Solving coordination failure with "all-or-none" group-level incentives

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Abstract

Coordinating activity among members is an important problem faced by organizations. When firms, or units within firms, are stuck in bad equilibria, managers may turn to the temporary use of simple incentives – flat punishments or rewards – in an attempt to transition the firm or unit to a more efficient equilibrium. We investigate the use of incentives in the context of the "minimum-effort," or "weak-link," coordination game. We allow groups to reach the inefficient equilibrium and then implement temporary, flat, "all-or-none" incentives to encourage coordination on more efficient equilibria. We vary whether incentives are positive (rewards) or negative (penalties), whether they have substantial or nominal monetary value, and whether they are targeted to a specific outcome (the efficient equilibrium) or untargeted (apply to more than one outcome). Overall, incentives of all kinds are effective at improving coordination while they are in place, but there is little long-term persistent benefit of incentives – once incentives are removed, groups tend to return to the inefficient outcome. We find some differences between different kinds of incentives. Finally, we contrast our results to other recent work demonstrating greater long-term effectiveness of temporary incentives.

Keywords: coordination, incentives, organizations, experiments

1. Introduction

The ability of independent, specialized agents to coordinate their actions is an important problem faced by firms (March and Simon, 1958; Arrow, 1974). Accordingly, economists have devoted considerable attention, particularly in laboratory experiments, to understanding precisely how to obtain efficient coordination among groups of independent agents (e.g., Van Huyck et al., 1990; Weber, 2006).

A considerable part of the research on coordination reveals that groups often end up coordinated on inefficient outcomes (Van Huyck et al., 1990; Weber et al., 2001; Brandts and Cooper, 2006). In such instances, group members could be better off by taking a different set of actions – coordinating on a more efficient equilibrium – but the inefficient outcome may also be an equilibrium, and therefore self-reinforcing. Thus, of particular importance to research on firms and organizations is the question of how one can resolve such coordination failure and drive groups towards more efficient equilibria.

This paper explores how temporary, flat incentives – of the kind often used in firms – might be useful for inducing such change. We study the effectiveness of such incentives using a version of the "minimum-effort" (or "weak-link") coordination game first studied experimentally by Van Huyck et al. (1990).[1] This game captures key features of the kinds of coordination problems faced by firms (Camerer and Knez, 1994; Nanda, 1999; Weber, 2000; Brandts and Cooper, 2006).

The minimum-effort game

The minimum-effort game corresponds to a situation in which the costly provision of effort determines group output through a production process involving perfect complementarity between all the inputs (Hirshleifer, 1983; Kremer, 1993). All outcomes in which every player selects the same strategy are Nash equilibria, but these equilibria can be Pareto-ranked, with the lowest effort level corresponding to the least efficient or risk-dominant equilibrium and the highest effort level corresponding to the Pareto-optimal or payoff-dominant equilibrium (Harsanyi and Selten, 1988). Thus, players' strategy choices must trade-off the security of the low-effort action with efficiency (provided by successful coordination on the high-effort equilibrium).

In the game (presented in Table 1), several players each independently choose an "effort level." Each player's payoff is a function of her own effort (row) and the minimum effort of others in the group (column). The payoff received by any player *i* can also be represented by the formula:

$$\pi_i = A + B\min(x) - Cx_i \tag{1}$$

where x_i is the player's choice and min(x) is the minimum choice in the group.[2] Thus, Table 1 corresponds to the case in which $A = 200$, $B = 6$, and $C = 5$. With these payoffs, efficient coordination is quite difficult, and groups of size four almost always converge quickly to the inefficient outcome (Brandts and Cooper, 2005 and 2006).[3]

<u>Our experiment</u>

Given the fragile nature of the game, it is not surprising that groups often end up at the inefficient equilibrium (Van Huyck et al., 1990). It is important for economics and organization to understand how groups can be induced away from such equilibria. In our study, as in Brandts and Cooper (2006), we first allow groups to converge to the inefficient equilibrium, after which we introduce a temporary change in incentives in an attempt to improve efficiency (coordination on higher-effort equilibria). The incentives are subsequently removed in order to test for any long-term benefit to their introduction.

We explore different kinds of incentives, with two common features that are often present in the way incentives are used in real firms. The first common feature is that all our incentives are *group-level*, meaning that any penalty or reward is imposed on all group members. Second, our incentives consist of a *fixed* amount that is either added to or subtracted from each member's earnings, based on the group outcome. Thus, our incentives have an "all-or-none" property – they apply equally to a set of outcomes and do not vary based on which of those outcomes results – and correspond to the kinds of "across-the-board" incentives often used in real firms.[4]

To explore the effectiveness of different kinds of incentives, we vary them along three dimensions. Specifically, we vary the magnitude of the incentive (substantive or nominal), its valence (positive or negative), and whether it applies to one or more outcomes (targeted or untargeted). Each treatment potentially allows us to draw inferences from our experiment regarding prescriptions for the best kinds of simple incentives that might be used in firms to improve coordination.

The first distinction we explore is between *substantive* and *nominal* incentives. We explore whether incentives must be large in magnitude to encourage coordination on better equilibria, or whether token incentives are sufficient. There is conflicting evidence on this issue. Some previous research (Brandts and Cooper, 2005 and 2006) finds that incentive magnitude, using a more complicated linear increase in incentives, has almost no influence on helping groups coordinate on more efficient equilibria. Moreover, Dugar (2005) finds that even non-monetary incentives (informal expressions of disapproval) can induce efficient coordination. However, other research shows that behavior in closely related coordination games is responsive to the magnitude of incentives (e.g., Battalio et al., 2001; Goeree and Holt, 2005). Thus, the precise role of incentive magnitude remains unclear and warrants further investigation.

Our second treatment explores the relative effectiveness of *positive* and *negative* incentives. Considerable evidence suggests that losses have a greater psychological impact than comparable gains (Kahneman and Tversky, 1991). Indeed, equilibrium selection in coordination games can be influenced by the desire to avoid a loss (Cachon and Camerer, 1996; Rydval and Ortmann, 2005). Thus, negative incentives, imposed when groups fail to coordinate efficiently, might be more effective than positive incentives for successful coordination.[5]

Our third treatment explores whether incentives must be *targeted* (they apply to only the most efficient equilibrium) or *untargeted* (they apply to more than one of the "better" equilibria). To understand how best to induce groups away from the least efficient equilibrium, an important issue is whether simple incentives should target the optimal action or simply an improvement. Incentive use in firms often targets an improvement, rather than only the best possible outcome (Knez and Simester, 2001). However, there is little evidence on the relative effectiveness of incentives that only apply to the best outcome, versus incentives that apply to several better outcomes. Untargeted incentives might make it easier for the group to move away from the inefficient outcome. Alternatively, untargeted incentives might create strategic uncertainty about which incentivized action to take, making them less effective for inducing change to a more efficient equilibrium.

Broadly, our contribution is to explore the effectiveness of the simplest kinds of incentives, similar to those often used in real firms, to induce coordination on better equilibria. Managers frequently promise a flat, group-level reward for obtaining an improvement (e.g., meeting a deadline or sales target) or threaten a penalty if no improvement occurs. Thus, our research explores the simplest kinds of incentives that we observe in the real world, and their effectiveness for producing improvements in situations corresponding to the minimum-effort coordination game. Moreover, since our laboratory procedure is very similar to the one used by Brandts and Cooper, our results can be compared to theirs (though cautiously, due to location, population, and other differences between the experiments) to determine the relative effectiveness of the linear incentive schemes they utilize and our all-or-none, non-linear incentives.[6]

2. Experimental Design

Subjects were recruited from an e-mail list of graduate and undergraduate students at Carnegie Mellon and the University of Pittsburgh with little or no formal training in game theory. Sessions consisted of 12 subjects, and each subject received a $6 show-up fee in addition to money accumulated from the game.

Subjects were seated at computer terminals and randomly assigned participant numbers. They then received instructions via a computer interface,[7] which informed them that the experiment would involve three parts and that the first part would consist of 10 periods. They were told that they would be in a group with three other people and that group composition would remain fixed throughout the experiment. The instructions then presented the basic structure of the minimum-effort coordination game, including the formula in equation 1 and the payoffs in Table 1.

As in Brandts and Cooper (2006), our experiment was divided into three parts. Subjects were told that in Part 1 they would play this game for 10 periods and that payoffs represented an experimental currency that would be converted into dollars at the end of the experiment at a rate of $1/500. Subjects then took a quiz to ensure that they understood the instructions and payoff calculation.

Part 1 consisted of 10 periods of the game in Table 1. Subjects made choices by clicking on an effort level and confirming this choice. After each period, the screen displayed the choices of all four group members, the minimum choice in the group, the subject's payoff in the current period, and a history of outcomes for all previous periods. Subjects never received any information on outcomes in other groups.

At the conclusion of Part 1, subjects received instructions for Part 2. These instructions differed based on whether there was any change in incentives. In a *control* condition there was no mention of a change, and subjects were simply told that they would play the same game for another 10 periods. In all other conditions subjects received instructions stating that they would play the game for another 10 periods, but with the introduction of either a "penalty" if the minimum fell below a certain threshold or a "bonus" if the minimum exceeded a threshold.[8]

The threshold varied, depending on whether incentives were *targeted* or *untargeted*. For targeted incentives (Table 2a), the bonus (penalty) was obtained (avoided) only if all group members chose the action corresponding to the efficient equilibrium. For untargeted incentives (Table 2b), the bonus (penalty) was obtained (avoided) if all members chose either of the actions corresponding to the two most efficient equilibria.

Aside from targeted vs. untargeted incentives, we varied whether incentives were *positive* ($z = 0$ and $y > 0$ in Tables 2a and 2b) or *negative* ($z > 0$ and $y = 0$). We also varied the size of the incentives (z or y), from *nominal* (5) to *substantive* (40). In all cases, the set of pure-strategy equilibria for the game was unchanged. Table 3 presents the treatments for Part 2, which consists of a 2x2x2 design, in addition to the control condition. For each of the nine conditions, we conducted two sessions, each with three groups and 12 subjects. Thus, each cell consists of six groups and 24 subjects, and in total we employed 216 subjects (54 groups).

Following Part 2, in which there was either no change in payoffs (control) or a temporary introduction of incentives (all other treatments), subjects received new instructions for Part 3. Subjects were told that for the final 10 periods of the experiment, they would again play the same game as in Part 1.

At the conclusion of Part 3, subjects completed a series of questionnaires containing demographic measures and individual difference scales.[9] After completing the questionnaires, subjects were paid, privately, in cash and dismissed.

3. Results

As expected, a large majority of groups converged to the inefficient outcome in Part 1. The minimum was zero in 48 of the 54 groups (89 percent) in the final period of Part 1 (period 10).[10] Having obtained reliable coordination failure, we now explore whether our incentives improve coordination efficiency. We first explore the general, effectiveness of incentives, and then the relative effectiveness of different kinds of incentives. Finally, we compare our results to those of Brandts and Cooper (2006).

Since we are primarily interested in the effectiveness of incentives for improving coordination in groups stuck at bad equilibria, we restrict our attention to groups experiencing "initial failure" in coordination (period 10 minima of zero). For instance, Figures 1 through 4, which present average choices and minima by treatment, include only groups in which the period 10 minimum was zero. Similarly, Table 4 presents the distributions of subject choices in period 11, only for subjects in groups with period 10 minima of zero. However, our econometric analyses in Tables 5 and 6 include all groups (and use the period 10 minimum as an explanatory variable).

General effectiveness of incentives

In the control, where there was no change in incentives for Part 2, groups were unable to increase the minimum. Of six groups, all but one converged to a minimum of zero by the final three periods of Part 1, and these five groups were never subsequently able to obtain a higher minimum.[11] As shown in Figure 1 and Table 4, some control subjects increased their choices in period 11.[12] However, since the proportion of subjects doing so was small, this had no effect on the minima. Thus, consistent with Brandts and Cooper (2006) we observe coordination failure to be prevalent for the payoffs in Table 1 throughout the 30 periods of the control.

In the incentive treatments, however, average choices and minima increase substantially when the incentive is introduced. The average choice in period 10, for all groups exhibiting initial failure, is 2.1. In the control condition, the average period 11 choice in these groups rises to 8.0, but the average choice in the incentive conditions rises even more to 29.5 (M-W: $z = -4.96$, $p < 0.001$).[13] The third column in Table 4 reveals a strong reaction, at the individual level, to the introduction of incentives in period 11 (a majority of subjects, 97/172, select the maximum choice).

The increase in choices produces an increase in group minima. The average period 11 minimum for (initial failure) control groups is zero, while for groups with incentives it is 17.0 (M-W: $z = -2.34$, $p < 0.03$). Thus, both individual behavior and group outcomes respond initially to incentives.

The improvement with incentives continues beyond period 11. Average choices are higher with incentives than in the control in periods 11-15 (24.5 vs. 2.7) and 16-20 (18.9 vs. 0.3). Average minima, always equal to zero in the control (for initial failure groups), are higher with incentives in periods 11-15 (18.0) and 16-20 (17.5). Figure 1 shows that the introduction of incentives coordinates behavior (average minima and choices) on an effort level of about 18. This increase in average choices and minima with incentives can also be seen in Table 5, in the regressions for Periods 11, 11-15 and 16-20. There is a clear positive effect of incentives, which raise average minima and choices roughly half of the way to the efficient equilibrium.

We can also examine the proportion of groups demonstrating improved efficiency. Among all groups with initial failure, incentive groups were more likely than control groups to coordinate on minima higher than zero in at least one period between 11 and 20 (29/43 vs. 0/5; $p < 0.01$, Fisher's Exact), to obtain median minima greater than zero for those periods (22/43 vs. 0/5; $p < 0.04$, Fisher's Exact), and to coordinate on minima above zero for all 10 periods (18/43 vs. 0/5; $p < 0.09$, Fisher's Exact). However, this analysis also highlights that the improvement with incentives does not apply to all groups. Instead, the effect of incentives is largely bimodal. If we derive the median minimum for each incentive group for Periods 11-20, we find that 21 of 43 groups had median minima of zero, while 16 of the 43 groups had median minima of 40. Therefore, we should be cautious in interpreting the positive effect of incentives: roughly half of the groups exhibit sustained improvement with incentives, while an equal proportion exhibit no sustained improvement.

The positive effect of incentives persists only slightly once the incentives are removed. Average choices over the last 10 periods in the incentive treatment are higher than in the control (8.5 vs. 1.1, M-W: $z = -2.84$, $p < 0.005$). However, of the 18 groups coordinated on minima above zero in period 20 (the last period before incentives were removed), only six maintain regular coordination on minima above zero following the removal of incentives (three maintain minima greater than zero in all 10 final periods, two in all but one of the final 10 periods, and one in all but two). All of the remaining groups have minima of zero in at least eight of the final 10 periods. All differences with the control are insignificant at the group level (see Table 5). Therefore, following the removal of incentives we observe almost no persistent improvement in coordination.

Thus, the overall effectiveness of the incentives used here – for turning around firms stuck at the least efficient outcome – is mixed. While present, incentives are effective for increasing choices and obtaining coordination on minima greater than zero (roughly half the firms are able to do so). Nevertheless, many groups remain coordinated on the inefficient equilibrium even when incentives are present, and almost all groups return to this outcome once the incentives are removed.

Effectiveness of different kinds of incentives

Our research also deals with the relative effectiveness of different kinds of incentives. Having shown that incentives – of the group-level and all-or-none kind – at least temporarily improve the efficiency of coordination, we now turn to an exploration of what properties of incentives make them more effective.

Since our analyses above reveal that incentives only produce a strong effect on outcomes when they are in effect, and this is driven largely by period 11 reactions to the introduction of incentives, we restrict our statistical analysis (Table 6) to the effectiveness of incentives in the first period in which they are introduced (period 11) and the average over all periods in which they are in effect (periods 11-20).[14]

Substantive vs. nominal incentives

Figure 2 and Tables 4 and 6 reveal a difference in the effectiveness of substantive and nominal incentives. In the first period with incentives, the average choice is 32.6 with substantive incentives and 26.2 with nominal incentives. The difference in corresponding minima is even larger (22.3 vs. 11.4). The differences persist in subsequent periods, while incentives are in effect. Groups with substantive incentives converge to average choices and minima near 24, while groups with nominal incentives converge to average minima and choices of around 13. The stronger effectiveness of substantive incentives is confirmed in regressions (Table 6), using both individual-level (choice) and group-level (minima) data. The coefficient on *substantive* is always positive and statistically significant, indicating that larger incentives exert a stronger effect both on immediate reactions to the introduction of incentives (period 11) and on groups' ability to coordinate while incentives are in effect (periods 11-20).

Following the removal of incentives, however, both kinds of groups converge to average minima and choices around six. Thus, even though substantive incentives are more effective at inducing efficient coordination when they are present, this advantage disappears entirely once they are removed. A regression of period 21 choices (using only groups with incentives and initial failure) on period 20 choices, period 10 minima, and a dummy variable for substantive (vs. nominal) incentives reveals a significantly greater decrease in choices following the removal of substantive incentives.[15]

Positive vs. negative incentives

In Figure 3 and Table 4, we see that positive incentives are slightly more effective in period 11 than negative incentives. Average period 11 choices are higher for positive incentives (31.5) than for negative incentives (27.4), and the average period 11 minimum is also higher for positive incentives (19.1 vs. 14.8). However, with repetition, both sets of minima and choices converge towards 18.

The regressions in Table 6 confirm these findings. The effect of positive incentives on period 11 choices is marginally significant. However, all other differences are statistically insignificant, suggesting that positive incentives are no more likely than negative incentives to produce coordination on more efficient equilibria.

Targeted vs. untargeted incentives

In the first period after the introduction of incentives (period 11), there is very little difference in either average choices or minima under targeted or untargeted incentives (see Table 4 and Figure 4). Subsequently, targeted incentives do slightly better, with groups converging towards an average minimum of 20, while groups with untargeted incentives converge to a minimum of around 16. But a comparison of the difference in means, either using choices or group minima, is not significant (Table 6).

However, a closer inspection of Table 4 reveals that, while the average period 11 choices are virtually identical, the distributions of period 11 choices differ in an interesting way ($\chi^2(4) = 29.25$, $p < 0.001$). The distribution under targeted incentives is bimodal, with the largest proportion of subjects choosing the highest effort level (68 percent) and a significant proportion of subjects remaining at zero (19 percent). In the untargeted treatment, however, the two most frequent choices correspond to the two outcomes for which the incentive is attainable: 30 (33 percent of choices) and 40 (44 percent). Thus, the relative effectiveness of the two kinds of incentives is mixed. Fewer subjects under targeted incentives choose above zero (presumably because the incentive is less attainable), but those that do are much more likely to select the maximum choice. With untargeted incentives, by contrast, there are more subjects selecting choices above zero, but of those many select 30. This suggests differences in whether subjects decide to go after the incentive and in where they "aim" if they do.

The consequence of these period 11 differences can be seen in Table 7. This table presents, for groups with median minima in periods 11-20 greater than zero (i.e., groups that coordinated above zero in more periods than not), the median minimum across those periods. As the first two comparisons reveal, for the nominal/substantive and negative/positive treatments there is no difference in distributions – conditional on coordinating above zero, groups did so overwhelmingly on the efficient equilibrium. However, for targeted versus untargeted incentives, we see that coordination on minima above zero occurred almost exclusively on a minimum of 40 for targeted incentives, but on both minima of 30 and 40 for untargeted incentives. The distributions in the last two columns differ significantly. Thus, while the likelihood of coordination on minima above zero does not differ significantly for targeted and untargeted incentives, the likelihood of coordination on the most efficient equilibrium does.

Differences with Brandts and Cooper

While our all-or-none incentives are effective at improving coordination while present, they produce little persistent improvement once they are removed. Brandts and Cooper (2006, henceforth BC), by contrast, find that temporary incentive increases – though of a different kind than what we use here – are both more effective while they are in effect and also produce more sustained coordination after they are removed. To make prescriptions regarding incentives for improving coordination, it is important to understand why BC's incentives are more effective than the ones explored here.[16]

One important difference between the studies is that BC increased the bonus that applied to *all* minima greater than zero (*B* in equation 1). Thus, in their framework, when incentives are present any improvement in group coordination yielded a higher payoff. By contrast, subjects in our experiment only obtain the all-or-none incentives by coordinating on a minimum of 40 (or 30 in the untargeted treatment). Thus, perhaps it is easier for subjects to "climb" their way out of the inefficient outcome using the BC kind of incentives (where each successive improvement in efficiency yields a higher reward), whereas groups in our experiment that fail to reach the incentive threshold immediately may give up.

To explore this possibility, we compare our group minima with those in BC (see Figure 5).[17] The immediate behavioral response to all-or-none incentives is much larger – the average period 11 minimum is 17.0 for our data, while it is 8.4 for the BC data ($t_{78} = 2.68$, $p < 0.01$). In our case, the minimum subsequently changes very little in periods 12-20. In the case of BC's data, however, there is a steady increase in average minima following the introduction of incentives, and the average minimum in their experiment exceeds the average minimum in our data by period 14 (19.5 vs. 18.6).

Their groups obtain higher average minima for the remainder of the experiment. Thus, while our incentives initially produce a stronger effect, they are less effective in the long run.

The difference in minima following the removal of incentives can be explained by group outcomes at the end of periods 11-20 (the portion with incentives). A regression of period 21 minima on a BC dummy variable yields a significant coefficient for the dummy ($p < 0.05$). However, when the same regression includes period 20 minima, the coefficient on the dummy variable is no longer significant, while the coefficient for lagged minima is significant.

Thus, we find a likely explanation for differences between the two experiments. With all-or-none incentives, any improvement to the minimum must occur right away. In BC's experiment however, the linear increase in the bonus means that groups can "climb" their way out of the inefficient outcome. Therefore, BC's experiment obtains greater improvement with incentives, and this advantage persists into the period following the removal of incentives.

4. Conclusion

Our experiment explores the effectiveness of different types of temporary, all-or-none incentives for inducing efficient coordination in groups stuck at "bad" equilibria. We find the incentives to differ somewhat in their effectiveness. Substantive incentives induce greater improvement than nominal ones. Targeted and untargeted incentives produce virtually identical average choices when introduced, but differ in whether they induce subjects to go after the incentive and in where subjects "aim" if they do. We find virtually no difference in effectiveness of positive and negative incentives.

More generally, pooling across incentive treatments, we find that all-or-none incentives do induce coordination on better equilibria, though they do not completely "solve" coordination failure. Slightly over half of the groups experiencing initial coordination failure are able to coordinate on higher minima when incentives are introduced. However, this improvement virtually disappears following the removal of the incentive, in contrast to the continued effectiveness of the linear incentives introduced by Brandts and Cooper (2006). In their study the change in incentives allowed group members to "climb" out of payoff-dominated equilibria, while our incentive structure requires groups to "leap" to the payoff-dominant outcome.

Our work highlights the relative practical effectiveness of different kinds of incentives. For instance, if incentives can only be applied for a few periods, the kind of incentives we use here (ans especially substantive and targeted ones) might be more effective. However, if a longer time period with incentives is possible, then the incentives of Brandts and Cooper are likely to prove more effective. Our work reveals the potential value in understanding when and how different kinds of incentives are likely to work.

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Notes

[1] See also Hirshleifer (1983) and Crawford (1995) for theoretical discussions of the game.

[2] One can think of *A* as representing a "fixed wage" for every employee; *B* the linear group reward for obtaining higher minima; and *C* the cost associated with higher effort levels.

[3] Several experiments on the minimum-effort game reveal that group size plays an important role in the ability of groups to coordinate efficiently – small groups (n=2) find it very easy, while for groups of 6 or more, efficient coordination is almost impossible (Van Huyck et al., 1990; Weber et al., 2001; Weber, 2006; see also, Crawford, 1995).

[4] An example of the successful use of such all-or-none firm-wide incentives is Continental Airlines' introduction in 1995 of a $65 bonus per employee for every month in which the airline ranked in the top-five in on-time departures (see Knez and Simester, 2001). Airport departures and minimum-effort coordination games share many properties (Weber, 2000; Gittell, 2001).

[5] Related evidence on public goods games reveals that punishments are more effective for inducing and maintaining high contribution levels than are comparable rewards (Sefton et al., 2002; Fehr and Gächter, 2000; Sutter et al., 2006).

[6] Of course, incentives are not the only way firms can induce efficient coordination. For instance, several papers study the effectiveness of communication and leadership in allowing groups to coordinate efficiently, and find mixed results (Charness, 2000; Weber et al., 2001; Chaudhuri et al., 2001; Blume and Ortmann, 2005). In a direct comparison of the efficacy of communication and (linear) incentives, Brandts and Cooper (2005) find that communication is more effective. Other studies have attempted to improve coordination by introducing incentives for outperforming other groups (Bornstein et al., 2002), changing the cost associated with more efficient actions (Goeree and Holt, 2005), and varying the time horizon of the repeated game (Berninghaus and Erhart, 1998). Finally, Weber (2006) shows that a firm's growth rate can be critical for maintaining coordination.

[7] The experiment was programmed and conducted with the software *z-Tree* (Fischbacher, 1999).

[8] While the terms "bonus" and "penalty" are certainly not context-neutral, we use them for two reasons. First, the introduction of incentives in firms often uses these types of labels. Second, we follow Brandts and Cooper (2006), who described their positive incentive as a "bonus."

[9] We included measures of demographic characteristics and individual differences – risk attitude and time preference – to determine whether any individual differences predicted responsiveness to incentives. Responses to these questions did not correlate with behavior in the experiment.

[10] Of the remaining groups, two obtained minima of 10, one a minimum of 20, and three minima of 40.

[11] The remaining group coordinated on a minimum of 10 during Part 1 and remained at this equilibrium for the remainder of the experiment.

[12] This is not unusual, even when incentives do not change. It represents responses to a "re-start" or "bell-ringing" effect, whereby players attempt to use the uniqueness in a particular period to coordinate on a higher equilibrium (see Crawford, 1991; Brandts and Cooper, 2006).

[13] This and subsequent tests use a non-parametric Mann-Whitney test, with the normal approximation.

[14] Regressions of behavior and outcomes in periods 21-30, after incentives are removed, reveal no differences in effectiveness between our treatments. In the six groups that managed to coordinate on minima above zero for eight or more of the last 10 periods, the breakdown by treatments was as follows: 3 each with substantive and nominal incentives, 3 each with positive and negative incentives, and 4 with targeted and 2 with untargeted incentives.

[15] The model is: $choice_{21} = \alpha + \beta_1 choice20 + \beta_2 min_{10} + \beta_3 substantive + \varepsilon$. The estimated coefficients (robust standard errors) are: $\alpha = 8.11$ (1.74), $\beta_1 = 0.62$ (0.05), $\beta_2 = 0.24$ (0.06), $\beta_3 = -4.14$ (1.95); N= 192, $R^2 = 0.43$.

[16] One possible explanation, of course, is subject pool differences (Pittsburgh vs. Cleveland/Barcelona). However, this explanation is less likely once one considers that the behavior in periods 1-10 is quite similar between the two experiments.

[17] We use only groups with initial failure (period 10 minima of zero). In the BC data, for periods 1-20 we use all treatments in which the bonus (*B*) was initially six in periods 1-10 and increased for periods 11-20. For periods 21-30, we use only the treatment in which the bonus was returned to the initial value of six.

Table 1. Payoffs in minimum-effort game (baseline case)

		Minimum choice by others in group				
		0	**10**	**20**	**30**	**40**
Choice by player	**0**	200	200	200	200	200
	10	150	210	210	210	210
	20	100	160	220	220	220
	30	50	110	170	230	230
	40	0	60	120	180	240

Table 2a. Payoffs in minimum-effort game (targeted incentives)

		Minimum choice by others in group				
		0	**10**	**20**	**30**	**40**
Choice by player	**0**	200 – z	200 – z	200 – z	200 – z	200 – z
	10	150 – z	210 – z	210 – z	210 – z	210 – z
	20	100 – z	160 – z	220 – z	220 – z	220 – z
	30	50 – z	110 – z	170 – z	230 – z	230 – z
	40	0 – z	60 – z	120 – z	180 – z	240 + y

Positive incentives: y > 0, z = 0; Negative incentives: z > 0, y = 0

Table 2b. Payoffs in minimum-effort game (untargeted incentives)

		Minimum choice by others in group				
		0	**10**	**20**	**30**	**40**
Choice by player	**0**	200 – z	200 – z	200 – z	200 – z	200 – z
	10	150 – z	210 – z	210 – z	210 – z	210 – z
	20	100 – z	160 – z	220 – z	220 – z	220 – z
	30	50 – z	110 – z	170 – z	230 + y	230 + y
	40	0 – z	60 – z	120 – z	180 + y	240 + y

Positive incentives: y > 0, z = 0; Negative incentives: z > 0, y = 0

Table 3. Experimental treatments

	Nominal	**Substantive**
Positive	z = 0, y = 5	z = 0, y = 40
Negative	z = 5, y = 0	z = 40, y = 0

Targeted (Table 2a) **/ Untargeted** (Table 2b)

Table 4. Distributions of period 11 choices (groups with initial failure only)

	Control	*Incentives*	*Nominal*	*Substantive*	*Negative*	*Positive*	*Untargeted*	*Targeted*
0	14	25	18	7	15	10	8	17
10	2	12	5	7	8	4	7	5
20	1	7	6	1	5	2	4	3
30	0	31	17	14	12	19	28	3
40	3	97	38	59	44	53	37	60
Average	8.0	29.5	26.2	32.6	27.4	31.5	29.4	29.5
$\chi^2(4)$	36.90 ($p < 0.001$)		13.50 ($p = 0.009$)		5.94 ($p = 0.203$)		29.25 ($p < 0.001$)	
t-test	$t_{190} = 6.13$ ($p < 0.001$)		$t_{170} = 2.90$ ($p = 0.004$)		$t_{170} = 1.82$ ($p = 0.070$)		$t_{170} = 0.06$ ($p = 0.951$)	

Table 5. Effect of incentives on subject choices and group minima

	Dependent variable: choice or average choice *(standard errors clustered by group)*						***Dependent variable: minimum or average minimum***					
	Period		**Periods**				**Period**		**Periods**			
	11	21	11-15	16-20	21-25	26-30	11	21	11-15	16-20	21-25	26-30
Period 10 minimum	0.27^{***}	0.58^{***}	0.36^{***}	0.49^{***}	0.71^{***}	0.78^{***}	0.54^{**}	0.89^{***}	0.51^{**}	0.52^{**}	0.84^{***}	0.79^{***}
	(0.04)	(0.06)	(0.06)	(0.09)	(0.06)	(0.09)	(0.22)	(0.15)	(0.23)	(0.24)	(0.16)	(0.17)
Incentive	20.19^{***}	11.40^{***}	20.58^{***}	17.42^{***}	8.44^{***}	5.15^{**}	15.89^{**}	3.92^{**}	16.84^{**}	16.37^{**}	4.81	4.52
	(3.82)	(3.15)	(2.41)	(2.74)	(2.11)	(2.01)	(6.85)	(4.49)	(7.04)	(7.30)	(4.92)	(5.35)
Constant	9.14^{**}	5.29^{**}	3.64^{***}	1.11	2.15^{**}	0.79^{*}	0.77	0.19	0.82	0.80	0.26	0.36
	(3.62)	(2.51)	(1.35)	(0.79)	(1.02)	(0.42)	(6.46)	(4.23)	(6.64)	(6.89)	(4.64)	(5.05)
N	216	216	216	216	216	216	54	54	54	54	54	54
R^2	0.20	0.14	0.24	0.17	0.26	0.27	0.19	0.42	0.18	0.17	0.36	0.29

* - p <.1; ** - p <.05; *** - p <.01

Table 6. Effect of varying kinds incentives on subject choices and group minima (incentive treatments only)

	Dependent variable: choice or average choice *(standard errors clustered by group)*						***Dependent variable: minimum or average minimum***					
	Period 11			Periods 11-20			Period 11			Periods 11-20		
Period 10 minimum	0.27^{***} (0.06)	0.27^{***} (0.05)	0.25^{***} (0.04)	0.44^{***} (0.10)	0.42^{***} (0.08)	0.43^{***} (0.09)	0.56^{**} (0.23)	0.54^{**} (0.24)	0.52^{**} (0.25)	0.54^{**} (0.24)	0.51^{**} (0.25)	0.53^{**} (0.26)
Substantive (vs. Nominal)	6.07^{***} (2.27)			9.15^{**} (4.21)			9.86^{**} (4.64)			10.38^{**} (4.83)		
Positive (vs. Negative)		3.98^{*} (2.38)			2.12 (4.41)			4.01 (4.83)			1.09 (5.07)	
Targeted (vs. Untargeted)			-0.20 (2.47)			1.04 (4.53)			-0.54 (4.94)			2.26 (5.14)
Constant	26.27^{***} (1.79)	27.33^{***} (1.98)	29.47^{***} (1.55)	16.74^{***} (3.10)	20.32^{***} (3.43)	20.85^{***} (3.10)	11.66^{***} (3.38)	14.64^{***} (3.52)	16.97^{***} (3.65)	12.15^{***} (3.52)	16.89^{***} (3.70)	16.25^{***} (3.79
N	192	192	192	192	192	192	48	48	48	48	48	48
$\mathbf{R^2}$	0.08	0.05	0.03	0.15	0.07	0.07	0.18	0.11	0.10	0.17	0.08	0.09

Table 7. Distributions of period 11-20 median minima (groups with median minima > 0 only)

	Nominal	*Substantive*	*Negative*	*Positive*	*Untargeted*	*Targeted*
10	1	0	0	1	0	1
20	0	1	1	0	1	0
30	2	2	1	3	4	0
40	5	11	8	8	5	11
$\boldsymbol{\chi^2(3)}$	2.82 (p = 0.420)		2.84 (p = 0.417)		8.14 (p = 0.043)	

Figure 1. Average minima and choices across periods (control vs. incentives; initial failure only)

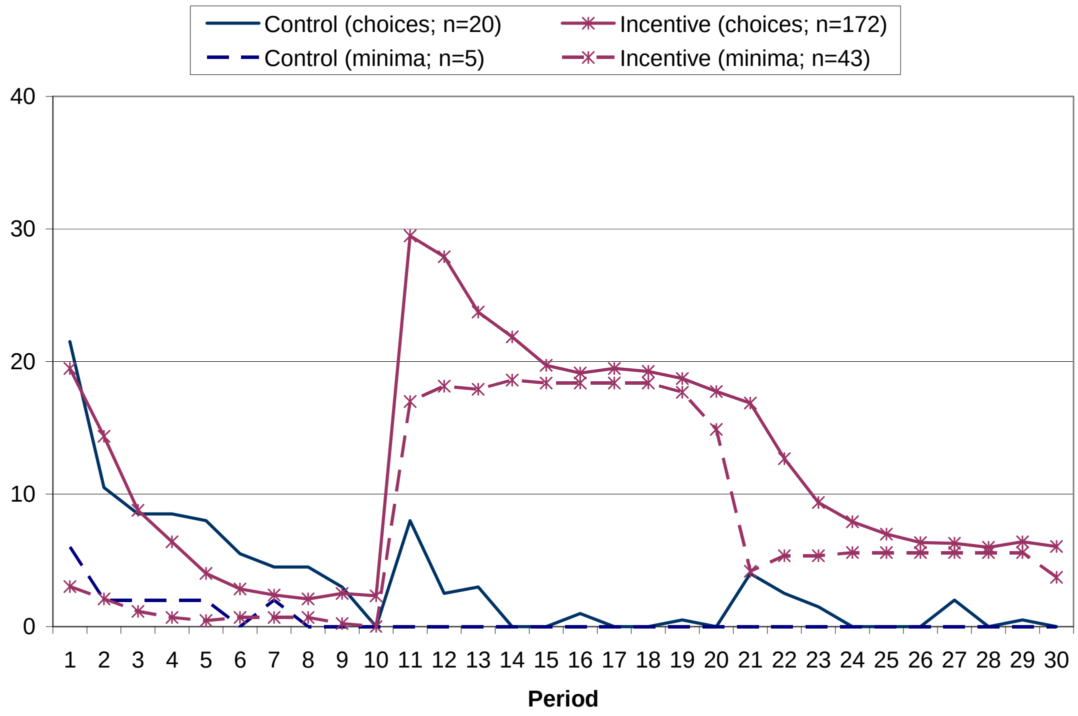

Figure 2. Average minima and choices across periods (substantive vs. nominal incentives; initial failure only)

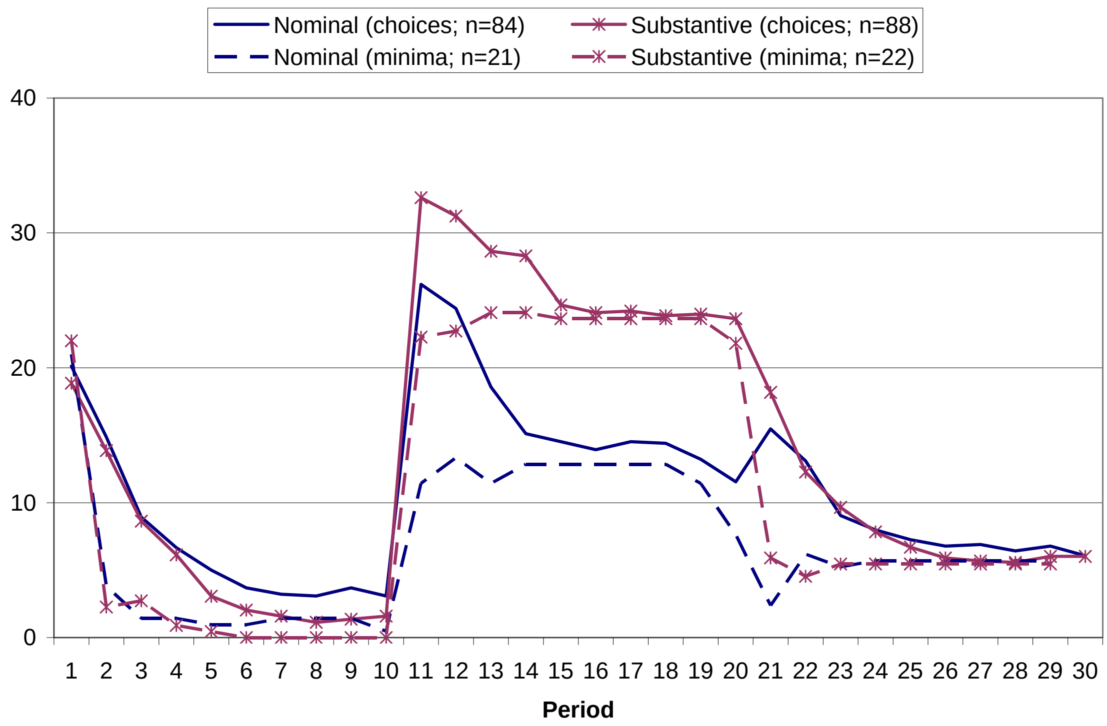

Figure 3. Average minima and choices across periods (positive vs. negative incentives; initial failure only)

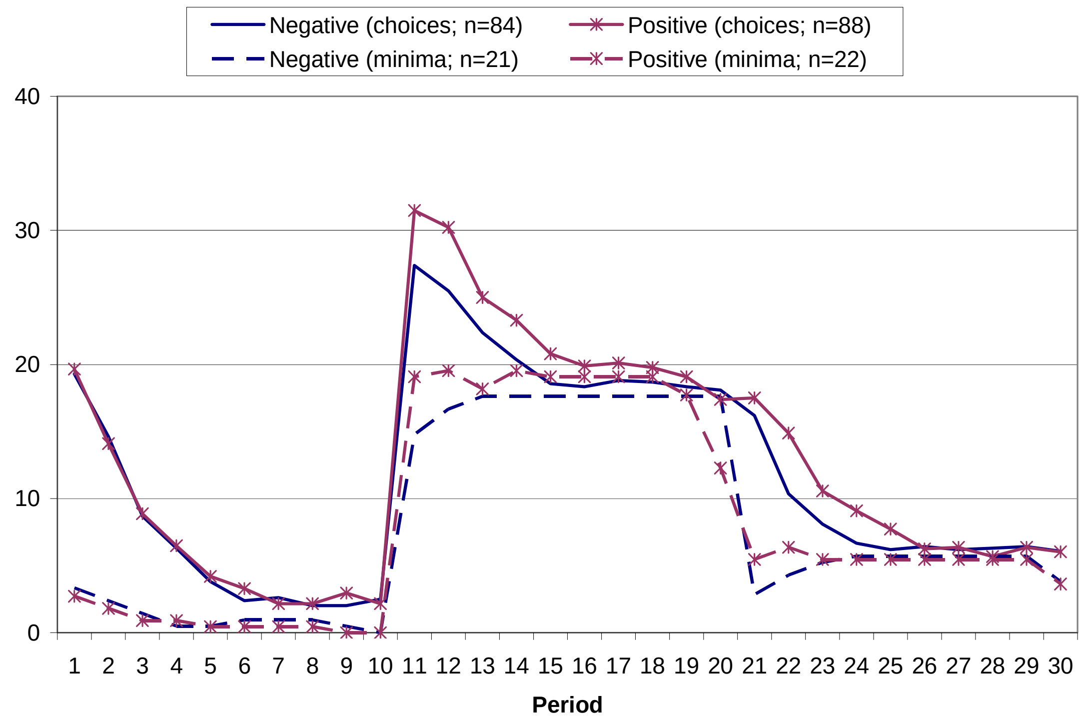

Figure 4. Average minima and choices across periods (targeted vs. untargeted incentives; initial failure only)

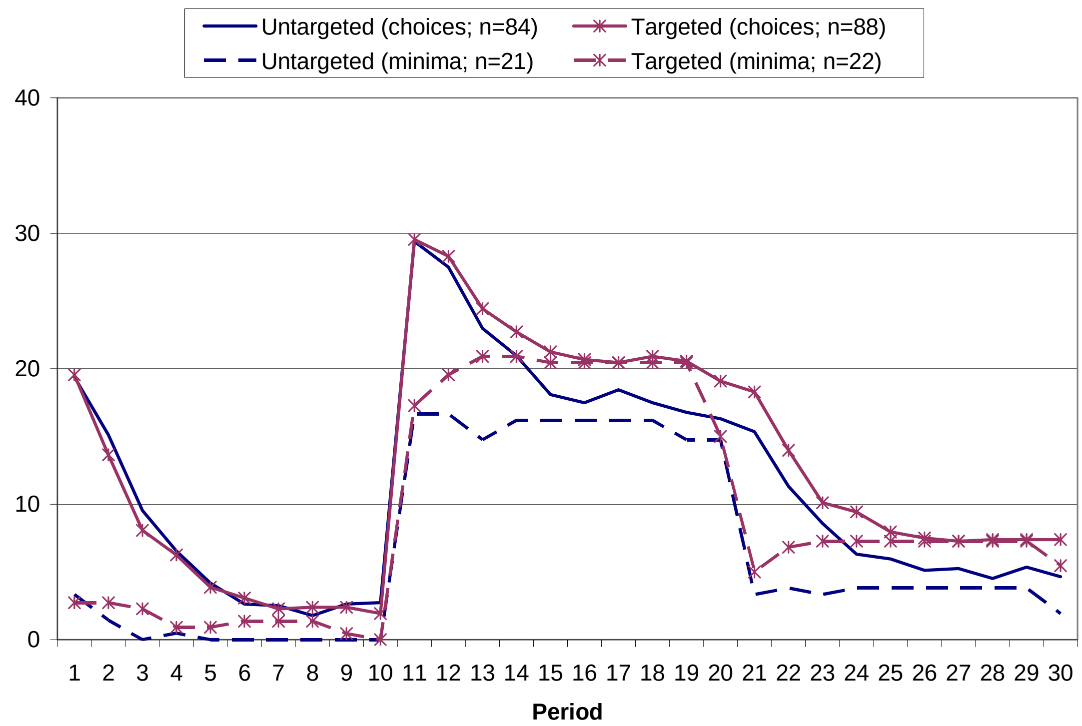

Figure 5. Average minima across periods (this experiment vs. Brandts and Cooper; initial failure only)