DO ELECTIONS DELAY REGULATORY ACTION?

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August 26, 2013

Abstract

This paper investigates whether elections delay regulatory action against failing financial institutions in a country with strong institutions and property rights. Exploiting the cross-sectional and time-series heterogeneity in the exogenous electoral cycles of U.S. insurance regulators and governors, we find causal evidence that regulators delay interventions before elections. The extent of the delay is larger before competitive elections and when the regulatory task is assigned to a politician (elected regulator) rather than a bureaucrat (appointed regulator). Regulatory governance mechanisms that constrain the discretion of regulators reduce the politicization of regulatory supervision. The delays induced by elections substantially increase the ultimate costs of failure.

JEL classification: G28; G22; D72; D73
Keywords: Government Policy and Regulation; Political Economy; Insurance; Electoral Cycles; Bureaucrats; Politicians; Incentives.

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The theory of political business cycles predicts that public officials will be proactive and use fiscal and monetary policy before elections to increase their popularity (Nordhaus, 1975; MacRae, 1977).[1] Elections, however, elicit a different response from firms and consumers. Political uncertainty delays investments by firms (Bernanke, 1983; Bloom, Bond and Van Reenen, 2007; Bloom, 2009; Pástor and Veronesi, 2010; and Julio and Yook, 2012) and consumers (Romer, 1990; Canes-Wrone and Park, 2012). Is it possible that elections cause public officials to delay actions too?

One area where public officials may delay is when dealing with failing financial institutions. There are several reasons why officials may delay intervening on failing firms before an election (Brown and Dinç, 2005). First, public officials might face questions about their competency when firms under their watch fail and thus have an incentive to pander to voter beliefs by deferring action until after elections (Canes-Wrone, et al., 2001; Maskin and Tirole, 2004; and Prat, 2005). Second, public officials want to generate favorable economic news prior to elections (Rogoff and Sibert, 1988). Third, the costs of closing a company fall on a relatively small group with strong interests in the outcome (the owners, employees, and customers), while the benefits, such as a healthy industry and economy, are widespread, making public officials more susceptible to interest group pressure before elections (Stigler, 1971; Peltzman, 1976).

Prior research finds that the elections of senior politicians (Prime Ministers or Presidents) influence the timing of regulatory interventions on failing banks in emerging market countries (Brown and Dinç, 2005). The degree to which public officials can affect real outcomes, however, depends on the strength of the country's institutions (Shi and Svennsson, 2006). In emerging market countries government ownership of banks is prominent (La Porta, et al., 2002), financial

[1] There is a large empirical literature on political business cycles (Alesina and Roubini, 1992; Alesina, et al., 1997, Gonzalez, 2002, Shin and Svensson, 2006; Cole, 2009; Schneider, 2010).

systems are underdeveloped, governments are generally inefficient, and there are poor protections of property rights. In a country with strong institutions are electoral delays possible?

This study aims to answer this question by analyzing the timing of the regulatory interventions of U.S. property-liability (P/L) insurance companies. This industry provides an ideal laboratory to study this issue. The U.S. insurance industry is regulated at the state level and each state has a head regulator, the insurance commissioner. Commissioners are elected by popular vote in some states and appointed by the governor in others.[2] The election dates of commissioners and governors are pre-determined, so there is no potential endogeneity in the timing of the election relative to firm failures.[3] Moreover, the elections are not nationally synchronized, so the cross-sectional and time-series heterogeneity of the elections provides clean causal identification of the impact of electoral politics on the timing of interventions.

There are reasons unrelated to electoral politics that could explain cross-sectional variation in interventions (some states have more aggressive regulators). Other explanations, unrelated to elections, could explain time-series variation in interventions (catastrophes or macroeconomic fluctuations). None of these reasons, however, could explain why we would observe a relationship before elections, but not in non-election years. Thus, the failure of U.S. insurance companies should not exhibit electoral cycles. If anything, in the absence of politics there should be more interventions prior to elections as the regulators competence will increase through experience (Padro i Miquel and Snyder, 2006).[4]

[2] Some states have used both types (California and Louisiana). In a few states, governors appoint legislative commissioners who are tasked with appointing the insurance commissioner (for instance, the Hawaiian Department of Commerce and Consumer Affairs appoints the Commissioner of Insurance).

[3] If the government can call an early election, then the timing of the election may not be exogenous relative to firm failures as the timing of elections and failures could be influenced by unobserved variables, such as financial or economic crises. This potential endogeneity problem is not a concern with fixed election dates.

[4] On average, the regulator will have a longer tenure before an election than after an election, since the regulator will have served, at a minimum, almost an entire term.

We combine detailed company-level data and failure data from 1989 to 2011 with matched data on the electoral cycles of the insurance commissioner, or the governor if the commissioner is appointed, to study the potential politicization of insurer supervision and to illuminate the mechanism through which political incentives influence the behavior. The sample, which is considerably larger than prior studies, includes approximately 3,200 firms and consists of 321 separate elections in 50 states over 21 years.[5]

We first investigate whether there are differences in the behavior of regulators in the pre-election period versus the post-election period. We find fewer interventions occur before the elections than after, suggesting that political concerns play a role in delaying interventions. We then perform a survival analysis to examine the statistical pattern of insurer failures and to investigate whether there is any tendency to delay insurer failures prior to elections. By controlling for the solvency screening tools used by regulators, insurer-specific factors, state-specific factors, and differences across time and states, we can cleanly identify the impact of electoral politics on the timing of regulatory interventions. We find regulatory interventions are 51 to 71 percent less likely to occur one year before elections. These results are robust to various specifications and controlling for changes in the insurance commissioner, macroeconomic factors, risk measures produced by the private sector, the seasonal nature of the solvency regulatory process, incumbents, term limits, and electoral cycle seasons.

We then investigate whether there is a difference between political and bureaucratic control of regulation. Elected public officials (politicians) strive for re-election, while appointed officials (bureaucrats) are career-concerned. Whether politicians or bureaucrats exert more effort or simply pander to voters and special interest groups depends crucially on the strength of the

[5] For comparison, Brown and Dinç (2005) study 210 banks and 40 elections in 21 developing economies over 7 years.

implicit incentives that reelection and career concerns provide (Besley and Coate, 2003). These incentives are strongest pre-election as voters and special interest groups rely more on recent information about policy choices (Canes-Wrone, et al., 2001). We find that the regulatory supervision provided by politicians (elected regulators) is significantly more prone to incentives to delay action before elections than that provided by bureaucrats (appointed regulators). Specifically, we find that political control of insurance regulation increases electoral delays by 19-28 percentage points. This finding provides empirical support for recent theoretical work emphasizing the comparative advantage of bureaucratic control for technical policy making (Maskin and Tirole 2004; Alesina and Tabellini 2007). Empirically, the results are consistent with Iaryczower, et al. (2013) and Whalley (2013).

We also study the influence of competitive elections on the responsiveness of public officials (Besley and Burgess, 2002; List and Sturm, 2006; Ashworth and Shotts, 2010; Ferraz and Finan, 2011). The incentive to pander to voters should be especially acute if elections are competitive and not losing votes is particularly valuable. We find that electoral delays increase by 34 percentage points before tightly contested elections. The competitive election effect is mainly due to the incentives of bureaucrats. Appointed regulators do not delay interventions before elections in which the appointing governor is likely to be elected, but do delay before competitive elections. Elected regulators delay interventions before all elections, regardless of the competitiveness.

We then explore whether regulatory governance mechanisms reduce politicization and yield better regulatory supervision (Williamson, 1985; Levy and Spiller, 1994). A 1994 law mandated that regulators take prompt corrective action against insurers with low levels of capital. The intent of the law was to reduce the discretion of regulators in when and whether to intervene in

failing firms. We find that the mandate reduces electoral delays. It is especially effective in reducing the discretion of appointed regulators.

Finally, we study whether the ultimate costs of failure increase because of the inaction induced by elections. Prior studies investigate the cost of resolving bank (James, 1991; Kaufmann, 2001) and insurer insolvencies (Bohn and Hall, 1999; Hall, 2000; Leverty and Grace, 2012). This is the first study to examine whether political incentives increase the cost of insolvency. We find that election induced inaction increases the ultimate costs of failure by $0.36 to $0.40 for every dollar of pre-insolvency assets.

The paper is organized as follows. The next section provides details on the institutional setting. Section II describes the data. The regression analysis of elections and their impact on the timing of regulatory interventions of failing insurers is presented in Section III; it also includes robustness checks. Section IV examines the effects of political versus bureaucratic control of regulation. Section V investigates the impact of competitive elections. Section VI analyzes whether regulatory governance mechanisms reduce politicization and yield better regulatory supervision. Section VII studies the costs of insurer failures. A concluding section follows.

I. INSTITUTIONAL SETTING

In this section we provide the institutional details relevant for understanding the U.S. P/L insurance industry. We focus on the aspects of the industry that make it ideally suited for studying the impact of politics of regulation. The industry is regulated at the state level and the primary responsibility for the solvency regulation of an insurance company is delegated to the state in which it is domiciled (Klein, 1996; Grace and Klein, 2009). The costs of an insurer's insolvency are distributed among all the states in which the firm operates, but the insurer's domiciliary state tends to reap the lion's share of the direct economic benefits of its operations

(for example, employment and payrolls). As a result, economic and political considerations could cause a domiciliary regulator to exercise too much forbearance in dealing with a distressed multi-state insurer.

The National Association of Insurance Commissioners (NAIC), an association of the state regulators, coordinates solvency oversight.[6] The NAIC also develops the solvency screening mechanisms used by state regulators. The Insurance Regulatory Information System (IRIS) is a set of 12 ratios that help state insurance departments examine the financial condition of an insurer.[7] The Financial Analysis and Surveillance Tracking (FAST) system is also a set of ratios that the NAIC uses to create a solvency score. The insurers with FAST scores deemed suspect are subject to further regulatory review.[8] A number of studies show that the FAST system dominates IRIS in solvency prediction (see for example, Grace, Harrington, and Klein, 1998; Cummins, Harrington, and Klein, 1995; and Cummins, Grace, and Phillips, 1999).

The NAIC instituted a risk based capital (RBC) standard for P/L insurers in 1994. The basic idea of RBC is that firms with riskier profiles, in terms of asset, credit, reserve, and underwriting risk, should hold higher levels of capital.[9] RBC does not add much discriminating power to solvency prediction models (Grace, Harrington, and Klein, 1998; Cummins, Harrington, and Klein, 1995; Cummins, Grace, and Phillips, 1999). It is not, however, strictly an early warning system. The RBC law also mandates prompt corrective action by regulators. Prior to the adoption of the law significant litigation arose over whether a company was insolvent. The law gives a regulator the authority to act. If an insurer's ratio of its capital to its RBC is between 70 and 100 percent, then the law *allows* the regulator to take control of the company even if the company is

[6] For a detailed description of the solvency oversight process see Klein (1995).
[7] IRIS traditionally consisted of eleven ratios, but a twelfth ratio was added in 1993.
[8] For a detailed explanation of the FAST system see Grace, Harrington and Klein (1998).
[9] For more details on the construction of the RBC formula and a critique see Feldblum (1996).

technically solvent. If the insurer's ratio is below 70 percent, a regulator is *required* to take control of the insurer. Thus, the law reduces regulatory discretion for severely weak firms.

II. DATA

The variables used in this paper are extracted from multiple sources. The financial information on insurance companies comes from the 1989-2009 NAIC Property-Casualty Annual Statement Database. The database is the most complete source of insurance company financial information available for the U.S. It contains the yearly regulatory filings of approximately 2,300 P/L insurers.

We use the insurer financial information to construct the analytical tools that state insurance regulators use to monitor the financial condition of insurers. Grace, Harrington, and Klein (1995) indicate there are diminishing marginal returns to incorporating additional balance sheet and income statement ratios not already included in the FAST system. The FAST ratios focus on an insurer's leverage, liquidity, operations, and asset quality. These variables are similar to those used to model corporate debt ratings or bank failures (Altman, 1968, or Shumway, 2001) but there are more of them and they are tailored to the specifics of the insurance industry. By controlling for the solvency screening tools used by regulators we can identify the role of political motivations on the timing of formal regulatory interventions.

The list of firms subject to formal regulatory action from 1989 to 2011 is collected from the NAIC's Global Receivership Information Database (GRID). We classify an insurer as under formal regulatory intervention if it is subject to proceedings for conservation of assets, rehabilitation, receivership, or liquidation in year t+1. In an effort to include as many interventions in the analysis as possible, we also classify insurers as subject to formal intervention if they have data two years prior to formal regulatory action but not one year prior.

We use all interventions for which we have sufficient financial information to conduct our multivariate analyses.

Table 1 presents sample statistics for the full sample of firms, the insurers that failed, and the insurers that did not. The table reveals differences between the insurers that eventually failed and the other insurers. Larger firms, liquid firms, firms with growing equity capital, mutual insurers, firms that are part of a group of insurers,[10] and insurers domiciled in states with relatively higher GSP per Capita are less likely to fail. Highly leveraged firms, rapidly growing firms, firms with adverse loss reserve development, firms that write more business in long-tail lines, and firms that conduct business in relatively fewer states are more likely to fail.

The main sources of the election dates and results are the *Insurance Legislative Fact Book & Almanac*, *The Almanac of American Politics*, and the Stateline database (www.stateline.org).[11] The *Insurance Legislative Fact Book & Almanac* also provides information about whether the insurance commissioner is elected or appointed. Table 2 shows the states that have elected and appointed insurance commissioners. From 1989 to 2011, 38 states use an appointed regulator, 10 states use an elected regulator, and 2 states, California and Louisiana, use both types.[12]

There is diversity in the election cycles of elected commissioners and the governors who appoint non-elected commissioners. States also have different election frequencies. During our sample period, 33 states hold their gubernatorial or insurance commissioner elections in sync with the Presidential mid-term election.[13] Nine states hold elections in sync with the Presidential

[10] Many insurance firms are organized as a group of firms under common ownership. State Farm, for example, has seventeen separately capitalized companies within its group.

[11] We also use other sources for gubernatorial and insurance commissioner election results: the CQ Electronic Library (http://library.cqpress.com), www.uselectionatlas.org, www.ourcampaigns.com, and newspaper articles.

[12] The Insurance Commissioner for the District of Columbia (D.C.) is appointed by the Mayor. Insurers domiciled in D.C. are not included in our main sample, but the results are robust to the inclusion of these firms.

[13] Alabama, Alaska, Arizona, Arkansas, California, Colorado, Connecticut, Florida, Georgia, Hawaii, Idaho, Illinois, Iowa, Kansas, Maine, Maryland, Massachusetts, Michigan, Minnesota, Nebraska, Nevada, New Mexico, New York,

election.[14] Two states have elections every two years in even numbered years.[15] Five states hold their elections in odd years: two in the year after a Presidential election;[16] and three in the year before the Presidential election.[17] Rhode Island switched, in 1994, from having elections every two years in even numbered years to having elections every four years in sync with the Presidential mid-term election.[18] The combination of cross-sectional and time-series variation in electoral cycles provides clean causal identification of electorally-motivated delay.

Figure 1 shows the number of elections and formal regulatory interventions of insurers from 1990 to 2011. The number of elections is shown in the gray bars and the number of interventions is shown by the black dotted line. The figure highlights the fact that elections are not nationally synchronized and that the election dates of commissioners and governors are pre-determined. The number of interventions, in contrast, varies widely over the time period. The average number of interventions per year is 11.7. The most interventions occur in 1992 with 26 and the least occur in 2007 and 2008 with 1. There is no discernible relationship between the timing of interventions and elections.

A different story emerges, however, when we look at the number of interventions over the electoral cycle. Figure 2 shows the number of formal regulatory interventions of insurers from 1990 to 2011 over the electoral cycle. The electoral cycle is broken into six month increments. The first half of the electoral cycle (after the election) is shown in gray and the second half (before the election) is shown in black. In classifying formal regulatory interventions, we

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Ohio, Oklahoma, Oregon, Pennsylvania, South Carolina, South Dakota, Tennessee, Texas, Wisconsin, and Wyoming.

[14] Delaware, Indiana, Missouri, Montana, North Carolina, North Dakota, Utah, Washington, and West Virginia.

[15] New Hampshire and Vermont.

[16] New Jersey and Virginia.

[17] Kentucky, Louisiana, and Mississippi.

[18] There are only three special elections during our sample period: the Louisiana insurance commissioner election in 2006; the Utah gubernatorial election in 2010; and the West Virginia gubernatorial election in 2011. Our results are robust to excluding these states.

considered the number of days since the previous election to the intervention date and the number of days from the intervention date to the next election. If the former is smaller, the intervention is considered as taking place after the election, and if it is larger, the intervention is recorded as taking place before the election.

On average the regulator will have a longer tenure before an election than after, since the regulator will have served, at a minimum, almost an entire term. Observing more interventions before the election than after is consistent with the regulator gaining competence through experience. Figure 2 shows that of the 260 interventions, 136 (52.3%) occur before elections and 124 (47.7%) after. The difference though is not statistically significant (p-value = 0.458).

As the election draws closer fewer interventions occur before than after. In the year around the election, 57 (21.9%) occur before elections and 66 occur after (25.4%) (p-value = 0.418). In the six-months around the election, when the influence of politics is strongest, there are significantly fewer interventions before (21, 8.1%) than after (43, 16.5%) (p-value = 0.006). These results suggest elections play a role in delaying the interventions of failing insurers.

III. ELECTIONS AND REGULATORY INTERVENTIONS

The null hypothesis that the timing of formal regulatory action against an insurer does not depend on the electoral cycle is tested using the following model:

$$I_{i,t} = \beta_1 BeforeElection_{i,t} + \beta_2 X_{i,t} + \tau_t + \omega_i + \varepsilon_{i,t}, \quad (1)$$

where for insurer i and year t: I_{it} is the unobserved propensity for regulatory intervention; $BeforeElection_{i,t}$ is an indicator variable that equals one if the intervention occurs one year before the elections or, in the case of no intervention, the end of the insurer's accounting year is one year before the election; $X_{i,t}$ is a vector of explanatory variables (time-varying firm and

state characteristics and regulatory solvency ratios); τ_t's are year fixed effects; and ω_i's are state fixed effects; and $\varepsilon_{i,t}$ is the error term. The standard errors are adjusted for clustering within firm and are robust to heteroskedastisticy.[19]

The firm characteristics are size (the natural logarithm of total assets), an indicator variable for firms organized as mutual insurers, and an indicator of whether the firm is a member of a group of insurers. Given the literature on the potential for regulatory competition (Laffont and Martimort, 1999), we also account for the number of states the insurer does business. We also control for economic differences amongst the states using real gross state product (GSP) per Capita.[20] The regulatory solvency tools are financial ratios in the NAIC's FAST system.

The inclusion of state fixed effects means that the analysis controls for time-independent political, legal, institutional, and geographic differences across states. The diversity in election cycles and frequencies, in combination with state and year fixed effects, means the cross-state nature of the analysis improves the identification of the influence of politics during elections and prevents spurious correlation between the election year and other one-time events in the U.S. economy.

The coefficient on *BeforeElection* will indicate whether regulatory interventions are less likely to occur within one year before the elections, even when the regulator's solvency screening tools and insurer-specific factors are controlled for. It is important to note that since we use annual data, *BeforeElection* is defined for the year before the elections. However, as Figure 2 shows, there is a difference in the two six month periods in the year prior to the elections. Of the 260 interventions, 36 (13.8 percent) occur 7 to 12 months before elections, while 21 (8.1 percent)

[19] It is possible that a regulatory intervention may not be independent from another intervention within the same state. Accordingly, in robustness regressions we correct the standard errors for clustering at the state level. In general, these models yield smaller standard errors.

[20] Data on real gross state product (GSP) is from the Bureau of Economic Analysis. Population data for each state comes from the *Statistical Abstract of the United States*.

occur 6 months prior. The difference is statistically significant (p=0.047). If most of the delay occurs in the six months before elections, then examining the year before may underestimate the extent of the regulator's politically motivated inaction.

We estimate equation (1) using a discrete-time hazard model. In robustness tests we also use a semi-parametric hazard model. Unlike static outcome models, hazard models make more efficient use of the data by explicitly incorporating information about the timing of insurer failure, which is important in this study when comparing two similar failed insurers if one insurer failed later than the other because of political consideration. In addition, hazard models are also shown to generate more accurate forecasts of bankruptcy than static discrete outcome models (Shumway, 2001).

Table 3 shows that regulatory interventions are less likely to occur one year before elections. In Column (1) the coefficient on *BeforeElection* is negative and statistically significant, signifying interventions are less likely to occur one year before elections. The effect is economically significant. The coefficient implies a decrease in the likelihood of intervention of 71 percent in the year before elections. In an effort to include as many interventions in the analysis as possible, the specification in Column (1) includes only 11 regulatory solvency (FAST) ratios. Column (2) includes all 25 of the FAST ratios. The additional data requirements reduce the number of regulatory interventions from 260 to 232. The coefficient on *BeforeElection* remains negative and statistically significant. The coefficient implies that the likelihood of intervention deceases by 70 percent in the year before elections. The results show regulatory supervision is prone to political influence that delays the resolution of insolvency, even after controlling for insurer health and state economic conditions.

Robustness. The regression samples in Columns (1) and (2) include all insurer observations for which we have sufficient data. We also investigate the electoral effect using matched samples. The idea behind the matching is to select a group of non-failing firms that are similar to the failing firms except for the fact that there is no formal regulatory intervention. We match the failing firms with a non-failing firm(s) in the first year the failing firm entered the sample.

We explore a number of matched sample combinations (four-to-one, three-to-one, two-to-one, and one-to-one), but since the results are similar for all combinations we show only the results for the one-to-one matched sample. We create the matched sample by estimating a propensity score using a logit model. We include the following variables in the propensity score model: firm size (natural logarithm of total assets), organizational form (mutual indicator), geographical diversification (geographical Herfindahl index),[21] line of business diversification (line of business Herfindahl index),[22] leverage (net premiums written to equity capital and reserves to equity capital), investment yield, adverse reserve development to equity capital, the percent of business written in long-tailed lines, and the ratio of receivables from affiliates to equity capital.

Table 3, Column (3) shows the results for the matched sample.[23] The coefficient on *BeforeElection* remains negative and statistically significant. The coefficient indicates a decrease in the likelihood of intervention of 66 percent in the year before elections.

Table 3, Columns (4)-(6) show the results using a Cox proportional hazard model. Column (4) uses the full sample of firms and includes the same 11 of the regulatory solvency (FAST)

[21] The NAIC annual statements detail the premiums that insurers write in all 50 states and the District of Columbia. The geographical Herfindahl index is the sum of the squares of the percentages of premiums written by state.
[22] The NAIC annual statements document the premiums that that insurers write in 26 lines of business. The line of business Herfindahl index is the sum of the squares of the percentages of premiums written by line.
[23] The regression includes the same regulatory solvency (FAST) ratios used in Table 3, Column (1). Using the full set of regulatory ratios yields similar results.

ratios used in Column (1). Column (5) uses the full sample of firms and includes all 25 FAST ratios. Column (6) uses the one-to-one matched sample and includes 11 regulatory ratios. The coefficient on *BeforeElection* is negative and statistically significant in all three specifications. The coefficients indicate that the likelihood of intervention decreases by 51 to 62 percent in the year before elections.

Seasonal Nature of Solvency Regulatory Process. A potential concern is that our electoral effect is a result of the seasonal nature of the solvency regulatory process. Annual reports must be filed with state insurance commissioners during the first quarter of the year, providing regulators with a once-a-year source of detailed information on which to base regulatory action. Prior research shows that, other things equal, the insurer insolvency rate is greatest during the first quarter of the year (Browne and Hoyt, 1995). Elections, however, typically occur in November. Thus, a potential concern is that our results are spurious—the observed pre-election effect may be due to the release of detailed information after elections, rather than the influence of the elections themselves.

To a large extent our research design mitigates this concern. The election cycles of governors and insurance commissioners in the U.S. are non-synchronized and there are different election frequencies. This, in combination with state and year fixed effects, means the cross-state nature of the analysis provides clean causal identification of the influence of politics during elections. Moreover, our multivariate analysis uses annual data, so quarterly effects should not influence the results. Nevertheless, we analyze the robustness of the pre-election effect to the seasonal nature of the solvency regulatory process.

To determine whether the electoral effects are an artifact of the seasonal nature of the solvency regulatory process, we perform a falsification (placebo) test. We re-estimate our main

specification on a placebo treatment of *BeforeElection* constructed using randomly assigned electoral cycles (for example, the electoral cycle of California may be assigned to Nebraska). The reshuffling of election dates maintains the seasonal nature of the solvency regulatory process, but it strips away the election effect. If we find the reshuffled *BeforeElection* dummy variable is insignificant, then this suggests our results are not confounded by seasonal factors.

A potential criticism of a single placebo treatment is that we may cherry-pick the reassigned electoral cycles, so we do the random reshuffling of the electoral cycles 1,000 times. The 1,000 regressions yield a mean coefficient on the placebo of *BeforeElection* of -0.122, a median of -0.105, and a standard deviation of 0.283. The coefficient is -0.880 at the 1st percentile and 0.490 at the 99th percentile. To put this in context, the coefficient of *BeforeElection* in our main regression (Table 3, Column (1)) is -1.727. The placebo tests confirm that the effect is confined to elections.

New Regulator. Another potential concern is that the election effect may be due to a change in the regulator. An insurance commissioner that is incompetent in dealing with failing insurance companies may lose the election, resign, or be fired by the governor; and another, more competent insurance regulator may come into office. If that is the case, the pre-election effect detected above may only reflect the change from an incompetent regulator to a competent one. Accordingly, we analyze the robustness of the pre-election effect to changes in the regulator.

Naturally, the changes in the regulator in office are correlated with elections. However, the correlation is not perfect for at least three reasons. First, incumbent regulators may win the elections and stay in office. Second, incumbent appointed regulators may stay in power even if there is a change in governor. Third, the regulator may resign in the middle of the electoral cycle.

These differences will allow the analysis to distinguish the effect of a new regulator from that of elections.

Table 4, Column (1) includes *NewRegulator*, a dummy variable that is one in the year that a new regulator comes into office. *NewRegulator* has a positive coefficient, but it is not statistically significant. The coefficient of *BeforeElection* remains negative, has a magnitude that is similar to previous regressions, and is statistically significant. This result indicates that the absence of interventions before elections is different from any effect that results from a change in the regulator holding office.

Business Cycle. Given the literature on the relationship between the electoral cycle and macroeconomic variables, it is important to study the robustness of the results to potential macroeconomic changes. Table 4, Column (2) includes *Cycle*, a dummy variable indicating an NBER-dated recession. The coefficient on *Cycle* is positive and statistically significant, which implies an increasing likelihood of failure during economic downturns. The main variable of interest—*BeforeElection*—continues to have a negative and statistically significant coefficient. The business cycle therefore is superfluous to the paucity of regulatory interventions before elections.

Private Sources of Information on Insurer Quality. Prior studies document that risk measures (credit ratings) produced by the private sector may provide superior predictive ability relative to the measures created by regulators (Ambrose and Seward, 1988). For that reason, it is important to study the robustness of the pre-election effect to the inclusion of these measures as it is conceivable that regulators take these measures into consideration when determining whether to pursue action against a weak firm.

A.M. Best is the dominant credit rating agency in the insurance industry (Doherty, Kartasheva, and Phillips, 2012). A.M. Best's ratings reflect the rating agency's opinion of the insurer's ability to meet its policyholder obligations. Similar to regulatory solvency ratios, A.M. Best's ratings measure the financial strength of firms. However, unlike regulatory ratios, the ratings also incorporate qualitative information about firms. Table 4, Column (3) includes *A.M. Best Rating*, which is an integer value ranging from 0 for firms with low financial strength (ratings of C or lower, including firms not rated) to 6 for firms with high financial strength (ratings of A+ or A++ rating). The coefficient on *A.M. Best Rating* is negative and statistically significant, which implies that highly rated firms are less likely to fail. The coefficient of *BeforeElection* remains negative, has a magnitude that is similar to previous regressions, and is statistically significant, indicating that the lack of interventions before elections is not a result of regulators relying upon information from private sources.

Other Robustness Tests. In unreported regressions, we also control for the different electoral cycle seasons and the influence of incumbents (Besley and Cane, 1995a) and term limits (Besley and Cane, 1995b). To determine whether the election cycle seasons matter, we include election cycle season indicators (for example, indicators if the election is in sync with the presidential election, in sync with presidential midterm, etcetera). To test whether incumbents influence our results, we include an indicator for whether the elected politician (the regulator if the commissioner is elected and the governor if the commissioner is appointed) is an incumbent. To investigate whether term limits influence our results, we include an indicator for whether the elected politician is working under a term limit. In these robustness tests, the coefficient of *BeforeElection* remains negative, has a magnitude that is similar to previous regressions, and is statistically significant.

IV. *APPOINTED V. ELECTED REGULATORS*

We next investigate whether there are differences between elected and appointed regulators. Elected and appointed regulators face different incentives. Elected regulators are politicians that are directly accountable to voters and their incentive is to gain reelection. In contrast, appointed regulators are accountable to their professional peers and potential employers and exert effort to gain future job postings or professional recognition. Besley and Coate (2003) argue that elected regulators will be more likely to be pro-consumer as their position on regulation is the only salient issue for voters, while for appointed officials regulatory policy is bundled with the other policy issues of the appointing politician. In the context of insurer insolvencies, pro-consumer actions would come in the form of prompt action against failing firms as solvent insurers are assessed to pay the claims of insolvent insurers and the burden of these assessments are ultimately shared with consumers (through higher insurance rates) and taxpayers (because of state premium tax offsets and deductions for federal income taxes) (Barrese and Nelson, 1994). Maskin and Tirole (2004) and Alesina and Tabellini (2007) argue that bureaucrats have a comparative advantage in technical policy making, suggesting that appointed regulators will be less likely to delay intervention on failing insurers prior to elections.

Whether elected or appointed regulators exert more effort or simply pander to voters and special interest groups depends crucially on the strength of the implicit incentives that reelection and career concerns provide. Pandering incentives are strongest pre-election because the voter relies more on recent information about policy choices.

Figure 3 shows the number of formal regulatory interventions by appointed regulators over the gubernatorial electoral cycle. Of the 165 interventions performed by appointed regulators, 89 (54.0%) occur before elections and 76 (46.0%) occur after (p-value = 0.373). Forty-one insurers

(24.8%) are subject to formal regulatory action the year prior to elections and 36 (21.8%) the year after elections (p-value = 0.570). A reversal occurs in the six months around the election as less interventions occur in the six months before elections (14, 8.5%) than in six months after (23, 13.9%). The difference, however, is not statistically significant (p-value = 0.140).

Figure 4 shows the number of interventions by elected regulators over the elected insurance commissioner electoral cycle. In the second half of the electoral cycle, the number of interventions falls in each six-month period as the election draws closer. The number of interventions then abruptly increases in the six months after the election. Of the 95 interventions conducted by elected regulators, 47 occur before elections and 48 occur after. In the year before the elections there are 16 interventions (16.8%), while in the year after there are 30 (31.6%). The difference is statistically significant (p-value=0.038). There is a large increase in the number of interventions in the six months after elections relative to the six months before. Twenty interventions (21.1%) occur in the six months after elections, while only 7 (7.4%) occur in the 6 months before (p-value=0.012). Figures 3 and 4 show there are differences between elected and appointed regulators.

We further analyze these differences using the following model:

$$I_{i,t} = \beta_1 BeforeElect_{i,t} + \beta_2 BeforeAppt_{i,t} + \beta_3 X_{i,t} + \tau_t + \omega_i + \varepsilon_{i,t}, \quad (2)$$

Before_Elect is an indicator variable that equals one if the insurer is domiciled in a state with an elected insurance commissioner and it is one year before the election. *Before_Appt* is a similar variable but for insurers' domiciled in a state with an appointed insurance commissioner.

Table 5 also shows there are differences between elected and appointed regulators. Column (1) shows that the coefficients on *Before_Elect* and *Before_Appt* are both negative and statistically significant. The coefficient on *Before_Elect*, however, is significantly greater than

the coefficient on *Before_Appt*. The coefficient on *Before_Elect* indicates that the likelihood of an intervention by an elected regulator decreases by 84 percent in the year before election, while the coefficient on *Before_Appt* implies that the likelihood of an intervention by an appointed regulator decreases by 63 percent in the year before gubernatorial elections. Political control of regulation increases delays against weak insurers before elections by 21 percentage points.

The results are robust to using the full set of regulatory solvency (FAST) ratios, matched samples, and a Cox proportional hazard model.[24] It is also robust to controlling for the effect of a change in regulator (Column (2)), the business cycle (Column (3)), for private sources of information on the quality of insurers (Column (4)), and for the seasonal nature of the solvency regulatory process (unreported). The estimates indicate that political control of insurer solvency regulation increases delays against weak insurers before elections by 19-28 percentage points.

V. *COMPETITIVE ELECTIONS*

It is possible that regulatory interventions depend on the political strength of the party in power or how highly contested the elections are. If the elections are expected to be particularly tight or the party in power is not strong, the politicians' incentives to delay insurer interventions may be greater. To examine whether the incentive to delay is greater before competitive elections we construct a measure of the closeness of elections using the results of the governor and insurance commissioner elections from 1989 to 2012. A simple proxy for a tight election is an election decided by a narrow margin. We therefore construct an indicator, *Contested*, which equals one if the election is decided by a margin that is in the bottom quintile (20%) of the sample election results (a margin of approximately 4.8% or less). The results are robust to using tighter margin thresholds—the bottom 15% (3.6%), 10% (2.9%), and 5% (1.1%).

[24] These results are not tabulated to conserve space, but they are available from the authors upon request.

To estimate the impact of close elections we add to equation (1) the interaction $BeforeElection_{i,t}$ *x* $Contested_{i,t}$. In this model $\beta_{BeforeElection}$ measures the electoral effect for elections decided by a relatively large margin, while $\beta_{BeforeElection} + \beta_{BeforeElection\ x\ Contested}$ measures the electoral effect for highly contested elections. If the regulator's incentive to delay is greater before competitive elections, then $\beta_{BeforeElection\ x\ Contested} < 0$.

Table 6 shows that the incentive to delay is greater before competitive election. In Column (1) the coefficient of *BeforeElection* is negative and statistically significant; however, the magnitude is smaller than in previous regressions. The coefficient on the interaction term is negative and statistically significant, indicating the incentive to delay regulatory action against a failing insurer is greater before competitive elections. The decrease in the likelihood of intervention before uncompetitive elections is 49.1%, while it is 83.6% before highly contested elections; a difference of 34.5 percentage points.

Column (2) shows that highly contested elections have a different effect on appointed and elected regulators. The regression includes, in addition to *Before_Elect* and *Before_Appt*, the interactions $BeforeElect_{i,t}$ *x* $Contested_{i,t}$ and $BeforeAppt_{i,t}$ *x* $Contested_{i,t}$. The coefficient on *Before_Elect* remains negative and statistically significant. The coefficient for *Before_Elect* x *Contested* is not statistically significant; indicating that elected regulators do not delay *more* before tight elections. The coefficient for *Before_Appt* is negative, suggesting regulatory delays before elections, although the estimate is statistically weak. The coefficient for *Before_Appt* x *Contested* is negative and statistically significant. The estimates indicate that the likelihood of regulatory intervention by appointed regulators is 86.9% less before highly contested elections, but only 33.8% less before other elections; a difference of 53.1 percentage points. The results indicate elected regulators have an incentive to delay before all elections, while appointed

regulators have a stronger incentive to delay interventions before elections in which the appointing governor is in a tight race.

VI. REGULATORY GOVERNANCE

As we discuss above, the Risk Based Capital (RBC) law requires regulators to take action against certain financially weak insurers. As such, its introduction in 1994 may reduce some of the discretion that a regulator has to delay the intervention of a weak firm before elections. To test whether RBC's mandate for prompt corrective action reduces the regulator's ability to delay the politically costly resolution of insurers before elections, we add to equation (1) $AfterRBC_{i,t}$ and the interaction $BeforeElection_{i,t} \; x \; AfterRBC_{i,t}$. *AfterRBC* is an indicator variable that is one for the years 1994 and greater. In this model $\beta_{BeforeElection}$ measures the electoral effect before RBC, while $\beta_{BeforeElection} + \beta_{BeforeElection \; x \; AfterRBC}$ measures it after RBC. If mandated prompt corrective action reduces the regulator's discretion to delay interventions before elections, then $\beta_{BeforeElection_{i,t} \; x \; AfterRBC_{i,t}} > 0.$

Table 7 shows that the implementation of prompt corrective action reduces the discretion of regulators. In Column (1) the coefficient on *BeforeElection* is negative and statistically significant. The coefficient implies that prior to RBC the likelihood of regulatory intervention decreases by 93 percent before elections. The coefficient on *BeforeElection x AfterRBC* is positive and statistically significant, indicating prompt corrective action restrains the regulator's ability to delay interventions. The electoral effect after RBC, $\beta_{BeforeElection} + \beta_{BeforeElection \; x \; AfterRBC}$, however, remains negative and statistically significant (p-value = 0.047). After RBC the likelihood of intervention decreases by 49 percent before elections, which is a 44 percentage point reduction relative to the pre-RBC period.

Column (2) shows that prompt corrective action is most effective in reducing the discretion of bureaucrats. The coefficients on *BeforeElect* and *BeforeAppt* are negative and statistically significant. The coefficients imply that prior to RBC the decrease in the likelihood of regulatory intervention before an election is 88 percent for elected regulators and 94 percent for appointed regulators. The coefficient on *BeforeElect x AfterRBC* is not statistically significant, while the coefficient on *BeforeAppt x AfterRBC* is positive and statistically significant. Thus, the introduction of RBC does not reduce the discretion of elected regulators, but it does for appointed regulators. After RBC the decrease in the likelihood of intervention before an election is not statistically significant for appointed regulators ($\beta_{Before\_Appt} + \beta_{Before\_Appt\ x\ AfterRBC}$ = -0.928 (p-value = 0.117)). These results suggest that regulatory rules that constrain the discretion of regulators are more effective when the regulatory function is separated from the political one.

VII. THE COST OF ELECTORAL DELAY

We next examine whether regulatory inaction due to elections increases the cost of insolvency. Previous studies examine the difference in the cost of resolving insolvencies across insurers (Hall, 2000; Grace, Klein, and Phillips, 2009; Leverty and Grace, 2012). Our study differs in that while controlling for the influence of the incentive structure on regulators (Hall, 2000) and managers (Lee, Mayers, and Smith, 1997; Grace, Klein, and Phillips, 2009), we examine whether political motivations increase the costs of insolvency.

The cost of liquidating an insurer's insolvency comes from the *Assessment and Financial Information Report* published by the National Conference of Insurance Guaranty Funds (NCIGF,

2012). The NCIGF report records the cumulative payments, recoveries, and net cost through 2012 for each insolvency that triggered a guaranty fund assessment.[25]

The relative cost of insolvency is measured as the ratio of cumulative net guaranty association assessments from the insolvency as of 2012 to the assets of the firm prior to the regulator taking formal regulatory action. We have data for all firms with claims covered by guaranty associations.[26] We only observe the net costs when the assets of the insurer are insufficient to pay the covered insurance claims. For that reason, the underlying baseline regression of the latent variable is:

$$y_i^* = \alpha + \beta X_i^{fc} + \phi X_i^{reg} + \tau_t + \omega_i + \varepsilon_i, \quad (3)$$

where y_i^* is the latent resolution cost variable for insurer *i* (it equals the ratio of net cumulative guaranty assessments by 2012 to insurer *i*'s total assets in the year prior to formal regulatory action); X_i^{fc} is a vector of firm characteristics; X_i^{reg} is a vector of regulatory variables; τ_t's are year fixed effects; ω_i's are state fixed effects; and ε_i is the error term. The dependent variable is censored at 0; it equals y_i^* when y_i^* is greater than 0 and equals 0 otherwise. We use Tobit estimation techniques to account for the censoring.

The firm characteristics are size (the natural logarithm of total assets), an indicator variable for firms organized as mutual insurers, an indicator of whether the firm is a member of a group of insurers, leverage (net premiums written to equity capital), liquidity, line of business diversification (line of business Herfindahl), the number of states a firm conducts business, and the percent of premiums written in catastrophe prone lines and areas. The regulatory variables

[25] Guarantee funds are state specific funds that assess solvent insurers to pay the claims of insolvent insurers.

[26] Most state guarantee funds cover personal lines of insurance (like auto and homeowners), but there is less homogeneity amongst the states regarding coverage for commercial lines of insurance. There is also heterogeneity in the deductibles and coverage limits applied to covered lines.

account for prompt corrective action, single-state insurers, and the type of first regulatory action against the insurer (rehabilitation, conservation, or liquidation). All independent variables are recorded in the year prior to formal regulatory action.

Table 8 presents summary statistics of the cost to resolve insolvencies. For all the firms in the sample, the average cost is roughly $0.71 for every dollar of pre-insolvency assets. For the firms that access the guaranty fund system, the average cost is roughly $0.95. The average cost for the five most expensive insolvencies is $10.69. The most expensive failure is $27.92.

The skewness in the dependent variable (8.90 for the 176 firms that access the guaranty fund system and 9.99 for all the firms in our sample) creates some econometric challenges in estimating equation (3). Tobit maximum likelihood estimates yield inconsistent estimates when the disturbances are non-normal (Arabmazar and Schmidt, 1982). To control for the extreme skewness of the dependent variable, we adopt two strategies: (1) we trim the dependent variable values above the 95th percentile (this decreases the skewness of our dependent variable to 1.83); and (2) we drop all observations above the 95th percentile from the sample (this reduces the skewness of the remaining 223 observations to 1.89).[27]

To test whether electoral forbearance leads to higher costs of insolvency, we need to identify the firms for which intervention was delayed because of an election.[28] Regulators do not reveal which insurers are suspected of financial distress, but they do reveal the insurers subject to

[27] We also use two other approaches. First, we estimate the equation under the assumption that the disturbances are drawn from a logistic distribution, a heavy-tailed distribution that will better cope with the skewness of the dependent variable. Second, instead of using maximum likelihood procedures we use Powell's semiparametric estimator for censored data – the censored least absolute deviation (CLAD) model (Powell, 1984, 1986), which provides unbiased, consistent estimates that are robust to non-normality and heteroskadaticity. Our results are robust to these two alternative approaches.

[28] Not all of the formal regulatory interventions that occur after the election are due to regulators delaying action because of political motivations. Thus, we want to identify the weak firms that regulators were aware of before the election, yet did not take action against until after the election.

formal regulatory intervention. Therefore, assuming regulators cost-effectively allocate their limited resources, we can identify the insurers subject to regulatory scrutiny.

Specifically, we first estimate the probability of formal regulatory intervention using the regulatory solvency tools. We then classify a firm as being under regulatory scrutiny if it has a probability of formal regulatory intervention that is greater than a "probability cutoff point" which recognizes the cost-effective allocation of regulators limited resources. The cost-effective allocation is based on the relative cost of misclassifying a failing firm (Type I error) and misclassifying a solvent firm (Type II error). The cost of misclassifying a failing firm is the total guarantee fund assessment due to a firm's failure. The cost of misclassifying a solvent firm is the opportunity cost of the regulators' formal examination of the firm. We use a 40:1 relative cost ratio, which is roughly the ratio of total insurer payments to New York's guaranty fund to total funds reimbursed to the New York Department of Insurance for its examinations of insurers.[29] New York is generally considered to have the most rigorous regulatory system and it is the only state that requires ex-ante guarantee fund assessments (Meier, 1988; Cummins and Sommer, 1996). The probability cut-off point for our sample of 37,830 firm-years is 2.3 percent.[30]

To investigate the cost of electoral forbearance, we add *Forbear*, to equation (3). *Forbear* is an indicator variable that is set equal to one if the firm is under regulatory scrutiny before the election, but the intervention occurs after the election. Table 9 shows that the inaction induced by elections increases the cost of insolvency. In Columns (1) and (2) the values of the dependent variable above the 95th percentile are truncated. Column (1) shows electoral forbearance increases the cost of insolvency by \$0.36 for every dollar of pre-insolvency assets; a 68% increase in the cost of the average insolvency. For the average firm in the sample, which has pre-

[29] Source: Annual Report of the Superintendent of the Insurance to the New York Legislature, various years.
[30] The results are also robust to using different relative cost ratios, 10-, 20-, 30-, 50-, and 60-to-1.

insolvency assets of roughly $92 million, electoral forbearance increases the cost of insolvency by $33 million. The coefficient on *ForBear_Appt* in Column (2) is not statistically significant, while the coefficient on *ForBear_Elect* is positive and significant. Electoral forbearance by elected regulators increases the cost of insolvency by $0.42 for every dollar of pre-insolvency assets. The general pattern observed in Columns (1) and (2) is repeated in Columns (3) and (4), when the values of the dependent variable above the 95th percentile are dropped, but the estimates are more precise, as the standard errors for the forbearance effect drop. Column (3) shows the forbearance effect increases the cost of insolvency by $0.40 for every dollar of pre-insolvency assets; a 97% ($38 million) increase in the cost of the average insolvency. Column (4) shows the forbearance effect for elected regulators increases the cost by $0.52.

VIII. CONCLUSION

Policy considerations motivate this research. If regulators intervene to minimize social costs, failures would be determined solely by firm health and economic conditions. This paper demonstrates that this is not the case. We find regulators are reluctant to take over failing firms if it negatively influences their own political or career goals. In particular, we find that regulatory interventions are delayed prior to elections. Prior studies document this effect in emerging economies, but this paper shows that developed countries, and the U.S. in particular, are not immune to similar incentive problems. Studying the U.S. allows us to determine the influence of competitive elections on the responsiveness of public officials. We find that the incentive to pander to voters is particularly acute when elections are competitive and not losing votes is particularly valuable. This study also documents that politically motivated delays in regulatory intervention substantially increase the ultimate costs of failure.

This type of microeconomic study is helpful in understanding how regulatory supervision can be politicized and what kinds of institutions are necessary to provide adequate incentives to regulators to achieve better supervision. This study shows that the governance structure of the regulatory system can be structured to curb regulatory opportunism. In particular, regulatory rules, if appropriately constructed, can be effective in mitigating the impact of politics on regulatory supervision.

The results also highlight the role of politics and career concerns in the implementation of regulations. We find that assigning the regulatory task to a bureaucrat reduces the delay induced by elections by 19-28 percentage points. Thus, different political institutions create meaningful differences in regulatory policy making. This result suggests other new research questions. Using data on the expertise of regulators, it would be informative to determine whether the technical expertise of regulators affects regulatory policy choices and performance. This question is especially interesting in light of recent theoretical and empirical research, including the results of this study, that appointed officials are more adept at technical policy.

This study focuses on solvency regulation. However, regulatory activities also include market regulation – the regulation of prices, products, and trade practices. The influence of elections on market regulation is an interesting topic for future research. Do regulators restrict prices prior to elections to pander to voters? Do regulators grant rate increases or approve new products around elections to curry favor with the industry?

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Figure 1: Number of Elections and Regulatory Interventions (1990-2011)

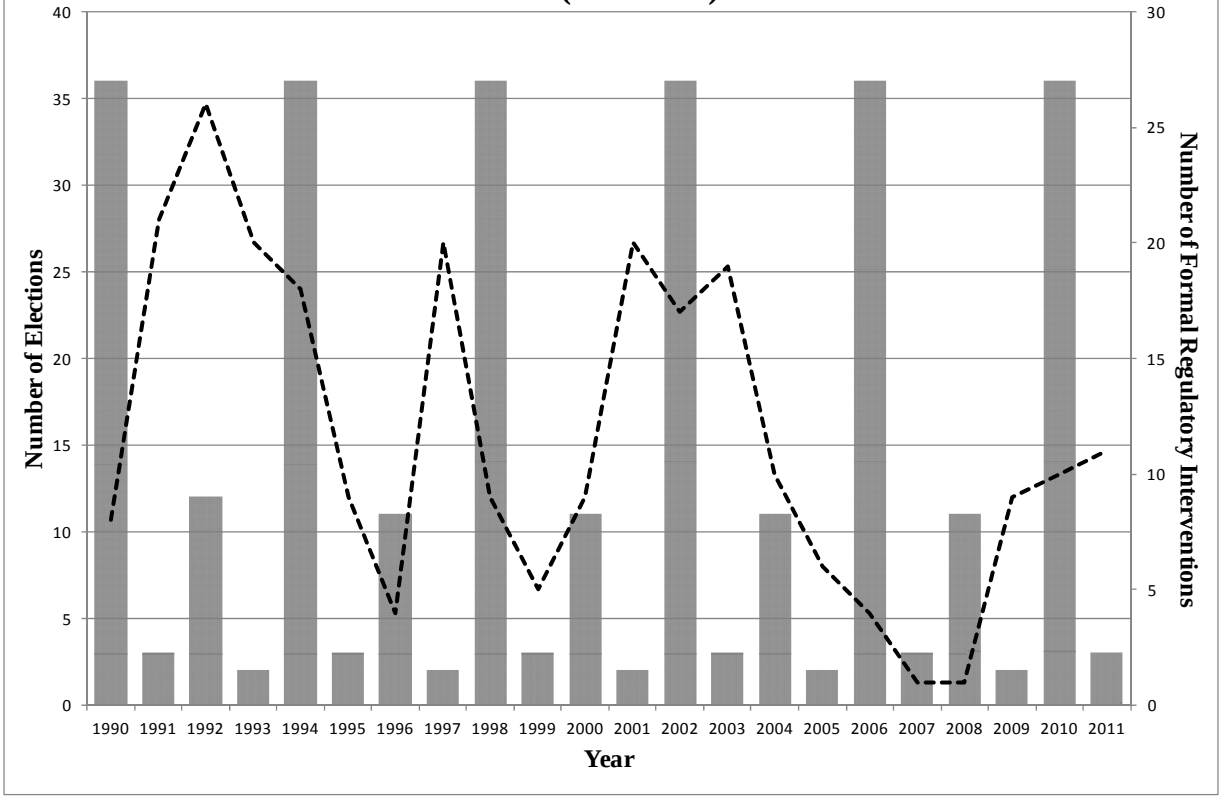

Figure 2: Electoral Cycle and Regulatory Interventions (1990-2011)

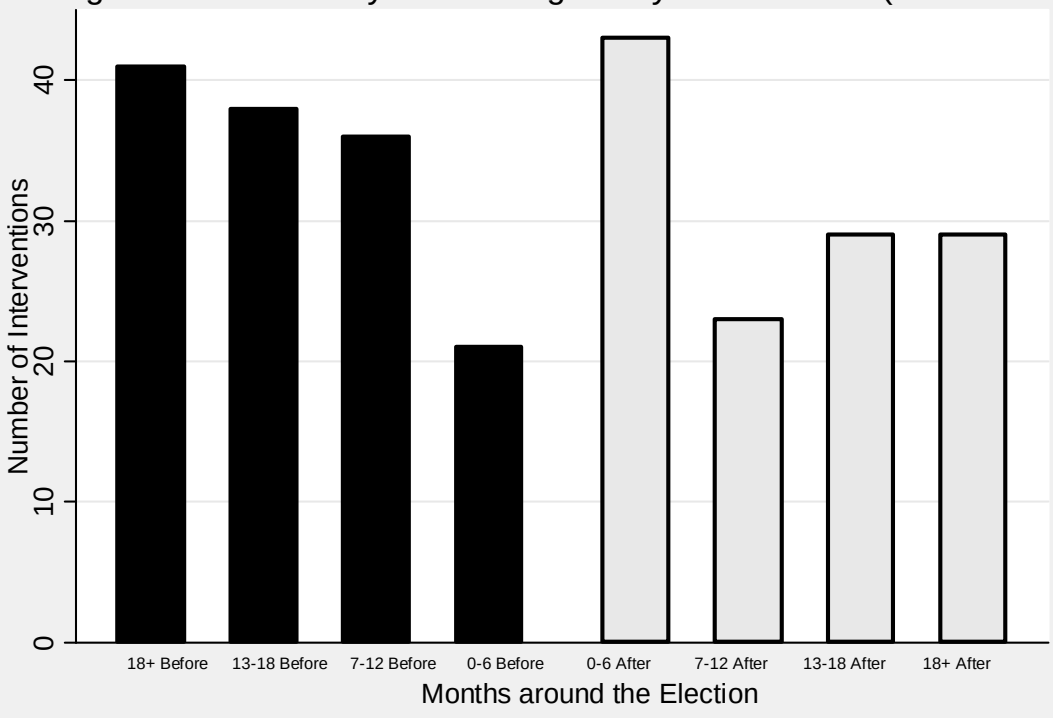

Figure 3: Electoral Cycle and Regulatory Interventions (1990-2011)

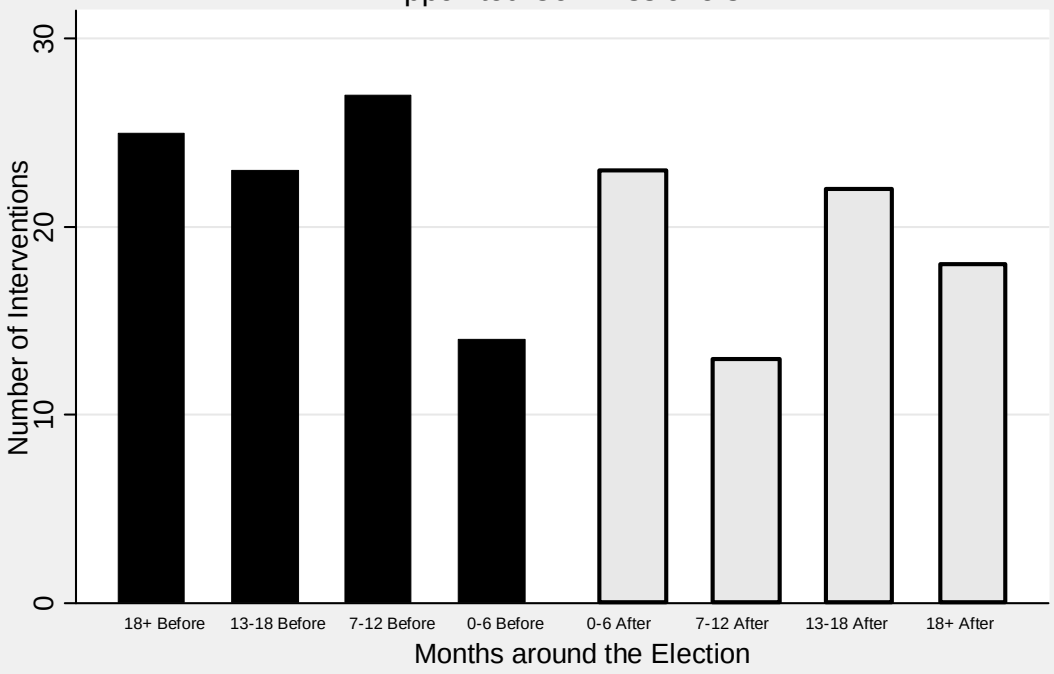

Figure 4: Electoral Cycle and Regulatory Interventions (1990-2011)

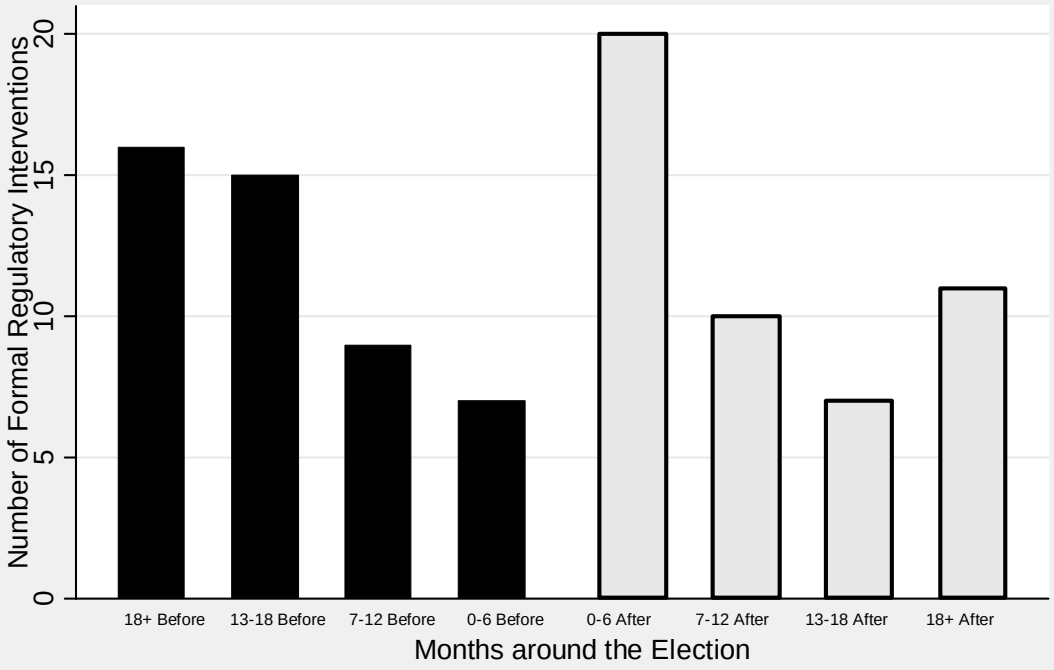

Table 1
Summary Statistics

	All Insurers (N=37,830)		Insolvent Insurers (N=1,717)		Solvent Insurers (N=36,113)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
Size	17.887	2.005	17.215 ***	1.633	17.919	2.016
Mutual	0.217	0.412	0.125 ***	0.331	0.221	0.415
Group	0.636	0.481	0.422 ***	0.494	0.646	0.478
Ln(Number of States)	1.664	1.546	1.52 ***	1.448	1.671	1.550
Gross State Product per Capita	34.049	9.822	29.243 ***	7.725	34.277	9.852
Net premiums written to equity capital	1.160	0.910	1.767 ***	1.277	1.131	0.879
Reserves to equity capital	1.047	1.083	1.673 ***	1.607	1.017	1.042
1 yr. growth in net premiums written	0.195	0.884	0.326 ***	1.151	0.189	0.869
Investment yield	0.050	0.020	0.054 ***	0.023	0.050	0.019
1 yr growth in equity capital	0.104	0.263	0.044 ***	0.361	0.107	0.258
Adverse reserve development to equity capital	-0.003	0.236	0.15 ***	0.479	-0.010	0.215
1 yr growth in combined ratio	-0.011	0.401	0.006 *	0.451	-0.011	0.399
1 yr change in liquid assets	0.108	0.267	0.095 **	0.354	0.109	0.262
Percent of business in long-tailed lines	0.641	0.304	0.702 ***	0.300	0.638	0.304
Receivables from affiliates to equity capital	0.023	0.067	0.046 ***	0.101	0.021	0.065
Non-investment grade bonds to equity capital	0.007	0.023	0.008	0.030	0.007	0.023

The table provides summary statistics for the insurers in the sample. *Insolvent Insurers* are the firms that were subject to formal regulatory intervention during the sample period. *N* indicates the number of insurer-years. *Size* is the natural logarithm of total assets. *Mutual* is an indicator that takes the value of one if the insurer is a mutual. Group is an indicator that equals one if the insurer is a member of a group of insurers, and zero otherwise. *Ln(Number of States)* is the natural logarithm of the number of states the firm does business. *Gross State Product per Capita* is Gross Domestic Product by State (thousands of current dollars) divided by the population of the state. The remaining variables are balance sheet and income statement ratios from the National Association of Insurance Commissioner's (NAIC) Financial Analysis and Surveillance Tracking (FAST) system. *, **, *** denote statistical significance at the 10, 5, and 1 percent levels, respectively, in a two-sided test of the mean with the insolvent insurers and the solvent insurers.

Table 2
Insurance Commissioners

Appointed			Elected	Both
AK	MD	OR	DE	CA[1]
AL	ME	PA	FL	LA[2]
AR	MI	RI	GA	
AZ	MN	SC	KS	
CO	MO	SD	MS	
CT	NE	TN	MT	
HI	NH	TX	NC	
IA	NJ	UT	ND	
ID	NM	VA	OK	
IL	NV	VT	WA	
IN	NY	WI		
KY	OH	WV		
MA		WY		

[1] California switched from an Appointed to an Elected insurance commissioner in 1990. In 2000, the elected commissioner of California, Charles Quackenbush, resigned from office rather than face impeachment. From July 2000 to January 2003, two insurance commissioners, J. Clark Kelso, and Harry W. Low, were appointed by the Governor.

[2] The elected insurance commissioner of Louisiana, James H. Brown, was convicted of lying to an FBI agent and he resigned from office in the Fall of 2000. As a result, J. Robert Wooley was appointed by the Governor to be the insurance commissioner. Wooley was elected the insurance commissioner in the fall of 2003. Wooley resigned on February 15, 2006 and Jim Donelon was appointed by the Governor. Donelon was elected insurance commissioner in a special election held on September 30, 2006.

Table 3
Elections and Regulatory Interventions

	(1)	(2)	(3)	(4)	(5)	(6)
BeforeElection	-1.727 ***	-1.676 ***	-1.506 ***	-1.085 ***	-1.323 ***	-0.708 **
	(0.473)	(0.477)	(0.417)	(0.382)	(0.409)	(0.282)
Additional Controls:						
Regulatory Solvency Ratios	11	25	11	11	25	11
Firm Controls	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
p-value Wald test	0.000	0.000	0.000	0.000	0.000	0.000
Number of Observations	37,830	33,232	5,325	36,113	32,086	4,967
Number of Firms	3,261	2,956	520	3,215	2,918	482
Number of Failed Firms	260	232	260	242	221	241
Log Likelihood	-1035.52	-876.83	-703.34	-1440.68	-1238.08	-1138.06

The table presents hazard analyses for formal regulatory interventions of insurers for the years 1989 to 2009. *BeforeElection* is a dummy variable that equals one if the formal intervention of the insurer occurs one year before the election or, in the case of no failure, the end of the insurer's accounting year is one year before the elections. The following are omitted from the table to conserve space: the constant, insurance company characteristics (Size, Mutual, Group, and Ln(Number of States)), state characteristics (GSP per Capita), and the regulatory solvency (FAST) ratios . Year fixed effects and state fixed effects are included for all regressions. Columns (1)-(3) are discrete-time hazard models: (1) uses the full sample of firms and includes 11 regulatory solvency ratios; (2) uses the full sample of firms and includes 25 regulatory solvency ratios; and (3) uses the matched sample and includes 11 solvency ratios. Columns (4)-(6) are Cox proportional hazard models: (4) uses the full sample of firms and includes 11 solvency ratios; (5) uses the full sample of firms and includes 25 solvency ratios; and (6) uses the matched sample and includes 11 solvency ratios. p-value of Wald test that all variables other than state and time dummies are jointly zero is reported. Heteroskedasticity-robust standard errors, corrected for clustering at the firm level, are in parentheses. ***, **, and * indicate two-tailed statistical significance at 0.01, 0.05, and 0.10 levels.

Table 4
Elections and Regulatory Interventions: New Regulators, Business Cycles, and A.M. Best Ratings

	(1)	(2)	(3)
BeforeElection	-1.705 ***	-1.741 ***	-1.635 ***
	(0.473)	(0.474)	(0.466)
NewRegulator	0.255		
	(0.206)		
Cycle		1.165 **	
		(0.468)	
A.M. Best Rating			-0.902 ***
			(0.066)
Additional Controls:			
Firm Controls	Yes	Yes	Yes
State Controls	Yes	Yes	Yes
State Dummies	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes
Regulatory Solvency Ratios	11	11	11
p-value Wald test	0.000	0.000	0.000
Pseudo R^2 (%)	0.335	0.334	0.402
Log Likelihood Function Value	-1034.13	0.00	-928.86

The table presents discrete-time hazard analyses for formal regulatory interventions of insurers for the years 1989 to 2009. *BeforeElection* is a dummy variable that equals one if the formal intervention of the insurer occurs one year before the election or, in the case of no failure, the end of the insurer's accounting year is one year before the elections. *NewRegulator* is a dummy variable that is one if a new insurance commissioner is in office. *Cycle* is a business cycle indicator; it equals one if the National Bureau of Economic Research (NBER) designates that the economy is in a recession. *A.M. Best Rating* is an integer value ranging from 0 (C or lower rating, including firms not rated) to 6 (A+ or A++ rating, superior). The following are omitted from the table to conserve space: the constant, insurance company characteristics (Size, Mutual, Group, and Ln(Number of States)), state characteristics (GSP per Capita), regulatory solvency (FAST) ratios, and state and year indicators. *p-value* of Wald test that all variables other than state and time dummies are jointly zero is reported. Heteroskedasticity-robust standard errors, corrected for clustering at the firm level, are in parentheses. There are 37,819 firm-years, 3,260 firms, and 260 failed firms in the sample. ***, **, and * indicate two-tailed statistical significance at 0.01, 0.05, and 0.10 levels.

Table 5
Appointed Versus Elected Regualtors

	(1)	(2)	(3)	(4)
Before_Elect	-2.326 ***	-2.300 ***	-1.500 ***	-2.222 ***
	(0.602)	(0.602)	(0.414)	(0.589)
Before_Appt	-1.482 ***	-1.463 ***	-0.761 ***	-1.399 ***
	(0.478)	(0.478)	(0.229)	(0.466)
NewRegulator		0.248		
		(0.206)		
Cycle			0.478 ***	
			(0.153)	
A.M. Best Rating				-0.903 ***
				(0.066)
H_0: Before_Elect = Before_Appt (Chi-Square Statistic)	6.010	5.860	5.800	7.010
p-value	[0.014]	[0.016]	[0.016]	[0.008]
Additional Controls:				
Firm Controls	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Regulatory Solvency Ratios	11	11	11	11
p-value Wald test	0.000	0.000	0.000	0.000
Pseudo R^2 (%)	0.335	0.336	0.319	0.404
Log Likelihood Function Value	-1033.59	-1032.22	-1058.85	-926.97

The table presents discrete-time hazard analyses for formal regulatory interventions of insurers for the years 1989 to 2009. *Before_Elect* is a dummy variable that equals one if the insurer is domiciled in a state with an elected insurance commissioner and it is subject to formal intervention one year before the election or, in the case of no failure, the end of the insurer's accounting year is one year before the elections. *Before_Appt* is a similar variable for insurers domiciled in a state with an appointed insurance commissioner. *NewRegulator* is a dummy variable that is one if a new insurance commissioner is in office. *Cycle* is a business cycle indicator; it equals one if the National Bureau of Economic Research (NBER) designates that the economy is in a recession. *A.M. Best Rating* is an integer value ranging from 0 (C or lower rating, including firms not rated) to 6 (A+ or A++ rating, superior). The following are omitted from the table to conserve space: the constant, insurance company characteristics (Size, Mutual, Group, and Ln(Number of States)), state characteristics (GSP per Capita), regulatory solvency (FAST) ratios, and state and year indicators. *p-value* of Wald test that all variables other than state and time dummies are jointly zero is reported. Heteroskedasticity-robust standard errors, corrected for clustering at the firm level, are in parentheses. There are 37,819 firm-years, 3,260 firms, and 260 failed firms in the sample. ***, **, and * indicate two-tailed statistical significance at 0.01, 0.05, and 0.10 levels.

Table 6
Elections and Regulatory Interventions: Tightly Contested Elections

	(1)	(2)
BeforeElection	-1.054 **	
	(0.454)	
BeforeElection x Contested	-1.129 **	
	(0.548)	
Before_Elect		-1.810 ***
		(0.66)
Before_Elect x Contested		-0.013
		(0.857)
Before_Appt		-0.791 *
		(0.454)
Before_Appt x Contested		-1.616 **
		(0.715)
Additional Controls:		
Firm Controls	Yes	Yes
State Controls	Yes	Yes
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Regulatory Solvency Ratios	11	11
p-value Wald test	0.000	0.000
Pseudo R^2 (%)	0.325	0.327
Log Likelihood Function Value	-1011.53	-1008.98

The table presents discrete-time hazard analyses for formal regulatory interventions of insurers for the years 1989 to 2009. *Contested* is an indicator that equals one if the election is decided by a margin that is in the bottom quintile (20%) of the sample election results (a margin of approximately 4.8% or less). *BeforeElection* is a dummy variable that equals one if the formal intervention of the insurer occurs one year before the election or, in the case of no failure, the end of the insurer's accounting year is one year before the elections. *Before_Elect* is the *BeforeElection* variable for insurers domiciled in a state with an elected insurance commissioner. *Before_Appt* is the *BeforeElection* variable for insurers domiciled in a state with an appointed commissioner. The following are omitted from the table to conserve space: the constant, insurance company characteristics (Size, Mutual, Group, and Ln(Number of States)), state characteristics (GSP per Capita), regulatory solvency (FAST) ratios, and state and year indicators. *p-value* of Wald test that all variables other than state and time dummies are jointly zero is reported. Heteroskedasticity-robust standard errors, corrected for clustering at the firm level, are in parentheses. There are 37,819 firm-years, 3,260 firms, and 260 failed firms in the sample. ***, **, and * indicate two-tailed statistical significance at 0.01, 0.05, and 0.10 levels.

Table 7
Regulatory Governance

	(1)	(2)
AfterRBC	-5.862 ***	-5.537 ***
	(1.283)	(1.288)
BeforeElection	-3.144 ***	
	(0.700)	
BeforeElection x AfterRBC	1.955 **	
	(0.811)	
Before_Elect		-2.983 ***
		(0.935)
Before_Elect x AfterRBC		0.997
		(1.087)
Before_Appt		-3.279 ***
		(0.768)
Before_Appt x AfterRBC		2.351 ***
		(0.869)
Additional Controls:		
Firm Controls	Yes	Yes
State Controls	Yes	Yes
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Regulatory Solvency Ratios	11	11
p-value Wald test	0.00	0.00
Number of Firm-years	37,830	37,830
Number of Firms	3,261	3,261
Number of Failed Firms	260	260
Pseudo R^2	0.34	0.35
Log Likelihood Function Value	-1019.72	-1017.43

The table presents discrete-time hazard analyses for formal regulatory interventions of insurers for the years 1989 to 2009. *AfterRBC* is a dummy variable that equals one if the year is 1994 or greater. *BeforeElection* is a dummy variable that equals one if the formal intervention of the insurer occurs one year before the election or, in the case of no failure, the end of the insurer's accounting year is one year before the elections. *Before_Elect* is the *BeforeElection* variable for insurers domiciled in a state with an elected insurance commissioner. *Before_Appt* is the *BeforeElection* variable for insurers domiciled in a state with an appointed commissioner. The following are omitted from the table to conserve space: the constant, insurance company characteristics (Size, Mutual, Group, and Ln(Number of States)), state characteristics (GSP per Capita), regulatory solvency (FAST) ratios, and state and year indicators. p-value of Wald test that all variables other than state and time dummies are jointly zero is reported. Heteroskedasticity-robust standard errors, corrected for clustering at the firm level, are in parentheses. ***, **, and * indicate two-tailed statistical significance at 0.01, 0.05, and 0.10 levels.

Table 8
Characteristics of U.S. Property & Liability Insurer Insolvencies

	Obs.	Mean	Median	Std. dev.	Min.	Max.
Cost of insolvency						
All observations	234	0.712	0.173	2.085	0.000	27.916
Values truncated at 95th percentile	234	0.526	0.173	0.761	0.000	2.812
Values above 95th percentile are eliminated	223	0.414	0.160	0.579	0.000	2.812
Only insurers that access the guaranty funds	176	0.947	0.459	2.359	0.000	27.916
Firm Characteristics						
Size	234	16.768	16.685	1.523	12.862	22.629
Mutual	234	0.085	0.000	0.280	0.000	1.000
Group	234	0.397	0.000	0.490	0.000	1.000
Net premiums written to equity capital	234	2.243	1.902	1.702	-0.319	7.350
1 yr change in liquid assets	234	-0.045	-0.112	0.389	-0.586	1.509
Lines of business diversification	234	0.619	0.552	0.270	0.091	1.000
Ln(Number of States)	234	1.209	0.693	1.332	0.000	3.932
Catastrophe	234	0.108	0.000	0.213	0.000	1.000
Regulatory						
First event year (FEY)	234	1998.791	1998.000	6.366	1990.000	2011.000
FEY > 1993	234	0.637	1.000	0.482	0.000	1.000
Single	234	0.436	0.000	0.497	0.000	1.000
First event is rehabilitation	234	0.453	0.000	0.499	0.000	1.000
First event is conservation	234	0.235	0.000	0.425	0.000	1.000
First event is liquidation	234	0.308	0.000	0.463	0.000	1.000

This table provides summary statistics for U.S. property-liability insurers that fail between 1989 and 2011. The cost of insolvency is calculated as the net guarantee fund assessments in 2012 divided by the firm's pre-insolvency assets. *Size* , *Mutual* , and *Group* are defined in Table 1. *Net premiums written to equity capital* is a measure of leverage in the insurance industry. *Liquid assets* is the percent of total assets in stocks, investment grade bonds, and cash. *Lines of business diversification* is the sum of the squares of the percentages of premiums written by line, for the 26 lines of insurance. *Ln(Number of States)* is the natural logarithm of the number of states where the insurer writes business. *Catastrophe* is the percent of total premiums written in catastrophe prone lines and areas. *First event year* is the year of formal regulatory action against the insurer for conservation of assets, rehabilitation, or liquidation. *FEY > 1993* is an indicator variable set equal to one if the first event year is greater than 1993 and zero otherwise. *Single* is an indicator if the firm conducts business in only one state. The remaining three variables record whether the first formal regulatory action against the insurer was a rehabilitation, conservation, or liquidation.

Table 9
The Effect of Electoral Forbearance on the Cost of Insolvency

	(1)	(2)	(3)	(4)
ForBear	0.476 **		0.537 ***	
	[0.358]		[0.397]	
	(0.240)		(0.192)	
ForBear_Elected		0.553 *		0.704 ***
		[0.416]		[0.521]
		(0.302)		(0.243)
ForBear_Appt		0.348		0.273
		[0.262]		[0.202]
		(0.387)		(0.303)
H_0: ForBear_Elected = ForBear_Appt				
(F-Statistic)		0.18		1.26
[p-value]		[0.672]		[0.264]
Regulatory Controls	Yes	Yes	Yes	Yes
Firm Controls	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Number of Firms	234	234	223	223
Pseudo R^2	0.298	0.298	0.333	0.336
Log likelihood	-201.710	-201.620	-153.202	-152.579

This table presents the relationship between electoral forbearance and the cost of insolvency. The dependent variable is the cost of insolvency, which is calculated as the net guarantee fund assessments in 2012 divided by the firm's pre-insolvency assets. Columns (1) and (2) report Tobit maximum likelihood estimates for all the observations, but the observations with a cost of insolvency above the 95th percentile are truncated to the 95th percentile. Columns (3) and (4) report Tobit maximum likelihood estimates for the observations with a cost of insolvency at or below the 95th percentile. *ForBear* is an indicator variable that is set equal to one if the firm is under regulatory scrutiny before the election but the regulatory intervention occurs after the election. *ForBear_Elect* is *ForBear* for elected regulators, and *ForBear_Appt* is for the appointed regulators. Each regression also includes state and year dummies, firm characteristic controls (size, mutual, group, leverage, liquidity, line of business diversification, number of states the firm does business, and the percent of premiums written in catastrophe prone lines and areas), and regulatory controls (indicators for prompt corrective action, single state insurers, and for the type of first regulatory action against the insurer (rehabilitation or conservation)). Unconditional marginal effects are reported in brackets below the coefficient and standard errors are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.