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Team versus individual behavior in the minimum effort coordination game

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ABSTRACT

We compare coordination success of individuals and teams in the minimum effort coordination game. The game is played by groups of either five individuals or five two-person teams with either fixed or random re-matching protocols. When groups are fixed, teams perform at least as well as individuals, if not better, in terms of coordinating to the payoff dominant outcome. But with random re-matching, teams experience pervasive coordination failures. A public recommendation to a strategy or a performance bonus exhorting players to coordinate to the payoff-dominant equilibrium has similar impact on coordination for both individuals and teams playing with fixed matching. However, coordination is far more difficult to achieve with teams playing under random re-matching. Our results have implications for the design of work-groups in organizations.

1. Introduction

Coordination problems are endemic to any organization engaged in team production. These situations are often characterized by strategic complementarities between worker efforts and take on a weak-link structure where the least performing worker exerts a large negative influence on overall productivity. Such games typically give rise to multiple payoff-ranked equilibria leading to questions about equilibrium selection. Successfully coordinating the actions of multiple agents is crucial to achieving optimal outcomes in such cases.

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, Shaw, and Prennushi (1997)

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describe how successful steel-mills adopt innovative human resource management practices that foster coordination along their production lines in an attempt to boost productivity. Field studies, like the ones noted above, are certainly instructive about how organizations can go about resolving coordination problems. But it is not always clear whether policies implemented in one organization will necessarily translate to others given that the successful resolution of such failures is tied up integrally with the corporate culture at particular organizations.

Consequently economists have often resorted to laboratory experiments to study the essence of the problem under the belief that generic lab environments enable us to hone in on crucial aspects of the coordination problem – where and why failures may happen – and the lessons learned will be applicable across a wide range of organizations. Cooper, De Jong, 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 2×2 and 3×3 coordination games. Van Huyck, Battalio, and Beil (1990, 1991) document similar failure to coordinate to the payoff dominant outcome in a set of more elaborate "order-statistic" games where the payoff to individuals depends on one's own choice and either the minimum effort or the median effort chosen by a group member, giving rise in each case to multiple payoff ranked equilibria.

However, we also know that a number of interventions such as communication among group members, advice from one generation of players to the next, public announcements recommending a particular strategy choice, exhortative messages from a manager to workers and performance bonuses for achieving improved coordination can all be effective in resolving coordination failures. See for instance Bangun, Chaudhuri, Prak, and Zhou (2006), Brandts and Cooper (2006, 2007), Brandts and MacLeod (1995), Chaudhuri, Schotter, and Sopher (2009), Chaudhuri and Paichayontvijit (2010), Cooper et al. (1992), Hamman, Rick, and Weber (2007) and Van Huyck, Gillette, and Battalio (1992) for representative publications in this area. Devetag and Ortmann (2007) and Chaudhuri (2009, chap. 5) provide an overview of this line of work.

However, in studying problems of coordination failure the extant literature has focused on decisions made by individuals who are typically interacting with other individuals in work-groups. But the reality is that in most organizations increasingly workers operate in teams and have to coordinate their actions with other teams. Such is the case, for instance, every time a plane takes off where almost all the tasks such as pre-flight checks or loading fuel or luggage or food-carts, etc. are carried out by teams working in tandem. Indeed the Knez and Simester (2001) study of Continental Airlines is focused primarily on solving such team level coordination problems.

This raises at least two relevant questions: (1) *does resolving coordination failures become even more difficult when teams of workers need to coordinate their actions*? (2) *Do the policy interventions that improve coordination among individuals have similar efficacious impact for teams*?

Ex ante, it is not clear whether teams would be more or less successful in solving coordination problems. On the one hand, given that teams involve more players coordination may become more difficult since typically the larger the group size, the more difficult it is to coordinate the actions of many agents. On the other hand, given that team members can talk to one another, even if they are not able to communicate directly with other teams, this may help coordinate actions given the role of communication in resolving coordination failures.

A pioneering study by Feri, Irlenbusch, and Sutter (2010) addresses this gap in the literature by comparing the coordination success of individuals and three-person teams. Each group consists of either five individuals or five three-person teams who interact for twenty rounds and the group composition remains unchanged for the entire time. Feri et al. look at behavior in six different games. Two of these are weak-link games along the lines of Van Huyck, Battalio, and Beil (1990). Here each group member chooses one number from the set {1, 2, 3, 4, 5, 6, 7}. The efficient outcome is for everyone to choose 7 while choosing 1 is the secure action. Three of the games are "average opinion" games taken from Van Huyck, Battalio, and Beil (1991). Once again each group member chooses one number from the set {1, 2, 3, 4, 5, 6, 7}. The efficient outcome is for everyone to choose 7 while the minimum payoff is maximized by choosing 3. Finally, game 6 is the "continental divide game" taken from Van Huyck, Cook, and Battalio (1997). Here each player chooses a number from the set {1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14} with two symmetric strict equilibria: everyone choosing 3 or 12.

Feri et al. report that teams are much better at coordinating to the payoff dominant outcome. Whether one looks at average effort choices or the appropriate order statistic (such as the minimum or the median effort choice depending on the specific game under consideration) teams in their study coordinate to a higher order statistic compared to individuals. This in turn also implies that teams on average earn more than individuals. Furthermore, not only do teams manage to coordinate to higher minimum/median effort choices, they are also better at coordinating their choices per se. The frequency of *mis-coordination*, in the sense of failing to coordinate choices to any of the available equilibria in the underlying stage-game, is much lower for teams compared to individuals.

The authors apply the *Experience Weighted Attraction* model of Camerer and Ho (1999) to study the dynamics of behavior and conclude that teams demonstrate "*a higher probability of playing more profitable strategies, leading ultimately to more efficient coordination when equilibria are Pareto ranked*" (Feri et al., 2010, p. 1904).

In this paper, we extend the Feri et al. study in two ways. First, in Feri et al. (2010) group composition – whether teams or individuals – is fixed. We compare behavior of teams and individuals under both fixed matching and random re-matching from one round to the next. It is quite likely that where teams are required to work in a coordinated manner, as in the Continental Airlines example discussed above, group composition tends to be fixed over extended periods. This is the matching protocol that Feri et al. study. However, as Chaudhuri and Paichayontvijit (2010) point out, one can think of situations involving short-lived interactions and frequent turn-over of personnel. Such may include (i) post-offices hiring additional temporary workers during Christmas; (ii) immigration authorities hiring temporary workers following a spike in visa applications and (iii) tax authorities hiring additional hands following the tax-return deadline. Fast food outlets also rely on coordinated action among workers and typically experience rapid turn-over of personnel. In these cases the resulting interaction is better modeled as a one-shot game via random re-matching of participants over time.

Second, looking at the data for the two weak-link games in the Feri et al. study, it is clear that while teams may have coordinated better than individuals, nevertheless team interactions are not devoid of coordination failure. If one looks at their WL-BASE game which is identical to the one studied by Van Huyck et al. (1990) (and the one we will focus on in this paper) then we find that roughly about 40% of team choices and 60% of individual choices are different from 7, the strategy commensurate with the pay-off dominant outcome. (Fig. 3, Panel A, p. 1900) So the second issue we look at is the impact of two types of interventions in fostering group coordination: (1) a "*public recommendation*", which is essentially a non-binding public message provided by an external arbiter instructing the players to adopt a particular strategy; and (2) a "*performance bonus*" for coordinating to the payoff dominant outcome.

Similar to Feri et al., we find that when groups are fixed two-person teams perform at least as well as individuals if not better. However, the differences between individual and team performance in our study is less dramatic than that in Feri et al. (2010) where the teams are much more successful at coordinating to the payoff-dominant outcome. Further we find that when teams are randomly re-matched from one round to the next, coordination failures are pervasive and such teams are worse at coordinating compared to individuals with random re-matching or teams with fixed matching.

We proceed as follows. In the next section we describe the design of our experiments. We present our results in Section 3 and make some concluding remarks in Section 4.

2. Experimental design and procedures

Five hundred and ninety-five participants took part in this study. Participants are first year undergraduate students in Business and Economics at the University of Auckland and have no prior experience in the minimum effort game. All experiments were conducted in DECIDE, the University of Auckland Business Decision Making Laboratory using the Veconlab website developed by Charles Holt (2009) at the University of Virginia.

We rely on a modified version of the Van Huyck et al. (1990) minimum effort coordination game in this study. This version of the game was introduced by Goeree and Holt (2001). In this version of the game participants are asked to pick an effort level from the following set {1.1, 1.2, 1.3, 1.4, 1.5, 1.6, 1.7}. Each group consists of five participants (either individuals or two-person teams). The payoff to participant i is determined by the effort level chosen by participant i and the smallest effort chosen by other group members. Specifically, $\pi_i = Min(e_i, e_{-i}) - 0.5(e_i)$ where e_i, is the effort chosen by participant i, while e_{-i} is the set of effort choices made by the other group members. Participants get to see the effort choices made by the other group members once all decisions have been entered. But all information is displayed using ID numbers only and so participants never learn the identity of other group members.

The payoff matrix generated by this function is presented in Table 1. There are seven Pareto-ranked Nash equilibria located on the diagonal. These are depicted in bold. All subjects choosing 1.1 is the least efficient outcome while all subjects choosing 1.7 is the efficient outcome. The payoffs shown in Table 1 are in actual NZ dollars and cents. Participants are paid NZ $5 show-up fee in addition to the earnings from the experiment. They are paid privately at the end of the experiment. On average, participants make NZ $16.95. Each session lasts around 45 min.

While the payoff matrix is different from that in Van Huyck et al. (1990) or Feri et al. (2010), nevertheless prior studies using this modified game document similar patterns of failure to coordinate to the payoff dominant outcome. See for instance Goeree and Holt (2001), Chaudhuri and Paichayontvijit (2010) and Bangun et al. (2006). In presenting our findings, we use the phrase "*coordination failure*" to refer to a failure to coordinate to the efficient outcome where each group member chooses 1.7. Under this nomenclature, coordination to other, inefficient, equilibria is still considered coordination failure. "*Mis-coordination*", on the other hand, will refer to failure to coordinate to any of the available equilibria.

All our experiments consist of interactions within groups of five members. In one set of experiments the five members of the groups are *individuals* while in a second set of experiments each group consists of five two-person *teams*. Groups interact for a total of 15 rounds. When all participants for a session have arrived, they are led into the lab and asked to sit down at any free computer of their choice. We then read them the instructions for either the individual or the team treatment as the case

Table 1
Payoff matrix in our study adopted from Goeree and Holt (2001).

Your choice of X	Smallest value of X chosen						
	1.7	1.6	1.5	1.4	1.3	1.2	1.1
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**

may be. The instructions are provided in Appendix A. Participants then log into their respective computers and proceed to play the first five rounds of the game described in Table 1 above. They are asked to stop at the conclusion of Round 5 and await further instructions. Prior to the start of Round 6 subjects are assigned to one of three different treatments.

In the *public recommendation* treatment, prior to the start of Round 6, each group member receives a piece of paper stating the following message. The instructions make it clear that every participant in the room is looking at the exact same message as everyone else. Participants then play another ten rounds with no further interruptions.

> *"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."*

In the *performance bonus* treatment, prior to Round 6, we hand out to each group member a piece of paper with the following message. Once again participants are told that everyone is looking at the exact same message. Participants then proceed to play another ten rounds after that.

> *"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."*

Behavior in these two experimental treatments is compared to a *control* treatment, where, at the beginning of Round 6, participants are told that there are no further instructions. Here play continues exactly as in the first five rounds for an additional ten rounds.

This generates a $3 \times 2 \times 2$ experimental design following the initial five rounds which are identical across all treatments. Three treatments: (i) *control*; (ii) *recommendation* and (iii) *bonus*; two types of group composition: (i) five *individuals* or five two-person *teams*; and two matching protocols: (i) *fixed* and (ii) *random*.

In the team sessions, when participants come into the lab, we ask them to sit down at a computer cubicle as usual. We then read them the instructions pertaining to the team treatment as described in Appendix A telling them that for this particular experiment they will be playing as a member of a two-person team. The pairing into teams is *ad hoc*. We simply remove the cubicle partition between pairs of participants. We tell them that the two people sitting next to each other now constitute the two-person team. Teams are still separated by cubicle partitions. We ask each team to log into a single computer and make all decisions using that computer alone. Each team-member is told that he/she will each separately earn the amount earned by the team.

We do not impose any restrictions on the discussions they can have. We also do not dictate how they should arrive at their decisions. We leave it up to the participants. We do not impose a time restriction but found that while people deliberated longer in the early rounds, after a while matters progressed expeditiously. We only request that team members speak softly so that other participants cannot hear the content of their discussions. We do not record or attempt to quantify team conversations in any way or form.

The team sessions proceed exactly like the individual sessions with one difference. In the individual sessions, both *before* and *after* we provide the message regarding the recommended strategy or the performance bonus we elicit beliefs of each participant regarding choices to be made by group members in Round 6. We did not elicit these beliefs for all the team sessions. For teams that play with fixed matching we run two types of sessions – ones where we elicit beliefs exactly as in the sessions with individuals and ones where we did not. We have a total of 24 groups of five two-person teams in the fixed matching protocol. Out of these we have the beliefs data for 10 groups but not for the remaining 14. More importantly we have exactly the same number of groups (8) in the two treatments – public recommendation and performance bonus – with four groups in each case where we have beliefs data and four groups where we do not. This allows us to undertake an analogous comparison of the behavior of teams and individuals with fixed matching but it also allows us to see whether eliciting beliefs makes a difference or not.

As we will show below, belief elicitation either makes no difference or leads to greater coordination failure. Therefore, not eliciting beliefs seems to actually help foster greater coordination. Given this, in running the sessions with teams playing under random re-matching we did not collect beliefs any more. The randomly re-matched teams perform quite badly to start with; eliciting beliefs would have likely made matters worse. The way to think about this is that not eliciting beliefs makes coordination success more likely; eliciting beliefs would lead to more pervasive coordination failures. Therefore the data for teams with random re-matching without belief elicitation likely overestimates the degree of coordination. Table 2A provides the details of the design with fixed matching while Table 2B does so for random re-matching.

2.1. Caveats

We need to note three important caveats before proceeding. First, this study was originally designed to complement the results reported in Chaudhuri and Paichayontvijit (2010) and as such we re-use some, but not all, of the data from that paper. Specifically, all the individual data for both fixed and randomly re-matched groups with the exception of the data for individuals in fixed groups paid a performance bonus comes from that earlier study and is identified by asterisks in Tables 2A and 2B.

Table 2A
Experimental design for the fixed matching protocol.

Matching method	Grouping method	Treatment	Abbreviations	No. of participants	No. of groups	Total no. of observations
FIXED	Individual	First five rounds	FI-Five	85	17	425[a]
FIXED	Individual	Control *With belief elicitation*	FI-Control	35	7	350[a]
FIXED	Individual	Recommendation *With belief elicitation*	FI-Rec	35	7	350[a]
FIXED	Individual	Bonus *With belief elicitation*	FI-Bonus	15	3	150
FIXED	Team	First five rounds	FT-Five	120	24	600
FIXED	Team	Control				
		Without belief elicitation	FT-Control-WOB	30	6	300
		With belief elicitation	FT-Control-WB	10	2	100
FIXED	Team	Recommendation				
		Without belief elicitation	FT-Rec-WOB	20	4	200
		With belief elicitation	FT-Rec-WB	20	4	200
FIXED	Team	Bonus				
		Without belief elicitation	FT-Bonus-WOB	20	4	200
		With belief elicitation	FT-Bonus-WB	20	4	

[a] Data from Chaudhuri and Paichayontvijit (2010).

Table 2B
Experimental design for the random re-matching protocol.

Matching method	Grouping method	Treatment	Abbreviations	No. of participants	No. of groups	Total no. of observations
RANDOM	Individual	First five rounds	RI-Five	120	24	600[a]
RANDOM	Individual	Control *With belief elicitation*	RI-Control-Ten	40	8	400[a]
RANDOM	Individual	Recommendation *With belief elicitation*	RI-Rec	40	8	400[a]
RANDOM	Individual	Bonus *With belief elicitation*	RI-Bonus	40	8	400[a]
RANDOM	Team	First five rounds	RT-Five	75	15	375
RANDOM	Team	Control *Without belief elicitation*	RT-Control	15	3	150
RANDOM	Team	Recommendation *Without belief elicitation*	RT-Rec	30	6	300
RANDOM	Team	Bonus *Without belief elicitation*	RT-Bonus	30	6	300

[a] Data from Chaudhuri and Paichayontvijit (2010).

This in turn leads to our second caveat and explains the discrepancy between the Feri et al. and our experimental design. Given that Feri et al. had their participants play for 20 rounds, ideally we should have done the same for the purposes of comparison. But by the time we became aware of the Feri et al. study we had already made considerable progress with a different design and collected all the individual data with this design. Given that we had 15 rounds of data for individuals, we were restricted to doing the same for teams.

Finally, we use two-person teams instead of three. This is a potential source of difference. Luhan, Kocher, and Sutter (2009) provide a discussion on differences in team size and how that may impact strategic play in games. Charness and Jackson (2007) show that the procedure for arriving at a joint group decision – whether based on unanimity or simple majority – makes a difference in terms of decisions arrived at. But Luhan et al. (2009, pp. 36–37) also point out that

> "*...there is little systematic variation of the size of teams in economic experiments and little evidence regarding its influence on team decisions where social preferences are important. Yet the scarce evidence does not support the conjecture that three-person teams act differently than two-person teams. Wildschut, Lodewijkx, and Insko (2001) do not report differences between two-person and three-person teams in a prisoner's dilemma game ... Our evidence ...also implies that the decision-making rule (unanimity versus majority rule, with the latter not being possible in two-person teams) does not cause significant differences.*"

For beauty-contest games, Sutter (2005), shows that larger teams (of four subjects) win the game more often than smaller teams (of two subjects) or individuals. It is possible that team discussion forces team members to approach a problem more systematically and this effect is more pronounced for larger teams. Davis and Harless (1996) provide a discussion of the advantage of group decision-making.

In Feri et al. teams achieve considerably greater coordination as opposed to individuals. The differences are not as pronounced in our study. It is possible that differences in either team size or decision making process or both explain some of this. In Feri et al., communication between team-members occur via computer chat and while each individual submits an effort choice separately, choices have to be unanimous in order to be implemented. When disagreement happened participants are asked to engage in another round of chat prior to submitting individual decisions. In almost all cases unanimity was attained by the second round. In contrast our discussions were face to face and we do not know what rules teams used to implement their decisions, particularly given that a majority rule is infeasible with two-member teams. It might well be the case that if one person wanted to choose 1.7 and the other 1.1 and agreement could not be reached, then they decided to alternate between these two choices which resulted in lower average choices for our teams while in Feri et al. the dynamics of the odd number of team members led to greater instances of unanimity. We proceed with our results with these caveats in mind.

3. Results

We organize our results into three sections. First, we examine whether teams and individuals differ systematically in their ability to coordinate to the payoff-dominant outcome in the absence of any intervention. This provides a clean test for the ability of the respective entities to coordinate to the payoff dominant equilibrium. Second, we look at the impact of a recommendation or a bonus on coordination success among teams and individuals with fixed matching. Finally, we explore the impact of the same interventions with random-re-matching. In what follows, for the ease of exposition we abuse language to refer to teams (individuals) playing with fixed matching as fixed teams or fixed individuals and randomly rematched teams (individuals) as random teams or random individuals respectively.

3.1. Comparing the behavior of teams and individuals in the first five rounds

We start by providing an overview of effort choices among teams and individuals with fixed or random matching prior to any intervention. To that end the four panels of Fig. 1 show the actual distribution of effort choices in the four different treatments and provide a quick preview of results reported below. As is clear from this figure, the majority of choices in fixed teams is 1.7 (Panel B) while for random teams it is 1.1 (Panel D). The choices for the individuals, whether with fixed or random re-matching, are more diffuse except comparing Panels A and C one could argue that fixed individuals on average choose higher numbers than random individuals.

Next we proceed to look at group minima (since that is the appropriate order statistic in this game) rather than the aggregate choices presented in Fig. 1. Fig. 2 shows the average minimum effort over time in the first five rounds. From Table 2A, we have data for 17 groups of five individuals and 24 groups of five two-person teams under fixed matching. Each group generates one observation for the minimum effort in each round. This implies that we have 17 observations in each round for fixed individuals for a total of 85 observations over five rounds. For the 24 groups of fixed teams we have 120 total observations. From Table 2B we have data for 24 groups of random individuals. This implies 24 observations per round for a total of 120 observations over five rounds. For random teams we have 15 groups in all for a total of 75 observations over five rounds. For each group of five teams or individuals we look at the minimum effort chosen in the group in a particular round and then take the average of these.

The solid lines represent individual choices while the broken ones do the same for teams. Lines with circles represent fixed matching while lines with diamonds are for random re-matching. As is clear from Fig. 1, fixed groups, whether composed of teams (FT-Five) or individuals (FI-Five), on average achieve significantly higher minima than either random teams (RT-Five) or individuals (RI-Five). Moreover, fixed teams on average achieve higher minima compared to fixed individuals as well as random teams.

In Fig. 3 we present a more disaggregated picture and show the distribution of the minimum chosen. In doing so, we aggregate over the first five rounds in the different configurations and so the results have no temporal context. Here, for each of the first five rounds we simply count how many times the group minimum ended up at 1.1, 1.2, 1.3, 1.4, 1.5, 1.6 or 1.7. It is clear that a majority of outcomes end up at a minimum of 1.1. However, fixed teams clearly perform better with 15% of minimum choices (18 out of 120) being 1.7 and almost 40% of choices (44 out of 120) being 1.4 or more. For fixed individuals the minimum is 1.7 in only 2% cases (2 out of 85) and the minimum choice is 1.4 or more in 27% of cases (23 out of 85). The difference in the rates of coordinating to the payoff dominant outcome of 1.7 (15% for teams as opposed to 2% for individuals) is significant using a sample proportions test ($z = 3.7$, $p < 0.01$).[1]

The degree of coordination failure is far more pervasive with random re-matching. Neither teams nor individuals ever manage to achieve a minimum of 1.7. In fact teams here never even managed to reach a minimum of 1.6. 71% of choices (53 out of 75) for random teams end up at the minimum of 1.1 and only 9% of choices (7 out of 75) are 1.4 or 1.5. Things are a little better for individuals where 1.1 is the minimum in 43% of cases (51 out of 120) and 25% of choices (27 out of 120) are 1.4, 1.5 or 1.6.

In Table 3 we present results of non-parametric Wilcoxon ranksum tests. The unshaded boxes provide results for Round 1 choices, which constitute independent observations. We also take the average of choices made by each subject over the first five rounds and treat these as independent observations for the purposes of non-parametric tests. This gives us the same

[1] $z = \frac{p_1 - p_2}{\sqrt{\frac{p_1(1-p_1)}{n_1} + \frac{p_2(1-p_2)}{n_2}}}$, $p_1 = 0.15$, $p_2 = 0.02$, $n_1 = 120$ and $n_2 = 85$.

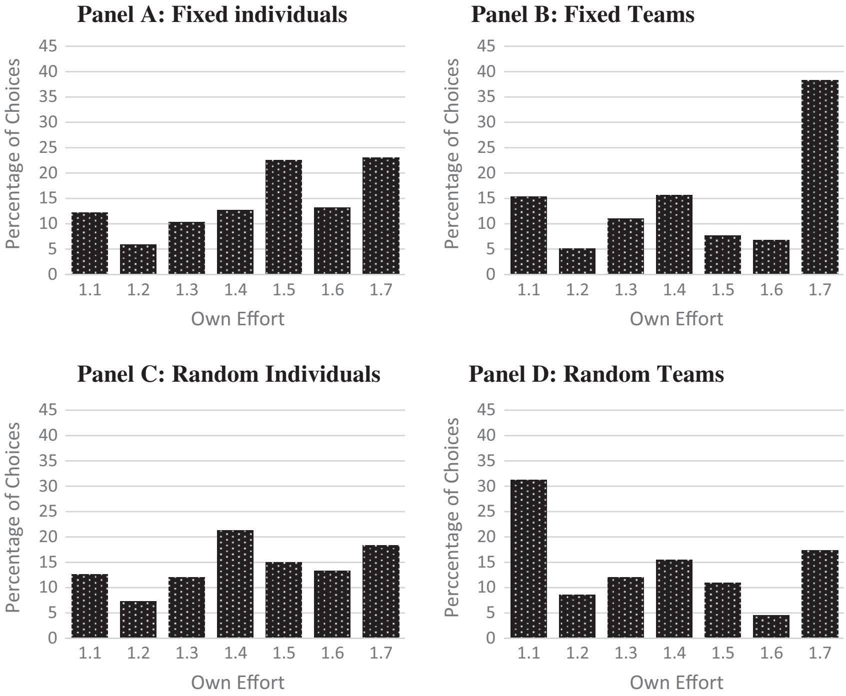

Fig. 1. Distribution of effort choices in the first five rounds.

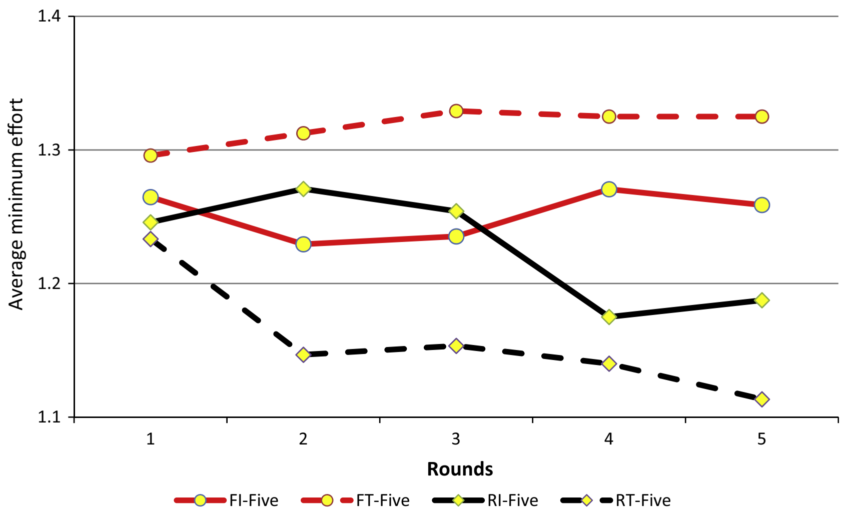

Fig. 2. Average minimum effort for teams and individuals in the control treatment.

number of observations in each case and these are noted in the table. The results for subject averages are provided in the shaded boxes in Table 3. The non-parametric results suggest no significant difference between fixed teams and fixed individuals either in the first round or in subject averages. However, random individuals and teams do worse than fixed teams both in Round 1 and in terms of the 5 round subject averages.

In Table 4, we present results of random effects ordered probit regressions with the standard errors clustered at the level of the individual. The dependent variable is an individual's (or a team's) effort choice in each round. We present results from two different models. In the first model the independent variables include round and three treatment dummies for fixed teams, random individuals and random teams respectively with the fixed individuals being the reference category and lagged earnings, which is what a particular subject earned in the previous round. In the second model we introduce three additional terms involving the treatment dummies interacted with round.

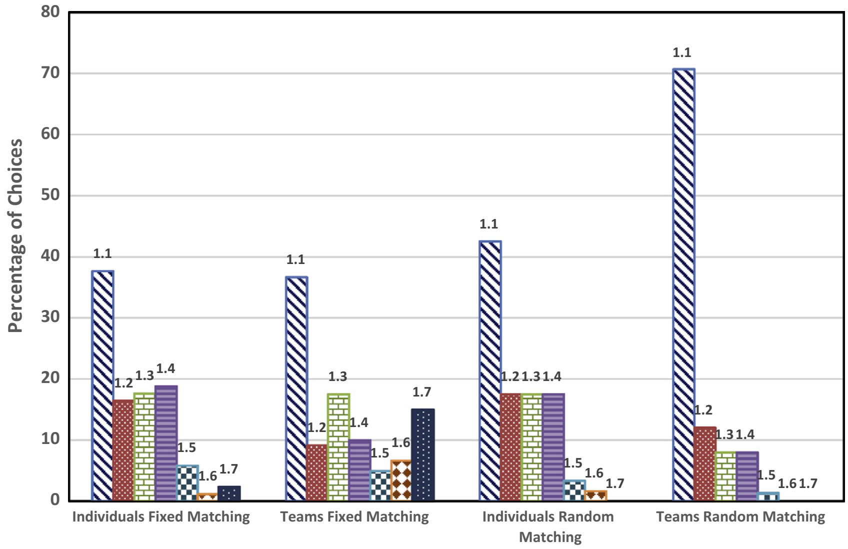

Fig. 3. Distribution of minimum effort choices among teams and individuals in the first five rounds.

Table 3
Ranksum test for effort choices in Round 1 and average by subject over the first five rounds.

	Fixed team ($n = 120$)		Random individual ($n = 120$)		Random team ($n = 75$)	
	Round 1	5 Round subject average	Round 1	5 Round subject average	Round 1	5 Round subject average
Fixed individual ($n = 85$)	$\|z\| = 1.62$ $p = 0.11$	$\|z\| = 0.49$ $p = 0.63$	$\|z\| = 1.53$ $p = 0.13$	$\|z\| = 1.66$ $p = 0.1$	$\|z\| = 0.43$ $p = 0.67$	$\|z\| = 4.77$ $p = 0.00$
Fixed team ($n = 120$)			$\|z\| = 3.05$ $p = 0.00$	$\|z\| = 1.78$ $p = 0.08$	$\|z\| = 1.71$ $p = 0.09$	$\|z\| = 4.51$ $p = 0.00$
Random individual ($n = 120$)					$\|z\| = 0.75$ $p = 0.45$	$\|z\| = 3.99$ $p = 0.00$

Table 4
Random effects ordered probit regressions for the first five rounds.

Own effort	Model 1		Model 2	
	Coefficient	Standard error	Coefficient	Standard error
Fixed team	0.035	0.194	0.554	0.364
Random individual	−0.192	0.165	−0.191	0.324
Random team	−0.914***	0.202	0.03	0.423
Round	−0.340***	0.03	−0.254***	0.068
Fixed team * round			−0.149*	0.09
Random individual * round			−0.001	0.081
Random team * round			−0.275**	0.108
Lagged earnings	2.549***	0.277	2.636***	0.279
Cut 1	−1.544***	0.218	−1.213***	0.289
Cut 2	−1.167***	0.218	−0.831***	0.287
Cut 3	−0.608***	0.22	−0.266	0.289
Cut 4	0.063	0.218	0.41	0.286
Cut 5	0.685***	0.221	1.038	0.287
Cut 6	1.182***	0.229	1.538	0.293
Number of observations	1600		1600	
Log pseudolikelihood	−2656.97		−2648.08	
Wald χ^2	220.67		229.18	
Probability $>\chi^2$	0.00		0.00	

*** Significance at 1%.
** Significance at 5%.
* Significance at 10%.

Not surprisingly, both models show that effort choices decrease over time. Model 1 suggests that on average compared to fixed individuals, random teams make lower effort choices (significant at 1%). In fact Wald tests suggest that the choices made by random teams are significantly below both fixed teams and random individuals. However, once we control for lagged earnings in Model 2 the coefficient for the random teams dummy is no longer significant. But Model 2 suggests that the effort choices by random teams decline faster than fixed individuals. There is some evidence that the same is true of fixed teams but the coefficient of the fixed team∗round variable is only marginally significant. Furthermore Wald test suggests that the effort choices made by random teams decline much faster than random individuals.

The above evidence suggests that in the absence of any interventions fixed teams do at least as well as fixed individuals if not better while random teams clearly perform worst amongst the four separate entities. But clearly the differences in team and individual performance in our study are not as pronounced as that in Feri et al. which may well have to do with the caveats noted above in Section 2.1.

However we do replicate the Feri et al. (2010) finding that teams experience less *mis-coordination* in the sense that teams are better at coordinating to one of the available equilibria even if not the payoff dominant one. In order to do this, within each group *j*, for each individual *i* and round *t* we calculate the difference between individual *i*'s effort choice and the minimum choice in group *j* for that round. We then sum those differences and divide by 5 (the number of group members) to arrive at the average deviation for group *j* in round *t*, which is defined as

$$av\_dev_{jt} = \{sum(effort_{ijt} - min\_effort_{jt})\}/5$$

$av\_dev_{jt} = 0$ implies symmetric Nash play, where all members of group *j* choose the same effort in round *t*. This in turn implies that closer is av_dev_{jt} to zero, the greater is the degree of coordination. Of course, the point to bear in mind here is that this says nothing about coordinating to the payoff dominant outcome of 1.7. Here we are simply asking whether, along the lines of Feri et al., groups of teams choose effort levels that are close to one another as opposed to groups of individuals.

We treat the average deviation within each group *j* in round *t* as an independent observation and use Wilcoxon ranksum tests to see if these are drawn from the same distribution. Since each group generates one observation per round, in each of the first five rounds, we have 17 observations for fixed individuals, 24 for fixed teams, 24 for random individuals and 15 for random teams. We present our results in Table 5, where we compare across the four different entities overall for the first five rounds and then separately for each of the first five rounds. If we look at the first column which aggregates the data for all five rounds (i.e., 85 observations for fixed individuals, 120 for fixed teams, 120 for random individuals and 75 for random teams), then it is clear that fixed teams exhibit smaller degree of *mis-coordination* than fixed individuals. This difference increases over time and is particularly noticeable by Round 5 where the average deviation for fixed teams is close to zero. While the difference between random teams and individuals is not pronounced in early rounds, by Round 5 random teams exhibit significantly lower deviation compared to random individuals. The differences between fixed and random individuals in terms of dispersion of choices are not very pronounced. Finally – from the bottom row of Table 5 – it is clear that fixed teams exhibit much smaller degree of *mis-coordination* compared to random teams.

However, the above results need to be accompanied by a caveat that, as clearly highlighted in Fig. 1, while both fixed and random teams seem better at avoiding *mis-coordination*, for fixed teams the coordination usually happens at higher effort choices compared to random teams.

3.2. Impact of interventions on teams and individuals with fixed matching

In this section we compare the impact of our interventions on teams and individuals within fixed groups. As discussed before, subjects first played five rounds of the game without any interventions followed by ten rounds with a recommenda-

Table 5
Average deviation from minimum within each group over five rounds by treatment.

	First 5 rounds	Round 1	Round 2	Round 3	Round 4	Round 5
Averages						
FI ($n = 17$ in each round)	0.207 ($n = 85$)	0.247	0.262	0.221	0.163	0.144
FT ($n = 24$ in each round)	0.152 ($n = 120$)	0.246	0.193	0.148	0.114	0.057
RI ($n = 24$ in each round)	0.205 ($n = 120$)	0.230	0.208	0.187	0.229	0.173
RT ($n = 15$ in each round)	0.192 ($n = 75$)	0.255	0.275	0.172	0.143	0.116
Wilcoxon ranksum test results						
FI = FT	$\|z\| = 3.439$	$\|z\| = 0.173$	$\|z\| = 1.960$	$\|z\| = 2.217$	$\|z\| = 1.595$	$\|z\| = 2.892$
	$p = 0.00$	$p = 0.86$	$p = 0.05$	$p = 0.03$	$p = 0.11$	$p = 0.00$
RI = RT	$\|z\| = 0.922$	$\|z\| = 0.824$	$\|z\| = 2.124$	$\|z\| = 0.347$	$\|z\| = 2.875$	$\|z\| = 1.820$
	$p = 0.36$	$p = 0.41$	$p = 0.03$	$p = 0.73$	$p = 0.00$	$p = 0.07$
FI = RI	$\|z\| = 0.055$	$\|z\| = 0.358$	$\|z\| = 1.366$	$\|z\| = 1.034$	$\|z\| = 2.040$	$\|z\| = 0.503$
	$p = 0.9$	$p = 0.72$	$p = 0.17$	$p = 0.30$	$p = 0.04$	$p = 0.61$
FT = RT	$\|z\| = 2.319$	$\|z\| = 0.319$	$\|z\| = 2.152$	$\|z\| = 1.143$	$\|z\| = 0.971$	$\|z\| = 1.832$
	$p = 0.0$	$p = 0.75$	$p = 0.03$	$p = 0.25$	$p = 0.33$	$p = 0.07$

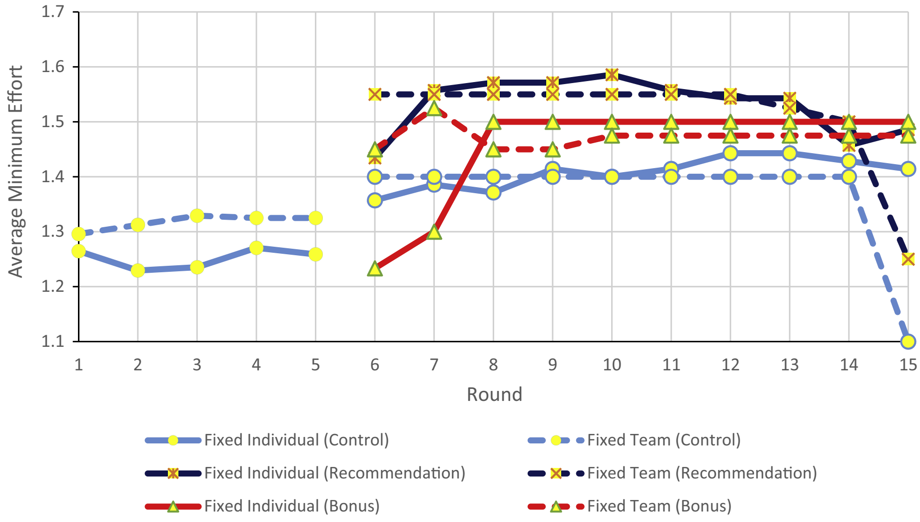

Fig. 4. Average minimum effort for teams and individuals with fixed matching in different treatments.

tion, a performance bonus or no intervention (the control treatment). One other issue here is that we have two sets of data for the fixed teams – ones with belief elicitation and ones without. In the interest of consistency we will start by comparing the data for teams and individuals with belief elicitation.

Fig. 4 shows the impact of the different interventions. The data for individuals is represented by solid lines while broken lines represent teams. The vertical axis shows the average minimum chosen in any particular round. We have limited data for the teams in the control treatment. But given that our focus is on the interventions, we felt that this should not be a concern because in the experimental treatments of interest we have the exactly same number of observations. The data for the first five rounds are the same as that presented in Fig. 2 and are reproduced here purely for the purposes of comparison to show what happens in the aftermath of an intervention. The results suggest that the impact on team and individuals are roughly similar with both announcements and bonus leading to an increase in the minimum effort chosen. While the average minimum is around 1.3 in the first five rounds, this jumps up to around 1.5 following an intervention. There is a pronounced end-game effect for the teams but it needs to be borne in mind that at least in the case of the control treatment we have data for two groups only.

In Fig. 5 we provide a break-down of the minimum effort level chosen in the different treatments aggregated over all rounds following our intervention. Here we are simply looking at the number of times groups chose 1.1, 1.2, 1.3, 1.4, 1.5,

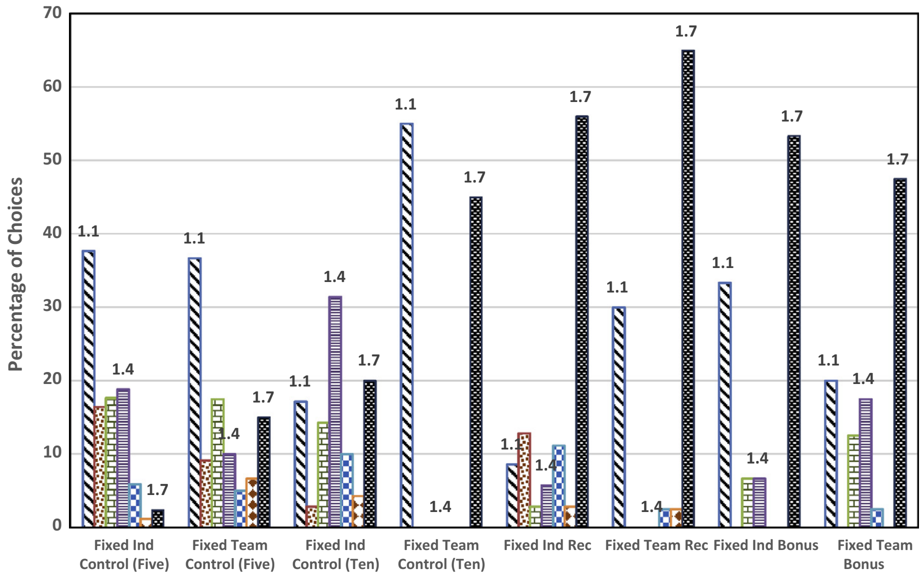

Fig. 5. Distribution of minimum effort choices among teams and individuals with fixed matching.

Table 6
Random effects ordered probit regressions for teams and individuals with fixed matching across different treatments.

Own effort	Model 1		Model 2	
	Coefficient	Standard error	Coefficient	Standard error
Fixed team control	−1.020	1.279	−0.692	1.919
Fixed individual recommendation	1.992***	0.543	2.755***	0.818
Fixed team recommendation	1.670**	0.673	5.512***	2.082
Fixed individual bonus	2.520***	0.651	4.242***	1.142
Fixed team bonus	0.282	0.562	1.364*	0.734
Round	−0.213***	0.028	−0.126***	0.038
Fixed team control ∗ round			−0.073	0.163
Fixed individual recommendation ∗ round			−0.094	0.079
Fixed team recommendation ∗ round			−0.519**	0.232
Fixed individual bonus ∗ round			−0.288**	0.130
Fixed team bonus ∗ round			−0.169**	0.084
Lagged earnings	4.713***	0.718	4.995***	0.713
Cut 1	0.000	0.554	0.591	0.626
Cut 2	0.263	0.549	0.879	0.622
Cut 3	0.625	0.562	1.268**	0.636
Cut 4	1.600***	0.576	2.288***	0.649
Cut 5	2.088***	0.578	2.790***	0.654
Cut 6	2.475***	0.599	3.196***	0.679
Number of observations	1215		1215	
Log pseudolikelihood	−987.53		−963.47	
Wald χ^2	117.29		109.59	
Probability $>\chi^2$	0.00		0.00	

*** Significance at 1%.
** Significance at 5%.
* Significance at 10%.

1.6 or 1.7 as the minimum effort choice in a particular round. A few things stand out. First, looking at the first four sets of histograms it is clear that in the control treatment, both in the first five rounds and later, the majority of minimum choices correspond to 1.1. Second, both the public recommendation and the bonus manage to break the pattern with a clear majority of minimum choices clustered at 1.7. Both interventions seem to have affected choices to the same extent. The proportion of 1.7 as the minimum is considerably higher for teams than individuals following a recommendation (65% versus 55% respectively). The proportions are roughly similar following a bonus (52% for individuals versus 48% for teams).

In Table 6 we rely on random effects ordered probit regressions to examine the impact of our treatments on choices. As in Table 4, the dependent variable is an individual's (or a team's) effort choice in a particular round. We present two different specifications. In Model 1, the independent variables include five treatment dummies – fixed team control, fixed individual recommendation, fixed team recommendation, fixed individual bonus and fixed team bonus – with the fixed individual control treatment serving as the reference category; round and lagged earnings. Model 2 also includes five interaction terms involving the treatment dummies interacted with round.

The ordered probit regressions are in line with what is apparent from Figs. 4 and 5. There are no significant differences between individuals and teams in the control treatment. Model 1 suggests that both individuals and teams coordinate to higher effort choices following an intervention. The effects are similar for a recommendation but teams do not perform as well as individuals following a bonus. In fact a Wald test suggest that individuals chose higher numbers following a bonus compared to fixed teams in Model 1 and the difference is significant at 1%. However in Model 2 the coefficient of the fixed team bonus dummy is marginally significant suggesting that the bonus raised team effort choices compared to the control treatment.

Therefore, the main insight coming out of Figs. 4 and 5 and Table 6 is that teams and individuals perform about the same following a recommendation and a bonus. But, it must be borne in mind that there is a clear upper bound to the choices at 1.7 and so the choices made by teams and individuals can improve only to an extent. So based on the results in this section and those reported in Section 3.1 above the conclusion is that in the context of such coordination problems fixed teams will do at least as well as individuals if not better. The distribution of beliefs prior to and following an intervention is similar between teams and individuals. This is not surprising given the similarities in effort choices between teams and individuals following an intervention. Therefore we dispense with a detailed description of these beliefs since it is not clear that they add much to the current discussion.

3.2.1. Role of belief elicitation

As noted above for the teams in fixed matching we have two sets of data, one where we elicit beliefs and another where we did not. In Fig. 6, we show the difference made by belief elicitation. Since the first five rounds are identical across all treatments, we can merge this data so that the single line represents the first five rounds of interaction. The differences are not

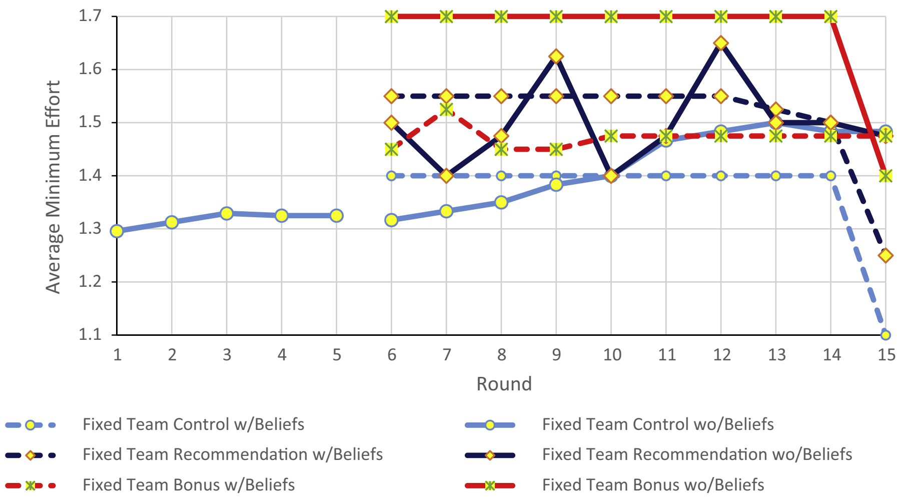

Fig. 6. Average minimum effort for fixed teams with and without belief elicitation.

pronounced in the case of the recommendation except that the average minimum jumps around for teams without belief elicitation. (See the point about team members alternating their choices in Section 2.1 above.) Belief elicitation had a significant impact in the case of the bonus. Following a bonus teams without belief elicitation managed to coordinate to the payoff dominant outcome in 9 out of 10 rounds. Teams where we elicit beliefs perform less well. Also as in Fig. 4, there are pronounced end-game effects with a sharp drop in choices in Round 15. Fig. 6 then suggests that coordination becomes more difficult following belief elicitation than without.

Croson (2000) looks at the impact of belief elicitation in a public goods experiment. She reports that when players were asked about their beliefs regarding the contribution levels of group members, this resulted in greater strategic behavior and lower contributions. Rabin (1993) has argued that in the presence of reciprocal preferences one can think of the public goods game as a coordination problem that allows at least two equilibria, a payoff dominant outcome where everyone contributes and a secure outcome where everyone free-rides. Croson's results then suggest that belief elicitation leads to greater convergence towards the secure outcomes. Our results corroborate this finding that asking people to think about what their group members may do leads them to choose the payoff dominant strategy of 1.7 less often.

We provide corroboration for this finding via results of random effects ordered probit regression presented in Table 7. Here we are looking at effort choices by teams in each round with or without belief elicitation. The reference category is fixed team control without belief elicitation. We include five dummies – fixed team control with beliefs, fixed team

Table 7
Random effects ordered probit regressions for team choices with or without belief elicitation.

Own effort	Coefficient	Standard error	z	$p > \|z\|$
Fixed team control *with* beliefs	−1.07	1.388	−0.77	0.44
Fixed team recommendation *without* beliefs	2.194***	0.751	2.92	0.00
Fixed team recommendation *with* beliefs	1.47*	0.79	1.86	0.06
Fixed team bonus *without* beliefs	2.164**	1.1	1.96	0.05
Fixed team bonus *with* beliefs	0.258	0.72	0.36	0.72
Round	−0.136***	0.04	−3.40	0.00
Lagged earnings	3.313***	0.734	4.51	0.00
Cut 1	0.046	0.674	0.07	0.95
Cut 2	0.112	0.678	0.17	0.87
Cut 3	0.189	0.68	0.28	0.78
Cut 4	0.711	0.684	1.04	0.3
Cut 5	0.979	0.688	1.42	0.16
Cut 6	1.308*	0.688	1.9	0.06
Number of observations	1080			
Log pseudolikelihood	−581.995			
Wald χ^2	52.45			
Probability $>\chi^2$	0.00			

*** Significance at 1%.
** Significance at 5%.
* Significance at 10%.

recommendation with and without beliefs and fixed team bonus with and without beliefs; round and lagged earnings. In the interests of parsimony and unlike in Tables 4 and 6 we do not present a second model with treatment dummy and round interactions because the results are roughly similar and do no shed any further light on the main insight.

Once again the regression results bear out what is apparent from Fig. 6. Belief elicitation does not make any difference in the case of the control and recommendation treatments. Average choices are higher following a recommendation but Wald test suggests no significant difference with or without belief elicitation. However, teams fare better following a bonus in the absence of belief elicitation. This is borne out by a Wald test for equality of coefficients with $\chi^2 = 4.94$, $p = 0.03$. This suggests that belief elicitation either does not make a difference or leads to lower effort choices.

3.3. Impact of interventions on teams and individuals with random re-matching

In this section we compare the behavior of random individuals and teams with the caveat that for random individuals we do elicit belief data prior to and following an intervention while we do not do so for random teams but as we argue in Section 3.2.1 above, the fact that we do not elicit beliefs for random teams most likely does not make a difference or overestimates the choices made by them.

Fig. 7 shows the average minimum choices before and after the intervention. This figure shows quite clearly that randomly re-matched teams experience significant coordination failures, which are far in excess of what happens with individuals. Individuals on average choose higher minimum effort levels during the first five rounds of the control treatment. For the next ten rounds in the control treatment there is little to choose among the two with the average minimum hovering around 1.1. Both for teams and individuals, a recommendation has an initial impact in raising the average minimum but fails to sustain it at a higher order statistic with the average minimum dropping to about 1.2 by the end of Round 15. The intervention that makes a difference is the payment of a bonus but even here teams do worse than individuals with a sharp drop-off in the average minimum towards the end of the session. For teams, the minimum effort in the bonus treatment is around 1.5 for much of the time before dropping to 1.2 at the end. For the individuals the bonus is successful in stabilizing the minimum around 1.6 for the entire duration.

Fig. 8 shows the distribution of the minimum choice aggregated across all rounds. Once again the picture that emerges is one of pervasive coordination failures both for teams and individuals, with teams performing worse than individuals across the board. For instance, in the control treatment 66% of plays among individuals and all 100% of plays among teams ending in a choice of 1.1. The performance bonus is successful in facilitating coordination particularly among individuals, where the proportion of 1.1 choices drops to zero while that of 1.7 choices increases to 43%. With teams following a bonus, 58% of plays end at a minimum of 1.7 but at the same time a significant proportion of plays (40%) still end up at a minimum effort of 1.1.

In Table 8 we present random effects ordered probit regression results for effort choices by individuals or teams in each round. The approach is the same as that in Table 6 where we presented results for the fixed matching protocol. We present

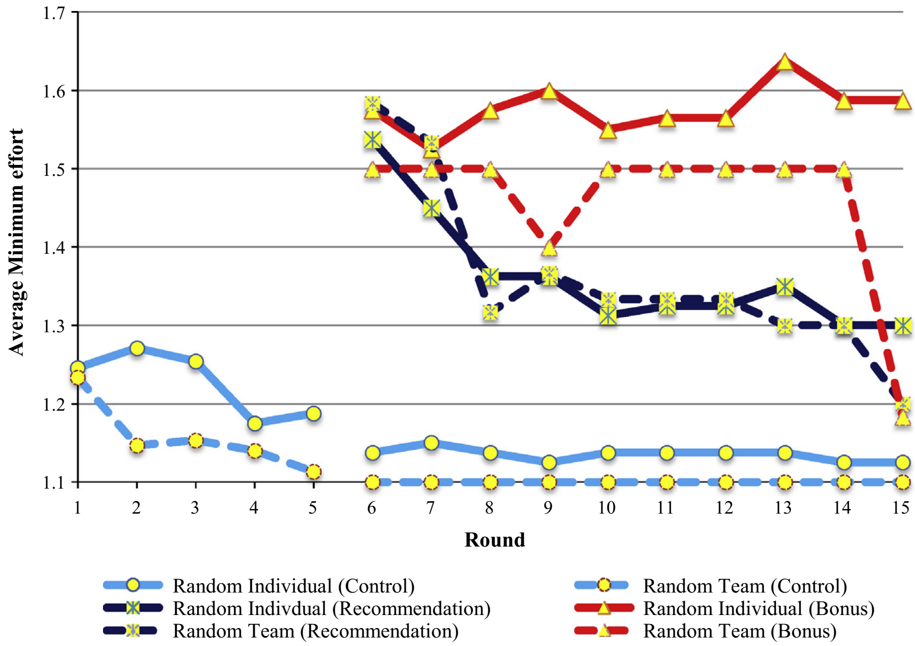

Fig. 7. Average minimum effort for teams and individuals with random re-matching in different treatments.

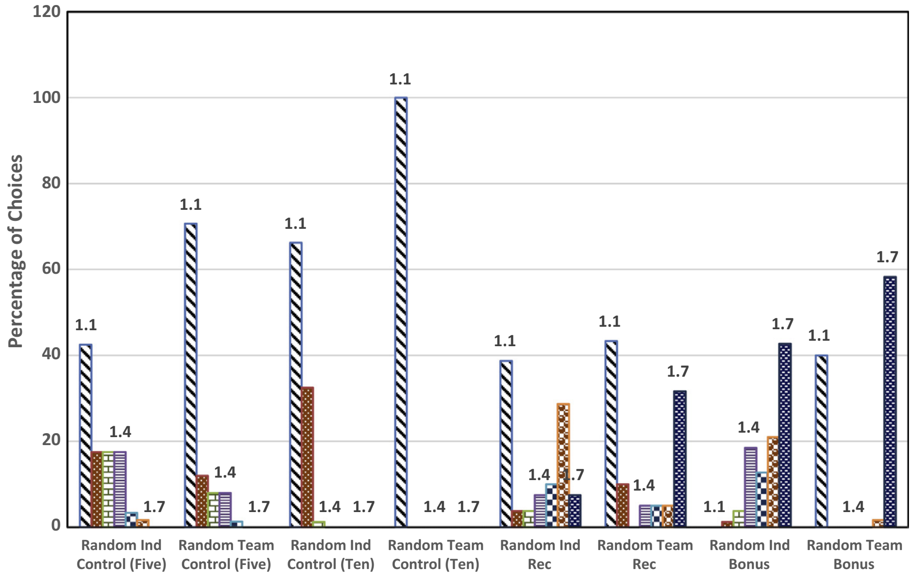

Fig. 8. Distribution of minimum effort choices among teams and individuals with random-re-matching.

Table 8
Random effects ordered probit regressions for teams and individuals with random re-matching across different treatments.

Own effort	Model 1		Model 2	
	Coefficient	Standard error	Coefficient	Standard error
Random team control	-1.607^{**}	0.658	-1.061	0.802
Random individual recommendation	3.001^{***}	0.433	4.372^{***}	0.634
Random team recommendation	2.892^{***}	0.501	5.472^{***}	0.842
Random individual bonus	4.674^{***}	0.507	4.923^{***}	0.69
Random team bonus	2.91^{***}	0.6	6.951^{***}	1.425
Round	-0.246^{***}	0.024	-0.121^{***}	0.025
Random team control * round			-0.154^{*}	0.09
Random individual recommendation * round			-0.173^{***}	0.064
Random team recommendation * round			-0.361^{***}	0.079
Random individual bonus * round			0.05	0.045
Random team bonus * round			-0.547^{***}	0.162
Lagged earnings	2.418^{***}	0.323	2.456^{***}	0.3
Cut 1	0.959^{***}	0.358	1.808^{***}	0.419
Cut 2	1.551^{***}	0.347	2.464^{***}	0.419
Cut 3	2.005^{***}	0.34	2.977^{***}	0.424
Cut 4	2.42^{***}	0.343	3.452^{***}	0.44
Cut 5	2.812^{***}	0.359	3.897^{***}	0.46
Cut 6	3.38^{***}	0.383	4.515^{***}	0.488
Number of observations	1755		1755	
Log pseudolikelihood	-1540.781		-1483.018	
Wald χ^2	224.89		203.96	
Probability $>\chi^2$	0.00		0.00	

*** Significance at 1%.
** Significance at 5%.
* Significance at 10%.

two models – one with treatment dummies, round and lagged earnings and a second one where we interact the treatment dummies with round to look for dynamic features in the data.

The regression results corroborate the patterns identified in Figs. 7 and 8. Model 1 suggests that in the control treatment random teams experience greater coordination failure compared to random individuals but this difference is no longer significant when we introduce the interaction terms involving round. Both teams and individuals experience an improvement in coordination following an intervention, whether recommendation or bonus. However, Wald tests suggest that there are no significant differences between teams and individuals following a recommendation. But individual choices following a bonus are significantly higher than teams. This latter difference is significant at 1% in Model 1 and at 9% in Model 2. Among individuals a bonus leads to much higher efforts as opposed to a recommendation but for teams effort choices following a bonus are not any higher than that following a recommendation.

4. Conclusion

In this paper we look at the degree of coordination success (or failure) among groups consisting of five individuals or five two-member teams. The group composition is either fixed over time or group members are randomly re-matched from one round to the next. When group composition is fixed over time then we replicate the Feri et al. (2010) result that teams are at least as successful as individuals if not better. This success takes two different forms. First, in the absence of any interventions teams coordinate to higher effort choices as compared to individuals. Second, teams are better at coordinating to the same effort choice. Further we find that the introduction of an intervention in the form of a public recommendation or a performance bonus has similar salutary effects on choices made by teams and individuals. The differences in coordination success between teams and individuals in our study are not as pronounced as that in Feri et al. which may be due to differences in either team size or process of arriving at team decisions or both.

The picture changes radically when we look at random re-matching. Here teams perform much worse when it comes to coordinating to the payoff dominant outcome. This is true both in the absence of an intervention as well as following one. Even with a performance bonus, which leads to improved coordination among individuals, teams continue to struggle and do not manage to do better with a bonus than with a recommendation. These results suggest that the greater success of teams in coordinating to the payoff dominant outcome depends crucially on the matching protocol.

Our results have implications for the performance of organizations involved in team-production. When groups are fixed over time, having workers work in teams actually leads to improved coordination. As Feri et al. point out, as long as group composition is stable, one way of enhancing coordination within organizations is to have people work in teams. But many organizations routinely rely on frequent transfers of workers from one unit to another for training purposes among other things. This clearly leads to a series of short-term interactions among group members. Organizations that are engaged in team production should be particularly wary of transferring workers, especially those who work in teams, given the efficiency implications of such transfers. Alternatively if transfers need to happen, then it might be a good idea to transfer groups of workers at a time in order to maintain continuity in their interactions. This is because teams fare especially badly in terms of coordinating their actions when the interaction between those teams undergoes frequent changes.

Acknowledgements

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Appendix A

A.1. Instructions for "Individual" treatment

A.1.1. General instructions

Welcome. The University of Auckland 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 into the server.

A.1.2. Specific instructions

In this experiment you will be in a market with 4 other players. There will be 15 rounds. In each round you will be randomly re-matched so that you will not be playing with the same individuals 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 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 or 1.2 or 1.3 or 1.4 or 1.5 or 1.6 or 1.7} The value you pick for X and the smallest value picked for X by any participant in your group, 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.

Your choice of X	Smallest value of X chosen						
	1.70	1.60	1.50	1.40	1.30	1.20	1.10
1.70	0.85	0.75	0.65	0.55	0.45	0.35	0.25
1.60	–	0.80	0.70	0.60	0.50	0.40	0.30
1.50	–	–	0.75	0.65	0.55	0.45	0.35
1.40	–	–	–	0.70	0.60	0.50	0.40
1.30	–	–	–	–	0.65	0.55	0.45
1.20	–	–	–	–	–	0.60	0.50
1.10	–	–	–	–	–	–	0.55

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 5th round and before the beginning of the 6th 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. **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 fixed matching:*** "in the session" is replaced with "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 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.

Box 1. Prediction for Round 6 <u>before</u> message.

Predicted average	Actual average	Absolute difference	Earnings ($1 – absolute difference)

Box 2. Prediction for Round 6 <u>after</u> message.

Predicted average	Actual average	Absolute difference	Earnings ($1 – absolute difference)

RECORD SHEET

Show-up Fee:	**$5.00**
Earnings from Prediction 1:	__________
Earnings from Prediction 2:	__________
Earnings from Experiment:	__________
TOTAL	__________

A.2. Instructions for "Team" treatment

A.2.1. General instructions

Welcome. The University of Auckland 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 into the server.

A.2.2. Specific instructions

In this experiment you will be playing as a part of a two-person team. In a minute we will tell you how these teams will be formed. You will be in a market with 4 other teams. There will be 15 rounds. In each round you will be randomly re-matched so that you will not be playing with the same teams 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 teams 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 or 1.2 or 1.3 or 1.4 or 1.5 or 1.6 or 1.7} The value you pick for X and the smallest value picked for X by any participant in your group, 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.

Your choice of X	Smallest value of X chosen						
	1.70	1.60	1.50	1.40	1.30	1.20	1.10
1.70	0.85	0.75	0.65	0.55	0.45	0.35	0.25
1.60	–	0.80	0.70	0.60	0.50	0.40	0.30
1.50	–	–	0.75	0.65	0.55	0.45	0.35
1.40	–	–	–	0.70	0.60	0.50	0.40
1.30	–	–	–	–	0.65	0.55	0.45
1.20	–	–	–	–	–	0.60	0.50
1.10	–	–	–	–	–	–	0.55

You will be able to read some of these instructions again once you have logged in. For each team, the two of you will log into only one computer and make all your decisions jointly with your team-member using that one computer. You are free to discuss the game with the other member of your team except we ask that you speak softly so that other participants cannot hear what you are talking about. After you have finished reading the instructions you will proceed to play the first 5 rounds of this game. After the end of the 5th round and before the beginning of the 6th 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. **Please do NOT continue on to the 6th round of this game till asked by the experimenter to do so**.

We will pay you your earnings from the experiment at the end of the session. Both team members will earn the exact same amount as that earned by the team. 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.

Following this we move across the room removing the partition between adjoining pairs of participants telling them that the two people sitting next to each other will form a two-person team. Teams are still separated by privacy partitions with at least one free computer between them.

A.2.3. Login instructions

- Login to the computer (using your UPI and password).
- Check that you are logged into your Net Account. If not, then please login 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.
- The session name for today's session is written on the white-boards in the front of the lab.
- Enter the Session Name then 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.

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