Exposing The Revolving Door in Executive Branch Agencies

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Abstract

We develop an extensive mapping of the revolving door phenomenon by examining the work experience of 420,153 individuals in top corporate positions at 12,869 firms. More than half of these firms have at least one such individual with prior experience in one of 187 executive branch agencies. We find that firms are more likely to receive procurement contracts following the appointment of a former regulator transitioning within two years of leaving the agency, a result consistent with the "knowledge" hypothesis. Less-complex contracts signed following the appointment of former regulators are more likely to be renegotiated, increasing costs for the government.

I.   Introduction

The flow of personnel from government to the private sector ("the revolving door") has been the subject of numerous academic studies and regulatory debates. Yet, no comprehensive mapping of its prevalence exists, with existing evidence being either anecdotal or focused on specific industries or settings.[1] This lack of clarity is somewhat unsettling, as the revolving door presents a pressing ethical concern. Critics of the revolving door suggest that regulators in pursuit of an industry job may signal their interest by acting leniently. After transitioning, former regulators may also use their inside knowledge to benefit their new employer. Others suggest that the possibility of a transition to the private sector generates incentives for regulators to invest in their human capital to develop valuable knowledge and expertise while in the public

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[1] 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. Agrawal and Knoeber (2001) document that, among Forbes 800 firms, in the late 1980s, it was common for outside directors to have prior political experience, including experience at agencies, or a legal background. The authors further document that, in the cross-section, such directors are more common among firms that deal more with government.

sector. With these possibilities in mind, we develop an extensive mapping of the revolving door phenomenon in the U.S. across firms, industries, regions, and years. We then use an event study framework to examine the dynamics of the revolving door and investigate the possible motivations behind it.

Specifically, we examine the prior work experience in executive branch agencies of 420,153 individuals with career histories available in *BoardEx*.[2] These individuals cover "top" corporate positions, as determined by *BoardEx*, in 12,869 unique firms during 2000-2018. We document that half of all firms in the sample have at least one individual in a top corporate position who has prior work experience in one of 187 U.S. executive branch agencies (we refer to these individuals as "former regulators"). A substantial fraction of these former regulators consists of individuals who transitioned to a top corporate position within two years of leaving an agency (following Cohen (1986), we henceforth refer to these as "direct transitions").

Former regulators tend to be appointed from agencies that are more relevant to the firm, specifically in that they regulate the firm to a greater extent. Direct transitions from an agency

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[2] A different set of papers investigate the opposite path, i.e., transitions from an industry to its regulatory agencies. One example is Gormley (1979), who studies flows of personnel from the broadcasting industry to the Federal Communications Commission (FCC), and documents that such transitions are associated with an increase in the likelihood of decisions that are favorable to the broadcasting industry. Cohen (1986) extends Gormley's analysis dynamically by examining both the entrance and the exit patterns of FCC commissioners. In contrast to Gormley, he finds that the industry background of FCC commissioners is unimportant in explaining their voting behavior. Consistent with the notion that appointing regulators is beneficial to firms, Luechinger and Moser (2014) and Shive and Forster (2017) present evidence that the appointment of former regulators results in positive abnormal returns in the U.S. Luechinger and Moser (2020) document similar findings in the European Union and additionally show that the stock price reaction to the appointment of former E.U. commissioners is more pronounced for direct transitions.

tend to endogenously occur around increases in the restrictiveness of the agency's regulations that apply to the firm. We find that, relative to the sample mean, firms appoint between 10.3% and 28.9% more former regulators, on average, following a one standard deviation increase in regulatory restrictiveness. Thus, agency experience appears to be relevant in explaining transitions.

The panel structure of the data combined with the dynamic event study framework we employ allow us to investigate the presence of benefits in the awarding of procurement contracts across a large number of agencies. To do this, we examine the dynamics of the incidence and value of procurement contracts in the years surrounding a regulator's transition to (or departure from) the firm. Our tests, which are based on granular data, allow us to mitigate omitted variable concerns. The unit of observation in our tests is the firm-agency-year triplet, which enables us to include firm-agency, agency-industry-year, and firm-year fixed effects in our specifications. These fixed effects leave only firm-agency pair time-varying omitted variables that correlate with *both* the appointment (or departure) of former regulators and with the "outcome" analyzed as the possible confounding source of variation.

Previous studies of the revolving door have focused on two non-mutually exclusive hypotheses regarding how firms benefit from the revolving door. The first hypothesis states that former regulators are appointed in exchange for favors provided to the firm *before* leaving the regulatory agency. We refer to this as the "quid pro quo" hypothesis. Examples of benefits include preferential treatment in the awarding of contracts, lenient monitoring, and tighter restrictions over the entry of new rivals. One example of a quid pro quo is the case of 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 her future employer, Boeing.[3]

The second hypothesis states that former regulators are appointed for their knowledge and expertise. We refer to this as the "knowledge" hypothesis. While this hypothesis does not necessarily make any predictions about ex-post firm behavior, firms may benefit after the appointment of a former regulator either because of the former regulator's technical knowledge or personal connections. Moreover, these benefits could reflect either improved efficiency in the allocation of government resources, if for example former regulators help firms cut through government bureaucracy, or a distortion of government resources, if for example former regulators help firms game the system.

We find systematic evidence that firms are more likely to be awarded procurement contracts *following* direct transitions of former regulators. Specifically, in a given year following a direct transition, the probability of the firm being awarded a procurement contract increases by between 74.3% and 87.1% compared to the average incidence three or more years prior to the transition. These results are consistent with the "knowledge" hypothesis. We also find some, albeit limited, evidence that firms are more likely to be awarded procurement contracts *prior* to direct transitions. In particular, firms are 75.0% more likely to be awarded a procurement contract in the year prior to the transition. However, we cannot exclude the possibility that these former regulators are appointed to help the firm execute the contract signed in the previous year. Moreover, these benefits cannot be linked to agency employees who hold the most powerful

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[3] 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/

positions at government agencies (e.g., Presidential appointees) and are not more pronounced when the incentives for these employees to engage in a quid pro quo are greater (e.g., prior to elections). These findings make it difficult to interpret the evidence of benefits prior to direct transitions as indisputably supportive of the "quid pro quo" hypothesis. Therefore, the overall evidence from direct transitions suggests that firms tend to appoint former regulators from an agency when they expect to engage in more contracting activities with that agency.

Additionally, we find no evidence of an increase in the likelihood that a firm is awarded procurement contracts following the appointment of former regulators who joined the firm more than two years after leaving the agency.[4] To the extent that the agency experience of these former regulators is outdated, this result supports the interpretation that firms benefit from the "current" knowledge or connections of former regulators.

Further tests are inconsistent with the observed trends being driven by increased regulator appointments during contract negotiations by successful firms, growing firms, or firms anticipating growth. In fact, the increase in the incidence of procurement contracts following the appointment of former regulators is present across firms with varying levels of quality. Finally,

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[4] Indirect (i.e., later) transitions should be less likely to provide evidence supportive of either revolving door hypothesis (Gormley (1979) and Cohen (1986)). For example, former regulators who joined a firm 14 years after leaving an agency (i.e., the median cooling-off period for indirect transitions) could not have provided agency-related favorable treatment in the few years that immediately preceded their transition to the firm. Furthermore, their "agency-knowledge" (either current technical information or connections at the agency) is also likely to be severely diminished. Therefore, we use indirect transitions as a counterfactual. Consistent with a weakened role of agency experience and knowledge, we find no evidence of benefits for indirect transitions involving former regulators who joined the firm more than two years after leaving government.

the increase in the likelihood that a firm is awarded procurement contracts following the appointment of former regulators is robust to analyzing connected firms relative to a propensity-score matched set of control firms.

Irrespective of whether these documented benefits are legally obtained, a natural concern is that procurement contracts allocated to firms with former regulators may be more poorly executed (Stigler (1971), Peltzman (1976)). We therefore follow the literature and investigate the propensity for contracts to be renegotiated more often and/or renegotiated for larger amounts (Haselmann, Schoenherr, and Vig (2018), Schoenherr (2019)). We find that firms are more than twice as likely to renegotiate procurement contracts following the appointment of former regulators. We also find that firms with former regulators renegotiate 24.0% larger price increases conditional on renegotiation. Thus, the benefits that accrue to firms ex-post appear to involve disproportionate increases in cost to the government. Importantly, these results are concentrated in less-complex and more-complete contracts. Therefore, the results do not appear to be driven by firms appointing former regulators when they expect contract complexity to increase.

Numerous studies have explored whether the revolving door phenomenon is indicative of quid pro quo or knowledge exchange. Nevertheless, this extensive body of literature has not reached a conclusive answer. For example, Cohen (1986), Agarwal, Lucca, Seru, and Trebbi (2014), Lucca, Seru, and Trebbi (2014), and deHaan, Kedia, Koh, and Rajgopal (2015) document that regulatory lenience is associated with a lower proportion of regulators who subsequently switch to the telecommunication and/or financial sectors, while aggressive behavior appears to pay off. These results are interpreted as broadly consistent with Che's (1995) signaling model in which a regulator of unobservable quality signals her quality to the industry, and any prospective

employers, through aggressive (rather than lenient) monitoring. Also consistent with the knowledge hypothesis, Shive and Forster (2017) find that financial firms become less risky after appointing a former financial regulator. The authors show that this effect is at least in part due to an increase in risk management activities. Finally, Cen, Cohen, Wu, and Zhang (2023) document that firms employing individuals with prior work experience in executive branch agencies benefit from disruptions to the global supply chain compared to firms with no former government employees. These firms exhibit an enhanced capacity to increase their import activities, at least in part thanks to tariff exemptions that they receive disproportionately more often than firms without former government employees.

Conversely, Tabakovic and Wollmann (2018), who study transitions from the USPTO to the private sector, document that patent examiners who grant more patents to a firm are more likely to be subsequently appointed by that firm. That is, the authors find evidence of leniency being rewarded on average. 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. Lambert (2019) documents a propensity among regulators to act leniently towards lobbying firms, while Heese (2022) finds similar evidence among German firms that have incumbent regulators on their boards. In a similar vein, Kalmenovitz, Vij, and Xiao (2022) document that agency employees who later transition to the private sector tend to issue a reduced number of regulations and regulations with lower costs of

compliance compared to their counterparts who remain in government.[5]

Our main findings, which are descriptive in nature, relate to the literature on the revolving door in three ways. First, our results support the "knowledge" hypothesis as the primary explanation for the revolving door phenomenon in the context of procurement contracts. Although we do find some evidence of an increase in the likelihood that a firm is awarded government contracts prior to transitions, this increase cannot be linked to former agency employees with more power and incentives to engage in illegal activities. Second, we complement studies that find evidence in support of the knowledge hypothesis by providing evidence across a large number of agencies. Third, our results show that firms are more likely to renegotiate their procurement contracts after appointing former regulators. Thus, any knowledge comes at the expense of the government. This evidence points to a pervasive, economically important, and understudied cost of the revolving door.

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[5] Outside of government, Kempf (2020) tracks the career paths and credit ratings issued by 245 analysts at Moody's. Consistent with the "knowledge" 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. 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 "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)).

II.   Empirical Approach

Throughout our analyses, we attempt to understand where the revolving door phenomenon is most prevalent and why it occurs. In addressing these questions, omitted variable concerns represent a non-trivial empirical challenge. The granularity of our data, however, enables us to measure firms' needs and benefits as narrowly as at the firm-agency-year level. This allows us to mitigate omitted variable concerns through the inclusion of three sets of two- or three-dimensional fixed effects.

We start by investigating the relevance of agency experience to the appointment of former regulators. An intuitive way to do this is to assess whether the appointment of former regulators to top corporate positions correlates with agencies' actions. We therefore estimate the following model to assess the "relevance" of agency experience:

(1) $\text{N Former Regulators}_{i,a,t} = \sum_{n=-1}^{+1} \alpha_{t+n} \times \text{Restrictions}_{i,a,t+n} + \eta_{i,a} + \zeta_{a,t} + \lambda_{i,t} + \varepsilon_{i,a,t}$

N Former Regulators$_{i,a,t}$ is the number of individuals covering top corporate positions at firm *i* in year *t* with prior work experience at agency *a*, i.e., former regulators. We primarily focus on appointees who are most likely to possess up-to-date technical knowledge and connections. To do so, we identify former regulators who are appointed by the firm within two years of leaving an agency (see, for example, Cohen (1986)). For those individuals, it is also easier to make the case that it is their experience in government, rather than their subsequent experience at another firm, that gives rise to any observed correlation.

The coefficient α reflects the extent to which the appointment of former regulators is correlated with the regulatory activity of the agency in which the regulator has prior work

experience. *Restrictions*$_{i,a,t}$ is the number of phrases indicating legally binding obligations and prohibitions present in the Code of Federal Regulations (CFR) (the variable is described in greater detail in Section IV.A). We focus on *Restrictions* only in the three years surrounding the transitions from the government to top corporate positions after verifying that the inclusion of additional leads and lags does not change the conclusions (it does, however, restrict the sample period we can examine).

$\eta_{i,a}$ are firm-agency fixed effects. These account for any time-invariant firm-agency pair specific omitted variables, such as the proximity of the agency's and firm's headquarters. The coefficient α consequently isolates how much the appointment (departure) of former regulators with experience at agency *a* to (from) firm *i* varies over time as the regulations issued by agency *a* that apply to firm *i* change.

$\zeta_{a,t}$ are agency-year fixed effects. These reflect any time-varying as well as time-invariant agency-specific characteristics, such as staffing, funding, political regimes, propensity to act harshly in general, etc.

$\lambda_{i,t}$ are firm-year fixed effects. These account for any firm-level time-varying or time-invariant omitted variables. Their inclusion is possible because our analyses include a large set of agencies, thus allowing for numerous firm-agency pairs. Since the model accounts for the specific relationship between each agency and each firm, agency changes over time, and firm changes over time, it is only variation at the firm-agency-year level that drives the results.

After investigating the relevance of agency experience, we analyze the possible benefits that can precede or follow the use of the revolving door. For this purpose, we estimate the following dynamic event study model:

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(2) $Y_{i,a,t} = \sum_{e=-2}^{e=+2} \alpha_e \times \mathbf{1}\{E = e\} + \alpha_{+3} \times \mathbf{1}\{E \geq 3\} + \eta_{i,a} + \xi_{a,ind,t} + \lambda_{i,t} + \varepsilon_{i,a,t}$

The event is either an increase ("appointment"), or a decrease ("departure"), in the number of

former regulators from a given agency appointed to top positions in a firm in a given year. The

unit of observation is, again, the firm-agency-year triplet; i denotes the firm, a denotes the

agency, ind denotes firm i's industry (i.e., its primary six-digit NAICS industry), t denotes the

calendar year, and E denotes the time, in years, relative to the event. The benefit of the event

study methodology, as opposed to a traditional difference-in-different model, is that it allows for

the outcome to vary in intensity over the different event years examined. As discussed in

Borusyak, Jaravel, and Spiess (2024), the model assumes homogeneous treatment effects (α_e),

relative to average levels after accounting for fixed effects, that only depend on the time relative

to the event. Identification in the model also relies on specifying a baseline period (via omission

in the model) in which there are no pre-trends. Model (2) specifies this period as three or more

years before the event. The results are robust to defining alternative baseline periods.

Importantly, the "time relative to the event" indicator variables isolate the timing of

potential benefits experienced by a firm. This allows us to distinguish between benefits that

accrue prior to and following the appointment (or departure) of former regulators, thus

distinguishing between the "quid pro quo" and "knowledge" hypotheses. In particular, the "quid

pro quo" hypothesis specifically predicts that firms receive benefits *before* appointing regulators

(e.g., former regulators are appointed as compensation for providing the firm benefits while in

office). The "knowledge" hypothesis predicts that firms benefit from the former regulator's

agency experience, if at all, *after* appointing the former regulator. Benefits, if accruing due to the

revolving door, should vanish following the departure of former agency employees, or should be

less present among individuals that are "out of touch" with the agency, i.e., those who have left the agency several years prior to joining the firm.[6]

$Y_{i,a,t}$ 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 investigated is procurement contracts, measured via an indicator variable or dollar values.

Because the events are staggered, and the unit of observation is the firm-agency-year triplet, we are able to include three sets of fixed effects ($\eta_{i,a}$, $\xi_{a,ind,t}$, and $\lambda_{i,t}$) to mitigate potential sources of confounding variation. In these specifications, the remaining source of potentially confounding variation is firm-agency pair time-varying omitted variables that correlate with *both* the appointment (or departure) of former regulators and with the "outcome" analyzed.

Many of the possible firm-agency pairs are uninformative. Consider, for example, the link between agricultural regulators and firms in finance. The inclusion of such pairs introduces a large number of uninformative zeros in the panel of data, potentially distorting results. We mitigate this concern by primarily focusing our analyses on the subset of agencies that are more relevant to the firm. In our analysis of regulations (Section IV), we focus on agencies whose regulations apply to the firm's industry. In our analysis of procurement contracts (Section V), we focus on agencies that sign a contract with at least one firm in the focal firm's industry at any point during the sample period.

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[6] Admittedly, firms should rationally appoint individuals who are expected to be beneficial in some way. Whether the benefits of appointing former regulators should specifically relate to their "agency knowledge," rather than to other aspects of their human capital, is, however, not obvious ex-ante.

Finally, we cluster standard errors at the firm-agency level. Given that the "outcomes" we study, as well as the appointment of former regulators, are primarily determined by a firm's relations with specific agencies, "treatment" in our setting is at the firm-agency level. Following Abadie, Athey, Imbens, and Woolridge (2023), we therefore cluster standard errors by firm-agency pair. Nonetheless, we recognize that there may be cases where "treatment" is correlated across a firm's relations with different agencies. In unreported tests, we alternatively cluster standard errors at the firm level. We find that the two methods generally result in similar standard errors, with clustering at the firm-agency (firm) level resulting in more conservative t-statistics in 72 (75) instances.

III.   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* the revolving door is being used before we ask *why* it is plausibly being used. We begin by identifying executive branch agencies and the top corporate individuals 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 (2020)).

A.   Data

Our study uses *BoardEx* to classify the career histories of individuals covering top

corporate positions in a sample of 12,869 unique U.S. firms during 2000-2018.[7] *BoardEx* is an

extensive directory of top corporate individuals (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. *BoardEx* attempts to

cover top individuals affiliated with all publicly traded firms and large private firms. The profile

of individuals affiliated, at any point, with those companies includes (backfilled) information on

their past employment. To minimize the risk of using backfilled data, *BoardEx* data are only

used starting in 2000, and our analyses only include firm-years labeled by *BoardEx* as "fully

profiled" (i.e., firm-years with information on all the top individuals disclosed by a firm).[8]

We extract information on the past government experience of each top corporate

individual who has work experience in federal executive branch agencies (including the various

departments of the government) that appear in *BoardEx*. We require each position to have non-

missing start and end dates to permit the creation of a time series of employment. We then define

an individual, working at a given company at a given point in time, as a former regulator from a

given agency if they joined the company after working for that agency. Following the literature,

we distinguish between two types of former regulators: those who join the firm within two years

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[7] 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, and Division President.

[8] Furthermore, *BoardEx* assigns a new identifier to a firm when it goes public or private. We merge these identifiers in order to track firms across such events.

after leaving the agency (direct transitions), and those who join the firm more than two years after leaving the agency (indirect transitions).

We obtain a list of federal executive branch agencies from the *Federal Register* (https://www.federalregister.gov/agencies). While no list of federal executive agencies is officially comprehensive,[9] the *Federal Register* provides the largest list, comprised of 433 such agencies. Our analysis is restricted to the 187 executive branch agencies from the *Federal Register* that appear in both *BoardEx* and the database on procurement contracts discussed in Section V.A.

B.   Summary Statistics

Panel A of Table 1 reports the fraction of firm-years that have at least one former regulator (from any agency) appointed to a top corporate position. Panel B reports the fraction of firm-agency-year triplets involving firms with former regulators—the focus of our regression analyses. The averages reported are calculated for the full sample period. As shown in the first row of Panel A, 51.8% of firms have at least one former regulator in a top corporate position, and 17.8% of the firms have at least one former regulator in a top corporate position who joined the firm within two years of leaving the agency. The transitions we study are, thus, *substantially* more prevalent than corporate political connections, lobbying, or campaign contributions (all of which are accounted for in our regression analyses through the inclusion of firm-year fixed effects). The table also reports summary statistics for three subsamples that are used in some of the tests that follow: (1) firms for which we could determine the NAICS industry ("w/ NAICS

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

Codes"); (2) firms operating in industries that are regulated by a given agency ("Restrictions > 0"); and (3) firms operating in an industry that has signed at least one contract with the agency in question ("Industry Contracts > 0").

[Insert Table 1 about here]

Direct transitions involve former regulators with a median (average) work experience at government agencies of five (eight) years and observed a 0.1 (0.4) year cooling-off period prior to moving to a top position in the private sector (not tabulated). Thus, these are truly "direct" transitions. Indirect transitions involve former regulators with a median (average) work experience at the agency of three (five) years and observed a median cooling-off period of 14 (17) years prior to joining the firm in question. Thus, not only have these individuals left the agency long before joining the firm on average, questioning the relevance of their agency experience, but they also have less agency experience on average.

As shown in Panel C of Table 1, direct transitions of former regulators are predominantly from the Department of Defense, the Federal Reserve, the Executive Office of the President, the Department of State, and the Department of Energy. Using BoardEx's classification of position titles, the most common positions held by directly transitioning former regulators while at the agency are Officer (8.58%), Attorney (4.49%), Committee Member (3.71%), Director - Non-Board (2.51%), and Member (2.33%). For the set of indirectly transitioning former regulators, the most common positions are Officer (18.38%), Captain (4.96%), Attorney (4.58%), Various Positions (3.78%), and Military Service (3.68%). While it would be useful to classify these positions based on their power within the agency, any classification system would be ad hoc without a careful investigation of each agency's organizational structure. In Section VI, we provide a more rigorous classification of powerful agency position titles by using the U.S.

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Government Publishing Office's (2016) "Plum Book" to identify Presidential appointees. For this set of former regulators, the most common positions are Commissioner (19.34%), Secretary (17.38%), Administrator (12.46%), Attorney General (11.15%), and Chairman (7.21%).

The data also enable us to examine how the propensity to appoint former regulators varies across firms in different industries, at least for the subset of firms with available industry classification data. *BoardEx* does not systematically report industry classifications. We therefore 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 North American Industry Classification System (NAICS) industry code. The industry distribution of the appointment of former regulators is tabulated in Panel D of Table 1. The industry classification used in Panel D of Table 1, for the sake of conciseness, is the highest NAICS industry level. In terms of direct transitions, among the industries for which we have at least 10 firms, the percentage of firms that appoint former regulators is highest among firms operating in the "Utilities," "Educational Services," and "Agriculture, Forestry, Fishing and Hunting" industries.

Figure 1 plots the evolution of the revolving door phenomenon, specifically focusing on direct transitions, over time across broad NAICS industries. The industries are ordered based on the prevalence of the revolving door phenomenon during 2000-2018. Many industries show declines in the percentage of firms with appointed former regulators in the first three years of the sample. However, this appears to be primarily a function of a large increase in the number of firms with full coverage in *BoardEx* during 2000-2003. The number of firms in the sample stabilizes by 2003. We find notable increases in the appointment of former regulators in the "Education Services" and "Agriculture, Forestry, Fishing and Hunting" industries. We find

moderate declines in the appointment of former regulators in the "Professional, Scientific, and Technical Services," "Retail Trade," and "Mining" industries.

Most industries, however, display relatively stable trends in the appointment of former regulators. From 2003 to 2018, only four industries exceed an eight percentage point change in the percentage of firms appointing former regulators. Thus, cross-sectional differences explain a large portion of the variation in the appointment of former regulators. This result underscores the importance of including cross-sectional fixed effects in our regressions. We therefore report all regressions with firm-agency fixed effects.

[Insert Figure 1 about here]

Finally, Figure 2 provides a visualization of how the appointment of former regulators varies across states. Darker colors indicate a higher fraction of firms headquartered in that state with at least one former regulator directly appointed from an agency in an average year. The states with the highest fraction, and at least 10 firms, are the District of Columbia (51.3% of firm-years), Delaware (39.4%), and Virginia (37.2%) while West Virginia (10.6%), New Hampshire (10.3%), and Mississippi (7.7%) have the lowest incidence of firms that have former regulators in top corporate positions.[10] In general, and in line with earlier evidence by Agrawal and Knoeber (2001), the revolving door phenomenon appears to be more prevalent in states closer to the District of Columbia. This result is consistent with former regulators preferring not to significantly relocate when transitioning to the private sector.

[Insert Figure 2 about here]

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[10] Wyoming (36.7%), North Dakota (6.8%), and New Mexico (0%) are also outliers, although only three, six, and seven firms in our sample are headquartered in each state, respectively.

IV.   Regulations

The data summarized in Section III.B cover individuals who previously worked at executive branch agencies and the firms that subsequently appoint them to top corporate positions. This does not, however, necessarily imply that the agency experience of these individuals is relevant for their appointment. To provide a case for the relevance of agency experience, we examine the relation between the appointment of former regulators (specifically direct transitions) and the restrictiveness of the regulations that govern each firm. We recognize, of course, that regulations are one of many aspects of agency activities that are relevant to firms. They are, nevertheless, a source of complexity and uncertainty that pervasively affects most, if not all, firms.

A.   Data

For this purpose, we use a proxy for the extent of regulation as identified by *RegData*,[11] a database containing regulatory data from 1970 through 2019. The proxy, *Restrictions,* is an estimate of the number of phrases indicating legally binding obligations and prohibitions present in the 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 to publish their regulations. *RegData* also uses textual analysis to estimate the relevance of each portion of the CFR to each NAICS industry, allowing an estimate of regulations at the agency-industry-year level. Table 2 tabulates the average extent of regulation

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[11] https://www.quantgov.org/history

in the 10 most and 10 least regulated four-digit NAICS industries during 2000-2018. Industries that heavily employ chemicals are easy to spot among the most regulated.

[Insert Table 2 about here]

As previously discussed, *BoardEx* does not systematically report industry classifications. We therefore retrieve each firm's primary NAICS industry code from *Capital IQ*. This ensures the widest match between the companies that appear in *BoardEx* and the restrictions in *RegData*.

B.   Regulations and the Revolving Door

To make a case that former regulators are not appointed arbitrarily, we investigate the extent to which increased regulation is associated with an increase in regulated firms appointing top individuals with experience at the agency in question, specifically focusing on direct transitions. For this purpose, we estimate Model (1) using *ln(Restrictions + 1)*, 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*, plus one. The sample includes all firms with industry-affiliation available in *Capital IQ*. All non-indicator independent variables included in Table 3 and in all the tables that follow are standardized to facilitate interpretation.

The results in Columns 1, 2, and 3 of Table 3 include all firms with NAICS data; Columns 4, 5, and 6 include only agencies that regulate the firm in question in a given year (i.e., with strictly positive *Restrictions*.) Specifications 1 and 4 include firm-agency fixed effects, specifications 2 and 5 include both firm-agency and firm-year fixed effects, and specifications 3 and 6 include firm-agency, firm-year, and agency-year fixed effects.

[Insert Table 3 about here]

In Column 1 we find that, in the time series, former regulators are appointed both prior to and following the enactment of new regulations. Economically speaking, firms appoint 15.8% more former regulators, on average, in the year after a one standard deviation increase in regulation restrictiveness. This result is robust to the inclusion of firm-year fixed effects in Column 2 but becomes less significant, both statistically and economically, after including agency-year fixed effects in Column 3. Much of the correlation between changes in regulation and the appointment of former regulators therefore appears to be attributable to time-varying agency characteristics, such as agency-wide regulation policy, rather than how those regulations apply across industries. In Columns 4, 5, and 6, we focus on agencies that regulate the firm in a given year. We continue to find evidence supporting the "relevance" of the former regulator's agency experience. The magnitude of the coefficients become greater once we focus on "relevant" agencies, indicating that the high number of firm-agency-year pairs with zero *Restrictions* in Column 1 biases the results downward. In particular, we find that firms appoint between 16.3% (Column 6) and 28.9% (Column 5) more former regulators, on average, in the year after a one standard deviation increase in regulation restrictiveness. Coefficient estimates of Columns 4, 5, and 6, along with 90% confidence intervals, can be visualized in Figure 3.[12]

[Insert Figure 3 about here]

The results in this section show that firms tend to appoint former regulators surrounding, and particularly following, changes in regulation. We interpret these results as indicative that agency experience is an important consideration when firms appoint former regulators and

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[12] The coefficient of ln(Restrictions + 1) [t-1] in Regression 6 of Table 3 loses its significance at the 10% level when the clustering is done at the firm level.

proceed to investigate the benefits firms may receive from this agency experience. Naturally, regulation is just one of many ways through which agencies affect firms, thus making agency experience relevant for firms. Having shown the significance of this aspect of agency actions, we control for any unspecified sources of relevance in the subsequent regressions by including agency-industry-year fixed effects.

V.   Possible Benefits

Having provided evidence that the revolving door phenomenon is more prevalent where it is more relevant, we next investigate our two hypotheses concerning why firms appoint former regulators (the "quid pro quo" and "knowledge" hypotheses). To do so, we focus on procurement contracts and investigate whether there is any evidence of an abnormal *increase* (or decrease) in the signing of procurement contracts in the years that surround the appointment (or departure) of former regulators. While procurement contracts are separate from the regulations studied in the previous section, they represent a second important, and perhaps orthogonal, source of relevance of an agency to a firm. From an empirical perspective, analyzing procurement contracts is beneficial in at least two ways. First, given that multiple agencies sign contracts with private contractors, we can greatly mitigate omitted variable concerns through the inclusion of firm-agency, agency-industry-year, and firm-year fixed effects. Second, the data allow us to observe a measure of contract execution: contract renegotiations. This, in turn, allows us to investigate whether any benefits to firms come at the expense of the government.

A.   Procurement Contracts: Data

Data on procurement contracts are from *USASpending.gov*. The dataset contains contracts signed by 187 federal agencies that can be matched to *BoardEx*.[13] We use these data to construct two measures of contracting with the U.S. government. *Contract* is an indicator variable that takes the value of one if the agency in question signs a contract with the firm in a given year, and 0 otherwise. *Ln(Contract Value + 1)* is the natural log of the total initial amount to be paid by a federal government agency across all contracts signed by that agency with the firm in a given year.[14] The sample includes all government contracts but excludes indefinite delivery vehicles, grants, direct payments, loans, insurance, sub awards, and other financial assistance. We remove contracts with a missing or negative "Federal Action Obligation" value, and match to firms in *BoardEx* using the same fuzzy string-matching algorithm used in Section III.B. We are able to match procurement contracts to 7,149 unique firms.

Table 4 lists the top 15 agencies ranked by total dollar value of contracts signed during 2000-2018. Among those, the agencies with the highest total value of contracts are the Army, Navy, Air Force, Defense Logistics Agency, and Veterans Administration. These agencies are closely linked to the Department of Defense, which is the agency most represented among our sample of former regulators (see Panel C of Table 1), providing a suggestive link between procurement contracts and the appointment of former regulators.

[Insert Table 4 about here]

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[13] Some agencies are operated within parent agencies, such as the United States Patent and Trademark Office operating within the U.S. Department of Commerce. When a sub-agency does not appear in *BoardEx*, we assign the contracts to the parent agency.

[14] It is the "Federal Action Obligation" variable in *USASpending.gov.*

B.   Procurement Contracts: Results

We use the procurement contracts data to investigate whether the appointment (or departure) of former regulators, in particular direct transitions, is associated with a change in the incidence or dollar value of procurement contracts. We use the event study framework of Model (2) discussed in Section II and focus on firm-agency pairs in which the agency signs a contract with at least one firm in the focal firm's industry at any point in the sample period (i.e., *Industry Contracts* > 0). The unit of observation is the firm-agency-year triplet. The event is either the appointment (Columns 1 and 2), or departure (Columns 3 and 4) of a former regulator to/from the firm. For each appointment or departure event, we require that the firm has observations in *BoardEx* in the two years preceding that event so we can observe the change in the allocation of contracts (if any) over time surrounding the appointment or departure of the former regulator. This condition results in slight differences in sample size depending on the type of event examined.

[Insert Table 5 about here]

Contract is the dependent variable in Columns 1 and 3 of Table 5 and *ln(Contract Value + 1)* is the dependent variable in Columns 2 and 4. To facilitate visualizing the results in Table 5, Figure 4 plots the coefficients corresponding to regressions on *Contract* (i.e., Columns 1 and 3 of both panels in Table 5). As mentioned in Section II, these coefficients are estimated relative to the average level of the dependent variable three or more years prior to the event.

[Insert Figure 4 about here]

The "quid pro quo" hypothesis predicts a significantly higher incidence or value of procurement contracts *prior* to the direct transition of former regulators to the private sector—

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recall the Druyun case. In contrast, a higher incidence or value of contracts *following* the appointment of former regulators would be consistent with the "knowledge" hypothesis.

The results presented in Panel A of Table 5 show an upward trend in both the incidence and value of contracts prior to the appointment of the former regulator, culminating in a significantly higher incidence of contracts in event year -1 (Column 1) and value of contracts in event years -1 and 0 (Column 2) relative to years -3 and earlier. These coefficients correspond to a 75.0% higher probability of receiving a contract and a 82.5% greater value of contracts in event year -1. While it is tempting to interpret this evidence as supportive of the "quid pro quo" hypothesis, more evidence linking benefits to firms with agency employees who possess the power and incentives to engage in a quid pro quo is needed to make this determination. We further investigate this possibility in Section VI.

The event study also shows significant evidence of an abnormally high incidence and value of contracts *following* the appointment of former regulators, including three or more years after the appointment. For example, firms are 81.7% more likely to be awarded a contract and receive 91.5% more value of contracts in the year after the appointment relative to years -3 and earlier. [15] This result is consistent with a version of the "knowledge" hypothesis in which firms either benefit from the former regulator's up-to-date technical knowledge or agency connections. We do not take a stance on whether the higher incidence in the likelihood of signing contracts with an agency following the transition of a former regulator stems from information acquired by the firm before or after the transition. However, we can dismiss the possibility that the post-

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[15] Note that the relatively larger magnitude for the value of contracts compared to the incidence of contracts indicates that the value per contract is also larger.

transition surge in the incidence of contracts is solely a result of contracts that the regulator helped the firm win while still serving in the agency. We believe that it is highly unlikely that those situations would protract through years +3 and after.

In Columns 3 and 4, we use departures to investigate whether the results are reversed after the departure of former regulators.[16] We find no evidence of an abnormally high incidence of contracts prior to the regulator's departure. This lack of benefits may be the endogenous reason for the departure. The outcomes turn negative, though statistically insignificant, three or more years after the departure.

To investigate the possibility that these results for appointments are spurious, we consider, as a placebo, the pattern of procurement contracts around the appointment (and, for symmetry, departure) of regulators that are likely to have less current technical knowledge or connections because they left the agency several years before joining the firm (recall that the median cool-off period for such indirect transitions is 14 years). Panel B of Table 5 presents these results. We find that indirect transitions are not associated with significant benefits, in terms of procurement contracts, following their appointment. We also find no change in the rewarding of procurement contracts around the departure of this set of former regulators.

In conclusion, the evidence we document can be interpreted as supportive of the "knowledge hypothesis" that is specific to the subset of direct transitions, i.e., the set of events for which recent agency experience is likely to be most relevant. This suggests that the results

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[16] The coefficient of Event Year = 0 in Column 4 of Panel A of Table 5 loses its significance at the 10% level when clustering is done at the firm level.

are a function of the current knowledge or connections provided by direct transitions, rather than other aspects of former regulators' human capital.

C.   Alternative Interpretations

We next consider the possibility that the observed trends are attributable to endogenous matching between regulators and firms. One potential scenario is that regulators gain insights into firm quality during the procurement contract application process and subsequently decide to transition to firms that have demonstrated strong performance, growth, or are anticipated to grow. To address this issue, we utilize cross-sectional variation in firm quality in the year before a transition occurs.

To measure performance, we use return-on-assets (ROA), defined as net income divided by total assets in the year that precedes the transition. We then interact the event-time indicators with the firm's ROA in the year before the transition. We standardize ROA to have a mean of zero and standard deviation of one such that the main effects for the event-time indicators correspond to the effect for firms that have the average level of ROA. The results are shown in Column 1 of Table 6.

[Insert Table 6 about here]

Among the coefficients of the interaction terms, only one (*Event Year = +1*) is statistically significant, and only at the 10% level. This result provides only limited support for the idea that firms are awarded contracts and appoint former regulators because they have better prior performance.

As measures of growth, we employ sales growth (defined as the percentage change in annual sales) and Tobin's Q (defined as total assets minus book equity plus market capitalization, all divided by total assets). As with ROA, we interact the event-time indicators

with standardized values of these variables in the year before the transition. The results are reported in Columns 2 and 3 of Table 6. Among the 12 interaction terms, only one coefficient is statistically significant. Specifically, the interaction of *Event Year = +2 x Sales Growth = -1* shows a negative coefficient, which is negative and statistically significant at the 10% level. The general lack of significance among the interaction-term coefficients appears to contradict the notion that contracts are more likely to be awarded to firms with former regulators that are experiencing higher growth or are anticipated to grow more.

Importantly, the main effects for the event-time indicators *following* the transition are statistically significant across all three regressions. This indicates that the increase in the incidence of contracts following the appointment of former regulators is present across firms with varying levels of profitability, sales growth, or Tobin's Q.[17]

To further account for selection, e.g., larger firms being able to attract more former regulators and, at the same time, being more likely to win procurement contracts, we create a propensity-score matched sample of control firms. We identify control firms by matching firms with a direct transition to firms without a direct transition (at any point in the sample period) in the year before the transition takes place. The matched firms must be in the same four-digit NAICS industry and are matched on total assets, profitability, and Tobin's Q using a nearest-neighbor algorithm. We then run our analysis on a sample consisting of firms with a direct transition and the set of matched control firms.

[Insert Table 7 about here]

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[17] The coefficients of Event Year = -1 of Columns 1 and 3 of Table 6 become significant at the 10% level when the clustering is done at the firm level.

[Insert Figure 5 about here]

The results are shown in Table 7 and can be visualized in Figure 5. We find that the increase in contracting following direct transitions, as documented in Table 5, remains significant in this subsample. Comparing the coefficients between the two tables, the results in Table 7 are, in most cases, even stronger. The notable exception is the year prior to the direct transition, in which the coefficients are smaller in magnitude and statistically insignificant in Table 7. Nonetheless, the increase in contracting following direct transitions is robust using a propensity-score matched sample of control firms.

The results in Table 6 and Table 7 indicate that the benefits we observe (i.e., increased contracting) surrounding the appointment of former regulators do not appear to be entirely explained by differences in firm quality. We interpret this as indicating that it is unlikely that these benefits are merely a function of regulators selectively transitioning to higher quality firms.

VI.   Quid Pro Quo?

We now turn to further investigating the evidence of benefits prior to the direct transition of former regulators. Should we conclude that this is evidence of the "quid pro quo" hypothesis? Such a conclusion would be premature without further investigation. For example, it is possible that firms appoint former regulators to help execute (or renegotiate) contracts they signed in the year prior to the transition. If the benefits prior to direct transitions are the result of quid pro quos, then the benefits should plausibly come from regulators who possess the power and, possibly, the incentives to engage in quid pro quos. We therefore investigate whether these results stem from such regulators.

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Specifically, we focus on regulators who are likely to possess sufficient power to engage in a quid pro quo: Presidential appointees. To determine which of the former regulators in our sample were Presidential appointees, we match their agency position title in *BoardEx* with the titles of Presidential appointees listed in the U.S. Government Publishing Office's (2016) "Plum Book." We then consider how incentives may change over time. In particular, a non-trivial number of Presidential appointees are replaced at the end of each President's term. Powerful individuals expecting to be replaced may attempt to facilitate a transition to the private sector by behaving leniently toward the industry immediately prior to their expected replacement, i.e., leading up to a Presidential election.[18] To investigate this possibility, we classify 2004, 2008, 2012, and 2016 as Presidential Election Years.

We focus on transitions of Presidential appointees that occur in the year that follows a Presidential election (i.e., Event Year = 0 and Election Year = +1). We investigate whether those transitions are preceded by an abnormally high incidence of procurement contracts as the "quid pro quo" hypothesis would predict. As before, we exploit the dynamic structure of our panel in an event study setting.

[Insert Table 8 about here]

Table 8 presents the results. For this set of transitions of Presidential appointees, we find no evidence of an increase in the likelihood that the firm is awarded procurement contracts in the

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[18] This is especially true in the case of a different political party winning the Presidency. For example, while both President Clinton and President Trump signed executive orders creating mandated cooling-off periods for Presidential appointees, both Presidents revoked their orders just before leaving office, effectively allowing their appointees to directly transition to jobs in the private sector.

two years that precede the transition. That is, the significant results in Table 5 do not appear to stem from agency employees with more power and greater incentives.

In unreported tests, we also investigate the possibility that benefits may occur (prior to direct transitions) more frequently in more corrupt states or in firms with a history of financial misstatements. None of these tests provide support for the "quid pro quo" hypothesis. While a complete rejection of the "quid pro quo" hypothesis for the average firm would require the ability to properly identify all instances in which a quid pro quo may occur, we are able to conclude that the evidence across many settings does not suggest that quid pro quos are the primary motivation behind the revolving door in executive branch agencies. Rather, it appears that the increase in the incidence and value of contracts prior to direct transitions is the result of firms appointing government employees to manage the contracting process. For instance, firms might appoint former regulators when their sales to the government (although not their general sales) grow, or are anticipated to grow, in the future.

VII.   Contract Renegotiations

While the evidence thus far supports the "knowledge" hypothesis, distortions could still be present. In particular, firms receiving government contracts could worsen contract execution, at the expense of the government, after appointing former regulators.

To investigate whether this is the case, we follow Schoenherr (2019) and Brogaard, Denes, and Duchin (2021) and examine contract execution following the appointment of former regulators. This can be operationalized by identifying price-increasing revisions for contracts signed under the guidance of former regulators. We therefore examine the probability of a contract being renegotiated conditional on the presence of former regulators in top corporate positions at the firm.

In this test, the unit of observation is each individual contract signed between a firm and an agency. The dependent variable, *Renegotiation*, is an indicator variable denoting whether a signed contract experiences a price increase at any point in time before its completion. Our main variable of interest, *Former Regulator*, is an indicator variable that takes the value of one if the firm in question has an appointed former regulator when the contract is signed, and zero otherwise. We also control for the initial value of the contract and the number of offers for the contract received by the agency during the bidding process. If the number of offers is not reported, we set it equal to one and set an indicator variable, *N Offers Missing*, equal to one. Given that the vast majority of firms sign many contracts with the same agency, we are able to include the same sets of fixed effects used in previous tests, to which we add award-type fixed effects and pricing-type fixed effects.[19]

[Insert Table 9 about here]

The results for this test, corresponding to direct transitions, are reported in Column 1 of Panel A of Table 9. We find evidence that the presence of former regulators benefits firms that execute those contracts less effectively, more than doubling the likelihood of renegotiation (coefficient of 0.0124 vs. a sample average of 0.0116).

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[19] Award types include government-wide agency contracts approved by the Office of Management and Budget, indefinite delivery contracts, General Services Administration or Veterans Affairs federal supply schedules, basic ordering agreements, and blanket purchase agreements. Pricing types include fixed price redetermination, fixed price level of effort, firm fixed price, fixed price with economic price adjustment, fixed price incentive, fixed price award fee, cost plus award fee, cost no fee, cost sharing, cost plus fixed fee, cost plus incentive fee, time and materials, labor hours, order dependent, combination, other, and not reported.

We next investigate whether the appointment of former regulators is associated with a higher increase in price when a contract does end up being renegotiated. Conditioning on renegotiation, we aggregate all changes in negotiated value over the life of the contract and divide by the initial obligation to calculate *Renegotiation %*. We then winsorize this quantity at the 1% and 99% levels. In our sample, the average *Renegotiation %* is 206%, which is consistent with prior estimates in the literature (Brogaard et al. (2021)). We use this quantity as the dependent variable in Column 2. The results indicate that, when renegotiated, contracts signed while former regulators are in top corporate positions are associated with 24.0% larger price-increasing revisions compared to contracts signed between the same firm-agency pair prior to the appointment of a former regulator.

Columns 3 and 4 represent a falsification test examining contracts signed when firms have an appointed former regulator that indirectly transitioned from the agency. The results show that renegotiations and larger price increases are not more pronounced for this group. That is, the results are economically and statistically stronger when firms have appointed a former regulator that directly transitioned from the agency. We interpret these results as consistent with firms benefiting from the up-to-date knowledge or connections of former regulators.

One possible concern with the interpretation of these results is that firms may appoint former regulators to help with the execution of more complex contracts. The complexity of these contracts may then increase the probability of subsequent renegotiation and result in larger price-increases. To explore this concern, we classify contract complexity based on the contract's pricing type, following Bajari and Tadelis (2001), Bajari, Houghton, and Tadelis (2014), and Brogaard et al. (2021). These papers argue that cost-plus pricing can better accommodate contracting concerns for complex projects and are more likely to be incomplete, while fixed

pricing is more likely to be used for complete contracts related to relatively straightforward projects. Following Brogaard et al. (2021), we define fixed-price contracts as contracts with pricing types "fixed price redetermination," "fixed price level of effort," "firm fixed price," "fixed price with economic price adjustment," "fixed price incentive," and "fixed price award fee." Similarly, we define cost-plus contracts as contracts with pricing types "cost plus award fee," "cost plus fixed fee," "cost plus incentive fee," and "time and materials." We then run our analysis separately for fixed-price and cost-plus contracts. If firms with former regulators are more likely to renegotiate contracts because of contract complexity, then we would expect the results to be stronger for cost-plus contracts.

Columns 1 and 2 of Panel B of Table 9 report the results for these contracts. Firms are more likely to renegotiate fixed-price contracts, and for a larger amount, when the contract is signed while the firm has a former regulator that directly transitioned from the agency. That is, contrary to the concern that firms may have appointed former regulators to help with the execution of more complex contracts, we find that our results are concentrated in fixed-price contracts. In contrast, the results for cost-plus contracts (Columns 3 and 4) are both statistically and economically insignificant.

Thus, overall, the evidence in Table 9 can be interpreted as supporting the idea that the revolving door distorts the allocation of contracts to the benefit of firms that subsequently deliver a more-expensive-than-anticipated product. That is, contracts signed after the appointment of former regulators result in a substantially higher cost to the government even when contract complexity is relatively lower. Our calculations imply incremental costs of nearly $30 billion during 2000-2018. Firms without direct-transition former regulators renegotiate 0.86% of their

contracts, for an average price increase of 188% of the initial contract value.[20] Firms with direct-transition former regulators renegotiate contracts 2.10% of the time (0.0210 = 0.0086 + 0.0124 from Table 9, Column 1), for an average price increase of 238% of initial value (2.38 = 1.88 + 0.50 from Table 9, Column 2). Thus, for firms with former regulators, the incremental cost per contract is 3.38% (0.0375 = 2.10% x 238% - 0.86% x 188%). Over the entire sample period (2000-2018), firms with direct-transition former regulators signed contracts worth a total of $813 billion in initial value. The results therefore imply that the revolving door phenomenon resulted in the government paying an additional $27.5 billion (813b x 3.38%) over this period.

VIII.   Conclusion

The revolving door between government and the private sector often draws criticism from the media, as well as from academics. However, there has been no systematic documentation of the phenomenon. In this paper, we develop an extensive mapping of the revolving door phenomenon tracking the flow of personnel from 187 federal executive branch agencies (in the U.S.) to top corporate positions. We show that the revolving door phenomenon is indeed pervasive and that a substantial fraction of those flows are direct transitions from an agency to a firm. Moreover, the agency experience of former regulators appears highly relevant for their appointment to top corporate positions, as former regulators tend to be appointed following the enactment of new regulations.

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[20] These figures are calculated as the average dependent variables for Columns 1 and 2 of Table 9, respectively, for firms without a former regulator that directly transitioned from the regulatory agency.

As to why firms appoint former regulators to top positions, the results are largely consistent with the "knowledge" hypothesis. Specifically, firms benefit *following* the appointment of former regulators in terms of a higher incidence (and value) of procurement contracts relative to average levels three or more years prior to the appointment. This result is present across a large set of agencies. In contrast, the results fail to support the "quid pro quo" hypothesis among sets of employees with sufficient power and incentives to engage in such illegal activities.

Placebo tests exploiting regulators that are less likely to possess current knowledge or connections do not show the aforementioned patterns, suggesting the results are closely tied to the recency of the former regulator's agency experience. Importantly, these results are present in an econometrically stringent event study setting that allows examination of the timing of possible benefits and the inclusion of firm-year, agency-industry-year, and firm-agency fixed effects, narrowing space for confounding sources of variation. Our results therefore present novel systematic evidence on the use of the revolving door across a large set of agencies and firms.

While we find support for the "knowledge" hypothesis, our results nevertheless highlight concerns associated with the revolving door phenomenon. We find that transitions of former regulators are associated with additional costs to the government in the form of price-increasing contract renegotiations. Moreover, this result is not accounted for by contract complexity. Back-of-the envelope calculations indicate that these incremental costs are large, adding up to nearly $30 billion during the sample period. While it is not possible to draw policy implications without a comprehensive analysis of all costs and benefits associated with transitions, our results highlight an important cost that should be considered in future research.

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TABLE 1

Prevalence of the Revolving Door

This table provides averages of the prevalence of the revolving door (i.e., the prevalence of former regulators in top corporate positions) in the U.S. during 2000-2018. The prevalence of former regulators is measured either at the firm-year level (Panels A, C, and D) or at the firm-agency-year level (Panel B). In the former, each panel reports the percentage of firms with at least one individual in a top corporate position with work experience in at least one of 187 executive branch agencies. In the latter case, the panel reports the percentage of individuals covering top corporate positions who have work experience at the agency in question. Direct transitions involve individuals who were appointed to a top position in the firm within two years of leaving the agency. Indirect transitions involve individuals who are appointed to a top position in the firm more than two years after leaving the agency. Panels A and B report statistics for the different samples used in our analysis, Panel C reports statistics by agency, and Panel D reports statistics by NAICS industry, whenever available in *Capital IQ*.

Panel A. Firm-Year Pairs (%)

	Direct Transitions	Indirect Transitions	Total	N. Obs.
Full Sample	17.8%	48.1%	51.8%	103,074
w/ NAICS Codes	18.0%	49.8%	53.6%	75,602
Restrictions > 0	17.6%	49.6%	53.6%	55,406
Industry Contracts > 0	9.2%	41.0%	43.3%	71,633

Panel B. Firm-Agency-Year Triplets (%)

	Direct Transitions	Indirect Transitions	Total	N. Obs.
Full Sample	0.16%	0.54%	0.65%	19,274,838
w/ NAICS Codes	0.15%	0.54%	0.65%	14,137,574
Restrictions > 0	0.16%	0.58%	0.70%	9,175,044
Industry Contracts > 0	0.21%	1.13%	1.28%	4,438,542

Panel C. Agency Distribution of Former Regulators. Firm-Year Pairs (%).

Agency	Direct Transitions
Department of Defense	6.122%
Federal Reserve	4.253%
Executive Office of the President	2.176%
Department of State	1.074%
Department of Energy	0.917%
Securities and Exchange Commission	0.840%
Department of Justice	0.745%
Department of Commerce	0.659%
Department of the Treasury	0.626%
Department of Health and Human Services	0.573%
Food and Drug Administration	0.525%
Federal Deposit Insurance Corporation	0.491%
National Aeronautics and Space Administration	0.470%
Federal Communications Commission	0.468%
National Science Foundation	0.410%
…	…

Panel D. Industry Distribution of Former Regulators. Firm-Year Pairs (%).

NAICS Industry	Direct Transitions	N. Obs.
Public Administration	100.0%	3
Utilities	35.2%	1,889
Educational Services	34.2%	351
Agriculture, Forestry, Fishing and Hunting	30.7%	251
Transportation and Warehousing	25.3%	1,824
Professional, Scientific, and Technical Services	24.4%	2,680
Health Care and Social Assistance	21.1%	1,555
Other Services (except Public Administration)	21.0%	233
Finance and Insurance	19.5%	15,716
Information	18.9%	7,680
Accommodation and Food Services	18.4%	1,409
Administrative and Support and Waste Management and Remediation Services	16.3%	1,136
Manufacturing	16.0%	27,153
Retail Trade	15.5%	3,000
Wholesale Trade	15.3%	1,779
Construction	14.5%	1,008
Mining	13.9%	3,856
Real Estate Rental and Leasing	13.3%	3,396
Arts, Entertainment, and Recreation	13.0%	683

TABLE 2

Industry Distribution of Regulations

This table reports the 10 most and 10 least regulated four-digit NAICS industries based on the average of *Restrictions* during 2000-2018. *Restrictions* is an estimate of the number of phrases indicating legally binding obligations and prohibitions present in the CFR. The variable is obtained from *RegData*, a database that uses textual analysis to identify regulatory phrases in the CFR.

NAICS Descriptions	NAICS Code	Restrictions
Pesticide, Fertilizer, and Other Agricultural Chemical Manufacturing	3253	70,175
Petroleum and Coal Products Manufacturing	3241	66,342
Resin, Synthetic Rubber, and Artificial and Synthetic Fibers and Filaments Manufacturing	3252	65,901
Pharmaceutical and Medicine Manufacturing	3254	63,647
Other Chemical Product and Preparation Manufacturing	3259	62,747
Soap, Cleaning Compound, and Toilet Preparation Manufacturing	3256	57,126
Architectural, Engineering, and Related Services	5413	54,722
Basic Chemical Manufacturing	3251	51,701
Paint, Coating, and Adhesive Manufacturing	3255	49,953
Radio and Television Broadcasting	5151	49,167
…	…	…
Glass and Glass Product Manufacturing	3272	467
Management of Companies and Enterprises	5511	434
Converted Paper Product Manufacturing	3222	425
Coating, Engraving, Heat Treating, and Allied Activities	3328	409
Computing Infrastructure Providers, Data Processing, Web Hosting, and Related Services	5182	406
Computer and Peripheral Equipment Manufacturing	3341	362
Lessors of Real Estate	5311	356
Software Publishers	5112	276
Electric Lighting Equipment Manufacturing	3351	275
Drycleaning and Laundry Services	8123	215

TABLE 3

Regulations and the Revolving Door

This table presents regressions of the presence of the revolving door on agency regulations. The unit of observation is the firm-agency-year triplet. The dependent variable, *N Former Regulators*, is the number of individuals in top corporate positions with work experience at agency *a*. The analyses focus on direct transitions. *Restrictions* is an estimate of the number of phrases indicating legally binding obligations and prohibitions present in the CFR. The variable is obtained from *RegData*, which uses textual analysis to estimate the relevance of each part of the CFR to each six-digit NAICS industry, allowing an estimate of regulations at the agency-industry-year level. The sample includes firms with primary 6-digit NAICS industry codes available in *Capital IQ*. Columns 1 through 3 include all possible firm-agency-year triplets. Columns 4 through 6 include only firm-agency-year triplets with a strictly positive value of *Restrictions*. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, and *, respectively.

	1	2	3	4	5	6
ln(Restrictions + 1) [t-1]	0.000301***	0.000334***	0.000196*	0.000486***	0.000606***	0.000342*
	(3.47)	(3.83)	(1.65)	(3.66)	(4.47)	(1.70)
ln(Restrictions + 1) [t]	0.000085*	0.000095*	0.000025	0.000362**	0.000390**	0.000028
	(1.66)	(1.86)	(0.35)	(1.99)	(2.15)	(0.17)
ln(Restrictions + 1) [t+1]	0.000184	0.000222*	-0.000057	0.000204	0.000274	-0.000021
	(1.49)	(1.82)	(-0.37)	(1.10)	(1.49)	(-0.09)
Number of Observations	14,137,574	14,137,574	14,137,574	9,175,044	9,175,044	9,175,044
Adjusted R^2	0.824	0.824	0.825	0.796	0.796	0.797
Firm-Agency FEs	Y	Y	Y	Y	Y	Y
Agency-Year FEs			Y			Y
Firm-Year FEs		Y	Y		Y	Y
Y Time Period	2000-2018	2000-2018	2000-2018	2000-2018	2000-2018	2000-2018
Sample	w/ NAICS Code	w/ NAICS Code	w/ NAICS Code	Res. > 0	Res. > 0	Res. > 0
Y Sample Mean	0.001910	0.001910	0.001910	0.002094	0.002094	0.002094

TABLE 4

Procurement Contracts by Agency (in Dollars)

This table reports, by agency, the number and value of procurement contracts signed with firms that could be matched in *BoardEx*. Procurement contract data come from *USASpending.gov*.

Agency	Number of Contracts	Total Value of Contracts
Department of the Army	238,626	270,419,148,800
Department of the Navy	282,050	267,614,289,920
Department of the Air Force	136,962	234,390,175,744
Defense Logistics Agency	8,038,997	161,116,520,448
Veterans Administration	1,133,545	107,731,148,800
Defense Information Systems Agency	79,119	23,414,081,536
Missile Defense Agency	1,292	21,134,835,712
Centers for Disease Control and Prevention	14,184	19,077,253,120
National Aeronautics and Space Administration	20,571	18,553,731,072
US Transportation Command	14,130	18,278,019,072
US Special Operations Command	9,721	15,966,738,432
Defense Health Agency	2,055	15,648,045,056
Department of Energy	6,936	14,750,518,272
National Institutes of Health	66,048	10,815,989,760
Department of State	33,395	8,786,188,288
…	…	…

TABLE 5

Procurement Contracts

This table presents regressions of the incidence and value of procurement contracts on the appointment and departure of former regulators. The unit of observation is the firm-agency-year triplet. The dependent variable in Columns 1 and 3, *Contract*, is an indicator variable that takes the value of one if the agency in question signs a contract with the firm in a given year, and zero otherwise. The dependent variable in Columns 2 and 4, *ln(Contract Value + 1)*, is the total "Federal Action Obligation" across all contracts the agency in question signs with the firm in a given year, plus one. The sample includes all government contracts issued by 187 government agencies that could be matched to *BoardEx*. In Columns 1 and 2, the events analyzed are appointments of former regulators to top corporate positions. In Columns 3 and 4, the events analyzed are departures of former regulators from the firm. *Event Year = t* denotes the time relative to the event in calendar years. Panel A includes transitions to a top corporate position (i.e., the Event) that occur within two years of leaving the agency. Panel B includes transitions to top corporate positions involving former regulators that join the firm more than two years after leaving the agency. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, and *, respectively.

Panel A. Direct Transitions

Event type	Appointments of Former Regulators		Departures of Former Regulators	
	Contract	ln(Contract Value+1)	Contract	ln(Contract Value+1)
	1	2	3	4
Event Year = -2	0.010835	0.161889	0.005023	0.126302
	(1.06)	(1.27)	(0.68)	(1.36)
Event Year = -1	0.020616**	0.261725**	0.005688	0.136455
	(2.20)	(2.21)	(0.70)	(1.31)
Event Year = 0	0.015264	0.249474*	0.011724	0.170412*
	(1.51)	(1.90)	(1.49)	(1.68)
Event Year = +1	0.022462**	0.290256**	0.001963	0.047559
	(2.06)	(2.12)	(0.21)	(0.40)
Event Year = +2	0.023962**	0.350062**	0.009385	0.162244
	(2.15)	(2.50)	(0.95)	(1.27)
Event Year ≥ +3	0.020436**	0.240499*	-0.007738	-0.071923
	(2.06)	(1.84)	(-0.84)	(-0.56)
Number of Observations	4,438,542	4,438,542	4,439,796	4,439,796
Adjusted R^2	0.509	0.636	0.576	0.637
Firm-Agency FEs	Y	Y	Y	Y
Agency-Industry-Year FEs	Y	Y	Y	Y
Firm-Year FEs	Y	Y	Y	Y
Y Time Period	2000-2018	2000-2018	2000-2018	2000-2018
Sample	Industry Contracts > 0	Industry Contracts > 0	Industry Contracts > 0	Industry Contracts > 0
Y Sample Mean	0.0275	0.3171	0.0276	0.3178

Panel B. Indirect Transitions

Event type	Appointments of Former Regulators		Departures of Former Regulators	
	Contract	ln(Contract Value+1)	Contract	ln(Contract Value+1)
	1	2	3	4
Event Year = -2	0.007284*	0.094887**	0.000392	0.027132
	(1.93)	(2.05)	(0.12)	(0.68)
Event Year = -1	0.003334	0.054136	-0.001894	-0.018795
	(0.86)	(1.15)	(-0.57)	(-0.46)
Event Year = 0	-0.002041	-0.003541	0.002615	0.051149
	(-0.51)	(-0.07)	(0.77)	(1.23)
Event Year = +1	0.001305	0.027030	-0.001031	-0.019624
	(0.30)	(0.52)	(-0.27)	(-0.41)
Event Year = +2	-0.005581	-0.066301	0.000904	-0.003276
	(-1.25)	(-1.21)	(0.22)	(-0.06)
Event Year $\geq$ +3	-0.008270*	-0.107929*	-0.004506	-0.075386
	(-1.91)	(-1.93)	(-1.06)	(-1.35)
Number of Observations	4,426,036	4,426,036	4,432,098	4,432,098
Adjusted R^2	0.573	0.632	0.575	0.636
Firm-Agency FEs	Y	Y	Y	Y
Agency-Industry-Year FEs	Y	Y	Y	Y
Firm-Year FEs	Y	Y	Y	Y
Y Time Period	2000-2018	2000-2018	2000-2018	2000-2018
Sample	Industry Contracts > 0	Industry Contracts > 0	Industry Contracts > 0	Industry Contracts > 0
Y Sample Mean	0.0271	0.3106	0.0274	0.3155

TABLE 6

Cross-sectional Differences in the Awarding of Procurement Contracts

This table presents regressions of the incidence of procurement contracts on the direct appointment of former regulators interacted with proxies of firm quality: ROA, sales growth, and Tobin's Q. The unit of observation is the firm-agency-year triplet. The dependent variable in each regression is *Contract*, an indicator variable that takes the value of one if the agency in question signs a contract with the firm in a given year, and zero otherwise. The events analyzed are direct appointments of former regulators to top corporate positions. Direct appointments occur within two years of leaving the agency. *Event Year = t* denotes the time relative to the event in calendar years. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, and *, respectively.

	1	2	3
	Char = ROA	Char = Sales Growth	Char = Tobin's Q
Event Year = -2	0.008684	0.013809	0.013453
	(0.76)	(1.18)	(1.21)
Event Year = -2 x Char = -1	0.016928	-0.001564	0.002688
	(1.61)	(-0.18)	(0.27)
Event Year = -1	0.014919	0.018283*	0.016398
	(1.40)	(1.70)	(1.54)
Event Year = -1 x Char = -1	0.013921	0.008757	0.000322
	(1.46)	(1.00)	(0.03)
Event Year = 0	0.017170	0.016963	0.014969
	(1.52)	(1.46)	(1.30)
Event Year = 0 x Char = -1	-0.004175	0.004353	0.001995
	(-0.46)	(0.54)	(0.22)
Event Year = +1	0.019792*	0.025073**	0.023508*
	(1.65)	(2.03)	(1.95)
Event Year = +1 x Char = -1	0.016941*	0.004163	0.005115
	(1.76)	(0.62)	(0.37)
Event Year = +2	0.025725**	0.026886**	0.025484**
	(2.11)	(2.14)	(2.07)
Event Year = +2 x Char = -1	0.007700	-0.020144*	0.003559
	(0.85)	(-1.91)	(0.22)
Event Year >= +3	0.022475**	0.025375**	0.022512**
	(2.11)	(2.30)	(2.00)
Event Year >= +3 x Char = -1	0.003828	0.007660	0.007840
	(0.44)	(1.57)	(0.54)
Number of Observations	3,170,234	3,144,523	3,216,741
Adjusted R^2	0.582	0.582	0.584
Firm-Agency FEs	Y	Y	Y
Agency-Industry-Year FEs	Y	Y	Y
Firm-Year FEs	Y	Y	Y
Y Time Period	2000-2018	2000-2018	2000-2018
Sample	Industry Contracts > 0	Industry Contracts > 0	Industry Contracts > 0
Y Sample Mean	0.035063	0.0344451	0.0353294

TABLE 7

Propensity Score Match

This table presents regressions of the incidence and value of procurement contracts on the appointment and departure of former regulators. The unit of observation is the firm-agency-year triplet. The sample includes firms that experienced a direct transition along with a control set of firms that were matched (in the year before the transition took place) within the same four-digit NAICS industry based on total assets, profitability, and Tobin's Q, using a nearest-neighbor algorithm. In Column 1, *Contract* is an indicator variable that takes the value of one if the agency in question signs a contract with the firm in a given year, and zero otherwise. In Column 2, *ln(Contract Value + 1)* is the total "Federal Action Obligation" across all contracts the agency in question signs with the firm in a given year, plus one. The sample includes all government contracts issued by 187 government agencies that could be matched to *BoardEx*. The events analyzed are direct appointments of former regulators to top corporate positions. Direct appointments occur within two years of leaving the agency. *Event Year = t* denotes the time relative to the event in calendar years. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, and *, respectively.

	Appointments of Former Regulators	
	Contract	ln(Contract Value+1)
	1	2
Event Year = -2	0.010632	0.154578
	(0.87)	(1.03)
Event Year = -1	0.013773	0.179481
	(1.24)	(1.28)
Event Year = 0	0.014574	0.220737
	(1.25)	(1.46)
Event Year = +1	0.024882**	0.311246**
	(1.97)	(1.98)
Event Year = +2	0.031225**	0.429040***
	(2.42)	(2.64)
Event Year ≥ +3	0.021127*	0.254049*
	(1.82)	(1.67)
Number of Observations	1,115,716	1,115,716
Adjusted R^2	0.604	0.663
Firm-Agency FEs	Y	Y
Agency-Industry-Year FEs	Y	Y
Firm-Year FEs	Y	Y
Y Time Period	2000-2018	2000-2018
Sample	Industry Contracts > 0	Industry Contracts > 0
Y Sample Mean	0.0562	0.6700

TABLE 8

Transitions of Presidential Appointees

This table presents regressions of the incidence and value of procurement contracts on the direct appointment of Presidential appointees that occur in the year that follows a Presidential election (i.e., Event Year = 0 and Election Year = +1). The unit of observation is the firm-agency-year triplet. In Column 1, *Contract* is an indicator variable that takes the value of one if the agency in question signs a contract with the firm in a given year, and zero otherwise. In Column 2, *ln(Contract Value + 1)* is the total "Federal Action Obligation" across all contracts the agency in question signs with the firm in a given year, plus one. The events analyzed are direct appointments of Presidential appointees to top corporate positions. Direct appointments occur within two years of leaving the agency. *Event Year = t* denotes the time relative to the event in calendar years. *Election Year = t* denotes the time relative to a U.S. Presidential election. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, and *, respectively.

	Contract	ln(Contract Value+1)
	1	2
Event Year = -2	0.011509	0.281368
	(0.15)	(0.30)
Event Year = -2 x Election Year = -1	-0.052137	-1.081255
	(-0.66)	(-1.29)
Event Year = -1	-0.045356	-0.287110
	(-0.75)	(-0.35)
Event Year = -1 x Election Year = 0	0.093433	0.587039
	(1.05)	(0.60)
Event Year = 0	-0.019929	-0.000593
	(-0.30)	(-0.00)
Event Year = 0 x Election Year = +1	0.056592	0.554752
	(0.60)	(0.49)
Event Year = +1	0.036974	0.616264
	(0.49)	(0.66)
Event Year = +1 x Election Year = +2	0.040569	0.327911
	(0.36)	(0.25)
Event Year = +2	0.087767	0.655854
	(0.98)	(0.63)
Event Year = +2 x Election Year = +3	-0.089769	-0.517138
	(-0.86)	(-0.47)
Event Year >= +3	-0.006195	0.088661
	(-0.05)	(0.07)
Event Year >= +3 x Election Year = +4	0.004804	-0.083363
	(0.04)	(-0.07)
Number of Observations	4,442,664	4,442,664
Adjusted R^2	0.578	0.640
Firm-Agency FEs	Y	Y
Agency-Industry-Year FEs	Y	Y
Firm-Year FEs	Y	Y

Y Time Period	2000-2018	2000-2018
Sample	Industry Contracts > 0	Industry Contracts > 0
Y Sample Mean	0.0278	0.3209

TABLE 9

Contract Outcomes and The Revolving Door

This table presents regressions of contract outcomes on the presence of a former regulator in a top corporate position at the firm. The unit of observation is the contract level. Observations only include firm-agency-years in which contracts are signed. *Renegotiation* is an indicator variable that equals one if the price of the contract is renegotiated upwards at some point in the contract's life. *Renegotiation %* is the sum of all price renegotiations for the contract divided by the initial value of the contract. *Former Regulator* is an indicator variable that takes the value of one if the firm in question has an appointed former regulator when the contract is signed, and zero otherwise. *Contract Value* is the initial value of the contract in dollars. *N Offers* is the number of offers received by the agency for the contract during the bidding process. If *N Offers* is missing, it is set to one and the indicator variable *N Offers Missing* is set to one. Columns 1 and 3 in both panels include all contracts while Columns 2 and 4 in both panels only include contracts that were renegotiated. Columns 1 and 2 of Panel A and all columns of Panel B examine direct transitions (i.e., appointments within two years of leaving the agency) and Columns 3 and 4 of Panel A examine indirect transitions (i.e., appointments more than two years after leaving the agency). In Panel B, Columns 1 and 2 examine fixed-price contracts and Columns 3 and 4 examine cost-plus contracts. T-stats based on standard errors clustered at the firm-agency level are reported in parentheses below the coefficients. Statistical significance at the 1%, 5%, and 10% levels is denoted by ***, **, and *, respectively.

Panel A. All Contracts

Sample	Direct Transitions		Indirect Transitions	
	Renegotiation	Renegotiation %	Renegotiation	Renegotiation %
	1	2	3	4
Former Regulator	0.012439***	0.496217**	0.004811	0.072693
	(2.72)	(2.03)	(1.53)	(0.45)
ln(Contract Value + 1)	0.008291***	-1.897167***	0.008291***	-1.897083***
	(3.11)	(-6.01)	(3.11)	(-6.01)
ln(Number of Offers + 1)	0.004656**	0.137775***	0.004659**	0.138065***
	(2.20)	(2.63)	(2.20)	(2.63)
Number of Offers Missing	0.007094**	0.084038	0.007100**	0.084091
	(2.36)	(0.30)	(2.36)	(0.30)
Number of Observations	10,576,506	114,198	10,576,506	114,198
Adjusted R^2	0.301	0.213	0.301	0.213
Award Type FEs	Y	Y	Y	Y
Pricing Type FEs	Y	Y	Y	Y
Firm-Agency FEs	Y	Y	Y	Y

Agency-Industry-Year FEs	Y	Y	Y	Y
Firm-Year FEs	Y	Y	Y	Y
Y Time Period	2000-2018	2000-2018	2000-2018	2000-2018
Sample	Contract Outcomes	Contract Outcomes	Contract Outcomes	Contract Outcomes
Y Sample Mean	0.0115736	2.063993	0.0115736	2.063993

Panel B. Fixed-Price versus Cost-Plus Contracts

Sample	Fixed Price		Cost Plus	
	Renegotiation 1	Renegotiation % 2	Renegotiation 3	Renegotiation % 4
Former Regulator	0.009365**	0.625318***	0.001754	0.378298
	(2.17)	(2.75)	(0.08)	(0.84)
ln(Contract Value + 1)	0.005659***	-1.912776***	0.179789***	-1.238586***
	(3.04)	(-4.35)	(23.60)	(-6.03)
ln(Number of Offers + 1)	0.003555*	0.297290***	0.030002***	-0.108575
	(1.72)	(3.77)	(5.08)	(-1.14)
Number of Offers Missing	0.005166*	1.333549**	-0.030436	-0.730509***
	(1.86)	(2.36)	(-0.92)	(-2.86)
Number of Observations	10,314,732	76,734	114,844	28,582
Adjusted R^2	0.275	0.271	0.298	0.068
Award Type FEs	Y	Y	Y	Y
Pricing Type FEs	Y	Y	Y	Y
Firm-Agency FEs	Y	Y	Y	Y
Agency-Industry-Year FEs	Y	Y	Y	Y
Firm-Year FEs	Y	Y	Y	Y
Y Time Period	2000-2018	2000-2018	2000-2018	2000-2018
Sample	Contract Outcomes	Contract Outcomes	Contract Outcomes	Contract Outcomes
Y Sample Mean	0.0081756	1.884156	0.2559037	1.90191

FIGURE 1

The Revolving Door across Industries and over Time

This figure depicts the evolution of the revolving door across broad NAICS industries during 2000-2018. The vertical axis is the percent of firms in the industry with a former regulator that was appointed to a top corporate position within two years of leaving an agency.
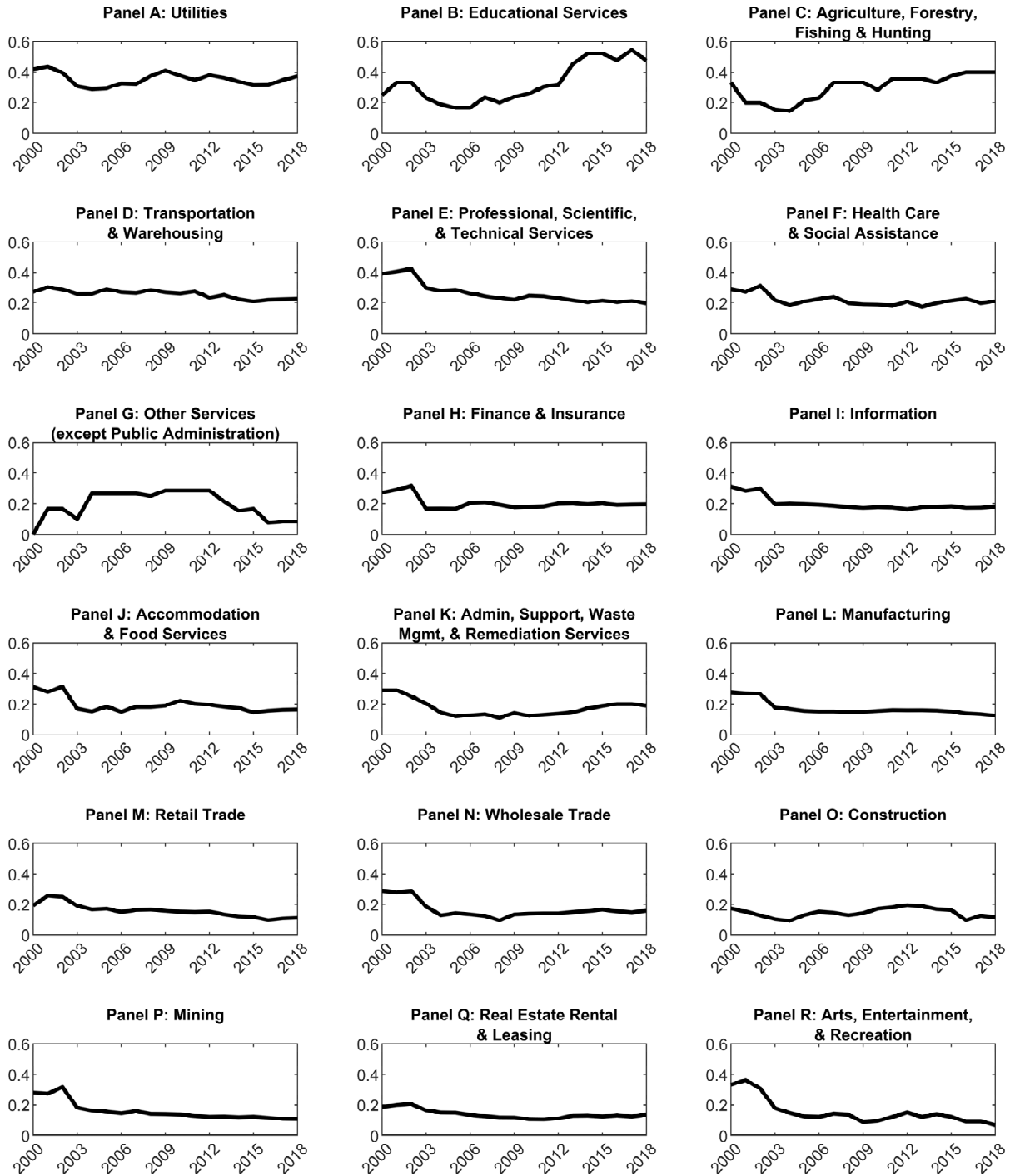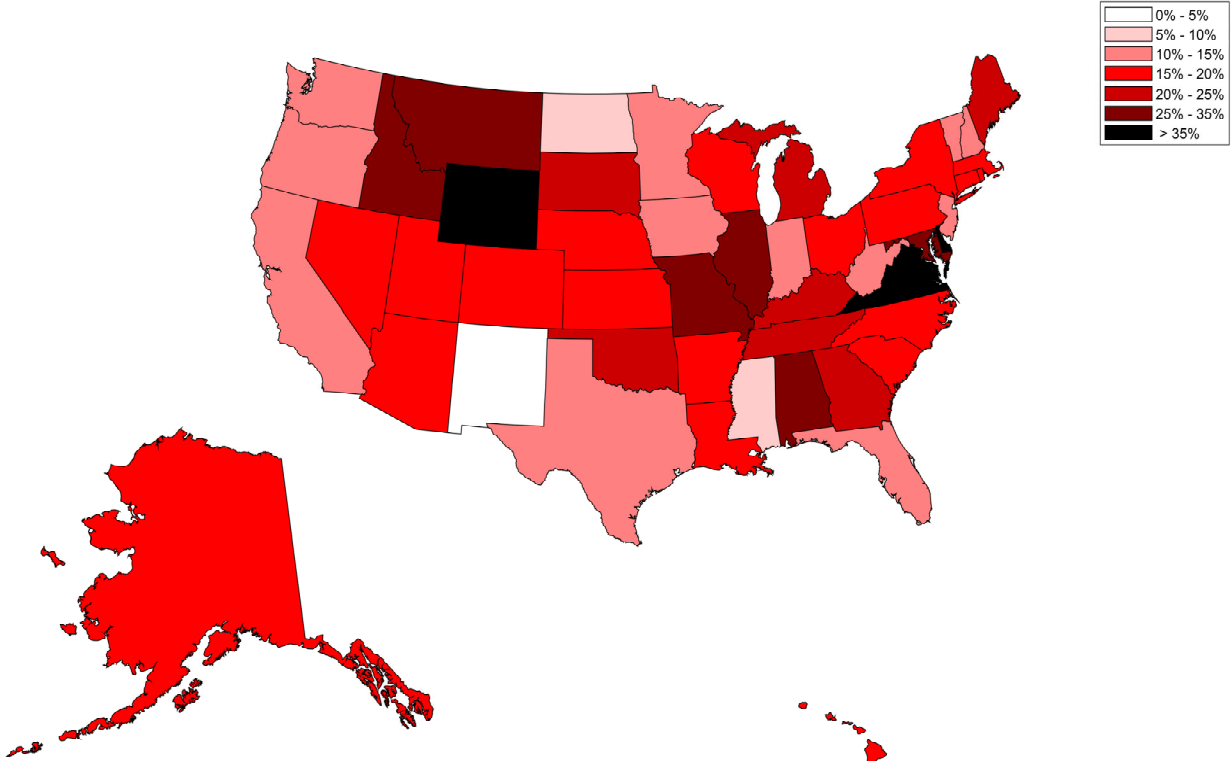

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FIGURE 2

The Revolving Door across States

This figure depicts the presence of the revolving door across U.S. states during 2000-2018. A darker color indicates a higher percentage of firms headquartered in the state in question with a former regulator that was appointed to a top corporate position within two years of leaving an agency.

FIGURE 3

Regulations

This figure plots the coefficients for regressions of *N Former Regulators* on regulations corresponding to Columns 4 through 6 from Table 3. Time on the x-axis corresponds to regulations in that year with time *t* being the year in which the number of former regulators is measured. Each coefficient is plotted along with 90% confidence intervals.

Graph A. Column 4 of Table 3
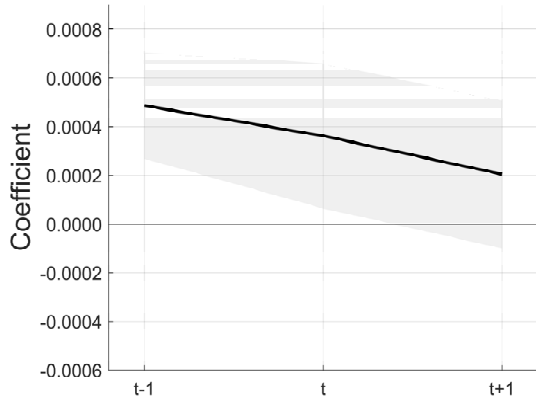
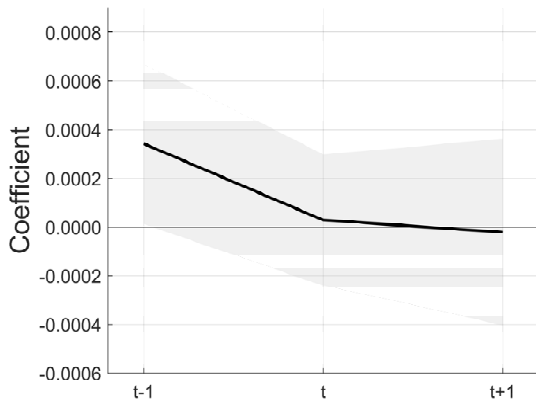
*Graph B. Column 5 of Table 3*
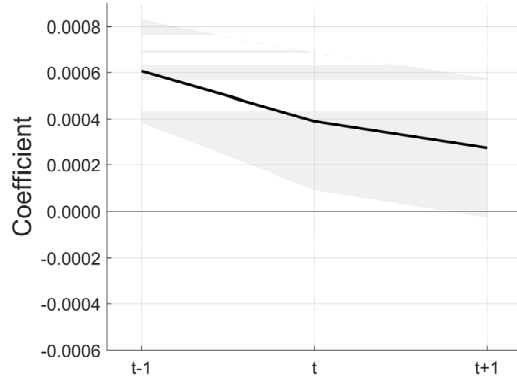
Graph C. Column 6 of Table 3

FIGURE 4

Procurement Contracts

This figure plots the coefficients for regressions of *Contract* from Table 5. Time *t* corresponds to the year the event took place, namely the appointment or departure of former regulators. Graph A focuses on direct transitions (Column 1 of Panel A of Table 5), and Graph B focuses on indirect transitions (Column 1 of Panel B of Table 5). Each coefficient is plotted along with 90% confidence intervals.

Graph A. Appointments of Former Regulators
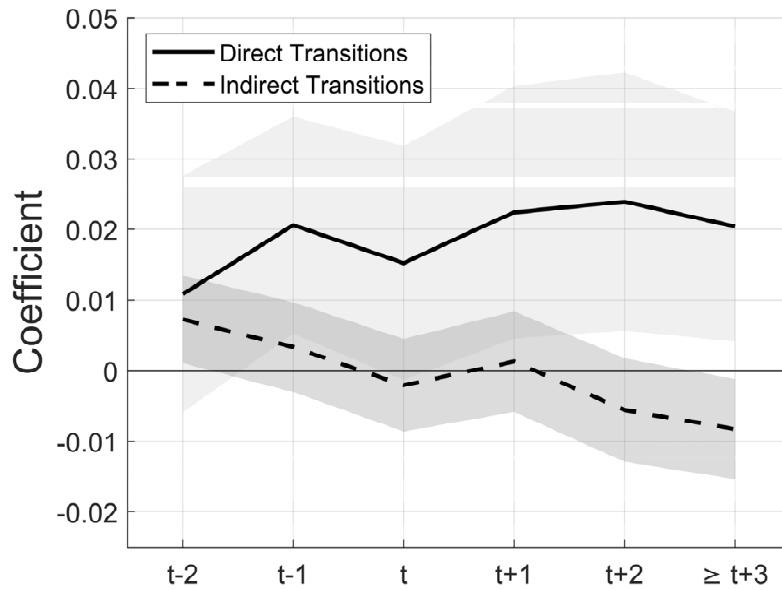
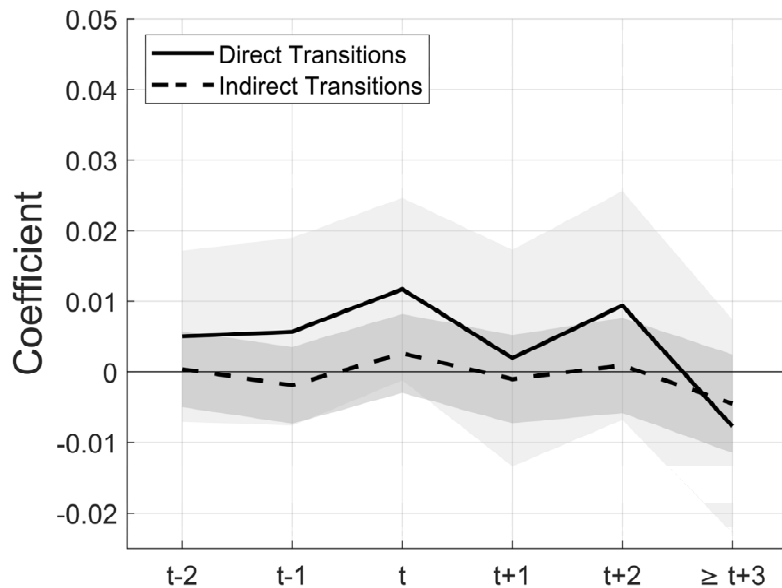*Graph B. Departures of Former Regulators*

FIGURE 5

Propensity-Score-Matched Results

This figure plots the coefficients for regressions of *Contract* from Table 7. The sample includes firms that experienced a direct transition along with a control set of firms that were matched (in the year before the transition took place) within the same four-digit NAICS industry based on total assets, profitability, and Tobin's Q, using a nearest-neighbor algorithm. Time *t* corresponds to the year the event took place, namely the appointment of former regulators. Each coefficient is plotted along with 90% confidence intervals.