The Revolving Door and Insurance Solvency Regulation

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Abstract

Financial solvency regulation of the U.S. insurance industry occurs at the state level, and is led by insurance commissioners. Insurance commissioners wield significant discretion over the regulatory process, but their incentives may be affected by post-term job opportunities ("revolving door"). I construct a novel data set of the employment history of insurance commissioners from 2000 to 2018 and find 38% of them work in the insurance industry after their term ends ("post-term revolvers"). Before leaving office, post-term revolvers are laxer financial regulators along several dimensions: they perform fewer financial exams per year, the exams they perform have fewer negative consequences for firms, and post-term revolvers are less likely to respond to insurers' risk-taking. Post-term revolvers' behavior responds to changes in incentives. Specifically, commissioners more likely to be post-term revolvers ex ante perform more exams in states where revolving door laws have been tightened. Overall, my results suggest the revolving door induces insurance regulators to be less strict.

Keywords: insurance regulation; revolving door; career concerns; insurance commissioners; financial strength ratings; revolving door state laws

JEL classifications: G28; G22; G14; G38; G18; J45; P48; H73

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1 Introduction

Insurance is an $8.5 trillion industry that affects most households and firms in the United States.[1] It is also an industry prone to market failures. Notably, customers must assess whether the insurer will be solvent when they need its services, but most consumers are unable to evaluate the financial solvency of an insurer (Helveston (2015)). To alleviate these concerns, insurance firm solvency is heavily regulated.

Solvency regulation occurs at the state level and includes financial examinations and punitive actions. The top regulators (insurance commissioners) have significant personal discretion, but little is known about the factors that affect their decisions. Anecdotal evidence suggests one of these factors may be the revolving door: the phenomenon of public regulators exiting for jobs in the industry they regulated. For example, former commissioner Sally McCarty claims her colleagues rarely take a hard stance against the insurance industry, because "many [commissioners] consider the job an audition for a better-paying job" (Mishak, 2016).[2]

From an academic perspective, the effect that the revolving door would have on regulation is unclear. One strand of theory predicts it may incite insurance commissioners to be more lenient, as a quid pro quo favor for their future employers (Stigler, 1971; Peltzman, 1976; Eckert, 1981). Alternatively, if insurance firms hire commissioners for their expertise, the revolving door may incite commissioners to be more strict and put more effort into their job (Che, 1995; Salant, 1995; Bar-Isaac and Shapiro, 2011). From an empirical point, which of these two effects prevails depends on the particular situation.

This paper studies how the revolving door affects insurance solvency regulation. I find that commissioners who leave office to work in the insurance industry ("post-term revolvers") are less strict in their solvency regulation along several dimensions. There is suggestive evidence that less information reaches markets as a result of regulatory laxness. These findings raise the question of whether post-term revolvers are laxer regulators because they respond to revolving door incentives or because they are fundamentally different types of

[1]According to the Insurance Information Institute, the cash and invested assets for Property/Casualty and Life insurance are $8.5 trillion, and the premiums written across insurance sectors were $1.2 trillion in 2017. `https://www.iii.org/fact-statistic/facts-statistics-industry-overview`

[2]The investigative journalist report by Mishak (2016) documents several examples in which insurance commissioners acted consistently with quid pro quo, supposedly as a result of revolving door incentive distortion.

regulators. I find that post-term revolvers respond to incentives - specifically, they become more strict in response to exogenous changes in post-term industry opportunities .

To assess the effects of the revolving door, I hand-collect the employment history of insurance commissioners in each state from 2000 to 2018. The data come from professional network sites and press releases. I find a significant fraction of commissioners leave office to work in the insurance industry ("post-term revolvers"). Among the 271 commissioners, 38% work in the insurance industry at some point after their term ends. Using a more narrow definition, I also find that 29% exit into the insurance industry within a year of leaving office (immediate post-term revolvers).

The main proxy for financial oversight strictness that I use is the number and content of financial exams completed each year. Financial examinations are a good setting to look for incentive distortions for two reasons. First, they are important for both firms and commissioners. Specifically, firms care about exams, because they can have large direct and indirect costs. At the same time, commissioners report spending a significant part of their time ensuring financial solvency, and insolvencies negatively affect their careers. Second, commissioners have significant personal discretion over when and whom to examine, as well as the consequences for the firms. Although some standardization exists (firms should be examined at least once every 5 years, and some exam guidelines are common), a commissioner can always conduct an early exam, and ultimately she is the one to decide what actions to take as a result of the exam.

In my analysis, I use both a state-year panel and a firm-year panel.[3] I collect aggregate state-year data on the number of exams from archives of NAIC's Insurance Department Resources Reports (2000-2017). Data on individual exams (date completed and consequences) comes from two sources. The first source is firm annual regulatory reports (2006-present, provided by SNL Financial); annual regulatory reports also provide firm-year risk control variables. The second source is data collected from state insurance department websites and Freedom of Information Act (FOIA) requests.

I document that post-term revolvers are laxer regulators along a number of dimensions. I begin by showing that post-term revolvers perform between 8% and 20% fewer exams for every year they are in office than do non-revolvers. The result is larger in both statistical

[3]In the firm-year panel, for firms that do business in multliple states, I connect each firm to its regulatory headquarter ("domicile" state).

and economic significance for immediate post-term revolvers. I test whether this result is driven by post-term revolvers performing fewer but stricter exams. The empirical evidence is inconsistent with this hypothesis. Using exam-level data I find that exams conducted by post-term commissioners are also less likely to have negative consequences for the firms. Specifically, the exams are 6% to 29% less likely to result in financial restatements.

Are post-term revolvers performing fewer and less consequential exams because they examine firms early, at the first signs of financial distress? I show that post-term revolvers are in fact less likely to examine companies early, and are less sensitive to firm-level risk. Specifically, commissioners call for early exams (less than 5 years since the previous exam) for firms that are looking troubled or are taking too much risk. Therefore, early exams are highly discretionary. Using exam-level data, I identify the variables that are predictive of an early exam for each firm. I find that with other risk variables held fixed, post-term revolvers are less likely to call for an early exam. Moreover, they respond less to decreases in the level of regulatory capital. Finally, even when post-term revolvers conduct early exams, the exams are less likely to result in negative consequences (financial restatements).

Another alternative explanation is that examinations are a poor proxy for overall regulatory strictness. I test whether post-term revolvers substitute the laxer examinations with other forms of punishment (commissioners can temporarily suspend firms' certificate to do business in the state). However, I find no evidence for substitution between exams and punitive actions. Consistent with post-term revolvers being laxer regulators, they do not perform more non-exam actions against financially troubled companies; in fact, they perform fewer of most punitive actions.

Does post-term revolver behavior change in the last two years before commissioners leave office? Post-term revolvers' incentives are most influenced by the revolving door at the end of their term. I do find that post-term revolvers increase their examination rate the year before they leave office. Also, they are more likely to conduct early exams in these two years. Therefore, the spike in examinations is not driven by post-term commissioners wrapping up overdue regular exams. However, the exams are still less likely to result in negative consequences for the firm. Taken together, this result is consistent with revolvers introducing themselves to potential employers and with insurance firms avoiding the regulatory uncertainty of a new, potentially tougher, commissioner.

The second part of my analysis documents the effects of regulatory laxness on firms. Specifically, I use insurers' AM Best's financial strength ratings (Best's FSR) to test if examinations have real-world consequences for firms. These ratings measure insurers' ability to meet ongoing insurance policy and contract obligations, and they have been documented to affect demand for insurance products (Koijen and Yogo, 2015, 2016). Additionally, a wide literature shows that other types of credit ratings affect many aspects of firms' activities, such as capital structure (Kisgen, 2006), corporate bond yields (Crabbe and Post, 1994; Ederington et al., 1987), and stock prices (Hand et al., 1992). I show that firms' financial strength ratings decrease in response to negative news about financial restatements resulting from exams. The result is robust to and increases in magnitude when the sample is limited to less financially strong companies. Because post-term revolver exams are less likely to result in financial restatements, and financial restatements are correlated with AM Best downgrades, taken together, these results suggest post-term revolver laxness may result in less information reaching the market.

Finally, I address the question of whether differences in behavior are driven by post-term revolvers being a fundamentally different type of regulator, or by incentives distortion due to the revolving door. I find that post-term revolvers, as well as commissioners who are ex ante more likely to become post-term revolvers, respond to changes in incentives in employment opportunities. Specifically, I use the tightening of state revolving door laws as an exogenous shock to incentives. In the 2000 to 2017 period, I find 14 revolving door law changes across 12 states. Within a difference-in-differences (DiD) setting, I define the treatment group to be commissioners who are ex ante more likely to become post-term revolvers based on observables. After the law changes, commissioners within this treatment group significantly increase their examination rate, and the likelihood of financial restatements among early exams.

The rest of the paper is organized as follows. Section 2 provides literature related to this study. Section 3 provides institutional background and explains the choice of financial examinations as a proxy for financial oversight strictness. Section 4 details the data-collection process used for the study, and gives summary statistics of the used variables. Section 5 includes the analysis documenting changes in regulatory behavior between post-term revolvers and non-revolvers. Section 6 analyses the effects of negative exam outcomes on Best's FSR. Section 7 provides evidence that post term revolvers respond to incentives. Section 8 concludes.

2 Related Literature

This study contributes to the literature on regulatory design. Public interest theory maintains that regulators make decision with society's welfare in mind (Pigou, 1938; Laffont and Tirole, 1993). This view is challenged by capture theory, which emphasizes the potential for distortion when the industry captures regulators (Stigler, 1971; Peltzman, 1976; Shleifer and Vishny, 1993). There is a rich theoretical literature on optimal regulatory design, especially for the banking sector (Dewatripont, 1994; Boot and Thakor, 1993; Hellmann et al., 2000). However, there is less empirical work on how regulation plays out in practice in general and in insurance in particular. Insurers respond to financial solvency regulations by making significant changes in their balance sheets (Merrill et al., 2012; Becker and Ivashina, 2015; Becker and Opp, 2013; Ellul et al., 2012; Koijen and Yogo, 2016, 2015; Kim, 2017; Ge, 2019; Sen, 2019). Therefore, understanding the factors behind insurance solvency regulation is important.

More narrowly, the paper contributes to studies on regulatory design by providing a source for insurance regulation heterogeneity. Commissioners' personal discretion increases regulatory uncertainty, which can have significant effects on firms (Brennan and Schwartz, 1982; Viscusi, 1983; Prager, 1989; Teisberg, 1993; Agarwal et al., 2014). Ellul et al. (2012); Koijen and Yogo (2016); Kim (2017) document that states differ in how they apply insurance regulatory rules across states, and that these changes have large aggregate effects on firms and markets. However, few papers explain the source of the regulatory heterogeneity. One such source is the election cycle. Specifically, Grace and Phillips (2007) document that insurance commissioners are less likely to put a troubled firm into conservatorship near elections, and Liu and Liu (2018) document that commissioners are less likely to approve premium increases near elections. Here, I focus on the revolving door as a source of regulatory uncertainty and heterogeneity.

The paper is also part of a bigger literature on the effect of the revolving door on regulatory incentives. The main contribution is that it is the first to explore the revolving door effects on insurance solvency regulation. The closest studies on the effect of the revolving door on solvency focus on banking and the rest of the financial sector (Lucca et al., 2014; DeHaan et al., 2015; Johnson and James, 2010; Shive and Forster, 2017). Within the insurance literature, Grace and Phillips (2007) study the effect of the revolving door on auto insurance premiums for an earlier time period (1985-2002). By contrast, this paper focuses on a broader range of outcomes. Other studies on the effect of the revolving door between government and industry have focused on Federal Communications Commissioners (Cohen, 1986), the value of lobbying (Blanes I Vidal et al., 2012; Bertrand et al., 2014), and U.S. patent officers (Tabakovic and Wollmann, 2018).[4]

Note the findings in this paper show insurance regulators become laxer as a result of the revolving door. This finding runs contrary to results from studies on other banking regulators (Lucca et al., 2014; DeHaan et al., 2015). I believe the difference stems party from insurance regulation happening at the state level, whereas banking regulation happens mostly at the federal level. Agarwal et al. (2014) andCharoenwong et al. (2019) show that state and federal level regulators act differently, with state level regulators being more lenient toward industry. My findings are also consistent with the revolving door result from other government regulators. Specifically, firms hire former staffers for their political connections, not their expertise (Blanes I Vidal et al., 2012; Bertrand et al., 2014), and U.S. patents officers are more likely to grant patents for their potential future employees (Tabakovic and Wollmann, 2018).

3 Institutional Setting: Understanding Financial Examinations

Financial exams provide a good environment for studying the effects of the revolving door on insurance solvency regulation. First, financial exams are an important part of solvency regulation. Second, a commissioner is actively involved in and has personal discretion over financial exams. Moreover, the exams can have significant consequences for the firm.

What is a financial exam in the insurance context? Broadly, a financial exam is an audit of an insurance firm to ensure it is in good financial health and able to meet its insurance obligations. More specifically, when a commissioner orders a financial exam, a team of auditors is sent to the firm to estimate the insurer's solvency risk. The team needs to assess whether the insurer's self-reported quarterly and annual regulatory statements are true, whether there are undocumented sources of risk, and whether the insurer adhered to the laws of the state. After the exam is over, the auditors share their findings and

[4] Additionally, a related strand of literature examines financial analysts from rating firms who work for the firms they previously rated (Kempf, 2018; Cornaggia et al., 2016; Horton et al., 2017; Lourie, 2019)

recommendations with the firm and the commissioner, and the commissioner decides what further steps are necessary. A financial exam can be triggered by red flags on annual statements or it can be regularly scheduled.[5] Financial exams can be performed whenever a commissioner deems them necessary, but should be conducted at least once every five years.

Insurance firms prefer to be examined rarely, and by a laxer commissioner, because exams can be disruptive and expensive, and can result in various negative consequences. To start with, firms have to cover the exam costs, which can be up to millions of dollars, and they are, on average, eight months long. Additionally, the exam outcomes can vary considerably. An exam can have no recommendations, or require only minor changes, such as "get an additional board member". However, on the more severe end, exams can require firms to make costly changes ("create risk model") or to restate their regulatory financial statements. Restatements can potentially hurt firms' credit rating, which in turn can affect both the demand for the firms' products and the firms' ability to raise capital. Finally, an exam's findings can trigger the state to put the insurance firm into state receivership (usually, a precursor to liquidation).[6]

Commissioners' strictness regarding exams can change depending on their career goals. Commissioners who perform fewer exams can put themselves in the good graces of future employers and signal they are pro-industry. However, performing too few exams can negatively affect a commissioner's current job. Specifically, if a firm engages in poor management practices it can eventually become insolvent, which in turn can negatively affect the commissioner. State guarantee funds set a limit on the maximum payouts consumers can receive, and they force the remaining firms in each state the insolvent firm operated in to take over the liabilities up to that limit. Therefore, an insolvent insurer hurts both the remaining insurers, who must take on liabilities of the bankrupt firms, and the consumers, who may face a limit on the payouts they receive. These side effects of firm insolvency hurt commissioners from political perspective, which is why they seem to reduce the number of firms they take over due to insolvency in the year before an election (Leverty and Grace, 2018). These political pressures likely force commissioners to perform more exams and be

[5](Klein, 2005) explains that all firms' regulatory statements are reviewed on a quarterly basis for red flags.

[6]For example, in 2011, the California domiciled worker compensation insurer Majestic Capital Ltd was forced into state receivership after a financial exam found its reported capital reserves were not accurate (S&P Global, 2011).

more stringent.

Firms are usually monitored by only one commissioner, so the incentive distortion due to revolving door considerations creates fragility in the system. Although commissioners are responsible for the solvency of all firms that sell insurance in their state, the main burden falls on the domicile state (i.e., the state of the firm's regulatory headquarter). As a result, a commissioner typically accepts a financial exam conducted by the domicile state, in lieu of conducting her own exam. In practice, 99.5% of all conducted exams are of domestic firms. On one hand, this practice avoids duplicate examinations. On the other hand, incentive distortion in financial examinations has more serious consequences, since only one regulator systematically monitors each firm. If the domicile commissioner does not disclose and correct firms' risky behavior, markets may be misinformed, and consumers from both domicile and non-domicile states can be affected.

4 Data

To assess the extent of the revolving door among insurance commissioners, I construct a database on the employment history of all commissioners in office between 2000 and 2018. I construct the database using publicly available professional network profiles, and I supplement them with press releases. The database reveals 38% of commissioners work for insurance firms after their term ends.

Additionally, to assess the effect of the revolving door on solvency regulation, I measure financial oversight strictness using number of examinations and actions taken against insurers. I assemble this variables using the archives of NAIC's Insurance Department Resources Reports, 2000-2017.

I also construct an exam firm-year panel to focus on exam outcomes, which firms are more likely to be examined, and the sensitivity of commissioners to these variables. I construct the panel from firms' annual reports through *SNL Financial*, and supplement it with exam information via state insurance department website information and freedom of information requests. Some key variables are the date the exams were completed for the given firm, whether the exam resulted in any recommendations, whether the recommendations required financial statement restatements, and firm-year risk variables (assets, statutory ratios, leverage ratio, operational loss). The resulting exam-level panel starts in 2000 for some states, but for most it starts in 2006 and continues to 2018.[7]

Finally, I construct a more firm-restricted firm-year panel of Best's FSR for insurance firms. This firm-year panel is restricted, because firms pay for AM Best's rating services, and not every company chooses to do so.

4.1 Gathering data to measure the revolving door in insurance regulation

There is no ready-made employment history database for insurance commissioners. To address this challenge, I construct one using online professional network profiles and supplement employment gaps with online media releases. The resulting database has at least one employment history event for all commissioners in office between 2000 and 2018 in addition to their commissioner job. I classify each job in one of six general categories: the insurance industry, government, consulting or lobbying, law firm, related industry (e.g., finance or real estate), or other. On average, I find 3.8 jobs for commissioners before they start office and 2.7 after they leave. I also determine each commissioner's age and gender.[8] See Appendix A for more information on the data gathering procedure.

The newly constructed data set reveals a widespread practice of commissioners either coming from, or moving back to, the industry. I find that 51.5% of commissioners had at least one job before/after their term in the insurance industry. More specifically, 38% had at least one job after the end of their term (ever post-term revolvers) in the insurance industry. Additionally, 29% exited immediately, or within a year into the insurance industry after their term ended (immediate post-term revolvers). Furthermore, 35% of commissioners had at least one job in insurance before their commissioner term started (pre-term revolvers), and 16% came from and exited into insurance.

Apart from insurance, the job background of commissioners often includes other government jobs and law firms, as illustrated in Figure 1 for both ever pre- and post-term employment.[9] I find that 85% of commissioners have pre-term experience in government (other regulator position, elected office, or working as a staffer), and 49% of commissioners work

[7] The restriction here comes from the examinations data. Risk Variables are available 1996-present.

[8] For determining age, I use publicly available information about birth year or college graduation year.

[9] See Figure 2 for commissioners' jobs immediately before/after their terms.

in government after their term ends. The second most common experience is insurance, both before and after commissioners' terms. The third most popular pre-term job experience is lawyer (26% pre-term and 18% post-term). A related category is consultants and lobbyists, who also experience the biggest jump from pre- to post-term: from 8% to 22%. This finding makes sense because consultants and lobbyists often work as liaisons between insurance departments and the firms that employ them.

Many of the jobs that revolvers take are in government relations positions. This result is notable because these jobs are more likely to use commissioners' connections rather than expertise. Using job descriptions and/or job titles, I classify each insurance industry job into three categories: government relations job, not government relations job, or unclear. I find that 22% of pre- and 35% of post-term revolvers have jobs that rely on government connection. Additionally, a third of all revolvers work only jobs that cannot be classified based on whether they have contact with regulators. These findings are shown at Figure 3.

Also consistent with the incentives revolving door theory, I find that commissioners often seek to stay within state, where their connections are likely more valuable. I look into geographical preferences of commissioners, and find that commissioners often come from and stay in the state they regulated (see Table A.1). Specifically, 87% of commissioners have at least one pre-term job, and 79% have at least one post-term job in the same state as their commissioner job. Among revolvers who have government relations jobs, these numbers are respectively, 64% for pre- and 50% for post-term revolvers (with unknown job locations counting as out of state).

How does the revolving door extent compare to other studies? The revolving door is similar for insurance commissioners from the 1985-2002 period (Grace and Phillips, 2007). The levels are slightly higher than they are in studies from different fields that provide equivalent statistics, which is likely due to the shorter nature of commissioners' terms. Kempf (2018) finds post-term revolvers are 27% among financial rating analysts, while DeHaan et al. (2015) finds post-term revolvers are 31% among SEC lawyers. The lower revolving rate in their studies is likely due to the fact that I look at higher-level employees, whose appointment mechanism prevents them from spending prolonged periods of time on the job. Specifically, in 31 states, the commissioners are appointed by and serve at the pleasure of the governor, and when a new governor comes into office, they often appoint a new commissioner. Eleven of the remaining states elect their commissioner every four years.

4.2 Aggregate data on financial examinations

I use the number of financial exams as a proxy for financial oversight strictness, which is a variable I can measure from two sources. First, NAIC's Insurance Department Resource Report provides the aggregate number of examinations completed in a given state in a given year. Second, I assemble firm-level data on financial exams from insurance departments' websites. From the Resource Report, I also extract other variables, such as actions taken against companies.

Table 1 presents the summary statistics of the panel used for the regressions in the empirical analysis. A state conducts on average 30 exams per year, but this distribution is very skewed. I observe that the distribution of domestic exams seems to match very closely the distribution of all exams. The reason is that the main responsibility for solvency regulation falls on the domestic state. As a result, using domestic, instead of all exams allows for a better comparison of commissioners' productivity, so I use the number of domestic exams as the response variable in the empirical analysis. However, results are robust to using the number of total exams.

On average, 160 firms are domiciled in each state in a given year, and firms are examined once every 4.6 years. However, this number varies widely, and I exploit the source of variation to estimate commissioner productivity. To isolate the effect of post-term revolvers on examination rate, I control for the number of domestic firms, as well as for the resources available to state insurance departments: budget in a given year, and the number of financial analysts and examiners (both on staff and contracted). I lag the latter variable to account for the fact that examinations begin around eight months before they are completed.

4.3 Exam-level data on financial examinations

The main source of firm-level exam data comes from the annual financial reports, which every Life, Health and Property/Casualty company must submit to its domicile state.[10] In these annual reports, firms must answer questions about their most recent financial exams, specifically when the most recent examination completed was, the end of the period the exam covered, and which department conducted it.

The variables I construct using the annual reports include the date each exam was completed and individual exam outcomes. Specifically, I assess if the exam resulted in any recommendations (true in 60% of the cases), and whether the exam conclusion forced the firm to restate its financial statements to reflect findings during the exams (30% of the cases).[11]

The earliest annual reports are from 2006, so I supplement my data by requesting older exam information from state departments. This approach allows me to extend the panel pre-2006 for 13 states. I discuss further the coverage of the data and how it compares to aggregates in Appendix C.1.

Using the annual reports, I also construct firm-specific variables on the balance sheets of the insurance companies in order to control for their solvency risk. The variables of interest are total assets, which proxies for firm size, and various measures of how much risk the firm has taken, including the ACL RBC ratio (available capital to capital required by regulation to be held), leverage ratio (liability over assets, admitted by the regulator), and operational loss-to-assets ratio (the denominator being positive minus negative cash flow). These variables are summarized in panel E of Table 1.

Finally, I add Best's FSR to the firm-year panel.[12] Although the full exam-level panel covers 5,183 firms, only 618 firms have requested Best's FSR rating since 2006. Ratings are assessed approximately once a year, and 10% of the reassessments result in rate changes. I use AM Best's 10-year historical default data as of 2018 to construct the implied default probability for each rating (more details are in Appendix E.1). The distribution of all

[10]I accessed these reports through *SNL Financial*.

[11]The specific annual report questions that allow me to infer outcomes of the examination are (1) whether the firm complied with exam recommendations and (2) whether the firm has revised its financial statements to reflect findings during the financial exam. The answer options to these questions are "yes", "no", or "not applicable", with "no" being filled in for 1% of the answers.

[12]AM Best rating data are also provided by *SNL Financial*.

ratings and each ratings-implied probability are plotted in Figure 5 and Panel F in Table 1 provides summary for exam outcomes and default probabilities on the FSR sample, Finally, I compare the observables of firms with and without ratings at Appendix E.2.

5 Empirical Analysis: Documenting Post-term Revolver Behavior

In this section, I describe the main empirical setup and results. My main finding is that post-term revolvers are laxer financial regulators along several dimensions.

5.1 Post-term revolvers perform fewer financial exams

I test if a post-term revolver in office correlates with fewer financial examinations per state per year. To do so, I estimate the following regression for state s and year t:

$$Y_{s,t} = \alpha_s + \alpha_t + \beta I^{POST}_{s,t} + \gamma_x X_{s,t} + \epsilon_{s,t}. \tag{1}$$

In equation(1), the outcome variable $Y_{s,t}$ is a measure of the number of exams completed in state s in year t, the variable of interest is $I^{POST}_{s,t}$, which is an indicator variable equal to 1 whenever the commissioner in office in state s, and year t is post-term revolver. Control variables include state fixed effect α_s, year fixed effect α_t, and $X_{s,t}$ is a matrix of variables for state s and year t: number of domestic firms, log of the insurance department budget, log of the number of employees working on financial solvency in year $t-1$, and whether the commissioner in office is a pre-term revolver (an indicator variable that equals 1 when the commissioner worked in the insurance industry before her term started). All errors are clustered at the state level.

I use two different specifications for the dependent variable $Y_{s,t}$: the number of financial exams of domestic firms and the log of that number. Using a log of the number of exams ensures the results are not driven by the long tail of the variable documented in Table 1. I use domestic as opposed to all financial exams, because domestic exams are a better measure of commissioner output: domestic firms should be regularly examined by the domicile state commissioner, while out-of-state companies are examined only when there is a solvency concern not addressed by the domicile commissioner, and when resources permit. However, the results are robust to using the total number of exams (see Appendix B.1).

I also use two measures of post-term revolver: whether the commissioner works in the insurance industry at any point after leaving office ($I_{s,t}^{POST,ever}$), or whether she immediately, or within year, started working for the insurance industry after leaving office ($I_{s,t}^{POST,immed}$). Note that finishing an exam takes around eight months, I exclude commissioners with terms shorter than one year. Still, I test that the results are robust to including all commissioners (see Appendix B.2).

I estimate that post-term revolvers perform 8% to 20% fewer examinations per year, which is consistent with post-term revolvers being laxer regulators. The results are summarized in Table 2, and they are statistically and economically significant.

When the dependent variable is the number of exams, β from equation (1) is -3.7 for all post-term revolvers with no control variables, except time and year, and -2.9 with control variables. Given that the average number of exams is 29.6 per state per year, post-term revolvers in this specification perform between 10% and 12% fewer exams. When the definition of post-term revolver is narrowed to immediate employment after office, the effect increases in both absolute size and significance: β decreases to -6 with no controls and -4.8 with controls. The increase in the effect is consistent with incentives being stronger near the end of the term.

Results are still significant, though a bit smaller in size, when the outcome variable is the log of the number of domestic exams. Post-term revolvers perform between 8% and 10% fewer exams than non-revolvers. The examination rate decreases even further for immediate post-term revolvers, who perform 10% to 12.7% fewer exams.

These results are robust to using all financial exams, instead of domestic exams only (Table B.2), as well as to including all commissioners, instead of only the ones who served more than a year (Table B.3). I also confirm that results are not driven by one particular state by rerunning regression (1) and excluding each of the 51 states one at a time. I find the results preserve their magnitude and significance (Table B.7 and Figure B.1).

5.2 Exam-level analysis: Exam outcomes and the likelihood of early exams

I use firm level examination analysis to clarify the exact mechanism driving the difference in examination rate, and to account for individual firm level control variables. I construct a firm-year panel by connecting individual examinations to firm-specific measures of risk and exam outcomes. Using firm level data, I test two alternative channels that could lead to post-term revolvers performing fewer exams, but not being laxer regulators. First, I test whether post-term revolvers perform fewer exams, but the exams are less likely to have negative consequences for the firm. Second, I test if post-term revolvers perform fewer exams, but intervene in a more timely manner, whenever risk increases.

Empirical setup

Do post-term revolvers perform fewer but stricter exams? To answer this question, I limit the firm-year panel to only the years in which the given firm has an exam, and run the following regression:

$$ExamOutcome_{i,s,t} = \alpha_s + \alpha_t + \beta I_{s,t}^{POST} + \beta_r RiskVars_{i,t} + \gamma_x X_{i,s,t} + \epsilon_{i,t}. \quad (2)$$

In equation (2), $ExamOutcome_{i,s,t}$ is an indicator variable that equals 1 whenever the exam for firm i, conducted by state s in year t, results in a negative outcome for the firm. I use two proxies for exam-outcome strictness: whether any recommendations were made during an exam (true for 57% of the exams) and whether the exam outcomes required the firm to make corrections to their financial statements (true for 29% of exams). The underlying assumption here is that the more recommendations a commissioner makes, the stricter he is. The variable of interest is $I_{s,t}^{POST}$, which is an indicator variable that equals 1 when the commissioner examining the firm is a post-term revolver. The coefficient of interest here is β: it measures the increase in the likelihood of the exam resulting in a negative outcome for the firm when a post-term revolver is in office.

$RiskVars_{i,t}$ and $X_{i,s,t}$ are, respectively, risk-specific and non-risk-specific control variables. Specifically, the risk variables include lagged yearly level, and percent difference in log assets, leverage ratio, regulatory capital, and operational loss (summary statistics are in

Panel E of Table 1). The non-risk-specific variables include the number of years since the previous exam, pre-term revolver status of the examining commissioner, log state insurance budget of state s and year t, log of the number of employees working on financial solvency in year $t-1$, and state and year fixed effects. All standard errors are clustered at the state level.

In addition to considering two outcome variables, I also use two definitions for $I_{s,t}^{POST}$: immediate and ever post-term revolver. I also limit the sample to early (discretionary) exams, and test how strict the exam is when it occurs three years or less since the most recent exam. I test if results are robust to defining early exam as an exam two or four years since the most recent exams (see Appendix C.2) and to limiting the sample to firms similar in size to the firms that end up hiring insurance commissioners (see Appendix C.3).

Why are early exams examined separately? The primary answer is that early exams are discretionary, and they show the willingness of the regulator to intervene early for companies suffering solvency shocks. Consistent with the requirement that firms be examined every five years, 91.5% of all exams happen within five years of the previous exam. Only 30% of all exams happen within three years of the most recent examination, and I refer to these exams as "early." The cumulative distribution of years between exams is shown in Figure 4. On the other hand, the probability that a firm is examined in a given year is 18%.

The second question I address using the firm-level panel is whether post-term revolvers perform fewer exams but intervene in a more timely manner, whenever risk increases. To answer this question, I focus on a firm-year panel and exclude all firm-year observations which are more than two, three or four years since the firm's last exam. Base on this panel, I run the following regression:

$$isExamYr_{i,s,t} = \alpha_s + \alpha_t + \beta I_{s,t}^{POST} + \beta_r RiskVars_{i,t} + \gamma_R \left(I_{s,t}^{POST} \times RiskVars_{i,t} \right) + \gamma_x X_{i,s,t} + \epsilon_{i,t}. \quad (3)$$

In equation (3), $isExamYr_{i,s,t}$ is an indicator variable that is 1 whenever firm i is examined by state s in year t, and 0 if this is not an exam year for the firm. $I_{s,t}^{POST}$, $RiskVars_{i,t}$, and $X_{i,s,t}$ are defined as in equation (2). The coefficients of interest are β and the vector of coefficients γ_R. Specifically, $\beta + \gamma \times \overline{RiskVars_{i,t}}$ measures the change in the likelihood of an early examination by post-term revolvers, evaluated at the mean of the risk variables, $\overline{RiskVars_{i,t}}$. γ_R captures the increase in the early exam probability once risk variable R increases by a unit.

Results: exam outcomes

I find that exams conducted by post-term revolvers are less likely to result in financial restatements and, in some cases, any recommendations. Results are shown in Table 3.

The results are stronger for financial restatements, especially for early exams. On average, 34% of all exams and 36% of early (within 3 years of last) exams result in financial restatements. Post-term revolver exams are 2.2% less likely to force firms to make financial restatements, which is 6.5% of the total effect. Among exams within three years of the most recent previous exams, the effect increases in magnitude, and in statistical and economical significance: post-term revolver exams are 10% less likely to result in financial restatements, which is 27.5% of the total effect. Therefore, whenever post-term revolvers use their discretion to order an early exam they are less likely to force a firm to restate due to exam findings. Immediate post-term revolver exams are, on average ,less likely to result in restatements among all exams, though the result is not statistically significant. However, the results for early exams are similar in magnitude and statistical significance.

I check the robustness of the results to modifying the definition of an early exam to one within two, three or four years within the last exam in Appendix C.2. Table C.9 shows the results for ever post-term revolvers on the likelihood of financial restatement are robust to all three definitions of early exams. In fact, among earlier exams, the difference in the likelihood of examination increases. Results also become stronger when the sample is limited to only these firms that are comparable in size to future employers. Specifically, I repeat the analysis shown in Table 3 for firms whose log assets are between the smallest and largest log assets observed for a firm that hired a former commissioner; see Table C.12.

The next step is to expand the definition of the exam outcome to also include exam recommendations, which do not result in financial restatements. On average, 65% of all exams and 70% of early exams result in any recommendation (financial restatement or other). Post-term revolver exams are still less likely to result in negative outcomes; however, the result is not statistically significant except for immediate post-term revolvers and early exams. In this case, immediate post-term revolver exams are 6% less likely to result in any recommendations, which is 9% of the total effect.

I check the robustness of the results on any recommendations to the definition of early exams; results are in Table C.10, Appendix C.2. Results are consistent, larger in magnitude and statistically significant for the effect on ever post-term revolvers and immediate post-term revolvers on exams within four years of the most recent exam. Results for both ever and immediate post-term revolvers are not significant for exams within two years of the previous examination. However, this finding is likely due to the number of observations falling to around 500. Furthermore, I find that immediate and ever post-term revolver exams are statistically less likely to result in any financial recommendations when the sample is limited to firms that are comparable in size to firms that hire former commissioners (see Table C.12).

Note that exams are more likely to result in negative consequences for the firms that take more risk. Specifically, the control variables in Table 3 show that negative exam outcomes are more likely whenever (i) firms have smaller asset levels, (ii) the firms are more levered in level, or experience an increase in leverage since year $t-1$; (iii) have weaker regulatory capital ratio level, or the regulatory capital ratio decreased since year $t-1$.

Results: Predicting early exams

Do post-term revolvers perform fewer exams, but intervene in a more timely manner, whenever risk increases? I find that post-term revolvers are less likely to conduct an early (discretionary) exam, and are less responsive to key risk variables. Therefore, the data will be consistent with this theory only if the post-term revolvers have expertise that allows them to pick out risky companies using a signal that is uncorrelated with traditional risk variables.

Results are shown in Table 4. The probability that a firm experiences an early exam (within 3 years of last exam) is 1.2% less if an ever post-term revolver is in office. This decrease is significant because the unconditional probability of an early exam for a given firm is 8%. Adding control variables only increases the coefficient in size.

Additionally, post-term revolvers are less sensitive to risk, with a differential response observed for changes in the level and changes in regulatory capital (ACL RBC) and operational loss. Whenever the ratio of operational loss to assets decreases by one standard deviation, the likelihood of an early exam increases between 0.32% and 0.4%; however, having a post-term revolver in office fully offsets this effect. Similarly, although on average, a standard deviation increase in regulatory capital decreases the probability of an early exam by 0.54%, an ever post-term revolver in office fully offsets this effect. Specifically, a standard deviation increase in regulatory capital increases the probability of an early exam by 0.8%.

I run robustness checks over the definition of post-term revolver and early exams. Results are shown in Table C.11. The result preserves its direction if the definition of an early exam is changed to two or four years since the latest exam. The statistical significance is preserved for all immediate post-term revolvers, however, the error is quite large for ever post-term revolvers and exams two and four years since the last.

Looking across the columns of Table C.11, note that the relevant measure of risk attitude changes. Two potential explanations are possible. First, this shift happens because the variables are correlated with each other. Second, at different stages, different risk variables indicate troubled insurers: close to the previous exams, commissioners seem to respond to changes in variables, whereas later, they seem to respond to levels.

5.3 Post-term revolvers and actions against firms

I test if post-term revolvers perform fewer exams, which have fewer negative effects on the firms, but keep firms disciplined by being harsher with the penalties against insurers once a firm is troubled. This hypothesis is not consistent with the observed behavior of commissioners. In fact, even accounting for the fewer number of examinations, post-term revolvers perform fewer actions against firms.

To test if substitution exists between exams and punishment, I run regression (1) with the dependent variable being the number of actions against insurers in state s in year t. I include the number of domestic exams completed in state s, year t as a control variable in addition to all control variables from matrix $X_{s,t}$ in regression (1): pre-term revolver status, the number of financial domestic exams, log of the budget of the insurance department in state s in year t, and log of the number of examiners in state s in year $t-1$. I include the number of exams in order to be able to interpret the coefficient β as the difference in the number of actions per state per year once the lower examination rates are accounted for (some of the actions can be taken as a result of exam findings). I also include state and year fixed effects and cluster the standard errors at the state level.

I use as dependent variables all three available state-year aggregates provided by NAIC's IDR Report: the number of certificates suspended, certificates revoked, and delinquency orders in state s and year t. A firm's certificate is suspended when the firm is prohibited from doing business in a state until certain solvency conditions are met. On average, 3.5 such events occur per state per year. A more severe and permanent punishment is revoking a firm's certificate, which is a permanent ban on the firm doing business in the state. On average, two such events occur per state per year. If a firm is fully insolvent, the domicile state steps in and puts the company in state-run receivership, which often is the first step toward liquidation: this process is known as a delinquency order. On average, 0.7 delinquency orders occur per state per year. The outcome variables are summarized in Panel D of Table 1.

Table 5 shows commissioners are not stricter in any of the used measures. In fact, for two out of the three measures, they perform statistically and economically significantly fewer actions against firms. Post-term revolvers suspend, on average, one less certificate and issue 0.4 less delinquency orders. These effects are comparatively large because on average, there are 3.5 suspended certificates and 0.7 delinquency orders per state per month.

I run several robustness checks (see Appendix D). The results are robust to log actions specification, shown in Table D.18. Post-term revolvers suspend 17% fewer certificates and make 11% fewer delinquency orders. Note that certificates suspended results lose significance, with a p-value of 13% once control variables are added. Results further weaken if we focus on immediate post-term revolvers, when certificates suspended are no longer significantly less, and the number of delinquency orders is not significant once controls are added (p-value becomes 19.6%). The last result means the number of delinquency orders in this specification is not significantly smaller once I account for the smaller number of financial exams.

The coefficients on $I_{s,t}^{POST}$ are consistently negative across specifications. Therefore, the results are not consistent with post-term revolvers substituting exams with other financial solvency actions. Taken together, the results imply that even accounting for the decreased number of financial exams, post-term revolvers perform fewer actions against insurers.

5.4 Revolving door effects near the end of commissioner's term

The incentives effects of the revolving door are stronger at the end of the commissioners' terms, so I test whether the commissioners' behavior changes near that time. Specifically, I focus on the last two years in office for the commissioners.[13] I start by looking at the aggregate number of financial exams, so I modify regression (1) as follows:

$$\begin{aligned} Y_{s,t} =& \beta I_{s,t}^{POST} + \beta_T I_{s,t}^{T} + \beta_{T-1} I_{s,t}^{T-1} + \gamma_T \left(I_{s,t}^{POST} \times I_{s,t}^{T} \right) + \gamma_{T-1} \left(I_{s,t}^{POST} \times I_{s,t}^{T-1} \right) + \\ & + \gamma_x X_{s,t} + \alpha_s + \alpha_t + \epsilon_{s,t}. \end{aligned} \tag{4}$$

The new variables in (4) are $I_{s,t}^{T}$ and $I_{s,t}^{T-1}$. These indicator variables equal 1 if in state s and year t the commissioner is in, respectively, her last year in office or in her penultimate year in office. Another difference between regressions (1) and (4) is that the control variables in (4) include the year in the election cycle (note results are robust to excluding election cycle variables). I control for the election cycle to rule out that the results are driven by the fact that approximately half of departures are after an election year (as opposed to by the commissioner's career concerns due to the departure itself) .

In equation (4), the variables of interest are $I_{s,t}^{POST}$, $I_{s,t}^{POST} \times I_{s,t}^{T}$ and $I_{s,t}^{POST} \times I_{s,t}^{T-1}$. β measures the difference in examination rates between post-term revolvers and non-revolvers in all but the last two term years, whereas $\beta + \gamma_T$ and $\beta + \gamma_{T-1}$ measure the difference between the two groups in the last and penultimate year of the commissioner term.

Results are in Table 6. Consistent with the findings from (1), post-term revolvers perform fewer exams per state per year during most of their term (12% to 23% fewer exams in all but the last two years). However, the year before they leave office, they increase the number of examinations so that in this year, their rate matches that of non-revolvers. The result is robust for both ever- and immediate post-term revolvers and to adding control variables.

[13] The average term length for commissioners who stays in office at least one year is five years.

Since post-term revolver exams are less likely to result in negative outcome for the firms, the difference in examination rates supports a theory in which firms prefer to be examined under the laxer regime of a post-term revolver. Meanwhile, the post-term revolvers can use these examinations as "interviews": firms get an easy exam, and the commissioner gets an introduction to a potential employer. If this hypothesis is true, firms will be more likely to be examined early in the last two years. To test this theory, I modify equation (3) to allow for differences in behavior in the last two years of the term:

$$isExamYr_{i,s,t} = \beta I_{s,t}^{POST} + \beta_T I_{s,t}^{T} + \beta_{T-1} I_{s,t}^{T-1} + \gamma_T \left(I_{s,t}^{POST} \times I_{s,t}^{T} \right) + \gamma_{T-1} \left(I_{s,t}^{POST} \times I_{s,t}^{T-1} \right) + \\ + \beta_r RiskVars_{i,t} + \gamma_R \left(I_{s,t}^{POST} \times RiskVars_{i,t} \right) + \gamma_x X_{i,s,t} + \alpha_s + \alpha_t. \quad (5)$$

Results are in Table 7. Firms are between 2% and 7% less likely to be examined early by post-term revolvers. However, this difference decreases if the post-term revolver is in her penultimate term year. These results are consistent with post-term revolvers increasing the examination rate as an industry-friendly gesture. Note that once term end fixed effects are included, the result that firms are less likely to be subject to post-term revolvers for most of their term becomes significant across all specifications.

An alternative explanation is that the increase in examinations is driven by post-term revolvers knowing who their future employers are and, more importantly, who their future competitors are. A testable implication will be an increase in negative exam outcomes. However, I observe no change in the likelihood of exam outcomes in the two years leading up to commissioner departure (see Table C.17).[14] Further, in Table C.15 of Appendix C.4, I document that post-term revolvers' employers are more likely to be examined (early or otherwise) in years that their future employee is commissioner.[15]

6 Best's FSR: Response to Financial Restatements

In this section, I study whether the examination outcomes have real consequences for the firms. I find that a negative exam outcome is correlated with an increase in the default

[14] Similarly, this result is inconsistent with a theory in which the spike in examination rate reflects that a post-term revolver is being forced out due to being too lax, so he is attempting to overcompensate.

[15] Exams of future employers are less likely to result in restatements, but more likely to result in recommendation; see Table C.16.

probability implied by Best's FSR for insurance companies. This finding implies that financial examinations reveal information that the market has not already incorporated. Recall that post-term revolver exams are less likely to result in restatement: taken together, the two results suggest that the laxer regulatory regime of post-term revolvers may make the market less informed.

Best's FSRs vary between A++ and F. A firm's rating gets re-evaluated approximately once a year. I use the 10-year historical default probability provided by AM Best to estimate the implied default probability of each default rating (see Appendix E.1 for more details on how the default probability was estimated). The distribution of the ratings and the implied default probability of each rating are shown in Figure 5.

I use the following equation to test whether newly released information on an exam with financial restatement is correlated with a change in the default probability:

$$\Delta DefaultProb_{i,t}\% = \alpha_s + \alpha_t + \beta_f newFinRstmt_{i,s,t} + \gamma_r RiskVars_{i,s,t} + \gamma_x X_{i,s,t} + \epsilon_{i,s,t} \quad (6)$$

The variable of interest is $newFinRstmt_{i,s,t}$, and it is an indicator variable that equals 1 whenever an exam for firm i was conducted in year t by state s and resulted in financial restatement. The outcome variable $\Delta DefaultProb_{i,s,t}\%$ is the difference between firm i's default probability in percents between years $t-1$ and t. The control variables $X_{i,s,t}$ include an indicator variable that is 1 if there was an exam for firm i was examined in year t and the number of years since the most recent exam. I include both state and year fixed effects, as well as state $\times$ year fixed effects. All errors are clustered at the state level.

The results are shown in Table 8. I find that the release of an exam which required financial restatement is associated with a 7-basis-point increase in the default probability (in a given year, the average change in default probability is 2.4 basis points [bp]). In Table 9, I estimate regression (6) only on observations whose rating in year t is below A++, A+, A and A-. The last cut limits the sample to half. The magnitude of the effect increases from 7 bp to 15 bp for restrictions below A, and the result is not statistically significant for restricting the sample to A- and below.

The results imply that in years with financial restatements, Best's FSR decreases. This conclusion is consistent with information provided by AM Best in personal correspondence, according to which the company incorporates financial exams in its rating, and pays particular attention to financial restatements. AM Best receives a summary of financial examinations once they are out, and according to its representatives, has limited information on the exam before it is completed. According to AM Best, it pays particular attention to restatements that result in lower capital.

7 Commissioners' Response to Revolving Door Laws Changes

Studies in the revolving door literature often discuss whether the differences in the behavior between revolvers and non-revolvers are due to the fact that the people who become revolvers are inherently different types of people, or whether the differences are driven by incentive distortion. The question is important because it provides a very different channel to explain the empirical observations. Additionally, the policy implications of either case are very different: in the first case more attention should be paid to who is hired; in the second case more attention should be paid to the rules regulating exit options for employees.

I use changes in revolving door laws as exogenous changes to incentives and DiD setting to test whether revolvers respond to them. These laws are state-level rules limiting the type of job a former executive department head or elected official can have within a given period. I find that although the laws do not seem to directly affect the employment choice of ex-commissioners, they do seem to affect their behavior. Specifically, the post-term revolvers who are affected by laws that become stricter perform more exams than the ones who are not. Since post-term revolver is an ex post variable, I estimate which commissioners are ex ante more likely to be post-term revolvers, and confirm that those predicted to be immediate post-term revolvers perform more exams when affected by the change.

7.1 Law Changes

I find 14 laws in 12 states that affect commissioners' post-term labor options between 2000 and 2017. All but one put more restrictions on the type of activities a commissioner can engage in after leaving office (the exception is South Dakota, 2011 law change). The changes in states where multiple changes occurred were all in the same direction, so I use the earliest year as the shock year. The states and years of the law changes are summarized in Table

10. See Appendix F.1 for more information on how I identified this set of laws.

Most of the laws deal with bans on lobbying, representing others in front of the department they served, and bans on assisting formerly regulated firms. The law changes are plausibly exogenous to the commissioners' behavior because the laws don't directly target insurance commissioners. Rather, they affect either all state government employees, department heads, or elected officials (in the states in which commissioners are elected).

The way these laws potentially affect the commissioners is by making them less useful to potential employers. In my analysis, I find that 30% of post-term revolvers work in government relations positions, and many of them are lawyers by education. If the former commissioner cannot represent his employer in front of the insurance department, for say, two years, someone else needs to be hired to perform his functions. The value of the former commissioner likely decreases for the firms, so the salary offered or the probability of an offer is smaller. As a result, non-insurance industry job options become comparatively more attractive.

The states that experienced law change are a good representation of all states. I show in Table 11 that states with and without law changes have similar levels of and changes in populations, insurance premiums written, and GDP (total and from insurance).

7.2 Effect of law changes on the number of exams

I use a DiD setting to test if post-term revolvers respond to incentive changes. In the DiD, *treatment group* is post-term revolvers ($I^{POST}_{s,t}$) and the shock is change in laws ($I^{\Delta LAW}_{s,t} = 1/-1$ whenever a law strengthening/weakening occurred in state s in the years before t). I modify regression (1) to fit this DiD setting as follows:

$$Y_{s,t} = \alpha_s + \alpha_t + \beta I^{POST}_{s,t} + \beta_L I^{\Delta LAW}_{s,t} + \gamma_L \left(I^{POST}_{s,t} \times I^{\Delta LAW}_{s,t} \right) + \gamma_x X_{s,t} + \epsilon_{s,t}. \quad (7)$$

The variable of interest is $I^{POST}_{s,t} \times I^{\Delta LAW}_{s,t}$. I start by focusing on the number of financial exams as an outcome variable. In this case, if post-term revolvers respond to incentives, the cross-term coefficient γ_L will be positive, and post-term revolvers will perform more exams once revolving door laws toughen.

Interpreting the results as evidence for the incentives theory requires an implicit assumption that the law changes do not result in changes in the type of person who becomes commissioner. I test this assumption by comparing the commissioners in affected states before and after the law changes. Results are in Table F.25. Although the number of commissioners included in this comparison is low (37 commissioners before and 20 after the change), the observable characteristics do not change significantly after the law takes effect.

The problem with the setting above is that the treatment group is not known ex ante. Therefore, I use a linear model to predict each commissioner's post-term revolver status (I_i^{POST}) using ex ante characteristics:

$$I_i^{POST} = \alpha_s + \alpha_T + \beta_i^{PRE} X_i^{PRE} + \beta_i Pers.Characteristics_i + \epsilon_i. \tag{8}$$

X_i^{PRE} includes pre-term employment indicators showing whether, before his term started, the commissioner had employment history in insurance(I_i^{PRE}), government ($I_i^{\text{government, } PRE}$), and so on. $Pers.Characteristics_i$ includes personal characteristics predictors age and age^2 at the beginning of the term and a gender indicator variable I_i^{Man}. The regression also includes the state in which the commissioner served, as well as fixed effects for the year in which a commissioner started her term. The result of this fitting is further discussed in Appendix F.2. I form predicted post-term status $Pred.I_i^{POST}$ using the fitted values from regression (F.1) and use it in place of $I_{s,t}^{POST}$ in regression (7).

Table 12 shows results from regression (7) for both realized and predicted ever post-term revolvers. In states where revolving door laws got stronger, post-term revolvers respond by significantly increasing their examination rate compared to the non-revolvers. The results are robust for the absolute and log number of exams, as well as predicted and realized post-term revolvers. Figure 6 shows the difference in examination rates by years to law change. No pre-trend seems to exist before the law changes, and it takes one to two years after the law change for the laws to take effect.

7.3 Effect of law changes on exam outcomes

If post-term revolvers respond to incentives, the exams they conduct will get more difficult after law changes. I test whether post-term revolver exams are more likely to result in financial restatements after revolving door laws strengthen. I modify regression (2) to fit the DiD setting described in the previous section, and for each exam, I run the following regression:

$$Any\ Fin\ Restatement_{i,s,t} = \beta I_{s,t}^{POST} + \beta_L I_{s,t}^{\Delta LAW} + \gamma_L \left(I_{s,t}^{POST} \times I_{s,t}^{\Delta LAW} \right) + \\ + \gamma_r RiskVars_{i,t} + \gamma_x X_{i,s,t} + \alpha_s + \alpha_t + \epsilon_{i,s,t}. \quad (9)$$

In this case, if post-term revolvers respond to incentives, the cross-term coefficient γ_L will be positive, and post-term revolver exams will result in more financial restatements after the changes take effect. I compare the results for predicted and realized post-term revolvers, for both all exams and early exams, and for firms of all sizes and firms that are comparable in size to potential future employers.

Results are shown in Table 13. I find that early post-term revolver exams are more likely to result in financial restatement after revolving door laws strengthen. The result is robust to using both predicted and realized post-term revolvers as a control group. Also, the effect is of a larger magnitude for firms comparable in size to potential employers. When I include all exams, the results preserve their direction. However, the only statistically significant γ_L is for realized post-term revolvers and for firms of comparable sizes. Errors on the estimate increase for predicted post-term revolvers. Figure 7 shows the difference in the likelihood of financial restatements by years to law change among early exams for firms that are comparable in size to potential employers for predicted and realized post-term revolvers. No pre-trend seems to exist before the law changes, and it takes two or more years after the law change for the behavior to change.

8 Conclusion

In this paper, I study the effect of the revolving door on insurance solvency regulation. I show that 38% of insurance commissioners enter the insurance industry after their term, and while in office, these post-term revolvers are laxer regarding financial oversight.

Some of this laxer behavior results in less information available on the market about firms' solvency. Specifically, AM Best's FSRs respond to financial restatements, and post-term revolvers are less likely to force restatements. Taken together, these two findings imply that when a post-term revolver is in office, AM Best's FSRs contain more uncertainty.

The ratings are used by consumers hoping to purchase insurance products, investors in the insurance firms, and the stock market, so lack of transparency can negatively affect markets in general.

I also find that post-term revolvers respond to changes in incentives. Whenever a commissioner is affected by a law, which restricts their ability to work for insurance after leaving office, post-term revolvers increase their rate of examination. These findings have implications for revolving door laws. If states prefer tougher insurance regulation, one way to encourage the behavior is to put in place stronger revolving door laws.

Laxer solvency regulation can have potentially far-reaching consequences. The current system tasks the domicile state with ensuring the solvency of each firm. If the domicile state commissioners are being overly lax regulators, the other states are likely carrying a risk they are not aware of. Therefore, further research is needed on the effects of laxer financial regulation on social welfare.

Additionally, Becker and Opp (2013) document that regulators exercise regulatory forbearance and in times of crises they have changed the rules in favor of companies. When regulators are too close to insurers, the system can become more fragile. Another avenue for potential further research is to explicitly try to quantify these effects using a model.

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Figures

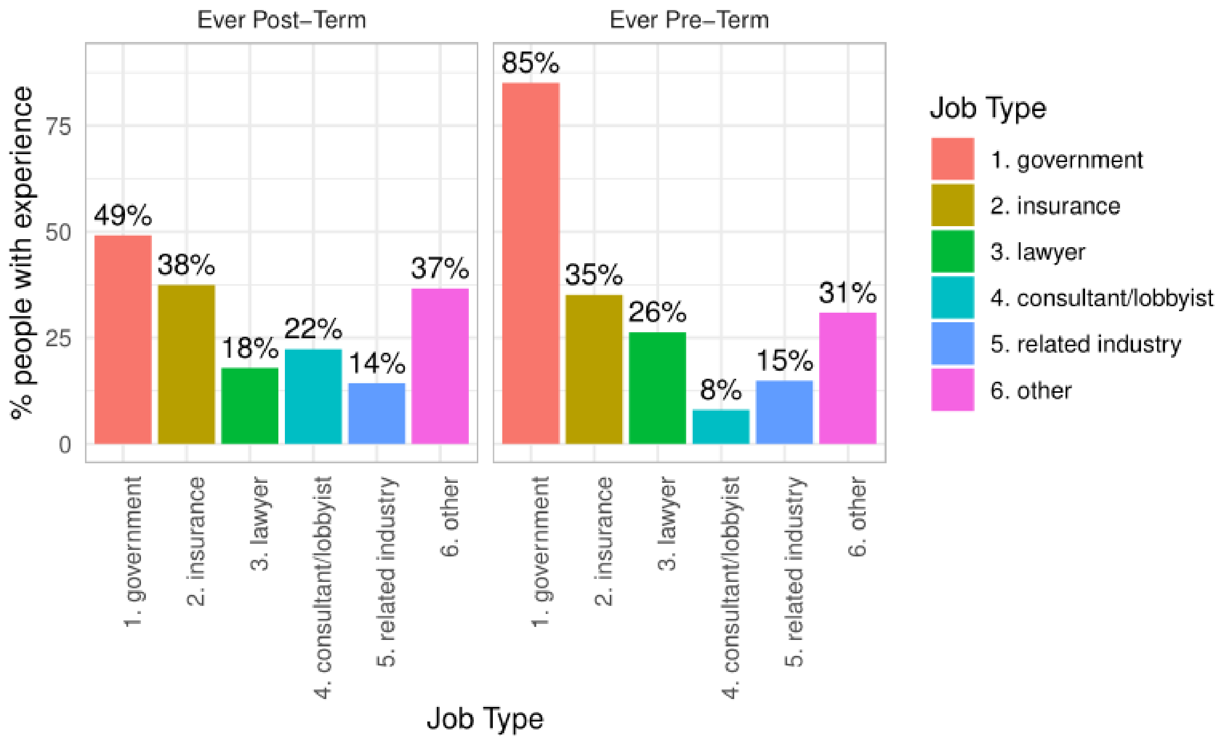

Figure 1: Percent of commissioners with given experience - full employment history

I take the full employment history of each commissioner in the data set. Each event is classified as one of the six categories described in the Figure. Each bar represents the percent of commissioners with at least one employment event in the given job category.

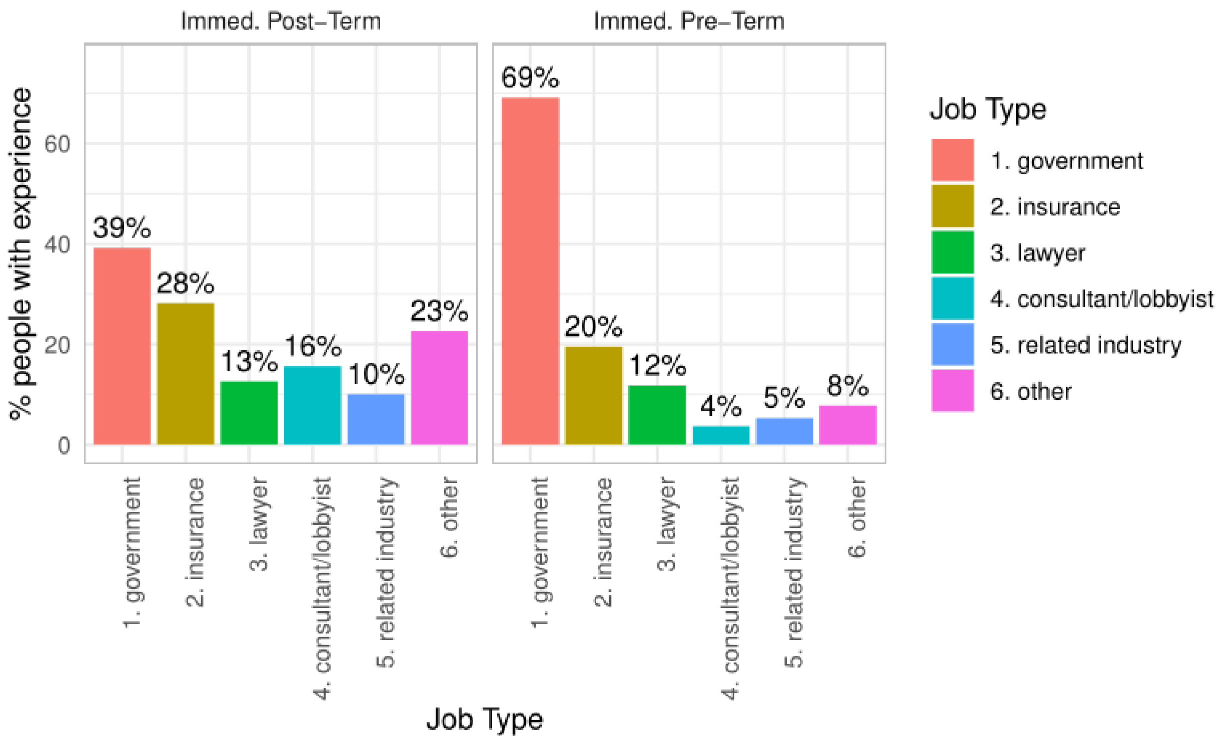

Figure 2: Percent of commissioners with given experience - employment history immediately before/after commissioner term

I take the employment history of each commissioner within a year of the beginning or end of their term. Each event is classified as one of the six categories described in the Figure. Each bar represents the percent of commissioners with at least one employment event in the given job category.

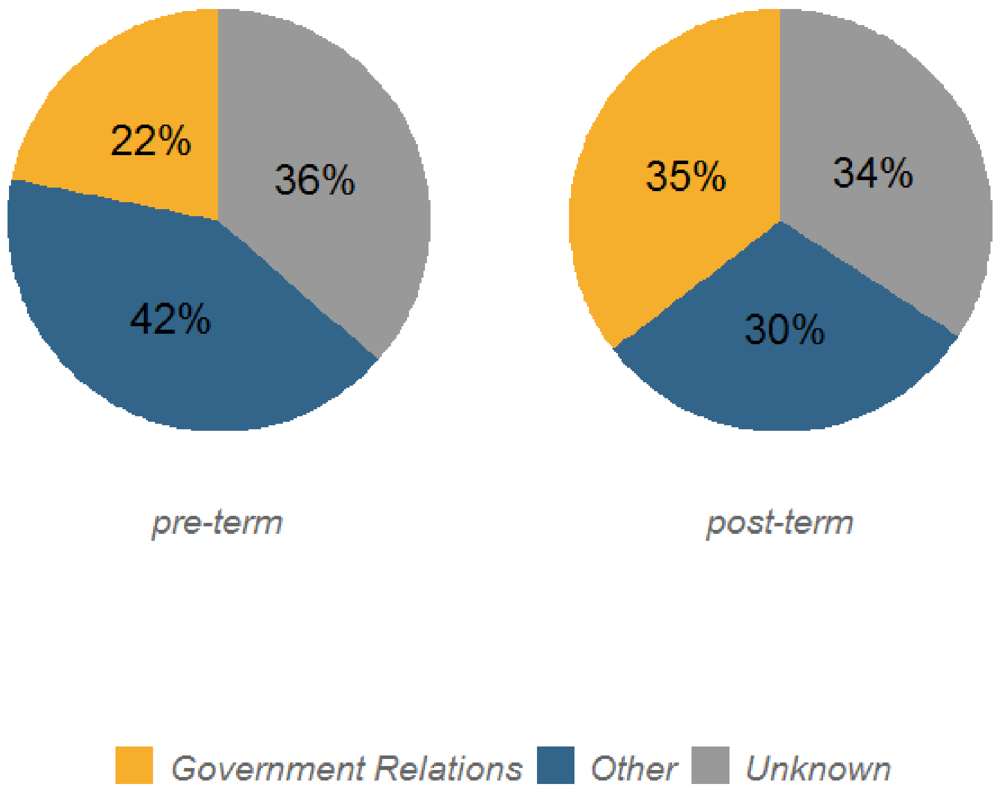

Figure 3: Percent of revolvers whose job involves having government connection (e.g., VP government affairs)

The information is collected by classifying each insurance job. Some job titles were descriptive (e.g. "VP government strategy," which is government relations job, or "VP of Strategy," which is not a government relations job). Other job events had a more expanded job description, from which I can classify whether the revolver worked in government relations. Still, around 35% of all jobs were too vague to classify definitively one way or another. For example, it was only known that the revolver worked as "president", or "CEO".

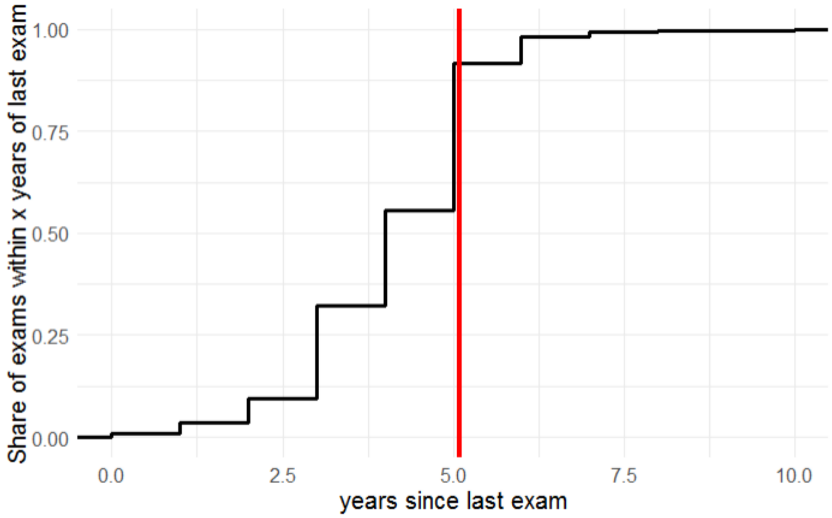

Figure 4: Distribution of the years between completion of financial examinations

This graph represents the cumulative distribution of the time between exams. Specifically, the y axis shows the share of exams which are completed within no more than x years of the previous exam. The red line is at 5.1 years to show that most exams are completed within 5 years of the previous examinations. The number of years between exams is winzorsized at 10 years to make the plot easier to read - the change is negligible because it affects only 17 exams, or 0.1% of the sample.

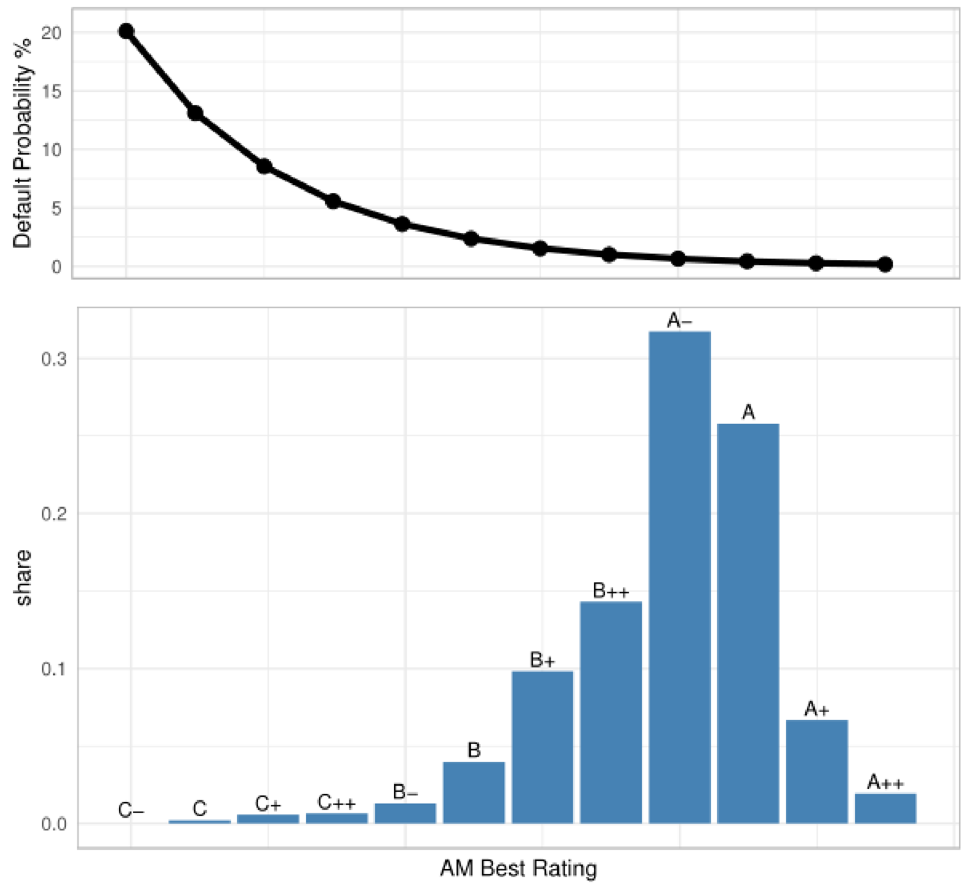

Figure 5: Distribution of Best's Financial Strength Ratings and their corresponding implied default probabilities

The lower panel plots the distribution of all firm-year-level ratings between 2006 and 2018. The upper panel plots the implied default probability of each rating, based on the 10-year default probabilities reported by AM Best in 2018. Appendix E.1 provides more information on how implied default probability was estimated.

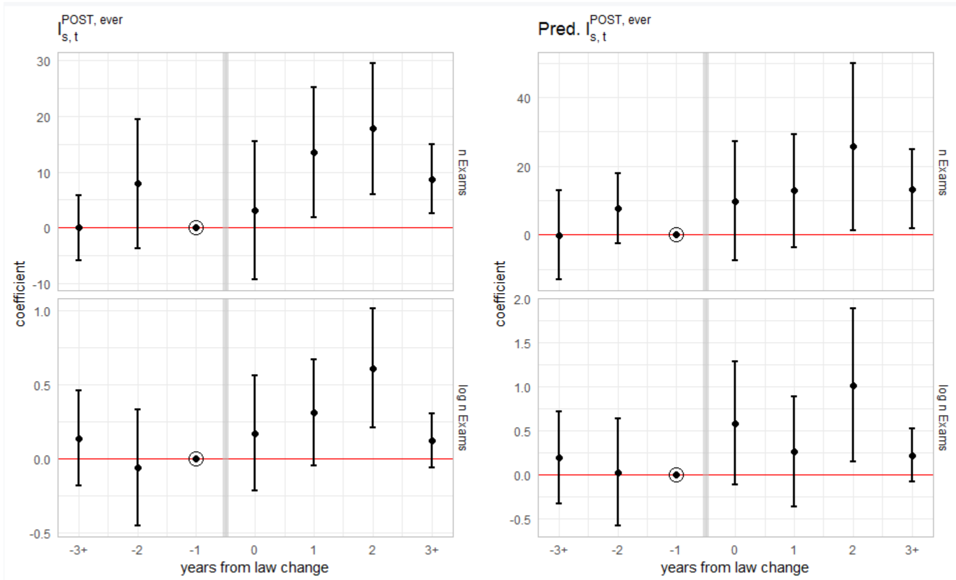

Figure 6: Difference in examination rates by years to law change

This figure presents the coefficient estimate β_m from the equation below, against years to treatment m. The estimate represents the difference in financial examinations between the treatment group of post-term revolvers and the control group of non-revolvers, 3 years before and after the change. The rest of the years are grouped because those bins have too few observations. The graphs on the left use realized post-term revolver as a treatment group, and graphs on the right use predicted post-term revolvers as a treatment group. The top row uses as the LHS the number of financial exams in state s and year t, and the bottom uses log of that number.

$$Y_{s,t} = \sum_m \beta_m \{treated_s \times \text{m yrs from law change}\} + \alpha_s + \alpha_t + X_{s,t}$$

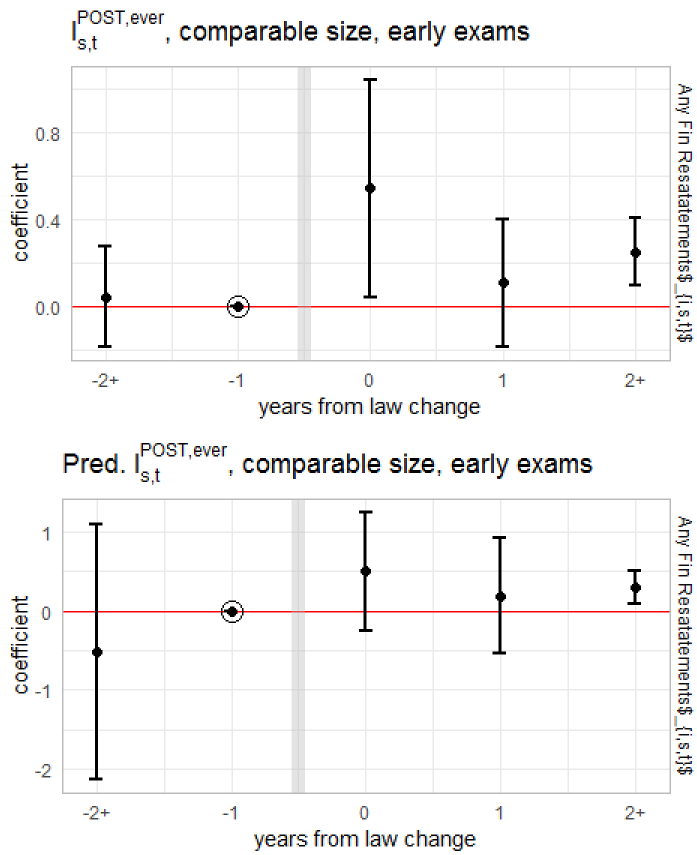

Figure 7: Difference in likelihood of exam resulting in financial restatement, by years to law change

This figure presents the coefficient estimate β_m from the equation below, against years to treatment m, among early exams in firms that are comparable in size to the potential revolver employers. The estimate represents the difference in the likelihood of a financial examination results in financial restatement between the treatment group of post-term revolvers and the control group of non-revolvers, 2 years before and after the change. The rest of the years are grouped because those bins have too few observations. The graph on the top uses realized post-term revolvers as a treatment group, and the graph on the bottom uses predicted post-term revolver as a treatment group.

$$AnyFinRestatement_{i,s,t} = \sum_m \beta_m \{treated_s \times \text{m yrs from law change}\} + \alpha_s + \alpha_t + X_{i,s,t}$$

Tables

Table 1: Summary Statistics (2000-2017)

Variable	n	mean	sd	min	q10	median	q90	max
Panel A: Revolver V								
$I_{s,t}^{POST,ever}$	834	0.43	0.5	0	0.0	0	1.0	1.0
$I_{s,t}^{POST,immed}$	739	0.35	0.5	0	0.0	0	1.0	1.0
$I_{s,t}^{PRE}$	992	0.35	0.5	0	0.0	0	1.0	1.0
Panel B: Number of financial exams								
n Fin Exams Total$_{s,t}$	997	29.78	29.7	0	4.0	20	70.4	187.0
n Fin Exams Domestic$_{s,t}$	996	29.64	29.6	0	4.0	20	70.0	187.0
log(1+n Fin Exams Domestic$_{s,t}$)	996	2.99	1.0	0	1.6	3	4.3	5.2
Panel C: State-year control variables								
n Domestic Firms$_{s,t}$	997	159.93	161.0	4	26.0	106	390.0	1264.0
n All Firms$_{s,t}$	997	1551.48	265.5	679	1262.4	1536	1868.4	2586.0
log(budget$_{s,t}$)	997	16.47	0.9	14.1	15.5	16.4	17.5	19.2
n examiners$_{s,t-1}$	997	29.01	37.0	0.0	4.0	18.0	68.0	320.0
log(1 + n examiners$_{s,t}$)	997	2.86	1.1	0.0	1.6	2.9	4.2	5.8
Panel D: Actions against insurers based on solvency concerns								
n cert. revoked$_{s,t}$	989	1.95	3.9	0	0.0	0	6.0	48.0
n cert. suspended$_{s,t}$	989	3.50	5.3	0	0.0	1	10.0	40.0
n delinquency order$_{s,t}$	830	0.68	3.0	0	0.0	0	1.0	67.0
Panel E: Firm-level risk-based variables								
ACL RBC$_{L1}$ (std)	69924	0.0	0.9	-0.4	-0.4	-0.3	0.6	6.8
ΔACL RBC (std)	69016	0.0	0.9	-0.6	-0.2	-0.1	0.0	22.1
op.Loss/tot.Assets (std)	71736	0.0	0.8	-6.5	-0.5	-0.1	0.4	18.5
Δtot.Assets (std)	72928	0.0	0.8	-1.2	-0.3	-0.1	0.1	22.5
leverage Ratio$_{L1}$ (std)	73580	0.0	0.9	-5.0	-1.5	0.1	1.2	5.2
Δlev. Ratio (std)	72633	0.0	0.8	-1.2	-0.1	-0.1	0.0	26.4
tot.Assets$_{L1}$ (std)	73581	0.0	1.1	-0.2	-0.2	-0.2	0.1	11.0
Panel F: FSR variables								
Δ Default Prob$_{i,s,t}$%	5855	0.0	0.8	-11.6	0	0	0	28.5
new Fin Rstmt$_{i,s,t}$	6530	0.1	0.3	0.0	0	0	0	1.0
isExamYear$_{i,s,t}$	6669	0.2	0.4	0.0	0	0	1	1.0
n yrs since last exam$_{i,s,t}$	6669	1.8	1.5	0.0	0	2	4	10.0
Panel G: Other firm-level variables								
any Recommendations$_{i,t}$	59943	0.6	0.5	0.0	0.0	1.0	1.0	1.0
any Fin. Restatements$_{i,t}$	59943	0.3	0.5	0.0	0.0	0.0	1.0	1.0
$I_{s,t}^{POST,immed}$	46967	0.3	0.5	0.0	0.0	0.0	1.0	1.0
$I_{s,t}^{POST,ever}$	49711	0.4	0.5	0.0	0.0	0.0	1.0	1.0
n yrs since last exam$_{i,s,t}$	59943	1.7	1.6	0.0	0.0	1.0	4.0	28.0

Table 2: Number of exams by post-term revolver status

The table below summarizes results from regressing a measure of exams conducted in state s and year t on whether the commissioner in office is a post-term revolver:

$$Y_{s,t} = \alpha_s + \alpha_t + \beta I^{POST}_{s,t} + \gamma_x X_{s,t} + \epsilon_{s,t}.$$

The dependent variable $Y_{s,t}$ is either absolute number of domestic financial exams in state s and year t (columns (1) through (4)), or the logged version of the variable (columns (5) through (8)).

$I^{POST,ever}_{s,t}$ is an indicator variable that is 1 if the commissioner in office in state s in year t will work for insurance industry at any point after being commissioner. This is the variable of interest in columns (1), (2), (5), and (6). $I^{POST,immed}_{s,t}$ is an indicator variable that is 1 if the commissioner's job following leaving office is in insurance industry. This is the variable of interest in columns (3), (4), (7), and (8).

The control variables in $X_{s,t}$ include (i) whether the commissioner worked for the insurance industry at any point prior to his commissioner term ($I^{PRE}_{s,t}$), (ii) the number of domestic firms in state s, year t (n Dom Frms), (iii) log of the budget that the state insurance department had in year s and state t, and (iv) log of the number of financial analysts available to the insurance department in state s, year $t-1$. Regressions (2), (4), (6), and (8) include these control variables. All regressions include state fixed effects and year fixed effects and standard errors are clustered at the state level.

	Dependent variable:							
	n Dom Fin Exams$_{s,t}$				log(1 + n Dom Fin Exams$_{s,t}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$I^{POST,ever}_{s,t}$	-3.748^{**}	-2.948^{*}			-0.109^{**}	-0.084^{**}		
	(1.854)	(1.644)			(0.050)	(0.042)		
$I^{POST,immed}_{s,t}$			-6.006^{**}	-4.826^{**}			-0.127^{**}	-0.100^{*}
			(2.440)	(2.240)			(0.062)	(0.054)
$I^{PRE}_{s,t}$		0.211		0.584		0.062		0.093^{*}
		(1.535)		(1.757)		(0.048)		(0.052)
n Dom Firms$_{s,t}$		0.025		0.021				
		(0.021)		(0.020)				
log(n Dom Firms$_{s,t}$)						0.173^{***}		0.148^{**}
						(0.058)		(0.059)
log(budget$_{s,t}$)		5.048		4.376		0.126		0.115
		(4.828)		(4.790)		(0.092)		(0.091)
log(1+n examiners$_{s,t-1}$)		-4.594		-4.188		0.003		0.022
		(3.651)		(3.954)		(0.056)		(0.060)
E[LHS]	29.6	29.6	29.6	29.6	3	3	3	3
Empl.Hist.	full	immed	full	immed	full	immed	full	immed
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s	s	s
Observations	834	829	739	737	834	829	739	737
R^2	0.871	0.876	0.878	0.882	0.876	0.880	0.880	0.883
Adjusted R^2	0.860	0.864	0.866	0.869	0.865	0.868	0.868	0.871

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

Table 3: Exam outcomes by post-term revolver status

The regression estimates which factors lead to negative outcomes of the exams. Each observation is a unique firm exam-year combination:

$$Exam\ Outcome_{i,s,t} = I^{POST,}_{i,s,t} + RiskVars_{i,s,t} + X_{i,s,t} + \epsilon_{i,s,t}.$$

The LHS variable in columns (1-4) is indicator variable *Any Financial Restatements*$_{i,s,t}$ which is 1 if the exam that took place in year t for firm i resulted in any financial restatements. The LHS variable in columns (5-8) is indicator variable *Any Recommendations*$_{i,s,t}$, which is 1 if the exam resulted in any recommendations for the firm.

In columns (1), (2), (5), and (6), $I^{POST}_{s,t} = 1$ when the commissioner works in insurance at any point after leaving office. In columns (3), (4), (7), and (8), $I^{POST}_{s,t} = 1$ when the commissioner is a post-term revolver immediately after leaving office. Columns (1), (3), (5), and (7) include all exams. Columns (1), (3), (5), and (7) include only the exams conducted within 3 years of the firm's previous exam. All regressions include state fixed effects and year fixed effects, and standard errors are clustered at the state level.

	Dependent variable:							
	Any Financial Restatements$_{i,s,t}$				Any Recommendations$_{i,s,t}$			
	$I^{POST,ever}_{s,t}$		$I^{POST,immed}_{s,t}$		$I^{POST,ever}_{s,t}$		$I^{POST,immed}_{s,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$I^{POST}_{s,t}$	-0.027^{**}	-0.044^{**}	-0.026^{**}	-0.045^{***}	-0.026	-0.047^{**}	-0.026	-0.066^{***}
	(0.013)	(0.020)	(0.010)	(0.014)	(0.018)	(0.024)	(0.018)	(0.020)
regulatory ratio$_{L1}$ (std)	-0.039^{***}	-0.035^{***}	-0.038^{***}	-0.033^{***}	-0.051^{***}	-0.060^{***}	-0.051^{***}	-0.065^{***}
	(0.007)	(0.012)	(0.007)	(0.012)	(0.010)	(0.011)	(0.010)	(0.011)
ΔACL RBC (std)	-0.016^{**}	-0.026^{***}	-0.016^{**}	-0.025^{***}	-0.017	-0.025	-0.017	-0.024
	(0.007)	(0.008)	(0.007)	(0.008)	(0.012)	(0.021)	(0.012)	(0.021)
tot.Assets$_{L1}(std)$)	-0.009	-0.017^{*}	-0.010	-0.016^{*}	-0.023^{**}	-0.023^{**}	-0.023^{**}	-0.021^{**}
	(0.007)	(0.009)	(0.006)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)
Δtot.Assets (std)	0.003	0.0003	0.003	-0.0001	-0.002	-0.002	-0.002	-0.002
	(0.010)	(0.014)	(0.010)	(0.014)	(0.009)	(0.017)	(0.009)	(0.017)
leverage Ratio$_{L1}$ (std)	0.003	0.015	0.004	0.018	0.009	0.009	0.009	0.009
	(0.009)	(0.012)	(0.009)	(0.012)	(0.011)	(0.012)	(0.011)	(0.012)
Δlev. Ratio (std)	0.015^{*}	0.020^{**}	0.015^{*}	0.020^{**}	0.009^{***}	0.010^{***}	0.009^{***}	0.011^{***}
	(0.009)	(0.009)	(0.009)	(0.009)	(0.003)	(0.002)	(0.003)	(0.002)
op.Loss/Assets (std)	-0.016^{*}	-0.014	-0.014	-0.013	-0.001	0.002	-0.001	0.003
	(0.009)	(0.014)	(0.009)	(0.014)	(0.011)	(0.013)	(0.011)	(0.013)
$I^{PRE}_{s,t}$	0.011	0.037	0.021	0.055^{**}	-0.030^{*}	-0.029	-0.030^{*}	-0.007
	(0.016)	(0.024)	(0.018)	(0.025)	(0.017)	(0.024)	(0.017)	(0.025)
n yrs since last exam	0.001	0.011	0.001	0.011	0.009^{*}	0.007	0.009^{*}	0.008
	(0.006)	(0.013)	(0.006)	(0.014)	(0.005)	(0.008)	(0.005)	(0.009)
log(budget$_{s,t}$)	0.009	-0.016	0.009	-0.019	0.008	0.007	0.008	0.017
	(0.038)	(0.041)	(0.037)	(0.039)	(0.049)	(0.047)	(0.049)	(0.042)
log(1 + n examiners$_{s,t-1}$)	-0.015	-0.063	0.002	-0.036	-0.033	-0.049	-0.033	-0.026
	(0.045)	(0.049)	(0.043)	(0.049)	(0.045)	(0.054)	(0.045)	(0.053)
E(LHS)	0.34	0.35	0.34	0.35	0.66	0.7	0.66	0.7
exams	all	$\leq$ 4y	all	$\leq$ 4y	all	$\leq$ 4y	all	$\leq$ 4y
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s	s	s
Observations	8,233	4,657	7,975	4,461	8,233	4,657	8,233	4,461
R^2	0.078	0.095	0.077	0.095	0.109	0.126	0.109	0.127
Adjusted R^2	0.070	0.081	0.069	0.080	0.102	0.112	0.102	0.113

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

Table 4: Predicting early exams

$$isExamYear_{i,s,t} = \beta I_{s,t}^{POST,ever} + \beta_r RiskVars_{i,t} + \gamma \left(I_{s,t}^{POST,ever} \times RiskVars_{i,t} \right) + \gamma_x X_{i,s,t} + \epsilon_{i,s,t}$$

This regression limits firm-year observations to n years after firm i's most recent examination, and tries to estimate which factor predict early (within 4 years of most recent exam). $isExamYear_{i,s,t}$ is an indicator variable that equals 1 whenever firm i was examined in firm t.

	Dependent variable:					
	$isExamYear_{i,s,t}$					
	(1)	(2)	(3)	(4)	(5)	(6)
$I_{s,t}^{POST,ever}$	-0.013^{**}	-0.013^{**}	-0.014^{**}	-0.014^{**}	-0.013^{**}	-0.014^{**}
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
ACL RBC_{L1} (std)	-0.005	-0.007^{***}	-0.005	-0.005	-0.005	-0.008^{***}
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
$I_{s,t}^{POST,ever} \times$ ACL RBC_{L1} (std)		0.006^{*}				0.009^{**}
		(0.003)				(0.004)
ΔACL RBC (std)	0.001	0.003	0.001	0.001	0.001	0.003
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
$I_{s,t}^{POST,ever} \times$ ΔACL RBC (std)		-0.004				-0.004
		(0.004)				(0.004)
$\text{tot.Assets}_{L1}(std))$	-0.006^{***}	-0.006^{***}	-0.007^{***}	-0.006^{***}	-0.006^{***}	-0.007^{***}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
$I_{s,t}^{POST,ever} \times$ $\text{tot.Assets}_{L1}(std))$			0.002			0.001
			(0.003)			(0.003)
Δtot.Assets (std)	0.002	0.001	0.004	0.002	0.002	0.004
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
$I_{s,t}^{POST,ever} \times$ Δtot.Assets (std)			-0.005			-0.005
			(0.004)			(0.004)
leverage Ratio_{L1} (std)	0.003	0.003	0.003	0.001	0.003	0.0001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
$I_{s,t}^{POST,ever} \times$ leverage Ratio_{L1} (std)				0.004		0.006
				(0.003)		(0.004)
Δlev. Ratio (std)	-0.00001	-0.00001	0.0001	-0.00003	0.00001	-0.0001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
$I_{s,t}^{POST,ever} \times$ Δlev. Ratio (std)				0.00002		0.0003
				(0.005)		(0.005)
op.Loss/Asset (std)	0.002	0.002	0.002	0.002	0.003	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
$I_{s,t}^{POST,ever} \times$ op.Loss/Asset (std)					-0.004	-0.002
					(0.004)	(0.004)
n yrs since last exam	0.105^{***}	0.105^{***}	0.105^{***}	0.105^{***}	0.105^{***}	0.105^{***}
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
$I_{s,t}^{PRE}$	0.023^{*}	0.023^{*}	0.023^{*}	0.023^{*}	0.023^{*}	0.023^{*}
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
$\log(\text{budget}_{s,t})$	-0.034	-0.033	-0.034	-0.034	-0.034	-0.033
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
$\log(1 + \text{n examiners}_{s,t-1})$	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002
	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)	(0.012)
E(LHS)	0.12	0.12	0.12	0.12	0.12	0.12
State FE and Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s
Observations	43,599	43,599	43,599	43,599	43,599	43,599
R^2	0.166	0.166	0.166	0.166	0.166	0.166
Adjusted R^2	0.164	0.164	0.164	0.164	0.164	0.164
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 5: Regulatory actions taken against company based on solvency concern by post-term revolver status

The table below summarizes results from regressing a measure of regulatory actions based on solvency concerns in state s and year t on whether the commissioner in office is a post-term revolver:

$$Y_{s,t} = \alpha_s + \alpha_t + \beta I_{s,t}^{POST} + \gamma_x X_{s,t} + \epsilon_{s,t}$$

The dependent variable $Y_{s,t}$ is number of certificates suspended (columns (1-2)), number of certificates permanently revoked (columns (3-4)), and number of delinquency orders (columns (5-6)) in state s in year t.

$I_{s,t}^{POST,ever}$ is an indicator variable which is 1 if the commissioner in office in state s in year t works in insurance industry at any point after her term ends.

The control variables $X_{s,t}$ include: (i) whether the commissioner worked for insurance industry at any point prior his commissioner term ($I_{s,t}^{PRE}$), (ii) the number of domestic firms in state s, year t (n Dom Frms), (iii) log of the budget that the state insurance department had in year s and state t, (iv) log of the number of financial analysts available to the insurance department in state s, year $t-1$. Regressions (2), (4) and (6) include these control variables. All regressions include state fixed effects and year fixed effects and standard errors are clustered at the state level.

	Dependent variable:					
	n certificates suspended$_{s,t}$		n certificates revoked$_{s,t}$		n delinquency orders$_{s,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
$I_{s,t}^{POST}$	-1.072^{**}	-0.865^{*}	-0.045	0.017	-0.424^{**}	-0.408^{**}
	(0.495)	(0.510)	(0.439)	(0.445)	(0.207)	(0.192)
$I_{s,t}^{PRE}$		-0.380		-0.043		-0.252
		(0.637)		(0.323)		(0.283)
n Dom Fin Exams$_{s,t}$		0.034^{**}		0.010		0.014
		(0.016)		(0.008)		(0.012)
log(budget$_{s,t}$)		0.664		0.971		-0.424
		(0.892)		(0.632)		(0.536)
log(1 + n examiners$_{s,t-1}$)		-0.633		0.209		-0.119
		(0.778)		(0.368)		(0.514)
E[LHS]	3.5	3.5	1.9	1.9	0.7	0.7
Empl.Hist.	full	full	full	full	full	full
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s
Observations	830	825	830	825	682	682
R^2	0.577	0.585	0.353	0.356	0.259	0.262
Adjusted R^2	0.539	0.545	0.295	0.294	0.183	0.181
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 6: Number of exams by post-term revolver status and years to leaving office

The table below summarizes results from regressing a measure of exams conducted in state s and year t on whether the commissioner in office is a post-term revolver and whether this is commissioner's last two years in office:

$$Y_{s,t} = \alpha_s + \alpha_t + \beta I^{POST}_{s,t} + \beta_T I^{T}_{s,t} + \beta_{T-1} I^{T-1}_{s,t} + \gamma_T \left(I^{POST}_{s,t} \times I^{T}_{s,t} \right) + \gamma_{T-1} \left(I^{POST}_{s,t} \times I^{T-1}_{s,t} \right) + X_{s,t} + \epsilon_{s,t}$$

The dependent variable $Y_{s,t}$ is either absolute number of domestic financial exams in state s and year t (column (1) through (4)), or the log-ed version of the variable (column (5) through (8)).

$I^{POST,ever}_{s,t}$ is an indicator variable which is 1 if the commissioner in office in state s in year t will work for insurance industry at any point after being commissioner. This is the variable of interest in columns (1), (2), (5) and (6). $I^{POST,immed}_{s,t}$ is indicator variable which is 1 if the commissioner's job following leaving office is in insurance industry. This is the variable of interest in columns (3), (4), (7) and (8). $I^{T}_{s,t}/I^{T-1}_{s,t}$ are indicator variables which equal 1 if year t is the last/the year before the last for the commissioner currently in office in state s.

The control variables in $X_{s,t}$ include: i) whether the commissioner worked for insurance industry at any point prior his commissioner term ($I^{PRE}_{s,t}$), (ii) the number of domestic firms in state s, year t (n Dom Frms), (iii) log of the budget that the state insurance department had in year s and state t, (iv) log of the number of financial analysts available to the insurance department in state s, year $t-1$, (v) fixed effects for the election cycle (0,1,2 or 3 years to the next election). Regressions (2), (4) and (6), (8) include these control variables. All regressions include state fixed effects and year fixed effects and standard errors are clustered at the state level.

	Dependent variable:							
	n Dom Fin Exams$_{s,t}$				log(1+ n Dom Fin Exams$_{s,t}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$I^{POST,ever}_{s,t} \times I^{T-1}_{s,t}$	3.603*	4.048**			0.135*	0.119*		
	(2.111)	(1.896)			(0.070)	(0.063)		
$I^{POST,immed}_{s,t} \times I^{T-1}_{s,t}$			5.284*	5.623**			0.190**	0.170**
			(2.705)	(2.517)			(0.081)	(0.072)
$I^{POST,ever}_{s,t} \times I^{T}_{s,t}$	−0.518	0.711			0.028	0.011		
	(3.096)	(2.706)			(0.087)	(0.085)		
$I^{POST,immed}_{s,t} \times I^{T}_{s,t}$			−2.552	−1.325			−0.035	−0.053
			(3.747)	(3.223)			(0.095)	(0.092)
$I^{POST,ever}_{s,t}$	−4.675**	−4.088**			−0.146**	−0.109*		
	(2.142)	(1.957)			(0.068)	(0.058)		
$I^{POST,immed}_{s,t}$			−6.961**	−6.028**			−0.164**	−0.126*
			(2.969)	(2.895)			(0.079)	(0.066)
$I^{T}_{s,t}$	0.781	0.245	1.152	0.609	0.006	0.026	0.026	0.045
	(1.675)	(1.908)	(1.860)	(2.022)	(0.051)	(0.053)	(0.048)	(0.050)
$I^{T-1}_{s,t}$	−1.340	−1.934	−2.167	−2.688	−0.022	−0.016	−0.038	−0.028
	(1.528)	(1.763)	(2.059)	(2.142)	(0.049)	(0.048)	(0.048)	(0.047)
E[LHS]	29.6	29.6	29.6	29.6	3	3	3	3
Empl.Hist.	full	immed	full	immed	full	immed	full	immed
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s	s	s
Observations	824	815	729	723	824	815	729	723
R^2	0.871	0.877	0.879	0.883	0.877	0.882	0.881	0.886
Adjusted R^2	0.859	0.864	0.866	0.869	0.865	0.869	0.868	0.872
Note:								*p<0.1; **p<0.05; ***p<0.01

Table 7: Early exams only: are early exams more likely at the end of tenure?

$$isExamYear_{i,s,t} = \beta I_{s,t}^{POST} \times \left(I_{s,t}^{T-1} + I_{s,t}^{T}\right) + \beta_r RiskVars_{i,t} + \gamma \left(I_{s,t}^{POST} \times RiskVars_{i,t}\right) + \gamma_x X_{i,s,t} + \epsilon_{i,s,t}$$

This regression limits firm-year observations to n years after firm i's most recent examination, and tries to estimate which factors predict early exams. $isExamYear_{i,s,t}$ is an indicator variable, which equals 1 whenever firm i was examined in firm t. $I_{s,t}^{T}/I_{s,t}^{T-1}$ are indicator variables which equal 1 if year t is the last/the year before the last for the commissioner currently in office in state s.

Post-term revolver is defined as follows: in columns (1-3): commissioner who works in insurance at any time after leaving office; in columns (4-6): commissioner who works in insurance immediately after term. Early exam is defined as follows: in columns (1) and (4): an exam conducted 2 years or less after firm's previous exam; in columns (2) and (5): an exam conducted 3 years or less after firm's previous exam; in columns (3) and (6): an exam conducted 4 years or less after firm's previous exam.

$RiskVars_{i,t}$ control variables include the ratio of operation loss to total assets in state s and year t, as well as lagged level and changes in log total assets, regulatory capital ratio, and leverage ratio. The control variables in $X_{s,t}$ include: (i) whether the commissioner worked for insurance industry at any point prior his commissioner term ($I_{s,t}^{PRE}$), (ii) the number of domestic firms in state s, year t (n Dom Frms), (iii) log of the budget that the state insurance department had in year s and state t, (iv) log of the number of financial analysts available to the insurance department in state s, year $t-1$. All regressions include state fixed effects and year fixed effects and standard errors are clustered at the state level.

	Dependent variable:					
	isExamYear $_{i,s,t}$					
	$I_{s,t}^{POST,ever}$			$I_{s,t}^{POST,immed}$		
	(1)	(2)	(3)	(4)	(5)	(6)
$I_{s,t}^{POST} \times I_{s,t}^{T-1}$	0.015**	0.025**	0.026***	0.005	0.021**	0.041***
	(0.007)	(0.011)	(0.009)	(0.010)	(0.011)	(0.013)
$I_{s,t}^{POST} \times I_{s,t}^{T}$	0.001	0.012	0.016	0.003	0.005	0.019
	(0.012)	(0.012)	(0.014)	(0.011)	(0.013)	(0.013)
$I_{s,t}^{POST}$	−0.025**	−0.059***	−0.040**	−0.047**	−0.065**	−0.074***
	(0.012)	(0.019)	(0.020)	(0.021)	(0.029)	(0.028)
$I_{s,t}^{T-1}$	−0.010	−0.006	−0.001	−0.004	−0.006	−0.005
	(0.007)	(0.009)	(0.009)	(0.006)	(0.008)	(0.009)
$I_{s,t}^{T}$	0.0005	0.009	0.008	−0.001	0.007	0.005
	(0.008)	(0.008)	(0.009)	(0.006)	(0.007)	(0.008)
E(LHS)	0.03	0.08	0.12	0.03	0.08	0.12
exams	$\leq$ 2y	$\leq$ 3y	$\leq$ 4y	$\leq$ 2y	$\leq$ 3y	$\leq$ 4y
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s
Observations	21,642	30,298	36,267	20,307	28,547	34,316
R^2	0.036	0.117	0.159	0.036	0.110	0.154
Adjusted R^2	0.032	0.114	0.156	0.031	0.107	0.152

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

Table 8: Change in AM Best default probability when an exam results in financial restatement

In the firm-year panel below, LHS variable is the % change in default probability for firm i between years $t-1$ and t.

new fin. $\text{rstmt}_{i,s,t}$ is an indicator variable which equals 1 if in year t, an exam was released for firm i, domiciled in state s, and the exam resulted in financial restatement.

Column (1) includes state FE and year FE. Column (2) includes state × year FE and standard. Other control variables in $X_{i,s,t}$ include an indicator variable $\text{isExamYear}_{i,s,t}$, which equals 1 if exam was released in year t and number of years since last exam. All standard errors are clustered at the state level.

$$\Delta\text{Default Probability}_{i,s,t} = \text{new fin. rstmt}_{i,s,t} + RiskVars_{i,s,t} + X_{i,s,t} + \epsilon_{i,s,t}$$

	Dependent variable:	
	Δ Default $\text{Probability}_{i,s,t}$%	
	(1)	(2)
new fin $\text{rstmt}_{i,s,t}$	0.072*	0.079*
	(0.043)	(0.047)
$\text{isExamYr}_{i,s,t}$	−0.038	−0.026
	(0.037)	(0.035)
n yrs since last $\text{exam}_{i,s,t}$	−0.002	0.003
	(0.011)	(0.011)
regulatory ratio_{L1} (std)	0.046*	0.008
	(0.024)	(0.023)
ΔACL RBC (std)	0.033	0.039
	(0.030)	(0.035)
$\text{tot.Assets}_{L1}(std)$)	−0.011**	−0.009*
	(0.005)	(0.005)
Δtot.Assets (std)	0.241	0.226
	(0.160)	(0.164)
leverage Ratio_{L1} (std)	0.091***	0.085**
	(0.035)	(0.036)
Δlev. Ratio (std)	−0.057	−0.038
	(0.065)	(0.061)
op.Loss/Assets (std)	−0.314**	−0.284**
	(0.126)	(0.122)
E(—LHS—)	0.087	0.087
Year FE + State FE	Yes	No
State-Year FE	No	Yes
Cluster	s	s
Observations	5,683	5,668
R^2	0.039	0.129
Adjusted R^2	0.026	0.022
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 9: Exclude high ratings

In the firm-year panel below, LHS variable is the % change in default probability for firm i between years $t-1$ and t.

new fin. $\text{rstmt}_{i,s,t}$ is an indicator variable which equals 1 if in year t, an exam was released for firm i, domiciled in state s, and the exam resulted in financial restatement.

All regressions include state × year FE and standard errors are clustered at the state level. Other control variables in $X_{i,s,t}$ include an indicator variable $\text{isExamYear}_{i,s,t}$, which equals 1 if exam was released in year t and number of years since last exam. Column (1) includes all ratings, while columns (2), (3), (4), (5) exclude all ratings above, correspondingly, A+, A, A- and B+. Note in AM Best the highest possible rating is A++.

$$\Delta\text{Default Probability}_{i,s,t} = \text{new fin. rstmt}_{i,s,t} + RiskVars_{i,s,t} + X_{i,s,t} + \epsilon_{i,s,t}$$

	Dependent variable:				
	Δ Default $\text{Probability}_{i,s,t}$%				
	(1)	(2)	(3)	(4)	(5)
new fin $\text{rstmt}_{i,s,t}$	0.079*	0.081*	0.089*	0.152*	0.338
	(0.047)	(0.048)	(0.053)	(0.087)	(0.227)
$\text{isExamYr}_{i,s,t}$	−0.026	−0.027	−0.031	−0.058	−0.310
	(0.035)	(0.036)	(0.039)	(0.059)	(0.210)
n yrs since last $\text{exam}_{i,s,t}$	0.003	0.002	0.003	0.003	−0.046
	(0.011)	(0.011)	(0.012)	(0.015)	(0.029)
regulatory ratio_{L1} (std)	0.008	0.011	0.016	0.010	0.023
	(0.023)	(0.023)	(0.026)	(0.045)	(0.258)
ΔACL RBC (std)	0.039	0.038	0.038	0.050	0.130
	(0.035)	(0.035)	(0.035)	(0.050)	(0.238)
$\text{tot.Assets}_{L1}(std))$	−0.009*	−0.014*	−0.011**	0.006	0.078
	(0.005)	(0.008)	(0.005)	(0.022)	(0.136)
Δtot.Assets (std)	0.226	0.224	0.234	0.453	1.047
	(0.164)	(0.165)	(0.178)	(0.303)	(0.781)
leverage Ratio_{L1} (std)	0.085**	0.089**	0.099**	0.132**	0.243**
	(0.036)	(0.038)	(0.043)	(0.059)	(0.107)
Δlev. Ratio (std)	−0.038	−0.036	−0.035	−0.121	−0.002
	(0.061)	(0.062)	(0.067)	(0.102)	(0.173)
op.Loss/Assets (std)	−0.284**	−0.284**	−0.298**	−0.438**	−0.733**
	(0.122)	(0.123)	(0.127)	(0.186)	(0.330)
E(—LHS—)	0.087	0.024	0.026	0.041	0.092
sample	full	<A++	<A+	<A	<A-
State-Year FE	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s
Observations	5,668	5,555	5,159	3,587	1,743
R^2	0.129	0.129	0.132	0.167	0.319
Adjusted R^2	0.022	0.020	0.014	0.004	0.074
Note:					*p<0.1; **p<0.05; ***p<0.01

Table 10: Revolving door state law changes (2000-2017)

state	year	law strength	description
AK	2007	↑	(1) ban on assisting expanded; (2) can't be on board of reg. firms for 1yr
GA	2007	↑	can't register or act as lobbyist for 1yr
ME	2015	↑	can't register as lobbyist for 1yr
MA	2009	↑	increases penalties for appearing in front of agency as agent or attorney for 1yr
NJ	2004	↑	can't register as "government affairs agent" for 1yr
NJ	2006	↑	increases penalties for appearing in front of agency as agent or attorney for 2yr
NM	2011	↑	can't assist businesses affected by regulation
NY	2007	↑	can't appear or practice before any state agency for 2yr
NC	2007	↑	can't register as lobbyist for 6mo
TN	2006	↑	can't be lobbyist for 1yr
VA	2013	↑	ban on lobbying expanded in meaning
WV	2005	↑	ban on appearing in front of agency: from 6mo to 1yr
WV	2011	↑	can't register as lobbyist for 1yr
SD	2011	↓	1yr ban on lobbying removed

Table 11: Comparing state with and with no law changes on observable variables

	n		mean		sd	
Variable	No Change	Change	No Change	Change	No Change	Change
GDP variables (BEA):						
$GDP_{s,t}$ (insurance) [$M, adj]	661	204	8,639	9,292	9,330	11,790
$\Delta GDP_{s,t}$ (insurance) %	620	192	3	3	14	12
$GDP_{s,t}$ [$M, adj]	700	216	327,020	376,085	431,783	369,628
$\Delta GDP_{s,t}$%	659	204	2	2	5	4
State population (2010 Census):						
$Population_{s,2010}$	39	12	5,992,121	6,254,403	7,257,966	5,443,429
$Population_{s,2010}$ % USA	39	12	1.94	2.03	2.35	1.76
Total Insurance Premiums Written (NAIC IDRR):						
Total Premium $Volume_{s,t}$ [$M]	700	216	30,569	33,406	41,065	36,781
ΔTotal Premium $Volume_{s,t}$%	659	204	11	6	112	13

Table 12: DiD around revolving door law changes: number of financial exams

Regression results for:

$$Y_{s,t} = \alpha_s + \alpha_t + \beta^P Pred.I_{s,t}^{POST} + \beta_L I_{s,t}^{\Delta LAW} + \gamma_L^P \left(\text{Pred. } I_{s,t}^{POST} \times I_{s,t}^{\Delta LAW}\right) + \gamma_x X_{s,t} + \epsilon_{s,t}$$

In the DiD setting above $Y_{s,t}$ is either number of financial domestic exams (columns 1-2) or log 1+number of financial domestic exams for year t, state s (columns 3-4). The treatment group is realized post-term revolver status ($I_{s,t}^{POST,ever}$, columns (1) and (3)) or predicted post-term revolver status (Pred. $I_{s,t}^{POST}$, columns (2) and (4)). Predicted post-term revolver status comes from column (1) of Table F.23. The shock indicator $I_{s,t}^{\Delta LAW}$ equals 1/-1 if there has been a law change in state s in the years before $t-1$.
Control variables $X_{s,t}$ include: pre-term revolver realized status, number or log number of domestic firms in state s and year t, log of the budget in state s and year t, and log number of examiners in state s and year $t-1$. All regressions include state and year fixed effects and standard errors are clustered at the state level.

	Dependent variable:			
	n dom fin ex$_{s,t}$		log(1+n dom fin ex$_{s,t}$)	
	$I_{s,t}^{POST,ever}$	Pred. $I_{s,t}^{POST,ever}$	$I_{s,t}^{POST,ever}$	Pred. $I_{s,t}^{POST,ever}$
	(1)	(2)	(3)	(4)
$I_{s,t}^{POST}$	-4.732^{**}	-7.440^{***}	-0.114^{**}	-0.178^{**}
	(1.915)	(2.797)	(0.047)	(0.085)
$I_{s,t}^{\Delta LAW}$	-4.472	-6.243^{*}	-0.146^{***}	-0.213^{***}
	(2.920)	(3.430)	(0.049)	(0.068)
$I_{s,t}^{POST} \times I_{s,t}^{\Delta LAW}$	9.257^{***}	12.659^{**}	0.218^{***}	0.331^{***}
	(2.917)	(5.014)	(0.056)	(0.103)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Cluster	s	s	s	s
Observations	960	943	960	943
R^2	0.865	0.862	0.877	0.872
Adjusted R^2	0.854	0.851	0.867	0.862
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 13: DiD around revolving door law changes: likelihood for exam resulting in financial restatements

Below are the results for regressing for each exam of firm i conducted by state s in year t:

$$AnyFin.Restatements_{s,t} = \alpha_s + \alpha_t + \beta I^{POST}_{s,t} + \beta_L I^{\Delta LAW}_{s,t} + \gamma_L \left(I^{POST}_{s,t} \times I^{\Delta LAW}_{s,t} \right) + \gamma_r RiskVars_{i,t} + \gamma_x X_{s,t} + \epsilon_{s,t}$$

In the DiD setting above, the dependent variable is whether the exam resulted in financial restatement. The treatment group is realized post-term revolver status ($I^{POST,ever}_{s,t}$, columns (1-2) and (5-6)) or predicted post-term revolver status (Pred. $I^{POST}_{s,t}$, columns (3-4) and (7-8)). Predicted post-term revolver status comes from column (1) of Table F.23. The shock indicator $I^{\Delta LAW}_{s,t}$ equals 1/-1 if there has been a law change in state s and the years before $t-1$. In columns (1), (3), (5) and (7) all firms are included. In columns (2), (4), (6), (8) the firms are limited to those whose log assets are between the smallest and largest of the firms which hire commissioners. Columns (1-4) include all exams, and Columns (5-6) include only exams within 3 years of most recent exams.

Risk-related control variables $RiskVars_{i,t}$ include the ratio of operation loss to total assets in state s and year t, as well as lagged level and changes in log total assets, regulatory capital ratio, and leverage ratio. Non-risk control variables $X_{s,t}$ include pre-term revolver realized status, number or log number of domestic firms in state s and year t, log of the budget in state s and year t, and log number of examiners in state s and year $t-1$. All regressions include state and year fixed effects and standard errors are clustered at the state level.

	Dependent variable:							
	Any Financial Restatement$_{i,s,t}$							
	$I^{POST,ever}_{s,t}$		Pred. $I^{POST,ever}_{s,t}$		$I^{POST,ever}_{s,t}$		Pred. $I^{POST,ever}_{s,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$I^{POST}_{s,t}$	-0.034^{**}	-0.070^{***}	-0.032^{*}	-0.043	-0.132^{***}	-0.161^{**}	-0.128^{*}	-0.148
	(0.015)	(0.022)	(0.019)	(0.031)	(0.046)	(0.064)	(0.070)	(0.118)
$I^{\Delta LAW}_{s,t}$	-0.025	-0.044	-0.034	-0.067	-0.014	0.024	0.064^{*}	0.027
	(0.059)	(0.048)	(0.067)	(0.055)	(0.070)	(0.069)	(0.033)	(0.086)
$I^{POST}_{s,t} \times I^{\Delta LAW}_{s,t}$	0.050	0.098^{**}	0.026	0.014	0.132^{**}	0.229^{***}	0.113	0.206^{**}
	(0.032)	(0.041)	(0.032)	(0.061)	(0.063)	(0.082)	(0.069)	(0.105)
exams	all	all	all	all	$\leq$ 3y	$\leq$ 3y	$\leq$ 3y	$\leq$ 3y
firms	all	comp	all	comp	all	comp	all	comp
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s	s	s
Observations	8,500	4,070	8,582	4,152	2,519	1,052	2,504	1,050
R^2	0.091	0.100	0.091	0.098	0.120	0.193	0.118	0.182
Adjusted R^2	0.083	0.084	0.083	0.082	0.094	0.137	0.091	0.127

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

- Appendix -

A Employment history data

To construct the employment history database, I first establish the identity of the commissioners using a list of the insurance commissioners in office since 1980, with the start and end of their term.The list of commissioners was available in the 2017 Proceedings of the NAIC, and supplement via internet search to include changes that took place in 2018. I limit the list to commissioners between 2000 and 2018, which is 271 commissioners.

Next, I look for professional networks and record all listed jobs and education level. Third, if the profile is missing or sparse I try to supplement the data using an online search. Usual sources include press releases by insurance departments on appointments/departures of commissioners, Bloomberg executive profiles, press releases by firms for appointing a former commissioner, and journalistic articles.

Finally, I classify each job in one of six general categories: insurance industry, government, consulting or lobbying, law firm, related industry (e.g. finance or real estate), or other.

The resulting database offers at least some information for all commissioners: there is at least one job for each of the 271 commissioners. Further, I miss pre- commissioner-post-term jobs on 5 of the 271 commissioners, and post-term jobs for 12 of the 219 former commissioners (50 are still in office).[16] On average, I find 3.8 jobs for commissioners before they start office and 2.7 after they leave.

Table A.1: % of Commissioners with jobs in same state as their commissioner term

commissioner subset	pre-term (ever)	post-term (ever)	pre-term (immed)	post-term (immed)
any job	87.0	77.1	78.9	69.2
revolver job	51.6	52.4	55.6	41.1
revolver job: gov.rel	64.0	50.0	55.6	37.5

[16] It is easier to find data on pre-term employment since, first, insurance department press releases on commissioner appointment are very reliable source of supplement data, and second, the average age of assuming office is 50, which is after the mid-point of most peoples' careers.

B Number of exam regressions: specification robustness checks

B.1 Using total, instead of domestic financial exams

The baseline specification uses number of financial domestic exams as an outcome measure (not total). It was used since it was a more consistent measure across states of commissioner effort: it is possible that some departments lack the resources to examine foreign insurers. However, domiciled firms have to be regularly examined. Still, for consistency, we show that all results shown in the baseline specification in Table 2 are robust to using total exams. Results with total financial exams as dependent variable are shown in Table B.2.

Table B.2: Number of total exams by post-term revolver status

The table below summarizes results from regressing a measure of exams conducted in state s and year t on whether the commissioner in office is a post-term revolver:

$$Y_{s,t} = \alpha_s + \alpha_t + \beta I_{s,t}^{POST} + \gamma_x X_{s,t} + \epsilon_{s,t}$$

The dependent variable $Y_{s,t}$ is either absolute number of **total** financial exams in state s and year t (columns (1) through (4)), or the log-ed version of the variable (columns (5) through (8)).

$I_{s,t}^{POST,ever}$ is an indicator variable which is 1 if the commissioner in office in state s in year t will work for insurance industry at any point after being commissioner. This is the variable of interest in columns (1-2) and (5-6). $I_{s,t}^{POST,immed}$ is indicator variable which is 1 if the commissioner's job following leaving office is in insurance industry. This is the variable of interest in columns (3-4) and (7-8).

The control variables in $X_{s,t}$ include: (i) whether the commissioner worked for insurance industry at any point prior his commissioner term ($I_{s,t}^{PRE}$), (ii) the number of domestic firms in state s, year t (n Dom Frms), (iii) log of the budget that the state insurance department had in year s and state t, (iv) log of the number of financial analysts available to the insurance department in state s, year $t-1$. Regressions (2), (4), (6), (8) include these control variables.

Standard errors are clustered at the state level, and all regressions include state fixed effects and year fixed effects.

	Dependent variable:							
	n Dom Fin Exams$_{s,t}$				log(1 + n Dom Fin Exams$_{s,t}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$I_{s,t}^{POST,ever}$	-3.676^{**}	-2.890^{*}			-0.104^{**}	-0.080^{*}		
	(1.849)	(1.645)			(0.049)	(0.041)		
$I_{s,t}^{POST,immed}$			-5.952^{**}	-4.796^{**}			-0.121^{**}	-0.096^{*}
			(2.431)	(2.235)			(0.060)	(0.052)
$I_{s,t}^{PRE}$		0.154		0.519		0.054		0.084^{*}
		(1.514)		(1.737)		(0.046)		(0.050)
n Dom Firms$_{s,t}$		0.025		0.021				
		(0.021)		(0.020)				
log(n Dom Firms$_{s,t}$)						0.166^{***}		0.141^{**}
						(0.057)		(0.058)
log(budget$_{s,t}$)		4.863		4.213		0.115		0.105
		(4.789)		(4.758)		(0.091)		(0.090)
log(1+n examiners$_{s,t-1}$)		-4.553		-4.145		0.005		0.024
		(3.667)		(3.972)		(0.057)		(0.061)
E[LHS]	29.8	29.8	29.8	29.8	3	3	3	3
Empl.Hist.	full	immed	full	immed	full	immed	full	immed
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s	s	s
Observations	834	829	739	737	834	829	739	737
R^2	0.872	0.876	0.878	0.882	0.874	0.877	0.877	0.881
Adjusted R^2	0.860	0.865	0.866	0.869	0.863	0.866	0.865	0.868

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

B.2 Including commissioners with term shorter than a year

The baseline specification focuses on commissioners who have been in office at least a year. This was done to exclude interim commissioners who likely had little power to make significant changes. I show that the results of the baseline regression shown in Table 2 are robust to using all commissioners. Results are in Table B.3.

Table B.3: OLS: Number of domestic exams by post-term revolver status. Include commissioners with term shorter than a year.

The table below summarizes results from regressing a measure of domestic exams conducted in state s and year t on whether the commissioner in office is a post-term revolver, **but includes all commissioner terms, not only the ones longer than a year**.

$$Y_{s,t} = \alpha_s + \alpha_t + \beta I^{POST}_{s,t} + \gamma_x X_{s,t} + \epsilon_{s,t}$$

The dependent variable $Y_{s,t}$ is either absolute number of **domestic** financial exams in state s and year t (columns (1) through (4)), or the log-ed version of the variable (columns (5) through (8)).

$I^{POST,ever}_{s,t}$ is an indicator variable which is 1 if the commissioner in office in state s in year t will work for insurance industry at any point after being commissioner. This is the variable of interest in columns (1-2) and (5-6). $I^{POST,immed}_{s,t}$ is indicator variable which is 1 if the commissioner's job following leaving office is in insurance industry. This is the variable of interest in columns (3-4) and (7-8).

The control variables in $X_{s,t}$ include: (i) whether the commissioner worked for insurance industry at any point prior his commissioner term ($I^{PRE}_{s,t}$), (ii) the number of domestic firms in state s, year t (n Dom Frms), (iii) log of the budget that the state insurance department had in year s and state t, (iv) log of the number of financial analysts available to the insurance department in state s, year $t-1$. Regressions (2), (4), (6), (8) include these control variables. Standard errors are clustered at the state level, and all regressions include state fixed effects and year fixed effects.

	Dependent variable:							
	n Dom Fin Exams$_{s,t}$				log(1 + n Dom Fin Exams$_{s,t}$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$I^{POST,ever}_{s,t}$	−3.573**	−2.869*			−0.099**	−0.080**		
	(1.770)	(1.564)			(0.047)	(0.039)		
$I^{POST,immed}_{s,t}$			−5.686**	−4.590**			−0.115**	−0.096*
			(2.250)	(2.049)			(0.057)	(0.049)
$I^{PRE}_{s,t}$		0.428		0.846		0.066		0.097**
		(1.475)		(1.662)		(0.045)		(0.049)
n Dom Firms$_{s,t}$		0.026		0.022				
		(0.020)		(0.020)				
log(n Dom Firms$_{s,t}$)						0.171***		0.147***
						(0.056)		(0.056)
log(budget$_{s,t}$)		5.266		4.604		0.123		0.112
		(4.685)		(4.663)		(0.090)		(0.089)
log(1+n examiners$_{s,t-1}$)		−4.654		−4.230		0.007		0.024
		(3.514)		(3.830)		(0.055)		(0.059)
E[LHS]	29.9	29.9	29.9	29.9	3	3	3	3
Empl.Hist.	full	immed	full	immed	full	immed	full	immed
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s	s	s
Observations	908	901	806	803	908	901	806	803
R^2	0.872	0.877	0.878	0.882	0.877	0.881	0.881	0.885
Adjusted R^2	0.861	0.866	0.867	0.871	0.867	0.871	0.870	0.873

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

B.3 Using Ratios of domestic exams to number of domestic firms as a dependent variable

Table B.4: Summary Statistics (2000-2017)

Variable	n	mean	sd	min	q10	median	q90	max
Number of domestic exams to number of domestic firms:								
$\frac{\text{n Dom Fin Ex}_{s,t}}{\text{n Dom Firms}_{s,t-1}}$ %	996	21.8	11.8	0.0	8.4	20.0	36.7	85.7
$\log \frac{\text{1+n Dom Fin Ex}_{s,t}}{\text{n Dom Frms}_{s,t-1}}$	996	-1.6	0.6	-5.2	-2.4	-1.5	-0.9	-0.1
Number of domestic exams to number of domestic LA, PC, and Health firms:								
$\frac{\text{n Dom Fin Ex}_{s,t}}{\text{n Firms LA,PC,H}_{s,t-1}}$ %	996	42.5	68.6	0.0	16.0	29.2	63.5	726.3
$\log \frac{\text{1+n Dom Fin Ex}_{s,t}}{\text{n Firms LA,PC,H}_{s,t-1}}$	996	-1.1	0.6	-3.5	-1.7	-1.2	-0.4	2.0
Number of total exams to number of all licensed firms:								
$\frac{\text{n Fin Tot Ex}_{s,t}}{\text{n All Firms}_{s,t-1}}$ %	997	1.8	1.7	0.0	0.3	1.3	4.1	9.4
$\log \frac{\text{1+n Fin Tot Ex}_{s,t}}{\text{n All Firms}_{s,t-1}}$	997	-4.3	0.9	-7.3	-5.5	-4.3	-3.2	-2.4

Table B.5: Scaling number of domestic exams to number of all domestic firms

	Dependent variable:			
	$\frac{\text{n Dom Fin Ex}_{s,t}}{\text{n Dom Firms}_{s,t-1}}\%$		$\log\frac{1+\text{n Dom Fin Ex}_{s,t}}{\text{n Dom Frms}_{s,t-1}}$	
	(1)	(2)	(3)	(4)
$I_{s,t}^{POST,ever}$	−1.397		−0.018	
	p = 0.270		p = 0.759	
$I_{s,t}^{POST,immed}$		0.120		0.055
		p = 0.920		p = 0.473
E[LHS]	21.8	21.8	-1.6	-1.6
Empl.Hist.	full	immed	full	immed
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Cluster	s	s	s	s
Observations	834	739	834	739
R^2	0.435	0.380	0.507	0.461
Adjusted R^2	0.385	0.318	0.463	0.407
Note:		*p<0.1; **p<0.05; ***p<0.01		

Table B.6: Scaling number of domestic exams to number of large domestic insurance firms (Life/Annuity, health or Propert/Casualty), and scaling the number of total exams to number of all firms licensed to do business in the state

	Dependent variable:							
	$\frac{\text{n Dom Fin Ex}_{s,t}}{\text{n Firms LA,PC,H}_{s,t-1}}\%$		$\log\frac{\text{1+n Dom Fin Ex}_{s,t}}{\text{n Firms LA,PC,H}_{s,t-1}}$		$\frac{\text{n Fin Tot Ex}_{s,t}}{\text{n All Firms}_{s,t-1}}\%$		$\log\frac{\text{1+n Fin Tot Ex}_{s,t}}{\text{n All Firms}_{s,t-1}}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$I^{POST,ever}_{s,t}$	-11.372^{*}		-0.110		-0.274^{**}		-0.095^{*}	
	p = 0.082		p = 0.138		p = 0.035		p = 0.070	
$I^{POST,immed}_{s,t}$		-15.766^{*}		-0.146		-0.394^{**}		-0.114^{*}
		p = 0.058		p = 0.112		p = 0.018		p = 0.077
E[LHS]	42.5	42.5	-1.1	-1.1	1.8	1.8	-4.3	-4.3
Empl.Hist.	full	immed	full	immed	full	immed	full	immed
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s	s	s
Observations	834	739	834	739	834	739	834	739
R^2	0.690	0.695	0.587	0.591	0.855	0.862	0.846	0.853
Adjusted R^2	0.663	0.664	0.551	0.549	0.842	0.848	0.832	0.838

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

B.4 Results' robustness to excluding each state

In this appendix I show that the baseline results are not driven by any particular state. I rerun the baseline specification for both number of financial domestic exams and log number of financial exams, and I exclude states one at a time. The coefficient on post-term revolver and its corresponding t-value are plotted at Figure B.1 and Table B.7. The coefficient for absolute number of exams varies from -2.57 (t-value -2.28) to -3.74 (t-value -3.74). The coefficient for log number of exams varies from -0.07 (t-value 1.89) to -0.11 (t-value -3.21).

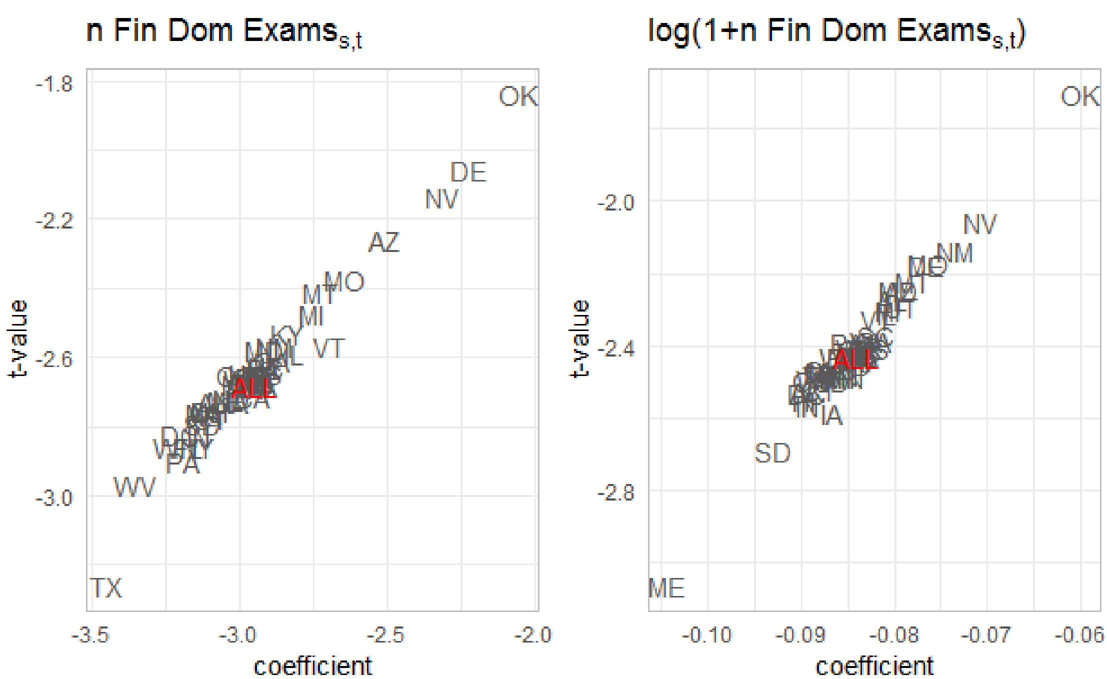

Figure B.1: Coefficients and t-values of baseline regressions on $I_{s,t}^{POST,ever}$

In the figure above I run the baseline regression and exclude state one at a time:

$$Y_{s,t} = \alpha_s + \alpha_t + \beta I_{s,t}^{POST,ever} + \gamma_x X_{s,t} + \epsilon_{s,t}$$

I plot β estimates on each state subset, and the t-value. $X_{s,t}$ includes whether the commissioners were employed in insurance industry pre-term, (log)number of domestic firms, log budget of state s and Year t and log number of examiners available to the insurance department of state s in year $t-1$. In the left plot I show the results for dependent variable number of domestic exams, and for the right plot - depended variable is log number of domestic exams.

Table B.7: Coefficients of regressing Y on post-term revolver, removing one state at a time

In the table below I run the baseline regression and exclude state one at a time:

$$Y_{s,t} = \alpha_s + \alpha_t + \beta I_{s,t}^{POST,ever} + \gamma_x X_{s,t} + \epsilon_{s,t}$$

Below I show β estimates on each state subset, and the t-value. $X_{s,t}$ includes whether the commissioners were employed in insurance industry pre-term, (log)number of domestic firms, log budget of state s and Year t and log number of examiners available to the insurance department of state s in year $t-1$. In the first two columns I show the results for dependent variable number of domestic exams, and for the last two columns - depended variable is log number of domestic exams.

	n fin Dom Exam$_{s,t}$		log(1+n fin Dom Exam$_{s,t}$)	
Excluded state	$I_{s,t}^{POST}$	t-value	$I_{s,t}^{POST}$	t-value
ALL	**-2.95**	**-2.68**	**-0.08**	**-2.43**
AK	-3.08	-2.73	-0.09	-2.53
AL	-3.12	-2.76	-0.08	-2.27
AR	-2.94	-2.65	-0.08	-2.40
AZ	-2.51	-2.26	-0.08	-2.25
CA	-2.96	-2.71	-0.08	-2.44
CO	-3.00	-2.65	-0.09	-2.50
CT	-3.10	-2.77	-0.09	-2.52
DC	-3.20	-2.82	-0.09	-2.53
DE	-2.22	-2.06	-0.08	-2.18
FL	-3.17	-2.86	-0.09	-2.43
GA	-2.89	-2.61	-0.08	-2.39
HI	-2.99	-2.65	-0.09	-2.55
IA	-3.00	-2.73	-0.09	-2.58
ID	-2.94	-2.65	-0.08	-2.45
IL	-2.81	-2.59	-0.08	-2.31
IN	-3.14	-2.83	-0.09	-2.56
KS	-2.92	-2.64	-0.08	-2.41
KY	-2.84	-2.53	-0.08	-2.42
LA	-2.94	-2.68	-0.08	-2.43
MA	-3.12	-2.76	-0.09	-2.47
MD	-2.92	-2.58	-0.08	-2.25
ME	-3.04	-2.72	-0.10	-3.06
MI	-2.76	-2.47	-0.08	-2.30
MN	-2.98	-2.69	-0.09	-2.49
MO	-2.64	-2.38	-0.08	-2.17
MS	-2.95	-2.67	-0.08	-2.43
MT	-2.73	-2.41	-0.08	-2.22
NC	-2.95	-2.67	-0.08	-2.42
ND	-3.03	-2.72	-0.09	-2.49
NE	-2.94	-2.67	-0.08	-2.43
NH	-3.00	-2.68	-0.09	-2.49
NJ	-3.11	-2.76	-0.09	-2.47
NM	-2.88	-2.57	-0.07	-2.14
NV	-2.31	-2.14	-0.07	-2.06
NY	-3.14	-2.86	-0.09	-2.47
OH	-3.10	-2.76	-0.09	-2.46
OK	-2.06	-1.84	-0.06	-1.70
OR	-2.96	-2.66	-0.08	-2.41
PA	-3.19	-2.90	-0.08	-2.39
RI	-2.95	-2.66	-0.08	-2.44
SC	-2.91	-2.63	-0.08	-2.37
SD	-3.12	-2.79	-0.09	-2.69
TN	-2.97	-2.68	-0.09	-2.48
TX	-3.45	-3.26	-0.09	-2.48
UT	-2.91	-2.63	-0.08	-2.29
VA	-2.92	-2.63	-0.08	-2.39
VT	-2.70	-2.57	-0.08	-2.33
WA	-2.95	-2.68	-0.08	-2.43
WI	-3.24	-2.86	-0.09	-2.49
WV	-3.35	-2.97	-0.09	-2.43
WY	-2.98	-2.66	-0.08	-2.41

C Exam-level robustness checks

C.1 Comparing state-year exam aggregated data to exam-level data

I compare the readily aggregated data from NAIC's IDRR used for number of financial exam reports and the combined micro data, aggregated at the state-year level. In Figure C.2 I plot the two numbers. I also regress the two numbers on each other - results are summarized in Table C.8. Both the figure and the regressions show that micro data is close to the aggregate data, but somewhat lower.

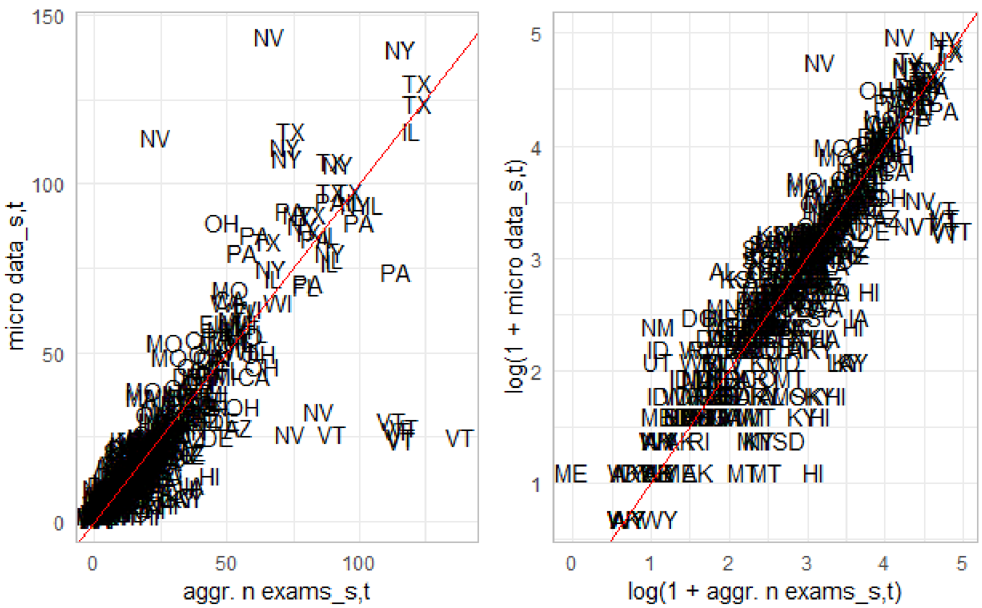

Figure C.2: Aggregate state-year number of exams vs state-year micro exams

Table C.8: Micro data regressed on the aggregate reported micro data through IDRR, 2000 to 2017

	Dependent variable:	
	$\text{nFinEx}^{micro}_{s,t}$	$\log(1 + \text{nFinEx}^{micro}_{s,t})$
	(1)	(2)
$\text{nFinEx}^{agg}_{s,t}$	0.741***	
	(0.022)	
$\log(1 + \text{nFinEx}^{agg}_{s,t})$		0.781***
		(0.025)
Constant	2.379**	0.393***
	(0.934)	(0.080)
Observations	817	817
R^2	0.579	0.540
Adjusted R^2	0.578	0.540
Note:		*p<0.1; **p<0.05; ***p<0.01

C.2 Robustness of results on early exam outcomes to definition of early exam

In the main text, I show that exams conducted by post-term revolvers result in fewer financial restatements for the firm, for both ever- and immediate post-term revolvers. I also show the result gets stronger for early exams, defined as no more than 3 years after the most recent exam. In Table C.9 I show this result is robust to defining yearly exams as an exam within 2 or 4 years since the most recent exam. The fewer the years since latest exams, the stronger the result gets, even when we account for the baseline likelihood increasing slightly. Specifically, the ever post-term revolver effect is 38% of the baseline examination rate among 2 years or earlier exams, and 12% of the baseline examination rate among 4 years or earlier exams. Note that the result is not statistically significant for immediate post-term revolver for exams 2 years and earlier, however the sign and direction of the coefficient are consistent with the rest of the results.

In Table C.10 I show the same regressions with dependent variable being whether the exam results in any recommendation. Results are weaker than the ones for financial restatements, however they are directionally consistent with them. They are stronger for immediate post-term revolvers and as the definition of early exam expands. Exams 2 years or earlier since most recent exam are not likely to result in any recommendations if they are led by post-term revolvers. Exams 3 or 4 years within the last exam are between 4% and 6.4% less likely to result in recommendations, which is between 6% and 9% of the average effect.

Table C.9: Exam outcomes: financial restatements. Robustness to definition of early exams

The regression estimates which factors lead to negative outcomes of the exams. Each observation is a unique exam-year-firm combination:

$$AnyFinancialRestatements_{i,t} = I^{POST,}_{i,s,t} + RiskVars_{i,s,t} + X_{i,s,t} + \epsilon_{i,s,t}$$

LHS indicator variable *Any Financial Restatements*$_{i,t}$ which is 1 if after the exam that took place in year t for firm i financial restatements were required.

In columns (1-3), $I^{POST}_{s,t} = 1$ when the commissioner is post-term revolver at any point after leaving office. In columns (4-7), $I^{POST}_{s,t} = 1$ when the commissioner is post-term revolver immediately after leaving office. Columns (1), (4), include only exams within 2 years of last exam. Columns (2), (5), include only exams within 3 years of last exam. Columns (3), (6), include only exams within 4 years of last exam. All regressions include state FE and year FE and standard errors are clustered at the state level

	Dependent variable:					
	Any Financial Restatements$_{i,t}$					
		$I^{POST,ever}_{s,t}$			$I^{POST,immed}_{s,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
$I^{POST}_{s,t}$	−0.185***	−0.099***	−0.044**	−0.187**	−0.103***	−0.045***
	(0.062)	(0.028)	(0.020)	(0.076)	(0.033)	(0.014)
regulatory ratio$_{L1}$ (std)	−0.025	−0.016	−0.035***	−0.023	−0.018	−0.033***
	(0.045)	(0.019)	(0.012)	(0.043)	(0.019)	(0.012)
ΔACL RBC (std)	−0.025***	−0.029***	−0.026***	−0.022***	−0.028***	−0.025***
	(0.005)	(0.007)	(0.008)	(0.006)	(0.007)	(0.008)
tot.Assets$_{L1}(std)$)	0.075	−0.021**	−0.017*	0.080	−0.021**	−0.016*
	(0.062)	(0.008)	(0.009)	(0.068)	(0.008)	(0.009)
Δtot.Assets (std)	−0.107	0.007	0.0003	−0.088	0.009	−0.0001
	(0.113)	(0.014)	(0.014)	(0.117)	(0.014)	(0.014)
leverage Ratio$_{L1}$ (std)	−0.020	0.012	0.015	−0.019	0.009	0.018
	(0.035)	(0.014)	(0.012)	(0.034)	(0.013)	(0.012)
Δlev. Ratio (std)	0.137	0.203**	0.020**	0.112	0.203**	0.020**
	(0.123)	(0.080)	(0.009)	(0.185)	(0.101)	(0.009)
op.Loss/Assets (std)	−0.026	−0.028*	−0.014	−0.033	−0.028*	−0.013
	(0.038)	(0.014)	(0.014)	(0.039)	(0.015)	(0.014)
$I^{PRE}_{s,t}$	0.025	0.016	0.037	0.082	0.048*	0.055**
	(0.061)	(0.030)	(0.024)	(0.082)	(0.028)	(0.025)
n yrs since last exam	−0.008	0.003	0.011	−0.006	0.003	0.011
	(0.066)	(0.013)	(0.013)	(0.064)	(0.014)	(0.014)
log(budget$_{s,t}$)	−0.143	−0.037	−0.016	−0.166	−0.049	−0.019
	(0.180)	(0.054)	(0.041)	(0.168)	(0.049)	(0.039)
log(1 + n examiners$_{s,t-1}$)	−0.134	−0.124**	−0.063	−0.083	−0.086**	−0.036
	(0.159)	(0.051)	(0.049)	(0.150)	(0.039)	(0.049)
E(LHS)	0.38	0.36	0.35	0.38	0.36	0.35
exams	≤ 2y	≤ 3y	≤ 4y	≤ 2y	≤ 3y	≤ 4y
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s
Observations	608	2,567	4,657	584	2,444	4,461
R^2	0.159	0.107	0.095	0.144	0.107	0.095
Adjusted R^2	0.051	0.081	0.081	0.031	0.080	0.080

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

Table C.10: Exam outcomes: any recommendations. Robustness to definition of early exams

The regression estimates which factors lead to negative outcomes of the exams. Each observation is a unique exam-year-firm combination:

$$AnyRecommendations_{i,t} = I^{POST,}_{i,s,t} + RiskVars_{i,s,t} + X_{i,s,t} + \epsilon_{i,s,t}$$

LHS variable is indicator variable *Any Recommendations*$_{i,s,t}$ if latest exam had recommendations the firm needed to comply with.

In columns (1-3), $I^{POST}_{s,t} = 1$ when the commissioner is post-term revolver at any point after leaving office. In columns (4-7), $I^{POST}_{s,t} = 1$ when the commissioner is post-term revolver immediately after leaving office. Columns (1), (4), include only exams within 2 years of last exam. Columns (2), (5), include only exams within 3 years of last exam. Columns (3), (6), include only exams within 4 years of last exam. All regressions include state FE and year FE and standard errors are clustered at the state level

	Dependent variable:					
	Any Recommendations$_{i,t}$					
	$I^{POST,ever}_{s,t}$			$I^{POST,immed}_{s,t}$		
	(1)	(2)	(3)	(4)	(5)	(6)
$I^{POST}_{s,t}$	0.024	−0.056	−0.047**	0.025	−0.071*	−0.066***
	(0.060)	(0.040)	(0.024)	(0.080)	(0.038)	(0.020)
regulatory ratio$_{L1}$ (std)	−0.064*	−0.030*	−0.060***	−0.058	−0.034**	−0.065***
	(0.035)	(0.016)	(0.011)	(0.036)	(0.015)	(0.011)
ΔACL RBC (std)	−0.001	−0.021	−0.025	−0.001	−0.021	−0.024
	(0.017)	(0.019)	(0.021)	(0.017)	(0.019)	(0.021)
tot.Assets$_{L1}(std)$)	0.041	−0.018**	−0.023**	0.055	−0.018**	−0.021**
	(0.038)	(0.008)	(0.009)	(0.038)	(0.007)	(0.008)
Δtot.Assets (std)	−0.009	−0.006	−0.002	−0.007	−0.006	−0.002
	(0.083)	(0.022)	(0.017)	(0.082)	(0.024)	(0.017)
leverage Ratio$_{L1}$ (std)	0.037	0.008	0.009	0.036	0.006	0.009
	(0.024)	(0.017)	(0.012)	(0.025)	(0.017)	(0.012)
Δlev. Ratio (std)	0.099	0.035	0.010***	0.101	0.017	0.011***
	(0.131)	(0.095)	(0.002)	(0.225)	(0.112)	(0.002)
op.Loss/Assets (std)	−0.050	−0.009	0.002	−0.047	−0.008	0.003
	(0.034)	(0.019)	(0.013)	(0.035)	(0.019)	(0.013)
$I^{PRE}_{s,t}$	0.040	−0.026	−0.029	0.066	0.009	−0.007
	(0.056)	(0.033)	(0.024)	(0.050)	(0.030)	(0.025)
n yrs since last exam	0.052	−0.012	0.007	0.051	−0.005	0.008
	(0.045)	(0.027)	(0.008)	(0.046)	(0.027)	(0.009)
log(budget$_{s,t}$)	−0.117	0.033	0.007	−0.106	0.037	0.017
	(0.187)	(0.054)	(0.047)	(0.181)	(0.055)	(0.042)
log(1 + n examiners$_{s,t-1}$)	0.045	−0.040	−0.049	−0.008	−0.013	−0.026
	(0.121)	(0.069)	(0.054)	(0.121)	(0.061)	(0.053)
E(LHS)	0.73	0.71	0.7	0.73	0.71	0.7
exams	≤ 2y	≤ 3y	≤ 4y	≤ 2y	≤ 3y	≤ 4y
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s
Observations	608	2,567	4,657	584	2,444	4,461
R^2	0.202	0.135	0.126	0.213	0.137	0.127
Adjusted R^2	0.100	0.110	0.112	0.109	0.112	0.113
Note:						*p<0.1; **p<0.05; ***p<0.01

Table C.11: Predicting early exams: robustness to definition of early exam and definition of post-term revolver

$$isExamYear_{i,s,t} = \beta I^{POST}_{s,t} + \beta_r RiskVars_{i,t} + \gamma \left(I^{POST}_{s,t} \times RiskVars_{i,t} \right) + \gamma_x X_{i,s,t} + \epsilon_{i,s,t}$$

This regression limits firm-year observations to n years after firm i's most recent examination, and tries to estimate which factor predict early exam. $isExamYear_{i,s,t}$ is an indicator variable, which equals 1 whenever firm i was examined in firm t. Post-term revolver is defined as follows: in columns (1-3): commissioner who works in insurance at any time after leaving office; in columns (4-6): commissioner who works in insurance immediately after term. Early exam is defined as follows: in columns (1) and (4): an exam conducted 2 years or less after firm's previous exam; in columns (2) and (5): an exam conducted 3 years or less after firm's previous exam; in columns (3) and (6): an exam conducted 4 years or less after firm's previous exam. All regressions include state FE and year FE. Standard errors are clustered at state level.

	Dependent variable:					
	$isExamYear_{i,s,t}$					
	$I^{POST,ever}_{s,t}$			$I^{POST,immed}_{s,t}$		
	(1)	(2)	(3)	(4)	(5)	(6)
$I^{POST}_{s,t}$	−0.019	−0.048***	−0.025	−0.043*	−0.056*	−0.051*
	(0.012)	(0.018)	(0.018)	(0.024)	(0.029)	(0.028)
ACL RBC$_{L1}$	−0.00001	−0.00004**	−0.0001***	−0.00000	−0.00003	−0.00005**
	(0.00003)	(0.00002)	(0.00002)	(0.00003)	(0.00002)	(0.00002)
$I^{POST}_{s,t}\times$ ACL RBC$_{L1}$	0.00000	0.0001***	0.0001***	−0.00002	−0.00000	0.00004
	(0.00002)	(0.00002)	(0.00002)	(0.00003)	(0.00003)	(0.00003)
ΔACL RBC (std)	0.005*	0.002	0.001	0.004*	0.003	0.002
	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)
$I^{POST}_{s,t}\times$ ΔACL RBC (std)	−0.006*	−0.003	−0.004	−0.006**	−0.005	−0.006
	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)
log(tot.Assets$_{L1}$)	−0.003***	−0.004**	−0.003	−0.004***	−0.005***	−0.004**
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
$I^{POST}_{s,t}\times$ log(tot.Assets$_{L1}$)	0.001	0.003	−0.0001	0.003**	0.006***	0.004*
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
Δtot.Assets (std)	0.001	0.003	0.003	0.001	0.003	0.003
	(0.005)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)
$I^{POST}_{s,t}\times$ Δtot.Assets (std)	−0.001	−0.003	−0.004	−0.001	−0.001	−0.005
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
leverage Ratio$_{L1}$	−0.0002	0.015	0.002	0.001	0.021	0.011
	(0.013)	(0.014)	(0.014)	(0.017)	(0.017)	(0.016)
$I^{POST}_{s,t}\times$ leverage Ratio$_{L1}$	0.012	0.004	0.018	0.007	−0.018	−0.008
	(0.012)	(0.016)	(0.016)	(0.017)	(0.020)	(0.019)
Δlev. Ratio(std)	−0.0002	0.001	0.0004	−0.0005	0.0001	0.0002
	(0.001)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)
$I^{POST}_{s,t}\times$ Δlev. Ratio(std)	−0.0005	−0.003	0.001	0.0002	−0.002	0.002
	(0.001)	(0.003)	(0.005)	(0.001)	(0.003)	(0.006)
op.Loss/tot.Assets	0.004***	0.004*	0.002	0.005***	0.005*	0.002
	(0.001)	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)
$I^{POST}_{s,t}\times$ op.Loss/tot.Assets	−0.004**	−0.006**	−0.002	−0.007**	−0.008**	−0.001
	(0.002)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)
n yrs since last exam	0.023***	0.087***	0.101***	0.020***	0.083***	0.099***
	(0.008)	(0.013)	(0.011)	(0.007)	(0.013)	(0.011)
$I^{PRE}_{s,t}$	0.001	0.017	0.019*	0.006	0.024*	0.024**
	(0.006)	(0.013)	(0.011)	(0.005)	(0.013)	(0.011)
log(budget$_{s,t}$)	−0.006	−0.026	−0.027	−0.006	−0.027	−0.026
	(0.011)	(0.023)	(0.024)	(0.010)	(0.024)	(0.025)
log(1 + n examiners$_{s,t-1}$)	0.017*	0.016	0.005	0.015	0.022*	0.011
	(0.010)	(0.012)	(0.011)	(0.010)	(0.013)	(0.014)
E(LHS)	0.03	0.08	0.12	0.03	0.08	0.12
exams	≤ 2y	≤ 3y	≤ 4y	≤ 2y	≤ 3y	≤ 4y
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s
Observations	21,779	30,500	36,519	20,444	28,749	34,568
R^2	0.036	0.116	0.157	0.035	0.110	0.153
Adjusted R^2	0.032	0.114	0.155	0.031	0.107	0.151

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

C.3 Robustness of results to limiting the sample to firms of similar size to future employers

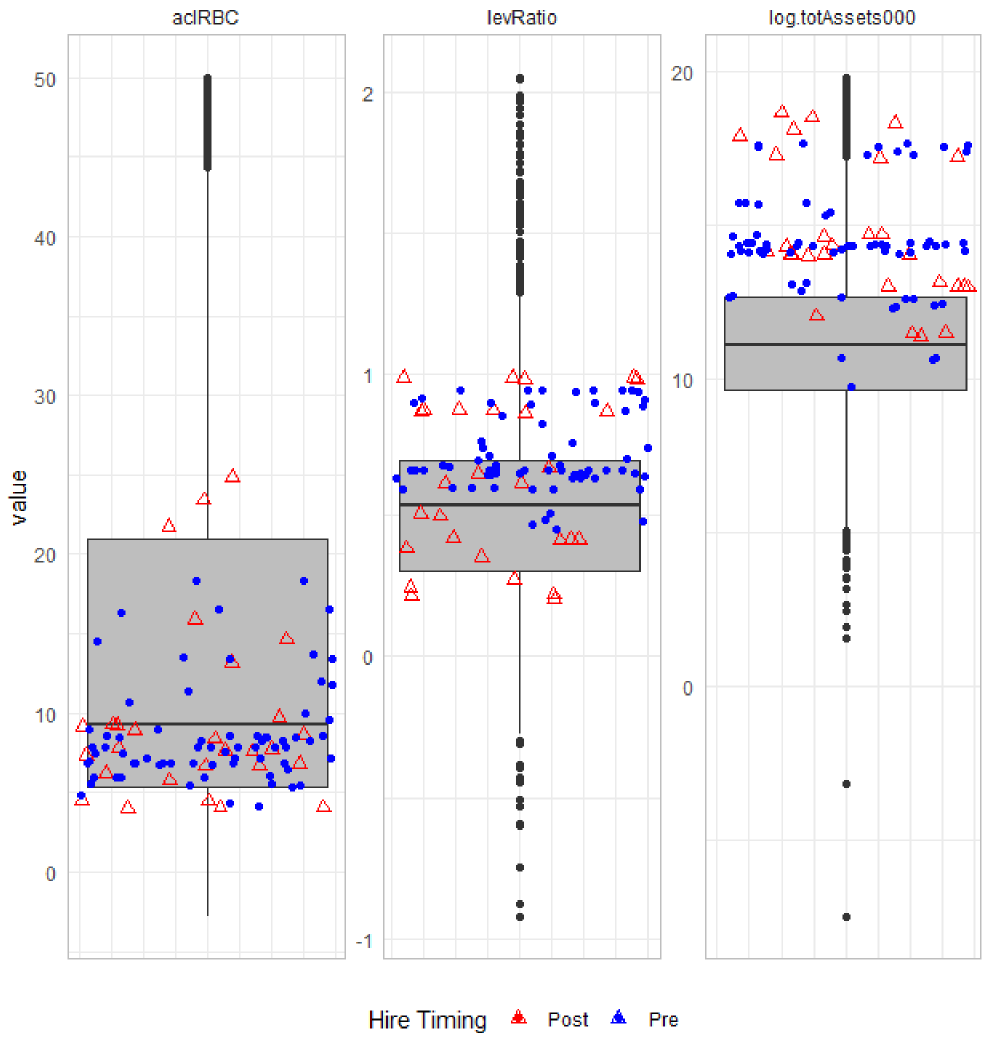

Figure C.3: Distribution of all insurance firms' risk variables and the risk variables for firms which employed commissioners

The plots show the distribution of the level of regulatory capital, leverage ratio, and log total assets in $000 for all firms. The red triangles show the post-term revolver firms' risk variables, and the blue dots - the pre-term employer risk variables.

Table C.12: Exam outcome results using a data subset, such that the size of firms by log assets is within the range of future employers

Results from the regrssions in Table 3 on a subset of firms whose log assets are between the smallest and largest possible log assets of a company employing a post-term revolver. The regression estimates which factors lead to negative outcomes of the exams. Each observation is a unique exam-year-firm combination:

$$Exam\ Outcome_{i,s,t} = I^{POST,}_{i,s,t} + RiskVars_{i,s,t} + X_{i,s,t} + \epsilon_{i,s,t}$$

LHS variable in columns (1-4) is indicator variable *Any Financial Restatements*$_{i,s,t}$ which is 1 if after the exam that took place in year t for firm i financial restatements were required. LHS variable in columns (5-8) is indicator variable *Any Recommendations*$_{i,s,t}$ if latest exam had recommendations the firm needed to comply with.

In columns (1), (2), (5), (6), $I^{POST}_{s,t} = 1$ when the commissioner is post-term revolver at any point after leaving office. In columns (3), (4), (7), (8), $I^{POST}_{s,t} = 1$ when the commissioner is post-term revolver immediately after leaving office. Columns (1), (3), (5), (7) include all exams. Columns (1), (3), (5), (7) include only exams within 3 years of last exam. All regressions include state FE and year FE and standard errors are clustered at the state level

	Dependent variable:							
	Any Financial Restatements$_{i,s,t}$				Any Recommendations$_{i,s,t}$			
	$I^{POST,ever}_{s,t}$		$I^{POST,immed}_{s,t}$		$I^{POST,ever}_{s,t}$		$I^{POST,immed}_{s,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$I^{POST}_{s,t}$	-0.053^{**}	-0.083^{**}	-0.052^{**}	-0.112^{***}	-0.039^{*}	-0.085^{**}	-0.039^{*}	-0.120^{***}
	(0.021)	(0.035)	(0.024)	(0.038)	(0.023)	(0.037)	(0.023)	(0.031)
regulatory ratio$_{L1}$ (std)	-0.113^{***}	-0.144^{***}	-0.114^{***}	-0.147^{***}	-0.097^{**}	-0.068^{**}	-0.097^{**}	-0.072^{**}
	(0.017)	(0.026)	(0.018)	(0.026)	(0.044)	(0.034)	(0.044)	(0.033)
ΔACL RBC (std)	−0.002	−0.014	−0.003	−0.015	−0.016	-0.066^{***}	−0.016	-0.066^{***}
	(0.016)	(0.014)	(0.016)	(0.014)	(0.015)	(0.019)	(0.015)	(0.019)
tot.Assets$_{L1}(std)$)	−0.002	−0.013	−0.003	−0.011	-0.022^{*}	-0.022^{**}	-0.022^{*}	-0.020^{*}
	(0.008)	(0.011)	(0.008)	(0.011)	(0.012)	(0.011)	(0.012)	(0.010)
Δtot.Assets (std)	0.004	0.003	0.004	0.001	−0.010	−0.012	−0.010	−0.012
	(0.007)	(0.012)	(0.007)	(0.011)	(0.010)	(0.016)	(0.010)	(0.016)
leverage Ratio$_{L1}$ (std)	0.018	0.044^{*}	0.019	0.047^{**}	−0.009	−0.006	−0.009	−0.004
	(0.016)	(0.023)	(0.016)	(0.024)	(0.017)	(0.022)	(0.017)	(0.021)
Δlev. Ratio (std)	-0.013^{***}	-0.011^{***}	-0.012^{***}	-0.010^{***}	0.007^{*}	0.009^{***}	0.007^{*}	0.010^{***}
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.002)
op.Loss/Assets (std)	−0.004	−0.010	−0.002	−0.008	0.019^{*}	0.019^{*}	0.019^{*}	0.019^{*}
	(0.012)	(0.019)	(0.012)	(0.019)	(0.011)	(0.011)	(0.011)	(0.011)
$I^{PRE}_{s,t}$	0.002	0.031	0.022	0.068^{*}	−0.027	−0.002	−0.027	0.036
	(0.026)	(0.034)	(0.027)	(0.038)	(0.025)	(0.028)	(0.025)	(0.035)
n yrs since last exam	0.005	0.034	0.005	0.038	0.008	0.006	0.008	0.010
	(0.009)	(0.026)	(0.009)	(0.027)	(0.009)	(0.018)	(0.009)	(0.020)
log(budget$_{s,t}$)	0.051	−0.003	0.044	−0.011	0.007	−0.022	0.007	−0.003
	(0.073)	(0.065)	(0.068)	(0.053)	(0.049)	(0.055)	(0.049)	(0.046)
log(1 + n examiners$_{s,t-1}$)	0.007	−0.051	0.038	0.010	−0.072	−0.083	−0.072	−0.026
	(0.080)	(0.070)	(0.074)	(0.063)	(0.067)	(0.078)	(0.067)	(0.082)
E(LHS)	0.34	0.35	0.34	0.35	0.65	0.7	0.66	0.7
exams	all	≤ 4y	all	≤ 4y	all	≤ 4y	all	≤ 4y
firms	comp	comp	comp	comp	comp	comp	comp	comp
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s	s	s
Observations	3,971	2,237	3,865	2,152	3,971	2,237	3,971	2,152
R^2	0.095	0.132	0.095	0.137	0.122	0.143	0.122	0.143
Adjusted R^2	0.079	0.104	0.079	0.109	0.106	0.115	0.106	0.115

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

Table C.13: Predicting early exams using a data subset, such that the size of firms by log assets is within the range of future employers

Results from the regerssions in Table C.11 on a subset of firms whose log assets are between the smallest and largest possible log assets of a company employing a post-term revolver.

$$isExamYear_{i,s,t} = \beta I^{POST}_{s,t} + \beta_r RiskVars_{i,t} + \gamma\left(I^{POST}_{s,t} \times RiskVars_{i,t}\right) + \gamma_x X_{i,s,t} + \epsilon_{i,s,t}$$

Post-term revolver is defined as follows: in columns (1-3): commissioner who works in insurance at any time after leaving office; in columns (4-6): commissioner who works in insurance immediately after term. Early exam is defined as follows: in columns (1) and (4): an exam 2 years or less since last exam; in columns (2) and (5): an exam 3 years or less since last exam; in columns (3) and (6): an exam 4 years or less since last exam; All regressions include state FE and year FE. Standard errors are clustered at state level.

	Dependent variable:					
	$isExamYear_{i,s,t}$					
	$I^{POST,ever}_{s,t}$			$I^{POST,immed}_{s,t}$		
	(1)	(2)	(3)	(4)	(5)	(6)
$I^{POST}_{s,t}$	-0.027^{*}	-0.025	-0.051	-0.055^{**}	-0.018	-0.054
	(0.015)	(0.035)	(0.045)	(0.023)	(0.041)	(0.047)
ACL RBC_{L1}	0.00001	-0.0001	-0.0001	-0.00001	-0.0001	-0.0001^{*}
	(0.00003)	(0.0001)	(0.0001)	(0.00003)	(0.0001)	(0.00004)
$I^{POST}_{s,t} \times$ ACL RBC_{L1}	-0.00002	0.00001	0.00003	0.00003	-0.00000	0.00003
	(0.00004)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
ΔACL RBC (std)	0.006	0.007	0.006	0.005	0.004	0.004
	(0.007)	(0.007)	(0.008)	(0.006)	(0.006)	(0.007)
$I^{POST}_{s,t} \times \Delta$ACL RBC (std)	-0.008	0.0004	0.002	-0.007	0.008	0.009
	(0.007)	(0.008)	(0.008)	(0.006)	(0.009)	(0.009)
log(tot.$Assets_{L1}$)	-0.003^{***}	-0.005^{***}	-0.008^{***}	-0.003^{***}	-0.005^{***}	-0.008^{***}
	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)
$I^{POST}_{s,t} \times$ log(tot.$Assets_{L1}$)	0.002	0.002	0.003	0.004^{**}	0.005	0.006^{*}
	(0.001)	(0.003)	(0.004)	(0.002)	(0.003)	(0.004)
Δtot.Assets (std)	0.003	0.004	0.004	0.001	0.003	0.002
	(0.005)	(0.005)	(0.005)	(0.002)	(0.003)	(0.003)
$I^{POST}_{s,t} \times \Delta$tot.Assets (std)	-0.004	-0.004	-0.006	0.0002	-0.002	-0.003
	(0.006)	(0.006)	(0.005)	(0.003)	(0.004)	(0.005)
leverage $Ratio_{L1}$	0.015	0.015	0.002	0.013	0.025	0.011
	(0.010)	(0.018)	(0.019)	(0.009)	(0.023)	(0.022)
$I^{POST}_{s,t} \times$ leverage $Ratio_{L1}$	-0.002	-0.020	-0.008	-0.003	-0.051	-0.034
	(0.015)	(0.023)	(0.024)	(0.016)	(0.034)	(0.032)
Δlev. Ratio(std)	0.004	0.017	0.020	0.027	0.037^{*}	0.030^{***}
	(0.006)	(0.016)	(0.013)	(0.030)	(0.020)	(0.010)
$I^{POST}_{s,t} \times \Delta$lev. Ratio(std)	-0.004	-0.016	-0.018	-0.028	-0.036^{*}	-0.029^{***}
	(0.005)	(0.017)	(0.013)	(0.030)	(0.021)	(0.009)
op.Loss/tot.Assets	-0.002	0.002	-0.001	0.001	0.005	-0.0003
	(0.004)	(0.004)	(0.005)	(0.002)	(0.005)	(0.005)
$I^{POST}_{s,t} \times$ op.Loss/tot.Assets	0.004	-0.004	0.001	-0.002	-0.009	-0.0003
	(0.006)	(0.005)	(0.007)	(0.003)	(0.006)	(0.008)
n yrs since last exam	0.027^{***}	0.091^{***}	0.109^{***}	0.024^{***}	0.088^{***}	0.106^{***}
	(0.006)	(0.015)	(0.013)	(0.006)	(0.015)	(0.013)
$I^{PRE}_{s,t}$	0.006	0.026	0.028^{**}	0.010^{*}	0.031^{**}	0.031^{**}
	(0.006)	(0.017)	(0.014)	(0.005)	(0.015)	(0.013)
log($budget_{s,t}$)	-0.008	-0.023	-0.022	-0.009	-0.026	-0.023
	(0.007)	(0.028)	(0.029)	(0.007)	(0.029)	(0.030)
log(1 + n $examiners_{s,t-1}$)	0.027^{**}	0.042^{*}	0.028	0.026^{**}	0.047^{**}	0.034
	(0.011)	(0.021)	(0.021)	(0.010)	(0.022)	(0.022)
E(LHS)	0.02	0.07	0.12	0.02	0.07	0.12
exams	$\leq$ 2y	$\leq$ 3y	$\leq$ 4y	$\leq$ 2y	$\leq$ 3y	$\leq$ 4y
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s
Observations	10,048	14,271	17,270	9,477	13,525	16,435
R^2	0.031	0.136	0.185	0.030	0.133	0.182
Adjusted R^2	0.022	0.130	0.181	0.021	0.128	0.178

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

C.4 Examinations of future employers of post-term revolvers

I find that post-term revolvers are more likely to have examined their future employer, to have done so early, and to have given them exam with negative consequences. I run(3) for all exams and for early exams - results are summarized in Table C.15. The loss of significance for early exam may be driven that these regressions are estimated using very few data points. Among all exams $I_{i,s,t}^{HIRE,POST}$ is 1 for only 28 points, and among early exams - only 18. This is also the reason why state-year fixed effects cannot be included.

Table C.14: Summary statistics on control variables between all firms that did and didn't hire commissioners

	n			mean			sd		
Variable	non-hire	hire post	hire pre	non-hire	hire post	hire pre	non-hire	hire post	hire pre
Yearly Risk Variable									
ACL RBC$_{L1}$	57202	28	70	51.85	9.51	9.70	135.37	5.76	11.28
ΔACL RBC (std)	56493	28	70	-0.01	-0.09	-0.08	0.91	0.04	0.08
log(tot.Assets$_{L1}$)	60240	28	70	11.20	14.61	14.36	2.30	2.27	1.75
Δtot.Assets (std)	59737	28	70	-0.04	-0.02	-0.09	0.81	0.34	0.19
leverage Ratio$_{L1}$	60240	28	70	0.50	0.62	0.70	0.27	0.28	0.14
Δlev. Ratio	59498	28	70	-0.02	-0.06	-0.06	0.80	0.00	0.01
op.Loss/tot.Assets	58737	28	69	-0.03	-0.06	0.01	0.81	0.38	0.46
Most Recent Exam Outcome									
any Recommendations$_{i,s,t}$	53827	24	57	0.67	0.67	0.67	0.47	0.48	0.48
any Fin. Restatements$_{i,s,t}$	53827	24	57	0.35	0.38	0.19	0.48	0.49	0.40
n yrs since last exam$_{i,s,t}$	76143	34	70	1.67	1.53	1.76	1.59	1.50	1.30
Post-term revolver indicators									
$I_{s,t}^{POST,ever}$	62382	34	65	0.42	1.00	0.43	0.49	0.00	0.50
$I_{s,t}^{POST,immed}$	58563	34	65	0.29	0.76	0.37	0.45	0.43	0.49
$I_{s,t}^{POST,ind.immed}$	57095	30	65	0.06	0.17	0.03	0.24	0.38	0.17

Table C.15: Predicting early exams: post-term revolver only if hired by firm

$$isExamYear_{i,s,t} = \beta I^{HIRE,POST}_{s,t} + \beta_r RiskVars_{i,t} + \gamma_r \left(I^{HIRE,POST}_{s,t} \times RiskVars_{i,t}\right) + \gamma_x X_{i,s,t} + \epsilon_{i,s,t}$$

Measuring the likelihood of firm (early) examination as a function of whether the examining commissioner ended up working in the firm after term's end. $I^{HIRE,POST}_{i,s,t}$ is indicator which equals 1 when the commissioner in office in year s and year t ends up being hired by firm i after their term ends. Control variables include (i) $I^{HIRE,PRE}_{i,s,t}$: indicator which is 1 when the examining commissioner was previously hired by firm is, (ii) number of years since last exam, (iii) log of the insurance department budget from state s and year t and (iv) log of the number of financial analysts available to the insurance department in state s, year $t-1$. Columns (1), (2), (3) limit the sample to years no more than correspondingly 2, 3, and 4 years since last exam. Column (4) includes all exams.All regressions include state FE and year FE and are clustered at the state level.

	Dependent variable:			
	$isExamYear_{i,s,t}$			
	(1)	(2)	(3)	(4)
$I^{HIRE,POST}_{i,s,t}$	3.877	3.603	4.158**	4.351***
	(2.607)	(2.362)	(1.622)	(1.500)
ACL RBC_{L1} (std)	−0.0005	−0.004*	−0.004*	−0.003
	(0.002)	(0.002)	(0.002)	(0.003)
$I^{HIRE,POST}_{i,s,t}$ × ACL RBC_{L1} (std)	−0.132	0.353	−0.899	−0.605
	(2.526)	(2.163)	(1.218)	(0.816)
ΔACL RBC (std)	0.004*	0.003**	0.001	−0.001
	(0.002)	(0.002)	(0.002)	(0.002)
$I^{HIRE,POST}_{i,s,t}$ × ΔACL RBC (std)	1.062	1.395	1.508	1.360*
	(2.024)	(1.212)	(1.206)	(0.751)
tot.$Assets_{L1}(std)$)	−0.002***	−0.004***	−0.006***	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
$I^{HIRE,POST}_{i,s,t}$ × tot.$Assets_{L1}(std)$)	−0.030	0.0003	0.027	0.010
	(0.037)	(0.027)	(0.025)	(0.016)
Δtot.Assets (std)	0.004	0.004*	0.002	0.003
	(0.003)	(0.002)	(0.002)	(0.002)
$I^{HIRE,POST}_{i,s,t}$ × Δtot.Assets (std)	−0.705	−0.245	−0.561***	−0.532**
	(0.621)	(0.385)	(0.178)	(0.210)
leverage $Ratio_{L1}$ (std)	−0.0003	0.004	0.004	0.002
	(0.003)	(0.004)	(0.004)	(0.004)
$I^{HIRE,POST}_{i,s,t}$ × leverage $Ratio_{L1}$ (std)	0.010	−0.168	−0.295*	−0.217**
	(0.278)	(0.199)	(0.175)	(0.110)
Δlev. Ratio (std)	−0.001	−0.0004	0.001	0.002
	(0.001)	(0.001)	(0.002)	(0.002)
$I^{HIRE,POST}_{i,s,t}$ × Δlev. Ratio (std)	61.007	54.934	71.519**	73.568***
	(54.255)	(48.832)	(29.060)	(25.839)
op.Loss/Assets (std)	0.003	0.003	0.002	−0.002
	(0.002)	(0.002)	(0.002)	(0.002)
$I^{HIRE,POST}_{i,s,t}$ × op.Loss/Assets (std)	0.068	−0.209	−0.007	−0.061
	(0.319)	(0.270)	(0.105)	(0.188)
$I^{HIRE,PRE}_{s,t}$	0.002	0.018	0.019**	0.013
	(0.004)	(0.011)	(0.009)	(0.009)
n yrs since last exam	0.031***	0.091***	0.105***	0.111***
	(0.005)	(0.013)	(0.010)	(0.011)
$\log(budget_{s,t})$	−0.007	−0.034	−0.034	−0.020
	(0.009)	(0.028)	(0.028)	(0.027)
$\log(1 + n\ examiners_{s,t-1})$	0.003	0.006	−0.002	−0.026
	(0.010)	(0.015)	(0.015)	(0.023)
exams	≤ 2y	≤ 3y	≤ 4y	all
E(LHS)	0.03	0.08	0.12	0.28
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Cluster	s	s	s	s
Observations	31,826	44,958	54,037	61,279
R^2	0.032	0.119	0.162	0.223
Adjusted R^2	0.029	0.117	0.161	0.222
Note:				*p<0.1; **p<0.05; ***p<0.01

Table C.16: Exam outcomes by whether commissioner was hired by firm after end of her term

The regression estimates which factors lead to negative outcomes of the exams. Each observation is a unique exam-year-firm combination:

$$Exam\ Outcome_{i,s,t} = I^{POST,}_{i,s,t} + RiskVars_{i,s,t} + X_{i,s,t} + \epsilon_{i,s,t}$$

LHS variable in columns (1-4) is indicator variable *Any Financial Restatements*$_{i,s,t}$ which is 1 if after the exam that took place in year t for firm i financial restatements were required. LHS variable in columns (5-8) is indicator variable *Any Recommendations*$_{i,s,t}$ if latest exam had recommendations the firm needed to comply with.

$I^{HIRE,POST}_{i,s,t} = 1$ when the commissioner works for firm i after the end of their term.. Columns (1) and (5)include all exams. Columns (2) and (6) include only exams within 2 years of last exam. Columns (3) and (7) include only exams within 3 years of last exam. Columns (4) and (8) include only exams within 4 years of last exam. All regressions include state FE and year FE and standard errors are clustered at the state level

	Dependent variable:							
	Exam Outcome$_{i,s,t}$							
	Any Financial Restatements$_{i,s,t}$				Any Recommendations$_{i,s,t}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$I^{HIRE,POST}_{i,s,t}$	−0.220	−0.375***	−0.223***	−0.557***	0.146	0.211***	0.257***	−0.082
	(0.152)	(0.039)	(0.080)	(0.136)	(0.123)	(0.047)	(0.039)	(0.101)
regulatory ratio$_{L1}$ (std)	−0.040***	−0.033***	−0.015	−0.040	−0.056***	−0.062***	−0.031**	−0.072**
	(0.007)	(0.010)	(0.017)	(0.039)	(0.008)	(0.010)	(0.014)	(0.033)
ΔACL RBC (std)	−0.009	−0.021***	−0.022***	−0.021***	−0.013	−0.020	−0.018	−0.003
	(0.006)	(0.007)	(0.007)	(0.003)	(0.009)	(0.018)	(0.017)	(0.017)
tot.Assets$_{L1}(std)$)	−0.013**	−0.021**	−0.017	0.064	−0.024***	−0.027***	−0.019***	0.039
	(0.005)	(0.010)	(0.011)	(0.077)	(0.008)	(0.007)	(0.007)	(0.046)
Δtot.Assets (std)	0.008	0.013	0.006	−0.003	0.008	0.010	0.005	0.011
	(0.008)	(0.010)	(0.007)	(0.014)	(0.008)	(0.014)	(0.012)	(0.012)
leverage Ratio$_{L1}$ (std)	0.005	0.022**	0.026*	−0.024	0.009	0.011	0.013	0.036*
	(0.008)	(0.010)	(0.014)	(0.029)	(0.009)	(0.010)	(0.013)	(0.022)
Δlev. Ratio (std)	0.011*	0.013	0.202**	0.114	0.003	0.003	0.050	0.120
	(0.007)	(0.010)	(0.088)	(0.111)	(0.004)	(0.006)	(0.085)	(0.091)
op.Loss/Assets (std)	−0.022**	−0.024**	−0.026*	−0.051***	0.001	0.0003	−0.007	−0.033
	(0.008)	(0.011)	(0.014)	(0.019)	(0.009)	(0.011)	(0.015)	(0.027)
$I^{HIRE,PRE}_{i,s,t}$	0.011	0.012	−0.016	0.010	−0.019	−0.027	−0.045**	0.023
	(0.013)	(0.019)	(0.027)	(0.063)	(0.015)	(0.018)	(0.021)	(0.043)
n yrs since last exam	0.001	0.004	0.011	0.006	0.006	−0.001	−0.002	0.048
	(0.005)	(0.013)	(0.012)	(0.057)	(0.005)	(0.010)	(0.023)	(0.046)
log(budget$_{s,t}$)	0.010	−0.019	−0.071	−0.186	−0.009	−0.017	−0.013	−0.138
	(0.032)	(0.036)	(0.047)	(0.156)	(0.052)	(0.048)	(0.052)	(0.150)
log(1 + n examiners$_{s,t-1}$)	−0.043	−0.072*	−0.099**	−0.150	−0.043	−0.059	−0.029	0.056
	(0.037)	(0.040)	(0.040)	(0.097)	(0.043)	(0.048)	(0.062)	(0.125)
E(LHS)	0.33	0.35	0.36	0.38	0.69	0.7	0.71	0.73
exams	all	≤ 4y	≤ 3y	≤ 2y	all	≤ 4y	≤ 3y	≤ 2y
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s	s	s
Observations	10,748	5,841	3,124	711	10,748	5,841	3,124	711
R^2	0.081	0.091	0.101	0.148	0.124	0.131	0.133	0.212
Adjusted R^2	0.075	0.079	0.079	0.052	0.118	0.119	0.112	0.124

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

C.5 Difference in exam outcomes in the last two years of term

Table C.17: Are financial restatements more likely in last two years of commissioners' terms?

	Dependent variable:							
	Any Financial Restatements $_{i,s,t}$							
	$I_{s,t}^{POST,ever}$				$I_{s,t}^{POST,immed}$			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$I_{s,t}^{POST}$	-0.024	-0.063^{*}	-0.088^{**}	-0.140^{*}	-0.004	-0.073^{**}	-0.070^{*}	-0.097^{*}
	(0.020)	(0.033)	(0.044)	(0.072)	(0.020)	(0.031)	(0.039)	(0.055)
$I_{s,t}^{POST} \times I_{s,t}^{T}$	-0.015	0.044	-0.017	0.127	-0.038	0.058	-0.048	0.074
	(0.033)	(0.049)	(0.046)	(0.092)	(0.041)	(0.051)	(0.063)	(0.096)
$I_{s,t}^{POST} \times I_{s,t}^{T-1}$	0.015	0.026	-0.005	0.048	-0.003	0.065	-0.040	0.047
	(0.041)	(0.059)	(0.079)	(0.104)	(0.044)	(0.069)	(0.054)	(0.086)
$I_{s,t}^{T}$	0.027	-0.013	0.047	-0.027	0.020	-0.028	0.020	-0.026
	(0.027)	(0.040)	(0.039)	(0.083)	(0.026)	(0.034)	(0.034)	(0.070)
$I_{s,t}^{T-1}$	-0.004	0.007	-0.003	-0.019	0.003	-0.005	-0.001	-0.016
	(0.029)	(0.048)	(0.045)	(0.073)	(0.029)	(0.046)	(0.042)	(0.057)
ACL RBC$_{L1}$	-0.0003^{***}	-0.001^{***}	-0.0002	-0.001^{***}	-0.0003^{***}	-0.001^{***}	-0.0002	-0.001^{***}
	(0.00005)	(0.0001)	(0.0001)	(0.0003)	(0.00005)	(0.0001)	(0.0001)	(0.0003)
ΔACL RBC (std)	-0.015^{*}	-0.003	-0.026^{***}	-0.001	-0.015^{*}	-0.004	-0.025^{***}	-0.001
	(0.008)	(0.015)	(0.007)	(0.015)	(0.008)	(0.015)	(0.007)	(0.016)
log(tot.Assets$_{L1}$)	-0.030^{***}	-0.023^{***}	-0.037^{***}	-0.047^{***}	-0.031^{***}	-0.024^{***}	-0.039^{***}	-0.048^{***}
	(0.005)	(0.008)	(0.008)	(0.013)	(0.005)	(0.008)	(0.008)	(0.014)
Δtot.Assets (std)	0.001	0.002	0.005	0.001	-0.00004	0.001	0.005	0.0001
	(0.010)	(0.008)	(0.016)	(0.024)	(0.010)	(0.008)	(0.016)	(0.024)
leverage Ratio$_{L1}$	0.125^{***}	0.108^{**}	0.171^{***}	0.214^{**}	0.129^{***}	0.113^{**}	0.174^{***}	0.195^{**}
	(0.038)	(0.054)	(0.057)	(0.090)	(0.038)	(0.053)	(0.055)	(0.090)
Δlev. Ratio(std)	0.019^{***}	-0.013^{***}	0.205^{***}	4.730^{***}	0.019^{***}	-0.012^{***}	0.207^{**}	4.816^{***}
	(0.007)	(0.003)	(0.079)	(1.786)	(0.007)	(0.003)	(0.100)	(1.760)
op.Loss/tot.Assets	-0.010	-0.003	-0.024	-0.041	-0.008	-0.002	-0.024	-0.040
	(0.010)	(0.012)	(0.020)	(0.027)	(0.010)	(0.012)	(0.021)	(0.029)
n yrs since last exam	0.0002	0.001	0.010	0.052^{**}	0.0001	0.0004	0.011	0.055^{**}
	(0.006)	(0.009)	(0.016)	(0.026)	(0.006)	(0.010)	(0.017)	(0.025)
$I_{s,t}^{PRE}$	0.010	-0.003	0.020	0.009	0.019	0.016	0.050^{**}	0.043
	(0.017)	(0.025)	(0.028)	(0.038)	(0.018)	(0.025)	(0.025)	(0.040)
log(budget$_{s,t}$)	-0.007	0.022	-0.076	-0.043	-0.010	0.018	-0.074	-0.051
	(0.035)	(0.065)	(0.048)	(0.064)	(0.035)	(0.058)	(0.048)	(0.069)
log(1+n examiners$_{s,t}$)	-0.045	-0.035	-0.122^{**}	-0.138	-0.028	-0.004	-0.085^{*}	-0.089
	(0.037)	(0.058)	(0.058)	(0.096)	(0.036)	(0.055)	(0.049)	(0.078)
E(LHS)	0.34	0.3	0.36	0.3	0.34	0.3	0.36	0.3
exams	all	all	≤ 3y	≤ 3y	all	all	≤ 3y	≤ 3y
firms	all	comp	all	comp	all	comp	all	comp
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s	s	s
Observations	6,932	3,375	2,070	926	6,675	3,269	1,947	877
R^2	0.091	0.101	0.122	0.197	0.089	0.102	0.123	0.202
Adjusted R^2	0.080	0.081	0.088	0.130	0.079	0.082	0.088	0.134

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

D Actions against insurers: specification robustness checks

Table D.18: Log Regulatory actions taken against company based on solvency concern by post-term revolver status

The table below summarizes results from regressing a measure of regulatory actions based on solvency concerns in state s and year t on whether the commissioner in office is a post-term revolver:

$$Y_{s,t} = \alpha_s + \alpha_t + \beta I^{POST}_{s,t} + \gamma_x X_{s,t} + \epsilon_{s,t}$$

The dependent variable $Y_{s,t}$ is **log of**: number of certificates suspended (columns (1-2)), number of certificates permanently revoked (columns (3-4)), and number of delinquency orders (columns (5-6)) in state s in year t.

$I^{POST}_{s,t}$ is an indicator variable which is 1 if the commissioner in office in state s in year t will work for insurance industry at any point after being commissioner.

The control variables in $X_{s,t}$ include: whether the commissioner worked for insurance industry at any point prior his commissioner term ($I^{PRE}_{s,t}$) and number of financial exams in year s and state t. Regressions (2), (4) and (6) include these control variables.

Standard errors are clustered at the state level, and all regressions include state fixed effects and year fixed effects.

	Dependent variable:					
	log(1+n cert-s suspended$_{s,t}$)		log(1+n cert-s revoked$_{s,t}$)		log(1+n delinquency orders$_{s,t}$)	
	(1)	(2)	(3)	(4)	(5)	(6)
$I^{POST}_{s,t}$	−0.175*	−0.130	−0.040	−0.034	−0.123**	−0.116**
	p = 0.053	p = 0.137	p = 0.608	p = 0.680	p = 0.048	p = 0.049
$I^{PRE}_{s,t}$		0.060		0.045		−0.004
		p = 0.502		p = 0.518		p = 0.959
n Dom Fin Exams$_{s,t}$		0.008***		0.002		0.002
		p = 0.004		p = 0.245		p = 0.320
E[LHS]	1	1	0.6	0.6	0.2	0.2
Empl.Hist.	full	full	full	full	full	full
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s
Observations	830	825	830	825	682	682
R^2	0.650	0.660	0.529	0.528	0.408	0.410
Adjusted R^2	0.619	0.629	0.487	0.485	0.347	0.347

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

Table D.19: Regulatory actions taken against company based on solvency concern by immediate post-term revolver status

The table below summarizes results from regressing a measure of regulatory actions based on solvency concerns in state s and year t on whether the commissioner in office is an immediate post-term revolver:

$$Y_{s,t} = \alpha_s + \alpha_t + \beta I_{s,t}^{POST,immed} + X_{s,t} + \epsilon_{s,t}$$

The dependent variable $Y_{s,t}$ is number of certificates suspended (columns (1-2)), number of certificates permanently revoked (columns (3-4)), and number of delinquency orders (columns (5-6)) in state s in year t.

$I_{s,t}^{POST}$ is an indicator variable which is 1 if the commissioner in office in state s in year t will work for insurance industry **immediately** after being commissioner.

The control variables in $X_{s,t}$ include whether the commissioner worked for insurance industry at any point prior his commissioner term ($I_{s,t}^{PRE}$) and number of financial exams in year s and state t. Regressions (2), (4) and (6) include these control variables.

Standard errors are clustered at the state level, and all regressions include state fixed effects and year fixed effects.

	Dependent variable:					
	n certificates suspended$_{s,t}$		n certificates revoked$_{s,t}$		n delinquency orders$_{s,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
$I_{s,t}^{POST,immed}$	−0.699	−0.491	−0.405*	−0.334	−0.205*	−0.146
	p = 0.266	p = 0.403	p = 0.085	p = 0.148	p = 0.089	p = 0.196
$I_{s,t}^{PRE}$		−0.173		0.157		−0.007
		p = 0.798		p = 0.610		p = 0.983
n Dom Fin Exams$_{s,t}$		0.035**		0.010		0.015
		p = 0.036		p = 0.318		p = 0.245
E[LHS]	3.5	3.5	1.9	1.9	0.7	0.7
Empl.Hist.	immed	immed	immed	immed	immed	immed
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s
Observations	738	736	738	736	614	614
R^2	0.580	0.585	0.374	0.375	0.438	0.444
Adjusted R^2	0.537	0.541	0.311	0.309	0.373	0.378
Note:						*p<0.1; **p<0.05; ***p<0.01

Table D.20: Log Regulatory actions taken against company based on solvency concern by immediate post-term revolver status

The table below summarizes results from regressing a measure of regulatory actions based on solvency concerns in state s and year t on whether the commissioner in office is an immediate post-term revolver:

$$Y_{s,t} = \alpha_s + \alpha_t + \beta I_{s,t}^{POST,immed} + X_{s,t} + \epsilon_{s,t}$$

The dependent variable $Y_{s,t}$ is the **log** number of: certificates suspended (columns (1-2)), number of certificates permanently revoked (columns (3-4)), and number of delinquency orders (columns (5-6)) in state s in year t.

$I_{s,t}^{POST}$ is an indicator variable which is 1 if the commissioner in office in state s in year t will work for insurance industry immediately after being commissioner.

The control variables in $X_{s,t}$ include whether the commissioner worked for insurance industry at any point prior his commissioner term ($I_{s,t}^{PRE}$) and number of financial exams in year s and state t. Regressions (2), (4) and (6) include these control variables.

Standard errors are clustered at the state level, and all regressions include state fixed effects and year fixed effects.

	Dependent variable:					
	log(1+n cert-s suspended$_{s,t}$)		log(1+n cert-s revoked$_{s,t}$)		log(1+n delinquency orders$_{s,t}$)	
	(1)	(2)	(3)	(4)	(5)	(6)
$I_{s,t}^{POST,immed}$	−0.091	−0.047	−0.078	−0.064	−0.079*	−0.069*
	p = 0.461	p = 0.688	p = 0.175	p = 0.274	p = 0.065	p = 0.099
$I_{s,t}^{PRE}$		0.110		0.082		0.037
		p = 0.243		p = 0.231		p = 0.673
n Dom Fin Exams$_{s,t}$		0.007**		0.002		0.002
		p = 0.011		p = 0.360		p = 0.358
E[LHS]	1	1	0.6	0.6	0.2	0.2
Empl.Hist.	immed	immed	immed	immed	immed	immed
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Cluster	s	s	s	s	s	s
Observations	738	736	738	736	614	614
R^2	0.661	0.669	0.548	0.549	0.426	0.429
Adjusted R^2	0.627	0.635	0.502	0.501	0.360	0.361
Note:						*p<0.1; **p<0.05; ***p<0.01

E Best's FSR robustness checks

E.1 Estimating default probability of each of Best's FSR

To compute the implied default probability of each of Best's FSR, I use the 10-Year Default Rates reported by AM Best for the period between December 31, 2008 and December 31, 2018.[17]

The provided 10-year realized default probability rates are shown in E.4. Not every rating is provided with 10-year default rate, but the realized default probability decreases exponentially in the rating, as shown in Figure E.5.

I estimate the implied default probability by fitting an exponential function through the available rating, using a linear fit between log of the realized 10-year default probability and the rating measured from 1 (E) to 15 (A++). Results are shown in Figure E.5 and Table ??. The linear fit has adjusted R^2 of 95.7%.

Credit Ratings as of 12/31/2008		Credit Ratings as of 12/31/2018 (Percent)													Other Outcomes During 12/31/2008-12/31/2018 (Percent)		
Credit Rating	Number of Ratings Outstanding	A++	A+	A	A-	B++	B+	B	B-	C++	C+	C	C-	D	Default	Paid off*	Withdrawn (other)
A++	128	64.8%	16.4%	0.8%	1.6%												16.4%
A+	634	11.4%	51.7%	17.8%	1.6%	1.4%	0.2%		0.3%								15.6%
A	1055	0.9%	13.1%	57.2%	8.2%	0.7%	0.5%	0.2%									19.3%
A-	1052	0.1%	5.7%	30.9%	31.6%	4.0%	1.2%	0.2%	0.1%						0.8%		25.5%
B++	362	0.6%	1.1%	9.1%	27.6%	15.2%	3.3%	1.7%			0.3%				1.4%		39.8%
B+	286		0.7%	5.9%	11.2%	11.9%	9.8%	4.5%	1.4%	0.7%	1.0%	1.0%	0.7%		1.0%		50.0%
B	120		0.8%	3.3%	12.5%	6.7%	4.2%	5.0%	1.7%	2.5%		0.8%			2.5%		60.0%
B-	61	1.6%	1.6%		4.9%		4.9%	1.6%		1.6%	3.3%	1.6%			3.3%		75.4%
C++	49			2.0%	6.1%	6.1%				2.0%			2.0%		4.1%		77.6%
C+	26				11.5%	3.8%											84.6%
C	13			7.7%													92.3%
C-	10														20.0%		80.0%
D	5														40.0%		60.0%
Total	3,801																

*Category Not Applicable to Insurance Companies

Figure E.4: Insurance Companies (Financial Strength Ratings) - 10-Year Transition and Default Rates (December 31, 2008 through December 31, 2018). Source: A.M. Best Rating Services, Inc. 2018 Ratings Performance Measurement Statistics for Exhibit 1 Form NRSRO.

[17] These numbers were provided by A.M. Best Rating Services, Inc. 2018 Ratings Performance Measurement Statistics for Exhibit 1 Form NRSRO.

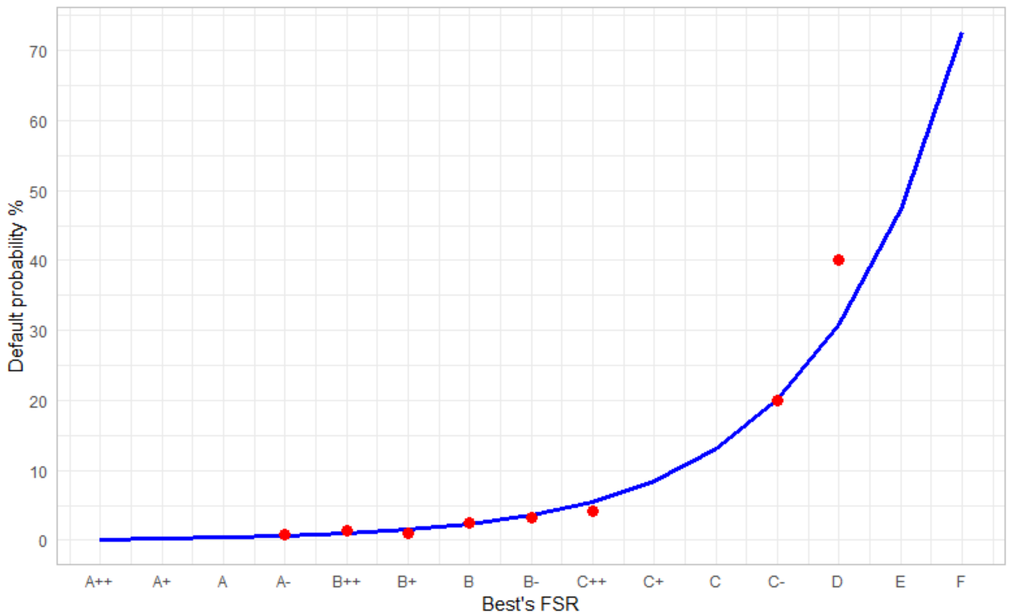

Figure E.5: Implied (fitted) vs 10-year realized default probabilities

I compute implied default probability by fitting a linear function of the log of default probability on ratings. Ratings were varying from 1 (F) to 15 (A++). Below are shown the fitted vs the realized default probabilities. In the main analysis, I use the fitted, or implied probabilities of each rating. The red dots show the AM Best realized default probabilities, and the blue line is the exponential fit through the available dots.

Table E.21: Implied (fitted) vs 10-year realized default probabilities

I compute implied default probability by fitting a linear function of the log of default probability on ratings. Ratings were varying from 1 (F) to 15 (A++). Below are shown the fitted vs the realized default probabilities. In the main analysis, I use the fitted, or implied probabilities of each rating.

	Default Probability [%]	
Best's FSR	Fitted	Realized
A++	0.18	–
A+	0.28	–
A	0.43	–
A-	0.66	0.8
B++	1.01	1.4
B+	1.54	1
B	2.37	2.5
B-	3.63	3.3
C++	5.57	4.1
C+	8.55	–
C	13.11	–
C-	20.11	20
D	30.85	40
E	47.32	–
F	72.59	–

E.2 Compare firms with and without Best's FSR rating

In Table E.22 I compare firms which have never had been rated by AM Best for financial strength and ones which have at least one rating. The level of observation is firm-year. The firms with rating tend to be larger: mean log total assets[$000] is 11.8 for FSR and 11.09 for non-FSR firms, which in dollars is $137M for FSR and $65M. However, the difference is within a standard deviation. The non-FSR firms tend to be better capitalized on average and slightly less likely to have exams resulting in recommendations. The likelihood for a firm in a given year to be monitored by post-term revolver is 43% for FSR firms, and 38% for non-FSR firms.

Table E.22: Summary statistics on variables between firms with at least one Best's FSR and never rated firms

	n		mean		sd	
Variable	fsr	rest	fsr	rest	fsr	rest
Yearly Risk Variable						
ACL RBC_{L1}	6518	56827	18.51	55.69	47.69	141.40
ΔACL RBC (std)	6509	55957	-0.08	0.00	0.35	0.98
log(tot.$Assets_{L1}$)	6615	60600	11.83	11.09	2.03	2.34
Δtot.Assets (std)	6614	59932	-0.08	-0.04	0.46	0.85
leverage $Ratio_{L1}$	6615	60600	0.53	0.50	0.21	0.28
Δlev. Ratio	6612	59636	-0.04	-0.02	0.68	0.81
op.Loss/tot.Assets	6614	59096	-0.04	-0.04	0.37	0.85
Most Recent Exam Outcome						
any $Recommendations_{i,s,t}$	6487	54311	0.72	0.67	0.45	0.47
any Fin. $Restatements_{i,s,t}$	6487	54311	0.36	0.35	0.48	0.48
n yrs since last $exam_{i,s,t}$	6669	81051	1.82	1.67	1.48	1.59
Post-term revolver indicators						
$I_{s,t}^{POST,ever}$	5436	68476	0.43	0.38	0.50	0.48
$I_{s,t}^{POST,immed}$	5089	64786	0.32	0.27	0.46	0.44

F Difference-in-Difference robustness nalysis

F.1 Collecting the set of law changes

The method of collecting the revolving door law changes followed the following steps:

1. I identified all present and past legal statutes which place restrictions on the commissioner after leaving office using three sources.The three sources are as follows:

    (a) Ethics Laws Section concerning commissioners from (National Association of Insurance Commissioners, 1999, 2015);

    (b) A publication on the current state revolving door laws affecting executive branch collected in 2005 and 2011 by the NGO *Public Citizen* (Public Citizen, 2005, 2011);

    (c) A database maintained by *National Conference of State Legislatures*, which keeps track of all law changes in state revolving door laws, 2010 to 2019.

2. I tracked the historical changes in the statutes identified by the sources above using Westlaw. This way, I narrowed the changes which are relevant to insurance commissioners.

3. I excluded from the final sample laws changes regarding bans affecting working for a firm which was former contractor for the government, since this is irrelevant for insurance commissioners working for insurance firms. The states where multiple changes took place where all in the same direction, so I use the earliest year as the shock year.

F.2 Predicting post-term revolver

I use a linear model to predict each commissioner's post-term revolver status (I_i^{POST}) using ex ante characteristics:

$$I_i^{POST} = \alpha_s + \alpha_T + \beta_i^{PRE} X_i^{PRE} + \beta_i Pers.Characteristics_i + \epsilon_i \tag{F.1}$$

X_i^{PRE} includes pre-term employment indicators showing whether, before his term started, the commissioner had employment history in insurance(I_i^{PRE}), government ($I_i^{\text{government, } PRE}$), etc. $Pers.Characteristics_i$ includes personal characteristics predictors age and age^2 at beginning of term and gender indicator variable I_i^{Man}. The regression also includes state in which commissioner served as well as fixed effect for the year in which a commissioner started her term.

Results from predicting ex ante post-term revolvers are in Table F.23. The law changes variable does not seem to affect the choice to be post-term revolver since the coefficient on $I_i^{\Delta Law}$ is not statistically different from 0, once the other variables are included.

The fitted model for predicted ever-post-term revolver seems to be slightly worse fit than the model for immediate post-term revolvers: the R^2 for the former is 54% (adj. $R^2 = 11\%$), which is less than the R^2 for the latter, which is 66% (adj $R^2 = 21\%$). Still, the R^2 is fairly high, given the outcome variable is binary.

The estimated models' accuracy is evaluated in Table F.24. It shows that the predicted value of the post-term revolver status is well predicted ex ante for both cases, but better for close than for all revolvers. The predicted value (rounded to 0 or 1) for all post-term revolvers matches the observed one for 85.1% of the commissioners. The predicted value (rounded to 0 or 1) for immediate post-term revolvers matches the observed one for 88.4% of the commissioners.

Table F.23: Predicting whether a commissioner will be post-term revolver

The regressions show results for the following regression, which tries to predict each commissioner's post-term revolver status (I_i^{POST}) using ex ante characteristics: pre-term employment indicators summarized in matrix X_i^{PRE} and personal characteristics matrix $Pers.Characteristics_i$ (includes age at beginning of term and gender indicator variable I_i^{Man})

$$I_i^{POST} = \alpha_s + \alpha_{T1} + \beta_i^{PRE} X_i^{PRE} + \beta_i Pers.Characteristics_i + \epsilon_i$$

Matrix X_i^{PRE} includes indicators showing whether, before his term started, the commissioner had employment history in insurance(I_i^{PRE}), government($I_i^{\text{government, } PRE}$), etc.
Columns (1-2) predict whether the commissioner ever becomes post-term revolver, and columns (3-4) predict whether he becomes post-term revolver immediately after his term ends. Columns (2) and (4) are identical to respectively (1) and (3) but they also include the indicator variable $I_i^{\Delta LAW}$, which is 1 if the commissioner's term started after the state experienced change in revolving door laws to test whether the laws affected the labour choices of commissioners. All regressions include state and year FE (year of beginning of term).

	Dependent variable:			
	$I_{s,t}^{POST,all}$		$I_{s,t}^{POST,immed}$	
	(1)	(2)	(3)	(4)
$I_i^{\Delta LAW}$		0.246		0.003
		p = 0.278		p = 0.990
I_i^{PRE}	−0.005	−0.009	0.114	0.114
	p = 0.967	p = 0.940	p = 0.531	p = 0.535
$I_i^{\text{government, } PRE}$	−0.153	−0.155	0.067	0.067
	p = 0.304	p = 0.297	p = 0.700	p = 0.703
$I_i^{\text{consultant/lobbyist, } PRE}$	0.194	0.217	−0.473	−0.472
	p = 0.292	p = 0.240	p = 0.195	p = 0.203
$I_i^{\text{lawyer, } PRE}$	0.210*	0.192	0.081	0.081
	p = 0.074	p = 0.104	p = 0.647	p = 0.652
$I_i^{\text{related industry, } PRE}$	0.418***	0.431***	0.108	0.108
	p = 0.003	p = 0.003	p = 0.607	p = 0.610
$I_i^{\text{other, } PRE}$	−0.076	−0.053	−0.300	−0.300
	p = 0.549	p = 0.678	p = 0.159	p = 0.163
I_i^{Man}	0.129	0.130	−0.018	−0.018
	p = 0.230	p = 0.228	p = 0.868	p = 0.869
Age at start of term	−0.033	−0.029	0.051	0.051
	p = 0.435	p = 0.491	p = 0.343	p = 0.347
(Age at start of term)^2	0.0003	0.0003	−0.0005	−0.0005
	p = 0.417	p = 0.478	p = 0.359	p = 0.363
Empl. Hist	full	immed	full	immed
Year Term Beginning FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	174	174	144	144
R^2	0.540	0.546	0.664	0.664
Adjusted R^2	0.106	0.108	0.212	0.199

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

Table F.24: Evaluating the predictive models' fit: predicted vs actual post-term revolver status

In Panel A, the fit is evaluated using regression (1) in Table F.23, and in Panel B, the fit is evaluated using regression (3) in Table F.23. Prediction for commissioner i is 1 if the fitted function is more than 0.5, and 0 otherwise.

Panel A: Predicted vs Actual Ever Post-term Revolver Status		
	$I_i^{POST,ever} = 0$	$I_i^{POST,ever} = 0$
Pred. $I_i^{POST,ever} = 0$	85	13
Pred. $I_i^{POST,ever} = 1$	13	63
Panel B: Predicted vs Actual Immediate Post-term Revolver Status		
	$I_i^{POST,immed} = 0$	$I_i^{POST,immed} = 0$
Pred. $I_i^{POST,immed} = 0$	88	11
Pred. $I_i^{POST,immed} = 1$	6	41

F.3 Commissioners in affected states: before and after the changes

Table F.25: Observable characteristics of commissioners in affected states - before and after the law changes

For each commissioner of states with law changes, I classify if they were in office before/during or after the law changed. I estimate for each group mean age, gender, and whether they had given work experience at any point before or after their job.

	n		mean		sd	
Variable	Pre	Post	Pre	Post	Pre	Post
age_i	36	19	47.06	53.42	10.31	10.20
I_i^{man}	37	20	0.59	0.80	0.50	0.41
$I_i^{government,PRE}$	36	20	0.92	0.75	0.28	0.44
$I_i^{insurance,PRE}$	36	20	0.25	0.30	0.44	0.47
$I_i^{lawyer,PRE}$	36	20	0.28	0.45	0.45	0.51
$I_i^{consultant,Lobbyist,PRE}$	36	20	0.06	0.05	0.23	0.22
$I_i^{relatedIndustry}$	36	20	0.11	0.20	0.32	0.41
$I_i^{other,PRE}$	36	20	0.28	0.30	0.45	0.47
$I_i^{insurance,POST}$	31	14	0.35	0.36	0.49	0.50
$I_i^{lawyer,POST}$	31	14	0.23	0.21	0.43	0.43