Credible Assignments and Performance Bonuses in the Minimum Effort Coordination Game[1]

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Abstract

We use experiments to investigate the efficacy of credible assignments and performance bonuses in resolving coordination failures in a stag-hunt type coordination game with multiple Pareto-ranked equilibria, often referred to as a "weak link" game. Participants routinely find it difficult to coordinate to the payoff-dominant outcome in such games. We look at performance in both fixed and randomly re-matched groups. A credible assignment to the payoff-dominant outcome is successful in resolving coordination failures with fixed groups. Resolving coordination failures is harder with randomly re-matched groups and can be achieved only with the payment of performance bonuses.

JEL Classification: C91, C72, L23, M52

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1. Introduction

A range of phenomena in life – both economic and non-economic - require coordinated action by multiple agents. For instance, think of an airplane sitting on the tarmac prior to take off. The on-time departure of this flight depends on successful coordination between a disparate group of people including the pilots carrying out pre-flight checks; baggage handlers loading the baggage; the cleaning crew; those loading the fuel; the ones loading the food containers and so on. The only way the plane will get off on time is if all these groups manage to successfully *coordinate* their actions and work at the same pace; if even one of these groups lag behind then the flight gets delayed.

Such coordination problems arise in any organization or industry that is engaged in team production along an assembly line, which includes steel-mills, automobile factories and fast-food outlets. When someone walks in to buy a burger, in order to get that burger from the person who is frying them, to the person who puts them inside the buns, to the person who puts on the cheese, onions and pickles and wraps them, to the person at the front of the store, who finally hands it over to the customer, a complex coordination problem has just been addressed where success depends on how quickly one can get the burger to the customer and reduce the time people are waiting in line.

The successful resolution of possible coordination failures is crucial to achieving optimal outcomes in such cases. Very often the resulting game is characterized by strategic complementarities and gives rise to multiple Pareto-ranked equilibria. Kremer (1993) in his O-Ring theory of development proposes extensive coordination failures as the cause of under-development in many countries. Here countries may be caught in a low-level equilibrium "trap" when development requires the simultaneous industrialization of many sectors of the economy but no sector can break even industrializing alone. Kremer's arguments are an extension of the original "big push" literature which suggested that an economy might under-perform in the absence of coordinated action by different sectors of the economy. (See for example Rosenstein-Rodan, 1943, Hirschman, 1958 as well as Murphy, Shleifer and Vishny, 1989).

In a macroeconomic context, an economy can get trapped in a Pareto-inferior under-employment equilibrium. In such instances no firm wishes to expand production unless it can be assured that others will do so yet not doing so leads to an outcome that is worse for everyone concerned. Bryant (1983) presents a Keynesian model with a continuum of underemployment equilibria. Cooper (1999) provides an extensive overview of this literature. Similar considerations arise in models of currency crises or speculative attacks (see Morris and Shin, 1998), models of bank-runs (see Diamond and Dybvig, 1983) and models of political revolution (see Cason and Mui, 2007, Kuran 1987, 1995 and Weingast, 1995, 1997, 2005).

Knez and Simester (2001) provide a detailed account of how successful resolution of coordination failures in various operations led to the remarkable turn-around at Continental Airlines after 1995. Ichniowski et al. (1997) describe how successful steel-mills adopt innovative human resource management practices that foster coordination along their production lines in an attempt to boost productivity. In situations like these it is the slowest worker who determines the speed of the line. This fact has led to this class of games being often referred to as a "weak-link game".

A large body of prior research suggests that in coordination games with multiple Pareto-ranked equilibria participants often find it difficult to coordinate to the payoff-dominant outcome leading to widespread coordination failure.[2] Cooper, DeJong, Forsythe and Ross (1990, 1992) provide some of the earliest evidence of such failure to coordinate to the payoff dominant outcome in a number of 2X2 and 3X3 coordination games.

A more dramatic example of coordination failure comes from Van Huyck, Battalio and Beil's (1990) (henceforth VHBB) study of the minimum effort coordination game which is an n-person stag-hunt game with multiple Pareto-ranked equilibria. In this game each subject in a group has to choose an integer, c_i from the set {1, 2, 3, 4, 5, 6, 7}. Individual payoffs are determined for each subject by the payoff function $\Pi_i = k + a[min\{c_i...c_n\}] - bc_i$. That is, the payoff for any player *i* is equal to a constant, *k* plus another constant, *a*, times the minimum choice of any subject minus a third constant, *b*, times *i*'s choice. The highest payoff occurs when all participants choose 7 but since the cost of one's choice is subtracted from the common payoff to all, higher choices are more risky. In fact, the mini-max or secure strategy choice is to choose c_i =1.

VHBB ran 7 sessions with groups of 14 – 16 participants playing for 10 rounds. They found that none of the 7 groups managed to achieve a minimum greater than 4. Moreover the minimum chosen never stayed above 1 for more than three rounds. In two of the seven groups (Groups 6 and 7) the minimum is 1 in each round.

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2 As Devetag and Ortmann (2007) point out, coordination failure can refer to two separate issues: (1) a failure to coordinate to any of the multiple equilibria in a pure coordination game such as the battle-of-the-sexes game or (2) a failure to coordinate to the payoff dominant outcome in a stag-hunt type coordination game with multiple Pareto-ranked equilibria. In talking about coordination failure we are talking about the latter situation.

These results, demonstrating wide-spread coordination failures, led to a number of subsequent studies, which look at interventions that might help participants in coordinating their actions. We review a few of these – the ones most relevant to the current study - in the next section. Devetag and Ortmann (2007) provide a comprehensive review of this literature.

In this paper we look at the success of two possible interventions designed to enhance coordination in weak-link games: (1) a "*credible assignment*" and (2) a "*performance bonus*".

An "*assignment*" is a non-binding pre-game announcement made by an external arbiter instructing the players to adopt a particular strategy. Van Huyck, Gillette and Battalio (1992) (henceforth VHGB) were the first to study such credible assignments in a 2-player 3X3 game. As VHGB point out (p. 606),

> *"A strict equilibrium in a game is defined as an assignment to each player of a strategy that is a unique best response for him when the others use the strategies assigned to them. An equilibrium assignment has the desirable property that the prescribed behavior is individually rational and mutually consistent."*

VHGB find that assignments to the payoff dominant outcome are credible to most players. When players are instructed to choose the strategy commensurate with the payoff-dominant equilibrium, 98% of the pairs managed to coordinate to this equilibrium following the assignment. Given that a credible but non-binding assignment made by an external arbiter succeeds in resolving coordination failures in prior studies, we wish to see if a similar assignment can be successful in the minimum effort coordination game where participants have an especially difficult time coordinating to the payoff-dominant outcome.

Brandts and Cooper (2006) study the "*corporate turn-around game*", a modified and simpler version of the minimum effort game, where players choose one of five effort levels rather than the seven effort choices available in the VHBB game. They find that if players are given a performance bonus for successful coordination, then this leads to a significant reduction in the extent of coordination failures. What is surprising is that the magnitude of the bonus does not seem to matter in that larger bonuses were no more successful in enhancing coordination than smaller bonuses.

However one shortcoming of the papers that study how to improve coordination in the minimum effort game is that they focus only on *fixed groups* of players.[3] One drawback to focusing on fixed groups is pointed out by Benoit and Krishna (1985, p. 905)

> *"…the fact that games with multiple Nash equilibria may have interesting perfect equilibria when repeated a finite number of times, if not unrecognized, has certainly escaped wide notice."*

It is possible that previous authors have tended to focus on fixed groups because many of these coordination failures are endemic to situations where the same agents interact

[3] The papers by Cooper et al. (1990, 1992) that study 2X2 and 3X3 stag-hunt games used random re-matching. Clark and Sefton (2001) look at the performance of both fixed and randomly re-matched groups in a simple 2X2 coordination game – the simple coordination game (SCG) - studied in Cooper et al (1992).

repeatedly. But as Benoit and Krishna (1985) point out, theories of equilibrium selection are essentially based upon one-shot play and repeated interactions (such as the ones modeled in papers like VHBB) can support a richer set of equilibria than would be the case in one-shot games. Thus in order to understand how to resolve coordination failures in such games, it is essential to study the performance of both *fixed* and *randomly re-matched* groups.

Furthermore, there are real-life examples where the game being played has the characteristic of one-shot interactions. Examples of such situations include (1) post-offices hiring additional temporary workers during Christmas; (2) immigrations authorities hiring temporary workers following a sudden sharp increase in the number of visa applications and (3) tax authorities hiring additional hands immediately after the annual deadline for filing tax returns. Fast food outlets experience rapid turn-over of its labor force. These organizations all require coordinated actions among the agents involved, including the temporary workers. In these cases the resulting interaction is better modeled as a one-shot game rather than a repeated game.

We study two different treatments vis-à-vis the assignment. In the *public knowledge* treatment we hand out a sheet of paper to each subject instructing them to choose the strategy that will lead to the payoff-dominant outcome. Here each subject knows that every other subject is looking at the exact same message. In the *common knowledge* treatment, in addition to handing out the exact same message to each subject, the experimenter, typically a research assistant, also reads this message out loud. In the latter case then, each subject not only knows that every other subject is receiving the same message, but now they also know that every other subject has heard the same message being read aloud.[4] We study the efficacy of such assignments in resolving coordination failures among both fixed and randomly re-matched groups.

Among randomly re-matched groups we also study, in addition to the assignment, the success of a performance bonus in facilitating coordination to the payoff dominant outcome.

We find that a credible assignment is successful in resolving coordination failures among fixed groups as long as that announcement is public *and* common knowledge. Resolving such coordination failures in randomly re-matched groups is more difficult and can be achieved only by paying a performance bonus but not by a credible assignment.

We proceed as follows. In Section 2 we undertake a very brief review of the experimental literature which examines how to resolve coordination failures in the minimum effort game. We explain the experimental design and procedures in Section 3. We present our results in Section 4. In Section 5 we offer some concluding remarks.

[4] The reason we introduced the public and common knowledge treatments is the following. Two prior papers – Chaudhuri, Schotter and Sopher (2008) which studies the VHBB minimum effort game and Chaudhuri, Graziano and Maitra (2006) which studies a linear public goods game – report that convergence to the payoff-dominant outcome is improved when participants are given a message that is both public (i.e. everyone in the group gets the same message and knows that everyone else is getting the same message) and common knowledge (i.e. this message is also read aloud). Both these studies report, however, that when the message is only public but *not* common knowledge, participants find it difficult to coordinate to the payoff-dominant outcome. In the VHGB study the message is projected on the lab wall and is also read aloud. Their treatment, then, is analogous to our "common knowledge" treatment.

2. A Brief Literature Review

A number of prior studies have explored the issue of coordinating to the payoff dominant outcome in games with multiple Pareto-ranked equilibria. In this section we review those papers that are the most relevant to our study.

VHGB study the role of assignments in a two-person 3X3 coordination game with three Pareto-ranked equilbria. There is an external arbiter who instructs the participants to choose a particular strategy in the game. They find that when the arbiter instructs the participants to choose a strategy commensurate with the payoff-dominant outcome 98% of the pairs playing the game do coordinate to that outcome.

However, in the game studied by VHGB, there is no obvious conflict between payoff dominance and risk dominance. As they also point out in their paper (p. 611) the payoff-dominant outcome {1, 1} is also the outcome which requires the smallest "minimum sufficient degree of credibility". Therefore, the {1, 1} outcome is the most credible outcome in this game, which is followed by {2, 2} and then by {3, 3}. Bangun, Chaudhuri, Prak and Zhou (2006) extend VHGB's work by studying the efficacy of credible assignments in a 2X2 coordination game with a clear distinction between the payoff-dominant outcome and the risk-dominant outcome. Bangun et al. find that a credible assignment along the lines of VHGB work quite well in their game with more than 90% of the pairs managing to coordinate to the payoff-dominant outcome. Seely, Van Huyck and Battalio (2005) find that such credible assignments are successful in increasing contributions and reducing free-riding in a voluntary contributions mechanism.

Blume and Ortmann (2007) look at the impact of costless pre-play communication in the context of the minimum and median effort games. We will confine our focus to the former for purposes of comparison with our results. These authors conduct 8 sessions with 9 participants in each session who play the minimum effort game for 8 periods. The payoff matrix is the exact same one as in VHBB. Each period consists of two stages where in the first stage each subject makes a non-binding announcement regarding what number between 1 and 7 he proposes to choose in the second stage. Participants get to see the distribution of these messages on their computer screen. These pre-play messages are followed by an action stage where each subject actually chooses one of those numbers. Despite this opportunity to communicate with others in the group, participants in Blume and Ortmann's study still find it difficult to coordinate to the Pareto-dominant outcome. Out of 8 sessions there is only one session where all participants choose 7 for all 8 periods.[5]

Brandts and Cooper (2006, 2007) study the corporate turn-around game which is a modified version of the VHBB minimum effort game. The game involves five players – a manager and four workers – who together constitute a firm. The groupings are fixed from one round to the next. Brandts and Cooper (2006) show that starting from a situation where the firm is experiencing significant coordination problems – in that the workers are not able to coordinate their actions so as to reach the Pareto optimum

[5] Blume and Ortmann (2007) do not provide a detailed breakdown of the actual choices made and the minimum chosen in each period for the 8 sessions. However looking at Table V in their paper it is clear that there was only one session where the minimum chosen was 7 for all 8 periods. This is because a choice of 7 by all members of the group will yield a payoff of $1.30 to each subject in any period. Over 8 periods this would amount to a total earning of $10.40. This was true of only one session – session M8Min. The other sessions achieved various degrees of coordination but none of them achieved coordination at the Pareto-dominant outcome for all 8 periods.

– a performance bonus paid by the manager to the firms is successful in resolving these coordination failures. Surprisingly it turns out that while the payment of a bonus does help workers within the firm coordinate their actions, the magnitude of the bonus itself seems less important in that a larger bonus does not necessarily lead to any greater coordination than a smaller one. Brandts and Cooper (2007) extend the previous paper by providing managers with two different tools for resolving coordination failures: (1) payment of performance bonuses and (2) direct communication with employees. They find that while both the bonus and communication are successful in enhancing greater coordination, communication is a more effective tool than incentive changes in leading organizations out of coordination failure traps.

Van Huyck, Battalio and Beil (1993) and Cachon and Camerer (1996) use forward induction/loss avoidance arguments to solve the coordination problem. There are two stages. In stage 1 subjects have to pay for the right to participate in a median effort (rather than a minimum effort) coordination game in stage 2. These studies find that when subjects pay for the right to participate in the coordination game they are better at coordinating to the payoff dominant outcome since the amount they pay serves as a signal of their intended course of action in the game itself.

Chaudhuri, Schotter and Sopher (2008) study coordination in the minimum effort coordination game using the same payoff matrix as in VHBB. They introduce an "*inter-generational paradigm*", where subjects in any generation, consisting of a group of 8 players, play the stage game for 10 rounds. After that each subject in generation *t* can leave free-form advice for their generation *t+1* successors and the process continues for a number of generations. Payoff to each player in any generation is the sum of his own earnings plus the earnings of his successor in the next generation. These authors find that this process of leaving advice for successive generations can foster coordination to the payoff-dominant outcome but only when the advice is "public" (i.e. the advice from all the members of one generation is made available to all the members of the next generation) and also "common knowledge" (i.e. this advice is read aloud for all group members to hear, so that each subject not only knows that every one is getting the same message, but they have also heard this message being read out loud).

Finally Weber (2006) introduces a "growth path" paradigm and shows that while it is often difficult to achieve efficient coordination in large groups, efficiently coordinated large groups can be "grown." By starting with small groups (typically of size two) that find it easier to coordinate, one can add people at a sufficiently slow rate to create efficiently coordinated large groups.

3. Experimental design and procedures

Two hundred and ten participants were recruited from our University. Participants are under-graduate and post-graduate students in commerce and economics, who have no prior experience with the minimum effort game. All experiments were conducted in a computer laboratory at the university using the Veconlab website[6] developed by Charles Holt at the University of Virginia.

We use a modified version of the VHBB minimum effort game, one that was introduced by Goeree and Holt (2001). Participants in this version of the minimum effort game are asked to choose numbers belonging to the set {1.1, 1.2, 1.3, 1.4, 1.5, 1.6 and 1.7}. The payoff to subject *i* is given by $\pi_i = Min(c_i, c_{-i}) - 0.5(c_i)$ where c_i is the number chosen by player *i* while c_{-i} is the smallest number chosen by any member of the group. As opposed to the original VHBB study where subjects only learn the minimum number chosen in each round, in our study each subject *gets to see the effort levels chosen by the other players in the group at the end of each round*; of course they never learn the identity of those group members. The payoff matrix generated by this payoff function is shown in Table 1. There are 7 Pareto-ranked Nash equilibria located along the diagonal and depicted in bold.

<Table 1 here>

We carry out a total of 11 sessions with 20 subjects in each one of 9 sessions and 15 subjects each in 2 sessions. At the beginning of each session the instructions of the game were read out loud to the subjects. Once they log-on to the website, subjects can also read this instruction privately on their computer screen. They are given 10-15 minutes to read through online instructions and ask any questions that they might have.

[6] http://veconlab.econ.virginia.edu/admin.htm

Subjects are put into groups of 5 for each session. Each session consists of 15 rounds of play of the stage game. In each round subjects choose their effort levels simultaneously.

We look at two different interventions: (1) *an assignment to a strategy* and (2) *a performance bonus*. The assignment involved providing the subjects with the following message, which is adapted from VHGB:

> You should pick 1.7 in each round.
>
> NOTICE, from the payoff matrix, that if every participant in a group follows the message then every participant will earn $0.85. However, if even one of the participants does not follow the message and chooses a number different from 1.7, then each participant will make less money than if everyone chose 1.7.

We have two different treatments involving the assignment. In the "*public knowledge*" treatment each participant is given the above message on a typed sheet of paper and each subject knows that every other subject is getting a similar sheet with the exact same message typed on it. In the "*common knowledge*" treatment, in addition to providing each subject with a sheet of paper with the message typed on it, this message is also *read out aloud* by the experimenter. Therefore in this treatment, each subject not only knows that every other subject has been given the exact same message but also that each subject has heard this message being read out loud.

In the performance bonus treatment we provide an additional payment to each subject for each round that they manage to coordinate to the payoff-dominant outcome. This is done by providing each subject with a sheet of paper with the following message and then *reading the message out loud* for all subjects to hear. So each subject knows that everyone else is getting the exact same message and they have also heard it read out loud.

> "If in a particular round all 5 players in your group choose 1.7 so that the minimum number chosen is 1.7, then in that round each player will earn an additional 50 cents on top of the 85 cents that you get for choosing 1.7. Hence for that round, each player will earn 1.35 dollars. This will be true for each and every round where the minimum is 1.7."[7]

We implement two different matching protocols: (1) *fixed* groups, whose composition remains unchanged for the entire duration of the game and (2) *randomly re-matched* groups, whose members are randomly re-assigned to groups at the end of each round by picking five players per group out of the subject population for the session.

This then defines a 3X2 design: *three* treatments – (1) public knowledge assignment, (2) common knowledge assignment and (3) performance bonus and *two* matching protocols – (1) random and (2) fixed. We fill out all the cells except we do not look at the bonus in fixed groups. This is because the fixed groups manage to achieve efficient coordination with the assignment and would have likely done so with the bonus, which provides stronger incentives to coordinate. We look at the bonus only in randomly-re-matched groups since these groups exhibited significant coordination failures otherwise. Table 2 shows the design of our experiments.[8]

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[7] We should point out that our performance bonus works differently than the one implemented by Brandts and Cooper (2006). The bonus in their study is more "continuous" in the sense that any non-zero minimum effort level will earn the group a performance bonus. Of course, the higher that minimum effort the higher is the bonus, with the highest possible bonus accruing in the event that every one coordinates to the highest possible effort level. Our bonus is "discontinuous" in that there is no bonus for coordinating to any effort level other than 1.7. Many real life bonus schemes operate in the same way where the group members get a bonus only if they achieve a specific target such as coming first or exceeding a certain amount of sales revenue.

[8] For the randomly re-matched groups, we also tried another treatment where the information about the performance bonus was distributed to players on sheets of paper but this information was *not read out loud*. Here, each subject knows that every other subject is getting the same information, except the information is not announced publicly any more. We have 15 subjects, i.e., three groups of five, in this

<Table 2 here>

In all treatments subjects first play 5 rounds with no assignment or bonus. Immediately after the conclusion of the 5th round and prior to the beginning of the 6th round we provide the subjects with the message which varies depending on the treatment. This message is either the one which instructs subjects to choose the strategy commensurate with the payoff-dominant outcome or informs the subjects of the bonus for efficient coordination. We should point out that in our study the assignment to a strategy is done *only once* prior to the beginning of round 6 of the 15 rounds of play as opposed to VHGB where the authors make an announcement prior to each round of play. The subjects then play an additional 10 rounds of the game after receiving the message without any further interruptions.

While subjects play for 15 rounds in all treatments, in one session of the common knowledge assignment treatment with fixed groupings we had to terminate the session after ten rounds.[9] This session had 15 subjects (i.e. 3 groups of five). These subjects played the first 5 rounds with no assignment and then only 5 rounds rather than 10 with the common knowledge assignment. These groups, like the rest of their peers in the common knowledge assignment with fixed matching protocol, achieve efficient coordination for all the 5 rounds following the assignment. However in reporting our

treatment and the subjects were randomly re-matched into groups at the end of each round. They played for 15 rounds as in the other treatments, the first five with no intervention and the last ten with a performance bonus. However this treatment did not facilitate coordination to the payoff dominant outcome. In fact, it was clear that this treatment did not fare any better than a public knowledge or common knowledge assignment to a strategy for these randomly formed groups. As a result we did not carry out more sessions of this treatment and have excluded a discussion of this treatment in the current paper.

[9] Reasons beyond our control delayed the start of this session and we realized that we would not finish running all the rounds in the allotted lab time. We decided to end the session after 10 rounds.

results below we exclude this session and report the results for the remaining 195 subjects (39 groups).

We also elicit the subjects' beliefs immediately before and immediately after receiving the message. Immediately after the subjects learn about the choices and outcomes for round 5, we ask them to predict the *average* choice for round 6 of that session. We ask them to write down a number from the set {1.1, 1.2, 1.3, 1.4, 1.5, 1.6, or 1.7} in the sheet provided. After all subjects have entered their prediction, they receive the message assigning them to a strategy or telling them about the bonus. Immediately after this we ask the subjects to predict the *average* choice again. After all subjects have made their second prediction, they then continue to play the game for another 10 rounds without any further interruptions.

For subjects in the random re-matching protocol we ask the subjects to predict the *average* for the *session as a whole* since here the groupings are not fixed and therefore asking them about the group they were a part of in round 5 does not make sense since they will most likely be grouped with different subjects in round 6. In the fixed matching protocol the groups are unchanged from one round to the next, and here we do ask the subjects to predict the average of round 6 choices made by the subjects in their own group.

We elicit beliefs about the average rather than about the minimum for the following reason. Suppose, prior to an announcement, a subject believes that 3 out of 5 group members will choose 1.1, one person will choose 1.5 while the other will choose 1.7. This person's belief about the minimum choice in the group is 1.1 but his belief regarding the average choice is 1.3. Now suppose, after the announcement, the subject comes to believe that 4 out of 5 group members will choose 1.7 while one person will still choose 1.1. Here his beliefs about the minimum chosen in the group are unchanged but he now believes that the average choice in the group is 1.58. Therefore his beliefs about the choices to be made by his fellow group members have certainly become more optimistic but eliciting beliefs about the minimum will not reflect this fact while eliciting beliefs about the group average will.

The subjects are paid for these predictions in the following way. For each prediction a subject earns $1 minus the absolute difference between his predicted average and the actual average. So for instance suppose a subject predicts that the average choice in Round 6 will be 1.4. Suppose the actual average turns out to be 1.6. In this case the absolute difference between the predicted choice and the actual average is 0.2 and the subject earns $1.00 - $0.20 = $0.80.

All payoffs are denoted in actual dollars and cents. Subjects were paid NZ $5 show-up fee in addition to the earnings from the experiment and predictions. They are paid privately at the end of the session. Each session lasts around 45 minutes and participants make NZ$16.40 on average. The highest earning in this experiment is NZ$20 and the lowest is NZ$11.[10]

[10] At the time the experiments were carried out the exchange rate was approximately NZ $1 = US $0.75.

3. Results

3.1 Choices in the minimum effort game

Observation 1: Fixed groups are more coordinated than randomly re-matched groups.

We have 120 subjects across three treatments in the random re-matching protocol and 75 subjects across the two treatments in the fixed matching protocol (excluding the 15 subjects who played for 10 rounds only). Given that there are 5 players in each group, this gives us 24 groups in the former treatment and 15 in the latter. Subjects play the basic stage game in the first five rounds of each treatment with no intervention. Therefore if we focus exclusively on the first 5 rounds then this allows us to isolate the effect of matching protocol on coordination.

Given that there are 5 players in each group and *one* play of the stage game in any particular round is generated once all 5 members of a group have made an effort choice this then gives us 120 plays of the game (24 groups generating 24 plays of the game for each one of 5 rounds) in the *random matching* protocol and 75 plays of the game (15 groups generating 15 plays of the game for each one of 5 rounds) in the *fixed matching* protocol. Figure 1 shows the behavior of the average minimum effort and the proportion of 1.1 choices (the lowest possible effort level) over the first five rounds. The darker shaded bars summarize behavior in the random matching protocol while the lighter shaded bars do the same for the fixed matching protocol.

A number of things stand out. First while the average minimum across the 15 groups in the fixed matching protocol hover around 1.3 for all five rounds the average minimum across the 24 groups in the random matching protocol drops to less than 1.2 by round 4.

Next, we look at the proportion of plays where the minimum effort chosen was 1.1. For ease of exposition we will focus only on the lowest possible effort choice here. So what we are looking at is, in each matching protocol, what proportion of the groups involved ended up choosing a minimum of 1.1 (i.e. at least one member of the group chose 1.1) across all plays during the first five rounds. Here again the differences are clear. In the fixed groups, the proportion of groups choosing 1.1 is relatively stable over the first five rounds. This proportion starts out at 14% in round 1, dips to 9% in round 2 and stays there till round 4 before increasing marginally to 14% in round 5 again. The situation is quite different among the randomly re-matched groups. Here the proportion of groups ending up at the minimum possible effort level increases from 27% in round 1 to 50% in both rounds 4 and 5.

<Figure 1 here>

Using a Wilcoxon ranksum test we can reject the null hypothesis that the effort choices in the two different matching protocols are drawn from the same population ($z = 2.79$, $p < 0.01$). This result is perhaps not surprising. Clark and Sefton (2001) report a similar finding using a simpler 2X2 stag-hunt game. They find that fixed groups are better able to coordinate compared to randomly re-matched ones. They attribute the greater success at coordination by fixed groups to signaling. The authors argue that the risk involved in playing the efficient equilibrium strategy may be too high to justify its use in the one-shot game because by doing so a subject simply leaves himself vulnerable to getting a low payoff. However in the repeated game, some subjects may find it worthwhile to signal that they desire the efficient equilibrium outcome. Even if they get a low payoff initially, if they stick to their choice, then future payoffs improve if their opponent adjusts behavior in response.

Observation 2: A common knowledge assignment is successful in resolving coordination failures in fixed groups.

In Figures 2 and 3 we show the behavior of the groups in the *fixed* matching protocol. Figure 2 shows the behavior of the average minimum across the first 5 rounds of play (prior to the subjects receiving the message) and the following 10 rounds (after they get the message). As one can see from Figure 2, there is significant coordination failure during the first 5 rounds with the average minimum hovering around 1.3 across the different groups. Thus there is a substantial scope for improvement in performance. As Figure 2 makes clear both the public knowledge and common knowledge assignments have significant effects on the effort choices with the common knowledge announcement being especially effective. When the assignment to the efficient equilibrium strategy is common knowledge the average minimum choice across the groups jumps up to 1.6 in round 6 immediately following the announcement and is 1.7 (the payoff-dominant outcome) for all the remaining rounds. So in the common knowledge assignment treatment, all subjects in all groups chose 1.7 for each one of the last nine rounds, rounds 7 through 15.

<Figure 2 here>

Figure 3 emphasizes the same point. Here rather than looking at the temporal sequence of effort choices we look at the outcomes of multiple plays of the game. As mentioned before when all five members of a group make an effort choice that gives us one play of the stage game for that group. We have 40 subjects (8 groups of 5) in the public knowledge assignment treatment and 35 subjects (7 groups of 5) in the common knowledge assignment treatment. As mentioned before we exclude the 15 subjects (3 groups) in the common knowledge assignment treatment who played for 10 rounds only. These 3 groups managed to achieve consistent coordination to 1.7 for all rounds following the assignment. This gives us 80 plays of the game (8 groups generating 8 plays of the game for each one of 10 rounds) in the public knowledge treatment and 70 plays of the game (7 groups generating 7 plays of the game for each one of 10 rounds) in the common knowledge treatment.

We can now look at the outcomes in each of these interactions across all plays of the game. In Figure 3 we show the distribution of the minimum choice over all plays of the game in fixed groups in three cases: (1) before any assignment is made; (2) with public knowledge of the assignment and (3) with common knowledge of the assignment. As one can see from Figure 3, the modal outcome in terms of the minimum choice prior to the assignment is 1.1. 27% of all plays of the game ended with 1.1 as the minimum while 1.7 was the minimum chosen in only 1% of cases. The proportion of 1.1 as the minimum drops and the proportion of 1.7 increases once the assignment is public knowledge; however, even with public knowledge of the assignment, only 56% of plays achieve a minimum of 1.7 and 22% plays of the game end with a minimum of 1.1 or 1.2. However, once the assignment is common knowledge, subjects manage to achieve the efficient equilibrium in 88% plays of the game. In fact with a common knowledge assignment, all plays of the game end with a minimum choice of either 1.7 (88%) or 1.6 (13%). Thus no one chooses anything less than 1.6 when the assignment is common knowledge. A common knowledge assignment then is successful in resolving coordination failures in fixed groups.

<Figure 3 here>

Using a non-parametric Kruskal-Wallis test we can reject the null hypothesis of equality in the distributions of the effort choices for the three treatments – no assignment, public knowledge of assignment, common knowledge of assignment ($\chi^2 =$ 380.511 (2d.f.), $p < 0.01$). In Table 3 we report on the results of pair-wise non-parametric Wilcoxon rank-sum tests which analyze whether the effort choices in different treatments is drawn from the same population. Table 3 reports that the choices in the common knowledge assignment treatment are higher than those in the public knowledge treatment, which in turn are higher than the no assignment treatment and these differences are significant at the 1% level.

<Table 3 here>

Next we analyze the choices of subjects in each treatment by using an ordered probit model with a random effects specification of the error term. We use a random effects specification rather than fixed effects because each subject has individual-specific components in choice behavior. An ordered response model is the appropriate one here because there is a natural ordering of the choices leading to seven Pareto-ranked equilibria in this game. The random effects ordered probit model estimates six separate cut-points (or threshold parameter). Let $\alpha_1 < \alpha_2 < ..... < \alpha_6$ be the six cut-points provided by the ordered Probit model. The effort choice e_{it}^* is determined by the following latent equation:

$$e_{it}^* = X_{it}^{'}\beta + v_i + e_{i0}\xi_0 + X_i^{'}\xi + \varepsilon_{it}$$

where $i = 1, \ldots, n$ and $t = 1, \ldots, T$. The random effects (ν_i) are IID $N(0, \sigma_\nu^2)$ and the errors (ε_{it}) are $N(0, \sigma_\varepsilon^2)$ independent of ν_i. The independent variables in our model include: (1) *round*; (2) *two treatment dummies*, one for the *public knowledge* treatment and the other for the *common knowledge* treatment with the no assignment treatment being the reference category; (3) *effort chosen by a subject in the previous round* ($e_{i,t-1}$) as well as (4) *earnings in the previous round* ($earn_{i,t-1}$); (5) *interaction terms* between the two treatment dummies and round; (6) *lag difference* which is the difference between one's own choice in the previous period and the smallest one of the four effort choices made by the other four members of the group in the previous period ($e_{i,t-1} - e^m{}_{j,t-1}$), $j \neq i$, which could be positive, negative or zero; and (7) two interaction terms involving the two treatment dummies – one for public knowledge and the other for common knowledge – and this lag difference term.

Since we include lagged values of the dependent variable (effort choice) in our model, we include e_{i0}, the first observation of e_{it} (or initial condition) and $X_i = (X_{i1}, \ldots, X_{iT})$ as independent variables to obtain unbiased coefficient estimates. This is the correction suggested by Woolridge (2002, pp. 493-495) for dynamic panel data models of this nature. X_{it} does not contain a constant since the model already provides cut-points.

Table 4 reports the result. The regression results show that round is negative and significant at 1%. The coefficients for the two treatment dummies are positive and highly significant suggesting that subjects made higher effort choices in both the public knowledge assignment as well as the common knowledge assignment treatments.

The lagged value of one's own choice is positive and significant at 1% level. This implies that if subject *i* chooses a higher (lower) effort in the previous round then he will also choose a higher (lower) effort in the current round. Earnings in the previous round, however, do not have a significant effect on the effort choice in the current round.

The interaction terms between the two treatment dummies and round are positive and significant. We carried out a Wald test to compare these interaction coefficients. The Wald test does not reject the null hypothesis that round has the same effect on the effort chosen in the public and common knowledge assignment treatments ($\chi^2 = 0.00$, probability> χ^2 = 0.9485). This implies that over time the effort choice decreases at a faster rate in the no assignment treatment compared to either the public or the common knowledge assignment treatments.

The coefficient on the difference between one's own choice in the previous period and the minimum effort chosen among the other four group members in the previous period is negative and significant. If the effort chosen by a subject is higher than the minimum effort among the other group members in a particular round, i.e., when the lag difference is positive, then this subject tends to decrease his effort choice in the subsequent round; while if the subject's own choice was lower than the minimum effort among others in any round then this subject tends to increase his effort choice in the following round. The coefficients of the two interaction terms involving the two treatment dummies and the lag difference term are positive and significant for both the public knowledge assignment treatment and the common knowledge assignment treatment suggesting that the lag difference term has the strongest negative impact in the no assignment treatment compared to the public knowledge assignment or the common knowledge assignment treatments.

<Table 4 here>

Table 5 shows the estimated probabilities of making a particular effort choice averaged over rounds and subjects for each treatment. It shows that in both the no assignment and public knowledge of assignment treatments there is a small but non-zero probability of someone choosing 1.1 while in the common knowledge assignment treatment the probability of choosing anything less than 1.5 is zero. The probability of choosing 1.7 is 0.19 in the no assignment treatment, 0.68 in the public knowledge assignment treatment and 0.94 in the common knowledge assignment treatment.

<Table 5 here>

Observation 3: ***Achieving coordination to the efficient equilibrium is more difficult among randomly re-matched groups and can only be achieved with the payment of performance bonuses.***

Figures 4 and 5 show the behavior of subjects in the randomly re-matched groups. Figure 4 looks at the evolution of the average minimum effort level chosen before and after an intervention. As one can see in the first 5 rounds prior to a message the minimum outcome in most groups is low and hovers around 1.2. All three interventions have a salutary effect on effort choices in that choices jump up in round 6 immediately following an announcement.

But unlike in the fixed groups, the public and common knowledge assignments have limited success in raising minimum choices. While the choices do increase immediately following the assignment, this increase is not sustained and the numbers chosen start to decline over time. The only intervention that is successful is the performance bonus. Here the average minimum jumps up to about 1.6 following the introduction of the incentive scheme and stays there for the remaining time. However it should be noted that even the bonus is less successful than the common knowledge announcement in fixed groups since the bonus does not manage to facilitate coordination to the efficient equilibrium consistently.

<Figure 4 here>

In Figure 5 we focus on actual plays of the game and the minimum effort chosen across different treatments. We have 40 subjects (8 groups) in each of the three treatments here: public knowledge assignment, common knowledge assignment and performance bonus. This gives us 80 plays of the game (8 groups generating 8 plays of the game for each one of 10 rounds) in each of the three treatments.

Prior to an intervention, 46% of the plays of the game end with a minimum of 1.1 and none of the groups manage to achieve coordination at 1.7. With an assignment – whether public or common knowledge – matters improve somewhat but not substantially. The proportion of 1.1 as the minimum chosen drops slightly to 39% with public knowledge of the assignment and to 33% with common knowledge of the assignment but the proportion of plays where all group members managed to coordinate to 1.7 – and consequently a minimum of 1.7 – is a meager 8% with public knowledge of the assignment and 5% with common knowledge. The intervention that generates truly different behavior among randomly re-matched groups is the payment of a performance bonus. While it does not completely get rid of sub-optimal outcomes, nevertheless once subjects are paid a bonus for coordinating their actions so as to achieve the payoff-dominant outcome, there are no longer any plays of the game where the minimum is 1.1 and 95% of plays of the game achieve a minimum of 1.4 or more and 66% manage to coordinate to a minimum of 1.6 or 1.7.

<Figure 5 here>

A non-parametric Kruskal-Wallis test rejects the null hypothesis of equality of distributions of the effort choice in the four treatments – no assignment, public knowledge assignment, common knowledge assignment and bonus. (χ^2 = 340.889

(3d.f.), $p < 0.01$). We carry out pair-wise Wilcoxon ranksum tests to analyze whether the effort choices in two particular treatments are drawn from the same distribution. Table 6 reports the results of these pair-wise ranksum tests. Choices in the public knowledge of assignment, common knowledge of assignment and bonus treatments are higher than those of the no assignment treatment. Choices in the bonus treatment are higher than those in the other treatments. However the choices in the public knowledge and common knowledge treatments are not significantly different under the random re-matching protocol.

<Table 6 here>

As we did in the case of the fixed matching protocol we use a random effects ordered probit model to analyze effort choices. The independent variables include: (1) round; (2) *three treatment dummies* – one for the public knowledge assignment treatment, one for common knowledge and the third for the bonus treatment with the no assignment treatment being the base category; (3) *lag own effort* ($e_{i,t-1}$); (4) *lag earnings* ($earn_{i,t-1}$); (5) *interaction terms* involving the three treatment dummies and round; (6) *lag difference*, which is the difference between one's own choice in the previous period and the smallest one of the four effort choices made by the other four members of the group in the previous period ($e_{i,t-1} - e^{m}{}_{j,t-1}$), $j \neq i$, which could be positive, negative or zero; and (7) *three interaction terms* involving the three treatment dummies interacted with this lag difference term.

Table 7 reports the random effects ordered probit model. Notice from Table 7 that the coefficients for the treatment dummies are not significantly different from zero though the coefficient for the bonus treatment dummy only narrowly misses conventional levels of significance (p = 0.11). Using Wald tests we do not find a significant difference between the coefficients for the treatment dummies.

However if we carry out joint Wald tests to compare the joint effect of different treatments (interacted with other variables) on the effort choice then we get a significant difference in the effort choice between all treatment pairs except for the comparison between public knowledge and common knowledge of assignments. This implies that when comparing the treatment dummies alone we do not find any significant difference; however when we interact the treatment dummies with other variables we find that the effort choice in the bonus treatment is significantly higher compared to the no assignment, public knowledge of assignment and common knowledge of assignment treatments. However the effort choices in the public knowledge assignment and common knowledge assignment treatments are not significantly different.

This result is consistent with figure 5 which shows that the distribution of the effort choices in the public knowledge assignment and the common knowledge assignment treatments are similar but many more subjects choose 1.7 in the bonus treatment.

The regression shows that, as in the case of the fixed matching protocol, the lagged value of one's own effort choice is positive and significant at 1% level. This implies that if subject *i* chooses a higher (lower) effort in the previous round then he will also choose a higher (lower) effort in the current round.

The coefficient on the difference between one's own choice in the previous period and the minimum effort among the other four group members in the previous period is negative and significant. As in the fixed matching protocol this implies that a subject whose effort choice was higher (lower) than the minimum effort chosen among the other four group members in the previous round will reduce (increase) his choice in the current round. However the coefficients of the interaction terms between the three treatment dummies and the difference term are positive and significant. We use Wald tests to compare these coefficients across treatments where the null hypothesis is that two coefficients are equal. A Wald test rejects the null hypothesis for all treatment pairs except for the pairing between common knowledge assignment and bonus treatments ($\chi^2 = 2.50$, p = 0.11). Combining the results from the random effects ordered probit regression and the Wald tests we can say that the difference between one's own effort and the lowest effort chosen among the other group members in the previous round has the strongest negative effect in the no assignment treatment and the weakest negative effect in both the common knowledge assignment and the bonus treatments.

<Table 7 here>

Table 8 shows the estimated probabilities of choosing a particular effort choice averaged across rounds and subjects for each treatment. As one can see from this table, the probability of choosing an effort level of 1.7 is 0.78 in the bonus treatment; this probability is 0.40 in the common knowledge assignment treatment, 0.39 in the public knowledge assignment treatment and only 0.19 in the no assignment treatment.

<Table 8 here>

3.2 Beliefs

3.2.1. Beliefs prior to any announcements

As mentioned before, subjects played the first 5 rounds of the game without an assignment or a bonus, as the case may be. After the conclusion of the fifth round, we elicit subject beliefs about the average choice in round 6. Then we make the announcement regarding an assignment to a strategy or a bonus. We elicit subject beliefs regarding the average choice again after this announcement and prior to the commencement of play in round 6.

We begin by looking at the beliefs held by subjects *prior* to any announcement in the fixed and random groupings. These beliefs provide a clue regarding the impact of the matching protocol only on the predictions made by the subjects prior to any announcement. Using a non-parametric Wilcoxon ranksum test we can reject the null hypothesis that the beliefs held by the subjects in the two different matching protocols are drawn from the same population ($z = -3.79$, $p < 0.01$). Hence the matching protocol clearly has an effect on subjects' beliefs. Figure 6 shows the distribution of beliefs prior to an announcement over the seven possible effort choices for both the fixed (darker shaded bars) and the random re-matching protocol (lighter shaded bars). It is clear that the distribution of beliefs for the fixed grouping protocol is skewed to the right showing that subjects in this protocol hold more optimistic beliefs regarding average choices compared to their compatriots in the random re-matching protocol. Prior to an announcement, around 17% of the subjects in the fixed grouping protocol predict that the average choice in round 6 will be 1.7. The proportion of the subjects who believe the same in the random re-matching protocol is about 1%. It is clear from this figure that beliefs are more optimistic in the fixed grouping protocol possibly due to the fact that the fixed grouping provides signaling opportunities as mentioned before. It is not surprising therefore that these groups are more successful in achieving coordination in the game.

Of course, one needs to bear in mind that, the subjects in the fixed grouping protocol are predicting the average choice for their own groups, that is, for the other four group members. The subjects in the random re-matching protocol are predicting for all the subjects in the session, typically nineteen excluding the subject. This may explain part of the greater pessimism among subjects in the random re-matching protocol. There is no easy way to control for this difference. Still, it is clear that the matching protocol has a profound impact on the beliefs held by the subjects.

<Figure 6 here>

3.2.2 Beliefs after the announcement

Next, we analyze the predictions made immediately after the assignment or the announcement of a bonus in the fixed grouping and random re-matching treatments. After all, if an announcement is going to have a positive impact on actions, then we would expect it to do so by first generating more optimistic beliefs about the effort choices to be made by other group members. If a subject expects at least one of the other group members to choose 1.1, then that subject's best response is clear: choose 1.1. Thus higher effort choices must be predicated on beliefs regarding higher effort choices to be made by group members.

We will begin by looking at the impact of an announcement in the fixed-grouping protocol. Figure 7 shows the distribution of beliefs prior to and after an announcement. It is clear that the announcement, whether it is public knowledge or common knowledge, has a significant positive impact on the beliefs held by subjects. Prior to the announcement only 17% of the subjects predict that the average choice in round 6 would be 1.7. After the public knowledge announcement, the proportion of subjects who believe that the average choice in the next round will be 1.7 increases to 63% and after the common knowledge announcement, this proportion increases further to 83%.

<Figure 7 here>

Using a non-parametric Wilcoxon ranksum test we can say that the distribution of beliefs in both the public knowledge assignment and the common knowledge assignment treatments are significantly different from those in the no assignment treatment. ($z = -4.234$, $p < 0.01$ for the public knowledge assignment and no assignment comparison and $z = -4.153$, $p < 0.01$ for the common knowledge assignment and no assignment comparison.)

However, when we carry out a Wilcoxon ranksum test to see if the beliefs in the public knowledge and the common knowledge treatments are drawn from the same population, then we cannot reject the null hypothesis that they are, at least not at conventional levels of significance, even though the p-value is close to significance. ($z = -1.544$, $p = 0.12$). This implies that the public knowledge assignment and common knowledge assignments have a similar impact on the subjects' beliefs even though behavior in the two treatments are substantially different, with the common knowledge assignment resulting in much greater coordination. However, the mapping from beliefs to actions is not continuous and as Chaudhuri, Schotter and Sopher (2008) point out, even small (and insignificant) differences in the beliefs held by individual subjects can lead to large aggregate changes in behavior. It is also noteworthy that while both the public knowledge and common knowledge assignments led to more optimistic beliefs, it is only in the common knowledge treatment that the probability placed on someone choosing 1.1 is zero, while in the public knowledge treatment this probability is small but positive. It is possible that the confidence on the part of the subjects in the common knowledge treatment, that no one will choose 1.1, allows them to make higher choices in the later rounds of the game.

We turn to the random re-matching protocol next. Figure 8 shows the distribution of beliefs prior to and after an announcement in this treatment. It is clear that here the intervention that has the greatest positive impact on beliefs is the announcement of a bonus. Prior to the announcement, just about 1% of the subjects predict that the average choice in round 6 would be 1.7. After the public knowledge announcement the proportion of subjects who believe that the average choice in the next round will be 1.7 increases to 50% and the impact of the common knowledge announcement is very similar. However after the announcement of the bonus, the proportion of subjects who believe that the average choice in the next round will be 1.7 increases to 73%.

<Figure 8 here>

Using a non-parametric Wilcoxon ranksum test we can say that the distributions of beliefs in all three experimental treatments - public knowledge assignment, common knowledge assignment and performance bonus - are significantly different from those in the no assignment treatment. ($z = -8.338$, $p < 0.01$ for the public knowledge assignment and no assignment comparison; $z = -7.840$, $p < 0.01$ for the common knowledge assignment and no assignment comparison and $z = -8.605$, $p < 0.01$ for the performance bonus and no assignment comparison.)

Furthermore a Wilcoxon ranksum test shows that the beliefs in the bonus treatment are significantly different from those in the common knowledge assignment treatment ($z = -1.931$, $p = 0.05$) as well as those in the public knowledge assignment treatment ($z = -1.886$, $p = 0.06$). However the distribution of beliefs in the public knowledge assignment and common knowledge assignment treatments are not significantly different ($z = 0.053$, probability = 0.96). The announcement of a bonus clearly leads to more optimistic beliefs in the random re-matching protocol and this in turn translates into greater success in coordination later in the game.

4. Concluding Remarks

We look at the efficacy of two separate interventions in helping subjects attain the efficient equilibrium in a weak-link game. We find that when the groupings are fixed – and provide opportunities for signaling future moves – a credible assignment to the payoff-dominant outcome is sufficient to guarantee coordination as long as the assignment is "common knowledge"; that is, the assignment is both public (every subject receives the same message and knows that every other subject is receiving the same message) and also read aloud for all subjects to hear.

We find coordination harder to attain among groups that are randomly re-matched from one round to the next. The mechanism that succeeds in fostering coordination in this case is a performance bonus paid every time subjects manage to coordinate to the efficient equilibrium.

By and large, the interventions that facilitate successful coordination do so via the creation of more optimistic beliefs. It is only when a subject is convinced that no one else in the group will choose 1.1, that this subject chooses a higher number.

Our findings have implications for the performance of organizations involved in team-production. It appears that organizations where the groupings are essentially fixed over time – say, for instance, among the workers involved in ramp and gate operations at Continental Airlines – a public announcement that makes sure that everyone is getting the same message and every one knows that every one else is getting the same message is sufficient to lead to improved coordination. Such announcements are enough to create the optimistic beliefs necessary to lead to coordinated actions.

On the other hand, in situations where the groups are not fixed – such as in the case of the Internal Revenue Service hiring additional workers around the tax-filing deadline or at fast food outlets which typically experience high rates of labor turnover – an assignment is not adequate and one would need to resort to performance bonuses, likely to be a costlier option for the organization, to foster coordination.

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Table 1: Payoff Matrix used in this study

		Smallest Value of X Chosen						
		1.7	*1.6*	*1.5*	*1.4*	*1.3*	*1.2*	*1.1*
Your Choice of X	*1.7*	**0.85**	0.75	0.65	0.55	0.45	0.35	0.25
	1.6	---	**0.80**	0.70	0.60	0.50	0.40	0.30
	1.5	---	---	**0.75**	0.65	0.55	0.45	0.35
	1.4	---	---	---	**0.70**	0.60	0.50	0.40
	1.3	---	---	---	---	**0.65**	0.55	0.45
	1.2	---	---	---	---	---	**0.60**	0.50
	1.1	---	---	---	---	---	---	**0.55**

Table 2: Experimental Design

Matching Method	Treatment	No. of Subjects	No. of Groups	No. of plays of the stage game after receiving message	Total No. of Observations
RANDOM	**Public Knowledge Assignment**	**40**	**8**	**80**	**600**
RANDOM	**Common Knowledge Assignment**	**40**	**8**	**80**	**600**
RANDOM	**Common Knowledge Announcement of Bonus**	**40**	**8**	**80**	**600**
FIXED	**Public Knowledge Assignment**	**40**	**8**	**80**	**600**
FIXED	**Common Knowledge Assignment**	**35**[11] **(50)**	**7 (10)**	**70**	**525**
	TOTAL	**195 (210)**	**39 (42)**	**390**	**2925**

[11] As explained above we have 50 subjects in this treatment but 15 subjects (three groups) in one session played for 10 rounds in all rather than 15 and these 15 subjects and the observations relating to them have been excluded from all further data analysis.

Table 3: Wilcoxon ranksum tests for treatments in the fixed-grouping protocol

	Public Knowledge Assignment	**Common Knowledge Assignment**
No Assignment	-10.95 $p < 0.01$	-19.44 $p < 0.01$
Public Knowledge Assignment	---	-7.22 $p < 0.01$

Table 4: Random effects ordered probit model for effort choices in the fixed-grouping protocol

Effort choice	**Coefficient**	**z-statistic**	**P>|z|**
Round	-0.22	-3.75	0.00
Public Knowledge dummy	0.93	2.16	0.03
Common Knowledge dummy	1.66	2.75	0.01
Lag own effort ($e_{i,t-1}$)	8.76	11.34	0.00
Lag earning ($earn_{i,t-1}$)	-0.03	-0.04	0.97
Public Knowledge*Round	0.17	2.45	0.01
Common Knowledge*Round	0.18	2.16	0.03
($e_{i,t-1} - e^m_{j,t-1}$)	-6.38	-7.41	0.00
Public*($e_{i,t-1} - e^m_{j,t-1}$)	1.62	2.19	0.03
Common*($e_{i,t-1} - e^m_{j,t-1}$)	3.35	2.06	0.04
Cut1	8.14	8.01	0.00
Cut2	8.94	8.77	0.00
Cut3	9.51	9.25	0.00
Cut4	10.11	9.76	0.00
Cut5	10.90	10.42	0.00
Cut6	11.58	11.23	0.00
Rho	0.10	1.82	0.07
Likelihood Ratio $c^2(28)=424.23$	Prob> c^2=0.000	N=910	Log Likelihood= -676.89

Note: $e^m_{j,t-1}$ is the minimum effort chosen by the other four group members in the previous period where $j \neq i$

Table 5: Probability of effort choice in the fixed-grouping protocol

	1.1	1.2	1.3	1.4	1.5	1.6	1.7
No assignment	0.07	0.08	0.09	0.13	0.21	0.22	0.19
Public Knowledge Assignment	0.06	0.04	0.03	0.04	0.05	0.09	0.68
Common Knowledge Assignment	0.00	0.00	0.00	0.00	0.01	0.04	0.94

Note: Numbers may not add to 1 due to rounding up or down to 2 decimal places.

Table 6: Wilcoxon ranksum tests for treatments in the random-re-matching protocol

	Public Knowledge Assignment	Common Knowledge Assignment	Bonus
No Assignment	-5.31 $p < 0.01$	-7.53 $p < 0.01$	-19.11 $p < 0.01$
Public Knowledge Assignment	---	-0.98 $p < 0.33$	-10.74 $p < 0.01$
Common Knowledge Assignment	---	---	-10.88 $p< 0.01$

Table 7: Random effects ordered probit model for effort choices in the random-grouping protocol

Effort choice	Coefficient	z-statistic	P>|z|
Round	-0.09	-2.04	0.04
Public Knowledge dummy	0.46	1.50	0.13
Common Knowledge dummy	0.24	0.86	0.39
Bonus dummy	0.49	1.60	0.11
Lag own effort ($e_{i,t-1}$)	7.35	15.23	0.00
Lag earning ($earn_{i,t-1}$)	-0.20	-0.35	0.73
Public Knowledge*Round	0.02	0.44	0.66
Common Knowledge*Round	0.03	0.68	0.50
Bonus*Round	0.04	0.82	0.41
($e_{i,t-1} - e^{m}_{j,t-1}$)	-5.56	-10.08	0.00
Public*($e_{i,t-1} - e^{m}_{j,t-1}$)	0.92	2.12	0.03
Common*($e_{i,t-1} - e^{m}_{j,t-1}$)	1.85	4.09	0.00
Bonus*($e_{i,t-1} - e^{m}_{j,t-1}$)	2.89	4.77	0.00
Cut1	8.67	15.06	0.00
Cut2	9.06	15.67	0.00
Cut3	9.61	16.48	0.00
Cut4	10.30	17.48	0.00
Cut5	10.92	18.39	0.00
Cut6	11.69	19.60	0.00
Rho	0.03	0.65	0.53
Likelihood Ratio χ^2**(42)=819.38**	Prob> χ^2=0.000	N=1560	Log Likelihood= -1770.85

Note: $e^{m}_{j,t-1}$ is the minimum effort chosen by the other four group members in the previous period where $j \neq i$

Table 8: Probability of effort choice in the random-grouping protocol

	1.1	1.2	1.3	1.4	1.5	1.6	1.7
No Assignment	0.16	0.06	0.11	0.16	0.15	0.16	0.19
Public Assignment	0.19	0.05	0.07	0.09	0.09	0.14	0.39
Common Assignment	0.12	0.04	0.06	0.10	0.11	0.18	0.40
Bonus Announcement	0.00	0.00	0.01	0.03	0.05	0.13	0.78

Note: Numbers may not add to 1 due to rounding up or down to 2 decimal places.

Figure 1: Proportion of 1.1 choices and the average minimum choice over the first 5 rounds in the fixed and random re-matching protocols

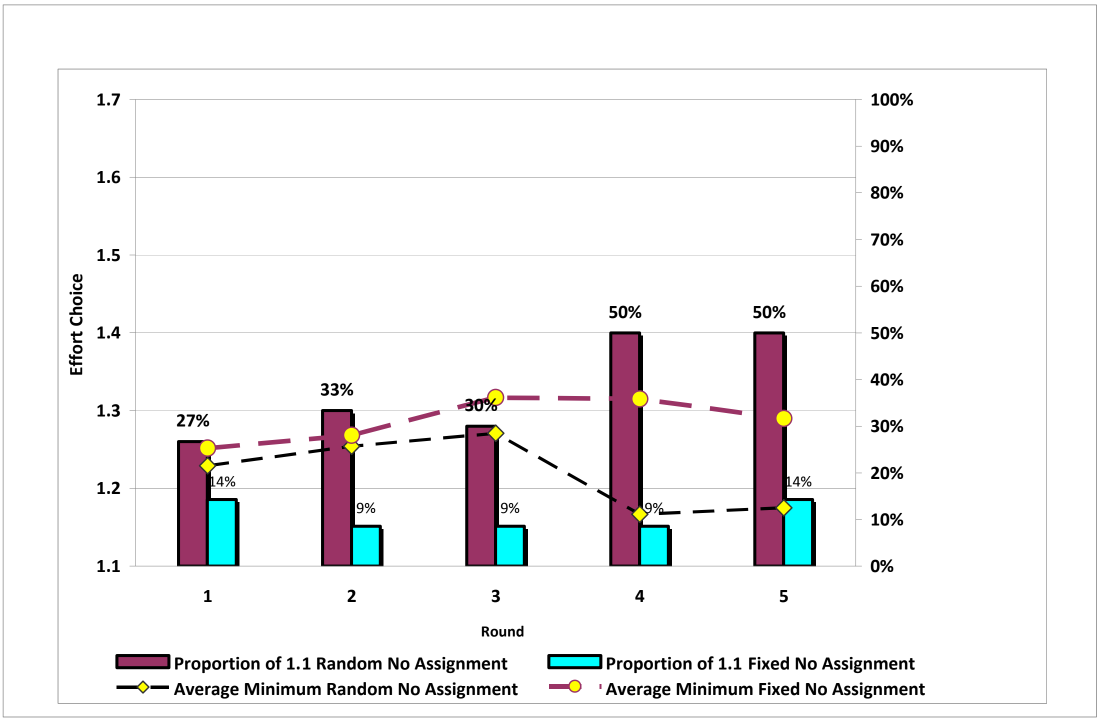


Figure 2: Average minimum effort across treatments in the fixed-grouping protocol

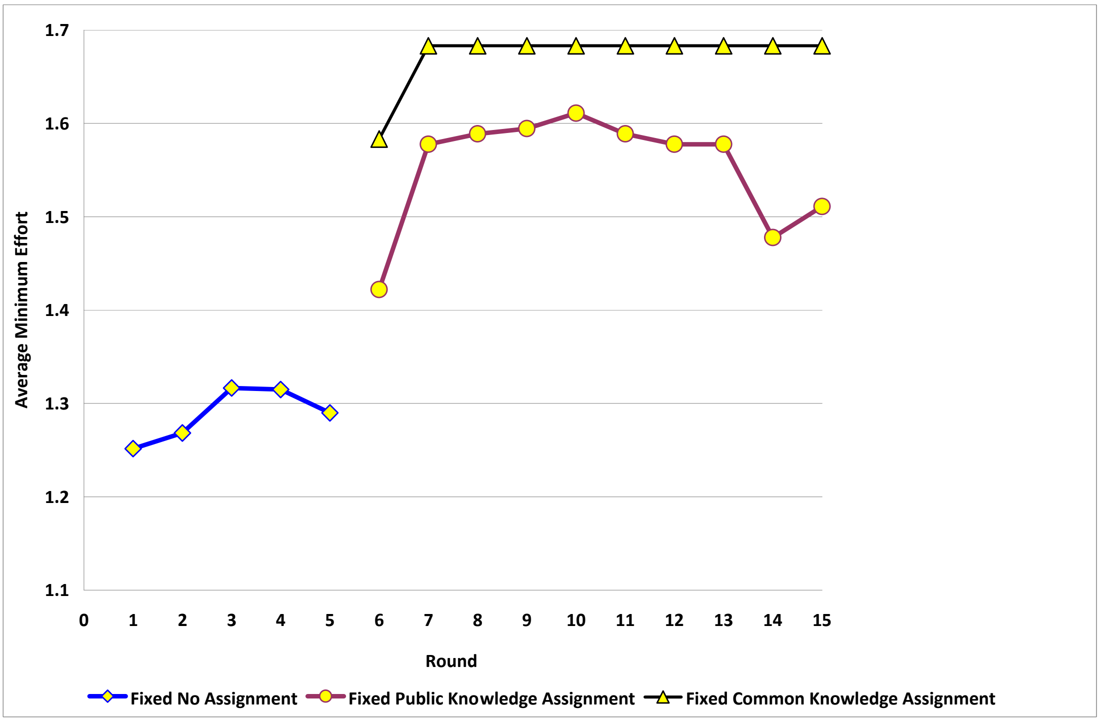
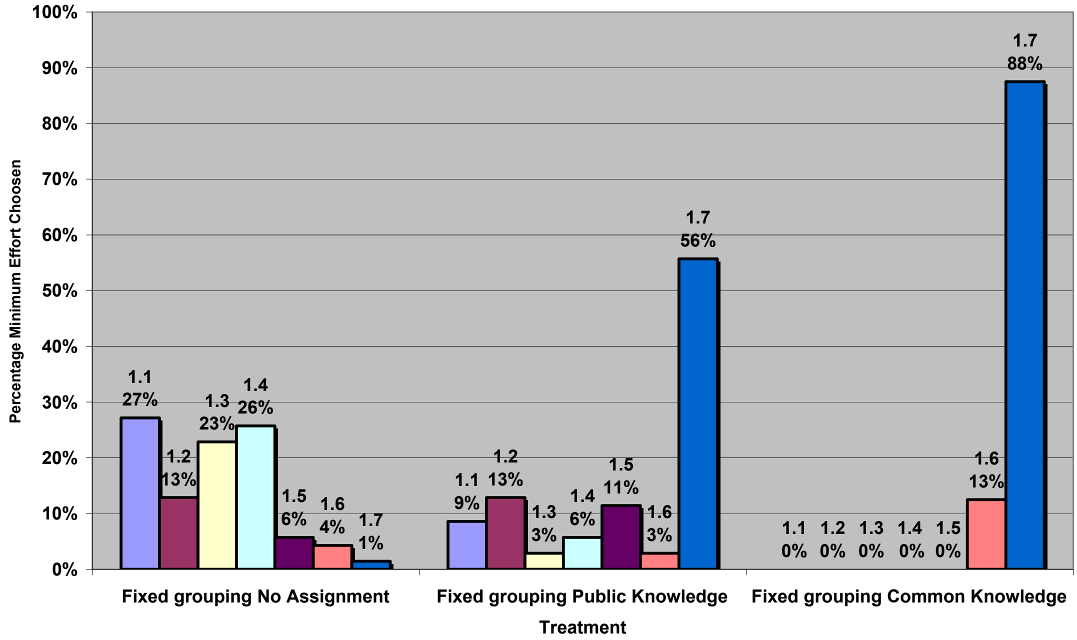


Figure 3: Distribution of minimum choices across treatments in the fixed-grouping protocol



Percentage numbers may not add to 100 due to rounding.

Figure 4: Average minimum effort across treatments in the random re-matching protocol

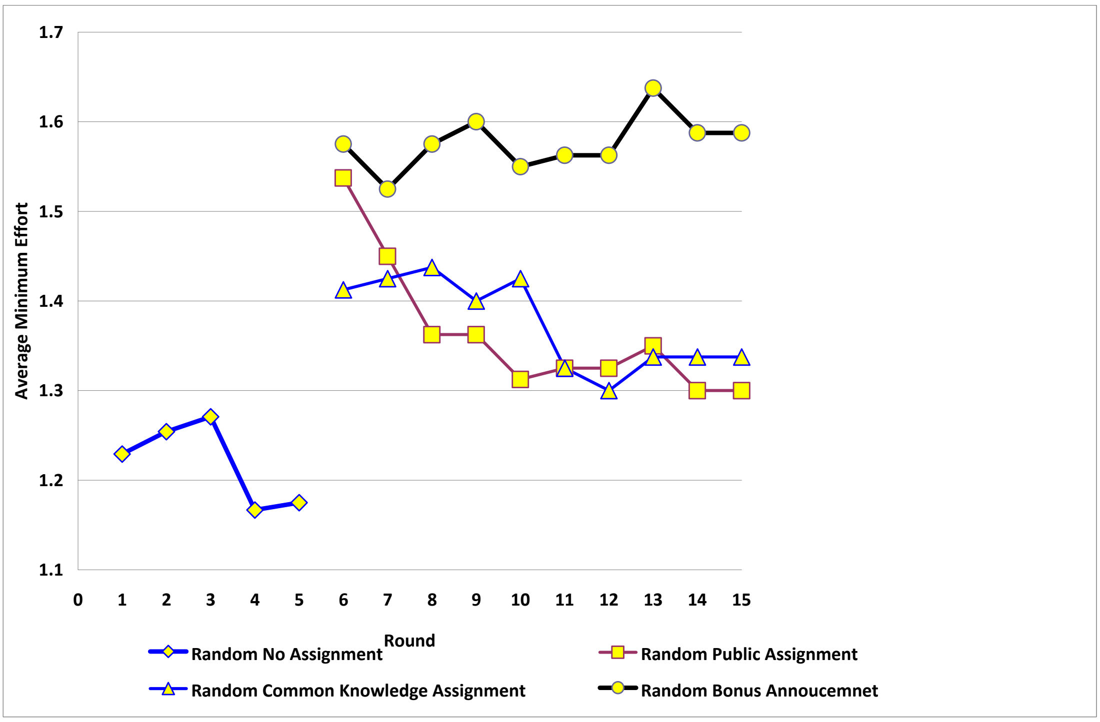
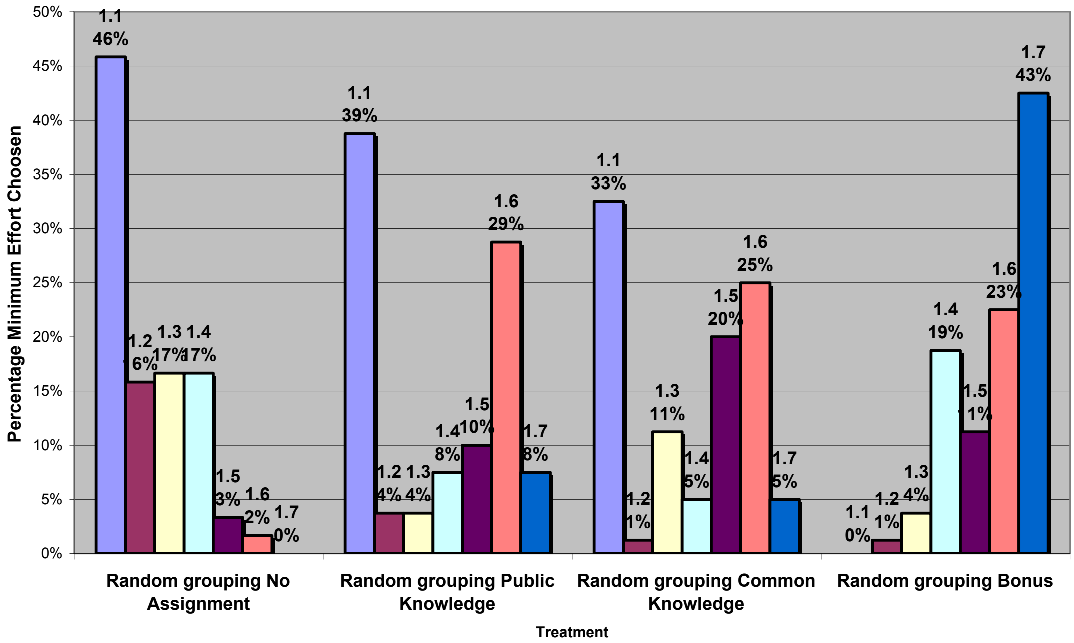


Figure 5: Distribution of minimum choices across treatments in the Random-grouping protocol



Percentage numbers may not add to 100 due to rounding.

Figure 6: Distribution of beliefs prior to an announcement in the random re-matching and fixed grouping protocols

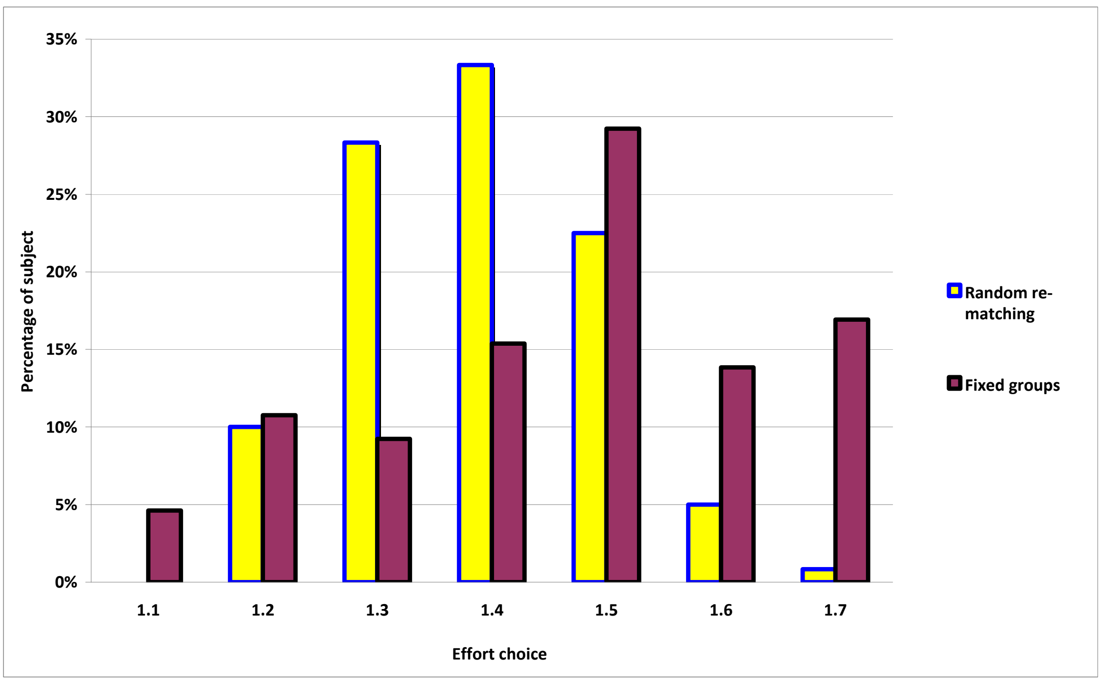
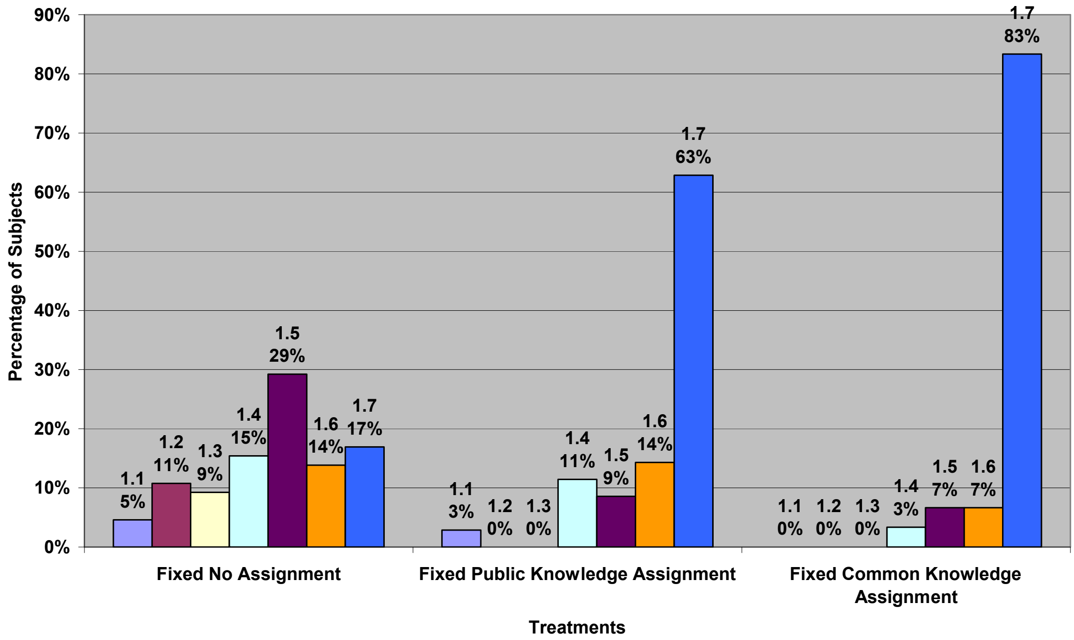
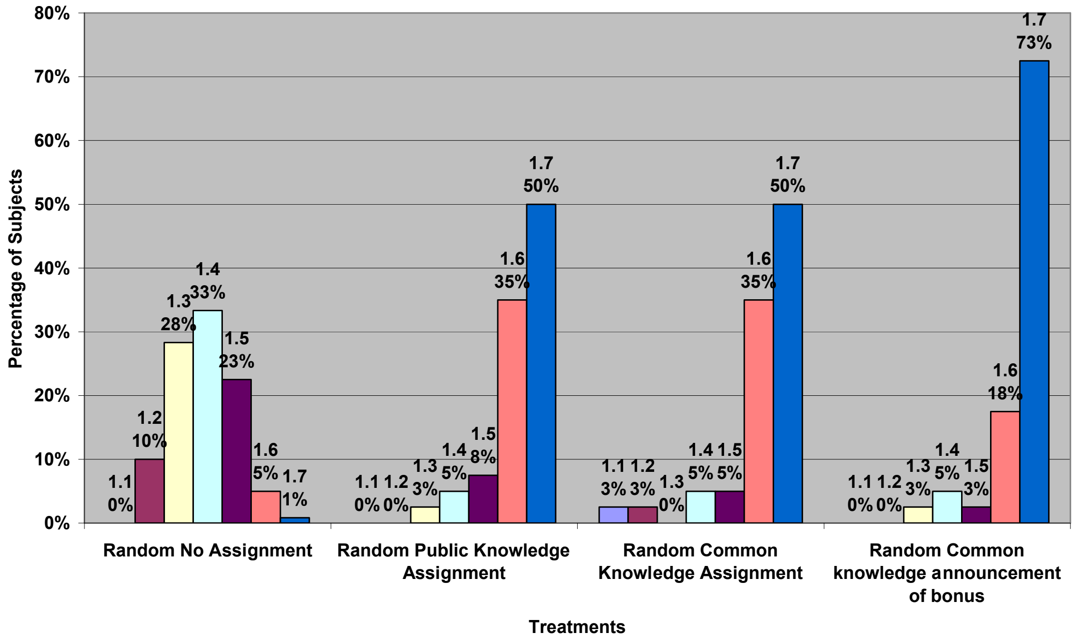


Figure 7: Distribution of beliefs prior to and immediately after an announcement in the fixed grouping protocol



Percentage numbers may not add to 100 due to rounding.

Figure 8: Distribution of beliefs prior to and immediately after an announcement in the random re-matching protocol



Percentage numbers may not add to 100 due to rounding.

Appendix

<u>General Instructions</u>

Welcome. The University of (…) has provided funding in order to conduct this research. The instructions are simple. If you follow them closely and make appropriate decisions, you may make an appreciable amount of money. All earnings are denoted in actual dollars. At the end of the session you will be paid your earnings in cash. This money is in addition to the show-up fee that you get.

In a minute we will give you the instructions for logging in to the server.

<u>Specific Instructions</u>

You will be in a market with 4 other people. In this experiment there will be a number of rounds. In each round you will be *randomly re-matched* so that you will not be playing with the same people for more than one round. ***[For the fixed matching protocol the previous sentence is replaced with:** The composition of the group will not change and you will be playing with the same group of players for all rounds.]* You will not know the identity of the people in your group in any round. In each round every participant will pick a value of X. The values of X you may choose are {1.1, 1.2, 1.3, 1.4, 1.5, 1.6, or 1.7} The value you pick for X and the smallest value picked for X by any participant, including your choice of X, will determine the payoff you receive.

The payoff table below tells you the potential payoffs you may receive. The earnings in each period may be found by looking across from the value you choose on the left hand side of the table and down from the smallest value chosen by any participant from the top of the table. For example, if you choose 1.4 and the smallest value chosen is 1.3 then you will earn 60 cents for that round. If you choose 1.5 and the smallest value chosen is 1.2 then you will earn 45 cents for that round.

		Smallest Value of X Chosen						
		1.7	**1.6**	**1.5**	**1.4**	**1.3**	**1.2**	**1.1**
Your Choice of X	**1.7**	0.85	0.75	0.65	0.55	0.45	0.35	0.25
	1.6	---	0.80	0.70	0.60	0.50	0.40	0.30
	1.5	---	---	0.75	0.65	0.55	0.45	0.35
	1.4	---	---	---	0.70	0.60	0.50	0.40
	1.3	---	---	---	---	0.65	0.55	0.45
	1.2	---	---	---	---	---	0.60	0.50
	1.1	---	---	---	---	---	---	0.55

The experiment will consist of 15 rounds. You will be able to read some of these instructions again once you have logged in. After you have finished reading the instructions you will proceed to play the first 5 rounds of this game. After the end of the 5^{th} round and before the beginning of the 6^{th} round the experimenter will provide you with a message about how to play the game for the last 10 rounds. Each of you will receive a sheet of paper containing this message. Each of you is looking at the exact same message as everybody else. *[**In the common knowledge treatment we add the following sentence:** In addition to providing you with this sheet of paper with the message on it, the experimenter will also read the message out loud]***.**

Please do NOT continue on to the 6th round of this game till asked by the experimenter to do so.

Before we go on to the 6th round and before you receive the message we will ask you to do the following. We will ask you to predict the **AVERAGE** choice of the people in the session for round 6. [***For the fixed matching protocol the previous sentence is replaced with:*** *We will ask you to predict the **AVERAGE** choice of the people in your group for round 6.*] When asked to do so please pick a number from the set {1.1, 1.2, 1.3, 1.4, 1.5, 1.6, 1.7} that you think will be the closest to the average choice and enter this number in the first column of Box 1 on the next page. (Please take a look now.) You will be paid for your predictions in the following way. You will earn $1 minus the **absolute difference** between your predicted average and the actual average.

EXAMPLE: Suppose you predict that in Round 6 the average choice will be 1.6. Suppose the actual average turns out to be 1.4. In this case the absolute difference between your predicted choice and the actual average is 0.2. Your earnings will then be $1.00 - $0.20 = $0.80. On the other hand suppose that you predict that in Round 6 the average choice will be 1.2. The actual average turns out to be 1.5. Then the absolute difference between your predicted choice and the actual choice is 0.3. In this case your earning will be $1.00- $0.30 = $0.70.

You will be asked to make this prediction once **before** you receive the message (using Box 1) and once **after** you receive the message (using Box 2). We will tell you the actual round 6 choices at the end of the session. The experimenter will help you to calculate your earnings from the two predictions.

We will proceed with round 6 of the game after this.

We will pay you your earnings from the experiment at the end of the session. You are free to go once you have been paid. Your earnings are private information and we encourage you to keep this information private. If at any point you have any questions or problems, please raise your hand for assistance.

The Assignment

You should pick 1.7 in each round.

NOTICE, from the payoff matrix, that if every participant in a group follows the message then every participant will earn $0.85. However, if even one of the participants does not follow the message and chooses a number different from 1.7, then each participant will make less money than if everyone chose 1.7.

The Bonus

If in a particular round all 5 players in your group choose 1.7 so that the minimum number chosen is 1.7, then in that round each player will earn an additional 50 cents on top of the 85 cents that you get for choosing 1.7. Hence for that round, each player will earn 1.35 dollars. This will be true for each and every round where the minimum is 1.7.

Prediction

BOX 1: PREDICTION BEFORE MESSAGE

Predicted Average	Actual Average	Absolute Difference	Earnings ($1 – Column 3)

BOX 2: PREDICTION AFTER MESSAGE

Predicted Average	Actual Average	Absolute Difference	Earnings ($1 – Column 3)

Record Sheet

Show-up Fee: **$5.00**

Earnings from Prediction 1: ___________

Earnings from Prediction 2: ___________

Earnings from Experiment: ___________

TOTAL ___________

Login Instructions

- Login to the computer (using your user name and password).

- Check that you are logged in to your Net Account.

- Open Internet Explorer.

- Enter the following web address and press enter:
http://veconlab.econ.virginia.edu/login.htm

- The "Veconlab Participant Login Screen" screen should be displayed.

- Click on 'Login'.

- The 'Veconlab: Enter Session Name' screen should be displayed.
Enter the Session Name: xauc538. Click on 'Submit'.

- The 'Veconlab Participant Login' screen should be displayed.
Fill in the boxes. Click on 'Continue'.

- The computer will assign you a Subject ID Number. Please write down your ID number and Password on the top of EACH page of your instructions in the space provided. It is important that you remember the password! This password will help us to go back and retrieve your data should something go wrong during the session.

- Please follow the instructions displayed on screen.