Coordination with Endogenous Contracts:
Incentives, Selection, and Strategic Anticipation

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April, 2017

Abstract

We examine the effects of endogenous assignment to incentive contracts on worker productivity. Assignment to high performance pay via a market mechanism is roughly twice as effective as imposing the same contract exogenously. This positive effect is largely offset by a negative effect for workers that endogenously choose low performance pay. We decompose the positive effect of endogenous assignment to high performance pay into effects due to selection and strategic anticipation. We estimate that 73% of the effect is due to selection.

Keywords: Experiment, coordination, incentive contracts, selection
JEL Classification Codes: C92, J31, M52, C73, C51

Acknowledgements: We would like to thank the NSF (SES-1127704) for financial support. We thank Brent Davis, Phil Brookins and Laura Magee for their work as research assistants on this project. We thank Jordi Brandts, Vince Crawford, Tony Kwasnica, Arno Riedl, Ernan Haruvy and Katya Sherstyuk for helpful discussion of this paper as well as seminar participants at the SEA meetings, Florida State, Heidelberg, Southampton, Monash, Sydney, Oxford, Nottingham, Mannheim, Amsterdam, GATE, Pittsburgh, and Penn State. Any errors are solely our own.

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1 Introduction

The performance of many work groups is constrained by a single weak link. A paper cannot be completed until all co-authors finish their assigned sections, a meeting cannot start until all critical personnel are present, and an assembly line moves no faster than its slowest worker. If pay is based on group performance, as must be the case when only output rather than individual effort is observed, the strong complementarities generated by a weak link technology can cause *productivity traps* where pessimistic beliefs create a self-fulfilling prophecy: nobody works hard in the (correct) expectation that any effort will be wasted given that no individual can unilaterally improve productivity. Escaping such a trap is difficult since coordinated change by *all* members of the group is needed to increase productivity.

A number of mechanisms have been proposed to achieve the coordinated increase in effort needed to escape a productivity trap (defined as coordination at low effort levels). A simple option that has consistently proven effective is increasing incentives to coordinate at high rather than low effort.[1] This has been shown to help groups escape productivity traps in lab studies of the weak-link game (Brandts and Cooper, 2006; Hamman, Rick, and Weber, 2007; Brandts, Cooper, and Weber, 2007) as well as in field settings (Knez and Simester, 2001). The preceding lab studies feature a common element: assignment of individuals to incentive contracts is random and exogenous. This was a natural first step for the literature, but in labor markets, where workers frequently choose between jobs with different incentive contracts, assignment to incentive contracts is often endogenous. The primary purpose of the experiments presented below is to explore the effect of having the assignment of individuals to contracts take place endogenously through a market mechanism.

All of our experiments feature an initial phase in which subjects repeatedly play a weak-link game under an incentive contract featuring high fixed pay and low incentives to coordinate at high effort levels ("low performance pay"). This reliably induces a productivity trap. In the second phase of the experiment, half of the subjects continue with the initial contract and half are as-

[1] Other mechanisms known to improve coordination in weak-link games or stag-hunt games include costless pre-play communication (Cooper, De Jong, Forsythe, and Ross, 1992; Duffy and Feltovich, 2002; Duffy and Feltovich, 2006; Blume and Ortmann, 2007; Brandts and Cooper, 2007; Cason, Sheremeta, and Zhang, 2012), intergenerational advice (Chaudhuri, Schotter, and Sopher, 2009), competition between groups (Bornstein, Gneezy, and Nagel, 2002; Myung, 2012), gradual growth of groups (Weber, 2006), help with commitment (Brandts, Cooper, Fatas, and Qi, 2016), and endogenous group formation (Riedl, Rohde, and Strobel, 2016; Salmon and Weber, 2017).

signed to a new incentive contract with lower fixed pay and larger incentives to coordinate at high effort levels ("high performance pay"). Treatments vary by whether the assignment of subjects to incentive contracts for the second phase is random or endogenous through a market mechanism. The positive effect of introducing high performance pay almost doubles with endogenous assignment. However, a matching negative effect from endogenous assignment to low performance pay largely offsets the positive effect of endogenous assignment to high performance pay. The *total* effect of endogenous assignment to incentive contracts, averaging over both contracts, is virtually zero.

Beyond identifying the effects of endogenous assignment to incentive contracts, a major goal of our study is to understand why high performance pay is more effective when assigned endogenously. In work environments with the weak-link property, strategic uncertainty makes incentive contracts "fragile." Individuals may fail to respond to high performance pay if they fear others won't increase effort in response. Endogenous assignment to high performance pay facilitates coordination at the highest possible effort level ("efficient coordination") by reducing strategic uncertainty through two channels, selection and strategic anticipation. "Selection" refers to the tendency of market mechanisms to assign high performance pay to individuals ("optimists") who have inherently optimistic prior beliefs about the chance of coordination at the efficient outcome *independent of how individuals are assigned to groups.* Because long-run outcomes in coordination games are largely driven by initial beliefs (see Van Huyck, Battalio, and Beil, 1990 and 1991), systematic assignment of optimists to high performance pay increases the chance of efficient coordination. Beyond selection, participants can infer that assignment to high performance pay is *not* random based on the information available in a market mechanism. "Strategic anticipation" is the ability of an individual to (correctly) anticipate that optimists are more likely to be assigned high performance pay by a market mechanism than with random assignment.[2] Individuals with strategic anticipation understand the effects of selection and become more optimistic under high performance pay *when contract assignment is endogenous.* This increased optimism improves the chance of efficient coordination with high performance pay.

We use an innovative "Sort" treatment to measure how much of the positive effect of endoge-

[2] This is related to forward induction, as both rely on individuals understanding that past choices reveal information about others' beliefs, but is not based on iterated removal of dominated strategies (Ben-Porath and Dekel, 1992).

nizing assignment to high performance pay is due to selection rather than strategic anticipation. In this treatment, subjects' characteristics and initial choices are used to predict which incentive contract they would have been assigned by the market mechanism. We then exogenously implement the predicted contract assignments to (imperfectly) reproduce the market's outcome. The Sort treatment preserves the effects of selection as it inherits the market's tendency to assign optimists to high performance pay, but eliminates the effects of strategic anticipation by depriving subjects of any information allowing them to anticipate the selection process. We find that 73% of the improvement with endogenous assignment to high performance pay is due to selection.[3]

The effect of selection is so strong that it can overcome the direct effect of lower incentives to coordinate at high effort levels. We demonstrate this through a "Reverse Sort" treatment that flips the contract assignments from the Sort treatment, switching the sign of the selection effect. This results in higher effort levels with low performance pay than high performance pay!

Our work contributes to two strands of the existing literature. Both lab and field studies have examined how endogenous choice of performance pay schemes (e.g. piece-rate systems) affects performance in individual tasks. These studies generally find that such schemes improve productivity and that the majority of this positive effect is due to selection of more able individuals (i.e. Lazear, 2000; Cadsby, Song, and Tapon, 2007; Eriksson and Villeval, 2008; Dohmen and Falk, 2011). Our work has similar findings - endogenous choice of high performance pay improves productivity with selection accounting for most of the effect - but differs from the existing literature because of the critical role played by strategic uncertainty. Selection is based on individuals' beliefs rather than ability to perform a task. The market mechanism assigns those who are inherently optimistic about the likelihood of efficient coordination to groups with high performance pay. Group productivity in field settings generally does not just depend on skill, but also depends on being able to work together. It makes sense that selection takes place along the latter dimension as well as the former.[4]

[3]In a companion paper (Cooper, Ioannou, and Qi, 2017), we use a structural model to estimate what the selection effect would look like if the Sort treatment *perfectly* replicated the assignment to incentive contracts from the Auction treatment. Using this approach, we estimate that the true proportion of the improvement with endogenous assignment to high performance pay that can be attributed to selection is 90%.

[4]Dohmen and Falk (2011) consider interactive settings (revenue sharing and tournaments) as well as piece rate compensation. Strategic uncertainty and beliefs play a role in their results, albeit different from our experiments, as self-assessment has a significant effect on selection into tournaments.

Kosfeld and von Siemens (2009, 2011) present a related theoretical model that predicts sorting into or out of workplaces based on being a conditional cooperator. Their theory involves strategic uncertainty as conditional cooperators would like to be matched with other conditional cooperators. For experimental evidence of sorting

Our experiments also relate to the literature on buying the right to play a coordination game, notably Van Huyck, Battalio, and Beil (1993). Van Huyck et al. study repeated play of a two-stage game where players bid for the right to play a median game. They find that winning bids converge to the payoff from the efficient equilibrium and winning subjects' play in the median game converges to the efficient equilibrium.[5] Van Huyck et al. attribute the latter result to forward induction, while Crawford and Broseta (1998) argue that it reflects an interaction between learning, forward induction, and an optimistic subject effect analogous to selection.[6] Our result that endogenous assignment to high performance pay improves the likelihood of efficient coordination is obviously related, but we reach different conclusions about the source of this effect. Using the Sort treatment, we show that the effect of endogenizing contract assignment is primarily due to selection rather than strategic anticipation (taking the place of forward induction). This has an important implication for the interpretation of our results. Selection effects do not rely on workers understanding the selection process, and hence are more likely to carry over to field settings.

A feature that differentiates our experiments from the existing literature is that the outside option for individuals who are not assigned high performance pay by the auction is continued play with *low* performance pay contracts. For experiments with endogenous assignment to performance pay, the outside option is typically either a flat payment or individual work under pre-existing incentives (usually flat-rate pay). The same forces of selection and strategic anticipation that push groups assigned high performance pay towards efficient coordination also lead groups assigned

based on social preferences, see Lazear, Malmendier, and Weber (2012).

[5]See Broseta, Fatas, and Neugebauer (2003) and Sherstyuk, Karmanskaya, and Teslia (2014) for similar results. Cachon and Camerer (1996) show that loss aversion plays a major role in the Van Huyck et al. results if subjects pay to play the coordination game. Our design does not have "pay to play," limiting the role of loss aversion. Other papers showing effects of endogenous assignment include Bohnet and Kübler (2005) and Dal Bó, Foster, and Putterman (2010). Kogan, Kwasnica, and Weber (2011) show that incorporating markets, albeit in a very different manner than we do, does not necessarily lead to increased efficiency in the coordination game.

[6]The learning process in Van Huyck et al. is somewhat different from our experiments since their design features many auctions rather than one. There is a feedback loop between the auctions and the coordination game. Forward induction and selection of optimistic subjects increase the effort chosen by auction winners. This in turn leads to higher future bids as subjects' expectations about the outcome of the coordination game become more optimistic. Crawford and Broseta develop a structural model to capture this learning process. They attribute the efficient coordination outcome to two major components, forward induction and a combined effect of an "optimistic subjects" effect and a "robustness" effect, along with two minor components that are technical in nature. The optimistic subjects effect captures the tendency of subjects who are optimistic *ex ante* about the outcome of the coordination game to win the auction. The robustness effect captures the interaction between learning and strategic uncertainty. The Van Huyck et al. experiments are not designed to directly measure these effects, but Crawford and Broseta estimate them by fitting their model to the data and then running simulations. They assign roughly half of the effect of the auctions to forward induction and half to the combination of the optimistic subjects and robustness effects. For technical reasons it is not possible to separate the optimistic subjects and robustness effects.

low performance pay to coordinate at inefficient outcomes. The negative effect of endogenous assignment to low performance pay offsets the positive effect of endogenous assignment to high performance pay, making the total effect of endogenous assignment neutral. This counterbalancing effect cannot occur with a fixed outside option. We discuss the economic implications of the preceding observations in the conclusion.

This paper is organized as follows. Section 2 lays out the experimental design, procedures, and hypotheses. Section 3 presents the main results of the paper. Section 4 concludes.

2 Experimental Design

2.1 The Corporate Turnaround Game: The turnaround game (Brandts and Cooper, 2006) is designed to study how a group caught in a productivity trap can escape (i.e. coordinate at a higher effort level). We use a variation of the turnaround game from Brandts and Cooper consisting of twenty rounds of a weak-link coordination game, split into two ten-round blocks. Subjects play in groups of four experimental subjects. They are randomly assigned to a group for the first ten-round block, and then are assigned to a new group for the final ten rounds. The method of assigning subjects to new groups for the final block varies by treatment as explained below.

The structure of the turnaround game played follows from a pair of basic design choices. First, the firm's technology has a weak-link structure. As described by Kremer (1993), for many organizations the individual (or unit) doing the worst job - the "weak link" - determines the overall productivity of an organization. Imposing a weak-link structure creates a worst-case scenario for escaping a productivity trap since a unanimous increase in effort is necessary to improve group output. Presumably many organizations face more forgiving environments where positive change is easier, but if we can understand how to escape productivity traps in tough environments, our insights should carry over to less difficult circumstances.

Second, we assume the firm can observe group output, equivalent to observing minimum effort in the absence of noise, but cannot observe any individual employee's effort level. It therefore cannot implement an incentive system based on individual effort rather than group output. Presumably it is neither more nor less realistic to make individual effort unobservable, but this assumption simplifies the environment while increasing the difficulty of escaping a productivity trap.[7]

At the beginning of each ten-round block, an incentive contract is announced for the group. This contract specifies a subject's compensation in experimental currency units (ECUs) as a function of the minimum effort across the four subjects in the group. A contract consists of a flat base wage (W) that each subject receives regardless of the outcome of the game and a bonus rate (B) that determines how much additional pay each subject receives per unit increase in the minimum effort. Higher values of B provide greater incentives to coordinate at high effort levels.

The incentive system in our experiment is a linear revenue sharing scheme. Straightforward revenue sharing systems are common in practice and well-suited to our experimental design. (1) This is a very simple incentive system. Subjects can easily understand how the incentives work as well as the implications of their assignment to an incentive contract. (2) There is an obvious link between the resolution of strategic uncertainty and workers' choices between incentive contracts. Workers should be willing to accept lower fixed wages in exchange for a higher value of B if and only if they believe that higher values of B are associated with a higher probability of coordination at high effort levels. This link is central to our experimental design.

In each round, the subjects simultaneously choose effort levels, where E_i is the effort level chosen by the i^{th} subject ($i \in \{1, 2, 3, 4\}$). Effort choices are restricted to be in ten-unit increments: $E_i \in \{0, 10, 20, 30, 40\}$. To make effort costly, payoffs are reduced by 5 ECUs per unit of effort expended. The payoff π_i for subject i is given by the following equation:

$$\pi_i = W - 5E_i + (B \times \min_{j \in \{1,2,3,4\}} \{E_j\}).$$

If $B > 5$, as is always the case in our experiments, the resulting stage game is a weak-link coordination game. Coordination by all four subjects on any of the five available effort levels is a Nash equilibrium. The five equilibria are Pareto ranked. The most desirable equilibrium is to coordinate *at the highest possible effort (i.e. efficient coordination).*

Two different types of contracts are used in our experiment. Contract 1 always offers $W = 300$ and $B = 6$. The payoff matrix in the top panel of Table 1 results from Contract 1. Contract 2

[7]Along similar lines, we have eliminated communication either between the firm and employees or among employees. This design choice simplifies the environment and increases the difficulty of escaping a productivity trap. Presumably there exist field setting in which communication is relatively easy and others in which it is more difficult.

increases the bonus rate from $B = 6$ to $B = 10$. Thus, Contract 2 features higher incentives to coordinate at high effort levels than Contract 1. The precise manner in which W is determined for Contract 2 is a treatment variable as explained in Section 2.2. For a given base wage W, the payoff matrix resulting from Contract 2 is shown in the bottom panel of Table 1.

TABLE 1: PAYOFF MATRICES BY CONTRACT

CONTRACT 1

		Minimum Efforts by Subjects of the Group				
		0	10	20	30	40
Effort by Subject i	0	300	-	-	-	-
	10	250	310	-	-	-
	20	200	260	320	-	-
	30	150	210	270	330	-
	40	100	160	220	280	340

CONTRACT 2

		Minimum Efforts by Subjects of the Group				
		0	10	20	30	40
Effort by Subject i	0	W	-	-	-	-
	10	$W - 50$	$W + 50$	-	-	-
	20	$W - 100$	W	$W + 100$	-	-
	30	$W - 150$	$W - 50$	$W + 50$	$W + 150$	-
	40	$W - 200$	$W - 100$	W	$W + 100$	$W + 200$

NOTES: Contract 1 offers a base wage $W = 300$ and a bonus factor $B = 6$. For Contract 2, W is the base wage determined by the Auction, and the bonus factor is $B = 10$. When the base wage $W \in (140, 300)$, the payoff from coordinating at 0 is always higher in Contract 1 and the payoff from coordinating at 40 is always higher in Contract 2.

Contract 1 features low incentives for coordination at a high effort level. Subjects get a riskless payoff of 300 ECUs by choosing 0. Attempting to coordinate at higher effort levels is risky with little potential upside. For simplicity, assume that all subjects choose either 0 or 40. Relative to a choice of 0, increasing effort to 40 incurs a sunk effort cost of 200 ECUs in exchange for a potential net gain of only 40 ECUs. For this to have positive expected value, the probability of *all* of the three other group members choosing 40 must be greater than 5/6.

Contract 2 offers higher incentives to take the risk of trying to coordinate at a higher effort level. Consider again the situation facing a subject choosing between 0 and 40. Relative to a choice of 0, choosing 40 still incurs a sunk effort cost of 200 ECUs, but the potential net gain is increased to 200 ECUs. The probability of *all* of the other group members choosing effort level 40 only needs to be 1/2 for choosing 40 to yield higher expected payoff than choosing 0.

We did not restrict *a priori* the base wage of Contract 2 to be in the range of $140 < W < 300$, but the realized values were always within this range. These two conditions imply that the payoff from coordinating at 0 was always higher in Contract 1 than in Contract 2 and the payoff from coordinating at 40 was always higher in Contract 2 than in Contract 1.

2.2 Design and Treatments: We began all sessions by gathering measures of individual characteristics. We used the method of Eckel and Grossman (2008) to measure risk attitudes (see Appendix B for a full description).

Subjects were also asked to complete a questionnaire collecting information on their cognitive and demographic characteristics. As a measure of intelligence, we used self-reported SAT scores (mathematics and comprehensive).[8] The SAT is the primary college entry exam in the United States. There is high correlation between SAT scores and IQ scores (Frey and Detterman, 2004). We measured a subject's personality traits using a brief version of the Big-Five Personality Test provided in Gosling, Rentfrow, and Swann (2003). Survey questions from Glaeser, Laibson, Scheinkman, and Soutter (2000) were used to measure how trusting/trustworthy a subject was. We also gathered basic demographics (age, gender, race, and major).

Because beliefs play a critical role in coordination games, we considered eliciting beliefs prior to Blocks 1 and 2. We chose not to do so due to the risk of demand-induced effects. Forcing subjects to think about others' future actions could have stimulated more sophisticated reasoning including strategic anticipation.

After all of the individual measures were gathered, subjects played Block 1 (Rounds 1 - 10) of the turnaround game. Subjects were randomly assigned to groups of four, which remained fixed

[8] Some subjects did not take the SAT, but had ACT scores. For those subjects, we converted their score using the SAT-ACT Concordance Chart provided on http://www.collegeboard.org. Reflecting local IRB policies, subjects were given the option of skipping questions to protect their privacy. There were few missing values for other questions, but a substantial fraction of the subjects (31%) chose not to report an SAT/ACT score. We discuss below how the analysis reflects these missing values.

for the block. All subjects in all sessions were assigned to Contract 1 in Block 1. Given the weak incentives to raise effort above 0, we anticipated that play would converge to low effort levels. The goal was to generate a productivity trap that subjects would need to escape from in Block 2. Achieving efficient coordination in a weak-link game is a challenging task, and having to overcome a history of low effort makes it even harder.

Prior to the start of Block 2 (Rounds 11 - 20) of the turnaround game, subjects were assigned to either Contract 1 or Contract 2, new groups were formed with subjects assigned to the same contract, and W was set for Contract 2. The four treatments varied how this was done.

In the **Auction** treatment, the subjects' assignment into contracts and the base wage W of Contract 2 was based on a reverse English auction. This clock auction is strategically equivalent to a sealed bid uniform price auction and shares the property of having a dominant strategy (bid or drop out at your reservation value), but is less prone to mispricing than the sealed bid auction.[9] While auctions are used in many real world markets, including some labor markets, they are not an especially common market mechanism for labor markets.[10] Nevertheless, clock auctions have a number of advantages for our experiments. They are easy to implement in a lab setting, quickly equilibrate, and make the selection process transparent for subjects (giving strategic anticipation a better chance of having an effect). In a broader sense, the auction has two critical features: workers must sacrifice fixed wages (W) to obtain higher performance pay (B) *and* workers are aware that they and others made a choice whether or not to accept lower fixed wages in exchange for higher performance pay. Any reasonable mechanism for a labor market should share these features, so our qualitative conclusions should not strongly depend on our choice of a market mechanism.

At the beginning of the auction, W was set to 400 ECUs with 400 seconds on the clock. The clock then ticked down towards zero. The base wage was reduced by 5 ECUs every five seconds (400, 395, 390, etc). We used discrete changes to give subjects time to react before W dropped again. At any given time, subjects could press a button labeled "Contract 1." As soon as a subject clicked on this button, he/she was immediately assigned to Contract 1. If there were N subjects

[9]Experimental evidence (Kagel and Levin, 1993) shows that subjects tend to use the dominant strategy in English auctions but not in the equivalent Vickrey auction.

[10]A number of online labor markets use auctions to set wages. An example is http://www.freelancer.com, the largest outsourcing marketplace with 15.6 million freelancers and almost 8 million projects posted. An employer posts a project and freelancers make bids. The employer decides on a freelancer based on the price and ratings.

in the session, the auction continued until $N/2$ subjects had dropped out by pressing the Contract 1 button. The remaining $N/2$ subjects were assigned to Contract 2 with the value of W where the last dropout took place. Subjects saw the payoff tables under Contracts 1 and 2 throughout the auction, with the Contract 2 table adjusting to reflect changing values of W. At no point during the auction or subsequently were subjects given any information about dropouts prior to the final dropout. After the auction was completed, subjects assigned to Contract 2 only knew the value of W where the final dropout took place. Once subjects were assigned to contracts, they were randomly placed into four-person groups with others assigned to the same contract.

Depending on the show-up rate of subjects, we had either 16 or 24 subjects in each session of the Auction treatment. The realized base wages of Contract 2 are summarized in Table 2, broken down by session size.

Table 2: Realized Base Wages in Contract 2

	16-Subject Sessions	24-Subject Sessions
Base Wages (in ECUs)	180, 210	190, 195, 225, 235

Prior to the auction, our detailed instructions stressed the relationship between W and the payoff table with Contract 2 and provided comparisons between the payoff tables of the two contracts. The goal was to have subjects understand the relationship between dropping out or not and their payoff table for Block 2. Subjects also watched a short movie demonstrating how the auction mechanism worked and participated in an unpaid practice round played against computerized opponents with randomly determined dropout times.[11]

In Block 2 of the Auction treatment, we attribute differences in behavior between Contracts 1 and 2 to three possible sources: (1) Direct Incentive Effect: subjects have higher incentives to coordinate at the efficient outcome under Contract 2 than Contract 1. (2) Selection Effect: subjects assigned to Contract 2 by the auction may have inherently different characteristics than those assigned to Contract 1. Selection could occur along many dimensions (e.g. risk preferences, personality traits) but initial beliefs are particularly germane given the importance of beliefs in coordination games. Contract 2 is relatively attractive to "optimists", defined as subjects who are

[11]Dropout times were distributed uniformly over the range [0, 400]. Subjects knew that the dropouts in the practice round were randomly determined and contained no useful information.

optimistic about the chance of efficient coordination *independent of the auction outcome*, implying that optimists are relatively more willing to accept a lower base wage in order to obtain Contract 2. It follows that the auction tends to sort optimists into Contract 2 and pessimists (defined as subjects who are pessimistic about the chance of efficient coordination independent of the auction outcome) into Contract 1. (3) Effect of Strategic Anticipation: subjects who anticipate that individuals assigned to Contract 2 by the auction are likely to be optimists become more optimistic when assigned to Contract 2 by the auction and more pessimistic when assigned to Contract 1. This optimism/pessimism *depends on the auction outcome*. Strategic anticipation can only have an effect when subjects are given information that makes it possible to anticipate selection due to the auction.

In the **Random Assignment** treatment, subjects were randomly assigned to new groups for Block 2. The groups were then randomly assigned to either Contract 1 or Contract 2, with a 50/50 split between contracts imposed in each session. To maintain parallelism between the Auction and Random Assignment treatments, we matched the session sizes and base wages of Contract 2 in the Random Assignment sessions to those used in the Auction treatment sessions.

The instructions gave subjects no indication there was anything systematic about how groups or contracts were assigned. Subjects were told that the groups had changed, and the instructions stressed that they were almost certainly not with the same people as in Block 1. As for the contract, subjects were told, "You will be assigned to a new contract in Block 2. This may be a different contract than the one you had in Block 1, or it may be the same." Selection cannot be present in the Random Assignment treatment and subjects had no reason to believe that their contract assignment told them anything about the type of people in their group. *Differences between Contracts 1 and 2 in the Random Assignment treatment can be attributed solely to the direct incentive effect.*

Differences between the Random Assignment and Auction treatments capture the difference between exogenous and endogenous assignment to contracts. The **Sort** treatment separates this difference into selection and strategic anticipation effects by assigning subjects into contracts through a mechanism designed to mimic (imperfectly) the selection occurring in the Auction treatment. Specifically, we used data from the Auction treatment to generate a model predicting each subject's dropout time as a function of his/her individual characteristics and his/her choice in Round 1 of Block 1. Specifics of this model are given below in Section 2.4. After Block 1, subjects in the Sort treatment were ranked from the highest to the lowest predicted dropout time. The clock in the auction showed the time *remaining*, so subjects with high dropout times dropped out and took Contract 1 while those with low dropout times stayed in and were assigned to Contract 2. In the Sort treatment, subjects ranked 1st, 2nd, 3rd and 4th were assigned to Group 1, subjects ranked 5th, 6th, 7th and 8th were assigned to Group 2, and so on. Groups 1, 2, and 3 were assigned to Contract 1 (Groups 1 and 2 for sessions with only 16 subjects) and Groups 4, 5, and 6 were assigned to Contract 2 (Groups 3 and 4 for sessions with only 16 subjects). In other words, subjects who were predicted to have chosen Contract 1 if they had participated in the Auction treatment were assigned to Contract 1 in the Sort treatment, and subjects who were predicted to have chosen Contract 2 in the Auction treatment were assigned to Contract 2.

Subjects in the Sort treatment received the same information as subjects in the Random Assignment treatment about how groups and contracts were assigned. This information was intentionally vague, giving subjects no indication there was anything systematic about how groups or contracts were assigned. The goal was to mimic (imperfectly) the selection present in the Auction treatment while disabling strategic anticipation. *The difference between the Sort and Random Assignment treatment gives a lower bound on the size of the selection effect.*[12]

The Auction and Sort treatments use different methods of assigning subjects to groups conditional on their assigned contract. By sorting subjects into groups within contracts, we can use the differences between groups within contracts as an additional measure of the impact of selection on Block 2 of the Sort treatment.[13]

The **Reverse Sort** treatment is designed to illustrate the power of selection. This treatment was identical to the Sort treatment, except subjects were assigned to the contract they were *not* predicted to get. In other words, subjects predicted to get Contract 1 in the Auction treatment were assigned to Contract 2 in the Reverse Sort treatment and subjects predicted to get Contract 2 in the Auction treatment were assigned to Contract 1 in the Reverse Sort treatment. Strategic anticipation should play no role in the Reverse Sort treatment and the selection effect should

[12]Dal Bó, Foster, and Putterman (2010) faced a similar methodological challenge of separating selection effects from other effects of endogenous assignment in their study of the effects of democracy. We could have used a similar mechanism to separate selection effects from the effects of strategic anticipation, but this has major drawbacks in our experiment. The largest problem is that their method would cause an enormous loss of power given the structure of our experiment, necessitating a vastly larger sample size. For a detailed discussion of this issue, see Appendix A.

[13]In the companion paper, we use a structural model to study the effect of the assignment method (random or sorted within contract). Simulation exercises indicate that the assignment method has little effect on our results.

increase effort for Contract 1 and reduce effort for Contract 2. Since the selection effect counteracts the direct incentive effect, the difference between Contracts 1 and 2 is expected to be narrowed in Block 2 relative to the Random Assignment treatment, or possibly even reversed.

2.3 Procedures: All experimental sessions were conducted in the XS/FS computer lab of Florida State University using the software z-Tree (Fischbacher, 2007). All FSU undergraduates were eligible to participate, although subjects were drawn primarily from students taking social sciences classes (economics, political science, and sociology). Subjects' recruitment was done using the software ORSEE (Greiner, 2015), and subjects were allowed to participate in *only* one session. Subjects were guaranteed \$10 for arriving on time. Average earnings per participant were \$17.32 including the show-up fee, and sessions typically took 45 - 60 minutes. Each treatment had six sessions and 128 subjects (two sessions with 16 subjects and four with 24 subjects).

At the beginning of each session, subjects were randomly seated. Instructions (see Appendix B) were read aloud by the experimenter prior to each stage of the experiment, and subjects were given a short comprehension quiz.

At the beginning of each round, subjects were shown the base wage, the bonus rate, and the resulting payoff-matrix. It was common knowledge that all subjects of a given group faced the *same* contract, and that the group and contract were fixed throughout the ten-round block. While viewing this information, subjects were asked to pick an effort level for the round. These choices were made simultaneously, so subjects did not know the effort choices of the others in their group before making their own choices. After all group members had made decisions, subjects were shown a summary of the round's results including the minimum effort, their payoff for the round, and their cumulative earnings. They were also shown, sorted from low to high, the effort levels that all the subjects of the group had chosen. Subjects were provided with a summary of results from the previous rounds of the block.

At the end of the session, each subject was paid via check the earnings for all rounds played plus the \$10 show-up fee. Payment was done on an individual and private basis.

2.4 Predicting Dropout Times: The Sort and Reverse Sort treatments required us to predict when subjects would have dropped out of the auction if they had been in the Auction treatment. This was done in two steps. First, we ran a regression, fitting the times subjects dropped out (i.e. pressed the "Contract 1" button) in the Auction treatment as a function of their individual characteristics and their first choice in the turnaround game (i.e. before they interacted with other subjects). A Tobit model was used since the dropout times are censored. Technical details of the model are given in Appendix A. In the second step, the fitted parameters for this Tobit model were used to generate predicted dropout times for subjects in the Sort and Reverse Sort treatments. After the first round of play in the turnaround game (Block 1) was complete, a research assistant entered data into a program that generated predictions, and subjects were sorted into groups for Block 2 using these predicted dropout times. Each stage of the experiment used a separate z-Tree program, so an experimenter could enter information about groupings into the Block 2 program while Block 1 was running. The data entry and calculations were sufficiently rapid that subjects observed no delays prior to the start of Block 2.[14]

Table 4 reports the results of two regressions, with Table 3 providing a brief description of the independent variables. Model 1 gives the results of the Tobit regression used to predict dropout times. By far the most important variable is the dummy for subjects who chose 40 in the first round of the turnaround game. The parameter estimate is large and significant at the 1% level. Consistent with the Tobit results, subjects who chose 40 in the first round were more than twice as likely to be assigned to Contract 2 versus Contract 1 (69% vs. 31%). The extroversion component of the Big 5 and the dummy variable for subjects who reported math scores also have significant effects, but only at the 10% level.[15]

[14]This was the main reason for not including the Round 10 choice in the Tobit model. Waiting until the end of Block 1 to start entering information would have caused a long delay before the beginning of Block 2. If the Round 10 choice is added to the Tobit model along with the Round 1 choice, then both variables are statistically significant but the ability to predict which contract a subject is assigned only improves slightly. A secondary concern was that endogeneity of the Round 10 choice could bias our estimates.

[15]As noted previously, about a third of subjects chose not to report scores. The regressions include a dummy for subjects who did report a score as well as a slope parameter. The missing scores were set equal to the median reported score. Subjects who did not report a math score were less likely to be assigned to Contract 2 in the Auction treatment (36% vs. 58%).

Table 3: Tobit Regression Independent Variables

Variables	Explanation	Data Source
$RISK$	Choice of option in the Risk Aversion Test	Risk Aversion Test
$MATH$	SAT Math Score if reported	Questionnaire
I_{MATH}	Indicator function: "=1" if Math Score reported & "=0" otherwise	Questionnaire
$SCORE$	SAT Comprehensive Score if reported	Questionnaire
I_{SCORE}	Indicator function: "=1" if Comp. Score reported & "=0" otherwise	Questionnaire
$EXTROVERT$	Big 5 Personality Test: Extroversion	Questionnaire
$AGREEABLE$	Big 5 Personality Test: Agreeableness	Questionnaire
$CONSCIENTIOUS$	Big 5 Personality Test: Conscientiousness	Questionnaire
$EMOTIONSTABLE$	Big 5 Personality Test: Emotional Stability	Questionnaire
$OPENNESS$	Big 5 Personality Test: Openness	Questionnaire
$FAIR$	Trust Test: "Would most people try to be fair?"	Questionnaire
$HELP$	Trust Test: "Would most people try to be helpful?"	Questionnaire
$TRUST$	Trust Test: "Can most people be trusted?"	Questionnaire
$TRANSFER$	Trust Test: "Should personal income be determined by work?"	Questionnaire
$STRANGER$	Trust Test: "Can you count on strangers?"	Questionnaire
$TRUSTME$	Trust Test: "Am I trustworthy?"	Questionnaire
$EFFORT_l$	Effort Choice Dummies in round 1 of Block 1 & $l \in \{10, 20, 30, 40\}$	Block 1
$CONS$	Constant	

Table 4: Regression Models to Predict Dropout Times

	Model 1	Model 2
$RISK$	2.802	0.055
	(4.779)	(0.075)
$MATH$	0.147	0.008***
	(0.167)	(0.003)
I_{MATH}	−30.279*	0.253
	(16.972)	(0.303)
$SCORE$	0.041	−0.002*
	(0.051)	(0.001)
I_{SCORE}	−5.000	−0.038
	(19.168)	(0.326)
$EXTROVERT$	−13.668*	−0.212*
	(7.469)	(0.112)
$AGREEABLE$	4.210	−0.084
	(7.050)	(0.098)
$CONSCIENTIOUS$	−6.422	0.067
	(8.099)	(0.117)
$EMOTIONSTABLE$	1.315	0.097
	(8.224)	(0.114)
$OPENNESS$	−1.718	0.036
	(8.023)	(0.114)
$FAIR$	−10.029	−0.252*
	(9.034)	(0.140)
$HELP$	6.356	0.178
	(8.241)	(0.142)
$TRUST$	−12.462	0.092
	(8.499)	(0.143)
$TRANSFER$	−6.977	0.101
	(9.509)	(0.155)
$STRANGER$	−2.737	0.040
	(9.765)	(0.164)
$TRUSTME$	18.686	−0.095
	(12.067)	(0.145)
$EFFORT_{10}$	−18.824	
	(35.701)	
$EFFORT_{20}$	−23.875	
	(30.918)	
$EFFORT_{30}$	−24.710	
	(28.812)	
$EFFORT_{40}$	−101.631***	
	(27.084)	
$CONS$	141.473	−2.435
	(135.634)	(1.897)

Notes: Model 1 uses a Tobit regression to predict dropout times in the Auction Treatment. Model 2 uses a probit model to predict the probability of choosing effort 40 in the 1^{st} round of Block 1. All regressions are based on 128 observations in the Auction Treatment. Standard errors are given in parentheses. Three (***), two (**), and one (*) stars indicate statistical significance at the 1%, 5%, and 10% respectively.

The measures gathered prior to Block 1 have little predictive power in Model 1 for dropout times. One possible explanation is that these measures act on dropout times through their effect on the probability of choosing 40 in the first round. Model 2 supports this argument. This is a Probit model where the dependent variable is the dummy for choice of 40 in the first round of Block 1 and the independent variables are the individual measures gathered before Block 1. There is a strong positive relationship between math scores and choosing 40, and a number of the other measures also have weakly significant relationships with the likelihood of choosing 40. Somewhat to our surprise, risk aversion has minimal effect in Models 1 and 2. There is no indication that the auction sorts individuals based on their risk preferences.

To check how well the model replicates contract assignment by the auction, we compared the contract assignments predicted by the Tobit model with the actual contract assignments in the Auction treatment. The model correctly predicts the contract assignment for 69% of the subjects (88/128). Among subjects for whom our model incorrectly predicts the contract assignments, most are close to indifferent with 65% having predicted dropout times within 30 seconds of the cutoff to switch to the other (correct) contract. As an alternative method of checking how well the model predicts contract assignment by the auction, we fit Model 1 using three sessions from the Auction treatment and then predicted the contract assignments in the other three sessions. The prediction is correct for 72% of the subjects (46/64). Overall, the model does a reasonable job of replicating the contract assignment that occurred in the Auction treatment.

Subjects could decline to answer many of the questions asked prior to Block 1. For the six questions relating to attitudes towards fairness and trust, we systematically made an error in the coding when subjects chose not to answer.[16] This caused 16 subjects to be flipped between contracts (6 in the Sort treatment and 10 in the Reverse Sort treatment). Since the number of subjects getting each contract is fixed within a session, flips necessarily took place in pairs where one subject who should have gotten Contract 1 got Contract 2 and vice versa. These flips affected a small percentage of the subjects (6%) and had little effect on characteristics by contract since flipped subjects tended to be close to the cutoff. The regressions reported in Table 5 (Model 2) include controls for the sorting mistakes. These have little impact on the results.

[16] All six questions used a Likert scale over how much subjects disagreed or agreed with a statement. They also had an option labeled "prefer not to answer." Averaging across the six questions, 2.2% of the subjects responded "prefer not to answer." These responses should have been coded as neutral responses, but instead were coded as strong disagreement due to our error. The error was only noticed after the fact.

2.5 Hypotheses: The difference between effort under Contract 2 and Contract 1 in the Random Assignment treatment is due to the direct incentive effect. The effect of selection is also present in the Sort treatment, and strategic anticipation effects as defined in Section 2.2 are present (along with the direct incentive and selection effects) in the Auction treatment. All three effects are expected to work in the same direction, leading to higher effort under Contract 2 than Contract 1.

H1: Average effort in Block 2 will be higher for Contract 2 than Contract 1 in the Random Assignment, Sort, and Auction treatments.

Compared with effort under Contract 1 in the Random Assignment treatment, effort in Contract 2 reflects only the direct incentive effect. The Sort treatment adds in the selection effect and the Auction treatment adds in the effect of strategic anticipation for Contract 2. All of these effects are positive, so effort under Contract 2 should be increasing from Random Assignment to Sort to Auction. In the Reverse Sort treatment, the direct effect of incentives is present but the selection effect is reversed. Given that pessimists tend to get Contract 1 rather than Contract 2 in the Auction treatment, the assignment process in the Reverse Sort treatment should tend to assign pessimists to Contract 2 rather than Contract 1. Instead of getting the direct incentive effect *plus* the selection effect, as in the Sort treatment, effort in the Reverse Sort treatment is affected by the direct incentive effect *minus* the selection effect. This implies that effort under Contract 2 should be decreasing as we move from Random Assignment to Reverse Sort.

H2: Average effort in Block 2 for Contract 2 will be increasing in the following order across treatments: Reverse Sort, Random Assignment, Sort, and Auction.

Our predictions about differences between treatments under Contract 1 are a flipped version of our hypotheses for Contract 2, given that the direct incentive effect, selection effect, and strategic anticipation effect are all expected to *reduce* effort under Contract 1.

H3: Average effort in Block 2 for Contract 1 will be decreasing in the following order across treatments: Reverse Sort, Random Assignment, Sort, and Auction.

H1, *H2*, and *H3* are all ordinal in nature. Our experimental design is largely concerned with measuring the magnitude of the predicted effects, especially the difference between the Sort and Random Assignment treatments relative to the difference between the Auction and Random Assignment treatments. We had no *ex ante* hypotheses about the size of these differences as they depend on the fraction of optimists, pessimists, and individuals with strategic anticipation in the population. These are purely empirical matters.

Likewise, *H2* and *H3* jointly imply that the difference between Contracts 1 and 2 in Block 2 should be smaller in the Reverse Sort treatment than in the Random Assignment treatment. While we suspected the sign of the difference would flip, with Contract 1 yielding higher effort than Contract 2, we had no reason to formally hypothesize this outcome in the absence of data.

3 Results

Section 3.1 briefly describes the results from Block 1 and Section 3.2 gives an overview of the experimental results from Block 2. Section 3.3 contains analysis establishing the statistical significance of the various effects discussed in Section 3.2.

3.1 Block 1: All groups played Block 1 using Contract 1. The goal was to establish a history of low effort that must be overcome in Block 2. We largely achieved this goal as 73% of groups had zero minimum effort in the 10th round and 61% of individuals chose an effort level of zero. Subjects were randomly assigned to treatments, and there were no significant differences between treatments in Block 1.[17]

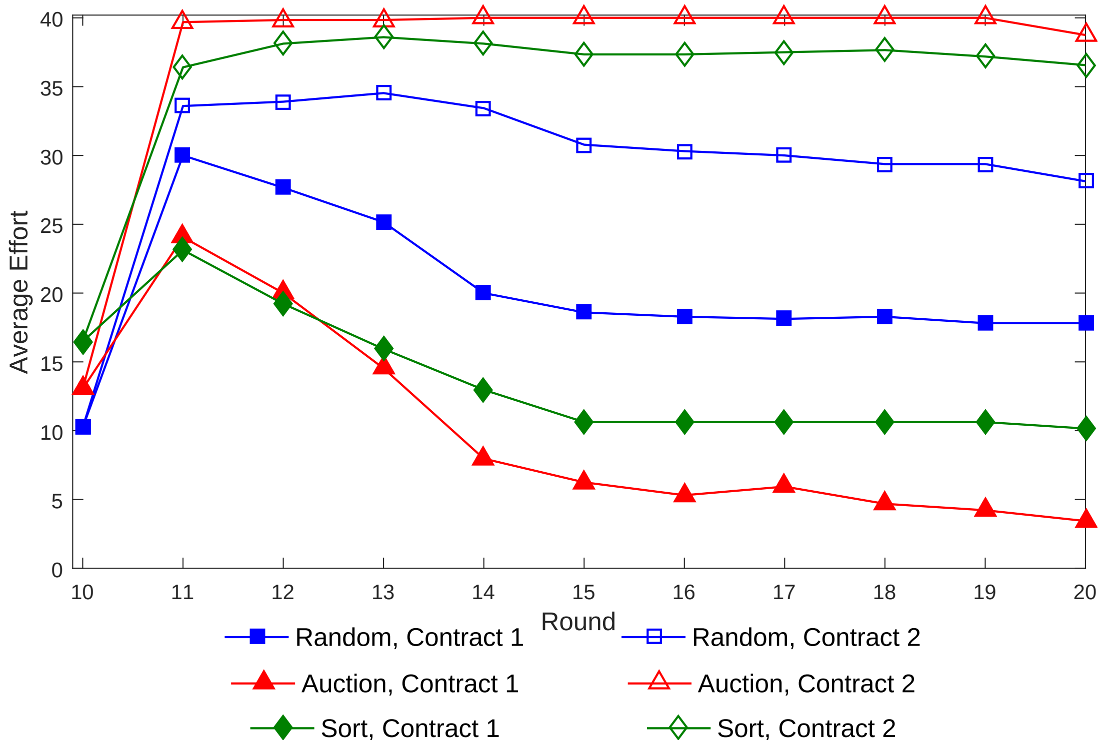
3.2 Block 2: Figure 1 compares average effort in Block 2 for the Random Assignment, Auction, and Sort treatments. Data from the final round of Block 1, Round 10, are included to give a better sense of how the incentive contracts change behavior.

In the Random Assignment treatment, groups assigned to Contract 2 performed modestly better than those assigned to Contract 1, consistent with *H1*. This difference increased sharply with endogenous assignment to incentive contracts in the Auction treatment. Groups assigned to Contract 2 in the Auction treatment achieved nearly perfect coordination at the efficient equilibrium, with all four group members choosing effort level 40 in 96% of the observations. The equivalent figure in the Random Assignment treatment was 54%. On the flip side, groups assigned to Contract 1 in the Auction treatment did terribly with a minimum effort of zero in 87%

[17]This is based on Wilcoxon rank-sum tests. There is a single observation per group giving the group's average effort across Rounds 1 - 10.

of the observations as compared to 57% in the Random Assignment treatment. The effects of the Auction treatment are consistent with *H2* and *H3*.

FIGURE 1: COMPARISON OF RANDOM ASSIGNMENT, AUCTION AND SORT TREATMENTS IN BLOCK 2



Comparing the performance under Contracts 1 and 2 in the Auction treatment illustrates a critical point. If we look solely at groups assigned to Contract 2, it appears that endogenous assignment to contracts has a strong positive effect. Looking at *all* of the groups paints a different picture, as the positive effect for groups assigned to Contract 2 is almost perfectly offset by the negative effect for groups assigned to Contract 1. Averaging across both contracts, the average effort in Block 2 is almost identical in the Random Assignment and Auction treatments (26.3 vs. 24.7). To appreciate the effect of endogenous assignment of incentive contracts, we have to understand the effect for groups that choose high performance pay *as well as groups that do not.*

Conclusion 1. *The positive effect of high performance pay is larger with endogenous assignment to contracts. This is offset by an increased negative effect for groups assigned low performance pay, so the overall effect of endogenous assignment to incentive contracts is neutral.*

The Sort treatment attempts to replicate the selection process of the Auction treatment while eliminating effects due to strategic anticipation. Even though this replication is obviously imperfect, the effect of the Sort treatment is similar to the effect of the Auction treatment. As in the Auction treatment and consistent with *H2*, groups assigned to Contract 2 performed better in the Sort treatment than in the Random Assignment treatment. For 78% of the observations, all four group members chose effort level 40. For Contract 1, effort is lower for the Sort treatment than Random Assignment (consistent with *H3*). This does not reflect a big increase in groups with a minimum effort of zero (61% vs. 57%) but instead shows up as a large decrease in incidents of efficient coordination (16% vs. 40%). Pooling across contracts, the positive effect for Contract 2 is largely offset by the negative effect for Contract 1, so average effort is almost the same for the Random Assignment and Sort treatments (26.3 vs. 25.5).

The Sort treatment gives a lower bound on how much of the effect of endogenous assignment to high performance pay is due to selection. For groups assigned to Contract 2, the average efforts over Block 2 are 31.3, 37.5, and 39.8 in the Random Assignment, Sort, and Auction treatments, respectively.[18] It follows that selection accounts for *at least* 73% of the increased performance with endogenous assignment to high performance pay.[19] We can also decompose the total effect of high performance pay into the direct effect of increased incentives (difference between Contracts 1 and 2 in the Random Assignment treatment), selection (difference between Sort and Random Assignment treatments for Contract 2), and strategic anticipation (difference between Auction and Sort treatments for Contract 2).[20] The average effort over Block 2 with Contract 1 is 21.2 in the Random Assignment treatment. The proportions of the effect on average effort of high performance pay that can be assigned to the direct incentive effect, selection, and strategic anticipation are 54%, 33%, and 12%, respectively. This presumably overestimates the fraction of the effect due to strategic anticipation and underestimates the fraction due to selection.

Conclusion 2. *Roughly three quarters of the effect of making high performance pay endogenous is due to selection.*

Figure 2 illustrates the power of sorting. Recall that subjects in the Sort treatment were put

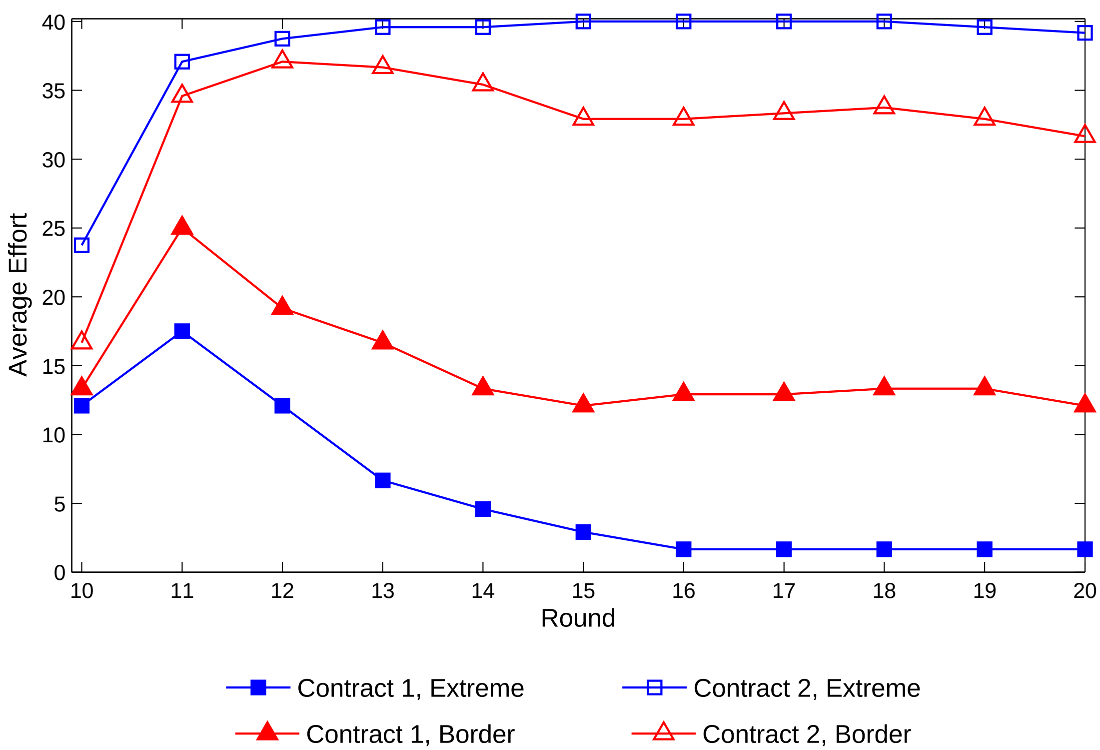
[18]The total effect of endogenous assignment is given by the difference between the Auction and Random Assignment treatments (39.8 - 31.3). The portion due to selection is bounded below by the difference between the Sort and Random Assignment treatments (37.5 - 31.3). The ratio of these two values gives the proportion of the improvement due to selection (73%).

[19]The analogous value for low performance pay (Contract 1) is 67%.

[20]The correct baseline is Contract 1 in the Random Assignment treatment in Block 2, not Block 1. Groups are re-matched in Block 2, leading to a small increase in effort for Contract 1 in the Random Assignment treatment. We want to isolate effects that are due to changes in the incentive contract rather than restart effects.

into groups by predicted dropout time. Within a contract, the different groups only differed in how strongly we predicted they would have gotten the same contract in the Auction treatment. Any differences between these groups were purely due to selection. Figure 2 separates data from groups that were on the border between the two contracts and groups that were at the extremes in the Sort treatment (i.e. most likely to be assigned to Contract 1 or Contract 2). The extreme groups have lower average effort than the border groups for Contract 1, and the fraction of observations with a minimum effort of 0 rises from 52% for border groups to 80% for extreme groups. The effect is reversed for Contract 2. Extreme groups have higher average effort and the frequency of all members choosing 40 rises from 62% for border groups to 85% for extreme groups. This illustrates how powerful selection can be in isolation, and shows that our attempt to sort subjects into groups picks up something which is relevant for game play.

Figure 2: Comparison of Border and Extreme Groups in Block 2



The Reverse Sort treatment is intended to drive home the power of selection. We intentionally created a situation that presumably never occurs in field settings, assigning subjects who would most likely have been assigned to Contract 1 in the Auction treatment to Contract 2 and vice versa. The selection effect now works in the opposite direction of the direct incentive effect. If the selection effect is sufficiently strong, the Reverse Sort treatment should yield *lower* effort in Contract 2 than Contract 1.

Figure 3: Comparison of Reverse Sort and Random Assignment Treatments in Block 2

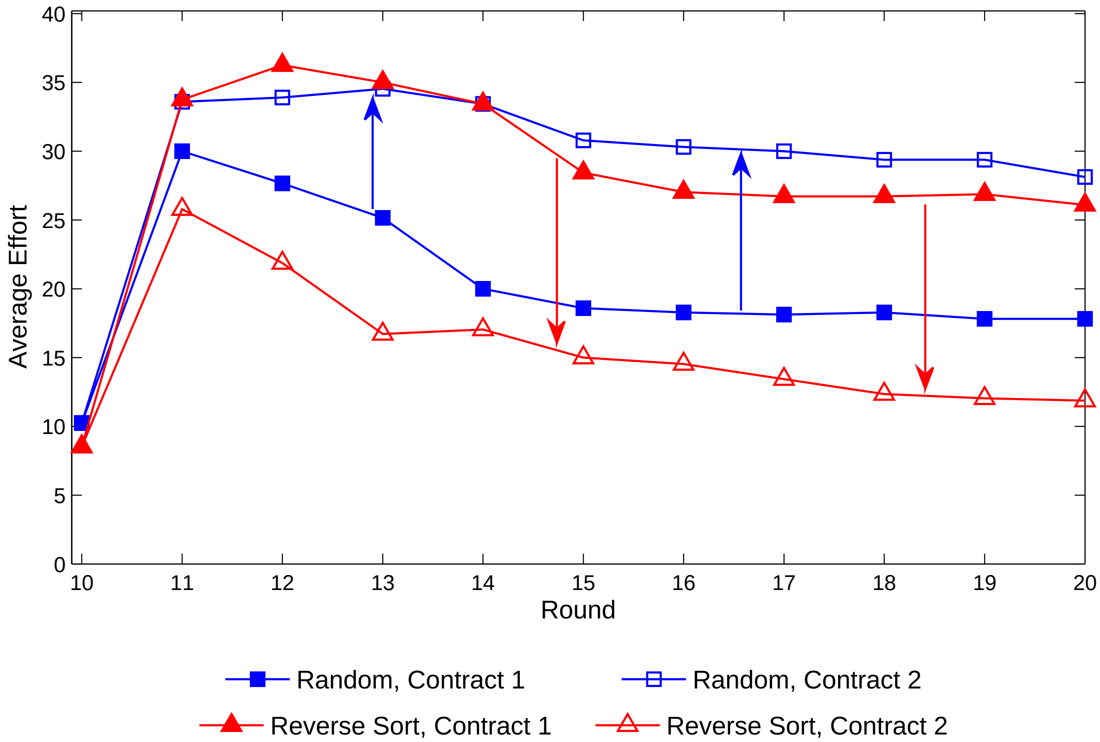


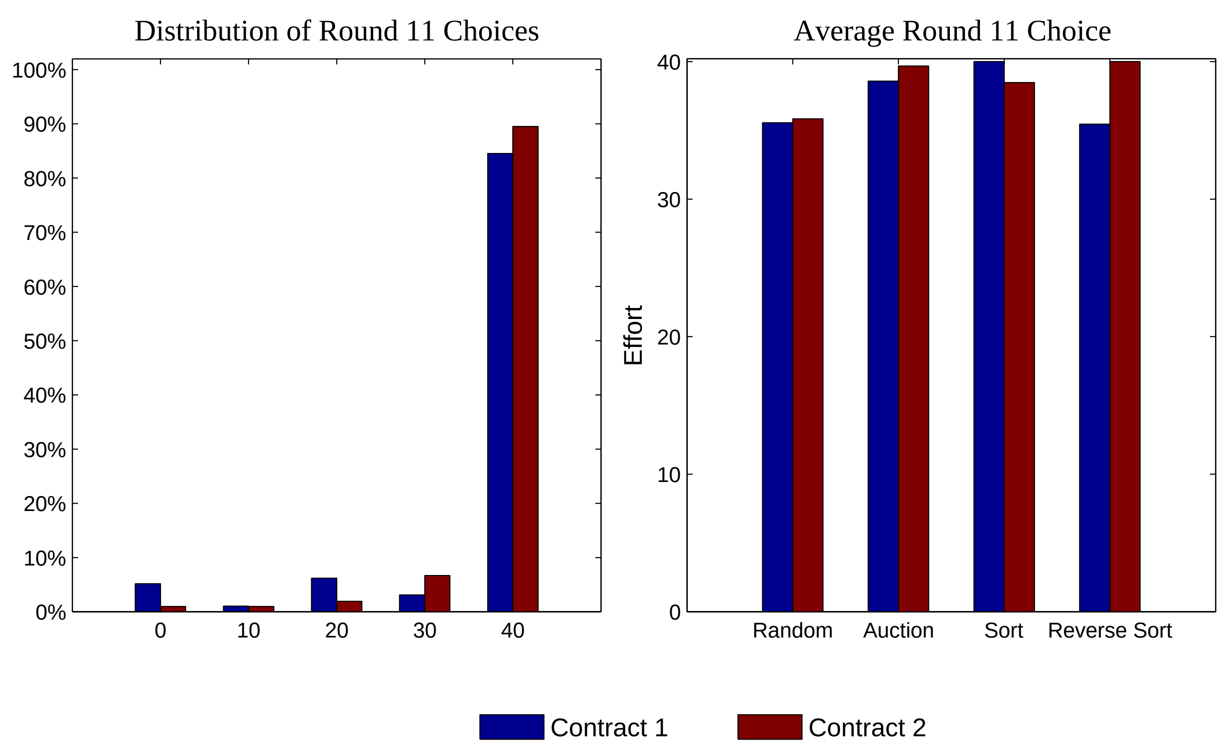
Figure 3 compares outcomes for the Random Assignment and Reverse Sort treatments. Colored arrows have been added, pointing from Contract 1 to Contract 2, to make it easier to see the difference between the two incentive contracts by treatment. In Random Assignment, where only the direct incentive effect is present, effort is higher under Contract 2. This is reversed in the Reverse Sort treatment. The selection effect is sufficiently strong that effort is higher under Contract 1 in spite of lower incentives to coordinate. Across Block 2, efficient coordination occurs in 54% of the observations of Contract 1 as opposed to only 18% of Contract 2. Likewise, the group's minimum effort is zero in 61% of the observations of Contract 2 versus only 28% of Contract 1.

Conclusion 3. *The direct effect of increased incentives to coordinate with Contract 2 is reversed in the Reverse Sort treatment by assigning subjects to the contract that they were* ***not*** *predicted to get in the Auction treatment.*

To understand why selection is so powerful, recall from Table 4 that Round 1 choices, particularly choice of 40, were by far the best predictor of dropout times. The tendency of the auction to assign subjects who chose 40 in Round 1 to Contract 2 is important because those subjects were extremely likely to also choose 40 in Round 11 regardless of their circumstances.

Figure 4 illustrates this point. The data for this figure come from all subjects that chose 40 in Round 1. The left hand panel displays the distribution of their choices in Round 11, broken down by the contract they were assigned in Block 2. For either contract, subjects who chose effort level 40 in Round 1 almost always chose 40 in Round 11. This is not because they were failing to learn across Block 1. Most groups had low minimum efforts by the end of Block 1, and even the subset of initially optimistic subjects learned to adjust their choices during the course of Block 1. Only 36% chose 40 in Round 10, and only 10% chose something other than 0 in Round 10 if they were in a group with a minimum effort of 0. Rather, these subjects returned to their initial optimism when the game restarted with a new group and, possibly, a new contract. The right hand panel demonstrates how general this phenomenon was. It reports average effort in Round 11 broken down by treatment and contract. In all cases the average is close to 40.

FIGURE 4: ROUND 11 BEHAVIOR BY SUBJECTS CHOOSING 40 IN ROUND 1



Subjects who chose 40 in Round 1 were far more likely to choose 40 in Round 11 than other subjects (85% vs. 39% in Contract 1 and 90% vs. 56% in Contract 2).[21] Initial optimism is not transient but instead seems to represent a subject's type. The auction tended to assign subjects who chose 40 in Round 1 to Contract 2, leaving others behind in Contract 1.

3.3 Regression Analysis: This subsection addresses the statistical significance of the treatment effects discussed in Section 3.2. The results of two regressions are reported in Table 5. The dependent variable is a group's average effort across Block 2. Each group is treated as a single observation (for a total of 128 observations) since effort choices from the same group are highly correlated across individuals and rounds. Every group member chose 40 for all ten rounds of Block 2 in almost a quarter of the groups, so we use a Tobit model to correct for this censoring. The model includes a session-level random effect to correct for session effects.

Turning to the explanatory variables, the omitted category is the Random Assignment treatment under Contract 1. The Contract 2 dummy captures the difference between Contracts 1 and 2 for the Random Assignment treatment. Interactions between a Contract 1 dummy and treatment dummies (Contract 1 × Auction, Contract 1 × Sort, and Contract 1 × Reverse Sort) capture treatment effects for Contract 1 using the Random Assignment treatment under Contract 1 as the base. Likewise, interactions between a Contract 2 dummy and treatment dummies (Contract 2 × Auction, Contract 2 × Sort, and Contract 2 × Reverse Sort) capture treatment effects for Contract 2 using the Random Assignment treatment under Contract 2 as the base. The top panel of Table 5 reports parameter estimates and the bottom reports additional estimated differences between treatments.

Model 1 tests for treatment effects with no additional controls. The difference between Contracts 1 and 2 in the Random Assignment treatment is significant at the 5% level and all of the treatment effects are significant at the 10% level or better. In the bottom panel, the difference between the Sort and Auction treatments is significant under Contract 2 but not Contract 1. An important feature of our data is that effort is lower with Contract 2 than Contract 1 for the Reverse Sort treatment. This difference is significant at the 1% level.

[21] Based on Probit regressions, we find that the likelihood of choosing 40 in Round 11 did not vary significantly across choices 0 - 30 in Round 1, but jumped significantly if 40 was chosen in Round 1.

Table 5: Regression Analysis of Treatment Effects

	Model 1	Model 2
Parameter Estimates		
Contract 2	9.630**	9.683**
	(4.716)	(4.495)
Contract 1 × Auction	−13.132***	−12.145**
	(4.913)	(5.008)
Contract 2 × Auction	19.611***	19.974***
	(5.800)	(5.794)
Contract 1 × Sort	−9.369*	−11.315**
	(4.915)	(4.961)
Contract 2 × Sort	8.442*	10.125**
	(5.052)	(5.109)
Contract 1 × Reverse Sort	8.815*	12.006**
	(4.967)	(5.393)
Contract 2 × Reverse Sort	−15.636***	−18.487***
	(4.917)	(5.369)
Round 1 Effort, Session Average		0.366
		(0.554)
# of Mistaken Assignments to		−6.188
Contract 2 in Sort / Contract 1 in Reverse Sort		(4.575)
# of Mistaken Assignments to		13.152**
Contract 1 in Sort / Contract 2 in Reverse Sort		(5.304)
Other Differences Between Treatments		
Contract 1×Auction - Contract 1×Sort	−3.763	−0.829
	(4.861)	(4.924)
Contract 2×Auction - Contract 2×Sort	11.169*	9.849*
	(5.872)	(5.832)
Contract 2×Reverse Sort - Contract 1×Reverse Sort	−14.821***	−20.810***
	(4.704)	(4.978)

Notes: All regressions, based on 128 observations, are Tobit regressions with session-specific random effects. The dataset contains 24 sessions and 128 groups (using Block 2 data only). Standard errors are given in parentheses. Three (***), two (**), and one (*) stars indicate statistical significance at the 1%, 5%, and 10% respectively.

Due to a mistake in coding missing values (see Section 2.4), a small number of subjects were accidentally assigned to the wrong contract in the Sort and Reverse Sort treatments. To correct for this, Model 2 adds a variable giving the number of group members who were accidentally assigned to Contract 2 (Contract 1) in the Sort (Reverse Sort) treatment and a variable giving the number of group members accidentally assigned to Contract 1 (Contract 2) in the Sort (Reverse Sort) treatment. The parameter estimates for the two new variables in Model 2 have the expected signs and are jointly significant at the 5% level ($\chi^2 = 8.20$; $p = .017$).[22] As an additional control for session effects, Model 2 adds the average Round 1 effort, *by session*, as a new variable. This control has the expected positive sign but is not significant and has little impact on our conclusions. Comparing Model 2 to Model 1, the estimated parameters change little, indicating that the assignment mistakes do not qualitatively affect the treatment effects.

Because increases with Contract 2 are largely offset by decreases with Contract 1, average effort across both contracts is roughly the same for all four treatments. Not surprisingly, none of the differences between treatments is significant when we average across contracts. This can be seen by running a regression where the only independent variables are dummies across the two contracts for the Auction, Sort, and Reverse Sort treatments. None of the treatment dummies approaches significance at standard levels ($p = .752$, .867, and .409, respectively).

Conclusion 4. *The regression analysis largely confirms that the treatment effects described in Section 3.2 are statistically significant. The notable exception is the difference between the Sort and Auction treatments under Contract 1. We confirm H1, H2, and H3.*

4 Conclusion

The primary purpose of this paper was to investigate how endogenous assignment to incentive contracts affects the efficacy of high performance pay as a tool for escaping a productivity trap. The impact on groups that receive such performance pay is much greater when assignment is endogenous, with groups achieving almost-perfect coordination at the efficient equilibrium. However,

[22] To understand the expected signs, consider a subject who should have been assigned Contract 1 but is accidentally assigned Contract 2 in the Sort treatment. This subject should tend to choose low effort levels in Block 2, pulling down effort for his/her entire group. This implies that the expected sign for "# of Mistaken Assignments to Contract 2 in Sort / Contract 1 in Reverse Sort" is negative. The same logic implies a positive sign for the other control variable.

at a global level, this is entirely offset by the negative impact on groups that keep the original incentive contract with low performance pay. Our design makes it possible to identify a lower bound on how much of the increased effect of high performance pay with endogenous contract assignment is due to selection rather than strategic anticipation. We find that almost three quarters of the increase is due to selection, a value that presumably would be larger if not for our imperfect replication of the selection process in the Auction treatment. The effect of selection is sufficiently strong that it can overcome the direct effect of incentives, as manipulating the assignment process in the Reverse Sort treatment generates higher productivity with *low* performance pay.

While a number of studies have found that selection accounts for a high proportion of the effect to incentive pay, the flavor of our results is rather different. In the existing literature, selection has an effect by choosing people who are good at performing some task - solving puzzles, multiplying numbers, etc. In our experiment, selection chooses people who are optimistic about coordinating at efficient outcomes (or believe that others are optimistic). Selection is primarily based on beliefs rather than abilities.

A surprising finding from our experiments is the minuscule total effect, averaging over both contracts, of endogenous assignment to incentive contracts. The underlying issue is that assigning more optimists to Contract 2 necessarily means assigning more pessimists to Contract 1. If we were giving a manager advice, it is clear we would suggest using higher performance pay, but it does not follow that all firms can achieve efficient coordination by choosing high performance pay. If all firms offer high performance pay, no selection takes place and, by extension, strategic anticipation has no role to play. It follows that universal adoption of high performance pay improves productivity relative to universal use of low performance pay, but only by the same amount as under *exogenous* assignment to high performance pay.

To understand the economic implications of the preceding observations, note that our experiments model a "closed" labor market meaning that workers can easily move between firms within the market, but cannot easily enter and exit the market. High performance pay works in large part by attracting optimists to a firm. In a closed market, those optimists must come from other firms within the industry. This implies that a firm's decision to adopt high performance pay imposes a negative externality on other firms in the market. Increased performance pay is not sufficient for *all* firms to achieve efficient coordination in a closed market. This suggests that other mechanisms that do not rely on selection may be more attractive.

The preceding analysis does not apply to an "open" market where workers can easily move into and out of the market. Optimists can move into the market and pessimists can move out in response to changing incentive contracts. It is far more likely that all firms in an open market can achieve efficient coordination via adoption of high performance pay. We conjecture that many labor markets are closed in the short run and open in the long run.[23]

Our experiments take advantage of the control available in the laboratory. The insights we could draw from our data would be limited without our Sort or Reverse Sort treatments, but it is hard to imagine a real world setting that would mimic these treatments. A natural question is how our results apply to labor markets in the field. Real labor markets are far more complex than our lab experiments. In the simple environment of our experiment, the data suggests that workers generally do not take into account the implications of endogenous assignment into incentive contracts and the resulting effects of selection. What are the odds that workers in more complex field settings understand these subtle issues? This implies that strategic anticipation plays an even smaller role in field settings than in the lab.

[23]For example, consider a market containing jobs that require a large amount of specialized training. In the short run it is hard to train new workers and those who leave the market sacrifice the value of their specialized skills. In the long run more workers can be trained and brought into the market.

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Coordination with Endogenous Contracts:
Incentives, Selection, and Strategic Anticipation

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April, 2017

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APPENDIX A: Details of the Sort Treatment

A.1 Tobit Model to Predict Drop Out Time

We use a Tobit regression model to fit the time of subjects choosing to push "Contract 1" button in the Auction treatment. Let $TIME^*_{i,s}$ be the latent dependent variable corresponding to the time at which a subject i in session s would have pushed the "Contract 1" button if the auction clock never stops. Let $X_{i,s}$ denote a vector of observable characteristics of a subject i in session s. The notation and variables used in $X_{i,s}$ are summarized in Table 3 of the manuscript.

We allow $TIME^*_{i,s}$ to be linearly dependent on $X_{i,s}$ via a vector of parameters ψ. We assume the error terms are normally distributed, with $F(\cdot)$ being the cumulative distribution function and $f(\cdot)$ being the probability density function of a standard normal distribution with standard deviation σ.

We only observe $TIME^*_{i,s}$ for those who actually chose Contract 1 in the experiment. Let $CUTOFF_s$ denote the cutoff time at which the last subject in session s pressed the "Contract 1" Button. We define the observable dependent variable $TIME_{i,s}$ for a subject i participating in session s to be:

$$TIME_{i,s} = \begin{cases} TIME^*_{i,s} & \text{if } TIME^*_{i,s} > CUTOFF_s \\ CUTOFF_s & \text{otherwise .} \end{cases}$$

We can also define an indicator function $I(TIME_{i,s})$

$$I(TIME_{i,s}) = \begin{cases} 0 & \text{if } TIME_{i,s} = CUTOFF_s \\ 1 & \text{otherwise} \end{cases}$$

Then given the number of subjects in each session N_s, we can construct and estimate the likelihood function as:

$$L(\theta) = \prod_s \prod_{i=1}^{N_s} \left(\frac{1}{\sigma} f\left(\frac{TIME_{i,s} - X_{i,s}\psi}{\sigma} \right) \right)^{I(TIME_{i,s})} \left(1 - F\left(\frac{X_{i,s}\psi - CUTOFF_s}{\sigma} \right) \right)^{1-I(TIME_{i,s})} .$$

Estimated values and standard errors of the parameters ψ are reported in Table 4 of the manuscript.

A.2 Alternative Design to Identify Selection Effects

Dal Bo, Foster, and Putterman (2010) are interested in a different issue than us (the effect of democratic selection of institutions), but face a similar methodological problem. They want to separate the direct effect of democracy from a selection effect. Their solution to this problem is elegant - each group holds an election over what payoff table to use (prisoners dilemma or coordination game), but it is random whether the result of the election is implemented.

We considered a similar approach for our experiment. In this alternative design, all subjects participate in Block 1 and then in the auction as in the Auction treatment. We then flip a virtual coin. In half the sessions, the outcome of the auction is ignored and groups for Block 2 are formed randomly as in the Random Assignment. The base wage for Contract 2 is drawn from the auction. In the remaining sessions, we implement the outcome of the auction as in the Auction treatment. Subjects are told whether contract assignment was random or determined by the outcome of the auction (otherwise strategic anticipation cannot be identified). To determine the effect of selection, we compare Block 2 effort levels for groups formed via the auction to groups with randomly assigned members who would all have been assigned to the same contract by the auction. In other words, we compare groups assigned to Contract 2 (Contract 1) via the auction with groups where all of the members would have been assigned to Contract 2 (Contract 1) if the outcome of the auction had been implemented.

The alternative design has one major advantage. Rather than estimating to which contract the auction would have assigned a subject, we observe this directly. Our Sort treatment imperfectly replicates the selection mechanism in the Auction treatment, but this is not an issue using the alternative design. However, because our experimental setting differs along a number of dimensions from the setting of Dal Bo et al., adapting their method of group assignment to our experiment has a number of disadvantages. In descending order of importance, these are the following:

1. The relevant unit of observation in our experiment is a group of four subjects. In our Auction treatment, all groups contain four subjects that the auction assigned to the same contract. Given the nature of a weak link game, the composition of a group should matter in a non-linear fashion. To get clean identification of the selection effect, we need to compare groups in the auction treatment with groups in the random assignment treatment that contain four subjects who would have been assigned to the same contract in the auction. This implies that

only 1/16 of the data from the random assignment treatment can be used for comparisons. Without an unrealistically large sample, the experiment has little explanatory power.

To see this point, consider the following example. Suppose we have 320 groups (1280 subjects!). 160 groups have contracts assigned via the auction, 80 for each contract. The other 160 groups are assigned randomly, 80 per contract. The comparison of random assignment and endogenous assignment via the auction uses all of the groups and has no shortage of power. But to separate the selection effect from the effect of strategic anticipation, we can only use groups where all four members are randomly assigned to the same contract they would receive if the outcome of the auction is implemented. Of the 80 groups randomly assigned to Contract 1, 5 groups (on average) would have all four members be individuals that would have been assigned to Contract 1 by the auction. Of the 80 groups randomly assigned to Contract 2, 5 groups (on average) would have all four members be individuals that would have been assigned to Contract 2 by the auction. The tiny fraction of usable groups yields little power to separate the selection effect from the effect of strategic anticipation in any statistically meaningful way.

To understand why we have a problem when Dal Bo et al. do not, notice that in Dal Bo et al. a group of four subjects democratically assigned to one payoff table can contain 0, 1 , or 2 subjects who voted for the other payoff table. In our experiment, subjects assigned to a group of four subjects by the auction either all dropped out and were assigned Contract 1 or all did not drop out and were assigned to Contract 2. The endogenously formed groups in Dal Bo et al. are heterogeneous in a way that our endogenously formed groups are not. This difference in designs creates a power problem in our experiment that does not exist in their experiment.

2. Unlike the election, participating in the auction is informative even if the outcome is not implemented. Observing the market price carries information about the expectations of others. Casting a vote gives you no information about others preferences or beliefs.[1]

3. The base wage in the auctions is determined endogenously. Even if we get groups randomly assigned to Contract 1 (Contract 2) where all of the members would have been assigned to

[1] We could eliminate this problem by using a sealed bid auction. This, however, creates a different and even more severe problem since overbidding is prevalent in sealed bid Vickrey auctions, but not the equivalent English auctions (Kagel and Levin, 1993).

Contract 1 (Contract 2) by the auction, it is unlikely that the base wages will match. This exacerbates the power problem described above.

As is often the case with experimental designs, there is no perfect solution and the experimenters must pick their poison. The Dal Bo et al. approach to correcting for selection has disadvantages within our experiment than did not exist in their environment. Given that our method tends to underestimate the effect of selection, making our main conclusion somewhat conservative, we felt the benefits of our approach outweighed the benefits of the Dal Bo et al. approach.

APPENDIX B: Experimental Instructions

B.1. Stage 1

The purpose of this experiment is to study how people make decisions in a particular situation. The experiment consists of **three** parts to be described at the appropriate time. Your earnings will depend upon the decisions you make, as well as the decisions that other people make. Your accumulated earnings in Experimental Currency Units (ECUs) will be converted into dollars at an exchange rate of 1,000 ECUs = $1. For your participation in the experiment, you will receive an initial payment of 10,000 ECUs. At the end of the session, you will be paid in cash your total earnings. None of the other participants will be informed of your earnings, and likewise you will not be informed of the earnings of others. The instructions are simple, yet if you have a question, please raise your hand. Aside from these questions, any communication with other participants, or looking at other participants' screens is not permitted and will lead to your immediate exclusion from the experiment.

B.1.1. Risk Aversion Test: In this part of the study you are asked to choose one of the five options shown below. Regardless of which option you choose, there are two possible outcomes (Outcome A and Outcome B). These outcomes are equally likely for all five options - there is a 50% chance of Outcome A and a 50% chance of Outcome B, just like the flip of a coin. The options differ only in how much each outcome pays. The table below tells you how much you will be paid for each outcome. For example, if you choose Option 2, you will earn $0.70 from Outcome A and $1.75 from Outcome B. If you choose Option 4, you will earn $0.30 from Outcome A and $2.75 from Outcome B. The computer will randomly choose between Outcome A and Outcome B at the end of the experiment. You can imagine the computer flipping a virtual coin so that the chance of each outcome is equal. You will only find out your outcome from Part 1, and how much you will be paid for Part 1 at the end of the experiment. Please choose your option by clicking on a radio button.

Option	Outcome	Payoff	Probabilities
1	A	$1.00	50%
	B	$1.00	50%
2	A	$0.70	50%
	B	$1.75	50%
3	A	$0.50	50%
	B	$2.25	50%
4	A	$0.30	50%
	B	$2.75	50%
5	A	$0.10	50%
	B	$3.25	50%

B.1.2. Questionnaire: In this part of the study you will complete a questionnaire. The questionnaire asks you to answer some questions about yourself. You will always have the option to not answer a question if you don't wish to provide some information. Failure to provide information will NOT affect your ability to participate in the remainder of the experiment. Please note that your individual data will be kept strictly confidential.

1. What is your age? (Please leave this blank if you do not wish to share your age.)

2. What is your gender?

   ○ Male ○ Female ○ Prefer not to answer

3. What do you consider your racial background?

   ○ White ○ Black ○ Hispanic ○ Asian ○ Other ○ Prefer not to answer

4. What is your major? (Please leave this blank if you do not wish to share your major.)

5. Have you served in a leadership position for an organization?

   ○ Yes ○ No ○ Prefer not to answer

6. If yes, can you tell us the most important position you have held?

7. Have you ever had a job that required you to supervise others?

○Yes ○No ○ Prefer not to answer

8. Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?

○ Would take advantage of me ○ Would try to be fair ○ Prefer not to answer

9. Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?

○ Try to be helpful ○ Just look out for themselves ○ Prefer not to answer

10. Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?

○ Most people can be trusted ○ Can't be too careful ○ Prefer not to answer

11. "Personal income should be determined by work."

○ Disagree strongly ○ Disagree somewhat ○ Agree somewhat ○ Agree strongly
○ Prefer not to answer

12. "You can't count on strangers anymore."

○ More or less agree ○ More or less disagree ○ Prefer not to answer

13. "I am trustworthy."

○ Disagree strongly ○ Disagree somewhat ○ Disagree slightly
○ Agree slightly ○ Agree somewhat ○ Agree strongly ○ Prefer not to answer

Below we ask you some questions on your performance on some standardized examinations. If you did not take one of these tests, or do not remember your score, or do not wish to share your score, please leave the gap blank. You are free to not share your score. You may continue to participate in the experiment and your earnings will not be affected.

14. What was your SAT score in Mathematics?

15. What was your SAT Composite score in all three sections; that is, Critical Reading, Writing, and Mathematics?

16. What was your ACT score in Mathematics?

17. What was your ACT Composite score; that is, the whole number average of English, Mathematics, Reading, and Science Reasoning?

Here are a number of personality traits that may or may not apply to you. Please write a number next to each statement to indicate the extent to which you agree or disagree with that statement. You should rate the extent to which the pair of traits applies to you, even if one characteristic applies more strongly than another.

Please use the following codes: 1. Disagree strongly; 2. Disagree moderately; 3. Disagree a little; 4. Neither agree or disagree; 5. Agree a little; 6. Agree moderately; 7. Agree strongly.

I see myself as:

a. Extroverted, enthusiastic

b. Critical, quarrelsome

c. Dependable, self-disciplined

d. Anxious, easily upset

e. Open to new experiences, complex

f. Reserved, quiet

g. Sympathetic, warm

h. Disorganized, careless

i. Calm, emotionally stable

j. Conventional, uncreative

B.2. Stage 2

This part of the experiment consists of blocks of 10 periods each. The conditions of the experiment are identical **within** each block. Sometimes though, there will be changes **between** blocks; in these cases, we will promptly inform you of any changes. In each period you will be part of a group of 4 participants (called *employees* from now on). The group is called *the firm*. So, there are 4 employees within a firm. Given that nobody will know the identity of the employees of a firm, all the decisions you make during the experimental session will be anonymous.

As an employee in this firm, you will be asked to decide how to split your week of 40 working hours between Activity A and Activity B. Since the hours that you do not assign to one activity are automatically assigned to the other activity, you will only be asked to decide how many hours you devote on Activity A. Given that the available choices are to devote 0, 10, 20, 30 or 40 hours on Activity A, this implies devoting 40, 30, 20, 10 or 0 hours on Activity B, respectively.

Your payoff will depend on (1) the base wage, (2) the cost of the effort, and (3) on a bonus. The base wage is a fixed amount of ECUs that you will receive regardless of your choice and the choices of others. The *bonus* is a function of the minimum (lowest) number of hours devoted on Activity A by any employee of the firm and a bonus factor. The *cost of the effort* is a function of the number of hours you devote on Activity A and the effort-cost per hour. The effort-cost per hour is **fixed** throughout the entire experimental session at 5 ECUs per hour. On the other hand, the base wage and the bonus factor are chosen by the central server of the lab, which simulates the decisions of the firm's management. Before making any decision, you will receive information about the value of the base wage and the value of the bonus factor. More specifically, the payoff of the i^{th} employee in each period is given by the following formula:

$$\text{Payoff of Employee } i = \text{Base Wage} - (5 \times H_i) + (\text{Bonus Factor} \times min_{\text{Activity A}})\,,$$

where H_i is the number of hours devoted by the i^{th} employee of the firm on Activity A, and $min_{\text{Activity A}}$ is the lowest number of hours that the employees of the firm devoted on Activity A.

You do not need to memorize this formula. The computer program will provide the payoff tables whenever you need to make a decision.

Please note that your firm of 4 employees will be **the same** for the entire duration of the block. At the time when you make your decision you will not know what the other three employees in your firm have chosen, but after each period, the number of hours that the employees of your firm have devoted on Activity A will be shown from low to high. In addition, please note that at no point in time the identity of the other employees in your firm will be identified. In other words, the actions you take in this experiment will remain confidential just like those of the other employees.

Examples

(i) Using the payoff table below, let us assume that you choose to devote 20 hours on Activity A, and that the other three employees of your firm decide to devote on Activity A 30, 30, and 40 hours, respectively. What is the minimum number of hours devoted on Activity A by the employees of the firm? What is your payoff?

		Minimum Effort by Employees of the Firm				
		0	10	20	30	40
	0	300	-	-	-	-
Effort	10	250	310	-	-	-
by	20	200	260	320	-	-
Employee i	30	150	210	270	330	-
	40	100	160	220	280	340

Answer: The minimum number of hours that an employee of the firm devotes to Activity A is 20 hours. Thus, your payoff is 320 ECUs.

(ii) Now, assume that you choose to devote 30 hours on Activity A, and that the other three employees of your firm decide to devote on Activity A 30, 20, and 40 hours, respectively. What is the minimum number of hours devoted on Activity A by the employees of the firm? What is your payoff?

Answer: The minimum number of hours that an employee of the firm devotes on Activity A is 20 hours. Your effort is 30 hours. Thus, your payoff is 270 ECUs.

We will now take a short quiz to make certain you understand the rules for this block of the experiment.

<u>Quiz</u>

Question 1: Suppose that the base wage is set at 300 ECUs, and the bonus factor at 6.

		Minimum Effort by Employees of the Firm				
		0	10	20	30	40
	0	300	-	-	-	-
Effort	10	250	310	-	-	-
by	20	200	260	320	-	-
Employee i	30	150	210	270	330	-
	40	100	160	220	280	340

Using the payoff table provided, assume that you decide to devote 40 hours on Activity A. The other three employees of your firm choose to devote on Activity A 30, 30, and 40 hours, respectively. What is the minimum number of hours devoted on Activity A by the employees of the firm? What is your payoff?

Answer: The minimum number of hours that an employee of the firm devotes on Activity A is *30*. My payoff in ECUs is *280*.

Question 2: Suppose that the base wage is set at 300 ECUs, and the bonus factor is set at 6. Using the payoff table provided, assume that you decide to devote 30 hours on Activity A. The other three employees of your firm choose to devote on Activity A 30, 0, and 40 hours, respectively. What is the minimum number of hours devoted on Activity A by the employees of the firm? What is your payoff?

Answer: The minimum number of hours that an employee of the firm devotes on Activity A is *0*. My payoff in ECUs is *150*.

Question 3: I am grouped with the same individuals for the entire duration of the block.

Answer: True.

Question 4: My actions and payoffs will remain confidential.

Answer: True.

B.2.1. Block 1: From this period on until the end of the block, the management has decided to set the base wage at 300 ECUs and the bonus factor at 6. In each period, your computer screen will indicate the payoff table for the base wage of 300 ECUs and the bonus factor of 6. The payoff table is the following.

		Minimum Effort by Employees of the Firm				
		0	10	20	30	40
	0	300	-	-	-	-
Effort	10	250	310	-	-	-
by	20	200	260	320	-	-
Employee i	30	150	210	270	330	-
	40	100	160	220	280	340

An employee chooses the number of hours devoted on Activity A using the prompts. You can change your choice, as often as you want, but once you click the "OK" button, the decision will be final.

Please recall that your firm of 4 employees will be **the same** for the entire duration of this block. At the time when you make your decision you will not know what the other three employees in your firm have chosen, but after each period, the number of hours that the employees of your firm have devoted on Activity A will be shown from low to high. In addition, you will be shown your payoff for the period and your total profit so far in the block. You will also be provided with a summary of the results of the previous periods of the block.

Please enter your choice.

B.3. Stage 3

B.3.1. Auction Treatment: For Block 2 you will be assigned to one of two possible contracts: Contract 1 or Contract 2. You and the other participants in today's experiment will participate in a mechanism assigning people to play with either Contract 1 or Contract 2. Recall that a contract specifies your base wage and your bonus factor. The base wage is a fixed amount of ECUs that you will receive regardless of your choice and the choices of others. The bonus is a function of the minimum (lowest) number of hours devoted on Activity A by any employee of the firm and a bonus factor. That is, the bonus factor determines how much additional money you earn as the minimum hours spent by an employee of your firm on Activity A increases. Twelve people will play with Contract 1. This is the same contract that was used in Block 1. It has a base wage of 300 and a bonus factor of 6. Contract 2 is a different contract from Contract 1. The bonus factor for Contract 2 will always be 10. This means that the bonus increase from increasing the minimum hours spent by a group member on Activity A is always larger for Contract 2 than for Contract 1. The base wage for Contract 2 is determined at the same time as the assignment to contracts. A description of the mechanism for assigning contracts and choosing the base wage for Contract 2 follows.

On your screen, you will see a clock and a value for the base wage. Both the clock and the base wage start with a value of 400. The clock will begin counting down towards zero. Every ten seconds that pass will cause the base wage to fall by 5 ECUs (400, 395, 390, etc). Your screen will show a payoff table for Contracts 1 and 2. As the base wage falls for Contract 2, the payoff table for Contract 2 will automatically be adjusted. There will be a button on your screen labeled "Contract 1." If you push this button, you are immediately assigned to play with Contract 1 in Block 2. The first twelve people to push this button will play with Contract 1. Once the twelfth person pushes the "Contract 1" button, the remaining twelve people who have not pushed this button are assigned to Contract 2. Their base wage is equal to the value displayed when the twelfth person pressed the "Contract 1" button. They will be using that payoff table shown for Contract 2 when the twelfth person pressed the "Contract 1" button.

Once you have been assigned a contract, you will be placed in a firm with three other people assigned to the same contract as you. This assignment is done randomly, so there is little chance that you are playing with the same people as in Block 1. You will be with your new firm for the duration of Block 2. You will be playing with your assigned contract for the entire block of 10 periods. You will be choosing hours to spend on Activity A, just like you did in Block 1.

If all of this seems a little confusing, don't worry. We will go through a number of examples, as well as a practice round before beginning play of Block 2.

Examples

(i) Let us suppose that the first 11 subjects press the "Contract 1" button as follows: 2 subjects press it when the base wage is at 380 ECUs (20 seconds after the start of the timer), 4 subjects press it at 370 ECUs (30 seconds after the start of the timer), 3 subjects press it at 360 ECUs (40 seconds after the start of the timer), 2 subjects press it at 350 ECUs (50 seconds after the start of the timer). The 12^{th} subject presses the "Contract 1" button at 340 ECUs (60 seconds after the start of the timer). Thus, the first 12 subjects to press the "Contract 1" button will be sorted into the Contract 1 payoff scheme.

		Minimum Effort by Employees of the Firm				
		0	10	20	30	40
	0	300	-	-	-	-
Effort	10	250	310	-	-	-
by	20	200	260	320	-	-
Employee i	30	150	210	270	330	-
	40	100	160	220	280	340

On the other hand, the 12 participants that did not press the "Contract 1" button, will be sorted into the Contract 2 payoff scheme with a base wage of 340 ECUs.

		Minimum Effort by Employees of the Firm				
		0	10	20	30	40
	0	340	-	-	-	-
Effort	10	290	390	-	-	-
by	20	240	340	440	-	-
Employee i	30	190	290	390	490	-
	40	140	240	340	440	540

(ii) Let us suppose that the first 11 subjects press the "Contract 1" button as follows: 2 subjects press it when the base wage is at 380 ECUs (20 seconds after the start of the timer), 4 subjects press it at 370 ECUs (30 seconds after the start of the timer), 3 subjects press it at 360 ECUs (40 seconds after the start of the timer), 2 subjects press it at 350 ECUs (50 seconds after the start of the timer). The 12^{th} subject presses the "Contract 1" button at 300 ECUs (100 seconds after the start of the timer). Thus, the first 12 subjects to press the "Contract 1" button will be sorted into the Contract 1 payoff scheme.

		Minimum Effort by Employees of the Firm				
		0	10	20	30	40
	0	300	-	-	-	-
Effort	10	250	310	-	-	-
by	20	200	260	320	-	-
Employee i	30	150	210	270	330	-
	40	100	160	220	280	340

On the other hand, the 12 participants that did not press the "Contract 1" button, will be sorted into the Contract 2 payoff scheme with a base wage of 300 ECUs.

		Minimum Effort by Employees of the Firm				
		0	10	20	30	40
	0	300	-	-	-	-
Effort	10	250	350	-	-	-
by	20	200	300	400	-	-
Employee i	30	150	250	350	450	-
	40	100	200	300	400	500

(iii) Let us suppose that the first 11 subjects press the "Contract 1" button as follows: 2 subjects press it when the base wage is at 330 ECUs (70 seconds after the start of the timer), 4 subjects press it at 320 ECUs (80 seconds after the start of the timer), 3 subjects press it at 310 ECUs (90 seconds after the start of the timer), 2 subjects press it at 300 ECUs (100 seconds after the start of the timer). The 12^{th} subject presses the "Contract 1" button at 290 ECUs (110 seconds after the start of the timer). Thus, the first 12 subjects to press the "Contract 1" button will be sorted into the Contract 1 payoff scheme.

		Minimum Effort by Employees of the Firm				
		0	10	20	30	40
	0	300	-	-	-	-
Effort	10	250	310	-	-	-
by	20	200	260	320	-	-
Employee i	30	150	210	270	330	-
	40	100	160	220	280	340

On the other hand, the 12 participants that did not press the "Contract 1" button, will be sorted into the Contract 2 payoff scheme with a base wage of 290 ECUs.

		Minimum Effort by Employees of the Firm				
		0	10	20	30	40
	0	290	-	-	-	-
Effort	10	240	340	-	-	-
by	20	190	290	390	-	-
Employee i	30	140	240	340	440	-
	40	90	190	290	390	490

[Video is shown.]

Quiz

Suppose that the first 11 subjects press the "Contract 1" button as follows: 2 subjects press it when the base wage is at 320 ECUs (80 seconds after the start of the timer), 4 subjects press it at 310 ECUs (90 seconds after the start of the timer), 3 subjects press it at 300 ECUs (100 seconds after the start of the timer), 2 subjects press it at 290 ECUs (110 seconds after the start of the timer). The 12^{th} subject presses the "Contract 1" button at 280 ECUs (120 seconds after the start of the timer).

Question 1a: What is the base wage of Contract 1 in ECUs?

Answer: 300.

Question 1b: What is the bonus factor of the Contract 1 payoff scheme?

Answer: 6.

Question 1c: What is the base wage of Contract 2 in ECUs?

Answer: 280.

Question 1d: What is the bonus factor of the Contract 2 payoff scheme?

Answer: 10.

Question 2: How many subjects out of the 24 subjects in the session will earn payoffs with the Contract 1 payoff scheme?

Answer: 12.

Question 3: How many subjects out of the 24 subjects in the session will earn payoffs with the Contract 2 payoff scheme?

Answer: 12.

Question 4: What is the initial value of the base wage of Contract 2 in ECUs before the timer starts the countdown?
Answer: 400.

Question 5: By how many ECUs does the base wage in Contract 2 go down every 10 seconds?
Answer: 10.

Question 6: Your firm, in the next stage, will be composed of employees that chose an identical payoff contract.
Answer: True.

<u>Practice Round</u>

Next, there will be a practice round. In this practice round, you will be playing with the computer. The computer will be clicking the button at random times. Please note that clicking the button early will NOT expedite the enitre procedure given that you will still have to wait for the computer to complete the necessary number of clicks.The base wage determined in the practice round will have NO consequence on your payoffs or choices in the next block.
[Practice Round.]

B.3.2. Other Treatments: You will be assigned to a new contract for Block 2. This may be a different contract than the one you had in Block 1, or it may be the same. The base wage and the bonus factor, along with the payoff table of your new contract will be shown next.

B.4. Stage 4

Your contract in Block 2 will have a base wage of (W) ECUs and a bonus factor of (B). Your contract is shown above. You will be asked next to indicate the number of hours you would like to devote on Activity A for a block of 10 periods, just like you did in Block 1.

The members of your firm have been randomly re-shuffled, so it is very unlikely you are playing with the same three people as in Block 1. However, the members of your firm will have the SAME contract as yours. In other words, they will have the same base wage and the same bonus factor as you have. In addition, please note that your firm of 4 employees will be retained for the entire duration of Block 2.