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

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Abstract

We examine whether campaign contributions help U.S. health insurers obtain firm-specific political rents through public program contracts and supervisory forbearance, with implications for health insurance affordability. Using insurer-state-year data from 2002 to 2022, we find that insurers' donations are associated with higher total, Medicare, and Medicaid premiums and enrollment. Our identification exploits two legal shocks that change the returns to pay-to-play in opposite directions: the staggered adoption of state pay-to-play (PTP) laws that increase legal constraints and the Supreme Court's 2016 decision in McDonnell v. United States that weakens federal anti-corruption deterrence. In staggered difference-in-differences analyses, PTP laws reduce insurer donations, lower public program premiums among donating insurers, and increase regulatory guidance. The effects are larger for smaller insurers, and insurers more exposed to PTP states experience weaker profitability, consistent with reduced political rents. Conversely, after McDonnell, donations and public program premiums rise disproportionately in high-corruption states. In states that adopt pay-to-play laws before 2016, Medicaid spending per enrollee falls by about 15%, family deductibles by about 40%, and family premiums by about 11%, and cost-related forgone care also decreases. Overall, the evidence supports a transactional rent-seeking mechanism and suggests that campaign finance is an underrecognized source of high health care costs.

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

1. Introduction

Health insurance affordability remains a central policy concern in the United States. According to KFF polling in 2024, around 50% of U.S. adults find health insurance and health care unaffordable. A large body of literature shows that market structure, program design, and regulation shape premiums, participation, and access in insurance markets. This paper asks whether campaign finance is an additional source of distortion. Health insurers are heavily regulated financial intermediaries that derive a large proportion of their revenue from public programs, primarily Medicare and Medicaid.[1] The health insurance industry is also among the largest corporate donors to political campaigns. If political giving helps health insurers secure firm-specific advantages, then campaign finance may distort both public and private insurance markets, contributing to rising health care costs.

Campaign contributions may generate insurer-specific rents through two complementary mechanisms. First, insurers may obtain more favorable access to public-program business. On this allocation margin, state officials influence insurers' access to government-financed business through contracting, licensing, pricing, and participation rules. Politically connected insurers may therefore secure more valuable public program contract awards and renewals, as well as more favorable pricing. The second mechanism is supervisory forbearance. On this oversight margin, state officials influence the intensity of prudential supervision through financial examinations and corrective guidance. Politically connected insurers may face fewer financial examinations, softer guidance, and weaker corrective pressure. These mechanisms are plausible because elected officials can influence appointments, budgets, agendas, and coordination even when they do not sign contracts or issue regulatory orders themselves. Because most health insurers' revenue concentrates in a few states and public program contracting relationships are often persistent, state-specific political favors can generate durable rents.

We test these predictions by combining political contribution records with NAIC insurer-state-year data on premiums, enrollment, and supervisory outcomes from 2002 to 2022. The

[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.

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 outcomes. 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.

To strengthen causal interpretation, we exploit two changes in the legal environment for identification. The first is the staggered adoption of statewide pay-to-play (PTP) laws. These laws raise the expected cost of exchanging campaign money for contracts or regulatory favors by banning, limiting, or requiring disclosure of contributions tied to public business. The second institutional change is the Supreme Court's 2016 ruling in McDonnell v. United States, which works in the opposite direction by raising the threshold for corruption prosecutions and lowering the expected cost of access-based exchange. Pay-to-play should be more prevalent where legal constraints are weaker, so we use PTP adoption as a shock that should reduce transactional giving and McDonnell as a shock that should increase it where institutions are weaker.

Using hand-collected state-level pay-to-play (PTP) laws, we conduct staggered difference-in-differences (DID) tests, in which treatment states are those with PTP laws legally in force. We report estimates based on the full sample and an early-adopting cohort, whose treatment timing is less exposed to post-2016 shifts in federal standards for corruption enforcement. Event-time plots show broadly similar pre-adoption trajectories for treated and never-treated states, with divergence emerging after adoption, especially for total donations and for corporate donations to Democrats and to federal election recipients. By contrast, corporate donations to local elections show weaker and less stable divergence. Consistent with those patterns, our staggered DID estimates show that PTP laws reduce aggregate insurer donations and lower public program premiums among donating insurers. In the main state-year models, PTP laws reduce total insurer donations by about 69% in the full sample and 79% in the pre-2016 sample. On the revenue side, Medicare premiums written by donating insurers fall by about 50% in the full sample and 63% in the early-adopting cohort. Placebo tests show no comparable effects on donations in states where insurers have no premium business and no comparable premium effects for non-donating insurers.

Our heterogeneity tests show that these effects are stronger for smaller insurers. Following the adoption of PTP laws, insurers with assets below the sample median experience larger declines in donations and premiums than do larger insurers. Relative to larger insurers, smaller insurers experience additional declines of about 17% in total donations, 13% in total premiums, 13% in Medicare premiums, and 7% in Medicaid premiums. 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. Therefore, PTP laws have a higher impact on smaller insurers' political donations and revenue.

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

We also find that insurers with greater exposure to PTP states have lower ROA, lower operating ROE, lower cash ratios, and higher claim ratios. These effects are economically meaningful. Moving from zero to full exposure to PTP 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. State officials who receive donations from insurers may use their discretion and networks to provide regulatory slack and more favorable access to public-program business, which increases insurers' profitability. Stronger legal constraints compress insurers' political rents and reduce operating slack.

Our DID tests based on McDonnell v. United States point in the same direction. After 2016, donations and public-program outcomes rise disproportionately more in high-corruption states than in low-corruption states. The post-2016 interaction estimates imply about 6%-13% higher insurer donations, 65% higher Medicare premiums, and 26% higher Medicaid premium rates in high-corruption states. These estimates are consistent with weaker anti-corruption constraints increasing the return to political giving where institutional frictions are greatest. Taken together, these patterns fit a transaction-specific mechanism better than a purely ideological account of political giving.

These private benefits appear to impose broader social costs. Insurers' rent-seeking should cause higher public spending and household costs due to allocative distortion and rent extraction. 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. In early-adopting states, Medicaid spending per enrollee falls by about 15% (roughly $1,000 at the sample mean), family deductibles fall by about 40% (roughly $1,100), and family premiums fall by about 11% (roughly $1,960). These cost reductions are accompanied by fewer cost-related barriers to care, lower uninsured rates, and lower suicide rates. The decline in cost-related foregone care is larger for Black adults than for White adults. These distributional results suggest that the costs of political rent-seeking fall disproportionately on socioeconomically disadvantaged groups. Campaign finance may therefore be an underrecognized source of high health insurance costs and unequal access to health care in the United States.

Our paper makes three contributions. First, we provide evidence consistent with regulatory capture in health insurance through a transactional rent-seeking mechanism. Prior work shows that political connections and campaign contributions can affect the allocation of public resources and the intensity of enforcement through municipal underwriting contracts (Butler, Fauver, and Mortal, 2009), government funding during crises (Duchin and Sosyura, 2012), lenient sanctions for fraudulent executives (Fulmer, Knill, and Yu, 2023), and pay-to-play arrangements between municipal issuers and rating agencies (Cornaggia, Cornaggia, and

Israelsen, 2023). We extend this literature to the health insurance market, where political activity is linked to public program business, supervisory outcomes, health care affordability, and health outcomes. 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.

Second, we show that legal design and institutional quality shape the returns to rent-seeking in a heavily regulated market. State PTP laws raise the cost of seeking political rents, thereby reducing insurer donations, strengthening supervisory oversight, and lowering insurance premiums and health care costs. In contrast, weaker anti-corruption enforcement after McDonnell lowers the expected cost of rent-seeking, leading to larger increases in donations and public-program outcomes in high-corruption states. By showing that legal design and institutional quality shape insurers' business and political activity, our findings complement those of Oh, Sen, and Tenekedjieva (2025), who show that regulatory frictions shape insurers' pricing and market participation. One policy implication is that targeted PTP rules can reduce rent extraction in health insurance and potentially in other heavily regulated markets with substantial administrative discretion.

Third, the paper contributes to the debate over whether campaign contributions are investments or expressions of ideology. 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 is more consistent with an investment motive for at least some insurer contributions, especially where legal constraints are weaker and the private value of political rents is plausibly greater.

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 regulatory oversight.

Health insurance is a natural setting for this question, because the industry is economically large, heavily regulated, and deeply intertwined with public spending. High industry concentration and opaque pricing make the health insurance industry prone to 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.[2] State officials, including insurance commissioners and health department officials,

[2] 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

can influence insurers' revenues and profitability by allocating Medicaid Managed Care contracts, setting or negotiating capitation rules and participation requirements, approving premium changes, licensing Managed Care Organizations (MCOs), and directing solvency supervision. [3] State officials possess substantial discretion in Medicaid contracting, as subjective assessments, rather than mechanical rules, govern dimensions such as network adequacy, plan credibility, and proposal reasonableness. State officials also indirectly influence Medicare through licensing, implementation, network enforcement, and dual-eligible coordination.

Campaign finance is especially relevant in this setting because elected officials and their appointees influence decisions that matter economically to insurers. 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).[4] Existing evidence shows that insurance regulatory discretion responds to political incentives (Liu and Liu, 2024; Tang, 2022), and Born, Karl, and Powell (2024) show that insurers donate more to candidates from the same party as incumbent insurance regulators. 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 has skyrocketed, with insurers among the top political donors.[5] Escalating election costs may

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(MCOs) at a median annual cost of about $7,800 per enrollee, which amounts to around $472 billion in total contract value.

[3] 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.

[4] 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

[5] 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

compel politicians to solicit campaign contributions more aggressively than in previous decades.

The combination of large public spending, broad regulatory discretion, and intensive political activity in health insurance creates fertile ground for political influence. These institutional features motivate the question of whether campaign contributions further distort regulation and pricing beyond the economic and institutional forces already documented. In the market for regulatory influence, politicians and regulators exchange favorable policies for political or financial support from firms (Stigler, 1971; Laffont and Tirole, 1991). When regulatory discretion is broad and enforcement constraints are weak, the expected private return on such exchanges can be high. Governors, insurance commissioners, Medicaid agencies, procurement officials, and appointed regulators all influence economic outcomes that matter for insurers. Even when politicians 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.

The relevant insurer-specific rents arise through two complementary channels. The first is public program allocation. Politically connected insurers may obtain more favorable access to government-financed business through contract awards and renewals, pricing decisions, or participation rules. These advantages should appear in higher premiums and enrollment, especially in public programs. The second channel is supervisory forbearance. Politically connected insurers may face softer examination guidance, delayed remediation, or less intensive financial review. 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

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campaign donor in recent election cycles, as on this website: https://www.opensecrets.org/political-action-committees-pacs/industry-detail/F/2022 disclosure increase the legal and reputational cost of influence-seeking. These laws make it harder to convert campaign money into transaction-specific regulatory favors and should reduce the value of political access. By contrast, weaker anti-corruption enforcement should increase the expected return to access-based exchange. 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, which can deter PTP by raising the expected legal cost. If deterrence of corruption weakens, the return to political giving should rise most in states with lower institutional quality and more persistent corruption. By contrast, if insurer contributions mainly reflect ideology or broad firm expansion, the estimates should be less sensitive to these transaction-specific legal shocks.

The value of political rents may also differ across firms. Smaller or more financially constrained insurers are likely to place greater value on political access 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. In addition, small insurers lack the resources to lobby for industry-wide policy changes, so political contributions are a more viable vehicle of rent-seeking for them. 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 this discussion, we develop four hypotheses that guide the empirical analysis.

H1 (Allocation channel). Greater insurer political giving is associated with larger premium and enrollment outcomes, especially in public programs.

H2 (Oversight channel). Political giving buys supervisory forbearance, and stricter supervisory outcomes follow legal restrictions on PTP.

H3. (Legal enforcement and institutions). Pay-to-play is stronger where anti-corruption enforcement and institutional constraints are weaker.

H4 (Firm heterogeneity). The effects of PTP restrictions should be stronger for smaller insurers, for whom politically mediated rents are more valuable.

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

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 begin with insurer-linked contribution records and aggregate them 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.

Sample sizes vary by specification and by the availability of the underlying data. The state-year donation panel used in the aggregate DID tests contains about 1,150 observations, the insurer-state-year panels contain up to about 483,000 observations, and the insurer-year financial panel contains 5,650 observations. Regressions that use supervisory or downstream health outcomes have smaller samples.

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 statewide 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 and election authorities that administer and enforce campaign finance and public integrity rules. 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, agency-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 from the Bureau of Economic Analysis and Unemployment from the Bureau of Labor Statistics. For downstream 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 SAHIE, cost barrier to accessing health care data from the Behavioral Risk Factor Surveillance System, and suicide data from CDC WONDER from the Centers for Disease Control and Prevention, and the share of adults who report not seeing a doctor in the past year because of cost, by race, from the Kaiser Family Foundation (KFF).

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. Corruption and Treatment Variables

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, 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. Wealthier populations can afford larger or more frequent contributions, so the employees of insurance companies may donate more to political campaigns if they have higher disposable income. 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 macro controls are Real GDP, Disposable Income, GDP Growth, and Unemployment. Specifications that additionally control for market structure include the Herfindahl-Hirschman Index (HHI) of insurance market concentration, which we compute from insurer premium shares within each state-year. We also control for state population, as larger populations provide insurers with larger markets, especially for government contracts with fixed per-capita pricing, and 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 A7 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 A1 and A2 summarize the institutional coding underlying our identification strategy. Table A1 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 A2 catalogs narrower sector-, agency-, 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 weaker-institution states.[6]

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[6] 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

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.

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

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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.

unemployment rate. The model includes year fixed effects, τ_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 targeted political giving using the following two-way fixed-effects difference-in-differences specification:

$$D_{st} = \theta \,\mathrm{Post}_{st} + \Gamma' X_{st} + \alpha_s + \tau_t + u_{st} \qquad (2)$$

In Equation (2), D_{st} is a state-year donation outcome. Depending on the specification, the dependent variable is the log of total donations, corporate PAC donations to Republican candidates, corporate PAC donations to Democratic candidates, corporate PAC donations to federal elections, or corporate PAC donations to local elections. The treatment indicator Post_{st} equals one once a PTP law is in force in state s in year t, and zero otherwise. The control vector X_{st} includes real GDP, disposable income, GDP growth, the unemployment rate, and the insurance-market Herfindahl-Hirschman Index. State fixed effects, α_s, absorb time-invariant differences across states, and year fixed effects, τ_t, absorb nationwide shocks common to all states. A negative estimate of θ indicates that PTP laws reduce targeted political giving, which is the first institutional implication of H3. We estimate this specification in the full sample and separately in the pre-2016 and post-2016 subsamples.

To estimate the effect of PTP laws on public-program premium allocation, we use a staggered-adoption estimator that is robust to treatment timing heterogeneity:

$$\mathrm{ATT}(g,t) = \mathrm{E}\left[\mathrm{Y}_{st}(1) - \mathrm{Y}_{st}(0) \mid \mathrm{G}_s = g,\ t \geq g\right] \qquad (3)$$

In Equation (3), Y_{st} denotes the state-year premium outcome aggregated across donating insurers only, and G_s denotes the first year in which state s adopts a PTP law, with never-treated states coded separately and not-yet-treated states serving as comparison units. We estimate this specification using the Callaway and Sant'Anna (2021) estimator. The dependent variable is alternatively the logarithm of total premiums, Medicare premiums, or Medicaid premiums. The control vector includes real GDP, disposable income, GDP growth, and the unemployment rate. We report treatment effects for the full sample and for the early-adopting cohort, together with pre-trend diagnostics.

This staggered DID design is the paper's main institutional test of H1 and H3 on the allocation margin. If PTP laws make quid pro quo exchange more difficult, then premium outcomes for donating insurers should become less favorable after adoption. The use of group-time average treatment effects avoids the bias that can arise in staggered settings under heterogeneous treatment effects, and the pre-trend results help us to determine where the identifying support is stronger and where the estimates should be treated more cautiously.

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:

$$Y_{ist} = \beta_1 \text{Post}_{st} + \beta_2 \text{LowAssets}_{it} + \beta_3 (\text{Post}_{st} \times \text{LowAssets}_{it}) + \Gamma' X_{st} + \mu_{is} + \tau_t + \varepsilon_{ist} \tag{4}$$

In Equation (4), 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, corporate donations to Republican candidates, corporate donations to Democratic candidates, total premiums, Medicare premiums, or Medicaid premiums. For donation outcomes, the control vector includes real GDP, disposable income, GDP growth, the unemployment rate, and HHI. For premium outcomes, the controls include real GDP, disposable income, GDP growth, and the unemployment rate. We add insurer-state fixed effects, μ_{is}, and year fixed effects, τ_t, to control for unobserved heterogeneity due to firm growth or macroeconomic trends. We cluster standard errors at the state level.

The coefficient of interest is β_3, which captures whether the effect of PTP laws differs for below-median-asset insurers. This heterogeneity design operationalizes H4 using below-median total assets as the empirical proxy for more limited financial capacity and private value of public insurance programs and regulatory leniency. A negative estimate of β_3 indicates that PTP laws more strongly reduce donations or premiums for smaller insurers, which support our hypothesis that insurers with less financial slack have stronger incentives to seek political rents and lose more when those rents become harder to obtain.

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 90th 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 shows that public-program exposure also varies with insurers' financial condition. Panels (a)–(d) map Medicare premium shares for high- and low-leverage insurers before and after PTP adoption, while Panels (e)–(h) present the corresponding Medicaid shares. The maps reveal substantial cross-state heterogeneity within both leverage groups, and they also show that the geographic distribution of exposure differs across Medicare and Medicaid. Although the formal heterogeneity tests below are based on total assets rather than leverage, the figure provides complementary descriptive evidence that financially different insurers operate in different public-program environments and may therefore face different incentives to seek 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 our regression results based on Equation (2). In the full sample, adoption of a PTP law is associated with a 1.168 log-point decline in total donations and a 1.267 log-point decline in corporate PAC donations to Democrats.

Table 3 also compares the association between state PTP laws and insurer political giving before and after 2016. The declines are larger and broader in the pre-2016 sample, where total donations fall by 1.543 log points, corporate PAC donations to Republicans fall by 2.157 log points, and corporate PAC donations to Democrats fall by 1.166 log points.

The post-2016 sample shows that the contraction in giving persists, but the affected channels change. Total donations remain lower after adoption, and the sharpest additional declines appear in corporate PAC donations to Democrats and to federal recipients. By contrast, local corporate PAC donations are imprecisely estimated throughout and show no consistent response. The economic magnitude of these estimates is large. Because the donation outcomes are logged and treatment is an indicator, we translate the coefficients from log points into percent changes using $100 \times (e^{\beta} - 1)$. In the full sample, the coefficient on total donations implies roughly a 69% decline after PTP adoption, while the pre-2016 estimate implies about a 79% decline. In the post-2016 sample, the coefficient on corporate PAC donations to Democrats implies an approximately 98% decline, and the coefficient on federal corporate PAC donations implies about an 82% decline. The overall pattern is therefore not a uniform collapse in all political giving, but donations most plausibly linked to transaction-specific political exchange, which is consistent with PTP laws lowering the return to rent-seeking.

[Insert Table 3 here]

Our results show that PTP laws most clearly reduce overall insurer giving and several business-linked corporate PAC channels. In the main TWFE specifications, total donations fall in every sample, Democratic-directed corporate PAC giving falls in the full and post-2016 samples, Republican-directed giving falls in the pre-2016 sample, and federal corporate PAC giving falls in the post-2016 sample. The effects on insurer donation to local elections are not significant. Overall, the evidence points to a broad contraction in targeted insurer giving after PTP adoption rather than a uniform effect on every recipient type.

Our results suggest that state PTP laws have the strongest effects in federal election donations rather than in local giving. The stronger results for federal elections may be due to politicians' career concerns in the state government. State officials running for federal positions aim to leave the state government and will face less close monitoring from the local constituency if they are successfully elected to the federal office. In contrast, state officials who run for state elections have greater career concerns in the state government, as they will continue to be closely monitored by local constituencies upon election success. Re-election incentives can improve political accountability and reduce corruption (Ferraz and Finan, 2011). State officials who run for state and local re-elections are more incentivized to protect consumers' interests and maintain high-quality and accessible health care. Given their incentive differences, federal election candidates are more likely to seek political rents using the discretionary power from their current positions than local election candidates.

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 depicts the patterns reported in Tables 3 and 4. 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. By contrast, the divergence is weaker and less stable for local donations, which is consistent with the regression evidence that local giving does not respond systematically to PTP laws.

[Insert Figure 3 here]

Overall, the evidence in this section supports H3 by showing that stronger state anti-PTP rules reduce insurer giving and appear to constrain some downstream public program rents, but the revenue effects are not uniform across all lines of business.

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 (4), 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 carriers, while the interaction term Post × Low Total Assets shows how below-median-asset insurers differ. In the full sample, the interaction term has a significantly negative coefficient for every donation and premium outcome. These interaction effects are also economically meaningful. Relative to larger insurers, below-median-asset insurers experience an additional 16.6% decline in total donations, a 12.7% decline in total premiums, a 13.1% decline in Medicare premiums, and a 6.9% decline in Medicaid premiums after PTP adoption. The sharper response among smaller insurers is consistent with our hypothesis that political rents are especially valuable for firms with less financial slack.

[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 A3 reports our placebo DID test results, which strengthen the causal interpretation of our DID tests. 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 placebo results make it less likely that the main findings are driven by general political ideology or by unrelated statewide shocks. Instead, the evidence points to business-linked contributions as the relevant channel through which legal constraints affect premium allocation. Instead, the evidence points to business-linked contributions, especially corporate PAC giving rather than local or non-business donations, as the relevant channel through which legal constraints affect premium allocation.

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 (5) below.

$$\mathrm{Oversight}_{s,t} = \alpha + \beta \mathrm{Post}_{s,t} + \gamma \mathrm{X}_{s,t} + \mu_s + \varepsilon_{s,t}, \qquad (5)$$

where $\mathrm{Post}_{s,t}$ equals one once state shas 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 our regression results based on Equation (5), which support the view that PTP laws are followed by less lenient supervision. The coefficient on $\mathrm{Post}_{s,t}$ 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 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 a reduction of 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]

Table A6 in Appendix A provides additional insurer-year evidence on the association between lagged corruption exposure and subsequent regulatory guidance. The coefficient on Lagged Corruption Exposure is negative, indicating that insurers with larger prior-year donations to high-corruption states are less likely to receive regulatory guidance in the following period, consistent with reduced forbearance following donations to high-corruption states.

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 (6) below.

$$\text{Performance}_{i,t} = \alpha + \beta\ \text{Treat\_Exposure}_{i,t} + \mu_i + \varepsilon_{i,t}, \qquad (6)$$

In Equation (6), $\text{Treat\_Exposure}_{i,t}$ 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 longrun 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).

$$\mathrm{D_{ist}} = \lambda(\mathrm{Post2016_t} \times \mathrm{HighCorr_s}) + \Gamma'\mathrm{X_{st}} + \mu_{\mathrm{is}} + \tau_{\mathrm{t}} + \nu_{\mathrm{ist}} \qquad (7)$$

In Equation (7), $\mathrm{D_{ist}}$ is alternatively the log of donations to committees, the log of donations to Republicans, or the log of total donations. The dummy $\mathrm{HighCorr_s}$ equals one if the corruption index value of a state is above the median and zero otherwise. The dummy $\mathrm{Post2016_t}$ 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 μ_{is} and τ_{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, λ. 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:

$$\mathrm{R_{st}} = \kappa(\mathrm{Post2016_t} \times \mathrm{HighCorr_s}) + \Gamma'\mathrm{X_{st}} + \alpha_{\mathrm{s}} + \tau_{\mathrm{t}} + \mathrm{e_{st}} \qquad (8)$$

In Equation (8), $\mathrm{R_{st}}$ 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, κ, 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.

Table 8 reports the regression results based on Equations (7) and (8). Across specifications, the interaction term $\mathrm{Post2016 \times High\ DOJ\ Corruption}$ 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.

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 race-specific measures of forgone doctor visits because of cost. These outcomes allow us to assess whether limiting politically mediated rent extraction improves affordability and access, and whether its burden is likely to fall disproportionately on socioeconomically disadvantaged groups.

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). If political rent-seeking distorts the allocation of health care resources and extracts value from the system, health insurance costs are higher, 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 9 shows that PTP laws are associated with lower health care costs. In the early-adopting cohort sample, 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 in the early-adopting cohort sample. 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 still imply economically meaningful reductions of about 3.6% in Medicaid spending per enrollee and 9.5% in family deductibles, or roughly $250 and $272 at the sample means. The decline in deductibles means lower out-of-pocket exposure households face when they access health care.

[Insert Table 9 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, coverage, and health outcomes accompany these cost reductions.

Table 10 reports state-year regressions for six outcomes: the BRFSS Cost Barrier measure, the uninsured rate, the log suicide rate, and three race-specific KFF measures of the share of White, Black, and Hispanic adults who report not seeing a doctor in the past year because of cost. We estimate each outcome on Post_st, an indicator that equals one after a state adopts a PTP law. We control for the macroeconomic conditions and the HHI of the health insurance industry in each state. We include year fixed effects and cluster standard errors at the state level. For outcomes measured as shares, multiplying the coefficient by 100 yields the percentage-point change. For the uninsured rate, which is already expressed in percent, the coefficient can be read directly as a percentage-point effect. For the logged suicide rate, we use $100 \times (e^{\beta} - 1)$ to express the estimate as a percent change.

In Column (1) of Table 10, PTP law adoption reduces the aggregate Cost Barrier measure by 1.4 percentage points, significant at the 5% level. Consistent with the price effects in previous tests, the evidence on consumer access supports the conclusion that PTP restrictions reduce the rents available through political influence, thereby increasing health care affordability.

[Insert Table 10 here]

Column (2) shows that PTP law adoption is associated with a 3.658 percentage-point decline in the uninsured rate, significant at the 1% level. Relative to the sample mean of about 14.3%, this corresponds to about a 25.5% decline in the uninsured share. The result suggests that limiting politically mediated rent extraction improves not only observed prices but also the extensive margin of insurance coverage.

Column (3) shows that adoption of PTP laws is associated with a 0.171 log-point decline in the total suicide rate, significant at the 1% level. Using the log-to-percentage conversion, this coefficient implies about a 15.7% decline in the suicide rate. Because suicide is affected by many factors beyond health insurance, we interpret this result more cautiously than the affordability and coverage estimates. Even so, its sign is consistent with the broader welfare channel that PTP laws reduce the rents available through political influence, which reduces cost pressures and improves downstream health outcomes.

Columns (4)–(6) provide suggestive evidence on distributional incidence. After PTP adoption, the share of adults who report not seeing a doctor because of cost falls by 1.25 percentage points for White adults and 2.35 percentage points for Black adults, both statistically significant, while the estimate for Hispanic adults is also negative, at 0.98 percentage points, but statistically insignificant. The largest affordability improvement, therefore, appears among Black adults, who appear to be more affected by higher health care costs than White adults. Taken together, the results suggest that the cost burden of political rent-seeking falls disproportionately on socioeconomically disadvantaged groups.

Overall, the evidence in Tables 9 and 10 points to meaningful welfare gains from stronger legal constraints on politically mediated rent-seeking in health insurance. After the adoption of PTP laws, states experience lower Medicaid spending per enrollee and lower family deductibles, while family premiums also decline in early-adopting states. These cost reductions are accompanied by fewer cost-related barriers to care, lower uninsured rates, and lower suicide rates. The race-specific affordability estimates further suggest that these gains are not evenly distributed. The decline in cost-related foregone care is largest for Black adults and statistically significant for White adults, while the Hispanic estimate is negative but imprecise. 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

Table A3 reports two placebo DID tests, where insurers have no business stake in the state or do not donate at all. In columns (1)–(4), we re-estimate the donation specifications using donations made in states where the insurer has 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 much weaker or absent. Consistent with that logic, the estimated ATT is small and statistically insignificant for total donations (-0.113), individual donations to Republicans (-0.431), individual donations to Democrats (0.039), and combined individual-and-corporate donations to committees (-0.945).

In columns (5)–(6), we estimate the effect of PTP laws on total premiums for 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 our expectations, the placebo estimates are economically modest and statistically insignificant in both the full sample (-0.184) and the early-adopting cohort sample (-0.183). 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.[7] 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.

[7] 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 verify the robustness of our donation results in Table 3 using two additional approaches. First, we re-estimate the effects using the Callaway and Sant'Anna (2021) staggered-adoption estimator, which allows for heterogeneous treatment effects across cohorts and time. Table A4 shows negative and statistically significant ATT estimates for total, Democratic, Republican, and local donations in most reported specifications, but the evidence is not uniform. Second, we conduct a leave-one-out analysis, sequentially excluding each treated state and re-estimating the model. Table A5 shows that the ATT remains negative and statistically significant in all cases, confirming that no single state drives our results.

9. Conclusions

This paper studies whether campaign contributions help health insurers obtain insurer-specific rents in markets where state officials influence public-program business and prudential oversight. Using insurer-state-year data on political giving, premiums, and enrollment, as well as data on supervisory outcomes and institutional constraints 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 post-2016 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 insurer giving.

The results point to favorable access to public-program business and supervisory forbearance as two complementary mechanisms. After PTP adoption, donating insurers' Medicare premiums fall most clearly, treated states receive more regulatory guidance, and early-adopting states see more frequent financial examinations, patterns consistent with reduced access rents and tighter oversight. The largest downstream welfare gains are lower Medicaid spending per enrollee and lower family deductibles, with family premiums also falling in early-adopting states. These cost reductions are accompanied by lower uninsured rates and less cost-related forgone care. The decline in foregone care because of cost is larger for Black adults than for White adults, suggesting that the burden of politically mediated rent extraction may fall disproportionately on socioeconomically disadvantaged groups.

Overall, the findings suggest that campaign finance can distort regulated health insurance markets through transaction-specific exchanges that affect contracting, pricing, and oversight. The broader implication is that legal design and institutional quality shape the returns to political rent-seeking in sectors with large public spending and administrative discretion. Targeted PTP restrictions, therefore, appear capable of reducing rent extraction and improving affordability and access in health insurance, and may have similar value in other heavily regulated markets.

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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.

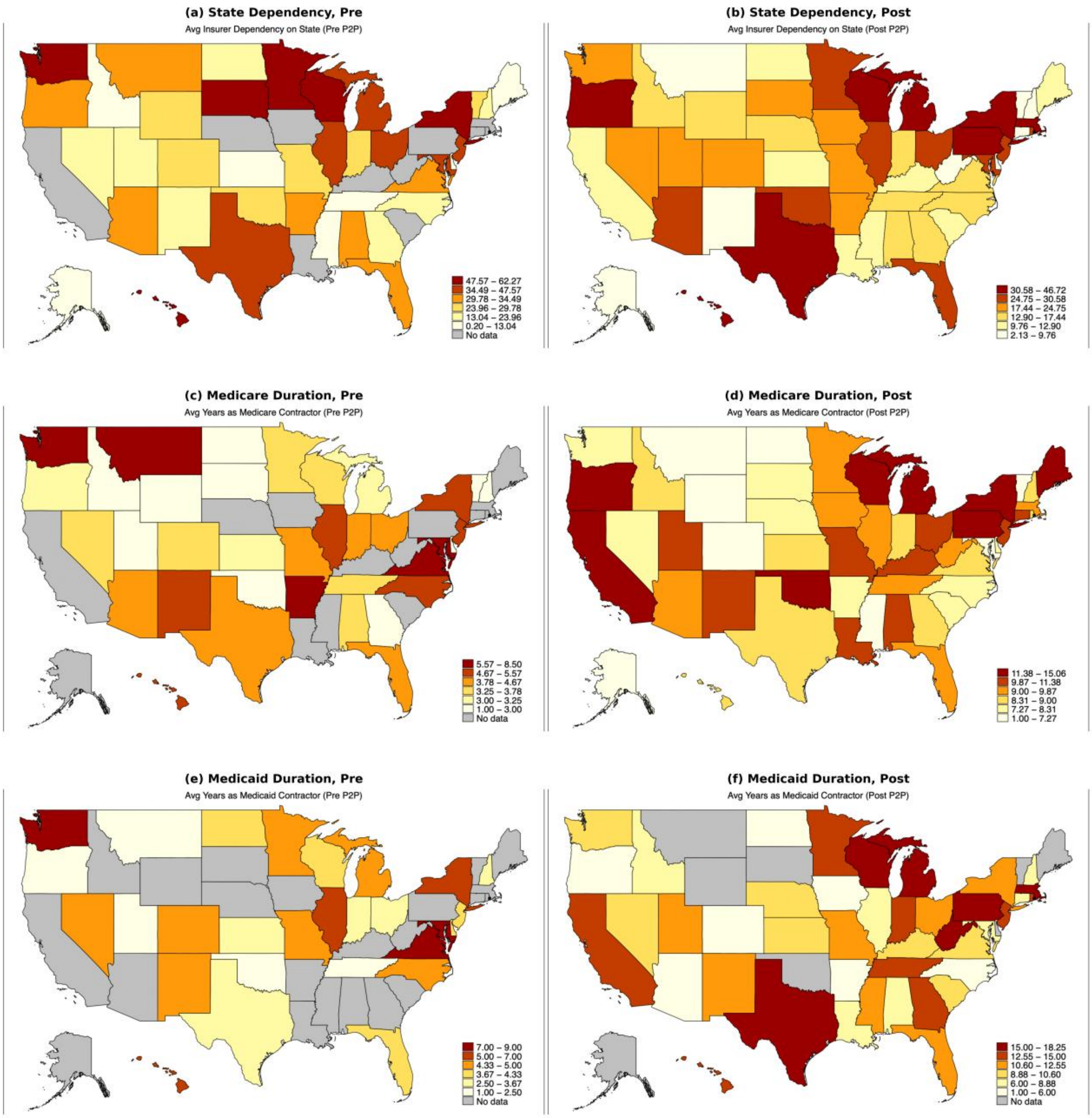

Figure 2. Medicare and Medicaid Premium Shares by Insurer Leverage.

This figure maps Medicare and Medicaid premium shares separately for high-leverage and low-leverage insurers in the pre- and post-PTP periods. Across both leverage groups, public-program premium shares vary substantially across states, and the spatial pattern differs across Medicare versus Medicaid, showing that exposure to public-program markets is highly heterogeneous even within leverage categories.

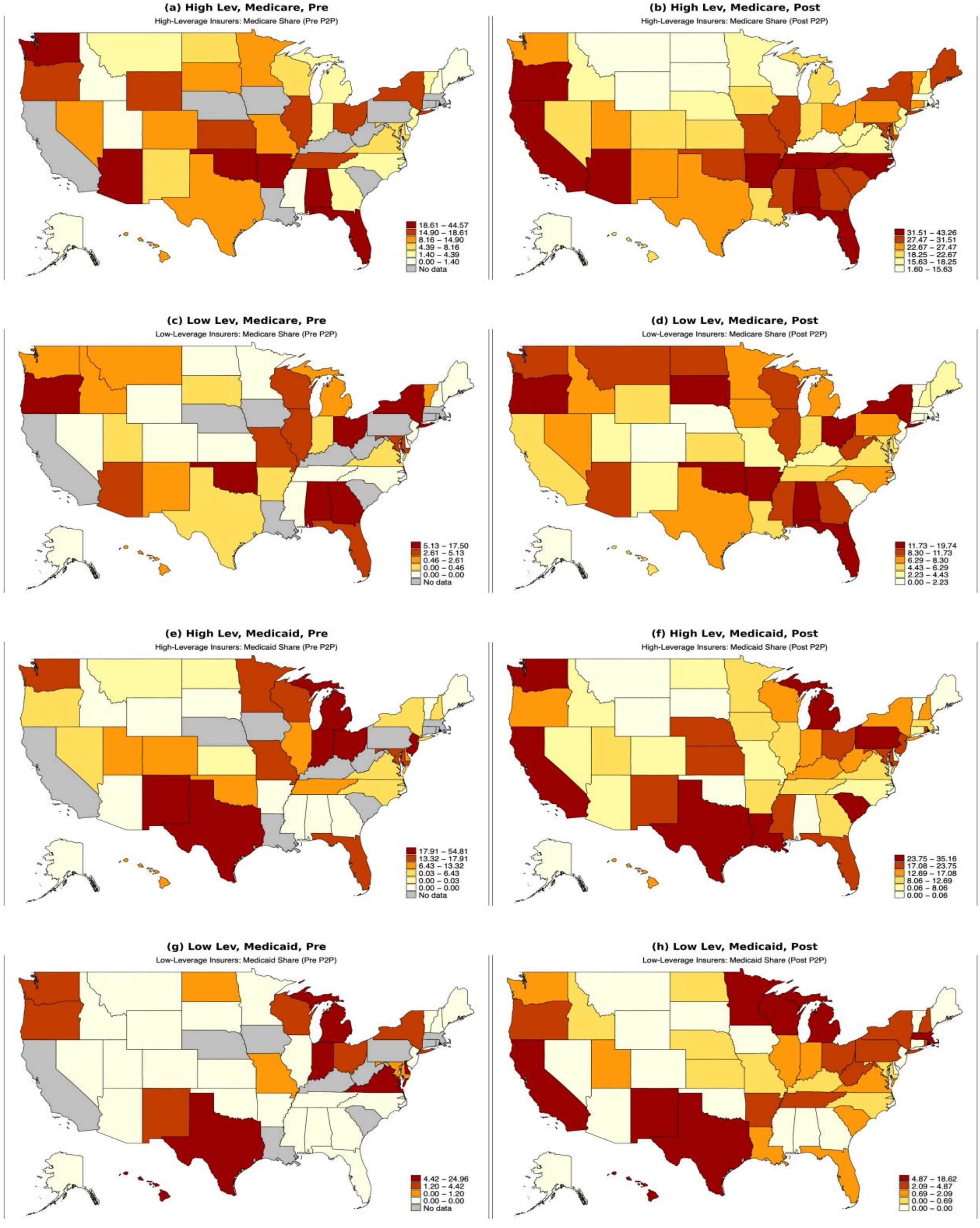

Figure 3. Normalized Event-Time Trends for Political Donations and Insurer Premiums.

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.

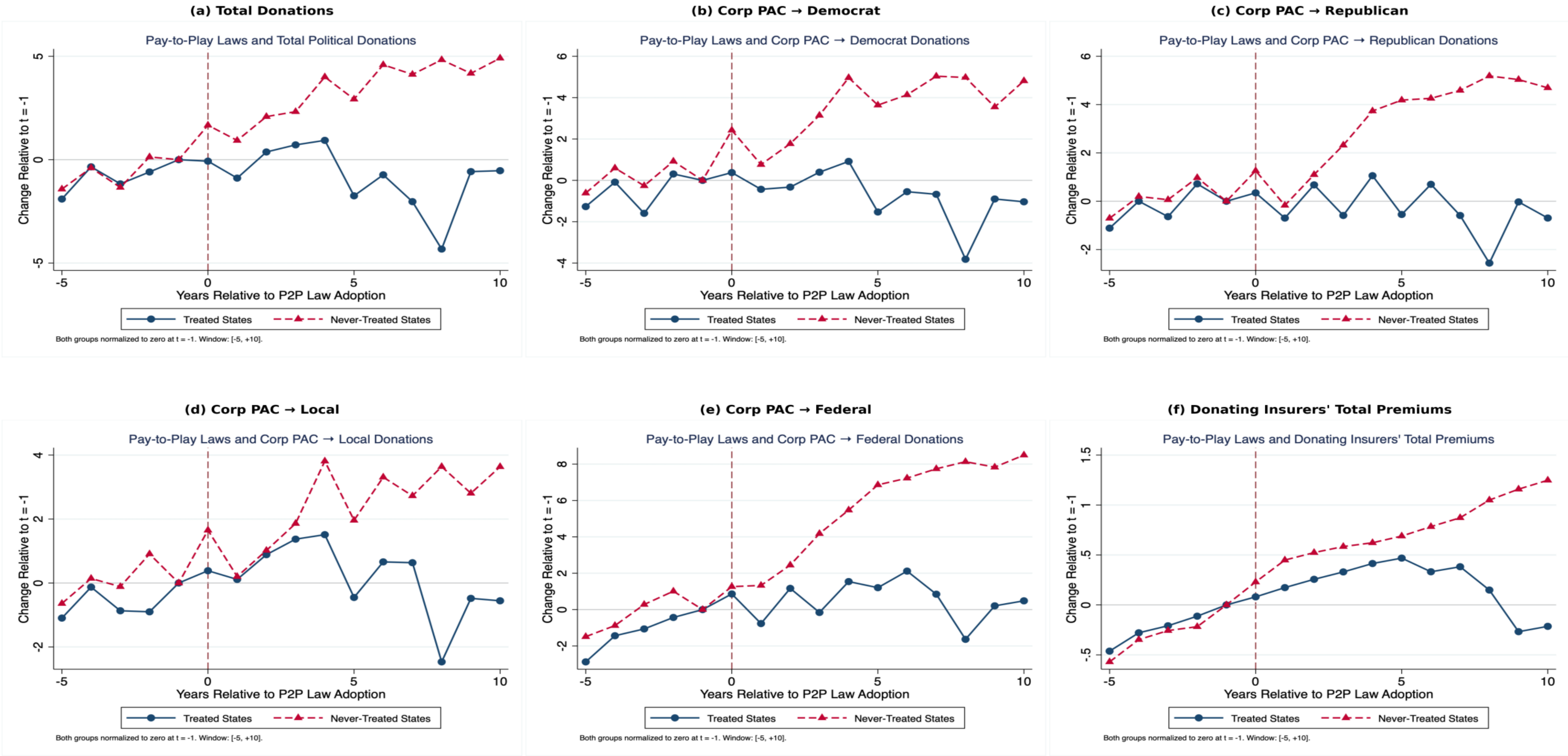

Figure 4. Medicare and Medicaid Premium Shares by Insurer Size.

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.

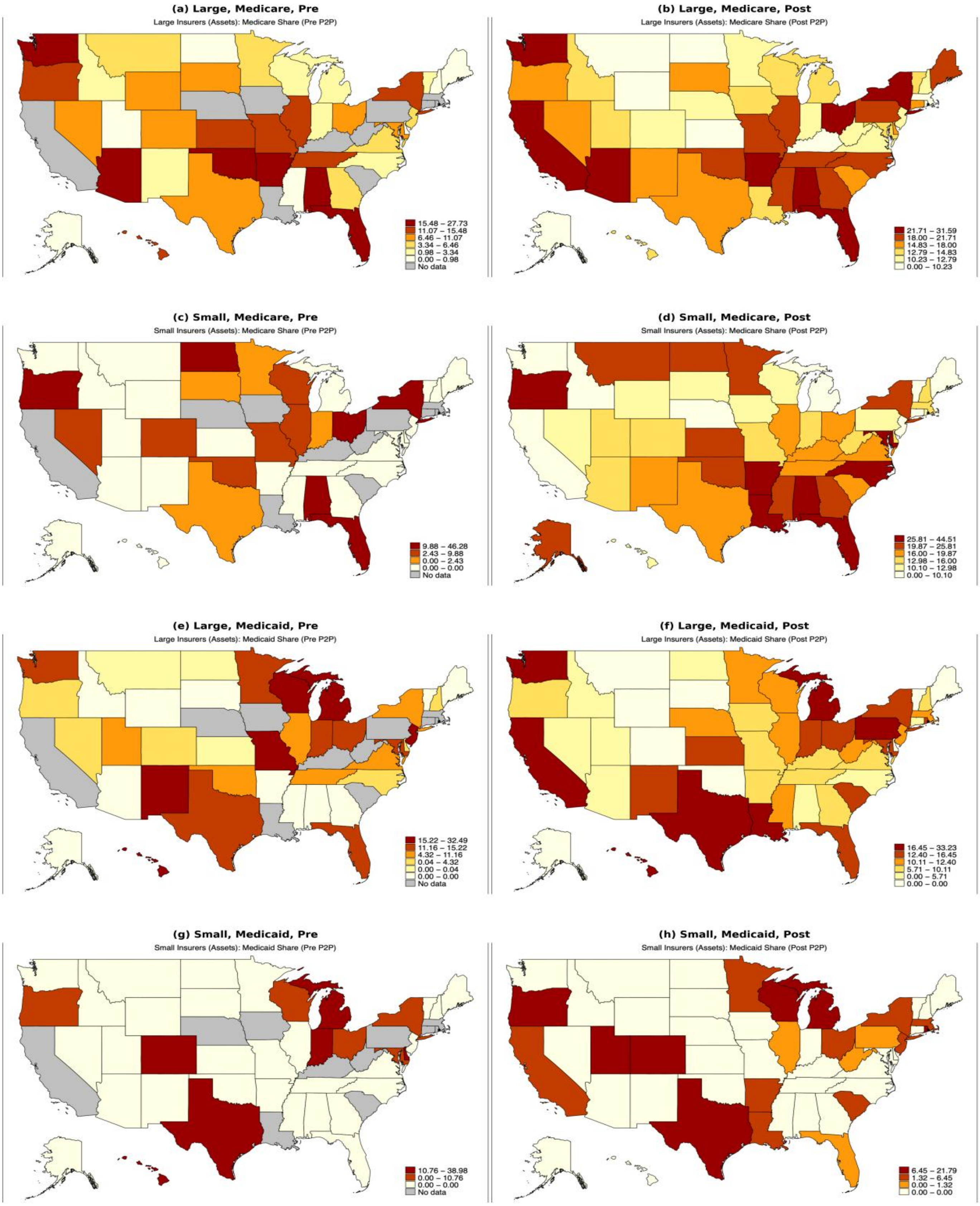

Table 1. Summary Statistics.

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. State-Level 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)_{st}. 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.

VARIABLES	(1) *ln(Total Premium)*	(2) *ln(Medicare Premium)*	(3) *ln(Medicaid Premium)*	(4) *ln(Total Enrollment)*	(5) *ln(Medicare Enrollment)*	(6) *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 Aggregate Political Contributions by Health Insurers.

This table reports state-year difference-in-differences estimates of the effect of PTP laws on aggregate political contributions by health insurers. The dependent variables are alternative state-year measures of insurer political contributions by recipient type and electoral level. Post_{st} equals one in years in which a PTP law is in force in state s and zero otherwise. The table reports estimates for the full sample, the pre-2016 sample, and the post-2016 sample. All specifications include state and year fixed effects and controls 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.

	(1)	(2)	(3)	(4)	(5)
VARIABLES	*ln(Total Donations)*	*Corp PAC → Republican*	*Corp PAC → Democrat*	*Corp PAC → Federal*	*Corp PAC → Local*
Panel A. Full Sample.					
Post_st (PTP Law)	**-1.168****	**-1.467**	**-1.267****	**-0.545**	**-0.378**
	(0.560)	**(0.996)**	**(0.540)**	**(0.611)**	**(0.624)**
Real GDP	-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)
GDP Growth	-0.011	-0.095***	-0.042	-0.081**	-0.046
	(0.033)	(0.022)	(0.035)	(0.035)	(0.030)
Unemployment	0.280**	-0.075	0.147	-0.089	0.094
	(0.125)	(0.119)	(0.129)	(0.127)	(0.117)
HHI	-5.934***	-2.643*	0.019	-0.018	-2.339*
	(1.663)	(1.356)	(1.225)	(1.085)	(1.333)
Observations	1,154	1,154	1,155	1,154	1,153
Panel B. Pre-2016.					
Post_st (PTP Law)	**-1.543****	**-2.157***	**-1.166***	**0.172**	**-0.615**
	(0.678)	**(1.098)**	**(0.604)**	**(1.007)**	**(0.506)**
Real GDP	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)

GDP Growth	-0.022	-0.111***	-0.059	-0.131***	-0.034
	(0.035)	(0.024)	(0.048)	(0.044)	(0.034)
Unemployment	0.158	-0.129	0.151	-0.268	-0.056
	(0.191)	(0.206)	(0.233)	(0.180)	(0.165)
HHI	-4.531**	-0.880	0.478	0.758	-0.222
	(1.924)	(1.265)	(1.381)	(1.225)	(1.169)
Observations	758	758	758	757	758
Panel C. Post-2016.					
Post_st (PTP Law)	**-1.391****	**-1.513**	**-3.896*****	**-1.713*****	**1.053**
	(0.584)	**(1.376)**	**(0.575)**	**(0.591)**	**(1.326)**
Real GDP	-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)
GDP Growth	0.050	0.083	-0.037	0.067	-0.056
	(0.040)	(0.071)	(0.052)	(0.045)	(0.064)
Unemployment	-0.074	-0.277	-0.230	-0.216	-0.181
	(0.066)	(0.221)	(0.196)	(0.194)	(0.201)
HHI	-4.538***	-4.111	-11.004**	-3.908*	-2.655
	(1.361)	(3.238)	(4.733)	(2.214)	(5.143)
Observations	229	229	229	229	228
State FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes

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

This table reports staggered difference-in-differences estimates of the effect of PTP laws on premiums written by donating insurers. The dependent variables are ln(Total Premium), ln(Medicare Premium), and ln(Medicaid Premium), each aggregated to the state-year level for donating insurers only. 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 the full treated sample and for early-adopting states, and it also reports pre-trend diagnostics. All specifications control for real GDP, disposable income, GDP growth, and unemployment. 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)
VARIABLES	*ln(Total Premium)*	*ln(Medicare Premium)*	*ln(Medicaid Premium)*
Panel A. Full Sample.			
ATT	**-0.034**	**-0.683****	**-0.129**
	(0.134)	**(0.303)**	**(0.532)**
Observations	850	780	551
Panel B. Early-adopting Cohort.			
ATT	**-0.047**	**-0.998*****	**0.216**
	(0.198)	**(0.353)**	**(0.930)**
Observations	743	686	450

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

This table reports triple-difference regressions that test whether the effects of PTP laws differ for below-median-asset insurers. Low Total Assets equals one for insurers with total assets below the sample median. Columns 1–3 use donation outcomes, and Columns 4–6 use premium outcomes. All specifications include insurer-by-state and year fixed effects. Donation regressions control for real GDP, disposable income, GDP growth, unemployment, and HHI, whereas premium regressions control 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.

VARIABLES	(1) *ln(Total Donations)*	(2) *ln(Corp→Rep Donations)*	(3) *ln(Corp→Dem Donations)*	(4) *ln(Total Premium)*	(5) *ln(Medicare Premium)*	(6) *ln(Medicaid Premium)*
Post_st (PTP Law)	0.066*	0.025**	0.058	0.032	0.085***	0.043**
	(0.058)	(0.016)	(0.017)	(0.037)	(0.029)	(0.021)
Post_st × Low Total Assets	**-0.181****	**-0.064*****	**-0.072*****	**-0.136*****	**-0.140*****	**-0.071*****
	(0.068)	**(0.021)**	**(0.025)**	**(0.050)**	**(0.037)**	**(0.022)**
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	0.004	-0.001	0.001	0.001	0.003***	-0.000
	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Unemployment	-0.028	-0.003	0.004	-0.008	-0.002	0.001
	(0.018)	(0.003)	(0.003)	(0.005)	(0.003)	(0.005)
HHI	0.066	-0.024	0.075**			
	(0.116)	(0.030)	(0.029)			
Insurer×State FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

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.

VARIABLES	(1) *Regulatory Guidance*	(2) *Regulatory Guidance*	(3) *Year of Financial Exam*	(4) *Year of Financial 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 and no additional controls 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. 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.

VARIABLES	(1) *ln(Medicaid Per Capita)*	(2) *ln(Medicaid Per Capita)*	(3) *ln(Family Deductible)*	(4) *ln(Family Deductible)*	(5) *ln(Family Premium)*	(6) *ln(Family Premium)*
ATT (PTP Law)	**-0.158***** **(0.039)**	**-0.037**** **(0.017)**	**-0.503***** **(0.073)**	**-0.100**** **(0.047)**	**-0.118***** **(0.034)**	**-0.030** **(0.019)**
Observations	614	547	460	547	460	547
Sample	Early-adopting Cohort	Full	Early-adopting Cohort	Full	Early-adopting Cohort	Full

Table 10. Pay-to-Play Laws, Health Care Affordability, Insurance Coverage, and Suicide Rates.

This table reports state-year regressions of affordability, coverage, and health outcomes on the PTP-law adoption indicator. Columns (1) - (3) examines aggregate health outcomes: Cost Barrier, Pct. Uninsured, and ln(Suicide Total). Columns (4) - (6) decompose health access by race, measuring the share of adults who report not seeing a doctor in the past year because of cost (KFF), separately for White, Black and Hispanic populations. Control variables include real GDP, disposable income, GDP growth, unemployment, HHI, and ln(Population). The table reports estimates for early-adopting states and for the full sample. All specifications include year fixed effects. State FE are absorbed by treatment. ***, **, * 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	*Cost Barrier*	*Pct. Uninsured*	*ln(Suicide Total)*	*Cannot Afford Doctor (White)*	*Cannot Afford Doctor (Black)*	*Cannot Afford Doctor (Hispanic)*
Post_st (PTP Law)	**-0.014****	**-3.658*****	**-0.171*****	**-0.0125****	**-0.0235*****	-0.0098
	(0.005)	**(1.113)**	**(0.049)**	**(0.0047)**	**(0.0083)**	(0.0115)
Real GDP	0.000	0.000	0.000	-0.0000	-0.0000	-0.0000***
	(0.000)	(0.000)	(0.000)	(0.0000)	(0.0000)	(0.0000)
Disposable Income	-0.000***	-0.000***	-0.000***	-0.0000***	-0.0000***	-0.0000
	(0.000)	(0.000)	(0.000)	(0.0000)	(0.0000)	(0.0000)
GDP Growth	0.000	-0.030	0.009**	0.0002	-0.0007	0.0003
	(0.000)	(0.049)	(0.004)	(0.0004)	(0.0008)	(0.0008)
Unemployment	0.007***	0.549*	0.013	0.0044***	-0.0065	-0.0033
	(0.002)	(0.277)	(0.020)	(0.0016)	(0.0043)	(0.0032)
HHI	0.031*	5.803**	0.095	0.0358**	-0.0228	0.0559**
	(0.016)	(2.868)	(0.176)	(0.0152)	(0.0277)	(0.0273)
ln(Population)	0.009**	0.250	0.872***	0.0074**	-0.0012	0.0265***
	(0.004)	(0.916)	(0.055)	(0.0037)	(0.0106)	(0.0095)
Observations	439	798	992	532	388	439
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Full	Full	Full	Full	Full	Full

Table A1. 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: 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
State	Adopt	Abolish	Note

Table A2. Sector-, Agency-, and Municipal-Specific 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. Panel B lists agency-specific regimes that apply only to a particular state authority or financing entity rather than to statewide procurement more generally. Panel C 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, agency, 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)		
State	Adopt	Abolish
Arkansas	2009	-
Louisiana	1996	-
Nebraska	1995	-
Nevada	2012	-
West Virginia	1985	-
Indiana	1996*	-
Iowa	1989*	-
Panel B: Agency-specific		
State	Adopt	Abolish
Florida (FHFC)	1997	-
Panel C: Municipal		
Jurisdiction	Adopt	Abolish
New York City	2007	-
Seattle (WA)	2015	-

Table A3. Placebo Tests: Nonbusiness Donations and Non-Donor Premiums.

This table reports placebo staggered difference-in-differences tests. Columns 1–4 examine donation outcomes for insurers with no premium revenue in the state, and Columns 5–6 examine total premiums for non-donating insurers. 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. Early-adopting cohort restricts the treated sample to states that adopted PTP laws on or before 2016, with never-treated states serving as controls. Columns 1–4 use the donation-control set, and Columns 5–6 use the premium-control set. ***, **, * 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)
	ln(Total Donations)	*ln(Indiv → Rep Donations)*	*ln(Indiv → Dem Donations)*	*ln(IC→Comm Donations)*	*ln(Total Prem) Full Sample*	*ln(Total Prem) Early-adopting Cohort*
ATT (PTP Law)	**-0.113**	**-0.431**	**0.039**	**-0.945**	**-0.184**	**-0.183**
	(0.233)	**(0.512)**	**(0.575)**	**(1.264)**	**(0.338)**	**(0.447)**
Observations	848	848	848	848	803	705

Table A4. Robustness Tests: Callaway and Sant'Anna (2021) Estimation of Pay-to-Play Law Effects on Political Donations.
This table re-estimates the effects of PTP laws on political donations in Table 3 using the Callaway and Sant'Anna (2021) staggered-adoption difference-in-differences estimator, with not-yet-treated states serving as controls. Columns 1–5 use the full sample, and Columns 6–9 restrict the treated sample to states that adopted PTP laws on or before 2016 (early-adopting cohort), with never-treated states serving as controls. The dependent variables are the natural log of donation amounts plus one. ATT reports the overall average treatment effect on the treated. Post-treatment avg report the average post-treatment event-study coefficients, respectively. ***, **, * stand for statistical significance at the 1%, 5%, and 10% level, respectively, and standard errors are reported with coefficients.

VARIABLES	(1) *ln(Total Donations)*	(2) *Corp PAC → Democrat*	(3) *Corp PAC → Republican*	(4) *Corp PAC → Local*	(5) *Corp PAC → Federal*	(6) *ln(Total Donations)*	(7) *Corp PAC → Democrat*	(8) *Corp PAC → Republican*	(9) *Corp PAC → Local*
ATT (PTP Law)	**-1.498****	**-1.841*****	**-2.564****	**-1.605****	1.594	**-1.722***	**-2.283*****	**-3.349*****	**-2.076*****
	(0.625)	(0.652)	(1.022)	(0.684)	(1.098)	(0.966)	(0.572)	(1.253)	(0.649)
Post-treatment avg	**-1.622****	**-2.133*****	**-2.770*****	**-2.019*****	**1.782***	**-1.780***	**-2.439*****	**-3.381*****	**-2.255*****
	(0.734)	(0.573)	(0.955)	(0.717)	(1.051)	(1.041)	(0.546)	(1.210)	(0.691)
Sample	Full	Full	Full	Full	Full	Early Cohort	Early Cohort	Early Cohort	Early Cohort
Observations	888	888	888	887	888	776	776	776	775

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. Lagged Corruption Exposure and Regulatory Oversight.

This table reports insurer-year fixed-effects regressions of regulatory oversight on lagged state corruption exposure. The dependent variable is Regulatory Guidance, a dummy variable that equals one if an insurer receives regulatory guidance from a financial examination in the previous election cycle and zero otherwise. Lagged Corruption Exposure is the natural log of the interaction between insurer i's total donations and the average Corruption Reflections Index (CRI) of each state, summed across all states in year t−1. Column (1) includes controls for ln(Total Assets), ROA, and leverage. Column (2) excludes firm-level controls. All specifications include insurer fixed effects and year fixed effects. Standard errors are clustered at the insurer level and reported in parentheses. ***, **, * stand for statistical significance at the 1%, 5%, and 10% level, respectively.

	(1)	(2)
VARIABLES	*Regulatory Guidance*	*Regulatory Guidance*
Lagged Corruption Exposure	**-0.066****	**-0.061****
	(0.030)	(0.026)
ln(Total Assets)	-0.009	–
	(0.015)	–
ROA	0.001	–
	(0.000)	–
Leverage	-0.097*	–
	(0.057)	–
Insurer FE	Yes	Yes
Year FE	Yes	Yes
Observations	5,234	8,580

Table A7. 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