that at least one of n choices drawn from $F(x)$ is less than x. Since $1 - F(x)$ is less than one by definition, the c.d.f. of the minimum is higher (for all values of x such that $F(x) < 1$) as n increases. Therefore, the minimum is stochastically greater when n is larger. If $\sigma_t^2 = 0$, then $F(x)$ jumps from zero to one at the mean, and the c.d.f. of the minimum is unaffected by n. *Q.E.D.*

Proof of Proposition 3.2. Recall that every players' choice is determined by an increasing function of the x_{it}, where these are determined by:

$$x_{it} = (1-b)s_{it-1} + by_{t-1} + \epsilon_{it}$$

and that $0 < b \leq 1$. It follows that since the minimum is determined by the smallest of the x_{it} and the x_{it} are all increasing in y_{t-1}, then the expected value of the minimum will also increase with y_{t-1}. *Q.E.D.*

Appendix B: Instructions for dirty faces game

Initial Instructions

This is an experiment in decision making, and you will be paid for your participation in cash. Different participants may earn different amounts. What you earn depends partly on your decisions and partly on the decisions of others.

During the course of the experiment, all interaction between participants will take place through the experimenter. It is important that you not talk or in any way try to communicate with other participants during the experiment. If you disobey the rules, we will have to ask you to leave the experiment.

If you have any questions during the instruction period, raise your hand and your question will be answered so that everyone can hear. If you have any further questions after the experiment has begun, raise your hand, and an experimenter will come and assist you.

At this time, one of the participants will be randomly selected as the monitor for the experiment. The monitor will assist in conducting the experiment and will be paid a fixed sum. Each participant will roll a die at the front of the room and the participant who rolls the highest number will be the monitor for the experiment. Ties will be resolved by another roll. Please note that each participant has an equally likely chance of being selected to be the monitor for the experiment.

The remaining participants will each be randomly assigned a participant number by selecting an envelope from the experimenter. Please select an envelope now as the experimenter passes around the room. Inside the envelope you have selected, there is an index card with your participant number on it. This will be your participant number for the entire experiment.

There will be four groups, each containing two participants. Each participant will interact only with the other participant in his or her group. The groups will be labeled A, B, C, and D. The participants in each group will also have a number which will identify which participant they are within the group. This number will be either 1 or 2. Therefore, the following will be the participant numbers for the experiment: A1, A2, B1, B2, C1, C2, D1, and D2. Please note that the participant numbers are private and should not be shared with anyone during the experiment.

The experiment will consist of three sessions, Session I , Session II, and Session III. Each session will consist of up to 3 periods. At the beginning of Session I, the monitor will roll the die to determine the type of each participant, beginning with A1. The type of each participant will be either "X" or "O". If the monitor rolls a value of 1 or 2, then this participant will be of type "O". If the monitor rolls a value of 3, 4, 5, 6, 7, 8, 9, or 10, then the participant will be of type "X".

When this process is done, all of the participants will be of either type "X" or type "O". Note that each participant has an 8 out of 10 chance of being a type "X" and a 2 out of 10 chance of being a type "O", and that the type of each participant is independent of the types of all other participants. Each participant's type will remain the same for the entire session.

While the monitor is determining the type of each participant, he or she will not be visible to any of the other participants. The outcome of the rolls will therefore not be known by any of the participants. The monitor will record the type of each participant on a sheet identical to the one at the front of the room labeled Type Sheet. Once the monitor has recorded each participant's type on the sheet, it will be placed inside the cardboard display at the front of the room.

The experimenter will then show each participant the type of the other participant in his or her group by lifting the corresponding flap on the cardboard display. When the experimenter comes to you, please record the type of the other participant in your group on the sheet labeled "Session I Record Sheet". The flap corresponding to the participant who is currently viewing the display, however, will remain closed. Thus, every participant will know only the type of the other participant in their group. No participant, however, will be aware of his or her own type. Note that it is the same sheet which is being shown to everyone.

For example, participant A1 will now know the type of participant A2, but will not know the types of participants A1 (him or herself), B1, B2, C1, C2, D1, and D2. Likewise, participant B2 will now know the type of participant B1, but will not know the types of participants B2, A1, A2, C1, C2, D1, or D2.

At the end of the session, the experimenter will hold up the Type Sheet for Session I at the front of the room so that each participant can observe the types of all the participants and verify that the information received at the beginning of the session was correct.

Once every participant has recorded the type of the other participant in their group, then the first period of Session I will begin. In each period, you will have the opportunity to earn or lose money. Please look at Table 1 now, it describes how your payoffs for each period will be determined. In each period, you will choose between one of two actions: Up or Down. Your earnings in each period will be determined by the action you choose and by your own type. Looking at Table 1, you can see that if you choose Up, then you will earn 0 cents, regardless of whether your type is "X" or "O". If you choose Down and your type is "X", then you earn 20 cents. Finally, if you choose Down and your type is "O", then you lose 1 dollar. Please note that the type of the other person in your group does not affect your earnings in each period.

Session I will continue for each group until either participant selects Down in any period, or until three periods have passed. That is, if in any period one of the two participants in a group selects Down, then Session I will end for that group after that period.

I will now describe what happens in each period. At the beginning of each period, each participant will place a mark in one of the three boxes on their Reporting Sheet for that period. If you wish to choose an action of Up in that period, then place a check in the box corresponding to that choice. Similarly, if you would like to choose Down, then you should place a check in that box. Finally, if you or the other participant in your group has previously chosen Down, then you should place a check in the box labeled "Session Over". This box should only be checked in periods where the experimenter has instructed participants in that group to do so. Participants should also record their choice of action for each period on their Session I Record Sheet.

Once you have placed a check in one of the three boxes, then tear the Reporting Sheet for that period off and place it face down on you desk. The experimenter will come by to collect the Reporting Sheets for all participants once they have all done so.

Once the experimenter has collected all of the Reporting Sheets, he will write the actions selected by each participant, referring to them only by their participant number, on the board at the front of the room. Once this is done, all of the participants should record the action selected by the other participant in their group on their Session I Record Sheet. After all participants have done so, the experiment will proceed to the next period. Once three periods have passed, Session I will end and we will proceed to Session II. After Session III, you will be paid, in private, the total you have earned in all three sessions plus an $11 participation bonus. No other person will be told how much cash you earned in the experiment. You need not tell any other participants how much you earned.

Are there any questions before we begin Session I?

If there are no further questions, we will now begin with the experiment by selecting the monitor. If there are any problems or questions from this point on, raise your hand and an experimenter will come and assist you.

Inter-Session Instructions (After Session I)

Session I is now completed. I will now place the Type Sheet for Session I at the front of the room. Please record your type on your Session I Record Sheet. Using your type for Session I, please calculate your earnings for this session and record this amount at the bottom of your Session I Record Sheet. Once everyone has done that, then we will begin Session II.

Sessions II and III will proceed in exactly the same manner as in Session I except for two differences. First, the payoffs will be different from those used in Session I. Please look at Table 2 which the experimenter handed to you with these instructions. Notice that the payoffs are different from those in Table 1. Your earnings in each period will again be determined by the action you choose and by your own type. Looking at Table 2, you can see that if you choose Up, then you will earn 0 cents, regardless of whether your type is "X" or "O". If you choose Down and your type is "X", then you earn 1 dollar. Finally, if you choose Down and your type is "O", then you lose 5 dollars. Please note that the type of the other person in your group does not affect your earnings in each period.

Second, once the type of each participant has been determined, the experimenter will make an announcement for each group, indicating whether it is the case that there is at least one player of type "X" in that particular group. For example, if it is the case that no participants in group D are of type "X", then the experimenter will announce, "There are no participants of type 'X' in group D". Otherwise, the experimenter will announce, "There is at least one participant of type 'X' in group D".

Are there any questions before we begin Session II?

If there are no further questions, we will now begin with Session II. If there are any problems or questions from this point on, raise your hand and an experimenter will come and assist you.

Inter-Session Instructions (After Session II)

Session II is now completed. I will now place the Type Sheet for Session II at the front of the room. Please record your type on your Session II Record Sheet. Using your type for Session II, please calculate your earnings for this session and record this amount at the bottom of your Session II Record Sheet. Once everyone has done that, then we will begin Session III.

Final Instructions

Session III is now completed. I will now place the Type Sheet for Session III at the front of the room. Please record your type on your Session III Record Sheet. Using your type for Session III, please calculate your earnings for this session and record this amount at the bottom of your Session III Record Sheet.

After your are done, add together your Session I total, Session II total, Session III total, and the $11 participation bonus to get your final payoff for the experiment. Please record this on your Experiment Earnings sheet along with your name, social security number, and today's date. Please wait until after you have received payment to write your signature. If there are any problems or questions, please raise your hand.

After you are done calculating your earnings for the experiment, please remain seated. You will be paid at the front of the room one at a time in the order indicated by the experimenter. Please bring all of your things with you when you come to the front of the room. You can leave the experiment as soon as you are paid.

Please refrain from discussing this experiment while you are waiting to receive payment so that privacy regarding individual choices and payoffs may be maintained. Please place all of your experiment materials, except for the Experiment Earnings sheet, inside of the envelope which you were given at the beginning of the experiment.

Thank you all very much for participating in this experiment.

Appendix C: Instructions for weak-link game (experiment 1)

This is an experiment in the economics of decision making. Several research institutions have provided funds for this research. You will be paid for your participation in the experiment. The exact amount you earn will be determined during the experiment and will depend on your decisions and the decisions of others. Your earnings will be paid to you in cash at the conclusion of the experiment. If you have a question during the experiment, raise your hand and an experimenter will assist you. Please do not talk, exclaim, or try to communicate with other participants during the experiment. Participants violating the rules will be asked to leave the experiment and will not be paid.

Please look at the number at the top of this page. This is your participant number for the experiment. This participant number is private and should not be shared with anyone. Your participant number will be the same for the entire experiment and should be the same on all your sheets.

In this experiment, you will be one member of a project team that is responsible for producing a series of reports. Each report that the team prepares consists of several sections, where each member of the team is responsible for contributing one of the sections. A report is considered complete only after all members of the team have contributed their sections. Your team will be responsible for producing a total of twenty-two reports. Until a particular report is finished, no member of the team can work on his or her section of the next report.

You earn money based on how rapidly each report is completed. Each report is due in 7 weeks. However, every team member receives a bonus if the team completes the report in less than seven weeks. There are six possible early completion times: 1, 2, 3, 4, 5, or 6 weeks ahead of schedule. Hence, as a team member you must decide whether to contribute your section of a report during week 1, week 2, week 3, week 4, week 5, week 6, or week 7. The earlier a report is completed the larger the bonus. The bonuses associated with the alternative completion times are described in Table 1.

Note that the only way a report can be completed in just one week is if every team member contributes his or her section during week 1. Likewise, the only way a report can be completed in just 2 weeks is if every team member contributes his or her section within two weeks (either 1 or 2 weeks), and so on. Hence, the completion time of a report is equal to the maximum time taken by any one of the team members to contribute his or her section. Every team member receives the same bonus.

As a team member, you must decide how long to take to contribute your section of each report, without knowing how long the other members of the team will take to contribute their sections. Completing your section imposes a personal cost on you. This cost is higher if your contribution time is earlier. Table 2 describes the dollar value of the personal costs you could incur for the possible contribution times of your own section. Note that the sooner you contribute your section the higher the personal cost you incur. However, the sooner that all team members are finished, the higher is the completion time bonus for everyone on the team.

Your total earnings from a given report will be equal to the completion time bonus minus your personal cost. The various possible earnings are given in Table 3 below. Each column in the table represents a possible completion time of the report, which is equal to the maximum time selected by any team member (including you) to contribute his or her section of the report. Each row of the table represents a personal contribution time that you might select. Each of the cells in the table represents the earnings associated with a combination of your personal contribution time and the report completion time for the team.

During the experiment, the size of the team will vary. Please look at Table 4. Each row in the table corresponds to one of the twenty-two reports to be completed. We will start with Report 1 and will proceed sequentially through Report 22. The second column in the table indicates which participants will be on the team for a given report. For example, the team for Report 1 will consist of only Participants 1 and 2. Only the participants on the team will select a personal contribution time, pay a cost, and receive the bonus.

The remaining participants will not have any effect on the completion of the report and will not pay the cost or receive the bonus. These participants will be able to observe the report completion time and will earn a fixed amount per report. The exact amount that these participants will earn will not be revealed until the end of the experiment, however, because we do not want this amount to influence your choices during the experiment. The experimenter will now hand out an envelope to everyone in the room. Inside the envelope is a card with the amount that will be earned each round by participants that are not part of the team. This number is the same for everyone and will be announced at the conclusion of the experiment. Please do not open this envelope until the end of the experiment.

In your folder you are provided with a stack of Reporting Sheets. Please look at these sheets now. There is one sheet for each of the twenty-two reports. On every Reporting Sheet, there is a box corresponding to each one of the seven personal contribution times and an additional box marked "Not on team". You should also have a Record Sheet. At the end of each report, please make sure that you fill out the line on the Record Sheet corresponding to that report.

Each of the twenty-two reports will proceed as follows:

1) First, the experimenter will announce which of the participants are on the team for the current report. This information is also in Table 4.

2) Each of the team members will select a personal contribution time by checking one of the boxes on the Reporting Sheet corresponding to the current report and circling the personal contribution time on their Record Sheet. At the same time, each of the participants not currently on the team will check the "Not on team" box both on their Reporting Sheet and circle "Not on team" on their Record Sheet. Please make sure that your choices on both the Record Sheet and the Reporting Sheet are the same.

3) The experimenter will then collect all the Reporting Sheets, and will announce the report completion time for the team and the corresponding bonus. Every participant should record both the report completion time and corresponding bonus on their Record Sheet.

4) Each participant on the team will then determine his or her earnings for that report by subtracting the personal cost associated with their contribution time from the bonus. These earnings can also be determined from Table 3 by identifying the row corresponding to the contribution time you selected and the column corresponding to the maximum contribution time selected by the team members. The amount earned for the report should be recorded on the Record Sheet. Participants not on the team for that report should leave the earnings column blank.

5) At the conclusion of the twenty-second report, your earnings for the experiment will be the sum of your earnings for each of the reports plus the $9 participation fee. If there are any reports for which you are not on the team, your earnings for those reports will be the amount in the envelope you have received. Note that it is possible for the sum of your earnings for the 22 reports to be negative, but that the sum of your earnings plus the participation fee will always be positive.

Before we begin the experiment, you will answer a short set of questions to make sure that everyone understands the instructions. Please take a moment to answer the questions on the next page. Once you are done, raise your hand and the experimenter will come by to check your answers. After everyone has completed the questionnaire, the experimenter will read the correct answers aloud and we will proceed with the experiment. There should be no talking from this point on. If you have a question, raise your hand.