

# Cleaning House: The Impact of Information Technology Monitoring on Employee Theft and Productivity<sup>\*</sup>

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In this paper, we study how firm investments in technology-based employee monitoring impact both misconduct and productivity. We use unique and detailed theft and sales data from 392 restaurant locations from five different firms that adopt a theft monitoring information technology (IT) product. Since the specific timing of individual locations' technology adoption is plausibly exogenous, we can use difference-in-differences models to estimate the treatment effect of IT monitoring on theft and productivity within each location for all employees. We find significant treatment effects in reduced theft and improved productivity that appear to be driven by changing the behavior of individual workers rather than selection effects. Although workers with past patterns of theft appear more likely to leave treated locations than others, individual behavioral changes by existing workers drive restaurant-level improvements. These findings suggest multi-tasking by employees under a pay-for-performance system, as they increase effort toward sales following monitoring implementation in order to compensate for lost theft income. This suggests that employee misconduct is primarily a result of managerial policies rather than individual differences in ethics or morality.

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# I. Introduction

Employee theft and fraud are widespread problems in firms, with workers stealing roughly \$200 billion in revenue from U.S. firms to supplement their income (Murphy 1993).<sup>1</sup> A growing empirical literature on forensic economics has clarified when and how theft and other misconduct occur (e.g., Jacob and Levitt 2003; Fisman and Wei 2009; Zitzewitz 2012a),<sup>2</sup> but says little about the overall impact of firms' use of forensics to monitor and reduce theft.<sup>3</sup> This is a critical shortfall in the literature, given the substantial investments made by firms in monitoring employees (Dickens et al. 1989), as well as the growing forensic and monitoring capabilities enabled by information technology (IT) systems. This raises two important yet unanswered questions about the economic impact of monitoring employee crime. First, if monitoring is indeed effective in reducing theft, as theory (Becker 1968; Dickens et al. 1989) and some evidence (Nagin et al. 2002) suggests, do these gains primarily result from changing worker behavior or instead from replacing less honest workers with more honest ones? Second, if increased monitoring reduces theft of existing workers, how do they adjust effort on other tasks in response to this lost income, and what is the overall impact on firm productivity?<sup>4</sup> Recent research on corruption suggests that reducing one type of misconduct through monitoring might invoke a multitasking response that increases other corrupt activities that substitute for lost income (Olken 2007; Yang 2008).<sup>5</sup>

In this paper we address these questions by examining the impact of improved theft monitoring from information technology in the American casual dining sector, using a unique dataset that details employee-level theft and sales transactions at 392 restaurants in 38 American states. We focus on this setting for several reasons. First, detailed theft and sales data allow us to identify specific worker-level productivity,

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<sup>1</sup> See Dickens et al. (1989) or Chen and Sandino (2012) for more extensive discussion on the magnitude of employee theft.

<sup>2</sup> The literature on other types of misconduct is vast. There is a large related literature on government corruption in economics. For example, see Fisman and Miguel (2007), Di Tella and Schargrodsky (2003), Bandiera et al. (2009), and Niehaus and Sukhtankar (2013). Also see Duggan and Levitt (2002), Wolfers (2006), and Zitzewitz (2010) for examples from sports. There is a large related research stream on discrimination (e.g. Knowles et al. 2001; Bertrand and Mullainathan 2004) and other misconduct such as tax evasion (Fisman and Wei 2004) and stock-option backdating (Heron and Lie 2007). The vast majority of this research, however, observes this at the firm level. See Zitzewitz (2012a) for a detailed survey of the broader field of forensic economics.

<sup>3</sup> To the best of our knowledge, the one significant exception is Nagin et al. (2002), whose field experiment tests how increased audit risk in call centers reduces the fabricated donation reports of some workers, but does not produce productivity implications based on this treatment.

<sup>4</sup> In addition, theory from behavioral economics suggests that decreases in intrinsic motivation (e.g., Benabou and Tirole 2006) or decreases in trust (Frey 1993) might hinder the gains from monitoring.

<sup>5</sup> In contrast, Duflo and colleagues (2012) find no multitasking response from teachers whose attendance is monitored. Di Tella and Schargrodsky (2004) also find no neighborhood crime substitution following a shock to policing.

theft, and sorting responses to changes in firm monitoring. Second, unlike previous research on monitoring (e.g. Duflo et al. 2012; Zitzewitz 2012b), restaurants provide a firm-based setting where workers receive commission-based pay-for-performance compensation that incentivizes substitution from the monitored task (theft) to the unmonitored and productive one (sales). Third, the increased monitoring in our setting results from the staggered implementation of improved IT monitoring systems across multiple locations. Although the impact of IT on productivity increases in firms is well documented (David 1992; Brynjolfsson 1993; Griliches 1994; Nordhaus 2001; Bharadwaj 2000; Bresnahan et al 2002), no research examines potential productivity gains through reduced theft or other misconduct. Recent work by Bloom, Sadun, and Van Reenen (2012) shows that the productivity gains from IT have been most substantial in industries, such as restaurants, that have “tougher” human resource practices with higher-powered incentives.

We conceptualize the employee theft issue as a stylized multitasking problem (e.g., Holmstrom and Milgrom 1991), where workers under a pay-for-performance scheme (such as tips) can derive earnings from two tasks: sales productivity and theft. Earnings from each task are increasing and concave in effort. The cost of effort from each task is convex and increasing, but theft bears two additional costs. First, the employee will be detected and punished by management (the principal) with some probability  $p$  that is increasing in theft. Second, the employee may suffer moral or ethical costs based on identity or preferences that make theft costly even when it is effortless and unmonitored (e.g. Akerlof and Dickens 1982; Mazar et al. 2009; Bénabou and Tirole 2011; Dal Bó and Terviö 2013).

Such a setup has three immediate implications for the impact of increased IT monitoring on employee effort allocation. First, any employee with existing non-zero theft levels will reduce effort allocated to theft in response to increased monitoring by management. Second, the resulting decrease in earnings will thus motivate them to increase effort allocated toward productivity. Third, employees with existing non-zero theft levels will be more likely to leave the firm as outside employment options become relatively more attractive than before.

We use approximately two years of detailed theft and sales data from 392 restaurant locations from five restaurant firms (hereafter referred to as “chains”) that adopt an IT monitoring product, NCR Corporation’s Restaurant Guard, that reveals theft by specific employees. Restaurant servers (also called waiters) use multiple techniques to steal from their employers and customers, including voiding and “comping” sales after pocketing cash payment from customers, and transferring food items from customers’ bills after they have paid.<sup>6</sup> Restaurant Guard alerts managers to egregious examples of these actions in a

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<sup>6</sup> We will detail these techniques later in the paper.

weekly report.<sup>7</sup> These alerts represent the “tip of the theft iceberg”, since the product is designed to identify instances of theft that are so obvious as to be indefensible by servers. Consequently, while the weekly alerts in our data average only \$108 per location, interviews with managers indicate the losses to be considerably larger.

Our data provide the identity of each server, as well as the revenue, theft alerts, tips, shifts, and food items sold for each day. The data also provide the date on which Restaurant Guard was implemented at each location. The Restaurant Guard product was rolled out to individual store locations in a plausibly piecemeal way not related to individual store needs or theft levels. Rather, the rollout pattern was driven by the schedule and week-to-week geographic location of the vendor’s Restaurant Guard implementation team. This rollout strategy allows us to treat adoption dates as plausibly exogenous to the individual restaurant location and not correlated with revenue or theft levels. Our quasi-experimental setting thus enables us to estimate behavioral and productivity changes within each location across all employees. We use difference-in-differences models to estimate the treatment effect of the monitoring technology on theft, sales productivity, employee turnover, and other performance metrics at both the individual and restaurant level. The different implementation dates for each location allow us to control for time trends and time-invariant location-specific and worker-specific fixed effects.

Our empirical models identify a 22% (or \$24/week) decrease in identifiable theft after the implementation of IT monitoring. This treatment effect is persistent, with the magnitude growing from \$7 in the first month to \$48 in the third month. The treatment effect on total revenue, however, is much larger. Total revenue increases by \$2,975/week (about 7% for the average location) following implementation of Restaurant Guard, suggesting either a considerable increase in employee productivity or a much larger latent theft being eliminated by the IT product. Furthermore, the implementation of Restaurant Guard increases drink sales (the primary source of theft) by \$927/week (about 10.5%). This result is particularly important because the profit margins on drinks in casual dining are between 60 and 90 percent, representing approximately half of all restaurant profits. Furthermore, we observe an increase in average tip levels of 0.3%, which represents one sixth of a standard deviation improvement from a base rate of 14.8%. This result suggests improvement in customer service from IT monitoring.

While these results show considerable impact on theft, revenue, and profitability for the restaurants, they do not explain the mechanisms through which these improvements are gained. To disentangle these mechanisms, we examine the impact of the IT product on individual employee outcomes. We employ a similar difference-in-differences approach, alternatively including worker and restaurant fixed effects to

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<sup>7</sup> The system currently also sends real-time text alerts to managers, but this feature was implemented after our data period.

examine whether our results are due to behavioral changes in existing workers or selection effects as the worst workers leave the restaurants (e.g. Lazear 2000; Hamilton et al. 2003).

These individual worker models show that Restaurant Guard reduces average hourly theft by between \$0.05 and \$0.06 in both models. This suggests that all the decrease in theft found in our restaurant-level models can be explained by employees changing their behavior, as opposed to a change in the group of employees working at the restaurant. We also find that IT monitoring also increases hourly sales by \$2.02 for existing workers, with similar increases for drink sales and tip percentage. In each case, the worker fixed models suggest behavioral changes by workers rather than a selection effect. Given the pay-for-performance compensation policy of our restaurants, these results are consistent with multi-tasking and principal-agent models of worker behavior (Alchian and Demsetz 1972; Holmstrom and Milgrom 1991). When a worker's ability to gain money from theft is reduced due to increased monitoring, he or she reallocates effort toward increasing sales and customer service in order to regain some of that loss.

Finally, our models shed light both on how management responds to the new theft information and on workers' endogenous choices to leave the firm. To do so, we separate workers into "known thieves" and "unknown" groups based on their observed (by the researchers, not by the managers) pre-treatment theft. *Known thieves* are those with observable pre-treatment theft. Cox hazard models show employees with known pre-treatment theft levels have higher attrition rates than do employees without observable theft. The observation that these exits are unlikely to happen within two weeks of a theft report to management suggests that this attrition is voluntary and not due to termination following theft revelation to management. This voluntary attrition by thieves following increased monitoring is consistent with workers selecting out of jobs after monitoring limits theft income. The apparent rarity of termination also echoes Dickens et al.'s (1989) observation that firms infrequently employ the efficient low-monitoring, high-punishment crime deterrence strategy described in Becker (1968). We also observe that while *known thieves*' weekly hours remain unchanged following the IT implementation, other workers' weekly hours increase on average by 2.25 hours, which is consistent managers reallocating the hours toward more honest workers.

This paper has implications for several important research streams. First, we contribute to the literatures on forensic economics and corruption. Only a few studies focus on explicitly illegal behavior by employees of private firms, and those that do almost exclusively rely on empirical evidence aggregated at the firm level (Fisman and Wei 2004; 2009; Zitzewitz 2006; Heron and Lie 2007; DellaVigna and La Ferrara 2010; Chen and Sandino 2012; Pierce and Snyder 2012).<sup>8</sup> Our worker-level data, like Nagin et al.'s (2002)

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<sup>8</sup>A few notable studies of illegal behavior by individual workers exist in the psychology and management literatures (e.g. Greenberg 1990; Pierce and Snyder 2008; Gino and Pierce 2010). A growing literature in finance also examines potentially illegal insider information sharing at the individual analyst level (Cohen et al. 2010).

study of call center fraud, allow us to disentangle firm-level misconduct from individual-level decisions that run counter to firm profitability. Unlike their work, however, our multi-firm longitudinal data allow us to more comprehensively examine the impact of monitoring on selection and treatment across multiple tasks, including productivity.<sup>9</sup> Our results show that employee productivity and misconduct are linked through organizational policies such as compensation or information technology monitoring. This unique finding is particularly important because it has roots in foundational models of compensation that allow for both productivity and sabotage (Lazear 1989).<sup>10</sup>

Our results also contribute to work in personnel and organizational economics on employee response to compensation systems. While theory modeling counterproductive employee behavior is extensive (Alchian and Demsetz; Jensen and Meckling 1976; Holmstrom 1979; Lazear and Rosen 1981; Holmstrom and Milgrom 1991), only recently has empirical work examined incentives impact explicitly illegal behavior in firms. A growing literature on bonus gaming examines employees' strategic responses to incentive systems (e.g. Oyer 1998), but these behaviors are not clearly corrupt or illegal. The fundamental difference between counter-productive and explicitly illegal behaviors goes beyond standard principal-agent and multi-tasking models because effort allocated toward illegal behaviors not only indirectly hurts the firm through foregone production, but also directly hurts the firm through such costs as stolen revenue and legal liability. Furthermore, our study suggests that the effort that workers allocate toward corrupt or illegal behavior can be redirected toward more productive behavior through incentives. Interventions can simultaneously reduce theft and improve productivity, a result that to the best of our knowledge has not been observed in the field.

Finally, we contribute to the literature showing the impact of technology on productivity (Brynjolfsson 1993; Brynjolfsson and Hitt 1996; David 1992; Griliches 1994; Athey and Stern 2000; Nordhaus 2001). One of the key findings from this research has been the impact of IT on labor productivity growth (Jorgenson and Stiroh 2000; Oliner and Sichel 2000). While other studies show that IT can also improve productivity by reducing mild forms of misconduct such as shirking and absenteeism (Hubbard 2000; Baker and Hubbard 2003; Duflo et al. 2012), our paper is the first to show both the direct impact in reducing explicitly illegal behavior such as theft as well as the secondary effect of incentivizing increased productivity. Furthermore, our paper supports the view that the impact of IT systems is intimately tied to other elements of firm policy such as asset ownership (Baker and Hubbard 2003; Rawley and Simcoe 2013), human resource policy (Bloom et al. 2012), and other organizational practices such as products and services

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<sup>9</sup> The key advantage of their study is the true experimental design.

<sup>10</sup> Our results also suggest that "corrupt" employees can be remediated through managerial intervention. Consistent with widespread evidence from behavioral ethics (e.g. Mazar et al. 2008; Shu et al. 2011), theft appears to be the work of many individuals stealing relatively small amounts rather than a few "bad apples" who can be eliminated to remove the problem.

(Bresnahan et al. 2002). The impact of IT monitoring on sales and customer service increases in our setting is likely dependent on the tip-based compensation system that incentivizes wait staff to increase productivity after theft is constrained by monitoring.

## II. Field Setting

The context of our study is the “casual dining” segment of the United States restaurant industry. The casual dining segment is situated between the fast food and fine dining segments and is characterized by table service, no meal courses, and mid-range prices. Examples—not necessarily in our sample—include restaurant chains like Applebee’s, Chili’s, and The Olive Garden. This segment is economically significant, generating about \$33 Billion of the annual revenue total of \$110 Billion in the American restaurant industry. Profit margins in the casual dining segment are thin, averaging 3.5% in 2010 (Sweeney and Steinhauser 2010). Much of this profit comes from sales of both alcoholic and non-alcoholic beverages, which have margins of 60 to 90%.

Almost all casual dining restaurants employ point of service (POS) systems that track orders, sales, and server<sup>11</sup> assignments. When servers receive food and beverage order from a customer or table (a “ticket”), they enter it into a touchscreen panel, which then transfers the orders to the kitchen as well as to the POS database. After customers have paid and left the restaurant, the server closes out the ticket. Servers typically have multiple tickets open simultaneously. All the restaurants in our sample use the basic NCR POS product in each week for which we have data.

Important for our research purposes, the compensation model for service staff at nearly all American casual dining restaurants combines fixed wages with variable pay-for-performance. In the US, most servers in the casual dining segment are paid a fixed wage at or below the legal minimum wage. This is legally permissible so long as the legal minimum wage is exceeded when adding a variable pay-for-performance component from customer tips. Social norms in the United States strongly suggest a minimum tip of 15% of the total check, with a lower percentage usually reserved for poor service or an unpleasant dining experience. Customers often increase the tip percentage to reward particularly good service. Servers must also distribute a portion of their tips to the support staff, which include bussers, bar staff, hosts/hostesses, and others.<sup>12</sup> Since tip percentage is at the discretion of the customer, servers can therefore increase their income through both increased sales revenue (bill size) and customer service (tip percentage). Servers can increase revenue by

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<sup>11</sup> Wait staff include waiters, servers, and bartenders. We refer to these generically in this paper as “servers.”

<sup>12</sup> Tip data are based on credit card sales only, since cash tips are unobservable in our data. This is important because credit card tips are difficult to lie about, while cash tips can be misreported.

increasing effort toward selling additional drinks, desserts, or add-ons.<sup>13</sup> Even though tipping behavior is relatively standardized in American culture, even simple efforts toward customer service can significantly raise percentages. Past studies have found substantial increases in tipping due to touching customers (Crusco and Wetzel 1984), writing “thank you” on the check (Rind and Bordia 2006), and increased general service quality (Bodvarsson et al. 2003).

Although theft by servers and other restaurant workers is a constant problem, to the best of our knowledge there exist no studies on theft or other misconduct in restaurants.<sup>14</sup> Some of these losses are due to inventory shrinkage, when employees steal food and drinks for personal outside use. Other shrinkage losses are due to on-the-job consumption of alcohol and food items. Perhaps the largest problem stems from servers stealing cash sales by either not reporting the sale or by using one of a number of techniques to remove it from restaurants’ IT systems. Although there are many ways in which restaurant employees steal from their employer, we focus on the six types detected by our data provider. These “scams” are well-known in the industry, even having nicknames and books written about them (Francis and DeGlinkta 2004). The most common type of server theft in our data is called the Wagon Wheel Scam. In this scam, after a customer pays for a food or drink item, the server transfers that item in the POS system to another newly seated guest that has ordered the same item. The original check is then reprinted after the customer leaves and the waiter pockets the difference.<sup>15</sup> The other scams involve one of two techniques. The first involves “comping”, or refunding meals of customers in the system, after they have already paid but before the ticket has been closed. The second involves “voiding” a transaction as erroneous, after charging a customer for the food or beverage item.

Given their pay-for-performance compensation system and their opportunities for theft-based income, servers face a special type of multi-tasking problem of allocating effort or attention toward two classes of tasks: productive (i.e. sales and customer service) and corrupt tasks (i.e. theft). Servers can earn income through both types of tasks, but restaurant management benefits only from servers’ productive tasks, instead losing direct income from theft. If management alters the relative costs of engaging in the two activities by increasing the monitoring of theft, then effort displaced away from the now more costly theft task to the productive task with unchanged incentives. The manager thus faces the challenge of trying to redirect server effort from corrupt to productive behaviors through employee selection and monitoring. So long as the

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<sup>13</sup> Examples of add-ons include side salads, bacon on a hamburger, or chicken on a Caesar salad.

<sup>14</sup> Jin and Leslie (2003; 2009) are one possible exception, finding that mandatory hygiene disclosure policies improved food safety and reduced food-borne illness, although there is no evidence in their studies of intentional wrongdoing by employees.

<sup>15</sup> This scam’s nickname comes from the pattern that occurs when an item is transferred multiple times to and from the cash register terminal, ultimately resembling wagon wheel spokes.



manager can credibly punish workers when theft is detected, increased monitoring through information technology should motivate workers to reduce theft and increase productivity through sales and customer service.

The data in our study come from the Hospitality division of NCR Corporation, which provides point of sale (POS) IT systems to restaurants and other hospitality providers. NCR provides an add-on product to its core POS system, NCR Restaurant Guard, that utilizes proprietary algorithms to detect theft and fraud by restaurant workers. Restaurant Guard reports incidences of theft to restaurant managers. During our data period, this reporting occurred through weekly reports sent to managers.<sup>16</sup> The algorithms are constructed with a strong bias toward false negatives rather than false positives because of the high cost of falsely flagging an employee. In operation, this means that the IT system must observe significant and large occurrences of these transfers before signaling the likely presence of theft. Local restaurant managers seem to understand the mechanics of these scams. All of the local restaurant managers from our sample that we interviewed had intimate knowledge of the techniques through which servers stole money, having previously worked as servers.

During the sample period, all of the restaurant locations in our data sample implemented the Restaurant Guard product, allowing us to estimate its impact on theft and worker productivity. Each restaurant chain in the sample rolled out the Restaurant Guard product to its store locations in a piecemeal way that, according to our interviews with managers at the IT provider, was not related to individual store needs or theft levels. Rather, the implementation strategy was driven by the schedule of the Restaurant Guard vendor rollout team. This rollout strategy allows us to treat adoption dates as plausibly exogenous and not driven by revenue or theft levels.

Interviews with the data provider and local restaurant managers detail the impact of the IT monitoring product on local restaurants. While all managers were able to observe clear instances of theft provided in weekly Restaurant Guard reports, the use of this information was not uniform or immediately obvious. While a few managers indicated they fired identified thieves, most were reluctant to, since training new servers is time intensive and expensive, and the magnitude of the identified theft would require replacing a large percentage of their workers. Similarly, some managers indicated that directly confronting detected thieves was difficult because accusations of theft can generate resentment and potentially lower productivity. Since managers often have worked as servers, the revelation of specific theft instances is not surprising to them. Consequently, most managers appear to have either leaked information on the new product to the staff

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<sup>16</sup> The system has since been updated to send both weekly reports and real-time text alerts to managers.

through non-confrontational discussions with servers, or else made announcements of the new product to pre-emptively reduce theft.

### III. Identification Strategy

#### 3.1 Data

The data for this study were provided directly by the IT vendor NCR. Since NCR provides both basic point-of-sales (POS) software services as well as the Restaurant Guard add-on, the data contain all transactions at each restaurant for up to two years.<sup>17</sup> Our sample includes five chains with a total of 392 restaurant locations in 38 American states between March 2010 and February 2012. Figure 1 presents a map of these locations.<sup>18</sup> The data include detailed information on each food and beverage items sold, prices, tips, and customers at each table. All transactions are time stamped and identify the specific server associated with the check. There are 30,114 unique workers in our data who average 22 work hours per week. Of these, 22,150 work exclusively as either servers or bartenders. Our panel is unbalanced in the sense that each restaurant location does not appear in the same week, due to a combination of different POS system adoption dates and the inconsistent deletion of older records by the data provider.<sup>19</sup> Figure 2 presents the number of locations for each week in our data.

These transaction data are combined with weekly theft data from the Restaurant Guard product. The Restaurant Guard vendor agreed to apply its theft algorithms retroactively to the entire sample, regardless of when Restaurant Guard was implemented at restaurant locations. This allows us as researchers to observe likely thefts during the entire sample period. Managers were only made privy to contemporaneous theft alerts after implementation, and they were unable to see pre-implementation theft. These alerts provided managers with weekly server-specific reports on the six categories of theft: transfers (i.e. “the wagon wheel”) and five types of sales voids and comps. An example of such a report is provided in Figure 3. We sum these six categories to generate a measure of total weekly theft: *total losses*. As we discussed earlier, these losses represent the total dollar amount of suspected theft reported to the manager, which is likely the small portion of the actual total losses that the system can definitively prove. We aggregate the POS data to the weekly level in order to combine it with the weekly Restaurant Guard theft data.

We use two such combined datasets in our analysis. The first is a weekly location-level dataset that details total check revenue, drink revenue, credit card tips, and total losses for each restaurant. The second

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<sup>17</sup> NCR deletes older data due to storage limitations. Our data, which represent a small sample of their customer list, constitute over a terabyte of raw transaction data.

<sup>18</sup> We do not differentiate chains on the map to protect the anonymity of the restaurants.

<sup>19</sup> All models use location or individual worker fixed effects as well as week dummies, which reduces concerns about the unbalanced panel. Our models are also robust to truncating the data on both the left and right side at different time points. We present several of these models in appendix Table A4.

dataset is a weekly server-level dataset with the same measures, plus an additional measure of total hours worked by the server. We restrict this dataset to the 22,150 workers whose job description in the dataset is designated as server, waiter, or bartender.<sup>20</sup> We calculate *tip percentage* as the weekly sum of credit card tips divided by the sum of credit card sales revenue. We are unable to observe tips for cash sales, so we must assume that any changes in credit card tips parallel changes in cash tips. The data provider also gave us the precise date of implementation at each restaurant. We designated the first full week after implementation (Sunday-Saturday) as the first treatment week.<sup>21</sup> One advantage of our data is that the treatment dates are dispersed throughout the time period, even within chain, which will allow us to separate week-specific shocks and time-trends from the treatment effect of Restaurant Guard. We present these implementation dates in Figure 4.

The descriptive statistics for our sample are presented in Table 1 for both the weekly location and weekly server datasets. There are 22,329 location-week observations and 438,679 server-week observations of servers.<sup>22</sup> The average weekly total losses for each location is \$108.24, which emphasizes how little of the widespread theft detailed in interviews and anecdotal accounts is reported by Restaurant Guard. Average weekly restaurant total check revenue is \$43,697, with \$8,879 coming from drink sales. Tip percentage averages 14.8%. Each restaurant has approximately 15.5 servers working in any given week. The weekly worker dataset shows comparable statistics, but also represents a much wider variance across workers, particularly in theft and tip percentage.

Table 2 presents the mean values separated by the five restaurant chains. Chains 1, 2, and 4 are remarkably similar in size, theft, and revenue, while Chains 3 and 5 are much smaller with less theft. Some chains on average adopt Restaurant Guard later than others, which is why it is critical for our empirical models to include individual restaurant fixed effects to account for restaurant heterogeneity and week dummies to account for time trends. For example, the vast majority of observations for Chains 2 and 4 are post-treatment. The restaurants in the five chains are almost exclusively corporate owned. Only one of the chains offers franchises, and does so for a limited number of locations.

### 3.2 Identification Strategy

We use a difference-in-differences (DiD) design to estimate the impact of the implementation of

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<sup>20</sup> This primarily excludes workers who are assigned to takeout orders, hosts/hostesses, managers, and catering. Including these workers in our models does not change the majority of our results, with the exception of tip percentage, where it generates considerable noise due to the infrequency with which these excluded workers receive tips.

<sup>21</sup> Our results are robust to which week we designate as the first “treatment” week (see Figure 11). Regardless of this choice, the first weeks tend to be noisy due to unobservable variation in when managers view and act on the first report.

<sup>22</sup> There are only 20,912 restaurant-week and 437,860 server-week observations for tip percentage, since one of the five chains does not report this.

Restaurant Guard on theft, total revenue, drink revenue, and customer service (as measured by tip percentage) at the location and server levels. Our DiD design treats the Restaurant Guard adoption as the treatment on each location, using the pre-adoption locations as control groups. The DiD design “differences out” differences not related to the passage of time in the locations and uses control groups to provide a counterfactual for what would have happened at the treatment location had they not adopted Restaurant Guard. The DiD statistical approach is widely used by social scientists to examine the impact of policy changes (Gertler et al. 2011).

Although DiD designs do not require that the control and treatment locations in any week be identical, ours are similar given the relative uniformity of restaurants within a given chain. Similarly, our locations in our data adopt Restaurant Guard at different times for what appear to be non-strategic reasons. This allows us to account for time trends, since treatment is not perfectly correlated with a specific time treatment (as in many DiD models). Furthermore, Restaurant Guard adoption does not appear to be strongly correlated with pre-adoption weekly theft or revenue levels. Figure 5 provides the weekly pre-treatment loss levels for each restaurant plotted against its adoption date. Linear regression without controls yields a slight negative relationship of -0.013. Figure 6 shows a similarly weak relationship between adoption date and revenue. The data provider explained that the chains roll the product out based on the schedule of the IT provider’s implementation team.

We use a standard DiD specification to first estimate the impact of Restaurant Guard adoption on the dependent variables in the study for each location:

$$Y_{it} = \alpha_i + \beta_1 * TREATED_t + \Upsilon_t + \varepsilon_{it}$$

where  $Y_{it}$  is the dependent variable for restaurant  $i$  in week  $t$ ,  $\alpha_i$  is the set of restaurant fixed effects to account for unobserved location heterogeneity,  $\Upsilon_t$  is a set of week fixed effects to control for time trends, and  $TREATED$  is a dummy variable equal to 1 for all weeks  $t$  at treated restaurant  $i$  that are after the Restaurant Guard adoption week. Our specification exploits the panel nature of the data by introducing a full set of fixed effects. Each of our dependent variables is a continuous variable. Consequently, we estimate all regressions using Ordinary Least Squares (OLS) regressions, clustering our standard errors at the location level.<sup>23</sup>

Our second specification estimates treatment effects at the individual worker level using a nearly identical DiD approach on our weekly worker dataset:

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<sup>23</sup> Per Bertrand et al. (2004), we alternatively calculate standard errors by bootstrapping using restaurant and server blocks, with 400 iterations. This results in minor improvements in the standard errors, which are presented in columns 1-4 in Appendix Table A1

$$Y_{ijt} = \alpha_i + \beta_2 * TREATED_{jt} + \gamma_t + \epsilon_{ijt}$$

where  $Y_{ijt}$  is the dependent variable for worker  $j$  at restaurant  $i$  in week  $t$ . Our base specification includes restaurant fixed effects ( $\alpha_i$ ), since we cannot observe any servers switching restaurants. We will also implement specifications with worker fixed effects ( $\alpha_j$ ) to estimate how much of the impact on restaurants is due to selection versus worker treatment.  $TREATED_{jt}$  is a time varying dummy equal to 1 for all weeks  $t$  following the treatment date of restaurant  $i$ . We divide check revenue, losses, and drink revenue by the total hours worked during the week, in order to control for heterogeneity in time worked. This transforms these three measures into weekly averages of hourly productivity and theft. We again cluster standard errors at the restaurant level.

## IV. Results

### 4.1 Restaurant Treatment Effects

Table 3 presents the regression results for the basic treatment effects of Restaurant Guard on individual restaurants. The models include restaurant fixed effects, which account for time invariant restaurant heterogeneity, as well as weekly fixed effects to control for flexible time trends. Column 1 shows that the adoption of Restaurant Guard reduced weekly total theft losses by over \$23, or approximately 22% of the pre-period average. Again, we note that this represents the proverbial “tip of the iceberg” of losses that can be definitively proven. The impact of IT monitoring on total check revenue, presented in column 2, is much larger, accounting for an increase of \$2,975 or approximately 7% of average pre-treatment revenue. A similarly large impact on drink revenue is observed in column 3, with sales rising 10.5% by \$927. This result is important because drink revenues represent almost pure profits to the restaurants. If operating margins at these restaurants are near the 3.5% industry segment average, such an increase in drink revenues would represent at least a 36% increase in operating margins by itself.<sup>24</sup> We also observe a small increase in tip percentage of 0.3%, which represents one sixth of a standard deviation.<sup>25</sup> These improvements occur despite no identifiable increase in the average length of time customers spend occupying tables (see Appendix Table A3).

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<sup>24</sup> This increase is calculated based on conservative 60% margins on \$927, or \$556.2 in profit increases. Assuming the 3.5% segment average of operating margins (or \$1529.40 per week), this represents at least a 36% increase in profits solely from drink sales.

<sup>25</sup> Table A2 in the appendix presents the loss, revenue, and drink revenue models using logged values. Results are highly consistent.

Figure 7 represents the impact of Restaurant Guard on total revenue for 28 locations in one of the chains (Chain 1 from Table 2).<sup>26</sup> The figure represents the average weekly revenue as linear spline functions of weeks until treatment, with 95% confidence intervals. The vertical line represents four separate treatment dates within the chain. Figures A1-A3 in the Appendix show the impact on total losses, drink revenue, and tip percentage. The impact of IT monitoring on all outcomes is clear from the figures, even for a small sample of the population, and also suggests a persistent effect, which we examine next.

#### 4.2 Persistence of Restaurant Treatment Effects

One concern in our initial specification might be that we observe a Hawthorne Effect, where the implementation of a new policy has a short-lived impact due to elevated managerial attention or organizational change (French 1950).<sup>27</sup> To examine this, we adjust our DiD specification to allow for separate treatment effect magnitudes in each of the first three months following Restaurant Guard adoption. We present these results in Table 4. While we observe changes in the magnitude of treatment effects across time, we see no evidence that our results are short-lived or consistent with a Hawthorne effect. Treatment effects are relatively consistent across time, although they increase in magnitude for losses and tip percentage and decrease for check revenue. We note that one must be careful in interpreting these changes, since we cannot observe some restaurants in their third treatment month (see Figures 2 and 3). Consequently, these coefficients represent a different treatment group, and changes in magnitude may reflect heterogeneity across restaurants in the magnitude of Restaurant Guard's impact.

#### 4.3 Robustness Checks

Our DiD models rely on several identifying assumptions, which we address with additional empirical tests. First, we address the concern that nearly all of our restaurants are treated near the end of our sample, yielding few control observations for later observations. While our use of location and week fixed effects reduce concerns that this might bias our estimates, we alternatively truncate our data at different end and start points to test for consistency in our estimated treatment effect. Given that the last week in our sample is 2711, we rerun our models in Table 3 for samples truncated in weeks 2691 and 2681. Each of these samples produces statistically significant and directionally consistent estimates with our main model, with most estimated treatment effects actually being larger. Columns 1-4 in Table A4 in the Appendix present our four main models for the 2681 sample, which are very similar to our primary results. Similarly we left-truncate our model at weeks 2650 and 2660, and find similar results. Results for the 2660 sample are presented in columns

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<sup>26</sup>We present this chain because it has a consistent sample of restaurants with long pre- and post-treatment periods.

<sup>27</sup> See Levitt and List (2011) for a discussion and critique of the original Hawthorne research.

5-8 of Table A4.

Second, we address the assumption that trends among treated and untreated restaurants would be identical in the absence of treatment. To do so, we repeat our models in Table 3 with separate weekly time trends for treated and untreated restaurants. These results, presented in columns 1-4 of Table A5 in the Appendix, are also consistent with our main models, suggesting that our assumption of common trends is reasonable.

Third, we further address concerns of endogenous treatment dates. Although Figures 5 and 6 suggest that treatment dates are not strongly correlated with pre-treatment theft or revenue, it cannot dispel reverse causality concerns that increases in local theft motivate IT adoption (Granger 1969; Ashenfelter and Card 1985).<sup>28</sup> We follow the approach of Granger (1969) and Autor (2003) by including both lags and leads that indicate the twenty weeks before and twenty weeks after the treatment date. We present the coefficients and 95% confidence intervals for these dummy variables in Figure 8. While there is an imprecise increase two weeks before implementation, this is largely due to one outlier restaurant that suffered losses of \$3,273 and \$5,470 in consecutive weeks. Given that IT implementation must be scheduled at least four weeks ahead of time, we are not concerned that this might yield reverse causality. The rest of the figure is consistent with IT causing reduced theft.

Finally, given the frequency of false positives in DiD models (Bertrand et al. 2004), we implement placebo tests to demonstrate our estimated treatment effects are not purely artifacts of our data structure. To do so, we randomly assign actual treatment dates from our data to each location, then repeat our primary models for each of the four dependent variables from Table 3. We run 60 placebo models for each dependent variable. Figure 9 presents the treatment estimates and 95% confidence intervals for each placebo and for the true data (in bold) for the total revenue model. The placebo tests for the other models are provided in Tables A4-A6 in the Appendix). At most, one or two placebo models produce coefficients significant at the 5% level for any given dependent variable, and all are considerably smaller and more weakly identified than our true data parameter estimates. This significantly reduces concerns that our results are due to the nature of our data structure.

#### **4.4 Worker Treatment Effects**

While our weekly restaurant DiD models provide clear treatment effects on restaurant-level revenue and losses, they shed little light on the internal organizational mechanisms through which these improvements are achieved. The adoption of IT monitoring is clearly reducing theft and increasing revenue and profits, but

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<sup>28</sup> For a comprehensive discussion of causality issues and pre-program dips, see Heckman and Smith (1999).

through which mechanisms is it doing so? One possibility is that management is using Restaurant Guard to identify and replace dishonest servers, which would represent a selection effect in restaurant improvements. The alternative explanation is that Restaurant Guard is “treating” existing workers, changing their incentives and behavior by monitoring.

To separate these worker selection and treatment explanations, we run pairs of regressions for each of our four dependent variables: hourly revenue, hourly drink revenue, hourly losses, and tip percentage. Each pair consists of one regression that includes restaurant fixed effects and one regression with worker fixed effects. This approach identifies selection effects as the coefficient in the facility FE model minus the coefficient from the individual FE model (Lazear 2000; Hamilton et al. 2003). Since the individual FE model only estimates the treatment effect on workers who span the treatment date, any productivity change due to changes in the pool of workers should be differenced out in the individual fixed effects. In addition to week fixed effects, we add controls for shift allocation. *Highday* indicates the percentage of shifts allocated to higher traffic days of Thursday, Friday, or Saturday. *Daypart* indicates the percentage of shifts that are dinner shifts, which also have higher traffic and revenue.

We present these models in Table 5 with standard errors clustered at the restaurant level. Columns (1) and (2) show nearly identical treatment effects on losses of between \$0.05 and \$0.06 per hour, which suggests that theft reductions at the restaurant level are due to existing employees changing their behavior rather than better employees replacing thieves. Because 86.7% of all server-weeks involve *total losses* of 0, we present linear probability models of likelihood of any theft in columns (3) and (4), regressing a dummy representing any losses with worker or restaurant fixed effects.<sup>29</sup> These models show nearly identical and substantial 1% to 2% decreases in the likelihood of having any total losses in both models, against a base rate of 13.3%.

The effects on hourly total revenue in columns (5) and (6) and drink revenue in columns (7) and (8) are similarly indistinguishable, showing an hourly revenue increase of between \$1.05 and \$2.69 per server, with approximately \$0.52-0.88 of this from drinks. In both cases, the individual fixed effect model parameter estimates are larger and better identified, suggesting a treatment effect on servers. Columns (9) and (10) present similar tip increases for both models. In sum, these results strongly suggest that the majority of productivity improvements and theft reduction is due to behavioral changes among existing workers, rather than selection effects due to managers replacing problem workers revealed by the IT system. Figure 10 presents the pre- and post-treatment distributions of raw hourly productivity for all workers using Epinechnikov kernel density functions, showing a substantial increase in hourly revenue post-treatment.

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<sup>29</sup> We alternatively ran conditional worker fixed-effect logit models which were consistent with our linear probability models. Models with conditional restaurant fixed effects failed to converge.



Distributions for drink revenue, losses, and tip percentage are presented as Figures A7-A9 in the Appendix.

To examine possible gender differences in response to increased monitoring, we use the Social Security Administration’s list of all names given in 1987 (representing 24-year olds) to assign a probability that any given worker is female. We designate any server with greater than 90% probability as female (13,166 servers) and those with less than 10% as male (7,889). We repeat the individual fixed effect models from Table 5 for each group, but observe few substantial difference in treatment effect (see Table A7). Although the impact to men’s total losses and revenue appear larger (but are not statistically different), this likely reflects a higher pretreatment base rate of hourly theft for men (\$0.15 vs \$0.09).

The worker-specific decreases in theft, combined with increases in revenue, drink revenue, and tip percentage are consistent with a multi-tasking explanation of the information technology impact. Workers who had previously derived income from allocating effort toward theft are now constrained by IT monitoring. Because this income source is now restricted, they reallocate effort toward the only other way they can generate income: increasing tips. Under their pay-for-performance compensation system, they can increase their income by increasing total revenue or increasing the percentage of total revenue that they capture in tips. It appears, based on the coefficients in Table 5, that they are increasing tips. The adoption of Restaurant Guard’s IT monitoring system appears to reallocate worker effort from counter-productive behavior (i.e. theft) to productive behavior (revenue and customer service).

#### 4.5 Managerial and Worker Responses

We next examine how managers respond to new information provided by Restaurant Guard. While we cannot directly observe this response, we can examine several possible responses through observations of staffing patterns. We first examine whether managers, after Restaurant Guard adoption allows them to observe theft by existing servers, allocate hours differently. To do so, we repeat our DiD specification, using weekly hours as our dependent variable. We apply this model to split samples based on the occurrence of pre-treatment theft. We designate the 4,034 servers with any observable theft in their specific restaurant before Restaurant Guard adoption as “known thieves”, while the remaining servers without observable theft are designated “unknown”. Given the likely prevalence of unobservable thefts, many of the *unknown* servers are likely involved in theft, creating substantial measurement error in our sample split that biases against identifying differences in effect.<sup>30</sup> We therefore test whether *known thieves* begin to receive fewer or worse

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<sup>30</sup> We also ran our four core models (revenue, drinks, losses, and tips), which are presented in Table A6, but these models suffer from mean reversion concerns common in split-sample DID models where the split designation is based on a variable correlated with the dependent variables of interest. This makes a comparison of coefficients across samples to be problematic. For example, columns 1 and 2 of Table A6 show obvious mean reversion on total losses.

hours than *low theft* workers once their behavior is revealed by repeating our DiD model with individual fixed effects for our split samples.<sup>31</sup>

Columns (1) and (2) of Table 6, which present the total hours results, show that while the hours of *known thieves* do not change following IT monitoring adoption, the hours of *unknown* workers increase by over ten percent. While we cannot observe why this occurs, it suggests that managers may be selectively allocating additional hours to servers that appear honest. Columns (3)-(4) present similar models predicting percentage of time staffed for high-traffic weekend and dinner shifts. The effects are roughly zero and indistinguishable across the two worker types.

To examine whether *known thieves* are more likely to leave after Restaurant Guard adoption, we next implement Cox proportional hazards models. In these models, workers are at risk for attrition immediately upon hiring, with time-varying covariates that impact their likelihood in any week. We include a quartic time trend to capture seasonal worker turnover, and implement random effects as shared frailties within chains to account for specific corporate human resource policies.<sup>32</sup> We present our results for these models in Table 7 as odds ratios, with robust standard errors.<sup>33</sup> Column (1) presents the basic treatment effect of Restaurant Guard adoption on average worker attrition. The odds ratio of 0.975 suggests a slight decrease in attrition following Restaurant Guard adoption, although it is imprecisely estimated. Columns (2) and (3) show that attrition increases, however, for *known thieves*, while significantly decreasing for *unknown* workers. We present pre- and post-treatment survivor curves for both groups in Figures 11 and 12, showing the substantial differences in survival after treatment for the two groups.<sup>34</sup>

These survival results are consistent with two possible explanations. First, managers may be firing workers after observing theft, which forces them to keep many workers they might otherwise fire. Second, workers who formerly derived some of their income from theft might voluntarily leave after their income has been constrained by IT monitoring. The job might be less attractive without theft opportunities, particularly compared to other restaurants without IT monitoring. For the remaining workers, more attractive hours might also increase the attractiveness of the job, as might a sense of fairness from less ethical workers being detected and forced out.

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<sup>31</sup> We again note that these pre-treatment behavioral patterns are not observable to the manager after Restaurant Guard adoption..

<sup>32</sup> Models using weekly fixed effects fail to consistently converge to a global maximum. Similarly, shared frailties at the restaurant level are computationally impossible in this case.

<sup>33</sup> STATA does not allow for clustering standard errors in random-effects shared-frailty models.

<sup>34</sup> We note that these results are sensitive to the designation of random-effect levels. Models with shared frailties at the restaurant level produce differences between the two groups of similar magnitudes, but decrease the odds ratios of both groups such that *known thieves* are negative. This could reflect an overall decrease in odds ratios post treatment based on overall performance increases at the restaurants.

To test whether the increased attrition among known thieves is due to self-selection or termination, we estimate additional Cox hazard models that add a dummy variable that indicates any theft by the worker in the previous two weeks as well as an interaction of this dummy with the treatment dummy. If management is firing workers after observing theft, we should observe an increased hazard in the two weeks immediately following a theft revelation. We present this model in column (4). The odds ratio for the non-interacted dummy for theft in the previous weeks indicates that those recently able to steal are less likely to leave the firm. The interaction, however, indicates that this hazard does not increase following the implementation of Restaurant Guard. This casts doubt on the explanation that workers are being fired following theft detection. Instead, the results in columns (2)-(4) suggest that restaurants that effectively use IT monitoring to decrease theft opportunity are doing so proscriptively rather than reactively, likely informing staff of the new IT monitoring system. While this is largely speculative, it is consistent with our interviews with restaurant managers. Those workers who leave the restaurant are likely doing so for better theft opportunities elsewhere.

#### **4.6 Robustness to Server Rankings Treatment**

An additional concern in our data is that the restaurants in our sample simultaneously adopted a second IT product during our period. The IT service provider also sent weekly productivity rankings for each restaurant's servers based on ten criteria. While interviews with managers suggested these rankings were both rarely used and already known, we are concerned that this second treatment might explain many of the productivity gains observed in our regression models if impacted management's staffing or termination decisions.

To examine this alternative explanation, we first exploit a set of workers who were impacted by the theft monitoring but not the productivity reports—bartenders. If theft monitoring is responsible for the productivity improvements in our earlier models, we should observe similar results for bartenders. We repeat our difference-in-differences models with individual fixed effects and present them in Table 8. We see remarkably similar results for the models in columns (1)-(4). Although the number of observations is greatly reduced, we see large and identifiable improvements in check revenue, drinks, and tip percentage, as well as a weakly identified decrease in theft. These collectively cast doubt on the possibility that server productivity ranking reports are driving our main results.

We also examine whether management responded to the rankings revelation by reallocating shifts and hours to highly-ranked employees. Using the ranking algorithm provided by the data provider, we calculated the ranking sent to management each week for each server.<sup>35</sup> We split all employees who worked the first four

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<sup>35</sup> Because bartenders were not ranked, they are excluded from this analysis.

weeks following Restaurant Guard adoption into two groups based on whether their average weekly ranking during this period was above (*high rank*) or below (*low rank*) the median ranking. A server with an average ranking of 3.5, for example, would be *high rank*, while a server with an average of 20 would be *low rank*. If management is using these ranking systems, we would expect highly ranked employees to receive more high-traffic shifts (i.e. weekends and evenings) and hours following Restaurant Guard adoption at the expense of lower ranked workers. We present the results for these models in columns (5)-(10) of Table 8. We observe no difference in shift staffing between groups following the ranking implementation.. While this cannot rule out the role of productivity rankings in the overall observable treatment effect, it casts additional doubt that it is the primary impact of Restaurant Guard.

## V. Discussion and Conclusion

In this paper, we show evidence that the adoption of information technology can substantially impact both the productive and corrupt behaviors of employees. Our results suggest that when management implements increased monitoring under a pay for performance scheme, employees will redirect effort toward productivity because their incentives have been realigned. Furthermore, our results suggest that the majority of improvement in organizational performance and productivity stems from the improved behavior of existing employees, not from the firing of those engaged in theft. The treatment of individual workers, not worker selection, appears to drive most productivity improvements and theft reductions. This does not mean that worker selection is unimportant in our story. In fact, those workers who stole under the weaker monitoring regime appear to self-select out of the more highly-monitored restaurants, perhaps to other more easily pilfered establishments.

Each of these results is highly consistent with a multi-tasking story where workers (agents) seek to trade off costly effort for income from either productivity under a pay-for-performance scheme or theft. Increased monitoring by management (principal) reduced the net gains from theft, necessarily increasing the equilibrium effort allocation toward productive behavior. In such a model, where workers are free to select out of the firm, increased monitoring also makes outside options more attractive, thereby increasing the likelihood of attrition for all workers who previously derived any income from theft. We note, however, that other cost-based worker activities remain unobservable in our data. We cannot, for example, observe whether reducing one type of theft (stealing revenue) through monitoring increases other forms of theft or misconduct such as inventory shrinkage. Given Olken's (2007) results on substitution across types of corruption, such costs may very well exist and thereby reduce the profit gains from monitoring.

Another possible explanation for our results is that Restaurant Guard, by reducing the effort or

attention required of managers toward theft monitoring, frees them to focus on managing service and food quality. Such a reallocation of managerial effort across dimensions could also result in the productivity and service quality improvements observed in our data. Although we are unable to separate these two mechanisms, both are based in the fundamental multitasking tradeoff between misconduct and productivity. A technology tool that improves monitoring of employee misconduct has the potential to improve productivity both by providing financial incentives for employees to redirect effort and through freeing managerial attention toward improving production efficiency.

The results in this paper are important for at least three reasons. First, they represent the measurement of an important economic activity, employee theft, that has largely been observed only indirectly or anecdotally in firms. Although there is a considerable literature on corruption (e.g. Olken and Barron 2009), direct evidence on illegal behavior by firm employees is rare in the economics literature. Nagin et al. (2002) is an exception, demonstrating employee reductions in fraud following audit increases. We are able to show not only the direct effect of monitoring on theft, as they do, but also the secondary employee adjustments to other productive tasks to account for lost income. Second, our results illustrate the value of information technology when it complements human resource practices that motivate productive effort. Similar to arguments made by Bloom and colleagues (2012), the pay for performance system in American restaurants is likely important in how the IT monitoring system in our setting redirects effort from theft toward productivity. Finally, our results suggest a counterintuitive and hopeful pattern in human behavior: employee theft is a remediable problem *at the individual employee level*. While individual differences in moral preferences may indeed exist, realigning incentives through organizational design can have a powerful effect in reducing corrupt behaviors. This runs counter to the common view in the human resource management literature that productivity and integrity is largely about selection rather than managerial practice or technology (e.g. Ones et al. 1993). We show that firms can use information about employee theft not simply to fire the culprits, but rather to alter their behavior in ways that improve productivity.

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Table 1: Descriptive Statistics for Weekly Restaurant and Weekly Worker Samples

Variable	Weekly Location		Hourly Worker	
	Mean	SD	Mean	SD
Total Losses	\$108.47	\$207.24	\$0.15	\$0.92
Check Revenue	\$43,697	\$18,659	\$79	\$32
Drink Revenue	\$8,879	\$5,738	\$14	\$11
Tip Percentage	14.8%	1.9%	15.5%	3.6%
Treated	0.77	0.42	0.80	0.40
Workers/Day	15.5	4.3	---	---
Observations	22,329		439,838	

Note: Tip percentage has fewer observations because Chain 5 does not report tips.

Table 2: Weekly Restaurant Means by Chain

Variable	Chain 1	Chain 2	Chain 3	Chain 4	Chain 5
Total Losses	\$92.06	\$124.01	\$4.85	\$180.00	\$18.79
Check Revenue	\$50,639	\$48,863	\$25,257	\$50,126	\$17,199
Drink Revenue	\$17,149	\$7,194	\$3,401	\$13,485	\$4,711
Tip Percentage	13.6%	14.7%	14.1%	16.0%	---
Week Added	2677.2	2659.0	2666.5	2668.5	2697.2
Treated	0.41	0.92	0.44	0.97	0.31
Locations	29	181	36	119	27
Workers/Day	18.8	14.3	13.1	19.6	10.5
Observations	2,219	10,081	3,588	5,143	1,298

Note: Week 2656 is the first week in March, 2011. All numbers represent means. Chain 5 does not report tips.

Table 3: Impact of Information Technology on Weekly Restaurant Performance

Dependent Variable:	(1) Losses	(2) Check Revenue	(3) Drink Revenue	(4) Tip Pct.
<i>Treated with Rest. Guard</i>	-23.61*** (7.03)	2,975.21*** (508.91)	926.97*** (148.14)	0.0030*** (0.0005)
Week Fixed Effects	Included	Included	Included	Included
Location Fixed Effects	Included	Included	Included	Included
Constant	122.31*** (3.73)	30,162.30*** (705.73)	7182.84*** (127.56)	0.1475*** (0.0011)
Adjusted R-squared	0.180	0.817	0.891	0.726
Observations	22,329	22,329	22,329	20,912

Note: Standard errors clustered at the location level in parentheses. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level. Tip models have fewer observations due to some locations not tracking tips.

Table 4: Persistence of Information Technology Impact

Dependent Variable:	(1) Losses	(2) Check Revenue	(3) Drink Revenue	(4) Tip Pct.
<i>Treatment Month 1</i>	-6.75 (8.92)	2,189.034*** (629.96)	699.64*** (178.11)	0.0029*** (0.0005)
<i>Treatment Month 2</i>	-28.81*** (7.27)	2,874.90*** (622.70)	935.51*** (186.44)	0.0032*** (0.0006)
<i>Treatment Month 3</i>	-47.78*** (8.10)	1,235.45** (545.17)	546.12*** (181.78)	0.0043*** (0.0006)
Week Fixed Effects	Included	Included	Included	Included
Location Fixed Effects	Included	Included	Included	Included
Constant	122.53*** (4.01)	30,238.04*** (781.70)	7196.69*** (135.46)	0.1473*** (0.0012)
Adjusted R-squared	0.180	0.824	0.893	0.730
Observations	22,329	22,329	22,329	20,901

Note: Standard errors clustered at the location level in parentheses. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level. Tip models have fewer observations due to some locations not tracking tips.

Table 5: Impact of Information Technology on Hourly Worker Performance

Dependent Variable:	(1) Hourly Losses	(2) Hourly Losses	(3) Any Losses	(4) Any Losses	(5) Hourly Revenue	(6) Hourly Revenue	(7) Hourly Drink Rev	(8) Hourly Drink Rev	(9) Tip Pct.	(10) Tip Pct.
<i>Treated with Rest. Guard</i>	-0.053*** (0.009)	-0.056*** (0.009)	-0.017*** (0.005)	-0.012*** (0.004)	1.054 (0.679)	2.690*** (0.557)	0.523*** (0.136)	0.883*** (0.125)	0.0027*** (0.0005)	0.0019*** (0.0004)
Week FE	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included
Shift Controls	Included	Included	--	Included	Included	Included	Included	Included	Included	Included
Location FE	Included	--	Included	--	Included	--	Included	--	Included	--
Worker FE	--	Included		Included	--	Included	--	Included	--	Included
Constant	0.163*** (0.006)	0.164*** (0.010)	0.163*** (0.009)	0.153*** (0.009)	65.028*** (0.830)	60.941*** (0.866)	13.868*** (0.256)	12.009*** (0.427)	0.1574*** (0.0015)	0.1574*** (0.0015)
Adjusted R-squared	0.012	0.063	0.067	0.138	0.484	0.648	0.364	0.604	0.231	0.331
Observations	439,838	439,838	439,838	439,838	439,838	439,838	439,838	439,838	437,860	437,860

Note: Standard errors in parentheses. Errors clustered at location level in all models except (4), where they are clustered at individual level by necessity. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level. Tip models have fewer observations due to some locations not tracking tips.

Table 6: Staffing Response of Managers to Information Technology Implementation

	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Known Thief	Unknown	Known Thief	Unknown	Known Thief	Unknown
Dependent Variable:	Weekly Hours	Weekly Hours	Weekend	Weekend	Dinner	Dinner
<i>Treated with Rest. Guard</i>	0.083 (0.265)	2.249*** (0.242)	0.005 (0.004)	0.002 (0.004)	-0.003 (0.008)	-.004 (0.006)
Week Fixed Effects	Included	Included	Included	Included	Included	Included
Worker Fixed Effects	Included	Included	Included	Included	Included	Included
Constant	13.778*** (0.651)	12.956*** (0.483)	0.016** (0.008)	0.037*** (0.013)	0.323*** (0.019)	0.214*** (0.019)
Adjusted R-squared	0.418	0.493	0.302	0.364	0.635	0.646
Observations	123,091	177,541	123,091	177,541	123,091	177,541

Note: "Known thieves" are designated by observable theft events prior to treatment date. "Unknown" includes everyone else, many of whom may be engaged in theft. Standard errors clustered at the location level in parentheses. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level.

Table 7: Cox Models of Post-Adoption Attrition

	(1)	(2)	(3)	(4)
Sample:	All	Known Thieves	Unknown	All
<i>Treated with Rest. Guard</i>	0.975 (0.033)	1.141** (0.075)	0.825*** (0.033)	0.972 (0.034)
<i>Theft Occurred in Previous Two Weeks</i>	-	-	-	0.802*** (0.037)
<i>Theft Occurred and Was Reported to Management in Previous Two Weeks</i>	-	-	-	1.019 (0.052)
Week Fixed Effects	Included	Included	Included	Included
Chain Random Effects	Included	Included	Included	Included
Subjects	26,049	5,548	20,501	26,049
Failures	14,337	3,137	11,200	14,337
Observations	488,989	159,166	329,823	488,989

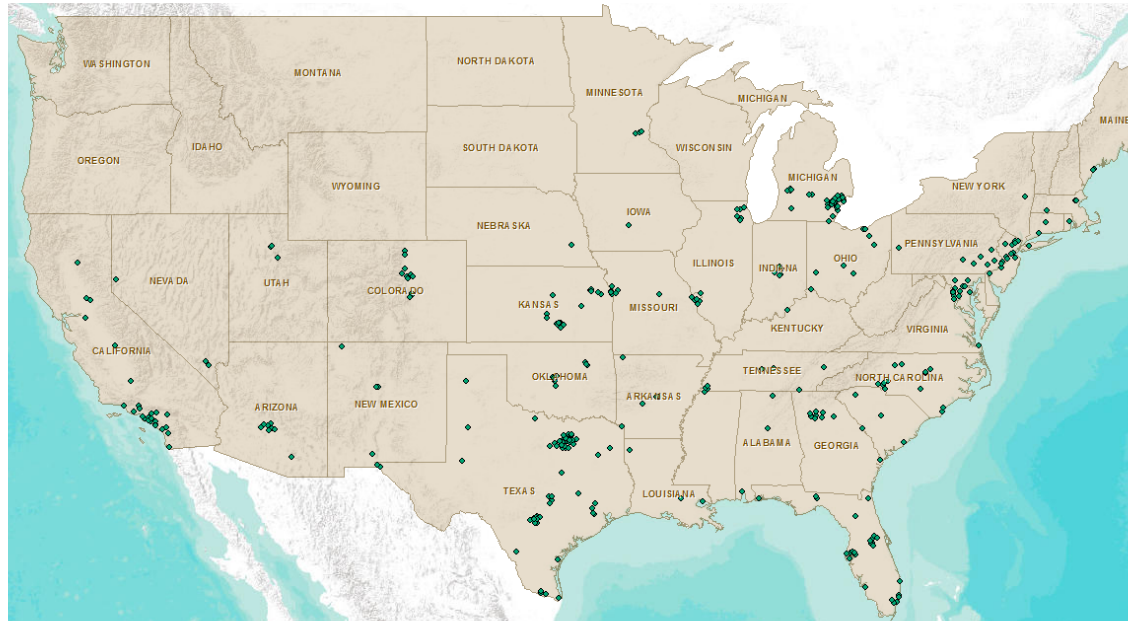
Note: Odds ratios presented, with standard errors in parentheses. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level. All models account for right-hand censoring.

Table 8: Tests for Alternative Productivity Rankings Treatment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Sample:	Bartenders	Bartenders	Bartenders	Bartenders	High Rank Servers	Low Rank Servers	High Rank Servers	Low Rank Servers	High Rank Servers	Low Rank Servers
Dependent Variable:	Hourly Losses	Hourly Revenue	Hourly Drink Rev	Tip Pct.	Weekend Shifts	Weekend Shifts	Dinner Shifts	Dinner Shifts	Weekly Hours	Weekly Hours
<i>Treated with RestAssure</i>	-0.046 (0.032)	3.314*** (1.004)	1.013*** (0.376)	0.0031* (0.018)	0.0035 (0.0036)	0.0055 (0.0049)	-0.0004 (0.0074)	0.0089 (0.0082)	1.397*** (0.272)	1.047*** (0.274)
Week FE	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included
Worker FE	Included	Included	Included	Included	Included	Included	Included	Included	Included	Included
Constant	.443*** (0.168)	79.099*** (1.963)	8.307*** (1.063)	0.0979*** (0.0130)	.056*** (0.012)	0.050*** (0.018)	0.315*** (0.022)	0.177*** (0.029)	12.07*** (0.55)	14.29*** (0.690)
Adjusted R-squared	0.016	0.584	0.622	0.353	0.344	0.320	0.624	0.584	0.457	0.447
Observations	33,753	33,753	33,753	33,492	155,148	110,127	155,148	110,127	155,148	110,127

Note: Bartenders samples include only those employees who worked the entire week under a bartender job code. Most bartenders at casual dining restaurants also serve food at the bar. High rank servers are defined by those with above median weekly rank, based on the data provider's proprietary ranking system. Standard errors clustered at the location level in parentheses. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level. Tip models have fewer observations due to some locations not tracking tips.

Figure 1: Geographic Distribution of All Restaurant Locations



Note: Three Alaska locations and one Hawaii location are not shown.

Figure 2: Number of Restaurant Locations By Week in the Dataset

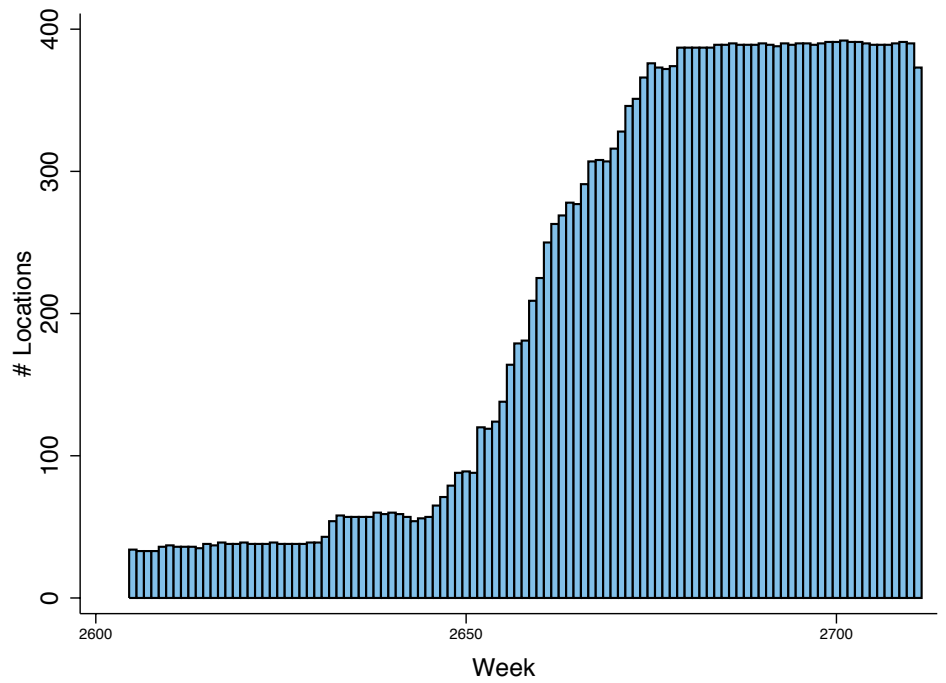


Figure 3: Example of Restaurant Guard Incident Report

**RECENT INCIDENTS**

The following table displays the most recent instances of this type of activity.

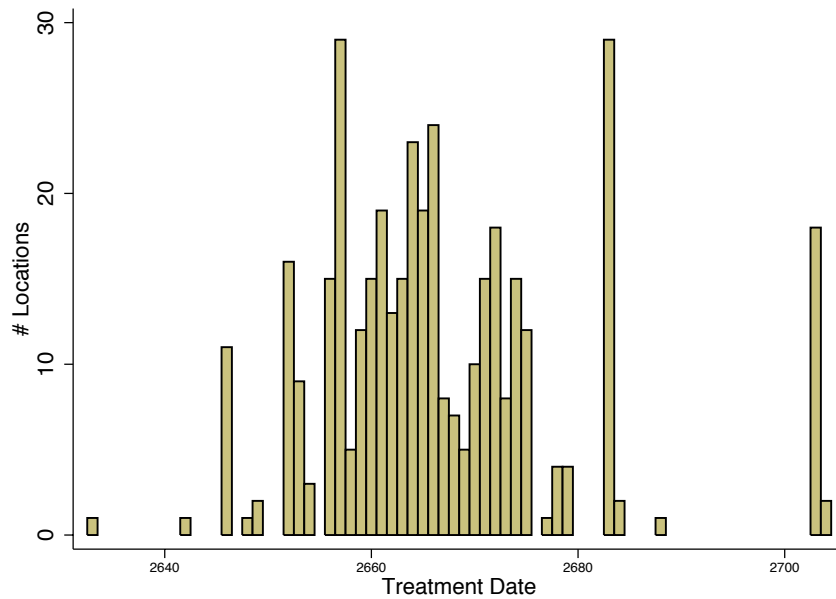
Date	Item	First Check	Last Check	# Transfers	Item Price	Potential Loss
02/07/2009	Soft Drink	1001	2032	2	1.79	3.58
02/07/2009	Iced Tea	1046	3012	5	1.79	8.95
02/07/2009	Iced Tea	1005	1014	3	1.79	5.37
02/05/2009	Coffee	2210	2215	2	1.59	3.18
02/05/2009	Iced Tea	1114	1154	3	1.79	5.37
02/05/2009	Soft Drink	3100	1146	6	1.79	10.74
02/05/2009	Soft Drink	1111	1121	2	1.79	3.58
02/04/2009	Soft Drink	1002	3004	4	1.79	7.16
02/04/2009	Chocolate Cake	1021	1033	2	4.99	9.98
02/03/2009	Coffee	2023	2040	3	1.59	4.77
02/03/2009	Soft Drink	2026	1141	3	1.79	5.37
02/03/2009	Iced Tea	2028	2033	4	1.79	7.16
02/03/2009	Iced Tea	2031	1100	2	1.79	3.58
02/03/2009	Side Salad	3114	2106	2	2.99	5.98
<b>Total Potential Loss:</b>						<b>\$84.77</b>

**INCIDENT HISTORY ASSOCIATED WITH THIS EMPLOYEE ID**

The following table displays a summary of all alerts generated for this employee ID.

Scam Type	Event Count	First Incident	Last Incident	Avg Loss / Incident	Total Potential Loss
Transfer	26	01/27/2009	02/07/2009	\$6.06	\$157.43
Comp after Print	3	01/29/2009	02/02/2009	\$8.41	\$25.23

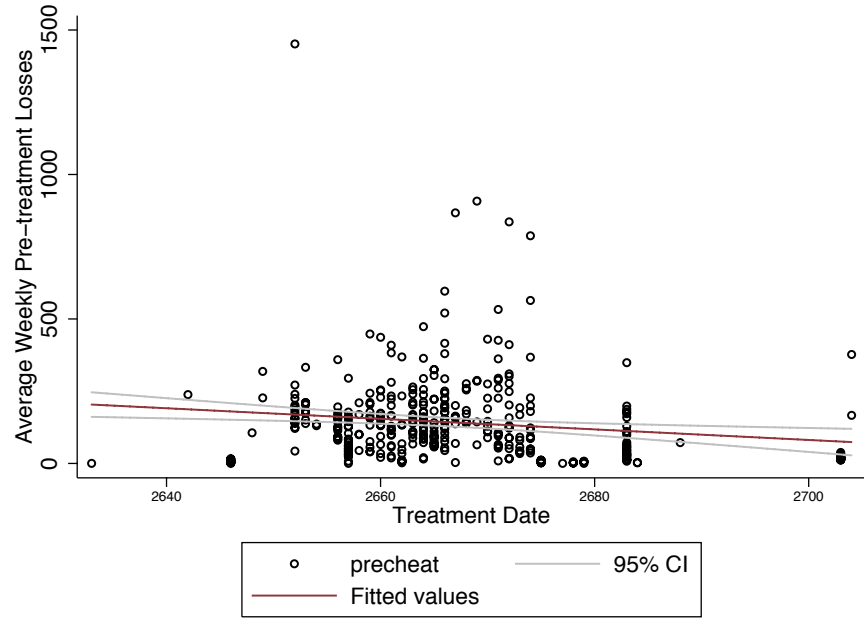
Figure 4: Adoption Weeks by Location



Note: Weeks are represented as standardized dates from Stata, counting from January 1, 1960. For example, 2660 is the last week in March, 2011.

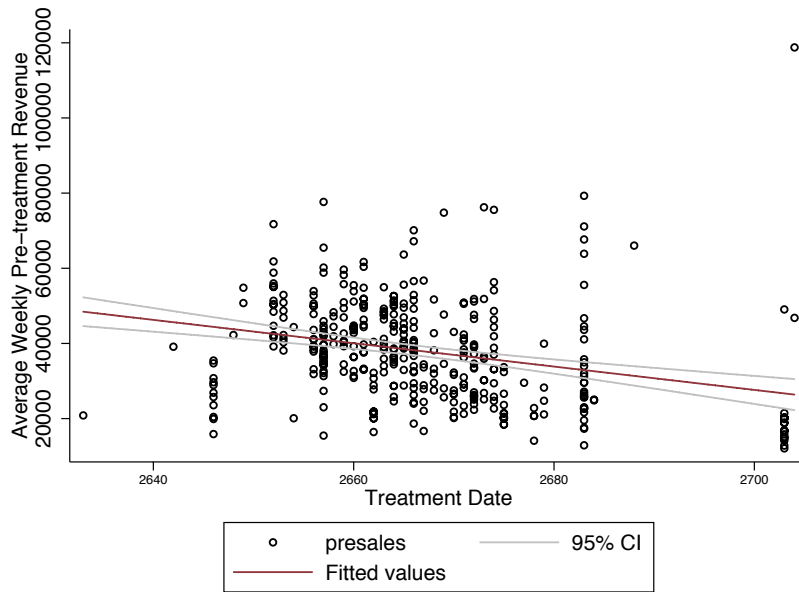
Figure 5: Adoption Weeks by Pretreatment Losses





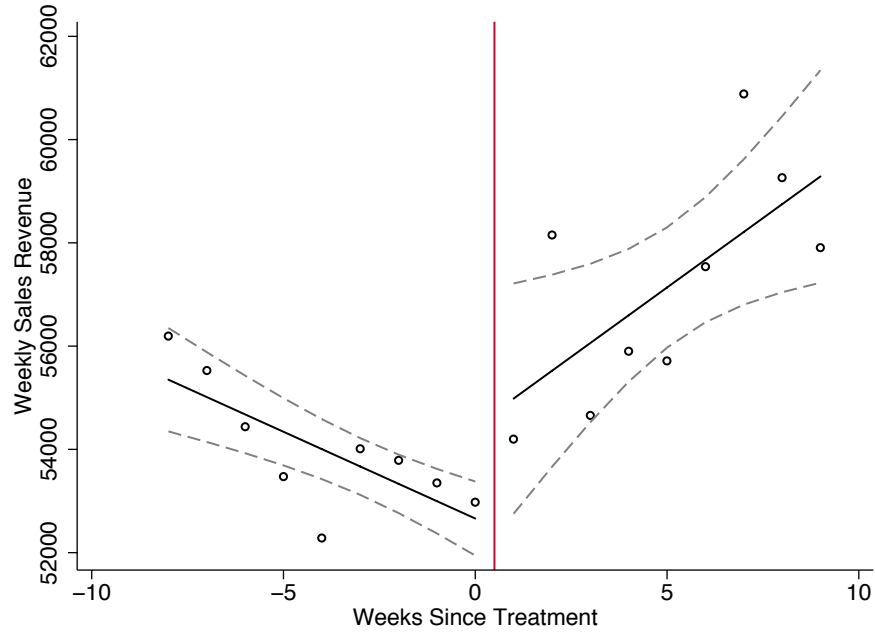
Note: Each point represents the average weekly total losses for one restaurant, for all weeks prior to treatment. Regressing treatment week on the weekly loss amount with no controls yields a coefficient of  $-0.013$  ( $p=.001$ ).

Figure 6: Adoption Weeks by Pretreatment Sales



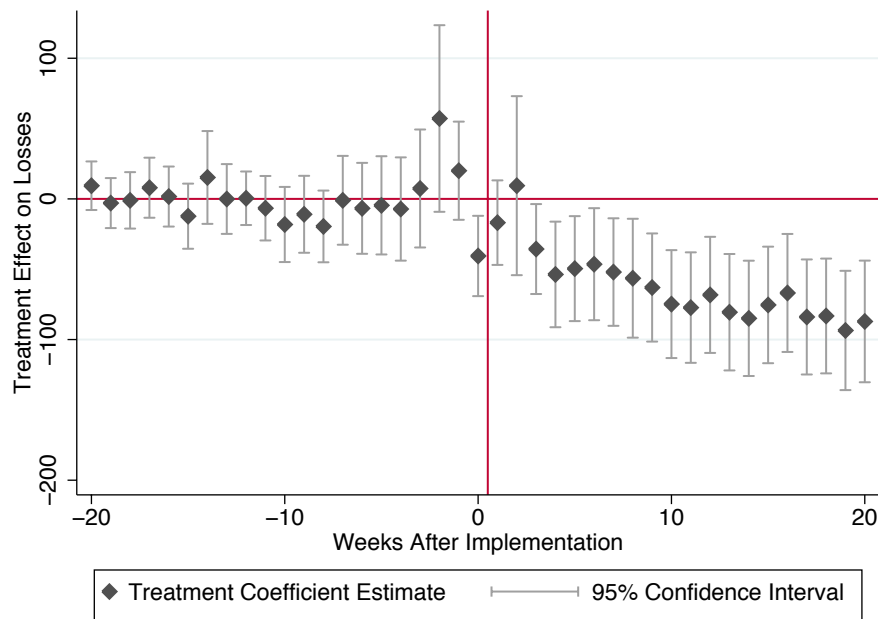
Note: Each point represents the average weekly check revenue for one restaurant, for all weeks prior to treatment. Regressing treatment week on the weekly revenue with no controls yields a coefficient of  $-0.00025$  ( $p=.01$ ).

Figure 7: Treatment Effect on Weekly Revenue in Chain 1



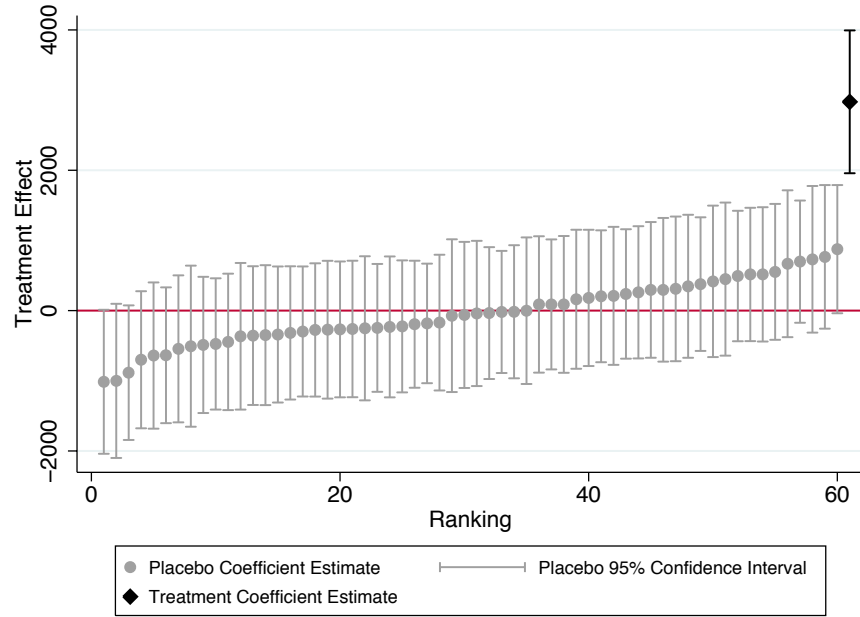
Note: Each point represents the average weekly revenue for 28 restaurants in Chain 1. Dashed lines represent 95% confidence intervals.

Figure 8: Treatment Effect Estimates for Total Losses Before and After IT Implementation



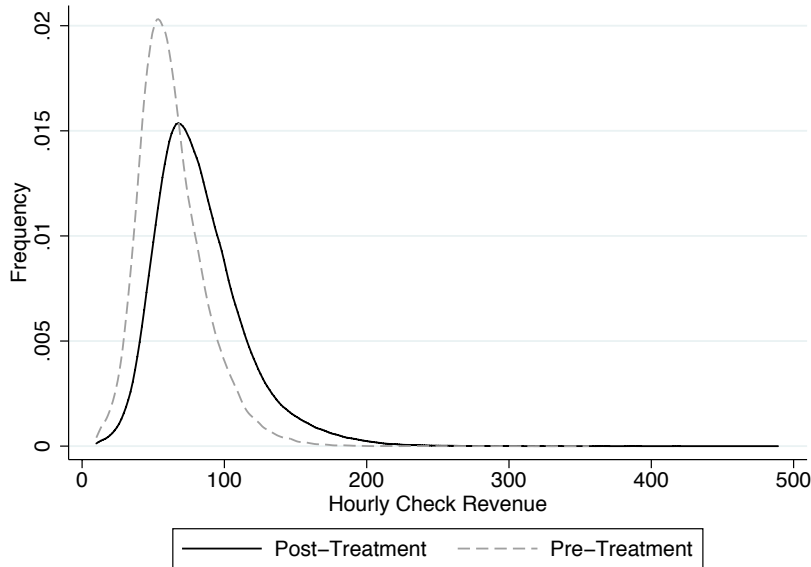
Note: Imprecise pre-treatment jump in losses based on one outlier location with \$3,273 and \$5,470 in losses three and two weeks before implementation, respectively.

Figure 9: Placebo Tests of Treatment Effect on Total Revenue



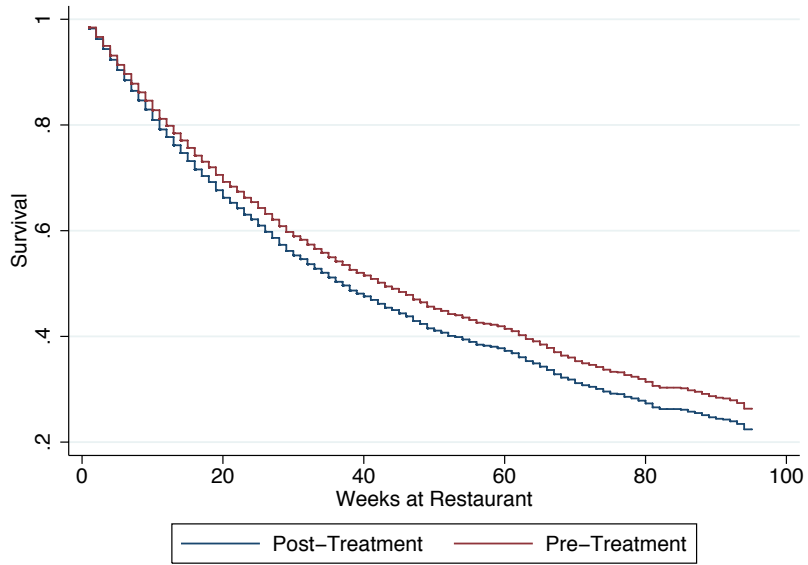
Note: Actual treatment dates were randomly assigned to restaurants sixty times. Each point and whiskers represents an OLS regression with restaurant fixed effects and full controls, using the weekly restaurant data. The coefficient and confidence intervals for the true data are represented in black.

Figure 10: Distribution of Hourly Productivity Pre- and Post-Treatment



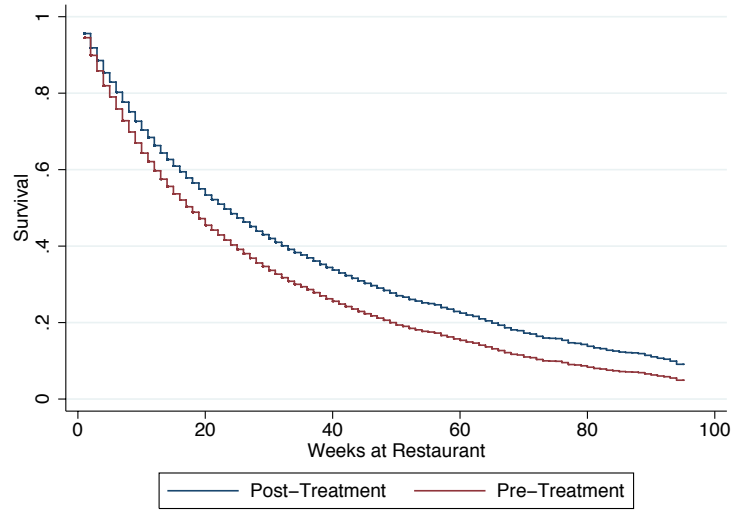
Note: Epinechnikov kernel density estimates of raw hourly productivity averages at the worker week level.

Figure 11: Cox Survivor Functions for Known Thieves



Note: Survivor functions based off Cox proportional hazard models in Table 7, estimated at frailty=1. Known thieves are those workers observable to the researcher as engaging in theft prior to Restaurant Guard implementation.

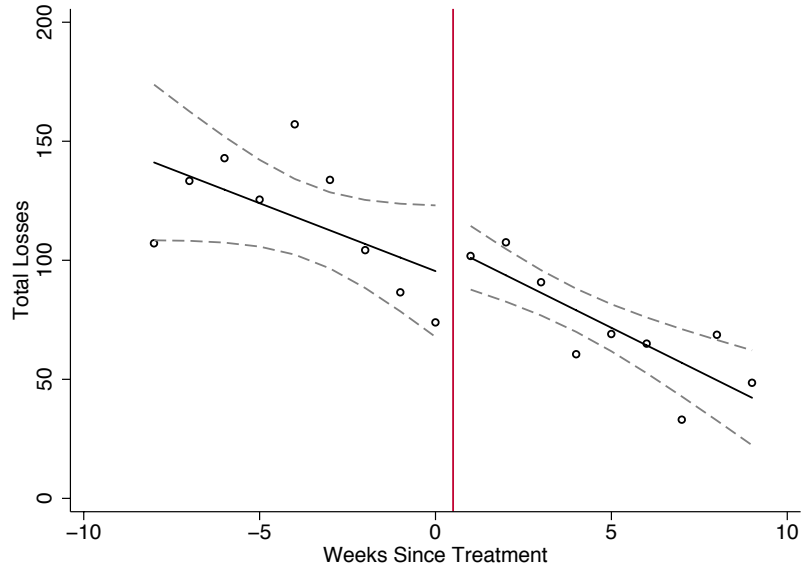
Figure 12: Cox Survivor Functions for Workers with Unknown Theft



Note: Survivor functions based off Cox proportional hazard models in Table 7, estimated at frailty=1. Unknown workers are those workers with no observable theft prior to Restaurant Guard implementation.

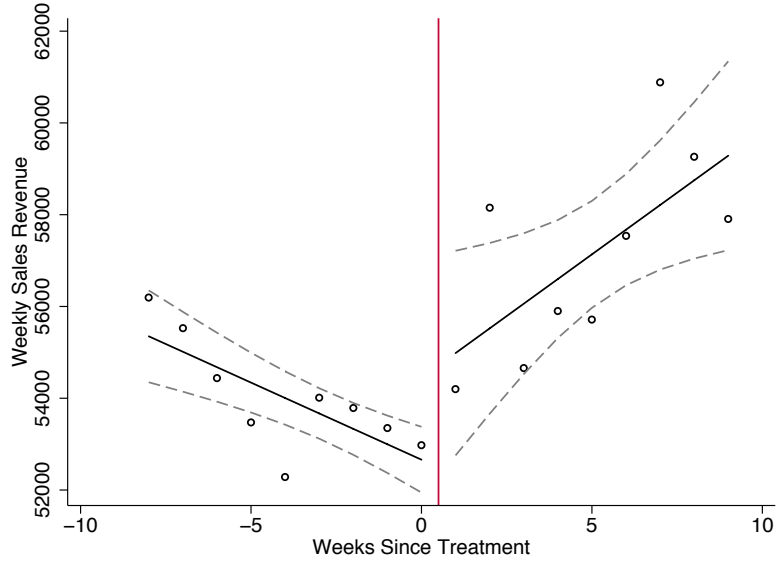
# Appendix

Figure A1: Treatment Effect on Weekly Losses in Chain 1



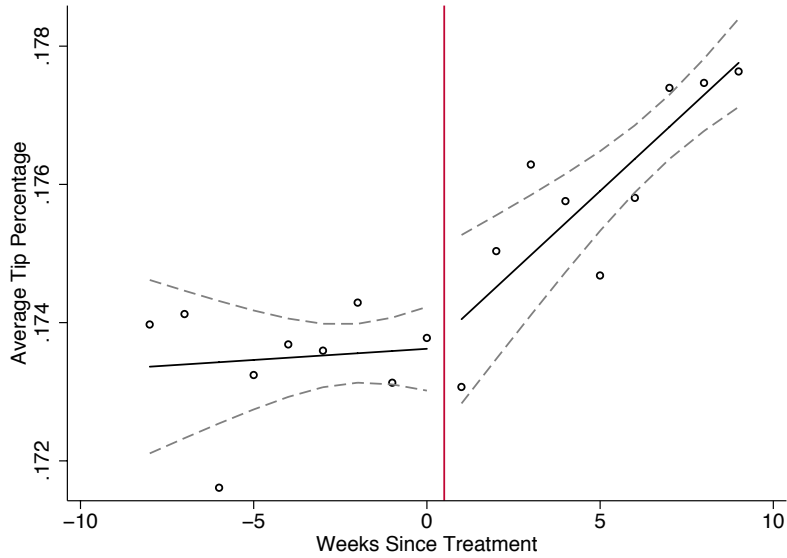
Note: Each point represents the average weekly losses for 28 restaurants in Chain 1. Dashed lines represent 95% confidence intervals.

Figure A2: Treatment Effect on Drink Revenue in Chain 1



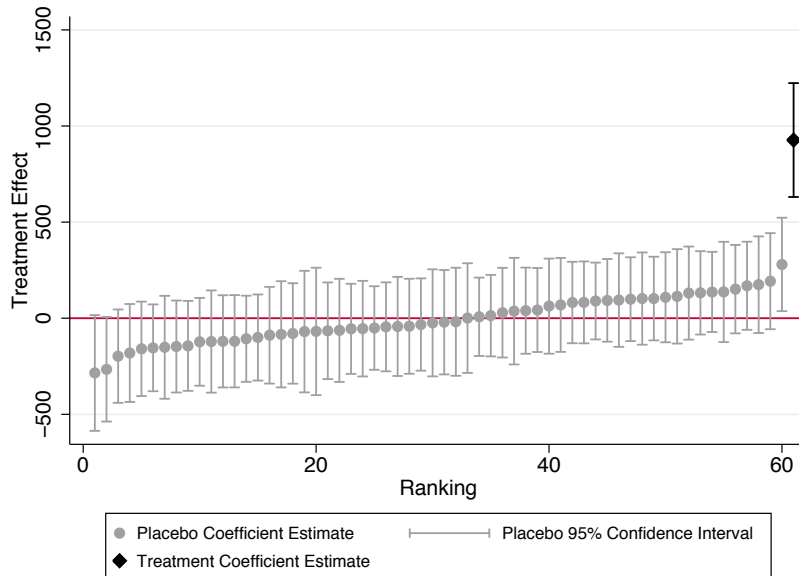
Note: Each point represents the average weekly drink revenue for 28 restaurants in Chain 1. Dashed lines represent 95% confidence intervals.

Figure A3: Treatment Effect on Tip Percentage in Chain 1



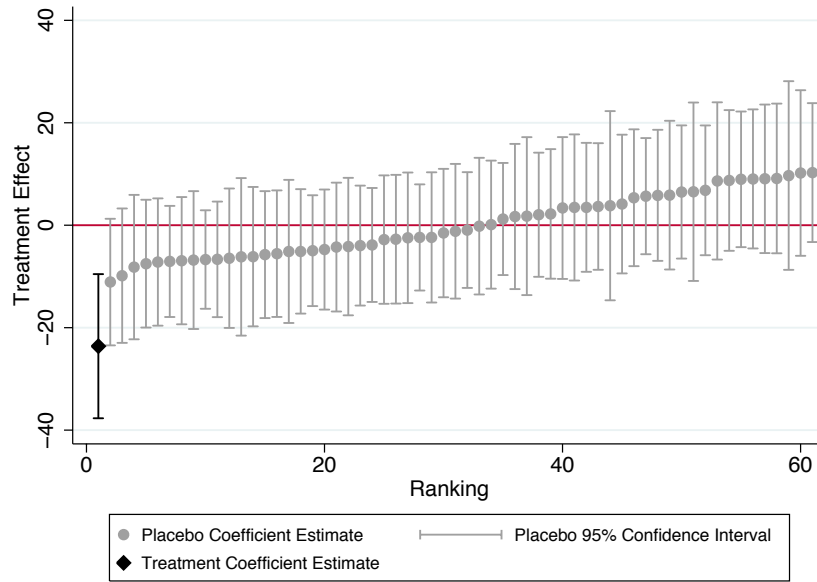
Note: Each point represents the average weekly tip percentage for 28 restaurants in Chain 1. Dashed lines represent 95% confidence intervals.

Figure A4: Placebo Tests of Treatment Effect on Drink Revenue



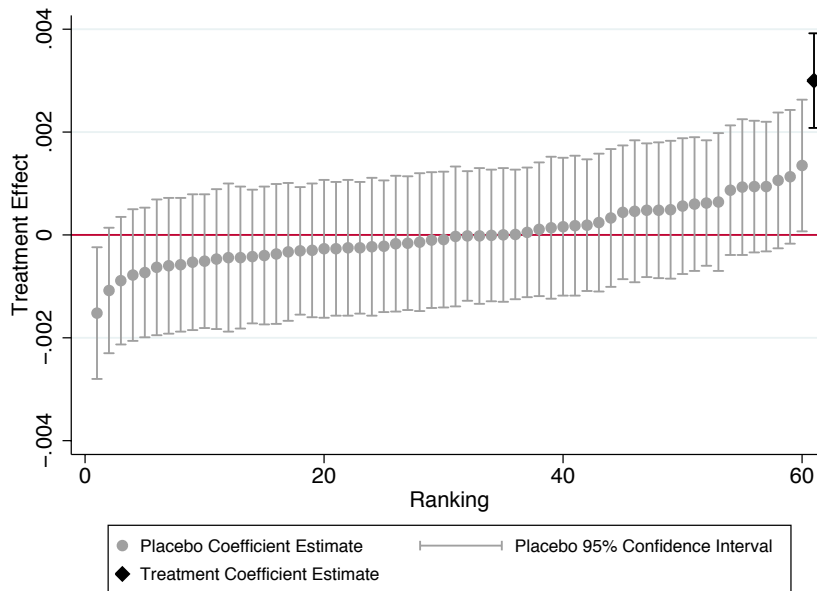
Note: Actual treatment dates were randomly assigned to restaurants sixty times. Each point and whiskers represents an OLS regression with restaurant fixed effects and full controls, using the weekly restaurant data. The coefficient and confidence intervals for the true data are represented in black.

Figure A5: Placebo Tests of Treatment Effect on Total Losses



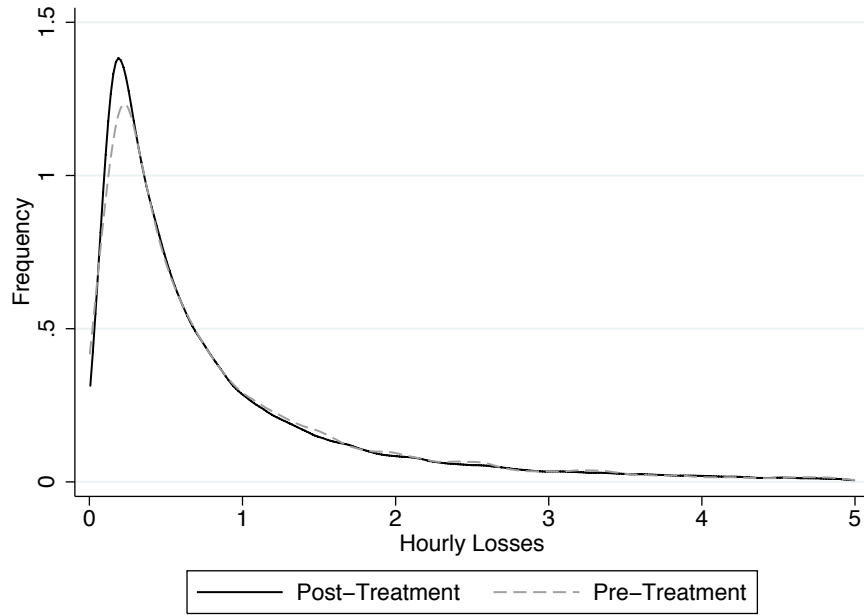
Note: Actual treatment dates were randomly assigned to restaurants sixty times. Each point and whiskers represents an OLS regression with restaurant fixed effects and full controls, using the weekly restaurant data. The coefficient and confidence intervals for the true data are represented in black.

Figure A6: Placebo Tests of Treatment Effect on Tip Percentage



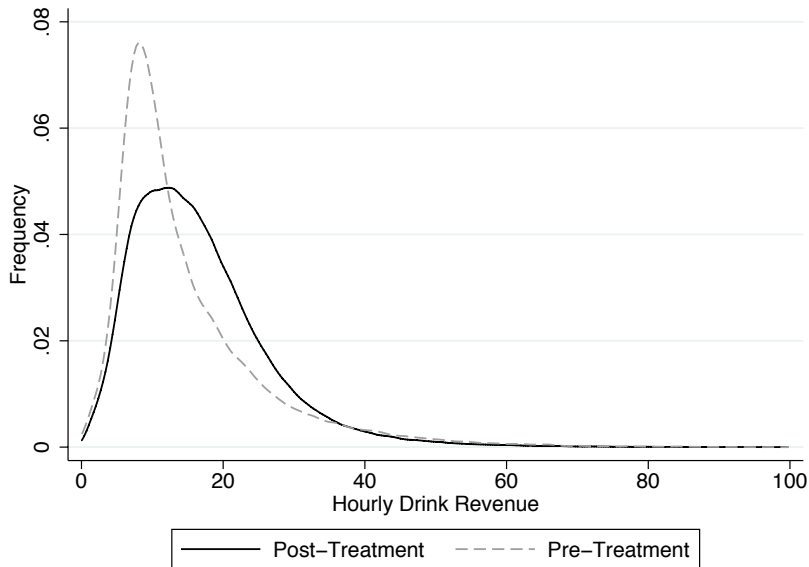
Note: Actual treatment dates were randomly assigned to restaurants sixty times. Each point and whiskers represents an OLS regression with restaurant fixed effects and full controls, using the weekly restaurant data. The coefficient and confidence intervals for the true data are represented in black.

Figure A7: Distribution of Hourly Total Losses Pre- and Post-Treatment



Note: Epinechnikov kernel density estimates of raw hourly loss averages at the worker week level.

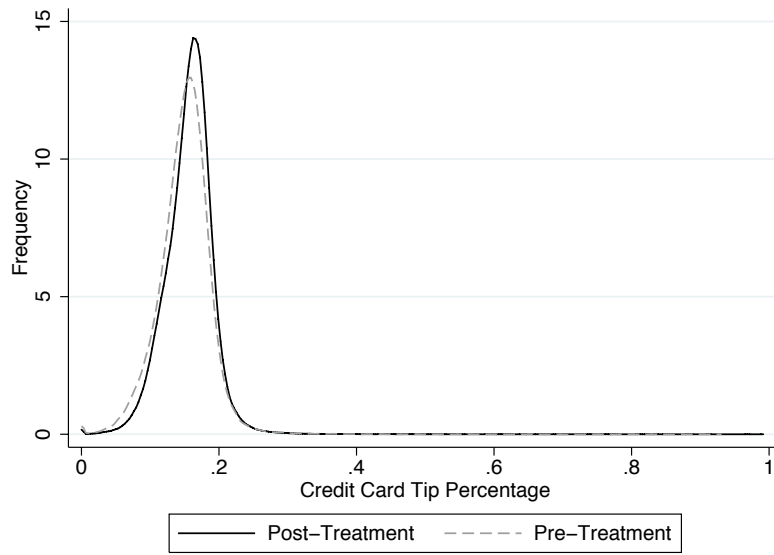
Figure A8: Distribution of Hourly Drink Revenue Pre- and Post-Treatment



Note: Epinechnikov kernel density estimates of raw hourly drink revenue averages at the worker week level.



Figure A9: Distribution of Tip Percentages Pre- and Post-Treatment



Note: Epinechnikov kernel density estimates of raw hourly drink revenue averages at the worker week level.

**Table A1: Models Using Block-Bootstrapped Standard Errors**

Dependent Variable:	Location Models				Worker Models			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Losses	Check Revenue	Drink Revenue	Tip Pct.	Hourly Losses	Hourly Check Rev.	Hourly Drink Rev.	Tip Pct.
<i>Treated with Rest. Guard</i>	-23.61*** (6.65)	2,975.21*** (470.20)	926.97*** (142.88)	0.0030*** (0.0005)	-0.056*** (0.008)	2.690*** (0.186)	0.883*** (0.071)	0.0019*** (0.0003)
Week FE	Included	Included	Included	Included	Included	Included	Included	Included
Location FE	Included	Included	Included	Included	-	-	-	-
Worker FE	-	-	-	-	Included	Included	Included	Included
Constant	122.31*** (7.08)	30,162.30*** (960.69)	7182.84*** (274.00)	0.1475*** (0.0011)	0.164*** (0.007)	60.941*** (0.585)	12.009*** (0.185)	0.1574*** (0.0015)
Observations	22,329	22,329	22,329	20,901	439,838	439,838	439,838	437,860

Note: Standard errors are block bootstrapped by location level in columns 1-4, and block bootstrapped by worker in columns 5-8. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level. Tip models have fewer observations due to some locations not tracking tips.

**Table A2: Models Using Logged Dependent Variables**

	(1)	(2)	(3)	(4)
Dependent Variable:	Logged Losses	Logged Check Rev.	Logged Drink Rev.	Logged Tip Pct.
<i>Treated with Rest. Guard</i>	-.261*** (.057)	.100*** (.014)	.104*** (.014)	.0027*** (.0004)
Week Fixed Effects	Included	Included	Included	Included
Location Fixed Effects	Included	Included	Included	Included
Constant	4.065*** (.241)	9.905*** (.023)	8.414*** (.044)	0.1374*** (.0010)
Adjusted R-squared	0.626	0.835	0.897	0.726
Observations	22,329	22,329	22,329	20,912

Note: Standard errors clustered at the location level in parentheses. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level. Tip models have fewer observations due to some locations not tracking tips.

**Table A4: Average Check Time Models**

	(1)	(2)	(3)
Sample:	Restaurant	Worker	Worker
Dependent Variable:	Check Time (Minutes)	Check Time (Minutes)	Check Time (Minutes)
<i>Treated with Rest. Guard</i>	0.327 (0.272)	-0.161 (0.158)	-0.225 (0.162)
Week Fixed Effects	Included	Included	Included
Shift Controls	--	--	Included
Location Fixed Effects	Included	Included	Included
Worker Fixed Effects	--	--	--
Constant	47.348*** (1.104)	50.993*** (0.486)	51.219*** (0.458)
Adjusted R-squared	0.838	0.656	0.483
Observations	22,262	438,679	438,679

Check time is the average length of stay in minutes for tables. Standard errors clustered at the location level in parentheses. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level.

**Table A4: Models Using Left- and Right-Truncated Samples**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Losses	Check Revenue	Drink Revenue	Tip Pct.	Losses	Check Revenue	Drink Revenue	Tip Pct.
Sample Start Week					2660	2660	2660	2660
Sample End Week	2681	2681	2681	2681				
<i>Treated with Rest. Guard</i>	-25.22*** (8.65)	4,621.76*** (619.12)	764.68*** (172.48)	0.0025*** (0.0003)	-17.87** (8.62)	2,804.30*** (650.44)	1329.28*** (185.72)	0.0045*** (0.0005)
Week Fixed Effects	Included	Included	Included	Included	Included	Included	Included	Included
Location Fixed Effects	Included	Included	Included	Included	Included	Included	Included	Included
Constant	119.79*** (4.86)	27,635.63*** (694.29)	6369.74*** (121.07)	0.1708*** (0.0007)	129.41*** (9.43)	42,029.44*** (802.18)	7688.61*** (193.89)	0.1523*** (0.0006)
Adjusted R-squared	0.275	0.832	0.898	0.792	0.163	0.823	0.907	0.706
Observations	10,271	10,271	10,271	9,707	18,584	18,584	18,584	17,376

Note: Columns (1)-(4) do not left-truncate. Columns (5)-(8) do not right truncate. Standard errors clustered at the location level in parentheses. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level. Tip models have fewer observations due to some locations not tracking tips.

**Table A5: Models Using Treatment-Specific Time Trends**

Dependent Variable:	(1) Losses	(2) Check Revenue	(3) Drink Revenue	(4) Tip Pct.
<i>Treated with Rest. Guard</i>	-17.44** (8.86)	6,861.18*** (1566.38)	1932.81*** (625.27)	0.0028*** (0.0008)
Week FE X Treatment	Included	Included	Included	Included
Location Fixed Effects	Included	Included	Included	Included
Constant	122.38*** (3.80)	29,991.54*** (692.93)	7140.51*** (127.81)	0.1474*** (0.0011)
Adjusted R-squared	0.178	0.824	0.893	0.728
Observations	22,329	22,329	22,329	20,912

Note: Standard errors clustered at the location level in parentheses. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level. Tip models have fewer observations due to some locations not tracking tips.

**Table A6: Split Sample Models By Pre-Treatment Theft Observation**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	Known Thief	Unknown	Known Thief	Unknown	Known Thief	Unknown	Known Thief	Unknown
Dependent Variable:	Hourly Losses	Hourly Losses	Hourly Revenue	Hourly Revenue	Hourly Drink Rev	Hourly Drink Rev	Tip Pct.	Tip Pct.
<i>Treated with RestAssure</i>	-0.234*** (0.023)	0.104*** (0.007)	1.876*** (0.724)	4.513*** (0.486)	0.964*** (0.182)	1.028*** (0.121)	0.0028*** (0.006)	.0011** (0.0005)
Shift Controls	Included	Included	Included	Included	Included	Included	Included	Included
Week Fixed Effects	Included	Included	Included	Included	Included	Included	Included	Included
Worker Fixed Effects	Included	Included	Included	Included	Included	Included	Included	Included
Constant	0.218*** (0.017)	0.075*** (0.006)	62.690*** (0.941)	63.641*** (0.986)	12.924*** (0.529)	13.176*** (0.296)	0.154*** (0.002)	0.1587*** (0.0020)
Adjusted R-squared	0.077	0.015	0.657	0.654	0.626	0.618	0.377	0.290
Observations	123,091	177,541	123,091	177,541	123,091	177,541	122,800	177,093

Note: "Known thieves" are designated by observable theft events prior to treatment date. "Unknown" includes everyone else, many of whom may be engaged in theft. Standard errors clustered at the location level in parentheses. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level.

**Table A7: Split Sample Models By Server Gender**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample	Men	Women	Men	Women	Men	Women	Men	Women
Dependent Variable:	Hourly Losses	Hourly Losses	Hourly Revenue	Hourly Revenue	Hourly Drink Rev	Hourly Drink Rev	Tip Pct.	Tip Pct.
<i>Treated with RestAssure</i>	- 0.067*** (0.018)	-0.048*** (0.010)	3.501*** (0.642)	2.288*** (0.577)	0.843*** (0.152)	0.914*** (0.147)	0.0016** (0.0006)	0.0020*** (0.0004)
Week FE	Included	Included	Included	Included	Included	Included	Included	Included
Shift Controls	Included	Included	Included	Included	Included	Included	Included	Included
Worker FE	Included	Included	Included	Included	Included	Included	Included	Included
Constant	0.180*** (0.014)	0.153*** (0.011)	60.492*** (1.352)	60.448*** (0.903)	11.453*** (0.607)	12.227*** (0.437)	0.1540*** (0.0044)	0.1569*** (0.0014)
Adjusted R-squared	0.061	0.066	0.647	0.648	0.614	0.599	0.325	0.332
Observations	153,705	265,609	153,705	265,609	153,705	265,609	153,705	265,609

Note: Standard errors in parentheses. Errors clustered at location level in all models. \* significant at the 10% confidence level, \*\* significant at the 5% confidence level, \*\*\* significant at the 1% confidence level. Tip models have fewer observations due to some locations not tracking tips.