# Campaign Contributions and Pay-to-Play: Evidence from U.S. Health Insurers

**Jingshu Wen**
Said Business School, University of Oxford
Jingshu.Wen.DPHIL@said.oxford.edu
**Tonglin Zhang**
The University of Hong Kong
tonglinz@hku.hk

## Abstract

We study whether campaign finance generates firm-specific rents in the U.S. health insurance sector. Using hand-collected state pay-to-play laws and insurer-state-year level data on political contributions, premiums, and enrollment, we exploit two legal shocks that change the expected return to political exchange: staggered state pay-to-play laws and the Supreme Court's 2016 decision in McDonnell v. United States. In difference-in-differences analyses, pay-to-play laws reduce insurers' corporate PAC and candidate-directed contributions and lower Medicare premiums written by donating insurers. They also increase regulatory guidance and reduce insurer profitability, consistent with lower public program rents and less supervisory forbearance. In contrast, after McDonnell weakened federal anti-corruption deterrence, insurer donations and public program premiums increased more in high-corruption states. States adopting pay-to-play laws also experience lower Medicaid spending per enrollee and household insurance costs. The evidence suggests that campaign finance can convert political access into economic rents in insurance markets and that legal design affects the private rents and public costs.

*Keywords*: Campaign contribution; Health insurance; Pay-to-play; Rent-seeking.
*JEL Codes:* D72, D73, H75, I10, I13

## 1. Introduction

How does campaign finance convert economic power into political advantage in regulated markets? In sectors where firms depend on public programs and administrative discretion, political influence need not operate through legislation alone. Elected officials and their appointees can affect contract awards and renewals, licensing, pricing review, regulatory enforcement, and supervisory intensity. These channels create scope for campaign contributions to function as investments in firm-specific regulatory advantages. This paper studies this mechanism in U.S. health insurance, a large regulated sector in which private insurers receive substantial revenue from public programs and interact repeatedly with state officials.

Health insurance is an especially useful setting for studying this question. Health insurers operate in state-specific markets and derive a large proportion of their revenue from public programs, primarily Medicare and Medicaid.[1] Private insurers are subject to state oversight over licensing, solvency, market conduct, and pricing review. The health insurance industry is also among the largest corporate donors to political campaigns. These features make the sector well suited for examining whether campaign contributions translate into favorable regulatory or public program outcomes. If political giving helps health insurers secure firm-specific advantages, such as public program business and softer supervisory treatment, then campaign finance may distort both public and private insurance markets, contributing to the lack of health insurance affordability in the US.[2]

Campaign contributions may generate insurer-specific rents through two complementary mechanisms. The first is public program allocation. Politically connected insurers may obtain more favorable access to government-financed business through contract awards, renewals, participation rules, or pricing decisions. State officials can also indirectly affect access to public program business via licensing decisions. The second mechanism is supervisory forbearance. Politically connected insurers may face fewer financial examinations, softer corrective

[1] Data from the National Association of Insurance Commissioners (NAIC) (2025) shows that around two thirds of U.S. health insurers' premium revenue came from public programs in 2024.

[2] According to KFF polling in 2024, around 50% of U.S. adults find health insurance and health care unaffordable.

guidance, or weaker remediation pressure. These mechanisms are plausible because elected officials can influence appointments, agency priorities, budgets, and interagency coordination even when they do not personally sign contracts or issue regulatory orders.

We test these predictions by combining political contribution records with insurer-state-year data on premiums, enrollment, and supervisory outcomes from 2002 to 2022. Throughout the paper, we use premiums to refer to premiums written, or insurers' underwriting revenue, and we use premium rates or rates to refer to price measures, defined as premiums per enrollee. The granularity of the data allows us to link campaign spending to program-level economic outcomes for each insurer in each state. We find that higher state-level insurer donations are positively associated with total, Medicare, and Medicaid premiums, as well as with the corresponding enrollment. The magnitudes are economically meaningful. A 10% increase in state-year donations is associated with about 1.2% higher total premiums, 1.4% higher Medicare premiums, and 0.9% higher Medicaid premiums, together with roughly 1.0% to 1.3% higher enrollment across the corresponding lines.

For identification, we exploit two legal shocks that change the expected return to pay-to-play. The first is the staggered adoption of statewide pay-to-play (PTP) laws. These laws raise the legal and reputational cost of exchanging campaign money for public business or regulatory favors. The second institutional change is the Supreme Court's 2016 ruling in McDonnell v. United States, which works in the opposite direction by weakening federal anti-corruption deterrence for access-based exchanges. This design allows us to ask whether political giving responds to legal constraints and whether changes in those constraints affect public-program outcomes and supervisory oversight for insurers.

Using hand-collected state-level pay-to-play (PTP) laws, we estimate staggered difference-in-differences (DID) models in which treatment begins when a statewide PTP law is legally in force. Event-time plots show broadly similar pre-adoption trajectories for treated and never-treated states, with divergence emerging after adoption. We find that PTP laws reduce targeted, business-linked giving rather than all insurer political activity. In the main Callaway and Sant'Anna estimates, PTP adoption significantly reduces corporate PAC giving,

Republican-directed corporate PAC giving, local corporate PAC giving, and candidate-directed giving. PTP law adoption reduces insurers' donations to individual candidates by 28.1%.

On the revenue side, PTP adoption reduces donating insurers' Medicare premiums by about 49.5% in the full sample and by about 63.1% in the early-adopting cohort. Placebo tests show no comparable effects on donations in states where insurers have no premium business and no comparable effects on premiums for non-donating insurers.

Our heterogeneity tests further support a transaction-specific interpretation. After PTP law adoption, small insurers, or those with assets below the sample median, experience significantly larger relative declines than large insurers in donations and premiums. Relative to large insurers, small insurers experience additional declines of about 39.5% in total donations, 30% in candidate-directed donations, 20.8% in total premiums, 13.8% in Medicare premiums, and 5.6% in Medicaid premiums. The pattern is consistent with political rents being more valuable for firms with less financial slack and more concentrated public-program exposure. Smaller insurers are likely to value political rents more highly because access to public program cash flows and relief from supervisory pressure can be especially important for firms with less financial slack, while broader policy lobbying is too resource-intensive for small insurers. If campaign contributions are risky investments in political access, smaller insurers should have stronger incentives to exchange them for specific private benefits. In addition, smaller insurers' public program business is more concentrated in a narrow set of states than that of larger insurers.

On the oversight margin, after states adopt PTP laws, insurers in treated states receive less lenient regulation. Insurers in treated states receive more regulatory guidance, and the share undergoing financial examinations also rises in the early-adopting cohort. Pay-to-play law adoption is associated with a 17- to 24-percentage-point increase in regulatory guidance and a 2.3-percentage-point increase in the frequency of financial examinations in the early-adopting sample. These patterns are consistent with reduced supervisory forbearance when PTP laws increase the cost of favor exchanges.

Insurers with greater exposure to PTP-law states also exhibit deteriorating financial performance, including lower ROA, lower operating ROE, lower cash ratios, and higher claim

ratios. Moving from zero to full exposure to PTP law states is associated with about a 5 percentage point decline in ROA and a 16 percentage point decline in operating ROE in the full sample, with even larger declines before 2016. These results are consistent with rent compression when legal constraints reduce the return to political exchange. Loose campaign finance and procurement regulations may increase insurers' profitability and lead to operating slack via favorable access to public program business and lenient oversight. Stronger legal constraints curb the political rents and operating slack.

Our DID tests based on McDonnell v. United States provide complementary evidence from a weakening of federal anti-corruption deterrence. After the Supreme Court's 2016 decision in McDonnell v. United States, insurer donations and public program revenues rise disproportionately more in high-corruption states than in low-corruption states. The post-2016 interaction estimates imply about 6%-13% higher insurer donations, 64.9% higher Medicare premiums, and 26% higher Medicaid premium rates in high-corruption states. A supplemental triple-difference test shows that the post-McDonnell increase in corporate PAC giving is the largest in high-corruption states without strong pre-2016 PTP laws. These results suggest that weaker federal anti-corruption constraints increase the return to political giving most where corruption risk is high and state legal constraints are weak.

These private benefits appear to impose broader social costs. We find that states that adopt PTP laws experience lower Medicaid spending per enrollee and lower family deductibles, with family premiums also falling in early-adopting states. The largest cost reductions are concentrated in states that adopted PTP laws before 2016, or early-adopting states. In early-adopting states, Medicaid spending per enrollee falls by about 14.6% (roughly $1,000 at the sample mean), family deductibles fall by about 39.5% (roughly $1,100), and family premiums fall by about 11.1% (roughly $1,960). In the full sample, Medicaid spending per enrollee and family deductibles also decline significantly. These cost reductions are accompanied by fewer cost-related barriers to care, fewer reports of no doctor visit, and lower suicide rates after PTP adoption. Taken together, the results suggest that campaign finance can raise public and household health-insurance costs by enabling politically mediated rents, and that targeted PTP restrictions can reduce those rents and improve affordability and access.

Our paper makes three contributions. First, we contribute to research on corporate political activity and regulatory capture by showing that campaign finance can facilitate firm-specific regulatory rents. Prior work shows that political connections and campaign contributions can affect government contract allocation, government funding during crises, enforcement intensity, and firm value (e.g., Butler, Fauver, and Mortal, 2009; Duchin and Sosyura, 2012; Akey, 2015; Fulmer, Knill, and Yu, 2023; Cornaggia, Cornaggia, and Israelsen, 2023). We extend this literature to U.S. health insurance, where firms depend heavily on state-administered public programs and repeatedly interact with officials who exercise regulatory discretion. The mechanism of regulatory capture in this paper differs from lobbying and revolving doors in that politicians advance their political careers by offering firm-specific regulatory favors via public-program allocation and supervisory forbearance. By converting economic power into political advantage, campaign finance is an underrecognized source of distortion in public programs and regulated insurance markets, which may contribute to the high costs and unequal access to health care in the United States.

Second, we show that legal design and institutional quality shape the returns to rent-seeking. State PTP laws raise the cost of seeking political rents, thereby reducing insurer donations, lowering public premiums written by politically active insurers, increasing regulatory guidance, and reducing profitability among insurers with higher PTP law exposure. In contrast, weaker anti-corruption enforcement after McDonnell lowers the expected cost of access-based exchange, leading to larger increases in insurer donations and public program premiums in high-corruption states. These contrasting results help to distinguish the political rent interpretation from alternative explanations based on persistent differences across insurers, states, or markets. By showing that legal constraints affect insurers' political and business activity, our findings complement Oh, Sen, and Tenekedjieva (2025), who show that regulatory frictions shape insurers' pricing and market participation. Our results also suggest that legal design affects whether campaign finance weakens accountability and supervisory capacity in regulated markets with substantial administrative discretion.

Third, the paper contributes to the debate over whether campaign contributions are primarily ideological expressions or investments in political access. Prior work shows that

political connections and contributions can generate private benefits for firms, including higher stock returns, political access, regulatory leniency, bailouts, and preferential resource allocation (Adelino and Dinc, 2014; Akey, 2015; Ayyagari, Knill, and Syvrud, 2024; Butler et al., 2009; Claessens, Feijen, and Laeven, 2008; Cooper, Gulen, and Ovtchinnikov, 2010; Duchin and Sosyura, 2012; Faccio, Masulis, and McConnell, 2006; Fazekas, Ferrali, and Wachs, 2023; Fulmer, Knill, and Yu, 2023; Kalla and Broockman, 2016; Schoenherr, 2019). However, other studies argue that corporate donations often yield limited financial gains or reflect other motives (Aggarwal, Meschke, and Wang, 2012; Ansolabehere, de Figueiredo, and Snyder, 2003; Fouirnaies and Fowler, 2022; Fowler, Garro, and Spenkuch, 2020; Goldman, Rocholl, and So, 2009). Our evidence supports an investment motive in settings with substantial public program revenue, repeated regulatory interaction, and weak legal constraints.

The remainder of the paper proceeds as follows. Section 2 presents the background and hypotheses. Section 3 describes the institutional setting, data, variables, and empirical strategy. Section 4 reports the main evidence on political giving and public-program premiums. Section 5 examines oversight and insurer financial performance. Section 6 presents corroborative evidence based on McDonnell v. United States. Section 7 studies the effects of PTP on health care affordability, access, and health outcomes. Section 8 reports placebo and robustness tests. Section 9 concludes.

## 2. Background and hypothesis development

U.S. health care is among the most expensive in the world, whose high and rising costs reflect income growth, medical progress, transfer-program expansion, market structure, and institutional frictions (Cebul et al., 2011; Chan and Dickstein, 2019; Chatterji et al., 2024; Dafny, 2010; Dafny, Duggan, and Ramanarayanan, 2012; Decarolis, 2015; Duggan, 2004; Dunn et al., 2024; Hall and Jones, 2007; Hamilton et al., 2018; Jones, 2002; Tebaldi, 2024; Tilipman, 2022). Regulations governing pricing, solvency, and coverage also shape premiums, enrollment, plan variety, market concentration, and counterparty risk in insurance markets (Aizawa and Ko, 2023; Curto, 2023; Dickstein et al., 2015; Oh, Sen, and Tenekedjieva, 2025; Sastry, Sen, and Tenekedjieva, 2023). We examine whether campaign finance introduces an

additional distortion into this environment by affecting the allocation of public program business and the intensity of regulatory oversight.

The health insurance industry provides a useful setting for studying how campaign finance may generate firm-specific rents in regulated markets. The sector combines large public program revenues, substantial administrative discretion, and repeated interactions between insurers and public officials. High industry concentration and opaque pricing increase the likelihood of political rent-seeking (Mauro, 1998; Shleifer and Vishny, 1993). In addition, a substantial share of insurer business comes from Medicaid managed care, Medicare Advantage, and Medicare Part D, where large public expenditures and substantial administrative discretion are central features.[3] These programs create large revenue opportunities, while participation and profitability depend on procurement and other decisions.

State officials can influence insurers' revenues and profitability by procuring and monitoring Medicaid Managed Care contracts, establishing plan requirements, evaluating plan performance, and participating in rate-setting processes subject to federal rules. These decisions often involve subjective assessments of plan credibility, administrative capacity, network sufficiency, and proposal reasonableness. Medicare Advantage and Part D are federally administered. Nevertheless, insurers participating in Medicare-related markets remain subject to state Managed Care Organizations (MCOs) licensing, solvency oversight, network enforcement, dual-eligible coordination, and market conduct rules.[4]

State insurance departments also monitor insurer solvency, conduct financial and market conduct examinations, review pricing in relevant markets, and issue corrective guidance. Even when statutes constrain agency decisions, regulators retain discretion over implementation,

[3] According to data from the Centers for Medicare & Medicaid Services (CMS), in 2022, around 75% of 80.9 million Medicaid enrollees received their coverage through private Managed Care Organizations (MCOs) at a median annual cost of about $7,800 per enrollee, which amounts to around $472 billion in total contract value.

[4] MCO licenses are prerequisites for eligibility as contractors in various government programs, including Medicaid, Medicare Part C and D, and Medigap. State officials can select Medicaid contractors and set capitation rates, but they can only indirectly influence Medicare Part C, Medicare Part D, and Medigap premiums via licensing, market environment, and Medicaid coordination. According to State Effective Rate Review Programs from Centers for Medicare & Medicaid Services, premium increases of 15% or more for individual or small group products require regulatory reviews.

enforcement intensity, timing, remediation, and follow-up. These institutional features create scope for firm-specific advantages that need not appear in legislation. A politically connected insurer may benefit from favorable access to public program business, a more accommodating interpretation of program requirements, delayed remediation, or softer supervisory treatment.

Campaign finance is relevant because elected officials and their appointees can influence many decisions that affect insurer revenues and costs. State governors appoint health department heads and insurance commissioners in most states, while voters directly elect insurance commissioners in a few states (such as California, Delaware, North Carolina, Georgia, and Oklahoma). Officeholders with authority over insurance regulation have strong incentives to raise funds, as election success depends heavily on campaign spending (Jacobson and Carson, 2019).[5] Even when elected officials do not personally sign contracts or issue corrective orders, they can still shape appointments, budgets, agendas, and interagency coordination. For example, governors can connect insurers with appointed officials, such as the head of state health departments. Campaign finance can therefore target politicians with usable jurisdiction over the firm's economic interests. State officials' demand for campaign money may be especially high in recent decades. Citizens United v. FEC in 2010 loosened soft money restrictions, after which campaign spending skyrocketed, with insurers among the top political donors.[6] Rising campaign costs also increase politicians' demand for contributions, making regulated industries with high-stakes administrative exposure natural candidates for political exchange.

This mechanism of favor exchange is consistent with regulatory capture and political rent-seeking, in which firms seek private benefits from public officials who control valuable administrative decisions (Stigler, 1971; Laffont and Tirole, 1991). Existing evidence shows that insurance regulatory discretion responds to political incentives (Liu and Liu, 2024; Tang, 2022),

[5] Campaign spending is a critical factor for election success. According to OpenSecrets, the top spending candidates win 71.43%-97.54% of the races in U.S. House or Senate elections during 2000-2024. See https://www.opensecrets.org/elections-overview/winning-vs-spending

[6] According to OpenSecrets, the cost of Congressional races increased from $3.6 billion in 2010 to $8.9 billion in 2022. Data from OpenSecrets also shows that the insurance industry is consistently the top campaign donor in recent election cycles, as on this website: https://www.opensecrets.org/political-action-committees-pacs/industry-detail/F/2022

and insurers donate more to candidates from the same party as incumbent insurance regulators (Born, Karl, and Powell, 2024).

The relevant insurer-specific rents can arise through two complementary channels. The first is public program allocation. Politically active insurers may receive more favorable access to public program business through contract awards and renewals, pricing decisions, or participation rules. These advantages should appear in higher public program premiums written or enrollment, especially in business lines where public payments and administrative discretion are central. The second channel is supervisory forbearance. Politically active insurers may face less intensive examination, delayed remediation, weaker corrective guidance, or more accommodating supervisory treatment. Such forbearance lowers regulatory costs and may allow insurers to preserve margins, expand business, or avoid costly intervention. If political giving helps insurers secure either type of rent, then legal constraints that raise the cost of quid pro quo exchange should reduce targeted giving, weaken favorable public-program outcomes, and strengthen supervisory oversight.

Institutional constraints should therefore shape the return to political giving. State PTP laws that link campaign contributions to contracting eligibility, contract award or renewal, or disclosure increase the legal and reputational cost of influence-seeking. Although many states lack effective PTP laws for public officials (Bromberg, Hartley, and Mohammed-Spigner, 2017), several states have adopted targeted PTP laws that tie campaign contributions to eligibility for public contracts or related public business. Some laws restrict contributions by firms seeking or holding public contracts, while others require disclosure or impose timing restrictions around procurement and contracting. Ban-based rules can directly limit targeted giving, while disclosure-based rules can increase transparency, reputational risk, and enforcement exposure. Both types of rules can make it harder to convert campaign contributions into transaction-specific regulatory favors. By contrast, if deterrence of corruption weakens, the return to political giving should rise most in states with lower institutional quality and more persistent corruption.

The value of political rents may also differ across firms. Smaller or more financially constrained insurers are likely to place greater value on politically mediated rents, because

public-program business and supervisory decisions can materially affect their scale, liquidity, and operating margins, and because they are less able to absorb adverse contract or regulatory shocks. As political contributions are highly risky investments (Gordon, Hafer, and Landa, 2007), higher expected returns incentivize firms to gamble on investments in political access. The higher returns to small insurers increase the likelihood of rent-seeking. In addition, small insurers lack the resources to lobby for industry-wide policy changes, making targeted campaign contributions a more attractive instrument of political access. Therefore, smaller insurers should have stronger incentives to invest in political connections, especially when enforcement is weak and administrative rents are large.

If campaign finance reallocates public-program business toward politically connected insurers or weakens supervisory discipline, the result may be higher public spending, weaker procurement discipline, protected incumbents, and higher household insurance costs. Previous studies show that political rent-seeking leads to negative social externalities, such as financial losses from misallocation of public funding (Duchin and Sosyura, 2012; Khwaja and Mian, 2005; Faccio, Masulis, and McConnell, 2006). Conversely, if stricter anti-PTP rules compress these rents, then insurers that rely more heavily on politically mediated advantages should experience weaker financial performance, while taxpayers and households may benefit through lower costs and better access to care.

Based on the discussion above, we develop the following hypotheses that guide the empirical analysis.

H1 (Public-program allocation). Insurers exchange political contributions for public program premiums and enrollment.

H2 (Supervisory forbearance). Political giving is associated with softer regulatory oversight.

H3 (Legal enforcement and institutions). The relationship between political contributions and economic rent is stronger where anti-corruption enforcement and institutional constraints are weaker.

Together, these hypotheses test whether campaign finance facilitates a transactional form of political influence in health insurance and whether legal constraints limit that influence. The

effects should be stronger for smaller or more financially constrained insurers, for whom public-program business and supervisory decisions are more economically material.

## 3. Data and Empirical Strategy

### 3.1. Data

Our sample period is between 2002 and 2022. We use the political contribution data from the Database on Ideology, Money in Politics, and Elections (DIME), provided by Bonica (2023). We aggregate insurer-linked contribution records to the state-year, insurer-state-year, or insurer-year level, depending on the specification. Our insurance operating and financial data come from NAIC annual statements accessed through S&P Capital IQ Pro. These files provide insurer-state-year premiums and enrollment by line of business, as well as insurer-year financial statements. We also use insurer supervisory data and aggregate them to the state-year level.

To measure institutional constraints, we use public-corruption data from the U.S. Department of Justice (DOJ)'s annual Report to Congress on the Activities and Operations of the Public Integrity Section, along with supplementary data on corruption from the Institute for Corruption Studies. We hand-collect state-level pay-to-play (PTP) laws from publicly available statutory and regulatory sources. The primary sources are state statute databases maintained by state legislatures and the websites of state ethics commissions, election agencies, procurement authorities, and related administrative bodies. We also use public policy databases, including those maintained by the National Conference of State Legislatures (NCSL), to support cross-state comparison and verification. Using these sources, we code PTP law adoption years for the strong and general statewide regimes as well as sector-specific and municipal regimes. We treat the District of Columbia as a separate jurisdiction.

For state economic conditions, we use real GDP, disposable income, and GDP growth data from the Bureau of Economic Analysis, and we use unemployment data from the Bureau of Labor Statistics. For state-level health care cost and access outcomes, we use family premium and family deductible costs from the Medical Expenditure Panel Survey–Insurance Component, Medicaid per capita cost from the CMS State Health Expenditure Accounts, the

percentage of uninsured individuals from the U.S. Census Bureau's Small Area Health Insurance Estimates (SAHIE), and cost barrier to accessing health care data from the Behavioral Risk Factor Surveillance System. We also use state-level suicide data from CDC WONDER provided by the Centers for Disease Control and Prevention, and health care affordability survey data from the Kaiser Family Foundation (KFF).

Sample sizes vary by specification and data availability. The insurer-state-year panels contain up to about 483,000 observations, and the insurer-year financial panel contains 5,650 observations. Specifications using supervisory outcomes, health care costs, or downstream access and health outcomes have smaller samples because those variables are not available for all state-years.

### 3.2. Variables

#### 3.2.1. Political Donations

The recipient committee reports federal contributions to the FEC, state contributions to state election offices or secretaries of state offices, and local contributions to local election agencies. Based on the types of recipients, political donations can be divided into donations to candidates or political committees, donations to the Democratic or Republican Party, and donations supporting federal or local elections. Political committees include candidate committees, political party committees, political action committees (PACs), and other types of committees. Donations to candidates refer to donations to candidates' official campaign committees. In contrast, donations to political committees refer to contributions made to other types of political committees, such as party committees, super PACs, hybrid PACs, and joint fundraising committees, whose purpose is not to support a single candidate. By donor type, political donations include those made by individuals (employees) and companies (corporate PACs).

We use different donation categories and also aggregate all donations across different donor and recipient categories to derive total donations. In addition, we aggregate individual and corporate giving by recipient type and electoral level. Depending on the specification, these

donation variables are measured at the state-year, insurer-state-year, or insurer-year level. All political-donation variables are transformed as ln(x+1).

### 3.2.2. State-level Corruption

We follow Glaeser and Saks (2006) to construct a Corruption Conviction Index based on the DOJ's annual Report to Congress on the Activities and Operations of the Public Integrity Section. These reports provide state-by-year counts of federal, state, and local public officials convicted in federal court of corruption-related offenses, including bribery, extortion, fraud, conflicts of interest, campaign-finance violations, and obstruction of justice. We compile annual conviction counts for each state and merge them with U.S. Census Bureau population estimates. We then construct the corruption index as the number of federal public corruption convictions per 100,000 residents in each state-year, which allows for comparisons across states of different sizes. We define a dummy variable, High DOJ Corruption, which equals 1 if the average DOJ conviction rate for a state is above the sample median and 0 otherwise.

### 3.2.3. Control Variables

We control for state characteristics that may affect the demand for health insurance or political donations. As higher disposable income increases individuals' ability to afford private or supplemental health insurance and to make larger or more frequent contributions, we control for per capita disposable personal income, defined as total disposable personal income divided by total midyear population estimates of the Census Bureau in each state in each year. We also control for state-level employment, which affects health insurance premiums through the composition of insurance demand, as higher employment rates mean more employer-sponsored insurance and lower demand for Medicaid or subsidized ACA (Affordable Care Act) marketplace plans. Higher employment may be associated with higher or lower levels of political engagement (Aalen et al., 2024; Österman and Brännlund, 2024). Finally, we control for real GDP and GDP growth, which affect a state's health care infrastructure, public health investment, and economic security. Insurers in high-income states may also donate more to politicians due to the higher potential gains from higher pricing in states with strong insurance

demand. In summary, our baseline state-year macroeconomic controls are real GDP, disposable income, GDP growth, and unemployment. Specifications that additionally control for market structure include the Herfindahl-Hirschman Index (HHI) of state insurance markets, which we compute from insurer premium shares within each state-year. We also control for state population, because larger populations provide insurers with larger markets, especially for government contracts with fixed per-capita pricing, and because population size also affects monitoring intensity and governance quality (Campante and Do, 2014; Campante, Do, and Guimaraes, 2019).

In addition to state characteristics, we also control for insurer characteristics. Firms' profitability affects their ability to donate, so we control for return on assets, or ROA. We also control for financial leverage, calculated as total debt divided by total assets. Insurers with higher leverage tend to face greater financial and regulatory risk, which may incentivize them to make political contributions to gain access and secure government contracts. Besides risk exposure, leverage ratios also affect insurance companies' capital via capital adequacy requirements and cost of capital via credit ratings, which could affect insurers' pricing and premiums.

The detailed variable definitions are in Table A1 of Appendix A.

### 3.3. Institutional setting and identification strategies

Our first identification strategy exploits state PTP laws. We classify a state as having a PTP regime if its law links political contributions to eligibility for public contracting or other covered public business through at least one of three channels: bans or tight caps on contributions during covered procurement periods, contribution-triggered consequences for contract eligibility, award, or renewal, or contractor-specific disclosure requirements. We distinguish between ban-based regimes, which prohibit or sharply restrict contributions by current or prospective contractors, and disclosure-based regimes, which allow contributions but require covered firms to report them. States with contractor contribution bans include Connecticut and New Jersey, and states with disclosure-based regimes include Maryland and

Pennsylvania. Treatment begins in the first year a qualifying law is legally in force and ends upon repeal or judicial invalidation.

We code PTP laws in three steps. First, we identify the relevant statutory and regulatory provisions in state legal databases and record each law's effective date, scope, covered actors, and enforcement mechanism. Second, we review guidance from state ethics commissions, election agencies, procurement authorities, and related administrative bodies to verify how the rule operates in practice. Third, we cross-check these determinations against public-policy databases and institutional summaries to ensure consistent coding across states.

Appendix Tables A2 and A3 summarize the institutional coding underlying our identification strategy. Table A2 identifies the statewide PTP regimes used in the main treatment definition, distinguishing strong contractor-ban states from broader general or disclosure-based regimes and reporting the adoption and abolition dates that determine treatment timing. Table A3 catalogs narrower sector-specific and municipal-specific rules that are excluded from the baseline specification and used only in robustness tests.

The staggered timing of adoption provides the core source of identification. These laws raise the legal, disclosure, and reputational costs of converting campaign money into firm-specific regulatory favors. Using PTP adoption as a shock to the cost of converting campaign money into insurer-specific rents, we use a staggered difference-in-differences (DID) design to test whether tighter transaction-specific constraints attenuate the relationship between insurers' political giving and rent margins. Because qualifying laws take effect in different years across states, we compare outcomes in adopting states before and after adoption with outcomes in states that are never treated or not yet treated in the same years. The donation regressions test whether tighter transaction-specific legal constraints reduce targeted political giving. The premium regressions examine whether premiums written by donating insurers fall after adoption, and the oversight regressions test whether supervisory outcomes become stricter after adoption.

Our second identification strategy exploits McDonnell v. United States (2016) as a nationwide shock to the expected cost of access-based exchanges. In McDonnell, the Supreme Court narrowed the set of actions that qualify as "official acts" in federal corruption

prosecutions, thereby raising the evidentiary threshold for cases involving gifts, meetings, introductions, and other forms of access rather than discrete, formal governmental decisions. Consistent with this interpretation, Aggarwal and Litov (2023) document a decline in corruption cases and an increase in the average bribe size after McDonnell. The ruling is especially relevant in our setting because governors and other elected officials can affect insurers without personally signing contracts or issuing regulatory orders. They can shape appointments, agenda-setting, interagency coordination, and informal access to insurance commissioners, Medicaid officials, and procurement staff. We therefore interpret McDonnell as a reduction in the federal enforcement risk for indirect or access-based quid pro quo arrangements.

The effect of this shock should vary with state institutional quality. We capture this heterogeneity using predetermined cross-state corruption intensity, which reflects persistent differences in enforcement capacity, media scrutiny, and political norms rather than post-McDonnell outcomes. States with stronger institutions can partially offset the narrowing of the federal standard through more active state prosecutors, broader state-law definitions of corruption, or stronger local oversight. In contrast, federal enforcement is more likely to have served as the binding deterrent to borderline quid pro quo conduct in states with weaker legal institutions.[7] If so, McDonnell should produce a larger increase in the return to political giving in high-corruption states. Because the shock is national, identification comes from differential exposure to the post-2016 decline in the risk of federal anti-corruption enforcement. We therefore estimate DID specifications that compare insurers' political giving, public-program revenues, and supervisory outcomes in high- and low-corruption states before and after 2016.

[7] For example, South Dakota and Massachusetts have low corruption index values, and the two states adapt to McDonnell v. United States by strengthening local laws or using broader definitions of corruption. In 2016, South Dakota voters approved the South Dakota Anti-Corruption Act, which includes campaign finance reforms and an ethics commission, though the measure was repealed in 2017. In Massachusetts, state prosecutors mitigate the restrictive impact of the McDonnell ruling by using broader statutory language to encompass a wider range of corrupt activities. In contrast, states with high corruption levels tend to align their prosecutorial strategies with narrowed federal standards after McDonnell v. United States, as seen from the case of Commonwealth v. Veon in Pennsylvania and that of former New York Assembly Speaker Sheldon Silver.

Under a PTP mechanism, the effects should be stronger where discretion is greatest (public programs), institutions are weakest (high-corruption states and the post-McDonnell period), and political rents are most valuable (small insurers). By contrast, if campaign contributions primarily reflect broad ideology or general firm expansion, the estimates should be insensitive to transaction-specific legal shocks, less concentrated in public insurance programs, and less amplified by corruption and firm scale.

### 3.4. Empirical tests

#### 3.4.1 Baseline model

We first test whether health insurers' contributions are associated with higher premiums and enrollment with a reduced-form association model:

$$Y_{ist} = \beta \ln(\text{Donations}_{st}) + \Gamma' X_{st} + \tau_t + \varepsilon_{ist} \qquad (1)$$

In this paper, i denotes insurer, s denotes state, and t denotes year. Donation variables are in natural logarithm form, or ln(donation+1). We cluster standard errors at the state level. In Equation (1), the dependent variable $Y_{ist}$ is alternatively the log of total premiums, Medicare premiums, Medicaid premiums, total enrollment, Medicare enrollment, or Medicaid enrollment for insurer $i$ in state $s$ and year $t$. The main explanatory variable, $\ln(\text{Donations}_{st})$, is the logarithm of total political donations in state $s$ and year $t$. The control vector $X_{st}$ includes state real GDP, per capita disposable income, GDP growth, and the unemployment rate. The model includes year fixed effects, $\tau_t$, and is estimated on the pre-2016 subsample.

This specification provides a descriptive benchmark for H1. It asks whether insurers operating in states with higher levels of political giving also receive higher premiums and enroll more members, especially in Medicaid and Medicare, where public discretion is most relevant to the proposed rent-allocation mechanism. Because Total Premium combines both private and public lines of business, we also examine Medicare and Medicaid premiums and enrollment separately. The baseline association model pools all states in the pre-2016 sample.

3.4.2 Staggered DID based on PTP law adoptions

We next examine whether PTP laws reduce insurer political giving and the premiums associated with political influence using difference-in-differences (DID) tests.

Because states adopt PTP laws at different times, we use the Callaway and Sant'Anna (2021) staggered-adoption DID estimator, which is robust to treatment timing heterogeneity. Let $G_s$ denote the first year in which a PTP law is legally in force in state $s$, with $G_s = \infty$ for never-treated states. Thus, $G_s = g$ identifies the cohort first treated in year $g$, and $G_s > t$ identifies states that are not yet treated in year $t$.

For donation outcomes, we estimate group-time average treatment effects and aggregate them as follows:

$$ATT^D_{g,t} = \mathbb{E}\big[D_{st} - D_{s,g-1} \mid G_s = g\big] - \mathbb{E}\big[D_{st} - D_{s,g-1} \mid G_s > t\big], t \geq g,$$
$$\theta^{CS}_D = \sum_{g \in \mathcal{G}} \sum_{t \geq g} w_{g,t}\ ATT^D_{g,t}. \quad (2)$$

In Equation (2), $D_{st}$ denotes a state-year political donation outcome, measured as $\ln(1 + \text{amount})$. Depending on the specification, the dependent variable is the log of total donations, aggregate corporate PAC donations, corporate PAC donations by party and electoral level, candidate-directed donations, or employee donations. For the full-sample specification, $\mathcal{G}_S$ includes all treated cohorts and $\mathcal{C}_{g,t,S}$ consists of not-yet-treated states. For the early-adopting cohort specification, $\mathcal{G}_S$ is restricted to states that adopted PTP laws on or before 2016, and never-treated states serve as the comparison group. $w_{g,t}$ are cohort-time aggregation weights and $\theta^{CS}_D$ is the aggregate average treatment effect on the treated (ATT). A negative estimate indicates that PTP laws reduce insurer political giving after adoption, which is the first institutional implication of H3.

Because the Callaway and Sant'Anna estimator does not report covariate coefficients, we estimate the coefficients on the control variables from a parallel two-way fixed-effects regression estimated on the same sample. The control vector includes $\ln(\text{real GDP})$, $\ln(\text{per-capita disposable income})$, GDP growth, the unemployment rate, and the insurance-

market Herfindahl-Hirschman Index, and ln (population). We cluster standard errors at the state level.

We use the same staggered DID framework to estimate the effect of PTP laws on premiums written by donating insurers by changing donations to premiums in Equation (2). For premium outcomes, the dependent variable is alternatively the logarithm of total premiums, Medicare premiums, or Medicaid premiums.

This staggered DID design provides the paper's main institutional test of H1 and H3 on the political giving and allocation margins. If PTP laws make transaction-specific political exchange more costly, targeted political giving should decline after adoption, and premium outcomes for donating insurers should become less favorable. Our use of group-time average treatment effects avoids the bias that can arise in staggered settings when treatment effects differ across cohorts or over time.

3.4.3 Insurer size heterogeneity

We next examine whether the effects of PTP laws are stronger for smaller insurers by estimating the following interacted DID, which is equivalent to a triple-difference design:

$$Y_{ist} = \beta_1 \text{Post}_{st} + \beta_2 \text{LowAssets}_{it} + \beta_3 (\text{Post}_{st} \times \text{LowAssets}_{it}) + \Gamma' X_{st} + \mu_{is} + \tau_t + \varepsilon_{ist} \quad (3)$$

In Equation (3), $\text{LowAssets}_{it}$ is a dummy variable that equals one if insurer $i$'s total assets are below the sample median in year $t$, and zero otherwise. The dependent variable $Y_{ist}$ is alternatively the natural logarithm of total donations, candidate-directed donations, total premiums, Medicare premiums, or Medicaid premiums. The control vector includes log real GDP, log per-capita disposable income, GDP growth, unemployment rate, HHI, and log population. We include insurer-by-state fixed effects, $\mu_{is}$, and year fixed effects, $\tau_t$, to absorb time-invariant insurer-state heterogeneity and common shocks. We cluster standard errors at the state level.

The coefficient of interest is $\beta_3$, which captures whether PTP laws affect below-median-asset insurers differently from larger insurers. This design tests the firm-size prediction that political rents are more valuable for insurers with less financial slack and more concentrated

exposure to public-program business. A negative interaction estimate indicates that PTP laws reduce donations or premiums more strongly for smaller insurers than for larger insurers.

## 4. Public-Program Rents: Pay-to-Play Laws, Political Giving, and Premium Allocation

### 4.1 Descriptive statistics and baseline associations

Table 1 reports summary statistics for the main variables used in our empirical tests. The sample spans 2002 to 2022. Sample sizes differ by outcome and level of aggregation. The state-year donation panel contains about 1,205 observations, the insurer-state-year panels used in the premium and donation regressions contain up to roughly 483,000 observations, and the insurer-year financial panel contains 5,650 observations. The number of observations is smaller in specifications that use supervisory or downstream health variables.

**[Insert Table 1 here]**

The political donations variable has large variation and is highly skewed, as most insurers do not make political contributions and some companies donate large amounts. After log transformation, donation activity remains highly dispersed. The variable ln(Total Donations) has a mean of 10.823, a standard deviation of 2.797, and a maximum of 17.577. The donor indicator averages 0.272 in the insurer-state panel, because most insurer-state observations are non-donor observations. In recent decades, political donations have increased in prevalence and magnitude.

Health insurers' political donations also concentrate in several states. The average health insurer makes positive political contributions to officials and committees from 14 states, according to the mean, and two states, according to the median. These insurers donate to all states: Centene Corp., Dental Services Org. LLC, Presbyterian, Guardian, Santa Clara County, Molina Healthcare Inc., Kaiser Permanente, MetLife, MG Insurance Co., Banner Health, County Santa Clara, Blue Shield of California, The University of Utah, Intermountain Healthcare, Network Health, Sutter Health Plan, CVS Health Corp., Blue Cross & Blue Shield of AL, Humana Inc., Highmark, UnitedHealth Group, Blue Cross NC, HCSC, Principal Financial Group Inc., and Mutual of Omaha.

Health insurance companies' sales are segmented by markets. The average health insurer has positive premium revenue in only four states, consistent with geographical segmentation. Nine operating states are the 90$^{th}$ percentile value for health insurers in my sample. However, some large insurers operate in multiple states. Over our sample period, MetLife and Southern Guaranty Insurance Co. received health insurance premiums from 49 states, and Mutual of Omaha and First Continental Life & Accident Insurance Company operate in 50 states. These insurers have positive premium revenue from all 51 states in our sample period: Delta Dental Plan of Oklahoma, Centene Corp., Delta Dental Dentegra, Humana Inc., Elixir Insurance Co., Highmark, UnitedHealth Group, The Cigna Group, and CVS Health Corp.

Figure 1 illustrates this geographic segmentation more directly. Panels (a) and (b) map average insurer dependence on each state before and after PTP adoption, and Panels (c)–(f) report the average duration of Medicare and Medicaid contractor relationships. Two patterns stand out. First, insurer dependence is concentrated in a relatively small set of states rather than distributed evenly across jurisdictions. Second, Medicare and Medicaid contractor relationships are often persistent over time. Together, these patterns indicate that insurers' exposure to politically valuable public-program business varies sharply across states and is frequently embedded in durable insurer–state relationships.

**[Insert Figure 1 here]**

Public program business is large but heterogeneous across markets. ln(Total Premium) averages 15.346, compared with 13.496 for ln(Medicare Premium) and 13.895 for ln(Medicaid Premium). State environments also vary meaningfully in both market structure and institutional conditions. HHI averages 0.361, 34.5% of state-year observations occur after a PTP law is in force, and 49.5% are classified as high-corruption under the DOJ-based measure. Insurer size is highly skewed as well, with a median total assets of about $73 million, the 75th percentile of about $395 million, and the maximum exceeding $111 billion. There is some variation in corruption across states. The two states with the lowest average corruption index values are New Hampshire and Vermont, and the two states with the highest average corruption index values are New Jersey and Florida.

Figure 2 provides additional descriptive evidence on the durability of insurers' public-program relationships. The figure plots the distribution of Medicare and Medicaid contractor spell lengths, where a spell is defined as a maximal run of consecutive years in which an insurer earns positive program premium in a given state. Over the 2000–2023 sample, there are 862 Medicare spells with an average duration of 8.9 years and 391 Medicaid spells with an average duration of 10.7 years. The distribution shows both many short spells and a sizable mass of long-lived relationships, including spells that extend through the full sample window. This persistence is consistent with repeated insurer-state interactions in public-program markets and supports the premise that durable public-program relationships can create scope for politically mediated rents.

**[Insert Figure 2 here]**

Taken together, these patterns indicate substantial cross-state and cross-insurer variation in political activity, market opportunity, and the institutional returns to rent-seeking. Our univariate analysis shows that the distribution of health insurance premiums overlaps with the distribution of insurers' political contributions across states, and the positive correlations are still significant after controlling for insurer and state characteristics.

Table 2 provides baseline OLS evidence for the pre-2016 period based on Equation (1). Higher political giving is associated with higher insurer premiums and enrollment across all specifications. A one-log-point increase in total donations is associated with 0.122 log points higher total premiums, 0.14 log points higher Medicare premiums, and 0.088 log points higher Medicaid premiums. Because both donations and the outcome variables are in logs, these coefficients are elasticities. A 10% increase in donations is associated with about a 1.2% increase in total premiums, a 1.4% increase in Medicare premiums, and a 0.9% increase in Medicaid premiums. The coefficients are statistically significant at the 5% to 1% level and economically meaningful. Using the sample dispersion of donations, a one-standard-deviation increase in log donations is associated with about a 0.24 standard deviation increase in total premiums. These results show that politically active insurers tend to write more public-program business, consistent with our hypothesis H1.

**[Insert Table 2 here]**

4.2 Pay-to-play laws and insurer political giving

H3 implies that if PTP laws raise the expected legal cost of exchanging contributions for regulatory favors, insurer political giving should decline after those laws take effect. Table 3 reports the main staggered DID estimates based on the Callaway and Sant'Anna estimator. The evidence shows that PTP laws reduce several targeted political-giving channels, although the effect is not uniform across all donation categories.

The strongest effects appear in corporate PAC and candidate-directed giving. PTP adoption reduces corporate PAC donations by 3.112 log points, Republican-directed corporate PAC donations by 3.037 log points, local corporate PAC donations by 1.896 log points, and candidate-directed donations by 0.330 log points. These estimates are statistically significant and economically large. Using the standard log-to-percentage conversion, they imply approximate reductions of 95.5% in corporate PAC giving, 95.2% in Republican-directed corporate PAC giving, 85% in local corporate PAC giving, and 28.1% in candidate-directed giving.

**[Insert Table 3 here]**

By contrast, the ATT for total donations is negative but statistically insignificant. The estimates for Democratic-directed corporate PAC donations, federal corporate PAC donations, and employee donations are also not statistically significant. The evidence is consistent with PTP laws reducing business-linked and candidate-directed giving, especially corporate PAC activity. This pattern supports a transaction-specific interpretation in which PTP laws reduce the expected return to political exchange while leaving broader or less targeted forms of giving less affected.

4.3 Pay-to-play laws and donating insurers' public-program premiums

If political giving helps insurers obtain public-program rents, then restricting the return to giving should reduce the premiums of the insurers most likely to use that channel. We test the effects of PTP laws on donating insurers' public program premiums based on Equation (3). We report both full-sample and early-adopting cohort estimates. The full sample uses all

staggered state adoptions and therefore captures the broad average effect of PTP laws across cohorts. The early-adopting cohort, defined as states adopting on or before 2016, is less exposed to the nationwide shift in corruption enforcement standards. Because year fixed effects absorb only shocks common to all states, reporting both samples allows us to assess whether the main patterns persist in a cleaner timing window while retaining the broader coverage of the full sample.

Table 4 reports the estimation results. The evidence is concentrated in Medicare rather than Medicaid. In the staggered DID estimates, the full-sample ATT for total premiums is -0.034 and statistically insignificant, and the ATT for Medicaid premiums is -0.129 and statistically insignificant. By contrast, the full-sample ATT for Medicare premiums is -0.683 and statistically significant at the 5% level.

**[Insert Table 4 here]**

Consistent with the logic above, the Medicare estimate for the early-adopting cohort is even larger, at -0.998, and is significant at the 1% level. For these log premium outcomes, the ATT estimates are converted into percentage changes using $100 \times (e^{\beta} - 1)$. The full-sample ATT implies roughly a 49.5% decline in Medicare premiums for donating insurers, and the early-adopting cohort estimate implies about a 63.1% decline. By contrast, the estimates for total and Medicaid premiums are smaller and statistically insignificant, indicating that the revenue response is concentrated in the public-program margin where political relationships appear most valuable. The results suggest that stricter state legal constraints reduce at least some of the revenue gains politically active insurers can obtain from public-program business. Tighter legal constraints reduce Medicare-related premium revenue among donating insurers, but the evidence does not show a comparably robust decline in total or Medicaid premiums. Based on the whole sample, the law-based evidence for public-program rent allocation is therefore concentrated on the Medicare margin.

Figure 3 provides visual evidence on event-time patterns around PTP adoption. The plots show broadly similar pre-adoption movements between treated and never-treated states, with post-adoption divergence in some donation and premium series. Relative to the year before adoption, treated states do not show a clear break from never-treated states prior to

implementation, and their post-adoption paths become flatter or more negative for total donations and for several corporate PAC donations. The visual evidence is strongest for total donations and Democratic contributions.

**[Insert Figure 3 here]**

Overall, the evidence in this section supports H3 by showing that stronger state anti-PTP rules reduce targeted political-giving channels and constrain some downstream public program rents.

4.4 Insurer size heterogeneity and donation-channel attribution

Figure 4 provides descriptive motivation for the insurer-size heterogeneity results. Panels (a)–(d) map Medicare premium shares for large and small insurers, defined by asset size, before and after PTP adoption, while Panels (e)–(h) show the corresponding Medicaid shares. The maps suggest that smaller insurers' public-program business is concentrated in a narrower set of states than that of larger insurers. Although the panels use different legend scales, the spatial pattern is consistent with the idea that state-specific public-program rents are relatively more important for below-median-asset insurers, which aligns with the theoretical predictions in our hypotheses.

**[Insert Figure 4 here]**

In Table 5, we report the regression results based on Equation (3), which examines whether the post-law response varies by insurer size. The omitted group is above-median-asset insurers, so the coefficient on Post captures the effect for larger insurers, while the interaction term Post × Low Total Assets captures the differential effect for below-median-asset insurers. In the full sample, the interaction term has a significantly negative coefficient for every donation and premium outcome. The interaction estimates imply additional relative declines for smaller insurers of 0.502 log points in total donations, 0.356 log points in candidate-directed donations, 0.233 log points in total premiums, 0.148 log points in Medicare premiums, and 0.058 log points in Medicaid premiums. These correspond to approximate additional relative declines of 39.5%, 30.0%, 20.8%, 13.8%, and 5.6%, respectively. The sharper response among

smaller insurers support that political rents are more valuable for firms with less financial slack and more concentrated public-program exposure.

**[Insert Table 5 here]**

Heterogeneity tests sharpen the mechanism of favor exchange between health insurers and state officials and help rule out broad demand or market-wide shocks. The effects of PTP concentrate among small firms, whose public-program revenues and donations decrease in states that adopt PTP laws, consistent with the higher value of public program rents for small insurers. Officials in state health departments can negotiate rates and contracts with MCOs for Medicaid and Medicare Supplement (Medigap), and state insurance commissions can license and oversee MCOs that operate Medicaid and Medicare plans. Using their discretionary power, state officials may approve more of the applications and negotiate higher rates from insurers that donate to their campaigns directly or indirectly. Pay-to-play laws reduce the quid pro quo between health insurers and state officials, and smaller, potentially more constrained insurers appear more exposed to the removal of PTP opportunities.

Appendix Table A4 reports our placebo DID test results, which strengthen the causal interpretation of our main results. In the placebo tests, PTP laws do not significantly affect donations made by insurers in states where they have no premium revenue, and they do not significantly affect total premiums among non-donating insurers. These null results make it less likely that the main findings are driven by general political ideology or unrelated statewide shocks. Instead, the evidence points to business-linked contributions as the relevant channel through which legal constraints affect premium allocation.

Figure 5 summarizes the channel attribution visually. We plot Callaway and Sant'Anna average treatment effects with 95% confidence intervals for business-relevant channels and comparison outcomes in the clean sample that excludes repeal-state years. The figure shows that the most negative and precisely estimated effects appear in corporate PAC giving, while donations by insurers without premium business in the state, premiums of non-donating insurers, and employee or individual donations are close to zero. This pattern reinforces the interpretation that PTP laws affect business-linked insurer-state political exchange rather than broad shifts in political activity or statewide insurance demand.

**[Insert Figure 5 here]**

## 5. Supervisory Oversight and Insurer Financial Performance

The earlier sections show that PTP laws are associated with lower political giving and with changes in public-program and cost outcomes. We now examine the mechanism more directly by focusing on supervisory forbearance as the second channel of political rents and on the firm-level consequences of stronger legal constraints. If campaign contributions help insurers obtain softer oversight and preserve political rents, then PTP laws should be followed by stricter supervision and weaker financial performance for insurers that are more exposed to treated states.

### 5.1 Pay-to-play laws and regulatory oversight

We test whether PTP laws are associated with stronger prudential oversight or less lenient regulations from insurance commissioners. The insurance departments of insurers' domicile states are responsible for conducting financial examinations, which evaluate solvency, compliance with laws, governance, and other issues. Regulatory guidance refers to state insurance departments' warnings and required corrective actions about issues or risks, such as liquidity problems, internal control issues, and inadequate capital or reserves. It is costly for insurers receiving regulatory guidance to increase capital reserves or improve internal controls. If insurers use political donations to exchange for regulatory slack, PTP laws should cause prudential oversight to strengthen. To test changes in regulatory oversight, we aggregate examination outcomes to the state-year level and estimate Equation (4) below.

$$\text{Oversight}_{s,t} = \alpha + \beta \text{Post}_{s,t} + \gamma X_{s,t} + \mu_s + \varepsilon_{s,t}, \qquad (4)$$

where $\text{Post}_{s,t}$ equals one once state $s$ has adopted a PTP law. The dependent variables are Regulatory Guidance and Year of Financial Exam, both measured at the state-year level as aggregates of insurer examination outcomes. Regulatory Guidance is the mean regulatory guidance score across insurers in a state-year. The controls in $X_{s,t}$ are ROA and leverage, also

aggregated to the state-year level. We include state fixed effects and cluster standard errors at the state level. We report estimates for the early-adopting cohort and full-sample specifications.

Table 6 reports the results based on Equation (4). The estimates support the view that PTP laws are followed by less lenient supervision. The coefficient on $\text{Post}_{s,t}$ is positive and statistically significant in both Regulatory Guidance regressions: 0.243 ($p < 0.01$) in the early-adopting cohort sample and 0.174 ($p < 0.05$) in the full sample. Relative to the sample mean of 0.18, these are economically large effects. The second oversight measure, Year of Financial Exam, is also significantly positive in the early-adopting cohort sample (0.023, $p < 0.05$), although the full-sample estimate is not statistically significant. The early-adopting cohort estimate for Year of Financial Exam further implies a 2.3-percentage-point increase in the share of insurers examined by insurance commissioners in the current year. The overall pattern is therefore consistent with reduced supervisory forbearance and more active supervisory intervention after PTP laws take effect. The evidence suggests that more rigorous anti-corruption laws lead to tighter prudential monitoring and more active regulatory intervention.

**[Insert Table 6 here]**

5.2 Exposure to PTP laws and insurer financial performance

We next examine whether stronger exposure to PTP laws is associated with deteriorating insurer financial performance. If part of insurers' profitability reflects political rents obtained through public-program business or softer supervision, then insurers with greater exposure to states that adopt PTP laws should perform worse when those rents become harder to sustain. We estimate Equation (5) below.

$$\text{Performance}_{i,t} = \alpha + \beta\, \text{Treat_Exposure}_{i,t} + \mu_i + \varepsilon_{i,t}, \qquad (5)$$

In Equation (5), $\text{Treat_Exposure}_{i,t}$ is the share of insurer $i$'s premiums written in states with PTP laws. The dependent variables are ROA, Operating ROE, Cash Ratio, Leverage, and Claim Ratio. We add firm-fixed effects, and cluster standard errors at the firm level. We report estimates for both the full sample and the pre-2016 subsample.

Table 7 shows that insurer performance is weaker when a larger share of business is exposed to PTP laws. $\text{Treat_Exposure}_{i,t}$ is negatively related to both profitability measures

in every specification. In the full sample, the coefficient is -4.965 ($p < 0.01$) for ROA and -15.938 ($p < 0.01$) for Operating ROE. In the pre-2016 sample, the corresponding coefficients are even larger in magnitude, -6.472 and -21.787 (both $p < 0.01$). Because Treat Exposure ranges from 0 to 1, the coefficient captures the effect of moving from zero to full exposure, and we multiply the coefficient by 0.1 to derive the effect of a 10-percentage-point increase in exposure. A 10-percentage-point increase in Treat Exposure is associated with about a 0.5 percentage point decline in ROA and a 1.59 percentage point decline in operating ROE in the full sample, with still larger declines in the pre-2016 sample. A one-standard-deviation increase in Treat Exposure implies roughly a 2 percentage point decline in ROA and a 6.6 percentage point decline in operating ROE, while moving from zero to full exposure corresponds to declines of about 5 and 15.9 percentage points, respectively.

**[Insert Table 7 here]**

The non-profitability measures also suggest deteriorating performance. Greater exposure is associated with lower Cash Ratios in both the full sample (-0.065, $p < 0.05$) and the pre-2016 sample (-0.099, $p < 0.01$), and with higher Claim Ratios (2.459, $p < 0.10$), consistent with weaker liquidity and underwriting performance. The coefficient of Leverage is negative and only marginally significant in the full sample (-0.038, $p < 0.10$) and statistically insignificant in the pre-2016 sample. The economic magnitudes are meaningful. Full exposure is associated with about a 6.5-percentage-point lower cash ratio and a 2.5-percentage-point higher claim ratio, indicating weaker liquidity and greater pressure from claims.

These findings are more naturally interpreted as evidence that PTP laws compress insurer rents than as evidence that political giving merely reflects ideology. The results show that performance is weaker when a larger share of an insurer's business is subject to stronger anti-PTP constraints. Together with the earlier evidence on donations and public program outcomes, Tables 6 and 7 provide indirect support for H2 and H3. When the legal return to political exchange falls, supervision becomes less lenient and insurer profitability declines.

These findings also speak to the broader debate over whether campaign contributions are investments or expressions of ideology. Some studies find limited or even negative returns to corporate political support (Aggarwal, Meschke, and Wang, 2012; Bertrand et al., 2018),

whereas others document private benefits from political connections and contributions (Cooper, Gulen, and Ovtchinnikov, 2010; Fazekas, Ferrali, and Wachs, 2023). Our findings are more consistent with an investment motive for at least some insurer contributions than with a purely ideological account. Contributions targeted to specific state officials with influence over insurers' public-program business or supervisory oversight are more plausibly transactional. By contrast, donations to broad committees or local races may often reflect ideology or long-run relationship building, in line with Ansolabehere, de Figueiredo, and Snyder (2003). The weaker performance of insurers more exposed to PTP laws reflects rent compression rather than reduced donations for expressing ideology.

## 6. Anti-Corruption Enforcement, Institutional Quality, and Pay-to-Play

We then examine whether the weakening of federal anti-corruption constraints after McDonnell v. United States had larger effects on political giving, public-program revenues, and pricing in states with weaker institutions, proxied by an above-median DOJ public-corruption measure. For donation outcomes, we test whether states with pre-existing high levels of corruption experience a relative increase in political donations from health insurers after McDonnell v. United States, as in Equation (6).

$$D_{ist} = \lambda(\text{Post2016}_t \times \text{HighCorruption}_s) + \Gamma' X_{st} + \mu_{is} + \tau_t + \nu_{ist} \qquad (6)$$

In Equation (6), $D_{ist}$ is alternatively the log of donations to committees, the log of donations to Republicans, or the log of total donations. The dummy $\text{HighCorruption}_s$ equals one if the corruption index value of a state is above the median and zero otherwise. The dummy $\text{Post2016}_t$ equals one after 2016 and zero otherwise. The control vector includes real GDP, disposable income, GDP growth, the unemployment rate, and HHI. In some specifications, we also add insurer-state fixed effects and year-fixed effects to control for unobserved heterogeneity due to firm growth or macroeconomic trends, which are denoted by $\mu_{is}$ and $\tau_t$, respectively. We cluster standard errors at the state level. The standalone post-2016 and corruption indicators are absorbed by the fixed effects. The coefficient of interest is the interaction term, $\lambda$. A positive estimate of the interaction term's coefficient implies that the

weakening of federal anti-corruption enforcement after McDonnell produced a larger response in more corruption-prone states.

For premium and premium-rate outcomes, we estimate the corresponding state-year specification:

$$R_{st} = \kappa(\text{Post2016}_t \times \text{HighCorruption}_s) + \Gamma' X_{st} + \alpha_s + \tau_t + e_{st} \qquad (7)$$

In Equation (7), $R_{st}$ is alternatively the log of Medicare premiums, the log of Medicaid premium rates, or the log of Medicare premium rates. Premium rates are defined as premiums per enrollee. The control vector again includes real GDP, disposable income, GDP growth, the unemployment rate, and HHI. State fixed effects absorb time-invariant differences across states, including persistent corruption exposure, and year fixed effects absorb common nationwide shocks. The coefficient of interest, $\kappa$, captures whether premium outcomes increase more after 2016 in more corruption-prone states.

These post-2016 interaction models provide the paper's second institutional test of H3, in addition to the staggered DID tests based on state PTP laws. Based on a common nationwide change in the federal enforcement environment, these models test whether the response is disproportionately concentrated where institutional quality is weaker. Under the paper's PTP interpretation, the relaxation of federal anti-corruption constraints should increase the return to political giving most in high-corruption states and thus increase the observed quid pro quo proxies.

We also test whether strong state PTP laws mitigate the effect of the post-McDonnell weakening of federal anti-corruption deterrence. We estimate a triple-difference specification in which the coefficient of interest is Post2016 × High DOJ Corruption × No-Strong-PTP. No-Strong-PTP equals one for states without a strong PTP law in force as of 2016. The dependent variable is alternatively total donations, corporate PAC donations, candidate-directed donations, or employee donations at the state-year level. A positive coefficient supports that the post-McDonnell increase in insurer political giving is larger in high-corruption states where strong state-level PTP constraints were absent, which suggests that federal anti-corruption enforcement and strong state PTP laws act as complementary constraints on pay-to-play activity.

Table 8 reports the regression results based on Equations (6) and (7). Across specifications, the interaction term $\text{Post2016} \times \text{High DOJ Corruption}$ has positive coefficients. Relative to low-corruption states, high-corruption states experienced larger post-2016 increases in committee donations (0.118, 5% level), Republican-directed donations (0.06, 10% level), total donations (0.107, 10% level), Medicare premiums (0.5, 5% level), and Medicaid premium rates (0.231, 10% level), but not Medicare premium rates (0.097, insignificant). These are economically meaningful shifts. For the logged donation and premium outcomes, we convert the interaction coefficients into percentage differences using $100 \times (e^{\beta} - 1)$. The coefficients imply differential increases of approximately 12.5% for donations to political committees, 6.2% for Republican-directed donations, 11.3% for total donations, 64.9% for Medicare premiums, and 26% for Medicaid premium rates respectively.

**[Insert Table 8 here]**

After McDonnell, the relative increase in insurer political giving in high-corruption states is accompanied by more favorable public-program outcomes, especially in Medicare premiums and Medicaid premium rates. This evidence is consistent with H1 and H3. A common federal shock produces a larger PTP response where state institutions are weaker and the expected return to political rent-seeking is higher. This pattern is consistent with an access-investment channel in which weaker anti-corruption deterrence increases firms' incentives to purchase influence and is associated with regulatory or contracting advantages that raise public insurance rates or enrollment. Although state officials exert more direct influence over Medicaid than over Medicare, they can also indirectly affect Medicare-related business through licensing, network enforcement, implementation, and dual-eligible coordination. The Table 8 estimates therefore fit the broader argument that weaker anti-corruption deterrence increases the expected payoff to political investment where administrative discretion affects insurer profits. Because the significant post-2016 effects are concentrated in Medicare premiums and Medicaid premium rates, the evidence points to public-program margins rather than a uniform increase across all insurer outcomes.

Table 9 provides a supplemental triple-difference test of whether state PTP constraints mitigate the post-McDonnell increase in political giving. The interaction coefficient is positive

for all four donation outcomes, but it is statistically significant only for corporate PAC donations. The corporate PAC coefficient is 2.143 log points and is significant at the 10% level. The estimates for total donations, candidate-directed donations, and employee donations are positive but statistically insignificant. This pattern suggests that the McDonnell-related increase in pay-to-play activity is most visible in corporate PAC giving where corruption risk is high and strong state PTP constraints are absent.

**[Insert Table 9 here]**

Overall, the evidence supports the view that federal and state anti-corruption enforcement are more complementary in states with weaker institutions, so loosening federal corruption enforcement generates a larger deterrence shock and a larger increase in PTP activity in high-corruption environments. When states adopt targeted PTP laws, insurer political giving declines, and supervisory attention intensifies. When federal anti-corruption deterrence weakens after McDonnell, political giving and selected public-program outcomes rise disproportionately in high-corruption states. These findings are difficult to reconcile with a substitution view that low-corruption states would respond more strongly because they previously relied more on federal enforcement. Instead, low-corruption states exhibit smaller changes relative to high-corruption states, and the evidence suggests that weaker institutions magnify the response to a federal enforcement shock. Although the public-program evidence is not uniform across all outcomes, the overall pattern is consistent with a transactional-capture mechanism in which weaker legal constraints raise the expected return to political rent-seeking.

## 7. Health Insurance Costs and Health Care Access

This section examines the paper's welfare implications. The earlier sections test the paper's allocation, oversight, and legal-constraint hypotheses. If campaign contributions help insurers obtain more favorable public-program outcomes and softer supervisory treatment, then making quid pro quo exchange more costly should reduce the downstream costs borne by taxpayers and households and improve access to care.

We test that prediction using state-year measures of Medicaid spending per enrollee, family deductibles and premiums, cost-related barriers to care, insurance coverage, suicide

rates, and the share of adults reporting no doctor visit in the past year. These outcomes allow us to assess whether limiting politically mediated rent extraction improves affordability and access.

### 7.1 Health insurance costs

Previous studies show that health insurance subsidizes consumers and reduces mortality, which is especially important for low-income individuals (Crew, 1969; Garber, Jones, and Romer, 2006; Goldin, Lurie, and McCubbin, 2021). Rising private health insurance premiums reduce payroll and employment (Baicker and Chandra, 2005), leading to more suicides and drug overdoses (Brot-Goldberg et al., 2024). Political rent-seeking can increase health insurance costs by distorting the allocation of resources and extracting value from the health care system, especially in states with weak legal institutions. If PTP laws are effective, they would reduce health insurance costs.

Our measures of health care costs include Medicaid spending per enrollee, average family deductibles, and average family premiums. We test the effects of state-level adoption of PTP laws on health care costs using the Callaway and Sant'Anna (2021) staggered-adoption difference-in-differences estimator, with not-yet-treated states serving as controls. We cluster standard errors at the state level. We report both full-sample estimates and estimates for the early-adopting cohort, whose treatment timing is less exposed to post-2016 changes in campaign finance conditions.

Table 10 shows that PTP laws are associated with lower health care costs. In the early-adopting cohort, the estimated average treatment effects are -0.158 for log Medicaid spending per enrollee, -0.503 for log family deductibles, and -0.118 for log family premiums. All three estimates are statistically significant at the 1% level. Using the standard log-to-percentage conversion, these coefficients imply declines of about 14.6% in Medicaid spending per enrollee, 39.5% in family deductibles, and 11.1% in family premiums. Evaluated at the sample means, these effects correspond to roughly $1,004 less Medicaid spending per enrollee, $1,129 lower family deductibles, and $1,957 lower family premiums. In the full sample, the coefficients remain negative for all three outcomes, and the statistically significant estimates imply

reductions of about 3.6% in Medicaid spending per enrollee and 9.5% in family deductibles. The decline in deductibles means lower out-of-pocket exposure households face when they access health care.

**[Insert Table 10 here]**

These results fit the paper's central mechanism. If campaign contributions help insurers obtain favorable public-program terms or weaker regulatory pressure, then some of the resulting rents should appear as higher public spending and higher household cost sharing. The decline in Medicaid spending per enrollee and in family deductibles after PTP adoption is consistent with campaign finance inflating both public and private health insurance costs.

7.2 Health care affordability, insurance coverage, and health outcomes

Our earlier results show that PTP laws reduce health insurance costs, especially Medicaid spending per enrollee and family deductibles, with family premiums also falling in the early-adopting states. We next examine whether broader improvements in affordability, access, and health outcomes accompany these cost reductions.

Table 11 reports staggered DID estimates for four state-year outcomes: the BRFSS Cost Barrier measure, the uninsured rate, the log suicide rate, and the share of adults without a doctor visit in the past year. The ATT estimates are based on the Callaway and Sant'Anna estimator, with not-yet-treated states serving as controls.

**[Insert Table 11 here]**

In Column (1) of Table 11, PTP law adoption reduces the aggregate Cost Barrier by 0.013, or about 1.3 percentage points, significant at the 5% level. The uninsured rate estimate is statistically insignificant. Consistent with the cost evidence in Table 10, the evidence on consumer access suggests that stronger PTP constraints improve affordability by limiting politically mediated rent extraction.

Column (3) shows a log suicide-rate estimate of -0.053 that is significant at the 5% level, implying an approximate 5.2% decline in the suicide rate associated with adoption of PTP laws. Because suicide is affected by many factors beyond health insurance, we interpret this result cautiously than the affordability and coverage estimates. Still, the results are consistent with

the broader welfare channel that PTP laws reduce the rents available through political influence, which reduces cost pressure and improves downstream health outcomes.

Column (4) suggests that PTP adoption reduces the No Doctor Visit, or the share of adults who report not seeing a doctor because of cost, by 0.030, or about 3.0 percentage points, significant at the 1% level. This result provides additional evidence that PTP laws are associated with improved access to care.

Overall, the evidence in Tables 10 and 11 points to meaningful welfare gains from stronger legal constraints on politically mediated rent-seeking in health insurance. After PTP adoption, states experience lower Medicaid spending per enrollee and lower family deductibles, with family premiums also declining in early-adopting states. These cost reductions are accompanied by fewer cost-related barriers to care and lower suicide rates.

The combined evidence is consistent with political contributions helping insurers obtain rents through more favorable public-program terms or softer supervisory oversight, whose costs may fall disproportionately on socioeconomically disadvantaged groups. This interpretation is consistent with a broader literature showing that rent-seeking and corruption distort resource allocation and generate adverse social consequences (Bertrand et al., 2007; Colonnelli et al., 2020; Fisman et al., 2018; Fisman and Wang, 2015; Khwaja and Mian, 2005; Mauro, 1995; Sequeira, 2016; Xu, 2018). Stronger PTP constraints can improve affordability and access by limiting distortions in the allocation and regulation of health insurance.

## 8. Robustness Tests

### 8.1 Placebo DID: non-business donations and non-donor premiums

Appendix Table A4 reports placebo DID tests for settings where the main business-channel mechanism should be weak or absent. In Columns (1)–(4), we re-estimate the donation specifications using donations made by insurers in states where they have no premium revenue. In these insurer-state pairs, state officials have little direct leverage over the insurer's revenues, so a PTP mechanism should be weaker or absent. Consistent with that logic, the estimated ATT is statistically insignificant for total donations, individual donations to Republicans, individual donations to Democrats, and combined individual-and-corporate donations to committees.

In Columns (5)–(7), we estimate the effect of PTP laws on total premiums of non-donating insurers. These firms are not plausibly using campaign contributions to obtain public-program or supervisory rents, so the treatment should have little effect on their premiums. Consistent with this expectation, the placebo estimates are statistically insignificant in the full sample, the early-adopting cohort, and the specification excluding repeal-state years. Column (8) re-examines total donations of insurers with no premium revenue in the state for the early-adopting cohort and also finds an insignificant estimate. These null results support the identification strategy by showing that the effects are concentrated in settings where a transactional link between political giving and insurer-specific regulatory benefits is feasible.

8.2 Other robustness checks

Our main results remain qualitatively similar under alternative measures of political activity and corruption exposure. First, we broaden the donation measure to include both firm- and employee-level contributions, addressing the possibility that insurers channel political activity through individuals rather than through the firm. Previous studies show that CEOs' and employees' donations may be aligned with firms' donations (Richter and Werner, 2017) and CEOs can influence employees' donations (Babenko, Fedaseyeu, and Zhang, 2020).

Second, we re-estimate the post-McDonnell specifications using alternative definitions of high-corruption states, including a top-quartile cutoff based on average CRI and a composite indicator that classifies a state as highly corrupt when at least two of the three corruption indices exceed the sample median. The corruption indexes from the Institute for Corruption Studies are the Corruption Convictions Index, the Corruption Perceptions Index, and the Corruption Reflections Index.[8] The Corruption Reflections Index (CRI) constructed by Dincer and Johnston (2017) is based on corruption coverage in Associated Press (AP) news wires. Dincer and Johnston (2017) count the number of news articles that mention words like "corrupt", "fraud", and "bribe" in news articles and deflate it by the number of news stories mentioning politics in each state in each year. We compute the average CRI in each state to measure across-state variation in the prevalence and tolerance of corruption in each state, which reflects the legal institutions and cultural norms in each state.

[8] The website of the Institute for Corruption Studies is https://greasethewheels.org/.

Using the alternative measures, we still have stronger effects for lower-asset insurers and in higher-corruption environments, and we find that PTP laws reduce business-linked political giving and limit at least part of the insurer-specific rents. For the sake of brevity, we omit these tables here. Taken together, these tests suggest that our findings are not driven by a particular coding of donations or corruption exposure, but instead are concentrated in insurer-state pairs where a transactional PTP mechanism is most plausible.

We further examine the robustness of the donation results using leave-one-out estimation. Appendix Table A5 sequentially excludes each treated state and re-estimates the effect of PTP laws on total donations and corporate PAC donations to Democratic recipients. The leave-one-out estimates remain negative and statistically significant or marginally significant across exclusions, suggesting that these supplemental donation specifications are not driven by any single treated state. Appendix Table A6 reports the pre-treatment balance between PTP adopters and never-adopters. Most pre-treatment differences are statistically insignificant, including DOJ corruption, CRI corruption, donations, premiums, HHI, GDP growth, unemployment, and population.

## 9. Conclusions

This paper examines whether campaign contributions help health insurers secure insurer-specific rents in markets where state officials influence public program contracts and prudential oversight. Using insurer-state-year data from 2002 to 2022, we find that higher state-level insurer giving is associated with higher total, Medicare, and Medicaid premiums and enrollment. For identification, we exploit staggered adoption of state pay-to-play (PTP) laws and the weakening of federal anti-corruption enforcement after McDonnell v. United States. Across these designs, the evidence is more consistent with a transactional than a purely ideological account of insurers' political giving.

Our results suggest that health insurers donate to state officials to obtain favorable access to public program business and supervisory forbearance, which are two complementary mechanisms of rent-seeking. PTP laws significantly reduce corporate PAC giving, Republican-directed corporate PAC giving, local corporate PAC giving, and candidate-directed giving. On

the revenue margin, donating insurers' Medicare premiums fall significantly after PTP adoption, whereas effects on total and Medicaid premiums are not statistically significant.

Smaller insurers experience larger relative declines in donations and premiums after PTP adoption, consistent with political rents and supervisory discretion being especially valuable to firms with less financial slack. Treated states also issue more regulatory guidance, and early-adopting states conduct more frequent financial examinations, suggesting reduced supervisory forbearance. In contrast, after federal anti-corruption deterrence weakened in 2016, insurer donations and selected public-program outcomes rose more in high-corruption states, particularly where strong PTP laws were absent. Together, these findings suggest that state PTP laws and federal anti-corruption enforcement function as complementary constraints on access-based political exchange. After PTP adoption, states experience lower Medicaid spending per enrollee and lower family deductibles, with family premiums also declining in early-adopting states. These cost reductions coincide with fewer cost-related barriers to care, fewer reports of forgone doctor visits, and lower suicide rates.

Overall, the findings suggest that campaign finance can distort regulated health insurance markets through transaction-specific exchanges that affect contracting, pricing, and oversight. Targeted legal constraints may reduce rent extraction in sectors characterized by large public spending, administrative discretion, and repeated interaction between firms and public officials.

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**Figure 1. Geographic Distribution of Insurer State Dependency and Contractor Duration.**

This figure maps cross-state variation in insurers' average dependence on individual states and in the average duration of Medicare and Medicaid contractor relationships before and after PTP adoption. The maps show that both state dependency and contractor duration are concentrated in a subset of states rather than evenly distributed nationwide, indicating that insurers' exposure to politically valuable public-program business differs sharply across jurisdictions. The Medicare and Medicaid duration panels also show that contractor relationships are often long-lived, which is consistent with repeated interaction between insurers and state officials in public-program markets.

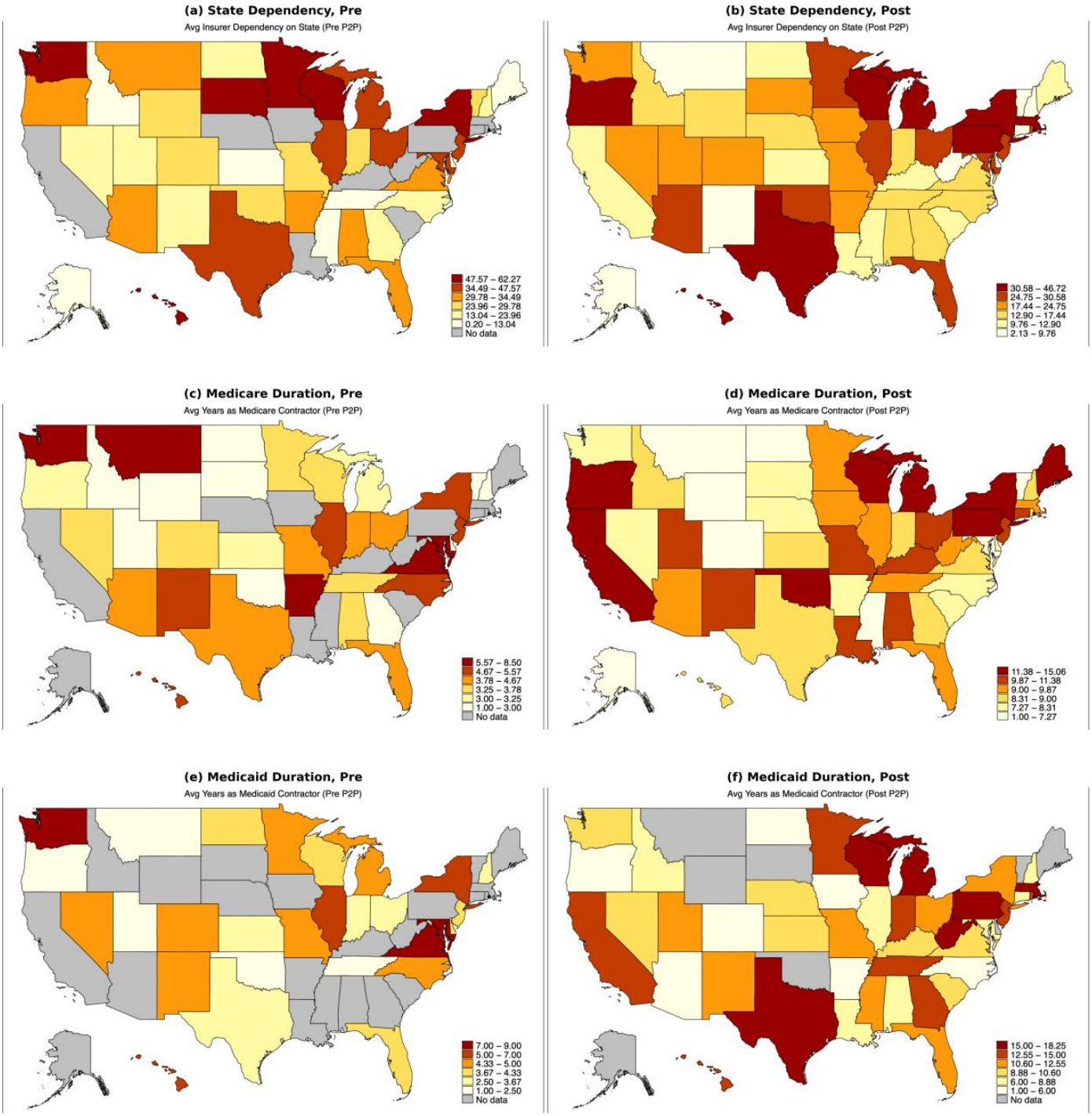


**Figure 2. Distribution of Medicare and Medicaid Contract Duration**

This figure plots the distribution of contractor spell lengths for health insurers in the Medicare and Medicaid programs. A spell is a maximal run of consecutive years in which an insurer earns positive program premium in a given state; spell length is measured at the end of each spell over the 2000–2023 sample. There are 862 Medicare spells (mean 8.9 years) and 391 Medicaid spells (mean 10.7 years). Spells are left-censored at 2000 (the start of the data) and right-censored at 2023; the mass at 23 years collects incumbents whose contracts span the entire window and whose true tenure is longer. Medicaid spells are longer on average than Medicare spells.

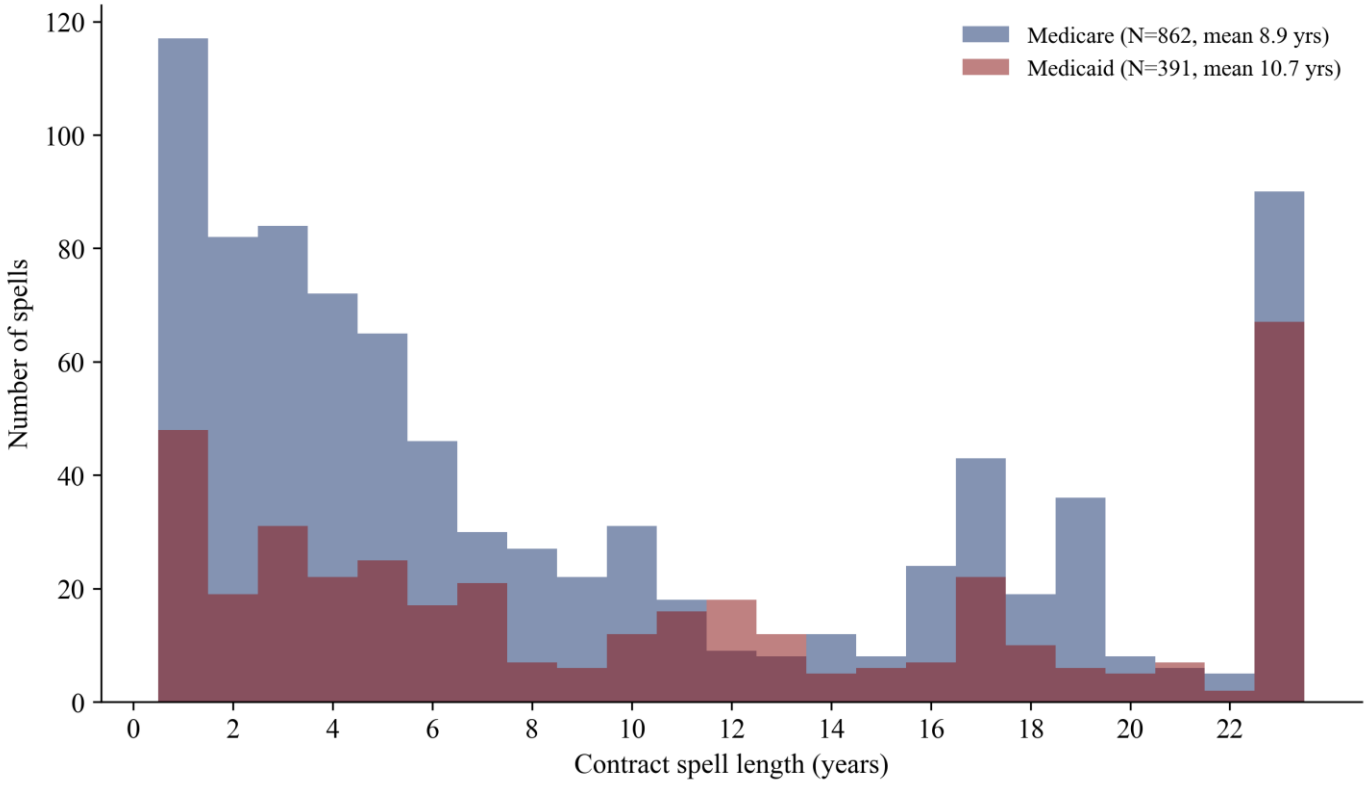


**Figure 3. Normalized Event-Time Trends for Political Donations and Insurer Premiums.**

This figure plots normalized event-time trends around the adoption of PTP laws for total political donations, alternative corporate PAC donation channels, and donating insurers' total premiums. In each panel, treated states and never-treated states are normalized to zero in the year before adoption and traced from five years before to ten years after adoption. Pre-adoption movements are broadly similar across groups, while post-adoption paths diverge, with treated states showing flatter or declining trajectories relative to the steady increases in never-treated states.

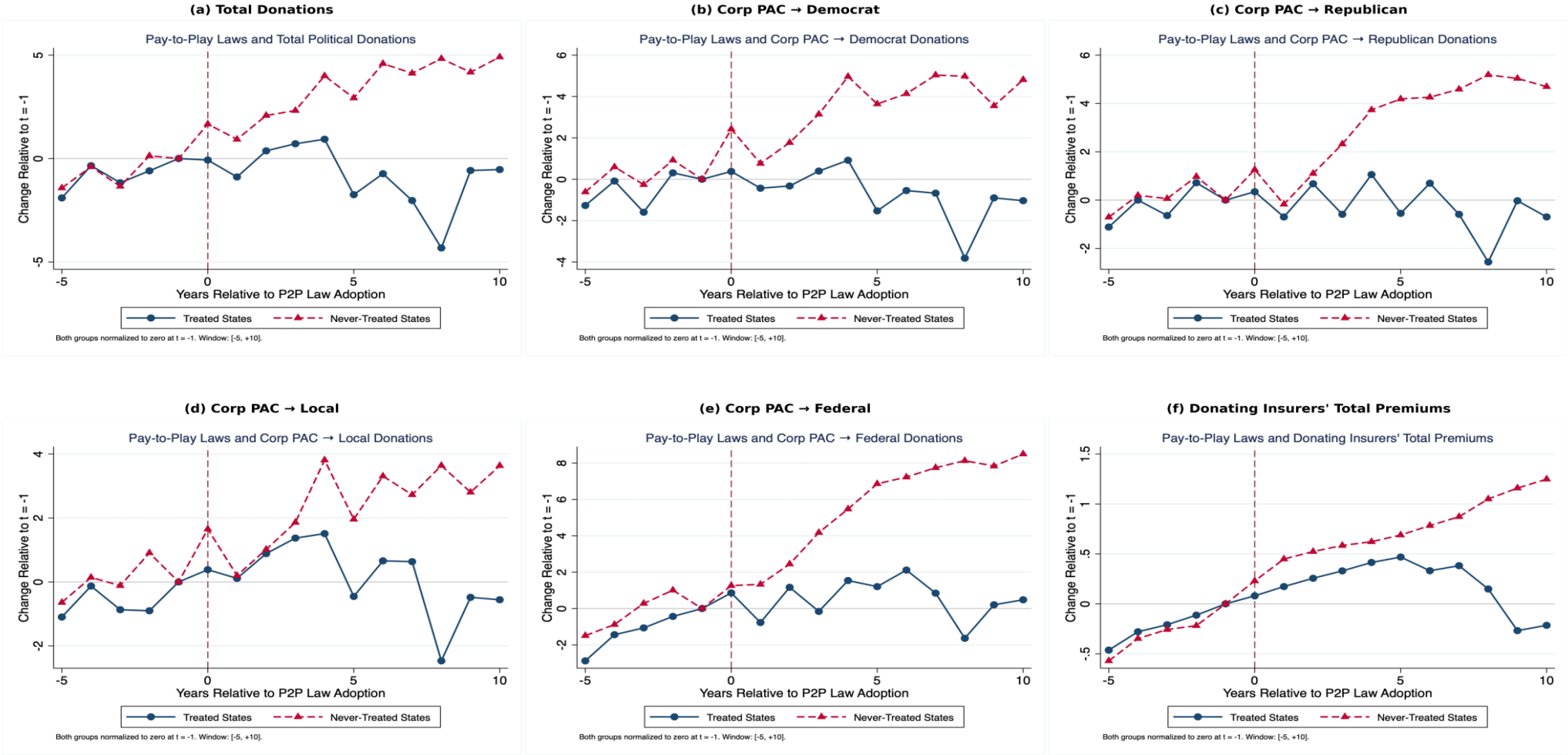


**Figure 4. Medicare and Medicaid Premium Share by Insurer Size.**

This figure maps Medicare and Medicaid premium shares separately for large and small insurers before and after PTP adoption. Relative to larger insurers, smaller insurers' public-program business appears more geographically concentrated in a narrower set of states, suggesting that state-specific public-program rents may be relatively more important for firms with less financial slack.

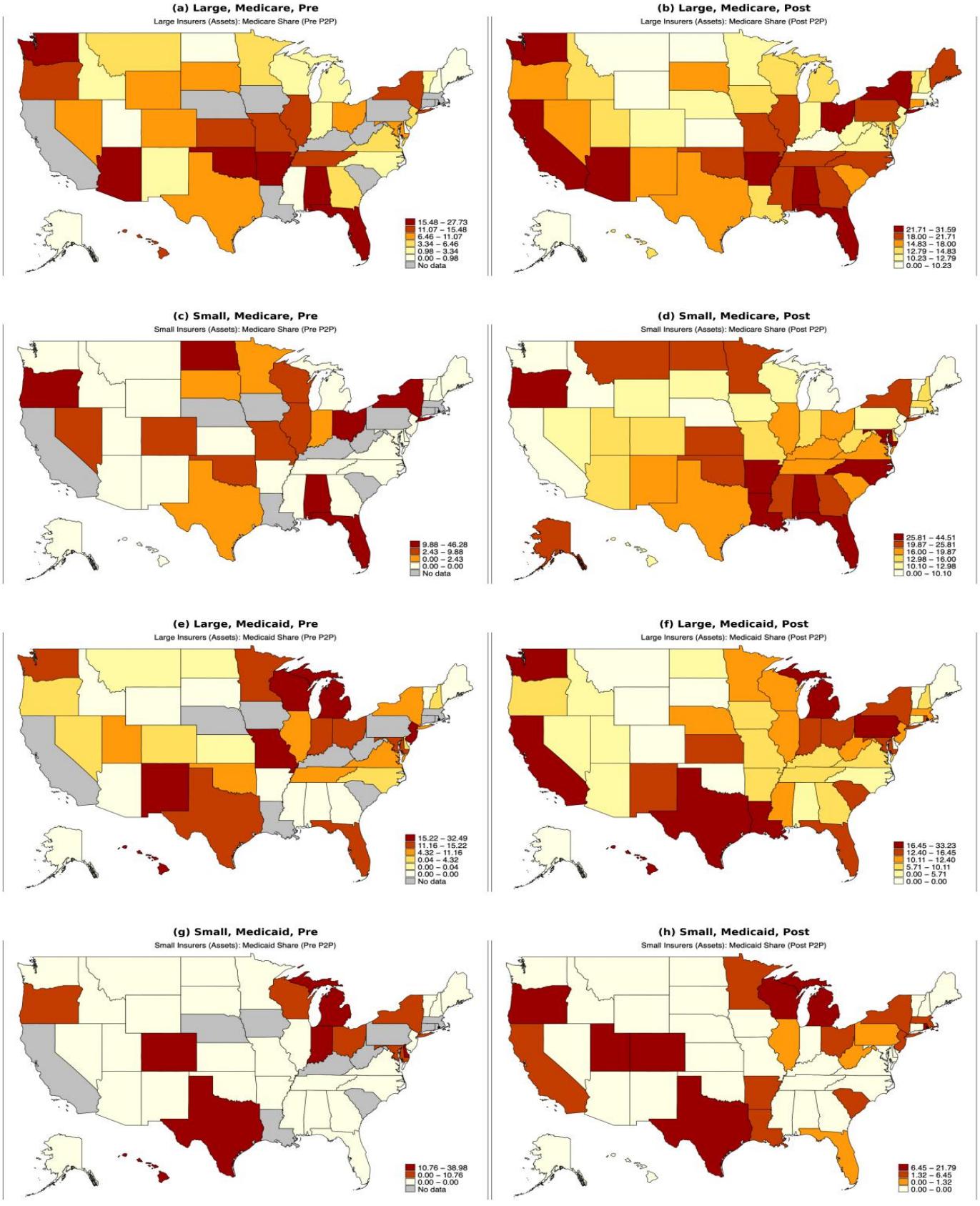


**Figure 5. Pay-to-Play Effects Are Concentrated in Business-Relevant Donation Channels.**

This figure reports Callaway and Sant'Anna (2021) average treatment effects on the treated, with 95% confidence intervals, on ln(1+amount) for nine outcomes, estimated on the clean sample that excludes repeal-state years. The placebo channels include donations of insurers with no premium business in the state, premiums of non-donating insurers, local PAC donations, and employee or individual donations. Estimates are shown in log points because the coefficients are large. Standard errors are clustered by state and all specifications include the same controls as in our main tests.

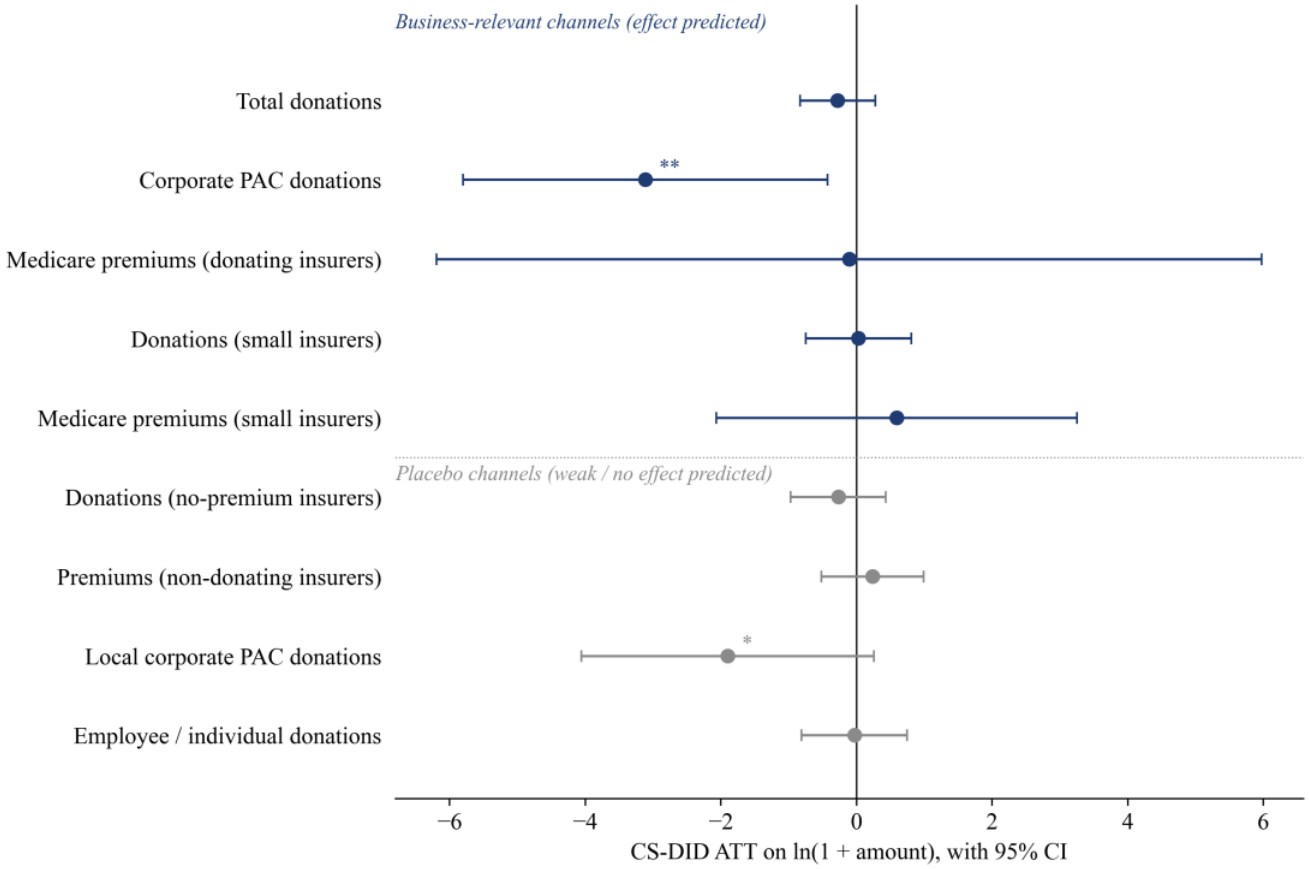


**Table 1. Summary Statistics.**

This table reports summary statistics for the variables used in the main analyses. Panels A–J cover political contributions, premiums, enrollment, premium rates, health care costs, health outcomes, regulatory oversight, insurer characteristics, state controls, and treatment variables. Donation variables are in U.S. Dollars and transformed as ln(x+1). Premium, enrollment, premium rate, health care cost, health outcome, regulatory oversight, and state control variables are measured at the state-year level unless otherwise noted; insurer characteristics are measured at the insurer-year level; and Donor is measured at the insurer-state level. Premium and asset variables are reported in thousands of dollars, following NAIC reporting conventions. The number of observations varies across variables because of data availability.

| VARIABLES | N | Mean | S.D. | Min | P25 | Median | P75 | Max |
|---|---|---|---|---|---|---|---|---|
| **Panel A. Political Donations (state-year).** | | | | | | | | |
| *ln(Corp PAC → Democrat)* | 1,206 | 7.293 | 4.388 | 0 | 5.707 | 8.938 | 10.437 | 14.590 |
| *ln(Corp PAC → Federal)* | 1,205 | 6.575 | 4.971 | 0 | 0 | 8.613 | 10.669 | 15.573 |
| *ln(Corp PAC → Local)* | 1,204 | 7.655 | 4.707 | 0 | 5.303 | 9.547 | 11.112 | 17.559 |
| *ln(Corp PAC → Republican)* | 1,205 | 7.676 | 4.563 | 0 | 5.526 | 9.492 | 11.122 | 15.001 |
| *ln(IC → Committees)* | 1,201 | 9.150 | 3.693 | 0 | 8.175 | 10.131 | 11.293 | 17.537 |
| *ln(Federal Donations)* | 1,206 | 9.792 | 2.860 | 0 | 8.917 | 10.294 | 11.405 | 16.040 |
| *ln(Local Donations)* | 1,204 | 9.471 | 3.517 | 0 | 8.529 | 10.432 | 11.547 | 17.563 |
| *ln(IC → Republican)* | 1,205 | 9.489 | 3.184 | 0 | 8.787 | 10.220 | 11.438 | 15.032 |
| *ln(Individual → Democrat)* | 1,205 | 8.352 | 3.004 | 0 | 7.601 | 9.051 | 10.158 | 14.573 |
| *ln(Individual → Republican)* | 1,204 | 8.260 | 3.021 | 0 | 7.697 | 9.076 | 10.070 | 13.429 |
| *ln(Total Donations)* | 1,205 | 10.823 | 2.797 | 0 | 10.115 | 11.318 | 12.320 | 17.577 |
| **Panel B. Insurance Premiums (state-year).** | | | | | | | | |
| *ln(Medicaid Premium)* | 821 | 13.895 | 2.224 | -5.809 | 12.895 | 14.254 | 15.376 | 17.977 |
| *ln(Medicare Premium)* | 1,073 | 13.496 | 2.316 | -0.038 | 12.390 | 13.876 | 14.991 | 17.744 |
| *ln(Total Premium)* | 1,155 | 15.346 | 1.419 | 5.397 | 14.470 | 15.469 | 16.328 | 18.529 |
| **Panel C. Enrollment (state-year).** | | | | | | | | |
| *ln(Medicaid Enrollment)* | 1,275 | 8.701 | 6.152 | 0 | 0 | 12.203 | 13.509 | 15.752 |
| *ln(Medicare Enrollment)* | 1,275 | 8.340 | 5.400 | 0 | 0 | 11.076 | 12.390 | 14.777 |
| *ln(Total Enrollment)* | 1,275 | 11.013 | 6.360 | 0 | 9.035 | 14.074 | 15.283 | 18.677 |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| **Panel D. Premium Rates (state-year).** | | | | | | | | |
| *ln(Medicaid Premium Rate)* | 763 | 1.165 | 0.750 | -3.877 | 0.756 | 1.191 | 1.615 | 4.681 |
| *ln(Medicare Premium Rate)* | 815 | 2.409 | 0.900 | -2.921 | 2.247 | 2.386 | 2.509 | 8.444 |
| *ln(Total Premium Rate)* | 849 | 0.979 | 0.897 | -4.470 | 0.810 | 1.057 | 1.284 | 4.223 |
| **Panel E. Health Care Costs (state-year).** | | | | | | | | |
| *ln(Avg Family Deductible)* | 858 | 7.952 | 0.330 | 6.887 | 7.712 | 8.019 | 8.220 | 8.720 |
| *ln(Avg Family Premium)* | 858 | 9.775 | 0.214 | 9.291 | 9.616 | 9.775 | 9.942 | 10.245 |
| *ln(Medicaid Per Capita)* | 931 | 8.840 | 0.251 | 8.129 | 8.653 | 8.824 | 9.010 | 9.538 |
| **Panel F. Health Outcomes (state-year).** | | | | | | | | |
| *Cost Barrier* | 439 | 0.114 | 0.033 | 0.050 | 0.088 | 0.111 | 0.136 | 0.196 |
| *ln(Suicide Rate Male)* | 996 | 6.052 | 0.911 | 3.912 | 5.458 | 6.150 | 6.680 | 8.157 |
| *ln(Suicide Rate Total)* | 992 | 6.284 | 0.917 | 4.263 | 5.666 | 6.372 | 6.911 | 8.410 |
| *Pct Uninsured (All)* | 816 | 14.338 | 6.071 | 3.300 | 9.300 | 13.650 | 18.450 | 31.600 |
| *Cannot Afford Doctor (White)* | 546 | 0.097 | 0.030 | 0.038 | 0.074 | 0.093 | 0.119 | 0.185 |
| *Cannot Afford Doctor (Black)* | 398 | 0.156 | 0.049 | 0.000 | 0.118 | 0.152 | 0.190 | 0.313 |
| *Cannot Afford Doctor (Hispanic)* | 453 | 0.212 | 0.053 | 0.077 | 0.176 | 0.210 | 0.247 | 0.392 |
| **Panel G. Regulatory Oversight (state-year).** | | | | | | | | |
| *Regulatory Guidance* | 1,003 | 0.180 | 0.242 | 0 | 0 | 0 | 0.333 | 1 |
| *Year of Financial Exam* | 1,003 | 0.071 | 0.146 | 0 | 0 | 0 | 0.100 | 1 |
| **Panel H. Insurer Characteristics (insurer-year).** | | | | | | | | |
| *Cash Ratio* | 311,398 | 0.416 | 0.313 | -0.233 | 0.154 | 0.359 | 0.634 | 1.196 |
| *Claim Ratio (S&P)* | 4,965 | 86.174 | 83.291 | -559.092 | 80.684 | 85.773 | 89.876 | 5,306.45 |
| *Low Total Assets (below median)* | 5,650 | 0.500 | 0.500 | 0 | 0 | 0.500 | 1 | 1 |
| *Operating ROE (%)* | 244,746 | 3.425 | 55.819 | -794.658 | -0.465 | 5.176 | 14.527 | 790.136 |
| *ROA (%)* | 290,369 | 2.363 | 26.237 | -847.919 | -0.264 | 4.354 | 10.115 | 310.614 |
| *Total Assets ($000s)* | 311,546 | 1130099.69 | 5170546.96 | 1.260 | 12421.28 | 73079.03 | 394667.42 | 111590000.00 |
| *Treat Exposure* | 4,262 | 0.343 | 0.412 | 0 | 0 | 0 | 0.667 | 1 |

| | | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| *Leverage* | 311,546 | 0.515 | 0.982 | 0 | 0.335 | 0.484 | 0.612 | 1 |
| *ln(Total Assets)* | 5,650 | 11.119 | 2.636 | 0.231 | 9.407 | 11.206 | 12.893 | 18.496 |
| **Panel I. State Controls (state-year).** | | | | | | | | |
| *HHI* | 1,155 | 0.361 | 0.199 | 0.087 | 0.216 | 0.305 | 0.456 | 1.000 |
| *Real GDP ($M)* | 1,206 | 348708.07 | 439020.68 | 24844.60 | 83857.60 | 203118.25 | 439074.60 | 3204016.40 |
| *GDP Growth (%)* | 1,206 | 4.531 | 3.785 | -15.019 | 2.702 | 4.297 | 6.307 | 25.184 |
| *ln(Population)* | 1,003 | 15.172 | 1.007 | 13.110 | 14.432 | 15.313 | 15.773 | 17.493 |
| *Per Capita Disp. Income ($)* | 987 | 39604.48 | 9933.56 | 21303.00 | 32121.00 | 38002.00 | 45879.00 | 82374.00 |
| *Unemployment Rate (%)* | 1,206 | 5.373 | 2.021 | 1.800 | 3.900 | 5.000 | 6.500 | 13.500 |
| **Panel J. Treatment Variables (state-year).** | | | | | | | | |
| *High DOJ Corruption* | 1,206 | 0.495 | 0.500 | 0 | 0 | 0 | 1 | 1 |
| *Post 2016 (McDonnell)* | 1,206 | 0.329 | 0.470 | 0 | 0 | 0 | 1 | 1 |
| *Post (PTP Law = 1)* | 1,206 | 0.345 | 0.476 | 0 | 0 | 0 | 1 | 1 |
| *Donor (insurer-state)* | 517,374 | 0.272 | 0.445 | 0 | 0 | 0 | 1 | 1 |
| *PTP Law Adoption Year* | 1,206 | 965.726 | 1000.42 | 0 | 0 | 0 | 2006.00 | 2022.00 |

**Table 2. Political Contributions and Insurer Premium and Enrollment Outcomes.**

This table reports OLS estimates of the association between state-year political contributions and insurer-state-year premium and enrollment outcomes in the pre-2016 subsample. The dependent variables are ln(Total Premium), ln(Medicare Premium), ln(Medicaid Premium), ln(Total Enrollment), ln(Medicare Enrollment), and ln(Medicaid Enrollment). The main regressor is ln(Total Donations). All specifications include year fixed effects and state-level controls for real GDP, disposable income, GDP growth, and unemployment. ***, **, * stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are reported with coefficients.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| VARIABLES | *ln(Total Premium)* | *ln(Medicare Premium)* | *ln(Medicaid Premium)* | *ln(Total Enrollment)* | *ln(Medicare Enrollment)* | *ln(Medicaid Enrollment)* |
| ***ln(Total Donations)*** | **0.122**$^{***}$ | **0.140**$^{**}$ | **0.088**$^{**}$ | **0.102**$^{***}$ | **0.126**$^{**}$ | **0.110**$^{***}$ |
| | **(0.033)** | **(0.058)** | **(0.043)** | **(0.033)** | **(0.051)** | **(0.037)** |
| *Real GDP* | 0.000$^{*}$ | 0.000$^{*}$ | 0.000 | 0.000$^{***}$ | 0.000$^{***}$ | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| *Disposable Income* | 0.000 | -0.000 | 0.000 | -0.000 | -0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| *GDP Growth* | -0.024 | -0.022 | 0.003 | -0.013 | -0.034$^{**}$ | 0.009 |
| | (0.015) | (0.023) | (0.025) | (0.013) | (0.015) | (0.019) |
| *Unemployment* | 0.061 | 0.092 | -0.042 | 0.120$^{**}$ | 0.021 | 0.002 |
| | (0.053) | (0.092) | (0.087) | (0.049) | (0.068) | (0.064) |
| Observations | 11,353 | 3,689 | 2,330 | 8,246 | 2,489 | 1,991 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |

**Table 3. Pay-to-Play Laws and Political Contributions by Health Insurers.**

Staggered difference-in-differences estimates of PTP laws on state-year insurer political contributions, ln(1 + amount) by recipient type and electoral level. PTP ATT is the Callaway and Sant'Anna (2021) average treatment effect on the treated (not-yet-treated controls). Because the estimator does not report covariates, control coefficients come from a parallel two-way (state and year) fixed-effects regression on the same sample and are descriptive. Controls: ln(real GDP), ln(per-capita disposable income), GDP growth, unemployment, HHI, and ln(population). Standard errors clustered by state in parentheses. $^{***}$, $^{**}$, $^{*}$ stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are reported with coefficients.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | Total | Corp PAC | Corp→Dem | Corp→Rep | Corp→Fed | Corp→Local | Candidate | Employee |
| *PTP ATT (CS-DID)* | -0.275 | -3.112$^{**}$ | -2.193 | -3.037$^{***}$ | 0.951 | -1.896$^{*}$ | -0.330$^{**}$ | -0.029 |
| | (0.284) | (1.370) | (2.054) | (0.772) | (1.033) | (1.100) | (0.158) | (0.398) |
| *ln(Real GDP)* | -1.832$^{***}$ | -2.006 | -0.226 | -1.956 | 3.064 | -2.808 | -2.939$^{***}$ | -0.756 |
| | (0.472) | (3.061) | (2.313) | (3.454) | (2.155) | (2.565) | (0.971) | (0.492) |
| *ln(Per-capita disp. income)* | 0.980 | 3.870 | -6.906$^{**}$ | 5.994 | -11.529$^{***}$ | 4.493 | 3.697 | -0.262 |
| | (0.985) | (5.433) | (3.281) | (5.834) | (4.167) | (4.939) | (2.405) | (0.903) |
| *GDP growth (%)* | -0.004 | -0.116$^{***}$ | -0.037 | -0.103$^{***}$ | -0.080$^{**}$ | -0.049 | -0.016 | 0.018 |
| | (0.012) | (0.033) | (0.034) | (0.025) | (0.034) | (0.032) | (0.011) | (0.012) |
| *Unemployment (%)* | 0.007 | 0.057 | 0.047 | -0.018 | -0.155 | 0.120 | -0.045 | -0.039 |
| | (0.041) | (0.088) | (0.121) | (0.135) | (0.137) | (0.109) | (0.040) | (0.043) |
| *HHI (%)* | -0.892$^{***}$ | -0.504 | -0.154 | -2.357 | -0.349 | -2.210 | -0.413 | -0.238 |
| | (0.286) | (1.151) | (1.270) | (1.571) | (1.137) | (1.449) | (0.336) | (0.372) |
| *ln(Population)* | 1.992 | -2.759 | -3.635 | -2.427 | -2.291 | 2.666 | 3.710$^{***}$ | 2.415$^{**}$ |
| | (1.189) | (3.945) | (4.207) | (4.393) | (3.129) | (4.452) | (1.282) | (1.004) |
| Fixed effects | State+Year | State+Year | State+Year | State+Year | State+Year | State+Year | State+Year | State+Year |
| Observations | 977 | 977 | 977 | 977 | 977 | 977 | 977 | 977 |

## Table 4. Pay-to-Play Laws and Premiums of Donating Insurers.

Staggered difference-in-differences estimates of PTP laws on premiums written by donating insurers, aggregated to the state-year level (donating insurers only): ln(total premium), ln(Medicare premium), ln(Medicaid premium). PTP ATT is the Callaway and Sant'Anna (2021) average treatment effect on the treated (not-yet-treated controls); because the estimator does not report covariates, control coefficients come from a parallel two-way (state and year) fixed-effects regression on the same panel and are descriptive. Panel A is the full treated sample; Panel B restricts treated states to the early-adopting cohort (adopting PTP on or before 2010), with never-treated states as controls. Controls: real GDP, per-capita disposable income, GDP growth, and unemployment. Standard errors clustered by state in parentheses. ***, **, * stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are reported with coefficients.

| | (1) | (2) | (3) |
|---|---|---|---|
| | *ln(Total* | *ln(Medicare* | *ln(Medicaid* |
| VARIABLES | *Premium)* | *Premium)* | *Premium)* |
| **Panel A. Full Sample.** | | | |
| *ATT* | -0.034 | -0.683** | -0.129 |
| | (0.134) | (0.303) | (0.532) |
| Real GDP | 0.000* | -0.000** | -0.000* |
| | (0.000) | (0.000) | (0.000) |
| Per-capita disp. income | -0.000** | -0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) |
| GDP growth | 0.004 | -0.005 | -0.019 |
| | (0.005) | (0.014) | (0.014) |
| Unemployment | -0.020 | -0.078 | -0.056 |
| | (0.040) | (0.054) | (0.075) |
| Observations | 850 | 780 | 551 |
| **Panel B. Early-adopting Cohort.** | | | |
| *ATT* | -0.047 | -0.998*** | 0.216 |
| | (0.198) | (0.353) | (0.930) |
| Real GDP | 0.000* | -0.000** | -0.000* |
| | (0.000) | (0.000) | (0.000) |
| Per-capita disp. income | -0.000* | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) |
| GDP growth | 0.004 | -0.000 | -0.021 |
| | (0.006) | (0.015) | (0.015) |
| Unemployment | -0.019 | -0.040 | -0.058 |
| | (0.043) | (0.047) | (0.076) |
| Observations | 743 | 686 | 450 |

**Table 5. Heterogeneity in Pay-to-Play Effects by Insurer Size.**

Triple-difference estimates of whether PTP effects differ for smaller insurers. Low Assets equals one for insurers with below-sample-median total assets; the coefficient of interest is PTP × Low Assets. Outcomes are insurer-state-year ln(1 + amount): donations (total and candidate-directed) and premiums (total, Medicare, Medicaid). All specifications include insurer-by-state and year fixed effects and control for ln(real GDP), ln(per-capita disposable income), GDP growth, unemployment, HHI, and ln(population). Standard errors clustered by state in parentheses. ***, **, * stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are reported with coefficients.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | ln(Donations) | ln(Candidate) | ln(Total Premium) | ln(Medicare Premium) | ln(Medicaid Premium) |
| *PTP × Low Assets* | -0.502*** | -0.356*** | -0.233*** | -0.148*** | -0.058** |
| | (0.074) | (0.074) | (0.033) | (0.023) | (0.028) |
| *PTP (Post)* | 0.381*** | 0.199** | 0.115*** | 0.077*** | 0.014 |
| | (0.082) | (0.077) | (0.024) | (0.026) | (0.027) |
| *ln(Real GDP)* | 0.337* | 0.265 | 0.179* | 0.242* | 0.150 |
| | (0.198) | (0.180) | (0.103) | (0.130) | (0.160) |
| *ln(Per-capita disp. income)* | -0.632* | -0.773** | -0.293** | -0.402*** | -0.074 |
| | (0.364) | (0.329) | (0.142) | (0.137) | (0.162) |
| *GDP growth (%)* | 0.004 | 0.005** | 0.001* | 0.003*** | 0.001 |
| | (0.003) | (0.002) | (0.001) | (0.001) | (0.001) |
| *Unemployment (%)* | -0.024* | -0.030** | -0.008** | -0.001 | 0.005 |
| | (0.014) | (0.013) | (0.004) | (0.005) | (0.007) |
| *HHI (%)* | 0.029 | -0.013 | -0.021 | -0.057 | -0.131 |
| | (0.092) | (0.083) | (0.035) | (0.056) | (0.080) |
| *ln(Population)* | 0.087 | 0.261 | 0.087 | 0.014 | -0.008 |
| | (0.324) | (0.312) | (0.122) | (0.118) | (0.123) |
| Fixed effects | Insurer×State + Year | Insurer×State + Year | Insurer×State + Year | Insurer×State + Year | Insurer×State + Year |
| Observations | 485622 | 485622 | 485622 | 485622 | 485622 |

**Table 6. Pay-to-Play Laws and Regulatory Oversight.**

This table reports state-year regressions of regulatory oversight measures on the PTP-law indicator. The dependent variables are Regulatory Guidance and Year of Financial Exam, each aggregated to the state-year level from insurer supervisory data. The table reports estimates for early-adopting states and for the full sample. All specifications include state fixed effects and controls for mean ROA and mean leverage aggregated to the state-year level. Early-adopting cohort restricts the treated sample to states that adopted PTP laws on or before 2016, with never-treated states serving as controls. ***, **, * stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are reported with coefficients.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | *Regulatory* | *Regulatory* | *Year of Financial* | *Year of Financial* |
| VARIABLES | *Guidance* | *Guidance* | *Exam* | *Exam* |
| ***Post_st (PTP Law)*** | **0.243**$^{***}$ | **0.174**$^{**}$ | **0.023**$^{**}$ | **0.0299** |
| | **(0.083)** | **(0.067)** | **(0.010)** | **(0.019)** |
| *Mean ROA* | -0.001$^{*}$ | -0.001 | -0.001$^{*}$ | -0.001$^{*}$ |
| | (0.001) | (0.001) | (0.000) | (0.000) |
| *Mean Leverage* | -0.009 | -0.01 | 0.001 | 0.002 |
| | (0.006) | (0.007) | (0.002) | (0.002) |
| State FE | Yes | Yes | Yes | Yes |
| Year FE | No | No | No | No |
| Sample | Early-adopting Cohort | Full | Early-adopting Cohort | Full |
| Observations | 907 | 974 | 907 | 974 |

**Table 7. Exposure to Pay-to-Play States and Insurer Financial Outcomes.**

This table reports insurer-year fixed-effects regressions of financial outcomes on Treat_Exposure, defined as the share of an insurer's premiums written in states with PTP laws. The dependent variables are ROA, Operating ROE, Cash Ratio, Leverage, and Claim Ratio (S&P), each reported for the full sample and the pre-2016 subsample. All specifications include firm fixed effects or year fixed effects. ***, **, * stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are reported with coefficients.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| VARIABLES | *ROA* | *ROA* | *Operating ROE* | *Operating ROE* | *Cash Ratio* | *Cash Ratio* | *Leverage* | *Leverage* | *Claim Ratio (S&P)* | *Claim Ratio (S&P)* |
| ***Treat Exposure*** | **-4.965**$^{***}$ | **-6.472**$^{***}$ | **-15.938**$^{***}$ | **-21.787**$^{***}$ | **-0.065**$^{**}$ | **-0.099**$^{***}$ | **-0.038**$^{*}$ | **-0.011** | **2.459**$^{*}$ | **2.959**$^{**}$ |
| | **(1.289)** | **(1.550)** | **(5.290)** | **(7.029)** | **(0.027)** | **(0.028)** | **(0.022)** | **(0.021)** | **(1.320)** | **(1.411)** |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | No | No | No | No | No | No | No | No | No | No |
| Sample | Full | Pre-2016 | Full | Pre-2016 | Full | Pre-2016 | Full | Pre-2016 | Full | Pre-2016 |
| Observations | 4,119 | 2,410 | 4,096 | 2,401 | 4,184 | 2,455 | 4,185 | 2,455 | 3,995 | 2,355 |

**Table 8. McDonnell v. United States, High-Corruption States, and Post-2016 Donation and Premium Outcomes.**

This table reports difference-in-differences estimates around the Supreme Court's 2016 decision in McDonnell v. United States. The coefficient of interest is Post2016 × High DOJ Corruption, where High DOJ Corruption equals one for states above the median DOJ public-corruption measure. Columns 1–3 examine donation outcomes at the insurer-year level, and Columns 4–6 examine premium and premium-rate outcomes at the state-year level. Donation specifications include insurer-by-state and year fixed effects, while premium specifications include state and year fixed effects. All specifications control for real GDP, disposable income, GDP growth, unemployment, and HHI. ***, **, * stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are reported with coefficients.

| VARIABLES | (1) *ln(IC→Comm Donations)* | (2) *ln(IC→Rep Donations)* | (3) *ln(Total Donations)* | (4) *ln(Medicare Premium)* | (5) *ln(Medicaid Prem Rate)* | (6) *ln(Medicare Prem Rate)* |
|---|---|---|---|---|---|---|
| ***Post2016 × High DOJ Corruption*** | **0.118**** | **0.060*** | **0.107*** | **0.500**** | **0.231*** | **0.097** |
| | **(0.055)** | **(0.035)** | **(0.062)** | **(0.241)** | **(0.126)** | **(0.076)** |
| *Real GDP* | 0.000 | 0.000* | 0.000** | -0.000** | 0.000 | -0.000** |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| *Disposable Income* | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| *GDP Growth* | 0.000 | -0.000 | 0.003 | 0.008 | 0.006 | -0.004 |
| | (0.002) | (0.001) | (0.003) | (0.010) | (0.007) | (0.004) |
| *Unemployment* | 0.010 | -0.009 | -0.015 | -0.058 | 0.059** | 0.018 |
| | (0.007) | (0.008) | (0.012) | (0.049) | (0.029) | (0.017) |
| *HHI* | 0.062 | 0.014 | 0.038 | -3.435*** | -1.767*** | -0.627* |
| | (0.068) | (0.045) | (0.087) | (0.980) | (0.550) | (0.324) |
| Donation Level | Insurer-Year | Insurer-Year | Insurer-Year | State-Year | State-Year | State-Year |
| Insurer×State FE | Yes | Yes | Yes | — | — | — |
| State FE | — | — | — | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 483,475 | 483,456 | 483,461 | 1,073 | 763 | 815 |

**Table 9. McDonnell v. United States and State Legal Constraints: Triple Difference**

Triple-difference estimates around the 2016 McDonnell v. United States decision. The coefficient of interest, Post2016 × High DOJ Corruption × No-Strong-PTP, tests whether the post-McDonnell change in insurer political contributions was larger in high-corruption states that lacked a strong pre-2016 PTP law. High DOJ Corruption is predetermined (above-median DOJ public-corruption convictions through 2015); No-Strong-PTP equals one for states without a strong PTP law in force as of 2016. Outcomes are state-year ln(1 + amount): total, corporate/PAC, candidate-directed, and employee contributions. All specifications include state and year fixed effects and control for ln(real GDP), ln(per-capita disposable income), GDP growth, unemployment, HHI, and ln(population). Standard errors clustered by state in parentheses. ***, **, * stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are reported with coefficients.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Total Donations | Corp PAC | Candidate | Employee |
| *Post2016 × High DOJ × No-Strong-PTP* | 0.455 | 2.143* | 0.901 | 0.349 |
| | (0.442) | (1.082) | (0.558) | (0.510) |
| *Post2016 × High DOJ* | -0.262 | -1.019 | -0.615 | -0.459 |
| | (0.415) | (0.953) | (0.523) | (0.486) |
| *Post2016 × No-Strong-PTP* | -0.024 | -0.118 | -0.106 | -0.366 |
| | (0.283) | (0.793) | (0.479) | (0.412) |
| *ln(Real GDP)* | -1.747*** | -1.077 | -2.953*** | -0.858* |
| | (0.468) | (2.790) | (0.864) | (0.484) |
| *ln(Per-capita disp. income)* | 0.603 | 2.589 | 3.327 | -0.086 |
| | (0.927) | (5.077) | (2.148) | (0.897) |
| *GDP growth (%)* | -0.005 | -0.116*** | -0.016 | 0.018 |
| | (0.012) | (0.030) | (0.011) | (0.012) |
| *Unemployment (%)* | -0.006 | 0.018 | -0.055 | -0.041 |
| | (0.040) | (0.076) | (0.038) | (0.045) |
| *HHI (%)* | -0.886*** | -0.598 | -0.390 | -0.231 |
| | (0.279) | (1.057) | (0.323) | (0.366) |
| *ln(Population)* | 2.060* | -2.573 | 3.765*** | 2.628*** |
| | (1.163) | (3.957) | (1.188) | (0.934) |
| Fixed effects | State+Year | State+Year | State+Year | State+Year |
| Observations | 1173 | 1173 | 1173 | 1173 |

**Table 10. Pay-to-Play Laws and Health Care Costs.**

This table reports staggered difference-in-differences estimates of the effect of PTP laws on health care cost measures. The dependent variables are ln(Medicaid Per Capita), ln(Family Deductible), and ln(Family Premium). Average treatment effects on the treated are estimated using the Callaway and Sant'Anna (2021) staggered-adoption difference-in-differences estimator, with not-yet-treated states serving as controls. The table reports estimates for early-adopting states and for the full treated sample. All specifications control for real GDP, disposable income, GDP growth, unemployment, and HHI. Early-adopting cohort restricts the treated sample to states that adopted PTP laws on or before 2016, with never-treated states serving as controls. ***, **, * stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are reported with coefficients.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| VARIABLES | *ln(Medicaid Per Capita)* | *ln(Medicaid Per Capita)* | *ln(Family Deductible)* | *ln(Family Deductible)* | *ln(Family Premium)* | *ln(Family Premium)* |
| ***ATT (PTP Law)*** | **-0.158*** ** | **-0.037** ** | **-0.503*** ** | **-0.100** ** | **-0.118*** ** | **-0.030** |
| | **(0.039)** | **(0.017)** | **(0.073)** | **(0.047)** | **(0.034)** | **(0.019)** |
| *Real GDP* | 0.000** | 0.000** | -0.000* | -0.000** | -0.000 | -0.000 |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| *Per-capita disp. income* | 0.000 | 0.000 | -0.000 | -0.000 | 0.000 | 0.000* |
| | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) | (0.000) |
| *GDP growth (%)* | -0.003* | -0.002* | -0.004** | -0.005*** | -0.001** | -0.001** |
| | (0.001) | (0.001) | (0.002) | (0.002) | (0.000) | (0.000) |
| *Unemployment (%)* | -0.004 | -0.007 | -0.001 | -0.003 | 0.000 | 0.001 |
| | (0.008) | (0.007) | (0.007) | (0.006) | (0.002) | (0.002) |
| *HHI (%)* | -0.072 | -0.107 | 0.055 | 0.064 | -0.001 | -0.011 |
| | (0.080) | (0.083) | (0.088) | (0.080) | (0.027) | (0.025) |
| Sample | Early adopting Cohort | Full | Early adopting Cohort | Full | Early adopting Cohort | Full |
| Observations | 614 | 547 | 460 | 547 | 460 | 547 |

**Table 11. Pay-to-Play Laws, Health Care Affordability, Insurance Coverage, and Suicide Rates.**

This table reports staggered difference-in-differences estimates of PTP laws on state-year health outcomes: the share of adults reporting a cost barrier to care, the percent uninsured, ln(suicide rate per 100,000), and the share of adults without a doctor visit in the past year. PTP ATT is the Callaway and Sant'Anna (2021) average treatment effect on the treated (not-yet-treated controls); the pre-trend p-value is reported below. Because the estimator does not report covariates, control coefficients come from a parallel two-way (state and year) fixed-effects regression on the same sample and are descriptive. Controls include ln(real GDP), ln(per-capita disposable income), GDP growth, unemployment, HHI, and ln(population). Standard errors clustered by state in parentheses. $^{***}$, $^{**}$, $^{*}$ stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are reported with coefficients.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Cost Barrier | Pct. Uninsured | ln(Suicide) | No Doctor Visit |
| *ATT (PTP Law)* | -0.013** | 0.354 | -0.053** | -0.030*** |
| | (0.006) | (0.528) | (0.027) | (0.009) |
| *ln(Real GDP)* | -0.007 | 4.868 | 0.254*** | 0.035 |
| | (0.028) | (3.091) | (0.094) | (0.037) |
| *ln(Per-capita disp. income)* | -0.081** | -12.329** | -0.035 | -0.013 |
| | (0.034) | (4.628) | (0.151) | (0.047) |
| *GDP growth (%)* | 0.000 | -0.024 | -0.001 | -0.001*** |
| | (0.000) | (0.021) | (0.001) | (0.000) |
| *Unemployment (%)* | 0.003** | 0.293* | 0.003 | 0.004*** |
| | (0.001) | (0.174) | (0.004) | (0.001) |
| *HHI (%)* | -0.015 | 0.402 | -0.070 | -0.011 |
| | (0.013) | (1.701) | (0.053) | (0.017) |
| *ln(Population)* | 0.042 | -7.070*** | -0.760*** | -0.228*** |
| | (0.036) | (2.112) | (0.141) | (0.059) |
| Observations | 286 | 576 | 849 | 286 |

**Table A1. Variable Definitions.**
This table provides definitions of the variables used in the empirical tests.

| Variables | Definition |
|---|---|
| **Panel A: Dependent Variables** | |
| *ln(Total Premium)* | Natural log of total premiums written in state s, year t |
| *ln(Medicare Premium)* | Natural log of Medicare premiums written in state s, year t |
| *ln(Medicaid Premium)* | Natural log of Medicaid premiums written in state s, year t |
| *ln(Medicaid Prem Rate)* | Natural log of Medicaid premium rate (premiums per enrollee) |
| *ln(Medicare Prem Rate)* | Natural log of Medicare premium rate (premiums per enrollee) |
| *ln(Total Enrollment)* | Natural log of total enrollment in state s, year t |
| *ln(Medicare Enrollment)* | Natural log of Medicare enrollment in state s, year t |
| *ln(Medicaid Enrollment)* | Natural log of Medicaid enrollment in state s, year t |
| *Medicaid Enrollment (millions)* | Medicaid enrollment scaled by 1,000,000 |
| *ln(Total Donations)* | Natural log of total political donations (individual + corporate combined) |
| *Corp→Rep (Donations)* | Natural log of corporate donations to Republican candidates/committees |
| *Corp→Dem (Donations)* | Natural log of corporate donations to Democratic candidates/committees |
| *Indiv→Rep (Donations)* | Natural log of individual donations to Republican candidates/committees |
| *Indiv→Dem (Donations)* | Natural log of individual donations to Democratic candidates/committees |
| *IC→Comm (Donations)* | Natural log of combined individual+corporate donations to political committees |
| *IC→Rep (Donations)* | Natural log of combined individual+corporate donations to Republican candidates/committees |
| *Federal (Donations)* | Natural log of combined corporate+individual donations to federal elections |
| *Local (Donations)* | Natural log of combined corporate+individual donations to local/state elections |
| *ln(Medicaid Per Capita)* | Natural log of Medicaid spending per enrollee (CMS data) |
| *ln(Family Deductible)* | Natural log of average family health insurance deductible (MEPS) |
| *ln(Family Premium)* | Natural log of average family health insurance premium (MEPS) |
| *Cost Barrier* | Share reporting cost as a barrier to health care (BRFSS) |
| *Pct. Uninsured* | Percentage of population without health insurance (SAHIE) |
| *ln(Suicide Total)* | Natural log of total suicide rate per 100,000 (CDC WONDER) |
| *Cannot Afford Doctor (White)* | Share of White adults not seeing a doctor due to cost (KFF) |
| *Cannot Afford Doctor (Black)* | Share of Black adults not seeing a doctor due to cost (KFF) |
| *Cannot Afford Doctor (Hispanic)* | Share of Hispanic adults not seeing a doctor due to cost (KFF) |
| *Regulatory Guidance* | At the insurer-year level, this is a binary indicator (0/1) for whether the insurer received financial examination guidance. In state-year regressions, it is the mean share across insurers in the state. |

| | |
|---|---|
| *Year of Financial Exam* | At the insurer-year level, this is a binary indicator (0/1) for whether the insurer had a financial examination that year. In state-year regressions, it is the mean share across insurers in the state. |
| *ROA* | Return on assets (net income / total assets), insurer-year |
| *Operating ROE* | Operating return on equity (net underwriting gain/surplus), insurer-year |
| *Cash Ratio* | Cash and short-term investments / total assets, insurer-year |
| *Leverage* | Total liabilities / total assets, insurer-year |
| *Claim Ratio (S&P)* | Net claims incurred / net premiums earned (S&P Global Market Intelligence), insurer-year |
| *Treat Exposure* | Share of insurer's premiums from states with PTP laws |
| **Panel B: Treatment Variables** | |
| *Post_st* | A dummy that equals 1 if state s has enacted PTP law by year t and 0 otherwise |
| *gvar* | Year state s first enacted PTP law (0 = never-treated) |
| *Post2016* | A dummy that equals 1 if year ≥ 2016 (McDonnell v. United States) and 0 otherwise |
| *High DOJ Corruption* | A dummy that equals 1 if state above the median in DOJ public corruption convictions and 0 otherwise |
| *Donor* | A dummy that equals 1 if insurer has any political donation records in state s and 0 otherwise |
| *Low Total Assets* | A dummy that equals 1 if insurer's total assets are below the sample median and 0 otherwise |
| **Panel C: Control Variables** | |
| *Real GDP* | State real GDP (Bureau of Economic Analysis) |
| *Disposable Income* | State per capita disposable income |
| *GDP Growth* | Annual state GDP growth rate |
| *Unemployment* | State unemployment rate (BLS) |
| *HHI* | Herfindahl-Hirschman Index of insurance market concentration |
| *ln(Total Assets)* | Natural log of insurer total assets |
| *Leverage (control)* | Insurer liabilities / total assets |
| *ln(Population)* | Natural log of state population |
| *Mean ROA* | Mean return on assets across insurers in state s, year t |
| *Mean Leverage* | Mean leverage ratio across insurers in state s, year t |

## Table A2. States with General or Strong Pay-to-Play Laws.

This table reports the states and the District of Columbia classified as having general or strong statewide PTP laws, together with each regime's adoption year, abolition year where applicable, and a brief note describing the law's operative feature. We classify a jurisdiction as having a PTP regime when its law links political contributions to eligibility for public contracting or other covered public business through at least one of three channels: (i) bans or tight caps on contributions during covered procurement periods, (ii) contribution-triggered consequences for contract eligibility, award, or renewal, or (iii) contractor-specific disclosure requirements. Panel A lists strong statewide contractor PTP regimes, which are primarily ban-based and prohibit or sharply restrict contributions by current or prospective contractors. Panel B lists general statewide PTP regimes, which include broader statewide procurement-linked restrictions, limit or recusal rules, and disclosure-based systems that require covered firms to report contributions. "Adopt" records the first year a qualifying law is legally in force, and "Abolish" records repeal, rescission, or judicial invalidation where applicable; treatment begins in the first year the law is in force and ends upon repeal or invalidation. Coding is based on a three-step review of state statutory and regulatory provisions, administrative guidance from ethics commissions, election agencies, procurement authorities, and related administrative bodies, and cross-checks against public-policy databases and institutional summaries, including NCSL, to ensure consistent classification across jurisdictions.

| State | Adopt | Abolish | Note |
|---|---|---|---|
| **Panel A: Strong Statewide Contractor PTP** | | | |
| Connecticut | 2005 | - | Comprehensive contractor ban |
| New Jersey | 2004 | - | Broad contractor ban |
| Illinois | 2009 | - | Ban + pension explicitly covered |
| Vermont | 2014 | - | Contractor contribution ban |
| District of Columbia | 2019 | - | Contractor PTP (Law 22-250) |
| Hawaii | 1979 | - | Contractor contribution prohibition |
| New Mexico | 2006 | - | Procurement-linked restriction |
| Kentucky | 1992 | - | Non-bid contract ban (statewide) |
| Colorado | 2008 | 2010 | Amendment 54 invalidated |
| Montana | 2018 | 2021 | EO 15-2018 rescinded in 2021 |
| **Panel B: Other General Statewide PTP** | | | |
| California | 1982 | - | Limit/recusal (Levine Act) |
| Virginia | 2010 | - | Large executive/procurement window |
| Ohio | 2006 | 2008 (broad) | 2006 broad invalidated; limited noncompetitive remains |
| South Carolina | 1992 | - | Noncompetitive contract restriction |
| Maryland | 1973 | - | Disclosure-based (public business) |
| Pennsylvania | 1975 | - | Disclosure-based (non-bid contracts) |
| Rhode Island | 1993 | - | Disclosure-based |

## Table A3. Sector-Specific and Municipal PTP Regimes.

This table reports narrower PTP regimes in addition to general or strong statewide PTP laws reported in Table A1. Panel A lists sector-specific regimes limited to particular lines of public business, such as gaming- or lottery-related contracting or licensing, and housing finance. Panel B lists municipal regimes that operate only within individual local jurisdictions and therefore do not constitute statewide treatment. We include a regime in this table when the relevant rule links political contributions to eligibility for covered public business through at least one of three channels: bans or tight caps on contributions during covered procurement periods, contribution-triggered consequences for contract eligibility, award, or renewal, or contractor-specific disclosure requirements. "Adopt" records the first year the restriction is legally in force, and "Abolish" records repeal, rescission, or judicial invalidation, where applicable. These regimes are excluded from the main treatment definition because their coverage is limited by sector or locality and thus does not establish a uniform statewide PTP framework. Coding is based on a review of state statutory and regulatory provisions, administrative guidance from ethics commissions, election authorities, procurement bodies, and related agencies, and cross-checks against public-policy databases and institutional summaries, including NCSL.

| **Panel A: Sector-specific (gaming/lottery/housing/bond finance)** | | |
|---|---|---|
| State | Adopt | Abolish |
| Arkansas | 2009 | - |
| Louisiana | 1996 | - |
| Nebraska | 1995 | - |
| Nevada | 2012 | - |
| West Virginia | 1985 | - |
| Indiana | 1996 | - |
| Iowa | 1989 | - |
| Florida (FHFC) | 1997 | - |
| **Panel B: Municipal** | | |
| Jurisdiction | Adopt | Abolish |
| New York City | 2007 | - |
| Seattle (WA) | 2015 | - |

**Table A4. Placebo Tests: Nonbusiness Donations and Non-Donor Premiums.**

This table reports placebo staggered difference-in-differences tests. Columns 1 – 4 examine donation outcomes (total contributions, individual contributions to Republicans, individual contributions to Democrats, and contributions to committees) for insurers with no premium revenue in the state, and Columns 5 – 7 examine total premiums for non-donating insurers; Column 8 re-examines total donations of insurers with no premium revenue in the state. The placebo design asks whether PTP laws affect outcomes in samples where the main business-channel mechanism should be weak or absent. Average treatment effects on the treated are estimated using the Callaway and Sant'Anna (2021) staggered-adoption difference-in-differences estimator, with not-yet-treated states serving as controls; the control-variable coefficients reported below the ATT are estimated from a parallel two-way (state and year) fixed-effects regression on the same sample and are descriptive. Columns 7 and 8 add two alternative specifications: the non-donor premium excluding repeal-state years, and no-premium-insurer donations restricted to the early-adopting cohort. The early-adopting cohort restricts the treated sample to states that adopted PTP laws on or before 2016, with never-treated states serving as controls. All columns control for ln(real GDP), ln(per-capita disposable income), GDP growth, unemployment, HHI, and ln(population). ***, **, * stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are clustered at the state level and reported with coefficients.

| VARIABLES | (1) ln(Total Don) | (2) ln(Indiv→Rep) | (3) ln(Indiv→Dem) | (4) ln(IC→Comm) | (5) ln(Total Prem) | (6) ln(Total Prem) | (7) ln(Total Prem) | (8) ln(Total Don) |
|---|---|---|---|---|---|---|---|---|
| *ATT (PTP Law)* | -0.049 | -0.935 | -0.225 | 1.074 | -0.144 | -0.094 | 0.236 | 0.357 |
| | (0.382) | (0.842) | (0.427) | (1.627) | (0.310) | (0.479) | (0.384) | (0.427) |
| *ln(Real GDP)* | -1.409 | -6.728*** | -0.225 | -2.943 | 0.937 | 1.473 | 0.932 | -1.437* |
| | (0.916) | (2.298) | (1.544) | (2.416) | (1.119) | (1.109) | (1.135) | (0.833) |
| *ln(Per-capita disp. income)* | 1.630 | 12.762*** | -4.181 | -0.792 | 2.283 | 0.545 | 2.498 | 1.200 |
| | (1.054) | (3.835) | (3.120) | (3.709) | (2.226) | (1.652) | (2.337) | (1.058) |
| *GDP growth (%)* | 0.001 | -0.035* | 0.030 | -0.027 | -0.011 | -0.007 | -0.010 | 0.001 |
| | (0.018) | (0.018) | (0.026) | (0.042) | (0.009) | (0.008) | (0.009) | (0.018) |
| *Unemployment (%)* | -0.028 | -0.003 | -0.157** | 0.117 | 0.053 | 0.052 | 0.054 | -0.044 |
| | (0.055) | (0.096) | (0.076) | (0.131) | (0.049) | (0.050) | (0.050) | (0.055) |
| *HHI (%)* | -0.064 | -0.152 | -0.225 | -0.301 | -3.094*** | -2.797*** | -3.060*** | 0.074 |
| | (0.393) | (0.669) | (0.806) | (1.138) | (0.584) | (0.568) | (0.613) | (0.394) |
| *ln(Population)* | 1.710 | 5.206** | 0.802 | 1.162 | -0.516 | -1.008 | -0.373 | 1.867 |
| | (1.449) | (2.328) | (2.277) | (4.435) | (1.373) | (1.366) | (1.446) | (1.422) |
| Sample | Full | Full | Full | Full | Full | Early | Excl-repeal | Early |
| Observations | 1010 | 1010 | 1010 | 1010 | 1010 | 941 | 977 | 941 |

## Table A5. Robustness Tests: Leave-One-Out Estimation of Pay-to-Play Law Effects on Political Donations.

This table reports Callaway and Sant'Anna (2021) estimates of the effect of PTP laws on political donations, sequentially excluding each treated state from the sample. The baseline row reports the estimate using all treated states. The dependent variables are the natural log of donation amounts plus one. Columns 1–3 report results for total donations, and Columns 4–6 report results for corporate PAC donations to Democratic candidates. Coefficients remain negative and at least marginally significant across all exclusions, confirming that results are not driven by any single state. ***, **, * stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are reported with coefficients.

| | (1) | | | (2) | | |
|---|---|---|---|---|---|---|
| | *ln(Total Donations)* | | | *Corp PAC → Democrat* | | |
| Excluded State | Coefficients | S.E | p-value | Coefficients | S.E | p-value |
| State 3 | -1.218* | (0.608) | 0.051 | -1.264** | (0.580) | 0.034 |
| State 5 | -0.985* | (0.562) | 0.086 | -1.329** | (0.534) | 0.016 |
| State 6 | -1.188** | (0.569) | 0.042 | -1.358** | (0.542) | 0.016 |
| State 7 | -1.077* | (0.557) | 0.059 | -1.128** | (0.526) | 0.037 |
| State 8 | -1.127* | (0.579) | 0.057 | -1.208** | (0.557) | 0.035 |
| State 12 | -0.902* | (0.516) | 0.087 | -1.020** | (0.508) | 0.050 |
| State 13 | -1.240** | (0.557) | 0.031 | -1.260** | (0.546) | 0.025 |
| State 15 | -1.015* | (0.571) | 0.082 | -1.116** | (0.547) | 0.047 |
| State 18 | -1.229** | (0.559) | 0.033 | -1.321** | (0.544) | 0.019 |
| State 19 | -1.180** | (0.563) | 0.041 | -1.287** | (0.545) | 0.022 |
| State 20 | -1.230** | (0.565) | 0.034 | -1.189** | (0.544) | 0.034 |
| State 21 | -1.329** | (0.585) | 0.027 | -1.454** | (0.547) | 0.011 |
| State 27 | -1.289** | (0.573) | 0.029 | -1.351** | (0.553) | 0.018 |
| State 30 | -1.104* | (0.562) | 0.055 | -1.302** | (0.547) | 0.021 |
| State 32 | -1.085* | (0.585) | 0.069 | -1.144** | (0.547) | 0.042 |
| State 33 | -1.326** | (0.573) | 0.025 | -1.307** | (0.567) | 0.025 |
| State 35 | -1.224** | (0.589) | 0.043 | -1.430** | (0.548) | 0.012 |
| State 36 | -1.057* | (0.579) | 0.074 | -1.188** | (0.559) | 0.038 |
| State 39 | -1.238** | (0.562) | 0.032 | -1.311** | (0.544) | 0.020 |
| State 40 | -1.227** | (0.563) | 0.034 | -1.304** | (0.545) | 0.021 |
| State 41 | -1.168** | (0.566) | 0.044 | -1.256** | (0.544) | 0.025 |
| State 46 | -1.366** | (0.561) | 0.019 | -1.426** | (0.549) | 0.012 |
| State 47 | -1.289** | (0.588) | 0.033 | -1.290** | (0.575) | 0.029 |
| State 48 | -1.077* | (0.597) | 0.077 | -1.281** | (0.580) | 0.032 |
| State 50 | -1.100* | (0.559) | 0.055 | -1.222** | (0.540) | 0.028 |
| **Baseline (none)** | **-1.168**** | (0.560) | **0.042** | **-1.267**** | (0.540) | **0.023** |

**Table A6. Pre-Treatment Balance: PTP Adopters vs. Never-Adopters.**

This table reports the pre-treatment (2000-2003) state averages for states that ever adopt a PTP law (17) versus never-adopters (34), with the difference (adopters minus never) and a two-sample t-test. The window predates the earliest within-sample adoption.

| VARIABLES | (1)<br>Mean (Adopters) | (2)<br>Mean (Never) | (3)<br>Difference | (4)<br>t-stat | (5)<br>p-value |
|---|---|---|---|---|---|
| DOJ corruption (pre-2015) | 0.680 | 0.330 | 0.350 | -1.612 | 0.113 |
| CRI corruption index | 0.292 | 0.281 | 0.011 | -0.334 | 0.740 |
| ln(Total donations) | 10.371 | 9.878 | 0.494 | -1.126 | 0.265 |
| ln(Corp PAC donations) | 7.755 | 6.221 | 1.534 | -1.410 | 0.165 |
| ln(Total premium) | 10.858 | 10.602 | 0.256 | -0.720 | 0.475 |
| ln(Medicare premium) | 7.320 | 6.803 | 0.517 | -0.433 | 0.667 |
| ln(Medicaid premium) | 7.082 | 5.794 | 1.288 | -1.006 | 0.319 |
| HHI (%) | 0.436 | 0.432 | 0.004 | -0.061 | 0.952 |
| ln(Real GDP) | 12.221 | 11.916 | 0.305 | -0.974 | 0.335 |
| GDP growth (%) | 4.682 | 4.766 | -0.083 | 0.243 | 0.809 |
| ln(Per-capita disp. income) | 10.253 | 10.166 | 0.087 | -2.140 | 0.037 |
| Unemployment (%) | 4.849 | 4.824 | 0.025 | -0.091 | 0.928 |
| ln(Population) | 15.144 | 14.965 | 0.179 | -0.574 | 0.568 |