EXPOSING THE REVOLVING DOOR IN EXECUTIVE BRANCH AGENCIES

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Abstract: We develop the first comprehensive mapping of the revolving door phenomenon in the U.S. by examining the work experience in executive branch agencies of 1,910,150 individuals covering top corporate positions in 373,011 firms. We document that the phenomenon is prevalent, with one out of three public firms employing a former regulator. Former regulators tend to be hired in response to or concomitant with increased regulation, or concomitant with firms receiving fines. More cheating-prone firms obtain benefits in the form of a lower incidence of fines after hiring former regulators. We do not observe other firms obtaining benefits on average.

JEL classifications: D72; K23; G38; L51; J45
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Executive Order 13770, "Ethics Commitments by Executive Branch Appointees," signed by President Trump on January 28, 2017, drew renewed attention to the flow of personnel between the government and the companies they once monitored or regulated ("the revolving door"). The revolving door phenomenon is believed to be common in the U.S., although no comprehensive mapping of its prevalence exists. This lack of clarity is somewhat unsettling, as the revolving door presents a pressing ethical concern. Government employees moving to executive positions in firms bring not only their industry expertise, but also an intimate knowledge of the regulatory system and how to exploit its loopholes. The evidence that does exist, however, is either anecdotal[1] or confined to specific industries or settings.[2] Perhaps surprisingly, it has yielded relatively little indication of unethical behavior on the behalf of firms.

In this paper, we develop the first comprehensive mapping of the revolving door phenomenon in the U.S. across firms, industries, and regions. Specifically, we examine the prior work experience in executive branch agencies of 1,910,150 individuals with employment history available in *BoardEx*. These individuals cover top corporate positions in 373,011 unique firms during 2002-2018.[3] We document that one out of every 35 top corporate employees has prior work experience in at least one of 204 U.S. executive branch agencies from the *Federal Register* that

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[1] The names of Dick Cheney, Dan Coats, Linda Fisher, Dick Gephardt, Philip Perry, Donald Rumsfeld, and Pat Toomey may come to mind.

[2] Bien and Prasad (2016) document that 15 of the 55 medical reviewers who reviewed oncology drug approvals at the Food and Drug Administration (FDA) between 2001 and 2010 subsequently either obtained jobs at biopharmaceutical firms or acted as consultants for the biopharmaceutical industry. For a sample of 994 publicly traded financial firms, Shive and Forster (2017) document that 31% of the firms have at least one board member or upper-level executive with prior experience at the Federal Reserve, the Office of the Comptroller of the Currency (OCC), the Federal Deposit Insurance Corporation (FDIC), the Securities and Exchange Commission (SEC), the Commodity Futures Trading Commission (CFTC), or the Financial Industry Regulatory Authority (FIRA). Tabakovic and Wollmann (2018) show that nearly 30% of patent applications are submitted by firms that have hired at least one former United States Patent and Trademark Office (USPTO) patent examiner. Tenekedjieva (2020) reports that, during 2000-2018, 38% of insurance commissioners move to the insurance industry after their term expires.

[3] A different set of papers investigate the opposite path, i.e., transitions from an industry to its regulatory agencies. An example is Gormley (1979), who studies flows of personnel from the broadcasting industry to the Federal Communications Commission, and documents that such transition are associated with an increase in the likelihood of decisions that are favorable to the broadcasting industry.

appear in *BoardEx* (we refer to these as “former regulators”). At the company level, we find that one out of every 15 firms in the sample has at least one top employee with prior work experience in executive branch agencies. This fraction increases dramatically with firm size. For example, one out of every three publicly traded firms has at least one top employee with prior work experience in executive branch agencies. We document that the revolving door phenomenon is especially prevalent among firms operating in the “utilities” and “finance and insurance” sectors and among companies headquartered in the District of Columbia, Virginia, and South Dakota.

The widespread interaction between politics and business naturally raises ethical questions as the hiring of former regulators may enable firms to obtain favorable treatment, potentially endangering allocative efficiency (Stigler, 1971, Peltzman, 1976). We therefore employ these data to investigate why firms are motivated to hire former regulators. Our analysis focuses on two main hypotheses.

The first hypothesis involves firms hiring former regulators either as payment for benefits received while the regulator was working at an executive branch agency or to obtain benefits down the road. We refer to this as the “cheating” hypothesis. Examples of benefits include lenient monitoring, preferential treatment in the award of contracts, tighter restrictions over entry of new rivals, etc. Benefits may either accrue ex-ante or ex-post.

The ex-ante version of this hypothesis is primarily characterized by firms receiving benefits prior to the hiring of the regulator. An example of this is former Principal Deputy Undersecretary of the Air Force Darleen Druyun. Druyun pleaded guilty to a corruption felony and was sentenced to nine months in jail for inflating the leasing price of a fleet of 767s (to $23.5 billion) in a contract described in the media as favorable to Boeing -- her future employer.[4]

[4] Cashing In For Profit? Who Cost Taxpayers Billions In Biggest Pentagon Scandal In Years? CBS News “60 Minutes”, 2005, https://www.cbsnews.com/news/cashing-in-for-profit/

The ex-post version of the “cheating” hypothesis conjectures that firms hire former regulators to obtain benefits down the road. These benefits may accrue either because of the contacts a former regulator has with incumbent executive branch agency employees, or because of their knowledge of how to manipulate the system. A report by “60 Minutes” and The Washington Post[5] illustrates the latter case: as associated chief counsel of the Drug Enforcement Agency (DEA), D. Linden Barber was an important architect and enforcer of agency regulations. According to the reports, he was then hired away by the drug industry in 2011, where he worked to undermine the very regulations he helped put in place.

The second hypothesis states that firms hire former regulators because of their industry knowledge and expertise, with this knowledge *not* being used to cheat the system. We refer to this as the “expertise” hypothesis. While this hypothesis does not necessarily make any predictions about ex-post firm behavior, hiring former regulators could lead to improvements in firm behavior. For example, their work at regulatory agencies may give former regulators the skills to help firms familiarize themselves and better comply with regulations, manage financial risk, or more efficiently dispose of dangerous chemical byproducts.

With these two hypotheses in mind, we start our analysis by documenting that former regulators tend to be hired from agencies that are more relevant to the firm, and not randomly. In particular, they tend to be hired following or concomitant with an increase in the regulations that apply to the firm. They also tend to be established in conjunction with an increase in aggressiveness by the enforcement agency in question. Specifically, we find that firms on average tend to hire former regulators from the fine-imposing agency in the year they receive a fine from the agency in question. Interestingly, firms also experience, on average, a reduction in the incidence of fines

[5] The drug industry’s triumph over the DEA, The Washington Post, October 15, 2017 (by Scott Higham and Lenny Bernstein).

in the year that follows the hiring of former regulators. These results are especially strong in the case of former regulators who transitioned to a firm immediately or soon after leaving government.

To investigate the extent to which former regulators are hired to cheat the system, we examine proxies of the likelihood that a firm cheats. The “cheating” hypothesis predicts that benefits will be especially prevalent among firms that are more likely to cheat. As measures of the likelihood that a firm cheats, we consider the corruption level of the state in which the firm is headquartered, firm-level norms about corruption tendency, and documented violations. Indeed, our analyses uncover evidence of the hiring of former regulators being associated with greater benefits (a lower incidence of fines) ex-post for firms that are more likely to cheat. Importantly, the incidence of fines declines for these firms on average despite some evidence of engaging in worse behavior (e.g., polluting more, rather than less) ex-post. That is, the reduced incidence of fines does not reflect an improvement in behavior, at least of the type we consider. The estimated benefits from hiring *one* government agency employee in terms of the incidence of fines are, on average, both statistically significant and economically large.

In contrast, at least on average, we do not observe benefits accruing (either before or after the hiring of former regulators) to firms that are less likely to cheat. Those firms, rather, appear to hire former regulators in conjunction with an increased incidence of fines.

Our study contributes to a growing academic literature on the revolving door phenomenon in government. This literature has largely focused on disentangling the “quid pro quo” and “schooling” hypotheses of the revolving door. The “quid pro quo” hypothesis states that former regulators are hired in exchange for favors received *before* leaving the regulatory agency; our “cheating” hypothesis considers this possibility in addition to former regulators helping firms receive favors *after* being hired. The “schooling” hypothesis states that former regulators are hired for their knowledge, although it does not traditionally distinguish whether or not that knowledge is used to cheat the system, which is an important distinction of our "expertise" hypothesis.

Systematic or large scale evidence in support of the "quid pro quo" hypothesis is perhaps surprisingly (but possibly reassuringly) limited. Two exceptions are Tabakovic and Wollmann (2018) and Tenekedjieva (2020). Tabakovic and Wollmann (2018) study transitions from the U.S. Patent and Trademark Office to the private sector and find evidence of leniency being on average rewarded. In particular, they document that patent examiners who grant more patents to a firm are more likely to be subsequently hired by that firm.[6] Tenekedjieva (2020) studies transitions of insurance commissioners to the private sector and finds evidence of leniency in financial oversight prior to the transition. She further documents that laws that restrict the ability of commissioners to transition to the private sector result in stricter oversight.

Shive and Forster (2017), however, find no flagrant evidence of more lenient monitoring or enforcement either before or after US financial regulators are hired by a regulated firm. Rather, consistent with the "expertise" hypothesis, they find that regulated firms become less risky after hiring a former financial regulator. They show that this is at least in part due to an increase in risk management activities. deHaan, Kedia, Koh, and Rajgopal (2015) use detailed career path data on 336 SEC trial lawyers who worked on SEC accounting-related civil litigation cases, and provide some evidence that SEC lawyers who subsequently join private law firms tend, if anything, to impose greater damages on average while at the SEC. Their results are broadly consistent with Che's (1995) signaling model, in which a regulator of unobservable quality signals her quality to

[6] In the private sector, Cornaggia, Cornaggia, and Xia (2016) document that analysts on average provide inflated credit ratings to the firms that subsequently hire them. Studies by Blanes i Vidal, Draca, and Fons-Rosen (2012) and Bertrand, Bombardini and Trebbi (2014) also find evidence consistent with the "quid pro quo" hypothesis in the context of revolving-door lobbyists. A larger literature on corporate political connections documents systematic evidence consistent with the ex-post "quid pro quo" hypothesis both internationally as well as in the U.S. (see, for example, Sapienza, 2004, Khwaja and Mian, 2005, Faccio, Masulis, and McConnell, 2005, Bunkanwanicha and Wiwattanakantang, 2009, and Goldman, Rocholl, and So, 2013).

the industry, and any perspective employers, through aggressive (rather than lenient) monitoring. Agarwal, Lucca, Seru, and Trebbi (2014) and Lucca, Seru, and Trebbi (2014)[7] also find that regulatory lenience is associated with a lower proportion of regulators who subsequently switch to the financial sector. The evidence in these studies suggests that, at least on average, regulators who subsequently move to the private sector do not appear to provide systematic favors to the industries they regulate or monitor while at the agency.[8] With the exception of Shive and Forster (2017), these studies undertake no investigation of whether hiring former regulators leads to different firm behavior ex-post.

Our results provide the first systematic evidence of the revolving door being used to cheat the system for a large set of executive branch agencies. The results are consistent with the ex-post version of the "cheating" hypothesis for various subsets of firms that are more likely to cheat.

Furthermore, our results are based on very granular data, which allow us to obtain very strong identification. The unit of observation is the firm-agency-year triplet, which enables us to include agency-year, firm-year, and/or firm-agency fixed effects in our specifications, thus leaving very little space for possible confounding sources of variation.

1. Empirical Approach

Throughout our analysis, we will attempt to understand how the revolving door phenomenon arises, where it is most prevalent, and how it is put to use. In addressing these questions, identification potentially presents a non-trivial empirical challenge. The granularity of

[7] Lucca, Seru, and Trebbi (2014) look at a sample of 35,604 regulators.

[8] Outside of government, Kempf (2020) tracks the career paths and credit ratings issued by 245 analysts at Moody's. Consistent with the "schooling" hypothesis, she documents that, on average, investment banks are more likely to hire more accurate (as opposed to more lenient) analysts. However, consistent with the "quid pro quo" hypothesis, she finds that leniency towards a particular bank increases the likelihood that the analyst lands a job at the bank in question.

our data, however, enables us to measure former regulators, firms' needs, and benefits as narrowly as at the firm-agency-year level, which allows us to greatly mitigate endogeneity concerns though the inclusion of a variety of fixed effects.

Specifically, by combining different sets of fixed effects, or including all of them at the same time, we are able to estimate different versions of the following model:

$$Y_{i,a,t} = \alpha \cdot \text{N. Former Regulators}_{i,a,t} + \zeta_{a,t} + \lambda_{i,t} + \eta_{i,a} + \varepsilon_{i,a,t} \qquad (1)$$

In this model, the unit of observation is the firm-agency-year triplet. $Y_{i,a,t}$ is the "outcome" variable for firm *i* in relation to agency *a*, in year *t* -- where "outcome" is not to be interpreted in a causal sense. In the analyses that follow, the outcome is either the number of regulatory restrictions or an indicator denoting whether a fine was imposed on firm *i* by agency *a* in year *t*. N. Former $\text{Regulators}_{i,a,t}$ is the number of top employees with prior work experience at agency *a* that firm *i* has in year *t*, i.e., former regulators (defined in Section 2.1). Its coefficient, α, reflects the extent to which the employment of former regulators is correlated with the "outcome" variable.

$\zeta_{a,t}$ are agency-year fixed effects. These reflect any time-varying as well as time-invariant agency-specific characteristics, such as staffing, funding, propensity to act harshly, career opportunities, incentives, etc. $\lambda_{i,t}$ are firm-year fixed effects. When included, they account for any firm-level time-varying or time-invariant omitted variables. When we include the firm-year fixed effects, the coefficient α isolates the extent to which, for a given firm, the "outcome" Y associated with agency *a* varies, on average, cross-sectionally *within a firm* as the number of top corporate employees with prior work experience at agency *a* varies. $\eta_{i,a}$ are firm-agency fixed effects. These account for any time-invariant firm-agency pair specific omitted variables, such as the proximity between the agency and the firm's headquarters. When included, these fixed effects de-facto demean the dependent and independent variables at the firm-agency pair over time. The coefficient α consequently isolates how much outcome Y issued by agency *a* that firm *i* is subject to varies over time, on average, as the number of firm *i*'s top corporate employees with prior work experience at agency *a* changes over time.

The inclusion of these three sets of fixed effects greatly reduces the scope for confounding sources of variation, leaving them, de-facto, to only firm-agency pair time-varying omitted variables. The setting therefore enables us to contribute to the literature in terms of quality of identification.

As firms may hire former regulators either in anticipation or following a change in regulations or fines, we also present first-difference regression specifications that focus on the annual change in the number of former regulators employed by the firm, with leads and lags. These models look like:

$$\Delta Y_{i,a,t} = \sum_{n=-2}^{+2} \alpha_{t+n} \cdot \Delta\ \mathrm{N.\,Former\ Regulators}_{i,a,t+n} + \zeta_{a,t} + \lambda_{i,t} + \varepsilon_{i,a,t} \tag{2}$$

where Δ denotes the change in the variable in question between year *t+n-1* and year *t+n*.

This first-difference regression specification allows exploiting the exact timing of the hiring of former regulators, which helps to shed light on the interpretation of results. For example, the ex-ante version of the "cheating" hypothesis predicts that firms receive fines less often *before* hiring regulators, implying negative signs on the lags of $\Delta\ \mathrm{N.\,Former\ Regulators}$. On the other hand, both the ex-post version of the "cheating" hypothesis and the "expertise" hypothesis predict that firms would receive fines less often *after* hiring executive branch employees, implying negative signs on the leads of $\Delta\ \mathrm{N.\,Former\ Regulators}$. While the "cheating" hypothesis predicts this result to be increasing with the likelihood that a firm cheats, the "expertise" hypothesis makes no such prediction.

2. Executive Branch Agency Ties

Our first task is the identification and characterization of the revolving door in executive branch agencies. It is important to first understand *where* it is being used before we ask *why* it is being used. We begin by identifying executive branch agencies and the top corporate employees who previously worked there. We focus on the flow of personnel from U.S. executive branch *agencies* because regulations in the U.S. have become increasingly (and are, at present, predominantly) generated by unelected personnel working at these agencies (Matsusaka, 2019).

2.1. Data

Our study uses *BoardEx* to classify the career histories of individuals covering top corporate positions in a large sample of 11,957 unique publicly traded and 361,054 unique private U.S. firms during 2002-2018.[9] *BoardEx* is an extensive directory of top corporate employees (defined in *BoardEx* as "individuals who lead [firms], including board members, C-suite executives and senior leaders") that contains their career histories, education, executive compensation, and career network. We extract information on the past government experience of each employee who has work experience in executive branch agencies (including the various departments of government) that appear in *BoardEx*. We require each position to have non-missing start and end dates to facilitate the creation of a time series of employment. We then define an individual, working at a company at a point in time, as a former regulator from a given agency if they joined the company after working for the regulating agency.

We obtain a list of executive branch agencies from the *Federal Register* (https://www.federalregister.gov/agencies). While no list of executive agencies is officially

[9] Frequent "top corporate positions" appearing in *BoardEx* are: Director; Partner; Independent Director; President; Vice President; President/CEO; Chairman; Associate; CEO; CFO; Consultant; Advisor; Senior VP; Manager; Executive VP; Principal; COO; Chairman/COO; Division President.

comprehensive,[10] the *Federal Register* provides the largest list, comprised of 433 such agencies. Our analysis focuses on the 204 executive branch agencies from the *Federal Register* that appear in *BoardEx*, including the largest agencies, such as the Securities and Exchange Commission, Environmental Protection Agency, and Department of Justice.

2.2. Summary Statistics

Table 1 reports the fraction of firms that employ at least one former regulator. It further reports the fraction of top employees with work experience in executive branch agencies in our sample. The averages reported are calculated across the firm-year level or employee-firm-year, respectively, for the full sample period. On average, 6.5% of firms have at least one employee with experience in at least one executive branch agency. Further, 2.9% of top employees have experience in executive branch agencies. The data show a large difference in the extent to which public and private firms have employees with experience in executive branch agencies, although this primary reflects the fact that public firms tend to have more employees. Nonetheless, 5.6% of private firms and 33.9% of public firms have employees with experience in executive branch agencies. Thus, the transitions we study in this paper are more prevalent than corporate political connections, lobbying, or campaign contributions.

[Insert Table 1 here]

Of the top corporate employees with prior experience at an executive branch agency, the median (average) employee has three (five) years of work experience at government agencies and observed a one (six) year cooling-off period prior to moving to the private sector. Both the agency experience and the cooling-off period are marginally greater for individuals hired by publicly traded firms compared to private firms.

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[10] See https://www.acus.gov/sites/default/files/documents/Sourcebook%202012%20FINAL_May%202013.pdf, pp. 14-15.

The data enable us to examine how the propensity to hire former regulators varies across firms in different industries, at least for the subset of firms with available industry classifications. The industry distribution of the employment of former regulators is tabulated in Panel B of Table 1. The industry classification used is the highest NAICS industry level, taken from *Capital IQ*. The percentage of firms that employ former regulators is highest among firms operating in the "utilities" and "finance and insurance" industries, and lowest among firms operating in the "management of companies and enterprises" and in the "public administration" sectors. Intuitively, it appears that former regulators are more likely to be hired by firms in industries typically thought of as having closer ties to government.

Figure 1 plots the evolution of the revolving door phenomenon over time across the four NAICS industries with the highest incidence of firms employing former regulators, as well as, on average, for the rest of the sample. It is evident that employment has increased the most among firms in the financial industry. Utilities experienced a somewhat large increase from 2002-2008, although employment of former regulators has declined since. For the rest of the sample, employment of former regulators appears to increase only marginally over time.

[Insert Figure 1 here]

Figure 2 provides a visualization of how the employment of former regulators varies across states. Darker colors indicate a higher fraction of firms headquartered in the state in question that employ at least one former regulator. The states with the highest fraction are the District of Columbia (41% of the firms), Virginia (35%), and South Dakota (34%), while Nevada (14%), Utah (14%) and Wyoming (10%) have the lowest incidence of firms employing former regulators.

[Insert Figure 2 here]

3. Regulations

The data summarized in Table 1 cover individuals who previously worked at executive branch agencies and the firms that subsequently employ them; however, this does not necessarily imply that these individuals are hired because of their agency experience. To provide a case for the relevance of this experience, we examine the relation between the hiring of former regulators and the regulations that govern each firm.

3.1. Data

For this purpose, we use a proxy for the extent of regulation as identified by *RegData*, a database developed by McLaughlin and Sherouse (2018).[11] The proxy, *Restrictions,* is an estimate of the number of phrases indicating legally binding obligations and prohibitions present in the Code of Federal Regulations (CFR). The database is formed using textual analysis to identify regulatory phrases for each part of the CFR. All regulations are published in the CFR, and each agency is given their own portion of the CFR publication to publish their regulations. *RegData* also uses textual analysis to estimate the relevance of each portion of the CFR to each six-digit NAICS industry, allowing an estimate of regulations at the industry-agency-year level. Table 2 tabulates the average extent of regulation in the 10 most regulated and in the 10 least regulated six-digit NAICS industries during 2002-2017. Industries that heavily employ chemicals are easy to spot among the most regulated. The 10 least regulated industries include "golf courses and country clubs" and "military armored vehicle, tank, and tank component manufacturing."

[Insert Table 2 here]

[11] https://quantgov.org/RegData-us/

While *RegData* reports the restrictions promulgated by each agency that apply to each six-digit NAICS industry, *BoardEx* does not systematically report industry classifications, let it alone at such a granular level. Therefore, we use a fuzzy name-matching algorithm to match firms from *BoardEx* to those in *Capital IQ*, from which we can retrieve each firm's primary six-digit NAICS industry code. This ensures the widest match between the companies that appear in *BoardEx* and the restrictions in *RegData*. As a consequence of the need to obtain industry classifications from a third source, however, we are only able to include in this analysis the sub-set of firms covered in *Capital IQ*, resulting in a sample of 15,591,201 firm-agency-year triplets for the *relevant* firm-agency-year triples, i.e., those with non-zero restrictions, and 73,540,773 for the broader sample that includes all possible firm-agency-year combinations.

3.2. Regulations and the Revolving Door

To establish that former regulators are not hired randomly, we investigate the extent to which more regulation increases the likelihood of affected firms hiring employees with experience at the regulator in question. For this purpose, we estimate model (1) using ln(Restrictions$_{i,a,t}$), the natural log of the number of phrases indicating legally binding obligations and prohibitions promulgated by agency *a* that apply to each six-digit NAICS industry, and thus each firm *i*, in year *t*. The sample includes all firms (public and private) with industry-affiliation available in *Capital IQ*.

The results in columns (1)-(5) of Table 3 restrict the sample to agencies that regulate the firm in question, as identified in the regulations data, i.e., firm-agency-year triplets that are accompanied with a strictly positive number of restrictions. Regression (1) of Table 3 reports an OLS regression, with no controls, of restrictions on the number of former regulators employed by the firm. The results show that, as one would expect, employing former regulators from agency *a* is more common among firms that are more heavily regulated by the agency in question. To rule out the possibility that the results capture agency-level time trends that reflect aggressiveness or lack thereof, or any firm-level time-invariant or time-varying omitted variables, we add agency-year fixed effects in Regression (2) as well as firm-year fixed effects. Once the firm-year fixed effects are included, the coefficient α indicates whether, *within a firm*, on average, employing former regulators is more common when regulators have issued more relevant regulations. The results show this to be the case, further confirming that, even *within a firm*, former regulators are more likely to be hired from agencies that are more relevant, in terms of being more active regulators of the firm in question.

[Insert Table 3 here]

We then account for firm-agency pair time-invariant omitted variables by including firm-agency pair fixed effects (Regression (3)). Doing so, de-facto, demeans both restrictions and the number of former regulators for each firm-agency pair relative to their sample mean. That, in turn, allows interpreting the coefficient α as indicative of how regulatory restrictions vary as the employment of former regulators varies over time. We find evidence of firms hiring former regulators at the time of an increase in regulations -- a result that is robust to the inclusion of agency-year, firm-year, and firm-agency fixed effects all together in Regression (4).

Regression (5) of Table 3 investigates the timing of former regulators being hired in more detail by focusing on the first-difference regression specification described in model (2). The results indicate that former regulators tend to be hired in the year that immediately follows the enactment of new regulations. A caveat with the interpretation of the results of Regression (5) is that, due to the inclusion of multiple leads and lags, that model only includes firms that survived at least a six-year period.

In column (6) we extend the sample to include all agencies, i.e., including those that do not regulate the firm in a given year (or in all years).[12] When we do so, the results indicate that former regulators are not only hired in response to the enactment of new regulations, but also in anticipation of or concomitant with their introduction. This result is perhaps not entirely surprising. New laws, especially laws introduced by an agency that did not regulate the firm before, present the greatest lack of familiarity; thus, firms may choose to hire former regulators from the agency in question during the drafting of the new legislation.

The results in this section generally show a significant correlation between the use of the revolving door and where it is relevant. The *within-firm* evidence is robust to controlling for agency-year fixed effects, meaning the results remain significant in the *time-series*.[13] The finding of such robust correlation motivates and justifies the rest of the investigation.

4. Regulatory Enforcement

Having established that the revolving door phenomenon is more prevalent where it is more relevant, we next seek to disentangle our two hypotheses concerning why firms hire former regulators (the "cheating" vs "expertise" hypotheses). To do so, we focus on regulatory enforcement (or lack thereof), as proxied for by regulatory fines, as one possible benefit that may accrue to firms. As with the data on restrictions, the data on regulatory fines covers many agencies, allowing us to once again exploit *within-firm* agency-level variation in both the hiring of former regulators and enforcement. The question that we address is whether there is any evidence of a *reduction* in the incidence of fines associated with the hiring of former regulators.

[12] To do so, the dependent variable is changed from $\ln(\text{Restrictions}_{i,a,t})$ to $\ln(\text{Restrictions}_{i,a,t} + 1)$.

[13] We reach similar conclusions if we use *Words*, the number of words present in the CFR, to proxy for the extent of regulation.

Financial penalties, or fines, are the most common way in which regulatory agencies enforce regulations. The general applicability of fines means that almost any agency can use them to punish firms that violate the regulations. Despite the fact that each agency may be enforcing different regulations, or punishing otherwise unrelated violations, regulatory fines give us a single measure that captures the extent of enforcement (or lack thereof) across all settings.

4.1. *Data*

The data on regulatory fines come from the Corporate Research Project of Good Jobs First's *Violation Tracker* (https://www.goodjobsfirst.org/violation-tracker). The dataset contains regulatory fines issued, since 2000, by more than 40 federal regulatory agencies and all divisions of the Department of Justice related to 327,000 civil and criminal cases adding up to more than $440 billion. We are able to match the fines to 2,257 unique parent firms.

Violation Tracker obtains its data through several sources, including the Department of Justice website and the individual agencies' websites. Many fines are identified using web scraping, but the data are also supplemented using Freedom of Information Act requests. Fines below $5,000 are not covered in *Violation Tracker*, as well as penalties with no dollar amount (such as some issued by the FDA, only requiring companies to suspend sales of dangerous products). *Violation Tracker* matches entities to a *current* parent company, even if they were not acquired until after the penalty was imposed. Our analysis focuses on enforcement actions of the 41 agencies that appear in *BoardEx* and are also associated with reported parent firms, leaving 15,912 such actions. Table 4 presents summary statistics of the fines that comprise our sample.

[Insert Table 4 here]

To provide an illustrative example, consider the EPA, which is discussed later when investigating regulated activities. The EPA develops and enforces regulations with the goal of protecting human health and the environment. It holds entities liable for violating the mandatory requirements set in those regulations. Our sample contains 2,614 allegations of environmental violations advanced by the EPA, the second most of any agency. These include the Volkswagen scandal involving cheating of emissions tests and the BP Gulf of Mexico oil spill.

4.2. Enforcement Actions

We use the regulatory fines to investigate whether the hiring of former regulators is associated with a change in the incidence of fines. We estimate a linear probability model of the relation between regulatory enforcement and the employment of former regulators. The model has the same structure as model (1) and the results are reported in Table 5.

In the Table 5 regressions, each firm is paired with each of the 41 federal regulatory agencies covered in the *Violation Tracker* database that could be matched to *BoardEx*, resulting in a panel of 1,306,301 firm-agency-year observations. $Fines_{i,a,t}$ indicates the extent of enforcement, i.e., whether a fine was imposed on firm *i* by agency *a* during year *t*. As in Table 3, $N.\ Former\ Regulators_{i,a,t}$ is the number of top employees with work experience at agency *a* that firm *i* has in year *t*; $\zeta_{a,t}$ are agency-year fixed effects; $\lambda_{i,t}$ are firm-year fixed effects; and $\eta_{i,a}$ are firm-agency fixed effects.

[Insert Table 5 here]

Regressions (1)-(5) restrict the analyses to firms that received at least one fine from any of the 41 agencies at any point in the sample period and have at least one employee reported in *BoardEx* during 2002-2018. In Regression (6) we extend the sample to also include firms that did not receive any fine during 2002-2018.

We start with a simple model that includes no fixed effects. In Regression (1) we find a statistically significant positive correlation between the number of former regulators employed and the incidence of fines. The coefficient of *N. Former Regulators*$_{i,a,t}$ indicates that the addition of one former regulator from agency *a* by firm *i* during year *t* is associated with a 2.5 percentage point *increase* in the incidence of fines for firm *i* from the agency in question. Given that the unconditional average probability of firm *i* receiving a fine from agency *a* in year *t* is 1.22 percentage points, this implies a 205% increase (i.e., 2.5/1.22) in the incidence of fines. In our view, this "effect" is sizeable.

We then add agency-year fixed effects as well as firm-year fixed effects, as the incidence of fines is likely to vary over time, in Regression (2). The results again confirm the statistically positive correlation between the incidence of fines and the employment of former regulators. Note that such correlation persists after controlling for any time-varying agency or firm-specific omitted variables. In Regression (3), we alternatively control for firm-agency pair fixed effects. The correlation remains positive and statistically significant, although the coefficient is substantially smaller than in Regressions (1) and (2).[14] Regression (4) includes all fixed effects, and conclusions are unchanged. Thus, both in the cross-section as well as in the time-series, the employment of former regulators is associated with an economically and statistically significant increase in the incidence of fines. Although the coefficient of the former regulators variable is substantially smaller in Regression (4), compared to Regression (1), the magnitude of the correlation remains economically meaningful: the addition of one former regulator from agency *a* by firm *i* during year *t* is associated with a 0.5 percentage point *increase* in the incidence of fines for firm *i* from the agency in question; given the sample mean of 1.22 percentage points, this implies a 41% increase (i.e., 0.5/1.22) relative to the sample mean.

[14] In untabulated tests we investigate whether firms tend to hire former regulators when their performance is good. If that were the case, the higher incidence of fines could be a by-product of firms expanding, rather than being attributable to the revolving door phenomenon. However, contrary to this hypothesis, we find no evidence that former regulators are hired following an increase in firm performance (i.e., ROE).

Does this positive coefficient found in the year a firm hires a former regulator reject the "cheating" hypothesis? Not necessarily. The positive coefficient could reflect (1) firms hiring former regulators because of more lenient (than normal) enforcement in the past, as the ex-ante version of the "cheating" hypothesis would predict; or (2) firms hiring former regulators in conjunction with an increased incidence of fines, with the hope of reducing the future incidence of fines either through improving their behavior or cheating the system.

Regression (5) of Table 5 exploits the precise timing in the hiring of former regulators to distinguish between these two possibilities. The model uses different leads and lags in the hiring of former regulators. The results indicate significant hiring of former regulators in the year in which a firm receives "more-frequent-than-normal" fines from a particular fine-imposing agency. Thus, former regulators are hired in response to a (negative) shock. We find no evidence of "less-frequent-than-normal" fines prior to the hiring of former regulators (as the coefficients of Δ *N RD [(t+1)-(t)]* and Δ *N RD [(t+2)-(t+1)]* are both insignificant). Thus, at least on average, we find no support for the ex-ante version of the "cheating" hypothesis.

Importantly, we find evidence of a *decline* in the incidence of fines in the year that *follows* the hiring of former regulators. A benign interpretation of this result is that it reflects either improved behavior or a regression to the mean, where fines revert to "normal" levels. An alternative, less benign, interpretation follows the ex-post "cheating" hypothesis. The increased incidence of fines in the year that a former regulator is hired, and the decline in the year that follows, could come from different sets of firms. One set of firms use former regulators to learn, while the other set use former regulators to cheat the system. In the Section 5 we present tests that allow us to distinguish between these two alternatives.

Regardless, the results leave little space for confounding sources of variation, as Regression (5) includes agency-year fixed effects, firm-year fixed effects, and first differences at the firm-agency level. The results of Regression (6), which includes all firms, even those that never received a fine, are directionally similar to those of Regression (5); statistical significance is also very similar between the two models. Naturally, because of the inclusion of many firms that never received any fine in Regression (6), the magnitude of the coefficients is much smaller than in Regression (5), which focuses only on firms that received at least one fine during the sample period.

In Panel B of Table 5 we verify that the results in question come about because of former regulators that are likely to be more relevant by focusing on hires of former regulators within two years of the regulators' departure from their agency. It is less likely that a former regulator who is hired 20 years after leaving the agency would provide much help, either in terms of learning or in terms of access and benefits. Thus, in Panel B we redefine the number of former regulators as the number of employees from agency *a* hired by firm *i* in year *t* who were hired within two years since leaving the agency. The other hires are not counted as former regulators. The results in Panel B of Table 5 demonstrate that it is the flow of "up to date" regulators, who quickly move from the government agency to the private sector, which gives rise to the results documented in Panel A. Both the coefficients and the significance of the former-regulator variables are greater when we focus on the flow of personnel who recently left the government.

The results in this section highlight that former regulators are, at least on average, hired in conjunction with harsh treatment by executive branch agencies. This is in line with prior evidence in deHaan et al. (2015), Agarwal et al. (2014), and Lucca et al. (2014); it is also consistent with Che's (1995) signaling model. Our results are thus consistent with learning on the part of firms *on* *average*. However, our analyses also show evidence that is consistent with favors being provided to firms: fines *decline* in the year that *follows* the hiring of a former regulator. It is of course possible that the favors and learning may be taking place in different groups firms. In the remainder of the paper, we present tests that allow us to distinguish between these two possibilities.

5. Likelihood to Cheat and the Revolving Door

To evaluate if firms are systematically hiring former regulators with the purpose of "cheating the system" or "learning how to comply with the rules," we partition the sample by employing several proxies of a firm's propensity to cheat. In total, we use six proxies. The first three provide alternative measures of corruption in the state of the firm's headquarters, the fourth seeks to capture firm-level cultural norms, and the fifth and sixth proxies identify firms that are established violators. We use the various proxies to partition the sample into two sub-samples (of above- and below-median levels of the propensity to cheat). If the documented reduction in the incidence of fines reflects "cheating", then the reduction should be predominantly among firms that are more likely to cheat.

The first state-level proxy is the number of federal public corruption convictions per capita during 1976-2010 from Simpson, Nowlan, Gradel, Zmuda, Sterrett, and Cantor (2012). Data on federal public corruption convictions used in Simpson et al. is provided by the Public Integrity Section of the Department of Justice. The second proxy comes from a survey of State House reporters conducted by Boylan and Long (2003). The authors received responses from 293 reporters on a series of questions, such as "Where would your state rank…in terms of level of corruption?" and "What is your best guess of the percentage of government employees…in your state who are corrupt?" They used the responses to construct state-level measures of corruption.

In our analysis, we use *Q6*, the average response to the question of ranking their state. Our third proxy at the state level is from Dincer and Johnston (2015), who surveyed 280 state political reporters to create a perceptions-based index of both legal and illegal corruption in U.S. states. Although there is no complete agreement of states' rankings across all three proxies, Louisiana, Illinois, and Alabama tend to be among the most corrupt states while Vermont, Oregon, and Colorado tend to be among the least corrupt.

For our fourth proxy, we construct a firm-level measure of cultural norms using each company's top employees. We assume highly ranked employees will be the corporate decision makers, so their cultural norms will impact corporate choices. We also assume the cultural beliefs and values of individuals are defined by their country of origin (see also DeBacker, Heim, and Tran, 2015, Fisman and Miguel, 2007, Guiso, Sapienza, and Zingales, 2006, Liu, 2016, Pan, Siegel and Wang, 2017). We use each top employee's nationality, as reported in *BoardEx*, to determine their country of origin, as well as the perception of corruption in that country using Transparency International's Corruption Perception Index (CPI). [15] We then compute the average CPI across all top employees in each firm each year.

Our fifth and sixth proxies identify firms that are established violators. The fifth proxy measures the average annual *amount* of fines, in U.S. dollars, received by each firm during the entire sample period, aggregated across all agencies. We assume that larger fines capture the more egregious violators. The sixth proxy identifies accounting misstatements using the number of "Accounting and Auditing Enforcement Releases" (AAERs) issued by the SEC during 1982-2018 pertaining to a given firm. Those include, but are not limited to, accounting restatements. This dataset

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[15] Transparency International defines corruption as "the abuse of entrusted power for private gain" (https://www.transparency.org/what-is-corruption).

was originally constructed by Dechow et al. (2011) and is currently maintained by the same authors.[16]

For each proxy, we partition the sample into two groups based on the median level of the proxy for all firms in each year: above the median level ("above-median") and at-or-below the median level ("below-median) of the likelihood to cheat. We use partitions based on these six proxies in two settings: regulatory fines (Section 5.1), and regulated activities (Section 5.2). While the broader setting of regulatory fines allows us to exploit variation across multiple agencies in a well-identified econometric setting, it does not allow us to closely examine changes in the regulated activities firms are being punished for. To do this, in Section 5.2, we narrow our focus to a specific activity regulated by a specific agency, the EPA.

5.1. Fines

In Table 6 we replicate the regressions for fines, reported in Table 5, after splitting the sample based on these proxies of the propensity to cheat. Panel A reports the results for the above-median group, while Panel B focuses on the rest of the sample. The "cheating" hypothesis predicts that benefits would mostly accrue to cheating-prone firms.

[Insert Table 6 here]

For conciseness, Table 6 only reports first-difference regression specifications of annual changes in the incidence of fines on the hiring of former regulators, along with two leads and two lags of hiring. For the above-median group, we document a significant *decrease* in incidence of fines in the year that follows the hiring of former regulators for five of the six proxies employed. This result is consistent with the ex-post version of the "cheating" hypothesis. The regressions include firm-year fixed effects, so the results indicate that the decrease in the incidence of fines is

[16] A description of the dataset is available at https://www.marshall.usc.edu/departments/leventhal-school-accounting/faculty/aaer-dataset.

observed after accounting for any firm-level time-varying (or time-invariant) omitted variables. This includes any general firm-level tendency to improve or worsen firm behavior after hiring former regulators. The economic magnitude of the "effect" is, in our opinion, very large: the average incidence of *Fines* at the firm-agency-year level in Panel A of Table 6 ranges from 1.42% to 1.88% across the six regressions, while the reduction in the incidence of fines in the year that follows the hiring of former regulators ranges between 0.7 and 2.0 percentage points.

The evidence from the below-median group (Panel B of Table 6) suggests that such firms hire former regulators in conjunction with a spike in the incidence of fines. Thus, in contrast to the above-median group, we do not find evidence of a reduced incidence of fines after hiring former regulators for the below-median group.

5.2. Regulated Activities: Emissions of Hazardous Chemicals

Could the reduction in the incidence of fines reflect an improvement in the behavior of the firms that are more likely to cheat? If that were the case, the results would still possibly be inconsistent with "cheating." To investigate this possibility, we examine changes in firm behavior related to a specific EPA-regulated activity. The EPA is one of the largest executive branch agencies and, as shown earlier in Table 4, it is the second most important agency in terms of number of fines imposed. It governs several aspects of firms' activities, with primary focus on the emission and disposal of hazardous chemicals used during the production process.

In our analysis, we focus on emissions of chemicals that are hazardous to human health, as reported the EPA's *Toxics Release Inventory* (*TRI*) database. The *TRI* database reports, for the 1987-2017 period, emissions of specific hazardous chemicals, of which there are 658, by plants exceeding a minimum threshold of employees (currently 10 full-time employees).[17] Examples of

[17] The data are available at https://www.epa.gov/toxics-release-inventory-TRI-program/TRI-basic-data-files-calendar-years-1987-2017.

hazardous chemicals subject to reporting include ammonia, arsenic, asbestos, dioxin, ethylene glycol, lead, and strychnine. The EPA audits the data and investigates misreporting. Evidence surveyed in Akey and Appel (2020) indicates that misreporting does not appear to be large or systematic in this database (also see Brehm and Hamilton, 1996, De Marchi and Hamilton, 2006, EPA, 1998, and Bui and Mayer, 2003).

We use the EPA's *TRI* data to investigate whether the decline in fines documented for the above-median group is associated with an improvement in their behavior (a decrease in emissions) following the hiring of former EPA regulators. If that were the case, it would become questionable whether the results can be interpreted as evidence of "cheating."

To investigate whether the hiring of former EPA regulators is associated with lower emissions, we estimate the following model:

$$\Delta\ln(\text{Emissions}_{i,c,t} + 1) = \sum_{n=-2}^{+2} \alpha_{t+n} \cdot \Delta\, \text{N. Former Regulators}_{i,EPA,t+n} + \nu_{c,t} + \qquad (3)$$

$$+ \beta \cdot \Delta x_{i,t} + \varepsilon_{i,c,t}$$

In these regressions, the unit of observation is the firm-chemical-year triplet. The analysis of emissions includes only firms that report to the EPA at least once during 2002-2017 matched to chemicals that they report emitting at least once during 2002-2017. We employ the data as reported in the EPA's *TRI* database. $\text{Emissions}_{i,c,t}$ denotes the weight of chemical *c* released by firm *i* in year *t*. $\text{N. Former Regulators}_{i,EPA,t}$ is the number of employees with work experience at the EPA that firm *i* has in year *t*.

To capture chemical-specific time-trends in emissions (it is well known that, in general, emissions have been declining over time), as well as differences in toxicity across chemicals, we include chemical-year fixed effects, $\nu_{c,t}$. Because of our focus on first-difference regressions, our results indicate how the emissions of chemical *c* by firm *i* vary over time, as the hiring of former EPA regulators varies.

The results of these regressions are reported in Table 7. As a consequence of focusing on only one agency, we cannot exploit within-firm variation in the hiring of former regulators through the inclusion of firm-year fixed effects. The results, therefore, do not have the same high level of identification present in all the previous analyses. To mitigate endogeneity concerns, we include time-varying firm specific control variables, $x_{i,t}$, namely the number of employees in *BoardEx*, to limit the scope for omitted firm-level time-varying variables. The coefficient of interest, α_{t+n}, reflects how emissions change surrounding the hiring of a former EPA regulator.

[Insert Table 7 here]

Contrary to the "expertise" hypothesis, the results in Table 7 provide no evidence of improvements in firm behavior for either group of firms. Rather, there is sporadic weak evidence of increases in emissions following the hiring of former EPA regulators by the above-median group. The correlation is positive in nine out of the 12 coefficients in the two years after a former EPA regulator is hired across the six regressions, and two of the coefficients are statistically significant. Given how important regulating emissions is to the EPA's agenda, this calls into question the extent to which firms are hiring former EPA regulators for their expertise.

Overall, the results in this section are most consistent with some firms to extracting benefits, rather than learning, from their former regulators. Of course, learning might still be occurring on aspects of firm behavior other than emissions of toxic chemicals.

8. Conclusion

The revolving door between the government and the private sector often draws criticism from the media, as well as from some academics. However, there has been neither systematic documentation nor significant evidence identifying cheating on the part of firms that hire former regulators. In this paper, we provide the first comprehensive database tracking flows of personnel from over 200 executive branch agencies (in the U.S.) to the private sector, and show that the revolving door phenomenon is indeed pervasive. This raises the question of why? Does the revolving door, as some academics have speculated, arise in response to a need to learn from the former regulators how to comply with the law and/or how to behave more ethically? Or is the revolving door an attempt to circumvent regulations or obtain ethically questionable favors from the government?

Our results show that former regulators are not hired randomly. Rather, former regulators are hired in response to regulator activity, specifically the enactment of new regulations or an increase in the incidence of fines. Importantly, these results are present in an econometrically stringent setting that includes firm-year, agency-year, and firm-agency fixed effects, leaving little space to confounding sources of variation.

When partitioning the sample based on the likelihood that a firm may cheat, we uncover evidence that firms headquartered in more corrupt states, firms that are seemingly more corruption-prone, and firms that are established violators benefit the most in terms of a lower incidence of fines after hiring former regulators. These results are most consistent with the ex-post version of the "cheating" hypothesis, with firms using the connections of former regulators to cheat the system down the road. Further, we document that the reduction in the incidence of fines is, in a well identified setting, both statistically and economically significant among the firms seemingly more likely to cheat.

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Table 1. Prevalence of the Revolving Door

The table provides averages of the prevalence of the revolving door, i.e., the employment of former regulators, in the U.S. during 2002-2018. The employment of former regulators is measured either at the company level (*Company* column) or at the top employee level (*Top Employee* column). In the former case the Table reports, for the panel, the percentage of firms with at least one top employee with work experience at the agencies in question. In the latter case, the Table reports, for the panel, the percentage of employees with work experience at the agencies in question. *Private* firms are labeled as either "Private" or "Partnership" by *BoardEx*. *Public* firms are labeled in *BoardEx* as "Quoted." When no specific agency is mentioned, the table reports the percentage of firms and employees with work experience in at least one of 204 executive branch agencies from the *Federal Register*. Panel A reports statistics for the full panel, while Panel B reports statistics by NAICS industry, whenever available in *Capital IQ*.

Panel A: Full Sample

	N. Obs.	**Company**	**N. Obs.**	**Top Employee**
Total	**3,024,282**	**6.549%**	**11,452,836**	**2.898%**
Private	2,918,229	5.554%	8,834,023	2.936%
Public	106,053	33.936%	2,618,813	2.768%
196 *RegData* Agencies	3,024,282	5.877%	11,452,836	2.568%
41 Fines Agencies	3,024,282	3.795%	11,452,836	1.620%
EPA	3,024,282	0.226%	11,452,836	0.069%

Panel B: Industry Distribution of Former Regulators

	Company	**Top Employee**
Total	**12.323%**	**1.443%**
Utilities	27.459%	3.019%
Finance and Insurance	17.492%	1.912%
Transportation and Warehousing	17.014%	1.990%
Health Care and Social Assistance	16.471%	1.786%
Educational Services	16.094%	1.749%
Agriculture, Forestry, Fishing and Hunting	14.308%	1.267%
Other Services (except Public Administration)	12.603%	1.958%
Professional, Scientific, and Technical Services	12.429%	1.387%
Manufacturing	10.885%	1.248%
Accommodation and Food Services	9.694%	1.079%
Retail Trade	9.577%	0.873%
Admin. and Support and Waste Mgmt and Remediation Services	9.537%	1.309%
Real Estate Rental and Leasing	9.355%	0.972%
Information	8.815%	1.023%
Construction	8.702%	1.209%
Arts, Entertainment, and Recreation	8.170%	1.080%
Wholesale Trade	7.650%	1.009%
Mining	7.395%	1.326%
Management of Companies and Enterprises	0.000%	0.000%
Public Administration	0.000%	0.000%

Table 2: Industry Distribution of Regulations

The Table reports the 10 most and 10 least regulated six-digit NAICS industries based on the average number of restrictions during 2002-2017. *Restrictions* is an estimate of the number of phrases indicating legally binding obligations and prohibitions present in the Code of Federal Regulations (CFR). The variable is from *RegData*, a database developed by McLaughlin and Sherouse (2018) using textual analysis to identify regulatory phrases for each part of the CFR.

NAICS Descriptions	**NAICS Code**	**Restrictions**
Gum and Wood Chemical Manufacturing	325191	74,536
Inorganic Dye and Pigment Manufacturing	325131	72,316
Polish and Other Sanitation Good Manufacturing	325612	69,304
Alkalies and Chlorine Manufacturing	325181	69,253
Cyclic Crude and Intermediate Manufacturing	325192	69,108
Paint and Coating Manufacturing	325510	68,834
Plastics Material and Resin Manufacturing	325211	68,091
Medicinal and Botanical Manufacturing	325411	66,207
Cellulosic Organic Fiber Manufacturing	325221	66,171
Soap and Other Detergent Manufacturing	325611	65,817
…	…	…
Coated Paper Bag and Pouch Manufacturing	322223	371
Golf Courses and Country Clubs	713910	362
Military Armored Vehicle, Tank, and Tank Component Manufacturing	336992	352
Lessors of Residential Buildings and Dwellings	531110	346
Other Pressed and Blown Glass and Glassware Manufacturing	327212	291
Ground or Treated Mineral and Earth Manufacturing	327992	284
Aircraft Engine and Engine Parts Manufacturing	336412	258
Electronic Capacitor Manufacturing	334414	245
Semiconductor and Related Device Manufacturing	334413	241
Electron Tube Manufacturing	334411	225

Table 3. Regulations and the Revolving Door

The unit of observation is the firm-agency-year triplet. *Restrictions* is an estimate of the number of phrases indicating legally binding obligations and prohibitions present in the Code of Federal Regulations (CFR). The variable is from *RegData*, a database developed by McLaughlin and Sherouse (2018) using textual analysis to identify regulatory phrases and word counts for each part of the CFR. *RegData* also uses textual analysis to estimate the relevance of each portion to each six-digit NAICS industry, allowing an estimate of regulations at the industry-agency-year level. *N. Former Regulators* is the number of top employees with work experience in agency *a.* The sample includes firms with industry classifications available in *Capital IQ*, from which we could retrieve each firm's primary 6-digit NAICS industry code. Columns (1) through (5) include firms that are regulated by a given agency in a given year, while column (6) includes all firm-agency-year triplets. The bottom three rows of the table indicate the fixed effects included in the specifications. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients.

	ln(Restrictions)	ln(Restrictions)	ln(Restrictions)	ln(Restrictions)	Δ ln(Restr.) [(t) - (t-1)]	Δ ln(Restr.+1) [(t) - (t-1)]
	(1)	(2)	(3)	(4)	(5)	(6)
N. Former Regulators	0.976***	0.332***	0.077***	0.015**		
	(5.71)	(6.61)	(6.88)	(2.02)		
Δ N. Former Regulators [(t-2) - (t-3)]					-0.006	0.000
					(-1.36)	(0.29)
Δ N. Former Regulators [(t-1) - (t-2)]					0.006	0.002***
					(1.39)	(2.65)
Δ N. Former Regulators [(t) - (t-1)]					-0.005	0.001*
					(-1.11)	(1.75)
Δ N. Former Regulators [(t+1) - (t)]					0.008**	0.003***
					(1.97)	(3.33)
Δ N. Former Regulators [(t+2) - (t+1)]					0.001	0.000
					(0.18)	(0.54)
Number of Observations	15,591,201	15,591,201	15,477,937	15,477,937	8,261,977	41,142,735
Cluster	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency
Agency-Year FEs		Y		Y	Y	Y
Firm-Year FEs		Y		Y	Y	Y
Firm-Agency FEs			Y	Y		

Table 4. Fines by Agency (in Thousands of Dollars).

This table reports, by agency, the fines imposed on firms that could be matched with *BoardEx*. Fines are from the Corporate Research Project of Good Jobs First's *Violation Tracker* (https://www.goodjobsfirst.org/violation-tracker).

Agency	N. of Fines	Mean	Median	Min	Max
Occupational Safety & Health Administration	5,419	50	14	5	50,692
Environmental Protection Agency	2,614	12,486	50	3	14,925,000
Department of Labor	1,604	377	29	5	84,000
National Labor Relations Board	1,143	338	31	5	97,183
Federal Railroad Administration	930	111	13	5	3,895
Department of Justice	622	274,128	15,214	9	20,800,000
Mine Safety & Health Administration	569	651	50	5	229,696
Federal Aviation Administration	472	344	41	5	27,441
Securities and Exchange Commission	414	41,508	7,000	25	800,000
Equal Employment Opportunity Commission	339	1,605	153	5	54,000
Pipeline and Hazardous Materials Safety Administration	198	107	47	5	1,045
Federal Motor Carrier Safety Administration	181	19	12	5	125
Bureau of Safety and Environmental Enforcement	146	131	69	5	1,143
Bureau of Industry and Security	127	16,934	85	5	1,400,000
Office of Foreign Assets Control	98	12,793	35	5	375,000
Commodity Futures Trading Commission	94	59,332	1,730	25	700,000
Office of the Comptroller of the Currency	82	148,893	6,375	5	2,886,578
Department of Transportation	78	3,200	100	20	225,000
Federal Communications Commission	75	20,059	1,120	10	614,300
Centers for Medicare & Medicaid Services	69	441	165	5	3,101
Federal Trade Commission	64	104,146	2,690	85	4,000,000
Consumer Financial Protection Bureau	58	138,672	33,425	34	2,125,000
Department of Energy	56	427	163	5	4,000
Federal Energy Regulatory Commission	56	18,223	3,222	80	410,000
Consumer Product Safety Commission	53	1,801	715	35	27,250
Federal Reserve	51	101,982	61,900	7	342,000
Health & Human Services Department Office of Inspector General	42	1,145	128	10	12,640
National Highway Traffic Safety Administration	36	39,827	5,677	20	900,000
Nuclear Regulatory Commission	35	233	60	6	5,450

Federal Deposit Insurance Corporation	28	21,868	138	5	140,000
Food and Drug Administration	28	316,961	68,030	550	2,201,200
Housing and Urban Development Department	24	11,032	143	11	200,000
Alcohol and Tobacco Tax and Trade Bureau	23	472	220	8	3,700
Department of the State	21	14,535	10,000	225	55,000
Department of the Interior	18	1,511	606	11	12,240
Drug Enforcement Administration	11	25,071	11,000	834	80,000
Federal Housing Finance Agency	10	2,124,650	717,500	99,500	9,300,000
National Credit Union Administration	9	159,794	129,600	5,250	445,000
Treasury Department Financial Crimes Enforcement Network	8	86,475	15,000	2,800	461,000
Federal Maritime Commission	4	115	118	25	200
U.S. Fish and Wildlife Service	3	1,283	1,000	350	2,500

Table 5. Enforcement Actions and the Revolving Door: Fines

The unit of observation is the firm-agency-year triplet. The dependent variable, *Fines*, is an indicator taking the value of one if a fine imposed on firm *i* by agency *a* during year *t*, and zero otherwise. Fines are from the Corporate Research Project of Good Jobs First's *Violation Tracker* (https://www.goodjobsfirst.org/violation-tracker). *N. Former Regulators* is the number of top employees with work experience in each of the 41 fine-imposing executive branch agencies, *a*, that firm *i* has in year *t*. Panel A includes all such employees, while Panel B restricts this definition to top employees hired within two years of leaving the executive branch agency. The sample includes both public and private firms. Columns (1) through (5) of both panels include firms that received at least one fine during 2002-2018, while column (6) in both panels include all firms in *BoardEx*. The bottom three rows of the table indicate the fixed effects included in the specifications. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients.

Panel A: All Former Regulators

	Fines (1)	Fines (2)	Fines (3)	Fines (4)	Δ Fines [(t) - (t-1)] (5)	Δ Fines [(t) - (t-1)] (6)
N. Former Regulators	0.025***	0.022***	0.006***	0.005**		
	(8.85)	(8.21)	(2.83)	(2.46)		
Δ N. Former Regulators [(t-2) - (t-3)]					0.002	0.0002
					(0.47)	(0.40)
Δ N. Former Regulators [(t-1) - (t-2)]					-0.009**	-0.0012**
					(-2.04)	(-1.99)
Δ N. Former Regulators [(t) - (t-1)]					0.008*	0.0010*
					(1.90)	(1.86)
Δ N. Former Regulators [(t+1) - (t)]					0.000	0.0001
					(0.07)	(0.13)
Δ N. Former Regulators [(t+2) - (t+1)]					0.002	0.0002
					(0.60)	(0.50)
Number of Observations	1,306,301	1,306,301	1,305,522	1,305,522	917,129	60,826,825
Cluster	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency
Average Incidence of *Fines*	0.0122	0.0122	0.0122	0.0122	0.0136	0.0002
Agency-Year FEs		Y		Y	Y	Y
Firm-Year FEs		Y		Y	Y	Y
Firm-Agency FEs			Y	Y		

Panel B: Former Regulators with Cooling-Off Period ≤ 2 Years

	Fines (1)	Fines (2)	Fines (3)	Fines (4)	Δ Fines [(t) - (t-1)] (5)	Δ Fines [(t) - (t-1)] (6)
N. Former Regulators	0.029***	0.026***	0.008***	0.007**		
	(7.09)	(6.84)	(2.73)	(2.55)		
Δ N. Former Regulators [(t-2) - (t-3)]					-0.002	-0.0003
					(-0.29)	(-0.31)
Δ N. Former Regulators [(t-1) - (t-2)]					-0.016**	-0.0022**
					(-2.36)	(-2.28)
Δ N. Former Regulators [(t) - (t-1)]					0.019***	0.0026***
					(3.14)	(3.12)
Δ N. Former Regulators [(t+1) - (t)]					-0.001	-0.0002
					(-0.23)	(-0.19)
Δ N. Former Regulators [(t+2) - (t+1)]					-0.008	-0.0011
					(-1.35)	(-1.38)
Number of Observations	1,306,301	1,306,301	1,305,522	1,305,522	917,129	60,826,825
Cluster	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency
Average Incidence of *Fines*	0.0122	0.0122	0.0122	0.0122	0.0136	0.0002
Agency-Year FEs		Y		Y	Y	Y
Firm-Year FEs		Y		Y	Y	Y
Firm-Agency FEs			Y	Y		

Table 6. Enforcement Actions and the Revolving Door by Likelihood to Cheat.

The unit of observation is the firm-agency-year triplet. The dependent variable is *Δ Fines*, where *Fines* is an indicator taking the value of one if a fine imposed on firm *i* by agency *a* during year *t*, and zero otherwise. Fines are from the Corporate Research Project of Good Jobs First's *Violation Tracker* (https://www.goodjobsfirst.org/violation-tracker). *Δ N. Former Regulators* is the number of top employees with work experience in each of the 41 fine-imposing executive branch agencies, *a*, that firm *i* has in year *t-n*, minus the number of top employees with work experience in each of the 41 fine-imposing executive branch agencies, *a*, that firm *i* has in year *t-n-1*, where *n*=-2, -1,… +2. The likelihood to cheat "proxy" row indicates the proxy employed to partition the sample. Panel A reports the results for firms with above-median likelihood to cheat; Panel B reports the results for firms with median or below-median likelihood to cheat. *Corruption Convictions per Capita* measures the number of public corruption convictions in the state per 10,000 residents (Simpson et al., 2012). *Q6* measures State House reporter's average response to ranking corruption in their state (Boylan and Long, 2003). *Reporter Ratings* measures state political reporters' perception of legal and illegal corruption in their state (Dincer and Johnston, 2015). *Nationality* is the average corruption perception index (from Transparency International) across the countries of nationality of company i's employees (as of the end of year t). *Average Dollar Fine* is the average annual *amount* of fines, in U.S. dollars, received by the firm during the sample period. Fines are aggregated across all agencies. *Accounting Misstatements* is the number of "Accounting and Auditing Enforcement Releases" (AAERs) issued by the SEC during 1982-2018 pertaining to a given firm, from Dechow et al. (2011). The bottom rows of each panel indicate the fixed effects included in the specifications. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients.

Panel A: Fines when the Likelihood to Cheat is *Above* Median; Change Regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Likelihood to cheat proxy:	Corruption Convictions per Capita	Q6	Reporter Ratings	Nationality	Average Dollar Fine	Accounting Misstatements
Δ N. Former Regulators [(t-2) - (t-3)]	0.010	0.004	0.011	0.007	0.004	0.018
	(1.49)	(0.44)	(1.44)	(0.99)	(0.62)	(1.50)
Δ N. Former Regulators [(t-1) - (t-2)]	-0.015**	-0.007	-0.017**	-0.015*	-0.013**	-0.020*
	(-2.20)	(-0.87)	(-2.43)	(-1.91)	(-2.24)	(-1.76)
Δ N. Former Regulators [(t) - (t-1)]	0.006	0.004	0.016**	0.009	0.011**	0.000
	(0.86)	(0.46)	(2.32)	(1.21)	(1.99)	(0.01)
Δ N. Former Regulators [(t+1) - (t)]	0.001	0.001	-0.003	0.002	0.002	0.020*
	(0.22)	(0.16)	(-0.49)	(0.22)	(0.35)	(1.74)
Δ N. Former Regulators [(t+2) - (t+1)]	-0.002	0.003	-0.001	-0.000	0.003	-0.005
	(-0.40)	(0.46)	(-0.15)	(-0.04)	(0.62)	(-0.55)
Number of Observations	344,113	291,797	315,413	263,835	467,605	74,825

Cluster	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency
Average Incidence of *Fines*	0.0142	0.0150	0.0143	0.0155	0.0175	0.0188
Agency-Year FEs	Y	Y	Y	Y	Y	Y
Firm-Year FEs	Y	Y	Y	Y	Y	Y

Panel B: Fines when the Likelihood to Cheat is *Equal or Below* Median; Change Regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Likelihood to cheat proxy:	Corruption Convictions per Capita	Q6	Reporter Ratings	Nationality	Average Dollar Fine	Accounting Misstatements
Δ N. Former Regulators [(t-2) - (t-3)]	-0.007	-0.004	-0.006	-0.002	-0.003	-0.007
	(-1.06)	(-0.66)	(-1.12)	(-0.34)	(-0.82)	(-1.39)
Δ N. Former Regulators [(t-1) - (t-2)]	-0.005	-0.008	-0.003	-0.005	0.003	-0.006
	(-0.71)	(-1.37)	(-0.49)	(-0.96)	(0.70)	(-1.23)
Δ N. Former Regulators [(t) - (t-1)]	0.011**	0.013**	-0.001	0.008	-0.001	0.012**
	(2.06)	(2.50)	(-0.24)	(1.47)	(-0.28)	(2.51)
Δ N. Former Regulators [(t+1) - (t)]	0.001	-0.000	0.007	-0.001	-0.005	-0.005
	(0.23)	(-0.08)	(1.36)	(-0.20)	(-1.04)	(-1.26)
Δ N. Former Regulators [(t+2) - (t+1)]	0.004	-0.002	0.002	0.005	-0.000	0.002
	(0.61)	(-0.30)	(0.33)	(0.93)	(-0.03)	(0.48)
Number of Observations	359,201	360,513	380,357	653,294	449,524	562,233
Cluster	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency	Firm-Agency
Average Incidence of *Fines*	0.0142	0.0138	0.0138	0.0129	0.0096	0.0140
Agency-Year FEs	Y	Y	Y	Y	Y	Y
Firm-Year FEs	Y	Y	Y	Y	Y	Y

Table 7. Emissions of Hazardous Chemicals and the Revolving Door by Likelihood to Cheat.

The unit of observation is the firm-chemical-year triplet. The dependent variable, *Δ Emissions*, is the weight of chemical *c* released by firm *i* in year *t* minus the weight of chemical *c* released by firm *i* in year *t-1*. *Δ N. Former Regulators* is the number of top employees with work experience at the EPA that firm *i* has in year *t-n*, minus the number of top employees with work experience at the EPA that firm *i* has in year *t-n-1*, where *n*=-2, -1,… +2. The likelihood to cheat "proxy" row indicates the proxy employed to partition the sample. Panel A reports the results for firms with above-median likelihood to cheat; Panel B reports the results for firms with median or below-median likelihood to cheat. *Corruption Convictions per Capita* measures the number of public corruption convictions in the state per 10,000 residents (Simpson et al., 2012). *Q6* measures State House reporter's average response to ranking corruption in their state (Boylan and Long, 2003). *Reporter Ratings* measures state political reporters' perception of legal and illegal corruption in their state (Dincer and Johnston, 2015). *Nationality* is the average corruption perception index (from Transparency International) across the countries of nationality of company i's employees (as of the end of year t). *Average Dollar Fine* is the average annual *amount* of fines, in U.S. dollars, received by the firm during the sample period. Fines are aggregated across all agencies. *Accounting Misstatements* is the number of "Accounting and Auditing Enforcement Releases" issued by the SEC during 1982-2018 pertaining to a given firm, from Dechow et al. (2011). The bottom rows of each panel indicate the fixed effects included in the specifications. T-stats based on standard errors clustered at the firm level are reported in parentheses below the coefficients.

Panel A: Emissions of Hazardous Chemicals when the Likelihood to Cheat is *Above* Median; Change Regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Likelihood to cheat proxy:	Corruption Convictions per Capita	Q6	Reporter Ratings	Nationality	Average Dollar Fine	Accounting Misstatements
Δ N. Former Regulators [(t-2) - (t-3)]	0.086**	0.071*	0.086	0.039	0.028	0.085
	(2.21)	(1.82)	(1.44)	(0.73)	(0.40)	(1.04)
Δ N. Former Regulators [(t-1) - (t-2)]	-0.051	-0.025	0.073	0.005	0.018	-0.001
	(-0.78)	(-0.27)	(1.34)	(0.07)	(0.18)	(-0.01)
Δ N. Former Regulators [(t) - (t-1)]	-0.005	-0.065*	0.034	0.059	0.044	0.031
	(-0.11)	(-1.95)	(0.40)	(1.19)	(0.47)	(0.15)
Δ N. Former Regulators [(t+1) - (t)]	0.023	0.005	0.103	0.123**	0.081	0.074
	(0.34)	(0.05)	(0.59)	(1.98)	(0.79)	(0.35)
Δ N. Former Regulators [(t+2) - (t+1)]	0.048	0.041	-0.046	0.061	0.042	0.061
	(1.00)	(0.75)	(-0.48)	(1.14)	(0.54)	(0.59)
Δ ln(N. Top Employees)	-0.013	0.018	0.015	0.047	-0.120*	0.020
	(-0.28)	(0.31)	(0.23)	(1.47)	(-1.75)	(0.13)

Number of Observations	47,277	37,384	32,563	71,813	59,890	6,104
Cluster	Firm	Firm	Firm	Firm	Firm	Firm
Chemical-Year FEs	Y	Y	Y	Y	Y	Y

Panel B: Emissions of Hazardous Chemicals when the Likelihood to Cheat is *Equal or Below* Median; Change Regressions

	(1)	(2)	(3)	(4)	(5)	(6)
Likelihood to cheat proxy:	Corruption Convictions per Capita	Q6	Reporter Ratings	Nationality	Average Dollar Fine	Accounting Misstatements
Δ N. Former Regulators [(t-2) - (t-3)]	0.065	0.114	0.069	0.073	0.134*	0.085
	(0.58)	(0.91)	(0.93)	(0.72)	(1.90)	(1.46)
Δ N. Former Regulators [(t-1) - (t-2)]	-0.026	-0.045	-0.073	-0.113	-0.080	-0.029
	(-0.20)	(-0.38)	(-0.97)	(-1.12)	(-1.63)	(-0.44)
Δ N. Former Regulators [(t) - (t-1)]	0.114	0.268***	0.032	0.064	-0.018	0.045
	(1.11)	(2.65)	(0.58)	(0.50)	(-0.26)	(0.81)
Δ N. Former Regulators [(t+1) - (t)]	-0.084	-0.109	-0.033	-0.058	-0.017	-0.022
	(-0.63)	(-0.84)	(-0.52)	(-0.46)	(-0.21)	(-0.35)
Δ N. Former Regulators [(t+2) - (t+1)]	0.075	0.074	0.117**	0.102	0.076	0.063
	(0.64)	(0.54)	(2.46)	(1.32)	(1.39)	(1.34)
Δ ln(N. Top Employees)	0.018	-0.007	-0.003	-0.036	0.035	-0.021
	(0.38)	(-0.17)	(-0.08)	(-1.13)	(1.55)	(-0.41)
Number of Observations	49,407	51,280	63,857	95,076	107,032	78,598
Cluster	Firm	Firm	Firm	Firm	Firm	Firm
Chemical-Year FEs	Y	Y	Y	Y	Y	Y

Figure 1. The Revolving Door across Industries and over Time

The figure depicts the evolution of the employment of former regulators across broad industries during 2002-2018. Employment of former regulators is measured at the company level.

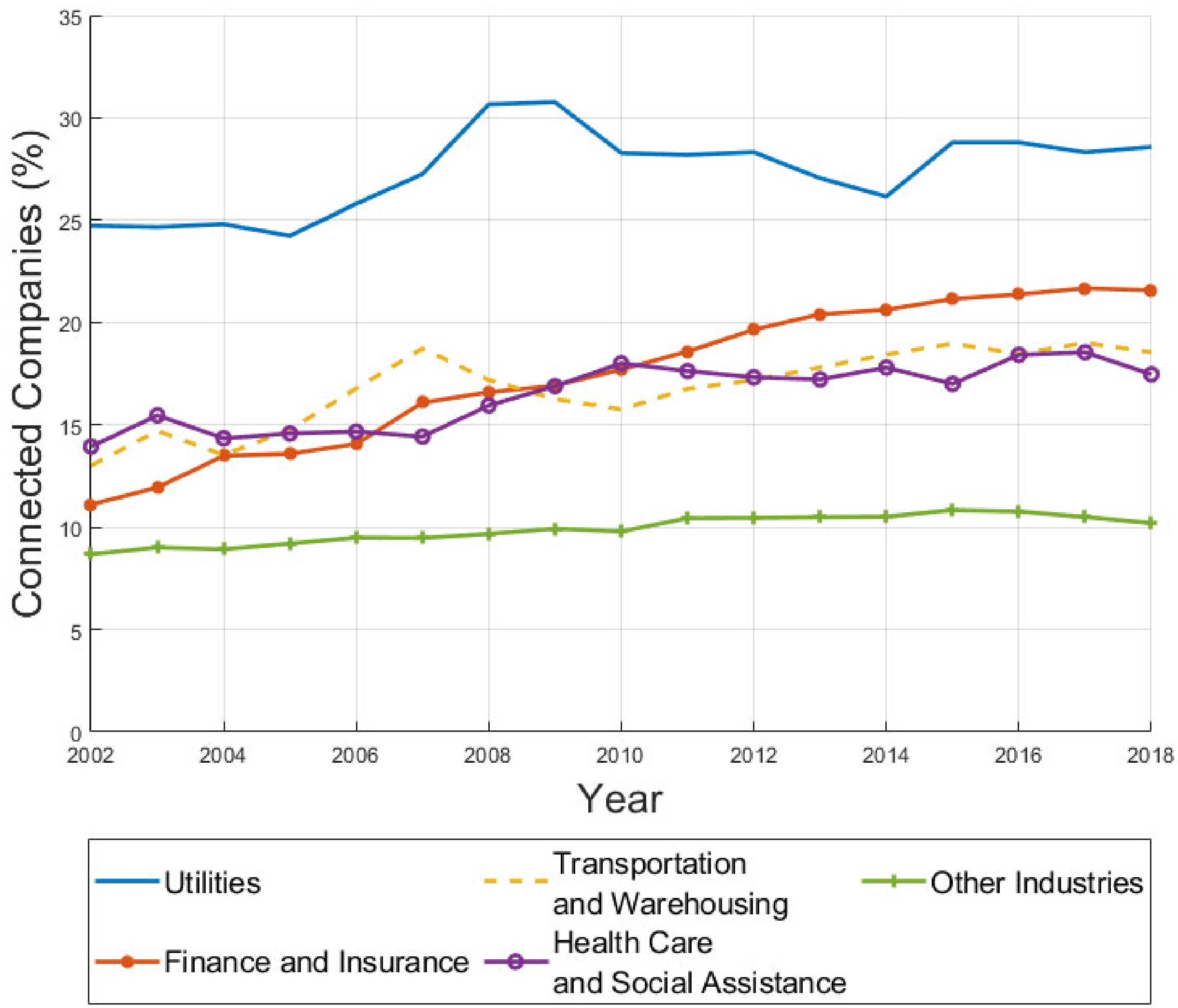

Figure 2. The Revolving Door across States

The figure depicts averages of the employment of former regulators across U.S. states during 2002-2018. Employment of former regulators is measured at the company level. A darker color reflects a higher percentage of firms headquartered in the State in question with at least one top employee with work experience at an executive branch agency that appears in *BoardEx*.