Do Pay-to-Play Rules Protect Public Pension Funds? Evidence from Private Equity

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Abstract

Motivated by the SEC's 2026 proposal to rescind its investment adviser pay-to-play rule, we examine whether campaign contributions distort the allocation of U.S. public pension capital to private equity (PE) managers and whether contribution restrictions mitigate these distortions. Using hand-collected data on 160 public pension plans' external managers over 2000-2022, we find that managers contributing to state politicians are 3.3 percentage points more likely to manage public pension assets in that state the following year. This association is concentrated in contributions to state officials who can influence pension investments, and is weaker for plans with separate investment boards. Triple-differences estimates indicate that the gap in the association between high- and low-corruption states narrows by approximately 80% following the SEC's 2010 pay-to-play rule. Higher contributions from PE firms also weaken the positive effects of investment delegation to external managers. Using the staggered adoption of state pay-to-play laws, we find that laws banning contributions by government contractors are followed by an estimated 84% decline in reported PE contributions and higher pension returns. Together, these findings suggest that contribution restrictions may mitigate politically distorted public pension capital allocation.

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Keywords: Public pension funds; Private equity; Political contributions; Pay-to-play; Corruption.

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1 Introduction

U.S. state and local public pension funds held about $6.5 trillion in assets in fiscal year 2025 (U.S. Census Bureau, 2026) on behalf of public employees and, ultimately, taxpayers. Public pension funds manage assets both internally and through external managers, whose compensation can substantially exceed that of in-house investment staff (Dyck et al., 2022). Because the selection of external managers is made by trustees, investment officers, and elected officials who are themselves embedded in the political process, it has long been suspected that mandates can be traded for political contributions. There are concerns about pay-to-play schemes that result in high fees paid to pension funds' external managers (Morgenson, 2025; Siedle, 2013). We examine whether campaign contributions distort the allocation of public pension capital to private equity (PE) managers. An investment adviser may contribute to the campaigns of officials who can influence the award of public business, and the officials, in turn, steer that business toward the adviser.

Several prominent cases illustrate the pay-to-play mechanism. In 2010, former New York State Comptroller Alan Hevesi, who had served as sole trustee of the Common Retirement Fund, pleaded guilty to a corruption charge. He admitted awarding $250 million in pension investments to Markstone Capital Partners in return for nearly $1 million in campaign support and other benefits. As another example, CalPERS board member Sean Harrigan reportedly raised $300,000 during 2000-2004 by soliciting donations from investment firms doing business with CalPERS (Malanga, 2013).

To curb such practices, the U.S. Securities and Exchange Commission (SEC) adopted Rule 206(4)-5, its investment adviser pay-to-play rule, on June 30, 2010. The rule generally prohibits an investment adviser from providing compensated advisory services to a government client for two years after the adviser or a covered associate contributes to an official who can influence adviser selection. On September 3, 2026, however, the SEC proposed rescinding the rule, citing compliance burdens and arguing that existing antifraud provisions, fiduciary obligations, and compliance requirements are likely sufficient (U.S. Securities and Exchange Commission, 2026). This proposed shift renews the question of whether targeted contribution restrictions provide investor protection beyond these safeguards. Despite the prominent cases, systematic evidence remains limited on whether political contributions distort public pension mandate allocation and whether restrictions mitigate these distortions. Our study informs this debate and helps policymakers weigh the potential investor-protection benefits of these restrictions against their regulatory burdens.

Public pensions' investments in PE provide an informative setting for examining political influence in manager selection and the effectiveness of pay-to-play restrictions. Elected officials can influence pension investments directly through board membership or indirectly through appointments and oversight. Private equity commitments are large and long-lived, and manager quality

is difficult to assess when capital is committed, giving decision-makers substantial discretion over selection. For managers, pension relationships provide capital and fee revenue and may support subsequent fundraising, creating incentives to cultivate officials who influence investment decisions. The potential benefits extend beyond the fees charged on an individual investment. Winning pension fund mandates can generate recurring revenue and support subsequent fundraising by attracting smaller clients. Political influence may therefore affect which managers receive pension capital even without higher fee rates or larger mandates among managers already serving pensions.

Our sample comprises 160 U.S. public pension plans in 47 states, 124 for which we hand-collect external-manager identities, assets managed, and fees paid from Comprehensive Annual Financial Reports (CAFRs), and 36 reporting no PE investments. We match the identified managers to Preqin's universe of approximately 5,300 U.S.-headquartered private equity general partners (GPs). We then link firms and affiliated individuals to itemized federal, state, and local campaign contributions in the Database on Ideology, Money in Politics, and Elections (DIME; Bonica, 2014), classifying contributions by recipient state and type. Because most Preqin firms neither donate nor manage public pension assets during the sample period, we construct a matched comparison sample at the GP level. We classify as focal GPs all firms that ever donate or manage public pension assets and implement propensity score matching (PSM) using a logit propensity score based on firm size, concentration, number of funds, the share of assets in liquidated funds, and vintage. Within each Preqin primary-strategy category, we use this score to select observationally similar control GPs. Each focal GP on the common support is assigned five matches with replacement, so the final sample need not contain five distinct control firms per focal GP. We retain each selected firm's full observed state-year history. The resulting panel spans 2000-2022 and contains 2,698 firms, comprising 929 focal firms and 1,769 distinct matched control firms, and 2.25 million firm-state-year observations. For each firm and state, we observe whether the firm donates to the state's politicians and whether it serves at least one public pension plan in that state. Baseline specifications include firm fixed effects, state-by-year fixed effects, and time-varying firm controls.

We present four main findings on political contributions and PE managers' access to public pension business and on the implications for manager growth and pension performance. First, we show that political contributions are positively associated with winning pension business. In our preferred specification, a PE manager that contributes to political recipients in a state is 3.3 percentage points more likely to manage assets for at least one public pension plan in that state the following year, an economically large association compared with the unconditional probability of 0.6%. The outcome captures both new and continuing relationships. The specification includes time-varying manager characteristics as controls, manager fixed effects, and state-by-year fixed effects, accounting for persistent manager attributes and conditions common to all managers within

a state-year. Estimates thus draw on variation in each manager's giving across states and over time, conditional on these controls. The association remains robust after adding manager controls, state-level pension characteristics, and fixed effects. When contribution indicators for the three preceding years enter jointly, each coefficient is positive and statistically significant. The coefficients sum to approximately 6.8 percentage points, consistent with a persistent association between political giving and subsequent pension business.

We next examine heterogeneity in the contribution-business relationship, motivated by the hypothesis that pay-to-play is more likely where legal institutions and public pension governance are weaker. Contributions to state and local candidates and committees predict subsequent pension business, including contributions to candidates for offices that can influence pension investments, such as governor, lieutenant governor, treasurer, comptroller, attorney general, and secretary of state. By contrast, contributions to federal candidates in the same state have no statistically detectable association with pension business. This recipient pattern is consistent with political influence linked to investment authority rather than general political engagement alone. The relationship is stronger for GPs headquartered in the pension's state and in states with larger populations and pension systems, consistent with a role for local political networks and the scale of available pension business. The effect is weaker, and even disappears entirely, among plans with separate investment boards, suggesting that governance arrangements that limit political discretion in investment decisions may constrain pay-to-play.

Second, we investigate whether regulation that raises the cost of pay-to-play weakens the association between political contributions and pension business. Our principal test exploits the SEC's 2010 pay-to-play rule, with compliance required for the relevant contribution provisions from March 2011. By generally prohibiting advisers from receiving compensation from affected government clients for two years after covered contributions, the rule targets the economic incentive underlying pay-to-play. Although national in scope, its effects should be strongest where pay-to-play was initially most prevalent. We therefore estimate a triple-differences model comparing contributing and non-contributing managers across high- and low-corruption states before and after the rule, using the Corruption Reflections Index of Dincer and Johnston (2017). Event-study estimates show no systematic differential pre-rule trend, consistent with the identifying assumption. In the baseline specification, which defines 2013 onward as the post period and excludes 2012, the high-minus-low-corruption difference in this association falls from approximately 8.4 to 1.4 percentage points, a decline of about 80%. The triple-differences coefficient remains negative after excluding states with their own pay-to-play laws. Recipient-level analyses show that the association weakens primarily for contributions to state and local recipients, including incumbent officials and pension board members. We also find that the difference approaches zero within two to three years after 2013. Results are similar when the post period begins in 2011 and when corruption

is measured continuously. Together, these findings are consistent with the SEC rule constraining contribution-linked access to pension business where the association was initially strongest.

Third, we show the benefits of public pension mandates for managers and their effects on investment performance. Among existing managers of public pensions, larger donations are associated with a modestly higher share of assets sourced from public pensions, but have no statistically significant association with reported fee ratios or total mandate values. Across the full manager sample, greater pension exposure predicts subsequent growth in assets under management (AUM) and the number of active funds, consistent with the reputational and fundraising value of public pension mandates. The interaction estimates show, however, that the associations of greater pension exposure with subsequent fund internal rates of return and AUM growth become more negative as log donation amounts rise. At the pension level, PE contributions, net of contributions by mutual fund and hedge fund firms in the same state, predict a larger share of plan assets delegated to PE managers. In addition, greater delegation is associated with higher subsequent pension returns when state-level PE contributions are low, but this association weakens as contributions increase. A similar negative interaction appears for pensions' reported PE returns. These findings suggest that public mandates can support managers' business growth, while the performance results are consistent with political influence over manager selection limiting the investment benefits of delegation to external managers.

Fourth, we exploit the staggered adoption of state and local pay-to-play laws to provide complementary evidence on political contributions, delegation, and pension performance. Using the heterogeneity-robust difference-in-differences estimator of Callaway and Sant'Anna (2021), we find that contribution-ban laws are followed by an estimated 84% decline in reported PE contributions directed to a pension's state. Under the broader definition that also includes disclosure-based regimes, the estimated decline is 77%. Thus, across the two preferred specifications, adoption is followed by an estimated decline of 77%-84% in reported PE contributions.[1] Adoption is also followed by higher pension investment returns, with no statistically detectable change in the share of assets delegated to PE managers. These findings complement our evidence that PE contributions predict greater PE delegation over the following one to three years, but that the positive association between delegation and subsequent pension returns weakens as contributions increase. The estimated return advantage is fully offset at contribution levels typical of state-years with positive PE giving. The stronger contribution-business association in high-corruption states further supports our political rent-seeking hypothesis because weaker legal institutions may permit greater scope for pay-to-play. Taken together, the evidence is consistent with political giving distorting the

[1] We calculate these percentage changes as $100[\exp(\widehat{\text{ATT}}) - 1]$, where $\widehat{\text{ATT}}$ is the estimated aggregate average treatment effect on the treated. Because the outcome used in estimation is the log of one plus contributions, the transformation is exact for one plus contributions and approximates the percentage change in contribution amounts when positive contributions are large relative to one dollar.

allocation of delegated pension capital and contribution restrictions mitigating these distortions. We nevertheless interpret the law-adoption estimates cautiously because relatively few jurisdictions adopt these laws, and adoption may coincide with corruption scandals or broader governance reforms.

Political contributions may reflect ideology, local ties, information exchange, or existing pension relationships, while manager characteristics may jointly influence giving and mandates. We mitigate these concerns using lagged contributions, fixed effects, and time-varying controls. Our robustness tests show that the results persist under alternative policy dates, a continuous corruption measure, and exclusion of states with their own pay-to-play laws. As a complementary falsification test, donations to federal recipients do not predict mandates, whereas the association and its post-rule decline are concentrated among state and local recipients. Together, these findings support the interpretation that pay-to-play laws curtail politically mediated manager selection.

Our findings suggest that political contributions facilitate PE managers' access to public pension capital, particularly where corruption or political discretion is high. Political rent-seeking may reduce the investment benefits of investment delegation. The regulatory evidence suggests that targeted restrictions can constrain this channel. Following the SEC's pay-to-play rule, the contribution-business association declined more in high- than low-corruption states, while state and local pay-to-play laws were followed by sharp declines in reported contributions. These findings support safeguards around manager selection, disclosure linking managers' and affiliated individuals' contributions to officials with investment authority, and monitoring of politically connected managers' investment performance.

A simple benchmark illustrates the potential financial importance of these safeguards in the debate over the SEC's proposed repeal. An annual return benefit of approximately 0.64 basis points on the $6.51 trillion in funded state and local defined-benefit pension assets in 2025Q2 would match the SEC's projected $416 million in annual cost savings from repeal (Board of Governors of the Federal Reserve System, 2025; U.S. Securities and Exchange Commission, 2026).[2] Although our estimates do not directly identify the SEC rule's effect on pension returns, this comparison highlights the importance of weighing potential investment-performance benefits alongside compliance costs.

Our study contributes to research on public pension governance, manager selection, and investment performance. Prior work documents substantial unfunded liabilities and associated taxpayer burdens (Novy-Marx and Rauh, 2009, 2011, 2014; Giesecke and Rauh, 2023) and shows how discounting liabilities at expected asset returns can understate economic liabilities and encourage

[2]The Federal Reserve Board's Financial Accounts in September 2025 reports $6.510 trillion in funded state and local defined-benefit pension assets for 2025Q2, excluding unfunded claims on sponsors. The SEC projects $416.326 million in annual monetized savings from rescission, primarily compliance savings (U.S. Securities and Exchange Commission, 2026). Dividing projected savings by funded assets yields approximately 0.64 basis points.

risk-taking (Lucas and Zeldes, 2009; Andonov et al., 2017). Related studies associate underfunding with greater risk-taking (Mohan and Zhang, 2014) and stronger governance with greater international diversification and higher excess returns (Ammann and Ehmann, 2017; Bregnard and Salva, 2023). Research also documents local investment bias in public pension portfolios (Hochberg and Rauh, 2013; Bradley et al., 2016) and links PE performance to political representation on pension boards and finance-industry campaign contributions (Andonov et al., 2018). Studies of delegated investment management examine the performance consequences of manager selection (Goyal and Wahal, 2008) and show that consultants' recommendations and managers' connections influence capital flows and hiring without corresponding improvements in returns (Jenkinson et al., 2016; Goyal et al., 2024). We extend this literature by linking PE managers' political contributions in a state to their subsequent business with that state's public pensions and examining how contribution restrictions and board structure shape this relationship. Our findings are consistent with political rent-seeking contributing to a divergence between managers' success in attracting pension capital and pensions' investment performance. They highlight potential political agency costs in the allocation and performance of delegated capital that fees and aggregate PE allocation shares alone may not fully capture.

We also contribute to research on political influence in public contracting and financial intermediation. Prior work examines how politicians exchange access and favorable policies for campaign support (Cotton, 2012) and links political connections to government contract allocation and performance (Goldman et al., 2013; Schoenherr, 2019; Titl and Geys, 2019). Related studies associate underwriters' political contributions with higher municipal bond issuance costs (Butler et al., 2009), law firms' contributions with higher attorney fees in pension-led securities litigation (Choi et al., 2011), and higher rating fees with more favorable municipal credit ratings at investors' expense (Cornaggia et al., 2023). These findings illustrate how pay-to-play arrangements can generate private benefits while imposing broader costs. Bromberg et al. (2017) document gaps in state-level restrictions beyond the SEC's rules for investment advisers. Our findings extend this literature by highlighting access to pension capital and related business opportunities as potential returns to political giving, with possible adverse consequences for pension investment performance and funding adequacy.

Our study complements and differs from closely related papers. Beggs and Harvison (2021) find that investment advisers with previous political donations have more state and local government business. Lee (2025) uses close state elections to show that PE firms receive more pension commitments when candidates they support gain pension board seats and that investments made through these connections subsequently underperform. Chen and Denes (2026) find that the SEC rule redirects pension commitments toward first-time PE funds and improves investment performance, consistent with political connections creating entry barriers. Mittal (2026) finds that more

severely underfunded public pensions invest with smaller PE managers and earn lower average PE returns, and documents labor-productivity declines at firms financed by the most underfunded pensions. Our study complements and extends previous evidence by examining political influence in public pension investment management using granular data and sharp identification. First, we exploit the staggered adoption of state and local pay-to-play laws, using hand-collected data on their timing and jurisdictional coverage to examine changes in political giving and pension performance following adoption. Second, we examine how the association between PE delegation and overall pension returns varies with political giving, extending the analysis beyond individual commitments and PE fund performance. Third, we assess whether the association between political contributions and pension business is weaker among plans with separate investment boards, providing evidence on governance arrangements that may constrain political influence over manager selection.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background and develops our hypotheses. Section 3 describes the data, the construction of the matched estimation panel, and the variables. Section 4 lays out the empirical strategy. Section 5 presents the baseline evidence and cross-sectional heterogeneity. Section 6 presents evidence from the SEC rule. Section 7 examines GP-level consequences. Section 8 studies state and local pay-to-play laws and pension-level outcomes. Section 9 reports robustness checks, and Section 10 concludes.

2 Institutional Background and Hypotheses

2.1 Public pensions, private equity, and the selection of external managers

U.S. public defined-benefit pension plans are among the largest institutional investors in the world. Their financial condition has deteriorated substantially over the past two decades. Measured with market-based discount rates, unfunded liabilities are several times larger than those reported under governmental accounting standards (Novy-Marx and Rauh, 2009, 2011; Giesecke and Rauh, 2023). Because public-sector accounting allows liabilities to be discounted at the expected return on assets, plans have strong incentives to tilt toward higher-expected-return, higher-risk asset classes (Andonov et al., 2017). Private equity has been one of the main beneficiaries of this reach for return. Unlike public equity, which can be managed in-house or through low-cost index mandates, private equity is almost always accessed through external managers, via limited partnership commitments to funds raised by general partners. Each commitment is typically large, illiquid, and locked in for a decade or more, and the management fees on committed capital plus carried interest are substantial.

These features make the selection of PE managers both consequential and difficult to monitor. Performance is reported with long lags and is sensitive to valuation choices. Benchmarks are

contested, and the universe of potential managers is large and heterogeneous. Manager selection therefore involves considerable discretion, which can be exercised by staff, consultants, trustees, or elected officials. Lerner et al. (2007) document that public pensions earn lower returns on their private equity investments than other limited partners, and Andonov et al. (2018) show that this underperformance is concentrated among pensions whose boards have more state officials and more members elected by plan participants.

The governance of public pensions varies widely across states and plans. In a few states, a single elected official acts as sole trustee with discretionary authority over investments: the New York State Comptroller is the sole trustee of the New York State Common Retirement Fund, and the state treasurers of Connecticut and North Carolina play similar roles for their states' main funds. In most states, pension funds are governed by boards of trustees that combine ex officio state officials (such as the treasurer or controller), members appointed by the governor or legislature, and members elected by active and retired plan participants. Some states assign investment authority to a separate investment board or council, distinct from the board that administers benefits, often with a mandate to include members with investment expertise. Even where boards are formally independent, elected officials can influence manager selection directly, by voting on or signing contracts, or indirectly, by appointing trustees, shaping investment policy, selecting consultants, and passing legislation that affects the pension.

2.2 Pay to play and its regulation

The discretion involved in manager selection creates an opportunity for an exchange between asset managers and politicians. Campaigns are expensive and their cost has risen sharply over time, especially after *Citizens United v. FEC* (2010) relaxed limits on independent political expenditures. Asset managers, for their part, value public mandates not only for the fees they generate but also for the certification and scale they provide in subsequent fundraising. A manager can therefore contribute to the campaigns of officials with influence over the pension directly, through its political action committee or the personal contributions of its partners and employees, in the hope of receiving favorable treatment when mandates are allocated. Several of the most prominent pension scandals of the 2000s involved private equity, including the New York Common Retirement Fund case and the CalPERS placement-agent case.

In response, the SEC proposed Rule 206(4)-5 under the Investment Advisers Act in 2009 and adopted it in mid-2010, with compliance required from March 2011. The rule bars an adviser from receiving compensation from a government entity for two years after the adviser or a covered associate makes a triggering contribution to an incumbent or candidate for an office that can influence the entity's selection of investment advisers, such as a governor, treasurer, or comptroller. The ban

applies to both prospective and incumbent advisers, including fees under existing advisory relationships. To accommodate the rule's initial implementation period, we treat 2012 as a transition year and define the post-rule period as 2013 onward. Using 2011 instead produces nearly identical results. In September 2026, after our main donation-mandate sample ends in 2022, the SEC proposed rescinding the rule (U.S. Securities and Exchange Commission, 2026), but the proposal had not been adopted as of October 2026 and therefore does not affect our empirical setting.

In parallel with the federal rule, a number of states and municipalities adopted their own pay-to-play laws. These laws differ substantially in scope. Some impose outright bans or caps on contributions by state contractors, prospective contractors, or investment managers, including California's Levine Act, Connecticut's contractor contribution ban, Illinois's procurement code provisions, New Jersey's pay-to-play rules, and the ordinances of New York City, Philadelphia, San Francisco, and Chicago. Other laws require disclosure of contributions as a condition of contracting, while some apply only to narrow industries such as gaming, lottery, or housing finance. Appendix A documents our coding of these laws, and Figure 6 maps their geographic distribution.

2.3 Corruption and political institutions across states

U.S. states differ persistently in the quality of their political institutions and in the prevalence of public corruption (Campante and Do, 2014; Smith, 2016). Where corruption is more prevalent, officials face a lower expected cost of exchanging public favors for private benefits, enforcement is weaker, and norms are more permissive. A long literature uses corruption convictions as a measure of local corruption (e.g., Butler et al., 2009; Hochberg and Rauh, 2013). Conviction-based measures, however, reflect prosecutorial priorities and largely capture low-level officials (Cordis and Milyo, 2016). We therefore use the Corruption Reflections Index of Dincer and Johnston (2017), which is based on news coverage of corruption in the Associated Press and is designed to capture the prevalence of corruption in state politics more broadly. To avoid contamination by the policy changes we study, we average the index over the years before 2011 and classify states with above-median pre-period values as high-corruption states.

2.4 Hypotheses

Our analysis is organized around the following hypotheses.

H1 (Access to pension business).

PE managers who contribute to political recipients in a state are more likely to subsequently manage assets for public pensions in that state, through new or continuing relationships. The association is stronger for contributions to recipients who can influence pension investments.

H2 (Political discretion and business opportunities).
The contribution-business association is stronger in high-corruption states, in states with larger pension systems, and for managers headquartered in the pension's state. It is weaker for plans with separate investment boards.

H3 (Regulatory constraints).
Pay-to-play restrictions reduce PE contributions and weaken the contribution-business association, with greater attenuation where the scope for political influence was initially greater.

H4 (Value of pension business).
A larger public pension share of a manager's assets predicts greater subsequent growth in assets under management and in the number of active funds.

H5 (Allocation distortions and pension performance).
The positive association between PE delegation and subsequent pension returns weakens as PE contributions rise. Restrictions that mitigate political distortions in manager selection improve pension returns.

3 Data and Sample Construction

3.1 Data sources

Our analysis combines five main sources of data.

3.1.1 Private equity firms

We obtain the universe of private equity general partners and their funds from Preqin, accessed through WRDS. We restrict attention to GPs headquartered in the United States. From Preqin's fund details, fund performance, and manager details files we construct annual GP-level variables: total assets under management (the sum of the sizes of the GP's funds), the Herfindahl index of fund sizes within the GP (a measure of how concentrated the GP's business is in a few funds), the number of active funds, the share of the GP's AUM in funds that have been liquidated or are no longer active, the GP's founding year (vintage), its primary strategy (buyout, venture capital, growth, real estate, and so on), and AUM-weighted fund performance measures such as the internal rate of return (IRR). When a fund reports performance more than once in a calendar year, we use the latest report.

3.1.2 Public pension external managers

We hand-collect the list of external investment managers of U.S. state and local public pension plans from the plans' Comprehensive Annual Financial Reports, which typically report each manager's name, the assets it manages, and the fees paid. We match manager names to Preqin GPs using fuzzy string matching followed by manual verification, and we aggregate to the GP-pension-year level, recording whether the GP manages any of the pension's assets, the value managed, and the management fees paid. Where several plans share a single consolidated CAFR, we retain the largest plan to avoid double counting. At the pension-year level, ExtShare equals the total value managed by the identified PE external managers divided by the pension's total AUM. We cap this ratio at one (100%) and express it in percentage points in the regressions.

3.1.3 Pension characteristics

We obtain plan-level financial and actuarial information from the Public Plans Data (PPD), maintained by the Center for Retirement Research at Boston College together with MissionSquare Research Institute, the National Association of State Retirement Administrators, and the Government Finance Officers Association. The PPD reports assets, liabilities, funded ratios, contributions, membership, assumed rates of return, investment returns, and asset allocation, including the allocation to and the return on private equity. Our pension universe consists of 160 plans in 47 states: 124 plans for which we observe external managers in their CAFRs, and 36 plans that never report any private equity investment and for which the absence of PE external managers is therefore a genuine zero rather than a missing observation. Plans that invest in private equity but for which we could not obtain manager lists are excluded rather than coded as having no managers. We supplement these data with hand-collected information on the composition of pension boards, including whether a plan has a separate investment board and the total number of board members.

3.1.4 Political contributions

We obtain itemized political contributions from the Database on Ideology, Money in Politics, and Elections (DIME; Bonica, 2014), which compiles contributions to federal, state, and local candidates and committees from the Federal Election Commission and state disclosure agencies. We match contributors to Preqin GPs in two ways: contributions made by the firm itself or its political action committee (corporate contributions), and contributions by individuals whose reported employer matches the GP (individual contributions). Following the literature on executives' and employees' political giving (Ovtchinnikov and Pantaleoni, 2012; Babenko et al., 2020), and consistent with the SEC rule's definition of covered associates, our main donation measure combines both. We remove duplicate transactions, set negative amounts (refunds) to zero, and date each

contribution by its transaction date. Each contribution is assigned to the state of its recipient and classified by recipient type: federal (House, Senate, presidential, and federal committees), state and local (state legislative and executive offices, local offices, and state and local committees), key-office (candidates for governor, lieutenant governor, treasurer, comptroller or controller, attorney general, and secretary of state), and incumbent officials (recipients matched by name to sitting state officials or pension board members). The key-office category is based on the office reported for the DIME recipient and therefore includes both challengers and incumbent officials seeking reelection. To construct the incumbent-official category, we obtain the names of sitting state officials from annual editions of the Council of State Governments' *Book of the States* (Council of State Governments, various years), supplement them with our hand-collected pension board rosters, and match those names to DIME recipients. The key-office and incumbent-official categories overlap rather than being mutually exclusive. Aggregating to the GP-state-year level yields a measure of how much each GP and its affiliated individuals gave to each state's politicians in each year. We also aggregate contributions by PE firms, mutual fund companies, and hedge funds to the state-year level to construct a measure of the political activity of the broader financial sector.

3.1.5 State characteristics and corruption

We obtain state GDP and GDP growth from the Bureau of Economic Analysis, unemployment rates from the Bureau of Labor Statistics, and population from the Census Bureau. Our corruption measure is the Corruption Reflections Index (CRI) of Dincer and Johnston (2017), which we obtain from the Institute for Corruption Studies (`https://greasethewheels.org/data/`). The index is available for 1977-2013, and we average it over the years before 2011. HighCRI equals one for states whose pre-2011 average lies above the sample median. Figure 2 maps this classification. State and local pay-to-play laws are hand-coded from statutes, regulations, executive orders, and local ordinances, as described in Appendix A. We winsorize selected continuous GP, pension, performance, and macroeconomic variables at the 1st and 99th percentiles. Donation variables are not winsorized because their distribution contains many genuine zeros.

3.2 The GP-state-year panel

Our main unit of observation is a GP i, a state j, and a year t. For each GP we form all combinations with the states in our pension universe and with the years in which the GP is observed, so that a GP that never interacts with a state's pensions or politicians contributes zero rows for that state. The outcome variable, $\text{Ext}_{i,j,t}$, equals one if GP i manages assets for at least one public pension in state j in year t. The main explanatory variable, $\text{Don}_{i,j,t-k}$, equals one if GP i or its affiliated individuals made any political contribution to recipients in state j in year $t-k$. We consider

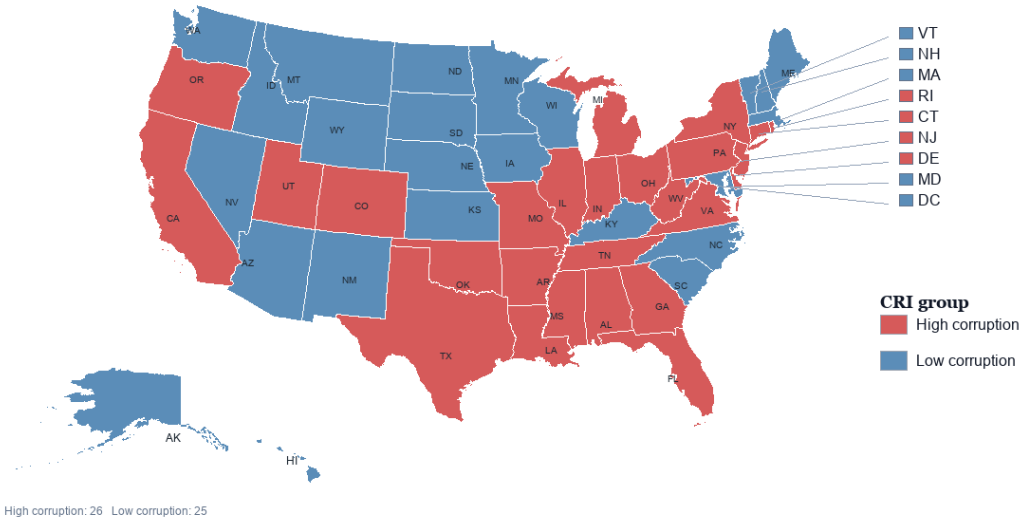


Figure 2. High- and low-corruption states.

lags $k = 1, 2, 3$, which span a full two-year election cycle and the following year. We focus on the extensive margin of donations because the distribution of donation amounts is extremely skewed. We show that results are the same when we consider only donations above $1,000, a threshold well above the SEC rule's de minimis amounts. Donation data are available through 2022. We therefore restrict GP-state-year regressions to 2000-2022 and pension-level regressions to 2001-2022.

3.3 Constructing a comparable panel: GP-level matched-sample construction

The raw panel built from the Preqin universe is very large and highly unbalanced. The overwhelming majority of U.S. PE firms in Preqin never donate to any politician in any state and never manage public pension money anywhere in our sample. These "background" GPs are, on average, much smaller and much younger than the firms that interact with public pensions and politics. Including all of them has two drawbacks. First, it generates millions of observations that contain no variation in either donation or external-manager status within GP, which raises computational cost without adding identifying information. Second, and more importantly, it makes the estimation sample depend on Preqin's coverage of small and newly formed firms, so that the comparison set used to estimate state-year fixed effects and control-variable coefficients is dominated by firms that are not plausible competitors for public mandates.

We address this problem by constructing a matched comparison sample at the level of the GP, rather than at the level of the GP-state-year observation (Rosenbaum and Rubin, 1983; Dehejia

and Wahba, 1999; Stuart, 2010; Imbens, 2015). We use a logit selection score only to choose a comparable subset from the large pool of control GPs. Focal status is a sample-selection indicator, not a treatment, and the matching procedure is not a treatment-effect estimator. Identification comes from the panel regressions described below. The key design principle is that matching selects which *firms* enter the estimation panel, but never selects observations *within* a firm. We proceed in four steps.

First, we classify a GP as *focal* if, at any time and in any state during 2000-2022, it served as an external manager of a public pension in our sample or made a political contribution. These are the firms whose behavior we wish to study. All remaining GPs form the control-GP pool. They never manage public pension money and never donate, anywhere. Using an "or" definition ensures that we do not retain only successful donor-manager combinations because both donors that never obtain a mandate and managers that never donate are included.

Second, we collapse the data to one observation per GP and compute matching covariates: the GP's average log AUM, average AUM Herfindahl index, average number of funds, and average share of AUM in liquidated funds, each computed over the GP's entire sample history, together with the GP's founding year. We use whole-period averages for all firms, rather than pre-period averages, because the panel includes many GPs founded after any natural pre-period cutoff. Using one consistent definition for every GP avoids constructing covariates differently for old and young firms. These averages are used to select the comparison sample, not as pre-treatment covariates in a causal matching design. Founding year is missing for some GPs, so we impute the sample mean. We also record the GP's primary strategy as reported by Preqin, coding missing strategy as a separate "unknown" category.

Third, we estimate the selection score with a logit model,

$$\Pr(\text{Focal}_i = 1 \mid X_i) = \Lambda\left(\alpha_s + X_i'\theta\right), \tag{1}$$

where X_i contains the five covariates above, and the dependent variable indicates whether the GP is focal rather than a control GP. We match each focal GP on the common support to its five nearest control GPs, with replacement. Matching is carried out separately within each primary-strategy stratum s, so that a focal GP can be matched only to control GPs that pursue the same strategy. Exact matching on strategy guarantees that buyout firms are compared with buyout firms and venture capital firms with venture capital firms, which is important because strategies differ markedly in typical fund size, holding period, and the likelihood of attracting public pension capital.

Fourth, we retain every focal GP on the common support and every control GP selected as a match, and we keep the *entire* GP-state-year history of each retained firm, including all of its zero rows. Retaining zero rows for kept firms is essential. Deleting zero rows within a firm would

select on the outcome and bias any within-firm comparison, whereas restricting which firms enter the panel only changes the composition of the comparison sample.

Table III reports the results. Panel A shows that, of 5,292 GPs with complete matching covariates, 986 are focal and 4,306 are controls. The matched sample retains 929 focal GPs (94%) and 1,769 distinct control GPs, for a total of 2,698 firms. The remaining focal GPs lack usable control matches within the common support of their primary-strategy categories and are therefore excluded. Before matching, focal GPs are substantially larger (a standardized mean difference, SMD, in log AUM of 1.02), more likely to have liquidated funds (SMD of 0.73), and much older (founded on average in 2007 versus 2016, an SMD of -1.01). Matching reduces every one of these differences: the SMD for log AUM falls to 0.56, that for the share of liquidated funds to 0.42, that for vintage to -0.57, and that for the AUM Herfindahl index from -0.40 to -0.25. The number of funds is balanced before and after matching. The residual imbalance reflects a genuine feature of the industry. Most large and long-established PE firms are themselves focal because they either donate or manage public pension capital, so the control-GP pool contains few large, old firms to which focal GPs can be matched. In earlier versions of the procedure we experimented with stratifying on size quartiles. This discarded focal GPs without materially improving balance because the shortage of large, old control firms is a feature of the pool rather than of the matching algorithm. Exact matching on strategy, by contrast, guarantees strategy balance by construction within each matched pair while retaining nearly all focal GPs.

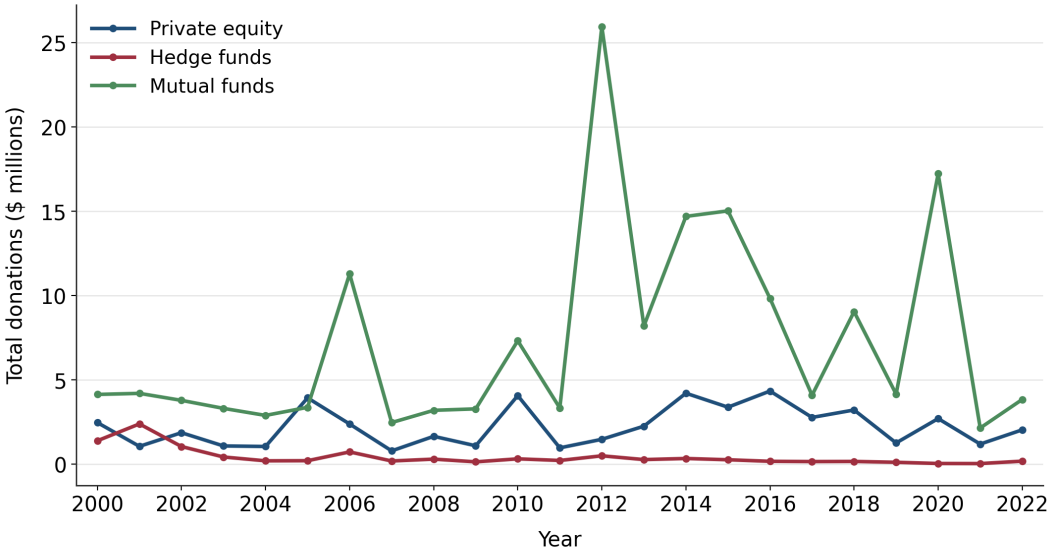
Two features of our design mitigate the consequences of the remaining imbalance. First, matching is used only to construct the comparison sample. All of our regressions include GP fixed effects, which absorb any time-invariant difference between focal and control firms, including size class, vintage, and strategy. They also include time-varying GP controls for log total assets under management (lnAUM), the Herfindahl index of fund-size concentration (AUMHHI), the number of active funds (NumFunds), and the share of AUM in dead or merged funds (DeadAUM). Second, the identifying variation in our main specifications comes from the same GP's donation activity across states and over time, compared within a state-year. The control GPs primarily help pin down the state-by-year fixed effects and the control-variable coefficients. They are not an untreated comparison group in a causal matching design. Panels B and C of Table III show how matching changes the composition of the GP-state-year panel. The raw panel contains 3.56 million GP-state-year observations. The matched panel contains 2.25 million, covering 46 states. Among focal GPs, the external-manager rate is 1.6% before and 1.4% after matching, and the donation rate is about 0.1% of GP-state-year cells in both samples. When we compare donating and non-donating GP-state-year observations directly (Panel C), matching reduces the SMD of log AUM from 0.58 to 0.25, of the share of liquidated funds from 0.40 to 0.08, and of vintage from -0.57 to -0.15, and essentially eliminates the difference in AUM concentration.

3.4 Variables and summary statistics

Table I defines all variables. Table II reports summary statistics for the estimation samples. Panel A describes the matched GP-state-year panel. The external-manager indicator has a mean of 0.006. Thus, in a typical GP-state-year, a matched GP has a 0.6% probability of managing money for the state's pensions. Positive donations occur in about 0.045% of GP-state-year cells, or 1,020 cells in total. Panel B describes donations and mandates conditional on being positive. Conditional on donating, the mean log donation is 6.9, corresponding to roughly $1,000. Donations to state and local recipients are more common (715 GP-state-years) than donations to federal recipients (326), and donations to key-office recipients and incumbent officials are rarer but larger on average. Conditional on being an external manager, the mean log external value is 17.0, corresponding to mandates of roughly $25 million, and the median fee ratio is 1.2% of assets managed, with a right-skewed distribution.

Panel C describes GP characteristics at the GP-year level. The average matched GP has log AUM of 6.2, a Herfindahl index of fund sizes of 0.61, and 3.7 active funds. About 47% of AUM is in liquidated funds on average, reflecting the long history of many matched firms. Panel D describes pensions. The average plan in our sample has log assets of 15.2, a funded ratio of 77%, an assumed rate of return of 7.5%, and a one-year investment return of 6.8%. ExtShare averages 11.9% with a median of zero, reflecting the fact that many plans in the sample report no PE managers in a given year. Panel E shows that 11% of pension-years have a separate investment board and that the average board has 9.6 members. Panel F reports state characteristics. By construction, roughly half of the state-years are in high-corruption states.

Figure 1 plots total political contributions, in millions of dollars, by PE firms, hedge funds, and mutual fund companies matched to our campaign-finance data, summed across all recipients by year. Mutual fund companies give the most and exhibit a strong electoral cycle, with spikes in presidential and midterm election years. PE contributions are smaller in aggregate, averaging about $2 to $4 million per year, and are more stable over time. Hedge fund contributions are the smallest and decline over the sample period. The differences across financial-sector types motivate our "net" donation measure for pension-level regressions, which subtracts mutual fund and hedge fund contributions from PE contributions to isolate the PE-specific component of financial-sector political activity in a state.



Source: matched campaign-finance donation records by GP/manager type, national totals by year.

Figure 1. Political donations by financial-sector type, 2000-2022.

4 Empirical Strategy

4.1 Baseline specification

Our baseline specification is a linear probability model at the GP-state-year level:

$$\text{Ext}_{i,j,t} = \sum_{k=1}^{K} \beta_k \, \text{Don}_{i,j,t-k} + \gamma' Z_{i,t} + \mu_i + \lambda_{j,t} + \varepsilon_{i,j,t}, \tag{2}$$

where i indexes GPs, j states, and t years. We set $K = 1$ in our most parsimonious specification and $K = 3$ when lags enter jointly. $Z_{i,t}$ contains log GP total assets under management (lnAUM), the Herfindahl index of the GP's fund-size concentration (AUMHHI), the share of GP AUM in dead or merged funds (DeadAUM), the number of active funds (NumFunds), and an indicator for whether the GP is headquartered in state j (SameState). μ_i is a GP fixed effect and $\lambda_{j,t}$ is a state-by-year fixed effect. We also report specifications with only year fixed effects, with separate state and year fixed effects, and with state-level aggregates of log total pension assets (lnPenAssets), the pension funded ratio (FundedRatio), the beneficiary share of plan members (RetireeShare), total contributions relative to pension assets (Contrib), and the GASB-standard assumed rate of return (AssumedRet).

The GP fixed effect absorbs all time-invariant GP characteristics that might jointly drive donations and mandates, including scale, reputation, track record, strategy, political orientation, and a general propensity to engage with government. The state-by-year fixed effect absorbs every shock

that affects all GPs equally in a state-year, including the identity and party of state officials, the electoral calendar, the size and funding status of the state's pensions, changes in their private equity targets, and state-level regulatory changes. The coefficient β_k is therefore identified from whether a given GP is more likely to manage a given state's pension money in the years after it donated to that state's politicians, relative to the same GP's status in other states and other years and relative to other GPs in the same state-year. We lag donations by at least one year so that contributions precede the mandate outcome, which mitigates concerns that incumbent managers donate because they already hold a mandate. Standard errors are two-way clustered by GP and by state (Cameron et al., 2011), which allows for arbitrary correlation of errors within a GP across states and years and within a state across GPs and years. We use a linear probability model for main results rather than a logit or probit because the large number of fixed effects makes nonlinear estimators subject to an incidental-parameters problem. We estimate all models with the high-dimensional fixed-effects estimator of Correia (2016).

4.2 The SEC pay-to-play rule: difference-in-differences and triple differences

To test whether the donation-mandate relationship reflects pay to play, we exploit the introduction of SEC Rule 206(4)-5. Let Post_t equal one for years 2013 and later, so that the post period begins once the rule's two-year look-back has fully elapsed. We drop the transition year 2012 from these regressions but compute lagged donations on the full, gap-free panel before doing so. Our difference-in-differences (DID) specification is

$$\text{Ext}_{i,j,t} = \beta_1 \text{Don}_{i,j,t-1} + \beta_2 \text{Don}_{i,j,t-1} \times \text{Post}_t + \beta_3 \text{Post}_t + \gamma' Z_{i,t} + \delta' W_{j,t} + \mu_i + \varepsilon_{i,j,t}, \qquad (3)$$

where $W_{j,t}$ contains the log of state gross domestic product (lnGDP), the state GDP growth rate (GDPGrowth), the state unemployment rate (Unemp), and the log of state population (lnPop). If the rule raised the cost of exchanging contributions for mandates, β_2 should be negative.

Because the rule applies to all registered advisers at the same time, the DID coefficient compares the donation-mandate link before and after 2013, and it could be contaminated by other changes over the same period, such as the growth of private equity allocations or the post-*Citizens United* rise in independent expenditures. To address this concern, we use the cross-sectional prediction of the pay-to-play hypothesis. The rule should have the largest effect where pay to play was most prevalent beforehand, namely in states with weak political institutions. Our triple-differences

(DDD) specification is

$$\begin{aligned}\text{Ext}_{i,j,t} = {} & \beta_1 \,\text{Don}_{i,j,t-1} + \beta_2 \,\text{Don}_{i,j,t-1} \times \text{Post}_t + \beta_3 \,\text{Don}_{i,j,t-1} \times \text{HighCRI}_j \\ & + \beta_4 \,\text{Don}_{i,j,t-1} \times \text{HighCRI}_j \times \text{Post}_t + \beta_5 \,\text{HighCRI}_j \times \text{Post}_t + \beta_6 \,\text{HighCRI}_j + \beta_7 \,\text{Post}_t \\ & + \gamma' Z_{i,t} + \delta' W_{j,t} + \mu_i + \varepsilon_{i,j,t}, \end{aligned} \tag{4}$$

where HighCRI_j is an indicator for states with above-median pre-2011 corruption. Before the rule, the donation effect is β_1 in low-corruption states and $\beta_1 + \beta_3$ in high-corruption states. The pay-to-play hypothesis predicts $\beta_3 > 0$. After the rule, the high-corruption premium becomes $\beta_3 + \beta_4$, so the hypothesis that the rule curtailed pay to play predicts $\beta_4 < 0$. In our "DDD FE" specification we additionally absorb state and year fixed effects, in which case β_5 is the only state-by-period term estimated and HighCRI_j and Post_t are absorbed.

The identifying assumption of the DDD design is that, absent the rule, the difference between the donation-mandate link in high- and low-corruption states would have evolved in parallel. This assumption permits the level of the donation effect to differ across the two groups before the rule. It requires that the difference not exhibit a systematic pre-rule trend that would have continued through the policy date. Because the corruption classification is predetermined and the rule is federal, this assumption is not mechanically violated by reverse causality. We assess it with event-study versions of Equations (3) and (4) in which the donation indicator (and its interaction with HighCRI) is interacted with a full set of event-time indicators, $\tau = t - 2013$, normalizing $\tau = -1$ to zero.

4.3 State and local pay-to-play laws: staggered difference-in-differences

State and local pay-to-play laws provide a second source of policy variation. Because the laws are adopted at different times by different jurisdictions, a two-way fixed-effects regression of outcomes on a post-adoption indicator can be severely biased when treatment effects are heterogeneous across cohorts or over time (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Sun and Abraham, 2021). We therefore use the estimator of Callaway and Sant'Anna (2021), which estimates group-time average treatment effects by comparing each adoption cohort with not-yet-treated and never-treated units, and aggregates them into an overall average treatment effect on the treated (ATT). Identification requires that, absent adoption, outcomes for each treated cohort would have followed the same trend as outcomes for the comparison group. We assess this condition using dynamic estimates to test for systematic trends before adoption. The unit of analysis is the pension-year. Each pension inherits the adoption date of the pay-to-play law that applies to it: state-administered pensions receive the state-law date, and local pensions receive the ear-

lier of the state-law date and the date of a pay-to-play law adopted by their own city or county, within each tier of law. We consider two tiers: *Ban* laws, which prohibit, cap, or otherwise restrict contributions by contractors or investment managers, and *Broad* laws, which additionally include disclosure regimes. The outcome is the log of one plus total PE political contributions directed to the pension's state. The controlled specifications include log total pension assets (lnPenAssets), the pension funded ratio (FundedRatio), the three-year pension investment return (Ret (3yr)), the beneficiary share of plan members (RetireeShare), total contributions relative to pension assets (Contrib), the log of state gross domestic product (lnGDP), the state GDP growth rate (GDPGrowth), and the state unemployment rate (Unemp). We cluster standard errors by state.

4.4 Pension-level outcomes

Finally, we study pension-level outcomes in a pension-year panel. We first ask whether the political activity of PE firms in a state predicts the externalization of the state's pension assets to PE managers:

$$Y_{p,t} = \sum_{k=1}^{K} \phi_k \, \text{NetDon}_{j(p),t-k} + \kappa' C_{p,t} + \alpha_p + \tau_t + u_{p,t}, \tag{5}$$

where $Y_{p,t}$ is either the log value of pension p's assets managed by PE GPs in our data or that value as a share of total pension assets, $\text{NetDon}_{j(p),t}$ is PE contributions minus mutual fund and hedge fund contributions in the pension's state $j(p)$ and year t, in millions of dollars, and $C_{p,t}$ contains log total pension assets (lnPenAssets), total contributions relative to pension assets (Contrib), the beneficiary share of plan members (RetireeShare), the pension funded ratio (FundedRatio), and the GASB-standard assumed rate of return (AssumedRet), together with the log of state gross domestic product (lnGDP), the state GDP growth rate (GDPGrowth), and the state unemployment rate (Unemp). We net out other financial-sector contributions not only because PE are competing for the external positions with other financial institutions such as mutual funds and hedge funds, but also because we need to remove the component of PE donations that reflects the general political climate of a state's financial community, such as a contested gubernatorial race that attracts contributions from all of finance, to isolates the PE-specific component that pay to play would predict.

We then ask whether externalization is associated with subsequent performance and asset allocation, and whether this association depends on the intensity of PE political contributions in the state:

$$Y_{p,t+1} = \psi_1 \, \text{ExtShare}_{p,t} + \psi_2 \, \ln \text{Don}_{j(p),t-1} + \psi_3 \, \text{ExtShare}_{p,t} \times \ln \text{Don}_{j(p),t-1} + \kappa' C_{p,t} + \alpha_p + \tau_t + u_{p,t+1}, \tag{6}$$

where $Y_{p,t+1}$ is the one-year-ahead investment return, the change in the funded ratio, the realized

private equity return, or the change in the allocation to riskless assets or to private equity. If mandates allocated in states with intense PE political activity are partly the product of pay to play, the benefits of externalization should be smaller there, that is, $\psi_3 < 0$ for performance outcomes. All pension-level regressions include pension and year fixed effects and cluster standard errors by pension. We also estimate reduced-form Callaway and Sant'Anna (2021) regressions of pension outcomes on the adoption of state and local pay-to-play laws.

5 Political Donations, External Manager Selection, and Heterogeneity

5.1 Baseline results

Table IV reports estimates of Equation (2). Columns (1) through (4) include only the one-year lag of the donation indicator. Column (1) includes GP and year fixed effects and no controls. The coefficient on Don_{t-1} is 0.042 and is significant at the 5% level. Column (2) adds GP controls and state fixed effects, which reduces the coefficient to 0.032. This reduction indicates that part of the raw association reflects state-level differences in the propensity of GPs to obtain mandates and the fact that larger and more diversified GPs both donate more and manage more public money. Column (3) adds state-level pension controls, which leaves the coefficient unchanged at 0.032, and column (4), our preferred specification, replaces the separate state and year fixed effects with state-by-year fixed effects. The coefficient is 0.033 (standard error 0.016).

The magnitude is economically large. In the matched panel, the unconditional probability that a GP manages money for a given state's pensions in a given year is 0.6%. Among focal GPs, it is 1.4%. A one-year-lagged donation to a state's politicians is associated with a 3.3 percentage point increase in this probability, which is more than five times the unconditional mean and more than twice the mean among focal GPs. Put differently, the estimates imply that a GP that donates to a state's politicians is several times more likely to hold that state's pension mandate in the following year than the same GP in a state-year in which it did not donate, holding fixed everything that is common to all GPs in that state-year.

Columns (5) through (8) include the first three lags of the donation indicator jointly. All three lags enter positively and significantly in every specification. In the preferred specification of column (8), the coefficients are 0.022 on the first lag, 0.020 on the second, and 0.026 on the third, all significant at the 10% level or better. The cumulative association over three years is about 0.068, roughly double the single-lag estimate. Two features of this pattern are worth emphasizing. First, the effect does not dissipate after one year. If anything, donations three years earlier are as strongly associated with mandates as donations one year earlier. This persistence is consistent with the in-

stitutional reality that the allocation of a private equity commitment, from initial contact through due diligence, consultant review, board approval, and the GP's next fundraising, takes time. It is also consistent with campaign contributions being made over an electoral cycle that typically spans two years. Past support may be remembered and may help sustain longer-term relationships between GPs and political officials, allowing its association with mandate allocation to persist beyond the contribution year. Second, because the lags enter jointly, the coefficients cannot simply reflect a GP that donates every year to a state in which it is an incumbent manager. Each lag has independent predictive power.

The control-variable coefficients reported in Table IV are sensible. Larger GPs and GPs with more funds are more likely to manage public pension money, GPs with a larger share of AUM in liquidated funds are less likely to do so, and GPs are substantially more likely to manage money for pensions in their home state. The within-R^2 of the regressions is small, as is typical in linear probability models of rare events with many fixed effects, but the fixed effects themselves explain about 10% of the variation in mandate status.

5.2 Which donations matter?

If the donation-mandate link reflects an exchange with officials who influence manager selection, it should be driven by contributions to recipients who are in a position to deliver mandates. Table V re-estimates the preferred specification separately for donations to four recipient categories. Columns (1) through (4) include only the first lag, and columns (5) through (8) include three lags jointly.

The contrast between federal and state-and-local recipients is stark. Donations to a state's federal candidates and committees, including members of Congress, Senate candidates, and federal party committees, have no association with mandates: the coefficient is 0.001 with a standard error of 0.007 in column (1), and all three lag coefficients in column (5) are small and insignificant, with mixed signs. Donations to state and local recipients, by contrast, have a coefficient of 0.046 in column (2), significant at the 5% level, and all three lags are positive and significant at the 5% or 1% level in column (6). Donations to key-office recipients are also strongly associated with mandates. These candidates run for governor, lieutenant governor, treasurer, comptroller, attorney general, or secretary of state, offices that exercise direct authority over pension investment or sit ex officio on pension boards. The coefficient is 0.028 for the first lag alone, and the coefficients are positive and significant at every lag in column (7). Finally, donations to incumbent officials whom we can match to sitting state officeholders or pension board members have positive coefficients that are significant for the second and third lags.

These results are difficult to reconcile with explanations that attribute the donation-mandate

link to a GP's general political engagement, ideology, or familiarity with a state. A GP that is politically active and well connected in a state would be expected to give to that state's federal delegation as much as to its state officials, yet only contributions to the officials who control pension investment predict mandates. The results also map closely onto the structure of the SEC rule, which targets contributions to state and local officials who can influence adviser selection while leaving contributions to most federal candidates outside its scope. Moreover, the fact that donations to federal recipients have no predictive power provides a useful placebo: whatever unobserved factor makes a GP more likely both to give in a state and to obtain mandates there would also, in most stories, make it more likely to give to that state's federal candidates.

5.3 Market characteristics

If donations are a payment for access to valuable mandates, the donation-mandate link should be stronger where mandates are more valuable. Table VI interacts the donation indicator with three market characteristics: log state population, log state pension assets, and SameState, an indicator for GPs headquartered in the pension's state. All specifications include GP and state-by-year fixed effects and GP controls, so the main effects of state characteristics are absorbed.

The donation-mandate link increases strongly with state size. In column (1), the interaction with log population is 0.026 and significant at the 1% level, while the main effect is -0.386. The implied donation effect is zero at a population of about 2.6 million (log population of 14.8), is approximately 1 percentage point at the sample mean (about 3.9 million), and rises to 3 percentage points for a state with 9 million residents and about 7 percentage points for the most populous states. A one-standard-deviation increase in log population raises the donation effect by about 2.7 percentage points. The same pattern holds for pension size (column (3)). The interaction with log state pension assets is 0.009 and significant at the 5% level, so that a one-standard-deviation increase in pension assets raises the donation effect by about 1.8 percentage points. In columns (2) and (4), the reported first-lag interactions remain positive and significant when the three donation lags and their interactions enter jointly. Large states run large pension systems with large private equity programs, so a mandate from these systems is more valuable to a GP. They also tend to have more expensive political campaigns, making contributions more valuable to officials. Both forces strengthen the incentives for an exchange.

Columns (5) and (6) show that the donation-mandate link is concentrated among in-state GPs. For GPs headquartered outside the pension's state, the donation effect is slightly negative (-0.009). For in-state GPs, it is about 19 percentage points ($-0.009 + 0.197$). The reported first-lag interaction remains positive and significant in column (6), which includes the three donation lags and their interactions jointly. This pattern should be interpreted carefully. It does not simply reflect

home bias, which is absorbed by the SameState control and by the state-by-year fixed effects. In-state GPs are more likely to manage local pension money whether or not they donate. Rather, the result shows that donations are associated with mandates mainly when they come from GPs that are embedded in the state's political and business networks. This is consistent with pay to play operating through local relationships, as in the cases of politically connected local private equity investments documented by Hochberg and Rauh (2013), and with the observation that officials are more responsive to the contributions of local constituents, who can also deliver votes, fundraising networks, and local economic activity.

5.4 Board governance

The ability of officials to deliver mandates depends on how investment decisions are made. Table VII interacts the donation indicator with two features of pension governance: Investment Board, an indicator for plans that delegate investment decisions to a separate investment board, and Board Size, the number of members on the plan's board. Because governance varies at the pension level while our panel is at the state level, these regressions use the subsample of GP-state-year observations for which board data are available.

The results for investment boards are striking. In column (1), the donation effect for pensions without a separate investment board is 0.046, significant at the 5% level, while the interaction with Investment Board is -0.057, also significant at the 5% level. The donation effect for pensions with a separate investment board is therefore approximately zero (-0.010). Column (2) shows that the interaction is negative and significant for every lag. A separate investment board typically has a narrower mandate, more members with financial expertise, and greater insulation from the officials who administer the plan and who are the targets of contributions. Our results suggest that this governance arrangement effectively neutralizes the donation-mandate channel.

The interaction with board size is positive and significant (column (3)): each additional board member raises the donation effect by about 0.09 percentage points, and the effect turns positive for boards with more than about 15 members. Larger boards in U.S. public pensions tend to include more ex officio officials and political appointees and to dilute individual accountability for investment decisions (Andonov et al., 2018). In larger boards, the monitoring of any single decision may be weaker and the number of officials whom a GP could cultivate is greater, both of which may facilitate pay to play.

6 Evidence from the SEC Pay-to-Play Rule

6.1 Main estimates

Table VIII reports the DID and DDD estimates. Column (1) reproduces a two-way fixed effects baseline on the sample used in this section, which excludes 2012 and adds state controls. The coefficient on Don_{t-1}, 0.031, is very close to the baseline estimate in Table IV.

Columns (2) and (3) report the DID specification of Equation (3), without and with year fixed effects. Before the rule, a donation raises the probability of a mandate by 5.2 percentage points, significant at the 5% level. The interaction with Post_t is -0.034, implying that the average donation-mandate link fell by about two-thirds after the rule took full effect, to approximately 1.8 percentage points. The interaction is economically large but imprecisely estimated, which is not surprising: if the rule mainly curtailed pay to play where it was prevalent, the average effect across all states would be diluted by states where little changed.

Columns (4) and (5) report the DDD specification of Equation (4), which allows the donation effect and its post-rule change to differ between high- and low-corruption states. The results reveal sharp heterogeneity. Before the rule, the donation-mandate relationship is entirely concentrated in high-corruption states. In column (4), the coefficient on $\text{Don}_{t-1} \times \text{HighCRI}$ is 0.090 (standard error 0.023), significant at the 1% level, while the coefficient on Don_{t-1}, which measures the pre-rule effect in low-corruption states, is -0.027. Thus, before the rule, a donating GP was about 6.3 percentage points more likely to obtain a mandate in a high-corruption state ($-0.027 + 0.090$), but slightly less likely to obtain one in a low-corruption state, where donations may even have attracted scrutiny.

The triple interaction $\text{Don}_{t-1} \times \text{HighCRI} \times \text{Post}_t$ is -0.070 (standard error 0.031), significant at the 5% level. The high-corruption premium therefore fell from 9.0 percentage points before the rule to about 2.1 percentage points after ($0.090 - 0.070$), a decline of more than three-quarters. In column (5), which additionally absorbs state and year fixed effects, the premium falls from 8.4 to 1.4 percentage points, a decline of 83%. In low-corruption states, by contrast, the donation effect moves slightly toward zero after the rule (the coefficient on $\text{Don}_{t-1} \times \text{Post}_t$ is 0.032 to 0.034), so that after 2013 the donation effects in high- and low-corruption states are much closer to each other. In short, the SEC rule appears to have largely eliminated the excess donation-mandate link in exactly the states in which pay to play was most likely to have been operating.

6.2 Dynamics

Figure 3 plots the event-study version of the DID specification. Each point is the coefficient on the donation indicator interacted with an event-time dummy, relative to 2012 ($\tau = -1$). The

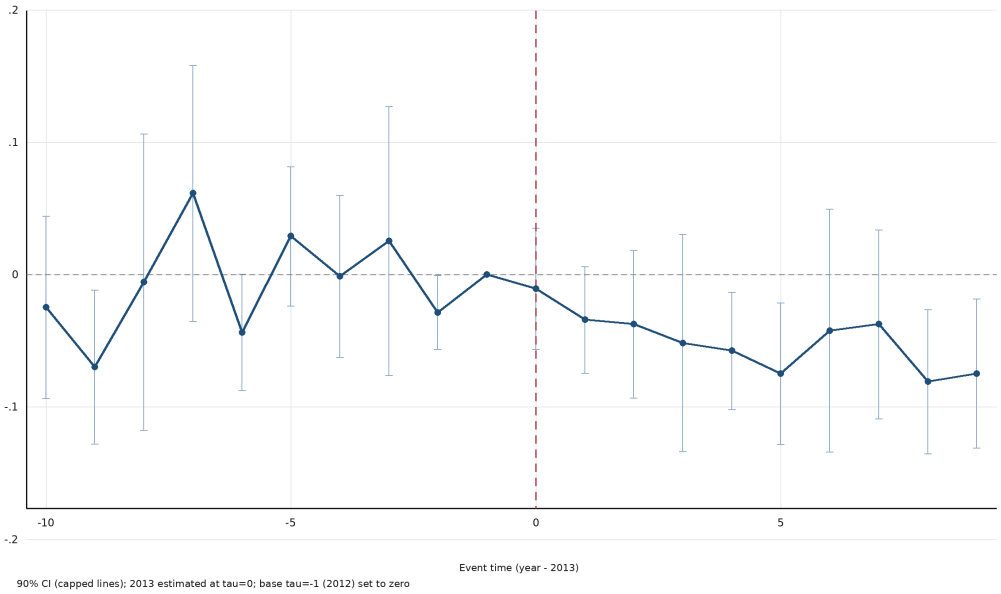


Figure 3. SEC rule: DID event study.

regression includes Don (t-1), GP and state controls, and firm fixed effects in the matched GP-State-Year panel. Capped lines show 90% confidence intervals based on standard errors clustered by firm and state. The estimates show no systematic pre-rule trend, supporting the parallel-trends assumption. After 2013, the coefficients decline steadily and become significantly negative at the 10% level four to five years and eight to nine years after the rule, consistent with a gradual erosion of the donation-mandate link as pre-rule mandates expired and new commitments were made under the rule's constraints. The gradual pattern is natural in private equity, where commitments are made at the time of a GP's fundraising and existing relationships persist for the life of a fund.

Figure 4 shows the implied donation effects separately for states with above- and below-median pre-2011 Corruption Reflections Index, estimated from the event-study version of the DDD specification. Capped lines show 90% confidence intervals, and the vertical line marks 2013. Before the rule, the donation effect is positive in high-corruption states and close to zero or slightly negative in low-corruption states. This difference in levels is consistent with the premise of the DDD design that pay to play was more prevalent in high-corruption states and does not itself violate parallel trends. The gap between the two series shows no systematic pre-rule trend, supporting the parallel-trends assumption. After 2013 the high-corruption series declines and, from 2016 onward, becomes indistinguishable from the low-corruption series, with both fluctuating around zero. Figure 5 plots the corresponding DDD event-study coefficients, the difference between the two series relative to 2012, with 2012 normalized to zero and capped lines showing 90% confidence intervals. The DDD event study likewise shows no systematic pre-rule trend, while the post-rule decline

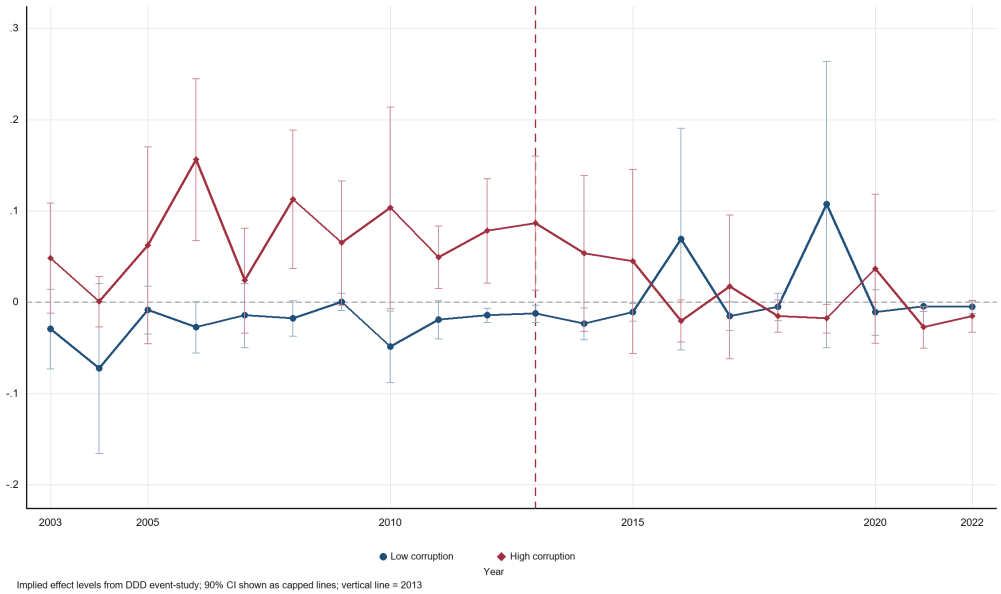


Figure 4. SEC rule: donation effects in high- and low-corruption states.

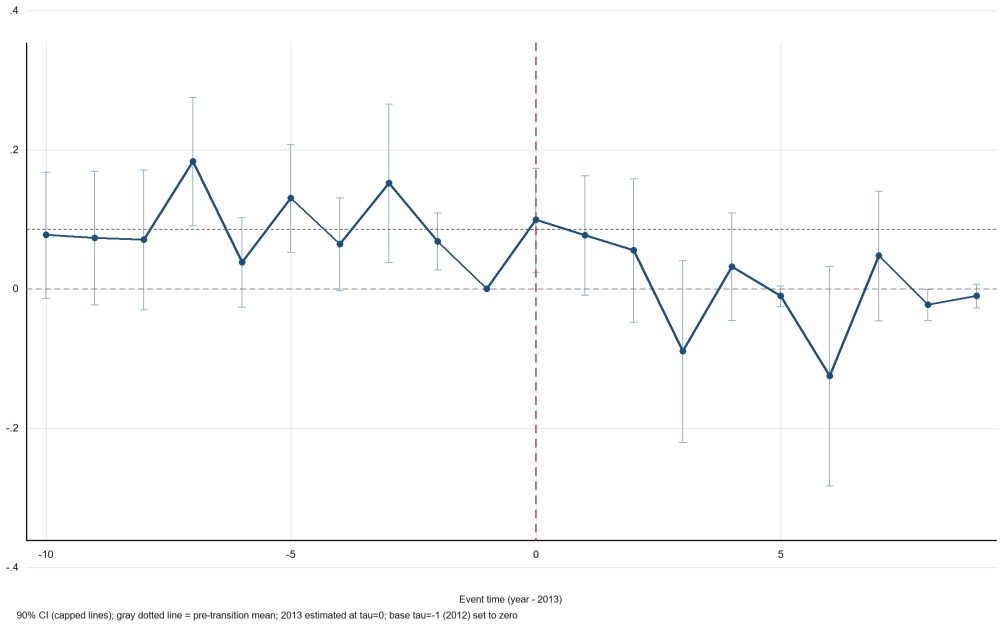


Figure 5. SEC rule: DDD event study.

mirrors the regression estimates.

6.3 Recipient types

Table IX repeats the DDD specification separately for the four recipient categories. Consistent with the baseline results, the triple interaction is large and significant for donations to state and local recipients (-0.106, significant at the 5% level) and to incumbent officials and pension board members (-0.123, significant at the 5% level), the categories most directly targeted by the rule. For these categories the pre-rule high-corruption premium is also large and significant (0.096 and 0.105, respectively). For donations to federal recipients, the high-corruption premium and the triple interaction are both smaller and statistically insignificant. For donations to key-office recipients, the point estimates are very large, with a triple interaction of -0.34, but are imprecisely estimated because such donations are rare. The sign and magnitude are nevertheless consistent with the rule having had its largest effect on contributions to candidates for offices that exercise the most direct control over pension investment.

7 Consequences for General Partners

7.1 The intensive margin among existing managers

Our results so far concern the extensive margin: whether a GP manages a state's pension money. We now ask whether donations also affect the terms of existing relationships and what public pension mandates are worth to GPs. Because fees and mandate sizes are reported in CAFRs at the level of the GP-pension relationship and are measured with considerable noise, we aggregate them to the GP-year level, where the outcome is the GP's value-weighted fee ratio across all of its public pension mandates, the log of the total value it manages for public pensions, or the share of its total AUM that comes from public pension mandates (capped at one). Table XI estimates regressions of these outcomes on the log amount of the GP's total donations, conditional on the GP already managing public pension money, with GP and year fixed effects and GP controls.

Donation amounts are not associated with higher fee ratios (columns (1) and (2)) or with larger total mandate values (columns (3) and (4)). They are, however, associated with a higher share of the GP's business coming from public pensions: among positive donation amounts, doubling donations is associated with an approximately 0.6 percentage point increase in the ratio of public pension mandates to AUM, significant at the 10% level (columns (5) and (6)). These results suggest that, at least as measured in CAFRs, pay to play operates mainly on the extensive margin of which GPs receive mandates, rather than on the fees that public pensions pay to existing managers. Public pension fee schedules for private equity are typically negotiated at the fund level and are often standardized across limited partners, leaving little room for donor-specific fee concessions. One interpretation is that in private equity the main rent from a public pension relationship is access to

large, long-lived commitments and the fundraising benefits they confer, rather than the fee rate per se. Another is that fees reported in CAFRs, which often exclude carried interest and fees netted from fund returns, understate the true cost of PE mandates and are too noisy to detect donor-specific differences.

7.2 What are public pension mandates worth to GPs?

Table XII examines the full GP-year panel, including GPs with and without public pension mandates. Columns (1) to (3) relate three GP outcomes to the share of the GP's AUM that comes from public pension mandates in the previous year (ExtRatio). An increase of 10 percentage points in the public pension share is followed by approximately 0.03 additional funds (significant at the 10% level) and 2.1% higher AUM growth (significant at the 5% level). Public pension mandates thus appear to help GPs raise subsequent funds and grow, consistent with the certification value of being selected by a large institutional investor. The public pension share is not significantly related to the GP's AUM-weighted fund IRR.

Columns (4) to (6) add the GP's log donation amount and its interaction with the public pension share. When lnDon is one unit higher, the estimated association between an increase of 10 percentage points in public pension exposure and subsequent IRR is 0.13 percentage points more negative (significant at the 10% level), and the association with AUM growth is approximately 0.23% more negative (significant at the 5% level). A doubling of donations, which raises lnDon by $\ln(2)$, corresponds to differences of 0.09 percentage points in IRR and 0.16% in AUM growth. The 0.13-percentage-point IRR difference equals approximately 1.8% of the estimation-sample mean of 7.45%. Thus, the performance and growth associations of public pension exposure become more negative as political giving rises. These estimates capture variation in donation amounts; they do not compare a donor indicator with a non-donor indicator.

8 State and Local Pay-to-Play Laws and Pension-Level Outcomes

8.1 State and local pay-to-play laws and PE donations

The SEC rule raised the cost of pay to play for all registered advisers at the same time. State and local pay-to-play laws provide staggered, jurisdiction-specific variation. Figure 6 maps the most restrictive category of state-level law in each state. Ten jurisdictions (including the District of Columbia and Hawaii, whose 2005 contractor contribution restriction supplements its earlier disclosure regime) have ban-type laws, four have disclosure-type laws, twelve have laws that apply

only to narrow industries, and the remaining states have no state-level pay-to-play law. Appendix A documents the coding. Several large cities and counties, including New York City, Los Angeles, San Francisco, Chicago, Cook County, Philadelphia, Jersey City, and Seattle, have adopted their own laws, which we assign to the local pensions in those jurisdictions.

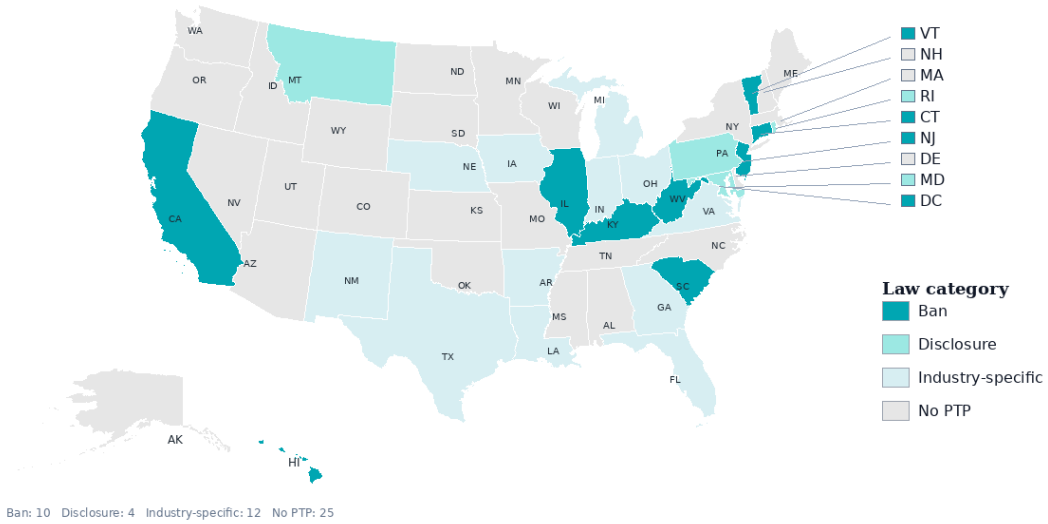


Figure 6. State pay-to-play laws.

Table XIII reports the Callaway and Sant'Anna (2021) estimates of the effect of law adoption on PE political contributions directed at each pension's state. The adoption of a ban-type law is followed by an estimated 75% decline in reported PE donations without controls and an 84% decline with pension and state controls, both significant. Broad laws, which include disclosure regimes, are followed by estimated declines of 89% and 77%, respectively. Focusing on the preferred specifications with pension and state controls, the estimated decline is therefore 77%-84%. The 84% estimate applies specifically to contribution-ban laws, whereas the 77% estimate applies to the broader definition that also includes disclosure-based regimes.

Figures 7 and 8 plot the Callaway and Sant'Anna (2021) dynamic ATT estimates for ban laws and broad laws, respectively, over event times -3 to 3. Capped lines show 95% confidence intervals based on standard errors clustered by state. The estimates show no systematic pre-adoption trend, supporting the parallel-trends assumption. After adoption, the estimates are negative in most years and significantly so two years after adoption, when a full election cycle has elapsed under the new law. Internet Appendix Figure IA.1 extends the event window from -5 to 5, with ban laws in the left panel and broad laws in the right panel. The longer window likewise shows no systematic pre-adoption trend and supports the parallel-trends assumption.

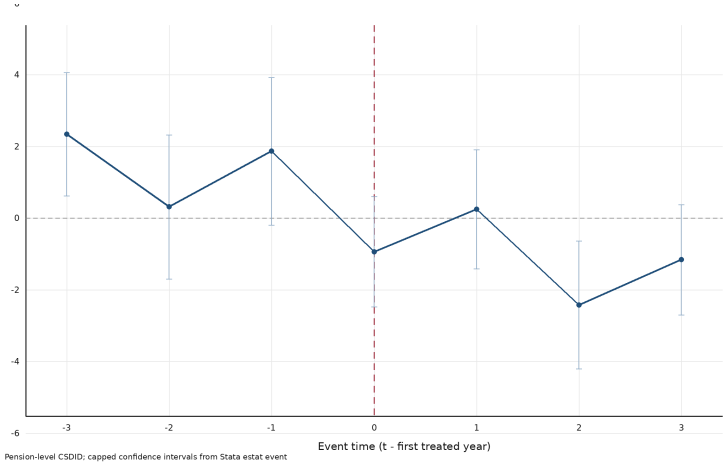


Figure 7. Ban laws and pension-level PE donations: event study.

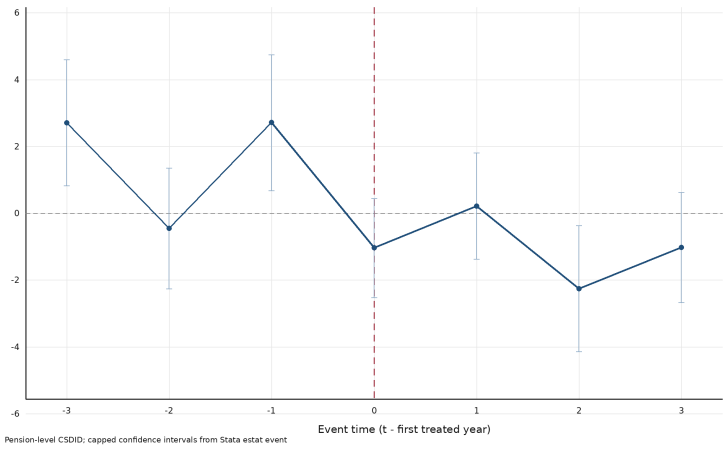


Figure 8. Broad laws and pension-level PE donations: event study.

8.2 PE donations and the externalization of pension assets

We next ask whether the political activity of PE firms in a state is associated with greater delegation of the state's pension assets to PE managers. Table XIV reports estimates of Equation (5) using our net donation measure, PE contributions minus mutual fund and hedge fund contributions in the state-year, in millions of dollars. A $1 million increase in net PE donations in the previous year is associated with an approximately 48% increase in the value of the pension's assets managed by PE GPs (column (1)) and with a 0.61 percentage point increase in the share of pension assets managed by PE GPs (column (3)), relative to a sample mean share of about 12%. When three lags enter jointly, each lag is positive and significant, and the cumulative effects are approximately 101% and 1.06 percentage points, respectively.[3]

The choice of the net measure deserves comment. Raw PE donations at the state-year level are highly correlated with donations by the rest of the financial sector, which spike with competitive statewide races, ballot measures, and national electoral cycles (Figure 1). Such variation reflects the general political environment of a state rather than PE-specific efforts to obtain mandates, and it is correlated with fiscal and political conditions that independently affect pension investment policy. When we use the log of raw PE donations instead of the net measure, the lagged coefficients are negative. Once the common financial-sector component is removed, the PE-specific component is robustly positively associated with externalization. We interpret the net measure as a cleaner proxy for the political activity that pay to play predicts, but we acknowledge that the pension-level evidence relies on state-year variation and is therefore more vulnerable to omitted state-level factors than our GP-state-year design.

8.3 Externalization, donations, and pension performance

If PE mandates were allocated purely on merit, a greater share of assets delegated to PE managers should, if anything, be associated with better subsequent performance. Otherwise, the pension would not delegate. If some mandates are allocated in exchange for political contributions, however, the benefit of externalization should be smaller where PE political activity is more intense. Table XV tests this prediction using Equation (6).

The ExtShare coefficient alone is positive but insignificant for all three outcomes (columns (1), (3), and (5)). Once we allow the relationship to depend on PE donations in the state, a clear pattern emerges. In column (2), the coefficient on ExtShare is 0.026 and significant at the 5% level, while the interaction with log PE donations is -0.0038, also significant at the 5% level. Thus, in states

[3]The value outcome is $\ln(1+\text{externally managed PE assets})$. The percentage transformation is therefore exact for one plus externally managed assets and approximates the percentage change in the asset amount when that amount is positive and large relative to one dollar.

with little PE political activity, an increase of one percentage point in the share of assets externally managed by PE GPs is associated with a 0.026 percentage point higher return in the following year, but this benefit declines with the log amount of PE donations and is fully offset at log donations of about 6.9, or approximately $1,000, which is about the median amount that a donating GP gives in a state in a year (Table II, Panel B). The same pattern holds for realized private equity returns (column (6)): the interaction is -0.009 and significant at the 10% level. For the change in the funded ratio (column (4)), the ExtShare coefficient is positive and significant at the 10% level, and the interaction is negative but insignificant. Externalization to PE managers is thus associated with better outcomes where PE political activity is low, but not where it is high. This is exactly the pattern pay to play predicts.

Table XVI examines asset allocation. A higher externally managed PE share is followed by a decline in the allocation to cash and fixed income (-0.019, significant at the 1% level) and an increase in the allocation to private equity (0.010, significant at the 1% level), consistent with externalization being part of a broader shift toward alternatives. In states with more PE political activity, both relationships are weaker: the interaction is positive for the riskless allocation (0.0026, significant at the 5% level) and negative for the PE allocation (-0.0009, significant at the 10% level). In other words, where PE donations are intense, a given amount of externalization is less tied to a deliberate strategic reallocation toward private equity. This pattern is consistent with some mandates in high-donation states being driven by the selection of particular managers rather than by the pension's investment policy.

8.4 Reduced form: pay-to-play laws and pension outcomes

Finally, Table XVII reports reduced-form Callaway and Sant'Anna (2021) estimates of the effect of state and local pay-to-play laws on pension outcomes. The estimated effects on the log value and the share of externally managed PE assets are positive but statistically insignificant for both ban and broad laws. The laws, therefore, do not detectably reduce the overall amount that pensions delegate to PE managers. They are, however, followed by significantly higher pension investment returns: 3.0 percentage points for ban laws (significant at the 10% level) and 4.0 percentage points for broad laws (significant at the 5% level). The effects on the funded ratio are positive but insignificant.

Taken together with the evidence in Table XV, these results suggest that pay-to-play laws operate mainly on the composition of mandates, or which managers are selected, rather than on the quantity of delegated capital. By reducing the role of political contributions in manager selection, the laws may allow pensions to retain the benefits of delegation while reducing the cost of politically driven choices. We note, however, that the magnitudes of the return effects are large relative

to plausible gains from improved PE manager selection alone, that they are only marginally significant, and that the same caveats about the small number of treated jurisdictions apply. One plausible interpretation is that pay-to-play laws are part of broader governance reforms that improve investment decision-making more generally. For this reason we do not use the laws as instruments for donations because the exclusion restriction, which requires that the laws affect pension outcomes only through PE donations, is unlikely to hold.

9 Robustness Tests

9.1 Baseline specifications and donation measures

The positive donation-mandate association is stable as we add time-varying GP controls, state-level pension controls, and increasingly demanding fixed effects in Table IV. It also appears at each of the one-, two-, and three-year donation lags. Because many political contributions recorded in DIME are small and the SEC rule exempts de minimis contributions by covered associates, Internet Appendix Table IA.I replaces the donation indicator with an indicator for contributions above $1,000. This threshold is well above the rule's de minimis thresholds and excludes most incidental giving by individual employees. The results are similar to, and slightly larger than, the baseline: in the preferred single-lag specification the coefficient is 0.035, and with three lags the coefficients are 0.026, 0.021, and 0.027. Internet Appendix Table IA.II further shows that the large-donation results are concentrated among contributions to state and local recipients and officials with authority over pension investments, while contributions to federal recipients remain statistically insignificant.

9.2 SEC rule specifications

Internet Appendix Table IA.III reports robustness to alternative rule timing and corruption measures. First, we date the post period from 2011, the year in which compliance with the rule became mandatory, and retain 2012 in the sample. The DDD estimates are essentially unchanged: the high-corruption premium before the rule is 0.098 in the DDD-full specification and the triple interaction is -0.070, both significant. Second, we replace the binary HighCRI indicator with the standardized continuous pre-2011 CRI, which avoids an arbitrary median split. The interaction $\text{Don}_{t-1} \times \text{CRI}$ is 0.062 to 0.071 per standard deviation and significant at the 1% level, and the triple interaction is -0.040 to -0.044 per standard deviation and significant at the 10% level, regardless of whether the post period starts in 2011 or 2013. Internet Appendix Table IA.IV shows that the recipient-type results are also unchanged under the 2011 timing.

Several states adopted their own pay-to-play laws around the same time as the SEC rule, raising the possibility that the DDD estimates capture the effect of state laws rather than the federal rule. Table X re-estimates the DID and DDD specifications after excluding all states that ever adopted a ban-category state law (columns (1) and (2)) or a broad-category state law (columns (3) and (4)). Excluding these states removes between 8 and 11 states from the sample. The triple interaction remains negative and significant in both subsamples: -0.051 when ban-law states are excluded and -0.025 when broad-law states are excluded. The smaller magnitude when broad-law states are excluded is consistent with states adopting their own laws accounting for a disproportionate share of pre-rule pay to play. The recipient-type estimates in Table IX and Internet Appendix Table IA.IV further show that the post-rule decline is concentrated among state and local recipients, including incumbent officials, rather than federal recipients.

Internet Appendix Table IA.V estimates the DID specification separately for high- and low-corruption states and for states with above- and below-median pension assets. In high-corruption states, the pre-rule donation effect is 0.057 and the post-rule interaction is -0.036. In low-corruption states, the pre-rule effect is -0.019 and the interaction is positive. The pre-rule donation effect is also concentrated in states with larger pension systems, where it is 0.049, versus an insignificant 0.011 in states with smaller systems. These subgroup estimates reproduce the DDD and cross-sectional patterns without imposing common control-variable coefficients across groups.

9.3 State and local laws

The staggered difference-in-differences estimates are reported separately for laws that restrict covered contributions and for the broader definition that also includes disclosure regimes. The estimated decline in reported PE contributions appears under both definitions and both with and without pension and state controls in Table XIII. The corresponding event studies are reported for event times -3 to 3 in Figures 7 and 8 and for event times -5 to 5 in Internet Appendix Figure IA.1. At the pension level, Table XVII shows that law adoption is followed by higher pension returns, while the estimated changes in the value and share of assets delegated to PE managers are statistically insignificant.

Internet Appendix Table IA.VI provides subgroup and placebo evidence. Panel A shows that the effect of ban laws is concentrated in high-corruption states (-0.91, significant at the 10% level, versus an insignificant -0.48 in low-corruption states) and in states with large pension systems (-1.95, significant at the 1% level, versus -0.34). Broad laws reduce donations significantly in all subgroups. Panel B randomly reassigns adoption dates across pensions and re-estimates the controlled specification twenty times. The placebo ATTs are centered near zero but dispersed, with standard deviations of about 1.8 to 2.0 and a nontrivial share of nominally significant estimates.

This exercise indicates that conventional state-clustered standard errors, with a small number of treated jurisdictions, may overstate precision. We therefore view the state-law results as strongly suggestive rather than definitive and place primary weight on the SEC rule for identification.

9.4 Pension-level aggregation

Internet Appendix Table IA.VII re-estimates the externalization analysis at the state-year level with state and year fixed effects. The second and third lags of net PE donations remain positive and significant for both the log value and the share of externally managed PE assets. The association between PE-specific political activity and externalization therefore does not depend on conducting the analysis at the individual-pension level.

10 Conclusion

This paper provides systematic evidence that political contributions help private equity GPs obtain mandates from U.S. public pension funds. Using hand-collected data on the external managers of 160 public pensions, linked to the universe of U.S. PE firms and to contribution-level campaign-finance data, and a matched GP-state-year panel that combines focal GPs with observably similar control GPs, we show that a GP that donates to a state's politicians is substantially more likely to manage that state's pension money in subsequent years. The relationship is driven by contributions to state and local officials, especially those with direct authority over pension investment, and not by contributions to the same state's federal candidates.

The SEC's 2010 pay-to-play rule sharply reduced the donation-mandate link precisely in the states where it had been strongest. Before the rule, the link was concentrated in high-corruption states. Afterward, the high-corruption premium declined by roughly 80%. The link is stronger in larger states and pension systems and among in-state GPs, and it vanishes for pensions with a separate investment board. State and local pay-to-play laws are associated with large declines in PE political contributions. At the pension level, PE-specific political activity predicts greater delegation to PE managers, but the return benefits of delegation decline with the intensity of donations, and the adoption of state and local pay-to-play laws is followed by higher pension returns. For GPs, public pension mandates support subsequent growth, but the mandate-performance relation weakens as political giving rises.

Our findings have several implications. First, they suggest that pay to play is a quantitatively meaningful channel through which political influence affects the allocation of public pension capital, complementing evidence that political representation on boards is associated with poor private equity performance. Second, they indicate that regulation can work. The SEC rule substantially

reduced the donation-mandate premium where it was largest, and state laws sharply reduced contributions. At the same time, the rule leaves open several channels, including contributions to federal candidates who later seek state office, to party and independent-expenditure committees, and by individuals who are not covered associates. The importance of these channels may have grown after *Citizens United*. Third, governance design matters. Delegating investment decisions to a separate investment board appears to neutralize the donation-mandate link entirely. Reducing the discretion of elected officials over manager selection, or insulating that discretion from campaign finance, may be a cost-effective complement to contribution rules in protecting pension beneficiaries and taxpayers.

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TABLE I

Variable Definitions

This table defines the variables used in the analysis. The first column gives the label used in the tables, the second column the corresponding variable in the Stata data, and the third column the definition. Donation variables are aggregated to the GP-State-Year level from DIME contributions by the GP and its affiliated individuals. External-manager variables are hand-collected from pension CAFRs. Pension variables come from the Public Plans Data.

Table label	Stata variable	Definition
Donations		
Don (t-k)	`L{k}.don_total_sum1_d`	Dummy: firm + affiliated individuals made any political donation in $t-k$
lnDon	`don_total_sum1_ln`; `L1_ln_don_total_gpyear`	Continuous variable equal to log(1 + total donation amount). It is not a donation dummy. The second variable is the lagged GP-year aggregate used in Table XII.
NetDon	`pe_minus_nonpe_mn`	(PE donations − mutual fund donations − hedge fund donations), state-year, USD mn
Federal	`don_CI_Fed_*`	Donation dummy restricted to federal recipients (House, Senate, presidential, federal committees)
State/Local	`don_CI_StateLocal_*`	Donation dummy restricted to state and local candidates and committees
Key-Office	`don_CI_Directstate_*`	Donation dummy restricted to candidates for governor, lt. governor, treasurer, comptroller/controller, attorney general, and secretary of state. Includes challengers and incumbent officials seeking reelection and can overlap with Incumbent Officials
Incumbent Officials	`don_CI_Incument_*`	Donation dummy restricted to recipients matched to sitting state officials or pension board members. Can overlap with Key-Office
Large Don	`don_total_sum1` > 1000	Dummy: total donation in $t-k$ exceeds \$1,000
External manager / performance		
Ext	`External_Manager_dummy`	Dummy: GP is an external manager of the pension (or the pension has one)
lnExt	`lnext`	$\ln(1 + \text{external managed asset value})$
ExtShare	`extprop`	Externally-managed PE assets / pension AUM. Raw 0-1 ratio; multiplied by 100 in Tables XIV-XVII and IA.VII, so coefficients use percentage points. Table II is unscaled.
ExtRatio	`extratio_cap`; `L1_ext_value_ratio`	GP externally-managed value / GP AUM, capped at one. Unscaled 0-1 ratio in Tables XI-XII.
FeeRatio	`fee_ratio_gpyear`	Management fees / externally-managed value. Unscaled 0-1 ratio in Table XI; displayed as percent in Table II.
Ret	`F1_Return_1yr`	One-year pension return at $t+1$, multiplied by 100 in Tables XV and XVII; units are percentage points.
dFR	`F1_delta_funded_ratio`	Change in the funded ratio from t to $t+1$, multiplied by 100; units are percentage points.
PERet	`F1_PE_Rtrn`	Realized PE return at $t+1$, multiplied by 100; units are percentage points.

Table label	Stata variable	Definition
dRiskless	dF1_riskless_alloc	Change in the cash plus fixed-income share from t to $t+1$, in percentage points.
dPEAlloc	dF1_pe	Change in the PE allocation share from t to $t+1$, in percentage points.
IRR	gp_irr_pct	AUM-weighted fund IRR multiplied by 100 in Table XII; units are percentage points.
ΔFunds	D.gp_numoffunds	Year-over-year change in the GP's number of active funds
ΔlnAUM	D.ln_gp_total_aum	Annual log AUM change, not multiplied by 100. Convert b to percent as $100[\exp(b)-1]$.
GP controls		
lnAUM	ln_gp_total_aum	Log GP total assets under management
AUMHHI	gp_aum_hhi	Fund-size Herfindahl index, stored as 0-1 and not multiplied by 100.
DeadAUM	gp_frac_dead_aum	Share of GP AUM in dead or merged funds, stored as 0-1 and not multiplied by 100.
NumFunds	gp_numoffunds	Number of the GP's active funds
SameState	same_state	Dummy: GP headquartered in the same state as the pension
Vintage	gp_firm_vintage	GP founding year
Pension controls		
lnPenAssets	lnpensionassets	Log pension (or state-year aggregate) total AUM
Contrib	contri_asset	Contributions / pension assets. Raw 0-1 ratio; multiplied by 100 only as a control in Tables XIV-XVI.
RetireeShare	retiree_share	Beneficiaries / plan members. Raw 0-1 ratio; multiplied by 100 only as a control in Tables XIV-XVI.
FundedRatio	funded_ratio	Pension assets / liabilities. Raw 0-1 ratio; multiplied by 100 only as a control in Tables XIV-XVI.
AssumedRet	Assume_Ret_GASB	GASB assumed return. Raw 0-1 rate; multiplied by 100 only as a control in Tables XIV-XVI.
Ret (3yr)	Return_3yr	Three-year pension return, stored as a 0-1 rate and used on that scale in Table XIII.
Board governance		
InvBoard	invboard	Dummy: pension has a separate investment board
BoardSize	boardsize	Total number of pension board members
State controls / identification		
lnPop	lnpop	Log state population
lnGDP	lngdp	Log state GDP
GDPGrowth	gdp_growth	State GDP growth already in percent (4.5 denotes 4.5%).
Unemp	unemployment	State unemployment rate already in percent (5.5 denotes 5.5%).
HighCRI	corr_dum_cri	Dummy: state's pre-2011 average Corruption Reflections Index at or above the sample median
Post12	post2012	Dummy: year $\geq$ 2013 (SEC Rule 206(4)-5 fully binding, 2-year look-back)
Ban / Broad	adopt_ban, adopt_broad	State/pension-specific PTP law timing (Appendix A)

TABLE II
Summary Statistics

This table presents summary statistics for the estimation samples used in Tables IV through XVII, over the 2000-2022 sample period, after the GP-level matching described in Table III (donation and external-manager data end in 2022, although coverage varies slightly by panel). Panel A reports the extensive-margin donation and external-manager indicators at the GP-State-Year level, the estimation panel used in Tables IV through X. Panel B reports the same donation and external-manager variables conditional on being positive: $\ln \text{Don}_r$ by recipient type r (Total, Federal, State/Local, Key-Office, and Incumbent, matching the Tables V and VII breakdown), each conditional on that type's own donation being positive, and lnExt and FeeRatio conditional on Ext=1. Panel C reports GP characteristics at the GP-Year level, the panel used in Tables XI and XII. Panels D through F report pension, board-governance, and state characteristics at the Pension-Year or State-Year level, the panels used in Tables XIII through XVII. Variable definitions are given in Table I. Selected continuous GP, pension, performance, and macroeconomic variables are winsorized at the 1st and 99th percentiles. Donation variables are not winsorized. FeeRatio (total management fee divided by total externally-managed asset value, in percent) excludes GP-State-Year cells with an externally-managed value below $100,000 or an implied ratio above 100%, both of which reflect data-matching artifacts (a mismeasured or implausibly small denominator) rather than genuine fee levels.

	Mean	SD	Median	*N*
Panel A: Donations and external manager status (GP-State-Year)				
Don (dummy)	0.000	0.021	0.000	2,251,976
lnDon	0.003	0.153	0.000	2,251,976
Ext (dummy)	0.006	0.078	0.000	2,251,976
Panel B: Donations and external value, conditional on positive (GP-State-Year)				
$\ln \text{Don}_{\text{Total}}$ ($\mid \text{Don}_{\text{Total}} > 0$)	6.914	1.911	6.909	1,020
$\ln \text{Don}_{\text{Federal}}$ ($\mid \text{Don}_{\text{Federal}} > 0$)	6.633	1.976	6.909	326
$\ln \text{Don}_{\text{State/Local}}$ ($\mid \text{Don}_{\text{State/Local}} > 0$)	7.026	1.881	6.909	715
$\ln \text{Don}_{\text{Key-Office}}$ ($\mid \text{Don}_{\text{Key-Office}} > 0$)	7.514	1.622	7.314	149
$\ln \text{Don}_{\text{Incumbent}}$ ($\mid \text{Don}_{\text{Incumbent}} > 0$)	7.412	1.513	7.091	109
lnExt ($\mid$ Ext=1)	17.039	3.000	17.383	3,912
FeeRatio, % ($\mid$ Ext=1)	4.900	10.800	1.200	2,811
Panel C: GP characteristics (GP-Year)				
lnAUM	6.223	1.746	6.246	15,908
AUMHHI	0.612	0.321	0.524	15,852
DeadAUM	0.472	0.439	0.386	15,852
NumFunds	3.713	5.811	2.000	15,908
Panel D: Pension characteristics (Pension-Year)				
lnPenAssets	15.166	1.976	15.453	3,524
Contrib	0.056	0.043	0.046	3,457
RetireeShare	0.421	0.132	0.416	3,524
FundedRatio	0.768	0.183	0.776	3,511
AssumedRet	0.075	0.007	0.075	3,539
Ret (1yr)	0.068	0.106	0.085	3,507
Ret (3yr)	0.067	0.050	0.074	3,376
ExtShare (PE)	0.119	0.321	0.000	2,632
Panel E: Board governance (Pension-Year)				
InvBoard	0.114	0.318	0.000	2,039
BoardSize	9.590	3.322	9.000	2,039
Panel F: State characteristics (State-Year)				
lnPop	15.173	1.020	15.313	1,033
lnGDP	12.254	1.029	12.242	1,074
GDPGrowth	4.484	3.721	4.290	1,074
Unemp	5.467	2.039	5.100	1,074
HighCRI	0.527	0.500	1.000	1,033

TABLE III

Sample Construction: GP-Level Matched Comparison Sample

This table reports construction of the GP-level matched sample. A GP is *focal* if it ever served as an external manager of a sample public pension or made a political donation during 2000-2022. All remaining U.S. PE GPs in Preqin form the control-GP pool. Focal status is a sample-selection indicator, not a treatment. We estimate the probability that a GP is focal using a logit model of whole-period averages of log GP total assets under management (lnAUM), the Herfindahl index of the GP's fund-size concentration (AUMHHI), the number of active funds (NumFunds), and the share of GP AUM in dead or merged funds (DeadAUM), together with the GP's founding year (Vintage). Within each Preqin primary-strategy category, each focal GP on the common support is matched to its five nearest control GPs with replacement. Focal GPs without usable within-strategy matches on the common support are excluded. This sampling step is not interpreted as a treatment-effect estimator. The full GP-State-Year history of every selected GP, including zero rows, is retained. Panel A reports GP-level balance, Panel B panel composition, and Panel C balance between donating and non-donating GP-State-Year observations. The standardized mean difference (SMD) is $(\bar{x}_F - \bar{x}_C)/\sqrt{(s_F^2 + s_C^2)/2}$.

Panel A: GP-level covariate balance

	Before matching			After matching		
	Focal	Control	SMD	Focal	Control	SMD
lnAUM	5.904	4.010	1.024	5.795	4.794	0.560
AUMHHI	0.636	0.749	−0.395	0.648	0.719	−0.248
NumFunds	4.012	3.618	0.057	3.812	3.432	0.083
DeadAUM	0.290	0.063	0.730	0.280	0.134	0.423
Founding year	2007.2	2016.3	−1.007	2007.9	2013.3	−0.573
Number of GPs	986	4,306		929	1,769	

Panel B: GP-State-Year panel before and after matching

	Before matching			After matching		
	Focal	Control	All	Focal	Control	All
Observations	1,038,956	2,520,110	3,559,066	974,602	1,277,374	2,251,976
GPs	986	4,306	5,292	929	1,769	2,698
States	46	46	46	46	46	46
Ext rate (%)	1.626	0.000	0.475	1.428	0.000	0.618
Don rate (%)	0.104	0.000	0.030	0.105	0.000	0.045

Panel C: GP characteristics of donating vs. non-donating GP-State-Years

	Before matching			After matching		
	Donated	Not donated	SMD	Donated	Not donated	SMD
lnAUM	6.018	4.787	0.577	5.931	5.433	0.249
AUMHHI	0.622	0.665	−0.132	0.636	0.636	0.000
NumFunds	5.213	4.290	0.115	5.017	4.095	0.135
DeadAUM	0.354	0.192	0.403	0.316	0.284	0.076
Founding year	2005.3	2010.9	−0.567	2006.5	2007.9	−0.148
Observations	781	2,605,717		723	1,580,343	

TABLE IV

Political Donations and External Manager Selection

This table presents GP-State-Year regressions in which the dependent variable is Ext, a dummy equal to one if the GP is an external manager of a state pension in year t. Don (t-k) equals one if the GP or its affiliated individuals made a political donation in year $t-k$. Columns (1)-(4) include Don (t-1), while columns (5)-(8) include the one-, two-, and three-year lags jointly. Within each block, columns (2) and (6) add GP controls, columns (3) and (7) add state fixed effects and pension controls, and columns (4) and (8) replace the separate state and year fixed effects with State×Year fixed effects. Table I defines all control variables. All columns include firm fixed effects. Standard errors are clustered by firm and state and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Don (t-1) only				Don (t-1)-(t-3)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Don (t-1)	0.0422**	0.0322**	0.0322**	0.0328**	0.0285**	0.0221*	0.0221*	0.0222*
	(0.0162)	(0.0157)	(0.0158)	(0.0161)	(0.0115)	(0.0110)	(0.0111)	(0.0112)
Don (t-2)					0.0219**	0.0189**	0.0194**	0.0195**
					(0.0085)	(0.0083)	(0.0084)	(0.0083)
Don (t-3)					0.0315***	0.0252***	0.0252***	0.0261***
					(0.0084)	(0.0084)	(0.0085)	(0.0086)
lnAUM		0.0049**	0.0051**	0.0049**		0.0050**	0.0051**	0.0050**
		(0.0024)	(0.0025)	(0.0024)		(0.0024)	(0.0024)	(0.0024)
AUMHHI		0.0073*	0.0076*	0.0073*		0.0081**	0.0083**	0.0081**
		(0.0039)	(0.0040)	(0.0039)		(0.0038)	(0.0039)	(0.0039)
DeadAUM		−0.0089**	−0.0089**	−0.0089**		−0.0075**	−0.0075**	−0.0075**
		(0.0036)	(0.0037)	(0.0036)		(0.0033)	(0.0034)	(0.0033)
NumFunds		0.0012***	0.0012***	0.0012***		0.0012***	0.0011***	0.0012***
		(0.0004)	(0.0004)	(0.0004)		(0.0004)	(0.0004)	(0.0004)
SameState		0.0347*	0.0350**	0.0345*		0.0377*	0.0380*	0.0375*
		(0.0172)	(0.0173)	(0.0173)		(0.0192)	(0.0193)	(0.0192)
lnPenAssets			−0.0004				−0.0007	
			(0.0025)				(0.0033)	
Contrib			−0.0054				−0.0097	
			(0.0176)				(0.0192)	
RetireeShare			−0.0076				−0.0123	
			(0.0105)				(0.0127)	
FundedRatio			−0.0009				0.0015	
			(0.0056)				(0.0063)	
AssumedRet			0.0897				0.0720	
			(0.1197)				(0.1278)	
N	1,879,422	1,267,254	1,221,458	1,267,254	1,645,742	1,121,204	1,083,209	1,121,204
R^2	0.078	0.093	0.096	0.097	0.082	0.098	0.100	0.101
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes		Yes	Yes	Yes	
State FE		Yes	Yes			Yes	Yes	
State×Year FE				Yes				Yes

TABLE V

External Manager Selection by Donation Recipient Type

This table presents results of regressions in which the dependent variable is Ext, as in Table IV, with the donation variable decomposed by recipient type. Don (t-k) is constructed separately using donations to Federal candidates/committees, State/Local candidates/committees, Key-Office recipients, and Incumbent recipients. Recipient types are defined in Table I. Columns (1)-(4) use the one-year lag only. Columns (5)-(8) use the one-, two-, and three-year lags jointly. All columns include firm fixed effects, State×Year fixed effects, and the GP controls from Table IV. Standard errors are clustered by firm and state and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(t-1) only				(t-1)-(t-3)			
	Federal	State/Local	Key-Office	Incumbent	Federal	State/Local	Key-Office	Incumbent
Don (t-1)	0.0011	0.0463**	0.0283**	0.0281	-0.0017	0.0347**	0.0329**	0.0247
	(0.0065)	(0.0199)	(0.0121)	(0.0209)	(0.0056)	(0.0142)	(0.0153)	(0.0241)
Don (t-2)					-0.0044	0.0299***	0.0388**	0.0502*
					(0.0047)	(0.0091)	(0.0176)	(0.0298)
Don (t-3)					0.0055	0.0320***	0.0168**	0.0261*
					(0.0073)	(0.0104)	(0.0075)	(0.0142)
lnAUM	0.0049**	0.0049**	0.0049**	0.0049**	0.0050**	0.0050**	0.0050**	0.0050**
	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)	(0.0024)
AUMHHI	0.0073*	0.0073*	0.0073*	0.0073*	0.0081**	0.0081**	0.0081**	0.0081**
	(0.0039)	(0.0039)	(0.0039)	(0.0039)	(0.0038)	(0.0039)	(0.0039)	(0.0038)
DeadAUM	−0.0089**	−0.0089**	−0.0089**	−0.0089**	−0.0075**	−0.0075**	−0.0075**	−0.0075**
	(0.0036)	(0.0036)	(0.0036)	(0.0036)	(0.0033)	(0.0033)	(0.0033)	(0.0033)
NumFunds	0.0012***	0.0012***	0.0012***	0.0012***	0.0011***	0.0011***	0.0011***	0.0011***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
SameState	0.0347*	0.0345*	0.0347*	0.0347*	0.0379*	0.0374*	0.0378*	0.0378*
	(0.0172)	(0.0173)	(0.0172)	(0.0172)	(0.0192)	(0.0193)	(0.0192)	(0.0192)
N	1,267,254	1,267,254	1,267,254	1,267,254	1,121,204	1,121,204	1,121,204	1,121,204
R^2	0.097	0.097	0.097	0.097	0.101	0.101	0.101	0.101
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

TABLE VI

Market Characteristics and the Donation-Mandate Relation

This table reports linear probability regressions in which the dependent variable is Ext, a dummy equal to one if the GP is an external manager of a given state pension in year t. The heterogeneity variable, Het., is the logarithm of state population (ln(Population)) in columns (1)-(2), the logarithm of total pension assets of the state (ln(Pension Assets)) in columns (3)-(4), and Same State in columns (5)-(6), where Same State equals one if the GP is headquartered in the pension's state. Columns (1), (3), and (5) include Don (t-1), Het., and their interaction. Columns (2), (4), and (6) jointly include the one-, two-, and three-year donation lags and their interactions with Het. Only the t-1 coefficients are displayed for space. A positive interaction coefficient indicates that the donation-mandate relation is stronger as the relevant characteristic increases, or for in-state GPs in the Same State specifications. All columns include firm fixed effects, State×Year fixed effects, and the GP controls described in Table I. Standard errors are clustered by firm and state and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Het. = ln(Population)		Het. = ln(Pension Assets)		Het. = Same State	
	(1)	(2)	(3)	(4)	(5)	(6)
	One lag	Three lags	One lag	Three lags	One lag	Three lags
Don (t-1)	-0.3859***	-0.2613***	-0.1228*	-0.0924*	-0.0085*	-0.0079**
	(0.0920)	(0.0617)	(0.0692)	(0.0486)	(0.0046)	(0.0039)
Don (t-1) × Het.	0.0261***	0.0177***	0.0089**	0.0065**	0.1965***	0.1407***
	(0.0061)	(0.0041)	(0.0041)	(0.0029)	(0.0576)	(0.0397)
N	1,496,564	1,336,300	1,496,564	1,336,300	1,491,688	1,331,976
R^2	0.089	0.093	0.089	0.093	0.089	0.093
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
GP controls	Yes	Yes	Yes	Yes	Yes	Yes

TABLE VII

Board Governance and the Donation-Mandate Relation

This table repeats the Table VI design using two pension board-governance measures as the heterogeneity variable: Investment Board, a dummy equal to one if the pension has a separate investment board, and Board Size, the total number of pension board members. All columns include firm fixed effects, State×Year fixed effects, and the GP controls described in Table I. Standard errors are clustered by firm and state and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Investment Board		Board Size	
	(t-1)	(t-1)-(t-3)	(t-1)	(t-1)-(t-3)
Don (t-1)	0.0464**	0.0313**	-0.0138	-0.0116
	(0.0192)	(0.0134)	(0.0113)	(0.0088)
Don (t-1) × Het.	-0.0565**	-0.0367**	0.0009***	0.0006***
	(0.0244)	(0.0177)	(0.0002)	(0.0002)
N	902,890	794,150	902,890	794,150
R^2	0.110	0.115	0.110	0.115
Firm FE	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes
GP controls	Yes	Yes	Yes	Yes

TABLE VIII

The SEC Pay-to-Play Rule, State Corruption, and External Manager Selection

This table presents results of regressions using the SEC's 2011/2012 pay-to-play rule (Rule 206(4)-5) as a natural experiment. Post12 is a dummy equal to one for years 2013 and later (the 2012 transition year is dropped, reflecting the rule's two-year look-back period). HighCRI is a dummy equal to one if the state's pre-2011 average Corruption Reflections Index is above the sample median. Column (1) reports the two-way fixed-effects estimate of Don (t-1) alone, with firm, state, and year fixed effects. Columns (2) and (3) add the Don (t-1) × Post12 interaction ("DID"). Column (3) additionally includes year fixed effects. Columns (4) and (5) add the HighCRI interactions and the triple interaction Don (t-1) × HighCRI × Post12 ("DDD"). Column (5) additionally includes state and year fixed effects. All columns include firm fixed effects and the GP and state controls described in Table I. Standard errors are clustered by firm and state and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	TWFE	DID full	DID FE	DDD full	DDD FE
Don (t-1)	0.0310**	0.0518**	0.0518**	-0.0273***	-0.0277**
	(0.0152)	(0.0205)	(0.0205)	(0.0099)	(0.0108)
Don (t-1) × HighCRI × Post12				-0.0698**	-0.0696**
				(0.0306)	(0.0302)
Don (t-1) × Post12		-0.0337	-0.0341	0.0320*	0.0341*
		(0.0215)	(0.0214)	(0.0180)	(0.0178)
Don (t-1) × HighCRI				0.0904***	0.0837***
				(0.0231)	(0.0228)
N	1,210,582	1,210,582	1,210,582	1,210,582	1,210,582
R^2	0.093	0.080	0.081	0.081	0.093
Firm FE	Yes	Yes	Yes	Yes	Yes
State FE	Yes				Yes
Year FE	Yes		Yes		Yes
GP controls	Yes	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes	Yes

TABLE IX

The SEC Pay-to-Play Rule by Donation Recipient Type

This table repeats the DDD-full specification of Table VIII (column (4)), decomposing total donations into Federal, State/Local, Key-Office, and Incumbent recipients, using the recipient-type definitions in Table I. Post12 and HighCRI are defined as in Table VIII. The HighCRI, Post12, and HighCRI × Post12 terms are included but not reported. All columns include firm fixed effects and the GP and state controls described in Table I. Standard errors are clustered by firm and state and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Total	Federal	State/Local	Key-Office	Incumbent
Don (t-1) × HighCRI × Post12	-0.0698**	-0.0400	-0.1062**	-0.3426	-0.1225**
	(0.0306)	(0.0489)	(0.0494)	(0.2606)	(0.0507)
Don (t-1) × Post12	0.0320*	0.0055	0.0785**	0.3426	0.0517**
	(0.0180)	(0.0104)	(0.0332)	(0.2502)	(0.0209)
Don (t-1) × HighCRI	0.0904***	0.0585	0.0957***	0.0614	0.1046**
	(0.0231)	(0.0429)	(0.0225)	(0.0381)	(0.0431)
Don (t-1)	-0.0273***	-0.0196*	-0.0303**	-0.0302	-0.0489**
	(0.0099)	(0.0102)	(0.0113)	(0.0214)	(0.0197)
N	1,210,582	1,210,582	1,210,582	1,210,582	1,210,582
R^2	0.081	0.080	0.081	0.080	0.080
Firm FE	Yes	Yes	Yes	Yes	Yes
GP controls	Yes	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes	Yes

TABLE X

The SEC Pay-to-Play Rule Excluding States with Their Own Pay-to-Play Laws

This table repeats the Table VIII specifications on subsamples that exclude states with a state-level pay-to-play law (Table XIII), to separate the SEC rule's timing from state-law timing. Columns (1) and (2) exclude states that ever adopt a ban-category state law. Columns (3) and (4) exclude states that ever adopt a broad-category state law (Appendix A). Columns (1) and (3) report the DID-full specification and columns (2) and (4) report the DDD-full specification, using the same Post12, HighCRI, and control definitions as Table VIII. All columns include firm fixed effects and the GP and state controls described in Table I. Standard errors are clustered by firm and state and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Excl. Ban-law states		Excl. Broad-law states	
	DID full	DDD full	DID full	DDD full
Don (t-1) × HighCRI × Post12		-0.0511**		-0.0252*
		(0.0242)		(0.0126)
Don (t-1) × Post12	-0.0034	0.0370	-0.0167*	0.0061
	(0.0124)	(0.0231)	(0.0093)	(0.0060)
Don (t-1)	0.0112	-0.0221**	0.0200	-0.0135
	(0.0175)	(0.0101)	(0.0180)	(0.0093)
N	1,000,046	1,000,046	921,095	921,095
R^2	0.079	0.080	0.082	0.082
Firm FE	Yes	Yes	Yes	Yes
GP controls	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes

TABLE XI

Donations and the Terms of Existing Mandates

This table presents results of GP-Year level regressions, conditional on the GP already being an external manager of at least one pension. The dependent variables are FeeRatio (external-value-weighted management fee ratio), lnExt (log total externally-managed asset value, summed across states), and ExtRatio (GP-level external-to-AUM ratio, computed across states). lnDon (t-k) is the log of total donation amount in year $t-k$. For each dependent variable, the first column uses the one-year lag only and the second column uses the one-, two-, and three-year lags jointly. FeeRatio excludes GP-State-Year cells with an externally-managed value below $100,000 or an implied ratio above 100% before aggregating to the GP-Year level, for the same reason given in Table II. All columns include firm and year fixed effects and the GP controls described in Table I. Standard errors are clustered by firm and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	FeeRatio		lnExt		ExtRatio	
	(t-1)	(t-1)-(t-3)	(t-1)	(t-1)-(t-3)	(t-1)	(t-1)-(t-3)
lnDon (t-1)	-0.0033	-0.0006	-0.0021	0.0001	0.0090*	0.0089*
	(0.0046)	(0.0041)	(0.0618)	(0.0551)	(0.0051)	(0.0053)
lnDon (t-2)		0.0014		-0.0709		-0.0060
		(0.0020)		(0.0531)		(0.0041)
lnDon (t-3)		-0.0006		-0.0602		0.0027
		(0.0015)		(0.0481)		(0.0030)
N	1,562	1,517	2,065	1,988	2,065	1,988
R^2	0.445	0.456	0.734	0.730	0.878	0.883
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
GP controls	Yes	Yes	Yes	Yes	Yes	Yes

TABLE XII

Public Pension Mandates and GP Outcomes

This table presents results of GP-Year level regressions on the full, unconditional panel of donor and non-donor, exposed and unexposed GPs. ExtRatio is the GP's total externally-managed value divided by the GP's total AUM. The dependent variables are IRR (AUM-weighted internal rate of return across the GP's funds, in percentage points), ΔFunds (year-over-year change in the GP's number of active funds), and ΔlnAUM (year-over-year change in log GP total AUM). Columns (1)-(3) include ExtRatio (t-1) alone. Columns (4)-(6) add lnDon (t-1) and the interaction lnDon (t-1) × ExtRatio (t-1). The mean IRR in the IRR estimation sample is 7.45%. All columns include firm and year fixed effects and the GP controls described in Table I. Standard errors are clustered by firm and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Mandate exposure only			lnDon × ExtRatio		
	IRR	ΔFunds	ΔlnAUM	IRR	ΔFunds	ΔlnAUM
ExtRatio (t-1)	-2.8362	0.3231*	0.2107**	-2.5123	0.3356*	0.2201**
	(3.3562)	(0.1735)	(0.0851)	(3.5007)	(0.1800)	(0.0881)
lnDon (t-1)				-0.0906	0.0025	0.0008
				(0.2131)	(0.0036)	(0.0013)
lnDon (t-1) × ExtRatio (t-1)				-1.3159*	-0.0321	-0.0233**
				(0.7233)	(0.0212)	(0.0097)
N	10,501	26,450	26,450	10,501	26,450	26,450
R^2	0.564	0.338	0.229	0.564	0.338	0.230
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
GP controls	Yes	Yes	Yes	Yes	Yes	Yes

TABLE XIII

State and Local Pay-to-Play Laws and Pension-Level Donations

This table presents Callaway and Sant'Anna (2021) staggered difference-in-differences estimates of the effect of state and local pay-to-play law adoption on pension-level donations, at the Pension-Year level over 2000-2022. The dependent variable is lnDon, the log of total pension-level PE political donations. ATT is the simple aggregate average treatment effect on the treated. Columns (1) and (2) use Ban-law adoption timing. Columns (3) and (4) use Broad-law adoption timing (Appendix A gives the underlying law coding and timing rules). Columns (2) and (4) add log total pension assets (lnPenAssets), the pension funded ratio (FundedRatio), the three-year pension investment return (Ret (3yr)), the beneficiary share of plan members (RetireeShare), total contributions relative to pension assets (Contrib), the log of state gross domestic product (lnGDP), the state GDP growth rate (GDP-Growth), and the state unemployment rate (Unemp). Standard errors are clustered by state and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Ban		Broad	
	No controls	+Controls	No controls	+Controls
ATT	-1.4048***	-1.8488**	-2.1980***	-1.4699*
	(0.5333)	(0.8630)	(0.2290)	(0.8336)
N	3,088	2,661	2,818	2,448
Pension controls		Yes		Yes
State controls		Yes		Yes

TABLE XIV

PE Donations and the Externalization of Pension Assets

This table presents results of Pension-Year level regressions, 2001-2022, in which the dependent variables are lnExt (log total externally-managed asset value) and ExtShare (total externally-managed PE asset value divided by total pension AUM). NetDon is PE donations minus non-PE finance-sector (mutual fund and hedge fund) donations, at the state-year level, in millions of dollars. For each dependent variable, the first column includes NetDon (t-1) only and the second column includes the one-, two-, and three-year lags jointly. All columns include pension and year fixed effects and controls for log total pension assets (lnPenAssets), total contributions relative to pension assets (Contrib), the beneficiary share of plan members (RetireeShare), the pension funded ratio (FundedRatio), the GASB-standard assumed rate of return (AssumedRet), the log of state gross domestic product (lnGDP), the state GDP growth rate (GDPGrowth), and the state unemployment rate (Unemp). Standard errors are clustered by pension and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	lnExt		ExtShare	
	(t-1)	(t-1)-(t-3)	(t-1)	(t-1)-(t-3)
NetDon (t-1)	0.3936**	0.3009**	0.6072***	0.4581***
	(0.1626)	(0.1221)	(0.2108)	(0.1583)
NetDon (t-2)		0.2531*		0.3728**
		(0.1314)		(0.1746)
NetDon (t-3)		0.1442*		0.2315*
		(0.0805)		(0.1297)
N	2,931	2,616	2,931	2,616
R^2	0.748	0.789	0.630	0.676
Pension FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Pension controls	Yes	Yes	Yes	Yes

TABLE XV

Externalization, PE Donations, and Future Pension Performance

This table presents results of Pension-Year level regressions, 2001-2022, in which the dependent variables are Ret (one-year-ahead pension investment return), dFR (one-year-ahead change in funded ratio), and PERet (one-year-ahead realized PE return). lnDon is the log of total state-level PE political donations. For each dependent variable, the first column includes ExtShare alone and the second column adds lnDon (t-1) and the interaction ExtShare $\times$ lnDon (t-1). All columns control for log total pension assets (lnPenAssets), total contributions relative to pension assets (Contrib), the beneficiary share of plan members (RetireeShare), the pension funded ratio (FundedRatio), the GASB-standard assumed rate of return (AssumedRet), the log of state gross domestic product (lnGDP), the state GDP growth rate (GDPGrowth), and the state unemployment rate (Unemp). The dFR specification omits the pension funded ratio (FundedRatio). All columns also include pension and year fixed effects. Standard errors are clustered by pension and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Ret		dFR		PERet	
	ExtShare	+Don×ExtShare	ExtShare	+Don×ExtShare	ExtShare	+Don×ExtShare
ExtShare	0.0119	0.0261**	0.0028	0.0120*	0.0235	0.0763
	(0.0089)	(0.0108)	(0.0080)	(0.0062)	(0.0300)	(0.0500)
ExtShare × lnDon (t-1)		-0.0038**		-0.0013		-0.0094*
		(0.0015)		(0.0012)		(0.0057)
N	2,956	2,851	2,942	2,839	1,349	1,325
R^2	0.665	0.648	0.299	0.297	0.741	0.738
Pension FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Pension controls	Yes	Yes	Yes	Yes	Yes	Yes

TABLE XVI

Externalization, PE Donations, and Future Asset Allocation

This table repeats the Table XV specification using portfolio-allocation outcomes: dRiskless (one-year-ahead change in the cash plus fixed-income allocation share) and dPEAlloc (one-year-ahead change in the PE allocation share), both constructed from Public Plans Database-reported allocation categories. For each dependent variable, the first column includes ExtShare alone and the second column adds lnDon (t-1) and the interaction ExtShare $\times$ lnDon (t-1). All columns control for log total pension assets (lnPenAssets), total contributions relative to pension assets (Contrib), the beneficiary share of plan members (RetireeShare), the pension funded ratio (FundedRatio), the GASB-standard assumed rate of return (AssumedRet), the log of state gross domestic product (lnGDP), the state GDP growth rate (GDPGrowth), and the state unemployment rate (Unemp), and include pension and year fixed effects. Standard errors are clustered by pension and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	dRiskless		dPEAlloc	
	ExtShare	+Don$\times$ExtShare	ExtShare	+Don$\times$ExtShare
ExtShare	-0.0187***	-0.0263***	0.0099***	0.0133***
	(0.0071)	(0.0095)	(0.0028)	(0.0039)
ExtShare $\times$ lnDon (t-1)		0.0026**		-0.0009*
		(0.0012)		(0.0005)
N	2,510	2,426	2,510	2,426
R^2	0.126	0.124	0.124	0.128
Pension FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Pension controls	Yes	Yes	Yes	Yes

TABLE XVII

Pay-to-Play Laws and Pension Outcomes: Reduced-Form Estimates

This table presents Callaway and Sant'Anna (2021) staggered difference-in-differences reduced-form estimates of state law adoption on pension outcomes directly, at the Pension-Year level, 2001-2022, using the same law adoption timing as Table XIII and no additional controls. The dependent variables are lnExt, ExtShare, Ret, and dFR, constructed as in Tables XIV and XV. Columns (1)-(4) use ban-law adoption timing. Columns (5)-(8) use broad-law adoption timing. ATT is the simple aggregate average treatment effect on the treated (`agg(simple)`). Standard errors are clustered by state and are reported in parentheses. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Ban Law				Broad Law			
	lnExt	ExtShare	Ret	dFR	lnExt	ExtShare	Ret	dFR
ATT	1.4107	2.3171	2.9777*	0.3474	1.8082	3.0514	3.9680**	0.1095
	(1.5340)	(2.7673)	(1.6430)	(0.9542)	(1.8036)	(3.2218)	(1.9060)	(1.1149)
N	2,836	2,836	2,633	2,589	2,588	2,588	2,397	2,360

Appendix

A State and Local Pay-to-Play Laws

This appendix documents the state, local, and pension-level pay-to-play law coding used in the empirical analysis (Tables X, XIII, XVII, and Internet Appendix Table IA.VI).

We define a pay-to-play (PTP) law as a statute, regulation, ordinance, executive order, or agency rule that links political contributions by government contractors, prospective contractors, regulated entities, investment advisers, placement agents, or their covered affiliates to eligibility for public contracts, investment mandates, procurement awards, or other official government business.

Ban laws prohibit, cap, or otherwise condition contributions by covered contractors, investment managers, placement agents, or related covered persons. These enter the main *Ban* specification. **Disclosure laws** require covered firms to disclose political contributions or related activity as part of procurement/contracting compliance. These enter the *Broad* specification together with Ban laws. **Industry-specific or restricted-scope laws** apply only to a narrow sector or class of public business (gaming, lottery, housing finance, etc.) and enter the *Robustness* specification only. The law variables are nested, with Ban $\subset$ Broad $\subset$ Robustness.

Table A1. State-Level PTP Laws

State	Type	Law	Adopt	End	Ban	Broad	Rob.
California	Ban	California Government Code Sec. 84308 (Levine Act)	1983		Yes	Yes	Yes
Connecticut	Ban	Connecticut CGA Chapter 155, Sec. 9-612	1995		Yes	Yes	Yes
District of Columbia	Ban	D.C. Law 22-250	2019		Yes	Yes	Yes
Illinois	Ban	30 ILCS 500/50-37	2009		Yes	Yes	Yes
Kentucky	Ban	KRS Sec. 121.330	1992		Yes	Yes	Yes
New Jersey	Ban	N.J.A.C. 17:16-4	2005		Yes	Yes	Yes
South Carolina	Ban	S.C. Code Ann. Sec. 8-13-1342	1992		Yes	Yes	Yes
Vermont	Ban	Vermont 32 V.S.A. Sec. 109	1997		Yes	Yes	Yes
West Virginia	Ban	W. Va. Code Sec. 3-8-12	1985		Yes	Yes	Yes
Hawaii	Disclosure	L Sp 1995, c 10, pt of Sec. 2(1)/HRS Sec. 11-205.5	1995			Yes	Yes
Maryland	Disclosure	MD Sec. 14-104	1973			Yes	Yes
Montana	Disclosure	Exec. Order No. 15-2018 / No. 3-2021	2019	2021		Yes	Yes
Pennsylvania	Disclosure	Election Code Sec. 1641 / 25 P.S. Sec. 3260a	1979			Yes	Yes
Rhode Island	Disclosure	R.I. Gen. Laws Sec. 17-27-2	1993			Yes	Yes
Arkansas	Industry	Ark. Code Sec. 23-115-504	2009				Yes
Florida	Industry	Florida Senate, Sec. 420.512	1997				Yes
Georgia	Industry	Georgia Code Sec. 21-5-30.1	1990				Yes
Indiana	Industry	Indiana Code Sec. 4-30-3-19.7	1996				Yes
Iowa	Industry	Iowa Code Sec. 99F.6	1989				Yes
Louisiana	Industry	La. R.S. 18:1505.2(L)	1996				Yes
Michigan	Industry	Michigan casino contribution rules	1997				Yes
Nebraska	Industry	Neb. Rev. Stat. Sec. 49-1476.01	1995				Yes
New Mexico	Industry	NMSA Sec. 13-1-191.1 / NM Stat Sec. 13-1-98	2006				Yes
Ohio	Industry	Ohio Rev. Code Sec. 3517.13	2006				Yes
Texas	Industry	Tex. Occ. Code Sec. 51.214	1997				Yes
Virginia	Industry	Va code Sec. 2.2-4376.1 / 51.1-124.32 / 2.2-4343	2010				Yes
Colorado	Not PTP	Amendment 54					

Table A2. Local PTP Laws

Jurisdiction	State	Type	Law	Adopt	Ban	Broad	Rob.
Los Angeles	California	Ban	LA City Charter Sec. 470(c)(12)	2011	Yes	Yes	Yes
San Francisco	California	Ban	SF Campaign and Governmental Conduct Code Sec. 1.126	2000	Yes	Yes	Yes
Chicago	Illinois	Ban	Municipal Code of Chicago Sec. 2-156-445	2012	Yes	Yes	Yes
Cook County	Illinois	Ban	Cook County vendor campaign finance restrictions	1993	Yes	Yes	Yes
St. Louis	Missouri	Industry	St. Louis Ordinance 63504	1995			Yes
Jersey City	New Jersey	Ban	Jersey City local PTP ordinance	2008	Yes	Yes	Yes
New York City	New York	Ban	NYC Local Law 34 of 2007	2008	Yes	Yes	Yes
Philadelphia	Pennsylvania	Ban	Philadelphia Code Chapter 17-1400	2006	Yes	Yes	Yes
Seattle	Washington	Ban	Seattle Initiative 122	2015	Yes	Yes	Yes

State-administered pensions receive the corresponding state-level law timing. Local pensions are matched to their city/county jurisdiction where a local law exists, and assigned the earliest timing between the state and matched local law within each tier (Ban/Broad/Robustness). St. Louis is industry-specific and enters Robustness only.

States without state-level PTP coding: Alabama, Alaska, Arizona, Colorado, Delaware, Idaho, Kansas, Maine, Massachusetts, Minnesota, Mississippi, Missouri, Nevada, New Hampshire, New York, North Carolina, North Dakota, Oklahoma, Oregon, South Dakota, Tennessee, Utah, Washington, Wisconsin, Wyoming.

Internet Appendix

for "Pay to Play in Public Pension Private Equity:
Political Donations and External Manager Selection"

Internet Appendix: Additional Results

This Internet Appendix reports the additional results referred to in the main text. Tables IA.I and IA.II use donations above $1,000. Tables IA.III, IA.IV, and IA.V report robustness checks for the SEC rule analysis. Table IA.VI reports subgroup and placebo results for state and local pay-to-play laws. Table IA.VII reports state-level externalization regressions. Figure IA.1 plots the state-law event studies over a longer window.

TABLE IA.I
Large Donations (above $1,000) and External Manager Selection

This table repeats Table IV replacing the donation dummy with an indicator equal to one if the GP and its affiliated individuals donated more than $1,000 to recipients in the state in year $t-k$. Column structure, fixed effects, controls, and clustering (by firm and state) are as in Table IV. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Large Don (t-1) only				Large Don (t-1)-(t-3)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Large Don (t-1)	0.0448**	0.0343*	0.0343*	0.0347*	0.0316**	0.0257*	0.0257*	0.0257*
	(0.0169)	(0.0177)	(0.0178)	(0.0182)	(0.0126)	(0.0135)	(0.0136)	(0.0139)
Large Don (t-2)					0.0243**	0.0198**	0.0205**	0.0205**
					(0.0095)	(0.0091)	(0.0092)	(0.0090)
Large Don (t-3)					0.0326***	0.0264***	0.0265***	0.0272***
					(0.0091)	(0.0094)	(0.0095)	(0.0096)
N	1,879,422	1,267,254	1,221,458	1,267,254	1,645,742	1,121,204	1,083,209	1,121,204
R^2	0.078	0.093	0.096	0.097	0.082	0.098	0.100	0.101
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes		Yes	Yes	Yes	
State FE		Yes	Yes			Yes	Yes	
State×Year FE				Yes				Yes
GP controls		Yes	Yes	Yes		Yes	Yes	Yes
Pension controls			Yes				Yes	

TABLE IA.II
Large Donations by Recipient Type

This table repeats Table V using indicators for donations above $1,000 to each recipient type. All columns include firm fixed effects, State×Year fixed effects, and GP controls. Standard errors are clustered by firm and state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	(t-1) only				(t-1)-(t-3)			
	Federal	State/Local	Key-Office	Incumbent	Federal	State/Local	Key-Office	Incumbent
Large Don (t-1)	-0.0021	0.0507**	0.0296**	0.0299	-0.0013	0.0395**	0.0345**	0.0267
	(0.0077)	(0.0237)	(0.0123)	(0.0224)	(0.0062)	(0.0178)	(0.0157)	(0.0257)
Large Don (t-2)					-0.0038	0.0319***	0.0403**	0.0524*
					(0.0050)	(0.0103)	(0.0184)	(0.0309)
Large Don (t-3)					0.0064	0.0341***	0.0176**	0.0276*
					(0.0078)	(0.0121)	(0.0078)	(0.0150)
N	1,267,254	1,267,254	1,267,254	1,267,254	1,121,204	1,121,204	1,121,204	1,121,204
R^2	0.097	0.097	0.097	0.097	0.101	0.101	0.101	0.101
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GP controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

TABLE IA.III

SEC Rule Timing and a Continuous Corruption Measure

Columns (1) and (2) repeat the DDD-full and DDD-FE specifications of Table VIII defining the post period as 2011 and later (Post11) and retaining 2012. Columns (3)-(5) replace HighCRI with the standardized continuous pre-2011 Corruption Reflections Index, CRI(std). Columns (3) and (4) use Post11 and column (5) uses Post12 (2012 dropped). All columns include firm fixed effects and GP and state controls. DDD-FE columns also include state and year fixed effects. Standard errors are clustered by firm and state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	HighCRI, Post11		Continuous CRI(std)		
	DDD full	DDD FE	DDD full	DDD FE	DDD FE, Post12
	(1)	(2)	(3)	(4)	(5)
Don (t-1) × Corr × Post	-0.0702**	-0.0695**	-0.0423*	-0.0397*	-0.0439*
	(0.0323)	(0.0316)	(0.0220)	(0.0214)	(0.0218)
Don (t-1) × Post	0.0365*	0.0380**	0.0044	0.0043	0.0022
	(0.0190)	(0.0187)	(0.0182)	(0.0167)	(0.0170)
Don (t-1) × Corr	0.0975***	0.0901***	0.0709***	0.0621***	0.0618***
	(0.0246)	(0.0245)	(0.0181)	(0.0160)	(0.0159)
Corr × Post	-0.0030*	-0.0034*	-0.0011	-0.0014*	-0.0014*
	(0.0018)	(0.0017)	(0.0008)	(0.0007)	(0.0008)
Don (t-1)	-0.0321**	-0.0321**	0.0093	0.0084	0.0085
	(0.0122)	(0.0131)	(0.0162)	(0.0143)	(0.0142)
N	1,214,216	1,214,216	1,214,216	1,214,216	1,157,544
R^2	0.081	0.093	0.081	0.093	0.093
Firm FE	Yes	Yes	Yes	Yes	Yes
State FE		Yes		Yes	Yes
Year FE		Yes		Yes	Yes
GP controls	Yes	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes	Yes

TABLE IA.IV
Recipient Types with Post-2011 Timing

This table repeats Table IX defining the post period as 2011 and later (Post11) and retaining 2012. All columns include firm fixed effects and GP and state controls. Standard errors are clustered by firm and state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	Total	Federal	State/Local	Key-Office	Incumbent
Don (t-1) × HighCRI × Post11	-0.0702**	-0.0682	-0.0920*	-0.3469	-0.1292**
	(0.0323)	(0.0667)	(0.0481)	(0.2586)	(0.0536)
Don (t-1) × Post11	0.0365*	0.0061	0.0758**	0.3492	0.0509**
	(0.0190)	(0.0082)	(0.0305)	(0.2481)	(0.0203)
Don (t-1) × HighCRI	0.0975***	0.0856	0.0999***	0.0639	0.1115**
	(0.0246)	(0.0632)	(0.0240)	(0.0416)	(0.0459)
Don (t-1)	-0.0321**	-0.0202*	-0.0348**	-0.0372	-0.0481**
	(0.0122)	(0.0105)	(0.0132)	(0.0238)	(0.0191)
N	1,214,216	1,214,216	1,214,216	1,214,216	1,214,216
R^2	0.081	0.081	0.081	0.081	0.081
Firm FE	Yes	Yes	Yes	Yes	Yes
GP controls	Yes	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes	Yes

TABLE IA.V

The SEC Pay-to-Play Rule by Subgroup

This table estimates the DID specification (Table VIII, column (3)) separately for states with above- and below-median pre-2011 Corruption Reflections Index and for states with above- and below-median pension assets. All columns include firm and year fixed effects and GP and state controls. Standard errors are clustered by firm and state. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	High CRI	Low CRI	High pension assets	Low pension assets
	(1)	(2)	(3)	(4)
Don (t-1) × Post12	-0.0363	0.0297*	-0.0266	-0.0181
	(0.0231)	(0.0163)	(0.0224)	(0.0174)
Don (t-1)	0.0570***	-0.0187**	0.0493**	0.0113
	(0.0197)	(0.0088)	(0.0206)	(0.0168)
N	631,608	578,974	614,100	596,482
States	24	22	26	31
R^2	0.104	0.071	0.120	0.057
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
GP controls	Yes	Yes	Yes	Yes
State controls	Yes	Yes	Yes	Yes

TABLE IA.VI

State and Local Pay-to-Play Laws: Subgroups and Placebo Tests

Panel A reports Callaway and Sant'Anna (2021) ATTs of state and local pay-to-play law adoption on pension-level lnDon (as in Table XIII, without controls), estimated separately for pensions in high- and low-CRI states and for pensions in states with above- and below-median pension assets. Standard errors clustered by state are in parentheses. Panel B summarizes 20 placebo draws in which law adoption dates are randomly reassigned across pensions and the Table XIII specification with pension and state controls is re-estimated. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Subgroup ATTs

	Ban law				Broad law			
	High CRI	Low CRI	High assets	Low assets	High CRI	Low CRI	High assets	Low assets
ATT	-0.9051*	-0.4813	-1.9510***	-0.3378	-1.4213***	-3.6226***	-2.3329***	-2.1148***
	(0.4689)	(0.8574)	(0.3576)	(1.3823)	(0.2601)	(0.3611)	(0.3453)	(0.2975)
N	2,099	987	1,450	1,638	1,949	867	1,327	1,489

Panel B: Placebo law dates (20 random draws)

	Ban law		Broad law	
	Mean ATT	SD of ATT	Mean ATT	SD of ATT
	0.282	1.982	−0.077	1.796
	Share $p < 0.10$	Share $p < 0.05$	Share $p < 0.10$	Share $p < 0.05$
	0.30	0.25	0.20	0.10
Actual ATT (Table XIII, col. 2/4)	−1.849		−1.470	

TABLE IA.VII

State-Year Level Externalization

This table repeats Table XIV at the State-Year level, 2001-2022. The dependent variables are the log of total PE assets externally managed for the state's sample pensions and the corresponding share of total pension assets. NetDon is PE donations minus mutual fund and hedge fund donations in the state-year (USD mn). All columns include state and year fixed effects and state-level pension and economic controls. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

	lnExt		ExtShare	
	(t-1)	(t-1)-(t-3)	(t-1)	(t-1)-(t-3)
NetDon (t-1)	0.6597	0.6184*	0.0303	0.0281*
	(0.4323)	(0.3094)	(0.0202)	(0.0143)
NetDon (t-2)		0.6349*		0.0297**
		(0.3167)		(0.0143)
NetDon (t-3)		0.4289***		0.0208***
		(0.1465)		(0.0071)
N	894	781	894	781
R^2	0.734	0.761	0.737	0.770
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

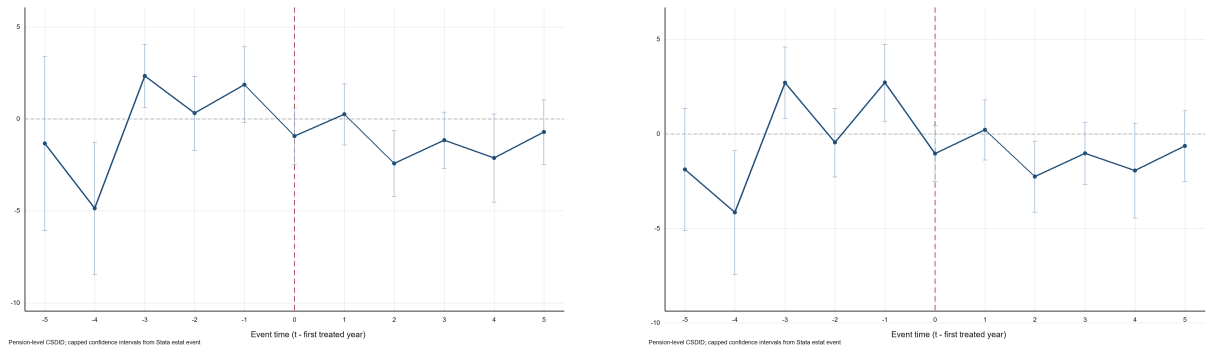


Figure IA.1. State and local pay-to-play laws and pension-level PE donations: longer window.