The Revolving Door and Insurance Solvency Regulation

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Abstract

Solvency regulation of the U.S. insurance industry occurs at the state level and is led by insurance commissioners who wield significant discretion. I construct a novel dataset of the employment history of these commissioners and find 38% of them work in the insurance industry after their term ("revolvers"). Revolvers are more lenient when regulating insurers' solvency along multiple dimensions. Consequently, insurers in revolver-led states over-reported their capitalization during the 2008 financial crisis by up to 10%. Revolvers leniency can lead to inflated insurer credit ratings, and consumers can be overpaying up to $27 billion in insurance premiums a year.

Keywords: solvency regulation; revolving door; state regulation; insurance; cooling-off laws

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I have nothing to disclose.

1. Introduction

Insurance is an $8.5 trillion industry that affects most households and firms in the U.S.[1] Compared with other financial institutions, insurance has historically been viewed as a stable sector, but growing evidence over the last 20 years shows insurers have increased their risk-taking.[2] These developments highlight the importance of insurance solvency regulation, especially given the long-term nature of insurance and the lack of transparency in insurers' risks.[3] The need to understand the consequences of insurance regulatory behavior has only increased in urgency with the COVID-19 pandemic, as the low-interest rate environment and corporate downgrades have put stress on insurers' balance sheets.[4]

One major concern is that insurance regulation occurs at the state level, which can lead to potentially inconsistent, and thus inefficient, regulation across states.[5] Within each state, insurance regulation is led by a commissioner, who has significant personal discretion. Anecdotal evidence suggests one of the factors affecting their behavior may be the revolving door: Public regulators exiting for jobs in the industry they regulated. Notably, 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."[6]

[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.

[2] The involvement of AIG's failure in the beginning of the 2008 financial crisis is well known. Less known is that in addition to AIG, several insurance companies applied for and received federal aid through the TARP program (Koijen and Yogo, 2015). Additionally, the systemic risk indicator SRISK by NYU Stern estimates that among the top 10 carries of systemic risks, four are insurers.

[3] 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). Moreover, Koijen and Yogo (2016) document that even regulators themselves often do not have the full information to assess insurers' solvency.

[4] See the news article by Cezary Podkul and Paul J Davies, published on June 17, 2020, by the *Wall Street Journal*, "Insurers Hit Brakes on Investments Designed to Make Risky Loans Safe."

[5] A rich literature on investigates why inconsistent regulation is inefficient; see Brennan and Schwartz (1982), Viscusi (1983), Prager (1989), Teisberg (1993), and Agarwal et al. (2014).

[6] 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.

This paper studies the effects of the revolving door on insurance solvency regulation, and its consequences for market participants. I find commissioners who leave office to work in the insurance industry ("revolvers") are more lenient in their solvency regulation along several dimensions. This more lenient regulation affects market transparency for both individual insurers and the whole insurance sector. I find that states monitored by revolvers leading up to the 2008 financial crisis let insurers over-report their solvency by up to $6 billion, or 10% of the total capital reported in 2008. Furthermore, I find insurers benefit from less strict regulation through higher insurance-specific credit ratings, which are key determinants of consumer demand and an insurer's ability to raise capital (Koijen and Yogo, 2015). Overall, I estimate that the revolvers' lenient regulation potentially leads consumers to overpay up to $27 billion a year. Finally, I explore the public policy implications. I find that laws restricting commissioners' move to industry are effective - revolvers become less lenient after their passing. This behavior shift suggests revolvers' leniency is influenced by exit incentives, and is not only driven by selection.

From a theoretical perspective, the revolving door effect on insurance regulation is unclear. One strand of theory predicts it may lead revolvers to be more lenient, as a quid pro quo for their future employers (Stigler, 1971, Peltzman, 1976, Eckert, 1981). Alternatively, if insurers hire commissioners for their expertise, revolvers may become stricter and put more effort into their job to signal high quality (Che, 1995, Salant, 1995, Bar-Isaac and Shapiro, 2011). Empirically, which effect prevails depends on the particular setting. For example, Tabakovic and Wollmann (2018) find the quid-pro-quo effect dominates among revolver patent officers, whereas Kempf (2020) finds the expertise effect dominates among revolver rating analysts.

To assess the effects of the revolving door within the insurance sector context, 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 work in the insurance industry after leaving office. Specifically, among the 271 commissioners, 37% are revolvers and 34% come with insurance industry experience. These sets are not fully overlapping: Only 16% come with insurance experience and eventually return to work there.

To test if revolvers differ in their financial solvency oversight strictness, I proxy for strictness using the frequency and outcomes of financial exams. These exams are a good setting in which to look for incentive distortions for two reasons. First, they are important for both insurers and commissioners. Insurers care about exams, because they can have large direct and indirect costs. At the same time, commissioners self-report spending a significant part of their time ensuring financial solvency, because a high-profile insolvency can negatively affect their government careers. Second, commissioners have significant personal discretion over when and whom to examine and over the exam outcomes for insurers. Indeed, some cross-state standards exist: Insurers should be examined at least once every five years, and some exam guidelines are common. However, a commissioner can always conduct an exam earlier than prescribed, and ultimately she determines the exam's consequences for the insurer.

I document that revolvers are more lenient regulators along a number of dimensions. I begin by showing revolvers perform 9% fewer exams for every year they are in office relative to non-revolvers. I show evidence that the revolvers' lower exam rate is not due to better expertise, as in Kempf (2020). Specifically, I test if revolvers perform fewer exams because they examine insurers at the first signs of financial distress, sooner than the regulatory allowed period between exams. These early exams are highly discretionary: commissioners call them when insurers look troubled or take too much risk. I find revolvers are in fact *less* likely to examine companies early. Using exam-level data, I find that all else equal, revolvers are 13.6% less likely to call for an early exam. Furthermore, revolvers are *less* sensitive to insurers' risk-taking behavior: They are less likely to call for an early exam after a drop in the RBC ratio (an insurer's capital held to regulatory required).

Next, using data on individual exams, I test whether revolvers compensate for their lower exam rate by being stricter with exam outcomes. The empirical evidence is inconsistent with this alternative hypothesis: I find that exams conducted by revolvers are in fact 7% less likely to require that insurers amend their financial statements to reflect the insurers' true financial state ("financial restatements"). Consistent with Tabakovic and Wollmann (2018), the effect is even larger for exams of potential employers (large insurers) and for early, thus discretionary, exams.

Another alternative explanation is that exams are a poor proxy for overall regulatory strictness. I test whether revolvers substitute the lenient exam environment with other punitive actions (e.g., restrictions on selling insurance in a state). However, I find no evidence for substitution between exams and such punitive actions. Consistent with revolvers being more lenient regulators, they do not perform more non-exam actions against financially troubled companies; in fact, they perform fewer of most punitive actions.

Lourie (2019) and Kempf (2020) show revolvers' incentives are more influenced by the revolving door at the end of their term. I check if revolver behavior changes in the last two years before commissioners leave office. On one hand, I do find revolvers increase their exam rate the year before they leave office, both overall and for early exams in these two years. On the other hand, the exams are still less likely to result in negative consequences for the insurer. Taken together, these findings imply revolvers use exams in the last two years to introduce themselves to potential employers, while insurers are avoiding the regulatory uncertainty of a new, potentially tougher, commissioner.

After establishing the connection between the revolving door and lenient regulation, I show the revolvers' leniency leads to less market transparency within the insurance sector. I begin by focusing on exams' effects on insurance specific credit ratings: A.M. Best's financial strength ratings (Best's FSR). 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). Additionally, a wider literature shows credit ratings affect firms' outcomes across sectors.[7] I show insurers' ratings decrease after exams that result in financial restatements. Because revolver exams are less likely to result in financial restatements, taken together, these results suggest revolver leniency may result in less information reaching the market. A simple back-of-the-envelope calculation shows that, more broadly, revolvers' leniency may result in consumers overpaying up to $27 billion a year in insurance.[8]

I further show evidence that the less strict regulation by revolvers allowed insurers to over-inflate their capitalization rates during the 2008 financial crisis. Specifically, Sen and Sharma (2020) show U.S. life insurers used internal valuation models to over-report the value of corporate bonds they held during the financial crisis. They estimate the aggregate levels of

[7] Ratings affect firms' capital structure (Kisgen, 2006), corporate bond yields (Crabbe and Post, 1994, Ederington et al., 1987), and stock prices (Hand et al., 1992).

[8] I show that following financial restatements, insurers sell less product (premiums fall). My estimate is based on this decrease, total yearly sales of the sector, and the estimated "missing" restatements due to lenient revolvers.

misreporting in each state and show higher levels of supervision can mitigate misreporting. I show that states supervised by revolvers have even higher levels of misreporting. The overall effect of overstatement resulted in insurers being 10% less capitalized than what was reported (by $3 billion to $6 billion more). This finding is especially troubling in light of the fact that multiple insurance insurers applied for and received federal reserve assistance, and given the consecutive discussion on how life insurers are carriers of systemic risk. It also emphasizes the far-reaching consequences of the revolving door not only on the regulated insurers, but also on the stability of the whole financial sector (Koijen and Yogo, 2015).

In the first two parts of my paper, I establish that revolvers are more lenient regulators, and I show this leniency has negative effects on market transparency. In the last part, I focus on a common policy to curb the revolving door: "cooling off" laws. These laws restrict former regulators for a given period from taking certain private sector jobs. Whether the laws are effective for commissioners depends on the mechanism driving revolvers to be more lenient (Bils and Judd, 2020). If revolvers are more lenient to appeal to future employers, a law change would alter their incentives, and they will become stricter. However, law changes will not change revolvers' behavior if they are more lenient only due to selection (e.g., some characteristic makes revolvers both more lenient and more willing to work for the private sector). In the 2000 to 2017 period, I find 14 revolving door law changes across 12 states. Consistent with the incentives mechanism, I find revolvers become less lenient toward insurers after these laws strengthen: They increase their exam rate, and the likelihood of financial restatements among early exams and exams of potential employers.

Related Literature

This study contributes to the literature on regulatory design by establishing an important driver of regulatory inconsistency across states. Understanding potential pitfalls is necessary because insurers have significantly increased their risk taking over the last 20 years (Koijen and Yogo, 2015) and the current low-risk environment puts further stress on many of them (Sen, 2019).[9] Recent studies have shown insurers are indeed highly sensitive to financial

[9] A rich literature examines optimal regulatory design in the banking sector (Dewatripont, 1994, Boot and Thakor, 1993, Hellmann et al., 2000, Kisin and Manela, 2014). However, the state-based regulatory model of the insurance sector, in combination with the increasing sophistication of insurers, creates unique challenges.

solvency regulations (Merrill et al., 2012, Becker and Ivashina, 2015, Becker and Opp, 2013, Koijen and Yogo, 2015, Ge, 2021) and that these regulations can vary dramatically across states and over time (Ellul et al., 2015, Koijen and Yogo, 2016, Kim, 2017, Sen and Sharma, 2020). Yet, little is known about the sources of this regulatory heterogeneity.[10] Thus, I contribute to this literature by establishing commissioners' post-term labor outcomes as a major driver of regulatory forbearance. Moreover, I show revolvers' leniency leads to less transparent insurance markets, which can lead to significant costs for consumers (Koijen and Yogo, 2015).[11]

The paper is also part of a large literature studying the effect of the revolving door on regulatory outcomes. It is the first to explore the revolving door effects on insurance solvency regulation.[12] My finding that the revolving door leads to *overly lenient* regulation is surprising. In fact, Tabakovic and Wollmann (2018) (who focus on U.S. patent officers) is the only other study to document the revolving door leads to less strict regulation in the executive branch. By contrast, other revolver financial regulators become *more* strict (federal reserve employees (Lucca et al., 2014) and SEC lawyers (DeHaan et al., 2015)). Evidence from the private financial industry is mixed: Kempf (2020) shows revolver financial analysts are more accurate, but she finds that consistent with Cornaggia et al. (2016) and Lourie (2019), they are more lenient toward their own future employers.[13]

The difference in findings likely stems from the level of regulation. Insurance is regulated mostly at the state level, whereas the rest of the finance industry is mostly regulated at the federal level. Specifically, Agarwal et al. (2014) and Charoenwong et al. (2019) show state- and federal-level regulators often act differently, with state-level regulators being more lenient

[10] Notable exceptions include Leverty and Grace (2018) and Liu and Liu (2020), who highlight election-cycle effects.

[11] These costs are in addition to the more general costs of regulatory uncertainty highlighted in Brennan and Schwartz (1982), Viscusi (1983), Prager (1989), Teisberg (1993), and Agarwal et al. (2014).

[12] Within the insurance literature, Grace and Phillips (2007) study the effect of the revolving door on a different outcome - auto insurance premiums - and for an earlier time period (1985-2002). By contrast, my paper focuses on financial solvency regulation, and a broader set of outcomes.

[13] Another strand of the revolving door literature focuses on why insurers employ revolvers, as opposed to how revolvers act while in office. Blanes I Vidal et al. (2012) and Bertrand et al. (2014) show the private sector hires former Congress staffers because of their connections rather than expertise. Consistent with these findings, Emery and Faccio (2020) show evidence that insurers that hire federal regulators are fined less but engage in more wrongdoing. In a narrower context, Shive and Forster (2017) provide evidence consistent with the expertise theory: insurers that hire U.S. financial sector regulators decrease their risk.

toward the industry.

The rest of the paper is organized as follows. Section 2 provides the institutional background and motivates the choice of financial exams as a main proxy for financial oversight strictness. Section 3 details the data-collection process used for the study, and summarizes the extent of the revolving door in insurance regulation. Section 4 empirically documents that revolvers are less strict regulators. Section 5 analyzes the effects of the less strict regulation on bond misreporting and on Best's FSR. Section 6 provides evidence that revolvers respond to incentive shifts caused by revolving door cooling-off laws. Section 7 concludes and discusses the findings' implications.

2. Institutional Setting: Financial Exams in Insurance

For insurers, a financial exam is an audit to ensure they are in good financial health and able to meet their potential obligations to consumers. When a commissioner orders an exam, a team of auditors is sent on location to assess the insurer's solvency risk. The team assesses whether the insurer's self-reported quarterly and annual regulatory statements are true, whether undocumented sources of risk exist, and whether the insurer adhered to the laws of the state. After the exam is over, the auditors share their findings and recommendations with the insurer and the commissioner, and the commissioner ultimately decides what further steps are necessary. Importantly, exams can be performed whenever a commissioner deems them necessary, but should be conducted at least once every five years.[14] Apart from the direct discretion over the exams, commissioners also select the teams that conduct the audits, and generally set the tone for the agency regulatory environment.

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

Insurers prefer to be examined rarely, and by a more lenient commissioner, because exams can be disruptive, expensive, and result in various negative consequences. To start with,

[14] Klein (2005) explains that all insurers' regulatory statements are reviewed on a quarterly basis for red flags, which may ultimately trigger a financial exam.

insurers have to cover the exam costs, which can add up to millions of dollars, and they are, on average, eight months long. Additionally, the exam outcomes can vary considerably. In the best-case scenario, an exam has no recommendations. Alternatively, minor recommendations could require only small changes (e.g., "get an additional board member"). However, at the more severe end of outcomes, exams can require insurers to make costly changes ("create a risk model for risk X") or make unfavorable change to their publicly observable regulatory financial statements. Restatements can hurt an insurer's credit rating, which in turn can affect both the consumer demand and the insurer's ability to raise capital. In the most extreme case, an exam's findings can trigger the state to put the insurer into state receivership (usually, a precursor to liquidation).[15]

Financial oversight strictness can differ dramatically depending on the commissioners' career goals. Lenient commissioners can establish a positive relationship with a future employer and signal they are pro-industry. However, being too lenient can negatively affect a commissioner's current job. Specifically, a commissioner can be negatively affected if an insurer engages in poor management practices and becomes insolvent. State guarantee funds set a limit on the maximum payouts consumers can receive, and they force the remaining insurers in the market to take over the liabilities up to that limit. Therefore, an insolvent insurer hurts both the remaining insurers, who must take on extra liabilities, and the consumers, who may face a delay and limit on the payouts they receive. Such insolvency externalities create political pressure on the commissioners, as documented by Leverty and Grace (2018), and force them to be stricter with financial oversight.

Insurers can do business in multiple states, but the main monitoring falls on 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 insurers that sell insurance in their state, the main burden falls on the domicile state (i.e., the state of the insurer's regulatory headquarter). As a result, non-domicile commissioners typically accept the financial exam conducted by the domicile state, in lieu of conducting their own exam. In practice, 99.5% of all conducted exams are of domestic (domiciled in the commissioner's

[15] 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. For more information, see the news report from *WorkCompWire* from April 24, 2011, "CA Insurance Commissioner Announces Conservation Rehabilitation of Majestic Insurance Co."

state) insurers. On one hand, this practice avoids duplicate exams. On the other hand, incentive distortion in financial exams has more serious consequences, because only one regulator systematically monitors each insurer. If the domicile commissioner does not disclose and correct for risky behavior, it may lead to asymmetric information between insurers and other market participants, and consumers from both domicile and non-domicile states can be affected.

3. Data

3.1. Gathering data on the revolving door in insurance regulation

No ready-made employment history database for insurance commissioners exists. 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. On average, I find 3.8 jobs for commissioners before they start office and 2.7 after they leave. See Appendix A for more information on the data-gathering procedure. I classify each job in one of five general categories: the insurance industry, government, consulting or law firm, related industry (e.g., finance or real estate), or other, unrelated, job. Figure 1 shows the prevalence of each job type.[16]

This newly constructed data set reveals a widespread practice of commissioners either coming from or moving back to the insurance industry. Specifically, 37% of commissioners had at least one job in insurance after their term ("revolvers"). Additionally, 28% exited to insurance immediately, or within a year after their term ended ("immediate revolvers"). Furthermore, 34% of commissioners had at least one job in insurance before their commissioner term (pre-term revolvers). Note that 16% came from and exited into insurance, so the pre- and post-term revolvers are not fully overlapping sets. This distinction is important because if the sets were overlapping it would not be possible to distinguish if the revolving door leniency is driven by selection or revolving door incentives.

What other jobs do commissioners take, apart from insurance? As shown in Figure 1, the

[16]See Figure D.4 for commissioners' jobs immediately before/after their terms.

commissioners most often work in (other) government positions. 85% of commissioners have pre-term experience in government (e.g., other regulator position, elected office, or working as a staffer), and 49% of commissioners work in government after their term ends. The second most common job is in the insurance industry, closely followed by lawyer/consultant (33% pre-term and 36% post-term). Note some of the commissioners in that category are also acting as revolvers, because they work to promote the insurers' interest. Identifying that subgroup in a consistent manner would be challenging, but the results in this paper are likely a lower bound for the true revolving door effect.

Many of the jobs that revolvers take are in government relations positions. This distinction is important because these jobs are more likely to use commissioners' connections (as in Bertrand et al., 2014), rather than their 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. The findings are shown at Figure 2. Approximately a third of all revolvers work only in jobs that cannot be classified based on whether they have contact with regulators. However, among the jobs that can be classified, I find 34% of pre- and 54% of post-term revolvers have jobs that rely on government connection.

How does the revolving door extent compare with other studies? The percentage of revolvers in my sample is similar to the one Grace and Phillips (2007) establish for insurance commissioners between 1985 and 2002. The levels are slightly higher than in studies from different fields that provide equivalent statistics, which is likely due to the shorter nature of commissioners' terms. Kempf (2020) finds revolvers are 27% among financial rating analysts, whereas DeHaan et al. (2015) find 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, that governor often appoints a new commissioner. Eleven of the remaining states elect their commissioner every four years.

3.2. Aggregate state-year data on financial exams

I use the number of exams as a proxy for financial oversight strictness. I access the aggregate number of exams completed in a given state in a given year through the archives of NAIC's Insurance Department Resource Report (IDRR). From it, I also extract other state-year aggregate variables, such as the number of actions taken against companies, resources available to insurance departments, and number of insurers domiciled.

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 closely matches the distribution of all exams conducted. 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 insurers are domiciled in each state in a given year, and insurers 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 revolvers on the exam rate, I control for the number of domestic insurers, 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 exams begin around eight months before they are completed.

3.3. Insurer-level data on financial exams and Best's FSR

The main source of insurer-level exam data is the annual financial reports, which every Life, Health and Property/Casualty company must submit to its domicile state. In these annual reports, insurers must answer questions about their most recent financial exams. The main source of these reports is *S&P Market Intelligence*, which I supplement with data collected from insurance departments' websites and FOIAs.

The variables I construct using the annual reports include the date each exam was completed and individual exam outcomes.[17] Specifically, I assess if the exam resulted in any recommendations (true in 60% of the cases) and whether the exam conclusion forced the insurer to restate its financial statements to reflect findings during the exams (30% of the cases).[18]

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.[19] I discuss further the coverage of the data and how it compares with aggregates in Appendix C.1.

Using the annual reports, I also construct insurer-specific variables on the balance sheets of the insurance companies to control for their solvency risk. The variables of interest are total assets, which proxy for insurer size, and various measures of how much risk the insurer has taken, including the 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 numerator being positive minus negative cash flow). These variables are standardized to have mean 0 and standard deviation 1.

Finally, I add Best's FSR to the insurer-year panel.[20] Although the full exam-level panel covers 5,183 insurers, only 618 insurers 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 F.1). The distribution of all ratings and each ratings-implied probability are plotted in Figure 4, and Panel F in Table 1 provides a summary for default and rating-universe rating probabilities on the FSR sample, Finally, I compare the observables of insurers with and without ratings at Appendix F.2.

[17] Given the low total number of non-domicile state exams, I assume each exam was conducted by its domicile state.

[18] The specific annual report questions that allow me to infer outcomes of the exam are (1) whether the insurer complied with exam recommendations and (2) whether the insurer 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.

[19] Although I don't observe exam outcomes for these early exams, I use these extra exams in the early exams analysis.

[20] AM Best rating data are also provided by *S&P Market Intelligence*.

4. Differences in Revolver Regulation

4.1. Revolvers perform fewer financial exams

I begin my analysis by showing revolvers conduct fewer financial exams per year. I estimate the following regression using a state s- and year t-level panel:

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

The outcome variable $Y_{s,t}$ is either the absolute or log of the number of exams completed in state s in year t. The variable of interest is $\mathrm{I}^{\mathrm{POST}}_{s,t}$, which is an indicator equaling 1 when the commissioner in office in state s and year t is a revolver. I control for state and year fixed effects to remove any underlying variation coming from state idiosyncrasies (e.g., state-specific rules) or shocks affecting all states at the same time, such as the financial crisis. Thus, estimates come from the variation within state and within each time period. $X_{s,t}$ is a matrix of control variables, which accounts for 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), and also time-varying resources available to commissioners for conducting the exams: number of domestic insurers, log of the insurance department budget, and log of the number of employees working on financial solvency in year $t-1$. All errors are clustered at the state level to account for correlation in variables at the state level.

Consistent with the hypothesis that revolvers are lenient regulators, Columns (1) and (2) of Table 2 show revolvers perform around 3.1 to 3.8 fewer exams for each year they are in office, depending on controls. Given that the average number of exams per state per year is 29.6, these estimates are both statistically and economically significant. The results are similar if the number of exams is logged: Revolvers perform between 9% and 10% fewer exams than non-revolvers for every year they are in office.

I perform several additional tests and show my main results are robust to using different measures of regulatory strictness and subsamples of the data. First, in the main specification, I measured regulatory strictness using the number of exams conducted on domestic insurers, because out-of-state companies are examined only when there is a solvency concern not addressed by the insurer's domicile commissioner, and is therefore much rarer. Table B.1 in Appendix B.1 shows the main results are robust to using the total number of exams. Second, in the main specification, I excluded commissioners with terms shorter than one year, because finishing an exam can take up to a year. However, my results are robust to including these commissioners as well (see Appendix B.2, Table B.2). Third, I confirm the results are not driven by one particular jurisdiction, by re-running regression (1) on a panel that excludes each of the 51 jurisdictions one at a time (see Table B.3 and Figure B.1). Fourth, I check that the results are robust to the model specification being Poisson, rather than OLS (see column (1) of Table E.13). Using an OLS may bias the results, since the number of exams cannot be negative, and Poisson is appropriate given that the response variable is counts.

Finally, I ensure the results are robust to revolver specification. Consistent with Lourie (2019), I define a commissioner as an immediate revolver if she starts working for the insurance industry within a year after leaving office ($\mathrm{I}_{s,t}^{\text{POST, immed}}$). Table D.10 shows that when using immediate revolvers, the effect increases in both absolute size and significance: β decreases to -6 with no controls and -4.8 with controls. In the log specification, the exam rate decreases even further for immediate revolvers, who perform 10% to 12.7% fewer exams.

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

I analyze insurer-level exams to test if revolvers perform fewer exams because they are indeed less strict regulators or because they are strict in other ways. To do so, I construct an insurer-year panel by connecting individual exams to insurer-specific measures of risk and exam outcomes. My insurer-level analysis allows me to control for individual insurers' risk characteristics. Thus, I control for different regulators being responsible for insurers with different levels of financial health. Specifically, I mitigate the concern that a revolver examines fewer insurers, because he supervises insurers in better financial health.

I test two alternative explanations under which we could see revolvers performing fewer exams but not being less strict regulators. One explanation is that revolvers are better at spotting poor performers early on, and are therefore more targeted in their exams and more likely to conduct exams early (consistent with the findings of Kempf, 2020). Therefore, I test if revolvers perform more early exams overall and more early exams in response to deteriorating risk observables. A second explanation is that revolvers perform fewer exams, but the exams they do perform are more likely to result in negative consequences. Therefore, I test if the exams led by revolver commissioners are more likely to result in negative consequences for insurers, after risk observables are accounted for.

Early exams and risk sensitivity

To check if revolvers have expertise and are better at spotting troubled insurers, I test whether revolvers are more likely to examine an insurer as soon as its risk increases, instead of waiting for the regulatory required fifth year. In other words, I test if, all else equal, revolvers are more likely to conduct early exams - exams that occur four years or less since the previous exam of the insurer. Note that because early exams are discretionary, they indicate a regulator is being proactive and exercising more effort.

The unconditional probability of an exam in a given year is 18%, consistent with our expectation that insurers should be examined at least once every five years. Figure 3 plots the cumulative distribution of years between exams split by whether the commissioner is a revolver. From the figure, I observe that only 9.5% of exams are late (more than five years since the previous exam). However, early exams are frequent: Over 50% of exams happen within four years of the most recent exam. From the figure alone, we can also see that although early exams are not rare, revolvers are unconditionally less likely to examine an insurer early.

Many factors may contribute to a regulator's propensity for an early exam, for example regulatory resources and the insurer's financial health. To account for these factors, I test whether revolvers are indeed less likely to examine early after controlling for observables. I only keep observations in the insurer-year panel that are within four years of the insurer's prior exam, and I test if revolver status predicts an early exam:

$$\text{Is exam year}_{i,t} = \beta \mathrm{I}^{\mathrm{POST}}_{s,t} + \beta_r \text{Risk Vars}_{i,t} + \gamma_x \mathrm{X}_{i,s,t} + \alpha_s + \alpha_t + \epsilon_{i,t}. \tag{2}$$

Is exam year$_{i,t}$ is an indicator equal to 1 if insurer i is examined in year t. Risk Vars$_{i,t}$ and $\mathrm{X}_{s,t}$ are, respectively, risk-specific and non-risk-specific control variables. The risk variables include lagged yearly level and percent difference in log assets, leverage ratio, regulatory capital, and operational loss.[21] These variables are selected in accordance with existing studies that measure insurers' risk (e.g., Koijen and Yogo, 2015, Sen and Sharma, 2020). The non-risk-specific variables include the number of years since the insurer's previous exam, plus all other variables included in regression (1).

Column (1) of Table 3 shows revolvers are, in fact, *less* less likely to perform early exams. An insurer is 1.5% less likely to be examined early when a revolver is in office. The result is economically and statistically significant. The unconditional probability for an early exam in any given year is 11%, so having a revolver in office decreases this likelihood by 14%.

Another alternative explanation for my findings is that revolvers perform early exams less frequently, but they are more knowledgeable, and therefore only intervene when the risk of the insurers increases (as in Kempf, 2020, who shows rating agency revolvers are more accurate in their forecasts). To test this explanation, I modify regression (2) and test whether revolvers respond differently to the risk observables:

$$\text{Is exam year}_{i,t} = \beta \mathrm{I}^{\mathrm{POST}}_{s,t} + \beta_r \text{Risk Vars}_{i,t} + \gamma_r \left(\mathrm{I}^{\mathrm{POST}}_{s,t} \times \text{Risk Vars}_{i,t}\right) + \\ + \gamma_x \mathrm{X}_{i,s,t} + \alpha_s + \alpha_t + \epsilon_{i,t}. \tag{3}$$

The coefficients of interest are β and the vector of coefficient is γ_r. Specifically, $\beta + \gamma_r \times \overline{\text{Risk Vars}}_{i,t}$ measures the change in the likelihood of an early exam by revolvers, evaluated at $\overline{\text{Risk Vars}}_{i,t}$, the mean of the risk variables. γ_r captures the increase in the early exam probability once risk variable r increases by a unit. Thus, if β and γ_r are both negative, revolvers can be more risk sensitive only if they can pick out risky companies using a signal uncorrelated with traditional risk variables.

I find revolvers are actually *less* sensitive to risk observables, with a differential response observed for changes in the level and changes in regulatory capital (RBC). The RBC ratio scales the capital which an insurer has to the capital it is required to have, and the larger the ratio, the less risky the insurer is. Although, on average, a one standard deviation increase

[21] Summary statistics for the control variables are in Panel E of Table 1.

in regulatory capital decreases the probability of an early exam by 0.54%, a revolver in office fully offsets this effect.

I run several robustness checks. First, I test the robustness of the results to the definition of an early exam, as an exam within two, three or four years since the previous exam. Results are shown in Table C.6. The coefficients preserve their direction. Second, I check if revolvers are even less strict with potential future employers. Tabakovic and Wollmann (2018) show patent-officer revolvers are extra lenient toward potential future employers. Based on the characteristics of revolvers' employers in Figure C.3, I assume revolvers consider large insurers as likelier employers. I rerun the regressions on a panel of large insurers.[22] The results are in Table C.8, and they confirm revolvers are even less likely to examine a potential employer early. For more details, see Appendix C.3.

Exam outcomes

Next, I test if the exams led by revolvers are more likely to result in negative consequences for an insurer, and therefore, an insurer would act more prudently to avoid them. To address this concern, I test which factors predict that a given exam will result in a negative consequence for the insurer. I observe two types of negative outcomes: whether an exam results in any recommendation, and whether one of those recommendations forces the insurer to make a restatement of their financial filings. If exams by revolvers result in fewer negative outcomes, this finding would be evidence that revolvers are not using strict outcomes as a preventative measure but are, rather, less strict regulators. I run the following regression:

$$\text{Exam Outcome}_{i,t} = \beta \mathrm{I}_{s,t}^{\mathrm{POST}} + \beta_r \text{Risk Vars}_{i,t} + \gamma_x \mathrm{X}_{i,s,t} + \alpha_s + \alpha_t + \epsilon_{i,t}. \tag{4}$$

In regression (4), the panel is limited to those insurer-year observations in which insurer i was examined. The outcome variable Exam Outcome$_{i,t}$ is an indicator that equals 1 whenever the exam for insurer i, conducted in year t, results in a negative outcome for the insurer. In total, 34% of exams result in financial restatements, and 65% of the exams result in recommendations of some form. The coefficient of interest here is β: it measures the increase in the likelihood of the exam resulting in a negative outcome for the insurer when a revolver

[22] I consider insurers "large" if their assets are at least as large as the smallest revolver employer.

is in office. Risk $\text{Vars}_{i,t}$ and $\text{X}_{i,s,t}$ are defined as in regression (3).

The underlying assumptions here are that (a) the more likely a commissioner is to make a recommendation, the stricter she is, and (b) a recommendation or restatement are negative events for the insurers. The latter assumption is especially true for financial restatements, since they can attract negative attention from investors. For example, it can trigger an automatic review of an insurer's credit rating, which is known to be important for investors and consumers (Koijen and Yogo, 2015).[23]

The results in Table 4 show revolver exams are less likely to result in negative outcomes. The results are stronger for financial restatements, especially for early (therefore discretionary) exams. Revolver exams are 2.4% less likely to result in a financial restatement, which is a 7.1% decrease in the unconditional restatement probability.[24] Early revolver exams are 11.7%(= 0.041/0.35) less likely to result in restatements relative to the unconditional probability. The result is smaller and less statistically significant for an exam resulting in any recommendation at all. Revolver exams are 2.6%(=0.017/0.65) less likely to result in any recommendation, but the result is not statistically significant. However, among early exams, the result is statistically significant and larger in magnitude: Revolver exams are 6.1%(= 0.043/0.7) less likely to result in any recommendations. These results are consistent with the hypothesis that revolvers are less strict regulators, and go against the alternative explanation that revolvers compensate for performing fewer exams by making them stricter.

I run several robustness checks. First, Table D.11 shows the results are similar in magnitude for immediate revolvers but increase in statistical significance. Second, I check the robustness of the early exam results to the definition of an early exam (one within two, three or four years since the previous exam; see Table C.5). For financial restatements, the earlier the exam, the less likely a revolver is to force an exam. The results for any recommendations lose significance for exams that are less than three years since the most recent one, but they are not consistent with the alternative hypothesis that revolvers will force more

[23]Note exams are more likely to result in negative consequences for the insurers that take more risk. Specifically, I find negative exam outcomes are more likely whenever (i) insurers have smaller asset levels, (ii) the insurers 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 $t-1$.

[24]The β estimate from regression (4) is -0.024, and the average probability for any exam resulting in a restatement is 0.34, so the change in unconditional probability is $-7.1 = \beta/E[LHS] = -0.024/0.35$.

recommendations.

Finally, I check if revolvers are even less strict with insurers comparable in size to their future employers. I rerun regression (4) on a panel of insurers whose assets are within the range of the smallest and largest future employer. The results, shown at Table C.1, confirm revolver exams are even less likely to result in negative consequences if the examined insurer is a potential employer. Specifically, revolvers are 10% (18%) less likely to force a restatement during an average (early) exam. For more details, see Appendix C.3.

4.3. Other punitive actions against firms

So far, I have established that revolvers are less strict when examining insurers. However, revolvers could still try to keep firms disciplined by imposing harsher penalties against insurers once an insurer is troubled.

I test this hypothesis by comparing the punitive actions taken due to solvency concerns by revolver status. If revolvers perform more of these actions, this would imply that revolvers substitute between less strict exams and stricter punishments. Alternatively, if revolvers are less strict with both exams and punitive actions, this would imply that financial exams are a good proxy for overall strictness and that revolvers are less strict regulators. Thus, I run regression (1) with the number of various punitive actions in state s in year t as dependent variables. I also control for the number of exams completed in state s and year t, since some of the actions are taken as a result of exam findings. This way, the coefficient β is the difference in punitive actions once the lower exam rates are accounted for.

My data source, NAIC's IDRR, provides three different punitive actions aggregated at the state-year level: the number of certificates suspended, certificates revoked, and delinquency orders. Suspending an insurer's certificate means the insurer is temporarily not allowed to sell insurance in the state until certain solvency conditions are met. Revoking a certificate is a more severe punishment, often taken by non-domicile states: It is a permanent ban on the insurer doing business in the state. Delinquency order is the harshest punitive action: If an insurer is fully insolvent, the domicile state steps in and puts the company in state-run receivership, which often is the first step toward liquidation.[25]

[25]On average, 3.5 certificates are suspended, 2 certificates are revoked, and 0.7 delinquency orders are

The results, shown in Table 5, show that even accounting for the lower number of exams, revolvers perform fewer punitive actions. This finding is consistent with revolvers being less strict regulators, and not consistent with revolvers substituting less strict exams with stricter punishment. From columns (2) and (6), we can see revolvers suspend 24.9% fewer certificates, and put 58% fewer insurers in delinquency proceeding. Regarding the number of certificates revoked, I find no statistical difference between revolvers and non-revolvers. However, note this action is more likely to be used by non-domicile insurers, so it is not a proxy for how a commissioner monitors the insurers that she is directly responsible for. Furthermore, this result still does not provide evidence for revolvers taking more punitive actions.

I run several robustness checks in Appendix E. To ensure the results are not driven by outliers, I rerun the regression with the log number of actions on the left-hand side. I also check that the results are robust to the model specification being Poisson and not OLS (see columns (2) through (4) of Table E.13). Using an OLS may bias the results, since the number of exams cannot be negative, and Poisson is appropriate given that the response variable is counts. The results are similar in both specifications.

4.4. Revolving door effects near the end of commissioner's term

Other revolving door studies show the revolving door incentives are stronger at the end of the regulators' terms (Lourie, 2019, Kempf, 2020). Thus, I test if the revolvers' behavior changes near that time. An impending departure may affect revolvers' incentives in several ways: it can make them more lenient, as in Tabakovic and Wollmann (2018), or more strict to everyone except their future employer, as in Lourie (2019) and Kempf (2020). Focusing on the last two years provides insight in the matching mechanism of revolvers to employers and the incentives in place.

Specifically, I focus on the commissioners' last two years in office.[26] I start by looking at the aggregate number of financial exams, so I modify regression (1) as follows:

placed per state per year. The outcome variables are summarized in Panel D of Table 1.

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

$$\text{(5)} \qquad \mathrm{Y}_{s,t} = \beta \mathrm{I}^{\mathrm{POST}}_{s,t} + \beta_T \mathrm{I}^{\mathrm{T}}_{s,t} + \beta_{T-1} \mathrm{I}^{\mathrm{T-1}}_{s,t} + \gamma_T \left(\mathrm{I}^{\mathrm{POST}}_{s,t} \times \mathrm{I}^{\mathrm{T}}_{s,t}\right) + \gamma_{T-1} \left(\mathrm{I}^{\mathrm{POST}}_{s,t} \times \mathrm{I}^{\mathrm{T-1}}_{s,t}\right) + \\ + \gamma_x \mathrm{X}_{s,t} + \alpha_s + \alpha_t + \epsilon_{s,t}.$$

The new variables in (5) are $\mathrm{I}^{\mathrm{T}}_{s,t}$ and $\mathrm{I}^{\mathrm{T-1}}_{s,t}$. These indicators equal 1 if the commissioner in state s and year t is in, respectively, her last or penultimate year in office. Note both Liu and Liu (2020) and Leverty and Grace (2018) find that political cycle affects the timing of insurance regulators' actions, so I control for the state-specific election cycle in (5).[27] By controlling for the election cycle, I ensure the timing results are driven by career concerns and not political pressure due to elections.

In regression (5), the variables of interest are $\mathrm{I}^{\mathrm{POST}}_{s,t}$, $\mathrm{I}^{\mathrm{POST}}_{s,t} \times \mathrm{I}^{\mathrm{T}}_{s,t}$, and $\mathrm{I}^{\mathrm{POST}}_{s,t} \times \mathrm{I}^{\mathrm{T-1}}_{s,t}$. β measures the baseline difference in exam rates between revolvers and non-revolvers, whereas $\beta + \gamma_T$ (or $\beta + \gamma_{T-1}$) measures the difference between the two groups in the last (or penultimate) year of the commissioner's term.

Results are in columns (1) and (2) of Table 6. Consistent with the findings from (1), revolvers perform 12% fewer exams per state per year during most of their term, but in the year before they leave office, they increase the number of exams to match the rate of non-revolvers.

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

[27]Including state election cycle effects is especially necessary because almost half of the commissioners' departures are in the year after a state election.

$$(6) \quad \text{Is exam year}_{i,t} = \beta \mathrm{I}^{\mathrm{POST}}_{s,t} + \beta_T \mathrm{I}^{\mathrm{T}}_{s,t} + \beta_{T-1} \mathrm{I}^{\mathrm{T-1}}_{s,t} + \gamma_T \left(\mathrm{I}^{\mathrm{POST}}_{s,t} \times \mathrm{I}^{\mathrm{T}}_{s,t} \right) + \\ + \gamma_{T-1} \left(\mathrm{I}^{\mathrm{POST}}_{s,t} \times \mathrm{I}^{\mathrm{T-1}}_{s,t} \right) + \beta_r \text{Risk Vars}_{i,t} + \gamma_x \mathrm{X}_{i,s,t} + \alpha_s + \alpha_t + \epsilon_{i,t}.$$

Results are in columns (3) to (5) of Table 6. Insurers are between 2% and 7% less likely to be examined early by revolvers. However, this difference decreases if the revolver is in her penultimate term year. These results are consistent with revolvers increasing the exam rate as an industry-friendly gesture.

An alternative explanation is that, as Lourie (2019) documents, revolvers examine more in order to "punish" competitors: The revolvers already know who their future employers are and, more importantly, who their future competitors are. In this case revolver exams will also become more strict. However, I observe no change in the likelihood of exam outcomes in the two years leading up to commissioner departure (see Table C.9). Thus, my results are consistent with Tabakovic and Wollmann (2018), where revolvers treat all potential employees leniently.

5. Consequences of the Less Strict Revolver Regulation

In the previous section, I provided evidence that revolvers are less strict regulators. However the optimal level of strictness is unclear. For example, revolvers may be too lenient, as in Tabakovic and Wollmann (2018). On the other hand, the non-revolvers may be overly strict, which can also lead to suboptimal regulation (Stigler, 1971, Djankov et al., 2002).

A major challenge is that there is no obvious benchmark for the optimal level of insurance regulation. Nevertheless, I provide evidence that lenient regulation by revolvers leads to markets being less transparent. I show financial exams (my main proxy for strictness) provide information that affects insurance-specific credit ratings. Thus, insurance solvency regulation is more than just a costly regulatory hurdle - it indeed informs market participants about an insurer's riskiness. Next, I quantify the costs of this lack of informativeness during the 2008 financial crisis. I show insurers that were regulated by states with more revolvers were more likely to over-report their bond holdings to appear more solvent. As a result, these insurers appeared 10% more capitalized than they actually were during the crisis.

5.1. Best's FSR: Response to financial restatements

Like all audits, financial exams are costly for insurers, but they reveal information about the underlying health of insurers. In this section, I quantify the informativeness of these financial exams and then discuss potential implications of my findings in light of revolvers performing fewer and less consequential exams.

To test whether exams are informative, I focus on changes in A.M. Best's financial strength ratings (Best's FSR) after financial exams. These insurance-specific credit ratings assess whether an insurer is solvent, and Koijen and Yogo (2015) show they affect the consumer demand for insurance products. A.M. Best revisits its ratings approximately once a year, and issues a grade (from A++ and F). I translate each rating into a default probability using the 10-year historical defaults, provided by A.M. Best.[28] The rating distribution and the implied default probability of each rating grade are shown in Figure 4. Note that like other sectors, credit ratings are a service paid for by the rated insurer and are voluntary. Thus, the insurers who are rated tend to be larger and are likely more systemically important (see Table F.15).

I use the following regression to test whether the negative exam outcomes (financial restatements) are followed by an increase in default probability:

$$\text{(7)} \qquad \mathrm{Y}_{i,t} = \beta_f \text{new fin. restatement}_{i,t-1} + \gamma_r \text{Risk Vars}_{i,t} + \gamma_x \mathrm{X}_{i,s,t} + \alpha_s + \alpha_t + \epsilon_{i,t}$$

Each observation is a rating event of insurer i in year t: the first rating issuance, rating re-evaluation (at one a year), or an exit from the rating universe. I focus on two outcome variables to capture rating events on the intensive and extensive margin. On the intensive margin, I track the change in rating in insurer i's default probability between years $t-1$ and t. On the extensive margin, if an insurer expects a negative rating, it may choose to no longer be rated, so I use an indicator variable equal to 1 if insurer i was removed from AM Best's rating universe in year t. The variable of interest is new fin restatement$_{i,t-1}$ -

[28] See Appendix F.1 for more details on how the default probability was estimated.

an indicator equaling 1 when insurer i was examined and needed to issue a restatement in the year leading up to the rating event. Note that to rule out reverse causality I only use ratings that come after the exam is published. The control variables $X_{i,s,t}$ include the same insurer-risk controls included in (2), plus an exam-year indicator (to capture exams that don't require restatements). In this regression, I include both state and year fixed effects, as well as state $\times$ year fixed effects. All errors are clustered at the state level.

Table 7 shows that following exams requiring restatements, Best's FSR tend to imply a higher default probability, and insurers are more likely to choose to stop being rated by A.M. Best. Specifically, I find the release of an exam that required financial restatement is associated with a 7-basis-point increase in the default probability (which is a significant change, given that the average yearly change in default probability is 2.4 basis points). Similarly, after a restatement, an insurer is 63% more likely to stop being rated. I also show the result is mostly driven by lower-rated insurers: In Table 8, I re-estimate regression (7) only on observations whose rating in year t is below A++, A+, A, and A−. The magnitude of the effect increases from 7 basis points to 34 basis points for insurers rated below A−. Therefore, the riskier an insurer is, the more likely a restatement is to be followed by a rating decrease.

My results are consistent with information provided by A.M. Best in personal correspondence. The company representatives reported that the rating procedure pays particular attention to financial exams. A.M. Best receives a summary of the financial exam only after it is completed and it has little pre-public release information on the exam content. Moreover, the representatives confirmed that any financial restatements trigger an automatic rating review, and restatements resulting in lower capital tend to receive extra attention.

Taken together, being regulated by a revolver is beneficial for insurers: they are more likely to avoid financial restatements, which can lead to a decrease in their rating. My earlier results imply revolvers force 18.1% fewer restatements for larger insurers.[29] Koijen and Yogo (2015) show a drop in the A.M. Best rating from A++/A+ to A/A− leads to 10% increase in demand elasticity. However, this leniency can lead to poor market transparency: Both

[29] In the previous section, I showed revolvers perform 9% fewer exams per year (Table 2) and are 10% less likely to force a restatement for a large insurer (Table C.7). Note potential employers of commissioners and A.M. Best rated insurers tend to be larger insurers.

consumers and investors rely on credit ratings' accuracy. If the credit ratings are overly-optimistic when a revolver is in office, consumers and investors are unknowingly taking too much risk or overpaying for a product.

I take a more direct approach to estimate how much consumers are overpaying due to lenient regulation by revolvers. Koijen and Yogo (2015) show ratings determine demand, but, more broadly, demand is a function of all information available on the market. Thus, I estimate the change in insurers' premiums sold the year following a restatement (see Appendix F.3 and Table F.16). I find that the year after a restatement, insurers' sales drop by 37%, a result that is stronger for early (discretionary) exams. Based on these results and the results from section 4, I make a back-of-the envelope calculation that if all revolvers were substituted with non-revolvers, consumers would have paid up to $27 billion less in premiums in 2018 across all insurance lines.[30]

5.2. Revolvers and aggregate bond misreporting during the 2008 crisis

In this section, I use the findings of Sen and Sharma (2020) to quantify the opacity that revolver regulation engendered during the 2008 financial crisis. These calculations provide another illustration of the consequences of lenient revolver regulations.

Sen and Sharma (2020) find that during the 2008 financial crisis, U.S. life insurers used internal valuation models to over-report the value of corporate bonds. As a result, their regulatory capital was inflated by $9 billion to $18 billion or by 30% of the capital reported in 2008. Sen and Sharma (2020) estimate the average level of misreporting in each state s in 2008, and find heterogeneity in how much misreporting each state allowed in their regulated insurers.[31] Finally, they show stricter regulation, specifically more exams, decreased misreporting.

I take this analysis a step further and show misreporting was higher in states with more revolvers leading up to the 2008 crisis. The raw data confirm this hypothesis: In Figure 5, I show the average state misreporting level is higher in states with at least one post-term commissioner between 2005 and 2008. I test this hypothesis explicitly using the following

[30] See F.3 for more details of this calculation.

[31] Their estimates for state-level misreporting are shown at Figure F.7.

regression:

$$\underbrace{\text{Misreporting}_{s,2008}}_{\text{from SS'20}} = \alpha + \beta \bar{\text{I}}^{\text{POST}}_{s,'08-\tau:'08} + \underbrace{\gamma_s \text{Supervision}_{s,2008} + \gamma_x \text{X}_s}_{\text{as in SS'20}} + \epsilon_s. \tag{8}$$

In regression (8), $\text{Misreporting}_{s,2008}$ comes from the state estimates in Sen and Sharma (2020), shown in Figure F.7, and the control variables are as in the paper.[32] The variable of interest is $\bar{I}^{POST}_{s,'08-\tau:'08}$, which is the average number of years that state s had a revolver commissioner between years $2008 - \tau$ and 2008, and τ can be one, three or five years. I include the average number of revolver commissioners over a period of a few years to account for the cyclical nature of supervision and for exams happening once every few years.

The results, shown in Table 9, imply monitoring was less strict in revolver states, and the aggregate solvency of insurers in the crisis was significantly overstated. Specifically, Sen and Sharma (2020) estimate that the average misreporting is 220 basis points per bond. The results in Table 9 show the average misreporting per bond in states lead by revolver commissioners is higher by between 60 and 80 basis points. This increase is a quarter to a third of the baseline effect in Sen and Sharma (2020) and is highly economically significant. Specifically, monitoring by revolvers translates to regulatory capital being inflated by up to \$3 billion to \$6 billion, or by up to 10% of the total capital reported in 2008.

6. Commissioners' Response to Revolving Door Law Changes

In the previous two sections, I document revolvers are less strict regulators, and as a result, markets are less transparent. In this section, I focus on revolving door cooling-off laws as a public policy tool to mitigate these effects. Specifically, I test if revolvers become stricter after the tightening of revolving door laws. These laws would not be effective if the difference in behavior is driven purely by selection (an unobservable characteristic drives both revolvers being less strict regulators and them joining industry). On the other hand, if revolvers are less strict due to incentives, the laws would decrease their ability to exit, making them more

[32]The authors control for supervisory variables (the number of financial analysts and examiners, number of discretionary exams, and budget per domestic insurer in a state, sourced from NAIC's IDR) and controls for average states' solvency (mean RBC ratio and log assets of all life insurers domiciled in each state).

strict.

I identify 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.[33] 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 G.17.[34]

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 affected states are representative of all states. In Table G.18, I compare states with and without law changes on their populations, insurance premiums written, and GDP (total and from insurance), and show few differences exist among the states.

These laws potentially affect revolvers by decreasing their value to a potential employer. As discussed in the data section, many of the revolvers work in government relations positions, and many of them are lawyers by education. If the revolver cannot represent his employer in front of the insurance department, someone else needs to be hired to perform his functions. The value of the revolver likely decreases for the employer, so the salary offered and the probability of an offer is smaller. As a result, non-insurance industry job options become comparatively more attractive.

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

[33]The exception is a South Dakota 2011 law change relaxing revolving door laws - results are robust to excluding this state.

[34]See Appendix G.1 for more information on the procedure I use to identify the laws.

$$(9) \qquad \mathrm{Y}_{s,t} = \beta \mathrm{I}^{\mathrm{POST}}_{s,t} + \beta_L \mathrm{I}^{\Delta\mathrm{LAW}}_{s,t} + \gamma_L \left(\mathrm{I}^{\mathrm{POST}}_{s,t} \times \mathrm{I}^{\Delta\mathrm{LAW}}_{s,t}\right) + \gamma_x \mathrm{X}_{s,t} + \alpha_s + \alpha_t + \epsilon_{s,t}.$$

As in regression (1), the outcome variable is the absolute/log number of exams. The variable of interest is $I^{POST}_{s,t} \times I^{\Delta LAW}_{s,t}$. If post-term revolvers respond to revolving door laws, they will perform more exams after laws get stricter, so the cross-term coefficient γ_L will be positive.

Columns (1) and (2) of Table 10 show results from regression (9). In states where revolving door laws got stronger, revolvers respond by significantly increasing their exam rate relative to the non-revolvers. The left panel of Figure 6 shows the difference in exam rates by years to law change. From it, we see no evidence for a pre-trend in outcomes before the law changes. Furthermore, one year passes before the law changes affect commissioners' behavior.

I also test if, after the laws get stricter, revolver exams are more likely to result in negative outcomes (restatements). I modify regression (4) to fit the DiD setting, and for each exam, I run the following regression:

$$\begin{aligned} \text{Any Fin Restatement}_{i,t} &= \beta \mathrm{I}^{\mathrm{POST}}_{s,t} + \beta_L \mathrm{I}^{\Delta\mathrm{LAW}}_{s,t} + \gamma_L \left(\mathrm{I}^{\mathrm{POST}}_{s,t} \times \mathrm{I}^{\Delta\mathrm{LAW}}_{s,t}\right) + \\ (10) \qquad & + \gamma_r \text{Risk Vars}_{i,t} + \gamma_x \mathrm{X}_{i,s,t} + \alpha_s + \alpha_t + \epsilon_{i,s,t}. \end{aligned}$$

In this case, if post-term revolvers respond to the policy change, the cross-term coefficient γ_L will be positive, and revolver exams will result in more financial restatements after the changes take effect. Similarly, if incentives matter, the more discretion goes into an exam, the stronger the response would be. Therefore, I expect the results to be stronger among early exams and exams of insurers large enough to be potential future employers.

The results in Table 10 show revolvers change their behavior following law changes. I find that after revolving door laws get stricter, revolvers force more financial restatements during earlier exams, and exams of potential employers (i.e., large insurers). Among early exams, before the laws strengthen, revolver exams are less likely to result in restatement, but after laws changes, all commissioner exams are more likely to result in a restatement. From column (5), the restatement probability for non-revolvers increases by β_L or 8.4%, and for revolvers, by $\beta_L + \beta + \gamma$, or 7.5%. Similarly, for large insurers that can be potential employers, the difference in exam outcomes between revolvers and non-revolvers disappears, as shown in column (7). Specifically, there is no difference in non-revolver exam outcomes before and after the law changes. However, revolver exams are 4.1% less likely to result in restatement before the law change, and 1.5% *more* likely after the law change. The result for all exams four years or more since the previous exam loses significance, but the cross-term coefficient is of comparable magnitude.

The right panel of Figure 6 shows the difference in the likelihood of financial restatements by years to law change among early exams for insurers that are comparable in size to potential employers for predicted and realized revolvers. There is no evidence for a pre-trend in outcomes before the law changes.

Taken together, the results imply revolving door laws are an effective tool to encourage revolvers to be stricter. These results have implications for the mechanism in place as well: the difference in behavior is not driven only by selection, and revolvers act differently due to post-term incentives. This finding is consistent with most revolving door findings (Kempf, 2020, Lucca et al., 2014, Cornaggia et al., 2016, Tabakovic and Wollmann, 2018).

Therefore, I predict that revolvers would respond to public policies that target revolvers' incentives - for example, cooling-off laws, currently used by only 35 states, and salary increases that can incentivize job performance and retention.[35]

An important caveat when discussing cooling-off laws is that such measures may disincentivize talent from entering a government job. In my current setting, the time frame is likely too short to explore the full extent of the selection. The average age of commissioners is mid-40s and often requires years of government experience. At this late-career stage, switching career paths would be difficult, but early-career potential employees may opt out of government altogether (Bils and Judd, 2020).

[35]For example, *The Hill* reported in 2018 that the U.S. Congress was considering increasing the salaries of its congressional staff to reduce revolving door activity. See the opinion piece by Donald Sherman, "Congress could raise salaries to close revolving door to lobbying," published on December 28, 2018, by *The Hill*.

7. Conclusion

In this paper, I study the prevalence and effect of the revolving door among top insurance regulators. I hand-collect employment history of insurance commissioners and show 38% of them enter the insurance industry after their term ends. I find that while in office, these revolvers are less strict with the financial oversight of insurers. Then, I show their more lenient regulatory regime affects the information that reaches markets. Finally, I provide evidence that revolving door cooling-off laws are effective in making revolvers stricter.

The importance of this paper lies in documenting a major source of distortion in the regulation of insurers, the largest of whom are major financial institutions. The financial crisis of 2008 emphasized the importance of a healthy insurance sector. As a consequence of the crisis, the Federal Reserve Board attempted to classify several major insurers as systemically important financial institutions (SIFIs) and to subject them to an extra level of oversight. However, the courts held that insurance regulation should be kept at the state level.[36] The COVID-19 epidemic has put a strain on life insurers' liabilities and renewed insurers' fears that the Biden administration may attempt to classify some of them as SIFIs again.[37]

Still, financial supervision of insurers is currently left to the states, so understanding the factors that affect the state regulators is important. This is especially true given that, in practice, an insurer's financial stability is monitored by a single state, but the insurer can do business in all other states. Therefore, the lenient regulation of one state can affect consumers in the rest of the country.

My paper focuses only on the supervision aspect of regulation. In that sense, my results are likely a lower bound, because they leave out the rule-making side of regulation. At the state level, revolvers can directly create less strict rules, since commissioners are left with some discretion on rule-making (e.g., OTTI rules (Ellul et al., 2015), and allowing shadow insurance (Koijen and Yogo, 2016)). Furthermore, revolvers can vote for more regulatory

[36] See *Pensions & Investments* article from January 19, 2018, "MetLife, FSOC end legal case over SIFI designation" by Hazel Bradford.

[37] See *Insurance Journal* article from November 17, 2020, "Federal Reserve Raises Questions Over Life Insurers Risk of Coronavirus Claims" by Alwyn Scot.

forbearance when creating model laws at the NAIC that will affect all states (Becker et al., 2021, documents such a law and its effect). Although rule-making is beyond the scope of this paper, my results on supervision suggest a promising avenue for further research.

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Figures

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

I classify each employment event (job) in the commissioners' employment database in one of five categories: government, insurance, lawyer/consultant, other related industry (e.g., finance or real estate), or other, unrelated category (e.g., non-profit). Each bar at the figure below represents the percent of commissioners with at least one employment event in the given job category. The left panel is focusing on the employment events before commissioners are in office, and the right panel is focusing on the employment events after commissioners leave office.

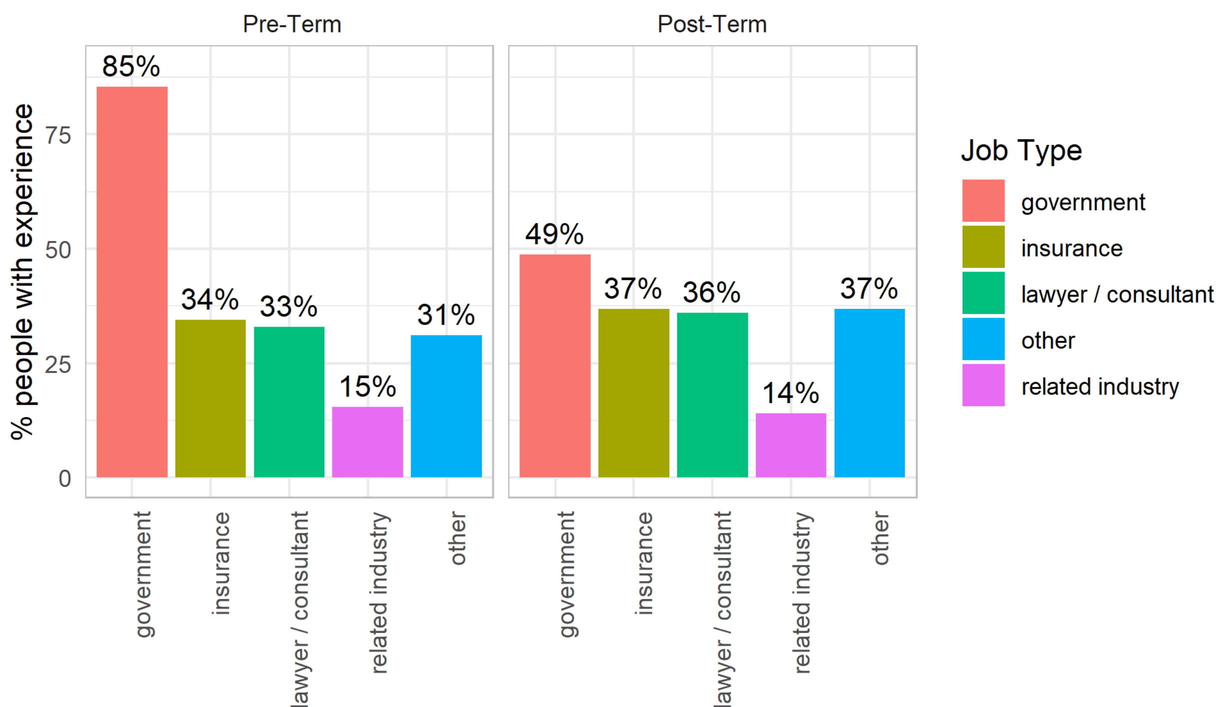


Figure 2: Percent of insurance jobs which require acting as a liaison between employer and insurance departments

I classify each insurance job based on whether it requires acting as a liaison between the employer and insurance departments (based on the job title or description). Most job titles/descriptions can be classified as government relations or not: for example, "VP of Government strategy," is a government relations job, and "VP of Marketing," is not a government relations job. 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". The left panel includes the insurance jobs which commissioners held before their term began. The right panel includes the insurance jobs which commissioners held after their terms ended.

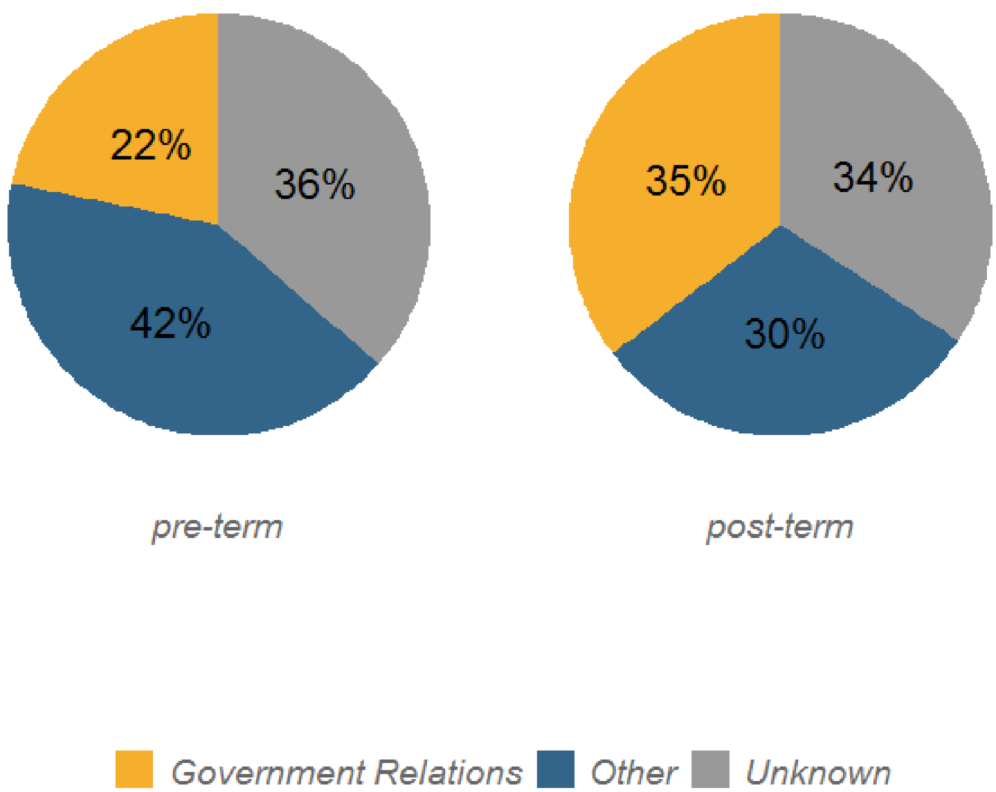


Figure 3: Distribution of the years between completion of financial exams

The figure shows the cumulative distribution of the time between exams for revolvers (blue dashed line) and non-revolvers (red solid line). The y-axis shows the share of exams which are completed within no more than x years of the previous exam. The gray vertical line is at 5.1 years to show that most exams are completed within 5 years of the previous exams. 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.

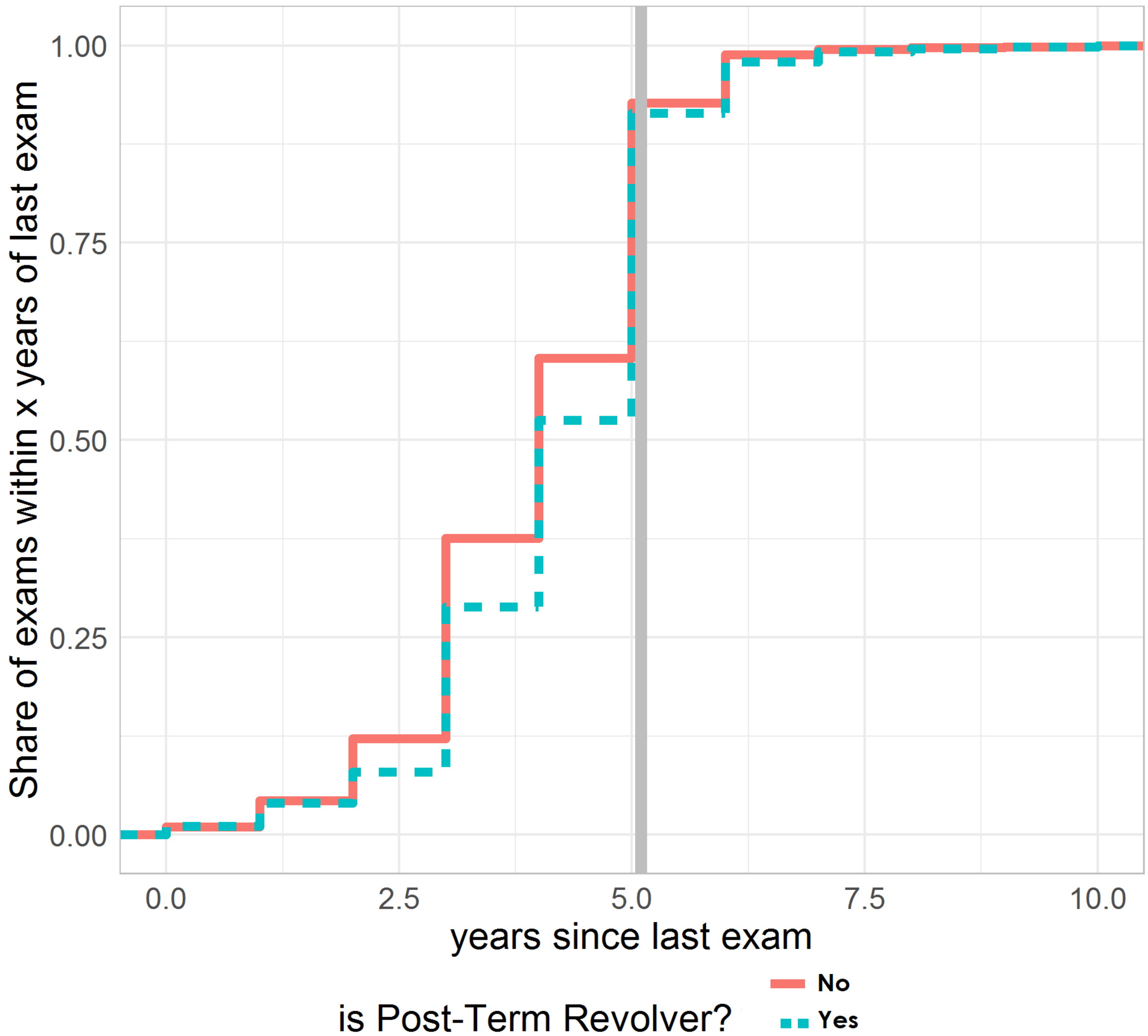


Figure 4: Distribution of A.M. Best's Financial Strength Ratings and their corresponding implied default probabilities

The lower panel plots the distribution of all insurers' ratings between 2006 and 2018. The upper panel plots the implied default probability of each rating grade, based on the 10-year default probabilities reported by A.M. Best in 2018. Appendix F.1 provides more information on how implied default probability was estimated.

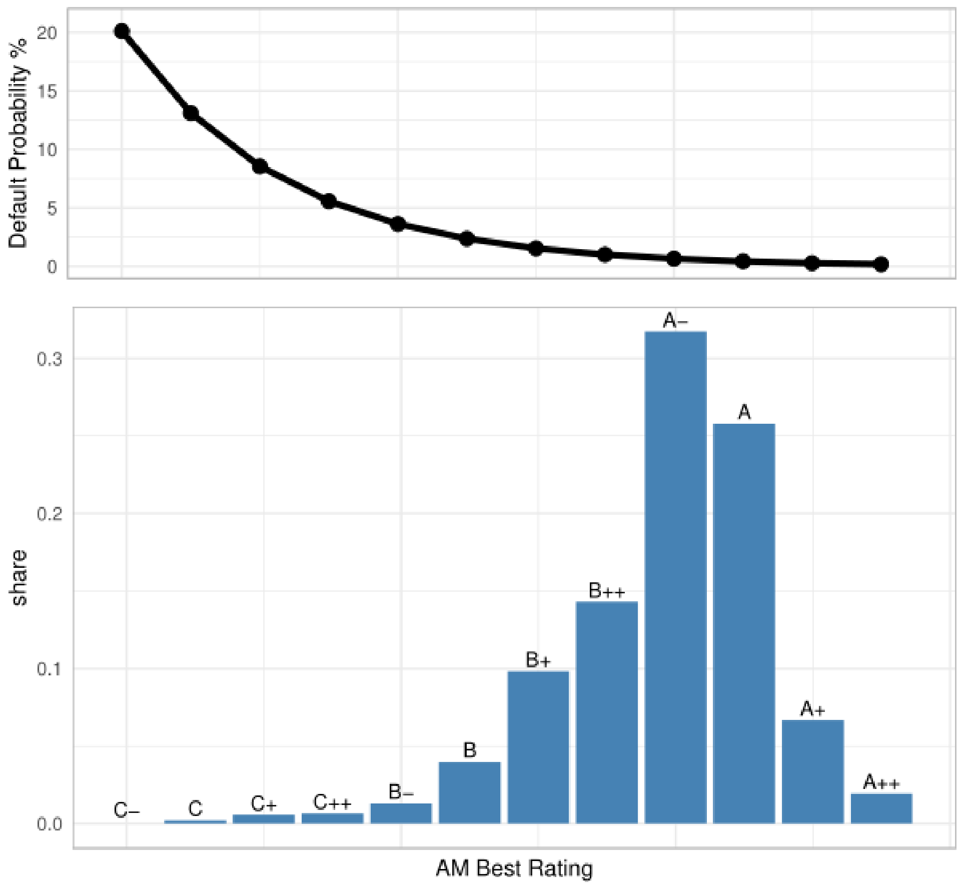


Figure 5: Level of state's misreporting by whether the state was supervised by any revolvers between 2005 and 2008

I plot whether the state had at least one revolver between 2005 and 2008 (on the x-axis) on the average state's level of misreporting in corporate bonds as document by from Sen and Sharma (2020). Each dot is an individual state. The shaded area is the mean level of misreporting for each group.

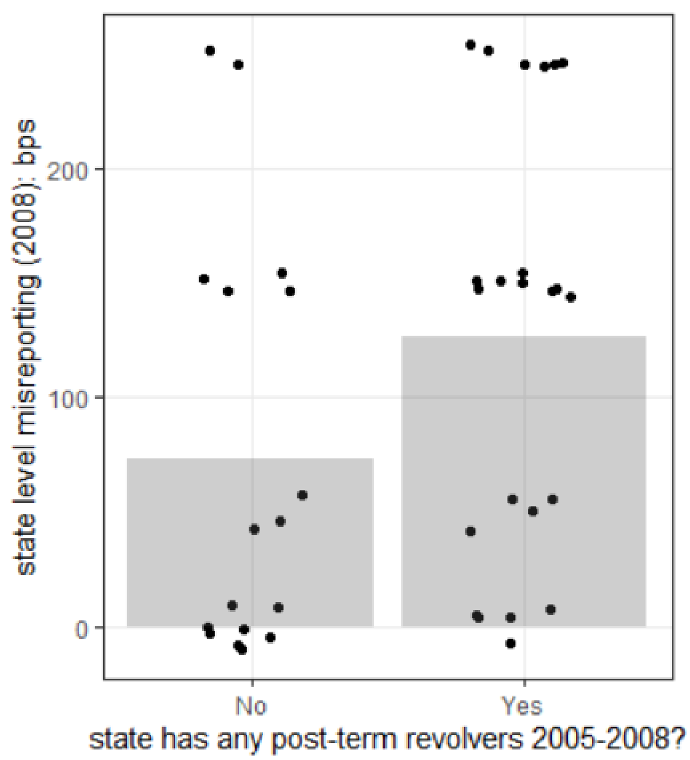
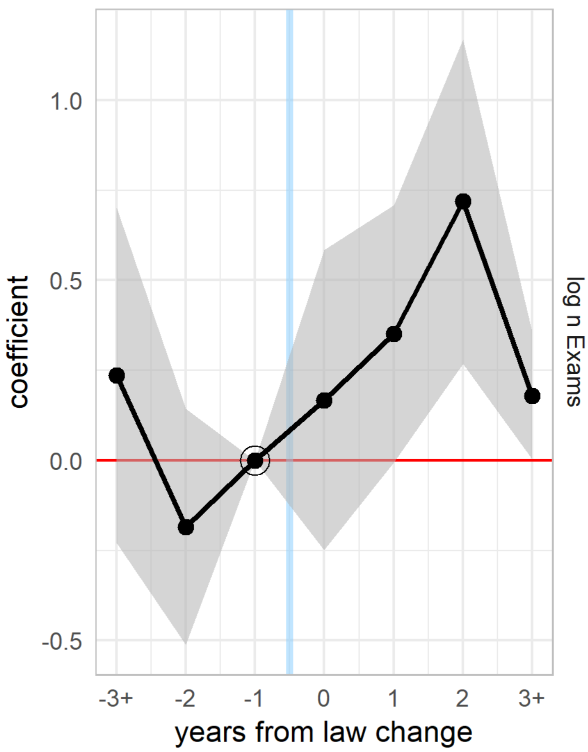


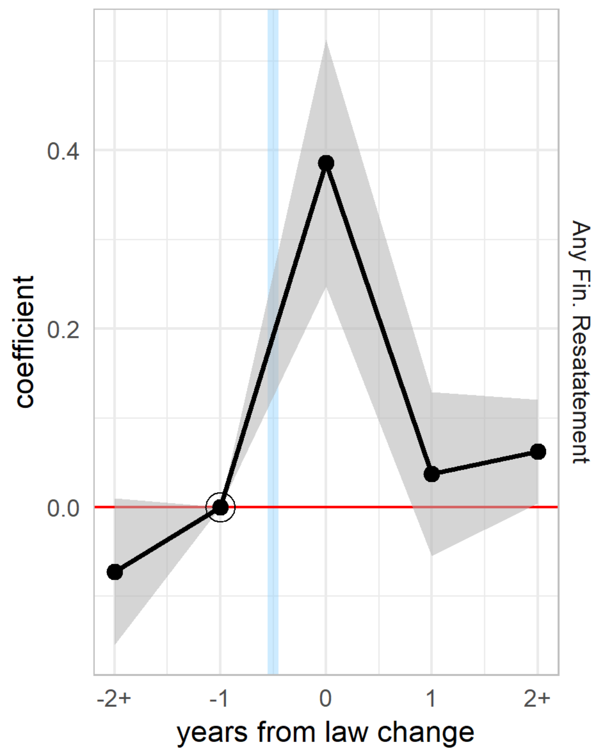
Figure 6: Difference in exam rates and outcomes by years to law change

The figure shows the coefficient estimate β_m from the regression below, against years to treatment m:

$$Y = \sum_m \beta_m \times I_{s,t}^{POST} \times I_{s,t}^{\Delta\text{LAW, m yrs from law change}} + \alpha_s + \alpha_t + X + \epsilon$$

The dependent variable in the left panel is log number of financial exams, and in the right panel – an indicator which is 1 when an exam results in restatement. The variable of interest $I_{s,t}^{POST} \times I_{s,t}^{\Delta\text{LAW, m yrs from law change}}$ is 1 when in state s, year t: (1) the commissioner is revolver, (2) state s is affected by a change in revolving door cooling off laws between 2000 and 2017, and (3) the change in the law is m years before/after year t. The estimate represents the difference in exam behavior between the treatment group of revolvers and the control group of non-revolvers, a given number of years from the change. The years beyond 2 or 3 years from the change are grouped because those bins have too few observations (also recall that financial restatement data is only available after 2006). The graphs on the left use realized revolver as a treatment group, and graphs on the right use predicted revolvers as a treatment group.





Tables

Table 1: Summary Statistics

Below are summary statistics of for the variables used in the analysis, estimated between 2000 and 2018. The variables are estimated over a panel at the level of: (1) a state-year in the table panels A, B, C, D; (2) insurer-year in the table panels E (3) FSR Best rating event per insurer (close to insurer-year) in table panel F. The statistics shown, from left to right, are number of observations, mean, standard deviation, 10^{th} and 25^{th} percentile, median, 75^{th} and 90^{th} percentile.

Variable	mean	sd	pctl_{10}	pctl_{25}	median	pctl_{75}	pctl_{90}
Panel A: Revolver variables							
$I^{POST}_{s,t}$	0.43	0.5	0.0	0.0	0.0	1.0	1.0
$I^{PRE}_{s,t}$ (pre-term revolver status)	0.36	0.5	0.0	0.0	0.0	1.0	1.0
Panel B: Number of financial exams							
n total $\text{exams}_{s,t}$	29.78	29.7	4.0	4.0	20.0	37.0	70.4
n exam $\text{domestic}_{s,t}$	29.64	29.6	4.0	4.0	20.0	37.0	70.0
Panel C: State-Year control variables							
n domestic $\text{insurers}_{s,t}$	159.93	161.0	26.0	26.0	106.0	208.0	390.0
n all $\text{insurers}_{s,t}$	1551.48	265.5	1262.4	1262.4	1536.0	1709.0	1868.4
$\text{budget}_{s,t}$ [$M]	24.04	34.4	5.2	5.2	13.1	24.3	39.3
n $\text{examiners}_{s,t}$	29.07	36.9	4.0	4.0	18.0	33.5	66.3
Panel D: Actions against insurers based on solvency concerns							
n cert. $\text{suspended}_{s,t}$	3.50	5.3	0.0	0.0	1.0	5.0	10.0
n cert. $\text{revoked}_{s,t}$	1.95	3.9	0.0	0.0	0.0	2.0	6.0
n delinquency $\text{order}_{s,t}$	0.68	3.0	0.0	0.0	0.0	0.0	1.0
Panel E: Exam-level variables							
any $\text{recommendations}_{i,t}$	0.67	0.5	0.0	0.0	1.0	1.0	1.0
any fin. $\text{restatements}_{i,t}$	0.35	0.5	0.0	0.0	0.0	1.0	1.0
is exam $\text{year}_{i,t}$	0.28	0.4	0.0	0.0	0.0	1.0	1.0
n yrs since last $\text{exam}_{i,t}$	1.67	1.6	0.0	0.0	1.0	3.0	4.0
Panel F: Best's FSR variables							
ΔDefault $\text{probability}_{i,t}$%	0.02	0.8	0.0	0.0	0.0	0.0	0.0
$I^{remove}_{i,t}$	0.02	0.2	0.0	0.0	0.0	0.0	0.0
new financial $\text{restatement}_{i,t}$	0.08	0.3	0.0	0.0	0.0	0.0	0.0

Table 2: Number of exams by revolver status

The table summarizes results from regressing a measure of exams conducted in state s and year t on whether the commissioner in office is a revolver, as shown in regression (1). The dependent variable $Y_{s,t}$ is the number of domestic financial exams in state s and year t in columns (1) and (2), and log of the number of exams (plus one) in columns (3) and (4). $I^{POST}_{s,t}$ 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. Regressions (2) and (4) control for whether the commissioner worked for the insurance industry at any point prior to his commissioner term, the number of domestic insurers in state s, year t, log of the budget that the state insurance department had in year s and state t, and 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.

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

	n exams$_{s,t}$		log(n exams$_{s,t}$+1)	
	(1)	(2)	(3)	(4)
$I^{POST}_{s,t}$	-3.748^{**}	-3.058^{*}	-0.109^{**}	-0.087^{**}
	(1.777)	(1.587)	(0.048)	(0.040)
E[LHS]	29.64	29.64	2.99	2.99
Controls	No	Yes	No	Yes
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$
Observations	834	829	834	829
Adjusted R^2	0.860	0.864	0.865	0.868

Table 3: Predicting early exams: timing and sensitivity by revolver status

The table shows results from regression (2) in column (1) and from regression (3) in columns (2) to (6). The insurer-year panel is limited to those observations which insurer i in year t is up to 4 years away from his most recent exam. Indicator is exam year$_{i,t}$ is 1 if insurer i is examined in year t. Control variables for all regressions include insurers' financial risk (lagged level and change in RBC and leverage ratio, total assets, and operational loss to assets ratio - all variables standardized to have mean 0 and standard error 1), the number of years since insurer's most recent exam, pre-term revolver status of the commissioner, log budget of the insurance department in year t and state s, log number of financial analysts available to the insurance department in state s, year t1. In column (2)/(3)/(4)/(5) revolvers can respond differently to levels and changes in RBC/assets/leverage/operational loss. In column (6) revolvers can respond differently to all financial risk variables. All regressions include state fixed effects and year fixed effects and standard errors are clustered at the state level. Note: *p<0.1; **p<0.05; ***p<0.01

	is exam year$_{i,t}$					
	(1)	(2)	(3)	(4)	(5)	(6)
$I^{POST}_{s,t}$	-0.015^{**}	-0.014^{**}	-0.015^{**}	-0.015^{**}	-0.015^{**}	-0.015^{**}
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
$I^{POST}_{s,t} \times$ RBC$_{i,t-1}$		0.007^{**}				0.009^{**}
		(0.003)				(0.003)
$I^{POST}_{s,t} \times$ ΔRBC$_{i,t}$		-0.004				-0.004
		(0.003)				(0.003)
$I^{POST}_{s,t} \times$ total assets$_{i,t-1}$			0.004			0.003
			(0.003)			(0.003)
$I^{POST}_{s,t} \times$ Δtotal assets$_{i,t}$			-0.004			-0.004
			(0.003)			(0.004)
$I^{POST}_{s,t} \times$ leverage ratio$_{i,t-1}$				0.001		0.004
				(0.003)		(0.004)
$I^{POST}_{s,t} \times$ Δleverage ratio$_{i,t}$				0.0003		0.0004
				(0.006)		(0.006)
$I^{POST}_{s,t} \times$ operational loss/assets$_{i,t}$					-0.003	-0.001
					(0.003)	(0.004)
E[LHS]	0.11	0.11	0.11	0.11	0.11	0.11
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$	$s+t$	$s+t$
Observations	36,773	36,773	36,773	36,773	36,773	36,773
Adjusted R^2	0.160	0.160	0.160	0.160	0.160	0.160

Table 4: Exam outcomes by revolver status

The table shows results from regression (4). The insurer-year panel is limited to those observations i, t, in which insurer i was examined in year t. Any Financial Restatements$_{i,t}$ (Any Recommendation) is an indicator variable that is 1 if insurer i is examined in year t and as a result it has to issue an edit of its financial statements (comply with any recommendations made by regulators). In columns (1) and (3) the panel includes all exams, and in columns (2) and (4) only early exams, i.e., exams happening no more than 4 years since the most recent exam. Control variables for all regressions include insurers' financial risk variables (lagged level and change in RBC and leverage ratio, total assets, and operational loss to assets ratio - all variables standardized to have mean 0 and standard error 1), number of years since insurer's most recent exam, pre-term revolver status of the commissioner, the number of domestic insurers in state s, year t, log budget ofthe insurance department in year t and state s, log number of financial analysts available to the insurance department in state s, year t1). All regressions include state fixed effects and year fixed effects and standard errors are clustered at the state level.

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

	Any Financial Restatements$_{i,t}$		Any Recommendations$_{i,t}$	
	(1)	(2)	(3)	(4)
$I_{s,t}^{POST}$	-0.024^{*}	-0.041^{*}	-0.017	-0.043^{*}
	(0.013)	(0.022)	(0.020)	(0.023)
E[LHS]	0.34	0.35	0.65	0.7
Exam Sample	all	early	all	early
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$
Observations	7,001	3,858	7,001	3,858
Adjusted R^2	0.071	0.082	0.101	0.114

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

The table summarizes results from regressing the number of regulatory actions based on solvency concerns on whether the commissioner in office is a revolver, as shown in regression (1). The dependent variable is the number of certificates suspended in columns (1) and (2), number of certificates permanently revoked in columns (3) and (4), and number of delinquency orders in columns (5) and (6), for state s in year t. $I_{s,t}^{POST}$ 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. Regressions (2), (4) and (6) control for number of financial exams conducted in state s and year t, whether the commissioner worked for the insurance industry at any point prior to his commissioner term, and resources available to the insurance department (the number of domestic insurers in state s, year t, log of the budget that the state insurance department had in year s and state t, and 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.

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

	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^{**}	-0.874^{*}	-0.045	0.001	-0.424^{**}	-0.401^{**}
	(0.475)	(0.484)	(0.421)	(0.428)	(0.197)	(0.178)
E[LHS]	3.5	3.5	1.95	1.95	0.68	0.68
Controls	No	Yes	No	Yes	No	Yes
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$	$s+t$	$s+t$
Observations	830	825	830	825	682	682
Adjusted R^2	0.539	0.545	0.295	0.293	0.183	0.180

Table 6: Timing of exams near the end of revolvers' terms

The table shows results from regression (5) in columns (1-2) and from (6) in columns (3-5). The panel used in columns (1) and (2) is all state-year observations and in columns (3)/(4)/(5) - those insurer-year observations, which are no more than 2/3/4 years after insurer i's most recent exam. The dependent in columns (1)/(2) is the absolute/log + 1 of number of domestic financial exams in state s and year t, and in columns (3) to (5) - an indicator *is exam year*$_{i,t}$, which is 1 if insurer i was examined in year t. $I_{s,t}^{POST}$ is an indicator 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. Indicators $I_{s,t}^{T}/I_{s,t}^{T-1}$ equal 1 if year t is the last/penultimate for the commissioner in state s and year t. All regressions control for pre-term employment status, log budget of the insurance department in year t and state s, log number of financial analysts available to the insurance department in state s, year $t-1$, and fixed effects for the election cycle (0, 1, 2 or 3 years to the next state election). Regressions (1) and (2) control for also the absolute/log number of domestic insurers in state s, year t, and regressions (3) to (5) control also for number of years since the last exam of insurer i in year t, as well as risk variables (lagged level and change in RBC and leverage ratio, total assets, and operational loss to assets ratio - all variables standardized to have mean 0 and standard error 1), and the interaction of revolver status with the risk variables. All regressions include state fixed effects and year fixed effects and standard errors are clustered at the state level.

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

	n exams$_{s,t}$	log(n exams$_{s,t}+1$)	is exam year$_{i,t}$		
	(1)	(2)	(3)	(4)	(5)
$I_{s,t}^{POST} \times I_{T-1}$	4.267**	0.125**	0.013*	0.021*	0.021**
	(1.788)	(0.060)	(0.007)	(0.011)	(0.009)
$I_{s,t}^{POST} \times I_{T}$	0.542	0.008	0.001	0.009	0.013
	(2.583)	(0.080)	(0.011)	(0.012)	(0.014)
$I_{s,t}^{POST}$	-4.222^{**}	-0.113^{**}	-0.011^{**}	-0.023^{**}	-0.027^{***}
	(1.872)	(0.055)	(0.005)	(0.010)	(0.010)
I_{T-1}	-1.896	-0.015	-0.008	-0.002	0.002
	(1.669)	(0.046)	(0.006)	(0.009)	(0.009)
I_{T}	0.353	0.028	0.002	0.012	0.011
	(1.852)	(0.051)	(0.008)	(0.008)	(0.009)
E[LHS]	29.64	2.99	0.03	0.08	0.11
Years since last?			$\leq$ 2y	$\leq$ 3y	$\leq$ 4y
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$	$s+t$
Observations	815	815	21,855	30,544	36,521
Adjusted R^2	0.863	0.869	0.032	0.120	0.161

Table 7: Change in Best's FSR after an exam results in financial restatement

The table shows the results of regression (7), or how an exam resulting in a financial restatement affects the an insurer's Best's FSR. The variable of interest is new fin. restatement$_{i,t}$, which is 1 if insurer i was examined between years $t-1$ and t, and the exam resulted in financial restatement. The outcome in columns (1) and (2) is ΔDefault Probability$_{i,t}$ which is the change in the implied default probability in the rating of insurer i between years $t-1$ and t. The outcome in columns (3) and (4) is $I^{remove}_{i,t}$ if between years $t-1$ and t, insurer i chose to not be rated further by A.M. Best. Columns (1) and (3) includes state fixed effects and year fixed effects. Columns (2) and (4) include state $\times$ year fixed effects. I also control for whether the insurer was examined in the year, the number of years since last exam, financial risk variables (both level at year $t-1$ and change between years $t-1$ and t in insurer i's RBC ratios, total assets, and leverage ratio, as well as operational loss to assets ratio in year t. Note all risk variables are transformed to have standard deviation 1 and mean 0 across the period). All standard errors are clustered at the state level.

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

	Δ Default Probability$_{i,t}$%		$I^{remove}_{i,t}$	
	(1)	(2)	(3)	(4)
new fin. restatement$_{i,t}$	0.072*	0.079*	0.015*	0.015*
	(0.044)	(0.044)	(0.008)	(0.008)
E[LHS]	0.0239	0.0239	0.0236	0.0236
Fixed Effects	$s+t$	$s \times t$	$s+t$	$s \times t$
Observations	5,658	5,643	6,384	6,349
Adjusted R^2	0.026	0.021	0.032	0.021

Table 8: Changes in Best's FSR after restatements: more pronounced among low-rated insurers

The table shows the results of regression (7) re-run on different sub-samples of the panel. The outcome is ΔDefault Probability$_{i,t}$ which is the change in the implied default probability in the rating of insurer i between years $t-1$ and t. The variable of interest is new fin. restatement$_{i,t}$, which is 1 if insurer i was examined between years $t-1$ and t, and the exam forced the insurer to restate its financial filings. 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++. All regressions control for state $\times$ year fixed effects. I also control for whether the insurer was examined in the year, the number of years since last exam, financial risk variables (both level at year $t-1$ and change between years $t-1$ and t in insurer i's RBC ratios, total assets, and leverage ratio, as well as operational loss to assets ratio in year t. Note all risk variables are transformed to have standard deviation 1 and mean 0 across the period). All standard errors are clustered at the state level.

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

	Δ Default Probability$_{i,t}$%				
	(1)	(2)	(3)	(4)	(5)
new fin. restatement$_{i,t}$	0.079*	0.080*	0.089*	0.152*	0.338*
	(0.044)	(0.045)	(0.050)	(0.079)	(0.195)
E[LHS]	0.024	0.024	0.026	0.041	0.092
Rating Sample	full	<A++	<A+	<A	<A-
Fixed Effects	$s \times t$	$s \times t$	$s \times t$	$s \times t$	$s \times t$
Observations	5,643	5,530	5,134	3,586	1,742
Adjusted R^2	0.021	0.019	0.014	0.004	0.073

Table 9: Bond misreporting in states lead by revolvers

The table shows estimates for the association between revolver supervision and misreporting revealed in the financial crisis. The estimates are from regression (8) and focus on the correlation between the average number of years with revolver in charge in state s in the period 2008$-\tau$ to 2008 and the average level of bond misreporting during the 2008 financial crisis. The variable of interest is the average 2008 state level of bond misreporting, and it comes from Sen and Sharma (2020). In column (1) the period is 2000 to 2008, and in column (2), the period is 2005 to 2008, to account for the periodical nature of the monitoring. I control for supervisory variables (number of financial analysts and examiners, number of discretionary exams, and budget per domestic insurer in a state and for the average states solvency (mean RBC ratio and log assets of all life insurers domiciled in each state).

	misreporting$_{s,2008}$		
	(1)	(2)	(3)
$I^{POST}_{s,t-i:\,t}$	73.055**	82.614***	59.546*
	(29.476)	(28.464)	(32.572)
E[LHS]	98.81	98.81	98.81
Period $t-i$ to t	2000 to 2008	2005 to 2008	2007 to 2008
Controls	Yes	Yes	Yes
Observations	40	39	38
Adjusted R^2	0.258	0.314	0.183

Table 10: DiD around revolving door law changes

I show the results from regression (9) in columns (1) and (2), and from regression (10) in columns (3) to (7). The outcome variable in columns (1)/(2) is the absolute/log number of exams conducted in state s in year t. The outcome variable in columns (3) to (7) is Any Financial Restatements$_{i,t}$, an indicator that is 1 if insurer i is examined in year t and as a result it has to issue an edit of its financial statements. $I^{POST}_{s,t}$ 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. The shock indicator $I^{\Delta LAW}_{s,t}$ equals 1/-1 if the revolving door cooling down laws have gotten stricter/more lenient in state s in the years before $t-1$. The panel in columns (1) and (2) is at the state-year level. The panel in columns (3) to (7) is limited to insurer-level exams: these insurer-year observation in which insurer i was examined in year t. Furthermore, in column (3), the panel includes all exams; in columns (4)/(5)/(6) the panel is limited to early exams, which are no more than 2/3/4 years since the latest exam; in column (7) the exams are limited to large insurers (their assets are at least as large as the size of the smallest potential future employer). Control variables include: pre-term revolver realized status, number or log number of domestic insurers 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 $t-1$. Furthermore, in columns (3) to (7) I control for insurer-year risk variables (lagged level and change in RBC ratio, leverage ratio, total assets, as well as operational loss to assets ratio - all these variables standardized to have mean 0 and standard error 1). All regressions include state and year fixed effects and standard errors are clustered at the state level.

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

	n exams$_{s,t}$	log(n exams$_{s,t}$+1)	Any Financial Restatements$_{i,t}$				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$I^{POST}_{s,t} \times I^{\Delta LAW}_{s,t}$	8.906***	0.221***	0.045	0.188*	0.116*	0.047	0.056*
	(2.357)	(0.055)	(0.032)	(0.100)	(0.061)	(0.052)	(0.032)
$I^{POST}_{s,t}$	-3.874^{**}	-0.105^{**}	-0.031^{**}	-0.200^{***}	-0.125^{**}	-0.049^{**}	-0.041^{**}
	(1.707)	(0.045)	(0.015)	(0.073)	(0.048)	(0.023)	(0.016)
$I^{\Delta LAW}_{s,t}$	-5.288^{**}	-0.172^{***}	-0.059	0.433***	0.084**	-0.100	-0.080
	(2.600)	(0.055)	(0.043)	(0.120)	(0.033)	(0.075)	(0.048)
E[LHS]	29.64	2.99	0.34	0.38	0.36	0.35	0.33
Exam Sample	all	all	all	≤2y	≤3y	≤4y	all
Insurer Sample	all	all	all	all	all	all	large
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$	$s+t$	$s+t$	$s+t$
Observations	829	829	7,001	503	2,088	3,858	6,411
Adjusted R^2	0.865	0.869	0.070	0.017	0.076	0.082	0.070

- Online 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.

Second, I look for professional networks profiles and record all listed jobs. 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 insurers for appointing a former commissioner, and journalistic articles.

Finally, I classify each job in one of five general categories: insurance industry, government, consulting/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).[38] On average, I find 3.8 jobs for commissioners before they start office and 2.7 after they leave.

[38] 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 chosen for a main proxy of strictness because it was a more consistent measure across states of commissioner effort. Specifically, it is possible that some departments lack the resources to examine insurers not domiciled in the state. However, domiciled insurers have to be regularly examined. To ensure the robustness of these choices, 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.1.

Table B.1: Number of total exams by revolver status

I re-estimate Table 2 using all (not just domestic) exams as an outcome variable.
Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

	n total exams$_{s,t}$		log(1 + n total exams$_{s,t}$)	
	(1)	(2)	(3)	(4)
$I_{s,t}^{POST}$	-3.676^{**}	-2.994^{*}	-0.104^{**}	-0.082^{**}
	(1.773)	(1.586)	(0.047)	(0.040)
E[LHS]	29.78	29.78	3	3
Controls	No	Yes	No	Yes
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$
Observations	834	829	834	829
Adjusted R^2	0.860	0.864	0.863	0.865

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 (and also the average exam took around 8 months). I show that the results of the baseline regression shown in Table 2 are robust to using all commissioners. Results are in Table B.2.

Table B.2: Number of exams by revolver status: Include commissioners with term shorter than a year.

I re-estimate Table 2 by modifying the variable of interest to include all commissioners, not only the commissioners with term-length of at least a year.

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

	n exams$_{s,t}$		log(n exams$_{s,t}$ + 1)	
	(1)	(2)	(3)	(4)
$I_{s,t}^{POST}$ (incl. short terms)	-3.573^{**}	-2.971^{*}	-0.099^{**}	-0.083^{**}
	(1.703)	(1.517)	(0.045)	(0.038)
E[LHS]	29.92	29.92	3	3
Controls	No	Yes	No	Yes
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$
Observations	908	901	908	901
Adjusted R^2	0.861	0.866	0.867	0.871

B.3. Robustness to excluding each state

In this appendix I show that the baseline results are not driven by any particular state. I rerun regression 1 for both number of financial domestic exams and log number of financial exams, and I exclude states one at a time. The coefficient on revolver and its corresponding t-value are plotted at Figure B.1 and Table B.3. The results imply that the finding that revolvers perform fewer exams per year is not driven by any single state. 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 $I^{POST}_{s,t}$ in Equation (1): excluding one state at a time

The figure summarizes the results from regressing a measure of exams conducted in state s and year t on whether the commissioner in office is a revolver, as shown in Equation (1), re-estimated by excluding each state. The dependent variable $Y_{s,t}$ is the number of domestic financial exams in state s and year t in the left panel, and log of the number of exams (plus one) in the right panel. I plot the regression coefficient estimates on each state subset on the x axis, and the corresponding t-value on the y-axis for $I^{POST}_{s,t}$, 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 regressions control for whether the commissioner worked for the insurance industry at any point prior to his commissioner term, the number of domestic firms in state s, year t, log of the budget that the state insurance department had in year s and state t, and 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. Standard errors are clustered at the state level.

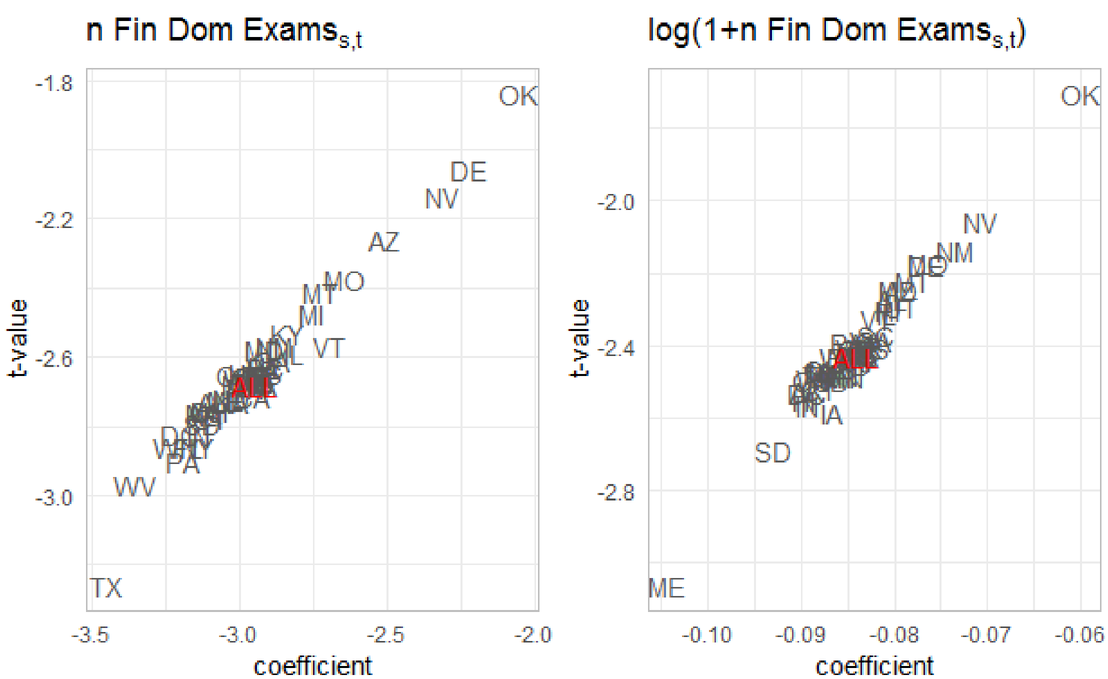


Table B.3: Coefficients of regressing number of exams on post-term revolver status: removing one state at a time

The table 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, as shown in regression 1, re-estimated by excluding each state. The dependent variable $Y_{s,t}$ is the number of domestic financial exams in state s and year t in columns (1) and (2), and log of the number of exams (plus one) in columns (3) and (4). I show regression coefficient estimates on each state subset, and the t-value on $I_{s,t}^{POST}$, 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 regressions control for whether the commissioner worked for the insurance industry at any point prior to his commissioner term, the number of domestic firms in state s, year t, log of the budget that the state insurance department had in year s and state t, and 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. Standard errors are clustered at the state level.

	n $\text{exams}_{s,t}$		$\log(\text{n exams}_{s,t}+1)$			n $\text{exams}_{s,t}$		$\log(\text{n exams}_{s,t}+1)$	
state	$I_{s,t}^{POST}$	t-value	$I_{s,t}^{POST}$	t-value	state	$I_{s,t}^{POST}$	t-value	$I_{s,t}^{POST}$	t-value
ALL	**-3.06**	**-2.77**	**-0.09**	**-2.52**	MS	-3.06	-2.76	-0.09	-2.51
AK	-3.20	-2.82	-0.09	-2.61	MT	-2.86	-2.52	-0.08	-2.31
AL	-3.24	-2.86	-0.08	-2.36	NC	-3.06	-2.77	-0.09	-2.51
AR	-3.05	-2.74	-0.09	-2.49	ND	-3.14	-2.81	-0.09	-2.58
AZ	-2.65	-2.38	-0.08	-2.37	NE	-3.06	-2.77	-0.09	-2.51
CA	-3.07	-2.81	-0.09	-2.52	NH	-3.11	-2.77	-0.09	-2.57
CO	-3.12	-2.74	-0.09	-2.58	NJ	-3.21	-2.84	-0.09	-2.54
CT	-3.21	-2.87	-0.09	-2.61	NM	-2.99	-2.66	-0.08	-2.22
DC	-3.30	-2.90	-0.09	-2.59	NV	-2.39	-2.21	-0.07	-2.12
DE	-2.25	-2.08	-0.08	-2.21	NY	-3.26	-2.95	-0.09	-2.55
FL	-3.23	-2.91	-0.09	-2.50	OH	-3.21	-2.85	-0.09	-2.55
GA	-3.00	-2.71	-0.09	-2.47	OK	-2.19	-1.95	-0.06	-1.80
HI	-3.11	-2.74	-0.09	-2.64	OR	-3.06	-2.75	-0.09	-2.49
IA	-3.12	-2.83	-0.09	-2.67	PA	-3.32	-3.01	-0.09	-2.49
ID	-3.05	-2.74	-0.09	-2.54	RI	-3.06	-2.76	-0.09	-2.53
IL	-2.93	-2.69	-0.08	-2.40	SC	-3.04	-2.74	-0.09	-2.48
IN	-3.25	-2.93	-0.09	-2.65	SD	-3.23	-2.88	-0.10	-2.76
KS	-3.03	-2.74	-0.09	-2.50	TN	-3.08	-2.77	-0.09	-2.57
KY	-2.99	-2.67	-0.09	-2.54	TX	-3.54	-3.33	-0.09	-2.54
LA	-3.05	-2.77	-0.09	-2.52	UT	-3.02	-2.72	-0.08	-2.39
MA	-3.23	-2.85	-0.09	-2.55	VA	-3.03	-2.72	-0.09	-2.47
MD	-3.03	-2.67	-0.08	-2.33	VT	-2.78	-2.65	-0.09	-2.42
ME	-3.15	-2.82	-0.11	-3.15	WA	-3.06	-2.77	-0.09	-2.52
MI	-2.89	-2.58	-0.08	-2.40	WI	-3.34	-2.95	-0.09	-2.57
MN	-3.09	-2.78	-0.09	-2.58	WV	-3.47	-3.06	-0.09	-2.51
MO	-2.76	-2.48	-0.08	-2.26	WY	-3.09	-2.75	-0.09	-2.49

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 exam) and the combined micro data from annual reports and FOIAs, 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.4. Both the figure and the regressions show that micro data is close to the aggregate data, but somewhat lower. This lower estimate is partly because the micro data from annual reports is available only for Life, Property and Casualty and Health insurers (excluding some of the smaller lines, such as Title and Fraternal Insurance).

Figure C.2: Number of exams reported vs collected for individual outcomes

In the left panel of the figure, I plot n $\text{exams}^{agg}_{s,t}$ on the x-axis and n $\text{exams}^{micro}_{s,t}$ on the y-axis. In the right panel, I plot the log transformation of the same variables. Here, n $\text{exams}^{micro}_{s,t}$ is the number of exams collected through annual reports and FOIAs, and n $\text{exams}^{agg}_{s,t}$ is the number of exams reported by NAIC IDRR, used in Table 2.

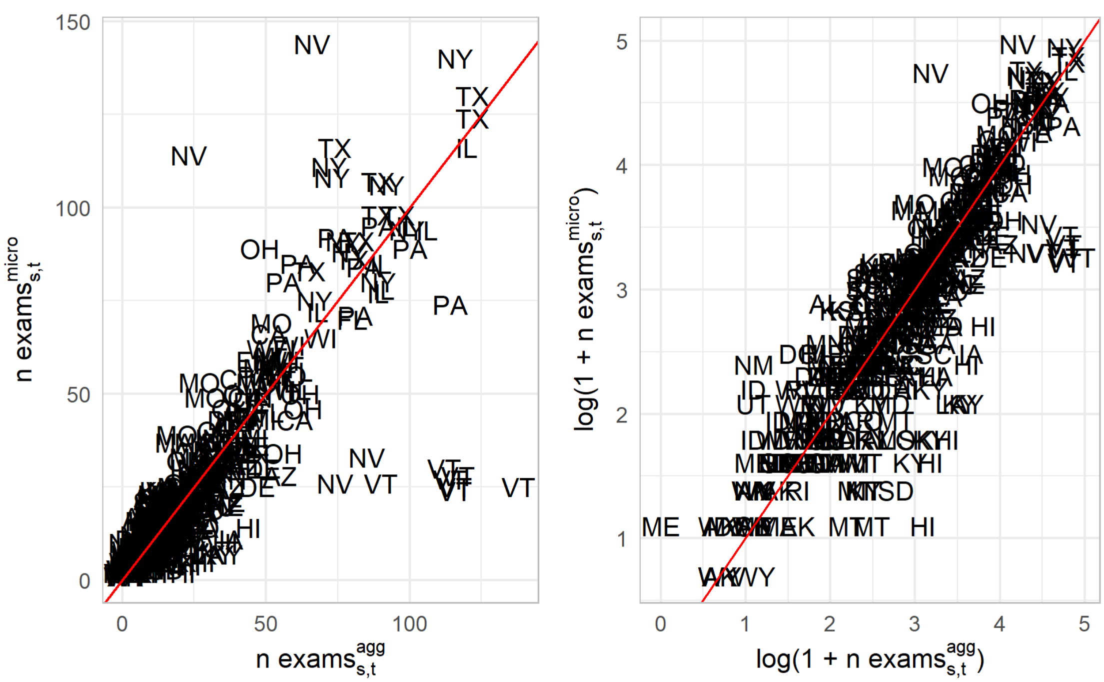


Table C.4: Number of exams reported vs collected for individual outcomes

In column (1), I regress n $\text{exams}_{s,t}^{micro}$ on n $\text{exams}_{s,t}^{agg}$. Here, n $\text{exams}_{s,t}^{micro}$ is the number of exams collected through annual reports and FOIAs, and n $\text{exams}_{s,t}^{agg}$ is the number of exams reported by NAIC IDRR, used in Table 2. In column (2) I repeat the regression but log both n $\text{exams}_{s,t}^{micro}$ and n $\text{exams}_{s,t}^{agg}$.

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

	n $\text{exams}_{s,t}^{micro}$	log(1 + n $\text{exams}_{s,t}^{micro}$)
	(1)	(2)
n $\text{exams}_{s,t}^{agg}$	0.813***	
	(0.026)	
log(1 + n $\text{exams}_{s,t}^{agg}$)		0.856***
		(0.022)
Constant	3.384***	0.359***
	(0.980)	(0.067)
Observations	457	457
R^2	0.678	0.773
Adjusted R^2	0.678	0.772

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

In the main text, I show that exams conducted by revolvers result in fewer financial restatements for the insurer. I also show the result gets stronger for early exams, defined as no more than 4 years after the most recent exam. In Table C.5, columns (1) to (3), I show this result is robust to defining early exams as an exam within 2 or 3 years since the most recent exam. The fewer the years since latest exams, the stronger the result gets. Specifically, When we define an early exam as one within 2/3/4 years since the most recent exam, the probability for a restatement is 16.6%/9.6%/0.41% lower if a revolver oversees it. This decrease is correspondingly 43%/26%/11.7% of the unconditional likelihood of restatement.

In Table C.5, columns (4) to (6), 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 for exams within 3 years of the most recent exam.

In the main text, I show that revolvers are less likely to conduct early (less than 4 years since latest) exams, and exams when risk observables deteriorate (see Table 3). I test the result's robustness to definition of early exam in Table C.6, where in columns (1)/(2)/(3) I repeat the analysis from column (6) of Table 3 and I use panels of those insurer-years which are no more than 2/3/4 years since the last exam. All else equal, the β coefficient (likelihood for an early exam of a revolver) preserves its sign for 2 and 3 years, but loses significance, likely since the probability of exams within 2 or 3 years of the latest is fairly low. In terms of risk variable sensitivity, post-term revolvers are less sensitive to switches in the level of RBC, or regulatory capital ratio. However, for the 2 years and less, this difference loses significance.

Table C.5: Exam outcomes: Robustness to definition of early exams

I re-estimate the outcomes of early exams in Table 4, (columns (2) and (4)), by defining an early exam as an exam 2/3/4 years since the most recent exam in columns (1) and (4)/(2) and (5)/(3) and (6).

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

	Any Financial Restatements$_{i,t}$			Any Recommendations$_{i,t}$		
	(1)	(2)	(3)	(4)	(5)	(6)
$I_{s,t}^{POST}$	-0.166^{**}	-0.096^{**}	-0.041^{*}	0.009	-0.045	-0.043^{*}
	(0.071)	(0.037)	(0.022)	(0.060)	(0.042)	(0.023)
E[LHS]	0.38	0.36	0.35	0.72	0.7	0.7
Exam Sample	$\leq$ 2y	$\leq$ 3y	$\leq$ 4y	$\leq$ 2y	$\leq$ 3y	$\leq$ 4y
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$	$s+t$	$s+t$
Observations	503	2,088	3,858	503	2,088	3,858
Adjusted R^2	0.018	0.076	0.082	0.069	0.107	0.114

Table C.6: Predicting early exams: robustness to definition of early exam

I re-estimate the probability of early exams in Table 3, column (6), by defining an early exam as an exam 2/3/4 years since the most recent exam in columns (1)/(2)/(3).

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

	is exam year$_{i,t}$		
	(1)	(2)	(3)
$I_{s,t}^{POST}$	-0.005	-0.012	-0.014^{**}
	(0.004)	(0.007)	(0.007)
$I_{s,t}^{POST} \times$ RBC$_{i,t-1}$	0.0003	0.007^{**}	0.009^{***}
	(0.004)	(0.003)	(0.003)
$I_{s,t}^{POST} \times$ ΔRBC$_{i,t}$	-0.006^{*}	-0.003	-0.004
	(0.003)	(0.003)	(0.003)
$I_{s,t}^{POST} \times$ total assets$_{i,t-1}$	0.001	0.003	0.003
	(0.001)	(0.003)	(0.003)
$I_{s,t}^{POST} \times$ Δtotal assets$_{i,t}$	-0.001	-0.003	-0.004
	(0.005)	(0.005)	(0.004)
$I_{s,t}^{POST} \times$ leverage ratio$_{i,t-1}$	0.004	0.004	0.004
	(0.003)	(0.004)	(0.004)
$I_{s,t}^{POST} \times$ Δleverage ratio$_{i,t}$	-0.001	-0.003	0.001
	(0.001)	(0.003)	(0.005)
$I_{s,t}^{POST} \times$ operational loss/assets$_{i,t}$	-0.003	-0.005^{*}	-0.002
	(0.002)	(0.003)	(0.004)
E[LHS]	0.03	0.08	0.12
Exam Sample	$\leq$ 2y	$\leq$ 3y	$\leq$ 4y
Fixed Effects	$s+t$	$s+t$	$s+t$
Observations	22,030	30,784	36,811
Adjusted R^2	0.031	0.113	0.155

C.3. Limiting the sample to insurers of similar size to future employers

Tabakovic and Wollmann (2018) show that patent officer revolvers are extra lenient with potential future employers. I check if revolvers are even less strict with potential future employers. At Figure C.3, I plot the risk characteristics of insurers and how do employers of commissioners fit within the distribution. We see that employers, tend to be larger, and to fit within the middle of the distribution of leverage and RBC ratio.

To test if revolvers act more lenient towards likelier employers, I need to make some assumptions to identify these insurers. I assume that revolvers consider as potential employers insurers whose risk variables lie within the range of actual employers. I limit the sample to these potential employers (e.g., focus on mostly larger insurers).

I rerun regressions (4) and (5) on this panel of larger insurers. Similarly to Tabakovic and Wollmann (2018), I find that revolvers are less strict towards potential employers. The results on exam outcomes among large insurers are at Table C.7 and they show that large insurers' exams by a revolver are even less likely to result in a restatement and to have any recommendation (if the exam is early). The results on early exams among large insurers are Table C.8 and they show that revolvers are even less likely to examine a potential employer early.

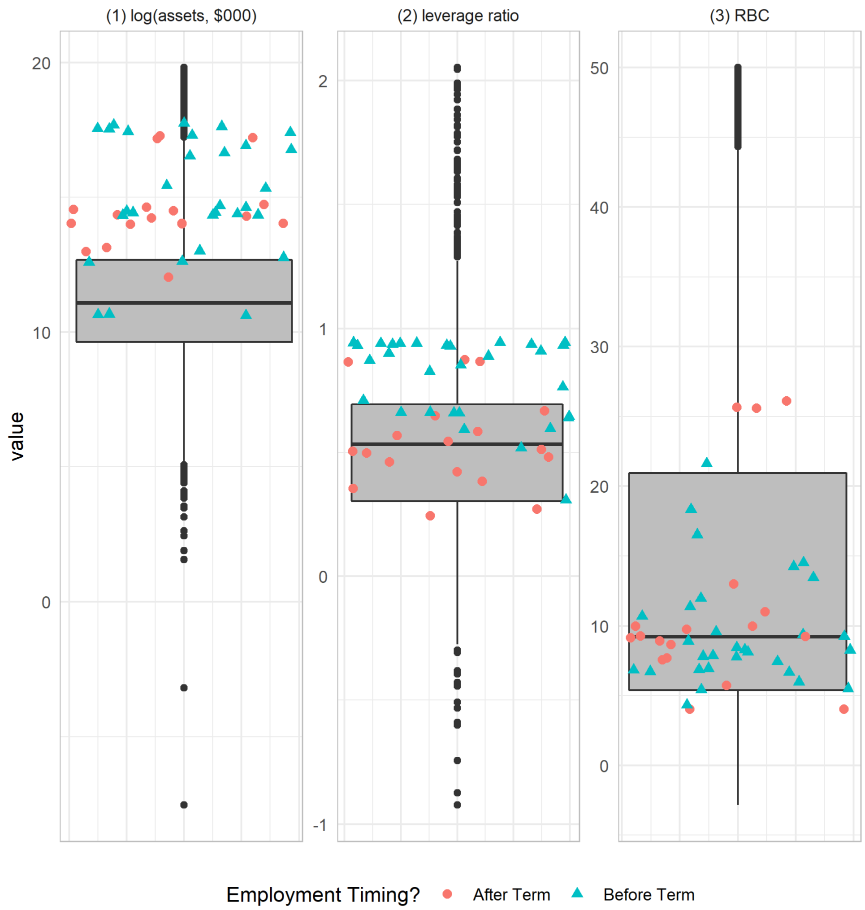


Figure C.3: Distribution of all insurers' risk variables and the risk variables for insurers which employed commissioners

The boxplot show the distribution of the level of regulatory capital, leverage ratio, and log total assets in $000 for all insurers, 2000-2018. The dots show how these rsik variables looked for insurers which employed the insurers before their term as a commissioner (blue triangles) or after their term as a commissioner (red circles).

Table C.7: Exam outcomes among large insurers

I re-estimate the regressions in Table 4 over a sample of insurers at least as large as the smallest potential employer (i.e., potential employers)

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

	Any Financial Restatements$_{i,t}$		Any Recommendations$_{i,t}$	
	(1)	(2)	(3)	(4)
$I_{s,t}^{POST}$	-0.032^{**}	-0.059^{***}	-0.019	-0.054^{**}
	(0.015)	(0.022)	(0.024)	(0.023)
E[LHS]	0.33	0.33	0.65	0.69
Exam Sample	all	early	all	early
Insurer Sample	large	large	large	large
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$
Observations	6,411	3,492	6,411	3,492
Adjusted R^2	0.070	0.082	0.103	0.116

Table C.8: Early exams among large insurers

I re-estimate the regression from column (6) in Table 3 over a sample of insurers at least as large as the smallest potential employer (i.e., potential employers), and by limiting the sample to year-insurer observations 2/3/4 years since the most recent exam in columns (1), (2) and (3).

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

	is exam year$_{i,t}$		
	(1)	(2)	(3)
$I^{POST}_{s,t}$	-0.006^{*}	-0.013^{*}	-0.016^{**}
	(0.004)	(0.007)	(0.007)
$I^{POST}_{s,t}\times$ RBC$_{i,t-1}$	-0.002	0.008^{**}	0.012^{***}
	(0.004)	(0.004)	(0.004)
$I^{POST}_{s,t}\times$ ΔRBC$_{i,t}$	-0.005	-0.001	-0.001
	(0.003)	(0.004)	(0.004)
$I^{POST}_{s,t}\times$ total assets$_{i,t-1}$	0.002^{*}	0.004	0.004
	(0.001)	(0.003)	(0.003)
$I^{POST}_{s,t}\times$ Δtotal assets$_{i,t}$	-0.005	-0.007	-0.009^{*}
	(0.007)	(0.006)	(0.005)
$I^{POST}_{s,t}\times$ leverage ratio$_{i,t-1}$	0.002	0.002	0.003
	(0.003)	(0.005)	(0.004)
$I^{POST}_{s,t}\times$ Δleverage ratio$_{i,t}$	-0.0005	-0.004	-0.005
	(0.002)	(0.003)	(0.005)
$I^{POST}_{s,t}\times$ operational loss/assets$_{i,t}$	-0.002	-0.007	-0.003
	(0.007)	(0.010)	(0.009)
E[LHS]	0.03	0.07	0.11
Exam Sample	$\leq$ 2y	$\leq$ 3y	$\leq$ 4y
Insurer Sample	large	large	large
Fixed Effects	$s+t$	$s+t$	$s+t$
Observations	19,921	27,947	33,491
Adjusted R^2	0.032	0.114	0.157

C.4. Difference in exam outcomes in the last two years of term

I modify regression (4) to test if exam outcomes change in the last two years of commissioners' terms:

$$\text{Any Financial Restatement}_{i,t} = \beta \mathrm{I}^{\mathrm{POST}}_{s,t} + \beta_T \mathrm{I}^{T}_{s,t} + \beta_{T-1} \mathrm{I}^{T-1}_{s,t} + \beta_r \text{Risk Vars}_{i,t} + \gamma_x \mathrm{X}_{i,s,t} + \alpha_s + \alpha_t + \epsilon_{i,t}.$$

Results are shown at Table C.9. We see that while β is negative, indicating that revolvers are less likely to request a restatement, β_T and β_{T-1} are not significant. This implies that revolvers don't change their exam outcome strictness near the end of their term.

Table C.9: Are financial restatements more likely in last two years of commissioners' terms?

In columns (1) and (2) I include in the sample all exams, while in columns (3) and (4) I limit it to early exams (ones within 4 years of the most recent exam). Furthermore, in columns (1) and (3) I include all insurers, while in columns (2) and (4) I limit the sample to large insurers, as described in Appendix C.3. Note: *p<0.1; **p<0.05; ***p<0.01

	Any Financial Restatements $_{i,t}$			
	(1)	(2)	(3)	(4)
$I^{POST}_{s,t} \times I_{T-1}$	0.026	0.029	0.048	0.041
	(0.041)	(0.041)	(0.062)	(0.065)
$I^{POST}_{s,t} \times I_{T}$	−0.015	−0.005	−0.034	−0.030
	(0.034)	(0.039)	(0.038)	(0.039)
$I^{POST}_{s,t}$	−0.028	-0.042^{*}	−0.044	-0.063^{**}
	(0.021)	(0.024)	(0.031)	(0.031)
I_{T-1}	−0.011	−0.006	−0.022	−0.003
	(0.030)	(0.029)	(0.040)	(0.042)
I_{T}	0.030	0.032	0.067^{*}	0.072^{**}
	(0.026)	(0.029)	(0.033)	(0.032)
E[LHS]	0.34	0.32	0.35	0.33
Exam Sample	all	all	early	early
Insurer Sample	all	large	all	large
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$
Observations	6,917	6,333	3,818	3,457
Adjusted R^2	0.069	0.069	0.083	0.084

D. Robustness: focusing on immediate revolvers

In the main text I define revolver as someone who ends up working in the insurance industry at some point after their commissioner term. However, the revolving door incentives are potentially stronger when the commissioner takes this job right after the end of the insurance term (Lourie, 2019). I call the subset of commissioners whose next job (or within a year) is in insurance insurance"immediate" revolvers and repeat the analysis to show robustness to "revovler" definition.

There are two reasons I don't use this definition of revolver as a main proxy. First, identifying "'immediate" job is can be challenging from data perspective: when constructing the employment history data set, I can't find "immediate" job for 25 (out of 270) commissioners who are in charge of 96 state-years. Second, as discussed in Section 6, various cooling off laws may prevent a commissioner from moving to the insurance industry straight away.

At Figure D.4 I plot the job types which commissioners take immediately before and after leaving office. 28% of commissioners are immediate revolvers and 20% have worked for the insurance industry immediately before becoming a commissioner (the intersection of the two set is only 9.5%).

I repeat the analysis in Section 4 by substituting the original variable of interest $\mathrm{I}_{s,t}^{\mathrm{POST}}$ with $\mathrm{I}_{s,t}^{\mathrm{immed,POST}}$, an indicator which is 1 if the commissioner in state s, year t is an immediate revolver. The results for number of exams by immediate revolver status are at Table D.10. We see that the results are stronger economically and statistically. for immediate revolvers, with immediate revolvers performing 16.7%/10.5% fewer exams in the specification which uses absolute/log numbers. Next, I test if exam outcomes differ by immediate revolver status at Table D.11. The results are very similar to the original specification, with statistical significance being somewhat stronger for early exams.

Figure D.4: 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 five categories described in the Figure. Each bar represents the percent of commissioners with at least one employment event in the given job category.

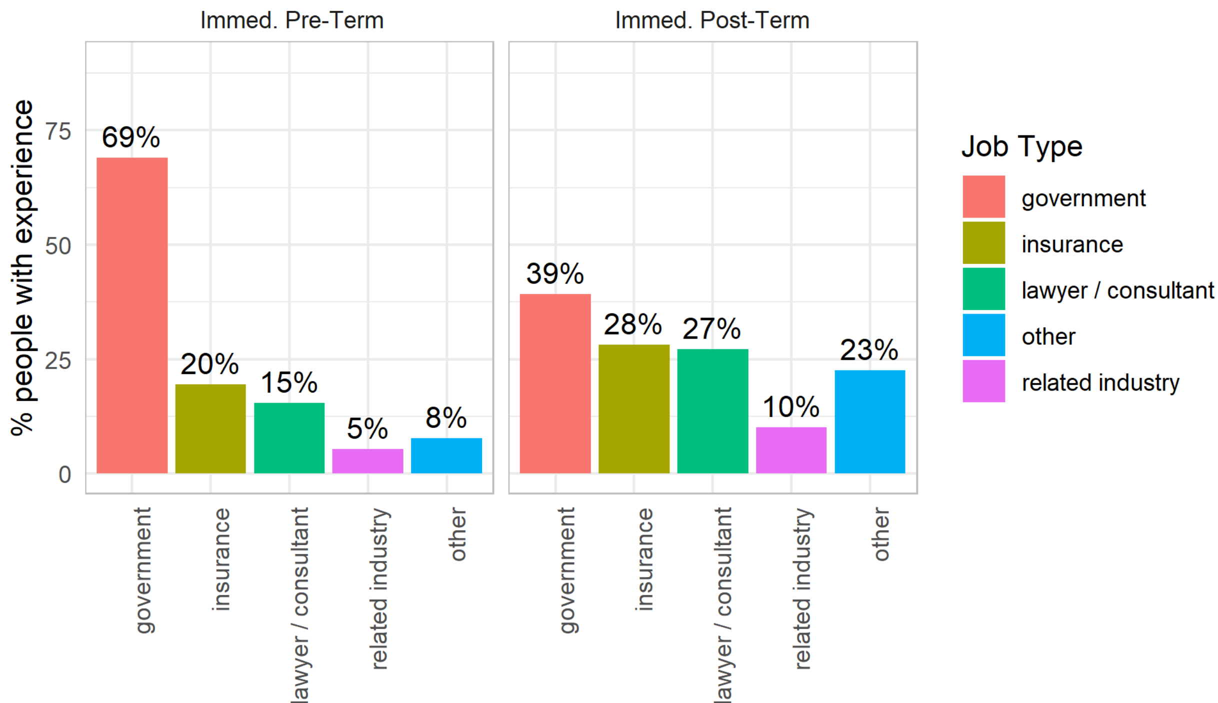


Table D.10: Number of exams by immediate revolver status

I rerun the regressions in Table 2 by changing the variable of interest $I_{s,t}^{POST,immed}$ to be immediate revolvers. Note: *p<0.1; **p<0.05; ***p<0.01

	n $\text{exams}_{s,t}$		log(n $\text{exams}_{s,t}$+1)	
	(1)	(2)	(3)	(4)
$I_{s,t}^{POST,immed}$	-6.006^{**}	-4.976^{**}	-0.127^{**}	-0.105^{**}
	(2.326)	(2.169)	(0.059)	(0.052)
E[LHS]	29.64	29.64	2.99	2.99
Controls	No	Yes	No	Yes
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$
Observations	739	737	739	737
Adjusted R^2	0.866	0.869	0.868	0.870

Table D.11: Exam outcomes by immediate revolver status

I rerun the regressions in Table 4 by changing the variable of interest $I_{s,t}^{POST,immed}$ to be immediate revolvers. Note: *p<0.1; **p<0.05; ***p<0.01

	Any Financial Restatements$_{i,t}$		Any Recommendations$_{i,t}$	
	(1)	(2)	(3)	(4)
$I_{s,t}^{POST,immed}$	-0.020^{*}	-0.039^{***}	-0.023	-0.064^{***}
	(0.010)	(0.014)	(0.017)	(0.021)
E[LHS]	0.34	0.35	0.65	0.7
Exam Sample	all	early	all	early
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$
Observations	6,744	3,662	6,744	3,662
Adjusted R^2	0.069	0.081	0.102	0.115

E. Actions against insurers: specification robustness checks

In this section I run two robustness tests for the results on actions against insurers. The main concerns are the long-tail of punitive events, and the many zeros. To deal with the former I rerun the regressions in Table 5 by changing the outcome variable to log number of negative events. The results are shown in Table E.12 and they look very similarly to the ones in Table 5. To deal with the issue of zeros, I use a Poisson, instead of an OLS specification for both number of exams and the number of punitive actions. Results are shown at Table **??**. We see that a revolver decreases (significantly) the number of exams by 6.3%, the number certificates suspended by 23% and the number of delinquency orders by 43%. The coefficient for number of certificates revoked is not significant, but it is also negative.

Table E.12: Log Regulatory actions taken against company based on solvency concern by revolver status

I rerun the regressions in Table 5 by changing the outcome variable to log number of negative events.
Note: *p<0.1; **p<0.05; ***p<0.01

	log(n certificates suspended$_{s,t}$+1)		log(n certificates revoked$_{s,t}$+1)		log(n delinquency orders$_{s,t}$+1)	
	(1)	(2)	(3)	(4)	(5)	(6)
$I^{POST}_{s,t}$	-0.175^{**}	-0.116	-0.040	-0.025	-0.123^{**}	-0.126^{**}
	(0.086)	(0.086)	(0.075)	(0.080)	(0.059)	(0.056)
E[LHS]	0.97	0.97	0.63	0.63	0.23	0.23
Control Variables	No	Yes	No	Yes	No	Yes
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$	$s+t$	$s+t$
Observations	830	825	830	825	682	682
Adjusted R^2	0.619	0.629	0.487	0.484	0.347	0.348

Table E.13: Model robustness: Poisson specification

I rerun regressions on the number of exams/actions using a Poisson regression, instead of OLS. In column (1), I re-estimate column (2) of Table 2, while in columns (2) to (4) I re-estimate columns (1) to (3) of Table 5.

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

	n exams$_{s,t}$	n certificates suspended$_{s,t}$	n certificates revoked$_{s,t}$	n delinquency orders$_{s,t}$
	(1)	(2)	(3)	(4)
$I_{s,t}^{POST}$	-0.063***	-0.263*	-0.076	-0.569**
	(0.021)	(0.151)	(0.269)	(0.234)
Fixed Effects	$s+t$	$s+t$	$s+t$	$s+t$
Observations	829	717	660	509
Squared Correlation	0.964	0.61	0.421	0.488
Pseudo R^2	0.834	0.519	0.439	0.562
BIC	4,726	3,348	2,880	1,400

F. Additional Analysis on Consequences of the Less Strict Revolver Supervision

F.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.[39]

The provided 10-year realized default probability rates are shown in F.5. Not every rating is provided with 10-year default rate, but the realized default probability decreases exponentially in the rating, as shown in Figure F.6.

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 F.6 and Table F.14. The linear fit has adjusted R^2 of 95.7%.

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

Figure F.5: 10-Year Transition and Default Rates for Best's FSR

Insurance Companies' Financial Strength Ratings (December 31, 2008 through December 31, 2018). Source: A.M. Best Rating Services, Inc. 2018 Ratings Performance Measurement Statistics for Exhibit 1 Form NRSRO.

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 F.6: 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.

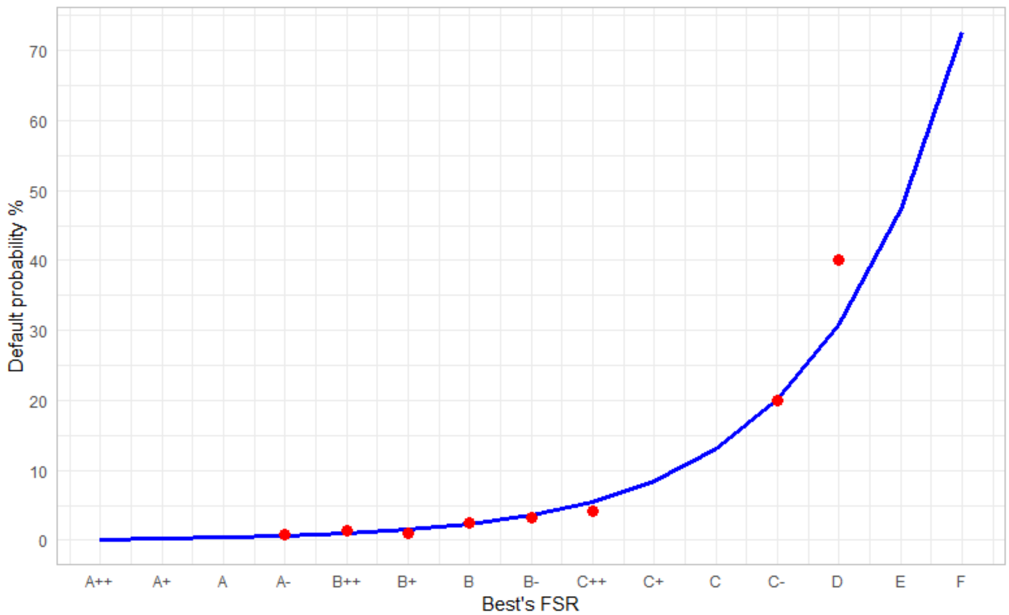


Table F.14: Implied (fitted) vs 10-year realized default probabilities

I compute the implied default probabilities 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	–

F.2. Comparing insurers with and without Best's FSR rating

In Table F.15 I compare insurers 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 insurer-year. The insurers with rating tend to be larger: mean log total assets[$000] is 11.8 for FSR and 11.09 for non-FSR insurers, which in dollars is $137 million for FSR and $65 million for the rest. However, the difference is within a standard deviation. The non-FSR insurers tend to be better capitalized on average and slightly less likely to have exams resulting in recommendations. The likelihood for an insurer in a given year to be regulated by revolver is 43% for FSR insurers, and 38% for non-FSR insurers.

Table F.15: Summary statistics on insurers with at least one Best's FSR and never rated insurers

	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
Revolver indicators						
$I_{s,t}^{POST}$	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.3. Decrease in premiums after a financial restatement

To estimate the change in premiums (gross sales) after a restatement I run the following regression:

$$\text{(F.1)} \quad \log \text{Premium}_{i,t+1} = \beta \text{Any Financial Restatements} + \Gamma_r \text{Risk Vars}_{i,t} + \alpha_s + \alpha_t + \epsilon_{i,t}$$

The regression is estimated on a panel of those insurer-year observations, in which insurer i was examined in year t. The outcome variable is log of the premiums sold to consumers by insurer i in year $t+1$ (e.g., the year after the exam). The variable of interest is Any Financial Restatements, an indicator which is 1 if the exam resulted in a restatement. As in regression (4), I control for the risk variables, state-year fixed effects, and the estimates are clustered at the state level. The interpretation of coefficient β is that for a given firm, a restatement is correlated with a β% change in premiums sold.

The results are shown in Table F.16. A restatement is associated with a 37.5% drop in premiums sold in the next year. The result is similar to insurers which are large (so, they are potential employers), and even stronger for the discretionary early exams.

Based on these results, I estimate that consumers are overpaying up to $28 billion [bn] a year due to the lenient regulation associated with the revolving door:

- Across all business, in 2018 the insurance industry sold $2,473 bn in premiums across all lines of businesses (from NAIC's IDRR).
- In a given year, 20% of insurers are due for an exam, and 34% of the exams result in restatement, so the premium which is subject to examination in a given year, the total premium affected by restatements is $168 bn (= $2,473 bn × 0.2 × 0.34) .
- Suppose the 37% of commissioners who are revolvers are substituted with (more strict) non-revolvers. Then they would have performed 9% more exams (see Table 2), and all revolver exams would have been 7.1% more likely to result in a restatement (see Table 4). In this scenario, the extra premium which would be affected by a restatement would have been $73 bn (= $166 bn × 0.37 × 1.09 × 1.07).

- I assume that the drop in sales after a restatement in Table F.16 is driven by a decrease in consumer demand: The product's quality is revealed to be lower than previously thought. After a restatement the sales drop by 37.5% (see Table F.16, column (1)), so consumers are overpaying up to \$27 bn (=\$73 bn × 0.375) a year for insurance.

Table F.16: Insurers' premiums after a financial restatement

Results from regression (F.1). In column (1) the estimates are based on a sample of all insurers and all exams. In column (2) the estimates are based on a sample of early exams (conducted less than 4 years since the insurer's last exam). In column (3) the estimates are based on a sample of large insurers (potential future employers).

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

	log (premium$_{i,t+1}$)		
	(1)	(2)	(3)
Any Fin. Restatementst$_{i,t}$	-0.375^{***}	-0.572^{***}	-0.353^{***}
	(0.107)	(0.169)	(0.102)
E[LHS]	9.57	9.76	9.86
Exam Sample	all	early	all
Insurer Sample	all	all	large
Fixed Effects	$s \times t$	$s \times t$	$s \times t$
Observations	11,913	4,282	10,817
Adjusted R^2	0.198	0.191	0.218

F.4. State Misreporting Estimates by Sen and Sharma (2020)

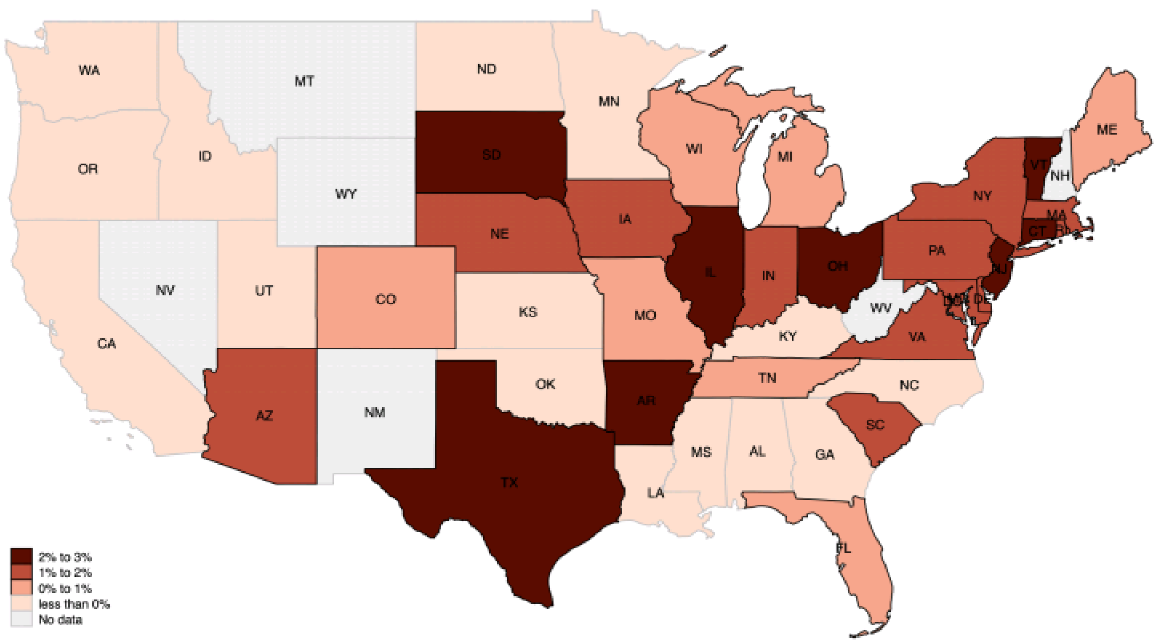
In my misreporting analysis, I use estimates of the average state-level misreporting by Sen and Sharma (2020). In Figure F.7 I display the state map they provide of state-level misreporting. They make their estimation using the following regression:

$$\overline{CS_{it}} = \gamma_s(IM_{is} \times Crisis_t) + \beta X_{it} + \alpha_i + \alpha_t + \epsilon_{it} \quad \text{(F.2)}$$

In equation F.2, $\overline{CS_{it}}$ is the cross-insurer average of the reported credit spreads for bond i at time t, $IM_{is} = 1$ if a bond is valued using internal models by at least one insurer domiciled in s in 2008. They interpret the coefficient γ_s to be $-misreporting_s$.

Figure F.7: Figure 3 from Sen and Sharma (2020): Misreporting Across U.S. States

Source of misreporting estimates: "The figure shows the extent of misreporting within each state. To quantify misreporting, we re-estimate the specification in equation [(F.2)] and compute the main coefficient of interest γ for each state separately. We split states into four groups by the estimated coefficient, e.g. 1%-2% means an estimated misreporting between 100 to 200 bps."



G. Difference-in-Difference robustness Analysis

G.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 I used are:
   (a) The *Ethics Laws* section from NAIC's *Compendium of State Laws on Insurance Topics* (archives from 1999 and 2016);
   (b) Technical reports by *Public Citizen* (a non profit) on the revolving door laws which affect the state executive branch (Public Citizen, 2005, 2011);
   (c) The 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 insurers.

By following this procedure, I find 14 laws from 12 states between 2000 and 2017 which I describe in Table G.17. In states where multiple changes took place, all changes were in the same direction, so I use the earliest year as the shock year. In Table G.18 I show that the states with law changes and states with no law changes look similarly on observable characteristics (overall state GDP, state GDP coming from the insurance sector, state population, and amount of insurance business written).

Table G.17: Revolving door state law changes

The table describes all changes in state laws concerning limits on insurance commissioners jobs after they leave office between 2000 and 2017 - the state in which the change took effect, the year the law was enacted, whether the change strengthened (↑), or weakened (↓) the existing laws, and a brief description of the laws. Most laws introduce a restriction, where none existed, but if not, the old law is described (e.g., South Dakota or West Virginia).

State	**Year**	**Direction**	**Change in restrictions on former employees**
AK	2007	↑	(1) Ban on assisting expanded; (2) Can't serve on the board of regulated firms for 1 year
GA	2007	↑	Can't register or act as lobbyist for 1 year
ME	2015	↑	Can't register as lobbyist for 1 year
MA	2009	↑	Increases penalties for appearing in front of agency as agent or attorney for 1 year
NJ	2004	↑	Can't register as "government affairs agent" for 1 year
NJ	2006	↑	Increases the penalties for appearing in front of agency as agent or attorney for 2 years
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 6 months
TN	2006	↑	Can't be lobbyist for 1 year
VA	2013	↑	Ban on lobbying expanded in meaning
WV	2005	↑	Ban on appearing in front of agency: from 6 months to 1 year
WV	2011	↑	Can't register as lobbyist for 1yr
SD	2011	↓	The 1 year ban on lobbying removed

Table G.18: States with and with no law changes on observable variables

I compare states with and with no law changes on observable variables on a set of observables' number of observations (columns (1) and (2)), means (columns (3) and (4)) and standard deviations (columns (5) and (6)). Each statistic, except population, is estimated based on a year-state panel between 2000 and 2018 for the 50 states plus District of Columbia. The first/second row compares levels/yearly changes in overall state GDP adjusted to millions of 2018 dollars and comes from BEA estimates. The third/forth row compares levels/yearly changes in state GDP coming from insurance, adjusted to millions of 2018 dollars and comes from BEA estimates. The fifth/sixth row compares average state population in 2010/state population as percent of overall U.S. population as of 2010, and estimates come from the 2010 Census. The seventh/eight rows are yearly levels/changes in total insurance premiums underwritten in each state, and they come from the annual NAIC's Insurance Department Resources Reports. The levels are in million dollars.

	n		mean		sd	
Variable	No Change	Change	No Change	Change	No Change	Change
GDP variables):						
$GDP_{s,t}$ (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:						
$Population_{s,2010}$	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:						
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