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

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Abstract

We examine the effects of endogenous assignment to incentive contracts on worker productivity. We find that assignment to high performance pay via a market mechanism is roughly twice as effective as imposing the same contract exogenously. However, this positive effect is almost completely offset by a negative effect for workers that endogenously choose low performance pay. The total effect from endogenous assignment to incentive contracts is virtually zero. We introduce an experimental technique that decomposes the positive effect of endogenous assignment to high performance pay into effects due to selection and strategic anticipation. We find that *at least* 73% of the effect is due to selection. Using a structural model of learning, we estimate that the true proportion of the effect due to selection exceeds 90%.

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

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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, the 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 (henceforth referred to as *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

[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 exclusion of low effort providers (Riedl, Rohde, and Strobel, 2016).

and half are assigned to a new incentive contract with lower fixed pay and larger incentives to coordinate at high effort levels (henceforth referred to as *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 is virtually zero.

Beyond identifying the effects of endogenous assignment to incentive contracts, a major goal of our study is to understand why high incentive 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 (henceforth referred to as *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 also increases 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 *at least* 73% of the improvement with endogenous assignment to high performance pay is due to selection, and estimate that the actual fraction due to selection exceeds 90%.

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!

We use a structural learning model, which combines stochastic fictitious play (Fudenberg and Levine, 1998) with sophisticated learning analogous to sophisticated EWA (Camerer, Ho, and Chong, 2002), to answer two important questions about our data. First, the Sort treatment imperfectly replicates the assignment to contracts by the market mechanism. Simulations of the learning model, using parameters fit from the data, allow us to estimate what the effect of selection would be if the Sort Treatment perfectly replicated the assignment to contracts by the market mechanism. This counterfactual exercise yields the 90% figure cited above, indicating that the actual effect of selection is substantially greater than the 73% figure observed in the data. Second, one of our main experimental findings is that the total effect of endogenous assignment to incentive contracts is minimal, as the positive effect for high performance pay is offset by a matching negative effect for low performance pay. But perhaps the total effect would be positive if all firms were free to offer high performance pay? Simulations of the model indicate that this is not the case. If all firms offer high performance pay, no selection takes place and, by extension, strategic anticipation has no role to play. Therefore, the long-run effect of endogenous assignment to incentive contracts equals the short-run effect of exogenous assignment to 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.[3] 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 - productivity is higher for groups composed of subjects who endogenously choose high performance pay, and selection accounts for most of the effect - but differs from the existing literature along two dimensions. First, the selection mechanism differs due to the critical role played by strategic uncertainty.[4] Rather than ability to perform a task, selection in our design is based on individuals' beliefs. The market mechanism assigns those who are inherently optimistic about the likelihood of coordinating at the efficient outcome to groups with high performance pay. Group productivity generally does not just depend on skill, but also depends on being able to work together. It makes sense that selection takes place along both dimensions. Second, selection to high performance pay does not take place in isolation. In our study, we observe the behavior of subjects who are endogenously assigned to both high *and low* performance pay contracts. A complete description of how endogenous assignment to incentive contracts affects worker performance needs to account for both sides of the coin. The same forces of selection and strategic anticipation that push groups assigned high performance pay towards efficient coordination also lead groups assigned low performance pay to coordinate at inefficient outcomes. This changes our view of endogenous assignment to incentive contracts from overwhelmingly positive to basically neutral.

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 eighteen players bid in an English clock auction for the right to play one round of a 9-player median game. They find that winning bids converge to the payoff from the efficient equilibrium and winning subjects' play in the subsequent median game converges to the efficient equilibrium.[5] Van Huyck et al. attribute the latter result to forward induction, while Crawford

[3]Important experimental papers studying the effects of exogenously-imposed incentive systems include Bull, Schotter, and Weigelt (1987), Nalbantian and Schotter (1997), and van Dijk, Sonnemans, and van Winden (2001).

[4]Dohmen and Falk consider interactive settings (revenue sharing and tournaments) as well as piece rate compensation, but find little difference between outcomes in the interactive and non-interactive settings. Strategic uncertainty does not play an important role in their experiments.

[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

and Broseta (1998) argue that the efficiency result 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 dramatically 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 finding 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. Given the dominant role of selection in our experiments, this implies our main qualitative observations will apply in field settings.

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 uses a learning model to address open questions about the data. Section 5 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

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 coordination game.

[6]The learning process in Van Huyck et al. is 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 the optimistic subjects effect 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.

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.

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. Unlike a pure coordination game, the five equilibria are Pareto ranked. The most desirable 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 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.

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.

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.

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 C 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).[7] The SAT is the primary college entry exam in the

[7] 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.

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 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.[8] While auctions are used in many real world markets, including some labor markets, they are not an especially common market mechanism for labor markets.[9] 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

[8]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.

[9]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.

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 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.[10]

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 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 (i.e. drop out at 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

[10]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.

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.*[11]

[11] 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.

The Auction and Sort treatments use different methods of assigning subjects to groups conditional on their assigned contract. Because we sorted subjects into groups within contracts, the differences between groups within contracts serve as an additional measure of the impact of selection on Block 2 of the Sort treatment. Simulation exercises with the learning model described in Section 4) indicate that the assignment method (random or sorted within contract) has little effect on our results (see Appendix B for details).

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 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 C) 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.[12]

[12]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.

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	
	(135.634)	

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.

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.[13]

The measures gathered prior to Block 1 have little predictive power 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. 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.[14] This caused 16 subjects to be flipped between

[13] 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%).

[14] 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

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 6 (Model 2) include controls for the sorting mistakes. These have little impact on the results.

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

"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.

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

Table 5 summarizes average individual effort, broken down by round and treatment. For Block 2 (Rounds 11 - 20), the data are broken down by contract. 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.[15]

[15]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.

Table 5: Summary of Average Effort

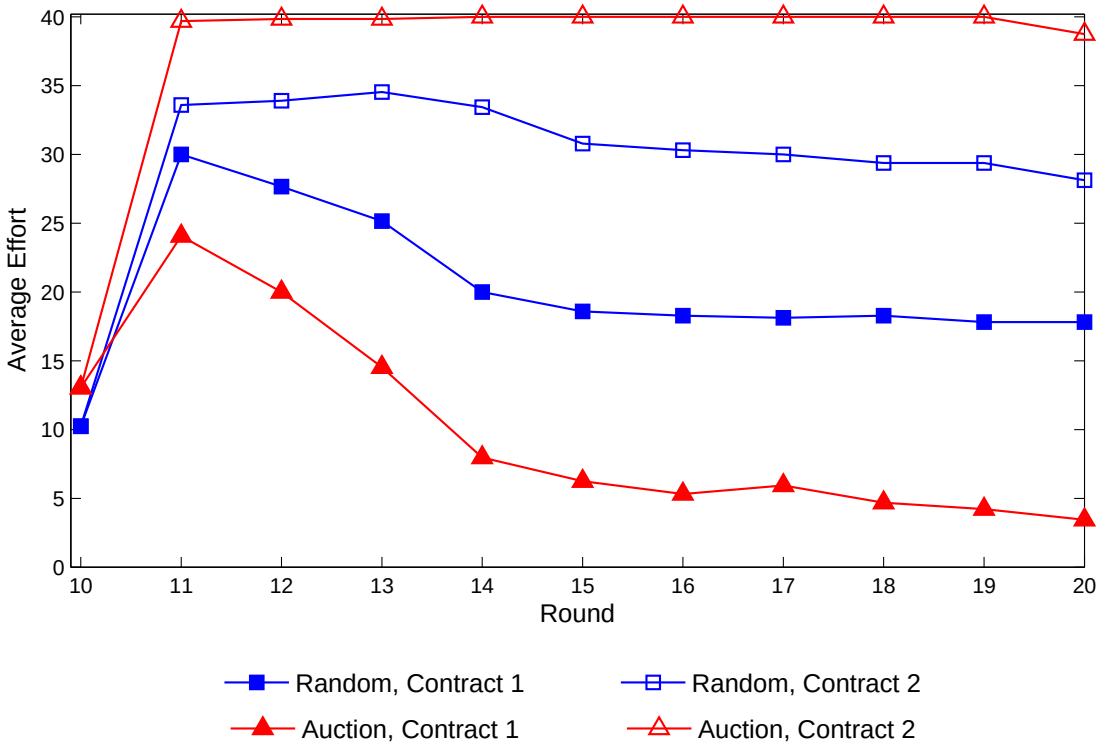
Block 1 (Rounds 1 - 10)				
Round	Random Assignment	Auction	Sort	Reverse Sort
1	28.4	25.9	27.1	25.1
2	25.8	22.3	24.4	22.2
3	22.0	19.2	22.2	20.2
4	17.8	17.0	19.7	17.5
5	14.8	17.0	18.0	13.9
6	13.1	15.2	18.2	11.6
7	12.4	14.3	17.3	9.2
8	11.4	13.8	17.3	8.2
9	10.9	13.7	17.3	8.0
10	10.2	13.0	16.5	8.5

Block 2 (Rounds 11 - 20)								
Round	Random Assignment		Auction		Sort		Reverse Sort	
	Contract 1	Contract 2	Contract 1	Contract 2	Contract 1	Contract 2	Contract 1	Contract 2
11	30.0	33.6	24.1	39.7	23.1	36.4	33.8	25.8
12	27.7	33.9	20.0	39.8	19.2	38.1	36.3	21.9
13	25.2	34.5	14.5	39.8	15.9	38.6	35.0	16.7
14	20.0	33.4	8.0	40.0	13.0	38.1	33.4	17.0
15	18.6	30.8	6.3	40.0	10.6	37.3	28.4	15.0
16	18.3	30.3	5.3	40.0	10.6	37.3	27.0	14.5
17	18.1	30.0	5.9	40.0	10.6	37.5	26.7	13.4
18	18.3	29.4	4.7	40.0	10.6	37.7	26.7	12.3
19	17.8	29.4	4.2	40.0	10.6	37.2	26.9	12.0
20	17.8	28.1	3.4	38.8	10.2	36.6	26.1	11.9

3.2 Summary of Results: The data from Block 2 are consistent with *H1*, *H2*, and *H3*. The critical feature that cannot be predicted without data is the magnitude of differences between treatments.

Figure 1 compares average effort in Block 2 for the Random Assignment and Auction 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.

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



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

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.*

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

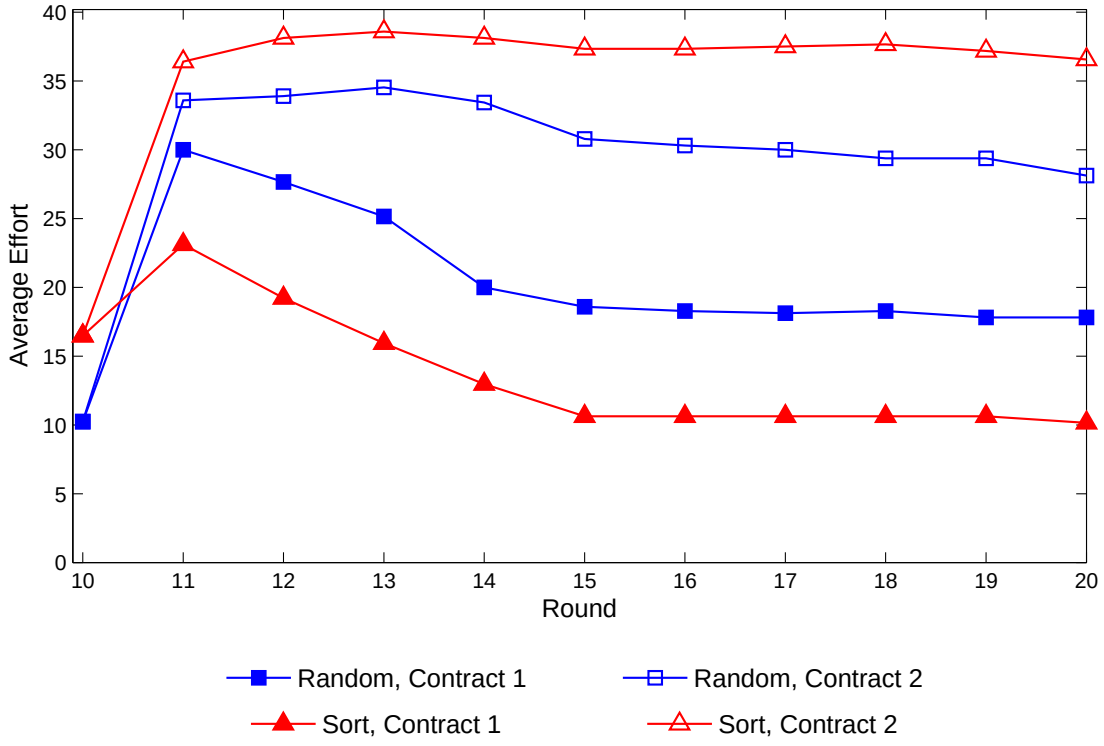


Figure 2 compares average effort for Block 2 in the Sort and Random Assignment treatments. The Sort treatment attempts to replicate the selection process of the Auction treatment while eliminating effects due to strategic anticipation. Even though this replication is obviously imperfect, the effect of 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).

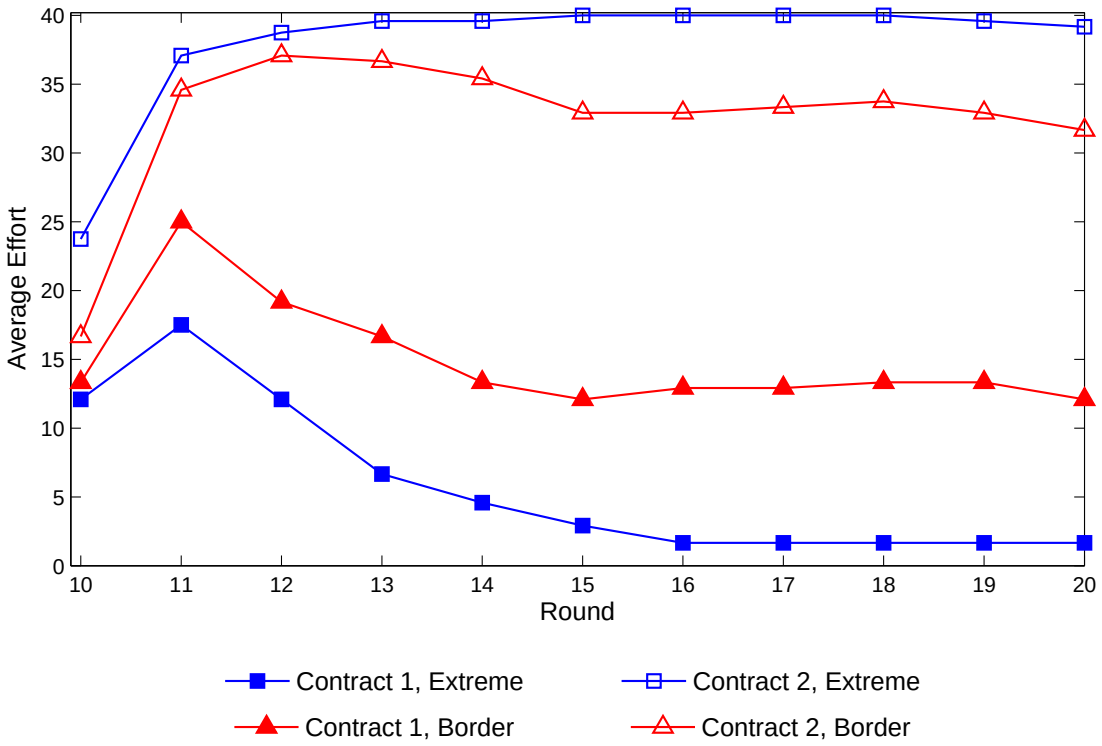
The Sort treatment gives a lower bound on how much of the effect of endogenous assignment to high performance pay is due to selection. For groups assigned to Contract 2, the average efforts over Block 2 are 31.3, 37.5, and 39.8 in the Random Assignment, Sort, and Auction treatments, respectively. It follows that selection accounts for *at least* 73% of the increased performance with endogenous assignment to high performance pay.[16] 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).[17] 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.

Figure 3 illustrates the power of sorting. Recall that subjects in the Sort treatment were put 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 3 separates data from groups that were on the border between the two contracts and groups that were at the extremes in the

[16]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 figures gives the proportion of the improvement due to selection. If we do the analogous calculations for low performance pay (Contract 1), the proportion of the decrease with endogenous assignment due to selection is 67%.

[17]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.

FIGURE 3: COMPARISON OF BORDER AND EXTREME GROUPS IN BLOCK 2



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.

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

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

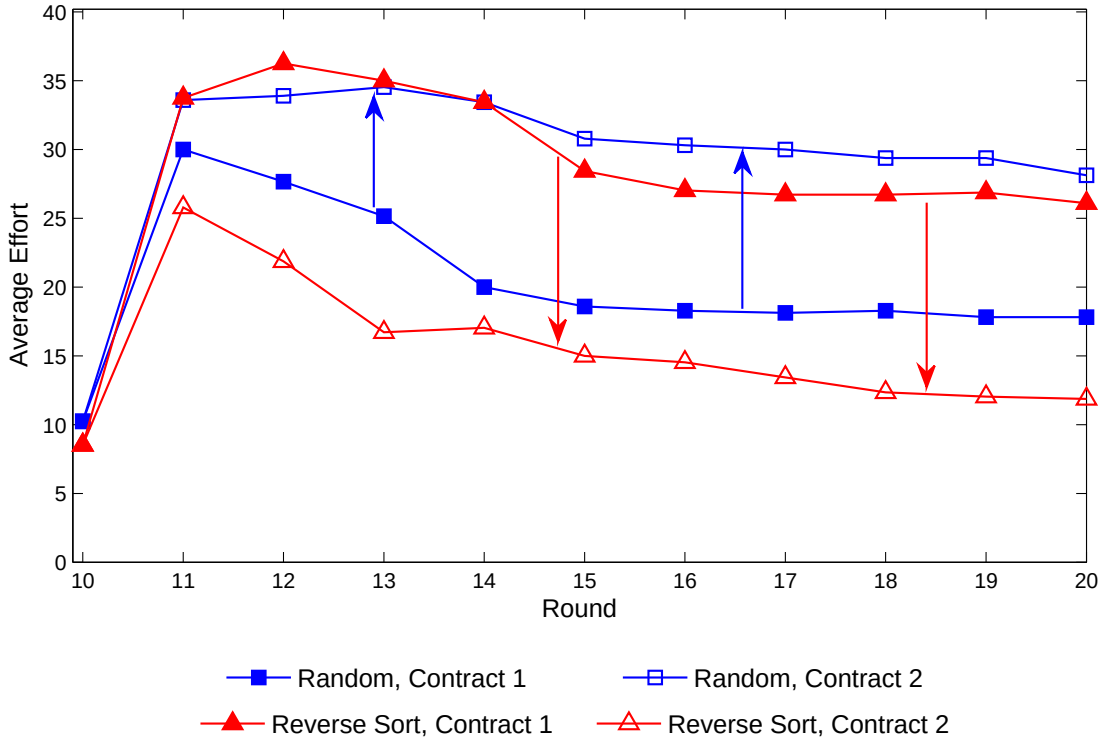


Figure 4 compares outcomes for the Random Assignment and Reverse Sort treatments. Colored arrows have been added, pointing from Contract 1 to Contract 2, to make it easier to see the difference between the two incentive contracts by treatment. In Random Assignment, where only the direct incentive effect 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.

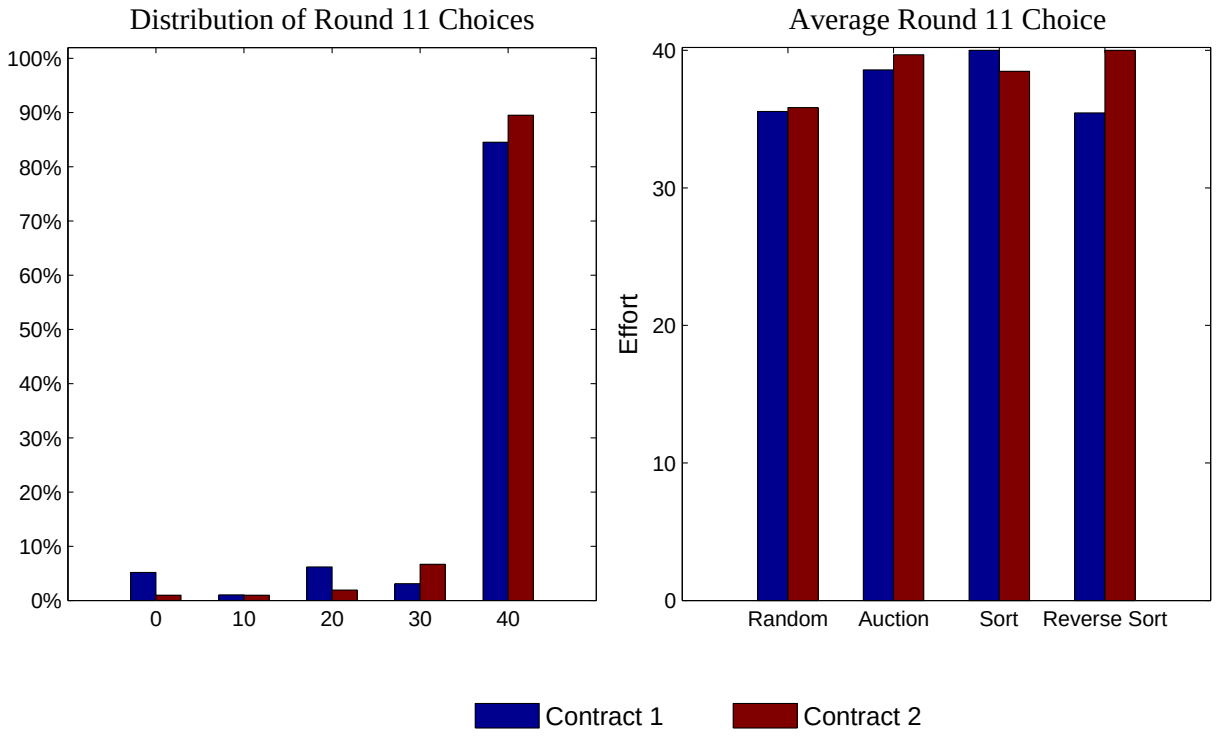
Conclusion 3. *The direct effect of increased incentives to coordinate with Contract 2 is reversed in the Reverse Sort treatment by assigning subjects to the contract they were* ***not*** *predicted to get in the Auction treatment.*

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 5 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 5: 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).[18] 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 6. 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 6 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. A dramatic 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.

[18] 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 6: 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 ($\chi^2 = 8.20$; $p = .017$).[19] 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 Learning Model

In this section, we introduce a learning model and use it to answer two important questions about our experimental results. First, based on the data we calculated that selection accounts for 73% of the increased performance with endogenous assignment to high performance pay. Because the Sort treatment only replicates the selection mechanism from the Auction treatment imperfectly, this

[19] 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.

73% figure probably underestimates the selection effect. We use the learning model to estimate how much the selection effect is underestimated in the data. Second, our experiment exogenously sets the number of subjects assigned to each contract to be equal. We use the learning model to study what might happen in the Auction treatment if the proportion of subjects assigned to Contracts 1 and 2 was determined endogenously via firms choosing which contract to offer.

The following gives an overview of how the model is constructed and the main results we generate using the model. For more details on the learning model, including the technical details of how the model is formulated, a discussion of fitting the model to the data, and estimated parameters, see Appendix B.

Our learning model is a variant of the stochastic fictitious play model (Fudenberg and Levine, 1998).[20] To capture the effects of selection and strategic anticipation, we modify the basic model by adding heterogeneity along two dimensions, subjects' initial beliefs and subjects' sophistication in modeling the learning of others. Individuals are first randomly assigned types, sophisticated or unsophisticated. Unsophisticated types follow a simple belief-based learned algorithm. In each round, they (noisily) best respond to their beliefs. Beliefs are updated from round to round based on the observed choices of others, with recent rounds receiving more weight than older observations. Sophisticated types anticipate the learning process and decision rule of unsophisticated types. Consistent with the level-k approach, they model all other individuals in their group as unsophisticated types. They use the information they observe about others' choices to predict others' beliefs and, by extension, a distribution over actions for the other members of their group.[21]

Unsophisticated learners are randomly assigned to two subtypes that differ in their initial beliefs about others' behavior. *Optimists* initially believe that all other subjects will choose 40, the highest possible effort level. *Pessimists* initially believe that all other subjects will choose 0,

[20] Previous papers that have used learning models to capture the features of coordination game experiments include Crawford (1995), Crawford and Broseta (1998), Battalio, Samuelson, and Van Huyck (2001), Brandts and Cooper (2006), and Brandts, Cooper, Fatas, and Qi (2016).

[21] The inclusion of sophisticated learners in our model is closely related to the sophisticated EWA model of Camerer, Ho, and Chong (2002). It differs in two significant ways: (1) unsophisticated types learn according to stochastic fictitious play rather than EWA and (2) our model has a level-k structure where sophisticated types do not account for the existence of other sophisticated types. The latter change is necessary to make the model computationally feasible. For other papers examining strategic sophistication, see Cooper and Kagel (2008), Hyndman, Terracol, and Vaksmann (2009), and Hyndman, Ozbay, Schotter, and Ehrblatt (2012). Also relevant is the literature on level-k models which provides a model of strategic sophistication in static settings. See Crawford, Costa-Gomes, and Iriberri (2013) for a survey.

the lowest possible effort level.[22]

Sophisticated learners are also randomly assigned to two subtypes, those with and without "strategic anticipation." The two subtypes differ in how they model belief formation during and after the auction in the Auction treatment. The model generates dropout times as noisy best responses to beliefs about effort levels in Block 2 under each contract.[23] The distinguishing feature of sophisticated types *with* strategic anticipation is that they take into account how the auction changes the expected proportion of optimists assigned to each contract. Unsophisticated types and sophisticated types *without* strategic anticipation do *not* account for optimists being more likely to be assigned to Contract 2 in Block 2. Sophisticated types *with* strategic anticipation form beliefs by modeling the decision-making of unsophisticated types, both optimists and pessimists, in the auction. As the value of *base wage* changes, they update the expected composition of unsophisticated subtypes (optimists vs. pessimists) under each contract. Sophisticated types with strategic anticipation understand that pessimists tend to choose Contract 1 and optimists tend to choose Contract 2. This makes them drop out later in the auction and choose higher (lower) effort levels if they are assigned to Contract 2 (Contract 1).[24]

The model allows for the possibility that the Sort and Reverse Sort treatments imperfectly replicate the selection mechanism from the Auction treatment by adding an additional noise term to the dropout time used to assign a subject to a group and contract *in the Sort and Reverse Sort treatments*. The standard deviation of this noise term determines how well the Sort and Reverse Sort treatments replicate the selection mechanism from the Auction treatment. A standard deviation of zero implies perfect replication. As the standard deviation of the noise term increases, more errors are made in terms of assigning individuals to the wrong contract.

The model is fit to the dataset using the Method of Simulated Moments. This is a mixture model, so we do not estimate the type or subtype of any specific individual but instead estimate the distribution of types and subtypes within the population. Subject heterogeneity adds complexity to the model but improves the fit and is necessary for the fitted model to track important features of

[22]Using a specification with two subtypes rather than a continuum makes fitting the model more computationally feasible and eases interpretation of the results. Our qualitative results do not depend on this simplifying assumption.

[23]Because we are, in essence, using a Vickrey auction, beliefs about others' strategies in the auction do not play an important role.

[24]The learning model only includes sorting based on beliefs, as opposed to sorting based on other personal characteristics. This is appropriate given that personal characteristics have little effect on dropout times beyond their effect on the probability of choosing 40 in the first round (see discussion of Table 4). This indicates that personal characteristics affect dropout times through their impact on initial beliefs.

the experimental data as well as predicting data out of sample.[25] Appendix B includes a detailed discussion of this issue, but an example drawn from the appendix serves to make the point. Based on the experimental data, we attributed 73% of the effect of making high performance pay endogenous to selection. Doing the same calculation using simulated data from the full model yields a similar figure of 77%. If we repeat this exercise using a model with two subtypes of unsophisticated learners (optimists and pessimists), *but no sophisticated learners*, 95% of the effect of making high performance pay endogenous is attributed to selection. This is substantially higher than the 73% figure from the data. A model with unsophisticated and sophisticated learners *but no subtypes (optimists and pessimists)* does even worse, attributing 100% of the effect of making high performance pay endogenous to selection. Finally, if we include *no heterogeneity* in the model (i.e. there are only unsophisticated learners and initial beliefs are homogenous), the simulated data is virtually identical for the three treatments used to make this calculation (Random Assignment, Auction, and Sort), making the result of the calculation meaningless. Having multiple types and subtypes makes the model more complex, but it is this complexity that allows the model to do a good job of replicating perhaps the single most important feature of the data.[26]

The main reason we developed a learning model is to conduct counterfactual exercises. Our first counterfactual exercise considers an important caveat to our results. In Conclusion 2 we state that " roughly three quarters of the effect of making high performance pay endogenous is due to selection." But we know that this understates the effect of selection because the Sort treatment imperfectly replicates the selection mechanism from the Auction treatment. To estimate the true effect of selection, we compare the results from two simulation exercises. The first simulates the full model including the noise term for predicted dropout times. The second set of simulations is identical to the first *except we do not include an error term for the predicted dropout times*

[25] Fitting a model to the data does not guarantee that the fitted model will do a good job of tracking existing data or predicting new data. The differences between treatments are not part of the objective function per se, and fitting one treatment well may come at the expense of fitting another poorly or missing a difference between treatments. To see this point, consider an incredibly simple-minded model of individual decision making in our experiments - in all rounds of all treatments under all contracts, subjects pick effort level $i \in \{0, 10, 20, 30, 40\}$ with probability p_i. There is nothing that prevents us fitting this simple-minded model to our data, but it is obvious that, even using fitted values for p_0, p_{10}, etc., it would fail to track the dynamics in the data as well as the differences between the various treatments. It would also fail to predict data from new treatments.

[26] The appendix also includes a brief discussion of why two types of sophisticated learners are included in the model. We do not argue that our model is the best possible model of learning from the vast universe of possible models, either in terms of tracking the major features of our experimental data or predicting out of sample. Identifying the best model is well beyond the scope of our paper.

in the Sort and Reverse Sort treatments. This allows us to estimate what would happen in the counterfactual case where we perfectly replicated the selection mechanism from the Auction treatment.

Based on the experimental data, we calculated that 73% of the effect of making high performance pay endogenous is due to selection. Doing the same calculation using simulated data from the full model (which includes errors in assigning subjects to contracts for the Sort treatment) yields a figure of 77%. There is little difference between the selection effects calculated using the experimental and simulated data. For the simulations *without* errors in the assignment of subjects to contracts for the Sort treatment, 93% of the effect of making high performance pay endogenous is due to selection. The difference between the two sets of simulations is of more interest than the levels. If the effect due to selection is underestimated by about the same amount in the experimental data and the simulated data *with* errors, the true effect of selection is roughly 90% of the effect of making high performance pay endogenous.

Conclusion 5. *Simulations of the learning model indicate that the effect of selection accounts for almost all of the improvement with endogenous assignment to high performance pay.*

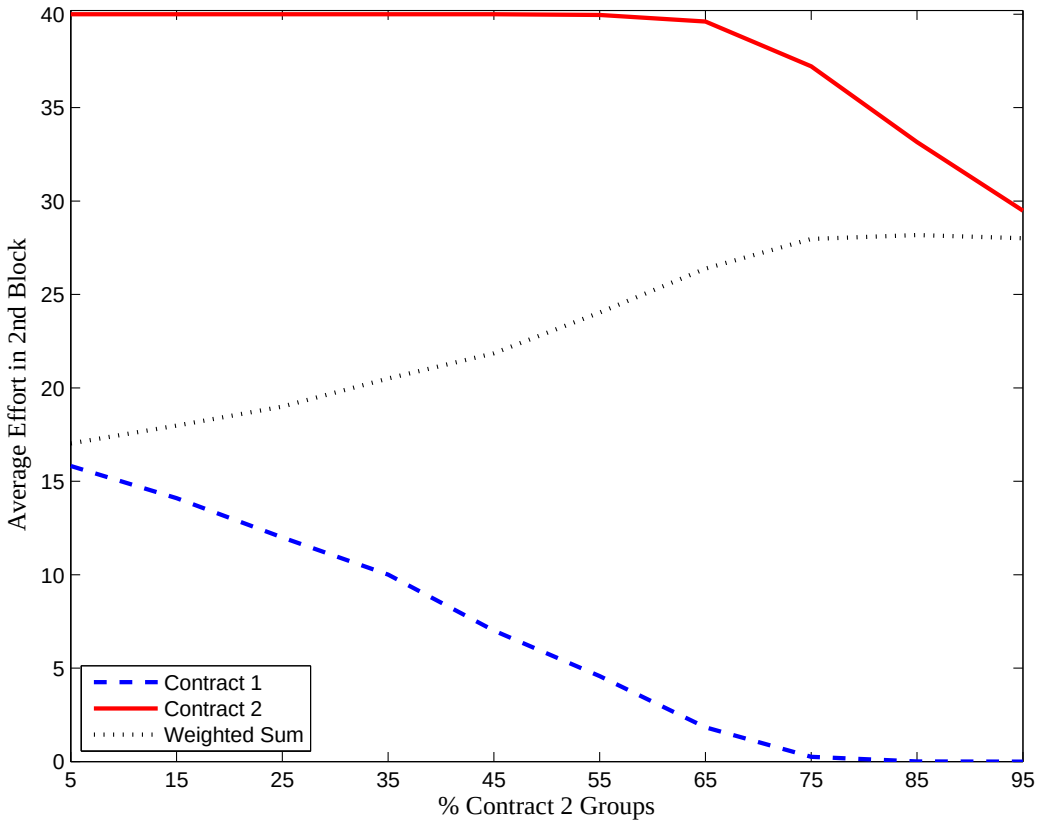
Our second counterfactual exercise explores what might happen in the long run if contract choice were endogenous *for firms*. The Auction treatment assigns a fixed and equal proportion of subjects to Contracts 1 and 2, implying that contract assignment is endogenous for workers but exogenous for firms. This probably is not a bad approximation of how most labor markets function in the short run, as firms hire workers more often than they switch incentive systems. In the long run, however, firms should gravitate to incentive systems that yield higher profits. The experimental data strongly suggest that Contract 2 is the more profitable option. Our experiments do not include subjects playing the manager's role and, by extension, do not include a profit function for managers. As such, we cannot directly compare managers' profits across contracts. However, imposing any linear manager profit function ($\pi_M = \alpha + \beta \times$min effort-total wages) where the variable component of revenue covers the variable component of wages under Contract 2 ($\beta \geq 40$),[27] average manager profits are higher with Contract 2 in the Auction treatment in spite

[27]Requiring that the variable component of revenue is greater than the variable component of wages implies that managers are better off as minimum effort increases. The variable component of revenue is $\beta \times$min effort. The variable component of bonuses is $4 \times \beta \times$min effort. Given $B = 10$ for Contract 2, setting the variable component of revenue greater than the variable component of wages yields $\beta \times$min effort $\geq 40 \times$ min effort. Simplifying, $\beta \geq 40$.

of the higher bonus rate. This is due to the combination of lower base wages and higher effort. If managers switch to more profitable incentive contracts, the distribution of incentive contracts should shift towards greater usage of Contract 2 over time. Does it follow that in the long run we should expect all groups to use Contract 2 *and* achieve the high effort levels observed with Contract 2 in the Auction treatment?

We address this question by simulating sessions of the Auction treatment with 20 groups. The number of groups receiving Contract 2 is varied from 0 to 20, with 100 simulated sessions for each split. The parameters of the model are held fixed at the fitted values. Figure 6 graphs the average effort in the Second Block as a function of the percentage of groups assigned to Contract 2. Average effort is broken down by contract, with the dotted line showing the weighted average across contracts.

FIGURE 6: SIMULATION OF AUCTION TREATMENT WITH 20 GROUPS



For any proportion of Contract 2 groups, average effort is far higher in Contract 2 than Contract 1. This supports our argument that all managers will move to Contract 2 in the long run as it is always more profitable. But as the proportion of groups using Contract 2 gets large, the average

effort with Contract 2 drops. Once all groups use Contract 2, selection cannot affect performance with Contract 2 since both the optimists and the pessimists are assigned to Contract 2. Strategic anticipation also cannot have an effect, since this changes behavior due to the ability of some sophisticated learners to anticipate selection due to the auction. With no selection, there is nothing to anticipate.

The average effort, weighted across contracts, rises gradually as the proportion of groups using Contract 2 rises. Moving from a regime with only Contract 1 to a regime with only Contract 2 has a modest positive effect on productivity. This effect must equal the direct incentive effect as measured by the difference between Contracts 1 and 2 in the Random Assignment treatment. With no selection and, by extension, no strategic anticipation, only the direct effect of higher incentives to coordinate at high effort levels can remain.

One of the surprising results of our experiment is the small total effect of making assignment to high performance pay endogenous. The underlying issue is that selection is a double-edged sword. Assigning more optimists to Contract 2 necessarily means assigning more pessimists to Contract 1. Our second counterfactual exercise makes a similar point about the long-run effect of high performance pay if the choice of contracts both by firms and by workers is endogenous. In the long run, we expect every firm to try to obtain higher effort by choosing high performance pay. But if every firm tries to get more than its fair share of the optimists, no firm can get more than its fair share.

Conclusion 6. *If groups are free to choose between Contracts 1 and 2, we predict that all groups will move to Contract 2 in the long run. The effect on average effort across all groups, regardless of contract, is predicted to be modest. With any possibility of selection eliminated, only the direct incentive effect remains.*

5 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,

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, and analysis based on our learning model suggests that the true figure is roughly 90%. 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 (over both contracts) of endogenous assignment to incentive contracts. The results of simulation exercises indicate that this is a general feature of this environment. In the long run, we anticipate that all groups would choose high performance pay, but the effect of moving all groups from low to high performance pay is limited to the modest direct incentive effect. If we were giving a manager advice, it is clear we would suggest using higher performance pay. But it does not follow that a trend towards stronger performance pay will have a huge effect at the macro level.

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, reducing the likelihood that workers understand how assignment into incentive contracts occurs and its implications. This implies that strategic anticipation plays a smaller role in field settings than in the lab. Since our main results are primarily due to selection, they are relatively likely to carry over to field settings.

There remain a large number of issues to be addressed in future work. (1) The experimental literature has identified a number of other mechanisms that are effective for escaping produc-

tivity traps (e.g. communication, gradualism, exclusion of low effort providers). More research is needed about how selection interacts with these other mechanisms, and what this implies for their effectiveness. (2) Intuition and our learning model make clear predictions about the effect of allowing firms to choose an incentive contract. We would like to confirm these predictions by running experiments where firms' choices of incentive contracts are endogenous. (3) We have taken a big step forward by considering what happens to workers who do *not* choose the contract with high performance pay. A natural next step is to consider economies with multiple sectors. Rather than moving to a firm with low performance pay, workers who do not choose a contract with high performance pay could move to a different sector, possibly one where the tasks to be performed were not interactive. The outcome isn't obvious and ultimately can only be determined by a combination of new theory and careful empirical work.

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

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Contents:

APPENDIX A: Details of the Sort Treatment

APPENDIX B: Structural Learning Model

APPENDIX C: Experimental Instructions

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. In Table A below, we present the summary statistics of observable characteristics for subjects in the Auction treatment.[1] 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.

[1] 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. A substantial fraction of the subjects (31%) chose not to report an SAT/ACT score. For those missing SAT/ACT math or overall composite scores, we set the dummy variable (I_{MATH} or I_{SCORE}) to be one. Otherwise, it is set to be zero.

Table A. Summary Statistics: Tobit Regression Variables

Panel A	**Regression Variables**					
	All		*Contract 1*		*Contract 2*	
	Obs	Mean/S.D.	Obs	Mean/S.D	Obs	Mean/S.D
RISK	128	3.03	64	3.08	64	2.98
		(1.61)		(1.70)		(1.52)
MATH	81	627.47	34	640.88	47	617.77
		(77.29)		(75.89)		(77.64)
SCORE	92	1,828.70	45	1,865.56	47	1,793.40
		(200.98)		(214.00)		(183.02)
EXTROVERT	128	4.79	64	4.70	64	4.89
		(1.24)		(1.25)		(1.24)
AGREEABLE	128	4.69	64	4.71	64	4.67
		(1.33)		(1.33)		(1.34)
CONSCIENTIOUS	128	5.45	64	5.49	64	5.41
		(1.16)		(1.17)		(1.16)
EMOTIONSTABLE	128	5.15	64	5.15	64	5.15
		(1.19)		(1.06)		(1.31)
OPENNESS	128	5.48	64	5.49	64	5.46
		(1.14)		(1.15)		(1.14)
FAIR	125	1.57	62	1.53	63	1.60
		(0.50)		(0.50)		(0.49)
HELP	120	1.41	60	1.40	60	1.42
		(0.49)		(0.49)		(0.50)
TRUST	126	1.35	64	1.31	62	1.39
		(0.48)		(0.47)		(0.49)
TRANSFER	123	3.50	62	3.48	61	3.52
		(0.64)		(0.65)		(0.65)
STRANGER	113	1.60	57	1.60	56	1.61
		(0.49)		(0.49)		(0.49)
TRUSTME	126	5.56	64	5.62	62	5.48
		(0.79)		(0.55)		(0.97)

Panel B	**Effort Levels in Round 1 of Block 1**					
	All		Contract 1		Contract 2	
	Freq.	Percent	Freq.	Percent	Freq.	Percent
$EFFORT = 0$	17	13.28%	12	18.75%	5	7.81%
$EFFORT = 10$	10	7.81%	6	9.38%	4	6.25%
$EFFORT = 20$	27	21.09%	12	18.75%	15	23.44%
$EFFORT = 30$	29	22.66%	20	31.25%	9	14.06%
$EFFORT = 40$	45	35.16%	14	21.88%	31	48.44%

Notes: The values of standard errors are reported in parentheses.

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.[2]

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

[2]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: Structural Learning Model

B.1 Model Setup

Let $e \in \{1,2,3,4,5\}$ correspond to the effort level of a subject. If a subject chooses $e = 1$, then he/she chooses effort level 0; if subject chooses $e = 2$, then he chooses effort level 10; ...; if subject chooses $e = 5$, then he chooses effort level 40. Let $m \in \{1,2,3,4,5\}$ correspond the minimum effort level of the other 3 subjects in his/her group. Payoffs of subject i are determined by a contract, consisting of a base wage, an effort cost, and a bonus rate. As described in the manuscript, two contracts are available. Henceforth, we use superscript c to denote the type of contract. In Contract 1, $c = 1$, base wage is $W_1 = 300$ ECUs, and in Contract 2, $c = 2$, base wage is $W_2 = w$ ECUs, which varies across different experimental sessions. The payoff of subject with contract c, who chooses effort level e and faces other subjects' minimum effort level m, is

$$\begin{aligned} \bar{\pi}^1(e,m) &= W_1 - 5 \cdot (e-1) \times 10 + B_1 \cdot (\min\{e,m\} - 1) \times 10, \\ \bar{\pi}^2(e,m|w) &= w - 5 \cdot (e-1) \times 10 + B_2 \cdot (\min\{e,m\} - 1) \times 10. \end{aligned}$$

For the following exposition, we model the experiment for two blocks, each consisting of 10 rounds. Subjects' payoffs differ by the contract they are assigned in the beginning of a Block. A subject retains the same contract throughout the entire block. Without loss of generality, let $\pi_{it}(e,m)$ denote the payoff function of subject i in round t. All subjects are assigned to Contract 1 in Block 1. In addition, every subject is randomly assigned to a group of four subjects at the

beginning of Block 1. In the beginning of Block 2, subjects' contracts and groups are re-assigned. Half the subjects are assigned to Contract 1, while the other half are assigned to Contract 2. The re-assignment rules differ across different treatments. As described in the manuscript, we consider four different treatments, namely, the Random, Auction, Sort, and Reverse Sort treatments.

The model consists of two types of subjects, sophisticated subjects and unsophisticated subjects. We denote unsophisticated learners by superscript u, and denote sophisticated learners by s. We allow for two subtypes of unsophisticated subjects, optimists (or subtype h) pessimists (or subtype l).

For the notational convenience, the contract c superscript is suppressed in the following subsections.

B.1.1 Unsophisticated Subjects: An unsophisticated subject updates his/her belief based on the current round experience. Let $N_{-it}(e)$ be the number of subjects other than subject i choosing effort level e. Let $W_{it}(e)$ be the subject's previous period experience, and the updating rule is

$$W_{it+1}(e) = \delta \cdot W_{it}(e) + N_{-it}(e)$$

where δ is the depreciation parameter to be estimated from data. This updating rule is drawn from the stochastic fictitious play model of Fudenberg and Levine (1998). The initial period weights for the optimists are $[0, 0, 0, 0, \omega]$, and the weights for the pessimists are $[\omega, 0, 0, 0, 0]$. In other words, optimists initially believe that all other subjects will choose 40 and pessimists initially believe that all other subjects will choose 0. The initial value of ω is a parameter to be estimated.

The proportion of unsophisticated subject types in the population are parameters estimated from data. The probability of being subtype l is μ_{pes}, and the probability of being subtype h is μ_{opt}.

An unsophisticated subject believes that the probability of any other subject in his/her group choosing effort level e in the next round $t+1$ is

$$p_{it+1}(e) = \frac{W_{it+1}(e)}{\sum_{k=1}^{5} W_{it+1}(k)}$$

Given the belief $p_{it+1}(e)$, an unsophisticated subject can infer the probability of minimum effort

being m in round $t+1$ is $pr_{it+1}(m)$ as a function of $p_{it+1}(e)$.

prob. of all three others choosing efforts $e = 5$: $pr_{it+1}(5) = (p_{it+1}(5))^3$,

prob. of all three others choosing efforts $e \geq 4$: $pr_{it+1}(4) = (p_{it+1}(4) + p_{it+1}(5))^3$,

prob. of all three others choosing efforts $e \geq 3$: $pr_{it+1}(3) = (p_{it+1}(3) + p_{it+1}(4) + p_{it+1}(5))^3$,

prob. of all three others choosing efforts $e \geq 2$: $pr_{it+1}(2) = (p_{it+1}(2) + p_{it+1}(3) + p_{it+1}(4) + p_{it+1}(5))^3$,

prob. of all three others choosing efforts $e \geq 1$: $pr_{it+1}(1) = 1$.

An unsophisticated subject can infer that the probability of minimum effort being m in round $t+1$ is $q_{it+1}(m)$, where

$$\begin{aligned}
q_{it+1}(5) &= pr_{it+1}(5), \\
q_{it+1}(4) &= pr_{it+1}(4) - pr_{it+1}(5), \\
q_{it+1}(3) &= pr_{it+1}(3) - pr_{it+1}(4), \\
q_{it+1}(2) &= pr_{it+1}(2) - pr_{it+1}(3), \\
q_{it+1}(1) &= pr_{it+1}(1) - pr_{it+1}(2).
\end{aligned}$$

Notice in the initial period, optimists $p_1^h(5) = 1$, and $q_1^h(5) = 1$. Conversely, pessimists $p_1^l(1) = 1$, and $q_1^l(1) = 1$. Given belief $q_{it}^u(m)$, the expected payoff of a subtype u player choosing effort e is

$$E\pi_{it}^u(e) = \sum_{m=1}^{5} q_{it}^u(m) \cdot \pi_{it}(e, m) \quad \text{for } u = h, l \text{ and } \forall e.$$

We assume the probability of a type u subject choosing effort level e follows a logit specification, where

$$\Phi_{it}^u(e) = \frac{\exp(\lambda \cdot E\pi_{it}^u(e))}{\sum_{k=1}^{5} \exp(\lambda \cdot E\pi_{it}^u(k))} \quad \text{for } u = h, l \text{ and } \forall e. \tag{1}$$

Each unsophisticated subject takes a random draw to determine his actual effort choice based on this probability Φ_{it}^u each round t.

B.1.2 Sophisticated Subjects: A sophisticated subject is denoted by s superscript. Our modeling of the sophisticated subjects and their learning process is similar to that of the sophisticated

EWA model (Camerer, Ho, and Chong, 2002). Sophisticated subjects assume all other subjects in the group are unsophisticated. A sophisticated subject knows how unsophisticated subjects' beliefs evolve and can therefore anticipate other subjects' behaviors. However, a sophisticated subject cannot identify other subjects in his group, therefore cannot track individual players' behaviors over time. We assume a sophisticated subject i uses the average of other subjects' $N_{-jt}(e)$ to formulate the updating rules of the unsophisticated types. For example, if a sophisticated type observes effort levels $5, 3, 2, 1$ in his group, and he knows his own effort choice is 2, then he can infer the subject choosing 5 would have $N = [0, 0, 1, 1, 1]$, the subject choosing 3 would have $N = [1, 0, 0, 1, 1]$, and the subject choosing 1 would have $N = [1, 0, 1, 1, 0]$. So the average is $\bar{N} = [2/3, 0, 2/3, 1, 2/3]$. The same updating rule is used where

$$W_{it+1}(e) = \delta \cdot W_{it}(e) + \bar{N}_{-it}(e).$$

An unsophisticated person's effort choice probability Φ_{it}^{us} can be readily computed. Notice that Φ_{it}^{us} is the same for all other subjects in the group. This probability is different from the actual choice probability unsophisticated subjects used.

A sophisticated subject does not know the type of the other subjects. He believes the probability of another subject being type l is $\tilde{\theta}_i$, and the probability of another subject being type h is $1 - \tilde{\theta}_i$, which may or may not be consistent with the distribution of unsophisticated subject types in the population, where the probability of being subtype l is $\theta = \frac{\mu_{pes}}{\mu_{pes}+\mu_{opt}}$, and the probability of being subtype h is $1 - \theta$.

In Block 1, beliefs about θ are correct, so $\tilde{\theta}_i = \theta \; \forall i \in \{i : \text{Sophisticated}\}$. In Block 2, they may be incorrect if sophisticated subjects fail to anticipate the effects of selection. The two subtypes of sophisticated subjects differ in their ability to anticipate the effects of selection caused by the auction ("strategic anticipation"). We denote the proportion of the subtype with the ability of strategic anticipation in the population μ_{sa}. Therefore, the probability of the subtype without strategic anticipation is $1 - \mu_{pes} - \mu_{opt} - \mu_{sa}$.

A sophisticated subject without strategic anticipation still assumes $\tilde{\theta}_i = \theta$ after the auction has taken place. In other words, a sophisticated learner without strategic anticipation does not adjust his beliefs post-auction for the effects of selection. On the other hand, a sophisticated subject with strategic anticipation alters his post-auction beliefs $\tilde{\theta}_i$ based on his second block contract

assignment in the Auction treatment. No such adjustment takes place in the other treatments as the subject has no reason anticipate selection effects, so $\tilde{\theta}_i = \theta$ in all other treatments.

A sophisticated subject's belief of the distribution of effort choices by another subject $j \neq i$ is

$$p^s_{t+1}(e) = \tilde{\theta}_{it+1}\Phi^{u,h}_{it+1}(e) + (1 - \tilde{\theta}_{it+1})\Phi^{u,l}_{it+1}(e) \quad \forall e.$$

Based on the belief $p^s_{it+1}(e)$, a sophisticated subject can infer the probability of minimum effort being m in round $t+1$, which is $pr^s_{it+1}(m)$.

prob. of all three others choosing efforts $e = 5$: $pr_{it+1}(5) = (p_{it+1}(5))^3$,

prob. of all three others choosing efforts $e \geq 4$: $pr_{it+1}(4) = (p_{it+1}(4) + p_{it+1}(5))^3$,

prob. of all three others choosing efforts $e \geq 3$: $pr_{it+1}(3) = (p_{it+1}(3) + p_{it+1}(4) + p_{it+1}(5))^3$,

prob. of all three others choosing efforts $e \geq 2$: $pr_{it+1}(2) = (p_{it+1}(2) + p_{it+1}(3) + p_{it+1}(4) + p_{it+1}(5))^3$,

prob. of all three others choosing efforts $e \geq 1$: $pr_{it+1}(1) = 1$.

A sophisticated subject can infer that the probability of minimum effort being m in round $t+1$ is $q_{it+1}(m)$, where

$$\begin{aligned}
q_{it+1}(5) &= pr_{it+1}(5), \\
q_{it+1}(4) &= pr_{it+1}(4) - pr_{it+1}(5), \\
q_{it+1}(3) &= pr_{it+1}(3) - pr_{it+1}(4), \\
q_{it+1}(2) &= pr_{it+1}(2) - pr_{it+1}(3), \\
q_{it+1}(1) &= pr_{it+1}(1) - pr_{it+1}(2).
\end{aligned}$$

Therefore, the expected payoff of sophisticated subject i choosing a effort level e in the next round is

$$E\pi^s_{it}(e) = \sum_{m=1}^{5} q^s_{it}(e)\pi_{it}(e, m) \quad \forall e.$$

We assume the probability of a type s subject choosing effort level e follows a logit specification, where

$$\Phi^s_{it}(e) = \frac{\exp(\lambda E\pi^s_{it}(e))}{\sum_{k=1}^{5} \exp(\lambda E\pi^s_{it}(k))} \quad \forall e. \tag{2}$$

Each sophisticated subject takes an random draw to determine the actual effort choice based on this probability Φ_{it}^s each round t.

B.1.3 Reset At the beginning of Block 2 (or round 11), reassignment of groups and contracts occur. We assume subject experience weights are reset at this point. The reset experience weight $\tilde{W}_{i,11}$ is a weighted average of realized experience at the end of Block 1 $W_{i,11}$, and a subject's initial belief weight $W_{i,0}$,

$$\tilde{W}_{i,11} = \rho \cdot W_{i,0} + (1 - \rho) \cdot W_{i,11}$$

Reset parameter ρ is fit from the data.

B.2 Treatments

We consider four treatments, which are different in the rules of assigning subjects into contracts and the rules of forming new groups within each contract in Block 2.

B.2.1 Random Assignment Treatment: Each individual subject draws a random shock ε_i from a common distribution. We rank ε_i's from high to low. Then the half of the subjects who have ε_i's above the median shock value are assigned to Contract 1 in Block 2. The other half of the subjects who have ε_i's below the median shock value are assigned to Contract 2 in Block 2. Subjects are then randomly assigned into groups.

Sophisticated subjects do not change their beliefs of the composition of the unsophisticated subtypes, so $\tilde{\theta}_i = \theta$, $\forall i \in \{i : \text{Sophisticated}\}$.

B.2.2 Auction Treatment: In the Auction Treatment, Contract 2 base wage ranges from 400 to 0. Every 5 seconds, the base wage drops by 5 ECUs. In every instant in time, a subject needs to decide whether to press a "Contract 1" button to drop out of the Auction process.

An unsophisticated subject, given reset experience weight $\tilde{W}_{i,11}$, can formulate beliefs about the distribution of minimum effort in a future group $q_{i,11}^u$, and the expectation on payoffs in round

11 under Contract 1 and under Contract 2 with any base wage.

$$E\bar{\pi}_{i,11}^{u,c=1}(e) = \sum_{m=1}^{5} q_{i,11}^{u}(m) \cdot \bar{\pi}^{c=1}(e,m),$$
$$E\bar{\pi}_{i,11}^{u,c=2}(e|w) = \sum_{m=1}^{5} q_{i,11}^{u}(m) \cdot \bar{\pi}^{c=2}(e,m|w).$$

Using the expected payoffs, we can calculate the effort choice probability $\Phi_{it}^{u,c=1}(e)$ and $\Phi_{it}^{u,c=2}(e|w)$ for every possible contract. Therefore, the expected payoff of choosing Contract 1 is always $E\bar{\pi}_{i}^{u,c=1} = \sum_e \Phi_{it}^{u,c=1}(e) \cdot E\bar{\pi}_{i,11}^{u,c=1}(e)$, and expected payoff of having Contract 2 with base wage w is $E\bar{\pi}_{i}^{u,c=2}(w) = \sum_e \Phi_{it}^{u,c=2}(e|w) \cdot E\bar{\pi}_{i,11}^{u,c=2}(e|w)$. Assume subjects make noisy dropout decisions based on logit decision rule, where the logit parameter is ζ. At any given w, the probability of subject i choosing Contract 1 for any given wage w_i is

$$d(w_i) = \frac{\exp(\zeta \cdot E\bar{\pi}_{i}^{u,c=1})}{\exp(\zeta \cdot E\bar{\pi}_{i}^{u,c=1}) + \exp(\zeta \cdot E\bar{\pi}_{i}^{u,c=2}(w))}$$

This is the conditional probability assuming the unsophisticated subject has not dropped out at a previous base wage $w' > w$. The unconditional probability of dropping out, $p_{dropout}^{u}$, and the cumulative dropout probability, $D^u(w)$, (the probability of dropping out before or at base wage w) at each base wage is

$$\begin{aligned}
&p_{dropout}^{u}(w=400) = d(w=400); && D^u(w=400) = p_{dropout}^{u}(w=400);\\
&p_{dropout}^{u}(w=395) = (1 - D^u(w=400)) \cdot d(w=395); && D^u(w=395) = D^u(w=400) + p_{dropout}^{u}(w=395);\\
&... && ...\\
&p_{dropout}^{u}(w) = (1 - D^u(w+5)) \cdot d(w); && D^u(w) = D^u(w+5) + p_{dropout}^{u}(w).
\end{aligned}$$

Compared to an unsophisticated subject, a sophisticated subject has the following information when making contract decisions. First, if $2n$ subjects participate in an auction, a sophisticated subject considers the other $2n-1$ subjects' contract choices. Second, a sophisticated subject does not know the type of other subjects, and assumes everyone else is unsophisticated. He knows the population distribution of subtypes, and there is a θ chance that the other subject is an unsophisticated pessimist. Third, a sophisticated subject forms beliefs on unsophisticated

subjects' experience weight $\tilde{W}^u_{11}(m)$. He assumes all unsophisticated subjects of the same subtype have the same beliefs. So using the same updating rules, he can predict $D^u(w|\tilde{q}^u_{11})$ for each subtype of subject for any given base wage w, where D^u is the probability a subtype u subject will choose to drop out of the auction at or before wage w.

A sophisticated type without strategic anticipation believes that the probability of having a unsophisticated pessimist within his own contract in Block 2 is still θ, which is the same as in the population. With the auction taking place, a sophisticated type with strategic anticipation has the ability to anticipate that optimists are more likely to be assigned to Contract 2, and pessimists are more likely to be assigned to Contract 1, so the relative proportions of pessimists to optimists are different across contracts. Therefore, the probability of meeting a pessimist $\tilde{\theta}$ can vary by individual subject's past experience in Block 1 and depends on which contract a sophisticated subject is ultimately assigned. We model strategic anticipation, and how beliefs of $\tilde{\theta}$ is formed as follows:

Without loss of generality, assume there are n_h type h subjects and n_l type l subjects, where $n_l = (2n-1) - n_h$. From a sophisticated subject's perspective, the probability of facing n_h type h subjects and n_l type l subjects is $\binom{2n-1}{n_l}(\theta)^{n_l}(1-\theta)^{n_h}$. Given any n_l and n, the probability of exactly $\bar{n} \in (0, 2n-1]$ individuals dropping out at w is

$$\begin{aligned}&Prob(\bar{n}|n_l, n)\\ &= \sum_x \binom{n_l}{x}(D^l(w))^x(1-D^l(w))^{n_l-x} \cdot \binom{2n-1-n_l}{\bar{n}-x}(D^h(w))^{\bar{n}-x}(1-D^h(w))^{(2n-1-n_l)-(\bar{n}-x)}.\end{aligned}$$

When $x \in [0, n_l]$ if $n_l < \bar{n}$, and $x \in [\bar{n}-n_h, \bar{n}]$ if $n_l \geq \bar{n}$.

Based on Bayes' rule, given any n_l, the probability that exactly $\bar{n} \in (0, 2n-1]$ individuals, and exactly $x \in [0, \min\{n_l, \bar{n}\}]$ of type l, have dropped out at or before any base wage w is

$$\begin{aligned}&P^{dropout}(x, w, \bar{n}|n_l)\\ &= \frac{\binom{n_l}{x}(D^l(w))^x(1-D^l(w))^{n_l-x} \cdot \binom{2n-1-n_l}{\bar{n}-x}(D^h(w))^{\bar{n}-x}(1-D^h(w))^{(2n-1-n_l)-(\bar{n}-x)}}{\sum_{k=0}^{\min\{n_l,\bar{n}\}}\binom{n_l}{k}(D^l(w))^k(1-D^l(w))^{n_l-k} \cdot \binom{2n-1-n_l}{\bar{n}-k}(D^h(w))^{\bar{n}-k}(1-D^h(w))^{(2n-1-n_l)-(\bar{n}-k)}}.\end{aligned}$$

Knowing the probability of other subjects' dropout wages w, a sophisticated learner decides upon his own dropout wage w'. In particular, a sophisticated subject understands that for any w to become the base wage of Contract 2 in the second block, two possibilities exist,

1. Exactly n unsophisticated subjects dropped out at or before w, and the sophisticated subject chose a dropout wage $w' < w$; or

2. Exactly $n-1$ unsophisticated subjects dropped out at or before w, and the sophisticated subject chose a dropout wage $w' \geq w$.

In case 1, the sophisticated individual gets Contract 2. If exactly number x of type l individuals are in Contract 1, the sophisticated subject knows that exactly $n_l - x$ individuals with Contract 2 are of subtype l, and exactly $n-1-(n_l-x)$ are of subtype h. So the sophisticated subject with strategic anticipation expects the probability of facing a subtype l in his/her Block 2 group to be $\tilde{\theta} = \frac{n_l - x}{n-1}$. A sophisticated subject's expected payoff conditional on getting Contract 2 is $E\bar{\Pi}_{i,11}^{s,c=2}(w,x,n_l)$. Similarly, in case 2, the sophisticated individual gets Contract 1. If exactly number x individuals with Contract 1 are of subtype l, then exactly $n-1-x$ of them are subtype h subjects. So the sophisticated subject with strategic anticipation expects the probability of facing a subtype l subject in his Block 2 group to be $\tilde{\theta} = \frac{x}{n-1}$. A sophisticated subject's expected payoff conditional on getting Contract 1 is $E\bar{\Pi}_{i,11}^{s,c=1}(x,n_l)$. Therefore, taking into account the probability of getting exactly x individuals of subtype l in either contract, the expected payoff of a sophisticated individual with strategic anticipation is

$$\text{Case 1:} \quad \binom{2n-1}{n_l}(\theta)^{n_l}(1-\theta)^{2n-1-n_l}P^{dropout}(x,w,n|n_l)E\bar{\Pi}_{i,11}^{s,c=2}(w,x,n_l),$$
$$\text{Case 2:} \quad \binom{2n-1}{n_l}(\theta)^{n_l}(1-\theta)^{2n-1-n_l}P^{dropout}(x,w,n-1|n_l)E\bar{\Pi}_{i,11}^{s,c=1}(x,n_l).$$

Sophisticated subjects of both subtypes can make a mistake in choosing contracts. The potential for mistakes is represented by an additional randomly drawn logit error term ϵ^c which is i.i.d. across different contracts. Similar to unsophisticated subjects, we can derive the dropout wage w_i, and the probability of dropping out for sophisticated types.[3]

[3]There is no closed-form probability of base wage choices for the sophisticated individuals. To simplify computation, we approximate $\tilde{\theta}_i^{s,c}$ first by

$$\tilde{\theta}_i^{s,c=1}(w) = \sum_{n_l=0}^{2n-1}\sum_{x=0}^{\min\{n_l,n\}}\binom{2n-1}{n_l}(\theta)^{n_l}(1-\theta)^{2n-1-n_l}P^{dropout}(x,w,n-1|n_l)\frac{x}{n-1},$$
$$\tilde{\theta}_i^{s,c=2}(w) = \sum_{n_l=0}^{2n-1}\sum_{x=0}^{\min\{n_l,n\}}\binom{2n-1}{n_l}(\theta)^{n_l}(1-\theta)^{2n-1-n_l}P^{dropout}(x,w,n|n_l)\frac{n_l-x}{n-1}.$$

Monte Carlo tests indicate that simulated subject behaviors do not change too much under such an approximation.

For all subjects, we simulate their choices of dropout wages w_i's. We then rank dropout wage w_i's from high to low. Then the half of the subjects who have w_i's above the median dropout wage are assigned to Contract 1 in Block 2. The other half of the subjects who have w_i's below the median dropout wage are assigned to Contract 2 in Block 2. The group assignment of subjects within a contract is random, as in the actual experimental setting.

B.2.3 Sort and Reverse Sort Treatments: We use the same contract assignments simulation from the Auction treatment in the Sort treatment. Sophisticated subjects do not change their beliefs of the composition of the unsophisticated subtypes, so $\tilde{\theta}_i = \theta$, $\forall i \in \{i : \text{Sophisticated}\}$. However, two additional steps are added to simulate the group assignment of subjects. First, the sorting of subjects is based upon $w_i + \varepsilon_i$, where $\varepsilon \sim N(0, \varsigma)$ is the sorting error. The standard deviation ς is a parameter estimated from the data. Then the half of the subjects who have $w_i + \varepsilon_i$'s above the median dropout wage are assigned to Contract 1 in Block 2. The other half of the subjects who have $w_i + \varepsilon_i$'s below the median dropout wage are assigned to Contract 2 in Block 2. In addition, differing from random group assignment within each contract in the Auction Treatment, subjects are sorted within a contract when assigned to groups, as in the actual experimental setting. This means, for example, the four subjects with the four lowest predicted dropout wages $w_i + \varepsilon_i$ are sorted into the same group in Contract 2, while the four subjects with the four highest predicted dropout wages are sorted into the same group in Contract 1.

As for the Reverse Sort treatment, we rank the predicted dropout wage $w_i + \varepsilon_i$'s from low to high. Then, the half of the subjects who have $w_i + \varepsilon_i$'s below the median dropout wage would be assigned to Contract 1 in Block 2. The other half of the subjects who have $w_i + \varepsilon_i$'s above the median dropout wage would be assigned to Contract 2 in Block 2. We also sort subjects into groups based on their dropout wages. Subjects are also sorted within a contract when assigned to groups. Sorting errors in the Sort and the Reverse Sort treatments are assumed to have the same standard deviation ς.

B.3 Estimation Procedure

Due to the structural complexity and non-linearity of the learning model, there is no closed-form solution for the model estimator. We therefore rely upon a moment-based simulation method

(Method of Simulated Moments) to estimate the model. To ensure a reasonable efficiency of our estimator, we generate 100 simulated datasets for each session of each treatment. In each of the 100 simulations, for each simulated subject, we redraw the noise term of effort choice each round, the noise term of auction (in the Auction treatment only), and the type of the individual (while maintaining the same proportions in the population). We use the average of the generated 100 simulated datasets to form model predictions.

The moments are the average efforts each round in Block 1 (10 moments), the average efforts each round in Block 2 by contract and by treatment (10 rounds $\times$ 2 contracts $\times$ 4 treatments). So we have a total of 90 moments. Let the set of moments be denoted by Γ.

For any particular set of parameters Θ, we take a set of aforementioned random draws for each subject and simulate subject behavior in an experimental session. For each session, we simulate the model $J = 100$ times using different sets of random draws. Each subject receives a random draw in the beginning of time to determine his/her own type and subtype (whether he/she is a sophisticated subject with/without strategic anticipation, a subtype l subject or a subtype h subject). The type and subtype of a subject are fixed for the entire simulated experimental session. The set of noise terms and type draws are fixed for all iterations of the estimation routine. To be consistent, we also use the same noise/type draws for all the estimation and counter-factual exercises in the paper.

For each $j \in \{1, 2, ..., J\}$, we can determine the simulated moments $\widehat{\Gamma}_m(\Theta)$, Then, the optimal estimator is defined as

$$\Theta^* = \arg\min_{\Theta} \left[\Gamma - \frac{1}{J}\sum_{m=1}^{J} \widehat{\Gamma}_j\right]' W \left[\Gamma - \frac{1}{J}\sum_{j=1}^{J} \widehat{\Gamma}_j\right]$$

where W is the optimal weighting matrix. Standard errors are obtained by bootstrapping. The estimation procedure takes into account that subject behaviors are potentially correlated across periods at the individual level. In other words, draws of the error term (see Equation 1 and 2) are independently distributed across different simulated subjects, but can be correlated for the same individual across different periods. The estimated parameter values from fitting the full model to data are presented in Table B.1.

Table B.1. Estimated Learning Model Parameters

Parameter	Brief Description	Estimated Values
μ_{pes}	Frac. of Unsoph. types who are pessimists	0.185***
		(0.027)
μ_{opt}	Frac. of Unsoph. types who are optimists	0.706***
		(0.034)
μ_{sa}	Frac. of Soph. types with strategic anticipation	0.031***
		(0.003)
ω	Initial experience weight	86.135***
		(29.176)
δ	Experience weight depreciation factor	0.859***
		(0.029)
λ	Effort choice logit parameter	0.458***
		(0.025)
ρ	Reset of belief at round 11	0.998***
		(0.025)
ζ	Auction dropout choice logit parameter	0.173***
		(0.065)
ς	Sort error standard deviation	85.900*
		(44.670)
	Sum of Squared Errors	1,202.20

Note: Standard errors are given in parentheses. Three (***), two (**), and one (*) stars indicate statistical significance at the 1%, 5%, and 10% respectively. The estimated proportion of sophisticated types without strategic anticipation in the population is $1 - \mu_{pes} - \mu_{opt} - \mu_{sa}$.

B.4 Alternative Model Specifications

The full model contains four different types of players, yielding a relatively complex model. It is reasonable to ask whether this complexity serves any useful purpose, either in terms of improving our ability to fit the data or, more importantly, our ability to track major features of the data and to predict out of sample. Subsection B.4 and B.5 address these questions. We fit four models, the full model and three models that simplify the full model by reducing the number of types. Ordered from least to most complex, the four models are as follows.

Model 1: Rather than having optimists and pessimists, all unsophisticated learners initially believe that all other subjects will choose effort 40 with probability θ^m and effort 0 with probability $1 - \theta^m$, where the parameter θ^m is estimated from data. There are no sophisticated learners ($\mu_{pes} + \mu_{opt} = 1$). Model 1 has no ex ante heterogeneity and no sophisticated learners.

Model 2: There are two subtypes of unsophisticated learners, optimists and pessimists, modeled as in the full model. There are no sophisticated learners ($\mu_{pes} + \mu_{opt} = 1$). Model 2 allows for

ex ante heterogeneity in beliefs but contains no sophisticated learners.

Model 3: This model adds a different type of heterogeneity than Model 2. As in Model 1, all unsophisticated learners initially believe that all other subjects will choose effort 40 with probability θ^m and effort 0 with probability $1-\theta^m$, where the parameter θ^m is estimated from data. Model 3 includes sophisticated learners. Since there is only one type of unsophisticated learner, there can be no selection of different types of unsophisticated learners into contracts. It follows that Model 3 does not include sophisticated learners with strategic anticipation as there is nothing for them to anticipate. We estimate the fraction of unsophisticated learners μ_{unsoph}, and the fraction of sophisticated learners is $1-\mu_{unsoph}$. Model 3 does not allow for ex ante heterogeneity in the beliefs of unsophisticated learners, but does allow for heterogeneity between unsophisticated and sophisticated learners.

Model 4: This is the full model presented in the manuscript and detailed above. Model 4 allows for *ex ante* heterogeneity in the beliefs of unsophisticated learners and heterogeneity between unsophisticated and sophisticated learners.

TABLE B.2. ESTIMATED PARAMETERS OF ALTERNATIVE MODELS

Parameter	Model 1	Model 2	Model 3	Model 4
μ_{pes}				0.185***
				(0.027)
μ_{opt}		0.715***		0.706***
		(0.041)		(0.034)
μ_{sa}				0.031***
				(0.003)
μ_{unsoph}			0.654***	
			(0.075)	
ω	43.787***	131.556**	79.6547**	86.135***
	(17.892)	(64.128)	(36.900)	(29.176)
δ	0.990***	0.870***	0.990***	0.859***
	(0.026)	(0.032)	(0.036)	(0.029)
λ	0.020***	0.400***	0.021***	0.458***
	(0.002)	(0.073)	(0.004)	(0.025)
ρ	0.938***	0.999***	0.999***	0.998***
	(0.026)	(0.027)	(0.024)	(0.025)
ζ	2.189	0.169***	0.585***	0.173***
	(2.827)	(0.075)	(0.185)	(0.065)
ς	0.060	59.987	0.163*	85.900*
	(0.084)	(43.701)	(0.090)	(44.670)
θ^m	0.998***		0.999***	
	(0.025)		(0.028)	
Sum of Squared Errors	6,041.980	1,934.620	3,600.385	1202.200

NOTE: Standard errors are given in parentheses. Three (***), two (**), and one (*) stars indicate statistical significance at the 1%, 5%, and 10% respectively.

We fit all four models to the full dataset; the parameter estimates are presented in Table B.2.[4] After fitting the model, we generate 100 simulated datasets for each of the fitted models. Table B.3 compares average effort in Block 2, broken down by treatment and contract, for the experimental data (Data) and the simulated data.

Table B.3. Comparisons of Average Efforts by Contracts and by Treatments Across Alternative Models

	Random Assign.		Auction		Sort		Reverse Sort	
	Contr.1	Contr.2	Contr.1	Contr.2	Contr.1	Contr.2	Contr.1	Contr.2
Data	21.17	31.34	9.64	39.81	13.45	37.48	30.03	16.06
Model 1	17.63	31.42	17.58	31.48	17.64	31.41	17.68	31.40
Model 2	19.02	28.13	8.17	39.90	8.51	39.30	38.18	16.36
Model 3	15.21	29.03	11.17	33.15	11.15	33.18	19.84	25.06
Model 4	16.75	28.62	7.23	39.95	8.53	37.32	31.43	19.67

As noted in Section 4 of the main text, fitting a model to the data does not guarantee either the ability to track existing data or to make predictions. The former point can easily be seen by comparing the experimental data and simulated data for Model 1 in Table B.3. At no point are the simulated data from Model 1 particularly close to the experimental data. The failure to track the data is especially bad for the Reverse Sort treatment, where even the flipped relationship between the two contracts is incorrect in the simulated data. It is unsurprising that Model 1 does poorly at tracking the data, as it contains no mechanism for capturing either the selection effect or the effect of strategic anticipation. Models 2 and 3 do better than Model 1 at tracking the data, but still have some failures (notably for the Reverse Sort treatment).

Table B.4 explores the ability of the various models (the full model and the three simplified models) to replicate one of the most important features of the experimental data. In the body of the paper, we calculate that 73% of the effect of endogenous assignment to high performance pay can be attributed to selection. One of our main uses of the learning model is to explore how this

[4]The inclusion of *both* subtypes of sophisticated learners improves the fit of the full model. Compared to the full model, if the model only allows for sophisticated learners *with* strategic anticipation, the sum of squared errors increases from 1202.2 to 1483.8. On the other hand, if the model only allows for sophisticated learners *without* strategic anticipation, the sum of squared errors increases from 1202.2 to 1432.7. The estimated fraction of sophisticated learners with strategic anticipation is small, at only 3.1%. Both strategic anticipation and sorting errors (captured by ς) contribute to differences in average effort between the simulated Auction and Sort treatments. If the model is fitted to the data without sorting error ($\varsigma = 0$), the fraction of sophisticated learners with strategic anticipation increases to 9.8%. This is consistent with our observation that the raw data overestimates the impact of strategic anticipation due to sorting errors.

figure would change if the Sort treatment perfectly replicated the selection mechanism from the Auction treatment. This exercise has little value if the model does a poor job of approximating the 73% figure with imperfect replication. The first column of Table B.4 reports, for the experimental data and simulated data from each of the four models, the percentage of the effect of endogenous assignment to high performance pay that can be attributed to selection.[5]

Table B.4. Comparison of Selection Effects Across Alternative Models

	% Due to Selection Contract 2	Reverse Sort Contract 2 - Contract 1
Data	73%	−13.97
Model 1	−20%	13.72
Model 2	95%	−21.83
Model 3	101%	5.22
Model 4	77%	−11.76

The final column of Table B.4 examines one of the most striking features of our data, the reversal in average effort between the two contracts for the Reverse Sort treatment. For the experimental data and the simulated data, the final column reports the difference in average Block 2 effort between Contract 2 and Contract 1 in the Reverse Sort treatment.

The full model (Model 4) does well along both dimensions, but it is the most complex of the four models. Is that complexity necessary to track major features of the experimental data, or is it sufficient to only incorporate heterogeneous initial beliefs (but no sophisticated learners) as in Model 2 or to only add sophisticated learners (but no heterogeneity of initial beliefs for unsophisticated learners) as in Model 3? Table B.4 indicates that the complexity of the full model is necessary. Because Model 2 has no mechanism for incorporating strategic anticipation, it overestimates the effect of selection (95% in the simulated data versus 73% in the data) and, as a side effect, overestimates the difference between Contracts 1 and 2 in the Reverse Sort treatment (-21.83 in the simulated data versus -13.97 in the data). While we don't expect any model to be perfect, these misses are far larger than those for Model 4. Model 3 is even worse, missing the flipped relationship between Contracts 1 and 2 for the Reverse Sort treatment. Model 3 also allows for selection, but like Model 2 cannot capture the effects of strategic anticipation. Strategic anticipation relies on sophisticated learners recognizing that the auction will sort different types of

[5]Recall that this is the difference between average Block 2 effort in the Sort and Random Assignment treatments divided by the difference between average Block 2 effort in the Auction and Random Assignment treatments.

unsophisticated learners into different contracts. When there is no heterogeneity of unsophisticated learners, this mechanism breaks down. The added complexity of Model 4 improves the model's ability to fit the data in the standard sense, meaning that the added parameters are statistically significant (see Table B.1), but more importantly the added features are necessary to capture the most interesting features of our experimental data.

B.5 Out-of-Sample Prediction

To validate our model's ability to predict out-of-sample, we estimate the full model using data from the Random, Auction and Reverse Sort Treatments, and simulate the model using the resulting parameters to predict outcomes in the Sort Treatment. The Sort treatment is designed to separate the effect of selection from the effect of strategic anticipation, and is one of the most important design elements in our experiment. Therefore, we use the model's ability to predict the Sort treatment to validate its ability to make out-of-sample predictions.

A comparison of the estimated parameters for the model fitted from the full dataset and the model fitted from three of the four treatments is presented in Table B.5. The parameters change very little. As a result, the differences in goodness of fits, as represented by the sum of squared errors, are negligible. This means, even when the model is fitted to the data of only three treatments, its predictions of the fourth (Sort) treatment are almost as good as the fully fitted version. In Block 2 of the Sort treatment under Contract 2, the predicted average effort (37.32) based on fitting the model to only three treatments is almost identical to the average effort (37.30) in the model fit to all four treatments and the experimental data (37.48). As a point of comparison, if we fit Models 1, 2, or 3 to the data from the Random, Auction and Reverse Sort treatments and simulate data for the Sort treatment, simulated average effort in Block 2 for the Sort treatment differs substantially for all three models from the observed data (30.33, 39.30, and 33.48 for Models 1, 2, and 3 respectively).

Table B.5. Comparison of Estimated Learning Model Parameters

Parameter	Brief Description	Fit 3, Predict Sort	Fit 4
μ_{pes}	Frac. of Unsoph. types who are pessimists	0.186***	0.185***
		(0.019)	(0.027)
μ_{opt}	Frac. of Unsoph. types who are optimists	0.708***	0.706***
		(0.016)	(0.034)
μ_{sa}	Frac. of Soph. types with strategic anticipation	0.030***	0.031***
		(0.003)	(0.003)
ω	Initial experience weight	84.253***	86.135***
		(25.669)	(29.176)
δ	Experience weight depreciation factor	0.863***	0.859***
		(0.031)	(0.029)
λ	Effort choice logit parameter	0.457***	0.458***
		(0.040)	(0.025)
ρ	Reset of belief at round 11	0.990***	0.998***
		(0.014)	(0.025)
ζ	Auction dropout choice logit parameter	0.172***	0.173***
		(0.067)	(0.065)
ς	Sort error standard deviation	86.033**	85.900*
		(35.094)	(44.670)
	Sum of Squared Errors	1,206.27	1,202.20

Note: Standard errors are given in parentheses. Three (***), two (**), and one (*) stars indicate statistical significance at the 1%, 5%, and 10% respectively. The estimated proportion of sophisticated types without strategic anticipation in the population is $1 - \mu_{pes} - \mu_{opt} - \mu_{sa}$.

Based on the same out-of-sample prediction simulations, Table B.6 expands on the preceding points. We carry out a similar exercise to what was reported in the first column in Table B.4. The full model does well at predicting average effort in the Sort treatment, and hence does a good job of predicting the percentage of the effect of endogenous assignment to high performance pay that can be attributed to selection. The other models once again do poorly.

Our point here and in Section B.4 is not that the full model is somehow the best possible model of learning from the vast universe of possible models, either in terms of tracking the major features of our experimental data or predicting out-of-sample within the framework of our experiment. Identifying the best model is well beyond the scope of our paper. Rather, our goal is to show that our model does a reasonably good job of tracking major features of the experimental data as well as predicting out-of-sample, and a simplified version of our model would do far worse. This gives us some confidence in the results of the counterfactual exercises based on the full model.

Table B.6. Comparison of Selection Effects Across Alternative Models, Fit-3-to-Predit-1

	% Due to Selection Contract 2	Reverse Sort Contract 2 - Contract 1
Data	73%	−13.97
Model 1	96%	12.19
Model 2	95%	−22.51
Model 3	101%	5.17
Model 4	77%	−11.92

B.6 Counterfactual Policy Experiments

As mentioned in the manuscript, we use the estimated model to make predictions of (1) the selection effect with sorting errors removed, and (2) the average efforts in both contracts with varying proportions of subjects assigned to Contracts 1 and 2. In both of these prediction exercises, we use the estimated parameters ("Fit 4") of the full model (Model 4). In the first exercise, to simulate the sorting process with no sorting error, we set the standard deviation parameter $\varsigma = 0$ while holding remaining parameters fixed. In the second exercise, we enlarge the simulated sessions to allow for 20 groups and vary the number of groups receiving Contract 2 from 0 to 20. For this counterfactual exercise, we simulate the average efforts for each contract under the Auction treatment. The predictions are reported in the manuscript.

After estimating the full model, we simulate a counterfactual experiment, where subjects are randomly assigned within-contract in the Sort and the Reverse Sort treatments rather than being sorted by their dropout wages. Random assignment to groups within-contract mirrors what we did in the Auction treatment. The difference of model outcomes between the original and the counterfactual settings are negligible. For example, the simulated average effort of groups in Contract 2 under the Sort treatment is 37.32 when group assignments are sorted, and it is 37.28 when groups assignments are random within the contract. These figures are 19.67 and 20.21 for the Reverse Sort treatment, respectively. This counterfactual exercise supports our claim that the choice to sort groups in the Sort and Reverse Sort treatments had little effect on our results.

APPENDIX C: Experimental Instructions

C.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.

C.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%

C.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

C.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.

Quiz

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.

C.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.

C.3. Stage 3

C.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.

Practice Round

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.]

C.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.

C.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.