(Don't) Go Woke, Go Broke? An Analysis of Public Pension Plan Investment Restrictions

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Abstract

I use a hand-collected dataset to examine the impact of recent politicized restrictions on public pension plans' investment decisions. Framing the analysis around three actors—plans, politicians, and constituents—I find that political interference leads to suboptimal asset allocation in public pension plans, with capital shifting from traditional assets to costlier, less transparent alternatives. These effects are concentrated in politically assertive, Republican-led states and result in higher fees and weaker performance. Politicians gain media attention, but the restrictions offer no measurable benefits to beneficiaries or other stakeholders. Targeted firms experience negative market reactions following restriction announcements. My study underscores the role of political incentives in shaping public pension plan governance and investment outcomes.

Keywords: Public pension fund, Investment behavior, Political Incentives, Governance

1. Introduction

How do the incentives of state politicians shape public pension plan investment decisions? Tasked with delivering retirement security for millions of beneficiaries, U.S. public pension plans manage over $6 trillion in assets.[1] Yet unlike corporations and other asset managers, they operate under a unique governance structure—sponsored by state governments and shaped by political oversight. This structure enables elected officials to exert influence over investment policy through legislation, board appointments, and statutory mandates (Mitchell and Hsin, 1994; Useem and Mitchell, 2000). It creates the potential for agency conflicts, where politicians may implement policies that prioritize short-term political objectives—such as boosting public visibility or ideological signaling —over value-maximizing goals that protect the long-term financial interests of beneficiaries. Such misaligned incentives can lead plans to deviate from optimal strategy, distorting investment policy and ultimately imposing costs on retirees and taxpayers (Romano, 1993, 1995; Coronado, Engen, and Knight, 2003; Eaton and Nofsinger, 2005). Understanding how political incentives translate into public pension investment decisions is central to evaluating whether these interventions are intended to impact plan portfolios or are primarily symbolic, serving self-interested political goals.

This paper focuses on one mechanism through which political interference in public pension plans materializes: state-imposed restrictions on investment choices. These restrictions offer a natural setting to evaluate whether an agency conflict exists between politicians and their constituents. I evaluate how these restrictions affect portfolio efficiency and explore their political and economic implications.

[1] Data is sourced from the National Association of State Retirement Administrators (NASRA, 2024) Retrieved from https://www.nasra.org/files/issue%20briefs/nasrainvreturnassumptbrief.pdf.

In recent years, a wave of U.S. legislation has politicized environmental, social, and governance (ESG) investing, often by restricting public retirement plans' investment choices. For instance, since 2022, nine states have collectively withdrawn $13.3 billion from BlackRock after adopting policies limiting ESG integration in investment and stewardship (Bashur, 2024).[2] Politicians have also publicly criticized financial institutions for adopting ESG criteria—West Virginia State Treasurer Riley Moore, for example, accused them of having "… weaponized our tax dollars against the very people and industry that have generated them…" (Catenacci, 2022). Ironically, although framed as safeguards for constituents, such restrictions may impose unintended costs. By narrowing the investable universe, they may compel plans to shift capital toward riskier or more expensive assets to meet required return targets. This raises a central question: Who benefits from these restrictions—and who bears the cost?

To understand the dynamics underlying this political interference, I center the analysis on public pension plans while also considering the incentives of two additional actors: state politicians and constituents (e.g., beneficiaries, taxpayers, firms). I evaluate i) the impact of recent investment restrictions on plan decisions and outcomes, ii) the political incentives motivating these policies, and iii) the downstream economic implications of such policies. This framework offers a multidimensional view of how political incentives shape public pension investment policy. It illustrates how investment restrictions serve as a vehicle for politicization— revealing political motives, altering plan investment behavior, and offering no clear public benefit.

My setting is the universe of U.S. state and local public pension plans. I hand-collect data on ESG-related investment restrictions. I begin by examining how public pension plans adjust their

[2] The list of states includes the following: West Virginia, Florida, Louisiana, South Carolina, Missouri, Utah, Arkansas, Arizona, and Texas.

asset allocations when faced with such restrictions. My main finding is that, in response to investment restrictions, public pension funds decrease their allocation to traditional assets (e.g., equity and fixed income) by approximately 3% and shift it to alternative asset classes (e.g., hedge funds, real estate). Regarding individual asset classes, I observe a 2.7% decrease in equity allocation, while the percentage of assets allocated to hedge funds and real estate increases by 1.4% and 1.8%, respectively. The results are statistically significant and robust across multiple empirical methods.

Next, I conduct cross-sectional tests to determine where these allocation changes are most pronounced. Specifically, I propose the political influence channel, under the premise that if these changes reflect political aspirations as a conflict of interest, they will be most evident in plans subject to greater political pressure. I measure political pressure using three proxies: (i) state's political affiliation, (ii) the degree of state control over the plan, and (iii) the public activism of the states' politicians. Several studies document that political influence and connections shape fund investment decisions and governance (Andonov, Hochberg, and Rauh, 2018; Bradley, Pantzalis, and Yuan, 2016; Lee, 2023; Wang and Mao, 2015; Tu, Wen, F. Zhang, H. Zhang, 2025).

My findings suggest that plans in states where politicians exert greater political influence exhibit the allocation shifts I document. The main results concentrate in plans i) headquartered in Republican-leaning states, ii) more directly controlled by state governments and iii) whose state politicians are more publicly vocal on ESG issues.

Next, I investigate the consequences of the allocation changes on plan returns. I find that the asset classes in which pension funds reduce their exposure, namely equities and fixed income, are those that perform better ex-post, while the assets in which plans increase their positions, such

as hedge funds, perform worse. These effects suggest that pension funds forego value by reducing their exposure to traditional assets, while also incurring losses from reallocating to alternatives.

To more directly quantify the costs of political interference, I present transaction-level evidence from three plans in my sample. I find that these plans respond to investment restrictions by reallocating capital from low-cost traditional assets, such as public equities and bonds, to higher-cost alternatives, including hedge funds, private equity, and real estate. This shift increases fees by up to 5.89%, or approximately $28 million. While the reallocation may aim to preserve a risk-adjusted profile comparable to the pre-divestment portfolio, the substantially higher fees associated with alternative investments likely erode any performance gains. The findings suggest that the restrictions distort investment choice and undermine effective portfolio management.

In addition to evaluating the effect of restrictions across asset classes, I examine the composition of assets within equities. Since the main findings show that plans reduce their allocation to traditional assets—mainly equities—after a restriction, I use 13F filings to analyze what this reduction entails. Specifically, I examine holdings of energy and weapons stocks, which are common targets of these policies. Blue-state plans reduce exposure to these industries post-restriction, consistent with the stated policy goals. Red-state plans show no comparable shift, suggesting that restrictions may serve political purposes beyond portfolio adjustment.

While the analysis so far centers on the consequences for pension plans, these laws originate from a second actor: the politicians who propose, promote, or inspire them. To assess whether politicians have measurable incentives, I examine whether they receive increased public attention following the implementation of investment restrictions. I conduct state-level regressions within the sample of states that impose such restrictions on their pension plans. The results show that politicians receive significantly greater media attention in the aftermath of a restriction, as

measured by article counts in major online news sources. This effect is concentrated in red states. These findings suggest that politicians may pursue investment restrictions not to improve plan outcomes or benefit stakeholders, but to increase their own political visibility and capital.

To address this possibility, I examine whether the political interference in this paper affects a third party: state residents and firms. Given the earlier evidence of investment decision distortion following restrictions, it is reasonable to infer that beneficiaries and taxpayers may ultimately bear the costs. However, one could argue that such policies are intended to benefit the broader state economy, for instance, by supporting local industries. I test this by estimating state-level regressions that examine changes in employment levels in industries tied to the premise of the restrictions—namely, fossil fuels and weapons—following their implementation. The results indicate no significant change in employment in these industries, suggesting no clear cost or benefit for this aspect of the broader state economy.

Additionally, I assess whether restrictions affect target firms. Using an event study approach, I find a significant market response to this political interference. Energy firms experience negative abnormal returns following restriction announcements in red states, while blue-state announcements are associated with positive abnormal returns. I also analyze Oklahoma's Energy Discrimination Act (HB 2034) and document a strong negative reaction among institutions named on its Restricted Financial Company List, such as Bank of America and State Street. These results suggest that markets view red-state restrictions as a source of political or regulatory uncertainty, while blue-state actions appear more aligned with investor expectations, eliciting a more muted response. I conclude that politicized investment restrictions have financial consequences that extend beyond the public pension plan portfolio.

My study makes three main contributions to the literature. First, I build on work examining political influence in public pension plans. Prior research has primarily focused on political bias in micro-level investment decisions, such as board connections, shareholder activism, local bias, and private rent extraction (e.g., linking donations to investment management) (Andonov et al., 2018; Bradley et al., 2016; Lee, 2023; Brown et al., 2015; Wang and Mao, 2015; Dyck, Manoel, Morse, 2022; Tu, et al., 2025). I shift the focus to a more observable channel through which political actors shape investment behavior: ESG-related investment restrictions. Specifically, I contribute new hand-collected data that captures overt state interference in investment strategy, moving beyond indirect proxies of political bias. I also examine how these restrictions affect asset allocation and risk profiles, providing insights into their broader effects on performance and fee structures. Finally, I incorporate measures of political incentives, linking the imposition of investment constraints to the pursuit of political gain.

Secondly, I contribute to the existing literature on public pension plan asset allocation and performance. A central concern in this literature is that government accounting standards permit optimistic assumptions about plan liabilities, often obscuring true funding gaps. These gaps have been shown to influence both asset allocation and risk-taking (Novy-Marx, 2009; 2011). While prior studies emphasize how funding pressures and discount rate assumptions shape portfolio decisions (e.g., Andonov, Bauer, and Cremers, 2017; Mohan and Zhang, 2014; Ang, Chen, and Sundaresan, 2013; Addoum, Jawad, van Binsbergen, and Brandt, 2010; Brown and Wilcox, 2009; Pennacchi and Rastad, 2011), I identify politically motivated investment restrictions as a novel driver of allocation policy and risk-shifting behavior.

Lastly, I highlight governance challenges in the public pension industry, an area that warrants greater attention given its scale. As of 2024, U.S. public-sector retirement systems serve

12.3 million beneficiaries and 15.3 million active workers. Unfunded liabilities are estimated at $2.5 trillion, underscoring the importance of prudent investment decisions (Apell, 2024).[3] Prior studies shows that governance structures influence board decisions, which in turn affect portfolio allocation and performance (Mitchell and Hsin, 1994; Useem and Mitchell, 2000; Mitchell and Yang, 2005). I build on work examining the consequences of external political pressures on pension fund management (Romano, 1993, 1995; Eaton et al., 2004; Coronado et al., 2003; Nofsinger, 1998). I provide new evidence that political incentives can influence portfolio decisions in a suboptimal manner. I quantify the financial costs of these distortions—enabled by governance structures that permit political interference—not only for the plans themselves, but also for stakeholders and the broader economy.

My study carries important implications for the governance of public pension funds. I reveal that political incentives, when unchecked by effective governance structures, can shape investment policy in a multidimensional way—distorting asset allocation and delivering no clear economic benefits for residents or firms. Politicians emerge as the primary beneficiaries, gaining media attention and political capital while imposing diffuse costs on their constituents. Strengthening institutional safeguards that insulate pension decision-making from political pressures may be critical to preserving the integrity and performance of public retirement systems.

2. Overview of Public Pension Plan Investment Behavior and Governance

Several studies discuss the effect of political influence on the investment decisions of public pension plans. Romano (1993,1995) exposes the susceptibility of plans to political pressures, arguing for the separation of politics and the interests of plan beneficiaries, as such interference

[3] Data on beneficiaries and active workers is sourced from the U.S. Census Bureau (2023) Annual Survey of Public Pensions. Retrieved from https://www.census.gov/programs-surveys/aspp.html.

can conflict with the responsibility of fund management. Related to my study, Garrett and Ivanov (2024) examine the impact of state-level restrictive laws, with a particular focus on legislation in Texas. While their analysis centers on the municipal bond market, the authors provide evidence that restrictions on borrower-underwriter relationships imposed by the law lead to increased borrowing costs, demonstrating the harmful effects of such restrictions in financial markets.

A host of studies associate political bias in public pension plan investment decisions with underperformance. One form of political bias examined is the impact of state politician representation on pension plan boards. For instance, Andonov et al., (2018) provide evidence that pension fund boards with greater representation by state politicians are associated with higher allocation to underperforming private equity funds. Similarly, Bradley et al., (2016) document inferior investment performance in plans with more state officials on the board, particularly in those that bias their asset allocation toward political connected public equities. Lee (2023) examines the impact of political connections on pension plans, finding that plans are more likely to invest in private equity funds connected to state politicians serving on the plan's board. Tu et al. (2025) demonstrate that U.S. politicians influence state pension plan investments to secure greater corporate campaign contributions, generating adverse effects for both the pension plans and their portfolio firms. Brown, Pollet, and Weisbenner (2015) find that state pension plans favor in-state equities, particularly firms in counties with strong political ties to the governor. Wang and Mao (2015) conclude that politicians on pension funds' boards use shareholder proposals to bolster their political power. While these studies demonstrate political influence on specific investment decisions, my paper contributes in two key ways. First, I examine a direct measure of state interference in pension fund investment strategies, moving beyond indirect proxies of political

bias. Second, I assess how this interference shapes overall asset allocation and risk profiles, revealing its broader impact on plan portfolios.

I contribute to the existing literature on how public pension plan governance shapes investment behavior and outcomes. Research demonstrates that governance structures directly influence the investment decisions of board members, ultimately impacting portfolio performance (Mitchell and Hsin, 1994; Useem and Mitchell, 2000; Mitchell and Yang, 2005). Agency problems arise from the organizational structure of public pension plans, with taxpayers ultimately bearing the costs. Specifically, a conflict of interest exists between the state and pension beneficiaries, as political incentives may diverge from retirees' financial interests. Prior literature highlights various ways this conflict materializes, such as economically targeted investments (ETIs)—mandated allocations intended to stimulate local economies—along with permissible investment statutes, funding requirements, and country or industry prohibitions (Eaton and Nofsinger, 2005; Romano, 1993; Mitchell and Hsin, 1994; Nofsinger, 1998; Coronado et al., 2003; Useem and Mitchell, 2000). Moreover, governance structures among public pension plans vary significantly. For example, board composition differs in terms of gubernatorial appointees, elected members, and ex-officio representatives (Andonov et al., 2018). A pension fund's board of administration plays a critical role in overseeing investment decisions, particularly in setting and approving asset allocation policies. These structural differences have been shown to influence both funding levels and investment performance, reinforcing the importance of governance in public pension management.

Finally, I contribute to the literature on public pension fund allocation and investment behavior. A key concern in pension fund management is that government accounting standards allow for flexibility in reporting pension liabilities, often leading to severe understatements of

obligations (Novy-Marx, 2009; 2011).[4] This results in funding gaps that influence both asset allocation and risk-taking. Several studies demonstrate that risk-taking is closely linked to the liability discount rate chosen by plans, with funding levels impacting allocation decisions. Underfunded plans tend to increase risk in their investment strategies, a practice negatively associated with performance (Andonov, Bauer, and Cremers, 2017; Mohan and Zhang, 2014; Addoum, Van Binsbergen, and Brandt, 2010). Plans justify higher discount rates by allocating more capital to riskier assets, mechanically reducing reported liabilities and shifting financial risks into the future. Other studies suggest that well-funded plans are better positioned to take on investment risk (Ang et al., 2013).

3. Data and Sample

I construct my sample using two primary sources. First, I obtain plan-level data from the Public Plans database (PPD) provided by the Boston College Center for Retirement Research. The data consists of annual plan-level data for 228 state and local pension plans from 2000 through 2023. It contains asset allocations and investment performance, along with other variables of interest used in my study.

My second data source is a hand-collected dataset of ESG-related investment restriction, drawn from Ropes & Gray LLP, a law firm that maintains a webpage dedicated to tracking state-level ESG-related actions, legislation, and initiatives across the United States.[5] Ropes & Gray documents all past (inactive), pending, and active measures. For each state, I manually collect all

[4] Notably, literature evidence shows that the Governmental Accounting Standards Board (GASB) enables plans to utilize the expected return on assets as the liability discount rate, even though this approach does not accurately represent the risk of liability cash flows (Andonov, Bauer, and Cremers, 2017; Pennacchi and Rastad, 2011). Interestingly, Brown and Wilcox (2009) argue that, due to the constitutional and legal protections binding public pension funds, their obligations resemble low risk, or 'risk-free' cash flows, warranting a lower discount rate that would, in turn, increase the present value of obligatory payments.

[5] The webpage can be accessed at https://www.ropesgray.com/en/sites/navigating-state-regulation-of-esg.

laws, policies, and actions that impose restrictions on public pension investments. For each restriction, I record key details, including the announcement and effective dates, its nature and scope, subject matter, and other pertinent information. I supplement the PPD and my hand-collected restrictions dataset with additional variables used throughout the study, including political affiliation, 13F filings, and other relevant metrics.

I construct a cross-sectional indicator variable, *Restriction,* which equals one for public pension plans subject to at least one identified investment restriction —either a state law or a public pension board policy— and zero otherwise. Although board policies are not always enacted at the state level, they still constitute political interference for several reasons. First, public pension plans operate under state and local government oversight, allowing political actors to exert influence. Second, plan boards often include political appointees or elected officials—such as governors, treasurers, comptrollers, etc.— creating avenues for political agendas to intervene in fund management.[6] Third, some public pension plans are housed within the offices of elected officials, meaning their investment operations are not independent from state political leadership.[7]

To examine the effects of investment restrictions, my main variable of interest is *Post*, a time-varying indicator set to one if a plan's fiscal year-end occurs after the effective date of a *Restriction*.[8] My final sample consists of 228 plans and 5,240 unique plan-FYEs from 2000 to 2023. All continuous variables are winsorized at the 1% and 99% levels.

[6] For example, the Arkansas Public Employees' Retirement System board is composed of 9 members, 33% of which are ex-officio trustees -- the Auditor of State, the Treasurer of State and the Director of the Department of Finance and Administration.

[7] For instance, in Connecticut and North Carolina, the State Treasurer is responsible for managing the pooling and investing the assets of the state pension plans.

[8] My observations are at the plan-fiscal year-end level rather than the plan fiscal year level, as public pension plans have varying fiscal year-ends. This approach allows me to account for unobserved heterogeneity and macroeconomic factors that cannot be fully controlled for within a single fiscal year.

Table 1, Panel A, presents distributional statistics of the *Restriction* sample, organized by state and year. The restrictions span 2020 to 2023. The *Restriction* sample includes 16 states and is characterized by policy-inducing divestments, restricted financial company lists ("financial blacklists") and industry-specific investment prohibitions. Beginning in 2020, Connecticut, New York, and Rhode Island implement industry-specific investment prohibitions, typically restricting investment in the public equity sector.[9] Similarly, Maine enacts a law in 2021 that bans investments in fossil fuel companies, and the California State Teachers' Retirement System issues a net-zero emissions pledge, effectively signaling a shift away from carbon-intensive industries. That same year, Texas adopts legislation requiring the creation of a financial blacklist, which bars investment in institutions that boycott or discriminate against energy firms. Kentucky, Oklahoma, West Virginia, and Arkansas soon follow with similar legislation. These blacklists constrain relationships with major asset managers central to public security markets; for example, Texas' list includes UBS, BlackRock, and 13 others.[10] In 2022, six states announce divestments from BlackRock Inc, due its integration of ESG into investment and proxy voting practices. These divestments arise from changes to pension board investment policies or public resolutions issued by state officials targeting ESG practices. In total, 87 plans (38.2%) in my sample are subject to an investment restriction (*Restriction* = 1), with ex-post data available for 84 of them (*Post* = 1).

Panel B of Table 1 provides summary statistics for the pension plans in my sample. The average (median) plan size in my sample has assets under management of $13.6 ($4.03 billion).

[9] For example, in January of 2020, the Rhode Island State Investment Commission announced a motion to restrict investment in companies that manufacture assault-style weapons for civilian use or operate private for-profit prisons.
[10] Section 809.051 of Texas Senate bill 13, effective in September of 2021, states that "The Comptroller shall prepare and maintain, and provide to each state governmental entity, a list of all financial companies that boycott energy companies…", restricting investment in the listed companies.

On average, the funding ratio of public pension plans in my sample is 77.72%, and the average (median) annual return is 6.72% (8.27%).

My two key asset allocation measures are *Traditional* and *Alternative*, defined at the plan-fiscal year-end (FYE) level. *Traditional* refers to the percentage of assets allocated to equities, fixed income, and cash. *Alternative* is the percentage allocated to private equity, hedge funds, and real estate. Plans on average allocate 79.33% of assets to traditional assets, 16.83% to alternative assets, and the residual to commodities or alternative miscellaneous assets. The average (median) plan allocates 51.73% (53%) of its assets to equity, 25.84% (25 %) to fixed income, 1.76% to cash (1%), 6.21% (4.75%) to private equity, 4.52% (0%) to hedge funds, 6.10% (6.30%) to real estate, 1.83% (0%) to commodities, 1.32% (0%) to alternative miscellaneous assets. Across asset classes, the highest average return is private equity at 11.86%, while cash yields the lowest at 1.99%.

4. Impact of Political Interference on Public Pension Plan Investment Outcomes

4.1. Public Pension Plan Asset Allocation

I begin by examining the impact of investment restrictions on public pension plan asset allocation. Table 2 presents a univariate analysis comparing the percentage of assets allocated to various asset classes in the period before and after the restriction takes effect. I conduct the analysis within plans identified as having an investment restriction (*Restriction* = 1).[11] First, I observe significant differences in the percentage of assets allocated to *Traditional*; specifically, plans on average offload about 14% of their allocation to traditional assets following the restriction, while *Alternative* increases by about 11%. When we breakdown these groupings into individual asset classes, the results corroborate my findings for *Traditional* and *Alternative.* Post-restriction, equity

[11] Results are statistically and economically similar if we conduct the analysis across the full sample.

allocation experiences an approximate 8.3% drop, while allocation to fixed income declines by about 5.4%. We do not see any meaningful change in the percentage of assets allocated to cash, implying that equity and fixed income drive the changes we see in *Traditional*. Private equity increases by about 6%, real estate by about 3%, and hedge funds by about 2%. All differences are statistically significant at the 1% level. These univariate results serve as a baseline, suggesting that public pension plans adjust their investment behavior in a risk-shifting manner in response to restrictions imposed by state politicians.

Because the analysis in Table 2 does not account for confounding factors, time trends, or unobserved heterogeneity, I proceed by estimating the following multivariate regression:

$$Allocation_{i,t} = \beta Post_{i,t} + X_{i,t-1} + \varphi_t + \delta_i + \varepsilon_{i,t} \qquad (1)$$

where i indexes plans and t indexes time. $Allocation_{i,t}$ is the percentage of assets allocated to an asset class or group. $Post_{i,t}$ is the key independent variable of interest, set to one for FYEs that occur after the date the restriction is effective, and zero otherwise. $X_{i,t}$ is a vector of lagged plan characteristics, including the natural log of plan size and plan funding ratio. φ_t and δ_i denote FYE and plan fixed effects, respectively. Standard errors are double clustered by plan and FYE.

Panel A of Table 3 presents estimates of equation (1) for allocations to *Traditional* and *Alternative.* Columns (1) and (2) incorporate plan fixed effects to absorb time invariant heterogeneity. The results show that following a restriction, plans reduce their allocation to traditional assets by approximately 5.7%, while increasing their allocation to alternative assets by about 4.1%. *Post* is statistically significant at the 1% level in both columns. Plan size is negatively (positively) related to *Traditional (Alternative),* implying that larger plans are more likely to take risks. Funding ratio is positively (negatively) related to *Traditional* (*Alternative),* consistent with

evidence in the literature that underfunded plans take more risk (Novy-Marx and Rauh, 2009; 2011; Andonov et al., 2017; Mohan and Zhang, 2014; Addoum et al., 2010). In Columns (3) and (4), I include FYE fixed effects to account for time-varying shocks common to all plans. This improves model fit as reflected by the increase in adjusted R^2. The coefficients remain consistent with a risk-shifting behavior. Following the restriction, allocation to *Traditional* declines by 2.7% with a corresponding increase in *Alternative*. These effects are economically meaningful. For instance, a 3% increase in alternative allocation represents an approximate 18% rise relative to the pre-restriction average of 16.83% (Table 1, Panel B).

Panel B presents the results for allocations to individual asset classes. Column (1) suggests that the decline in traditional assets is driven by reductions in equities and fixed income, although these effects are not statistically significant. In line with the rise in alternative exposure observed in Panel A, Columns (5) and (6) show that hedge fund and real estate allocations significantly increase by 1.3% and 1.6%, respectively. There is no meaningful change in cash or private equity allocations.

To reduce selection bias concerns, I employ a propensity score match (PSM) procedure. Specifically, I match plan-FYEs exposed to an investment restriction (*Restriction* =1) with unexposed plan-FYEs (*Restriction* = 0) without replacement. [12] I match on *Log Plan Size, Plan Funding %, State*, and FYE. *State* is an indicator variable equal to one for plans identified by the PPD as administered at the state-level, and equal to zero for local plans. The resulting matched sample contains 1,333 pairs, yielding 2,666 plan-FYEs.

[12] I use the nearest neighbor approach and enforce a caliper of 0.05. Results are economically similar if I enforce a stronger caliper such as 0.01.

Panel C displays the results of estimating equation (1) on the matched sample. Columns (1) and (2) show effects that are qualitatively consistent with those in Panels A and B, but notably larger in magnitude. In addition to reinforcing the overall shift toward alternative assets, the matched estimates provide new insight into the composition of that shift. Specifically, the decline in traditional assets is driven by a statistically significant 2.7% reduction in equity allocation, with corresponding increases of 1.4% in hedge funds and 1.8% in real estate. Given that many restrictions either directly limit equity investments or target asset managers with a significant equity market presence, the observed reduction in equities is consistent with the nature of these constraints. The reallocation from public equities and into more opaque, higher-cost alternatives may reflect an effort to preserve a similar risk-return profile despite a politically constrained opportunity set. Table 3 illustrates that political interference alters plans' asset allocation decisions.

The following analyses examine cross-sectional variation to assess where the allocation changes reported in Table 3 are most concentrated. I focus on one key channel through which these effects should emerge if the allocation shifts are motivated by political aspirations: political influence. I hypothesize that the political environment in which public pension plans operate shapes their response to investment restrictions.[13] Specifically, I consider three dimensions of political influence that may shed light on the baseline results: i) the political leaning of the state, ii) political actions taken by state officials, such as signing coalition letters to signal their stance on ESG-related issues, and iii) the degree of state control over the pension system. I use equation (1) to estimate multivariate regressions within subsamples defined along these three dimensions.

[13] Existing literature documents the role political influence plays fund investment decisions and governance structures (Romano, 1993, 1995; Andonov, Hochberg, and Rauh, 2018; Bradley, Pantzalis, and Yuan, 2016; Lee, 2023; Wang and Mao, 2015; Brown, Pollet, Weisbenner, 2015; Tu et al., 2025).

To examine the impact of state political ideology on a plans' change in investment behavior, I identify the political leanings of U.S. states using data from FiveThirtyEight's partisan lean metric for 2021 (most recent year available). This metric is calculated as the average margin difference between how a state votes and how the country votes.[14] I define Republican-leaning (*Red*) states as states with a lean of -10% or more and Democratic-leaning (*Bue*) states as states with a lean of 10% or more. About 27% of plans affected by a restriction are headquartered in a *Blue* state, while about 73% of these plans are *Red.* Provided in the appendix is a list of states by partisan lean.

Panel A of Table 4 presents the results of testing equation (1) within political lean subsamples. The findings are striking; the decline in traditional assets and the corresponding rise in alternative assets is driven entirely by public pension plans in red states. Following the implementation of restrictions, plans in red states reduce their allocation to traditional assets by 3.4%, reallocation capital to alternatives. Blue states do not exhibit a meaningful change in their asset allocation post-restriction. The results support the conjecture that the political environment, particularly the partisan makeup of state leadership, shapes how plans navigate political pressure. The pronounced shifts in red states may reflect a political climate in which policymakers are more assertive or stringent in their directives, exerting greater influence over fund managers to align investment strategies with prevailing political ideologies. Conversely, blue states may adopt a more measured or hands-off approach, resulting in more muted changes to portfolio allocation.

To evaluate the second form of political influence— the political actions taken by politicians —I use hand-collected data from Ropes & Gray on recent multi-state coalitions

[14] The data can be accessed at https://github.com/fivethirtyeight/data/tree/master/partisan-lean.

surrounding ESG and political issues.[15] These coalitions take the form of letters signed by state elected officials, including attorney generals, treasurers, or governors. A signature on a letter signifies that a state politician took action to endorse the initiative outlined in the letter. A greater number of signatures for a letter indicates a stronger multi-state coalition supporting the initiative. [16] In total, I collect 26 distinct multi-state coalition letters from 2021 to 2023. All U.S. states have a political official that signed at least one letter, with exception to Hawaii. Across all letters, there are a total of 80 Democratic state signatures and 360 Republican state signatures, foreshadowing that Republican politicians are more aggressive in advancing their stance on ESG policy. The median (mean) number of letters signed by a state is 7 (8.68), while the maximum is 19 (Utah) and the minimum is 0 (Hawaii).

I create two cross-sectional indicators by state. The first is *Active,* which is a cross-sectional indicator variable set to one if a public pension plan is headquartered in a state that has signed greater or equal to the median number of multi-state coalition letters signed in the full sample. The second is *Red (Blue) Signer,* which is an indicator variable set to one if a plan is headquartered in a state where a Republican (Democratic) political official signs at least one coalition letter. [17]

In Panel B of Table 4 I divide the sample based on the indicator variable, *Active*. I posit that plans in states endorsing more letters—and thereby demonstrating stronger commitment to their political stance—will exhibit the risk-transfer behavior I have documented. The findings

[15] The information is attainable at https://www.ropesgray.com/en/sites/navigating-state-regulation-of-esg/multi-state-initiatives.
[16] For example, published on August 4th, 2022, 19 Republican state attorney generals signed a letter to the CEO of BlackRock asserting their dissatisfaction with its uses of state pension plan assets to promote an ESG agenda, and to address general concern with the company's investment and shareholder welfare strategies. To view this letter, see https://www.texasattorneygeneral.gov/sites/default/files/images/executive-management/BlackRock%20Letter.pdf.
[17] It is important to note that these groups are not mutually exclusive; for example, Arizona's Republican state attorney general signed a letter in 2022 (*Red Signer = 1*), while the Democratic attorney general that took office in 2023 signed a letter in 2023 (*Blue Signer = 1*). This overlay occurs in 3 states (affecting 14 plans) during the period in which I collect the letters.

support my conjecture: public pension plans headquartered in states more actively promoting their stance on ESG are the plans shifting capital toward alternative assets. In the subsample *Active*, *Traditional* drops by 3.7% ex-post, while *Alternative* increases by about 4.2%. We do not observe any significant effects in states that fall at the lower end of the multi-state coalition spectrum (*Active* = 0). The results show that my main results are concentrated in pension funds headquartered in states where politicians are more vocal in their pursuit of their social or political agendas. [18] I re-estimate this analysis using *Red (Blue) Signer* and report the results in the appendix. I interpret the results as reinforcement of the findings from the political lean analysis in Panel A.

Panel C of Table 4 divides the sample into state- and local-administered public pension plans. Of the 87 plans in the *Restriction* sample, 48 are state plans. I expect the main effects to be concentrated among state plans, as most restrictions in my sample are imposed at the state level, and these plans are more likely to face political pressure or direct oversight from state officials. In contrast, local plans may be more responsive to city or county-level governance. [19] The results support this conjecture: the observed allocation shifts occur exclusively among state plans. Taken together, Table 4 highlights the multifaceted impact of political influence on public pension investment decisions, offering preliminary evidence that political incentives – particularly where control is strongest—shape how plans allocate their assets when faced with interference.

4.2 Public Pension Plan Investment Performance

[18] The findings hold when redefining the indicator variable to represent plans headquartered in states falling within the top quartile of active signers.

[19] At first glance, it may seem obvious that state plans would exhibit the allocation changes, given that the restrictions are primarily enacted at the state level. However, two important caveats warrant attention. First, some local plans are administered by the state rather than independent local governments. Second, these publicized investment restrictions may indirectly influence local plan investment behavior by increasing political pressure or prompting anticipation of future restrictions.

The previous empirical results indicate that plans shift their investments to alternative assets in response to politicized investment restrictions. In this subsection, I explore the consequences of these changes for investment performance. I estimate the following regression:

$$Performance_{i,t} = \beta Post_{i,t} + X_{i,t-1} + \varphi_t + \delta_i + \varepsilon_{i,t} \qquad (2)$$

where, $performance_{i,t}$ is the peer-adjusted return of plan *i* at time *t*. *Peer-adjusted return* is computed for the annual investment return, *Traditional* and *Alternative,* and individual asset class returns. It is calculated by subtracting the average return of all other plans from a plan's investment return during the same FYE. This calculation removes concerns that the return effects we pick up are due to the time trend in public pension plan performance. $X_{i,t-1}$ is a vector of lagged plan characteristics including *Log Plan Size*, *Plan Funding %,* asset allocations, and *peer-adjusted returns.* The annual investment return contains the lagged asset allocation and peer-adjusted returns of all asset classes as controls, while the regressions on individual asset returns contain the previous allocation and return of just that asset.[20] Traditional return is calculated as a weighted average of peer-adjusted returns for equities (EQ), fixed income (FI), and cash:

$$Traditional = w_{EQ} * R_{EQ}^{peer} + w_{FI} * R_{FI}^{peer} + w_{Cash} * R_{Cash}^{peer}$$

where *w* represents allocation weights and R^{peer} indicates peer-adjusted returns. I construct *Alternative* return analogously using the corresponding asset classes. φ_t and δ_i denotes FYE and plan fixed effects, respectively. Standard errors are double clustered by plan and FYE. Controls are suppressed for brevity; the full model is reported in the appendix.

[20] Andonov and Rauh (2022) show that public pension fund return expectations positively correlate to cross-sectional differences in their past performance, suggesting the importance to control for lagged performance and asset allocations.

Table 5 provides the results of equation (2). In column (1), the annual investment return is not significantly different following the restriction. However, when I analyze the asset groups in columns (2) and (3), the performance of traditional assets gains 30 basis points (bp) ex-post, while the performance of alternatives declines by 70 bps. Driving the improvement in *Traditional,* fixed income performance rises by 130 bps following the restriction. Equity rises by 50 bps, but the effect is not statistically significant. Meanwhile, a 230bp drop in hedge fund performance drives the decline in the performance of alternatives. Considering the documented allocation changes, these findings suggest that pension managers' investment choices in response to the restrictions are inefficient. After the investment restriction, traditional assets outperform, yet pension funds reduce exposure to these assets, foregoing positive returns. Instead, funds reallocate capital to lower performing, riskier assets such as hedge funds. Collectively, these patterns suggest that political interference distorts portfolio allocation in a manner that may impair overall performance.

4.3 Anecdotal Evidence on the Portfolio Costs of Political Interference

Although Table 5 provides suggestive evidence of the costs associated with politicized investment restrictions, finding direct evidence is challenging for two key reasons. First, the limited post-restriction time series prevents a comprehensive assessment of long-term performance impacts, as most restrictions are relatively recent. [21] Second, public pension plans oversee substantial assets, averaging $13.6 billion in this study. As a result, restriction-driven allocation shifts likely affect a small share of total assets, making performance impacts difficult to detect. [22]

[21] This is especially true for alternatives like private equity funds, where internal rates of return are not realized until exit. Before exit, reported returns primarily rely on interim valuations of illiquid assets with long holding periods.
[22] For instance, in August of 2022, the $32 billion Texas Employees Retirement System divested about $646 million to comply with the mandated financial blacklist. This $646 million comprised a mere 2% of its total portfolio.

To more directly assess the costs associated with the restrictions, I examine treated plans (*Restriction* = 1) at the transactional level. Appendix Table A6 details three cases: the Missouri States Employees' Retirement System (MOSERS), the Texas Employees' Retirement System (TX ERS), and the Texas Municipal Retirement System (TX MRS). For each case, I document both the divestments made and corresponding investments undertaken to comply with the restriction.

Case A: Missouri States Employees' Retirement System

Panel A presents the case of MOSERS. In June of 2022, the plan adopted a board policy eliminating ESG considerations in its proxy voting. MOSERS held a share in a commingled fund that included BlackRock, a firm known for incorporating ESG factors into its voting decisions. Despite efforts to prohibit BlackRock from voting proxies on behalf of MOSERS, the commingled fund structure made this infeasible. As a result, MOSERS withdrew from the fund ($500 million) and reallocated the capital to hedge funds. [23]

MOSERS' reallocation sought to maintain the investment manager's desired Sharpe ratio. [24] However, a fee comparison between the divestment and reinvestment reveals a substantial cost increase. The transition from a low-cost, diversified commingled fund to higher-cost hedge funds resulted in an additional $27.85 million in management fees—a 5.89% increase relative to market value. While the reallocation may enhance gross returns, the significant rise in fees could offset potential gains. Moreover, hedge funds require more time and resources to generate returns compared to commingled funds, which offer cost-effective, broad market exposure.

[23] Note that the new investment amount is not exactly $500,000,000. Data limitations prevent the ability to identify the investment to the exact dollar in these detailed cases.
[24] A possible motivation for shifting capital into alternatives to maintain the Sharpe ratio is that these assets are linked to lower reported volatility in plan returns (Munnell, Aubry, & Crawford, 2016).

Case B: Texas Employees' Retirement System

Panel B presents the case of the Texas ERS, which responded to the statutory restricted financial company list released by the TX comptroller in August of 2022. At the time, the ERS had exposure to four restricted financial institutions, totaling around $646 million in market value, which the law required the fund to divest. This exposure was primarily through externally managed holdings, with a small portion consisting of public securities held within the underlying portfolios of non-blacklisted external managers. To comply with the legislation, TX ERS withdrew capital from BlackRock, a blacklisted institution, and Pzena, due to its underlying exposure to restricted public securities. The fund also requested that external managers divest from individual public securities of blacklisted institutions, while retaining their relationships with those managers.

Following the divestment, the ERS reinvested the $646 million across three asset classes: approximately $526.3 million in alternatives—$57.4 million in private credit and $468.9 million in hedge funds—and $100 million in public equity. This shift into private assets and hedge funds increases management fees, nearly doubling the fee from 0.39% on the divested capital to 0.81% on the replacement investments. In essence, the ERS replaced low-cost external managers, like BlackRock, known for its predominantly passive products, with actively managed funds characterized by higher fees. The behavior reflects a risk transfer, likely intended to maintain the plan's target return-risk profile despite a constrained investment universe. In seeking to replicate pre-divestment portfolio performance, the ERS turned to alternatives—accepting higher fees, lower liquidity, and reduced transparency. While this shift may aid diversification and return objectives, elevated management fees likely offset much of the potential for performance gains.

Case C: Texas Municipal Retirement System

Panel C details the case of TX MRS, which faced the same financial blacklist as the ERs in Panel B. At the time of the list's release, TX MRS held a separately managed account with BlackRock for its core fixed income investment, valued at about $1.52 billion. In compliance with the law, the fund terminated its BlackRock account and redistributed the divested capital across private credit, real estate, real assets, private equity, and other alternative investments.

This reallocation led to an increase in fees by .43% of assets managed, translating to an additional $6.88 million relative to the prior BlackRock account. Notably, TX MRS transitioned from a relatively lower-risk, cost-effective fixed income strategy to investments requiring higher risk tolerance and incurring greater management fees. As in cases A and B, this reallocation appears driven by the investment team's effort to preserve the portfolio's risk-return profile, seeking alternative sources of yield to achieve comparable returns and stable Sharpe ratio.

The anecdotal evidence in Table A6 suggests that politically imposed investment restrictions lead to inefficient and costly portfolio adjustments. To preserve performance, plans shift capital away from traditional, cost-effective assets toward higher-risk, higher-fee alternatives. This reallocation increases opacity, reduces equity exposure, and raises management costs—introducing complexity and uncertainty without clear evidence of superior net returns (net of fees).

4.4 Analysis of Public Pension Plan Equity Holdings

The earlier results show that plans subject to political interference adjust their portfolio allocation across asset classes. This raises a further question: Do these restrictions also affect equity selection? In particular, do plans reallocate capital toward or away from the firms targeted by the restrictions, such as those in the energy (e.g., fossil fuel, coal) and firearms sectors? To explore

this, I analyze changes in equity holdings using 13F filings.[25] I examine whether there are observable shifts in holdings of energy and weapons firms by constructing custom industry classifications.[26] I estimate the following regression:

$$TargetedSectorExposure_{i,t} = \beta Post_{i,t} + Log(HV)_{i,t} + \varphi_t + \delta_i + \varepsilon_{i,t} \qquad (3)$$

where, i indexes plans and t indexes year-quarters. The dependent variable, $TargetedSectorExposure_{i,t}$ is measured as either *Energy(Guns)%_MV* – the proportion of a plan's equity holdings invested in energy (gun) stocks – or as *Energy(Guns)_Count,* the number of such stocks held. *Log(HV)* is the logged market value of a plan's equity portfolio as reported in 13F. φ_t and δ_i are year-quarter and plan fixed effects. I cluster standard errors by plan (13F manager).

Panel A of Table 6 presents the results of equation (3) for the full sample of 13F-reporting plans with an investment restriction (*Restriction* =1). Columns (1) and (2) show no meaningful change in exposure to energy stocks. In contrast, columns (3) and (4) reveal a significant decline in the number of gun-related stocks held. Only two states in the sample—Connecticut and Rhode Island—enact firearm-related restrictions, so I place limited weight on this finding.

Panel A highlights that the nature and subject matter of the restriction matters. Red-state plans in my sample face restrictions aimed at protecting the energy sector. For example, Texas (SB13) bands investment in companies that boycott energy, and Missouri's policy prohibits the integration

[25] Many public pension plans do not appear in Form 13F filings. This is due to several reasons, including: (i) the plan outsources all asset management, (ii) it does not meet the $100 million threshold in 13F-reportable securities, or (iii) its equity exposure is held through commingled funds, mutual funds, or other passive vehicles that do not require disclosure. See the appendix for the sample of 13F-reporting managers included in this study.

[26] My energy classification captures firms involved in extraction, refining, distribution, and infrastructure related to fossil fuels. This includes SIC codes used by Fama-French to define the Energy sector (1200–1399, 2900–2999) as well as select codes from Utilities (e.g., 4920–4939), Pipelines (4610–4629), and Wholesale sectors (5160–5172) that reflect midstream and downstream fossil fuel activity. This approach provides a more comprehensive measure of fossil-related holdings relevant to ESG divestment policies. For more details, see the appendix.

of ESG in proxy voting, prompting future divestments. Meanwhile, blue-state restrictions typically call for divestment from fossil fuel or weapons-related stocks, often as part of broader decarbonization or social responsibility goals. This suggests that the impact of restrictions on equity holdings is likely to differ by political alignment. Panel B of Table 6 investigates this by splitting the sample by political affiliation and comparing restricted plans to other red (blue) plans not subject to restrictions. Columns (1) - (4) reveal that red states do not significantly alter their portfolio holdings in the targeted sectors following political interference. This is noteworthy: if such policies are intended to promote energy investment, the results do not support that objective. However, columns (5)-(8) indicate that blue plans significantly reduce both the value and number of fossil fuel and weapon-related holdings relative to non-restricted blue plans. These findings align with the intent of blue state restrictions.

Panel C compares post-restriction changes between red and blue plans. No statistically significant difference exists between their energy or weapons holdings following an interference, except for a relative decline in firearm stocks among blue plans. This finding is somewhat surprising—if restrictions are politically motivated, we might expect red and blue states to respond in opposite directions. Yet the limited divergence suggests that, in some cases, restrictions endorsed by elected officials serve more as symbolic political gestures than binding mandates that materially alter equity choice.[27] Overall, Section 4 demonstrates that such interference does, in fact, disrupt plan investment behavior, with potentially costly consequences.

5. Incentives of State Politicians

[27] These findings are consistent with Rajgopal, Srivastava, and Zhao (2024), who find no meaningful difference in energy holdings composition between red and blue states, and conclude that many political statements on ESG lack tangible follow-through.

The previous section focuses on the impact of political interference on the public pension plan itself. This section shifts perspective to evaluate the potential benefits—or costs—for the politicians responsible for implementing or influencing investment restrictions. Specifically, I examine whether such restrictions lead to increased media attention for these officials.

I collect data from prominent news sources using Media Cloud, the world's largest open-source news database.[28] I count the number of times a politician's name co-occurs with ESG-related or investment restriction-related terms.[29] Politicians are identified using multiple name variants (i.e., full name, nickname plus last name, or title plus last name) to capture mentions accurately while minimizing false positives. For example, mentions of the Massachusetts governor could include "Maura Healey," "Healey," or "Governor Healey," so long as an ESG- or restriction-related keyword also appears in the same article. Based on manual reviews of press releases and media coverage related to the investment restrictions in this study, I aggregate annual article counts at the state level from 2018 to 2023, summing mentions of political figures commonly and publicly linked to these policies. These include (titles varying by state) Governor, Treasurer, Comptroller, Director of Administration, Auditor, and Chief Financial Officer. I estimate a state-level regression to test whether restrictions are associated with increased media attention to these officials:

$$TotalArticles_{i,t} = \beta Post_{i,t} + Election\ Cycle_{i,t} + \varphi_t + \delta_i + \varepsilon_{i,t} \qquad (4)$$

[28] The news source directory can be accessed here https://www.mediacloud.org/media-cloud-directory. I use the directory titled 'US Most Visited News Online (Mar 2025)' which includes sources such as *New York Times*, *Reuters*, *Forbes*, *politico*, among 42 others.

[29] The keyword group includes the following: anti-ESG legislation; ESG legislation; anti-ESG bill; ESG bill; ESG regulation and (pension or retirement); divest blackrock; divest fossil; boycott oil; boycott fossil; prohibit ESG; restricted company list; restricted financial institution list; (woke and pension); (public pension or pension or retirement system) and (ESG or environmental social and governance or anti-ESG); state divest and (ESG or environmental social and governance or anti-ESG); pension and (divest or boycott) and (ESG or anti-ESG); investment policy and (ESG or fossil fuel or social criteria); pension and (proxy vote or proxy voting) and ESG.

where i indexes states and t indexes years. $TotalArticles_{i,t}$ is the number of news articles in which the name of any of a state's covered political officials co-occurs with an ESG- or restriction-related term. To account for media garnered driven by election cycles rather than the investment restriction, $Election\ Cycle_{i,t}$ is an indicator variable set to one in gubernatorial election years. φ_t and δ_i represent year and state fixed effects, respectively. Standard errors are clustered by state. Due to sample size concerns, I present results using both an ordinary least squares (OLS) and matched sample approach.[30] OLS regressions include all states where politicians impose an investment restriction, while the matched sample included a treated (*Restricted*) and control state.

Panel A of Table 7 presents the results of estimating equation (4) for the full sample of treated states. Column (1) shows that, using OLS, politicians garner higher media attention following a restriction. I introduce year fixed effects in column (2); although the adjusted R_2 slightly improves, the coefficient on *Post* is no longer statistically significant. However, the matched sample estimates in columns (3) and (4) reveal a significant increase in article counts, suggesting that restrictions are indeed a form of interference associated with greater media visibility for state politicians.

Panel B presents results by political alignment. Similar to Panel A, the matched sample specification reveals that politicians receive greater media coverage following the imposition of a restriction. Specifically, Column (3) shows that red-state politicians experience a boost in attention after implementing a restrictive investment policy, relative to other red states without such a policy. In contrast, I observe no comparable increase among blue states. This pattern connects to the findings in Table 4, where the political environment in red states appears to drive more assertive and stringent restrictions that lead to tangible allocation changes. That same environment may also

[30] States are matched on the propensity to have a restricted law by political affiliation and year.

explain why these politicians garner more media coverage. In this way, the incentive to be vocal in promoting restrictions may reflect a strategy aimed more at garnering political capital than improving pension plan performance.

Panel C compares media coverage of politicians in red versus blue states. In column (2), the positive and significant interaction between *Red* and *Post* indicates that red state politicians receive more media coverage relative to their blue-state counterparts. Although the effect—roughly two additional articles—persists in Column (4), the estimate loses statistical significance. These findings suggest that red states may attract disproportionate media attention, consistent with the idea that such policies are more politically salient in those environments. However, the evidence is modest, and it remains possible that both affiliates receive comparable increases. Overall, Table 7 indicates that the politicization of pension plan investment policy may yield political benefits, even when the value to retirement plan performance is unclear.

6. Economic Welfare of State Constituents

In this section, I evaluate how political interference in pension investments affects broader state welfare, including beneficiaries, taxpayers, and firms. Section 4 shows that restrictions lead to inefficient portfolio decisions, which—given the governance structure of public pension plans—suggests taxpayers may bear long-term costs. Section 5 indicates that politicians may benefit by championing such policies. The question remains whether, and to what extent, constituents are affected. I address this by expanding the analysis to include additional state-level outcomes.

6.1 Employment Outcomes in Politically Targeted Sectors

I begin by testing whether state-level employment changes in the sectors targeted by investment restrictions. I hand-collect annual state-level employment data from the U.S. Bureau

of Labor Statistics for the years 2018–2023, using industry classifications based on NAICS codes. I construct two groupings: Fossil Fuel, which includes employment in oil, gas, and coal sectors, and Weapons, which includes firearms and ammunition manufacturing.[31] For each state-year, I compute the share of total industry employment attributable to each targeted sector. I estimate the following state-level regression within the sample of treated states (*Restriction* = 1):

$$Employment_{i,t} = \beta Post_{i,t} + \varphi_t + \delta_i + \varepsilon_{i,t} \qquad (5)$$

where, *i* indexes state and *t* indexes year. $Employment_{i,t}$ is measured as either *%FossilFuelEmpl* or *%WeaponEmpl*, defined as employment in the respective sector scaled by total industry employment. φ_t and δ_i represent year and state fixed effects, respectively. I cluster standard errors at the state level.

Panel A of Table 8 presents the results of equation (5) across the full sample. Columns (1) and (2) account for time-invariant state characteristics, while columns (3) and (4) include year fixed effects. Across all columns, I find observe no statistically significant change in fossil fuel or weapon-related employment.

Given that red and blue states pursue different political goals in crafting these policies—some aiming to support, others to divest from targeted sectors—a full-sample average may mask divergent effects. Panel B addresses this by splitting the sample by political alignment. However, neither red nor blue states exhibit statistically significant changes in sectoral employment.

[31] *%FossilFuelEmpl* includes sectors related to fossil fuel extraction and wholesale processing, such as Oil and Gas Pipeline Construction (NAICS 237120), Bituminous Coal and Lignite Surface Mining (NAICS 212111), and Petroleum Bulk Stations and Terminals (NAICS 424710), among seven others. *%WeaponEmpl* includes Small Arms Ammunition Manufacturing (NAICS 332994) and two additional weapon-related codes. I report more detail in the appendix.

Panel C tests whether red and blue states differ in their post-restriction employment trends. Again, no significant differences are detected. Overall, Table 8 explores one potential channel through which restrictions might affect the state economy. The results suggest that these policies do not measurably alter employment levels in the targeted sectors. Re-estimating the models using a matched sample yields consistent findings.

6.2 Market Reaction among Restriction Targets

I examine whether the political interference studied in this paper affects the firms at the center of it. Specifically, I test whether energy companies and financial institutions named on state financial blacklists experience significant market reactions to the announcement of public pension investment restrictions. To do so, I conduct an event study using a standard market model following MacKinlay (1997). The estimation window spans 250 trading days, beginning 31 days before the event window, and requires 90 to 120 days of valid return data. I focus on four event windows: (-3,3), (0,1), (0,3), and (-1,1), where day 0 represents the date of the restriction announcement.

I begin by analyzing firms in the energy sector, as defined by the Fama-French 12 industry classification. Because restriction announcement dates vary across states, I treat each event as a separate shock and apply the same set of energy firms to each event date, pooling them into a single stacked sample. I estimate cumulative abnormal returns (CARs) around each announcement using the event study framework described above. To test whether mean CARs differ significantly from zero, I conduct a difference-in-means test and cluster standard errors at the firm level to account for cross-sectional correlation in returns. I present the results in Panel A of Table 9. Columns (1)– (4) report average cumulative abnormal returns (CARs) across the full sample of restriction announcements. I observe a statistically significant negative reaction in the (0,3) and (–1,1) windows, suggesting that investors respond unfavorably to these announcements. Figure 1

provides visual context for the timing and direction of the market reaction, helping to explain why other event windows—though negative—lack statistical significance. Overall, Panel A suggests that energy firms experience a decline in market value following this political interference.

Columns (5) – (8) reveal a more nuanced story. For each window, I report the average CARs in the sample of restrictions announced in red states versus blue states. I do so by regressing the CAR for firm *i* in window *t* on *Red,* an indicator equal to one (zero) for red state (blue state) restrictions. The idea is that red- and blue-state restrictions reflect opposing policy stances toward energy firms: red states seek to promote or shield the energy industry by removing capital from institutions that boycott fossil fuels, whereas blue states aim to reduce exposure to the energy industry through divestment mandates. These divergent approaches may elicit different investor reactions. The results are striking; across all windows, the market response is asymmetric, with statistically positive (negative) market reaction to blue (red) state restrictions. One possible explanation is that blue-state divestment mandates are largely anticipated and interpreted as symbolic, signaling a long-term policy orientation rather than triggering immediate capital withdrawal. In contrast, red-state anti-boycott laws—despite their pro-energy framing—may introduce political and regulatory uncertainty. By targeting major financial intermediaries, they may disrupt capital access or raise reputational concerns among investors, creating unintended risks for the very energy firms they aim to protect.

Panels B and C present results from an alternative event sample. I focus on publicly traded firms listed in the initial version of Oklahoma's Restricted Financial Company List, published under the Energy Discrimination Act (HB 2034). The event date corresponds to the public

announcement of the list in May 2023.[32] Using the same estimation parameters as in Panel A, I test whether the listed firms experienced statistically significant cumulative abnormal returns (CARs) upon the announcement.

Panel B reports average CARs across the firms on the list. The results reveal a strong negative reaction to the blacklisting of these financial institutions, consistent across all event windows. Figure 2 illustrates this clear downward trend upon the announcement of the OK law. Panel C provides firm-level CAR estimates, all of which are negative, though the magnitude varies across firms and windows. Some institutions—such as State Street—exhibit particularly large and persistent losses, with CARs of –3% or more in nearly every window. Figure 3 presents these firm-level patterns. Table 9 highlights that political interference in investment policy—particularly when highly visible and politicized—can have real market consequences. Investors react, firms bear the cost, and the impact extends beyond the pension plans themselves.

7. Identification and Robustness

To ensure the validity of the baseline results, I perform robustness tests to address potential concerns. First, I re-estimate the matched sample regressions in Table 3 using alternative variables in the PSM; specifically, I add political lean to the set of matching variables and I match on fiscal year rather than fiscal year-end. The results remain robust to these alternatives.

A second robustness test is a restricted window analysis. To ensure that tabulated results are attributed to the regulation, rather than unrelated long-term trends or other external, confounding factors over time, I perform equation (1) on plan-FYE observations that are within

[32] The financial institutions on the list included in my sample are GCM Grosvenor Inc., Stepstone Group Inc., Wells Fargo & Company, JPMorgan Chase & Company, Bank of America Corporation, State Street Corporation, and BlackRock Inc. See the appendix for a copy of the list as released by the Oklahoma State Treasurer.

five years from the date the restriction takes effect. Table A1 reports the results, corroborating my baseline findings that post-restriction, public pension funds reduce their allocation to traditional assets in exchange for taking greater risk.

Thirdly, I confront the concern that the economy-wide low equity market returns in 2022 may mechanically reduce equity allocation. While the FYE fixed effects mitigate this concern, I further validate the findings by re-estimating equation (1) using the percentage of *target*, rather than actual, asset allocations as the dependent variable. Table A2 presents the results, which align with my baseline findings, reinforcing that the observed effects are driven by the investment team's response to the investment restriction rather than poor equity performance.

8. Conclusion

This study provides evidence of a fundamental governance conflict: state politicians, driven by political incentives, may prioritize their own objectives over the interests of public pension plans and the welfare of their constituents. Using state-imposed investment restrictions as a testing ground, I examine outcomes for three key actors—plans, politicians, and constituents. I find that restricted plans reallocate capital away from traditional assets, particularly equities, and toward higher-cost, less transparent alternatives such as hedge funds and real estate. These shifts are most pronounced in Republican-leaning states and in environments where politicians are especially active and assertive in pushing political agendas.

This reallocation comes at a cost. Plans forgo risk-adjusted returns from traditional assets and incur higher fees associated with alternatives, impairing overall performance. These changes appear driven not by portfolio optimization, but by political pressure to maintain assumed return targets or deflect scrutiny by investing in harder-to-monitor asset classes.

Within equities, I find that plans in blue states reduce exposure to energy and gun stocks—the specific industries targeted by their investment restrictions. In contrast, I find no evidence that red-state restrictions lead to increased investment in sectors such as energy, despite being framed as efforts to support those industries. The lack of change by red-state plans implies that these restrictions may be primarily performative—designed to publicize a political stance on ESG rather than to meaningfully shift capital allocation.

While politicians benefit by advancing their agendas and gaining media attention, the intended economic impact of these restrictions appears largely absent. I find no evidence of employment changes in the industries targeted by divestment mandates. Where political interference is most visible, however, investors respond. I document asymmetric market reactions to restriction announcements: red-state laws trigger negative CARs among energy firms and targeted financial institutions, while blue-state laws are met with modestly positive reactions. This asymmetry may stem from differences in investor expectations or perceptions about how restrictions are introduced and enforced across political contexts.

Together, these findings highlight the agency costs of political interference in public pension governance. The consequences—higher investment costs, weakened returns, and market distortions—underscore the risks of politicizing public capital. We need greater scrutiny to ensure that the burdens of incentive misalignment do not fall on the very retirees and workers the system is meant to serve.

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Figure 1. Average CAR Around Restriction Announcement. Average cumulative abnormal return (CAR) around the announcement of public pension investment restrictions.

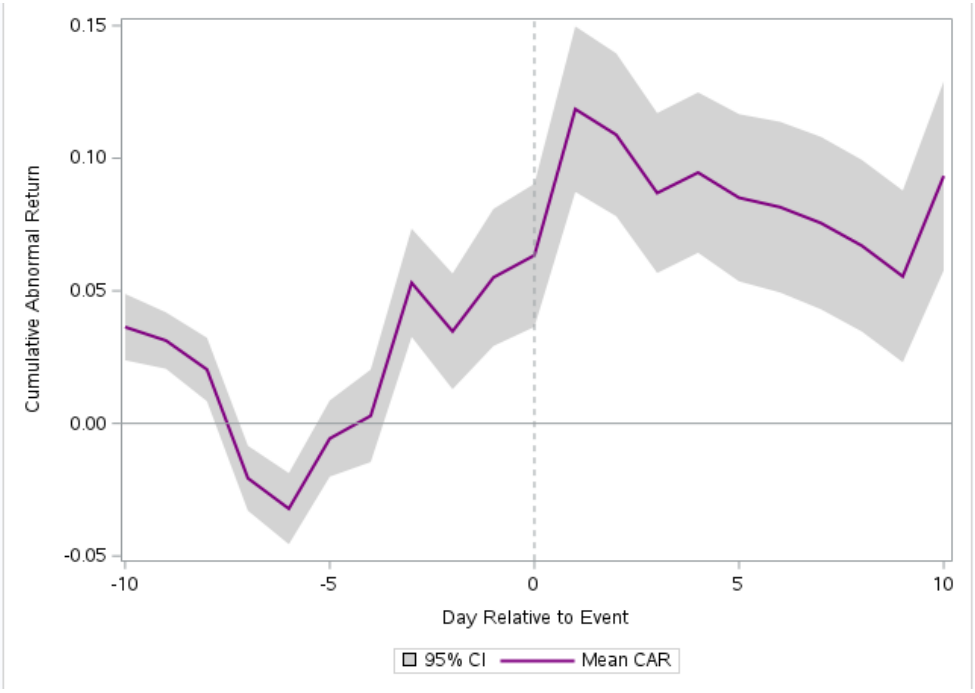

Figure 2. Average CAR for Firms on OK Restricted Financial Company List (HB 2034). Average CARs for firms targeted by Oklahoma's restricted financial company list.

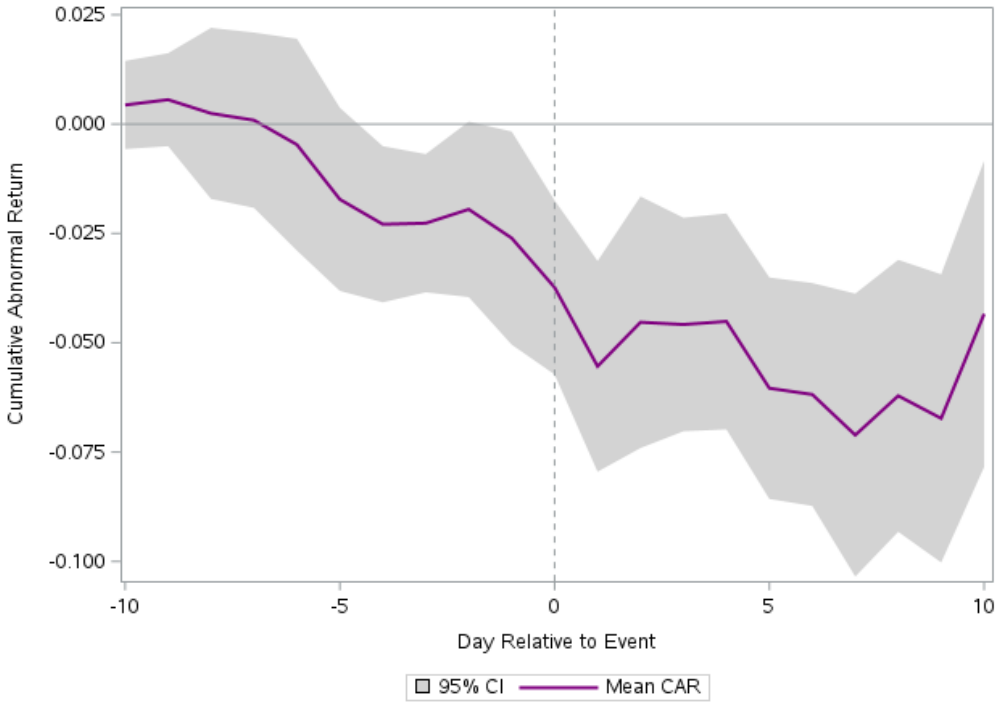

Figure 3. Firm-Level CARs for Firms on Oklahoma List. CARs by firm for companies listed on Oklahoma's restricted financial company list

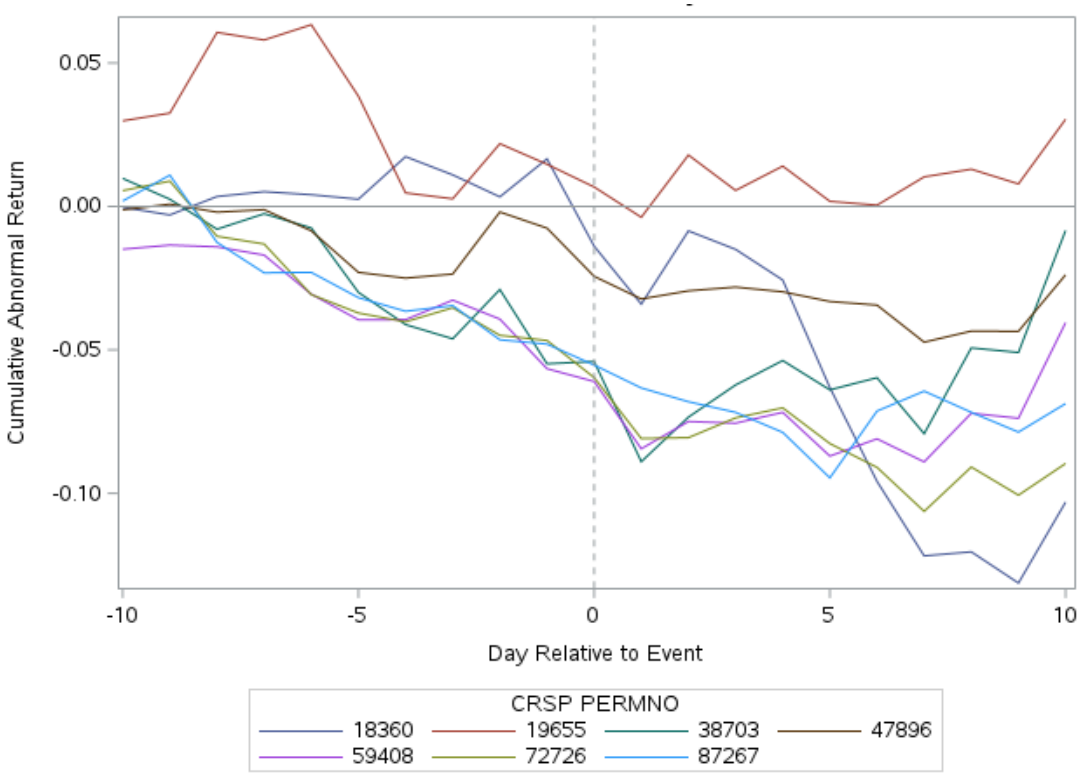

Figure 2 Legend	
Permno	Institution
18360	G C M GROSVENOR INC
19655	STEPSTONE GROUP INC
38703	WELLS FARGO & CO
47896	JPMORGAN CHASE & CO
59408	BANK OF AMERICA CORP
72726	STATE STREET CORP
87267	BLACKROCK INC

Table 1. Summary Statistics. This table contains summary statistics for the variables used in this study, tabulated at the plan-fiscal year-end level. Panel A contains distributional statistics for the states and plans affected by the restrictive regulations in the sample. The panel shows the year the restriction is effective, the number of plan-year observations per state, the number of pension funds per state where *Restriction* = 1, the number of funds per state where *Post* = 1, and the type of investment restriction. *Restriction* is an indicator variable equal to one for public pension plans subject to an investment restriction. *Post* is a time-varying indicator set to one if a plan's fiscal year-end occurs after the effective date of a Restriction. Panel B contains summary statistics for the pension fund variables in the sample. All continuous variables are winsorized at the 1% and 99% level.

Panel A: Restriction Statistics

States by Year of Law	N	Number of Plans		Restriction Type
2020		*Restriction*= 1	*Post* = 1	
Connecticut	95	4	4	Industry Specific
New York	184	8	8	Industry Specific
Rhode Island	71	3	3	Industry Specific
2021				
Maine	46	2	2	Industry Specific
Texas	299	13	13	Financial Blacklist
California	23	1	1	-
2022				
Arizona	115	5	5	Divestment
Florida	115	5	5	Divestment
Kentucky	92	4	4	Financial Blacklist
Louisiana	207	9	9	Divestment
Missouri	322	14	14	Divestment
Oklahoma	138	6	6	Financial Blacklist
South Carolina	69	3	3	Divestment
Utah	46	2	2	Divestment
West Virginia	92	4	4	Financial Blacklist
2023				
Arkansas	89	4	1	Financial Blacklist
Total	2003	87	84	

Panel B: Public Pension Plan Statistics

				Distribution				
	N	Mean	Standard Deviation	10th	25th	50th	75th	90th
Plan Size ($000)	5151	13,600,000	25,300,000	291,545.10	1,053,770	4,030,216	14,000,000	34,300,000
Plan Funding %	5050	77.72%	19.11%	55.02%	66.89%	78.81%	90.70%	99.84%
Annual Investment Return	5143	6.72%	10.68%	-7.10%	0.37%	8.27%	13.90%	18.60%
% Traditional	4302	79.33%	13.76%	60.00%	70.80%	80.93%	90.00%	96.00%
% Alternative	4302	16.83%	12.81%	0.00%	6.42%	15.50%	25.00%	35.70%
% Allocation Equity	4302	51.73%	11.53%	36.00%	44.48%	53.00%	60.40%	66.00%
% Allocation Fixed Income	4302	25.84%	8.14%	16.40%	20.53%	25.00%	30.37%	36.10%
% Allocation Cash	4302	1.76%	2.19%	0.00%	0.04%	1.00%	2.60%	4.85%
% Allocation Private Equity	4302	6.21%	6.52%	0.00%	0.00%	4.75%	10.09%	14.90%
% Allocation Hedge Funds	4302	4.52%	6.73%	0.00%	0.00%	0.00%	7.55%	14.60%
% Allocation Real Estate	4302	6.10%	4.78%	0.00%	0.26%	6.30%	9.40%	12.00%
% Allocation Commodities	4302	1.83%	3.57%	0.00%	0.00%	0.00%	2.10%	7.34%
% Allocation Alternative Misc	4302	1.32%	4.46%	0.00%	0.00%	0.00%	0.00%	2.80%
Equity Portfolio Return	4027	8.12%	16.88%	-15.60%	-3.22%	11.17%	20.02%	26.80%
Fixed Income Portfolio Return	3997	4.63%	5.56%	-1.10%	1.24%	5.27%	7.90%	10.90%
Cash Portfolio Return	1647	1.99%	2.62%	0.10%	0.30%	1.30%	2.80%	5.30%
Private Equity Portfolio Return	2512	11.86%	15.45%	-6.72%	4.10%	12.20%	19.60%	27.01%
Hedge Fund Portfolio Return	1742	5.13%	8.48%	-4.26%	0.47%	5.41%	9.60%	14.70%
Real Estate Portfolio Return	2911	8.69%	12.24%	-4.88%	5.10%	9.87%	14.84%	21.96%
Commodities Portfolio Return	1162	5.27%	11.70%	-9.26%	-0.20%	5.85%	12.00%	17.70%
Alternative Misc Portfolio Return	388	8.63%	13.40%	-7.00%	2.26%	8.75%	15.00%	22.90%

Table 2. Paired T-Tests on Public Pension Fund Allocation. This table reports the results of tests that compare the percentage of pension fund portfolios allocated to different asset classes before and after an investment restriction take effect. This set of tests is performed only within the sample of pension plans affected by an investment restriction (*Restriction* = 1). The main independent variable of interest is *Post*, a time-varying indicator set to one if a plan's fiscal year-end occurs after the effective date of a *Restriction*. I use Welch's test statistic adjustment.

		Asset Allocation							
	N	Traditional	Alternative	Equity	Fixed Income	Cash	Private Equity	Hedge Fund	Real Estate
Post = 0	1391	0.8084	0.1559	0.5259	0.2621	0.0205	0.0521	0.0512	0.0527
Post = 1	152	0.6707	0.2672	0.4426	0.2079	0.0202	0.1091	0.0736	0.0846
Difference		0.1377***	-0.1113***	0.0833***	0.0542***	0.0003	-0.057***	-0.0224***	-0.0319***
T-Statistic		(11.73)	(-9.46)	(9.10)	(7.99)	(0.15)	(-8.98)	(-3.96)	(-7.13)

Table 3. Multivariate Regressions of Public Pension Fund Allocation. This table reports results of multivariate regressions on the allocations to different assets of pension funds. Panel A examines the allocation to traditional assets, defined as the percentage of assets allocated to equity, fixed income, and cash, as well as to alternative assets, defined as the percentage of assets allocated to hedge funds, private equity, and real estate. Panel B examines the asset allocation to individual asset classes. Panel C reports results of matched sample regressions on the allocations to different assets of pension funds. The main independent variable of interest is *Post*, a time-varying indicator set to one if a plan's fiscal year-end occurs after the effective date of a *Restriction*. The standard errors are double clustered by plan and fiscal year-end (FYE).

Panel A: Traditional and Alternative Allocation

	(1)	(2)	(3)	(4)
VARIABLES	Traditional	Alternative	Traditional	Alternative
Post	-0.057***	0.041***	-0.027*	0.031**
	(-4.18)	(3.37)	(-1.93)	(2.31)
Log Plan Size (t-1)	-0.171***	0.134***	-0.017	0.014
	(-11.63)	(10.43)	(-0.80)	(0.68)
Plan Funding % (t-1)	0.298***	-0.234***	-0.007	-0.000
	(8.38)	(-7.44)	(-0.14)	(-0.01)
Plan FE	Yes	Yes	Yes	Yes
FYE FE	No	No	Yes	Yes
Observations	4,030	4,030	4,014	4,014
Adj. R-squared	0.670	0.690	0.733	0.732

Panel B: Individual Asset Allocation

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Equity	Fixed Income	Cash	Private Equity	Hedge Fund	Real Estate
Post	-0.011	-0.015	-0.001	0.001	0.013**	0.016***
	(-0.81)	(-1.39)	(-0.47)	(0.16)	(2.10)	(3.63)
Log Plan Size (t-1)	-0.009	0.000	-0.008**	0.017**	-0.008	0.005
	(-0.52)	(0.00)	(-2.26)	(1.99)	(-0.64)	(0.62)
Plan Funding % (t-1)	0.036	-0.036	-0.007	0.001	0.001	-0.003
	(0.84)	(-1.10)	(-0.85)	(0.05)	(0.04)	(-0.20)
Plan FE	Yes	Yes	Yes	Yes	Yes	Yes
FYE FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,014	4,014	4,014	4,014	4,014	4,014
Adj. R-squared	0.671	0.583	0.422	0.745	0.574	0.737

Panel C: Matched Sample

VARIABLES	(1) Traditional	(2) Alternative	(3) Equity	(4) Fixed Income	(5) Cash	(6) Private Equity	(7) Hedge Fund	(8) Real Estate
Post	-0.042***	0.038***	-0.027*	-0.014	-0.001	0.006	0.014**	0.018***
	(-2.95)	(3.19)	(-1.95)	(-1.18)	(-0.37)	(0.68)	(2.12)	(3.82)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FYE FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,542	2,542	2,542	2,542	2,542	2,542	2,542	2,542
Adj. R-squared	0.719	0.726	0.660	0.575	0.428	0.710	0.573	0.734

Table 4. Political influence. This table reports results multivariate subsample regressions on the allocations to *Traditional* and *Alternative*. In Panel A, funds are divided based on whether the plan is headquartered in a *Red* or *Blue* state. In Panel B, funds are divided based on the indicator variable *Active*, set to one if a public pension plan is headquartered in a state that has signed greater or equal to the median number of multi-state coalition letters signed in the full sample. In Panel C, funds are divided based on the indicator variable *State*, equal to one if the public pension fund is a state-level plan. The main independent variable of interest is *Post*, a time-varying indicator set to one if a plan's fiscal year-end occurs after the effective date of a *Restriction*. Standard errors are double clustered by plan and fiscal year-end (FYE).

Panel A: Asset Allocation Changes *Red* versus *Blue* States

	Red			*Blue*	
	(1)	(2)		(1)	(2)
VARIABLES	Traditional	Alternative	VARIABLES	Traditional	Alternative
Post	-0.034**	0.047***	Post	-0.009	0.007
	(-2.03)	(2.69)		(-0.39)	(0.39)
Controls	Yes	Yes	Controls	Yes	Yes
Plan FE	Yes	Yes	Plan FE	Yes	Yes
FYE FE	Yes	Yes	FYE FE	Yes	Yes
Observations	2,223	2,223	Observations	1,848	1,848
Adj. R-squared	0.759	0.729	Adj. R-squared	0.700	0.753

Panel B: Asset Allocation Changes in *Active* versus Non-Active States

	Active = 1			*Active* = 0	
	(1)	(2)		(1)	(2)
VARIABLES	Traditional	Alternative	VARIABLES	Traditional	Alternative
Post	-0.037*	0.042**	Post	-0.028	0.025
	(-1.84)	(2.31)		(-1.32)	(1.53)
Controls	Yes	Yes	Controls	Yes	Yes
Plan FE	Yes	Yes	Plan FE	Yes	Yes
FYE FE	Yes	Yes	FYE FE	Yes	Yes
Observations	1,892	1,892	Observations	2,179	2,179
Adj. R-squared	0.772	0.732	Adj. R-squared	0.699	0.737

Panel C: Asset Allocation Changes in State versus Local Funds

	State			*Local*	
	(12)	(13)		(1)	(2)
VARIABLES	Traditional	Alternative	VARIABLES	Traditional	Alternative
Post	-0.054**	0.058**	Post	0.008	-0.005
	(-2.60)	(2.54)		(0.34)	(-0.27)
Controls	Yes	Yes	Controls	Yes	Yes
Plan FE	Yes	Yes	Plan FE	Yes	Yes
FYE FE	Yes	Yes	FYE FE	Yes	Yes
Observations	2,507	2,507	Observations	1,549	1,549
Adj. R-squared	0.748	0.739	Adj. R-squared	0.723	0.715

Table 5. **Performance Effects of the Investment Restrictions**. This table reports results of multivariate regressions on pension plans' investment performance across different asset classes. The main performance measure is *Peer-Adjusted Return*, calculated by subtracting the average return of the other plans during the same fiscal year from a plan's return. The main independent variable of interest is *Post*, a time-varying indicator set to one if a plan's fiscal year-end occurs after the effective date of a *Restriction*. The standard errors are double clustered by plan and fiscal year-end (FYE).

	Peer-Adjusted Return								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	1 Year	Traditional	Alternative	Equity	Fixed Income	Cash	PE	Hedge Fund	Real Estate
Post	0.003	0.007***	-0.004*	0.005	0.013***	-0.001	0.004	-0.023**	-0.006
	(0.75)	(3.43)	(-1.97)	(1.24)	(3.74)	(-0.20)	(0.48)	(-1.99)	(-0.80)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FYE FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,009	1,463	1,212	3,698	3,667	1,383	2,266	1,487	2,621
Adj. R-squared	0.0373	0.0691	0.0247	0.0256	-0.0181	0.0323	0.0093	-0.0206	-0.0021

Table 6. Equity Holdings Analysis. This table reports of multivariate regressions on pension plans' equity holdings within the sample of pension plans affected by an investment restriction. Panel A examines the full sample, while Panel B examines subsamples of blue and red state plans, and Panel C compares blue to red plans. Energy (Guns)%_MV is the proportion of a plan's equity holdings invested in energy (gun) stocks. Energy (Guns)_Count is the number of energy (guns) stocks held. The main independent variable of interest is Post, a time-varying indicator set to one if a plan's fiscal year-end occurs after the effective date of a Restriction. The standard errors are clustered by plan and the period is 2000-2023.

Panel A: Energy and Guns Holdings

	(1)	(2)	(3)	(4)
VARIABLES	Energy%_MV	Energy_Count	Guns%_MV	Guns_Count
Post	0.000	0.002	-0.000	-0.001*
	(0.20)	(0.46)	(-0.87)	(-1.84)
LogHV	-0.012	-0.011**	-0.000	-0.000
	(-1.77)	(-2.52)	(-0.61)	(-0.68)
Year-Quarter FE	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes
Observations	759	759	759	759
Adj. R-squared	0.8256	0.4993	0.3762	0.5193

Panel B: Red and Blue State Subsamples

	Red				*Blue*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Energy%_MV	Energy_Count	Guns_Count	Guns%_MV	Energy%_MV	Energy_Count	Guns_Count	Guns%_MV
Post	-0.003	-0.001	-0.000	-0.001	-0.000	-0.004*	-0.001*	-0.000*
	(-1.10)	(-0.22)	(-0.34)	(-0.63)	(-2.00)	(-4.00)	(-3.02)	(-4.15)
LogHV	-0.014*	-0.010*	0.000	-0.000	0.005	-0.015**	-0.001	-0.001
	(-2.23)	(-2.16)	(0.57)	(-1.07)	(1.19)	(-8.87)	(-1.74)	(-1.86)
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	500	500	500	500	258	258	258	258
Adj. R-squared	0.7614	0.4279	0.4166	0.2718	0.9885	0.8397	0.8089	0.9375

Panel C: Red versus Blue

VARIABLES	(1) Energy%_MV	(2) Energy_Count	(3) Guns%_MV	(4) Guns_Count
Post	0.001	0.006	-0.000	-0.001*
	(0.28)	(0.72)	(-0.59)	(-2.14)
Post*Blue	-0.002	-0.011	-0.000	0.000
	(-0.28)	(-1.08)	(-0.88)	(0.68)
LogHV	-0.012	-0.011**	-0.000	-0.000
	(-1.70)	(-2.37)	(-0.71)	(-0.59)
Year-Quarter FE	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes
Observations	759	759	759	759
Adj. R-squared	0.8254	0.503	0.3762	0.519

Table 7. Media Attention of Politicians. This table reports regressions estimating changes in media coverage of politicians after investment restrictions. The analysis is limited to states with public pension plans subject to such restrictions, using media coverage data from 2018-2023. The dependent variable, *Total Articles,* is defined as the number of news articles in which a politician is mentioned in the same article as an ESG- or restriction-related term. Panel A presents estimates comparing pre- and post-restriction media attention using OLS and matched samples. Panel B divides the sample by state political lean (Red vs. Blue). Panel C compares red and blue states. Standard errors are clustered by state.

Panel A: Total Article Mentions				
	OLS		Matched sample	
	(1)	(2)	(3)	(4)
VARIABLES	Total Articles			
Post	2.900***	1.356	2.905***	1.963***
	(3.95)	(1.15)	(4.08)	(2.98)
Election Cycle	0.770	-0.251	0.164	-0.194
	(1.53)	(-0.29)	(0.61)	(-0.45)
Year FE	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes
Observations	96	96	192	192
Adj. R-squared	0.27	0.2995	0.2802	0.3094
Panel B: Red and Blue Subsamples				
	OLS		Matched sample	
	Red	Blue	Red	Blue
	(1)	(2)	(3)	(4)
VARIABLES	Total Articles			
Post	0.309	0.696	2.120**	1.094
	(0.13)	(0.65)	(2.52)	(1.11)
Election Cycle	-0.069	0.561	-0.084	0.090
	(-0.08)	(0.28)	(-0.18)	(0.19)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Observations	66	30	132	60
Adj. R-squared	0.3427	0.3932	0.3193	0.4487
Panel C: Media Coverage in Red vs. Blue States				
	OLS		Matched sample	
	(1)	(2)	(3)	(4)
VARIABLES	Total Articles			
Post	1.552**	0.250	1.615*	0.789
	(1.87)	(0.17)	(1.83)	(0.84)
Post*Red	2.065	2.051*	1.98	1.863
	(1.57)	(1.78)	(1.55)	(1.53)
Election Cycle	0.843	-0.078	0.197	-0.126
	(1.63)	(-0.09)	(0.72)	(-0.30)
Year FE	No	Yes	No	Yes
State FE	Yes	Yes	Yes	Yes
Observations	96	96	192	192
Adj. R-squared	0.1957	0.3142	0.2966	0.3231

Table 8. State-level Sectoral Employment. This table reports state-level regressions estimating changes in employment within sectors targeted by investment restrictions. The analysis is limited to states with public pension plans subject to such restrictions. *%FossilFuelEmpl* denotes the share of a state's workforce employed in fossil fuel–related industries (e.g., oil, gas, coal), while *%WeaponEmpl* captures the share employed in firearms and ammunition manufacturing. Panel A reports pre/post estimates across the full sample. Panel B presents results by political alignment. Panel C tests for differential effects between red and blue states. Standard errors are clustered at the state level. The sample period is 2018–2023.

Panel A: Employment in Fossil Fuel and Weapon-related Sectors

	(1)	(2)	(3)	(4)
VARIABLES	%FossilFuelEmpl	%WeaponEmpl	%FossilFuelEmpl	%WeaponEmpl
Post	-0.011	-0.001	0.169	0.012
	(-0.14)	(-0.12)	(1.26)	(0.69)
State FE	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes
Observations	96	90	96	90
Adj. R-squared	0.882	0.6811	0.9003	0.7079

Panel B: Red and Blue Subsamples

	Red		Blue	
	(1)	(2)	(3)	(4)
VARIABLES	%FossilFuelEmpl	%WeaponEmpl	%FossilFuelEmpl	%WeaponEmpl
Post	0.095	0.026	0.008	-0.003
	(0.51)	(0.90)	(1.28)	(-0.21)
State FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	66	66	30	24
Adj. R-squared	0.8846	0.7270	0.7523	0.6162

Panel C: Sector Employment in Red vs. Blue states

	(1)	(2)
VARIABLES	%FossilFuelEmpl	%WeaponEmpl
Post	0.230	-0.001
	(1.61)	(-0.06)
Post*Red	-0.109	0.021
	(-0.84)	(1.37)
State FE	Yes	Yes
Year FE	Yes	Yes
Observations	96	90
Adj. R-squared	0.8998	0.7169

Table 9. Firm Market Reaction to Investment Restrictions. This table reports cumulative abnormal returns (CARs) for firms around investment restriction announcements. Panel A presents average CARs for firms in the energy sector, pooling all restriction announcements. Columns (1)-(4) reports results for the full sample; columns (5)-(8) split the sample based on whether the restriction is announced in a red or blue state. Standard errors are clustered by firm. Panel B focuses on the Oklahoma Restricted Financial Company List (HB2034), reporting mean CARS for affected firms. Panel C displays firm-level CARs for each company named on Oklahoma's restricted list. All estimates are centered on the relevant state's announcement date.

Panel A: Market Reaction to Investment Restrictions (Energy Firms)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Event Window							
	(-3,3)	(0,1)	(0,3)	(-1,1)	(-3,3)	(0,1)	(0,3)	(-1,1)
CAR	-0.002	-0.003	-0.009***	-0.006***				
T-Statistic	(-0.62)	(-1.60)	(-3.93)	(-2.36)				
CAR_Blue					0.029***	0.025***	0.009***	0.036***
T-Statistic					(4.39)	(7.60)	2.11)	(7.87)
CAR_Red					-0.012***	-0.012***	-0.014***	-0.018***
T-Statistic					(-3.23)	(-5.07)	(-5.02)	(-7.01)
Observations	1267	1267	1267	1267	1267	1267	1267	1267
Adj. R-squared	0.0003	0.0024	0.0097	0.0052	0.0217	0.0585	0.02	0.0947
F-statistic	0.39	2.56	15.47	5.58	15.09	42.32	13.5	57.93

Panel B: Market Reaction to Oklahoma's Financial Company Blacklist Announcement

	(1)	(2)	(3)	(4)
	Event Window			
	(-3,3)	(0,1)	(0,3)	(-1,1)
CAR	-0.023***	-0.029***	-0.020***	-0.036***
T-Statistic	(-3.85)	(-6.56)	(-5.88)	(-6.80)
Observations	7	7	7	7
Adj. R-squared	0.7122	0.8776	0.852	0.8851
F-statistic	14.84	43.03	34.53	46.21

Panel C: Firm-Level CARs for Companies Named in Oklahoma's Blacklist

	(1)	(2)	(3)	(4)
Firms	Event Window			
	(-3,3)	(0,1)	(0,3)	(-1,1)
GCM GROSVENOR	-0.032	-0.051	-0.032	-0.037
STEPSTONE GROUP	0.001	-0.018	-0.009	-0.026
WELLS FARGO & CO	-0.021	-0.034	-0.007	-0.06
JPMORGAN CHASE & CO	-0.003	-0.025	-0.021	-0.03
BANK OF AMERICA	-0.036	-0.028	-0.019	-0.045
STATE STREET	-0.033	-0.034	-0.027	-0.036
BLACKROCK	-0.035	-0.015	-0.024	-0.017

Table A1. Multivariate Regressions of Public Pension Fund Allocation in a 5-Year Window. This table reports results of multivariate regressions of pension funds for plan-fiscal year-end observations within a five-year window from the date the restriction is effective. The dependent variable is fund allocation across different asset classes. The main independent variable of interest is *Post*, a time-varying indicator set to one if a plan's fiscal year-end occurs after the effective date of a *Restriction*. The standard errors are double clustered by plan and fiscal year-end (FYE).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Traditional	Alternative	Equity	Fixed Income	Cash	Private Equity	Hedge Fund	Real Estate
Post	-0.029**	0.017*	-0.007	-0.018*	-0.005	0.004	0.006	0.008*
	(-2.23)	(1.73)	(-0.57)	(-1.94)	(-1.22)	(0.66)	(1.14)	(1.71)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FYE FE	No	No	No	No	No	No	No	No
Observations	485	485	485	485	485	485	485	485
Adj. R-squared	0.861	0.905	0.849	0.779	0.502	0.869	0.830	0.852
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Traditional	Alternative	Equity	Fixed Income	Cash	Private Equity	Hedge Fund	Real Estate
Post	-0.019*	0.015*	-0.011	-0.007	-0.001	-0.008	0.014***	0.009*
	(-1.99)	(1.69)	(-0.89)	(-0.62)	(-0.15)	(-1.19)	(5.64)	(1.82)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FYE FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	485	485	485	485	485	485	485	485
Adj. R-squared	0.861	0.906	0.860	0.781	0.520	0.871	0.820	0.858

Table A2. Multivariate Regressions of Public Pension Fund Target Allocation. This table reports results of multivariate regressions of pension funds' target allocations to different asset classes. The main independent variable of interest is *Post,* a time-varying indicator set to one if a plan's fiscal year-end occurs after the effective date of a *Restriction*. The standard errors are double clustered by plan and fiscal year-end (FYE).

	Asset Allocation							
	(1)	(2)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Traditional	Alternative	Equity	Fixed Income	Cash	Private Equity	Hedge Fund	Real Estate
Post	-0.044***	0.057***	-0.028**	-0.016*	-0.001	-0.000	0.031***	0.026***
	(-3.52)	(4.86)	(-1.99)	(-1.70)	(-0.44)	(-0.04)	(4.12)	(3.28)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Plan FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FYE FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,688	3,688	3,688	3,688	3,688	3,688	3,688	3,688
Adj. R-squared	0.758	0.747	0.707	0.593	0.566	0.701	0.591	0.694

Table A3. **Political Lean of Multi-State Coalition Signers**. This table reports results of a subsample multivariate regression of pension funds' allocations to traditional and alternative assets. Funds are divided into subsamples based on the indicator variables *Red* (*Blue*) *Signer*, set to one if a public pension plan is headquartered in a state where a Republican (Democratic) political official signs at least one multi-state coalition letter. The main independent variable of interest is *Post*, a time-varying indicator set to one if a plan's fiscal year-end occurs after the effective date of a *Restriction*. The standard errors are double clustered by plan and fiscal year-end (FYE).

Asset Allocation					
	Red Signer			*Blue Signer*	
	(1)	(2)		(1)	(2)
VARIABLES	Traditional	Alternative	VARIABLES	Traditional	Alternative
Post	-0.058**	0.067***	Post	-0.024	0.025*
	(-2.43)	(3.88)		(-1.46)	(1.70)
Controls	Yes	Yes	Controls	Yes	Yes
Plan FE	Yes	Yes	Plan FE	Yes	Yes
FYE FE	Yes	Yes	FYE FE	Yes	Yes
Observations	2,050	2,050	Observations	2,063	2,063
Adj. R-squared	0.7707	0.7408	Adj. R-squared	0.6951	0.7392

Table A4. Performance Effects of Investment Restrictions—Full Model. This table presents the full multivariate regression results corresponding to Table 5. Standard errors are double clustered by plan and fiscal year-end.

	Peer-Adjusted Returns								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	1 Year	Traditional	Alternative	Equity	Fixed Income	Cash	Private Equity	Hedge Fund	Real Estate
Post	0.003	0.007***	-0.004*	0.005	0.013***	-0.001	0.004	-0.023**	-0.006
	(0.75)	(3.43)	(-1.97)	(1.24)	(3.74)	(-0.20)	(0.48)	(-1.99)	(-0.80)
Log Plan Size (t-1)	-0.003	-0.004	0.008	-0.006	-0.000	0.004	-0.011	0.012	0.021
	(-1.21)	(-0.98)	(1.43)	(-1.29)	(-0.11)	(0.83)	(-0.78)	(0.25)	(1.25)
Plan Funding % (t-1)	-0.020***	0.001	-0.007	-0.005	-0.002	-0.004	0.096***	-0.025	-0.035
	(-2.86)	(0.10)	(-0.64)	(-0.42)	(-0.24)	(-0.40)	(2.69)	(-0.30)	(-1.26)
Peer-Adjusted (t-1)	-0.079								
	(-1.30)								
% Equity Allocation (t-1)	0.003	0.010							
	(0.12)	(0.90)							
% Fixed Income Allocation (t-1)	-0.025	-0.008							
	(-1.14)	(-0.61)							
% Cash Allocation (t-1)	-0.070*	-0.056*							
	(-1.79)	(-1.74)							
% Private Equity Allocation (t-1)	0.002		-0.024						
	(0.06)		(-1.16)						
% Hedge Fund Allocation (t-1)	-0.048*		0.003						
	(-1.88)		(0.35)						
% Real Estate Allocation (t-1)	-0.004		-0.023						
	(-0.16)		(-1.02)						
% Commodity Allocation (t-1)	-0.032								
	(-0.77)								
% Alternatives Misc Allocation (t-1)	0.005								
	(0.20)								
Peer-Adj. Equity (t-1)		-0.052		-0.121**					
		(-1.30)		(-1.99)					
Peer-Adj. Fixed Income (t-1)		0.012			-0.054				
		(0.56)			(-0.73)				
Peer-Adj. Cash (t-1)		0.067***				0.120			

		(3.10)				(1.19)			
Peer-Adj. Risky			.						
			(.)						
Peer-Adj. Private Equity (t-1)			0.003				0.024		
			(0.44)				(0.52)		
Peer-Adj. Hedge Fund (t-1)			-0.012*					-0.066	
			(-1.67)					(-0.75)	
Peer-Adj. Real Estate (t-1)			-0.032**						-0.029
			(-2.15)						(-0.58)
Plan FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FYE FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,009	1,463	1,212	3,698	3,667	1,383	2,266	1,487	2,621
Adj. R-squared	0.0461	0.0722	0.1832	0.0313	0.0284	0.053	0.0614	-0.0235	0.0482

Table A5. Political Leaning by State.

Red States	Plan-FYEs	Blue States	Plan-FYEs
Alabama	88	California	330
Alaska	66	Colorado	110
Arizona	110	Connecticut	91
Arkansas	88	Delaware	88
Florida	110	District of Columbia	44
Georgia	110	Hawaii	22
Idaho	22	Illinois	223
Indiana	66	Maine	44
Iowa	66	Maryland	110
Kansas	66	Massachusetts	68
Kentucky	88	Minnesota	148
Louisiana	198	New Hampshire	44
Michigan	110	New Jersey	88
Mississippi	22	New Mexico	44
Missouri	308	New York	176
Montana	44	Oregon	22
Nebraska	88	Rhode Island	68
Nevada	44	Vermont	88
North Carolina	88	Virginia	110
North Dakota	88	Washington	110
Ohio	114	Total	2028
Oklahoma	132		
Pennsylvania	156		
South Carolina	66		
South Dakota	66		
Tennessee	88		
Texas	286		
Utah	44		
West Virginia	88		
Wisconsin	66		
Wyoming	22		
Total	2,998		

Table A6. Anecdotal Evidence on the Costs of Investment Restrictions. This table provides the transactional details for three public pension plans affected by an investment restriction. It summarizes what the restriction targets, along with the type of restriction, divested amounts, and fees (expressed as a percentage of investment market value) associated with both the divestment and subsequent investments.

Panel A: Missouri State Employees' Retirement System

Category	Description	Divestment	Investment	Fee Delta ($)	Fee % Change
Type	Board policy (6/2022) leads to BlackRock divestment (10/2022)	Commingled fund holding Blackrock Inc.	*Hedge Funds:* Blue Diamond Asset ($75 M); DE Shaw ($22,349,548.77); Eisler ($206,760,449.65); Millennium ($52.5 M); Aberdeen ($118,171,691)		
Amount ($)		$ 500,000,000.00	$ 474,781,689.42		
Fee ($)		$ 2,548,740.00	$ 30,398,488.00	$ 27,849,748.00	10.93%
Fee (%)		0.51%	6.40%	5.89%	11.56%

Panel B: Texas Employees' Retirement System

Category	Description	Divestment	Investment	Fee Delta ($)	Fee % Change
Type	Financial Institution Blacklist legislation effective (9/2021)	*Individual Public Securities held by external manager*: BNP Paribas SA held by GQG Partners Intl and Svenska Handelsbanken AB held by Arrowstreet Intl; *External Manager Contracts*: Pzena and BlackRock (Public equity)	*Public Equity Manager:* GQG Partners 10/1/22 ($99,996,808.7l); Global Transition 3/1/23 ($4,720.17). *Private Credit:* Benefit St Ptnr Opps T LP 9/1/22 ($752,103.12); Benefit St Ptn II 4/1/22 ($161,53,079.50); VWH Master Fund III LP 12/1/21 ($8,505,265.92); All Seas Capital I SCSP 12/1/21 ($31,976,042.59). *Hedge Funds:* Tenor Capital Management 11/1/21 ($75,369,823.55); Lijaro VI Global Equity Master Fund 12/1/21 ($59,810,948); Cinctive Global Macro 5/1/22 ($103,433,372); Tresidor European Cred Fd 6/1/23 ($75 M); Newton Dynamic US Eq 6/1/23 ($105,319,394.91); Serenitas Credit Gamma FD 9/1/23 9/1/23 ($50 M)		
Amount ($)		$ 645,899,676.29	$ 626,321,558.47		
Fee ($)		$ 2,488,886.45	$ 5,052,559.89	$ 2,563,673.44	1.03%
Fee (%)		0.39%	0.81%	0.42%	1.09%

Panel C: Texas Municipal Retirement System

Category	Description	Divestment	Investment	Fee Delta ($)	Fee % Change
Type	Financial Institution Blacklist legislation effective (9/2021)	*External Manager Contract:* BlackRock for Core Fixed Income (Separately Managed Account)	*Private Credit:* TCW Brazos Fund 12/22 ($200 M); Arrow Credit Opportunities II USD Feeder SCSp 3/23 ($150 M); Pemberton Mid-Market Debt Fund 10/22 ($50 M); *Real Estate*: Oak Street Capital 12/22 ($150 M); Stonepeak RE Partners 10/22 ($100 M); Platform TX fund 8/22 ($150 M); *Real Asset*: Appian Natural Resource (UST) Fund III LP 12/22 ($100 M); Warren Equity Partners Fund IV, L.P. 9/22 ($100 M); *Other Alternative:* Ara Fund III, LP 10/22 ($75 M); Ara Co-Investment T, LP 10/22 ($75 M); Gamut Investment Fund Bluebonnet Co-Invest 9/22 ($60 M); Gamut Investment Fund II, L.P. 8/22 (120 mill); Oberland Capital Healthcare 8/22 ($110 M); Oberland Capital Healthcare III Brazos Co-Invest, LP 8/22 ($55 M); *Private Equity:* Greenoaks Capital Opportunities Fund V LP 8/22 ($60 M)		
Amount ($)		$ 1,520,659,794.10	$ 1,555,000,000.00		
Fee ($)		$ 8,223,356.83	$ 15,101,161.30	$ 6,877,804.47	0.84%
Fee (%)		0.54%	0.97%	0.43%	0.80%

Table 6 Supplementary Details

13F Manager Names
alaska retirement mgmt bd
arizona state retirement sys
calif public emp. ret.
calif state teachers ret
colorado public emp ret
employees retirement sys of tx
florida st board/admin.
kentucky ret systems ins tr fd
kentucky teachers retrm
maryland state retirement pens
michigan state treasurer
new mexico educ. ret. bd
new york st common ret.
new york st teachers ret
ohio public emp ret sys
ohio state teach ret brd
oregon public emp ret fd_nle
pennsylvania public school emp
retirement systems of alabama
south dakota invt council
state nj common pension fd d
state nj common pension fd e
state of tn, treasury dept
texas teacher retmt sys
treasurer of the state of nc
utah retirement systems
washington state investment bd
wisconsin invst board

SIC Codes for Industry Holdings Measures

Guns and Defense (*Guns%_MV/Guns_Count*)	Energy (*Energy%_MV)/Energy_coun*t)	
3480–3489	1200–1299	4924–4925
3760–3769	1300–1399	4930–4931
3795	2900–2999	4932
	4610–4629	4939
	4920–4922	5160–5169
	4923	5170–5172

Table 8 Supplementary Details

NAICS Codes for Employment Measures

Firearms and Ammunition		Fossil Fuels	
332994	Small Arms, Ordnance, and Accessories Manufacturing	211120	Crude Petroleum Extraction
332992	Small Arms Ammunition Manufacturing	211130	Natural Gas Extraction
332993	Ammunition (except Small Arms) Manufacturing	212111	Bituminous Coal and Lignite Surface Mining
		213111	Drilling Oil and Gas Wells
		221210	Natural Gas Distribution
		221112	Fossil Fuel Electric Power Generation
		237120	Oil and Gas Pipeline Construction
		423520	Coal and Other Mineral and Ore Merchant Wholesalers
		424710	Petroleum Bulk Stations and Terminals
		424720	Petroleum and Petroleum Products Merchant Wholesalers (Except Bulk Stations and Terminals)
		325110	Petrochemical Manufacturing

Oklahoma Restricted Financial Company List (HB 2034)

TODD Russ
STATE TREASURER

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OKLAHOMA CITY, OK 73105

OKLAHOMA STATE TREASURER

RESTRICTED FINANCIAL COMPANY LIST

Pursuant to 74 O.S. § 12003 (A) of the Energy Discrimination Elimination Act of 2022, the Oklahoma State Treasurer is required to prepare and maintain a list of financial institutions that boycott energy companies as defined in 74 O.S. §12002(A)(1). Financial companies included on the list are referred to herein as "Restricted Financial Companies."

Pursuant to 74 O.S. §12003(B), not later than the thirtieth day after a state governmental entity receives notice of this list of Restricted Financial Companies, the state governmental entity shall notify the Treasurer of the Restricted Financial Company(s) in which the state governmental entity owns direct or indirect holdings, and, send a written notice to the Restricted Financial Company(s) as prescribed in 74 O.S. § 12003(C)(1)(a-c).

Not later than 90 days of a Restricted Financial Company's receipt of notice under 74 O.S. §12003(C)(2), the financial company must cease engaging in the energy company boycott to avoid qualifying for divestment by the state governmental entity.

The State Treasurer shall, at a minimum; update this list on a per annum basis.

Pursuant to 74 O.S. § 12002, et seq., and based upon thorough review of responses to a questionnaire requesting written verification sent by the Oklahoma State Treasurer, publicly available statements, commitments, and/or any company's failure to respond to the questionnaire required under 74 O.S. § 12003(A)(1)(2), the Oklahoma State Treasurer's Office has determined that the financial companies listed below are engaging in energy company boycotts. Accordingly, each is hereby placed on the Oklahoma State Treasurer's Restricted Financial Companies List.

- BLACKROCK, INC
- WELLS FARGO & CO.
- JPMORGAN CHASE & CO.
- BANK OF AMERICA N.A.
- STATE STREET CORP.
- GCM GROSVENOR
- LEXINGTON PARTNERS
- FIRSTMARK FUND PARTNERS
- STEPSTONE VC GLOBAL PARTNERS
- WCM INVESTMENT MANAGEMENT
- WILLIAM BLAIR
- ACTIS
- CLIMATE FIRST BANK

Signed:

Effective Date: May 3, 2023

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