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

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March 2015

ABSTRACT

This paper looks 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 examines how it affects long-run stock returns. The equally-weighted portfolio of firms where current public officials become future employees outperforms the equally-weighted portfolio of remaining firms by a highly statistically significant 7.43% per year in terms of four-factor alpha. Our results are robust to different weighting methodologies and risk adjustments, and we present results against potential reverse causality arguments. To give an economic explanation to this outperformance, we show that firms receive more government contracts from a government agency when a future firm employee is holding a post at that agency. Financial gains from these contracts are economically large, and these gains are significantly reduced during periods in which presidential executive orders restrict revolving door movements. Our results are consistent with the notion that some public officials could be favoring certain companies while in office with a view to gaining future corporate employment.

JEL classification: D73, G12, G18, L51.
Keywords: Corporate political connections, government contracts, regulatory capture, revolving door.

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." Mr. Obama also said 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.[1]

Candidate Obama's campaign remarks reflected the public unease with the movement of individuals between government positions and jobs in the private sector.[2] A non-exhaustive list of potential conflicts of interest raising public concern includes:[3] (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.

Therefore many on the street hold the view that revolving door movements are potentially corrupt activities and favor restrictions on such movements.[4] However, there are others who argue that unduly restrictive provisions on revolving door movements may deter qualified and competent people from joining government service.[5] Unfortunately,

[1] Excerpts taken from Zeleny (2007).

[2] Several revolving door movements aroused public ire in the U.S. The poster child example for the conflicts of interest created by revolving door movements is 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. See the Revolving Door Working Group (2005) report.

[3] See David-Barrett (2011).

[4] 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 Barrington, Macaulay, and Scott (2010).

[5] 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).

there is little systematic evidence on motives behind revolving door movements and their economic impact. Further 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 revolving door movements and their impact on corporate financial performance. To that end, we obtain data on revolving door movements from the Center for Responsive Politics' (CRP) Revolving Door Database. This is the most comprehensive source to date on movements between federal government positions and private sector jobs in the U.S.

With data in hand, we first investigate whether revolvers add shareholder value to their corporate employers during their corporate tenure.[6] In our analysis we only consider revolvers who join a firm within a year after their government employment. The difference between the four-factor alpha earned by the equally-weighted portfolio of firms which employ revolvers and that earned by the equally-weighted portfolio of firms which don't is statistically significant ($t = 1.94$).[7] The alpha of the long-short portfolio becomes economically and statistically insignificant once value-weighted portfolios are employed. So a robust return outperformance is not observed during revolvers' corporate tenure: revolvers may add shareholder value to some small firms, if at all, but they don't add shareholder value to large firms.

Second, we analyze whether revolvers add shareholder value to their *future corporate employers* during their government tenure.[8] We focus on the return performance of firms during the three years right before revolvers join them and while they still hold public office.[9] The four-factor alpha difference between the equally-weighted portfolio of firms where current public officials become future employees and the equally-weighted portfolio of the remaining firms is a highly statistically significant 7.43% per year ($t = 4.36$). The

[6] Revolvers are individuals moving from government positions to private sector jobs or vice versa.

[7] The four-factor alpha is the abnormal return with respect to the Fama-French-Carhart four-factor model (see Carhart, 1997).

[8] We define *future corporate employers* as firms that hire revolvers within a year after they leave their government positions.

[9] If revolvers are in public office continuously for less than three years before corporate employment, then we restrict the stock-return analysis to the period of revolvers' continuous government tenure prior to corporate employment.

result is qualitatively similar for value-weighted portfolios. Moreover, controlling for firm size, a firm that hires a larger number of revolvers enjoys higher abnormal returns during its revolvers' government tenure. Also, we find abnormal return performance of a firm at its strongest immediately before the hiring of the revolver: the outperformance diminishes and eventually vanishes as we move further away from the hiring date. The latter finding is consistent with the view that there is a quid pro quo relationship between some public officials and their future corporate employers – we would expect the likelihood of such a relationship to increase as we move closer to the date when the public official switches to the private sector.

Third, we investigate whether revolvers add shareholder value to their *former corporate employers* during their government tenure. We focus on the return performance of firms during the three-year period following revolvers' departure from these firms to join public office. We only consider revolvers who join government within a year after their corporate tenure. The difference between the four-factor alpha earned by the equally-weighted portfolio of firms which employ revolvers and those earned by the equally-weighted portfolio of firms which don't employ any revolvers is statistically insignificant. The result is same if value-weighted portfolios are employed in the long-short portfolio strategy.

What emerges from our analysis is that firms that employ revolvers enjoy significant abnormal returns during their revolvers' government tenure. However, this result is open to an alternative causal interpretation. Rather than public officials adding shareholder value to their future corporate employers, it may be that firms that generate superior returns in the recent past employ public officials as they can afford to hire more people. To assess the validity of this alternative interpretation, we look into the years that the revolvers are hired, and we match the firms that employ revolvers to a control group of firms that feature similar characteristics in number of employees, change in the number of employees, market capitalization, book-to-market ratio and industry group. The firms that employ revolvers outperform the control group of matched firms: the four-factor alpha difference between equally-weighted treated and controlled portfolios is a highly statistically significant 7.69% per year ($t = 3.00$). This is inconsistent with the alternative view that firms that exhibit superior performance in recent past simply hire more people (former public officials being among them) and that revolvers add no shareholder value.

The results, mentioned above, are robust to various matching techniques and controlling for different portfolio-weighting methodologies and different risk adjustments.[10]

In order to shed light on the return outperformance enjoyed by revolver-hiring firms during revolvers' government tenure, we next investigate the relationship between revolving door movements and government contract allocations. To that end, we obtain data on government contracts from the Bloomberg Government (BGOV) database and merge it to the revolver data from the CRP database. We find that a firm's having a revolver linkage to a government agency has a large and statistically significant positive effect on the value of government contracts allocated to the firm by that agency. Further results lend more support to the "quid pro quo" explanation of revolving door movements. First, we find that government contract allocations attributable to revolver linkages are significantly lower in value during Clinton and Obama presidency years in which presidential executive orders restrict revolving door movements. If no public official had a quid pro quo relationship to their future corporate employers, then the introduction of regulatory restrictions on revolving door movements would have no effect on the allocation pattern of government contracts. The fact that regulatory restrictions have a significant negative effect on contract allocations suggests that some public officials and firms are likely to be engaged in a quid pro quo relationship. Moreover, we observe two different patterns in government contract allocations before and after the hiring of revolvers by firms: Before hiring, the value of contracts awarded to future corporate employers increases as the time gets closer to revolvers' hiring dates. This is also consistent with the "quid pro quo" hypothesis: concerns about future employment would become more significant for public officials the nearer is their hiring date by a private sector firm. On the other hand, after hiring, the value of contracts awarded to current corporate employers decreases over time. Again, this is not surprising as revolvers' influence over their past government employers are expected to wane.

Taken together, our results lend empirical support to the existence of a quid pro quo relationship between public officials and corporations: some public officials may well be using their power while in office to favor potential future corporate employers. "The aim

[10]Different risk adjustments include computation of abnormal returns with respect to CAPM (see Sharpe, 1964 and Jensen, 1968), the Fama-French three-factor model (see Fama and French, 1993), and the characteristics-based benchmarks (see Daniel, Grinblatt, Titman, and Wermers, 1997).

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 displays that in the context of revolving door movements there is likely a need to reset the institutional incentives so that public officials act in the public interest. On the practical side, our results suggest that the presidential executive orders issued by Clinton and Obama, which restricted revolving door movements, were effective in curbing some of the conflicts of interest.

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. 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 provides data description. Section 4 presents our findings on the relationship between revolving door movements and long-term stock returns. Section 5 presents our findings on the relationship between revolving door movements and government contract allocations. Section 6 concludes.

2 Related literature

Our findings contribute to the extant literature on economic and regulatory implications of revolving door movements. Interest in the impact of revolving door movements emerged first in political science and the context has been primarily regulating utilities, broadcasters, and financial industry (see, e.g., Gormley, 1979; Cohen, 1986; Spiller, 1990; and

Grace and Phillips, 2008).[11] Our paper is, in particular, closely related to a couple of recent studies: 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. In a more recent study, 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 outcome variables of interest are stock returns as well as government contract allocations.

Another related literature is the one 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. Goldman, Rocholl, and So (2009, 2013), Jayachandran (2006) and Tahoun (2014) find that political connections have a positive impact on firm value. In particular, Goldman, Rocholl, and So (2009) investigate the announcement effect of the nominations of politically connected individuals to corporate boards in the U.S. and show that nomination announcements are followed by positive abnormal stock returns. Their 2013 paper, on the other hand, finds

[11] See Dal Bo (2006) for a detailed literature review of research associated with regulatory capture and revolving door movements.

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, and Khurana (2006) 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 complements evidence from the U.S. by establishing a significant positive relationship between political connections and firm value not only in turbulent economic times but also during regular times. Also, compared to Goldman, Rocholl, and So (2013) and Tahoun (2014), we analyze the effect of government connections on contract allocations at a finer level as we differentiate between government contracts awarded by different government agencies.

Finally, 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).

3 Data description

Our data on revolvers 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`. The primary source for the Revolving Door Database is a set of 7,745 people with details and biographical entries included in the comprehensive online directory of lobbyists published by Columbia Books, Inc., at `www.lobbyists.info`, as of September 2006. CRP uses proprietary and publicly available sources to continuously update this original data set with new information on existing people, and with information about new people who meet the criteria for inclusion in the database.

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 employment type (i.e., whether the employer is a government agency, a congressional committee, a member of 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.[12] Using this data, we identify where and in which positions revolvers worked in a given year. We concentrate on the revolvers that found corporate jobs right after their government tenure and the revolvers that found government jobs right after their corporate tenure. In both cases, we consider only the revolvers that found 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. In the database, the number of publicly listed

[12]This number corresponds to the latest update of the database as of December 2013.

firms that employ revolvers of the kind described above is limited before 1990: the time-series average of the number of these firms is 4 before 1990, with only one publicly listed firm hiring revolvers in some of those years, whereas the time-series average after 1990 is 45. Therefore we restrict our analysis to the period between 1990 and 2012; that is, our sample includes only revolvers who were employed by a publicly listed firm at some point between 1990 and 2012.

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 main analysis, we exclude financial firms (we report results including financial firms in the Appendix), 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. The table focuses specifically on revolvers who move from government jobs to publicly listed firm jobs within a year after their government tenure.[13] 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 52 public firms employing 67 revolvers in total and 26 of those revolvers were newly hired in that year. During the period of our study, an average of 62 revolvers worked in publicly traded firm each year, and 45 publicly listed firms employed at least one revolver. Panel B lists mean, median, mean and max 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.

[13]Our analysis mostly involves such revolvers, but we also later present a result on revolvers moving from public firm jobs to government jobs within a year after their corporate tenure.

– Insert Table 1 about here –

Table 2 considers revolvers of the kind described for Table 1 and lists the top 20 hirers of such revolvers as well as the industries these hiring firms belong to. The industries are defined using 2-digit SIC codes. Top hirers of revolvers (outside the financial industry) are Lockheed Martin Corp, Raytheon and Boeing – they employ 36 revolvers in total during the sample period. The most common revolver hiring industries (outside the financial industry) are Electric, Gas, and Sanitary Services, and Communications; each hiring approximately 10% of all revolvers in our sample. The financial industry, not included in our main dataset, is a substantial employer of revolvers, giving jobs to twice as many revolvers as any other industry..[14]

– Insert Table 2 about here –

In our analysis, we also use data on government contracts allocated to firms. We obtain this data from the Bloomberg Government (BGOV) database. BGOV provides data on government contracts that firms receive along with a detailed 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 the codes for the contract-allocating government agency (a high code and a low code, where the high code provides a broader categorization of the government agency),[15] Bloomberg ticker of the firm that received the contract, the total dollar amount of the contract, and the date the contract was allocated. We use the high codes to identify government agencies. 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.

[14] We exclude financial firms from our main dataset because the risk adjustment methods we use later in the paper, Fama-French factors and the characteristics-based benchmarks method of Daniel et al. (1997), exclude financial firms in the formation of factors and benchmarks.

[15] For instance, Food and Drug Administration has 7524 as its low code and 52 as its high code. Its low code is unique whereas it shares the same high code with all the other government agencies under the Department of Health and Human Services.

We match the BGOV data with the revolver data using PERMNOs. We manually match the high codes of the government agencies in the BGOV dataset to the employers of revolvers during their government tenure. If the employer is a government agency, such as an agency under Department of Commerce or the Department itself, a high code is readily available for the agency and the match is made. However, if the employer is a congressional committee, then we make a judgment about which contract-allocating government agency would be most influenced by the decisions of the committee. For instance, if the employer is the US Senate Armed Services Committee, then we match it to the high code of Department of Defense. If the revolver is employed by a Representative or a Senator, then we do not make a match[16] 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 from each government agency. 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. Looking into the government agencies that the revolvers have worked for and the contracts firms receive from those government agencies enables us to investigate the impact of revolvers on firms' government contract allocations at a very fine level.

During our sample period, the top three contract allocating agencies (by dollar value of contracts awarded) are the Department of Defense, the General Services Administration and the Department of Transportation. In this 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 either did not receive any government contracts, or received contracts from agencies we could not identify. As shown in Table 3, firms that employed revolvers obtained higher average contracts (by dollar value) during our sample period than firms that did not.

– Insert Table 3 about here –

[16]This is unless the Representative or the Senator is Chair Person or Ranking Member of a congressional commitee in which case we follow the matching procedure described for congressional committees.

4 Revolving door movements and the cross-section of stock returns

We assess whether revolvers add value to firms by estimating the abnormal returns obtained by firms that hire revolvers. For this purpose we create equal- and value-weighted portfolios of firms that employ revolvers and firms that do not.[17] 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 abnormal returns by running following factor-model regressions with the monthly returns of these portfolios:

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

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 α_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 (1993) three factor model 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 (1993) 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. 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 addition to these measures 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-market ratio, and momentum.[18]

[17] 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.

[18] The benchmarks are available from: `www.smith.umd.edu/faculty/rwermers/ftpsite/Dgtw/`

4.1 Revolvers' tenure in the firm

We first explore whether revolvers add value to firms during their tenure in the company. 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.[19] 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 revolvers 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.

– Insert Table 4 about here –

Table 4 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. In this table returns and alphas are expressed in percent per year. In all cases we report, in parenthesis, t-statistics based on standard errors, robust to conditional heteroscedasticity and serial correlation of up to two lags as in Newey and West (1987). Results in this table indicate that in the 23-year period of our study firms that employed revolvers did not reliably deliver higher returns than firms that did not employ revolvers. Although the equally-weighted portfolio returns of firms employing revolvers averaged 13.99% per year compared to average returns of 12.02% per year for all other firms in our sample, the annualized difference between the two, 1.97%, is not statistically significant. This difference becomes negative, -1.17%, when we look at

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[19] 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.

returns on a value-weighted basis. Similar results obtain when we exclude the first year of employment from the portfolio.

Since firms hiring revolvers tend to be concentrated on certain industries, raw returns, which fail to account for the risk characteristics of firms, may not be very appropriate for comparisons. Risk adjusting returns using one-, three-, or four-factor alphas, however, does not significantly change the picture. Using four-factor alphas, for instance, the difference in equally-weighted returns between these two portfolios is a barely statistically significant 3.10% per year. Similar results obtain if we risk adjust returns using the characteristics-based benchmarks of Daniel, Grinblatt, Titman, and Wermers (1997). On a value-weighted basis the performance of firms hiring revolvers is indistinguishable from the performance of firms not hiring revolvers. Using four-factor alphas, for instance, the difference between these two portfolios is a non-statistically significant -0.26% per year (0.10% if we risk-adjust returns using characteristics-based benchmarks).

Taken together these results suggest that revolvers do not consistently add value to the firms that employ them, during their tenure in these firms. They may benefit some small firms, if at all, but not the largest firms in the economy (as indicated by the difference between equal-weighted and value-weighted results). 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.

4.2 Revolvers' tenure in the government

Although we do not find conclusive evidence of revolvers adding value to firms during their years of firm employment, they could still be helping them before or after joining (and while working for the government). In this section we investigate this possibility by building portfolios of firms in the years immediately before revolvers joined them and immediately after revolvers left them, and portfolios of firms that have not or will not employ revolvers. As before, we assess whether revolvers benefit the firms by estimating one-, three- and four-factor alphas and excess returns over characteristic-based benchmarks.

We first explore firm performance in the years immediately before the hiring of re-

volvers. Table 5 displays excess returns and alphas of portfolios of US firms up to three years before revolvers joined them, and while revolvers were still working for the government. It also shows the performance of firms not employing (or about to employ) revolvers, and their difference expressed in percent per year. In this analysis we only consider revolvers that join a firm within one year at most after the end of their duty in the government. The equally-weighted portfolio of firms that hired revolvers delivered average returns of 18.95% per year in the three years prior to the hiring. These returns compare favourably to average returns of 11.98% per year for all other firms in our sample during the same 1990-2012 period. The annualized difference between the two, 6.97%, is statistically significant, and it remains so once risk-adjusted using the one-, three-, or four-factor asset pricing models. For instance, using four-factor alphas the difference between these two portfolios is a highly statistically significant 7.43% 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 5.96% per year, statistically significant at the 1% level. When we perform the same analysis on a value-weighted basis the abnormal returns we observe tend to be smaller, yet the difference between firms hiring revolvers and firms not hiring them remains highly statistically significant. For instance, using four-factor alphas the difference between the value-weighted portfolio returns of firms that hire revolvers and firms that do not is a highly statistically significant 6.10% per year in the three year period prior to the hiring (4.43% per year if we risk-adjust returns using characteristics-based benchmarks).[20]

– Insert Table 5 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 6, 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 almost twice as large for firms in the top third of revolvers-to-size ratio compared to firms in the

[20]Including financial firms in the sample does not significantly modify the results tabulated in Table 5 (see Appendix).

bottom third of this ratio. For example, the equally-weighted four-factor alpha of firms in the top tercile of revolver intensity is 12.26% per year in the three year period prior to the hiring whereas for firms in the bottom tercile it is only 6.49%.

– Insert Table 6 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 results of revolvers helping their future employers before making their move. Four or five years before the hiring the performance of the revolver-linked firms is indistinguishable from that of other firms in our sample, see Figure 1. Consistent with this finding, in untabulated results we also find that returns are smaller for revolvers who leave the government a number of years prior to their hiring by the firm, 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 –

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. In Table 7 we show the performance of portfolios of US firms during the three-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. Although, on an equal-weighted basis, firms whose employees leave to join public office seem to do marginally better than other firms during this period, the annualized difference in returns between the two, 2.72%, is not statistically significant, and it is even negative, -0.65%, on a value-weighted basis. The risk-adjusted difference in returns between these two groups of firms is not significant either. For instance, using four-factor alphas the difference in equally-weighted returns between the two portfolios is a statistically insignificant 4.02% per year (1.30% per year when evaluating portfolios on a value-weighted basis), and the difference virtually disappears if we risk-adjust returns using characteristics-based benchmarks.

– Insert Table 7 about here –

Taken together these results are consistent with the hypothesis that revolvers could actually be helping their future employers while working for the government. Employment at the firm would then be, at least in part, compensation for the benefits they have provided while in public office. A natural concern with that conclusion is that the 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 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, and other employees, will be more likely to end up working for them than for a less successful competitor. That could explain why firms that hire revolvers, and people in general, exhibit positive abnormal returns in the pre-hiring period.

We tackle this problem by matching the firms that employ revolvers to a control group of firms selected to match the numbers of employees and the change in the number of employees at the moment of hiring, as well as other characteristics (book/market and size quintiles and industry group) of the firms employing revolvers. If firms that hire revolvers do better than this control group, it would indicate that the returns obtained by firms employing revolvers are even higher than the returns obtained by firms that are growing their workforce in similar quantities (and therefore by firms that are a priori equally likely to hire revolvers as the firms that actually hire them, under the alternative hypothesis that revolvers are identical to any other employees), thus refuting the alternative story.

– Insert Table 8 about here –

We match firms using the familiar propensity score matching estimator (see, e.g., Heckman, Ichimura, and Todd, 1997 and 1998). Specifically, the matching method finds control firms, at most ten firms per each treated firm, that are the closest match in terms of the propensity score for each firm hiring a revolver. We match firms at the point of hiring, and, to avoid bad matches, we require the control's propensity score to be within a 1% radius (caliper) of the propensity score of the treated firm. We also drop firms with significant (10% or more) seasonal or part time employees from the treatment and control

groups.[21] With these control firms we build portfolios whose returns and one-, three- and four-factor alphas we compare to the returns and alphas obtained by the portfolio of firms that hire revolvers currently working for the government. To compute the control group portfolio returns, we do as follows. First, for each treated firm we value-weight the returns of each of the chosen control firms using the December (prior to the match) market capitalization of these firms. For instance, for a revolver-hiring firm matched to five control firms, we create a control return index from the value-weighted returns of these five control firms using these firms' market capitalization for weighting. Then, we use these individual treated firms' control returns to build equal- and value-weighted control group portfolio returns. In this second stage we use the revolver-hiring firms' weights when computing value-weighted control group portfolio returns.

As we did in Table 5, we concentrate in the three-year period prior to revolvers joining the firm. We report the results of this exercise in Table 8. Figures in this table reflect the difference in returns or alphas between the treated and the control portfolios. The first panel of this table shows results for firms matched using the number of employees and the change in the number of employees only and we additionally control for size, book to market and momentum effects as part of our risk-adjustment models. In this case the difference in returns between the treated and control portfolios is 7.38% (value-weighted) whereas the difference in four-factor alphas between the two is a statistically significant 7.24%. Similar results obtain if we also match to firms in the same industry defined using two digit SIC codes (second panel), or if we match according to size and book-to-market quintiles (third panel).[22]

These results suggest that causality is unlikely to run in the direction of firms that have done well in the recent past simply hiring more revolvers than other firms as a result of their unexpected success (which is completely independent of government connections of revolvers).

[21] These requirements mean that we are unable to match a few firms in our sample (particularly some large firms). This is unlikely to have a major impact in our results, however, as when we reproduce the analysis of Table 5 for the subsample of firms for which we are able to find close matches the results we obtain are reasonably close to those reported in Table 5 for the full sample.

[22] Given the nature of our test it is not possible to match firms based on momentum (prior performance). If we did it, we would be looking for firms that did as well or as badly as the firms that hire revolvers in the 12 month period previous to the hiring, basically negating what we want to measure (albeit for only 12 months).

5 Revolving door movements and government contracts

Our results from Section 4 lend support to the hypothesis that some revolvers could be using their positions of influence in public office for private benefit. This section further investigates this hypothesis. In particular, we examine if a firm that employs a revolver is awarded more valuable contracts by a government agency when the revolver works in that agency or has influence over it due to his public office. Our empirical strategy follows closely that of Kuziemko and Werker (2006).[23]

We start by measuring how firms' receipts of government contracts from different government agencies change as a function of firm-agency linkages through revolvers over time. To that end, our estimating equation is:

$$Contract_{i,j,t} = \alpha + \beta_1 * Revolver_{i,j,t} + \gamma * X_{i,t} + I_i + A_j + Y_t + \epsilon_{i,j,t}, \tag{2}$$

where i indexes firms, j indexes government agencies, t indexes years, $Contract_{i,j,t}$ is the US$ amount (in millions) of contract allocated to firm i by agency j in year t, $Revolver_{i,j,t}$ is a dummy variable coded as one if a revolver has influence over agency j in year t and if this person is later employed by firm i within three years from t and zero otherwise, $X_{i,t}$ are firm control variables, I_i is industry fixed effect, A_j is agency fixed effect, and Y_t is year fixed effect.[24] We define a revolver as having influence over agency j in year t if the revolver works in agency j in year t or serves in a congressional committee in year t that has oversight over agency j. 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 three-digit SIC code) as firm control variables.

As government contract allocations are bounded below by zero, we estimate Equation (2) using both OLS and Tobit. With corner solution data, regression coefficients in linear

[23] Kuziemko and Werker (2006) investigates how a country having a non-permanent seat on the UN Security Council affects the US foreign aid received by that country.

[24] We use the Fama and French (1997) 12-industry classification for the industry fixed effect.

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 ususally understood to be small (Greene, 2004). Given these different limitations, there are advantages and disadvantages to both OLS and Tobit. In the results that follow, we will report OLS estimates of the estimating equation taking into account industry, agency and year fixed effects. We will also report Tobit estimates ignoring all the fixed effects. Finally, we will report bootstrapped Tobit estimates, once again, taking into account industry, agency and year fixed effects.[25] In the bootsrapped Tobit regressions, we will use bootstrapped standard errors. For each iteration, we will draw 1,000 additional samples, with replacement, from our original sample and then re-estimate the slope coefficients. The bootstrapped standard errors will be the standard deviations across these 1,000 estimated slope coefficients.

Columns (1), (3) and (5) in Table 9 report OLS, Tobit and bootstrapped Tobit coefficient estimates of Equation (2), respectively. According to the OLS estimation, if a firm has a "revolver link" to a government agency in a given year, that is, if the dummy variable $Revolver_{i,j,t}$ equals one, then the value of contracts allocated to the firm by the agency increases by a statistically significant US$145.96 million in that year ($t = 1.92$). The Tobit estimations also reveal a significant positive association between government contracts allocated to firms by government agencies and the firm-agency linkages through revolvers over the years: the Tobit coefficient estimate on the revolver link dummy equals US$972.14 million ($t = 2.88$) whereas the bootsrapped Tobit coefficient estimate equals US$438.34 million ($t = 2.36$).

– Insert Table 9 about here –

The findings above are consistent with the hypothesis that there is a quid pro quo relationship between some public officials and their future corporate employers. However, they do not necessarily rule out alternative explanations involving assortative matching between revolvers and firms. For instance, it could be the case that if a firm receives more

[25]There is a large literature on bootstrap bias corrected estimation. Hall (1992) introduces general bootstrap algorithms for bias correction (see also Petersen, 2009).

valuable contracts from a certain government agency compared to others, the executives of the firm may get to know the officials working at that agency better and therefore may be more inclined to hire people from that agency. Or, if a firm wins more valuable contracts from an agency, it may be because officials there have a genuine preference for products and services offered by the firm and the executives of the firm may want to hire such like-minded people among public officials.

To further explore our hypothesis and its validity compared to alternative explanations, we therefore conduct a second test. To understand the logic of our second test, 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 provide favors hoping to cash later on from these actions. If regulations made revolving door movements more difficult, then we would not expect the first type to change their behavior. 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. 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. It would, however, under the alternative, as revolvers that provide favors hoping to obtain future employment would become less prevalent. We next put this to test.

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 appointees which restricted private employment 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 in the executive branch must abide by a two-year cooling off period after their government service which significantly

restricts their private sector employment opportunities.[26] Due to these two executive orders revolving door movements were relatively difficult during the years corresponding to Clinton and Obama presidencies in our sample period (namely, 1993-2000 and 2009-2012). Therefore, we will use a dummy variable for those years as our exogenous interaction variable with the $Revolver_{i,j,t}$ variable. Specifically, we will use the following equation for estimation:

$$\begin{aligned} Contract_{i,j,t} &= \alpha + \beta_1 * Revolver_{i,j,t} + \beta_2 * RestrictiveYear_t \\ &+ \beta_3 * Revolver_{i,j,t} * RestrictiveYear_t \\ &+ \gamma * X_{i,t} + I_i + A_j + Y_t + \epsilon_{i,j,t}, \end{aligned} \tag{3}$$

where $RestrictiveYear_t$ 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. If the null hypothesis is true, then the estimate of β_3 will not be statistically significantly different from zero. On the other hand, if there are revolvers inclined to participate in quid pro quo relationships with firms, then we expect the estimate of β_3 to be statistically significantly negative, that is, we expect revolver linkages to be less of an advantage in terms of government contract allocations during years of regulatory restrictions in which quid pro quo relationships are expected to be less prevalent.

Columns (2), (4) and (6) in Table 9 report OLS, Tobit and bootstrapped Tobit coefficient estimates of Equation (3), respectively. For all estimation methodologies, we find the estimate of β_3 to be statistically significantly negative. That is, government contract allocations attributable to revolver linkages are less valuable during Clinton and Obama presidency years compared to other years. This is consistent with the notion that there is a quid pro quo relationship between some public officials and firms.

One final estimation we carry out is on the pattern of government contract allocations during the years both prior to and after the hiring date of the revolver by a listed firm.

[26] 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.

Our estimation is described by the following equation:

$$\begin{aligned} Contract_{i,j,t} &= \alpha + \beta_{-3} * (-3Y)_{i,j,t} + \beta_{-2} * (-2Y)_{i,j,t} + \beta_{-1} * (-1Y)_{i,j,t} \\ &+ \beta_1 * (1Y)_{i,j,t} + \beta_2 * (2Y)_{i,j,t} + \beta_3 * (3Y)_{i,j,t} \\ &+ \gamma * X_{i,t} + I_i + A_j + Y_t + \epsilon_{i,j,t}, \end{aligned} \tag{4}$$

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

If revolvers' concerns about post-government employment indeed drive government contract allocations, then we expect two different patterns before and after the hiring of revolvers by firms: Before hiring, we expect the value of contracts awarded to future corporate employers to increase as the time tends towards revolvers' hiring dates, because concerns about future employment would become more signficant the nearer is the hiring date. On the other hand, after hiring, the value of contracts awarded to current corporate employers would be expected to decrease over time as revolvers' influence over their past government employers would wane. Our findings match the pattern explained above: Figure 2 shows the OLS and bootstrapped Tobit coefficient estimates for the indicator variables, $(nY)_{i,j,t}$, in Equation 4.

– Insert Figure 2 about here –

It is worth comparing our last result with the results from Section 4. Recall that we found firms exhibiting significant abnormal returns during the three-year period prior to the hiring of revolvers, however these returns significantly decreased or mostly vanished (depending on regression specification) during revolvers' tenure in the firms. Figure 2 shows that a revolver positively affects the value of government contract allocations received by a firm both before and after his joining the firm. The latter is not inconsistent with our findings about abnormal returns. Before the revolver joins the firm, the relationship between the firm and the revolver is not public information and therefore his positive impact on government contract allocations would not be fully reflected in the share price of the firm. On the other hand, once he joins the firm, his relationship to the firm becomes

public and therefore his impact on contract allocations gets compunded into the price. Hence, we have the aforementioned pattern for abnormal returns.

Overall, exploiting the dynamics of government contract allocations and firm-agency linkages through revolvers, we find strong support for the hypothesis that some public officials could be engaging in quid pro quo relationships with their future corporate employers. More specifically, our findings suggest that some public officials may be helping firms to receive more valuable government contracts in exchange for future employment.

6 Conclusion

In this paper we demonstrate that revolving door movements have a significant impact on corporate financial performance. In particular, we show that the portfolio of firms where current public officials become future employees outperforms the remaining firms by a highly statistically significant 4.83% to 10.22% per year.[27] Controlling for firm size, if a firm hires larger number of public officials, then it enjoys higher abnormal returns during these officials' government tenure. The abnormal returns are higher, the closer the time is to the hiring date of public officials. Once officials are hired by the firm, the abnormal returns begin to diminish and they eventually vanish as we move further away from the hiring date.

To show that abnormal returns are driven by hiring of public officials rather than the other way around, we run further tests. The reverse causality argument here is that firms which enjoyed abnormal returns in the recent past may be hiring more public officials as they can afford to do so. To assess the validity of this argument, during the years public officials are hired we match the firms that employ officials to a control group of firms that feature similar characteristics in number of employees, change in the number of employees, market capitalization, book-to-market ratio and industry group. The firms employing public officials significantly outperform the control group of matched firms, which is inconsistent with the reverse reasoning.

We also provide evidence on the underlying economic mechanism that drives abnormal returns. We find that firms receive more valuable government contracts from a government

[27] The exact outperformance figure depends on portfolio weighting methodology and risk adjustment.

agency when a future firm employee is holding a post at that agency. Also, government contract allocations attributable to hirings of public officials are significantly lower in value during Clinton and Obama presidency years in which presidential executive orders restrict revolving door movements. Moreover, there are two distinct patterns in government contract allocations before and after the hiring of revolvers by firms: Before hiring, the value of contracts awarded to future corporate employers goes up as the time gets closer to public officials' hiring dates. After hiring, the government contracts awarded to corporate employers gradually diminish in value.

Collectively, our findings are consistent with the view that some in the government service could be favoring certain firms in order to gain future employment there. This suggests that there is likely a need to monitor and reform the institutional incentives surrounding revolving door movements. The broader implication of this paper is that we need a better and deeper understanding of the formal and informal relationships between governments and firms.

Finally, let us note that 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. We leave these extensions for future work.

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Appendix

In the paper we excluded financial firms, because the risk adjustment methods we use, Fama-French factors and the characteristics-based benchmarks method of Daniel et al. (1997), exclude financial firms in the formation of factors and benchmarks. In this Appendix we report the results of replicating the analysis of Table 5 including financial firms. Table A1 shows the performance of all firms, including financials, in the years immediately before the hiring of revolvers. As shown in this table the results we obtain are essentially the same whether we include financials or not.

– Insert Table A1 about here –

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.

	Panel A				Panel B				Panel C			
	Revolving door movements				Market values of firms with revolvers (million US$)				Market values of firms w/o revolvers (million US$)			
Year	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	13	1	12	1	7,680	3,923	777	26,387	884	95	1	64,529
1991	15	3	14	3	8,693	3,900	78	27,713	1,226	161	0	75,653
1992	16	2	15	2	13,544	5,371	157	69,294	1,146	158	0	75,884
1993	16	3	15	3	15,535	11,192	101	48,773	1,033	133	1	89,452
1994	18	2	17	2	12,272	8,021	65	49,415	902	109	0	87,193
1995	20	3	19	3	18,260	13,436	90	75,335	1,172	150	1	120,260
1996	28	10	24	10	19,121	11,166	42	92,027	1,291	159	0	162,790
1997	37	9	31	8	24,030	11,935	52	109,639	1,490	156	0	240,136
1998	45	14	37	14	27,369	12,512	16	162,224	1,922	142	0	342,558
1999	52	12	45	11	41,973	7,737	25	602,433	2,535	191	2	507,217
2000	51	10	42	10	39,033	11,727	45	290,216	2,326	146	0	475,003
2001	67	26	52	22	35,143	9,322	105	357,949	2,149	232	1	398,105
2002	62	11	47	11	30,276	8,809	93	276,631	1,743	186	1	242,270
2003	63	12	46	11	42,624	17,210	238	311,066	2,451	377	5	271,002
2004	71	12	51	12	43,056	18,441	472	385,883	2,612	400	5	330,693
2005	84	21	56	19	47,216	19,003	466	370,344	2,610	397	3	198,839
2006	93	18	66	18	48,095	18,657	193	446,944	2,776	466	1	203,656
2007	103	26	70	25	54,641	23,432	358	511,887	2,892	411	1	228,016
2008	105	15	72	15	36,664	14,223	329	406,067	1,802	222	0	184,576
2009	112	27	75	25	43,377	22,452	474	322,668	2,600	442	3	190,983
2010	103	13	70	13	52,424	26,404	905	368,712	2,969	531	1	297,089
2011	125	33	81	26	54,523	24,144	99	406,272	2,742	462	2	211,894
2012	132	21	84	19	55,961	21,585	443	499,696	3,238	561	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

Rank	Top hiring firms	# of revolvers employed
1	Lockheed Corp	15
2	Raytheon Co	11
3	Boeing Co	10
4	Southwestern Bell Corp	9
5	General Electric Co	8
6	Northrop Grumman Corp	7
7	Google Inc	7
8	Microsoft Corp	5
9	Ford Motor Co Del	5
10	Wal Mart Stores Inc	5
11	Verizon Communications Inc	5
12	Boston Scientific Corp	5
13	Allied Corp	4
14	General Dynamics Corp	4
15	United Technologies Corp	4
16	Disney Walt Co	4
17	Duke Energy Corp	4
18	Monsanto Co New	4
19	Pepsico Inc	3
20	Westinghouse Electric Corp	3
21	Abbott Laboratories	3
22	Pfizer Inc	3
23	Middle South Utilities Inc	3
24	Lilly Eli & Co	3
25	America Online Inc Del	3

Panel B

Rank	Top hiring industries	Frequency
1	Electric, Gas, And Sanitary Services	10.22%
2	Communications	9.78%
3	Business Services	9.33%
4	Transportation Equipment	8.44%
5	Chemicals And Allied Products	8%
6	Electronic and Other Electrical Equipment and Components, except Computer Equipment	6.67%
7	Measuring, Analyzing, and Controlling Instruments; Photographic, Medical and Optical Goods; Watches and Clocks	5.78%
8	Industrial and Commercial Machinery and Computer Equipment	4.89%
9	Engineering, Accounting, Research, Management, and Related Services	4.44%
10	Food and Kindred Products	3.11%

Table 3: Government contracts

This table presents some basic descriptive statistics on the dollar amount of government contracts (in millions of US dollars) allocated to publicly traded firms and to the sub-sample of publicly traded firms that ever hired a revolver during our sample period. The data is from Bloomberg's BGOV database and the sample period is 1990 to 2012.

Panel A: All publicly traded firms

	Mean	Minimum	1st Quartile	Median	3rd Quartile	Maximum
Value of government contracts (per firm and year)	1.77	0	0	0	0	30,125.57
Value of government contracts (per firm, government agency and year)	0.02	0	0	0	0	29,281.33

Panel B: Publicly traded firms that ever hired a revolver

	Mean	Minimum	1st Quartile	Median	3rd Quartile	Maximum
Value of government contracts (per firm and year)	125.99	0	0	0	2.51	9,360.49
Value of government contracts (per firm, government agency and year)	1.68	0	0	0	0	9,360.49

Table 4: 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, one-, three- and four-factor alphas (corresponding to CAPM, the Fama-French three factor model and the Fama-French-Carhart model) and returns in excess of characteristics-based benchmarks as in Daniel et al. (1997). 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. Panel A shows the results for equally-weighted portfolios whereas Panel B shows the same statistics for portfolios of stocks weighted using firms equity market value 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. ***, **, * denotes statistical significance at 1%, 5% and 10% levels respectively.

Panel A: Equally-Weighted Portfolios

	Raw Returns	One-Factor Alpha	Three-Factor Alpha	Four-Factor Alpha	Excess Return over Characteristics-Based Benchmarks
Firms employing revolvers	13.99***	4.38***	3.69***	4.90***	4.43***
	(3.88)	(2.98)	(2.61)	(3.45)	(2.95)
Firms with no revolvers	12.02**	0.77	-0.70	1.79	1.52
	(2.27)	(0.31)	(-0.52)	(1.37)	(1.41)
Difference	1.97	3.61	4.39***	3.10*	2.91*
	(0.67)	(1.35)	(2.86)	(1.94)	(1.66)

Panel B: Value-Weighted Portfolios

	Raw Returns	One-Factor Alpha	Three-Factor Alpha	Four-Factor Alpha	Excess Return over Characteristics-Based Benchmarks
Firms employing revolvers	8.64***	-0.25	0.71	0.62	0.30
	(2.74)	(-0.16)	(0.52)	(0.43)	(0.17)
Firms with no revolvers	9.80***	-0.15	0.11	0.88	0.21
	(2.69)	(-0.20)	(0.15)	(1.19)	(0.44)
Difference	-1.17	-0.09	0.60	-0.26	0.10
	(-0.63)	(-0.05)	(0.39)	(-0.16)	(0.05)

Table 5: Firms' abnormal returns during revolvers' tenure in the government before joining the firm

The table shows the performance of portfolios of US firms during the three year-period prior to revolvers joining these companies, while revolvers were still working for the government. It also shows the performance of firms not employing revolvers, and the difference between these two groups of firms. Performance is measured using raw returns, one-, three- and four-factor alphas (corresponding to CAPM, the Fama-French three factor model and the Fama-French-Carhart model) and returns in excess of characteristics-based benchmarks as in Daniel et al. (1997). 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. Panel A shows the results for equally-weighted portfolios whereas Panel B shows the same statistics for portfolios of stocks weighted using firms equity market value 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. ***, **, * denotes statistical significance at 1%, 5% and 10% levels, respectively.

Panel A: Equally-Weighted Portfolios

	Raw Returns	One-Factor Alpha	Three-Factor Alpha	Four-Factor Alpha	Excess Return over Characteristics-Based Benchmarks
Firms that will employ revolvers	18.95***	9.94***	9.49***	9.20***	7.45***
	(5.25)	(5.13)	(5.18)	(5.21)	(4.59)
Firms that will not employ revolvers	11.98**	0.74	-0.73	1.77	1.49
	(2.27)	(0.30)	(-0.54)	(1.36)	(1.38)
Difference	6.97**	9.20***	10.22***	7.43***	5.96***
	(2.28)	(3.51)	(5.23)	(4.36)	(3.02)

Panel B: Value-Weighted Portfolios

	Raw Returns	One-Factor Alpha	Three-Factor Alpha	Four-Factor Alpha	Excess Return over Characteristics-Based Benchmarks
Firms that will employ revolvers	14.59***	5.69***	6.27***	6.50***	4.64***
	(4.21)	(2.85)	(3.28)	(3.25)	(3.11)
Firms that will employ revolvers	9.28***	-0.56	-0.22	0.40	-0.19
	(2.61)	(-0.79)	(-0.32)	(0.55)	(-0.37)
Difference	5.31**	6.25***	6.49***	6.10***	4.83***
	(2.34)	(2.81)	(3.10)	(2.70)	(2.86)

Table 6: 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 three 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, one, three and four factor alphas (corresponding to CAPM, the Fama-French three factor model and the Fama-French-Carhart model) and returns in excess of characteristics-based benchmarks as in Daniel et al. (1997). 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. Panel A shows the results for equally-weighted portfolios whereas Panel B shows the same statistics for portfolios of stocks weighted using firms equity market value 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. ***, **, * denotes statistical significance at 1%, 5% and 10% levels respectively.

Panel A: Equally-Weighted Portfolios

	Raw Returns	One-Factor Alpha	Three-Factor Alpha	Four-Factor Alpha	Excess Return over Characteristics-Based Benchmarks
Firms with a high revolvers-to-size ratio	19.53***	13.46***	12.37***	12.26***	9.23***
	(4.04)	(3.80)	(3.74)	(3.77)	(3.35)
Firms with a medium revolvers-to-size ratio	15.58***	10.08***	9.55***	9.40***	8.13***
	(3.66)	(3.21)	(2.97)	(2.95)	(2.69)
Firms with a low revolvers-to-size ratio	12.23***	6.64***	6.87***	6.49***	5.04***
	(3.55)	(3.28)	(3.53)	(3.32)	(2.86)

Panel B: Value-Weighted Portfolios

	Raw Returns	One-Factor Alpha	Three-Factor Alpha	Four-Factor Alpha	Excess Return over Characteristics-Based Benchmarks
Firms with a high revolvers-to-size ratio	16.68***	10.68***	9.33***	9.85***	11.09***
	(3.53)	(3.07)	(2.91)	(2.93)	(3.42)
Firms with a medium revolvers-to-size ratio	14.03***	8.38**	7.64**	8.34**	7.21**
	(3.05)	(2.43)	(2.21)	(2.42)	(2.29)
Firms with a low revolvers-to-size ratio	11.04***	5.53**	6.44***	6.46***	4.12**
	(3.16)	(2.45)	(2.97)	(2.82)	(2.41)

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 three-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, one, three and four factor alphas (corresponding to CAPM, the Fama-French three factor model and the Fama-French-Carhart model) and returns in excess of characteristics-based benchmarks as in Daniel et al. (1997). 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. Panel A shows the results for equally-weighted portfolios whereas Panel B shows the same statistics for portfolios of stocks weighted using firms equity market value 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. ***, **, * denotes statistical significance at 1%, 5% and 10% levels respectively.

Panel A: Equally-Weighted Portfolios

	Raw Returns	One-Factor Alpha	Three-Factor Alpha	Four-Factor Alpha	Excess Return over Characteristics-Based Benchmarks
Firms that employed revolvers in the past	14.75***	5.53**	4.87*	5.84**	3.74*
	(3.72)	(2.03)	(1.72)	(2.16)	(1.70)
Firms that did not employ revolvers in the past	12.03**	0.80	-0.66	1.82	1.53
	(2.28)	(0.32)	(-0.49)	(1.41)	(1.36)
Difference	2.72	4.73	5.53*	4.02	2.21
	(0.66)	(1.25)	(1.78)	(1.29)	(0.86)

Panel B: Value-Weighted Portfolios

	Raw Returns	One-Factor Alpha	Three-Factor Alpha	Four-Factor Alpha	Excess Return over Characteristics-Based Benchmarks
Firms that employed revolvers in the past	8.88**	0.01	0.58	2.10	-0.04
	(2.16)	(0.00)	(0.17)	(0.62)	(-0.02)
Firms that did not employ revolvers in the past	9.54***	-0.24	0.15	0.80	0.02
	(2.72)	(-0.38)	(0.23)	(1.32)	(0.04)
Difference	-0.65	0.25	0.43	1.30	-0.06
	(-0.20)	(0.07)	(0.13)	(0.38)	(-0.03)

Table 8: Firms' abnormal returns during revolvers' tenure in the government before joining the firm: Matching estimators

The table shows the difference in performance between a portfolio of US firms during the three-year period prior to revolvers joining them and a portfolio of firms that do not employ revolvers selected to match their general hiring patterns. The analysis uses the number of employees and the change in number of employees at the moment of hiring to match the treatment firms to the nearest neighbors. Some specifications also use size and book to market quintiles (Daniel et al., 1997) and industry as additional matching criteria. Industry is defined using two digit SIC codes. Matching is done according to propensity score to the nearest ten neighbors and using a caliper of 1 percent. Performance is measured using raw returns, and one-, three- and four-factor alphas (corresponding to CAPM, the Fama-French three factor model and Fama-French-Carhart model). 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 table shows the results for equally- and value-weighted portfolios. 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. ***, **, * denotes statistical significance at 1%, 5% and 10% levels respectively.

		Difference of average returns	Difference of one-factor alphas	Difference of three-factor alphas	Difference of four-factor alphas
Firms matched using number of employees and change in number of employees	Equal-weighted	9.21***	10.08***	10.36***	8.65***
		(3.97)	(4.39)	(4.37)	(3.76)
	Value-weighted	7.38***	7.65***	9.10***	7.24**
		(2.64)	(2.74)	(3.33)	(2.45)
Firms matched using number of employees, change in number of employees, and industry	Equal-weighted	6.33**	5.81*	6.08*	6.37**
		(2.06)	(1.90)	(1.92)	(2.03)
	Value-weighted	8.81**	7.07*	8.86**	8.96**
		(2.17)	(1.78)	(2.12)	(2.00)
Firms matched using number of employees, change in number of employees, size quintiles, and book-to-market quintiles	Equal-weighted	9.21***	10.26***	9.75***	7.69***
		(3.56)	(3.82)	(3.67)	(3.00)
	Value-weighted	5.11*	5.78**	6.10**	4.90*
		(1.94)	(2.20)	(2.27)	(1.73)

Table 9: Revolvers and government contracts

This table reports the results of pooled time-series cross-sectional OLS, Tobit and bootstrapped Tobit regressions of the value of government contracts (in millions of US dollars) allocated to publicly traded firms by different government agencies on an indicator variable that equals one if the firm hired a revolver from that agency in any of the following three years and zero otherwise and a set of additional instruments and controls. Additional instruments include an indicator variable that equals one for Clinton and Obama presidency years and zero otherwise and an interaction term between this variable and the revolver hired variable. Controls include the companys market capitalization, the ratio of the book value and market value of equity, the Herfindahl index, which is based on total sales in the 3-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. OLS and bootstrapped Tobit regressions also include full sets of industry, government agency and year fixed effects.
Each column represents a separate regression. In the bootsrapped Tobit regressions, we employ bootstrapped standard errors. For each iteration, we draw 1,000 additional samples, with replacement, from our original sample and then re-estimate the slope coefficients. The bootstrapped standard errors are the standard deviations across these 1,000 estimated slope coefficients. When drawing observations, we draw one cluster (at the firm-agency level) at a time.
t-statistics based on standard errors clustered at the firm-agency level are included in parenthesis. Observations are at the firm-agency-year level. Sample period is 1990 to 2012. The sample includes all firms that hired at least one revolver during the sample period and all agencies that ever awarded contracts.
***, **, * denotes statistical significance at 1%, 5% and 10% levels, respectively.

	Government contracts (million US$) allocated to firm by government agency					
	OLS		TOBIT w/o fixed effects		Bootstrapped TOBIT w/ fixed effects	
	(1)	(2)	(3)	(4)	(5)	(6)
Firm has a revolver link to government agency (β_1)	145.96*	232.17*	972.14***	1,129.46***	438.34**	610.28**
	(1.92)	(1.88)	(2.88)	(2.85)	(2.36)	(2.39)
Clinton and Obama presidency years (β_2)		0.25		-40.61***		309.22**
		(0.27)		(-2.98)		(2.44)
Firm has a revolver link to government agency		-183.88*		-408.30**		-466.18**
during Clinton or Obama presidency years (β_3)		(-1.75)		(-2.11)		(-1.97)
Constant (α)	-2.81	-2.80	-1,453.78***	-1,424.17***	-4,300.37***	-4,450.55***
	(-1.01)	(-1.01)	(-4.16)	(-4.17)	(-4.21)	(-4.45)
Firm control variables [ROA, CAPEX2SALES, COGS2SALES, BM, SIZE, HHI]	Yes	Yes	Yes	Yes	Yes	Yes
Industry, government agency and year fixed effects	Yes	Yes	No	No	Yes	Yes
Observations	119,325	119,325	119,325	119,325	119,325	119,325
R-squared	0,0517	0,0564				
Pseudo R-squared			0.0190	0.0197	0.1624	0.1628

Robust t-statistics in parentheses
*** significant at 1%; ** significant at 5%; * significant at 10%

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 or excess returns over benchmarks) 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.

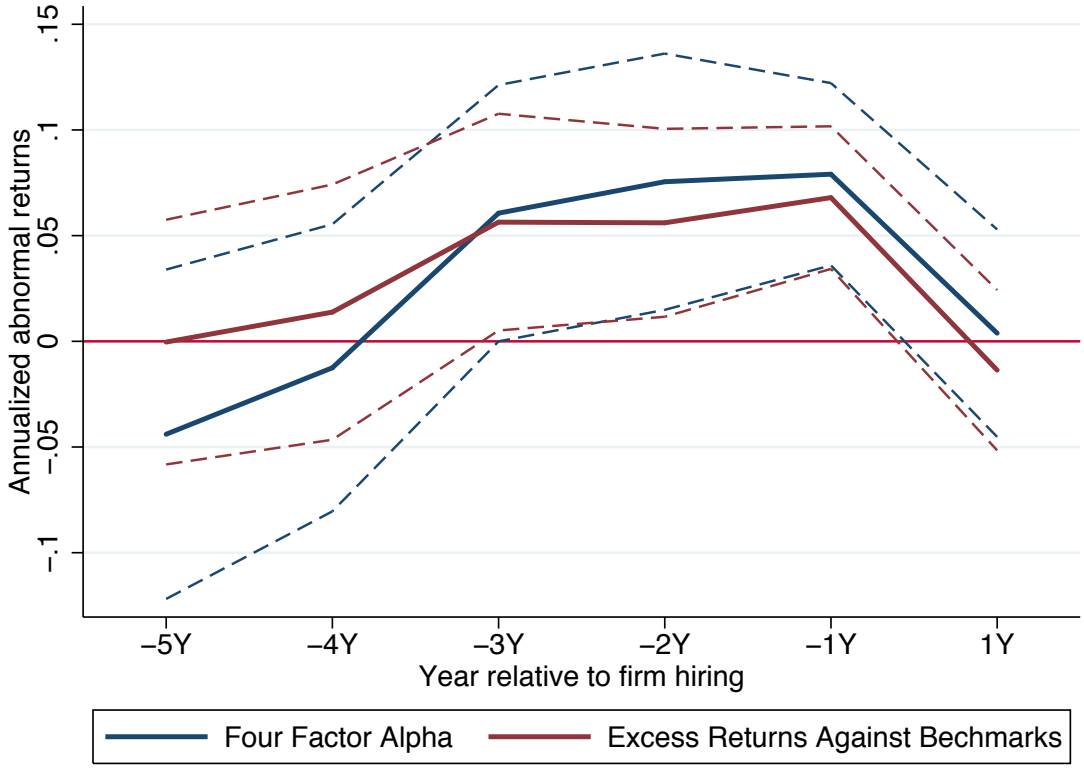

Figure 2: Government contract allocations around the hiring date of revolvers
This figure shows the OLS (left panel) and the bootstrapped Tobit (right panel) regression coefficients associated with a set of indicator variables defined according to whether or not the firm hired a revolver from the contract 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 9. 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.

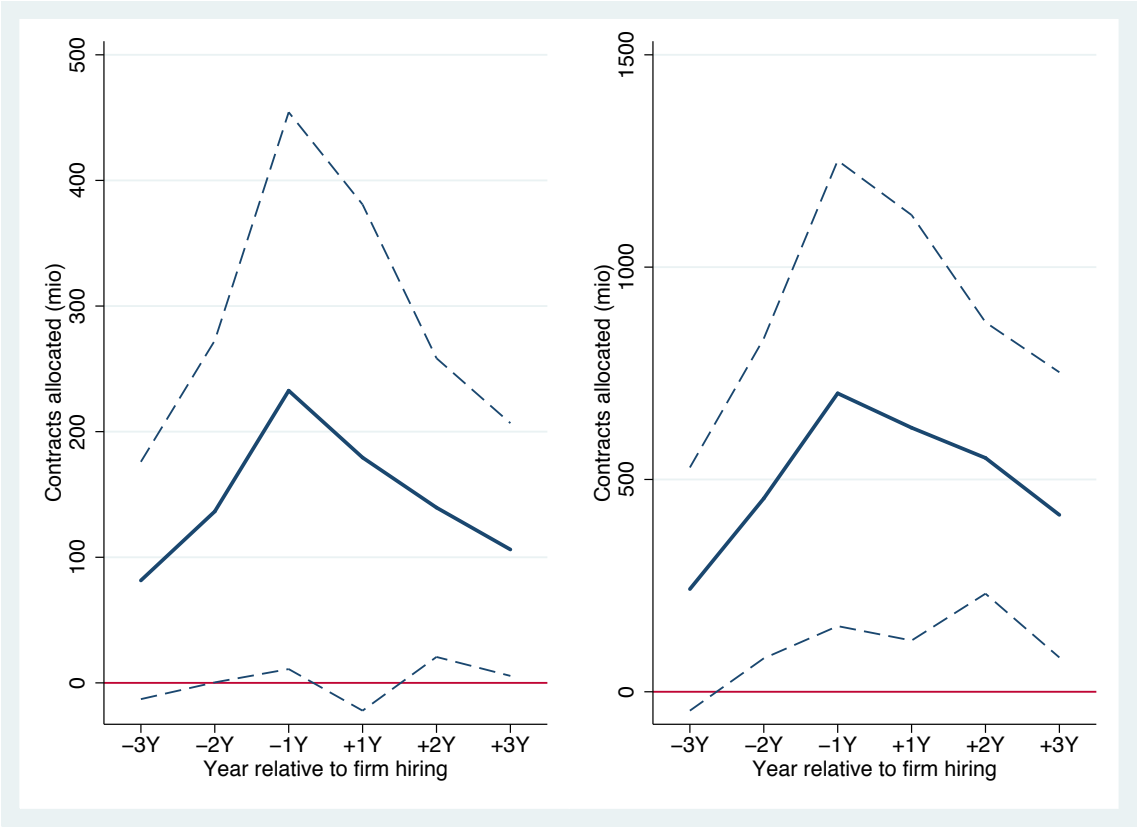

Table A1: Firms' abnormal returns during revolvers' tenure in the government before joining the firm: Using the sample including financial firms

The table shows the performance of portfolios of US firms during the three year-period prior to revolvers joining these companies, while revolvers were still working for the government. It also shows the performance of firms not employing revolvers, and the difference between these two groups of firms. Performance is measured using raw returns, one-, three- and four-factor alphas (corresponding to CAPM, the Fama-French three factor model and the Fama-French-Carhart model) and returns in excess of characteristics-based benchmarks as in Daniel et al. (1997). 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. Panel A shows the results for equally-weighted portfolios whereas Panel B shows the same statistics for portfolios of stocks weighted using firms equity market value 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. ***, **, * denotes statistical significance at 1%, 5% and 10% levels respectively.

Panel A: Equally-Weighted Portfolios

	Raw Returns	One-Factor Alpha	Three-Factor Alpha	Four-Factor Alpha	Excess Return over Characteristics-Based Benchmarks
Firms that will employ revolvers	18.61***	9.17***	8.38***	8.43***	7.04***
	(5.01)	(5.04)	(4.95)	(5.18)	(4.76)
Firms that will not employ revolvers	11.99**	1.23	-0.62	1.61	1.19
	(2.44)	(0.54)	(-0.55)	(1.51)	(1.41)
Difference	6.61**	7.94***	9.00***	6.81***	5.85***
	(2.44)	(3.20)	(4.95)	(4.25)	(3.42)

Panel B: Value-Weighted Portfolios

	Raw Returns	One-Factor Alpha	Three-Factor Alpha	Four-Factor Alpha	Excess Return over Characteristics-Based Benchmarks
Firms that will employ revolvers	14.30***	4.98***	5.19***	5.97***	3.90***
	(4.01)	(2.67)	(3.02)	(3.37)	(3.04)
Firms that will employ revolvers	9.12**	-0.71	-0.82*	-0.22	-0.24
	(2.56)	(-1.58)	(-1.69)	(-0.43)	(-0.80)
Difference	5.18***	5.69***	6.01***	6.19***	4.14***
	(2.60)	(2.90)	(3.27)	(3.11)	(2.91)