



# **The Dirty Laundry of Employee Award Programs: Evidence from the Field**

**Timothy Gubler  
Ian Larkin  
Lamar Pierce**

**Working Paper**

**13-069**

**February 11, 2013**

Copyright © 2013 by Timothy Gubler, Ian Larkin, and Lamar Pierce

Working papers are in draft form. This working paper is distributed for purposes of comment and discussion only. It may not be reproduced without permission of the copyright holder. Copies of working papers are available from the author.

# The Dirty Laundry of Employee Award Programs: Evidence from the Field

Timothy Gubler

Olin School of Business, Washington University in St. Louis  
tgubler@wustl.edu

Ian Larkin

Harvard Business School  
ilarkin@hbs.edu

Lamar Pierce

Olin School of Business, Washington University in St. Louis  
pierce@wustl.edu

*February 11, 2013*

Many scholars and practitioners have recently argued that corporate awards are a “free” way to motivate employees. We use field data from an attendance award program implemented at one of five industrial laundry plants to show that awards can carry significant spillover costs and may be less effective at motivating employees than the literature suggests. Our quasi-experimental setting shows that two types of unintended consequences limit gains from the reward program. First, employees strategically game the program, improving timeliness only when eligible for the award, and call in sick to retain eligibility. Second, employees with perfect pre-program attendance or high productivity suffered a 6-8% productivity decrease after program introduction, suggesting they were demotivated by awards for good behavior they already exhibited. Overall, our results suggest the award program *decreased* plant productivity by 1.4%, and that positive effects from awards are accompanied by more complex employee responses that limit program effectiveness.

## 1. Introduction

The use of employee award programs is widespread. Over 80 percent of companies use programs that provide awards such as “employee of the month”, “top salesperson”, or “best innovation” (Garr 2012). A small but growing literature in management and economics argues that employee award programs are so common because they positively influence the behavior and performance of individual workers, and thereby improve firm performance. This work suggests that even awards with minimal or no financial benefits can spur positive motivational effects through several psychological mechanisms. First, awards may increase self-esteem (Kuhnen and Tymula 2012) or identity with the employer (Akerlof and Kranton 2005; Neckermann et al. 2012), which can improve employee performance after winning an award. Second, competition driven by the desire for positive social comparisons with peers (Festinger 1954) may motivate effort even before an award is granted (Markham et al. 2002; Moldovanu et al 2007; Kosfeld and Neckermann 2011).<sup>1</sup> These ex-post and ex-ante performance gains have led several researchers to suggest that awards programs with low marginal costs are a “free” method by which firms can boost employee productivity (Besley and Ghatak 2008; Blanes i Vidal and Nossol 2011).<sup>2</sup>

Despite this emerging evidence of award program benefits, there are reasons to believe such programs might carry unexpected costs. Larkin (2010) finds that award programs increase strategic “gaming” by salespeople that may not benefit the firm. Malmendier and Tate (2009) find that CEOs suffer decreased performance after receiving an award from major financial publications, likely due to distraction and overly risky decisions. Other research suggests that simply providing feedback on relative standing, which is a frequent component of a corporate award system, can dramatically reduce performance (Barankay 2012). This lower performance could occur due to reduced motivation born of employee envy or perceptions of unfairness, particularly if an award program is viewed as a type of compensation that is not clearly linked to employee contributions (De Boer et al. 2002; Nickerson and Zenger 2008; Fehr et al. 2009; Larkin et al. 2012). Furthermore, an award program designed to reward prosocial behavior might “crowd out” the intrinsic motivation of employees (Frey and Meier, 2004), or it may reduce the signaling value of prosocial behavior might demotivate employees seeking to project a prosocial image (Ariely et al. 2009). If awards indeed lead to significant gaming, reduced motivation, or crowding out from perceptions of unfairness or decreased signaling value, are these “spillover costs” large enough to undermine potential program benefits to the firm?

In this paper, we simultaneously examine the costs and benefits of an attendance award program that

---

<sup>1</sup> Similarly, awards may improve employee career prospects, motivating workers through the potential to win extrinsic benefits

<sup>2</sup> An extensive practitioner literature also claims extensive benefits to the firm from awards programs (e.g. Davidson 1999; Boyle 1996).

was implemented in an industrial laundry plant. The award was independently implemented in only one of the firm's five plants by the plant manager, allowing us to investigate the effect of the award in a quasi-experimental way, using employees at the four plants that did not implement the award as a quasi-control group. We first examine the award program's intended impact on the attendance behavior of all relevant employees. Specifically, we investigate the heterogeneous responses of employees with poor vs. outstanding historical attendance records, as well as those of highly productive employees vs. those who are less productive. Second, our data allow us to examine the effect of the award not only on attendance, which was the rationale for the award, but also on individual worker productivity, which the manager instituting the award did not expect to be affected by the program.

Our setting is a privately-owned industrial laundry company with five plants distributed across three states in the Midwestern United States. In March 2011, the managers at one of the plants independently implemented an attendance award program, which continued for nine months until corporate headquarters intervened and discontinued it.<sup>3</sup> Each employee with perfect attendance for a given month, defined as not having any unexcused absences or tardy shift arrivals, was entered in a drawing for a \$75 gift card to a local restaurant or store. On the first Tuesday of the following month, the plant manager would hold a meeting attended by the entire plant where all award-eligible employees would put their name in a hat and the previous month's winner would then draw out one new winner. The plant manager then reminded the employees that they were all eligible for the next month's award, until they had an unexcused absence or tardy. At the end of the sixth award month, the plant manager held an additional drawing for a \$100 gift card for all employees with perfect records over the entire six-month period. The expected monetary value of each monthly award was less than \$5, but the award brought significant recognition from management and peers.

We use a 21-month panel of daily worker-level data at all five plants that spans a year before and nine months after the introduction of the award policy. The data, which measure attendance, tardiness, and productivity, cover all workers at both the award plant and the four plants that did not introduce the award. These daily data, combined with award eligibility lists from the plant manager, allow us to compare the program's impact on worker behavior in the "treated" plant with behavior of the baseline group in four "untreated" plants. We use a difference-in-differences model with both worker and time fixed effects to examine the causal effect of the award on tardiness, attendance, and productivity. We find that the award on average reduced the likelihood and average level of tardiness (defined as being more than five minutes late for a shift) and increased punctuality (defined as "punching in" on time) among workers that were participating in the program.

However, we find that the award did not appear to lead to a habituation of punctuality or other good

---

<sup>3</sup> The decision to shut down the program was primarily based on the owners' judgment that workers should not be given an award for behavior that was expected of all employees. The firm had not examined the effect of the award before sharing the data with us.

attendance behavior, but instead led to strategic gaming that varied by employee type. Employees who were often tardy before the award introduction did just enough to avoid being disqualified by being five minutes late, but still tended to arrive late for their shifts. Employees with few tardies in the pre-treatment period arrived somewhat earlier for their shifts, but this effect appears to be completely due to a desire to avoid disqualification (most likely from risk aversion). Once an employee became disqualified from the award, she immediately reverted to her previous behavior. Most interestingly, employees were 50% more likely to have an unplanned “single absence” after the award was implemented, suggesting that employees who would otherwise have arrived to work tardy on a certain day might instead either call in sick to avoid disqualification or else simply stay home because they would be disqualified from the award regardless.

Most seriously, we find that two highly valued employee groups – the most productive workers and the most consistently punctual workers – suffered a 6-8% decrease in productivity after the award was instituted. Employee pay was on average only weakly tied to individual productivity, but was unrelated to marginal productivity for the most productive employees, who suffered no decrease in pay from the 6-8% productivity loss.<sup>4</sup> In our setting, however, the extrinsic rewards (for attendance) reduced the intrinsic motivation in a completely separate domain: (uncompensated) productivity.

Although there are several explanations for why the award caused this productivity decrease, it is consistent with a “crowding out” effect (Deci and Ryan 1985), where the introduction of extrinsic rewards reduced intrinsic motivation, defined as motivation that does not stem from extrinsic benefits.<sup>5</sup> One likely mechanism for this “crowding out” is demotivation due to perceptions of unfairness (Akerlof and Yellen 1988; 1990; Cooper, Dyck, and Frohlich 1992; Larkin et al. 2012), since employees who “did the right thing” may have questioned why an award was necessary to promote this behavior, or felt it was unfair that those with poor pre-program attendance could win the award.<sup>6</sup> Another possible mechanism is that the introduction of the award for attendance decreased the image value of high productivity (e.g. Ariely et al. 2009). There were no other productivity effects from the award.

The plant manager anticipated that the award’s direct cost of \$75 per month would easily “pay for itself” through improved attendance, which would directly increase productivity. Better attendance should directly increase productivity, but more importantly should increase the productivity of peers, due to the assembly-line nature of the work and the potential for bottlenecks and idling time if an upstream worker were late or absent. At worst, the manager felt he would only be required to spend \$75 on the gift card every month. However, our basic regression results estimate that the award led to an overall *decrease* in plant

---

<sup>4</sup> We will explain the compensation system in Section III.

<sup>5</sup> However, as discussed in the final section of the paper, we cannot show that intrinsic motivation leads some employees to continue to be productive beyond the point of increased compensation. A host of extrinsic factors, such as peer pressure or cultural norms, might also explain why this group of employees continued to work even when not paid for it.

<sup>6</sup> In fact, throughout the course of the award program only one individual from the “consistently punctual” or “notardy” group won the award, and it was not until the September drawing—7 months after the program began.

productivity of 1.4%, and thereby increased costs of roughly \$1,500 per month. Overall, these results suggests awards do not always represent “free” motivation, and that even a very simple award program can have unplanned, negative outcomes that are significant to the overall performance of the firm.

This paper contributes to two important and growing literatures in economics and management. First, our paper is the first to demonstrate in the field the complex positive and negative impacts of employee awards programs. While we find similar positive effects to prior studies (e.g. Markham et al. 2002; Neckerman et al. 2012), we also demonstrate that negative effects such as employee gaming and crowding out may overwhelm these benefits and render the program as value destroying. We also contribute to the literature on the complex interaction between extrinsic and intrinsic motivation (e.g. Deci and Ryan 1985; Frey and Oberholzer-Gee 1997).<sup>7</sup> Our study shows that the introduction of rewards for one activity may crowd out intrinsic motivation in another, the first evidence of such spillovers of which we are aware.

The paper proceeds as follows. Section II discusses the literature and theoretical background behind award programs. Section III describes our empirical setting and data. Section IV presents our empirical approach and results, while Section V concludes.

## **2. Literature and Theoretical Background**

### **2.1 Benefits of Award Programs**

Why might awards with little monetary or career value improve positive behavior such as productivity and reduce negative outcomes such as absenteeism and tardiness? A small body of research on awards almost exclusively finds positive effects from awards. Meta-analysis by Stajkovic and Luthans (1997) found that the use of monetary performance feedback and social recognition increased productivity at manufacturing organizations by 33% and at service organizations by 13%. Kosfeld and Neckermann (2011) used experiments with students to demonstrate a short-term motivational effect from awards. In a field setting, Neckermann, Cueni, and Frey (2012) found that the introduction of several unannounced symbolic awards in a call center increased performance among both award recipients and non-recipients on tasks completely different than those for which the awards were given. While their study lacked a control group, it nevertheless suggests strong short-term motivation from awards.

A large body of research in sociology, economics and psychology suggests that much of this motivation may be based in employees caring about status and social recognition by peers and employers (Greenberg 1988). Economic theorists have argued that individuals are motivated to seek social approval (Akerlof 1980; Benabou and Tirole 2006; Andreoni and Bernheim 2007; Ellingsen and Johannesson 2008; Moldovanu et al. 2007), which explains widespread evidence of increased prosocial behavior in public settings (Andreoni and Petrie 2004; Dana et al. 2006; O’Reilly and Chatman 1986).

---

<sup>7</sup> See Gneezy et al. (2011) for more extensive discussion of this literature.

Empirical evidence on peer effects in prosocial and antisocial behavior also supports the motivational power of peer recognition. Ichino and Maggi (2000), for example, showed that peer culture negatively impacted the absenteeism of coworkers at an Italian bank. Mas and Moretti (2009) found that supermarket workers increased effort when high-skilled peers were watching them. Similarly, Hars and Ou (2001) showed that volunteer time and effort increased when recognized by important peers. Employees may even be willing to pay for peer recognition, based on evidence from a small number of laboratory (e.g., Huberman et al. 2004) and field (e.g., Larkin 2010) studies.

## **2.2 Potential Costs of Award Programs**

There are several reasons, however, to believe that award programs might not provide universally positive outcomes. Awards programs might carry costs simply because employees learn to “game” the system, much as they do with monetary rewards (Kerr 1975; Oyer 1998; Larkin 2013). “Gaming” is traditionally defined as taking unintended actions in response to an incentive system that increase the employee’s objective function, at the expense of the employer’s objectives. Employees motivated by the award program might either manipulate perceptions of their behavior or simply strategically engage in only those activities that improve their award eligibility, when those activities are relevant for the award. Similar to the calendar-based gaming behavior in Oyer (1998) and Larkin (2013), monthly or annual awards might only motivate desired behavior toward the end of the eligibility period among those employees who still perceive some likelihood of winning. Similar results have been theorized and observed among low-performers in tournament-based compensation systems (Che and Gayle 2003; Fullerton and McAfee 1999; Boudreau et al. 2011; Minor 2012).

Other potential costs may come from the same peer comparisons that generate some benefits. Although peer comparisons can have many positive effects (as detailed above), they have also been shown to have negative consequences. For example, a series of laboratory experiments suggest that employees make value-destroying decisions when these decisions would also lead to a salient reduction in peer recognition (e.g., Garcia et al. 2006). Recent empirical work shows that peer recognition can motivate cheating and dishonesty (e.g., Edelman and Larkin 2013) as well as decreased job satisfaction and intention to quit (Card et al. 2012). These problems may be exacerbated by awards based on subjective or unclear metrics, which might induce feelings of envy (Nickerson and Zenger 2008) or inequity (Adams 1963). This may explain recent contradictory field evidence on how peer comparisons impact worker performance. Barankay (2012), for example, shows that the introduction of a tool that reveals to all furniture salespeople their relative sales productivity led to large productivity decreases across the performance distribution. In contrast, Blanes i Vidal and Nossol (2011) find that a very similar ranking system used for warehouse employees led to large *increases* in productivity, again with no differences across employee types.

In addition, the introduction of an award for prosocial behavior could have negative effects on

motivation and productivity because it “crowds out” some employees’ intrinsic motivation. One mechanism through which intrinsic motivation might be crowded out is fairness. Perceptions of unfairness are known to motivate decreased effort and productivity (Akerlof and Yellen 1988; 1990; Cooper, Dyck, and Frohlich 1993) while increasing other counterproductive behaviors (Skarlicki and Folger 1997; Ambrose et al. 2002; Gino and Pierce 2009). Employees who were already performing at the level required to win an award may perceive it to be unfair that some employees receive awards despite only engaging in this behavior after the program was implemented. Recent work suggests another mechanism for crowding out based on workers being motivated to engage in prosocial behavior by the positive image that such acts project to others (Benabou and Tirole 2006; Lacetera and Macis 2010). The introduction of extrinsic rewards might be demotivating by reducing the signaling value of such behavior, thereby “crowding out” image motivation (Gneezy and Rustichini 2000a; Ariely et al. 2009). Additionally, workers may be demotivated if they perceive the award program as biased, similar to recent experimental work on performance feedback (Ederer and Fehr 2009).

Since employee awards are usefully thought of as an additional way to incentivize employees, it would be surprising if employees did not respond to these incentives in dysfunctional ways. However, this possibility has been largely ignored by the research on awards. In sum, there have been very few studies that attempt to evaluate a more comprehensive set of costs and benefits of awards, despite the clear predictions from the social comparisons and incentive systems gaming literatures that employee awards systems are likely to result in detrimental behavior in some circumstances.

### **2.3 Award Programs for Attendance**

Like other prosocial behaviors, firms commonly seek to motivate punctuality and attendance through award programs. Absenteeism, defined as unexcused job absence or significant unexcused late arrival to work, is a large cost for many businesses. Navarro and Bass (2006) estimate that absenteeism costs employers as much as 15% of payroll, while Delonzor (2005) puts the aggregate cost to US organizations at \$3 billion a year. Absenteeism is particularly costly in workplaces with traditional production lines, where jobs are highly interrelated, workers tend to specialize in only a few tasks, and production time is important. An unplanned absence or significant lateness can greatly affect productivity of downstream workers, meaning that the cost of absenteeism is not limited to the missing employee. Because of the significant costs of absenteeism, many companies have established formal programs to try to reduce it (Pauly et al. 2002).

The existing evidence on the efficacy of these programs is mixed, despite the historical perception among managers that such programs are effective (Scott and Markham 1982). Research in the management literature (Knight et al. 2001) asserts that awards tied to specific attendance goals should lead to employees reducing absenteeism. Camden, Price, and Ludwig (2011) find empirical evidence that a public feedback and credit reward system decreased absenteeism among grocery store workers, but their experiment lacked a



control group to account for the counterfactual. In a classic study, Pedalino and Gamboa (1974) find that the introduction of a lottery award system reduced absenteeism among hourly workers at a manufacturing plant. While this study did feature a control group, it did not statistically account for pre-treatment differences between the groups. In a quasi-experiment, Markham, Scott, and McKee (2002) find that public recognition decreases absenteeism at manufacturing plants. However, their attendance metrics were at the plant-level rather than the employee-level, so they could not explore the differential effect of awards on different worker groups nor pinpoint potential strategic gaming behaviors.

### 3. Empirical Setting

#### 3.1 Industrial Laundry Production

The setting for this study is a private commercial laundry services company in the Midwestern United States, which we will refer to as *LaundryCo*.<sup>8</sup> *LaundryCo* is a leading regional provider of work uniforms, professional work apparel, and commercial laundry cleaning services. Their services primarily include the selling, cleaning, and repairing of work uniforms and small workplace items such as mats, towels, and mop heads. Nearly all of the cleaning services occur at five plants, which are similar in layout, machinery, staffing positions, and products.<sup>9</sup> Figure 1 presents a photograph of the interior of a typical plant.

The basic production flow of the plants is detailed in Figure 2. Each day route-service representatives drop off soiled uniforms, and the dirty uniforms are taken through a complex, interrelated process so that the cleaned garments can be returned to customers. This involves sorting the uniforms by garment type and the amount of soil; washing the garment with the appropriate water temperature and chemical mix; drying the garment; pressing it; doing quality inspection so that garments with missing buttons or other problems are fixed; and ensuring the garment ends up on the correct rack, so that the RSR can quickly and easily load the clean garments to return to customers. Non-uniform items undergo a similar process, but extra steps are often involved, such as rolling laundered floor mats. Workers are cross-trained on many different tasks within the plant, but tend to specialize in a relatively small number of tasks. As with most labor-intensive service operations, *LaundryCo* is highly dependent on the efficiency of its workers. Inefficiency occurs both when there are mistakes, such as garments being sorted incorrectly, and when the productivity of upstream workers lags, leaving downstream workers idle as they wait for garments. For example, if the worker operating the dryer gets behind, then the worker operating the pressing machine will stand idle until she has uniforms to press.

*LaundryCo* tracks productivity for each worker, measuring how many garments a worker processes

---

<sup>8</sup> The largest public companies similar to *LaundryCo* are Cintas and Aramark.

<sup>9</sup> A very small volume of medical facility linens and uniforms are cleaned in a separate facility, since they require different standards.

for each task each day. The company then uses the task's time-studied rate,<sup>10</sup> determined by corporate headquarters, to calculate the employee's productivity on that task given the amount of time spent on it. The scores are normalized such that 100 reflects performance that meets expectations. For example, the time-studied rate for "pressing dress shirts" is 50.4 seconds, meaning an employee would have to press over 71 shirts in an hour to earn a score of 100. For each worker, the system computes an overall daily productivity rate, equal to the weighted average (by time spent) of the worker's productivity scores on each task that day. For example, a worker who spent two hours pressing dress shirts with an efficiency score of 80, two hours rolling mats with a score of 140, and four hours sorting cleaned clothes with a score of 160 would have a final daily productivity of 135.

Workers at *LaundryCo* are compensated partially based on daily productivity. For an efficiency rating above 100, indicating that a worker is beyond the time-studied rate, an employee's hourly wage for that day is increased by \$1. The hourly wage is increased by another \$1 if a worker's efficiency exceeds 120, and by a final \$1 for productivity of 140 or greater. Figure 3 plots the histogram of daily efficiency ratings for all workers in the study, and there is no discernible discontinuity at the three hurdle rates of 100, 120 and 140. This is not surprising because employees usually process several hundred items a day, each with different time-studied rates, and cannot accurately track their exact efficiency. Workers are able to use computers in a break room to check their historical productivity, but not their productivity on the day in question. Anecdotally, most workers only receive productivity information every two weeks when they receive their paychecks.

Given the externalities generated by the upstream-downstream nature of the plant, arriving consistently and on time is essential to operational efficiency. Unexpected worker absences and worker tardiness both contribute to inefficiency, since they mean that one section of the plant may be understaffed. Even if the plant manager finds a substitute for that section, that substitute must abandon another task, and will likely not be as efficient in the job as the missing worker, which can lead to downstream idling time.

*LaundryCo* therefore has relatively detailed policies around both tardiness, which the company defines as being more than five minutes late for a shift, and attendance. Repeated tardiness first precipitates a written warning from the plant manager. The third warning results in suspension from work, and the fourth results in termination. Each write-up expires a year after the date it was given. However, *LaundryCo* leaves the administration of this policy to individual plant managers, who are usually lenient towards occasional tardiness, particularly if an employee is only a few minutes late on rare occasions. The difficulty and cost of finding and training replacement workers discourages managers from terminating occasionally tardy or absent employees.

Employees also have a small financial incentive to avoid being overly tardy. If employees clock in for

---

<sup>10</sup> The item-rate is constant across all plants and all years within this time window.

their shift more than 7 minutes late, the clock-in time rounds up to the next 15 minute interval, resulting in the loss of 15 minutes of wages for the day. If they clock in less than 7 minutes late, their clock-in time is rounded down to the 15-minute interval below. It is important to note that these 15-minute thresholds only affect the number of minutes for which an employee was paid, and not the efficiency calculation, which uses an employee's actual clock-in and clock-out times. Also, employees often clock in a few minutes early, resulting in them "working for free" for the first few minutes of their shift.

Employees could be absent for excused reasons, such as vacation days. Sickness is considered an excused absence if the employee notified the manager of their illness even a few minutes before the start of their shift. There are no specific absence policies in place to avoid unexcused absences. If an employee does not show up and was not excused they simply lose one of their paid-time off (PTO) or vacation days. If unexcused absences become a problem, however, the worker is warned by the plant manager and potentially terminated.

Each of the plants has around 35 employees, many of whom have worked for *LaundryCo* for many years. As a family-owned company with a long history in the local area, *LaundryCo* puts significant effort into building a "friendly" workplace through programs such as birthday recognition and company events. Since employees switch between tasks on the production line, they interact with their colleagues frequently over the course of a day. Each plant utilizes a general manager and a plant manager. The plant manager focuses on plant efficiency and worker productivity. These managers enjoy substantial autonomy in the policies they pursue to increase plant efficiency.

### **3.2 Attendance Award Program**

In an effort to encourage higher employee attendance and decrease employee tardiness, the general and plant manager at one *LaundryCo* plant (hereafter referred to as "Plant 1") independently decided to implement an attendance awards program. The implementation of the program at Plant 1, as opposed to the other facilities, appears to be somewhat random, allowing us to treat it as "quasi-experimental." The corporate headquarters was unaware of the decision to implement the local program. Furthermore, the award program does not appear to have been created to address a problem exceptional to Plant 1.<sup>11</sup> All employees without an excused absence or tardy in the previous month were eligible for the award. For the purpose of the award, an unexcused absence was defined as an unexpected absence where the employee did not notify the plant management before the absence, while "tardy" was defined as being more than five minutes late. All eligible employees were announced at a monthly plant-wide meeting, where the plant manager or the previous winner randomly drew the name of one award-winning employee for a \$75 gift card to a local restaurant or store. Although we define the "award winner" as the employee receiving the gift card, employees who were

---

<sup>11</sup> We will address differences in the plants later in the descriptive statistics.

eligible for the award each month also received some recognition among peers, which could have small motivational effects.<sup>12</sup>

The program was announced by the plant manager in mid-February, 2011, and began on March 1. The plant-wide meeting and award drawing for the period of March 1 to March 30 was held in first week of April. The executive team of *LaundryCo* learned about the program in December, 2011, and abruptly terminated it, despite the objections of the general and plant manager, because they felt the program rewarded employees for behavior that should be expected of all employees. Figure 4 shows that between half and two-thirds of all workers were eligible to receive the award in any given month of the program. Figure 5 presents the number of months that each employee was eligible, with nine employees being eligible every month in the dataset.

## 4. Identification Strategy

### 4.1 Data and Identification Strategy

The data for this study were assembled by merging multiple datasets from *LaundryCo*'s IT system. These data include attendance measures as well as measures of daily worker productivity for 276 workers in five plants between March 1, 2010 and November 30, 2011. The typical observation details an individual worker's daily record, including clock-in and clock-out times, information on items worked on, productivity ratings for each item, overall daily productivity output, type of absence (e.g. paid time off day, holiday, funeral), and demographic variables such as tenure, age, gender, and marital status. The final number of worker/day observations is 62,995.

We did not have access to *LaundryCo*'s employee schedules, and therefore inferred a worker's shift from her observed clock-in and clock-out times and the list of actual shift times used at the company. To identify shift start-times for each worker, we manually compared each worker's frequency distribution of clock-in times and the list of actual shift times. We then assigned each worker the shift times that most closely corresponded to observed clock-in times. This resulted in assigning between one and five shift start-times to every worker each year.<sup>13</sup> To check the accuracy of this procedure, we compared the date of the first "tardy" observed in the identified shift time to the actual date of the first "tardy," which the manager of Plant 1 recorded for purposes of the award. While our manual coding was highly accurate for normal weekday shifts, this manual coding was less accurate on days when the worker arranged to come in late, switched shifts with other workers, or worked extra hours such as on a weekend or by covering for another worker's shift. To minimize these problems, we restricted our analysis to weekdays, since there are far more ad-hoc schedule

---

<sup>12</sup> Qualified employees were recognized only by announcing their names to facilitate the award drawing.

<sup>13</sup> While workers would usually have a set shift time that corresponded to most observed clock-in times, many workers varied their start time depending on seasonal demand, plant need, or shifting job responsibilities. For instance, a worker may have a shift time starting at 9:00am for a few months and then shift to 8:00am for the rest of the year, occasionally coming in at 7:30am during periods of high demand.

shifts on weekends. We also dropped observations where an employee worked less than 7.5 hours of an 8.5 hour shift, and when our calculations indicate a worker was more than an hour early or late.<sup>14</sup> This affected less than 1% of total weekday observations.

We use five main dependent variables in this paper. The first four (*tardy*, *minutes late*, *absent*, and *daily efficiency*), represent performance metrics that the plant manager hoped to improve through the attendance award program. *Tardy* is a dummy variable indicating that a given worker showed up more than five minutes late on a given day, and is the metric for disqualification in the award program. *Minutes late* is a continuous measure of the minutes and seconds late an employee showed up to work, and captures the extra time gained or lost in the program. *Minutes late* can take a negative value if the employee clocked in early. *Absent* is a dummy variable indicating that the worker was absent from work for reasons within the employee's control, such as for sickness, vacation, or paid time off. This excludes absences from holidays, workers compensation days, suspensions, family medical leave, and funerals. *Daily worker efficiency* is the worker's productivity efficiency number for the day.<sup>15</sup> Figure 3 shows that a typical employee will have an efficiency number around 110-120, with high performers consistently performing above 130 and low-performers at less than 100. The last dependent variable, *single absence*, is a dummy variable indicating the employee was absent that day but was not absent the previous or following workday. We will use this dependent variable to examine potential gaming behavior, since it may indicate workers calling in sick days to avoid disqualification on days in which they are behind schedule.

We use a difference-in-differences (DiD) design to assess the impact of the introduction of the attendance award on the attendance, punctuality, and productivity of Plant 1's workers. The DiD design treats *LaundryCo's* other four plants as a quasi-control group. The DiD strategy "differences out" fixed differences between Plant 1 and the control group plants, and essentially uses post-treatment changes at control group plants as a counterfactual for what would have happened at Plant 1 had it not enacted the policy. The DiD approach is the most widely used statistical methodology in the social sciences to examine the impact of policy changes (Gertler et al. 2011).

Although DiD strategies do not require that treatment and control groups be the same, in our case the treatment and control plants are very similar on most dimensions. All five plants work on essentially the same range of tasks, use the same production technology, and share common floor layouts. Also, two of the control group plants are geographically proximate to Plant 1 (32 and 34 miles, respectively), and therefore help control for local shocks such as bad weather that might affect tardiness. All five plants had the same

---

<sup>14</sup> Additionally, we reran our analysis dropping those employees that were more than either thirty or forty-five minutes late. The results for both of these analyses were very similar in significance and magnitude, although the results on the *single absence* variable became insignificant.

<sup>15</sup> To minimize the impact of single productivity observations that were outliers, we winsorized daily worker efficiency to two standard deviations from the mean. Based on our discussion with multiple plant managers about the normal range of worker productivity, any such observation was very likely an error.

attendance policies and enforcement procedures. Similarly, DiD studies do not require random assignment of treatment, but in our case the treatment is quite exogenous, given that the manager of Plant 1 introduced the award without discussing the idea with the other plant managers or *LaundryCo's* senior management.

The biggest difference between Plant 1 and the control group plants is that Plant 1 is the only plant that is unionized. This fact leads to some clear differences between the treatment and control groups, as seen in Table 1, which summarizes the data for the two groups at the worker-day level. Although fairly similar in age, employees at Plant 1 have much higher tenure and also significantly higher wages. There are also significantly more males at Plant 1. Table 1 also shows that employees at Plant 1 had similar levels of tardiness and attendance as control plant employees. Table 2 presents employee-level statistics. Although differences between the treatment and control plants here appear larger, they are biased by their equal weighting of employees with heterogeneous job tenures.

## 4.2 Empirical Specification

We use a standard DiD specification to estimate the impact of the award on the dependent variables in the study:

$$Y_{it} = \alpha_i + \beta_1 * TREAT\_GRP_i + \beta_2 * POSTTREAT_t + \beta_3 * TREAT\_GRP_i * POSTTREAT_t + \Upsilon_t + \varepsilon_{it}$$

where  $Y_{it}$  is the dependent variable,  $TREAT\_GRP$  is a dummy variable equal to 1 if the worker is in the treatment group and zero otherwise,  $POSTTREAT$  is a dummy variable equal to 1 for dates after the attendance program went into effect and zero otherwise, and  $TREAT\_GRP * POSTTREAT$  is the interaction of the two previous variables. The coefficient on this interaction ( $\beta_3$ ) estimates the treatment effect; to conserve space we simply label this variable as “Treatment” in the results tables. Our specification takes advantage of the panel nature of the data by introducing a full set of fixed effects. Specifically, we use worker fixed effects ( $\alpha_i$ ) to account for unobserved worker heterogeneity and time fixed effects ( $\Upsilon_t$ ) for time trends across months, days in the month, and days of the week. In specifications with worker fixed effects the variable  $TREAT\_GRP$  is absorbed, and in specifications with time fixed effects the variable  $POSTTREAT$  is absorbed.

All the regressions reported in the paper were estimated using Ordinary Least Squares (OLS). Three of the five dependent variables are binary, but logit or probit models perform poorly when predicting rare events such as the ones in this study (King and Zeng 2001). In contrast, OLS specifications result in unbiased coefficient estimates (Stewart 2009), but incorrect standard errors. However, DiD models typically underestimate standard errors regardless of the specification (Bertrand et al. 2004), so we bootstrap standard errors for all regressions in the study. Therefore, OLS is appropriate for coefficient estimation for all five of the study’s dependent variables. In contrast to other approaches such as the rare events logistic model, OLS is

also convenient in terms of time required to bootstrap, and in ease of coefficient interpretation.

## 5. Results

### 5.1 Basic Treatment Effects

Table 3 shows the regression results of the basic treatment effect for the four dependent variables that represent performance in the study. The models include fixed effects for employees, months, day of the week, and day of the month, and show substantial improvement on two of the four performance metrics. The award caused a 1.6 percentage point reduction in the level of tardiness at Plant 1, meaning tardies were reduced by nearly 40% from the baseline of 4%. In addition, the average worker clocked in nearly an entire minute earlier, which is also an effect size of nearly 50%. We do not, however observe a statistically significant treatment effect on absences or production efficiency, suggesting that the impact of the award program was limited to tardiness.

DiD models are known to result in false positives due to serial correlation issues (Bertrand et al. 2004). Random assignment of treatment groups and dates can lead to the estimation of significant effects some 45% of the time. While bootstrapping standard errors helps alleviate this problem, it is important to use placebo testing to demonstrate that the identified treatment effects were not spuriously generated. We ran two types of placebo tests: one in which we randomly assigned employees to the treatment group but kept the actual treatment date, and one in which we randomly assigned both the treatment employees and the treatment date. We used 50 placebos for each type. We would expect the placebo tests to result in statistically significant treatment effects at the 5% level only 5% of the time, and we would also expect the average treatment effect to be close to zero.

The average results of the placebo tests are shown in Table 4. For both types of placebos, the mean coefficient is both close to zero and significantly smaller than the corresponding coefficient from Table 3. Also, across the placebos the results are significant at close to the expected rate. We present the point estimate and 95% confidence intervals for each placebo regression of *minutes late* in Figure 6, which show that our point estimate has the smallest standard errors of any of the individual placebos.<sup>16</sup> Overall, the placebo test results suggest that the treatment effects shown in Table 3 were actually a result of the treatment.

### 5.2 Evidence on Employee Gaming

To examine potential gaming behavior by employees, we first examine whether employees were more likely to have single absences during the attendance award program. While we observed no effect on overall absences, column 1 of Table 5 shows that employees were over half a percentage point *more* likely to have a single absence (as opposed to one covering multiple days) after the introduction of the award, which

---

<sup>16</sup> Placebo regressions for our other models produced similar results.

represents an over 50% increase in the likelihood of having a single absence. This result, in combination with the results on tardies and minutes late, suggest an interesting story: workers show up earlier and are less likely to be tardy, but are also much more likely to have a single absence. The award may cause an increase in single absences by motivating employees who are running late to call in sick and stay eligible for the award. Alternatively, workers who would have been late and therefore become disqualified for the award may simply stay at home since they lost their chance for the award.

We also examine employee behavior conditional on their eligibility for the monthly award on a given day. To do this, we introduce two dummy variables for each employee-day, indicating whether the employee was still eligible for the award (“pre-fail”) or had a tardy or absence which disqualified her that month (“post-fail”). This essentially breaks the treatment dummy into two groups. The results of this analysis are shown in columns 2-6 of Table 5. Perhaps unsurprisingly, all of the effect of the award occurs due to the behavior of employees who are still eligible for it. Although Wald tests cannot definitely show that the pre- and post- coefficients are statistically different, the larger magnitude and statistical significance of the pre-fail coefficients is consistent with employees gaming the award system. These results, combined with those for single absences, suggest that employees strategically respond to the award program, changing their behavior only when such actions improve their likelihood of winning the award. The award therefore did not appear to habituate punctuality.

### 5.3 Heterogeneous Treatment Effects

We next investigate how different types of workers respond to the introduction of the award. The award was focused on improving the timeliness and attendance of employees who sometimes clocked in late or had unexcused absences; however, the award could have spillover effects on other employee types. We therefore focus on differential response to the award among two employee dimensions: pre-treatment attendance behavior, and pre-treatment efficiency.<sup>17</sup> There is only a small correlation between efficiency and punctuality; the correlation coefficient between efficiency and minutes late is -0.03, suggesting more efficient employees come to work slightly earlier.

There are two common statistical methods by which to compare heterogeneous treatment effects: the use of interaction effects and the use of sub-sample analysis. If implemented properly, the two methods are in fact equivalent (Fink et al. 2012). However, both methods are more often implemented improperly, resulting in false positives (Fink et al. 2012). Because the treatment effect in DiD models is already an interaction term, using another interaction term to model heterogeneous response would be effectively introducing a triple interaction into the model, which makes the raw regression output difficult to correctly interpret (but unfortunately easy to incorrectly interpret). We will therefore use sub-sample analysis in the

---

<sup>17</sup> We also investigated the impact of employee gender, age, and tenure level, and found few differences across employee groups.



paper.

**5.3.1 Treatment Effects on Tardiness by Level of Pre-Award Tardiness:** We first examine the behavior of employees who were never tardy in the 12 months before the award was introduced (“no tardy” sample), those who were tardy less than 1.5% of the time (“low tardy”), and those who were tardy greater than 1.5% of the time (“high tardy”).<sup>18</sup> These results are shown in Tables 6a and 6b. The *high tardy* group is the only group statistically less likely to show up tardy due to the award, but they do not change with regard to how many minutes they show up early. In contrast, the “low tardy” group does not decrease its rate of tardiness, but comes to work nearly a minute and a half earlier due to the award. Both of these groups are more likely to take single absences. Perhaps most interestingly, the only effect on the *no tardy* group is a large increase in the likelihood of being absent, meaning the award appears to be causing this group to take more planned absences. Overall, this analysis again suggests a very strategic response to the award by each of these groups. Highly tardy employees appear adept at reducing tardy rates while not consistently coming earlier to work; low tardy employees appear to push to come a bit earlier, possibly because they are even more averse to the risk of being tardy; and no tardy employees appear to be spurred to plan ahead with regard to absences.

Tables 7a and 7b examine workers who are almost always early for their shifts, workers who tend to “punch in” just as their shift starts, and workers who are on average at least one minute late to clock in.<sup>19</sup> Here the most striking result is that workers who are habitually early are three percentage points more likely to be tardy after the award introduction. The coefficient on the *minutes late* variable also suggests they stop arriving to work as early as they did, although this coefficient is not close to significant. Workers who were punching in early and in effect “working for free” for a few minutes appear to have reacted to the award in a dysfunctional way, perhaps because they resented the introduction of a formal reward for something they had been doing on a daily basis already.

**5.3.2.: Treatment Effects on Production Efficiency:** We finally examine heterogeneous treatment effects on efficiency. We look at two sub-samples: the “No/Low/High Tardy” sub-samples examined earlier, and also sub-samples composed of low efficiency employees (<110), average efficiency employees (110-130), and high efficiency employees (130+). The results of this analysis are shown in Table 8. In both cases, the “best” employees react to the award with significantly lower productivity; their average daily productivity drops by 8-9 points, or around 6%. Since high efficiency employees regularly surpassed the 140 threshold for the final pay bump, the benefits of any excess efficiency was captured by the company, not the worker. The award seems to have “crowded out” the desire of high efficiency employees to continue to work hard. That

---

<sup>18</sup> This classification cuts the “has tardies” employee group in half.

<sup>19</sup> This classification effectively places about 10% of the sample’s employees in the early group, 15% in the late group, and 75% in the on-time group.

is, employees who previously produced extra output that only benefitted the company, not themselves,, reacted to the award by reducing their productivity.

While we cannot observe the mechanism driving this demotivation, this result is consistent with the crowding out arguments presented earlier. It could be either due to perceived unfairness or decreased image motivation, although we are unable to separate these potential mechanisms in our data. While we only have three months of productivity data, it appears that in general the attendance awards program generated negative spillovers that decreased productivity. Most seriously, the spillovers affected those employees that were the best attenders, based on pre-treatment tardiness.

This interpretation is further supported by looking at the effect of disqualification on the three efficiency sub-samples, shown in Table 9. There is no treatment effect on low efficiency workers before they are disqualified for an award; however, after disqualification the efficiency of these workers decreases by over 12 points, or nearly 15%. However, for high efficiency workers, productivity decreases by approximately the same amount both before and after disqualification. These results are consistent with the hypothesis that low efficiency workers react in strategic or “rational” ways to the program, while high efficiency workers seem to believe the program itself is unfair.

## 6. Discussion

This study demonstrates that an award program that appears to be effective may also induce unintended consequences that severely reduce the net value of the program. At face value, the award used in the study was effective in that it reduced the average rate of tardiness among employees. However, the award also led to a host of potential spillover effects that the plant manager readily admits were not considered when designing the program. The largest spillover in this study was the significant productivity decrease by the plant’s most productive and punctual employees, which appears to be directly caused by the award. This finding is remarkable because it suggests that awards for one type of behavior have the potential to “crowd out” positive behavior in a completely different realm. It also suggests that Neckermann et al’s (2012) observation of spillovers from awards on one dimension to behavior on another can be negative as well as positive. Our results complement their paper by suggesting that even simple awards programs can have much broader and complex implications for employee behavior.

Overall our results suggest a negative and significant impact from the award program on net plant productivity. To measure the overall impact to the firm, we used our regression coefficients from Tables 6a, 6b, and 8 to roughly calculate productivity and financial implications.<sup>20</sup> These calculations, which assume

---

<sup>20</sup> This is a high-level calculation computed using the weighted impact of the awards program on the three attendance groups specified earlier (notardy, lowtardy and hightardy). We first calculated the impact of the program on efficiency, minutes late to work, and single absences for a single “model” employee in each group, then multiplied this effect by the number of workers in each group before summing the totals.

workers work eight hours a day and 22 days a month, find the awards program reduced Plant 1's productivity by 1.4%, or nearly half an additional full-time worker. The increased punctuality by low tardy workers only increased overall plant productivity by 0.1%. However, the increase in single absences reduced productivity by 0.5%. Therefore, even in its direct impact on productivity through attendance, the award decreased productivity. However, the largest impact of productivity came from reduced productivity from workers with the best pre-award attendance, which reduced the plant's overall productivity by 1%.

A conservative estimate of the financial impact, which includes wage costs plus 37% additional benefits for additional labor hours, suggests at least a \$1,458 monthly cost from the program.<sup>21</sup> We note that this estimate is extremely conservative, given that it ignores training costs, overhead, and other variable costs associated with hiring additional workers. These results highlight the impact such a program can have on the overall performance of the firm and suggest caution when designing and implementing such programs.

Our results stand in contrast to the emerging conventional wisdom that awards and other forms of on-the-job recognition provide a "free" way to motivate employees. The award used in our setting appeared to act much less like a subtle "nudge" than like a traditional incentive program to which employees strategically responded. Behavioral economists and social psychologists have long been interested in small interventions that can change behavior, often in ways unperceived by the actor, but our results suggest that a more nuanced view is needed that considers unintended consequences and the possibility of strategic response. An award in an employment setting is, after all, a formal program designed by a manager, and not simply a small difference in framing. Scholars have long known that any formal program carries both benefits and costs, and this study serves as a reminder of this important lesson.

Like many studies of this type, our study has a number of limitations. First, the assignment of workers to treatment and control groups was not random, and there existed differences between the two groups, such as the non-union status of the four control plants. This fixed difference, and any other pre-existing differences between the groups, is theoretically "differenced out" by the DiD methodology. However, the DiD methodology does assume common time trends across groups. We carried out interviews with managers at treatment and control plants, and apart from a health screening offered to employees of the control plants in April, 2011, managers reported no changes in policies or management approaches that changed around the time of the introduction of the award. Alternative models that estimate monthly treatment effects, represented in Figure 7 (for *minutes late*), show the treatment effect to be largest and statistically significant in March, 2011, the month the award program was implemented (and a month before the health screening intervention). This suggests that the health intervention, or any other unobserved change that wasn't exactly correlated with treatment, was unlikely to explain our results. However, as with any DiD study, we cannot definitively rule out the possibility of unobserved shocks correlated with treatment that

---

<sup>21</sup> This number is calculated based on average annual costs from wages (\$25,057), retirement matching (\$1,101), health insurance (\$8,269), payroll taxes (\$1,416), and the awards themselves (\$675).

affected only one group. Similarly, although one concern is that our split sample results on productivity reflect mean reversion, the sharp drop in productivity exactly in the month the award was introduced, combined with the lack of improvement by the low productivity group, make this explanation unlikely.

The mechanism leading to the strategic gaming of the award system is clear. However, we cannot definitively prove the mechanism by which the attendance award negatively affected productivity of some workers. The explanation that the award “crowded out” the intrinsic motivation of workers is theoretically appealing, especially since these workers were working hard far past the point where it increased their compensation. However, this interpretation is only speculative, since we do not know the factor(s) that caused highly productive workers to continue to work hard even when it did not increase their compensation. There are a number of explanations for this uncompensated productivity outside of intrinsic motivation. For example, peer or manager pressure may have led to increased effort even when that effort was uncompensated, and the award may have caused workers to be less responsive to these mechanisms. Our results do show, however, that the motivational mechanism responsible for the uncompensated productivity was reduced by the award for a subset of employees.

As is common in research of this type, our study follows a single company’s experience. While tardiness and absenteeism are large problems for many businesses, it is unclear whether the effect of the award found in this study would translate across organizations and industries. This problem is of particular concern given that Plant 1 was unionized, which surely affected employee response. It is plausible that the effect sizes found in this setting are lower than they would be in non-union settings, since non-union employees are arguably more concerned about the increased job security an award might bring. On the other hand, it is also plausible that the relative lack of concern for job security in our unionized environment may make the employees of Plant 1 more concerned about the status benefits accruing from the award.

Finally, our results consider an award program that randomly awards a single employee, chosen from all who qualify, rather than to a fixed number or percentage of employees. Many awards are given to only a few employees, such as “employee of the month” or “Circle of Excellence,” which recognizes the top 10% of employees and is common in the sales function (Larkin, 2010). As with monetary incentives, there are a large number of ways in which award programs might vary, and the costs and benefits of these programs likely vary across award type. We therefore suggest caution in applying the study’s results to all types of awards, but we also believe that scholars and practitioners should take seriously the strong possibility of spillovers and other unintended consequences in any award system.

### *Theoretical implications*

Scholars of incentives in economics have only recently started to consider the psychological effects of incentive programs. On the other hand, social psychologists have traditionally been focused on the response of individual employees to different incentive programs, rather than questions of firm-level effects

or optimal incentive design (Nickerson and Zenger 2008; Larkin et al. 2012). This study provides further evidence that scholars need to incorporate the strengths of both approaches: the holistic, cost-benefit lens of economics, and the realistic model of human behavior from social psychology incorporating biases and emotions.

Although the study does not directly show “crowding out” (Deci 1975; Deci and Ryan 1985), it does suggest that employees who previously behaved in ways that did not give them any extra monetary benefit – namely, showing up early for their shift and continuing to work hard when there was no monetary benefit to do so – cut back on this behavior due to the introduction of the reward. The notion of “crowding out” is usually applied to monetary rewards (e.g. Gneezy and Rustichini 2000b), with recent work suggesting that non-monetary rewards do not reduce prosocial blood donations (Goette and Stutzer 2008; Lacetera et al. 2012). In contrast, our results suggest that “crowding out” may also apply to extrinsic but non-monetary rewards such as corporate awards. In addition, “crowding out” is usually focused on reduction in motivation for actions that are compensated. Our results suggest that compensating one action can lead to “crowding out” in a completely different realm. Existing research further suggests that the long-term costs from demotivation, particularly following program termination, may be even greater than what our time range of data can show (Gneezy and Rustichini 2000a; Meier 2007).

Overall, the results of this study suggest that in the long-run, non-monetary but extrinsic rewards such as corporate awards act more like monetary rewards than they do intrinsic motivators such as love for the job or empowerment through autonomy. Much of the existing literature seems to contrast monetary compensation with extrinsic but non-monetary rewards, drawing on the arguments from the literature on intrinsic motivation. Our results suggest the opposite hypothesis requires investigation – that corporate awards are closer to a monetary reward than a purely intrinsic motivator.

Our results also suggest that award programs with a low likelihood of winning may be ineffective because employees learn relatively quickly that effort is unlikely to result in status, recognition, or rewards. This is consistent with a growing literature on how tournaments with more competition can produce less effort and productivity (Che and Gayle 2003; Fullerton and McAfee 1999; Boudreau et al. 2011) as well as sabotage (Carpenter et al. 2010). Our results are also consistent with recent models on the importance of rewarding non-winners as well as winners (Minor 2012). Demanding perfection from employees may remove all motivation for employees whose probability of winning can discretely drop to zero with a single mistake.

## References

- Adams, J.S. 1963. Toward an understanding of inequity. *Journal of Abnormal and Social Psychology* **67**(1) 422-436.
- Akerlof, G.A. 1980. A theory of social custom, of which unemployment may be on consequence. *Quarterly Journal of Economics* **94**(4) 749-775.
- Akerlof, G.A., R.E. Kranton. 2005. Identity and the economics of organizations. *The Journal of Economic Perspectives* **19**(1) 9–32.
- Akerlof, G.A., J.L. Yellen. 1988. Fairness and unemployment. *The American Economic Review* **78**(2) 44-49.
- Akerlof, G.A., J.L. Yellen. 1990. The fair wage-effort hypothesis and unemployment. *The Quarterly Journal of Economics* **105**(2) 255-283.
- Ambrose, M., M.A. Seabright, M. Schminke. 2002. Sabotage in the workplace: The role of organizational injustice. *Organizational Behavior and Human Decision Processes* **89**(1) 947–965.
- Andreoni, J., B. Bernheim. 2009. Social image and the 50-50 norm: A theoretical and experimental analysis of audience effects. *Econometrica* **77**(5) 1607-1636.
- Andreoni, J., R. Petrie. 2004. Public goods experiments without confidentiality: A glimpse into fund-raising. *Journal of Public Economics* **88**(7-8) 1605-1623.
- Ariely, D., A. Bracha, S. Meier. 2009. Doing good or doing well? Image motivation and monetary incentives in behaving prosocially. *American Economic Review* **99**(1) 544-555.
- Bandiera, O., I. Barankay, I. Rasul. 2010. Social incentives in the workplace. *The Review of Economic Studies* **77**(2) 417-458.
- Barankay, I. 2012. Rank incentives: Evidence from a randomized workplace experiment. Working paper, Wharton School, Pennsylvania.
- Benabou R., J. Tirole. 2006. Incentives and prosocial behavior. *American Economic Review* **96**(5) 1652-1678.
- Bertrand M., E. Duflo, S. Mullainathan. 2004. How much should we trust differences-in-differences estimates? *The Quarterly Journal of Economics* **119**(1) 249-275.
- Besley, T., M. Ghatak. 2008. Status incentives. *The American Economic Review* **98**(2) 206–211.
- Boudreau, K.J., N. Lacetera, K.R. Lakhani. 2011. Incentives and problem uncertainty in innovation contests: An empirical analysis. *Management Science* **57**(5) 843–863.
- Blanes i Vidal, J., M. Nossol. 2011. Tournaments without prizes: Evidence from personnel records. *Management Science* **57**(10) 1721–1736.
- Boyle, D. 1996. Divining the secrets of a successful employee recognition system. *Security Management* **40**(7) 21–24.
- Camden, M.C., V.A. Price, T.D. Ludwig. 2011. Reducing absenteeism and rescheduling among grocery store employees with point-contingent rewards. *Journal of Organizational Behavior Management* **31**(2) 140-149.
- Cameron, J., W.D. Pierce. 2002. *Rewards and Intrinsic Motivation: Resolving the Controversy*. Bergin & Garvey.
- Card, D., A. Mas, E. Moretti, E. Saez. 2012. Inequality at work: The effect of peer salaries on job satisfaction. *American Economics Review* **102**(6) 2981-3003.
- Carpenter, J., P. Matthews, J. Schirm. 2010. Tournaments and office politics: Evidence from a real effort experiment. *American Economic Review* **100**(1) 504-517.
- Che, Y-K., I. Gale. 2003. Optimal design of research contests. *The American Economic Review* **93**(3) 646.

- Cooper, C.L., B. Dyck, N. Frohlich. 1992. Improving the effectiveness of gainsharing: The role of fairness and participation. *Administrative Science Quarterly* **37**(3) 471-490.
- Dana, J., D. Cain, R. Dawes. 2006. What you don't know won't hurt you: Costly (but quiet) exit in dictator games. *Organizational Behavior and Human Decision Processes* **100**(2) 193-201.
- Davidson, L. 1999. The power of personal recognition. *Workforce* **78**(7) 44-49.
- De Boer, E.M., A.B. Bakker. Unfairness at work as a predictor of absenteeism. *Journal of Organizational Behavior* **23**(2) 181-197.
- Deci, E.L. 1975. *Intrinsic Motivation*. Plenum Press, New York, NY.
- Deci, E.L., R.M. Ryan. 1985. *Intrinsic Motivation and Self-Determination in Human Behavior*. Plenum Press, New York, NY.
- DeLonzor, D. 2005. Running late. *HRMagazine* **50**(11) 109-112.
- Edelman, B.G., I. Larkin. 2013. Social comparisons and deception across workplace hierarchies: Field and experimental evidence. *Unpublished working paper*.
- Ederer, F., E. Fehr. 2009. Deception and incentives: How dishonesty undermines effort provision. *Unpublished working paper*. <http://www.anderson.ucla.edu/faculty/florian.ederer/dishonesty.pdf>
- Ellingsen, T., M. Johannesson. 2008. Pride and prejudice: The human side of incentive theory. *American Economic Review* **98**(3) 990-1008.
- Fehr E., L. Goette, C. Zehnder. 2009. A behavioral account of the labor market: The role of fairness concerns. *Annual Review of Economics* **1**(1) 355-384.
- Festinger, L. 1954. A Theory of Social Comparison Processes. *Human Relations* **7**(1) 117-140.
- Fink, G., M. McConnell, S. Vollmer. 2012. Testing for heterogenous treatment effects in experimental data: False discovery risks and correction procedures. Available at: <http://www3.wiwi.uni-hannover.de/Forschung/Diskussionspapiere/dp-477.pdf> [Accessed February 7, 2013].
- Frey, B., F. Olberholzer-Gee. 1997. The cost of price incentives: An empirical analysis of motivation crowding-out. *American Economic Review* **87**(4) 746-755.
- Frey, B., S. Meier. 2004. Social comparison and pro-social behavior: Testing conditional cooperation in a field experiment. *American Economic Review* **94**(1) 1717-22.
- Fullerton, R.L., R.P. McAfee. 1999. Auctioning entry into tournaments. *The Journal of Political Economy* **107**(3) 573-605.
- Garcia, S.M., A. Tor, R. Gonzalez. 2006. Ranks and rivals: A theory of competition. *Personality and Social Psychology Bulletin* **32**(1) 970-982.
- Garr, S. 2012. *The State of Employee Recognition in 2012*. Research Report, Bersin and Associates, Oakland, CA.
- Gertler, P.J., S. Martinez, P. Permand, L.B. Rawlings, C.M.J. Vermeersch. 2011. *Impact Evaluation in Practice*. The World Bank, Washington DC.
- Gino, F., L. Pierce. 2009. Dishonesty in the name of equity. *Psychological Science* **20**(9) 1153-1160.
- Gneezy, U., A. Rustichini. 2000a. A fine is a price. *Journal of Legal Studies* **29**(1) 1-18.
- Gneezy, U., A. Rustichini. 2000b. Pay enough or don't pay at all. *Quarterly Journal of Economics* **115**(3) 791-810.
- Gneezy, U., S. Meier, P. Rey-Biel. 2011. When and why incentives (don't) work to modify behavior. *Journal of Economic Perspectives* **25**(4) 191-210.
- Goette, L., A. Stutzer. 2008. Blood donations and incentives: Evidence from a field experiment. IZA

- Working paper No. 3580. Available at: <http://ssrn.com.libproxy.wustl.edu/abstract=1158977> or <http://dx.doi.org.libproxy.wustl.edu/10.1111/j.0042-7092.2007.00700.x> [accessed February 11, 2013].
- Greenberg, J. 1988. Equity and workplace status: A field experiment. *Journal of Applied Psychology* **73**(4) 606-613.
- Hars, A., S. Ou. 2001. Working for free? Motivations of participating in open source projects. *Proceedings of the 34th Annual Hawaii International Conference on System Sciences*. pp. 9.
- Huberman, B.A., C.H. Loch, A. ÖNçüler. 2004. Status as a valued resource. *Social Psychology Quarterly* **67**(1) 103-114.
- Ichino, A., G. Maggi. 2000. Work environment and individual background: Explaining regional shirking differentials in a large Italian firm. *The Quarterly Journal of Economics* **115**(3) 1057-1090.
- Ichino, A., R.T. Riphahn. 2005. The effect of employment protection on worker effort: Absenteeism during and after probation. *Journal of the European Economic Association* **3**(1) 120-143.
- Kerr, S. 1975. On the folly of rewarding A, while hoping for B. *The Academy of Management Journal* **18**(4) 769-783.
- King, G., L. Zeng. 2001. Logistic regression in rare events data. *Political Analysis* **9**(1) 137-163.
- Knight, D., C. Durham, E. Locke. 2001. The relationship of team goals, incentives and efficacy to strategic risk, tactical implementation and performance. *Academy of Management Journal* **44**(2) 326-338.
- Kosfeld, M., S. Neckermann. 2011. Getting more work for nothing? Symbolic awards and worker performance. *American Economic Journal: Microeconomics* **3**(3) 86-99.
- Kuhnen, C.M., A. Tymula. 2010. Feedback, self-esteem, and performance in organizations. *Management Science* **58**(1) 94-113.
- Lacetera, N., M. Macis. 2010. Social concerns and prosocial behavior: Field evidence from a nonlinear incentive scheme. *Journal of Economic Behavior & Organization* **76**(2) 225-237.
- Lacetera, N., M. Macis, R. Slonim. 2012. Will there be blood? Incentives and displacement effects in pro-social behavior. *American Economic Journal: Economic Policy* **4**(1) 186-223.
- Larkin, I. 2010. Paying \$30,000 for a gold star: An empirical investigation into the value of peer recognition to software salespeople. Working paper, Harvard Business School, Cambridge.
- Larkin, I. 2013. The cost of high powered incentives: Employee gaming in enterprise software sales.
- Larkin, I, L. Pierce, F. Gino. 2012. The psychological costs of pay-for-performance: Implications for the strategic compensation of employees. *Strategic Management Journal* **33**(10) 1194-1214.
- Malmendier, U., G.A. Tate. 2009. Superstar CEOs. *Quarterly Journal of Economics* **124**(4): 1593-1638.
- Markham, S.E., K.D. Scott, G.H. McKee. 2002. Recognizing good attendance: A longitudinal, quasi-experimental field study. *Personnel Psychology* **55**(3) 639-660.
- Mas, A., E. Moretti. 2009. Peers at work. *The American Economic Review* **99**(1) 112-145.
- Meier, S. 2007. Do subsidies increase charitable giving in the long run? Matching donations in a field experiment. *Journal of the European Economic Association* **5**(6): 1203-1222.
- Minor, D.B. 2012. Increasing revenue through rewarding the best less (or not at all). Available at: [http://www.kellogg.northwestern.edu/faculty/minor/Papers/Increasing%20Effort%20\(with%20Figures\).pdf](http://www.kellogg.northwestern.edu/faculty/minor/Papers/Increasing%20Effort%20(with%20Figures).pdf) [Accessed September 21, 2012].
- Moldovanu, B., A. Sela, X. Shi. 2007. Contests for status. *Journal of Political Economy* **115**(2) 338-363.
- Navarro, C., C. Bass. 2006. The cost of employee absenteeism. *Compensation and Benefits Review* **38**(6) 26-30.



- Neckermann, S., R. Cueni, B. Frey. 2012. Awards at Work. *ZEW Discussion Paper No. 12-004*.
- Nickerson, J.A., T.R. Zenger. 2008. Envy, comparison costs, and the economic theory of the firm. *Strategic Management Journal* **29**(13) 1429-1449.
- O'Reilly, C.A., J. Chatman. 1986. Organizational commitment and psychological attachment: The effects of compliance, identification, and internalization on prosocial behavior. *Journal of Applied Psychology* **71**(3) 492-499
- Oyer, P. 1998. Fiscal year ends and nonlinear incentive contracts: The effect on business seasonality. *Quarterly Journal of Economics* 113(1) 149-185.
- Pauly, M.V., S. Nicholson, J. Xu, D. Polsky, P.M. Danzon, J.F. Murray, M.L. Berger. 2002. *Health Economics* **11**(3) 221-231.
- Pedalino, E., V.U. Gamboa. 1974. Behavior-modification and absenteeism - Intervention in one industrial setting. *Journal of Applied Psychology* **59**(6) 694-698.
- Scott, D., S. Markham. 1982. Absenteeism control methods: A survey of practices and results. *The Personnel Administrator* **27**(6) 73-84.
- Skarlicki, D.P., R. Folger. 1997. Retaliation in the workplace: The roles of distributive, procedural, and interactional justice. *Journal of Applied Psychology* **82**(3) 434-443
- Stajkovic, A.D., F. Luthans. 1997. A meta-analysis of the effects of organizational behavior modification on task performance, 1975-95. *Academy of Management Journal* **40**(5) 1122-1149.
- Stewart, J. 2009. Tobit or not Tobit? *IZA Discussion Paper No. 4588*. Available at: [http://papers.ssrn.com/sol3/papers.cfm?abstract\\_id=1515135](http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1515135) [Accessed October 30, 2012].

**Table 1: Descriptive Statistics at Worker/Day Level**

| <b>TREATMENT</b> |      | <i>Pre-Treatment</i> |       |       |       | <i>Post-Treatment</i> |       |       |       |       |
|------------------|------|----------------------|-------|-------|-------|-----------------------|-------|-------|-------|-------|
| Variable         | Obs  | Mean                 | Sd    | Min   | Max   | Obs                   | Mean  | Sd    | Min   | Max   |
| Tardy            | 7126 | 0.04                 | 0.20  | 0.0   | 1.0   | 5918                  | 0.03  | 0.16  | 0.0   | 1.0   |
| Minutes late     | 7126 | -1.73                | 5.37  | -59.7 | 54.0  | 5913                  | -2.09 | 5.14  | -49.0 | 58.2  |
| Total absences   | 7846 | 0.04                 | 0.19  | 0.0   | 1.0   | 6502                  | 0.05  | 0.21  | 0.0   | 1.0   |
| Single absences  | 7846 | 0.01                 | 0.07  | 0.0   | 1.0   | 6502                  | 0.01  | 0.11  | 0.0   | 1.0   |
| Daily efficiency | 6313 | 126.2                | 32.4  | 55.6  | 218.1 | 1726                  | 125.1 | 32.3  | 55.6  | 218.1 |
| Late             | 7126 | 0.15                 | 0.35  | 0.0   | 1.0   | 5918                  | 0.10  | 0.30  | 0.0   | 1.0   |
| Monthly absences | 7861 | 0.79                 | 1.65  | 0.0   | 8.0   | 6524                  | 1.00  | 1.78  | 0.0   | 10.0  |
| Total hrs worked | 7861 | 8.25                 | 1.64  | 0.0   | 23.7  | 6524                  | 8.17  | 1.67  | 0.0   | 13.1  |
| Tenure           | 7861 | 3365                 | 2690  | 69    | 9262  | 6524                  | 3098  | 2718  | 78    | 9262  |
| Age              | 7861 | 43.51                | 10.05 | 21.0  | 62.0  | 6524                  | 43.10 | 10.17 | 21.0  | 62.0  |
| Male             | 7861 | 0.43                 | 0.50  | 0.0   | 1.0   | 6524                  | 43.10 | 0.50  | 0.0   | 1.0   |
| Base salary      | 7861 | 25459                | 1650  | 15080 | 28600 | 6524                  | 25303 | 1752  | 18720 | 28600 |

| <b>CONTROL</b>   |       | <i>Pre-Treatment</i> |       |       |       | <i>Post-Treatment</i> |       |       |      |       |
|------------------|-------|----------------------|-------|-------|-------|-----------------------|-------|-------|------|-------|
| Variable         | Obs   | Mean                 | Sd    | Min   | Max   | Obs                   | Mean  | Sd    | Min  | Max   |
| Tardy            | 23600 | 0.03                 | 0.17  | 0.0   | 1.0   | 18349                 | 0.02  | 0.15  | 0.0  | 1.0   |
| Minutes late     | 23600 | -2.12                | 4.73  | -59.4 | 59    | 18349                 | -1.87 | 5.07  | -59  | 59    |
| Total absences   | 27517 | 0.05                 | 0.22  | 0.0   | 1.0   | 21130                 | 0.05  | 0.22  | 0.0  | 1.0   |
| Single absences  | 27517 | 0.02                 | 0.13  | 0.0   | 1.0   | 21130                 | 0.02  | 0.12  | 0.0  | 1.0   |
| Daily efficiency | 22114 | 121.6                | 34.8  | 55.6  | 218.1 | 5422                  | 119.3 | 37.9  | 55.6 | 218.1 |
| Late             | 23600 | 0.17                 | 0.38  | 0.0   | 1.0   | 18349                 | 0.14  | 0.35  | 0.0  | 1.0   |
| Monthly absences | 27752 | 1.06                 | 1.76  | 0.0   | 15.0  | 21314                 | 1.04  | 1.73  | 0.0  | 12.0  |
| Total hrs worked | 27752 | 8.01                 | 1.38  | 0.0   | 44.0  | 21314                 | 8.32  | 2.00  | 0.0  | 22.2  |
| Tenure           | 27752 | 1718                 | 1919  | 4     | 8566  | 20647                 | 1415  | 1903  | 6    | 8566  |
| Age              | 27752 | 39.33                | 12.88 | 18.0  | 69.0  | 21314                 | 39.18 | 12.59 | 16.0 | 69.0  |
| Male             | 27752 | 0.34                 | 0.47  | 0.0   | 1.0   | 21314                 | 0.31  | 0.46  | 0.0  | 1.0   |
| Base salary      | 27590 | 19827                | 4918  | 8320  | 48526 | 21314                 | 18912 | 4933  | 8320 | 48526 |

**Table 2: Descriptive Statistics at the Worker/Month Level**

| <b>TREATMENT</b>          |     | <i>Pre-Treatment</i> |      |      |       |     | <i>Post-Treatment</i> |      |      |       |  |
|---------------------------|-----|----------------------|------|------|-------|-----|-----------------------|------|------|-------|--|
| Variable                  | Obs | Mean                 | Sd   | Min  | Max   | Obs | Mean                  | Sd   | Min  | Max   |  |
| # tardies per month       | 33  | 1.22                 | 1.51 | 0.0  | 5.3   | 34  | 0.77                  | 1.04 | 0.0  | 3.8   |  |
| # minutes late per month  | 33  | -1.66                | 1.99 | -8.8 | 1.0   | 34  | -2.11                 | 2.07 | -7.1 | 1.2   |  |
| # of times late per month | 33  | 3.71                 | 3.77 | 0.0  | 13.7  | 34  | 2.65                  | 3.42 | 0.0  | 13.1  |  |
| # of absences per month   | 33  | 0.73                 | 0.53 | 0.0  | 1.8   | 34  | 1.00                  | 0.54 | 0.1  | 1.9   |  |
| Average efficiency        | 32  | 122.8                | 22.4 | 89.2 | 167.9 | 33  | 123.1                 | 25.3 | 59.5 | 174.3 |  |

| <b>CONTROL</b>            |     | <i>Pre-Treatment</i> |      |       |       |     | <i>Post-Treatment</i> |      |       |       |  |
|---------------------------|-----|----------------------|------|-------|-------|-----|-----------------------|------|-------|-------|--|
| Variable                  | Obs | Mean                 | Sd   | Min   | Max   | Obs | Mean                  | Sd   | Min   | Max   |  |
| # tardies per month       | 191 | 0.46                 | 0.73 | 0.0   | 5.5   | 164 | 0.43                  | 0.62 | 0.0   | 4.0   |  |
| # minutes late per month  | 191 | -1.84                | 2.06 | -12.4 | 5.4   | 164 | -1.68                 | 2.14 | -10.9 | 8.3   |  |
| # of times late per month | 191 | 2.71                 | 3.32 | 0.0   | 17.2  | 164 | 2.34                  | 2.61 | 0.0   | 15.3  |  |
| # of absences per month   | 191 | 0.90                 | 0.81 | 0.0   | 5.0   | 164 | 0.90                  | 0.82 | 0.0   | 5.7   |  |
| Average efficiency        | 178 | 112.0                | 30.6 | 55.6  | 217.5 | 134 | 113.7                 | 32.4 | 57.6  | 218.1 |  |

**Table 3: Basic Treatment Effect**

|                     | (1)                  | (2)                  | (3)                 | (4)                   |
|---------------------|----------------------|----------------------|---------------------|-----------------------|
| Dependent Variable: | Tardy                | Minutes Late         | Absent              | Efficiency            |
| Treatment           | -0.016***<br>(0.005) | -0.806**<br>(0.322)  | 0.007<br>(0.006)    | -2.731<br>(2.535)     |
| Constant            | 0.018***<br>(0.006)  | -2.075***<br>(0.182) | 0.048***<br>(0.009) | 119.528***<br>(1.414) |
| Time Controls       | Y                    | Y                    | Y                   | Y                     |
| Worker FE           | Y                    | Y                    | Y                   | Y                     |
| R <sup>2</sup>      | 0.004                | 0.005                | 0.004               | 0.007                 |
| # of employees      | 273                  | 273                  | 273                 | 273                   |
| Observations        | 54,993               | 54,988               | 62,995              | 35,575                |

Note: Bootstrapped standard errors in parentheses. Significance levels: \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. The variable "Treatment" is the interaction of the treatment group and post-treatment dummies.

**Table 4: Placebo Tests**

| Variable       | Random Treatment Group (50) |         |          | Random Treatment Group and Date (50) |         |          |
|----------------|-----------------------------|---------|----------|--------------------------------------|---------|----------|
|                | Mean Coefficient            | p < 0.1 | p < 0.05 | Mean Coefficient                     | p < 0.1 | p < 0.05 |
| Tardy          | -0.0006                     | 6%      | 4%       | -0.0003                              | 14%     | 6%       |
| Minutes late   | -0.1182                     | 2%      | 2%       | -0.0969                              | 12%     | 4%       |
| Single absence | 0.0015                      | 6%      | 4%       | 0.0025                               | 12%     | 8%       |

Note: Standard errors in parentheses are clustered at the employee level. Significance \* p<0.10, \*\* p<0.05, \*\*\* p<0.01.

**Table 5: Evidence of Gaming**

|                     | (1)                 | (2)                  | (3)                  | (4)                 | (5)                 | (6)                   |
|---------------------|---------------------|----------------------|----------------------|---------------------|---------------------|-----------------------|
| Dependent Variable: | Single Absence      | Tardy                | Minutes Late         | Absence             | Single Absence      | Efficiency            |
| Treatment           | 0.006***<br>(0.002) |                      |                      |                     |                     |                       |
| Pre-fail treatment  |                     | -0.019***<br>(0.006) | -0.900***<br>(0.336) | 0.007<br>(0.007)    | 0.006***<br>(0.002) | -3.243<br>(2.556)     |
| Post-fail treatment |                     | -0.004<br>(0.012)    | -0.428<br>(0.373)    | 0.005<br>(0.014)    | 0.006<br>(0.004)    | -0.17<br>(3.501)      |
| Constant            | 0.022***<br>(0.004) | 0.019***<br>(0.005)  | -2.063***<br>(0.212) | 0.048***<br>(0.009) | 0.022***<br>(0.004) | 119.557***<br>(2.136) |
| Time controls       | Y                   | Y                    | Y                    | Y                   | Y                   | Y                     |
| Worker FE           | Y                   | Y                    | Y                    | Y                   | Y                   | Y                     |
| R <sup>2</sup>      | 0.003               | 0.004                | 0.005                | 0.004               | 0.003               | 0.007                 |
| # of employees      | 273                 | 273                  | 273                  | 277                 | 277                 | 248                   |
| Observations        | 62,995              | 54,993               | 54,988               | 62,995              | 62,995              | 35,575                |

Note: Bootstrapped standard errors in parentheses based on 400 repetitions. Significance \* p<0.10, \*\* p<0.05, \*\*\* p<0.01. The variable "Treatment" is the interaction of the treatment group and post-treatment dummies.

**Table 6a: Treatment Effect by Pre-Award Level of Tardiness**

|                     | (1)               | (2)                | (3)                 | (4)                  | (5)                  | (6)                  |
|---------------------|-------------------|--------------------|---------------------|----------------------|----------------------|----------------------|
| Dependent Variable: | Tardy             | Tardy              | Tardy               | Minutes Late         | Minutes Late         | Minutes Late         |
| Sample:             | No Tardy          | Low Tardy          | High Tardy          | No Tardy             | Low Tardy            | High Tardy           |
| Treatment           | 0.001<br>(0.004)  | -0.006<br>(0.004)  | -0.020*<br>(0.010)  | -1.482<br>(1.325)    | -1.364***<br>(0.397) | 0.342<br>(0.442)     |
| Constant            | -0.002<br>(0.003) | 0.011**<br>(0.005) | 0.042***<br>(0.014) | -3.607***<br>(0.361) | -2.243***<br>(0.249) | -1.203***<br>(0.311) |
| Time controls       | Y                 | Y                  | Y                   | Y                    | Y                    | Y                    |
| Worker FE           | Y                 | Y                  | Y                   | Y                    | Y                    | Y                    |
| R <sup>2</sup>      | 0.009             | 0.005              | 0.009               | 0.048                | 0.011                | 0.011                |
| # of employees      | 72                | 74                 | 75                  | 72                   | 74                   | 75                   |
| Observations        | 9,401             | 22,832             | 18,736              | 9,401                | 22,832               | 18,731               |

Note: Standard errors presented in parentheses are bootstrapped with 400 repetitions. Significance levels: \* p<0.10, \*\* p<0.05, \*\*\* p<0.01.

**Table 6b: Treatment Effect by Pre-Award Level of Tardiness**

|                     | (1)                 | (2)                 | (3)                 | (4)                | (5)                 | (6)                 |
|---------------------|---------------------|---------------------|---------------------|--------------------|---------------------|---------------------|
| Dependent Variable: | Absence             | Absence             | Absence             | Single Absence     | Single Absence      | Single Absence      |
| Sample:             | No Tardy            | Low Tardy           | High Tardy          | No Tardy           | Low Tardy           | High Tardy          |
| Treatment           | 0.030***<br>(0.011) | -0.001<br>(0.010)   | 0.01<br>(0.010)     | 0.009<br>(0.006)   | 0.006**<br>(0.003)  | 0.006*<br>(0.003)   |
| Constant            | 0.045**<br>(0.018)  | 0.062***<br>(0.015) | 0.041***<br>(0.013) | 0.021**<br>(0.008) | 0.028***<br>(0.007) | 0.019***<br>(0.007) |
| Time controls       | Y                   | Y                   | Y                   | Y                  | Y                   | Y                   |
| Worker FE           | Y                   | Y                   | Y                   | Y                  | Y                   | Y                   |
| R <sup>2</sup>      | 0.012               | 0.005               | 0.006               | 0.006              | 0.004               | 0.005               |
| # of employees      | 75                  | 74                  | 75                  | 75                 | 74                  | 75                  |
| Observations        | 11,031              | 25,847              | 21,539              | 11,031             | 25,847              | 21,539              |

Note: Standard errors presented in parentheses are bootstrapped with 400 repetitions. Significance levels: \* p<0.10, \*\* p<0.05, \*\*\* p<0.01.

**Table 7a: Treatment Effect by Pre-Award Arrival Time**

|                     | (1)                | (2)                  | (3)               | (4)                  | (5)                  | (6)                 |
|---------------------|--------------------|----------------------|-------------------|----------------------|----------------------|---------------------|
| Dependent Variable: | Tardy              | Tardy                | Tardy             | Minutes Late         | Minutes Late         | Minutes Late        |
| Sample:             | Early              | On Time              | Late              | Early                | On Time              | Late                |
| Treatment           | 0.030**<br>(0.013) | -0.019***<br>(0.006) | -0.032<br>(0.025) | 1.293<br>(2.226)     | -0.959***<br>(0.285) | 1.458***<br>(0.429) |
| Constant            | 0.011<br>(0.014)   | 0.014***<br>(0.005)  | 0.05<br>(0.047)   | -3.679***<br>(0.493) | -2.268***<br>(0.198) | 0.781<br>(0.561)    |
| Time controls       | Y                  | Y                    | Y                 | Y                    | Y                    | Y                   |
| Worker FE           | Y                  | Y                    | Y                 | Y                    | Y                    | Y                   |
| R-squared           | 0.015              | 0.004                | 0.026             | 0.019                | 0.007                | 0.021               |
| # of employees      | 26                 | 206                  | 41                | 26                   | 206                  | 41                  |
| Observations        | 4,922              | 44,395               | 5,676             | 4,922                | 44,391               | 5,675               |

Note: Standard errors presented in parentheses are bootstrapped with 400 repetitions.  
Significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table 7b: Treatment Effect by Pre-Award Arrival Time**

|                     | (1)                | (2)                 | (3)              | (4)              | (5)                 | (6)               |
|---------------------|--------------------|---------------------|------------------|------------------|---------------------|-------------------|
| Dependent Variable: | Absence            | Absence             | Absence          | Single Absence   | Single Absence      | Single Absence    |
| Sample:             | Early              | On Time             | Late             | Early            | On Time             | Late              |
| Treatment           | 0.043**<br>(0.019) | 0.002<br>(0.006)    | 0.002<br>(0.029) | 0.002<br>(0.008) | 0.008***<br>(0.002) | -0.000<br>(0.006) |
| Constant            | 0.018<br>(0.012)   | 0.054***<br>(0.010) | 0.029<br>(0.036) | 0.005<br>(0.013) | 0.027***<br>(0.005) | -0.003<br>(0.011) |
| Time controls       | Y                  | Y                   | Y                | Y                | Y                   | Y                 |
| Worker FE           | Y                  | Y                   | Y                | Y                | Y                   | Y                 |
| R-squared           | 0.021              | 0.004               | 0.017            | 0.016            | 0.003               | 0.008             |
| # of employees      | 26                 | 206                 | 45               | 26               | 206                 | 45                |
| Observations        | 5,708              | 50,771              | 6,516            | 5,708            | 50,771              | 6,516             |

Note: Standard errors presented in parentheses are bootstrapped with 400 repetitions.  
Significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table 8: Treatment Effects on Efficiency by Employee Type**

|                     | (1)                   | (2)                   | (3)                  | (4)                   | (5)                   | (6)                   |
|---------------------|-----------------------|-----------------------|----------------------|-----------------------|-----------------------|-----------------------|
| Dependent Variable: | Efficiency            | Efficiency            | Efficiency           | Efficiency            | Efficiency            | Efficiency            |
| Sample:             | High Efficiency       | Average Efficiency    | Low Efficiency       | Notardy               | Lowtardy              | Hightardy             |
| Treatment           | -9.313**<br>(4.255)   | 0.833<br>(3.799)      | -5.791<br>(4.435)    | -7.870**<br>(3.493)   | -2.951<br>(4.090)     | -0.758<br>(4.401)     |
| Constant            | 150.793***<br>(3.803) | 120.079***<br>(1.635) | 96.052***<br>(2.604) | 117.038***<br>(2.176) | 118.742***<br>(2.265) | 122.736***<br>(2.212) |
| Time controls       | Y                     | Y                     | Y                    | Y                     | Y                     | Y                     |
| Worker FE           | Y                     | Y                     | Y                    | Y                     | Y                     | Y                     |
| R <sup>2</sup>      | 0.027                 | 0.008                 | 0.031                | 0.034                 | 0.013                 | 0.014                 |
| # of employees      | 42                    | 102                   | 103                  | 70                    | 70                    | 73                    |
| Observations        | 7,113                 | 19,441                | 8,978                | 6,202                 | 15,478                | 13,182                |

Note: Standard errors presented in parentheses are bootstrapped with 400 repetitions. Significance levels: \* p<0.10, \*\* p<0.05, \*\*\* p<0.01.

**Table 9: Treatment Effects on Efficiency by Employee Type and Eligibility**

|                     | (1)                   | (2)                   | (3)                   |
|---------------------|-----------------------|-----------------------|-----------------------|
| Dependent Variable: | Efficiency            | Efficiency            | Efficiency            |
| Sample:             | High Efficiency       | Average Efficiency    | Low Efficiency        |
| Pre-fail treatment  | -9.017**<br>(4.404)   | -0.142<br>(4.112)     | -5.438<br>(4.579)     |
| Post-fail treatment | -11.366***<br>(3.558) | 4.113<br>(4.740)      | -12.629***<br>(3.979) |
| Constant            | 150.774***<br>(3.804) | 120.134***<br>(1.647) | 96.039***<br>(2.604)  |
| Time controls       | Y                     | Y                     | Y                     |
| Worker FE           | Y                     | Y                     | Y                     |
| R <sup>2</sup>      | 0.027                 | 0.008                 | 0.032                 |
| # of employees      | 42                    | 102                   | 103                   |
| Observations        | 7,113                 | 19,441                | 8,978                 |

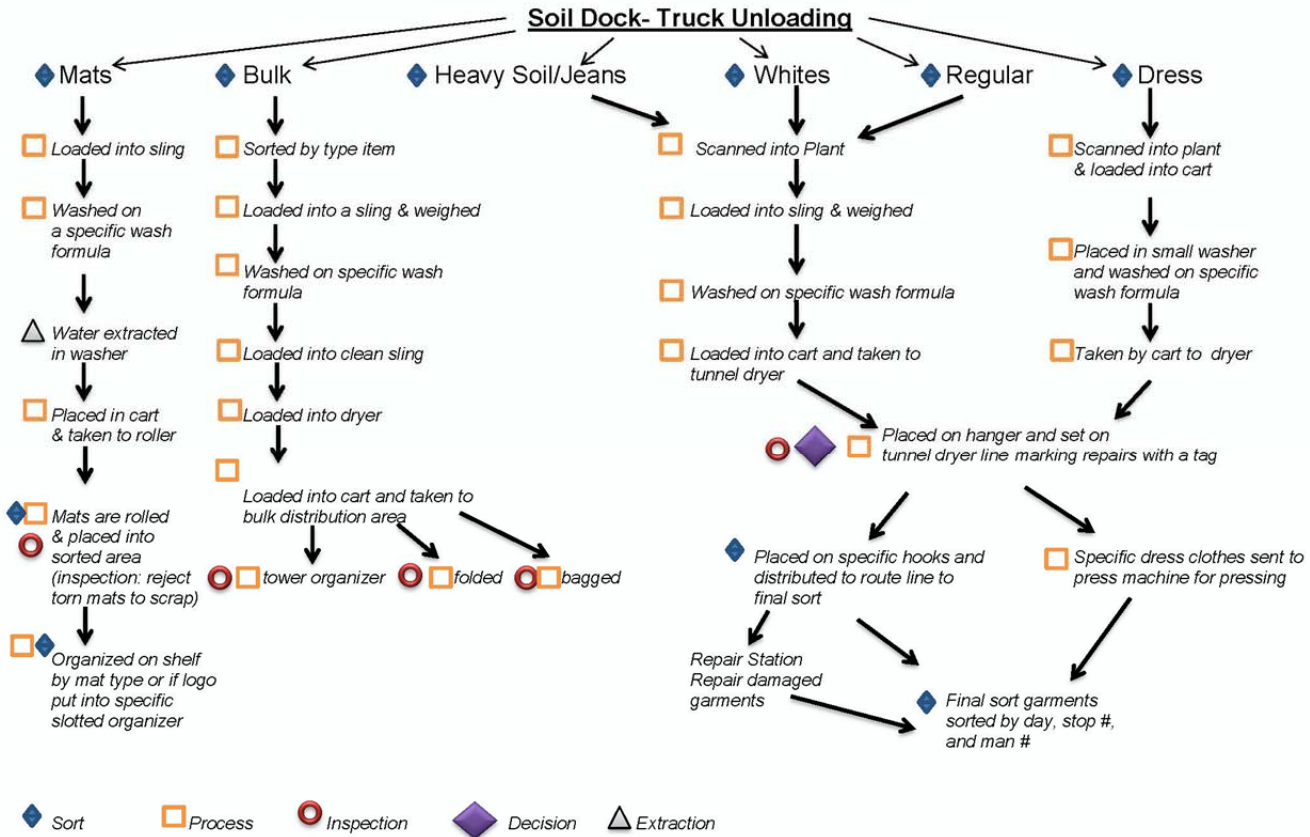
Note: Standard errors presented in parentheses are bootstrapped with 400 repetitions. Significance levels: \* p<0.10, \*\* p<0.05, \*\*\* p<0.01.

Figure 1: Interior of a Plant

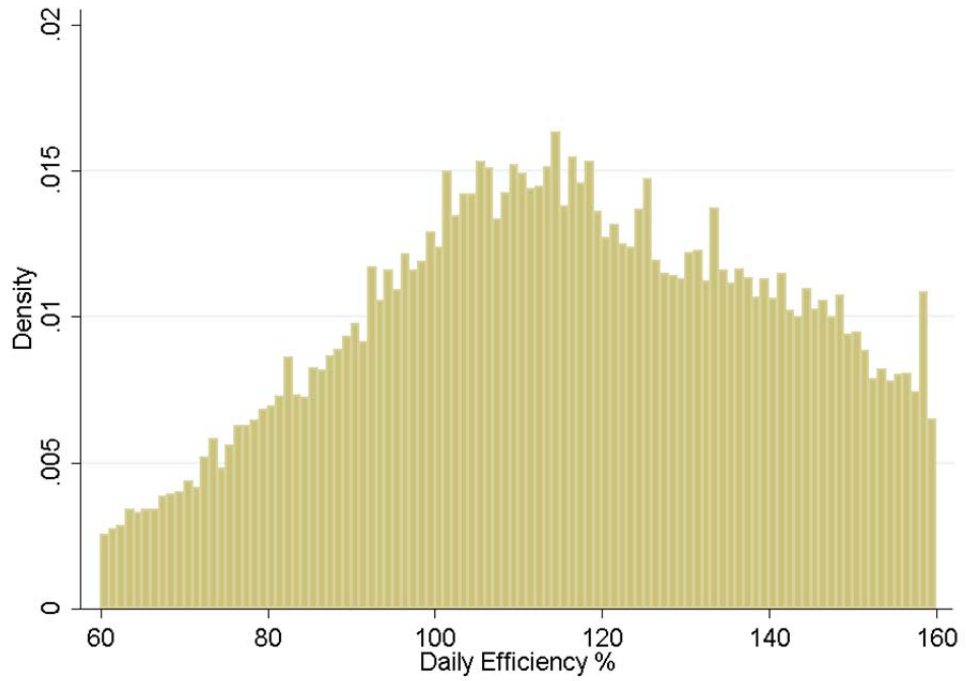




Figure 2: Plant Process Flow Chart



**Figure 3: Histogram of Daily Efficiency for All Workers**



**Figure 4: Award-Eligible Workers by Month**

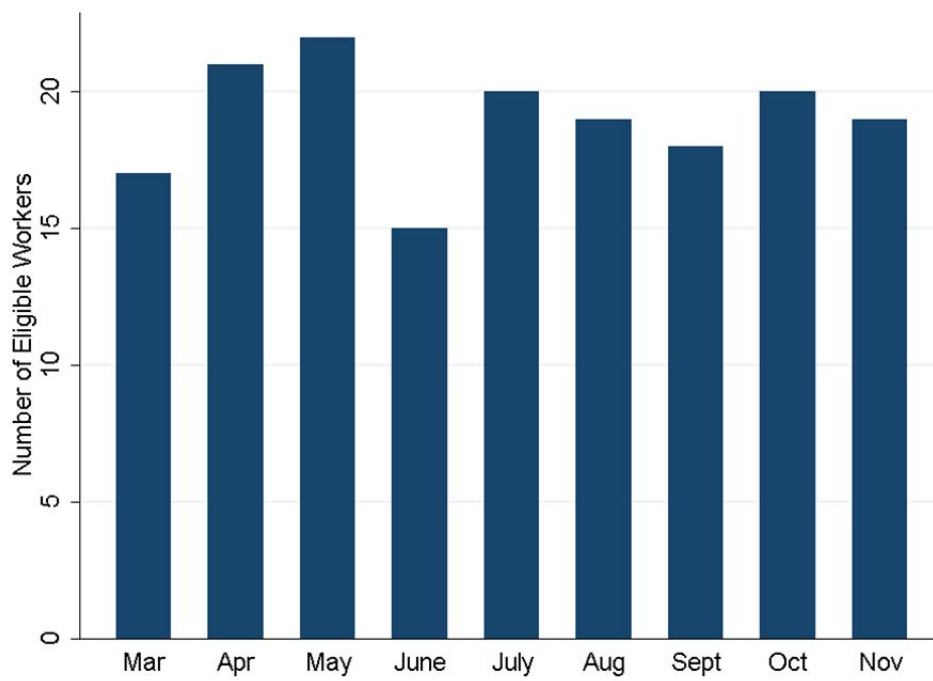


Figure 5: Eligible Months by Worker

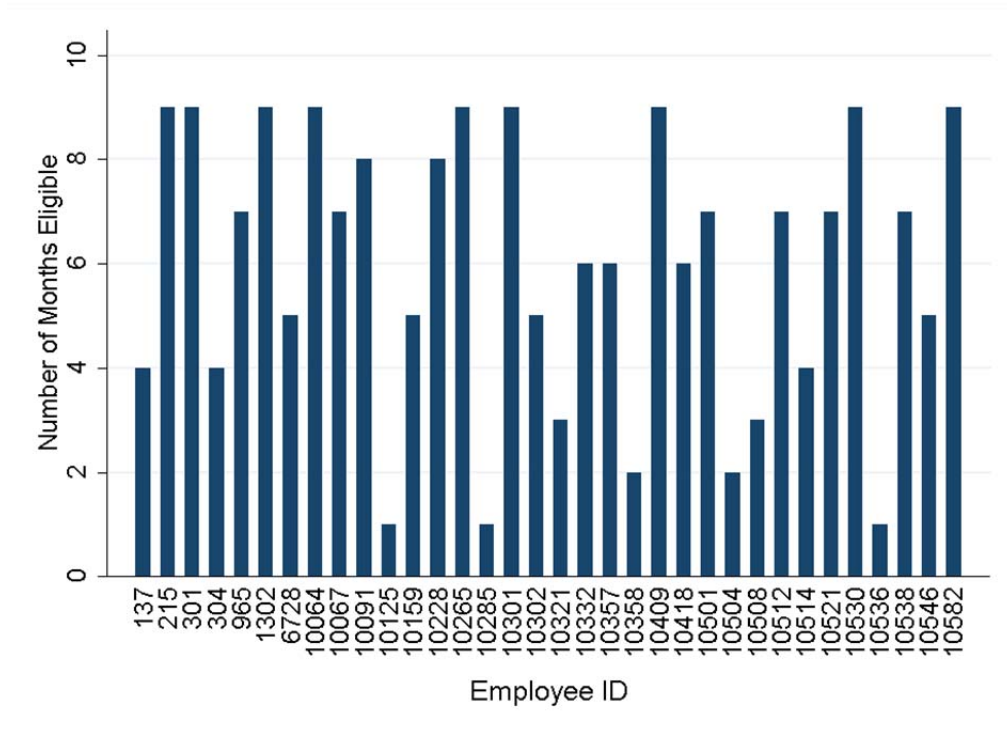


Figure 6: Placebo Regressions for Minutes Late

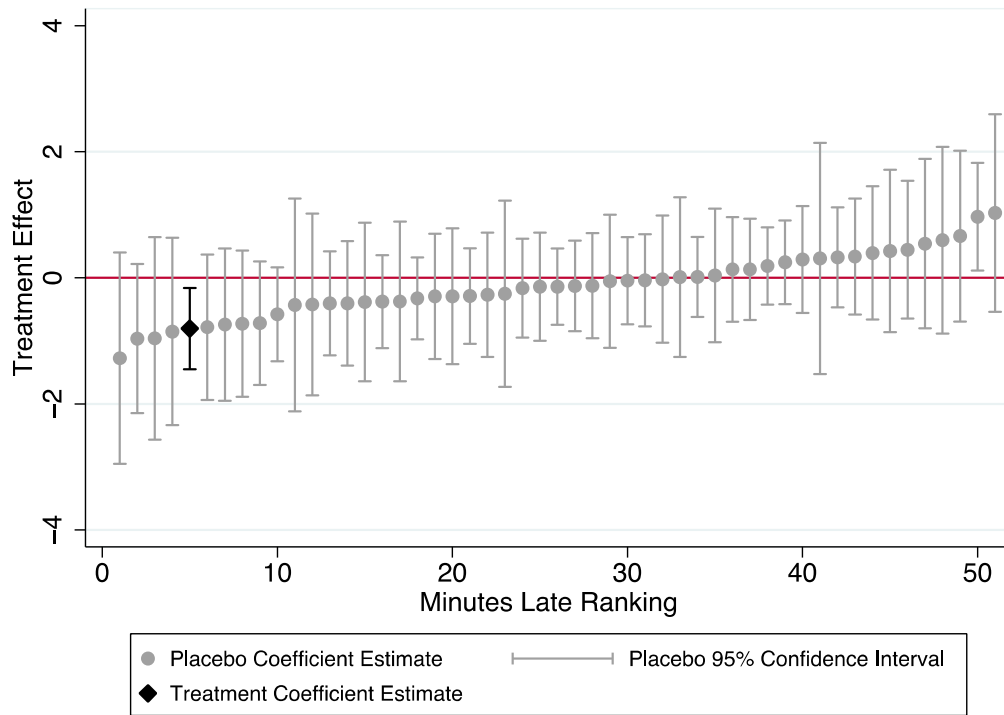


Figure 7: Monthly Effect on Minutes Late

