The Revolving Door and Regulatory Enforcement

– Firm-level Evidence on Tax Rates and Tax Audits

Abstract

Can firms extract policy concessions by hiring former Members of Congress (MCs)? I argue that firms use personnel with a background in politics to keep regulators away. With an empirical focus on tax enforcement, I present comprehensive evidence that firms can use MCs to avoid regulatory enforcement. Hiring MCs decreases firm-level tax rates—highly connected MCs who served on committees responsible for tax policy produce the largest decrease. Leveraging a novel hand-coded dataset of tax audits, I show that hiring an MC is associated with a lower probability of being audited. The change in enforcement has important consequences: Hiring an MC is associated with smaller fines and with uncertain tax positions being automatically accepted due to lapses in the statute of limitations. This indicates that rules are enforced differently against politically connected firms, shedding new light on the role of connections in the American political economy.

Keywords: Political connections; The Revolving Door; Regulatory enforcement; Lobbying the Bureaucracy; Corporations in politics.

1 Introduction

It routinely attracts great attention, when public officials leave office for private sector employment—the so-called revolving door phenomenon (e.g. Blanes i Vidal et al. 2012; McCrain 2018; Shepherd and You 2019). One reason for this is that former legislators stand to reap significant financial rewards in their post-elective lives (Palmer and Schneer 2016). The fact that firms are willing to pay extremely large salaries to hire former legislators raises concerns that they can use the legislator's connections for political gain. However, there is limited evidence that the revolving door actually allows private interests to extract policy concessions.

I argue that one important reason why former Members of Congress (MCs) are valuable employees is that they can help their private sector employers navigate the bureaucracy. Firms can leverage the MC's skill set to avoid enforcement of costly regulation in two ways. First, these so-called 'revolvers' can extract information about current and future enforcement activities from their former colleagues in Congress. Second, hiring them might act as a signal of the firm's willingness and ability to make any action against it costly for the regulatory agency (Gordon and Hafer 2007, 2005). Both mechanisms increase the agency's expected costs of examining the firm, making it prioritize its limited resources on other investigations. With an empirical focus on the enforcement of tax policy, I show that hiring a former legislator decreases the company's tax rate, and that the effect is likely to be driven by a lower probability of the firm being audited by the IRS.

To test this argument, I undertake an extensive data collection effort, aimed at dealing with three empirical obstacles. First, I construct a database of publicly listed companies that have hired former MCs. Second, it is not random what type of firm chooses to hire former legislators. To circumvent this problem, I leverage a difference-in-differences strategy, where I compare firms that hire MCs in the current year to those that hire within a brief time-frame. Third, data on the IRS's enforcement activities are confidential, which makes it difficult to observe enforcement activities. To build a database of tax enforcement at

the firm-level, I hand-code the sections of the 10-K reports, where the boards of directors inform the shareholders about business with the IRS. I show that it is less likely that the IRS will initiate an audit of a company that has recently hired an MC. To delve deeper into the consequences of the more lenient enforcement, I leverage a wealth of accounting data. Importantly, when firms submit their tax returns, they apply a range of assumptions. For some of those assumptions, it is uncertain whether the tax authority will accept them (i.e. the firm's tax position is uncertain). If such an uncertain position is not accepted, it can prove quite expensive for the firm. However, if an audit is not conducted after three years, uncertain tax positions are accepted due to expiration of statute of limitations. I show that when firms hire revolvers, more of their uncertain tax positions are automatically recognized due to lapses in the statute of limitations. Additionally, the firms are fined less by the IRS. Both these results are likely to arise, because the firm is audited less.

Throughout a battery of additional tests, I show that the drop in tax rates is largest, when firms hire former MCs who were centrally placed in Congress's cosponsorship network and served on committees responsible for IRS oversight. I investigate and reject an extensive set of alternative explanations. Specifically, I find no evidence that the findings are driven by lobbying activities, campaign donations, hiring IRS bureaucrats and congressional staffers, or changes to the tax code.

Overall, the results suggest that the IRS changes its enforcement activities, when firms become politically connected. The same legislation applies, but is enforced more leniently against connected firms. The estimated effects are meaningful to the individual firm—they save approximately $1.6 million on fines alone.

The IRS estimates that aggressive corporate tax strategies (i.e. tax schemes that reduce the effective tax rate below the level sought by the tax code) may cost the federal government as much as $1 trillion a year in taxes that should have been paid.[1] These finances are in

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[1]This estimate of the so-called 'tax gap' as well as the problems with resources for enforcement have been reported in multiple media outlets.

large part lost, because the IRS cannot keep up with the intricate tax avoidance strategies of large corporations—the agency simply does not have the necessary resources available for enforcement. This paper sheds new light on how firms can use political means to aid their tax strategy, which has been overlooked in the literature so far. The explicit focus on tax enforcement is particularly important, since the existing literature has focused on how lobbying shapes tax legislation (Richter et al. 2009). In advancing this view, the paper also adds to our understanding of how firms seek political influence (Fouirnaies and Hall 2018), specifically by shaping discretionary regulatory enforcement (Gordon and Hafer 2013, 2007, 2005).

Additionally, the paper adds to the literature on the revolving door. Existing research has mostly focused on how a background in politics is valuable for contract lobbyists. This line of research has documented that having a political background has large effects on the revenue a lobbyist generates for the lobbying firm that employs her (Blanes i Vidal et al. 2012; LaPira and Thomas 2017; McCrain 2018), and that revolvers who were effective legislators also seem to be more effective as lobbyists (Makse 2017). Recent advances show how the labor market for revolvers is inherently shaped by supply and demand factors (Strickland 2020; Weschle 2019). Similarly, research on the impact of political connections among publicly listed corporations has documented large effects on firm performance on the stock market (Luechinger and Moser 2014). However, both potential clients of lobbying firms and investors in publicly listed companies are likely to be attracted to politically connected firms in the *expectation* that their connections will attract economic rents, or that the new, politically connected employee is highly skilled. This does not necessarily imply that connected firms who experience increased lobbying revenue or abnormal stock market returns actually are successful in shaping political outcomes. I complement the existing literature by documenting effects of hiring revolvers on the firm's political and regulatory environment. This suggests that the flow of legislators out of office may have real political effects beyond changing the

See for example `https://bloom.bg/3aPaIVg`.

expectations of lobby clients and investors.

2   Using Revolvers to Lobby the Bureaucracy

The most seminal accounts of the political influence of special interests have considered how lobbying activities may shape legislative outcomes (e.g. Baumgartner et al. 2009; Hall and Deardorff 2006). However, a very large part of the lobbying activities that are associated with individual bills happen after bill passage (You 2017). This suggests that interactions with the bureaucracy are extremely important for influence-seeking interest groups (Bernstein 1955). The extant literature on these interactions emphasizes how organized interests can shape bureaucratic rule-making either by directly lobbying federal agencies, or by using their political connections to put pressure on them (Haeder and Yackee 2015; Hall and Miler 2008; Yackee and Yackee 2006; Yackee 2005).

However, even if a company manages to influence the implementation of a bill or the formulation of a rule, it might have to share the spoils from new pro-business regulation with other firms in its sector. If the company can circumvent the enforcement of rules and regulations altogether, it can gain a purely private good (Gordon and Hafer 2007). Importantly, in directing their enforcement activities, agencies respond to the expected costs and benefits associated with examining a firm. In the words of Gordon and Hafer (2005, p. 246), if an examination is too costly or associated with too few benefits, "the regulator [...] reacts to the realities of its limited abilities and pursues the rewards of regulation in other places". Building on this line of research, I argue that during a legislator's career in elective office, she becomes adept at navigating the bureaucracy. By hiring former politicians, a firm can reduce an agency's expected benefit of auditing it.

In what follows, I will outline the various assets that could make a revolving door MC helpful for a firm in its interactions with the bureaucracy. I will pay special attention to how the revolver may help the firm in its interactions with the IRS. In the following section on

the regulatory context, I then outline how expected costs and benefits figure into the IRS enforcement activities. In Appendix A, I provide more detail on the process through which IRS selects firms for examination.

2.1 Political Connections and Regulatory Enforcement

Revolvers make valuable employees, because their background allows them to help their new employer in their interactions with the political realm. This is spurred on by the revolver's background in public service, which endows her with a bundle of human capital that is difficult to obtain through other career paths. Previous research has explored three aspects of the revolver's skillset: social connections to decision-makers, substantive expertise and procedural expertise (Bertrand et al. 2014; Blanes i Vidal et al. 2012; LaPira and Thomas 2017; McCrain 2018). While they are often conceptualized as distinct forms of human capital (e.g. Bertrand et al. 2014), they are perhaps better considered as mutually reinforcing. This also implies that when a firm hires a revolver, it receives a bundle of strongly correlated assets, and distinguishing between its components may not be empirically feasible.

There is strong theory on the importance of a revolver's social connections to politicians, their staff, and bureaucrats. Simply put, decision-makers are more likely to grant access to people they know (Hirsch et al. 2019). This is empirically supported by a range of studies (Blanes i Vidal et al. 2012; Egerod et al. 2020; McCrain 2018). Importantly, information extraction is an intrinsic part of the non-market strategy of many firms (Finer 2018). Therefore, firms use social connections to obtain access allowing them to extract information (LaPira and Thomas 2017). Similarly, because of the revolver's procedural knowledge of the political game, and her experience overseeing the bureaucracy, she is likely to know who to target to obtain information. We can see how social connections and procedural expertise interact: The latter helps the revolver know whom to target—the former allows her to target them successfully.

A legislator might not have extensive knowledge about intricate details of the tax code.[2] Thus, if a firm is wants information on the current (or historical) tax code, it might be better off hiring a former staffer or bureaucrat. However, the legislator will be able to leverage her procedural knowledge and social connections in Congress to identify the most important entry points, and to gain access through them. As I discuss below, the revolver might use this to obtain insider knowledge about the type of tax position that the IRS is unlikely to challenge. Additionally, it would allow her to extract information about *upcoming* changes to the tax code, IRS interpretation thereof, and to enforcement strategy. Additionally, the revolver could use her connections to discuss the firm's tax position with bureaucrats before filing. Informally pre-clearing the firm's tax position—or finding out which aspects the IRS is unlikely to challenge—could be a particularly powerful way of avoiding tax enforcement. These types of private knowledge would allow the firm to incorporate changes in policy and IRS strategy in their tax planning before their competitors. Importantly, the revolver might also be able to extract details about how the agency chooses firms for examination. If the company uses the legislator to extract information about enforcement patterns, it can simply behave in a way that makes the IRS more likely to expect that it is in compliance with the tax code.

Additionally, the revolver could use her social connections to decision-makers to pressure the IRS to act more leniently towards the firm. There exists a number of anecdotes indicating that it has happened previously (Kiel 2020a,b). I will return to them below. If this happens more generally, it could be because revolvers can use their political knowledge to convince the legislator in question. In this regard it should be noted that previous research at the state and IRS district levels suggests that the IRS's auditing decisions are susceptible to political pressure (Hunter and Nelson 1995; Young et al. 2001).

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[2]Extensive theory, empirics and anecdotes suggest that legislators are less well-versed in the content of regulations than staff and bureaucrats (LaPira and Thomas 2017; McCrain 2018)

Importantly, the revolver does not actually need to take any action for her appointment to produce a change in IRS behavior—her arrival at the firm can be a signal in itself (Gordon and Hafer 2005). If the legislator is able to help in refining the firm's tax planning strategy (e.g. by extracting information), it would make it more difficult for the IRS to challenge the firm's tax position. This would make a conflict between the IRS and her employer very costly. If tax enforcers anticipate this increased enforcement cost, we could expect them to give the firm a wider berth—even in the absence of an actual change in the firm's behavior. A similar argument could be made in situations where revolvers use political connections to put pressure on the IRS. Viewed in this light, the effect on IRS leniency could come about, because hiring revolvers signals that the firm is intent on making any conflict costly.

In sum, I argue that firms can use the array of assets a revolver brings with her to shape the regulatory pressure facing them. Importantly, if the firm can predict that it will not be investigated, it will be more likely to take an aggressive tax position; for example, by aggressively taking uncertain positions on tax benefits[3] or pursuing loopholes in the tax code (Hoopes et al. 2012). Overall, we can think of the adoption of an aggressive tax position as an investment with varying levels of uncertainty: it yields a financial return here and now, but this benefit might be reversed, depending on the underlying probability that the company is selected for an IRS examination (Allingham and Sandmo 1972). However, if the firm is able to avoid being audited by hiring a former legislator, it can take aggressive positions with impunity.

One might object that the IRS should recognize the appointment of revolvers as a signal of the firm intent to decrease its compliance with the tax code. This should increase the agency's scrutiny of the firm accordingly. However, this depends entirely on how much hiring a revolver changes the regulator's perception of the enforcement cost. If the revolver

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[3]Tax benefits are tax laws that help taxpayers reduce their tax liabilities. They range from deductions (that reduces the firm's taxable income) to tax credits (which reduces the amount of taxes the firm owes after all deductions are made.

increases the expected cost of examining the firm by enough, the IRS will still avoid auditing the firm despite knowing that the firm is likely not to be in compliance with the tax code. Indeed, one of the main implications of the Gordon and Hafer (2005) model is that firms with the largest political muscle should be less compliant, but also afforded more leniency by bureaucrats. The agency's decision regarding whether or not to examine a firm is not driven solely by the regulator's beliefs about the firm's compliance—but by a joint assessment of the costs and benefits of examining a firm.

3   Context: Costs and Benefits in IRS Enforcement

The argument presented here is that firms can use the revolver's skillset to make enforcement of rules against it more costly. In that regard, it is important that the IRS is highly litigation averse (Winters 2011, Ch. 5). Therefore, if a firm can use revolvers to increase the cost of a tax audit (and any subsequent litigation), the IRS should be highly responsive to this. This makes tax enforcement a good setting for testing the argument presented here. In this section, I will briefly outline why.

The tax code is highly complex, and legislative changes (Auerbach and Hines Jr 1988) combined with a continuous stream of rulings (Hickman 2009) ensure that it remains complex in ever-changing ways. This makes the enforcement of the tax code very costly, and since the IRS is resource-constrained, it has to focus its activities on cases, where it expects the largest returns to examination at a minimal cost. This is the starting point of the canonical Allingham and Sandmo (1972) theory of tax non-compliance.[4]

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[4]This model considers an agency problem, where an enforcer of tax policy has incomplete information about a firm's true taxable income. The enforcer is resource constrained, and monitoring is costly, so the enforcer has to choose a subset of firms to examine for potential compliance issues. The enforcer uses observable characteristics of the population of companies in an effort to decide which are worthwhile examining.

This is mirrored in the guidelines for enforcers. They explicitly state that offers in compromise and even doubt as to liability should be assessed against the potential costs of litigating against a company (IRS 2016a,c). Winters (2011, p. 224) quotes an anonymous US tax lawyer employed with a 'magic circle' firm for saying that "[i]f you've got the resources, the IRS faces a big risk of litigation. That means you're going to be able to cut a better deal." The notion that the IRS is litigation averse, and shies away from conflict when it would be costly seems to be widespread (Kiel (2020a) cites another tax lawyer). Crucially, if a firm takes an aggressive tax position at the margin of legality, it is less likely to be challenged if it is costly to lift the burden of proof against the position (Winters 2011). Making use of complex and arcane rules in a potentially legal way helps this endeavor. And insider information can be immensely important for the firm in knowing which positions that are unlikely to be challenged. Anecdotally, we know that tax consultants are very often used to help firms figuring out which positions that are too costly for the IRS to challenge (Winters 2011, Ch. 5) – revolvers may simply be better at extracting up to date information about this.

Because the initiation of an audit is the first step in a potentially conflictual process, which may end in costly litigation, resource constraints are likely to play a large part in determining which companies the IRS selects for examination. If an IRS auditor notices a potentially problematic tax position, she is less likely to call for an audit, when it is more costly to challenge the position effectively.

Additionally, a growing body of qualitative evidence show how wealthy taxpayers pursue strategies aimed specifically at making enforcement costly for the IRS (Winters 2011). In particular, recent journalistic accounts outline how firms through lobbying can convince sympathetic legislators to put pressure on the IRS on their behalf. An important example of this is an ongoing audit of Microsoft. When faced with an IRS audit that could cost the company billions of dollars, Microsoft leveraged connections in Congress that contacted the IRS on their behalf and even pushed through legislation that hampered the IRS's ability to

litigate effectively (Kiel 2020a). The behavior of IRS in subsequent cases suggests that the powerful political response by Microsoft pushed the agency to avoid making use of some of its strongest tools in the future (Kiel 2020b).

4   Identification, Methods & Data

When investigating the proposition that firms can decrease their tax expenses by hiring former legislators, I am faced with two major obstacles to inference. First, it is not random which kind of firm it is that chooses to hire an MC. Second, data to test these propositions are not easily obtained. The IRS guards their data on monitoring activities carefully and only releases aggregate counts of audit conducted by broad categories of firms. Except for those legislators who register as lobbyists, there is no publicly available database of post-elective career trajectories. In this section, I describe my proposed solutions to problems concerning identification and measurement.

4.1   Identification and Sampling Strategy

Identifying the effect of hiring a former legislator on a firm's tax rate entails the inherent problem that it is not random what type of firm chooses to make such a hiring. There is a wide range of unobservable selection effects, and it is highly unlikely that they can be dealt with using methods of statistical control or matching. To obtain more plausibly identified estimates, I employ a particular strategy for sampling firms. Specifically, I constrain attention to the companies that at some point hire former legislators. I then proceed to estimate average treatment effects on the treated (ATTs) using difference-in-differences models. A standard difference-in-differences estimate would yield a weighted combination of the effect of a) choosing to hire a legislator at all, and b) of doing so at a different point in time (Athey and Imbens 2018). Crucially, because of the sample selection, I only compare the trends among firms that hire a legislator to the trends of firms that have recently hired one or will

do so soon. Thereby, I dispense with variation from (a). As a consequence, the identifying assumption is that tax rates would have evolved similarly among treated and untreated firms, had the treated firms chosen to hire an MC at a different point in time. I relax this global parallel trends assumption further by estimating flexible models, assuming that the parallel evolution only happens among firms in the same industries that hire the same type of legislators.

This combination of sample selection and estimation technique deals with a variety of selection effects by only comparing firms that choose to become connected, and identifying the effect based on timing of employment alone. As I show in Appendix D1, the strategy is effective, since the timing of hiring a former MC is uncorrelated with firm-level economic characteristics. This is a powerful result: Among firms that all choose to hire a former MC, the company's underlying economic situation matters little for when they hire a legislator. Overall, this makes the identifying parallel trend assumption plausible. The most important remaining threat to identification is the fact that firms may follow other non-market strategies when they hire former legislators. In section 5.2.2, I present evidence from an extensive set of alternative strategies, which suggests that this is not generally the case.

This sampling strategy also forms the basis of the main independent variable: A binary indicator for the year a publicly listed company hires a former MC. To select firms and code this timing variable, I started from a list of retiring MCs and relied on a variety of sources to identify their post-elective career trajectories. First, Bloomberg CVs keeps track on the careers of a number of influential businesspeople, including most MCs in the sample. This, therefore, presented a very useful helicoptor view over most careers. However, the selection of people Bloomberg reports on is obviously not random, and there might be omissions in their records. To capture additional positions on Boards of Directors, I supplemented this helicopter view with 10-K filings retrieved through the EDGAR database. To capture positions that were omitted by Bloomberg or are not directorships, I conducted extensive internet searches. Most firms send out press releases announcing when they establish, for

example, advisory boards with MCs on them. Additionally, because of the high-profile nature of most revolving door employments, most cases where former MCs are employed in large, publicly listed companies receive coverage. Finally, I use employment histories from The Center for Responsive Politics (CRP). The latter source is useful for keeping tabs on former MCs who register as lobbyists under the Lobbying Disclosure Act (LDA). I present single estimates for all positions held by revolvers in the main analyzes. In Appendix I, I show results separately for each position type. Besides the in-house positions I have described, firms may hire revolvers as contract lobbyists. These are not included in the main results. Recent work emphasizes the different role played by contract lobbyists relative to in-house personnel (Ellis and Groll 2018; Groll and Ellis 2017). Additionally, agency drift is more likely among contract lobbyists (Holyoke 2021). Thus, if contracting with a lobbying firm that employs former legislators also affects the firm's tax bill, it is likely to be through different mechanisms. However, I have also collected data on this type of connections, and investigate them in a set of auxiliary analyses. Since data on employment termination is mostly missing, I only use the first year a former MC was employed in a company.

The sample covers publicly listed companies that hired a former MC in the period 2004-2015. In total, I track 180 companies and 77 revolvers. This implies that the same people get hired in multiple positions. This is most common among legislators who get positions on boards of directors. There, the number of directorships ranges from one to six, and the average politician-director has hold more than two directorships in her post-political career (see appendix B for more descriptives). Some – mostly directors – also hold several different types of positions. A good example of this is former senator and secretary of energy Spencer Abraham, who, besides serving on multiple boards, was appointed to lead Uranium Energy Corp's Advisory Board.

The sample restriction described above has consequences for the estimand. Particularly, it will only characterize the ATT among firms that actually hire former legislators. Arguably, however, identifying an effect among firms that become connected (and not extrapolating

to, say, Fortune 500 firms) is the estimand of most interest. Additionally, even if it were feasible to sample a more general set of firms and deal with confounders through means of statistical control, the estimate conditional on controls would still be a local one, and not representative of the effect in the more general set of firms (Aronow and Samii 2016). In Figure 1 I show the distribution of hires across sectors and job types. In Appendix B, I present descriptive statistics.
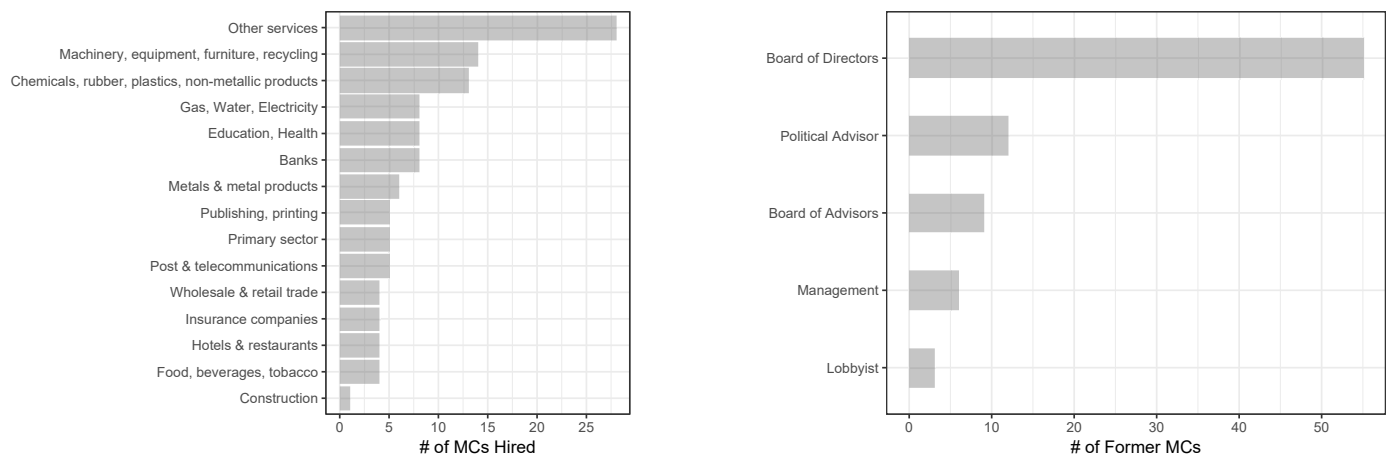

Figure 1: Characterizing Firms and Jobs in the Sample

Left panel shows the distribution of firms across sectors. 'Other Services' is a standard sector category in the NAICS 2-digit. In this sample it includes, for example, firms within private equity, real estate, asset management, acquisitions, and certain technology firms (e.g. Alphabet, Inc.). Right panel shows the distribution of legislators across job types.

4.2   Data on IRS Audits and Company Tax Rates

Tax Rates

To measure Effective Tax Rates, I follow Gupta and Newberry (1997) and use the current portion of total tax expense divided by total pre-tax book income (see also Richter et al. 2009). I put a one-year lead on the dependent variable for two reasons. First, most revolving door MCs are hired sometime during the year, and I have no data on when. Second, firms have different tax years, and have to file their return at different points in time during the year. To ensure that the revolver actually arrives before the firm decides its tax position, the

lead is necessary. There are a number of extreme observations on the tax rate variable. These are given less weight because I use the natural logarithm, but, to make sure the estimates are not artificially inflated, I discard the top and bottom 2.5 percent in the distribution. In Appendix E2, I document that excluding these observations yields conservative results, as the baseline estimates I present in the main text are approximately 40 percent smaller than the ones relying on the full sample. Using the natural log of a variable that can take on non-positive values poses some challenges. In the main results, I simply add a constant, but in Appendix E3, I test the robustness by applying two alternative transformations (the inverse hyperbolic sine and the bi-symmetrical log transformation), both of which behave like the log-transform, but allow for non-positive values. I also show models using the level of tax rate. These robustness checks indicate that the baseline approach yields conservative estimates.

IRS Auditing Activities

To measure IRS enforcement activities, I hand-code a binary indicator capturing whether an audit of the company's accounts was initiated during any given calendar year. Because the IRS's data on this are confidential, I have coded the sections of all 10-K reports that explain tax matters and interactions with the IRS to the shareholders of the companies. If the IRS disagrees in significant portions of a company's tax position, this could lead to very large additional tax expenses. Because of this, IRS audits entail potentially large liabilities for a company. Therefore, publicly listed corporations are required to inform their shareholders when the IRS plans to initiate an examination. These accounts are normally given in 10-K reports, which is why they provide an enormous amount of unstructured data on the interactions between the company and the taxing authorities. Because 10-K reports can be several hundred pages long, and every company organizes its reports differently (a structure which even changes from year to year) extracting these data is extremely cumbersome. To structure this extensive data-gathering process, I devised a two-step manual coding scheme,

where I first located the sections dealing with the relevant tax matters, and then ascertain whether an audit was initiated in the given year. The coding scheme is presented in Appendix C.

To provide more detail to the picture of IRS enforcement, I also collect accounting data on the financial consequences of the firm's interactions with the IRS from Compustat. Specifically, I use measures of the size of fines imposed by the IRS on the firm, and the dollar-amount of previously unrecognized tax positions the firm can recognize, because the IRS failed to examine it within the statute of limitations. I also collect data on the value of the firm's total unrecognized tax positions.

4.3   Data on Additional Implications

As I have argued previously, it is not meaningful to distinguish between different types of assets the revolver controls. However, it is very helpful to devise tests of whether the effects are driven by the revolver's characteristics or something else about the firm's non-market strategy. To do so, I conduct a number of auxiliary analyses aimed at capturing additional implications of the theory. To do this, I collect data from a variety of sources. First, I construct a measure of the former legislator's degree of connectedness. I follow Fowler (2006) and use the legislator's centrality in the cosponsorship network of Congress. I construct a directed network of cosponsorship for each Congress in both the Senate and the House for the period 1992-2015, where the directed connection between each pair of MCs increases in strength every time one cosponsors a bill proposed by the other. Cosponsoring a bill can be seen as a social act of support for the original sponsor, a tie which grows in strength for each act of cosponsorship. Since an MC does not actually have to meet or have lasting relationships with their cosponsors, however, these ties send a noisy signal of their connectedness. In an attempt to make the measure less noisy, I weight each act of cosponsorship by the total number of cosponsors on that bill. Combining these two sources of information should provide a reasonable measure of the strength of the connection

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between each pair of MCs (Fowler 2006). After the networks are constructed, I compute each MCs Congress-specific betweenness score, which measures the extent to which an MC has been able to garner support from cosponsors from different blocs in the network. To ease interpretation, I center each betweenness score by its Congressional mean and normalize it by its standard deviation. I then average each MCs standardized betweenness score over her tenure. This gives the score an easy interpretation: A positive score of one, for example, indicates that the revolving door MC on average scored 1 standard deviation above the Congress-specific mean throughout her tenure.

I also investigate an extensive set of alternative non-market strategies that could drive my main result. First, I collect data on lobbying activities filed under the Lobbying Disclosure Act (LDA) and made available by the Center for Responsive Politics. I manually identify whether firms in my dataset file lobbying reports, and create two binary variables for a) whether they file a lobbying report mentioning the IRS as a target of their lobbying effort, and b) whether they file any lobbying report at all. Second, I use the BoardEx database to identify which of the firms appoint former IRS bureaucrats, Congressional staffers, or people with a background in the Council of Economic Advisors (as members or staff) to their boards of directors. BoardEx contains the employment history of all directors of publicly held firms. This unique data allows me to identify which firms that hire people with a background at the IRS. This would otherwise be impossible, since the names of IRS' employees are FOIA exempt, and other sources therefore will not allow me to capture the career moves of the Services's bureaucrats. Finally, I use data from Stuckatz (2020) on campaign donations by publicly held firms. As with the lobbying variables, I create a binary indicator of whether the firm donated at all. However, the results from models with total expenditure are similar.

Finally, I construct a binary indicator of whether the former MC served in a committee responsible for IRS oversight (the Senate Finance Committee or the Ways and Means Committee in the House). I acquired data for this through Stewart III and Woon (2017).

4.4 Additional Firm-level Covariates

To adjust for a company's size and assets, I include the natural log of the total dollar value of its total assets, capital, enterprise value, and number of employees. To capture the company's operating performance, I include logged revenue and gross income, both measured in US dollars. Finally, I include the turnover of the company's stock and its share price. This is to capture potential effects of increased stock market attention. Because the financial variables can be negative, they are rescaled to have a minimum of 0.5, before being log transformed.

4.5 Model Specification and Identification

I consider variations of the following two-way fixed effects model:

$$lnETR_{c,t+1} = \delta_1 \cdot R_{ct} + \beta_1 \cdot X_{c,t-1} + \gamma_c + \phi_t + \epsilon_{c,t+1},$$

where ETR is the effective tax rate paid by firm c in year $t+1$. R is the variable of interest, capturing the year during which a former MC is hired by the firm. The two dimensions of fixed effects are denoted by γ, company fixed effects, and ϕ, a set of year effects. ϵ is the idiosyncratic error term, and X is a vector of firm-level controls.

The inclusion of twoway fixed effects makes this a difference-in-differences model (Goodman-Bacon 2018). As described previously, the combination of the sampling strategy and estimation technique deals with a range of threats to identification. However, two salient ones remain. First, the models are vulnerable to heterogeneous shocks. Therefore, I adopt a series of highly flexible models, relaxing the parallel trends assumption significantly by imposing it only locally, among highly similar firms and MCs. This also allows shocks to have heterogeneous effects across firms. Second, hiring revolvers might be related to other influence-seeking strategies. I investigate and find limited evidence that the effect of gaining a connection is driven by these other strategies.

Recent methodological advances show that two-way fixed effects can be a biased estima-

tor of the ATT if already-treated firms enter the control group, and if the ATT varies over time (Goodman-Bacon 2018). In Appendix H, I use the techniques proposed by Callaway and Sant'Anna (2021) and Imai et al. (2019a,b) to estimate the difference-in-differences that are not subject to this bias. The results suggest that the two-way fixed effects estimates are not biased in this situation. I also collect data on when politicians-turning-directors leave firms using BoardEx. In combination with the Imai et al. (2019a) estimator, I use these estimators to examine the longevity of the results. These results can be found in Appendix H2.

Clustered standard errors are biased when a significant part of the population is observed, there is treatment effect heterogeneity, and there is a finite set of treatment events (Abadie et al. 2017). Therefore, I obtain uncertainty estimates by randomly permuting the timing with which a firm hires a former legislator 3,000 times. That is, simulations are done under firm-blocking. This allows me to simulate the two-sided effect distribution under the sharp null. This technique resembles Conley and Taber (2011) by simulating alternative treatment schedules, thereby yielding non-parametric uncertainty estimates that are highly suited for the difference-in-differences setting. In Appendix E5, I show results from design-based firm-clustered standard errors Abadie et al. (2017).

5 Baseline Results

Figure 2 plots pooled corporate tax rates at $t + 1$ in the five years leading up to the hiring of a former MC. The fitted line is estimated using a lowess smoother indicating the expected tax rate across companies within each time period.

As we can see, the tax rate across companies is relatively stable throughout time. Importantly, this stability suggests that pre-treatment trends among connected and unconnected firms are approximately parallel. Additionally, we observe a sudden and sharp decrease in tax rates the year after a former MC is hired.
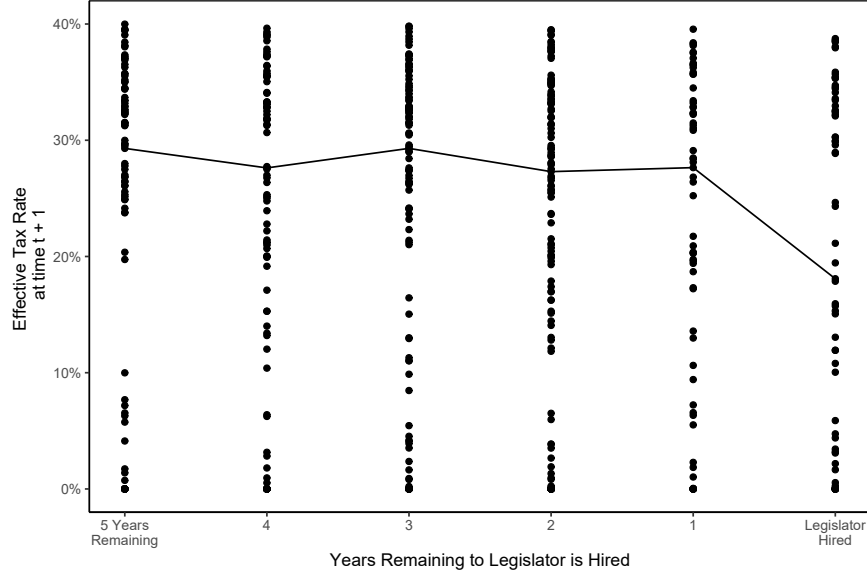
Figure 2: Corporate Tax Rate and Time Until Revolving Door Hire.

Note: Y axis is censored for presentational purposes.

In Table 1, I present a range of difference-in-differences specifications each adjusting for different potential violations of the parallel trends assumption. The first specification is the simple association between the hiring of a former MC and corporate tax rate the year after, adjusted only for firm and year fixed effects, thus imposing a global parallel trend. The coefficient suggests that for a firm with an average ETR, hiring a former legislator would lead to decrease in ETR of approximately 1.3 percentage points.[5] For a firm with the median pre-tax income, the tax saving approximately $2.4 million. The upper limit of the 95% Abadie et al. (2017) confidence interval suggests that the decrease could be as small as 0.23 percentage points. This would correspond to approximately $400,000 for the median firm. While the savings suggested by the estimates are large, they are not out of the ordinary for very large firms. For example, Magellan Health Services reported obtaining tax savings amounting to $35.7 million in 2012 by applying unrecognized tax positions (Magellan 2013). Additionally, large firms like Microsoft, Google and Pfizer save billions of dollars yearly through transfer pricing (Kiel 2020b).

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[5]Recall that the dependent variable is log transformed in the main results

One could imagine that when faced with certain types of challenges, firms hire legislators who followed political career trajectories that makes them particularly suited to help manage those challenges. This would cause a selection bias, where unobserved shocks move firms to hire certain legislators and also affect their tax rates. In column two, I include interactions between the year fixed effects and dummies for whether the legislator served in the Senate or the House. Additionally, several of the people included in the sample have served as secretaries in the administration or state governors. I interact indicators of these types of political careers as well as dummies for the number of previous political positions the revolver has had with the time fixed effects. Thereby, I apply a less restrictive identifying assumption, where the tax rates of firms that hire revolvers with the same political career trajectories are assumed to evolve in parallel. Trends among firms that hire legislators with different sets of political experiences, however, are allowed to evolve in an unconstrained fashion.

In column three, I adjust for a number of time-varying firm-level characteristics, including measures of firm size (the number of employees, enterprise value total assets, market value, and capital), operating performance (net revenue and gross income), and stock market attention (traded volume an share price). The results maintain. Coefficients on the controls are shown in Appendix E1.

In column four, I interact the year fixed effects with a set of dummies capturing the firm's sector of operation (NAICS two-digit classification). Thereby, I only assume that firms in the same sector follow parallel trends. Finally, column five includes all these adjustments, imposing the parallel trends assumption only among firms that hire revolvers with the same political careers and that operate in the same sectors.

The results maintain across these different specifications. While coefficients vary, they all indicate a strong negative impact of hiring a former MC on the firm's tax rate. Additionally, no matter whether I rely on the standard deviation of the permuted distribution or the exact P-value, all tests reject the (sharp) null at conventional levels of confidence.

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Table 1: The Revolving Door and Corporate Tax Rates

	Diff-in-Diff	MC History	Firm Covariates	Firm Sector	All Adjustments
			Dependent variable:		
			ln Tax Rate$_{t+1}$		
	(1)	(2)	(3)	(4)	(5)
Revolving Door$_t$	−0.066	−0.078	−0.134	−0.115	−0.146
	(0.029)	(0.031)	(0.044)	(0.049)	(0.053)
Exact P-value	0.031	0.018	0.013	0.043	0.02
		Placebo for Differential Pre-Trends			
Revolving Door$_{t+2}$	0.011	0.012	0.016	0.042	0.03
	(0.032)	(0.034)	(0.047)	(0.058)	(0.068)
Firm-level Covariates?	No	No	Yes	Yes	Yes
Firm FE?	Yes	Yes	Yes	Yes	Yes
Year FE?	Yes	Yes	Yes	Yes	Yes
Year FE X Chamber?	No	Yes	No	No	Yes
Year FE X Sector FE?	No	No	No	Yes	Yes
Observations	1,174	1,174	682	653	653
Residual Std. Error	0.248	0.252	0.249	0.267	0.275

Note: The dependent variable in the primary models is the natural log of the firm's tax rate with a one year lead. Standard deviations of firm-clustered permutations in parentheses. The exact, two-sided P-value shows the proportion of permuted estimates more extreme (positive and negative) than the observed estimate. 3,000 permutations used.

Two Placebo Tests

Additionally, I use two placebos. The parallel trend assumption is by definition untestable, but a violation is likely to produce differential trends in tax rates before the politician is hired. As the first placebo strategy (in the second panel of Table 1), I test whether the decision to hire revolving door personnel is correlated with the prior trend in Tax Rate. I do so by applying a two-year lead to the Revolving Door indicator. If it is correlated with the one-year lead of tax rates, it would suggest differential pre-trends, as the tax rate in previous years would predict future hires. In all specifications, the coefficient on hiring a former legislator is diminutive and statistically insignificant. The results are the same if I apply a one-year lag to the dependent variable and use contemporaneous hires. Overall, this provides reassurance that the results are not driven by pre-treatment trends.

Becoming politically connected is a specific type of corporate political strategy that the firm chooses. If firms choose this in response to particular circumstances, during which they also see reduced tax rates, this could bias the results. E.g., some managers might have a preference for aggressive tax-planning and for hiring former legislators. Hence, the managerial decision would drive both the hiring of the revolver and the reduced tax rate. In Appendix D2, I leverage that a small group of foreign firms have hired American MCs in their non-US headquarters. Therefore, those firms do not pay taxes in the US, but can be included in the dataset, because they are traded on US exchanges. If managerial decisions were behind the results, using this sample of non-American firms should yield estimates similar to the ones reported in Table 1. As I show in Appendix D2, among foreign firms, hiring an MC is associated with an *increase* in tax rates. It is important to note that the model only relies on 112 firm-year observations, and should be interpreted with caution.

Robustness Checks

In Appendix E, I conduct a number of robustness checks. I present results using the Abadie et al. (2017) clustered standard errors (E5). I show that adjusting for pre-treatment tax

rates yields the same results. In E2, I show that the baseline choice of excluding the top and bottom 2.5 percent in the distribution of tax rates yields an estimate 40 percent lower than if I had included all observations. I also show that the results in the baseline, trimmed sample are highly robust to excluding influential observations. Finally, in E3, I show that using the natural log provides conservative estimates compared to using the level of tax rate and two alternative transformations.

6 The Mechanism: Political Connections and Regulatory Enforcement

There are two turning points in the argument. First, the decrease in corporate tax rates should come about, because of a firm-level changes in the IRS's discretionary enforcement activities, not because of rule-changes. Second, the decrease in corporate tax rates should be driven by characteristics of the former legislator, not by other changes in corporate political strategy that accompany the hiring of a revolver.

6.1 Political Connections and IRS Enforcement

I investigate the effect of hiring a former legislator on the IRS's enforcement activities. Table 2 shows the association between hiring former MCs and the probability of being audited by the IRS. It also includes a test for differential pre-trends.

When firms become politically connected, they also become less likely to be audited. The year after a company hires a former MC, the IRS is approximately 15 percentage points less likely to initiate an audit of their tax returns. However, there is considerable uncertainty associated with this estimate, and the upper limit of the 95% Abadie et al. (2017) confidence interval suggests an 8 percentage points drop. Allowing for flexible trends through various adjustments in columns two through five leads to diminutive changes to the estimated coefficients. This strongly suggests that the results are not driven by these other factors.

Table 2: The Revolving Door and the Probability of Audit Initiation

	Diff-in-Diff	Chamber	Firm Covariates	Firm Sector	All Adjustments
			Dependent variable:		
			Audit Initiation$_{t+1}$		
	(1)	(2)	(3)	(4)	(5)
Revolving Door$_t$	−0.153	−0.153	−0.150	−0.170	−0.195
	(0.060)	(0.063)	(0.078)	(0.094)	(0.104)
Exact P-value	0.01	0.016	0.057	0.072	0.061
			Placebo for Differential Pre-Trends		
Revolving Door$_{t+2}$	-0.01	0.043	-0.016	-0.108	-0.067
	(0.06)	(0.063)	(0.078)	(0.094)	(0.104)
Firm-level Covariates?	No	No	Yes	Yes	Yes
Firm FE?	Yes	Yes	Yes	Yes	Yes
Year FE?	Yes	Yes	Yes	Yes	Yes
Year FE X Chamber?	No	Yes	No	No	Yes
Year FE X Sector?	No	No	No	Yes	Yes
Observations	748	748	502	487	487
Residual Std. Error	0.412	0.419	0.422	0.437	0.453

Note: The dependent variable in the primary models is an indicator for tax audit initiation with a one year lead. Standard deviations from firm-clustered permutations in parentheses. The exact, two-sided P-value shows the proportion of permuted estimates more extreme (positive and negative) than the observed estimate. 3,000 permutations used.

Estimating more complex models on fewer observations, however, predictably makes the estimates more noisy. Finally, it is important to note that coefficients in the first three of the placebo models are very small and indistinguishable from zero, statistically speaking. Including the adjustment for sectors in the final two models increases the coefficient somewhat. While the pre-trends are not close to statistical significance, the increase is a good reason to prefer the more simple difference-in-differences models presented in columns one through three.

Because I do not have data on when the MC leaves the firm again, investigating temporal dynamics in the effects is difficult and requires strong assumptions. However, in Appendix H, I present some tentative evidence that the effects are durable. It should be noted that the results in the appendix are noisy and future research should delve further into how long effects last.

6.2   How Savings Are Obtained

It is important to note that taking an aggressive position is not tantamount to tax fraud. Instead, the complexity of the tax code helps companies decrease the taxable part of their income by allowing them to apply arcane rules and precedents in new and creative ways that can be extremely difficult and costly for the authorities to challenge (Winters 2011). Oftentimes, such aggressive tax positions will take the form of claiming unrecognized tax benefits, which is a tax position that is likely to be challenged by the taxing authority. Savings obtained in this way can, of course, be overruled in the event of an audit. Importantly, however, if a company's tax return is not audited within the statute of limitations, which is three years for business returns, even unrecognized tax positions are automatically accepted. This implies that if an IRS examination could be avoided for a limited number of years, taking an aggressive tax position can yield a permanent tax saving. Anecdotal evidence suggests that avoiding examination until the expiration of the statute of limitations can yield large savings. For example, Magellan Health Services was—because of lapses in the statute

of limitations—able to obtain previously unrecognized income tax reductions amounting to $23.2 million in 2013, $35.7 million in 2012, and $10.4 million in 2011 (Magellan 2013, p. 48). While it obviously is uncertain whether the IRS would challenge the merits of these unrecognized tax benefits, the expiration of the statute of limitations made it very hard for the agency to do so.

In Table 3, I delve further into this quantitatively by looking at the financial consequences of IRS interactions.[6] First, in column 1, I investigate whether firms apply more unrecognized benefits in their tax returns. While I estimate a large increase of approximately 18%, this is associated with too much uncertainty to make any firm conclusions. The lower number of observations, and the fact that there are many ways of reducing the tax rate besides unrecognized benefits, are likely explanations for the unclear results.

When a firm is not audited, it becomes impossible for the IRS to fine it. This is another mechanism that makes avoiding audits beneficial for the firm. In colum 2, I use the amount a firm is fined by the IRS as the dependent variable, and estimate a drop of almost 30% in penalties the year after a legislator is hired. For the average firm, this amounts to a saving of $1.6 million in fines.

The statute of limitations on examining a tax return is an important reason why avoiding an audit can be lucrative. If a firm is successful in postponing an audit until the statute of limitations on a tax filing has expired, it can automatically recognize previously uncertain tax positions. In column 3, I look at the amount of dollars in unrecognized tax benefits a firm can recognize due to lapses in the statute of limitations two years after it hires a former legislator—this corresponds to the first year after the drop in audit probability. I estimate that the year after the average firm is less likely to be audited, the statute of limitations allow it to recognize 34% more of its previously uncertain tax positions. For the average

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[6]Because of limited data availability, the following results rely on fewer observations. While they should be given less weight in the overall conclusions, however, they still provide valuable insights into the interactions between firms and the IRS.

firm, this amounts to $2.2 million.

Table 3: Firm Strategy and Consequences of Enforcement Changes

	Dependent variable:		
	ln Unrecognized Benefits$_{t+1}$	ln Penalties$_{t+1}$	ln Statute of Limitations$_{t+2}$
	(1)	(2)	(3)
Revolving Door	0.183	−0.294	0.338
	(0.087)	(0.160)	(0.119)
Exact P-value	0.12	0.02	0.04
Firm FE?	Yes	Yes	Yes
Year FE?	Yes	Yes	Yes
Observations	508	330	483

Note: Dependent variable in unrecognized benefits, penalties of unrecognized benefits, unrecognized benefits recognized due to statute of limitations (all in US$ and logged). Standard deviations of the distributions of firm-clustered permutations in parentheses. The exact, two-sided P-value shows the proportion of permuted estimates (positive and negative) more extreme than the observed estimate. 3,000 permutations used.

Political Connections and Rule Changes

Previous research shows that companies can bring down their tax rates by lobbying for changes in the tax code, which grants them lucrative depreciation schedules that are tailored specifically to their asset portfolios (Richter et al. 2009). I propose, however, that political connections work differently than lobbying: the effect of the revolving door on tax rates runs through changes in enforcement activities.

To interrogate whether my results are driven by rule-changes, in Appendix G, I follow the strategy proposed by Richter et al. (2009): If firms are successful in changing the tax code, they would do so by tailoring the legislation to benefit firms with their specific asset portfolio. If so, the effect of political connections should be moderated by the firm's composition of assets. Estimating interactions with the variables previously used in the literature, I find no strong evidence that this is the case.

Additionally, if the drop in tax rates were driven by rule changes, we would expect other firms in the same sector to be affected as well. I find no evidence of any diffusion.

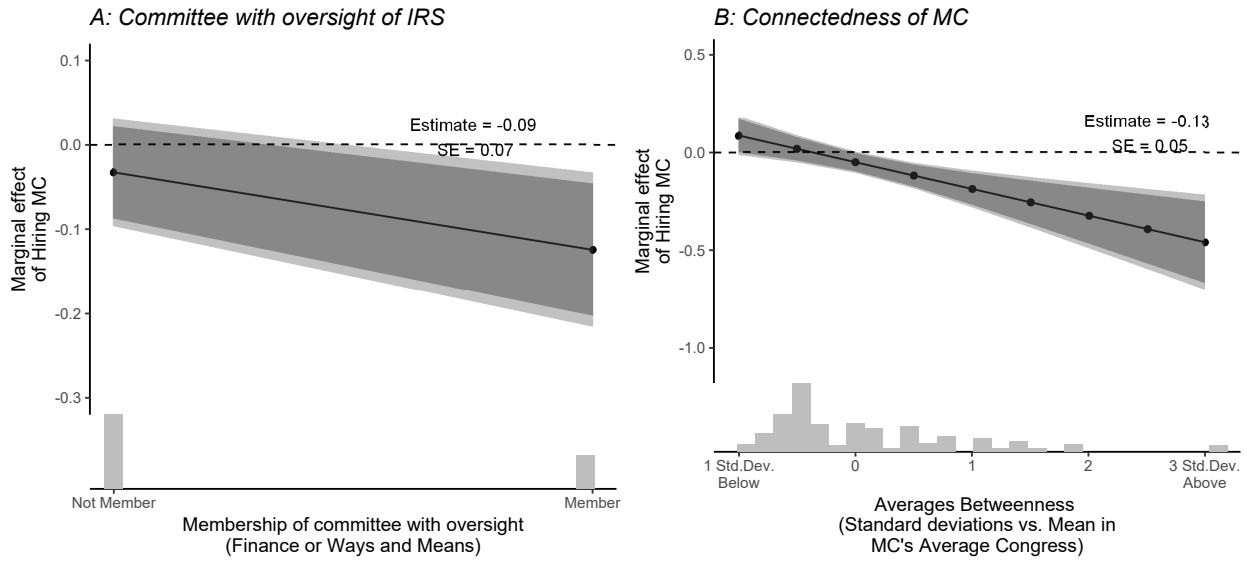## 6.3   Is Connectedness Just One of Many Non-Market Strategies?

Hiring a former MC may be one of many non-market strategies. Importantly, if these alternative strategies are used at the same time as firms hire former legislators, it becomes difficult to assess which strategy drives the effect. On the other hand, if the effect is driven by legislator characteristics, and hiring a revolver is not associated with other non-market strategies, this would suggest that the effect is driven by the former MC.

The Legislator's 'Connectedness' Drives the Association

I have previously argued that former legislators are valuable in interactions with enforcers in the bureaucracy. This is because they know whom to target through their procedural knowledge and can target them effectively through their social connections. Importantly, revolvers differ in how much of these assets they control. Here, I investigate whether the decrease in corporate tax rates is driven by legislators who either served in committees with oversight over IRS or who have extensive political connections more generally. The results are presented in Figure 3, where Panels A and B show the results from two sets of twoway interactions, while Panels C and D show a threeway interaction. Interaction coefficients and standard errors from randomization inference are printed in the top right corner of each plot.

In Panel A, I interact the dummy for the year in which a revolving door MC was hired with an indicator for whether or not she served on committees responsible for oversight with the IRS. This will provide the MC with subject specific procedural knowledge and connections. I estimate that the decrease in tax rate is 9% larger, when firms hire a former member of these committees. The marginal effect is only statistically significant for this group of former oversight committee members. The difference between groups, however, is not statistically significant at conventional levels.

A and B: Twoway interactions

A: Committee with oversight of IRS

B: Connectedness of MC

C and D: Threeway interaction

C: Connectedness - No oversight

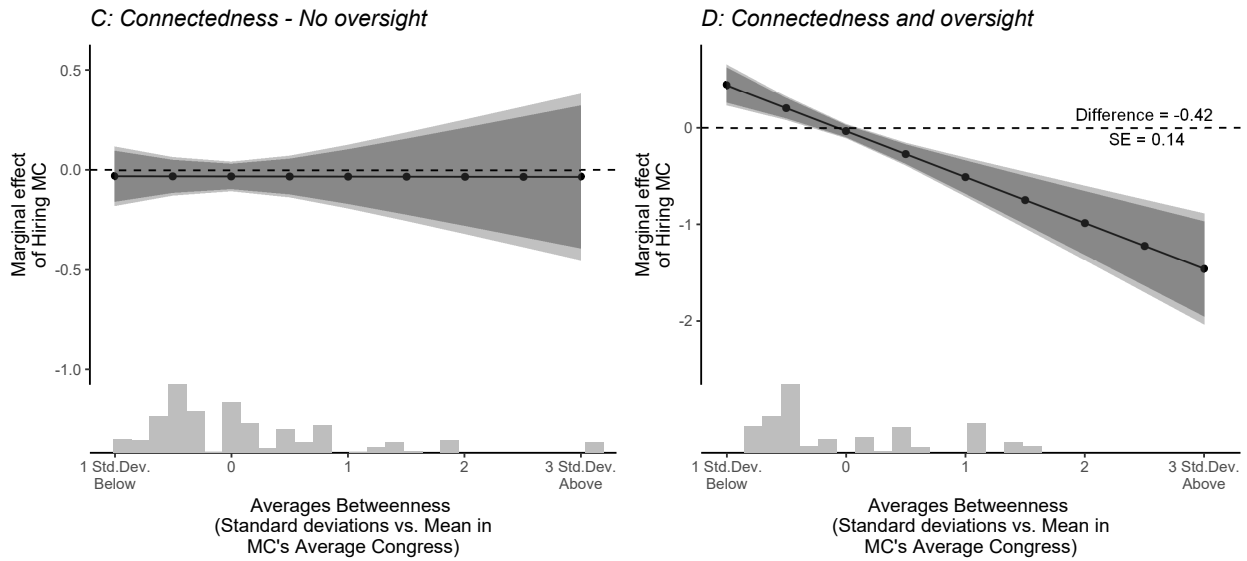*D: Connectedness and oversight*

Figure 3: Heterogeneous Effects for Well-Connected Legislators.

Note: Dependent variable is Tax Rate$_{t+1}$. Panels A and B show the marginal effect for MCs that did or did not serve on a committee with oversight of the IRS (Panel A), and across different levels of average betweenness centrality of the MC (Panel B). Panels C and D show marginal effects across different levels of betweenness centrality for MCs that did not serve on a committee with oversight (Panel C) and those who did (Panel D). All constitutive terms included in the threeway interaction. Fixed effects for firm and year included. Printed estimates are linear interaction estimates, standard errors are the variances from distributions of 3,000 random cluster-permutations. Shaded areas are panel-corrected confidence intervals, dark shaded are 90 percent, light are 95 percent.

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Second, I use the MC's average betweenness score in the cosponsorship network in Congress. Central legislators tend to be broadly involved in their party's legislative strategy. This will endow them with broadly applicable procedural knowledge and social connections. In Panel B, I show the results from an interaction between this measure and the revolving door indicator. This shows that connectedness strongly moderates the revolver's effect on the corporate tax rate. For each standard deviation above the Congress mean she has been throughout her career, her hiring decreases the tax rate by one additional percent. Looking at the marginal effect for MCs with average betweenness scores (i.e. zero in the plot), the impact is very small and statistically insignificant. However, it increases markedly and becomes significant in statistical terms at the five percent level as betweenness increases. It should be noted that there is one very outlying observations. In other models, which I do not present here, I have excluded it, which does not change the results substantively.

Finally, the MCs, who are both central in the cosponsorship network, and were members of the IRS oversight committees will control *both* broad and narrowly relevant procedural knowledge and connections. In Panels C and D, I present the results from a threeway interaction between hiring a revolving door MC, her betweenness score, and whether she served on a committee with oversight of the IRS. The interaction is highly statistically significant and shows that the moderating effect of hiring a well-connected MC increases if that MC also served on a committee with oversight. Conversely, the additional effect of hiring an MC who served on such a committee increases dramatically each time her betweenness improves by one standard deviation. Looking at the marginal effects, increasing betweenness adds close to no additional effect when the MC did not also serve in an oversight committee but adds very substantially when she did.

Having been consistently central in the cosponsorship network, or having served on the powerful Ways and Means or Finance committees will obviously be correlated with other characteristics of the former legislator. This type of legislator is likely to be 'high quality' more generally. Therefore, while these results are strong, the interactions are not

necessarily causal, in the sense that other unobserved legislator characteristics might be the 'true' moderators. Still, these results are important: At the very least, they suggest that the effect of hiring a legislator is moderated by her individual characteristics. If the results were driven by firms pursuing other non-market strategies, we would not expect this to be the case. Interestingly, in Appendix F, I estimate an interaction between hiring a legislator and her legislative effectiveness scores—an important measure of a different dimension of legislator quality—and find that it does *not* moderate the effect of hiring a MC.

Other Non-Market Strategies

Firms may pursue numerous non-market strategies simultaneously. For instance, they may lobby for tax relief (Richter et al. 2009), donate as a signal of political muscle (Gordon and Hafer 2005), or gain other forms of connections by hiring bureaucrats or congressional staff (Palmer and Schneer 2019). If firms systematically pursue these other strategies at the same time as hiring former legislators, and those alternative strategies also decrease tax rates, they can act as confounders. For example, if campaign contributions are strategic complements to hiring revolvers, tax rates would have decreased even if the former legislator had never arrived in the firm.

In Figure 4, I present the results from six regression models, each with a different measure of alternative strategies as the dependent variables and hiring a former legislator as the independent variable. The models include firm and year fixed effects. The first two models investigate different forms of lobbying, the next three[7] look into different forms of political connections, while the final one uses campaign donations as the dependent variable. The null cannot be rejected in any model, and the point estimates are all substantively small. This suggests that the association between hiring former legislators and tax enforcement cannot be explained by these alternative strategies. The null association between hiring

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[7]Since there only are three firm-years where a former CEA member is a director in my dataset, I only look into congressional staff and IRS bureaucrats separately.

legislators and lobbying is particularly interesting, because one might expect an association. However, most firms never lobby (Huneeus and Kim 2018). While there is an association between lobbying and following other political strategies very few firms pursue several forms of political activities simultaneously (Kim et al. 2020). Therefore, the lack of a statistically significant association between hiring revolvers and lobbying is likely to be driven by most firms following only one strategy at the time.
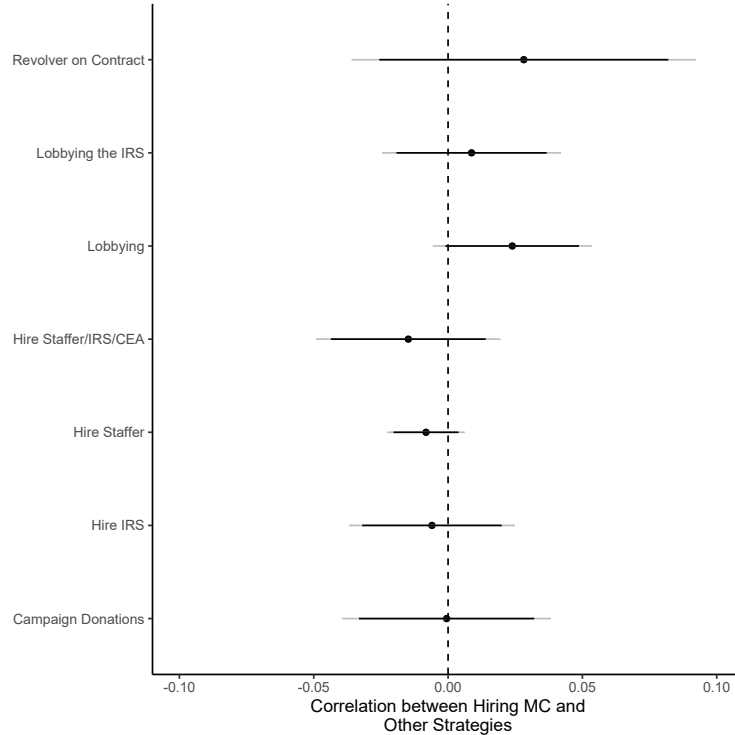

Figure 4: Hiring a former MC and Other Influence-Seeking Strategies.

Note: The figure shows contemporaneous (null) correlations between other non-market strategies and hiring former legislators. Confidence intervals are 90 percent (black) and 95 percent (gray) lines based on bootstrapped standard errors from 500 non-parametric resamples at the firm level. Firm and time fixed effects are included.

7 Discussion and Conclusion

The movement of public officials into the private sector is the object of intense research interest (McCrain 2018; Shepherd and You 2019; Strickland 2020). However, estimating

the returns to firms that hire so-called revolvers has proven elusive (Hadani and Schuler 2013). The weight of the evidence presented in this article indicates that firms can gain leniency from regulators by hiring revolvers. To substantiate this, my inquiry followed two tracks. First, I uncovered evidence that hiring a former MC decreases the probability of being audited. I also found that the connected firms were fined less by the IRS, and saw more uncertain tax positions accepted because of lapses in the statute of limitations.

Second, I investigated whether the effect could be attributed to gaining a politically connected employee, or to other non-market strategies that might be pursued simultaneously. I found that the association was driven by the most highly connected former legislators, especially if they also served on a committee with oversight of the IRS. I did not find strong evidence that firms pursue direct lobbying and political connections simultaneously. In combination, this suggests that it was hiring a revolver in itself, which impacted tax rates.

There remains some lingering questions regarding 1) mechanisms, 2) why—given the effect sizes—all firms do not hire revolvers, and 3) applicability to other contexts. I will end with a discussion of these points.

On mechanisms, I have argued that it is not necessarily meaningful to distinguish empirically between different dimensions of the human capital controlled by a revolver. It is most likely the combination of social connections and procedural expertise that make revolvers effective information extractors. Empirically, it is difficult to imagine hiring a counterfactual revolver who has one but not the other. However, an important question for future research to answer is whether the revolver takes an active role, directly extracting information about IRS enforcement, or, alternatively, whether her employment acts as a signal to enforcers (see Gordon and Hafer 2005). While both mechanisms are consistent with the evidence presented here, at least two questions remain which future research should probe. First, if hiring a former legislator is a signal to the bureaucracy, it is assumed that IRS bureaucrats know about the legislator's employment with the company. Given the effort involved in assembling the data for this article, it is not clear that IRS enforcers would have this knowledge. While

one could expect that the IRS keeps an eye on the post-elective careers of the legislators who used to be responsible for oversight with the agency, future research should present evidence on this. Alternatively, it is certainly possible that the revolver actively contacts the IRS, or that her former colleagues in Congress do so on her behalf. In this case, we need further theory and evidence on why a legislator would want to stick their neck out for the revolver. These two questions do not refute neither the active contact nor the signaling mechanism, but future research should probe them further.

One might raise the possibility of two alternative mechanisms relying on different assets controlled by revolvers: their own substantive expertise and the trust enforcers might have in them, because of their previous career in public service. I do not believe either to be likely mechanisms. First, on the expertise mechanism, previous research strongly suggests that legislators have less substantive knowledge than bureaucrats and legislative staffers (LaPira and Thomas 2017; McCrain 2018). Thus, if it were the revolver's own substantive expertise that drove the effects, it would raise the question of why the firms did not just hire these other—likely cheaper—types of revolvers. Additionally, the sample for this study includes mostly large companies that are likely to have a lot of in-house tax expertise already— quite often they even employ former IRS personnel. Furthermore, most of the companies are audited under the Large Business and International program, which requires the firm to work actively with IRS auditors during the examination of the tax returns (IRS 2016b). This makes it necessary for the companies to have large in-house tax expertise. Second, on the 'trust' mechanism, previous research suggests that agencies often base their enforcement decisions on whether they trust the firms in question (Reed 2009). From this perspective, a legislator, who used to interact with the IRS, is likely to be viewed as trustworthy by enforcers at the agency. By hiring her, the firm buys into her trustworthiness, and makes the IRS update its expectations about the firm's status as a (non-)complier. However, as I discuss previously, through backward induction IRS enforcers should realize that firms are more likely to be non-compliers when they hire a revolver. The trust based mechanism

cannot account for this.

In sum, I believe that the results show that the IRS reacts to the employment of the revolver because of her 'connectedness' broadly construed as a combination of whom she knows, and her procedural knowledge. Whether this happens because hiring revolvers is a signal, or stems from the revolver actively extracting information or pressuring the IRS, is a question left for future research.

Given the strength of the results, it is reasonable to ask why more firms do not hire revolvers. Importantly, the results show that effects are driven by the legislators, who were most central in the cosponsorship network, and who served on committees responsible for IRS oversight. Among former legislators, it is a select subset who are able to produce the effects—hiring any revolver will not do the trick.

A final question concerns whether the results are likely to travel outside of the US context. Importantly, tax enforcement is inherently complex, and requires a high degree of autonomy. This implies that bureaucrats in most contexts are afforded a lot of discretion in the enforcement of tax rules. However, some aspects of the proposed mechanisms may be more applicable in the US than others. Importantly, the legislature is far less involved in conducting bureaucratic oversight in, e.g., European countries. On one hand, this decreases the potential for pressuring bureaucrats through political connections. On the other hand, revolving door legislators may still extract information. Thus, the applicability of the results outside the US are likely to depend on the mechanisms producing the increased enforcement costs. The effects should be most externally valid if revolvers help firms improve their tax planning.

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Online appendix for: The Revolving Door and Regulatory Enforcement

Contents

A Discretion in Choosing Firms to Examine

Corporate taxes are determined based on a tax return, which is a self-determination of taxable income that is filed yearly. The IRS is responsible for auditing federal tax returns. This is done by periodically choosing companies for examination, and evaluating their tax positions. While the specific criteria for selecting returns for audit are kept secret, the Internal Revenue Manual (IRM) provides a broad outline. At the core of the process is the Discriminant Index Function (DIF), which provides a prediction of whether examining a return would yield a significant tax change (IRS 2016). The prediction is based on a comparison of the current tax return to the firm's previous returns as well as observable characteristics of historical (non-)compliers. The IRS also relies on a number of non-DIF criteria for more subjectively estimating the likelihood of non-compliance, including suspicions reported by other agencies, suspicion for illegal activities, bankruptcies, and specific issues that are current focal points of IRS enforcement (e.g., the periodic extra scrutiny of tax exempt organizations) (General Accounting Office 1999). IRS personnel manually examine the returns with a high predicted probability of being erroneous, and estimate the amount of work that would be involved if an audit of them were to be conducted, before they decide whether or not to initiate an in depth examination (General Accounting Office 1999).

B Firms and Job Types in the Sample

B.1 Firms

Below, I describe the sample of firms and job types that are included in my sample. Because the base sample restricts attention to firms with non-extreme values on the tax rate variable, we do so in the following description as well.

Table B.1 shows descriptive statistics of the finances of the firms in the sample.

Table B.1: Firm Finances

Statistic	N	Mean/Median	St. Dev.	25th Pctile	75th Pctile
Years w/ Revolver	1,489	0.08	0.271	0.000	0.000
Effective Tax Rate	1,519	27.9	19.917	0.3	35.7
$ Total Capital	1,503	3,053,155.000	1,682,341,399.000	307,148.000	21,186,681.000
$ Total Assets	1,505	5,076,412.000	2,917,315,826.000	559,620.000	40,682,696.000
$ Enterprise Value	1,397	6,461,596.000	120,556,503.000	628,690.000	32,829,844.000
# Employees	1,384	7,664.000	202,917.600	1,348.500	48,650.000
$ Net Sales	1,507	2,632,900.000	47,829,369.000	274,648.500	18,385,500.000
$ Gross Income	1,205	595,122.000	377,166,043.000	61,137.000	5,642,000.000
Turnover Volume	1,402	290,861.800	4,909,997.000	38,941.800	1,221,457.000
$ Market Value	1,407	3,559.970	1,775,743.000	422.000	26,163.970
$ Stock Price	1,407	25.690	754.724	10.545	47.560

Note: 'Years w/ Revolver' is a proportion. 'Effective Tax Rate' is in percent. 'Turnover Volume' is the number of stocks traded in a year. '# Employees' is the number of employees. 'Market Value' is in millions. 'Stock Price' is in dollars. The rest of the financial variables are measured in thousands. For the proportion of years with a revolver hired, the mean is shown. The median is shown for the rest of the variables.

B.2 Legislators

In the main text, we show the distribution of job types held by former legislators. Only five job types are represented, and corporate directorships are very overrepresented. The number of legislators-turned-directors, however, corresponds well with the estimates by Palmer and

Schneer (2016). Note also that each type of position is counted once for each MC, but that each can hold several positions. For instance, if the average MC holds upwards of two directorships, she is only counted once in the 'Board of Directors' category, but likely also holds at least one other position (e.g as a political advisor).

It is also of note that positions as in-house lobbyists includes the lowest number of former MCs. However, this is in part because of the way we define the job types, as positions in the category 'Political Advisor' are the second most numerous. The latter position includes, for instance, advising management on political affairs and directing the company's lobbying endeavors, neither of which necessarily requires directly lobbying. This contrasts with former MCs, who are now members of advisory boards, which do not need to be political in nature.

Figure B.1 shows the distribution of directorships among former legislators in the period we study. It is evident that most only hold one position on a Board of Directors. Some hold multiple, and a few hold as many as six positions. On average, former MCs hold more than two board positions, which is a bit less than what Palmer and Schneer (2016) estimate, and reflects the fact that members of the House of Representatives are included here. Table B.2 shows descriptive statistics on three political variables of the firm.

Table B.2: Legislator Characteristics and Firm Political Behavior

Statistic	N	Mean	St. Dev.	25th Pctile	75th Pctile
Average Betweenness	1,386	0.043	0.792	−0.498	0.517
Membership of Tax Committee	1,519	0.295	0.456	0	1
Lobbying the IRS	1,519	76,184.990	899,449.500	0	0

Note: Table shows firm-level aggregates. I.e. 'Average Betweenness' shows the firm's average betweenness of the revolvers it has hired. 'Membership of Tax Committee' shows the proportion of years the average firm has a member of the Senate Finance Committee or the House Ways and Means Committee. 'Lobbying the IRS' is the dollar spending with the IRS as a lobbying target.
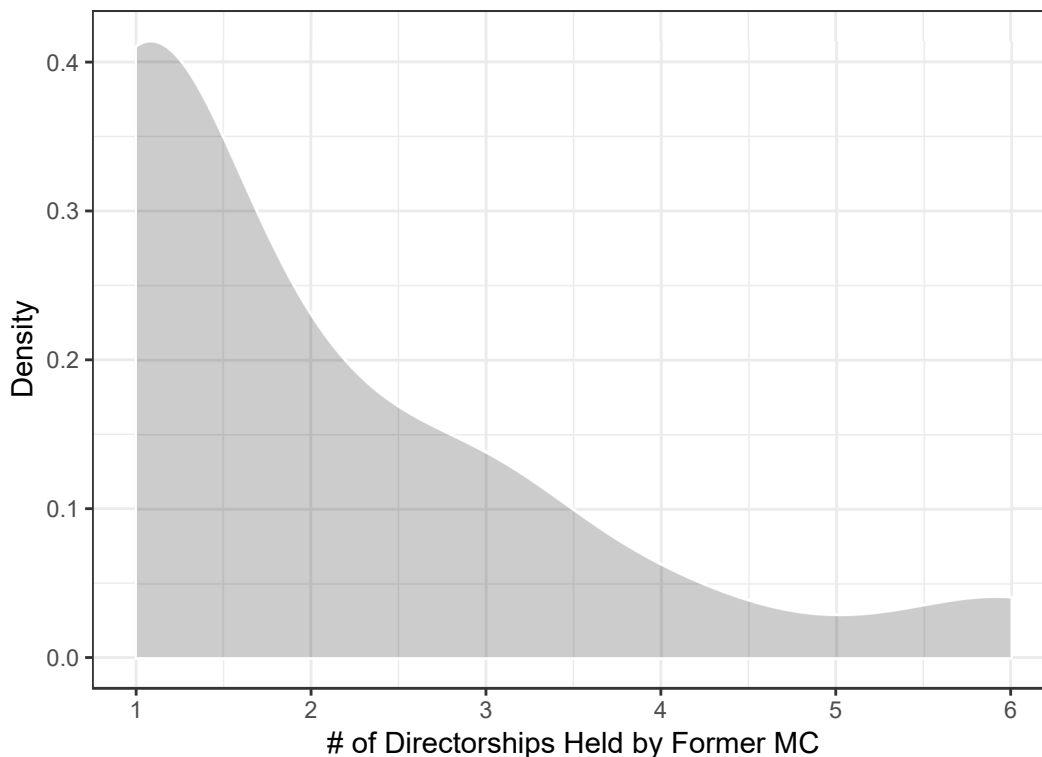
Figure B.1: How Many Directorships Do former Legislators Hold?

C   Extracting Information on IRS Audits from 10-Ks

The coding process contains two discrete phases. First, the section(s) regarding ongoing matters with tax authorities are located. Second, the information in those sections is coded with the purpose of classifying the years during which IRS audits are initiated.

Locating section(s) with information on audits:

Audit information is mostly scattered over many different sections, each detailing bits and pieces of information about tax matters. To systematically locate sections with information on audits without having to read the entire report, each 10-K is searched to locate the section(s) informing shareholders about ongoing matters with tax authorities using the following keywords: "tax au"; "tax re"; "tax po"; "tax ju"; "taxing"; "internal rev"; "irs ";

"irs)"; "i.r."; "examin"; "investig"; "review." The keywords are in many cases reduced to their stem to get general results. Often they include more than one word (e.g. in the case of taxes), to avoid too many results. If none of the keywords yield any results, the following broader search terms are used: "tax"; "audit." The organization of 10-K are highly variable between firms, but relatively stable within firms. This means that, while the individual firm does change its standards from time to time, when a relevant section is located, it is highly likely to be there the following year as well.

Classifying information on IRS audits within these sections:

When the sections on tax matters are located, the following coding rules are used to classify whether or not an audit was initiated during any given year:

Audit Initiated:

When a relevant section is located, it is normally relatively straightforward to determine whether an audit was initiated or not. We use the following rules to classify the initiation of an IRS audit:

1. Directly states that an audit was initiated (sometimes stated in another year).

2. States that the company is under audit by the IRS, and this has not been stated before, even though audits have been mentioned (e.g., by listing the years where statute of limitations have not expired).

3. If a company has been contacted by the IRS regarding an audit initiation, and it is expected to begin the following year, the following year is coded as the year of initiation if no information to the contrary arises in next year's 10-K.

4. If a company has been contacted by the IRS regarding an audit initiation, and no further information is included, the current year is coded as the year of initiation.

5. Lists the years currently being audited, and this list includes new years compared to the previous year.

6. If the report states than an audit is closed two years after the reporting year, the middle year is coded as the year of initiation. For example, if the 10-K for 2008 states that the audit of the 2006 returns has been settled, it can only have been initiated during 2007.

No Audit Initiated:

Many companies state when there are no ongoing audits. When they do not, however, proving the absence of an audit initiation is somewhat more error prone. To minimize errors, we use the following rules:

1. States that there are no ongoing audits.

2. When only old audit initiations, which were recorded in previous years, are mentioned

3. In other years it was mentioned in a specific section when an audit commenced, and that section remains unchanged with the exception that no audit initiation is mentioned.

4. When statute of limitations for federal audit (which is three years) expires three years in a row, no audit can have been initiated in the earliest year with an expiration. This is because only the three years that expired were open to examination in the first year with an expiration. Since all those years expired without being audited, no

investigation could have commenced in the first year. Consequently, the first year with an expiration is classified as "no audit initiated.". When this happens consecutively (i.e., expirations occur > 3 years in a row), the "no audit" classification is extended one year for every consecutive year with an additional statute of limitations expiration.

End of audit:

Information on endings of audits is more sparse than initiations. When there is information, however, it is typically quite clear:

1. It is stated that an audit is finalized.

2. A list of years currently under audit is presented, and one or more years are no longer present on the list compared to the previous year's 10-K.

Missing Data on Audits:

There is generally a lot of missing information before 2007, which coincides with a change in tax accounting standards.

1. No 10-K in EDGAR.

2. None of the abovementioned information (but see above on coding "No audits" for a qualification of this rule).

3. When the company is a part of the Compliance Assurance Program (CAP), where tax returns are audited automatically, in real-time.

4. If it is mentioned that the tax return for a given year no longer is under audit, but there is no way to discern when the audit was initiated, the date of initiation is coded as missing.

Figure C.1 shows the distribution of times that firms had been audited by the IRS. It shows that while most firms are not audited at all during this period, some firms are examined almost every year. On average, there is an unconditional probability of around 30% of being audited throughout the period.
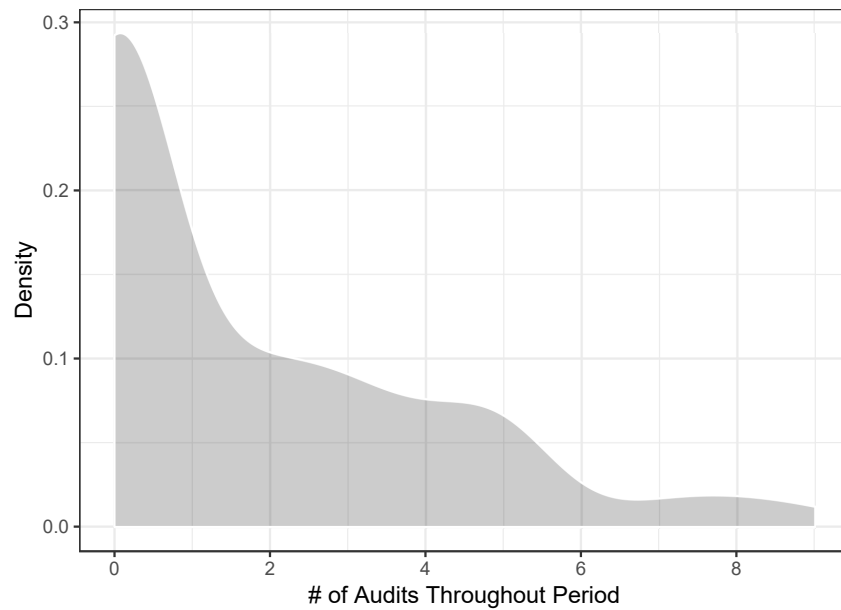

Figure C.1: How Often are Firms Audited by the IRS?

D   Additional Analyses

D.1   Correlation with Firm Economic Characteristics

In Table D.1 we investigate whether the timing of hiring a former MC is related to the firm's prior economic situation. Across the board, the coefficients are relatively small and insignificant, statistically speaking. The largest estimate is for Enterprise Value, which suggests that—under a causal interpretation—a one percent change would increase the probability of hiring a former MCs by 0.17 percentage points. A small effect, indeed. The very low R^2 values also indicate this. A Wald test for collective significance yields a P-value of approximately .8, and we cannot reject the null at any reasonable level of confidence.

This indicates that in this sample, companies do not generally choose to hire former legislators in specific economic situations.

D.2   Placebo Sample of Foreign Firms

Table D.2 shows the estimated impact of hiring American legislators among foreign firms. We estimate an *increased* tax rate, which is unprecisely estimated due to the small sample.

Table D.1: No Correlation Between Hiring and Changes in Firm Characteristics

	Dependent Variable: Revolving Door$_t$
ln Tax Rate$_{t-1}$	0.026
	(0.128)
ln Total Capital$_{t-1}$	0.033
	(0.076)
ln Total Assets$_{t-1}$	0.015
	(0.040)
ln Enterprise Value$_{t-1}$	-0.167
	(0.162)
ln Employees$_{t-1}$	0.004
	(0.030)
ln Net Revenue$_{t-1}$	-0.026
	(0.060)
ln Gross Income$_{t-1}$	0.008
	(0.015)
ln Turnover Volume$_{t-1}$	-0.004
	(0.015)
ln Market Value$_{t-1}$	0.022
	(0.024)
Share Price$_{t-1}$	0.0005
	(0.001)
Wald Stat	6.009
Wald P value	0.815
Firm FEs?	Yes
Time FEs?	Yes
Observations	818
Adjusted R^2	-0.199

Note: The dependent variable is the natural log of the revolving door indicator. Robust standard errors with firm-level clustering in parentheses.

Table D.2: Results for Sample of Foreign Firms

	Dependent variable:
	Tax Rate$_{t+1}$
Revolving Door$_t$	0.077
	(0.145)
Exact P-Value	0.609
Firm FE?	Yes
Year FE?	Yes
Observations	112
Residual Std. Error	0.346

Note: The dependent variable in the primary models is the natural log of the firm's tax rate with a one year lead. Standard errors from firm-clustered permutations in parentheses. The exact P-value shows the proportion of permuted estimates more extreme than the observed estimate. 3,000 permutations used.

E   Robustness Checks

E.1   Controlling for Pre-Hiring Tax Rates

If firms choose to hire a former legislator, when they pay high tax rates, we could expect a selection effect based on the dependent variable. While the results in Table D.1 suggests that this is not the case, we still test the robustness of the main results by running a set of models, where we control for the lagged (pre-treatment value) of the tax rate. The results are shown in Table E.1. They also allow for closer inspection of the other covariates in the models, which we add incrementally in columns one through four. It is clear that the main results are not driven by pre-treatment tax rates. While randomization inference is certainly a better way of estimating uncertainty, the table also shows the robustness of the findings

to the use of another type of standard error.

Table E.1: Controling for Pre-Hiring Tax Rates

	Bivariate	Assets	Performance	Attention
			ln Effective Tax Rate$_{t+1}$	
	(1)	(2)	(3)	(4)
Revolving Door$_t$	−0.076**	−0.092***	−0.143***	−0.134***
	(0.030)	(0.034)	(0.038)	(0.038)
ln Tax Rate$_{t-1}$	−0.282***	−0.314***	−0.249***	−0.264***
	(0.076)	(0.070)	(0.078)	(0.079)
ln Total Capital$_{t-1}$		−0.021	0.126	0.104
		(0.049)	(0.080)	(0.082)
ln Total Assets$_{t-1}$		−0.021	−0.066	−0.083*
		(0.015)	(0.041)	(0.045)
ln Enterprise Value$_{t-1}$		−0.163	0.006	−0.044
		(0.112)	(0.147)	(0.173)
ln Employees$_{t-1}$		0.006	0.053*	0.052*
		(0.028)	(0.030)	(0.030)
ln Net Revenue$_{t-1}$			−0.182**	−0.150*
			(0.086)	(0.087)
ln Gross Income$_{t-1}$			−0.076	−0.074
			(0.099)	(0.099)
ln Turnover Volume$_{t-1}$				0.019
				(0.014)
ln Market Value$_{t-1}$				0.026
				(0.022)
Share Price$_{t-1}$				0.0002
				(0.001)
Pre-Trend	0.011	0.011	0.007	0.004
	(0.021)	(0.026)	(0.032)	(0.032)
Observations	999	874	685	682
Company FEs?	Yes	Yes	Yes	Yes
Year FEs?	Yes	Yes	Yes	Yes

Note: The dependent variable is the natural log of the firm's tax rate with a one year lead. Beck-Katz panel corrected standard errors in parentheses. *; ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively

E.2 Robustness to Outlying Observations

In the results presented in the main text, we exclude the top and bottom 2.5% in the distribution of tax rates. This is because there are a number of very extreme observations, paying (sometimes negative) tax rates of several hundred percent. These observations are likely to represent either measurement errors on the part of Datastream or simply highly unrepresentative observations, and will carry weight in estimating average effects which can destabilize the estimates.

The first row of Figure E.1, presents the most simple difference-in-difference specification. In the second row, we estite the same model, but include the entire dataset. Including extreme observations inflates the estimate by approximately 40% and the confidence intervals by almost the same factor. This shows that while we would obtain the same substantive conclusions using the full sample, using it would provide larger, probably more wrong, and certainly more unstable estimates.

To make sure that the results in the trimmed baseline sample are not driven by potentially remaining extreme observations, Figure E.2 shows the distribution of estimates, where each observation is left out in turn, and the baseline specification is reestimated. This yields an average estimate that is very close to the baseline, and a very concentrated distribution of estimates. This suggests that the experiences of individual firms that are not representative of the wider sample do not skew the effects.
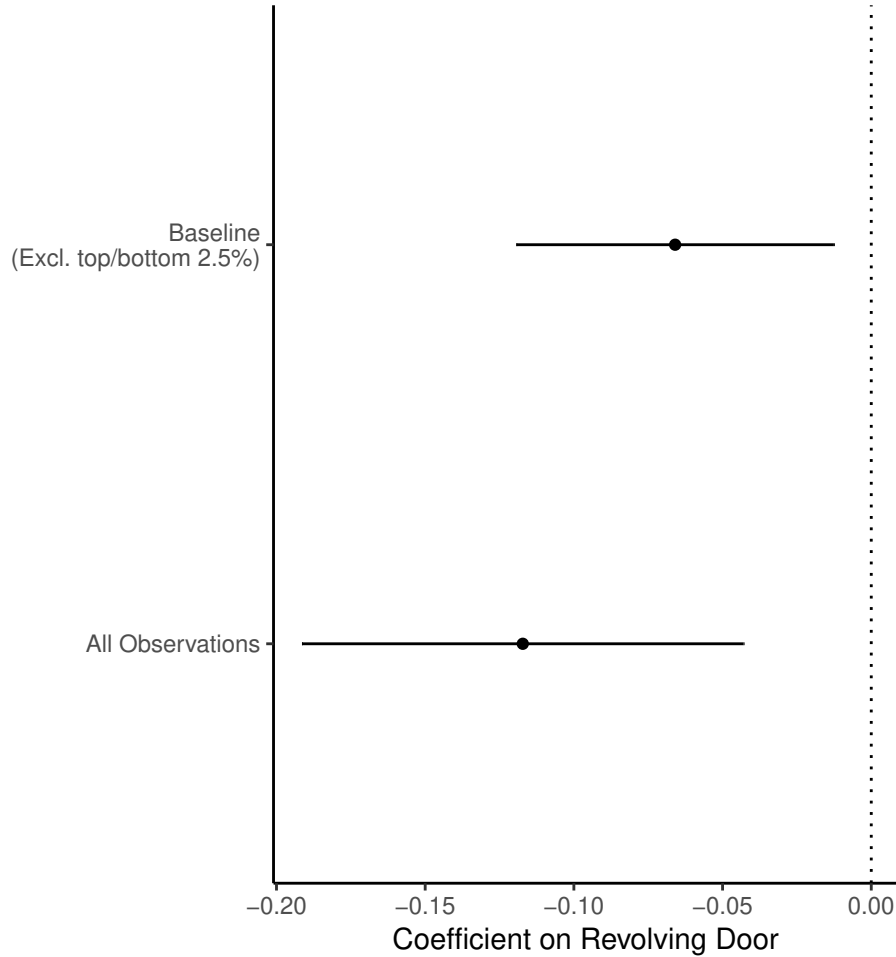
Figure E.1: Robustness of the Results to Including Extreme Observations

Note: Confidence intervals are +/- 1.96 firm-clustered permuted standard deviations. 3,000 permutations used. Firm and year fixed effects included.

E.3   Robustness to Choice of Transformation

Table E.2 shows the estimated coefficient on a revolving door hire for three different choices of transformations of tax rate. In columns one and two, we use an untransformed version with and without controls, respectively. As we can see, even in this mean-trimmed sample, the estimates using tax rate in levels are very large, amounting to approximately three and seven percentage points, respectively. Recall that the baseline results, using the natural log, indicated a decrease for the average firm amounting to 1.5 percentage points. Thus, the
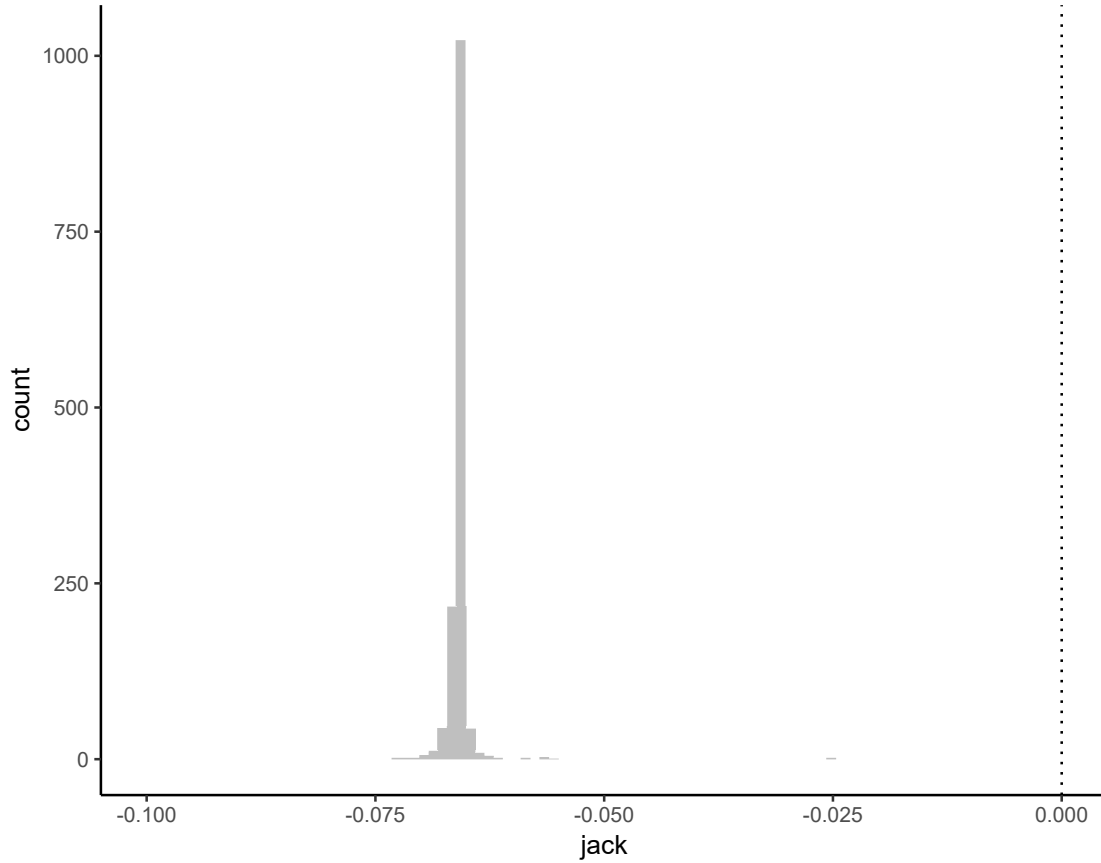
Figure E.2: Distribution of Leave-One-Out Estimates

Note: Histogram of jackknifed distribution of estimates. 100 bins used.

results when using the level of tax rates is double the size of the estimate using the natural log. This indicates that some form of transform is warranted to minimize the influence of extreme observations.

The models in columns three through six have as the dependent variables different transformations that behave like the logarithm but allow for negative values. In colum three and four, we use the bi-symmetrical logarithm (Webber [2012]), and in columns five and six, we transform tax rates using the inverse hyperbolic sine (IHS) (Bellemare and Wichman [2019]). There is more uncertainty around these estimates, but the results maintain thoughout all these stress tests. This indicates that our main findings are not driven by

the choice of transformation. Additionally, the alternative transformations all yield very substantially larger estimates—double and four times the size, respectively, for the IHS and bi-symmetrical transformations. This indicates that the baseline of adding a constant and log-transforming provides conservative estimates.

Table E.2: Robustness to Choice of Transformation

	Levels	Levels	Bi-Sym	Bi-Sym	IHS	IHS
	(1)	(2)	(3)	(4)	(5)	(6)
Revolving Door	−3.188**	−7.108***	−0.280	−0.632**	−0.116*	−0.257***
	(1.522)	(2.161)	(0.191)	(0.259)	(0.062)	(0.085)
Exact P-value	0.03	0.001	0.146	0.015	0.059	0.003
Covariates?	No	Yes	No	Yes	No	Yes
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes
Year FE?	Yes	Yes	Yes	Yes	Yes	Yes
Year X Chamber FE?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,174	682	1,174	682	1,174	682
Residual Std. Error	13.428	12.994	1.649	1.583	0.546	0.522

Note: The dependent variable in the primary models is different transformations of the firm's tax rates with a one year lead. Standard errors from firm-clustered permutations in parentheses. The exact P-value shows the proportion of permuted estimates more extreme than the observed estimate. 3,000 permutations used.

In Table E.3, we reproduce results in the interactions between hiring a former legislator and the person's connectedness using the three alternative transformations of the dependent variable. The results on the interaction between Revolving Door and Betweenness as well as the three-way interaction, which additionally includes an indicator of whether the revolver served on an oversight committee, are all highly robust across the different specifications. When it comes to the two-way interaction between Revolving Door and Oversight, the coefficients are generally of a similar (or larger) size compared to the baseline results. They are all estimated with significantly less noise and are all statistically significant at conventional

levels.

Table E.3: Robustness of Interactions to Choice of Transformation

					Dependent variable:				
					Audit Initiation				
	Levels	Levels	Levels	Bi-Sym	Bi-Sym	Bi-Sym	IHS	IHS	IHS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Revolving Door	−2.071	−2.379	−1.967	−0.159	−0.143	−0.144	−0.074	−0.077	−0.068
	(1.744)	(1.578)	(1.806)	(0.239)	(0.193)	(0.242)	(0.077)	(0.064)	(0.079)
Revolving Door X Oversight	−3.074**		−1.803	−0.264*		−0.081	−0.108**		−0.052
	(1.203)		(3.654)	(0.138)		(0.385)	(0.046)		(0.137)
Revolving Door X Betweenness		−4.713***	0.570		−0.594***	−0.034		−0.198***	−0.0005
		(0.701)	(1.242)		(0.077)	(0.135)		(0.027)	(0.048)
Revolving Door X Oversight X Betweenness			−13.191***			−1.409***			−0.497***
			(3.556)			(0.411)			(0.138)
Firm FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year X Chamber FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,174	1,072	1,072	1,174	1,072	1,072	1,174	1,072	1,072
Residual Std. Error	13.435	13.410	13.324	1.649	1.615	1.607	0.546	0.541	0.537

Note: The dependent variable in the primary models is the one-year lead of tax rate in levels and with with different transformations. Standard errors from firm-clustered permutations in parentheses. *; ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively. 3,000 permutations used.

E.4 Intensive Margin of Lobbying and Donations as Alternative Strategy

To simplify the presentation, I only examine the extensive margin of lobbying and donation activity in the main paper. In this appendix, I show the association between hiring revolvers and the intensive margin of lobbying and donations. That is, in Table E.4, the dependent variables are the log of the donated amount and the amount spent on lobbying, respectively. The results are similar to the ones presented in the main paper.

Table E.4: Limited Evidence of Relationship between Hiring Revolvers and Intensive Margin of Political Presence

	Dependent variable:	
	ln Donated Amount	ln Lobbied Amount
	(1)	(2)
Hire Revolver	−0.060	0.135
	(0.249)	(0.230)
Observations	1,141	1,141
Residual Std. Error	1.930	1.932

Note: Robust standard errors with firm-level clustering in parentheses. A constant of 1 is added to both dependent variables.

E.5 Alternative Standard Errors

The table below presents the results with alternative design-based standard errors. In particular, I use the recently developed Abadie et al. (2017) clustered standard errors. The authors of the paper find that sampling-based clustered standard errors are conservative when applying a design-based approach to uncertainty. The bias increases with the observed proportion of the population of treated units, and the within-unit treatment effect heterogeneity. Abadie et al. (2017) show how to correct the bias in the ordinary clustered standard errors. Below, these standard errors are implemented. In the model with ETR as the dependent variable, the permuted estimates in the main paper are approximately the same. With audit initiation as the dependent variable, the permutation test is conservative.

Table E.5: Results with Abadie et al Clustered Standard Errors

	Dependent variable:	
	ln ETR	Audit
	(1)	(2)
Revolving Door	−0.066**	−0.153***
	(0.026)	(0.030)
Firm FE?	Yes	Yes
Year FE?	Yes	Yes
Observations	1,286	748

*Note: The dependent variables in the primary models are measured with a one-year lead. Abadie et al. (2017) Standard errors with firm-clustering in parentheses. *; ** and *** indicate statistical significance at the 10%, 5% and 1% levels, respectively.*

E.6 Missing Data on Covariates

There are quite a few missing observations on the covariates included in the regression in the main paper. While part of the missingness arises, because the covariates are lagged to avoid post-treatment bias, some of it is due to missing data in Datastream.

I believe that we should place most confidence in the simplest estimates without controls, missing data on the covariates could still bias the estimates when including controls. While those estimates mainly serve to show the robustness of the main finding, severe bias would still be a concern. To gauge the extent of bias induced by non-random missingness, I investigate how strongly the arrival of a revolver correlated with non-available data on the covariates.

Table ?? shows the results from a regression of the arrival of a revolver on a set of dummies for each of the covariates capturing whether the firm has a missing value on the covariate.[1] Positive (negative) coefficients indicate that missingness is higher (lower) in the year where the revolver arrives.

While there are some sizable coefficients (namely on missingess of capital, total assets, turnover and market value) none reach standard thresholds for statistical significance. This suggests that while we cannot dismiss concerns about missing data entirely, it is unlikely that it will severely bias the results.

Because the arrival of a revolver quite rarely coincides with missing data, I cannot estimate robust standard errors on the OLS regression. To ensure that the null result is not driven by this, column two shows the results from a logistic regression.

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[1]Note that the coefficient on the missingness of stock prices could not be estimated, because the variable has no missing data in the years where revolvers arrive

Table E.6: Does Missingness Correlate with Revolver Arrival?

	Dependent variable:	
	Revolver	
	OLS	*logistic*
	(1)	(2)
Missing Capital	−0.086	−14.183
	(0.274)	(2,399.545)
Missing Assets	0.074	14.032
	(0.281)	(2,399.545)
Missing Enterprise Value	−0.037	−0.487
	(0.050)	(0.676)
Missing Employees	0.012	0.153
	(0.030)	(0.388)
Missing Sales	−0.042	−13.616
	(0.093)	(692.689)
Missing Income	0.008	0.107
	(0.018)	(0.232)
Missing Turnover	−0.083	−14.156
	(0.157)	(1,384.767)
Missing Market Value	0.115	14.570
	(0.164)	(1,384.767)
Constant	0.079***	−2.462***
	(0.008)	(0.114)
Observations	1,489	1,489
R^2	0.002	
F Statistic	0.289	

Note: *p<0.1; **p<0.05; ***p<0.01

F   Interaction with Legislative Effectiveness Scores

In the main text, we interact the revolving door indicator with measures of cosponsor centrality and service on committees with oversight of the IRS. While these are good measures of the legislator's connectedness, they almost certainly map onto a broader notion of the MC's 'quality', and thus do not capture connectedness alone.

To probe this further, we estimate an interaction between the revolving door indicator and the Volden and Wiseman (2014, 2018) legislative effectiveness scores. This is interesting, because it captures a different dimension of legislator quality; specifically the MC's ability to not only introduce a large number of bills but also make sure they progress through the legislative system. As in the main results, we average the effectiveness scores over the tenure of the MC.

Interestingly, there is very little evidence of an interaction with becoming politically connected. If anything, we observe the reverse pattern, where highly effective legislators are less adept at decreasing tax rates of their new employers.

G   Political Connections and Rule Changes

Previous research by Richter et al. (2009) shows that companies can bring down their tax rates by lobbying for changes in the tax code, which grants them lucrative depreciation schedules that are tailored specifically to their asset portfolios. Our claim, however, is that hiring revolvers represents a form of political activity very different from lobbying. Specifically, we propose that the effect of the revolving door on tax rates runs through changes in enforcement activities. Consequently, if hiring revolvers decreases tax rates, because they
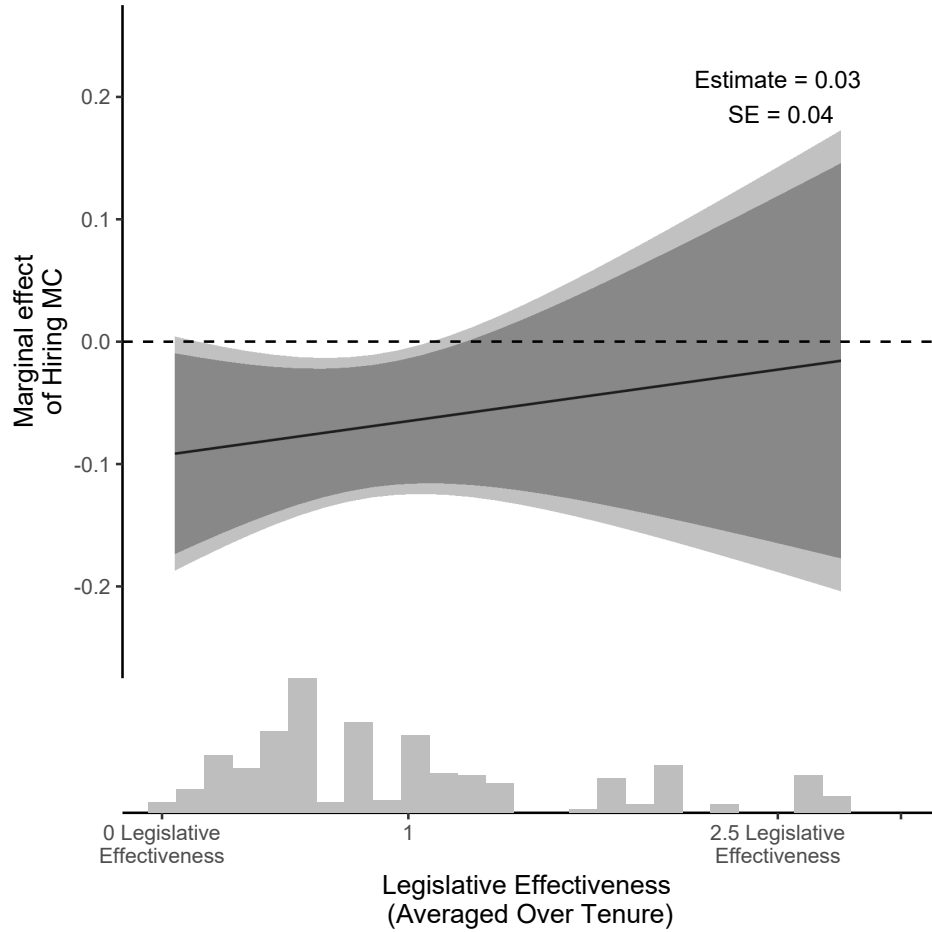
Figure F.1: Interaction with Legislative Effectiveness Scores. *Note: Confidence intervals are from panel-corrected standard errors.*

are able to bring about changes in the tax code, it would contradict our proposition.

The analysis in the preceeding section provided one indication that the results presented here were not driven by changes to the tax code. In this section, we investigate the possibility in two additional ways. First, if the drop in tax rates is driven by general rule changes, we would expect similar firms to experience it as well; it should not be concentrated with the company that gains the connection alone. We examine this by constructing a spatial weights matrix, where firms within the same sector are defined as neighbors. We use this weights matrix to spatially lag the independent variable. If the coefficient on this spatial lag of

the Revolving Door variable is negative as well, it would indicate that gaining a connection decreases not only the firm's own tax rate but also that of other companies in the sector. The coefficient is positive (but statistically insignificant), indicating that, if anything, gaining a connection increases the tax rates of other firms in the same sector. The estimate is small, however, only amounting to less than half the absolute size of the baseline estimate.

Second, we follow Richter et al. (2009) and interact the indicator for hiring a revolving door MC with a number of firm-level characteristics capturing an array of common asset types that depreciation schedules could be aimed at. We use capital intensity (the ratio of fixed to total assets), size (total and fixed assets), and returns on assets (the ratio of pre-tax income to total assets). The only statistically significant moderator used by Richter et al. (2009) that we do not investigate is R&D intensity, since we could not acquire data on it. We also add the number of employees as a moderator. If politically connected firms are able to get decision makers to implement changes to the tax code that would benefit firms with their portfolio of assets, we would expect negative and statistically significant interaction terms (this is the line of argument in Richter et al. 2009). Figure G.1 shows the coefficients on the interaction terms. As we can see, none of the interaction terms are statistically significant. In addition, they are very small compared to the baseline effect presented in the main paper.

Changes in the tax code are extremely difficult to observe, and even harder to attribute to a company's political activities. The strength of these two approaches is that they allow us to assess, whether the patterns in tax rates differ in a manner consistent with broad rule changes without actually having to observe such changes. The analysis provides limited evidence of the effect of hiring a legislator working through rule changes.
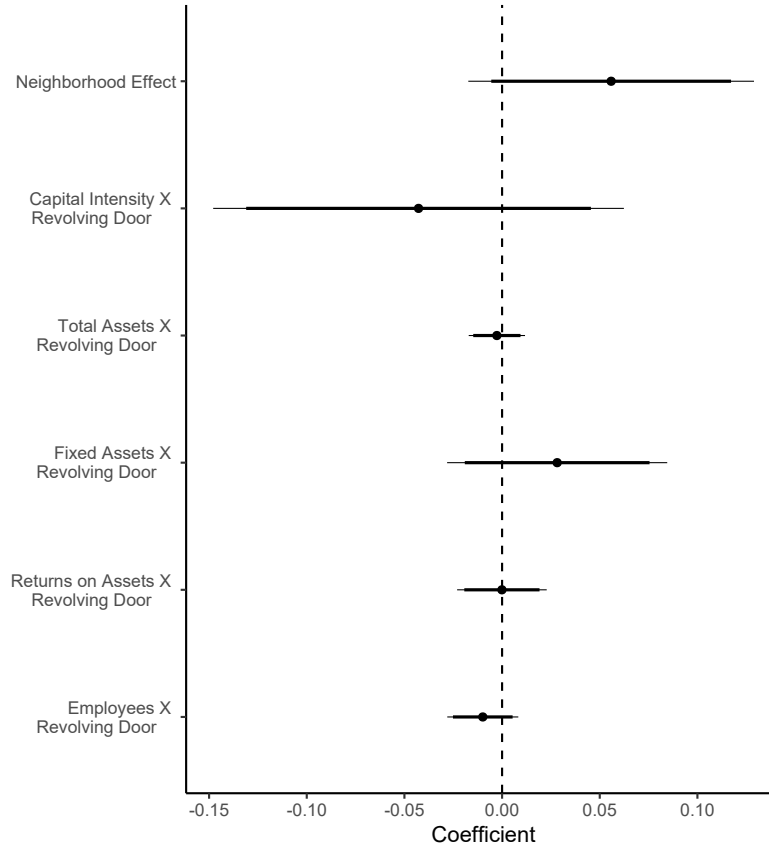
Figure G.1: Limited Evidence of Rule Changes Driving the Effect.

Note: The dependent variable is ln Tax Rate$_{t+1}$. Coefficients show 1) the effect of hiring an MC on the tax rates of other firms in the same sector. 2) The estimated interactions between an array of firm characteristics and the revolving door dummy. All moderating firm characteristics are logged. Each interaction is estimated in a separate model. Lines are +/- 1.96 (thin) and 1.65 (thick) standard deviations of permuted distributions. 3,000 firm-clustered permutations are estimated.

H Robustness to Staggered Treatment and Temporal Dynamics

Two important questions remain. First, as shown by Goodman-Bacon (2018) the two-way fixed effects estimator yields a biased estimate of the ATT if already-treated groups enter the control group and effects vary over time. Second, we would like to know how durable the effects are. While recent techniques deal with both the Goodman-Bacon bias and allows

estimation of the dynamic effects (Callaway and Sant'Anna 2021; Imai et al. 2019a,b), due to data quality we need to apply additional assumptions to identify the latter. In particular, I was not able to collect data on the timing of the revolver's departure from her position in the firm. Therefore, if we are to estimate how the effect varies over time, we have to make an assumption about how long the revolver remains in the firm. If certain types of revolvers systematically leave before others, this will bias the estimates. In particular, we could imagine that revolvers that do not manage to produce a tax decrease leave sooner. This would introduce a downward bias. In this appendix I first estimate dynamic treatment effects with that bias present using techniques proposed by Callaway and Sant'Anna (2021). I then use BoardEx to collect additional data on board of directors specifically, where it is know when the revolver leaves the firm, allowing me to present better estimates of the longevity of the results.

H.1 Event Study Assuming Permanent Treatment

H.2 presents results from the Callaway and Sant'Anna (2021) estimator of the event study difference-in-differences design assuming that firms are permanently treated once the revolver arrives. It allows me to gauge pre-treatment differential trend for a longer period, and to examine how treatment effects differ over time. It is important to note that the latter is likely to be biased.

Looking first at the estimated effects on tax rates (Panel A), there is no discernable pre-trend. The drop in ETR sets in with a one-year lead, and it is slightly larger in size compared to the estimate presented in the main paper. This is to be expected, as Goodman-

Bacon (2018) bias is downward, and barring sign change will be towards zero. The estimate is larger than any other change observed during the period. It is statistically insignificant, which is not surprising, because the technique used here will be much more demanding than using fixed effects regression. The take-away, therefore, is that the estimate presented in the main paper is robust to issues with staggering.

Looking at the estimates on audit probability in Panel B, there seems to be an increase at time 0. It is difficult what to make of this, as that result does not appear with when using Imai et al. (2019a) nor fixed effects regression. Thus, it may be a peculiarity with the particular technique. However, it could also indicate a pre-trend. To ensure that my results are not driven by a potential pre-trend, in Panel C, I match on pre-treatment audit probability, using inverse probability weighting. This removes the contemporaneous increase, while the drop at time $t+1$ remains. The fact that the drop remains – and still sets in with a one-year lag – reassures me that the findings are not driven by a differential trend.

H.1.1 Robustness to Goodman-Bacon (2018) Bias

To further probe the robustness of the difference-in-differences estimate, Figure H.3 presents an estimate that is more directly analogous to the ones presented in the paper, which only look at the effect immediately after hiring. As we can see, the estimated effect of connections on audit probability remains large and statistically significant. The impact on tax rates is very close to the estimate in the main text – 0.069 in this table versus 0.066 using two-way fixed effects. This is strong evidence that the baseline estimate is not severely biased. However, as discussed above, the Imai et al. (2019a,b) technique is less statistically efficient, and the estimate, thus, is less precise. Overall, however, this suggests that the results are
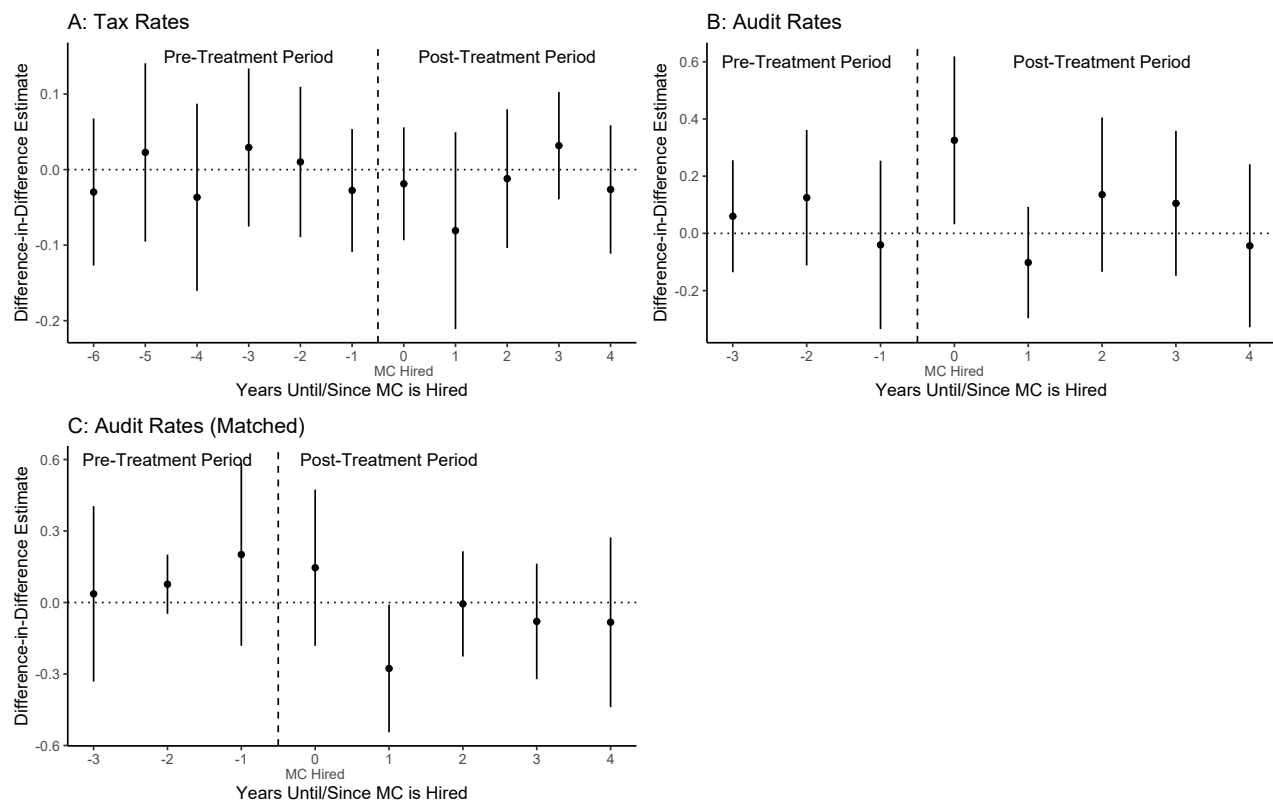
Figure H.2: Event Study Difference-in-Differences. *Notes: Estimates of dynamic treatment effects from the Callaway and Sant'Anna (2021) technique. Firm-clustered 90 % confidence intervals are bootstrapped with 1,000 trials. Panel C matches firms on pre-treatment audit probability.*
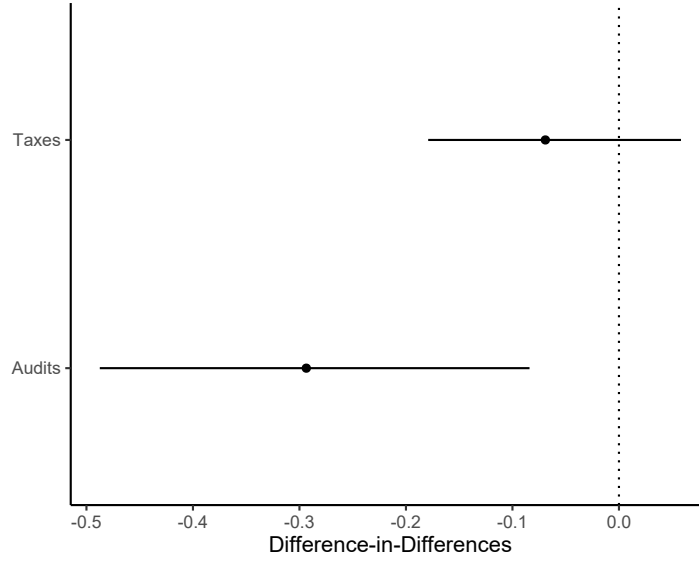
robust to Goodman-Bacon bias.

Figure H.3: Robustness of Two-Way Fixed Effects.

Note: The dependent variables are, respectively, IRS audits and the firm's tax rate. Estimates are obtained through the Imai et al. (2019a,b) difference-in-differences estimator. Firms are matched on pre-treatment values of their dependent variables and on the electoral cycle. Estimates without adjusting for covariates are similar in terms of statistical significance but smaller in size.

H.2 Event Study on Connected Boards

The estimates of the dynamic treatment effects presented in the previous section are likely to be biased. To better gauge the longevity of the results, I use BoardEx to collect data on legislators that take positions on corporate boards of directors. This is only a subset of the positions that are analyzed in the main results, but the data will allow me to examine how long effects last for an important type of position. I collect data on effective tax rates (ETR) from Compustat and match this with the BoardEx data. I then estimate the difference-in-differences at varying time-horizons after a former legislator becomes a board member at the firm. To estimate this, I use the Imai et al. (2019a,b) technique which handles the

Goodman-Bacon (2018) bias while also allowing firms to move out of the treatment group. I exclude firms that never appoint a revolver to their boards.

The results are presented in Figure H.4, where firms are matched on pre-treatment dependent variables and income. The results are similar but more noisy without matching. As remarked upon, the new difference-in-differences estimators (e.g. Callaway and Sant'Anna 2021; Imai et al. 2019a) are extremely greedy in terms of power. The matching helps with this.

Panel A shows the evolution of the difference-in-differences estimates after the legislator arrives at the board. We can see that the estimates are consistently negative. Averaging the estimated ATTs for the whole period shows that same picture.

Since the estimates are somewhat noisy, Panel B provides an additional test, where I examine the effect of a legislator leaving the board. As we can see, this increases the firm's ETR during the first year after the revolver leaves.

In Panel C, I examine effects on audit probability. Since the audit data was hand-collected for a different group of firms, it is quite incomplete. Therefore, the result should be interpreted with caution. However, it also suggests durable decreases in audit probability after the former legislator arrives in the firm.
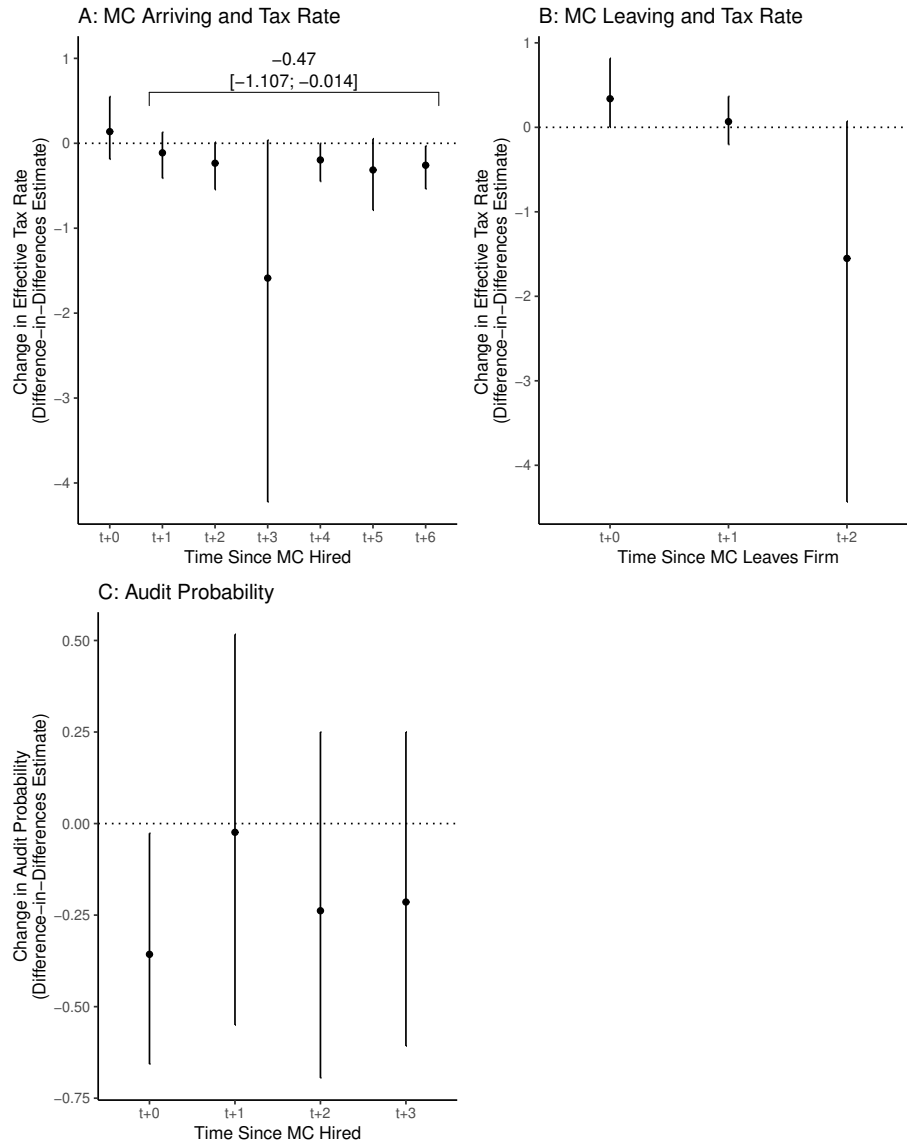
Figure H.4: Event Study on Politically Connected Boards of Directors. *Note: Estimated dynamic treatment effects using the Imai et al. (2019a,b) technique. Firm-clustered confidence intervals are 90% bootstrapped with 1,000 trials. Firms are matched exactly on treatment history to ensure that no treated firms enter the control group. They are also matched on pre-treatment levels of the dependent variable and income using Mahalanobis distance.*

I Heterogeneity by Position in Firm

Effects may differ depending on the revolver's role in the firm. As such, it is reasonable to expect that revolvers who are more closely attached to the firm, and who work directly with non-market strategy would be associated with the largest effects.

In Figure I.5 I explore this, by allowing for different effects depending on the revolver's position. The general pattern is the same for both ETR and audits, although the estimates for the former are more noisy. Lobbyists are associated with the largest effects, followed by directors and revolvers in c-suite positions. Interestingly, revolvers on more loosely associated boards (e.g. advisory boards) are associated with more audits and higher ETR.
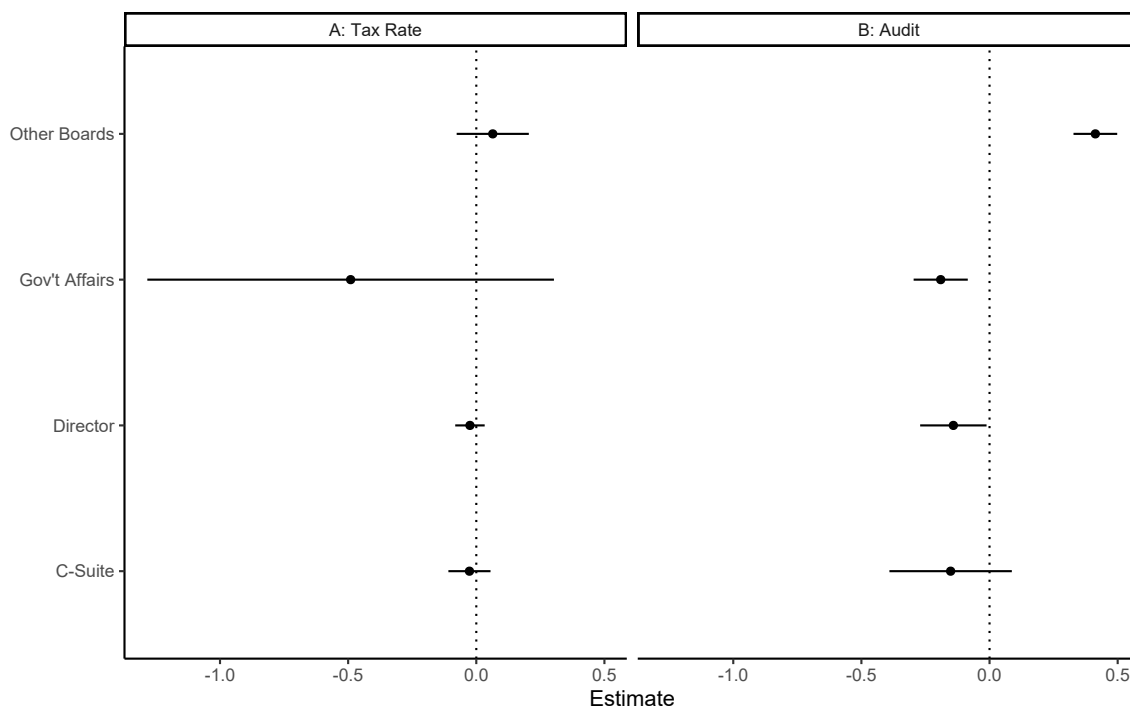

Figure I.5: Heterogeneity by the Revolver's Position in the Firm. *Note: The figure presents separate estimates for each different type of position observed in the dataset. Logged tax rates and audit initiation are the outcome variables in Panels A and B, respectively. Lines are 90% confidence intervals computed using firm-clustered robust standard errors. Firm and year fixed effects included in both models.*

J   Revolving Door Contract Lobbyists

In this appendix, I examine the effect of hiring revolvers as contract lobbyists instead of in in-house capacities. Table J.1 presents the results. In column 1, we can see that hiring revolvers as contractors decreases the firm's tax rate. However, from column 2, we can see that it does not affect the firm's audit probability. These results are interesting, as they suggest that firms can use revolvers as contract lobbyists to bring down their tax bill. However, this does not seem to be associated with less regulatory interaction. This resembles previous research on the topic, suggesting that lobby efforts may shape the tax code (Richter et al. 2009). This result could suggest that revolver contract lobbyists play an important role in those lobby efforts.

Table J.1: Contracting with Revolver Lobbyists and Corporate Tax Rates

	Dependent variable:	
	ln Tax Rate	IRS Audit
	(1)	(2)
Contract w. Revolver	−0.120***	0.014
	(0.038)	(0.045)
Firm FE?	Yes	Yes
Year FE?	Yes	Yes
Observations	1,310	632
Residual Std. Error	0.273	0.408

Note: The dependent variables are included with a one year lead. Robust standard errors with firm-level clustering in parentheses.

K   Sensitivity Analysis

In this appendix, I examine how sensitive the findings are to unobserved confounding. To do so, I conduct a sensitivity analysis, using the `tipr` package for R (McGowan 2021). In this approach, I investigate how large a confounder would have to be to render the estimate statistically insignificant.

Figure K.6 shows the results with ETR as the dependent variable. In Panel A, I investigate how strongly associated the confounder would have to be with ETR and hiring revolvers for the estimate to become insignificant at the 10% level. For reference, I plot three associations. First, since lobbying is shown, because it is the strategy most strongly associated with hiring. Second, I show total assets, since it is most strongly associated with hiring. Finally gross income is also shown, as it resembles most other associations.

I find these results reassuring for two reasons. 1) The results are robust to confounding in the sense that of all the things we observe, nothing is close to removing statistical significance. Even though it seems like total assets is close to being able to remove the statistical significance of the results, we would need 16 variables with the same associations for this to happen. This does not show that the results cannot be driven by confounding – they simply show that the estimates are not very sensitive to it. Importantly, the fact that nothing observable comes close makes it less likely that such a confounder exists. 2) While it is unlikely that the unobserved confounders would completely explain the results, they might still imply that the true effects may be smaller than the ones presented in the article. But even in the presence of quite strong confounding, the effects would still be meaningful.

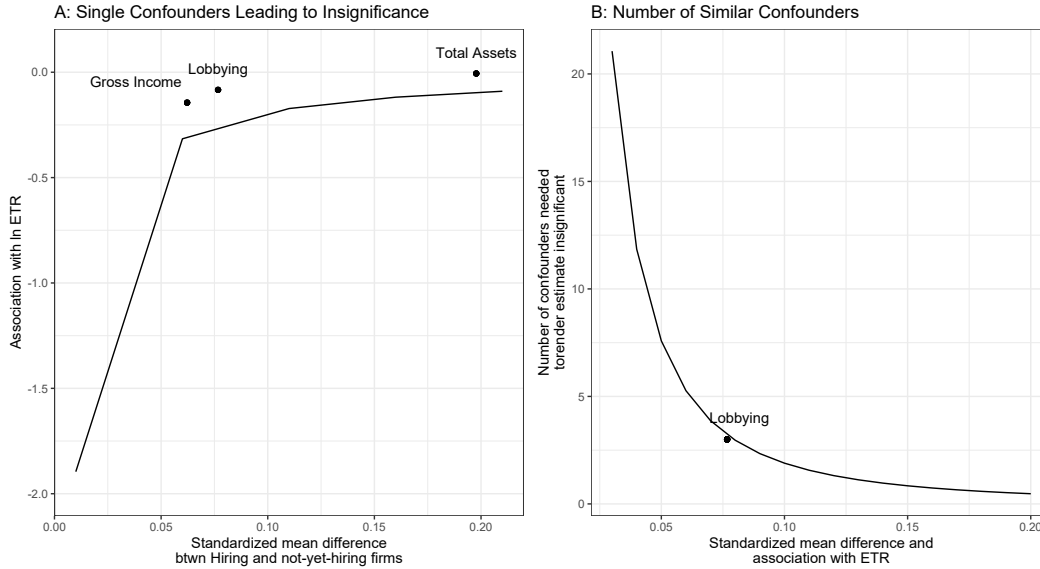In Panel B, I show the number of confounders that would be necessary to render result

Figure K.6: Tipping Point Analysis of Effective Tax Rate. *Notes: The figure shows the results of a set of tipping point analyses using the* `tipr` *package (McGowan 2021). Panel A: The line shows the combination of a) association with ETR and b) association with hiring revolvers that is necessary to render estimates statistically insignificant at the 10% level. Panel B: The line shows the number of similarly sized confounders necessary to render estimates statistically insignificant at the 10% level. Here, the confounder's association with the dependent and independent variables are held at the same level.*

statistically insignificant. For reference, I show how many non-market strategies of similar strength as lobbying to remove effect. As we can see, we would need slightly more than three alternative strategies with similar effects as lobbying to kill off the effect. Such strategies are unlikely to exists: Firms are generally not politically active. To the extent that some firms do engage in political activities, they tend only to follow one strategy, and that is normally lobbying. Importantly, if we benchmark against campaign donations – probably the second most prominent strategy – we would need 347 strategies with similar characteristics.

Next, I turn to the sensitivity of the results on audit initiation. Figure K.7 shows the results of a similar tipping point analysis as the one that was conducted above. The results are generally less sensitive than the estimated effect on ETR. However, as in all observational analyzes, there could exist confounders that would remove statistical significance if included.
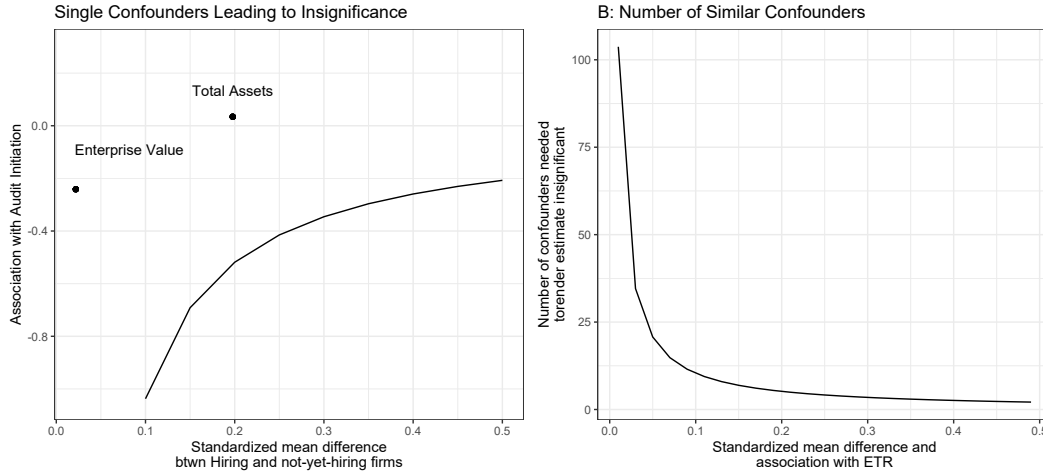
Figure K.7: Tipping Point Analysis of Audit Initiation. *Notes: The figure shows the results of a set of tipping point analyses using the* `tipr` *package (McGowan 2021). Panel A: The line shows the combination of a) association with ETR and b) association with hiring revolvers that is necessary to render estimates statistically insignificant at the 10% level. Panel B: The line shows the number of similarly sized confounders necessary to render estimates statistically insignificant at the 10% level. Here, the confounder's association with the dependent variable is held at the same level of enterprise value (-0.1), while the association with the independent variable is changed.*

Importantly, the enterprise value of the firm is strongly associated with audit probability. A similar confounder that was more strongly associated with hiring legislators would be a threat. However, the association with hiring would have to be larger than anything I observe in this sample. In Panel B, I show how many variables that exhibits the same association with audits as enterprise value we would need to render the results insignificant. I show how this number changes, as the correlation with the independent variable changes. When the correlation with hiring legislators is as low (like we observe with enterprise value), we would need approximately 100 similar confounders. However, if the association with hiring had been as high as the one exhibited by total assets, we would between four and five similarly sized confounders.