

# Is the revolving door of Washington a back door to excess corporate returns?\*

Mehmet İ. Canayaz<sup>1</sup>, Jose V. Martinez<sup>2</sup>, and Han N. Ozsoylev<sup>1,3</sup>

<sup>1</sup>Saïd Business School, University of Oxford

<sup>2</sup>School of Business, University of Connecticut

<sup>3</sup>College of Administrative Sciences and Economics, Koç University

December 2015

## ABSTRACT

In this paper, we look into the so-called “revolving door of Washington”, which is the movement of individuals between federal government positions and jobs in the private sector, and examine its link to long-run stock returns. We find that firms where current public officials become future employees outperform other firms by a statistically significant 7.96% per year in terms of four-factor alpha. We also show that firms receive more valuable government contracts when a future firm employee is holding a post in the government. Our findings are consistent with the hypothesis of a quid pro quo relationship between some public officials and their future corporate employers. We run a battery of robustness checks to mitigate endogeneity concerns and other alternative explanations.

**JEL classification:** D73, G12, G18, L51.

**Keywords:** Corporate political connections, government contracts, regulatory capture, revolving door.

---

\*We would like to thank Yann Bramoulle, Jess Cornaggia, Cem Demiroglu, Hulya Eraslan, Thomas Hellman, Benjamin Hermalin, Colin Mayer, Tom Noe, Oguzhan Ozbas, Chip Ryan, Marc Sangnier, Insan Tunali and Mungo Wilson for insightful comments. We also thank participants at various conferences and seminars where we presented various versions of this paper. We are grateful to the Center for Responsive Politics and Bloomberg Government for providing the data used in this work and answering our questions. We are thankful to Chris Flegg for her help with Thomson Reuters I/B/E/S Guidance data. All errors are our own.

“When we would become friendly with an office member important to us and the chief of staff was a confident person, I would say or my staff would say to him or her at some point: ‘You know, when you’re done working on the Hill, we would very much like you to consider coming to work for us.’ The moment I said that to them or any of our staff said that to them, that was it. We owned them, and what does that mean? Every request from our office, every request of our clients, everything that we want - they are gonna do. Not only that, they are gonna think of things we can’t think of to do!” - Jack Abramoff quoted in 60 Minutes, CBS, November 6th, 2011.

## 1 Introduction

In a July 2007 campaign appearance in Manchester, N.H., then-presidential candidate Barack Obama said, “When I am president, I will make it absolutely clear that working in an Obama administration is not about serving your former employer, your future employer, or your bank account. It’s about serving your country, and that’s what comes first.” Obama also stated that, for two years, employees would be prohibited from working on regulations or contracts directly related to their previous employers. That ban, he said, would close a “revolving door” for former and future employers.<sup>1</sup>

Obama’s campaign remarks reflected a public unease with the movement of individuals between government positions and jobs in the private sector. Several revolving door movements had aroused public ire in the U.S. The poster child example for the conflicts of interest created by these movements was Darleen Druyun. Druyun, who oversaw the management of the Air Force’s weapons acquisitions program, joined Boeing in 2003 as the Deputy General Manager for Missile Defense Systems. Subsequent disclosures revealed that she was negotiating the terms of her Boeing employment while she was handling a proposal to lease tankers from Boeing. The proposal was more costly than purchasing the tankers outright.<sup>2</sup>

It is not a surprise that many on the street hold the view that revolving door movements are potentially corrupt activities and favor restrictions on such movements.<sup>3</sup> How-

---

<sup>1</sup>Excerpts taken from Zeleny (2007).

<sup>2</sup>See the Revolving Door Working Group (2005) report.

<sup>3</sup>A Transparency International UK survey carried out in 2010 reveals that the revolving door between government and business comes a close second in the public’s ranking of potentially corrupt activities. See

ever, there are others who argue that unduly restrictive provisions on revolving door movements may deter qualified and competent people from joining government service.<sup>4</sup> Unfortunately, there is limited empirical evidence on how revolving door movements impact corporate employers' performance and their business with government. Such evidence would allow for more objective and informed assessment of policy prescriptions regarding this issue. It would also contribute to the broader economic debate on effective regulatory design – a debate that goes back to at least Pigou (1938).

This paper contributes to filling the evidence gap on the conflicts of interest generated by revolving door movements by investigating their impact on corporate financial and operating performance. The conflicts of interest we study include: (i) *Conflicts prior to corporate employment*: Public officials may abuse their power while in office to favor a certain company or industry, with a view to ingratiating themselves and gaining future employment. (ii) *Conflicts during corporate employment*: Former public officials, who switch to the private sector, may influence their former government colleagues to make decisions in a way that favors their new employers. Also, they may use confidential information to benefit their new private employers – for example during procurement procedures. (iii) *Conflicts after corporate employment*: Public officials may allow the agenda of their previous corporate employer to influence their government work.<sup>5</sup>

We obtain data on revolving door movements from the Center for Responsive Politics' (CRP) Revolving Door Database. With these data in hand, we first investigate whether revolvers add shareholder value to their future corporate employers during their government tenure *prior to corporate employment*. We find that firms where current public officials are to become future employees outperform other firms by a statistically significant 7.96% per year, on a value-weighted basis, during the two years before the officials join them. The outperformance, measured using the Fama-French-Carhart (1997) four-factor model, is at its strongest immediately before the hiring of the revolver, and diminishes and eventually vanishes as we move further away from the hiring date. The outperformance is also stronger for firms that hire a larger number of revolvers, relative to their size.

---

Barrington, Macaulay, and Scott (2010).

<sup>4</sup>See Maskell (2014) and discussions in the President's Commission on Federal Ethics Law Reform, "To Serve With Honor," Report and Recommendations to the President (1989).

<sup>5</sup>See David-Barrett (2011) for a detailed discussion.

Second, we analyze whether revolvers add shareholder value to their corporate employers *during their corporate employment*. In this case we do not observe a significant relationship between revolvers and corporate returns. In particular, the difference between the four-factor alpha earned by a portfolio of firms which employ revolvers and that earned by a portfolio of firms which do not is not statistically significant.

Third, we investigate whether revolvers add shareholder value *after corporate employment*. We find that revolvers' former corporate employers do not do better than other firms after revolvers leave them to work for the government. The difference between the four-factor alpha earned by a portfolio of firms which employ revolvers and that earned by a portfolio of firms which do not employ any revolvers is also statistically insignificant.

What emerges from this analysis is that firms that employ revolvers enjoy significant abnormal returns during their revolvers' government tenure prior to joining them. In order to shed light on the possible causes of the return outperformance enjoyed by revolver-hiring firms during revolvers' government tenure, we also investigate the relationship between revolving door movements and government contract allocations. We find that having a revolver linkage to a government agency has a large and statistically significant positive effect on the value of government contracts allocated to firms that will soon hire revolvers. Similar to the trend in abnormal returns government contract allocation peaks in the year prior to the revolver leaving the government to join the firm, having steadily increased until then, and it then decreases after revolver starts the corporate job.

In order to link the stock market performance and the government contracts cash flow channel that we propose, we run Fama-Macbeth regressions on monthly stock returns. First, we show that a revolver dummy commands a positively significant coefficient of 9.77%. Second, we show that unlike the total variation in government contracts, the variation in government contracts explained by revolving door connections significantly explains the cross section of stock market returns. Our results in portfolio returns, contract allocations and the cross section of firms are all robust to a placebo test, in which we falsely assume there was a revolver connection between the firm and the government before the revolver's government tenure. So, it's only the last few years of revolvers government tenure that we see these phenomena.

The above results are in line with the hypothesis that a *quid pro quo* relationship exists

between government employees and firms. It is however a rather difficult task to correctly identify a causal impact of revolvers on firm performance due to potential matching stories. We use consensus earnings forecasts of equity analysts, who are unaware of potential *quid pro quo* relationships between managers and revolvers, as observable counterfactuals i.e. earnings that firms would have achieved and managers would have forecasted in the absence of the revolvers<sup>6</sup>. Despite any muting effects of managers' mindfulness in not revealing *quid pro quo* relationships, we identify a revolving door effect by comparing the earnings forecasts of insiders (i.e. management guidance) and outsiders (i.e. consensus on equity analyst forecasts) in a difference (between insider and outsider earnings forecasts) in differences (in the event time) framework. Comparing insiders' and outsiders' earnings forecasts allows us to not only see whether the insiders are more optimistic in the presence of a revolver connection with the government, but it also allows us to analyze which side is more surprised when the actual earnings are announced.

We find that insiders' forecasts during revolvers' last year in government are more optimistic than outsiders', and they yield less positive earnings surprises<sup>7</sup>. This result is in line with the notion that the insiders are expecting higher earnings during revolvers' last government tenure year, and they are indeed right. In a falsification test we identify revolvers whose government agencies did not allocate any government contracts to their future corporate employers. We call these *clean sheet revolvers*, as they are less likely to have been involved in *quid pro quo* relationships. Insiders' and outsiders' earnings forecasts for firms that hire *clean sheet revolvers* are very much alike and they possess no earnings surprises. When we investigate the firms that received government contracts but did not hire any revolvers, we once again find no difference in insiders' and outsiders' earnings forecasts and earnings surprises. Finally, our placebo test, in which we falsely assume there was a revolver connection between the firm and the government before the revolver's government tenure, shows us that insiders' and outsiders' forecasts possess no difference in earnings forecasts and earnings surprises in the placebo periods.

---

<sup>6</sup>Between years minus 8 and minus 2 relative to the revolvers' job switch from government to firms and during the firm tenure of revolvers, managers and equity analysts possess no difference in earnings forecasts and earnings surprises.

<sup>7</sup>The difference between insiders and outsiders forecasts is also rather stable between years minus 8 and minus 2, and during revolvers' firm tenures. The former ensures that our difference-in-differences analysis satisfies the parallel trends assumption. Results are available upon request.

To further mitigate endogeneity concerns, we use presidential executive orders that only restrict the revolving door movements of appointed government employees by banning them from working at firms as lobbyists. During our sample period (1990-2012), there are two presidential executive orders that introduced such restrictions<sup>8</sup>. In a difference (between firms that hire appointees and non-appointees) in differences (between restrictive years and nonrestrictive years) framework, we show that the abnormal returns earned by and government contracts allocated to only those firms that hire presidential appointees disappear in the restrictive years.

Finally, we extend our results in the earnings forecasts analysis by adding the executive orders into the picture. We run difference (between appointed revolvers and non-appointed revolvers) in difference (between insider and outsider earnings forecasts) in differences (in the event time) regressions. Although managers of firms with appointed and non-appointed revolvers both possess more positive earnings expectations relative to outsiders, we find that only the managers with appointed revolver connections in the government lose their optimism in the restrictive years.

Taken together, our results lend empirical support to the hypothesis that there is a quid pro quo relationship between some public officials and corporations: some public officials are potentially using their power while in office to favor future corporate employers. Thus, our paper contributes to existing research on economic and financial implications of revolving door movements. Even though revolving door movements have been a subject of interest for long in political science, empirical economic research on this subject is recent and currently very limited (see Grace and Phillips, 2008; Blanes i Vidal, Draca, and Fons-Rosen, 2012; Lucca, Seru, and Trebbi, 2014). What sets our paper apart from these studies is its comprehensive nature. Our paper covers all industries listed in the SIC system whereas other studies focus on a particular industry, such as banking (Lucca, Seru, and Trebbi, 2014), insurance (Grace and Phillips, 2008) or lobbying (Blanes i Vidal, Draca, and Fons-Rosen, 2012). Also, unlike existing economic research, we cover multiple aspects of revolving door movements. We, for instance, investigate revolvers' career transitions from government to private sector as well as their transitions in the opposite

---

<sup>8</sup>These are the executive order, 12834, issued by President Clinton at the beginning of his presidency, and the executive order, 13490, issued by President Obama at the beginning of his presidency.

direction. Furthermore, we look at revolvers' corporate performance implications during their government tenure as well as during their private sector tenure. When we study revolvers' corporate performance implications, we look into both financial performance and operating performance – the latter in the form of government contract allocations. To the best of our knowledge, ours is the first paper to study revolvers' impact on their corporate employers' long-run stock return performance. From a policy perspective, this paper suggests that in the context of revolving door movements there may be a need to reset the institutional incentives so that public officials act in the public interest. On the practical side, our results indicate that the presidential executive orders, which restricted revolving door movements, were effective in curbing some of the conflicts of interest – at least, in the context of government contract allocations.<sup>9</sup>

This paper also contributes to the identification of *hidden* corporate political connections. Most of the literature identifies corporate political connections by using characteristics, which are public information, such as political campaign donations, board seat connections, or stock holdings by politicians.<sup>10</sup> Our measure for corporate political connections is complementary to these: we track public officials' career movements, in particular their movement from government service to private jobs. General investor population would not be privy to the relationship between public officials and their future corporate employers while these officials are still in public office. Our results indicate that hidden corporate political connections in the form of *revolver linkages* generate shareholder value for corporations.

The remainder of the paper is organized as follows. Section 2 describes the related literature. Section 3 describes the data used in the study. Section 4 presents our findings on the relationship between revolving door movements and long-run stock returns. Section 5 presents our findings on the relationship between revolving door movements and government contract allocations. Section 6 presents our findings on the earnings forecasts and surprises of management and equity analysts. Section 7 is on the impact of executive

---

<sup>9</sup>Of course, we cannot rule out potential distortionary effects of introducing regulatory restrictions on revolving door movements. For instance, these restrictions may deter qualified and competent individuals from joining public service. Also, such restrictions may isolate the government from private sector concerns and deprive it from private sector experience.

<sup>10</sup>See, e.g., Jayachandran(2006), Cooper, Gulen, and Ovtchinnikov (2010) and Tahoun (2014) for political campaign donations, Faccio (2006) and Goldman, Rocholl, and So (2009, 2013) for board seat connections, and Tahoun (2014) for stock holdings by politicians.

orders on earnings forecasts. Section 8 is the discussion section and Section 9 concludes.

## 2 Related literature

Interest in the implications of revolving door movements emerged first in political science and more recently in economics, primarily in the context of regulating utilities, broadcasters, and the financial industry (see, e.g., Gormley, 1979; Cohen, 1986; Spiller, 1990; and Grace and Phillips, 2008).<sup>11</sup> Our paper is closely related to a couple of recent studies on the subject: Blanes i Vidal, Draca, and Fons-Rosen (2012) find evidence consistent with revolving door lobbyists selling access to powerful politicians hence exercising undue influence based on former government employment. In particular, they show that lobbyists who worked for a US Senator suffer a 24% drop in generated revenue when that Senator leaves office. Lucca, Seru, and Trebbi (2014) trace career movements of federal and state US banking regulators. They find that more people choose to move into regulation during downturns and more people move from banking to regulatory jobs during periods of intense regulation. The authors suggest that their findings are inconsistent with a “quid pro quo” explanation of revolving door movements but consistent with a “regulatory schooling” hypothesis. The latter says that regulators have an incentive to implement sophisticated regulations as insider knowledge of complex rules makes regulators more appealing job candidates for banks. Unlike the two studies cited above, our focus in this paper is on all public officials listed in the CRP database (who moved from government service to private sector or vice versa) and all listed firms, and our variables of interest are stock returns as well as government contract allocations.

Our paper is also related to the literature on financial implications of corporate political connections. Numerous studies have examined the impact of political connections on firm value, with varying results. Studies carried out with data from countries with relatively weak institutions indicate that political connections have a significant positive effect on firm value (see Bunkanwanicha and Wiwattanakantang, 2009; Cingano and Pinotti, 2013; Faccio, 2006; Fisman, 2001; Johnson and Mitton, 2003; and Li, Meng, Wang, and Zhou, 2008). On the other hand, the evidence from U.S. is more ambiguous. Cooper, Gulen,

---

<sup>11</sup>See Dal Bo (2006) for a detailed literature review of research associated with regulatory capture and revolving door movements.



and Ovtchinnikov (2010), Goldman, Rocholl, and So (2009, 2013), Jayachandran (2006) and Tahoun (2014) find that there is a positive relationship between political connections and firm value. In particular, Cooper, Gulen, and Ovtchinnikov (2010) show that firm-level contributions to political candidates are positively and significantly correlated with the future returns of contributing firms in the U.S. Goldman, Rocholl, and So (2009) investigate the announcement effect of the nominations of politically connected individuals to corporate boards and show that nomination announcements are followed by positive abnormal stock returns. Their 2013 paper, in turn, finds that companies with boards connected to the election-winning (losing) party experience a significant increase (decrease) in procurement contracts after the election. Jayachandran (2006) looks into the so-called Jeffords Effect -named after a senator who left the Republican Party unexpectedly and tipped control of the U.S. Senate to Democrats- and finds that, following Jeffords' switch, firms which made soft money donations to the Republicans in the previous election cycle lost in market value while those which made donations to the Democrats gained in market value. In a more recent study, Tahoun (2014) shows that the stronger the association between firms and Members of Congress (measured by PAC contributions from firms to Members and stock holdings in the firms by the Members), the higher is the provision of overall government contracts to the firms. By contrast, Fisman, Fisman, Galef, Khurana, and Wang (2012) estimate the value of corporate ties to former Vice-President Cheney to be zero and interpret this as evidence that U.S. institutions are effective in controlling rent-seeking through personal ties with high-level government officials. Acemoglu, Johnson, Kermani, Kwak, and Mitton (2013) show that the announcement of Geithner as President-elect Obama's nominee for Treasury Secretary in November 2008 had a positive effect on the value of financial firms with which Geithner had a personal connection. Repeating their analysis for the nomination of Secretary Hank Paulson during regular times, they find no connection premium. In light of these results, the authors argue that political connections may be beneficial to firms in the U.S. but mainly in times of economic turbulence. Our paper provides further evidence for the U.S. by establishing a significant positive relationship between political connections and firm value, not only during turbulent economic times, but also during normal times.

Furthermore, in a broader context, our paper is also related to the recent literature

studying the impact of government policy on asset prices (see, e.g., Pastor and Veronesi, 2012; Belo, Gala, and Li, 2013; and Cohen, Diether, and Malloy, 2013). Finally, our paper contributes to quid pro quo relationships literature in other frameworks such as credit rating agencies (see, e.g., Cornaggia, Cornaggia, and Xia, 2015) and SEC lawyers (see, e.g., DeHaan, Koh, Kedia, and Rajgopal, 2012).

### 3 Data

In this section, we describe the data used in the empirical analysis.

#### 3.1 Revolving door movements and financial data

Our data on *revolvers*, i.e., individuals who move from government positions to private sector jobs or vice versa, come from the Revolving Door Database maintained by the Center for Responsive Politics (CRP). This database contains information on former and current US government employees who also held or currently hold positions in the private sector where they can be reasonably expected to influence public policy decisions. This type of private sector employment includes traditional lobbyists, executives, general counsels and consultants who specialize in public affairs, or who advise their corporate employers on regulatory or political law. CRP has a long list of criteria to determine whether or not a person belongs in the Revolving Door Database – the list is available on their website [www.opensecrets.org/revolving/methodology.php](http://www.opensecrets.org/revolving/methodology.php). They use proprietary and publicly available sources to continuously update their data set.

CRP’s Revolving Door Database allows us to track revolvers’ employment on a yearly basis. For each observation of a revolver-job pair, we have the name of the employer, the beginning year of job, the end year of job, and the employment type (i.e., whether the employer is a government agency, a congressional committee, a member of the House of Representatives, a Senator, a lobbying firm, a public firm, a PAC, etc.). A typical entry would be as follows: Mr. Brown was employed by ABC Inc. as Vice President of Government Affairs between 1993 and 1997. The database contains 29,188 observations of revolver-job pairs.<sup>12</sup> Using this data, we identify where and in which positions revolvers

---

<sup>12</sup>This number corresponds to the latest update of the database as of December 2013.

worked in a given year. We concentrate on the revolvers that started working in corporate jobs right after their government tenure and the revolvers that started working in government jobs right after their corporate tenure. In both cases, we consider only the revolvers that started working in their next job within a year after they left their former job. Regarding corporate jobs, we consider only employers that are publicly listed firms traded in the United States. We restrict our analysis to the period between 1990 and 2012. This is because the number of publicly listed firms in the database that employ revolvers of the kind described above is limited before 1990. The time-series average of the number of these firms is 9 between 1980 and 1990, with only one publicly listed firm hiring revolvers in some of those years, whereas the time-series average after 1990 is 75.

We match the above data with financial and accounting data from CRSP and COMPUSTAT. We determine the names of publicly listed firms that appear in the above sample, manually search for these firm names on CRSP to find their PERMNO numbers, and use the latter to extract data from CRSP and CRSP/COMPUSTAT Merged Database. In our analysis, we exclude firms with missing return data and firms with previous year market capitalization of less than 10 million US dollars (measured on the last day of the year). We also exclude observations associated with share codes other than ordinary common shares (i.e., we keep only the firms with CRSP share codes of 10 and 11).

We present summary statistics of the resulting sample in Table 1. Panel A of the table details the number of revolvers employed and the number of new revolvers hired in a given year along with the number of public firms that employ the revolvers and the number of them that hire new revolvers in any given year. For example, in year 2001, there were 61 public firms employing 78 revolvers in total and 28 of those revolvers were newly hired in that year. During the period of our study, an average of 75 revolvers worked in publicly traded firms each year, and 53 publicly listed firms employed at least one revolver. Panel B lists mean, median, minimum and maximum market capitalizations of public firms that employ revolvers in a given year. Panel C reports the same for public firms that do not employ any revolver in a given year. Data on market capitalization is obtained from CRSP and reflects the capitalizations of the firms at the end of each calendar year. As the table shows, firms that employ revolvers are on average larger than those that do not.

– Insert Table 1 about here –

Table 2 lists the top 25 corporate hirers of revolvers as well as the industries these hiring firms belong to. The industries are defined using 2-digit SIC codes. Top hirers of revolvers are Lockheed Martin Corp, Raytheon and Citigroup – they employ 37 revolvers in total during the sample period. The most common revolver hiring industries are Electric, Gas, and Sanitary Services, Communications and Business Services; each hiring approximately 8% of all revolvers in our sample.

– Insert Table 2 about here –

Table 3 provides information on the job positions held by revolvers. Panel A lists revolvers’ job positions within the executive branch, the number of such positions and their corresponding frequencies. Panels B and C report similar data and statistics for revolvers’ job positions within the legislative branch and publicly traded firms, respectively. As is evident from the table, the great majority of the revolvers in our sample held senior level positions both in the government and the private sector.<sup>13</sup>

– Insert Table 3 about here –

### **3.2 Government contracts**

We obtain data on government contract awards from the Bloomberg Government (BGOV) database. BGOV provides data on government contracts that firms receive along with a description of the government agencies that awarded these contracts. BGOV collects its contract data from the Federal Procurement Data System - Next Generation (FPDS-NG). The FPDS-NG, administered by the US General Services Administration, is the central repository of information on procurement contracts awarded by the US government. If contracts are awarded to subsidiaries of large corporations, BGOV identifies the parent corporation and assigns contracts accordingly. Specifically, for each government contract, BGOV provides information about the contract-allocating government agency, Bloomberg ticker of the firm that received the contract, the total dollar amount of the contract, and

---

<sup>13</sup>Note that the number of job positions within the executive branch and the legislative branch do not add up to the number of positions on the corporate side. This is so because some revolvers held multiple positions in the government before switching to the corporate side.

the date the contract was allocated. Bloomberg has a linking table between Bloomberg tickers and CUSIP numbers – this enables us to identify the firms by PERMNO after linking CUSIPs and PERMNOs.

We match the BGOV data with the revolver data using PERMNOs. We only use revolvers who are employed by contract-allocating government agencies or congressional committees which have influence over contract-allocating government agencies. Otherwise, we do not make a match<sup>14</sup> and therefore we do not include the revolver and the data cross-referenced to him in the matched sample. After matching the samples, we compute the total dollar amount of government contracts allocated to each firm for every year. If no contracts are allocated, we set the value to zero. We concentrate on firms that have hired at least one revolver during the sample period.

During our sample period, 1,221 different publicly traded firms (by PERMNO) obtained at least one government contract (out of a total of 12,044 distinct publicly traded firms). Of those, 111 employed at least one revolver during our sample period, whereas 42 firms that hired revolvers and matched our selection criteria did not receive any government contracts. Firms that employed revolvers obtained government contracts on average worth 125.99 million US dollars per year. The average value of government contracts received by firms that did not hire any revolvers was 1.77 million US dollars per year.

### 3.3 Earnings forecasts

In our analysis, we also compare analyst and management expectations on firms' operating performance (specifically, earnings). Analysts' earnings forecasts come from the Thomson Reuters I/B/E/S Estimates dataset and management earnings forecasts come from the Thomson Reuters I/B/E/S Guidance dataset<sup>15</sup>.

---

<sup>14</sup>This is unless the Representative or the Senator is Chair Person or Ranking Member of a congressional committee in which case we follow the matching procedure described for congressional committees.

<sup>15</sup>Guidance information is collected from earning press releases, interim releases, earning calls, conference presentations, M&A calls and analyst meetings in which the company representatives speak about financial expectations (for example: CEO, CFO, and COO) and data is provided in both textual and table format according type document.

## 4 Revolving door movements and abnormal returns

We assess whether revolvers add shareholder value to firms by estimating the abnormal returns obtained by firms that hire revolvers. For this purpose we create value-weighted portfolios of firms that employ revolvers and firms that do not.<sup>16</sup> The weight used for value-weighting is based on each firm’s market value of equity at the end of the previous calendar year. In building these portfolios we consider the period in which revolvers worked for the firms as well as the periods immediately before and after firm employment, as revolvers’ connections may be useful to their future or former employers even when they are in office.

We estimate (unconditional) abnormal returns by running following factor-model regressions with the monthly returns of these portfolios:

$$r_{p,t} = \alpha_p + \beta_p' f_t + \epsilon_{p,t}, \quad (1)$$

where  $r_{p,t}$  is the portfolio excess return (over the risk-free return),  $f_t$  is a vector of excess returns on benchmark factors, and  $\alpha_p$  is the abnormal performance measure of interest. We use three established factor models: the CAPM (see Sharpe, 1964 and Jensen, 1968), the Fama-French three factor model (see Fama and French, 1993) and the Fama-French-Carhart four factor model (see Carhart, 1997). To compute CAPM alphas we use the excess market return as the only factor. For Fama-French alphas we use market, size, and book-to-market factors. For the four-factor model, we use the three Fama-French factors plus momentum. We obtain these four factors from Kenneth French’s web site.

As returns are known to vary with business cycle fluctuations, we also identify abnormal returns by assuming a model of conditional expected returns that allows for time-variation in the risk factor loadings (namely, betas). To that end, we follow Ferson and Harvey (1999) closely and assume the following linear functional form for the vector of time-varying betas:

$$\beta_p(z_t) = b_{p0} + b_{p1}' z_t, \quad (2)$$

---

<sup>16</sup>If the asset pricing model is correctly specified, a test of whether the portfolio of firms employing revolvers delivers significant abnormal returns would suffice. We choose to err on the side of caution and compare two portfolios instead because of the exclusion restrictions in our sample.

where  $z_t$  is the vector of conditioning variables known at time  $t$ . Following Petkova and Zhang (2005) and Belo, Gala and Li (2013), we use dividend-price ratio, default premium, term spread, and risk-free rate as our conditioning variables.<sup>17</sup> These variables are known to have predictive power over business cycles. We test for abnormal returns by estimating the below specification:

$$\begin{aligned} r_{p,t} &= \alpha_p + (\beta_p(z_{t-1}))' f_t + \epsilon_{p,t} \\ &= \alpha_p + b'_{p0} f_t + (b'_{p1} z_{t-1})' f_t + \epsilon_{p,t}, \end{aligned} \tag{3}$$

where  $f_t$  is the vector of excess returns on Fama-French-Carhart four factors (namely, market, size, book-to-market, and momentum).

In addition to the abnormal return measures described above, we also compute the average returns of each portfolio in excess of the returns of a portfolio of characteristics-based benchmarks as in Daniel, Grinblatt, Titman, and Wermers (1997) and Wermers (2003). This procedure matches each firm in our portfolio of interest to a portfolio of firms with similar size, book-to-market ratio, and momentum.<sup>18</sup>

#### 4.1 Abnormal returns prior to revolvers' corporate employment

We first explore whether revolvers add shareholder value to firms during their tenure in the government prior to joining them. We investigate this possibility by building portfolios of revolver-hiring firms in the years immediately before they hire revolvers and comparing them to other firms in the same period. We assess whether revolvers benefit their future corporate employers by estimating abnormal returns.

Table 4 displays excess returns and alphas of revolver-hiring firms up to two years before revolvers joined them, and while revolvers were still working for the government. It also shows the performance of the rest of the firms during the same period and the difference between revolver-hiring firms and others expressed in percent per year. In this

---

<sup>17</sup>Default premium is Moody's seasoned Baa corporate bond yield minus Moody's seasoned Aaa corporate bond yield. Term spread is the difference between 10-year and 1-year treasury constant maturity rates. Dividend-price ratio measures dividends paid to the market portfolio over the price of market portfolio. We take default premium and term spread data from St. Louis Feds web page. Dividend-price ratio and risk-free rate are derived using CRSP data.

<sup>18</sup>The benchmarks are available from: [www.smith.umd.edu/faculty/rwermers/ftpsite/Dgtw/coverpage.htm](http://www.smith.umd.edu/faculty/rwermers/ftpsite/Dgtw/coverpage.htm).

analysis we only consider revolvers that join a firm within one year at most after the end of their duty in the government.

The value-weighted portfolio of revolver-hiring firms delivered average returns of 16.17% per year in the two years prior to the hiring. These returns compare favorably to average returns of 9.14% per year for all other firms in our sample during the same 1990-2012 period. The annualized difference between the two, 7.03%, is statistically significant, and it remains so once risk-adjusted using both unconditional and conditional asset pricing models. For instance, using four-factor alphas the difference between these two portfolios is a highly statistically significant 7.96% per year. Similar results obtain if we risk-adjust returns using characteristics-based benchmarks as in Daniel, Grinblatt, Titman, and Wermers (1997); in this case the difference between firms that are going to be joined by revolvers currently working for the government and other firms that satisfy the inclusion criteria of our sample equals 5.50% per year, statistically significant at the 1% level.

We also look into the four-factor alpha performance of the same firms during a placebo period which corresponds to the two-year period immediately prior to revolvers' government service. The placebo period four-factor alpha difference between firms that hire revolvers and firms that do not is statistically insignificant.

– Insert Table 4 about here –

The results we find are stronger the higher the number of revolvers to be hired relative to size, i.e., the higher the revolver intensity of the firms. In Table 5, we classify firms into three equally sized groups based on the ratio of number of revolvers to be hired to firm size as of the end of the previous calendar year. We find that abnormal returns are mostly higher for firms in the top third of revolvers-to-size ratio compared to firms in the bottom third of this ratio. For example, the value-weighted four-factor alpha of firms in the top tercile of revolver intensity is 9.30% per year in the two year period prior to the hiring whereas for firms in the bottom tercile it is 8.15%.

– Insert Table 5 about here –

Our results are also stronger in the years immediately before the hiring and significantly weaken as we move further away from that date, as we would expect if they were the



consequence of revolvers helping their future employers before making their move. Four or five years before the hiring the performance of the revolver-hiring firms is indistinguishable from that of other firms in our sample, see Figure 1<sup>19</sup>. Consistent with this finding, in untabulated results we also find that returns are smaller for revolver-hiring firms with revolvers who leave the government a number of years prior to their hiring by the firm (compared to those with revolvers who move from government to firm within one year at most), which also suggest that the abnormal returns we identify are less likely to be firm-specific or the consequence of risk-adjusting models mispricing some firms.

– Insert Figure 1 about here –

## 4.2 Abnormal returns during revolvers' corporate employment

We next investigate whether revolvers add shareholder value to firms during their tenure in the company<sup>20</sup>. In our analysis we only consider revolvers who join a firm within one year of ending their employment in government. Since we only have information about the year revolvers join firms, in most specifications we proceed as if the revolvers had been working for the firm for the entire year in which they joined, to make sure we capture announcement effects, if any.<sup>21</sup> For example, if a revolver leaves his government job in year 2000 and starts working at a firm in that same year, we count the entire year 2000 as part of his firm employment, and year 1999 as the last year of his government employment. The revolver's tenure at the firm would then be from year 2000 until the last full year he or she works for the firm (the year he or she leaves the firm is not included in this period). If a revolver leaves his government job in year 2000 and starts working at a firm in year 2001, on the other hand, we count year 2001 as the first year of his firm tenure, and year 2000 as the last year of his government employment.

---

<sup>19</sup>It is likely that the firms want to keep their revolvers in the government as long as possible. Namely, the firms want to keep their cash flow channels active as long as possible. On the other hand, firms would not want to signal to their revolvers that they are not committed to hiring them. This is important not only for the active cash flow channels (i.e. revolvers might find outside options) but also for future revolver links (i.e. it would be hard to reestablish trust once it's lost). We believe that the heterogeneity in the time series longevity of abnormal returns across firms may be a revelation of different bargaining issues between different firms, revolvers and government contracting environments.

<sup>20</sup>The case in which the revolver goes from firm to government and we look at firm returns yields no abnormal returns. We do not tabulate the results for brevity, but the results are available upon request.

<sup>21</sup>This is not a test of market's efficiency in absorbing information about revolvers. Whether markets are efficient and revolvers' value contribution is mostly reflected on the month of hiring, or whether they are not and as a result revolvers' value contribution is observed during their entire tenure in their jobs does not make a difference to our tests.

– Insert Table 6 about here –

Table 6 shows returns, excess returns over characteristics-based benchmarks and alphas of portfolios of US firms that employ revolvers, do not employ revolvers, and their difference for the 1990-2012 period. Results in this table indicate that in the 23-year period of our study firms that employed revolvers did not deliver higher returns than firms that did not employ revolvers. Similar results obtain when we exclude the first year of employment from the portfolio.

These results suggest that revolvers do not add shareholder value to the firms that employ them, during their tenure in these firms. In efficient markets, one could argue that it is the unexpected employment of revolvers, not the numbers of revolvers hired or working for the firm that should be related to abnormal returns. In practice, however, these two variables are likely to be highly correlated given how difficult it is to forecast accurately which firms are going to hire revolvers and which will not.

### **4.3 Abnormal returns after revolvers’ corporate employment**

We finally study whether revolvers add shareholder value to firms after leaving corporate employment (and while working for the government). In Table 7 we show the performance of portfolios of US firms during the two-year period following revolvers’ departure from these firms to join a government office. We only consider the revolvers that join government within one year at most after the end of their duty in the firm. In contrast to the finding of significant abnormal returns during the period prior to the hiring of revolvers, we do not find any similar evidence of firms doing abnormally well after revolvers leave them to take a job in the government.

– Insert Table 7 about here –

## **5 Revolving door movements and government contracts**

Next we investigate whether there is a relationship between revolving door movements and government contract allocations. In particular, we examine if a firm that employs a revolver is awarded more valuable contracts by the federal government when the revolver

works in a contract-allocating government agency or has influence over it due to his public office.

## 5.1 Government contracts prior to revolver’s corporate employment

We start by measuring how government contract allocation to firms changes as a function of firm-revolver linkages over time. To that end, our estimating equation is:

$$Contract_{i,t} = \alpha + \beta_1 * Revolver_{i,t} + \gamma * X_{i,t} + I_i + Y_t + \epsilon_{i,t}, \quad (4)$$

where  $i$  indexes firms,  $t$  indexes years,  $Contract_{i,t}$  is the US\$ amount (in millions) of contracts allocated to firm  $i$  by the federal government in year  $t$ ,  $Revolver_{i,t}$  is a dummy variable coded as one if a revolver has influence over a contract-allocating government agency in year  $t$  and if this person is later employed by firm  $i$  within two years from  $t$  and zero otherwise,  $X_{i,t}$  are firm control variables,  $I_i$  is industry fixed effect, and  $Y_t$  is year fixed effect.<sup>22</sup> We define a revolver as having influence over a government agency in year  $t$  if the revolver works in the agency in year  $t$  or serves in a congressional committee in year  $t$  that has oversight over that agency. Following Goldman, Rocholl, and So (2013) and Tahoun (2014), we use  $ROA_{i,t}$  (the return on assets for firm  $i$  in year  $t$ ),  $CAPEX2SALES_{i,t}$  (the ratio of capital expenditure to sales for firm  $i$  in year  $t$ ),  $COGS2SALES_{i,t}$  (the ratio of cost of goods sold to sales for firm  $i$  in year  $t$ ),  $BM_{i,t}$  (the book-to-market ratio for firm  $i$  at the end of year  $t$ ),  $SIZE_{i,t}$  (the market capitalization for firm  $i$  at the end of year  $t$ ), and  $HHI_{i,t}$  (the Herfindahl concentration index based on the total sales of all firms with the same two-digit SIC code) as firm control variables.

As government contract allocations are bounded below by zero, we estimate Equation (4) using both OLS and Tobit. With corner solution data, regression coefficients in linear models are known to provide reasonable approximations to the average marginal effects, or even equal average marginal effects under some restrictive assumptions (see Wooldridge, 2010). Tobit models usually provide better estimates of marginal effects (especially at extreme values). However, Tobit estimates are biased and inconsistent once fixed effects are introduced (Lancaster, 2000), although that bias is usually understood to be small

---

<sup>22</sup>We use the Fama and French (1997) 48-industry classification for the industry fixed effect.

(Greene, 2004). Given these different limitations, there are advantages and disadvantages to both OLS and Tobit. In the results that follow, we therefore report OLS and Tobit estimates of the estimating equation taking into account industry and year fixed effects.

Table 8 reports OLS and Tobit coefficient estimates of Equation (4). According to the OLS estimation with firm control variables (column 2), if a firm has a “revolver link” to a government agency in a given year, that is, if the dummy variable  $Revolver_{i,t}$  equals one, then the value of contracts allocated to the firm by the agency increases by a statistically significant US\$234.89 million in that year ( $t = 2.11$ ). The Tobit estimations also reveal a significant positive association between government contracts allocated to firms and the firm-revolver linkages over the years: according to the Tobit estimation with firm control variables (column 4), the coefficient estimate on the revolver link dummy equals US\$335.88 million ( $t = 2.56$ ).

– Insert Table 8 about here –

Next we examine the dynamic pattern of government contract allocations during the years both prior to and after the hiring date of the revolver by the publicly traded firm. The estimation we carry out to assess this pattern is described by the following equation:

$$\begin{aligned}
 Contract_{i,t} = & \alpha + \beta_{-3} * (-3Y)_{i,t} + \beta_{-2} * (-2Y)_{i,t} + \beta_{-1} * (-1Y)_{i,t} \\
 & + \beta_1 * (1Y)_{i,t} + \beta_2 * (2Y)_{i,t} + \beta_3 * (3Y)_{i,t} \\
 & + \gamma * X_{i,t} + I_i + Y_t + \epsilon_{i,t},
 \end{aligned} \tag{5}$$

where, for  $n \in \{1, 2, 3\}$ ,  $(-nY)_{i,t}$  is a dummy variable coded as one if the year  $t$  corresponds to  $n$  years before revolver joins firm  $i$  and zero otherwise, and  $(nY)_{i,t}$  is a dummy variable coded as one if the year  $t$  corresponds to  $n$ th year after revolver joins firm  $i$  and zero otherwise.

Figure 2 shows the Tobit coefficient estimates for the indicator variables,  $(nY)_{i,t}$ , in Equation 5. As this figure shows, there is an increasing positive association between revolvers and the value of government contracts received by their future corporate employers until revolvers switch from government jobs to corporate careers. This is consistent with revolvers’ concerns about post-government employment driving, or at least affecting, government contract allocations. Before hiring, the value of contracts awarded to future

corporate employers increases as the time tends towards revolvers’ hiring dates, because concerns about future employment would become more significant the nearer is the hiring date. After hiring, the value of contracts awarded to current corporate employers decreases over time.

– Insert Figure 2 about here –

## 5.2 Linking abnormal returns and government contracts

The previous analysis provides evidence on the positive association between firms’ receipt of government contracts and their subsequent hiring of revolvers. In this section, we examine whether this positive association manifests itself in the cross-section of firms’ stock returns. To that end, we run standard Fama and MacBeth (1973) regressions of firm-level monthly stock returns on firms’ revolver linkages, their government contract allocations and other firm-level characteristics. The Fama-Macbeth regressions are of the form:

$$r_{i,t+1} = a + bX_{i,t} + c'\gamma_{i,t} + \epsilon_{i,t+1}, \quad (6)$$

where  $r_{i,t+1}$  is the excess return of firm  $i$  in month  $t + 1$ ,  $X_{i,t}$  is the variable of interest in month  $t$  and  $\gamma_{i,t}$  is the vector of firm control variables, namely beta, log of market capitalization, log of book-to-market ratio and momentum (prior cumulated twelve month returns).

– Insert Table 9 about here –

In column 1 of Table 9, the variable of interest is the firm having a “revolver link”: a firm is defined to have a revolver link in month  $t$  if a revolver has influence over a contract-allocating government agency in that month and if this person is later employed by the firm within two years from  $t$ . We find that “firm-revolver linkage” has significant predictive power over the cross-section of excess returns. This is consistent with our portfolio-level analysis reported in Table 4.

In Column 2 of Table 9, the variable of interest is the total US\$ value (in millions) of government contracts allocated to the firm in a given month. Only the unexpected portion of government contract allocations would generate excess return for a given firm

as the expected portion is correctly priced by the market. We find that the total value of government contract allocations does not have predictive power over the cross-section of excess returns, which implies that most of the government contract allocations are expected and therefore correctly priced by the market.

Next we attempt to identify the unexpected portion of government contract allocations attributable to firm-revolver linkages. The market is not likely to know whether a public official would join a certain firm in the near future and whether this would incentivize a quid-pro-quo deal via government contract allocations. Therefore contract allocations attributable to firm-revolver linkages should generate excess returns. To test this hypothesis we carry out a two stage least squares estimation (regressing contract allocations on revolver linkages in the first stage) that yields instrumented value of contract allocations. Specifically, in the first stage, our estimating equation is:

$$Contract_{i,t} = \alpha + \beta Revolver_{i,t} + I_i + Y_t + \epsilon_{i,t},$$

where  $t$  indexes months and otherwise variables are as they are defined for equation (4). Once we estimate regression coefficients in this first stage, we let our variable of interest to be

$$X_{i,t} = \alpha + \beta Revolver_{i,t} + I_i + Y_t$$

and use it in equation (6) for our second stage regression. The result of this second stage regression is reported in column 3 of Table 9. We find that our variable of interest, i.e., the IV predicted value of government contract allocations to the firm, has predictive power over the cross-section of excess returns.

Column 4 of Table 9 reports the result of a placebo test where the firm-revolver linkage is falsely assumed to exist for up to a 2-year period (matching the duration of revolver's government tenure with 2 years being the upper bound) immediately before the revolver starts his government career. When we use this "*placebo firm-revolver linkage*" in the first stage regression explained above, we find that the *placebo IV predicted value of government contract allocations to the firm* has no predictive power over excess returns.

## 6 Revolving door movements and earnings forecasts

Identifying the causal impact of revolving door movements is challenging, because it is difficult to find observable counterfactuals i.e. financial and operating performance that would have been achieved by the firms in the absence of revolver linkages. However, we have access to analyst and management earnings forecasts and we will exploit differences between these forecasts to better identify revolvers' causal impact. Our thinking goes as follows.

If there is a quid pro quo relationship between a revolver and a firm, firm management is insider to this relationship but analysts are outsiders. Since equity analysts are unaware of the revolvers just like the market, analyst forecasts cannot take into account benefits brought by firm-revolver linkages whereas management forecasts can. Therefore, analyst forecasts provide us a reliable counterfactual: analysts' consensus forecast is a good proxy to what the firm would have earned and what the management would have forecasted in the absence of firm-revolver linkages. In fact, between years minus 8 and minus 2 relative to the revolvers' job switch from government to firms and during the firm tenure of revolvers, managers and equity analysts possess no difference in earnings forecasts and earnings surprises.

But why would management forecasts incorporate potential benefits brought by revolver linkages? Management earnings forecasts are voluntary disclosures and they are released to guide and influence investors. They have been shown to affect stock prices (Pownall et al., 1993), and they are known to influence analyst stock ratings and investor decisions to buy or sell the stock. Therefore, firm managements would have incentive to incorporate into their forecasts any piece of information that would have positive impact on earnings. Furthermore, management need not (and, of course, would not) share the true reason for their optimism in the case of revolver-linked expectations. There are many moving components in earnings forecasts and revolver-linked optimism could be easily camouflaged. Also, firm managements need not fully incorporate revolver-linked optimism into their forecasts, but we can still exploit the partially revealed difference between analyst and management expectations for identification purposes. Finally, any muting impact of manager mindfulness that causes them to hide their expectations would make

it harder for us to find our results<sup>23</sup>.

To sum up, if indeed some revolvers favor their future corporate employers while in public office, then we can potentially expect (i) managements of firms hiring revolvers to be more optimistic in their forecasts compared with analysts during the revolvers' government tenure prior to hiring and (ii) managements to better predict earnings attributable to revolver linkages compared with analysts. We next put these hypotheses to test.

## 6.1 Differences between analyst and management expectations

Our estimating equations for analysts' forecast surprise and management's forecast surprise are

$$\text{Consensus surprise}_{i,t} = \alpha_c + \beta_c R_{i,t} + \gamma'_c F_{i,t-1} + I_i + Y_t + \epsilon_{i,t}, \quad (7)$$

$$\text{Guidance surprise}_{i,t} = \alpha_g + \beta_g R_{i,t} + \gamma'_g F_{i,t-1} + I_i + Y_t + \epsilon_{i,t}, \quad (8)$$

respectively<sup>24</sup>. *Consensus surprise*<sub>*i,t*</sub> is the actual earnings per share (EPS) of firm *i* for the fiscal year ending in year *t* minus the median Institutional Brokers Estimate System (I/B/E/S) analyst forecast, deflated by stock price. The I/B/E/S consensus forecast is taken 8 months prior to the end of the forecast period (and therefore 4 months after the previous fiscal year-end).<sup>25</sup> *Guidance surprise*<sub>*i,t*</sub> is the actual EPS of firm *i* for the fiscal year ending in year *t* minus the I/B/E/S Guidance forecast (i.e., management forecast), deflated by the stock price. We take the I/B/E/S Guidance forecast provided on the closest date to the I/B/E/S consensus forecast used in *Consensus surprise*<sub>*i,t*</sub>. *R*<sub>*i,t*</sub> is a dummy variable coded as one if a revolver works in the government in year *t* and if this person is later employed by firm *i* within one year from *t* and zero otherwise. *F*<sub>*i,t-1*</sub> is the vector of firm control variables, the log of book-to-market ratio, and the log of book value of assets as of the end of year *t* - 1. *I*<sub>*i*</sub> is industry fixed effect and *Y*<sub>*t*</sub> is year fixed effect. We also look into the difference between analysts' forecast surprise and management's forecast

<sup>23</sup>To our knowledge, there has never been a federal investigation related to government connections in the history our U.S. that arose from an unexplained and bullish earning guidance.

<sup>24</sup>Cornaggia et al. (2015) follows a similar methodology when they analyze ratings of revolving door analysts from credit rating agencies to the firms that they used to rate. Their counterfactual is benchmark ratings of of non-transitioning analysts. Our advantage is we also observe the actual and we can directly compute earnings surprises.

<sup>25</sup>This specification ensures that analysts know prior earnings when making their forecasts as annual reports are mostly filed within 3 months after the fiscal year-end.



surprise:

$$\begin{aligned} \text{Difference of expectations}_{i,t} &= \text{Consensus surprise}_{i,t} - \text{Guidance surprise}_{i,t} \\ &= \alpha_f + \beta_f R_{i,t} + \gamma'_f F_{i,t-1} + I_i + Y_t + \epsilon_{i,t}. \end{aligned} \quad (9)$$

We estimate Equations (7)-(9) using pooled regressions.

– Insert Table 10 about here –

The results are shown in Panel A of Table 10. Both analysts and managements are positively surprised by the earnings announcements of firms that have a revolver linkage (see columns 1 and 2). However, the positive surprise associated with revolver-linked firms was statistically significantly greater in the analyst forecasts compared with the management forecasts (see column 3). This is in line with the hypothesis that there is a quid pro quo relationship between some public officials and their future corporate employers, that firm managements know about these relationships while analysts do not and that therefore management forecasts are better in predicting potential operating benefits brought by such relationships compared with analyst forecasts.

Panel B of Table 10 reports the result of a placebo test where the firm-revolver linkage is falsely assumed to exist for up to a 1-year period immediately before the revolver starts his government career.<sup>26</sup> The *placebo firm-revolver linkage* has no statistically significant effect on either analyst forecast surprises or management forecast surprises (see columns 4 and 5). Moreover, the difference between analyst and management forecast surprises is not statistically significant for firms with placebo revolver linkages (see column 6).

## 6.2 Falsification tests

We next offer two further falsification tests involving earnings forecasts. For our first falsification test, we identify revolvers who were employed in government agencies that did not allocate any contracts during the last year of their government tenure to the firms that subsequently hired them. We call these revolvers *clean sheet revolvers* as they are not likely to have been involved in quid pro quo relationships with their future corporate

---

<sup>26</sup>We match the duration of revolver’s government tenure with 1 year being the upper bound.

employers while in government service.<sup>27</sup> If indeed our earlier tests exploiting differences between analyst and management expectations correctly identify existence of quid pro quo relationships, then we would expect the difference between analyst and management expectations to be much more muted, if any, in the case of firms hiring clean sheet revolvers. In order to put this hypothesis to test, we re-define the dummy variable  $R_{i,t}$  in Equations (7)-(9) so that it is coded as one if only *clean sheet revolvers* work in the government in year  $t$  and if they are later employed by firm  $i$  within one year from  $t$  (and zero otherwise). By doing so, we restrict the revolver-firm linkages to only those between clean sheet revolvers and firms. Panel A of Table 11 reports the results of regressions run on modified equations explained above. The *linkage between clean sheet revolvers and firms* has no statistically significant effect on either analyst forecast surprises or management forecast surprises (see columns 1 and 2). Also, as expected, the difference between analyst and management forecast surprises is not statistically significant for firms that hire only clean sheet revolvers (see column 3).

– Insert Table 11 about here –

For our second falsification test, we identify firms which received government contracts but did not hire any revolvers later on. We then falsely assume that these firms have a revolver linkage, that is, we re-define the dummy variable  $R_{i,t}$  in Equations (7)-(9) so that it is coded as one if firm  $i$  received a government contract in year  $t$  and if it did not employ any revolvers after year  $t$  (and zero otherwise). Once again, if indeed our earlier tests exploiting differences between analyst and management expectations correctly identify existence of quid pro quo relationships, then we would not expect any statistically significant difference between analyst and management expectations in the case of firms which are falsely assumed to have revolver linkages. Panel B of Table 11 reveals that, as expected, the difference between analyst and management forecast surprises is not statistically significant for firms that are falsely assumed to have hired revolvers after receiving government contracts (see column 6).

---

<sup>27</sup>Revolvers have of course other means, such as regulations, to favor firms while in government service. However, government contract allocations provide us the most clearly identifiable channel to test existence of quid pro quo relationships between public officials and their future corporate employers.

## 7 Revolving door movements and executive orders

To further mitigate the concern for reverse causality and other alternative explanations, we execute new tests in this section that exploit time-series and cross-sectional variations in regulatory restrictions imposed on revolving door movements. For motivation, consider the following thought experiment: assume there are potentially two types of revolvers in the world, revolvers that provide no favors to corporations while in public office and revolvers that are willing to provide favors hoping to cash later on from these actions. If regulations made revolving door movements more difficult for a group of revolvers but not all of them, then we would not expect the first type to change their behavior whether or not they fell under regulatory restrictions. However, such regulations, by making it more difficult to secure future employment in favored firms, would disincentivize the latter type from providing favorable treatment in exchange for future employment (or from seeking public office to start with). Under the null hypothesis of all revolvers being of the first type, the introduction of regulatory restrictions on revolving door movements would not change the allocation pattern of government contracts. In particular, the correlation between contracts granted and future employment obtained would be the same with or without restrictions. Restrictions would have an impact, however, under the alternative, as revolvers that provide favors hoping to obtain future employment would become less prevalent during periods in which they fell under restrictions. As a result, we would expect the correlation between contracts granted and employment obtained to be lower in this latter case. A similar pattern would be also observed in the correlation between abnormal returns and revolvers' subsequent corporate employment: if government contract allocations are indeed affected by post-government employment concerns of revolvers, then markets, as outsiders to the relationship between revolvers and firms, would not be able to correctly price favors granted to firms during revolvers' government tenure.

During our sample period (1990-2012), there are two presidential executive orders that introduced restrictions on revolving door movements for varying durations. The first one is the executive order, 12834, issued by President Clinton at the beginning of his presidency (January 20, 1993). This executive order required up to five-year cooling off periods for presidential and vice-presidential appointees which restricted their private employment

opportunities after leaving government posts. The order was revoked by Clinton at the end of his presidency (December 28, 2000) and similar restrictions were not re-instituted during the subsequent Bush Administration. When President Obama assumed office, he issued an executive order on January 21, 2009, which was similar in nature to the Clinton executive order. According to Obama’s executive order, 13490, presidential and vice-presidential appointees must abide by a two-year cooling off period after their government service<sup>28</sup>. Neither Clinton nor Obama executive orders prohibit a former public official from working in a private firm merely because the firm had done business with or had been regulated by the official’s agency. Rather, they prohibit subsequent representational or advocacy types of activities, that is, where the former official makes “any communication or ... appearance” to or before the government agency where he worked at “with the intent to influence” his former colleagues about government policy or decisions.<sup>29</sup> Enforcement clauses of these executive orders are stringent: violations may trigger extension of restrictions for an additional 5 years, judicial civil proceedings as well as prosecution by the Attorney General in the United States District Courts.<sup>30</sup>

To sum up, the Clinton and Obama executive orders limit the appeal of senior public officials as job candidates for private firms which do business with the federal government. As a result, revolving door movements have been relatively difficult for presidential and vice-presidential appointees during the years corresponding to Clinton and Obama presidencies (namely, 1993-2000 and 2009-2012).

## 7.1 Abnormal returns and executive orders

We know from analysis conducted in Section 4.1 that firms exhibit significant abnormal returns during the two-year period immediately before the hiring of revolvers. We now in-

---

<sup>28</sup>In fact, paragraph 5 of EO 13490 forces appointees “upon leaving Government service, not to lobby any covered executive branch official or non career Senior Executive Service appointee for the remainder of the Administration”.

<sup>29</sup>See Maskell (2014), a Congressional Research Service report, for a detailed and comprehensive summary of laws and regulations for post-government employment of federal personnel.

<sup>30</sup>Many believe that presidential waivers are easily given to presidential or vice-presidential appointees. In 2014, there were 1157 government employees subject to executive order 13490 and only one appointee (Capricia P. Marshall, former Chief of Protocol of the United States) received a limited waiver of the restrictions. In 2013, there were 873 government employees subject to executive order 13490 and nobody received a waiver on the executive order. The pattern was similar for 2012 and 2011. For more information on this, see Annual Report on Executive Order 13490 at <http://www.oge.gov/Open-Government/Presidential-Appointee--Nominee-Records/>.

investigate how abnormal returns associated with hiring of presidential and vice-presidential appointees compare with those associated with hiring of other revolvers during Clinton and Obama presidencies. We measure abnormal returns as realized returns in excess of conditional expected returns with time varying betas for Fama-French-Carhart risk factors. In particular, we define abnormal return of firm  $i$  in month  $t$  to be

$$AR_{i,t} = r_{i,t} - (\beta_i(z_{t-1}))' f_t,$$

where  $r_{i,t}$  is the realized return and  $\beta_i(z_{t-1})$  and  $f_t$  are as they are specified in Equations (2)-(3). Our estimating equation is

$$AR_{i,t} = a + b \text{Restrictive}_t + \epsilon_{i,t}$$

where  $\text{Restrictive}_t$  is a dummy variable coded as one if month  $t$  is within the Clinton and Obama presidential terms and zero otherwise. We regress this equation for two groups of firm-month observations: *Treatment firms in month  $t$*  are the firms that hired a presidential or vice-presidential appointee from the government within the subsequent two years following month  $t$ . *Control firms in month  $t$*  are those that hired only those who are not presidential or vice-presidential appointees from the government within the subsequent two years following month  $t$ .

Table 12 reports the regression results: Clinton and Obama presidencies have a statistically significant and negative impact on abnormal returns of firms that subsequently hire presidential and vice-presidential appointees compared with abnormal returns of firms that hire non-appointee revolvers. During the nonrestrictive years the alpha difference between these two groups is statistically insignificant<sup>31</sup>. This is in line with the hypothesis that some revolvers engage in quid pro quo deals with their future corporate employers and that Clinton and Obama executive orders curb such quid pro quo deals for revolvers who are presidential and vice-presidential appointees.

---

<sup>31</sup>Abnormal returns difference between these two groups are not only significant but also flat during nonrestrictive years. Results are available upon request.

## 7.2 Government contract allocations and executive orders

Next we look into the impact of Clinton and Obama presidencies on government contract allocations. We use the following equation for estimation:

$$\begin{aligned}
 Contract_{i,t} = & \alpha + \beta_1 * Appointee_{i,t} + \beta_2 * Non - appointee_{i,t} \\
 & + \beta_3 * Restrictive_t + \beta_4 * Appointee_{i,t} * Restrictive_t \\
 & + \beta_5 * Non - appointee_{i,t} * Restrictive_t \\
 & + \gamma * X_{i,t} + I_i + Y_t + \epsilon_{i,t},
 \end{aligned} \tag{10}$$

where  $i$  indexes firms,  $t$  indexes years, and  $Contract_{i,t}$  is the value of government contracts (in millions of US dollars) allocated to firm  $i$  in year  $t$ .  $Appointee_{i,t}$  equals one if firm  $i$  hired a presidential or vice-presidential appointee from the government within the subsequent two years following month  $t$ , and zero otherwise.  $Non - appointee_{i,t}$  equals one if firm  $i$  hired only those who are not presidential or vice-presidential appointees from the government within the subsequent two years following month  $t$ , and zero otherwise.  $Restrictive_t$  is a dummy variable coded as one if year  $t$  is among the years when there were restrictions on revolving door movements and zero otherwise.  $X_{i,t}$  are firm control variables,  $I_i$  is industry fixed effect,  $Y_t$  is year fixed effect. We use the same firm control variables as the ones employed in Equation (4) and industry fixed effect is as it is specified in Equation (4).

Table 13 reports the regression results on Equation (10). In particular, we observe from column (4) that the coefficient estimate for the interaction term between  $Appointee_{i,t}$  and  $Restrictive_t$  is statistically significant and negative while the coefficient estimate for the interaction term between  $Non - appointee_{i,t}$  and  $Restrictive_t$  is statistically insignificant. In other words, Clinton and Obama presidential terms have a statistically significant negative impact on government contract allocations attributable to hirings of presidential and vice-presidential appointees, but the same presidential terms have no statistically significant impact on government contract allocations attributable to hirings of non-appointee revolvers. This is, once again, in line with the hypothesis that some revolvers favor certain firms in return for future employment and that Clinton and Obama executive orders curb such quid pro quo deals for presidential and vice-presidential appointees.

### 7.3 Earnings forecasts and executive orders

In this section, we first identify revolvers who were employed in government agencies which allocated contracts during the last two years of their government tenure to the firms that subsequently hired them. In the vernacular of Section 6.2, these revolvers do not have *clean sheet records*, because compared to others they are more likely to have been involved in quid pro quo relationships with their future corporate employers while in government service. Following our previous analysis on earnings forecasts, we expect management, compared with analysts, to better predict earnings attributable to revolvers without clean sheet records. However, if indeed Clinton and Obama executive orders curb presidential and vice-presidential appointees' quid pro quo deals with firms, then the difference between management and analyst earnings expectations should decrease in the case of firms that hire appointee revolvers during Clinton and Obama presidential terms. We put this hypothesis to test next.

Our estimating equations are

$$\begin{aligned}
 \text{Consensus surprise}_{i,t} &= c_0 + c_1 \text{Appointee}_{i,t} + c_2 \text{Non - appointee}_{i,t} \\
 &+ c_3 \text{Appointee}_{i,t} * \text{Restrictive}_t \\
 &+ c_4 \text{Non - appointee}_{i,t} * \text{Restrictive}_t \\
 &+ c'_5 F_{i,t-1} + Y_t + \epsilon_{i,t}, \\
 \text{Guidance surprise}_{i,t} &= g_0 + g_1 \text{Appointee}_{i,t} + g_2 \text{Non - appointee}_{i,t} \\
 &+ g_3 \text{Appointee}_{i,t} * \text{Restrictive}_t \\
 &+ g_4 \text{Non - appointee}_{i,t} * \text{Restrictive}_t \\
 &+ g'_5 F_{i,t-1} + Y_t + \epsilon_{i,t}.
 \end{aligned}$$

Also, as was the case earlier,

$$\text{Difference of expectations}_{i,t} = \text{Consensus surprise}_{i,t} - \text{Guidance surprise}_{i,t}.$$

Above,  $\text{Appointee}_{i,t}$  is a dummy variable coded as one if an appointee revolver without a clean sheet record works in the government in year  $t$  and if this person is later employed

by firm  $i$  within two years from  $t$  and zero otherwise.  $Non - appointee_{i,t}$  is a dummy variable coded as one if a non-appointee revolver without a clean sheet record works in the government in year  $t$  and if this person is later employed by firm  $i$  within two years from  $t$  and zero otherwise.  $Restrictive_t$  is a dummy variable coded as one if year  $t$  is among the years when there were restrictions on revolving door movements and zero otherwise. Other variables are as they are defined in Equations (7)-(9).<sup>32</sup>

As the number of observations involving presidential and vice-presidential appointees without clean sheet records is sparse during Clinton years, we restrict our sample period to 2001-2012 for our regressions on the above estimating equations. Table 14 reports the regression results. Column 3 reveals that, as hypothesized, the difference between management and analyst earnings expectations decrease in the case of firms that hire appointee revolvers during Clinton and Obama years and this decrease is statistically significant. On the other hand, there is virtually no change in the difference between management and analyst expectations for firms that hire non-appointee revolvers during Clinton and Obama years. All of these results point to existence of revolvers favoring firms in return for future employment and Clinton and Obama executive orders curbing such conflicts of interest for presidential and vice-presidential appointees.

## 8 Discussion

In addition to our quid pro quo hypothesis of firms luring revolvers by offering them high-paying jobs and receiving more valuable government contracts in exchange, there might be alternative explanations that are also in line with our results. In this section we go over some of the potential endogeneity concerns in our results and these alternative explanations.

A primary concern in revolving door research is the reverse causality problem. In our framework, the return outperformance of firms hiring revolvers in the period preceding the hiring could be as much the result of revolvers seeking and obtaining employment in firms that have done well (and to some extent unexpectedly well) in the recent past as they could be the result of revolvers actually helping their future employers. Firms

---

<sup>32</sup>Note that the estimating equations omit industry fixed effects. However, our results are robust to their inclusion.



that have done well in the recent past could simply have more resources and therefore be able to hire more aggressively than firms that do not do well. If that is the case, revolvers will more likely end up working for them than for a less successful competitor. That could explain why firms that hire revolvers exhibit positive abnormal returns in the pre-hiring period. Our results in Tables 12 and 13, however, are in line with the notion that the revolving door effect has more to do with lobbying than successful firms needing to hire more people. If the successful firms were hiring independent of lobbying concerns, they would have continued to hire revolvers independent of lobbying restrictions. Therefore, the abnormal returns of firms that hire appointed revolvers would not disappear during restrictive years and the government contract allocation to these firms would not be effected by executive orders<sup>33</sup>. Furthermore, we also show that managers command more bullish and correct earnings during the government tenure of the revolvers, which further supports the quid pro quo hypothesis and doesn't support this alternative scenario excluding a sincere manager optimism scenario that we explain below.

Another alternative hypothesis is sincere manager optimism. It may be the case that managers can genuinely forecast superior future earnings without the support of revolvers, while the equity analysts cannot<sup>34</sup>. After the unexpected (to outsiders) high performance of their firm, managers hire revolvers to lobby the government in order to get government contracts and sustain the superior performance. In this scenario, the managers are not corrupt. They honestly foresee the future, or they become optimistic for unobservable reasons and they are luckily right, and they want to hire lobbyists to sustain the firm performance by lobbying. Our results in Table 11, however, are not in line with this hypothesis. In Panel A, we show that only the revolving door effect is not significant for *clean sheet revolvers*. Furthermore, in Panel B of Table 11 we show that managers of firms that receive government contracts but didn't hire any revolvers do not have similar "clairvoyant" forecasts. Therefore, it's only the managers that have revolving door connections with the government that are more bullish and correct. These results are in line with the quid pro quo hypothesis but not with sincere manager optimism hypothesis. If

---

<sup>33</sup>Our results in Tables 12 and 13 also reject the hypothesis that the firms would still be able to lure revolvers into wrongdoing by offering them non-lobbying jobs after their government tenure. If this was the case, the executive orders would have no impact on portfolio returns and government contracts.

<sup>34</sup>This would however have to be a one-time event, because in the placebo periods we don't see such a phenomenon.

the managers are genuinely optimistic without the help of revolvers, we should get similar results from the *clean sheet revolvers* sample. If only the managers of firms that receive government contract are clairvoyant, then we should see more bullish and correct forecasts also from the managers of firms that receive government contracts but did not hire any revolvers.

Finally, it may also be the case that the following unlikely scenario holds: only the managers of firms that hire *non-clean sheet revolvers* possess sincere manager optimism. Maybe only they can genuinely forecast superior future earnings without the support of revolvers, while the equity analysts cannot. Our results Table 14 are however not in line with this hypothesis. If only the managers of firms that hire *non-clean sheet revolvers* are “clairvoyant” and our results are not driven by the quid pro quo relationships, then we should see no impact of executive orders on their optimism. In column (3) of Table 14, however, we see only that the managers of firms that have appointed revolver connections lose their optimism in restrictive years.

## 9 Concluding remarks

In this paper we link revolving door movements to corporate financial and operating performance. We show that firms that hire public officials outperform the remaining firms by a statistically significant 5.50% to 8.57% per year, in the two-year period immediately preceding the hiring. Similarly, we document that firms typically receive more valuable government contracts when a future firm employee is holding a post in the government. The results we find are stronger in the years immediately before the hiring and significantly weaken as we move further away from that date.

During Clinton’s and Obama’s presidencies, presidential executive orders restrict revolving door movements for presidential and vice-presidential appointees. We show that abnormal returns of firms that subsequently hire “appointee revolvers” significantly diminish during Clinton and Obama years compared with abnormal returns of firms that subsequently hire non-appointee revolvers. A similar pattern is observed for the relationship between government contract allocations, the hiring of public officials and Clinton’s and Obama’s presidencies.

We also exploit differences between analyst and management earnings forecasts to better identify the causal impact of revolving door movements. We find that management forecasts are more optimistic about and better in predicting earnings of firms that subsequently hire public officials compared with analyst forecasts. The latter result points to, in the case of revolver-hiring firms, management being insiders and analysts being outsiders to some information that have positive effect on firm earnings during revolvers' government tenure.

Collectively, our findings are consistent with the view that some in the government service could be favoring firms in order to gain future employment with them. "The aim of every political Constitution, is or ought to be, first to obtain for rulers men who possess most wisdom to discern, and most virtue to pursue, the common good of society; and in the next place, to take the most effectual precautions for keeping them virtuous whilst they continue to hold their public trust," wrote Madison (1788) in the Federalist Papers (No. 57). This paper highlights the need to monitor, and perhaps, reform the institutional incentives surrounding revolving door movements so that public officials act in the public interest. It also highlights the need for a better and deeper understanding of the formal and informal relationships between governments and firms.

There are natural extensions to our study. We currently focus on the impact of revolving door movements on returns and government contract allocations, but overlook the potential impact on legislation, government policy and regulation. A comprehensive picture of the problem would also require a better understanding of the potential deterrent effect that overly restrictive provisions on revolving door movements will have upon seeking and retaining talent for government service. Furthermore, adopting limitations on revolving door movements may insulate public officials from private sector concerns to a degree not desirable for public policy reasons. We leave these extensions and considerations for future work.

## References

- Acemoglu, D., S. Johnson, A. Kermani, J. Kwak, and T. Mitton (2015), “The value of connections in turbulent times: evidence from the United States,” *working paper*.
- Amemiya, T. (1973), “Regression analysis when the dependent variable is truncated normal,” *Econometrica*, 41(6), 997–1016.
- Barrington, R., M. Macaulay, and J. Scott (2010), Corruption in the UK: part one – national opinion survey, Transparency International UK. Available from [www.transparency.org.uk/our-work/publications/91-corruption-in-the-uk--part-one---national-opinion-survey](http://www.transparency.org.uk/our-work/publications/91-corruption-in-the-uk--part-one---national-opinion-survey).
- Belo, F., V. Gala, and J. Li (2013), “Government spending, political cycles, and the cross section of stock returns,” *Journal of Financial Economics*, 107(2), 305–324.
- Blanes i Vidal, J., M. Draca, and C. Fons-Rosen (2012), “Revolving door lobbyists,” *American Economic Review*, 102(7), 3731–3748.
- Bunkanwanicha, P. and Y. Wiwattanakantang (2009), “Big business owners in politics,” *Review of Financial Studies*, 22(6), 2133–2168.
- Carhart, M.M. (1997), “On persistence in mutual fund performance,” *Journal of Finance*, 52(2), 57–82.
- Cingano, F. and P. Pinotti (2013), “Politicians at work: the private returns and social costs of political connections,” *Journal of the European Economic Association*, 11(2), 433–465.
- Cohen, J.E. (1986), “The dynamics of the revolving door on the FCC,” *American Journal of Political Science*, 30(4), 689–708.
- Cohen, L., K. Diether, and C. Malloy (2013), “Legislating stock prices,” *Journal of Financial Economics*, 110(3), 574–595.
- Cooper, M., H. Gulen, and A. Ovtchinnikov (2010), “Corporate political contributions and stock returns,” *Journal of Finance*, 65(2), 687–724.
- Cornaggia, J., K.J. Cornaggia, and H. Xia (2015), “Revolving doors on Wall Street,” *working paper*.
- Dal Bo, E. (2006), “Regulatory capture: a review,” *Oxford Review of Economic Policy*, 22(2), 203–225.
- Daniel, K., M. Grinblatt, S. Titman, and R. Wermers (1997), “Measuring mutual fund performance with characteristics-based benchmarks,” *Journal of Finance*, 52(3), 1035–1058.

- David-Barrett, L. (2011), Cabs for hire? Fixing the revolving door between government and business, Transparency International UK. Available from [www.transparency.org.uk/our-work/publications/132-cabs-for-hire?--fixing-the-revolving-door-between-government-and-business](http://www.transparency.org.uk/our-work/publications/132-cabs-for-hire?--fixing-the-revolving-door-between-government-and-business).
- Faccio, M. (2006), "Politically connected firms," *American Economic Review*, 96(1), 369–386.
- Fama, E.F. and K.R. French (1993), "Common risk factors in the returns on stocks and bonds," *Journal of Financial Economics*, 33(1), 3–56.
- Fama, E.F. and K.R. French (1997), "Industry costs of equity," *Journal of Financial Economics*, 43(2), 153–193.
- Fama, E.F. and J.D. Macbeth (1973), "Risk, return, and equilibrium: empirical tests," *Journal of Political Economy*, 81(3), 607–636.
- Ferson, W.E. and C.R. Harvey (1999), "Conditioning variables and the cross section of stock returns," *Journal of Finance*, 54(4), 1325–1360.
- Fisman, R. (2001), "Estimating the value of political connections," *American Economic Review*, 91(4), 1095–1102.
- Fisman, D., R. Fisman, J. Galef, R. Khurana, and Y. Wang (2012), "Estimating the value of connections to Vice-President Cheney," *B.E. Journal of Economic Analysis & Policy*, 13(3), Article 5.
- Goldman, E., J. Rocholl, and J. So (2009), "Do politically connected boards affect firm value?" *Review of Financial Studies*, 22(6), 2331–2360.
- Goldman, E., J. Rocholl, and J. So (2013), "Political connections and the allocation of procurement contracts" *Review of Finance*, 13(5), 1617–1648.
- Gormley, W.T. (1979), "A test of the revolving door hypothesis at the FCC," *American Journal of Political Science*, 23(4), 665–683.
- Grace, M.F. and R.D. Phillips (2008), "Regulator performance, regulatory environment and outcomes: an examination of insurance regulator career incentives on state insurance markets," *Journal of Banking and Finance*, 32(1), 116–133.
- Greene, W. (2004), "Fixed effects and the incidental parameters problem in the Tobit model," *Econometric Reviews*, 23(2), 125–148.
- Hall, P. (1992). *The Bootstrap and Edgeworth Expansion*, New York: Springer Verlag.
- Jayachandran, S. (2006), "The Jeffords effect," *Journal of Law and Economics*, 49(2), 397–425.
- Jensen, M. (1968), "The performance of mutual funds in the period 1945–1964," *Journal*

*of Finance*, 23(2), 389–416.

Johnson, S. and T. Mitton (2003), “Cronyism and capital controls: evidence from Malaysia,” *Journal of Financial Economics*, 67(2), 351–382.

Kuziemko, I. and E.D. Werker (2006), “How much is a seat on the Security Council worth? Foreign aid and bribery at the United Nations,” *Journal of Political Economy*, 114(5), 905–930.

Lancaster, T. (2000), “The incidental parameters problem since 1948,” *Journal of Econometrics*, 95(2), 391–413.

Li, H., L. Meng, Q. Wang, and L. Zhou (2008), “Political connections, financing and firm performance: evidence from Chinese private firms,” *Journal of Development Economics*, 87(2), 283–299.

Lucca, D., A. Seru, and F. Trebbi (2014), “The revolving door and worker flows in banking regulation,” *Journal of Monetary Economics*, 65, 17–32.

Madison, J. (1788 [1961]). *The Federalist Papers: A Collection of Essays in Support of the Constitution of the United States*, New York: Doubleday.

Maskell, J. (2014). *Post-Employment, Revolving Door, Laws for Federal Personnel*, Washington, DC: Congressional Research Service.

Newey, W.K. and K.D. West (1987), “A simple, positive semi-definite, heteroskedasticity and autocorrelation consistent covariance matrix,” *Econometrica*, 55(3), 703–708.

Pastor, L. and P. Veronesi (2012), “Uncertainty about government policy and stock prices,” *Journal of Finance*, 64(4), 1219–1264.

Petersen, M.A. (2009), “Estimating standard errors in finance panel data sets: Comparing approaches,” *Review of Financial Studies*, 22(1), 435–480.

Petkova, R. and L. Zhang (2005), “Is value riskier than growth?” *Journal of Financial Economics*, 78(1), 187–202.

Pigou, A.C. (1938). *The Economics of Welfare*, 4th Ed., London: Macmillan and Co.

Pownall, G., C. Wasley, and G. Waymire (1993), “The stock price effects of alternative types of management earnings forecasts,” *Accounting Review*, 68(4), 896–912.

Revolving Door Working Group (2005), A matter of trust: how the revolving door undermines public confidence in government – and what to do about it. Available from [www.cleanupwashington.org/documents/RevovDoor.pdf](http://www.cleanupwashington.org/documents/RevovDoor.pdf).

Sharpe, W. (1964), “Capital asset prices: a theory of market equilibrium under conditions of risk,” *Journal of Finance*, 19(3), 425–442.

Spiller, P.T. (1990), "Politicians, interest groups, and regulators: a multiple-principals agency theory of regulation, or let them be bribed," *Journal of Law and Economics*, 33(1), 65–101.

Tahoun, A. (2014), "The Role of Stock Ownership by US Members of Congress on the Market for Political Favors," *Journal of Financial Economics*, 111(1), 86–110.

Wermers, R. (2003), "Is money really smart? New evidence on the relation between mutual fund flows, manager behavior, and performance persistence," *working paper*.

Wooldridge, J.M. (2010). *Econometric Analysis of Cross Section and Panel Data*, 2nd Ed., Boston: MIT Press.

Zeleny, J. (2007), "Obama says new rules would guide his administration," *New York Times*, 23 June. Available from [www.nytimes.com/2007/06/23/us/politics/23campaign.html](http://www.nytimes.com/2007/06/23/us/politics/23campaign.html).

Table 1: Summary statistics for revolvers and their corporate employers

This table presents year by year summary statistics for the sample of revolvers and firms used in our study. Panel A of the table shows, for each year between 1990 and 2012, the number of revolvers employed by public firms, the number of newly hired revolvers, the number of public firms that employ at least one revolver, and the number of public firms that hire a new revolver. Panels B and C of the table present the mean, median, minimum and maximum of market capitalizations of public firms that employ revolvers and that do not in each year, respectively.

Year	Panel A				Panel B				Panel C			
	Revolving door movements				Market values of firms with revolvers (million US\$)				Market values of firms w/o revolvers (million US\$)			
	Number of revolvers employed	Number of new revolvers hired	Number of firms that employ a revolver	Number of firms that hire a new revolver	Mean	Median	Minimum	Maximum	Mean	Median	Minimum	Maximum
1990	15	2	14	2	7,898	3,923	777	26,387	793	88	0	64,529
1991	18	4	17	4	9,865	6,030	78	27,713	1,131	159	0	75,653
1992	19	3	18	3	13,093	5,743	157	69,294	1,103	163	0	75,884
1993	19	4	18	4	15,174	10,055	101	48,773	1,006	140	1	89,452
1994	21	4	20	4	12,139	7,868	65	49,415	884	114	0	87,193
1995	27	4	24	4	16,739	10,523	90	75,335	1,191	159	1	120,260
1996	32	10	27	10	18,660	11,359	42	92,027	1,329	165	0	162,790
1997	41	9	34	8	24,880	12,180	52	109,639	1,601	176	0	240,136
1998	49	15	41	15	30,199	13,025	16	162,224	1,966	151	0	342,558
1999	59	15	50	13	42,376	8,199	25	602,433	2,467	183	2	507,217
2000	60	11	49	11	40,670	12,062	45	290,216	2,414	146	0	475,003
2001	78	28	61	24	36,274	9,729	105	357,949	2,256	233	1	398,105
2002	76	14	59	14	30,407	9,630	93	276,631	1,858	197	1	242,270
2003	82	18	58	14	43,477	18,028	238	311,066	2,548	369	5	271,002
2004	90	14	62	14	45,072	19,072	472	385,883	2,747	394	4	330,693
2005	103	23	65	21	50,185	25,462	466	370,344	2,798	400	3	198,839
2006	114	21	75	20	52,755	23,476	193	446,944	3,010	458	1	239,758
2007	124	29	79	28	57,663	27,305	358	511,887	2,944	381	1	228,016
2008	125	17	82	17	36,080	14,769	329	406,067	1,757	211	0	184,576
2009	133	32	88	30	43,468	22,721	385	322,668	2,475	380	2	190,983
2010	127	19	84	17	51,712	26,404	306	368,712	2,876	471	1	297,089
2011	159	44	99	36	51,336	23,649	99	406,272	2,613	423	1	211,894
2012	167	26	105	24	52,486	20,018	443	499,696	3,110	517	1	216,438



Table 2: Top hiring firms and industries

Panel A lists the publicly listed firms that employ the highest number of revolvers during the sample period, 1990-2012. Panel B presents the industries that revolvers most frequently find jobs in during the same sample period. The industries are classified according to 2-digit SIC codes, and the frequency of revolver employment is measured in terms of percentage of firm-year observations.

Panel A			Panel B		
Rank	Top hiring firms	# of revolvers employed	Rank	Top hiring industries	Frequency
1	Lockheed Aircraft	15	1	Electric, Gas, And Sanitary Services	8.42%
2	Raytheon	11	2	Communications	8.06%
3	Citigroup	11	3	Business Services	7.69%
4	Boeing	10	4	Transportation Equipment	6.96%
5	AT&T	9	5	Chemicals And Allied Products	6.59%
6	Goldman Sachs	9	6	Electronic And Other Electrical Equipment And Components except Computer Equipment	5.49%
7	General Electric	8	7	Measuring, Analyzing, and Controlling Instruments; Photographic, Medical and Optical Goods; Watches and Clocks	4.76%
8	Northrop	7	8	Depository Institutions	4.76%
9	JP Morgan Chase	7	9	Industrial And Commercial Machinery and Computer Equipment	4.03%
10	Google	7	10	Security And Commodity Brokers, Dealers, Exchanges, and Services	4.03%
11	Verizon Communications	6			
12	Microsoft	5			
13	Ford Motor	5			
14	Wal Mart Stores	5			
15	Boston Scientific	5			
16	Allied Chemical	4			
17	General Dynamics	4			
18	United Aircraft	4			
19	Disney – ABC Television	4			
20	Duke Power	4			
21	Monsanto	4			
22	Wellpoint	4			
23	Booz Allen Hamilton	3			
24	Pepsico	3			
25	CBS	3			

Table 3: Revolvers' job positions within the executive branch, the number of such positions and their corresponding frequencies. Panel A lists revolvers' job positions within the legislative branch. Panel B lists revolvers' job positions within the executive branch. Panel C lists revolvers' job positions within the publicly traded firms. The sample period is 1990 to 2012.

Panel A			
Code	Executive branch revolver positions	Number	Frequency
E0	Cabinet-ranking positions	8	3.9%
E1	Presidential appointees at departmental agencies and regulatory agencies	47	23.2%
E2	Other high-level senior officials at departmental agencies and regulatory agencies	24	11.8%
E3	Mid-level senior officials at departmental agencies and regulatory agencies	73	36.0%
E4	Remaining officials at departmental agencies and regulatory agencies	16	7.9%
W1	Executive Office (White House) senior staff	33	16.3%
W2	Remaining staff at Executive Office (White House)	2	1.0%
Total		203	100%

Panel B			
Code	Legislative branch revolver positions	Number	Frequency
L0	Members of Congress	14	4.2%
L1	Senior staff working for Congressional Leadership	14	4.2%
L2	Senior staff working for Congressional Committees	70	20.9%
L3	Senior staff working for Members of Congress	141	42.1%
L4	Other Congressional staff	96	28.7%
Total		335	100%

Panel C			
Code	Corporate revolver positions	Number	Frequency
C0	Executive in charge of public/external affairs or communications	21	4.1%
C1	Executive in charge of government/legislative/regulatory affairs or public policy	259	50.7%
C2	Board members, partners and other senior executives	96	18.8%
C3	Lobbyist (employed by a publicly listed firm)	118	23.1%
C4	Other corporate positions	17	3.3%
Total		511	100%

Table 4: Firms' abnormal returns during revolvers' tenure in the government before joining the firm. Columns 1 to 6 show the performance of portfolios of US firms during the two year-period prior to revolvers joining these companies, while revolvers were still working for the government. They also show the performance of firms not employing revolvers, and the difference between these two groups of firms. Performance is measured using raw returns (column 1), one-, three- and four-factor alphas (columns 2 to 4, respectively), returns in excess of characteristics-based benchmarks (column 5) and four-factor alphas with time varying betas (column 6). Column 7 shows the four-factor alpha performance of the same firms during the placebo period which corresponds to the two-year period immediately prior to revolvers' government service. Returns and alphas are expressed in percent per year. These statistics are computed on monthly returns and annualized by multiplying returns and alphas with 12. The results are shown for value-weighted portfolios where the weights correspond to firms equity market values at the end of the previous year. t-statistics based on standard errors, robust to conditional heteroscedasticity and serial correlation of up to two lags as in Newey and West (1987), are reported in parentheses. Sample period is 1990 to 2012. \*\*\*, \*\*, \* denote statistical significance at 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Raw returns	1-factor alpha	3-factor alpha	4-factor alpha	Excess return over characteristics-based benchmarks	4-factor alpha with time-varying betas	Placebo period excess return over 4-factor alpha
Firms with revolvers	16.17*** (4.37)	6.72*** (3.27)	6.93*** (3.57)	7.77*** (3.78)	5.29*** (3.54)	8.39*** (3.30)	2.05 (1.21)
Firms without revolvers	9.14** (2.58)	-0.65 (-1.50)	-0.75 (-1.58)	-0.19 (-0.39)	-0.21 (-0.75)	-0.19 (-0.37)	0.07 (0.14)
Difference	7.03*** (3.26)	7.38*** (3.41)	7.68*** (3.68)	7.96*** (3.55)	5.50*** (3.41)	8.57*** (3.06)	1.98 (1.06)

Table 5: Firms' abnormal returns during revolvers' tenure in the government before joining the firm: Using revolvers-to-size as measure of interest

The table shows the performance of portfolios of US firms during the two year-period prior to revolvers joining these companies, while revolvers were still working for the government. Firms are split into three groups based on the ratio of number of revolvers to be hired to firm size (market capitalization) at the end of the previous calendar year. Performance is measured using raw returns (column 1), one-, three- and four-factor alphas (columns 2 to 4, respectively), returns in excess of characteristics-based benchmarks (column 5) and four-factor alphas with time varying betas (column 6). Returns and alphas are expressed in percent per year. These statistics are computed on monthly returns and annualized by multiplying returns and alphas with twelve. The results are shown for value-weighted portfolios where the weights correspond to firms equity market values at the end of the previous year. t-statistics based on standard errors, robust to conditional heteroscedasticity and serial correlation of up to two lags as in Newey and West (1987), are reported in parentheses. Sample period is 1990 to 2012. \*\*\*, \*\*, \* denote statistical significance at 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Raw returns	1-factor alpha	3-factor alpha	4-factor alpha	Excess return over characteristics-based benchmarks	4-factor alpha with time-varying betas
Firms with a high revolvers-to-size ratio	16.37*** (3.15)	9.87** (2.35)	9.35** (2.25)	9.30** (2.24)	8.50** (2.29)	8.20** (2.50)
Firms with a medium revolvers-to-size ratio	15.35*** (3.12)	8.62*** (2.61)	7.45** (2.25)	7.09** (2.28)	8.26*** (2.63)	6.12* (1.82)
Firms with a low revolvers-to-size ratio	12.68*** (3.43)	6.66*** (2.84)	7.15*** (3.17)	8.15*** (3.35)	4.79*** (2.84)	9.12*** (3.17)

Table 6: Firms' abnormal returns during revolvers' tenure in the company

The table shows the performance of portfolios of US firms employing revolvers, not employing revolvers, and their difference. Performance is measured using raw returns (column 1), one-, three- and four-factor alphas (columns 2 to 4, respectively), returns in excess of characteristics-based benchmarks (column 5) and four-factor alphas with time varying betas (column 6). Returns and alphas are expressed in percent per year. These statistics are computed on monthly returns and annualized by multiplying returns and alphas with 12. The results are shown for value-weighted portfolios where the weights correspond to firms equity market values at the end of the previous year. t-statistics based on standard errors, robust to conditional heteroscedasticity and serial correlation of up to two lags as in Newey and West (1987), are reported in parentheses. Sample period is 1990 to 2012. \*\*\*, \*\*, \* denote statistical significance at 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Raw returns	1-factor alpha	3-factor alpha	4-factor alpha	Excess return over characteristics-based benchmarks	4-factor alpha with time-varying betas
Firms with revolvers	8.65*** (2.60)	-0.69 (-0.53)	-0.18 (-0.15)	0.10 (0.08)	-0.42 (-0.30)	0.78 (0.64)
Firms without revolvers	9.68*** (2.69)	-0.21 (-0.42)	-0.39 (-0.72)	0.34 (0.64)	0.09 (0.33)	0.17 (0.32)
Difference	-1.03 (-0.67)	-0.48 (-0.33)	0.21 (0.15)	-0.24 (-0.16)	-0.51 (-0.34)	0.61 (0.51)

Table 7: Firms' abnormal returns during revolvers' tenure in the government after leaving the firm

The table shows the performance of portfolios of US firms during the two-year period following revolvers' departure to join a government office. It also shows the performance of firms not employing revolvers, and their difference. Performance is measured using raw returns (column 1), one-, three- and four-factor alphas (columns 2 to 4, respectively), returns in excess of characteristics-based benchmarks (column 5) and four-factor alphas with time varying betas (column 6). Returns and alphas are expressed in percent per year. These statistics are computed on monthly returns and annualized by multiplying returns and alphas with 12. The results are shown for value-weighted portfolios where the weights correspond to firms equity market values at the end of the previous year. *t*-statistics based on standard errors, robust to conditional heteroscedasticity and serial correlation of up to two lags as in Newey and West (1987), are reported in parentheses. Sample period is 1997 to 2012. \*\*\*, \*\*, \* denote statistical significance at 1%, 5% and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Raw returns	1-factor alpha	3-factor alpha	4-factor alpha	Excess return over characteristics-based benchmarks	4-factor alpha with time-varying betas
Firms with revolvers	6.68 (1.50)	-2.55 (-0.72)	-2.72 (-0.74)	-1.05 (-0.29)	-0.09 (-0.04)	0.27 (0.08)
Firms without revolvers	9.51*** (2.70)	-0.28 (-0.68)	-0.37 (-0.83)	0.31 (0.76)	0.09 (0.31)	0.32 (0.73)
Difference	-2.83 (-0.85)	-2.27 (-0.63)	-2.34 (-0.63)	-1.36 (-0.37)	-0.17 (-0.08)	-0.04 (-0.01)

Table 8: Revolvers and government contracts

This table reports the results of pooled time-series cross-sectional OLS and Tobit regressions of the value of government contracts (in millions of US dollars) allocated to publicly traded firms on an indicator variable (namely, revolver linkage variable) that equals one if the firm hired a revolver from a contract-allocating government agency within the subsequent two years, and zero otherwise, and a set of controls. Controls include the company's market capitalization, the ratio of the book value and market value of equity, the Herfindahl index, which is based on total sales in the 2-digit SIC industry of the company, the ratio of capital expenditure to sales, the ratio of cost of goods sold to sales, and the return on assets. Regressions also include industry and year fixed effects. Each column represents a separate regression. t-statistics based on standard errors clustered at the firm level are included in parenthesis. Observations are at the firm-year level. Sample period is 1990 to 2012. The sample includes all firms that hired at least one revolver and received at least one government contract during the sample period. \*\*\*, \*\*, \* denote statistical significance at 1%, 5% and 10% levels, respectively.

	Government contracts (million US\$) allocated to firm			
	OLS		TOBIT	
	(1)	(2)	(3)	(4)
Firm has a revolver link to the government	227.47** (2.34)	234.89** (2.11)	467.52*** (3.38)	335.88** (2.56)
Constant	1.47 (0.49)	-282.61*** (-3.64)	-587.80** (-2.07)	-2,189.67*** (-3.02)
Firm control variables [ROA, CAPEX2SALES, COGS2SALES, BM, SIZE, HHI]	No	Yes	No	Yes
Industry and year fixed effects	Yes	Yes	Yes	Yes
Observations	17,768	15,558	17,768	15,558
Pseudo R-squared	0.0374	0.0492	0.0176	0.0238

Table 9: Linking abnormal returns and government contracts

This table reports the results of Fama and MacBeth cross-sectional regressions of monthly excess stock returns on different variables of interest and lagged firm characteristics. Beta is the firm-level current market beta computed from daily returns within the month, "size" is the natural log of firm's market capitalization, book-to-market is the natural log of firm book-to-market ratio, and momentum is firm momentum (prior cumulated 12 months returns). The table reports the average loadings for each cross-sectional regression and the corresponding Newey and West corrected t-value (in parenthesis). Sample period is 1997 to 2012. \*\*\*, \*\*, \*, \* denote statistical significance at 1%, 5%, 10% and 10% levels, respectively.

	Firm excess returns (monthly)			
	(1)	(2)	(3)	(4)
Firm has a revolver link	9.77*** (3.78)			
Government contracts allocated to the firm		19.88 (1.49)		
IV predicted value of government contracts allocated to the firm			0.53** (2.25)	0.31 (0.68)
Beta	1.98 (1.39)	1.98 (1.39)	1.98 (1.39)	1.99 (1.40)
Size	-1.23 (-1.30)	-1.17 (-1.25)	-1.22 (-1.29)	-1.22 (-1.29)
Book-to-market	4.18** (2.51)	4.18** (2.51)	4.18** (2.50)	4.17** (2.50)
Momentum	3.61 (1.03)	3.59 (1.03)	3.60 (1.04)	3.56 (1.02)
Constant	12.89 (1.40)	12.64 (1.38)	12.68 (1.38)	12.74 (1.39)
Observations	873,343	873,343	873,343	873,343



Table 10: Revolvers and earnings forecasts

Panel A of this table reports the results of regressions of earnings surprises on a dummy variable for whether a revolver worked in the government in that year and if this person was later employed by the firm within one year. Panel B reports the results of a placebo test where the firm-revolver linkage is falsely assumed to exist for up to a one-year period immediately before the revolver's government career. *Consensus surprise* is the actual EPS of the firm at the end of the fiscal year minus the median Institutional Brokers Estimate System (I/B/E/S) analyst forecast, deflated by the stock price. The I/B/E/S consensus forecast is taken 8 months prior to the end of the forecast period. *Guidance surprise* is the actual EPS of the firm for the fiscal year-end minus the I/B/E/S Guidance forecast, deflated by the stock price. I/B/E/S Guidance forecast is taken to be the one provided on the closest date to the I/B/E/S consensus forecast used in the same year's consensus surprise. Controls used in the regressions (log book-to-market and log book value of assets) are calculated at the previous year-end. All coefficients are multiplied by 1000. All regressions include industry and year fixed effects. t-statistics are in parentheses. Sample period is 1997 to 2012. \*\*\*, \*\*, \* denote statistical significance at 1%, 5% and 10% levels, respectively.

	Panel A			Panel B: Placebo Period		
	(1)	(2)	(3)	(4)	(5)	(6)
	Consensus Surprise (%)	Guidance Surprise (%)	Difference of Expectations (%)	Consensus Surprise (%)	Guidance Surprise (%)	Difference of Expectations (%)
Firm has a revolver link	0.33** (2.17)	0.19* (1.76)	0.15** (2.62)	-0.16 (-0.53)	-0.08 (-0.33)	-0.08 (-0.92)
Control variables [log(Size), log(Book-to-Market), Constant]	Yes	Yes	Yes	Yes	Yes	Yes
Industry and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	597	597	597	551	551	551
R-squared	0.090	0.256	0.438	0.091	0.260	0.438

Table 11: Falsification tests using earnings forecasts

Variables used in this table are same as the ones used in Table 10 with the exception of the independent variable on firm-revolver linkage. In Panel A, the firm-revolver linkage variable corresponds to a dummy variable that is coded as one if only *clean sheet revolvers* work in the government in that year and if they are later employed by the firm within one year (and zero otherwise). Clean sheet revolvers are the ones who were employed in government agencies that did not allocate any contracts during the last year of their government tenure to the firms that subsequently hired them. In Panel B, the firm-revolver linkage variable corresponds to a dummy variable that is coded as one if the firm received a government contract in that year and if it did not employ any revolvers afterwards (and zero otherwise). All coefficients are multiplied by 1000. All regressions include industry and year fixed effects. t-statistics are in parentheses. Sample period is 1997 to 2012. \*\*\*, \*\*, \* denote statistical significance at 1%, 5% and 10% levels, respectively.

	Panel A: Clean Sheet Revolvers (Placebo)			Panel B: Firms that received government contracts but did not hire any revolvers (Placebo)		
	(1)	(2)	(3)	(4)	(5)	(6)
	Consensus Surprise (%)	Guidance Surprise (%)	Difference of Expectations (%)	Consensus Surprise (%)	Guidance Surprise (%)	Difference of Expectations (%)
Firm has a revolver link	0.02 (0.17)	-0.04 (-0.31)	0.06 (1.18)	-0.02 (-0.13)	-0.05 (-0.31)	0.03 (1.10)
Control variables [log(Size), log(Book-to-Market), Constant]	Yes	Yes	Yes	Yes	Yes	Yes
Industry and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	354	354	354	1,621	1,621	1,621
R-squared	0.068	0.069	0.133	0.209	0.143	0.104

Table 12: Abnormal returns and executive orders

This table reports the results of regressions of monthly abnormal returns on a dummy variable for whether that month is within the Clinton and Obama presidential terms. Abnormal returns are measured as realized returns in excess of conditional expected returns with time varying betas for Fama-French-Carhart risk factors. Regressions are carried out for two groups of firm-month observations: *Treatment firm-months* are the observations in which the firm hired a presidential or vice-presidential appointee from the government within the subsequent two years following the month. *Control firm-months* are the observations in which the firm hired only those who are not presidential or vice-presidential appointees from the government within the subsequent two years following the month. Newey-West corrected t-statistics are reported in parentheses. Sample period is 1997 to 2012. \*\*\*, \*\*, \* denote statistical significance at 1%, 5% and 10% levels, respectively.

	Abnormal returns corresponding to treatment firm-month observations	Abnormal returns corresponding to control firm-month observations	Difference
Clinton & Obama presidential terms	-10.39* (-1.75)	7.07** (2.10)	-17.46** (-2.41)
Constant	10.36** (2.45)	2.82 (1.03)	7.54 (1.19)
Observations	275	275	275
R-squared	0.010	0.010	0.019

Table 13: Government contracts and executive orders

This table reports the results of pooled time-series cross-sectional Tobit regressions of the value of government contracts (in millions of US dollars) allocated to publicly traded firms on two indicator variables (namely, the appointee variable and the non-appointee variable) and a set of additional instruments and controls. The first indicator variable (appointee variable) equals one if the firm hired a presidential or vice-presidential appointee from the government within the subsequent two years, and zero otherwise. The second indicator variable (non-appointee variable) equals one if the firm hired *only* those who are not presidential or vice-presidential appointees from the government within the subsequent two years, and zero otherwise. Additional instruments include an indicator variable that equals one for Clinton and Obama presidency years and zero otherwise, interaction terms between this variable and the two indicator variables mentioned above. Controls include the company's market capitalization, the ratio of the book value and market value of equity, the Herfindahl index, which is based on total sales in the 2-digit SIC industry of the company, the ratio of capital expenditure to sales, the ratio of cost of goods sold to sales, and the return on assets. Regressions also include industry and year fixed effects. Each column represents a separate regression. t-statistics based on standard errors clustered at the firm level are included in parenthesis. Observations are at the firm-year level. Sample period is 1990 to 2012. The sample includes all firms that hired at least one revolver and received at least one government contract during the sample period. \*\*\*, \*\*, \* denote statistical significance at 1%, 5% and 10% levels, respectively.

	Government contracts (million US\$) allocated to firm			
	(1)	(2)	(3)	(4)
Firm has a revolver link to a presidential or vice-presidential appointee	814.40*** (2.63)	663.60** (2.15)	1,086.16** (2.54)	955.85** (2.15)
Firm has revolver link(s) to only those who are not presidential or vice-presidential appointees	381.50*** (3.69)	243.86*** (2.71)	438.69*** (3.55)	295.56*** (2.65)
Clinton and Obama presidency years			587.41*** (3.34)	527.49*** (2.93)
Firm has a revolver link to a presidential or vice-presidential appointee during Clinton or Obama presidency years			-814.20** (-2.07)	-845.75** (-1.96)
Firm has revolver link(s) to only those who are not presidential or vice-presidential appointees during Clinton or Obama presidency years			-121.55 (-0.96)	-109.80 (-0.71)
Constant	-590.38** (-2.08)	-2,184.35*** (-3.01)	-580.47** (-2.02)	-2,161.78*** (-2.98)
Firm control variables [ROA, CAPEX2SALES, COGS2SALES, BM, SIZE, HHI]	No	Yes	No	Yes
Industry and year fixed effects	Yes	Yes	Yes	Yes
Observations	17,768	15,558	17,768	15,558
Pseudo R-squared	0.0178	0.0241	0.0181	0.0244

Table 14: Earnings forecasts and executive orders

This table reports the results of regressions of earnings surprises on two indicator variables (namely, the appointee variable and the non-appointee variable) and a set of additional instruments and controls. The first indicator variable (appointee variable) equals one if the firm hired a presidential or vice-presidential appointee without a clean sheet record from the government within the subsequent two years, and zero otherwise. The second indicator variable (non-appointee variable) equals one if the firm hired *only* those who are not presidential or vice-presidential appointees without clean sheet records from the government within the subsequent two years, and zero otherwise. Revolvers without clean sheet records are those who were employed in government agencies which allocated contracts during the last two years of their government tenure to the firms that subsequently hired them. Additional instruments include an indicator variable that equals one for Clinton and Obama presidency years and zero otherwise, interaction terms between this variable and the two indicator variables mentioned above. *Consensus surprise* and *Guidance surprise* are as they are defined in Table 10. Also, firm control variables used in this table are same as the ones used in Table 10. All coefficients are multiplied by 1000. t-statistics are in parentheses. Sample period is 2001 to 2012. \*\*\*, \*\*, \* denote statistical significance at 1%, 5% and 10% levels, respectively.

	Revolvers without clean sheets records		
	(1)	(2)	(3)
	Consensus Surprise (%)	Guidance Surprise (%)	Difference of Expectations (%)
Firm has a revolver link to a presidential or vice-presidential appointee	1.09** (2.34)	0.68 (1.61)	0.41*** (3.10)
Firm has revolver link(s) to only those who are not presidential or vice-presidential appointees	0.19 (0.74)	0.05 (0.25)	0.14* (1.86)
Firm has a revolver link to a presidential or vice-presidential appointee during Clinton or Obama presidency years	-2.25*** (-4.10)	-1.60*** (-3.19)	-0.65*** (-5.19)
Firm has revolver link(s) to only those who are not presidential or vice-presidential appointees during Clinton or Obama presidency years	0.35 (0.55)	0.30 (0.51)	0.04 (0.35)
Control variables	Yes	Yes	Yes
[log(Size), log(Book-to-Market), Constant]			
Year fixed effects	Yes	Yes	Yes
Observations	362	362	362
R-squared	0.083	0.065	0.063

Figure 1: Abnormal returns in event time

This figure shows the performance of portfolios of US firms formed in event time relative to the year of hiring of a new revolver. The solid lines shows the value-weighted difference in annualized abnormal returns (four factor alphas) between a portfolio of firms with revolvers in the government and a portfolio of firms with no revolvers in the government. The x-axis denotes the time at which these portfolios are formed and the holding period is always one year. Returns at -1Y, for example, denote the abnormal firm performance during revolvers' last year in government before joining the firm. Returns at -2Y denote the abnormal firm performance two years before the revolver was hired. The dotted lines show 90% confidence interval. The sample period is 1990 to 2012.

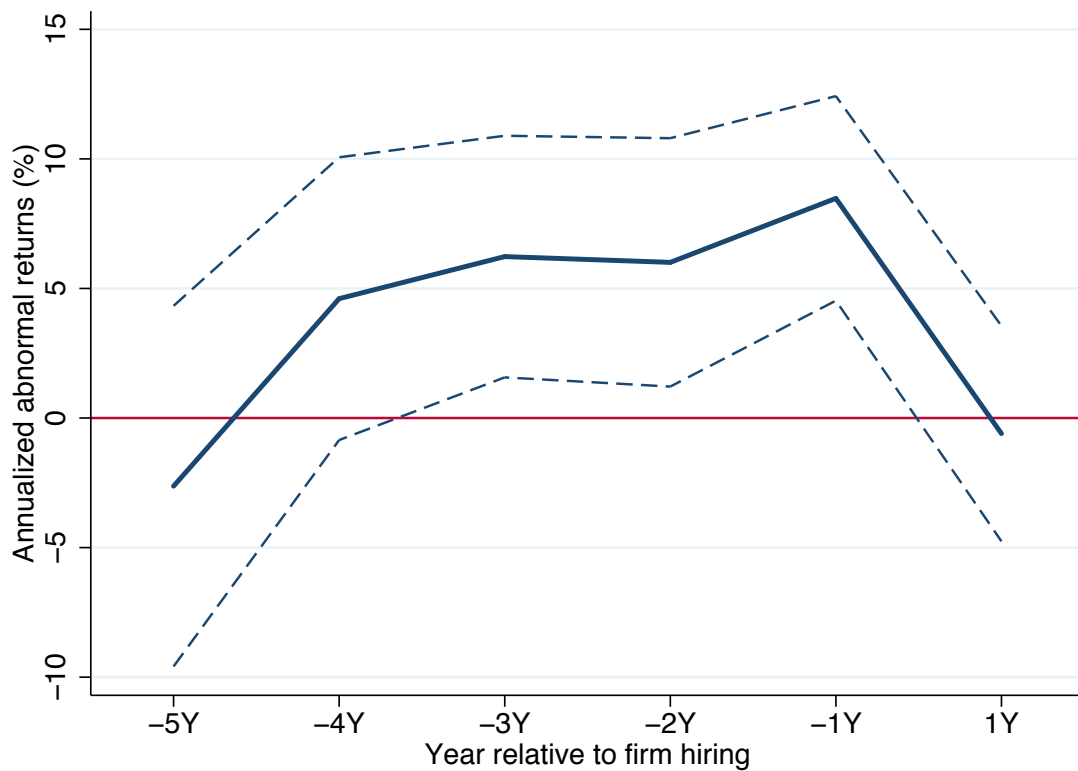


Figure 2: Government contract allocations around the hiring date of revolvers

This figure shows the Tobit regression coefficients associated with a set of indicator variables defined according to whether or not the firm hired a revolver from the contract granting agency in the neighbourhood of the contract granting date. The coefficient is obtained from a regression of the dollar value of government contracts (in millions of US dollars) allocated to publicly traded firms by different government agencies on the above mentioned set of indicator variables (one per year) and the same set of controls used in Table 8. The y-axis shows the value of the coefficients and the x-axis shows the year of the indicator variable relative to the hiring date of the revolver by the firm. The dotted lines show 90% confidence interval. The sample period is 1990 to 2012.

