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Ethical Spillovers in Firms: Evidence from Vehicle Emissions Testing

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In this paper, we explore how organizations influence the unethical behavior of their employees. Using a unique data set of over three million vehicle emissions tests, we find strong evidence of ethical spillovers from firms to individuals. When inspectors work across different organizations, they adjust the rate at which they pass vehicles to the norms of those with whom they work. These spillovers are strongest at large facilities and corporate chains, and weakest for the large-volume inspectors. These results are consistent with the economics literature on productivity spillovers from organizations and peers and suggest that managers can influence the ethics of employee behavior through both formal norms and incentives. The results also suggest that employees have persistent ethics that limit the magnitude of this influence. These results imply that if ethical conformity is important to the financial and legal health of the organization, managers must be vigilant in their hiring, training, and monitoring to ensure that employee behavior is consistent with firm objectives.

Key words: peer effects; spillovers; fraud; corruption; productivity; ethics
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1. Introduction

The influence of organizations on individual behavior has been broadly studied in the economics, sociology, and management literatures. Both theoretical work and empirical research have examined how organizations influence the behavior of individual workers through incentives, monitoring, acculturation, and training. The economics and economic sociology literatures have focused primarily on understanding how organizations influence the productivity of individuals as they move across firms (Long and McGinnis 1981, Almeida and Kogut 1990, Song et al. 2003), contract with multiple firms (Huckman and Pisano 2006), or interact with peers (Jones 1990, Kandel and Lazear 1992, Hamilton et al. 2003, Castilla 2005, Mas and Moretti 2008). Yet this literature has failed to examine how organizations influence the ethics of individual behavior.[1] Instead, this topic has been addressed primarily by the business ethics literature through experimental, descriptive, and self-reported data on attitudes, beliefs, and behavior (Sparks and Hunt 1998, Weaver and Trevino 1999, Greenberg 2002).[2] Results from this literature are mixed (O'Fallon and Butterfield 2005), undoubtedly due to the inherent problems in self reports (Schwarz 1999, Bertrand and Mullainathan 2001) and unobservable heterogeneity across individuals and organizations.

Although the economics literature has worked on identifying systematic fraud (Jacob and Levitt 2003, Wolfers 2006, Bertrand et al. 2007), economists have not attempted to measure how the fraudulent behavior of individuals changes with their transfer from one organization to another. This question is particularly interesting in a firm setting because unethical behavior may be deeply influenced by organizational context, in the form of incentives, rules, and culture of the workplace (Tirole 1996, Comer 1998, Ashforth and Anand 2003). Ethical norms that prevail at the organization may also be culturally based, as in Fisman and Miguel's (2007) study of diplomatic parking tickets and Ichino and Maggi's (2000) study of misconduct at an Italian bank. When individuals enter firms with specific ethical norms, individuals may adapt to the ethics of the organization, changing their behavior,

[1] The productivity spillovers observed in Mas and Moretti (2008) involve effort as does the shirking observed in Ichino and Maggi (2000). This shirking, at its nadir, might be considered by some to be "unethical."

[2] See O'Fallon and Butterfield (2005) for a review of the empirical business ethics literature.

attitudes, or both to conform to the new employer. These ethical spillovers from employer to employee may affect the ethics of worker behavior through both organization-level norms and the influence of peers in the same way that organization-specific skills can influence individual performance (Huckman and Pisano 2006) or workers can motivate the improved performance of coworkers (Mas and Moretti 2008). Yet individual ethics, like individual productivity, are partly a function of inherent characteristics born into the worker, inculcated by the family, or developed through a lifetime of education and training. Without observing individuals working across multiple organizations, separating individual ethics from organizational influence has remained elusive in the empirical literature on corruption and ethics.

This paper seeks to isolate these ethical spillovers by applying methodological techniques from the productivity and corruption literature to extensive panel data from the vehicle emissions testing market; a market where widespread anecdotal evidence and state enforcement records demonstrate the potential for fraudulent testing behavior in private firms. Economics studies have already examined the existence and motivation for this fraud. Hubbard (1998) identified the presence of moral hazard in the California vehicle inspection market, where he found that privately owned inspection facilities are markedly more lenient in their passing behavior than are state facilities. Hubbard (2002) showed that financial incentives motivated the private sector to pass vehicles that state-run facilities would have failed, as customers were more likely to return to inspection stations that have previously passed them.[3] In a related paper, we use a more complete market data set to exploit policy shifts in identifying the industry-wide magnitude of fraudulent passing (Pierce and Snyder 2008), and in the present paper seek to better understand the distribution of this unethical behavior through the influence of organizational norms on individual inspector behavior. The results from these papers suggest that if industry-wide pass rates reflect considerable fraud, facilities, and inspectors with inexplicably high pass rates, will on average reflect localized fraudulent behavior. Throughout this paper, we define this act of fraudulently passing a polluting vehicle as unethical, consistent with the Jones (1991) definition of unethical behavior as "either illegal or morally unacceptable to the larger community" (p. 367). The ethics of emissions testing fraud seems clear: this behavior is not only illegal, but is socially harmful as well. Mobile vehicle emissions have been repeatedly shown to aggravate respiratory problems, particularly in children,[4] and are partly responsible for acid rain and other environmental problems.[5]

Using a database of over three million emissions tests from a Northeastern metropolitan area in 2003–2004, we find strong evidence that statistically significant firm-specific differences in pass rates abound throughout the market. Given our ability to control for numerous vehicle characteristics, these differences reflect likely levels of fraud, where some privately owned facilities are much more likely to pass a vehicle than others. In examining the relationship between the pass rates of organizations and inspectors, we find strong "ethical" spillovers from organization to individual. When individuals work across different facilities, their behavior conforms to that of the facility that employs them. Our results suggest inspector behavior converges toward the norms of the employer nearly immediately, with little lag or gradual adaptation. Our work makes an important contribution to the literature on productivity and corruption by showing that unethical behavior and corruption can spill over from organizations to individuals in the same way as other dimensions of productivity. Although the literature on business ethics has extensively discussed this effect, empirical work has almost exclusively used case studies or small sample surveys, which are vulnerable to issues of selection bias and self-report bias.[6] Because our data represent the entire population from a state-designated market, this work provides a significant complement to this literature by being the first work to identify organizational influence on behavior for an entire market.

The remainder of this paper proceeds as follows: Section 2 describes the vehicle emissions testing procedure and the institutional details that lead to fraud. Section 3 discusses how organizations can influence the ethics of employee behavior. Section 4 introduces the data. Section 5 identifies ethical spillovers from organizations. Section 6 discusses the results, their implications, and our conclusions.

[3] Similar evidence has been found in other industries, such as in medical care (Gruber and Owings 1999) and automotive repair (Taylor 1995).

[4] For extrapolation on this, see Committee on Vehicle Emission Inspection and Maintenance Programs, National Research Council (2001).

[5] The use of the term "unethical behavior" in this paper will always refer to fraudulent testing. We do not contest that the determination of behavior as unethical can be a complicated analysis, and that there are likely some cases under which emissions fraud might qualify as "ethical." Differentiating these cases, though important, would require better understanding of the vehicle owners, which is not currently available in our data.

[6] See O'Fallon and Butterfield (2005) for an extensive review of this literature. See Bertand and Mullainathan (2001) for a discussion on self-report bias.

2. Market for Emissions Testing Fraud

The vehicle emissions testing market has great potential for unethical behavior. Inspectors are legally required to follow strict testing procedures, but they have numerous opportunities to diverge from this course for financial gain. With the dynamometer-based tailpipe testing still common in many areas, skilled mechanics can make nearly all vehicles pass through a number of temporary mechanical adjustments that do not address the underlying causes of the excess pollution. Even the worst cars can be certified clean though substituting other cars during the testing procedure. Not only do inspectors have opportunities to cheat, they will often have strong incentives. As Hubbard (2002) addressed in California, reputation, repeat business, and other repairs all provide incentives in certain facilities. Outright bribes and shopping around by customers can further motivate inspectors to help customers pass with gross polluting vehicles. Emissions fraud is believed to be widespread beyond California. A Massachusetts' study found that vehicles retested by the state had substantially higher levels of emissions (Massachusetts Office of the Inspector General 2003).

This illegal behavior by inspectors has clear costs for society, increasing air pollution in urban areas. The three tested pollutants, CO, HC, and NOx, all have proven health consequences. Carbon monoxide, an odorless, poisonous gas, inhibits the transport of oxygen from blood into tissues, and can cause general difficulties in the cardiovascular and neural systems. When combined in the presence of sunlight, HC and NOx form ground-level ozone that can aggravate respiratory problems, especially in children, and may cause permanent damage to lung tissue. The health cost of vehicle emissions has been estimated as between \$4.3 billion and \$93 billion in 1985 by the American Lung Association, which also attributed 120,000 deaths to general air pollution. A 10-year study of children conducted by the University of Southern California found evidence linking air pollution to reduced lung function growth, higher absenteeism from respiratory problems, and asthma exacerbation and development (Gauderman et al. 2000).

3. Theoretical Implications

The observed unethical behavior of individual workers can come from several sources. Underlying all unethical behavior are the personal morals and ethics of the individual. Personal ethics may be highly idiosyncratic to the individual, stemming from religious background, age, gender, culture, or educational background (Ford and Richardson 1994, Loe et al. 2000). In the case of vehicle emissions testing, individual inspector ethics may stem from attitudes regarding government regulation, honesty, the environment, or legal compliance. Certain inspectors may have no personal conflict with cheating due to these attitudes, and given any incentives to cheat, may seize the opportunity. Inspectors may even cheat out of a sense of moral responsibility to help friends and families, which they might value over a public health cost that is harder for them to conceptualize. For these inspectors, saving a friend \$500 in repairs is well worth the small marginal contribution of that friend's 1981 Chevrolet Camaro to childhood asthma. In this paper, we consider these ethics as an individual inspector "fixed effect" that is persistent throughout different incentive structures and employment situations.

The observed level of inspector cheating cannot be wholly explained by their personal ethics, as this behavior will be greatly influenced by other factors. Personal incentives, whether financial or otherwise, will drive decisions to stretch rules and regulations. These incentives may come from organization-level factors, such as the nature of their employment contract. Financial incentives or organizational goals (Schweitzer et al. 2004) can drive individuals toward or away from unethical behavior, as can a broad range of rewards and punishments (Trevino and Youngblood 1990, Tenbrunsel 1998). Sole proprietors or small partnerships will also tend to have strong incentives to help customers pass in order to generate future revenues, while these incentives may be much weaker for hourly employees in large chains. This explanation is consistent with Hubbard (1998), who models inspector utility as entirely driven by income and effort. In a principal-agent context, the inspector must deal with three potential principals: the organization that pays their wage, the consumer that pays them either explicitly or through some implicit relationship, and the regulating agency which enforces the lawful testing procedures.

Another major organization-level influence on the ethical conduct of inspectors could be the ethical context of the organizations in which they work. This may be categorized as the ethical climate (Trevino 1986, Tetlock 1992, Vidaver-Cohen 1998, Schminke et al. 2005), where the individual's personal ethics are constrained or altered by the norms and routines of the organization.[7] This influence may come from organizational leadership (Trevino and Brown 2004, Brown et al. 2005) or from more diffuse climates (Victor and Cullen 1988) or subclimates (Weber 1995).

[7] There is a much more extensive literature on influences and mechanisms of unethical behavior. For a comprehensive review, see Trevino et al. (2006).

Social pressure within the organization, whether formal or informal, might strongly influence the behavior of an individual inspector toward the norms of the firm. This process may ultimately lead toward what Ashforth and Anand (2003) refer to as "the normalization of corruption," where corruptive behavior "becomes embedded in organizational structures and processes, internalized by organizational members as permissible and even desirable behavior, and passed on to successive generations of members" (p. 3). Additionally, local or national culture associated with the organization may drive individual corruption (Fisman and Miguel 2007).

Although we may observe the influence on ethical conduct at the organizational level, this influence may also come at a more micro-level, from peers working with the inspector. Inspectors may personally influence the behavior of other employees therein, independent of organizational norms. An unethical inspector may transmit know-how on cheating, or provide peer pressure to influence existing employees into fraudulent testing. Similarly, a highly ethical inspector may provide some oversight against cheating. As in Mas and Moretti (2008), peer influence from other individuals working at the same time as the inspector may influence that person's decision to engage in fraud. The disutility suffered by workers from shame or more substantive punishment may encourage individuals to veer from their personal ethics toward those of workers observing them. This effect is consistent with the ethics literature, where social networks (Brass et al. 1998) and moral approval (Jones and Ryan 1997) play important roles in determining unethical behavior. Additionally, experimental work has shown the significance of peer effects in ethical behavior (Jones and Kavanagh 1996, Beams et al. 2003).

Explanations from both the sociology and economics literatures suggest that the relative ethicality of individual behavior will be influenced by the context of the organization, whether from organization-level norms, incentives and constraints, or from social pressure from peers. In the context of emissions testing, an inspector's decision to cheat should be influenced by levels of unethical behavior in her current employer. We therefore expect that an inspector's propensity to pass a vehicle should be influenced by the observed leniency of the facility at which she works.

4. Data

Our data set comes from the department of motor vehicles of a large northern state. It contains all vehicle inspections conducted in 2003 and 2004 for gasoline-powered vehicles under 8,500 lbs, and includes vehicles owned by individuals, corporations, fleets, and government agencies. Only those vehicles in dense urban areas are included, because these are the only vehicles that require testing. The data at the inspection level include the inspection date, the inspection time, vehicle identification number, facility identifiers, inspector identifiers, and inspection results. These data allow one to uniquely identify vehicles, including all characteristics such as make, model, model year, and odometer reading. Additionally, the name and address of the inspection station is known, as well as the date and time on which the test occurred. Finally, we can observe which inspector conducted the test through unique inspector IDs, although we do not know their names. Since we know exactly when and where the inspection took place, this allows us to follow the careers of inspectors as they change employment from one facility to another.

5. Empirical Approach and Results

Our empirical approach is designed to identify how the ethical behavior of an organization influences individual behavior after hiring. In the context of vehicle emissions testing, this involves identifying how pass rates at the testing facility influence the leniency of an employee. Our identification strategy relies on using inspector fixed effects to control for inspector invariant characteristics and then to observe how moving across organizational environments impacts their behavior. To estimate the impact of facility stringency on inspector behavior we use a two-step estimation procedure on a sample of inspectors who work at multiple stations.[8] In the first step we identify firm-specific pass rates for all facilities at which our focal group worked by using the population of all other inspectors employed there, absent the focal inspectors. This gives us firm-based measures of leniency not directly influenced by the focal inspectors.[9] We then employ these firm-based measures in a second step that uses the focal sample of multiple-facility inspectors, where we identify how firm leniency spills over to the behavior of the focal inspectors. In stage two, the effect of the firm measures estimated in stage one on the behavior of the focal inspectors identifies ethical spillovers.

5.1. Identifying Facility Fixed Effects

To identify firm-specific pass rates, we first select a sample of inspectors who either switch employers

[8] It is important to note that we cannot identify these spillovers for individuals working at only one facility during our time period, since these effects would be captured by their fixed effect. This of course limits what we can say about "nonswitchers."

[9] See our discussion of Manski's (1993) reflection problem later in the paper.

during the sample period or who work at multiple facilities concurrently.[10] We first estimate facility fixed effects for all firms at which these focal inspectors work, using only inspectors by their coworkers. For example, in the first stage, if Dan worked at station X and station Y, we estimate the fixed effects of station X and station Y by estimating all inspections *not performed by Dan*. These fixed effects are estimated simultaneously with all other facilities so as to accurately estimate control parameters. Because focal inspectors like Dan are not used in the first stage, the sample includes only those inspectors who work at a single facility. We believe this provides the cleanest approach to estimating the true facility culture because it avoids focal inspector observations estimating the facility fixed effects that would then be used to predict these same observations in the second stage.

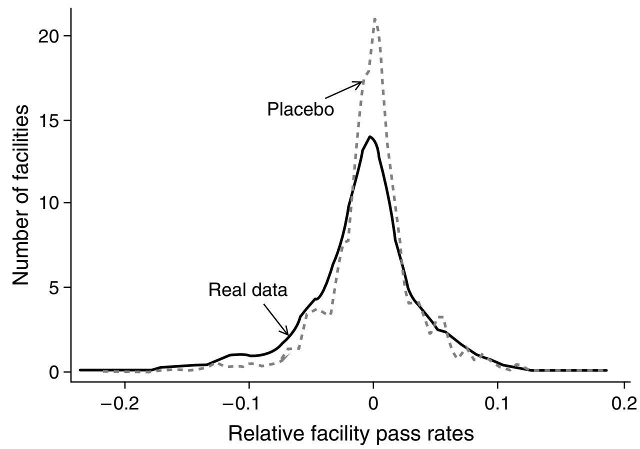
Our first stage specification at the vehicle inspection level is as follows:

$$Pass_{i,t} = \sum_{j=1}^{N} \beta_j FacilityEffect_j + \lambda InspectionControls_i + \varepsilon_{i,t}. \tag{1}$$

For each inspection i we estimate a linear pass probability based on the facility where the inspection is performed, cubic odometer controls, make/model group controls, manufacturer year dummies, month effects, and region controls at the level of the three-digit zip code.[11] Although including all observations in the specification would be ideal, this approach is computationally infeasible. In addition to including all observations from facilities employing individuals who work at more than one facility, we therefore include a 10% sample of the data from all other facilities to precisely estimate control variables. This sample ensures that our control variables reflect the influence of time and car characteristics on all cars, not just those from facilities relevant to our estimation of spillovers. This sampling decreases selection bias in control estimation, and reduces the risk that omitted variable drives our results. In practice, including this sample yields small differences in our final results.[12]

From our first stage regression we obtain predicted estimates of the facility fixed effects. The distribution

Figure 1 **Kernel Density Plots of Relative Facility Pass Rates: Placebo vs. Real Data**



of these estimates can be seen in Figure 1, labeled "real data." These fixed effects represent risk-adjusted pass rates that control for each facility's unique portfolio of cars, similar to productivity measures in Mas and Moretti (2008) and health care economics (Huckman and Pisano 2006).[13] Before identifying ethical spillovers, we first look to establish that fraud is not randomly and equally distributed across the industry. To test for the importance of organizations in this fraud, we seek to compare the patterns of organizational pass rates identified through facility fixed effects with a simulated, random assignment of tests to facilities. We randomly assign inspections to each facility in the population, while keeping their portfolio size equivalent to the real data. This involves creating a placebo distribution to compare with the real data. To understand this construction, consider Firm X, which tests 2,000 vehicles in our data set. To generate the placebo distribution in Figure 1 we replace the 2,000 cars that Firm X actually tested with 2,000 randomly selected vehicles from the entire population.[14] We then estimate the fixed effect for firm X and likewise for all other firms in the data set using this new, "randomly assigned" data.[15] From this procedure we generate the placebo distribution of facility fixed effects.

To understand the reasons for this method, consider the following hypothetical case. Suppose that there were complete homogeneity in the behavior of all facilities. The average facility fixed effect is −0.011.

[10] In order for an inspector to be working at a facility, we require that they inspect 50 or more vehicles at that facility. We restrict the sample to switchers in certain specifications.

[11] For computational reasons, we keep these controls used to estimate the first stage consistent throughout all of the regressions. When we vary the use of controls in the second stage regressions this does not mean the controls are being varied in the first stage.

[12] Models excluding the 10% sample change our coefficients in Table 3 by an average of less than 1%, and do not reduce statistical significance despite the lower observation count of $n = 426{,}922$.

[13] In a previous version of this paper, we used a similar methodology to Huckman and Pisano (2006), and found similar results. For the revision, we adapted Mas and Moretti's (2008) fixed-effect approach due to its superior robustness to selection biases.

[14] This assignment process is done without replacement.

[15] We run this procedure 25 times with similar results. For every run of the data we find that the standard deviation in the real data exceeds the standard deviation in the placebo data.

Because there is some inherent noise in the data, however, in some facilities we would observe the failure gap to be above −0.011 and in others below −0.011. This is a natural consequence of finite sample statistics. To truly identify facility-specific effects, we need to show that there are actual differences in facility fraud that extend beyond the expected noise in the data. This simulation allows us to create a counterfactual null-hypothesis about the random distribution of firm pass rates, and then reject it based on the real effects estimated in our specification.

In the placebo distribution in Figure 1 we observe that the variance is significantly less than the variance in the real data. Figure 1 presents both the first placebo and real distributions with a much tighter distribution in the placebo data. Thirty-three percent of the facility fixed effects in the real data lay outside one standard deviation in the placebo distribution, demonstrating that the facilities at the ends of the distribution of the real data are not there due to random chance, but due to actual differences in firms' behavior. The interpretation of those facilities in the "fat tail" on the right side of the distribution is straightforward; they represent highly unethical organizations. It is important to note, however, that a fixed effect equal to −0.011 represents behavior consistent with the industry average, which in this industry represents minor to moderate levels of fraud. Consequently, many of the facilities in the "fat tail" on the left side of the distribution may represent those firms with complete legal compliance, with their pass rates reflecting the real emissions of the cars they test. Additionally, some facilities on the left side may be engaging in the fraudulent *failing* of cars in order to increase repair business on transient customers (Taylor 1995).[16]

5.2. Identifying Ethical Spillovers

There is considerable heterogeneity in the unethical practices of facilities, but our primary concern is in understanding how these ethical norms influence individual behavior. The second stage of our analysis therefore involves identifying whether the firm-level differences in pass rates in step one influence the inspector's behavior. If an inspector moves from a facility that, all else equal, has a low passing rate to a station with a high passing rate, we estimate how this employment change impacts inspector behavior. The advantage of looking at those inspectors who switch stations rather than a purely cross sectional approach is that we condition on inspector fixed effects. Without inspector fixed effects, correlations between inspector and firm behavior could be explained purely through a selection effect, where ethical inspectors choose ethical employers and vice versa. While this matching process is certainly important both in the literature and in practice, we are focused here on the treatment effect of ethical spillovers.

The facility fixed effect estimated in the first stage becomes our independent variable *FacilityLeniency* for the second stage.[17] As facility leniency increases (i.e., they are more likely to pass a vehicle conditional on vehicle characteristics) *FacilityLeniency* increases. We then estimate the spillover effects *on the sample of switchers and those working at multiple employers*. In this regression the sample is distinct from the sample used in the first stage. The only common observations are the 10% subsample that comes from facilities that employ the inspectors of interest. Again our results are robust to the exclusion of this group, however their inclusion aids in precisely estimating the inspection controls. The second stage is given by the following specification:[18]

$$Pass_{i,t} = \alpha FacilityLeniency_i + \sum_{j=1}^{N} \beta_j InspectorEffect_j + \lambda InspectionControls_i + \varepsilon_{i,t}. \tag{2}$$

A positive coefficient on *FacilityLeniency* implies that as an inspector moves across facilities, her pass rate converges toward that of the current employer. We include the control variables used in the first stage plus some additional variables. The first addition addresses a significant concern in specification (2) in that *FacilityLeniency* is picking up mechanical variation in the testing procedure rather than social differences across facilities. For example, the *FacilityLeniency* variable might simply reflect mechanical differences in the quality of facilities' testing equipment. We attempt to control for this by creating an equipment quality variable by using only those cars manufactured after 2001. In our data this subsample very rarely fails an inspection. We measure the

[16] Each car must also pass a safety inspection as well. Because safety inspections are primarily visual, they provide better opportunities for inspectors to fraudulently fail a vehicle. It is much more difficult to fraudulently fail an emissions test than to pass it, because contaminants must be introduced into the system. For facilities with transient customers, however, incentives for fraudulent failures may induce such behavior.

[17] We use the term "leniency" here to reflect high risk-adjusted pass rates. Given our extensive vehicle, geographic, and equipment quality controls, high values are likely fraud, although not all these fixed effects are significant enough to raise accusations of wrongdoing by specific facilities.

[18] A previous version of this paper attempted to include same-day peer effects, which were concurrently estimated with organizational effects. With the help of two anonymous referees, we concluded that small firm size and high collinearity made this distinction impossible, and that this joint identification requires data with both large within-firm and across-firm variation.

quality of equipment at a given facility by examining the carbon monoxide readings on this subsample of cars, data which are minimally contaminated by fraud due to the almost certainty of legitimately passing. Facilities that have relatively higher carbon monoxide readings from this subsample are assumed to have "stricter" machines. To estimate this effect we use the following regression, where the *FacilityEffect* coefficients become our control variable *EquipmentQuality*.

$$CarbonMonoxide_{i,t} = \sum_{j=1}^{N} \beta_j FacilityEffect_j + \lambda InspectionControls_i + \varepsilon_{i,t} \quad (3)$$

All of our standard errors are clustered at the inspector level to address issues of correlation of the error terms within inspectors, which is a conservative error estimation procedure. We find minimal differences in results when we alternatively cluster at the facility level and implement two-way clustering on both facility and inspector (Cameron et al. 2006).

Our methodology does suffer from several common problems in the study of spillovers and peer effects. First, we acknowledge that switching between facilities is an endogenous process, where the choice to switch and the new employer are not randomly assigned. Second, we acknowledge that our study suffers from Manski's (1993) reflection problem, where identification of a group's influence on individuals is confounded by possible exogenous determinants of performance or correlations in unobserved individual characteristics. Although these problems are reduced by our identification of how *permanent* firm characteristics affects *current* inspector behavior, our problem is larger than in some studies (Mas and Moretti 2008) due to our small firm size. The endogenous switching process may exacerbate this problem if a homophilic sorting process clusters like-minded inspectors in certain facilities. Finally, we cannot eliminate the possibility that individuals are influencing organizations, which suggests that causality may go both ways. Although our facility fixed effect is estimated including time without the inspector, any reverse causality would suggest we are overestimating the spillover coefficient. As we will present later, however, we find larger spillovers at larger organizations and on individuals who conduct fewer tests, two groups where the influence of individuals on facilities would be smallest. This suggests that our findings are strongest in cases where reverse causality would be less of a problem. Regardless, we caution the reader that this identification strategy cannot meet the standard of a randomized experiment. Absent a randomized experiment, we cannot make a causal claim with absolute certainty.

Table 1 Summary Statistics

	Observations	Mean	Min.	Max.
Population: All inspectors				
Pass rate	14,766	0.935	0	1
Number of jobs	14,766	1.085	1	6
Number of inspections	14,766	310.664	1	5,833
Sample: Inspectors with multiple employers				
Pass rate	1,104	0.933	0.73	1
Number of jobs	1,104	2.131	2	6
Number of inspections	1,104	524.23	102	4,291
Average tenure at job in days	1,104	317.704	35.5	724
Sample: Facilities where inspectors work across multiple firms				
Average number of inspections per day	523,153	3.206	1	33
Average number of inspectors present per day	523,153	1.58	1	20

5.3. Spillover Results

Table 1 presents the summary statistics germane to the results. Our sample of 1,104 inspectors with multiple employers conducts 868,071 automobile inspections with an overall pass rate of 93%. This pass rate is nearly identical to the 14,766 inspectors in the entire population. The average number of jobs for our sample is 2.1, with an average inspection total of 524. Each job lasts 317 days. Additionally, each facility in our sample averages 3.2 inspections per day, with 1.6 inspectors per facility working each day. Nearly half of all facility days in our sample involve only one inspector.[19] In unreported results, as one would expect, pass rates are strongly negatively correlated with vehicle age and odometer readings.

Table 2 presents the spillover results for ordinary least squares (OLS) and probit specifications with multiple variations. Overall our results are quite consistent and robust to changes in the specification. In column (1), where only the inspector fixed effects were included, the impact of the change in facilities positively and significantly impacted an inspector's pass rate. The interpretation of the coefficient 0.178 indicates that if an inspector moves from one facility to a second facility with a pass rate 10% higher than the first, there will be a 1.78% increase in the inspector's pass rate. This estimate is quite consistent with estimates of productivity spillovers in prior literature, where Mas and Moretti (2008) and Falk and Ichino (2006) find peer spillovers of 15% and

[19] Inspectors inspecting less than 100 cars a year and less than 50 cars at any given facility were thrown out because that leads to the risk-adjustment being imprecisely estimated. Our results are robust to changes in this threshold.

Table 2 Impact of Facility Pass Rate on Inspector Pass Rate (Everyone)

	(1) Pass	(2) Pass	(3) Pass	(4) Pass	(5) Pass	(6) Pass	(7) Pass	(8) Pass
FacilityLeniency	0.178 (0.051)***	0.193 (0.054)***	0.177 (0.053)***	0.197 (0.055)***	0.186 (0.053)***	0.186 (0.044)***	0.139 (0.045)***	0.105 (0.035)***
Odometer controls	N	Y	Y	Y	Y	Y	N	Y
Make year dummies	N	Y	Y	Y	Y	Y	N	Y
Five-digit zip code effects	N	N	Y	N	N	N	N	N
Three-digit zip code effects	N	N	N	Y	Y	Y	N	Y
Model group effects	N	N	N	Y	Y	Y	N	Y
Month effects	N	N	N	Y	Y	Y	N	Y
Equipment quality control	N	N	N	N	Y	Y	N	Y
Inspector fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Standard error cluster	Inspector	Inspector	Inspector	Inspector	Inspector	Facility	Inspector	Inspector
Specification	OLS	OLS	OLS	OLS	OLS	OLS	Probit	Probit
Observations	868,017	868,017	868,017	868,017	868,017	868,017	866,575	860,140

Notes. Standard errors clustered at the inspector or facility level. For probit specifications, marginal effects at the average values are taken.
*, **, *** indicate significance at the 10%, 5%, and 1% confidence level, respectively.

14%, respectively. In column (2) we condition on time effects and control for the odometer reading and the squared odometer reading. We find that the precision of the estimate increases as well as the magnitude. In column (3) we add dummies for the year of production and make (e.g., Honda, Toyota, etc.) and for the five-digit zip code. These additions, intended to control for vehicle and geographic-based variation, do not significantly alter the results. Column (4) adds car model and month dummies, whereas column (5) adds our equipment quality control variable.[20] These additions do not significantly alter our estimated spillover effect. The consistent estimates on *FacilityLeniency* suggests that an omitted variables bias is likely to be a small problem, because as we include known variables that ought to confound the results, the identification only improves. The models in columns (1)–(5) all correct for clustering at the inspector level, but for robustness column (6) clusters at the facility level, the broadest level of aggregation in the data. This change reduces the standard error, suggesting that correlations within inspectors are more important than within facilities.

There are obvious concerns with using an OLS specification for a discrete choice model, given that the errors are not normally distributed. We therefore present a probit model with calculated marginal effects.[21] There is a persistent problem, however, when analyzing discrete choice data in a panel setting—the "incidental parameters problem." In panel settings where the within sample is small, nonlinear models are with few exceptions inconsistent and biased estimates of the true parameter. This is a major hurdle for studying nonlinear panel data. The conventional resolution of this problem is to use both OLS, where the estimator is unbiased and consistent, and where possible apply nonlinear methods that account for the discrete choice structure of the data. If comparisons between the two classes of models are not similar, this leads the researcher to worry that any one of the problems is driving their results. Unfortunately, more elaborate statistical solutions to this problem are still underdeveloped.[22] Columns (7) and (8) present results from the unconditional fixed-effect probit model. The coefficient for *FacilityLeniency* remains positive and significant, although now somewhat smaller. Our results are consistent across specifications.

Table 3 reports the results from the OLS and probit models using only the sample of inspectors who sequentially change employment. That is, we now exclude inspectors who work at multiple facilities simultaneously. The economics literature on free agency suggests there may be fundamental differences between workers who simultaneously hold multiple jobs and those who serially switch from one to another (Cymrot and Dunlevy 1987, MacDonald and Reynolds 1994). This removes approximately 124,000 inspections from our model. The results are consistent with those from the larger sample across columns (1)–(8).

The average effects in Tables 2 and 3 mask several important sources of firm heterogeneity. First,

[20] For columns (4) and (5) we switch from five-digit to three-digit zip code dummies due to reduced within variation.

[21] We implement the probit model with unconditional inspector fixed effects. There are concerns with using unconditional fixed effects when the within cell-size is less than 16 (Katz 2001). Because the average number of observations within each inspector is at least 50, conditional inspector fixed effects offer little benefit here. Furthermore, conditional probit models with marginal coefficients are extremely difficult to implement and computationally prohibitive here for large samples.

[22] For a detailed description of this problem, see Lancaster (2000).

Table 3 Impact of Facility Pass Rate on Inspector Pass Rate (Switcher Sample)

	(1) Pass	(2) Pass	(3) Pass	(4) Pass	(5) Pass	(6) Pass	(7) Pass	(8) Pass
FacilityLeniency	0.188 (0.071)***	0.212 (0.076)***	0.185 (0.072)**	0.214 (0.077)***	0.197 (0.073)***	0.197 (0.060)***	0.154 (0.066)**	0.117 (0.050)**
Odometer controls	N	Y	Y	Y	Y	Y	N	Y
Make year dummies	N	Y	Y	Y	Y	Y	N	Y
Five-digit zip code effects	N	N	Y	N	N	N	N	N
Three-digit zip code effects	N	N	N	Y	Y	Y	N	Y
Model group effects	N	N	N	Y	Y	Y	N	Y
Month effects	N	N	N	Y	Y	Y	N	Y
Equipment quality control	N	N	N	N	Y	Y	N	Y
Inspector fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Standard error cluster	Inspector	Inspector	Inspector	Inspector	Inspector	Facility	Inspector	Inspector
Specification	OLS	OLS	OLS	OLS	OLS	OLS	Probit	Probit
Observations	744,095	744,095	744,095	744,095	744,095	744,095	742,873	736,631

Notes. Standard errors clustered at the inspector or facility level. For probit specifications, marginal effects at the average values are taken.
*, **, *** indicate significance at the 10%, 5%, and 1% confidence level, respectively.

we might be interested in the difference in spillover magnitude when an inspector moves to a small facility versus when they join a large facility. Second, we might be interested in how the organizational form influences the magnitude of the spillover. Do corporate chains exert more influence on inspectors than do single-facility firms? Third, are high-volume inspectors more resistant to organizational influence? To explore these issues we alternately include dummy variables for large firms, high-volume inspectors, and corporate chains. We also interact these with *FacilityLeniency* to measure differential spillovers across facility and inspector types. We use only OLS models due to inherent problems with interaction terms in nonlinear models. Table 4 presents the results for these models.

Columns (1) and (2) present spillover results across firm size. We create a dummy variable equal to one if the facility performs more than 1,000 inspections over the two years in the data. The coefficient for the *FacilityLeniency* $*$ *Large* interaction term is positive and significant, suggesting that ethical spillovers are much stronger at large facilities than at small facilities. When an inspector moves into a smaller station, the impact of the spillover in column (2) is 0.118, which is less than 30% of the impact when they move into a larger station. The difference between these coefficients is significant at the 1% confidence level. Although we cannot point to a specific mechanism for this effect, this evidence suggests that large organizations exert a more powerful influence on employee behavior, perhaps through more formalized rules. This result also may stem from better monitoring in large facilities, due to multiple coworkers working in proximity to the inspector. Columns (3) and (4) indicate that inspectors who conduct the most inspections are much less influenced by their organizational norms. We designate a large inspector as one who performs more than 1,000 inspections during the two-year sample. Although the spillover for "small" inspectors is 0.36, it is 0.27 less for "large" inspectors, a difference statistically significant at the 1% level.[23] Corporate chains appear to have a much stronger spillover effect than single-facility firms in columns (7) and (8), with nearly double the magnitude of coefficient. This suggests that either more formal procedures or better monitoring in corporate chains force employees to conform to organizational norms, although we are cautious in our interpretation. An alternative explanation could involve nonconforming inspectors selecting into independent facilities.[24]

Finally, we examined whether or not the organizational spillover involved an acculturation process similar to what was observed in Fisman and Miguel (2007). If this were the case, we should observe the magnitude of the spillover increasing over time. Unfortunately, our data are left-censored, which prevents us from observing the true cumulative employment time for each inspector's first job. Consequently, we estimate the magnitude of the spillover for the first month of each subsequent job differenced against the observed spillovers at all other times. This is accomplished by creating a dummy variable equal to one for each inspection conducted in the first month of all facilities after the first employer. We then interacted *FacilityLeniency* with the first month dummy to observe the difference between spillovers in this first month and at all other times, including

[23] An alternative model identified large inspectors by their share of inspections at a facility. This produced very similar results.

[24] Ideally, we would jointly estimate both facility and chain fixed effects. Similar to a similar specification with peer effects, the lack of variation within chains makes this specification infeasible.

Table 4 Impact of Facility Pass Rate on Inspector Pass Rate (Switchers)

	(1) Pass	(2) Pass	(3) Pass	(4) Pass	(5) Pass	(6) Pass	(7) Pass	(8) Pass
FacilityLeniency	0.119	0.118	0.357	0.360	0.185	0.197	0.182	0.190
	(0.065)*	(0.061)**	(0.046)***	(0.044)***	(0.074)**	(0.075)***	(0.072)**	(0.073)***
Large facility	0.007	0.008						
	(0.003)***	(0.002)***						
*FacilityLeniency * Large facility*	0.231	0.277						
	(0.088)***	(0.087)***						
Large inspector			Absorbed	Absorbed				
*FacilityLeniency * Large inspector*			−0.280	−0.265				
			(0.072)***	(0.074)***				
First month					−0.005	−0.006		
					(0.002)**	(0.002)***		
*FacilityLeniency * First month*					0.039	0.007		
					(0.037)	(0.03)		
Chain							−0.007	−0.012
							(0.002)***	(0.002)***
*FacilityLeniency * Chain*							0.174	0.186
							(0.131)	(0.132)
Odometer controls	N	Y	N	Y	N	Y	N	Y
Make year dummies	N	Y	N	Y	N	Y	N	Y
Three-digit zip code effects	N	Y	N	Y	N	Y	N	Y
Model group effects	N	Y	N	Y	N	Y	N	Y
Month effects	N	Y	N	Y	N	Y	N	Y
Equipment quality control	N	Y	N	Y	N	Y	N	Y
Inspector fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
Standard error cluster	Inspector	Inspector	Inspector	Inspector	Inspector	Inspector	Inspector	Facility
Specification	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Observations	744,095	744,095	744,095	744,095	744,095	744,095	744,095	744,095

Note. Standard errors clustered at the inspector or facility level.
*, **, *** indicate significance at the 10%, 5%, and 1% confidence level, respectively.

inspections conducted at the first facility. Including these first facility inspections allows us to continue to difference off inspector fixed effects. Columns (5) and (6) present these results, which show no statistically significant effect, particularly when control variables are added to the model. Alternative time cutpoints show similar results. Similarly, replacing the first month dummy with a logged time variable produced no statistically significant time effects, and precluded us from using the left-censored first jobs. Although it appears from our data that these spillovers occur almost immediately, we are cautious in dismissing acculturation effects. Accurately identifying this effect would require a large sample of inspectors with two or more noncensored employments.

6. Discussion and Conclusion

The results from our models strongly suggest that individual ethical behavior is influenced by the ethics of the employer. When individuals move from one facility to another, their leniency appears to shift toward that of their employer. This heterogeneous leniency is consistent with fraud in the emissions testing market, and the organizational norms of the facility appear to influence levels of this unethical behavior in individual inspectors. Our results appear to be robust to geographic and firm quality controls, but suffer from endogenous selection and the reflection problem common in the peer-effect literature. These problems weaken our ability to definitely establish causality. Furthermore, we cannot identify whether this effect stems from firm-level policies regarding cheating, the norms of coworkers, or some sort of financial incentive. Our results are consistent with all three explanations, and the observations in the data are likely a combination of the three. Finally, we must be cautious about our ability to extrapolate our findings to the entire population of inspectors. Given the shortness of our panel, the majority of inspectors do not switch jobs, and we are unable to identify if our point estimates on spillovers would be accurate for this subsample as well.

How do we interpret our findings? As we have discussed throughout this paper, there are a number of theories that can help explain our empirical observations of ethical spillovers. The economics literature has shown that both financial incentives

(Hubbard 1998, 2002) and peer pressure (Mas and Moretti 2008) can influence how individuals behave within organizations. When individuals move from one organization to another, the new financial incentives or social pressure from other employees can motivate employees to conform their behavior to those of their peers. This conformity may also come from enforcement of strict organizational codes of conduct that penalize individuals who stray from accepted norms. The sociology literature (Tetlock 1992) also contributes an understanding of how the ethical norms of an organization can influence the choices of the individual through individual accountability. Psychology-based work on organizations has explained this with job satisfaction and organizational commitment (O'Reilly et al. 1991, Williams and Hazer 1986). This conformity may also stem from perception of what is "unethical" to the new individual. Gradual exposure to unethical emissions testing may shift the new inspector's perception of what is ethical, similar to evidence in Fisman and Miguel (2007) and findings from experimental studies (Cain et al. 2005). This process may affect only the individual or the entire organization through normalization of corruption (Ashforth and Anand 2003). Our analysis finds that these ethical spillovers occur nearly immediately, and we are unable to find any gradual acculturation effect in new employees. This suggests to us that we are primarily observing organizational influence on ethical *behavior* rather than fundamental shifts in employee ethical *beliefs*.

Although we have been able to identify organization-level spillovers, we are not able in this paper to parse out all the exact mechanisms through which these spillovers occur. Financial incentives likely play some role, due to anecdotal evidence on the limited existence of inspector bribes and the more convincing evidence of firm benefits from emissions fraud (Hubbard 1998). Similarly, social pressure, whether described in terms of individual utility theory or accountability, likely influences a number of inspectors to conform not only to their peers but also to their employers' ethical norms. We believe that our results reflect a combination of all these organization-level forces, and that further separating them requires detailed organizational and customer data. Recent work by Pinto et al. (2008) presses for the importance of further segmenting corrupt organizations, arguing that there are fundamental differences between "corrupt organizations" and "organizations of corrupt individuals." The incentives, constraints, and influences for individuals and organizations are at the heart of this distinction, and the vehicle emissions testing market undoubtedly includes some of both types.

It is important to note that many of the organizational spillovers that we observe may in fact come from peer effects. The average number of inspectors in each facility is less than two, which suggests that for many of the facilities the "organization" may reflect two individuals. Firm size is likely larger than this, given that most of these facilities engage in numerous other activities, including gasoline sales, service, safety inspections, and repairs. But the small firm size blurs the distinction between the peer effects in Mas and Moretti (2008) and the organizational effects in Huckman and Pisano (2006). It was our original intent to empirically isolate these two influences by exploring which inspectors worked together on the same day, but the small firm size and collinearity limited our ability to cleanly separate them. Our attempts at this suggest, however, that similar methodology with larger firms might allow for simultaneous estimation of firm and peer spillovers.

We believe this paper makes a significant contribution to the understanding of how the interaction between individual employees and the organizations that hire them influences their choices between ethical and unethical behavior. Our unique data set allows us to observe an entire market, where employees change their behavior as they move across numerous heterogeneous firms. Furthermore, this market is one that is inundated with fraud, a unique setting for examining unethical behavior. We believe this combination allows us to make a valuable contribution to the literature on productivity, ethics, and corruption. The context in which we study this problem is not a trivial one—vehicle emissions testing is widespread across the United States, and has serious implications both for the economy, the environment, and public health. Fraud in emissions testing has been directly linked to customer loyalty (Hubbard 2002), and can be extrapolated to elevated air pollution, and potentially infant mortality (Chay and Greenstone 2003). We therefore believe that this paper not only contributes to our understanding of ethics and organizations, but also to the management of employees and the design of environmental policy.

These findings have considerable implications for both managers and policy makers. When individuals join organizations, their personal ethics are persistent but not immutable. Managers can clearly influence the ethics of their employees, through both organization-level policies and incentives. Although managers may be able to influence the ethics of the individual to conform with organizational norms, the effect of this spillover is limited. If ethical conformity is essential to the financial and legal health of the organization, managers must be vigilant in the hiring process to vet applicants for severe misfits between organizational norms and personal beliefs. Where ethics are defined by legal compliance, such as in emissions testing, accounting fraud, or sexual harassment, the costs of hiring grossly unethical employees may be much higher.

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