

# Incentives, Distortions, and Peers\*

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Trevor Gallen, Yana Gallen, Steven Levitt, and John List <sup>†</sup>

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## Abstract

In this paper, we present the results of a natural field experiment in which workers operated telephones soliciting funds for a major charity in the US. We find that incentives increase targeted performance at the cost of other dimensions of a worker's task. Incentivized workers were paid for the fraction of pledges—promises to donate at a later date—which they secured. Pledges were 50% higher for callers paid a commission relative to callers paid a flat rate. However actual donations (excluding outliers) were 17% lower when a donor was called by a caller paid a commission rather than a flat rate. Incentives also caused workers to break the rules of their employment in order to increase their pay. Commission-based pay caused rule-breaking to nearly double. Finally, in our experiment, workers with different types of compensation worked at the same time and could observe one another's performance. We use the randomization-induced variation in worker performance to study whether incentives benefit firms via peer-effects. We find no evidence that productivity increases spill over onto peers, however, we find that rule-breaking generated by incentives does spill over onto peers.

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<sup>†</sup>Correspondence: Yana Gallen Tel.: (815) 558-8693. Email: yana@uchicago.edu

# 1 Introduction

Performance incentives have affected a growing fraction of the labor force in the past decades, but still make up a fairly small fraction of low-wage jobs. Lemieux et al. (2009) note that 45% of salaried workers had performance pay jobs in the late 1970s, but this figure increased to nearly 60% by the late 90s. In contrast, performance pay among hourly-wage workers is a steady and modest 20%. Beginning with Lazear (2000), studies of the effects of performance pay find that incentives substantially increase productivity and even firm profitability. These studies are generally concentrated in low-wage settings where, as noted in Lemieux et al. (2009), performance pay contracts are not the norm. Why do employers seem to avoid performance pay contracts if they are so profitable? To answer this question, we analyze the results of a natural field experiment in which compensation—piece-rate vs. flat rate—varied across participants, even those working the same day.

In our experiment, workers operated telephones in a “phone-a-thon” soliciting funds for a major charity in the US. If a potential donor were interested in making a donation, he would not pay over the phone, but rather would make a “pledge” to donate when he receives an envelope in the mail. Callers randomized into performance pay treatments were paid based on the pledges which they secured, rather than the donations to the charity which their calls eventually generated. We find that incentives increase performance but incentives also introduce undesirable distortions. In the long run, the effects of incentives on the actual giving behavior of donors is much weaker than the effect on pledges. In fact, there is some evidence of a negative effect of incentives on actual donations conditional on a caller securing a pledge.

Performance pay has been shown to improve the performance in a variety of settings: among teachers (Lavy, 2009), tree-planters (Shearer (2004), Paarsch and Shearer (1999)), assembly workers (Lazear, 2000), and of course in the lab (van Dijk et al., 2001). In our setting, commission-based incentives are strong, generating an almost 50% increase in the incentivized outcome. When jobs are multidimensional and it is difficult to tie worker effort to firm profit, Holmstrom and Milgrom (1991) argue that workers will target the incentivized dimension of effort at the cost of others. This prediction holds in our data: excluding outliers, a donor gives 17% less when contacted by a commission-pay caller. Even though pledges are closely tied to and predictive of donations, commissions which are based on pledges rather than donations increase pledges but do not increase donations.

We are able to get at the mechanisms behind the difference in pledges collected vs. donations. By recording calls, we found that incentivized callers were more likely to break the rules in order to secure a pledge. Incentivized callers are more aggressive in sending pledge cards to reluctant donors and are more likely to knowingly ask the wrong person for donations. Both aggressively pushing reluctant donors into making a pledge and asking for money from someone not on the caller's list was against the rules and examples of such violations were a part of caller training. Interestingly, there is some evidence that rule-breaking behavior spills over onto coworkers. Our research design allows us to look at how incentive effects—both increased pledges and increased rule-breaking—spillover onto peers.

The growing literature on peer effects in the workplace is quite related but so far unconnected to the effects of incentive-based pay. To the extent that peers respond to one-another's performance, incentives may increase not only a given worker's output, but also the output of his peers. Mas and Moretti (2009) find that grocery store scanners respond to the productivity of their coworkers. Workers scanned faster in those cases where high-productivity coworkers are positioned behind them and therefore able to observe their output. Social considerations also play a role. Falk and Ichino (2006) perform a laboratory test of peer effects in the workplace and find that average output of workers stuffing envelopes was higher the variance of output was lower when workers worked together compared to when they worked alone. Bandiera et al. (2010) find that fruit-pickers' performance converges to the productivity of their friends. Park (2015) finds that workers were willing to forgo 6% of their wage to socialize with friends.

Using German administrative data, Cornelissen et al. (2013) study to peer effects in wages (rather than productivity) and find that co-workers ability hardly impacts own-wages for high-skilled workers. However, for low-skilled workers, the effects of co-worker ability on own-wages about half the size of the productivity effects estimated in Mas and Moretti (2009).

In general, peer effects have not been found in higher-skill occupations. Using variation in the number of German researchers in physics, chemistry, and mathematics in a university dismissed by the Nazis between 1925 and 1938, Waldinger (2012) finds no evidence of peer effects in research productivity at the department of specialty level, however, he and Azoulay et al. (2010) do find effects at the co-author level. Turning to high stakes tournaments in professional golf, Guryan et al. (2009) find no evidence of peer effects, exploiting random assignment. Guryan et al. (2009) argue that the productivity of peers changes behavior when performance incentives

are otherwise weak.

To our knowledge, virtually all studies of peer effects relate day-to-day performance of a worker to the average “productivity” of his coworkers. We emphasize the fact that this productivity level can also be chosen by the firm via incentives. To the extent that incentives increase performance, they may also have spillover effects on the performance of co-workers. We test whether a worker’s performance on a variety of measures is affected by the extra effort of coworkers, exogenously introduced through performance incentives via randomization. We don’t find evidence of incentive spillovers onto the performance of peers, except in the negative outcome space: callers were 60% more likely to break the rules by asking the wrong person for a donation when their peers were induced to do so via incentives.

Overall, the results of our natural field experiment confirm the strong power of incentives to increase targeted performance at the cost of other dimensions of a worker’s task. In our case, incentives did not increase donations but did increase pledges. Incentives also cause workers to break the rules of their employment in order to increase their pay. Finally, there is evidence that this rule-breaking spills over onto peers, though we find no evidence that incentive-induced productivity increases spill over onto peers. This paper proceeds as follows. Section 2 outlines the experimental design. Section 3 presents our results on the direct effects of the experiment as well as peer effects. Section 4 concludes.

## 2 Experimental Design

In our experiment, 51 students ranging in age from 19-39 were interviewed for a job soliciting donations for a major national charity. Of those interviewed, all were offered a job and 30 of those came to work at least one day. Each day, callers worked a total of three hours making telephone solicitations.

Callers could choose, without restriction, how often they wanted to work. However, we sent callers home early if only one person came to work. We observe 104 work days, where each work day consists of an average of 3.48 callers in a given day (and a median of 3 callers a day). The distribution of number of callers across days by treatment is given graphically in Figure 1.

Callers were not paid identically. During the interview, callers were told how they would be paid. Callers were randomized into a treatment of either a flat rate payment of \$10 per hour worked (F), a commission of 60% of their reported pledges (C), a commission of 60% of reported

pledges and also 20% of donations collected from the same contacts six months later (CF), or 40% of pledges collected and also 20% of donations collected from the same contacts six months later (CFN).<sup>1</sup> Commissioned callers earning less than minimum wage (\$8/hr) on average over an entire pay period lasting two weeks were paid the minimum wage. Callers rarely made less than the minimum wage in pay-period. In Figure 2, we plot the compensation scheme for 60% commission callers working one day in the pay-period. There was no cap on commission-based pay.

During the first “burn-in” week of calling, we paid only the flat rate to all callers. The commission rate was established from data on pledge rates in this first week. Those working the first week were told that their payment would be a flat rate for that week, but may change the next week. Then, those working the first week were randomized into treatment via email<sup>2</sup>. This change in treatment of some callers from flat to commission provides the experiment with some treatment variation within agent as a complement to the main variation between subjects. One caller was fired due to suspicion of credit card fraud, all other callers left the experiment of their own volition. Finally, we note that several commissioned callers were eventually told that they would be transitioned to a flat rate in the following week. No caller on commission ever accepted this offer (all quit before we were able to observe them working at the flat rate). We discuss the possible effect of selection on our estimates in section 3.4 below.

Our data from this experiment consists of caller logs describing the outcome of each call and the amount of each pledge, transcriptions of each recording of a caller.<sup>3</sup>, an interview questionnaire that allowed us to collect demographic information on each caller, a traditional personality test, as well as a rough measure of intelligence and sociability, and outcomes of the phone-a-thon: the actual donation amounts of potential donors. We discuss the procedures in more detail below.

At the beginning of every shift, which lasted from 5:30PM to 8:30PM, callers were given a call log, which was a list of randomly generated donors and scripts associated with each potential donor (scripts differed by the ask-amount). The donor list was provided by the charity and randomly assigned to callers by experimenters. Callers could not control the logs which they

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<sup>1</sup>Another treatment referred to a CCF of 80 percent of pledges was dropped in the first weeks of calling for budgetary reasons. We do not analyze that data.

<sup>2</sup>Nine people interviewed before the experiment started had no contact (didn’t receive mandatory training and/or refused the job) before we sent randomization email, so they were never assigned to treatment

<sup>3</sup>We did not record both sides of the conversation, only the caller’s voice.

received. Each log had fifty potential donors on it, and callers were given three caller logs at the beginning of every workday. If they called everyone on these logs before 8PM, they were given additional logs. Donor names appearing on the logs were selected from a list of 69,154 past donors to the charity who had not donated in the past 36 months. These donors were excluded from other charity mailings over the course of the experiment. On their call logs, phone-a-thon callers saw a randomly generated donor id number, the donor name, the donor address, the donor phone number, the year in which the donor last gave to the charity, and the amount there were supposed to ask the donor to donate.

Callers were to (1) call the number listed for a potential donor, (2) give a “pitch” asking for the specified donation and whether the response was positive or negative, and (3) attempt to verify the address on file.<sup>4</sup> The callers also recorded whether or not they reached an answering machine, whether the person requested not to be contacted again, whether they were’t available to answer the call, and whether the number was disconnected. In the case that they were able to reach the donor, the callers recorded the amount of money the donor pledged to send back to the charity. If this number was greater than zero, the callers filled out a pledge card with the donor’s name and pledge amount. Experimenters later placed these pledge cards and a return envelope into a mailing which was sent to the donor. Donors interested in making a donation sent back a check, cash, or payment information for a credit card transaction using the pre-addressed envelope enclosed in the mailing. Of course, donors could also donate online or using any old mailings they had. We take into account all donations recorded by the charity by May of 2009 for donors contacted by phone-a-thon callers.

Callers used traditional dial-up telephones when making phone calls and sat in short cubicles. There were 8 available cubicles on a given day (though up to 4 extra cubicles could be cleared if necessary). The cubicles were in one row with two sides, so 4 callers sat with their backs to one wall of the room and 4 callers sat across from them, with a cubicle wall in-between them. Callers had instructions pasted to their desks (how to dial out, the rules governing their behavior) a telephone on their desks, the call logs, a pen, and a stack of pledge cards. In the case that a caller ran out of pledge cards, he could ask a manager (experimenter) for more. Callers were free to use the rest room, get drinks of water, and potentially socialize. A microphone was taped underneath each caller’s desk and connected to a small iPod in the corner of the floor under

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<sup>4</sup>The script callers were given as a sample can be found in the appendix. They were allowed to deviate from the script (within boundaries).

each cubicle which recorded the caller's voice. The experimenters did not discuss the recordings with workers except during the interview, when interviewees were told that their voice may be recorded. Each cubicle had three walls which gave the callers some privacy, but they could be overheard by everyone. An experimenter was always in the room with callers to answer any questions, but the experimenter was instructed to avoid interrupting callers or conspicuously monitoring callers as much as possible. Instead, the experimenter read a book or worked on their laptop, facing away from callers. The purpose of this was to give callers the sense that they would not be reprimanded if they broke the rules, used a new script, or simply didn't work hard.

Callers who arrived late were not reprimanded. Callers who arrived slightly early were permitted to start working early. Socialization was not encouraged and experimenters rarely spoke to callers except during training. Training consisted of a 15 minute demonstration of the room, review of the rules, and a sample call. An experimenter simply called another experimenter and read the script, putting the call on speaker phone. Callers were told that it was great if they secured money for the charity, but that they weren't working for the charity. Instead, they were working for a research professor and he was doing an experiment studying how ask amounts influence charitable giving.

Callers believed that they were part of an experiment on donors involving the script from which they read.<sup>5</sup> They were deliberately left relatively unsupervised to allow them to deviate from script if they wished. An indication of success on this dimension was given by a quote from a worker, asked what he thought of the job after the experiment was over:

“I thought the job was very poorly run. The participants were weakly supervised and had no hesitation going off the script. While I understand that this may have been part of the experiment,...I still strongly feel that the results of the experiment were probably largely skewed because of the poor job done by those managing the individuals calling donors.”

This comment reflects success on the part of experimenters in maintaining a low-supervision environment. We believe this environment was essential because it gives incentivized callers the maximum freedom to experiment with different asks, and it made callers more likely to break

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<sup>5</sup> Ask amounts were \$20, no specific amount, or \$20.0 $x$  where  $x$  ranged from 5-8. The results of this experiment are not reported here

the rules. Short of having no rules to break, we maximized caller freedom.

To pay callers on commission, the experimenters calculated their total pledges at the end of every day and recorded these. Every two weeks, callers were given a form to sign indicating how much they made that day. Callers were emailed payment information and could sign the forms at their convenience.<sup>6</sup> In this sense, the experiment simulated a typical job. Finally, after the experiment was over, callers were emailed asking them if they had any suggestions for improvements in the phone-a-thon or comments about their experience.

A summary of the important variables in the data set is displayed in Table 1. The callers had only a 22% chance of making contact with any donor on their list—most often, they reached an answering machine. Callers did not take payment over the phone, but instead, took pledges. The average pledge amount was \$25.86, but the donations that the charity saw as a result of these pledges were lower, \$20.80 on average (among those potential donors who callers contacted). Among those who promised to donate, 42% fulfilled their promise.

Turning to data collected in the call recordings, we see that one percent of calls involved rule-breaking in the form of asking the wrong person for money. Callers were instructed only to ask for a pledge from the potential donor whose name appears on their call log, not from their wife or anyone else who answers the phone. In addition to this rule, callers were ask instructed never “push” for pledges, specifically, never say something like “why don’t I just send you a pledge card for \$20 and you can decide later if you want to send money back?” Two and a half percent of connected calls involved pushing a potential donor for pledge. We emphasized these rules by passing them prominently on every desk in the workplace and on the walls in the workspace. The callers knew that engaging in this behavior was against the rules.

We are balanced on observable variables (race, gender, age, iq, and personality) in the initial randomization. Table 2 displays summary statistics on demographics and the results of a t-test of commission vs. flat rate callers. There are not statistically significant differences between commission callers and flat-rate callers in our measure of intelligence<sup>7</sup>, in the fraction who

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<sup>6</sup>Callers who were incentivized on future donations were also paid the additional amount after the experiment was over and notified of their payment via email. These extra payments were less than \$200 per caller, and calculated by taking a fraction of donor giving in the sixth month after he or she was contacted by a caller.

<sup>7</sup>We form an index of intelligence from the number of correct answers to the following three questions: 1. A bat and a ball cost \$1.10. The bat costs \$1 more than the ball. How much does the ball cost? 2. If it takes 5

are non-white<sup>8</sup>, in their reported GPA, the proportion male, age, or answers to a measure of sociability.<sup>9</sup> We also test whether caller answers to 20 traditional personality test questions vary by treatment<sup>10</sup>. One question has a p-value less than 10% using a Wilcoxon signed-rank test (though an additional two are approximately 10%), so we are balanced on the dimension of personality as well. The results of this test are in Table 3.

### 3 Results

In this section, we describe the results of the experiment outlined above on three dimensions. First, we outline the direct effects of commissions on outcomes measured and recorded by the callers, such as pledges collected and calls per day. We also describe the effects of the experiment on donations actually received by the charity. Second, we describe the effects of commissions on “cheating” by callers, measured in phone call recordings. Finally, we describe spillover effects of incentives on peers, looking at both spillovers in terms of caller output and in terms of cheating behavior.

#### 3.1 Direct effect of incentives

Table 4 reports the results of the following regression

$$Y_{itc} = \alpha + \beta D_{it}^{\text{Treat}} + \gamma X_{it} + \epsilon_{it} \quad (1)$$

where  $Y_{itc}$  is the outcome of interest for caller  $i$  on date  $t$  in call  $c$ ,  $D_{it}^{\text{Treat}}$  indicates whether or not a caller  $i$  was paid by commission on date  $t$ .  $X_{it}$  is a vector of controls, which (depending on the specification) include date fixed effects, a cubic in experience, and controls for caller race

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machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets? 3. In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half the lake?

<sup>8</sup>Most non-white callers are Asian.

<sup>9</sup>This was the sum of pro-social answers to the following three questions: 1. Generally speaking, would you say that most people can be trusted or that you can’t be too careful in dealing with people? 2. Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair? 3. Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?

<sup>10</sup>The personality test questions are page 2 of the employment application in the appendix. They are similar to questions employers use to measure “Big 5” personality traits.

and gender. The specific controls included in a given regression are indicated in the bottom panel of the table. All regressions are clustered on caller id.

As described above, commissioned callers were incentivized based on the pledges that they collected. These pledges were promises of future donations—a donor would pledge that when he received an envelope in the mail from the charity, he would send back some specific amount (on average, \$25). We find that callers paid a commission received about 22 cents more in pledges per call, almost a 50% increase over the experiment-wide average pledge of 48 cents per call. This result is robust to inclusion of date fixed effects, a cubic in caller experience, and demographic controls.

In our setting, there are multiple ways a caller can affect donor behavior: he can change the probability of a donation in response to his call both by securing a pledge and affecting the likelihood that the pledge is fulfilled, and the caller can change the overall predisposition of a donor to give in the future. In this sense, pledges collected are not necessarily the metric which the charity cares about. The charity cares about the present discounted value of all donations from a given donor, but this is difficult to directly incentivize. In particular, a donor may be contacted many times over time by different campaigns. The mapping of employees to outcomes over time quickly becomes complex and costly over the course of several fundraising campaigns. In addition, employees may have substantially higher discount rates than the charity or may not trust the charity to deliver payment over the course of many years. As discussed in Holmstrom and Milgrom (1991), incentivized agents may sacrifice performance on non-incentivized dimensions of a complex task. Consistent with this, we find that actual donations received by the charity are insignificantly higher for calls made by commissioned callers, but this difference is driven by one very high donation. When we cap donations at \$375 (the 95th percentile of donations), we find a significant *negative* effect of being contacted by a commissioned caller of 45 cents on the final donation amount.

Both the probability of a pledge and the probability of a donation are higher when a potential donor is assigned to a commissioned caller. However, conditional on making a pledge to donate, donors are not more likely to give larger amounts when they are contacted by commissioned callers. In the last row of table 4, the outcome Donation-Pledge gives the difference between

the donation amount collected in a given call and the pledge amount promised in that call. There is an insignificantly negative relationship between being assigned a commissioned caller and the difference between the amount donated and the amount pledged. Given the huge effect of being assigned a commissioned caller on the amount a donor pledges, these null effects of being assigned a commissioned caller on final donations (the outcome of interest to the charity) are striking. This could be because commissioned callers elicit pledges from more marginal potential donors, or because they have a directly negative effect on donations. We discuss these possibilities again when studying the recorded behavior of callers.

Caller-day outcomes ( $Y_{it}$  rather than  $Y_{itc}$ ) are reported in Table 5. In Table 4 we found that callers paid a commission received about 20 cents more per call. This adds up to about thirty dollars more per day of work, holding number of donations constant. However, as shown in the first row of Table 5, callers paid a commission also made 34 more calls per day when compared to flat rate callers. The sum of these effects translates to on average \$53 more in pledges received by callers in the commission treatments each day. Commissioned callers secure significantly more pledges and donations per day, as in the last two rows of Table 5. Truncating the top 5% of donations at \$375, this extra effort by commissioned callers does not make up for lower donations received by these callers and the total effect on donations is insignificantly negative. In the next section, we investigate how incentivized callers were able to secure more pledges by making use of call recordings.

### 3.2 Cheating

The callers' voices were recorded and transcribed while they were making calls. By analyzing these transcriptions, we can check whether callers are more likely to break workplace rules in order to secure a pledge. There were two rules which were easy to break and would result in an increased pledge count (so more pay for commission-based callers). Callers were instructed to (1) ask for pledges only from the person whose name appears on the call logs, and (2) never "push" for pledges, specifically, never say something like "why don't I just send you a pledge card for \$20 and you can decide later if you want to send money back?" We emphasized these rules by passing them on every desk in the workplace and on the walls in the workspace. Callers

were unambiguously aware that they were breaking rules when they engaged in this behavior.

After transcribing the recordings, we can check for both types of behavior. Transcribers coded whether or not a call included asking the wrong person for money and whether it included pushing the donor for a pledge card. We include transcriber's instructions in the appendix. Table 6 gives the results of incentives on the average number of times a caller asked the wrong person for a pledge (such as the relative of a listed donor, or whoever answered the phone instead of the potential donor on the charity's warm list). Commissioned callers asked the wrong person for money in one percentage point more of the calls they made in a given day. In the flat treatment, asking the wrong person for money occurred in only 0.51% of calls, so there is almost a 200% increase in this behavior among commission callers. Turning to the second row of Table 6, we see that commissioned callers "pushed the pledge" in about two percentage points more of the calls they made in a given day. Flat rate callers pushed the pledge in only 1.46% of their calls, so this is also a dramatic increase.

This increased propensity to cheat when making calls suggests that callers are in part increasing pledges by sending pledge cards to people unlikely to send back donations. When sending pledge cards to uninterested potential donors, callers are potentially harming the donor's opinion of the charity. Worse than that, callers are interfering with the experiment they are supposed to be performing. Because "pushing" these pledge cards requires the callers to suggest a specific donation amount even if the donor wasn't necessarily interested in giving that amount, the callers are contaminating the interpretation of the effect of different scripts on caller giving. Asking the wrong person for a donation was harmful because the pledge cards were automatically addressed to the donors on the call log and when the person on the call log receives a pledge card for a pledge he did not make, this naturally can affect long run giving behavior. In the next section, we consider the possibility that the changes (positive and negative) in worker behavior induced by paying them a commission spillover onto their coworkers.

### 3.3 Peer effects

We first use coworker treatment as an instrument for coworker performance, circumventing the reflection problem discussed in Manski (1993). We run the regression:

$$Y_{-it} = \omega + \xi \text{PropPeersTreated}_{it} + \tilde{\gamma} X_{it} + e_{it} \quad (2)$$

This gives the average performance of coworkers of worker  $i$ ,  $Y_{-it}$  on date  $t$ , controlling for the average treatment assignment of those coworkers (proportion in commission based pay schemes) and a variety of other controls, such as demographics of caller  $i$ , experience controls, week fixed effects and day of week fixed effects. We interpret the predicted outcome of worker  $i$ 's coworkers,  $\hat{Y}_{-it}$  as the variation in coworker performance induced by randomizing coworkers into commission pay. From section 3.1 and 3.2 above, we know that commission powerfully effects performance, giving a strong first stage.

In the second stage we study whether the increase in worker performance induced by incentives affects his peers. We run the regression

$$Y_{it} = \alpha + \beta \hat{Y}_{-it} + \gamma X_{it} + \epsilon_{it} \quad (3)$$

where  $Y_{it}$  is the outcome of interest of worker  $i$  at date  $t$ ,  $\hat{Y}_{-it}$  is the predicted performance of his peers from regression (2), and  $X_{it}$  is a vector of controls as described above.

We study seven outcomes: number of calls made, donations received, pledges received, number of pledges, number of donations, proportion of calls which resulted in asking the wrong person for money, and proportion of calls which resulted in pushing the pledge. These are the caller-day outcomes for which we have a strong first stage. Table 7 gives the second stage coefficients  $\beta$  for these seven outcomes using a variety of controls.

We can reject large positive peer effects in the number of calls made. On average, a caller makes 151 calls per day. Moving from no peers in the commission treatment to all peers in the commission treatment,  $Y_{-it}$  will increase by approximately 30 calls per day. The point estimates in column (3) suggest this increase in peer performance will decrease own-calls per day by 1.2

calls. We can reject that it increases own calls per day by more than 5.84 calls per day, or more than 3.7%. In column (4) we add day-of-week and week fixed effects to the set of controls and this gives significantly negative estimates for peer effects on calls per day, however, this should be interpreted with caution since date controls mechanically negatively bias the results<sup>11</sup>.

Our results for donations and pledges collected are quite noisy. Moving from all flat-rate coworkers to all commission-pay coworkers, a caller would see his coworkers pledges and donations increase by approximately \$50 and \$30, respectively. The point estimates in column (3) of Table 7 imply that such a change would decrease own-pledges by \$6.45. We can reject effects larger than \$14.55, or 20%, though 20% would be a large effect. Such a change would increase own donations by \$8.7, but this too is very noisy estimated. Effects on own-number of pledges and donations are similarly noisy.

One outcome, asking the wrong person for a pledge, is robustly positive. Moving from all flat-rate coworkers to all commission-pay coworkers, a caller would see his coworkers ask the wrong person for a pledge .9 percent more of the time, on average. Callers ask the wrong person for money 1 percent of the time, so the IV estimates in column (3) suggest that callers ask the wrong person for money 60% more when their peers move from being paid a flat rate \$10/hr to being paid a commission. The level of this outcome is robust to controls.

Though callers are more likely to break the rules by asking the wrong person for a pledge when their peers do so, we don't find evidence that they are more likely to "push the pledge" when their peers do so. This may be because pushing a potential donor into making a pledge involves aggressive salesmanship, something which is likely costly to callers. Asking the wrong person for money does not otherwise involve changing the script the caller is using. Another reason why callers may fail to adopt pledge-pushing behavior is that it's less black and white than asking the wrong person for money. Hearing a co-worker push a donor into accepting a pledge card may make him more aggressive in soliciting donations, even if he doesn't cross the line into pushing an unwanted pledge on a donor. Asking the wrong person for money is not a continuous outcome—the person on the phone either is or isn't the person on the call log and

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<sup>11</sup>Date fixed effects, for example, would de-mean the data by date so that if an employee were performing above average, his coworkers would mechanically be performing below average

the caller decides whether he will ask this person for a donation.

### 3.4 Selection

Two related types of selection can affect the interpretation of our results: (1) the relationship between caller skill and caller tenure (which may differ by treatment), and (2) the relationship between caller skill and the probability of agreeing to work under a given incentive scheme. Our results should be interpreted as giving the full effect of offering a particular compensation scheme to workers, not asking what a worker’s productivity would be were he forced to work under a given type of compensation. While we are well balanced at randomization (as in Table 2) we are somewhat unbalanced in gender conditional on working, as shown in Table 8. Women are more likely to elect to work in the flat rate and men are more likely to elect to work in the commission treatments. The most striking evidence of selection is in the proportion of workers who agree to work conditional on treatment. 80% of workers assigned to the flat-rate treatment agreed to work while only 67% of workers assigned to a commission treatment elected to work. Moreover, when commissioned callers were told that they would be transitioned to a flat rate in the following week, none agreed to work in that week. Of course, for these experienced workers, this would have effectively been a pay-cut so this selection is understandable.

Flat rate callers worked on average three days longer than commissioned callers, but there is more dispersion in tenure among commissioned callers. In addition, there is a difference in the relationship between caller skill and tenure by treatment. Figure 5 displays the relationship between caller fixed effects from a regression of total pledges per day on a cubic in experience and date fixed effects, which we interpret as caller skill, and a caller’s tenure. While flat rate callers stay fewer days when they are more skilled, commissioned callers stay in the experiment longer when they are more skilled.

Selection affects the interpretation of our results. The differences between flat rate and commission on pledges collected, donations received, and cheating are differences in offering workers a given treatment, and include selection into working. It should not be interpreted as the effect of forcing a given person to work under different incentive schemes. This of course is also true of our first stage in the peer-effects regressions.

Our peer-effects regressions are potentially plagued by another type of selection—selection into which days a caller works. In the extreme, imagine worker productivity was hit by a shock  $\alpha_t$  on each day. If the probability a commissioned caller works is positively correlated with  $\alpha_t$ , while a flat-rate caller’s decision to work is less correlated with  $\alpha_t$ , this would violate the exclusion restriction for our “fraction of callers on commission” instrument for worker productivity. The ideal experiment would assign workers to a given workday randomly and would have no attrition. To the extent that commissioned workers target working on productive days, our peer-effects regressions are biased towards finding peer effects. We hope to control for this type of selection using day of week and week of work fixed effects<sup>12</sup>.

We test for date-based sorting by simulating the distribution of callers we would expect if all callers randomly chose when to work within their start and end date, and compare this to our observed distribution of callers. For each caller  $i$ , we estimate his probability  $P_{it}$  of working on date  $t$  as

$$P_{it} = \begin{cases} 0 & \text{if } t < S_i \\ \frac{1}{N_i} & \text{if } S_i \leq t \leq F_i \\ 0 & \text{if } F_i < t \end{cases} \quad (4)$$

where  $S_i$  is caller  $i$ ’s first day of work,  $F_i$  is caller  $i$ ’s final day of work, and  $N_i$  is the total number of days caller  $i$  works. This gives caller  $i$  a uniform probability of working any day between his start and end date. For each caller and each date, we simulate the work decision (work or don’t work), given the Bernoulli parameter  $P_{it}$ . For each date, we then calculate the fraction of workers who are being paid a commission in the simulation. We do 10000 simulations for each work-day<sup>13</sup>. We then take the observed fraction of commissioned callers working on a given day and look up the cumulative probability of observing that fraction of workers on commission based on the simulation.<sup>14</sup>

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<sup>12</sup>Mechanically, we cannot control for date fixed effects in the peer effects regressions, but we can (and do) control for date fixed effects in all other regressions

<sup>13</sup>excluding the 5 day burn-in period, leaving us with 92 caller-days. We also simulate the work-day cancellations when only one person signs up to work, which happened in our actual experiment.

<sup>14</sup>Since there are only a finite number of callers of each type (flat- or commission-pay) the distribution is not continuous. We assign a day a random value in the probability bin. Even if we assigned the data to the maximum probability in the simulation (for example, assigning all observations with 100% commissioned callers a p-value of one), our K-S test’s p-value is 0.14. This extreme assignment rule would strongly bias us towards rejecting that workers were sorting randomly, so we are confident in the main result.

We plot the empirical distribution of p-values and compare it to the theoretical distribution (uniform) in Figure 6. The p-value for a Kolmogorov-Smirnov test is 0.978. We do not detect any special sorting by callers being paid a commission and so are confident in the exclusion restriction in our peer-effects IV regressions. Adding week and day of week fixed effects in column 4 in Table 7 introduces some as towards finding peer effects, but would mitigate sorting effects on these dimensions. Since we don't find evidence of sorting on date worked, we are most comfortable with the specification in column 3 of Table 7.

## 4 Conclusion

In our experiment, similar workers were privately offered different types of compensation. Some workers were paid based on the size of pledges—promises to make donations to specific charity in the future—they were able to generate when calling lapsed donors to the charity. Other callers were paid a flat rate of \$10/hr. We find that incentives greatly improved caller performance on the incentivized dimension. Callers being paid a commission had pledge amounts 50% higher than callers being paid a flat rate. This was both because they increased the number of calls they made (extensive margin) and because any given call resulted in larger pledges. Though on average a call made by a caller being paid a commission is more likely to result in a donation, this is because commissioned callers secure more pledges. Conditional on securing a pledge, a call from a commission-pay caller is less likely to result in a donation. Trimming outlier donations, commissioned callers do not secure more money for the charity and at the call-level actually receive smaller donations. The opposite is true of pledges.

In addition, callers being paid a commission are more likely to break the rules in order to secure pledges. Our experiment had two main rules which were broken in about 1% of calls: never push a reluctant donor into making a pledge and never ask anyone for money who does not appear on the call list. Callers being paid a commission were about twice as likely to break these rules as un-incentivized callers. These results overall support a Holmstrom and Milgrom (1991) picture of the potentially detrimental effects of incentives in complex tasks and help explain why despite their power to increase performance, performance-pay contracts are not the norm

in low-skilled jobs.

In our experiment, callers being paid using different compensation schemes often worked together. This set-up allows us to check whether the effects of incentive pay spill over onto peers. In particular, we use peer treatment assignment as an instrument for peer-performance in a traditional peer-effects regression. Because we isolate variation in peer-performance induced by incentives, we are looking for peer effects in a dimension relevant for employers—the dimension of performance they can control—but which to our knowledge has not previously been studied in the peer effects literature. We are able to tightly reject peer effects in effort (number of calls made) and we find some evidence that rule-breaking spills over onto co-workers. We find that callers ask the wrong person for money 60% more when their peers move from being paid a flat rate \$10/hr to being paid a commission.

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## Figures

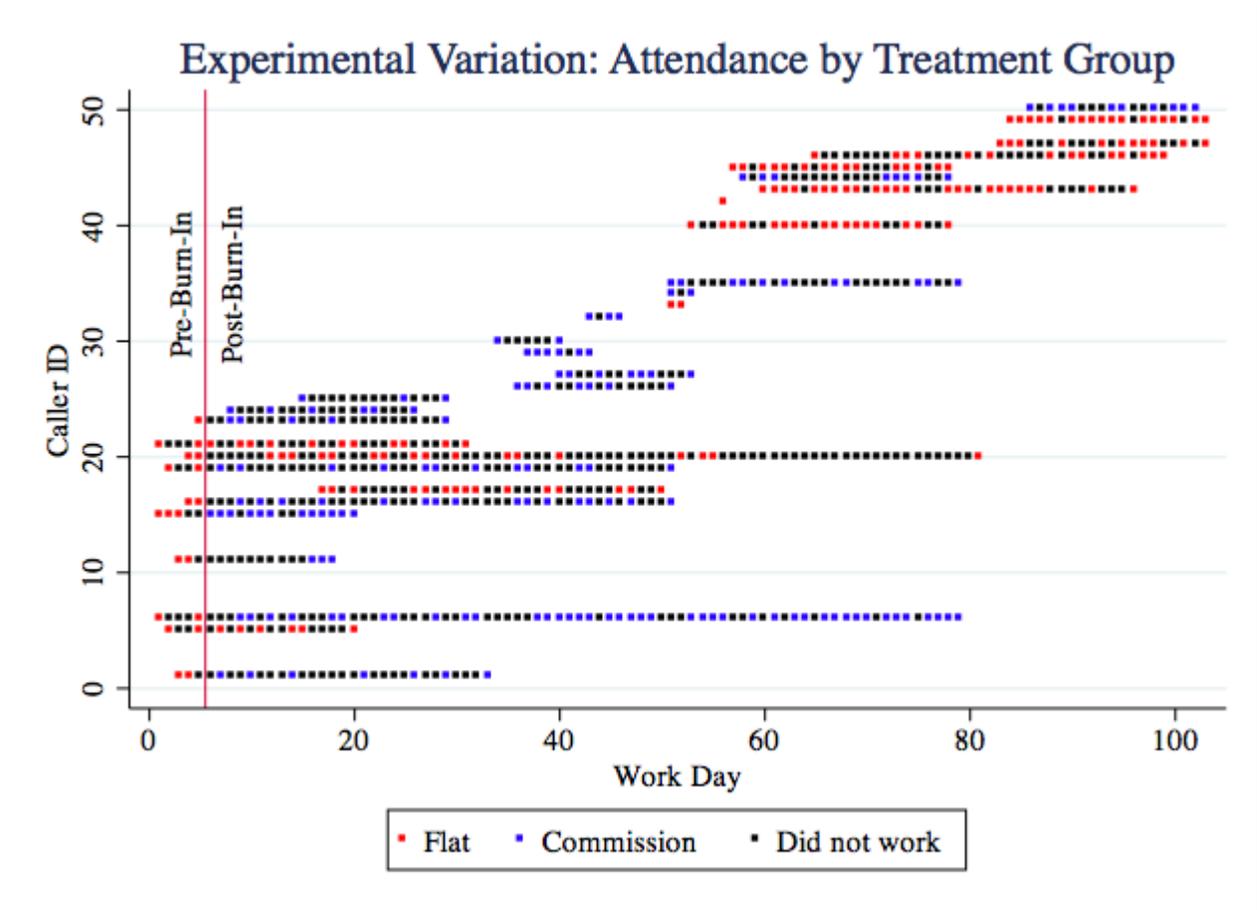


Figure 1: This figure shows treatment assignment, by callerid, over time.

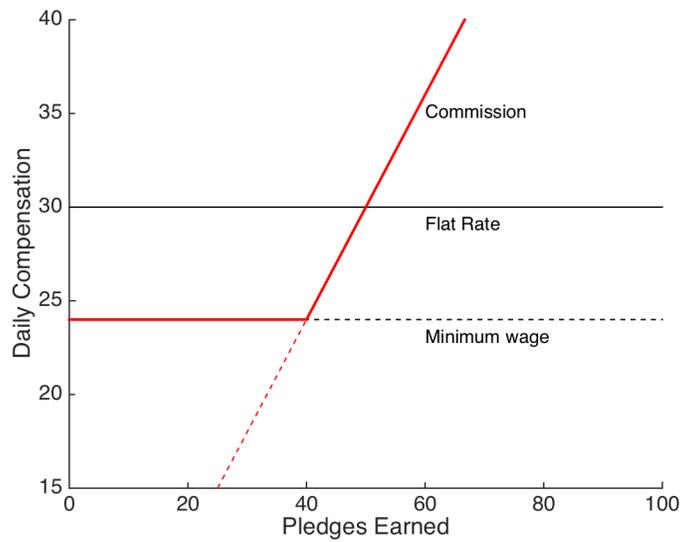


Figure 2: This figure displays the compensation scheme for workers on a 60% of pledges earned commission. If a caller did not make more than the minimum wage *over an entire two-week pay period*, he or she would be paid the minimum wage of \$8 per hour. Flat rate callers were paid \$10 per hour.

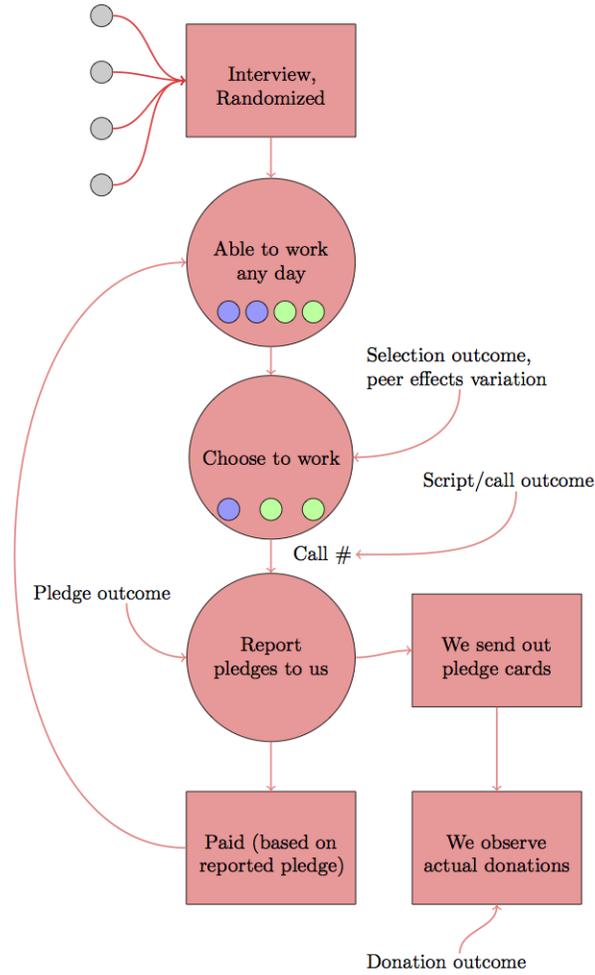


Figure 3: This figure shows the timeline facing callers. Subjects would interview and be randomized into a treatment. Knowing their treatment, they could choose when to work, letting the experimenter know their preferred schedule via email. The callers then made phone calls to donors on their call log and recorded pledges for each call. They are then paid based on the pledges they report over the course of a two-week period (if they are on a commission-based pay scheme). Experimenters, not callers, sent pledge cards to potential donors.

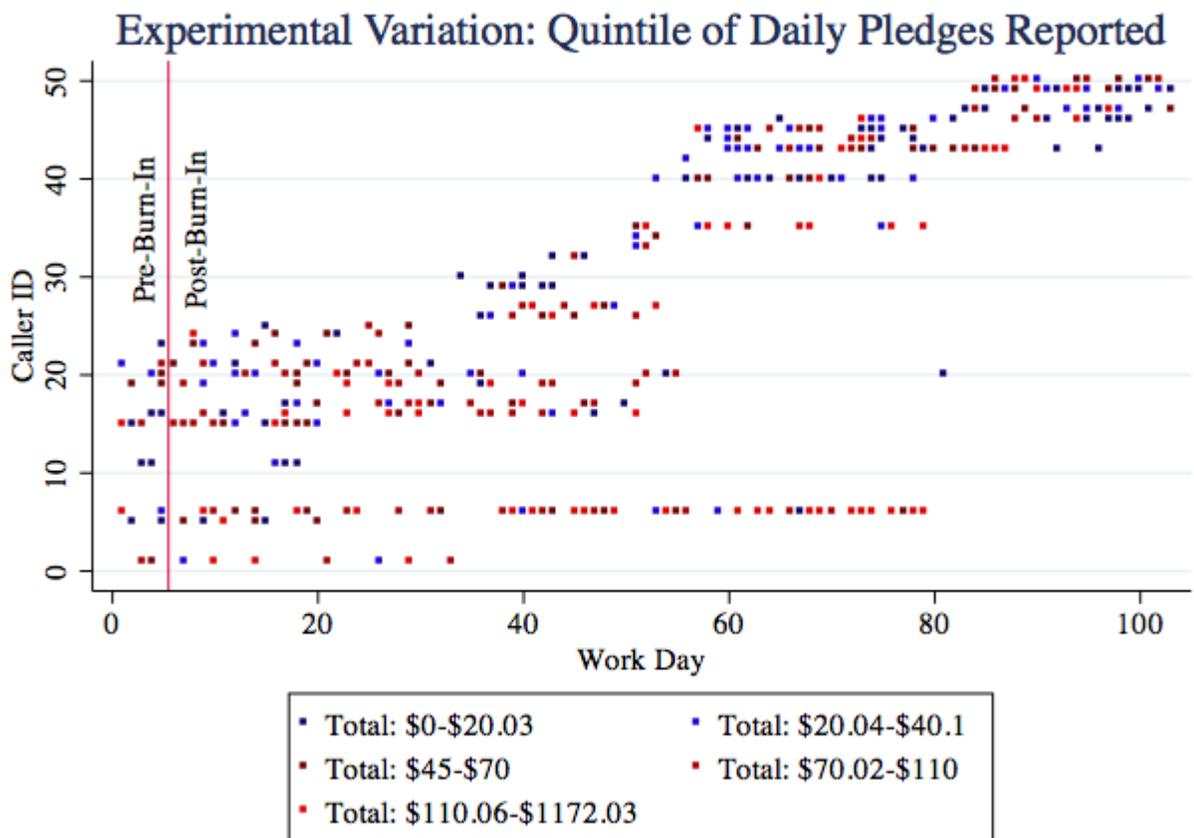


Figure 4: This figure summarizes our outcome variation, by callerid, over time. Callers improving in their ability to generate pledges shows up as a given callerid moving from blue to red.

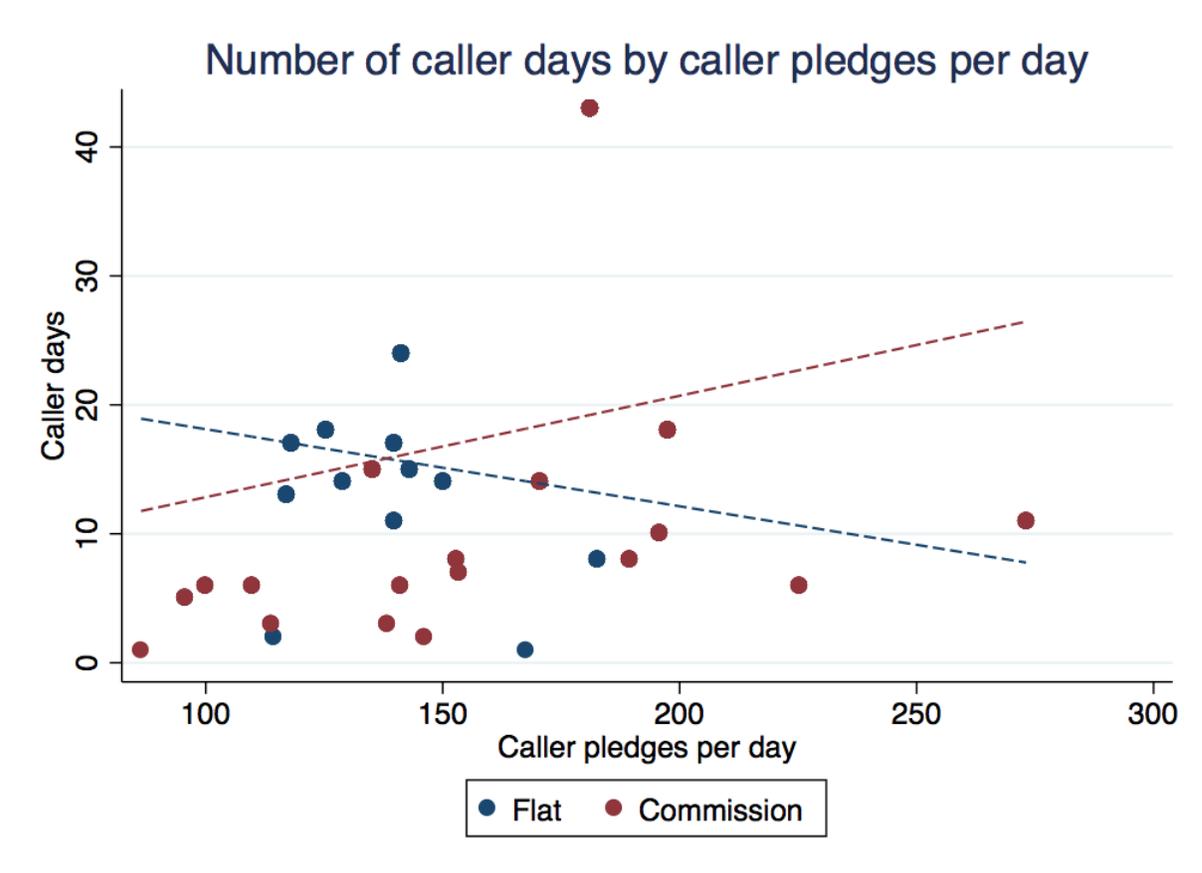
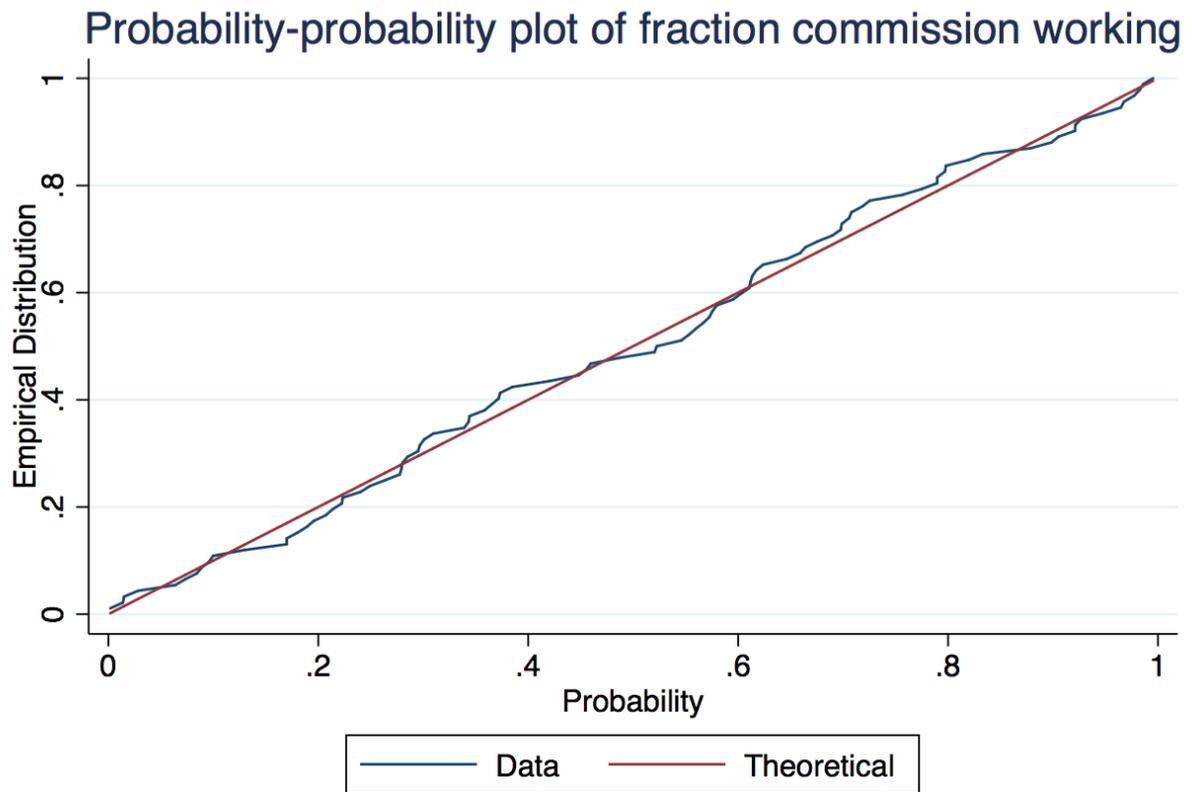


Figure 5: This figure displays the relationship between caller fixed effects from a regression of total pledges per day on a cubic in experience and date fixed effects, which we interpret as caller skill, and a caller's tenure. The dotted lines are from an OLS regression of caller tenure on caller fixed effects from a pledges per day regression. The positive relationship between skill and tenure is robust to excluding the outlier caller who worked for 42 days.



P-value for difference is greater than 0.978 (fail to reject no bunching).

Figure 6: This figure displays the empirical and theoretical distribution of p-values for the proportion of workers being paid a commission. Each date is an observation. We cannot reject that callers are sorting randomly into work-days.

## Tables

Table 1: Call Summary Variables

Variable	Number	Mean	SD	Min	Max
Pledge amt.	52677	.488	7.112	0	1000
Pledge amt.   Pledge > 0	994	25.86	45.012	1	1000
Donation	52677	.034	.181	0	1
Donation   Pledge > 0	994	.423	.494	0	1
Donation amt.	52677	5.419	243.101	0	45000
Donation amt.   Pledge > 0	994	20.798	83.95	0	2000
Donation amt.   Donations > 0	1788	159.641	1310.505	.25	45000
Trimmed Donations amt.	52677	2.583	22.982	0	375
Trimmed Donations amt.   Pledge > 0	994	18.257	46.766	0	375
Trimmed Donations amt.   Donations > 0	1788	76.10	99.849	.25	375
Talked	52677	.229	.42	0	1
Pledge amt.   Talked	12080	2.128	14.733	0	1000
Donation amt.   Talked	12080	4.606	51.706	0	2500
Trimmed Donation amt.   Talked	12080	3.569	24.712	0	375
Push the pledge (recording outcome)	13612	0.0259	0.159	0	1
Ask wrong person (recording outcome)	13612	0.010	0.101	0	1

Table 1: This table displays summary statistics for call-level outcomes. Pledges are what was promised over the phone while donations are what was received. Trimmed donations replace any donation above the 95th percentile was replaced with \$375 (the 95th percentile of donations).

Table 2: Balance t-tests

Treatment	Number	Mean	Std. Err.	Std. Dev.	P-value for t-test
IQ - Average number correct in 3 math questions					
Flat	15	1.13	0.307	1.187	0.5806
Commission	27	1.33	0.207	1.074	
Proportion non-white					
Flat	15	0.60	0.131	0.507	0.8542
Commission	27	0.63	0.095	0.492	
Reported college GPA					
Flat	11	3.36	0.096	0.317	0.4308
Commission	22	3.45	0.061	0.288	
Proportion male					
Flat	15	.27	0.118	0.458	0.6637
Commission	27	.33	0.092	0.480	
Age					
Flat	15	23.07	1.289	4.992	0.1729
Commission	27	21.41	0.537	2.791	
Fair+Trusting+Helpful					
Flat	15	2.40	0.214	0.828	0.4517
Commission	27	2.20	0.152	0.788	

Table 2: This table displays the results of a t-test of race, gender, age, our IQ measure, reported GPA, and a measure of sociability by treatment group. Callers who were interviewed and assigned treatment, but who possibly did not work are included (some callers interviewed early in the experiment who indicated they weren't interested in the job were never actually randomized into treatment due to the 1 week burn-in before treatment assignment). Standard errors are in parentheses. No differences are significant in comparison with the flat-rate group. To see the math questions and the sociability questions (fair, trusting, and helpful), see the last page of the interview questionnaire in the appendix.

Table 3: Balance t-tests

Question	p-value
Question 1	0.454
Question 2	0.854
Question 3	0.937
Question 4	0.101
Question 5	0.546
Question 6	0.125
Question 7	0.495
Question 8	0.694
Question 9	0.655
Question 10	0.101
Question 11	0.674
Question 12	0.068
Question 13	0.238
Question 14	0.222
Question 15	0.581
Question 16	0.713
Question 17	0.325
Question 18	0.478
Question 19	0.813
Question 20	0.590

Table 3: This table reports the p-values of a Wilcoxon signed-rank by assignment to flat rate compared to assignment to commission for each of the questions in the personality test. To see the questions, refer to the second page of the interview questionnaire in the appendix.

Table 4: Call outcomes

$Y_{itc} = \alpha + \beta D_{it}^{\text{Treat}} + \gamma X_{it} + \epsilon$				
Dependent Variable ( $Y_{itc}$ )	(1)	(2)	(3)	(4)
Pledge Amount	0.181** (0.0722)	0.222*** (0.0677)	0.221** (0.0813)	0.222*** (0.0567)
Donation amount	2.982 (1.772)	2.395 (1.966)	1.880 (1.844)	0.205 (1.798)
Donations (trimmed)	0.0481 (0.210)	-0.174 (0.198)	-0.369** (0.177)	-0.459** (0.183)
Pr(Pledge)	0.00584*** (0.00171)	0.00603*** (0.00177)	0.00453*** (0.00127)	0.00388*** (0.00136)
Pr(Donate)	0.00307* (0.00155)	0.00356** (0.00132)	0.00252* (0.00133)	0.00221 (0.00151)
Pr(Donate   Pledge)	-0.0118 (0.0305)	-0.0348 (0.0376)	-0.0350 (0.0385)	-0.0361 (0.0371)
Donation-Pledge	-16.25 (14.38)	-23.77 (16.96)	-28.56 (19.11)	-18.31 (19.24)
$N$	52677	52677	52677	52677
Date FE	.	X	X	X
Experience Controls	.	.	X	X
Demographic controls	.	.	.	X

Table 4: This table displays the results of regressions on call outcome (for covariate  $\beta$  of the regression listed in the table heading) on seven dependent variables with four specifications. Experience controls include a cubic in experience. Demographic controls include interacted race and gender dummies. All regressions are clustered on callerid. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% significance levels, respectively.

Table 5: Caller-Day outcomes

$Y_{it} = \alpha + \beta D_{it}^{\text{Treat}} + \gamma X_{it} + \epsilon$				
Dependent Variable ( $Y_{it}$ )	(1)	(2)	(3)	(4)
Number of calls	28.99** (13.29)	27.29** (9.913)	25.20*** (8.452)	34.08*** (6.175)
Donations	28.99** (13.29)	27.29** (9.913)	25.20*** (8.452)	34.08*** (6.175)
Donations trimmed	82.65 (51.24)	30.81 (60.13)	-6.320 (46.05)	-1.438 (46.90)
Pledges	43.28*** (11.74)	48.67*** (16.55)	47.13** (19.46)	52.92*** (13.88)
Donations-pledges	-18.31 (14.76)	-30.40 (26.94)	-33.74 (29.19)	-26.10 (25.00)
Number of Pledges	1.489*** (0.453)	1.462*** (0.505)	1.203*** (0.340)	1.267*** (0.326)
Number of Donations	1.479** (0.605)	1.473** (0.584)	1.195** (0.467)	1.442*** (0.422)
$N$	333	333	333	333
Date FE	.	X	X	X
Experience Controls	.	.	X	X
Demographic controls	.	.	.	X

Table 5: This table displays the results of regressions on caller-day outcomes (for covariate  $\beta$  of the regression listed in the table heading) on seven dependent variables with four specifications. Experience controls include a cubic in experience. Demographic controls include interacted race and gender dummies. All regressions are clustered on callerid. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% significance levels, respectively.

Table 6: Caller-Day outcomes: transcriptions

$Y_{it} = \alpha + \beta D_{it}^{\text{Treat}} + \gamma X_{it} + \epsilon$				
Dependent Variable ( $Y_{it}$ )	(1)	(2)	(3)	(4)
Ask wrong person	0.0115*** (0.00256)	0.0101*** (0.00288)	0.00944*** (0.00298)	0.00842** (0.00325)
Push the pledge	0.0211** (0.00786)	0.0225*** (0.00794)	0.0238*** (0.00775)	0.0237*** (0.00779)
Date FE	.	X	X	X
Experience Controls	.	.	X	X
Demographic controls	.	.	.	X

Table 6: This table displays the results of regressions (for covariate  $\beta$  of the regression listed in the table heading) on two dependent transcription variables with four specifications. Experience controls include a cubic in experience. Demographic controls include interacted race and gender dummies. All regressions are clustered on callerid. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% significance levels, respectively. Number of observations can be found in Table 1.

Table 7: Peer Effects outcomes

IV Second Stage: $Y_{it} = \alpha + \beta Y_{-i,t} + \gamma X_{it} + \epsilon_{it}$				
IV First Stage: $Y_{-it} = \omega + \xi \text{PropPeersTreated} + \tilde{\gamma} X_{it} + e_{it}$				
Dependent Variable ( $Y_{it}$ )	(1)	(2)	(3)	(4)
Number of calls	0.0110 (0.161)	-0.0255 (0.187)	-0.0416 (0.196)	-0.628*** (0.234)
Donations	0.0829 (0.389)	0.212 (0.496)	0.291 (0.456)	0.133 (0.422)
Pledges	-0.181 (0.411)	-0.189 (0.394)	-0.129 (0.428)	0.354 (0.585)
Number of pledges	0.0688 (0.229)	0.0904 (0.225)	0.200 (0.232)	0.411** (0.197)
Number of donations	0.137 (0.402)	-0.00970 (0.370)	0.118 (0.388)	-0.353 (1.037)
$N$	326	326	326	326
Ask wrong person	0.579*** (0.212)	0.676*** (0.261)	0.715** (0.331)	0.671* (0.391)
Push the pledge	0.133 (0.351)	-0.0782 (0.388)	0.0841 (0.343)	0.345 (0.324)
$N$	281	281	281	281
Demographic controls	.	X	X	X
Experience controls	.	.	X	X
Week & Day of Week FE	.	.	.	X

Table 7: This table displays IV results of regressions (for covariate  $\beta$  of the regression listed in the table heading) on seven dependent variables with four specifications. All regressions include treatment dummies. Experience controls include a cubic in experience. Demographic controls include interacted race and gender dummies. All regressions are clustered on callerid. \*, \*\*, and \*\*\* denote significance at the 10%, 5%, and 1% significance levels, respectively.

Table 8: Balance t-tests, conditional on working

Treatment	Number	Mean	Std. Err.	Std. Dev.	P-value for t-test
IQ - Average number correct in 3 math questions					
Flat	12	1.08	0.358	1.240	0.7515
Commission	18	1.22	0.263	1.114	
Proportion non-white					
Flat	12	0.417	0.148	0.515	0.8842
Commission	18	0.389	0.118	0.502	
Reported college GPA					
Flat	8	3.38	0.078	0.301	0.750
Commission	15	3.34	0.129	0.366	
Proportion male					
Flat	12	0.16	0.112	0.389	0.1218
Commission	18	0.44	0.121	0.511	
Age					
Flat	12	23.33	1.611	5.581	0.3642
Commission	18	21.83	0.789	3.348	
Fair+Trusting+Helpful					
Flat	12	2.50	0.261	0.905	0.5269
Commission	18	2.34	0.177	0.750	

Table 8: This table displays the results of a t-test of race, gender, age, our IQ measure, reported GPA, and a measure of sociability by treatment group. Only callers who worked at least one full day are included. Standard errors are in parentheses. To see the math questions and the sociability questions (fair, trusting, and helpful), see the last page of the interview questionnaire in the appendix.

## Appendix A: Recruitment and interview materials

### Recruitment ad:

Professor John List of the Economics Department at the University of Chicago seeks student help on a telephone fundraising project. Callers would work 3-6 hours per week for approximately 8 weeks. If you would like to know more about this job opportunity, or to schedule an interview, please email [recruiter] at [recruiter email]

### Reply to interested in recruitment ad:

Dear [caller],

Thank you for your interest in the telephone fundraising position! We would love to schedule an interview with you.

Please reply to this email with a time that you would prefer to come in, and I will try to schedule you as close to this time as possible.

The job will require you to make phone calls for three hours every night that you work soliciting past donors to [charity] for donations. We will provide a script and you will be required to attend a training session. No experience in soliciting is necessary, though it is certainly helpful. The hours are 5:30-8:30PM at least two days a week. We would discuss pay during the interview.

Interviews will be held in room 370 of the Booth School of Business. I look forward to hearing from you.

All the best,

[recruiter]

The employment application consisted of gathering demographic and personality data, as well as obtaining recording permissions. These are provided below.

**FUNDRAISING CAMPAIGN**  
Employment Application



THE UNIVERSITY OF  
**CHICAGO**

APPLICANT INFORMATION			
Last Name		First	M.I.
Street Address			Apartment/Unit #
City		State	ZIP
Phone		E-mail Address	
Date of Birth (M/D/Y)	Social Security No.		Gender <input type="checkbox"/> M <input type="checkbox"/> F
Are you a citizen of the United States?	YES <input type="checkbox"/> NO <input type="checkbox"/>	If no, are you authorized to work for the university?	YES <input type="checkbox"/> NO <input type="checkbox"/>
Check here if you are a permanent resident <input type="checkbox"/>			

EDUCATION	
Education: <input type="checkbox"/> Some High School <input type="checkbox"/> High School Diploma <input type="checkbox"/> Some College <input type="checkbox"/> College Diploma <input type="checkbox"/> Graduate School	
Primary Major:	Secondary Major (if applicable):
GPA	

RELATED WORK EXPERIENCE	
Company / Organization	
Job Title	
Responsibilities	
Starting Date	End Date

DISCLAIMER, CONSENT FORM, AND SIGNATURE	
I certify that my answers are true and complete to the best of my knowledge.	
If this application leads to employment, I understand that false or misleading information in my application or interview may result in my release.	
I understand that my voice will be evaluated by a computer program, and that this information may be used in analyzing the outcomes of the fundraising. I further understand that my personal information will not be released to third parties, and that my vocal characteristics and other information will never be linked with my name in any publication of the findings of the fundraiser.	
I consent to the use of my personal information for execution of a criminal history check and credit check to be conducted by First Staffing. I understand that my employment in this campaign is contingent upon the receipt and evaluation of my credit and/or background check.	
Signature	Date

**FUNDRAISING CAMPAIGN**  
Questionnaire



**NAME:** \_\_\_\_\_

**PART 1.** For the following twenty statements, please indicate how much you agree or disagree with the statement on a scale of (1) – (5), where (1) means "strongly disagree" and (5) means "strongly agree."

	Strongly disagree					Strongly agree
	↓					↓
I take control of things.	<input type="checkbox"/>					
	1	2	3	4	5	
I express myself easily.	<input type="checkbox"/>					
	1	2	3	4	5	
I am not highly motivated to succeed.	<input type="checkbox"/>					
	1	2	3	4	5	
I cannot come up with new ideas.	<input type="checkbox"/>					
	1	2	3	4	5	
I talk to a lot of different people at parties.	<input type="checkbox"/>					
	1	2	3	4	5	
I am skilled in handling social situations.	<input type="checkbox"/>					
	1	2	3	4	5	
I have difficulty expressing my feelings.	<input type="checkbox"/>					
	1	2	3	4	5	
I often feel uncomfortable around other people.	<input type="checkbox"/>					
	1	2	3	4	5	
I formulate ideas clearly.	<input type="checkbox"/>					
	1	2	3	4	5	
I am able to think quickly.	<input type="checkbox"/>					
	1	2	3	4	5	
I undertake few things on my own.	<input type="checkbox"/>					
	1	2	3	4	5	
I never challenge things.	<input type="checkbox"/>					
	1	2	3	4	5	
I set high standards for myself and others.	<input type="checkbox"/>					
	1	2	3	4	5	
I do more than what is expected of me.	<input type="checkbox"/>					
	1	2	3	4	5	
I do just enough work to get by.	<input type="checkbox"/>					
	1	2	3	4	5	
I think that in some situations it is important that I not succeed.	<input type="checkbox"/>					
	1	2	3	4	5	
I just know that I will be a success.	<input type="checkbox"/>					
	1	2	3	4	5	
I have a lot of personal ability.	<input type="checkbox"/>					
	1	2	3	4	5	
I often think that there is nothing I can do well.	<input type="checkbox"/>					
	1	2	3	4	5	
I question my ability to do my work properly.	<input type="checkbox"/>					
	1	2	3	4	5	

**PART 2.** Please agree (yes)/disagree (no) to the following statements.

2.1	Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?	<input type="checkbox"/> Yes (Agree with the first half of the statement) <input type="checkbox"/> No (Agree with the second half of the statement)
2.2	Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair?	<input type="checkbox"/> Yes (Agree with the first half of the statement) <input type="checkbox"/> No (Agree with the second half of the statement)
2.3	Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves?	<input type="checkbox"/> Yes (Agree with the first half of the statement) <input type="checkbox"/> No (Agree with the second half of the statement)

**PART 3.**

3.1	A bat and a ball cost \$1.10. The bat costs \$1 more than the ball. How much does the ball cost?	Cents
3.2	If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets?	Minutes
3.3	In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake?	Days

## Appendix B: Scripts and caller instructions

Caller script:

Hello, may I please speak to [donor name]? Hello, this is [name] calling for [charity name], where our mission is to [charity mission]!

[Charity slogan], and to help us reach our goal, were asking for a donation [from you today]  
OR [of year of last gift in dollars and cents from you today, since you last gave to [charity] in year of last gift] OR [of \$20.00 from you today].

Would you be interested in making a donation today?

IF YES:

[Confirm amount and where it will go] [If they want to pay by credit card]: That's not a problem, but in order to ensure a secure transaction, we would like to direct you to the [charity] website: [link here], where you can follow the donate link to make a donation online. We will send you a pledge card with all of this information, and you can send that back at your convenience.

Can I just confirm your address?

Thank you so much, we are extremely grateful for your support. Have a good evening!

IF NO:

We understand, Before you go, would you mind if I confirmed your address for our records? Please have a good evening.

Text

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Thank you for your pledge of \_\_\_ dollars to [charity]! If you wish to fulfill your pledge by personal check or cash, please place these items in the return envelope, seal it, and place the envelope in a United States Postal Service mailbox for return. If you wish to fulfill your pledge by credit card payment, please complete the form below and place it in the return envelope, seal it, and place the envelope in a United States Postal Service mailbox for return.

If paying by credit card:

Name (as it appears on the card): \_\_\_\_\_

Card Type:  Visa     Discover Card     Master Card

Card Number: \_\_\_\_\_

Expiration Date: \_\_\_\_\_

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